SENTENCING AND RECIDIVISM

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*Views in this document are those of the authors and do not necessarily represent the official position of the U.S. Department of Justice, The National Institute of Justice, or the New Jersey Administrative Office of the Courts.

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SENTENCING AND RECIDIVISM

EXECUTIVE SUMMARY

This report grew out of two grants from the National Institute of Justice to the State of New Jersey Administrative Office of the Courts. Data processing, analysis, and the writing of the report were subcontracted to the Institute for Criminological Research of Rutgers University. The original project was designed to evaluate how sentences affected the subsequent recidivism of a sample of convicted offenders. The report includes such an evaluation, as well as more general considerations of the study of recidivism and its use in the sentencing of offenders. In this summary, we discuss the contents of most chapters very briefly, and focus instead on the results of two of the chapters (Eight and Ten). A further overview of the contents of all chapters may be found in the Abstract.

The assessment, at sentencing, of an offender's risk for recidivism is taken as a premise of the research, and how this relates to the different goals of sentencing is discussed in Chapter One. In Chapter Two, several substantive and methodological issues pertaining to the use of actuarial or statistical models to measure and predict recidivism are raised. Choices among predictor variables are reviewed, with attention given to the logical and temporal nature of such variables. Chapter Three details the sample and data processing steps leading to our analyses. In Chapter Four, the specific indicators of recidivism that are studied in detail are enumerated. A general model of recidivism, based on the characteristics of the offender and his/her prior record, is developed and tested using a single recidivism indicator (rearrest) in Chapter Five. Other measures of recidivism are similarly modeled in Chapter Six. In Chapters Seven and Eight, the impacts of various

sentencing interventions are assessed. Chapter Nine addresses several substantive and methodological issues in the prediction of an individual's probability of recidivism. Policy implications are discussed in the concluding chapter.

Subject to several important caveats that are raised in Chapter Eight, we do find evidence of effects for the sentences studied. At the risk of oversimplifying here, we find that sentences to State Prisons are associated with decreased levels of recidivism, though the exact mechanisms leading to this decrease are unclear at best. Across most definitions of recidivism, we find that those sentenced to serve time in a State Prison are significantly less likely to recidivate. This "positive" outcome is tempered, however, by the fact that over 62% of those offenders receiving this form of sentence are eventually rearrested. Our interpretation of this apparent contradiction is that, given the types of individuals sent to a State Prison, levels of recidivism should have been higher than those actually observed. Consequently, we conclude that State Prison sentences have resulted in some "selective incapacitative" effects, and possibly some <u>temporary</u> specific deterrent, or rehabilitative, effects.

Given some definitions of recidivism, sentences to jail are also found to be related to lower levels of recidivism. Here too, however, the observed levels of recidivism are quite high, and we conclude that, at best, the intervention of a jail sentence has only "slowed down" the offending of this group. On a more general level, our findings point to successful decision making on the part of judges and to successful operation of the criminal justice system as a whole. The evidence suggests that judges were particularly good at identifying those individuals with longer prior records who would fare will under a sentence to probation.

Conversely, we find little evidence that the sentencing of young adult offenders to the Youth Complex at Yardville is "effective." This group has the highest levels of recidivism, however defined. Nor do we find many effects attributable to the "length" of the sentence (i.e., dollars fined, time sentenced to jail or probation, and, to a lesser extent, time sentenced to prison). Few of these kinds of variables are significantly related to recidivism and, when significant effects are observed, the coefficients are exceedingly small. Disappointing results are also found for the use of progressively more punitive sanctions. As well, those in this sample who had never been sanctioned previously fare quite poorly in terms of subsequent recidivism. Not surprisingly, we find strong evidence that offenders who have accumulated many arrests, prior to receiving the first sentence of their criminal career, show significantly higher levels of recidivism. The policy implications of our analyses are detailed in Chapter Ten.

Throughout, we find that the characteristics of the individual (e.g., prior offending, education, and employment) and the characteristics of the crime (e.g., type of offense) leading to sentencing are more predictive of subsequent recidivism than are the aspects of the sentence received. We suggest that the use of assessed risk for recidivism <u>at the point of sentencing</u> be more formalized than is currently the case. Through the identification of high and low risk offenders, more appropriate sentences can be fashioned and better use can be made of the limited criminal justice system resources. We view the use of risk assessment at sentencing as quite compatible with the increased use of "intermediate" punishments and the continued use of the more traditional forms of sentencing, such as probation and incarceration.

SENTENCING AND RECIDIVISM

ABSTRACT

The present report investigates how recidivism may be better utilized in the decision making of the Criminal Justice System (CJS). In particular, we focus on the evaluation of a broad range of CJS sanctions as represented by the sentencing of convicted offenders. More generally, we discuss philosophies of intervention and punishment and the relevance of considering recidivism for such philosophies. Our empirical results lead us to several suggestions as to how the risk that an offender will recidivate can be incorporated into fashioning appropriate CJS interventions.

The investigation is wide in scope. In Chapter One, we begin with a review of recent historical changes in sentencing practices, emphasizing the movement toward limiting judicial "discretion" through the use of sentencing guidelines, statutory minimum terms, legislated punishment, and so forth. This leads to a broader discussion of the, often competing, goals of sentencing. We suggest that these goals are not exclusive, and that a concern for reducing offender recidivism underlies most of them. We then offer some suggestions for the shape of an integrated sentencing policy.

Chapter Two discusses several aspects of recidivism as a component of risk assessment in CJS decision making. One of the barriers to use of risk of further criminal behavior is the vast array of ways in which recidivism has been conceptualized and measured. After reviewing this diversity, we propose a typology of recidivism indicators which is then used to guide our empirical analyses. How offender risk is currently incorporated in the CJS (e.g., probation risk assessments, parole release decisions) is also discussed. Particular attention is given to which variables are currently seen as legitimate for determining risk. Conversely, the use of some variables (e.g., offender race and ethnicity) as predictors raises several ethical issues. The debate surrounding these is reviewed and some potential solutions are offered.

The data used for our analyses is the topic of Chapter Three. The sample covers all offenders convicted for an indictable offense in a New Jersey State court during a one year period spanning 1976 and 1977. All data come from official records taken from different parts of the CJS. Presentence Investigation Reports prepared by probation officers are used to generate many of the variables describing the offender and the nature of his/her presenting offense. Judgement of Conviction sheets are the source of information about the sentence received. Official records supplied by the New Jersey State Police are used to yield various forms of prior criminal behavior and subsequent criminality up to over nine years after sentencing. Incarceration histories taken from the New Jersey Department of Corrections augment information about previous CJS interventions and yield additional data about recidivism. The procedures used to define our final sample of 11,749 offenders, as well as the handling of the data processing, are outlined. The empirical results in this chapter illustrate the basic fact that the sentences given this sample yield groups that are quite different.

In Chapter Four we empirically investigate the many ways that recidivism can be measured as well as the consequences of looking for recidivism over post-sentence observation periods (or "windows") of different lengths. Guided by the typology introduced in Chapter Two, we select 37 indicators of recidivism, measured over four different windows, for detailed study. We find

that how recidivism is defined, as well as when it is measured, matters as there are identifiable differences in the empirical behavior of these indicators across measures and windows. Guided by these results, we select 13 different forms of recidivism to be used in the models developed in later chapters.

We argue in Chapter Five that, in one sense, recidivism is an aspect of criminal behavior in general, and thus theories about the etiology of crime are relevant to developing models that attempt to explain or predict variability in recidivism across offenders. We briefly review theories of criminal behavior, noting where they point to different (and similar) independent variables for models of recidivism. Based upon this review, we categorize six types of independent variables that can be used to model recidivism and operationalize 104 indicators for these six domains. A winnowing procedure that allows us to find those independent variables with robust effects across definitions of recidivism and post-sentence observation windows is introduced and used to reduce our set of predictors. Also studied here are statistical interactions among these independent variables pointing to where levels of recidivism differ across subgroups. Finally, a preliminary look at how these independent variables are related to recidivism is provided by a detailed analysis of the likelihood that an offender will be rearrested for some offense.

One assumption underlying this study is that the independent variables of the previous chapter should be controlled prior to assessment of the effectiveness of the sentence itself. These variables represent what is known about the offender by the CJS before the introduction of the intervention represented by the sentencing of the offender. Consequently, Chapter Six focuses on the predictive utility of these variables across several definitions of recidivism. Three themes emerge from these analyses. First, a communality analysis finds that much of the predictive power of these variables is shared across domains, though each uniquely contributes to an ability to understand differences in the level of recidivism. Second, indicators of the individual's prior criminal behavior ("anamnestic" variables) and measures of the social structural characteristics of the offender (e.g., employment, education, place of residence) provide the best predictors of subsequent recidivism. Third, the way in which recidivism is measured matters as what predicts recidivism, and how much of the variation in recidivism can be explained, depends on the particular measure used.

Exactly how to measure the sentences received by this sample is the focus of Chapter Seven. In addition to the common aspects of whether or not the offender is incarcerated (the "in" versus "out" decision), where the offender is sentenced (e.g., probation, jail, prison), and how long the individual is to served (the "time" dimension), several other components of these sanctions are studied. We identify where the current sentence fits into his/her overall history of CJS interventions, as well as investigating how these components of the sentence are intercorrelated amongst themselves. Also discussed are the conceptual distinctions between evaluating the effects of the sentence itself as opposed to the treatment received as a consequence of that sentence. This distinction is important not only for the choice for measures of the sentence, but also for what can be concluded about the "effects" of these sentences. The limitations of evaluating only the sentence are discussed.

The winnowing procedure described in Chapter Five is again used in Chapter Seven to identify robust interactions of the sentence components with other independent variables, thus pointing to where the effects of these sanctions will differ across subgroups of offenders. One unanticipated result of Chapter Seven is the finding that almost half of these sentences are (or are likely to be) interdependent with sanctions other than those studied here. This introduces further difficulties in our ability to detect the impact of these sanctions. Finally, a preliminary look at the effects of sentences is given by a detailed reanalysis of the probability of rearrest first introduced in Chapter Five.

The bulk of our study of sentence effectiveness is contained in Chapter Eight. Mirroring the exposition in Chapter Six, we first show how knowledge of the sanction received can augment the explanation of differences in recidivism. Beyond what is known about the individual at the time of sentencing, the ability of the sentence to account for differences in recidivism across offenders is disappointingly slight. We then quantify these sentence effects through detailed models of the various forms of recidivism. As was the case in Chapter Six, we find that the substantive conclusions reached are, in part, dependent upon how recidivism is defined and the window over which it is observed.

The models developed in Chapters Five through Eight give an overview of what does, and does not, account for differences in levels of recidivism. The process of sentencing, however, requires that judges make decisions on an individual, case-by-case basis. Chapter Nine investigates how such decisions may differ depending upon what is used to predict recidivism. We show the empirical differences that arise when the different domains of independent variables, and the sanction received, are used to predict recidivism at the time of sentencing. Again we find that how recidivism is defined matters for the conclusions reached. More importantly, we find that which variables are used to predict recidivism (e.g., social structural, anamnestic, the sentence itself) has marked consequences for predicting whether a given individual will recidivate and whether that prediction is accurate.

In Chapter Ten, we reflect upon our findings in light of the issues raised in the earlier chapters. We argue that the offender's prospect of engaging in further criminal behavior is a legitimate concern to be considered at the point of sentencing. Our results point to which factors might be taken into account in assessing the risk of recidivism and the differences it makes in considering one set of factors at the expense of others. The consequences of our results surrounding the impact of sentences for the general goal of crime control are also discussed. These themes are then merged by our suggestions for a sentencing policy that incorporates the risk for recidivism with various sentencing interventions.

Two Appendices accompany this report. One of contentions raised throughout is that it is methodologically difficult to evaluate how sentences impact on subsequent levels of recidivism. Appendix A expands on these difficulties by discussing what has been called "sample selection bias." From the perspective of the CJS organization, the point of sentencing by a judge is just one aspect of the intervention itself. The characteristics of the offender and the presenting offense can influence the decisions of prior actors (e.g., the police, Grand Juries, prosecutors, juries) in ways that produce a nonrandom sample and, potentially, impact on the conclusions reached when that sample is used for any analyses. The focus in Appendix A is how to

measure sample selection bias and develop controls for it in our sample. Models for the probability that a case will pass several "filters" of the CJS are presented. In particular, we estimate how likely it was that each individual in our sample 1) had their arrested reported to the central record repository; 2) had further action taken after the arrest; 3) had his/her case presented to a Grand Jury; 4) was prosecuted before a Superior Court, and; 5) would be convicted in a Superior Court.

Appendix B consists solely of statistical results that control for our sample selection measures. The results are actually discussed in Chapters Six and Eight. In general, one major finding emerges from our use of these selection hazards: the probability that a case will, a priori, pass these filters accounts for about one quarter of the variance that can be explained in a given measure of recidivism. That is, the processing of a case through the CJS in and of itself can predict recidivism, thus reducing the explanatory power of the other variables discussed in this report. Aside from this impact, however, controls for selection bias yield few insights. We do find instances where the magnitude of the coefficients for a variable are changed by the introduction of statistical controls for the hazards, and there are very small suppressions of the impact of the sentences by these controls, but these too are slight. Overall, none of our substantive conclusions are altered by the nonrandom nature of the sample used.

Finally, the report represents the analytic component of our project. The other product of the research has been the creation of two data files. One contains the official arrest and incarceration histories of this sample. The other is comprised of information about the offender, presenting offense, sentence, and summary measures of prior record and subsequent recidivism. These files have already been released to the research community and are available from the Interuniversity Consortium for Political and Social Research. Two codebooks, and a descriptive account of the data processing leading to the creation of these files, are also available as documents separate from the current report.

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CHAPTER ONE

SENTENCING AND RECIDIVISM

This research addresses issues concerning the link between the concepts of sentencing and recidivism.[1] The topic will be considered by many to be archaic (decades of reform have aimed at removing considerations of recidivism from the sentencing decision) and by others to be timely. Linking these two concepts is even considered morally inappropriate by some ("just" sentences should not involve recidivism considerations), and a necessity by others (society must be protected from high risk offenders). The present work is about some of the theoretical and practical implications of connecting sentencing policies to research on the correlates of recidivism and about the effectiveness of sanctions imposed by the criminal courts.

Historically, the prospects that an offender will recidivate have been used explicitly and implicitly by judges in fashioning a sentence in criminal court. Through the early 1970s in all states recidivism entered into consideration in sentencing on a case-by-case, ad hoc basis. Various decision makers, such as judges, corrections personnel, and parole boards, for example, enjoyed virtually unbridled discretion in making decisions about the offender from the sentence to release. In more recent times, not only has the discretion of various decision makers been under attack in some states, but attempts have been made to make the nature of the criminal justice system's intervention more "prescribed" either through statutory sentence specifications or through sentencing guidelines. The aims of these reform

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[1] By sentencing it is meant the sanction given convicted offenders in criminal court, and by recidivism, the re-occurrence of criminal activity by those who are sentenced.

efforts have been in part to make the sentence less discretionary, less subject to change once issued, and more a function of the seriousness of the conviction offense (the "presenting offense"). Most states, however, have had only limited success at achieving these goals. It is one of our purposes here to outline some alternatives for the problems with existing sentencing practices and with attempts to reform it.

Lurking behind attempts at reform lie debates over the purposes of criminal justice system interventions. Here, the goals of retributionism (or "just deserts") have been given greater credence for the sentencing decision, while the assessment of an offender's risk to the community has seemingly taken on greater importance to the release decision. Thus, we have witnessed the popularity of the idea of sentencing guidelines for the former (even though few states have actually adopted meaningful guidelines), and the widespread use of risk assessment instruments for parole release and supervision decisions.

Formal consideration of the likelihood of recidivism has had an ambiguous role in these reform efforts. While it has been explicitly incorporated into release decisions, the so-called "back-door" decisions, it has not gained a firm foothold at the sentence itself (the "front door" to the system). The present work represents not only an attempt to examine how the prospect of an individual's recidivism may be incorporated into a sentence, but more generally addresses several fundamental issues in the conceptualization, measurement, modeling, and utilization of recidivism risk assessment for CJS decisions.

A brief historical overview will be given, not only of sentencing, but of other aspects of "subsequent" decision making in the criminal justice

system: decisions made after the sentence. Different organizations have traditionally shared in the responsibilities of defining and carrying out the sentence of the offender. Probation, corrections, parole, as well as sentencing, will be discussed in the context of the experience in New Jersey.

The historical considerations, as well as the attempts to eliminate recidivism from sentencing, lead us to conceptualize alternative ways in which sentencing and recidivism may be linked. The various goals of the sentencing process (and more generally of the criminal justice system's - CJS - "intervention" process) form part of the context for reform in sentencing. Several directions for future sentencing policy are discussed, leading to the formulation of fundamental questions about the role recidivism could play, if these directions were chosen by policy decision makers. As such, the work here is not prescriptive, but of a "what if" variety. Specifically, "what if" the sentencing process were to consider, in a more formal manner than is now the case, the probability of recidivism on the part of the individual offender? What are some of the basic questions that would have to be addressed to formulate such a policy directive? What are some of the implications that we can tentatively draw, based on our own empirical work on recidivism?

In subsequent chapters, the myriad of issues raised by the general concept of recidivism and how to incorporate the prospects of recidivism - both for evaluating the sentence and developing more informed sentencing policies, will be addressed. Focus will be given to the "choice" of measures of recidivism, as well as the choice of possible variables to predict or explain recidivism. In addition, what is meant by the "sentence" is discussed in terms of our ability to empirically measure the sentence and to ascertain

what effects, if any, sentences have on recidivism. Some implications are drawn for possible sentencing policy.

The tasks for the present chapter are more limited in scope. We begin with a review of the recent changes in sentencing practices and trends in other aspects of CJS decision making. This leads to a more general discussion of the various goals of sentencing and how, on the surface, these goals may appear to be incompatible. An alternative perspective which attempts to integrate the goals of sentencing is introduced and the place of recidivism within this framework is discussed. We close with a preview of the topics covered in the remaining chapters.

A BRIEF HISTORY OF SENTENCING AND RELATED PROCESSES

Diffused, Discretionary Decision Making.

The sentencing of a criminal offender is part of a more general sequence of events that constitute CJS intervention.[2] We will not focus here on arrest and prosecution components[3] of the chain of events that constitute intervention, although such processes are discussed in terms of their methodological implications for the study of recidivism in Appendix A. We are more interested in events that occur after sentencing, particularly who makes decisions as to the degree or nature of supervision and release. It is these

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[2] We refer to the sentence of a criminal court as an intervention rather than more narrowly as "punishment" since some aspects of the sentence (program participation, job training, supervision in the community) seem inappropriately described as "punishment." Moreover, "punishment" is often equated with intervention, as if that were the only purpose of intervention.

[3] This is not to say that the prosecutor's role is not important in influencing the judge's sentence. Prosecutor's decisions regarding the definition of the charges, plea bargaining and recommendation of sentence no doubt play an influential role in determining the judge's decision.

decisions that mold how the judge's sentence is actually administered to the individual.[4]

We begin by describing criminal sentencing in the U.S. (and New Jersey) prior to 1970 as important developments that have taken place since then are better understood within the historical context of what occurred in the decades prior to that time. Unfortunately, we know of no specific historical study of recent criminal sentencing per se to organize our discussions (although see Shane-Dubow, 1985), and thus we draw upon general work about sentencing and sentencing philosophies during the period of the 50's through the 70's.

This period has been characterized by many as one of "indeterminate sentencing" -- which itself suggests that there is an alternative type of sentencing to be called "determinate sentencing." Although there are dangers in using the term, we will occasionally do so for lack of a better one.[5] Rather than dwell on the "indeterminate" aspect to sentencing, we prefer to describe the sentencing process as a diffuse discretionary decision-making process. Organizationally, the system of intervention in the offender's life is one of multiple decision makers. That is, the form and nature of the intervention is only partially (yet importantly) determined by the court. Typically, the type of probation supervision (frequency and nature of contacts with the offender), restrictions on the degree of autonomy within probation,

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[4] This also raises the issue of the potential of a discrepancy between the judge's sentence and the treatment actually received by the individual. The implications of any discrepancy for the evaluation of sentence effectiveness are discussed more fully in Chapter Seven.

[5] We are reluctant to "characterize the period" in question as "indeterminate" because, as best we understand trends in sentencing, "determinate sentencing" would not be an appropriate general description for more recent approaches.

the specific institution sent to, the degree of supervision of the offender at the institution, and programs in which the offender participates, are usually determined by a decision maker or decision making body other than the judge. Actual intervention, or "treatment," consists of a series of decisions made by those who are limited in their discretion by the sentence imposed by the judge,[6] but nevertheless have considerable discretion in deciding the particular form and length of intervention.

The judge's sentencing decision is made in part as a result of formal, and often informal, recommendations from two (official) sources: the prosecuting attorney and the probation officer writing up the pre-sentence investigation. Although the degree of input from these two sources varies with the type of case before the judge, they generally are important sources for the judge's decision. The most important aspect of this decision is whether or not to incarcerate (versus some probation term). In New Jersey, judges often chose to follow the recommendations of prosecuting attorneys and probation officers, although the two may differ at times as to the nature of the CJS intervention to be imposed on the offender. Prosecuting attorneys and probation officers are both "officers of the court," and thus organizationally the same, but professionally quite different (Hagan, 1987).[7] Thus, as the judge begins to formulate a specific sentence for a case, input from within

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[6] How the sentence limits discretion depends on the particulars of the sanction. For example, offenders given a probationary sentence cannot be sent to prison without judicial approval.

[7] Probation officers generally do not have law backgrounds, but frequently do have college degrees or degrees in social work, counselling, etc. They also tend to differ in their perception of what the sentence goals are: prosecutors define the goals more (but not exclusively) in terms of retribution, while probation officers define sentencing in terms of a mixture of goals (Hagan, 1988).

his/her organization is forthcoming, but from somewhat competing perspectives.

At the opposite end of the decision making continuum are the decisions to release from prison and on the nature and degree of supervision over paroled offenders. In New Jersey, parole boards were, prior to 1979, (technically) free to grant parole to any offender sentenced to an adult state facility, but in practice tended to grant parole in a manner proportional to the "maximum time" sentenced, generally offering release after about one quarter of the sentence had been served. Upon release, the offender is subject to varying degrees of supervision by parole officers. Presumably, there is greater chance of revocation the more "intense" the level of supervision. Consequently, some offenders may be more subject to revocation due to decisions made by someone other than a judge, in this case the parole board making a supervision classification decision.[8] Thus, parole board decision making can be said to be influenced by the judge (especially for time served), but at the same time somewhat independent of the judge's sentence.

Institutional personnel, (representing the organization of the Department of Corrections in New Jersey), are also involved in decision making on the nature of the CJS intervention for offenders sentenced to specific institutions. Here, various personnel can make decisions as to the programs in which the offender participates, whether or not the offender serves any time in isolation for misbehavior in prison, whether or not the offender has earned his/her "good time" and receives time off for "good behavior". Reports from correctional personnel (counselors, program staff, guards) may

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[8] A revocation, however, is made by a judge. The probation or parole officer may decide to initiate the proceeding, however.

significantly determine the amount of time served by an offender, as well as the general nature of the time served.

Yet another source of decision making in CJS interventions is found for probation officers who supervise offenders sentenced to probation. Classification and revocation decision making is at least partially the responsibility of these officers. Although they seldom exercise their ability to have probationary sentences shortened, they are largely responsible for determining the degree of the supervision of offenders on probation.

While we have not exhausted a discussion of all the possible decision makers in determining the actual intervention received by convicted offenders, these are the major decision making bodies in a diffused process: many individuals make decision affecting the ultimate treatment received by the offender. In addition to intervention decisions being diffuse, they are hierarchical in the sense that the decision of the judge is temporally first and sets the parameters for other decision making. The judge's sentence is the "predominant" decision for the intervention. For example, it determines whether the offender serves time in jail or prison, and influences when release decisions are made, particularly for those offenders sentenced to prison. At times, the sentence of the judge can determine specific attributes of the sentence, including the institution sentenced to, or the general nature of the institution. Other times this is an "implicit" aspect of a sentence. Thus, for example, in New Jersey a sentence of an offender under the age of 26 to a "Youth Complex" incarceration "implies" one of several institutions that constitute a "Youth Complex" institution. The judge is explicitly determining that the offender should not be sent to other types of incarcerative institutions. A sentence to life imprisonment, for example, effectively meant

a sentence to Rahway or Trenton State Prison. Thus, although the specific nature of the intervention (including programs and time to release) were not strictly determined by the judge at sentencing, the general nature of the intervention is established at sentencing.

This diffuse, hierarchical decision-making process of CJS intervention after conviction is what has been referred to by friends and foes alike as "indeterminate" sentencing. But what perhaps best accounts for this label is that the decision is highly discretionary. That is, the judge, for example, typically has an array of sentencing alternatives to draw from for any specific offender: prison, probation, jail, fines, or combinations of these types are all possible for virtually any offender. This discretion can lead to inconsistency both across decision makers and for the same decision maker over time as to the sentence imposed on the same "presenting case." Parole boards have also had virtually unlimited discretion in making release decisions. Decisions may have been made which critics have called "subjective" or even capricious, despite the efforts of decision makers to be "professional" in their decisions or recommendations.

As such, beginning in the 1960s and extending to today, indeterminate sentencing has been subject to varying criticisms from several sources. Morris and Tonry's (1990) recent review notes several problems associated with indeterminate sentencing practices. First, critics of "rehabilitation" saw the indeterminate system as one with the primary goal of rehabilitating offenders. Offenders were only to be released when they were deemed rehabilitated, yet the majority of offenders eventually become recidivists (if followed for a long enough period of time), suggesting that little actual rehabilitation occurred. It was also charged that an indeterminate sentencing

process produced capricious and arbitrary variation in how offenders were treated (American Friends Service Committee, 1971). Blacks and poor defendants were receiving the harshest punishments under an "indeterminate" system, presumably because they were less likely to convince authorities that they were "rehabilitated" (and presumably because "rehabilitation" has middle-class connotations of respectability, family, and jobs). In short, discretion allows biases to be introduced.

Despite the appearance of fairness under indeterminate sentencing, there were few procedural safeguards for offenders after conviction, such that decisions could be made with real consequences for offenders who had little recourse as to appeals or challenges (Davis, 1969). Also criticized was what seemed to be enormous disparity in how "similar" offenders were treated by the decision makers of the CJS. Although it may be difficult to agree upon or to operationalize the definition of "similar," many examples of disparities in treatment for offenders convicted of the same type of offense could be identified. Finally, there was (and still is) no conclusive body of research to say that forms of intervention made any difference in the future criminal behavior of offenders (Martinson, 1974; Lipton et al., 1975; Sechrest et al., 1979). In the absence of known, effective treatments, the other goals, such as retribution, became the object of reform.

In addition to these points discussed by Morris and Tonry (1990), we highlight two other criticisms of the indeterminate system. First, diffuse decision making is criticized (it is perceived as "harder to control") than decisions made by one person, such as the judge. Second, even though many offenders recidivate (and even have a high probability of recidivism), they

are released from incarceration or from supervision.[9] Thus, decisions were (and are) not only diffuse in an indeterminate system, but there were (and continue to be) relatively high recidivism rates in a system where most offenders are released before their maximum term, and could not be truly said to have been "rehabilitated."[10]

In "response" to such criticisms, various sentencing reform efforts have been formulated. These include: presumptive sentencing guidelines, voluntary sentencing guidelines, statutory determinate sentencing, mandatory sentencing laws, the increased use of parole release risk instruments and, more recently, intermediate punishments. We briefly describe these efforts, as well as briefly discuss some of their shortcomings.

Presumptive Sentencing Guidelines

Perhaps the most successful reform of the discretionary, diffused decision-making process described above is that of presumptive sentencing guidelines. We confine our discussion here to Minnesota (although Washington could also be included) as it seems the most successful example of the development of an alternative form of sentencing intervention.[11] The Minnesota sentencing guidelines grew out of a philosophy of retribution ("just deserts") with some elements of other sentencing goals as well. The

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[9] To state that a high proportion of offenders recidivate is not the same as stating that many offenders have a high probability of failure. The former refers to offenders in the aggregate, while the latter refers to the ability to attach a high probability of failure to many offenders. The two are not the same.

[10] Note that we do not make this latter point to suggest that more offenders should be imprisoned.

[11] Note, however, that some recent literature suggests that the success may be more limited than previously thought, -- Frase, 1991; see also, Miethe and Moore, 1985; Moore and Miethe, 1986.

"centerpiece" of presumptive sentencing guidelines is a "grid" of two dimensions: presenting offense seriousness and a criminal history score. Figure 1.1 shows a recent example of the sentencing grid used in Minnesota. The vertical dimension consists of severity levels of the conviction offense. (Examples from each of the ten levels are presented here). The horizontal dimension consists of a criminal history score (essentially, the sum of the number of previous convictions). The cells above the heavy line are probationary sentences, while the cell values below the line are presumptive times to be served in prison, with "permissible" ranges below each of the presumptive times. These are the ranges within which a judge must sentence the offender unless he/she chooses to depart from these guidelines, in which case the departure must be "justified" as aggravating or mitigating. (It should be noted that judges are allowed to sentence offenders falling in the probationary categories to jail time of up to a year, as a condition of probation).

Relative to the indeterminate sentencing of offenders used prior to 1980 in Minnesota, the sentencing grid (as well as the one for Washington) has several advantages, according to its proponents. The first is that it reduces disparity. "Similar offenders," now defined as those with the same offense severity and same criminal history score, receive the same punishment within the range of permissible variation. Studies have shown, however, that judges' departure rates may be quite high. Frase estimates that, for recent years, around 33% of sentences are "dispositional departures." That is, offenders were sentenced to serve time (or not to) when the grid classification dictated

that they should not (or should) serve time, respectively.[12] An examination of these departures reveals that most of them are mitigated departures going against a prescribed incarceration. The offense or offense history of the offender was deemed "too harsh" (i.e., the offender's crime or criminal record was not well-represented by the grid categories) and a non-incarcerative sentence was imposed. Similarly, in terms of time sentenced, the durational departures (overall about a quarter of the cases in 1989 -- Frase, 1991) were for durations less than the prescribed ranges. If "departure rates" are computed retroactively for 1979, before Guidelines were in effect, the rates reported are about 40% for durational departures.

Thus, there has been some reduction in "disparity" defined relative to pre-guideline sentencing. It also seems to be the case, however, that charge reduction plea bargaining increased as a result of the guidelines (Tonry, 1987:39). Thus, offenders may have been charged with a less serious offense in response to the sentencing grid, resulting in less or no time sentenced or served in a state-run correctional institution (or in less time on probation). As such, the Sentencing Commission's intent to prescribe intervention for similar offenders has been circumvented to a considerable extent. Still, the overall effect has been to reduce the variation in sentencing for "similar" offenders.

In terms of the diffused, hierarchical, so called "indeterminate" system discussed above, the grid system in Minnesota has left in place some of the decision making authority of judges, probation officers (whose authority is

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[12] Note that Frase defines a departure using the percent of defendants eligible for each type of sentence as the base, while the Minnesota Sentencing Guidelines Commission has reported lower dispositional departure rates, using all sentenced offenders as the basis. This results in lower rates of departures than those reported by Frase (1991).

arguably enhanced in that they calculate the offense history score), and correctional personnel.[13] What has changed in Minnesota is the consistency with which the "in" versus "out" decision is made and the amount of time the offender is expected to serve if incarcerated in a state-run facility. It should be noted, however, that discretion is still exercised as to the nature of supervision, and, to some extent, release time, as offenders are still subject to early release due to earned "good time." Although parole boards do not decide release time, alternative organizations (e.g., committees to decide upon the degree of supervision while in the community) perform the function of monitoring released offenders and making decisions as to degree of supervision.

Voluntary Sentencing Guidelines

Voluntary sentencing guidelines have been in place in many states, but it is not known how successful these programs are in changing sentencing practices (Rich et al., 1982; Carrow et al., 1985; Sparks, 1983). The evidence suggests that departures from recommended sentences are more frequent than is the case where such departures must be justified formally. It seems safe to conclude that voluntary guidelines have not been effective in changing systematically the way judges have sentenced offenders, nor in the way that the diffused system of CJS intervention operates. In effect, voluntary sentencing guidelines produce many of the same results as the indeterminate policies of the 60s and 70s.

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[13] Moreover, if plea bargaining has increased as a consequence of guidelines, prosecutors have become an even more important part of the decision making process.

Statutory Determinate Sentencing

The most common type of sentencing reform has been the creation of statutorily mandated sentences for certain types of criminal activities. Numerous states have passed laws for mandatory punishment of certain types of offenders, and have essentially eliminated or reduced the need for a parole board decision to release offenders in several states. California, Illinois, Ohio, Vermont, Maine, Connecticut, and Florida are states that underwent radical decreases in the use of parole during the 1980s (Rhine et al., 1991). Much of the decrease in the use of parole release can be attributed to statutory determinate sentencing laws: judges are required to sentence offenders to a statutory minimum period of time. Although "good time" can still be earned in most cases, the judge is often limited in his/her ability to suspend sentence for offenders convicted of specific crimes that "require" set incarcerative penalties.

The impact of legislated determinate sentencing is difficult to evaluate. The existing evaluations of Maine and California have been critically reviewed by Cohen and Tonry (1983). North Carolina's determinate sentencing laws have been evaluated in Clarke (1987) and Clarke et al., (1983). From these reviews it seems that determinate sentencing does result in more certainty of imprisonment for set periods of time for some types of offenders. However, critics have challenged the basis for the selection of certain types of offenders over others (von Hirsch, 1987), and often consider the choice of offenders for prolonged punishments as somewhat arbitrary. Also, it is dubious that the harsher penalties imposed for some types of offenders has the general deterrent effect that motivated the determinate sentence law. Moreover, even in states with determinate sentencing and where

parole has been formally abolished, post-release supervision has remained in force, or has been "reinvented."[14]

Mandatory Sentencing Laws

As distinguished from legislated determinate sentencing, mandatory sentencing laws target select types of behaviors that may be involved in many types of crimes: for example, possession of firearms, or drugs. Various reviews of these laws across states such as New York, Massachusetts, Michigan, Florida, and Pennsylvania are available (Pierce and Bowers, 1981; Heumann and Loftin, 1979; Carlson, 1982; Loftin and McDowall, 1984; Pennsylvania Commission on Crime and Delinquency, 1986). Where judges have been behind the "spirit" of such laws, there have been increases in incarcerative punishments. Critics have argued, however, that prosecutors still have discretion as to the charging of the offender for these offenses, and judges or parole boards may "compensate" for the additional time due to these mandatory add-on times.

Increased Use of Risk Assessment Instruments

Perhaps the area of change in criminal justice intervention that most directly incorporates the potential for offender recidivism is the structuring of release decisions by the use of risk assessment instruments. At least 13 states have used a formal classification of incarcerated offenders for release to parole: Alaska, Florida, Georgia, Maryland, Missouri, Nevada, New York, Oregon, Utah, Ohio, Pennsylvania, South Carolina, and Texas (Rhine et al., 1991). Some of these states have explicitly developed grid systems, while others simply have cumulative risk scores. Parole release grids are quite unlike that of the Minnesota sentencing grid in that many more factors known

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[14] Colorado and Connecticut are two states that have at one time abolished parole only to have reestablished it in the past few years.

to predict recidivism are typically part of the parole release grid. For example, educational attainment, employment prospects, and family support are considered in assessing risk for recidivism while on parole. Most states have parole release guidelines for their parole boards to follow and predictors of recidivism are used in various ways in all of them (See Petersilia and Turner, 1987).

Lombardi (1981) and Mueller and Sparks (1982) have evaluated Florida and Oregon's parole release guidelines, respectively, and it seems that such instruments have been quite influential in determining release dates and structuring the release decision. That is, when used at least for parole release considerations, the correlates of recidivism can be useful for CJS decision making.

Intermediate Punishments

More recent reform efforts have been in the direction of establishing punishments as an alternative to traditional probation or imprisonment. These intermediate punishments include fines, intensive supervision on parole or probation, and house confinement. Morris and Tonry (1990) have called for reform efforts to move in this direction because too many offenders are in prison who should not be -- possible because of mandated sentences or judicial discretion -- and too many offenders are not being punished adequately under traditional probation supervision. Evaluations of some of these programs has suggested that they are effective (Pearson, 1987; 1991). It is too early to determine if the suggestions of Morris and Tonry will have widespread impact and lead to increased use of these alternative forms of intervention.

Summary

Several features of the historical development of these diverse forms of alternatives to "indeterminate" sentencing are of interest, given our goal to conceptually link sentencing to recidivism. First, many studies of recidivism have shown that the best predictor of subsequent criminal behavior is the number of prior convictions (or arrests). This is an important dimension of the Minnesota grid system and part of virtually every parole release grid or guideline system that we know of. Thus, although it may be implicit in the case of sentencing guidelines and explicit in the case of parole release guidelines, factors known to predict recidivism have, to some extent, a history of being incorporated into CJS decision making.

Limiting the discretion of judges through legislated determinate sentencing and mandatory sentencing requirements can also result in increases in the use of incarcerative punishments for some types of offenders, but possibly at a cost to the principles of proportionality and fairness in punishment. Also, there is at best unclear evidence that would-be offenders are deterred by relatively harsh determinate sentencing laws.

It is interesting to us that, whether or not the release decision is made using a grid system imposed at sentencing or by the parole board at release, discretion in either case is limited in decision making. Thus, whether two decisions are involved (sentence and parole) or only one decision (sentence), the result may be the same for limiting discretion in the system. Interestingly, many more states have opted to use grids after the offender is sentenced than have opted to use grids at sentencing.

Furthermore, such instruments explicitly invoke recidivism considerations in their formulation. Recidivism considerations are implicitly

or explicitly part of the decision making process for determining how offenders are treated not only where grid or risk assessment instruments are used (whether grids are used at sentencing or after). Yet this is also the case where grid systems are not used, as under a traditional "indeterminate" system where judges may either implicitly or explicitly sentence an offender on the basis of potential future criminal behavior. Although recidivism assessment is an integral part of the diffuse, hierarchical system of "indeterminate" sentencing described above, the evaluation of the likelihood of recidivism may be best characterized as a clinical, or professional "judgment-call", rather than one based on actuarial, or statistical models of recidivism (Monahan, 1981). Thus, what has replaced "indeterminate" sentencing in most states is a system that is best characterized as a mix of different sentencing processes and philosophies, rather than a monolithic "determinate" process.

Nationwide, during the height of indeterminate sentencing (as reported in the President's Commission on Law Enforcement and the Administration of Justice -- Task Force Report: Corrections, 1967), 60 percent of offenders were released on parole prior to the expiration of their sentence.[15] In 1987 after the implementation of sentencing guidelines, statutory determinate sentencing, and mandatory sentencing, 43 percent of the offenders nation-wide were released on parole.[16] Thus, the changes that have occurred, while

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[15] Not counting offenders released from prison to probation (so called split-sentences) increases the percentage somewhat. For example, nationwide data for 1974 show that 70 percent of non-split-sentence offenders were released on parole.

[16] The 1987 figure is 53 percent if California is dropped from consideration. Data from 1974 suggest that 70 percent of offenders nationwide were released on parole (not counting probation releases or so-called split sentences to prison and probation), compared to 43 percent in 1987.

dramatic in some states (such as California), nationwide represent a shift rather than a complete change from one form to another. What we see then is the reduction in parole releases as a result of some of the reform efforts, but not a wholesale shift. Moreover, these reductions in parole releases are consistent with a consequent increase in supervision by probation officers that would result if determinate incarceration sanctions were avoided by plea bargaining or charge reduction (or judges opt for "split sentences" to guarantee some supervision in the community).

It is also relevant to note that in general these reform efforts were not directed at the use of recidivism per se in CJS decision making (the exception being strict retributionist efforts). The aims of reform were many. In some instances, the purpose was to lessen discretion (judicial and parole board discretion), in some instances it was to ensure that more offenders (or certain types of offenders) serve time in prison (more punishment for some), in some instances it was to establish a more "just" sentencing system (proportionality in intervention). Of all the reforms, use of risk assessment instruments is most directly relevant to recidivism considerations: their increasingly wide-spread use suggests that recidivism is not only an important part of determining the degree of intervention, but one that seems to be preferred to that of sentencing guideline grids.

The reform efforts described above are also point to the diffuse nature of decision making in the CJS. The overriding theme of most of the reforms is to limit an individual decision maker's discretion in making a sentence or release decision. The underlying principle here seems to be the belief that having a lot of discretion or "left with a wide range of discretion," decision makers will be inconsistent in their decision making (both across decision

makers and across decisions of the same decision maker). Relative to the general goal of consistency, the sentencing guideline and the parole risk assessment approaches seem to have been somewhat successful at limiting the discretion of judges and parole boards, while mandatory, and legislated determinate sentencing approaches may have resulted more often in circumvention of the restrictions and a shifting of discretion to earlier points in the CJS (essentially to prosecutors and the types of plea bargains that are arranged), although similar shifting may also occur to a limited extent in sentencing guidelines.[17]

In general, the discretion of certain decision makers seem to have been only modestly affected by all these reform efforts. The case of Minnesota, where the most success has been achieved, has already been discussed above in terms of departure rates. Parole release guidelines also seem to have enjoyed success at limiting parole board's discretion. At the same time, however, it should be mentioned that correctional decision regarding the choice of institution and the type and degree of program participation, as well as, the calculation of good-time credits remain prerogatives in virtually all states, albeit limited by statute in many states. Similarly, decisions as to the degree of supervision on probation remain largely in the hands of probation officers, as does their autonomy in referring a client to the revocation process. It is only for decisions concerning probation and parole revocations that judges in all states have maintained their discretion.

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[17] Circumvention of these forms of determinate sentencing does not seem as likely to occur with parole's use of risk assessment instruments, however.

THE GOALS OF CJS INTERVENTION

The historical and descriptive account of post-conviction CJS intervention of the previous section leads to some generalizations about the nature of the changes that have occurred in the criminal justice system over the past few decades. Such changes have occurred, however, within a broader socio-political context of what such interventions should have as their goal(s). The various goals of sentencing, and the emphasis received by each, are thus important for understanding recent changes in sentencing and parole release practices. We now turn to a discussion of these goals with two additional purposes in mind: first, to show that no single purpose is agreed upon for such interventions, and, second, to show how recidivism is relevant to each of the goals.

A specific decision to sentence an offender, or to release an inmate to parole supervision, is made within the context of achieving multiple, sometimes conflicting, goals: retribution (or just deserts), deterrence, rehabilitation, and incapacitation (Clear and Cole, 1986; Burke, 1988). Other goals are often mentioned: maintaining tolerable population levels in prisons, maintaining order in prisons (Burke, 1988:22). Each of these goals is discussed below.

Retribution

In the past twenty years the most discussed goal of punishment has been retribution, or just deserts (von Hirsch, 1976). According to this goal, a sentence is appropriate ("just") if the offender is punished commensurate with the seriousness of the offense, as well as with his/her culpability for that offense. Factors of risk or likelihood of subsequent crimes are irrelevant to

a strict just deserts approach, though some indicators of risk (e.g., prior record) may enter into the determination of "culpability."

Depending on the state, attempts to achieve the goal of retribution have been most evident in the form of sentencing guideline grids, as discussed above in the case of Minnesota. The seriousness dimension of the grid reflects the retributionist argument that the severity of punishment should be proportional to the crime. To a lesser extent, retributionist interests have been reflected in the passage of determinate sentencing laws, and the adoption of parole release guidelines, but because these attempts have not been as careful to consider proportionality in sentencing, it is sentencing guidelines that seem to best manifest the goals of retribution.

The second dimension of grid systems such as those of Minnesota and Washington is that of prior convictions. Arguably there is a retributionist component here as well: those who repeatedly commit crime warrant harsher "punishment" for their crimes. For example, those with six or more prior convictions receive the maximum degree of intervention across the conviction offense severity levels of Figure 1.1.

Deterrence

Perhaps the most commonly cited reason for punishing offenders is that it will deter others from committing the same types of infractions that resulted in the incarceration of the specific offender. This is often defined as the "social order" function of punishment. The goal is to ensure society that an excessive number of crimes, especially those of a serious nature, are

not perpetrated. Because punishment is seen as generating a fear that deters other would-be offenders from crime, it is called general deterrence.[18]

A related, but narrower and conceptually distinct, type of deterrence is specific deterrence. Here the focus is on the impact of the punishment on the offender's subsequent behavior. This is often measured by his/her recidivism. It is assumed that the punishment experienced by the offender will deter him/her from continued criminal activity. Although there is evidence for specific deterrence effects of arrest (Sherman and Berk, 1984; Smith and Gatlin, 1989) and incarceration of juveniles (e.g., Murray and Cox, 1979), there is surprisingly little known about this goal in relation to various types of sentences for adults or parole release decisions (although, see Schmidt and Witte, 1979). For example, of the 154 studies used in a recent meta-analysis of correctional treatment studies published between 1975 and 1984 (Andrews et al., 1990a; 1990b), a mere 23 involved samples of adult offenders and many of those 23 studies did not investigate sentences of incarceration. In addition, there are methodological complexities involved in ascertaining specific deterrence effects (Maltz, 1984) which make the detection of such effects difficult.[19] Consequently, it is probably still widely believed that "nothing works" (Martinson, 1974) to deter offenders from further criminal activities or that "something works" but only to a limited extent (Lab and Whitehead, 1990).

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[18] Demonstrating general deterrence effects has been notoriously difficult, in part because of complex methodological issues, including inherent ambiguities in interpreting aggregate-level data on crime, arrests, and various forms of punishment (Blumstein et al., 1978).

[19] The methodological concerns surrounding the detection of specific deterrence are summarized in Appendix A.

Rehabilitation

Since the mid-1970s the philosophy of rehabilitation has seemingly suffered a series of setbacks. Many of the reforms based on retributionist goals have taken aim at the failures of rehabilitation as reason for establishing determinate sentencing laws and limiting discretionary decision making, particularly by judges and parole boards -- as discussed above. There is a widespread belief that "nothing works" (Martinson, 1974; Lipton et al., 1975; Sechrest et al., 1979).

Rehabilitation is usually distinguished from specific deterrence by the mechanisms presumed to operate. Deterrent effects are caused by fear of further punishment while true rehabilitation is the result of a change in personality, value orientation, life style, abilities, self-discipline, and so forth. These differences may be hard to distinguish if the only criterion of success or failure is recidivism: empirically the effect of rehabilitation and specific deterrence is the same.[20] Without detailed, longitudinal data measuring individuals' personalities and values, rehabilitative effects must only be postulated when low levels of recidivism are found.

There seems to be considerable debate over the issue of whether "nothing works," "nothing has been scientifically demonstrated," or "something works for some offenders." Some (e.g., Andrews et al., 1990a) contend that it is possible to deliver appropriate correctional services if offender needs are properly assessed. Yet there are methodological problems that make the detection of program effects difficult. Sechrest et al., (1979), for example,

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[20] Yet, in the absence of further information, exposure to certain types of treatment may be indicative of which of the two processes is at work. In that exposure to rehabilitative programs and exposure to punitive programs are distinct, it may be possible to determine whether deterrence or rehabilitation has any effect on recidivism.

concluded that research designs and implementations were such that it is not possible to draw strong conclusions that anything works. They cite several problems with the studies reviewed: discrepancies between program goals and implementation, variations in how recidivism is operationalized across studies, uncertainty about the nature of the treatment given, lack of adequate control groups or statistical controls, and small samples, among others. Thus, it may be fairer to say that it has not been clearly demonstrated that "something works."

Not all researchers agree with either the conclusions of Lipton et al., (1975) or Sechrest et al. (1979). A number of scholars cite evidence for some programs being effective (Glaser, 1974; Palmer, 1983; Martinson, 1979; Murray and Cox, 1979; Gendreau and Ross, 1979; Greenwood and Zimring, 1985), though they note that many of these studies report successful interventions for juvenile offenders. Others claim that, although nothing may work, humanitarian reasons dictate that correctional treatment programs be aimed at rehabilitation (Cullen and Gilbert, 1982).

Incapacitation

The containment or incapacitation of select offenders has often been discussed as a goal of incarceration, particularly since the publication of Greenwood's Selective Incapacitation in 1982. If the small proportion of offenders who commit crimes at a high rate (high "lambda" offenders) can be identified, society is saved many victimizations while the individuals are off the streets. In principle it is possible to identify such offenders, though the exact mechanisms, as well as the appropriate variables to do so, are unclear.

Considerable debate exists over the implementation of specific selective incapacitation strategies (Cohen, 1986). Doubts have been raised about the impact that a selective incapacitative strategy could have without building additional prisons (Vischer, 1986). Ethical concerns have been raised about what criteria should be used to justify incarcerating some offenders longer than others. Given that so few offenders are caught and imprisoned, the impact of incapacitative strategies on the overall crime rate may be minimal. Furthermore, there are difficulties in ascertaining the effect of incapacitation relative to any specific deterrent effects that might be observed if offenders are released. That is, if the individual is either deterred or rehabilitated as a result of the incarceration, no effect of "selective" incapacitation need be postulated. Moreover, the influence of aging on the commission of crime could account for reduced criminal behavior, negating the need to incapacitate selected offenders. Despite these criticisms, incapacitative strategies are routinely applied with the denial of parole based on an "unacceptable risk" of recidivism.

Incapacitation and specific deterrence goals may be directly conflicting in terms of policy decisions in particular cases. The former assumes offenders likely to commit crimes should not be released and that, overall, specific deterrent or rehabilitative effects are minimal. In fact, it is presumed that high lambda offenders (high rate offenders) cannot be deterred from committing further crimes by anything other than bars. To the extent that high rate offenders are, however, deterred, there is less of an incapacitative effect. The decision to keep an offender in prison for incapacitative reasons thus negates the possibility of specific deterrence effects over the same time period.

Note that at the individual level, both incapacitative and deterrent strategies force a decision as to which offenders should be released to the community. Extended imprisonment may be appropriate for offenders who are not likely to be deterred by normal imprisonment, while others may be deterred (or rehabilitated), and thus may be released. Two issues thus become paramount: What level of risk of future criminal behavior is acceptable, and how should high risk offenders be identified? Specific deterrence and incapacitation may well be conceptualized as two sides of the same coin.

Efficiency

In recent years the number of inmates imprisoned in most states is well beyond official capacity and this constraint of limited resources (jail and prison overcrowding) has been cited as a reason for the sentencing and incarceration patterns that have been observed. The system's response to overcrowding has prompted some to add "efficiency" as the goal of sentencing and release decisions (Gottfredson and Gottfredson, 1988:154). In a system strained by overcrowding, the use of incarcerative sentences, or the length of the sentences, may be reduced. Offenders who meet criteria of having served a proportion of their sentence and who represent relatively low risk may be released early. Also, offenders thought to be low risk and who have not committed too serious a crime may not receive an incarcerative sentence. Thus, the context of overcrowding may determine the nature of punishment.

Incarcerative sentences may become less common in a system with few bed spaces available. If so, it becomes increasingly important to decide which offenders require less punishment, and which more: determining "acceptable risk" is an even more central task in a system where levels of incarceration are reduced. Yet the pressure to put offenders back on the street may reduce

the social and statistical utility of the prediction. False negatives are more likely to occur under a relatively strained than unconstrained system and more dangerous offenders may be released (Petersilia et al., 1985).

As we see the efficiency goal, it is one that essentially incorporates a lower threshold for release of offenders, whose actual release is determined by other criteria, such as the likelihood of recidivism or retributionist considerations. Thus, logically, it may not be distinguishable from other goals, but points to the important consideration that availability of prison cells helps define the nature of decision making in the CJS. In short, lower thresholds for release develop, pointing again to the importance of factors such as recidivism, in that the released offenders should be the ones least likely to inflict serious harm in the community.

THE (IN)COMPATIBILITY OF GOALS

It is well known that the goals of the CJS may be in conflict (Gottfredson and Gottfredson, 1988:145). For example, an offender may be judged to have a low probability of recidivism, yet retributionist considerations prohibit release. Conversely, retributionist goals indicate the release of an offender, but predictive scales suggest there is a high likelihood of further, multiple crimes if released. Given the multiple goals of incarceration, the sentence or parole decision can be a difficult one -- and also hard to evaluate (Gottfredson and Gottfredson, 1988:234; Glaser and O'Leary, 1966).

Discussions over a reduced number of goals are almost always raised as <u>logical</u> arguments that are based on what <u>might</u> be differences between the goals, and not <u>empirical</u> arguments over the <u>actual</u> differences in

operationalizing the various philosophies. At the empirical level, it is often difficult to differentiate the goals of sentencing. If the dependent variable is solely a measure of recidivism, lower levels of criminal behavior are consistent with many of the diverse goals of sentencing.

Even if, however, distinctions blur between the various goals of sentencing at the empirical level, the question arises as to whether a single underlying philosophy of sentencing is either desirable or workable. We assume, following the lead of many others, that a single-philosophy system, such as a pure retributionist sentencing system is unworkable for several reasons. Following Morris and Tonry (1990), we argue that there is no inherent connection between a specific crime and a particular punishment. In their terms, punishments are interchangeable. The implication of this observation is that, although it is possible to rank order offenses according to severity, it is not possible to link them logically to a specific punishment. That is, a qualitative and quantitative leap occurs in deciding a punishment for an offender. The qualitative leap consists in choosing the type of punishment chosen for the crime, and the quantitative leap is the choice of the amount of whatever type of punishment is chosen. "Reasonable people" will not agree on the type or amount of punishment due an offender, based solely on the severity of the presenting offense (much less on the additional consideration of past convictions).

There are also difficulties in arriving at the "comparative culpability" of offenses (Morris and Tonry, 1990:87). Statutory labels tell us too little about the severity of the offense to say conclusively, for example, that all robberies are more serious crimes than all burglaries. Arguably, exceptions need be allowed, as is the case in states where sentencing guidelines have

been attempted, and as has always been the case in states with indeterminate sentencing.

In addition, a pure retributionist system is unlikely to succeed because too few decision makers in the criminal system subscribe to it. Police, prosecutors, judges, prison officials, and parole boards subscribe to a multiple goal approach -- not a purely retributionist one (Morris and Tonry, 1990). A related reason for not pursuing a purely retributionist approach is that it narrowly conceives of intervention as punishment: the state can apparently do nothing beneficial for the offender, such as provide job training, an education, counseling, etc. Yet, the criminal justice system routinely attempts to do these non-punitive actions (although the evidence suggests inadequately).

Whereas there are numerous reasons for believing that a purely retributionist sentencing system would not work, there are also reasons for rejecting such "pure" systems on ethical grounds or on principles of fairness. For example, Glaser has argued that punishing an offender solely for the presenting offense is like punishing based on a lottery. The reasoning is as follows. Criminal offending is quite varied: many offenders commit crimes of virtually all types. If only one in twenty or thirty such crimes results in arrest, and the nature of punishment is linked solely to that crime, than the degree of punishment is somewhat arbitrary. Alternatively, the property value stolen or damaged is a good correlate of severity in most studies of public attitudes toward crime (see Sellin and Wolfgang, 1964; Rossi et al., 1974), yet for many offenses the property value stolen is a serendipitous event: are offenders committing burglaries in a rich neighborhood rather than a poor one committing more serious crimes? Also, in that there is difficulty in

agreement on the severity of a crime, criminal codes that prescribe various punishments may (inadvertently) introduce more punitive treatment for some types of offenders, such as blacks or other minorities. Thus, the Minnesota sentencing grid (one widely regarded manifestation of retributionist principles) has resulted in more harsh punishment of blacks than they experienced before the sentencing grid was adopted: in part because violent crimes were targeted for more harsh punishment, and property crimes targeted for less harsh punishment.[21]

Perhaps the most compelling reason for not adopting a pure retributionist system of sentencing, is that it would be economically unfeasible. If all burglars were to get the same "punishment," and presumably that punishment in the current context would be imprisonment, the already overcrowded prison system would be swamped. Thus, for a retributionist system to work, compromises must be struck so as to include alternative considerations, such as number of prior convictions. To the best of our knowledge, there has been no complete justification of using prior record on retributionist grounds.[22]

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[21] Note that we are not arguing that African-Americans' perceive robbery to be any less serious a crime than to whites. (Blacks are more likely to be victimized by robbery than whites, all else being equal.) Nor are we arguing that robbery in general is a less serious crime than some property crimes, such as breaking and entry. A purely retributionist system of sentencing requires judgements of the form that all categories of offenses minimize the variation within categories. We maintain that imposing a punishment hierarchy on such categories may result in injustices.

[22] In fact, we would argue that an argument that prior convictions should be incorporated into the sentence on retributionist grounds borders on sophistry. How can punishment be a function only of the seriousness of the presenting conviction offense and of prior convictions? Even if one buys the argument that offender culpability is indexed by the number of prior convictions, presumably weighted by their seriousness, how much weight should be given those convictions versus the seriousness of the presenting offense? Note that we are not saying that the CJS should ignore these factors, but we

Conversely, adopting a sentencing philosophy based upon only one of the other goals would be similarly inadequate. Deciding to incarcerate an individual simply on the basis of available bed space would be viewed as "unfair" as the punishment would be contingent on chance factors. Allowing only considerations of incapacitation to enter into the decision making would raise the specter of bias or unequal treatment due to the inability to sufficiently identify high rate offenders. Policies based only on a rehabilitation model ignore the vast literature that suggests high rates of recidivism. No matter what the stated goal of sentencing and punishment, a single-goal model will be subject to the various criticisms that have historically led to changes in sentencing and supervision practices.

By arguing that a single philosophy system (such as a retributionist one) is inadequate, the question arises as to the specific manner in which alternatives could occur. Clearly, the direction of change in CJS intervention is likely to involve crime control considerations, as well as retributionist ones. The purpose of the research here is to review and formulate the various criteria involved in redirecting intervention or punishment processes. To the degree that crime control goals enter into decision making, it seems to us that several issues surrounding the definition and modeling of recidivism processes must be addressed, including a the different ways recidivism has been conceptualized in the past, the possible dimensions of recidivism, its measurement, the mathematical models of recidivism (variously conceptualized and measured), selection of independent variables to model recidivism, implications of choosing among independent

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are merely pointing out what we believe to be inconsistencies in the retributionist argument.

variables, and how successful model of recidivism are in predicting individual recidivism. As such, we refrain from making specific recommendations on implementing CJS reforms, but assess some specific issues in recidivism definition, modeling, and prediction, and draw out some implications for possible directions of sentencing policy and reform.

In summary, we think there are ample reasons for rejecting a single-goal sentencing system in general, and a purely retributionist system in particular. This approach is in general harmony with that of others, such as Morris and Tonry (1990). At the same time, we recognize and advocate that some aspects of retribution be maintained in sentencing. We discuss below avenues for integrating retributionist approaches with other goals in the next section.

INTEGRATING THE GOALS OF SENTENCING

Although empirically the goals of sentencing may not be at odds, there are no doubt times when they are. Here, an idea developed by Michael Smith seems important: the goals <u>at</u> sentencing. That is, in the case of an individual offender, if one philosophy suggests one sentence and another philosophy an alternative one, a choice needs to be made at the time of sentencing as to which goal should be sought. Presumably the severity of the crime could dictate the choice of (or degree of weight given to) a goal. For more serious crimes, weight would be given to retributionist goals (most would agree that murderers deserve longer sentences than other offenders), while for less serious crimes weight would be given to crime control goals (e.g., probation officers may help the "small time" burglar get a job and stay in the community). Alternatively, the choice of a goal at sentencing would be

dependent on other aspects to the crime or to the offender's previous criminal history. (See the discussion of Smith's argument in Morris and Tonry, 1990:90). Seemingly disparate goals may be combined, as in the case of sentencing an offender to a particularly harsh intensive supervision sentence with a stiff fine (retribution and crime control goals combined).

The above considerations lead us to a general formulation of sentencing and the directions for change in sentencing. We argue that it is useful to conceptualize four dimensions of the intervention of the criminal justice system after an offender has been convicted: crime severity, the general goal at sentencing (retribution versus crime control), organizational diffusion of decision making, and discretion. Figure 1.2 depicts these four elements. Each will be discussed in turn.

Crime Severity. The most important consideration for assessing the nature of intervention is the severity of the presenting case. As we depict the relation of crime severity to the other three elements in Figure 1.2, we suggest that the degree of weight to be given to retribution is a function of the severity of the crime. If an individual has been convicted of particularly serious crimes, it seems that retributionist goals should be given greater weight at sentencing than other goals. Whether this be conceptualized in absolute terms (e.g., all first degree murders receive the same sentence) or relativistic ones (e.g., first degree murders with prior convictions get longer sentences), is not our concern here. Nor is it an "either or" decision: it can be conceptualized as a matter of giving weight to one type of goal over another. However it is conceptualized, the general idea is that one type of goal takes precedence over another depending on the severity of the presenting case.

Goals. The observation that some of the distinct goals of sentencing are abstractly the same is not new. Some have argued, for example, that there are essentially only two general classes of goals: crime control (or utility) and retribution. Almost all of the various goals discussed above can be classified as having individual-level crime control as a component. This is particularly true for specific deterrence, rehabilitation, efficiency, and incapacitation.[23] What is common to these four is that recidivism considerations are used to minimize the degree of public harm that will occur as a result of a specific sentencing decision. Thus, meeting the goals of specific deterrence, rehabilitation, incapacitation, and efficiency depends on an assessment of the likelihood and extent of an individual's recidivism, or what is an acceptable risk.[24] Thus, in Figure 1.2 we have grouped all of these crime control goals into one broad category (later we will discuss separating them).

We are guided in our thinking about the nature of sentencing by the reality and practice of crime control goals in the CJS. We argue that crime control goals are central to the operating of the CJS, and that enhancing the understanding of recidivism can lead to more rational decision making in the

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[23] Although public harm can occur if others are not deterred by the punishment of a given offender, this is difficult to demonstrate empirically -- see Blumstein et al., 1978 -- so more often attention has been focussed on the future criminal activities of the offender sentenced.

[24] Much of the concern with efficiency goals also centers on acceptable risk: how much risk is tolerated by the criminal justice system may be affected by overcrowding. In situations where prison space is available, lower levels of risk may not be tolerable and offenders would be more likely to be incarcerated. When prison space is not available, offenders who are at higher levels of risk for recidivism are more likely to be released. Thus, overcrowding can be conceptualized as having a contextual effect on the sentencing of individuals independent of the recidivism criterion.

CJS (Gottfredson and Gottfredson, 1988). How this can be done, or even how it can be conceptualized is our general concern in this work.[25]

For simplicity sake, we have conceptualized the many possible goals of sentencing as two goals: retribution and crime control goals. Specific deterrence, rehabilitation, and incapacitation are argued to be the three more specific goals most relevant to crime control considerations, but we will not differentiate them just yet (see discussion below).[26] As weight is shifted from one to the other, the nature of the intervention may change to reflect these alternative goals. Fundamental to the crime control goal is an assessment of the chances of recidivism for an individual offender. In Figure 1.2, offenders are conceptualized as having a low or high recidivism probability, but in practice there may be both gradations of recidivism

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[25] The significance of crime control in the day-to-day operation of the CJS can be seen in many ways. Judges and other high level CJS professionals give high priority to rehabilitation and specific deterrence as their stated reasons for passing sentences (Berk and Rossi, 1977; Gottfredson and Stecher, 1979). For example, only 17% of judges in New Jersey surveyed by Don Gottfredson in the mid-1970s (about the time the data for the present study were collected) mentioned retribution as the "principal purpose" of sentencing. Of all the possible goals, rehabilitation was chosen most often (36%). Adding specific deterrence as a goal (9%), and incapacitation (4%) increases the concern over the future criminality of the offender to 49%. Other goals frequently mentioned include "other purposes, including general deterrence" (34%) (Gottfredson and Stecher, 1979). Thus, in the absence of sentencing grid systems, judges profess to sentence giving more weight to crime control than to retributionist goals.

[26] The relevance of general deterrence independent of other goals is difficult to justify, in our opinion. In individual cases, retributionist considerations (where the severity of the punishment is proportional to the seriousness of the crime) are adequate to meet the demands of general deterrence. That is, beyond the severity of the punishment attributable to the severity of the crime, it is hard to justify further punishment on the grounds that others will be deterred. In part we argue this on the belief that such general deterrent effects have not been demonstrated and seem quite difficult to demonstrate scientifically.

probability and different types of recidivism (e.g, a subsequent arrest, or a subsequent violent crime).

Diffusion and Discretion. The two other dimensions of Figure 1.2 refer to the extent to which the decision is diffused across multiple decision makers or made by a single decision maker (judge or jury), and the extent to which the decision is discretionary on the part of the decision maker(s). The latter refers to the use of grid or other instruments to limit or determine the decision. The former refers to the number of decisions that constitute the definition of the treatment received by the offender. Thus, in cell C1, where recidivism or crime control is the primary goal, the offender may be sentenced by a judge, for example, who is guided by a risk assessment instrument or grid. Whatever else the CJS does to the offender must be in accordance with the sentence. In C2 the decision is also a singular event, but one that is not made on the basis of such instruments, rather on the basis of the subjective assessment of the judge, guided perhaps by the subjective assessment of the probation officer preparing a recommendation to the judge. (Ultimately, it is hard to imagine a singular decision made by only one actor.) Yet one decision can be made for the offender: the sentence. In cell C3 the discretion exercised is limited (e.g., by decision-making instruments), but multiple decisions constitute the nature of the intervention. Thus, the judge with the help of the probation officer and the prosecuting attorney may decide on a sentence using discretion limiting means, but a parole board will make the release decision using their own explicit guidelines. In cell C4, the decision making process is diffuse and discretionary according to the "subjective" judgment of those involved.

In the right half of Figure 1.2, the decision on how to treat the offender is determined largely by retributionist goals. In cell R1 and R2 retributionist criteria are used guided by formal assessment instruments, or not, respectively. Although those who advocate a pure retributionist approach also seem to advocate a single decision, we allow for the possibility of a diffuse, retributionist decision making process that is either singular or diffuse. This is allowed (contrary to the wishes of retributionist advocates) because some element of crime control may be part of even these retributionist dominated decisions. Thus, to the extent that some crime control considerations enter into the predominantly retributionist sentencing process, that input can be in a diffuse form (either discretionary or not).[27]

As depicted in Figure 1.2, decision making may involve varying degrees of crime control or retribution, or varying degrees of discretion and diffusion. The choice of crime control or retributionist goals (or giving more weight to one than another) can be made based on the nature of the presenting case (severity of the presenting offense, past criminal history of the offender). Once that decision is made in individual cases, presumably a "set response" on the part of the CJS would be activated along the dimensions outlined. A response by the CJS would presuppose that the system has defined for itself the degree of discretion allowed the decision maker(s), and how many decisions of what type will occur. A decision to follow a retributionist oriented decision may suggest a less discretionary and less diffuse decision making process in a given jurisdiction than a crime control oriented decision.

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[27] It does not seem to make sense to discuss diffused "strict retributionist" decision making, since seemingly once a retributionist punishment is decided upon there is no need for further modifications of the punishment, thus no need for a diffuse component of the decision.

There, for example, the preferred mode may be one of diffuse, but low discretion decision making. Presumably, once the policy choices are made, all offenders would be subject to the same "system."

The issue of conflict among crime control goals is discussed next. This is also a rather complex issue, and we have devised Figure 1.3 to summarize our conceptualization of the problem. First, we introduce the more general concern for desistance or a lessening of criminal behavior as a form of either specific deterrence or rehabilitation. Second, we argue that although there may be important differences in whether one discontinues committing crime because of fear of rearrest (deterrence), or because of strengthened conventional values (rehabilitation), the idea of desistence can be used for both of them. Additional reasons for terminating criminal behaviors (e.g., too mature, requires too much energy, lack of opportunities, etc.) can also fall under the general heading of "desistence.". Although the reason for quitting or "slowing down" one's criminal activities may be important, we argue that frequently it is difficult to determine what the actual reasons are. From the point of view of the CJS, they may be functionally equivalent.

Thus, in Figure 1.3, two dimensions to the decision making process involving recidivism are depicted: the probability of recidivism (which can be defined in different ways) and the probability that some form of a CJS intervention has an effect on reducing the probability of recidivism (or lessening the severity or frequency of recidivism). In cell D1 offender with low recidivism probabilities are determined to be ones not amenable to influence by known CJS means. Presumably for crime control and efficiency purposes, these offenders would be subject to minimal CJS intervention (for humanitarian reasons, for example). In cell D2, the offenders are not

amenable to CJS treatments and are high recidivism risks. These offenders are candidates for incapacitative CJS responses in order to minimize the degree of injury they may inflict upon society. In cell D3 we have those who are not high risk offenders, (but whom nevertheless have some chance of recidivating) and whom are amenable to certain forms of CJS intervention (e.g., fines, employment or educational requirements) -- possibly with effects of (truly) enhancing the offender's life without "punishing." In cell D4, the offenders are at high risk of recidivism, but also are likely to be affected by CJS interventions such that the probability of recidivism is substantially reduced. This may take the form of imprisonment, drug program participation, or intensive supervision in the community. Whatever form such intervention takes, however, will be guided by considerations of recidivism probability (when and where such knowledge is available). Of course, if it is true that "nothing works" for any type of offender, then this is a null category, and incapacitation is the only alternative goal for these offenders.

In summary, Figures 1.2 and 1.3 give an overview of what we argue are the important variables in sentencing reform today. Recidivism is an integral part of these considerations because the perceived risk of releasing offenders into the community must be assessed and influence decision making for most cases that come to the attention of the CJS.

Implicit in Figure 1.3 is the idea that a CJS intervention may affect the recidivism chances of the offender. Traditionally, rehabilitation and specific deterrence goals are linked to interventions. How intervention is defined varies across the literature from specific treatments such as job training, educational, or therapy programs to more general interventions such as prison, jail, probation, and fines. Conceptually, the programs effects

literature has been primarily oriented toward rehabilitative effects, whereas evaluations of the effects of interventions such as prison have been guided by an interest in specific deterrence (the desistence or slowing down of criminal activity due to fear of further CJS interventions).

Our study of the effectiveness of sentences focuses on evaluations based on the subsequent recidivism of the offender. To the extent that interventions are shown to reduce the recidivism of offenders, we argue that the general concept of desistence is a helpful one toward describing such effects, and may be more useful than attempting to sort out whether such effects are specific deterrence, rehabilitation or something else.

AN OVERVIEW OF THE REMAINING CHAPTERS

The general intellectual problem that we address here is the problem of integrating utilitarian and retributionist considerations at sentencing in a systematic way. To this end, we will present some empirical evidence on the impact of sentencing on subsequent criminal behavior, leading us to interpret these results in light of the integrated system of sentencing described earlier. That is, the sentencing of an individual offender need not explicitly involve all the goals of sentencing, but a system of sentencing should interrelate the multiple goals of sentencing. This, hopefully, will lead to greater fairness and less disparity in sentencing practices. Utilitarian considerations, including the likelihood of recidivism, are part of the model of sentencing, as are retributive considerations.

Four major assertions underlie the current investigation. The first is that attempts to establish sentencing practices that ignore utilitarian questions of crime control are untenable. Crime control considerations, such

as deterrence, rehabilitation, and incapacitation, are an inherent part of sentencing, and rather than try to eliminate or ignore them, they should be explicitly recognized and integrated into a coherent program of sentencing.

Second, the prospect for recidivistic behavior is a legitimate (utilitarian) consideration at the time of sentencing. That is, the expectation that the individual will continue criminal activities may be a relevant factor at the time the judge decides on the punishment to be administered. The idea that the risk for recidivism be used in criminal justice decision making is not new, as it has been routinely used in parole release and probation supervision decisions, where there is a long history of research on determining acceptable levels of risk for release and supervision, respectively. In a similar vein, we suggest that sentencing policies can benefit by consideration of the prospects of offender recidivism.

Third, we contend that the factors that should enter into the assessment of acceptable levels of risk for recidivism need to be expanded. Currently, determinate sentencing practices are dominated by the use of few "predictor" variables. Under this model, only a limited number of variables (e.g., seriousness of the presenting offense and various prior record variables) are used in assessing the risk of recidivism. In states with sentencing grids, for example, only a measure of prior record and consideration of the nature of the presenting offense enters into the determination of the prescribed sentence. This practice ignores the rich literature on the etiology of crime and criminal behavior. By increasing the range of independent variables, we demonstrate that the statistical explanation of recidivism is enhanced as is, ultimately, the prediction of that behavior.

Our last assertion is that the determinants of recidivism must be considered prior to evaluating any impact that the sentence has upon recidivism. It is well known that criminal justice sanctions are nonrandomly allocated to offenders. Those with more serious presenting offenses and longer prior records are more likely to be incarcerated. Yet these are precisely the individuals who may be more likely to recidivate subsequent to serving their sentence. Consequently, without a better understanding of the precursors of recidivistic behavior, the ability to measure the effects of sentences on subsequent criminality is impaired.

The consideration of how recidivism can inform sentencing research and policy making raises many issues. Chapter Two outlines some general ways in which choice is involved in conceptualizing recidivism and the prediction of recidivism. Of related concern is which variables can enter into the prediction of recidivism. Some predictors (e.g., prior record and presenting offense) have become standard in grid systems for sentencing. Other variables (e.g., employment and educational history) are routinely used for parole release and probation supervision decision making. Still others, (e.g., offender race, socioeconomic class and sex) have a questionable status. Which variables may be used to model recidivism, and their empirical confirmation status are discussed.

Chapter Three presents the data used in the empirical assessments of recidivism focussed on in our research. We rely totally on data available from common Criminal Justice sources. These include the information from the Presentence Investigation Reports prepared to help judges decide the particulars of a given sentence, official arrest histories maintained by the State Police, and official incarceration histories provided by the Department

of Corrections. As such, our data mirror the information that is routinely available to judges.

In Chapter Four, we discuss the multiple ways recidivism can be measured with the data available to us. Following from the typology of measures introduced in Chapter Two, we investigate the empirical differences between various conceptualizations of recidivism. Of particular interest is how the various potential indicators of recidivism are intercorrelated among themselves and across post-sentence observation windows of varying widths. Thirty eight different recidivism measures are compared across four different observation windows. The results lead us to select 13 representative indicators for detailed analysis in the remaining chapters.

A general model of recidivism based on information known at the time of sentencing is presented in Chapter Five. Theories on the etiology of crime are used to select an initial set of 107 possible independent variables for our analysis. A winnowing procedure, designed to produce the most robust predictors of recidivism, is presented and used to reduce our list of independent variables to 43. Also introduced in this chapter are several interactions indicating how the effects of our predictor variables on recidivism are mediated in important ways. The basic model developed in Chapter Five is then applied to select measures of recidivism in Chapter Six.

Measuring the sentence itself is the focus of Chapter Seven. After examining the basic components of the sentences given to our sample, we show how these components are empirically related. Also discussed in this chapter are important distinctions between the sentence as mandated by the judge and the actual treatment the offender receives through CJS intervention. In

Chapter Eight, the impact of these sentences on subsequent criminal behavior is then assessed on the same select measures of recidivism.

The models developed in Chapters Six and Eight inform us about what influences recidivism at the aggregate level of the sample. For judges concerned with fashioning the particulars of a given sentence, the focus is much more on an individual, case-by-case basis. In Chapter Nine implications are drawn for the prediction of recidivism at the individual-level. Here, the concern is with how individual-level predictions differ depending upon which variables are used in the prediction equation and the extent to which different variables lead to different expectations that an individual will be a recidivist. Again, many representative indicators of recidivism are included in the analyses.

In Chapter Ten we use the results from previous chapters to suggest some general conclusions about recidivism considerations for sentencing. The integrative sentencing policy, introduced in the present chapter, is revisited in light of the ability to predict recidivistic behavior at the time of sentencing and control that behavior as a consequence of the sentence. We offer a series of principles, consistent with our empirical results, that may be used to guide judges in their sentencing practices.

Finally, a theme running throughout this book is that the cumulative impact of criminal justice decisions cannot be ignored. The fact that an offender appears for sentencing, and thus is in a sample such as the one used here, is the result of a series of prior decisions made by police officers, prosecutors, and other officials of the system. How so called "sample selection" processes impact on our models and our ability to predict recidivism is considered. Appendix A deals with the implications of sample

selection bias and how we address this issue throughout the analyses. A second appendix is devoted to results controlling for sample selection.

Figure 1.1

Minnesota Sentencing Guidelines Grid
(Effective August 1, 1989) (Months)

SEVERITY LEVELS OF CONVICTION OFFENSE	CRIMINAL HISTORY SCORE 0	1	2	3	4	5	6+
I. Sale of Simulated Controlled Subst.	12	12	12	13	15	17	19 18-20
II. Theft Related Crimes ($2500 or Less	12	12	13	15	17	19	21 20-22
III. Theft Crimes ($2500 or less)	12	13	15	17	19 18-20	22 21-23	25 24-26
IV. Nonresidential Burglary	12	15	18	21	25 24-26	32 30-34	41 37-45
V. Residential Burglary	18	23	27	30 29-31	38 36-40	46 43-49	54 50-58
VI. Criminal Sexual Conduct (2nd Degree)	21	26	30	34 33-35	44 42-46	54 50-58	65 60-70
VII. Aggravated Robbery	48 44-52	58 54-62	68 64-72	78 74-82	88 84-92	98 94-102	108 104-112
VIII. Criminal Sexual Conduct (1st Degree)	86 81-91	98 93-103	110 105-115	122 117-127	134 129-139	146 141-151	158 153-163
IX. Murder, 3rd Degree	150 144-156	165 159-171	180 174-186	195 189-201	210 204-216	225 219-231	240 234-246
X. Murder, 2nd Degree (with intent)	306 299-313	326 319-333	346 339-353	366 359-373	386 379-393	406 399-413	426 419-433

Figure 1.2

Dimensions of Sentencing Reform

Crime Severity

Low _____ High

Philosophical Basis

	Crime Control Goals			Retribution Goals		
		Discretion			Discretion	
		Low	High		Low	High
Organizational Diffusion	Low	C1	C2	Low	R1	R2
	High	C3	C4	High	R3	R4

Ideal Types

C1 = Judge alone makes determination of degree of punishment due offender based on grid derived from risk assessment instruments of recidivism potential of offender.

R1 = Judge alone makes determination of degree of punishment due offender based on grid of offense severity of the offender, possible including prior history dimension of offender's culpability.

C2 = Judge alone makes determination of degree of punishment based on subjective assessment of offender's likelihood of recidivating, or on recommendation of probation officer's clinical assessment

R2 = Judge alone makes determination of degree of punishment based on subjective assessment of offender's culpability

C3 = Decisions on how to treat offenders are diffused across judiciary, probation, corrections, and parole organizations, and are made based on recidivism considerations defined in grids and risk assessment instruments.

R3 = Decisions on degree of punishment are made by many (e.g., judge and parole board) using grids and possible risk assessment instruments, to the extent that crime control goals enter into consideration.

C4 = Decisions on how to treat offenders are diffused across judiciary, probation, corrections, and parole organizations, and are made based on subjectively perceived recidivism considerations

R4 = Decisions on how to punish the offender are made by many who make clinical or subjective judgements as to the degree of punishment.

Figure 1.3

Recidivism and Crime Control Goals

Specific Deterrence/ Rehabilitation Probability	Recidivism Probability: Low	Recidivism Probability: High
Low	D1	D2
High	D3	D4

D1 = Offenders judged to be low recidivism risks, but also not likely to be deterred or rehabilitated from what recidivism they would commit, if they were to recidivate. (Let go for humanitarian reasons)

D2 = Offenders who are high recidivism risks and who are unlikely to be deterred or rehabilitated. Such offenders are likely candidates for incapacitation. (Incapacitate)

D3 = Offender who are unlikely to recidivate but whom would benefit from some form of CJS intervention (e.g., probation or intensive supervision in the community). (Fine, Community Supervision)

D4 = Offenders who are high recidivism risks, but who may have their recidivism chances reduced by CJS interventions. (Whatever CJS intervention works)

CHAPTER TWO

RECIDIVISM: SOME BASIC PRINCIPLES

Various utilitarian claims made have been made as to the importance of recidivism for the numerous decisions made by the criminal justice system. While there is general agreement that better ways to predict recidivism are needed, little agreement exists as to how recidivism should be defined or how it should be measured. In this chapter we examine how recidivism has been used as a criterion the evaluating criminal justice treatments and for assessing offender risk.

Based upon our review, a three-fold classification of recidivism measures is developed. The theoretical implications of different conceptualizations of recidivism are then discussed. Ultimately, this typology of forms of recidivism guides the selection of dependent variables used to assess the effectiveness of sentences in later analyses. Thus, one of the central themes of this chapter is to develop some of the implications of choosing one outcome criterion as opposed to another in studying program success, risk assessment, and evaluating CJS interventions in general.

The second major theme of this chapter revolves around the issue of what constitute legitimate candidates for either predicting or explaining recidivism. That is, what independent variables are appropriate for models of recidivism? In addition to reviewing the types of variables previously used in the study of recidivism, we argue that the phenomenon should be considered more generally as a form of criminal behavior. Consequently, etiological factors should be included in models of recidivism.

To further lay the foundation for later chapters, other problems surrounding the study of recidivism are raised. The philosophical distinctions between predicting recidivism versus explaining it are discussed, as are issues surrounding the causal proximity of independent variables in models of recidivism. Finally, the temporality of recidivism is considered in terms of the length of time offenders should be followed to detect any recidivistic behavior.

RECIDIVISM AS A CRITERION OF EFFECTIVENESS

Reviews of the literature on the effectiveness of criminal justice programs reveal that a distinction must be drawn between outcome and recidivism. The former is more general, and often includes such factors as whether or not a released offender enters a job training program, obtains a job, keeps that job, gets to work on time, has a stable family life, and so forth. Recidivism is more narrowly defined as a return to criminal behavior or as the failure to meet the technical requirements of probation or parole. Ideally, for the purpose of understanding the etiology of recidivism, information should be available on outcome. Thus, the interrelationship of outcome variables, such as job, family life, and program participation, can help the researcher understand why an individual was a failure or success in terms of recidivism. In the review below, we focus on recidivism rather than these broader outcome measures, since the latter are not available as part of our data base (see Chapter Three). Moreover, inclusion of such outcome measures would further broaden the scope of our study.

In practice, recidivism has been defined many ways. Michael Maltz, in a thorough review of the such definitions, summarized 90 studies that used

recidivism as the dependent measure. The definitions were categorized into general categories, as follows:

* Arrest: number of arrests; recorded police contact; court appearance; time elapsed before the first arrest; did conviction result?
* Reconviction: jail or prison sentence; felony or less; new sentence.
* Incarceration: type of facility; seriousness of offense.
* Parole violation: nature of the violation; seriousness of the infraction; was it police-initiated?
* Parole suspension: new offense; number of suspensions.
* Parole revocation: new offense; seriousness of the offense; average number of good days on parole.
* Offense: seriousness; number; new offense.
* Absconding: was an absconder warrant issued?
* Probation: proportion redetained; length of time detained; number of violations; violation warrant (Maltz, 1984:62).

These measures, when ranked by frequency of use, were distributed as: return to prison (39 studies), technical parole-probation violation (26), reconviction (22), arrest (20), new offense (16), severity of offense (12), absconding (10), re-sentenced (8), parole suspension (8), parole revocation (8), probation violation (3), court appearance (3), and police contact (2) (Maltz, 1984:63).

Within each of these general categories, there is wide variation in how recidivism or "failure" is operationalized. Maltz notes that incarceration may be defined in one study as "return to prison" and in another study as "return to prison for a major offense" (Maltz, 1984:63). Thus, the actual number of operationalizations of recidivism in the 90 studies reviewed is even more heterogeneous than the general categories suggest.

Lee Sechrest and colleagues, in their report on rehabilitation to the National Research Council, discuss differences in the measurement of recidivism as one of the reasons we know so little about the effects of intervention programs:

> Recidivism has been the traditional measure for assessing effectiveness of rehabilitation efforts. As an outcome measure, however, recidivism presents difficulties, not the least of which is that there is no agreement on a definition of recidivism: it is assessed in whatever way is convenient, whether it makes sense conceptually or not. Recidivism is usually measured as if it involves a binary outcome, which results in the loss of considerable information, decreasing the sensitivity of tests for program effects. . . . Further empirical work on the standardization of measures of recidivism and on the suitability of multiple measures could have a high payoff (Sechrest, et al., 1979:7).

Since that time, there has been surprisingly little research aimed specifically at the substantive and methodological implications of choosing one recidivism measure over another (Maltz, 1984). Nor has there been much debate over some of the more specific recommendations on recidivism made in a paper commissioned by the National Research Council for the Panel on Research on Rehabilitative Techniques (chaired by Lee Sechrest). In that paper, Gordon Waldo and David Griswold (1979) made several recommendations regarding the measurement of recidivism:

> At a minimum, future recidivism studies should use FBI indicators of recidivism.
>
> An appropriate group of experts should be convened to determine what kinds of offenses to include when measuring recidivism.
>
> The use of continuous measures of recidivism should be more fully explored.
>
> Follow-up periods in studies of recidivism should range from a minimum of 3 years to a maximum of 5 years.
>
> There should be a continued reliance on official measures, although self-report measures should be used when possible.
>
> Greater attention should be focused on the reliability and validity of recidivism measures.
>
> At a minimum, studies of recidivism should only be compared within a context that considers: the sample, the length of follow-up, the quality of the research design, and how recidivism is measured (Waldo and Griswold, 1979:245-247).

These quotes illustrate that the problem of measuring recidivism is profound. It is in part responsible for the lack of knowledge about what works in criminal justice intervention. Considerably more needs to be known about the measurement of recidivism before such knowledge can result.

Research by several authors (see Blumstein et al., 1986:183) has supported the contention that choice of recidivism measure can make a difference in determining whether or not a given program is effective. More commonly, however, a specific measure of recidivism limits the definition of the effectiveness of a program, as success of a program is usually defined only in terms of a single recidivism measure (See Wilbanks, 1985; Hill, 1985).

ASSESSING RISK

Perhaps the most widespread use of recidivism measures is in the assessment of risk for probation supervision and parole release and supervision (Farrington and Tarling, 1985). Judges and parole boards must decide if an offender is an acceptable risk for release in the community. Traditionally, this has been a subjective decision, usually made after interviews, court appearances, or the reading of a probation officers recommendation. Where risk assessment instruments have been incorporated into the decision, it is often argued that more reliable and less disparate decisions result (Monahan, 1981; Carroll et al., 1982). Risk assessment instruments are discussed later in this chapter, but it should be mentioned here that the choice of a criterion variable for determining the predictive validity of the risk instruments may be a crucial one. For the most part, it seems that researchers and practitioners choose a binomial ("yes" or "no") failure variable, such as that used in the development and validation of the

Salient Factor Scale of the U.S. Parole Commission (Gottfredson, et al., 1978). There, the researchers defined recidivism failure as one or more of the following occurring within two years of release: a new conviction resulting in a sentence of sixty days or more; a return to prison for technical violation; or an outstanding absconder warrant.

At the state level, the most widely used scale for determining parole release and level of supervision, was developed by the Wisconsin Department of Corrections in the late 1970s. Revocation of parole was the definition of recidivism, and the Wisconsin "assessment of client risk" form was able to predict such failure very well: only 1 percent of the lowest risk group (lowest sixth of the cases) failed, while 39 percent of the highest one-eleventh of the cases failed. The instrument was able to explain 58 percent of the variance in criminal behavior by probationers and parolees while under supervision (Glaser, 1987:279). The Wisconsin risk instrument was found to work well in Los Angeles County and for the California Youth Authority, but not with New York City probationers (Wright et al., 1984).

Little is known about how sensitive risk instruments are to varying measures of recidivism as failure criteria. It seems that more effort goes into determining what are the good predictors of recidivism than into what a good measure of recidivism is or the difference it makes to choose one recidivism criterion over another. This is unfortunate as it is likely that the accuracy of a risk instrument will be tied to how "recidivism" is defined. If what predicts recidivism is contingent upon the measure used, then instruments calibrated using one form of recidivism (e.g., reincarceration) may be less successful in assessing the likelihood of other forms of recidivism.

Recidivism is presumably defined to suit the goals of a particular study or policy context. Yet many studies seem to choose a given operationalization without much, if any, explicit justification. Although no justification is necessary in the choice of one's research topic or, abstractly, the dependent variable, the specific measure needs a justification when selected instead of others. This is especially true if it may make a difference in the determination of the effectiveness of a program or in establishing the validity of a risk instrument. Often multiple operationalizations are possible given the data available, but most are simply ignored.

The decision to choose one over another is often justified on relatively weak a priori grounds. The arguments advanced include: re-imprisonment represents a more serious form of failure than arrest, the use of convictions is less objectionable than arrests because of greater certainty that the offender actually committed the behavior, arrests are more indicative of actual behavior than convictions, parole failure is preferable to arrest because the latter is less likely to be included in a data base, and a court appearance is preferable to a conviction as the latter is too stringent a criterion and contingent upon "proof beyond reasonable doubt".

Any of these arguments, and others, are reasonable, but are often based convenience rather than general theoretical, specific substantive, comparative, or methodological criteria. That is, it seems to us that theoretical justification can be given for the choice of operationalization. For example, choice could be based on criminal career concepts such as rates of arrest or time to failure models. Alternatively, a choice can be made after comparatively examining operationalizations to see if measures differ in their relationships with predictor variables. Finally, measures may be chosen

because they meet the criteria of greater reliability. We consider each of these in turn.

USING THEORY TO CHOOSE OPERATIONALIZATION(S) OF RECIDIVISM

As a heuristic device to organize the research on choice of recidivism measures, we differentiate three different general theories of recidivism measurement, based on what can be observed in the literature. The first we call binomial recidivism, and represents an interest in some form of a dichotomous variable -- did the offender fail or not? The second we call criminal-career recidivism, and represents an interest in the rate of an individual's offense behavior over a unit of time. The third is called failure-rate recidivism and focuses on the time to failure (with failure defined as in binomial recidivism) in groups or aggregates. These three do not exhaust the possible ways to categorize recidivism theory -- and a wide range of operationalizations are possible within each -- but they represent an attempt to examine explicitly the theory that is often unstated in the choice of recidivism measure.

Binomial Recidivism

The traditional approach of binomial recidivism (BR) makes several assumptions about the nature of recidivism. First, by definition, there is no degree of failure for an individual, only its presence or absence. For example, if reimprisonment is the failure criterion, simply whether or not someone is reimprisoned is considered important, not for how long or at what type of prison. This approach may be judged as relatively crude in its assessment of recidivism since variation within general types of failure is ignored. In addition, there is little concern for efficiency in the choice of

BR measures relative to other possibilities. All that is known is that the offender failed, not how long it took to failure or whether the failure consisted of multiple acts or offenses. Thus, for example, if two groups are being compared as to their BR over the course of a year, they may be the same even though the recidivism for one group occurred mostly within the first six months while that of the second group occurred within the second six months. Again, the BR approach is not sensitive to this possibility, whereas other measures of recidivism could reveal such a pattern.

Under the BR approach, overall recidivism rates for individuals or groups are strictly determined by the window of follow-up for the study. If offenders are followed up for three years, then the probability of failure is usually presented for three years. Although the probabilities of failure for shorter time periods are possible, they are not always studied or presented. Thus, BR measures represent a primitive form of indicators of failure-rate recidivism. If the window of the study is relatively short, then the possibility of misleading results might follow. For example, if a follow-up period of six months is chosen, differences may be found to exist across groups, whereas if the study window were extended to two years, the differences might dissipate. Other types of recidivism measures would be sensitive to this possibility -- within their study windows, of course.

BR measures also ignore the implications that offenders who fail (e.g., are rearrested or reincarcerated) are not eligible for further recidivism by definition. Research interest in failed subjects ceases within the BR approach. Subsequent behavior is not utilized. In contrast other approaches consider more than one "failure event".

On the positive side, the BR measures are easy to interpret. Knowing the percent of a group that has failed or the probability that an individual will fail, given certain characteristics, is easily interpretable and does not require special training or an understanding of complex statistical concepts. Unlike some criminal career measures (e.g., Murray and Cox, 1979), there is less danger of contamination within a quasi-or experimental design in which a BR measure is a dependent variable. For example, in an evaluation of the effect of imprisonment versus probation on subsequent recidivism, a "rate of rearrest" dependent variable may threaten the internal validity of the design in that recidivists are presumably subject to incarceration or probation for their subsequent arrests. These may well influence the chances of a subsequent arrest, thus representing a diffusion of treatments (Cook and Campbell, 1979:54). The BR approach, on the other hand, would not be subject to the same criticism since research interest in the case terminates with the first failure.

In practice the BR approach has been neutral as to the issue of whether there are special types of offenders based on their recidivism patterns. There are only failures and successes (or "not-yet-failures"). This is in contrast to certain criminal career approaches in which distinctions are made, for example, between high or low rate offenders, or between persisters and desisters. The BR approach tends to assume that anyone can recidivate, and takes a "wholistic" approach to analyzing recidivism in the sense of looking at the recidivism of all individuals, not special types.

Generally, success within the BR approach is often clearly interpreted relative to rehabilitation or specific deterrence theory. If someone ceases to commit crime, the BR theorist is possibly on "safer ground" than other

theorists in interpreting such successes in terms of rehabilitation (the offender's attitudes or beliefs have changed) or specific deterrence (the offender is too afraid to commit further crime for fear of additional punishment).[1]

Criminal-Career Recidivism

The second major type of recidivism approach is that of the criminal-career perspective, or criminal-career recidivism (CCR). In general, the approach may be characterized by counts or rates of offending. Thus, distinctions are made in the level of recidivism. Here too there are several advantages and disadvantages to the approach.

The underlying assumption of the CCR perspective is that some criminals commit many crimes over the course of time or their "criminal careers": they are chronic offenders. This is consistent with the observation that a large proportion of the crimes that come to the attention of the police are attributable to a small proportion of the general population. For example, in Philadelphia, Wolfgang and colleagues found that six percent of the cohort were responsible for 52 percent of the recorded juvenile offenses (Wolfgang, et al., 1972). Greenwood and Abrahamse (1982) found that the most active ten percent of burglars committed over 230 burglaries per year, while the average burglar committed a median value of 5.5 per year. Thus, the assumption of the existence of a separate group of offenders with high rates of offending seems to be true, and attempts to describe the criminal behavior of such groups in terms of rates of offending, seem promising. There are clearly numerous

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[1] The concepts of rehabilitation and deterrence seem to be less clear in the case of rates of offending. Here, terms need be understood in a "special way" to claim, for example, that an offender who now only commits one burglary a year instead of four is "deterred."

reasons for identifying these groups and developing policy programs to interrupt or end their criminal careers. The CCR approach is a relatively efficient approach, and one aimed at offenders of great concern to the general public.

The criminal justice system has been characterized as a "revolving door" through which some offenders pass. Frequently, arrested offenders quickly return to the streets and commit new crimes after making bail or serving approximately eight months in jail or an average of about two years in prison. At the risk of overstating the case, the CCR point of view is a "cynical" one in that offenders are believed to be not only chronic offenders, but that the CJS will nevertheless put them back on the streets to commit more crimes. If this is true, the CCR approach is well suited to capturing this phenomenon with the concept of rates of offending, adjusting for time on the streets.[2]

The CCR approach seems to be rich in quantifiable concepts that may prove fruitful for understanding recidivism processes. For example, crimes per unit of time (lambda) or arrests per unit of time (mu), have been widely discussed over the past several years (Blumstein et al., 1986). Other concepts, such as career length, time intervals between arrests, and offense specialization, active and quiescent states, are linked to the CCR approach. Although some of these concepts have been criticized (Gottfredson and Hirschi, 1988), it seems clear that to the extent that successful predictions can be made about criminal career length, the spacing of offenses, and specialization into highly predatory career patterns, there are policy benefits to be gained. Note too that the CCR theorist makes use of much more information about the

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[2] Rates of offending are usually adjusted for the amount of time that the offender is at risk during a time period. Thus, if an offender has been in prison for half of a year, the rate of crimes is doubled to yield a yearly rate.

behavior of the individual than does the BR analyst. The CCR approach may examine closely the effects of aging on offending rates, or the passage of offenders through stages of activity and inactivity -- concepts not well-suited to the BR approach.

The CCR approach typically assumes that there are heterogeneous groups of offenders, and that separate models need be examined for each. For example, distinctions are made between desisters and persisters, high rate and low rate offenders. To the extent that these groupings represent valid distinctions, more accurate models may be developed. Similarly, the CCR approach is well suited to the concept of selective incapacitation (and to some extent was developed for this purpose). Certain offenders may be deemed likely to behave in a manner that is highly injurious to society. If they can be identified and kept from this behavior, society may benefit. The rate concept provides a way of identifying those who should be selected for special treatment. Consequently, rates of offending may represent a more sensitive mechanism for identifying specific deterrence effects than the BR approach. It may be the case that offenders are slowed, but not stopped, by certain interventions. Conversely, some interventions may accelerate criminal behavior patterns. Thus, CCR measures may be in a better position to identify these processes relative to BR indicators.

On a more critical note, the CCR approach to date frequently glosses over the issue of ascertaining the effects of any specific CJS intervention on an offender's subsequent behavior. In part, this is because the concept of a rate of arrest subsequent to an intervention represents alternative occasions for the CJS to have an impact and thus contaminates experimental or quasi-experimental designs to evaluate the impact of any particular intervention.

Furthermore, the likelihood of subsequent intervention increases with the increase in criminal behavior rates, thus making such contamination more probable.

In sum, the CCR approach seems primarily useful for identifying those offenders who continue to commit crimes frequently enough to warrant measuring their rate of offending. If offenders desist, then that can be studied, often separately from the study of persisters. However, if offending behavior is infrequent at the individual level, then the case is considered uninteresting or a special category. Thus, it is not clear how useful the CCR approach is across the spectrum of offenders.

Failure-Rate Recidivism

The third general approach has been less widely developed than the other two, despite the efforts of Michael Maltz, who has advocated the failure rate recidivism (FRR) and made it more accessible through his lucid work (1984). The FRR approach implicitly dismisses the CCR approach, since each additional event or contact with the CJS after the one that defines the individual as a recidivist represents the occasion for a new presenting offense with its own subsequent recidivism to be studied. This approach could be considered a modification of the BR approach in which the time component of dichotomous failure outcomes is analyzed.

Like the class of BR measures, FRR measures are not contaminated by the further activities of the offender or the CJS. An offender who fails is classified as such for the remainder of the time period of the study. FRR goes beyond BR approaches in that it reveals the rate of failure at different points in time subsequent to release in the community, as well as the cumulative proportion of failures across experimental and control groups.

This allows for more detailed or sensitive evaluation of programs. Another advantage of the FRR approach is that results are readily observable, since they are usually presented graphically by rate of failure or cumulative proportion failing over time.

Unfortunately, there are several underlying models that can be used to characterize failure rates and choosing the most appropriate model requires considerable expertise, as is evident in Maltz's work (1984). Consequently, the FRR approach is not widely used among non-academics, nor is it routinely used in program evaluations. Since it is probably not well understood in this context, it is unlikely to be used widely. Nor is it clear that there is policy interest in the FRR approach beyond the information provided under the BR approach. For example, if after a one-year follow up of recidivism, the decision maker finds the BR approach reveals that 50% of a control group and 50% of an experimental group failed, how ful is to know that the experimental group failed at a slightly slower rate in the first six months and a faster rate in the last six months? For example, it is not easy to interpret such differences as evidence of a rehabilitative or deterrent effect. However, if there are theoretical reasons for expecting such a finding, the FRR approach may be relatively useful.

In summary, there are three general types of orientations to the measurement of recidivism. The choice of one or all of the approaches represents a selection among varying assumptions, with some positive and some negative implications for possible use in various research agendas. Seldom does one see in the literature an explicit discussion of the choice of recidivism measure based on these considerations.

SPECIFIC SUBSTANTIVE CONSIDERATIONS

Whether one chooses the binomial recidivism, criminal-career or failure rate approach, there are still choices to be made among the commonly used substantive definitions of failure. For example, aspects of technical violation of parole/probation requirements, arrest, conviction, or incarceration could be measured under any of the three orientations. To a limited extent, these represent a continuum of liberal (broad) to conservative (narrow) definitions of failure. If an offender violates a condition of parole, this is often considered a less serious infraction than committing a crime, and thus would result in larger proportions of a sample failing. However, not all releasees are on parole or probation (less than half of prison releasees across the nation today), so many offenders are not eligible to fail by this criterion.

Mark Moore and colleagues (1984) have implicitly argued for the use of arrest-based as opposed to conviction-based recidivism measures, noting that the former are more likely to represent the true behavior of the offender whether or not the offender was actually convicted). The contention is that probable cause standards are adequate for determining recidivism, whereas "beyond a reasonable doubt" is too stringent. Others contend that moral and even legal grounds exist to limit the measurement of prior criminal behavior to convictions (see Tonry, 1987). It is not clear that these same standards apply to the measurement of recidivism, since recidivism is usually used for research purposes, while prior criminal behavior may be used to make decisions pertaining to the treatment of the individual offender. Clearly, the use of arrests as opposed to convictions results in a larger proportion of failures for any sample. Arguably, the frequency of arrests also more closely

approximates the frequency of actual criminal behavior of the offender than does the frequency of convictions.

Re-incarceration is the most conservative (narrow) of the approaches discussed, since it is relatively unlikely for an offender to be re-incarcerated. Presumably, characteristics such as the seriousness of the offense, the availability of evidence, willing witnesses, and so forth would determine whether or not the offender is reincarcerated. One could argue that the deeper into the CJS that the offender must go to be defined as a recidivist, the more unclear and potentially biased is the recidivism measure. In part, this is due to many decisions that select out offenders in ways that are difficult to control for statistically (see Berk, 1983). This suggests, all else being equal, the use of arrests or technical violations as failure criteria, rather than convictions or imprisonment, if the goal is to measure the recidivistic behavior of the individual.

Offense Seriousness

In addition to considering the form of the CJS intervention that constitutes recidivism, there has been some interest in the seriousness of the subsequent criminal behavior of the offenders. For example, the selective incapacitation approach of Greenwood and Abrahamse (1982) attempts to define serious offenders as high volume offenders. Research by Chaiken and Chaiken (1982) focusses on violent predatory criminals -- those who commit serious crimes against the person. Thus, the seriousness of the recidivistic behavior can be an important aspect of its measurement.

Two commonly used ways to measure seriousness are to use general public assessments of offense vignettes and to use the punishments specified in criminal law. In the general criminological literature, both approaches have

been widely debated (see Wolfgang et al., 1985). Somewhat surprisingly, there has been little systematic research on offense seriousness other than that found in the selective incapacitation literature where the focus has primarily been on rates of crimes or commonly defined serious offenses such as robbery and aggravated assault.

Often, the dimension of offense seriousness is only implicitly treated in studies of recidivism. Distinguishing between types of crimes using a BR approach, or rates of offending for different offenses under the CCR model, is an indirect measure of offense severity at best. Note, however, that a consideration of the seriousness of the recidivistic crime(s) represents another way to measure differences in the level of recidivism across offenders. Rather than comparing offenders on say, rates of particular crimes, (e.g., robbery), they could be compared on the basis of summary measures for the seriousness of all crimes.

How Wide a Research Window?

In most research applications, it is possible to choose how long a period of time to follow up offenders. Sechrest et al., writing for the National Panel of Research on Rehabilitative Techniques (1979:246), have argued for a minimum of three year follow up periods and a maximum of five years. They contend that it is unreasonable to consider individuals who commit crimes after five years to be recidivists. Implicit in this decision is the assumption that after five years the impact of the incarceration and/or treatment would be negligible relative to other factors that would determine criminality. Sechrest et al. also note that recidivism windows of less than two years run the risk of missing delayed effects of treatment. Although there is no ideal time frame for recidivism to occur, and often the window's

width is determined by monetary considerations, researchers should examine the effects of choosing one window versus another.

The choice of a research window thus becomes a substantive decision. Sechrest et al.'s observation that recidivism after long periods of time cannot easily be attributed to the failure of a CJS intervention points to the fact that window width is directly tied to the definition of "success." Shorter windows allow for conclusions concerning program effectiveness. Longer windows address an intervention's impact on criminal behavior more generally defined. Consequently, as the length of the observation window increases, the implications for what is being studied changes.

The Comparative Approach

As stated above, deciding to use a specific form of recidivism measure is often done in an ad hoc way. In part, this is because no one knows what is an ideal form for measuring recidivism. For example, in discussions with various practitioners, there was little agreement about the best indicator of recidivism. Often there were multiple answers as to what would be an "interesting" measure of recidivism to study.

We think it reasonable to propose that multiple-measures of recidivism be used, whenever possible, to provide the researcher and the policy maker alternatives from which to interpret results and develop policy. This represents what is known as "sampling validity" -- determining whether or not it makes a difference to use one measure of recidivism as opposed to another in research settings. If some measures of recidivism are predicted well by the research models, and others are not, this is useful information in interpreting the observed effects.

Moreover, a comparative approach to measuring recidivism is needed to assess the sensitivity of risk assessment and program evaluation to definitions of the dependent variable. Rather than assuming that all indicators of recidivism are interchangeable, informed policy requires knowing what difference it makes to assess risk relative to one outcome versus another. Given that the literature is quite vague on the "appropriate" indicator for recidivism, a comparative approach would seem desirable.

Methodological Criteria

Choice of recidivism measures may be made in conjunction with information regarding the reliability of the measurement of the variables. Since recidivism is usually a dependent variable in most studies, the issue of random error in its measurement is of less importance than if it were an independent variable (Cook and Campbell, 1979:160). Random measurement error in a dependent variable can effect the precision of an estimate, but does not generate biased parameters in analytic techniques such as analysis of covariance or regression analysis. It is like random sampling variation.

Where there is systematic or non-random measurement error, however, one would ideally like to know its source and make statistical adjustments or controls, if possible. Non-random measurement error in recidivism may be the result of factors such as greater surveillance of certain offenders (e.g., those on parole or probation), or variations in reporting crimes by jurisdiction, or underreporting less serious crimes, to mention a few possibilities. If a particular measure is known to be less reliable and the reliability cannot be corrected or adjusted for, there is possibly less reason to use the variable, or less ability to interpret models predicting it.

POLICY CHOICE

Choice of a recidivism measure is often made in the context of a policy directive, typically that of a government agency. Thus, the choice of a certain variable may be determined by extra-scientific goals. Re-conviction, as opposed to rearrest, for example, may be chosen because policy makers are bound legally to so do when making decisions about individuals. It is often the case, however, that policy decision makers do not pay explicit attention to the scientific implications of choosing one measure of recidivism over another, even though legal restrictions are not in place. It seems that there is a rather widespread assumption that any measure of recidivism is interchangeable with any other. The truth of this is, of course, is an empirical question and one that we will address directly in later chapters.

Whether or not there are legal restrictions on policy makers in choosing measures of recidivism, there may be scientific reasons to know if offenders tend to rearrested (but not necessarily convicted) to inform general policy as opposed to specific decisions about individuals. This distinction is not often recognized, yet it is an important one (See Goldkamp, 1987).

Often policy makers fashion the context of research questions, and thus limit the extent to which there is real choice in selecting recidivism measures. Consequently, researchers may exclude from consideration some measures of recidivism, and even some predictor variables, based on general policy directives to focus on certain other measures of recidivism. Much research on recidivism must be evaluated in light of these restrictions.

Of course, the "policy context" of recidivism may be one of ignoring it all together. Over the past twenty years there has been considerable debate by scholars over issues relating the seriousness of the <u>presenting offense</u> to

the severity of the punishment. Von Hirsch (1975), for example, has discussed proportionality in sentencing as being essential to the attainment of retributionist goals. More recently, Tonry (1987) has argued for a revision of criminal codes across the states to provide a more detailed code with fewer generic categories to reflect the gravities of the criminal behaviors themselves. Interestingly, there has been a similar debate over the importance of the seriousness of the subsequent criminal behavior of offenders among advocates of utility criteria. Incapacitation proponents, for example, focus on the prediction of high rates of subsequent criminal behaviors and argue for prolonged incarceration of those offenders (Greenwood, 1982). Yet, the utility argument has been somewhat limited in scope. The criminal career approach is one promising avenue for a more systematic use of prediction and classification in CJS decision making. By developing more systematic evidence about how to measure and predict recidivism, researchers and practitioners may better understand its processes.

As outlined in Chapter One, it is our assumption that recidivism considerations in general are an important part of decision making processes in the CJS. We assume, along with most others, that strict retributionist approaches to decision making are not viable. Rather, some degree of one or more crime control goals will be part of most decisions on the official intervention of the CJS in the offender's life. Whether that decision is motivated by selective incapacitative, specific deterrence, or rehabilitative goals, the recidivism of the offender is in question.

Given the importance of recidivism to decision making, it seems logical to address issues surrounding its utilization in the CJS. Toward this goal, we discussed above three broad types of approaches to recidivism: binary,

criminal career and failure rate models of recidivism. Each seems to have some strengths and weaknesses as a general class of recidivism measures. Below we focus on the other half of the recidivism equation, the predictors of recidivism. Several fundamental problems to the modeling of recidivism are addressed in terms of broad methodological issues on the conceptualization and choice of independent variables.

RECIDIVISM AS CRIME: FURTHER METHODOLOGICAL ISSUES

Attempts to model recidivism, however measured, are confronted with several methodological difficulties identified in the literature. These fundamentally impact upon how recidivism is conducted and are difficult to resolve. Three general problem areas are discussed here; the distinction between prediction and explanation of criminal behavior; issues of causal proximity, and; the problem of temporality in causal models of crime.

Explanation and Prediction of Recidivism

The idea that there is a distinction between prediction and explanation may be puzzling to some, because it is often assumed that the two are interchangeable concepts in research applications: what explains must predict, and what predicts explains. The distinction is very problematic, of course, in discussion in the philosophy of science literature.[3] Problems associated with the distinction of explanation and prediction stem from the fact that, to an increasing degree, prediction of recidivism has taken

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[3] Thus, within the philosophy of science literature there are long standing issues of whether special forms of explanation are required by the social sciences, whether historical research has special status, and so on (see, for example, Scriven, 1969). It is perhaps widely held among scientists, however, that scientific explanations should be "in some sense," predictive of the observable empirical world.

precedence over explanation within much of recent empirical work on recidivism. The argument and the distinction itself requires some "explanation".

The "classic," and admittedly oversimplistic, distinction between prediction and explanation is that "not everything predictive is explanatory, but that which is explanatory should be predictive." Within the context of studying recidivism with regression models, for example, one can conceive of variables which may be predictive of recidivism, but not explanatory (e.g., being left-handed), and variables which may be predictive and are explanatory (e.g., being unemployed). Of course, variables not found to be predictive may be deemed "not explanatory" because of their failure to predict.[4]

Rather, the distinction between explanation and prediction in the study of recidivism centers around two related issues -- what might be call the "omitted variables issue" in prediction research and the "conceptual proximity issue" in etiological research. In general, if variables in a given empirical study are said to explain, it is necessary that they are found to be statistically significant predictors, and are linked to a theory recognized by the scientific community as relevant to the subject matter. However, almost any predictive variable can be construed as "theoretical".[5] In practice, the problem here is not the lack of any theoretical status of predictor variables,

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[4] Although the distinction between explanation and prediction so stated here may be objectionable (e.g., someone probably has a theory of left-handedness and crime; some aspects of explanations are not subject to empirical test -- Lakatos, 1972), it is not our intent here to explore all these distinctions.

[5] The fact that so many variables have "theoretical status" is itself problematic to those who would like to deem at least some types of variables (and their theories) as "falsified." Thus, for example, Hirschi has argued that social strain theories of crime should be jettisoned, based on their poor predictive performance in empirical tests (Hirschi, 1987).

but whether or not the prediction of criminal behavior by a subset of independent variables -- to the deliberate exclusion of other independent variables -- is appropriate scientifically, ethically, and even legally (Sparks, 1983).

That some degree of selectivity of independent variables already occurs in some policy-oriented research is made clear by the summary of Table 2.1. There, variables used in predicting recidivism in research pertaining to parole and sentencing guidelines have been compiled by Petersilia and Turner (1987). The specific variables are classified as to their relative frequency of use. Thus, for example, some variables (e.g., drug use) are used in more than three-fourths of the community supervision risk assessment instruments, while others (e.g., living arrangements) are used in fewer than one quarter. These results generally indicate that there is little consensus across states and across general type of application (at sentencing, parole release, or community supervision) as to which predictor variables are useful. Note also that offender race, ethnicity, and sex are not on the list, indicative of their omission as a consideration from these instruments.

There is the "scientific" argument that a model of recidivism that omits theoretically relevant variables may include biased estimates of the effects of specific predictor variables. Thus, the claim that a given variable has certain specified predictive power based on an incomplete model could be biased since predictive variables are often correlated and their inclusion can effect the size of the observed effect. (If the variables are not correlated, the estimates will not be biased, but may be inefficient, see Berk, 1987).

Suppose, for example, that a hypothetical recidivism Model A omits race as a predictor variable of recidivism on ethical grounds. Model B, which is

the same model as A, except race has been added as an independent variable, finds offender race to be a good predictor. The observed effect of another independent variable in Model A, say, number of prior convictions, is potentially biased because its effect may not be the same in Model A as in Model B. If race and number of prior convictions are correlated, there is likely to be a difference in the estimated effects across the equations. In the extreme, prior convictions could be significant in one equation and not the other. Using the results from Model A to derive a decision tool aimed at determining length of sentence or eligibility for release on parole could result in blacks serving longer time because they have on average more prior convictions than non-blacks. That is, blacks may be subject to longer terms than if Model B were used to determine their punishment.[6] Number of prior convictions, if positively correlated with race, may have a large effect in Model A (where it is ignored) than in Model B.

Critics of predictive instruments which omit variables such as race, have argued that this exclusion on ethical grounds "backfires" because the failure to remove the effects of race, and other socially relevant variables, leads to a subtle inclusion of their effects in the parameters estimated for

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[6] Goldkamp has shown how "race" may be used to determine the level of punishment for an individual, but not based on the individual's race per se (1987). Information in the aggregate about the effects of race may be "used" to derive the degree of punishment -- if one introduces an external criterion, such as "punish the least amount possible." Suppose that one were modeling recidivism and that one intended to use the results of the model to decide who should serve time in prison or how long they should serve in prison. Suppose further that one finds that the variable measuring "being black" is predictive of recidivism. To use the individual's race to increase the degree of punishment would be discriminatory and ethically reprehensible. In the punishment decision, however, one could decide to treat all offenders as if they were non-black, i.e., reduce their punishment (or likelihood of imprisonment) by x amount <u>for everyone</u>. This would be using the information on race in the model in the aggregate, but would not necessarily be discriminatory, since an individual's punishment is not derived from the <u>individual's</u> race.

the other variables. That is, biases in the estimation of the effects of the remaining factors can lead to the perpetuation or institutionalization of racial, and other types of discrimination.

Although this is rather well-known in the literature, it is usually ignored or minimalized. The impact in some applications has been shown to be small, in part perhaps due to the conflicting directions of correlations, as well as small correlations, between offender characteristics and race. Thus, research on bail-release decisions has shown that seven percent of the defendants would have been classified differently as a result of omitting gender and employment -- two variables often discussed as representing morally reprehensible factors in determining punishment (Goldkamp, 1987). Also, being black may be correlated negatively with some criminogenic attributes, such as having a drug dependency (see Petersilia and Turner, 1987). Thus omitting race and including drug dependency may well result in less punishment for blacks. In the literature on this topic, the focus has been on the small empirical magnitudes, leaving the basic philosophical conflicts logically unresolved.

However, the "minimal effects" argument has been presented in terms of relatively few variables, such as race, or gender. The impact of additional "morally or legally objectionable variables" -- variables omitted from many predictive instruments of crime -- such as various measures of employment, past adaptation in programs, various dependencies, social class, and the like has not been systematically studied empirically. Moreover, even the impact of one variable, race, has not been studied extensively.[7] It is one of the

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[7] Petersilia and Turner (1987) call for more study of this problem in terms of its policy impact.

goals of the current research to explore the implications of "omitted variables" further.

The ethical and legal arguments against omitting variables such as race may be confusing to some because including variables such as race in a predictive instrument seems to be ethically reprehensible and legally impermissible. While we do not support the position that race should be used as a criterion for increasing or decreasing punishment, ignoring race, or other similar factors, in statistical analyses may result in more punishment for blacks than if race is included in such analyses.

There are two general solutions, each with many variations, to the problems discussed above. Solution 1 has two parts, Solution 2 only one. Solution 1A involves including race as a predictor of recidivism in defining policy so as to achieve less statistical bias in estimates for other predictors of recidivism. Solution 1B (assuming 1A has been done) involves making a policy-decision to ignore the individual's race in making punishment decisions (following Goldkamp, 1987). For example, it can be decided to treat all races as if they were the racial group that qualifies for the least punishment (or most punishment). Solution 2 assumes that 1A has been adhered to, but explicitly uses race, sex, etc. to make decisions about the treatment or nature of some aspects of intervention (job training, education, drug therapy) but not punishment per se. The idea is something like "affirmative action" for "deprived" groups.

Solution 1 (A and B) probably has more appeal than Solution 2 in the current political environment. The criminal justice system is "there to punish, not coddle criminals." The ideas associated with "rehabilitation" of offenders have not been popular in most academic writings associated with the

U.S. criminal justice system for at least twenty years. The problem with Solution 1B is that it is limited to punishment decisions -- as if that is "all" the criminal justice system "does" to offenders. But, in fact, the various decision makers in the criminal justice system often define their purpose as much broader and this usually includes the possibility of helping the convicted. Although this may not be politically a "fashionable" idea, it is argued that it is more in harmony with the intentions of criminal justice agents (judges, parole boards, etc.) than the more strictly "punitive" orientation that enjoys political success. Our goal here is not to claim that either Solution 1 or 2 is preferable. Rather our purpose here is to discuss some general ways research on recidivism may be reconceptualized so as to be useful to either approach to the implementation of findings from research on recidivism.

We take the general stand that inclusion of all relevant predictor variables is preferable to selectively omitting variables on the basis of ethical or legal grounds. We view this as preferable if only to obtain unbiased estimates for the effects of other, less controversial variables such as prior convictions and presenting offense. Minimally, this also allows for an assessment of what might be lost were a given variables to be excluded on some ground. Moreover, identifying the magnitude of the impact of, say offender sex, on recidivism is clearly separable from how that information is used for purposes of policy and decision making at the individual level.

Causal Proximity and Distance

In addition to the "omitted variables issue" is the issue of the "causal

proximity" among etiological theories of crime and delinquency.[8] Causal proximity actually refers to two separate concerns: conceptual proximity between independent and dependent variables (e.g., using prior criminal involvement to explain future criminal involvement) and causal order among independent variables. That is, some causes of crime are themselves caused by other, "earlier" causes, are thus are more properly seen as intervening variables. Conceptual proximity will be discussed first.

Etiological theories maintain that certain factors are important causal elements in the occurrence of criminal or delinquent behaviors. As such, these theories have been developed with the goals of explaining criminal or delinquent behavior. Some of these theories may be drawn upon for particular policy purposes, such as the development of a risk assessment instrument. Factors known in the etiology of crime also may be selected for testing in risk assessment instruments.

The question of "conceptual proximity" of variables is directly relevant to the, often unstated, link between what causes criminal behavior and what variables are used to predict it. Predictors that are conceptually proximate (e.g., number of recent offenses) are often preferred in more policy oriented research over predictors that are less conceptually proximate (e.g., living with a family). In part, they are preferred because the conceptually proximate variables are better predictors. In part, the preference arises because they are easier to justify to policy decision makers who see at least some of the conceptually proximate variables as measuring a retributionist

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[8] Although most etiological theories of crime were actually developed primarily with delinquency in mind, we will assume here, as is generally done, that the theories of crime and delinquency are generally interchangeable, except in that logical impossibilities exclude the relevancy of some variables (holding a steady job) to delinquency, and other variables (truancy) to criminality.

dimension.

Thus, for example, the number of prior arrests or convictions is perhaps the most widely used predictor variable in recidivism research and in making decisions about how long a sentence should be or how much time an offender should serve in prison. The choice of this variable is made partly because it is a good predictor and partly because it is "viable" in a political environment focussed on punishment. The fact that is it also a "conceptually proximate" variable -- and that by the strength of its predictive ability various complexities are introduced in assessing the relative strength of alternative conceptions of why offenders commit crime -- has not been discussed frequently in the literature.

The focus on conceptually proximate variables is associated with the study of crime with a predictive emphasis. At best, there is a secondary concern with the theoretical nature of the predictive variables used in the prediction. A study primarily oriented toward explanation, on the other hand, takes on a more demanding task of choosing predictor variables based on explicit theory and with greater concern for the logical relations among the variables used in the analysis. It is our intention here to explore some of the implications of the tendency in recent years for conceptually proximate variables to "take precedence" over other variables.

The concern with conceptual proximity has been discussed in the context of debate over the relation between advocates and critics of the criminal career approach to the study of crime, an approach that has focussed on descriptive components of an offender's behavior, while taking a "neutral" stance on the causes or origins of criminal behavior per se. Thus, Blumstein has argued the criminal career paradigm is not formally a theory of criminal

behavior, but rather is a way of studying it. As such, the criminal career perspective can be seen as providing specific definition as to interesting ways to define the nature of the dependent variable, focussing on such concepts as lambda (the rate of offenses) or mu (the rate of arrests). Independent variables, traditionally defined by etiological theory, have been used within the criminal career paradigm, but with a greater focus on past criminal activity (including age of onset, prior arrests, convictions) and past criminal justice interventions (prior incarcerations, parole and probation failure) as predictive of the various forms of criminal behavior.

This is <u>not</u> to say that the basis for predictors such as these is atheoretical. Rather, the argument here is that, in practice, studies with a predictive focus express little concern with issues of conceptual proximity or of causal order. This arises because these studies seldom are directed to the broader issues of the origins of crime or to how the causes of crime vary over time. Many studies with a predictive focus are interested in policy decisions: is someone likely to commit crime in the next few years? If prior criminality, and often-used associated measures such as prior incarceration and age of onset, are utilized to predict recidivism, little attention is paid to the logical status of these variables relative to other types of variables. This is especially true for those variables associated with various social structural characteristics of the offender (race, gender, social class, etc).

The issue of conceptual proximity overlaps with those discussed above in the distinctions between prediction and explanation in research on crime. If the primary goal is to explain criminal behavior, the question arises as to whether or not all the variations in the measurement of past criminal behavior and criminal justice actions are referent to explicit theories of criminal

behavior. If they are not, it is arguable that the inclusion of these variables merely confounds the explanatory interpretation of the results obtained in analyses such as in regression applications. There is, of course, an explicit theory behind the use of at least some, if not all, predictors associated with the criminal career perspective. Various personality and social structural theories would suggest that there is considerable constancy in human behavior, including crime, over time; some life course theories suggest that early involvement in crime measures a more criminogenic tendency on the part of the individual. But there is little explicit theory beyond this. Also, the inclusion of variables that measure this constancy pose several interpretative problems in that these measures are not logically distinct from the dependent variable (crime) and also are presumably themselves a product of earlier non-criminal career causes. All criminal careers have a starting point and thus, by definition, there can be no criminal career independent variables prior to that point. Presumably, traditional etiological theories of crime are relevant to the prediction of the start of the criminal career, as they are to its continuance.

The inclusion of past criminal involvements in predictive equations of future criminal involvements has been more or less routinized in recidivism studies, in part because such measures are the best predictors. Yet there has been a bifurcation in the criminal justice field. In recidivism, the focus has been primarily (but not exclusively) on prediction. In studies of crime or delinquency the focus has been on explanation. Most recidivism studies have prediction as their goal because they are conducted within the policy context of assessing the risk that an offender presents to the public. In such a context, the explanation of criminal behavior is of secondary

importance to the prediction of criminal behavior. Although these predictive approaches do not rule out explanation of criminal behavior as a goal, explanation has not been their primary focus. Thus, for example, Greenwood developed a scale for predicting high rate offenders, one that incorporated factors on the basis their being "good predictors." Similarly, the Salient Factor Scale has gone through various adaptations, increasingly avoiding the use of "social" predictors, such as employment, educational attainment, and so forth -- factors that are likely to be included in explanatory empirical work on crime.

Causal Order Among Independent Variables

A second form of the general issue of causal proximity is that of causal ordering among independent variables. Causal ordering among independent variables is not seen as an issue if the goal is prediction, but is seen as an important issue in etiological works. A given empirical work on recidivism may not be able to maximize the two goals of prediction and explanation simultaneously. If the explicit goal of research is to maximize prediction, the explanatory component of the research is irrelevant. One simply finds the best predictors regardless of causal order among the predictors and, as discussed above, regardless of conceptual proximity between independent and dependent variables).

Researchers also attempt to find the set of best predictors which can satisfy some general policy framework. Prior criminal behavior has provided such a framework. Characteristics of prior criminal behavior have been defined as "fair game" for researchers with primarily predictive goals (such as incapacitation or delayed parole release) to use in making policy decisions for individuals. Thus, for example, in some research on recidivism, no

attempt will be made to determine all the causes of criminal behavior, if it can be predicted reasonably well with a set of good predictor variables. Often such a set of "good predictor" variables include variables such as the number of prior arrests or convictions, the age of onset of a criminal or delinquent career, or the seriousness of the crimes in a criminal career. Little concern is expressed over causal order issues among these variables or relative to other types of variables, such as employment, educational attainment, and so on. If behavior is reasonably well predicted, based on variables typically used by advocates of the criminal career perspective, it is considered adequate. The broader, and more ambitious goal of sorting out the relationship of criminal career characteristics with other type of characteristics, has yet to be developed, or has only been marginally pursued.

Following a similar line of reasoning, Hirschi and Gottfredson (1986) have argued against the criminal career perspective all together on the grounds that it is in essence prediction without explanation. They contend that the interesting research problem is explaining who becomes delinquent or criminal -- a process that occurs early in life. Using attributes of criminal careers to predict subsequent criminality confounds the ability of a given researcher's model to provide answers as to why the offender became delinquent in the first place. Thus, when Elliott and colleagues use an independent variable of prior delinquency to predict subsequent delinquency within a causal theory-testing approach, and do not also include prior social bond variables, Hirschi criticizes him for presenting a model which cannot fairly differentiate the relative explanatory power of social bond theory relative to the (implicit) alternative theory, a theory of constancy or stability over time.

In short, to test theories against one another, care must be exercised as to causal ordering and conceptual proximity of variables. In many policy applications such issues are not widely debated. Rather, prediction is the paramount goal and the two general causal proximity concerns discussed above (conceptual proximity and causal ordering among independent variables) are largely ignored. It is argued here that including variables in a regression equation on the grounds that they are predictive of crime is not an adequate reason for all analytic purposes -- nor for all policy aims. Our analyses in later chapters address some of the implications for conceptual proximity and causal ordering among independent variables in recidivism research.

Note that it is beyond the scope of the present research to resolve the disagreements that have been expressed in the literature on the value of the criminal career perspective within the context of the etiology of crime and delinquency (See Tittle, 1987; Hagan and Palloni, 1988). Rather, the prediction versus explanation issue manifests itself within specific contexts and goals of research on recidivism. We argue here that a reliance on "purely predictive" variables to the degree of ignoring other variables that do not lend themselves to, or cannot be used for, a policy decision for an individual is a mistake in research where the relative effect of variables on recidivism is the goal of the research. This has been stated by several others before us (Goldkamp, 1985; Sparks, 1981), but we reiterate it here.

The use of variables traditionally associated with the etiology literature for making individual decisions, may, or may not, be possible depending on the specific individual decisions to be made. To the extent that we are proposing that variables traditionally associated with the etiology literature be used to determine treatment, our argument is not new (the

criminal justice system does this both explicitly and implicitly all the time), nor is it new to argue that such decisions might be limited or structured by our knowledge of what predicts recidivism (see Gottfredson and Gottfredson, 1986). What we do hope to demonstrate here are some of the limitations and prospects for predicting and explaining criminal behavior with some possible implications for sentencing policy. Before proceeding, however, it is necessary to address another very practical issue, the length of time of follow-up.

Temporality in Explanation and Prediction

A central area of contention concerning both explanation and prediction is time. This pertains to both the time sequence of independent variables and the optimal length of time over which to study the dependent variable, in this case recidivism. Ideally, data would be available on all the factors thought relevant to crime over the entire time span of individuals' lives. Although such data would pose various practical problems for data analysis, we will ignore these issues since such data are not available here, though recent work by Earle and his colleagues at the Harvard School of Public Health will eventually yield such data. Rather, within the context of a predominantly cross-sectional study, the general problem is one of uncertainty as to when and how independent variables affect each other and crime. Reliance is euphemistically made "on theory".

Within the criminological literature, it is often suggested that offenders be studied over a period of three to five years (Sechrest et al., 1979), based in part on logical and in part on empirical observation. The logical argument is that it is unlikely that any form of intervention will have effects on the individual lasting longer than five years. The empirical

argument is that most offenders have recidivated within five years such that results of analysis obtained for a five-year follow-up will adequately reflect results that would be obtained if a longer follow-up period is analyzed. In the current research, the issue of whether or not there is a optimal logical and empirical timeframe is analyzed explicitly in later chapters.

More generally in the sociological literature, distinctions are drawn between variables such as gender, race and social class of parent, as these occur in time prior to other variables such as educational and occupational attainment. But even this often-made distinction is not entirely satisfactory, since the variables measuring gender, race, and social class may function differently at different points in the individual's life. Thus, such variables may have little impact on subsequent behavior until a child reaches a certain age: not all ages, e.g., one or two years old, are logically possible for social processes such as employment or delinquency. While gender, race and social class are "given" at birth, their meaning as variables may not be manifest until some later point in the individual's life. In contrast, other variables, such as having a steady job, or not having a job at all at a given point in time are sometimes treated as immediate in their impact on recidivism (e.g., not having a job is a "reason" for stealing). Other times they are taken as indicative of an underlying personality tendency (lack of self-control or lack of intelligence -- see Gottfredson and Hirschi, 1990) which displays constancy over time.

As such, the value of cross-sectional data analysis is limited by a lack of empirical data on when and how long variables have the impact often imputed in cross-sectional research. Longitudinal data, if available, could shed light on some of these relationships, as well as on how reasonable some of

these assumptions are in cross-sectional studies. In the current analysis, data are available primarily from one point in time for most variables. Although some more detailed time specific data are available as to the date of arrest, conviction, and imprisonment, for example, little use of such time-specific data will be made here, since time specific data on the majority of variables are not available. (Where time-specific aspects of a measure are relevant, they are mentioned in the text.)

What use of time specific data will be made concerns primarily the time frame over which measurement is made of the dependent variables of recidivism. That is, we will consider whether or not the effects of variables vary as a function of the length of follow-up time (from one to nine years, as will be described in subsequent chapters). Indirectly, then, we will be addressing the issue of the time-dependent nature of the relationship between independent and dependent variables.

SUMMARY

There are various general methodological issues that need be addressed toward the goal of making better use of recidivism information in the CJS, a system which we argued in Chapter One is geared toward utility considerations of crime control goals. In that the probability of desistence on the part of the offender (or less generally, rehabilitation, or specific deterrence) is a consideration in decision making, the concept of recidivism must be defined clearly at a conceptual and operational level. The same holds true for our more specific concerns with evaluating the effectiveness of sentences. Without a clear understanding of what is to be used for the purposes of evaluations, the utility of any empirical investigation becomes questionable.

The present chapter has served two broad purposes in clarifying what is to be studied and how it is to be studied. First, exactly what the dependent variable is has been discussed. Based on our reading of the literature, we highlighted three general approaches to the conceptualization of recidivism: binary, criminal career and failure rate conceptions of recidivism. Some general strengths and weaknesses of each have been discussed. In subsequent chapters the empirical linkages across these general types of recidivism measures will be assessed.

The second contribution of this chapter has been focussed primarily on concerns surrounding independent variables in the modeling of recidivism. Approaches to three broad methodological issues relevant to the modeling of recidivism have been discussed: aspects of the prediction versus explanation as goals of the analysis, conceptual proximity and causal ordering of independent variables, and issues of temporality in models of recidivism. Emphasis was given to distinctions among the logical properties of explanatory variables, and to the impact of omitting some types of variables from analyses of recidivism. As a result of these considerations, the subsequent chapters will focus on the general issues of "choice" not only in the selection of specific measures of recidivism, but of "choice" among types of independent variables as well. Furthermore, the timeframe of follow-up will also be examined in detail to ascertain what difference follow-up periods of varying lengths make to substantive recidivism prediction.

In sum, it is clear that no consensus exists for either side of the recidivism "equation." A wide array of measures have been used as the dependent variable and considerable debate surrounds which independent variables can be used, the logical ordering among independent variables, and

the different purposes served by those variables. While these issues have been raised in the context of program evaluation, risk assessment, and general recidivism research, the apply equally well to our more narrow focus on the effects of sentences on recidivism.

Table 2.1

Variables Used In Guidelines or Classification Instruments
(From Petersilia and Turner, 1987:158)

	Sentencing	Community Supervision	Parole Release
Criminal Record:			
No. of parole/probation revocations	c	a	a
No. of adult and juvenile arrests		b	c
Age at first arrest			
Nature of arrest crimes			
No. of adult and juvenile convictions	a	b	a
Age at first conviction		a	c
Nature of prior convictions	b	c	b
Repeat of conviction types	c		c
No. of previous felony sentences			b
No. of previous probation sentences		b	
No. of juvenile incarcerations			b
No. of jail terms served			b
No. of prison terms served			a
No. of incarcerations served	b		a
Age at first incarceration			b
Length of current term			c
Total years incarcerated			c
Commitment free period evidenced	c	b	b
On probation/parole at arrest	b	b	b
Nature of current crime:			
Multiple conviction crimes	b		
Involves violence	a		a
Is property crime			c
Weapon involved	c		c
Victim injured	a		b
Victim/offender forcible contact			c
Social factors:			
Current age			b
Educational level		c	c
Employment history		b	c
Mental health status		c	
Family relationships		c	
Living arrangements		c	c

a = 75 percent of instruments studied used this factor.
b = 50-74 percent of instruments studied used this factor.
c = 25-49 percent of instruments studied used this factor.

(continued)

Table 2.1 (continued)

Variables Used In Guidelines or Classification Instruments
(From Petersilia and Turner, 1987:158)

	Sentencing	Community Supervision	Parole Release
Social factors (continued):			
Drug use		a	b
Juvenile use/abuse		b	
Alcohol use/abuse		b	c
Companions		c	
Address changes last year	b		
Attitude	b		
Financial status	c		
Prison behavior:			
Infractions			b
Program participation			c
Release plan formulated			c
Escape history			c

a = 75 percent of instruments studied used this factor.
b = 50-74 percent of instruments studied used this factor.
c = 25-49 percent of instruments studied used this factor.

CHAPTER THREE

THE EFFECTS OF SENTENCES DATA BASE

In a recent summary of the state of decision making in the criminal justice system, Michael and Don Gottfredson (1988:262) express concern over the lack of feedback from data bases.

> At every decision point in the process, decision makers lack the feedback required in the making of decisions that are consistent and rational. Two critical forms of feedback are especially noteworthy by their nearly universal absence. The first is information about how colleague decision makers (and the individual decision makers themselves) have decided similar cases in the past. . .
>
> The second form of feedback routinely lacking is knowledge concerning the consequences of a decision choice. Given the goal of the decision in an individual case, was it achieved? (Gottfredson and Gottfredson, 1988:262-263)

The "acceptable risk" inherent in the sentencing of an offender falls under these concerns. At the time of sentencing judges, and other criminal justice actors, are often faced with incomplete information. Knowledge of the intricacies of prior sanctions, and the (potentially incompatible) goals behind those sanctions, may be unavailable. Unless a repeat offender reappears before the same judge, the outcome of the prior sentencing decision, especially in terms of recidivistic behavior, remains unknown to the judge. Thus the assessment of the offender's level of risk is often made in a partial vacuum of information, and the evaluation of that assessment is seldom returned to its maker.

One of the areas targeted by the Gottfredsons is the writing of a presentence investigation report, since this narrative may influence several subsequent decisions that are made about an offender. They argue that it imperative to improve not only the information gathering process, but the

quality of the data as well: information is needed on the information.

> A core set of the same data for each person, collected with attention to reliability concerns, is needed. Such data then can be examined to determine the relevance of individual items, or combinations of them, to a wide array of significant decision problems. . . Without such careful, systematic data collection, the probation or parole administrator is in the familiar correctional situation -- much data, collected unsystematically, variably, and subjectively for individual case studies, but no information demonstrably relevant to either program or individual decisions (Gottfredson and Gottfredson, 1988:185).

The availability of quality data, according to the Gottfredsons, is a prerequisite to any rational decision making in the criminal justice system. This is as true for the judge at the time of sentencing as it is for the parole and probation officers cited by the Gottfredsons. The absence of certain kinds of information for some offenders, the lack of knowledge about which items are most relevant to assessing offender risk, and the possibility for unreliable data from various sources can all impede the judge's decision making.

In this chapter we describe the data base constructed to assess the effects of sentences on subsequent criminal behavior. At the core of these data is the presentence investigation report, reflecting the information at hand at the time the offender is sentenced. Added to this source are official arrest and incarceration data, providing further information about pre-sentence criminal behavior that may not have been available on the presentence investigation report. It is from these latter data that measures of recidivism are constructed. After a description of the nature of the information compiled, we discuss the creation of the sample to be analyzed, focussing on the composition of this sample in terms of the characteristics of the offenders.

DATA SOURCES

The present study employs data taken from three sources of official records. The basic sample is defined by court cases collected by the State of New Jersey Administrative Office of the Courts in their 1976 Sentencing Guidelines Project. These data have been supplemented with official arrest histories maintained by the New Jersey State Police. Incarceration histories, supplied by the New Jersey Department of Corrections, have been merged with the arrest records for those individuals known to have been imprisoned at some point in their criminal career. Each source of data, and the information taken from them, is described below.

The Sentencing Guidelines Project.

The New Jersey Administrative Office of the Courts Sentencing Guidelines Project (McCarthy, 1979) was designed to assess sentencing in state courts. Motivated by a widespread concern about sentencing disparity in New Jersey, and in the nation more generally, policy makers and judges obtained information about actual practices as evidenced in the cases heard at that time. This information was then used to develop discretionary sentencing guidelines.

The time frame for the Guidelines Project was the one year period from October 1976 to September 1977. Consistent with the focus on the range of sentences given for various offenses, several scope conditions were placed on the state court cases sampled during this time frame. The primary restriction was that the case had to have resulted in a conviction on at least one charge: cases in which all charges were acquitted or dismissed obviously did not result in a sentence and thus were not appropriate for use in the development

of sentencing guidelines.[1] An additional restriction pertained to the nature of the offense(s) charged. If all charges were for a nonindictable offense, the case was not included in the Guidelines Project sample, even if a conviction resulted on one of the charges. Similarly, cases where the offender was charged with an indictable offense, but was convicted of a downgraded, nonindictable offense, did not fall under the scope of the Guidelines Project sample.

The last scope condition for the sample was that the appearance in court had to result in a "new" sentence. Cases in which the offender was in court on matters having to do with sentences given prior to the sampling frame were not automatically included in the Guidelines Project. Specifically, cases where the offender violated probation or parole, or was in court for resentencing, but had not been arrested for a new offense, were dropped from the sample. If, however, the offender had been arrested (and convicted) for another crime, and this led to a violation of probation or parole, the case was appropriate for the sample as a new sentence was given for the new conviction.[2] In all, 14,321 state court cases met these scope conditions.

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[1] A major consequence of this scope condition is that the present study has no information on offenders who were charged, but not convicted on any charge. Thus there is no comparison group of offenders who were not sentenced during the sampling period and the sample available represents a biased subset of all offenders appearing before a state court. This is not necessarily a problem if one wishes to evaluate the impact of the sentence in terms of any subsequent recidivism. For the more general concerns of recidivism after the intervention of the criminal justice system, even in instances where the offender goes to court but is not convicted, the Guidelines Project sample is less appropriate. The procedures outlined in Appendix A attempt to compensate for the differences between a sample of convicted offenders and a sample of offenders appearing before a state court.

[2] As this scope condition illustrates, an offender in the Guidelines Project sample may have already recidivated on an earlier sentence. Furthermore, the sentence administered in 1976-77 may not be independent of sentences given prior to the sampling period. These issues are addressed in Chapter Seven.

Note that the sampling unit for the Guidelines Project was the case, not the individual offender. As a result, a single crime, committed by multiple offenders, could be represented more than once in the sample. More importantly, one individual could legitimately appear in the sample multiple times if s/he appeared before a state court for different crimes during the one year sampling period. Of the 14,321 cases in the sample, at least 921 (6.4%) are for offenders who made two, and in some instances three, appearances for distinct crimes during 1976-77. The existence of duplicate and triplicate appearances in the Guidelines Project necessitates special treatment for there is only one arrest history and one incarceration history for these individuals. Moreover, these cases can lead to conceptual difficulties as described later in this chapter.

The main source of data for the Guidelines Project was the presentence investigation report (PSI) for each case. Under New Jersey law, a presentence investigation report must be prepared for every conviction on an indictment. New Jersey law also mandates that a judge consider only the factual information contained in the PSI when deciding cases. Thus, PSIs are central to the sentencing process and contain the all the information that can legally be used in deciding upon the sentence to be administered. The PSI for all cases falling under the scope conditions for the sample were mailed to the New Jersey Administrative Office of the Courts in Trenton. Follow-up contacts were regularly made with jurisdictions to ensure that data were obtained for all cases sentenced during the sampling frame.

Data were extracted from the comprehensive PSIs using a coding sheet constructed in consultation with judges, other criminal justice personnel and criminal justice researchers. Care was taken to retain as much information as

possible in the development of the coding sheets. The actual coding was done by law students who were given extensive training prior to the beginning of the actual data collection effort. It was often the case that coders had to make inferences on how to categorize the particulars of a given case. Inter-coder differences were minimized both by the initial training and by having coders work only within a particular type of offense (e.g, robbery, drugs), thus ensuring a greater familiarity with the legal aspects of the case to be coded. In addition, different coders were sometimes given the same case to evaluate more than once. Analyses of these cases suggested that inter-coder reliability was quite high for the Guidelines Project.[3]

Mirroring the extensive nature of the PSI, 826 variables covering many domains were coded. Characteristics of the offense included the number of victims, their age, race, and sex (when known), and any victim injury. For property crimes, drug crimes, crimes involving fraud, sex crimes, and gambling offenses, a series of items were used to capture the particulars of the crime(s) charged. The processing of the case through the Criminal Justice system was captured by the offender's plea, 19 indicators of prosecutor recommendations, and the dates of events such as when the indictment was handed down, when the initial and final pleas were entered and when any trial occurred.

All of the measures of the social aspects of the offender and his/her criminal career are also taken from information coded off the PSIs. These variables include evidence of any drug or alcohol problems, family history, employment history, including any employment at the time of the presenting

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[3] McCarthy (1979:13) reports that the data reliability coefficients "were well above the .80 cutoff, and the average was approximately .93."

offense, community background, educational and occupational status, military service, and physical and mental health. Also collected were information on the offender's parents and siblings, including any involvement in crime. The kinds of information available is extensive and multiple indicators exist for many of these factors. Consequently, we often make use of composite scales in the ensuing analyses.

Prior record, as taken from official "rap sheets," was also coded during the Sentencing Guidelines Project. The so-called "centerfold," (named for the format in which the data were recorded), allowed for up to 18 prior arrests. For each arrest, the year it occurred, whether the offender was a juvenile or an adult at the time, where the arrest occurred (in the same county as the presenting crime, elsewhere in New Jersey, or out-of-state), a broad measure of the severity of the offense, how many counts were charged at the arrest, a subjective indicator of how similar the crime was to the presenting offense, and whether or not the crime was a violent offense was collected. Also coded was a detailed indicator of the final disposition of the arrest.

Juvenile arrests are not routinely part of the official adult arrest histories that are described in the next section. However, juvenile records were not sealed in New Jersey at the time of the Guidelines Project, and thus information about involvement in crime as a juvenile were available to the courts. The centerfold arrest histories provide the best available measure of not only juvenile arrests, but also custodial sentences resulting from those arrests. These data become particularly important given the fact that approximately two thirds of the Guidelines Project sample were aged 27 or younger at the time of sentencing. As a consequence, the use of adult arrests only could impose severe left censoring on any measure of prior criminal

activity. All indicators of juvenile arrests used in the present analyses come from data provided by the Guidelines Project.

Judgement of Conviction sheets were the source of information for the sentence given to each case of the Guidelines Project. The disposition of each charge, and the sentence given for all charges yielding convictions, was coded from these records.[4] This information allowed for the determination of the most serious offense for which the offender was convicted as well as any convictions on lesser charges. From the data pertaining to the sentence given, it is possible to construct basic measures of the sentence such as whether the offender was sentenced to incarceration and if so, where, and how much time the individual was to serve either in custody or on probation. These measures are described in detail in Chapter Seven.

The Guidelines Project is also notable for several subjective measures of the case and the offender. Coders were asked to rate the severity of the charged offense (relative to offenses of that type), the "badness" of the offender, the offender's prior record, and the prognosis for probation (likelihood of rehabilitation versus recidivism), all using a 100 point scale. As well, a summary measure of the presentence report writer's prognosis for

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[4] The coding form allowed for compiling information on charges and dispositions of up to three different offenses as defined by the New Jersey Criminal Code at that time. Supplementary forms were created for cases where more than three different offenses were charged. When the supplementary forms were needed, the main form contained data about charges resulting in convictions or, if fewer than three convictions, the charges about the "most serious" remaining offenses, and information about the less serious offenses were put on the supplementary form. We use data supplied on only the main forms in all of the present analyses. This results in some loss of information about lesser crimes when the cases involved four or more different offenses. About 35% of the cases in the Guidelines Project were charged under only one statute and 34% were charged under two statutes. The remaining 31% were charged with at least three different crimes, but there is no way to ascertain what proportion of these were charged under more than three distinct statutes.

the offender was collected. While these measures are indirect, they do provide some indication of the impression of the offender by criminal justice agents and a global assessment of the risk of recidivism at the time of sentencing. Thus, the subjective indicators can be used in conjunction with the more objective measures available to the judge at the time of sentencing.

It is important to note that all of the data amassed by the Sentencing Guidelines Project was taken from officially reported information. At times, this information was incomplete, either because a shorter form was used for the Presentence Investigation Report or because of poor record keeping on the part of the criminal justice system. Not surprisingly, missing information is greatest on data pertaining to the social characteristics of the offender (e.g., parental socioeconomic status) or the social aspects of the offense (e.g., race or sex of the victim, victim's social class). Other times, data is effectively missing as the variables are inappropriate (e.g., characteristics of fraud cases, when the case is not a fraud or the extent of victim injury for victimless crimes.)

We view the presence or absence of information in the Guidelines Project data set as indicative of what was available to the judge at that time of sentencing. That is, the sentence that was administered was based on the information available in the data set and, more importantly, the assessment of risk for recidivism can be made on the basis of what was known from these official data sources alone. This leads us to treat missing data on many variables as reflecting the fact that the data was not known to the criminal justice system at the time and thus was not used in any decision making. For example, if variables covering offender drug use and history of drug treatment are missing, we treat this as if the offender had no history of drug use.

Similarly, if information about whether or not the offender lived with parents is missing, this is considered equivalent to the offender not living with parents.[5]

In summary, the Sentencing Guidelines data set provides an extensive array of variables to be used in the modeling of recidivism and assessing how the sentence administered influences any subsequent criminal behavior. It was not, however, the purpose of that project to evaluate the sentence or study recidivism and, consequently, there are no indicators of arrests after the project was completed in 1976-77. Our source of these data is described in the next section.

Arrest Histories.

Indicators of both pre- and post-sentence arrests come from the Offender Based Transaction Statistics/Computerized Criminal History maintained by the New Jersey State Police, Department of Systems and Communications (SAC). Information is collected about adult offenders for all stages of the criminal justice process once fingerprinting positively establishes the identity of the offender. The SAC data system was established in January, 1972, approximately three and one half years prior to the start of the Sentencing Guidelines Project. However, the arrest histories were backdated with known arrests prior to that time and for the offenders in the Guidelines project, SAC has arrests as early as the late 1930s. Within reporting constraints (see below), efforts are made to ensure the quality of the information in the data base.

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[5] Obviously, this strategy for dealing with missing or incomplete information fails in some instances. For the few cases where race and sex of the offender is coded as missing, for example, criminal justice agents clearly know the values on these variables. In general, however, equating missing data with the absence of information on the PSI allows us to model risk of recidivism using only what information is known at the time of sentencing.

All data entered are verified to check for accuracy, and periodic audits of identification, arrest, and conviction records are made. Overall, the system provides a comprehensive record of arrests and convictions with particularly accurate records dating from 1972 and is comparable to the best of state arrest information.

SAC data are organized by cycles keyed to the date of an arrest. For each date of arrest, charges are listed, followed by any charges brought to a municipal or state court. For those charges resulting in a conviction and a sentence to incarceration, dates of entry into and release from jail or prison are noted. Parole information is also part of the data provided for offenders who have served time in state prisons. If the conviction resulted in a sentence of probation, the date of the beginning and end of the probation term are indicated, as well as the reason for the termination of probation. Transfers of supervision of probation and between institutions are also contained in the arrest-court-custody cycles. The SAC data base thus contains detailed, sequential information for each cycle of arrest, adjudication, and, sometimes, custody.

The system is dependent upon the reporting of arrests and court appearances from New Jersey's counties and the state police. Custody and supervision information is supplied by the New Jersey Department of Corrections and probation officers around the state. Unfortunately, reporting from these sources is uneven. Arrests by state police officers and other state agencies are most likely to be found in the SAC file. However, arrests occurring in large, urban portions of the state are less likely to be recorded in SAC, especially if the charge is for a minor offense. As a case works its way through the criminal justice system to court appearances and possible

custody, some additional deterioration due to lack of reporting is possible. Furthermore, New Jersey enacted a "speedy-trial" statute in 1981. Our analysis of the volume of arrests in the SAC file found a small dip in reported arrests and court appearances for a one year period, most likely a result of the increased workload resulting from the statute.

Supervisory and custodial information in SAC is also dependent upon reporting by state agencies and here too, the information is, at times, uneven. Probation records were sometimes missing termination dates. More problematic were custody records for jail sentences.[6] Entry dates were often missing the corresponding release dates. This necessitated estimating the date of release given both the date of entry and the charge for which the offender was convicted. The steps taken create these records, as well as details about the processing of all SAC data, can be found in Smith and Smith (1990). Missing information about custody in prison and parole supervision is of less concern due to another source of this information. This is described in the next section.

The official arrest histories represented by the SAC data system are the source of much of the information about the criminal careers of the offenders in the Sentencing Guidelines Project. The SAC data are used to supplement the prior arrest and incarceration records captured by the project's centerfold. These data also provide detailed counts of the types of crimes for which the

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[6] Our experience in using the SAC data system suggests that information about time spent in jail is confined primarily to the serving of sentences to jail terms. Some time spent in jail while awaiting transfer to a state prison is also reported to SAC. However, short stays while waiting for bail are very unlikely to appear in the SAC data. This was confirmed by a search of the records in two of New Jersey's county penitentiaries. The movement into, and out of, these institutions was much greater than indicated in the SAC records. Consequently, when the SAC data are used to estimate time spent in jail, underestimates are likely to result.

individual was arrested or convicted prior to entry into the Guidelines Project sample. The SAC data are the primary source of information about time spent on probation or in jail prior to the focal sentence of the Guidelines Project. For some individuals in the project sample, the SAC histories are the only source of information about prior prison and parole times.

The SAC arrest histories are our sole source of known criminal behavior subsequent to the 1976-77 sentencing. All measures of recidivism involving any rearrest or reconviction, rejailing, or new sentences to probation (see Chapter Four) are derived from the information taken from the SAC files. For some offenders, reimprisonment measures are also computed from these arrest histories. When pre- and post-sentence arrest rates are estimated and adjusted for time at risk, the numerators are constructed from counts of the SAC files and the days in custody used in the denominator are, in part, the result of entry and release dates known to SAC.

Incarceration Histories.

While the SAC data base provides the best available information on adult arrests since the 1976-77 sentence, it is not the most accurate source of information on incarceration dates. The State of New Jersey Department of Corrections (DOC) maintains the Offender Based State Correctional Information System. The system keeps computerized records of incarcerations, escapes, transfers, releases, paroles, and furloughs for all of the state's prisons and the largest youth correctional facility. Established in 1974, the DOC system followed a backdating procedure similar to that used by SAC, and thus DOC incarceration data is available starting in the mid 1960s.

Admission data is collected from the time an offender is committed to one of the 13 institutions in the Department. The information contained in

the DOC system is extensive, as any movement is noted. For example, transfers between facilities are represented by several records, even though the end result is no change in the custodial status of the prisoner. Similarly, a short release for a court appearance results in several records, even though the time away from the prison was less than 24 hours. In the interest of parsimony, only those actions which resulted in a change in custodial or supervisory status were retained from the DOC file.

The DOC system is our best source of information on the timing of imprisonment and release and is thus central to estimates of time at risk. When possible, we use DOC data to estimate time spent in prison, both prior to, and after, the 1976-77 sentence. When DOC receives an inmate from a jail, the date of intake into the jail is noted. Therefore some DOC records were used to supplement the jail information available in the SAC system. Department of Corrections files also supply dates for release to parole and termination of parole and these were used to estimate time under parole supervision.

While we use the DOC file primarily for estimates of time at risk throughout the offender's career, some information from this source is also used in conjunction with the 1976-77 sentence. For those cases resulting in a prison incarceration, DOC data are used to follow the inmate while serving that sentence. Given the differences in the institutions of the correctional system (e.g., orientation towards rehabilitation, minimum versus maximum security), an indirect measure of the nature of the treatment received during the sentence can be computed.

Taken as a set, the three sources of data provide detailed information on the nature and timing of events in the criminal career. Through the

triangulation of the information in each of these files, a picture of these careers, as seen by criminal justice agencies, may be constructed. While a considerable amount of data processing was necessary to merge the information from these sources (see Smith and Smith, 1990), the result is a data set the reflects the current state of criminal justice information. More importantly, the data set is particularly appropriate for the study of recidivism and the evaluation of the 1976-77 sentence as the computerized record keeping was firmly established at that time.

CODING OFFENSES

Each of the three data sources had its own method of coding the crimes for which the individual was arrested, charged, convicted, or incarcerated. The Guidelines Project used a three-digit code derived from the New Jersey criminal code at that time. The SAC system relied upon the 4-digit Uniform Offense Code, while DOC used a six character alphanumeric indicator of the offense. In order to merge the three files, a common scheme was devised which, by necessity, had to be quite broad. Specificity in measuring the offense had to be balanced against differences in interpreting the offense and the lack of correspondence across the schemes used in the three data sources. For example, the act of "destroying boundary marks" (NJ Statute 2A-122-4) could reasonably be indicated by at least four of the offense codes used in the Uniform Offense Classification. To maintain this level of detail in coding offenses would lead to considerable error in any omnibus scheme applied to the Guidelines Project, SAC arrest histories, and DOC incarceration histories simultaneously.

Our solution involved a 22 category system that is based on the general categories of the Uniform Offense Classifications. Of the major 43 headings in the Uniform Offense scheme, some are for nonindictable crimes (e.g., traffic offenses) and thus were inappropriate given the sampling criteria for the Guidelines Project. Other classes of crimes (e.g., obstructing the police, obstructing the judiciary, smuggling, sovereignty crimes) are found infrequently, if at all, in the cases that appeared before a New Jersey state court in 1976-77. These kinds of offenses were placed in the residual "other" category under our compromise classification.

The remaining offense groups that were developed maintain necessary distinctions between types of crimes, while allowing a comparison of offenses across the three sources of data. Serious crimes against persons are captured by codes for homicide, kidnapping, sexual assault, robbery, and assault. The crimes of arson, burglary, larceny, stolen vehicle, stolen property, and damage property cover a broad range of property crimes. Additional codes were used for offenses of extortion, forgery, fraud, embezzlement, drugs, minor sex offenses, gambling, escape, bribery, and weapons. While the convictions in the Guidelines Project are not uniformly spread across the 22 categories used, (see the discussion of Table 3.2 below), these offense codes are sufficiently sensitive to capture substantive differences in types of crimes.

The present classification scheme takes on added importance in that it underlies all measures of "type of offense" used in our analyses. All charges in the original Guidelines Project have been converted into one of the 22 categories, as have the offenses for which the offender was convicted. Use of the SAC arrest histories to measure pre-sentence criminal record and post-sentence recidivism is done with reference to charges recoded into these

categories. Measures of the initial charge and first conviction of the offender's career, and indicators of the first rearrest and reconviction, if any, after the 1976-77 sentence, are derived from crimes coded into one of the 22 categories. When summary measures of the severity of the prior record and subsequent recidivism are computed, (following Wolfgang et al., 1985), offense weights are assigned on the basis of these categories. In short, whenever the interest lies in what crimes the offender has committed, offense type is measured in terms of the devised coding scheme.[7]

Arrests versus Charges.

A more general issue in coding offenses is how to use information from arrest histories to measure criminal behavior. The debate has historically centered around how to measure prior record so it can be used in conjunction with other measures when sentencing an offender or when considering an inmate for parole. (See, for example, Tonry, 1987). Some contend that arrest information is the more appropriate measure of prior record. Others argue that because of the potential for harassment through arrests (with the charges subsequently being dropped), coupled with the potential for discrimination in the kinds of offenders that are arrested, the use of prior convictions yields a measure that is superior to one based upon arrests.

The current data do not allow us to address this debate, and we thus compute measures of prior record using both arrests and convictions (Botsko,

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[7] Clearly, considerable variability in the exact nature of the crime remains when a coding scheme as broad as the present one is employed. As a consequence, some important distinctions get lost. For example, joyriding and stripping stolen vehicles are coded as the same general offense of "stolen vehicle." Similarly, dollar amounts of damage, fraud, and theft are ignored and thus theft of $10 is considered equivalent to a theft of $1,000. Maintaining such distinctions, however, results in a number of categories that is computationally unwieldy when the full spectrum of criminal behavior is studied.

et al., 1990; Smith and Smith, 1990). Note however, that the same concerns can be raised regarding the use of official arrest/conviction records in the measurement of recidivism. A reliance upon arrests alone may tend to overestimate the criminal behavior (as known to the authorities) of the individual, while sole use of conviction based measures may tend to underestimate criminal behavior. As described in Chapter Four, recidivism measures can be based upon either arrests or convictions.

Less common in the literature is a consideration of the differences between arrests and charges when official records are used to measure criminal behavior. Research on criminal careers, for example, has almost exclusively used arrests as the basis for computing an individual's offense rate (e.g., Panel on Research on Criminal Careers, 1986; Blumstein and Cohen, 1979). There is indeed good reason for doing so. Just as arrests may tend to overestimate the extent of an individual's illegal behavior when compared to convictions, charges may also yield high estimates when compared to arrests.

At any given arrest, more than one charge can be brought against the individual. The major question is whether multiple charges represent different acts. When one act yields multiple charges, as when a weapons charge is added to a robbery charge, or a larceny charge is added to one of burglary, measuring criminal behavior through charges will overestimate the actual behavior.[8] Similarly, the act of "docket clearing" through falsely charging an individual with crimes s/he did not commit will lead to charges overestimating the true criminal behavior. Conversely, offenders are often

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[8] The changes in the New Jersey criminal code, discussed below, were designed to eliminate "tacking on" charges to a single criminal act by limiting the kinds of offenses that could be charged. As in other states, the evidence suggests that prosecutors circumvented the limitation through the use of different charges that were permissible under the new criminal code.

charged with multiple offenses on a single arrest date for acts committed over a period of time. This is often seen in charges for burglary, where the multiple charges represent allegations for crimes committed at different times. In such instances, the use of charges, rather than arrests, yields a better indicator of (alleged) criminal behavior.

One solution is to retain only one of the charges when there are multiple charges on a given date of arrest. This is most often done through the creation of some hierarchy of seriousness, and only the most serious of the charges is then used to measure illegal behavior from that arrest. From the perspective of measuring recidivism, this approach creates three problems. First, there is no single, agreed upon hierarchy of seriousness that can be used to order multiple charges, and thus conclusion may become dependent upon the seriousness scale chosen. Second, if the measure of recidivism is a repetition of the crime for which the offender was originally sentenced, repeat offending could be missed if it occurs in conjunction with a charge that is deemed more serious. Finally, multiple charges for the same offense (e.g., burglary) can be indicative of levels of offending that would be underestimated if only one of those charges were used.

Note that exactly the same issues are raised if convictions, rather than charges, are to be used. Multiple charges on a single date of arrest, leading to multiple convictions on a single court date, can be treated either through a hierarchy of most serious conviction or through retaining all charges leading to a conviction.[9] While use of convictions will avoid the criticisms

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[9] It will be remembered that in the SAC arrest histories court appearances are tied to the date of arrest. When two different arrest dates lead to a single court date, we are able to distinguish between convictions on the basis of date of arrest. However, when multiple charges on a single arrest date lead to multiple convictions at one court appearance, either a hierarchy of offenses must

raised against arrest based measures of criminal behavior, simply taking the most serious charge the offender is convicted of can underestimate illegal behavior, while retaining all charges may overestimate that behavior.

It is likely that substantive conclusions may be sensitive to how prior record and subsequent recidivism are operationalized through the use of official arrest histories. In particular, how likely an individual is to be at risk for recidivism at the time of sentencing may be a function of the kinds of crimes he or she has committed, or been convicted of, in the past. Prediction of the types of offenses for which the individual is rearrested, or reconvicted, may be facilitated by the extent to which offenders specialize in types of crimes. This argues for retaining as much information as possible from the arrest and conviction histories and thus deriving measures based upon charges rather than arrests. To investigate whether this hypothesis is indeed true, we will compute prior record and recidivism measures based upon both unique arrests/convictions and charges/convictions for use in the analyses.

MATCHING CASES ACROSS DATA SOURCES

No single identification number linked the cases across the three data sources and thus it was necessary to match cases indirectly. Exactly how this was done is detailed in Smith and Smith (1990). Here, we briefly summarize the matching process.

The first step in matching cases was to find official arrest records for the cases in the Guidelines Project. This was achieved by using common identifiers such as name, alias (if any), sex, race, date of birth, date of arrest and charge, date of sentence and charge, and by comparing prior record

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be employed or all convictions must be retained.

information between the Guideline's centerfold and the SAC arrest histories. This provided an initial list of potential matches between the cases in the Guidelines Project and the SAC arrest histories. County probation and court records were searched for those cases unmatched by computer software programs and added to the list of matched cases when found.

When exact matches could not be obtained on all the identifying variables, slight differences were allowed. For example, when date of birth and county of arrest coincided, a match was considered acceptable even if the names differed by several characters. In general, the procedures erred toward dropping cases paired between the Guidelines Project and SAC rather that keeping questionable matches. Despite the conservative definitions used, several matches were dropped later in the data processing when discrepancies were found. Ultimately, 12,231 of the 14,329 cases (86.0%) in the Guidelines data base were identified in the SAC master list.[10]

Attempts were made to match cases to the DOC incarceration histories only when the individual had already been matched to the SAC arrest histories. For this step, the primary identifiers of name, alias, sex and date of birth were initially used. Some matches which were initially thought to be correct were later dropped when major inconsistencies were found between the events in the DOC file and information known from the Guidelines Project data and the SAC file. DOC records were found and retained for 35.5% of the 12,231 cases matched to SAC.

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[10] Note that a complete match of cases between these sources is not to be expected. SAC creates records for an individual once the identity is established through fingerprinting. It is possible that some of the individuals sentenced in 1976-77 were not entered into the SAC system because of this. In addition, official records may be purged as part of plea bargaining, and therefore some arrest histories may have been erased prior to our receiving the files.

The success in matching incarceration histories is difficult to evaluate, since an individual would appear in the DOC data base only if he or she had been incarcerated after the advent of the computerized record keeping. Offenders known (from the Guidelines centerfold or information in SAC) to have been incarcerated prior to that time would not have records in the DOC system. Of the base of 12,231, the centerfold data identified 807 individuals (6.5%) who had been incarcerated prior to the 1976-77, but we were unable to find in the DOC files. (Some of these may have been incarcerations not under the auspices of the New Jersey Department of Corrections.) For 266 cases (2.2%), the sentence administered in 1976-77 was incarceration to a state facility, though we were not able to find the individual's records in the DOC file.[11]

The failure to match cases across the three data sources has important consequences. Foremost is the fact that no measures of recidivism are available for those cases in the Guidelines Project (14.0%) that were not matched to SAC arrest histories. For these individuals we are unable to evaluate the sentence administered and they must be dropped from the analysis. A more subtle consequence of not matching all of the Guidelines Project cases to the SAC file is that those cases that were matched represent a nonrandom subset of the original Guidelines Project sample. We were more successful in matching offenders with longer prior records, those charged with certain kinds of offenses, and those sentenced in certain counties. Indeed, if the 2008 unmatched cases are compared to the 12,231 matched cases on almost any variable, significant differences are likely to be found.

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[11] The failure to match cases to the DOC incarceration file does not necessarily mean that dates of incarceration and release are not available for those individuals. The SAC system maintains this information as well. For a small percentage of cases sentenced to prison, however, it was necessary to create imprisonment records. See Smith and Smith (1990).

Any existing bivariate relationship between being matched or not and some other variable may be spurious when other variables are controlled. To investigate this possibility, the outcome of the matching process was modelled using independent variables taken from the Guidelines Project data set. Variables were chosen either to reflect the characteristics used in the matching process, or to suggest the sentence that might be given based on the recommendations of the prosecutor. (The actual sentence administered was not used as an independent variable because this would make the model of the matching process inappropriate if the dependent variable were the sentence itself.) The final model, removing variables with coefficients that were not significant, is summarized in Table 3.1.

The dependent variable for this model is a dummy variable coded "1" if the case in the Guidelines Project was matched to the SAC arrest histories, and "0" otherwise. The charge for which the individual was convicted is significantly related to the likelihood that the case was matched to an arrest history, with those convicted of burglary and drug crimes more likely to be matched and those convicted of fraud, bribery, and the residual category of "other," significantly less likely to be matched.[12] Those convicted for more than one kind of crime were more likely to be matched then those charged with simply one type of offense.

The county in which the case was heard is also strongly related to the probability of identifying the individual in the SAC file. Those sentenced in Atlantic, Burlington, Cumberland, Salem, Sussex, or Union county were

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[12] A bivariate crosstabulation between the most serious charge for which the offender is convicted and whether or not the case was matched finds significant effects for all types of charges. The probit model in Table 3.1 indicates that, for many types of charges, that effect is spurious.

significantly more likely to be matched, while cases from either Essex or Mercer county were significantly less likely to be matched.[13] The probability of being matched was significantly increased if the offender was black and significantly decreased if he or she was hispanic. Males and females were equally likely to be matched to arrest histories when the other independent variables were controlled.[14] Older offenders were less likely to be matched, while the greater the use of alcohol (a four point ordinal scale) the greater the probability that arrest histories were obtained for the individual. Those matched were also marginally more likely to live with family members.

Prior record, as measured by the centerfold arrest histories, is also strongly related to the probability that the case was matched to the SAC master list. Each previous arrest decreases the probability that the case was matched, while each prior conviction increases that probability. The more often the individual had been incarcerated, the less likely a match was effected between the Guidelines Project case and the SAC file. How the prosecutor felt about the disposition of the case prior to sentencing is also related to the likelihood of being matched. Of the nineteen recommendation variables available, four proved significant in the final probit model. Cases where the prosecutor recommended dismissal, probation, or a place of imprisonment were more likely to be matched. Those where the recommendation

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[13] The effect of county here is due to several factors. As we argue in Appendix A, the county indicators may be tapping record keeping practices. Our field staff had varying success in finding additional names from the probation departments of the counties, and this too influenced the probability that the automated programs could be used to start the matching process.

[14] Sex was retained in the model to be consistent with the analyses of Appendix A.

was restitution were significantly less likely to be matched.

As the pseudo R-squared measure (Aldrich and Nelson, 1984) indicates, the overall success in modelling the matching process is limited. About 7.7% of the differences between the matched and unmatched subgroups can be accounted for by the variables in the model. However, these results do establish that the matched cases represent a nonrandom subset of those sentenced before a New Jersey state court during the 1976-77 sampling period. Furthermore, the loss of cases through a failure to match with the SAC arrest histories is a function of variables from several domains. Convictions for certain types of offenses, the county in which the case was heard, characteristics of the offender and his/her prior record, and prosecutor recommendations are all significantly related to the probability that a case was matched across data sources. As we argue below, the nonrandom nature of the subset available for evaluating the sentence and any subsequent recidivism must be addressed in the analysis.

DUPLICATE COURT APPEARANCES

The sampling unit for the original Sentencing Guidelines Project was the court case. Individuals who were sentenced more than once during the sampling period, for different crimes occurring at different points in time, could thus appear multiple times in the Guidelines data base. Unfortunately, these duplicated individuals pose problems, both for assessing the effects of the sentence upon recidivism and for the assumptions underlying the statistical techniques employed in our analyses.

It was possible to determine if an individual contributed to more than one case in the Guidelines data base only after cases were matched to the SAC

arrest histories. At that point, the use of name, sex, race, date of birth, and prior record allowed for the identification of offenders who appeared multiple times during the sampling period. Nine hundred and twenty one cases in the Guidelines data base (6.4% of the total sample, 7.5% of the matched subsample) were found to have duplicate court appearances. Of these, 439 offenders appeared twice in the sample, thus contributing a total of 878 cases to the original data base, and 21 individuals appeared three times and were responsible for 63 of the cases in the Guidelines sample. The arrest and sentencing sequences for these individuals are shown in Figure 3.1.

As can be seen from Figure 3.1, the vast majority of duplicate and triplicate sequences (88.4% of all sequences) involve a pattern where the arrests occur and then the sentences are administered (the first and third sequences of Figure 3.1).[15] The confounding introduced by such patterns has to do with disentangling the sentences from one another. To anticipate the discussion of Chapter Seven, the later sentence is not independent from the initial sentence and any impact of the second (or third sentence) must take into consideration that of early sentences. For example, if the second sentence adds consecutive time to a prison sanction, then the sentence is given with full knowledge of the prior sanction and the real time served for the second sentence becomes a composite of two separate sanctions. Moreover, it becomes extremely difficult to ascertain how much of the time served was due to the first sentence of the sequence and how much is attributable to the

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[15] The ordering of multiple court appearances is determined by the date of sentencing. For the discussion that follows, and the patterns of Figure 3.1, it is not necessary to distinguish dates of arrest. Thus, sequences where the first sentence was for what was chronologically the second arrest, are not differentiated from those where the first sentence was for the initial arrest of the sequence.

later sentences.[16]

Conversely, the impact of the initial sentence of the sequence is much more ambiguous. The sentence may be given without any knowledge that another matter is pending if records available to the judge are incomplete. The parameters of the initial sentence are certainly decided upon in the absence of full information about the subsequent sentence: The second conviction may not have occurred at the time the first sentence is administered. While the initial sentence of the sequence may well be given independent of subsequent sanctions, its effects may not be. Subsequent criminal behavior will be subject to the impact of both sentences and not just the initial sentence of the sequence.

In addition to the lack of independence among sanctions, the remaining sequences of duplicate court appearances (11.5% of all duplicate patterns) raise another conceptual problem. Here, a subsequent arrest occurs after the initial sentence of the sequence (panels two and four of Figure 3.1). By definition, the individual is a recidivist after the first sentence and, under most interpretations, the initial sentence was a failure. Indeed, recidivism occurred rapidly within the early months of the sampling period. While this is useful information, inclusion of this initial sentence in the analysis would seem to bias the results toward "failed" sentences and short times to recidivism, in much the same fashion as would including the sanctions received by these offenders prior to the sampling period.

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[16] Note that the same concerns can be raised for all other, non duplicate, cases that appear in the Guidelines data set. The sentences administered may not be independent of other sanctions given prior to the sampling period. These issues are treated in Chapter Seven.

The lack of statistical independence between cases containing duplicated individuals is also cause for concern. One of the basic assumptions in most analytic techniques is that error in measurement is independent across the cases included in the analysis (Bohrnstedt and Carter, 1971; Pedhazur, 1982). Violations of this assumption lead to bias in the estimates of coefficients. Inclusion of all sentences involving offenders with multiple court appearances would guarantee correlated measurement error for over 7% of the sample being analyzed and most likely introduce bias into the estimated effects of independent variables.

The problems arising from duplicate court appearances are best avoided by considering only the last sentence of the sequence. Doing so results in a loss of 471 cases from the matched subsample, but conceptually and statistically simplifies the analysis. Of course, information about the earlier arrests and sanctions will be retained in the form of prior record indicators, prior sanctions, and other sentences being served as pertains to the last sentence of the sequence. In this sense, the earlier portions of multiple court appearances will be treated similar to those prior sanctions that occurred outside the sampling period.

Finally, deletion of the initial parts of duplicate court appearances has the added bonus of returning the unit of the analysis to the individual. Recidivism is an individual-level phenomenon, reflected in the behavior of the individual, and not a phenomenon attached to a court case or the sanction received from that court. This important distinction would not be maintained if multiple court appearances were allowed to remain in the sample to be analyzed. Ignoring the initial portions of duplicated individuals yields a conceptually simpler, and more easily interpreted, analysis.

DEFINING THE SAMPLE TO BE ANALYZED

The failure to match individuals in the Sentencing Guidelines data set and the decision to drop the initial portions of duplicate court appearances are not the only factors influencing the size of the final sample to be analyzed. Some offenders died, either while serving their sentence or after completion of the sentence. Other offenders had little or no time at risk after serving the sentence, and thus it was not possible to observe any post-sentence recidivism. In this section, we detail the additional considerations that further reduced the size of the final sample.

Juvenile Offenders.

A small proportion of individual were juveniles at the time of their 1976-77 state court appearance. Eleven offenders (.09% of the matched subsample) were under age eighteen when sentenced. These individuals were more likely to have been convicted of serious crimes against persons (e.g., robbery, sexual assault) and, consequently, were more likely to have received a sanction of incarceration.

The decision to remove these individuals from the final sample was guided by two considerations. First, SAC arrest histories have information only on adult arrests and thus it is likely that prior record information on these offenders, as well as the arrest leading to entry into the Guidelines sample, would be spotty at best. While there is a general concern over the lack of prior arrest records for the younger offenders, it is likely to be exacerbated for those individuals who were juveniles at the time of sentencing.

Second, these juveniles would be likely to produce range problems in estimating the effects of age upon recidivism. In one sense, juveniles appear

before a state court, especially during the time at which these data were collected, are "outliers" representing extreme cases where juveniles were tried as adults. It is unlikely that one would want to extrapolate the current results, either in terms of the effect of age or the influence of other independent variables on recidivism, to a nonadult population. Consequently, the sample to be analyzed will consist only of individuals over age 18 when sentenced.

Deaths.

Information on the deaths of individuals came from both the SAC arrest histories and from the DOC incarceration histories. In the former case, deaths were recorded either from court records, where the case was never disposed of because the offender had died, or from custodial records, where the death was reported by parole or probation officers or by prison authorities. The DOC incarceration histories supplemented the information in SAC, indicating those individuals who had died while in custody or while on parole. A total of 148 deaths (1.2%) were reported for the cases in the matched subsample. Considering duplicate court appearances, 138 individuals were reported to have died during the post-sentence observation period.

The probability that the offender died is not equally distributed across presenting offenses or the sanctions received. Those whose most serious conviction was for a robbery or a burglary were significantly more likely to have died during the post-sentence observation period than were those whose conviction was for another offense. (One hundred of the reported deaths come from the convicting offenses of robbery, assault, burglary, drugs, and weapons.) These offenders were also significantly more likely to have received a sanction of imprisonment, and significantly less likely to have

been given a sentence of probation.[17]

In general, offenders who had died were retained in the final sample. However, the date of death was used as the right censoring date, rather than the date of July 1, 1986. This provides a more accurate estimate of the actual time at risk subsequent to the 1976-77 sentence, though it can lead to exceedingly short risk times. Some offenders died within weeks of receiving their sentence, while two died while serving a sentence of incarceration and were thus never at risk after the sentence. These two individuals were dropped from the final sample (see below). An additional 10 cases involving offenders who died were dropped from the final sample as they involved the initial portion of duplicate court appearances and one case was lost as the offender who died was under age 18 at the time of sentence. A total of 135 individuals who were reported dead remain in the final sample.

Time at Risk.

From an incapacitative perspective, a sentence in which the offender is not on the street is effective. However, in extreme cases (life sentences and prison terms longer than the post-sentence observation period) the offender is never at risk after sentencing. Except for an occasional arrest while in custody, those serving long sentences are not rearrested and many of the measures of recidivism to be employed in the analysis (e.g., arrest rate adjusted for time at risk) are undefined when the offender is never at risk.

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[17] Whether the relationship between presenting conviction, and sanction, and the probability of death subsequent to the sentence is truly accurate is unclear. This could indeed reflect the more violent lifestyle of these offenders. However, this relationship could also have resulted from better record keeping in the Department of Corrections which would have oversight of those sentenced to prison.

As sentences leading to no time at risk may, a priori, be deemed "effective," these cases were removed from the final sample. A total of 74 individuals (.6% of the matched subsample) were eliminated under this restriction as they were in custody on the day of sentencing and were not released prior to the right censoring date. An additional 11 individuals were dropped because all of their time at risk occurred prior to the point at which they began serving a long prison sentence. In such instances, the individual did not immediately go into custody (sometimes it was just a matter of days, but in one case it was over a year), but once in custody, the offender was not released prior to the right censoring date. Finally, five more cases were deleted from the final sample because while they were legitimately at risk after being sentenced, there total time at risk was less than two weeks.[18]

Table 3.2 summarizes, by the most serious offense of conviction, the loss of cases from the original Sentencing Guidelines data base due to the various defining criteria for the final sample. Overall, 82% of the Guidelines sample remains in the subsample to be analyzed. Failure to match cases accounts for the largest cases loss (15.3%) with the other restrictions on the sample accounting for an additional 4 percent. There is, however, considerable variation in the loss of cases by the crime for which the offender was convicted. While small in absolute numbers, cases involving either kidnapping or bribery convictions are most poorly represented in the final sample, as fewer than 56% of the original cases were retained. Offenders sentenced for kidnapping were lost primary due to restrictions on

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[18] There is no pattern leading to these exceeding short times at risk. Two cases were individuals who died shortly after being sentenced to probation. Another two cases were escapes from custody who were rearrested and reconfined within two weeks. The final case was a combination of furloughs from prison and medical leaves which ultimately led to long-term psychiatric confinement.

risk time, while we failed to match a large proportion of those convicted for bribery. Not surprisingly, convictions for homicide are underrepresented in the final sample, as 17.2% of them were not matched, and an additional 11.1% were lost due to restrictions on time at risk. Conversely, convictions, and thus sentences, for the remaining offense types are well represented in the final sample. All extortion cases were retained, and over 85% of offenders convicted of burglary and drug crimes are carried over from the Guidelines data base. Thus the two offenses responsible for the greatest volume of crime were least likely to be dropped in the final sample. Two other high volume offenses, robbery and weapons, had were retained over 85% of the time.

Table 3.3 shows the distribution of case loss by the most serious sanction received in 1976-77. (Unmatched cases are not included in this table.) As is to be expected, restrictions on the final sample produced the greatest case loss among sentences to prison. Those with multiple court appearances were more likely to have been sentenced to prison, and thus 7.2% of the prison sentences were lost as initial parts of duplicate individuals. Long sentences of incarceration were also responsible for the loss of an additional 2.9% of prison sentences due to restrictions on time at risk.

CHARACTERISTICS OF THE FINAL SAMPLE

The restrictions on time at risk after sentencing, the failure to match all cases across data sources, and the original Guidelines Project scope conditions have yielded a sample of 11,749 individuals for the analysis. A large number of variables are available for assessing the individual's risk for recidivism at the time of the 1976-77 sentence. Many possible indicators of recidivistic behavior are also available, as are ways of measuring the

sentence itself. The exact nature of these variables will unfold in subsequent chapters as the various domains of independent and dependent variables are introduced. Here, we present a brief statistical overview in order to establish some basic characteristics of the final sample.

Table 3.4 presents means for selected variables covering several domains. Consider first the characteristics of the sample as a whole. Over half the sample is comprised of minorities, with 43.8% of these individuals being Black and an additional 8.8% identified as Hispanic. Slightly over 11% are female. On average, these individuals are 28 years old when sentenced, though this distribution is highly skewed toward younger offenders. The estimated socioeconomic status of the sample (Botsko et al., 1990) averages between a skilled blue collar worker and an unskilled blue collar worker. About half the sample was employed at the time of sentence and a full 85% were living with other family members. Note that almost half the sample were not born in New Jersey.

About 13% of the sample had evidence of drug use as a juvenile in their PSI. Twelve percent also had evidence of using drugs as an adult. More surprising is the extent of mental health or alcohol problems. Over one quarter of the sample have a history of treatment for mental health problems and 25.7% have been treated for problems with alcohol.

The impressionistic evidence supplied by coder ratings and the writer of the PSI suggest that, on average, the offense(s) charged are more severe than the offender is "bad." The average prior record is seen as relatively short and the expectation is that the average offender is more likely to be rehabilitated than s/he is to recidivate. Consistent with the evaluations made by the Guidelines Project's coders, the PSI writer, on average, leans

toward favoring a sentence of probation.

The sample to be analyzed is biased towards those convicted of burglary or drug crimes. These two categories account for almost 40% of the sample. Convictions for robbery, assault, and weapons offenses account for an additional 26.0% of the final sample. Convictions for the serious persons crimes of homicide, kidnapping, and sexual assault were relatively infrequent during the sampling period, as were convictions on charges of bribery, damaging property, and embezzlement. About 30% of the sample were convicted of more than one type of charge.

Prior record, as measured by counts taken from the SAC arrest histories, shows that the average offender in the sample had been arrested 3 times before the arrest that led to inclusion in the Guidelines Project sample. Multiple charges were leveled during at least one of those arrests, as the sample averages 5 prior charges. The "typical" offender in this sample had been convicted 1.77 times, based on those 3 arrests, and had been convicted on about half the charges. Prior to the sentence in 1976-77, the average individual had served just over 2 months on probation, less than one month in jail, and almost one half year in prison.[19]

Use of prior record indicators taken from the Guidelines Project's "centerfold" finds the average individual to have a longer record than suggested by the SAC arrest histories. Given the fact that the centerfold incorporates juvenile records, this is reasonable. Most measures double, suggesting that these offenders had been criminally active as juveniles, in

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[19] As with the distribution of age, the distributions of prior record indicators are highly skewed. Modal values on these variables tend toward few prior arrests, charges, convictions, and no prior probation, jail or prison terms.

addition to the known crimes as an adult. The sample averages over one incarceration prior to the 1976-77 sentence. The difference between the total incarcerations and incarcerations as an adult (.17) implies that a substantial portion of the sample served time as a juvenile.

The distribution of county in which the case was heard mirrors the demographics of New Jersey at the time the sample was collected. The most densely populated, urban county (Essex) accounts for 16.4% of the cases in the sample. Relatively rural northern counties (Sussex, Warren, Hunterdon) account for only 2.1% of the cases to be analyzed. The southern counties of Salem, Camden, Burlington, Atlantic, Cape May, and Cumberland provide 26.3% of the convictions that define the Guidelines Project sample.

Also shown in Table 3.4 are means broken down by the most severe sanction to which the individual was sentenced. As before, we differentiate fines (or restitution or lesser sanctions), a probation term, and time in jail from prison terms. However, we divide sentences to prison into those where the offender was sent to the Youth Correctional Complex at Yardville and those where the individual was sentenced to one of New Jersey's adult facilities.

When broken down by sanction, these means show clear evidence of sentencing practices that were guided by a retributionist philosophy. All measures of prior record as taken from the centerfold data, and all measures of prior arrests and convictions from the SAC arrest histories are monotonically related to the severity of the sanction. The longer the prior record, the more severe the sentence. Similarly, the impressionistic ratings have a strong monotonic relationship with the severity of the sanction. As the severity of the sanction increases, so too, on average, does the perceived severity of the offense, the perceived offender "badness," the perceived

likelihood of recidivism, the overall assessment of prior record, and the PSI writer's recommendation for incarceration.

The kind of offense for which the individual was convicted is also related to the sanction in expected ways. The proportion of the sample receiving a particular sanction monotonically increases for the serious persons crimes of homicide, kidnapping, and sexual assault, as does the proportion of those convicted for robbery and burglary. (For the latter two crimes, the proportions are higher for sentences to Yardville than to state prison, but this is simply a reflection of the age of the offender.) Convictions on multiple types of crimes is also strongly related to the sentence, as over half of those sent to a prison were conviction for more than one offense.

Conversely, the largest percentage of convictions for certain types of crimes are found among the less severe sanctions. The proportion convicted on weapons charges monotonically decreases with the severity of the sentence. Convictions on drug crimes (most likely possession) have the highest proportion of cases, 27.5%, for sentences to probation. Convictions on charges of fraud and damaging of property also "peak" for probation sentences.

Offender characteristics also show strong relationships to the severity of the sentence. Race, socioeconomic status, drug use, both as a juvenile and an adult, employment status, and history of treatment for mental health problems are all, on average, associated with the sanction received. There are also relationships between the county in which the case was heard and the sentence given in 1976-77, though here the patterns are less obvious. For example, the proportion of cases from Atlantic county is highest for sentences to jail, while for Monmouth county, the largest proportion is observed for

those fined.

Of particular concern is the manner in which offender age is related to the sanction received. On average, those sentenced to either a fine or an adult facility tend to be the oldest. As a group, those sent to the Youth Correctional facility at Yardville are the youngest. Such differences are to be expected as they are built into the custodial practices of New Jersey. However, age is known to be strongly correlated with rates of offending (see Panel on Research on Criminal Careers, 1986, for a review). Indeed, it has been argued (Gottfredson and Hirschi, 1986) that age alone can account for observed rates of criminal behavior with involvement decreasing with the offender's age. If this is true, then assessing the risk for recidivism and the effectiveness of the sentence administered in 1976-77 will be highly confounded by the differential age composition of the various sanctions.

A priori there is the expectation that subsequent recidivism would be lowest for those given a fine or sent to prison, while it would be highest for those incarcerated at Yardville. The confounding of groups by age would thus make sentences of fines or state prison terms appear more "effective" than those involving sentences to Yardville, but such results could be an artifact of the differing distributions of age between the groups. Consequently, a control for age will be needed in our analysis.

More generally, the means shown in Table 3.4 establish several, not unexpected, aspects of the criminal justice system. The sentencing of offenders is not a random process. Those given a particular type of sanction represent a substantively different subgroup from those given another form of sentence. Furthermore, by virtue of correlations between the kinds of individuals likely to commit certain kinds of crimes, prior record is

intimately associated with the sociological characteristics of the offender, and, consequently, with the sentence received. This raises the likelihood, to be borne out by our analyses, that it will be extremely difficult to attribute any recidivistic behavior as uniquely due to the offender's characteristics, prior record, or the sentence administered. Assessing the risk for recidivism involves the use of factors that are highly intercorrelated.

A more sobering suggestion that arises out of Table 3.4 is that any subsample used for an analysis represents a nonrandom subset of the entire sample. Distributions on key variables may be truncated for the subsample[20] and the statistical impact of these variables may vary depending upon the subgroup selected. Thus, for example, when looking only at offenders sentenced to a state prison, the results are confounded by the factors that led the individual to be sentenced there. To find that, say, prior record and type of presenting offense is not predictive of recidivism for this subgroup ignores how these variables were used in the definition of the subgroup. In order for groups, defined nonrandomly on the basis of sanction, to be compared, some mechanism is needed to account for initial differences that exist between the groups.

SAMPLE SELECTION BIAS

The average differences between groups defined by sanction (Table 3.4) and the systematic differences between the subsample defined by the matching process (Table 3.1), means we run the risk of comparing "apples and oranges"

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[20] Not evident from Table 3.4 is the fact that the variability of the measures is also related to the sanction. The variance in age, for example, is much less among those sentenced to Yardville, than for those receiving other sanctions.

when assessing the risk for recidivism and the effect of the 1976-77 sentence. Groups defined on the basis of sanction do not have the full range of values on the independent variables (e.g., age, prior record) and the matched subset of cases is not fully representative of the Guidelines Project sample on these variables. The potential thus exists for misstating how the independent variables predict the risk for recidivism as well as misestimating how the sentence impacts upon recidivism.

Relatively recent statistical techniques are available to attempt to control for the nonrandom aspects of data taken from the criminal justice system. Falling under the general heading of "sample selection bias," (Heckman, 1976; Berk, 1983; Klepper et al., 1983), these techniques provide a way to control for a variable's effect in defining the sample analyzed, prior to assessing its impact in a substantive analysis. In Appendix A, we provide a detailed exposition of sample selection in the criminal justice system and review its application in criminal justice research to date. Here, we briefly summarize the main issues and how selection bias measures will be incorporated in the present study.

Consider the probit model (Table 3.1) discussed earlier in conjunction with the analysis of the probability that a case was matched across data sources. The results of that model clearly established that the matched subset of cases was nonrandom in that many variables were significantly related to the probability of being matched. Thus, these variables (e.g., age, total prior convictions, presenting conviction for drugs, prosecutor recommending probation) are important for the determination of the final sample to be analyzed. If, for example, the number of prior convictions is correlated with the number of arrests after the 1976-77 sentence, the

estimated correlation is potentially due to two distinct components. One is the "real" predictive effect of prior convictions on subsequent recidivism and this is what we wish to estimate. The other is the effect of prior convictions on defining the cases to be studied. If this component is large, it may confound the estimated correlation, in essence, double counting the effect of prior convictions.

The solution, attributed to Heckman (1976), is to conduct the analysis in two steps. First, the selection of the sample is modeled using whatever variables are relevant to the definition of the sample to be analyzed (the so-called "selection equation"). The results of this step are then used to create a statistical control variable to be employed in the "substantive equation" that is of interest. The utility of the approach rests on the ability to adequately model the selection process, and thus control for the definition of the sample, and the technique is not always successful in meeting the stated objectives (Stolzenberg and Relles, 1990), but it does offer some method for controlling for the processes that created nonrandom samples.

The probit model of Table 3.1 can be viewed as a selection equation for the matching process. Using the coefficients of this equation, each case in the Guidelines Project data set can be assigned a predicted probability of being matched. Cases with a high probability of being matched would be those convicted of certain offenses (burglary, drugs), from certain counties (Burlington, Sussex), with a relatively great number of prior convictions, and where the offender had certain characteristics (e.g., was Black, male, young and used alcohol). Cases less likely to be matched would have the converse of this variables and cases with a mixture would have a predicted probability of

being matched that falls somewhere in between.

Through statistical transformations described by Berk (1983), the predicted probability of being matched can be converted into a continuous "hazard" rate reflecting the likelihood that the case was excluded from the sample being analyzed. Cases low on this hazard rate are expected to be in the sample by virtue of the configuration of independent variables used in the selection equation model. Cases high on this hazard rate are, a priori, not expected to be in the sample and thus their inclusion indicates relatively 'different' about the cases (e.g., mitigating circumstances surrounding the case, or, for the matching process perhaps a unique name).

For each case in the Guidelines Project sample, we have computed the hazard rate representing the likelihood of exclusion due to failing to match the case across data sources. We refer to this as the "matching hazard." When used, the computed hazard rate is entered into an analytic equation as a statistical control, an instrument representing the sum of the nonrandom processes that defined the subsample that was matched. The measure has no direct substantive meaning. Rather it serves as a way to 'adjust' the estimated effects of other variables for how those variables may have contributed to the success of matching the case across data sources.

Unfortunately, just as one may be concerned about the nonrandom nature of the matched subset, the original Guidelines Project sample may also be seen as nonrandom. Those cases resulting in a conviction before a State Court are a nonrandom subset of all those cases making it to that level court. Cases brought to court are a subset of those indicted, those indicted a proper subset of those arrested, and so forth. As Berk (1983) has noted, there is a problem of "infinite regress." The "funneling" nature of the criminal justice

system (Lisefski and Manson, 1988) makes the potential for infinite regress quite strong as there are many places in the system where cases could be nonrandomly removed from the system.

In Appendix A we detail the construction of several hazard rates designed to increase our understanding of how a case came to be included in the Guidelines Project data set. Like the matching hazard, these measures can be used to control for sample selection processes, in this instance the likelihood that a case in the Guidelines Project data failed to pass key filters in the criminal justice system. The measures are:

1) A "reporting" hazard which measures the likelihood that an arrest appearing in the New Jersey Uniform Crime reports would not appear in the offenders SAC arrest history.

2) A "past arrest" hazard which models the likelihood that the charges would be dropped and thus the case would not go to municipal or state court or to a Grand Jury.

3) A "Grand Jury" hazard which incorporates how the nature of the case influences the likelihood that a case will proceed to a Grand Jury for indictment rather than having the charges disposed of in a municipal court.

4) A "Superior Court" hazard reflecting the probability that the case was heard in a superior court if it made it to a Grand Jury.

5) A "Superior Court conviction" hazard measuring the likelihood that an offender charged with a certain type of crime would be convicted in a Superior Court. Note that this hazard measures the primary sampling criteria for the Sentencing Guidelines Project.

For each matched case in the Guidelines Project data set, we have computed hazard rates for the likelihood of not continuing past the filter of that stage. As with the matching hazard, these instruments can be used to control for the selection processes that ultimately led to the sample to be analyzed.[21]

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[21] Just as prior selection processes can be problematic, so too can the sample selection defined by the sentencing process. It can be argued that sentencing is a series of binary decisions, each resulting in a nonrandom sample. The "in/out" decision leads to one of fine or probation where the initial

Exactly how to use these measures in our substantive equations is not clear. To our knowledge, no studies have explicitly incorporated a two-step correction for sample selection bias in the study of recidivism.[22] Moreover, the utility of the Heckman approach is hotly debated and the statistical consequences for using multiple hazard variables simultaneously are not known. We therefore adopt a conservative strategy where substantive models will be estimated both with and without hazard instruments.

POST-SENTENCE OBSERVATION WINDOWS

The present study offers an unusually long period in which to observe post-sentence criminal behavior. Those offenders not sentenced to incarceration during the early part of the sampling period in 1976 could be observed for up to ten years after sentencing. More standard in the literature are windows of either three or five years in length. In windows of any length, the actual amount of time at risk for criminal behavior is a function of three factors, the length of incarceration for the initial sentence, any additional time served on subsequent incarcerations, and the possible death of the offender.

For all cases in the final sample, the post-sentence observation windows begin the day the offender is free after being sentenced. For those sentenced

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decision is to not incarcerate. If the first decision is "in," then a model for jail versus prison could be developed. It is precisely this filter that leads to differences in the distributions of independent variables across sanctions that was seen in Table 3.4. A full treatment of sample selection biases would incorporate hazard rates derived from the sentencing process. However, this is beyond the scope of the present study.

[22] Some studies of similar processes, most notably Zatz and Hagan (1985), have used event history models that incorporate many of the concerns of sample selection bias. However, the nature of the present data do not allow us to adopt this approach.

to probation or time served, or fined, this is the date of sentencing. The beginning of the post-sentence window is define by the date of release for those given jail or prison terms. Table 3.5 displays how risk time is distributed across the sanctions given in 1976-77 for post-sentence windows of differing length.[23]

As can be seen in Panel A of Table 3.5, at least three full years of time at risk can be observed for over 97% of all cases in the final sample.[24] A few offenders have windows that are less than one full year (but greater than two weeks as determined by the restrictions discussed earlier).[25] Panel B of the table indicates that full five-year windows are available for 92.5% of offenders sentenced to prison. (Sentences to an adult state prison and the Youth Correctional Complex at Yardville are combined for these tables.) For those receiving other sanctions, full five-year windows are available for over 99% of the cases.

If the full post-sentence observation period is used (Panel C of Table 3.5), the amount of time at risk becomes associated with the 1976-77 sentence. For those whose most serious sanction was a fine, 80.7% were at risk for at least 9 full years. Of those given probation during the Guidelines study,

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[23] Time at risk was calculated in terms of the number days defined by the three year, five year, or full windows minus the days spent in jail or prison during the appropriate time period. These were then rounded into 1 year widths assuming 365 days in each year. A small amount of measurement error is introduced by leap years.

[24] Note that three individuals are not included in Panel A of Table 3.4. These offenders were sentenced to probation while serving time on another matter. For the first three years after receiving their probation sentences, they were not at risk. All were released and are included when the observation window is 5 years or longer.

[25] Not shown in Table 3.5 is the fact that 99.7% of the final sample were at risk for at least one full year.

75.4% had at least nine full years of time at risk after that sentence. The maximum risk time was observed for 39.2% of those receiving sentences to jail, and drops to only 9.6% for those given prison sentences, though at least 8 full years of risk time is found for 94.7% of those sentenced to jail and 49.4% of those receiving prison terms.

The panels of Table 3.5 reflect, in part, the effect of incarceration sentences upon the time available to observed subsequent criminal behavior. The longer the incarceration, especially for prison sentences, the more real time is needed to observe a given amount of time at risk. Yet these distributions also indicate that the longer the post-sentence window, the less likely some offenders are to be at risk. Not only are those sentenced to prison off the streets while serving the initial sentence, they are also more subject to reincarceration either through violation of parole or the commission of additional offenses.[26]

THE QUALITY OF THE DATA

The Effects of Sentences data set described in this chapter represents the intersection of official data as maintained by several state agencies. The Guidelines Project data set incorporates the factors known to criminal justice actors at the time of sentencing as taken from the Presentence Investigation Reports. The SAC arrest histories provide known arrests and convictions both before and after the 1976-77 sentence. Incarceration histories from the Department of Corrections yield measures of prior and

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[26] As is demonstrated in later chapters, younger offenders sentenced to prison tend to recidivate at a much higher rate than older offenders. Consequently, their time at risk will be reduced by incarcerations due to new crimes.

subsequent prison terms, as well as exceedingly accurate estimates of time at risk for criminal behavior. As such, the merger of information from these sources provides a complete as possible picture of the offender and his/her actions as can be constructed from official record data.

The reliance upon only official sources of data is at once the major strength and the major weakness of the present study. From the perspective of assessing risk for recidivism and the decision making necessary to administer criminal justice sanctions, official records is the most appropriate source of data. Criminal justice actors make decisions on the basis of information that is available to them, information that is used here to model recidivism. While other information (e.g., treatment received during incarceration, subsequent unemployment, offender attitudes) may be more predictive of recidivism, it is not possible to use this information in determining the risk for recidivism if the information is not there to be used. Similarly, prior record may be only poorly measured by official rap sheets (see below), as the offender may have committed many more crimes than appear on the rap sheet. Here too, acceptable risk must be ascertained using what is known, not what is unknown. By using only official record data, we can mirror the decision making processes at the time the focal sentence was administered.

Yet by relying solely upon official maintained data sources, much is missed. Some recidivism will go undetected by the authorities. Conversely, if no subsequent rearrests are reported, we have know way of ascertaining what differentiates the "successes" from the "failures," save from reference to the variables available at the time of sentencing. What is "effective" about a sentence may be a function of information we are not privy to in official records. A comparison group of individuals not convicted is also unavailable,

and, even if they were, the full range of independent predictors would not have been collected as PSI reports would not have been completed under those circumstances.

Aside from the general advantages and disadvantages inherent in the use of official records, there are some that impact in our ability to study recidivism in particular. This, in turn, influences our understanding of the effectiveness of the 1976-77 sentence as "effective" is, by necessity, determined with reference to subsequent criminal behavior. Several cautions must be raised, as they are important for evaluating the results in the analytic chapters that follow.

Limitations.

There are many known errors arising from the use of official records to measure criminal activity. (See Panel on Research on Criminal Careers, 1986, for a review.) Chief among these is the fact that such data are limited only to behavior known to authorities through arrest or conviction. Much illegal behavior goes undetected, as is evidenced by clearance rates for crimes known to the police. Self-reported rates of offending are also much higher than those found in official records (see, for example, Chaiken and Chaiken, 1982; Greenwood, 1982; Visher, 1986).[27]

Coupled with this is the potential for differential recording of arrests and convictions across jurisdictions and the misclassification or different interpretation of the particular crimes that are recorded. This raises the very real concern that we know more about the criminal behavior of given

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[27] Self-reported crimes are not without error. Among the problems cited are distortion, responses error, and memory recall error (Weis, 1986). Were such data available for the individuals in the current sample, a different set of potentially damaging concerns would thus arise.

individual solely because s/he has been arrested in a particular municipality or county and exactly what the individual has been arrested for (the charges) is contingent upon how the act is defined in the county of arrest. Moreover, reporting is known to vary with the type of offense, with more serious crimes more likely to be reported. This means that reporting error is heterogeneous, with the type of offense correlated with reporting either through clearance by arrest or through the reporting of certain types of crimes to the central repository, in this instance the SAC arrest histories.

Because our source of arrest and custody information comes from state agencies, the data are limited primarily to the criminal behavior known by the New Jersey authorities. While some out-of-state arrests are found in the SAC data file, they are relatively few in number. Similarly, when inmates are transferred into a New Jersey prison, or released to the custody of an out-of-state prison, we have some evidence to use in adjusting time at risk. Overall though, it is safe to assume that our sources of data underestimate both the criminal behavior known to all authorities, and the amount of time that the average offender has spent in custody, especially for short stays in jail. Consequently, reporting errors within New Jersey are likely to be compounded with a lack of information about official arrests and convictions that occurred out-of-state.

Exactly how the biases that are known to exist in official records can influence the study of recidivism is unclear, though several speculations can be offered. In general, it is expected that the more sensitive the measure of recidivism, the more subject it is to the influences of these biases. Simple dichotomous measures, (e.g., was the offender rearrested, reconvicted, reimprisoned and so forth) are apt to be least effected as they rely upon

neither the type of crime or the volume of subsequent crimes. Count-based measures of recidivism are open to error introduced by lack of reporting and failure to clear reported crimes, as well as to the differential reporting across jurisdictions. When rates of arrest, charges, convictions, and so forth, adjusted for time at risk are introduced, it must be remembered that they are subject to the same errors as the counts that comprise the numerator of the rate.

Measures of recidivism that rely upon the charge at rearrest (e.g., charge at first rearrest, charge at first reconviction) are dependent upon both the interpretation of the act by the reporting jurisdiction and the fact that certain kinds of offenses are more likely to be cleared and reported than others. Those who are identified as being rearrested for a relatively serious offense at the first arrest after sentencing may well have been arrested for a lesser, unreported, crime prior to the charge that appears on the official arrest record. Counts of rearrests for certain kinds of charges (e.g., serious persons crimes versus property crimes) may be more accurate simply because the type of offense is more likely to appear in official records. Similarly, recidivism measures that incorporate a time dimension (days to failure through rearrest, days to failure through reconviction) are likely to overestimate time as some crimes go unreported or undetected.[28]

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[28] Note that exactly the same cautions can be raised when official records are used to measure criminal behavior prior to the sentence. For example, the age at first arrest may be earlier due to unreported or undetected offenses, the charge at first conviction may be differentially interpreted across jurisdictions, and the total number of prior arrests may be an underestimate. These are not necessarily random errors, and thus using prior record, measured through official arrest histories, to predict recidivism, taken from the same source, may be introducing correlated error into the analysis.

The issue of the completeness of official record information raises some additional cautions that are more particular to the merged data set described earlier. In triangulating the information from the three sources of data, it became apparent that one or more sources was missing events (i.e., arrests, convictions, incarcerations) that were implied from the data in another source. This necessitated the creation of many records in order to reconcile the inconsistencies that were observed. In some instances, arrest/convictions were needed to reflect the Sentencing Guidelines Project offense(s). In others, DOC had incarcerations that were prior to the earliest arrests and convictions recorded in the SAC files and additional records had to be created.

For the exact details on the situations leading to the creation of arrest, conviction, and custodial records, and for an indication of the number of records created, the reader is referred to Smith and Smith (1990). However, for the purposes of understanding the measures of criminal behavior and the sentence the are developed in later chapters, two points are important. First, for a relatively small proportion of individuals, the supervisory or custodial actions taken as a result of the 1976-77 sentence were not indicated in the SAC arrest histories. These had to be created, and thus we can only estimate the amount of time actually served on sentences to probation, or the time spent in jail or prison while serving a sentence of incarceration.[29]

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[29] The conservative decision rules adopted make it unlikely that the need to create these records was due to improper matches between the case in the Guidelines Project and the SAC arrest histories. Rather, this is more likely an indicator of the incompleteness of the SAC files.

Second, our experience in processing the data from the SAC custodial records leads us to conclude that sentences to jail are poorly reported to the State Police. In particular, release dates were often missing. The vast majority of created custodial records pertain to estimated dates of release from county jails. Consequently, time not at risk due to incarceration in a county jail, both before and after the 1976-77 sentence, is likely to be underestimated. Measures of recidivism that use information relying upon jail records (i.e., rejailed after sentencing, days to rejailing after sentencing) are particularly suspect to the underreporting of jail terms in the SAC file.

Historical changes in the New Jersey Criminal Justice system may also be responsible for some of our findings in later chapters. As mentioned earlier, the enactment of a "speedy trial" statute in 1981 resulted in a small dip in the arrests reported to SAC for a one-year period. This appears to be solely a drop in <u>reporting</u>, perhaps because of the initial increase in paperwork mandated by the statute. It is of consequence because the change occurred during our post-sentence observation window and most likely will produce a small underestimation in recidivism measures that use counts of arrests and convictions in much the same manner the known underreporting biases in official records tend to underestimate criminal behavior. For those offenders serving a sentence of between four and five years (depending upon the date sentenced), release may have coincided with the observed decrease in reporting. If so, any recidivism may have gone unmeasured in the SAC arrest histories, or may be reported as occurring later in time than would have been the case if the statute had not taken effect.

Potentially more serious is a massive change in the New Jersey criminal code that took effect in 1979. One of the primary motivations behind the

change was to reduce the numbers and types of charges that could be leveled for certain types of offenses. For example, the change in the code made it unnecessary to charge the offender for larceny in conjunction with a burglary. We have used the SAC arrest histories to compare charges per arrest prior to the change to charges per arrest after the change and have found the differences to be nonsignificant. As has happened in other states (Knapp, 1987), it appears that New Jersey's prosecutors may have circumvented some of the intent of the new criminal code by charging offenders with different crimes, permissible under the new code.

The change in the criminal code is thus unlikely to seriously impact on most of our measures of recidivism, even though it coincided with the post-sentence observation window. To the extent that it succeeded in reducing multiple charges for a single act, it may have actually increased the accuracy of charges as a measure of criminal behavior when compared to using arrests alone. However, charge-based measures of recidivism (e.g., total charges, charges for persons crimes adjusted for time at risk, charge at first rearrest or reconviction) must be interpreted in light of this change, for strictly speaking, charges (but not arrests) prior to the change in the criminal code are not comparable to those after the change. Furthermore, any observed decrease between charge rates prior to the sentence and after the sentence could be attributed to this historical artifact rather than other variables such as the increasing age of the offender or the impact of the sentence administered.

Advantages.

While certain cautions must be raised by biases in official records and the historical changes during the post-sentence observation period, they are

not insurmountable. Nor are the problems unique to New Jersey as official record keeping is indeed quite good throughout the state. In addition, any longitudinal study that covers a long time period is likely to encounter potential problems with changes in official practices over time. While the concerns raised earlier are real, they are counterbalanced by several advantages to the current data set.

We have at our disposal an exceedingly wide range of variables to use in the prediction of risk of recidivism at the time of sentencing, as well as for controls to be used in assessing the effectiveness of the sentence. The measures of prior record computed from the SAC arrest histories, and the detailed information culled from the Presentence Investigation reports go well beyond that normally found in criminal justice data. Moreover, these variables tap many of the factors thought to be important in understanding the etiology of criminal behavior and are not normally available in most data sets. It is the existence of these measures that allow us to broaden the study of recidivism to include the more sociological correlates of criminal behavior.

Similarly, the information taken from the Judgement of Conviction sheets, in conjunction with prior record measures and the PSI data, will allow us to operationalize many more dimensions of the sentence than are commonly available. In addition to the more standard measures of in versus out, time sentenced, and where the individual is sent if s/he is incarcerated, we will be able to measure how the 1976-77 sentence fits into the entire history of sanctions and how the sentence is correlated with other sanctions in force at the time of sentencing. These measures are the gist for Chapter Seven.

The DOC incarceration histories, when used in conjunction with the custodial records available in the SAC arrest histories, allow for uncommonly good estimates of time at risk for criminal behavior. While there is some concern that time incarcerated in jail is underestimated, we know a great deal about the timing of entry and release from prison, how many different times the individual was incarcerated in prison, and how long s/he served at each incarceration. Rates of offending (arrests, charges, convictions, and so forth) can thus be adjusted for time at risk to yield more accurate estimates of both pre- and post-sentence criminal behavior.

As noted earlier, the present data set is characterized by unusually long windows in which to observe any post-sentence recidivism. For over 96% of the final sample, full three year windows are available and full five year windows can be used for over 92%. As the length of the window increases, so too does the association between the sanction and the maximum window (those incarcerated have shorter windows), but for those not sentenced to incarceration, a post-sentence observation period of at least eight years is available for over 98%. And, the longer the window used, the greater the confidence that any undarreporting on official record data will not seriously distort basic recidivism measures such as being rearrested or reconvicted.

The availability of measures to control for the potential influence of sample selection bias is also a distinguishing feature of the Effects of Sentences data set. The sentencing literature is becoming increasingly sophisticated in the use of such measures and the evidence there is that a failure to control for selection bias effects can lead to false conclusions concerning the effects of some independent variables. However, recidivism studies have not kept pace with that literature and thus it is unknown whether

selection effects can distort results in recidivism research. Given the breadth of selection bias measures computed for the current data, it will be possible to investigate selection bias effects in detail.

SUMMARY

The sheer magnitude of the data available for the present study has meant that we have had to be selective in the exposition here. While additional details will unfold in later chapters as the range of independent and dependent variables are introduced, many of the complexities of the data and the data processing cannot be covered. We refer the reader to other sources for this information. McCarthy (1979) describes the original Sentencing Guidelines Project. The merger of data from the three sources of official records is discussed in Smith and Smith (1990). The structure of the combined arrest, court, and incarceration histories is overviewed in Smith and Smith, (1990). Finally, the variables available in the combined data set, as well as the construction of key composite measures of the offender, prior record, and the sentence administered, may be found in Botsko et al., (1990).

We began this chapter by reiterating the call for high quality data in criminal justice research. Such information is needed for informed decision making at all phases of the criminal justice system. The data set compiled reflects both the best and the worst of the information available to decision makers: often voluminous data, but of unknown reliability and poorly understood utility for predictive purposes. It is within this framework that recidivism and the effects of sentences on recidivism are studied in later chapters.

Figure 3.1

Patterns of Duplicate Court Appearances in Sample

Arrest -> Arrest -> $Sentence_1$ -> $Sentence_2$

(N=378, 84.0% of Duplicate Patterns)

Arrest -> $Sentence_1$ -> Arrest -> $Sentence_2$

(N=51, 11.3% of Duplicate Patterns)

Arrest -> Arrest -> Arrest -> $Sentence_1$ -> $Sentence_2$ -> $Sentence_3$

(N=20, 4.4% of Duplicate Patterns)

Arrest -> $Sentence_1$ -> Arrest -> Arrest -> $Sentence_2$ -> $Sentence_3$

(N=1, .2% of Duplicate Patterns)

Table 3.1

Probit Model for the Matching Process
(N=14,328)

Independent Variable	Coefficient	Std. Error	T-Ratio	Significance
Constant	1.288	.083	15.509	.000
CONVICTED FOR				
Burglary	.246	.046	5.342	.000
Fraud	-.290	.054	-5.387	.000
Drugs	.134	.038	3.532	.000
Bribery	-.691	.222	-3.117	.002
Other (UCR Misc.)	-.129	.055	-2.336	.020
Multiple Types	.088	.033	2.683	.007
COUNTY				
Atlantic	.194	.074	2.614	.009
Burlington	.422	.075	5.637	.000
Cumberland	.313	.126	2.492	.013
Essex	-.626	.035	-17.670	.000
Mercer	-.272	.058	-4.711	.000
Salem	.302	.133	2.271	.023
Sussex	.346	.191	1.811	.070
Union	.227	.062	3.673	.000
OFFENDER				
Black	.144	.032	4.444	.000
Hispanic	-.187	.046	-4.042	.000
Female	-.062	.044	-1.409	.159
Age (Years)	-.014	.001	-9.580	.000
Degree of Alcohol Use	.063	.021	2.925	.003
Lives with Family	.068	.037	1.819	.069
PRIOR RECORD				
Previous Arrests	-.009	.005	-2.058	.040
Convictions as an Adult	.026	.010	2.685	.007
Incarcerations as Adult	-.024	.012	-2.066	.039
PROSECUTOR RECOMMENDS				
Dismissal	.110	.028	3.919	.000
Restitution	-.274	.084	-3.265	.001
Probation	.124	.058	2.149	.032
Place of Imprisonment	.078	.041	1.904	.057

SUMMARY STATISTICS	
Mean of Dep. Var.	.860
Likelihood Ratio	1182.1
Degrees of Freedom	27
Significance	<.001
Pseudo R-squared	.076

Table 3.2

Distribution of Case Loss by Offense of Most Serious Conviction
(N=14,329)

Offense of Most Serious Conviction	Retained	Not Matched	Initial Part of Duplicate	Under Age 18	No Risk Time	All Risk Prior to Serving	Total Risk Less Than 14 Days
Homicide	68.7% (228)	17.2% (57)	2.7% (9)	.3% (1)	8.4% (28)	1.8% (6)	.9% (3)
Kidnapping	53.3% (8)	13.3% (2)	-	-	26.7% (4)	6.7% (1)	-
Sexual Assault	74.4% (125)	13.7% (23)	5.4% (9)	.6% (1)	6.0% (10)	-	-
Robbery	82.5% (1012)	11.7% (143)	4.2% (52)	.2% (3)	1.2% (15)	.2% (2)	-
Assault	83.6% (930)	13.3% (148)	2.4% (27)	.2% (2)	.5% (6)	-	-
Arson	84.8% (78)	12.0% (11)	3.3% (3)	-	-	-	-
Extortion	100.0% (20)	-	-	-	-	-	-
Burglary	87.3% (2083)	7.2% (172)	5.4% (128)	.1% (2)	.1% (2)	-	-
Larceny	83.7% (584)	13.0% (91)	3.2% (22)	-	.1% (1)	-	-
Stolen Vehicle	78.9% (165)	15.8% (33)	5.3% (11)	-	-	-	-
Forgery	78.9% (373)	13.7% (65)	7.4% (35)	-	-	-	-
Fraud	71.2% (825)	26.3% (304)	2.5% (29)	-	-	-	-
Embezzlement	75.5% (105)	22.3% (31)	2.2% (3)	-	-	-	-
Stolen Property	82.5% (302)	15.0% (55)	1.9% (7)	-	.5% (2)	-	-

Table 3.2
(Continued)

Distribution of Case Loss by Offense of Most Serious Conviction
(N=14,329)

Offense of Most Serious Conviction	Retained	Not Matched	Initial Part of Duplicate	Under Age 18	No Risk Time	All Risk Prior to Serving	Total Risk Less Than 14 Days
Damage Property	84.8% (56)	9.1% (6)	6.1% (4)	-	-	-	-
Drugs	86.0% (2600)	11.6% (350)	2.3% (71)	.0% (1)	-	-	-
Sex Offenses	84.7% (210)	12.5% (31)	2.4% (6)	-	.4% (1)	-	-
Gambling	72.3% (81)	23.2% (26)	2.7% (3)	-	.9% (1)	.9% (1)	-
Escape	75.4% (141)	19.3% (36)	3.7% (7)	-	1.6% (3)	-	-
Bribery	55.9% (19)	41.2% (14)	-	-	-	-	2.9% (1)
Weapons	82.4% (1120)	15.3% (208)	2.1% (28)	.1% (1)	.1% (1)	.1% (1)	-
All Others	75.7% (684)	23.3% (202)	1.9% (17)	-	-	-	.1% (1)
Total	82.0% (11749)	15.3% (2008)	3.3% (471)	.1% (11)	.5% (74)	.1% (11)	.0% (5)

Table 3.3

Distribution of Case Loss by Most Serious Sanction - Matched Cases Only (N=12,321)

Most Serious Sanction	Retained	Initial Part of Duplicate	Under Age 18	No Risk Time	All Risk Prior to Serving	Total Risk Less Than 14 Days
Fine or less	98.0% (431)	2.0% (9)	-	-	-	-
Probation	97.6% (6224)	2.3% (147)	.0% (2)	.0% (1)	-	.0% (1)
Jail	95.9% (2431)	3.9% (100)	.1% (2)	.0% (1)	-	-
Prison	89.6% (2663)	7.2% (215)	.2% (7)	2.4% (72)	.4% (11)	.1% (4)
Total	95.4% (11749)	3.8% (471)	.1% (11)	.6% (74)	.1% (11)	.0% (5)

Table 3.4

Means for Selected Variables
(Maximum N in parentheses)

Variable	Total Sample (11,749)	Most Severe Sanction: Fine (431)	Probation (6,224)	Jail (2,431)	Yardville (1,145)	Prison (1,518)
OFFENDER CHARACTERISTICS						
Black [0-1]	.438	.346	.376	.482	.533	.576
Hispanic [0-1]	.088	.077	.088	.089	.093	.088
Female [0-1]	.113	.128	.151	.089	.037	.051
Age at Sentencing [Years]	28.011	31.529	28.018	28.637	22.491	30.143
Socioeconomic Status* [1-7]	4.434	4.071	4.357	4.511	4.704	4.530
Employed at time of Sentence* [0-1]	.455	.667	.499	.443	.290	.352
Lives with Family* [0-1]	.848	.842	.869	.834	.862	.773
Born Out of State [0-1]	.459	.520	.452	.491	.398	.465
Drug Use as Juvenile* [0-1]	.128	.056	.111	.110	.229	.174
Drug Use as an Adult* [0-1]	.119	.056	.113	.104	.136	.177
History of Mental Health Prob.* [0-1]	.258	.125	.241	.235	.338	.340
History of Alcohol Problems* [0-1]	.257	.183	.234	.302	.292	.275
CODER/PSI ESTIMATE OF						
Severity of Offense [1-99]	42.446	28.673	36.916	42.986	54.553	59.000
Offender "Badness" [1-99]	37.267	22.663	29.650	39.404	51.494	58.492
Prognosis for Rehab. [1-99]	53.699	68.070	61.512	50.405	39.283	33.725
Offender Prior Record [1-99]	30.343	16.126	22.211	33.136	43.438	53.409
PSI Writer's Prognosis [1-5]	2.778	2.342	2.346	3.091	3.559	3.587

* Composite Variable

Table 3.4
(Continued)

Means for Selected Variables
(Maximum N in parentheses)

Variable	Total Sample (11,749)	Most Severe Sanction: Fine (431)	Probation (6,224)	Jail (2,431)	Yardville (1,145)	Prison (1,518)
MOST SERIOUS CONVICTION FOR [0-1]:						
Homicide	.019	.007	.008	.013	.020	.081
Kidnapping	.001	.000	.000	.000	.001	.003
Sexual Assault	.011	.002	.004	.010	.015	.038
Robbery	.086	.007	.032	.070	.271	.217
Assault	.079	.049	.062	.108	.085	.106
Arson	.007	.000	.007	.009	.006	.003
Extortion	.002	.005	.002	.001	.001	.002
Burglary	.177	.049	.165	.190	.270	.173
Larceny	.050	.035	.051	.064	.047	.028
Stolen Vehicle	.014	.009	.013	.021	.014	.010
Forgery	.032	.021	.036	.028	.031	.024
Fraud	.070	.088	.098	.058	.005	.019
Embezzlement	.009	.019	.011	.008	.003	.005
Stolen Property	.026	.026	.028	.031	.018	.012
Damage Property	.005	.005	.007	.004	.000	.002
Drug Offenses	.221	.227	.275	.177	.138	.132
Other Sex Offenses	.018	.014	.022	.010	.010	.022
Gambling	.007	.005	.006	.006	.001	.015
Escape	.012	.023	.004	.014	.017	.035
Bribery	.002	.012	.001	.000	.000	.005
Weapons	.095	.283	.107	.095	.037	.041
All Other Crimes	.058	.116	.060	.084	.011	.028
Multiple Types	.295	.023	.226	.277	.504	.530
PRIOR RECORD MEASURES - (Arrest Histories)						
Number of Arrests	3.138	1.383	2.179	3.492	3.721	6.569
Number of Charges	4.950	2.183	3.291	5.332	6.279	10.919
Number of Convictions (Charges)	2.405	1.125	1.515	2.640	3.074	5.535
Number of Convictions (Arrests)	1.770	.752	1.152	1.968	2.205	3.945
Days on Probation	70.703	31.722	51.245	80.560	118.718	109.551
Days in Jail	24.308	12.239	13.789	33.988	23.645	55.861
Days in Prison	156.041	47.258	65.171	138.130	149.463	593.148

Table 3.4
(Continued)

Means for Selected Variables
(Maximum N in parentheses)

Variable	Total Sample (11,749)	Most Severe Sanction: Fine (431)	Probation (6,224)	Jail (2,431)	Yardville (1,145)	Prison (1,518)
PRIOR RECORD MEASURES - (Centerfold)						
Number of Arrests	6.649	3.719	5.094	7.441	8.767	10.991
Number of Charges	10.894	6.028	8.160	11.963	15.081	18.615
Convictions as an Adult (Arrests)	2.666	1.367	1.913	3.039	2.981	5.283
Incarcerations as an Adult	1.146	.490	.603	1.350	1.331	3.090
Total Convictions (Arrests)	3.338	1.571	2.379	3.764	4.470	6.237
Total Incarcerations	1.315	.538	.696	1.528	1.710	3.436
COUNTY IN WHICH CASE WAS HEARD [0-1]						
Atlantic	.056	.016	.036	.114	.056	.057
Bergen	.064	.104	.068	.065	.044	.050
Burlington	.074	.084	.074	.103	.046	.042
Camden	.071	.058	.082	.038	.090	.072
Cape May	.020	.035	.019	.024	.011	.019
Cumberland	.023	.030	.022	.026	.022	.024
Essex	.164	.084	.150	.209	.130	.198
Gloucester	.011	.028	.010	.011	.015	.007
Hudson	.055	.042	.057	.044	.080	.047
Hunterdon	.005	.016	.005	.005	.002	.003
Mercer	.056	.097	.050	.056	.068	.057
Middlesex	.060	.032	.075	.038	.045	.049
Monmouth	.083	.118	.077	.082	.086	.097
Morris	.022	.007	.018	.034	.017	.026
Ocean	.045	.077	.054	.040	.030	.019
Passaic	.059	.032	.050	.045	.093	.097
Salem	.019	.051	.019	.016	.018	.018
Somerset	.019	.016	.022	.004	.032	.023
Sussex	.009	.012	.010	.007	.009	.004
Union	.079	.021	.098	.028	.100	.088
Warren	.007	.039	.044	.011	.004	.003

Table 3.5

Distributions of Post-Sentence Time at Risk
By Most Serious Sanction

A) Three Year Windows
(N=11,746)

Time at Risk	Fine/Less	Probation	Jail	Prison
Less Than 1 Full Year	-	.1% (8)	.1% (2)	.7% (19)
Less Than 2 Full Years	.5% (2)	.2% (13)	.0% (1)	.8% (21)
Less Than 3 Full Years	-	.1% (6)	.1% (3)	2.0% (54)
Three Years	99.5% (429)	99.6% (6194)	99.8% (2425)	96.5% (2569)

B) Five Year Windows
(N=11,749)

Time at Risk	Fine/Less	Probation	Jail	Prison
Less Than 1 Full Year	-	.1% (8)	.1% (2)	.7% (19)
Less Than 2 Full Years	.5% (2)	.2% (13)	.0% (1)	.8% (21)
Less Than 3 Full Years	-	.1% (6)	.1% (3)	2.0% (54)
Less Than 4 Full Years	-	.1% (8)	.1% (3)	1.8% (48)
Less Than 5 Full Years	-	.1% (8)	.2% (5)	2.2% (59)
Five Years	99.5% (429)	99.3% (6181)	99.5% (2417)	92.5% (2462)

Table 3.5
(Continued)

Distributions of Post-Sentence Time at Risk
By Most Serious Sanction

C) Entire Post-Sentence Period
(N=11,749)

Time at Risk	Fine/Less	Probation	Jail	Prison
Less Than 1 Full Year	-	.1% (8)	.1% (2)	.7% (19)
Less Than 2 Full Years	.5% (2)	.2% (13)	.0% (1)	.8% (21)
Less Than 3 Full Years	-	.1% (6)	.1% (3)	2.0% (54)
Less Than 4 Full Years	-	.1% (8)	.1% (3)	1.8% (48)
Less Than 5 Full Years	-	.1% (8)	.2% (5)	2.2% (59)
Less Than 6 Full Years	-	.0% (2)	.3% (7)	7.1% (189)
Less Than 7 Full Years	.2% (1)	.1% (5)	.7% (16)	12.8% (341)
Less Than 8 Full Years	.5% (2)	.3% (18)	3.9% (94)	23.1% (616)
Less Than 9 Full Years	18.1% (78)	23.5% (1465)	55.5% (1348)	39.8% (1061)
At Least 9 Full Years	80.7% (348)	75.4% (4691)	39.2% (952)	9.6% (255)

CHAPTER FOUR

THE MEASUREMENT OF RECIDIVISM

The choice of a dependent measure (or measures) of recidivism should not be uninformed. As decisions are made concerning the measure to be analyzed, potentially unanticipated consequences for the results arise. For example, Maltz (1984, Chapter 5) demonstrates how differences in the organizational characteristics of criminal justice agencies (parole boards) can produce differences in recidivism rates that are not necessarily due to the "effectiveness" of those agencies but rather to the types of clients the agencies service and the nature of the data available to measure recidivism. His discussion is telling for it suggests that conclusions concerning the effects of various independent variables (e.g., prior record, living conditions, education) or criminal justice sanctions (i.e, prison sentences, probation) may be linked to how recidivism is measured.

The focus of this chapter is thus how to measure recidivism. Three major aspects of recidivism are investigated. The first is <u>what</u> is being tapped by the measure of recidivism. This refers to both the theoretical orientations toward recidivism discussed in Chapter Two and the empirical intercorrelations among various operational definitions of recidivistic behavior. Second, we look at the importance of <u>when</u> recidivism is measured. The discussion here focusses on the post-sentence window employed to study recidivism. As will become clear in this and later chapters, the choice of observation period can have important consequences for the substantive conclusions reached. Finally, we look at what can be called the <u>predictability</u> of recidivism measures. Here the interest is in the

correlations of various measures of recidivism with scales taken from the Criminology literature.

The exposition proceeds as follows. Under the assumption that the measurement of recidivism is comparable to the measurement of prior record, we first review the literature on measuring prior record for insights on deciding how to measure recidivism. We then present a conceptual typology for the ways in which recidivism can be operationalized. A subset of all possible recidivism measures is then selected for detailed study. The intercorrelations among them, both within and across observations windows, are presented. An alternative means of selecting recidivism measures is based upon the ability to predict that behavior. Thus, we look at how our measures are correlated with common predictive scales. Finally, we close by identifying a greatly reduced subset of measures for detailed study in later chapters.

MEASURING PRIOR RECORD

If the intent is to operationalize criminal behavior from official records, then there is little difference between measuring recidivism and prior record. Both seek to somehow combine data on arrests, convictions, or incarcerations into a summary of an individual's criminal activity. From this perspective, recidivistic behavior is distinguished from prior record mainly because the activity occurred after the intervention of the criminal justice system for some presenting offense. Thus one guide to the selection of a measure(s) of recidivism is to follow the procedures, and cautions, from the literature on operationalizing prior record.

Many of the issues raised in Chapter Three surrounding the limitations of official arrest records are also found in discussions of measuring prior record. Only a subset of an individual's criminal behavior appears on his/her official record. Arrests can be more a reflection of policing practices or harassment than the actual behavior of the offender. Convictions may represent more certainty on the part of the criminal justice system than arrests, with incarcerations indicating even greater confirmation of an offense than either arrests or convictions (Nelson, 1989). These points hold whether the focus is measuring prior record or subsequent recidivism.

Reviews of the literature on prior record reveal that it has been measured in a variety of ways and, somewhat surprisingly, there is no agreed upon definition of the "best" way to operationalize it. Some researchers have used simple dummy variables for any prior arrest, prior conviction, or prior incarceration. Others have used a dummy variable to indicate a prior arrest for a certain kind of offense. (See Welch et al., 1984 and Nelson, 1989 for brief reviews of these and other measures of prior record.) Such approaches clearly follow a binomial classification of prior record.

More sensitive measures of prior record have appeared in the form of counts of prior arrests, prior convictions, and prior incarcerations. Some studies (e.g., Murray and Cox, 1979; Schmidt and Witte, 1980) operationalize prior record in terms of rate of offending. These measurements of prior behavior are consistent with the criminal career measures of recidivism described in Chapter Two (and below).

The analogy between measuring prior record and recidivism is less complete when considering definitions employing "time." While there is a growing literature using time to recidivism as the dependent variable (e.g.,

Schmidt and Witte, 1984), comparable indicators are seldom found in measures of prior record. "Time" is loosely considered in terms of age at first arrest, first conviction, first drug use, and so forth, but these are measures of chronicity rather than prior record. Perhaps the closest analog to the failure-rate measures of recidivism comes from Nelson's (1989) suggestion to truncate prior record to a specified time frame (he uses 10 years) and then use a decay function so that recent offenses have more import than those committed earlier.

Two studies have empirically compared various ways of measuring prior record. In both, measures were contrasted in their ability to predict the sentence received by the offender.[1] Welch et al., (1984) compared eleven operational definitions of prior record ranging from a dichotomy for a prior arrest to four-point scales summarizing the offender's entire prior record. Also included were counts of prior felony convictions and prison terms. A wide range of intercorrelations among these measures was found as well. Correlations of over .90 were found between dummy variables for any prior arrest and any prior arrest for a felony offense, and between the two summary measures of prior record. Conversely, correlations under .20 were observed between the number of prison terms over one year and the arrest dummy and between the total number of prior prison terms and the arrest dummy. The remainder of correlations among the eleven measures are diverse with no

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[1]. Note that this is the exact opposite of the approach to be taken here. Ultimately, we are concerned with how the sentence is related to the measure of recidivism rather than how a measure of prior record is predictive of the sentence.

particular pattern in the correlation matrix, though about one third of the intercorrelations were under .50.[2]

Welch et al. also regressed sentence severity and sentenced to incarceration on each of the measures of prior record. Measures tapping the prior incarceration history of the offender were significantly related to the sentence received, while others tended not to be (though there were some interactions with race of the defendant). They conclude that measures of prior record are not interchangeable, especially among subgroups of offenders defined by race. As well, if the "best" measure of prior record is to be defined by the ability to predict the sentence received, then measures incorporating prior incarceration history are most appropriate.

Nelson (1989) investigated the correlations of seventeen measures of prior record with whether or not the offender was incarcerated. The observed correlations ranged from .13 to .28. Of interest is the finding that the impact of criminal history score on the sentence received was strongest for presenting offenses involving a theft component. The measure of prior record also interacted with the seriousness of the presenting offense. Prior record was most predictive of a sentence of incarceration for moderately serious presenting offenses and less predictive for the more serious and less serious instant arrests.

These studies of measures of prior record lead to some expectations concerning operational definitions of recidivism. We should not find uniform correlations between various measures of recidivistic behavior as these are not observed among varying measures of prior record. Furthermore, there is

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[2]. Even correlations as high as .90 share just over 80% of their variance in common. Thus we agree with Welch et al. that these indicators of prior record are measuring substantively different aspects of prior record.

evidence that measures of prior record are unevenly related to outcome measures such as the sentence received. Whether the converse is true (i.e., that the sentence received has differing impacts depending upon the particular measure of recidivism) is speculative, though such a finding would not be surprising given the results from these studies. Finally, the research of Welch et al. and Nelson alerts us to the possibility that interactions involving both presenting offense and offender characteristics might be found. In terms of recidivism measures, this suggests that specific operational definitions may be better predicted depending upon the offense for which the offender was sentenced or some characteristics of the offender.

Ultimately however, the literature on measuring prior record provides little guidance on <u>selecting</u> appropriate measures of recidivism. While there are many similarities between the two in terms of form of measurement and limitations on those forms, few concrete suggestions are offered. Nor are there many obvious conclusions for measuring recidivism that can be drawn from the empirical comparisons among measures of prior record. Consequently, we shift to a more theoretical approach for classifying recidivism measures.

MEASURING RECIDIVISM

Recidivism can be measured in multiple ways. As a guide to the various possibilities, we develop a general typology of recidivism measures based on their uses in the literature. The central organizing feature of the typology is the general theoretical orientation of the measure as introduced in Chapter Two. This refers to whether the binomial, criminal career, or time to failure perspective is chosen. Within any of these three approaches, there is the further classification based on <u>offense type</u>, such as property, persons,

repeat of presenting offense, and a measure of the seriousness of the specific offense(s).[3] Furthermore, recidivism measures may be differentiated by degree of failure by considering the criminal justice system's response to the behavior. Some recidivism events result in simply a new charge or arrest, others in a parole or probation revocation, and still others in a new conviction that leads to a probation sentence, jail sentence, or imprisonment. That is, offenders who recidivate may elicit differential responses on the part of the CJS ranging from being charged with an offense to being imprisoned for one. This orders the response along a severity dimension. The classification of possible logical variations of measures of recidivism are represented in Figure 4.1.

In Figure 4.1, the columns could be considered variables in a three-way (3 x 4 x 5) classification of measures in which there 60 logically possible combinations. For example, within the criminal career perspective, recidivism may be measured either in terms of counts or rates (e.g., arrests per year). By considering the degree of CJS response, counts or rates of revocations, distinct arrests, charges, convictions, jailings, or imprisonments could be constructed for any type or subset of offenses. Similarly, under the time to failure approach, recidivism measures capturing time to any combination of events (e.g., charge for a violent offense leading to an imprisonment) could also be constructed.

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[3]. It is possible to get quite specific in terms of the nature of the recidivistic offense. That is, the actual type of crime (e.g., burglary, drugs, robbery, and so forth) could be distinguished. As well, other summary measure, such as index offenses, might be used to differentiate the nature of the recidivistic crime(s). Doing so complicates an already complex discussion and we feel that the offense groupings chosen to represent this dimension of recidivism are sufficient to illustrate the points to be made.

Note that many of the common recidivism measures are actually from two of the marginal tables of the three-way classification implied by Figure 4.1. For example, binomial measures of rearrest often ignore the type of offense for which the offender was rearrested. Measuring recidivism by whether or not the offender was incarcerated often ignores the type of offense as well. Rates of post-sentence offending seldom consider the severity of the CJS response to each rearrest or new charge. Thus, the thecretical range of measures represented in Figure 4.1 is closer to 87 different measures of recidivism.

The classification of recidivism measured in Figure 4.1 results in a considerable variety of measures of failure. Since more logical possibilities exist (e.g., other offense types), it is necessary to impose restrictions on the number of actual measures to be used in the current study. We propose to systematically winnow the list using the following approach. We first choose 38 "representative" types of measures from those implied by the theoretical classification of Figure 4.1. After investigating the empirical similarities and differences among these measures, we reduce the number of measures to be studied in greater detail to thirteen. The process by which this latter reduction occurred is described below.

Our initial list of recidivism measures is shown in Table 4.1. In general, an attempt was made to balance measures of recidivism that are commonly used in the literature with a fair representation of the three general orientations toward recidivism measures (binomial, criminal career, and time to failure). Following from the binomial perspective, twelve measures have been chosen. Six pertain to whether the offender was recharged or reconvicted for a property offense, persons offense, or a repeat of the

offense for which the individual was sentenced. For these and subsequent variations, "persons" crimes were defined as rearrests or conviction for offenses from the categories of homicide, kidnapping, sexual assault, robbery, or assault, as discussed in Chapter Three. "Property" crimes were taken from crimes in the categories of arson, burglary, larceny, stolen vehicle, forgery, fraud, embezzlement, stolen property or damage to property.[4]

The use of a repeat of the instant offense as a measure of recidivism is not as common in the literature as some other measures, and its use here deserves some discussion. During informal meetings with judges we asked for the kinds of indicators of a "successful" sentence, that is, how would they measure recidivism if given the opportunity. Not surprisingly, we received a range of responses mirroring the variety of measures used in the literature (e.g., getting rearrested, committing a violent offense, incarceration due to a probation revocation). Such measures are adequately captured by those shown in Table 4.1. While there was a marked lack of consensus over the "best" measure to be used, several judges mentioned that if the offender was recharged or reconvicted for the same crime that s/he had just been sentenced, they would view that sentence as a "complete failure." Thus, in the interest of providing information suggested by practitioners, we include recidivism measures derived from a repeat of the presenting offense.[5]

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[4]. Two frequent categories of crimes, drugs and weapons, are not detailed in the types of offenses to be used. These offenses are, however, included in other recidivism measures to be investigated.

[5]. In operationally defining a "repeat" of the crime for which the offender was sentenced, we used a new charge or conviction for <u>any</u> of the offenses for which the individual was convicted of in 1976-7. Thus for those sentenced for multiple offenses (see Chapter Three) a number of different crimes would be considered a "repeat" of the presenting offense.

Also included in Table 4.1 under the binomial measures are two measures of the seriousness of the recidivism crime. One operationalizes the seriousness of the first new crime charged after the sentence, the other taps the seriousness of the first new charge leading to a conviction. (The charges at rearrest and reconviction need not be identical to one another as the first new charge may not have led to a conviction.) Wolfgang et al.'s (1985) scale was used to measure the seriousness of crimes for these and other recidivism indicators that incorporate a component of seriousness. In computing these two binomial recidivism measures, the first post-sentence (or post-release) charge/conviction charge encountered in the individual's arrest history was used. No attempt was made to distinguish among multiple charges/conviction charges on the same date and thus some measurement error may be introduced in this indicators. For those never rearrested, a value of zero was assigned.

The final group of binomial measures of recidivism to be investigated in detail are dummy variables for a rearrest on any charge (the most common measure in the literature), a reconviction for any offense, a new conviction leading to a jail term, and a new conviction leading to an incarceration in prison. For those individuals who were imprisoned, jail confinements prior to imprisonment were not considered as a rejailing. Thus, the dummy variable for a post-sentence jail term reflects new sentences to jail only and its accuracy is contingent upon the reporting of jailings in the arrest histories (see Chapter Three; Smith and Smith, 1990)

As indicated in Table 4.1, we have selected for more detailed study twenty two recidivism measures consistent with the criminal career perspective. These include simple counts of all post-sentence charges, distinct arrests, charges resulting in convictions, distinct conviction days,

and distinctions by type for persons, property, and repeat of instant offense for both charges and charge convictions. Similarly, yearly rates of recidivism, adjusted for time at risk ("street time") have been computed for the general offense types - property, persons, and repeating the presenting offense - both in terms of new charges and new charge convictions. Summary measures of rates of recidivism for charges per year at risk, arrests per year at risk, and charge convictions and arrest convictions will also be investigated. Two measures capturing the overall seriousness of the individual's recidivism career have also been selected. One sums the seriousness of all subsequent charges, the other the seriousness of all subsequent charge convictions. These two indicators, also based on Wolfgang et al.'s (1985) seriousness scale, are useful both for operationalizing the total seriousness of recidivistic crimes and for "equalizing" some of the other count variables. Offenders with many small, less serious, crimes will tend to have summary scores similar to those with fewer, more serious, offenses. Thus, these seriousness measures tend to capture more of a "burden" to society than absolute magnitude of criminal activity.

The final class of recidivism measures, time to failure variables, will be represented by indicators of four events. The first is the number of days to failure due to rearrest (on any charge). Second is the number of days to a new conviction after sentencing. The remaining measures are days to any rejailing or reimprisonment. For all of these time to failure indicators, those who did not fail were assigned the maximum possible value in the observation period, plus one day.[6]

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[6]. As discussed in the next section, the choice of a recidivism measure is complicated by the availability of different windows over which to look for any recidivistic behavior. Practically, this means that the time to failure

It should be noted that in the interest of some degree of parsimony, various combinations are not being studied here. These include such combinations as the type of offense and degree of CJS response combinations. For example, failure for a property crime resulting in an imprisonment is not examined, either at the binomial, criminal career or time to failure level. As well, days to failure for persons offenses, or property offenses are not included in the "working list." Although these omissions may be important, it is beyond the scope of this research to study all the possible measures of recidivism. Moreover, there is probably empirical redundancy between some of the specific measures that have been excluded and those that will be studied, such that their exclusion may be of little consequence.[7] [8]

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variables are defined differently for those who do not fail. For those who were not rearrested during a one year period, a value of 366 was assigned. For a three-year window, a value of 1,096 days was used and for a five-year window 1,826 indicated that the event (e.g., arrest, imprisonment, etc.) did not occur. A value of 3,496 days was used for the full post-sentence observation period which is the longest possible observation window plus one day.

[7]. One major group of CJS responses to recidivism - revocation - must be ignored entirely here. Given the arrest history data available to us, there are no records for simple revocations. We find <u>new arrests</u> for violations of parole or probation rather than records indicating that a revocation has occurred. Empirically then, the lesser response of revocation is not identifiable. Consequently, all of the possible CJS responses to the recidivistic event begin with an arrest record in the files to be used.

[8]. Also not investigated are different functional forms for these measures of recidivism. Counts and rates, in particular, are known to be skewed and Nelson's (1989) analysis of prior record indicators found that logarithmic transformations of these measures were more strongly related to the sentence received than were the variables in their unlogged form. The analyses of the present chapter investigate only measures in their raw form. In later chapters when recidivism is treated as a dependent variable, a logarithmic transformation is used for all rates.

SELECTING POST-OBSERVATION WINDOWS

The investigation of measures of recidivism is complicated by the fact that any of the forms described in the previous section can be measured at varying points in time. That is, the theoretical classification contained in Figure 4.1 contains no restrictions on the time frame during which recidivism is observed. For example, adjusted arrest rates can be computed for a one-year period after the sentence, a two-year period, and so forth. Thus, some decision needs to made concerning the width of the post-sentence observation window.

One alternative is to employ the longest post-sentence observation window available. As discussed in Chapter Three, we have an unusually long period in the present study, ranging to over nine and one half years for some offenders. There are several reasons, however, for not uncritically using the maximum window available. First, the results would not be comparable with those in the literature. Studies have usually followed the prescriptions of Waldo and Griswald (1979) to use windows of three or five years; yet many follow-up periods are as short as one year or six months.

Second, the empirical behavior of measures of recidivism over different length windows is unknown. It could be that one form of recidivism is a "better" indicator for some periods but not for others. For example, arrest-based measures may be more appropriate for shorter windows as they tap behaviors that could violate the conditions of the sentence, while conviction-based measures may be better over longer windows as they show greater confirmation of the individuals criminal activities after the sentence. The results presented below will help to answer such questions.

The final reason for not simply using the maximum available window alone is that substantive conclusions may be tied to the period over which recidivism is measured. (The results in later chapters confirm this expectation.) Even the use of time-to-failure models, where one would think that the width of the observation window is not an issue, has uncovered coefficients that vary by the period during which recidivism is investigated. For example, Linster et al (1990) broke their sample into early recidivists (rearrested within twelve weeks) and others, running separate models for each. They found some variables had different effects across the two models. Thus, care should be taken in choosing the post-sentence period during which to measure recidivism.

One possible route for the selection of observation windows is empirical. If identifiable breaks in the distribution of an indicator of recidivism can be observed over time, it would suggest natural periods to be used. Figure 4.2 shows the cumulative proportion of the sample rearrested (for any offense) over post-sentence windows defined by half-year intervals.[9] The distribution of proportion rearrested rises smoothly from the initial 15% that were rearrested during the first half-year after sentence or release and there are no obvious discontinuities in the graph.[10] This finding is consistent with other comparable graphs (See Maltz, 1984). Therefore, no

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[9]. We choose to use rearrest here as the organization of the official arrest histories is such that a new arrest precedes all other information (e.g., a new conviction or a new imprisonment). Thus all of the 38 measures of recidivism to be investigated here derive from at least one new arrest. Using rearrest alone as a surrogate for the others should be sufficient to make the points here.

[10]. The slight upward shift at the last period, labeled year '8' is an artifact of collapsing all windows of eight years or greater for convenience of presentation. Maintaining the distinctions between half-year windows within this group reproduces the smooth curve.

clear choices for post-sentence windows are suggested by the empirical distribution of rearrests.

In the absence of any clear guidance in the selection of a width for post-sentence windows, we focus on four different window widths as suggested by the recidivism literature. The shortest window used is a one-year period, the time frame most common in early recidivism and program evaluation studies (Maltz, 1984). Intermediate width windows cover both three and five years. The longest window is the maximum window which we refer to as the "full" or "nine-year" follow-up window.[11] All windows represent the maximum amount of time an offender may be on the street after the sentence. Some offenders will spend less time at risk due to subsequent jailings or incarcerations. Others will not be at risk for the full period due to serving the sentence that is the focus of this study. Reduced time at risk during a window of a given length is considered when the various rates of offending are adjusted for street time.

Given the focus on four distinct post-sentence observation periods, the 38 measures of recidivism lead to a total of 152 different variables to be considered. That is, a dummy variable for whether the individual was rearrested will be computed for each of the four window widths. These can be compared among themselves or to other recidivism measures from the same window or from different windows. To (slightly) simplify the presentation, we first

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[11]. "Nine-years" is somewhat of a misnomer here as for a few individuals sentenced to probation or fined, the maximum window is almost ten years. For some offenders sentenced to long prison terms, the maximum window is considerably shorter than nine years (see Chapter Three). We use nine years for the full window primarily to have a terminology that is consistent with that for one, three, and five year windows.

investigate how each measure correlates with itself over the four windows and then focus on the correlations among measures averaged over windows.

REDUNDANCY ACROSS THE WINDOWS

Before identifying the extent to which the width of the window produces redundant information in a measure of recidivism, we first present descriptive statistics for the full array of recidivism measures to be investigated. These are shown in Table 4.2. The logically necessary relationships between many of these measures are evident in the descriptive statistics. For all binomial recidivism variables, mean levels increase monotonically with the width of the observation window: the longer one looks for recidivism, the more likely one is to find it.[12]

Within windows, a consistent pattern emerges for the binomial recidivism measures. Arrest-based measures are always higher than conviction-based measures (as they should be). Indicators that are not offense-specific (i.e., rearrested or reconvicted on any charge) always have the highest mean. Measures based on property offenses have higher mean levels, followed by those based on a repeat of the presenting offense (which may include property crimes). Recidivism measures using violent persons crimes consistently have the lowest mean levels. These patterns clearly reflect the known differences in the prevalence of types of crimes.

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[12]. Another consequence of this monotonic increase is that the variance in all dummy variables increases with the width of the window. This is due to the fact that the means do not rise over 55% on any of the dummy recidivism indicators. If some subgroups of offenders recidivate at rates greatly above 50%, the variance of the dummy variables would <u>decrease</u> with the width of the observation window.

Recidivism measures capturing the seriousness of the offense display mean levels that are somewhat unexpected. Conviction-based measures are always lower than arrest-based measures as would be expected by plea bargaining arrangements. However, the greater the width of the post-sentence observation window, the more serious the offense that produced the first rearrest or reconviction. This same trend is found for the two summary measures of the seriousness of all recidivistic crime: The greater the period of time one looks for recidivistic behavior, the more serious the crimes observed. The steady increase in the mean levels of seriousness-based recidivism indicators could reflect the CJS response to offender behavior. Early recidivism could be in part due to violation of parole or probation or crimes such as weapons possession. These are less serious offenses that may be detected due to increased supervision shortly after sentencing or release. As supervision decreases (e.g., the parole or probation term is completed) only the more serious crimes will come to the attention of the CJS. Thus over longer observation windows the average seriousness of both the initial recidivistic act and the total crimes committed will rise.

The binomial measures tapping the highest levels of CJS confirmation of the offender's behavior also monotonically increase with the width of the window. One consequence of the sporadic nature of the reporting of jail terms to the official arrest histories is the fact that the proportion of the sample rejailed is consistently lower than the proportion reincarcerated. Fewer than 10% of the sample is rejailed during the full observation period while almost 25% are (re)incarcerated during that time.

As with the binomial measures, simple counts of recidivism events increase, on average, with the width of the observation window. This too is

to be expected as longer observation periods allow for more arrests, convictions, and so forth to accumulate. Like the binomial recidivism indicators, the count variables display increasing variance over the four window widths. Charge-based indicators are always higher than arrest-based indicators which have higher average levels than conviction-based measures. The ordering within windows of charges, property crimes, crimes that repeat the instant offense, and persons crimes is also found among these count-based recidivism measures.

Converting counts of recidivism events to rates adjusted for time at risk produces recidivism measures with properties unlike those discussed above. First, with the exception of the persons conviction rate, all of these criminal career recidivism indicators decrease with the width of the observation window. A corresponding decrease in the standard deviation is also found. Also decreasing with the width of the window is the maximum rate found in this sample.[13] This pattern of decreasing average values for the rate-based recidivism measures is to be expected. Low rate offenders, and those whose careers have terminated, will remain on the street, and at risk, during the vast majority of any observation window. For these subgroups, the average rate on any particular indicator will decrease as the width of the window increases. Conversely, high rate offenders will be (re)incarcerated so that their contribution to the average rate of the sample will remain constant. When all offenders are considered together, as they are in Table

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[13]. Note that shorter windows can produce adjusted rates that are quite high. For example, the maximum charge rate, adjusted for time at risk, is 365 for the one year window. Rates of this magnitude are reasonable in the short run as an offender can be recharged with a number of crimes soon after sentencing and incarcerated for the remainder of the window.

4.2, the mean values of rate measures must decrease with longer post-sentence observation periods.

As with the other indicators, the rate variables display a consistent ordering within windows. Charges lead to higher average rates than do arrests, and convictions based on charges have higher mean values than do those based on distinct arrest days. Property-based rates are consistently higher than those involving a repeat of the presenting offense which are, on average, greater than those using crimes against person.

Recidivism measures suggested by the time to failure literature steadily increase in both mean levels and variability with window width. These patterns are to be expected. Even though a longer observation period will allow for more individuals to fail, percentage of those who do not fail remains considerable. (See Figure 4.2.) This, in conjunction with the assignment of the width of the window plus one day to those who do not fail, guarantees that the average values on time to failure measures will increase with the width of the window.

Within windows, the time to failure indicators show the expected relationships. On average, it takes longer to be rejailed or reimprisoned than to be reconvicted. Rearrest, the necessary event for the other CJS responses to occur, takes the shortest period of time.

The intercorrelations of each of the 38 recidivism indicators, across the four different windows under study, are given in Table 4.3. In general, the smaller the difference between window widths, the higher the correlation of a measure with itself. For example, correlations between indicators measured over 3 and 5 years, or 5 and 9 years tend to be quite high, often reaching a value of over .90. The lowest correlations are found for the most

disparate windows - 1 versus 5 years and 1 versus 9 years. These patterns in the comparability of information across windows are invariant over the particular measure of recidivism.

Further consistency in the intercorrelations across the different windows is found for the binomial measures of recidivism. While there is some variability by measure, the magnitudes of these intercorrelations are remarkably similar. In terms of dependence upon the width of the observation window, the binomial measures of recidivism appear almost interchangeable.

Count-based measures are similar to the binomial variables, though the correlations tend to be a bit higher in magnitude. Notable differences are found, however, for the rate-based recidivism measures. Some (e.g., persons charge rate) tend to have "high" correlations independent of the window widths being compared. Others (e.g., arrest rate, conviction rate, and property conviction rate) have quite low correlations involving a one year window. It is not until windows of 3 years or more are considered that the correlations rise to levels that characterize the rest of Table 4.3.

Although these correlations may seem high by many social scientific standards, they do indicate from a third to a half of the variance of the majority of these recidivism measures is not shared with the corresponding variable measured over windows of different lengths. For example, the dummy for having a subsequent property offense correlates .852 between the three and five year windows, and .754 between the three and nine year windows. In terms of common variation, this means that 73% is shared between the 3 and 5 year windows and 57% between the 3 and 9 year windows. For the highest correlation in Table 4.3, that between the 3 and 5 year windows for days to rearrest, the percentage of variation in common is 93 percent.

It is hard to claim that the consideration of windows of different width produces redundant information given the results in Tables 4.2 and 4.3. Some properties of these recidivism measures, such as the orderings of mean levels of the variables, is independent of the width of the observation window employed. Yet other aspects of these measures, such as variability across offenders, are tied to the particular length of the window. In general the larger the time difference, the lower the correlation across a recidivism indicator measured at different points in time.

The observed differences are troubling as they suggest that for a given measure of recidivism, any results obtained for windows of a one length may be different than those for windows of another length. While many of the mean changes in these recidivism indicators can be predicted a priori, these changes appear to make the measure empirically more different as the post-sentence observation period increases. What is captured as recidivistic behavior shortly after sentencing can be quite different from the same behavior measured nine years later. As the number of offenders who have recidivated increases, the mix of the offenses committed changes as does the nature of recidivism as measured by rate-based indicators.

The lack of any clear and obvious redundancy in recidivism measures across windows provides little guidance in selecting a few "key" measures for a more detailed analysis or for selecting the "best" window over which to observe recidivism. In the next section, we look at the correlations across indicators to see the extent to which different operational definitions of recidivism are redundant.

REDUNDANCY ACROSS MEASURES

The task of selecting a subset of recidivism measures for a more detailed analysis would be simplified if it could be demonstrated that many of the possible measures are highly intercorrelated. Were this the case, then only those measures that were relatively unique (i.e., had small correlations with other measures) could be selected. To investigate any redundancy in the 38 recidivism measures, we present a composite correlation matrix in Table 4.4.

The correlations in Table 4.4 are averaged across the four post-sentence observation windows discussed in the previous section. That is, a 38 by 38 correlation matrix was computed for each window and then the values were averaged across matrices for presentation here. For example the correlation of .666 between dummy variables for rearrest on any charge and recharged for a property crime is the simple average of the correlation between these two measures found for one, three, five, and nine year windows.

The absolute values of the correlations in Table 4.4 range from .057 (between the adjusted persons charge rate and days to rejailing) to .928 (between the conviction rate and the charge conviction rate). The average correlation across the absolute value of all off-diagonal cells in the matrix is .392. In general, these indicators of recidivism are empirically distinct, sharing less than 10 percent of their variance in common. This suggests that it will be difficult to identify highly redundant measures based upon correlations alone.

The lowest average correlations tend to be between indicators representing the different theoretical orientations toward measuring recidivism. For example, the adjusted persons conviction rate correlates .095

with the binomial measure of recharged for a property crime. Days to reconviction correlates -.223 with the adjusted persons charge rate, and days to rejailing correlates -.093 with the seriousness of the charge at first rearrest.

Within domains of variables, distinctions by type of offense are to be found. Being recharged for a violent persons crime correlates .324 with being charged for a repeat of the presenting offense. The number of persons charges correlates .156 with the number of property convictions; the persons conviction rate .110 with the repeat PO charge rate. Even within the time to failure indicators, the observed correlations are relatively low. Days to rearrest correlates .261 with days to rejailing and .413 with days to reimprisonment.

Conversely, some (relative) redundancy is to be found among these recidivism measures. The distinction between charges and arrests produces measures that are correlated .883 for counts and .910 for adjusted rates. Charge conviction measures tend to be highly related to conviction measures correlating .907 for simple counts and .751 for adjusted rates. Note too that converting counts to rates yields indicators that tend to be more highly interrelated than the average correlations in Table 4.4. Total post-sentence arrests are correlated .612 with the adjusted arrest rate, property charges .735 with the property charge rate, and persons convictions .605 with the persons conviction rate.

Overall though, we are struck by the relatively low levels of intercorrelations among these recidivism indicators. While there is a tendency for some measures to yield redundant information, a point made in the previous section bears repeating: even the highest of these correlations

share but 80% of the variation in common, leaving 20% unique to a given indicator.

One way of reducing the information contained in these recidivism measures is to factor analyze them. A principal components factor analysis was conducted on the full matrix of 152 recidivism measures (38 indicators measured over 4 different windows). Twenty three factors meeting the conventional criterion of an eigen value greater than 1.0 emerged. The factor model does an adequate job of accounting for the variability in these recidivism measures as the lowest communality is .738 (for reimprisoned within one year). Thus some reduction of information can be achieved.

As a set, the 23 factors account for 87.4% of the variance in all of the indicators under study. After the first factor, which accounts for 37.2% of the variance, there is a marked drop in the explained variation. The second factor is responsible for 8.0% of the explained variance, with 5.7% and 5.1% attributed to the third and fourth factors respectively.

The first factor contains only criminal career measures and only those measured at the 3, 5, and 9 year windows. Representative of the variables that load highly on this factor are property convictions, total convictions, property charges, total arrests and total charges all measured by simple counts and adjusted rates. Also loading on the first factor are summary measures of the seriousness of the arrests and convictions. This primary factor is clearly the <u>volume</u> of criminal activity as measured <u>at least three years</u> subsequent to sentencing and release. Note that property crimes and all crimes are virtually indistinguishable on this dimension, probably due to the

fact that those engaging in property crime tend to commit more offenses and are charged with more crimes when arrested.[14]

The second factor is distinguished by variables tapping charge convictions for a repeat of the presenting offense after at least three years. Interestingly, dummy indicators, counts, and adjusted rates all load on this dimension. The third factor is also comprised of criminal career indicators. All adjusted rates for persons charges and charge convictions load highly on this dimension. Conviction rates and charge conviction rates load on both the first and third factors that were uncovered by this analysis.

The next factor is best labeled as an "early recidivism" dimension as it is comprised primarily of indicators computed over a one year window. Included on this dimension are binomial variables for rearrest, charged with a persons crime, reconviction, the seriousness of first rearrest and first reconviction, and time to rearrest and reconviction. Time to rearrest and reconviction over a three year window also load on this fourth factor.

The fifth factor repeats the dimension of persons crimes, this time as measured by binomial indicators and counts. Also loading on this factor are sums of the seriousness of all subsequent charges measured over three and five year windows. A reiteration of a "persons" dimension, now expressed as binomial variables and counts for convictions, after at least three years, emerges as the sixth factor.

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[14]. The actual ordering of these factors is, in part, due to the number and types of variables put into the analysis. There are considerably more representatives of the criminal career perspective than of the binomial or time to failure orientations. Thus it is not surprising that career-based indicators are found to load highly on the most important factor. It is the content rather than the actual ordering of these factors that is most instructive here.

The seventh factor is also a "persons" dimensions, this time comprised of dummy variables for rearrest on a persons charge and counts of charges. The major aspect of the eighth factor identified is the repeating of the presenting offense, this time operationalized by counts and adjusted rates. The ninth factor is also clear as all dummy variables for reimprisonment and all time to reimprisonment measures are distinguished. Mirroring this dimension is the tenth factor which is comprised of comparable variables for rejailing.

The remaining factors extracted tend to have fewer variables loading on them, but they too can be interpreted. Dimensions covering short term rates (both total and property over one year), medium term rates (both total and property over three years), and persons crimes over one year (dummy, count, and seriousness measures) are found on the eleventh, thirteenth, and fifteenth dimensions. The fourteenth dimension is a short-term property/arrest/charge factor as measured by dummy and count variables for the one year window. The seriousness of the first reconviction and the first rearrest (both measured over windows of at least three years) emerge on both the twelfth and fourteenth factors.

While the results of the factor analysis yield a relatively simple and interpretable factor structure, they also point to the complexity underlying the various measures of recidivism. Many factors are needed to adequately account for the patterns of correlations among the indicators studied here and no single factor captures the majority of common variation between the different measures. Yet, together the bivariate correlations of Table 4.4 and the descriptions of the exploratory factor analyses provide some direction for the selection of a subset of recidivism measures. However, before selecting

any subset, we investigate the associations of these measures with common predictive scales.

CORRELATIONS WITH PREDICTIVE SCALES

Knowing the relationships between recidivism measures and independent variables can provide a source of justification for choosing indicators of recidivism and the width of the window over which to observe recidivism. If it were found, for example, that one set of windows was better predicted by commonly used and cited scales this might justify the choice of a window on the grounds of predictive utility. Similarly, recidivism measures that are not well predicted may prove less useful as they may be evidence of primarily random behavior on the part of the offender.

The correlations of the 38 recidivism indicators with seven scales, over each of the four windows, are presented in Table 4.5. The seven scales used were: the Minnesota Offense Chronicity Scale, an early version of the Salient Factor Scale, a 1981 version of SFS, the INSLAW scale, the Iowa general offender risk assessment scale, the Iowa violent offender risk assessment

scale, and the Greenwood selective incapacitation scale.[15] All scales were computed using information taken from presentence investigation reports.

The four panels of Table 4.5, corresponding to the four different windows, contain generally low correlations: recidivism is rather poorly predicted by these scales. This is borne out by the average of the absolute values of the correlations which is about .220.[16] The correlations range from low values of about -.05 (days to rejailing and the Iowa General scale - with recidivism measured over a one year window) to high values around .44 (both the Salient Factor Score '81 and the INSLAW scale with days to reimprisonment measured using the full window).

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[15]. The basic components of these scales are as follows:

Minnesota: number of prior convictions
Salient Factor Score: number of prior convictions, number of prior incarcerations, age first incarcerated, presenting offense for auto theft, prior parole revocations, evidence of drug use, less than high school graduate, unemployed, and lives with family.
Salient Factor Score '81: number of prior convictions, number of prior incarcerations, offender age, served time during the past two years, in jail at arrest, and prior heroin use.
INSLAW: record of alcohol use, record of heroin use, offender age, length of criminal career, number of prior violent crimes, number of prior property crimes, number of prior drug charges, sum of prior time served, number of times on probation, and presenting offense for a violent crime.
Iowa General: type of presenting offense, number of prior violent offense charges, time on street since age 14, Wolfgang seriousness score for prior charges, record of escape, and history of substance abuse. Each is weighted following Blumstein et al. (1978:185).
Iowa Violent: uses the same items as the Iowa General scale, but weights are different. (See Blumstein et al., 1986:185.)
Greenwood: prior conviction for instant offense type, incarcerated during the preceding two years, any conviction as a juvenile, served time in a state juvenile facility, drug use as a juvenile, drug use as an adult, and employed less than fifty percent of the preceding two years.

In subsequent chapters, we break these scales into component parts using the constituent parts to predict recidivism.

[16]. Absolute values for the correlations are again used as it is the magnitude, rather than the direction, of the correlation which is of import.

These correlations provide some evidence of the differences across recidivism measures in terms of their deterministic nature. Across all scales and all windows, the simple dummy variable for rearrest has the highest average correlations corresponding to about 10% of the variance explained (the average correlation is about .33). The total number of post-sentence arrests and the binomial measure of reimprisonment are also relatively highly correlated to these scales, averaging correlations of about .32. Conversely, persons crime-based recidivism measures show relatively weak correlations with the seven scales. The adjusted persons conviction rate has correlations averaging about .10 and the adjusted persons charge rate and the number of persons convictions average correlations around .14. From these scales, little of the variation in persons-based recidivism measures can be explained.

The information in Table 4.5 is also instructive about the relationship between the width of the observation window and the prediction of recidivism. The ability of these scales to predict recidivism increases as the length of the window gets longer. The average correlations involving all seven scales monotonically increase over the four window widths. The increase in the average correlations is most dramatic between the one year and three year windows. For example, for the one year window, the INSLAW scales averages a correlation of .18 with the 38 recidivism measures and this increases to an average of .25 when recidivism is observed after three years. The increase in the average correlations between the five and nine year windows is minimal and for many of these scales the average correlations are virtually identical. Thus, using the criterion of predictability, there is little to distinguish between five and nine year post-observation windows.

Yet despite the consistencies in the correlations with these scales, there are many idiosyncracies as well. For a given measure of recidivism, one scale may offer the best prediction for one window, while a different scale may be better for another width. As an example, the 1981 Salient Factor Score has the highest correlation with the binomial indicator for being recharged with a property offense (-.2705) over a one year period, while this measure of recidivism is more highly correlated with INSLAW scale (.3518) over a three-year window. Moreover, while the results generally show that recidivism over shorter windows is more poorly predicted than over the five or nine-year windows, some measures are more strongly related at the five-year windows, and others at the nine-year windows.[17]

ASSESSING RECIDIVISM MEASURES

The findings presented in this chapter on the empirical behavior of different operational definitions of recidivism allow us to assess the advantages and disadvantages of choosing one form of recidivism over another. Several principals have emerged from our analysis that can be used to select a subset of measures for detailed analysis in subsequent chapters.

There is no one "best" measure of recidivism. Choosing a single measure of recidivism will have a number of consequences for one's analysis. First, the extent of recidivism varies considerably with the measure used. This is

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[17]. The results in Table 4.5 are also instructive as to differences in the behavior of the scales themselves. When comparing each scale in its ability to predict the various forms of recidivism, we find little substantive difference in the magnitude of the correlations. All scales show the same general ability to better predict recidivism over longer post-sentence observation periods and no one scale clearly outperforms the others, nor is one scale a consistently better predictor of a given indicator of recidivism. Consequently, we find little to recommend one scale over another.

evident in Table 4.2 where the mean level of recidivism is linked to how recidivism is operationalized. Choosing arrests over other possible CJS responses will, logically, produce higher mean levels of recidivism than other indicators. The use of counts yields still higher levels of criminal behavior, as all post-sentence arrests, convictions, and so forth comprise the indicators of recidivistic behavior. Converting these counts to rates results in still another, on average higher, measure of recidivism. Using a time-to-failure based measure provides a substantively different approach to measuring recidivism.

Second, these differences extend beyond simply the volume of recidivism that is observed. The analyses of correlations among these measures indicates that the choice of one measure over another limits the information that comprises the recidivism indicator. Some measures (e.g., total post-sentence charges) serve as acceptable surrogates for others (e.g., criminal career indicators for either total crimes or the subset based on property offenses; seriousness-based measures) while less adequately capturing what is tapped by other forms (e.g., binomial measures or indicators based on persons crime or time to failure measures). Put another way, what is being measured by a recidivism indicator can be quite different from other possible operationalizations depending upon the measure selected.

Third, the ability to predict the extent of recidivism is contingent on the measure used. The correlations found with commonly used scales (Table 4.5) indicate that the variability in recidivism across offenders that can be explained a prior is, in part, a function of the measure chosen. Recidivism measures suggested by the binomial orientation appear to have slightly higher

correlations with those following from the criminal career or time to failure orientations.

This last point is perhaps the most troubling for it bears directly upon issues of program evaluation and decision making for the identification of high rate offenders. Conclusions reached concerning whether a program "works" or a sentence is "effective" are likely to be contingent upon the form used to measure recidivism and thus what is meant by something "working" or being "effective." A CJS intervention may appear more effective when recidivism is measured simply by a binomial indicator than if it is operationalized by a more elaborate measure such as a rate or how long the offender takes to fail. Simply taking the most convenient measure available represents a choice with consequences for the conclusions of a study or evaluation. Similarly, validating risk assessment instruments using a given measure of recidivism may lead to an instrument that only poorly predicts different forms of recidivistic behavior.[18]

The theoretical orientations to recidivism thus become more than simply a convenient organizational device. As no one "best" measure of recidivism has emerged here, the choice of a particular measure takes on added importance for both the criminal behavior measured by the indicator and the consequences for the conclusions reached using it. Measuring recidivism by whether or not

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[18]. One conclusion that can be reached here is that more attention needs to be paid to the conceptual aspects of recidivism prior to the evaluation of a program or the development of a risk assessment instrument. If the purpose of a sentence is, for example, to rehabilitate the offender, then using a binomial measure for rearrest may be less appropriate than a criminal career based indicator. The same may be said for the goal of deterrence. Thus we see the appropriate choice of a recidivism measure as ultimately linked to policy considerations surrounding the goals of a sentence. Much more conceptual work is needed here to link the goals of sentencing and risk assessment to what can be considered appropriate measures of recidivism.

some event occurs, the volume of crimes committed by the recidivist, the rate at which the offender recidivates, or the time for which it takes the individual to fail makes a difference.

Recidivism as measured over a one-year window is clearly separable from recidivism measured over longer periods of time. The average bivariate correlations among the different recidivism measures (Table 4.3), the results of the factor analysis, and the correlations with predictive scales (Table 4.5) consistently demonstrate that what is measured as recidivism over a short period of time is empirically distinct from that behavior observed over longer windows. This holds true across the indicators suggested by all three orientations to the measurement of recidivism. Consequently, when one measures recidivism may determine what is being measured as well.

Exactly how the width of the post-sentence observation window is linked to the content of what is tapped by a recidivism measure appears to depend upon the orientation of the measure itself. We have seen that a given measure of recidivism does not correlate highly with the same measure computed over a longer window and the greater the time period between windows, the lower the correlation. One explanation for this is the finding, supported by Figure 4.2, that the longer the post-sentence window, the greater the number of offenders who will ultimately fail. Given this, there must necessarily be lower cross-window correlations for measures computed following the binomial orientation. The greater the width between windows, the greater the proportion of individuals who will have shifted from no rearrest, reconviction, reimprisonment, and so forth to having the event indicated by the binomial variable. This produces lower correlations over more disparate windows.

A substantive explanation for the decreasing correlations over greater window widths is that there are definable subpopulations of recidivists such that those who recidivate early are somehow distinguishable from those who recidivate later or not at all. Postulating distinct subgroups of offenders offers an explanation for why total counts of recidivist events also are less correlated over more disparate windows. Knowledge of the number of reconvictions within the first year, for example, is less predictive of convictions after five years because the mix of offenders with reconvictions has changed and offenders do not uniformly accrue convictions.

A more subtle conclusion concerning the effect of the window width is that the ability to predict recidivism is a function of the window used. The results are quite consistent in that the components of recidivism that can be explained by the scales studied here monotonically increase with the width of the window available. Thus while there is a clear differentiation between the one-year window and the others, there are also differences among the longer windows. To simply contrast recidivism measured at one year with that measured over one of the longer periods would ignore any differences in the deterministic aspects of recidivism and, in particular, would not allow for the detection of independent variables that might differentially predict levels of recidivism over longer window widths.

The response of the criminal justice system has marked implications for the substantive content of any recidivism measure. In one sense, this conclusion is true solely because the CJS response was one of the dimensions contained in the theoretical organization of recidivism measures offered earlier in this chapter and thus, by definition, it must be so. What is less obvious is that even when the CJS response is not explicitly considered as a

component, a given measure of recidivism is contingent upon CJS response and the greater the contingency, the less the measure reflects only the behavior of the offender.

This principal is most easily seen in the correlations of Table 4.4. As the CJS response gets more severe (or shows greater confirmation of the act using Nelson's 1989 terminology), the correlations among recidivism measures becomes lower. Thus charge-based measures correlate more highly with arrest-based measures than they do with conviction-based indicators. Conviction-based measures have higher average correlations with charge-based measures than do those based on (re)imprisonment. This pattern is confirmed in the correlations with predictive scales (Table 4.5) where a given scale correlates more highly with CJS responses of lower degree (i.e., charges or arrests) than with those of higher degree (i.e., convictions or incarcerations).

The implication here is that the higher the degree of CJS response, the less the recidivism measure taps only the criminal behavior of the offender. Conversely, the greater the degree of CJS response, the more the recidivism measure operationalizes something other than the criminal behavior of the individual. While it is obvious to state that measures such as time to reimprisonment confound the time it takes an individual to fail through rearrest with court processing time needed to lead to a new conviction (and possibly appeals) and the time to actually be imprisoned, the empirical evidence presented here shows that recidivism measures based on a high degree of CJS response are relatively poor surrogates for those based on a lower degree of response.

Clearly the choice of a single indicator measure of recidivism must therefore give serious consideration to what is to be operationalized by that

measure. As the measure involves a greater degree of CJS response, what is being measured as "recidivism" becomes conceptually more of the "cost" or "burden" to the CJS and less of the "behavior" of the individual offender. Indeed, basing the recidivism measure on a high degree of CJS response may tell us as much about the behavior of CJS actors as it does about the illegal activities of the recidivist.

Basing measures on the type of crime producing the recidivism yields substantively distinct indicators. The recidivism measures compared here have contrasted those which make no distinction concerning the type of crime with those based on only subsequent charges for property offenses, charges for violent crimes against persons, and charges that repeated the offense that led to the 1976-77 sentence. Basing recidivism measures on all types of offenses produces measures that are roughly comparable to those based on property crimes alone. This suggests that, for the present data set at least, property crimes dominate the offenses that produce recidivism. This is a reasonable expectation for, as was shown in Chapter Three, more charges tend to be leveled against property offenders than those who commit other types of crimes: The "volume" of offending tapped by general recidivism measures is clearly dominated by property offenses.

When recidivism measures are constructed on the basis of either a repeat of the presenting offense or persons crimes, they are less strongly related to either property based measures or indicators that make no distinctions among crimes. Moreover, such measures are less well predicted by the scales used in Table 4.5. Persons crime-based recidivism measures have the lowest correlations, on average, with those scales. Incorporating type of crime by a repeat of the presenting offense (which therefore includes drug and weapons

offenses) yields a recidivism measure that is more highly correlated with predictive scales, but still not as strongly as property-based indicators.

The empirical differences in offense-based recidivism measures can be interpreted as lending support to offense specialization (e.g., Blumstein et al., 1988). The repeating of the instant offense is, by definition, a form of specialization and empirically these types of measures display lower correlations with both other recidivism indicators and with the predictive scales than do other forms of measuring recidivism. The fact that measures based on persons offenses have relatively low correlations with other measures also suggests that individuals with charged with these crimes have patterns of offending that are distinct from other offenders.

The correlations of the various offense-based measures of recidivism with the predictive scales also alert us to the possibility that the understanding of recidivism will be related to the etiology of crime in general. Certain kinds of offenses appear to be more predictable than others as property based measures are much more strongly related to offender characteristics at the time of sentencing than are persons-based recidivism indicators. Thus it may be that the variables needed to determine who will be rearrested for violent crimes or the levels of those kinds of crimes are different from the variables needed to predict other forms of recidivism.

SELECTING A SUBSET OF VARIABLES FOR DETAILED ANALYSIS

In light of these findings and conclusions, it is evident that a single measure fails to capture the full range of behaviors and considerations that fall under the rubric of "recidivism." Choosing one measure over the others can result in the loss of a considerable amount of information about the

volume of an offender's crimes, the types of crimes committed, or the CJS response to the offender's illegal act. As well, it appears that a single measure of recidivism could prove misleading in that some indicators may be better predicted than others. Consequently, the analysis in subsequent chapters will use several of the recidivism indicators at our disposal.

We further delimited the number of variables to be examined in greater detail to a final list of 13. We were guided in the choice of the 13 variables by the goals of having some measures from each of the three general orientations and having variables that potentially measured different domains of recidivism as evidenced by the results presented earlier. Another major consideration was to use recidivism measures widely found in the literature.

Since the count of the total number of charges or convictions seemed to be driven by the property offenses, the special categories of property are probably largely redundant with the totals for all offense types. Thus property specific measure were dropped from further consideration. Crimes against the person dependent variables appear to represent a different dimension and thus some persons-based measures will be retained. Also, the use of convictions to operationalize recidivism yields measures that are relatively more similar to charge-based indicators, but seemingly less well predicted. This leads us to give priority to indicators using charges over those employing convictions. Finally, at least one indicator of the seriousness of the offender's post-sentence crimes was retained as these forms of recidivism are conceptually different from others, as well as relatively distinct empirically.

This reasoning resulted in the retention of the thirteen recidivism measures listed in Table 4.6. Dummy variables for rearrest on any charge, a

persons charge, a repeat of the presenting offense, or subsequent imprisonment will represent the binomial orientation to measuring recidivism. The first of these is the most commonly employed recidivism indicator, though a dummy measure for reimprisonment is often found in recidivism studies as well. The binomial indicator for a repeat of the instant offense will be the only form of this aspect of recidivism investigated in later chapters.

Seven measures will be used to represent the criminal career orientation to measuring recidivism. These are the counts of subsequent charges, charges resulting in conviction on distinct court dates, and persons charges. Rates of offending, adjusted for time at risk, will be analyzed for measures based on distinct arrest days, person charges, and total charges. These indicators will allow for the comparison of the standard criminal career parameter (the adjusted arrest rate) with similar measures based on all charges and just those involving violent crimes against persons. The final summary measure retained is the summed seriousness of all subsequent offenses. The two most frequently used time to failure variables, time to failure by arrest and time to failure by imprisonment, comprise the last of the recidivism indicators selected for analysis.

One last decision to be made concerns the window widths over which to compute these thirteen measures of recidivism. Here the choice of the one year window and the full nine year window are obvious choices. As we have seen, the former produces indicators of recidivism that are clearly different from those computed over longer post-sentence observation periods. The latter is also advantageous as it allows us to utilize all the information available. However, two concerns argue against simply using the shortest and longest windows alone. First, the descriptive statistics presented in Chapter Three

showed that the maximum effective window width is correlated with the most severe sanction received. For those sentenced to prison, 27.4% have less than seven full years at risk after release. This provides a more compelling argument for using the intermediate width windows (three or five years) as collinearity with the sentenced received is less of a concern.

Second, the results using the predictive scales indicate that there is no clear break in window width in the ability to account for variation. That is, some gain in understanding the variability in recidivism across offenders is achieved with successively longer windows. The scales used, and thus the variables comprising the components of these scales, become more strongly related to levels of recidivism as the window increases. Unknown is how individual variables are related to indicators of recidivism over observation periods of differing widths. For example, is the effect of an independent variable constant over window widths or is the effect itself a function of how long one measures recidivism? Such nuances cannot be ascertained using only the shortest and longest post-sentence observation periods.

Consequently, we will use all four windows in the analyses that follow. While this increases the comparability of the present findings with earlier studies (one, three, and five year windows are most likely to be found in prior research), maintaining the full range of windows greatly complicates the ensuing analytic task. The thirteen operational definitions of recidivism will be computed over four time frames, leading to effectively 52 different dependent variables to be investigated.

SUMMARY

We began this chapter with a discussion of the similarities between measuring prior record and recidivism from official arrest histories. Many of the conclusions reached concerning recidivism indicators would seem equally applicable to prior record measures. The conceptual orientations to recidivism (binomial, criminal career, and time to failure) appear quite serviceable for organizing the various approaches to measuring prior record. Based upon our findings, we would postulate that different prior record indicators based on the degree of CJS involvement (e.g., prior arrest versus prior convictions or incarcerations) would produce substantively different measures.[19] Distinguishing among the various offense types should produce quite distinct indicators. Similarly, the period over which prior record is measured (e.g., one year, three years and so forth) is likely to yield contrasting measures.

While a wide variety of indicators of recidivism have been compared here, we have not investigated some of the more sophisticated forms used to operationalize recidivism. These use maximum likelihood estimation to model the probability of rearrest over time (Maltz, 1984) or the probability that the offender will terminate his or her criminal career in conjunction with the offender's latent yearly arrest rate (Golub, 1990). Like survival time models of recidivism (Schmidt and Witte, 1984) the basic measure of recidivism is the time it takes for some event such as rearrest, reconviction, or reimprisonment to occur.

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[19]. Indeed in the next chapter we introduce multiple indicators of prior criminal record.

Given the findings of this chapter we can offer several speculations concerning how the results from such models may different depending upon the exact measure of recidivism used. The particular event used is likely to influence the conclusions. Similar findings and coefficients are not to be expected across models using rearrest as opposed to reimprisonment or models that look at time to failure for different offense types. Coupled with this is the observation that conclusions may be influenced by the width of the window employed: the effects of independent variables are likely to change as the window width increases. Even with the more sophisticated forms of measuring and modeling recidivism we expect substantively different results when shorter windows are contrasted with longer ones.

Finally, we have not investigated the interrelationships of different functional forms for each of the recidivism measures. Simple linear, bivariate correlations have provided the main empirical evidence for our conclusions. It is possible that transforming the continuous measures of recidivism (the criminal career and time to failure variables) would strengthen the relationships among them. However, it is unlikely that doing so would change the main conclusions reached here. No single measure would emerge as clearly preferable over the others, differentiating by offense type would still yield distinct recidivism indicators, and the width of the post-sentence observation window would remain an important consideration for any measure of recidivism.

Figure 4.1
Classification of Recidivism Measures

Theoretical Classification	Offense Type	Degree
Binomial	Property	Revocation
Criminal Career	Violence	Charge/arrest
Time To Failure	Repeat Presenting Offense	Conviction
	Any offense	Jailed
		Imprisoned

FIGURE 4.2
Cumulative Proportion Rearrested
by Half-Year Windows

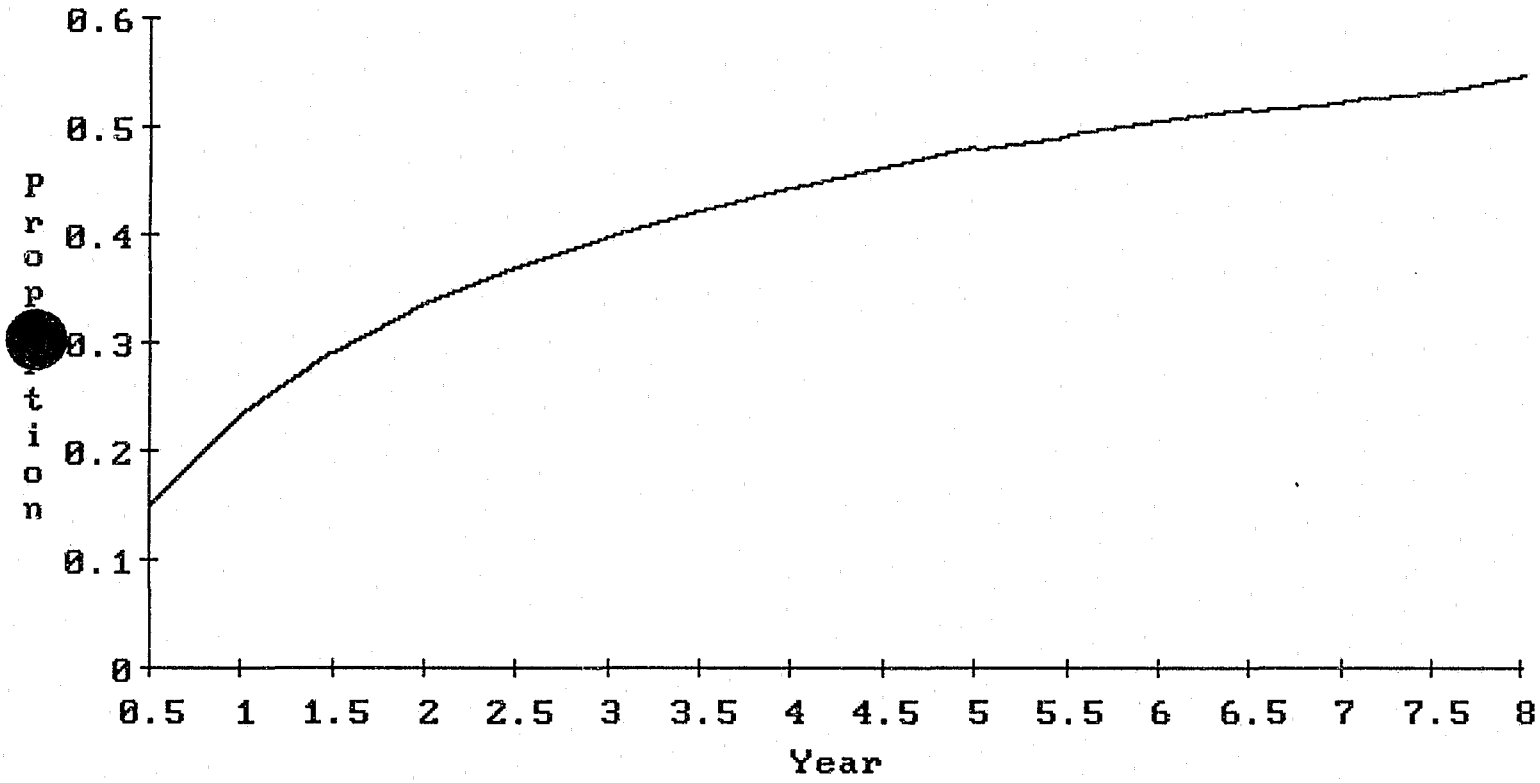

Table 4.1

Preliminary Working List of Recidivism Measures

Binomial Recidivism Measures

Most of the following are dummy variables, coded "0" or "1," depending on whether or not offender was:

Type
1. Rearrested for property offense charge
2. Rearrested for persons offense charge
3. Rearrested for repeat offense charge
4. Property offense conviction
5. Persons offense conviction
6. Repeat offense conviction

Seriousness
7. Seriousness of first subsequent offense charge (not a dummy)
8. Seriousness of first subsequent offense conviction (not a dummy variable)

Degree
9. Rearrested
10. Reconvicted
11. Convicted and sentenced to Jail
12. Convicted and sentenced to Prison

Criminal Career Recidivism Measures

Variables below are counts or rate variables, the latter are adjusted for time at risk:

Counts
13. Number of charges
14. Number of arrest events
15. Number of charges resulting in convictions
16. Number of arrest events resulting in convictions
17. Number of property offenses
18. Number of persons offense charges
19. Number of repeat presenting offense charges
20. Number of property offense convictions
21. Number of persons offense convictions
22. Number of repeat presenting offense convictions

Rates by Type
23. Property offense charges per year at risk
24. Persons offense charges per year at risk
25. Repeat present offense charges per year at risk
26. Property offense convictions per year at risk
27. Persons offense convictions per year at risk
28. Repeat presenting offense convictions per year at risk

Table 4.1 (continued)

Preliminary Working List of Recidivism Measures

Criminal Career Recidivism Measures (con't)

General

29. Charges per year at risk
30. Arrest events per year at risk
31. Charges resulting in convictions per year at risk
32. Arrest events with conviction per year at risk

Seriousness

33. Summed seriousness of all subsequent convictions (not a rate)
34. Summed seriousness of all subsequent charges (not a rate)

Time To Failure Recidivism Measures

All are measured as number of days to failure:

Degree

35. Days from sentencing or release to arrest
36. Days from sentencing or release to reconviction
37. Days from sentencing or release to rejailing
38. Days from sentencing or release to reimprisonment

Table 4.2

Descriptive Statistics for Selected Measures of Recidivism

	1 Year Window (N=11,714)			3 Year Window (N=11,746)			5 Year Window (N=11,749)			9 Year Window (N=11,749)		
	Mean	SDev.	Max.	Mean	SDev.	Max.	Mean	SDev.	Max.	Mean	SDev.	Max.
Recharged-Property	.12	.33	1	.23	.42	1	.29	.45	1	.34	.47	1
Recharged-Persons	.07	.26	1	.15	.36	1	.20	.40	1	.26	.44	1
Recharged-Repeat PO	.08	.27	1	.15	.36	1	.20	.40	1	.25	.43	1
Reconvicted-Property	.08	.28	1	.16	.37	1	.20	.40	1	.24	.43	1
Reconvicted-Persons	.03	.17	1	.06	.24	1	.09	.28	1	.11	.31	1
Reconvicted-Repeat PO	.05	.21	1	.09	.29	1	.12	.33	1	.15	.36	1
Wolfgang Ser.-1st Rearr	1.43	3.74	42.3	2.51	4.75	42.3	3.05	5.09	42.3	3.53	5.33	42.3
Wolfgang Ser.-1st Recon	.85	2.73	42.3	1.58	3.62	42.3	1.99	3.99	42.3	2.35	4.30	42.3
Rearrested-Any Charge	.23	.42	1	.40	.49	1	.48	.50	1	.55	.50	1
Reconvicted-Any Charge	.15	.35	1	.28	.45	1	.34	.48	1	.40	.49	1
Jailed After Sentence	.02	.14	1	.05	.22	1	.07	.26	1	.09	.29	1
Imprisoned After Sent.	.06	.23	1	.14	.34	1	.19	.39	1	.23	.42	1
Number of Charges	.66	1.80	40	1.59	3.13	49	2.42	4.33	76	3.59	6.11	84
Number of Arrests	.37	.81	10	.91	1.60	18	1.38	2.25	19	2.03	3.23	37
Number of Charge Cnvs	.28	.86	18	.68	1.57	20	1.00	2.08	48	1.36	2.69	50
Number of Convictions	.20	.54	6	.49	1.01	11	.73	1.38	14	1.00	1.86	28
N Property Charges	.30	1.25	38	.72	2.05	47	1.10	2.88	73	1.59	3.92	81
N Persons Charges	.12	.51	9	.26	.80	19	.38	1.01	19	.56	1.32	23
N Repeat PO Charges	.14	.70	24	.31	1.04	24	.46	1.35	24	.66	1.81	29
N Property Convictions	.14	.60	18	.35	1.12	19	.52	1.51	42	.70	1.94	42
N Persons Convictions	.04	.25	8	.09	.41	8	.12	.48	8	.16	.56	8
N Repeat PO Convicts.	.06	.36	17	.14	.55	17	.20	.68	17	.26	.83	22
Property Charge Rate	.39	3.01	273.8	.35	1.36	60.83	.32	1.05	31.22	.28	.93	31.22
Persons Charge Rate	.15	.86	52.14	.13	.70	52.14	.13	.69	52.14	.11	.64	52.14
Repeat PO Charge Rate	.16	.87	26.07	.14	.53	12.72	.13	.42	8.80	.11	.37	12.17
Property Conv. Rate	.19	1.25	91.25	.17	.73	45.63	.15	.52	16.81	.12	.42	16.81
Persons Conv. Rate	.05	.44	26.07	.05	.37	26.07	.05	.36	26.07	.04	.32	26.07
Repeat PO Conv. Rate	.08	.55	17.00	.07	.33	13.52	.06	.22	4.28	.04	.16	3.07
Charge Rate	.87	5.04	365.0	.76	2.17	91.25	.71	1.79	52.14	.63	1.59	52.14
Charge Conviction Rate	.37	1.98	121.7	.33	1.15	52.14	.30	.95	52.14	.25	.82	52.14
Arrest Rate	.50	3.85	365.0	.42	1.04	45.63	.39	.85	26.07	.34	.72	26.07
Conviction Rate	.27	1.66	121.7	.23	.76	45.63	.21	.56	26.07	.17	.46	26.07
Sum Wolfgang Ser.-Arr.	3.81	11.12	224.0	9.25	18.86	270.3	14.10	25.18	270.6	21.07	35.01	354.6
Sum Wolfgang Ser.-Conv.	1.54	5.39	115.4	3.73	9.45	146.4	5.47	11.91	146.4	7.47	15.04	191.5
Days to Rearrest	315.34	104.99	366	804.45	405.15	1096	1211.42	725.33	1826	2005.17	1471.3	3496
Days to Reconviction	335.13	84.62	366	903.12	350.92	1096	1404.79	652.05	1826	2433.61	1387.7	3496
Days to Rejailing	362.41	30.23	366	1065.68	147.32	1096	1748.62	305.86	1826	3276.37	733.8	3496
Days to Reimprisonment	356.38	46.71	366	1011.70	238.20	1096	1622.41	478.09	1826	2932.65	1101.2	3496

Note: Minimum value for all binomial and criminal career recidivism measures is 0 (the offender did not get rearrested). For all time to failure variables the minimum is one day.

Table 4.3

Correlations of Selected Recidivism Measures Across Windows

	1 vs 3	1 vs 5	1 vs 9	3 vs 5	3 vs 9	5 vs 9
Recharged-Property	.6857	.5486	.5178	.8516	.7538	.8851
Recharged-Persons	.6662	.5539	.4733	.8313	.7103	.8545
Recharged-Repeat PO	.6903	.5881	.5130	.8514	.7426	.8723
Reconvicted-Property	.6894	.5958	.5363	.8629	.7768	.9002
Reconvicted-Persons	.6878	.5783	.5006	.8405	.7276	.8659
Reconvicted-Repeat PO	.6854	.5900	.5313	.8602	.7747	.9007
Wolfgang Ser.-1st Rearr	.7030	.6160	.5538	.8756	.7891	.9016
Wolfgang Ser.-1st Recon	.6908	.5987	.5292	.8633	.7647	.8863
Rearrested-Any Charge	.6774	.5749	.5002	.8486	.7384	.8702
Reconvicted-Any Charge	.6727	.5725	.5079	.8501	.7532	.8861
Jailed After Sentence	.5896	.4936	.4356	.8375	.7378	.8809
Imprisoned After Sent.	.6138	.5120	.4480	.8337	.7298	.8752
Number of Charges	.7439	.6344	.5443	.8783	.7702	.8885
Number of Arrests	.7404	.6473	.5673	.8977	.7935	.9086
Number of Charge Cnvs	.7062	.6177	.5503	.8780	.7828	.9114
Number of Convictions	.7274	.6308	.5612	.8881	.7932	.9160
N Property Charges	.7714	.6425	.5537	.8627	.7572	.8883
N Persons Charges	.7113	.6051	.5098	.8493	.7341	.8624
N Repeat PO Charges	.7633	.6434	.5292	.8779	.7508	.8696
N Property Convictions	.7042	.6247	.5539	.8747	.7794	.9082
N Persons Convictions	.6599	.5825	.5176	.8763	.7788	.8885
N Repeat PO Convicts.	.7433	.6468	.5633	.8830	.7914	.9038
Property Charge Rate	.3708	.3679	.3293	.7343	.6515	.8991
Persons Charge Rate	.8509	.7433	.7281	.8949	.8598	.8691
Repeat PO Charge Rate	.7317	.5983	.5128	.8558	.7261	.8384
Property Conv. Rate	.4066	.4369	.3930	.7273	.6500	.9012
Persons Conv. Rate	.8278	.7516	.7263	.9298	.8935	.9387
Repeat PO Conv. Rate	.6183	.5136	.4402	.8098	.7028	.8961
Charge Rate	.3525	.3367	.3054	.7788	.6756	.8455
Charge Conviction Rate	.4914	.4806	.4429	.8532	.7651	.9228
Arrest Rate	.2089	.1981	.1818	.7589	.6840	.8448
Conviction Rate	.3145	.3307	.2970	.7333	.6593	.8882
Sum Wolfgang Ser.-Arr.	.7221	.6154	.5237	.8727	.7579	.8787
Sum Wolfgang Ser.-Conv.	.6753	.5915	.5237	.8759	.7705	.8986
Days to Rearrest	.8418	.7413	.6262	.9626	.8528	.9501
Days to Reconviction	.8314	.7286	.6133	.9615	.8593	.9488
Days to Rejailing	.7709	.6363	.5125	.9411	.8207	.9436
Days to Reimprisonment	.7633	.6442	.5252	.9512	.8342	.9408

Note: All correlations are significant at the .01 level. N for correlations involving 1 year windows is 11,714. For correlations involving 3 year windows, N is 11,746. Correlations between 5 and 9 year windows are based on a sample size of 11,749.

Table 4.4

Correlations Among Selected Measures of Recidivism -
Averaged Across One, Three, Five, and Nine Year Windows

	1)	2)	3)	4)	5)	6)	7)	8)	9)	10)	11)
1) Rearrested-Any Charge	1.000	.666	.517	.522	.529	.319	.392	.641	.529	.752	.251
2) Recharged-Property	.666	1.000	.291	.442	.746	.203	.334	.292	.345	.616	.237
3) Recharged-Persons	.517	.291	1.000	.324	.231	.557	.201	.625	.436	.407	.151
4) Recharged-Repeat PO	.522	.442	.324	1.000	.375	.198	.680	.303	.301	.472	.164
5) Reconvicted-Property	.529	.746	.231	.375	1.000	.193	.415	.213	.366	.704	.284
6) Reconvicted-Persons	.319	.203	.557	.198	.193	1.000	.235	.338	.606	.424	.153
7) Reconvicted-Repeat PO	.392	.334	.201	.680	.415	.235	1.000	.193	.332	.522	.181
8) Wolfgang Ser.-1st Rearr	.641	.292	.625	.303	.213	.338	.193	1.000	.537	.437	.116
9) Wolfgang Ser.-1st Recon	.529	.345	.436	.301	.366	.606	.332	.537	1.000	.703	.192
10) Reconvicted-Any Charge	.752	.616	.407	.472	.704	.424	.522	.437	.703	1.000	.306
11) Jailed After Sentence	.251	.237	.151	.164	.284	.153	.181	.116	.192	.306	1.000
12) Imprisoned After Sent.	.384	.376	.319	.303	.382	.316	.298	.247	.330	.425	.109
13) Number of Charges	.600	.606	.457	.500	.569	.327	.414	.349	.396	.605	.289
14) Number of Arrests	.683	.658	.477	.505	.621	.330	.425	.403	.433	.669	.359
15) Number of Charge Cnvs	.518	.539	.356	.413	.649	.414	.476	.286	.462	.689	.327
16) Number of Convictions	.570	.579	.356	.434	.691	.382	.487	.307	.498	.759	.378
17) N Property Charges	.407	.611	.197	.384	.589	.146	.333	.156	.226	.446	.200
18) N Persons Charges	.398	.261	.770	.285	.212	.544	.188	.467	.392	.350	.145
19) N Repeat PO Charges	.349	.306	.190	.669	.288	.125	.561	.183	.210	.346	.152
20) N Property Convictions	.373	.540	.171	.327	.703	.147	.382	.141	.246	.496	.256
21) N Persons Convictions	.272	.183	.483	.175	.175	.852	.212	.297	.520	.361	.138
22) N Repeat PO Convicts.	.308	.285	.152	.548	.353	.166	.785	.141	.247	.410	.169
23) Property Charge Rate	.285	.428	.155	.277	.426	.132	.250	.107	.167	.321	.142
24) Persons Charge Rate	.224	.135	.435	.150	.116	.363	.104	.261	.240	.208	.064
25) Repeat PO Charge Rate	.309	.291	.186	.592	.280	.147	.510	.160	.199	.318	.132
26) Property Conv. Rate	.282	.403	.143	.251	.532	.140	.304	.104	.186	.375	.194
27) Persons Conv. Rate	.161	.095	.290	.095	.098	.506	.121	.170	.297	.214	.063
28) Repeat PO Conv. Rate	.264	.250	.148	.453	.321	.179	.673	.115	.213	.351	.142
29) Charge Rate	.378	.389	.313	.314	.379	.264	.275	.216	.265	.395	.191
30) Charge Conviction Rate	.326	.329	.251	.250	.405	.318	.301	.183	.298	.433	.191
31) Arrest Rate	.406	.404	.319	.310	.398	.261	.278	.227	.273	.419	.248
32) Conviction Rate	.350	.355	.251	.267	.428	.291	.305	.185	.305	.466	.234
33) Sum Wolfgang Ser.-Arr.	.588	.516	.622	.468	.467	.431	.365	.520	.489	.570	.258
34) Sum Wolfgang Ser.-Conv.	.482	.441	.442	.363	.515	.612	.418	.391	.662	.642	.288
35) Days to Rearrest	-.889	-.548	-.494	-.516	-.543	-.330	-.405	-.548	-.501	-.720	-.270
36) Days to Reconviction	-.675	-.585	-.382	-.455	-.680	-.411	-.508	-.377	-.625	-.899	-.304
37) Days to Rejailing	-.219	-.208	-.129	-.143	-.253	-.141	-.164	-.093	-.165	-.264	-.893
38) Days to Reimprisonment	-.327	-.323	-.271	-.257	-.335	-.271	-.255	-.200	-.272	-.362	-.084

Table 4.4 (continued)

Correlations Among Selected Measures of Recidivism -
Averaged Across One, Three, Five, and Nine Year Windows

	12)	13)	14)	15)	16)	17)	18)	19)	20)	21)	22)
1) Rearrested-Any Charge	.384	.600	.683	.518	.570	.407	.398	.349	.373	.272	.308
2) Recharged-Property	.376	.606	.658	.539	.579	.611	.261	.306	.540	.183	.285
3) Recharged-Persons	.319	.457	.477	.356	.356	.197	.770	.190	.171	.483	.152
4) Recharged-Repeat PO	.303	.500	.505	.413	34	.384	.285	.669	.327	.175	.548
5) Reconvicted-Property	.382	.569	.621	.649	91	.589	.212	.288	.703	.175	.353
6) Reconvicted-Persons	.316	.327	.330	.414	.382	.146	.544	.125	.147	.852	.166
7) Reconvicted-Repeat PO	.298	.414	.425	.476	.487	.333	.188	.561	.382	.212	.785
8) Wolfgang Ser.-1st Rearr	.247	.349	.403	.286	.307	.156	.467	.183	.141	.297	.141
9) Wolfgang Ser.-1st Recon	.330	.396	.433	.462	.498	.226	.392	.210	.246	.520	.247
10) Reconvicted-Any Charge	.425	.605	.669	.689	.759	.446	.350	.346	.496	.361	.410
11) Jailed After Sentence	.109	.289	.359	.327	.378	.200	.145	.152	.256	.138	.169
12) Imprisoned After Sentence	1.000	.437	.425	.411	.412	.357	.297	.243	.335	.284	.260
13) Number of Charges	.437	1.000	.883	.762	.759	.835	.513	.604	.652	.320	.460
14) Number of Arrests	.425	.883	1.000	.785	.859	.705	.470	.476	.667	.304	.428
15) Number of Charge Cnvs	.411	.762	.785	1.000	.907	.660	.359	.434	.843	.450	.561
16) Number of Convictions	.412	.759	.859	.907	1.000	.639	.338	.410	.757	.356	.498
17) N Property Charges	.357	.835	.705	.660	.639	1.000	.186	.517	.748	.142	.416
18) N Persons Charges	.297	.513	.470	.359	.338	.186	1.000	.205	.156	.554	.149
19) N Repeat PO Charges	.243	.604	.476	.434	.410	.517	.205	1.000	.378	.118	.682
20) N Property Convictions	.335	.652	.667	.843	.757	.748	.156	.378	1.000	.135	.505
21) N Persons Convictions	.284	.320	.304	.450	.356	.142	.554	.118	.135	1.000	.170
22) N Repeat PO Convictions	.260	.460	.428	.561	.498	.416	.149	.682	.505	.170	1.000
23) Property Charge Rate	.345	.616	.497	.490	.465	.735	.154	.378	.547	.133	.309
24) Persons Charge Rate	.243	.284	.240	.217	.188	.096	.573	.099	.083	.371	.079
25) Repeat PO Charge Rate	.302	.569	.431	.418	.385	.514	.203	.884	.374	.139	.622
26) Property Conv. Rate	.341	.514	.502	.648	.576	.584	.140	.293	.754	.137	.390
27) Persons Conv. Rate	.219	.182	.159	.270	.197	.072	.340	.058	.072	.605	.094
28) Repeat PO Conv. Rate	.300	.407	.368	.494	.432	.376	.147	.564	.448	.189	.844
29) Charge Rate	.409	.654	.553	.513	.493	.557	.366	.378	.436	.264	.303
30) Charge Conviction Rate	.370	.482	.472	.633	.555	.417	.266	.262	.520	.357	.347
31) Arrest Rate	.406	.572	.612	.519	.547	.469	.327	.302	.441	.248	.285
32) Conviction Rate	.384	.487	.524	.576	.614	.417	.248	.256	.481	.277	.317
33) Sum Wolfgang Ser.-Arr.	.422	.895	.810	.673	.672	.629	.735	.519	.498	.430	.378
34) Sum Wolfgang Ser.-Conv.	.405	.662	.687	.868	.799	.500	.466	.354	.616	.687	.448
35) Days to Rearrest	-.426	-.630	-.712	-.554	-.606	-.445	-.405	-.367	-.413	-.287	-.334
36) Days to Reconviction	-.449	-.603	-.663	-.695	-.761	-.462	-.336	-.347	-.521	-.359	-.421
37) Days to Rejailing	-.112	-.257	-.322	-.298	-.343	-.179	-.129	-.137	-.237	-.134	-.156
38) Days to Reimprisonment	-.888	-.370	-.356	-.352	-.349	-.310	-.250	-.201	-.294	-.244	-.225

Table 4.4 (continued)

Correlations Among Selected Measures of Recidivism -
Averaged Across One, Three, Five, and Nine Year Windows

	23)	24)	25)	26)	27)	28)	29)	30)	31)	32)	33)
1) Rearrested-Any Charge	.285	.224	.309	.282	.161	.264	.378	.326	.406	.350	.588
2) Recharged-Property	.428	.135	.291	.403	.095	.250	.389	.329	.404	.355	.516
3) Recharged-Persons	.155	.435	.186	.143	.290	.148	.313	.251	.319	.251	.622
4) Recharged-Repeat PO	.277	.150	.592	.251	.095	.453	.314	.250	.310	.267	.468
5) Reconvicted-Property	.426	.116	.280	.532	.098	.321	.379	.405	.398	.428	.467
6) Reconvicted-Persons	.132	.363	.147	.140	.506	.179	.264	.318	.261	.291	.431
7) Reconvicted-Repeat PO	.250	.104	.510	.304	.121	.673	.275	.301	.278	.305	.365
8) Wolfgang Ser.-1st Rearr	.107	.261	.160	.104	.170	.115	.216	.183	.227	.185	.520
9) Wolfgang Ser.-1st Recon	.167	.240	.199	.186	.297	.213	.265	.298	.273	.305	.489
10) Reconvicted-Any Charge	.321	.208	.318	.375	.214	.351	.395	.433	.419	.466	.570
11) Jailed After Sentence	.142	.064	.132	.194	.063	.142	.191	.191	.248	.234	.258
12) Imprisoned After Sent.	.345	.243	.302	.341	.219	.300	.409	.370	.406	.384	.422
13) Number of Charges	.616	.284	.569	.514	.182	.407	.654	.482	.572	.487	.895
14) Number of Arrests	.497	.240	.431	.502	.159	.368	.553	.472	.612	.524	.810
15) Number of Charge Cnvs	.490	.217	.418	.648	.270	.494	.513	.633	.519	.576	.673
16) Number of Convictions	.465	.188	.385	.576	.197	.432	.493	.555	.547	.614	.672
17) N Property Charges	.735	.096	.514	.584	.072	.376	.557	.417	.469	.417	.629
18) N Persons Charges	.154	.573	.203	.140	.340	.147	.366	.266	.327	.248	.735
19) N Repeat PO Charges	.378	.099	.884	.293	.058	.564	.378	.262	.302	.256	.519
20) N Property Convictions	.547	.083	.374	.754	.072	.448	.436	.520	.441	.481	.498
21) N Persons Convictions	.133	.371	.139	.137	.605	.189	.264	.357	.248	.277	.430
22) N Repeat PO Convicts.	.309	.079	.622	.390	.094	.844	.303	.347	.285	.317	.378
23) Property Charge Rate	1.000	.141	.489	.788	.102	.363	.774	.568	.603	.591	.471
24) Persons Charge Rate	.141	1.000	.176	.130	.802	.118	.501	.566	.454	.482	.407
25) Repeat PO Charge Rate	.489	.176	1.000	.372	.110	.650	.492	.346	.391	.336	.487
26) Property Conv. Rate	.788	.130	.372	1.000	.116	.475	.655	.702	.585	.693	.400
27) Persons Conv. Rate	.102	.802	.110	.116	1.000	.150	.388	.623	.371	.480	.244
28) Repeat PO Conv. Rate	.363	.118	.650	.475	.150	1.000	.374	.446	.352	.401	.337
29) Charge Rate	.774	.501	.492	.655	.388	.374	1.000	.751	.910	.760	.590
30) Charge Conviction Rate	.568	.566	.346	.702	.623	.446	.751	1.000	.716	.928	.432
31) Arrest Rate	.603	.454	.391	.585	.371	.352	.910	.716	1.000	.777	.526
32) Conviction Rate	.591	.482	.336	.693	.480	.401	.760	.928	.777	1.000	.435
33) Sum Wolfgang Ser.-Arr.	.471	.407	.487	.400	.244	.337	.590	.432	.526	.435	1.000
34) Sum Wolfgang Ser.-Conv.	.383	.289	.348	.490	.401	.404	.458	.568	.465	.518	.701
35) Days to Rearrest	-.329	-.245	-.342	-.329	-.185	-.302	-.425	-.373	-.454	-.400	-.604
36) Days to Reconviction	-.354	-.223	-.338	-.416	-.234	-.383	-.425	-.471	-.448	-.503	-.558
37) Days to Rejailing	-.134	-.057	-.123	-.190	-.062	-.138	-.188	-.183	-.248	-.225	-.227
38) Days to Reimprisonment	-.352	-.256	-.289	-.345	-.228	-.295	-.420	-.385	-.413	-.399	-.351

Table 4.4 (continued)

Correlations Among Selected Measures of Recidivism -
Averaged Across One, Three, Five, and Nine Year Windows

	34)	35)	36)	37)	38)
1) Rearrested-Any Charge	.482	-.889	-.675	-.219	-.327
2) Recharged-Property	.441	-.648	-.585	-.208	-.323
3) Recharged-Persons	.442	-.494	-.382	-.129	-.271
4) Recharged-Repeat PO	.363	-.516	-.455	-.143	-.257
5) Reconvicted-Property	.515	-.543	-.680	-.253	-.335
6) Reconvicted-Persons	.612	-.330	-.411	-.141	-.271
7) Reconvicted-Repeat PO	.418	-.405	-.508	-.164	-.255
8) Wolfgang Ser.-1st Rearr	.391	-.548	-.377	-.093	-.200
9) Wolfgang Ser.-1st Recon	.662	-.501	-.625	-.165	-.272
10) Reconvicted-Any Charge	.642	-.720	-.899	-.264	-.362
11) Jailed After Sentence	.288	-.270	-.304	-.893	-.084
12) Imprisoned After Sent.	.405	-.426	-.449	-.112	-.888
13) Number of Charges	.662	-.630	-.603	-.257	-.370
14) Number of Arrests	.687	-.712	-.663	-.322	-.356
15) Number of Charge Cnvs	.868	-.554	-.695	-.298	-.352
16) Number of Convictions	.799	-.606	-.761	-.343	-.349
17) N Property Charges	.500	-.445	-.462	-.179	-.310
18) N Persons Charges	.466	-.405	-.336	-.129	-.250
19) N Repeat PO Charges	.354	-.367	-.347	-.137	-.201
20) N Property Convictions	.616	-.413	-.521	-.237	-.294
21) N Persons Convictions	.687	-.287	-.359	-.134	-.244
22) N Repeat PO Convicts.	.448	-.334	-.421	-.156	-.225
23) Property Charge Rate	.383	-.329	-.354	-.134	-.352
24) Persons Charge Rate	.289	-.245	-.223	-.057	-.256
25) Repeat PO Charge Rate	.348	-.342	-.338	-.123	-.289
26) Property Conviction Rate	.490	-.329	-.416	-.190	-.345
27) Persons Conviction Rate	.401	-.185	-.234	-.062	-.228
28) Repeat PO Conv. Rate	.404	-.302	-.383	-.138	-.295
29) Charge Rate	.458	-.425	-.425	-.188	-.420
30) Charge Conviction Rate	.568	-.373	-.471	-.183	-.385
31) Arrest Rate	.465	-.454	-.448	-.248	-.413
32) Conviction Rate	.518	-.400	-.503	-.225	-.399
33) Sum Wolfgang Ser.-Arr.	.701	-.604	-.558	-.227	-.351
34) Sum Wolfgang Ser.-Conv.	1.000	-.510	-.638	-.265	-.339
35) Days to Rearrest	-.510	1.000	.781	.261	.389
36) Days to Reconviction	-.638	.781	1.000	.296	.413
37) Days to Rejailing	-.265	.261	.296	1.000	.086
38) Days to Reimprisonment	-.339	.389	.413	.086	1.000

Table 4.5

Correlations of Selected Recidivism Measures with Common Scales

a) One Year Window

	Minnesota	Salient Factor Score	Salient Factor Score '81	INSLAW	Iowa General	Iowa Violent	Greenwood
Recharged-Property	.1986	-.2250	-.2705	.2642	.1925	.1766	.2413
Recharged-Persons	.1535	-.1663	-.1839	.1889	.1550	.1540	.1760
Recharged-Repeat PO	.1482	-.1601	-.1737	.1610	.1518	.1372	.1661
Reconvicted-Property	.1708	-.1977	-.2366	.2337	.1581	.1437	.2199
Reconvicted-Persons	.1128	-.1304	-.1417	.1486	.1015	.1044	.1528
Reconvicted-Repeat PO	.1120	-.1247	-.1338	.1217	.1123	.0952	.1367
Wolfgang Ser.-1st Rearr	.1722	-.1810	-.2110	.2044	.1784	.1742	.1764
Wolfgang Ser.-1st Recon	.1527	-.1724	-.2003	.1930	.1566	.1525	.1781
Rearrested-Any Charge	.2515	-.2738	-.3166	.2976	.2558	.2469	.2774
Reconvicted-Any Charge	.2061	-.2357	-.2754	.2608	.2135	.2048	.2494
Jailed After Sentence	.0824	-.0904	-.1080	.1052	.0644	.0683	.0968
Imprisoned After Sent.	.2092	-.2334	-.2539	.2608	.1854	.1828	.2380
Number of Charges	.2069	-.2298	-.2601	.2540	.2007	.1871	.2393
Number of Arrests	.2412	-.2651	-.3095	.2982	.2439	.2316	.2744
Number of Charge Cnvs	.1847	-.2097	-.2396	.2305	.1785	.1670	.2277
Number of Convictions	.2014	-.2315	-.2703	.2565	.2050	.1943	.2461
N Property Charges	.1457	-.1717	-.1959	.1990	.1318	.1135	.1739
N Persons Charges	.1343	-.1479	-.1628	.1698	.1364	.1369	.1641
N Repeat PO Charges	.0974	-.1071	-.1067	.0997	.1017	.0871	.1071
N Property Convictions	.1410	-.1627	-.1943	.1956	.1262	.1103	.1794
N Persons Convictions	.0984	-.1098	-.1184	.1287	.0898	.0894	.1346
N Repeat PO Convicts.	.0856	-.0952	-.1024	.0909	.0834	.0676	.1080
Property Charge Rate	.0720	-.0834	-.1036	.1088	.0655	.0561	.0909
Persons Charge Rate	.1125	-.1199	-.1324	.1397	.1095	.1138	.1404
Repeat PO Charge Rate	.0963	-.1095	-.1097	.1055	.0962	.0844	.1118
Property Conv. Rate	.0942	-.1047	-.1323	.1401	.0814	.0716	.1198
Persons Conv. Rate	.0850	-.0934	-.0981	.1089	.0749	.0783	.1178
Repeat PO Conv. Rate	.0891	-.0973	-.1065	.1038	.0817	.0690	.1073
Charge Rate	.0994	-.1049	-.1256	.1241	.0907	.0858	.1139
Charge Conviction Rate	.1225	-.1224	-.1483	.1511	.1039	.1017	.1423
Arrest Rate	.0726	-.0739	-.0900	.0864	.0653	.0632	.0772
Conviction Rate	.1003	-.0968	-.1229	.1220	.0868	.0867	.1119
Sum Wolfgang Ser.-Arr.	.1959	-.2137	-.2411	.2392	.1941	.1854	.2246
Sum Wolfgang Ser.-Conv.	.1665	-.1856	-.2098	.2049	.1581	.1509	.2013
Days to Rearrest	-.2184	.2435	.2816	-.2656	-.2282	-.2201	-.2575
Days to Reconviction	-.1754	.2081	.2420	-.2326	-.1853	-.1785	-.2291
Days to Rejailing	-.0680	.0767	.0903	-.0805	-.0473	-.0527	-.0798
Days to Reimprisonment	-.1744	.1859	.2095	-.2175	-.1597	-.1561	-.1865

Table 4.5 (Continued)

Correlations of Selected Recidivism Measures with Common Scales

b) Three Year Window

	Minnesota	Salient Factor Score	Salient Factor Score '81	INSLAW	Iowa General	Iowa Violent	Greenwood
Recharged-Property	.2718	-.2809	-.3430	.3518	.2648	.2512	.2965
Recharged-Persons	.2082	-.2105	-.2451	.2403	.2025	.2054	.2155
Recharged-Repeat PO	.2027	-.1989	-.2228	.2074	.2018	.1860	.2030
Reconvicted-Property	.2318	-.2491	-.3047	.3132	.2250	.2092	.2713
Reconvicted-Persons	.1418	-.1512	-.1762	.1812	.1329	.1355	.1775
Reconvicted-Repeat PO	.1631	-.1684	-.1846	.1691	.1645	.1423	.1781
Wolfgang Ser.-1st Rearr	.2060	-.2043	-.2393	.2311	.2075	.2089	.1962
Wolfgang Ser.-1st Recon	.1953	-.2011	-.2419	.2375	.1999	.1952	.2095
Rearrested-Any Charge	.3171	-.3229	-.3815	.3717	.3260	.3189	.3225
Reconvicted-Any Charge	.2811	-.2964	-.3505	.3443	.2863	.2761	.3055
Jailed After Sentence	.1308	-.1401	-.1656	.1609	.1182	.1236	.1515
Imprisoned After Sent.	.3034	-.3395	-.3793	.3775	.2778	.2728	.3448
Number of Charges	.2904	-.2993	-.3504	.3458	.2731	.2582	.3103
Number of Arrests	.3067	-.3171	-.3796	.3692	.2965	.2831	.3256
Number of Charge Cnvs	.2414	-.2552	-.3055	.2998	.2335	.2205	.2738
Number of Convictions	.2629	-.2840	-.3383	.3310	.2621	.2482	.2962
N Property Charges	.2192	-.2309	-.2778	.2885	.1983	.1769	.2360
N Persons Charges	.1834	-.1890	-.2164	.2177	.1745	.1771	.2129
N Repeat PO Charges	.1587	-.1572	-.1627	.1452	.1585	.1395	.1555
N Property Convictions	.1867	-.1957	-.2473	.2508	.1727	.1565	.2130
N Persons Convictions	.1180	-.1241	-.1449	.1562	.1080	.1108	.1572
N Repeat PO Convicts.	.1388	-.1387	-.1561	.1352	.1362	.1140	.1510
Property Charge Rate	.1784	-.1966	-.2277	.2424	.1635	.1492	.2016
Persons Charge Rate	.1325	-.1399	-.1538	.1609	.1164	.1235	.1675
Repeat PO Charge Rate	.1647	-.1726	-.1787	.1710	.1574	.1400	.1805
Property Conv. Rate	.1493	-.1663	-.2001	.2198	.1385	.1276	.1782
Persons Conv. Rate	.0988	-.1058	-.1146	.1285	.0848	.0904	.1328
Repeat PO Conv. Rate	.1372	-.1442	-.1596	.1488	.1258	.1075	.1615
Charge Rate	.2306	-.2474	-.2801	.2892	.2136	.2075	.2652
Charge Conviction Rate	.1895	-.2073	-.2357	.2503	.1767	.1716	.2267
Arrest Rate	.2498	-.2679	-.3088	.3138	.2379	.2336	.2835
Conviction Rate	.1909	-.2138	-.2437	.2575	.1844	.1810	.2283
Sum Wolfgang Ser.-Arr.	.2776	-.2816	-.3284	.3228	.2637	.2550	.2969
Sum Wolfgang Ser.-Conv.	.2163	-.2246	-.2674	.2663	.2070	.1986	.2438
Days to Rearrest	-.3039	.3236	.3769	-.3604	-.3109	-.3022	-.3290
Days to Reconviction	-.2623	.2899	.3402	-.3292	-.2687	-.2581	-.3067
Days to Rejailing	-.1170	.1257	.1491	-.1414	-.0986	-.1042	-.1354
Days to Reimprisonment	-.2805	.3144	.3472	-.3545	-.2561	-.2510	-.3228

Table 4.5 (Continued)

Correlations of Selected Recidivism Measures with Common Scales

c) Five Year Window

	Minnesota	Salient Factor Score	Salient Factor Score '81	INSLAW	Iowa General	Iowa Violent	Greenwood
Recharged-Property	.2964	-.2960	-.3655	.3777	.2959	.2819	.3151
Recharged-Persons	.2273	-.2281	-.2676	.2673	.2263	.2302	.2378
Recharged-Repeat PO	.2225	-.2186	-.2508	.2372	.2348	.2160	.2231
Reconvicted-Property	.2596	-.2684	-.3288	.3392	.2582	.2427	.2896
Reconvicted-Persons	.1515	-.1664	-.1927	.1966	.1493	.1515	.1962
Reconvicted-Repeat PO	.1801	-.1827	-.2061	.1883	922	.1672	.1950
Wolfgang Ser.-1st Rearr	.2029	-.1947	-.2299	.2275	.2110	.2151	.1894
Wolfgang Ser.-1st Recon	.2046	-.2018	-.2458	.2463	.2118	.2110	.2148
Rearrested-Any Charge	.3346	-.3290	-.3948	.3888	.3523	.3472	.3312
Reconvicted-Any Charge	.3056	-.3109	-.3738	.3705	.3192	.3109	.3234
Jailed After Sentence	.1532	-.1590	-.1887	.1862	.1404	.1417	.1727
Imprisoned After Sent.	.3412	-.3695	-.4207	.4184	.3178	.3108	.3680
Number of Charges	.3094	-.3146	-.3742	.3723	.3000	.2842	.3307
Number of Arrests	.3243	-.3333	-.3990	.3930	.3198	.3064	.3487
Number of Charge Cnvs	.2594	-.2760	-.3301	.3258	.2587	.2459	.3014
Number of Convictions	.2795	-.3015	-.3591	.3530	.2838	.2704	.3184
N Property Charges	.2370	-.2416	-.2970	.3085	.2198	.1985	.2541
N Persons Charges	.1990	-.2088	-.2382	.2381	.1933	.1968	.2318
N Repeat PO Charges	.1767	-.1772	-.1928	.1716	.1906	.1671	.1762
N Property Convictions	.2065	-.2160	-.2696	.2720	.1968	.1805	.2367
N Persons Convictions	.1294	-.1415	-.1625	.1723	.1232	.1266	.1775
N Repeat PO Convicts.	.1482	-.1538	-.1785	.1556	.1577	.1314	.1684
Property Charge Rate	.2195	-.2318	-.2768	.2972	.1983	.1804	.2445
Persons Charge Rate	.1294	-.1352	-.1505	.1549	.1179	.1286	.1595
Repeat PO Charge Rate	.1899	-.1973	-.2147	.2027	.1892	.1681	.2045
Property Conv. Rate	.2050	-.2214	-.2666	.2801	.1904	.1770	.2405
Persons Conv. Rate	.0970	-.1045	-.1142	.1254	.0857	.0919	.1299
Repeat PO Conv. Rate	.1647	-.1735	-.1973	.1820	.1593	.1361	.1947
Charge Rate	.2653	-.2770	-.3217	.3316	.2461	.2392	.2992
Charge Conviction Rate	.2142	-.2314	-.2650	.2749	.2020	.1969	.2560
Arrest Rate	.2908	-.3067	-.3589	.3610	.2772	.2738	.3284
Conviction Rate	.2471	-.2702	-.3122	.3166	.2389	.2349	.2919
Sum Wolfgang Ser.-Arr.	.3041	-.3074	-.3605	.3581	.2968	.2872	.3254
Sum Wolfgang Ser.-Conv.	.2423	-.2555	-.3034	.3035	.2382	.2309	.2811
Days to Rearrest	-.3327	.3439	.4054	-.3917	-.3435	-.3358	-.3486
Days to Reconviction	-.2947	.3151	.3728	-.3641	-.3027	-.2926	-.3300
Days to Rejailing	-.1416	.1502	.1775	-.1716	-.1242	-.1283	-.1610
Days to Reimprisonment	-.3236	.3600	.4017	-.4046	-.2984	-.2924	-.3631

Table 4.5 (Continued)

Correlations of Selected Recidivism Measures with Common Scales

d) Nine Year Window

	Minnesota	Salient Factor Score	Salient Factor Score '81	INSLAW	Iowa General	Iowa Violent	Greenwood
Recharged-Property	.3032	-.2964	-.3663	.3792	.3023	.2883	.3153
Recharged-Persons	.2475	-.2383	-.2857	.2818	.2425	.2454	.2505
Recharged-Repeat PO	.2219	-.2203	-.2512	.2326	.2433	.2236	.2294
Reconvicted-Property	.2647	-.2720	-.3329	.3460	.2614	.2467	.2918
Reconvicted-Persons	.1545	-.1660	-.2004	.1967	.1541	.1565	.1973
Reconvicted-Repeat PO	.1740	-.1795	-.2028	.1802	.2004	.1768	.1948
Wolfgang Ser.-1st Rearr	.1900	-.1752	-.2068	.2032	.2007	.2046	.1703
Wolfgang Ser.-1st Recon	.1945	-.1884	-.2287	.2288	.2077	.2088	.1993
Rearrested-Any Charge	.3251	-.3151	-.3804	.3764	.3545	.3497	.3184
Reconvicted-Any Charge	.3041	-.3071	-.3710	.3665	.3270	.3184	.3179
Jailed After Sentence	.1587	-.1648	-.1951	.1993	.1506	.1540	.1750
Imprisoned After Sent.	.3497	-.3713	-.4254	.4204	.3286	.3229	.3704
Number of Charges	.3017	-.3034	-.3641	.3621	.2998	.2850	.3264
Number of Arrests	.3092	-.3138	-.3803	.3744	.3108	.2982	.3338
Number of Charge Cnvs	.2512	-.2658	-.3249	.3193	.2574	.2467	.2969
Number of Convictions	.2628	-.2802	-.3418	.3362	.2733	.2615	.3013
N Property Charges	.2390	-.2407	-.2975	.3102	.2253	.2039	.2588
N Persons Charges	.2063	-.2050	-.2426	.2354	.2014	.2042	.2305
N Repeat PO Charges	.1762	-.1774	-.1941	.1687	.1978	.1743	.1790
N Property Convictions	.2030	-.2114	-.2687	.2723	.1954	.1812	.2379
N Persons Convictions	.1337	-.1441	-.1707	.1698	.1266	.1314	.1820
N Repeat PO Convicts.	.1432	-.1504	-.1768	.1492	.1636	.1398	.1668
Property Charge Rate	.2186	-.2327	-.2755	.3000	.2004	.1823	.2469
Persons Charge Rate	.1186	-.1253	-.1383	.1442	.1096	.1181	.1511
Repeat PO Charge Rate	.1824	-.1925	-.2098	.2005	.1879	.1668	.2018
Property Conv. Rate	.2036	-.2199	-.2652	.2847	.1884	.1743	.2393
Persons Conv. Rate	.0888	-.0930	-.1004	.1113	.0772	.0837	.1160
Repeat PO Conv. Rate	.1679	-.1762	-.2014	.1870	.1664	.1428	.1918
Charge Rate	.2586	-.2752	-.3167	.3341	.2459	.2389	.3011
Charge Conviction Rate	.1990	-.2145	-.2463	.2613	.1885	.1842	.2374
Arrest Rate	.2916	-.3127	-.3641	.3748	.2830	.2786	.3378
Conviction Rate	.2381	-.2611	-.3034	.3132	.2323	.2278	.2813
Sum Wolfgang Ser.-Arr.	.3025	-.3010	-.3580	.3526	.3014	.2923	.3231
Sum Wolfgang Ser.-Conv.	.2397	-.2511	-.3046	.3006	.2418	.2368	.2820
Days to Rearrest	-.3526	.3523	.4195	-.4104	-.3703	-.3641	-.3563
Days to Reconviction	-.3170	.3282	.3923	-.3861	-.3314	-.3220	-.3425
Days to Rejailing	-.1589	.1670	.1968	-.1958	-.1460	-.1492	-.1788
Days to Reimprisonment	-.3579	.3886	.4397	-.4383	-.3327	-.3266	-.3881

Table 4.6

Recidivism Measures Selected for Detailed Analysis

Binomial Recidivism Measures

All are measured as dummy variables coded as "1" if the offender was:

1. Rearrested for any charge
2. Rearrested for a violent crime against persons
3. Rearrested for a repeat of the presenting offense
4. Convicted and sentenced to prison

Criminal Career Recidivism Measures

All are measured as counts or rates, the latter adjusted for time at risk:

5. Total number of charges
6. Total number of convictions
7. Total number of crimes against persons
8. Arrest events per year at risk
9. Charges per year at risk
10. Persons offense charges per year at risk
11. Summed seriousness of all subsequent charges (not a rate)

Time to Failure Recidivism Measures

These are measured as number of days to failure:

12. Days from sentencing or release to arrest
13. Days from sentencing or release to reimprisonment

SENTENCING AND RECIDIVISM

Volume Two (of Three)

Final Report to
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*Views in this document are those of the authors and do not necessarily represent the official position of the U.S. Department of Justice, The National Institute of Justice, or the New Jersey Administrative Office of the Courts.

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**This report represents a truly collaborative effort and we have listed the authors in alphabetical order.

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Vol. II

CHAPTER FIVE

PREDICTORS OF RECIDIVISM: A THEORETICALLY AND EMPIRICALLY BASED SELECTION

When convicted offenders come before the court for sentencing, they are evaluated not only in terms of the severity of their presenting offense, but also in terms of their recidivism risk to the community. As such, the judge, in cooperation with probation officer investigation reports and prosecutor recommendations, makes a decision as to the general nature of the intervention the offender will receive. Part of the decision of those involved in this process is that of determining what risk the offender presents to the community, based on what is known about him/her at the time. The basis of such decisions may be in general beliefs about what is predictive of recidivism, or on some scientific evidence that a given court uses, possibly in the form of a risk assessment instrument. As such, the judge and supporting staff must function as scientists as well as perform their other duties.

The Criminological literature provides many guides for making risk assessment decisions. In Chapter Two we discussed general concerns surrounding the prediction of recidivism, in particular which variables may, or may not, be appropriate for use in modeling recidivism from a policy perspective. In the present chapter we take a more theoretical approach. Under the assumption that recidivistic behavior is simply a form of general criminal behavior, we use theories of the etiology of crime to suggest independent variables that may be predictive of post-sentence recidivism.

Specifically, we review several theories of criminal behavior that lead us to categorize types of independent variables into six domains. Our

considerations lead us to discuss a general model of recidivism, one that is derived from various theoretical perspectives. This model, in effect, represents what recidivism behavior might be expected of an offender, based on information at the time of sentencing. How these variables are operationalized in the present data set is then discussed. We then introduce the analytic approach that is used in this, and subsequent, chapters. This approach allows for assessing the relative ability of variables from the six domains to predict recidivism. To illustrate the approach, we apply it to an analysis of the probability of rearrest, measured over post-sentence observation windows of varying widths.

Focus then shifts to an investigation of how the main effects of these independent variables are modified by important statistical interactions. In an effort to achieve parsimony, a search strategy is implemented that allows for the identification of mediating effects that are relatively robust across all available indicators of recidivism. (See Chapter Four). The explanatory power of these interactions is then tested, again using the probability of rearrest as an example. As a prelude to Chapter Nine, how these independent variables influence individual-level prediction is also discussed.

The current chapter occupies a central position in this study. In the previous chapter, many indicators of recidivism that can serve as dependent variables were identified. Here, we investigate the choice of independent variables and set forth how these variables will be employed in the remaining chapters. In general, our goal in this and subsequent chapters is to ascertain the ability of the models to predict recidivism and, in particular, to determine if these models vary across the specific thirteen measures of recidivism. This leads us to investigate whether different types of independent variables have relative strengths or weaknesses in predicting

recidivism, and to determine the relative importance of specific independent variables across the models.

The only measure of recidivism analyzed in this chapter is the probability of rearrest. In Chapter Six, all measures of recidivism are considered. In Chapters Seven and Eight, the analyses are repeated after introducing various criminal justice interventions as additional independent variables to predict rearrest (Chapter Seven) and other dependent variables (Chapter Eight).

SOME CAVEATS ON CLASSIFYING PREDICTORS OF RECIDIVISM

If recidivism is a more specific form of general criminal behavior, then there are theoretical reasons for predicting some offenders to be recidivists, regardless of what intervention the CJS implements for a given individual. It is these theories that can be used to suggest independent variables for the modeling of recidivism. A wide variety of theories are available and we shall attempt to classify variables as "representative" of some theoretical perspectives. Doing so, however, can be problematic and several caveats should be mentioned

First, theories may be classified in a number of ways according to varying degrees of abstraction. At a specific level, theories about what leads an individual to commit a crime are quite distinct. Yet as we look at the more abstract features of these theories, the distinctions blur. Thus any general classification of theoretical perspectives can highlight differing aspects of the theories discussed. Ultimately, our classification will not be exhaustive, nor identical to others' classifications, although we think that it is similar to traditional "textbook" classifications.

Second, there are specific variables that may be interpreted as measuring the same concepts across different theories. While examples of this are discussed below, we note here that it is fruitless to attempt to exclusively identify a given variable as representing only one theory. This is especially true given the level of abstraction used and the indicators available in the current data set. For example, the fact that an individual lacks a formal education could easily be taken as indicating both a lack of conventional bonds to society and as a precursor to social psychological strain. Without detailed attitudinal measures, it is difficult to attribute a lack of formal education to one theoretical perspective.

Third, not all variables known to be relevant to the prediction of criminal behavior are available in the present study (or any study). Thus all the models are misspecified in the sense that variables crucial to a particular theoretical perspective may be lacking. What is available to us are measures commonly found in CJS records <u>for all offenders</u> at the time of sentencing. While some individuals may undergo psychiatric testing, or be interviewed for attitudinal measures, this is not the norm. Our approach is to thus take information available at the time of sentencing as representative of one or more theoretical perspectives, though this can be at a cost of important variables suggested by a given perspective.

Fourth, the theoretical basis for some of the most commonly used variables is often vague or seldom discussed. Sometimes researchers make links to etiological theories discussed here, and other times such links are not made explicit. We will include variables in our general model, even though the linkages with specific theories are not always clear. That is, variables that have been empirically validated as predictive of recidivism will also be used, even if the variables cannot be associated with one of the

prominent theoretical perspectives on the etiology of crime.

Finally, some of the variables used here are objective, whereas others are subjective. Few theories in Criminology point to subjective measures of the likelihood that an individual will commit a crime. However, we have available variables that involve subjective evaluations by the law student coders and the pre-sentence investigators. These measures are included in our models as they represent the kinds of subjective decisions that judges make about offenders prior to determining the particulars of the sentence. These variables represent initial impressions of the probability that the individual recidivate and are thus relevant for our later concern with the effects of sentences on recidivism. The effects of these subjective variables must, however, be interpreted somewhat differently from those of more "objective" variables, such as social class, race, offense type, and so forth.

Although some of these problems are intractable, it is important to be aware of them. As a step toward addressing some of these issues, we present a classification of theories that meets our criteria of being reasonably reflective of the general theories that are used to predict criminal behavior and for which we have some empirical measures. We try to resist the temptation of classifying all variables into one theoretical camp or another because the status of a variable is often ambiguous. For example, "having a job" might be linked to any of a number of theoretical perspectives (as will be discussed below). Where we are aware of such ambiguities, we discuss them and their possible impact for the conclusions we draw. It will also be remembered that the logical status of the variables, as discussed in Chapter Two, suggests that conceptually proximate variables may constitute a type of variable requiring special treatment in the context of recidivism research.

Although we do not know how to resolve all of the issues and debates mentioned above, we are sensitive to them and have adapted our research strategy to an extent to explore further some of these issues. In general the analysis here has two primary components: one focuses on the effects of groups of variables (a variance partitioning strategy), while the other focusses on the effects of specific variables. The latter will be addressed by presenting regression coefficients. In this chapter these take the form of logistic regression coefficients computed as effects at the mean. All the regression analyses are done for follow-up periods varying in length of time from one half year to eight and a half years or more (referred to as "nine years"). The strategy of examining several time periods is the result of there being no agreed upon optimal time period during which to study recidivism. Also, the study of varying effects across different time periods may reveal patterns of effects of the independent variables.

THREE GENERAL TYPES OF VARIABLES

We divide the independent variables into three general types: social structural, criminal careers, and general control variables. For purposes of discussing the theories behind the specific measures, we then subdivide the first two into components. Social structural theory, which has several branches, focusses on the stratification, segmentation, and informal social control aspects of recidivism prediction. Some social structure theorists focus on the strain that unequal distributions of wealth can cause, generally leading to higher criminal activity among lower social strata (Merton, Cohen). Others focus on the variations in subcultures, particularly across racial and ethnic groups (Sellin, Miller). From the social structural perspective, it is hypothesized that criminal participation will be positively correlated with

characteristics of individuals such as being a minority or poor, and having low socioeconomic status or low social controls. Another branch of social strain theory is social ecology theory. Here, the sources of criminality are rooted in the neighborhood of the offender -- poor neighborhoods, with high rates of transition for residents, are thought likely to generate high crime rates. Also, some offenders will have developed certain social psychological symptoms of the strain, leading to alcohol abuse, mental illness and drug dependencies. These problems may further compound the strain experienced by the offender and lead to further criminal activities.

The social control theory branch also consists of several subtheories. Hirschi has emphasized a social psychological approach in which the commitment of the individual to conventional values of mainstream institutions in society provide an integrative mechanism to insure conformity. Thus, those who have achieved educationally and occupationally have "more to lose" by the commission of crime than those with low educational or occupational achievement, and would be less likely to commit crime. Similarly, those with attachments to family, either parents, or spouse, or children, would be less likely to commit crime. Commitment and attachment represent two of the primary forms of the bond of the individual to conventional society -- the greater the bond, the less likely criminal activity. As such, there is some overlap across these three general theories.

Social learning theories of crime are primarily concerned with the mechanisms through which individuals learn the motivations, rationalizations, and techniques associated with criminal activities. Those who associate with criminals or delinquents are more likely to learn these prerequisites for crime. Emphasis is thus given to crime committed by different social strata and subgroups. If criminal behavior is reinforced, it will persist. Ability

to rationalize behavior, peer group pressure to commit crime, and having an attitude favorable to the commission of crime are important aspects of social learning theory.

Criminal career paradigms focus on the prior delinquent or criminal behavior of the offenders, as well as on presenting offense characteristics and prior intervention experiences. The perspective has a somewhat different logical status from the other theories in that it essentially focuses on variations in criminal activity levels over time, and on the identification of special subgroups of offenders (e.g., high rate offenders, violent offenders, those with long criminal careers), rather than on the prediction of behavior for all potential offenders.[1] We distinguish between different types of predictor variables within the criminal career approach. Following Morris and Miller (1985), we refer to one aspect of the criminal career approach as anamnestic theory. Here, the offenders behavior in the future is presumed to resemble his/her behavior in the past: prior arrests are indicative of future arrests. The basis for this may be in psychological theory that links behavior to general personality predispositions, or to a general sociological explanation that offenders tend to find themselves in similar social contexts over time, or even to biological explanations, often associated with variables such as the age of the offender. Because this basis is often unarticulated, and may in fact overlap logically with other theoretical domains, we argue that the status of anamnestic variables may be special, relative to other theories (see, for example, Gottfredson and Hirschi, 1987; Tittle, 1987).

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[1] The status of this theory as explanatory of behavior, as opposed to purely predictive, has been challenged in the literature (Hirschi and Gottfredson, 1987), but we chose not to enter fully into this debate here and simply classify criminal career paradigm as one of the major explanatory theories of criminal behavior.

In addition to anamnestic variables, the criminal career approach has also focussed on age of onset and various delinquent career attributes in the prediction of recidivism. For example, those who begin delinquent involvements at an early age would be more likely to continue criminal activities than those who begin later in life. Finally, the prior intervention of the criminal justice system into the lives of the offender, and their responses to such intervention, are of concern to the criminal career perspective. For example, the number of prior incarcerations may be considered indicative of a propensity to commit serious criminal activity. Or, those who previously did not respond appropriately to probation or parole may be considered unlikely to do so again.

In addition to these variables, part of the criminal career concern has been with offense specialization. This is a complex issue, and we are aware that we are not doing justice to the literature on specialization by our handling of it here. Essentially we propose to look only at broad presenting offense characteristics, such as property, violence, auto theft, and drug offenses. We ignore the classification possibilities associated with past (officially recorded) criminal activities. Our main reason for doing so is to keep the analysis as simple as possible. The classification literature does not yield any simple formula for the classification of criminal careers by type of criminal activity.

Our interest in presenting offense characteristics extends to the offense's severity, as well as the legal status of the individual at the time of the offense -- are there pending charges, or detainers, and is the offender on parole or probation? Although some may question whether or not such variables will be good predictors of recidivism, they are included here in part because they are indicative of the most recent behaviors of the

individual (whereas the anamnestic variables above refer to life-long attributes of the offender). As well, characteristics of the instant offense are quite relevant for retributionist sentencing strategies.

A variable not easily classified is age. Often it has been treated as a social variable, but recent debates have challenged this classification (Hirschi and Gottfredson, 1986). We use it here as a separate variable that possibly could be defined as a sociological variable, or as some type of biological variable, or as relevant to the domain of criminal career theory. Also, separate treatment is given to subjective assessments, among them the probation officer's evaluation of the likelihood of recidivism for an individual. Since these could be based on any or all of the other criteria, initially we treat them as logically distinct from other types of variables.

Since the data here are drawn primarily from one state, we include whether or not an offender was born out of state as a control variable for the quality of our recidivism data. Those born out of state may be more likely to migrate from the state and appear not be recidivists when in fact they have simply moved to a different jurisdiction. Other controls for the quality of both the PSI and official arrest histories are also investigated. These serve as checks for the integrity of the independent variables.

There are empirical and conceptual overlaps among and between the theories discussed above. For example, many offenders have social structural and criminal career characteristics that both lead to the expectation that the offender is likely to recidivate. Similarly within each of these two domains, there is overlap. For example, offenders who have not completed high school would be expected to be among those more likely to be unemployed. The former characteristic may be better described as what Hirschi has called "commitment" (social control theory, Hirschi, 1969) and the latter labor market

participation (derived from strain theory). Similarly, number of prior arrests as an adult and number of prior convictions as an adult are arguably measuring the same or a similar anamnestic concept. Despite the possibilities of such overlaps, in the discussion below, we make an effort to link specific theoretical orientations to specific measures, as we think is reasonable, relative to the literature on recidivism. This will allow us to better discuss their general importance within a broader theoretical framework.[2]

SPECIFIC THEORIES AND THEIR MEASUREMENT

Social Structural Strain Theories

At the center of social structural strain theories is the assumption that there is a relationship between social position -- particularly social class -- and crime. Those who are unable to achieve the symbols of economic success through conventional means feel anger and frustration toward a society that put them in their position and may resort to criminal means to achieve success or because of their frustration resort to violent behavior. These concepts are operationalized by the following distinct variables: the socioeconomic status of the offender, offender is unemployed, has no job to go to when released from custody, committed crime out of necessity, or because financial difficulty, resides in a house (versus apartment or other), is illiterate, is supported by welfare, and sells drugs for income.[3]

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[2] It should be noted that not all the variables discussed below will appear in the subsequent analyses -- many were dropped after an empirical examination revealed that they failed to be systematically related to recidivism, as measured in multiple ways. The selection procedure used to reach this conclusion will be described shortly. Those variables meeting the inclusion criterion are asterisked in Tables 5.1 and 5.2.

[3] The source of data for all representatives of social structural theories was the Presentence Investigation Report (see Chapter Three). At times, there is a one-to-one correspondence between a measure on that form and the variables used (e.g., financial necessity caused the presenting crime.) Other times. our

Four socioeconomic classes were used in the current analysis: professional, white collar, skilled blue collar, and unskilled blue collar. As can be seen in Table 5.1, the status of the offenders in the sample is primarily working and lower middle class. The modal category is unskilled blue collar. Approximately 35% of the sample were unemployed at time of arrest, while only 46% were known to have a job to return to after sentencing. Only about 4% of the sample were judged (by the PSI writer) to have committed the crime out of financial necessity, yet 31% were judged to have some financial problem as a motivation for their crime. Over a quarter of the sample was receiving welfare benefits of some kind. Most did not live in a house, but in an apartment or other form of housing. Approximately 13% of them were known to have sold drugs, probably most of those sold drugs to help supplement their earnings. In brief, the sources of structural strain are plentiful in the sample.

Cultural Conflict Theories

Another variant of social structural theories is cultural conflict theory. According to its advocates, there are cultural values and traditions

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measures are a composite based upon several items taken from the PSI. The computation of several of these measures (e.g., socioeconomic status) is detailed in Botsko et al., 1990. A few undocumented variables (e.g., offender sells drugs) were constructed with reference to multiple variables. If any showed evidence of the phenomenon to be measured, the characteristic (e.g., selling drugs) was taken as being present.

Missing data are not a major problem for the independent variables to be presented here. In part, this is due to the composite nature of many of our measures. But this is also due to the fact that some concepts -- criminal history of family members is a good example -- had measures with too many missing cases and thus could not be considered. For the dummy measures listed in Tables 5.1 and 5.2, missing cases were coded as '0' or "not present." The rationale for this is that it reflects that way the information is used by the judge when sentencing the offender. Information that is not known is treated as if a characteristic (e.g., being on welfare) is not present. Continuous variables with missing values were assigned the mean. Consequently, all 11,749 cases are used for the ensuing analysis. For no variable were more than about 8% of cases missing prior to substitution.

of being "male", of the lower classes and minority groups, which "call for" or elicit the breaking of the rules of conventional society. Non-conventional values are handed down from one generation to another. Obedience to the cultural norms of these classes or groups puts one at risk of violating conventional laws. We include here the role expectations associated with gender, in that it is widely held that role expectations are different for men than women. According to culture conflict theorists, such differences are partly responsible for the differences observed in male-female participation rates.

These concepts are measured here by the following: offender is male, black, Hispanic, uses an alias, was uncooperative with authorities since arrest, had hostile attitude at crime, has illegitimate children, used judgment in accordance with values in committing crime, and the degree of "badness" estimated by the coder upon reading over the PSI report.

Although most offender sentenced are male, about 11% are female (Table 5.1). Half of the sample are either black (44%) or Hispanic (7%), though predominantly the former. (The latter may be an underestimate in that Hispanics are not well-identified as such in the CJS.) As many as 15% of the sample is known to have used an alias. About 20% were coded as showing no remorse for their crime, although most (75%) were cooperative with authorities at and after arrest. The overall "badness" rating was 27.44 on a scale of 1 to 99, where 99 was the "worst" offender. About 19% of the offenders were judged to have committed their crimes out of "bad judgement" rather than a long held criminal motivation or orientation, or other reason. Finally, the average offender had .419 illegitimate children, seemingly a somewhat high average. It is perhaps reflective of a failure to define legal marriage as important or to "bother" with fulfilling the "technical" obligations of

legally marrying.

Social Ecological Theories

The social ecology approach of the Chicago School has been linked to social control theory as well as social structural strain theory. In neighborhoods where people demonstrate concern for the quality of life, have organized activities for youth, and sanction deviant behavior, criminal activities are discouraged. In socially disorganized communities, social control mechanisms break down, and criminal activities flourish. Living in environments with low social control is measured by the following: offender lives in an urban neighborhood, lives in a poor neighborhood, and years of residency in home neighborhood, and has not lived in most recent home more than a year.

Most offenders live in a suburban or rural environment in New Jersey, and not strictly from urban areas. The majority (59%) are from poor neighborhoods. About 15% had moved with the past year, and the time at the same address was quite lengthy, averaging about 27 years.

Social Psychological Strain Theories

Some offenders have not coped well with strain in the past, leading them to develop various social-psychosocial problems that exacerbate their lives and cause them further strain. Alcoholism, drug dependency, and mental illness are three such symptoms of strain. We measure the following aspects of these symptoms: offender has a history of mental problems, has an alcohol problem, has a drug problem, has been in treatment for alcohol or drug abuse, offender abused as a child, offender a known user of drugs, has needle marks, has health problems that led to crime, has emotional problems linked to crime, had an inadequate upbringing as a child, and birth order.

The extent of the social psychological problems in the current sample is substantial. Over a quarter have a history of some mental problems or treatment. A quarter have alcohol problems; and a quarter drug problems. About 17% received treatment in the past for drugs or alcohol. Almost 8% of the sample were abused as children, and 7% were noted as having needle marks from drug use. Although health problems were not frequently identified as the cause of the presenting offense, emotional problems are cited in 11% of the cases. Family upbringing was not often mentioned as a cause of the PO crime either. Birth order was quite low, averaging 4.43 and thus these offenders tended to come from large families.

Social Bond Theories

The degree of attachment of individuals to conventional others and the commitment of the individual to traditional institutions of society, such as education and the economy, form the basis for social control theories of crime. Either the attachment or the commitment (or both) is too costly for the would-be offender to risk via criminal activity. So those with such bonds are less likely to commit crime. Here these concepts are measured by the following: educational attainment (grade achieved), school dropout, having a steady job, number of jobs, living with family, having children, supporting a family, religious participation, and participation in clubs.

The average offender had a tenth grade education -- 62% had dropped out of school before completing high school. Only 36% had a steady job. Job changing was very frequent over the course of the past five years, averaging about four changes a year. Most offenders lived with spouse or family, although most were not married (70%). The average number of children was 1.24. Only about a quarter of the sample was actively supporting children. Approximately 22% were known to attend church recently or in the past, and

another 11% attended a club of some form.

Social Learning Theory

Social learning theory has focussed on the peer group influence on the potential offender, as well as on the favorable definitions toward crime held by offenders. These include the motivations, rationalizations and know-how to commit crimes. The present data do not contain direct measures of these concepts. Rather, it is necessary to rely upon variables more distantly related to the theory. We use mostly social characteristics of the offense to provide clues as to the prior "criminal" socialization of the offender. In particular: offender had bad companions that led him to crime, offender was ringleader of groups committing crime, offender victimized a stranger, offender victimized a business, and victim was of a different race or sex.

Having bad companions, perhaps the central variable within differential association theory, was cited in only 11% of the presenting offenses as a cause of the crime. In ten percent of the cases the offender was defined as the ringleader, even though most crimes were committed with others (53%). The victim was a stranger in only about a third of the offenses, and of a different sex or race in 14% of the cases. Business establishments constituted 16% of the "victims." The location of most crimes was not predominantly on the public streets and parks: only about 38% of the crimes were identified as occurring in an "open space". In that these characteristics identify a special type of offender (one who preys on strangers, those of another race or sex, or in a public place), these variables measure such an offender.

Presenting Offense

Presenting offense characteristics include not only the nature of the offense, but also the status of the offender within the criminal justice

system at the time of the offense. As is shown in Table 5.2, relatively few of the presenting offenses are for auto theft or weapons, though almost one quarter are for drug crimes. Not surprisingly, the modal category is "property offense." Offenders frequently possessed a weapon (28%) but were not as frequently charged (15.6%). The average dollar value of stolen or damaged property was $359 though this variable is quite skewed by some large dollar amounts. On the Wolfgang scale ranging from 1 to 68, the average offense was a 7.2, and on the coder's severity scale of 1 to 99, a 42.45. At arrest, offenders frequently have other connections with the CJS in the form of pending charges (20%), detainers (6.5%), being in prison or jail (2.6% or 7.1%, depending on the source of the information and whether at the offense or the sentence), or being on probation (20%) or parole (10%). Thus, the sentence for the presenting offense was only one of many connections with the system. Drugs and alcohol were not widely used at the presenting offense (9% and 16%, respectively).

Anamnestic Theory

Prior criminal activities have been found to be good predictors of subsequent arrests: the offender's criminological future tends to resemble his/her past. These insights have been incorporated into the study of recidivism for decades, and are reflected in virtually every risk assessment instrument ever used. The term "anamnestic" is borrowed from Morris and Miller (1985), and refers to future criminal behavior as a "remembrance" of past criminal behavior. We distinguish between arrests (one arrest may be for multiple charges), charges, charges convicted of, and arrests resulting in convictions (see Chapter Three).

The extensive nature of our data allow us to operationlize many aspect of previous criminal behavior. The prior offense features of the offender are

measured as follows: prior number of arrests, prior number of arrest convictions, prior number of charges convicted on, prior number of charges, prior charges in the past five years, number of FBI Part I offenses, prior property charges, prior charges for crimes against the person, prior weapons charges, prior property convictions, prior convictions for crimes against persons, prior weapons convictions, prior escape record, and no incarcerations for the past two years.[4]

The past offense and arrest behaviors of the offenders was extensive. For example, the average number of charges over the past five years was almost one per year (Table 5.2). If one examines the arrests (one arrest could be for multiple charges), the average is 3.13. Convictions are more difficult to accumulate, resulting in an average of only 2.80. Despite this seemingly chronic criminal behavior, about 71% of the sample was not incarcerated in the two years prior to the presenting arrest -- suggestive of the skewed nature of the prior offense distributions.

Delinquent Career and Age of Onset

Those individuals who started their criminal activities in their juvenile years have been shown to be more likely to commit crimes as adults. The chronicity of juvenile offending, as well as the age of first (officially measured) criminal participation as indicated by arrest, incarceration, and drug use are often used to predict subsequent criminality as adults. We measure the following such characteristics: number of prior juvenile arrests, number of prior juvenile convictions, age at first arrest, age at first incarceration, age at first arrest for drugs, and length of criminal career.

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[4] These measures of adult prior record were taken from either the official arrest histories or from information contained in the PSI "centerfold" (see Chapter Three). When either source could be used to compute a count of prior charges/arrests, the larger of the two values was used to resolve any discrepancy.

The average number of juvenile arrests was 1.35, while the average number of convictions was only .30 (Table 5.2). The average age of first arrest was 20.9, and that of first incarceration was 21.3. The average length of the criminal career, counting juvenile years, was 6.23 years. Given the relatively large standard deviations of these onset variables, it is clear that a large proportion of the sample began their criminal involvement at an early age.

Prior CJS/Offender Action

The prior intervention of the CJS represents, on the one hand, a determination as to "how bad" the offender is, such that an intervention was deemed necessary. Alternatively, this may represent an influence on the offender's subsequent criminal activities due to factors such as deterrence, rehabilitation, or criminalization. The following indicators are available: number of prior incarcerations (jail or prison), number of prior probations, number of prior paroles, number of prior parole violations, number of prior probation revocations, prior time served in prison or jail, negative parole release decision, negative probation release decision, and overall evaluation of the offender's record by the law student coders.[5]

These offenders averaged over one prior incarceration or jailing, and a half of probation sentence per offender (Table 5.2). Between 10 and 30 percent of the sample had a prior probation (18%) or parole sentence (12%). Many were rated as not performing adequately while on their latest parole (8.5%) or probation (18.7%). Approximately 6% had their most recent parole revoked. These descriptive statistics suggest that failure to adapt to the requirements of previous supervision was not unusual and are not surprising given the nature of the sample under study.

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[5] These measures are taken from all available data sources.

General Control Variables

As discussed earlier, we also include the several variables that do not fall neatly into the previous domains. One of these is offender age. Others are either overall impressions or serve as general controls for the quality of the data available. These include: whether or not the offender was born out of state, the probation officer's global prognosis for the offender's recidivism chances, the coder's overall assessment of the offender's prior record, (again on a scale from 1 to 99), two dummy variables to tap the probation officer's recommendation for a sentence, and two controls for the general quality of the data. For the latter, one measure was an assessment of the quality of the PSI as made by the law student coders, the other assessed discrepancies between the arrest histories contained on the PSI and those taken from the official arrest records.[6]

The average offender was 28 years old, and had an evaluation score of 53.70 on a scale from 1 to 99, based on the probation officer prognosis. This falls almost exactly between a high score of "likely to rehabilitate" and a low score of "likely to recidivate." A little less than half of the offenders were born out of state. For 35% of the cases, a sentence of probation was recommended, while for 21% of the cases, the probation officer was in favor of some form of incarceration. Slightly less than 25 percent of the PSIs were deemed to be either incomplete or sketchy. Missing presenting charges on the PSI ranged from 0 to 15, with a mean of .113, though only 7% of all cases had at least one charge missing from the PSI data. See Smith and Smith (1990) for

[6] Simple counts of the number of presenting offense charges missing from the PSI were used for this purpose.

the data processing steps that led to the creation of this measure.[7]

A VARIABLE SELECTION STRATEGY

The richness of the current data set in terms of potential independent variables is both an advantage and a disadvantage. On the one hand, indicators for many of the common theories in Criminology are available. Indeed, the previous sections have identified 107 different measures consistent with those theories. On the other hand, the shear volume of independent variables works against parsimony in any model of recidivism. In the interest of obtaining a more manageable set of predictor variables, we have adopted a strategy for winnowing down this list.

One criterion used was excessive redundancy among measures. After applying the diagnostic procedures outline by Belsley et al. (1980), four variables were dropped due to severe multicollinearity with others that were retained. The anamnestic variables of total number of prior charges and prior number of convictions for property crimes were highly collinear with other counts of prior charges. Years since first arrest, a key measure from criminal careers perspective, is a virtual linear combination of age at first arrest and age at sentencing, and thus could not be used. One subjective indicator, offender "badness," was not surprisingly, highly collinear with

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[7] It should be mentioned that we also utilized the several common scales of recidivism introduced in the previous chapter. The scales used were the Minnesota Offender Chronicity Scale (referred to as the Minnesota Scale); the Salient Factor Score (SFS) Scale, derived from one of the early versions of the SFS; SFS from the year 1981; INSLAW scale (Rhodes et al., 1982); Iowa general offender risk assessment scale (Fisher, 1984); Iowa violence offender risk assessment score; and Greenwood's selective incapacitation scale (1982). Although these do not exhaust all the possible scales that could be used, they are reflective many existing scales used in recidivism research. We found that these scales did not increase our ability to predict any measure of recidivism beyond that predicted by the other variables in our analysis, suggesting that the scales add little to the prediction of recidivism beyond that provided by their components.

other measures of prior record and the presenting offense. It is clear that no information is being lost by removing these variables.[8]

Selecting from among the remaining 103 independent variables is not as straightforward. Again we relied upon an empirical strategy, but one that was designed to retain those variables most predictive of recidivism, across all definitions of the phenomenon. Our approach is based on the assumption that the most useful way to model multiple forms of recidivism is to employ identical sets of independent variables for each predictive equation. That is, the most parsimonious results can be obtained when there is a single equation, based on a common list of independent variables, that models recidivism. Given this, only the definition of recidivism (i.e., the dependent variable) will differ between models.

Consider the alternative to the single equation approach. Later in this chapter we will see that having needle marks at sentencing (a sign of a serious drug dependency problem) is not predictive of rearrest using a one year window. This variable is, however, a significant, positive predictor of recidivism over the full nine-year observation period. It is possible to selectively fashion one equation for rearrest at one year, dropping the needle marks variable from that equation, while simultaneously trimming the model for rearrest at nine years. The result would be two different models of rearrest with potentially vastly different sets of predictor variables. As the differences in the independent variables of each equation increase, it becomes more difficult to compare one to the other.

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[8] The collinearity diagnostics also identified some dependencies involving the constant term of the regression equation. These were resolved by reversing the coding of five dichotomous variables. Changed were the measures for: offender cooperative at/after arrest, lives in poor neighborhood; is a high school dropout, lives with family, and, not incarcerated in the last two years. Subsequent tables reflect these changes in coding.

This problem gets exacerbated as the number of forms of recidivism, measured over observation windows of different widths, increases. Selectively trimming each model of recidivism runs the risk of being left with a staggering variety of models, each with its own list of independent variables, which may or may not be comparable to models predictive of other forms of recidivism. When these results are then interpreted, any comparison is confounded by the differences in what is used to predict a given form of recidivism.

Guided by the desire to select a single set of independent variables to use when modeling all forms of recidivism, we adopted the following selection strategy. All 103 independent variables were regressed on 153 measures of recidivism, defined in numerous ways.[9] Those variables that were statistically significant in 22% or more of the 153 equations were deemed sufficiently robust predictors of recidivism for inclusion in the final list of independent variables. Those variables not meeting this inclusion criterion were dropped from all remaining analyses.[10]

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[9] The measures used all the forms of recidivism introduced in the previous chapter, less the time to failure indicators measured at each of the four post-sentence observation windows. The time to failure versions of recidivism were replaced by dummy variables for rearrest at half-year windows which will be described later in the present chapter.

[10] This procedure raises several interesting statistical issues. First, by chance, a variable would be expected to yield a significant coefficient about five percent of the time, or for about eight of the 153 equations. The 22% cutoff is sufficiently stringent to guard against capitalizing on chance in the selection of "robust" independent variables. Second, in the absence of any established guidelines, the cutoff of 22% was empirically determined. Visual inspection of the distributions showed the figure of 22% to clearly differentiate independent variables in their ability to predict recidivism. Moreover, the criterion reduces the list of potential independent variables by over half. Third, as this procedure is clearly exploratory, multiple regression was used for all forms of recidivism indicators. Logistic regression is preferable when the dependent variable is a dichotomy (e.g., probability of reimprisonment within three years). Given this, we opted for the "liberal" cutoff of 22% based on the exploratory regressions.

There are several important consequences of this strategy given our subsequent use of the independent variables selected. One is that the list of variables becomes sample specific. Applying this method on a different sample might yield a set of predictor variables that diverges from the one found here. This is especially true for those few independent variables that hover around the 22% cutoff figure, though it is unlikely for variables such as number of arrests as a juvenile, which was found to be statistically significant in 97% of the 153 equations.

In addition, the fact that an independent variable has been dropped from our analyses does not mean that, for a given form of recidivism, the variable is of no use in modeling recidivism. Were the focus simply upon one measure of recidivism, over a single observation window, a variable excluded by our procedure may indeed be a significant predictor. Moreover, variables which are not significant, even if retained as a result of the selection procedure, might be important if other variables from the list were not controlled. The variable selection procedure is designed to identify those variables that uniquely contribute to the prediction of recidivism beyond that due to the other variables controlled. As the list of independent variables changes, so too would the predictive utility of variables that appear to be insignificant.[11]

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[11] A good example of this caveat is provided by the variables "years of education completed" and "offender dropped out of school." The selection procedure identified being a dropout as a relatively robust predictor of recidivism, while the years of education completed was removed from further analysis as it did not contribute beyond that provided by the dropout indicator. Had the dropout variable not been part of the initial list of independent variables, it is likely that the years of education measure would have been retained. This is simply the often noted problem that the results of any regression analysis are contingent upon those variables that are included in the equation. The point is worth reiterating given the variable selection strategy used.

One of the more desirable consequences of our selection strategy is that it allows for a fuller interpretation of the effects of a given independent variable when its impact on one measure of recidivism is not significant. In the previous chapter it was decided to present detailed results for only 13 of the 38 forms of recidivism at our disposal. By definition, independent variables retained in our models are known to be relatively robust predictors of recidivism. Thus, even if an independent variable is found to have few effects for the 13 variables analyzed in the next chapter, the selection strategy ensures that the variable is important for one of the other forms of recidivism that is unanalyzed here. The result is that the set of independent variables retained is appropriate for more than just the definitions of recidivism implied by the results to be presented.

Of the original 103 variables listed in Table 5.1, 43 met the inclusion criterion of being significant in at least 22% of the 153 recidivism equations. Thus, the majority of potentially important independent variables suggested by the various theoretical perspectives were found not to be so. Once numerous variables are controlled, many more variables have been found to add little to the prediction of recidivism. Those variables that have been dropped from all subsequent analyses will be briefly mentioned here so that the reader can better evaluate what is, and what is not, significantly related to recidivism in general.

Variables Dropped from the Analysis

Despite the wide array of variables suggested by theories on the etiology of criminal behavior, relatively few passed the inclusion criterion of the variable selection strategy: Numerous "social" independent variables were dropped because they were not found to be predictive of any form of recidivism. Those variables omitted from further consideration include the

following:

offender's socioeconomic class;
presenting offense evaluated as being committed out of financial necessity;
presenting offense evaluated as being committed for other financial reasons, not necessity;
offender resides in a house, not an apartment;
offender is illiterate;
offender sells drugs for income;
offender uses an alias;
offender was uncooperative with authorities since the arrest;
offender was hostile toward victim at PO;
offender has illegitimate children;
offender used "bad judgment" in committing PO;
offender general "badness" as coded by law student;
offender lives in a poor neighborhood;
number of years at current address;
offender has not lived in current home more than a year;
offender daily drug habit cost;
offender a known drug user;
offender abused as a child;
offender has history of mental problems;
offender has health problems;
offender has emotional problems;
offender had an inadequate upbringing as a child;
offender's birth order;
offender's educational attainment;
offender has a steady job;
number of jobs in past five years;
offender is married;
number of children offender has;
offender supports a family;
offender's religious participation;
offender's participation in clubs;
offender has "bad" companions;
offender was a group leader in PO;
offender victimized someone of different sex or race;
victim was a business;
offender committed crime in an open place;
offense was not committed in a home

Thus, in general, variables measuring economic need (including social class and poverty of neighborhood) were found to be poor predictors of recidivism. So too were certain indicators of a criminal or negative value orientation. Some of the variables mentioned may be conceptually redundant with other variables, and this may account for a variable being non-significant: not living in current home more than one year and number of

years at current address; educational attainment and dropping out of high school are good examples of redundant pairs. Some "group process" variables (having bad companions, being the leader of the group, victimizing someone of the opposite sex or different race) were also found not to be predictive of the many forms of recidivism.

While our goal is not to evaluate the different theories of criminal behavior, we note that some support was found for each of the perspectives discussed above. Some of the variables classified within each perspective were found to be statistically significant predictors of recidivism, however defined. Yet it is also difficult to claim that those variables retained (indicated by a double asterisk in Table 5.1) are exemplars of one perspective over another. Consequently, we will treat the group of variables suggested by the various etiological theories under the more general heading of "social" or "structural" variables.

Several variables measuring aspects of the <u>presenting offense</u> did not meet the inclusion criteria. These were:

PO of auto theft;
PO of or involving weapons;
victim injury at PO;
dollar value of items stolen or damaged;
number of PO convictions;
coder's severity score of PO;
number of victim's at PO;
in jail/prison at time of charging for PO;
used drugs at PO;
used alcohol at PO;
on parole at PO;
in jail/prison at sentencing.

There are several interesting findings here. For example, the severity of the offense as measured by the Wolfgang scale may adequately capture the seriousness dimension, such that the various other indicators of PO seriousness, such as victim injury and dollar loss, are not predictive of recidivism. In that the Wolfgang scale, which is meant to capture many

dimensions of seriousness, adequately does so, this may account for the first seven variables listed above not being significant.

Being in jail or prison at time of charging or at time of sentence, and being on parole are not good recidivism predictors (whereas being on probation is). Again it is possible that other variables, probably those suggested by anamnestic theory or prior CJS actions, (e.g., prior arrests and prior incarcerations) may be taking away from the predictive power of these variables. The use of drugs or alcohol at the PO are also not found to be predictive of the various forms of recidivism, possibly because of the poor measurement of these independent variables though other research has also shown that alcohol use, at least, is not a good predictor of criminal activities (Kulhorn, 1984).

After removing the two highly collinear indicators from the group representing anamnestic theory (prior number of charges as an adult and prior number of property charges), three additional representatives of this domain did not pass the inclusion criterion. These were:

prior charges for crimes against persons;
prior weapons charges;
has history of escape.

Of interest here is the number of indicators that <u>did</u> pass the selection procedure. Prior criminal offending is often measured using one summary indicator, but our results suggest that many different ways of measuring prior offending each uniquely contribute to the explanation of variation in levels of multiple forms of recidivism. We will expand on this as further results are presented.

Indicators of the individual's delinquent career and age of onset of that career were all found to be relatively robust predictors of recidivism. Indeed, all variables here met the inclusion criteria <u>except</u> the length of the

offender's criminal career and that was deleted only because of severe collinearity with other independent variables. As with the measures suggested by anamnestic theory, we find that multiple ways of measuring delinquent career and onset each offer unique contributions to the understanding of recidivism.

The majority of measures from the domain of prior CJS-offender action also passed the selection criterion. Variables from this group dropped from further consideration include the following:

number of prior probation sentences;
number of prior probation revocations;
prior time incarcerated;
bad conduct on most recent parole.

Given these variables, it appears that the distinction between previous sanctions of probation and incarceration is important for understanding recidivistic behavior, though not in obvious ways. Total time incarcerated adds little to the prediction of recidivism, beyond that captured by other variables. Yet, for parole experiences, counts of previous events (i.e., incarcerations and revocations) have been retained. For probation experiences, counts of previous events were dropped with only the indicator for a bad conduct during the latest probation sentence having passed the inclusion criterion.

Finally, five variables from the group of general control variables did not meet the criterion for selection. These were:

the completeness of the data on an individual;
the number of PO charges missing on the rap sheet;
the subjective measure of the offender's prior record;
probation officer favors a sentence to probation;
probation officer is against a sentence to probation;

It is reassuring to note that, once other variables are controlled, indicators of the quality of the overall data for a given individual are not found to be significantly related to levels of recidivism. Similarly, the majority of our

impressionistic summary measures are also found to be poor predictors of recidivism. Once the variables upon which these impressions are presumably based have been controlled, most subjective indicators are found to not significantly augment the prediction of recidivism.

The 43 independent variables retained by the selection strategy, when compared to those variables that were dropped, point to several conclusions. The richness of variables offered by the many theories on the etiology of crime is not matched by the ability of those variables to significantly add to the understanding of levels of recidivism. By far the largest group of variables dropped came from those under the general rubric of "Social Structural" indicators. This should not be taken as an indictment of those theories, however. As was discussed in Chapter Two, there is a problem with the causal proximity of these types of variables to the outcome of recidivism. If many of the anamnestic and prior CJS action variables intervene in the relationship between social structural variables and recidivism, then it is not surprising to find many structural independent variables dropped once prior record is controlled. The results of our selection strategy indicate only that anamnestic variables tend to be better predictors of recidivism than do those suggested be etiological theories when both are simultaneously used to predict recidivism.

An alternative explanation comes from the ability to measure the concepts underlying the etiological theories. Many of the social structural variables that were retained (e.g., offender race, sex, being a dropout, being unemployed) are relatively easily measured, especially in the context of the kind of information contained in Presentence Investigation reports. Those variables dropped tend to be less amenable to accurate measurement (e.g., attending church regularly) in the context of PSIs or only indirect indicators

of the central concepts (e.g., living in a poor neighborhood as an indicator for Social Ecology theory). In a similar vein, many of the counts of previous criminal involvement were retained for analysis and this information is easily measured from existing data.[12]

In summary, guided by the desire to model recidivism with reference to a constant set of independent predictors, we have used a selection strategy that maximizes the ability to identify independent variables that are significant across the many ways the recidivism may be measured. By retaining only those variables that meet the relatively liberal criterion of reaching statistical significance for at least 22% of the forms of recidivism available, a surprisingly large number (60 out of 103) of variables were dropped from further consideration. In this sense, some parsimony has been achieved.

AN ANALYTIC STRATEGY

In the previous sections, we have identified numerous independent variables representing six domains of predictors for recidivistic behavior. Our basic analytic goal in this chapter is to relate these variables to levels of recidivism. This is accomplished in two general ways. First, a communality analysis is conducted. This allows for a comparison and discussion of the unique and shared contributions to explained variance in recidivism for each set of independent variables, as well as combinations of sets. Since in most analyses of data such as those studied here the independent variables are correlated (to varying degrees), variance partitioning allows one to determine what is uniquely attributable to one variable or to a set of variables, relative to all the other variables in an

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[12] The ease of counting prior events is distinct from the accuracy of those events. See Chapter Three for a discussion of the validity of the official arrest histories.

equation. Conversely, it also allows one to examine the extent to which a variable or set of variables shares its predictive power with other variables.

Through such analyses, several insights can be gained. We are able to assess the extent to which the predictors from the six domains are redundant in their explanation of differences in recidivism. To the extent that each set of variables shares an ability to account for recidivism, policy may be developed using predictors from any set. Alternatively, as variables from each domain uniquely contribute to the variance in recidivism that can be explained, sentencing policies based on only one set of criteria (e.g., presenting offense in the case of just deserts sentencing) receive less empirical support.

The results of our communality analyses also provide some evidence as to the relative strengths and weaknesses of the theories that earlier led to the identification of potential independent variables. As representatives of some domains prove to be more strongly related to recidivism than others, these theoretical perspectives are supported. Thus, our communality analyses can be viewed as a form of theory testing.[13]

The second analytic approach we adopt is a linear modeling of recidivism indicators. Unlike the communality analysis, these models allow for an assessment of how individual variables are related to levels of recidivism. Here the focus is thus on the relative merit of each independent variable with

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[13] The ability to test theories here is only indirect. In addition to the problems of causal proximity and uneven ability to measure indicators across sets of variables, Pedazur (1982) notes other potential problems with communality analysis. As is clearly the case here, the independent variables are intercorrelated, thus making attribution of unique variation more difficult. What is being decomposed -- the multiple correlation -- is sample specific, reducing the generalizability of the findings. The fact that there are differing numbers of predictors across the sets also tends to confound the support for each set. However, one of the strengths of a communality analysis is its emphasis on prediction (as opposed to explanation). This allows us to provide some evidence on the predictive utility of the various theoretical perspectives.

the concomitant implications for risk assessment at the time of sentencing. Through a comparison of specific coefficients across forms of recidivism and post-sentence observation windows, we can assess what are the important predictors of recidivism.

The dual analytic approaches of variance partitioning and linear modeling are applied for all indicators of recidivism. These analyses lead to a general discussion of what are the important theories and predictors for recidivism (rearrest in this chapter, then twelve other measures of recidivism in the next). In the two ensuing chapters on intervention effects, the analysis is repeated using the theories and variables of this and the next chapters. As such, the analytic strategy will allow us to determine the impact of the criminal justice system's interventions with controls for all the independent variables selected earlier.

For the remainder of this chapter, the exposition proceeds slowly, using only the rearrest measure of recidivism. Doing so allows us to emphasize these independent variables and how they influence the most common indicator of recidivism. This will simplify the presentation when multiple forms of recidivism are considered in the next chapter. As the dependent variable studied in this chapter is a dichotomy (rearrested or not), logistic regression is an appropriate analytic technique. We have adopted the strategy of breaking the post-sentence time period into a series of sixteen half-year windows though, for presentational purposes, we only present four of the windows in the discussion of proportion of variance explained. In the analysis of the logistic coefficients, we will present the results from sixteen different equations, one for the probability of being rearrested after one-half year, one for the probability of being rearrested after one year, and so forth. The last equation represents the model for being rearrested after

"eight or more" years, which collapses information from the maximum available window of nine years.[14]

Note that the appropriate dependent variable is the cumulative proportion of offenders rearrested by a certain window, and not the percentage rearrested during a particular window.[15] Thus, each of the equations presented below can be interpreted as "what would the results, and conclusions, be if a post-sentence observation window of width X were used." By comparing the effects of independent variables across equations, we can track differences and thus infer how conclusions about the predictive ability of indicators, (as well as the sentence itself in Chapters 7 and 8), are a function of the width of the observation window chosen. All communality analyses use the multiple R-square from regression models, while individual coefficients are estimated using logistic regression for dichotomous variables and multiple regression for continuous indicators of recidivism. Finally, it will be remembered that all independent variables for the results presented below have passed common diagnostic tests for collinearity (Belsley et al., 1980) based on preliminary results using multiple regression analysis.

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[14] While the present analysis uses logistic regression, other classes of models such as failure rate models or event history analyses would clearly be more elegant. However, if the effect of a given independent variable is dependent upon the length of the window chosen for post-sentence observations, such models would have to allow for heterogeneity in the models parameters so that the parameter itself could be a function of time (window length). This greatly complicates the analytic task and would make our findings less accessible to a broader audience. Our points are more simply made with the series of logistic regressions.

[15] Windows are defined relative to release after serving of the sentence. Thus for those offenders not given incarceration (i.e., probationers or those fined), the window starts the day after sentencing. For sentences to jail, the youth complex, or prison, the window starts the day after release from custody.

ATTRIBUTION OF EXPLAINED VARIANCE

Our independent variables may be classified into six categories: social (or social structural), presenting offense, anamnestic, delinquent career, prior CJS actions, and general control variables. The social structural variables are the combined variables suggested by social structural strain, cultural deviance, social ecology, social control, and social learning theories. The presenting offense variables are all those variables that measure legal aspects of the presenting offense. Anamnestic variables are counts of adult arrests and convictions, while delinquent career variables are comparable counts plus age of onset. Prior CJS action indicators measure previous incarcerations and offender behavior while under supervision. Age, offender born out of state, and probation officer prognosis are the general control variables. For some analyses, we make use of the group of "criminal career variables" which subsumes the anamnestic variables, presenting offense measures, juvenile, age of onset, and prior CJS intervention variables. A seventh set of variables, the selection hazards, consist of six variables which represent the probabilities of the case making it to each stage of the criminal justice system (e.g., from arrest, to grand jury, to superior court, to conviction -- see Appendix A for a description of the computation of these variables).[16]

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[16] It should be noted that the selection hazards usually serve as corrective devices for the analysis of the regression coefficients. Here we evaluate models with and without these hazards, so as to determine the difference it makes to include them in partitioning the relative contributions to explained variance of the other types of variables. In this sense, the hazards stand for the processing of the case by the Criminal Justice System prior to the point of sentencing. The general control variables are included to determine if they affect the relative contributions of the other variables of more substantive interest, as well as to ascertain the nature of their own effects.

Consider the general ability to predict who recidivates.[17] This is shown at the top of Table 5.3 and is reported as the R-squared from a multiple regression (as there is no analogous summary statistic for logistic regression). As the width of the post-sentence observation window increases, so too does the explanatory power of the independent variables. In the first year on the street, 16.5% of the variance in who is rearrested is accounted for by the variables in the model. This figure rises to 23.8% if a three year window is used, 25.4% for a five year window, and drops slightly to 25.0% if the full post-sentence period is used.

The results of the communality analysis for the "rearrested or not" variable are presented in detail in Tables 5.3 and 5.4.[18] Table 5.3 indicates the explained variance attributable to six of the seven categories discussed above: the selection hazard variables are omitted. Table 5.4 includes the hazard variables. For both tables, values are expressed as the percentage of explained variation attributable to the set or groups of variables indicated on the rows of the table. The actual numeric value for the R-square is given in parentheses. Note that these numeric values are quite small in many

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[17] Not evident from Table 5.3 is the fact that the proportion of individuals rearrested does depend on the width of the post-sentence observation. Adopting the "standard" window of three years, for example, leads to the conclusion that 34.2% of the sample is rearrested, while this figure increases to 44.4% if the window is increased to a five year period. Fewer than 10% fail within the first half year, and it is not until after two and one half years that over 30% of the sample has been rearrested. Use of the longest available window finds that 53.3% of the offenders have been rearrested. Such information provides support for the argument that a period of at least three years is necessary in order to study recidivism and evaluate sentence and treatment effects. The proportion of offenders rearrest by each half year window will be presented shortly in Table 5.6.

[18] The unadjusted R-square is used in the communality analyses of explained variance as it can be decomposed into additive components. As this statistic does not compensate for the degrees of freedom used, the estimated proportions of variance attributed is likely to be high.

instances. Asterisks in these tables indicate that the variation attributable to a given set of variables was negative as a consequence of a suppressor effect.

To aid in the interpretation of these results, Figure 5.1 provides a visual summary of the distribution of unique explained variances for the nine-year window. This figure graphically displays the main use that will be made of the results from the communality analyses. It is the relative contribution to the explainable variance, partitioned into sets and pairs of sets, that we find most instructive.

The social structural variables uniquely account for about 11% of the explainable variance in the nine-year window (the highest proportion uniquely attributable to any one set of variables), rising from a base of only 6.17% of the variance that can be accounted for at the one-year window (see Table 5.3). Presenting offense variables uniquely account for a relatively small share of the explainable variance in the one-year window (3.46%), and an even smaller share of the 9-year window (2.25%). Approximately 9% of the variance that can be explained is attributable to the anamnestic variables and this holds across all windows. Delinquent career/onset variables account for relatively small proportions (from 3.80% to 2.42%), while prior CJS/offender actions uniquely account for less than 1% of the explainable variance. We estimate that the general control variables uniquely augment explainable variance by about 4% to 6% across the windows.

The general patterns of attributable explained variance for the individual sets of variables over time are quite interesting. Some types of variables seem to have an increased ability to account for variation with window length, some stay the same, and others decrease. For example, the variance identified as uniquely attributable to either social or general

control variables tends to increase across windows, while anamnestic variables, as well as prior CJS/offender action variables (albeit small) remain relatively constant over time. Note that these patterns are about the same whether the percentage of explainable variance or the actual proportion of variance explained is used. Presenting offense and delinquent career/onset variables tend to decrease in relative strength over time, though this pattern is seen only for the percentage of explainable variance. In actual values, these sets of variables account for a small, constant proportion of variance over each window.

Explained variance that cannot be uniquely attributed to any one set of variables may be further divided into that which can be explained by pairs of sets of variables. This allows us to identify the extent of redundancy across the various domains of independent variables. Each pair of sets may explain more combined than the sum of their unique contributions or, alternatively, a group of variables may account for variation that is unique from that attributable to other sets. The largest proportions of explained variance attributable to pairs of sets of variables across all windows is the social/prior CJS-offender action combination, accounting for between 7.06% to 11.66% of the attributable variance. The effects of other "pairs" of variable types tend to be considerably smaller. Social/delinquent career, presenting offense/anamnestic, anamnestic/delinquent career, anamnestic/general controls, and delinquent career/general controls, account, respectively, for 2.50%, 1.96%, 1.44%, 2.15%, and 6.81% of the explainable variance in the nine-year window.

Changes over the windows in the percentages of shared variance attributable to pairs of sets of variables exhibit a pattern similar to that described above for the sets of variables themselves. Some decline slightly

over time, some remain steady, while others decrease. Interestingly, it is frequently the case that pairs of sets of variables involving either the social or general control variable sets tend to increase over time which is similar to the what was observed above for the individual sets. Little systematic patterns are observed for the other pairs of variables, although sets of variables coupled with anamnestic variables tend to decrease in their share of attributable variance over time. This is unlike the relatively steady effect for anamnestic variables as a individual set. Moreover, with the exception of the shared variation accounted for by the social/prior CJS action pair and the noticeable decrease for the anamnestic/prior CJS action pair, the actual numeric values of explained variance for the all sets remains relatively constant across all windows. This suggests that, at least for recidivism defined in terms of rearrest for any crime, variation in common across pairs of sets is related to recidivism in a constant fashion across differing post-sentence observation windows.

About 40% of the explained variance cannot be attributed to the sets or pairs of sets at the nine-year window. This is a drop from the 46.38% that cannot be attributed to these same sets or pairs of sets at the one-year window. Thus, under half of the variance explained may be conceptualized as being "shared" across the sets of variables -- and is not unique to any one set or pair of sets of variables.[19] Clearly each domain of independent variables adds in the ability to account for variation in post-sentence rearrest, though there is also considerable overlap in the explanatory power

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[19] Actually, combining groups of variables into sets of three would result in a further attribution of this nonunique variance, though given the findings for pairs of variables, we would expect the actual proportion of variance explained to be quite small. The process could be continued to the point of including the set of all six groups. This would result in the saturated model, which, by definition, would allow for all of the explained variance to be attributed in some fashion.

of these domains.

Although we present no formal model of these processes over time, it is interesting to speculate as to what may be accounting for these patterns. The impact of variables associated with social attributes (e.g., gender, ethnicity, high school dropout, etc.), as well as that of the general control variables (e.g., age), may be characterized by a stochastic process -- one in which leads to the relative effects increasing over time.[20]

Consider, for example, the effect of being female upon recidivism. We find below that females are, all else equal, less likely to be rearrested. Thus, for every time period, the proportion of males who have recidivated will increase, even if the only generating mechanism is random with females having a lower probability of being rearrested at any given point. The major consequence of this will be that the variable "being female" will become more strongly predictive of rearrest as the width of the post-sentence observation window increases. Similarly, being a high school dropout may result in greater proportions of attributable explained variance simply because its impact is lasting: every year some will recidivate as a result of the disadvantages associated with being a dropout. As more individuals recidivate because of their status as dropouts, the impact the "dropout" variable may be greater. (See discussion below on the impact of individual variables over time).

Such an underlying stochastic model serves as an alternative explanation for any <u>increasing</u> ability of individual variables (i.e., those of the previous examples) or sets of variables (i.e., the group of social structural indicators) to account for variation in levels of recidivism Note, however,

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[20] We would like to thank an anonymous reviewer of an earlier paper for suggesting this line of reasoning.

that postulating a simple stochastic model is less successful in accounting for explained variation when individual variables or sets show either no change, or a decrease, in their ability to account for variation over time. In these circumstances either a more elaborate stochastic model (e.g., one with a decay parameter) must be assumed, or the findings can be taken at "face value."[21]

A general assessment of Table 5.3 would probably lead most observers to say that there are few substantial differences over time in the distribution of attributable explained variance. There seem to be two notable exceptions: the differences observed between the one-year window and the other windows, and the change in the impact of the social variables as a set, as well as in conjunction with other sets, across windows. But even these exceptions may not seem to involve "large" differences: the proportion of variance in rearrest attributable to the social variables alone varies from .01 to .028 from the one-year to nine-year window and this is the largest difference observed across windows for any one set or pair of sets of variables. Thus, in allocating variance uniquely to sets or pairs of sets of variables, the "difference it makes" in choosing one window follow-up over another is less than two percent of the explained variance for any one category. At the same time, the fact that some types of variable change in their relative proportion of explained variance (however small), is somewhat disconcerting to those who

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[21] An assessment of the relative impact of variables (or groups of variables) over time is a complex subject, and if data were available on the independent variables over time, this would lead us to a more appropriate type of modeling such as event history analysis (Yamaguchi, 1991). Lacking the best data for an event history analysis, and lacking formal models of the increasing or deteriorating impact of variables over windows of varying lengths, we will focus here on some of the practical implications of choosing window lengths within the study design used here. Put simply, does choice of, say, a nine-year window over a five-year window lead to substantively different assessments of the relative impact of the various types of variables discussed here?

would argue that it makes no difference whether one chooses a short or long follow-up window. Choice of a long follow-up window seems to result in a general shift toward demonstrating the relative explanatory strength of the social and general control variables (though this is arguably a small shift).

Table 5.4 repeats the previous analysis, this time controlling for the CJS processing of the case as represented by the sample selection bias indicators. Because these variables represent factors which are "logically prior" to the definition of the sample, they were entered first hierarchically in the regression. (Note that none of the other effects for the sets of variables in Tables 5.3 and 5.4 represent non-unique contributions to explained variance, only the hazard component is given hierarchical priority.) Adding the hazard variables has a negligible impact on the total variance explained. The explained variance is approximately the same for the models presented in Table 5.4 with the hazards and to those in Table 5.3 without the hazard variables.

The effect of including the hazard variables is to reduce the proportion of explained variance attributable to each of the other sets of variables. The social variables now uniquely account for 9.84% of the variance that can be explained in the nine-year window, while the anamnestic variables are responsible for 8.93% of the predictable variance in a nine-year window. Thus, controls for sample selection bias decrease the ability of these variables to account for differences in the probability of rearrest. Looking at pairs of types of variables reveals results generally similar to that found in Table 5.3: few differences across windows, but nevertheless some improvement in relative strength for the social and general control variables over time. In conclusion, including the hazards in the models has little substantive impact on the interpretation of the attributable variance and the

substantive conclusions reached from the communality analysis with hazards is similar to that of the results without hazards.

Controls for sample selection bias do lead to one major insight, however. A large discrepancy is seen when comparing Tables 5.3 and 5.4 in the unattributed explained variance. In the nine-year window, for example, 39.38% of the explained variance is unattributed, and this drops to 20.38% in the equations with the hazards. In general, almost half of the explained variance that could not be attributed to sets or pairs of sets can be attributed to the hazard controls. This is a result of allowing the hazard variables to explain variance first in the equations represented in Table 5.4. Given that the set of hazard variables were allowed "first crack" at the variance of the dependent variables, and given that these measures are correlated to some extent with the other sets in Table 5.4, it is not surprising to find that the unique contributions of the sets of independent variables, as well as the pairs of sets are generally reduced by a few percentage points relative to their effects shown in Table 5.3. Despite this fact, a considerable amount of the explained variance in recidivism can be attributed to the nature of the case prior to sentencing as represented by the hazard variables.

There are several implications to this finding concerning how sample selection relates to the ability to attribute explanatory power to various independent variables. Much of what predicts recidivism also predicts the offenders' being in the sample studied here. Thus, the hazard rates themselves share a large proportion of the unattributed variance, as can be seen by comparing the second rows of Tables 5.3 and 5.4. Moreover, there is not only a sizeable degree of shared explained variance across the social, anamnestic, delinquent career, prior CJS, presenting offense, and general control variables, but this is held in common with the hazard variables. If

one is willing to make the assumption of temporal ordering represented by the hazard variables, about one quarter of the explainable variation in recidivism can be attributed to the processing of the case through the criminal justice system.

In addition, for the purposes of predicting this measure of recidivism (rearrest), there is a substantial proportion of explainable variance that cannot be attributed to any one set (excluding pairs for the moment) of variables. Approximately 67.2% of the explainable variance in Table 5.3 and 44.0% of the explainable variance of the nine-year window in Table 5.4 is not unique to any of the seven domains of predictors. Adding that which may be uniquely explained by pairs of sets of variables reduces the unattributed variance to 39.38% and 20.38%, respectively across the two tables for the nine year windows. This is indicative of the considerable commonality across the sets of variables. Should this finding maintain when other measures of recidivism are analyzed, it would suggest that there is some latitude in the choice of variables used to predict recidivism, and assess risk, at the time of sentencing. While some degree of accuracy might be lost if some domain of variables were not used, the high degree of communality suggests that this loss will not be too great.

One final inference from these results is that although other types of variables are reasonably good predictors, the social variables alone account for the most unique variance. While the anamnestic and general control variables, as individual sets, account for substantial amounts of the explained variance (either with or without the hazard controls), the social variables account for the largest single percentage of explainable variance at the nine year window, accounting for 11.08% and 9.84% of the explained variance across the two tables. Thus, the size of the unique contribution of

the social variables is relatively substantial. Yet, at the same time, it must be recognized that it is less than the total unique variance attributable to the other domains combined.

The previous results addressed the unique contribution of each of the six types of predictor variables (seven, counting the hazard variables) when the other types were statistically controlled. It is also informative to know the variance accounted for by each of the types of variables individually in the absence of these controls. CJS decisions are often based on the assumption that only certain kinds of information is to be used. For example, grid sentencing systems use only some aspects of anamnestic variables and presenting offense measures. Some risk assessment instruments limit themselves to use of prior record variables. How well each group of variables accounts for variation in rearrest by itself is especially important given that a premise of much recidivism research in the past decade is that what we call structural variables here do not add appreciably to prediction of recidivism.

The explained variance for each type of predictor variable entered alone and with the hazard variables entered first is presented in Table 5.5. This provides an indication of the extent to which rearrest can be successfully predicted using each domain of variables alone and addresses the general question of what proportion of variance could be accounted for if measures from the other domains of predictors were not available. As can be seen, the ability to account for differences in recidivism can be seriously impaired by the use of only certain types of variables.

Table 5.5 shows that criminal career variables, particularly the anamnestic and delinquent career variables, account for the largest amounts of variance in rearrest. Only about two to four percent of the variance in

rearrest explained by all domains cannot be explained by the criminal career variables, defined as all domains save social structure and general controls. That is, the social structural and general control variables augment the variation explained by about two to four percent. Interestingly, the delinquent career/onset variables account for almost as much variance by themselves as the anamnestic variables (with only about one to two percent below the anamnestic variables' contribution to explained variance.) The social variables, however, also do well, ranking third, after anamnestic and delinquent career/onset variables and accounting for between seven and thirteen percent of the variance in rearrest from the one-year to nine-year windows.

The contributions to explained variance are appreciably lower when the hazard variables are entered first in the regression equations and each of the six groups are entered second (bottom panel of Table 5.5). The explained variance attributable to each of these groups drops to about two/thirds of their strength without the hazards in the equation, indicating considerable overlap between the probability of being in the sample and all the characteristics measured by the six types of variables used here. In general, the rank ordering of the sets of variables is the same with or without the hazard variables in the models. The total variance explained is about the same (as was observed in the discussion for Tables 5.3 and 5.4), also indicative of the sharing of variance explained between the hazard variables and the six groups of independent variables.

Note that it is possible to approximate the results of Table 5.5 using those from Tables 5.3 and 5.4. The actual proportion of variation in rearrest attributable to a set of predictors by themselves always falls between the lower bound of the set's unique contribution added to its contribution in

conjunction with other domains, and the upper bound of the unique contribution added to that part of the explained variance that is unattributed. Consequently, the analyses of subsequent chapters is simplified in that results such as those represented by Table 5.6 can be inferred from the communality analyses.

In conclusion, the variance partitioning analysis of rearrest has revealed that: 1) high percentages (20 to 46% across models) of the explained variance cannot be uniquely assigned to any one category of predictor variables; 2) of the specific classes of independent variables, the social variables constitute the best single set of predictor variables in terms of unique contribution to explained variance of rearrest; 3) only prior CJS/offender actions could be lost without affecting the total explained variance of rearrest to some extent -- a loss ranging between .001 and .008 of variance explained across the models presented in Tables 5.3 and 5.4 for the nine-year windows; 4) criminal career variables, when entered hierarchically first in the equation, account for most of the variance that is explained with all types of variables in the equations; 5) when hazard variables are entered first, the contribution of all types of variables drops appreciably in the hierarchical regression analyses described above, including the criminal career variables, which drop from approximately from 20 to 15 percent of the variance in rearrest across most windows.

THE IMPACT OF INDIVIDUAL VARIABLES

While the previous analysis indicate how the groups of variables from each domain are related to the probability of rearrest after sentencing, these results do not indicate how each predictor is related to recidivism. For decision making purposes such as risk assessment or fashioning an appropriate

sentence for an offender, it is the direction and magnitude of individual predictors that is of more concern. Thus we turn to the results of linear models for the probability of rearrest.[22]

Table 5.6 represents the results of logistic regression analysis of the rearrest recidivism measure for 16 windows defined by six-month intervals from the first six months to the full window of eight and a half years or more. The six hazard variables are added to the analysis and discussed in Table 5.7. Logistic regression is inherently a nonlinear model and thus the actual magnitude of a given effect is dependent upon where the values of the independent variables fall on the logistic curve. That is, relatively small or large values for independent variables will tend to have smaller effects on the probability of rearrest than intermediate values -- the effect on the dependent variable is not constant across all values of the independent variable. To compensate for this, we follow the suggestion of Peterson (1985) and represent coefficients as changes at the mean of the dependent variable. These effects, which are analogous to unstandardized regression coefficients, are shown for all independent variables.[23]

The variables are listed in the tables according to the same theoretical scheme developed above for the variance partitioning analysis. Each of the individual social theory variables are listed, followed by each category of criminal career variables (presenting offense variables, anamnestic,

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[22] For all our analyses, continuous variables have been centered at their mean. This helps to further reduce collinearity and is the preferred method (Aiken and West, 1991) when using interaction terms, as we do shortly.

[23] It should also be noted that these coefficients are quite robust. Separate analyses with non-significant and conceptually redundant variables removed resulted in few large changes in the magnitude of the coefficients. Also, separate regressions were done with the six general scales discussed in Chapter Four, but since they were very highly collinear with the other variables -- and added virtually nothing to the explained variance -- they were dropped.

delinquent career/onset, and prior CJS/offender action variables), and then the general control variables. The discussion of these results mirrors this ordering.

Social Structural Variables.

Rearrest is significantly related to one social strain variable, unemployment, across all the windows, and another (having a job after the sentence) for the first four and a half years of windows. Being on welfare at the time of the sentence is not related to rearrest across almost all the windows. Variables derived from cultural conflict theory, including race, ethnicity, and gender represent significant predictors of rearrest. Being black or Hispanic increases the likelihood of recidivism, while being female decreases the likelihood. We interpret the impact of being black or Hispanic as indicative of the social reality of being a minority in society. For example, educational and occupational opportunities, beyond those measured by other variables in the current analysis, may be limited due to discrimination experienced in these contexts. Some researchers have argued that there are subcultures within society that are more conducive to violence and other forms of crime, and that these subcultures center around racial and ethnic minorities (Wolfgang and Feracutti, 1972). We do not address the issue of whether it is a purely subcultural phenomenon or related to the social structural position of blacks and Hispanics in society, but there seems to be a strong relationship, and one that is observed to increase with window length. The relationship between these social statuses and recidivism as measured by rearrest is persistent.

Similarly, the gender effects are strong and may be interpreted in terms of differences in the socialization processes of women versus men, or to differences in the social structural conditions that women find themselves in.

If the latter, the relatively weak position of women in the social structure of society serves a quite different function than that of minorities, decreasing the chances and motivations for crime among women, while enhancing these factors among minorities. The single most important finding here is that basic social characteristics -- race and gender -- are strongly related to subsequent recidivism.

Living in an urban area seems to increase the chances of recidivism a measured by rearrest, while years at current address decreases the chances. No systematic patterning of effects across the windows are observed. The main finding here is that urban areas are somewhat more conducive to recidivism than other geographic areas, and geographic mobility is associated with enhanced recidivism prospects.

Although none of the variables measuring aspects of the social psychological strain experienced by the offenders was consistently significant across all windows, those offenders who are known to have alcohol or drug problems were found to be more recidivistic over time, but generally not in the first few years. After five years, each of the three variables included here (history of drug problems, treated for drug/alcohol problems, and having needle marks) is statistically significant, while none of them is in the first two and a half years. Having a history of drug problems was found to result in an increase in the chances of rearrest by about three to four percent across windows of three through "eight plus" years. Interestingly, the effects remain relatively constant over time, as compared to the effects observed for race and gender, which generally rise monotonically.

The effects for other social psychological strain variables are similar, and even more pronounced. Having been treated for alcohol or drug problems seems to have no effect in the short run, but then it has a positive

relationship to rearrest. This could be interpreted as meaning that in the long-term the effect of such programs wears off -- but the effect never is to significantly reduce the chances of rearrest. Having needle marks, possibly indicative of serious drug use, is not found to be predictive until after a four and a half-year follow-up. One possible reason for this is that the addiction effects of the drugs persist, or even get worse, over time -- possibly while the effects of some other variables shrink. Overall, there is a marked consistency in the results for these strains variables. Having an a drug problem or having been treated as having an alcohol or drug problem is predictive of rearrest, as is having needle marks, but all these effects to not "emerge" until after a few years of follow-up.

Social control theories, which focus on the commitment or bond of the individual to the economic, educational and familial institutions of society, also find some support in the current analysis. Those who finished high school are less likely to be rearrested across all windows. In six months, nondropouts are less likely to be rearrested by about three percentage points, while after eight years the observed effect in Table 5.6 is even greater with the likelihood of rearrest is reduced by almost six percent, net of the effects of other variables in the table.

Somewhat surprisingly, living with a family or spouse increases, rather than decreases, the chances of rearrest, contrary to the hypothesis of social control theory, though the effect is not statistically significant until after four years. One possible explanation for this is that stability in residence makes the offender more accessible to authorities and thus new offenses are more likely to be detected. (See the results for the "born out of state" variable below.) However, this explanation is not consistent with the fact that increased residential stability, per se, is significantly negatively

related to the probability of rearrest.

If the presenting offense involved a victim who was unknown to the offender (a stranger), or the offense was committed by more than one individual, then some significant effects on recidivism were identified. Perpetration by groups was found to be negatively associated with rearrest across all the windows, suggesting the group dynamic aspects of crime may be important, independent of other variables' effects. Specifically, those who commit crime alone are more likely to be rearrested, possibly because of a greater motivation or commitment to criminal activities. This interpretation is somewhat reinforced by the fact that victimization of strangers is positively associated with subsequent rearrest. Those who victimize strangers may be considered as participants in a more "predatory" type of crime than those who are known by their victims.

Characteristics of the Presenting Offense.

The results from the set of variables used to measure the presenting offense characteristics suggest some support for the idea of their being different types of offenders, at least relative to likelihood of rearrest. For example, those who committed a crime against property were likely to be rearrested, while those who committed a more serious offense (as measured by the Wolfgang scale) were less likely to be rearrested across almost all windows. It may be that the Wolfgang scale, which is heavily weighted to tap the severity of characteristics in crimes against person, may be more of an index of those forms of crime than of all crimes in general (Wolfgang et al. 1985). If so, than the severity rating is really a weighing of the degree of injury to a victim, and thus the negative associations with rearrest is more

understandable, as such crimes not as prevalent as property crimes.[24] For the dependent variable of rearrest, those whose presenting offense is either a crime against persons or a drug offense are neither more nor less likely to recidivate.

The current CJS status of the offender (at the time of the presenting offense or sentencing) is found to be a consistent predictor of rearrest. Having detainers at arrest was found to have a negative effect on recidivism, while having pending charges were found to be positively related to rearrest across all the windows. The latter finding is not surprising, as it indicates that the offender is active or in an active phase, (Maltz, 1984), such that subsequent arrest is likely. One seemingly puzzling finding is the negative impact of having detainers on subsequent rearrest. It may be, if the detainers are from out of state jurisdictions, the offender is less likely to return to New Jersey and be rearrested and that having detainers is indicative of our inability to monitor out of state incarcerations and arrests. If so, the detainer variable, like the "born out of state" control variable, represents a data control instrument, rather than a criminogenic attribute of the offender.

Being on probation at the time of arrest is positively related to rearrest and this is consistent across all the windows. Being on probation and committing the presenting offense may indicate a disregard for criminal justice system interventions and reveal another dimension of an "active phase" in the offender's career. Moreover, this indicates that the previous sanction was unsuccessful in deterring the offender.

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[24] The results in the next chapter for the probability of rearrest for a persons crime confirm this suspicion.

Anamnestic Measures.

The variables with rather consistent positive associations with rearrest are the number of prior arrests as adults, the number of charges in the past five years, and being incarcerated at some time during the two years prior to the instant offense. All three are robust predictors of recidivism. Note, however, that the effect of the number of prior adult charge convictions, is negative, suggesting a possible specific deterrent effect of the conviction beyond the charges or arrest. This result, supported in later chapters as well, has implications for the choice of prior record indicators used in grid sentencing systems. It has been argued that convictions is a more appropriate measure of prior record as it is less susceptible to possible discrimination than are charges or arrests. Our finding is that, once charges and arrests are controlled, some counts of previous convictions are related to recidivism in a direction opposite to that assumed by grid systems. Thus, previous convictions may not be the most appropriate single indicator of prior record.

Breaking down previous convictions into types of offenses, we find only the number of prior weapons convictions to be positively associated with the likelihood of rearrest. However, this effect is sporadic and not seen across all follow-up periods. It appears that, for recidivism indicated by rearrest, global counts of previous convictions are more important predictors than are those for property or persons crimes.

The other anamnestic variables studied here were found not to be consistently predictive of rearrest: number of adult convictions, number of prior Part I charges, number of prior property convictions, and number of prior persons convictions. In part this may be due to conceptual redundancy with some of the above mentioned variables, at least for this dependent variable. It will be remembered, however, that all independent variables were

specifically selected for their ability to predictive numerous forms of recidivism so that those anamnestic variables that are not significant in Table 5.6 can be expected to be significantly related to other forms of recidivism.

In conclusion, prior criminal behavior, measured in several ways, is indeed predictive of subsequent criminal behavior. The association is generally positive, but the one negative association may be speculatively interpreted as a specific deterrent effect. Future criminal behavior is well predicted by past criminal behavior, but particularly well by recent (two or five years) behavior.

Delinquent Career and Age of Onset.

Independent of the effects of the anamnestic variables discussed above are the effects of various characteristics of the offender's juvenile delinquency career. Specifically the number of juvenile arrests as well as the number of juvenile charges were found to effect rearrest positively. After one year, arrest as a juvenile increases the probability of rearrest by about 1 percent and by two years, each juvenile charge increments this probability by about 4 percent.

The younger an offender serves his first incarceration, the more likely rearrest. Thus, by this measure of age of onset, the individual who began a criminal career early was found more likely to persist in crime beyond the point of the presenting offense. Age at first arrest was not found to predict rearrest, and time since first drug use is not a consistent predictor across the windows studied for this dependent variable.

As was suggested by the communality analyses, including aspects of the juvenile delinquent career reveals some strong relationships with rearrest. Both number of prior arrests and number of prior charges as a juvenile were

found to be positively associated with rearrest. Note that these effects are observed independent of the anamnestic variables discussed above. Thus, there is some evidence of the "lingering effect" of delinquent involvements on subsequent adult recidivism, independent of the effects of the adult anamnestic variables.

Prior CJS-Offender Action.

The number of prior incarcerations experienced by the offender is found to reduce the prospects for subsequent rearrest, further suggesting a type of specific deterrent effect. Other variables did not have systematic effects over the time periods studied, although some effects are observed in the first few years of follow-up for those who exhibited bad prior probation behavior. This is consistent with the general results for the anamnestic variables in that recent behavior is the best predictor of subsequent behavior. Having one's most recent parole revoked, and number of prior parole revocations did not have any detectable impact on rearrest.

General Control Variables.

The negative effects of age are observed across all the windows. Each year of age results in about a one percent drop in the chance of rearrest. Thus, the contention that individuals "age out" of crime receives some support from our analysis. Net of all other variables, older offenders are less likely to recidivate than younger ones.

As a control for the nature of the recidivism data base, we include born out of state as a proxy for the possibility of moving out of state after sentencing. It was found, as expected, to be negatively related to rearrest. The magnitude of this effect is quite large, with the probability of rearrest decreasing by .13 at the full nine-year window. Clearly those individuals with family ties outside of New Jersey are less likely to have a rearrest

reported in this sample.

The evaluation of the probation officer's prognosis, as interpreted and scored by the coders, was found to be negatively related to rearrest from the first year on. Thus, the general evaluation of the offender's likelihood of recidivism was reasonably good, roughly, for every ten points higher on the 100 point scale, just under a one percent increase in recidivism is observed. This supports the contention that subjective evaluations can indeed be reasonable predictors of subsequent criminal behavior.

In sum, the general control variables of age, born out of state, and coders evaluation of the probation officer's prognosis, were all found to be related in predictable ways to rearrest. Not only are these effects negative, they are consistently so across all half-year windows.

Controlling for Sample Selection

The use of indicators for sample selection bias is motivated by the possible misestimation of the effects of independent variables when this bias is not considered. This can be evaluated in Table 5.7 where the hazard variables are added to the equations represented in Table 5.6. In one respect, the consideration of selection bias has little impact. The individual coefficients for the variables of the six general types are not affected by controlling for the hazard variables to the point that substantive conclusions would change. That is, the interpretations offered for the results of Table 5.6 also apply to Table 5.7. This is reassuring as, at least at a general level, the effects of individual predictor variables are not distorted by the likelihood of being in the sample.

At the more detailed level of the actual magnitude of the coefficients, some differences are apparent. The constants are somewhat lower in the models with the hazard variables. Some variables (e.g., presenting offense for a

property crime) have estimated coefficients that are slightly lower when the hazard variables are controlled. Others, (e.g., being unemployed, black, Hispanic, or female) now have slightly larger effects on the probability of recidivism. Yet we find no systematic effect in the direction of such changes and those differences that do exist are often in the found in the third decimal place. Thus, for dichotomous independent variables, sample selection bias does not lead to a major distortion in the predicted probability of rearrest.

It is also possible to interpret the hazard variables as indicators of the CJS processing of the case prior to inclusion into the sample. The one surprising finding in this regard is the relatively large effect of "case proceeds past arrest" on recidivism, particularly as observed in the first three years of follow-up. The effect is not only statistically significant, but quite large relative to the other hazard variables. If a case has the characteristics that enhances its chances of going beyond arrest, the offender is more likely to recidivate, net of the other variables in the equation. Although the fact that this particular hazard variable stands out was not anticipated, the result makes sense. That is, the effect of this variable is not surprising in that offenders whose cases are dropped after arrest may be deemed low risk offenders at that point in time. Also, this may be the most crucial decision made by the CJS as to who are likely to recidivate -- in part because the "cost" of taking a case beyond that point (i.e., going to court), is high. It is likely that prosecutors screen the cases that they think judges will not convict on or will only impose minimal sentences.

Some General Conclusions

The results from the regression analyses are in harmony with the expectation that all of the theories, variously measured, are predictive of

recidivism. There is at least one variable from almost every specific theory discussed that is related significantly to rearrest. The findings thus support one of our basic contentions that recidivism is simply an aspect of criminal behavior in general. Little support is found for social strain theories, except in that not having a job increases the likelihood of rearrest. Social class and other variables measuring "economic" motivation to commit crime are not predictive of rearrest. This is an important finding, and one not in disharmony with several research efforts which have found support (and lack of support) for some of the same theories evaluated here (Elliott et al., 1985; Kornhauser, 1978; LaGrange and White, 1983).

The simple fact of rearrest for any crime is a central measure in recidivism research. All other indicators, especially those to be studied in subsequent chapters, are derivative of this variables. Thus, it is likely that the basic themes for the findings in this chapter will be repeated when more specific forms of recidivism are analyzed. To briefly summarize, some of the variables found among the most consistent predictors of <u>rearrest</u>, across any period of post-sentence observation, include the following:

<u>Social</u>
- Black
- Hispanic
- Female
- Living in urban area
- Residential stability
- Treated for drug/alcohol problems
- History of drug problems/needle marks
- School dropout
- Does not live with family
- Committing crime with others
- Victim of PO is a stranger

<u>Presenting Offense</u>
- PO is property crime
- Wolfgang measure of seriousness of PO
- Has detainers at arrest
- Has pending charges at PO
- On probation at arrest
- In prison/jail at sentencing

Anamnestic
- Number of prior adult arrests
- Number of prior adult charge convictions
- Number of prior charges in past five years
- Number of prior weapons convictions
- In jail/prison in last two years

Delinquent Career/Onset
- Number of arrests as juvenile
- Number of charges as juvenile
- Years since first incarceration

Prior CJS/Offender Action
- Number of prior incarcerations

General Control Variables
- Age
- Born out of state
- Coder evaluation of probation prognosis

INDIVIDUAL PREDICTION

Communality analyses and linear modeling provide evidence of the impact of recidivism at an aggregate level. That is, we can identify how predictive certain types of variables are of differences in recidivism and in particular how given variables are related to increases or decreases in the probability of recidivism. However, decision making in the CJS is done at the level of the individual. Assessment of acceptable risk for recidivism proceeds on a case-by-case basis. Moreover, how accurate predictions of recidivism are becomes more salient.

The topic of individual-level prediction of recidivism is the focus of Chapter 9. It is there that the details and implications of individual prediction of various forms of recidivism are elaborated. The results presented above do bear on the prediction of recidivism at the individual level, and thus we briefly discuss these findings in this light. Note that comparable results will be given in later chapters.

Tables 5.6 and 5.7 contain information regarding the prediction of individual rearrest across the 18 windows studied in the form of summary statistics for each window. The mean of the dependent variable represents the

observed proportion of the offenders who have been rearrested by each window. As can be seen, the proportion is about 15% of the cases by the first six months, rising to 55% by the time the maximum time frame is reached. Interestingly, the proportion rearrested continues to climb by about 2% a year even after six or seven years. Thus, even those who stay "clean" for several years have a quite high failure rate.

The pseudo-R2 measure is defined as the ratio of the chi-square from the logistic regressions to the chi-square plus the number of cases (Aldritch and Nelson, 1984). As such, the pseudo R2 represents an indication in the aggregate of how well the model does at predicting rearrest. By this indicator, the ability to predict arrest seems to reach a maximum after four years at about 22%, though the predictive ability of the general model does not increase appreciably after two or three years. For example, at two years the pseudo-R2 is about 19% and it increases to 22% by year four, and changes very little thereafter.

The degree of successful prediction can also be ascertained at the individual level. One indicator of success is simply the proportion of the sample correctly predicted to fail (i.e., get rearrested) or succeed (i.e., not be rearrested).[25] In the current study the base rates, or percent failing, are low in the early windows, and in the mid-range (35 to 55%) for the rest of the time. The results here show that ability to predict correctly decreases from 85% to 75% in the first year and a half, then drops to about 73%, where it hovers for the remaining time frames. Thus, to an extent, the percent correctly predicted and the pseudo R2 indicators are give quite

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[25] This measure is a function of the base rate (the proportion observed to fail) and the selection ratio (the proportion predicted to fail). If a very low or very high proportion are rearrested, one should be able to predict quite well by simply picking everyone to either fail or succeed.

different interpretations in the short run, but similar ones in the long run. When the proportion rearrested is relatively low, the ability to predict overall is high, while the pseudo R2 is relatively low. After a couple of years of follow-up, however, the percent correctly predicted does not change appreciably and neither does the pseudo R2. This suggests that the ability to "predict rearrest" at the individual and aggregate level (given the current model) is not appreciably enhanced by a long follow-up window. Adding the hazard variables to the equations yields little improvement in either the pseudo R2 values or the percentage of correct predictions (see Table 5.7).

Another indication of the success of the model is the individual predictions that are false positives as opposed to false negatives. Both are presented in the bottom of Table 5.6. False positives are defined as those predicted to be rearrested who are not, while false negatives are those predicted to succeed who are rearrested. The results for this model show that the false positive rate (number predicted to fail who actually succeed divided by the total number in the samp increases from about one percent to eight percent in two years, and then continues to rise gradually to almost 14 percent by the end of the study. The false negative rate, on the other hand, follows a quite different pattern of increasing somewhat in the first two years and then decreasing gradually over time. Generally one could conclude that, after the base rate of rearrest has increased to about a third, the false positives increase while the false negatives decrease over time. That is, the more offenders who fail, the more errors are made in predicting such failure, and the fewer errors in prediction of success. Thus while the percentage of individuals whose rearrest status is correctly predicted remains constant after about two years, there are systematic changes seen in the prediction of false positives and false negatives. Including the hazard

variables in the equations (Table 5.7) again has a negligible impact on the percent correctly predicted, the number of false positives, or the proportion of false negatives.

We will give more interpretation to individual prediction in Chapter Nine, but it can be said here that the if one were to arbitrarily choose a two-year follow-up for rearrest rather than a longer one, there would be a relatively high false negative rate (the highest observed in Table 5.6), but a fairly low false positive rate (8.32%). Thus, as Blumstein et al., (1985) have argued more generally, there are tradeoffs that can be weighed in terms of false positives and false negatives, so as to maximize overall benefits, or minimize specific costs. Thus, for example, if for "individual civil libertarian" reasons, more weight is given to avoiding false positives than false negatives, a shorter window may be a better choice than a longer window. (The extent to which this generalizes to other binary dependent variables can be seen in the next chapter).

Aware of some of the limitations regarding prediction indicators, Loeber and Dishion (1983) introduced the RIOC measure (Relative Improvement over Chance). RIOC is defined as the ratio of actual number of correct predictions minus those that would be expected by chance to the maximum possible number of correct predictions minus randomly expected number of correct predictions. Although the RIOC measure is designed to be independent of the base rate and selection ratio, Copas and Tarling (1986) have criticized it as increasing as the selection ratio (the proportion predicted to recidivate) increases. Although the selection ratio increases (as does the base rate) across the windows, we did not find a corresponding increase in RIOC. Instead RIOC was found to peak at two and a half years (.499) and drops slightly to .455 by eight plus years. Thus, this indicator of successful prediction corresponds

to the finding above regarding false positives and overall percent correct -- after two and a half years there is a small deterioration in the predictive power of the general model presented in Tables 5.6 and 5.7. Again the impact of adding the hazard variables is negligible in terms of an increase in individual level prediction.

In general the RIOCs reported here are higher than those reported by some researchers and lower than others. For example, Blumstein et al., (1986:189) report RIOCs of .24, .54, .74 and .31 for some traditionally used scales (Salient Factor Score, Iowa Assessment Scale, INSLAW Scale, and Rand Inmate Scale, respectively -- note that the Rand Inmate Scale uses a different criterion variable of high frequency rate of committing crimes.) However, caution should be exercised in comparing RIOCs, as differences in the definition of the criterion variables (here, rearrest), differences in the selection ratios, and differences in the nature of the sample (e.g., conviction sample, incarcerated sample, federal cases, state cases, etc.) may account for differences in RIOC. Also, no attempt is made here to validate the equations on another sample, an exercise that would result in a lower RIOC than reported here on this "construction sample" (see Gottfredson and Gottfredson, 1988). For example, the Salient Factor Score and Iowa Assessment Scale dropped .04 and .05, respectively, when applied to validation samples (Blumstein et al., 1986:189).

Our brief investigation into the prediction of recidivism at the individual level leads to several conclusions. First, in spite of the wide variety of independent variables at our disposal, prediction, especially in the short run, is problematic. Summary statistics offer two contradictory interpretations: while variation in rearrest is more difficult to account for over shorter windows, the fact that fewer individuals recidivate makes it

easier to predict rearrest. Second, our accuracy in prediction, when compared to that observed in other samples (e.g., Blumstein et al., 1986), is moderate. Some studies have been better able to predict individual-level recidivism, others have not. Finally, after about a two year period, the proportion of offenders correctly predicted levels off. It is only within incorrect predictions where we find systematic changes with increasing window width.

INTERACTION TERMS

The Criminology literature is replete with studies of recidivism for subgroups of offenders. Some research uses who have committed a particular type of crime (e.g., burglary, robbery, drugs). Others focus upon groups defined by a particular sanction, such as samples of parolees. The list of possible comparisons is endless as subsamples can be defined by offender age, sex, race and so forth. We are in a position to duplicate such research in that the present sample can be divided along the basis of many different characteristics. Yet to do so generates many different equations -- one for each group of offenders. To compare, say property offenders to drug offenders, across all the variables identified in the present chapter becomes a cumbersome task. Moreover, analyses by subgroup make it exceedingly difficult to determine when the impact of an independent variable is the same for all subgroups and which variables operate differently on the recidivism of subgroups.

A more parsimonious approach is to continue to operate with the same basic model used above, but allow for subgroup differences to be identified in the form of interaction terms between independent variables. From this perspective, the general model of rearrest presented in all the tables above represents essentially the impact of variables without consideration of

possible conditional relationships among them. As such, these results focus on the "main effects" of variables with no attention paid to how these effects might differ across different types of offenders. It seems quite plausible, however, that there are conditional relationships among the predictor variables, and in this section we outline our approach to studying them and the results obtained by incorporating these interaction terms into our analysis.

Two general problems arise when attempting to identify important interactions for the study of recidivism. First, there are not very many theoretical discussions in the literature of interaction effects. Often the importance of interaction terms is implicit in the comparison of empirical findings across groups of offenders. This leads us to characterize the basis for testing interaction effects as one grounded in "weak theory." Second, in the absence of "strong theory" dictating relatively few interactions to test for, there are potentially many interaction effects to be tested. That is, there are many "plausible" interactions that might investigated -- too many to actually test.

In the absence of theoretical guidance, we adopted a strategy that looked at two general classes of variables likely to mediate the impact of our independent variables on recidivism. One class are those variables which clearly define offender subgroups and are often used to generate specific samples. Race, ethnicity, and gender are considered to be basic variables to many sociological analyses, especially including those that pertain to crime and delinquency. The inclusion of presenting offense variables represents an attempt to differentiate (crudely) types of offenders according to three broad categories of offenses (property, violence, and drugs). Although offenders tend to be versatile in their offense behavior, there seems to be some

evidence in the literature as to a degree of specialization (Blumstein et al., 1988). In that presenting offense measures a type of offender, it was thought that any number of possible interactions were possible.

Since so many plausible interactions involving these variables came to mind, it was thought more expedient to simply test for all of them. Thus, for example, we tested bivariate interactions between the offender being African-American and all the other variables that met the inclusion criteria of the "main-effect" analysis presented above in Tables 5.6 and 5.7. Using the selection strategy adopted earlier, we regressed interaction terms on the same of dependent variables used to winnow the list of independent variables. Those interactions reaching statistical significance in at least 35% of all regressions were deemed sufficiently robust and retained. In essence, we have thus empirically determined where the basic model of Tables 5.6 and 5.7 must be modified for groups of males, females, whites, blacks, Hispanics, property offenders, persons offenders, drug offenders, and those with other types of presenting offenses.

The second class of interaction effects is less easily identified. These often involve continuous measures that do not neatly define subgroups or types of offenders. Here, we were more selective in which interactions were to be tested. In general, these interactions terms were selected to include variables representative of each of the six general sets of variables discussed above. A complete list of these potentially important interactions is given in Table 5.8.

Age, a "general control" variable, has been linked with recidivism in many studies, and the constancy of its effect has been greatly debated. Thus, several interaction effects were hypothesized involving age, especially variables hypothesized to mitigate the generally negative aging effect on

recidivism: unemployment, (not) living with a family, having a drug problem, having been treated for alcohol or drug problem, having a long history of offenses and prior incarcerations. That is, it was thought that having one or more of these problems would lessen the impact of aging, which was hypothesized to be negative in accordance with previous studies. Those interaction terms that passed the 35% inclusion criteria are marked with triple asterisks in Table 5.8.

Beyond age, several other variables were thought to be likely candidates for predicting recidivism. Several social characteristics of the offender were thought likely to be involved in interactions with one another and with other types of variables. Thus, family situation, whether or not high school was completed, unemployment status, drug and/or alcohol dependency were selected. As it turns out none of these passed the inclusion criteria, and all were dropped from the analysis.

It was also thought that there would be possible interaction effects between various social variables and anamnestic variables, such as number of charges in past five years (though that any of the anamnestic "count" variables could have been chosen for this purpose). Prior incarcerations were also tested for interactions across all domains of independent variables. Juvenile arrests were hypothesized to be involved in interaction with adult anamnestic variables, essentially, juvenile offense chronicity and adult offense chronicity.[26]

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[26] We are aware of the arbitrary nature of some of these selections, especially for the second class of interaction terms. Other variables could have been chosen but were not. Future research may focus on interaction effects not tested for here. Nevertheless, the interactions represent a broad spectrum of types of variables and theories, such that we think it is a reasonable foray into all the possible interaction effects that could have been hypothesized.

Using the selection strategy discussed earlier, interaction terms that passed the inclusion criteria of being statistically significant predictors at least 35% of the equations for the various indicators of recidivism were identified. This selection criterion resulted in surprisingly few interaction terms being retained. We find that the effects of being on probation at the presenting offense, prior number of adult arrests, prior property convictions, and prior charges as a juvenile operate differently on recidivism for blacks than for whites. For females, only an interaction term involving prior Part I charges was retained, and no interaction terms involving Hispanics were deemed robust. For groups defined on the basis of presenting offense, only the interaction between having a detainer and a presenting offense of violent persons crime was kept. For property offenders, interactions with adult arrests, prior property convictions, juvenile arrests, age as first incarceration, and years since first incarceration were found. Drug offenders differed in the ways in which adult convictions, prior Part I charges, and having their last parole revoked influenced their recidivism. Among the second class of interactions, only those involving age were retained. In particular, the effect of age on recidivism is mediated by having a drug problem, prior treatment for drugs/alcohol, being unemployed, having a presenting offense of property, and the number of charges in the last five years.

Interaction terms are inherently symmetric and it is not possible to give primacy to one of the variables involved in an interaction over the other. Yet several points can be made given our findings about which statistical interactions tend to be robust across different forms of recidivism. First, there are few differences in the main effects of our independent variables across subgroups defined by offender characteristics.

In terms of the absolute number of robust interactions identified, the volume of terms retained is small. This is reassuring and supports our main concern with a single equation used for all kinds of offenders.

Second, for those significant interactions retained, we find that how these independent variables influence recidivism is virtually identical for whites and Hispanics, and for males and females. Some differences are found for Black offenders, but these are localized to some criminal career variables (see below). For the vast majority of social structural variables in our analysis, their impact on recidivism is comparable across racial groups.

Third, type of offender, as represented by the nature of the presenting offense, does indeed matter, but only to a limited extent. Many more interactions involving the type of instant offense were dropped than were retained. At most, we find some evidence that these variables operate on recidivism differently for property offenders than for those convicted of other types of crimes. Age, as a mediating variable, is also important, but again where effects on recidivism differ by age at sentencing is quite limited.

Finally, the vast majority of robust interactions involve some aspect of criminal career indicators, usually ones suggested by anamnestic theory or juvenile delinquency. This is perhaps the most important insight to be gained from our search for mediating effects on recidivism. What we have found is that how prior record influences levels of recidivism is not constant across groups defined on the basis of some other variable. This raises serious questions about the unconditioned use of prior record indicators for decision making in the form of risk assessment or grid sentencing systems.

We repeat our earlier analyses, now incorporating the interaction terms that passed the selection criterion. The results of the communality analyses

(Tables 5.9 and 5.10) show that there is little impact on the unique contribution of the interactions to the explained variance of rearrest. The terms as a whole do not explain more than .007 of the variance in rearrest and this is constant across all windows. Relative to that variance that can be explained, the set of interactions have greater import at the one year window, uniquely accounting for 4% of the explainable variation in recidivism. This drops to around 2.5% for the remaining windows and this holds with or without hazards included in the model. This suggests that neither the hazard controls, nor the presence of interaction terms, make much difference to the overall prediction of rearrest.

The individual coefficients for particular variables are affected somewhat by the presence of interaction effects (Tables 5.11 and 5.12), as would be expected by the nature of interaction effects. Thus, when we examine the results of Table 5.11, we notice that the coefficient for being black is generally a little higher across the windows, compared to the coefficients presented in Table 5.6. The presence of a significant interaction effect, however, indicates that the effect of a variable cannot be ascertained by looking at its "main effect" coefficient alone. Rather, the effect of a variable is conditional upon the value of another variable, the other variable of the statistically significant interaction term.

There are four interaction terms involving the variable "being black" -- on probation at PO, number of prior adult arrests, number of prior property convictions, and number of charges as a juvenile. The first two of these variables can be interpreted as mitigating the overall positive relationship between being black and rearrest. Blacks on probation and blacks with more prior arrests as an adult are <u>less</u> likely than other blacks to recidivate. Alternatively, one could choose to interpret these results in terms of the

variables "on probation at PO" and "number of prior adult arrests." In general it is found that being on probation at the PO has a positive relation with rearrest. If the offender is black, this effect is mitigated. Similarly, number of prior adult arrests generally has a positive relationship with rearrest, but is again mitigated in the case of black offenders. This does not mean that black offenders on probation at the instant offense are less likely than others to be rearrested, it simply indicates that there is not an additive effect of these two variables. The effect of being black and the effect of being on probation at PO are still positive (i.e., both characteristics still enhance the likelihood of rearrest). Blacks with prior property convictions, however, are more likely to be rearrested than the simple additive "main effects" model indicates. The interactive term for blacks and number of charges as a juvenile is not statistically significant.

These results involving race in general are intriguing. They might be interpreted as meaning that there are specific deterrent effects of prior arrests and being on probation for blacks, but not for others. However, if a black offender is a repeat property offender, the chances of recidivism are increased. These results are indicative of the importance of testing for interaction effects in general. Minimally, it can no longer be assumed that the impact of being black can be understood adequately in terms of simple linear main effect models. The effect of being black on rearrest can be conceptualized as being an effect that depends on other variables.

While the general main effects model holds for females, it is conditioned by one interaction term: females with more prior Part I offenses were more likely to recidivate than those with less or none. After one year, <u>each</u> prior Part I charge increases the probability that a female will be rearrested by between 4 and 6 percent. (Alternatively, being female makes the

impact of prior Part I offenses significant, whereas it is not for males.) This suggests that chronicity involving serious crimes, when found among females, has an aggravating effect on the likelihood of rearrest so that even one previous Part I charge "erases" the lowered recidivism seen for females.

Two of the five interaction terms involving age were found to have fairly consistent mitigating effects on the rearrest dependent variable. Older offenders who had either been in an alcohol or drug treatment program or who had a presenting offense for property crime were less likely to be rearrested than older offenders in general. Another term, offender age and history of drug problems also had a mitigating effect on the probability of rearrest, but only in short run (first year and a half). The age and prior treatment interaction, as well as the short-run effects of age and having a history of problems suggest a "burn out" effect for offenders involved in drugs/alcohol or, alternatively, a rehabilitative effect. The effect of age and "presenting property offense" is more difficult to interpret. It is perhaps the case that older offenders are participating in property offenses of a different type than younger offenders, such that they are more likely to involve fraud or illegal gambling (no forms of gambling had yet been legalized in New Jersey). When committed by older people, these particular crimes may be less likely to be repeated and thus a specific deterrent effect is seen for the CJS intervention.

The interaction effect observed between "PO of violence" and "having detainers" gives further support to the idea that having detainers represents a data quality control variable -- offenders with detainers frequently end up out of New Jersey and less likely to be have their subsequent recidivism registered in New Jersey. Here, the results suggest that this is especially true of offenders committing violent crimes (and who thus are more likely to

serve longer sentences out of state, if that is the reason for their detainer). If part or all of the sentence is served out of state, that time may be missed in the present data set.

Only one of the five interactions involving the presenting property offense was consistently statistically significant in the models for rearrest -- number of juvenile arrests. It was found to have a negative effect on rearrest, which is a somewhat puzzling finding. Property offenders are generally more likely to recidivate, as are those with more arrests as a juvenile. In the case of property offenders with more frequent juvenile arrests however, the risks of recidivism are reduced. Given the magnitudes of the main and interaction effects for these variables, we estimate that, for property offenders, there is no relationship between prior juvenile arrests and the likelihood of rearrest. When the main effect of arrests as a juvenile are added to the impact of the interaction, the effects cancel each other.

Finally, the interactions involving the presenting offense of drugs, while statistically significant, involve "main effects" that are not statistically significant. Thus, interpreting these results must be done cautiously, as there is no relationship with the dependent variable for the interaction term effect to modify and observed statistically significant interaction terms cannot be interpreted in the presence of "main-effects" that are not statistically significant (Jaccard et al., 1990). Alternatively, we do find that anamnestic measures operate differently for drug offenders than for those with other presenting offenses. Increases in prior Part I charges are associated with lower recidivism for this group, while previous convictions as an adult are associated with an increased likelihood of rearrest.

Including the hazard variables has a negligible impact on the coefficients (see Table 5.12). As before, there are some differences in the magnitude of the estimated coefficients, but they are small and often only in the third or fourth decimal place. It is ironic that the interaction terms have a greater impact on the estimates of the main effects than the sample selection controls.

As is to be expected from the communality results, the interaction terms do little to aid in the prediction of rearrest. The pseudo R2 values reported on the bottom of Tables 5.11 and 5.12 are vary little from those discussed earlier. For example, at the "eight plus" window with the interactions, the pseudo R2 is .228, compared to the value of .223 without the interaction terms reported in Table 5.6. Similarly, the percent correct, percent false positives, percent false negatives, and RIOC values are virtually the same between Table 5.11 and Table 5.6. Controlling for the hazard variables has a negligible impact on the ability to predict as is seen by comparing Table 5.12, which includes the hazard variables and the interactions, with either Table 5.11, which excludes the hazard variables, but not the interactions, or with Table 5.7, which excludes the interactions, but not the hazards. Thus including interaction terms in the analysis results in some interesting qualifications as to the impact of several variables on rearrest, but has virtually no impact on predicting recidivism overall, according to the indicators of successful prediction interpreted here.

GENERAL SUBSTANTIVE CONCLUSIONS

The analysis of the probability of rearrest lays the framework for interpreting the results for other indicators of recidivistic behavior. Here we offer some of the more salient substantive implications of these results.

The structural variables, defined as those derived from social structural strain, cultural conflict, social ecology, social psychological strain, social bond, and social learning theories, provide general support for the idea that social attributes are important for understanding why offenders return to crime after sentencing. The evidence above suggests, for example, that it is not the offender's socio-economic status per se, but having no job, or no job to go to, that affects his or her recidivism chances. Thus support can be given to the idea that having a job is an important aspect of a convict's life in the community, but that social class is unimportant, at least as far as rearrest is concerned and once other variables are controlled. Somewhat to our surprise, interaction effects involving the offender's unemployment are not observed.

Racial and cultural differences in recidivism persist despite the statistical control of various factors known to be associated with these variables. Thus, for example, beyond unemployment, social class, and having drug problems, blacks and Hispanics are more likely to recidivate and to recidivate at higher rates than others. Similarly, men are more recidivistic than women. There are two fundamentally different interpretations commonly available in the literature, neither one of which can be dismissed on the basis of the research findings here. A "culturalist" interpretation defines the racial effect, for example, as attributable to the different value orientations of blacks in society. Criminal involvement of blacks may be due to alleged different value systems, which provide some positive reinforcement for risk-taking, for resort to violence, and for in-group status based on various offending behaviors. A "social structuralist" interpretation of the racial effects observed here may point to the relative lack of prospects of upward mobility among blacks, to the general demoralizing impact of the

experience of poverty among many blacks, and to the feeling of being excluded from the predominant institutions of the majority (see Loftin and Hill, 1974). It should be noted that the social structural interpretation would imply that various control variables such as unemployment, living in an urban area, having a history of drug problems, and so forth do not adequately measure the social structural position of minorities, and thus the effects of the variables black and Hispanic persist as representatives of structural differences. Similar alternative interpretations have been offered for the effects of gender.

Several interaction terms involving race and gender were observed to be statistically significant, such that one cannot properly speak of a unilateral race or gender effect: their respective effects vary as a function of other variables. Probation status and number of prior adult arrests among blacks results in a lower chance of rearrest than would be observed if each of these variables had unconditional effects on rearrest. Blacks convicted of prior property offenses have a higher likelihood of rearrest than others. Women with prior Part I offenses are more likely to recidivate than those without such prior offenses.

Offenders who return to live in an urban area, rather than a suburban or rural, arguably have more opportunities to commit crime, as well as being in an environment associated with low levels of informal social control and a high relative degree of anonymity. Detection of some forms of criminal involvement may also be easier within an urban environment. All factors may be at work.

Having a history of drug use has long been associated with other criminal activities. In that drugs are addictive, users need to buy or exchange services to maintain their disease. As such, drug addiction can be

seen as a cause of criminal activities. Another interpretation is that drug use is associated with criminal activities much as other life style attributes might be. Drug problems are symptomatic of crime problems. Whatever causes the individual to be an offender also cause drug taking. Although it is not possible to sort out this causal order question here, there is a relationship found across most windows between at least one of the "drug" history variables and recidivism. The predictor variables "history of drug problems" and "treatment for drugs/alcohol" are not related to rearrest in the presence of the interaction terms discussed above. However, having needle marks becomes statistically significant after four years whether or not interaction terms are in the equation (and whether or not hazards are controlled for). These findings may reflect the fact that there is some degree of redundancy across the drug history variables and the interaction terms that pertain to a drug presenting offense.

Other non-criminal social problems, health, emotional and mental problems were not found to be important, nor was the quality of the family life of the offender's parents. If drug taking and, possibly alcohol abuse, have a causal impact on crime, as many have argued, it does not seem that one can generalize the result to other social psychological conditions.

Breaking the bond of the individual to the occupational and educational institutions of society seems to enhance the chances and degree of criminal activities. It has been debated whether the primary mechanism through which this occurs is fear of negative sanctions, including loss of respect (those with jobs and high school diplomas have more to lose by criminal involvement than those without), or fear of losing the feeling of attachment to others that may result from knowledge of the individual's criminal activities (Hirschi, 1969). Whatever the intervening social psychological mechanism, it

does not seem to extend to the nature of family ties, since living with family or alone does not seem to have a negative effect, but rather has a positive one on rearrest (at least for most windows) -- a rather puzzling finding.

Crimes that are arguably more predatory, in that they involve strangers as victims, or were committed alone (although not crimes where the victim is of another race or sex) may be indicative of a type of offender who demonstrates a willingness to rationalize such behaviors and act (alone) on them. If these rationalizations are learned and are part of the more general dispositions, it is understandable that such individuals were be tempted to commit further crimes.

The interpretation of the theoretical significance of the criminal career variables centers around the notion of continuity in the offending behavior of the individuals. The more frequent the offending in the past, the more likely and frequent the offending in the future. Prior arrests and charges are related to rearrest, as is prior adult charges for which there are convictions. The latter effect is observed to be negative, suggesting a possible specific deterrent effect resulting from accumulated sanctions. Recent charges (last five years) or an incarceration in the last two years are both important to the prediction of rearrest, further supporting an emphasis on the continuity of behavior.

Both adult criminality, as well as juvenile delinquency, are important to predicting recidivism. Thus, it is not only what has the individual "done lately," but at what point in the individual's life cycle did criminal involvement begin. As well, the age at which the offender was first incarcerated and the number of prior incarcerations are significantly related to recidivism of some form.

One of the more interesting and puzzling findings of the analysis is the role that several anamnestic variables play. Their effects are not always positive on rearrest, and are frequently conditional on other variables, particularly race, gender and presenting offense of property. Furthermore, number of adult arrests and number of juvenile arrests are both involved in interactions resulting in less of a chance of rearrest than the additive linear model effects observed without interaction terms included. Specifically, blacks with prior adult arrests, and presenting offense property offenders with juvenile arrests are less likely to be rearrested than the additive linear model would suggest. These are effects that are difficult to interpret and we know of no precedent in the literature. It must not be ruled out, however, that the observed attenuation of the usual positive impact of the anamnestic variables is due to the redundancies of prior arrest variables in the model. In spite of the care taken to avoid the use of highly collinear independent variables, several measures of what some might call "the same thing" remain in the model (e.g., number of arrests, number of convictions, number of charge convictions), and the results observed may be a consequence.

As established in the section of variance partitioning, predicting recidivism may be defined as an activity of choosing among sets of predictor variables. If prediction alone is the goal, and one were limited to choosing one general type of predictor variable from the three categories of criminal career, presenting offense, or social characteristics, then the choice of criminal career variables would be optimal. If one utilizes all the variables, prediction would be enhanced somewhat by inclusion of the social factors and general control variables (e.g., age). If one chose one type of variable over the others, based on some a priori considerations, the general loss of predictive ability would not be total, and would only be about a third

if social attributes alone were chosen. Including the hazard variables in the equations has a substantial impact on the contributions to explained variance that can be attributed to any one category of independent variable or to the criminal career variables as a whole. Since the hazard variables themselves represent a variety of variable types, this result is not surprising. An interesting consequence is that the predictive power of the criminal career variables appear to be less than they are without the hazard variables.

If one's goal is not only to predict recidivism, but to achieve a greater understanding of the causal factors that generate recidivism, then the analysis of the regression coefficients would best allow for interpreting the impact of the various factors discussed above. Some have argued that the interpretive contribution of the social variables seems superior to that of the criminal career, even if the predictive ability is not (Gottfredson and Hirschi, 1986). Such a claim is contingent upon the conclusions of other researchers as to the causal importance of the social variables. This is a somewhat controversial matter in the criminological literature (Blumstein et al., 1987).

In short, some would argue that whereas prediction is maximized through the use of criminal career, and to a lesser extent by social structural variables, explanation is not appreciably enhanced (at least according to critics such as Hirschi). Whether or not one accepts the critiques of the criminal career approach, it seems clear that a large proportion of the prediction of recidivism is possible without it. About half of the variance explained can be attributed to social variables, whereas most of the variance explained can be accounted for by criminal career attributes. However, when controls for sample selection are introduced, the proportion of variance attributed to criminal career variables (and structural) drop considerably

(for structural variables, to about a third of the variance of rearrest; for criminal career variables, to about two-thirds of the variance of rearrest). If one accepts the etiological criticism of the criminal career variables and draws a conclusion to drop criminal career variables from consideration, then one must accept the consequential loss in predictive ability.

SUMMARY

The present chapter has been ambitious in scope, laying the groundwork for analyses that follow. Guided by various theories on the etiology of criminal behavior in general, a wide array of possible independent variables for the study of recidivism was identified. Six general domains for predictors of recidivism were established. An empirically based selection strategy was introduced to allow us to reduce the list to a more parsimonious set of independent variables. In the paring of reducing the list of independent variables, it was found that many representatives of the group of social structural variables were not robust predictors of recidivism. Once other factors have been controlled, variables such as socioeconomic status, ties to the church and community, and poor upbringing were found to add little to the prediction or understanding of variation in recidivism.

Also introduced in this chapter was our analytic strategy to have one basic model for the study of recidivism. Rather than having separate equations for each possible dependent variable, a single set of independent variables is to be used. Consistent with this philosophy, we used interaction terms as a surrogate for different models and recidivism processes within subgroups of offenders. While numerous interaction terms were investigated, the empirical selection strategy identified relatively few subgroups for which the implications of the basic model differed. Rather than finding that

recidivistic processes differed by groups of offenders defined on the basis of discrete characteristics (e.g., being Hispanic or female, type of presenting offense), our study of interaction effects has led to the conclusion that it is the anamnestic and prior record variables central to the criminal career perspective that do not have uniform effects on recidivism.

Finally, this chapter has featured an analysis of rearrest over 16 half-year windows, both as a way of familiarizing the reader with the independent variables to be used in subsequent analyses and to provide a preliminary investigation into the study of recidivism. As well, issues concerning the prediction of individual-level recidivism were raised: these are the focus of Chapter Nine.

The results here led to several general findings. First, the length of the post-sentence observation matters for the substantive conclusions reached. What is predictive of recidivism over the short run (e.g., a one-year window) differs from the long run. The magnitudes and significance of the effects of independent variables is tied to the width of observation window used. Second, variables from each domain have unique impacts upon recidivism defined by rearrest. While criminal career indicators are best at accounting for variation in recidivism, social structural indicators have identifiable relationships to subsequent criminal behavior. Third, the CJS processing of the case prior to sentencing, as represented by controls for sample selection bias, is itself a useful predictor of recidivism, accounting for about one quarter of the variance that can be explained. These results will be elaborated in the next chapter.

Fig. 5.1 Attribution of Explained Variance for Rearrest at Nine Years

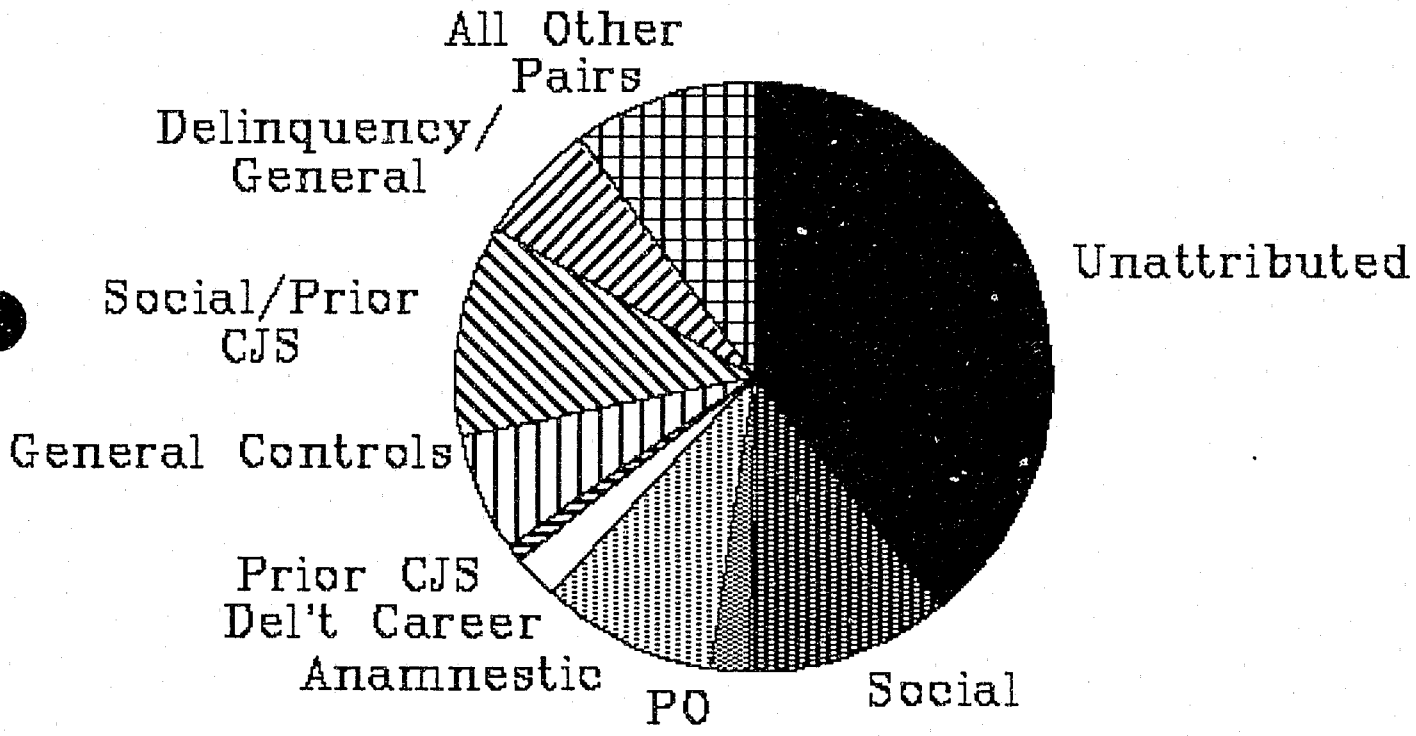

Table 5.1
List of Social Variables
(Minimum and Maximum Values in Brackets)

Social Structural Strain Theory	Mean	Std. Dev.
Socioeconomic status [1-4]	3.532	.687
Unemployed [0-1] **	.354	.478
Has job arranged after sentence [0-1] **	.463	.499
Financial necessities cause of crime [0-1]	.041	.197
Other financial problems cause of crime [0-1]	.307	.461
Offender is illiterate [0-1]	.041	.199
Offender is on welfare [0-1] **	.267	.443
Offender lives in house [0-1]	.373	.484
Offender sells drugs [0-1]	.127	.333
Cultural Conflict Theory		
Black [0-1] **	.438	.496
Hispanic [0-1] **	.073	.260
Female [0-1] **	.113	.317
Uses alias [0-1]	.149	.356
Cooperative at arrest [0-1]	.745	.436
Shows no remorse [0-1]	.196	.397
General "badness" [1-99] oo	37.267	24.294
Demonstrated poor judgment [0-1]	.186	.389
Number of illegitimate offspring [0-7]	.419	.913
Social Ecology Theory		
Lives in urban area [0-1] **	.429	.495
Lives in poor neighborhood [0-1]	.588	.492
Number of years at address [0-95] **	27.441	34.711
Not lived at address more than year [0-1]	.146	.353
Social Psychological Strain Theory		
Has history of mental problems [0-1]	.258	.437
Has history of alcohol problems [0-1]	.257	.437
Has history of drug problems [0-1]	.241	.427
Has been treated for drugs/alcohol [0-1] **	.174	.379
Offender known to use drugs [0-1] **	.182	.386
Has needle marks [0-1] **	.070	.255
Offender abused as a child [0-1]	.077	.266
Health problems cause of crime [0-1]	.014	.119
Emotional problems cause of crime [0-1]	.106	.307
Family life/upbringing cause of crime [0-1]	.045	.208
Birth Order [1-9]	4.427	3.284

** Variable retained for analyses
oo Variable dropped due to multicollinearity

Table 5.1 (Continued)

List of Social Variables
(Minimum and Maximum Values in Brackets)

Social Bond Theory	Mean	Std. Dev.
Years of education completed [1-19]	10.315	2.366
Dropped out of school [0-1] **	.620	.485
Has steady job [0-1]	.359	.480
Number of jobs past five years [0-97]	22.862	39.138
Lives with spouse/family [0-1] **	.786	.410
Married [0-1]	.305	.460
Number of children [0-7]	1.241	1.572
Supports children [0-1]	.264	.441
Attends church frequently [0-1]	.222	.416
Attends clubs frequently [0-1]	.105	.307
Social Learning Theory		
"Bad companions" cause of crime [0-1]	.114	.317
Offender ringleader of crime [0-1]	.108	.301
Offender committed crime with others [0-1] **	.533	.499
Victim was a stranger [0-1] **	.302	.459
Victim was of other sex or race [0-1]	.141	.348
Victim was a business establishment [0-1]	.164	.370
Crime committed in open place [0-1]	.380	.485
Crime not committed in a home [0-1]	.467	.499

** Variable retained for analyses
oo Variable dropped due to multicollinearity

Table 5.2

Presenting Offense, Criminal Career, and Control Variables
(Minimum and Maximum Values in Brackets)

Characteristics of Presenting Offense	Mean	Std. Dev.
PO property crime [0-1] **	.408	.491
PO crime against the person [0-1] **	.198	.398
PO auto theft [0-1]	.017	.127
PO weapons [0-1]	.156	.363
PO drug possession or sale [0-1]	.240	.420
Offender had weapon [0-1]	.281	.450
Victim injured at PO [0-1]	.098	.298
Dollar value stolen/damaged [0-88887]	368.886	3033.290
Number of PO convictions [1-68]	1.691	1.913
PO Wolfgang severity score [1-68] **	7.210	7.836
Coders severity score [1-99]	42.446	24.769
Number of victims [0-93]	8.830	26.250
In jail/prison at arrest [0-1]	.026	.158
Has detainers at arrest [0-1] **	.065	.246
Has pending charges at arrest [0-1] **	.200	.400
Used drugs at PO [0-1]	.095	.293
Used alcohol at PO [0-1]	.163	.369
On probation at PO [0-1] **	.199	.399
On parole at PO [0-1]	.103	.304
In prison/jail at PO or sentencing [0-1]	.071	.257
Sentenced with other matters [0-1]	.072	.259
Anamnestic Theory		
Number of prior arrests [0-40] **	3.138	4.448
Number of prior adult convictions [0-31] **	2.802	3.474
Number of prior conviction charges [0-61] **	2.405	4.121
Number of prior adult charges [0-98] oo	4.950	7.488
Number of charges in past five years [0-101] **	4.824	5.168
Number of Part 1 charges [0-29] **	1.099	2.323
Number of prior property charges [0-93] oo	2.181	4.644
Number of prior persons charges [0-28]	.709	1.542
Number of prior weapons charges [0-8]	.187	.567
Number of prior property chg. convictions [0-52] **	1.143	2.680
Number of prior persons chg. convictions [0-15] **	.287	.799
Number of prior weapons chg. convictions [0-5] **	.087	.347
Has history of escape [0-1]	.075	.263
Free on street for past two years [0-1] **	.709	.454
Delinquent Career and Age of Onset		
Number of juvenile arrests [0-18] **	1.351	2.522
Number of juvenile convictions [0-10] **	.301	.531
Age of first arrest [8-68] **	20.976	7.601
Age of first incarceration [9-66] **	21.307	6.032
Age of first drug use [1-53] **	17.305	4.575
Length of criminal career [0-66] oo	6.239	7.163

** Variable retained for analyses
oo Variable dropped due to multicollinearity

Table 5.2 (Continued)

Presenting Offense, Criminal Career, and Control Variables
(Minimum and Maximum Values in Brackets)

Prior CJS-Offender Action	Mean	Std. Dev.
Number of prior incarcerations/jailings [0-20] **	1.234	2.205
Number of prior probation sentences [0-10]	.490	.900
Number of prior parole revocations [0-6] **	.119	.475
Number of prior probation revocations [0-9]	.184	.569
Prior days incarcerated [0-8000]	200.340	602.920
Bad conduct in latest parole [0-1]	.085	.279
Bad conduct in latest probation [0-1] **	.187	.390
Recent parole revoked [0-1] **	.059	.235
Other Variables		
Offender age at sentencing [18-73] **	28.011	9.260
Offender born out of state [0-1] **	.459	.498
Probation officer prognosis [1-99] **	53.699	28.164
Coder's rating of prior record severity [1-99]	30.340	26.780
Probation officer for a probation sentence [0-1]	.350	.480
Probation off. for incarcerative sentence [0-1]	.210	.400
Coder assesses PSI sketchy or incomplete [0-1]	.230	.421
Number of PO charges missing on rap sheet [0-15]	.113	.528

** Variable retained for analyses
oo Variable dropped due to multicollinearity

Table 5.3
Attribution of Explained Variance for Rearrest by Selected Windows

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.165)	(.238)	(.254)	(.250)
Percent R2 Unattributed	46.38	45.69	43.83	39.38
	(.077)	(.109)	(.111)	(.098)
Social Variables	6.17	8.04	9.48	11.08
	(.010)	(.019)	(.024)	(.028)
Presenting Offense	3.46	2.91	2.47	2.25
	(.006)	(.007)	(.006)	(.006)
Anamnestic Variables	9.28	9.34	8.54	9.42
	(.015)	(.022)	(.022)	(.024)
Delinquent Career/Onset	3.80	3.29	3.04	2.42
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.65	.30	.40	.82
	(.001)	(.001)	(.001)	(.002)
General Controls	4.28	5.32	5.65	6.43
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.23	.43	.39
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	1.28	1.54	1.66	1.89
	(.002)	(.004)	(.004)	(.005)
Social/Delinquent Career	1.88	2.14	2.50	2.50
	(.003)	(.005)	(.006)	(.006)
Social/Prior CJS Actions	7.06	8.36	9.80	11.66
	(.012)	(.020)	(.025)	(.029)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.39	2.10	1.87	1.96
	(.004)	(.005)	(.005)	(.005)
Presenting Offense/Delinquency	.00*	.11	.18	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.24	.41	.40	.48
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.31	.44	.43	.46
	(.001)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.85	1.84	1.56	1.44
	(.005)	(.004)	(.004)	(.004)
Anamnestic/Prior CJS Actions	4.44	1.92	1.16	.03
	(.007)	(.005)	(.003)	(.000)
Anamnestic/General Controls	3.34	2.98	2.36	2.15
	(.006)	(.007)	(.006)	(.005)
Delinquency/Prior CJS Actions	.77	.13	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	2.04	3.56	5.37	6.81
	(.003)	(.008)	(.014)	(.017)
Prior CJS Actions/Controls	.13	.20	.20	.26
	(.000)	(.000)	(.001)	(.001)

Table 5.4
Attribution of Explained Variance for Rearrest by Selected Windows

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.167)	(.238)	(.255)	(.250)
Percent R2 Unattributed	25.29	24.78	23.31	20.38
	(.042)	(.059)	(.059)	(.051)
Attributed to Hazards	26.08	26.91	27.45	25.38
	(.044)	(.064)	(.070)	(.064)
Social Variables	5.25	6.97	8.12	9.84
	(.009)	(.017)	(.021)	(.025)
Presenting Offense	3.21	2.59	2.14	2.00
	(.005)	(.006)	(.005)	(.005)
Anamnestic Variables	8.57	8.74	7.98	8.93
	(.014)	(.021)	(.020)	(.022)
Delinquent Career/Onset	3.69	3.20	2.95	2.40
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.67	.25	.34	.71
	(.001)	(.001)	(.001)	(.002)
General Controls	4.37	5.29	5.65	6.44
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.00*	.04	.03
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.90	.92	.97	1.10
	(.002)	(.002)	(.002)	(.003)
Social/Delinquent Career	1.44	1.57	1.85	1.93
	(.002)	(.004)	(.005)	(.005)
Social/Prior CJS Actions	6.10	7.24	8.42	10.40
	(.010)	(.017)	(.021)	(.026)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.79	1.44	1.21	1.33
	(.003)	(.003)	(.003)	(.003)
Presenting Offense/Delinquency	.00*	.00*	.01	.01
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.17	.34	.31	.38
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.10	.35	.39	.42
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.60	1.55	1.29	1.21
	(.004)	(.004)	(.003)	(.003)
Anamnestic/Prior CJS Actions	4.14	1.78	1.07	.07
	(.007)	(.004)	(.003)	(.000)
Anamnestic/General Controls	3.59	3.18	2.54	2.36
	(.006)	(.008)	(.006)	(.006)
Delinquency/Prior CJS Actions	.77	.16	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	1.98	3.15	4.63	5.92
	(.003)	(.008)	(.012)	(.015)
Prior CJS Actions/Controls	.11	.19	.19	.24
	(.000)	(.000)	(.000)	(.001)

Table 5.5

Proportion of Variance Explained in Rearrest by Set of Independent Variables, Entered Alone

Type of Variable	1 Year	3 Year	5 Year	9 Year
Social	.077	.118	.130	.128
Presenting Offense	.046	.069	.072	.067
Anamnestic	.121	.163	.165	.155
Delinquent Career/Onset	.098	.143	.158	.151
Prior CJS/Offender Action	.065	.081	.079	.068
General Controls	.073	.113	.123	.125
All Criminal Career	.149	.208	.218	.210
Total R2 (Unadjusted)	.165	.238	.254	.250

Proportion of Variance Explained in Rearrest by Set of Independent Variables, Entered After Hazard Controls

Type of Variable	1 Year	3 Year	5 Year	9 Year
Social	.046	.070	.078	.080
Presenting Offense	.025	.037	.039	.037
Anamnestic	.083	.108	.107	.102
Delinquent Career/Onset	.064	.091	.102	.099
Prior CJS/Offender Action	.039	.044	.042	.035
General Controls	.053	.083	.090	.092
All Criminal Career Vars.	.108	.146	.151	.149
Hazard Variables	.044	.064	.070	.064
Total R2 (Unadjusted)	.167	.238	.255	.250

Table 5.6

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0192*	.0217*	.0360**	.0368**	.0315**	.0286*	.0302*	.0327**
Has job after sentence	-.0210**	-.0369***	-.0318**	-.0287*	-.0300*	-.0245*	-.0214	-.0267*
Offender is on welfare	-.0078	-.0232*	-.0206	-.0087	-.0009	.0055	-.0041	-.0014
Offender is Black	.0390***	.0654***	.0854***	.0945***	.1140***	.1252***	.1286***	.1395***
Offender is Hispanic	.0543**	.0722***	.0863***	.1028***	.1256***	.1375***	.1452***	.1529***
Offender is female	-.0257	-.0527***	-.0641***	-.0741***	-.0723***	-.0889***	-.0952***	-.0999***
Lives in urban area	.0123	.0209*	.0302**	.0404***	.0345**	.0382**	.0408***	.0368**
Years at current address	-.0013**	-.0014**	-.0017**	-.0018**	-.0016**	-.0014*	-.0012*	-.0012*
History of drug problems	.0075	.0197	.0218	.0218	.0231	.0371*	.0336*	.0331*
Treated for drugs/alch.	.0018	-.0048	.0014	.0147	.0177	.0150	.0190	.0233
Has needle marks	.0005	.0014	.0023	.0204	.0279	.0282	.0296	.0428
Not a school drop out	-.0278***	-.0347***	-.0430***	-.0487***	-.0524***	-.0536***	-.0508***	-.0507***
Doesn't live with family	.0051	-.0037	-.0077	-.0022	-.0084	-.0133	-.0211	-.0343*
Commited PO with group	-.0218**	-.0244**	-.0312***	-.0306**	-.0307**	-.0389***	-.0390***	-.0396***
Victim was a stranger	.0206*	.0234*	.0270*	.0352**	.0396**	.0396**	.0342*	.0377**
Presenting Offense								
PO property crime	.0212	.0183	.0309*	.0379*	.0462**	.0428**	.0420**	.0363*
PO crime against person	-.0062	-.0302*	-.0084	-.0170	-.0076	-.0009	.0042	.0010
PO drug offense	.0115	-.0122	-.0001	-.0059	-.0035	-.0083	-.0132	-.0190
PO Wolfgang severity	-.0017**	-.0017*	-.0018*	-.0017*	-.0017*	-.0025**	-.0023**	-.0023**
Has detainers at arrest	-.0282*	-.0350*	-.0456**	-.0579**	-.0537**	-.0568**	-.0649**	-.0616**
Has pending charges	.0414***	.0577***	.0665***	.0793***	.0813***	.0868***	.0936***	.0942***
On probation at PO	.0241**	.0340**	.0328**	.0333*	.0354**	.0423**	.0436**	.0423**
Anamnestic Theory								
N prior adult arrests	.0075***	.0116***	.0139***	.0183***	.0220***	.0253***	.0263***	.0274***
N prior adult conviction	.0006	.0047	.0069*	.0046	.0051	.0031	.0024	.0024
N prior adult chg. conv.	-.0051*	-.0083**	-.0093**	-.0109**	-.0118**	-.0136***	-.0148***	-.0172***
N charges past 5 years	.0029**	.0061***	.0064***	.0077***	.0091***	.0095***	.0108***	.0108***
N prior Part 1 charges	.0031	.0073*	.0070	.0057	.0069	.0075	.0064	.0101
N prior property conv.	-.0002	-.0013	.0004	.0003	-.0013	.0004	.0000	.0006
N prior persons conv.	.0027	.0010	.0013	.0018	-.0010	.0024	.0064	.0035
N prior weapons conv.	.0111	.0317**	.0337*	.0404**	.0414**	.0261	.0205	.0393*
Off street last 2 years	.0250**	.0535***	.0701***	.0860***	.0955***	.0996***	.0981***	.0971***
Delinquent Career/Onset								
N arrests as juvenile	.0038*	.0065**	.0089***	.0083**	.0090**	.0099**	.0092**	.0092**
N charges as juvenile	.0143	.0152	.0187	.0357**	.0380**	.0303*	.0325*	.0363*
Age at first arrest	.0003	-.0003	-.0007	-.0004	-.0009	.0003	-.0001	-.0004
Yrs since first incarc.	.0009***	.0011***	.0014***	.0016***	.0017***	.0017***	.0017***	.0018***
Yrs since first drug use	-.0002	-.0005	-.0005	-.0008*	-.0007	-.0008	-.0005	-.0003
Prior CJS-Offender Action								
N prior incarcerations	.0034	-.0024	-.0065	-.0061	-.0107*	-.0110*	-.0088	-.0084
N prior parole revokes	-.0001	-.0039	-.0174	-.0113	-.0027	.0003	.0034	-.0024
Bad conduct last probat.	.0103	.0231*	.0403**	.0304*	.0318*	.0255	.0309*	.0260
Recent parole revoked	.0190	.0359	.0376	.0175	.0130	.0069	-.0021	.0090
General Control Variables								
Offender age at sent.	-.0074***	-.0079***	-.0088***	-.0090***	-.0088***	-.0100***	-.0100***	-.0103***
Off. born out of state	-.0354***	-.0478***	-.0604***	-.0739***	-.0832***	-.0895***	-.0937***	-.1004***
Coder prob. prognosis	-.0003	-.0005**	-.0007***	-.0008***	-.0009***	-.0011***	-.0009***	-.0009***
Constant	-.1290***	-.1702***	-.1904***	-.1915***	-.1864***	-.1716***	-.1528***	-.1289***
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.100	.143	.169	.186	.198	.206	.210	.218
Model Classifications:								
% Correct Predictions	85.23	78.53	75.28	73.67	73.59	72.98	72.68	72.63
% False Positives	.97	4.27	6.80	8.32	9.10	10.01	10.57	11.10
% False Negatives	13.80	17.20	17.92	18.00	17.31	17.01	16.75	16.27
RIOC	.413	.466	.470	.477	.499	.494	.493	.491

* p<.05 ** p<.01 *** p<.001

Table 5.6 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(N = 11,749)

Independent Variable	Proportion Rearrested by Year: 4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0318*	.0329**	.0358**	.0305*	.0302*	.0330**	.0326*	.0279*
Has job after sentence	-.0268*	-.0221	-.0186	-.0194	-.0221	-.0216	-.0172	-.0182
Offender is on welfare	.0012	.0059	.0008	.0065	.0003	.0033	.0102	.0108
Offender is Black	.1396***	.1375***	.1435***	.1397***	.1447***	.1441***	.1465***	.1495***
Offender is Hispanic	.1540***	.1534***	.1563***	.1580***	.1623***	.1605***	.1630***	.1578***
Offender is female	-.1026***	-.1130***	-.1120***	-.1104***	-.1142***	-.1131***	-.1160***	-.1257***
Lives in urban area	.0403**	.0433***	.0443***	.0424***	.0422***	.0429***	.0395**	.0366**
Years at current address	-.0012*	-.0014*	-.0013*	-.0012*	-.0014*	-.0015*	-.0015**	-.0013*
History of drug problems	.0336*	.0327*	.0390*	.0397*	.0375*	.0385*	.0360*	.0361*
Treated for drugs/alch.	.0229	.0254	.0344*	.0487**	.0519**	.0497**	.0464**	.0416*
Has needle marks	.0557*	.0737**	.0748**	.0742**	.0746**	.0698**	.0692**	.0812**
Not a school drop out	-.0512***	-.0568***	-.0561***	-.0572***	-.0561***	-.0563***	-.0567***	-.0586***
Doesn't live with family	-.0436**	-.0514***	-.0465***	-.0485***	-.0471***	-.0508***	-.0498***	-.0397**
Commited PO with group	-.0455***	-.0401***	-.0365**	-.0363**	-.0331**	-.0305**	-.0360**	-.0414***
Victim was a stranger	.0284*	.0272	.0270	.0283*	.0299*	.0315*	.0317*	.0314*
Presenting Offense								
PO property crime	.0405**	.0495**	.0548***	.0552***	.0523***	.0521***	.0463**	.0485**
PO crime against person	.0049	.0157	.0212	.0294	.0248	.0262	.0204	.0265
PO drug offense	-.0217	-.0084	-.0055	-.0020	-.0067	-.0074	-.0059	.0000
PO Wolfgang severity	-.0020*	-.0019*	-.0018*	-.0020*	-.0021*	-.0021**	-.0021*	-.0024**
Has detainers at arrest	-.0612**	-.0633**	-.0656**	-.0697**	-.0794**	-.0736**	-.0700**	-.0780**
Has pending charges	.0945***	.0907***	.0911***	.0870***	.0873***	.0838***	.0834***	.0764***
On probation at PO	.0399**	.0358*	.0329*	.0334*	.0376*	.0386*	.0410**	.0380*
Anamnestic Theory								
N prior adult arrests	.0281***	.0272***	.0267***	.0264***	.0274***	.0289***	.0320***	.0335***
N prior adult conviction	.0045	.0068	.0058	.0077	.0090*	.0119**	.0104*	.0079
N prior adult chg. conv.	-.0174***	-.0165***	-.0149***	-.0143***	-.0136**	-.0152***	-.0153***	-.0164***
N charges past 5 years	.0107***	.0102***	.0107***	.0108***	.0107***	.0107***	.0106***	.0124***
N prior Part 1 charges	.0098	.0094	.0076	.0069	.0056	.0075	.0072	.0085
N prior property conv.	.0009	.0022	.0026	.0028	.0017	.0013	.0004	-.0002
N prior persons conv.	.0060	.0072	.0079	.0085	.0087	.0064	.0043	.0019
N prior weapons conv.	.0332	.0309	.0318	.0438*	.0450*	.0471*	.0442*	.0522**
Off street last 2 years	.0976***	.1029***	.1040***	.1036***	.1066***	.1045***	.1068***	.1016***
Delinquent Career/Onset								
N arrests as juvenile	.0106**	.0105**	.0095**	.0112**	.0110**	.0109**	.0121**	.0120**
N charges as juvenile	.0383*	.0369*	.0408**	.0395*	.0435**	.0444**	.0408**	.0406**
Age at first arrest	-.0006	-.0013	-.0017	-.0019	-.0016	-.0015	-.0015	-.0017
Yrs since first incarc.	.0018***	.0017***	.0016***	.0017***	.0016***	.0015***	.0015***	.0012***
Yrs since first drug use	-.0003	-.0006	-.0007	-.0007	-.0006	-.0005	-.0004	-.0005
Prior CJS-Offender Action								
N prior incarcerations	-.0128*	-.0151**	-.0172**	-.0211***	-.0226***	-.0270***	-.0269***	-.0263***
N prior parole revokes	-.0003	-.0110	-.0163	-.0127	-.0134	-.0095	-.0115	-.0090
Bad conduct last probat.	.0132	.0148	.0114	.0036	-.0040	.0001	-.0015	-.0009
Recent parole revoked	.0045	.0079	.0264	.0206	.0277	.0246	.0176	.0272
General Control Variables								
Offender age at sent.	-.0108***	-.0100***	-.0092***	-.0090***	-.0089***	-.0089***	-.0092***	-.0092***
Off. born out of state	-.1009***	-.1089***	-.1122***	-.1134***	-.1176***	-.1213***	-.1218***	-.1273***
Coder prob. prognosis	-.0009***	-.0009***	-.0009***	-.0009***	-.0008***	-.0008***	-.0009***	-.0008***
Constant	-.1041***	-.0920***	-.0899***	-.0738***	-.0585**	-.0482*	-.0348	-.0064
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.222	.223	.223	.225	.224	.225	.225	.223
Model Classifications:								
% Correct Predictions	72.47	72.38	72.15	72.46	72.47	72.86	72.91	72.92
% False Positives	11.63	12.20	12.55	12.68	12.89	12.89	13.08	13.69
% False Negatives	15.91	15.42	15.30	14.86	14.64	14.25	14.01	13.40
RIOC	.485	.476	.468	.470	.466	.469	.465	.455

* p<.05 ** p<.01 *** p<.001

Table 5.7

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean (Controlling for Sample Selection) (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0199*	.0228*	.0370***	.0377**	.0322**	.0294*	.0309*	.0333**
Has job after sentence	-.0212**	-.0370***	-.0318**	-.0283*	-.0296*	-.0244*	-.0212	-.0265*
Offender is on welfare	-.0081	-.0234*	-.0206	-.0086	-.0008	.0055	-.0040	-.0012
Offender is Black	.0346***	.0584***	.0752***	.0853***	.1067***	.1224***	.1267***	.1381***
Offender is Hispanic	.0525**	.0702***	.0843***	.1033***	.1252***	.1355***	.1425***	.1489***
Offender is female	-.0316*	-.0592***	-.0705***	-.0769***	-.0770***	-.0943***	-.1000***	-.1059***
Lives in urban area	.0068	.0130	.0220*	.0351**	.0286*	.0325**	.0353**	.0306*
Years at current address	-.0013**	-.0014**	-.0017**	-.0019***	-.0017**	-.0015*	-.0012*	-.0012*
History of drug problems	.0090	.0212	.0232	.0224	.0239	.0380*	.0340*	.0335*
Treated for drugs/alch.	.0023	-.0045	.0019	.0146	.0177	.0152	.0191	.0235
Has needle marks	-.0030	-.0035	-.0035	.0171	.0249	.0252	.0266	.0392
Not a school drop out	-.0291***	-.0365***	-.0450***	-.0501***	-.0537***	-.0548***	-.0519***	-.0519***
Doesn't live with family	.0059	-.0025	-.0063	-.0009	-.0072	-.0126	-.0207	-.0341*
Commited PO with group	-.0180**	-.0191*	-.0257**	-.0263**	-.0259*	-.0341**	-.0348**	-.0353**
Victim was a stranger	.0237**	.0262*	.0305*	.0378**	.0424**	.0424**	.0361**	.0397**
Presenting Offense								
PO property crime	.0180	.0074	.0196	.0268	.0383*	.0350*	.0328*	.0281
PO crime against person	-.0089	-.0377*	-.0160	-.0241	-.0152	-.0088	-.0051	-.0088
PO drug offense	.0249	.0001	.0181	.0140	.0156	.0079	-.0014	-.0082
PO Wolfgang severity	-.0017**	-.0018**	-.0018*	-.0018*	-.0018*	-.0025**	-.0024**	-.0023**
Has detainers at arrest	-.0283*	-.0354*	-.0455*	-.0578**	-.0536**	-.0571**	-.0656**	-.0624**
Has pending charges	.0417***	.0581***	.0670***	.0790***	.0807***	.0861***	.0929***	.0935***
On probation at PO	.0235*	.0329**	.0316*	.0318*	.0343*	.0412**	.0424**	.0413**
Anamnestic Theory								
N prior adult arrests	.0071***	.0109***	.0130***	.0176***	.0215***	.0248***	.0258***	.0269***
N prior adult conviction	.0002	.0042	.0063	.0039	.0047	.0028	.0022	.0023
N prior adult chg. conv.	-.0049*	-.0080**	-.0091**	-.0110**	-.0118**	-.0135***	-.0147***	-.0170***
N charges past 5 years	.0030**	.0063***	.0065***	.0077***	.0091***	.0096***	.0108***	.0109***
N prior Part 1 charges	.0032	.0074*	.0072	.0059	.0071	.0076	.0065	.0102
N prior property conv.	-.0002	-.0014	.0004	.0003	-.0014	.0003	-.0001	.0004
N prior persons conv.	.0024	.0004	.0008	.0015	-.0016	.0019	.0059	.0030
N prior weapons conv.	.0095	.0293*	.0310*	.0379*	.0385*	.0244	.0189	.0377*
Off street last 2 years	.0246**	.0531***	.0690***	.0850***	.0949***	.0992***	.0979***	.0970***
Delinquent Career/Onset								
N arrests as juvenile	.0033*	.0058**	.0081**	.0078**	.0085**	.0093**	.0086**	.0086**
N charges as juvenile	.0159	.0175	.0207	.0370**	.0398**	.0322*	.0343*	.0383**
Age at first arrest	.0003	-.0003	-.0008	-.0005	-.0010	.0002	-.0002	-.0005
Yrs since first incarc.	.0009***	.0012***	.0014***	.0016***	.0017***	.0017***	.0017***	.0018***
Yrs since first drug use	-.0002	-.0005	-.0005	-.0008*	-.0007	-.0008	-.0005	-.0003
Prior CJS-Offender Action								
N prior incarcerations	.0044	-.0008	-.0051	-.0044	-.0089	-.0094	-.0073	-.0070
N prior parole revokes	.0014	-.0023	-.0161	-.0105	-.0016	.0017	.0045	-.0013
Bad conduct last probat.	.0097	.0218	.0390**	.0297*	.0309*	.0246	.0299	.0248
Recent parole revoked	.0203	.0380	.0394	.0189	.0144	.0082	-.0009	.0101
General Control Variables								
Offender age at sent.	-.0080***	-.0087***	-.0097***	-.0096***	-.0096***	-.0107***	-.0107***	-.0111***
Off. born out of state	-.0348***	-.0468***	-.0591***	-.0728***	-.0820***	-.0883***	-.0927***	-.0993***
Coder prob. prognosis	-.0003	-.0006**	-.0007***	-.0008***	-.0009***	-.0011***	-.0010***	-.0009***

* p<.05 ** p<.01 *** p<.001

Table 5.7 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (Controlling for Sample Selection) (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Selection Hazards								
UCR to SAC arrest histry	-.0152	-.0374**	-.0306	-.0234	-.0197	-.0176	-.0240	-.0237
Case proceeds past arrst	.2311**	.2829***	.3079***	.2591**	.2245**	.1697*	.1321	.1162
Case to Grand Jury	-.0002	-.0030	-.0019	-.0060	-.0128	-.0064	-.0083	-.0104
Case to Superior Court	.0572	.0818*	.0554	.0502	.0667	.0737	.0715	.0712
Superior Court Convict.	-.0072	-.0180	-.0296	-.0391	-.0199	-.0245	-.0242	-.0183
Match over data sources	-.0063	-.0092	-.0147	-.0386	-.0308	.0000	.0170	.0351
Constant	-.1345***	-.1839***	-.2077***	-.2052***	-.2086***	-.2004***	-.1772***	-.1572***
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.101	.145	.171	.186	.199	.207	.211	.219
Model Classifications:								
% Correct Predictions	85.28	78.48	75.30	73.89	73.43	72.93	72.65	72.69
% False Positives	.91	4.26	6.78	8.24	9.23	10.07	10.67	11.10
% False Negatives	13.81	17.25	17.92	17.87	17.35	17.00	16.67	16.21
RIOC	.429	.464	.471	.484	.494	.492	.490	.492

* p<.05 ** p<.01 *** p<.001

Table 5.7 (continued)

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean (Controlling for Sample Selection) (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0322*	.0333**	.0359**	.0305*	.0304*	.0333**	.0329**	.0283*
Has job after sentence	-.0267*	-.0217	-.0183	-.0191	-.0223	-.0220	-.0175	-.0187
Offender is on welfare	.0015	.0063	.0012	.0070	.0005	.0034	.0101	.0106
Offender is Black	.1374***	.1325***	.1389***	.1354***	.1430***	.1426***	.1449***	.1489***
Offender is Hispanic	.1488***	.1510***	.1536***	.1537***	.1573***	.1565***	.1602***	.1559***
Offender is female	-.1102***	-.1181***	-.1179***	-.1172***	-.1237***	-.1219***	-.1248***	-.1337***
Lives in urban area	.0332**	.0375**	.0391**	.0365**	.0355**	.0368**	.0336**	.0317*
Years at current address	-.0012*	-.0014*	-.0013*	-.0012*	-.0014*	-.0015*	-.0016**	-.0014*
History of drug problems	.0341*	.0331*	.0395*	.0400*	.0385*	.0397*	.0374*	.0376*
Treated for drugs/alch.	.0234	.0257	.0348*	.0492**	.0527**	.0506**	.0472**	.0424*
Has needle marks	.0508*	.0702**	.0714**	.0699**	.0699**	.0656*	.0659**	.0789**
Not a school drop out	-.0525***	-.0578***	-.0569***	-.0583***	-.0574***	-.0575***	-.0578***	-.0596***
Doesn't live with family	-.0434**	-.0508***	-.0460***	-.0481***	-.0468***	-.0502***	-.0489***	-.0388**
Commited PO with group	-.0413***	-.0361**	-.0332**	-.0331**	-.0287*	-.0258*	-.0307**	-.0363**
Victim was a stranger	.0307*	.0289*	.0291*	.0305*	.0333*	.0348*	.0353*	.0349*
Presenting Offense								
PO property crime	.0343*	.0424*	.0529**	.0533**	.0511**	.0494**	.0442**	.0464**
PO crime against person	-.0043	.0075	.0159	.0235	.0190	.0207	.0145	.0214
PO drug offense	-.0113	.0021	.0050	.0096	.0057	.0033	.0078	.0119
PO Wolfgang severity	-.0020*	-.0019*	-.0018*	-.0020*	-.0021*	-.0021*	-.0021*	-.0023**
Has detainers at arrest	-.0618**	-.0636**	-.0654**	-.0696**	-.0792**	-.0733**	-.0696**	-.0777**
Has pending charges	.0940***	.0903***	.0907***	.0865***	.0871***	.0838***	.0831***	.0762***
On probation at PO	.0391**	.0350*	.0325*	.0330*	.0374*	.0383*	.0406**	.0377*
Anamnestic Theory								
N prior adult arrests	.0275***	.0267***	.0263***	.0259***	.0269***	.0283***	.0316***	.0332***
N prior adult conviction	.0047	.0068	.0060	.0081	.0094*	.0121**	.0105*	.0079
N prior adult chg. conv.	-.0171***	-.0163***	-.0147***	-.0140***	-.0133**	-.0149***	-.0150***	-.0161***
N charges past 5 years	.0108***	.0102***	.0107***	.0108***	.0108***	.0108***	.0107***	.0125***
N prior Part 1 charges	.0100	.0095	.0077	.0071	.0059	.0078	.0075	.0088
N prior property conv.	.0007	.0020	.0023	.0026	.0015	.0012	.0002	-.0003
N prior persons conv.	.0054	.0066	.0073	.0080	.0081	.0058	.0035	.0012
N prior weapons conv.	.0314	.0286	.0296	.0418*	.0436*	.0460*	.0427*	.0510**
Off street last 2 years	.0972***	.1026***	.1037***	.1032***	.1062***	.1041***	.1067***	.1016***
Delinquent Career/Onset								
N arrests as juvenile	.0099**	.0099**	.0091*	.0106**	.0103**	.0103**	.0116**	.0115**
N charges as juvenile	.0403**	.0385*	.0422**	.0410**	.0453**	.0461**	.0427**	.0423**
Age at first arrest	-.0007	-.0014	-.0018	-.0020	-.0017	-.0017	-.0016	-.0018
Yrs since first incarc.	.0018***	.0017***	.0017***	.0017***	.0016***	.0016***	.0015***	.0012***
Yrs since first drug use	-.0004	-.0006	-.0007	-.0007	-.0006	-.0005	-.0004	-.0005
Prior CJS-Offender Action								
N prior incarcerations	-.0117*	-.0139*	-.0164**	-.0204***	-.0219***	-.0262***	-.0257***	-.0251***
N prior parole revokes	.0007	-.0103	-.0158	-.0123	-.0122	-.0082	-.0100	-.0074
Bad conduct last probat.	.0117	.0135	.0103	.0023	-.0053	-.0011	-.0026	-.0017
Recent parole revoked	.0055	.0088	.0270	.0210	.0284	.0254	.0187	.0284
General Control Variables								
Offender age at sent.	-.0116***	-.0107***	-.0098***	-.0098***	-.0098***	-.0097***	-.0100***	-.0098***
Off. born out of state	-.0995***	-.1076***	-.1108***	-.1119***	-.1160***	-.1198***	-.1202***	-.1258***
Coder prob. prognosis	-.0010***	-.0009***	-.0009***	-.0009***	-.0008***	-.0008***	-.0009***	-.0008***

* p<.05 ** p<.01 *** p<.001

Table 5.7 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Controlling for Sample Selection)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Selection Hazards								
UCR to SAC arrest histry	-.0207	-.0217	-.0094	-.0096	-.0045	-.0062	-.0034	-.0014
Case proceeds past arrst	.1138	.1420	.1159	.1075	.1071	.1129	.1308	.1185
Case to Grand Jury	-.0117	-.0137	-.0152	-.0161	-.0083	-.0026	-.0060	-.0019
Case to Superior Court	.0593	.0491	.0342	.0315	.0478	.0474	.0630	.0626
Superior Court Convict.	-.0091	-.0074	.0073	.0019	.0046	.0011	.0060	.0064
Match over data sources	.0499	.0068	.0090	.0322	.0454	.0339	.0111	.0018
Constant	-.1362***	-.1162***	-.1158***	-.0991**	-.1058**	-.0965**	-.0917**	-.0640
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.222	.224	.223	.225	.224	.226	.226	.223
Model Classifications:								
% Correct Predictions	72.56	72.41	72.30	72.44	72.59	72.81	72.77	72.97
% False Positives	11.61	12.15	12.52	12.72	12.93	12.98	13.21	13.62
% False Negatives	15.83	15.43	15.18	14.84	14.48	14.21	14.02	13.41
RIOC	.486	.477	.471	.469	.466	.467	.461	.456

* p<.05 ** p<.01 *** p<.001

Table 5.8

Interaction Terms Tested Using
the Selection Procedure

Specific Pairs of Variables Hypothesized to Interact on Recidivism

Offender age at arrest	X	Offender is unemployed***
Offender age at arrest	X	Not a school dropout
Offender age at arrest	X	Does not live with family
Offender age at arrest	X	Treated for drugs/alcohol***
Offender age at arrest	X	History of drug problems***
Offender age at arrest	X	N charges in past 5 years***
Offender age at arrest	X	N prior incarcerations
Not a school dropout	X	Does not live with family
Not a school dropout	X	Offender is unemployed
Not a school dropout	X	Treated for drugs/alcohol
Not a school dropout	X	History of drug problems
Not a school dropout	X	N charges in past 5 years
Not a school dropout	X	N prior incarcerations
Does not live with family	X	Offender is unemployed
Does not live with family	X	Treated for drugs/alcohol
Does not live with family	X	History of drug problems
Does not live with family	X	N charges in past 5 years
Does not live with family	X	N prior incarcerations
Offender is unemployed	X	Treated for drugs/alcohol
Offender is unemployed	X	History of drug problems
Committed PO with group	X	Victim was a stranger
History of drug problems	X	N charges in past 5 years
History of drug problems	X	N prior incarcerations
Treated for drugs/alcohol	X	N charges in past 5 years
Treated for drugs/alcohol	X	N prior incarcerations
N arrests as juvenile	X	N charges in past 5 years
N arrests as juvenile	X	N prior incarcerations
Age at first arrest	X	N charges in past 5 years
Age at first arrest	X	N prior incarcerations
Off street past two years	X	N charges in past 5 years
Off street past two years	X	N prior incarcerations
Has pending charges	X	N charges in past 5 years
Has pending charges	X	N prior incarcerations

*** = Met inclusion criteria

Table 5.9
Attribution of Explained Variance for Rearrest by Selected Windows

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.173)	(.244)	(.261)	(.256)
Percent R2 Unattributed	55.11	55.24	54.81	52.45
	(.095)	(.135)	(.143)	(.134)
Social Variables	5.91	7.83	9.25	10.82
	(.010)	(.019)	(.024)	(.028)
Presenting Offense	3.32	2.83	2.41	2.20
	(.006)	(.007)	(.006)	(.006)
Anamnestic Variables	8.90	9.09	8.33	9.20
	(.015)	(.022)	(.022)	(.024)
Delinquent Career/Onset	3.64	3.20	2.97	2.36
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.63	.29	.40	.80
	(.001)	(.001)	(.001)	(.002)
General Controls	4.10	5.18	5.52	6.27
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.22	.42	.38
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	1.23	1.50	1.62	1.85
	(.002)	(.004)	(.004)	(.005)
Social/Delinquent Career	1.80	2.08	2.44	2.44
	(.003)	(.005)	(.006)	(.006)
Social/Prior CJS Actions	.23	.02	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.30	2.05	1.83	1.91
	(.004)	(.005)	(.005)	(.005)
Presenting Offense/Delinquency	.00*	.11	.18	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.23	.40	.39	.46
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.30	.43	.42	.45
	(.001)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.74	1.79	1.53	1.41
	(.005)	(.004)	(.004)	(.004)
Anamnestic/Prior CJS Actions	4.26	1.87	1.14	.03
	(.007)	(.005)	(.003)	(.000)
Anamnestic/General Controls	3.21	2.91	2.30	2.10
	(.006)	(.007)	(.006)	(.005)
Delinquency/Prior CJS Actions	.74	.13	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	1.95	3.46	5.24	6.65
	(.003)	(.008)	(.014)	(.017)
Prior CJS Actions/Controls	.12	.20	.19	.25
	(.000)	(.000)	(.001)	(.001)
All Interactions	4.09	2.62	2.38	2.40
	(.007)	(.006)	(.006)	(.006)

Table 5.10 -- Attribution of Explained Variance for Rearrest by Selected Windows

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.174)	(.245)	(.261)	(.257)
Percent R2 Unattributed	33.99	33.79	33.40	32.59
	(.059)	(.083)	(.087)	(.084)
Attributed to Hazards	25.02	26.20	26.79	24.77
	(.044)	(.064)	(.070)	(.064)
Social Variables	5.04	6.79	7.93	9.60
	(.009)	(.017)	(.021)	(.025)
Presenting Offense	3.08	2.52	2.09	1.96
	(.005)	(.006)	(.005)	(.005)
Anamnestic Variables	8.23	8.51	7.79	8.71
	(.014)	(.021)	(.020)	(.022)
Delinquent Career/Onset	3.54	3.12	2.88	2.35
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.64	.24	.33	.69
	(.001)	(.001)	(.001)	(.002)
General Controls	4.19	5.16	5.51	6.29
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.00*	.04	.03
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.87	.90	.94	1.07
	(.002)	(.002)	(.002)	(.003)
Social/Delinquent Career	1.39	1.53	1.80	1.88
	(.002)	(.004)	(.005)	(.005)
Social/Prior CJS Actions	.17	.02	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.71	1.40	1.18	1.29
	(.003)	(.003)	(.003)	(.003)
Presenting Offense/Delinquency	.00*	.00*	.01	.01
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.16	.33	.30	.37
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.10	.34	.38	.41
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.49	1.51	1.26	1.18
	(.004)	(.004)	(.003)	(.003)
Anamnestic/Prior CJS Actions	3.97	1.74	1.04	.07
	(.007)	(.004)	(.003)	(.000)
Anamnestic/General Controls	3.44	3.09	2.48	2.30
	(.006)	(.008)	(.006)	(.006)
Delinquency/Prior CJS Actions	.74	.15	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	1.90	3.07	4.52	5.78
	(.003)	(.008)	(.012)	(.015)
Prior CJS Actions/Controls	.11	.18	.18	.24
	(.000)	(.000)	(.000)	(.001)
All Interactions	4.04	2.63	2.40	2.41
	(.007)	(.006)	(.006)	(.006)

Table 5.11

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean (Interactions Included) (N = 11,749)

Independent Variable	Proportion Rearrested by Year 1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0172	.0165	.0308**	.0301*	.0253*	.0250*	.0266*	.0285*
Has job after sentence	-.0197*	-.0360***	-.0313**	-.0285*	-.0297*	-.0240*	-.0212	-.0268*
Offender is on welfare	-.0078	-.0213*	-.0174	-.0041	.0040	.0102	.0007	.0038
Offender is Black	.0497***	.0782***	.0972***	.1056***	.1291***	.1414***	.1420***	.1557***
Offender is Hispanic	.0552**	.0698***	.0824***	.0977***	.1225***	.1340***	.1415***	.1492***
Offender is female	-.0287*	-.0487**	-.0484*	-.0562**	-.0479*	-.0627**	-.0663**	-.0538*
Lives in urban area	.0130	.0214*	.0306**	.0408***	.0349**	.0389**	.0413***	.0376**
Years at current address	-.0013**	-.0013**	-.0016**	-.0018**	-.0016**	-.0014*	-.0012*	-.0012
History of drug problems	-.0084	.0017	.0021	.0067	.0079	.0229	.0188	.0192
Treated for drugs/alch.	-.0012	-.0136	-.0102	-.0019	.0021	-.0016	.0050	.0093
Has needle marks	.0026	.0024	.0019	.0193	.0279	.0276	.0289	.0419
Not a school drop out	-.0258***	-.0311***	-.0382***	-.0429***	-.0466***	-.0472***	-.0443***	-.0440***
Doesn't live with family	.0041	-.0054	-.0097	-.0038	-.0101	-.0151	-.0223	-.0363**
Commited PO with group	-.0212**	-.0235**	-.0307**	-.0304**	-.0310**	-.0398***	-.0403***	-.0409***
Victim was a stranger	.0222*	.0246*	.0282*	.0362**	.0404**	.0401**	.0345*	.0379**
Presenting Offense								
PO property crime	.0405**	.0228	.0294*	.0363*	.0431**	.0386*	.0382*	.0331*
PO crime against person	-.0098	-.0260	.0058	.0006	.0112	.0210	.0243	.0196
PO drug offense	.0135	-.0080	.0095	.0079	.0083	.0058	-.0003	-.0050
PO Wolfgang severity	-.0015**	-.0017*	-.0019*	-.0019*	-.0019*	-.0027***	-.0025**	-.0025**
Has detainers at arrest	-.0236	-.0203	-.0161	-.0180	-.0127	-.0135	-.0251	-.0235
Has pending charges	.0403***	.0566***	.0658***	.0792***	.0809***	.0868***	.0936***	.0942***
On probation at PO	.0333**	.0560***	.0534**	.0532**	.0619***	.0711***	.0681***	.0724***
Anamnestic Theory								
N prior adult arrests	.0094**	.0140***	.0180***	.0243***	.0273***	.0296***	.0312***	.0329***
N prior adult conviction	-.0010	.0024	.0042	.0016	.0024	.0007	-.0005	-.0007
N prior adult chg. conv.	-.0050*	-.0081**	-.0088**	-.0102**	-.0113**	-.0133***	-.0145***	-.0170***
N charges past 5 years	.0033**	.0063***	.0060***	.0068***	.0081***	.0083***	.0095***	.0094***
N prior Part 1 charges	.0050	.0094*	.0075	.0053	.0068	.0071	.0059	.0097
N prior property conv.	.0029	-.0002	.0010	-.0009	-.0007	.0009	.0000	.0003
N prior persons conv.	.0034	.0026	.0038	.0043	.0015	.0049	.0099	.0071
N prior weapons conv.	.0105	.0320**	.0351*	.0423**	.0430**	.0278	.0228	.0421*
Off street last 2 years	.0225*	.0485***	.0664***	.0834***	.0930***	.0972***	.0966***	.0962***
Delinquent Career/Onset								
N arrests as juvenile	.0085***	.0134***	.0158***	.0148***	.0164***	.0146***	.0139**	.0143**
N charges as juvenile	.0184	.0053	.0037	.0213	.0244	.0167	.0203	.0202
Age at first arrest	-.0002	-.0013	-.0008	.0000	-.0001	.0008	.0007	.0002
Yrs since first incarc.	.0010***	.0011***	.0012***	.0015***	.0015***	.0016***	.0016***	.0016***
Yrs since first drug use	-.0002	-.0005	-.0006	-.0009*	-.0009*	-.0009*	-.0006	-.0004
Prior CJS-Offender Action								
N prior incarcerations	.0044	-.0004	-.0040	-.0031	-.0076	-.0078	-.0053	-.0045
N prior parole revokes	.0027	-.0014	-.0148	-.0080	.0010	.0039	.0070	.0009
Bad conduct last probat.	.0108	.0232*	.0399**	.0294*	.0311*	.0251	.0304*	.0249
Recent parole revoked	.0083	.0269	.0274	.0080	-.0091	-.0150	-.0311	-.0153
General Control Variables								
Offender age at sent.	-.0060***	-.0042**	-.0050***	-.0054***	-.0054***	-.0065***	-.0071***	-.0073***
Off. born out of state	-.0343***	-.0458***	-.0581***	-.0714***	-.0812***	-.0881***	-.0919***	-.0985***
Coder prob. prognosis	-.0003	-.0005**	-.0006**	-.0007***	-.0008***	-.0010***	-.0009***	-.0009***

* p<.05 ** p<.01 *** p<.001

Table 5.11 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (Interactions Included) (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Interactions								
Black x on prob. at PO	-.0166	-.0410*	-.0440*	-.0467*	-.0617*	-.0693**	-.0624*	-.0773**
Black x prior adult arrs	-.0050*	-.0081**	-.0106**	-.0116**	-.0110**	-.0102*	-.0122**	-.0137**
Black x n prior prop cnv	.0031	.0064*	.0089**	.0110**	.0091*	.0097*	.0113**	.0134**
Black x n charges as juv	-.0104	.0125	.0219	.0203	.0183	.0199	.0170	.0272
Female x Part 1 charges	.0062	.0177	.0341**	.0332*	.0390*	.0386*	.0402*	.0639**
Off. age x drug problem	-.0044*	-.0052*	-.0057*	-.0041	-.0043	-.0039	-.0041	-.0036
Off. age x prior trtment	-.0010	-.0039	-.0056*	-.0082**	-.0079**	-.0086**	-.0075**	-.0078**
Off. age x unemployed	-.0007	-.0018	-.0020	-.0027	-.0028	-.0019	-.0020	-.0023
Off. age x PO property	-.0009	-.0055**	-.0053**	-.0046*	-.0044*	-.0059**	-.0047*	-.0050*
Off. age x chg pst 5 yrs	.0002	.0003*	.0003	.0001	.0001	.0000	.0000	.0000
PO viol x has detainers	-.0145	-.0512	-.0962**	-.1267***	-.1263**	-.1326**	-.1247**	-.1194*
PO prop x n adl.arrests	.0006	.0032	.0022	-.0002	.0006	.0019	.0021	.0022
PO prop x prior prop con	-.0059**	-.0051	-.0052	-.0030	-.0045	-.0042	-.0042	-.0047
PO prop x n juv. arrests	-.0070*	-.0105**	-.0117**	-.0128**	-.0148**	-.0106*	-.0113*	-.0120*
PO prop x age at 1st arr	.0014	.0033	.0004	-.0015	-.0026	-.0020	-.0032	-.0025
PO prop x yrs. 1st incar	-.0003	.0000	.0003	.0000	.0002	.0000	.0001	.0001
PO drugs x n adl. convs.	.0076*	.0131**	.0149**	.0165**	.0146**	.0150**	.0170**	.0179**
PO drugs x Part 1 chgs.	-.0196***	-.0243***	-.0201*	-.0184*	-.0200*	-.0205*	-.0217*	-.0254*
PO drugs x last par. rev	.0896*	.0603	.0569	.0382	.1344	.1272	.1757*	.1293
Constant	-.1301***	-.1721***	-.1929***	-.1961***	-.1924***	-.1807***	-.1615***	-.1391***
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.104	.148	.174	.191	.203	.211	.215	.223
Model Classifications:								
% Correct Predictions	85.30	78.89	75.91	74.29	73.61	73.08	72.68	72.77
% False Positives	.91	4.17	6.67	8.16	9.20	9.99	10.66	11.02
% False Negatives	13.79	16.94	17.41	17.55	17.19	16.92	16.67	16.21
RIOC	.435	.487	.490	.494	.497	.496	.491	.494

* p<.05 ** p<.01 *** p<.001

Table 5.11 (continued)

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0278*	.0304*	.0350**	.0285*	.0297*	.0325*	.0322*	.0269*
Has job after sentence	-.0266*	-.0221	-.0183	-.0193	-.0219	-.0213	-.0169	-.0180
Offender is on welfare	.0062	.0108	.0055	.0117	.0048	.0077	.0147	.0158
Offender is Black	.1568***	.1557***	.1662***	.1608***	.1652***	.1632***	.1672***	.1689***
Offender is Hispanic	.1497***	.1482***	.1508***	.1524***	.1574***	.1563***	.1588***	.1539***
Offender is female	-.0583*	-.0686**	-.0672**	-.0682**	-.0723**	-.0724**	-.0771**	-.0859***
Lives in urban area	.0413***	.0445***	.0459***	.0440***	.0440***	.0449***	.0417***	.0387**
Years at current address	-.0011	-.0014*	-.0013*	-.0012*	-.0014*	-.0015*	-.0015**	-.0013*
History of drug problems	.0196	.0201	.0262	.0278	.0247	.0276	.0246	.0244
Treated for drugs/alch.	.0084	.0088	.0169	.0321	.0366*	.0345	.0304	.0260
Has needle marks	.0552*	.0733**	.0743**	.0729**	.0737**	.0686**	.0679**	.0801**
Not a school drop out	-.0445***	-.0504***	-.0497***	-.0515***	-.0507***	-.0509***	-.0513***	-.0530***
Doesn't live with family	-.0459***	-.0537***	-.0491***	-.0506***	-.0494***	-.0530***	-.0520***	-.0424**
Commited PO with group	-.0469***	-.0411***	-.0377***	-.0373***	-.0344**	-.0319**	-.0371**	-.0430***
Victim was a stranger	.0288*	.0274	.0272	.0286*	.0303*	.0318*	.0322*	.0317*
Presenting Offense								
PO property crime	.0366*	.0463**	.0520**	.0530***	.0512**	.0509**	.0454**	.0466**
PO crime against person	.0227	.0355	.0422*	.0475*	.0434*	.0456*	.0407*	.0483*
PO drug offense	-.0061	.0106	.0121	.0151	.0111	.0080	.0106	.0169
PO Wolfgang severity	-.0022**	-.0021*	-.0020*	-.0022**	-.0023**	-.0024**	-.0023**	-.0026**
Has detainers at arrest	-.0187	-.0163	-.0127	-.0217	-.0332	-.0249	-.0166	-.0219
Has pending charges	.0947***	.0912***	.0917***	.0876***	.0882***	.0844***	.0841***	.0769***
On probation at PO	.0709***	.0661***	.0706***	.0676***	.0715***	.0711***	.0757***	.0699***
Anamnestic Theory								
N prior adult arrests	.0358***	.0363***	.0343***	.0321***	.0311***	.0325***	.0354***	.0369***
N prior adult conviction	.0018	.0043	.0030	.0049	.0067	.0097*	.0083	.0055
N prior adult chg. conv.	-.0173***	-.0161***	-.0146***	-.0136**	-.0130**	-.0148***	-.0148***	-.0158***
N charges past 5 years	.0093***	.0088***	.0094***	.0097***	.0098***	.0098***	.0098***	.0116***
N prior Part 1 charges	.0093	.0072	.0061	.0054	.0034	.0059	.0052	.0071
N prior property conv.	-.0006	-.0021	-.0011	-.0010	-.0007	-.0011	-.0023	-.0018
N prior persons conv.	.0092	.0108	.0120*	.0129*	.0129*	.0104	.0083	.0061
N prior weapons conv.	.0358*	.0340	.0347	.0460*	.0471*	.0489**	.0460*	.0534**
Off street last 2 years	.0973***	.1038***	.1051***	.1043***	.1068***	.1041***	.1064***	.1005***
Delinquent Career/Onset								
N arrests as juvenile	.0162***	.0152**	.0140**	.0174***	.0166***	.0170***	.0179***	.0191***
N charges as juvenile	.0194	.0191	.0222	.0193	.0259	.0324	.0298	.0297
Age at first arrest	.0003	-.0003	-.0008	-.0015	-.0012	-.0013	-.0013	-.0012
Yrs since first incarc.	.0018***	.0015***	.0015***	.0015***	.0015***	.0014***	.0013***	.0009*
Yrs since first drug use	-.0005	-.0007	-.0008	-.0008	-.0007	-.0006	-.0005	-.0006
Prior CJS-Offender Action								
N prior incarcerations	-.0092	-.0117*	-.0140*	-.0179**	-.0195***	-.0240***	-.0241***	-.0235***
N prior parole revokes	.0033	-.0071	-.0123	-.0089	-.0095	-.0049	-.0063	-.0038
Bad conduct last probat.	.0121	.0136	.0102	.0020	-.0056	-.0013	-.0030	-.0030
Recent parole revoked	-.0168	-.0084	.0069	.0036	.0092	.0001	-.0063	.0077
General Control Variables								
Offender age at sent.	-.0078***	-.0075***	-.0066***	-.0061***	-.0061***	-.0060***	-.0062***	-.0059***
Off. born out of state	-.0987***	-.1063***	-.1098***	-.1105***	-.1152***	-.1193***	-.1198***	-.1252***
Coder prob. prognosis	-.0009***	-.0008***	-.0008***	-.0008***	-.0007**	-.0008***	-.0008***	-.0007**

* p<.05 ** p<.01 *** p<.001

Table 5.11 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Interactions								
Black x on prob. at PO	-.0825**	-.0841**	-.1045***	-.0970***	-.0971***	-.0946**	-.1021***	-.0974***
Black x prior adult arrs	-.0154***	-.0176***	-.0177***	-.0174***	-.0155***	-.0150**	-.0147**	-.0151**
Black x n prior prop cnv	.0155***	.0202***	.0224***	.0230***	.0213***	.0210***	.0223***	.0227***
Black x n charges as juv	.0345	.0328	.0352	.0389	.0331	.0203	.0181	.0165
Female x Part 1 charges	.0614**	.0584**	.0575**	.0526**	.0502*	.0481*	.0449*	.0439*
Off. age x drug problem	-.0037	-.0029	-.0027	-.0023	-.0027	-.0021	-.0022	-.0022
Off. age x prior trtment	-.0085**	-.0099***	-.0106***	-.0104***	-.0097***	-.0095**	-.0099***	-.0100***
Off. age x unemployed	-.0021	-.0015	-.0009	-.0016	-.0009	-.0009	-.0010	-.0015
Off. age x PO property	-.0046*	-.0038	-.0042*	-.0049*	-.0051*	-.0056**	-.0054**	-.0056**
Off. age x chg pst 5 yrs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0001
PO viol x has detainers	-.1326**	-.1458**	-.1628***	-.1513**	-.1462**	-.1517**	-.1651**	-.1754***
PO prop x n adl.arrests	-.0010	-.0027	.0000	.0038	.0066	.0061	.0063	.0061
PO prop x prior prop con	-.0036	-.0014	-.0035	-.0045	-.0062	-.0060	-.0062	-.0077
PO prop x n juv. arrests	-.0129*	-.0120*	-.0115*	-.0144*	-.0134*	-.0138*	-.0130*	-.0158*
PO prop x age at 1st arr	-.0028	-.0034	-.0031	-.0017	-.0018	-.0012	-.0011	-.0019
PO prop x yrs. 1st incar	-.0001	.0002	.0002	.0002	.0001	.0003	.0003	.0005
PO drugs x n adl. convs.	.0166**	.0153*	.0178**	.0181**	.0147*	.0138*	.0133*	.0153*
PO drugs x Part 1 chgs.	-.0243*	-.0163	-.0227*	-.0240*	-.0197	-.0204	-.0175	-.0236*
PO drugs x last par. rev	.1031	.0592	.0757	.0585	.0700	.1128	.1041	.0748
Constant	-.1148***	-.1049***	-.1049***	-.0876***	-.0734***	-.0619**	-.0495*	-.0197
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.229	.228	.228	.230	.229	.230	.230	.228
Model Classifications:								
% Correct Predictions	72.77	72.48	72.41	72.68	72.70	72.93	73.14	72.98
% False Positives	11.51	12.02	12.24	12.38	12.67	12.81	12.92	13.52
% False Negatives	15.72	15.50	15.35	14.95	14.63	14.27	13.94	13.50
RIOC	.491	.481	.477	.478	.473	.472	.471	.455

* p<.05 ** p<.01 *** p<.001

Table 5.12

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included; Controlling for Sample Selection)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0178	.0175	.0316**	.0309*	.0260*	.0258*	.0274*	.0291*
Has job after sentence	-.0200*	-.0362***	-.0314**	-.0282*	-.0294*	-.0241*	-.0212	-.0267*
Offender is on welfare	-.0080	-.0213*	-.0173	-.0039	.0042	.0103	.0009	.0041
Offender is Black	.0444***	.0701***	.0862***	.0957***	.1215***	.1385***	.1402***	.1543***
Offender is Hispanic	.0544**	.0681***	.0799***	.0979***	.1216***	.1310***	.1380***	.1442***
Offender is female	-.0332*	-.0539**	-.0537**	-.0581**	-.0520*	-.0678**	-.0708**	-.0598*
Lives in urban area	.0082	.0137	.0220	.0350**	.0285*	.0322**	.0348**	.0303*
Years at current address	-.0013**	-.0013**	-.0017**	-.0018**	-.0016**	-.0014*	-.0012*	-.0012*
History of drug problems	-.0069	.0031	.0037	.0076	.0090	.0241	.0194	.0198
Treated for drugs/alch.	-.0006	-.0131	-.0097	-.0018	.0024	-.0013	.0052	.0096
Has needle marks	-.0003	-.0025	-.0043	.0155	.0242	.0236	.0248	.0371
Not a school drop out	-.0268***	-.0326***	-.0401***	-.0442***	-.0478***	-.0484***	-.0453***	-.0451***
Doesn't live with family	.0050	-.0041	-.0082	-.0024	-.0089	-.0145	-.0220	-.0362**
Commited PO with group	-.0179*	-.0186*	-.0255**	-.0264*	-.0262*	-.0350**	-.0360**	-.0365**
Victim was a stranger	.0248**	.0267*	.0310*	.0380**	.0425**	.0422**	.0358**	.0394**
Presenting Offense								
PO property crime	.0348**	.0082	.0143	.0223	.0320	.0271	.0257	.0220
PO crime against person	-.0112	-.0321*	-.0009	-.0060	.0039	.0132	.0153	.0104
PO drug offense	.0245	.0015	.0239	.0230	.0223	.0163	.0063	.0009
PO Wolfgang severity	-.0016**	-.0018**	-.0019**	-.0019*	-.0020*	-.0028***	-.0026**	-.0026**
Has detainers at arrest	-.0239	-.0214	-.0168	-.0186	-.0132	-.0144	-.0264	-.0246
Has pending charges	.0407***	.0573***	.0667***	.0793***	.0808***	.0867***	.0936***	.0940***
On probation at PO	.0329**	.0548***	.0522**	.0517**	.0607***	.0701***	.0671***	.0716***
Anamnestic Theory								
N prior adult arrests	.0091**	.0133***	.0171***	.0235***	.0267***	.0288***	.0304***	.0321***
N prior adult conviction	-.0013	.0019	.0038	.0011	.0021	.0005	-.0007	-.0006
N prior adult chg. conv.	-.0048*	-.0079**	-.0087**	-.0103**	-.0112**	-.0131***	-.0143***	-.0167***
N charges past 5 years	.0034**	.0064***	.0061***	.0068***	.0082***	.0084***	.0095***	.0095***
N prior Part 1 charges	.0052	.0095*	.0076	.0055	.0069	.0072	.0059	.0097
N prior property conv.	.0026	-.0006	.0007	-.0011	-.0011	.0006	-.0003	-.0001
N prior persons conv.	.0032	.0022	.0035	.0041	.0011	.0046	.0095	.0066
N prior weapons conv.	.0092	.0298*	.0326*	.0399**	.0404**	.0263	.0215	.0407*
Off street last 2 years	.0220*	.0478***	.0651***	.0823***	.0923***	.0965***	.0961***	.0958***
Delinquent Career/Onset								
N arrests as juvenile	.0081***	.0128***	.0149***	.0143***	.0159***	.0140***	.0133**	.0136**
N charges as juvenile	.0201	.0074	.0057	.0228	.0265	.0190	.0224	.0225
Age at first arrest	.0000	-.0011	-.0006	.0001	.0000	.0009	.0008	.0004
Yrs since first incarc.	.0010***	.0011***	.0012***	.0015***	.0015***	.0016***	.0016***	.0016***
Yrs since first drug use	-.0003	-.0006	-.0006	-.0008*	-.0009*	-.0009*	-.0006	-.0005
Prior CJS-Offender Action								
N prior incarcerations	.0053	.0012	-.0027	-.0017	-.0060	-.0065	-.0040	-.0034
N prior parole revokes	.0038	-.0001	-.0137	-.0074	.0020	.0053	.0081	.0021
Bad conduct last probat.	.0103	.0220	.0384**	.0285*	.0299*	.0238	.0290	.0233
Recent parole revoked	.0091	.0286	.0286	.0089	-.0080	-.0148	-.0301	-.0145
General Control Variables								
Offender age at sent.	-.0065***	-.0047***	-.0057***	-.0059***	-.0060***	-.0070***	-.0076***	-.0079***
Off. born out of state	-.0338***	-.0450***	-.0570***	-.0705***	-.0801***	-.0872***	-.0911***	-.0977***
Coder prob. prognosis	-.0003	-.0006**	-.0007***	-.0008***	-.0008***	-.0011***	-.0009***	-.0009***

* p<.05 ** p<.01 *** p<.001

Table 5.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included; Controlling for Sample Selection)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Interactions								
Black x on prob. at PO	-.0165	-.0410*	-.0440*	-.0464*	-.0614*	-.0692**	-.0624*	-.0774**
Black x prior adult arrs	-.0051*	-.0081**	-.0108**	-.0117**	-.0110**	-.0102*	-.0121**	-.0136**
Black x n prior prop cnv	.0032	.0064*	.0090**	.0110**	.0092*	.0097*	.0112**	.0133**
Black x n charges as juv	-.0106	.0127	.0220	.0202	.0181	.0196	.0166	.0268
Female x Part 1 charges	.0065	.0180	.0350**	.0336*	.0395*	.0391*	.0405*	.0644**
Off. age x drug problem	-.0043*	-.0052*	-.0056*	-.0041	-.0042	-.0038	-.0041	-.0035
Off. age x prior trtment	-.0009	-.0038	-.0055*	-.0081**	-.0078**	-.0086**	-.0075**	-.0078**
Off. age x unemployed	-.0007	-.0018	-.0020	-.0028	-.0028	-.0019	-.0020	-.0023
Off. age x PO property	-.0011	-.0060**	-.0059**	-.0050*	-.0048*	-.0063**	-.0051*	-.0054*
Off. age x chg pst 5 yrs	.0002	.0004**	.0003	.0002	.0001	.0001	.0000	.0000
PO viol x has detainers	-.0133	-.0490	-.0940**	-.1249***	-.1247**	-.1313**	-.1238**	-.1189*
PO prop x n adl.arrests	.0006	.0032	.0024	-.0001	.0006	.0021	.0024	.0025
PO prop x prior prop con	-.0056*	-.0045	-.0047	-.0026	-.0041	-.0037	-.0039	-.0044
PO prop x n juv. arrests	-.0070*	-.0105**	-.0116**	-.0127**	-.0147**	-.0105*	-.0113*	-.0119*
PO prop x age at 1st arr	.0010	.0026	-.0002	-.0018	-.0030	-.0024	-.0036	-.0030
PO prop x yrs. 1st incar	-.0002	.0001	.0004	.0000	.0003	.0001	.0001	.0001
PO drugs x n adl. convs.	.0076*	.0131**	.0148**	.0165**	.0146*	.0149*	.0169**	.0176**
PO drugs x Part 1 chgs.	-.0192***	-.0236**	-.0195*	-.0181*	-.0196*	-.0201*	-.0213*	-.0248*
PO drugs x last par. rev	.0937*	.0650	.0612	.0415	.1370	.1297	.1778*	.1314
Selection Hazards								
UCR to SAC arrest histry	-.0145	-.0409**	-.0371*	-.0309	-.0282	-.0283	-.0341*	-.0330
Case proceeds past arrst	.2133**	.2781***	.3055***	.2540**	.2191**	.1648*	.1291	.1129
Case to Grand Jury	-.0010	-.0014	.0012	-.0023	-.0084	-.0009	-.0028	-.0052
Case to Superior Court	.0475	.0733*	.0476	.0424	.0605	.0690	.0677	.0667
Superior Court Convict.	-.0025	-.0153	-.0273	-.0357	-.0172	-.0220	-.0218	-.0153
Match over data sources	-.0164	-.0136	-.0101	-.0337	-.0216	.0175	.0328	.0529
Constant	-.1346***	-.1839***	-.2083***	-.2073***	-.2125***	-.2081***	-.1855***	-.1675***
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.105	.150	.176	.192	.204	.211	.217	.224
Model Classifications:								
% Correct Predictions	85.23	78.95	76.21	74.31	73.62	73.04	72.87	72.93
% False Positives	1.02	4.14	6.62	8.20	9.22	10.14	10.72	11.01
% False Negatives	13.75	16.91	17.17	17.49	17.16	16.83	16.41	16.06
RIOC	.413	.491	.499	.494	.497	.492	.492	.497

* p<.05 ** p<.01 *** p<.001

Table 5.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included; Controlling for Sample Selection)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0283*	.0309*	.0351**	.0287*	.0299*	.0329*	.0325*	.0273*
Has job after sentence	-.0266*	-.0218	-.0181	-.0191	-.0222	-.0218	-.0173	-.0186
Offender is on welfare	.0067	.0114	.0061	.0124	.0052	.0079	.0148	.0158
Offender is Black	.1545***	.1507***	.1617***	.1568***	.1640***	.1622***	.1660***	.1687***
Offender is Hispanic	.1435***	.1447***	.1468***	.1470***	.1510***	.1508***	.1545***	.1504***
Offender is female	-.0655**	-.0738**	-.0732**	-.0749**	-.0817**	-.0810**	-.0860***	-.0942***
Lives in urban area	.0332**	.0376**	.0396**	.0371**	.0361**	.0375**	.0344**	.0323*
Years at current address	-.0011	-.0014*	-.0013*	-.0012*	-.0014*	-.0015*	-.0016**	-.0014*
History of drug problems	.0203	.0207	.0270	.0285	.0262	.0292	.0265	.0263
Treated for drugs/alch.	.0090	.0092	.0175	.0327	.0374*	.0354	.0312	.0269
Has needle marks	.0493*	.0686**	.0697**	.0675**	.0679**	.0632*	.0635*	.0766**
Not a school drop out	-.0457***	-.0514***	-.0505***	-.0526***	-.0520***	-.0521***	-.0525***	-.0540***
Doesn't live with family	-.0458***	-.0532***	-.0486***	-.0505***	-.0493***	-.0527***	-.0514***	-.0418**
Commited PO with group	-.0426***	-.0370**	-.0343**	-.0341**	-.0298**	-.0270*	-.0317**	-.0376**
Victim was a stranger	.0305*	.0286*	.0287*	.0303*	.0332*	.0346*	.0353*	.0347*
Presenting Offense								
PO property crime	.0278	.0370*	.0480**	.0492**	.0479**	.0462**	.0414*	.0425*
PO crime against person	.0145	.0279	.0370	.0420*	.0379	.0402*	.0347	.0428*
PO drug offense	-.0009	.0162	.0174	.0219	.0185	.0141	.0198	.0243
PO Wolfgang severity	-.0023**	-.0022**	-.0021*	-.0022**	-.0023**	-.0023**	-.0023**	-.0026**
Has detainers at arrest	-.0194	-.0168	-.0126	-.0216	-.0332	-.0250	-.0167	-.0222
Has pending charges	.0948***	.0914***	.0919***	.0876***	.0884***	.0848***	.0842***	.0771***
On probation at PO	.0706***	.0656***	.0706***	.0676***	.0716***	.0712***	.0756***	.0698***
Anamnestic Theory								
N prior adult arrests	.0349***	.0356***	.0337***	.0314***	.0304***	.0317***	.0347***	.0364***
N prior adult conviction	.0021	.0044	.0034	.0054	.0073	.0101*	.0086	.0057
N prior adult chg. conv.	-.0169***	-.0158***	-.0142***	-.0133**	-.0125**	-.0144***	-.0144**	-.0154***
N charges past 5 years	.0094***	.0089***	.0095***	.0098***	.0099***	.0099***	.0100***	.0118***
N prior Part 1 charges	.0094	.0073	.0061	.0055	.0036	.0061	.0054	.0073
N prior property conv.	-.0010	-.0025	-.0015	-.0013	-.0011	-.0014	-.0027	-.0022
N prior persons conv.	.0086	.0102	.0114	.0123*	.0124*	.0099	.0076	.0055
N prior weapons conv.	.0343*	.0319	.0328	.0443*	.0460*	.0481**	.0447*	.0525**
Off street last 2 years	.0967***	.1032***	.1047***	.1036***	.1063***	.1036***	.1061***	.1004***
Delinquent Career/Onset								
N arrests as juvenile	.0154***	.0146**	.0135**	.0168***	.0158**	.0162**	.0172***	.0185***
N charges as juvenile	.0217	.0209	.0238	.0211	.0282	.0344	.0320	.0318
Age at first arrest	.0004	-.0002	-.0008	-.0015	-.0012	-.0012	-.0013	-.0012
Yrs since first incarc.	.0018***	.0015***	.0015***	.0015***	.0015***	.0014***	.0013***	.0009*
Yrs since first drug use	-.0005	-.0007	-.0009*	-.0008	-.0007	-.0006	-.0006	-.0006
Prior CJS-Offender Action								
N prior incarcerations	-.0084	-.0108	-.0135*	-.0175**	-.0191***	-.0236***	-.0232***	-.0226***
N prior parole revokes	.0043	-.0065	-.0117	-.0083	-.0082	-.0036	-.0048	-.0022
Bad conduct last probat.	.0103	.0119	.0087	.0003	-.0074	-.0029	-.0045	-.0042
Recent parole revoked	-.0161	-.0078	.0073	.0039	.0097	.0007	-.0054	.0086
General Control Variables								
Offender age at sent.	-.0085***	-.0081***	-.0072***	-.0067***	-.0068***	-.0066***	-.0069***	-.0065***
Off. born out of state	-.0975***	-.1052***	-.1086***	-.1092***	-.1137***	-.1180***	-.1183***	-.1239***
Coder prob. prognosis	-.0009***	-.0009***	-.0009***	-.0009***	-.0008***	-.0008***	-.0009***	-.0008***

* p<.05 ** p<.01 *** p<.001

Table 5.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean
(Interactions Included; Controlling for Sample Selection)
(N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Interactions								
Black x on prob. at PO	-.0829**	-.0843**	-.1049***	-.0975***	-.0977***	-.0949**	-.1024***	-.0975***
Black x prior adult arrs	-.0153***	-.0175***	-.0177***	-.0173***	-.0156***	-.0151**	-.0149**	-.0152**
Black x n prior prop cnv	.0155***	.0202***	.0225***	.0231***	.0214***	.0211***	.0224***	.0228***
Black x n charges as juv	.0342	.0327	.0351	.0386	.0327	.0201	.0178	.0164
Female x Part 1 charges	.0621**	.0588**	.0581**	.0533**	.0512*	.0489*	.0457*	.0446*
Off. age x drug problem	-.0037	-.0029	-.0027	-.0022	-.0025	-.0019	-.0020	-.0021
Off. age x prior trtment	-.0085**	-.0099***	-.0105***	-.0104***	-.0097***	-.0095**	-.0099***	-.0100***
Off. age x unemployed	-.0021	-.0015	-.0009	-.0016	-.0009	-.0009	-.0010	-.0016
Off. age x PO property	-.0051*	-.0042*	-.0045*	-.0052*	-.0055**	-.0060**	-.0058**	-.0059**
Off. age x chg pst 5 yrs	.0000	.0000	.0000	.0000	.0000	.0000	.0001	.0001
PO viol x has detainers	-.1324**	-.1454**	-.1626***	-.1517**	-.1460**	-.1511**	-.1641**	-.1741***
PO prop x n adl.arrests	-.0008	-.0026	.0000	.0039	.0068	.0064	.0065	.0063
PO prop x prior prop con	-.0033	-.0011	-.0033	-.0043	-.0059	-.0056	-.0058	-.0073
PO prop x n juv. arrests	-.0129*	-.0119*	-.0114*	-.0142*	-.0131*	-.0136*	-.0129*	-.0157*
PO prop x age at 1st arr	-.0033	-.0038	-.0034	-.0020	-.0022	-.0016	-.0015	-.0023
PO prop x yrs. 1st incar	-.0001	.0002	.0002	.0002	.0002	.0003	.0004	.0005
PO drugs x n adl. convs.	.0162**	.0151*	.0176**	.0177**	.0142*	.0134*	.0130*	.0150*
PO drugs x Part 1 chgs.	-.0237*	-.0157	-.0223*	-.0235*	-.0191	-.0198	-.0170	-.0231
PO drugs x last par. rev	.1051	.0614	.0773	.0600	.0716	.1146	.1058	.0765
Selection Hazards								
UCR to SAC arrest histry	-.0297	-.0305	-.0183	-.0178	-.0139	-.0154	-.0123	-.0109
Case proceeds past arrst	.1089	.1357	.1075	.0989	.0976	.1045	.1216	.1098
Case to Grand Jury	-.0072	-.0099	-.0117	-.0124	-.0039	.0015	-.0025	.0017
Case to Superior Court	.0527	.0440	.0293	.0274	.0450	.0457	.0614	.0619
Superior Court Convict.	-.0043	-.0026	.0122	.0055	.0074	.0034	.0083	.0082
Match over data sources	.0653	.0257	.0298	.0520	.0704	.0596	.0378	.0306
Constant	-.1461***	-.1289***	-.1302***	-.1127***	-.1216***	-.1119***	-.1077**	-.0795*
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.227	.229	.229	.230	.229	.230	.230	.229
Model Classifications:								
% Correct Predictions	72.64	72.44	72.43	72.59	72.69	72.81	72.97	72.97
% False Positives	11.54	12.14	12.34	12.42	12.67	12.84	13.02	13.50
% False Negatives	15.81	15.42	15.23	14.99	14.64	14.34	14.01	13.53
RIOC	.488	.478	.476	.477	.473	.470	.467	.454

* p<.05 ** p<.01 *** p<.001

CHAPTER SIX

MODELS OF RECIDIVISM: BINARY, CRIMINAL CAREER AND TIME TO FAILURE MEASURES

One of our central themes is the contention that any impact of a judge's sentence must be evaluated after assessing the likelihood of recidivism prior to the imposition of that sentence. That is, attribution of any effect to this form of CJS intervention can only be made in light of the recidivism to be expected ("risk") given the nature of the offender to be sentenced. Our rationales for this stance deserved reiterating.

First, the model for rearrest developed in the previous chapter represents what can be predicted, and explained, for recidivistic behavior at that point in the individual's career when he/she comes before the court for sentencing. The variables of that model were purposely chosen to reflect what is known by the CJS, and the judge, about the offender at that time. On the basis of this information, some individuals can be expected to ultimately recidivate. It is crucial to evaluate the impact of the sanction relative to such expectations.

Second, the nonrandom nature of the funneling of individuals through the CJS mandates controls for this process. In part, we have tried to do this through the use of hazard variables to control for sample selection bias. Yet, as was also made clear in Chapter Three, the process of sentencing itself creates quite different groups of offenders. Judges use some of the information captured by our basic model in the determination of the particulars of the sentence. For example, we saw in Chapter Three that those incarcerated at a state prison were, on average, more likely to have been previously arrested. Put another way, our basic model is also predictive of

the actual sentence received, and failure to control for the variables of that model would lead to serious misestimations of the effects of the sentence.[1]

Together, these considerations lead us to seek a more thorough understanding of recidivism prior to the consideration of the sanction(s) represented by the sentences received in this sample. To this end, the present chapter concentrates on results for thirteen measures of recidivism. Again the independent variables representing the domains of social, presenting offense, anamnestic, delinquent career/onset, prior CJS-offender actions, and general control variables are used.[2] As in the previous chapter, the focus will be on variance partitioning and on the effects of specific variables as indicated by coefficients from linear models. In particular, the concern is on how the results of the basic model for rearrest presented in the last generalize to the other twelve dependent variables to be studied. Tables for rearrest are reproduced in this chapter to facilitate the comparison with other dependent variables.

The thirteen dependent variables used here were chosen in light of the results found in Chapter Four. The reader is reminded that, as such, the thirteen are drawn from a larger pool of measures, a pool that represents the

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[1] This last point is not immediately obvious. Continuing the example using prior arrests as an adult, we saw in the last chapter that each prior adult arrest significantly increases the probability that the individual will be rearrested. Suppose this variable were not controlled in any attempt to assess the impact of the sentence. Then a sentence to a State Prison would serve as a partial surrogate for prior arrests, and, consequently, this sentence would likely be associated with increased levels of recidivism after release. The conclusion would be one of a criminogenic effect for the sentence, when, in fact, the result is an artifact of failing to control for prior arrests before evaluating the sentence. Note, however, that we do not pursue the idea of explicitly modeling the sentencing process.

[2] In the subsequent two chapters, CJS interventions for the presenting offense are added to these equations.

three general classes of measures of recidivism discussed in Chapter Two. Four specific binary dependent variables are studied: rearrest (analyzed in the last chapter), rearrest for persons crime, rearrest for a repeat of presenting offense, and reimprisonment.[3] The choice of these four represents a focus on distinctions between those offenders who subsequently commit what are generally considered to be more serious offenses (against persons), those who may specialize in offending through a repeat of the specific instant offense, and those whose recidivistic behavior warrants imprisonment. The latter is arguably another measure of the seriousness of the subsequent offense, though it can also be interpreted as either an indicator of the CJS response to recidivism or the additional "burden" the offender places on the system.

Seven criminal career dependent variables will also be studied. Three are "count" variables, three are rate variables, and one is a seriousness scale of all subsequent offenses. The three count variables are differentiated according to whether or not the counts are of charges (any type of offense), of convictions (any crime type), or of persons offenses. The three rate variables differentiate between arrests, charges, and again the subset of persons charges only. All rate variables have been adjusted for time at risk and both the count and rate variables have been logarithmically transformed prior to the analysis.[4] The seriousness score is the sum of the

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[3] While we use the term "reimprisonment," this variable simply reflects whether or not the offender is subsequently imprisoned after sentencing and release. The (re)imprisonment could actually be the first incarceration of the offender's career.

[4] There is no strict parallelism in the comparison of the count and rate variables or in the distinction between charges, convictions, and arrests. We have chosen these criminal career dependent variables to demonstrate the possible diversity of measures of recidivism.

offense seriousness scores for each individual subsequent offenses charged. These weights are taken from Wolfgang et al. (1985).

Finally, two indicators will be used to capture the temporal aspects of recidivism. These time-to-failure measures consist of days to rearrest and days to reimprisonment. Results for each specific dependent variable are presented for four time periods of follow-up. Thus, each recidivism measure is studied using observation windows of one, three, five and nine years.

The data analysis strategy in this chapter is largely inductive in the sense that we test the same general model across a variety of dependent variables. In so doing, we do <u>not</u> follow the convention of dropping variables from specific equations and presenting trimmed models. Rather, the emphasis is on comparison across dependent variables of the same basic model that was developed previously. This model, it will be remembered, involved the exclusion of independent variables that do not have some general predictive power across the 153 dependent variables tested. Thus, we are in effect "stacking the deck" against finding <u>different</u> results across the various dependent variables to be studied, since a variable must be predictive in about a quarter of the equations tested to pass the inclusion criteria. As well, the analyses of Chapter Four demonstrated that all of the dependent variables are correlated to a degree, and this too works against finding different results.[5]

One final consideration is the degree of detail to be used in the discussion of our findings. The analytic task of this chapter is extensive,

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[5] One further reminder pertains to independent variables that are found not to be significant across any of the dependent variables studied in this chapter. Given the variable selection strategy used, such variables are known to be robust predictors of forms of recidivism not detailed here.

covering 13 dependent variables, measured at 4 different follow-up windows, and studied from the perspectives of communality analysis and linear modeling. As will soon become apparent, at the very general level of the substantive conclusions to be reached, only a few important differences are observed across dependent variables and observation windows. Conversely, at the very specific level of the magnitude of the estimated effects, many real differences can be seen. In order to avoid some tedium in the presentation of our findings, we peg the discussion at an intermediate level of detail. The reader is encouraged to note the specific differences in the size and significance of coefficients across dependent variables.

BINOMIAL RECIDIVISM MEASURES

Communality Analyses

The communality analysis results for the binary variables are listed in Tables 6.1 through 6.4.[6] In general the models for rearrest (Table 6.1) and reimprisonment (Table 6.4) are able to explain higher proportions of variance than those of rearrest for persons crime or for repeat offending. By the three-year window, twice as much variance is explainable for rearrest and reimprisonment than for the other two dependent variables. Unlike the results for rearrest, for the other three binary variables the proportion of variance that can be explained monotonically increases the longer the follow-up period.

Roughly speaking, about half of the explained variance is shared among the predictive domains in that it is attributable neither to sets or pairs of sets of variables. This holds true across the four tables. The

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[6] It will be remembered that it is the R-squared from ordinary least squares regression that is being partitioned here. In addition, the interaction terms are assessed after entering all individual sets of variables.

reimprisonment recidivism measure stands out, however, as more of explainable variation is not attributable, reaching 58% of the variance that can be explained at nine years. Thus, focussing on reimprisonment as a measure of recidivism does not lend itself to differentiating sources of variation into unique contributions by variable group. In other words, the factors explaining reimprisonment have more "predictive communality" than found for other dependent variables.

The results for unattributed explained variance of reimprisonment can be accounted for, in part, by the social variables. They lack unique contribution to the explained variance of reimprisonment (Table 6.4), relative to the other three binary dependent variables. While about 10% of the explained variance can be uniquely attributed to social variables for three of the four binary variables, only about 5% of the variance for reimprisonment can be attributed to social variables. Note too, that the pattern of an increasing ability of the social variables to account for differences in outcomes, seen for the rearrest indicators, is reproduced for the other binomial measures.

Presenting offense variables explain very little of the binary dependent variables, save for rearrest for presenting offense type. There between 9 and 15% of the explainable variance can be attributed to presenting offense characteristics. As such, the results are supportive of a degree of specialization among offenders: those with certain presenting offense characteristics are more likely to repeat the presenting offense, and part of their repeating can only be predicted on the basis of presenting offense information.

Anamnestic variables, which rank second in unique contribution to the explained variance of rearrest, generally rank highest in unique contribution to the explained variance of the other three binary variables. Across all the binary dependent variables, about 10% of the explained variance can be uniquely attributed to anamnestic variables, with even higher percentages found for rearrest for persons crimes (Table 6.2). Thus, the results of previous research are corroborated here in that the anamnestic variables' unique contribution to explained variance is high.

Variables measuring the offender's delinquent involvements and onset of criminal career offer little explanatory power that is uniquely attributable to these variables. The exception is, perhaps, reimprisonment where 7 to 8% of the explainable variance can be shown to belong to these variables. In general, however, less than one percent of the variance of these dependent variables can be attributed to the delinquent career/onset variables.

Prior CJS/offender action variables also uniquely explain a negligible proportion of the variance of the four binary dependent variables. Somewhat surprisingly, the general control variables cannot uniquely account for much variance across all the four binary dependent variables either. Contributions range from less than one percent of explained variance of reimprisonment to a high of 6% of the explained variance of rearrest at nine years. Thus, for example, the ubiquity of the age variable (one of the general control variables) to <u>uniquely</u> explain recidivism is called into question.

The results represented in Tables 6.1 through 6.4 show that the unique contributions of <u>pairs</u> of social variables with other types do not predict reimprisonment and repeat offending as well as rearrest or rearrest for persons offenses. Presenting offense pairs do very poorly across all four

binary dependent variables. The anamnestic variables in conjunction with other criminal career type independent variables (delinquent career, prior CJS action) and with the general controls fair better, uniquely accounting for between one and seven percent of the explainable variances across models.

Generally about 3 or 4 percent of the attributable variance for rearrest, rearrest for persons crimes, and reimprisonment is unique to the set of interaction terms. Interaction terms are most effective for a rearrest for presenting offense type. At one year of follow-up, 13 percent of the explainable variation is uniquely attributable to the interaction terms. There is also a slight pattern associated with an increase in post-sentence observation window for the interactions. In absolute magnitude, the proportion of explained variance increases for all binomial measures, save the rearrest variable where the proportion is constant. This suggests that the mediation of the main effects is of greater import for rearrest for a persons crime, a repeat of the presenting offense, and being reimprisoned. Note, however, when these proportions are converted to a base of explainable variation, the unique impact of the interaction terms decreases with longer follow-ups.

In summary, the variance partitioning results suggest that there are some differences, as well as broad similarities across the four binary dependent variables. Shared variances range from the high 40s to high 50s in percent of unattributable explained variance across the four binary measures. Anamnestic variables and social variables are generally the two categories of independent variables that can uniquely account for the most explained variances in three of the four binary variables, but the social variables are weak in their unique contribution to explained variance of reimprisonment.

Pairs of sets of variables involving anamnestic variables uniquely account for more variance than other types of pairs of sets. Interaction variables vary in their unique predictive ability, but account for a higher percentage of the explained variance of repeat offending than the other three binary dependent variables. Yet, even here only one percent of the explained variance of repeat offending is attributable to the interaction terms.

Based on these results, it appears that how recidivism is defined will make some difference for the overall conclusions. Some indicators ar more amenable to statistical prediction than others, and what accounts for differences in levels of recidivism is, in part, tied to the particular measure. The nature of the measure-specific conclusions are more easily seen in the findings of the next section.

Regression Coefficients

The results for the logistic regression analyses of the four binary dependent variables are presented in Tables 6.5a-d. All the independent variables and interaction terms introduced in the last chapter are included in the models here. As before, coefficients are expressed as effects at the mean of the dependent variable. To simplify the discussion, we will refer to an "effect" if it is statistically significant across two or more time periods of follow-up.[7] Such independent variables are described here as "consistently" predictive of a dependent variable. Independent variables that are frequently not-significant across dependent variables are also noted.

The general theme of the results described below is that there are broad similarities across the models of the four dependent variables, but also

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[7] We do this with the recognition that many of the nuances in our results will go undiscussed.

sufficient differences such that one cannot claim the general model yields comparable conclusions across all four forms of binary recidivism. This is true both because the impact of independent variables is not uniform across dependent variables and because the patterning of coefficients across windows differs by independent-dependent variable combinations.

Differences among the four regression models in Table 6.5 are exemplified by the structural variables. In general, rearrest and rearrest for a repeat of the presenting offense are predicted by a large number of structural variables, while persons crimes and reimprisonment dependent variables are predicted by a relatively few structural variables. For example, the following variables are statistically significant predictors of rearrest across at least two follow-up windows, but are not statistically significant predictors of rearrest for persons crime: years at current address, has needle marks, doesn't live with family, committed PO with a group, and victim was a stranger. Of all the structural variables other than race, gender, and lives in a urban area, only "not a school dropout" predicts rearrest for persons crime (in two of the four windows presented).

The impact of these variables is tied to the length of the post-sentence follow-up. Consistent with the variance partitioning results, we find that the magnitude of the coefficients for the significant structural variables tends to increase with longer follow-ups. Where this pattern does not hold (e.g., victim was a stranger in the rearrest models; has a job after sentence in the reimprisonment model) tends to be specific to a particular dependent variable.

Also particular to the dependent variables is the magnitude of the coefficients estimated. Take, for example, how offender race is related to

each of the binomial recidivism variables, measured over a period of five years. Blacks have an increased probability of .156 of being rearrested, an increase of .113 of rearrest for a persons crime, .062 for a repeat of the presenting offense, and .108 of being reimprisoned. The impact is over twice as much for some variables than others. More importantly, these differences are <u>not</u> related to the overall ability of the model to account for variation in the dependent variable. The reader is alerted to the fact that more variable-specific results can be found in these and ensuing tables.

The effects of the structural variables on repeat of presenting offense are similar as those found for rearrest. In general, however, the magnitude of the effects are smaller. For example, in a nine-year follow-up, Hispanics have a 15.4% higher chance of rearrest than non-minorities, but a 7.45% higher chance of rearrest for the same type of offense as the presenting offense. Also, being unemployed has a positive relation to rearrest, while it is not related to repeat offending. However, a similar concept, having a job after sentencing, has a negative effect on repeat offending across the one and three year windows.

The results of the model for reimprisonment are similar to those of rearrest for persons crimes in that relatively few structural variables are statistically significant. Here too, there are several differences in the models, as reimprisonment is predicted by having a job after sentence, having a history of drug problems, and having victimized a stranger, while rearrest for persons crimes is not predicted by these variables.

In summary, the effects of the structural variables seem more pronounced for rearrest and for repeat of presenting offense, than for persons crimes or for reimprisonment. This suggests support for the idea that there are

different motives behind the commission of crimes against persons and for serious crimes resulting in imprisonment. Rearrest and rearrest for same crime type are largely driven by property offending (the most common type of crime), and the results here suggest the importance of structural variables (such as being a school dropout, living alone, and committing crime in a group) for these dependent variables. Race is predictive of all four binary dependent variables, while gender and urbanness, predict three of the four (all but reimprisonment). Thus, the theories of subcultural or social ecological explanations receive some support here, across all the binary dependent variables.

The effects of the presenting offense variables across these dependent variables are somewhat easier to characterize. A presenting offense of property crime predicts rearrest and rearrest for another property crime, but neither rearrest for persons crime nor reimprisonment. A PO of persons crime, on the other hand, predicts subsequent persons crime arrest, as well as a repeat of the presenting crime. Thus, again there is some evidence of support for the idea of specialization in offending.

The Wolfgang severity score for the presenting offense is negatively associated with rearrest and rearrest for presenting offense type. This corresponds to the specialization hypothesis in that offenders who commit serious crimes are not as likely to engage in the more frequently committed property crimes. At the same time it is somewhat surprising that the Wolfgang severity scale does not predict subsequent rearrest for persons crimes across any window, since persons crimes tend on average to be more serious crimes.[8]

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[8] In should be noted, however, that the Wolfgang scale score can be high when an offender has committed many property offenses in the past, so the interpretation of this variable as predominantly a measure of a tendency to

Two indicators of "recent chronicity" -- having pending charges and being on probation at PO -- are found to predict all four binary measures of recidivism (although the effects are somewhat smaller for rearrest for persons crimes). The results from all presenting offense variables suggest that the there is some degree of specialization in recidivism that can be predicted based on presenting offense characteristics and that the offender's legal status at the time of sentencing, specifically the existence of other charges and being on probation, effects the chances of recidivism.

The results for the anamnestic variables (again Table 6.5) show that the number of prior adult arrests predicts all four binary variables in the expected, positive, direction. Interestingly, the hypothesized "specific deterrent" effect of "number of prior adult charge convictions" discussed in the last chapter is found again for rearrest for persons crimes and rearrest for repeat offense, but not for reimprisonment. Also, number of prior property convictions is found to negatively impact the likelihood of a repeated presenting offense. Thus, there seems to be some "generality" to the specific deterrent effect of a conviction as opposed to an arrest, yet this can be offset by the fact that the number of arrests tends to have a positive impact in recidivism. The level of charges in the past five years is also found to be predictive of all four binary variables, providing support for the importance of the "recent criminal activity" explanation of recidivism.

The number of prior persons convictions is found to positively impact the chances of rearrest for persons crimes and for reimprisonment. Again support is found for specialization in recidivism. Also, it is interesting that this "conviction" variable has a positive effect -- suggestive that prior

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commit individual serious crimes is possibly incorrect.

convictions for persons crimes does not have a specific deterrent effect, in contrast to the results seen for the number of prior convictions for any type of crime. Number of prior weapons convictions also positively impacts the chances of rearrest for persons crimes, again supporting the idea of specialization, although this variable also positively impacts the chances of rearrest. Finally, being off the street within the past two years (serving some time in jail or prison) positively affects the chances of all four binary variables. While this again lends support to a "recent chronicity" component of recidivism, it also undermines the notion of specific deterrence. Under a specific deterrence hypothesis, it would be expected that those recently caught and incarcerated would be less likely to recidivate. In sum, the anamnestic variables' effects reveal some support for the claims of specialization and mixed evidence for specific deterrence in recidivism processes.

The number of arrests as a juvenile positively affects the chances of recidivism across three of the four dependent variables (and is also positively associated with a repeat of the presenting offense at nine years). As well, years since first incarceration is found to positively effect all four binary measures of recidivism. In contrast, years since first drug use is negatively associated with rearrest for persons crimes. This could be interpreted as suggesting the importance of specialization (in drug crimes) in that those who begin drug taking early are less likely to participate in subsequent persons crimes, although the effects here are relatively weak. Overall, delinquency and age of onset are important predictors of binary recidivism, despite the presence of numerous other variables in the models. As such, these variables yield a quite different interpretation from the

"recent chronicity" effects seen for other independent variables.

Prior incarcerations were not found to effect any of the binary measures of recidivism except rearrest, where the effect is negative. This is a somewhat surprising result in that, a priori, one could argue that prior incarcerations could have a positive impact either as an independent measure of the offender's "badness," offense chronicity, or due to a labeling effect from being imprisoned. Conversely, a negative effect as a consequence of specific deterrence might be postulated. The fact that prior incarcerations is neither may be reflective of the conflicting processes underlying this measure. Number of prior parole revocations was found to negatively impact the chances of rearrest for persons crimes and repeat of presenting offense, again suggestive of possible specific deterrent effects. However, recent parole revocation is generally positively associated with repeat of presenting offense. An evaluation of bad conduct at last probation was not found to be predictive of any of the binary variables.

The results for prior CJS/offender action are difficult to characterize and may be reflective of the complexity of interpreting these kinds of variables. That is, those individuals who have prior incarcerations and revocations represent individuals who have penetrated the criminal justice system the furthest in the past. They have survived the various "filters" to experience imprisonment and probation and parole revocations only to appear in the sample analyzed here. Thus, these individuals are likely to be among the "worst" offenders and the "most punished." As the prior CJS-offender action variables are "competing" to explain recidivism against all the other variables listed in the models of Table 6.5, these other variables may be largely redundant with prior CJS-offender variables. Thus, it is not that

surprising that they are poor predictors of recidivism in the models presented here.

The general control variables are all found to be good predictors of the four binary measures of recidivism, save age at sentencing for rearrest for a repeat offense. All the statistically significant effects of these three control variables are negative. Thus, the older the offender, the less likely recidivism. If the offender is born out of New Jersey or, if the coder prognosis is good, the less likely recidivism. This last result in interesting in that it is widely believed that subjective evaluation variables are entirely redundant with more objective indicators of recidivism. The present results suggest that there is an independent, albeit small, predictive component to the coder's prognosis for recidivism.

Results for the interaction terms are evidence of the complexity of the phenomena under study. Some have no impact for any of the binomial recidivism indicators. Some are found to affect recidivism similarly, while others show differential effects depending upon the form of recidivism. For example, although being black generally results in enhanced chances of all four measures of "binary recidivism," this effect is mitigated in the presence of offenders on probation at the presenting offense (PO) or by the number of prior adult arrests. However, the product term of black and prior property convictions increases the chances of rearrest across all four binary measures of recidivism.

The product term for female and prior Part I offenses is positive for rearrest, but not for the other three binary recidivism measures. Thus, the main effects for being female and for prior Part I offenses are unmitigated for particular forms of offending (i.e., persons crimes and repeating the

instant offense) and for the CJS's response to recidivism in terms of reimprisonment. It is only for rearrest for any type of offense where we see a greater impact of prior Part I offenses for females.

The generally negative impact of age on rearrest and reimprisonment measures of recidivism seems to be even greater in the presence of prior treatment for drug/alcohol problems. This is consistent with a "burnout" explanation. Similarly, age and prior property crimes interact so that increases in either result in lowered recidivism.

The product term for PO of violent crime and "has detainers" is related to three of the four binary dependent variables, lending support to the idea that this is tapping a "data control" aspect of the analysis. Specifically, offenders convicted of violent crime and who have detainers are probably often sent to other states and remain there, thus avoiding further detection in New Jersey for crimes they might commit. Hence the estimated effect is negative and relatively large. The exception to this applies to reimprisonment, where this product term is not statistically significant.

The interaction term of PO property and number of juvenile arrests also has a negative impact on the chances of rearrest and rearrest for persons crimes. As well, a PO of property crime and age at first arrest interact to negatively impact upon a subsequent arrest for a persons crime. At the same time, a PO for property crimes and years since first incarceration has a negative impact on repeat offense recidivism. These results lend support to a "burn out" interpretation for property offenders, as well as support for offense specialization in that some aspects of our basic model are quite different for property offenders.

Having a presenting offense for drugs and number of prior adult convictions has a positive impact on all four binary measures of recidivism. A presenting offense of drugs and number of prior Part I offenses is negatively associated with rearrest and rearrest for repeat offense. Thus, there is some support for the idea that drug offending for chronic offenders enhances recidivism probability, while drug offending in conjunction with prior Part I offenses lessens it -- again a result that might possibly be interpreted as a type of offender specialization effect.

In summary, the statistically significant interaction terms suggest that the effect of two of the most consistent predictors of recidivism, being black and age, are mitigated or enhanced by the presence of other offender characteristics. Also, presenting offense property and drug offenders have their effects mitigated or enhanced by the presence of other offender characteristics.

The results of Table 6.5 may be summarized according to several emergent themes. At the risk of oversimplifying the specifics of our findings, we point to the following ten conclusions:

1. There are differences across the four binary measures of recidivism such that one cannot say that a model for one is adequate for the explanation of all binary dependent measures.

2. The models for the four different dependent variables suggest that the distinction between property and persons offenders is an important one. Several structural variables seem to be more consistently predictive of property than either persons crimes or reimprisonment.

3. At the same time, some variables predict all forms of binary recidivism. For example, race, gender and urbanness are generally significant in all four models of binary recidivism, at all post-sentence observation windows.

4. Indicators of "recent offense chronicity" are important predictors of recidivism.

5. There is some evidence of "specific deterrent effects" resulting from prior convictions.

6. Early involvement in crime and drugs are important aspects to explaining recidivism as measured by distinct events.

7. Age has a negative effect on recidivism, yet more so for some types of offenders than others.

8. A subjective measure of the offender's likelihood of recidivism retains its predictive ability even when controlling for many objective measures known to be related to recidivism.

9. Some support is found for the idea of maturation effects or "burn out" of criminal involvement, possibly as a result of drug involvement, or a history of property offenses.

10. The impact of several variables is mitigated or enhanced by the presence of other variables. Specifically, being black, type of presenting offense, and number of prior arrests/convictions seem to be involved in several empirically important interaction effects.

These conclusions are offered tentatively, prior to assessing how these variables influence recidivism as measured by other indicators. Still, these 10 themes are the most consistent across binomial aspects of recidivism.

Individual Prediction and Binary Dependent Variables

The results on the bottom of Table 6.5 regarding individual prediction reveal several patterns. Rearrest, of course, is the most central of the four binary dependent variables, since logically one must be rearrested for any other form of recidivism to occur. Consequently, it is the more frequent form of binomial recidivism we observe here. While over half those studied are rearrested by nine years, about a quarter are rearrested for persons crimes or for a repeat offense or reimprisoned. Pseudo R-square values are relatively low for rearrest for persons crimes and for rearrest for repeated crime type reaching maximum values of .157 and .126 at nine years compared to .228 and .230 for rearrest and reimprisonment, respectively. Thus, the general model presented in Table 6.5 is better able to predict recidivism in the form of

rearrest and reimprisonment, than persons or repeat offense rearrests. In this sense we find both rearrest and reimprisonment to be more deterministic than rearrests for particular types of offenses.

The "percent correct" predictions correspond to the expectation that the rarer the event predicted, the higher the percent correctly predicted. Thus, the percent correctly predicted is highest for rearrest for persons and repeat offense recidivism. These types of recidivism, along with reimprisonment, have low false positive rates, never exceeding 6 percent of the sample even after nine years. The percent of false positives for rearrest, however, reaches 13.5%. Most of the errors in prediction, then, result from false negatives, particularly for the rarer recidivism events (i.e., those with low base rates). RIOC values are generally between 40 to 50 percent, except for reimprisonment, where they are somewhat higher. These vary from 46 to 56 percent between one and nine years. Interestingly, for rearrest, the percentage of false negatives decreases with longer windows, but the opposite is true for the other three binary dependent variables.

In summary, errors in prediction increase with length of follow-up and this coincides with changes in the base rate. Generally there is an increase in the false positives, but not necessarily in false negatives with longer follow-up windows. Furthermore, the predictions of our models, as measured by the RIOC statistic, reach their maximum efficiency at the five-year window and this does not correspond to the maximum base rate observed. Reimprisonment is better predicted according to the RIOC measure, than are the other three binary dependent variables. This can be attributed to a relatively low false negative rate for reimprisonment, compared to that found for the other dependent variables.

CRIMINAL CAREER DEPENDENT VARIABLES

The findings of the previous section have focussed on the likelihood that some recidivistic event occurs after sentencing and release. That some form of recidivism takes place is conceptually distinct from how often it occurs or, more generally, the level of recidivism displayed by the offender. The latter is more appropriately captured by what we have labeled criminal career measures of recidivism. It is to these results that we now turn.

Communality Analyses

The results of the variance partitioning of the seven criminal career dependent variables are presented in Tables 6.6 through 6.11.[9] In terms of overall variance explained, the results across the seven models show that approximately a third of the variance in the count and rate variables is explained after at least the three-year window, although less is explained for the persons count, persons rate, and summed seriousness score. Note that more variance is explained when using the count and rate variables than when the binary variables (Tables 6.1 through 6.4 above) are the focus.

The patterns of shared variance or communality is similar in that most of the count and rate variables, as well as "summed seriousness score," have about 55% of their explained variance unattributed to any one set or pair of sets of variables. Again, the persons count and persons rate variables show a different pattern -- somewhat less of their explained variance is unattributed, reaching about 48 for the three to nine year follow-up windows. Thus, as with the binomial forms of recidivism, there is considerable

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[9] It will be remembered that all count and rate variables have been logarithmically transformed prior to the analysis. To make our discussion more readable, we will not refer to the logged form of these variables. Rather we leave these transformations implicit in the text.

predictive communality across the sets independent variables for the seven criminal career dependent variables.

Looking at the unique contributions of individual sets of variables, we find the social variables accounting for slightly more of the count and seriousness variables than the rate variables. Roughly 8 or 9 percent of explainable variance for the former and 5 to 7 percent of the explainable variance of the latter can be assigned to the set of social structural independent variables. Presenting offense characteristics do not fair very well across all seven models, never exceeding more than 3 percent of that variance which can be explained.

Anamnestic variables (which are predominantly "count" measures themselves), are the best single category of independent variables predicting each of the seven criminal career recidivism measures. This is consistent with the results seen for the binary variables discussed earlier. Somewhat surprisingly, anamnestic measures also uniquely account for relatively large proportions of the variances of the persons count and rate variables, as well as of the seriousne of subsequent offenses. Approximately 12 to 14% of the explainable variance of the persons and seriousness dependent variables can be uniquely attributed to the anamnestic variables, compared to about 10 to 11% of the other criminal career dependent variables.

Delinquent career/onset variables uniquely account for between 3 to 7 percent of the explainable variances across the seven dependent variables, but do a little better for the persons and seriousness recidivism measures. Prior CJS/offender action variables, on the other hand, do poorly across the board. General control variables account uniquely for between 3 and 5% of the variance across the various measures, and generally perform a bit more weakly

for the rate variables than the count variables.

In terms of the variance accounted for uniquely by pairs of sets of variables, the anamnestic variables in conjunction with delinquent career, prior CJS action, and general controls represent the strongest group of predictive pairs of variable sets -- a finding that was noted above in the discussion of the binary variables. Similarly, the interaction terms (as a set), account for about 3 to 5% of the explainable variance across all seven of the dependent variables, again similar to the results found for the four binary dependent variables discussed above.

In summary, the analysis of the variance partitioning for the criminal career variables once again suggests that again there is a great deal of communality among the sets of predictor variables -- over half for most dependent variables. There are, however, some notable differences in the models for persons crimes and crime seriousness relative to other criminal career dependent variables. Some small differences in attributed variance also seem to occur for count as compared to rate variables. Anamnestic and social variables rank first and second in their unique explanatory power, while it is the anamnestic variables in conjunction with other sets of variables that represent the strongest "group" of pairs of sets of variables. Overall, the results are generally similar to that reported above for the binary dependent variables. In addition, and perhaps not surprisingly, the results of the variance partitioning across the count and rate variables are quite similar as all are based on counts of arrests, or charges or charge convictions.

Regression Coefficients

Results from the regression analyses for the seven criminal career

dependent variables are described next. As for the binary dependent variables above, we describe the findings across each of the dependent variables focussing on each set of variables sequentially. In Tables 6.13 through 6.19 standardized regression coefficients are also presented in parentheses.[10]

The structural variables are more consistently predictive of the "non-persons"[11] count dependent variables (charges and convictions) and of rate dependent variables (adjusted arrest and charge rates), as well as of the seriousness dependent variable, and less consistently predictive of the persons count and persons rate variables. Of the 15 structural variables listed across Tables 6.13 through 6.19, between 10 and 13 are significantly related to the non-persons count and rate dependent variables at the nine-year follow-up. In contrast, only 6 structural variables are predictive of persons count and rate dependent variables. While the actual number of statistically significant coefficients varies somewhat over time, this general pattern holds true across all the windows.

Five of the structural variables are quite robust for all seven criminal career dependent variables. These are: offender is unemployed, black, Hispanic, female and lives in an urban area. The strongest effects are generally observed for being black, Hispanic, and female across most of the models presented. After these, whether or not recidivism is measured by persons or nonpersons indicators becomes important for assessing the impact of

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[10] Computation of standardized coefficients is not straightforward when interaction terms are included in the model. Following the procedures outlined by Aiken and West (1991), we have computed the correct standardized coefficients for all regression models.

[11] By "non-persons" dependent variable we mean those dependent variables that count all subsequent offenses, whether persons, property or other types of crimes.

the social structural independent variables. In the models of persons count and rate dependent variables, "not a school drop out" and "has job after sentence" are also significant across the nine-year windows of Tables 6.15 and 6.18. Overall, persons-based recidivism can be accounted for by relatively few of the structural variables: predominantly race, ethnicity, and gender, but also employment. Residential stability, drug problems and treatment, living with a family, group participation in crime and victimizing stranger are not found to be important predictors of persons count or persons rate variables.

When recidivism takes the form of nonpersons count and rate measures, additional structural variables are found to exert an impact. Particularly important are variables measuring drug involvement: having needle marks, a history of drug problems, and prior treatment for drugs/alcohol. Being unemployed, having a job after sentencing, and being a high school dropout also are predictive of the non-person count and rate variables. Furthermore, committing crimes in a group is predictive (negatively) of the count variables of total charges and of convictions, but not of the rate dependent variables. Victimizing strangers, on the other hand, is positively related to all non-persons count and rate variables, as well as offense seriousness.

Compared to the results reported above for the binary dependent variables, the structural effects on rearrest are quite similar to those found for the non-persons count and rate variables reported here, while rearrest for persons crime and reimprisonment are similar to those reported here for persons counts and rates. Thus, at some level of generality, the effects of the structural variables are similar for binary and criminal career dependent variables. These effects are noticeably different when persons-based

dependent variables are taken as the indicator of recidivism.

The patterns of effects for presenting offense independent variables are relatively clear: persons counts and adjusted persons rates are different than are those for the other dependent variables. Specifically, the "non-person" dependent variables are related to a presenting offense for a property crime, having pending charges, and being on probation at the PO. In addition, the count variables are associated with the Wolfgang severity score for the PO, while the rate variables are not. The summed seriousness dependent variable is predicted only by two variables: PO property crimes and pending charges. The persons count and rate variables, on the other hand, are predicted by PO of persons crimes and pending charges. Again, as with the binary variables discussed earlier, there is an important distinction between persons-based and other forms of recidivism. Of all the presenting offense variables, the number of pending charges emerges as the most consistent and strongest overall predictor of recidivism. Whether or not the presenting offense is property or persons crime (for non-persons and persons dependent variables respectively) is also important. In short, specialization in offending patterns, and its implications for prediction, are supported again here as it was for the analysis above on binary dependent variables. However, the results there were not as clearly differentiated between persons and non-persons as they are here for the criminal career dependent variables.

Results for the anamnestic variables show that three measures of offending chronicity are predictive of all seven of the criminal career dependent variables: number of adult arrests, number of charges in the past five years, and off the street in the past two years. "Prior persons convictions" emerges as a predictor of subsequent persons count and rate

recidivism. Prior weapons convictions are predictive of most of the seven criminal career dependent variables, all but total convictions. Number of adult charge convictions is again found to have negative effects on recidivism for all dependent variables except total charges, though relatively inconsistent negative effects are seen for total convictions and the adjusted arrest rate. The model for the binary dependent variable rearrest for persons crime did not reveal this pattern. For the person count and rate dependent variables, the effects of prior adult convictions are relatively large with standardized coefficients of -.038 and -.076 (Tables 6.15 and 6.16). The results for the summed seriousness score are quite similar to those found for the non-persons models. Overall, the results for the anamnestic variables demonstrate that the distinction between persons and other dependent variables is important. Yet, there are some variables that are good predictors of both general forms of recidivism.

Two delinquent career/onset variables are found to be predictive of all seven criminal career dependent variables: number of arrests as a juvenile, and years since first incarceration. Both have positive effects on recidivism. Age at first arrest is also significantly related to all of the criminal career dependent variables, but not as consistently across follow-up periods. Years since first drug use is found to negatively effect total convictions, total persons charges, adjusted charge rate, and summed seriousness. In contrast to earlier findings, the delinquent career/onset variables do not provide support for the specialization argument in that most are generally predictive of all the criminal career dependent variables. In general though, these results are not unlike those found for the binary dependent variables discussed above.

Prior CJS/offender action variables are rather weak and inconsistent in their relationships to levels of recidivism, as they were for the binary variables above. For example, number of prior incarcerations is found to be negatively associated with total convictions and positively associated with persons charge rate, but unrelated to any other measures. Bad conduct on the last probation is positively associated with total convictions, adjusted arrest and charge rates, and summed seriousness. Having one's recent parole revoked results in more subsequent arrests and charges per year, as well as in more persons charges. Again, several of the prior CJS/offender action variables are predictive of some criminal career dependent variables, but general patterns across the dependent variables are not evident.

All general control variables are found to be significantly related to all seven criminal career dependent variables. Age consistently has the strongest effects of the three general control variables, followed by born out of state and coder probation prognosis, respectively. While there are specific differences in terms of the magnitudes of the coefficients for these variables, at a more general level, how these variables impact upon recidivism is not dependent upon how recidivism is defined.

Several interaction terms were also found to be statistically significant across the spectrum of criminal career dependent variables. The interaction terms for being black and on probation at PO, and being black and number of prior adult arrests are found to negatively impact recidivism for all the count and rate dependent variables, save the adjusted charge rate where being black and on probation is significant only at five years. Being black in interaction with the number of prior property convictions is found to predict all seven criminal career dependent variables except total persons

charges. The black-number of charges as a juvenile interaction positively impacts all seven criminal career dependent variables. The product term of female and number of Part I charges is positively associated with all the criminal career variables except the persons count and rate variables. All of the product terms involving age are negatively associated with the non-persons criminal career dependent variables, including summed seriousness. At the same time, there are some sporadic interaction effects involving age for the persons crimes count and rate variables.

As was generally found for the binary dependent variables, the combination of a PO of violence and having detainers is found to predict recidivism. Like many of the other interactions, this effect is significant for all seven criminal career dependent variables. Again, this can be interpreted as a type of "quality of data control" variable -- offenders with detainers and a violent offense may end up serving time in another state and possibly not returning to New Jersey.

Presenting offense of property in interaction with number of adult arrests tends to have a positive effect on recidivism across most measures, but not at all follow-up periods. The product term of PO of property and prior property convictions, on the other hand, tends to have negative effects on recidivism, supporting a possible specific deterrent effect. The product term for PO of property offense and years since first incarceration has a positive influence total post-sentence charges at one and three years, total convictions, adjusted arrest rate, and adjusted charge rate for the one and nine-year windows.

Presenting offense of drug offense interaction terms have both positive and negative impacts on criminal career measures of recidivism. In

interaction with number of adult convictions, the effects tend to be positive across the dependent variables. In interaction with number of prior Part I charges, the effects are negative. Finally, PO of drugs and having one's last parole revoked generally enhances recidivism measured by counts and rates, although not persons counts and adjusted persons rates.

The interaction terms have a variety of effects across the seven criminal career dependent variables studied here. There is a subset of interaction terms statistically significant across all the models, while others are significant for only the persons dependent variables or the general count and rate variables. The results show that race and age effects, as well as the effects of presenting offense, are not linear, but are a function of the values of other variables. These include the anamnestic variables, as well as some structural, and juvenile career/onset variables. In general, the results are similar to those reported earlier for the binary dependent variables.

In all, the analyses of the criminal career dependent variables has shown several general patterns. In contrast to the findings from the binary recidivism measures, we tend to account for more variation in these forms of recidivism. As a consequence, more independent variables are significantly related to differences in levels of recidivism. This, in turn, increases the number of substantive themes that have emerged. These are:

1. As was found for the binary variables, there are differences across models in what predicts specific measures of recidivism.

2. There are broad similarities in the results across the count and rate variables for the non-persons offenses, as well as for summed offense seriousness.

3. The distinction of persons crimes compared to all others continues to be important here as it was for the binary dependent variables.

4. There is a subset of predictor variables that are consistently (and strongly) predictive of all forms of recidivism.

5. Drug involvement is predictive of the various non-persons count and rate variables.

6. Evidence of recent criminal activity predicts subsequent criminal involvement.

7. Anamnestic variables, especially number of prior adult arrests, are the best predictors of criminal career measures of recidivism.

8. Prior convictions may have a type of a specific deterrent effect independent of any additional sanctions imposed.

9. Juvenile arrests and years since first incarceration are generally important predictors of criminal career recidivism.

10. Prior CJS interventions and offender action variables have weak and inconsistent effects upon levels of recidivism.

11. The general control variables are consistent and strong predictors of criminal career recidivism.

12. Interaction terms reveal that race, age, and presenting offense effects are mitigated by other offender characteristics.

TIME TO FAILURE VARIABLES

A focus on the time it takes for an offender to experience a recidivistic event offers the potential for a different picture of how the independent variables are related to recidivism. By incorporating a temporal aspect to recidivism, the criminal justice system's response to subsequent criminal behavior becomes more salient as it becomes an integral part of the definition of recidivism. Differences in time to failure now represent not only differences in offender recidivism, but also, potentially systematic, differences in how long it takes the CJS to respond that recidivism.

We use two variables to represent the general class of time to failure measures -- days to rearrest and days to reimprisonment. Both may contaminate

the individual's actual behavior with processes within the CJS. Those who are rearrested quickly may differ from those taking longer simply because CJS agents took longer to detect, report, and process illegal behavior. Similarly, longer time until reimprisonment may reflect delays in obtaining court-mandated custody rather than a delay in an offender committing an offense that leads to custody. Consequently, there are no expectations that the results seen earlier in this chapter will be reproduced when time to failure measures constitute the definitions of recidivism.

Communality Analyses

The variance partitioning analyses for days to rearrest and days to reimprisonment are presented in Tables 6.20 and 6.21, respectively. Somewhat surprisingly, the results show broad similarities to those found for the binary and criminal career dependent variables. In general, about 30% of the variance of each is explained at the maximum window of nine years. Unattributed variance is approximately 55% of the explainable variance, indicating that most of the explained variance is shared by three or more domains of independent variables.

The attribution of unique variance results are somewhat different than what was reported above for the binary and criminal career dependent variables. The social and anamnestic variables are again highest (and quite similar in magnitude) in their unique contribution to the proportion of variance that can be explained in days to rearrest, but not in days to reimprisonment. Here, the delinquent career/onset variables rank highest (at nine years), and are second to the anamnestic variables for the other years. Thus, days to reimprisonment stands out as distinct from all the other patterns of unique variance attribution found in this chapter.

Prior CJS/offender action variables are generally not predictive of the timing of failure, a result parallelling that found for the other forms of recidivism studied here. The general control variables account for about 5% of the explainable variance in days to rearrest, a result similar to that found generally above. However, these factors are less important for the timing of reimprisonment. About one percent of the explainable variance in days to reimprisonment is uniquely attributable to the prior CJS/offender action set.

Turning to pairs of sets of variables, the results for days to rearrest indicate that the anamnestic variables in conjunction with other variable types (especially delinquent career, prior CJS actions, and general control variables) account for most of the variance uniquely attributable to pairs of domains. However, the relative percentages are quite low (2 or 3%). Generally, the same pattern is found for days to reimprisonment, with slightly stronger effects of the anamnestic pairs (4 or 5%). The interaction terms, as for the other types of dependent variables above, account uniquely for about 3 or 4% of the explainable variance of these failure variables.

In summary, the variance partitioning results for the failure variables suggest that there are some differences relative to the results found for the other types of dependent variables. Delinquent career/onset variables uniquely account for more explained variance than in the earlier models. Time to reimprisonment stands apart from time to rearrest. In other respects, however, the results of the variance partitioning are similar to those found for the other forms of recidivism.

Regression Coefficients

Tables 6.22 and 6.23 show the results for the regression analyses of the

time to rearrest and reimprisonment measures. The structural variables predict days to rearrest in a manner quite similar to those reported for the binary rearrest variable as well as the non-persons criminal career count and rate variables.[12] Time to rearrest is lengthened if the offender has a job to go to after sentencing, is female, has not dropped out of school, doesn't live with his/her family, and committed the crime in a group. Time to rearrest is lessened if the offender is unemployed, black, Hispanic, lives in an urban area, has needle marks, and victimized a stranger at the PO. Time to reimprisonment results, not surprisingly, are similar to the binary reimprisonment variable results presented in Table 6.5. Being unemployed, black, Hispanic, having drug problems and victimizing strangers results in less time to reimprisonment, while having a job to go to, and being on welfare lengthen the time to reimprisonment. Thus, the two "general" time to failure models are somewhat different in their attribution of importance to the structural variables: the variable days to rearrest is predicted by more structural variables than is the variable days to reimprisonment. Still, the models are <u>generally</u> similar to ones reported earlier for binary and criminal career dependent variables.

Days to rearrest are lessened if the offender's presenting offense is a property offense, he/she has pending charges, or is on probation at the time of the PO. The Wolfgang severity index for the instant offense is associated with longer time to rearrest. Days to reimprisonment results are broadly similar those of days to rearrest for this domain of independent variables.

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[12] A source of possible confusion is the fact that the direction of the signs of "comparable effects" are reversed. Variables which increase the likelihood of a recidivistic event will decrease the time in which it takes that event to occur.

Anamnestic variables show that time to rearrest is shortened by the number of prior adult arrests, number of charges in the past five years, number of prior persons convictions, number of prior weapons convictions, and being off the street in the past two years. Only number of prior adult charge convictions lengthens the time to rearrest. There are only significant effects that shorten the time to reimprisonment: all of those just listed minus number of prior weapons convictions. Thus, no "specific deterrent" effect is observed for days to reimprisonment, but otherwise the results for the anamnestic variables are quite similar across the two time-to-failure dependent variables.

Two delinquent career/onset variables, number of arrests as a juvenile and years since first incarceration were found to be negatively associated with time to failure either by rearrest or by reimprisonment. Years since first incarceration is found to be negatively associated with days to reimprisonment. These same variables were most frequently significant for the binary and criminal career dependent variables as well.

Of the prior CJS/offender action variables, number of prior incarcerations was found to be positively associated with time to rearrest and negatively associated with time to reimprisonment. Two other variables are statistically significant for time to reimprisonment: bad conduct on last probation and having the most recent parole revoked. It is this set of independent variables that perhaps most sharply differentiates the timing of rearrest from the timing of reimprisonment.

All three of the general control variables are found to be predictive of both time-to-failure dependent variables. However, their effects are not as consistent across time periods as was seen for the binary and criminal career

dependent variables.

The results for the interaction terms differ somewhat across the two time-to-failure variables. For days to rearrest, the interaction terms of being black and either on probation at PO or the number of prior adult arrests are positive (i.e., the combination increases the time it takes to recidivate), while being black and number of prior property convictions results in less time to rearrest. For days to reimprisonment, two of these same interaction terms are statistically significant while being black and on probation at PO is not. In addition, for days to reimprisonment, the interaction term of being black and number of charges as a juvenile has a negative effect.

The interaction term for being female and number of Part I offenses is statistically significant for days to rearrest, but not for time to reimprisonment. Age and having prior treatment for drugs/alcohol is positively associated with both time to rearrest and to reimprisonment, as is the interaction term of age and PO of property crime. Age and being unemployed, as well as age and number of charges in the past five years are positively associated with only time to reimprisonment. In general these results broadly correspond to what was reported earlier: various factors make the linear aging effect occur "faster," as it were, in the presence of these other characteristics.

For the effects of presenting offense interaction terms, a violent PO in interaction with having detainers is positively associated days to rearrest. This corresponds with what was observed for other forms of recidivism. The results also indicate that a PO of property crime and number of prior juvenile arrests delays rearrest, but hastens reimprisonment. This latter finding is

not characteristic of the results for other dependent variables. A presenting offense of property in conjunction with both age at first arrest and years since first incarceration is negatively associated with time to reimprisonment, but not related to time to rearrest. Two PO drug interactions (with number of adult convictions and with number of Part I charges) are related to days to rearrest and days to reimprisonment: the first negatively (shorter times to failure) and the second positively (longer times to failure). In summary, the pattern of effects for the interaction terms on days to rearrest is similar to that found for several of the other dependent variables, while those for days to reimprisonment are somewhat different in that some of the effects are in the reverse direction. The results for days to reimprisonment are even quite different from those for the binary reimprisonment variable. Thus the processes for the timing of reimprisonment stand out as being somewhat distinctive to those for all of the other dependent variables, perhaps reflecting the confounding influence of the CJS's response to offender recidivism.

Once again, there are several themes that emerge from the analysis of the time-to-failure variables. We summarize them as follows:

1. There are differences between days to rearrest and days to reimprisonment models in terms of both what explains differences in the timing of recidivism and the direction of variables' effects.

2. The variable "days to rearrest" is predicted by more structural variables than is days to reimprisonment.

3. The type of presenting offense is important for explaining time to failure.

4. There is some evidence of a "specific deterrence" for prior sanctions on the time to rearrest, but not for time to reimprisonment.

5. "Recent chronicity" is important to predicting time to failure, even for a period as long as nine years of follow-up.

6. Evidence of juvenile criminal involvements and age of onset are important to predicting time to failure.

7. Prior CJS intervention effects are weak and not unlike those reported earlier for other dependent variables. Any observed effects are conflicting and difficult to interpret.

8. The general control variables are not as consistently predictive of time to failure as was seen for other forms of recidivism. Nevertheless, these variables remain important for modeling the timing of recidivistic events.

9. Interaction terms again reveal that the effects of race, age, and presenting offense on the timing of recidivism are mitigated by the presence of other characteristics. These results are generally similar to those found earlier for other forms of recidivism.

THE IMPACT OF CONTROLS FOR SAMPLE SELECTION

Appendix B contains all 23 tables discussed so far in this chapter, replicated with the hazard variables included in the models. In general, the consequences for including the hazard variables for the variance partitioning results are substantial, while the effects for the individual regression coefficients are minimal. In the variance partitioning analyses, allowing the hazard variables to enter the regression equations first results in far less explained variance being shared by the domains of independent variables studied here. Controlling for sample selection also generally leads to a slight reduction in the unique variances for each set of variables, relative to the models without hazards.

As for the effects on the coefficients, they are inconsequential. Although there are a few instances across the models where variables are statistically significant in the models with hazards and not in the ones without hazards, they are very few in number. Similarly, some instances can

be seen where variables cease to be significant once the hazard measures are controlled. Perhaps the biggest impact of the sample selection indicators is on the actual magnitudes of the estimated coefficients. However, here too the changes are quite small and do not affect the generalizations drawn in the discussions above.

Our assessment of the impact of sample selection bias on the full range of forms of recidivism thus mirrors the conclusions reached in Chapter Five. The criminal justice system's processing of the case prior to sentencing shares much explanatory power with those variables that can be used to predict recidivism prior to the imposition of any sentence. In this sense, some aspects of recidivism can be explained simply by the nature of the case that appears before the court for sentencing. Generally, from 20 to 25 percent of the explainable variation in any measure of recidivism can be attributed to the impact of the selection hazards. Yet beyond this, few biases appear to be introduced in the estimated effects for individual independent variables if the impact of the hazard controls is ignored.

GENERAL PATTERNS OF EFFECTS

The analyses of this chapter have led to a wide array of results across many different aspects of recidivism. At times, the results have been conflicting and confusing, and this has been confounded by differences seen as a function of the width of the post-sentence observation window. While we have deliberately focussed on the broad similarities across the models for various forms of recidivism, the complexity of the results is still evident. This simply reaffirms our contention that how recidivism is defined, and the period over which it is observed, matters for the substantive conclusions

reached.[13]

At the expense of this complexity, some additional parsimony in the conclusions can be achieved by considering just the direction of a variable's relationship to forms of recidivism. Table 6.24 contains a summary of all the results for the 13 dependent variables discussed in this chapter. A plus sign in this table means that, for two or more of the four windows studied, a variable's effect is statistically significant in a positive direction, while a negative sign means that, in two or more windows, the effect is statistically significant in a negative direction. The absence of any sign means the coefficients met neither of these criteria. The far right column gives a summary count for the number of times an independent variable was statistically significant across all recidivism measures.

Ten of the 62 variables analyzed met the significance criteria for all 13 dependent variables. These are: being black, Hispanic, having pending charges, number of prior adult arrests, number of charges in the past five years, off the street in the past two years, years since first incarceration, coder probation prognosis, product terms of being black and prior adult arrests, as well as of being black and number of property convictions. At the other extreme, two variables have no relationship with any of these forms of recidivism: treated for drugs/alcohol problems, and number of charges as a juvenile -- though both are involved in interactions that do. Another three variables are related to only one of these dependent variables: PO of drug

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[13] That the particular dependent variable-observation window combination influences the conclusions reached is even more evident by looking at the individual coefficients. Doing so finds substantial differences in the magnitudes of effects for the same independent variable across different windows. Similarly, the relative impact of different predictors can vary dramatically across different dependent variables. Our discussion of the results in this chapter has not highlighted such findings.

offense, having detainers at arrest, and number of prior property convictions. The remaining 47 independent variables (75% of them) are predictive of two to 12 of the dependent variables. It seems clear one cannot claim that a single set of predictor variables can explain all the various forms of recidivism studied here. Most of the independent variables predict some forms of recidivism but not others.[14]

Another way of evaluating the similarities of the dependent variables is to use the number of independent variables that predict them. The column marginals in Table 6.24 show the number of "signs" for each dependent variable. As can be seen, the rate variables of rearrest and charges are "explained" by the most independent variables (48 of the 62), while at the other extreme, the reimprisonment and persons offense dependent variables are explained by the least number of independent variables (25 and 29, respectively). In general the rate variables are predicted by more of the independent variables than are the count variables, or the binary or time to failure variables. Recidivism measures based upon persons offenses and reimprisonment rank low in terms of number of significant independent variables. Although these indicators are somewhat crude, (there are complex issues of the degree of dependency among the dependent variables, for example), they do suggest that there are quite different processes involved in the prediction of some forms of recidivism over other forms.[15]

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[14] The variable selection strategy used makes this true by definition.

[15] In part, these column totals reflect the amount of variation that can be explained for these dependent variables: as the percentage of explained variance increases, so too does the number of significant independent variables. However, as there are no restrictions on how much variance is explained by any predictor variable -- one variable could account for most of the variance -- we interpret these results as indicative of different underlying processes. The results from the communality analyses support this interpretation.

A further summarization of the results of this chapter can be achieved by positing the existence of three general forms of recidivism: recidivism for any crime type, recidivism for persons crime, and recidivism resulting in reimprisonment. These three distinctions seem to reoccur in the analyses above. The models for rearrest, total charges, charge convictions, arrest rate and charge rate, as well as days to rearrest, are roughly comparable in terms of which independent variables are significant. We refer to this group as "All Crimes." A "Persons" crimes group can be seen in the models for rearrest for persons crime and total persons crimes. Similarly, the patterns of significant coefficients suggests an "Imprisonment" group can be constructed from the models for reimprisonment and days to reimprisonment.[16]

A small subset of independent variables that consistently predict these three forms of recidivism can be identified through a consideration of the magnitude of the effects. We exclude some variables with relatively small (but consistent) effects in order to highlight what appear to be the major independent variables for these general forms of recidivism. Table 6.25 contains the summary results. Twenty-eight variables are consistently predictive of <u>all</u> versions of one or more of the three forms of recidivism. The "all crimes" predictor variables total 25 in number, a reduction of 20 to 25 variables from the basic model used for the analyses of this chapter. The persons crimes variables are consistently predicted by 17 variables, while the imprisonment variables by 15.

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[16] A repeat of the presenting offense and the summed seriousness dependent variables are excluded from consideration here. Although the models for both of these dependent variables look quite similar to the "all crimes" models, there are sufficient differences so that we cannot view them as similar to the recidivism measures comprising the "all crimes" group.

Variables with rather large effects on the dependent variables, defined as standardized coefficients were equal to or larger than an absolute .05 in value, are starred in Table 6.25.[17] The results show that 13 variables have large effects for the "all crimes" dependent variables, while 12 and 10 have large effects for the persons and imprisonment dependent variables, respectively. All 10 of the variables found to be strong predictors of reimprisonment are also strong predictors of the "all crimes" group. The differences among the strong predictors of "all crimes" versus persons crimes can be accounted for by offense specialization. PO of persons and number of prior persons convictions predict persons recidivism, while PO of property crimes strongly predicts "all crimes" variables, and not persons recidivism variables.

In summary, a subset of "consistent and strong" or "best" independent variables, as those terms are defined above, can be identified for the three general forms of recidivism. This results in a much smaller number of predictor variables being relevant to the explanation of recidivism as more broadly defined. Interestingly, the best predictors of imprisonment are also the best predictors of persons and of all crimes. The importance of specialization in offending is seen in that two of the independent variables (PO of persons and prior persons convictions) are among the "best" predictors of persons recidivism, but not the other forms. Overall though, the more broadly recidivism is conceptualized, the greater the number of independent variables that are needed to explain it. More independent variables meet the criteria of "consistent and strong" for binary, count and rate recidivism

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[17] Coefficients for the binary dependent variables are ignored here since they are not strictly comparable to standardized regression coefficients.

measures that involve all crimes, than do for persons or imprisonment dependent variables.

SUMMARY

The analyses of this chapter have been prospective in that characteristics of the individual, known at the time of sentencing, have been used to model his/her recidivism. In this sense, our focus has been on predicting recidivism prior to including any information about the sanction received as a result of that sentence. Our findings suggest that assessing risk at that point of the CJS intervention is quite feasible.

In general, our ability to explain differences in levels of recidivism compares favorably to those reported elsewhere in the literature. The proportions of variance explained in these different forms of recidivism reaches a high of about 37% for the adjusted arrest rate measured over the full nine-year window. While this still leaves almost two thirds of the variance unaccounted for, few studies can claim this level of success.[18] Clearly, much about the recidivistic behavior of convicted offenders can be predicted.

However, these predictive successes must be put in the context of several other observations. First, as seem in the results for the binomial recidivism measures, many of the individual-level predictions of our models are incorrect. For some forms of recidivism, measured over some windows, levels of false positives and false negatives can be quite high. The exact consequences of these predictions for individual-level risk assessment is the

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[18] There is that expectation that a knowledge of changes in independent variables (e.g., an increase or decrease in drug use; change in employment status) would improve the explanatory power of models such as these.

focus of Chapter Nine.

Second, how well our models fare is intimately tied to the length of the post-sentence observation window. No matter how recidivism is measured, more variation is explained the longer we look for recidivistic behavior. In part, this may be due to the fact that as the mix of recidivists changes over time, the independent variables do a better job of differentiating recidivists from non-recidivists -- this would be true even if the mechanism for generating recidivism were purely random. Yet this may also be true because our models treat recidivism as another form of criminal behavior. Over time, the factors leading individuals to commit crime (e.g., lack of education and employment opportunities) take their toll, and we are better able to explain differences in recidivism. No matter which explanation is used, however, it is clear that long-run prediction of recidivism is easier than short-run prediction.

Third, the success of our models is also tied to how the phenomenon of "recidivism" is defined. The general model developed in the last chapter does not equally predict all forms of recidivism measures, and some variables are able to predict some measures of recidivism but not others. Even at the most general level, three substantively different types of recidivism have emerged from the analysis. We have seen that recidivism measured by all types of crimes, the subset of persons crimes, and (re)imprisonment constitute substantively different phenomena. Moreover, for these dependent variables, there are considerable differences in our ability to explain their variation and in which specific independent variables can predict these forms of recidivism. This diversity suggests that policy geared toward only one aspect of recidivism could be misdirected.

These cautions are counterbalanced by some empirical regularities that are relatively robust across all the forms of recidivism that have been studied in this chapter: there is a subset of variables that are found to be consistent and relatively strong predictors of "recidivism" defined quite generally. Anamnestic variables, measuring all previous arrests and convictions, are consistently the strongest predictors of recidivism of all types. Measures of the offender's race, ethnicity, and gender are also strongly related to subsequent recidivism. However, the effects for these variables are frequently mitigated by other characteristics, most notably the anamnestic measures.

The general control variables of age, being born out of state, and coder probation prognosis also do well in predicting all forms of recidivism, though arguably less well for the time-to-failure models. That older offenders are less likely to recidivate is not surprising, though we are stuck by how many other factors are related to recidivism after age effects have been controlled. Our interpretation of the robust impact of being born out of state centers around the quality of our data. It is likely that most criminal activities outside New Jersey are not appearing in our data. This is consistent with the results seen for having detainers and the interaction between having detainers and a presenting offense for a violent crime. Also of interest is the fact that a statistically significant effect exists for the subjective assessment variable across all dependent variables. This holds despite all of the objective control variables in the general model. Thus there is some predictive utility for subjective indicators.

Our analyses also allow for an evaluation of the relative strengths of the different domains of independent variables to predict recidivism. The

findings of the communality analyses demonstrate that each of the six sets of independent variables uniquely contributes to the explanation of differences in recidivism. The possible exception here are the variables from the prior CJS/offender action domain which are not uniquely predictive of most forms of recidivism. Generally, across all models, except for reimprisonment, the structural and anamnestic variables, as individual sets, account for the highest proportions of unique variance. When pairs of domains are considered, we find it is the anamnestic variables in conjunction with other sets that contribute uniquely to the explained variance of all the dependent variables studied here.

Once we get beyond these general statements, however, the predictive utility of these domains becomes more a function of how recidivism is defined. More structural variables are consistently predictive of rearrest, as well as of general count and rate variables, than of persons-based recidivism or reimprisonment. For days to reimprisonment, variables from the juvenile/onset variables also contribute relatively high explanatory power.

While it has been possible to uniquely attribute explained variance to these domains, there is a considerable communality shared among the predictor variables. This overlap in explanatory ability is so great that no one set, or pairs of sets, can uniquely account for much of the variance explained in any dependent variable. Instead, roughly half of the explained variance for most dependent variables, at any observation window, is common to three or more domains. Moreover, if controls for sample selection bias are introduced, the communality attributable to the sets of independent variables is reduced substantially. We find, then, that much of the power to predict post-sentence recidivism is shared by the independent variables used here.

The results of this chapter are also informative as to how our independent variables impact on more general criminal behavior. Recent criminal behavior is predictive of subsequent criminal activity, or, alternatively stated, recent inactivity (other than the presenting offense) is predictive of future inactivity. Many of our measures of "chronicity" (e.g., incarcerated in the past two years, charges in the past five years) are significantly related to all the forms of crime tapped by our recidivism measures. As well, drug involvement is predictive of most forms of criminal behavior as measured by our recidivism indicators.

Two general patterns in our results lessen the import of "recent chronicity" on subsequent offending. First, the variables capturing prior CJS/offender actions are somewhat inconsistent in their effects. Recent revocations of parole and offender behavior while on probation tend not to predict recidivism. Second, juvenile offending (delinquency), as well as age of onset processes, are important for all forms of recidivism. While these may be taken as measures of chronicity for the younger offenders in our sample, such an interpretation is less appropriate for older offenders. Consequently, how long the offender has been criminally active, and not just recent activity, is important for understanding recidivism.

We also find some evidence to support specialization in offending as our findings point to some specialization patterns in recidivism. Distinctions can be made between persons crimes, all crimes (which are driven primarily by property offenses), and, possibly, drug offenses. All of these are indicative of different types of offenders. The existence of specialized offenders is supported by the results for our interaction terms where we found that the basic model for recidivism was mediated by a presenting offense of either

property or drug crimes.

Some results also point to positive outcomes for criminal behavior. We have seen evidence of possible "specific deterrence" effects for the number of prior charge convictions. For most of our dependent variables, as the number of previous convictions increases, the probability or level of recidivism decreases: accumulated CJS interventions of this form seem to impact on an individual's criminal behavior. We did not find support, however, for specific deterrence in that, generally, the number of previous incarcerations had little relationship to recidivism, once the other independent variables were controlled. This latter finding suggests that imprisonment, per se, may not deter recidivism. This will be investigated further in the next chapters.

We have also seen some evidence for a tapering off in criminal activities. Some types of offenders seems to desist from crime or slacken their pace of offending more quickly than others. In particular, individuals with frequent property offenses (possibly those early in their adult years) and who show evidence of prior drug use seem to age out of crime more rapidly than other types of offenders.

To summarize, much can be predicted about the subsequent criminal behavior of this sample prior to any consideration of the sentence received for the presenting offense conviction. Yet these results are also characterized by a high degree of complexity. For every factor that decreases the likelihood of recidivism, there is another that increases the chances of further criminal involvement. As we become more specific about what is meant by "recidivism," the ability to explain differences in it, and the variables that are best able to predict it, become specific to the definition of recidivism used.

These complexities in the explanation and prediction of recidivism serve as the baseline for evaluating the impact of the sentence imposed. It is clear from the results of this chapter that some offenders are more at risk for recidivism than others and it is against this expectation of risk that we evaluate the effectiveness of sentences. In the next chapter we detail the measurement of the sentence received. In Chapter Eight we assess the impact of these sentences on subsequent recidivism.

Table 6.1
Attribution of Explained Variance for Probability of Rearrest

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.173)	(.244)	(.261)	(.256)
Percent R2 Unattributed	55.11	55.24	54.81	52.45
	(.095)	(.135)	(.143)	(.134)
Social Variables	5.91	7.83	9.25	10.82
	(.010)	(.019)	(.024)	(.028)
Presenting Offense	3.32	2.83	2.41	2.20
	(.006)	(.007)	(.006)	(.006)
Anamnestic Variables	8.90	9.09	8.33	9.20
	(.015)	(.022)	(.022)	(.024)
Delinquent Career/Onset	3.64	3.20	2.97	2.36
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.63	.29	.40	.80
	(.001)	(.001)	(.001)	(.002)
General Controls	4.10	5.18	5.52	6.27
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.22	.42	.38
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	1.23	1.50	1.62	1.85
	(.002)	(.004)	(.004)	(.005)
Social/Delinquent Career	1.80	2.08	2.44	2.44
	(.003)	(.005)	(.006)	(.006)
Social/Prior CJS Actions	.23	.02	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.30	2.05	1.83	1.91
	(.004)	(.005)	(.005)	(.005)
Presenting Offense/Delinquency	.00*	.11	.18	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.23	.40	.39	.46
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.30	.43	.42	.45
	(.001)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.74	1.79	1.53	1.41
	(.005)	(.004)	(.004)	(.004)
Anamnestic/Prior CJS Actions	4.26	1.87	1.14	.03
	(.007)	(.005)	(.003)	(.000)
Anamnestic/General Controls	3.21	2.91	2.30	2.10
	(.006)	(.007)	(.006)	(.005)
Delinquency/Prior CJS Actions	.74	.13	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	1.95	3.46	5.24	6.65
	(.003)	(.008)	(.014)	(.017)
Prior CJS Actions/Controls	.12	.20	.19	.25
	(.000)	(.000)	(.001)	(.001)
All Interactions	4.09	2.62	2.38	2.40
	(.007)	(.006)	(.006)	(.006)

* Partitioned variance is negative due to suppressor effect

Table 6.2
Attribution of Explained Variance for Probability of Rearrest for Persons Crime

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.083)	(.132)	(.157)	(.177)
Percent R2 Unattributed	46.84	48.86	49.60	49.93
	(.039)	(.065)	(.078)	(.089)
Social Variables	6.79	8.22	8.99	9.86
	(.006)	(.011)	(.014)	(.017)
Presenting Offense	1.59	2.03	2.23	1.64
	(.001)	(.003)	(.003)	(.003)
Anamnestic Variables	18.27	14.30	12.08	11.09
	(.015)	(.019)	(.019)	(.020)
Delinquent Career/Onset	5.63	5.00	4.89	4.69
	(.005)	(.007)	(.008)	(.008)
Prior CJS/Offender Actions	1.19	.62	.52	.62
	(.001)	(.001)	(.001)	(.001)
General Controls	1.65	3.71	4.30	4.92
	(.001)	(.005)	(.007)	(.009)
Social/Presenting Offense	1.20	1.63	1.91	1.70
	(.001)	(.002)	(.003)	(.003)
Social/Anamnestic Variables	2.22	2.21	2.46	2.32
	(.002)	(.003)	(.004)	(.004)
Social/Delinquent Career	2.27	2.59	2.95	3.08
	(.002)	(.003)	(.005)	(.005)
Social/Prior CJS Actions	.32	.17	.15	.11
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.93	.90	.98	.88
	(.001)	(.001)	(.002)	(.002)
Presenting Offense/Delinquency	.34	.56	.62	.54
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Prior CJS	.00*	.00*	.04	.17
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.00*	.00*	.03	.12
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	4.57	3.13	2.71	2.47
	(.004)	(.004)	(.004)	(.004)
Anamnestic/Prior CJS Actions	2.52	2.23	1.70	1.40
	(.002)	(.003)	(.003)	(.002)
Anamnestic/General Controls	1.48	1.91	1.49	1.56
	(.001)	(.003)	(.002)	(.003)
Delinquency/Prior CJS Actions	1.26	.69	.61	.38
	(.001)	(.001)	(.001)	(.001)
Delinquency/General Controls	1.56	2.15	2.90	3.97
	(.001)	(.003)	(.005)	(.007)
Prior CJS Actions/Controls	.00*	.04	.07	.13
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.21	4.84	3.84	3.89
	(.004)	(.006)	(.006)	(.007)

* Partitioned variance is negative due to suppressor effect

Table 6.3
Attribution of Explained Variance for Probability
of Repeating Presenting Offense

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.066)	(.103)	(.129)	(.142)
Percent R2 Unattributed	52.72	51.36	49.91	46.80
	(.035)	(.053)	(.065)	(.067)
Social Variables	8.26	8.80	9.52	10.46
	(.005)	(.009)	(.012)	(.015)
Presenting Offense	8.90	9.80	11.50	15.29
	(.006)	(.010)	(.015)	(.022)
Anamnestic Variables	8.76	10.48	10.47	9.63
	(.006)	(.011)	(.014)	(.014)
Delinquent Career/Onset	2.48	3.59	2.70	2.57
	(.002)	(.004)	(.003)	(.004)
Prior CJS/Offender Actions	1.88	.69	.63	.86
	(.001)	(.001)	(.001)	(.001)
General Controls	2.35	2.38	3.24	3.48
	(.002)	(.002)	(.004)	(.005)
Social/Presenting Offense	.00*	.00*	.00*	.02
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	1.12	1.76	1.77	1.63
	(.001)	(.002)	(.002)	(.002)
Social/Delinquent Career	1.10	1.46	1.39	1.50
	(.001)	(.001)	(.002)	(.002)
Social/Prior CJS Actions	.27	.14	.09	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.13	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.62	1.46	1.54	1.36
	(.001)	(.002)	(.002)	(.002)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.08	.09	.18
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.80	.98	1.15	1.43
	(.001)	(.001)	(.001)	(.002)
Anamnestic/Delinquent Career	3.36	2.83	2.12	1.73
	(.002)	(.003)	(.003)	(.002)
Anamnestic/Prior CJS Actions	4.22	2.10	2.39	1.26
	(.003)	(.002)	(.003)	(.002)
Anamnestic/General Controls	2.86	2.61	2.75	2.54
	(.002)	(.003)	(.004)	(.004)
Delinquency/Prior CJS Actions	.64	.26	.12	.00*
	(.000)	(.000)	(.000)	(.000)
Delinquency/General Controls	.47	.32	.44	.72
	(.000)	(.000)	(.001)	(.001)
Prior CJS Actions/Controls	.00*	.09	.05	.09
	(.000)	(.000)	(.000)	(.000)
All Interactions	13.20	8.97	8.08	6.74
	(.009)	(.009)	(.010)	(.010)

* Partitioned variance is negative due to suppressor effect

Table 6.4
Attribution of Explained Variance for Probability of Reimprisonment

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.119)	(.239)	(.279)	(.289)
Percent R2 Unattributed	50.79	56.27	57.48	58.02
	(.060)	(.135)	(.160)	(.168)
Social Variables	3.48	3.71	4.46	5.84
	(.004)	(.009)	(.012)	(.017)
Presenting Offense	1.23	1.78	1.45	1.59
	(.001)	(.004)	(.004)	(.005)
Anamnestic Variables	10.24	7.21	7.17	6.16
	(.012)	(.017)	(.020)	(.018)
Delinquent Career/Onset	6.91	6.99	7.58	7.86
	(.008)	(.017)	(.021)	(.023)
Prior CJS/Offender Actions	5.16	1.98	.75	.34
	(.006)	(.005)	(.002)	(.001)
General Controls	.56	1.32	1.60	2.20
	(.001)	(.003)	(.004)	(.006)
Social/Presenting Offense	.15	.58	.60	.52
	(.000)	(.001)	(.002)	(.001)
Social/Anamnestic Variables	.87	.93	1.05	1.05
	(.001)	(.002)	(.003)	(.003)
Social/Delinquent Career	.77	1.66	2.02	2.49
	(.001)	(.004)	(.006)	(.007)
Social/Prior CJS Actions	.58	.47	.33	.27
	(.001)	(.001)	(.001)	(.001)
Social/General Controls	.46	.56	.51	.26
	(.001)	(.001)	(.001)	(.001)
Presenting Offense/Anamnestic	1.40	1.38	1.15	1.22
	(.002)	(.003)	(.003)	(.004)
Presenting Offense/Delinquency	.34	.67	.66	.53
	(.000)	(.002)	(.002)	(.002)
Presenting Offense/Prior CJS	.00*	.05	.03	.06
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.04	.10	.22	.29
	(.000)	(.000)	(.001)	(.001)
Anamnestic/Delinquent Career	5.52	4.48	3.98	3.56
	(.007)	(.011)	(.011)	(.010)
Anamnestic/Prior CJS Actions	5.90	4.85	4.36	2.95
	(.007)	(.012)	(.012)	(.009)
Anamnestic/General Controls	3.03	3.01	2.90	2.86
	(.004)	(.007)	(.008)	(.008)
Delinquency/Prior CJS Actions	2.46	1.80	1.42	1.08
	(.003)	(.004)	(.004)	(.003)
Delinquency/General Controls	.15	.14	.27	.80
	(.000)	(.000)	(.001)	(.002)
Prior CJS Actions/Controls	.03	.04	.04	.04
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.19	4.35	3.70	3.67
	(.006)	(.010)	(.010)	(.011)

* Partitioned variance is negative due to suppressor effect

Table 6.5

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	.0165	.0250*	.0304*	.0269*	.0016	.0084	.0170	.0117
Has job after sentence	-.0360***	-.0240*	-.0221	-.0180	-.0168**	-.0108	-.0153	-.0192
Offender is on welfare	-.0213*	.0102	.0108	.0158	.0045	.0133	.0156	.0173
Offender is Black	.0782***	.1414***	.1557***	.1689***	.0473***	.1020***	.1133***	.1438***
Offender is Hispanic	.0698***	.1340***	.1482***	.1539***	.0391**	.0947***	.1094***	.1389***
Offender is female	-.0487**	-.0627**	-.0686**	-.0859***	-.0433***	-.0827***	-.1086***	-.1199***
Lives in urban area	.0214*	.0389**	.0445***	.0387**	.0180**	.0268**	.0343***	.0340**
Years at current address	-.0013**	-.0014*	-.0014*	-.0013*	-.0004	-.0005	-.0005	-.0003
History of drug problems	.0017	.0229	.0201	.0244	.0038	-.0079	-.0077	-.0154
Treated for drugs/alch.	-.0136	-.0016	.0088	.0260	-.0119	-.0128	-.0092	-.0131
Has needle marks	.0024	.0276	.0733**	.0801**	-.0073	-.0202	-.0172	-.0095
Not a school drop out	-.0311***	-.0472***	-.0504***	-.0530***	-.0018	-.0106	-.0267**	-.0268**
Doesn't live with family	-.0054	-.0151	-.0537***	-.0424**	-.0067	-.0103	-.0079	-.0128
Commited PO with group	-.0235**	-.0398***	-.0411***	-.0430***	-.0073	-.0138	-.0100	-.0138
Victim was a stranger	.0246*	.0401**	.0274	.0317*	.0030	.0003	.0051	.0065
Presenting Offense								
PO property crime	.0228	.0386*	.0463**	.0466**	-.0008	-.0124	-.0115	.0025
PO crime against person	-.0260	.0210	.0355	.0483*	.0244*	.0538***	.0709***	.0753***
PO drug offense	-.0080	.0058	.0106	.0169	-.0064	-.0186	-.0236	-.0169
PO Wolfgang severity	-.0017*	-.0027***	-.0021*	-.0026**	-.0004	-.0008	-.0012	-.0009
Has detainers at arrest	-.0203	-.0135	-.0163	-.0219	.0016	-.0106	-.0110	-.0067
Has pending charges	.0566***	.0868***	.0912***	.0769***	.0131*	.0154	.0245*	.0254*
On probation at PO	.0560***	.0711***	.0661***	.0699***	.0029	.0198	.0308*	.0434**
Anamnestic Theory								
N prior adult arrests	.0140***	.0296***	.0363***	.0369***	.0078***	.0142***	.0175***	.0189***
N prior adult conviction	.0024	.0007	.0043	.0055	-.0033	-.0011	-.0014	-.0003
N prior adult chg. conv.	-.0081**	-.0133***	-.0161***	-.0158***	-.0017	-.0032	-.0069*	-.0085**
N charges past 5 years	.0063***	.0083***	.0088***	.0116***	.0019*	.0024	.0022	.0032*
N prior Part 1 charges	.0094*	.0071	.0072	.0071	.0024	-.0039	.0006	.0004
N prior property conv.	-.0002	.0009	-.0021	-.0018	-.0036*	-.0019	-.0024	-.0009
N prior persons conv.	.0026	.0049	.0108	.0061	.0082***	.0173***	.0232***	.0254***
N prior weapons conv.	.0320**	.0278	.0340	.0534**	.0199**	.0212*	.0379***	.0533***
Off street last 2 years	.0485***	.0972***	.1038***	.1005***	.0113	.0325***	.0342**	.0397***
Delinquent Career/Onset								
N arrests as juvenile	.0134***	.0146***	.0152**	.0191***	.0042**	.0072**	.0085**	.0091**
N charges as juvenile	.0053	.0167	.0191	.0297	.0075	.0006	.0100	.0165
Age at first arrest	-.0013	.0008	-.0003	-.0012	-.0020	-.0002	-.0008	-.0031
Yrs since first incarc.	.0011***	.0016***	.0015***	.0009*	.0005*	.0009***	.0010**	.0009**
Yrs since first drug use	-.0005	-.0009*	-.0007	-.0006	-.0002	-.0005*	-.0005	-.0008*
Prior CJS-Offender Action								
N prior incarcerations	-.0004	-.0078	-.0117*	-.0235***	.0013	.0015	.0014	-.0012
N prior parole revokes	-.0014	.0039	-.0071	-.0038	-.0044	-.0164*	-.0184*	-.0248*
Bad conduct last probat.	.0232*	.0251	.0136	-.0030	.0111	.0140	.0194	.0245*
Recent parole revoked	.0269	-.0158	-.0084	.0077	.0143	.0218	.0203	.0296
General Control Variables								
Offender age at sent.	-.0042**	-.0065***	-.0075***	-.0059***	-.0004	-.0037***	-.0053***	-.0053***
Off. born out of state	-.0458***	-.0881***	-.1063***	-.1252***	-.0141**	-.0271***	-.0406***	-.0588***
Coder prob. prognosis	-.0005**	-.0010***	-.0008***	-.0007**	-.0001	-.0005**	-.0004*	-.0005*

* p<.05 ** p<.01 *** p<.001

Table 6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0410*	-.0693**	-.0841**	-.0974***	-.0178	-.0374*	-.0483**	-.0551**
Black x prior adult arrs	-.0081**	-.0102*	-.0176***	-.0151**	-.0055***	-.0090***	-.0126***	-.0144***
Black x n prior prop cnv	.0064*	.0097*	.0202***	.0227***	.0034*	.0043	.0071**	.0073*
Black x n charges as juv	.0125	.0199	.0328	.0165	-.0113	.0036	-.0074	-.0139
Female x Part 1 charges	.0177	.0386*	.0584**	.0439*	-.0003	-.0003	.0085	.0182
Off. age x drug problem	-.0052*	-.0039	-.0029	-.0022	-.0015	-.0037*	-.0051*	-.0054*
Off. age x prior trtment	-.0039	-.0086**	-.0099***	-.0100***	-.0023	-.0029	-.0029	-.0045
Off. age x unemployed	-.0018	-.0019	-.0015	-.0015	-.0012	-.0012	-.0009	-.0014
Off. age x PO property	-.0055**	-.0059**	-.0038	-.0056**	-.0024*	-.0025	-.0024	-.0047*
Off. age x chg pst 5 yrs	.0003*	.0000	.0000	.0001	-.0001	-.0002	-.0002	-.0002
PO viol x has detainers	-.0512	-.1326**	-.1458**	-.1754***	-.0275	-.0464*	-.0641*	-.0931**
PO prop x n adl.arrests	.0032	.0019	-.0027	.0061	.0018	.0022	.0047	.0099**
PO prop x prior prop con	-.0051	-.0042	-.0014	-.0077	-.0005	-.0026	-.0055	-.0084**
PO prop x n juv. arrests	-.0105**	-.0106*	-.0120*	-.0158*	-.0038*	-.0080**	-.0071*	-.0028
PO prop x age at 1st arr	.0033	-.0020	-.0034	-.0019	-.0011	-.0072**	-.0070**	-.0023
PO prop x yrs. 1st incar	.0000	.0000	.0002	.0005	-.0002	-.0003	-.0002	-.0005
PO drugs x n adl. convs.	.0131**	.0150**	.0153*	.0153*	.0017	.0079*	.0099*	.0123*
PO drugs x Part 1 chgs.	-.0243***	-.0205*	-.0163	-.0236*	.0000	-.0096	-.0064	-.0108
PO drugs x last par. rev	.0603	.1272	.0592	.0748	-.0181	-.0166	-.0049	.0123
Constant	-.1721***	-.1807***	-.1049***	-.0197	-.0688***	-.1330***	-.1652***	-.1882***
Mean of Dep. Var.	.234	.399	.480	.549	.072	.148	.201	.256
N of cases	11,749	11,749	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.148	.211	.228	.228	.069	.115	.138	.157
Model Classifications:								
% Correct Predictions	78.89	73.08	72.48	72.98	92.78	85.29	81.15	77.00
% False Positives	4.17	9.99	12.02	13.52	.15	1.23	2.72	5.29
% False Negatives	16.94	16.93	15.50	13.50	7.08	13.49	16.14	17.70
RIOC	.487	.496	.481	.455	.296	.435	.491	.461

* p<.05 ** p<.01 *** p<.001

Table 6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	-.0064	.0091	.0125	.0190	.0003	.0173	.0144	.0131
Has job after sentence	-.0128*	-.0175*	-.0121	-.0071	-.0208***	-.0362***	-.0401***	-.0376***
Offender is on welfare	-.0116	-.0154	-.0129	-.0029	-.0082	-.0194*	-.0263*	-.0187
Offender is Black	.0158*	.0357***	.0620***	.0870***	.0047	.0700***	.1080***	.1309***
Offender is Hispanic	.0171	.0425**	.0769***	.0949***	.0036	.0595**	.0828***	.1237***
Offender is female	-.0161	-.0468***	-.0580***	-.0745***	-.0052	-.0142	-.0289	-.0552**
Lives in urban area	.0064	.0195*	.0196*	.0320**	-.0069	-.0073	.0014	.0201
Years at current address	-.0005	-.0009*	-.0010*	-.0012*	-.0001	-.0009*	-.0008	-.0005
History of drug problems	.0014	.0079	-.0037	.0002	.0174*	.0245*	.0461**	.0466**
Treated for drugs/alch.	-.0057	.0009	.0177	.0144	.0053	.0178	.0186	.0187
Has needle marks	.0284**	.0420**	.0365*	.0380*	.0058	.0156	.0165	.0307
Not a school drop out	-.0182***	-.0165*	-.0210*	-.0169	.0021	-.0115	-.0151	-.0178
Doesn't live with family	-.0069	-.0149	-.0325***	-.0374***	.0044	-.0018	-.0035	-.0104
Commited PO with group	-.0161***	-.0250***	-.0336***	-.0370***	-.0091*	-.0022	-.0006	-.0105
Victim was a stranger	.0111	.0257**	.0285**	.0320**	.0115*	.0234*	.0349**	.0375**
Presenting Offense								
PO property crime	.0396***	.0486***	.0673***	.0550***	.0094	.0015	.0196	.0359*
PO crime against person	.0310**	.0542***	.0642***	.0616***	-.0006	.0142	.0289	.0321
PO drug offense	.0765***	.1500***	.2104***	.2518***	-.0108	-.0328*	-.0112	.0020
PO Wolfgang severity	-.0021***	-.0031***	-.0037***	-.0046***	.0000	.0003	.0004	-.0005
Has detainers at arrest	-.0046	-.0023	-.0061	-.0096	-.0058	.0087	.0104	.0142
Has pending charges	.0171*	.0292**	.0463***	.0558***	.0236***	.0604***	.0674***	.0690***
On probation at PO	.0220*	.0291*	.0337*	.0460**	.0184*	.0496***	.0652***	.0551**
Anamnestic Theory								
N prior adult arrests	.0082***	.0157***	.0195***	.0208***	.0025	.0055*	.0082*	.0102**
N prior adult conviction	.0003	.0001	.0003	.0051	-.0014	.0017	.0021	.0025
N prior adult chg. conv.	-.0035*	-.0066**	-.0077**	-.0072*	.0007	-.0015	-.0009	-.0023
N charges past 5 years	.0019*	.0024*	.0031*	.0031*	.0021**	.0033**	.0034*	.0045**
N prior Part 1 charges	.0060**	.0051	.0050	.0042	-.0001	.0014	.0022	.0010
N prior property conv.	-.0065***	-.0068**	-.0087**	-.0094**	-.0008	.0010	.0020	.0030
N prior persons conv.	.0005	-.0004	-.0003	.0004	.0033*	.0044	.0085*	.0120**
N prior weapons conv.	.0031	.0152	.0018	.0041	-.0002	.0111	.0070	.0111
Off street last 2 years	.0133	.0341***	.0448***	.0568***	.0204**	.0593***	.0914***	.0902***
Delinquent Career/Onset								
N arrests as juvenile	.0025	.0042	.0033	.0063*	.0028*	.0048*	.0045	.0049
N charges as juvenile	.0018	-.0025	-.0025	.0067	.0042	.0137	.0245	.0185
Age at first arrest	-.0012	.0013	.0006	.0003	.0011	.0019	.0037*	.0024
Yrs since first incarc.	.0000	.0006*	.0007*	.0004	.0021***	.0030***	.0036***	.0037***
Yrs since first drug use	-.0001	-.0002	-.0002	-.0001	.0001	.0001	.0000	-.0004
Prior CJS-Offender Action								
N prior incarcerations	.0020	-.0017	-.0006	-.0066	.0028	.0024	.0021	.0015
N prior parole revokes	-.0050	-.0124	-.0215*	-.0238*	.0045	-.0016	.0006	-.0021
Bad conduct last probat.	.0008	.0061	.0033	-.0002	.0019	.0089	.0058	.0059
Recent parole revoked	.0267*	.0245	.0416*	.0426	.0147	.0434**	.0182	.0182
General Control Variables								
Offender age at sent.	-.0003	-.0017	-.0019	-.0023	-.0016	-.0035**	-.0041**	-.0042**
Off. born out of state	-.0170**	-.0243**	-.0437***	-.0470***	-.0006	-.0167*	-.0247**	-.0400***
Coder prob. prognosis	-.0003*	-.0006***	-.0006***	-.0008***	-.0003**	-.0008***	-.0010***	-.0011***

* p<.05 ** p<.01 *** p<.001

Table 6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0213*	-.0345*	-.0372*	-.0431*	-.0150	-.0452**	-.0560***	-.0366
Black x prior adult arrs	-.0038*	-.0066**	-.0081**	-.0099***	-.0050***	-.0078***	-.0104***	-.0142***
Black x n prior prop cnv	.0032*	.0051*	.0054*	.0064*	.0040***	.0045*	.0043	.0048
Black x n charges as juv	.0022	.0070	.0129	-.0005	-.0024	.0096	.0222	.0326
Female x Part 1 charges	.0045	.0044	.0070	.0041	.0021	.0110	.0159	.0230*
Off. age x drug problem	-.0017	-.0009	-.0035	-.0025	.0001	-.0020	-.0012	.0004
Off. age x prior trtment	-.0015	-.0031	-.0026	-.0046*	-.0022	-.0049*	-.0053*	-.0080***
Off. age x unemployed	-.0018*	-.0003	-.0009	-.0013	-.0003	-.0012	-.0030*	-.0048**
Off. age x PO property	-.0025*	-.0038*	-.0053**	-.0065***	-.0015	-.0062***	-.0069***	-.0079***
Off. age x chg pst 5 yrs	.0001	.0000	.0000	.0000	.0002**	.0003**	.0002	.0002
PO viol x has detainers	-.0262	-.0601*	-.0781**	-.0996**	.0001	-.0159	-.0240	-.0385
PO prop x n adl.arrests	-.0033	-.0041	-.0031	-.0025	.0002	.0034	.0051	.0074*
PO prop x prior prop con	.0058**	.0083**	.0108***	.0115***	-.0004	-.0013	-.0020	-.0035
PO prop x n juv. arrests	.0002	.0016	.0020	.0014	-.0002	.0011	.0028	.0037
PO prop x age at 1st arr	.0038*	.0012	.0025	.0038	.0005	.0019	.0015	.0026
PO prop x yrs. 1st incar	.0007*	.0005	.0006	.0011*	-.0001	.0003	.0002	.0003
PO drugs x n adl. convs.	.0039	.0078*	.0091*	.0082	.0040	.0111**	.0103*	.0079
PO drugs x Part 1 chgs.	-.0126**	-.0111*	-.0017	-.0025	-.0074*	-.0089	-.0117	-.0092
PO drugs x last par. rev	.0218	.0294	-.0063	.0360	.0189	.0218	.0789	.0573
Constant	-.0749***	-.1377***	-.1715***	-.2024***	-.0557***	-.1292***	-.1707***	-.2006***
Mean of Dep. Var.	.079	.153	.199	.246	.057	.137	.186	.230
N of cases	11,714	11,746	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.056	.088	.112	.126	.092	.186	.218	.230
Model Classifications:								
% Correct Predictions	92.12	84.74	80.59	76.85	94.34	87.73	84.98	82.31
% False Positives	.08	.99	2.34	3.74	.20	2.51	3.93	5.50
% False Negatives	7.80	14.27	17.07	19.41	5.46	9.76	11.09	12.19
RIOC	.534	.413	.436	.446	.459	.551	.579	.563

* p<.05 ** p<.01 *** p<.001

Table 6.6
Attribution of Explained Variance for Log of
Total Post-Sentence Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.188)	(.293)	(.331)	(.344)
Percent R2 Unattributed	53.75	55.55	55.61	54.05
	(.101)	(.163)	(.184)	(.186)
Social Variables	4.17	5.61	7.42	9.26
	(.008)	(.016)	(.025)	(.032)
Presenting Offense	3.31	2.57	2.32	2.52
	(.006)	(.008)	(.008)	(.009)
Anamnestic Variables	10.73	10.44	9.67	10.05
	(.020)	(.031)	(.032)	(.035)
Delinquent Career/Onset	3.97	3.99	3.63	3.04
	(.007)	(.012)	(.012)	(.010)
Prior CJS/Offender Actions	.76	.29	.23	.38
	(.001)	(.001)	(.001)	(.001)
General Controls	3.14	4.01	4.41	4.97
	(.006)	(.012)	(.015)	(.017)
Social/Presenting Offense	.00*	.03	.11	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	1.14	1.47	1.70	1.82
	(.002)	(.004)	(.006)	(.006)
Social/Delinquent Career	1.57	1.79	1.98	2.10
	(.003)	(.005)	(.007)	(.007)
Social/Prior CJS Actions	.25	.16	.10	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.62	2.30	2.21	2.34
	(.005)	(.007)	(.007)	(.008)
Presenting Offense/Delinquency	.00*	.08	.10	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.09	.25	.27	.31
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.28	.32	.32	.36
	(.001)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.90	2.75	2.18	1.91
	(.007)	(.008)	(.007)	(.007)
Anamnestic/Prior CJS Actions	4.70	2.88	2.43	1.49
	(.009)	(.008)	(.008)	(.005)
Anamnestic/General Controls	4.05	3.52	3.09	2.91
	(.008)	(.010)	(.010)	(.010)
Delinquency/Prior CJS Actions	.85	.49	.23	.00*
	(.002)	(.001)	(.001)	(.000)
Delinquency/General Controls	1.17	1.75	2.41	3.31
	(.002)	(.005)	(.008)	(.011)
Prior CJS Actions/Controls	.05	.14	.14	.18
	(.000)	(.000)	(.000)	(.001)
All Interactions	5.07	3.91	3.02	3.00
	(.010)	(.011)	(.010)	(.010)

* Partitioned variance is negative due to suppressor effect

Table 6.7
Attribution of Explained Variance for Log of
Total Post-Sentence Convictions

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.148)	(.239)	(.276)	(.288)
Percent R2 Unattributed	53.28	54.78	54.74	53.70
	(.079)	(.131)	(.151)	(.155)
Social Variables	4.52	5.47	6.96	8.40
	(.007)	(.013)	(.019)	(.024)
Presenting Offense	3.74	3.07	2.79	3.32
	(.006)	(.007)	(.008)	(.010)
Anamnestic Variables	8.69	10.28	9.97	9.91
	(.013)	(.025)	(.028)	(.029)
Delinquent Career/Onset	5.65	5.37	4.72	3.58
	(.008)	(.013)	(.013)	(.010)
Prior CJS/Offender Actions	.84	.36	.50	.73
	(.001)	(.001)	(.001)	(.002)
General Controls	3.65	3.91	4.44	5.25
	(.005)	(.009)	(.012)	(.015)
Social/Presenting Offense	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.75	1.36	1.57	1.75
	(.001)	(.003)	(.004)	(.005)
Social/Delinquent Career	1.50	1.82	1.96	1.82
	(.002)	(.004)	(.005)	(.005)
Social/Prior CJS Actions	.24	.05	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.92	2.88	2.68	2.73
	(.004)	(.007)	(.007)	(.008)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.16	.37	.41	.45
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.34	.44	.39	.41
	(.001)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.71	2.75	2.36	1.90
	(.005)	(.007)	(.007)	(.005)
Anamnestic/Prior CJS Actions	4.63	2.48	1.90	1.23
	(.007)	(.006)	(.005)	(.004)
Anamnestic/General Controls	3.80	3.32	3.16	3.12
	(.006)	(.008)	(.009)	(.009)
Delinquency/Prior CJS Actions	1.04	.28	.05	.00*
	(.002)	(.001)	(.000)	(.000)
Delinquency/General Controls	1.33	1.34	1.69	2.49
	(.002)	(.003)	(.005)	(.007)
Prior CJS Actions/Controls	.07	.17	.22	.28
	(.000)	(.000)	(.001)	(.001)
All Interactions	5.90	4.74	3.89	4.14
	(.009)	(.011)	(.011)	(.012)

* Partitioned variance is negative due to suppressor effect

Table 6.8
Attribution of Explained Variance for Log of
Total Post-Sentence Persons Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.082)	(.139)	(.165)	(.187)
Percent R2 Unattributed	44.71	47.68	48.87	48.74
	(.037)	(.066)	(.081)	(.091)
Social Variables	5.72	7.18	7.62	8.75
	(.005)	(.010)	(.013)	(.016)
Presenting Offense	1.61	1.83	1.61	1.11
	(.001)	(.003)	(.003)	(.002)
Anamnestic Variables	20.18	15.80	13.70	13.85
	(.017)	(.022)	(.023)	(.026)
Delinquent Career/Onset	5.41	5.33	5.90	5.37
	(.004)	(.007)	(.010)	(.010)
Prior CJS/Offender Actions	1.43	.75	.36	.37
	(.001)	(.001)	(.001)	(.001)
General Controls	2.06	3.68	4.46	4.69
	(.002)	(.005)	(.007)	(.009)
Social/Presenting Offense	.95	1.43	1.51	1.38
	(.001)	(.002)	(.002)	(.003)
Social/Anamnestic Variables	2.18	2.09	2.36	2.51
	(.002)	(.003)	(.004)	(.005)
Social/Delinquent Career	1.99	2.41	2.75	2.90
	(.002)	(.003)	(.005)	(.005)
Social/Prior CJS Actions	.34	.20	.16	.13
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.91	.80	.85	.75
	(.001)	(.001)	(.001)	(.001)
Presenting Offense/Delinquency	.19	.42	.50	.37
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Prior CJS	.00*	.00*	.00*	.03
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.00*	.00*	.00*	.06
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	5.34	4.07	3.65	3.42
	(.004)	(.006)	(.006)	(.006)
Anamnestic/Prior CJS Actions	3.19	2.32	1.85	1.42
	(.003)	(.003)	(.003)	(.003)
Anamnestic/General Controls	1.98	2.29	1.94	1.99
	(.002)	(.003)	(.003)	(.004)
Delinquency/Prior CJS Actions	1.38	.95	.76	.46
	(.001)	(.001)	(.001)	(.001)
Delinquency/General Controls	1.16	1.71	2.15	2.91
	(.001)	(.002)	(.004)	(.005)
Prior CJS Actions/Controls	.00*	.01	.02	.05
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.47	5.19	4.27	4.63
	(.004)	(.007)	(.007)	(.009)

* Partitioned variance is negative due to suppressor effect

Table 6.9
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Arrest Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.195)	(.318)	(.353)	(.370)
Percent R2 Unattributed	53.88	54.68	55.53	54.94
	(.105)	(.174)	(.196)	(.203)
Social Variables	4.02	4.37	5.93	6.90
	(.008)	(.014)	(.021)	(.026)
Presenting Offense	2.96	1.86	1.50	1.27
	(.006)	(.006)	(.005)	(.005)
Anamnestic Variables	10.57	11.57	10.78	10.91
	(.021)	(.037)	(.038)	(.040)
Delinquent Career/Onset	4.66	4.63	4.19	4.20
	(.009)	(.015)	(.015)	(.016)
Prior CJS/Offender Actions	.86	.50	.41	.49
	(.002)	(.002)	(.001)	(.002)
General Controls	2.97	3.21	3.38	3.36
	(.006)	(.010)	(.012)	(.012)
Social/Presenting Offense	.00*	.17	.26	.24
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	.94	1.22	1.54	1.57
	(.002)	(.004)	(.005)	(.006)
Social/Delinquent Career	1.48	1.57	1.68	1.70
	(.003)	(.005)	(.006)	(.006)
Social/Prior CJS Actions	.26	.22	.24	.22
	(.001)	(.001)	(.001)	(.001)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.63	2.39	2.03	1.87
	(.005)	(.008)	(.007)	(.007)
Presenting Offense/Delinquency	.06	.22	.27	.24
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Prior CJS	.05	.07	.11	.08
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.25	.24	.22	.24
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.89	3.61	3.13	3.21
	(.008)	(.012)	(.011)	(.012)
Anamnestic/Prior CJS Actions	5.34	4.48	4.14	3.89
	(.010)	(.014)	(.015)	(.014)
Anamnestic/General Controls	3.61	3.67	3.41	3.37
	(.007)	(.012)	(.012)	(.012)
Delinquency/Prior CJS Actions	.99	.78	.64	.58
	(.002)	(.002)	(.002)	(.002)
Delinquency/General Controls	.85	.68	.80	.94
	(.002)	(.002)	(.003)	(.003)
Prior CJS Actions/Controls	.03	.05	.07	.08
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.79	3.95	3.89	3.94
	(.009)	(.013)	(.014)	(.015)

* Partitioned variance is negative due to suppressor effect

Table 6.10
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Charge Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.195)	(.310)	(.346)	(.361)
Percent R2 Unattributed	53.65	55.03	55.77	55.33
	(.104)	(.171)	(.193)	(.200)
Social Variables	3.72	4.10	5.34	6.30
	(.007)	(.013)	(.018)	(.023)
Presenting Offense	3.06	2.00	1.65	1.38
	(.006)	(.006)	(.006)	(.005)
Anamnestic Variables	10.87	11.27	10.64	11.03
	(.021)	(.035)	(.037)	(.040)
Delinquent Career/Onset	4.68	4.57	4.24	4.25
	(.009)	(.014)	(.015)	(.015)
Prior CJS/Offender Actions	.95	.61	.43	.47
	(.002)	(.002)	(.002)	(.002)
General Controls	2.53	2.98	3.20	3.16
	(.005)	(.009)	(.011)	(.011)
Social/Presenting Offense	.00*	.11	.20	.18
	(.000)	(.000)	(.001)	(.001)
Social/Anamnestic Variables	1.07	1.27	1.50	1.54
	(.002)	(.004)	(.005)	(.006)
Social/Delinquent Career	1.53	1.55	1.60	1.69
	(.003)	(.005)	(.006)	(.006)
Social/Prior CJS Actions	.26	.26	.26	.21
	(.000)	(.001)	(.001)	(.001)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.58	2.26	2.00	1.88
	(.005)	(.007)	(.007)	(.007)
Presenting Offense/Delinquency	.04	.21	.25	.20
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Prior CJS	.02	.06	.08	.05
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.25	.24	.22	.24
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	4.31	3.72	3.21	3.26
	(.008)	(.012)	(.011)	(.012)
Anamnestic/Prior CJS Actions	4.93	4.25	4.26	3.84
	(.010)	(.013)	(.015)	(.014)
Anamnestic/General Controls	3.89	3.82	3.55	3.48
	(.008)	(.012)	(.012)	(.013)
Delinquency/Prior CJS Actions	1.01	.90	.74	.58
	(.002)	(.003)	(.003)	(.002)
Delinquency/General Controls	.90	.87	.98	1.09
	(.002)	(.003)	(.003)	(.004)
Prior CJS Actions/Controls	.02	.05	.05	.06
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.04	4.38	3.94	3.89
	(.010)	(.014)	(.014)	(.014)

* Partitioned variance is negative due to suppressor effect

Table 6.11
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Persons Charge Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.088)	(.147)	(.173)	(.194)
Percent R2 Unattributed	44.42	47.62	48.02	48.43
	(.039)	(.070)	(.083)	(.094)
Social Variables	4.91	5.33	5.28	5.32
	(.004)	(.008)	(.009)	(.010)
Presenting Offense	1.47	1.76	1.70	1.44
	(.001)	(.003)	(.003)	(.003)
Anamnestic Variables	18.92	14.51	13.65	14.05
	(.017)	(.021)	(.024)	(.027)
Delinquent Career/Onset	6.91	6.68	6.95	7.09
	(.006)	(.010)	(.012)	(.014)
Prior CJS/Offender Actions	1.90	1.64	1.52	1.24
	(.002)	(.002)	(.003)	(.002)
General Controls	1.75	2.69	3.24	2.88
	(.002)	(.004)	(.006)	(.006)
Social/Presenting Offense	.84	1.02	1.26	1.24
	(.001)	(.001)	(.002)	(.002)
Social/Anamnestic Variables	1.83	1.66	1.84	1.89
	(.002)	(.002)	(.003)	(.004)
Social/Delinquent Career	1.96	2.11	2.11	2.19
	(.002)	(.003)	(.004)	(.004)
Social/Prior CJS Actions	.32	.30	.29	.29
	(.000)	(.000)	(.001)	(.001)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.85	.73	.89	.88
	(.001)	(.001)	(.002)	(.002)
Presenting Offense/Delinquency	.26	.41	.48	.43
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Prior CJS	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.02	.00*	.00*	.07
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	6.34	5.69	5.23	5.47
	(.006)	(.008)	(.009)	(.011)
Anamnestic/Prior CJS Actions	3.32	3.22	3.20	2.83
	(.003)	(.005)	(.006)	(.006)
Anamnestic/General Controls	2.07	2.68	2.47	2.61
	(.002)	(.004)	(.004)	(.005)
Delinquency/Prior CJS Actions	1.60	1.58	1.58	1.21
	(.001)	(.002)	(.003)	(.002)
Delinquency/General Controls	.92	.98	.97	.99
	(.001)	(.001)	(.002)	(.002)
Prior CJS Actions/Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.47	5.72	5.00	5.44
	(.004)	(.008)	(.009)	(.011)

* Partitioned variance is negative due to suppressor effect

Table 6.12
Attribution of Explained Variance for
Summed Seriousness of All Post-Sentence Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.116)	(.202)	(.235)	(.242)
Percent R2 Unattributed	52.78	55.19	55.47	53.98
	(.061)	(.111)	(.131)	(.130)
Social Variables	4.04	5.17	6.73	8.68
	(.005)	(.010)	(.016)	(.021)
Presenting Offense	1.93	1.13	1.08	1.39
	(.002)	(.002)	(.003)	(.003)
Anamnestic Variables	12.64	12.10	11.30	12.24
	(.015)	(.024)	(.027)	(.030)
Delinquent Career/Onset	4.63	4.58	4.68	3.82
	(.005)	(.009)	(.011)	(.009)
Prior CJS/Offender Actions	1.10	.41	.15	.25
	(.001)	(.001)	(.000)	(.001)
General Controls	2.55	3.67	3.75	3.79
	(.003)	(.007)	(.009)	(.009)
Social/Presenting Offense	.00*	.22	.13	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	1.16	1.60	1.97	2.18
	(.001)	(.003)	(.005)	(.005)
Social/Delinquent Career	1.62	1.81	1.97	2.01
	(.002)	(.004)	(.005)	(.005)
Social/Prior CJS Actions	.26	.24	.17	.09
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.25	1.23	1.24	1.45
	(.001)	(.002)	(.003)	(.004)
Presenting Offense/Delinquency	.05	.17	.16	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.02	.06	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.23	.20	.17	.24
	(.000)	(.000)	(.000)	(.001)
Anamnestic/Delinquent Career	4.95	3.81	3.32	3.07
	(.006)	(.008)	(.008)	(.007)
Anamnestic/Prior CJS Actions	5.05	3.42	2.85	1.97
	(.006)	(.007)	(.007)	(.005)
Anamnestic/General Controls	3.89	3.45	3.24	3.24
	(.004)	(.007)	(.008)	(.008)
Delinquency/Prior CJS Actions	1.28	.90	.54	.22
	(.001)	(.002)	(.001)	(.001)
Delinquency/General Controls	1.00	1.01	1.27	1.81
	(.001)	(.002)	(.003)	(.004)
Prior CJS Actions/Controls	.01	.05	.08	.12
	(.000)	(.000)	(.000)	(.000)
All Interactions	6.46	5.81	5.02	5.10
	(.007)	(.012)	(.012)	(.012)

* Partitioned variance is negative due to suppressor effect

Table 6.13

Regression Coefficients for Log of Total Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0271*	(.0233)	.0393**	(.0239)	.0470**	(.0248)	.0445*	(.0208)
Has job after sentence	-.0419***	(-.0374)	-.0434**	(-.0275)	-.0413*	(-.0227)	-.0392*	(-.0191)
Offender is on welfare	-.0317**	(-.0236)	-.0245	(-.0129)	-.0216	(-.0099)	-.0036	(-.0014)
Offender is Black	.0635***	(.0492)	.1449***	(.0823)	.2015***	(.0998)	.2780***	(.1233)
Offender is Hispanic	.0512*	(.0239)	.1421***	(.0469)	.1928***	(.0553)	.2709***	(.0688)
Offender is female	-.0425*	(-.0242)	-.0856***	(-.0345)	-.1056***	(-.0369)	-.1410***	(-.0437)
Lives in urban area	.0172	(.0152)	.0463**	(.0291)	.0673***	(.0367)	.0788***	(.0381)
Years at current address	-.0010*	(-.0188)	-.0014*	(-.0181)	-.0015*	(-.0168)	-.0013	(-.0131)
History of drug problems	.0024	(.0018)	.0299	(.0162)	.0505*	(.0238)	.0549*	(.0229)
Treated for drugs/alch.	-.0199	(-.0135)	.0060	(.0029)	.0427	(.0178)	.0747**	(.0277)
Has needle marks	.0173	(.0079)	.0604*	(.0195)	.1079***	(.0303)	.1273***	(.0317)
Not a school drop out	-.0247*	(-.0214)	-.0412**	(-.0254)	-.0558***	(-.0298)	-.0638***	(-.0302)
Doesn't live with family	-.0050	(-.0036)	-.0121	(-.0063)	-.0647***	(-.0293)	-.0654***	(-.0262)
Commited PO with group	-.0240*	(-.0215)	-.0288*	(-.0183)	-.0263	(-.0145)	-.0351*	(-.0171)
Victim was a stranger	.0144	(.0114)	.0494**	(.0277)	.0553**	(.0269)	.0731***	(.0315)
Presenting Offense								
PO property crime	.0320*	(.0282)	.0640***	(.0399)	.0832***	(.0451)	.0995***	(.0478)
PO crime against person	-.0200	(-.0198)	.0089	(-.0039)	.0129	(-.0021)	.0085	(-.0051)
PO drug offense	-.0004	(.0022)	-.0012	(.0072)	.0047	(.0082)	.0227	(.0159)
PO Wolfgang severity	-.0016*	(-.0222)	-.0029**	(-.0286)	-.0030**	(-.0256)	-.0047***	(-.0359)
Has detainers at arrest	-.0035	(-.0120)	.0181	(-.0102)	.0166	(-.0102)	-.0065	(-.0175)
Has pending charges	.0717***	(.0514)	.1182***	(.0600)	.1373***	(.0605)	.1476***	(.0576)
On probation at PO	.0522**	(.0244)	.0791***	(.0240)	.0979***	(.0246)	.1160***	(.0248)
Anamnestic Theory								
N prior adult arrests	.0191***	(.1188)	.0408***	(.2043)	.0493***	(.2114)	.0543***	(.2193)
N prior adult conviction	-.0077*	(-.0185)	-.0084	(-.0060)	-.0003	(.0250)	.0037	(.0389)
N prior adult chg. conv.	-.0103**	(-.0735)	-.0187***	(-.0939)	-.0236***	(-.1033)	-.0235***	(-.0910)
N charges past 5 years	.0125***	(.1131)	.0152***	(.0975)	.0162***	(.0899)	.0195***	(.0962)
N prior Part 1 charges	.0210***	(.0570)	.0103	(.0067)	.0116	(.0220)	.0074	(.0129)
N prior property conv.	-.0003	(.0112)	.0059	(.0385)	.0045	(.0347)	.0064	(.0277)
N prior persons conv.	-.0025	(-.0069)	-.0015	(-.0029)	.0017	(.0029)	.0016	(.0024)
N prior weapons conv.	.0355*	(.0221)	.0518**	(.0228)	.0387	(.0148)	.0675**	(.0229)
Off street last 2 years	.0793***	(.0646)	.1610***	(.0929)	.1880***	(.0942)	.2049***	(.0910)
Delinquent Career/Onset								
N arrests as juvenile	.0179***	(.0715)	.0269***	(.0724)	.0284***	(.0645)	.0343***	(.0678)
N charges as juvenile	-.0089	(.0053)	-.0087	(.0136)	.0044	(.0211)	.0106	(.0195)
Age at first arrest	.0017	(.0491)	.0049**	(.0527)	.0049*	(.0446)	.0033	(.0281)
Yrs since first incarc.	.0008*	(.0561)	.0023***	(.0824)	.0031***	(.0902)	.0029***	(.0789)
Yrs since first drug use	-.0007	(-.0161)	-.0012*	(-.0199)	-.0013*	(-.0197)	-.0012	(-.0150)
Prior CJS-Offender Action								
N prior incarcerations	.0052	(.0205)	-.0004	(-.0011)	-.0074	(-.0179)	-.0232**	(-.0498)
N prior parole revokes	.0086	(.0073)	-.0019	(-.0012)	-.0138	(-.0072)	-.0195	(-.0090)
Bad conduct last probat.	.0301*	(.0210)	.0568**	(.0281)	.0516*	(.0222)	.0348	(.0133)
Recent parole revoked	.0582*	(.0300)	.0052	(.0190)	.0000	(.0133)	.0222	(.0196)
General Control Variables								
Offender age at sent.	-.0034**	(-.1596)	-.0082***	(-.1987)	-.0112***	(-.2094)	-.0128***	(-.2087)
Off. born out of state	-.0470***	(-.0420)	-.1013***	(-.0642)	-.1382***	(-.0760)	-.1815***	(-.0884)
Coder prob. prognosis	-.0004	(-.0189)	-.0012***	(-.0414)	-.0012***	(-.0385)	-.0015***	(-.0402)

* p<.05 **p<.01 ***p,.001

Table 6.13 (continued)

Regression Coefficients for Log of Total Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0413	(-.0147)	-.0728*	(-.0183)	-.0959**	(-.0209)	-.1197**	(-.0232)
Black x prior adult arrs	-.0137***	(-.0523)	-.0169***	(-.0455)	-.0206***	(-.0482)	-.0238***	(-.0494)
Black x n prior prop cnv	.0137***	(.0545)	.0152***	(.0428)	.0183***	(.0447)	.0191***	(.0413)
Black x n charges as juv	.0331	(.0154)	.0663**	(.0219)	.0731**	(.0210)	.0627*	(.0159)
Female x Part 1 charges	.0190	(.0247)	.0285*	(.0263)	.0465**	(.0372)	.0594**	(.0422)
Off. age x drug problem	-.0055**	(-.0393)	-.0073**	(-.0368)	-.0084**	(-.0365)	-.0072*	(-.0280)
Off. age x prior trtment	-.0077**	(-.0485)	-.0128***	(-.0571)	-.0128***	(-.0495)	-.0161***	(-.0551)
Off. age x unemployed	-.0028*	(-.0223)	-.0041**	(-.0232)	-.0043*	(-.0209)	-.0040*	(-.0173)
Off. age x PO property	-.0063***	(-.0514)	-.0080***	(-.0465)	-.0087***	(-.0438)	-.0106***	(-.0470)
Off. age x chg pst 5 yrs	-.0003*	(-.0269)	-.0006**	(-.0371)	-.0006**	(-.0332)	-.0005*	(-.0240)
PO viol x has detainers	-.1193**	(-.0210)	-.2562***	(-.0319)	-.2736***	(-.0296)	-.3352***	(-.0321)
PO prop x n adl.arrests	.0058	(.0220)	.0102	(.0272)	.0109	(.0253)	.0210**	(.0431)
PO prop x prior prop con	-.0106**	(-.0417)	-.0141**	(-.0392)	-.0135*	(-.0325)	-.0206***	(-.0441)
PO prop x n juv. arrests	-.0051	(-.0113)	-.0104	(-.0164)	-.0127*	(-.0173)	-.0165*	(-.0200)
PO prop x age at 1st arr	.0046*	(.0307)	.0013	(.0061)	.0011	(.0044)	.0012	(.0043)
PO prop x yrs. 1st incar	.0015**	(.0295)	.0013*	(.0188)	.0013	(.0164)	.0015	(.0169)
PO drugs x n adl. convs.	.0199***	(.0524)	.0298***	(.0554)	.0292***	(.0472)	.0330***	(.0473)
PO drugs x Part 1 chgs.	-.0396***	(-.0690)	-.0479***	(-.0591)	-.0346**	(-.0371)	-.0355*	(-.0337)
PO drugs x last par. rev	.0557	(.0100)	.2481**	(.0314)	.2190*	(.0241)	.2678*	(.0261)
Constant	.2353***	(-.0092)	.4118***	(-.0112)	.5516***	(-.0077)	.7062***	(-.0023)
R squared	.188		.293		.331		.344	
Adjusted R squared	.184		.289		.328		.340	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.14

Regression Coefficients for Log of Total Post-Sentence Convictions
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0110	(.0175)	.0190*	(.0197)	.0271**	(.0236)	.0248*	(.0190)
Has job after sentence	-.0217***	(-.0357)	-.0281**	(-.0304)	-.0310**	(-.0282)	-.0294*	(-.0235)
Offender is on welfare	-.0147*	(-.0202)	-.0105	(-.0094)	-.0073	(-.0055)	.0045	(.0030)
Offender is Black	.0278***	(.0399)	.0730***	(.0713)	.1072***	(.0877)	.1483***	(.1069)
Offender is Hispanic	.0181	(.0156)	.0494**	(.0279)	.0700***	(.0332)	.0920***	(.0383)
Offender is female	-.0037	(-.0039)	-.0183	(-.0126)	-.0223	(-.0129)	-.0353	(-.0179)
Lives in urban area	.0063	(.0103)	.0232**	(.0249)	.0354***	(.0320)	.0394***	(.0312)
Years at current address	-.0004	(-.0144)	-.0003	(-.0077)	-.0004	(-.0079)	-.0005	(-.0078)
History of drug problems	-.0056	(-.0079)	.0026	(.0024)	.0172	(.0134)	.0208	(.0143)
Treated for drugs/alch.	-.0021	(-.0027)	.0132	(.0108)	.0203	(.0140)	.0512**	(.0311)
Has needle marks	.0059	(.0050)	.0398*	(.0221)	.0612**	(.0285)	.0656**	(.0268)
Not a school drop out	-.0098	(-.0158)	-.0189*	(-.0199)	-.0294**	(-.0260)	-.0329**	(-.0255)
Doesn't live with family	.0010	(.0014)	-.0059	(-.0053)	-.0317**	(-.0237)	-.0346**	(-.0227)
Commited PO with group	-.0245***	(-.0405)	-.0265***	(-.0287)	-.0258**	(-.0235)	-.0274**	(-.0219)
Victim was a stranger	.0164*	(.0240)	.0295**	(.0283)	.0399***	(.0321)	.0582***	(.0411)
Presenting Offense								
PO property crime	.0148*	(.0241)	.0466***	(.0498)	.0504***	(.0451)	.0606***	(.0477)
PO crime against person	-.0112	(-.0178)	-.0027	(-.0080)	-.0067	(-.0101)	-.0201	(-.0186)
PO drug offense	.0034	(.0109)	.0197	(.0267)	.0199	(.0235)	.0222	(.0247)
PO Wolfgang severity	-.0012**	(-.0319)	-.0018**	(-.0301)	-.0025***	(-.0351)	-.0032***	(-.0399)
Has detainers at arrest	-.0136	(-.0169)	-.0037	(-.0126)	-.0120	(-.0153)	-.0300	(-.0227)
Has pending charges	.0246***	(.0325)	.0513***	(.0445)	.0671***	(.0490)	.0783***	(.0501)
On probation at PO	.0309***	(.0309)	.0480***	(.0285)	.0581***	(.0257)	.0651***	(.0221)
Anamnestic Theory								
N prior adult arrests	.0065**	(.0781)	.0197***	(.1831)	.0250***	(.1908)	.0293***	(.1993)
N prior adult conviction	-.0003	(.0265)	.0013	(.0385)	.0077*	(.0703)	.0100**	(.0761)
N prior adult chg. conv.	-.0020	(-.0268)	-.0065*	(-.0562)	-.0105**	(-.0761)	-.0120***	(-.0763)
N charges past 5 years	.0048***	(.0803)	.0073***	(.0798)	.0095***	(.0877)	.0113***	(.0909)
N prior Part 1 charges	.0098***	(.0467)	.0072	(.0293)	.0097*	(.0479)	.0086	(.0405)
N prior property conv.	.0001	(.0057)	.0005	(.0065)	-.0029	(-.0085)	-.0021	(-.0076)
N prior persons conv.	-.0075**	(-.0375)	-.0115**	(-.0378)	-.0112**	(-.0307)	-.0123*	(-.0296)
N prior weapons conv.	.0027	(.0031)	.0103	(.0077)	-.0018	(-.0011)	-.0013	(-.0007)
Off street last 2 years	.0379***	(.0569)	.0876***	(.0865)	.1088***	(.0901)	.1214***	(.0883)
Delinquent Career/Onset								
N arrests as juvenile	.0074***	(.0637)	.0138***	(.0741)	.0152***	(.0655)	.0157***	(.0572)
N charges as juvenile	.0052	(.0290)	-.0003	(.0266)	-.0072	(.0235)	-.0135	(.0198)
Age at first arrest	.0010	(.0549)	.0028**	(.0665)	.0035**	(.0646)	.0027*	(.0481)
Yrs since first incarc.	.0003	(.0544)	.0011***	(.0770)	.0017***	(.0897)	.0015***	(.0790)
Yrs since first drug use	-.0001	(-.0056)	-.0007*	(-.0194)	-.0009*	(-.0220)	-.0011**	(-.0236)
Prior CJS-Offender Action								
N prior incarcerations	.0008	(.0058)	-.0058	(-.0276)	-.0127**	(-.0507)	-.0192***	(-.0677)
N prior parole revokes	-.0012	(-.0018)	-.0063	(-.0065)	-.0042	(-.0036)	-.0087	(-.0066)
Bad conduct last probat.	.0140	(.0181)	.0224*	(.0190)	.0281*	(.0200)	.0363*	(.0227)
Recent parole revoked	.0324*	(.0390)	.0013	(.0198)	-.0113	(.0131)	-.0115	(.0172)
General Control Variables								
Offender age at sent.	-.0013*	(-.1449)	-.0040***	(-.1962)	-.0063***	(-.2148)	-.0073***	(-.2170)
Off. born out of state	-.0263***	(-.0433)	-.0459***	(-.0497)	-.0650***	(-.0591)	-.0954***	(-.0761)
Coder prob. prognosis	-.0002	(-.0183)	-.0005**	(-.0291)	-.0006**	(-.0298)	-.0007***	(-.0320)

* p<.05 **p<.01 ***p,.001

Table 6.14 (continued)

Regression Coefficients for Log of Total Post-Sentence Convictions
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0173	(-.0113)	-.0345	(-.0148)	-.0521*	(-.0188)	-.0697**	(-.0221)
Black x prior adult arrs	-.0065**	(-.0456)	-.0088**	(-.0404)	-.0102**	(-.0396)	-.0129***	(-.0437)
Black x n prior prop cnv	.0064***	(.0470)	.0085**	(.0407)	.0113***	(.0454)	.0131***	(.0465)
Black x n charges as juv	.0263*	(.0226)	.0540***	(.0306)	.0724***	(.0344)	.0848***	(.0354)
Female x Part 1 charges	.0113	(.0272)	.0249**	(.0393)	.0328**	(.0434)	.0419***	(.0487)
Off. age x drug problem	-.0041***	(-.0537)	-.0061***	(-.0524)	-.0067***	(-.0483)	-.0076***	(-.0483)
Off. age x prior trtment	-.0026*	(-.0306)	-.0055**	(-.0418)	-.0064**	(-.0410)	-.0075**	(-.0422)
Off. age x unemployed	-.0016*	(-.0228)	-.0029**	(-.0281)	-.0035**	(-.0286)	-.0038**	(-.0271)
Off. age x PO property	-.0034***	(-.0516)	-.0056***	(-.0558)	-.0061***	(-.0507)	-.0070***	(-.0513)
Off. age x chg pst 5 yrs	-.0001	(-.0118)	-.0003*	(-.0266)	-.0004*	(-.0310)	-.0003*	(-.0261)
PO viol x has detainers	-.0360	(-.0117)	-.1007**	(-.0214)	-.1118**	(-.0200)	-.1394**	(-.0219)
PO prop x n adl.arrests	.0047*	(.0325)	.0094**	(.0429)	.0096*	(.0370)	.0132**	(.0445)
PO prop x prior prop con	-.0063**	(-.0456)	-.0086**	(-.0410)	-.0075*	(-.0300)	-.0116**	(-.0409)
PO prop x n juv. arrests	.0005	(.0019)	-.0008	(-.0020)	-.0024	(-.0054)	-.0039	(-.0076)
PO prop x age at 1st arr	.0028*	(.0347)	.0029	(.0237)	.0029	(.0198)	.0031	(.0183)
PO prop x yrs. 1st incar	.0009**	(.0339)	.0013**	(.0303)	.0011*	(.0226)	.0018**	(.0315)
PO drugs x n adl. convs.	.0112***	(.0545)	.0165***	(.0526)	.0148**	(.0396)	.0159**	(.0374)
PO drugs x Part 1 chgs.	-.0209***	(-.0671)	-.0174*	(-.0367)	-.0083	(-.0148)	-.0099	(-.0154)
PO drugs x last par. rev	.0755*	(.0249)	.1596**	(.0345)	.1781**	(.0324)	.2426***	(.0387)
Constant	.1069***	(-.0092)	.1804***	(-.0120)	.2499***	(-.0121)	.3153***	(-.0089)
R squared	.148		.239		.276		.288	
Adjusted R squared	.143		.235		.272		.284	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.15

Regression Coefficients for Log of Total Post-Sentence Persons Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0066	(.0128)	.0156*	(.0207)	.0263**	(.0293)	.0216*	(.0204)
Has job after sentence	-.0143**	(-.0287)	-.0114	(-.0157)	-.0148	(-.0172)	-.0178	(-.0175)
Offender is on welfare	-.0013	(-.0022)	-.0005	(-.0006)	.0013	(.0012)	.0054	(.0044)
Offender is Black	.0244***	(.0436)	.0664***	(.0812)	.0789***	(.0810)	.1120***	(.0989)
Offender is Hispanic	.0186*	(.0195)	.0504***	(.0362)	.0661***	(.0400)	.0991***	(.0509)
Offender is female	-.0338***	(-.0431)	-.0643***	(-.0564)	-.0863***	(-.0638)	-.1081***	(-.0677)
Lives in urban area	.0113*	(.0226)	.0188**	(.0258)	.0270**	(.0312)	.0317**	(.0309)
Years at current address	-.0003	(-.0107)	-.0005	(-.0151)	-.0005	(-.0130)	-.0002	(-.0038)
History of drug problems	.0017	(.0028)	-.0027	(-.0031)	-.0001	(-.0001)	-.0192	(-.0162)
Treated for drugs/alch.	-.0105	(-.0160)	-.0125	(-.0131)	-.0089	(-.0079)	-.0086	(-.0065)
Has needle marks	-.0069	(-.0071)	-.0253	(-.0178)	-.0231	(-.0137)	-.0027	(-.0013)
Not a school drop out	.0010	(.0019)	-.0049	(-.0066)	-.0156	(-.0177)	-.0217*	(-.0208)
Doesn't live with family	-.0037	(-.0061)	-.0026	(-.0029)	-.0076	(-.0073)	-.0076	(-.0062)
Commited PO with group	-.0027	(-.0055)	.0031	(.0042)	-.0026	(-.0031)	-.0040	(-.0039)
Victim was a stranger	.0032	(.0056)	.0052	(.0063)	.0136	(.0140)	.0209	(.0182)
Presenting Offense								
PO property crime	.0024	(.0047)	.0031	(.0042)	.0042	(.0048)	.0078	(.0076)
PO crime against person	.0199*	(.0264)	.0451***	(.0432)	.0570***	(.0459)	.0601***	(.0377)
PO drug offense	.0004	(-.0027)	-.0100	(-.0137)	-.0121	(-.0121)	-.0076	(-.0047)
PO Wolfgang severity	-.0001	(-.0036)	-.0005	(-.0102)	-.0008	(-.0147)	-.0009	(-.0141)
Has detainers at arrest	-.0046	(-.0149)	-.0162	(-.0234)	-.0159	(-.0223)	-.0020	(-.0190)
Has pending charges	.0139*	(.0224)	.0175*	(.0193)	.0255**	(.0238)	.0341**	(.0270)
On probation at PO	-.0069	(-.0199)	.0001	(-.0174)	.0072	(-.0115)	.0229	(-.0010)
Anamnestic Theory								
N prior adult arrests	.0069***	(.0879)	.0133***	(.1266)	.0163***	(.1295)	.0198***	(.1430)
N prior adult conviction	-.0063***	(-.0856)	-.0065**	(-.0537)	-.0056	(-.0361)	-.0055	(-.0245)
N prior adult chg. conv.	-.0010	(-.0165)	-.0024	(-.0265)	-.0063*	(-.0582)	-.0087**	(-.0682)
N charges past 5 years	.0042***	(.0841)	.0064***	(.0890)	.0060***	(.0701)	.0072***	(.0721)
N prior Part 1 charges	.0041	(.0242)	-.0029	(-.0428)	.0023	(-.0023)	.0002	(-.0115)
N prior property conv.	-.0042*	(-.0647)	-.0024	(-.0353)	-.0034	(-.0393)	-.0027	(-.0315)
N prior persons conv.	.0141***	(.0857)	.0250***	(.1039)	.0328***	(.1151)	.0397***	(.1181)
N prior weapons conv.	.0220**	(.0307)	.0210*	(.0202)	.0332**	(.0268)	.0556***	(.0381)
Off street last 2 years	.0163**	(.0298)	.0372***	(.0467)	.0442***	(.0468)	.0614***	(.0551)
Delinquent Career/Onset								
N arrests as juvenile	.0080***	(.0678)	.0111***	(.0667)	.0136***	(.0723)	.0168***	(.0805)
N charges as juvenile	-.0065	(.0033)	-.0174	(.0146)	-.0053	(.0248)	-.0055	(.0223)
Age at first arrest	.0005	(.0179)	.0021*	(.0340)	.0027**	(.0421)	.0018	(.0344)
Yrs since first incarc.	.0002	(.0178)	.0006*	(.0360)	.0008**	(.0475)	.0009**	(.0380)
Yrs since first drug use	-.0002	(-.0087)	-.0005*	(-.0187)	-.0007*	(-.0217)	-.0010**	(-.0255)
Prior CJS-Offender Action								
N prior incarcerations	.0040	(.0355)	.0057	(.0346)	.0051	(.0262)	.0031	(.0135)
N prior parole revokes	-.0049	(-.0093)	-.0221*	(-.0290)	-.0214*	(-.0237)	-.0315**	(-.0295)
Bad conduct last probat.	.0138*	(.0217)	.0238*	(.0257)	.0200	(.0181)	.0234	(.0180)
Recent parole revoked	.0321*	(.0227)	.0321	(.0163)	.0283	(.0152)	.0331	(.0192)
General Control Variables								
Offender age at sent.	-.0010	(-.0730)	-.0034***	(-.1244)	-.0048***	(-.1499)	-.0056***	(-.1680)
Off. born out of state	-.0152**	(-.0305)	-.0306***	(-.0422)	-.0442***	(-.0513)	-.0591***	(-.0582)
Coder prob. prognosis	.0000	(-.0051)	-.0003*	(-.0263)	-.0004*	(-.0234)	-.0004*	(-.0237)

* p<.05 **p<.01 ***p,.001

Table 6.15 (continued)

Regression Coefficients for Log of Total Post-Sentence Persons Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0126	(-.0101)	-.0363*	(-.0199)	-.0446*	(-.0206)	-.0553*	(-.0216)
Black x prior adult arrs	-.0045**	(-.0382)	-.0076**	(-.0448)	-.0115***	(-.0567)	-.0142***	(-.0597)
Black x n prior prop cnv	.0025	(.0225)	.0041	(.0251)	.0065*	(.0334)	.0070*	(.0305)
Black x n charges as juv	.0184*	(.0193)	.0626***	(.0451)	.0586***	(.0355)	.0618***	(.0318)
Female x Part 1 charges	-.0075	(-.0220)	-.0081	(-.0163)	-.0045	(-.0076)	.0041	(.0059)
Off. age x drug problem	-.0007	(-.0111)	-.0016	(-.0172)	-.0028	(-.0256)	-.0040*	(-.0314)
Off. age x prior trtment	-.0014	(-.0194)	-.0021	(-.0208)	-.0026	(-.0214)	-.0048*	(-.0332)
Off. age x unemployed	-.0011	(-.0191)	-.0015	(-.0182)	-.0020*	(-.0207)	-.0021*	(-.0184)
Off. age x PO property	-.0005	(-.0093)	-.0005	(-.0060)	-.0008	(-.0086)	-.0025	(-.0223)
Off. age x chg pst 5 yrs	-.0002*	(-.0351)	-.0004***	(-.0494)	-.0004**	(-.0432)	-.0005**	(-.0437)
PO viol x has detainers	-.0526**	(-.0208)	-.0918**	(-.0249)	-.1162***	(-.0265)	-.1872***	(-.0362)
PO prop x n adl.arrests	.0003	(.0030)	.0019	(.0110)	.0041	(.0201)	.0082*	(.0341)
PO prop x prior prop con	-.0012	(-.0109)	-.0055*	(-.0332)	-.0078**	(-.0398)	-.0097**	(-.0420)
PO prop x n juv. arrests	-.0032	(-.0161)	-.0038	(-.0130)	-.0031	(-.0091)	-.0016	(-.0039)
PO prop x age at 1st arr	.0002	(.0024)	-.0011	(-.0114)	-.0008	(-.0072)	.0012	(.0089)
PO prop x yrs. 1st incar	.0001	(.0043)	.0000	(.0005)	.0002	(.0042)	-.0001	(-.0024)
PO drugs x n adl. convs.	.0007	(.0043)	.0036	(.0144)	.0046	(.0158)	.0080	(.0232)
PO drugs x Part 1 chgs.	-.0027	(-.0106)	-.0124*	(-.0334)	-.0093	(-.0210)	-.0137	(-.0264)
PO drugs x last par. rev	-.0345	(-.0138)	-.0295	(-.0081)	-.0024	(-.0005)	.0353	(.0069)
Constant	.0509***	(-.0094)	.0969***	(-.0131)	.1502***	(-.0087)	.2045***	(-.0076)
R squared	.082		.139		.165		.187	
Adjusted R squared	.078		.134		.161		.183	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.16

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0139	(.0145)	.0168*	(.0206)	.0197**	(.0261)	.0171**	(.0249)
Has job after sentence	-.0401***	(-.0435)	-.0276***	(-.0354)	-.0227***	(-.0313)	-.0193***	(-.0292)
Offender is on welfare	-.0267**	(-.0242)	-.0142	(-.0151)	-.0144*	(-.0166)	-.0100	(-.0126)
Offender is Black	.0516***	(.0459)	.0673***	(.0790)	.0766***	(.0973)	.0809***	(.1167)
Offender is Hispanic	.0430**	(.0244)	.0550***	(.0368)	.0581***	(.0419)	.0593***	(.0469)
Offender is female	-.0207	(-.0143)	-.0260*	(-.0212)	-.0236*	(-.0207)	-.0240**	(-.0232)
Lives in urban area	.0125	(.0135)	.0195**	(.0247)	.0236***	(.0323)	.0229***	(.0344)
Years at current address	-.0008*	(-.0181)	-.0007*	(-.0181)	-.0005	(-.0141)	-.0003	(-.0082)
History of drug problems	.0024	(.0022)	.0112	(.0123)	.0194*	(.0230)	.0146	(.0190)
Treated for drugs/alch.	-.0182	(-.0150)	-.0021	(-.0020)	.0126	(.0133)	.0209**	(.0241)
Has needle marks	.0266	(.0148)	.0297*	(.0195)	.0379**	(.0268)	.0362***	(.0280)
Not a school drop out	-.0184*	(-.0194)	-.0157*	(-.0195)	-.0145*	(-.0195)	-.0087	(-.0129)
Doesn't live with family	-.0088	(-.0079)	-.0138	(-.0146)	-.0207**	(-.0235)	-.0182**	(-.0227)
Commited PO with group	-.0267***	(-.0290)	-.0136*	(-.0175)	-.0087	(-.0121)	-.0089	(-.0135)
Victim was a stranger	.0202*	(.0194)	.0198*	(.0224)	.0205**	(.0251)	.0200**	(.0268)
Presenting Offense								
PO property crime	.0267*	(.0286)	.0299***	(.0378)	.0300***	(.0408)	.0283***	(.0423)
PO crime against person	-.0154	(-.0169)	.0053	(-.0021)	.0093	(.0028)	.0086	(.0046)
PO drug offense	-.0091	(-.0040)	-.0045	(.0014)	-.0057	(-.0009)	-.0004	(.0058)
PO Wolfgang severity	-.0012*	(-.0209)	-.0006	(-.0114)	-.0002	(-.0044)	-.0001	(-.0024)
Has detainers at arrest	-.0176	(-.0161)	.0151	(-.0045)	.0152	(-.0035)	.0034	(-.0086)
Has pending charges	.0525***	(.0457)	.0566***	(.0581)	.0483***	(.0534)	.0435***	(.0528)
On probation at PO	.0546***	(.0298)	.0348**	(.0235)	.0340***	(.0235)	.0242**	(.0198)
Anamnestic Theory								
N prior adult arrests	.0147***	(.1232)	.0177***	(.1940)	.0171***	(.2020)	.0153***	(.2070)
N prior adult conviction	-.0062*	(-.0180)	-.0064**	(-.0218)	-.0032	(-.0009)	-.0027	(-.0031)
N prior adult chg. conv.	-.0046	(-.0399)	-.0075***	(-.0766)	-.0090***	(-.0994)	-.0077***	(-.0929)
N charges past 5 years	.0108***	(.1183)	.0108***	(.1400)	.0090***	(.1255)	.0089***	(.1364)
N prior Part 1 charges	.0129***	(.0314)	.0085**	(.0232)	.0089**	(.0394)	.0094***	(.0510)
N prior property conv.	.0005	(.0041)	.0031	(.0279)	.0024	(.0303)	.0014	(.0144)
N prior persons conv.	-.0014	(-.0047)	-.0022	(-.0087)	.0002	(.0007)	-.0006	(-.0025)
N prior weapons conv.	.0268*	(.0203)	.0202*	(.0180)	.0203*	(.0195)	.0199**	(.0210)
Off street last 2 years	.0649***	(.0642)	.0745***	(.0870)	.0767***	(.0965)	.0701***	(.0968)
Delinquent Career/Onset								
N arrests as juvenile	.0172***	(.0823)	.0152***	(.0849)	.0117***	(.0733)	.0130***	(.0889)
N charges as juvenile	-.0061	(.0113)	-.0028	(.0241)	-.0069	(.0280)	-.0146	(.0214)
Age at first arrest	.0023*	(.0616)	.0033***	(.0810)	.0029***	(.0785)	.0024***	(.0744)
Yrs since first incarc.	.0008**	(.0624)	.0010***	(.0827)	.0011***	(.0849)	.0009***	(.0782)
Yrs since first drug use	-.0004	(-.0127)	-.0004	(-.0151)	-.0004	(-.0137)	-.0004*	(-.0173)
Prior CJS-Offender Action								
N prior incarcerations	.0047	(.0224)	.0034	(.0190)	.0012	(.0074)	-.0011	(-.0075)
N prior parole revokes	.0044	(.0046)	-.0021	(-.0026)	-.0056	(-.0074)	-.0007	(-.0010)
Bad conduct last probat.	.0232*	(.0197)	.0208*	(.0208)	.0214**	(.0231)	.0145*	(.0172)
Recent parole revoked	.0560*	(.0383)	.0467**	(.0422)	.0419**	(.0401)	.0460**	(.0470)
General Control Variables								
Offender age at sent.	-.0029**	(-.1553)	-.0039***	(-.2003)	-.0042***	(-.2132)	-.0037***	(-.2098)
Off. born out of state	-.0337***	(-.0366)	-.0409***	(-.0524)	-.0433***	(-.0598)	-.0418***	(-.0634)
Coder prob. prognosis	-.0002	(-.0142)	-.0004**	(-.0303)	-.0004**	(-.0279)	-.0004***	(-.0317)

* p<.05 **p<.01 ***p,.001

Table 6.16 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0463*	(-.0200)	-.0270	(-.0138)	-.0292*	(-.0160)	-.0181	(-.0109)
Black x prior adult arrs	-.0117***	(-.0540)	-.0092***	(-.0502)	-.0088***	(-.0517)	-.0090***	(-.0583)
Black x n prior prop cnv	.0107***	(.0514)	.0080***	(.0452)	.0087***	(.0533)	.0082***	(.0551)
Black x n charges as juv	.0366*	(.0208)	.0472***	(.0316)	.0597***	(.0431)	.0639***	(.0505)
Female x Part 1 charges	.0204*	(.0322)	.0182**	(.0340)	.0187**	(.0376)	.0157**	(.0347)
Off. age x drug problem	-.0053**	(-.0455)	-.0047***	(-.0476)	-.0042***	(-.0465)	-.0032**	(-.0385)
Off. age x prior trtment	-.0049*	(-.0373)	-.0053***	(-.0474)	-.0047***	(-.0458)	-.0051***	(-.0546)
Off. age x unemployed	-.0018	(-.0174)	-.0022**	(-.0252)	-.0020**	(-.0244)	-.0016**	(-.0217)
Off. age x PO property	-.0050***	(-.0499)	-.0042***	(-.0494)	-.0040***	(-.0500)	-.0037***	(-.0511)
Off. age x chg pst 5 yrs	-.0001	(-.0115)	-.0003**	(-.0358)	-.0004***	(-.0508)	-.0004***	(-.0514)
PO viol x has detainers	-.0632	(-.0135)	-.1125***	(-.0283)	-.1033***	(-.0281)	-.0752***	(-.0224)
PO prop x n adl.arrests	.0089**	(.0407)	.0098***	(.0528)	.0093***	(.0543)	.0112***	(.0718)
PO prop x prior prop con	-.0115***	(-.0550)	-.0101***	(-.0568)	-.0092***	(-.0561)	-.0097***	(-.0643)
PO prop x n juv. arrests	-.0054	(-.0147)	-.0051	(-.0162)	-.0029	(-.0100)	-.0034	(-.0127)
PO prop x age at 1st arr	.0035*	(.0287)	.0021	(.0198)	.0022	(.0223)	.0021	(.0241)
PO prop x yrs. 1st incar	.0012**	(.0298)	.0009**	(.0261)	.0007*	(.0226)	.0006*	(.0219)
PO drugs x n adl. convs.	.0162***	(.0518)	.0167***	(.0629)	.0133***	(.0541)	.0101***	(.0450)
PO drugs x Part 1 chgs.	-.0380***	(-.0804)	-.0280***	(-.0700)	-.0206***	(-.0554)	-.0163***	(-.0483)
PO drugs x last par. rev	.0800	(.0174)	.0986*	(.0252)	.0842*	(.0232)	.0838*	(.0254)
Constant	.2013***	(-.0052)	.1799***	(-.0112)	.1609***	(-.0144)	.1418***	(-.0138)
R squared	.195		.318		.353		.370	
Adjusted R squared	.191		.315		.350		.367	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.17

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0271*	(.0211)	.0284**	(.0247)	.0289**	(.0268)	.0267**	(.0267)
Has job after sentence	-.0496***	(-.0402)	-.0362***	(-.0329)	-.0279**	(-.0270)	-.0251**	(-.0261)
Offender is on welfare	-.0369**	(-.0249)	-.0237*	(-.0179)	-.0244*	(-.0197)	-.0200*	(-.0173)
Offender is Black	.0603***	(.0403)	.0852***	(.0702)	.0993***	(.0869)	.1087***	(.1058)
Offender is Hispanic	.0505*	(.0214)	.0772***	(.0365)	.0821***	(.0414)	.0891***	(.0484)
Offender is female	-.0378	(-.0195)	-.0494**	(-.0285)	-.0432**	(-.0266)	-.0464***	(-.0307)
Lives in urban area	.0191	(.0154)	.0253**	(.0228)	.0302***	(.0290)	.0287***	(.0297)
Years at current address	-.0011*	(-.0187)	-.0011*	(-.0202)	-.0009*	(-.0185)	-.0006	(-.0119)
History of drug problems	.0114	(.0079)	.0236	(.0184)	.0326**	(.0270)	.0244*	(.0217)
Treated for drugs/alch.	-.0236	(-.0146)	.0006	(.0004)	.0230	(.0169)	.0325**	(.0257)
Has needle marks	.0186	(.0077)	.0323	(.0150)	.0435**	(.0215)	.0492**	(.0262)
Not a school drop out	-.0215	(-.0170)	-.0202*	(-.0178)	-.0185*	(-.0174)	-.0128	(-.0129)
Doesn't live with family	-.0114	(-.0076)	-.0132	(-.0098)	-.0298**	(-.0237)	-.0259**	(-.0222)
Commited PO with group	-.0290**	(-.0235)	-.0153	(-.0139)	-.0082	(-.0080)	-.0090	(-.0094)
Victim was a stranger	.0204	(.0146)	.0248*	(.0200)	.0291**	(.0249)	.0294**	(.0271)
Presenting Offense								
PO property crime	.0371*	(.0296)	.0414***	(.0370)	.0426***	(.0407)	.0423***	(.0434)
PO crime against person	-.0197	(-.0177)	.0100	(-.0016)	.0110	(.0001)	.0117	(.0023)
PO drug offense	-.0070	(-.0016)	-.0073	(.0009)	-.0072	(-.0009)	.0037	(.0085)
PO Wolfgang severity	-.0015	(-.0193)	-.0011	(-.0152)	-.0004	(-.0058)	-.0003	(-.0047)
Has detainers at arrest	-.0110	(-.0136)	.0337	(-.0016)	.0329	(-.0001)	.0198	(-.0038)
Has pending charges	.0722***	(.0469)	.0814***	(.0593)	.0737***	(.0572)	.0666***	(.0556)
On probation at PO	.0690***	(.0300)	.0464**	(.0218)	.0458**	(.0199)	.0330*	(.0152)
Anamnestic Theory								
N prior adult arrests	.0207***	(.1150)	.0256***	(.1851)	.0239***	(.1872)	.0203***	(.1832)
N prior adult conviction	-.0116**	(-.0352)	-.0134***	(-.0488)	-.0080**	(-.0230)	-.0062*	(-.0188)
N prior adult chg. conv.	-.0080*	(-.0517)	-.0105***	(-.0759)	-.0113***	(-.0869)	-.0088***	(-.0725)
N charges past 5 years	.0148***	(.1211)	.0140***	(.1280)	.0118***	(.1153)	.0119***	(.1248)
N prior Part 1 charges	.0214***	(.0480)	.0123**	(.0191)	.0122**	(.0316)	.0115**	(.0362)
N prior property conv.	-.0005	(.0089)	.0046	(.0411)	.0040	(.0438)	.0036	(.0356)
N prior persons conv.	-.0017	(-.0042)	-.0017	(-.0047)	.0015	(.0043)	.0019	(.0060)
N prior weapons conv.	.0355*	(.0200)	.0358**	(.0226)	.0334**	(.0225)	.0379***	(.0275)
Off street last 2 years	.0890***	(.0658)	.1068***	(.0884)	.1109***	(.0978)	.1040***	(.0987)
Delinquent Career/Onset								
N arrests as juvenile	.0207***	(.0796)	.0189***	(.0795)	.0147***	(.0682)	.0174***	(.0845)
N charges as juvenile	-.0128	(.0056)	-.0063	(.0188)	-.0028	(.0300)	-.0128	(.0244)
Age at first arrest	.0021	(.0553)	.0038**	(.0699)	.0034**	(.0710)	.0029**	(.0685)
Yrs since first incarc.	.0010**	(.0656)	.0015***	(.0846)	.0015***	(.0870)	.0013***	(.0800)
Yrs since first drug use	-.0008	(-.0171)	-.0008*	(-.0199)	-.0007*	(-.0177)	-.0006*	(-.0171)
Prior CJS-Offender Action								
N prior incarcerations	.0075	(.0269)	.0079	(.0316)	.0053	(.0227)	-.0003	(-.0015)
N prior parole revokes	.0121	(.0094)	-.0003	(-.0002)	-.0091	(-.0084)	-.0026	(-.0026)
Bad conduct last probat.	.0288	(.0183)	.0340**	(.0241)	.0314**	(.0237)	.0175	(.0143)
Recent parole revoked	.0757*	(.0361)	.0622*	(.0411)	.0606**	(.0390)	.0695***	(.0457)
General Control Variables								
Offender age at sent.	-.0031*	(-.1495)	-.0049***	(-.1929)	-.0054***	(-.2043)	-.0049***	(-.2004)
Off. born out of state	-.0429***	(-.0347)	-.0576***	(-.0523)	-.0637***	(-.0617)	-.0596***	(-.0620)
Coder prob. prognosis	-.0004	(-.0166)	-.0007***	(-.0350)	-.0006***	(-.0319)	-.0007***	(-.0382)

* p<.05 **p<.01 ***p,.001

Table 6.17 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0521	(-.0168)	-.0376	(-.0135)	-.0458*	(-.0176)	-.0336	(-.0139)
Black x prior adult arrs	-.0175***	(-.0604)	-.0147***	(-.0570)	-.0133***	(-.0550)	-.0136***	(-.0605)
Black x n prior prop cnv	.0159***	(.0572)	.0122***	(.0493)	.0121***	(.0518)	.0113***	(.0523)
Black x n charges as juv	.0440*	(.0186)	.0594***	(.0281)	.0736***	(.0372)	.0799***	(.0434)
Female x Part 1 charges	.0245*	(.0290)	.0179	(.0236)	.0203*	(.0287)	.0177*	(.0269)
Off. age x drug problem	-.0062**	(-.0400)	-.0056**	(-.0401)	-.0053**	(-.0407)	-.0036*	(-.0298)
Off. age x prior trtment	-.0075**	(-.0425)	-.0088***	(-.0560)	-.0075***	(-.0510)	-.0084***	(-.0614)
Off. age x unemployed	-.0030*	(-.0217)	-.0034**	(-.0275)	-.0029**	(-.0246)	-.0023*	(-.0214)
Off. age x PO property	-.0073***	(-.0539)	-.0062***	(-.0511)	-.0059***	(-.0523)	-.0056***	(-.0535)
Off. age x chg pst 5 yrs	-.0002	(-.0182)	-.0005***	(-.0441)	-.0006***	(-.0544)	-.0005***	(-.0529)
PO viol x has detainers	-.1168*	(-.0186)	-.1881***	(-.0336)	-.1671***	(-.0318)	-.1372***	(-.0281)
PO prop x n adl.arrests	.0084	(.0289)	.0113**	(.0433)	.0108**	(.0443)	.0152***	(.0666)
PO prop x prior prop con	-.0128**	(-.0456)	-.0120***	(-.0477)	-.0102**	(-.0435)	-.0117***	(-.0536)
PO prop x n juv. arrests	-.0032	(-.0065)	-.0037	(-.0084)	-.0019	(-.0046)	-.0032	(-.0083)
PO prop x age at 1st arr	.0058*	(.0353)	.0032	(.0215)	.0035	(.0253)	.0034*	(.0265)
PO prop x yrs. 1st incar	.0018**	(.0330)	.0014**	(.0289)	.0012**	(.0263)	.0009*	(.0221)
PO drugs x n adl. convs.	.0227***	(.0542)	.0239***	(.0639)	.0197***	(.0561)	.0154***	(.0472)
PO drugs x Part 1 chgs.	-.0482***	(-.0761)	-.0416***	(-.0736)	-.0316***	(-.0596)	-.0253***	(-.0513)
PO drugs x last par. rev	.0802	(.0130)	.1441*	(.0262)	.1052*	(.0204)	.1008*	(.0210)
Constant	.2538***	(-.0073)	.2445***	(-.0148)	.2295***	(-.0173)	.2097***	(-.0154)
R squared	.195		.310		.346		.361	
Adjusted R squared	.191		.307		.342		.357	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.18

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0069	(.0119)	.0118*	(.0235)	.0132**	(.0285)	.0124**	(.0303)
Has job after sentence	-.0168**	(-.0301)	-.0115*	(-.0240)	-.0112*	(-.0252)	-.0092*	(-.0236)
Offender is on welfare	-.0013	(-.0020)	-.0028	(-.0048)	-.0043	(-.0080)	-.0055	(-.0118)
Offender is Black	.0255***	(.0402)	.0367***	(.0683)	.0353***	(.0704)	.0348***	(.0788)
Offender is Hispanic	.0192	(.0179)	.0270**	(.0294)	.0267***	(.0314)	.0258***	(.0344)
Offender is female	-.0351***	(-.0400)	-.0350***	(-.0464)	-.0337***	(-.0484)	-.0314***	(-.0510)
Lives in urban area	.0116*	(.0206)	.0098*	(.0203)	.0093*	(.0208)	.0091*	(.0230)
Years at current address	-.0003	(-.0121)	-.0004*	(-.0185)	-.0004*	(-.0188)	-.0002	(-.0118)
History of drug problems	.0040	(.0062)	.0015	(.0027)	.0056	(.0108)	-.0042	(-.0092)
Treated for drugs/alch.	-.0111	(-.0152)	-.0060	(-.0096)	-.0057	(-.0098)	-.0010	(-.0019)
Has needle marks	-.0110	(-.0101)	-.0206*	(-.0219)	-.0174*	(-.0201)	-.0048	(-.0062)
Not a school drop out	.0015	(.0026)	-.0017	(-.0034)	-.0051	(-.0112)	-.0042	(-.0104)
Doesn't live with family	-.0053	(-.0079)	-.0051	(-.0088)	-.0049	(-.0092)	-.0038	(-.0079)
Commited PO with group	-.0035	(-.0063)	.0012	(.0024)	.0009	(.0020)	.0009	(.0022)
Victim was a stranger	.0033	(.0052)	.0014	(.0026)	.0064	(.0128)	.0057	(.0129)
Presenting Offense								
PO property crime	.0037	(.0065)	.0037	(.0077)	.0033	(.0073)	.0045	(.0114)
PO crime against person	.0220*	(.0271)	.0268***	(.0392)	.0251***	(.0400)	.0211***	(.0379)
PO drug offense	.0014	(-.0026)	-.0038	(-.0121)	-.0053	(-.0149)	-.0012	(-.0063)
PO Wolfgang severity	.0001	(.0018)	.0001	(.0039)	.0003	(.0116)	.0004	(.0139)
Has detainers at arrest	-.0104	(-.0174)	-.0198	(-.0304)	-.0134	(-.0249)	-.0061	(-.0173)
Has pending charges	.0151*	(.0217)	.0133*	(.0222)	.0148**	(.0268)	.0169***	(.0346)
On probation at PO	-.0047	(-.0161)	.0005	(-.0130)	.0032	(-.0102)	.0054	(-.0061)
Anamnestic Theory								
N prior adult arrests	.0068**	(.0745)	.0067***	(.0924)	.0060***	(.0855)	.0039**	(.0682)
N prior adult conviction	-.0076***	(-.0866)	-.0063***	(-.0779)	-.0057***	(-.0779)	-.0050***	(-.0763)
N prior adult chg. conv.	-.0007	(-.0102)	-.0008	(-.0126)	-.0013	(-.0226)	-.0003	(-.0059)
N charges past 5 years	.0050***	(.0899)	.0050***	(.1059)	.0040***	(.0902)	.0038***	(.0983)
N prior Part 1 charges	.0053*	(.0231)	.0011	(-.0173)	.0037*	(.0131)	.0029	(.0072)
N prior property conv.	-.0042*	(-.0575)	-.0020	(-.0377)	-.0023	(-.0446)	-.0015	(-.0393)
N prior persons conv.	.0162***	(.0875)	.0162***	(.1020)	.0172***	(.1174)	.0164***	(.1266)
N prior weapons conv.	.0218**	(.0272)	.0149*	(.0215)	.0194***	(.0304)	.0231***	(.0410)
Off street last 2 years	.0196**	(.0320)	.0247***	(.0468)	.0242***	(.0498)	.0255***	(.0593)
Delinquent Career/Onset								
N arrests as juvenile	.0097***	(.0789)	.0070***	(.0726)	.0068***	(.0764)	.0079***	(.0970)
N charges as juvenile	-.0076	(.0047)	-.0087	(.0254)	-.0014	(.0401)	-.0067	(.0314)
Age at first arrest	.0007	(.0251)	.0014*	(.0450)	.0016**	(.0582)	.0012**	(.0590)
Yrs since first incarc.	.0002	(.0218)	.0003*	(.0359)	.0003*	(.0400)	.0003*	(.0350)
Yrs since first drug use	-.0002	(-.0084)	-.0003	(-.0170)	-.0002	(-.0140)	-.0003*	(-.0178)
Prior CJS-Offender Action								
N prior incarcerations	.0043	(.0336)	.0057**	(.0526)	.0055**	(.0550)	.0036*	(.0402)
N prior parole revokes	-.0073	(-.0125)	-.0169**	(-.0335)	-.0151**	(-.0324)	-.0132**	(-.0320)
Bad conduct last probat.	.0126	(.0176)	.0135*	(.0220)	.0100	(.0177)	.0054	(.0109)
Recent parole revoked	.0530***	(.0345)	.0546***	(.0418)	.0536***	(.0465)	.0498***	(.0519)
General Control Variables								
Offender age at sent.	-.0010	(-.0694)	-.0019***	(-.1138)	-.0020***	(-.1238)	-.0017***	(-.1289)
Off. born out of state	-.0158**	(-.0284)	-.0176***	(-.0367)	-.0209***	(-.0472)	-.0170***	(-.0434)
Coder prob. prognosis	-.0001	(-.0052)	-.0002*	(-.0216)	-.0002*	(-.0212)	-.0002*	(-.0256)

* p<.05 **p<.01 ***p,.001

Table 6.18 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0149	(-.0106)	-.0189	(-.0156)	-.0202*	(-.0181)	-.0192*	(-.0194)
Black x prior adult arrs	-.0058**	(-.0442)	-.0061***	(-.0538)	-.0065***	(-.0628)	-.0064***	(-.0697)
Black x n prior prop cnv	.0037*	(.0296)	.0041**	(.0381)	.0046***	(.0463)	.0042***	(.0480)
Black x n charges as juv	.0231*	(.0216)	.0464***	(.0505)	.0417***	(.0491)	.0419***	(.0558)
Female x Part 1 charges	-.0093	(-.0244)	-.0065	(-.0198)	-.0056	(-.0185)	-.0044	(-.0163)
Off. age x drug problem	-.0007	(-.0106)	-.0007	(-.0116)	-.0008	(-.0138)	-.0007	(-.0142)
Off. age x prior trtment	-.0009	(-.0113)	-.0013	(-.0189)	-.0012	(-.0183)	-.0014	(-.0246)
Off. age x unemployed	-.0013*	(-.0199)	-.0013*	(-.0242)	-.0013**	(-.0255)	-.0011**	(-.0259)
Off. age x PO property	-.0006	(-.0104)	-.0004	(-.0070)	-.0003	(-.0064)	-.0006	(-.0146)
Off. age x chg pst 5 yrs	-.0002**	(-.0379)	-.0003***	(-.0564)	-.0002***	(-.0488)	-.0002***	(-.0489)
PO viol x has detainers	-.0468*	(-.0165)	-.0494**	(-.0202)	-.0455**	(-.0202)	-.0389**	(-.0196)
PO prop x n adl.arrests	.0015	(.0115)	.0028	(.0242)	.0032*	(.0309)	.0049***	(.0526)
PO prop x prior prop con	-.0026	(-.0201)	-.0044**	(-.0407)	-.0048**	(-.0478)	-.0052***	(-.0581)
PO prop x n juv. arrests	-.0023	(-.0104)	-.0002	(-.0013)	-.0003	(-.0019)	-.0010	(-.0064)
PO prop x age at 1st arr	.0005	(.0065)	.0001	(.0016)	.0003	(.0045)	.0008	(.0153)
PO prop x yrs. 1st incar	.0001	(.0060)	.0001	(.0065)	.0002	(.0083)	.0001	(.0043)
PO drugs x n adl. convs.	.0025	(.0133)	.0039	(.0242)	.0031	(.0205)	.0030	(.0229)
PO drugs x Part 1 chgs.	-.0061	(-.0213)	-.0093*	(-.0377)	-.0077*	(-.0338)	-.0074*	(-.0369)
PO drugs x last par. rev	-.0519	(-.0186)	-.0512	(-.0213)	-.0418	(-.0189)	-.0284	(-.0145)
Constant	.0546***	(-.0100)	.0545***	(-.0160)	.0576***	(-.0131)	.0498***	(-.0123)
R squared	.088		.147		.173		.194	
Adjusted R squared	.084		.143		.169		.190	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.19

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.4933*	(.0212)	1.0873**	(.0276)	1.7348***	(.0329)	1.9090**	(.0261)
Has job after sentence	-.6671**	(-.0299)	-.8619*	(-.0228)	-1.0735*	(-.0213)	-1.4261*	(-.0203)
Offender is on welfare	-.2745	(-.0103)	-.4720	(-.0104)	-.6029	(-.0100)	-.5953	(-.0071)
Offender is Black	.9902***	(.0401)	3.0904***	(.0727)	4.6334***	(.0833)	7.9422***	(.1059)
Offender is Hispanic	.7147	(.0167)	2.3391***	(.0322)	3.8376***	(.0396)	7.1061***	(.0528)
Offender is female	-1.1917**	(-.0340)	-2.7978***	(-.0470)	-3.2185***	(-.0405)	-4.9952***	(-.0452)
Lives in urban area	.1898	(.0084)	.7929*	(.0208)	1.6512***	(.0325)	2.2677***	(.0321)
Years at current address	-.0209*	(-.0194)	-.0456**	(-.0249)	-.0563**	(-.0231)	-.0324	(-.0095)
History of drug problems	-.2371	(-.0091)	.0939	(.0021)	.8197	(.0139)	-.1592	(-.0019)
Treated for drugs/alch.	-.4806	(-.0164)	-.1913	(-.0038)	.6487	(.0098)	2.0665*	(.0224)
Has needle marks	.4615	(.0106)	.9731	(.0131)	1.6453	(.0167)	3.0849*	(.0225)
Not a school drop out	-.2219	(-.0097)	-.4294	(-.0110)	-.6083	(-.0117)	-.8287	(-.0115)
Doesn't live with family	-.0692	(-.0026)	-.3020	(-.0066)	-1.3755**	(-.0224)	-1.7832*	(-.0209)
Commited PO with group	-.2581	(-.0116)	.0409	(.0011)	.0096	(.0002)	-.4997	(-.0071)
Victim was a stranger	.0200	(.0008)	.6712	(.0157)	1.0482*	(.0184)	1.9181**	(.0242)
Presenting Offense								
PO property crime	.3638	(.0161)	.8644	(.0225)	1.2590*	(.0246)	1.9552*	(.0274)
PO crime against person	.0281	(-.0056)	.7168	(.0048)	.6179	(-.0007)	.0103	(-.0110)
PO drug offense	.2155	(.0090)	.0518	(.0063)	.1794	(.0088)	1.2411	(.0217)
PO Wolfgang severity	.0053	(.0037)	.0038	(.0016)	-.0065	(-.0020)	-.0469	(-.0104)
Has detainers at arrest	.0627	(-.0110)	.9214	(-.0074)	1.7488	(-.0026)	1.5036	(-.0104)
Has pending charges	1.2396***	(.0446)	2.1249***	(.0450)	2.7811***	(.0442)	3.8704***	(.0442)
On probation at PO	.4475	(.0087)	.8550	(.0029)	1.1361	(.0039)	1.6238	(.0068)
Anamnestic Theory								
N prior adult arrests	.4412***	(.1245)	1.1059***	(.2167)	1.5142***	(.2271)	1.9612***	(.2316)
N prior adult conviction	-.2761***	(-.0658)	-.4281***	(-.0508)	-.3349*	(-.0224)	-.3394	(-.0076)
N prior adult chg. conv.	-.1677*	(-.0598)	-.4576***	(-.0962)	-.7790***	(-.1227)	-.9554***	(-.1082)
N charges past 5 years	.2004***	(.0908)	.4120***	(.1101)	.5131***	(.1027)	.7999***	(.1151)
N prior Part 1 charges	.5033***	(.0741)	.2600	(-.0047)	.5526**	(.0395)	.5369	(.0252)
N prior property conv.	-.1149	(-.0262)	-.0431	(.0021)	-.1658	(-.0087)	-.1847	(-.0192)
N prior persons conv.	.0870	(.0118)	.1351	(.0108)	.2452	(.0147)	.2523	(.0109)
N prior weapons conv.	.4383	(.0137)	1.1510*	(.0212)	.8108	(.0112)	2.1466*	(.0213)
Off street last 2 years	1.1933***	(.0488)	2.9126***	(.0702)	4.3181***	(.0779)	6.3583***	(.0825)
Delinquent Career/Onset								
N arrests as juvenile	.3015***	(.0611)	.5564***	(.0670)	.7273***	(.0618)	.9897***	(.0617)
N charges as juvenile	-.0814	(.0112)	-.5467	(.0131)	-.1897	(.0251)	-.3355	(.0226)
Age at first arrest	.0233	(.0384)	.1197**	(.0543)	.1508**	(.0564)	.1345	(.0424)
Yrs since first incarc.	.0045	(.0342)	.0365**	(.0586)	.0611***	(.0706)	.0697**	(.0563)
Yrs since first drug use	-.0034	(-.0041)	-.0237	(-.0168)	-.0478**	(-.0254)	-.0561*	(-.0214)
Prior CJS-Offender Action								
N prior incarcerations	.1659	(.0328)	.2800	(.0326)	.1455	(.0127)	-.2188	(-.0137)
N prior parole revokes	.2740	(.0117)	-.0252	(-.0006)	-.1519	(-.0029)	-1.0745	(-.0146)
Bad conduct last probat.	.4748	(.0166)	1.2568**	(.0260)	1.2477*	(.0193)	1.4695	(.0164)
Recent parole revoked	.8720	(.0201)	.1221	(.0129)	-.4092	(.0090)	.5355	(.0184)
General Control Variables								
Offender age at sent.	-.0497*	(-.1152)	-.1589***	(-.1572)	-.2479***	(-.1776)	-.3514***	(-.1852)
Off. born out of state	-.6839**	(-.0307)	-1.9997***	(-.0528)	-2.7873***	(-.0552)	-3.8440***	(-.0547)
Coder prob. prognosis	-.0047	(-.0119)	-.0194**	(-.0289)	-.0245**	(-.0273)	-.0368**	(-.0295)

* p<.05 **p<.01 ***p,.001

Table 6.19

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.4668	(-.0083)	-1.6397*	(-.0172)	-2.0399	(-.0160)	-2.3535	(-.0133)
Black x prior adult arrs	-.3127***	(-.0597)	-.5275***	(-.0594)	-.7308***	(-.0617)	-.9214***	(-.0559)
Black x n prior prop cnv	.2728***	(.0543)	.4965***	(.0582)	.6814***	(.0599)	.7979***	(.0504)
Black x n charges as juv	.7303	(.0171)	2.3271***	(.0321)	3.1866***	(.0329)	4.2120***	(.0313)
Female x Part 1 charges	.0160	(.0010)	.1462	(.0056)	1.0159*	(.0293)	1.3622*	(.0283)
Off. age x drug problem	-.0581	(-.0207)	-.1268	(-.0266)	-.2278*	(-.0358)	-.3384**	(-.0383)
Off. age x prior trtment	-.1107*	(-.0350)	-.2121**	(-.0395)	-.2356*	(-.0328)	-.4493**	(-.0451)
Off. age x unemployed	-.0413	(-.0164)	-.0795*	(-.0187)	-.1118*	(-.0197)	-.1393	(-.0176)
Off. age x PO property	-.1002**	(-.0410)	-.1612**	(-.0389)	-.2445***	(-.0442)	-.3430***	(-.0446)
Off. age x chg pst 5 yrs	-.0089**	(-.0374)	-.0207***	(-.0512)	-.0287***	(-.0531)	-.0385***	(-.0513)
PO viol x has detainers	-2.8275**	(-.0249)	-7.5313***	(-.0391)	-10.2020***	(-.0397)	-15.0472***	(-.0421)
PO prop x n adl.arrests	.0470	(.0089)	.1964	(.0219)	.3462	(.0289)	.8243***	(.0495)
PO prop x prior prop con	-.1716*	(-.0338)	-.4060**	(-.0472)	-.4462**	(-.0388)	-.7742***	(-.0484)
PO prop x n juv. arrests	-.0785	(-.0087)	-.1342	(-.0088)	-.2690	(-.0132)	-.3230	(-.0114)
PO prop x age at 1st arr	.0809	(.0272)	.0371	(.0073)	.0885	(.0131)	.1500	(.0160)
PO prop x yrs. 1st incar	.0302**	(.0303)	.0300	(.0178)	.0425	(.0188)	.0420	(.0134)
PO drugs x n adl. convs.	.2755*	(.0364)	.6428***	(.0500)	.7324**	(.0427)	1.1172***	(.0468)
PO drugs x Part 1 chgs.	-.6177***	(-.0540)	-1.3427***	(-.0691)	-.9916*	(-.0383)	-1.3017*	(-.0361)
PO drugs x last par. rev	.3288	(.0029)	3.8772	(.0205)	5.8482*	(.0231)	9.3971*	(.0267)
Constant	3.1731***	(-.0085)	6.4100***	(-.0126)	9.5008***	(-.0098)	13.9662***	(-.0092)
R squared	.116		.202		.235		.242	
Adjusted R squared	.111		.198		.231		.238	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.20
Attribution of Explained Variance for
Days to Rearrest after Sentencing

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.147)	(.240)	(.274)	(.296)
Percent R2 Unattributed	54.11	55.93	55.80	55.06
	(.080)	(.134)	(.153)	(.163)
Social Variables	5.80	6.51	7.30	8.75
	(.009)	(.016)	(.020)	(.026)
Presenting Offense	3.72	2.92	2.80	2.51
	(.005)	(.007)	(.008)	(.007)
Anamnestic Variables	8.44	8.54	8.51	8.63
	(.012)	(.021)	(.023)	(.026)
Delinquent Career/Onset	3.92	3.81	3.43	3.01
	(.006)	(.009)	(.009)	(.009)
Prior CJS/Offender Actions	.73	.34	.25	.38
	(.001)	(.001)	(.001)	(.001)
General Controls	4.62	4.53	4.94	5.41
	(.007)	(.011)	(.014)	(.016)
Social/Presenting Offense	.00*	.05	.19	.32
	(.000)	(.000)	(.001)	(.001)
Social/Anamnestic Variables	1.07	1.32	1.43	1.60
	(.002)	(.003)	(.004)	(.005)
Social/Delinquent Career	1.62	2.02	2.14	2.34
	(.002)	(.005)	(.006)	(.007)
Social/Prior CJS Actions	.30	.16	.08	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.01	2.04	1.99	1.95
	(.003)	(.005)	(.005)	(.006)
Presenting Offense/Delinquency	.00*	.03	.08	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.04	.30	.34	.40
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.33	.37	.39	.41
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.33	2.37	1.99	1.62
	(.005)	(.006)	(.005)	(.005)
Anamnestic/Prior CJS Actions	3.98	2.97	2.34	1.26
	(.006)	(.007)	(.006)	(.004)
Anamnestic/General Controls	3.89	3.05	2.85	2.42
	(.006)	(.007)	(.008)	(.007)
Delinquency/Prior CJS Actions	.97	.50	.27	.00*
	(.001)	(.001)	(.001)	(.000)
Delinquency/General Controls	1.88	2.74	3.52	4.85
	(.003)	(.007)	(.010)	(.014)
Prior CJS Actions/Controls	.05	.15	.17	.20
	(.000)	(.000)	(.000)	(.001)
All Interactions	4.82	3.38	2.84	2.47
	(.007)	(.008)	(.008)	(.007)

* Partitioned variance is negative due to suppressor effect

Table 6.21
Attribution of Explained Variance for
Days to Reimprisonment after Sentencing

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.085)	(.211)	(.267)	(.309)
Percent R2 Unattributed	49.35	54.68	56.22	57.63
	(.042)	(.115)	(.150)	(.178)
Social Variables	4.48	3.51	3.72	4.53
	(.004)	(.007)	(.010)	(.014)
Presenting Offense	1.79	1.85	1.70	1.59
	(.002)	(.004)	(.005)	(.005)
Anamnestic Variables	11.14	8.49	7.69	6.86
	(.009)	(.018)	(.021)	(.021)
Delinquent Career/Onset	7.26	6.47	6.87	7.43
	(.006)	(.014)	(.018)	(.023)
Prior CJS/Offender Actions	4.92	2.84	1.88	.93
	(.004)	(.006)	(.005)	(.003)
General Controls	.60	1.02	1.29	1.67
	(.001)	(.002)	(.003)	(.005)
Social/Presenting Offense	.18	.49	.53	.58
	(.000)	(.001)	(.001)	(.002)
Social/Anamnestic Variables	.93	.88	.93	.99
	(.001)	(.002)	(.002)	(.003)
Social/Delinquent Career	1.01	1.37	1.61	2.03
	(.001)	(.003)	(.004)	(.006)
Social/Prior CJS Actions	.52	.51	.46	.37
	(.000)	(.001)	(.001)	(.001)
Social/General Controls	.53	.59	.58	.46
	(.000)	(.001)	(.002)	(.001)
Presenting Offense/Anamnestic	1.52	1.46	1.32	1.26
	(.001)	(.003)	(.004)	(.004)
Presenting Offense/Delinquency	.53	.55	.62	.63
	(.000)	(.001)	(.002)	(.002)
Presenting Offense/Prior CJS	.13	.10	.05	.03
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.05	.11	.13	.20
	(.000)	(.000)	(.000)	(.001)
Anamnestic/Delinquent Career	4.79	4.68	4.34	3.93
	(.004)	(.010)	(.012)	(.012)
Anamnestic/Prior CJS Actions	5.36	5.20	5.05	4.09
	(.005)	(.011)	(.013)	(.013)
Anamnestic/General Controls	2.71	3.09	3.03	2.92
	(.002)	(.007)	(.008)	(.009)
Delinquency/Prior CJS Actions	2.02	1.87	1.75	1.45
	(.002)	(.004)	(.005)	(.004)
Delinquency/General Controls	.13	.18	.17	.41
	(.000)	(.000)	(.000)	(.001)
Prior CJS Actions/Controls	.06	.05	.04	.03
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.03	4.83	4.24	3.79
	(.004)	(.010)	(.011)	(.012)

* Partitioned variance is negative due to suppressor effect

Table 6.22

Regression Coefficients for Days to Post-Sentence Rearrest
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	-5.4784*	(-.0250)	-24.6049**	(-.0290)	-42.5119**	(-.0280)	-82.4085**	(-.0268)
Has job after sentence	6.4980**	(.0309)	25.9797***	(.0320)	42.2009**	(.0290)	71.2059**	(.0241)
Offender is on welfare	4.6016*	(.0182)	11.3525	(.0116)	9.2242	(.0053)	-6.8575	(-.0019)
Offender is Black	-11.8921***	(-.0523)	-70.0713***	(-.0776)	-150.585***	(-.0932)	-360.420***	(-.1088)
Offender is Hispanic	-11.9893**	(-.0297)	-63.8006***	(-.0409)	-143.898***	(-.0516)	-356.136***	(-.0629)
Offender is female	6.2098	(.0188)	36.5511**	(.0286)	82.6923***	(.0361)	215.4708***	(.0464)
Lives in urban area	-3.2015	(-.0151)	-23.4573**	(-.0287)	-45.2122***	(-.0308)	-96.7828***	(-.0326)
Years at current address	.2570**	(.0253)	1.0708**	(.0273)	1.6786**	(.0239)	3.2764**	(.0230)
History of drug problems	.4960	(.0020)	-5.5337	(-.0058)	-17.4825	(-.0103)	-57.1571	(-.0166)
Treated for drugs/alch.	.4661	(.0017)	1.6083	(.0015)	-4.7678	(-.0025)	-46.0680	(-.0119)
Has needle marks	-2.8060	(-.0068)	-16.6090	(-.0104)	-42.7899	(-.0150)	-120.346*	(-.0208)
Not a school drop out	5.5765**	(.0257)	31.4288***	(.0376)	58.5450***	(.0391)	130.6473***	(.0430)
Doesn't live with family	-1.5667	(-.0061)	1.2314	(.0012)	19.8107	(.0112)	79.1230**	(.0220)
Commited PO with group	6.1088**	(.0290)	23.1933***	(.0286)	44.6744***	(.0307)	85.9956***	(.0292)
Victim was a stranger	-6.3755**	(-.0268)	-28.7507***	(-.0313)	-49.5741***	(-.0302)	-92.5658**	(-.0278)
Presenting Offense								
PO property crime	-4.2938	(-.0201)	-29.1804**	(-.0354)	-58.3393***	(-.0395)	-135.650***	(-.0453)
PO crime against person	4.8964	(.0215)	1.3436	(.0070)	-13.2326	(-.0014)	-72.3808	(-.0136)
PO drug offense	.2268	(-.0048)	-4.7983	(-.0101)	-5.7702	(-.0085)	-20.9562	(-.0098)
PO Wolfgang severity	.3633**	(.0268)	1.5256**	(.0291)	2.8621**	(.0305)	5.8623***	(.0308)
Has detainers at arrest	6.6151	(.0210)	18.9017	(.0221)	33.7799	(.0225)	70.1043	(.0231)
Has pending charges	-12.6956***	(-.0483)	-58.1007***	(-.0573)	-112.249***	(-.0619)	-220.899***	(-.0600)
On probation at PO	-7.2532*	(-.0206)	-41.6763***	(-.0265)	-82.9028***	(-.0283)	-190.281***	(-.0290)
Anamnestic Theory								
N prior adult arrests	-3.2312***	(-.1000)	-17.2996***	(-.1515)	-35.4592***	(-.1743)	-77.3778***	(-.1858)
N prior adult conviction	.9662	(.0137)	.2134	(-.0212)	-1.3108	(-.0291)	-12.7124	(-.0517)
N prior adult chg. conv.	1.7475**	(.0660)	8.3236***	(.0814)	16.4547***	(.0899)	30.7303***	(.0828)
N charges past 5 years	-1.8683***	(-.0897)	-6.4696***	(-.0805)	-10.5510***	(-.0733)	-18.3476***	(-.0628)
N prior Part 1 charges	-2.2275*	(-.0201)	-7.1227*	(-.0241)	-10.4708	(-.0226)	-13.2923	(-.0144)
N prior property conv.	-.3608	(-.0227)	-1.2389	(-.0240)	-2.4585	(-.0253)	-3.4145	(-.0241)
N prior persons conv.	-.6947	(-.0100)	-3.0307	(-.0113)	-8.3143	(-.0173)	-22.3125*	(-.0228)
N prior weapons conv.	-5.2774	(-.0174)	-28.2441**	(-.0242)	-48.2380**	(-.0231)	-104.064**	(-.0245)
Off street last 2 years	-11.7990***	(-.0511)	-68.2119***	(-.0765)	-130.964***	(-.0820)	-269.960***	(-.0834)
Delinquent Career/Onset								
N arrests as juvenile	-3.5068***	(-.0583)	-15.1506***	(-.0687)	-25.5744***	(-.0647)	-50.0647***	(-.0608)
N charges as juvenile	-2.4313	(-.0187)	-9.5342	(-.0276)	-24.0121	(-.0289)	-73.6127	(-.0307)
Age at first arrest	-.4958*	(-.0442)	-1.9765*	(-.0368)	-3.2102*	(-.0275)	-2.7832	(-.0041)
Yrs since first incarc.	-.1851**	(-.0516)	-.9660***	(-.0651)	-2.0711***	(-.0722)	-4.2210***	(-.0708)
Yrs since first drug use	.1113	(.0142)	.5699*	(.0188)	.8739	(.0161)	1.5908	(.0145)
Prior CJS-Offender Action								
N prior incarcerations	-1.0048	(-.0210)	1.2909	(.0070)	5.9981	(.0182)	33.0899**	(.0494)
N prior parole revokes	-2.6442	(-.0120)	1.5453	(.0018)	2.0550	(.0013)	15.1079	(.0049)
Bad conduct last probat.	-5.0707	(-.0188)	-27.7741**	(-.0267)	-42.0183*	(-.0226)	-43.7981	(-.0116)
Recent parole revoked	-5.9814	(-.0261)	-12.1125	(-.0183)	.3077	(-.0112)	6.0630	(-.0075)
General Control Variables								
Offender age at sent.	.8214***	(.1512)	3.7014***	(.1754)	7.8619***	(.1858)	16.6030***	(.1793)
Off. born out of state	10.0297***	(.0476)	54.0314***	(.0665)	111.9447***	(.0769)	269.1421***	(.0912)
Coder prob. prognosis	.0746	(.0200)	.4659**	(.0323)	.9830***	(.0381)	2.1758***	(.0416)

* p<.05 **p<.01 ***p,.001

Table 6.22 (continued)

Regression Coefficients for Days to Post-Sentence Rearrest
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	4.1863	(.0079)	33.8304*	(.0165)	71.8108*	(.0196)	190.2591**	(.0256)
Black x prior adult arrs	2.1273**	(.0430)	8.3735***	(.0439)	15.3349***	(.0449)	33.7126***	(.0487)
Black x n prior prop cnv	-2.3604***	(-.0497)	-7.8562***	(-.0429)	-12.2308**	(-.0373)	-25.3182**	(-.0381)
Black x n charges as juv	-2.9895	(-.0074)	-26.9729*	(-.0173)	-36.3227	(-.0130)	-28.6405	(-.0051)
Female x Part 1 charges	-2.7544	(-.0191)	-17.6850*	(-.0317)	-36.5952**	(-.0366)	-75.8609**	(-.0374)
Off. age x drug problem	.9670*	(.0365)	3.7904*	(.0370)	5.8594*	(.0320)	8.2219	(.0221)
Off. age x prior trtment	1.0420*	(.0348)	5.4945**	(.0476)	10.3597***	(.0501)	21.5172***	(.0513)
Off. age x unemployed	.4791*	(.0202)	2.1563**	(.0236)	3.5201*	(.0215)	5.3966	(.0162)
Off. age x PO property	.7601*	(.0329)	3.2894**	(.0369)	5.4830**	(.0344)	10.4355*	(.0323)
Off. age x chg pst 5 yrs	.0443	(.0197)	.0905	(.0104)	.1085	(.0070)	-.0977	(-.0031)
PO viol x has detainers	11.8906	(.0111)	88.6355**	(.0215)	164.5369**	(.0222)	344.3837**	(.0230)
PO prop x n adl.arrests	-.3720	(-.0075)	-1.7317	(-.0090)	-1.9318	(-.0056)	-3.0060	(-.0043)
PO prop x prior prop con	2.1093**	(.0440)	6.1344*	(.0332)	9.0609*	(.0274)	16.0947	(.0240)
PO prop x n juv. arrests	2.6452**	(.0312)	10.0733**	(.0308)	17.0111**	(.0290)	35.7211***	(.0301)
PO prop x age at 1st arr	-.2837	(-.0101)	.0337	(.0003)	1.4258	(.0073)	4.8598	(.0123)
PO prop x yrs. 1st incar	-.1318	(-.0140)	-.4830	(-.0133)	-.5790	(-.0089)	-.9018	(-.0068)
PO drugs x n adl. convs.	-2.3425*	(-.0327)	-11.4553**	(-.0415)	-20.3970**	(-.0413)	-39.3682**	(-.0393)
PO drugs x Part 1 chgs.	6.8779***	(.0636)	20.7162***	(.0497)	31.7209**	(.0425)	53.7712*	(.0355)
PO drugs x last par. rev	-24.0807	(-.0229)	-82.8246	(-.0204)	-148.200	(-.0204)	-225.237	(-.0153)
Constant	321.1626***	(.0027)	856.1805***	(.0038)	1316.057***	(.0013)	2238.402***	(-.0034)
R squared	.147		.240		.274		.296	
Adjusted R squared	.143		.236		.270		.292	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.23

Regression Coefficients for Days to Post-Sentence Reimprisonment
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	1.2041	(.0123)	-7.5458	(-.0151)	-23.0268*	(-.0230)	-58.3250**	(-.0253)
Has job after sentence	3.7744***	(.0403)	22.4833***	(.0471)	42.6823***	(.0445)	93.5470***	(.0424)
Offender is on welfare	2.4188*	(.0215)	16.1095**	(.0281)	36.1064***	(.0314)	77.0187***	(.0291)
Offender is Black	-.1141	(.0065)	-16.6415**	(-.0272)	-51.4400***	(-.0445)	-164.808***	(-.0678)
Offender is Hispanic	-1.9791	(-.0110)	-19.9128*	(-.0217)	-47.9541**	(-.0261)	-147.6.1***	(-.0349)
Offender is female	-.6633	(-.0045)	-2.9511	(-.0039)	-1.3540	(-.0009)	32.7754	(.0094)
Lives in urban area	.6666	(.0071)	3.9392	(.0082)	6.2496	(.0065)	-10.6879	(-.0048)
Years at current address	.0118	(.0026)	.2026	(.0088)	.6181	(.0133)	1.3213	(.0124)
History of drug problems	-3.7800**	(-.0346)	-14.1692*	(-.0254)	-33.8066**	(-.0302)	-93.7748***	(-.0364)
Treated for drugs/alch.	-1.1457	(-.0093)	-11.8053	(-.0188)	-25.2138*	(-.0200)	-50.5279	(-.0174)
Has needle marks	.3376	(.0018)	-6.9268	(-.0074)	-25.7556	(-.0137)	-86.1913*	(-.0199)
Not a school drop out	-.0686	(-.0007)	2.5545	(.0052)	9.5423	(.0097)	25.6584	(.0113)
Doesn't live with family	-.4571	(-.0040)	-.5592	(-.0010)	-1.3755	(-.0012)	7.0014	(.0026)
Commited PO with group	1.0415	(.0111)	7.1364	(.0149)	8.8613	(.0092)	18.2464	(.0083)
Victim was a stranger	-2.1340*	(-.0202)	-16.2645**	(-.0301)	-33.0691***	(-.0305)	-85.3851***	(-.0342)
Presenting Offense								
PO property crime	.1101	(.0012)	-11.4446*	(-.0236)	-25.4986*	(-.0262)	-83.0145***	(-.0370)
PO crime against person	.6333	(.0091)	-5.1649	(-.0059)	-18.0789	(-.0128)	-65.6068*	(-.0218)
PO drug offense	2.5337	(.0237)	8.2493	(.0147)	18.0286	(.0137)	16.2605	(.0019)
PO Wolfgang severity	-.0203	(-.0034)	-.1093	(-.0036)	-.2427	(-.0039)	.4016	(.0028)
Has detainers at arrest	-4.6224*	(-.0174)	-25.0394*	(-.0207)	-42.5927*	(-.0177)	-72.7731	(-.0127)
Has pending charges	-3.4031**	(-.0291)	-28.8385***	(-.0484)	-64.7017***	(-.0541)	-154.168***	(-.0560)
On probation at PO	-1.7229	(-.0011)	-7.4089	(.0009)	-24.8750	(-.0049)	-68.7688*	(-.0134)
Anamnestic Theory								
N prior adult arrests	-.2212	(.0263)	-3.2696	(-.0164)	-8.3361*	(-.0387)	-21.5638**	(-.0574)
N prior adult conviction	1.0497**	(.0688)	5.0923**	(.0566)	7.8618**	(.0382)	12.2002	(.0223)
N prior adult chg. conv.	-.8639**	(-.0733)	-1.9042	(-.0317)	-.8487	(-.0070)	4.1140	(.0148)
N charges past 5 years	-.8071***	(-.0871)	-4.6559***	(-.0985)	-8.4054***	(-.0886)	-17.7985***	(-.0814)
N prior Part 1 charges	.1446	(.0332)	-3.7861	(-.0164)	-7.4397	(-.0173)	-14.4975	(-.0196)
N prior property conv.	.0200	(-.0367)	.4608	(-.0319)	-1.3497	(-.0436)	-7.8220	(-.0463)
N prior persons conv.	-.8886*	(-.0286)	-2.3676	(-.0150)	-6.9229	(-.0218)	-22.0797**	(-.0302)
N prior weapons conv.	.2194	(.0016)	-6.0169	(-.0088)	-12.7500	(-.0092)	-25.1634	(-.0079)
Off street last 2 years	-3.7474***	(-.0364)	-37.1058***	(-.0708)	-93.6180***	(-.0890)	-236.294***	(-.0975)
Delinquent Career/Onset								
N arrests as juvenile	-.3958	(-.0395)	-2.3010	(-.0490)	-5.3505	(-.0529)	-14.1490*	(-.0527)
N charges as juvenile	.7737	(.0072)	-.9086	(-.0246)	-4.0714	(-.0358)	-5.6920	(-.0421)
Age at first arrest	-.1567	(-.0496)	-1.2184*	(-.0746)	-3.4298**	(-.0902)	-8.9199***	(-.0930)
Yrs since first incarc.	-.1452***	(-.0867)	-.7292***	(-.1074)	-1.8018***	(-.1224)	-5.2378***	(-.1421)
Yrs since first drug use	-.0240	(-.0069)	-.0676	(-.0038)	-.0749	(-.0021)	.3271	(.0040)
Prior CJS-Offender Action								
N prior incarcerations	-.7339	(-.0345)	-4.6386*	(-.0428)	-9.7323**	(-.0447)	-19.6437*	(-.0392)
N prior parole revokes	-1.5647	(-.0159)	-5.9707	(-.0119)	-12.1804	(-.0121)	-19.1246	(-.0082)
Bad conduct last probat.	-1.6243	(-.0136)	-11.1972	(-.0183)	-25.0937*	(-.0205)	-51.1132*	(-.0181)
Recent parole revoked	-10.5436***	(-.0515)	-66.8086***	(-.0660)	-110.583***	(-.0595)	-169.448***	(-.0458)
General Control Variables								
Offender age at sent.	.0233	(.0469)	.7723	(.1173)	2.3690*	(.1423)	6.9759**	(.1583)
Off. born out of state	-.4610	(-.0049)	3.0318	(.0063)	11.8295	(.0123)	54.1658**	(.0245)
Coder prob. prognosis	.0278	(.0167)	.2494**	(.0294)	.5996***	(.0353)	1.5971***	(.0408)

* p<.05 **p<.01 ***p,.001

Table 6.23 (continued)

Regression Coefficients for Days to Post-Sentence Reimprisonment
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	3.6466	(.0155)	18.1150	(.0151)	43.2675*	(.0179)	72.4986	(.0130)
Black x prior adult arrs	1.2982***	(.0590)	7.2238***	(.0644)	13.6323***	(.0606)	30.3272***	(.0585)
Black x n prior prop cnv	-1.2164***	(-.0576)	-6.4230***	(-.0596)	-11.2654***	(-.0521)	-20.1034**	(-.0404)
Black x n charges as juv	-.3051	(-.0017)	-23.4788**	(-.0257)	-65.2147***	(-.0355)	-188.847***	(-.0446)
Female x Part 1 charges	.1058	(.0016)	-3.1089	(-.0095)	-9.7997	(-.0149)	-31.0750	(-.0205)
Off. age x drug problem	.0723	(.0061)	1.0019	(.0166)	2.3409	(.0194)	3.5365	(.0127)
Off. age x prior trtment	.3487	(.0262)	4.1881***	(.0617)	8.9574***	(.0658)	21.5105***	(.0686)
Off. age x unemployed	.1465	(.0139)	1.1237*	(.0209)	2.7506**	(.0255)	7.8673***	(.0316)
Off. age x PO property	.2053	(.0200)	2.1556**	(.0412)	4.6257***	(.0440)	10.9896***	(.0454)
Off. age x chg pst 5 yrs	-.0057	(-.0057)	.1298	(.0254)	.3209*	(.0313)	.8252**	(.0350)
PO viol x has detainers	6.6824	(.0140)	25.1729	(.0104)	41.5298	(.0085)	80.9651	(.0072)
PO prop x n adl.arrests	-.1474	(-.0066)	-1.9757	(-.0174)	-4.7851	(-.0211)	-15.8796*	(-.0303)
PO prop x prior prop con	.3133	(.0147)	1.5919	(.0146)	3.9497	(.0181)	12.6979	(.0253)
PO prop x n juv. arrests	-.8244*	(-.0218)	-5.7280**	(-.0298)	-11.4882**	(-.0297)	-21.8511**	(-.0246)
PO prop x age at 1st arr	-.3642	(-.0291)	-2.7474**	(-.0431)	-5.5102**	(-.0430)	-11.2022**	(-.0380)
PO prop x yrs. 1st incar	-.0814	(-.0194)	-.9777***	(-.0458)	-1.9042***	(-.0444)	-4.0691***	(-.0412)
PO drugs x n adl. convs.	-.5157	(-.0162)	-5.0991*	(-.0314)	-10.9853**	(-.0337)	-21.7002*	(-.0289)
PO drugs x Part 1 chgs.	2.2133**	(.0460)	10.3388**	(.0422)	20.9893**	(.0426)	36.6305*	(.0323)
PO drugs x last par. rev	1.3089	(.0028)	-.2428	(-.0001)	-44.7021	(-.0093)	-191.747	(-.0174)
Constant	357.4017***	(.0037)	1043.856***	(.0182)	1710.707***	(.0203)	3187.358***	(.0188)
R squared	.084		.211		.267		.309	
Adjusted R squared	.080		.207		.253		.305	
N of cases	11,714		11,746		11,749		11,749	

* p<.05 **p<.01 ***p,.001

Table 6.24

Summary of Statistically Significant Effects (62 Variable Models)

Independent Variable	Re-arrest	Re-arrPers	Repeat PO	Re-Imprisoned	Log of Chgs.	Log of Conv. Chgs.	Log of Pers.	Log of Arrest Rate	Log of Chg. Rate	Log of Pers. Chg. Rate	Summed Seriousness	Days to Re-Arrest	Days to Re-Impris.	Numb. Sign.
Structural Variables														
Offender is unemployed	+				+	+	+	+	+	+	+	-	-	10
Has job after sentence	-		-	-	-	-		-	-	-	-	+	+	11
Offender is on welfare				-				-	-				+	4
Offender is Black	+	+	+	+	+	+	+	+	+	+	+	-	-	13
Offender is Hispanic	+	+	+	+	+	+	+	+	+	+	+	-	-	13
Offender is female	-	-	-		-		-	-	-	-	-	+		10
Lives in urban area	+	+	+		+	+	+	+	+	+	+	-		11
Years at current add.	-		-		-			-	-	-	-	+		8
History of drug probs.				+	+				+				-	4
Treated for drug/alch.														0
Has needle marks	+		+		+	+		+	+	-				7
Not a school dropout	-	-	-		-	-		-	-			+		8
Doesn't live with fam.	-		-		-	-		-	-		-			7
Committed PO with group	-		-		-	-		-				+		6
Victim was a stranger	+		+	+	+	+		+	+		+	-	-	10
Presenting Offense														
PO property crime	+		+		+	+		+	+		+	-	-	9
PO crime against person		+	+				+			+				4
PO drug offense			+											1
PO Wolfgang severity	-		-		-	-						+		5
Has detainers at arrest													-	1
Has pending charges	+	+	+	+	+	+	+	+	+	+	+	-	-	13
On probation on PO	+	+	+	+	+	+		+	+			-		9
Anamnestic Variables														
N prior adult arrests	+	+	+	+	+	+	+	+	+	+	+	-	-	13
N prior adult convicts.						+	-	-	-	-	-		+	7
N prior adult chg. conv.	-	-	-		-	-	-	-	-		-	+		10
N charges past 5 years	+	+	+	+	+	+	+	+	+	+	+	-	-	13
N prior Part I charges						+		+	+	+	+	-		6
N prior property conv.			-											1
N prior persons conv.		+		+			+			+				4
N prior weapons conv.	+	+			+		+	+	+	+	+	-		9
Off street last 2 years	+	+	+	+	+	+	+	+	+	+	+	-	-	13

Table 6.24 (continued)

Summary of Statistically Significant Effects (62 Variable Models)

Independent Variable	Re-arrest	Re-arrPers	Re-peat PO	Re-Im-pri-son-ed	Log of Chgs.	Log of Conv. Chgs.	Log of Pers.	Log of Ar-rest Rate	Log of Chg. Rate	Log of Pers. Chg. Rate	Log of Ser-ious-ness	Days to Re-Ar-rest	Days to Re-Im-pris.	Numb. Sign.
Delinquent Career/Onset														
N arrests as juvenile	+	+		+	+	+	+	+	+	+	+	-		11
N charges as juvenile														0
Age at first arrest					+	+	+	+	+	+	+	-	-	9
Yrs since first incarc.	+	+	+	+	+	+	+	+	+	+	+	-	-	13
Yrs since first drug use					-	-	-		-		-			5
Prior CJS-Offender Action														
N prior incarcerations	-					-				+			-	4
N prior parole revokes		-	-				-			-				4
Bad conduct last probat.					+	+	+	+	+		+	-	-	8
Recent parole revoked			+					+	+	+			-	5
General Control Variables														
Offender age at sent.	-	-		-	-	-	-	-	-	-	-	+	+	12
Off. born out of state	-	-	-	-	-	-	-	-	-	-	-	+		12
Coder prob. prognosis	-	-	-	-	-	-	-	-	-	-	-	+	+	13
Interactions														
Black x on prob at PO	-	-	-	-	-	-	-	-		-		+		10
Black x prior adl. arrs	-	-	-	-	-	-	-	-	-	-	-	+	+	13
Black x n prop conv	+	+	+	+	+	+	+	+	+	+	+	-	-	13
Black x n chges as juv					+	+	+	+	+	+	+		-	8
Female x Part I charges	+				+	+		+	+		+	-		7
Off. Age x drug probs		-			-	-		-	-		-			6
Off. Age x treatment	-			-	-	-		-	-		-	+	+	9
Off. Age x unemploy.				-	-	-	-	-	-	-	-	+	+	10
Off. Age x PO property	-	-	-	-	-	-		-	-		-	+	+	11
Off. Age x chgs 5 yrs				+	-	-	-	-	-	-	-		+	9
PO viol x has detainers	-	-	-		-	-	-	-	-	-	-	+		11
PO prop x n ad. arrests						-		+	+	+				4
PO prop x n prop con			+		-	-	-	-	-	-	-	+		9
PO prop x n juv arrests	-	-			-							+	-	5
PO prop x age 1st arr		-											-	2
PO prop x yrs 1st inc			+		+	+		+	+				-	6
PO drugs x n adl. conv.	+	+	+	+	+	+		+	+		+	-	-	11
PO drugs x Part I chgs.	-		-		-	-		-	-	-	-	+	+	10
PO drugs x par. rev.					+	+		+	+		+			5
Number of Independent Variables Significant	37	29	36	25	47	46	31	48	48	36	40	39	33	

Table 6.25

Summary of Results for Three General Forms of Recidivism

	All Crimes	Persons	Imprisonment
Structural			
Unemployed	+		
Has job after sentence	-		-
Black	+*	+*	+*
Hispanic	+*	+*	+*
Female	-	-	
Lives in urban area	+	+	
Has needle marks	+		
Not a school drop out	-		
Doesn't live with family	-		
Victim is a stranger	+		+
Presenting Offense			
PO property	+*		
PO persons		+*	
Has pending charges	+*	+*	+*
Anamnestic Variables			
N prior adult arrests	+*	+*	+*
N of charge convictions	-	-	
N charges past five years	+*	+*	+*
N prior persons convict.		+*	
Off street past two years	+*	+*	+*
Delinquent Career/Onset			
N arrests as juvenile	+*	+*	+*
Years since first incarc.	+*		+*
Prior CJS/Offender Action			
N of prior incarcerations	-*		
General Control Variables			
Offender age at sentencing	-*	-*	-*
Offender born out of state	-*	-*	
Coder prob. prognosis	-	-	-
Interactions			
Black x prior ad. arrests	-*	-*	-*
Black x n property conv.	+	+	+
Offender age x PO prop.	-		
Offender age x chgs. 5 yrs.			-

* = Standardized coefficients tend to be greater than or equal to .05 across models. Variables presented tend to be consistent predictors of one or more of the dependent variables listed. Several interaction terms are excluded because their "main effects" components did not meet inclusion criteria.

CHAPTER SEVEN

DIMENSIONS OF THE SENTENCE

The focus of this chapter is the sentence administered in 1976-77. Our purpose is primarily descriptive as we attempt to identify the major components of the sentence and show their interrelationships. Consistent with previous literature, we take the view that a sentence is multidimensional. This leads us to look at the basic distinctions of whether the offender was sentenced to incarceration or not (the "in/out" decision) and, if incarcerated, to where the offender was sentenced (i.e., jail versus prison). The so-called "time" dimension (how long a term of incarceration or probation is given) completes the traditional trilogy of the components of the sentence.

However, we depart from the literature in viewing the multidimensionality of the sentence as being more encompassing than just these three components. In particular, we look at how the sentence received in 1976-77 fits into the pattern of prior sanctions in the offender's career. Several components are identified, including whether the sentence was the first sanction received, the first time the offender was placed on probation, or the first time that s/he was incarcerated. This allows for identifying the overall patterning of the sanctions of the career. Here the interest is in "progressive" sentences -- ones in which the sanction received was more severe than those previously given the offender.

Another dimension investigated is the independence of the sentence from other sanctions. Many of the individuals in the Guidelines Project sample were on probation, on parole, or serving time for another matter when sentenced. As a consequence, any effect of the sentence upon subsequent

recidivism may not be independent of the effects of those other, concurrent sanctions. The extent to which sentences are confounded is investigated, as is the lack of independence between the basic dimensions of the sentence.

We make two assumptions in this chapter. The first is that, with the exception of the "time" dimension, all components of the sentence can be represented as discrete indicator (dummy) variables. That is, the sentence as a whole can be captured by the configuration of the constituent elements. Is the offender fined? Is s/he incarcerated? Is this the first sanction of the career? By breaking the dimensions of the sentence into dichotomous elements, we can maintain the complexity of each sanction.

The second assumption is that these sanctions can be ordered by severity according to what is essentially the "where" dimension. We will often refer to the most severe sanction received. In doing so, it is assumed that any sentence involving incarceration to either a state prison or the youth complex at Yardville[1] is more severe than other sentences. Incarceration in a jail or county penitentiary is seen as less severe than a prison term, but more severe than other forms of sanctions. Sentences to probation, in the absence of any incarceration, are considered less severe than terms of incarceration. The least severe sanction is where the individual was not sentenced to any form of

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[1] For many purposes, it is not necessary to distinguish between sentence to a state prison and those to the youth complex at Yardville: both can be considered as sentences to "prison." At times we will collapse these types of sentences into one group. However, as was established in Chapter Three, the mean age of offenders at Yardville is significantly lower than those sent to a state prison and age is known to be strongly correlated with criminal behavior. Ultimately, the distinction between the types of prison sentences will be maintained.

custody or supervision.[2]

We also investigate how the various dimensions of the sentence are confounded with one another. As will become evident, there is a high degree of intercorrelation between some components. Some intercorrelation is to be expected because of sentencing practices. For example, initial sanctions tend to be less severe and split sentences to prison are seldom given. Some dimensions of the sentence are intercorrelated by definition. A sentence that is progressive must involve a sanction more severe than previous ones and thus the sentence (i.e., fine, probation, jail, or prison) is the first of its type.

The organization of this chapter follows that used in Chapter Five where the major independent variables of the study were introduced. The components of the sentence are investigated. We then discuss important distinctions between the sentence given and the sanction received. Interrelationships among the dimensions of the sentence are specified. These components, along with the variables introduced in earlier chapters, are then used to predict the probability of rearrest over the cumulative half-year windows. These results form the basis of the analyses of the thirteen recidivism measures in the next chapter.

THE BASIC DIMENSIONS OF THE SENTENCE

A preliminary look at the kinds of sentences given to the 1976-77 sample

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[2] This hierarchy of severity is arguably false when the specifics of some sentences are considered. For example, a fine of several thousand dollars is probably more onerous to the individual than spending a few days in jail. However, this hierarchy does, in general, hold and probably captures the intent of the judge at the time of sentencing. Furthermore, these assumptions are consistent with other assessments of punishment severity (Von Hirsch, 1976; Morris and Tonry, 1990).

is shown in Figure 7.1. The modal sentence did not involve some kind of incarceration, as 56.5% of the sample were "out" after sentencing.[3] By far the most common sentence within this group was a sentence to some term on probation (93.5% of those who were "out") and, indeed, this is the most common sanction received by the entire sample.

"Where" an individual is sentenced usually refers to a type of incarceration. However, we can maintain the in/out dimension by also considering types of nonincarcerative sanctions as "places." This allows us to distinguish between the "out" sanctions of probation, fine, and a residual category of "other." Doing so finds that 4.4 percent of those out after sentencing received a sentence where the most severe sanction required payment of a fine or restitution.[4] Some form of probation was the most severe sanction for 93.5% of those out after sentencing, with the remaining two percent of this subgroup receiving some other sentence.

The group of all "other" sanctions is mixed. Some cases were held for resentencing and we have no evidence that the resentencing actually occurred. Others were never sentenced, a small number were sentenced to community service, and some were sentenced to treatment programs such as drug or alcohol rehabilitation (with no probation restrictions).[5] As these kinds of sentences

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[3] As shown at the bottom of Figure 7.1, an additional 1.73% (203) of the sample were released at the point of sentencing as they were sentenced to only time served. These individuals could also be considered "out" on the in versus out dimension. As they served time, we treat them as "in" on this dimension.

[4] Under the operative principal of requiring payment, we treat fines and restitution as interchangeable. While we will generally refer to this aspect of the sentence as "fines," it should be remembered that it also implies court-mandated restitution as well.

[5] It is interesting to note that few cases fall into this category. What this suggests is that the kinds of interventions commonly investigated in the search for something that "works" constituted a relatively small proportion of

involve a small proportion of those given to this sample, it serves no purpose to make further distinctions within this group.

Within the sentences involving a probation term, there is a roughly even split between those that required extra conditions on the probation, and those that did not. Exactly what constituted these conditions is not clear from the information taken from the Judgement of Conviction sheets. In some cases it was community service, others involved drug or alcohol counseling. What is known is that these sentences involved a greater degree of intervention than those requiring a simple term on probation, and thus we keep the distinctions between probation with and without extra conditions.

The breakdown of sentences involving some form of incarceration shows a much more even distribution of most severe sanction. Almost half (47.7%) of these sentences involved a term to a county jail or penitentiary; 22.5% of the sentences were to the youth correctional complex at Yardville.[6] The remaining 29.8% of the incarcerative sentences required a term at one of the State correctional facilities. These state prisons vary widely in the availability of treatment and counseling, as well as the level of security at the prison. The exact institution to which the individual was ultimately sent is not known from the Judgement of Conviction sheet, nor would such information be appropriate under the judge-centric approach to sentencing to

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the kinds of sanctions mandated by judges in this sample. If these interventions were part of the conditions of the sentence, they were more likely to be used in conjunction with other sanctions (e.g., probation) and not as the only component of the sentence.

[6] The Yardville complex is the central clearing house for incarcerations for all young adult offenders. After intake at Yardville, some inmates are sent to other institutions, some remain at the Yardville complex. This category of the sentence thus represents a heterogeneous mix of custodial, treatment, and training programs, though all are a form of prison for young adults.

be adopted below.

The last row of Figure 7.1 indicates whether those sentenced to incarceration had to serve time after the date of sentencing. Those given a jail term were most likely to be released on the day of sentencing, though the vast majority (92.5%) of sentences to jail required serving further time. In only a handful of cases were sentences to Yardville (.6%) or state prison (.9%) fulfilled by time served prior to sentencing.[7] In practice, the incarcerative sentences given to this sample required custody after the date of sentencing.

The breakdown of the type of sentences shown in Figure 7.1 hides the fact that a particular sentence can contain multiple sanctions. For example, fines can be given in conjunction with probation or incarceration. Split sentences requiring both jail and probation are also relatively common. Conversely, some sanctions (e.g., Yardville versus state prison; jail versus prison) are either logically exclusive or in practice are not given as part of the same sentence. Thus use of some forms of sanctions are more prevalent than would be suggested by Figure 7.1.

The distribution of the various parts of the sentences given to the entire sample are shown in Table 7.1. The residual "other" category comprises 1.1% of the sanctions administered to this sample. Almost 35 percent of these sentences contained a fine or restitution as part of the sentence and nearly two thirds (63.1%) of the sentences used probation as part of the imposed sanctions. Twenty-nine percent of all sentences required additional conditions as part of the probation as well. Over the entire sample of 1976-

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[7] Most likely, the credit for time served given was for time accrued while serving another sentence. This points to the fact that some sentences are not independent of other existing sanctions. We address this below.

77 sentences, one fifth of the individuals were sentenced to jail as part of the sanction, and more serious incarcerations were part of 22.6% of all sentences.

Given this overlap between the forms of sanctions, especially those involving fines and probation, the use of most severe sanction alone to represent the judge's sentence could prove too simplistic. Table 7.2 details the various combinations of sanctions that comprised these sentences. The distribution within the most serious sanction of probation is relatively uniform. About 24% of these sentences were to probation with no fines or conditions. An additional 28.6% received probation in conjunction with a fine. Twenty-five percent of the probation sentences involved conditions on the probation, but no fine while 23% of probationers received a fine and conditions.

Over half of the sentences requiring a jail term had no other sanctions imposed. Fines were added to 2.7% of the sentences to jail. Split sentences, requiring both probation and jail, comprised 18.3% of the jail sanctions and an additional 10% were split sentences with conditions placed on the probation. Split sentences with a fine constituted 11% of the sentences with the most serious sanction of a jail term and 7.1% of these were split sentences with both a fine and conditions on the probation.

Within sentences of time at the Youth Complex at Yardville, there is little variation in additional sanctions. Almost 96% of these sentences involved no additional sanctions. A fine alone was added in fewer than one percent of these sentences. Some kind of split sentence (custody and probation) was used for 3.3% of the Yardville sentences. Much the same picture emerges for sentences to a state prison. While fines alone were more

likely to be added (3.2% of these sentences), nearly 95% of all sentences to state prison involved no additional sanctions. In less than two percent of these sentences was a probation term added to the time to be spent in custody.

In assessing exactly how judges combined the various sanctions at their disposal to form the 1976-77 sentence, we find several empirical consistencies. "Hard time" to a correctional institution is effectively given in the absence of additional sanctions. In these instances the use of other sanctions (i.e., fines or probation) is so infrequent that it is not possible to distinguish within either sentences to Yardville or those to state prison. Consequently, we will combine all sentences within these groups for the subsequent analyses. Conversely, sentences with the most severe sanction of probation are much more varied. Here, judges made greater use of the available options, and it is possible to maintain the distinctions within the various kinds of sentences to probation.

It is within sentences to jail that some decisions have to be made concerning the collapsing of sentences. Maintaining the full breadth of sentences involving a jail term invites analytic complications as some combinations (e.g., jail in conjunction with a fine) are empirically infrequent. This is especially true given the range of independent variables used in earlier chapters. Yet, to ignore distinctions within jail sentences would lose the intent of the judges' sentences as there was clearly a wide use of additional sanctions with a jail term. Consequently, we will maintain some sensitivity in types of jail sentences, though conditions on probation will be ignored. Thus, jail terms with fines and probation (18.1% of all sentences involving jail) will be differentiated from split sentences that did not require payment (28.3%). In addition, jail sentences with fines will be

combined with jail sentences that did not involve additional sanctions (53.9%).

To summarize, the "in" versus "out" and "where" dimensions of the sentences will be captured by a series of eleven dummy variables. No distinctions will be made within custodial sentences for the Youth Complex at Yardville or state prison because empirically few sentences involving these institutions used additional sanctions. Some minor loss of information is necessitated by the distributions within sentences to jail. As these dummy variables exhaust the range of sentences as defined here, one must be omitted during the analysis. To avoid potentially severe collinearity problems among these indicators (fines alone or lesser sanctions constitute a small percentage of all sentences), the sentence of only probation will serve as the referent category for these dimensions of the sentence.[8]

CONTINUOUS COMPONENTS OF THE SENTENCE

The third common dimension of the sentence is the time component. Unlike the two dimensions previously discussed, "time" cannot be captured by discrete indicators without making additional measurement assumptions. Rather than make such assumptions, we will treat the time aspect of the 1976-77 sentence as continuous. More generally, we look at all interval-level

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[8] In preliminary analyses we adopted a more classical analysis of variance approach to measuring these dimensions of the sentence. Dummy variables for any fine, any probation, any jail, and any prison (Yardville and state prison) were constructed. The various combinations of sentences shown in Table 7.2 were then conceptualized as the interactions among the main components of the sentence. Such an approach has the advantage of providing answers to questions such as "What is the effect of jail upon recidivism?" However, as the cells in Table 7.2 indicate, many of the possible combinations of sanctions were infrequent and thus certain kinds of interactions were untestable. Moreover, the ANOVA approach does not really operationalize the sentence as a distinct combination of sanctions. We thus opted for the dummy variable approach described above.

components of the sentence in this section.

Unfortunately, not all sentences specify a fixed-term of incarceration. More common is a range of time to be spent in jail or prison as dictated by the judge as part of the sentence. Elsewhere (Botsko et al., 1990) we describe how the specified minimum and maximum terms were combined to yield an estimate of the prescribed length of the sentence. More problematic are the so called "indeterminate terms" where no minimum or maximum was specified. For those individuals given an indeterminate term, or a life sentence to prison, some coding decisions had to be made for the time sentenced.

The use of the term "indeterminate sentence" represents a relative concept as all the sentences in New Jersey during the time period studied were indeterminate in the sense that even offenders sentenced to what we call a "fixed-term" were subject to parole, technically at any time after sentencing. (For most offenses, this changed in 1979.) Nevertheless, a specific maximum term for the sentence does set an upward limit on how much time can be served, and thus functions as a kind of "communication" to the parole board or review committee of the judge's intended severity of the punishment.

Table 7.3 shows the frequency with which indeterminate terms were used by the judges for this sample; 8.1% of all sentences involved unspecified time for some sanction. The greatest use of indeterminate time was made for probation sentences (2.4% of the sample) and incarceration at Yardville (5.4%). Only a small fraction of these sentences involved indeterminate terms to jail or state prison. Note too, that even with the restrictions on time at risk that defined the sample (see Chapter Three), seven individuals who were given a life sentence to prison (.1% of all sentences) served short enough terms to qualify for inclusion in this analysis.

When we look within sanctions (the conditional percentages of Table 7.3) a different picture emerges. While indeterminate terms were generally used infrequently, they represent the modal form of time given to those sentenced to the Youth Complex at Yardville. About forty-five percent of sentences given to the Youth Complex for young adults were for terms of specified lengths, suggesting that the judge wanted to communicate an intended punishment to the review committee making the release decision. Conversely, the judge's intent, more often that not, was to let officials at the complex decide on the length of incarceration. Consequently, we will not consider the time dimension for sentences to Yardville. For all other indeterminate sentences, the mean time sentenced for comparable "places" (e.g., probation, jail, or state prison) will be assigned.

The continuous components of the sentence are summarized in Table 7.4. Across the sample, the average amount fined was $881 with $500 being the median. The average probationer was to serve about 27 months, with a median of two years. The jail terms given this sample averaged about eleven months with a one year term being the median sentence. Sentences to state prison were, on average, for terms of about 7.6 years.

It is also evident from the descriptive statistics in Table 7.4 that these continuous components of the sentence are associated with the most severe sanction received. Fines tended to be higher when given in conjunction with a prison term than when payment was the most severe aspect of the sentence.[9] Fines given in conjunction with jail terms were higher, on

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[9] For the results in Table 7.4 the most severe sanction of "prison" combines sentences to either Yardville or state prison. This masks some heterogeneity in the actual sentences as fines given to young adults tend to be lower than those assessed on adult offenders (perhaps a reflection of the judge's perception of the offender's ability to pay). The same holds true for probation

average, then those levied on probationers. Split sentences to prison had longer probation terms than did those to jail. When additional sanctions were used as part of the sentence, they tended to mirror the most severe aspect of the sentence.

The last finding of note in Table 7.4 is that all of the continuous components of the sentence are quite skewed toward higher values. When these sanctions were used by judges, lower values, rather than higher values, predominated. While some extremely large fines were levied, and uncommonly long terms to probation, jail or prison were given to a few offenders, they were quite rare for this sample.

When the sample as a whole is considered, this skew will be even more apparent. Sentences involving no prison term, for example, logically have a value of zero on this component of the sentence. When the judge decides not to require payment of some kind, the value of zero dollars can be assigned. Maintaining the full range of sentences for the subsequent analyses will thus have the consequence of exacerbating the existing skew of the continuous components of the sentence.

DIFFERENTIATING THE SENTENCE FROM THE TREATMENT

When "the sentence" is treated as a dependent variable, it is usually broken into its constituent parts. Thus we find models for whether or not the offender was incarcerated (e.g., Wheeler et al., 1982) and, if incarcerated, models for the length of time sentenced to incarceration (e.g., Zatz, 1984). Alternatively, a summary measure of the sentence is constructed (e.g., Welsh

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terms. Consistent with the decision not to measure the time dimension for sentences to the Youth Complex, the prison column in Table 7.4 pertains to days sentenced to state prison only.

et al., 1984) to capture a fuller range of the nature of the sentence. In these circumstances, "the sentence" is clearly that ultimately decided upon by the judge. The investigation (and usually the data) stops at that point in the CJS. Consequently, "the sentence" is what the court has mandated for the individual, and not necessarily what actually happened as a result of that sentence.

The situation is somewhat different when the sentence is treated as an independent variable. Often, only a subset of possible sentences is investigated as samples for the analysis are defined by the sanction received. For example, only those sentenced to prison are studied in an effort to understand recidivism as defined by return to prison (e.g., Schmidt and Witte, 1984). Another approach is to break the sample into subgroups based on the court's sanction (e.g., Murray and Cox, 1979) in order to compare levels of recidivism across groups. The time actually served on the sentence is a common indicator of the sanction. In these instances, the sentence is, in more common terms, the treatment received.

However, there is an important disjuncture between the "sentence" that serves as a dependent variable and that used as an independent variable. The former reflects decisions made by the judge while the latter is an amalgam of both the judge's decision and the input of other CJS actors. While the court-mandated sanction shapes what happens to the individual after sentencing, the specific final treatment is in the hands of parole officers, probation officers, counselors, and others. No single decision maker in the criminal justice system controls the treatment received by the offender. Rather, separate decisions are made in what has been described as a "loosely coupled" system. Thus, for example, a judge will sentence an offender to probation or

state prison, but will not necessarily know, or control, the extent of supervision the offender will be under (although the judge may stipulate a level of supervision), nor which specific institution or type of therapy program in which the offender may subsequently participate. As this implies, the sentence as conceived by the judge may not accurately reflect the specific nature of what actually happens to the individual.

It is therefore possible to distinguish between the sentence given to the offender (the "sentence" as a dependent variable) and the specific treatment received as a consequence of that sentence (the "sentence" as an independent variable). While the decision of the judge is a necessary condition for the offender to receive treatment, it is clearly not sufficient for the actual treatment is in the hands of other criminal justice actors. Fines may never be collected, supervision under probation may be more lax (or more stringent) than expected, drug or alcohol programs may not be attended, or the incarceration experience envisioned by the judge (e.g., a job training program) may never occur. The actual time served is also often determined by others such as parole boards or youth complex committees.[10] Thus, some of the more general characteristics of the actual sentence (treatment) are determined

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[10] This is less true in states with determinate sentencing laws where a judge can be fairly certain that an offender will serve the prescribed minimum term. Yet even in these situations, the sentence may not be administered exactly according to a judge's expectations. Offenders may (rarely) serve more time than sentenced (due to behavior while incarcerated) or overcrowded conditions may lead to an early release. Similarly, the majority of a sentence to prison may be served in jail because of overcrowding. Thus, there can be discrepancies between what the sentence mandates and that administered even in the presence of determinate sentencing practices. Determinate sentencing laws were not in force at the time the present data were collected.

by the judge, but not all of the specific aspects of the intervention.[11]

The Potential for Slippage between the Sentence and Treatment.

While not immediately obvious, the amount of discrepancy that can exist between the sentence and the treatment is tied to the breadth of information available about each and the precision used to measure that information. (We refer to these dual aspects of breadth and precision as the "specificity" of the data, distinguishing between the extremes of very general data or information and very specific data or information.) That is, depending upon what reduces to measurement considerations, there can be little or considerable difference between the judge's sentence and the treatment actually received by the individual. In this section, we evaluate the conditions under which the distinction between the sentence and the treatment is irrelevant and those where the distinction is consequential.

Figure 7.2 serves as a heuristic guide to the ensuing discussion. On the horizontal axis, the sentence as mandated by the judge is contrasted with what actually happened to the individual (the "treatment"). A given empirical study will fall somewhere along this dimension. For example, studies of racial disparity in sentencing (the sentence as a dependent variable) clearly fall on the left-most side of Figure 7.2. In contrast, much of the literature on the effects of criminal justice intervention focuses on very specific types of interventions such as job training programs, group or individual therapy programs (see Andrews et al., 1990a), support payments (Berk et. al., 1980),

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[11] Research on which treatments are effective clearly informs judicial decision making, though the literature is inconclusive in the search for programs that "work." But the information provided by these studies is also quite removed from what a judge can do in the course of sentencing. The actual sentence given is much closer to that part of the CJS over which judges have the greatest control. Hence, researching the effectiveness of the sentence, rather than the treatment, received provides an evaluation of the judge's decision per se.

or community based programs of various types (Greenwood and Turner, 1985). Such studies obviously pertain more to the treatment received by the individual than to the expectations for that treatment.[12]

The horizontal dimension in Figure 7.2 refers to the general or specific nature of the information available about the sentence or actual treatment. In the abstract, we differentiate between situations where only a few aspects of the sentence/treatment are crudely measured ("general information") and those where a broad range of components are precisely measured ("specific information"). Our basic contention is that these two dimensions in combination place limits on how discrepant the sentence can be from the treatment received as a consequence of that sentence. More importantly, as the potential for disjuncture increases, an evaluation of the effects of the sentence may <u>not</u> be the same as the evaluation of the treatment received, just as an evaluation of a CJS treatment intervention may tell us little about the impact of the sentence that led to the intervention.

The points label "A, B, C, and D" represent four ideal types that can be contrasted. Consider first point A where the interest is in the sentence as captured by a wide array of specific information. To the basic components of in/out, where and time, we might add the judge's <u>expectation</u> for the amount of time the defendant will actually serve. While a sentence may specify a minimum and maximum amount of time to be served, judges are aware of how these

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[12] It is interesting to note that one of the explanations for the paucity of research demonstrating that "something works" is that intervention programs are not carried out in accordance with program goals. That is, there is some slippage between the ideals of the intervention and how it is experienced by a given individual. In terms of the current discussion, "program goals" could replace "the sentence" in Figure 7.3. Thus much of what will be said about the potential for discrepancy between the sentence and the treatment will also apply to differences between a treatment program's goals and what actually happens to those in the program.

translated to time actually in custody and thus the official time sentenced may reflect this expectation. Similarly, another aspect of this ideal type would be the purpose of the sentence in terms of specific deterrence, incapacitation, rehabilitation, and so forth. What the judge hopes to accomplish in light of the particulars of the sentence can be a factor in fashioning the features of the sentence. For example, the "symbolic" nature of the length of time sentenced could be as important as the length of time served in affecting recidivism (Myers, 1989).

As was shown earlier in this chapter, conditions are often placed on the terms given probationers. Ideally, one would like to know what such conditions were. Whether the offender was to undergo drug rehabilitation, therapy, job training, obtain a GED or perform community service is a specific component of the sentence, as is the judge's expectations for the offender's compliance with such conditions. While more examples of what constitutes the sentence as a whole could be cited, the basic contention remains the same: we can conceptualize many aspects that, when combined, produce the "sentence" as fashioned by the judge.

In contrast, the ideal type for the treatment received (point B in Figure 7.2) would consist of broad and detailed information about a wide variety of factors, only some of which mirror the components of sentence. The time actually served in custody or under parole or probation supervision are the most common. As well, the amount of time spent in any rehabilitation, therapy, or training program are relatively easily measured and complement aspects of the sentence. The offender's perception of the sentence is also likely to be relevant to recidivistic behavior. Ideally, we would like measures of whether the individual thought the sentence was "light" or "harsh"

and what s/he thought the sentence symbolized.

However, as the concern shifts towards what actually happened to the offender, additional specific information is pertinent to the treatment received. For example, the conditions experienced in custody (e.g., overcrowding, the quality of the food and the environment, the level of security) and the type of supervision (e.g., number and nature of contacts) while under probation and parole are likely to be important aspects. Which job or counselling programs are entered and the characteristics of the people encountered (e.g., guards, probation officers, teachers, counsellors) in terms of their abilities, charisma, and so forth may well influence both the impact of the treatment and the likelihood that the offender will recidivate. Note, however, that as more of these kinds of factors are introduced as legitimate components of the true nature of the intervention, they are less derivative of the sentence itself and less under the control of the judge at the time of sentencing.

We see then that as the definition of what constitutes the sentence and the treatment gets more specific, the potential for a divergence between the two increases. The discrepancy, represented by the distance between points A and B in Figure 7.2., arises from two distinct sources. First, what the judge mandates may not actually occur for a variety of reasons (e.g., failure to complete a program, prison overcrowding, or the failure of the offender to interpret the sentence in the manner intended by the judge). Second, what constitutes the appropriate ideal measures of the treatment include factors that are unrelated to the sentence itself. Specific measures of the people encountered, the treatment received from other CJS actors, and the individual's psychological/emotion responses to these experiences are all

legitimately part of the treatment "package" if the interest is in evaluating recidivistic behavior.

While it is relatively easy to list the concepts important to a full understanding of the sentence or the treatment received, and it is even possible to imagine valid, reliable, and detailed operationalizations of these concepts, the practical constraints imposed by data cannot be avoided. No single study attempts to measure all of these simultaneously for either the sentence or the treatment, let alone for both. Thus, in one sense, the slippage between the sentence and the treatment received can never be fully evaluated. But even when some subset of indicators is available, the ability to conceptualize the components of the sentence or the treatment is outstripped by the limitations of the data.

A good example of this is provided by the detailed types of sentence measures available in the present study. As was shown in Table 7.2, we can be relatively specific in combining the in/out and where dimensions of the sentence to identify various combinations of sanctions. Sanctions involving custody at the Youth Complex or a State Prison are, at times, combined with probation, conditions on probation, or fines. Presumably, the judge had some expectation or symbolic reason for imposing these additional sanctions and thus maintaining these distinctions would put us closer to actually measuring the sentence as conceptualized by the judge (i.e., place us closer to point A in Figure 7.2). However, because lesser sanctions in conjunction with incarceration are relatively infrequent, we cannot keep the nuances in these sentences without introducing severe collinearity in the analysis. In practice, constraints introduced by how various sanctions are actually used

limit the ability to empirically represent the "sentence."[13]

The irony in the discrepancy between the ideal types of the sentence and the treatment is found when only simplistic measures of each are used. This situation is represented by points C and D in Figure 7.2. If the sentence is measured by the dichotomous in/out dimensions alone (point C), then it bears a close correspondence to the treatment actually received (point D). If the offender is sentenced to custody (either jail, or prison of some form), then this is effectively the treatment received as the vast majority sentenced to some time in custody will indeed serve some time. Note, however, that points C and D in Figure 7.2 are not identical as there is a small possibility that those given a sentence involving incarceration will not serve any time. Similarly, using only a dichotomous measure for probation or not and ignoring all other aspects of the sentence would place the measure of the sentence close to the treatment received, though here too, there may be some slight slippage between those given probation and those actually received by the probation department.

Thus it is possible to reduce any potential discrepancy between the sentence and the treatment received by focusing on only a small aspect of either and measuring that aspect rather crudely. This is done, however, at a great cost. Projecting points C and D back to the Sentence-Treatment dimension finds that they are quite removed from the poles of that dimension. A single, crude measure of the sentence is a poor surrogate for the ideal type conceptualization of the sentence, just as a similar measure is for the ideal

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[13] The same can be said for practically measuring the ideal type for the treatment received. No one study can measure all of the treatment components that might be identified, nor is it likely that data would support the detailed distinctions needed to fully measure the "treatment."

type conceptualization of the treatment.

Connecting point A to point C, and B to D, visually illustrates the dilemma facing those who wish to evaluate criminal justice interventions. The "fan" produced by the four ideal types in Figure 7.2 captures the inherent problem that must be addressed. As the information about the intervention becomes more specific, both in terms of breadth and detail, that intervention becomes more conceptually distinct from other interventions which is a desirable property. But this is at the expense of the logical interrelationships that should exist between the parts of the criminal justice system. The sentence can bear less of a relationship to the treatment actually received, just as the goals of a treatment program can bear less of a relationship to the treatments actually delivered, or the policies of a probation department become distorted in the delivery of supervision to clients.

As one intervention in the criminal justice system, the sentence occupies a distinct position coming after the interventions of the police and prosecutors, but prior to those of prison officials, parole officers, and probation departments. A focus on the impact of any one of these interventions implies little about the effects of those preceding or following, unless a relatively simplistic measurement scheme is employed. However, an intervention at any stage is a necessary, though not sufficient, condition for subsequent interventions. In general, assessing the impact of any particular intervention may tell us little about the impacts of others. Moreover, as we become more precise in the conceptualization and measurement of a given intervention, it is likely to depart, often substantially, from the actual outcome at later points in the system.

In this light, an evaluation of the effects of sentences upon recidivism may tell us little about the impact of treatments (e.g., prison, probation, drug rehabilitation, and so forth) on recidivism. Conversely, if we study what actually happened to the individual (the treatment) as it relates to further criminal activity, we are unlikely to learn much about how the sentence received enhanced or reduced recidivism. Though treatment decisions are made by those other than judges, the judicial decision is perhaps the most important one as it specifies the types of treatment that are widely used in the CJS. Thus, the judge determines whether an individual serves any time in an institution or not, as well as if fines, probation, youth corrections, or adult state prison will constitute the treatment. Furthermore, the amount of the fine, the amount of time on probation, and the amount of time sentenced to incarceration are all initially set at the time of sentencing. As such, the judicial decision can be seen as a necessary condition for any treatment to occur.[14]

The "Time" Dimension.

Of the three major dimensions of the sentence, "time" is the most deceptive for it gives the illusion of lending itself to precise, detailed measurement. We can easily measure the amount of time to be served on the sentence (either in custody or under supervision). It is also possible to measure how much time was actually served after sentencing, thus yielding a specific component of the treatment received. The widespread use of time as either a measure of the sentence or the CJS treatment of the offender further

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[14] This is especially true for adult offenders such as those analyzed here. The majority of treatments received by adult offenders are mandated at the time of sentencing. Thus, the chain of events that leads to receiving some form of treatment will usually feature a conviction and sentence.

obscures what is being measured by each.

Consider first "time to be served" as one component of the sentence. While it is clearly a measure of part of the "sentence," it is a relatively poor approximation of the ideal type of what the sentence actually is in the eyes of the judge. Time to be served represents, among other things, expectations the judge holds for how long the offender will actually be in custody, a symbolic statement about the gravity of the sentence (and the offense), and some statement about the philosophy behind the sentence. For sentences involving incarceration, the amount of time to be served is a central consideration arising from policies derived from general deterrence, specific deterrence, incapacitation, and just deserts.[15] The exact intent behind the amount of time to be served cannot be ascertained without further, more specific information. Consequently, we view time to be served as falling about where the "x" lies in Figure 7.2. Indeed, all the measures of the sentence discussed earlier in this chapter are seen to fall in that general region. That is, they are relatively detailed, but less than ideal, indicators of the totality of the "sentence."

Time actually served is also a common indicator of treatment by the criminal justice system. But as with time to be served, time actually served is a poor approximation of the underlying ideal type concept. Of more importance is what happens <u>during</u> that time. If the goal of treatment is rehabilitation in particular, simply knowing how long the individual was in custody or under supervision tells us little about the experiences that may

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[15] Note that the purpose of long periods of time to be served differs depending upon the particular sentencing policy employed. A long sentence is primarily symbolic for general deterrence, simply just a long time in custody for incapacitation, and "equitable" under a just deserts philosophy. It may be a little of each if specific deterrence is the goal.

hinder or facilitate receiving the desired rehabilitation. This is not to deny the central importance of time served in custody or under supervision for studies of the criminal justice system or for the formulation of system policy. Rather, it is to suggest that after any incapacitative effect is acknowledged, what is measured by time served is not straightforward.

One of the central themes of this section is that as precision of the information increases the potential for a discrepancy between the judge's sentence and the treatment received increases. This discrepancy is most apparent for the time dimension. We can evaluate some aspects of the slippage between the sentence imposed by the court and what happened to the individual after sentencing along the time component of the sentence. For those sentenced to a state prison, the correlation between the time sentenced and that actually served is .508. Comparable correlations for those sentenced to jail or whose most serious sanction was probation are .392 and .482 respectively. Not surprisingly, there is a considerable difference between the time mandated by the court and that actually served by the offender. But how discrepant this is from what the judge thought the individual would actually serve is not known.

As was noted earlier, part of this discrepancy is due to decisions made by other criminal justice actors (e.g., parole boards) which more properly belong under the heading of "treatment" than "sentence." Shorter times served than sentenced may reflect appropriate behavior of the individual (i.e., "good time") which may be attributable, at least in part, to a positive effect of the sentence. Other differences between time sentence and time served are likely to be due to the interdependence of the sentence with other sanctions. Earlier in this chapter it was demonstrated that about 43% of the sentences

under study were confounded with either existing sanctions or the likelihood of future sanctions. This interdependence can produce a discrepancy between time sentenced and time served in several ways. For example, additional time on probation or in custody could come about from new sanctions arising from charges pending at the time the sentence under study was given.[16] There is no way to disentangle which aspects of time served are due to the sentence itself and which are attributable to factors other than the sentence and we shall not attempt to do so. As an indicator of one aspect of the sentence, time to be served is clearly preferable, while time served in the better indicator if the interest is in what actually happened to the individual.

The time dimension is also unique in that, depending upon the place sentenced, there is not a one-to-one correspondence between the sentence and the treatment, even at a very general level of detail. This was seen earlier for sentences to the Youth Complex where the majority of all times were to be of indeterminate length. Short of knowing what the judge expected the defendant to serve, there is know way of evaluating the extent of discrepancy of the judge's intent and how long the individual actually spent at the Complex. What is clear in these sentences is that the judge intended to blur the distinction between the sentence and the treatment, explicitly leaving the determination of time to be served in the hands of other criminal justice actors. This observation alone, however, is not a compelling argument for using time served as an indicator of the sentence for the actual amount of

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[16] In extreme cases, the discrepancy between time to serve on probation and time actually under the supervision of the probation department can be attributed to the offender's recidivism. This happens when an individual sentenced to probation (about half of the current sample) is reconvicted and the probation term is extended. As there is no way of knowing when one sentence to probation stops and another begins, the individual appears to serve a probation terms that is much longer than that given at the sentencing of interest here.

time spent in custody may be a function of more than simply the sentence under investigation here.

It is tempting to consider incorporating time served in the models to be developed in the next chapter. For both reasons of evaluation and policy, it is important to know what the impact of actual treatment is upon further criminal behavior. The use of time served would address these issues. Doing so would, however, take us further away from the focus upon an assessment of the effects of sentences at the expense of a measure that only poorly taps the actual treatment received. In addition, sentence and parole policies based on fixed, inflexible, times served are undesirable in that they would remove useful decision making power from CJS actors. Moreover, there is a logical reason for avoiding the use of time served with the other variables available for the analysis.

The Place of Independent Variables in the Sentence-Treatment Distinction.

One of the conclusions reached in Chapters Five and Six is that knowledge of variables from the domains of social structural, presenting offense, anamnestic theory, delinquent career/onset, prior offender behavior, and some general control variables is quite predictive of further criminal involvement measured in many different ways. These variables, taken from information available to the judge at the time of sentencing, are fixed at moment that the sentence is given. As such, they are contemporaneous to the sentence when used to predict recidivism after sentencing. Using them in the assessment of the impact of the sentence given the offender is not problematic.

A full study of the effects of the treatment received by the offender would have to incorporate any changes in these variables prior to assessing

the impact of the treatment. Some variables, for example, offender race and sex, age at first arrest, and earliest drug use, will not change after the point of sentence or after the release from the intervention treatment. Others, such as age, will naturally change over the course of treatment, while some, such as length at residence or living in an urban area, will be altered by simple geographic mobility. Recidivism will change the values of some anamnestic values (e.g., prior arrests or charges, prior number of incarcerations) and if the dependent variable is a rate or count of subsequent offending, these changes should be considered. Some aspects of the treatment intervention are designed to change the values of independent variables. In fact, a major goal of rehabilitation polices is to manipulate levels of drug use, employment, or education. Knowing that these levels had changed would comprise one aspect of a good measure of the treatment received.

To conduct a study of the impact of the treatment received as a consequence of the sentence would thus require data that are unavailable to us, and with the exception of small studies of particular treatments, generally unavailable when a wide range of sentences is studied. In fact, it is only when the individual recidivates that changes in these independent variables become known to the criminal justice system. Of particular concern is that failure to consider changes in these independent variables leads to a grossly misspecified model (Yamaguchi, 1991). Consequently, the results from a model assessing the effects of the treatment received could easily lead to faulty conclusions as pertains to both the effects of the independent covariates and the effects of the treatment actually received.

The few measures available to us are not sufficient to justify either an attempt at assessing the treatment received or incorporating some measures of

the actual treatment into models that assess the impact of the sentence upon subsequent recidivism. While we have exceeding good estimates of time under custody in either a state prison or at the Youth Complex, and can indicate the level of security at the institution -- a factor thought by many to be important in assessing the impact of prison environment (e.g., Street et al. 1966) -- we have no comparable measures for those sentenced to jail, or given probation or a fine. The lack of symmetry of treatment indicators across the full range of sanctions, coupled with a wide variety of independent variables whose potential change after the point of sentencing is unknown, reenforces the importance of the distinction between the sentence and the treatment and our decision to evaluate the former rather than the latter.

Consequences of the Distinction.

The exact consequences of evaluating the sentence as opposed to the actual treatment are hard to determine a priori, especially in the absence of detailed information about what actually happened to the offender. A reasonable expectation is that the actual treatment (or lack thereof) will be more predictive of recidivistic behavior than the general guidelines for that treatment as specified by the judge's sentence. Knowing, for example, the extent of supervision under probation would probably be more closely related to any recidivism than simply knowing if the offender was sentenced to probation. Similarly, knowledge of the kinds of job training received while in prison should more accurately reflect recidivism rates than the use of "only" the sentence to prison (with the judge's vague expectation that some form of job training might be received). From the perspective of the judge, the future prospects for the offender are inexact and maintaining a focus on the sentence itself preserves this uncertainty at the expense of our ability

to better predict recidivism.

Few studies have been done of the traditional forms (e.g., fines, probation, jail, and prison) of criminal justice intervention, nor have the different groups receiving these sanction been contrasted in terms of recidivistic behavior. This is unfortunate from a policy point of view in that most offenders who go through the criminal justice system do not experience the sometimes innovative programs that have been the focus of much of the evaluation literature. Rather, the vast majority receive the "routine" treatments as captured by the measures of the sentence described earlier. By viewing the sentence as a form of "treatment," distinct from the what actually happened to the offender as a consequence of that treatment, we are positioned to assess what the judge has done, but not how other criminal justice actors may have influenced the offender's later behavior.

Finally, it is possible to interpret many of our measures of the sentence as indicators of the treatment received by the individual. This is particularly true of the dummy indicators combining the in/out and where dimensions. The vast majority of those sentenced to prison do indeed serve time in a prison, those given probation are received by the probation department, and so forth. However, we caution against such an over interpretation for the reasons discussed above. Once aspects of the time to be served on these various sanctions are included in the analysis, we clearly are measuring the sentence and not what actually happened to the individual as a consequence of that sentence.

PATTERNS OF SENTENCING

Depending upon the particular goals of a sentence, the nature of the

sentence can be construed more broadly than simply along the dimensions of in versus out, where, and time. Judges may impose a sentence in light of the previous sanctioning history of the offender. At the time of sentencing, a judge is aware of the defendant's prior criminal record, including any previous convictions, incarcerations, or jail terms. Therefore, the judge is able to place any new sanctions in that context.[17] Is this the first time the offender has been sentenced? Has imprisonment been tried before and did it fail to deter the individual? If prior sanctions have been ineffective, is a more severe sentence required?

That judges take into consideration the individual's prior sanction history is clear from relatively informal sentencing practices. One of these is the use of progressive sentencing where more severe sanctions are tried after earlier ones have been unsuccessful in deterring or rehabilitating the offender. Another is the use of "shock" sentences which, at the time these data were collected, referred to a sentence of (brief) incarceration with the hope that the experience would deter the offender from future criminal involvement.[18] More formalized policies such as habitual offender statutes and some sentencing grid systems also implicitly incorporate the individual's response to previous sanctions as prescribed sentences become more severe with greater numbers of previous sentences.

Thus it is reasonable to expect that a judge's decision over the basic dimensions of the sentence is made in light of prior sanctions that may have been received by the offender. If this is true, the components of a sentence

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[17] Note, however, that advocates of just deserts (e.g., Von Hirsch, 1976) argue that the impact of sanction history on the sentence should be minimal.

[18] As will be clear shortly, these kinds of "shock" sentences were too infrequent in the sample to allow us to empirically evaluate them.

should not be seen in isolation from earlier sanctions. If the intent of a judge is to provide the first incarceration experience to the defendant, simply knowing that the sentence required incarceration will not capture this intent. Nor will knowledge that the sentence specified a term on probation reflect the fact that it was the initial probation term of the career, though the judge was fully aware of this in determining the final sentence.

Treating the sentence in isolation from prior sanctions is also likely to impair the ability to detect any effects of the sentence upon subsequent recidivism. Individuals who have been incarcerated before are apt to react differently than those receiving their first sentence of incarceration. As suggested by the anamnestic theory presented in Chapter Five, the prior behavior of the individual is a good predictor of future behavior. If offenders have already served time in prison, yet have, by definition, failed by virtue of being included in the present sample, an additional term in prison may be unlikely to deter future crime. In these instances, the intent of an incarcerative sentence is likely to be incapacitative. For those who have never been incarcerated, however, it is possible that some deterrence or rehabilitation can be produced by the time spent in custody. Whatever the reason or the effect, the sanction alone may not reflect these implications.

Thus we view the pattern of the sentence within the overall sanction history of the individual as an important component of the sentence itself. Judges position the particulars of the sentence within the prior sanction history, and, the consequences of a given sentence are likely to vary depending upon the sanctions that have preceded it.

Exactly how to measure the place of the 1976-77 sentence within the offender's sanction history is unclear. Table 7.5 provides some evidence of

the general characteristics of the sentence relative to previous sentences received. For just over 30% of the sample, the sentence studied here was the first one received.[19] Twenty percent of the sample received their first fine as a consequence of the 1976-77 sentence. Given the prevalence of probation sanctions in this sample, it is not surprising to find that 37.3% of these sentences represent the first time the offender was sentenced to probation. About twelve percent of the sample received their first jail sentence as a consequence of the sampling conviction. Despite the fact that almost 23% of these sentences were for incarceration at the Youth Complex or a state prison, this sanction represented the first prison term of the career for only 3.9% of the sample. Note too, that 22.2% of all sentences can been seen as falling into a progressive pattern as defined below.

A further understanding of these initial sanctions can be gained from the more detailed breakdown in Table 7.6. Probation terms constituted 69% of all initial sanctions, with probation in conjunction with a fine being the modal sentence for those receiving their first sanction. Not only is probation a common sentence for the first sanction of the career, this pattern of sentencing dominates the sample. One fifth of all sentences administered by the State Courts during this period were initial sanction probation terms.

The next most common initial sanction was a jail term given by itself or in conjunction with other sanctions; 15.4% of all initial sanctions involved a jail sentence. Relative to the entire sample, 3% of all sentences were

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[19] In determining this and other "firsts" of the offender's sanction history, we used the full range of data at our disposal. Official arrest histories were searched for prior probation terms and incarcerations in either jail or a state institution. Prior record information coded on the PSIs were used to supplement the official arrest data. All information on previous fines were taken from PSI data.

split sentences to jail as the first sanction in the individual's career with another 1.7% straight jail sentences (with or without a fine). The less severe sanctions of fines alone or a residual sentence constitute 4.8% and 1.6% of all initial sanctions, respectively, translating to about two percent of all of the sentences in the sample.

Custody at one of the state institutions, young adult or adult, constitute about ten percent of all initial sanctions with only a handful of sentences adding additional sanctions to the custodial terms. Viewed relative to all sentences given to this sample, less than three percent involved custodial terms that were the initial sanction.

While there is variability in exactly what constitutes the sentence given to those for whom the 1976-77 sentence was the first sanction of the career, it will not be possible to make distinctions beyond the simple dummy variable for whether or not the sentence is the initial sanction: Doing so would spread the data too thin across the various first sanctions. Rather, we must be content with an indication of the sentence being the initial sanction received by the offender.

The interpretation of any effects of this aspect of the sentence will be debatable. Minimally, a measure of initial sanction serves as a control on the other dimensions of the sentence allowing for detecting any effects beyond that of the initial sanction. If, for example, the deterrent effect of prison differs for those who have previously been incarcerated and those who have not, controlling for the initial sanction pattern will make the interpretation of prison sentences clearer. Maximally, first sanctions, independent of their content, may produce lower rates of recidivism. If this is true, then the initial sanction dummy variable could be interpreted as a substantive effect

of this aspect of the sentence.

The other sentencing pattern investigated here is a progression in sanctions received. As was shown in Table 7.5, 22.2% of all sentences given to this sample fall into a progressive pattern. If sanctions are ordered by the hierarchy of fine or less, probation, and incarceration of some form, a progressive pattern is one where only less severe sanctions had been imposed in the past and the current sentence imposes the next most severe sanction. Under this definition, there is some overlap between initial sanctions and progressive patterns, as the first sanction of the career could be considered "progressive." That is, if the offender had never been sentenced before and received a fine or probation as the first sanction of his or her career, a progressive sentencing pattern has been followed.

Table 7.7 details our operational definition of progressively patterned sentences for this sample. Almost half (49.2%) of all progressive sentences were given to those who had received no prior sanctions in their career. By far, the most common pattern in this group is no prior sanction to a term of probation. One quarter of all progressive sentences involve offenders who had previously received fines only and were given the initial probation sentence of their career in 1976-77. Once again the frequent use of probation in this sample dominates the sentencing patterns as 16.7% of all sentences in the sample are progressive sentences employing probationary status.

Almost one quarter of all progressive sentences involved the use of custodial sanctions; 11.1% of these were instances where the offender had previously been fined and served time on probation and the judge imposed the first jail term. Five percent of progressive sentences fit the pattern of incarceration at the Youth Complex or a state prison after previous sanctions

of probation or less.[20] The remaining six percent of all progressive sentences are those where the individual had been previously been fined, sentenced to probation, and served time in jail, and the current sentence imposed the first prison term of the career.[21]

As with the various forms of initial sanctions, the distribution of types of progressive sentencing patterns are not sufficiently frequent to support detailed distinctions. Thus, while we will be able to measure the fact that the sentence falls into a progressive pattern within the individual's history of prior sanctions, no attempt will be made to ascertain any effects of particular patterns of progressive sentencing. The specific types of progressive sentences constitute too small a percentage of the sentences given to this sample.

The results of this section indicate that, at a general level, it is possible to place the basic dimensions of the sentence in the context of any prior sanctions. Not all probation sentences are alike in that some may be the first sanction of the individual's career, others may given after the offender had previously been fined. Similarly, custodial sentences may be differentiated not only by where and how long the individual is to serve, but also by whether the custody is the first sentence received by the individual

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[20] The justification for defining these more severe forms of incarceration as "progressive" without first including a (less severe) jail term is the progression from noncustodial to custodial sanctions. From the perspective of the judge, the offender did not respond to earlier fines and probation, and the next sanction in the sequence is custody. Where the custody takes place is of less concern than the fact of custody itself.

[21] Note that certain sequences of sentencing are <u>not</u> defined as progressive. For example, sentencing an individual who had only been previously fined to jail or prison is not considered progressive as the intermediate sanction of probation had not occurred. Initial sanctions requiring custody are also not defined as progressive as the less severe sentences of fines or probations are not part of the sanction history.

or is being imposed after lesser sanctions have failed to deter or rehabilitate the offender. Whether such conceptual distinctions translate to quantifiable effects on recidivism is the empirical question to be answered in the next chapter.

INTERDEPENDENCE WITH OTHER SANCTIONS

Where a particular sentence fits in the sanction history of the offender is reasonably a characteristic of the sentence itself. However, to find differences in rates of recidivism across individuals given different sentences does not mean that the differences can be uniquely attributed to those sentences. In part, this is because the effects of a particular sentence are often confounded with those of other sentences. In this section, we briefly review the extent to which the 1976-77 sentence was interdependent with other sanctions.

At the time of sentencing, a judge is presumably aware of other sentences being served by the defendant. This allows for the sentence to be fashioned in light of these existing sanctions, though it also means that it can be difficult to separate the particulars of the new sentence from those currently being enforced. For example, a defendant who is already serving a probation term may be sentenced to additional time on probation, an individual currently serving time for an earlier conviction may be given additional time to serve after completion of the first sentence. In essence, the "new" sentence simply extends an earlier one and it is not possible to separate out the effects of each. No new supervisory or incarcerative experience is created by the new sentence and the time added by the new sentence simply extends that of the previous one.

The extreme case of interdependence among sentences is when it is built into the particulars of the sanction. In instances where sentences are to be given concurrent to other matters, the judge has explicitly merged the sentence with ongoing sanctions. In these situations the sentence adds little to the existing one as time in custody or under supervision is not extended and it can be argued that, in reality, no new sanctions are imposed.[22] Even though a new sanction may appear on the individual's record, the actual punishment added is minimal.

Even when a sentence is given to a defendant who is free of CJS supervision and custody at the time, it is possible that a judge is aware of the likelihood of future sanctions and constructs his or her sentence accordingly. Other charges may be pending or the offender may have detainers for earlier offenses. Here, while the actual sanctions to be levied in response to these charges or detainers may be unknown, there is the expectation that further CJS intervention will be forthcoming.

These scenarios all share the common feature that any effects for the new sentences are confounded by those of existing or subsequent ones. While there is a range of interdependence among the sanctions in these instances, in all cases there is some "contamination" of the new sentence being administered to the offender. Where an existing sentence stops and a new one begins is vague at best and this is true whether the sentence under study is the first or second of the sequence. Moreover, it appears to be a fruitless task to

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[22] In some cases the "new" sentence would appear to impose no sanctions at all. There are a couple of cases where 1976-77 sentence required a new term to probation, all of which was completed while the individual was imprisoned on some other conviction. Whether the judge expected the offender to be released shortly after sentencing, and constructed the sentence under this assumption, or not, practically speaking such a sentence can be expected to have little impact by itself.

disentangle the effects of one sanction from the other. Without a priori assumptions about what these effects might be, it is difficult to partition one sentence from another.[23]

While we are unable to determine the unique effects of a sentence which is interdependent with others, we can provide some evidence on the extent of this phenomenon. Table 7.8 presents several indicators of ongoing or likely future sanctions faced by this sample at the time of sentencing. Some of these measures (offender on probation or in prison at sentencing; offender has detainers or pending charges) were introduced in Chapter Five as they may also be conceptualized as characteristics of the presenting offense and past CJS involvement with the offender. It will be remembered that in both Chapters Five and Six, these variables were found to have significant impact on levels of post-sentence recidivism. We reintroduce them here as indicators of the interdependence of the 1976-77 sentence with other sanctions.

At the time of sentencing, one third of the sample was under CJS supervision of some form, and thus any effects of the new sentence would be coterminous with these existing sanctions. Almost twenty percent were serving a probation term at that time, ten percent were on parole, and seven percent were serving time in prison.[24] The conditional distributions of the most

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[23] There is yet another way to confound one sentence with another. If an individual is sentenced, unencumbered by existing or expected future sentences, and then recidivates, the original sentence is likely to become interdependent with any sentence resulting from the recidivistic crime. However, unlike the situations described above, the judge cannot be aware of these forms of future sanctions and thus is not able to take them into consideration when deciding upon a sentence. This serves as a reminder that the greater the level of recidivism, the more likely sentences are going to become confounded with additional sanctions.

[24] The percentages add to more than 33.6% as the individual sanctions are not exclusive. An offender could simultaneously be on parole and probation, for example. All of the specific indicators in Table 7.8 are not exclusive.

severe sanction added by the 1976-77 sentence are given on the right hand panel of Table 7.8. These distributions indicate that the judge's sentence was most likely to coincide with the existing sanctions. Those already on probation were likely to receive a probation term as the most severe sanction of the new sentence. Individuals on parole were apt to be returned to prison, and those already in prison were likely to get time added to the existing sentence.

Yet despite these similarities between the new sentence and those currently in force, there are also numerous discrepancies. Supervision, this time under the auspices of the probation department, was a common sentence given to those on parole at the time of sentencing. Some offenders in prison at sentencing were given jail or probation terms to serve upon completion of the prison sentence. About one fifth of probationers were sent to prison as a consequence of the new conviction.

Perhaps the clearest indication of a judge's intent to merge sanctions is the condition that the sentence be served in conjunction with others. As shown in Table 7.8, the stipulation that the sentence be executed concurrently or consecutively with other matters was used in 7.2% of all sentences. By far, these conditions were most likely to be specified for custodial sentences with the bulk of these being sentences to prison.

Over twenty-two percent of the individuals sentenced in this sample had charges pending or detainers on other matters. Thus, at the time of sentencing, there was a strong likelihood that the sentence given would become interdependent with one administered in the future.[25] The sanctions

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[25] In previous chapters we interpreted the effects of having pending charges and detainers are controls for the quality of our data. Those with pending charges or detainers may have subsequently served time out of state and thus

administered to these cases were more likely to employ custody than those given the sample as a whole, though the modal sentence for those with pending charges is still a term to probation.

Most surprising is the fact that nearly half (45.4%) of all sentences in this sample stood a least some chance of an interdependence with one given at some other point in time. Whether judges considered this when deciding upon the particulars of a given sentence is unclear, though in at least seven percent of the sentences the directives explicitly linked the sanctions with those of other sentences. The patterning of most severe sanctions within indicators of interdependence suggests that some attempt was made to match the new sanctions with existing sentences (e.g., those in prison at sentencing tended to receive additional or concurrent time to prison). However, these patterns can also be accounted for by the fact that the more serious, active offenders are more likely to be serving other sentences or have charges pending when they entered the current conviction sample.

As mentioned earlier, it is virtually impossible to separate the effects of the 1976-77 sentence from those existing (or likely future) sentences. The prevalence of interdependence with other sentences is quite high for this sample and this necessitates at least some minimal controls for the phenomenon. In preliminary analyses, it was found that after controlling for being on probation at sentencing, and having pending charges or detainers at sentencing, being sentenced concurrent, or consecutive, to other matters did not significantly improve the prediction of recidivism as measured in a

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these variables are associated with lower recidivism because further crimes were not reported to New Jersey authorities. From the perspective of the present chapter, an alternative explanation for the impact of these variables is that additional sentences, confounded with the one under study, were imposed and these new sanctions were effective in reducing recidivism.

variety of ways. Nor did parole status at sentencing or being in prison at the time of sentencing improve the predictive ability of the models. Empirically, the three indicators of interdependence used in Chapters Five and Six appear sufficient to relate the multiplexity of sentences to recidivism.

CORRELATIONS AMONG THE SENTENCE DIMENSIONS

With the eleven dummy variables representing the "in" versus "out" and "where" components of the sentence, the four continuous components, and the two indicators tapping sentencing patterns, we have seventeen variables that operationalize the dimensions of the sentences given this sample. Added to these are four controls for interdependence with other possible sanctions that can confound any effects of the sentence variables. In this section we assess the extent to which these components are intercorrelated. To reduce potential problems with collinearity all continuous components of the sentence (amount fined, months on probation, and time sentenced to jail or state prison) have been centered around their respective means. To simplify later interpretations, time sentenced to jail and state prison has been converted to months by dividing by thirty days.

The correlation matrix for our measures of the sentence is presented in Table 7.9. While the vast majority of these correlations are statistically significant, they are of surprisingly low magnitude. The highest correlations are between measures of time sentenced with the dummy variables for the corresponding places. Time sentenced to prison correlates .639 with the variable indicating a sentence to prison. Time sentenced to jail correlates between .35 and .50 with the various forms of jail sentences (jail alone, jail and probation, and jail and probation in conjunction with a fine).

Reflecting judges' tendency not to give split sentences to prison, months on probation correlates -.37 with a sentence to state prison and -.31 with a sentence to the Youth Complex at Yardville. Months to probation is also relatively highly correlated (.313) with the sentence of probation with conditions, suggesting that the more stringent form of probation is more likely to produce longer terms as well. The same may be said of the correlation between months to probation and the sentence of probation with conditions and fine (r=.264). Progressive sentences tend to involve sentences to probation only (r=.254) and initial sanctions are also positively correlated (.216) with a progressive sentencing pattern.

Some of the indicators of sentence interdependence have relatively high correlations with components of the sentence. Being in prison at sentencing correlates .292 with a sentence to state prison and .179 with a sentence to the Youth Complex. Having detainers at sentencing correlates .157 with a sentence to state prison. All four measures of interdependence correlate negatively with the initial sanction variable, as they should.

Yet, aside from these particular correlations, we find quite low intercorrelations among the various parts of the sentence when the sample as a whole is analyzed.[26] An exploratory factor analysis of this matrix confirms the multiplexity of sentence components as measured here. Eleven factors

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[26] The low correlations between the eleven dummy variables for the "in" versus "out" and "where" dimensions is, in part, an artifact of the sensitivity used to measure these components. We have attempted to keep a detailed measurement with a relatively uniform distribution across the various types of sentences. This will lower the bivariate correlation between any two indicators of the set. Together, however, the eleven dummy variables are exhaustive of the sentences given to this sample and thus the set is collinear. Omitting one of the infrequent sentences (fined only or lesser sanctions) does not reduce the collinearity among the remaining set. This leads us to use probation only as the omitted referent in the subsequent analyses.

emerged and many were not easily interpreted. The first factor was clearly the state prison elements of the sentence with the dummy variable for a sentence to state prison and months sentenced to state prison loading highly on this factor. The second factor was identified by months to probation and (the negative of) sentence to jail with no other sanctions. A sentence of jail with probation, and days sentenced to jail comprised the third factor, with a sentence to the Youth Complex at Yardville being the only variable that loaded highly on the fourth factor. Existing sanctions of prison and probation and the converse of being an initial sanction dominated the fifth factor. Lower order factors showed similar patterns of only a few variables loading on a particular factor.

The conclusion is that these components of the sentence are relatively distinct, and more is to be gained by maintaining distinct indicators than by combining them. While there is, by definition, some conceptual overlap between the dimensions of the sentence (e.g., a sentence to prison implies some time to serve in prison; people serving a prior sentence cannot receive their initial sanction when being resentenced), the empirical overlap is relatively low when the sample as a whole is analyzed. By keeping the detailed measurement of the dimensions of the sentence, we allow for the detection of those parts of a sanction that can be related to levels of recidivism.

INTERACTIONS WITH OTHER INDEPENDENT VARIABLES

The rationale for looking at interactions among the independent variables used in previous chapters extends to the sentence variables as well. We should not expect the impact of the sentence to be uniform across all types

of offenders because how the sentence is experienced may depend upon characteristics of the individual. For example, those who are incarcerated for the first time may react more strongly, and positively, to the sentence than do those for whom the sentence represents a reincarceration. The effects of initial sanctions may be greater for those who are experiencing their first contact with the criminal justice system than for those who have had prior contact (but have not been previously convicted). Those who are in need of additional treatment for some form of dependency may respond differently to a particular sanction than those who are not.

In a more general sense, social structural factors are likely to moderate any effects of the sentence. Those from higher classes are likely to perceive fines as less onerous than those from lower classes (if indeed fines are used at all for offenders who will be less likely to pay them). Having relatively strong ties to the community disrupted by an incarceration may serve as a greater deterrent than if the ties are weak. Individuals with a long history of criminal involvement stand less of a chance of being rehabilitated by a prison sentence. This suggests that the impact of the sentence may differ across the levels of prior record as measured by the sets of anamnestic, onset, and prior CJS action variables introduced in earlier chapters.

We contend that some interactions between the sentence variables used in this chapter and other characteristics of the offender are likely. There is, however, little theoretical or empirical guidance as to which aspects of the sentence are likely to differ according to particular independent variables. Theoretically important interactions cannot be specified <u>a priori</u>, nor are there previous findings that point to interactions with the dimensions of the

sentence as measured here. In the absence of strong reasons for limiting the search for how the effects of sentences may be modified by offender characteristics, we follow the same selection strategy for investigating interactions that was adopted in Chapter Five.

Interaction terms with each of the independent variables used in earlier chapters were constructed for all seventeen measures of the sentence. These interactions were then tested for extreme collinearity with their component variables.[27] After dropping highly collinear interactions, the remaining ones were regressed on the full set of 153 recidivism variables.[28] Terms which were significant in at least 35 percent of the 153 equations were deemed sufficiently stable to warrant inclusion in the final analyses.

The caveats to this approach raised earlier deserve reiteration here. By searching for global interactions across all dependent variables we are able to identify relatively robust interactions involving the sentence measures. While this allows us to focus on one basic equation for all dependent variables, with the interaction terms representing modifications to this equation for specific groups (in this case those sentenced to a particular sanction), the model estimated may not be the most appropriate for a particular group or a given dependent variable. As was seen in earlier chapters, some independent variables appear not to be significant in the

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[27]. For example, one interaction term was constructed by multiplying months sentenced to prison by prior number of arrests. The diagnostics techniques detailed by Belsley et al. (1980) were then used to ensure that there was not excessive correlations between the interaction and either months sentenced or prior arrests to preclude a meaningful estimation of parameters.

[28] To refresh one's memory, these 153 recidivism measures were: rearrest, measured over 17 half year windows; the other recidivism indicators of Chapter Five (save the time-to-failure indicators), measured over windows of one, three, five, and nine years; and the log of total charges measured over the four different windows.

results presented, though it is known that they would increase prediction if a different dependent variable were analyzed. Similarly, while some interaction terms are deleted by our procedures, if models were developed for particular subgroups, the effect for the variables might become significant within that subgroup.

For example, we find that being sentenced to state prison has relatively strong interactions with the offender's prior number of arrests as a juvenile. This indicates that the "effect" of receiving a prison sentence is, in general, not the same for those with lower numbers of juvenile arrests than for those with higher number of arrests as a juvenile. Rather, the prison experience is somehow different for individuals defined by this characteristic. Knowing, however, that this interaction is relatively robust across the myriad of ways of measuring recidivism does not ensure that it will be significant for a particular measure. For all binomial measures of rearrest, this interaction is not significant. If recidivism were conceptualized differently, (e.g., as a rate of offending, or by property crimes), this interaction may indeed be significant. Similarly, if separate models were constructed for groups with high and low numbers of juvenile arrests, we can, in general, expect the "effect" of prison to be different across those groups, though it may not be different for a particular dependent variable.

The converse of omission also holds. If the substantive interest is only one way of measuring recidivism, say as a rate of persons crimes over a three year window, then interactions that are not generally robust might prove to be significant for that dependent variable. For example, we do not find that the sentence interacts, in general, with the offender "type" defined by a

presenting offense of a crime against persons. Yet, if the focus is solely upon the rate of persons crimes per year over three years, it might well be found that offenders sentenced to prison for an instant offense of a crime against person have significantly different rates of recidivism than those sentenced to prison for a different kind of presenting offense. Thus while the search for interactions involving the sentence variables allows for some degree of certainty that important modifications to the sentence effects in the main model will be identified, its does not ensure either that these modifications will be relevant for a given dependent variable, nor that these modifications are <u>not</u> relevant for a given dependent variable. Rather, as with the interactions among the independent variables, we identify relatively robust interactions at the expense of relatively particular ones.

As before, the goal is to achieve some degree of parsimony in studying recidivism as defined broadly by numerous indicators. By focusing only upon one main model, with its robust main effects and interactions, we avoid proliferating equations for particular subgroups (e.g., offender or sanction types) and the need to test for whether coefficients differ across subgroups (e.g., does the effect of months sentenced to prison differ between drug offenders and other offenders). Such tests are naturally implied by the appropriate interaction terms. Moreover, a single model allows for a simpler contrast of recidivistic behavior of offenders given different sanctions. The cost of this parsimony, however, is a single model with many parameters, some of which are not significant for a given dependent variable and the omission of others that are significant for that variable.

Figure 7.3 lists those interactions between sanction measures and independent variables that were significant across at least 35 percent of the

dependent variables available for study. Of the possible seventeen sentence variables, ten were involved in relatively robust interactions with a total of fourteen interaction terms retained for the analyses.

The dummy variable for a sentence to state prison interacts with two measures of prior record - number of arrests as an adult and number of arrests as a juvenile. The interpretation of these and other sanction interactions is problematic as interactions are inherently symmetric. It is as legitimate to say, for example, that the effect of a state prison sentence depends upon the number of adult arrests accrued by the individual as it is to say that the effect prior adult arrests is contingent upon whether or not the offender was sentenced to state prison. Each is consistent with the basic fact that the interaction term has been found to be relatively robust across dependent variables. Finding that the number of prior arrests as a juvenile interacts with a state prison sentence implies both that the effect of prior juvenile arrests on recidivism is conditional upon whether the sentence is to prison and that the relationship between state prison sentences and recidivism varies by level of prior juvenile arrests.

Thus it is debatable whether these interactions can be interpreted as the "effects" of the sanction per se, as adjustments to any sanction effects, or as pre-existing expectations for recidivistic behavior that are not to be considered as the effect of the sanction. Using the state prison by prior adults arrests as an example, it could be argued that a) this is really a component of the sentence since part of the process of sentencing is to send those who are more likely to recidivate to prison and consequently it is the sentence that has created two distinct groups of offenders; b) how prison is experienced is different for those with many prior arrests than for the groups

defined by lower prior arrests, or; c) there is some main effect of being a prior record on the likelihood of recidivism and the extent of this effect is contingent upon the nature of the sanction.[29] Our decision is to treat these interactions as effects attributable to the sanction, interpreting the sanction-independent variable coefficients as modifications to the main effects of the sanction variables. We do so using the justification that sentence is a necessary part of the interaction term and without the sanction, there would be no interaction to be studied. However, when the variance in recidivism explained by the sanction variables is partitioned in the next chapter, we will keep the distinction between that attributable to the sentence indicators and that attributable to sanction interactions.

Returning to Figure 7.3, three significant interactions involving those sentenced to the Youth Complex have also been identified. All interactions with a sentence to Yardville relate to prior record and prior CJS interventions. Counts of prior adult convictions, charges in the past five years, and prior incarcerations are related to recidivism differently for those sent to the Youth Complex than for those given other sanctions.

One robust interaction was found for those given a split sentence to jail. Offenders who had been incarcerated at some time during the two years prior to sentencing tend to recidivate differently than those who were on the streets for the two year period prior to sentencing. Any effect of a simple

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[29] The inherent ambiguity is more evident in the common practice of selecting subgroups for analysis. If a subsample of those sentenced to prison were selected, the interaction term would be subsumed under the main effect of prior adult arrests and thus would not be considered an "effect" related to the sentence itself. Conversely, if a subsample of those with long prior records were selected for the analysis, the interaction would be incorporated into the effect of being sentenced to a state prison. Thus, when subgroups are defined for any analysis, the decision as to how to interpret sanction-independent variables has been made implicitly.

jail sentence appears to be moderated by drug use chronicity. Three interactions involving some measure of prior record with a form of probation sentence were also found. The impact of a sentence of probation with conditions, in conjunction with a fine, varies by level of prior adults arrests. The effect of probation (with no conditions) and a fine is conditioned by the number of prior Part 1 charges and those given a sentence of probation with conditions recidivate differently depending upon previous charge convictions as an adult.

The remaining interactions in Figure 7.3 involve sanction indicators other than those tapping the "where" dimension. The impact of the number of months sentenced to jail varies by whether or not the presenting offense was for a property crime. The effect of the length of time sentenced to prison was found to depend on the offender's prior number of convictions for property offenses. Finally, the effect of the initial sanction on recidivism differs for black offenders and varies according to the number of prior arrests as an adult.

The exact manner in which these variables moderate any effects of the sentence must await an inspection of the particular coefficients in the next chapter. However, some interpretation of what is behind these interactions can be offered. Unlike the interactions presented in Chapter Five, interactions with the sanction measures tend <u>not</u> to involve offender "types" as defined by race or the kind of presenting offense. Of the 14 interactions listed in Figure 7.3, one involves the race of the offender and one involves the type of presenting offense. In general then, while there is evidence for differences between types of offenders in terms of their subsequent criminality (the interactions of earlier chapters), there is much less

evidence that sanctions impact upon those types differently. Rather, the effect of a sentence (defined by the coefficient for a given sanction measure) seems to be relatively stable across those of different races, sex, and kinds of crimes leading to the sanction.

The theme that does emerge from the set of sanction interactions is that prior record mediates the sentence in important ways. With the exceptions of the interaction involving simple jail terms and months sentenced to jail, all of the sentence components listed in Figure 7.3 have at least one interaction with an indicator of prior record or response to prior sanctions. For those sentenced to either a State Prison or the Youth Complex, all five interactions indicate that the effect of the experience is mediated by the offender's prior criminal record or earlier sanctions. The same is true for the interactions involving some form of probation. The expectation is that "criminality" (broadly defined) conditions the impact of the sanction received. The more "hardened" the individual is, the less likely the sentence is to have an effect.

A PRELIMINARY LOOK AT THE EFFECTS OF SENTENCES

The sentencing of an offender can be viewed as a matching process. Under a retributionist model of sentencing, the characteristics of the presenting offense (and possible some aspects of prior criminality) are matched to the sanction received. The use of rehabilitative goals adds structural variables to the mix in an attempt to match more of the characteristics of the offender to the sentence received. Incapacitative goals implied a greater reliance upon the offender's prior record and response to prior CJS interventions when deciding upon the parameters of the sentence

to be administered. Even under the goals of general or specific deterrence, the nature of the presenting offense and the past criminality of the individual are matched to the severity of the sanctions imposed by the judge. The principals used to match offenders to sentences are thus determined by the goals of sentencing.

One consequence of this matching is that the more serious offenders, defined by either prior criminal involvement or the nature of the presenting offense, receive the more severe sanctions. A priori, we know then that the groups defined by type of sanction will not be strictly comparable. Those with longer periods of criminal involvement, more previous arrests, more prior sanctions, and convictions for certain kinds of offenses (e.g., serious persons crimes, property crimes such as burglary) are apt to receive the more severe sanctions. In short, there is the initial expectation that recidivism should be positively related to the severity of the sanction with greater levels of post-sentence criminal behavior to be found among the groups receiving incarcerative sentences.

Table 7.10 bears out this expectation. Over the full post-sentence observation period, 62.2% of those sentenced to prison are rearrested upon release. Almost 80 percent (78.1%) of those sentenced to the Youth Complex at Yardville are rearrested. This compares with an overall average of 54.9% rearrested and, in the bivariate crosstabulation of type of sentence by proportion rearrested, we find significantly more offenders sentenced to prison or the Youth Complex being rearrested than would be expected based on the marginal distributions of that table.

The general principal that the more serious offenders, defined by the type of sentence received, are more likely to recidivate holds for other

categories of the sentence as well. With the exception of those given jail, probation, and a fine, all individuals receiving some form of custodial sentence are rearrested more than would be expected by chance. Those given sentences involving some form of probation in conjunction with a fine, simple probation, a fine only, or one of the residual sanctions, recidivate at significantly less than chance levels. Among those who are "out" after sentencing, it is individuals who received probation with conditions, or probation only who recidivate at higher than expected levels, though the proportion rearrested from these groups does not reach that observed among those given custodial sentences.

It is against this backdrop that any effects of sentences must be evaluated. Over a long period of time, those given more severe sanctions are more likely to recidivate, and this cannot be interpreted as an "effect" of the sentence itself.[30] Rather, this long-term difference in rearrest rates reflects initial differences between the groups of offenders, differences produced by the sentencing process itself. Therefore, results (like those of Table 7.10) that do not somehow control for these differences should be interpreted cautiously. In particular, the battery of independent variables used in Chapters Five and Six should be considered. This is done in Table 7.11.

Logistic regressions for the proportion rearrested over each of the sixteen half-year windows are shown in Table 7.11. These models differ from those of Chapter Five only by the inclusion of the sentence measures and their interactions. That is, the independent variables, and the robust interactions

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[30] Only if one postulates some criminogenic effect of incarceration can the sentence be said to produce higher levels of recidivism. Results to be presented shortly argue against this interpretation.

among them, are identical to those used earlier. Consequently, the discussion here will focus on the coefficients for the sanction indicators and their interactions.

The group of offenders sentenced to State Prison are significantly less likely to be rearrested over all the observation windows. During the first three half year windows, the coefficient for the prison indicator is negative and significant at the .001 level. The magnitude of the coefficient for a State Prison sentence varies over the observation periods as well. Receiving this sentence decreases the probability of rearrest during the first half-year after release by .0739 and this decrease rises to .1256 after one and a half years and then levels off to about .10 after three years.[31]

The group sentenced to jail, probation, and a fine also is significantly less likely to recidivate. After the first half-year following release, the coefficient for this sanction becomes significantly negative, going as low as -.1682 for the six and a half year window. Offenders sentenced to simple jail terms are also generally less likely to be rearrested than the reference group of simple probationers. However, the patterning of coefficients for the jail variable is less consistent, fluctuating between nonsignificance and a significant decrease in the probability of rearrest of about seven percent. The group receiving only fines is also less likely to recidivate, again with a decrease in the probability of rearrest of about -.10, though this effect does

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[31] The exact magnitude of these and other effects should be interpreted cautiously as these are calculated at the mean of the dependent variable. As we have seen, the proportion rearrested gradually increases with the width of the post-sentence observation window. As a result, these effects on the probability of rearrest are evaluated at different means across the equations of Table 7.11. It is safer to note the pattern and sign of the effects than it is to strictly interpret their magnitude. However, we have also recalculated the coefficients for these models using a common, grand mean for all windows and have found that their magnitude is not appreciably different from the values shown in Table 7.11.

not emerge as statistically significant until after a period of six and one half years.

The other sanction indicators combining the "in" versus "out" and "where" dimensions of the sentence show essentially no effect upon the probability of rearrest and this conclusion is consistent across the various post-sentence observation windows. Subject to the interactions discussed below, the probability of recidivating does not differ among those given split sentences and those given probation either by itself or in conjunction with fines and/or conditions on the probation. In essence, once the independent variables representing the various domains of the etiology of crime and prior criminal history are controlled, the groups represented by these particular sanctions are all rearrested at about the same rate.

Among the indicators of the continuous components of the sentence, only the months sentenced to a State Prison has a consistently significant effect which emerges after a three year period. While the exact magnitude of this effect varies in part as a function of the proportion of the sample rearrested at any point in time, it appears that each month sentenced to a state prison (but not necessarily each month served) decreases the probability of rearrest by about .01.[32]

The indicators of the patterning of the sentence are generally poor predictors of the probability that the offender will be rearrested. The indicator for the sentence following a progressive pattern does not emerge as

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[32] The magnitude of this effect, especially over the shorter windows is relatively small. For example, after a four year period, the coefficient for months sentenced to prison is -.0004. Thus a typical one year sentence would be expected to reduce the probability of rearrest by about -.0048. Exceedingly long terms would be needed to produce appreciable decreases in the probability of rearrest.

significant in any of the sixteen equations of Table 7.11. Sentences that are the first sanction of the career would also seem to be generally ineffective. For the half year immediately after sentencing and the window two and one half years after sentencing, those for whom the 1976-77 sentence was the first sanction of the career are significantly more likely to be rearrested than those given simple probation.

The main effects for some sanction measures are clearly moderated by the interactions shown in Table 7.11. For those given a sentence to the Youth Complex, each prior conviction as an adult decreases the probability of rearrest by about .01, but only for a period of up to one year after release. Another short term effect is found for the interaction of a Youth Complex sentence with prior number of incarcerations. Here, each prior incarceration increases the probability of rearrest by about .02 over a two year period. Together, these interactions tend to cancel each other.

Decreases in the probability of rearrest result from interactions of the various probation sentences with measures of prior record. Each prior adult arrest decreases the likelihood that an offender sentenced to probation with conditions and fined with be rearrested between the period of one to four years after sentencing. Each charge conviction as an adult decreases the probability of rearrest by about .01 for those given a sentence of probation with conditions.

Property offenders sentenced to jail are differentially rearrested depending upon the number of months sentenced. Each month sentenced decreases the probability of rearrest by .0001 for this subgroups of offenders. However, as with the main effect of months sentenced to prison, the magnitude of this interaction is exceedingly small.

The remaining interaction of note involves initial sanctions by the number of prior arrests as an adult. This interaction is significant across all sixteen half-year windows. The term is positive with an increase of about .05 for every prior adult arrest that goes unsanctioned in some form. The interpretation of this term is straightforward. The more an offender is arrested with no consequences, the less effective the first sanction will be.

Table 7.12 replicates the results for the previous table, this time controlling for sample selection hazards. In essence, there are few differences between the findings of Table 7.11 and those of Table 7.12. The conclusions reached above hold, even when potential biases in the sample are considered. The main effect of the selection hazards upon the coefficients for the sentence components and their interactions is minimal, usually resulting in a change in the third or fourth decimal place. The largest effect is a very slight suppression of the initial sanction by offender is black interaction, but even this is negligible. Overall, the impact of the controls for sample selection is more noticeable on the coefficients for the independent variables than on those for the sentence measures.

SUMMARY

The present chapter has focused on many aspects of the sentence that led to inclusion in the 1976-77 sample. Several key dimensions of that sanction were operationalized and descriptive statistics for them have been presented. Also introduced were two indicators of the patterning of the sentence capturing whether the sanctions were progressive within the offender's overall history of involvement with the CJS and whether the sentence under consideration was the first of the individual's career.

What can be called a "judge-centric" perspective has characterized our approach to both measuring the sentence and to searching for any effects of that sentence. From this perspective, the sentence is conceptually distinct from the treatment received as a consequence of it. Thus, for example, time sentenced rather than time served becomes the appropriate measure of that component of the sentence. The advantage of this approach is that it is closer to what judge's actually do in constructing sanctions that they deem appropriate. The sentence sets the conditions for what subsequently happens to the offender, but the actual treatment received as a consequence of the sentence is in the hands of other CJS actors. What happens to the offender (the "treatment") may bear little resemblance to the sanction originally mandated (the "sentence"). As discrepancies between the treatment and sentence increase (e.g., offenders serve more or less time than originally sentenced, prison terms are served in jail rather than in a state institution) it becomes questionable whether the treatment received is equivalent to the sentence given by the judge.

While the maintaining this distinction places us closer to the sentence per se, it is at the expense of evaluating the effects of the treatments received. Ultimately, this is the more important question since knowledge of any deterrent effects of incarceration, or the positive influence of job training or drug rehabilitation programs, will guide future sentencing policy. However, strong evidence in support of these treatments has not, to date, been forthcoming in the literature. In the absence of treatments that have uniformly positive effects, the current judge-centric perspective can be seen as evaluating the general parameters of the sentence. Does the sentence, guided by the particular goals of sentencing, put the individual in position

to receive appropriate treatments?

Critics may argue that the judge-centric nature of our design contains a fatal flaw. A preferred approach would be to have detailed information on the nature of the probation or imprisonment programs. Yet, even if such detailed information were available, it could still be argued that knowledge of whether the program delivered on its premises, whether the individual truly participated, and so forth was still absent. (Many research effects in the program evaluation literature have faltered because of the lack of such specific information.) Our response to these criticisms is to note that our purpose is to ascertain whether the broad categories of CJS interventions, <u>at the point of sentencing</u>, make any difference in levels of recidivism. Whether the success or failure of these interventions (i.e., sentences) is due to some unmeasured attribute of the subsequent treatment received as a consequence of the sentence cannot be determined here. In the context of a literature that says nothing matters, any finding of an effect is worth noting and becomes subject to further inquiry in subsequent research.

One unanticipated finding of this chapter is the fact the nearly half of the sentences under study were confounded by other sanctions being served at the time or stood a strong likelihood of being contaminated by additional sanctions in the near future. This interdependence of sanctions was independent of any new sentences introduced as a consequence of recidivistic behavior on the part of the individual. Rather, at the time the sentence was decided upon by the judge, oversight due to parole or probation, incarceration from previously existing sentences, or the possibility of new sentences as a consequence of detainers or pending charges were apt to influence the impact of the 1976-77 sentence. While we are able to control for variables

representing the confounding sanctions, attributing effects to the 1976-77 sentence alone is difficult.

Looking at the proportion rearrested by the "in" versus "out" and "where" indicators reveals that those given the most severe sanctions are most likely to be rearrested. Over sixty percent of those sentenced to a state prison are eventually rearrested and the percent is higher for those sent to the Youth Complex. These figures set the stage for the paradox that emerges from the preliminary look at the effects of sentences in this chapter. Once the battery of independent variables is controlled, we find that being given some form of prison sentence significantly reduces the probability of rearrest. This effect, as measured by the coefficients in the logistic regressions, holds over a much longer period of time for those sentenced to prison than for those sent to the Youth Complex. In light of the high proportions rearrested from these two groups of offenders, the effect of these sanctions appears to only "slow down" the criminal behavior to be expected given the seriousness of these individual's prior record. Those sentenced to prison are the more serious offenders and this becomes evident in the extent to which they are rearrested.

Though mediated by several important interactions, the groups comprised of any form of probation, split sentences without fines, and simple jail sentences appear indistinguishable once social structural factors and prior record are controlled. It is only for the more severe sanction of a prison term significant, negative coefficients are observed. This finding, borne out by the more detailed analyses of the next chapter, is supported by the consistent negative effect of the time sentenced to prison. The implication is that "getting tough" works, but only as a relatively short term deterrent.

Perhaps the greatest weakness of the judge-centric perspective is our inability to determine precisely what it is that produces the observed negative coefficients seen in this preliminary analysis. The experiences resulting from the sentence (the "treatment") would appear beneficial, at least to some individuals. Some components of the sentence are significantly related to the sentence in desirable ways. However, whether this benefit accrues from specific deterrence, job training, drug rehabilitation, or some other aspect of the experience cannot be determined from the data available.

In summary, the measures operationalizing the sentence and the manner in which they are to be treated have been the focus of this chapter. For the probability of rearrest, at least, some promising findings have emerged from the preliminary analysis. These findings are examined further in the next chapter where additional measures of recidivism are analyzed.

Figure 7.1

The Basic Dimensions of the Sentence

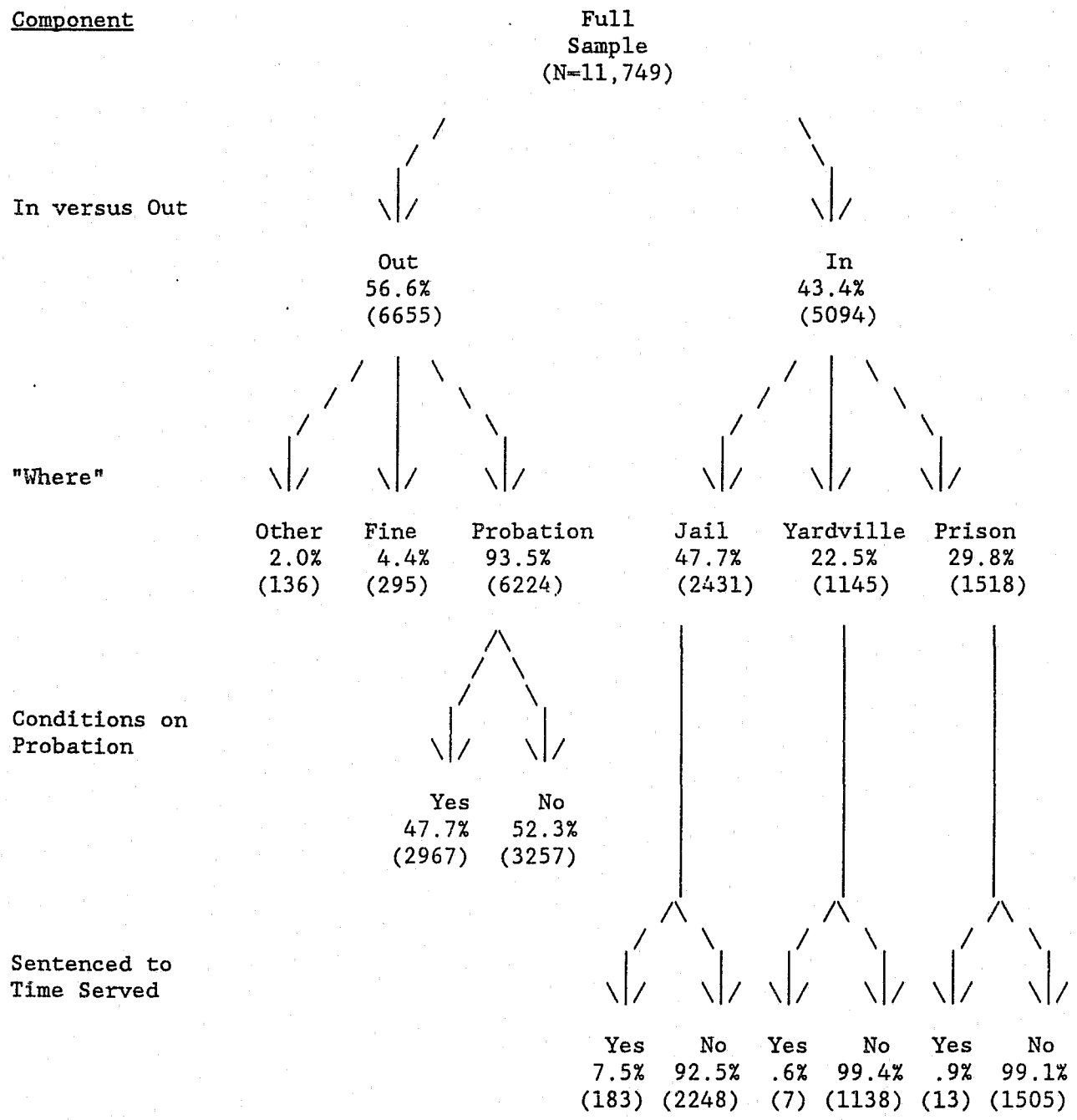

Figure 7.2

Discrepancies Between the Sentence and the Treatment

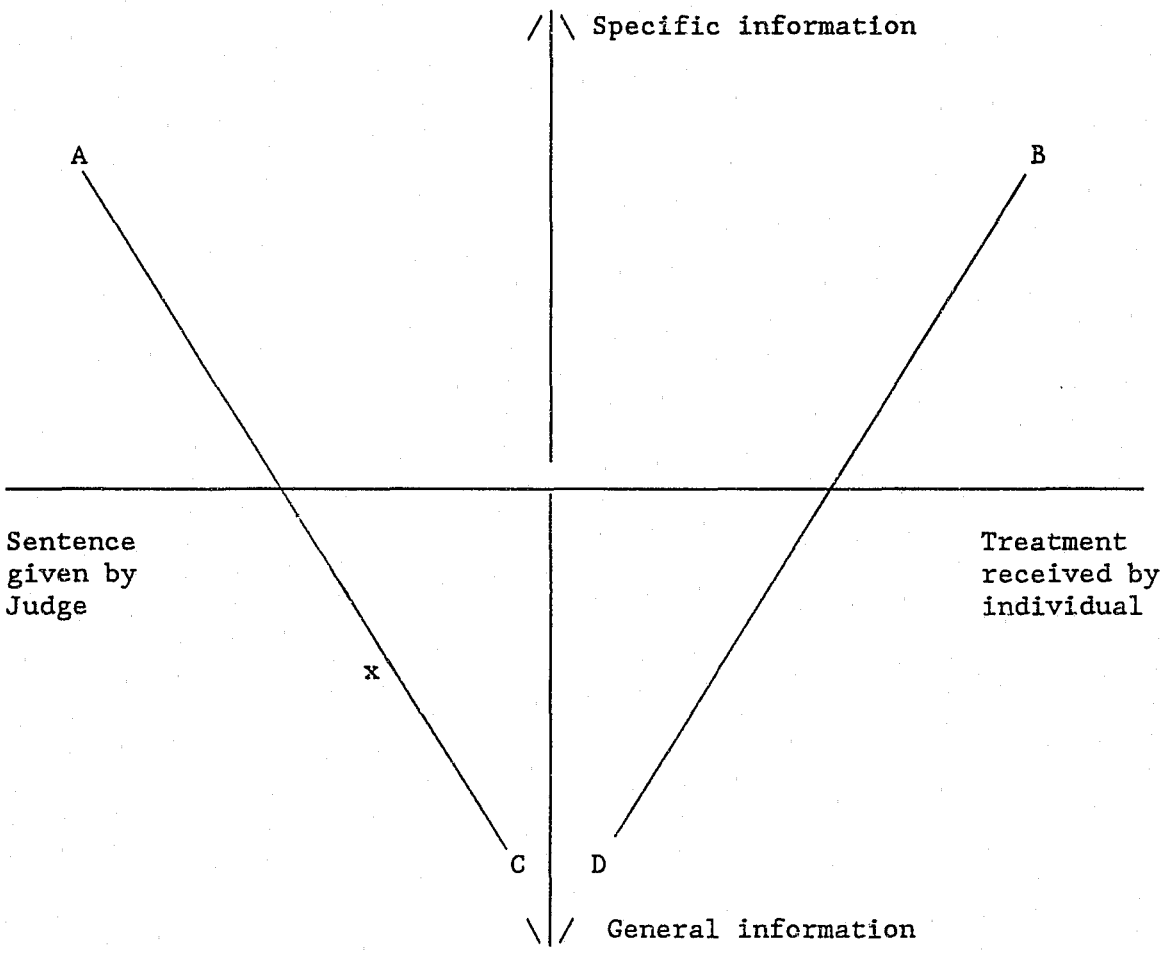

Figure 7.3

Interactions with Sentence Variables

Sentence Component	Interacts with
Sentenced to State Prison	Number of arrests as an adult Number of arrests as a juvenile
Sentenced to Youth Complex	Number of convictions as an adult Number of charges in last five years Number of prior incarcerations
Sentenced to Jail and Probation	Incarcerated over part of last 2 yrs
Sentenced to Jail	Number of years using drugs
Sentenced to Probation with Conditions and Fined	Number of arrests as an adult
Sentenced to Probation and Fined	Number of prior Part 1 charges
Sentenced to Probation with Conditions	Number of charged convictions as an adult
Months to Jail	Presenting offense for property
Months to Prison	Number of prior property convictions
Initial Sanction	Offender is Black Number of arrests as an adult

Table 7.1

Sanctions Administered
(N=11,749)

Sanction Given as Part of Sentence	Percent of Sample Receiving Sanction
Lesser sanctions	1.2% (136)
Fine or resititution	34.8% (4086)
Probation - All Kinds	63.1% (7412)
Probation - With Conditions	29.0% (3404)
Jail	20.7% (2431)
Youth Complex at Yardville	9.7% (1145)
State Prison	12.9% (1518)

Table 7.2

Most Severe Sentence by Additional Lesser Sanctions
(N=11,749)

Additional Sanctions	Most Severe Sanction: Fine/ Restitution Or Less	Probation	Jail	Youth Complex at Yardville	State Prison
None	100.0% (431)	23.7% (1478)	51.2% (1244)	95.9% (1098)	94.9% (1441)
Fine		28.6% (1779)	2.7% (65)	.8% (9)	3.2% (49)
Probation			18.3% (446)	1.5% (17)	.7% (10)
Probation w/ Conditions		24.7% (1537)	10.0% (237)	1.2% (14)	.5% (8)
Fine and Probation			11.0% (267)	.4% (5)	.4% (6)
Fine and Probation w/ Conditions		23.0% (1430)	7.1% (172)	.2% (2)	.3% (4)
Total	3.1% (431)	53.0% (6224)	20.7% (2431)	9.7% (1145)	12.9% (1518)

Table 7.3

Indeterminate and Life Terms
(N=11,749)

Indeterminate Term	Percentage of Sample	Conditional Percentage
Any indeterminate term	8.1% (950)	
To probation	2.4% (280)	3.8%
To jail	.0% (5)	.2%
To Yardville	5.4% (634)	55.4%
To State Prison	.3% (35)	2.3%
Life sentence to prison	.1% (7)	

Table 7.4

Summary Statistics for Continuous Components of the Sentence

Most Severe Sanction	Component: Fine (Dollars)	Probation (Months)	Jail (Days)	Prison (Days)
Fine/Resitution/Less				
Mean	504.54	-	-	-
Median	250.00	-	-	-
Minimum	21.00	-	-	-
Maximum	20,500.00	-	-	-
Standard Deviation	1,358.35	-	-	-
Skewness/Std. err.	11.62/.14	-	-	-
Valid N/Missing	296/0	-	-	-
Probation				
Mean	799.35	27.06	-	-
Median	500.00	24.00	-	-
Minimum	25.00	3.00	-	-
Maximum	58,000.00	87.00	-	-
Standard Deviation	1,991.84	13.90	-	-
Skewness/Std. err.	16.02/.04	1.00/.03	-	-
Valid N/Missing	3,109/0	6,136/88	-	-
Jail				
Mean	1,140.30	24.59	339.37	-
Median	500.00	24.00	364.00	-
Minimum	21.00	3.00	0.00	-
Maximum	80,150.00	68.00	4,927.00	-
Standard Deviation	4,607.74	11.76	290.75	-
Skewness/Std. err.	14.96/.11	.91/.07	4.40/.05	-
Valid N/Missing	480/24	1117/5	2,431/1	-
Prison				
Mean	4,493.97	35.51	-	2,778.76
Median	1,000.00	36.00	-	1,825.00
Minimum	100.00	12.00	-	0.00
Maximum	75,000.00	60.00	-	40,880.00
Standard Deviation	10,539.41	15.50	-	3,120.38
Skewness/Std. err.	5.14/.29	.34/.30	-	5.50/.06
Valid N/Missing	67/8	63/4	-	1,518/0
Total Sample				
Mean	881.31	26.76	339.37	2,778.76
Median	500.00	24.00	364.00	1,825.00
Minimum	21.00	3.00	0.00	0.00
Maximum	80,150.00	87.00	4,927.00	40,880.00
Standard Deviation	2,816.66	13.67	290.75	3,120.38
Skewness/Std. err.	17.81/.04	1.01/.03	4.40/.05	5.50/.06
Valid N/Missing	3,952/135	7,316/100	2,430/1	1,518/0

Table 7.5

Patterns of Sentencing
(N=11,749)

Sentence Received is:	No	Yes
First sanction of career	69.4% (8152)	30.6% (3597)
First fine of career	79.7% (9363)	20.3% (2386)
First probation term of career	62.7% (7365)	37.3% (4384)
First jail term of career	87.6% (10291)	12.4% (1458)
First prison term of career	96.1% (11292)	3.9% (457)
Progressive	77.8% (9143)	22.2% (2606)

Table 7.6
Patterns of Initial Sanctions
(N in parentheses)

Most Severe Sentence	Additional Sanctions	Percentage of Initial Sanctions (3597)	Percentage of All Sentences (11749)	N
Other	-	1.6%	.5%	59
Fine	-	4.8%	1.5%	173
Probation	None	30.4%	9.3%	1092
	Fine	38.6%	11.8%	1390
Jail	None	5.0%	1.5%	181
	Fine	.6%	.2%	21
	Probation	4.5%	1.4%	161
	Probation and Fine	5.3%	1.6%	190
Yardville	None	4.5%	1.4%	161
	Fine	.1%	.0%	4
	Probation	.2%	.0%	6
	Probation and Fine	.1%	.0%	2
Prison	None	3.6%	1.1%	131
	Fine	.5%	.2%	19
	Probation	.1%	.0%	2
	Probation and Fine	.1%	.0%	5

Table 7.7

Patterns of Progressive Sentences
(N in parentheses)

Prior Sanctions	Sentence Received	Percentage of: Progressive Sentences (2606)	All Sentences (11749)
None	Fine or less	6.6% (173)	1.5%
	Probation	42.6% (1111)	9.5%
Fine only	Probation	25.7% (671)	5.7%
Fine and probation	Jail	11.1% (290)	2.5%
	Yardville	5.1% (133)	1.1%
	Prison	2.8% (74)	.6%
Fine, probation and jail	Yardville	3.3% (86)	.7%
	Prison	2.6% (68)	.6%

Table 7.8

Interdependence with Other Sanctions
(N=11,749)

Evidence of Other Sanction	Percentage of Sample		Condition Percentage Most Severe Sanction: Fine/ Less	Prob.	Jail	Pris.
Prior sanctions still in force	33.6% (3951)		1.8	38.1	20.4	39.7
Offender is on probation at time of sentencing		19.9% (2337)	1.7	47.6	22.4	21.3
Offender is on parole at time of sentencing		10.3% (1207)	.8	32.3	25.5	41.4
Offender is in prison at time of sentencing		7.1% (833)	3.2	13.7	5.8	77.3
Sentence is concurrent/ consecutive to other matter	7.2% (851)		.9	.0	17.4	81.7
Future sanctions likely	22.4% (2633)		2.0	39.4	23.4	35.2
Offender has detainers		6.5% (761)	1.7	19.3	27.2	51.8
Offender has pending charge		20.0% (2345)	2.0	42.2	22.6	33.1
Any interdependence of sanctions	45.4% (5338)		2.0	40.0	22.0	36.0
Total	100.0% (11749)		3.7	53.0	20.7	22.7

Table 7.9

Intercorrelations Among the Components of the Sentence
(N=11,749)

	1)	2)	3)	4)	5)	6)	7)	8)	9)	10)	11)
1) Prison	1.000	-.127**	-.076**	-.096**	-.136**	-.143**	-.163**	-.150**	-.146**	-.062**	-.041**
2) Youth Complex	-.127**	1.000	-.065**	-.082**	-.116**	-.122**	-.139**	-.127**	-.125**	-.053**	-.035**
3) Jail, Pb & Fn	-.076**	-.065**	1.000	-.049**	-.070**	-.073**	-.083**	-.077**	-.075**	-.032**	-.021*
4) Jail & Prob.	-.096**	-.082**	-.049**	1.000	-.088**	-.093**	-.105**	-.096**	-.094**	-.040**	-.027**
5) Jail Only	-.136**	-.116**	-.070**	-.088**	1.000	-.132**	-.150**	-.137**	-.134**	-.057**	-.038**
6) Prob w/Cond &Fn	-.143**	-.122**	-.073**	-.093**	-.132**	1.000	-.157**	-.144**	-.141**	-.060**	-.040**
7) Prob and Fine	-.163**	-.139**	-.083**	-.105**	-.150**	-.157**	1.000	-.164**	-.160**	-.068**	-.045**
8) Prob w/Cond	-.150**	-.127**	-.077**	-.096**	-.137**	-.144**	-.164**	1.000	-.147**	-.063**	-.042**
9) Probation only	-.146**	-.125**	-.075**	-.094**	-.134**	-.141**	-.160**	-.147**	1.000	-.061**	-.041**
10) Fined only	-.062**	-.053**	-.032**	-.040**	-.057**	-.060**	-.068**	-.063**	-.061**	1.000	-.017
11) Other sanctions	-.041**	-.035**	-.021*	-.027**	-.038**	-.040**	-.045**	-.042**	-.041**	-.017	1.000
12) Dollars fined	-.026**	-.058**	.101**	-.045**	-.056**	.160**	.078**	-.070**	-.069**	.019*	-.019*
13) Months to Prob	-.370**	-.309**	.099**	.107**	-.355**	.264**	.182**	.313**	.191**	-.162**	-.107**
14) Months to Jail	-.142**	-.121**	.351**	.499**	.346**	-.137**	-.156**	-.143**	-.140**	-.059**	-.039**
15) Months to Prison	.639**	-.081**	-.049**	-.061**	-.087**	-.092**	-.104**	-.096**	-.093**	-.040**	-.026**
16) Initial sanct	-.169**	-.111**	.060**	-.043**	-.117**	.106**	.138**	-.015	.082**	.098**	.028**
17) Progressive	-.107**	-.024**	-.053**	-.046**	-.096**	-.080**	-.073**	.138**	.254**	.139**	-.057**
18) Has detainers	.157**	.080**	-.028**	-.015	.083**	-.088**	-.103**	-.018	-.051**	-.038**	.008
19) Pending charges	.081**	.085**	-.033**	.016	.039**	-.075**	-.084**	.031**	-.028**	-.043**	-.015
20) On prob at sent	.003	.091**	-.032**	-.002	.048**	-.051**	-.077**	.053**	-.001	-.052**	-.015
21) In pris at sent	.292**	.179**	-.047**	-.056**	-.061**	-.087**	-.103**	-.050**	-.080**	-.026**	.027**

	12)	13)	14)	15)	16)	17)	18)	19)	20)	21)
1) Prison	-.026**	-.370**	-.142**	.639**	-.169**	-.107**	.157**	.081**	.003	.292**
2) Youth Complex	-.058**	-.309**	-.121**	-.081**	-.111**	-.024**	.080**	.085**	.091**	.179**
3) Jail, Pb & Fn	.101**	.099**	.351**	-.049**	.060**	-.053**	-.028**	-.033**	-.032**	-.047**
4) Jail & Prob.	-.045**	.107**	.499**	-.061**	-.043**	-.046**	-.015	.016	-.002	-.056**
5) Jail Only	-.056**	-.355**	.346**	-.087**	-.117**	-.096**	.083**	.039**	.048**	-.061**
6) Prob w/Cond &Fn	.160**	.264**	-.137**	-.092**	.106**	-.080**	-.088**	-.075**	-.051**	-.087**
7) Prob and Fine	.078**	.182**	-.156**	-.104**	.138**	-.073**	-.103**	-.084**	-.077**	-.103**
8) Prob w/Cond	-.070**	.313**	-.143**	-.096**	-.015	.138**	-.018	.031**	.053**	-.050**
9) Probation only	-.069**	.191**	-.140**	-.093**	.082**	.254**	-.051**	-.028**	-.001	-.080**
10) Fined only	.019*	-.162**	-.059**	-.040**	.098**	.139**	-.038**	-.043**	-.052**	-.026**
11) Other sanctions	-.019*	-.107**	-.039**	-.026**	.028**	-.057**	.008	-.015	-.015	.027**
12) Dollars fined	1.000	.136**	-.005	.048**	.102**	-.025**	-.018*	-.033**	-.043**	-.014
13) Months to Prob	.136**	1.000	-.040**	-.240**	.113**	.045**	-.130**	-.056**	-.011	-.186**
14) Months to Jail	-.005	-.040**	1.000	-.091**	-.063**	-.088**	.029**	.042**	.032**	-.078**
15) Months to Prison	.048**	-.240**	-.091**	1.000	-.083**	-.072**	.112**	.057**	-.008	.170**
16) Initial sanct	.102**	.113**	-.063**	-.083**	1.000	.216**	-.131**	-.162**	-.280**	-.159**
17) Progressive	-.025**	.045**	-.088**	-.072**	.216**	1.000	-.071**	-.050**	-.062**	-.117**
18) Has detainers	-.018*	-.130**	.029**	.112**	-.131**	-.071**	1.000	.278**	.047**	.131**
19) Pending charges	-.033**	-.056**	.042**	.057**	-.162**	-.050**	.278**	1.000	.133**	.048**
20) On prob at sent	-.043**	-.011	.032**	-.008	-.280**	-.062**	.047**	.133**	1.000	.029**
21) In pris at sent	-.014	-.186**	-.078**	.170**	-.159**	-.117**	.131**	.048**	.029**	1.000

* $p < .05$ ** $p < .01$

Table 7.10

Proportion Rearrested by Type of Sentence
(N in parentheses)

Sentence	Proportion Rearrested
Prison	.622 + (1518)
Youth Complex	.781 + (1145)
Jail, Probation, and Fine	.362 - (439)
Jail and Probation	.652 + (683)
Jail only	.620 + (1309)
Probation with conditions and Fine	.446 - (1429)
Probation and Fine	.422 - (1779)
Probation with conditions	.607 + (1538)
Probation only	.512 - (1478)
Fine only	.235 - (298)
Other	.399 - (133)
Total	.549 (11749)

\+ Significant surplus of cases in bivariate crosstabulation
\- Significant deficit of cases in bivariate crosstabulation

Table 7.11

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0199*	.0189	.0341**	.0332**	.0280*	.0280*	.0303*	.0319*
Has job after sentence	-.0232**	-.0393***	-.0341**	-.0306**	-.0321**	-.0263*	-.0245*	-.0293*
Offender is on welfare	-.0103	-.0251*	-.0227	-.0099	-.0017	.0038	-.0059	-.0026
Offender is Black	.0577***	.0869***	.1104***	.1196***	.1411***	.1539***	.1557***	.1716***
Offender is Hispanic	.0598***	.0747***	.0867***	.1018***	.1261***	.1373***	.1449***	.1516***
Offender is female	-.0280*	-.0451**	-.0424*	-.0500*	-.0420*	-.0586**	-.0617**	-.0492*
Lives in urban area	.0095	.0163	.0248*	.0359**	.0302*	.0343**	.0373**	.0335**
Years at current address	-.0013**	-.0013*	-.0016**	-.0017**	-.0015**	-.0014*	-.0011	-.0011
History of drug problems	-.0049	.0025	.0020	.0059	.0091	.0231	.0179	.0189
Treated for drugs/alch.	-.0007	-.0150	-.0138	-.0066	-.0026	-.0072	-.0011	.0032
Has needle marks	.0026	.0010	.0006	.0184	.0278	.0284	.0310	.0440
Not a school drop out	-.0241**	-.0278**	-.0340**	-.0383***	-.0418***	-.0420***	-.0385***	-.0384**
Doesn't live with family	.0064	-.0020	-.0067	-.0013	-.0081	-.0130	-.0201	-.0345*
Commited PO with group	-.0192**	-.0197*	-.0258**	-.0253*	-.0252*	-.0332**	-.0329**	-.0344**
Victim was a stranger	.0242**	.0248*	.0271*	.0340**	.0382**	.0374**	.0317*	.0346*
Presenting Offense								
PO property crime	.0397**	.0180	.0233	.0310*	.0388*	.0331*	.0310	.0268
PO crime against person	-.0001	-.0138	.0174	.0106	.0226	.0319	.0349	.0277
PO drug offense .	.0129	-.0090	.0066	.0045	.0039	.0014	-.0043	-.0101
PO Wolfgang severity	-.0010	-.0009	-.0010	-.0009	-.0009	-.0016	-.0014	-.0014
Has detainers at arrest	-.0221	-.0176	-.0139	-.0162	-.0117	-.0131	-.0237	-.0220
Has pending charges	.0393***	.0541***	.0630***	.0767***	.0784***	.0849***	.0925***	.0928***
On probation at PO	.0355**	.0535***	.0495**	.0489**	.0565**	.0649***	.0614**	.0666***
Anamnestic Theory								
N prior adult arrests	.0079**	.0112**	.0157***	.0222***	.0254***	.0286***	.0304***	.0316***
N prior adult conviction	-.0005	.0017	.0024	-.0004	.0002	-.0022	-.0039	-.0040
N prior adult chg. conv.	-.0030	-.0054	-.0057	-.0082*	-.0096*	-.0111**	-.0120**	-.0143***
N charges past 5 years	.0028*	.0052***	.0047**	.0049**	.0062**	.0062**	.0072***	.0071**
N prior Part 1 charges	.0067*	.0116**	.0100*	.0067	.0083	.0088	.0079	.0120*
N prior property conv.	.0042	.0014	.0031	.0012	.0018	.0036	.0022	.0029
N prior persons conv.	.0044	.0034	.0048	.0052	.0026	.0061	.0111*	.0082
N prior weapons conv.	.0113	.0333**	.0357**	.0404**	.0408**	.0251	.0205	.0387*
Off street last 2 years	.0271**	.0489***	.0706***	.0885***	.0989***	.1027***	.1023***	.1005***
Delinquent Career/Onset								
N arrests as juvenile	.0090***	.0129***	.0165***	.0157***	.0174***	.0168***	.0163***	.0171***
N charges as juvenile	.0201	.0006	-.0042	.0133	.0160	.0056	.0057	.0068
Age at first arrest	.0006	.0006	.0019	.0028	.0029	.0040*	.0042*	.0039*
Yrs since first incarc.	.0013***	.0019***	.0020***	.0022***	.0022***	.0022***	.0022***	.0024***
Yrs since first drug use	-.0003	-.0006	-.0007	-.0010*	-.0008*	-.0009*	-.0007	-.0005
Prior CJS-Offender Action								
N prior incarcerations	.0030	-.0014	-.0044	-.0026	-.0054	-.0055	-.0027	-.0026
N prior parole revokes	.0060	.0037	-.0088	-.0028	.0074	.0109	.0135	.0080
Bad conduct last probat.	.0102	.0201	.0347**	.0232	.0251	.0187	.0241	.0175
Recent parole revoked	.0132	.0309	.0316	.0105	-.0067	-.0132	-.0289	-.0129
General Control Variables								
Offender age at sent.	-.0058***	-.0042**	-.0054***	-.0059***	-.0061***	-.0073***	-.0081***	-.0083***
Off. born out of state	-.0336***	-.0443***	-.0554***	-.0687***	-.0784***	-.0849***	-.0882***	-.0948***
Coder prob. prognosis	-.0003	-.0006**	-.0007**	-.0007***	-.0008***	-.0010***	-.0009***	-.0008***

* p<.05 ** p<.01 *** p<.001

Table 7.11 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Interactions								
Black x on prob. at PO	-.0207	-.0436*	-.0490*	-.0521*	-.0667**	-.0745**	-.0689**	-.0863**
Black x prior adult arrs	-.0050*	-.0086**	-.0115***	-.0126***	-.0119**	-.0109**	-.0131**	-.0150***
Black x n prior prop cnv	.0027	.0059*	.0082*	.0106**	.0088*	.0091*	.0105*	.0126**
Black x n charges as juv	-.0105	.0143	.0225	.0189	.0166	.0198	.0171	.0236
Female x Part 1 charges	.0053	.0170	.0320*	.0301*	.0335*	.0320*	.0333*	.0556**
Off. age x drug problem	-.0038*	-.0047*	-.0052*	-.0037	-.0037	-.0033	-.0036	-.0031
Off. age x prior trtment	-.0007	-.0036	-.0050	-.0075**	-.0072**	-.0078**	-.0067*	-.0070*
Off. age x unemployed	-.0006	-.0020	-.0022	-.0030*	-.0030*	-.0021	-.0023	-.0026
Off. age x PO property	-.0008	-.0054**	-.0052*	-.0043*	-.0042*	-.0056**	-.0045*	-.0047*
Off. age x chg pst 5 yrs	.0002	.0004**	.0004*	.0003	.0003	.0002	.0001	.0001
PO viol x has detainers	-.0064	-.0408	-.0821*	-.1107**	-.1061*	-.1091*	-.1010*	-.0942*
PO prop x n adl.arrests	.0014	.0045	.0034	.0010	.0019	.0030	.0030	.0028
PO prop x prior prop con	-.0066**	-.0060*	-.0060	-.0041	-.0059	-.0053	-.0049	-.0056
PO prop x n juv. arrests	-.0080**	-.0112**	-.0129**	-.0137**	-.0154**	-.0120*	-.0128*	-.0137*
PO prop x age at 1st arr	.0012	.0030	.0002	-.0016	-.0027	-.0023	-.0035	-.0027
PO prop x yrs. 1st incar	-.0002	.0001	.0005	.0002	.0004	.0003	.0005	.0005
PO drugs x n adl. convs.	.0083*	.0132**	.0147**	.0161**	.0141*	.0143*	.0155**	.0165**
PO drugs x Part 1 chgs.	-.0201***	-.0239***	-.0199*	-.0181*	-.0205*	-.0209*	-.0211*	-.0251*
PO drugs x last par. rev	.0978*	.0660	.0667	.0465	.1468*	.1397	.1901*	.1423
Sentence								
Prison	-.0739***	-.1150***	-.1256***	-.1226***	-.1235***	-.0988**	-.0963**	-.1012**
Youth complex	-.0238	-.0463*	-.0383	-.0322	-.0333	-.0136	-.0099	-.0103
Jail, probation, fine	-.0025	-.0719*	-.0881**	-.1038**	-.1073**	-.0865*	-.0851*	-.1115**
Jail, probation	.0484	-.0039	.0065	.0195	.0172	.0407	.0457	.0275
Jail only	-.0161	-.0369	-.0543*	-.0570*	-.0609*	-.0497	-.0565	-.0715*
Probation w/cond., fine	-.0180	-.0226	-.0136	-.0201	-.0277	-.0161	.0066	-.0045
Probation, fine	-.0131	-.0316	-.0350	-.0211	-.0274	-.0220	-.0204	-.0217
Probation w/conditions	.0004	.0043	.0133	.0202	.0050	.0136	.0120	.0048
Fined only	-.0250	-.0390	-.0527	-.0528	-.0513	-.0589	-.0688	-.0727
Other sanction	-.0236	-.0936*	-.1088*	-.1172*	-.1089*	-.0982	-.0774	-.1013
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0001	-.0001	-.0003	-.0005	-.0005	-.0003	-.0002	-.0003
Months to jail	-.0004	.0003	.0012	.0015	.0007	.0004	.0006	.0010
Months to prison	.0000	-.0002	-.0002	-.0002	-.0003	-.0004*	-.0005**	-.0004*
First sanction of career	.0558*	.0548	.0541	.0644	.0712*	.0533	.0487	.0577
Progressive sanction	.0036	.0019	.0088	.0061	.0094	.0113	.0089	.0072
Sentence Interactions								
Prison x n adult arrests	.0020	.0068*	.0054	.0054	.0039	.0018	.0006	.0022
Prison x n arrsts as juv	-.0007	.0037	.0005	.0007	.0018	-.0016	-.0018	-.0038
Yth. comp x n adlt convs	-.0086*	-.0101*	-.0093	-.0009	.0074	.0063	.0031	.0041
Yth. comp x chgs in 5 yr	.0015	.0035	.0008	.0011	-.0021	-.0030	-.0031	-.0037
Yth. comp x prior n incs	.0167**	.0213*	.0284**	.0298**	.0197	.0246	.0256	.0278
Jl & prob x inc lst 2 yr	-.0047	.0740	.0420	.0529	.0539	.0526	.0461	.0540
Jail x yrs using drugs	.0005	.0002	.0003	.0004	-.0005	-.0001	-.0002	.0003
Prb w/cnd, fn x adlt arr	-.0071	-.0144**	-.0163***	-.0149**	-.0129**	-.0158**	-.0167**	-.0154**
Prb & fn x Part 1 chgs.	-.0128	-.0268*	-.0310**	-.0129	-.0131	-.0132	-.0215	-.0273*
Prb w/cnd x adlt chg cnv	-.0076**	-.0089**	-.0112**	-.0068	-.0087*	-.0106*	-.0098*	-.0101*
Mths to jail x PO prop.	-.0001	-.0001*	-.0001**	-.0002**	-.0001*	-.0002**	-.0002**	-.0002**
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0180	-.0274	-.0420	-.0435	-.0392	-.0389	-.0436	-.0527
Init sanc x n adult arrs	.0228**	.0401***	.0480***	.0511***	.0559***	.0514***	.0537***	.0551***
Constant	-.1284***	-.1568***	-.1699***	-.1688***	-.1567***	-.1490***	-.1271***	-.0948***
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.111	.157	.184	.200	.211	.219	.224	.232
Model Classifications:								
% Correct Predictions	85.44	79.05	76.11	74.56	73.76	73.55	73.27	73.30
% False Positives	1.06	4.46	6.87	8.28	9.41	10.02	10.51	10.89
% False Negatives	13.50	16.49	17.02	17.16	16.83	16.44	16.22	15.81
RIOC	.465	.487	.491	.497	.496	.502	.502	.504

* p<.05 ** p<.01 *** p<.001

Table 7.11 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0311*	.0334*	.0380**	.0314*	.0324*	.0351**	.0346**	.0292*
Has job after sentence	-.0295*	-.0255*	-.0214	-.0231	-.0256*	-.0252	-.0209	-.0240
Offender is on welfare	-.0003	.0040	-.0015	.0044	-.0025	.0001	.0073	.0079
Offender is Black	.1774***	.1721***	.1836***	.1790***	.1819***	.1803***	.1841***	.1843***
Offender is Hispanic	.1514***	.1496***	.1522***	.1534***	.1588***	.1577***	.1604***	.1557***
Offender is female	-.0523*	-.0638**	-.0623*	-.0633*	-.0669**	-.0668**	-.0713**	-.0821**
Lives in urban area	.0370**	.0397**	.0417***	.0393**	.0393**	.0405**	.0372**	.0342**
Years at current address	-.0011	-.0013*	-.0013*	-.0011	-.0014*	-.0014*	-.0015*	-.0013*
History of drug problems	.0185	.0182	.0243	.0257	.0223	.0256	.0227	.0235
Treated for drugs/alch.	.0016	.0004	.0080	.0223	.0264	.0243	.0204	.0164
Has needle marks	.0559*	.0748**	.0760**	.0752**	.0759**	.0710**	.0707**	.0841**
Not a school drop out	-.0394***	-.0450***	-.0438***	-.0455***	-.0446***	-.0446***	-.0450***	-.0463***
Doesn't live with family	-.0441**	-.0515***	-.0468***	-.0481***	-.0470***	-.0506***	-.0494***	-.0387**
Commited PO with group	-.0404***	-.0335**	-.0298**	-.0294*	-.0262*	-.0235*	-.0288*	-.0342**
Victim was a stranger	.0250	.0232	.0230	.0241	.0254	.0274	.0279	.0281
Presenting Offense								
PO property crime	.0310	.0398*	.0453**	.0464**	.0443**	.0447**	.0389*	.0400*
PO crime against person	.0291	.0421*	.0494*	.0549**	.0510*	.0542**	.0490*	.0586**
PO drug offense	-.0122	.0042	.0052	.0083	.0043	.0017	.0041	.0104
PO Wolfgang severity	-.0011	-.0009	-.0008	-.0009	-.0011	-.0011	-.0010	-.0011
Has detainers at arrest	-.0164	-.0134	-.0104	-.0184	-.0302	-.0218	-.0135	-.0186
Has pending charges	.0931***	.0896***	.0903***	.0860***	.0869***	.0831***	.0829***	.0762***
On probation at PO	.0670***	.0599**	.0638***	.0606**	.0629**	.0625**	.0677***	.0614**
Anamnestic Theory								
N prior adult arrests	.0348***	.0350***	.0332***	.0310***	.0299***	.0312***	.0338***	.0350***
N prior adult conviction	-.0016	.0007	-.0013	.0006	.0020	.0050	.0039	.0011
N prior adult chg. conv.	-.0148***	-.0138**	-.0121**	-.0115**	-.0113*	-.0131**	-.0130**	-.0138**
N charges past 5 years	.0071**	.0063**	.0069**	.0073**	.0071**	.0072**	.0074**	.0094***
N prior Part 1 charges	.0119*	.0097	.0086	.0076	.0052	.0079	.0074	.0096
N prior property conv.	.0016	.0000	.0010	.0010	.0012	.0010	-.0003	.0002
N prior persons conv.	.0102	.0121*	.0132*	.0143*	.0143*	.0117	.0096	.0076
N prior weapons conv.	.0322	.0310	.0316	.0432*	.0437*	.0459*	.0431*	.0517**
Off street last 2 years	.1004***	.1085***	.1087***	.1082***	.1113***	.1084***	.1111***	.1067***
Delinquent Career/Onset								
N arrests as juvenile	.0175***	.0159**	.0155**	.0191***	.0174***	.0185***	.0197***	.0214***
N charges as juvenile	.0085	.0061	.0080	.0056	.0105	.0170	.0147	.0137
Age at first arrest	.0036*	.0032	.0028	.0021	.0025	.0024	.0024	.0025
Yrs since first incarc.	.0026***	.0025***	.0024***	.0025***	.0026***	.0024***	.0023***	.0020***
Yrs since first drug use	-.0006	-.0009	-.0010*	-.0009*	-.0009	-.0007	-.0006	-.0006
Prior CJS-Offender Action								
N prior incarcerations	-.0072	-.0082	-.0104	-.0140*	-.0153*	-.0197**	-.0199**	-.0186**
N prior parole revokes	.0108	-.0002	-.0054	-.0025	-.0032	.0020	.0008	.0039
Bad conduct last probat.	.0044	.0061	.0028	-.0048	-.0130	-.0081	-.0094	-.0081
Recent parole revoked	-.0170	-.0070	.0102	.0081	.0123	.0035	-.0027	.0125
General Control Variables								
Offender age at sent.	-.0088***	-.0086***	-.0077***	-.0071***	-.0072***	-.0072***	-.0074***	-.0071***
Off. born out of state	-.0945***	-.1019***	-.1058***	-.1065***	-.1113***	-.1156***	-.1160***	-.1214***
Coder prob. prognosis	-.0008***	-.0008***	-.0008***	-.0008***	-.0007**	-.0007**	-.0008***	-.0007**

* p<.05 ** p<.01 *** p<.001

Table 7.11 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Interactions								
Black x on prob. at PO	-.0957***	-.0953***	-.1165***	-.1097***	-.1085***	-.1063***	-.1146***	-.1095***
Black x prior adult arrs	-.0176***	-.0191***	-.0193***	-.0190***	-.0170***	-.0165***	-.0161**	-.0160**
Black x n prior prop cnv	.0151***	.0197***	.0217***	.0223***	.0206***	.0203***	.0215***	.0218***
Black x n charges as juv	.0243	.0266	.0288	.0324	.0264	.0140	.0124	.0142
Female x Part 1 charges	.0533**	.0500*	.0485*	.0436*	.0414*	.0390*	.0359	.0341
Off. age x drug problem	-.0032	-.0023	-.0022	-.0017	-.0021	-.0015	-.0016	-.0015
Off. age x prior trtment	-.0077**	-.0091**	-.0098***	-.0097***	-.0090**	-.0088**	-.0093**	-.0093**
Off. age x unemployed	-.0023	-.0016	-.0010	-.0017	-.0010	-.0011	-.0011	-.0018
Off. age x PO property	-.0044*	-.0038	-.0042*	-.0049*	-.0053*	-.0057**	-.0055**	-.0058**
Off. age x chg pst 5 yrs	.0002	.0001	.0002	.0002	.0002	.0002	.0002	.0003
PO viol x has detainers	-.1078*	-.1202*	-.1364**	-.1238*	-.1190*	-.1230*	-.1364**	-.1428**
PO prop x n adl.arrests	-.0008	-.0023	.0001	.0038	.0067	.0060	.0061	.0057
PO prop x prior prop con	-.0043	-.0022	-.0039	-.0048	-.0066	-.0064	-.0065	-.0078
PO prop x n juv. arrests	-.0136*	-.0125*	-.0124*	-.0154*	-.0137*	-.0146*	-.0140*	-.0172**
PO prop x age at 1st arr	-.0026	-.0032	-.0029	-.0013	-.0014	-.0008	-.0006	-.0014
PO prop x yrs. 1st incar	.0004	.0007	.0006	.0006	.0005	.0007	.0008	.0010
PO drugs x n adl. convs.	.0151*	.0133*	.0157*	.0157*	.0120	.0112	.0106	.0127*
PO drugs x Part 1 chgs.	-.0240*	-.0146	-.0209	-.0217	-.0169	-.0180	-.0148	-.0207
PO drugs x last par. rev	.1189	.0744	.0901	.0719	.0828	.1239	.1152	.0866
Sentence								
Prison	-.0973**	-.0984**	-.0911**	-.0914*	-.0914*	-.0817*	-.0869*	-.1033**
Youth complex	-.0071	-.0008	-.0083	-.0075	-.0082	-.0094	-.0099	-.0219
Jail, probation, fine	-.1319***	-.1428***	-.1451***	-.1559***	-.1682***	-.1491***	-.1552***	-.1393***
Jail, probation	.0057	.0023	-.0012	-.0080	-.0198	-.0119	-.0127	.0001
Jail only	-.0793*	-.0802*	-.0719*	-.0799*	-.0845**	-.0766*	-.0804*	-.0829*
Probation w/cond., fine	.0027	.0001	-.0022	-.0076	-.0047	-.0007	.0009	-.0063
Probation, fine	-.0142	-.0136	-.0134	-.0078	-.0080	-.0071	-.0032	-.0036
Probation w/conditions	.0145	.0206	.0198	.0202	.0181	.0192	.0180	.0097
Fined only	-.0649	-.0659	-.0719	-.0830	-.0860*	-.0834*	-.0961*	-.1090**
Other sanction	-.0975	-.0921	-.0881	-.0922	-.0968	-.1088	-.1011	-.0977
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0003	.0000	.0000	.0001	.0001	.0001	.0000	.0001
Months to jail	.0030	.0031	.0024	.0025	.0026	.0020	.0021	.0016
Months to prison	-.0005**	-.0006**	-.0006**	-.0007***	-.0007***	-.0008***	-.0008***	-.0009***
First sanction of career	.0545	.0471	.0402	.0416	.0258	.0309	.0425	.0470
Progressive sanction	.0087	.0086	.0089	.0042	.0035	-.0005	-.0032	-.0021
Sentence Interactions								
Prison x n adult arrests	.0028	.0009	.0002	-.0007	-.0001	-.0001	-.0001	-.0004
Prison x n arrsts as juv	.0017	.0034	.0005	-.0001	.0037	.0011	.0002	-.0011
Yth. comp x n adlt convs	.0044	.0062	.0044	.0065	.0097	.0093	.0100	.0091
Yth. comp x chgs in 5 yr	-.0040	-.0024	-.0032	-.0041	-.0035	-.0036	-.0050	-.0062
Yth. comp x prior n incs	.0264	.0199	.0223	.0181	.0160	.0123	.0104	.0097
Jl & prob x inc lst 2 yr	.0633	.0573	.0796	.0662	.0655	.0576	.0434	.0124
Jail x yrs using drugs	.0006	.0005	.0003	.0005	.0007	.0003	.0000	-.0003
Prb w/cnd, fn x sdlt arr	-.0102	-.0071	-.0068	-.0054	-.0037	-.0030	-.0022	-.0027
Prb & fn x Part 1 chgs.	-.0258*	-.0211	-.0208	-.0161	-.0130	-.0122	-.0141	-.0127
Prb w/cnd x adlt chg cnv	-.0113*	-.0119*	-.0102*	-.0097	-.0087	-.0087	-.0097	-.0118*
Mths to jail x PO prop.	-.0002***	-.0002***	-.0002***	-.0002**	-.0002**	-.0002**	-.0002**	-.0002**
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0687*	-.0548	-.0594*	-.0620*	-.0578*	-.0589*	-.0588*	-.0517
Init sanc x n adult arrs	.0494***	.0497***	.0487***	.0469***	.0441***	.0447***	.0473***	.0493***
Constant	-.0715*	-.0572	-.0561	-.0355	-.0160	-.0090	.0040	.0378
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.235	.237	.237	.239	.238	.239	.239	.238
Model Classifications:								
% Correct Predictions	73.14	72.98	72.95	73.31	73.16	73.18	73.39	73.61
% False Positives	11.46	12.07	12.38	12.39	12.75	12.95	13.17	13.48
% False Negatives	15.41	14.95	14.67	14.30	14.09	13.87	13.44	12.91
RIOC	.496	.486	.481	.485	.476	.472	.468	.472

* p<.05 ** p<.01 *** p<.001

Table 7.12

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Structural Variables								
Offender is unemployed	.0204*	.0196	.0348**	.0338**	.0285*	.0285*	.0308*	.0324*
Has job after sentence	-.0233**	-.0394***	-.0340**	-.0303**	-.0319**	-.0262*	-.0244	-.0292*
Offender is on welfare	-.0102	-.0248*	-.0223	-.0095	-.0013	.0042	-.0055	-.0020
Offender is Black	.0525***	.0798***	.1002***	.1102***	.1340***	.1518***	.1546***	.1713***
Offender is Hispanic	.0573**	.0707***	.0812***	.0988***	.1217***	.1303***	.1377***	.1429***
Offender is female	-.0314*	-.0490**	-.0465*	-.0511*	-.0454*	-.0634**	-.0659**	-.0553*
Lives in urban area	.0053	.0092	.0166	.0302*	.0238	.0275*	.0306*	.0259*
Years at current address	-.0013**	-.0013**	-.0016**	-.0018**	-.0016**	-.0014*	-.0011	-.0011
History of drug problems	-.0043	.0028	.0025	.0060	.0094	.0234	.0176	.0189
Treated for drugs/alch.	.0000	-.0141	-.0129	-.0062	-.0020	-.0067	-.0007	.0038
Has needle marks	-.0005	-.0043	-.0066	.0133	.0226	.0225	.0253	.0373
Not a school drop out	-.0254***	-.0296**	-.0362***	-.0399***	-.0434***	-.0436***	-.0399***	-.0399***
Doesn't live with family	.0069	-.0013	-.0059	-.0005	-.0075	-.0131	-.0206	-.0351*
Commited PO with group	-.0171*	-.0165	-.0225*	-.0229*	-.0223*	-.0304**	-.0306**	-.0317**
Victim was a stranger	.0264**	.0267*	.0297*	.0359**	.0405**	.0396**	.0331*	.0363**
Presenting Offense								
PO property crime	.0344*	.0050	.0103	.0189	.0300	.0243	.0212	.0185
PO crime against person	-.0035	-.0212	.0104	.0045	.0160	.0248	.0269	.0198
PO drug offense	.0273	.0058	.0268	.0259	.0244	.0180	.0083	.0016
PO Wolfgang severity	-.0010	-.0010	-.0010	-.0010	-.0009	-.0016	-.0014	-.0015
Has detainers at arrest	-.0225	-.0189	-.0148	-.0168	-.0121	-.0138	-.0248	-.0229
Has pending charges	.0392***	.0543***	.0635***	.0765***	.0779***	.0844***	.0920***	.0924***
On probation at PO	.0348**	.0523***	.0481**	.0473**	.0553**	.0638***	.0604**	.0659***
Anamnestic Theory								
N prior adult arrests	.0076*	.0105**	.0146***	.0212***	.0245***	.0276***	.0294***	.0306***
N prior adult conviction	-.0006	.0014	.0023	-.0007	.0001	-.0021	-.0037	-.0037
N prior adult chg. conv.	-.0031	-.0055	-.0059	-.0087*	-.0099**	-.0113**	-.0121**	-.0143***
N charges past 5 years	.0028*	.0052***	.0046**	.0048*	.0061**	.0061**	.0071***	.0070**
N prior Part 1 charges	.0069*	.0118**	.0103*	.0071	.0086	.0090	.0081	.0122*
N prior property conv.	.0038	.0010	.0029	.0011	.0016	.0034	.0021	.0027
N prior persons conv.	.0042	.0032	.0048	.0054	.0025	.0061	.0110*	.0081
N prior weapons conv.	.0099	.0312*	.0335*	.0386**	.0388*	.0242	.0196	.0379*
Off street last 2 years	.0264**	.0478***	.0689***	.0869***	.0976***	.1014***	.1013***	.0995***
Delinquent Career/Onset								
N arrests as juvenile	.0085***	.0122***	.0156***	.0150***	.0166***	.0160***	.0155***	.0163***
N charges as juvenile	.0216	.0027	-.0022	.0147	.0181	.0079	.0079	.0093
Age at first arrest	.0008	.0008	.0021	.0029	.0030	.0041*	.0044*	.0040*
Yrs since first incarc.	.0012***	.0019***	.0020***	.0022***	.0022***	.0022***	.0022***	.0023***
Yrs since first drug use	-.0003	-.0006	-.0006	-.0009*	-.0008	-.0009*	-.0007	-.0005
Prior CJS-Offender Action								
N prior incarcerations	.0037	-.0002	-.0036	-.0016	-.0043	-.0046	-.0019	-.0020
N prior parole revokes	.0066	.0044	-.0083	-.0027	.0078	.0116	.0139	.0086
Bad conduct last probat.	.0097	.0189	.0331*	.0222	.0239	.0173	.0225	.0157
Recent parole revoked	.0134	.0316	.0322	.0110	-.0061	-.0128	-.0286	-.0126
General Control Variables								
Offender age at sent.	-.0064***	-.0049***	-.0063***	-.0066***	-.0069***	-.0081***	-.0089***	-.0091***
Off. born out of state	-.0332***	-.0437***	-.0545***	-.0679***	-.0774***	-.0841***	-.0875***	-.0941***
Coder prob. prognosis	-.0003*	-.0006**	-.0007***	-.0008***	-.0008***	-.0011***	-.0009***	-.0009***

* p<.05 ** p<.01 *** p<.001

Table 7.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Interactions								
Black x on prob. at PO	-.0212	-.0444*	-.0498*	-.0524*	-.0672**	-.0752**	-.0697**	-.0873**
Black x prior adult arrs	-.0051*	-.0087**	-.0116***	-.0127***	-.0119**	-.0109**	-.0130**	-.0150***
Black x n prior prop cnv	.0029	.0060*	.0084**	.0106**	.0089*	.0091*	.0105*	.0126**
Black x n charges as juv	-.0110	.0134	.0216	.0183	.0156	.0186	.0158	.0220
Female x Part 1 charges	.0057	.0175	.0330*	.0307*	.0341*	.0326*	.0336*	.0560**
Off. age x drug problem	-.0038*	-.0047*	-.0051*	-.0035	-.0035	-.0031	-.0035	-.0030
Off. age x prior trtment	-.0006	-.0035	-.0049	-.0074**	-.0071**	-.0078**	-.0067*	-.0070*
Off. age x unemployed	-.0006	-.0020	-.0022	-.0030*	-.0030*	-.0021	-.0023	-.0025
Off. age x PO property	-.0010	-.0058**	-.0056**	-.0046*	-.0045*	-.0060**	-.0048*	-.0051*
Off. age x chg pst 5 yrs	.0002	.0005**	.0004*	.0003	.0003	.0002	.0002	.0002
PO viol x has detainers	-.0054	-.0394	-.0807*	-.1096**	-.1055*	-.1089*	-.1011*	-.0946*
PO prop x n adl.arrests	.0014	.0045	.0036	.0012	.0020	.0032	.0032	.0031
PO prop x prior prop con	-.0063**	-.0056	-.0056	-.0038	-.0056	-.0050	-.0047	-.0054
PO prop x n juv. arrests	-.0079**	-.0111**	-.0126**	-.0135**	-.0151**	-.0117*	-.0126*	-.0135*
PO prop x age at 1st arr	.0009	.0026	-.0002	-.0018	-.0029	-.0025	-.0037	-.0030
PO prop x yrs. 1st incar	-.0002	.0001	.0005	.0003	.0004	.0004	.0005	.0006
PO drugs x n adl. convs.	.0061*	.0130**	.0144**	.0159**	.0139*	.0138*	.0151*	.0160**
PO drugs x Part 1 chgs.	-.0199***	-.0233**	-.0194*	-.0178*	-.0202*	-.0205*	-.0207*	-.0246*
PO drugs x last par. rev	.1010*	.0698	.0702	.0489	.1483*	.1410	.1910*	.1433
Sentence								
Prison	-.0736***	-.1146***	-.1258***	-.1247***	-.1248***	-.0993**	-.0967**	-.1010**
Youth complex	-.0231	-.0456	-.0376	-.0342	-.0343	-.0136	-.0106	-.0099
Jail, probation, fine	-.0004	-.0671*	-.0850*	-.1021**	-.1045**	-.0834*	-.0800*	-.1062**
Jail, probation	.0520	.0020	.0139	.0257	.0232	.0468	.0523	.0345
Jail only	-.0145	-.0335	-.0508	-.0546	-.0576	-.0462	-.0526	-.0670*
Probation w/cond., fine	-.0186	-.0232	-.0159	-.0221	-.0291	-.0187	.0044	-.0070
Probation, fine	-.0112	-.0279	-.0322	-.0193	-.0247	-.0197	-.0178	-.0188
Probation w/conditions	-.0006	.0027	.0114	.0182	.0034	.0126	.0111	.0041
Fined only	-.0202	-.0321	-.0452	-.0462	-.0451	-.0542	-.0648	-.0690
Other sanction	-.0231	-.0927*	-.1068*	-.1152*	-.1073	-.0966	-.0754	-.0995
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0001	-.0001	-.0002	-.0005	-.0005	-.0003	-.0002	-.0003
Months to jail	-.0006	.0001	.0009	.0012	.0004	.0001	.0003	.0007
Months to prison	.0000	-.0002	-.0002	-.0002	-.0003	-.0004*	-.0005**	-.0005*
First sanction of career	.0552*	.0550	.0536	.0632	.0699	.0525	.0481	.0571
Progressive sanction	.0034	.0015	.0087	.0061	.0095	.0113	.0089	.0072
Sentence Interactions								
Prison x n adult arrests	.0020	.0068*	.0054	.0055	.0039	.0018	.0006	.0022
Prison x n arrsts as juv	-.0008	.0036	.0003	.0004	.0016	-.0019	-.0020	-.0041
Yth. comp x n adlt convs	-.0087*	-.0102*	-.0093	-.0010	.0073	.0062	.0029	.0038
Yth. comp x chgs in 5 yr	.0016	.0036	.0009	.0012	-.0019	-.0029	-.0030	-.0037
Yth. comp x prior n incs	.0169**	.0218**	.0291**	.0304**	.0201	.0252	.0263	.0285*
Jl & prob x inc lst 2 yr	-.0058	.0720	.0393	.0504	.0521	.0507	.0440	.0522
Jail x yrs using drugs	.0005	.0003	.0003	.0004	-.0005	-.0001	-.0002	.0002
Prb w/cnd, fn x adlt arr	-.0070	-.0144**	-.0164***	-.0149**	-.0129**	-.0160**	-.0168**	-.0156**
Prb & fn x Part 1 chgs.	-.0126	-.0263*	-.0304**	-.0122	-.0125	-.0128	-.0211	-.0270*
Prb w/cnd x adlt chg cnv	-.0075**	-.0087**	-.0109**	-.0066	-.0085*	-.0104*	-.0097*	-.0099*
Mths to jail x PO prop.	-.0001	-.0001*	-.0001**	-.0002**	-.0001*	-.0002**	-.0002**	-.0002**
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0192	-.0302	-.0445	-.0447	-.0409	-.0409	-.0460	-.0555
Init sanc x n adult arrs	.0226**	.0400***	.0479***	.0507***	.0555***	.0513***	.0535***	.0550***

* p<.05 ** p<.01 *** p<.001

Table 7.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	1/2	1	1 1/2	2	2 1/2	3	3 1/2	4
Selection Hazards								
UCR to SAC arrest histry	-.0116	-.0362*	-.0313	-.0243	-.0209	-.0210	-.0275	-.0263
Case proceeds past arrst	.1799*	.2429**	.2697**	.2261**	.1899*	.1286	.0937	.0822
Case to Grand Jury	-.0057	-.0069	-.0030	-.0051	-.0119	-.0047	-.0083	-.0087
Case to Superior Court	.0318	.0528	.0221	.0168	.0318	.0395	.0401	.0403
Superior Court Convict.	-.0173	-.0356	-.0506	-.0607*	-.0430	-.0472	-.0452	-.0378
Match over data sources	.0058	.0202	.0354	.0123	.0285	.0757	.0878	.1081
Constant	-.1316***	-.1658***	-.1800***	-.1717***	-.1681***	-.1664***	-.1383***	-.1131**
Mean of Dep. Var.	.148	.234	.292	.337	.371	.399	.423	.444
Pseudo R squared	.111	.158	.185	.200	.212	.220	.224	.232
Model Classifications:								
% Correct Predictions	85.45	79.16	76.26	74.67	73.89	73.55	73.22	73.27
% False Positives	1.11	4.42	6.86	8.21	9.41	10.07	10.54	10.92
% False Negatives	13.43	16.42	16.88	17.12	16.70	16.38	16.24	15.81
RIOC	.466	.493	.495	.501	.498	.501	.501	.503

* p<.05 ** p<.01 *** p<.001

Table 7.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Structural Variables								
Offender is unemployed	.0314*	.0336*	.0379**	.0312*	.0324*	.0352**	.0347**	.0293*
Has job after sentence	-.0294*	-.0252*	-.0211	-.0228	-.0256*	-.0252	-.0210	-.0240
Offender is on welfare	.0004	.0048	-.0007	.0053	-.0018	.0007	.0078	.0083
Offender is Black	.1761***	.1680***	.1797***	.1756***	.1810***	.1797***	.1832***	.1841***
Offender is Hispanic	.1420***	.1428***	.1446***	.1442***	.1485***	.1484***	.1523***	.1483***
Offender is female	-.0595*	-.0688**	-.0685**	-.0702**	-.0767**	-.0760**	-.0806**	-.0906***
Lives in urban area	.0288*	.0328*	.0350**	.0319*	.0308*	.0324*	.0295*	.0272*
Years at current address	-.0011	-.0014*	-.0013*	-.0012*	-.0014*	-.0014*	-.0015**	-.0013*
History of drug problems	.0187	.0182	.0245	.0257	.0232	.0266	.0240	.0246
Treated for drugs/alch.	.0023	.0009	.0085	.0227	.0271	.0250	.0211	.0170
Has needle marks	.0484	.0685**	.0695**	.0677**	.0680**	.0635*	.0643*	.0785**
Not a school drop out	-.0409***	-.0462***	-.0449***	-.0469***	-.0462***	-.0462***	-.0466***	-.0477***
Doesn't live with family	-.0447**	-.0516***	-.0471***	-.0487***	-.0477***	-.0511***	-.0496***	-.0390**
Commited PO with group	-.0378**	-.0313**	-.0283*	-.0281*	-.0236*	-.0206	-.0255*	-.0311**
Victim was a stranger	.0268	.0247	.0249	.0262	.0287*	.0306*	.0314*	.0313*
Presenting Offense								
PO property crime	.0249	.0336	.0448*	.0463**	.0450**	.0438*	.0389*	.0400*
PO crime against person	.0223	.0363	.0462*	.0517*	.0472*	.0503*	.0444*	.0544**
PO drug offense	-.0017	.0163	.0175	.0226	.0194	.0151	.0208	.0255
PO Wolfgang severity	-.0011	-.0009	-.0008	-.0009	-.0010	-.0010	-.0010	-.0011
Has detainers at arrest	-.0168	-.0136	-.0099	-.0179	-.0298	-.0216	-.0130	-.0183
Has pending charges	.0929***	.0894***	.0900***	.0856***	.0866***	.0830***	.0825***	.0757***
On probation at PO	.0667***	.0595**	.0638***	.0606**	.0630**	.0626**	.0677***	.0613**
Anamnestic Theory								
N prior adult arrests	.0337***	.0341***	.0324***	.0301***	.0290***	.0303***	.0330***	.0343***
N prior adult conviction	-.0011	.0011	-.0007	.0014	.0028	.0057	.0045	.0017
N prior adult chg. conv.	-.0146***	-.0138**	-.0121**	-.0115**	-.0112*	-.0130**	-.0128**	-.0137**
N charges past 5 years	.0071**	.0062**	.0068**	.0072**	.0070**	.0071**	.0074**	.0094***
N prior Part 1 charges	.0121*	.0100	.0090	.0081	.0057	.0084	.0078	.0101
N prior property conv.	.0014	-.0002	.0008	.0008	.0010	.0008	-.0006	.0000
N prior persons conv.	.0099	.0118*	.0130*	.0141*	.0140*	.0115	.0092	.0073
N prior weapons conv.	.0312	.0294	.0302	.0420*	.0431*	.0454*	.0422*	.0510**
Off street last 2 years	.0993***	.1074***	.1076***	.1070***	.1102***	.1074***	.1102***	.1060***
Delinquent Career/Onset								
N arrests as juvenile	.0165***	.0152**	.0148**	.0183***	.0164**	.0175***	.0187***	.0206***
N charges as juvenile	.0109	.0081	.0097	.0074	.0129	.0193	.0173	.0161
Age at first arrest	.0038*	.0033	.0029	.0021	.0026	.0025	.0024	.0025
Yrs since first incarc.	.0026***	.0024***	.0024***	.0025***	.0026***	.0023***	.0023***	.0020***
Yrs since first drug use	-.0006	-.0009	-.0010*	-.0009*	-.0009	-.0007	-.0006	-.0006
Prior CJS-Offender Action								
N prior incarcerations	-.0069	-.0077	-.0104	-.0141*	-.0154*	-.0197**	-.0194**	-.0181**
N prior parole revokes	.0112	-.0003	-.0055	-.0027	-.0026	.0027	.0016	.0047
Bad conduct last probat.	.0023	.0043	.0011	-.0068	-.0152	-.0102	-.0113	-.0098
Recent parole revoked	-.0168	-.0067	.0104	.0083	.0126	.0037	-.0023	.0130
General Control Variables								
Offender age at sent.	-.0097***	-.0094***	-.0086***	-.0081***	-.0083***	-.0082***	-.0084***	-.0081***
Off. born out of state	-.0936***	-.1009***	-.1047***	-.1052***	-.1099***	-.1143***	-.1145***	-.1200***
Coder prob. prognosis	-.0009***	-.0008***	-.0008***	-.0008***	-.0008**	-.0008***	-.0009***	-.0008***

* p<.05 ** p<.01 *** p<.001

Table 7.12 (continued)

Logit Regression Coefficients for Cumulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Interactions								
Black x on prob. at PO	-.0969***	-.0963***	-.1177***	-.1110***	-.1099***	-.1075***	-.1156***	-.1104***
Black x prior adult arrs	-.0176***	-.0191***	-.0193***	-.0190***	-.0171***	-.0166***	-.0163***	-.0162**
Black x n prior prop cnv	.0151***	.0197***	.0218***	.0224***	.0208***	.0204***	.0217***	.0220***
Black x n charges as juv	.0228	.0254	.0276	.0310	.0247	.0125	.0107	.0126
Female x Part 1 charges	.0538**	.0503*	.0490*	.0443*	.0423*	.0398*	.0367	.0348
Off. age x drug problem	-.0031	-.0022	-.0021	-.0015	-.0019	-.0013	-.0013	-.0013
Off. age x prior trtment	-.0078**	-.0091**	-.0098***	-.0097***	-.0090**	-.0088**	-.0092**	-.0093**
Off. age x unemployed	-.0023	-.0016	-.0010	-.0017	-.0010	-.0011	-.0012	-.0018
Off. age x PO property	-.0048*	-.0041	-.0044*	-.0052*	-.0056**	-.0060**	-.0058**	-.0060**
Off. age x chg pst 5 yrs	.0002	.0002	.0002	.0002	.0002	.0002	.0002	.0003
PO viol x has detainers	-.1082*	-.1205*	-.1368**	-.1244*	-.1195*	-.1234*	-.1369**	-.1433**
PO prop x n adl.arrests	-.0005	-.0022	.0002	.0039	.0070	.0063	.0063	.0060
PO prop x prior prop con	-.0042	-.0020	-.0039	-.0048	-.0064	-.0062	-.0063	-.0076
PO prop x n juv. arrests	-.0134*	-.0123*	-.0121*	-.0151*	-.0133*	-.0142*	-.0136*	-.0169**
PO prop x age at 1st arr	-.0030	-.0035	-.0031	-.0015	-.0017	-.0011	-.0009	-.0017
PO prop x yrs. 1st incar	.0004	.0007	.0006	.0006	.0005	.0007	.0008	.0010
PO drugs x n adl. convs.	.0145*	.0129*	.0151*	.0150*	.0112	.0104	.0100	.0121
PO drugs x Part 1 chgs.	-.0234*	-.0141	-.0205	-.0213	-.0163	-.0174	-.0143	-.0203
PO drugs x last par. rev	.1197	.0753	.0903	.0719	.0831	.1245	.1158	.0872
Sentence								
Prison	-.0963**	-.0987**	-.0911**	-.0917*	-.0906*	-.0808*	-.0865*	-.1033**
Youth complex	-.0060	-.0013	-.0086	-.0084	-.0066	-.0074	-.0080	-.0203
Jail, probation, fine	-.1266**	-.1383***	-.1420***	-.1528***	-.1661***	-.1472***	-.1534***	-.1379***
Jail, probation	.0129	.0087	.0049	-.0008	-.0132	-.0059	-.0075	.0047
Jail only	-.0744*	-.0759*	-.0678*	-.0755*	-.0802*	-.0727*	-.0764*	-.0794*
Probation w/cond., fine	.0000	-.0018	-.0042	-.0104	-.0083	-.0044	-.0019	-.0090
Probation, fine	-.0111	-.0107	-.0112	-.0059	-.0060	-.0051	-.0006	-.0015
Probation w/conditions	.0139	.0195	.0190	.0194	.0178	.0189	.0175	.0094
Fined only	-.0613	-.0618	-.0687	-.0799	-.0832	-.0807	-.0928*	-.1063*
Other sanction	-.0957	-.0901	-.0864	-.0903	-.0959	-.1081	-.1009	-.0976
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0002	.0000	.0000	.0002	.0002	.0001	.0001	.0002
Months to jail	.0026	.0027	.0020	.0021	.0021	.0016	.0017	.0012
Months to prison	-.0005**	-.0006**	-.0006**	-.0007***	-.0007***	-.0008***	-.0008***	-.0008***
First sanction of career	.0538	.0459	.0385	.0396	.0241	.0295	.0409	.0454
Progressive sanction	.0087	.0088	.0092	.0046	.0037	-.0004	-.0031	-.0020
Sentence Interactions								
Prison x n adult arrests	.0028	.0009	.0001	-.0007	-.0002	-.0002	-.0001	-.0005
Prison x n arrsts as juv	.0014	.0032	.0002	-.0005	.0033	.0008	.0000	-.0013
Yth. comp x n adlt convs	.0042	.0060	.0043	.0064	.0096	.0092	.0098	.0089
Yth. comp x chgs in 5 yr	-.0039	-.0024	-.0032	-.0041	-.0035	-.0035	-.0048	-.0061
Yth. comp x prior n incs	.0272	.0206	.0229	.0189	.0165	.0128	.0107	.0099
Jl & prob x inc lst 2 yr	.0615	.0557	.0783	.0643	.0642	.0565	.0429	.0119
Jail x yrs using drugs	.0005	.0005	.0003	.0005	.0006	.0003	.0000	-.0003
Prb w/cnd, fn x adlt arr	-.0105	-.0073	-.0070	-.0056	-.0040	-.0033	-.0024	-.0029
Prb & fn x Part 1 chgs.	-.0256*	-.0208	-.0207	-.0161	-.0130	-.0121	-.0139	-.0125
Prb w/cnd x adlt chg cnv	-.0110*	-.0118*	-.0100*	-.0095	-.0085	-.0085	-.0095	-.0116*
Mths to jail x PO prop.	-.0002***	-.0002***	-.0002**	-.0002**	-.0002**	-.0002**	-.0002**	-.0002**
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0720*	-.0576*	-.0619*	-.0645*	-.0605*	-.0615*	-.0612*	-.0537
Init sanc x n adult arrs	.0492***	.0493***	.0483***	.0464***	.0438***	.0445***	.0470***	.0490***

* p<.05 ** p<.01 *** p<.001

Table 7.12 (continued)

Logit Regression Coefficients for Cummulative Proportion Rearrested - Expressed as Change at the Mean - Controlling for Sample Selection (N = 11,749)

Independent Variable	Proportion Rearrested by Year							
	4 1/2	5	5 1/2	6	6 1/2	7	7 1/2	8+
Selection Hazards								
UCR to SAC arrest histry	-.0235	-.0225	-.0095	-.0086	-.0038	-.0061	-.0030	-.0020
Case proceeds past arrst	.0793	.1049	.0766	.0673	.0691	.0748	.0936	.0765
Case to Grand Jury	-.0109	-.0147	-.0177	-.0191	-.0103	-.0053	-.0097	-.0078
Case to Superior Court	.0272	.0157	-.0001	-.0036	.0154	.0174	.0346	.0337
Superior Court Convict.	-.0255	-.0260	-.0107	-.0178	-.0148	-.0176	-.0121	-.0139
Match over data sources	.1143	.0758	.0838	.1033	.1244*	.1142	.0921	.0882
Constant	-.0913*	-.0659	-.0660	-.0437	-.0500	-.0442	-.0397	-.0018
Mean of Dep. Var.	.462	.480	.493	.505	.516	.524	.532	.549
Pseudo R squared	.236	.237	.237	.239	.238	.239	.239	.238
Model Classifications:								
% Correct Predictions	73.08	73.16	72.96	73.17	73.16	73.24	73.38	73.59
% False Positives	11.51	12.00	12.32	12.44	12.64	12.84	13.12	13.52
% False Negatives	15.41	14.84	14.72	14.38	14.20	13.92	13.50	12.89
RIOC	.494	.489	.482	.482	.478	.474	.469	.472

* p<.05 ** p<.01 *** p<.001

CHAPTER EIGHT

THE EFFECTS OF SENTENCES ON SUBSEQUENT CRIMINAL BEHAVIOR

The focus of this chapter is the impact of the 1976-77 sentence on the further criminal behavior of the offender. The chapter represents the intersection of the differences between indicators of recidivism (Chapter Four), how offender characteristics, prior record, and aspects of the presenting offense are related to recidivism (Chapters Five and Six), and the measures of the sentence used in this research (Chapter Seven). As such, many of the issues raised in those chapters are relevant for a full understanding of the results to be presented here. Two of these bear reiteration.

First, it will be remembered that we are studying the effects of sentence received and not necessarily the actual treatment given to the offender as a consequence of that sentence. Only in a broad sense are the two equivalent. Those sentenced to incarceration (without probation) will almost always serve some time in custody, those given terms of probation will usually be supervised by the probation department, and so forth. But beyond this simple isomorphism, the sentence and the treatment received are quite distinct and to equate the two is fallacious. Thus, the safest interpretation of any effects observed for the sentence indicators is that the groups defined by the various sanctions differ in their recidivistic behavior and these differences may be attributed to the sentence itself. However, exactly why such differences arise (i.e., the effect of the treatment) remains unaddressed by our analysis.

Second, it should be remembered that comparing results for the same dependent variable across different post-sentence observation windows can be

problematic as many of the differences are likely to be more apparent than real. That is, increasingly larger coefficients (in either a positive or a negative direction), in addition to an increase in the predictive ability of the model as a whole, is to be expected simply by chance. For example, if a definable subgroup, say males, is more likely to recidivate, then, as the length of the post-sentence observation window increases, so too does the proportion of males in the pool of recidivists. The increase in the relative proportion of male recidivists will be reflected by larger coefficients for the sex variable over longer windows and in an increase in the variation in recidivism that can be explained by the model as a whole. Consequently, the most appropriate use for the results to be presented in this chapter is in the comparison of different measures of recidivism at the same time period.

The organization of this chapter mirrors that used in Chapter Six. Representatives of the binomial class of recidivism measures are discussed first, followed by recidivism indicators suggested by the criminal careers perspective and then time to failure measures. For each recidivism measure studied, we present summary measures of the explanatory power of the sentence variables using a variance partitioning strategy and then investigate the results from linear models. All independent variables introduced in Chapter Five (e.g, social structural, anamnestic, etc., and interactions among them) are controlled for in all models. Comparable results observed when controls for sample selection processes are introduced are contained in Appendix B. Finally, the observed effects for all sentence measures are contrasted as a means for summarizing the findings of this chapter.

BINOMIAL MEASURES OF RECIDIVISM

Recidivism may be defined as whether or not a particular event occurs subsequent to the sentence. Of the possible events of interest, we focus on four representatives of this perspective -- being rearrested, being (re)arrested for a violent crime against persons, repeating the presenting offense that led to the sentence of interest, and being (re)imprisoned after the sentence.[1] Average levels for the four binomial indicators, by type of sanction and width of observation window, are given in Table 8.1. As is to be expected, average levels for each measure monotonically increase with longer windows (the row labeled "TOTAL" in each panel of Table 8.1) and this increase is reproduced within each type of sanction.

The groups defined by the sanctions differ markedly under any binomial measure of recidivism and at any post-sentence observation window. For example, after five years, those sentenced to the Youth Complex are more likely to be rearrested, reimprisoned, repeat the presenting offense, and be arrested for a persons crime at levels over 50 percent higher than the sample as a whole. Conversely, those receiving a sanction of a fine are much less likely to display high levels of recidivism as measured by these indicators. In general, all these binomial measures show the same pattern found in Table 7.10 in that those receiving the more severe sanctions are more likely to

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[1] Note that for both the probability of (re)arrest for a persons crime and the probability of (re)imprisoned, the event could be the first of the offender's criminal history. Individuals who had never been charged with a crime against persons prior to the 1976-77 sentence, but were after that sentence, are considered a recidivist through the commission of a qualitatively new type of offense. Similarly, those who had never been imprisoned may ultimately be incarcerated, either through violation of probation conditions set by the 1976-77 sanction or by new sentences resulting from further criminal activity. The binomial measures used here are not sensitive to such nuances as they represent different "events" contingent upon the individual's prior record and incarceration history.

score high on recidivism.[2]

However, there are also subtle differences in the relative rank order of sanction groups across the various recidivism measures. After those sent to the Youth Complex, those given jail and probation are most likely to be rearrested. In contrast, the group next most likely to be rearrested for a persons crime are those given a simple jail confinement. Those sentenced to jail also rank second in terms of repeating the presenting offense, while it is those sentenced to a State Prison who rank second in the probability of being reimprisoned.

That a particular definition of recidivism matters can also be seen by the ordering of mean levels within a given sanction. For example, again at five years, those sentenced to jail only are most likely to be rearrested (55.5%), with 26.6% being rearrested for a persons crime, 24.3% repeating the presenting offense, and 20.1% being reimprisoned. In contrast, those given the cluster of "other" sanctions, while also most likely to be rearrested (.331), are more likely to repeat the presenting offense (.173) than they are to be rearrested for a persons crime (.135) or reimprisoned (.098). Possibly reflecting a greater supervision due to parole, those sentenced to a State Prison are most likely to be rearrested, (over 57 percent are), then most likely to be reimprisoned (35.3%), followed by being rearrested for a persons crime (26.3%) and an arrest for a repeat of the presenting offense (23.6%).

While the conclusions to be reached from Table 8.1 are limited to only these representatives of the class of binomial recidivism measures, it is clear that the process of sentencing has resulted in different groups of

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[2] Again caution is urged in interpreting these means as an indication of the "effect" of the sentence as they are not adjusted for the effects of other independent variables.

offenders in terms of their subsequent criminal behavior: the more serious offenders, as measured by prior record and ultimate recidivism, have received the more severe sanctions.[3] This confounds any assessment of how the sanction impacts on levels of recidivism. In addition, which indicator is taken to represent "recidivism" will matter as the relative ordering of levels of recidivism vary both within and between sanction groups depending upon both the width of the post-sentence observation window and the measure used.

The Predictive Power of the Sentence

We take as the "effects" of sanctions the ability to account for variation in recidivism beyond that attributable to the independent variables discussed in Chapters Five and Six. That is, to the extent that knowledge of the sanction received increases the explanatory power of the models developed in earlier chapters, the sentence can be seen as influencing the individual's level of recidivism. Whether this influence is "positive" as would be expected under rehabilitative or specific deterrence sentencing philosophies, or "negative" if certain experiences have a criminogenic impact, is separable from the basic question of how much of an effect can be documented. In this section the focus is simply on how much the sentence indicators augment the explained variability in recidivism.

Tables 8.2 through 8.5 present the communality analyses for the four binomial recidivism measures under consideration.[4] In absolute terms, the

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[3] Clearly the nonrandom assignment of offenders to sanctions is mirrored in levels of subsequent recidivism.

[4] Following the strategy adopted in Chapter Six, all independent variables and the interactions among them are entered first, though these variables are omitted from the tables. The main effects of the sanction are then assessed (the rows labeled "Sentence Variables" in Tables 8.2 through 8.5) and then the interactions are tested. The rows labeled "All Sanction Measures" are the sum of the variance due to the main effects for the sanction measures and that due

quantifiable impact of all of these sanctions is relatively minimal, ranging from a high of an increment of 2.6 percent in the variance explained (for the probability of reimprisonment after three years) to a low of .5 percent (repeating the presenting offense within a three year window). While these absolute levels of explained variation are disappointing, it must be recognized that they are bounded by the total amount of variability that can be explained. As we saw in Chapter Six, variability in the likelihood that an individual will recidivate, as defined by binomial measures, is particularly difficult to account for. Here we find that, with the inclusion of the sanction indicators, more variability can be explained in the probability of reimprisonment (31.1 percent over the full nine year window) than can explained in the probability of rearrest (27.1%), the probability of being rearrested for a persons crime (18.8%), and the probability of repeating the presenting offense (15.0%).

Converting the variance attributable to the sentence to the base of total explained variance makes it easier to see how and where the sanction measures are having their greatest impact.[5] First, the general pattern of increasing predictive ability with longer windows is not found for the variance attributable to the sentence. Rather, the absolute magnitudes are

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to the interactions.

[5] Doing this, however, can be deceptive as small increments to the explained variance are proportionally large when the total explainable variance is itself minimal. For example, 9.2% of the variance in the probability that the individual will be rearrested for a persons crime within one year can be explained by the ninety-eight variables in our full model. The sanction measures uniquely account for .8%. In relative terms though, 9.03% of the explainable variance is due to some aspect of the sanction. Thus while in general it is difficult to predict, at the time of sentencing, who will recidivate through imprisonment, knowing the sentence received substantially increases the differences that can be predicted.

relatively constant and when these are made relative to the increasing amount of variance that can be explained, the proportion of variance attributable to the sentence is seen to decrease over time. The exception here is the probability of reimprisonment. The proportion of variance attributable to the sentence almost doubles between the one and three year windows, remaining constant (in absolute terms) thereafter.

Second, a small paradox emerges for the variance accounted for by the main effects for the sanction variables when compared to how those variables interact with the other independent variables. In spite of the fact that the ability of other independent variables to predict recidivism increases over longer windows, the interactions of these variables with the sanction show a decreasing ability to predict recidivism both in absolute and relative terms. This suggests that differences in recidivism across subgroups of individuals within sanctions blur the longer the post-sentence observation window. That is, the conditioning of the effect of the sanction by various offender characteristics is less the further in time from the sanction.

In contrast, the main effects of the sanction display, somewhat surprisingly, an increasing ability to predict recidivism. While in absolute terms the increase is small, Tables 8.2 through 8.5 yield the general sense that how the sanction impacts on levels of recidivism is more over longer windows than over shorter ones. (Again we emphasize that the magnitudes of these increases are small.)

Exactly which aspects of the sentence are responsible for the ability to account for variation in recidivism is quite consistent across these binomial measures. Neither the measures of the continuous components of the sentence (e.g., time) nor the measures of sentence pattern show much ability to account

for variation in levels of recidivism as measures by rearrest for a persons crime, a repeat of the presenting offense and reimprisonment. For the rearrest measure, there is some indication of a effect of sentence pattern at the longer windows, but it too is small and, as with the other binomial measures, the continuous components are unable to uniquely account for variation across offenders.

It is the place sentenced (broadly defined) that drives the explained variance that can be attributed to the sanction main effects. Knowing the type of sentence received does allow for a better prediction of the likelihood of recidivism beyond the basic offender and offense characteristics introduced in Chapter Five. Exactly how well the sentence can predict subsequent recidivism depends on the particular measure used. For the probability of rearrest, the place sentence uniquely accounts for .3% or roughly one percent of the total explained variance. For the probability of rearrest for a persons crime, the absolute magnitude is the same, though this translates to about twice as much of the total variance explained. An effect of a similar magnitude is found for the probability of repeating the presenting offense. The largest proportion of variance attributable to the kind of sentence received is found for the likelihood of reimprisonment, where place sentence accounts for up to two percent, or a relative seven percent, of all variance that can be explained.

To summarize, the results of the variance partitioning for the binomial recidivism indicators suggest that the quantifiable impact of the sentence is small relative to what can be predicted on the basis of other information available at the time of sentencing. Still, these effects are nonnegligible. The place sentenced accounts for most of the explained variance attributable

to the sanction measures. The interactions involving the sanction measures are less useful for differentiating levels of recidivism as the post-sentence observation window increases, though the initial differences themselves are small. This decrease in explained variation is contrasted by a small increase in the impact of the type of sentence received and little unique influence of either the time sentenced or the patterning of the sentence.

Models of Recidivism

Exactly how the groups defined by the various sanctions differ on the binomial recidivism measures is shown in Table 8.6. There, coefficients for the sixteen sentence measures, and the fourteen interactions with other independent variables, are given. Note that these effects are estimated controlling for all other independent variables, thus allowing us to determine the impact of the sentence beyond what can be predicted at the time of sentencing. While coefficients for all independent variables are presented in this and subsequent tables, only those coefficients relating to the sanction and sanction interactions are discussed here.[6]

Panel "a" of Table 8.6 presents the logistic regression results when the probability of rearrest is used as the dependent variable.[7] Overall, once the independent variables are controlled, the groups given the various types of sentences display similar levels of rearrest relative to the reference group of simple probationers. Those given a sentence to a State Prison or a split

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[6] We are operating under an implicit causal model where the effects of variables derived from anamnestic theory, structural causes of crime and so forth are given temporal priority over the impact of the sentence. As such, the coefficients in Table 8.5 and subsequent tables represent an aspect of the reduced form of the underlying model. The actual effects for these logically prior models are more properly represented by the results presented in Chapters Five and Six.

[7] These are reproduced from Table 7.11.

sentence to jail in conjunction with a fine are significantly less likely to be rearrested than simple probationers across all windows. Sentences to the Youth Complex result in lower levels of rearrest, but only for the shortest window. Some additional effects emerge over longer windows, where those sentenced to a simple jail confinement or given only a fine are significantly less likely to be rearrested. Among the other measures of the sanction, only months sentenced to prison influence the probability of rearrest, with small, negative, effects being found over the longer observation periods.

In the short run, several interactions modify the main effects of the sanction variables. Prison sentences are less "effective" for those with more prior adult arrests and greater numbers of convictions as an adult tend to lower levels of rearrest for those sent to the Youth Complex. For those given a sentence of probation with conditions and a fine, longer prior records, as measured by prior adult arrests, result in significantly lower levels of recidivism. Similarly, prior record, this time measured as Part I charges, interacts with the sentence of probation and a fine, such that those with longer records are less likely to be rearrested.

Three sanction interactions show robust interactions over all time periods. Charge convictions as an adult, in conjunction with a sentence of probation and a fine, lead to more significant decreases in rearrest for those with more prior convictions. Similarly, property offenders appear to respond more to the length of a jail confinement than do those with other types of presenting offenses. Increases in levels of rearrest are found for the initial sanction by prior adult arrests interaction. Each prior arrest as an adult increases the probability of rearrest by about .05 for those who

received their first sanction in the 1976-77 sentence.[8]

If the interest is in the probability of being rearrested for a persons crime as the measure of recidivism (Panel "b" of Table 8.6), fewer effects can be attributed to the sentence. Once the independent variables are controlled, only those given a sentence to State Prison are significantly less likely to commit another persons crime, and then only at the shortest or longest window. A significant decrease in persons arrests is also found for those receiving a sentence of jail, probation and a fine, but only for a window of nine years. The only other significant sanction effect is for months sentenced to prison, but this too is limited to the longest window.

Mirroring the lack of main effects on this dependent variable, few interactions are found to be significant. In the short run those who were incarcerated at some time during the two years prior to sentencing and receive a split sentence to jail at sentencing are significantly more likely to be rearrested for a persons crime. (Note that this interaction was not significant for the rearrest recidivism measure.) The impact of months sentenced to jail for property offenders emerges after five years, though it is still weak. Only the strong interaction between prior arrests and initial sanction persists when the likelihood of rearrest for a persons crime is the dependent variable.

Panel "c" of Table 8.6 shows how the sentence received is related to the probability of repeating the presenting offense. As with a rearrest, those sentenced to a State Prison are significantly less likely to repeat the

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[8] One possible interpretation of this finding is that offenders who were previously arrested but not punished (i.e., not convicted) see the CJS as ineffective in punishing offenders and are thus "encouraged" to commit further crimes. The fact that a "first sanction" is not likely to be incarcerative may add to that perception.

instant offense, though the magnitude of the coefficients are slightly smaller. Similarly, those given a split sentence and fined are less likely to commit a crime similar to the presenting offense (after at least 3 years). Unlike the other binomial measures, significant decreases are found for those given a sentence of probation in conjunction with a fine (again after at least three years). The number of months sentenced to a State Prison also influences the likelihood that the individual will be rearrested for a repeat of the presenting offense after the five-year window. Here the magnitude of the effect is five times that found for the previous binomial measures.

Again, few interactions are found to modify the main effects of the sanctions. Property offenders are less likely to repeat the presenting offense as the number of months sentenced to jail increases, and the effects of an initial sanction continue to dissipate with more prior adult arrests. The major difference emerging with the repeating dependent variable is a strong interaction, across all time periods, between sentences to the Youth Complex and prior convictions as an adult. Every prior adult conviction reduces the probability of this type of recidivism by one percent for this group of offenders.

Reimprisonment represents the deepest (re)penetration in the criminal justice system among the binomial recidivism measures, and the observed effects of the sanction on this form of recidivism are quite different. If the type of sanction is other than some form of jail sentences, all types of sentences are equally likely to lead to a reimprisonment. Groups given jail sentences -- split or not, fined or not -- are significantly less likely to be reimprisoned, and the coefficients for these effects are among the largest found for all the binomial indicators. After nine years, for example, those

receiving a split sentence and a fine, show a decrease in the probability of reimprisonment of .14.[9]

The continuous components of the sentence are more predictive of reimprisonment than the other binomial measures. The term of the sentence to a State Prison is still found to significantly decrease recidivism for post-sentence observations windows of five or nine years. In addition, effects for the length of the probation term are found. Each month sentenced to probation increases the likelihood of reimprisonment by .001 for post-sentence observation windows of three or more years.[10]

Considerably more interactions that influence the probability of reimprisonment are detected than were found for either the likelihood of rearrest for a persons crime or a repeat of the presenting offense. These interactions also tend to be more robust than those for the probability of rearrest for any type of crime. The initial sanction by prior adult arrests continues to be significant across all windows. As with the binomial measure of repeating the instant offense, each prior adult conviction leads to a decrease in the likelihood of reimprisonment for those sent to the Youth Complex. Prior record also mediates the impact of sentences to probation with "conditions." Each prior charge conviction as an adult decreases the

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[9] Note that this effect cannot be accounted for by a lack of post-sentence supervision for these groups. That is, one might expect that those given sentences of probation, or those on parole after a prison sentence, would be more likely to be reimprisoned simply because violations of probation or parole would be more easily detected. However, those given split sentences to jail are also under supervision. Consequently, some other mechanism is needed to account for why those given jail sentences are significantly less likely to be reimprisoned.

[10] Unlike the explanation for the effects of a jail sentence on reimprisonment, the impact of the length of the sentence to probation could be interpreted as a supervision effect. The longer one is under the auspices of probation supervision, the more likely any violation of the conditions of probation is to be detected and probation revoked.

probability of reimprisonment by about one percent.

These generally "positive" results for the probability of reimprisonment are offset by the significance of two interactions. First, if the sentence is one to the Youth Complex, each prior incarceration increases the chance of being reincarcerated by about .02 (after at least three years). Second, the effectiveness of split sentences to jail (without a fine) is compromised by the offender having been incarcerated some time within the two years prior to sentencing. For both the three and five-year windows, those given this form of jail sentence are more likely to be reincarcerated and those coefficients are, relatively, quite high (.11 and .12 respectively).

Overall, the findings for the effects of the sanctions upon the binomial measures of recidivism are mixed. No forms of sanctions are found to significantly reduce the probability of these recidivistic events across all lengths of the observation and all indicators defining recidivism. Indeed, the only sanction effect that appears to be independent of time and definition is that pertaining to the initial sanction by prior adult arrests interaction. The longer the offender accumulates arrests without any sanctions, the more likely s/he is to recidivate.

Yet, there appears to also be some beneficial aspects to the sanctions if one is willing to make these benefits contingent on how recidivism is defined and the period over which one looks for recidivism. Sentences to State Prison tend to reduce the probability of recidivating, and this main effect tends not to be modified by any interactions. Jail sentences are found to be more effective for reducing the subsequent reimprisonment of the offender than they are for reducing the likelihood of rearrest for some types of crime. The length of the sentence (note that this is not the time actually

served) tends to reduce the recidivistic behavior of those sentenced to prison, and, for property offenders, those sentenced to jail. Overall, incarcerations to the Youth Complex appear to be no more effective than those of simple probation, once the battery of independent variables is controlled. Yet prior CJS interventions, as represented by convictions as an adult, can increase the effectiveness of these Youth Complex sanctions. That is, Youth Complex sanctions are associated with less recidivism for those offenders with more prior adult convictions.

The Impact of Sample Selection

In Chapter Six we saw that controlling for sample selection bias was both relatively consequential and inconsequential for the conclusions reached. While the factors that lead an individual case to be included in this sentencing sample did not significantly augment the ability to predict recidivism, about half the variance that could not be uniquely attributed to the domains of the independent variables could be attributed to the influence of sample selection. That is, much of what is predictive of recidivism is shared between the characteristics of the offender and the offense, and the processing of the case through the CJS. As well, controlling for the selection hazards produced some changes at the specific level of the magnitude of the coefficients for some variables. At a more general level, the substantive conclusions reached were not contingent upon selection biases.

In some respects, the effects of sample selection on the effectiveness of the sentence are quite similar. Tables B8.2 through B8.6 in Appendix B parallel the analyses just discussed for the binomial recidivism measures. Introducing the selection hazards has little influence on the variance uniquely attributable to the sanction measures. The ability of the sentence

to predict recidivism beyond that implied by the independent variables remains quite small with the proportions of variance hovering in the general range of .001 to .01. Unlike what was observed for the independent variables, however, we find that controlling for sample selection bias does not detract from the variance attributable to any of the sentence measures. In fact, there is some slight evidence that, for the five and nine-year windows, sample selection suppresses the quantifiable impact of the sentence. That is, once selection biases are controlled, the effects of the sentence are measurably higher, though the absolute magnitudes are still quite small.

There are slight perceptible changes in the individual coefficients as well. (See Table B8.6.) Some small effects (e.g., the effect of a prison sentence on the probability of repeating the presenting offense after five years) are no longer significant once selection biases are considered. Some coefficients are virtually unchanged, while others decrease. For example, prior to controlling for the selection hazards, being given a split sentence to jail and fined decreased the likelihood of reimprisonment by -.137. After the hazards are controlled, this decrease is estimated to be -.135.

The small suppression effect produced by selection biases is also observed as some coefficients increase once the hazards are controlled. For example, we saw in Table 8.6 that each adult charge conviction decreased the probability of reimprisonment for those sentenced to probation with conditions by .0075 over a five-year window. Controlling for selection hazards changes this estimate to a decrease of .0081[11]

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[11] While these changes in magnitude are exceedingly small, they can result in different predicted levels of recidivism on the individual level, especially when continuous independent variables are involved. An additional ten prior adult charge convictions, for example, decreases the probability of reimprisonment by .006 more under the hazard model than the model without the

Nonetheless, we are struck by how little the substantive conclusions change in the presence of controls for sample selection bias. The descriptions given above for the results of the uncontrolled models apply equally well to the models which include the selection hazards. At the more general level of describing how the sentence impacts on levels of recidivism, sample selection processes matter little. It is only at the very detailed level of individual, case-by-case, prediction that we might find an appreciable change due to the consideration of sample selection bias.

CRIMINAL CAREER RECIDIVISM MEASURES

Recidivism measures suggested by the criminal career perspective offer a potentially divergent look at the differences between the groups defined by the sentences. Unlike the binomial measures just discussed, continuous measures of recidivism allow for more "degrees of failure" in that what is contrasted is levels of subsequent crime rather than merely the presence or absence of some form of criminal event. Thus, the "success" of the sentence might be seen in relatively low numbers or rates of further criminal behavior for an individual when criminal career indicators are employed, while such an individual would be considered a "failure" when binomial measures are used.

Table 8.7 shows means levels of recidivism, by type of sanction received, for the seven criminal career indicators considered here. By and large, the general behavior over time for these measures found in Chapter 4 -- count-based measures steadily increasing, both in terms of level and variability, and the means and variances for rate-based indicators steadily

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hazards. This effect is of the same magnitude of other significant effects for continuous variables.

decreasing over time -- are observed within the types of sanctions. As well, the general skewness in all of these forms of recidivism is to be found within a given form of sentence.

However, these general trends in levels of recidivism over measure and post-observation window are not found within all types of sanctions. For example, for those given a sentence to a State Prison, the adjusted persons charge rate increases for the first three windows, then drops off for the full observation period.[12] The adjusted arrest rate found for those given the residual, "other sanction" remains relatively constant across all windows rather than decreasing as is the case for most other groups. The other sanction group also shows increased levels on the adjusted charge rate for the full observation period. The extent of decrease in the adjusted rate measures also varies by group as some groups (e.g., those given a split jail sentence in conjunction with a fine) tend to drop off more slowly than those receiving other forms of sanctions.

For all continuous measures of recidivism, those sentenced to the Youth Complex display the highest average levels, and this is found no matter what post-sentence observation window is used. Differences are found, however, for that group showing the next highest level of recidivism. When counts of different events (or the sum of the seriousness of charges) are used, those given a simple sentence to jail rank second. When these counts are converted to rates adjusted for time at risk, the group of offenders sentenced to a State Prison are seen as more recidivistic than those receiving a simple jail term. At the other extreme, the group displaying the lowest average levels of

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[12] It is possible that this drop is due to the fact the approximately 30% of those given a prison sentence were not at risk for a period of more than five years. See Chapter Three.

recidivism varies by indicator and observation window. In general, those receiving only a fine tend to fare quite well, averaging the lowest levels of recidivism. However, there are places in Table 8.7 (e.g., total post-sentence persons charges, some rate indicators measured over one year) where the group defined by a split jail sentence in conjunction with a fine display the lowest average levels of recidivism.

The caution that these average levels of recidivism are not controlled for other variables should be remembered: in general, we see that those receiving the most severe sanctions are most likely to recidivate. (This was true for the binomial indicators as well.) However, given that the more serious offenders receive the more severe sanctions, this is not surprising, and these generally high levels of recidivism cannot be directly interpreted as a lack of an "effect" of the sanction. Rather, the mean levels displayed in Table 8.7 serve as a context in which sanction effects must be viewed. Sentences found to decrease levels of recidivism (e.g., incarcerations at the Youth Complex or State Prison) must be interpreted in light of the fact that those receiving these sanctions recidivate at relatively high levels.

What is suggested by the means of Table 8.7 -- again subject to the caveat about lack of control for other variables -- is that different conceptualizations of recidivism represented by the criminal career indicators may matter in terms of finding sentence effects. The relative ranking of the recidivism of groups produced by the 1976-77 sentence varies depending upon the particular form of recidivism and the period over which recidivism is measured. In addition, within sanction groups, the empirical behavior of these indicators is not the same across measures (e.g., the slight increase in the adjusted persons charge rate for those sent to a State Prison).

Consequently, the relative impact of the sentence can be expected to depend, to some extent, on how recidivism is measured.

The Predictive Power of the Sentence

The ability of the sanction to account for variation in levels of recidivism, beyond that due to the independent variables used in Chapters Five and Six, is shown in Tables 8.8 through 8.14. As was the case for the binomial indicators, the absolute magnitude of explainable variance unique to the effects of the sentence is quite low and disappointing. For example, all measures of the sentence increase our ability to explain variation in the total number of post-sentence charges by .8% over the shortest window, and by the same amount for the adjusted persons charged rate measured over a three year window. Indeed, the "best" the set of sentence measures can add to an understanding of differences in recidivism is the increase of 2.2 percent found when the post-sentence convictions are counted over the full nine year window.

Despite these low levels of variation unique to the sentence, the same principles used to describe the comparable tables for binomial indicators can be applied to the criminal career results. In absolute terms, there are (slight) increases in the ability of the sentence to account for differences in the count-based measures. Relative to the variation that can be explained, this increase is often less apparent. For example, the sanction variables account for 7.63% of the explainable variance in the summed seriousness of all charges over a one year window and this drops to 4.68% after three years, eventually rising to 5.48% over the full observation period.

In contrast, the sentence becomes less predictive of rate-based recidivism over time, though again the absolute magnitude of these changes is

slight. When converted to a base of "variance explained," these changes are more dramatic. For example, after one year, the sentence measures account for 8.42% of the explainable variance in the adjusted persons charge rate, and this steadily drops to 3.09% at the nine year period. Note that this decrease occurs despite the fact that the variance that can be explained in the adjusted persons charge rate doubles between the one and nine year windows.

As was noted in the discussion of the binomial measures, even though the absolute magnitudes of the "effects" of the sentence are quite small, the increases in the relative ability to account for variation in recidivism can be substantial when the indicator is less amenable to prediction. Thus we find that knowledge of the sentence received increases the variance explained in total post-sentence convictions after one year by 9.06% and by 8.68% for the total post-sentence persons charges, again over a one year window. When the nature of the presenting offense and offender characteristics and prior record do a poor job of predicting recidivism, the sentence, relatively speaking, can improve that prediction.

Tables 8.8 through 8.14 also suggest that sentence effects will be more observable for some forms of recidivism than others. The absolute levels of explainable variance unique to the sentence are higher for counts and rates that use all kinds of offenses than they are for the measures based on persons crimes. Furthermore, the sanction is slightly better able to account for differences is recidivism when it is defined by counts rather than rates. Again, however, in absolute terms, these differences are quite small.

One pattern that is consistent across the criminal career measures is the steady decline in the impact of the sentence <u>in interaction</u> with other independent variables. This tends to be found whether the "effect" of the

sentence is defined in absolute or relative terms. The implication is that we find the effects of the sanction more mediated by offender characteristics in the short run than over longer windows. Given the nature of the interactions terms in our models, this means that differences in how the sanction influences early levels of recidivism will depend more heavily on the length of prior record than it will for later levels of recidivism. Thus, no matter how we define recidivism from the criminal career perspective, the effects a particular sentence become more similar for different types of offenders as we lengthen the period over which recidivism is observed.

The steady decline in the predictive ability of the sanction in interaction with other variables is counterbalanced by slight increases (in absolute terms and, to a lesser extent, in relative terms) of the main effects of the sentence indicators when count-based recidivism is studied. For the rate-based measures, the main effects of the sentence uniquely account for approximately constant proportions of variance across the various windows. In relative terms, these small constant proportions result in a decrease in the proportion of all variance that can be explained. The conclusion to be reached is that the sanction received is better able to predict recidivism levels when the interest is in recidivism defined by simple counts than when recidivism is viewed in terms of rates of offending.

Exactly which aspects of the sanction are responsible for the little variation that is attributable to the sentence mirrors what was found for the binomial measures. The bulk of the effect is due to the "place" sentenced with little explanatory power attributable to the time component of the sentence. For no criminal career measure does the proportion of variance attributable to the time indicators rise above .1% in absolute terms. In

relative terms this translates to at most .54% (for the nine-year total for persons charges) of the variance that can be explained in any form of recidivism. The two indicators of the pattern of the sentence within the individual's sanction history are similarly poor predictors of subsequent recidivism. Thus, given the knowledge of interactions between the sentence and offender characteristics and the type of sentenced received, little additional explanatory power is found for either the continuous or pattern components of the sentence.

In summary, the results in Tables 8.8 through 8.14 do not point to large effects of the sentence upon levels of recidivism. While our ability to explain variance in recidivism noticeably increases with the width of the observation period, this increase is due more to factors associated with the offender characteristics than to aspects of the CJS intervention. This is consistent with the results found for the binomial recidivism measures. Also consistent with the comparable analysis of the binomial measures is the conclusion that the mediation of sentence effects by these independent variables lessens over time with the small impact of the sanction becoming more similar across different type of individuals given the same sanction. Finally, it is the type of sentence received, rather than the amount of time per se or how that type fits into a pattern with previous sentences, that is most responsible to what little explanatory power can be attributed to the sentence.

Models of Recidivism

In discussing the similarities and differences in how the particular aspects of the sentence impact on the levels of recidivism represented by criminal career indicators, we face several challenges. There are seven

different dependent variables under consideration, each measured over four different observation windows. In addition, equations can be discussed in either their standardized or unstandardized form. Thus 56 sets of results can potentially be compared and contrasted. To facilitate the presentation, we adopt a strategy of highlighting what is observed for each type of recidivism measure, across observation windows. A more detailed comparison of findings across different conceptualizations of recidivism is deferred until the last section of this chapter.[13]

As was the case for models of the binomial recidivism measures, we will not take full advantage of the many nuances to be found in our results. That is, the exposition is geared toward an intermediate level of detail as to the results of these models. Doing so misses much that can be uncovered. For example, at nine years, the effect of a prison sentence upon the total number of charges accumulated by the offender is -.3154.[14] A prison sentence reduces the total number of convictions over the same time frame by .2377. Through the, more proper, comparison of the standardized coefficients we find that a sanction involving a state prison incarceration is actually over 20% more effective in reducing subsequent convictions than it is in reducing subsequent charges, with standardized coefficients of -.128 and -.103 respectively. Many more examples of such findings can be observed and the interested reader is urged to pay careful attention to such specifics. However, we will continue the approach of gearing the discussion to a more general level of detail.

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[13] There, summary tables will emphasize the similarities and differences across recidivism indicators within a given observation window.

[14] As was done in Chapter Six, we leave the fact that all count and rate variables were logarithmically transformed prior to the analysis implicit in the text.

Table 8.15 investigates how the sentence influences cumulative levels of criminal involvement.[15] Only two indicators, a State Prison sentence and a split sentence to jail in conjunction with a fine, are found to significantly decrease total post-sentence charges across all windows. Those given probation and fined are significantly less likely to accumulate charges than the reference group of simple probationers, though this effect dissipates by the nine-year window. In contrast, by the fifth year window, all groups receiving some form of jail confinement, some aspect of probation with a fine, and a State Prison all display significantly lower levels of post-sentence charges than those given simple probation.

As is to be expected from the results of the previous section, few indicators of the continuous components of the sentence have a measurable impact on the volume of post-sentence charges. Only months sentenced to State Prison has any detectable, possibly deterrent, effect and this is not observed until at least a five year window. One sanction variable is found to significantly <u>increase</u> levels of post-sentence charges. Offenders for whom the 1976-66 sentence was the first sanction of their career are significantly more likely to have more charges than those who had been previously sentenced.

There are as many significant mediations of these main effects on post-sentence charges as there are main effects themselves. A comparison of the standardized coefficients[16] indicates that shortly after release, an increase

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[15] As before, we limit the discussion to the coefficients for the sentence main effects and interactions, though coefficients for all variables are presented in the tables.

[16] Properly estimating the standardized effects in the presence of interactions is not straightforward. Following the procedures outlined by Aiken and West (1990), we standardized all variables prior to computing the interaction terms and then regressed the dependent variables on the new, correctly standardized, variables.

of two standard deviations in prior adult arrests increases the number of subsequent charges such that the negative effect of a prison sentence is almost halved. It is only after the one-year window that prior adult arrests do not mediate the main effect of the prison sanction. While split sentences to jail appear to effectively reduce the number of post-sentence charges over at least a five-year period, such sentences appear to be much less effective for those who had been incarcerated at some point two years prior to receiving the 1976-77 sentence. Here, the interaction term indicates significant increases in levels of recidivistic charges for the subgroup of split sentence offenders with a history of recent incarceration.

While the main effects of the various forms of probation sentences lead to little difference in average levels of post-sentence charges, all interactions involving these types of sentences are remarkably robust. Each of these main effects is significantly mediated by some aspect of the offenders prior record. Those with longer prior records, in conjunction with the various forms of probation sentences, are significantly less likely to accumulate post-sentence charges. This suggests that judges were able to accurately distinguish which individuals with long records were less likely to recidivate if given probation.

Two interactions, which compete against one another, are also found for the continuous components of the sentence. Each month sentenced to jail significantly decreases the number of recidivistic charges amassed by the individual for those with an instant offense of property, though for other kinds of presenting offenses, there is no impact on the length of the jail term. Property offenders also appear to react differently to a prison confinement as each prior property conviction, in interaction with the number

of months sentenced to prison, significantly increases the number of post-sentence charges.

Finally, while the general effect of receiving an initial sanction is to increase subsequent charges for crimes, the effect is moderated in two important ways. Even greater increases are found with each additional prior adult arrest that did not receive a sanction. (Note that the standardized coefficients for this interaction term are the largest observed at any time period.) However, black offenders who received their initial sanction at this sentence were significantly less likely to have high levels of subsequent charges. Consistent with the variance partitioning results of the previous section, the magnitude of all of these interactions, as represented by the standardized coefficients, tends to decrease as the post-observation window lengthens.

A conviction may represent a greater confirmation of a criminal act, and thus total post-sentence convictions may be a better measure of offender's actual level of recidivistic behavior than the charge-based measure just discussed. Table 8.16 shows that, at a very general level, this is not true. The patterning of significant effects follows that seen for post-sentence charges. Again, terms of incarceration in prison or jail significantly reduce the number of times the individual is reconvicted. How much the offender is fined or the length of the term sentenced to probation matter little in predicting the actual level of recidivism. Initial sanctions appear to be quite ineffective and those interaction terms found to significantly mediate the main effects of the sentence also emerge when convictions are used to indicate recidivism.

However, the results in Table 8.16 also tell a slightly different story

about how these aspects of the sentence influence recidivism. First, the magnitude of these effects, as represented by the standardized coefficients, tend to be slightly stronger. Second, the sanction of probation in conjunction with a fine now is seen to have a robust effect in decreasing the number of post-sentence convictions relative to the reference group of simple probationers. As well, months sentenced to prison are found to have an increasingly negative effect on the levels of post-sentence convictions.

Perhaps the biggest difference found when post-sentence counts of convictions, rather than charges, are used as the recidivism measure, pertains to the influence of sentences to the Youth Complex at Yardville. At either a one or nine-year window, those offenders given this form of sentence are significantly less likely to accumulate convictions relative to the probationer reference group. These effects, however, are not straightforward as all interaction terms involving Youth Complex sentences are found to be significant in at least one window. Each prior incarceration significantly increases the number of further convictions for those sent to the Youth Complex, thus reducing any beneficial impact the sentence may have. For the one-year window, each prior charge in the last five years also increases in the number of reconvictions found for those incarcerated at the Youth Complex. This short run increase is offset, however, by the interaction involving prior convictions as an adult, as each additional prior conviction reduces the number of subsequent reconviction.

If recidivism is defined according to a specific class of crime, in this case crimes against person, quite different conclusions are reached concerning how the sentence impacts on levels of recidivism. Table 8.17 shows that, overall, it is difficult to account for variation in the total number of post-

sentence persons charges and the sentence indicators are much less important for explaining this variable than they are for the previous two measures of recidivism.

For windows of either one or three years, the only main effect that is significantly related to the volume of subsequent persons charges is that for a sentence to State Prison. There, the impact is to significantly reduce the number of persons charges. For the longer windows, the State Prison effect is still observed, but five additional aspects of the sanction emerge as significant. Those receiving a split jail sentence (with a fine) have lower levels of new charges for persons crimes than those receiving simple probation. As well, at nine years, longer prison sentences are negatively related to the volume of subsequent persons charges. Small effects for the dollar value of the fine are also seen with those receiving larger fines significantly more likely to be charged with a new persons offense.

The patterning of the few significant sanction interactions is the reverse seen for the main effects: there are more found for shorter windows than for longer windows.[17] Those receiving a split jail sentence and having been previously incarcerated at some point two years prior to sentencing have higher levels of post-sentence persons charges. The lack of any main short-term effect for a Youth Complex sentence is, in part, due to the canceling influence of two interactions. Each charge in the five years prior to sentencing increases the number of post-sentence persons charges amassed by those sent to the Youth Complex, while each prior adult conviction significantly decreases these kinds of charges. Significant interactions also

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[17] This is consistent with the overall decrease in the ability of the interaction terms to uniquely account for variation in our dependent variables.

influence the effect of the various types of probation sanctions, though only that involving probation with conditions interacting with prior adult charge convictions is robust across windows. In light of the few significant main effects found for any aspect of the sentence on levels of recidivistic persons charges, these interactions take on an added importance.

Turning to the rate-based recidivism measures, we find results that are both reassuring and complex. We start with Table 8.18 which presents our models for the adjusted post-sentence arrest rate. All effects for the type ("place") of sentence that are significant are negative suggesting that the sentence had a "positive" influence on recidivism. Moreover, these effects are robust in that they are significant across all windows.[18] The findings in Table 8.18 suggest that, with the exception of sentences to the Youth Complex, the more severe sanctions tend to lead to reduced post-sentence arrest rates relative to the group of simple probationers. Significant decreases in the adjusted arrest rate are seen for sentences to a State Prison, all forms of jail confinements, and types of probation sentence that involve a fine. Conversely, those given the less severe sanctions tend to have arrest rates roughly comparable to those of simple probationers.[19]

Aside from these aspects of the sentence, however, few other components

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[18] In light of the fact that the mean levels of rate-based variables tend to decrease as the observation window lengthens, it is not surprising to find that the magnitude of these main effects also decreases over windows. For example, the absolute values of the standardized coefficients for a State Prison sentence steadily decrease. More surprising are those effects which tend to increase (e.g., simple jail terms, probation with conditions and a fine) as the post-observation window gets longer.

[19] This statement must be considered in light of the mean levels of adjusted arrest rates presented in Table 8.7. In absolute terms, those given the more severe sanctions have higher mean arrest rates than those given the less severe sanctions. The effects in Table 8.18 essentially adjust those means for the impact of the other independent variables and their interactions.

of the sentence are related to the arrest rate. After at least three years, increases in the time sentenced to State Prison are associated with lower adjusted arrest rates. The first sanction dummy variable again emerges as important in that those who receive their initial sanction have significantly higher arrest rates.

The presence of numerous important interactions precludes many sweeping statements about the main effects found in Table 8.16: Of the fourteen interactions terms, only two have no impact across all windows. Moreover, the magnitudes of the standardized coefficients indicate that ways in which the sanction main effects are mediated by other variables often has greater consequences for the adjusted arrest rate than the main effects themselves. For those sentenced to a State Prison, the sanction is less effective in reducing the adjusted arrest rate as the number of prior adult arrests increases. The lack of any main effect for sanctions involving the Youth Complex is again attributable, in part, to the offsetting effects of interactions involving previous adult convictions and charges in the previous five years.

Conversely, some individuals given various forms of probation have even lower adjusted arrest rates than would be expected on the basis of the negative main effects. As the number of previous adult arrests increases, even greater decreases in the subsequent arrest rate are found for those sentenced to probation with conditions and fined. Increases in prior record, as measured by Part I charges for those sentenced to probation and fined and adult charge convictions for those receiving probation with conditions, are also associated with additional decreases in adjusted arrest rate for those sanctions.

Sanction interactions involving property offenders, seen earlier for the same count-based measures, are also found when the adjusted arrest rate is taken as the measure of recidivism. Again, property offenders appear to be more responsive to the length of the sentence to jail, although greater numbers of prior property convictions reduces the impact of months sentenced to a State Prison. And, as before, initial sanctions are found to be more effective for black offenders than for those of other races and ethnicities.

When total post-sentence charges are converted to a charge rate, the results (Table 8.19) are much more consistent between the summary arrest and charge recidivism measures. In fact, there is little to distinguish the findings of Tables 8.19 from those of 8.18: the patterning of where significant coefficients are found, the signs of those coefficients, how the main effects are mediated by interactions, and the substantive conclusions to be reached are virtually identical. It is only at the detailed level of the magnitudes of these effects that any differences emerge. That comparison finds that sanction effects on the adjusted post-sentence charge rate tend to be greater than those seen for the adjusted arrest rate.

Table 8.20 presents the results when the adjusted post-sentence persons charge rate is taken as the indicator of recidivism. As with the simple count of charges for crimes against person, we find that few aspects of the sentence are related to levels of this form of recidivism rate. The notable effect is the negative impact of a sentence to State Prison, which is found across all widths of observation window. Only three other sanction measures reach statistical significance -- amount of fine for the five and nine-year windows and initial sanction for the three-year window. Given the magnitude of these effects, and the fact that they are not robust across windows, little

substantive import can be attached to them.

As was the case with the count of recidivistic persons crimes, more statistically significant interactions than main effects are observed. Similar to what was seen in Table 8.17, we find robust interactions for sentences to the Youth Complex in conjunction with charges up to five years prior to sentencing (the impact is positive) and probation with conditions interacting with adult charge convictions (a negative effect). What differs between the interactions for the adjusted persons charge rate relative to the simple count of post-sentence charges is the presence of significant interactions mediating the negative main effects of a State Prison sentence. For the five and nine-year windows, as the prior criminal behavior, measured by number of arrests as an adult and a juvenile, increases, so too do levels of the post-sentence persons charge rate. In fact, a one standard deviation increase in each of these aspects of prior record is sufficient to cancel out any benefits of the main effect for a State Prison.[20]

The final recidivism measure suggested by the criminal career perspective that we investigate here is the summed seriousness of all post-sentence charges. Unlike the other measures, this indicator captures a dimension of the perceived severity of the recidivism career. Those committing few, but relatively serious additional crimes will tend to have the same summed serious score as those who commit many, relatively minor offenses. Thus there is not a straightforward correspondence between this recidivism variable and the charge-based indicators previously discussed and there is no inherent reason for similar sanction effects to be found.

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[20] Of the criminal career measures discussed to this point, this is the first instance where we have seen where the interaction terms could easily cancel out decreased recidivism attributable to a State Prison sentence.

Despite the conceptual differences between a summed seriousness score and charge-based count and rate indicators, when the summed score is regressed on the set of independent variables, we find many of the same results (Table 8.21); significant decreases due to the more severe types of sanctions with little other effects for the less severe forms of sentences or the continuous components of the sentence or the pattern of the sentence. What distinguishes these findings from those discussed earlier is that, by and large, these effects do not emerge until at least a five-year window. It is only for sentences to a State Prison that we find a robust negative impact across all windows widths.[21]

There is a much more mixed pattern for how the sanction interactions influence the level of post-sentence seriousness of all crimes. Only the interactions for Youth Complex by number of prior adult arrests, and the probation with conditions and fine by prior adult arrests, reach significance regardless of observation window. Both interactions result in decreases in the summed seriousness of the recidivism career. The interactions involving other forms of probation sentences also reduce the total seriousness of all charges, but these impacts hold only over the shorter windows. Conversely, after the first window, those given a State Prison sentence have lower seriousness scores as the number of arrests as a juvenile increases, as do the longer prison sentences in interaction with prior property convictions and

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[21] It is tempting to interpret the fact that the majority of sentence main effects do not emerge until after five years as the result of right censoring of those given sentences of incarceration. This explanation is untenable for several reasons. First, the group most subject to right censoring of the post-sentence window are those given a State Prison sentence and this is the only group for whom the main effect is significant over all windows. Second, jail confinements are relatively short so that the censoring is slight (see Chapter Three). Finally, there is little censoring for the group receiving probation with conditions and a fine.

black offenders receiving their initial sanction.

Summarizing the sanction and interaction results across all criminal career recidivism measures is somewhat difficult as the conclusions reached tend to depend on the particular measure.[22] In one sense, how the "sentence" influences levels of recidivism is contingent upon how recidivism is defined. Earlier it was seen that a focus on subsequent crimes against persons finds few aspects of the sentence significantly related to recidivism in either a positive or negative direction. Exactly which components of the sentence are related to levels of recidivism also tends to vary by measure. The more similar the measures (e.g., total charges versus adjusted charge rate as opposed to total convictions versus the summed seriousness of charges), the more similar the patterning of sanction effects.

At a more general level, several findings emerge that are relatively consistent across the criminal career recidivism indicators. First, few aspects of the continuous components of the sentence show significant influences on levels of recidivism. Months to jail, dollars fined, and months sentenced to probation seldom account for variability in the criminal career measures once independent variables and other aspects of the sentence are controlled. At times, months sentenced to jail is important, but only in interaction with a presenting offense for property. Time sentenced to prison has a more pervasive influence, both in its main effect and in interaction with prior property convictions, but the magnitude of the its effect is often small.

Results for the sentence pattern indicators are relatively consistent.

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[22] This is not surprising as the measures were chosen because they represent different aspects of recidivism.

The fact that a sentence follows a progressive pattern within the offender's sanction history appears to matter little for levels of recidivism. Those who receive their initial sanction tend to recidivate at higher levels, even more so the greater the prior number of arrest as an adult. It is only for black offenders receiving their initial sanction where significantly lower levels of recidivism are seen.

Across types of sentences, no sanctions were found to increase levels of recidivism as measured by the criminal career indicators. However, groups receiving a fine, the residual "other" form of sanction, and often, some form of probation, tended to fare no better than the reference group of simple probationers. Those receiving a State Prison sentence were often found to display significantly lower levels of recidivism relative to the probationer reference group, once other variables were controlled. In contrast, the relatively severe penalty of an incarceration at the Youth Complex produced recidivism levels comparable to those found for the group of probationers (again after controls for other variables were introduced). Results were much less consistent for the impacts of various forms of a jail confinement. However, many of the interactions involving the "place" sentenced were also found to be significant, and this precludes making sweeping generalizations about the effects of the sentence on levels of recidivism as measured by the criminal career indicators.

The Impact of Sample Selection

Appendix B presents further results for all criminal career recidivism measures, this time with controls for the selection hazards. Tables B8.6 through B8.14 parallel the variance partitioning analyses described above. Tables B8.15 through B8.21 are the companion models presenting the

coefficients for the full model of each recidivism measure.

Controlling for the influence of sample selection does relatively little to change the amount of variance uniquely attributable to the sentence measures. These unique proportions are relatively small to begin with and remain so when the selection hazards are introduced. While there are some slight changes in the third decimal place of the absolute values of the variances unique to the sanction components, and these changes are a bit more evident in terms of the proportion of explainable variance, numerically the results are virtually identical to those found when the hazards are ignored. More importantly, the substantive conclusions to be reached from the variance partitioning analyses of the criminal career indicators remain unchanged.

A focus upon the coefficients themselves finds a bit more influence of the selection hazards. When total post-sentence charges is the dependent variable, we find a slight suppression of the main effects for the sentence indicators with a corresponding slight decrease in the magnitude of the coefficients for the sentence interaction terms. However, there is no change in the patterning of the coefficients such that what was significant prior to the introduction of the hazards remains so when sample selection bias is controlled.

No change in the patterning of significant coefficients is also found when the total post-sentence convictions is the dependent variable. As before, the hazards produce a slight suppression in the magnitude of the main effects and, if anything, also lead to a small suppression of the interaction terms as well. When selection hazards are controlled, we now find a significant decrease associated with a sentence of probation with conditions at the one year window. Also appearing as significant, at the five-year

window, is the interaction between months sentenced to jail and a presenting offense of a property crime which, as for the other windows, reduces levels of post-sentence convictions.

It will be remembered that little explained the variance in the total persons crimes and this remains true when the hazards are controlled. A few (small) effects that were significant disappear and these are replaced by a few new effects. A slight suppression of both the interaction and main effects is also observed.

The patterning of the significant effects remains the same when the model for the adjusted arrest rate is controlled for sample selection. Most of the main effects are reduced slightly by the hazard variables, but there is little change in the magnitude of the coefficients for the sentence interactions. Since the findings for the adjusted charge rate were quite similar to those for the adjusted arrest rate, it is not surprising to find that the patterning of significant coefficients for the adjusted charge rate also remains the same when the selection hazards are introduced. However, unlike what was found for the influence of the hazards on the adjusted arrest rate, we see a slight suppression of the main effects upon the adjusted charge rate. The impact of the hazards on the sanction interactions is more mixed with some coefficients showing very small increases, others small decreases.

Controls for selection bias also do little to the results found when the adjusted persons crime rate is the dependent variables. Here too the patterning of the significant coefficients remains. A noticeable suppression is found for the impact of a State Prison sentence on the adjusted persons charge rate, as well as slight reductions in the magnitude of the interaction effects. Similar findings are observed when the summed seriousness of

recidivistic charges is used as the dependent variable. What was significant before introducing the sample selection controls remains so and the magnitude of the main effects rises ever so slightly. The impact of the hazards on the interaction terms is more mixed with a few increasing in magnitude and a few displaying decreases.

Overall however, there are few substantive differences to be found between the criminal career recidivism models with the selection bias measures and those without. Certainly the substantive conclusions remain unaltered by the consideration of the potential influences due to the processing of cases through the criminal justice system. We emphasize that the changes found as a result of these additional statistical controls are slight at best. Note though, that exactly how the models for the different measures of criminal career recidivism respond to these controls does vary by the particular indicator. Sometimes the hazards controls produce slight suppressions, other times the magnitude of the coefficients is reduced, and for some dependent variables, there is little change.

TIME TO FAILURE INDICATORS OF RECIDIVISM

The final two recidivism measures we consider are the number of days to rearrest and the number of days to (re)imprisonment.[23] Table 8.22 presents the mean levels of these dependent variables broken down by type of sentence received. It is clear from the relative rank ordering of these means that these variables inform us about different aspects of the recidivism process. After one year, those given a sentence to the Youth Complex are rearrested the

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[23] For each of these measures, those who did not recidivate were assigned the maximum number of days in the observation window, plus one day.

soonest, followed by the groups receiving split sentences to jail, a simple jail confinement, and then the group that was sentenced to a State Prison. When days to reimprisonment is taken as the recidivism measure, again at one year, those sent to the Youth Complex again rank first. However, those receiving a State Prison sentence are found to be imprisoned more rapidly than the groups defined by the other sanctions with some groups (e.g., fined only, probation with conditions and a fine, probation with conditions) quite unlikely to be imprisoned within one year after sentencing or release.

Differences are also seen in the relative rank orders of these sanction groups when longer windows are used. A Youth Complex sentence is still found to be the least "effective" under these time to failure definitions of recidivism. In general though, those receiving a split sentence to jail or a simple jail sentence are more rapidly rearrested than those given other forms of sanctions. It is the group of those sentenced to a State Prison that are reimprisoned more rapidly than all other groups, save those given a Youth Complex sentence.[24]

While the nature of the time-to-failure indicators is quite different than those of either the binomial or criminal career indicators discussed earlier in this chapter, the means of Table 8.22 lead to essentially the same conclusions as those reached earlier: the groups given the more severe sanctions are more likely to recidivate than are the groups given lesser sanctions. Thus the context in which the findings of our models must be

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[24] In part, these differences in the relative rank orderings on the time to failure measures reflect factors other than the behavior of the individual offender. Those on parole are more likely to be reimprisoned for a parole violation and thus a new arrest for these individuals is more likely to lead to an imprisonment than is new arrest for groups receiving one of the other forms of sentences.

interpreted remains the same.

The Predictive Power of the Sentence

Tables 8.23 and 8.24 display the results of our variance partitioning analyses for days to rearrest and days to reimprisonment respectively. In contrast to the comparable analyses discussed earlier, the sanction measures are able to account for relatively higher proportions of variance in these time to failure recidivism measures. For days to rearrest, the set of sentence indicators uniquely accounts for between 1.2% and 1.4% of the variation explained and remains relatively constant across all windows. When the explained variance is converted to a relative proportion, we find that about 7.8% of what can be predicted in the days to rearrest one year after sentencing can be attributed to some aspect of the sentence. This figure decreases to 4.5% when the full nine-year window is used.

The sanction measures are more responsible for the variance explained in days to reimprisonment. Starting with an absolute increase in explained variance of .1% (or a relative 9.9% of all explained variance) after one year, we find increases to 2.3% (again a relative 9.9%) using a three-year window, 2.8% (9.5%) after five years, and 2.8% (8.2%) when the full nine-year observation window is employed. These are among the highest unique proportions of explained variance due to the sentence that we have seen across all recidivism indicators.

The patterns of explained variation and which aspects of the sentence are responsible for these effects of the sentence when the dependent variables is days to rearrest are similar for those found using other recidivism measures. The impact of the sanction interactions declines steadily, more so in relative terms than in absolute terms. Conversely, the sentence main

effects show a slight increase in explanatory power over the four observation windows, though this is not evident in terms of the proportion of explainable variation. Once again the continuous components of the sentence and the two pattern indicators are found to be poor predictors of recidivism.

As when days to rearrest is the recidivism measure, the set of main effects is found to account for an increasing proportion of the variance explained when days to reimprisonment is the dependent variable. (Again this is less evident in terms of the proportion of explainable variance.) Also consistent with what was found for other independent variables is the lack of explanatory power attributable to either the "time" sentence measures or the pattern indicators. What is different when days to reimprisonment is the dependent variable is the overall increase in the explained variance attributable to the sanction interactions. This increase is most notable between the one and three-year windows. The drop over time in the ability of these interactions to account for differences in recidivism which has characterized the other dependent variables studied in this chapter is not found when days to reimprisonment is used.

In general, the variance partitioning results for the time to failure measures lead to many of the same conclusion previously reached. Main effects for the continuous aspects of the sanction, and the place of the sentence in the offender's sanction history add little to our ability to account for differences in recidivism across individuals. Yet these time-to-failure measures reveal two differences from the results for binomial and criminal career indicators. First, the 1976-77 sentence is better able to explain variation, beyond the other independent variables, in failure time recidivism measures. Second, when days to reimprisonment are taken as the definition of

recidivism, the sanction interactions remain good predictors across the four post-sentence observation windows. This suggests that we should find less "leveling" over time of individuals in the different sanction groups in the models for days to reimprisonment.

Models of Recidivism

Table 8.25 presents the models for days to rearrest. Among the main effects, only two variables, a sentence to State Prison and the initial sanction dummy, show robust effects across all windows. Those sentenced to Prison take longer to be rearrested than the group of simple probationers, while those receiving their first sanction are rearrested more rapidly.

By the three-year window, several addition significant main effects emerge. Those given a sentence of jail, probation and a fine take longer before their first recidivistic arrest, as do those receiving a simple jail confinement or the residual other group of sanctions. By the five-year window, time sentenced to a State Prison is also seen to contribute to significantly longer times before rearrest. At the full nine-year observation period, receiving a fine only is found to significantly increase the time to rearrest by about one half a year.

Ten of the fourteen sanction interactions are found to be significant for at least one time period. The fact the those receiving a term at a State Prison take longer to be rearrested is not mediated by any interaction, nor is the increase attributable to simple jail sentences. The fact that those sent to the Youth Complex are rearrested as quickly as the group of simple probationers is mediated by each prior conviction as an adult adding four days to the rearrest in the one-year window. However, each prior incarceration is found to significantly decrease the time it takes for an individual sentenced

to the Youth Complex to be rearrested and the effect is present for up to five years.

Two other interactions also significantly decrease the time to rearrest. Those receiving a split jail sentence who had also been previously incarcerated within two years get rearrested more quickly for the first two observation windows. Each prior arrest as an adult is also found to decrease the days to rearrest for those receiving their first sanction in 1976-77.[25]

The remaining significant interaction terms in Table 8.25 all increase the time it takes for the individual to recidivate. All probation terms are found to lead to delayed recidivism contingent upon the mediating variable. Each prior arrest as an adult increases the time to recidivate for those given probation with conditions in conjunction with a fine. Each prior Part I charge produces a similar effect for those receiving probation and a fine, as does each prior adult charge conviction for those given a sentence of probation with conditions. Each month sentenced to jail is found to increase the days to recidivism for those with a presenting offense of property though this effect does not appear until the three-year window. Time to failure is also increased over the first two windows as a consequence of the months sentenced to prison by prior property convictions interaction. Finally, black offenders who receive their first sanction take significantly longer to be rearrested after the first observation window.

Very different conclusions about the impact of the sentence are reached when days to reimprisonment are used as the time to failure dependent

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[25] Note that by the nine-year window the standardized coefficient for the initial sanction by number of adult arrests interaction is -.174. Relative to the other coefficients we have seen throughout this chapter, this is exceedingly large.

variable. Now, those receiving a State Prison are, with the exception of the three-year window, reimprisoned as rapidly as the reference group of simple probationers. After the one-year window, we also find that the group given a sentence to the Youth Complex are returned significantly faster. Also evident after at least three years is the faster reimprisonment of those receiving their initial sanction and an effect of the length of the probation sentence. Each month sentenced to probation decreases the time it takes to be imprisoned after sentencing.[26] With the exception of the three-year window, sentences of probation with conditions in conjunction with a fine are also found to increase the time to failure.

The three forms of sentences to jail are all found to have robust impacts on the time it takes to be reimprisoned after sentencing. The magnitudes of these effects are roughly comparable for split sentences with or without a fine. The effect of a simple jail sentence is also to delay any return to prison, though it is smaller in magnitude than those seen for the other two types of jail sentences.

As would be expected from the variance partitioning results, we find that the sanction interactions tend to be more important as the post-sentence observation window lengthens. Few interactions reach statistical significance for the one-year window, but by the three-year window, the majority of these interactions are significant. Indeed, with the exceptions of the jail by years using drugs, prison by prior juvenile arrests, and probation with conditions by adult charge convictions interactions, all these interactions somehow mediate the main effects of the sentence for at least one of the

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[26] Again the implication is that increased supervision makes it more likely that violations will be detected and sanctioned, thus increasing the chance of an imprisonment.

observation windows.

One interaction involving a sentence to State Prison leads to faster returns to prison. As prior adult arrests increase, we find significant decreases in the time it takes to be reimprisoned. For the group given incarceration at the Youth Complex, each prior conviction as an adult lengthens the time to reimprisonment. The effect is, however, offset by the other two interactions: each charge in the five years prior to sentencing and each prior incarceration significantly decrease the time to fail under this criterion. Using the nine-year window, we find that for those incarcerated at some point prior to sentencing, a split sentence to jail leads to a more rapid reincarceration. The other interaction term significantly shortening the time to failure is the one for receiving an initial sanction by prior arrests as an adult, though the effect is apparent for only the three and five-year windows.

The remaining interaction effects all increase the time it takes to be reimprisoned. Both forms of probation involving a fine, conditioned by some measure of prior record, lead to longer time before any reimprisonment. The interactions of time for property offenders -- months to jail by an instant offense of property, months to prison by prior property convictions -- also produce an increase in the time to failure through reimprisonment. The positive impact of initial sanctions upon black offenders is also seen in that they take longer to recidivate under this measure.

Defining recidivism as the time it takes for some form of recidivistic event to occur has resulted in a few conclusions that are different than those reached when other conceptualizations of recidivism are used. First, we are more likely to observe effects that can be interpreted as a "negative" impact of the sentence. That is, some groups defined by the sanctions may be

significantly more likely to recidivate when recidivism is defined by time to failure measures. This conclusion was much less prevalent for the recidivism indicators modeled earlier in this chapter.

Second, a suspicion raised by the results for the binomial reimprisonment variable is strongly supported by the findings when days to reimprisonment is used as the indicator of recidivism. The main effects for the sentence components clearly support an interpretation that increased supervision, resulting from either a regular probation sentence or parole release from prison, makes some kinds of recidivistic events much more likely. Thus we find that those given a sentence to State Prison are just as likely to be reimprisoned as those receiving simple probation, and there are few differences among the various types of probation sentences in terms of the time to reimprisonment. This conclusion is reinforced by the finding that months sentenced to probation also tend to decrease the time it takes to fail through reimprisonment. It is only for split sentences to jail where we find that the implied supervision effect does not hold.

These observations reinforce our contention that the actual measure of recidivism matters. When recidivism is defined by time to failure variables, we are more likely to conclude that sanctions don't work in at least delaying a return to crime of some form. If the indicator of recidivism requires a deeper (re)penetration into the criminal justice system such as the case of reimprisonment (and to a lesser extent reconviction), some aspects of the sentence can be expected to lead to more recidivism by virtue of the increased supervision resulting from the sentence. This too can produce effects that make the sanction appear to be less effective.

The Impact of Sample Selection

The final tables of Appendix B show how the conclusions reached using the time to failure recidivism measures are modified by the consideration of sample selection bias. For the variance partitioning results (Tables B8.23 and B8.24), we find little difference produced by including the selection hazards in the analyses. There is virtually no change in the absolute proportions of variance uniquely attributable to the sentence, though slight differences are seen when these proportions are made relative to the total explained variation. Moreover, the magnitudes of the unique variance attributable to the sentence indicators remains quite high, given the small amounts found for the other recidivism indicators.

How sample selection influences the parameter estimates themselves (Tables B8.25 and B8.26) is, by now, a familiar story. There is no change in the patterning of significant coefficients: if a sentence variable is statistically significant prior to the introduction of the hazard variables, it remains so after these controls are entered. It is also difficult to find major changes produced in the magnitude of the coefficients as a consequence of controlling for selection bias. Few differences, for either the main effects or the sanction interactions, are found for the days to rearrest variable. Sanction interactions also appear unaffected by the hazards when the days to reimprisonment variable is used. It is only for the main effects in the days to reimprisonment model where a slight suppression attributable to the selection bias controls is found. Overall, however, there is little impact of the hazard variables when time to failure recidivism indicators are analyzed.

SUMMARY

This chapter has covered much ground concerning how the sanctions under study influenced the subsequent criminal behavior of this sample. Ultimately, our results lead to several conclusions about how effective these sentences were and suggest several avenues for sentencing policy that are developed in Chapter Ten. However, before offering any further interpretations of the findings in this chapter, we briefly summarize the nature of the sanction effects that we have found. In doing so, we contrast the differences across definitions of recidivism within the same post-sentence observation window.

Table 8.27 summarizes where we find significant impacts when a one-year period constitutes the time frame for studying recidivism.[27] No single sanction measure or sentence interaction is a significant predictor of all the forms of recidivism studied. While all interactions reach significance for at least two of the thirteen recidivism variables, there are several sentence measures that we find to be unrelated to recidivism, however conceptualized, across all the dependent variables studied. Main effects for the groups receiving probation with conditions or simply a fine show levels of recidivism comparable to that of the reference group of simple probationers. What does differentiate the probation with conditions and fine from this reference group is the fairly strong interaction (significant for eight of the thirteen equations summarized in Table 8.27) with prior adult arrests.

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[27] A '+' in this, and other, summary tables indicates that the coefficient was positive and significant at at least the .05 level. A '-' identifies where significant negative coefficients were observed. These summary tables thus ignore major differences in the magnitude of the effects, as well as blurring important distinctions is how significant the sanction effects are. Rather, we highlight the general ability of the sanction variables to predict recidivism as defined by the measures used.

The continuous aspects of the sentence are also unimportant for understanding recidivism within one year from sentencing or release. Only months sentenced to prison reaches significance -- for one equation. For the sentence pattern indicators we find no predictive utility for the progressive patterning of the sentence. The fact that the sentence is the first of the offender's career does increase our ability to predict recidivism after one year, but its influence is limited primarily to recidivism indicators derived from the criminal career perspective.

Some components of the sentence are strongly related to short-term recidivism. Those sentenced to a State Prison are less likely to recidivate across all definitions, save the probability of reimprisonment. There is also evidence that the forms of jail confinement lead to lower levels of recidivism, but these effects are confined primarily to the criminal career indicators. Moreover, the "positive" influence of split sentences to jail dissipates for those incarcerated within two years prior to sentencing. While a main effect for a sentence to the Youth Complex tends not to exist, this form of sanction tends to lead to significantly lower levels of recidivism contingent upon levels of prior adult convictions, but also significantly higher levels of recidivism as conditioned by both the number of charges up to five years prior to sentencing and the prior number of incarcerations.

Any effect of the continuous components of the sanction appears to be limited to the property offenders captured by the interaction terms. After one year, longer jail terms produce lower levels of recidivism for those convicted of a property offense, but months sentenced to State Prison is associated with more recidivism for those with higher levels of previous property convictions. One of the more robust interactions found using a one

year window is that between receiving an initial sanction and prior adult arrests. While initial sanctions appear to be generally ineffective for this sample, these sanctions are even less so for offenders with longer prior records.

The patterning of significant effects found in Table 8.27 is also instructive as to the behavior of the various domains of recidivism indicators. Over a one-year window, the aspects of the sanction related to recidivism seem to be greater for criminal career based measures of recidivism than for indicators derived from either a binomial or time to failure perspective. Not only do we tend to find more significant influences for the main effects when criminal career measures are employed, we also tend to find greater mediation of these effects by the interaction terms. Overall, the results in this summary table support our basic contention that how recidivism is conceptualized matters for the substantive conclusions reached.

When recidivism is observed over a longer, three-year window (Table 8.28), we tend to find that more aspects of sanction are significantly related to recidivism. While some effects found for the one-year window are no longer significant, they have been replaced by other effects: generally more has been gained than lost. A few effects (e.g., a split sentence to jail with a fine, initial sanction, initial sanction by offender is black) have become more robust in that they are now found to be significant predictors for more measures of recidivism. Other sentence variables (e.g., the State Prison by adult arrests interaction) have lost some predictive power as they are now significant in fewer equations.

Increasing the post-sentence observation window from one to three years does yield an increase in those aspects of the sentence that are related to

recidivism and, as was seen in earlier tables, the overall ability to explain variation in recidivism increases as well. However, we are also struck by how little the most general conclusions change as a consequence of this increase in observation window. The broad conclusions about which components of the sentence are related to reduced or increased levels of recidivism, and which aspects of the sentence are totally unrelated, change little with the use of a longer window. True, the particulars -- which sentence components are related to recidivism and how strong those relations are -- may evidence substantial changes when the results of the one-year window are compared to those of the three-year window. This was seen as the results unfolded earlier in the chapter.. But no matter whether Table 8.27 or 8.28 is used, we still come to common basic conclusions such as the continuous dimensions of the sentence are relatively unrelated to recidivism and those sentenced to a State Prison are less likely to recidivate, however defined.

One noticeable difference that does emerge with the use of a three-year window is the increase in the number of sanction variables that are significantly related to the time to failure indicators. By three years there are as many sentence indicators predictive of these forms of recidivism as there are for the criminal career measure based on counts or rates of all type of offenses. In this sense, the contrast between binomial or time to failure recidivism versus criminal career recidivism has shifted to one of binomial recidivism versus criminal career or time to failure recidivism.

Table 8.29 summarizes our modeling efforts when a five-year window constitutes the post-sentence observation period. Some effects have disappeared and been replaced by others. What is not seen, however, is an overall increase in the number of sanction measures related to these forms of

recidivism. Rather, what has changed are the details of which particular sentence variables are predictive of which particular forms of recidivism. Thus, for example, we no longer find a State Prison significantly increasing the time to return to prison, but we do find that a split sentence to jail is negatively related to the total post-sentence charges amassed by the offender.

While the use of a five-year window leads to some changes in the specific conclusions, relative to those reached under a three-year window, the basic general conclusions remain unchanged from those reached under either the one- or three-year windows. As before, we get the same overall picture of which aspects of the sentence are positively related to recidivism and which are associated with lower levels of recidivism. One major difference, however, pertains to the influence of months sentenced to state prison. Under a five-year window, a relatively consistent negative impact of months to state prison emerges across the recidivism indicators. This was generally not seen at three years. This 'new' main effect is to be contrasted with the mediating impact of prior number of property convictions, which is robust for both the three and five-year window models.

Our final summary table (Table 8.30) covers the modeling results using the full nine-year window. Some differences are indeed apparent in that the months to prison main effect is even more robust and significant for all but one of the dependent variables, while the main effect of receiving an initial sanction is now relatively weak. There are now also fewer interaction terms that reach statistical significance. This is consistent with the earlier finding that these interactions lose their explanatory power over time with any impact of the sanctions becoming more uniform across different types of offenders receiving the same sentence. When a nine-year window is used,

differences between the various types of recidivism indicators are also less apparent as there are a few more significant predictors of binomial recidivism.

Again the key is the level of generality used to describe and interpret the patterns of effects summarized by Table 8.30. At the most general level, many of the same conclusions are reached as to what is negatively related to recidivism (e.g., a State Prison sentence), what is associated with significantly higher levels of recidivism (e.g., higher numbers of adult arrests for those receiving their initial sanction), and what is unrelated to subsequent recidivism (e.g., the number of months sentenced to probation). And, at one level of generality, such conclusions tend to hold across definitions of recidivism and differing post-sentence observation windows. Yet there is also a danger is coming to such broad conclusions as, for a particular indicator of recidivism at a specific observation window, these conclusions may not hold.[28]

We thus must proceed cautiously when attempting to interpret the results of our analyses as the conclusions could easily differ given the particulars of the sanction measure, the post-observation window, and the definition of recidivism. In the next section we offer some broad conclusions that we feel are generally supported by our results. But we remind the reader that the complexity of our findings argues against sweeping generalizations and

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[28] For example, the coefficient for a State Prison sentence is not significant when recidivism is defined as the probability of rearrest for a persons crime over a three or five-year window. The initial sanction by offender black interaction is never significant when recidivism is defined as either the adjusted persons charge rate or the summed seriousness of post-sentence charges. Months sentenced to probation is indeed significant if the observation window is at least three years and the recidivism indicator is either binomial reimprisonment or days to reimprisonment.

conclusions.

DISCUSSION AND CONCLUSIONS

Several important caveats must be stated prior to interpreting what our findings can say about the effects of these criminal justice interventions. First, the historical context in which these sentences were administered is important. The sentencing policies operating in New Jersey during the 1976-77 period which defined the present sample could best be described as "indeterminate." As we reviewed in Chapter One, several major changes in the nature of sentencing have occurred since then. Sentencing guidelines have been instituted[29], often in the form of "grid" systems, statutory minimums have been enacted for some crimes, and legislated mandatory sentences for some offenses have since appeared. In New Jersey, legislation influencing the nature and types of sentences that could be given by judges did not occur until after the sentences studied here. Similarly, a major overhaul of New Jersey's criminal code occurred after the base data had been collected.[30]

Consequently, there is likely to have been considerably more latitude for the judge in fashioning the sentence studied here than may be the case at the present time. This means that the sentences in our sample may be subject to the criticisms surrounding "indeterminate" sentencing in general (e.g., too much discretion for judge's, unequal treatment of similar offenders, and so

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[29] Informing the creation of guidelines was in fact one of the main reasons for the collection of the original data based that has been augmented for the purposes of the present research. However, In New Jersey these guidelines were not widely used and were superseded by the new Criminal Code instituted in 1979.

[30] This change in the Criminal Code has less influence on our findings as we believe that the crime categories used here as sufficiently general to be consistent with either the old or new version of crime definitions.

forth -- see Chapter One for a review). Yet, given this historical context for our sample, we are in a position to assess the effects of these sentences in the relative absence of the constraints produced under more current policies guiding sentencing. At that time, judges, led by whatever goal of sentencing, attempted to "match" the offender and his/her characteristics (e.g., prior record, educational attainment, the nature of the instant offense) to a suitable sanction.[31] We are in a position to evaluate the effectiveness of the "matching," though there are no assurances a given offender in our sample would receive a similar sentence under today's sentencing practices.

Second, our results showing the mean levels of recidivism by sanction confirm the suspicion that judges tended to give the more serious offenders the more severe sanctions. This is to be expected under most of the goals underlying sentencing policies. Unfortunately, this also leads to the finding that those given the more severe sanctions are more likely to recidivate, however defined. A priori, one might conclude that the more severe sanctions are not "effective" as levels of recidivism are quite high. We have seen that those sentenced to a State Prison or the Youth Complex are more likely to be rearrested for any crime, in a shorter time period, rearrested for crimes against persons, be reimprisoned -- again more rapidly, and accumulate more arrests, charges, and convictions after sentencing than other those given less severe sanctions. The same tends to hold for the groups receiving some form of jail confinement. It is hard to argue that these more severe sanctions are effective in meeting any sentencing goal predicated on some form of crime

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[31] Of course, we do not know what goal a particular judge was trying to achieve for any given sentence. Thus, we use the "matching" concept in a broad sense.

control.

In one sense this is true. It is undeniable that the levels of recidivism experienced by those receiving jail or prison confinement are easily deemed unacceptable. With almost 80% of those sentenced to the Youth Complex at Yardville, and over 62% of those sent to a State Prison, being rearrested at least once in a nine-year period, we are hard pressed to argue that "positive" effects have been found for the sentence. What is important to note, however, is that these figures fail to consider the levels of recidivism to be <u>expected</u> based on the kinds of offenders sentenced to such institutions.

It is here that the statistical controls introduced by our battery of independent variables come into play. After controlling for all of the variables in these models, we still found what can be interpreted as "favorable" effects for at least sentences to a State Prison. We can thus interpret these coefficients using a "what if" approach: "if the offender had not received that sanction, recidivism would have been even greater than that actually observed." That is, the levels of recidivism were lower than that expected based upon the expectations of the individual's prior record, prior incarceration history, presenting offense, and so forth. Our effects are thus interpreted in a relative fashion -- relative to how likely we are to find recidivism given the vector of characteristics that define the individual receiving the sentence.

This observation removes the apparent inconsistency between the high absolute levels of recidivism found when uncontrolled means are broken down by type of sanction received, yet sanctions with high absolute levels of recidivism are found to reduce the likelihood of recidivating. We will indeed

interpret many of these sanction effects as having a beneficial impact on recidivism, but we do so with the recognition that, for some of these beneficial sanctions, the result is still levels of recidivism that can be deemed unacceptably high.

Another caveat to our conclusions arises from the findings of the variance partitioning analyses. These place an upper bound on how strongly we can interpret any sentence effects. Practically speaking, the ability of the sanction measures to augment the prediction of recidivism is low, never reaching more that two percent of the variability of a given indicator. One is much better able to predict and explain recidivism using other factors such as the offender's prior record, educational attainment, employment, and nature of the presenting offense. These factors are known at the time of sentencing, prior to the introduction of the sentence and its subsequent intervention in the offender's career, and these variables are much more strongly related to levels of recidivism than any aspects of the sanction itself. While we will argue that there are beneficial effects of some sentences, relative to the 'effects' of what the individual brings to the courtroom, the intervention effects are small.

Fourth, a finding from Chapter Seven deserves reiteration. For approximately half of these sentences, there is at least the strong likelihood that the sentence is not independent of other sentences coming after the ones we are evaluating here. This makes it difficult to attribute any effects observed to the particular sanctions studied as they could easily be confounded by other sanctions that are not studied here. We have also seen relatively high average levels of reconviction and reimprisonment for some of the groups defined by the 1976-77 sentence and these recidivistic events could

easily produce still more interventions that confound the "effects" of the sentences of interest. It is especially difficult to separate out the unique effects of one sanction from another, thus adding further cautions to any interpretations of our findings.

Finally, it must also be remembered that we argued at length in Chapter Seven that an assessment of the effectiveness of the sentence can be something quite different from assessing the effectiveness of the treatment received as a consequence of that sentence. Our findings pertain to the former and, at best, tangentially apply to the latter. While these results point to identifiable differences in the recidivism of the various sanction groups, what it is about those interventions that led to these differences, and the nature of what actually happened to an individual as a result of the sentence, have gone unexplored.

Together, these caveats provide major qualifications to any interpretations of what is suggested by our findings and the conclusions reached from our analyses. As well, we have seen that the complexity of these results makes any generalizations risky undertakings. In spite of these limitations we feel our results point to several positive conclusions about how these sentences have impacted upon recidivistic behavior, as well as offering suggestions as to where these interventions have been relatively ineffective. We organize the discussion around three themes: the crime control achieved by the interventions, what these findings say about the judges' decisions as represented by the sentence, and how our results address the performance of the criminal justice system as a whole.

Crime Control

In Chapter One we noted that, with the exception of "just deserts"

approaches to sentencing, a desire to achieve crime control is the common denominator for all sentencing philosophies. Whether the interventions represented by these sentences have met this common goal is unclear given our results. Our findings suggest that, for some groups, a degree of crime control has been achieved, though it is questionable whether rehabilitation or specific deterrence has also resulted.

One of the most pervasive effects we have observed is a decrease in recidivism associated with a sentence to a State Prison. After controlling for all other variables, those given this sanction have significantly lower levels of recidivism relative to the group of simple probationers. Even though we find that those entering a State Prison tend to get rearrested more than individuals receiving other forms of sanctions and tend to accumulate more recidivistic arrests and convictions, it takes them longer to do so. That is, the evidence suggests that, minimally, a State Prison sentence delays or slows down the resumption of criminal behavior. We are not alone in this finding as other studies (Schmidt and Witte, 1979; Maltz, 1984) have produced similar results.

It is hard to interpret this effect as representing rehabilitation of the individual as, more often than not, further criminal activity is observed. Similarly, if this effect represents specific deterrence, its impact is short-lived. A prison confinement may deter crime only to the extent that it takes longer for the individual to readjust to "life on the outside." More consistent with our results is the interpretation that these prison sentences have produced some selective incapacitation. If our "what if" assessment of these beneficial effects of prison is accurate, this group of offenders would have accumulated even more crimes than have actually been observed. Under

this perspective, these prison sentences have indeed produced some crime control.

The relatively beneficial results of the length of time sentenced to prison (as opposed to the time actually served) supports these interpretations. We have seen that, all else equal, as the length of the prison sentence increases, less recidivism results. Given what we have found, the suggestion is that, in the aggregate, as the experience in custody lengthens, reduced levels of recidivism are seen. Whether this is due to increasing any short-term specific deterrent effect, or simply a cumulative impact of selective incapacitation, cannot be determined.

We also tend to find what can be considered "positive" effects of jail confinements. The levels of recidivism seen for those given some form of sentence to jail tend to be significantly lower than those of the simple probation reference group. Reduced recidivism is especially likely to result as the length of time sentenced to jail increases for that subgroup defined by a presenting offense of a property crime. Again, however, when faced with the relatively high levels of recidivism seen for even those receiving a jail confinement, it is unlikely that the jail experience has stopped the offender from further criminal activity. Rather, we contend that some short-term specific deterrence, or even incapacitative effect, has resulted.[32]

One intervention that we have found to be particularly ineffective is a sentence to the Youth Complex. All else equal, this group of individuals is as likely to be rearrested as are those given a sentence of simple probation.

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[32] Although we do not explicitly address the question of whether there are incapacitative effects, it seems quite likely that there are some. That is, more offenders would have continued committing crimes in the absence of an incarcerative intervention.

Moreover, the Youth Complex group has been found to have the highest mean levels of recidivism, however defined, of all the groups represented by the different sanctions. Clearly, no goal of crime control has been served from these interventions.

More troubling is how this lack of an effect for the Youth Complex incarceration is modified by the interactions with prior record. We have found some evidence to support sending relatively older offenders to these facilities as the more convictions received as an adult, the less likely the individual is to recidivate. But even this small beneficial effect of incarceration for some subgroups is offset by other, and stronger, interactions. The more serious offenders, defined by charges in the five years prior to sentencing and prior number of incarcerations, fare quite poorly after release. Based on these findings, it is evident that serious youthful offenders are not deterred or rehabilitated as a consequence of their confinement in one of the institutions at the Complex. Nor is there any evidence that these forms of sentences lead to any incapacitative effect.

Our results surrounding how the sentence fits into the individual's sanction history are more mixed. In general, those given their initial sanction tend to recidivate, however the term is defined. Thus we find that the first sanction leads to greater likelihood of being rearrested or repeating the present offense, higher numbers of post-sentence arrests and convictions, higher charge rates, and shorter times to rearrest and initial imprisonments. First sanctions are even more ineffective as the number of previous adult arrests go unsanctioned, either through fines, probation or incarceration. The implication is that greater attention needs to be paid to the prior records of those appearing before the court for the first time as

they are more likely to engage in subsequent crime. A more promising finding is that, under all criminal career and time to failure measures of recidivism, black offenders who receive their initial sanction fare better. While it is difficult to offer an explanation for this particular result, the impact of this interaction is quite strong and merits further study.

Judicial Decision Making

Several aspects of the results in this chapter point to positive evaluations of the effectiveness of the judges' decision making as represented by the sanctions administered to this sample. Under a relatively "indeterminate" sentencing policy, one aspect of sentencing is to "funnel" offenders into sanctions, and an unstated component of this funneling is the risk for recidivism. Crime control considerations lead to the expectation that one factor in the funneling of offenders into sanctions is acceptable risk for recidivism.[33] Given this, the current findings provide an opportunity to assess the "success" of the matching of offenders to sanctions as an "effect" of the sentence.

First, the previously noted fact that the more severe sanctions were received by those most likely to recidivate suggests that, in the aggregate, the decisions represented by the sentence were accurate. That is, those who presented relatively high levels of risk were incarcerated either in a State Prison, at the Youth Complex, or in jail. Admittedly there is considerable variation within sanction groups in the extent of recidivism such that some individuals who received less severe sentences recidivated at levels

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[33] Our findings are clearly relevant to which factors are related to acceptable risk for recidivism. In Chapter Nine we investigate the empirical consequences of the factors selected. Based on the results from that chapter and the present, we offer several suggestions for sentencing policy in Chapter Ten.

comparable to those seen for the groups receiving the more severe sentences. Thus the funneling was not perfect. But, overall, we have found the groupings produced by the sanctions to produce significantly different levels of recidivism.

Another, more subtle, finding also points to the effectiveness of judicial decision making. As we saw in Chapters Five and Six, all else equal, the longer the prior record of the individual, the more likely he/she is to recidivate over many definitions of both prior record and recidivism. Therefore longer prior records should make the individual a greater risk for recidivism and would, a priori, make a sentence to probation unlikely to be effective in producing crime control. However, we have seen that for all forms of probation sentences (other than the reference group of simple probation), the impact of the main effects of these probation sentences is mediated by some aspect of prior criminal involvement.

It is the signs of these interactions that point to a successful determination of risk for recidivism: all are negative. Thus, even though a longer prior record would point to the possibility of increased recidivism, judges deemed some of these offenders to be less of a risk and sentenced them to a probation term rather than giving them an incarceration. Our results suggest that these decisions were well founded.[34]

One final indication of successful judicial decision making pertains to our results surrounding the use of fines. By themselves fines appear to have little impact on recidivism as neither the coefficient for the "fine only"

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[34] An alternative explanation is that the interactions represent effective treatments on the part of the criminal justice system. Probation officers, seeing the longer prior records of these offenders, subject them to increased supervision which, in turn, produces less recidivism. Other aspects of our results pertaining to system-wide effects are discussed shortly.

sanction nor that for the dollar amount fined reach statistical significance for most recidivism indicators. We have consistently found that groups given a fine in addition to another sanction recidivate less than those given otherwise comparable sanctions. Thus, for example, those given a fine in conjunction with a split sentence to jail recidivate less than those given only the split sentence. This holds true for both the mean levels in recidivism and the magnitude of the coefficients reviewed in this chapter.

The decision to include a fine as <u>one</u> component of a broader sentence is thus related to lower levels of recidivism. While it is doubtful that the fine, in and of itself, has produced these lower levels, especially given the lack of a main effect for either the amount fined or a fine alone, the differentiation produced by adding a fine to other sanctions has produced groups that are clearly different in levels of recidivism. In this respect, the judges' decisions have proved to be appropriate.

<u>Intervention of the Criminal Justice System</u>

For some of our results, the most reasonable conclusion is that the system <u>as a whole</u> has "worked." While the sentences themselves may not have produced desired levels of rehabilitation, deterrence, or incapacitation, ultimately, CJS actors have done what they were supposed to do. Even though crime control has not resulted, the system has successfully monitored the behavior of offenders.

The findings concerning time sentenced to probation, and the dependent variables of reimprisonment and days to reimprisonment are most relevant here. We have seen that the longer the sentence to probation, the more likely the offender is to be reimprisoned and the more rapidly this reimprisonment occurs. Furthermore, we have found that those sentenced to a State Prison are

just as likely to be reimprisoned, and be returned to prison at the same speed, as those given simple probation, once the independent variables have been controlled.

It is difficult to interpret these findings as representing "effective" sentencing. The fact that these individuals have failed through reimprisonment precludes arguing that the sentence worked. More plausible, however, is the interpretation that the system has been effective in identifying those who have failed and responded to them accordingly. Increased supervision, either through probation or parole, allows for the detection of individuals who recidivate and they are ultimately (re)sanctioned for their offenses.

In summary, we have noted throughout this book the many methodological barriers to assessing the effectiveness of criminal justice interventions. These, in addition to the caveats raised earlier, make it difficult to come to strong conclusions about the impact that a sentence has on levels of recidivism. Also troubling are our findings that how recidivism is defined and the period over which one looks for recidivism can have important consequences for the nature of the effects estimated. Subject to these limitations, we have found that several forms of sentences (e.g., State Prison incarcerations, jail confinements) are associated with lower levels of recidivism, while others (e.g., incarcerations at the Youth Complex, fines by themselves) have no appreciable impact on recidivism.

Our interpretations of these results have centered around three different aspects of "effectiveness." Some sentences are indeed effective in at least producing a desired level of crime control. There is less evidence in our results in support of goals such as rehabilitation and long-term

specific deterrence, though we admittedly do not have as complete data as would be needed to fully investigate such goals. Other results point to these sentences representing successful decisions on the part of judges. Finally, other findings suggest effective monitoring of recidivism by the criminal justice system as a whole.

Table 8.1

Mean Levels of Recidivism by Type of Sentence - Binomial Measures Only
(Standard Deviations in Parentheses)

a) Rearrested

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.278 (.448)	.487 (.500)	.573 (.495)	.622 (.485)
Youth complex	.411 (.492)	.644 (.479)	.733 (.443)	.781 (.414)
Jail, probation, fine	.125 (.331)	.242 (.429)	.296 (.457)	.362 (.481)
Jail, probation	.312 (.464)	.512 (.500)	.596 (.491)	.652 (.477)
Jail only	.300 (.459)	.478 (.500)	.555 (.497)	.620 (.486)
Probation w/cond., fine	.152 (.359)	.286 (.452)	.368 (.483)	.446 (.497)
Probation, fine	.133 (.339)	.253 (.435)	.333 (.472)	.422 (.494)
Probation w/conditions	.254 (.436)	.436 (.496)	.527 (.500)	.607 (.489)
Simple probation	.206 (.404)	.352 (.478)	.429 (.495)	.512 (.500)
Fined only	.083 (.278)	.158 (.365)	.201 (.402)	.235 (.425)
Other sanction	.135 (.343)	.263 (.442)	.331 (.472)	.399 (.491)
TOTAL	.234 (.423)	.399 (.442)	.480 (.472)	.549 (.498)

b) Rearrested for Persons Crime

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.096 (.294)	.195 (.396)	.263 (.440)	.307 (.462)
Youth complex	.147 (.354)	.287 (.452)	.374 (.484)	.454 (.498)
Jail, probation, fine	.023 (.149)	.067 (.249)	.091 (.288)	.123 (.329)
Jail, probation	.104 (.306)	.204 (.403)	.259 (.439)	.332 (.471)
Jail only	.109 (.311)	.211 (.409)	.266 (.442)	.325 (.468)
Probation w/cond., fine	.038 (.191)	.074 (.261)	.116 (.321)	.159 (.366)
Probation, fine	.033 (.178)	.074 (.262)	.110 (.313)	.157 (.364)
Probation w/conditions	.064 (.245)	.149 (.356)	.210 (.407)	.274 (.446)
Simple probation	.053 (.224)	.119 (.323)	.164 (.370)	.227 (.419)
Fined only	.027 (.162)	.057 (.232)	.084 (.278)	.101 (.301)
Other sanction	.046 (.210)	.090 (.288)	.135 (.343)	.181 (.386)
TOTAL	.072 (.258)	.148 (.355)	.201 (.401)	.256 (.437)

c) Repeat of Presenting Offense

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.091 (.287)	.186 (.389)	.236 (.425)	.264 (.441)
Youth complex	.141 (.348)	.259 (.438)	.330 (.471)	.376 (.485)
Jail, probation, fine	.034 (.182)	.073 (.260)	.098 (.298)	.132 (.339)
Jail, probation	.091 (.288)	.186 (.389)	.239 (.427)	.290 (.454)
Jail only	.100 (.301)	.195 (.396)	.243 (.429)	.300 (.459)
Probation w/cond., fine	.055 (.228)	.102 (.303)	.130 (.337)	.177 (.382)
Probation, fine	.045 (.206)	.084 (.277)	.116 (.320)	.153 (.360)
Probation w/conditions	.095 (.293)	.172 (.377)	.235 (.424)	.297 (.457)
Simple probation	.067 (.251)	.137 (.344)	.186 (.389)	.251 (.434)
Fined only	.040 (.197)	.081 (.273)	.091 (.288)	.111 (.314)
Other sanction	.053 (.226)	.120 (.327)	.173 (.380)	.211 (.409)
TOTAL	.079 (.270)	.153 (.360)	.199 (.399)	.246 (.431)

d) Reimprisoned

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.115 (.319)	.267 (.442)	.353 (.478)	.386 (.487)
Youth complex	.172 (.377)	.402 (.491)	.500 (.500)	.565 (.496)
Jail, probation, fine	.007 (.083)	.036 (.187)	.055 (.228)	.089 (.285)
Jail, probation	.031 (.173)	.101 (.302)	.152 (.360)	.218 (.413)
Jail only	.062 (.241)	.135 (.342)	.201 (.401)	.254 (.435)
Probation w/cond., fine	.013 (.115)	.045 (.206)	.067 (.252)	.092 (.290)
Probation, fine	.012 (.108)	.032 (.176)	.053 (.224)	.087 (.282)
Probation w/conditions	.062 (.242)	.146 (.354)	.195 (.396)	.258 (.437)
Simple probation	.031 (.174)	.084 (.277)	.120 (.326)	.164 (.371)
Fined only	.007 (.082)	.024 (.152)	.030 (.171)	.030 (.171)
Other sanction	.031 (.173)	.068 (.252)	.098 (.298)	.135 (.343)
TOTAL	.057 (.231)	.137 (.344)	.187 (.390)	.230 (.421)

Table 8.2
Explained Variance Attributable to the Sentence for Probability of Rearrest

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.184)	(.254)	(.273)	(.271)
All Sanction Measures	6.28	4.13	4.43	5.36
	(.012)	(.011)	(.012)	(.014)
Sentence Variables	3.13	2.68	3.21	4.08
	(.006)	(.007)	(.009)	(.011)
Place Sentenced	1.84	1.14	1.15	1.04
	(.003)	(.003)	(.003)	(.003)
Time Sentenced	.09	.15	.15	.35
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.11	.68	1.06	1.43
	(.000)	(.002)	(.003)	(.004)
Interactions w/Ind. Vars.	3.15	1.45	1.23	1.27
	(.006)	(.004)	(.003)	(.003)

Table 8.3
Explained Variance Attributable to the Sentence for Probability of Rearrest for Persons Crime

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.092)	(.141)	(.165)	(.188)
All Sanction Measures	9.03	6.42	4.77	5.41
	(.008)	(.009)	(.008)	(.010)
Sentence Variables	4.00	3.65	3.18	4.40
	(.004)	(.005)	(.005)	(.008)
Place Sentenced	2.75	1.95	1.45	1.75
	(.003)	(.003)	(.002)	(.003)
Time Sentenced	.10	.09	.12	.38
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.25	.51	.80	.77
	(.000)	(.001)	(.001)	(.001)
Interactions w/Ind. Vars.	5.03	2.77	1.59	1.01
	(.005)	(.004)	(.003)	(.002)

Table 8.4
Explained Variance Attributable to the Sentence for Probability of Repeating Presenting Offense

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.072)	(.108)	(.135)	(.150)
All Sanction Measures	7.80	4.39	4.27	4.98
	(.006)	(.005)	(.006)	(.007)
Sentence Variables	2.71	1.74	2.04	3.32
	(.002)	(.002)	(.003)	(.005)
Place Sentenced	2.11	1.19	1.30	1.96
	(.002)	(.001)	(.002)	(.003)
Time Sentenced	.08	.08	.16	.23
	(.000)	(.000)	(.000)	(.000)
Sentence Pattern	.18	.05	.14	.41
	(.000)	(.000)	(.000)	(.001)
Interactions w/Ind. Vars.	5.09	2.65	2.22	1.66
	(.004)	(.003)	(.003)	(.002)

Table 8.5
Explained Variance Attributable to the Sentence for Probability of Reimprisonment

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.134)	(.265)	(.304)	(.311)
All Sanction Measures	11.39	9.83	8.37	7.18
	(.015)	(.026)	(.025)	(.022)
Sentence Variables	7.03	7.51	6.51	5.98
	(.009)	(.020)	(.020)	(.019)
Place Sentenced	3.79	4.96	4.23	3.49
	(.005)	(.013)	(.013)	(.011)
Time Sentenced	.06	.20	.35	.33
	(.000)	(.001)	(.001)	(.001)
Sentence Pattern	.20	.07	.05	.01
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.36	2.32	1.86	1.20
	(.006)	(.006)	(.006)	(.004)

Table 8.6

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	.0189	.0280*	.0334*	.0292*	.0029	.0100	.0193	.0143
Has job after sentence	-.0393***	-.0263*	-.0255*	-.0240	-.0176**	-.0104	-.0151	-.0222*
Offender is on welfare	-.0251*	.0038	.0040	.0079	.0037	.0101	.0118	.0111
Offender is Black	.0869***	.1539***	.1721***	.1843***	.0492***	.1026***	.1123***	.1352***
Offender is Hispanic	.0747***	.1373***	.1496***	.1557***	.0423**	.0991***	.1143***	.1447***
Offender is female	-.0451**	-.0586**	-.0638**	-.0821**	-.0418***	-.0803***	-.1055***	-.1156***
Lives in urban area	.0163	.0343**	.0397**	.0342**	.0171**	.0244**	.0316**	.0293**
Years at current address	-.0013*	-.0014*	-.0013*	-.0013*	-.0004	-.0005	-.0005	-.0002
History of drug problems	.0025	.0231	.0182	.0235	.0043	.0074	-.0082	-.0142
Treated for drugs/alch.	-.0150	-.0072	.0004	.0164	-.0113	-.0131	-.0114	-.0179
Has needle marks	.0010	.0284	.0748**	.0841**	-.0081	-.0222	-.0183	-.0080
Not a school drop out	-.0278**	-.0420***	-.0450***	-.0463***	.0003	-.0071	-.0229*	-.0211*
Doesn't live with family	-.0020	-.0130	-.0515***	-.0387**	-.0060	.0017	-.0061	-.0096
Commited PO with group	-.0197*	-.0332**	-.0335**	-.0342**	-.0057	.0001	-.0055	-.0076
Victim was a stranger	.0248*	.0374**	.0232	.0281	.0033	.0004	.0046	.0060
Presenting Offense								
PO property crime	.0180	.0331*	.0398*	.0400*	-.0021	-.0149	-.0164	-.0017
PO crime against person	-.0138	.0319	.0421*	.0586**	.0308**	.0616***	.0784***	.0903***
PO drug offense	-.0090	.0014	.0042	.0104	-.0070	-.0206	-.0274	-.0198
PO Wolfgang severity	-.0009	-.0016	-.0009	-.0011	-.0001	-.0003	-.0005	.0002
Has detainers at arrest	-.0176	-.0131	-.0134	-.0186	.0035	-.0112	-.0111	-.0056
Has pending charges	.0541***	.0849***	.0896***	.0762***	.0125	.0139	.0232*	.0243*
On probation at PO	.0535***	.0649***	.0599**	.0614**	.0025	.0150	.0234	.0313
Anamnestic Theory								
N prior adult arrests	.0112**	.0286***	.0350***	.0350***	.0067**	.0129***	.0154***	.0160***
N prior adult conviction	.0017	-.0022	.0007	.0011	-.0038*	-.0031	-.0041	-.0033
N prior adult chg. conv.	-.0054	-.0111**	-.0138**	-.0138**	-.0007	-.0018	-.0055	-.0068*
N charges past 5 years	.0052***	.0062**	.0063**	.0094***	.0013	.0015	.0010	.0019
N prior Part 1 charges	.0116**	.0088	.0097	.0096	.0028	-.0031	.0015	.0018
N prior property conv.	.0014	.0036	.0000	.0002	-.0032	-.0007	-.0011	.0013
N prior persons conv.	.0034	.0061	.0121*	.0076	.0092***	.0179***	.0237***	.0266***
N prior weapons conv.	.0333**	.0251	.0310	.0517**	.0197**	.0203*	.0366**	.0533***
Off street last 2 years	.0489***	.1027***	.1085***	.1067***	.0102	.0341***	.0367**	.0469***
Delinquent Career/Onset								
N arrests as juvenile	.0129***	.0168***	.0159**	.0214***	.0041*	.0077**	.0091**	.0113***
N charges as juvenile	.0006	.0056	.0061	.0137	.0046	-.0054	.0009	.0025
Age at first arrest	.0006	.0040*	.0032	.0025	-.0011	.0016	.0016	-.0002
Yrs since first incarc.	.0019***	.0022***	.0025***	.0020***	.0007**	.0010**	.0011**	.0012**
Yrs since first drug use	-.0006	-.0009*	-.0009	-.0006	.0000	-.0005	-.0004	-.0007*
Prior CJS-Offender Action								
N prior incarcerations	-.0014	-.0055	-.0082	-.0186**	.0012	.0024	.0030	.0016
N prior parole revokes	.0037	.0109	-.0002	.0039	-.0026	-.0128	-.0143	-.0197
Bad conduct last probat.	.0201	.0187	.0061	-.0081	.0109	.0122	.0173	.0231
Recent parole revoked	.0309	-.0132	-.0070	.0125	.0170	.0254	.0234	.0371
General Control Variables								
Offender age at sent.	-.0042**	-.0073***	-.0086***	-.0071***	-.0004	-.0040***	-.0059***	-.0059***
Off. born out of state	-.0443***	-.0849***	-.1019***	-.1214***	-.0143**	-.0267***	-.0396***	-.0573***
Coder prob. prognosis	-.0006**	-.0010***	-.0008***	-.0007**	-.0001	-.0004*	-.0003	-.0004*

* p<.05 ** p<.01 *** p<.001

Table 8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	a) Rearrested — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years	b) Rearrested for Persons Crime — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0436*	-.0745**	-.0953***	-.1095***	-.0193	-.0376*	-.0479**	-.0516*
Black x prior adult arrs	-.0086**	-.0109**	-.0191***	-.0160**	-.0055***	-.0084***	-.0117***	-.0124***
Black x n prior prop cnv	.0059*	.0091*	.0197***	.0218***	.0032*	.0036	.0063*	.0061*
Black x n charges as juv	.0143	.0198	.0266	.0142	-.0097	.0077	-.0016	.0010
Female x Part 1 charges	.0170	.0320*	.0500*	.0341	-.0004	-.0017	.0066	.0146
Off. age x drug problem	-.0047*	-.0033	-.0023	-.0015	-.0013	-.0034	-.0047*	-.0048*
Off. age x prior trtment	-.0036	-.0078**	-.0091**	-.0093**	-.0022	-.0025	-.0025	-.0041
Off. age x unemployed	-.0020	-.0021	-.0016	-.0018	-.0013	-.0013	-.0009	-.0013
Off. age x PO property	-.0054**	-.0056**	-.0038	-.0058**	-.0024*	-.0023	-.0025	-.0049*
Off. age x chg pst 5 yrs	.0004**	.0002	.0001	.0003	.0000	.0000	.0000	-.0001
PO viol x has detainers	-.0408	-.1091*	-.1202*	-.1428**	-.0265	-.0377	-.0556	-.0800*
PO prop x n adl.arrests	.0045	.0030	-.0023	.0057	.0025	.0031	.0056	.0108**
PO prop x prior prop con	-.0060*	-.0053	-.0022	-.0078	-.0006	-.0031	-.0058*	-.0090**
PO prop x n juv. arrests	-.0112**	-.0120*	-.0125*	-.0172**	-.0038	-.0083**	-.0076*	-.0045
PO prop x age at 1st arr	.0030	-.0023	-.0032	-.0014	-.0014	-.0077**	-.0074**	-.0024
PO prop x yrs. 1st incar	.0001	.0003	.0007	.0010	-.0003	-.0004	-.0002	-.0004
PO drugs x n adl. convs.	.0132**	.0143*	.0133*	.0127*	.0017	.0076	.0095*	.0112*
PO drugs x Part 1 chgs.	-.0239***	-.0209*	-.0146	-.0207	.0003	-.0088	-.0054	-.0101
PO drugs x last par. rev	.0660	.1397	.0744	.0866	-.0146	-.0108	.0036	.0256
Sentence								
Prison	-.1150***	-.0988**	-.0984**	-.1033**	-.0387**	-.0360	-.0440	-.0654*
Youth complex	-.0463*	-.0136	-.0008	-.0219	-.0161	-.0012	-.0015	-.0176
Jail, probation, fine	-.0719*	-.0865*	-.1428***	-.1393***	-.0337	-.0345	-.0535	-.0689*
Jail, probation	-.0039	.0407	.0023	.0001	-.0056	.0254	.0279	.0529
Jail only	-.0369	-.0497	-.0802*	-.0829*	-.0014	.0304	.0193	-.0051
Probation w/cond., fine	-.0226	-.0161	.0001	-.0063	.0040	-.0164	-.0067	-.0220
Probation, fine	-.0316	-.0220	-.0136	-.0036	-.0064	-.0107	-.0085	-.0110
Probation w/conditions	.0043	.0136	.0206	.0097	.0009	.0164	.0263	.0181
Fined only	-.0390	-.0589	-.0659	-.1090**	.0089	.0099	.0051	-.0334
Other sanction	-.0936*	-.0982	-.0921	-.0977	-.0031	-.0068	.0040	-.0220
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0001	-.0003	.0000	.0001	-.0002	.0002	.0000	-.0001
Months to jail	.0003	.0004	.0031	.0016	-.0001	-.0004	-.0004	-.0010
Months to prison	-.0002	-.0004*	-.0006**	-.0009***	.0000	-.0001	-.0002	-.0005**
First sanction of career	.0548	.0533	.0471	.0470	.0104	.0066	.0034	-.0015
Progressive sanction	.0019	.0113	.0086	-.0021	.0078	.0150	.0228	.0114
Sentence Interactions								
Prison x n adult arrests	.0068*	.0018	.0009	-.0004	.0012	.0006	.0012	-.0009
Prison x n arrsts as juv	.0037	-.0016	.0034	-.0011	.0009	.0009	.0009	-.0038
Yth. comp x n adlt convs	-.0101*	.0063	.0062	.0091	-.0029	.0003	-.0014	-.0019
Yth. comp x chgs in 5 yr	.0035	-.0030	-.0024	-.0062	.0015	.0012	.0013	-.0001
Yth. comp x prior n incs	.0213*	.0246	.0199	.0097	.0069	.0113	.0139	.0127
Jl & prob x inc lst 2 yr	.0740	.0526	.0573	.0124	.0687**	.0657*	.0631	.0179
Jail x yrs using drugs	.0002	-.0001	.0005	-.0003	-.0016*	-.0005	-.0008	-.0012
Prb w/cnd, fn x adlt arr	-.0144**	-.0158**	-.0071	-.0027	-.0020	-.0020	.0013	.0014
Prb & fn x Part 1 chgs.	-.0268*	-.0132	-.0211	-.0127	-.0107	-.0022	.0043	.0047
Prb w/cnd x adlt chg cnv	-.0089**	-.0106*	-.0119*	-.0118*	-.0032	-.0063*	-.0052	-.0056
Mths to jail x PO prop.	-.0001*	-.0002**	-.0002***	-.0002**	.0000	-.0001	-.0001*	-.0001*
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0274	-.0389	-.0548	-.0517	-.0055	.0007	.0052	.0323
Init sanc x n adult arrs	.0401***	.0514***	.0497***	.0493***	.0169**	.0293***	.0364***	.0429***
Constant	-.1568***	-.1490***	-.0572	.0378	-.0683***	-.1317***	-.1612***	-.1752***
Mean of Dep. Var.	.234	.399	.480	.549	.072	.148	.201	.256
N of cases	11,749	11,749	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.157	.219	.237	.238	.075	.122	.145	.166
Model Classifications:								
% Correct Predictions	79.05	73.55	72.98	73.61	92.79	85.59	81.29	77.44
% False Positives	4.46	10.02	12.07	13.48	.19	1.26	2.82	5.41
% False Negatives	16.49	16.44	14.95	12.91	7.03	13.15	15.89	17.15
RIOC	.487	.502	.486	.472	.360	.492	.498	.476

* p<.05 ** p<.01 *** p<.001

Table 8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	-.0064	.0093	.0127	.0193	.0015	.0202*	.0174	.0159
Has job after sentence	-.0141*	-.0179*	-.0109	-.0086	-.0189***	-.0313***	-.0329**	-.0318**
Offender is on welfare	-.0125	-.0165	-.0148	-.0064	-.0098	-.0225*	-.0304**	-.0247*
Offender is Black	.0158	.0386**	.0657***	.0842***	.0058	.0747***	.1214***	.1417***
Offender is Hispanic	.0178	.0433**	.0760***	.0947***	.0006	.0519**	.0747***	.1161***
Offender is female	-.0139	-.0436**	-.0538***	-.0701***	-.0041	-.0112	-.0250	-.0527**
Lives in urban area	.0049	.0171*	.0156	.0262*	-.0092	-.0131	-.0056	.0110
Years at current address	-.0005	-.0009*	-.0010*	-.0011*	.0000	-.0007	-.0007	-.0004
History of drug problems	-.0002	.0064	-.0064	-.0016	.0158*	.0196	.0406**	.0397**
Treated for drugs/alch.	-.0072	.0001	.0152	.0098	.0031	.0119	.0116	.0094
Has needle marks	.0283**	.0410**	.0351*	.0380*	.0055	.0185	.0193	.0356*
Not a school drop out	-.0173**	-.0148	-.0187*	-.0137	.0031	-.0106	-.0132	-.0145
Doesn't live with family	-.0060	-.0137	-.0311**	-.0354***	.0058	.0005	-.0008	-.0058
Commited PO with group	-.0153**	-.0227***	-.0311***	-.0329***	-.0095*	- 0036	-.0014	-.0100
Victim was a stranger	.0112	.0256**	.0275**	.0323**	.0102	193*	.0298**	.0324**
Presenting Offense								
PO property crime	.0365***	.0451***	.0628***	.0493**	.0095	-.0016	.0156	.0332
PO crime against person	.0351**	.0595***	.0674***	.0722***	-.0014	.0056	.0194	.0286
PO drug offense	.0793***	.1511***	.2072***	.2514***	-.0115	-.0358**	-.0144	-.0026
PO Wolfgang severity	-.0021***	-.0028***	-.0032***	-.0039***	.0001	.0005	.0008	.0002
Has detainers at arrest	-.0024	-.0018	-.0052	-.0068	-.0057	.0122	.0123	.0197
Has pending charges	.0172*	.0283**	.0446***	.0550***	.0227***	.0592***	.0660***	.0665***
On probation at PO	.0203*	.0276*	.0309*	.0402*	.0174*	.0462**	.0597***	.0497**
Anamnestic Theory								
N prior adult arrests	.0072**	.0143***	.0185***	.0188***	.0017	.0046	.0075*	.0089*
N prior adult conviction	.0006	.0000	-.0003	.0043	-.0016	.0013	.0008	.0019
N prior adult chg. conv.	-.0020	-.0048	-.0053	-.0050	.0016	.0010	.0021	.0005
N charges past 5 years	.0013	.0015	.0020	.0019	.0016*	.0022	.0018	.0030
N prior Part 1 charges	.0062**	.0056	.0059	.0056	.0003	.0023	.0033	.0028
N prior property conv.	-.0061**	-.0066*	-.0089**	-.0092**	-.0001	.0020	.0033	.0042
N prior persons conv.	.0008	-.0002	.0001	.0012	.0032	.0047	.0087*	.0124**
N prior weapons conv.	.0035	.0154	.0022	.0063	-.0009	.0093	.0036	.0081
Off street last 2 years	.0145*	.0371***	.0467***	.0594***	.0174**	.0516***	.0826***	.0838***
Delinquent Career/Onset								
N arrests as juvenile	.0006	.0029	.0022	.0051	.0029	.0065*	.0062*	.0072*
N charges as juvenile	.0016	-.0038	-.0060	-.0007	.0048	.0128	.0232	.0159
Age at first arrest	-.0006	.0025	.0021	.0021	.0017	.0034	.0057**	.0046*
Yrs since first incarc.	.0004	.0008*	.0010*	.0008	.0027***	.0038***	.0044***	.0050***
Yrs since first drug use	-.0002	-.0002	-.0002	.0000	.0000	-.0001	-.0003	-.0007
Prior CJS-Offender Action								
N prior incarcerations	.0014	-.0019	-.0002	-.0062	.0017	.0004	-.0010	-.0015
N prior parole revokes	-.0041	-.0109	-.0195*	-.0210	.0057	.0002	.0042	.0013
Bad conduct last probat.	-.0004	.0049	.0020	-.0009	.0009	.0067	.0018	.0023
Recent parole revoked	.0250	.0233	.0414	.0441	.0154	.0459**	.0161	.0184
General Control Variables								
Offender age at sent.	-.0004	-.0020	-.0024*	-.0027*	-.0017	-.0036**	-.0044**	-.0042**
Off. born out of state	-.0161**	-.0228**	-.0417***	-.0443***	.0020	-.0113	-.0194*	-.0346***
Coder prob. prognosis	-.0003*	-.0006***	-.0006***	-.0009***	-.0003*	-.0007***	-.0008***	-.0010***

* p<.05 ** p<.01 *** p<.001

Table 8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures - Expressed as Change at the Mean

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years	d) Reimprisoned — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0208*	-.0356*	-.0393*	-.0427*	-.0148	-.0459**	-.0584***	-.0388
Black x prior adult arrs	-.0038*	-.0068**	-.0082**	-.0094**	-.0050***	-.0078***	-.0107***	-.0140***
Black x n prior prop cnv	.0031*	.0050*	.0052*	.0061*	.0038***	.0043*	.0041	.0045
Black x n charges as juv	.0019	.0071	.0155	.0075	-.0038	.0093	.0180	.0333
Female x Part 1 charges	.0049	.0045	.0066	.0040	.0014	.0093	.0133	.0197
Off. age x drug problem	-.0017	-.0010	-.0035*	-.0026	.0005	-.0014	-.0006	.0012
Off. age x prior trtment	-.0014	-.0028	-.0022	-.0044*	-.0018	-.0043*	-.0048*	-.0075**
Off. age x unemployed	-.0018*	-.0003	-.0009	-.0014	.0000	-.0008	-.0026	-.0044**
Off. age x PO property	-.0027*	-.0039*	-.0056**	-.0069***	-.0016	-.0066***	-.0072***	-.0084***
Off. age x chg pst 5 yrs	.0001	.0000	.0000	.0001	.0002**	.0003**	.0003*	.0004*
PO viol x has detainers	-.0267	-.0570*	-.0752*	-.0951**	.0018	-.0131	-.0193	-.0324
PO prop x n adl.arrests	-.0025	-.0035	-.0027	-.0022	.0002	.0027	.0042	.0063
PO prop x prior prop con	.0055**	.0080**	.0108***	.0114***	-.0008	-.0015	-.0025	-.0039
PO prop x n juv. arrests	.0008	.0018	.0017	.0009	-.0006	-.0013	.0001	.0001
PO prop x age at 1st arr	.0039*	.0012	.0026	.0040	.0006	.0018	.0010	.0022
PO prop x yrs. 1st incar	.0007*	.0007	.0008	.0014**	-.0001	.0004	.0004	.0005
PO drugs x n adl. convs.	.0030	.0071*	.0081*	.0067	.0038	.0108**	.0095*	.0070
PO drugs x Part 1 chgs.	-.0109*	-.0098	.0002	-.0006	-.0073*	-.0084	-.0114	-.0087
PO drugs x last par. rev	.0249	.0337	.0016	.0495	.0170	.0169	.0765	.0627
Sentence								
Prison	-.0420**	-.0469*	-.0487*	-.0899***	-.0138	.0029	.0281	-.0143
Youth complex	-.0131	-.0216	-.0244	-.0669**	.0007	.0587*	.0772**	.0497
Jail, probation, fine	-.0348	-.0614*	-.0910***	-.1121***	-.0502***	-.0962***	-.1277***	-.1373***
Jail, probation	-.0082	.0096	-.0097	-.0276	-.0481***	-.1020***	-.1247***	-.1196***
Jail only	-.0114	-.0101	-.0296	-.0537*	-.0248*	-.0618***	-.0715**	-.0954***
Probation w/cond., fine	.0023	-.0066	-.0289	-.0338	-.0206	-.0269	-.0361	-.0551*
Probation, fine	-.0118	-.0306*	-.0440**	-.0693***	-.0125	-.0334	-.0366	-.0256
Probation w/conditions	.0055	-.0063	-.0018	-.0177	.0122	.0221	.0230	.0398
Fined only	.0271	.0261	-.0090	-.0496	-.0114	.0167	-.0226	-.0992
Other sanction	-.0202	-.0154	-.0031	-.0476	-.0232	-.0465	-.0509	-.0568
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	.0001	.0001	.0000	-.0003	.0004	.0013**	.0013**	.0011*
Months to jail	.0000	.0003	.0014	.0013	.0000	.0009	.0014	.0007
Months to prison	.0001	-.0001	-.0004*	-.0005**	.0000	-.0001	-.0004**	-.0005***
First sanction of career	.0269	.0605*	.0495	.0406	.0472*	.0549	.0307	.0328
Progressive sanction	-.0077	-.0037	-.0044	-.0225	.0092	.0164	.0217	.0083
Sentence Interactions								
Prison x n adult arrests	.0013	.0020	.0007	.0008	.0011	.0014	.0020	.0020
Prison x n arrsts as juv	.0064*	.0051	.0041	.0050	-.0001	-.0035	-.0029	-.0046
Yth. comp x n adlt convs	-.0085**	-.0082*	-.0107*	-.0111*	-.0020	-.0087*	-.0124**	-.0130*
Yth. comp x chgs in 5 yr	.0029	.0033	.0042	.0046	.0005	.0014	.0029	.0038
Yth. comp x prior n incs	.0033	.0046	.0037	.0035	.0059	.0152**	.0257***	.0247**
Jl & prob x inc lst 2 yr	.0075	-.0099	-.0017	.0025	.0780	.1134*	.1155*	.0582
Jail x yrs using drugs	.0008	.0004	.0001	-.0007	.0000	.0005	.0011	.0005
Prb w/cnd, fn x adlt arr	-.0055*	-.0045	-.0017	.0010	-.0030	-.0047	-.0056	-.0024
Prb & fn x Part 1 chgs.	-.0117	-.0108	-.0151	-.0167	-.0023	-.0149	-.0067	-.0157
Prb w/cnd x adlt chg cnv	-.0020	-.0028	-.0052	-.0038	-.0033*	-.0073**	-.0075*	-.0105**
Mths to jail x PO prop.	.0000	-.0001*	-.0001**	-.0001**	.0000	.0000	.0000	-.0001
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0043	-.0134	-.0154	.0017	-.0150	-.0193	-.0396	-.0276
Init sanc x n adult arrs	.0173**	.0334***	.0340***	.0339***	.0163**	.0324***	.0284**	.0256*
Constant	-.0740***	-.1350***	-.1647***	-.1849***	-.0553***	-.1272***	-.1671***	-.1930***
Mean of Dep. Var.	.079	.153	.199	.246	.057	.137	.186	.230
N of cases	11,714	11,746	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.060	.093	.117	.133	.100	.198	.231	.242
Model Classifications:								
% Correct Predictions	92.12	84.66	80.70	76.99	94.40	88.07	85.33	82.76
% False Positives	.09	1.11	2.41	4.09	.20	2.76	4.18	5.34
% False Negatives	7.79	14.23	16.89	18.92	5.40	9.17	10.49	11.90
RIOC	.522	.389	.446	.446	.529	.563	.584	.579

* p<.05 ** p<.01 *** p<.001

Table 8.7

Mean Levels of Recidivism by Type of Sentence - Criminal Career Measures Only
(Standard Deviations in Parentheses)

a) Total Post-Sentence Charges

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.822 (1.95)	2.084 (3.48)	3.098 (4.92)	4.047 (6.19)
Youth complex	1.396 (2.81)	3.247 (4.46)	4.693 (5.62)	6.647 (7.51)
Jail, probation, fine	.285 (1.00)	.763 (1.85)	1.121 (2.51)	1.752 (3.64)
Jail, probation	.794 (1.78)	1.921 (3.22)	2.795 (4.27)	4.111 (5.77)
Jail only	.924 (2.22)	2.191 (3.68)	3.241 (5.10)	4.757 (7.31)
Probation w/cond., fine	.417 (1.56)	.881 (2.18)	1.337 (2.91)	2.156 (4.45)
Probation, fine	.308 (1.12)	.753 (1.95)	1.216 (2.87)	1.959 (4.29)
Probation w/conditions	.627 (1.56)	1.601 (3.00)	2.671 (4.44)	4.180 (6.45)
Simple probation	.520 (1.44)	1.343 (2.84)	2.210 (4.09)	3.340 (6.07)
Fined only	.192 (.81)	.497 (1.48)	.705 (1.84)	1.030 (2.68)
Other sanction	.382 (1.27)	.993 (2.91)	1.639 (3.94)	3.301 (8.91)
TOTAL	.656 (1.80)	1.594 (3.13)	2.423 (4.334)	3.595 (6.11)

b) Total Post-Sentence Convictions

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.226 (.56)	.577 (1.08)	.824 (1.42)	.994 (1.67)
Youth complex	.403 (.73)	.982 (1.35)	1.465 (1.84)	1.930 (2.38)
Jail, probation, fine	.098 (.42)	.255 (.74)	.367 (.92)	.531 (1.25)
Jail, probation	.258 (.61)	.594 (1.10)	.817 (1.37)	1.127 (1.78)
Jail only	.279 (.65)	.688 (1.24)	1.013 (1.73)	1.368 (2.40)
Probation w/cond., fine	.119 (.43)	.274 (.72)	.389 (.90)	.575 (1.25)
Probation, fine	.084 (.34)	.222 (.65)	.343 (.88)	.507 (1.19)
Probation w/conditions	.201 (.53)	.517 (.98)	.849 (1.46)	1.231 (2.13)
Simple probation	.171 (.49)	.426 (.94)	.648 (1.31)	.943 (1.78)
Fined only	.044 (.24)	.114 (.43)	.171 (.55)	.262 (.80)
Other sanction	.115 (.47)	.301 (.98)	.519 (1.39)	.767 (2.17)
TOTAL	.196 (.54)	.485 (1.01)	.725 (1.38)	.996 (1.86)

c) Total Post-Sentence Persons Charges

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.155 (.58)	.345 (.88)	.511 (1.12)	.644 (1.30)
Youth complex	.270 (.81)	.536 (1.10)	.743 (1.29)	1.030 (1.60)
Jail, probation, fine	.025 (.17)	.103 (.43)	.182 (.86)	.262 (.99)
Jail, probation	.176 (.63)	.373 (.93)	.524 (1.12)	.748 (1.41)
Jail only	.172 (.58)	.406 (1.09)	.558 (1.32)	.792 (1.72)
Probation w/cond., fine	.060 (.37)	.116 (.49)	.199 (.83)	.325 (1.18)
Probation, fine	.049 (.30)	.117 (.49)	.190 (.69)	.318 (.98)
Probation w/conditions	.100 (.47)	.249 (.76)	.383 (.95)	.577 (1.29)
Simple probation	.084 (.43)	.201 (.71)	.295 (.87)	.475 (1.21)
Fined only	.027 (.16)	.084 (.37)	.114 (.41)	.171 (.62)
Other sanction	.061 (.30)	.165 (.62)	.256 (.75)	.474 (1.46)
TOTAL	.116 (.51)	.261 (.80)	.384 (1.01)	.558 (1.32)

d) Adjusted Arrest Rate

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.726 (3.64)	.648 (1.79)	.602 (1.34)	.545 (1.19)
Youth complex	.949 (1.77)	.867 (1.22)	.822 (1.12)	.703 (.87)
Jail, probation, fine	.189 (.64)	.166 (.39)	.147 (.33)	.135 (.30)
Jail, probation	.512 (.97)	.458 (.77)	.401 (.66)	.349 (.55)
Jail only	.595 (1.25)	.561 (1.31)	.498 (.97)	.421 (.70)
Probation w/cond., fine	.246 (.74)	.207 (.57)	.178 (.38)	.157 (.33)
Probation, fine	.245 (2.25)	.170 (.46)	.161 (.40)	.143 (.34)
Probation w/conditions	.693 (9.42)	.425 (.88)	.412 (.75)	.369 (.68)
Simple probation	.343 (.89)	.322 (.74)	.316 (.76)	.283 (.69)
Fined only	.189 (1.55)	.098 (.29)	.089 (.25)	.069 (.19)
Other sanction	.260 (.80)	.247 (.70)	.242 (.63)	.252 (.73)
TOTAL	.498 (3.85)	.416 (1.04)	.387 (.85)	.341 (.72)

Table 8.7 (continued)

Mean Levels of Recidivism by Type of Sentence - Criminal Career Measures Only
(Standard Deviations in Parentheses)

e) Adjusted Charge Rate

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	1.217 (4.53)	1.200 (3.48)	1.140 (2.66)	1.050 (2.57)
Youth complex	1.945 (4.53)	1.742 (3.26)	1.577 (2.65)	1.364 (2.09)
Jail, probation, fine	.309 (1.32)	.279 (.70)	.250 (.58)	.234 (.53)
Jail, probation	.874 (2.05)	.785 (1.49)	.698 (1.26)	.618 (1.07)
Jail only	1.094 (2.80)	1.032 (2.73)	.898 (1.95)	.770 (1.43)
Probation w/cond., fine	.445 (1.66)	.354 (1.11)	.304 (.74)	.276 (.65)
Probation, fine	.499 (6.64)	.288 (.853)	.279 (.76)	.254 (.65)
Probation w/conditions	.958 (9.52)	.706 (1.57)	.721 (1.57)	.655 (1.40)
Simple probation	.593 (1.86)	.576 (1.57)	.569 (1.83)	.517 (1.72)
Fined only	.276 (1.71)	.189 (.658)	.172 (.59)	.126 (.36)
Other sanction	.415 (1.38)	.446 (1.36)	.442 (1.21)	.602 (2.22)
TOTAL	.873 (5.04)	.757 (2.17)	.706 (1.79)	.632 (1.59)

f) Adjusted Persons Charged Rate

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	.231 (1.57)	.240 (1.53)	.264 (1.64)	.231 (1.54)
Youth complex	.374 (1.27)	.327 (.95)	.287 (.75)	.253 (.65)
Jail, probation, fine	.025 (.17)	.039 (.17)	.042 (.19)	.038 (.18)
Jail, probation	.204 (.94)	.164 (.48)	.146 (.41)	.124 (.32)
Jail only	.211 (.91)	.185 (.55)	.162 (.47)	.135 (.37)
Probation w/cond., fine	.066 (.42)	.047 (.24)	.046 (.23)	.044 (.21)
Probation, fine	.050 (.30)	.043 (.19)	.044 (.17)	.041 (.14)
Probation w/conditions	.108 (.51)	.109 (.37)	.109 (.33)	.094 (.26)
Simple probation	.093 (.50)	.090 (.39)	.082 (.32)	.075 (.26)
Fined only	.027 (.16)	.031 (.14)	.027 (.11)	.021 (.08)
Other sanction	.064 (.31)	.070 (.27)	.067 (.21)	.137 (.98)
TOTAL	.146 (.86)	.134 (.70)	.128 (.69)	.114 (.64)

g) Summed Seriousness of all Charges

Sentence Type	Post-Sentence Observation Window 1 Year	3 Years	5 Years	9 Years
Prison	5.008 (12.85)	12.570 (21.28)	18.671 (27.68)	24.270 (34.40)
Youth complex	7.950 (16.32)	18.563 (25.71)	26.760 (31.94)	38.324 (42.13)
Jail, probation, fine	1.431 (5.65)	4.149 (11.01)	6.203 (15.22)	10.040 (22.76)
Jail, probation	4.851 (11.83)	11.541 (20.24)	16.842 (26.43)	24.777 (34.98)
Jail only	5.610 (14.19)	13.271 (23.69)	19.376 (30.64)	28.733 (43.91)
Probation w/cond., fine	2.286 (9.12)	4.766 (12.31)	7.472 (17.34)	12.216 (26.20)
Probation, fine	1.687 (6.27)	4.323 (11.83)	7.178 (17.32)	11.834 (25.42)
Probation w/conditions	3.520 (9.66)	9.150 (17.99)	15.279 (25.44)	23.914 (36.55)
Simple probation	3.050 (9.52)	7.467 (16.26)	11.863 (22.63)	19.150 (33.50)
Fined only	.926 (3.87)	2.970 (9.49)	4.272 (11.93)	6.295 (17.70)
Other sanction	2.307 (8.04)	5.890 (19.10)	9.840 (25.00)	17.775 (40.02)
TOTAL	3.811 (11.12)	9.252 (18.86)	14.098 (25.18)	21.065 (35.01)

Table 8.8
Explained Variance Attributable to the Sentence for Log of Total Post-Sentence Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.200)	(.304)	(.344)	(.359)
All Sanction Measures	6.17	3.71	3.58	4.29
	(.012)	(.011)	(.012)	(.015)
Sentence Variables	2.36	1.90	2.25	3.27
	(.005)	(.006)	(.008)	(.012)
Place Sentenced	1.64	.96	1.01	1.18
	(.003)	(.003)	(.003)	(.004)
Time Sentenced	.05	.11	.14	.26
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.01	.21	.31	.44
	(.000)	(.001)	(.001)	(.002)
Interactions w/Ind. Vars.	3.81	1.82	1.33	1.02
	(.008)	(.006)	(.005)	(.004)

Table 8.9
Explained Variance Attributable to the Sentence for Log of Total Post-Sentence Convictions

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.163)	(.253)	(.294)	(.310)
All Sanction Measures	9.06	5.62	6.21	6.96
	(.015)	(.014)	(.018)	(.022)
Sentence Variables	3.18	2.58	3.28	4.46
	(.005)	(.007)	(.010)	(.014)
Place Sentenced	1.56	1.12	1.47	1.83
	(.003)	(.003)	(.004)	(.006)
Time Sentenced	.20	.29	.27	.35
	(.000)	(.001)	(.001)	(.001)
Sentence Pattern	.05	.02	.06	.14
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	5.88	3.04	2.94	2.50
	(.010)	(.008)	(.009)	(.008)

Table 8.10
Explained Variance Attributable to the Sentence for Log of Total Post-Sentence Persons Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.090)	(.148)	(.174)	(.198)
All Sanction Measures	8.68	6.05	4.74	5.72
	(.008)	(.009)	(.008)	(.011)
Sentence Variables	4.34	3.49	3.10	4.63
	(.004)	(.005)	(.005)	(.009)
Place Sentenced	2.95	1.81	1.35	1.78
	(.003)	(.003)	(.002)	(.004)
Time Sentenced	.08	.12	.32	.54
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.06	.20	.48	.56
	(.000)	(.000)	(.001)	(.001)
Interactions w/Ind. Vars.	4.34	2.55	1.64	1.09
	(.004)	(.004)	(.003)	(.002)

Table 8.11
Explained Variance Attributable to the Sentence for Log of Adjusted Post-Sentence Arrest Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.209)	(.330)	(.366)	(.381)
All Sanction Measures	6.43	3.57	3.48	2.74
	(.013)	(.012)	(.013)	(.010)
Sentence Variables	2.25	1.52	1.69	1.43
	(.005)	(.005)	(.006)	(.005)
Place Sentenced	1.36	.80	.94	.67
	(.003)	(.003)	(.003)	(.003)
Time Sentenced	.09	.08	.07	.10
	(.000)	(.000)	(.000)	(.000)
Sentence Pattern	.00	.02	.02	.04
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.18	2.05	1.79	1.31
	(.009)	(.007)	(.007)	(.005)

Table 8.12
Explained Variance Attributable to the Sentence for Log of Adjusted Post-Sentence Charge Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.208)	(.322)	(.357)	(.371)
All Sanction Measures	6.28	3.54	3.21	2.69
	(.013)	(.011)	(.011)	(.010)
Sentence Variables	2.21	1.45	1.52	1.37
	(.005)	(.005)	(.005)	(.005)
Place Sentenced	1.47	.80	.85	.67
	(.003)	(.003)	(.003)	(.002)
Time Sentenced	.06	.08	.09	.12
	(.000)	(.000)	(.000)	(.000)
Sentence Pattern	.00	.03	.03	.03
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.07	2.09	1.69	1.32
	(.008)	(.007)	(.006)	(.005)

Table 8.13
Explained Variance Attributable to the Sentence for Log of Adjusted Post-Sentence Persons Charge Rate

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.097)	(.156)	(.180)	(.200)
All Sanction Measures	8.42	5.25	3.55	3.09
	(.008)	(.008)	(.006)	(.006)
Sentence Variables	3.96	2.25	1.36	1.43
	(.004)	(.004)	(.002)	(.003)
Place Sentenced	2.54	1.19	.61	.58
	(.002)	(.002)	(.001)	(.001)
Time Sentenced	.12	.12	.29	.40
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.06	.02	.06	.08
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.47	3.00	2.19	1.66
	(.004)	(.005)	(.004)	(.003)

Table 8.14
Explained Variance Attributable to the Sentence for
Summed Seriousness of All Post-Sentence Charges

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.125)	(.212)	(.246)	(.256)
All Sanction Measures	7.63	4.68	4.18	5.48
	(.010)	(.010)	(.010)	(.014)
Sentence Variables	2.74	1.96	2.24	3.98
	(.003)	(.004)	(.006)	(.010)
Place Sentenced	1.95	.95	.94	1.60
	(.002)	(.002)	(.002)	(.004)
Time Sentenced	.11	.17	.35	.48
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.01	.05	.14	.20
	(.000)	(.000)	(.000)	(.001)
Interactions w/Ind. Vars.	4.90	2.72	1.93	1.50
	(.006)	(.006)	(.005)	(.004)

Table 8.15

Regression Coefficients for Log of Total Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0273*	(.0234)	.0398**	(.0241)	.0473**	(.0249)	.0455*	(.0213)
Has job after sentence	-.0455***	(-.0407)	-.0460**	(-.0291)	-.0450**	(-.0248)	-.0495**	(-.0241)
Offender is on welfare	-.0345**	(-.0257)	-.0303	(-.0160)	-.0301	(-.0138)	-.0171	(-.0069)
Offender is Black	.0777***	(.0466)	.1763***	(.0795)	.2483***	(.0968)	.3213***	(.1204)
Offender is Hispanic	.0519**	(.0242)	.1416***	(.0468)	.1901***	(.0545)	.2679***	(.0680)
Offender is female	-.0410*	(-.0233)	-.0777**	(-.0313)	-.0930***	(-.0325)	-.1256***	(-.0389)
Lives in urban area	.0138	(.0123)	.0411**	(.0258)	.0595***	(.0325)	.0681***	(.0330)
Years at current address	-.0010	(-.0179)	-.0013	(-.0165)	-.0013	(-.0148)	-.0011	(-.0109)
History of drug problems	.0043	(.0033)	.0332	(.0180)	.0509*	(.0240)	.0575*	(.0240)
Treated for drugs/alch.	-.0191	(-.0130)	.0022	(.0010)	.0309	(.0129)	.0551*	(.0204)
Has needle marks	.0148	(.0068)	.0590*	(.0191)	.1074***	(.0302)	.1317***	(.0328)
Not a school drop out	-.0219*	(-.0190)	-.0351**	(-.0216)	-.0479**	(-.0256)	-.0516**	(-.0244)
Doesn't live with family	-.0026	(-.0019)	-.0094	(-.0049)	-.0596***	(-.0269)	-.0565**	(-.0226)
Commited PO with group	-.0200*	(-.0179)	-.0226	(-.0143)	-.0182	(-.0100)	-.0227	(-.0111)
Victim was a stranger	.0140	(.0111)	.0466**	(.0261)	.0505**	(.0246)	.0694***	(.0299)
Presenting Offense								
PO property crime	.0276*	(.0244)	.0576**	(.0360)	.0730***	(.0396)	.0853***	(.0410)
PO crime against person	-.0107	(-.0122)	.0216	(.0036)	.0271	(.0050)	.0341	(.0059)
PO drug offense	-.0007	(.0017)	-.0058	(.0047)	-.0045	(.0041)	.0111	(.0117)
PO Wolfgang severity	-.0008	(-.0113)	-.0017	(-.0166)	-.0015	(-.0127)	-.0025*	(-.0186)
Has detainers at arrest	-.0041	(-.0104)	.0180	(-.0082)	.0223	(-.0070)	.0066	(-.0123)
Has pending charges	.0700***	(.0502)	.1144***	(.0581)	.1316***	(.0580)	.1418***	(.0554)
On probation at PO	.0574***	(.0242)	.0811***	(.0200)	.0992***	(.0191)	.1094***	(.0171)
Anamnestic Theory								
N prior adult arrests	.0163***	(.1512)	.0387***	(.2638)	.0468***	(.2649)	.0500***	(.2810)
N prior adult conviction	-.0062	(-.0089)	-.0097	(-.0116)	-.0034	(.0127)	-.0013	(.0202)
N prior adult chg. conv.	-.0059	(-.0720)	-.0146**	(-.0903)	-.0194***	(-.1001)	-.0193***	(-.0881)
N charges past 5 years	.0108***	(.1068)	.0125***	(.0845)	.0139***	(.0786)	.0175***	(.0849)
N prior Part 1 charges	.0245***	(.0424)	.0145*	(-.0013)	.0166*	(.0172)	.0142	(.0146)
N prior property conv.	.0014	(.0188)	.0095	(.0522)	.0084	(.0493)	.0121	(.0472)
N prior persons conv.	-.0018	(-.0048)	-.0003	(-.0006)	.0033	(.0055)	.0046	(.0067)
N prior weapons conv.	.0344*	(.0214)	.0488*	(.0215)	.0366	(.0140)	.0693**	(.0235)
Off street last 2 years	.0713***	(.0644)	.1530***	(.0935)	.1781***	(.0941)	.1985***	(.0918)
Delinquent Career/Onset								
N arrests as juvenile	.0190***	(.0681)	.0305***	(.0726)	.0314***	(.0628)	.0385***	(.0664)
N charges as juvenile	-.0080	(.0036)	-.0082	(.0102)	.0063	(.0169)	.0055	(.0149)
Age at first arrest	.0027*	(.0587)	.0079***	(.0775)	.0088***	(.0756)	.0087***	(.0676)
Yrs since first incarc.	.0021***	(.1093)	.0036***	(.1218)	.0052***	(.1467)	.0054***	(.1380)
Yrs since first drug use	-.0008*	(-.0141)	-.0012*	(-.0192)	-.0015**	(-.0197)	-.0013	(-.0151)
Prior CJS-Offender Action								
N prior incarcerations	-.0004	(.0132)	-.0012	(.0030)	-.0070	(-.0112)	-.0190*	(-.0368)
N prior parole revokes	.0150	(.0127)	.0082	(.0049)	-.0027	(-.0014)	-.0053	(-.0025)
Bad conduct last probat.	.0256	(.0179)	.0493**	(.0244)	.0423*	(.0182)	.0268	(.0102)
Recent parole revoked	.0615*	(.0308)	.0136	(.0215)	.0113	(.0168)	.0434	(.0256)
General Control Variables								
Offender age at sent.	-.0026*	(-.1429)	-.0083***	(-.1955)	-.0116***	(-.2098)	-.0137***	(-.2137)
Off. born out of state	-.0435***	(-.0389)	-.0959***	(-.0607)	-.1299***	(-.0714)	-.1707***	(-.0831)
Coder prob. prognosis	-.0005*	(-.0237)	-.0012***	(-.0419)	-.0013***	(-.0389)	-.0015***	(-.0416)

*p<.05 **p<.01 ***p<.001

Table 8.15 (continued)

Regression Coefficients for Log of Total Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0539*	(-.0192)	-.0952**	(-.0239)	-.1276***	(-.0279)	-.1499***	(-.0290)
Black x prior adult arrs	-.0152***	(-.0581)	-.0198***	(-.0533)	-.0248***	(-.0582)	-.0264***	(-.0548)
Black x n prior prop cnv	.0129***	(.0512)	.0147***	(.0414)	.0182***	(.0443)	.0185***	(.0400)
Black x n charges as juv	.0271	(.0126)	.0537*	(.0177)	.0526	(.0151)	.0537	(.0137)
Female x Part 1 charges	.0187	(.0244)	.0229	(.0211)	.0389*	(.0311)	.0482**	(.0342)
Off. age x drug problem	-.0051*	(-.0365)	-.0067*	(-.0334)	-.0076*	(-.0331)	-.0060	(-.0231)
Off. age x prior trtment	-.0071**	(-.0447)	-.0120***	(-.0535)	-.0120***	(-.0464)	-.0154***	(-.0529)
Off. age x unemployed	-.0031**	(-.0247)	-.0044**	(-.0248)	-.0045**	(-.0222)	-.0043*	(-.0186)
Off. age x PO property	-.0059***	(-.0483)	-.0074***	(-.0430)	-.0084***	(-.0420)	-.0106***	(-.0470)
Off. age x chg pst 5 yrs	-.0001	(-.0112)	-.0004	(-.0245)	-.0004	(-.0213)	-.0003	(-.0133)
PO viol x has detainers	-.0989*	(-.0174)	-.2239***	(-.0279)	-.2422***	(-.0262)	-.2927***	(-.0280)
PO prop x n adl.arrests	.0070	(.0266)	.0117*	(.0314)	.0121*	(.0280)	.0221***	(.0454)
PO prop x prior prop con	-.0116**	(-.0457)	-.0164***	(-.0457)	-.0155**	(-.0374)	-.0230***	(-.0493)
PO prop x n juv. arrests	-.0077	(-.0170)	-.0147*	(-.0231)	-.0172**	(-.0235)	-.0223**	(-.0270)
PO prop x age at 1st arr	.0038	(.0257)	.0004	(.0019)	.0004	(.0018)	.0009	(.0034)
PO prop x yrs. 1st incar	.0015**	(.0290)	.0015*	(.0210)	.0015	(.0190)	.0019*	(.0208)
PO drugs x n adl. convs.	.0203***	(.0535)	.0299***	(.0556)	.0284***	(.0460)	.0310***	(.0444)
PO drugs x Part 1 chgs.	-.0407***	(-.0709)	-.0500***	(-.0616)	-.0355**	(-.0380)	-.0367*	(-.0348)
PO drugs x last par. rev	.0495	(.0089)	.2493**	(.0316)	.2275*	(.0250)	.2902**	(.0283)
Sentence								
Prison	-.1741***	(-.1047)	-.2005***	(-.0854)	-.2428***	(-.0898)	-.3154***	(-.1034)
Youth complex	-.0453	(-.0241)	-.0398	(-.0150)	-.0546	(-.0179)	-.0874	(-.0253)
Jail, probation, fine	-.1131**	(-.0385)	-.1584***	(-.0382)	-.2571***	(-.0538)	-.2769***	(-.0513)
Jail, probation	-.0622	(-.0097)	-.0628	(-.0053)	-.1364**	(-.0226)	-.1305*	(-.0203)
Jail only	-.0536	(-.0303)	-.0808*	(-.0323)	-.1433***	(-.0497)	-.1661***	(-.0511)
Probation w/cond., fine	-.0339	(-.0199)	-.0647*	(-.0269)	-.0754*	(-.0272)	-.0810*	(-.0259)
Probation, fine	-.0474*	(-.0305)	-.0602*	(-.0274)	-.0612*	(-.0242)	-.0598	(-.0210)
Probation w/conditions	-.0194	(-.0117)	-.0229	(-.0098)	-.0044	(-.0016)	-.0063	(-.0021)
Fined only	-.0130	(-.0037)	-.0259	(-.0052)	-.0466	(-.0081)	-.1049	(-.0161)
Other sanction	-.0739	(-.0140)	-.1180	(-.0159)	-.1066	(-.0124)	-.1137	(-.0118)
Dollars fined	.0000	(.0043)	.0000	(.0037)	.0000	(.0087)	.0000	(.0093)
Months on probation	-.0001	(-.0035)	-.0003	(-.0074)	.0000	(.0002)	.0002	(.0027)
Months to jail	.0012	(-.0036)	.0009	(-.0123)	.0026	(-.0028)	.0016	(-.0136)
Months to prison	.0000	(-.0030)	-.0003	(-.0204)	-.0005*	(-.0266)	-.0008***	(-.0402)
First sanction of career	.0740*	(.0438)	.1030*	(.0341)	.1016*	(.0181)	.1004	(.0174)
Progressive sanction	.0002	(.0002)	.0044	(.0023)	.0112	(.0051)	.0012	(.0005)
Sentence Interactions								
Prison x n adult arrests	.0092**	(.0236)	.0084	(.0153)	.0083	(.0131)	.0041	(.0057)
Prison x n arrsts as juv	-.0063	(-.0095)	-.0141*	(-.0152)	-.0138	(-.0129)	-.0190*	(-.0157)
Yth. comp x n adlt convs	-.0233***	(-.0492)	-.0112	(-.0167)	-.0129	(-.0167)	-.0120	(-.0138)
Yth. comp x chgs in 5 yr	.0100**	(.0268)	.0068	(.0129)	.0031	(.0051)	-.0022	(-.0033)
Yth. comp x prior n incs	.0389***	(.0454)	.0239	(.0198)	.0245	(.0176)	.0188	(.0120)
Jl & prob x inc lst 2 yr	.1344**	(.0256)	.1546**	(.0209)	.1673**	(.0196)	.1430*	(.0149)
Jail x yrs using drugs	.0020	(.0150)	.0011	(.0056)	.0016	(.0074)	.0008	(.0034)
Prb w/cnd, fn x adlt arr	-.0211***	(-.0531)	-.0270***	(-.0479)	-.0220**	(-.0340)	-.0133	(-.0183)
Prb & fn x Part 1 chgs.	-.0445***	(-.0655)	-.0383**	(-.0399)	-.0390*	(-.0353)	-.0299	(-.0240)
Prb w/cnd x adlt chg cnv	-.0146***	(-.0351)	-.0174***	(-.0296)	-.0169**	(-.0250)	-.0173**	(-.0225)
Mths to jail x PO prop.	-.0001*	(-.0206)	-.0002**	(-.0232)	-.0002**	(-.0254)	-.0003***	(-.0285)
Mths to pris x prop cnvs	.0000*	(-.0214)	.0000**	(-.0266)	.0000**	(-.0240)	.0000**	(-.0254)
Init sanc x black	-.0478	(-.0196)	-.1023**	(-.0297)	-.1508***	(-.0380)	-.1410***	(-.0315)
Init sanc x n adult arrs	.0282**	(.0998)	.0517***	(.1297)	.0551***	(.1200)	.0680***	(.1311)
Constant	.2793***	(.0123)	.4816***	(.0304)	.6500***	(.0328)	.8399***	(.0495)
R squared	.200		.304		.344		.359	
Adjusted R squared	.194		.299		.338		.354	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.16

Regression Coefficients for Log of Total Post-Sentence Convictions
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0113	(.0178)	.0191*	(.0199)	.0272**	(.0237)	.0255*	(.0195)
Has job after sentence	-.0245***	(-.0404)	-.0312***	(-.0338)	-.0343***	(-.0312)	-.0365**	(-.0292)
Offender is on welfare	-.0168*	(-.0231)	-.0142	(-.0128)	-.0136	(-.0103)	-.0048	(-.0032)
Offender is Black	.0331***	(.0385)	.0901***	(.0691)	.1349***	(.0851)	.1767***	(.1044)
Offender is Hispanic	.0180	(.0155)	.0484**	(.0273)	.0681***	(.0323)	.0900***	(.0375)
Offender is female	-.0053	(-.0055)	-.0168	(-.0115)	-.0198	(-.0114)	-.0310	(-.0157)
Lives in urban area	.0036	(.0059)	.0185*	(.0199)	.0287**	(.0259)	.0304**	(.0241)
Years at current address	-.0004	(-.0138)	-.0003	(-.0066)	-.0003	(-.0061)	-.0003	(-.0056)
History of drug problems	-.0031	(-.0044)	.0065	(.0061)	.0204	(.0159)	.0252	(.0172)
Treated for drugs/alch.	-.0018	(-.0022)	.0112	(.0092)	.0129	(.0090)	.0391*	(.0237)
Has needle marks	.0045	(.0038)	.0381*	(.0211)	.0595**	(.0277)	.0662**	(.0270)
Not a school drop out	-.0085	(-.0137)	-.0161*	(-.0170)	-.0249**	(-.0220)	-.0261*	(-.0203)
Doesn't live with family	.0029	(.0040)	-.0035	(-.0031)	-.0285**	(-.0213)	-.0294*	(-.0193)
Commited PO with group	-.0219***	(-.0362)	-.0224**	(-.0242)	-.0199*	(-.0181)	-.0189	(-.0151)
Victim was a stranger	.0171*	(.0250)	.0295**	(.0283)	.0390***	(.0314)	.0576***	(.0407)
Presenting Offense								
PO property crime	.0122	(.0198)	.0425***	(.0454)	.0457***	(.0409)	.0540***	(.0425)
PO crime against person	-.0042	(-.0071)	.0083	(.0029)	.0098	(.0034)	.0041	(-.0016)
PO drug offense	.0032	(.0103)	.0170	(.0241)	.0158	(.0202)	.0166	(.0211)
PO Wolfgang severity	-.0007	(-.0183)	-.0009	(-.0144)	-.0013	(-.0183)	-.0016*	(-.0197)
Has detainers at arrest	-.0153	(-.0155)	-.0046	(-.0105)	-.0124	(-.0127)	-.0249	(-.0177)
Has pending charges	.0237***	(.0314)	.0490***	(.0426)	.0639***	(.0466)	.0747***	(.0478)
On probation at PO	.0338***	(.0315)	.0512***	(.0261)	.0618***	(.0223)	.0656***	(.0168)
Anamnestic Theory								
N prior adult arrests	.0041	(.1063)	.0171***	(.2230)	.0225***	(.2238)	.0257***	(.2375)
N prior adult conviction	.0010	(.0426)	.0019	(.0447)	.0077*	(.0710)	.0091*	(.0707)
N prior adult chg. conv.	.0002	(-.0260)	-.0039	(-.0557)	-.0076*	(-.0743)	-.0087*	(-.0744)
N charges past 5 years	.0042***	(.0778)	.0062***	(.0727)	.0085***	(.0782)	.0101***	(.0812)
N prior Part 1 charges	.0122***	(.0325)	.0108**	(.0149)	.0146***	(.0331)	.0149**	(.0320)
N prior property conv.	.0021	(.0230)	.0032	(.0240)	.0009	(.0133)	.0029	(.0183)
N prior persons conv.	-.0070**	(-.0351)	-.0106**	(-.0346)	-.0099*	(-.0272)	-.0100*	(-.0241)
N prior weapons conv.	.0030	(.0034)	.0103	(.0078)	-.0020	(-.0013)	.0008	(.0005)
Off street last 2 years	.0334***	(.0555)	.0805***	(.0849)	.1000***	(.0886)	.1119***	(.0879)
Delinquent Career/Onset								
N arrests as juvenile	.0093***	(.0621)	.0171***	(.0741)	.0196***	(.0663)	.0214***	(.0578)
N charges as juvenile	.0073	(.0321)	.0030	(.0277)	-.0023	(.0231)	-.0114	(.0193)
Age at first arrest	.0013	(.0569)	.0039***	(.0800)	.0051***	(.0830)	.0051***	(.0744)
Yrs since first incarc.	.0010***	(.1093)	.0021***	(.1305)	.0036***	(.1646)	.0036***	(.1570)
Yrs since first drug use	-.0003	(-.0024)	-.0008*	(-.0174)	-.0011**	(-.0208)	-.0013**	(-.0225)
Prior CJS-Offender Action								
N prior incarcerations	-.0024	(-.0037)	-.0077*	(-.0299)	-.0162***	(-.0527)	-.0213***	(-.0628)
N prior parole revokes	.0030	(.0048)	.0009	(.0009)	.0049	(.0042)	.0023	(.0018)
Bad conduct last probat.	.0120	(.0154)	.0186	(.0158)	.0222	(.0158)	.0307*	(.0192)
Recent parole revoked	.0368*	(.0414)	.0097	(.0236)	.0016	(.0185)	.0038	(.0252)
General Control Variables								
Offender age at sent.	-.0008	(-.1232)	-.0036***	(-.1829)	-.0056***	(-.1982)	-.0068***	(-.2045)
Off. born out of state	-.0240***	(-.0396)	-.0422***	(-.0457)	-.0595***	(-.0541)	-.0880***	(-.0702)
Coder prob. prognosis	-.0003*	(-.0243)	-.0005**	(-.0330)	-.0007***	(-.0335)	-.0008***	(-.0373)

*p<.05 **p<.01 ***p<.001

Table 8.16 (continued)

Regression Coefficients for Log of Total Post-Sentence Convictions
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0226	(-.0148)	-.0481*	(-.0207)	-.0714**	(-.0258)	-.0899***	(-.0285)
Black x prior adult arrs	-.0068***	(-.0480)	-.0103***	(-.0476)	-.0124***	(-.0482)	-.0144***	(-.0491)
Black x n prior prop cnv	.0059**	(.0432)	.0081**	(.0390)	.0107***	(.0430)	.0122***	(.0430)
Black x n charges as juv	.0256*	(.0221)	.0486**	(.0275)	.0605***	(.0287)	.0784***	(.0327)
Female x Part 1 charges	.0109	(.0262)	.0223*	(.0352)	.0283**	(.0374)	.0360**	(.0419)
Off. age x drug problem	-.0039**	(-.0508)	-.0056***	(-.0484)	-.0060**	(-.0431)	-.0067**	(-.0422)
Off. age x prior trtment	-.0023	(-.0265)	-.0050**	(-.0380)	-.0057*	(-.0364)	-.0069**	(-.0388)
Off. age x unemployed	-.0017**	(-.0251)	-.0031***	(-.0298)	-.0037***	(-.0300)	-.0040**	(-.0284)
Off. age x PO property	-.0032***	(-.0484)	-.0054***	(-.0531)	-.0058***	(-.0481)	-.0068***	(-.0498)
Off. age x chg pst 5 yrs	.0000	(.0016)	-.0001	(-.0151)	-.0002	(-.0164)	-.0002	(-.0115)
PO viol x has detainers	-.0189	(-.0061)	-.0758*	(-.0161)	-.0797*	(-.0143)	-.1009*	(-.0159)
PO prop x n adl.arrests	.0052*	(.0362)	.0103**	(.0472)	.0104**	(.0398)	.0140***	(.0471)
PO prop x prior prop con	-.0076***	(-.0553)	-.0106***	(-.0504)	-.0095**	(-.0381)	-.0139***	(-.0489)
PO prop x n juv. arrests	-.0020	(-.0082)	-.0044	(-.0119)	-.0072	(-.0163)	-.0100*	(-.0197)
PO prop x age at 1st arr	.0023	(.0288)	.0023	(.0186)	.0021	(.0145)	.0024	(.0145)
PO prop x yrs. 1st incar	.0010***	(.0375)	.0013**	(.0304)	.0010*	(.0208)	.0016**	(.0294)
PO drugs x n adl. convs.	.0118***	(.0571)	.0172***	(.0548)	.0151**	(.0403)	.0157**	(.0369)
PO drugs x Part 1 chgs.	-.0231***	(-.0741)	-.0203**	(-.0429)	-.0113	(-.0200)	-.0135	(-.0210)
PO drugs x last par. rev	.0697	(.0230)	.1554**	(.0337)	.1766**	(.0321)	.2476***	(.0395)
Sentence								
Prison	-.0844***	(-.0937)	-.1339***	(-.0976)	-.1788***	(-.1094)	-.2377***	(-.1277)
Youth complex	-.0336*	(-.0330)	-.0432	(-.0278)	-.0491	(-.0265)	-.0783**	(-.0372)
Jail, probation, fine	-.0577**	(-.0362)	-.1017***	(-.0419)	-.1415***	(-.0489)	-.1519***	(-.0461)
Jail, probation	-.0359	(-.0143)	-.0805**	(-.0270)	-.1302***	(-.0407)	-.1431***	(-.0369)
Jail only	-.0279	(-.0290)	-.0598**	(-.0408)	-.0910***	(-.0523)	-.1185***	(-.0597)
Probation w/cond., fine	-.0263*	(-.0285)	-.0397*	(-.0282)	-.0613**	(-.0366)	-.0680**	(-.0356)
Probation, fine	-.0333**	(-.0396)	-.0513**	(-.0400)	-.0622***	(-.0407)	-.0715***	(-.0411)
Probation w/conditions	-.0192	(-.0214)	-.0200	(-.0147)	-.0030	(-.0018)	-.0034	(-.0018)
Fined only	-.0132	(-.0069)	-.0286	(-.0098)	-.0340	(-.0098)	-.0602	(-.0152)
Other sanction	-.0434	(-.0152)	-.0731	(-.0168)	-.0633	(-.0122)	-.0964	(-.0163)
Dollars fined	.0000	(.0052)	.0000	(.0074)	.0000	(.0069)	.0000	(.0062)
Months on probation	.0001	(.0053)	-.0001	(-.0049)	.0000	(.0014)	.0000	(.0006)
Months to jail	.0007	(-.0067)	.0013	(.0017)	.0005	(-.0074)	-.0001	(-.0154)
Months to prison	-.0002*	(-.0253)	-.0003**	(-.0361)	-.0005***	(-.0406)	-.0006***	(-.0476)
First sanction of career	.0526**	(.0689)	.0734**	(.0500)	.0804**	(.0355)	.0822*	(.0317)
Progressive sanction	.0032	(.0044)	.0047	(.0043)	.0050	(.0038)	.0056	(.0037)
Sentence Interactions								
Prison x n adult arrests	.0056**	(.0267)	.0075**	(.0235)	.0071*	(.0186)	.0050	(.0114)
Prison x n arrsts as juv	-.0082**	(-.0229)	-.0135**	(-.0248)	-.0173***	(-.0266)	-.0235***	(-.0318)
Yth. comp x n adlt convs	-.0120***	(-.0469)	-.0084	(-.0214)	-.0071	(-.0152)	-.0107	(-.0202)
Yth. comp x chgs in 5 yr	.0047*	(.0234)	.0046	(.0149)	.0006	(.0017)	-.0005	(-.0013)
Yth. comp x prior n incs	.0193***	(.0416)	.0144	(.0204)	.0314***	(.0373)	.0355***	(.0370)
Jl & prob x inc lst 2 yr	.0599*	(.0211)	.0942**	(.0218)	.1192**	(.0231)	.1531***	(.0261)
Jail x yrs using drugs	.0021**	(.0294)	.0018	(.0166)	.0024*	(.0182)	.0023	(.0154)
Prb w/cnd, fn x adlt arr	-.0121***	(-.0560)	-.0145***	(-.0441)	-.0190***	(-.0486)	-.0152**	(-.0341)
Prb & fn x Part 1 chgs.	-.0246***	(-.0670)	-.0368***	(-.0657)	-.0475***	(-.0712)	-.0471***	(-.0619)
Prb w/cnd x adlt chg cnv	-.0078***	(-.0347)	-.0133***	(-.0386)	-.0149***	(-.0365)	-.0151***	(-.0322)
Mths to jail x PO prop.	-.0001**	(-.0268)	-.0001*	(-.0196)	-.0001	(-.0162)	-.0001*	(-.0174)
Mths to pris x prop cnvs	.0000***	(-.0500)	.0000***	(-.0458)	.0000***	(-.0478)	.0000***	(-.0477)
Init sanc x black	-.0170	(-.0129)	-.0536**	(-.0266)	-.0872***	(-.0364)	-.0896***	(-.0328)
Init sanc x n adult arrs	.0162**	(.1060)	.0259**	(.1112)	.0285**	(.1027)	.0351**	(.1108)
Constant	.1308***	(.0190)	.2246***	(.0202)	.3105***	(.0187)	.4001***	(.0309)
R squared	.163		.253		.294		.310	
Adjusted R squared	.156		.247		.289		.304	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.17

Regression Coefficients for Log of Total Post-Sentence Persons Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0068	(.0131)	.0159*	(.0210)	.0265**	(.0295)	.0220*	(.0208)
Has job after sentence	-.0149**	(-.0298)	-.0119	(-.0165)	-.0164	(-.0190)	-.0229*	(-.0226)
Offender is on welfare	-.0018	(-.0030)	-.0029	(-.0033)	-.0019	(-.0019)	-.0007	(-.0006)
Offender is Black	.0302***	(.0417)	.0791***	(.0798)	.0949***	(.0796)	.1246***	(.0982)
Offender is Hispanic	.0193*	(.0202)	.0511***	(.0368)	.0665***	(.0402)	.0995***	(.0511)
Offender is female	-.0323***	(-.0412)	-.0617***	(-.0541)	-.0823***	(-.0608)	-.1030***	(-.0645)
Lives in urban area	.0111*	(.0222)	.0178*	(.0244)	.0258**	(.0297)	.0288**	(.0281)
Years at current address	-.0003	(-.0106)	-.0005	(-.0148)	-.0005	(-.0123)	-.0001	(-.0027)
History of drug problems	.0027	(.0047)	-.0012	(-.0015)	.0006	(.0006)	-.0171	(-.0145)
Treated for drugs/alch.	-.0097	(-.0148)	-.0133	(-.0139)	-.0118	(-.0104)	-.0151	(-.0113)
Has needle marks	-.0080	(-.0082)	-.0280*	(-.0197)	-.0248	(-.0147)	-.0012	(-.0006)
Not a school drop out	.0018	(.0034)	-.0026	(-.0035)	-.0125	(-.0141)	-.0166	(-.0159)
Doesn't live with family	-.0036	(-.0060)	-.0020	(-.0022)	-.0060	(-.0057)	-.0041	(-.0033)
Commited PO with group	-.0013	(-.0027)	.0062	(.0085)	.0015	(.0018)	.0024	(.0023)
Victim was a stranger	.0034	(.0061)	.0051	(.0063)	.0129	(.0132)	.0205	(.0178)
Presenting Offense								
PO property crime	.0017	(.0034)	.0026	(.0035)	.0013	(.0015)	.0036	(.0035)
PO crime against person	.0223**	(.0310)	.0507***	(.0505)	.0638***	(.0531)	.0726***	(.0487)
PO drug offense	.0000	(-.0032)	-.0111	(-.0148)	-.0155	(-.0152)	-.0112	(-.0072)
PO Wolfgang severity	.0001	(.0047)	.0000	(.0008)	-.0002	(-.0028)	.0002	(.0026)
Has detainers at arrest	-.0042	(-.0132)	-.0180	(-.0224)	-.0152	(-.0203)	.0016	(-.0152)
Has pending charges	.0134*	(.0215)	.0164*	(.0182)	.0241*	(.0225)	.0329**	(.0259)
On probation at PO	-.0045	(-.0200)	.0019	(-.0197)	.0078	(-.0155)	.0193	(-.0074)
Anamnestic Theory								
N prior adult arrests	.0066**	(.1166)	.0135***	(.1628)	.0155***	(.1689)	.0188***	(.1805)
N prior adult conviction	-.0061***	(-.0807)	-.0072**	(-.0600)	-.0071*	(-.0485)	-.0080*	(-.0426)
N prior adult chg. conv.	.0003	(-.0156)	-.0011	(-.0253)	-.0052	(-.0590)	-.0074*	(-.0691)
N charges past 5 years	.0031***	(.0756)	.0050***	(.0768)	.0046**	(.0599)	.0060***	(.0622)
N prior Part 1 charges	.0055*	(.0153)	-.0012	(-.0449)	.0038	(.0063)	.0024	(.0019)
N prior property conv.	-.0037*	(-.0614)	-.0014	(-.0285)	-.0025	(-.0336)	-.0008	(-.0202)
N prior persons conv.	.0147***	(.0889)	.0254***	(.1056)	.0331***	(.1163)	.0410***	(.1220)
N prior weapons conv.	.0217**	(.0302)	.0198*	(.0190)	.0318**	(.0257)	.0562***	(.0385)
Off street last 2 years	.0133*	(.0319)	.0342***	(.0495)	.0409***	(.0487)	.0623***	(.0575)
Delinquent Career/Onset								
N arrests as juvenile	.0080***	(.0644)	.0115***	(.0664)	.0141***	(.0709)	.0195***	(.0811)
N charges as juvenile	-.0054	(.0032)	-.0165	(.0118)	-.0053	(.0215)	-.0102	(.0189)
Age at first arrest	.0008	(.0250)	.0028**	(.0482)	.0040***	(.0641)	.0038**	(.0642)
Yrs since first incarc.	.0003	(.0271)	.0007*	(.0448)	.0012**	(.0673)	.0014**	(.0633)
Yrs since first drug use	.0000	(-.0082)	-.0005	(-.0177)	-.0007*	(-.0209)	-.0009*	(-.0247)
Prior CJS-Offender Action								
N prior incarcerations	.0034	(.0382)	.0054	(.0419)	.0063	(.0379)	.0063	(.0308)
N prior parole revokes	-.0024	(-.0045)	-.0177*	(-.0233)	-.0168	(-.0186)	-.0255*	(-.0239)
Bad conduct last probat.	.0128	(.0201)	.0211*	(.0228)	.0171	(.0155)	.0211	(.0163)
Recent parole revoked	.0336*	(.0244)	.0341	(.0183)	.0312	(.0173)	.0433	(.0250)
General Control Variables								
Offender age at sent.	-.0008	(-.0638)	-.0033***	(-.1178)	-.0050***	(-.1512)	-.0060***	(-.1722)
Off. born out of state	-.0147**	(-.0295)	-.0297***	(-.0409)	-.0424***	(-.0492)	-.0561***	(-.0552)
Coder prob. prognosis	-.0001	(-.0080)	-.0004**	(-.0276)	-.0004*	(-.0240)	-.0005*	(-.0255)

*p<.05 **p<.01 ***p<.001

Table 8.17 (continued)

Regression Coefficients for Log of Total Post-Sentence Persons Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0180	(-.0144)	-.0450**	(-.0247)	-.0556**	(-.0257)	-.0655**	(-.0256)
Black x prior adult arrs	-.0052**	(-.0442)	-.0088***	(-.0519)	-.0129***	(-.0638)	-.0145***	(-.0608)
Black x n prior prop cnv	.0023	(.0202)	.0038	(.0230)	.0064*	(.0329)	.0064*	(.0281)
Black x n charges as juv	.0156	(.0164)	.0562***	(.0404)	.0522***	(.0317)	.0550***	(.0334)
Female x Part 1 charges	-.0079	(-.0231)	-.0099	(-.0199)	-.0071	(-.0121)	-.0005	(-.0007)
Off. age x drug problem	-.0005	(-.0082)	-.0013	(-.0141)	-.0024	(-.0225)	-.0034	(-.0267)
Off. age x prior trtment	-.0014	(-.0202)	-.0019	(-.0187)	-.0024	(-.0193)	-.0045*	(-.0313)
Off. age x unemployed	-.0011*	(-.0204)	-.0016*	(-.0194)	-.0021*	(-.0220)	-.0022*	(-.0197)
Off. age x PO property	-.0004	(-.0079)	-.0003	(-.0032)	-.0007	(-.0077)	-.0025	(-.0225)
Off. age x chg pst 5 yrs	-.0001	(-.0221)	-.0003**	(-.0364)	-.0003*	(-.0325)	-.0004*	(-.0339)
PO viol x has detainers	-.0459*	(-.0181)	-.0753**	(-.0204)	-.1021**	(-.0233)	-.1660***	(-.0321)
PO prop x n adl.arrests	.0011	(.0092)	.0028	(.0161)	.0051	(.0252)	.0091*	(.0377)
PO prop x prior prop con	-.0018	(-.0154)	-.0064*	(-.0387)	-.0086**	(-.0439)	-.0106**	(-.0457)
PO prop x n juv. arrests	-.0036	(-.0180)	-.0044	(-.0152)	-.0041	(-.0117)	-.0043	(-.0105)
PO prop x age at 1st arr	.0000	(.0001)	-.0013	(-.0132)	-.0010	(-.0088)	.0011	(.0082)
PO prop x yrs. 1st incar	-.0001	(-.0024)	-.0001	(-.0018)	.0001	(.0033)	.0000	(.0000)
PO drugs x n adl. convs.	.0011	(.0067)	.0039	(.0158)	.0047	(.0162)	.0075	(.0217)
PO drugs x Part 1 chgs.	-.0038	(-.0150)	-.0137*	(-.0367)	-.0096	(-.0217)	-.0140	(-.0269)
PO drugs x last par. rev	-.0331	(-.0133)	-.0255	(-.0070)	.0013	(.0003)	.0445	(.0088)
Sentence								
Prison	-.0433**	(-.0584)	-.0528**	(-.0489)	-.0726**	(-.0568)	-.1131***	(-.0749)
Youth complex	-.0016	(-.0019)	.0027	(.0022)	-.0086	(-.0059)	-.0273	(-.0160)
Jail, probation, fine	-.0227	(-.0173)	-.0331	(-.0173)	-.0572*	(-.0253)	-.0795*	(-.0298)
Jail, probation	-.0015	(.0183)	.0073	(.0216)	.0005	(.0142)	.0214	(.0141)
Jail only	.0034	(.0044)	.0202	(.0176)	.0015	(.0011)	-.0197	(-.0122)
Probation w/cond., fine	-.0011	(-.0015)	-.0186	(-.0168)	-.0131	(-.0100)	-.0274	(-.0177)
Probation, fine	-.0080	(-.0115)	-.0109	(-.0108)	-.0005	(-.0004)	-.0016	(-.0011)
Probation w/conditions	-.0027	(-.0037)	.0065	(.0060)	.0174	(.0137)	.0117	(.0078)
Fined only	.0015	(.0010)	.0110	(.0048)	.0005	(.0002)	-.0166	(-.0051)
Other sanction	-.0045	(-.0019)	-.0017	(-.0005)	.0056	(.0014)	-.0002	(.0000)
Dollars fined	.0000	(-.0004)	.0000	(-.0014)	.0000*	(.0178)	.0000*	(.0201)
Months on probation	-.0001	(-.0091)	.0000	(.0020)	-.0001	(-.0052)	-.0001	(-.0036)
Months to jail	.0002	(.0003)	.0004	(-.0079)	.0007	(-.0056)	.0006	(-.0143)
Months to prison	.0000	(-.0017)	-.0001	(-.0183)	-.0002	(-.0203)	-.0004**	(-.0346)
First sanction of career	.0210	(.0240)	.0253	(.0104)	.0226	(.0012)	.0104	(-.0054)
Progressive sanction	.0036	(.0061)	.0044	(.0051)	.0111	(.0107)	.0041	(.0033)
Sentence Interactions								
Prison x n adult arrests	.0007	(.0039)	.0011	(.0043)	.0036	(.0120)	-.0008	(-.0022)
Prison x n arrsts as juv	-.0014	(-.0048)	-.0013	(-.0030)	-.0027	(-.0052)	-.0113*	(-.0188)
Yth. comp x n adlt convs	-.0082**	(-.0388)	-.0031	(-.0102)	-.0032	(-.0087)	-.0040	(-.0093)
Yth. comp x chgs in 5 yr	.0060***	(.0363)	.0048	(.0199)	.0050	(.0175)	.0023	(.0068)
Yth. comp x prior n incs	.0091	(.0237)	.0155*	(.0279)	.0117	(.0178)	.0085	(.0110)
Jl & prob x inc lst 2 yr	.0719***	(.0307)	.0893**	(.0263)	.0878*	(.0218)	.0309	(.0065)
Jail x yrs using drugs	-.0010	(-.0175)	-.0002	(-.0018)	-.0001	(-.0005)	-.0005	(-.0042)
Prb w/cnd, fn x adlt arr	-.0036	(-.0205)	-.0067*	(-.0261)	-.0033	(-.0106)	-.0028	(-.0076)
Prb & fn x Part 1 chgs.	-.0137**	(-.0453)	-.0105	(-.0237)	.0029	(.0056)	.0092	(.0149)
Prb w/cnd x adlt chg cnv	-.0039*	(-.0209)	-.0069*	(-.0253)	-.0066*	(-.0205)	-.0081*	(-.0214)
Mths to jail x PO prop.	.0000	(-.0044)	-.0001	(-.0175)	-.0001*	(-.0198)	-.0001**	(-.0260)
Mths to pris x prop cnvs	.0000	(-.0070)	.0000	(-.0100)	.0000	(-.0083)	.0000	(-.0065)
Init sanc x black	-.0185	(-.0170)	-.0390*	(-.0247)	-.0491*	(-.0261)	-.0372	(-.0168)
Init sanc x n adult arrs	.0076	(.0605)	.0120	(.0653)	.0161*	(.0739)	.0184	(.0719)
Constant	.0528***	(.0026)	.0997***	(.0053)	.1614***	(.0161)	.2377***	(.0248)
R squared	.090		.148		.174		.198	
Adjusted R squared	.083		.141		.167		.192	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.18

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0143	(.0149)	.0166*	(.0204)	.0192**	(.0254)	.0164**	(.0238)
Has job after sentence	-.0428***	(-.0464)	-.0290***	(-.0372)	-.0231***	(-.0319)	-.0191***	(-.0290)
Offender is on welfare	-.0298**	(-.0270)	-.0167*	(-.0178)	-.0166*	(-.0192)	-.0116	(-.0146)
Offender is Black	.0640***	(.0433)	.0821***	(.0762)	.0919***	(.0939)	.0913***	(.1133)
Offender is Hispanic	.0432**	(.0244)	.0545***	(.0364)	.0568***	(.0409)	.0582***	(.0460)
Offender is female	-.0207	(-.0143)	-.0251*	(-.0204)	-.0227*	(-.0199)	-.0233*	(-.0225)
Lives in urban area	.0089	(.0096)	.0165*	(.0210)	.0204**	(.0280)	.0204***	(.0308)
Years at current address	-.0008	(-.0171)	-.0006*	(-.0166)	-.0004	(-.0123)	-.0002	(-.0066)
History of drug problems	.0040	(.0037)	.0123	(.0135)	.0195*	(.0231)	.0139	(.0181)
Treated for drugs/alch.	-.0182	(-.0150)	-.0036	(-.0035)	.0104	(.0110)	.0191*	(.0220)
Has needle marks	.0237	(.0131)	.0286*	(.0187)	.0362**	(.0255)	.0351***	(.0272)
Not a school drop out	-.0160	(-.0169)	-.0139*	(-.0173)	-.0130*	(-.0175)	-.0075	(-.0110)
Doesn't live with family	-.0070	(-.0062)	-.0120	(-.0126)	-.0187**	(-.0212)	-.0163**	(-.0203)
Commited PO with group	-.0228**	(-.0248)	-.0108	(-.0139)	-.0065	(-.0090)	-.0071	(-.0108)
Victim was a stranger	.0198*	(.0190)	.0187*	(.0212)	.0191**	(.0233)	.0185**	(.0248)
Presenting Offense								
PO property crime	.0250*	(.0267)	.0278**	(.0350)	.0281***	(.0383)	.0270***	(.0404)
PO crime against person	-.0060	(-.0077)	.0119	(.0057)	.0136	(.0084)	.0118	(.0089)
PO drug offense	-.0098	(-.0048)	-.0058	(-.0001)	-.0083	(-.0039)	-.0031	(.0024)
PO Wolfgang severity	-.0005	(-.0088)	.0000	(.0001)	.0003	(.0062)	.0003	(.0067)
Has detainers at arrest	-.0186	(-.0146)	.0150	(-.0028)	.0152	(-.0021)	.0036	(-.0073)
Has pending charges	.0507***	(.0442)	.0548***	(.0563)	.0464***	(.0514)	.0422***	(.0513)
On probation at PO	.0601***	(.0306)	.0391***	(.0229)	.0384***	(.0229)	.0273**	(.0197)
Anamnestic Theory								
N prior adult arrests	.0128***	(.1483)	.0161***	(.2147)	.0160***	(.2151)	.0140***	(.2153)
N prior adult conviction	-.0048	(-.0070)	-.0058*	(-.0157)	-.0027	(.0051)	-.0023	(.0018)
N prior adult chg. conv.	-.0009	(-.0386)	-.0049*	(-.0760)	-.0069***	(-.1007)	-.0058**	(-.0946)
N charges past 5 years	.0100***	(.1134)	.0101***	(.1359)	.0082***	(.1204)	.0083***	(.1353)
N prior Part 1 charges	.0158***	(.0146)	.0110***	(.0101)	.0113***	(.0262)	.0114***	(.0385)
N prior property conv.	.0019	(.0130)	.0047	(.0399)	.0037	(.0400)	.0024	(.0219)
N prior persons conv.	-.0011	(-.0036)	-.0019	(-.0075)	.0002	(.0009)	-.0007	(-.0032)
N prior weapons conv.	.0250*	(.0189)	.0187*	(.0166)	.0184*	(.0177)	.0183*	(.0193)
Off street last 2 years	.0570***	(.0634)	.0676***	(.0852)	.0705***	(.0946)	.0655***	(.0949)
Delinquent Career/Onset								
N arrests as juvenile	.0179***	(.0799)	.0160***	(.0817)	.0113***	(.0675)	.0115***	(.0806)
N charges as juvenile	-.0050	(.0088)	.0005	(.0229)	-.0025	(.0265)	-.0105	(.0206)
Age at first arrest	.0030**	(.0694)	.0041***	(.0914)	.0035***	(.0886)	.0028***	(.0823)
Yrs since first incarc.	.0022***	(.1309)	.0022***	(.1474)	.0023***	(.1624)	.0019***	(.1502)
Yrs since first drug use	-.0006	(-.0107)	-.0005	(-.0148)	-.0005*	(-.0138)	-.0005*	(-.0183)
Prior CJS-Offender Action								
N prior incarcerations	-.0016	(.0109)	-.0002	(.0118)	-.0026	(-.0020)	-.0039	(-.0153)
N prior parole revokes	.0104	(.0108)	.0022	(.0027)	-.0020	(-.0026)	.0014	(.0020)
Bad conduct last probat.	.0190	(.0161)	.0174*	(.0175)	.0175*	(.0189)	.0118	(.0140)
Recent parole revoked	.0572*	(.0386)	.0491**	(.0435)	.0420**	(.0403)	.0447**	(.0462)
General Control Variables								
Offender age at sent.	-.0021*	(-.1331)	-.0033***	(-.1826)	-.0036***	(-.1934)	-.0031***	(-.1912)
Off. born out of state	-.0303***	(-.0329)	-.0380***	(-.0487)	-.0402***	(-.0555)	-.0393***	(-.0595)
Coder prob. prognosis	-.0003	(-.0194)	-.0005***	(-.0337)	-.0004***	(-.0310)	-.0004***	(-.0343)

*p<.05 **p<.01 ***p<.001

Table 8.18 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0567**	(-.0245)	-.0382*	(-.0194)	-.0404**	(-.0221)	-.0254*	(-.0153)
Black x prior adult arrs	-.0129***	(-.0597)	-.0106***	(-.0579)	-.0103***	(-.0608)	-.0100***	(-.0647)
Black x n prior prop cnv	.0100***	(.0482)	.0076***	(.0429)	.0084***	(.0514)	.0078***	(.0528)
Black x n charges as juv	.0289	(.0164)	.0378**	(.0253)	.0473***	(.0341)	.0534***	(.0422)
Female x Part 1 charges	.0194*	(.0307)	.0165*	(.0308)	.0172**	(.0347)	.0147**	(.0325)
Off. age x drug problem	-.0049**	(-.0419)	-.0044**	(-.0445)	-.0039**	(-.0433)	-.0030**	(-.0363)
Off. age x prior trtment	-.0042*	(-.0320)	-.0049**	(-.0441)	-.0043**	(-.0422)	-.0049***	(-.0527)
Off. age x unemployed	-.0020*	(-.0192)	-.0024**	(-.0268)	-.0021**	(-.0256)	-.0017**	(-.0230)
Off. age x PO property	-.0047***	(-.0462)	-.0040***	(-.0462)	-.0038***	(-.0476)	-.0036***	(-.0496)
Off. age x chg pst 5 yrs	.0000	(.0042)	-.0002	(-.0210)	-.0003**	(-.0329)	-.0002**	(-.0352)
PO viol x has detainers	-.0438	(-.0093)	-.0982***	(-.0247)	-.0922***	(-.0250)	-.0678**	(-.0202)
PO prop x n adl.arrests	.0095**	(.0433)	.0102***	(.0550)	.0097***	(.0563)	.0116***	(.0740)
PO prop x prior prop con	-.0122***	(-.0584)	-.0111***	(-.0622)	-.0101***	(-.0613)	-.0104***	(-.0690)
PO prop x n juv. arrests	-.0074*	(-.0199)	-.0070*	(-.0223)	-.0042	(-.0144)	-.0038	(-.0144)
PO prop x age at 1st arr	.0029	(.0239)	.0015	(.0145)	.0017	(.0179)	.0018	(.0202)
PO prop x yrs. 1st incar	.0012**	(.0296)	.0009**	(.0263)	.0007*	(.0227)	.0006*	(.0218)
PO drugs x n adl. convs.	.0166***	(.0531)	.0171***	(.0646)	.0137***	(.0556)	.0105***	(.0466)
PO drugs x Part 1 chgs.	-.0392***	(-.0830)	-.0294***	(-.0734)	-.0213***	(-.0574)	-.0168***	(-.0497)
PO drugs x last par. rev	.0774	(.0168)	.0974*	(.0249)	.0845*	(.0233)	.0848*	(.0257)
Sentence								
Prison	-.1391***	(-.1016)	-.1011***	(-.0871)	-.0956***	(-.0889)	-.0752***	(-.0767)
Youth complex	-.0387	(-.0250)	-.0210	(-.0160)	-.0191	(-.0157)	-.0153	(-.0138)
Jail, probation, fine	-.1017***	(-.0420)	-.0921***	(-.0449)	-.1069***	(-.0562)	-.0830***	(-.0478)
Jail, probation	-.0699*	(-.0177)	-.0673**	(-.0243)	-.0899***	(-.0431)	-.0667***	(-.0360)
Jail only	-.0595**	(-.0408)	-.0546**	(-.0441)	-.0654***	(-.0570)	-.0564***	(-.0540)
Probation w/cond., fine	-.0341*	(-.0243)	-.0335*	(-.0281)	-.0390***	(-.0353)	-.0346**	(-.0344)
Probation, fine	-.0376*	(-.0293)	-.0311*	(-.0286)	-.0317**	(-.0315)	-.0267*	(-.0291)
Probation w/conditions	-.0084	(-.0062)	-.0105	(-.0091)	-.0066	(-.0061)	-.0045	(-.0046)
Fined only	-.0055	(-.0019)	-.0048	(-.0019)	-.0070	(-.0031)	-.0151	(-.0072)
Other sanction	-.0610	(-.0141)	-.0472	(-.0128)	-.0350	(-.0103)	-.0189	(-.0061)
Dollars fined	.0000	(.0015)	.0000	(.0037)	.0000	(.0076)	.0000	(.0099)
Months on probation	-.0001	(-.0041)	.0001	(.0043)	.0002	(.0092)	.0002	(.0082)
Months to jail	.0009	(-.0062)	.0005	(-.0087)	.0010	(-.0020)	.0003	(-.0115)
Months to prison	-.0001	(-.0151)	-.0002*	(-.0221)	-.0001*	(-.0201)	-.0001*	(-.0216)
First sanction of career	.0595*	(.0416)	.0478*	(.0316)	.0378*	(.0199)	.0274	(.0160)
Progressive sanction	.0009	(.0008)	.0062	(.0066)	.0072	(.0083)	.0116	(.0147)
Sentence Interactions								
Prison x n adult arrests	.0073**	(.0227)	.0063**	(.0234)	.0052**	(.0209)	.0050**	(.0217)
Prison x n arrsts as juv	-.0022	(-.0041)	-.0037	(-.0080)	.0004	(.0010)	.0044	(.0112)
Yth. comp x n adlt convs	-.0147**	(-.0378)	-.0124**	(-.0373)	-.0110**	(-.0360)	-.0129***	(-.0462)
Yth. comp x chgs in 5 yr	.0036	(.0116)	.0038	(.0144)	.0048*	(.0198)	.0052**	(.0235)
Yth. comp x prior n incs	.0400***	(.0567)	.0236***	(.0394)	.0229***	(.0413)	.0162**	(.0321)
Jl & prob x inc lst 2 yr	.1204***	(.0279)	.0925**	(.0253)	.0803**	(.0236)	.0551*	(.0178)
Jail x yrs using drugs	.0019	(.0175)	.0004	(.0038)	.0007	(.0083)	.0002	(.0030)
Prb w/cnd, fn x adlt arr	-.0186***	(-.0566)	-.0149***	(-.0537)	-.0122***	(-.0474)	-.0104***	(-.0441)
Prb & fn x Part 1 chgs.	-.0384***	(-.0687)	-.0279***	(-.0588)	-.0270***	(-.0615)	-.0236***	(-.0590)
Prb w/cnd x adlt chg cnv	-.0164***	(-.0478)	-.0102***	(-.0349)	-.0094***	(-.0348)	-.0058**	(-.0234)
Mths to jail x PO prop.	-.0001**	(-.0231)	-.0001*	(-.0199)	-.0001**	(-.0228)	-.0001**	(-.0203)
Mths to pris x prop cnvs	.0000**	(-.0265)	.0000***	(-.0352)	.0000**	(-.0294)	.0000**	(-.0289)
Init sanc x black	-.0412*	(-.0205)	-.0482**	(-.0283)	-.0508***	(-.0322)	-.0365**	(-.0254)
Init sanc x n adult arrs	.0204*	(.0877)	.0161*	(.0817)	.0115	(.0630)	.0091	(.0549)
Constant	.2378***	(.0131)	.2101***	(.0063)	.1943***	(-.0051)	.1681***	(-.0086)
R squared	.209		.330		.366		.381	
Adjusted R squared	.202		.325		.361		.376	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.19

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0270*	(.0210)	.0278**	(.0242)	.0277**	(.0257)	.0253**	(.0253)
Has job after sentence	-.0531***	(-.0430)	-.0375***	(-.0340)	-.0281**	(-.0272)	-.0246**	(-.0256)
Offender is on welfare	-.0399**	(-.0270)	-.0263*	(-.0199)	-.0268**	(-.0216)	-.0218*	(-.0189)
Offender is Black	.0778***	(.0376)	.1076***	(.0671)	.1244***	(.0836)	.1257***	(.1024)
Offender is Hispanic	.0510*	(.0215)	.0763***	(.0361)	.0799***	(.0403)	.0869***	(.0472)
Offender is female	-.0368	(-.0190)	-.0468**	(-.0270)	-.0405**	(-.0250)	-.0446**	(-.0295)
Lives in urban area	.0150	(.0121)	.0218*	(.0197)	.0265**	(.0255)	.0256**	(.0265)
Years at current address	-.0011	(-.0178)	-.0010*	(-.0189)	-.0009*	(-.0170)	-.0005	(-.0106)
History of drug problems	.0136	(.0094)	.0256	(.0199)	.0323**	(.0268)	.0235*	(.0210)
Treated for drugs/alch.	-.0226	(-.0140)	-.0006	(-.0004)	.0199	(.0146)	.0296*	(.0234)
Has needle marks	.0155	(.0064)	.0309	(.0143)	.0416*	(.0206)	.0482**	(.0257)
Not a school drop out	-.0189	(-.0149)	-.0177	(-.0157)	-.0165	(-.0155)	-.0108	(-.0109)
Doesn't live with family	-.0087	(-.0058)	-.0107	(-.0080)	-.0268**	(-.0213)	-.0229*	(-.0196)
Commited PO with group	-.0248*	(-.0201)	-.0121	(-.0110)	-.0058	(-.0056)	-.0069	(-.0072)
Victim was a stranger	.0198	(.0142)	.0233*	(.0187)	.0267**	(.0229)	.0271**	(.0250)
Presenting Offense								
PO property crime	.0336*	(.0269)	.0382**	(.0342)	.0393***	(.0375)	.0397***	(.0407)
PO crime against person	-.0092	(-.0099)	.0178	(.0050)	.0157	(.0044)	.0147	(.0054)
PO drug offense	-.0072	(-.0021)	-.0091	(-.0007)	-.0108	(-.0041)	-.0005	(.0047)
PO Wolfgang severity	-.0006	(-.0080)	-.0003	(-.0046)	.0002	(.0036)	.0003	(.0041)
Has detainers at arrest	-.0125	(-.0124)	.0327	(-.0003)	.0330	(.0012)	.0203	(-.0026)
Has pending charges	.0700***	(.0455)	.0792***	(.0577)	.0711***	(.0552)	.0647***	(.0540)
On probation at PO	.0762***	(.0304)	.0519***	(.0204)	.0515***	(.0184)	.0368**	(.0140)
Anamnestic Theory								
N prior adult arrests	.0172***	(.1432)	.0232***	(.2211)	.0219***	(.2096)	.0181***	(.2031)
N prior adult conviction	-.0095*	(-.0227)	-.0125***	(-.0426)	-.0074*	(-.0180)	-.0057*	(-.0141)
N prior adult chg. conv.	-.0033	(-.0520)	-.0071*	(-.0760)	-.0086**	(-.0890)	-.0063*	(-.0756)
N charges past 5 years	.0130***	(.1154)	.0123***	(.1215)	.0104***	(.1111)	.0109***	(.1240)
N prior Part 1 charges	.0253***	(.0334)	.0158***	(.0076)	.0152***	(.0221)	.0142***	(.0273)
N prior property conv.	.0014	(.0164)	.0069*	(.0523)	.0055	(.0519)	.0050	(.0424)
N prior persons conv.	-.0015	(-.0037)	-.0016	(-.0043)	.0013	(.0038)	.0015	(.0048)
N prior weapons conv.	.0330*	(.0186)	.0334*	(.0211)	.0309**	(.0208)	.0359**	(.0260)
Off street last 2 years	.0793***	(.0646)	.0983***	(.0866)	.1020***	(.0951)	.0965***	(.0959)
Delinquent Career/Onset								
N arrests as juvenile	.0213***	(.0756)	.0197***	(.0754)	.0141***	(.0611)	.0153***	(.0752)
N charges as juvenile	-.0100	(.0037)	-.0003	(.0184)	.0054	(.0300)	-.0059	(.0247)
Age at first arrest	.0031*	(.0636)	.0050***	(.0830)	.0044***	(.0829)	.0037***	(.0785)
Yrs since first incarc.	.0028***	(.1301)	.0028***	(.1405)	.0030***	(.1545)	.0025***	(.1407)
Yrs since first drug use	-.0010*	(-.0152)	-.0009**	(-.0193)	-.0009**	(-.0176)	-.0008*	(-.0176)
Prior CJS-Offender Action								
N prior incarcerations	-.0003	(.0150)	.0041	(.0249)	.0017	(.0149)	-.0029	(-.0083)
N prior parole revokes	.0195	(.0151)	.0058	(.0050)	-.0046	(-.0043)	.0006	(.0006)
Bad conduct last probat.	.0231	(.0146)	.0289*	(.0205)	.0260*	(.0197)	.0135	(.0110)
Recent parole revoked	.0763*	(.0355)	.0640*	(.0414)	.0606**	(.0384)	.0677**	(.0447)
General Control Variables								
Offender age at sent.	-.0021	(-.1294)	-.0043***	(-.1798)	-.0048***	(-.1906)	-.0043***	(-.1874)
Off. born out of state	-.0387***	(-.0314)	-.0538***	(-.0488)	-.0597***	(-.0578)	-.0562***	(-.0585)
Coder prob. prognosis	-.0005*	(-.0213)	-.0007***	(-.0374)	-.0006***	(-.0340)	-.0007***	(-.0395)

*p<.05 **p<.01 ***p<.001

Table 8.19 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0672*	(-.0216)	-.0543*	(-.0196)	-.0633**	(-.0244)	-.0456*	(-.0189)
Black x prior adult arrs	-.0194***	(-.0669)	-.0170***	(-.0657)	-.0160***	(-.0659)	-.0153***	(-.0677)
Black x n prior prop cnv	.0151***	(.0544)	.0118***	(.0477)	.0119***	(.0511)	.0111***	(.0511)
Black x n charges as juv	.0328	(.0139)	.0445*	(.0211)	.0549***	(.0278)	.0650***	(.0353)
Female x Part 1 charges	.0238*	(.0281)	.0154	(.0203)	.0184*	(.0260)	.0159	(.0242)
Off. age x drug problem	-.0057*	(-.0368)	-.0052**	(-.0375)	-.0050**	(-.0387)	-.0034*	(-.0281)
Off. age x prior trtment	-.0066*	(-.0378)	-.0083***	(-.0528)	-.0070***	(-.0480)	-.0082***	(-.0598)
Off. age x unemployed	-.0033*	(-.0237)	-.0036***	(-.0291)	-.0030**	(-.0260)	-.0025**	(-.0228)
Off. age x PO property	-.0069***	(-.0509)	-.0058***	(-.0483)	-.0057***	(-.0505)	-.0055***	(-.0526)
Off. age x chg pst 5 yrs	.0000	(-.0015)	-.0003*	(-.0289)	-.0004**	(-.0387)	-.0004**	(-.0388)
PO viol x has detainers	-.0936*	(-.0149)	-.1690***	(-.0302)	-.1540***	(-.0293)	-.1280***	(-.0262)
PO prop x n adl.arrests	.0096*	(.0329)	.0123***	(.0470)	.0116***	(.0472)	.0157***	(.0691)
PO prop x prior prop con	-.0141***	(-.0502)	-.0138***	(-.0549)	-.0116***	(-.0495)	-.0130***	(-.0593)
PO prop x n juv. arrests	-.0059	(-.0118)	-.0063	(-.0143)	-.0038	(-.0091)	-.0041	(-.0107)
PO prop x age at 1st arr	.0050*	(.0304)	.0024	(.0164)	.0029	(.0210)	.0029	(.0230)
PO prop x yrs. 1st incar	.0018**	(.0324)	.0015**	(.0299)	.0013**	(.0273)	.0010*	(.0241)
PO drugs x n adl. convs.	.0233***	(.0557)	.0245***	(.0654)	.0201***	(.0573)	.0158***	(.0485)
PO drugs x Part 1 chgs.	-.0496***	(-.0783)	-.0435***	(-.0769)	-.0324***	(-.0610)	-.0259***	(-.0526)
PO drugs x last par. rev	.0714	(.0116)	.1389*	(.0252)	.1008	(.0195)	.0994*	(.0207)
Sentence								
Prison	-.1878***	(-.1025)	-.1371***	(-.0837)	-.1265***	(-.0824)	-.1029***	(-.0721)
Youth complex	-.0462	(-.0223)	-.0269	(-.0145)	-.0235	(-.0135)	-.0136	(-.0084)
Jail, probation, fine	-.1438***	(-.0443)	-.1237***	(-.0427)	-.1485***	(-.0547)	-.1177***	(-.0466)
Jail, probation	-.0944*	(-.0204)	-.0870**	(-.0233)	-.1182***	(-.0403)	-.0904***	(-.0332)
Jail only	-.0756*	(-.0387)	-.0698**	(-.0400)	-.0849***	(-.0519)	-.0755***	(-.0496)
Probation w/cond., fine	-.0390	(-.0207)	-.0458*	(-.0273)	-.0539**	(-.0342)	-.0492**	(-.0336)
Probation, fine	-.0474*	(-.0276)	-.0447*	(-.0292)	-.0409*	(-.0285)	-.0367*	(-.0275)
Probation w/conditions	-.0197	(-.0108)	-.0221	(-.0136)	-.0106	(-.0069)	-.0078	(-.0055)
Fined only	-.0046	(-.0012)	-.0041	(-.0012)	-.0080	(-.0024)	-.0220	(-.0072)
Other sanction	-.0829	(-.0143)	-.0658	(-.0127)	-.0409	(-.0084)	-.0101	(-.0022)
Dollars fined	.0000	(.0035)	.0000	(.0040)	.0000	(.0107)	.0000	(.0134)
Months on probation	-.0001	(-.0031)	.0000	(.0001)	.0002	(.0078)	.0002	(.0063)
Months to jail	.0015	(-.0031)	.0006	(-.0109)	.0014	(-.0015)	.0007	(-.0102)
Months to prison	-.0001	(-.0092)	-.0002	(-.0193)	-.0002	(-.0180)	-.0002*	(-.0212)
First sanction of career	.0845*	(.0442)	.0791**	(.0393)	.0640*	(.0251)	.0495*	(.0230)
Progressive sanction	-.0015	(-.0010)	.0049	(.0037)	.0082	(.0067)	.0101	(.0088)
Sentence Interactions								
Prison x n adult arrests	.0128***	(.0300)	.0100**	(.0262)	.0098***	(.0274)	.0088***	(.0265)
Prison x n arrsts as juv	-.0039	(-.0053)	-.0050	(-.0077)	-.0005	(-.0008)	.0051	(.0089)
Yth. comp x n adlt convs	-.0234**	(-.0447)	-.0181**	(-.0387)	-.0170**	(-.0388)	-.0189***	(-.0464)
Yth. comp x chgs in 5 yr	.0108*	(.0262)	.0099**	(.0268)	.0097**	(.0280)	.0092**	(.0288)
Yth. comp x prior n incs	.0460***	(.0488)	.0222*	(.0264)	.0181*	(.0229)	.0107	(.0145)
Jl & prob x inc lst 2 yr	.1401**	(.0242)	.1107**	(.0214)	.1011**	(.0209)	.0774*	(.0172)
Jail x yrs using drugs	.0024	(.0165)	.0012	(.0090)	.0018	(.0145)	.0012	(.0105)
Prb w/cnd, fn x adlt arr	-.0226***	(-.0514)	-.0199***	(-.0507)	-.0159***	(-.0432)	-.0128***	(-.0375)
Prb & fn x Part 1 chgs.	-.0490***	(-.0654)	-.0363***	(-.0543)	-.0310***	(-.0493)	-.0281***	(-.0482)
Prb w/cnd x adlt chg cnv	-.0189***	(-.0412)	-.0125***	(-.0305)	-.0097**	(-.0252)	-.0074*	(-.0206)
Mths to jail x PO prop.	-.0001*	(-.0218)	-.0001**	(-.0212)	-.0001**	(-.0230)	-.0001**	(-.0236)
Mths to pris x prop cnvs	.0000**	(-.0274)	.0000***	(-.0363)	.0000***	(-.0307)	.0000***	(-.0312)
Init senc x black	-.0584*	(-.0217)	-.0737**	(-.0307)	-.0818***	(-.0363)	-.0584**	(-.0279)
Init senc x n adult arrs	.0294*	(.0944)	.0285**	(.1023)	.0204*	(.0781)	.0173*	(.0713)
Constant	.3021***	(.0108)	.2869***	(.0098)	.2742***	(-.0028)	.2474***	(-.0032)
R squared	.208		.322		.357		.371	
Adjusted R squared	.202		.317		.352		.366	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.20

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0072	(.0123)	.0117*	(.0234)	.0129**	(.0279)	.0121**	(.0296)
Has job after sentence	-.0172**	(-.0309)	-.0117*	(-.0243)	-.0115**	(-.0261)	-.0097*	(-.0248)
Offender is on welfare	-.0018	(-.0026)	-.0036	(-.0063)	-.0048	(-.0090)	-.0060	(-.0127)
Offender is Black	.0323***	(.0382)	.0441***	(.0666)	.0426***	(.0688)	.0387***	(.0767)
Offender is Hispanic	.0200	(.0187)	.0273**	(.0297)	.0266***	(.0313)	.0255***	(.0339)
Offender is female	-.0336***	(-.0383)	-.0345***	(-.0457)	-.0334***	(-.0479)	-.0315***	(-.0512)
Lives in urban area	.0113*	(.0201)	.0092	(.0189)	.0088*	(.0197)	.0086*	(.0219)
Years at current address	-.0003	(-.0120)	-.0004	(-.0184)	-.0004*	(-.0186)	-.0002	(-.0115)
History of drug problems	.0057	(.0088)	.0028	(.0050)	.0060	(.0117)	-.0036	(-.0080)
Treated for drugs/alch.	-.0100	(-.0136)	-.0058	(-.0092)	-.0061	(-.0104)	-.0012	(-.0023)
Has needle marks	-.0126	(-.0115)	-.0225*	(-.0240)	-.0189*	(-.0218)	-.0056	(-.0074)
Not a school drop out	.0021	(.0036)	-.0009	(-.0019)	-.0046	(-.0102)	-.0038	(-.0093)
Doesn't live with family	-.0053	(-.0079)	-.0047	(-.0080)	-.0041	(-.0076)	-.0028	(-.0058)
Commited PO with group	-.0020	(-.0035)	.0027	(.0057)	.0022	(.0049)	.0020	(.0052)
Victim was a stranger	.0038	(.0060)	.0016	(.0029)	.0063	(.0125)	.0056	(.0127)
Presenting Offense								
PO property crime	.0033	(.0058)	.0037	(.0076)	.0024	(.0054)	.0039	(.0098)
PO crime against person	.0248**	(.0319)	.0302***	(.0460)	.0277***	(.0453)	.0226***	(.0414)
PO drug offense	.0008	(-.0033)	-.0043	(-.0129)	-.0065	(-.0172)	-.0026	(-.0091)
PO Wolfgang severity	.0004	(.0105)	.0004	(.0138)	.0006	(.0201)	.0006*	(.0223)
Has detainers at arrest	-.0104	(-.0160)	-.0213*	(-.0299)	-.0137	(-.0241)	-.0060	(-.0165)
Has pending charges	.0145*	(.0209)	.0127*	(.0212)	.0143**	(.0258)	.0168***	(.0343)
On probation at PO	-.0016	(-.0155)	.0028	(-.0130)	.0050	(-.0106)	.0065	(-.0065)
Anamnestic Theory								
N prior adult arrests	.0062**	(.1043)	.0062**	(.1283)	.0050**	(.1161)	.0033*	(.0886)
N prior adult conviction	-.0071***	(-.0793)	-.0060***	(-.0713)	-.0055***	(-.0732)	-.0049***	(-.0729)
N prior adult chg. conv.	.0007	(-.0100)	.0001	(-.0144)	-.0007	(-.0268)	.0006	(-.0109)
N charges past 5 years	.0037***	(.0803)	.0040***	(.0966)	.0032***	(.0857)	.0034***	(.0986)
N prior Part 1 charges	.0071**	(.0152)	.0025	(-.0220)	.0046*	(.0163)	.0037*	(.0108)
N prior property conv.	-.0034	(-.0525)	-.0012	(-.0308)	-.0018	(-.0418)	-.0011	(-.0363)
N prior persons conv.	.0166***	(.0898)	.0161***	(.1015)	.0169***	(.1149)	.0161***	(.1242)
N prior weapons conv.	.0210**	(.0262)	.0138*	(.0200)	.0182**	(.0286)	.0223***	(.0395)
Off street last 2 years	.0168*	(.0343)	.0226***	(.0482)	.0222***	(.0499)	.0249***	(.0594)
Delinquent Career/Onset								
N arrests as juvenile	.0091***	(.0747)	.0063***	(.0684)	.0054***	(.0686)	.0059***	(.0861)
N charges as juvenile	-.0053	(.0045)	-.0058	(.0256)	.0018	(.0404)	-.0043	(.0312)
Age at first arrest	.0010	(.0307)	.0016**	(.0511)	.0019***	(.0663)	.0014**	(.0660)
Yrs since first incarc.	.0004	(.0325)	.0006**	(.0544)	.0006***	(.0689)	.0005**	(.0624)
Yrs since first drug use	.0000	(-.0080)	-.0003	(-.0161)	-.0003	(-.0132)	-.0003*	(-.0170)
Prior CJS-Offender Action								
N prior incarcerations	.0032	(.0346)	.0048*	(.0512)	.0050*	(.0532)	.0033	(.0393)
N prior parole revokes	-.0043	(-.0074)	-.0143*	(-.0283)	-.0133*	(-.0285)	-.0118*	(-.0286)
Bad conduct last probat.	.0113	(.0158)	.0117	(.0190)	.0085	(.0150)	.0043	(.0087)
Recent parole revoked	.0532***	(.0350)	.0536***	(.0410)	.0512***	(.0438)	.0468***	(.0488)
General Control Variables								
Offender age at sent.	-.0008	(-.0578)	-.0017**	(-.1012)	-.0019***	(-.1168)	-.0015***	(-.1211)
Off. born out of state	-.0152**	(-.0273)	-.0168***	(-.0351)	-.0201***	(-.0454)	-.0163***	(-.0415)
Coder prob. prognosis	-.0001	(-.0083)	-.0002*	(-.0242)	-.0002*	(-.0233)	-.0002**	(-.0269)

*p<.05 **p<.01 ***p<.001

Table 8.20 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0210	(-.0150)	-.0241*	(-.0199)	-.0248*	(-.0222)	-.0220*	(-.0224)
Black x prior adult arrs	-.0066***	(-.0505)	-.0068***	(-.0608)	-.0074***	(-.0708)	-.0068***	(-.0736)
Black x n prior prop cnv	.0035*	(.0278)	.0040**	(.0367)	.0047***	(.0470)	.0042***	(.0477)
Black x n charges as juv	.0177	(.0165)	.0398***	(.0433)	.0348***	(.0410)	.0364***	(.0485)
Female x Part 1 charges	-.0100	(-.0261)	-.0075	(-.0227)	-.0063	(-.0208)	-.0052	(-.0192)
Off. age x drug problem	-.0005	(-.0070)	-.0005	(-.0085)	-.0006	(-.0116)	-.0006	(-.0112)
Off. age x prior trtment	-.0010	(-.0121)	-.0012	(-.0173)	-.0011	(-.0167)	-.0013	(-.0240)
Off. age x unemployed	-.0013*	(-.0207)	-.0014**	(-.0253)	-.0013**	(-.0268)	-.0012**	(-.0268)
Off. age x PO property	-.0006	(-.0092)	-.0003	(-.0048)	-.0003	(-.0055)	-.0006	(-.0147)
Off. age x chg pst 5 yrs	-.0001	(-.0234)	-.0002**	(-.0415)	-.0002**	(-.0365)	-.0002**	(-.0372)
PO viol x has detainers	-.0384	(-.0135)	-.0393*	(-.0161)	-.0404*	(-.0180)	-.0357*	(-.0179)
PO prop x n adl.arrests	.0024	(.0184)	.0035*	(.0310)	.0040*	(.0379)	.0054***	(.0583)
PO prop x prior prop con	-.0034	(-.0270)	-.0055***	(-.0501)	-.0056***	(-.0556)	-.0058***	(-.0649)
PO prop x n juv. arrests	-.0024	(-.0106)	-.0003	(-.0014)	.0001	(.0005)	-.0003	(-.0017)
PO prop x age at 1st arr	.0003	(.0042)	-.0001	(-.0010)	.0001	(.0016)	.0007	(.0130)
PO prop x yrs. 1st incar	.0000	(-.0019)	.0001	(.0024)	.0001	(.0036)	.0001	(.0033)
PO drugs x n adl. convs.	.0031	(.0164)	.0044	(.0269)	.0034	(.0225)	.0032	(.0240)
PO drugs x Part 1 chgs.	-.0078	(-.0272)	-.0106**	(-.0429)	-.0081*	(-.0356)	-.0077*	(-.0383)
PO drugs x last par. rev	-.0498	(-.0178)	-.0507	(-.0211)	-.0427	(-.0193)	-.0267	(-.0136)
Sentence								
Prison	-.0433**	(-.0522)	-.0329*	(-.0461)	-.0297*	(-.0452)	-.0252*	(-.0433)
Youth complex	.0004	(.0004)	.0010	(.0012)	-.0043	(-.0057)	-.0007	(-.0011)
Jail, probation, fine	-.0258	(-.0176)	-.0224	(-.0178)	-.0264	(-.0227)	-.0223	(-.0217)
Jail, probation	-.0044	(.0143)	-.0038	(.0097)	-.0099	(.0005)	-.0014	(.0023)
Jail only	.0017	(.0020)	.0044	(.0058)	-.0059	(-.0084)	-.0100	(-.0161)
Probation w/cond., fine	.0014	(.0016)	-.0108	(-.0148)	-.0079	(-.0117)	-.0082	(-.0137)
Probation, fine	-.0081	(-.0105)	-.0091	(-.0136)	-.0021	(-.0034)	-.0015	(-.0028)
Probation w/conditions	-.0041	(-.0050)	.0003	(.0005)	.0058	(.0088)	.0018	(.0031)
Fined only	.0015	(.0009)	.0080	(.0053)	.0042	(.0030)	-.0012	(-.0010)
Other sanction	-.0058	(-.0022)	-.0004	(-.0002)	.0027	(.0013)	.0129	(.0070)
Dollars fined	.0000	(-.0002)	.0000	(-.0004)	.0000*	(.0191)	.0000**	(.0256)
Months on probation	-.0002	(-.0111)	.0000	(.0022)	.0000	(.0000)	.0000	(-.0017)
Months to jail	.0001	(.0008)	.0001	(-.0078)	.0002	(-.0032)	.0002	(-.0078)
Months to prison	.0000	(-.0087)	-.0001	(-.0211)	-.0001	(-.0193)	-.0001	(-.0219)
First sanction of career	.0262	(.0274)	.0281*	(.0342)	.0221	(.0250)	.0114	(.0129)
Progressive sanction	.0050	(.0074)	.0023	(.0041)	.0031	(.0059)	.0044	(.0093)
Sentence Interactions								
Prison x n adult arrests	.0013	(.0066)	.0028	(.0171)	.0044**	(.0286)	.0025*	(.0186)
Prison x n arrsts as juv	.0008	(.0024)	.0022	(.0078)	.0044*	(.0170)	.0069***	(.0297)
Yth. comp x n adlt convs	-.0083*	(-.0350)	-.0039	(-.0190)	-.0037	(-.0196)	-.0064**	(-.0387)
Yth. comp x chgs in 5 yr	.0074***	(.0397)	.0059***	(.0370)	.0057***	(.0383)	.0045***	(.0342)
Yth. comp x prior n incs	.0118*	(.0277)	.0082	(.0224)	.0034	(.0100)	.0021	(.0071)
Jl & prob x inc lst 2 yr	.0732**	(.0280)	.0472*	(.0210)	.0358*	(.0172)	.0114	(.0062)
Jail x yrs using drugs	-.0013*	(-.0203)	.0001	(.0013)	.0005	(.0096)	.0006	(.0126)
Prb w/cnd, fn x adlt arr	-.0027	(-.0135)	-.0043*	(-.0252)	-.0022	(-.0141)	-.0022	(-.0154)
Prb & fn x Part 1 chgs.	-.0150*	(-.0442)	-.0100*	(-.0342)	-.0026	(-.0097)	-.0022	(-.0092)
Prb w/cnd x adlt chg cnv	-.0044*	(-.0212)	-.0045*	(-.0254)	-.0034*	(-.0208)	-.0036**	(-.0248)
Mths to jail x PO prop.	.0000	(-.0022)	.0000	(-.0127)	.0000	(-.0097)	.0000	(-.0159)
Mths to pris x prop cnvs	.0000	(-.0153)	.0000**	(-.0275)	.0000*	(-.0257)	.0000*	(-.0254)
Init sanc x black	-.0220	(-.0181)	-.0236*	(-.0226)	-.0230*	(-.0239)	-.0136	(-.0159)
Init sanc x n adult arrs	.0085	(.0604)	.0089	(.0735)	.0076	(.0681)	.0048	(.0487)
Constant	.0547***	(.0022)	.0548***	(.0004)	.0599***	(.0008)	.0535***	(-.0029)
R squared	.097		.156		.180		.200	
Adjusted R squared	.189		.149		.173		.194	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.21

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.4852*	(.0209)	1.0635**	(.0270)	1.6988***	(.0323)	1.8904**	(.0258)
Has job after sentence	-.7105**	(-.0319)	-.9017*	(-.0238)	-1.2003*	(-.0238)	-1.8364**	(-.0262)
Offender is on welfare	-.2904	(-.0109)	-.5504	(-.0121)	-.7923	(-.0131)	-1.0217	(-.0121)
Offender is Black	1.1929***	(.0378)	3.8333***	(.0711)	5.8704***	(.0816)	9.0406***	(.1044)
Offender is Hispanic	.7244	(.0169)	2.3375***	(.0322)	3.7807***	(.0390)	7.0352***	(.0522)
Offender is female	-1.1491**	(-.0328)	-2.7013***	(-.0454)	-3.0599***	(-.0385)	-4.7554***	(-.0431)
Lives in urban area	.1550	(.0069)	.7267*	(.0191)	1.5132**	(.0297)	1.9849**	(.0281)
Years at current address	-.0208*	(-.0193)	-.0452**	(-.0247)	-.0545*	(-.0223)	-.0294	(-.0087)
History of drug problems	-.1866	(-.0072)	.2219	(.0050)	.9226	(.0157)	.0975	(.0012)
Treated for drugs/alch.	-.4345	(-.0148)	-.1668	(-.0034)	.4520	(.0068)	1.5675	(.0170)
Has needle marks	.4313	(.0099)	.8991	(.0121)	1.5739	(.0159)	3.1535**	(.0230)
Not a school drop out	-.1859	(-.0081)	-.3444	(-.0089)	-.4625	(-.0089)	-.4922	(-.0068)
Doesn't live with family	-.0356	(-.0013)	-.2509	(-.0055)	-1.2312*	(-.0200)	-1.4980*	(-.0175)
Commited PO with group	-.2037	(-.0091)	.1466	(.0039)	.2098	(.0042)	-.0859	(-.0012)
Victim was a stranger	.0319	(.0013)	.6606	(.0155)	1.0110	(.0177)	1.9396**	(.0245)
Presenting Offense								
PO property crime	.2868	(.0127)	.8014	(.0209)	1.0781	(.0210)	1.6663*	(.0234)
PO crime against person	.1357	(-.0009)	1.0088	(.0121)	1.1438	(.0086)	1.1449	(.0032)
PO drug offense	.2158	(.0084)	.0361	(.0053)	.0231	(.0058)	1.0217	(.0191)
PO Wolfgang severity	.0164	(.0114)	.0297	(.0122)	.0326	(.0100)	.0298	(.0066)
Has detainers at arrest	.0158	(-.0106)	.7552	(-.0075)	1.7002	(-.0013)	1.6269	(-.0071)
Has pending charges	1.2409***	(.0446)	2.1002***	(.0445)	2.7226***	(.0432)	3.8125***	(.0435)
On probation at PO	.5279	(.0082)	.9894	(.0005)	1.2840	(.0002)	1.4984	(.0010)
Anamnestic Theory								
N prior adult arrests	.3918***	(.1410)	1.0876***	(.2350)	1.4762***	(.2330)	1.8898***	(.2455)
N prior adult conviction	-.2501**	(-.0569)	-.4279***	(-.0497)	-.3875*	(-.0292)	-.4597*	(-.0204)
N prior adult chg. conv.	-.0703	(-.0598)	-.3533**	(-.0979)	-.6656***	(-.1248)	-.8152***	(-.1089)
N charges past 5 years	.1719***	(.0906)	.3464***	(.1041)	.4587***	(.0998)	.7620***	(.1119)
N prior Part 1 charges	.5701***	(.0611)	.3502*	(-.0122)	.6756**	(.0388)	.7573**	(.0272)
N prior property conv.	-.0897	(-.0224)	.0163	(.0088)	-.0791	(.0011)	.0235	(-.0007)
N prior persons conv.	.0995	(.0135)	.1471	(.0117)	.2619	(.0157)	.3582	(.0154)
N prior weapons conv.	.4512	(.0141)	1.1058*	(.0203)	.7801	(.0107)	2.2990**	(.0228)
Off street last 2 years	1.0688***	(.0477)	2.7080***	(.0689)	4.0001***	(.0764)	6.1526***	(.0826)
Delinquent Career/Onset								
N arrests as juvenile	.3211***	(.0556)	.6578***	(.0653)	.8356***	(.0582)	1.2444***	(.0605)
N charges as juvenile	-.0120	(.0134)	-.3538	(.0150)	.1031	(.0265)	-.2901	(.0238)
Age at first arrest	.0327	(.0414)	.1533***	(.0635)	.2143***	(.0716)	.2498**	(.0651)
Yrs since first incarc.	.0164	(.0569)	.0554***	(.0797)	.1142***	(.1177)	.1472***	(.1081)
Yrs since first drug use	-.0089	(-.0012)	-.0307*	(-.0144)	-.0588***	(-.0234)	-.0651**	(-.0194)
Prior CJS-Offender Action								
N prior incarcerations	.0986	(.0276)	.2470	(.0317)	.1251	(.0143)	-.0956	(-.0042)
N prior parole revokes	.3901	(.0167)	.2076	(.0052)	.1382	(.0026)	-.6219	(-.0084)
Bad conduct last probat.	.4203	(.0147)	1.1143*	(.0230)	1.0524	(.0163)	1.2905	(.0144)
Recent parole revoked	.9678	(.0208)	.3369	(.0142)	-.0304	(.0117)	1.6038	(.0258)
General Control Variables								
Offender age at sent.	-.0344	(-.1019)	-.1454***	(-.1475)	-.2339***	(-.1709)	-.3357***	(-.1802)
Off. born out of state	-.6518**	(-.0292)	-1.9497***	(-.0515)	-2.6620***	(-.0527)	-3.5991***	(-.0512)
Coder prob. prognosis	-.0060	(-.0151)	-.0202**	(-.0301)	-.0262**	(-.0293)	-.0416***	(-.0334)

*p<.05 **p<.01 ***p<.001

Table 8.21 (continued)

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.6812	(-.0121)	-2.2005**	(-.0231)	-2.8990**	(-.0228)	-3.2300*	(-.0183)
Black x prior adult arrs	-.3302***	(-.0631)	-.5974***	(-.0673)	-.8396***	(-.0708)	-.9531***	(-.0578)
Black x n prior prop cnv	.2535***	(.0504)	.4759***	(.0558)	.6649***	(.0584)	.7413***	(.0468)
Black x n charges as juv	.6777	(.0159)	2.0398**	(.0281)	2.6701**	(.0276)	4.2943***	(.0319)
Female x Part 1 charges	.0375	(.0024)	.0779	(.0030)	.8842	(.0255)	1.0580	(.0219)
Off. age x drug problem	-.0543	(-.0193)	-.1174	(-.0246)	-.2162*	(-.0340)	-.3051*	(-.0345)
Off. age x prior trtment	-.1069*	(-.0338)	-.1991*	(-.0371)	-.2219*	(-.0309)	-.4370**	(-.0438)
Off. age x unemployed	-.0465	(-.0185)	-.0892*	(-.0209)	-.1256*	(-.0221)	-.1591*	(-.0201)
Off. age x PO property	-.0976**	(-.0399)	-.1486**	(-.0359)	-.2352**	(-.0425)	-.3430***	(-.0446)
Off. age x chg pst 5 yrs	-.0063	(-.0263)	-.0157**	(-.0389)	-.0223**	(-.0412)	-.0304**	(-.0405)
PO viol x has detainers	-2.5032**	(-.0221)	-6.7213***	(-.0349)	-9.2824***	(-.0362)	-13.3437***	(-.0374)
PO prop x n adl.arrests	.0738	(.0140)	.2463	(.0275)	.3936*	(.0329)	.8716***	(.0524)
PO prop x prior prop con	-.1895*	(-.0373)	-.4601***	(-.0534)	-.5050**	(-.0439)	-.8680***	(-.0543)
PO prop x n juv. arrests	-.1305	(-.0145)	-.2559	(-.0168)	-.4263*	(-.0210)	-.6123*	(-.0217)
PO prop x age at 1st arr	.0685	(.0230)	.0107	(.0021)	.0568	(.0084)	.1229	(.0131)
PO prop x yrs. 1st incar	.0281*	(.0282)	.0268	(.0158)	.0403	(.0179)	.0482	(.0154)
PO drugs x n adl. convs.	.2864**	(.0378)	.6686***	(.0520)	.7433**	(.0433)	1.0775***	(.0452)
PO drugs x Part 1 chgs.	-.6449***	(-.0563)	-1.4053***	(-.0724)	-1.0385**	(-.0401)	-1.4070**	(-.0390)
PO drugs x last par. rev	.0615	(.0006)	3.4129	(.0180)	5.4604	(.0216)	9.5243*	(.0271)
Sentence								
Prison	-2.6022***	(-.0785)	-3.6152***	(-.0643)	-5.6286***	(-.0750)	-10.2588***	(-.0983)
Youth complex	-.4809	(-.0128)	-.6911	(-.0109)	-2.3943	(-.0282)	-4.3846*	(-.0371)
Jail, probation, fine	-1.2799	(-.0218)	-2.5761*	(-.0259)	-5.7338***	(-.0432)	-7.8190***	(-.0424)
Jail, probation	-.4231	(.0014)	-.8280	(-.0007)	-3.2143*	(-.0191)	-4.3193*	(-.0219)
Jail only	-.0994	(-.0028)	-.1458	(-.0024)	-1.9079	(-.0238)	-3.3352*	(-.0300)
Probation w/cond., fine	-.2372	(-.0070)	-1.4916*	(-.0259)	-2.3829**	(-.0309)	-3.6277**	(-.0339)
Probation, fine	-.8457*	(-.0273)	-1.1171	(-.0212)	-1.1637	(-.0166)	-1.8768	(-.0192)
Probation w/conditions	-.4504	(-.0137)	-.6273	(-.0112)	-.1517	(-.0020)	-.5652	(-.0054)
Fined only	-.4627	(-.0065)	-.1282	(-.0011)	-.8938	(-.0056)	-2.4280	(-.0109)
Other sanction	-.8125	(-.0077)	-1.1010	(-.0062)	-.9322	(-.0039)	.0502	(.0002)
Dollars fined	.0000	(.0030)	.0000	(.0042)	.0003*	(.0191)	.0004*	(.0170)
Months on probation	-.0072	(-.0109)	-.0084	(-.0075)	-.0063	(-.0042)	-.0099	(-.0048)
Months to jail	.0011	(-.0071)	.0012	(-.0096)	.0252	(-.0071)	.0220	(-.0153)
Months to prison	.0000	(-.0001)	-.0081	(-.0209)	-.0158**	(-.0305)	-.0311***	(-.0431)
First sanction of career	1.0353	(.0304)	1.5638	(.0139)	1.3653	(-.0053)	1.4131	(-.0009)
Progressive sanction	.1308	(.0049)	.1562	(.0034)	.5908	(.0098)	.7019	(.0083)
Sentence Interactions								
Prison x n adult arrests	.1377	(.0178)	.2134	(.0162)	.2479	(.0141)	-.0469	(-.0019)
Prison x n arrsts as juv	-.1764	(-.0134)	-.4977**	(-.0223)	-.6176**	(-.0207)	-1.1965***	(-.0289)
Yth. comp x n adlt convs	-.7063***	(-.0747)	-.6012**	(-.0375)	-.7507**	(-.0351)	-.8706*	(-.0292)
Yth. comp x chgs in 5 yr	.2886***	(.0388)	.4436***	(.0351)	.4111*	(.0244)	.1631	(.0070)
Yth. comp x prior n incs	.4228*	(.0248)	.2555	(.0088)	.3927	(.0102)	.2886	(.0054)
Jl & prob x inc lst 2 yr	1.6841	(.0161)	2.6596	(.0150)	3.9928*	(.0169)	3.5870	(.0109)
Jail x yrs using drugs	.0707**	(.0267)	.0932*	(.0208)	.1324*	(.0221)	.1276	(.0153)
Prb w/cnd, fn x adlt arr	-.3212**	(-.0404)	-.6981***	(-.0518)	-.7615***	(-.0423)	-.9353**	(-.0374)
Prb & fn x Part 1 chgs.	-.8330***	(-.0615)	-.8519*	(-.0371)	-.6935	(-.0226)	-.8668	(-.0203)
Prb w/cnd x adlt chg cnv	-.2183*	(-.0263)	-.4111**	(-.0291)	-.4108*	(-.0218)	-.4704	(-.0180)
Mths to jail x PO prop.	-.0011	(-.0094)	-.0024	(-.0120)	-.0044	(-.0163)	-.0087**	(-.0233)
Mths to pris x prop cnvs	.0000	(-.0188)	-.0001*	(-.0234)	-.0001**	(-.0257)	-.0002**	(-.0286)
Init sanc x black	-.6898	(-.0142)	-2.2693**	(-.0275)	-3.7768***	(-.0343)	-3.3835*	(-.0221)
Init sanc x n adult arrs	.3599	(.0639)	.5437	(.0569)	.5287	(.0414)	.9782	(.0551)
Constant	3.6125***	(.0006)	7.1402***	(-.0033)	11.2315***	(-.0023)	17.5381***	(.0131)
R squared	.125		.212		.246		.256	
Adjusted R squared	.118		.206		.240		.250	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.22

Mean Levels of Recidivism by Type of Sentence - Time to Failure Measures Only
(Standard Deviations in Parentheses)

	a) Days to Rearrest				b) Days to Reimprisonment			
	Post-Sentence Observation Window				Post-Sentence Observation Window			
Sentence Type	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Prison	309.2 (105.8)	747.8 (412.8)	1085.5 (728.2)	1738.4 (1463.2)	347.5 (63.9)	933.3 (311.1)	1432.7 (606.2)	2470.0 (1351.8)
Youth complex	273.4 (127.2)	597.9 (434.3)	822.2 (719.6)	1215.4 (1336.4)	338.1 (73.1)	843.8 (356.3)	1241.0 (671.7)	1998.4 (1414.3)
Jail, probation, fine	334.7 (89.6)	927.9 (341.4)	1460.0 (630.3)	2571.6 (1341.5)	365.3 (13.3)	1080.5 (97.2)	1778.4 (228.2)	3317.6 (620.3)
Jail, probation	291.7 (125.0)	707.1 (437.6)	1030.4 (753.6)	1645.1 (1473.1)	360.6 (35.6)	1039.4 (192.8)	1679.0 (403.6)	3023.2 (982.9)
Jail only	299.5 (118.6)	733.0 (429.7)	1085.0 (752.7)	1754.2 (1483.1)	353.0 (56.8)	1006.7 (252.8)	1614.7 (491.2)	2893.7 (1121.3)
Probation w/cond., fine	334.1 (86.4)	898.0 (355.0)	1384.4 (656.4)	2359.6 (1390.7)	364.4 (16.0)	1071.6 (126.2)	1759.4 (276.1)	3291.9 (692.3)
Probation, fine	338.1 (82.1)	923.2 (337.3)	1436.9 (629.4)	2458.8 (1346.2)	364.0 (21.9)	1077.4 (116.4)	1776.6 (244.0)	3322.4 (623.4)
Probation w/conditions	312.7 (105.7)	779.2 (411.0)	1159.5 (732.4)	1862.4 (1455.0)	354.9 (50.6)	1005.5 (248.1)	1610.0 (493.4)	2887.5 (1125.1)
Simple probation	322.0 (98.6)	840.6 (389.5)	1284.9 (706.1)	2152.1 (1447.7)	360.2 (37.2)	1044.9 (189.6)	1698.7 (388.3)	3121.0 (918.6)
Fined only	348.9 (66.8)	984.6 (281.5)	1584.4 (5236.4)	2883.0 (1176.7)	364.7 (20.5)	1083.0 (93.1)	1793.4 (201.1)	3413.0 (477.7)
Other sanction	334.3 (88.3)	916.5 (347.4)	1425.7 (641.8)	2475.0 (1371.8)	363.9 (15.1)	1053.0 (167.7)	1720.2 (353.3)	3183.9 (851.1)
TOTAL	315.3 (105.0)	804.4 (405.2)	1211.4 (725.3)	2005.2 (1471.3)	356.4 (49.7)	1011.7 (238.2)	1622.4 (478.1)	2932.6 (1101.2)

Table 8.23
Explained Variance Attributable to the Sentence for
Days to Rearrest after Sentencing

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.160)	(.253)	(.287)	(.309)
All Sanction Measures	7.77	5.09	4.49	4.48
	(.012)	(.013)	(.013)	(.014)
Sentence Variables	4.18	2.97	2.81	3.21
	(.007)	(.008)	(.008)	(.010)
Place Sentenced	2.53	1.67	1.32	1.15
	(.004)	(.004)	(.004)	(.004)
Time Sentenced	.13	.09	.12	.18
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.00*	.29	.56	.94
	(.000)	(.001)	(.002)	(.003)
Interactions w/Ind. Vars.	3.59	2.12	1.68	1.27
	(.006)	(.005)	(.005)	(.004)

Table 8.24
Explained Variance Attributable to the Sentence for
Days to Reimprisonment after Sentencing

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.094)	(.234)	(.295)	(.337)
All Sanction Measures	9.90	9.93	9.45	8.23
	(.009)	(.023)	(.028)	(.028)
Sentence Variables	6.85	7.08	7.10	6.56
	(.006)	(.017)	(.021)	(.022)
Place Sentenced	3.39	4.23	4.46	4.06
	(.003)	(.010)	(.013)	(.014)
Time Sentenced	.20	.14	.20	.29
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.24	.08	.05	.02
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	3.05	2.85	2.35	1.67
	(.003)	(.007)	(.007)	(.006)

Table 8.25

Regression Coefficients for Days to Post-Sentence Rearrest
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	-5.7604**	(-.0262)	-25.6011**	(-.0302)	-44.8508**	(-.0296)	-87.6415**	(-.0285)
Has job after sentence	7.6332***	(.0363)	28.1854***	(.0347)	45.2628***	(.0311)	77.4605**	(.0263)
Offender is on welfare	5.4206*	(.0215)	15.1976	(.0156)	16.6397	(.0095)	10.1226	(.0029)
Offender is Black	-14.3094***	(-.0511)	-84.0912***	(-.0753)	-179.863***	(-.0907)	-421.271***	(-.1060)
Offender is Hispanic	-12.2681**	(-.0304)	-64.4343***	(-.0413)	-144.512***	(-.0518)	-355.530***	(-.0628)
Offender is female	6.2775	(.0190)	32.2491*	(.0252)	71.2743**	(.0312)	183.5743***	(.0396)
Lives in urban area	-2.5162	(-.0119)	-20.3364**	(-.0248)	-39.6190**	(-.0270)	-84.0940**	(-.0283)
Years at current address	.2511**	(.0247)	1.0058**	(.0256)	1.5298*	(.0218)	2.9265*	(.0205)
History of drug problems	-.3583	(-.0015)	-6.3393	(-.0067)	-17.3192	(-.0102)	-53.7116	(-.0156)
Treated for drugs/alch.	.4787	(.0017)	4.5649	(.0043)	2.6246	(.0014)	-23.5602	(-.0061)
Has needle marks	-2.5377	(-.0062)	-15.1741	(-.0095)	-41.5608	(-.0146)	-121.669*	(-.0211)
Not a school drop out	4.9497*	(.0229)	27.6680***	(.0331)	50.7788***	(.0339)	112.2096***	(.0370)
Doesn't live with family	-1.9995	(-.0078)	-.0324	(.0000)	17.5542	(.0099)	72.3361*	(.0201)
Commited PO with group	5.1169**	(.0248)	19.0200**	(.0234)	36.9399**	(.0254)	69.1027**	(.0234)
Victim was a stranger	-6.5859**	(-.0277)	-27.2317**	(-.0297)	-45.7274**	(-.0278)	-82.8486**	(-.0249)
Presenting Offense								
PO property crime	-3.9312	(-.0184)	-25.9143**	(-.0314)	-50.5715**	(-.0343)	-114.295***	(-.0382)
PO crime against person	2.2668	(.0103)	-6.1881	(-.0016)	-24.2674	(-.0085)	-90.6808*	(-.0195)
PO drug offense	.3241	(-.0045)	-2.2266	(-.0079)	.6959	(-.0053)	-2.1717	(-.0052)
PO Wolfgang severity	.1753	(.0129)	.8409	(.0161)	1.6595	(.0177)	3.3180	(.0174)
Has detainers at arrest	6.2846	(.0179)	17.3754	(.0190)	29.4789	(.0190)	54.7298	(.0187)
Has pending charges	-12.4171***	(-.0473)	-55.9198***	(-.0552)	-107.869***	(-.0594)	-210.874***	(-.0573)
On probation at PO	-8.2812*	(-.0209)	-42.2653***	(-.0231)	-81.3980***	(-.0232)	-177.259***	(-.0213)
Anamnestic Theory								
N prior adult arrests	-2.8450***	(-.1362)	-16.1009***	(-.2154)	-33.8260***	(-.2531)	-73.6554***	(-.2814)
N prior adult conviction	.7247	(.0044)	1.0520	(-.0138)	1.8671	(-.0129)	-2.3290	(-.0252)
N prior adult chg. conv.	.9249	(.0628)	6.1312*	(.0750)	13.0741**	(.0821)	24.4647**	(.0738)
N charges past 5 years	-1.6639***	(-.0841)	-5.4857***	(-.0678)	-8.7581***	(-.0578)	-14.5823**	(-.0450)
N prior Part 1 charges	-2.9547**	(-.0103)	-9.3062**	(-.0154)	-14.0630*	(-.0152)	-19.9126	(-.0125)
N prior property conv.	-.8224	(-.0351)	-3.1248	(-.0386)	-5.7161	(-.0406)	-9.4527	(-.0394)
N prior persons conv.	-1.0095	(-.0145)	-3.9368	(-.0146)	-9.8062	(-.0204)	-25.2468*	(-.0258)
N prior weapons conv.	-5.3456	(-.0177)	-27.0071**	(-.0231)	-45.6809**	(-.0218)	-100.017**	(-.0236)
Off street last 2 years	-10.8546***	(-.0526)	-66.1839***	(-.0791)	-129.298***	(-.0848)	-271.992***	(-.0865)
Delinquent Career/Onset								
N arrests as juvenile	-3.9310***	(-.0579)	-16.1931***	(-.0696)	-27.3663***	(-.0665)	-52.2062***	(-.0623)
N charges as juvenile	-2.6882	(-.0190)	-7.3518	(-.0219)	-17.3995	(-.0206)	-52.4456	(-.0201)
Age at first arrest	-.6608*	(-.0528)	-3.4760***	(-.0624)	-6.6893***	(-.0623)	-11.7954***	(-.0509)
Yrs since first incarc.	-.3424***	(-.0843)	-1.5826***	(-.1020)	-3.0766***	(-.1078)	-6.1873***	(-.1058)
Yrs since first drug use	.1157	(.0118)	.5914*	(.0181)	.9281	(.0162)	1.7243	(.0153)
Prior CJS-Offender Action								
N prior incarcerations	-.2521	(-.0218)	1.9888	(-.0014)	4.8992	(.0054)	24.9799*	(.0315)
N prior parole revokes	-4.0468	(-.0183)	-3.3401	(-.0039)	-6.8181	(-.0045)	-3.6928	(-.0012)
Bad conduct last probat.	-4.5646	(-.0169)	-23.9050*	(-.0230)	-34.4526*	(-.0185)	-29.1754	(-.0077)
Recent parole revoked	-7.5052	(-.0298)	-16.1209	(-.0217)	-5.5951	(-.0145)	-4.1298	(-.0109)
General Control Variables								
Offender age at sent.	.7041**	(.1347)	3.7628***	(.1699)	8.6257***	(.1889)	19.6570***	(.1931)
Off. born out of state	9.4424***	(.0448)	50.9233***	(.0626)	105.6240***	(.0726)	254.7873***	(.0863)
Coder prob. prognosis	.0974*	(.0261)	.4999***	(.0347)	.9799***	(.0380)	2.0314***	(.0388)

*p<.05 **p<.01 ***p<.001

Table 8.25 (continued)

Regression Coefficients for Days to Post-Sentence Rearrest
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	6.3470	(.0120)	43.0888*	(.0211)	89.7932**	(.0245)	225.5667***	(.0304)
Black x prior adult arrs	2.3055**	(.0466)	9.5938***	(.0503)	17.8997***	(.0524)	38.5805***	(.0557)
Black x n prior prop cnv	-2.1834***	(-.0460)	-7.4748**	(-.0408)	-11.8814**	(-.0362)	-25.2940**	(-.0380)
Black x n charges as juv	-2.5439	(-.0063)	-21.8018	(-.0140)	-25.2797	(-.0091)	-8.7826	(-.0016)
Female x Part 1 charges	-2.4719	(-.0171)	-15.2253*	(-.0273)	-30.5130*	(-.0305)	-60.3678*	(-.0298)
Off. age x drug problem	.8428*	(.0318)	3.3724*	(.0329)	5.1260	(.0280)	6.6314	(.0178)
Off. age x prior trtment	.9689*	(.0324)	5.0509**	(.0438)	9.5480**	(.0462)	20.0638***	(.0479)
Off. age x unemployed	.5124*	(.0216)	2.2752**	(.0249)	3.6598*	(.0223)	5.5304	(.0166)
Off. age x PO property	.6652*	(.0288)	2.8862*	(.0324)	4.8699*	(.0305)	9.7670*	(.0302)
Off. age x chg pst 5 yrs	.0158	(.0070)	-.0112	(-.0013)	-.0422	(-.0027)	-.3324	(-.0105)
PO viol x has detainers	6.7346	(.0063)	70.2328*	(.0170)	134.4281*	(.0182)	289.4797**	(.0193)
PO prop x n adl.arrests	-.5646	(-.0113)	-2.4313	(-.0126)	-2.9702	(-.0086)	-4.9296	(-.0070)
PO prop x prior prop con	2.3345**	(.0487)	7.1010**	(.0384)	10.5683*	(.0319)	18.4367*	(.0274)
PO prop x n juv. arrests	3.1665***	(.0373)	11.4056***	(.0348)	18.9893***	(.0324)	38.1348***	(.0321)
PO prop x age at 1st arr	-.1707	(-.0061)	.3656	(.0034)	1.8066	(.0093)	4.7480	(.0120)
PO prop x yrs. 1st incar	-.1173	(-.0125)	-.5848	(-.0161)	-.9087	(-.0140)	-1.6548	(-.0125)
PO drugs x n adl. convs.	-2.5166*	(-.0352)	-11.3429**	(-.0411)	-19.4652**	(-.0394)	-35.6821**	(-.0356)
PO drugs x Part 1 chgs.	7.4716***	(.0691)	21.5852***	(.0517)	32.3357**	(.0433)	52.1387*	(.0344)
PO drugs x last par. rev	-24.6783*	(-.0234)	-90.5912*	(-.0223)	-166.819*	(-.0229)	-273.374	(-.0185)
Sentence								
Prison	30.2260***	(.0966)	114.3899***	(.0947)	177.2196***	(.0820)	310.8157***	(.0709)
Youth complex	8.1789	(.0231)	19.4403	(.0142)	21.4861	(.0088)	28.8071	(.0058)
Jail, probation, fine	8.5160	(.0154)	71.1421**	(.0333)	138.3757**	(.0362)	325.8654***	(.0420)
Jail, probation	-4.1879	(-.0239)	-6.1012	(-.0161)	-20.6817	(-.0166)	-33.5682	(-.0119)
Jail only	6.7549	(.0202)	41.4364*	(.0322)	80.9665*	(.0351)	170.9955*	(.0366)
Probation w/cond., fine	7.4005	(.0230)	27.1555	(.0219)	31.7939	(.0143)	38.7604	(.0086)
Probation, fine	7.0012	(.0239)	27.7123*	(.0245)	38.7926	(.0192)	45.8230	(.0112)
Probation w/conditions	2.7963	(.0090)	-2.3125	(-.0019)	-10.2941	(-.0048)	-36.8164	(-.0084)
Fined only	2.9322	(.0044)	20.7415	(.0081)	52.1157	(.0113)	168.2055*	(.0180)
Other sanction	13.5475	(.0137)	70.8471*	(.0185)	120.8215*	(.0176)	252.0733*	(.0181)
Dollars fined	-.0002	(-.0028)	-.0002	(-.0007)	.0005	(.0011)	.0018	(.0021)
Months on probation	.0343	(.0055)	.2407	(.0100)	.3950	(.0083)	.3285	(.0038)
Months to jail	.0321	(.0162)	-.7787	(.0106)	-1.6079	(.0109)	-3.5668	(.0089)
Months to prison	.0289	(.0134)	.1379	(.0165)	.3256*	(.0218)	.9335**	(.0308)
First sanction of career	-14.8285*	(-.0511)	-52.4771*	(-.0369)	-87.9863*	(-.0291)	-157.646*	(-.0215)
Progressive sanction	-.3628	(-.0014)	-3.1647	(-.0032)	-7.1222	(-.0041)	-5.5871	(-.0016)
Sentence Interactions								
Prison x n adult arrests	-.5833	(-.0080)	-3.4081	(-.0121)	-4.5415	(-.0090)	-4.9627	(-.0048)
Prison x n arrsts as juv	1.7482	(.0141)	2.6615	(.0056)	3.6332	(.0042)	2.1765	(.0013)
Yth. comp x n adlt convs	3.6621**	(.0410)	4.1616	(.0121)	1.2310	(.0020)	-5.3883	(-.0043)
Yth. comp x chgs in 5 yr	-.9038	(-.0129)	.3444	(.0013)	4.5216	(.0093)	14.7939	(.0150)
Yth. comp x prior n incs	-8.1125***	(-.0503)	-23.0750**	(-.0371)	-31.8875*	(-.0286)	-40.0376	(-.0177)
Jl & prob x inc lst 2 yr	-22.3908**	(-.0227)	-74.9949*	(-.0197)	-105.511	(-.0155)	-141.233	(-.0102)
Jail x yrs using drugs	-.2100	(-.0084)	-.3912	(-.0041)	-.4569	(-.0027)	-.3328	(-.0010)
Prb w/cnd, fn x adlt arr	3.2177***	(.0429)	13.2942***	(.0459)	19.7065***	(.0380)	19.9870	(.0190)
Prb & fn x Part 1 chgs.	6.6553**	(.0520)	21.3438**	(.0432)	33.7749*	(.0382)	42.8363	(.0239)
Prb w/cnd x adlt chg cnv	2.9241***	(.0372)	8.6261**	(.0285)	13.9424**	(.0257)	26.3965**	(.0240)
Mths to jail x PO prop.	.0192	(.0172)	.1190**	(.0275)	.2333***	(.0301)	.4610***	(.0294)
Mths to pris x prop cnvs	.0004*	(.0251)	.0014*	(.0219)	.0021	(.0184)	.0027	(.0121)
Init sanc x black	7.2972	(.0159)	45.7589**	(.0258)	96.3198**	(.0304)	203.2024***	(.0316)
Init sanc x n adult arrs	-5.1941*	(-.0976)	-28.3285***	(-.1380)	-57.1556***	(-.1555)	-129.721***	(-.1740)
Constant	315.5859***	(-.0276)	823.3125***	(-.0426)	1255.599***	(-.0544)	2097.784***	(-.0710)
R squared	.160		.253		.287		.309	
Adjusted R squared	.153		.247		.281		.304	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.26

Regression Coefficients for Days to Post-Sentence Reimprisonment
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	1.3688	(.0140)	-6.7654	(-.0136)	-21.4649*	(-.0215)	-55.1117**	(-.0239)
Has job after sentence	3.6271***	(.0387)	19.4882***	(.0408)	34.9934***	(.0365)	75.3886***	(.0341)
Offender is on welfare	2.6095*	(.0232)	16.2720**	(.0284)	36.3356***	(.0316)	79.7921***	(.0301)
Offender is Black	-.6772	(.0083)	-22.5312***	(-.0235)	-64.5793***	(-.0405)	-201.535***	(-.0641)
Offender is Hispanic	-1.8067	(-.0101)	-18.0788*	(-.0197)	-43.2867**	(-.0235)	-135.478***	(-.0320)
Offender is female	-.6271	(-.0043)	-3.2637	(-.0043)	-2.2440	(-.0015)	28.9770	(.0083)
Lives in urban area	.9855	(.0104)	5.8520	(.0122)	10.7752	(.0112)	1.1963	(.0005)
Years at current address	.0042	(.0009)	.1349	(.0058)	.4594	(.0099)	.9415	(.0088)
History of drug problems	-3.6677**	(-.0336)	-11.6644	(-.0209)	-27.4660*	(-.0246)	-78.5134**	(-.0305)
Treated for drugs/alch.	-.8836	(-.0072)	-10.1743	(-.0162)	-21.3807	(-.0170)	-36.6258	(-.0126)
Has needle marks	.4112	(.0022)	-6.7614	(-.0072)	-25.4914	(-.0136)	-86.7726*	(-.0201)
Not a school drop out	-.0965	(-.0010)	2.4294	(.0049)	9.3266	(.0095)	23.4297	(.0103)
Doesn't live with family	-.6815	(-.0060)	-1.8932	(-.0033)	-4.0543	(-.0035)	.0910	(.0000)
Commited PO with group	.9518	(.0102)	7.3600	(.0154)	9.5846	(.0100)	18.7234	(.0085)
Victim was a stranger	-1.9666	(-.0186)	-13.8772**	(-.0257)	-26.9292**	(-.0249)	-69.9467**	(-.0280)
Presenting Offense								
PO property crime	.0953	(.0010)	-11.1388*	(-.0230)	-24.4045*	(-.0251)	-79.2521**	(-.0354)
PO crime against person	.3970	(.0065)	-1.5476	(-.0002)	-7.5176	(-.0043)	-41.7479	(-.0135)
PO drug offense	2.6209	(.0250)	9.0045	(.0164)	20.4278	(.0162)	25.5222	(.0057)
PO Wolfgang severity	-.0502	(-.0083)	-.2684	(-.0087)	-.5670	(-.0092)	-.5558	(-.0039)
Has detainers at arrest	-4.3558*	(-.0171)	-23.9558*	(-.0203)	-40.5362*	(-.0172)	-71.6312	(-.0130)
Has pending charges	-3.2328**	(-.0277)	-27.4878***	(-.0461)	-61.3945***	(-.0513)	-145.648***	(-.0529)
On probation at PO	-2.1224	(-.0024)	-10.6217	(-.0013)	-31.0328*	(-.0067)	-80.3245**	(-.0137)
Anamnestic Theory								
N prior adult arrests	-.0111	(.0216)	-1.5004	(-.0387)	-5.0430	(-.0534)	-15.4580*	(-.0619)
N prior adult conviction	.8357*	(.0524)	3.4651*	(.0331)	4.8684	(.0169)	7.3451	(.0077)
N prior adult chg. conv.	-.9931**	(-.0710)	-2.9473*	(-.0301)	-2.9578	(-.0046)	-.4285	(.0179)
N charges past 5 years	-.7177***	(-.0837)	-3.8363***	(-.0913)	-6.8308***	(-.0810)	-15.0467***	(-.0751)
N prior Part 1 charges	-.0180	(.0414)	-5.0604*	(-.0060)	-9.8954**	(-.0056)	-20.2031*	(-.0102)
N prior property conv.	-.0997	(-.0426)	-.0540	(-.0350)	-2.2626	(-.0461)	-10.3652	(-.0511)
N prior persons conv.	-.8635*	(-.0278)	-2.0111	(-.0127)	-6.1078	(-.0192)	-20.6419*	(-.0282)
N prior weapons conv.	.4390	(.0033)	-3.1904	(-.0046)	-6.1612	(-.0045)	-10.3899	(-.0033)
Off street last 2 years	-3.4475**	(-.0333)	-33.1084***	(-.0657)	-83.5840***	(-.0827)	-209.476***	(-.0906)
Delinquent Career/Onset								
N arrests as juvenile	-.5562	(-.0373)	-1.7901	(-.0416)	-4.2240	(-.0451)	-12.3607	(-.0450)
N charges as juvenile	.1581	(.0041)	-4.9853	(-.0240)	-11.4967	(-.0336)	-20.9892	(-.0392)
Age at first arrest	-.1660	(-.0491)	-1.2177*	(-.0708)	-3.4669**	(-.0865)	-9.4920***	(-.0928)
Yrs since first incarc.	-.3025***	(-.1635)	-1.5654***	(-.1902)	-3.5191***	(-.2080)	-9.0456***	(-.2259)
Yrs since first drug use	-.0166	(-.0063)	.0486	(-.0003)	.1940	(.0024)	.9492	(.0088)
Prior CJS-Offender Action								
N prior incarcerations	-.1752	(-.0173)	.0438	(-.0205)	-.2150	(-.0219)	-.9280	(-.0199)
N prior parole revokes	-1.7280	(-.0176)	-6.1402	(-.0122)	-12.5704	(-.0125)	-21.7160	(-.0094)
Bad conduct last probat.	-1.3145	(-.0110)	-8.2398	(-.0135)	-18.3804	(-.0150)	-35.2745	(-.0125)
Recent parole revoked	-10.6548***	(-.0509)	-63.2770***	(-.0616)	-101.849***	(-.0544)	-153.305**	(-.0418)
General Control Variables								
Offender age at sent.	-.0753	(.0246)	.0775	(.0854)	.9599	(.1095)	4.0726	(.1282)
Off. born out of state	-.7847	(-.0084)	.4814	(.0010)	6.0936	(.0064)	40.5227*	(.0183)
Coder prob. prognosis	.0319	(.0192)	.2369**	(.0280)	.5391**	(.0317)	1.4076***	(.0359)

*p<.05 **p<.01 ***p<.001

Table 8.26 (continued)

Regression Coefficients for Days to Post-Sentence Reimprisonment
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	4.2043	(.0178)	22.4532*	(.0187)	52.6270**	(.0218)	97.3449*	(.0175)
Black x prior adult arrs	1.3298***	(.0605)	7.6172***	(.0679)	14.5373***	(.0646)	32.6526***	(.0630)
Black x n prior prop cnv	-1.1760***	(-.0557)	-6.1549***	(-.0571)	-10.7599***	(-.0498)	-19.0606**	(-.0383)
Black x n charges as juv	.4766	(.0027)	-13.5106	(-.0148)	-43.6154**	(-.0237)	-139.775***	(-.0330)
Female x Part 1 charges	.1253	(.0019)	-3.0418	(-.0093)	-9.1779	(-.0139)	-27.1514	(-.0179)
Off. age x drug problem	.0448	(.0038)	.8353	(.0139)	1.9970	(.0165)	2.5989	(.0093)
Off. age x prior trtment	.2938	(.0221)	3.8081***	(.0561)	8.1175***	(.0596)	19.8170***	(.0632)
Off. age x unemployed	.1444	(.0137)	1.0018*	(.0186)	2.4471**	(.0227)	7.1369***	(.0287)
Off. age x PO property	.2124	(.0207)	2.2141**	(.0423)	4.7522***	(.0452)	11.2381***	(.0464)
Off. age x chg pst 5 yrs	-.0196	(-.0196)	.0124	(.0024)	.0804	(.0078)	.3137	(.0133)
PO viol x has detainers	5.5959	(.0117)	21.8810	(.0090)	36.4203	(.0075)	68.7721	(.0061)
PO prop x n adl.arrests	-.1473	(-.0066)	-1.8004	(-.0159)	-4.1696	(-.0183)	-13.8130	(-.0264)
PO prop x prior prop con	.4132	(.0194)	2.1625	(.0199)	4.9870	(.0228)	14.9578*	(.0298)
PO prop x n juv. arrests	-.5466	(-.0145)	-4.6136*	(-.0240)	-9.0699*	(-.0235)	-15.4655	(-.0174)
PO prop x age at 1st arr	-.3334	(-.0266)	-2.4577**	(-.0385)	-4.8596**	(-.0379)	-9.7137*	(-.0329)
PO prop x yrs. 1st incar	-.0835	(-.0199)	-1.0563***	(-.0495)	-2.1195***	(-.0494)	-4.6986***	(-.0476)
PO drugs x n adl. convs.	-.5483	(-.0172)	-5.0590*	(-.0312)	-10.7663*	(-.0330)	-20.7883*	(-.0277)
PO drugs x Part 1 chgs.	2.2648**	(.0471)	10.2551**	(.0418)	20.6079**	(.0419)	36.2503*	(.0320)
PO drugs x last par. rev	2.2947	(.0049)	3.6356	(.0015)	-37.3374	(-.0078)	-182.124	(-.0165)
Sentence								
Prison	6.6059*	(.0474)	24.2066*	(.0341)	31.3088	(.0220)	57.0374	(.0174)
Youth complex	.9870	(.0063)	-41.4332***	(-.0516)	-107.560***	(-.0667)	-258.867***	(-.0697)
Jail, probation, fine	12.6528***	(.0514)	74.8083***	(.0596)	168.8584***	(.0670)	411.1688***	(.0708)
Jail, probation	13.0483***	(.0661)	83.9231***	(.0758)	189.8880***	(.0845)	438.2750***	(.0823)
Jail only	6.8343**	(.0460)	52.1818***	(.0689)	121.7137***	(.0801)	299.6637***	(.0856)
Probation w/cond., fine	3.5963*	(.0252)	15.7761	(.0217)	33.0812*	(.0226)	91.3333*	(.0271)
Probation, fine	1.9284	(.0148)	14.6747	(.0221)	32.9380*	(.0247)	69.2729	(.0226)
Probation w/conditions	.0742	(.0005)	-3.0417	(-.0043)	-7.7346	(-.0055)	-36.3634	(-.0111)
Fined only	-.9395	(-.0032)	-12.0250	(-.0079)	-24.9411	(-.0082)	-25.3462	(-.0036)
Other sanction	5.5342	(.0125)	7.5058	(.0033)	11.5525	(.0026)	28.6430	(.0028)
Dollars fined	.0000	(.0008)	-.0003	(-.0023)	-.0010	(-.0034)	-.0058	(-.0088)
Months on probation	-.0375	(-.0135)	-.4015*	(-.0284)	-.9649**	(-.0340)	-2.1233**	(-.0324)
Months to jail	-.0334	(.0061)	-.5782	(.0023)	-1.8494	(-.0052)	-4.7445*	(-.0056)
Months to prison	.0165	(.0172)	.0716	(.0146)	.2051	(.0209)	.7261**	(.0320)
First sanction of career	-5.1588	(-.0421)	-39.8239**	(-.0582)	-67.9078**	(-.0446)	-114.101*	(-.0238)
Progressive sanction	-.4803	(-.0043)	-4.2326	(-.0074)	-11.5936	(-.0101)	-25.1047	(-.0095)
Sentence Interactions								
Prison x n adult arrests	-.5679	(-.0175)	-5.7716***	(-.0348)	-12.1507***	(-.0365)	-24.2668***	(-.0316)
Prison x n arrsts as juv	.6799	(.0123)	-2.0640	(-.0073)	-4.9386	(-.0087)	-7.7611	(-.0060)
Yth. comp x n adlt convs	.7391	(.0186)	9.7521***	(.0481)	19.0399***	(.0468)	39.7152***	(.0424)
Yth. comp x chgs in 5 yr	-.5955	(-.0191)	-4.9099**	(-.0308)	-8.7828**	(-.0275)	-13.9670*	(-.0190)
Yth. comp x prior n incs	-1.9827*	(-.0276)	-23.2135***	(-.0635)	-46.6128***	(-.0635)	-92.6337***	(-.0548)
Jl & prob x inc lst 2 yr	.4896	(.0011)	-23.4089	(-.0104)	-59.1038	(-.0131)	-174.406*	(-.0168)
Jail x yrs using drugs	-.0495	(-.0045)	-.4817	(-.0085)	-.9635	(-.0085)	-2.0290	(-.0078)
Prb w/cnd, fn x adlt arr	.8826*	(.0264)	4.6991*	(.0276)	10.1501**	(.0297)	21.7723**	(.0277)
Prb & fn x Part 1 chgs.	2.0835*	(.0366)	15.6885***	(.0541)	32.4649***	(.0557)	65.1155**	(.0485)
Prb w/cnd x adlt chg cnv	.6452	(.0185)	1.4415	(.0081)	4.1456	(.0116)	11.6188	(.0141)
Mths to jail x PO prop.	.0064	(.0128)	.0542*	(.0213)	.1191**	(.0233)	.3090**	(.0263)
Mths to pris x prop cnvs	.0002*	(.0223)	.0009*	(.0257)	.0019**	(.0254)	.0046**	(.0269)
Init sanc x black	2.0323	(.0100)	22.2333*	(.0213)	49.3110*	(.0236)	130.6353**	(.0271)
Init sanc x n adult arrs	-1.0124	(-.0428)	-10.0568*	(-.0833)	-17.1642*	(-.0708)	-28.2470	(-.0506)
Constant	354.7565***	(-.0048)	1034.044***	(.0022)	1688.915***	(.0099)	3127.466***	(.0128)
R squared	.094		.234		.295		.337	
Adjusted R squared	.087		.228		.289		.331	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 8.27
Summary of Effects Observed Using a One Year Window

Independent Variable	Dependent Variables: Binomial 1	2	3	4	Criminal Career 5	6	7	8	9	10	11	Time 12	13
Sentence													
Prison	-	-	-		-	-	-	-	-	-	-	+	+
Youth complex	-					-							
Jail, probation, fine	-			-	-	-		-	-				+
Jail, probation				-				-	-				+
Jail only				-				-	-				+
Probation w/cond., fine						-		-					+
Probation, fine					-	-		-	-		-		
Probation w/conditions													
Fined only													
Other sanction	-												
Dollars fined													
Months on probation													
Months to jail													
Months to prison						-							
First sanction of career				+	+	+		+	+			-	
Progressive sanction													
Sentence Interactions													
Prison x n adult arrests	+				+	+		+	+				
Prison x n arrsts as juv			+			-							
Yth. comp x n adlt convs	-		-		-	-	-	-	-	-	-	+	
Yth. comp x chgs in 5 yr					+	+	+		+	+	+		
Yth. comp x prior n incs	+				+	+		+	+	+	+	-	
Jl & prob x inc lst 2 yr		+			+	+	+	+	+	+		-	-
Jail x yrs using drugs		-				+				-	+		
Prb w/cnd, fn x adlt arr	-		-		-	-		-	-		-	+	
Prb & fn x Part 1 chgs.	-			-	-	-	-	-	-	-	-	+	+
Prb w/cnd x adlt chg cnv	-				-	-	-	-	-	-	-	+	+
Mths to jail x PO prop.	-				-	-		-	-				
Mths to pris x prop cnvs					+	+		+	+			+	
Init sanc x black								-	-				+
Init sanc x n adult arrs	+	+	+	+	+	+		+	+			-	

Legend:

1) Probability of Rearrest
2) Probability of Rearrest for Persons Crime
3) Probability of Repeating Presenting Offense
4) Probability of Reimprisonment
5) Log of Total Post-Sentence Charges
6) Log of Total Post-Sentence Convictions
7) Log of Total Post-Sentence Persons Charges
8) Log of Adjusted Post-Sentence Arrest Rate
9) Log of Adjusted Post-Sentence Charge Rate
10) Log of Adjusted Post-Sentence Persons Charge Rate
11) Summed Seriousness of All Post-Sentence Charges
12) Days to Rearrest after Sentencing
13) Days to Reimprisonment after Sentencing

Table 8.28
Summary of Effects Observed Using a Three Year Window

Independent Variable	Dependent Variables: Binomial 1	2	3	4	Criminal Career 5	6	7	8	9	10	11	Time 12	13
Sentence													
Prison	-		-		-	-	-	-	-	-	-	+	+
Youth complex				+									-
Jail, probation, fine	-		-	-	-	-		-	-		-	+	+
Jail, probation				-		-		-	-				+
Jail only				-	-	-		-	-			+	+
Probation w/cond., fine					-	-		-	-		-		
Probation, fine			-		-	-		-	-			+	
Probation w/conditions													
Fined only													
Other sanction												+	
Dollars fined													
Months on probation				+									-
Months to jail													
Months to prison	-					-		-					
First sanction of career			+		+	+		+	+	+		+	-
Progressive sanction													
Sentence Interactions													
Prison x n adult arrests						+		+	+				-
Prison x n arrsts as juv					-	-					-		
Yth. comp x n adlt convs			-	-				-	-		-		+
Yth. comp x chgs in 5 yr									+	+	+		-
Yth. comp x prior n incs				+			+	+	+			-	-
Jl & prob x inc lst 2 yr		+		+	+	+	+	+	+	+		-	
Jail x yrs using drugs											+		
Prb w/cnd, fn x adlt arr	-				-	-	-	-	-	-	-	+	+
Prb & fn x Part 1 chgs.					-	-		-	-	-	-	+	+
Prb w/cnd x adlt chg cnv	-	-		-	-	-	-	-	-	-	-	+	
Mths to jail x PO prop.	-		-		-	-		-	-			+	+
Mths to pris x prop cnvs					+	+		+	+	+	-	+	+
Init sanc x black					-	-	-	-	-	-	-	+	+
Init sanc x n adult arrs	+	+	+	+	+	+		+	+			-	-

Legend:

1) Probability of Rearrest
2) Probability of Rearrest for Persons Crime
3) Probability of Repeating Presenting Offense
4) Probability of Reimprisonment
5) Log of Total Post-Sentence Charges
6) Log of Total Post-Sentence Convictions
7) Log of Total Post-Sentence Persons Charges
8) Log of Adjusted Post-Sentence Arrest Rate
9) Log of Adjusted Post-Sentence Charge Rate
10) Log of Adjusted Post-Sentence Persons Charge Rate
11) Summed Seriousness of All Post-Sentence Charges
12) Days to Rearrest after Sentencing
13) Days to Reimprisonment after Sentencing

Table 8.29
Summary of Effects Observed Using a Five Year Window

Independent Variable	Dependent Variables: Binomial 1	2	3	4	Criminal Career 5	6	7	8	9	10	11	Time 12	13
Sentence													
Prison	-		-		-	-	-	-	-	-	-	+	
Youth complex				+									-
Jail, probation, fine	-		-	-	-	-	-	-	-		-	+	+
Jail, probation				-	-	-		-	-		-		+
Jail only	-			-	-	-		-	-			+	+
Probation w/cond., fine					-	-		-	-		-		+
Probation, fine			-		-	-		-	-				+
Probation w/conditions													
Fined only													
Other sanction												+	
Dollars fined							+			+	+		
Months on probation				+			-						-
Months to jail													
Months to prison	-		-	-	-	-		-			-	+	
First sanction of career					+	+		+	+			-	-
Progressive sanction													
Sentence Interactions													
Prison x n adult arrests						+		+	+	+			-
Prison x n arrsts as juv						-				+	-		
Yth. comp x n adlt convs			-	-				-	-		-		+
Yth. comp x chgs in 5 yr								+	+	+	+		-
Yth. comp x prior n incs				+		+		+	+			-	-
Jail & prob x inc lst 2 yr		+			+	+	+	+	+	+	+		
Jail x yrs using drugs						+					+		
Prb w/cnd, fn x adlt arr					-	-		-	-		-	+	+
Prb & fn x Part 1 chgs.					-	-		-	-			+	+
Prb w/cnd x adlt chg cnv	-			-	-	-	-	-	-	-	-	+	
Mths to jail x PO prop.	-		-		-		-	-	-			+	+
Mths to pris x prop cnvs					+	+		+	+	+	-		+
Init sanc x black					-	-	-	-	-	-	-	+	+
Init sanc x n adult arrs	+	+	+	+	+	+	+		+			-	-

Legend:

1) Probability of Rearrest
2) Probability of Rearrest for Persons Crime
3) Probability of Repeating Presenting Offense
4) Probability of Reimprisonment
5) Log of Total Post-Sentence Charges
6) Log of Total Post-Sentence Convictions
7) Log of Total Post-Sentence Persons Charges
8) Log of Adjusted Post-Sentence Arrest Rate
9) Log of Adjusted Post-Sentence Charge Rate
10) Log of Adjusted Post-Sentence Persons Charge Rate
11) Summed Seriousness of All Post-Sentence Charges
12) Days to Rearrest after Sentencing
13) Days to Reimprisonment after Sentencing

Table 8.30
Summary of Effects Observed Using a Nine Year Window

Independent Variable	Dependent Variables: Binomial 1	2	3	4	Criminal Career 5	6	7	8	9	10	11	Time 12	13
Sentence													
Prison	-	-	-		-	-	-	-	-	-	-	+	
Youth complex			-			-					-		-
Jail, probation, fine	-	-	-	-	-	-	-	-	-		-	+	+
Jail, probation				-	-	-		-	-		-		+
Jail only	-		-	-	-	-		-	-		-	+	+
Probation w/cond., fine				-	-	-		-	-		-		+
Probation, fine			-			-		-	-				
Probation w/conditions													
Fined only	-											+	
Other sanction												+	
Dollars fined							+			+	+		
Months on probation				+									-
Months to jail													-
Months to prison	-	-	-	-	-	-	-	-	-		-	+	+
First sanction of career						+			+			-	-
Progressive sanction													
Sentence Interactions													
Prison x n adult arrests								+	+	+			-
Prison x n arrsts as juv					-	-	-			+	-		
Yth. comp x n adlt convs			-	-				-	-	-	-		+
Yth. comp x chgs in 5 yr								+	+	+			-
Yth. comp x prior n incs				+		+		+					-
Jl & prob x inc lst 2 yr					+	+		+	+				-
Jail x yrs using drugs													
Prb w/cnd, fn x adlt arr						-		-	-		-		+
Prb & fn x Part 1 chgs.						-		-	-				+
Prb w/cnd x adlt chg cnv	-			-	-	-	-	-	-	-		+	
Mths to jail x PO prop.	-	-	-		-	-	-	-	-		-	+	+
Mths to pris x prop cnvs					+	+		+	+	+	-		+
Init senc x black					-	-		-	-		-	+	+
Init senc x n adult arrs	+	+	+	+	+	+			+			-	

Legend:

1) Probability of Rearrest
2) Probability of Rearrest for Persons Crime
3) Probability of Repeating Presenting Offense
4) Probability of Reimprisonment
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SENTENCING AND RECIDIVISM

Volume Three (of Three)

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*Views in this document are those of the authors and do not necessarily represent the official position of the U.S. Department of Justice, The National Institute of Justice, or the New Jersey Administrative Office of the Courts.

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**This report represents a truly collaborative effort and we have listed the authors in alphabetical order.

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Vol. III

CHAPTER NINE

THE PREDICTION OF RECIDIVISM FOR INDIVIDUALS

The decisions made throughout the criminal justice system are often characterized by binary choices. CJS agents must decide whether or not to arrest a suspect, release an inmate to parole, forward a case for prosecution, or to revoke probation or not. The process of sentencing may also be seen as a series of "yes" or "no" decisions centered around whether or not to incarcerate, if incarcerated, to jail or prison, and additional considerations as to conditions on probation, participation in various programs, sentence length and so forth.

These decisions do not exist in a vacuum as general policy directives can help shape them. Police "crackdowns" can make a decision to arrest more likely. Limited jail and prison space can make parole release more frequent. Mandatory sentencing policies can influence that likelihood that prosecutors charge for a certain offense. Both CJS policy and the philosophies guiding sentencing can influence the choices made by judges. Judges' sentences may reflect the influence of a single goal behind a sentence, equal weight given to multiple goals, or disproportionate weight given to some goals. Legislated determinate sentencing practices influence sentencing decisions, as do grid sentencing systems. Considerations of efficiency reduce that chances that a sentence will involve the decision to incarcerate.

Yet while policy may help fashion decisions, it can only guide them. On a day-to-day basis, CJS agents must continually make concrete decisions about <u>individuals</u>. Effective operation of the CJS requires that these repeated decisions about individuals be made accurately and with considerations of

fairness and justice. A sentence to probation (or a release to parole) given to an individual who subsequently commits a violent crime was, in hindsight, an inaccurate decision. The incarceration of an offender who poses little risk to the community (either by virtue of prior behavior or severity of the presenting offense) represents a tragic decision for that individual.

As the examples above suggest, a (sometimes implicit) factor in these determinations is the expectation for the prospective behavior of the individual. Discrete decisions are often made under that assumption that the offender poses an unacceptably high risk according to some criterion. To the extent that judges incorporate utility goals at sentencing, the recidivism prospects of the individual brought before the judge are relevant. As we saw in the previous chapter, expectations for recidivism appear to be related to the type of sentence received by the individual.

We thus return to some of the models used in earlier chapters, and assess their ability to predict subsequent criminal behavior at the individual level.[1] The relevance of "individual prediction" to sentencing has been discussed in earlier chapters: predictions of subsequent criminal behavior are part of what judges decide when they sentence. We suggest, following numerous others, that predicting recidivism at the individual level may be

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[1] The claim that, with the exceptions of RIOC and the percent correctly predicted, we have not yet addressed prediction at the individual level in earlier chapters may be puzzling to some. Assessments have been made of the extent to which the various models have predicted several measures of recidivism in the aggregate. The "explained variance" concept of regression analysis was used to compare, for example, the relative abilities of models to predict rearrest and rearrest for persons crimes, as well as to compare the relative contributions of particular types of independent variables. Some dependent variables were "better explained" than others, and some types of independent variables contributed uniquely to the explanation of recidivism more so than others. However, at the individual level, the predictive of success or failure these models is only poorly summarized by such statistics.

made less capricious by the use of statistical or actuarial predictions. In this chapter we attempt to answer the question of how well judges would do if they used the equation(s) developed thus far in our research.

Doing so raises several general issues pertaining to individual prediction that are taken up in this chapter. We will be discussing, for example, the proportion of "correct" predictions, of "false positives" (offenders predicted to fail who do not) and "false negatives" (offenders predicted to succeed, but who fail). As well, two statistics, RIOC (Relative Improvement Over Chance) and a traditionally used measure of association, phi, that summarize the overall accuracy of predictions will be discussed. Although related to the accuracy assessed for the models as a whole, differences in the predicted success or failure for <u>individuals</u>, across both dependent variables and observation windows represents yet another way to assess the adequacy of these models. Depending on the choices made, relatively few or many individuals may be predicted to fail or succeed for these kinds of analyses, and these choices have consequences for how well the predictions fare. We will argue, following others, that such choices cannot be based solely on statistical criteria. The subjective benefits and costs associated with the prediction of recidivism must also be considered.

The volume of material to be covered in this chapter forces us to be even more selective both in terms of the dependent variables studied and the observation windows used to study them. For the latter, one and five-year time windows for dependent variables will be used. As we have seen in earlier chapters, results using recidivism measured at one-year seem markedly different from those using longer follow-up periods. Retaining this short window thus follows from our earlier findings. Choice of only one from the

longer windows of follow-up is less straightforward. Since little substantive change was observed in models' parameters after five years, and since recidivism studies seldom look beyond five years, five-year windows will also be used in this chapter.

The selection of dependent variables also follows from the results of earlier chapters. The binary recidivism measures of rearrest, rearrest for a persons crimes, and reimprisonment are natural candidates for study. As the analyses in Chapter Six showed, substantively different models of recidivism emerged, and these three binary measures draw upon the distinctions we found in those results. As well, attention will be given to overall volume of subsequent offending, now defined as being in the top 10% of offenders as indicated by the yearly charge rate, adjusted for time at risk.[2] This binary measure is computed relative to each of the two windows to be used with 2.63 and 1.99 charges per year defining "high rate" offenders at one and five years respectively. That "high rate" offenders are also high volume offenders is seen by the fact that at one year, those in the top 10% of the adjusted charge rate distribution are responsible for 72.0% of all recidivistic charges, and 58.9% of all arrests, found in this sample. By five years, the high rate group accounts for 52.2% of all charges and 45.9% of all arrests.[3]

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[2] We excluded a number of forms of recidivism from consideration here on statistical grounds. Some variables measuring rates of rearrest (e.g., for persons crimes) were highly skewed to the right, thus invalidating the use of summary statistics to evaluate the predictive ability for dichotomous versions of these variables. Time to failure measures are similarly highly skewed, though the skew is to the left.

[3] Risk, and the accuracy of the predictions, need not be assessed in terms of dichotomous categories. The predicted probabilities of an event themselves could be used, as could the "degree" or "amount" of recidivism (e.g., number of rearrests) that a given model may predict for an individual. We concentrate on possible dichotomous decisions about predicted failure or success, in part, because they are more common and, in part, to simplify the analysis: the

These four variables represent a diversity of recidivism measures, as well as the dimensions of recidivism that best distinguish themselves from one another in our earlier analyses. At the same time, they are among the most common in the literature. These distinctions also seem to be of general policy importance: recidivism involving persons crimes is arguably more important than recidivism involving any type of crime, and the imprisonment process serves as a filter of less serious offenses, though, admittedly other factors are also at work. High rate offenders represent the CJS's interest in incapacitation, a potentially important component of intervention.

The focus on whether or not an individual is predicted to recidivate allows us to revisit many of the themes from earlier chapters. The effects of sentences on recidivism can be further evaluated by investigating how expected recidivism differs when the sentence received is included in the various models of recidivism. The difference made by the definition of recidivism is then studied through the comparison of predictions across the different dependent variables. This answers the basic question of "if recidivism is defined differently, are different individuals expected to fail?"

We then resurrect one of the central issues of Chapter Five: the difference it makes to choose among independent variables in the prediction of recidivism. That is, how many individuals would be predicted differently using independent variables from only one of the various domains included in our models? This leads us to once again raise the question of predicting an individual's recidivism based upon considerations of race, ethnicity, and

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statistical procedures and summary measures of accuracy are better developed for dichotomous outcomes. As well, binary measures more closely approximate the decision that CJS agents must maker. Policy needs frequently dictate making a "yes" or "no" CJS decision (to predict an offender as a recidivist or not), so dichotomous versions of the dependent variable seem a logical choice.

gender. We assess the extent to which it makes a difference to "adjust" the models to "remove" the effects of race, ethnicity, and gender from the prediction of failure and success.

These results are all predicated upon what turns out to be an absolutely crucial decision: how the predictions of a model are translated into a binary outcome of expected "success" (no recidivism) or expected "failure" (recidivism). The models of Chapters Five through Eight all make predictions for each individual. In the case of logistic regression, the predictions are the probability that an event (e.g., rearrest, reimprisonment) will be observed in the future. For least squares regression, it is predicted levels of future recidivism (e.g., number of convictions, rate of rearrest). Some decision must be made as to how to convert these continuous predictions into a dichotomous counterpart. It is to the possible options, and the consequences of selecting from among them, that we now turn.

CHOOSING "CUT-OFF" POINTS

Making predictions of an individual's behavior using the models developed in earlier chapters necessitates addressing additional questions which are mostly of a methodological nature. Many forms of recidivism are empirically infrequent and this entails several consequences for the determination of "failure" and "success" when these forms of recidivism are used. Indeed, one of the ironies that emerges surrounds the infrequent forms of recidivism. From the perspective of the CJS as a whole, low levels of recidivistic events (e.g., arrests for a crime against persons) are desirable, but from the perspective of <u>predicting</u> these events, this infrequency creates conceptual and statistical problems.

As the terminology of individual-level prediction differs from that used in previous chapters, we briefly define the central terms here. The "base rate" is the observed proportion of individuals who recidivate through some event (e.g., rearrest, reimprisonment, or being a "high rate" offender). The "selection ratio" is the proportion of the sample expected to recidivate through the event. Considerable choice is allowed in determining the "cutoff point" for the latter, and we address the possibilities shortly. Together, the base rate and the selection ratio determine both "false positives" which are expectations for recidivism that prove not to be true, and "false negatives" where the individual is predicted not to recidivate, but eventually does. The inaccurate predictions of false positives and false negatives may not be equally undesirable. A "civil-libertarian" perspective (Blumstein et al., 1985) stresses the concern over inaccurate individual predictions of expected recidivism (false positives). A competing perspective stresses crime control by giving greater emphasis to instances where subsequent recidivism was mispredicted (false negatives). The tradeoffs between these two perspectives can be assessed through the examination of "civil-libertarian ratios."

The first issue that we focus on is the choice of a strategy to select a cut-off point given a distribution of predicted probabilities for failure. That is, how should one go about deciding that a predicted probability of recidivating warrants the designation of "expected failure"? When the base rate (proportion actually failing) is approximately .5, the choice of a cut-off value other than .5 would only be based on some overriding policy concerns, such as wanting to avoid false positives at the expense of additional false negatives. However, as we saw in Chapters Four and Eight,

there are many forms of recidivism where the base rate departs considerably from .5. When this happens, three general strategies seem possible from a statistical perspective, and each has associated advantages and disadvantages. We examine these various cut-off point selection strategies in terms of making a choice based on several statistical considerations. This leads to a more general discussion of making a selection based on policy goals.

That there is a "choice" issue in making a cut-off decision may seem surprising to some, since the traditional .5 cut-off probability (the default value in most statistical packages) would seem to be a "natural" choice. If a probability is greater than .5, one can say that the estimated chances are greater that the individual will recidivate than he/she will not recidivate. But the choice of a .5 probability is somewhat arbitrary. In instances where the base rate is relatively low, some value lower than .5 may be a more optimal choice. Predictive accuracy may be enhanced by using a lower cut-off point, and various policy goals may be better attained. With regard to the latter consideration, for example, Blumstein et al., (1985) have argued it may be useful to conceptualize the choice of a cut-off criterion in terms of how beneficial society considers the balance of successfully identified recidivists versus those that are not successfully identified. To choose a cut-off point without consideration of its consequences often unwittingly leads to a choice that has relatively many false negatives compared to false positives. This is especially likely to happen when the predicted probabilities are highly skewed, as are most measures of recidivism.

Within the methodological literature, we find three general options to guide choosing a cut-off point to define predicted success or failure. The first is to use the .5 cut-off value. A second option is to choose a cut-off

point so as to select a proportion of failures equal to the proportion observed to fail. In this instance, the selection ratio is chosen to equal the base rate. A third approach is to select a cut-off point that is equal to the mean proportion of offenders who fail. Unlike the first, the latter two approaches result in cut-off points that are sensitive to the distribution of the dependent variable. It is only when the predicted probabilities are symmetrically distributed around an observed mean of .5 that all three strategies result in identical predictions of success and failure. When there is a low base rate, the distribution for the probability of recidivism becomes highly skewed, and the choice of a .5 cut-off probability may result in virtually no one being predicted to fail.[4]

The first alternative strategy to simply selecting .5 as a cut-off value aims at maximizing the chances of making correct predictions, and at equalizing the number of false positives and false negatives. Blumstein et al., (1985) discuss how choosing a cut-off point based on the proportion actually failing results in an approximately equal number of false positives and false negatives. That is, one may choose a cut-off point such that there will be an approximately equal number of each type of failure. This is a potentially desirable consequence in the aggregate. Copas and Tarling (1986) have shown that the optimal cut-off point for this outcome is one that ensures that the proportion predicted to fail equals the proportion who do fail, or, in the terms of this literature, the selection ratio (the proportion predicted to recidivate) equals the base rate (the proportion who recidivate).

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[4] We do not evaluate any examples of dependent variables here that are greatly negatively skewed, although they are logically possible, such as when time-to-failure measures are used.

More importantly, such a choice results in the logical possibility of making 100% correct predictions. The choice of any other cut-off probability necessitates some error in the predictions. For example, if we choose 20% to fail for a recidivism measure with a base rate of 30%, we must, by definition, be in error for at least 10% of the cases. If we choose a cut-off probability such that the proportion predicted to fail equals the proportion who fail, the possibility exists that all cases are predicted correctly.

Following Blumstein et al. (1985), we contend that more than simply statistical criteria should enter into the choice of a cut-off point. It is valuable to consider the costs and benefits of particular cut-off points to the more general values associated with identifying higher proportions of the population as "positive" or "negative." That is, as a policy matter, a choice may be made to identify higher proportions of individuals as "positives" if that is considered to be beneficial, with recognition of the possible greater risk of misidentifying such individuals. Alternatively, fewer individuals may be predicted to fail by choice of a relatively high cut-off point, with the possible greater risk to society of identifying relatively few to fail.[5] Choice of a cut-off then depends on the relative subjective assessment of the disutility of a incorrectly identified failure (false positive) and of an

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[5] A cut-off point may be selected to give greater weight to the risk of a false positive over a false negative, thus giving greater weight to making a prediction of failure over a prediction of success. A "libertarian" perspective may lead to the choice of a higher cut-off point to lessen the chances of wrongfully predicting a failure (recidivism) -- the so-called "false positive" problem. By lessening the chances of making a false positive identification, "libertarian" goals are protected, but, at the expense of "community safety" goals, since there will be more false negatives with the choice of a more stringent cut-off point. We assess these trade-offs below. However, we will not fully explore the possibilities of selecting cut-off points aimed at maximizing or minimizing either one type of error or the other, although such strategies may be useful for making some decisions on the nature of CJS interventions.

incorrectly predicted success (false negative). The "net disutility" has been shown by Blumstein and his colleagues to be formalized in a system of mathematical statements. These will be discussed after assessing the sensitivity of predictions to the choice of a cut-off point.

The Prediction of Rearrest at One and Five Years.

We begin by describing how the choice of a cut-off point influences individual-level prediction for the dependent variables of rearrested within one and five years. The full model, as defined in the previous chapter (all the independent variables, including interventions and interactions), is used to generate continuous variables for the probability of being rearrested within one of those periods. Histograms for the distributions of these predicted probabilities are shown in Figure 9.1. Superimposed on these graphs are the distributions to be expected if these probabilities followed a normal curve. As can be seen, the one-year distribution (at top of the figure) is more positively skewed than that for the predicted probability of rearrest in five years.[6] The differences in the form of the two distributions can be attributed, in part, to the differences in the base rates (the proportion who actually are rearrested), which are .23 and .48, respectively, for the one- and five-year windows. A choice of the standard .5 cut-off point would result in a relatively large proportion of offenders being predicted to fail over the five-year window, especially when compared to the one-year window.

One alternative to the choice of .5 is to select a cut-off value equal to the mean of the distribution (i.e., the base rate). Note that if these values (.23 and .48) are selected as cut-off points for the rearrest

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[6] Note that the distributions appear more similar to one another because the one-year distribution has a larger metric in order to fit on the printed page.

variables, the proportions predicted to fail constitute approximately half of each of the distributions as the cut-offs approximate the means of these two distributions.[7] Under the selection of .5 as a cut-off point, relatively few are predicted to fail by rearrest at one year, compared to almost half of the sample predicted to fail at five years. In contrast, the choice of the mean as a cut-off value would result in many more predicted to fail at one year, and have little effect on the number predicted to fail at five years.

The final strategy we consider is the use of a cut-off point that ensures that the proportion observed to fail equals that expected to fail. For rearrest at one year, this means defining the top 23% of predicted probabilities as "failures." For rearrest at five years, the top 48% of the distribution is used. The choice of a cut-off point such that the selection ratio equals the base rate would have little effect on the number of failures predicted at five years. Relative to other strategies, this choice would decrease the number of predicted failures at one year. Now, "only" 23% would be predicted to fail (equal to the base rate) after one year, while the use of mean as the cut-off leads to 39% being predicted to fail. Note that, for rearrest at one year about 16% of the sample would be "false positives" by definition when the mean of .23 is used as a cut-off. If the selection ratio is chosen to equal the base rate, the a priori minimum placed on the percentage of false positives is zero.

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[7] It may not be obvious that this is so, but the mean of the predicted probabilities of failure in all the models evaluated here equals the base rate (the observed proportion of failures). Thus the choice of the mean as the cut-off value is essentially a choice of everyone who is "above average" in the predicted probability of failure. However, since the predicted values will often be skewed, the mean and median will not necessarily be the same point and there is no restriction that using the base rate as a cut-off point will yield a prediction of failure for approximately half the sample.

Based on the consideration of the skewness of the predicted values for these recidivism variables, it would often seem desirable to select a cut-off point other than .5. To routinely use .5 would result in many false negatives when the base rate is low, as it often is in recidivism studies. When the distribution of failure probabilities is roughly symmetric around a mean of .5, as is the case with rearrest at five years, the choice of one cut-off strategy over another makes little difference. With skewed distributions (i.e., rearrest at one year), the differences are substantial. The use of .5 produces fewer predicted failures, leading to more false negatives as more fail than are expected to do so. The use of the mean overpredicts the extent of failure, leading to an overabundance of false positives. As discussed in previous research, and as can be seen in the distributions in Figure 9.1, the choice of a strategy such that the selection ratio equals the base rate has the desirable characteristic of allowing for the maximum number of correct predictions (as well as the desirable attribute of possibly equalizing the number of false negatives and false positives). If only 23% fail, and 23% are predicted to fail, prediction without errors is a logical possibility. Thus, these preliminary considerations point to some advantages of the latter approach, relative to using either .5 or the mean of the predicted failure distribution.

Other criteria for evaluating a cut-off point selection strategy focus on the accuracy of predictions. The percent correctly predicted is simply the percent of the sample at risk who are correctly predicted by the model to succeed (not recidivate) or to fail (recidivate). RIOC, or Relative Improvement Over Chance, was introduced by Loeber and Dishion (1983). It is defined as follows:

$$RIOC = \frac{AC - RC}{MC - RC}$$

where AC is the actual number of correct predictions, RC is the expected number of correct predictions by chance, and MC is the maximum possible number of correct predictions. RIOC is designed to provide a criterion of the relative efficiency of predictive devices with higher values indicating better predictive efficiency. However, the measure has been shown to vary as a function of the relation between the selection ratio and the base rate (Copas and Tarling, 1986). Finally, phi, a traditional measure of association is presented. It is computed as:

$$PHI^2 = \frac{((TP*N)-(R*C))^2}{(R*C)*((T-R)*(T-C))}$$

where TP is the number of true positives, N is the number of cases, R is the number of individuals predicted to fail, and C is the number of individuals who actually fail. Phi is derived from chi square, but has the advantage of not being inflated by large numbers of observations. However, it may have a value greater than 1.0. The larger the value of phi, the greater the association between the predicted and observed successes and failures. Thus, a larger value of phi is generally considered desirable for claims of greater predictability, although it too is sensitive to the skewness of the variables.

These summary statistics can be used to assess the sensitivity of predictive accuracy to the choice of a cut-off point. In Figure 9.2 these statistics are graphed against cut-off points ranging from .1 to .9 for rearrest at one year. Also included on this graph are the proportion of false negatives and false positives resulting from the use of a particular cut-off value, as well as the proportion predicted to fail (the selection ratio) given

the cut-off point.[8]

For rearrest at one year, the observed (and predicted) proportion failing is .23. At a cut-off of .35, we find the selection ratio equalling the base rate. Note that, in Figure 9.2, the number of false positives (FP) approximately equals the number of false negatives (FN) at this point. As higher cut-off values are chosen, the proportion of false negatives rises, while the proportion of false positives falls off to virtually none by the .7 cut-off value. The line indicating the selection ratio (proportion selected to fail under a given cut-off) generally parallels the line for false positives. This is to be expected as the more chosen to fail, the more likely are false positives. Ultimately, the proportion selected to fail and the proportion of false positives merge as no one is predicted to fail. Note too how the percent correctly predicted appears to plateau between .3 and .4.[9] Thus, for this form of recidivism, there is an asymmetry in total errors of prediction across the choice of cut-off points. A choice of a low cut-off point, while minimizing false negatives, results in more errors than the selection of a relatively high cut-off point, where false positives are minimized.

At first glance, this suggests that to achieve the two goals of minimizing overall error and minimizing false positives, higher cut-off points should be selected. However, upon further reflection, it should be clear that it is the rarity of the event being predicted that determines this tendency:

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[8] Note that the variables being graphed have different metrics. Thus the interpretation of the y, or vertical, axis is contingent upon which statistic is being considered. Sometimes the values refer to proportions, other times (e.g, phi) the values graphed are not proportions.

[9] It is actually highest at the .5 cut-off, with .792 correctly predicted to succeed or fail.

where failure events are relatively rare, the proportion correctly predicted is "driven" largely by the predictions that success is likely. That the proportion correct in Figure 9.2 "plateaus" is a consequence of the fact that beyond a certain value, false positives are unlikely. Predicting that no one will fail will result in a 77% success rate and, as can be seen in the figure, this is approximately the level at which the success rate plateaus. There is little difference between the .77 success rate one would get by predicting that everyone succeeds and the .79 success rate that one gets with a .50 cut-off point, or the .773 success rate at .35 cut-off where the selection ratio equals the base rate. Yet another way of looking at the "plateau" phenomenon is that too few are predicted to fail to allow the proportion correct to depart appreciably from the success rate observed when everyone is predicted to succeed.

As Figure 9.2 shows, the overall accuracy of predictions, as measured by RIOC, varies as a function of the cut-off values. More precisely, predictive accuracy is a function of how the cut-off values depart from the point at which the base rate equals the selection ratio. Either relatively high or relatively low cut-off points results in <u>higher</u> RIOC values than that which is observed when the selection ratio equals the base rate. Consequently, when the possible number of correct predictions is limited by a selection ratio that is different from the base rate, RIOC tends to be higher. When the proportion predicted to fail equals the observed proportion of failures, the most error is possible, and the RIOC statistic reflects this possibility with a relatively low value. Others (Copas and Tarling, 1986) have also noted this. This indicates a danger inherent in comparing RIOC values across samples or populations with different base rates and selection ratios. One

implication is that RIOC alone cannot be considered useful for the selection of a cut-off point, since not only is it sensitive to the base rate, but, oddly, it is higher for either relatively high or low cut-off points. The sole consideration of RIOC would lead to the selection of a cut-off value that reflects the fact that relatively few correct predictions are possible at those points in the distributions.[10]

Phi, another traditionally used measure of association between dichotomous variables, is highest when the selection ratio equals the base rate (here at .35). Phi, as is RIOC, is sensitive to the skewness of the distribution, but while RIOC is highest where the selection ratio departs most from the observed proportion failing, phi is lowest. That is, the association between predicted and observed failure is lower as cut-off points depart from approximately the point where the selection ratio equals the base rate.[11] Correspondingly, phi is highest when the selection ratio equals the base rate, and will equal RIOC at that point -- see Copas and Tarling, 1986. In fact, the phi values graphed here generally represent a reverse image of the RIOC values across cut-off points.[12]

In Figure 9.3, the various statistics for rearrest at five years are presented. The base rate (actual proportion rearrested) is .48. The results

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[10] While RIOC was not intended for use as a criterion for a cut-off point selection, the results here to demonstrate how this widely used statistic is sensitive to the cut-off point decision. Moreover, Figure 9.2 suggests that the illusion of predictive accuracy can be achieved simply by choosing a cut-off where either it is not possible to make many correct predictions of failure or of success.

[11] This quality of phi is not a general one, as later figures will show that phi tends to take on higher values at cut-off points below that at which the selection ratio equals the base rate.

[12] RIOC in Figure 9.2 has a dip at the .9 cut-off probability due to very few cases being selected to fail at that level.

show that the three choices for a cut-off point to define predicted failure result in few differences in the overall accuracy of this aspect of recidivism. If, however, one were to adopt a strategy of choosing a cut-off to minimize false positives, or maximize overall accuracy, cut-offs in the .4 to .6 range are suggested. As can be seen in Figure 9.3, the selection ratio (the proportion predicted to recidivate) declines from about 95% at a .1 cut-off point to about 6% at a .9 cut-off point. This sets the lower "limits" of the predictive accuracy for rearrest at five years. Unlike what was observed for the skewed rearrest at one year (Figure 9.2), in Figure 9.3 the percent correct is reduced appreciably as cut-off points are chosen above approximately .6 or below .4. As cut-off values above .5 are chosen, the proportion of correct predictions declines. This is in sharp contrast to the rarer event of rearrest at one year where the proportion of observed successes tends to approximate the proportion who are predicted successes.

Overall, the relative symmetry of the predicted probabilities for rearrest at five years are reflected in the symmetry of the summary statistics of RIOC, phi, and the proportion correctly predicted. All are centered around approximately .48 -- the point at which the base rate equals the selection ratio. As was the case for rearrest at one year, phi tends to be the inverse of RIOC. As noted above, phi equals RIOC if the selection ratio equals the base rate, and both are sensitive to the value of the selection ratio (Copas and Tarling, 1986). The latter property is clearly demonstrated here. RIOC is higher for probability cut-off values that result in selection ratios above or below the base rate. RIOC's usefulness for comparing the predictive efficiency of models that vary in their base rates would seem to be quite limited, as would the use of phi.

The implications of these "statistical" considerations for choosing one cut-off point over another point to a difficulty with such an approach: there is no single criterion that stands out as ideal to use when selecting a cut-off. No single cut-off succeeds in maximizing overall predictive accuracy, however defined, and this is especially true when the distribution of predicted failure is skewed. It may be that the choice of a cut-off point would be better made based on considerations more directly related to policy. For example, policy choices may be made to minimize or maximize one type of failure or another. Rather than consider strategies designed to produce the greatest accuracy for both predictions of success and failure, primacy may be given to one over the other. We will address this possibility below.

Thus far, the evaluations of the proportion correctly predicted, false positives and false negatives, as well as of phi and RIOC, all suggest that it makes a substantial difference as to which strategy of selecting a cut-off probability is chosen when the criterion variable is skewed. Choice of a cut-off point has implications for the percent correctly predicted, as well as for the overall degree of success in prediction. Before drawing any further conclusions, however, we examine the distributions and various statistics for several other forms of recidivism.

Other Measures of Recidivism.

The recidivism variables of rearrest for persons offense at one and five years represent two highly skewed dependent variables, with predicted probability distributions (again using the full model) very similar to that for rearrest at one year. Figure 9.4 presents histograms of these predicted probability distributions. The vast majority of cases have relatively low probabilities of expected rearrest for persons offenses at either one or five

years. Note that only a handful of cases are selected to fail at a .5 cut-off point in the histogram on top of the figure, and only a few more in the histogram at the bottom. The base rates are 7% and 20% for the one and five year windows, respectively. Interestingly, the use of these means as cut-offs would result in more individuals being predicted to fail in a one-year window than in a five-year window. Thus, in the case of an empirically rare event like rearrest for a persons offense in one year, the choice of a mean as a cut-off point would seem to be an even more undesirable choice than was seen above for rearrest at one year. The points at which the selection ratio equals the base rate are .21 and .34, respectively, and the use of these values as cut-offs predicts more failures through rearrest at five years than at one year.

In Figure 9.5, the univariate distributions of reimprisonment at one and five years are presented. In general, a similar pattern is observed. Here, the base rates are 5.6% and 18.6% for one and five-year windows, respectively. Again, relatively few individuals have probability values greater than .5, though considerably more cases are expected to fail at five years than after only one year.[13] The use of the base rates as cut-offs for these distributions leads to the expectation of roughly comparable numbers of failures at each time period. The points at which the selection ratio equals the base rate are .24 and .38 respectively. These values result in more predicted failures than do the use of .5, but considerably fewer than are

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[13] Interestingly, a comparison of reimprisonment at five years with rearrest for persons crimes at five years finds many more individuals expected to fail by reimprisonment than by rearrest for persons crimes, and a longer tail to the distribution. This suggests the general model is better able to differentiate individuals in the case of reimprisonment than rearrest for person's crimes, a suspicion that is confirmed by the pseudo R-squares reported in Chapter Eight.

expected when the means are used as a cut-off.

Figure 9.6 shows the predicted probabilities of being a high rate offender at one and five years. It will be remembered that a high rate offender is defined as one in the top 10 percent of the observed distribution for the adjusted charge rate at a given window. An important consequence of this definition is that the base rates (and the means) are 10% for both one and five-year windows. Not surprisingly, very similar distributions are observed across the two windows of follow-up, and the impact of the choice of a cut-off is virtually identical across windows. The use of .5 selects few cases to fail via being a high rate offender, and the mean is overly liberal in predicting this form of recidivism. The probability values for the top 10% of predicted failures are .27 and .32, respectively and these are the points at which the selection ratio equals the base rate.

We now consider the summary statistics for these additional forms of recidivism. Figures 9.7 and 9.8 show the results for rearrest for persons crimes for a one and five year follow-up periods. Here the base rates are .07 and .20, respectively, and the values at which the selection ratio equals the base rate are .21 and .34. As was observed above for rearrest, the lines for the selection ratio and the proportion of false positives closely parallel one another in both graphs: the more selected to fail, the more false positives. In Figure 9.7, note that virtually no one is predicted to fail above the .5 probability level. This is a direct consequence of the severe skew in the predicted probability of a persons rearrest in one year. The proportion correctly predicted rises to approximate the 93 percent who do not fail, reaching this plateau at quite small cut-off values. In Figure 9.8, the percent correct also "stabilizes" at about 80% of the sample which is the

percent who are not rearrested for a persons crime in five years. Again, across both figures, false negatives account for most of the failures as predicted probability values are selected above the .21 and .34 values. RIOC and phi do not "mirror" one another, although they would have the same value at the .21 and .34 cut-off points. hi tends to increase at cut-off values below the point where the selection ratio equals the base rate in the two figures. Thus, the phi statistic suggests a stronger association between predicted and observed at lower cut-off points. Note too, that the values of RIOC and phi generally do not follow a steady curve, but deviate slightly from such patterns, indicating that they are not measuring the "same thing." This is especially true at high cut-off values where there are few predicted failures.

In Figures 9.9 and 9.10, the relationships between choice of a cut-off point for reimprisonment at one and five years and the various criteria are presented. Here, the base rates are 5.6% and 18.6%, respectively, and .24 and .38 are the values at which the selection ratio equals the base rate. These graphs reinforce the generalizability of the results found above. The proportions of false negatives rise steadily as the definition of failure becomes more stringent. The proportion correctly predicted approximates the proportion who actually succeed as higher cut-off values are selected, although this is less true for reimprisonment at five years, where the proportion correctly predicted drops slightly with higher values. Note that the proportions correctly predicted are <u>not</u> at their highest when the selection ratio equals the base rate, but peak at slightly higher probability values -- and remain at the level which approximates the proportion who actually succeed. Thus, while the strategy of choosing a selection ratio

equal to the base rate has several appealing features, it seems to be not the best choice if the only concern is to maximize the proportion correctly predicted. But, if that were the sole criteria, choosing everyone to succeed would be a reasonable prediction, as roughly the same inaccuracy rate would be achieved. For these highly skewed prediction probabilities, the percent correct tends to change less than .01 percent across increments of .1 above the .4 cut-off point. Thus, selecting everyone to succeed becomes as reasonable a choice as selecting a proportion to fail that is less than the base rate.

Finally, the last two dependent variables, being high rate offenders at one and five years, are examined in Figures 9.11 and 9.12. Here, it will be recalled, the dependent variables have been defined by the top deciles of the observed distributions of adjusted charge rates. The cut-off points at which the selection ratios equal the base rates are .27 and .32 for the one and five-year windows. As before, the false positive proportions coincide with the selection ratio, intersecting when no one is predicted to fail. Again, RIOC is lowest at these points, but phi is not at its highest: phi obtains higher values at even lower cut-off points. This was also observed above for the more skewed distributions. As well, the percent correctly predicted at the points in the two distributions where the selection ratio equals the base rate are not the highest. As above, we observe that the proportion correctly predicted plateaus at approximately the percent who succeed, once again demonstrating the importance of the base rate.

Our initial purpose here was to find a cut-off point selection strategy that would be superior to all others. It is clear that, as base rates fall below .5, such strategies become necessary. Yet there is no single cut-off

strategy that satisfies all concerns when recidivistic events are infrequent. What we have found is that each strategy has liabilities, and that in this context, other criteria should be considered. Policy concerns offer some direction for choice of cut-off points. The three general strategies choice strategies can be evaluated relative to a policy emphasis on avoiding false positives, or avoiding false negatives, or striking a balance between the two.

Civil-Libertarian Ratios.

Additional factors can enter into the choice of a cut-off point when it is recognized that the designations of "expected recidivist" and "predicted success" may be used for decision making purposes. That is, CJS agents may act upon these individual level predictions. Those identified as likely failures may be subject to more severe punishment or lengthier supervision. Fewer CJS resources may be directed toward individuals who are likely to succeed. Once predictions for an individual's recidivism prospects are used to make decisions about treatment, issues surrounding the "costs" of misclassification are raised. For the criminal justice system, there are moral and ethical costs associated with false positive predictions, as well as for false negatives. For the individual, there can be very real costs associated with an inaccurate positive prediction. Such costs can be incorporated into the choice of a cut-off point.

Following a "libertarian" line of reasoning, a relatively high cut-off value should be chosen. In this case fewer would be selected to fail in accordance with libertarian values since fewer individual offenders would be subject to misclassification as a "likely recidivist." Alternatively, a low cut-off point could be chosen to select more individuals to fail in accordance with a lowered concern for the offender, and an arguably higher concern for

society at large. The implications for individual prediction of choosing a low versus high cut-off point can be seen clearly in earlier graphs. Note too, that such policy considerations may be deemed more important than any of those previously discussed: the statistical evaluations tell us about certain aspects of the models' predictions at various cut-off points, but there is no requirement that any particular statistic be used to decide a cut-off point.

The trade-offs of choosing one cut-off point strategy over another, relative to policy considerations can be illustrated using rearrest at five years (Figure 9.3). An extreme libertarian concern for a high degree of certainty in predicting rearrest will lead to about a .4% false positive rate even if only 6% of the offenders are predicted to fail at a selection ratio of .9.[14] This is, arguably, the "best case" scenario for the extreme libertarian concern. Note that at that level of certainty (.9), the percent of false negatives (offenders not predicted to fail, but who do) is at its highest of about 42%. This is the cost to the community of a high libertarian value on not making a false positive identification for the most possible

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[14] It would seem that relatively few false positives occur at the cut-offs of .8 or .9: 2.1% and .4%, respectively. Note, however, that the computation of "percent false positive" and "percent false negative" is based on the total number of cases in the study. If one looks at the percent who do not recidivate <u>of those who are predicted to recidivate</u>, the percent wrongly predicted to fail is substantially higher (7% at the .9 selection ratio). Thus, the large denominator for the traditional calculation of false positive and false negative rates may be misleading as to how successful prediction is. In other words, conditional probabilities suggest that there are more wrongly predicted recidivists than appear to be the case when general probabilities are presented. This is of import because of the assumption that these predictions will be used for individual-level decision making: <u>given</u> that someone is predicted to fail, some action will be taken. That action is conditioned on the prediction and, as just noted, the conditional percentage of false positives is higher relative to the unconditioned percentage.

individuals.[15]

If, on the other hand, one is only concerned with protecting "community safety," then a low cut-off point should be chosen. In Figure 9.3, that choice might be a cut-off of .1, in which case there would be relatively few false negatives (less than 2 percent of the cases), but a relatively high false positive rate of about 44% of the cases. Thus, as increasingly high cut-off points are selected from .1 to .9, there is a decrease in false positives at the expense of an increase in false negatives.

Balancing these concerns can be achieved by using a cut-off that leads to equal false positive and false negative rates. For rearrest at five years, the false negative and false positive rates intersect at a point slightly lower than the .5 probability cut-off point. At about .47, the cut-off point that results in 48% of the population being selected to fail, we find equal numbers of false positives and false negatives. This is approximately the point where RIOC is the lowest, though this is not obvious from the figure as RIOC values are graphed at .1 intervals. Thus, for the prediction of rearrest at five years, overall accuracy is minimized for one summary statistic, and maximized for others when civil libertarian concerns are balanced against those of community safety.

While balancing civil-libertarian and community safety concerns results in some desirable statistical properties for prediction, it is not necessary that they be given equal weight. Choice of a cut-off point may be guided by consideration of the relative costs associated with a cut-off selection strategy. Blumstein et al., (1985) have used the following equation to define

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[15] Of course, the extreme libertarian position calls for no one to be falsely predicted to recidivate. This occurs when no one is predicted to recidivate using a cut-off of 1.00.

the disutility associated with a given cut-off value:

$$U'(Z^*) = N(Z^*) + \frac{Up}{2Un} P(Z^*)$$

where U' is the disutility associated with the chosen cut-off value Z*, N(Z*) is the number of false negatives resulting from that cut-off, Up is the negative utility associated with the misidentification of someone as a failure (false positives), Un is the negative utility for inaccurate predictions of success (false negatives), and P(Z*) is the number of false positives as a consequence of the cut-off point Z*. Higher values of U' are undesirable as they indicate that the cut-off Z* leads to greater subjective costs.

Now consider the ratio of Up/Un, which can be called r, following Blumstein et al. (1985). This ratio represents the relative concern attached to false positives and false negatives. If the value of r is 1.0, this implies that false positives and false negatives are equally important. That is, it is of equal "cost" (or worry) to predict someone to fail who does not as it is to predict someone to succeed who ultimately fails. An r of 2.0 implies that there is twice as much subjective cost associated with misidentifying someone as a likely recidivist as there is to misidentifying someone as likely to succeed. An r of .5 means that there is twice as much subjective "cost" to mispredicting success as there is for mispredicting failure. The choice of a value of r itself depends on the nature of the policy associated with the identification of a "positive." Where punishment of a severe nature is the consequence of predictions for failure, higher values of r that should be required. On the other hand, if relatively benign intervention is the consequence of a positive identification, then a low value of r may be appropriate (Blumstein et al., 1985:204).

The results that we have discussed above can be used to examine the relationships between false negatives, false positives and the civil-libertarian index r:

$$U'(Z^*) = N(Z^*) + (r/2)\ P(Z^*)$$

where terms are defined as before. This equation allows for a succinct summary of how the subjective costs of misclassifying individuals is related to the overall disutility resulting from these incorrect predictions. By varying the emphasis given to civil-libertarian and community safety concerns (r), we can assess how counterproductive it might be to stress one over the other. More importantly, disutility scores (U') provide another means of evaluating the three strategies for determining a cut-off point (Z*).

The subjective disutilities associated with each cut-off strategy can be represented graphically as a function of the relative weights given to community safety and civil-libertarian concerns. Through visual inspection, we can identify which strategy leads to the lowest overall disutility. The most desirable cut-off point is the one that minimizes disutility, regardless of the relative weights represented by r. While the ideal outcome for our purposes is when one cut-off point consistently produces the lowest disutilities, we shall see that this is never the case.

Figure 9.13 shows the graph of the disutility scores (U') associated with rearrest at one year. Civil-libertarian ratios from .25 (a great concern with false negatives) to 2.5 (a high emphasis on avoiding false positives) are used. The three cut-off points presented represent those behind the three choice strategies: .5, .354, where the selection ratio equals the base rate, and .23, the mean. A desirable disutility score is the lowest one at each civil-libertarian ratio value. Thus, at low civil-libertarian ratios, the

disutility scores for rearrest at one year are minimized for the line associated with the .23 (mean) cut-off point.[16] Where concern is strongest that positives be identified, use of the mean as a cut-off point would thus be optimal. Where concern is strongest that no one be misidentified as a recidivist, the .5 cut-off point would be optimal as this minimizes the associated disutility. Where concern is somewhat balanced between false positives and false negatives, the point at which the number of predicted failures equals the number of observed failures, the .354 cut-off, would be the best choice. Thus, the optimal cut-off point to minimize the disutility score is the mean across a range of civil-libertarian ratios from .25 to about .85; the point at which the selection ratio equals the base rate from about .85 to 1.4; and .5 for ratios 1.5 and higher. No single cut-off point consistently yields minimal disutilities.

In Figure 9.14 the civil-libertarian ratios are graphed against the three cut-off point possibilities for rearrest at five years. Here, as was the case with the accuracy statistics, there is little choice: all three lines are very similar. At the low end of the r index, however, the line associated with the selection ratio equaling the base rate is slightly better at minimizing disutility. In general though, since there is very little difference among these three cut-off values, choice of a cut-off has few consequences for disutility.

Figure 9.15 plots the three cut-off options' disutility scores for rearrest for a person's crime within one year. Here, the graph of the choice of a cut-off point is somewhat misleading relative to r. The value of .5

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[16] The magnitude of disutility scores is a function of the number of cases analyzed. As our sample size is large, all disutility scores are divided by 100 for presentational purposes.

would clearly seem to be the most desirable choice above approximately a .85 r value, but it must be kept in mind that very few are predicted to fail (only 48 of 11,714 individuals) when this cut-off is used. Thus, one should also take into consideration how many individuals are predicted to fail in making the evaluation.[17] Using the cut-off defined by equating the selection ratio and the base rate, approximately seven percent of the individuals in the sample are predicted to fail, and thus this value would generally be the optimal choice in that it not only has relatively low disutility scores, but involves an appreciable number of individuals predicted to fail relative to the .5 cut-off point selection.

Rearrest for a persons crime at five years and the disutilities for the three cut-off points are presented in Figure 9.16. As for rearrest at one year, the disutility scores around an r value of 1 are lowest for the choice of a cut-off value such that the selection ratio equals the base rate (here .34). As lower r values are considered, the choice of the mean (.20) minimizes disutilities, and above ratios of 1.3, .5 yields the lowest disutility scores. However, here too, relatively few are chosen to fail under a .5 cut-off (fewer than 200 individuals) so that these low disutilities are deceptive.

In Figures 9.17 through 9.20 the disutilities associated with the four remaining forms of recidivism focussed on in this chapter are presented. For reimprisonment at one year (Figure 9.17), a very similar pattern is observed to that seen for rearrest for persons crime at one year. Minimal numbers of

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[17] When recidivism is as infrequent as it is for the criterion of a persons rearrest at one year, there may be little point to using a model. Predict no one to fail and, as Figure 9.15 shows, both civil-libertarian and community safety concerns will be well served. Unfortunately, this approach belies the social concern over these forms of crime.

individuals are selected to fail using a .5 cut-off value, so that a cut-off value where the selection ratio equals the base rate would be a more realistic choice for minimizing disutilities across all r values. In Figure 9.18, the analysis for reimprisonment at five years is presented. Here too the pattern is familiar. The choice of a cut-off point such that the proportion selected to fail equals the proportion who do fail results in the lowest disutility scores when about equal weight is given to civil-libertarian and community safety values. The range of r values at which the .38 cut-off is optimal extends from about .8 to 1.5. Identification as a high rate offender at one and five years follows similar patterns to those observed for other more highly-skewed probability distributions: relatively few are selected to fail at .5, thus making the choice of a selection ratio equal to the base rate the more realistic choice for most r values. In one respect, however, the disutility scores for reimprisonment at five years and for being a high rate offender in either time period stand out. Using the cut-off defined by equating the selection ratio to the base rate minimizes disutility over a broader range of civil-libertarian ratios than was observed for the other forms of recidivism.

Evaluating Cut-Off Strategies.

The graphs of the summary statistics for recidivism defined as rearrest, rearrest for persons crime, reimprisonment, and being a high rate offender suggest that each cut-off selection strategy has liabilities. The choice of .5 or above as the definition of predicted failure has the advantages of meaning that the expected probability of success is greater than the expected probability of failure. Under this strategy, the proportion correctly predicted is not appreciably higher at other points in the distributions

examined here. That is, relative to the other selection strategies, the .5 cut-off will result in the most cases being correctly predicted. In the case of dependent variables with low base rates, however, the selection of any high cut-off value will result in approximately the same percentages of correctly predicted cases. Thus, the accuracy of the .5 cut-off choice is a reflection of the extent to which successful prediction for these skewed variables is produced by the low base rates: choosing everyone to succeed has approximately the same effect as selecting everyone above the .5 cut-off value as likely to fail.

Many of our findings are clearly driven by the skewness of the predicted probabilities. When a form of recidivism is infrequent, this is reflected in the shape of the distribution for the probability of recidivism and has a marked impact on whether or not an individual will be correctly predicted recidivate. Where the predicted probabilities are highly skewed, as in the case of rearrest at one year (see Figure 9.2), there is an asymmetry in successful prediction with the choice of a cut-off above or below the point where the selection ratio equals the base rate. As higher cut-off probabilities are chosen, the false negative rates rise arithmetically; as lower cut-off probabilities are chosen, the false positive rates rise geometrically. If higher cut-off points are chosen, there is little cost to accurate prediction, as the percent correctly predicted does not depart from that which would be obtained making the prediction that everyone succeeds. This is the case for all of the highly skewed distributions studied here.

When the predicted probabilities are highly skewed as the result of low base rates, the use of a more stringent cut-off (e.g., .5) leads to consistent consequences for predictive error. Few false positives are found and false

negatives represent the majority of inaccurate predictions. Thus, the choice of .5 for a cut-off, coupled with a form of recidivism that is empirically infrequent, guarantees an outcome in which few are predicted to fail and many false negatives will be observed. In short, it is a manifestation of a "libertarian" value on voiding false positives at the cost to society of relatively many false negatives.

In contrast, the use of the mean as the cut-off point results in relatively many individuals being selected to fail when the base rates are low. This strategy ensures that some false positives will occur. In general, we observe far more false positives than false negatives if a dependent variable with a low base rate is chosen to represent "recidivism" and the mean is used as the cut-off value. As such, a cut-off at the mean implicitly represents a choice of selecting societal needs over the needs of individual offenders: a greater proportion of individuals are designated as "potential recidivists." However, such a choice is at a cost to the overall predictive accuracy, as relatively low proportions of individuals are correctly predicted where the base rates are low and the mean is used to define expected failure.

The cut-off point choice strategy of selecting a value such that the selection ratio equals the base rate provides some balance relative to the possible policy goals of emphasizing false negatives and false positives. Also desirable is the fact that this strategy allows prediction models the possibility to predict all the cases correctly, and that the numbers of false negatives and false positives are equalized. There is a cost of accuracy, however, as the percent correctly predicted is not as high as would be found using other cut-off values. Phi is not always highest, and RIOC is at its lowest, when the selection ratio equals the base rate.

Although several statistical arguments can be made in support of the various cutoff strategies, social policy provides the context for choosing among them. Choice may be dictated as a result of greater philosophical concern for "libertarian" or "community safety" priorities. Lowering the cut-off point enhances the interests of the latter orientation, while raising benefits the advocates of the former. An "equally balanced" approach would suggest the choice of a cut-off point that equalizes the percent false positive and percent false negative. Actual policy could fall any where along a continuum of such concerns.

The strategy of a cut-off value where the selection ratio equals the base rate is more reasonable if the policy context is one that defines equalizing false negatives and false positives as a desirable goal. Although results vary across dependent variables, it seems that the choice of the mean of the predicted value is not a very desirable cut-off point unless there is a low concern for misidentifying positives. Where the concern for false positives rises above the .5 to .85 range of civil-libertarian ratios, and the predicted probability values are rather skewed, the choice of a cut-off point such that the selection ratio equals the base rate would be a preferred strategy. Choice of .5 for such distribution may result in too few cases selected to fail to warrant serious consideration. Where the dependent variables are not as highly skewed (e.g., rearrest at one year, rearrest for person's crime at five years, reimprisonment at five years), the .5 cut-off point would be a desirable choice if concern for false positive outweighs concern for false negatives at a ratio of 1.3 to 1.5 or higher.

In conclusion, while no single cut-off point strategy for dependent variables with low base rates is without problems, it is necessary to choose

among them in order to proceed. We have opted for the choice of a selection ratio equal to the base rate as the most reasonable "general strategy." There are several reasons that make this the most attractive choice. The first is that it is the only cut-off point strategy that allows for the logical possibility of completely correct prediction. All other choices will result in some false negatives or false positives by definition. Second, from a policy perspective, it does not seem sensible to select a strategy where most of the failures are false positives, as would be the case if the mean of the predicted values were used, or false negatives (if .5 were used). The strategy of "selection ratio equals the base rate" allows for a greater differentiation of individuals in terms of their prospects for recidivism. Third, phi, a traditional measure of association is relatively high at this cut-off for most of the distributions, indicating a relatively strong association between predicted and observed recidivism. (However, phi is higher for some lower cut-off points, including the mean of some of the dependent variables with low base rates).

Finally, the choice of a cut-off value such that the selection ratio equals the base rate is generally an optimal choice for balancing concerns over false positives and false negatives. As was shown by the analysis of civil-libertarian ratios, the cut-off values associated with an equalization of false negatives and positives allow for the least disutility over a fairly wide range of civil-libertarian ratios for some forms of recidivism. Using this cut-off allows us to address both sets of values, even when one is given slightly more emphasis than the other.

In selecting a cut-off strategy such that the selection ratio equals the base rate, there are some costs. More incorrect predictions may result than

when other strategies are followed. However, the reduction of overall efficiency must be balanced against the gain in policy relevance stemming from an alternative cut-off point selection. As well, predicting as many people to fail as are observed to fail is not a standard approach for this kind of research. While we believe that there are sound reasons for selecting this cut-off strategy, there will be some loss in comparability to the existing literature.

The results in the remainder of this chapter are all predicated on defining expected failure such that the proportion selected to fail equals the proportion that do fail. We do so to simplify the presentation as it is not possible to investigate how sensitive the findings below are to the choice of a cut-off point. There are no assurances that similar conclusions would be arrived at if a different definition of predicted failure were used. For some policy reasons, another cut-off selection strategy might be more appropriate, and lead to different results.

INTERVENTION EFFECTS ON THE PREDICTION OF INDIVIDUAL-LEVEL RECIDIVISM

Given a decision on how to define predicted success or failure, it is now possible to explore the ways in which individual-level prediction is influenced by a number of considerations raised in earlier chapters. The first issue we address is the extent to which <u>expectations</u> for an individual's future recidivism are changed by the sentence received. This can be investigated through comparing predictions of failure based on the full intervention model (without hazard controls) of the previous chapter with those from a model that does not include intervention effects.

Such a comparison serves two purposes. First, it allows for a further evaluation of the effects of these sentences. In Chapters Seven and Eight, we saw that, in the aggregate, sanction effects were relatively small in terms of contribution to explained variance. However, these effects might be more substantial at the level of the individual. Offenders expected to fail when only prior record, presenting offense, and so forth are used for prediction might be expected to succeed when the sentence is added to make predictions. As expectations of recidivism shift from designations of "failure" to "success," more beneficial sanction effects are implied.[18]

The second purpose of comparing predicted failure between a model that includes intervention effects and one that does not is more subtle. Predicted recidivism based on a model including intervention effects, by definition, assumes that sentences have the impacts that were estimated in the previous chapter. That is, sentences of incarceration are expected to lower the probability of recidivism, some forms of sentences have no effect on recidivism, and many sanction effects are meditated by some aspects of prior record. Given these known effects, we can ask, <u>a priori</u>, how effective a sentence is likely to be. If an offender's predicted failure changes to one of predicted success when intervention effects are incorporated, that sanction is more likely to be effective. Conversely, if an initial prediction of failure is unchanged by adding intervention effects to make the prediction, that sentence is likely to be less effective.

The comparison of the predictions from these two models highlights an important substantive distinction about <u>when</u> predictions are made. The full

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[18] How accurate these predictions actually are is taken up in other sections.

model including intervention effects (Chapters Seven and Eight) represents an assessment of the likelihood of recidivism at the point at which the sentence is given. That is, shortly after the judge has sentenced an individual, what are the predictions for success and failure?[19] When the probability of recidivism is predicted using the basic model developed in Chapters Five and Six, recidivism prospects are assessed prior to the introduction of the sanction.

The significance of the distinction over when predictions are made depends upon the use of the predictions. In the previous sections, the intent was to evaluate the sensitivity of classification to the choice of a cut-off point. For that purpose, it was reasonable to use the full model as it is unlikely that the results would have differed had the basic model been used (see below). In the present section, we wish to demonstrate how individual predictions change when intervention effects are incorporated: this involves a direct comparison of predictions made prior to the sentence with those made just after the sentence is given. Later in this chapter use is made of either the full or attenuated form of the model contingent on the focus of the analysis. The major consequence of distinctions over when success or failure is predicted revolves around policy implications. These are developed in Chapter Ten.

Table 9.1 shows the changes in prediction for each of the eight measures of recidivism used in this chapter. The basic models' predictions are across the top of the table. Recidivism expectations generated using a model with intervention effects are on the rows of this table. Note that while the base

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[19] This is consistent with the arguments of Chapter Seven that we are evaluating the effects of the sentence and not the impact of any treatment received as a consequence of that sentence.

rates are the same for these sets of predictions, selection ratios are specific to each combination of model and recidivism variable. In the upper left hand corner, the differences in predictions for rearrest at five years are presented with similar cross-tabulations for the other forms of recidivism, measured at both one and five years, down the diagonal of Table 9.1.

In general, predictions change little when intervention effects are also used to forecast recidivism. The vast majority of individuals are classified similarly whether or not sanction effects are considered. For rearrest at five years, 340 individuals have a prediction of failure ("yes") changed to a prediction of success ("no") when intervention variables are included. This is mirrored by the 341 offenders whose initial expectation for success is now changed to one of failure.[20] The 5.8% of the sample whose rearrest at five years is predicted differently as a result of adding the intervention variables represents the largest discrepancy observed across forms of recidivism. High rate offending at five years yields the smallest shift with 265 (2.3%) individuals predicted differently as a result of including intervention effects. For other dependent variables, we find 5.4% (rearrest at one year), 4.9%, 3.2% (persons arrest at five and one year), 5.2%, 2.7% (imprisonment with five and one year), and 3.1% (high rate offender at one year) of the sample are classified differently as a consequence of predicting

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[20] The symmetry inherent in Table 9.1 is a direct consequence of predicting as many individuals to fail as actually do fail for both the full and attenuated models. For every offender who changes from a prediction of failure to success, there will tend to be one shifting from an expectation of success to failure. Note, however, there are considerable departures from this expected symmetry for both rearrest at one year and, especially, being a high rate offender over five years.

recidivism on the basis of the full intervention model.[21]

For the forms of recidivism that yield more highly skewed predicted probability distributions (e.g., arrest for a persons crime within one year), the number of individuals shifted is generally smaller. As well, the recidivism expected of individuals is changed more over the five-year window than over the one-year time period. The exception here is in being a high rate offender, where the consideration of intervention effects shifts the expected recidivism for more individuals at one year than at five years.

The results of Table 9.1 confirm the variance partitioning and regression results found in Chapter Eight in that relatively few of these individuals have their expected recidivism changed by inclusion of the sanction measures. When the estimated effects for these sentences are incorporated into the predictions, 5.8%, at most, have their predictions shifted. Yet, when compared to the levels of unique variances attributable to the sanction effects, the percentages shifted are more consequential. While

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[21] There is some concern over how sensitive these, and other, findings are to the problem "shrinkage" associated with the use of regression-like models in one sample and the ability of those models to predict in a different sample. That is, a critic may point out that we only have a "developmental" sample here, and that a validation sample is needed to determine how much we have maximized on chance in the models used to make predictions like those of Table 9.1. Although we do not have a validation sample to confirm these individual-level predictions, we do have a rather large sample so that the explained variance is less likely to be inflated (see Cohen and Cohen, 1983:106). If all of the independent variables in our models were uncorrelated with the dependent variables, about 1% of the variance would still be explained in any of our models. (See estimating "shrunken R-square in Cohen and Cohen, 1983.) Furthermore, the amount of cross-sample shrinkage is greater for models with relatively small amounts of explained variance and the amounts found here tend to be relatively high, compared to those reported by others. Whether the same proportions of individuals would be predicted differently across models in a validation sample is, of course, not known. In that we are comparing the predictions of models, each subject to some shrinkage in explained variance of about 1 percent, it seems dubious to claim that the differences observed here would disappear or be greatly reduced by shrinkage.

we saw in Chapter Eight that the sentence measures resulted in an increase of between 1 and 2.6% in the variances explained in recidivism measures, these small percentages translate to larger differences in individual-level predictions. In this respect, the sentence indicators have a greater import than was seen in earlier chapters.

COMPARING PREDICTIONS ACROSS MEASURES OF RECIDIVISM

One of our central claims throughout has been that how recidivism is defined "matters," a claim that has been supported by the results of Chapters Four through Eight. At the level of the individual, this contention takes on greater import. As noted earlier, the consequences of being labeled as a "likely failure" can be extreme for an individual. To the extent that interventions are tied to predictions, as is currently the case when clinical assessments are used in making an assessment of the likelihood of failure, it is desirable that classifications be consistent (and accurate) across forms of "failure." If predictions of success and failure vary according to the outcome used, then consequences for the individual are increased. This leads us to look at how sensitive the predictions of success and failure are to the events used to indicate a poor outcome. That is, we address the simple question "Are the same or different individuals predicted to succeed across forms of recidivism?" We use the full models developed for predicting each of the eight dependent variables for this purpose. As was just seen, results are likely to be similar if the basic model excluding intervention effects were used instead.

In Table 9.2 the dependent variables are arranged across the top and down the left side. Each are tabulated against the others by predictions of

success or failure. Note that these predictions are relative as variable-specific cut-off points, where the selection ratio equals the base rate, are used. In the first two columns of the table, those predicted to be rearrested within five years are tabulated against whether they are predicted to succeed or fail through the same event at one year. As is to be expected from the base rates for these variables, many more are predicted to fail at five years than at one year (a .354 cut-off is used for the former, and a .474 cut-off point for the latter).

However, fewer than half of those predicted to fail at five years are predicted to fail at one year. All those predicted not to fail at five years are predicted not to fail at one year. Note that this latter finding is not "true by definition" -- one conceivably could be predicted to rearrested at one year and not at five years.[22] Moreover, since different cut-off points are used at five years than at one year, it is somewhat surprising that there are not some individuals predicted to fail at one year who are not predicted to fail at five years. Thus, to claim that "it does not make much difference" whether a one-year or a five-year window is used would seem to be quite inaccurate, since a quarter of the sample would be predicted differently depending on which window were used. Moreover, if a one-year window were used, as would be likely in order to save time and money, 25% of the sample would not be predicted to fail, even though there is the expectation that they will fail within five years. (Note that this table does not evaluate whether or not these cases are "false negatives," within either follow-up period).

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[22] However, it is not likely that those predicted to fail at one year will not be predicted to fail at five years in that the coefficients in the model are similar across one and five-year windows.

Continuing across the row for rearrest at five years, we see that virtually no one is predicted to succeed by not being rearrested within five years and fail by the other criteria. In effect, there are no individuals who are predicted recidivists for any criteria who are predicted negatives using the model for rearrest at five years. However, there are many predicted positives under the model for rearrest within five years who are not expected to fail under other operational definitions of failure. In all the comparisons involving the first row of Table 9.2, more individuals are predicted to succeed across the other criteria than are expected to recidivate via rearrest in five years.

This asymmetry is not unexpected as the predictions for rearrest at five years serve to define who will not be predicted to fail by the models for the other dependent variables. The relatively low selection ratios for these other dependent variables virtually guarantee such results: relatively few are predicted to fail by these other criteria, so only a relatively small proportion of the "positives" as defined by the model for rearrest at five years can be predicted to fail. Again, the importance of the base rates of the criterion variables cannot be over stated. Note also, that our choice of a cut-off point such that the selection ratio equals the base rate is meant to increase the number of cases selected to fail under forms of recidivism with low base rates. Thus, the differences across dependent variables would be even more pronounced if a .5 probability of failure were routinely used across criteria. In general, it should be clear that it makes a considerable difference in who is predicted to fail to choose one criterion as opposed to another.

Comparing pairs of criteria with lower base rates is a more meaningful exercise in that more equal proportions are predicted to fail. The next two rows of Table 9.2 show the predictions for rearrest in one year across the remaining criteria. A similar pattern is found: relatively few individuals are predicted to fail by the models for the other criteria when the model for rearrest at one year predicts success. However, we now find more departures from the predictions of recidivism through rearrest. 425 individual who are not expected to fail by rearrest at one year are expected to fail by a persons arrest within five years. Where this rearrest model predicts success, the predictions for imprisonment over five years leads to the expectation of failure for 320 offenders.

A pattern underlies how predictions of rearrest at one year (the second row of Table 9.2) compare to these other forms of recidivism. There is greater agreement with the expectation of models based on five-year windows. Rearrest at one year yields predictions identical to those using five-year persons arrest 89.8% of the time. For reimprisonment at five years, 90.1% of the predictions agree and, for being a high rate offender at five years, 87.0% of the predictions are in agreement. This consistency arises, in part, because more failures are expected under these criteria over longer windows, making their base rates more similar to that for rearrest at one year. Those cases expected to fail by rearrest but not under the other criteria at one year, shift toward both joint predictions of failure and predictions of success in terms of rearrest, but failure otherwise. This produces considerable differences in an individual's expected success and failure across the recidivism criteria.

As for rearrest for persons crimes in five years, generally few predicted negatives by this criterion are predicted positives in models of other criteria. The exception to this pattern involves reimprisonment: 598 (6.7%) of the 9393 individuals predicted to succeed by the model for rearrest for persons crime in five years are predicted to be reimprisoned in a five-year window. Of those 2356 predicted to be rearrested for a persons crime, 1593 (67.6%) are predicted to be reimprisoned within five years. Thus, there is considerable overlap in predicting rearrest for persons crimes and reimprisonment in a five-year time frame; yet, even here 32% of the predicted positives by the persons crime criterion are predicted negatives by the reimprisonment one. Most of those predicted to be a high rate offender at for one or five year windows are also predicted to be reimprisoned. In contrast, most of those predicted to be reimprisoned at five years are <u>not</u> predicted to be high rate offenders at either one or five year windows.

Using arrest for a persons crime in one year as a criterion results in 817 predictions of failure, and many of these individuals are predicted to fail through the other forms of recidivism as well. Relatively few of these individuals are predicted to fail by the models for imprisonment in one year. At the same time, most of those predicted to fail by the criteria of reimprisonment within five years, or to be a high rate offender at one or five years, are not predicted to fail by the criterion of an arrest for persons crime at one year. Again, because of the skewness of the criteria (due to low base rates), most predictions are in agreement across models, and there are larger numbers of predicted negatives.

The remaining comparisons in the table pertain to reimprisonment and high rate offending. Almost all of those predicted to fail by models for

imprisonment at one year are predicted to fail at five years; but the converse is not true. Most of those predicted to be imprisoned in a five-year or one-year window are predicted to be high rate offenders at one or five years. Yet, more predicted high rate offenders are not expected to be imprisoned in one year than are predicted to be imprisoned in five years. (Again, this is not an unlikely finding because of the smaller base rate for reimprisonment at one year than for being a high rate offender.) Finally, comparing the high rate offender models, one finds most of those predicted to fail by one model are also predicted to fail by the other: about 81% of each model's positive predictions are echoed by the other model.

In summary, the comparison of predicted successes and failures across the eight criteria is arguably a comparison of apples and oranges. Since the base rate and selection ratio of each criteria different across criteria, we cannot necessarily expect there to be a high degree of agreement in the predictions. This comparison is also confounded when the predictions for one type of recidivism at one time period are contrasted with those of another type of recidivism measured at a different time period. Yet, this is what is often assumed in research involving recidivism: it does not matter which measure of recidivism is chosen or when it is measured. Clearly, the results here show that predicted successes and failures vary considerably across criteria. At the same time, it must be said that when base rates are low (less than 20%) there is a high percentage of cases agreed upon by pairs of models to be negatives. Thus, for example, 88% of the individuals are predicted to be negatives by both the models for high rate offenders at one

year and at five years.[23] The "conditional agreements" across models, i.e., the percent of predicted failures by one model predicted as failures by another model, generally demonstrate differences in who is predicted to fail. The "best case" for overlap in these conditional agreements involves high rate offenders at one and five years, where about 81% of the predicted failures by one model are predicted by the other. In all other comparisons, the percentage predicted positive by both models is far less, and is often below 50%. Twenty-three percent, or 41% of the 56 possible comparisons of predicted failures, are more often predicted successes by the other models of Table 9.2.

How recidivism is defined can thus make a difference for the predicted success or failure of the individual in two ways. The first is seen when expectations of a given form of recidivism are compared across windows. A longer window means a higher base rate and consequently, many individuals who are not expected to fail in the shorter period of time are expected to fail over the longer period. (Note that we seldom find the converse.) For a sizable proportion of individuals, this means that their designation as a likely success is contingent upon the window used to predict failure. Of those predicted to succeed in one year, 32.5%, 14%, and 13.8% of individuals are expected to fail over the longer window for rearrest, persons arrest, and reimprisonment, respectively.

The second way that choice of a recidivism measure can influence an individual's classification is seen when different forms of recidivism are compared, either for the same or different windows. As noted above,

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[23] Because of the manner in which being a high rate offender has been defined, the base rate has been set to 10% at both windows. In this regard, it is surprising to find that even 3.7%, or 436 of 11,714 individuals at risk at least one year, have predictions that are divergent across the two windows.

expectations for recidivism can change with this choice. While it is true that large percentages of agreement are found in Table 9.2, ranging from 96.3% for being a high rate offender at either time period down to 59.0% found between rearrest at five years and persons arrest within one year, it is not possible to conclude that these recidivism measures are interchangeable. Indeed, which recidivism measure is used appears to matter more for individual-level prediction that it did for the general conclusions of previous chapters.

THE IMPORTANCE OF INDEPENDENT VARIABLES FOR INDIVIDUAL-LEVEL PREDICTION

The choice of a cut-off point for any given criterion, as has been shown, may determine who is selected to fail or succeed, as can the choice of the measure of recidivism. Yet other choices are also consequential to the prediction of recidivism for the individual. One of the main themes that we have developed in our research has been that <u>choices</u> are frequently made by researchers among types of independent variables predicting recidivism. In this section we look at the extent to which choices among types of independent variables result in different predictions <u>at the individual level</u>. To accomplish this, we compare the predictions of seven models. Each model consists of a logistic regression equation in which only those variables representing each of the following domains are entered: sample selection bias hazards, social, presenting offense, anamnestic, juvenile career/onset, CJS/offender action, and general control variables.[24]

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[24] The particular variables representing each of these domains were discussed in Chapters Five and Six. As was the case above for the logistic regressions for being a high rate offender, we do not present the coefficients that lead to predictions based on each domain. Note that terms for interactions among independent variables are not considered in the analyses to follow. Of the

There are two basic reasons for looking at the individual-level predictions of recidivism based on each domain alone. First, it is widely believed that predicting recidivism using various subsets of independent variables makes little difference. Often such claims are made based only on the evaluation of the contribution to explained variance of a particular kind of predictor variable. By examining the predictions of separate models at the individual level (which we provide in greater detail here than is usually found in other research), it is possible to demonstrate more clearly how the predictions of success and failure are contingent upon the types of variables used to make those predictions.

Second, each domain's predictions can be compared to those of the others to identify both where offenders are classified similarly and how accurate those predictions are. This allows for some "structure" among the domains of predictor variables to be uncovered by finding overlaps among the predicted successes and failures of each domain. Although such structure may be found other ways, such as looking at the correlations among variables across domains, or evaluating the shared explained variance as discussed in Chapter Five, the results of an examination of the communality in predictions at the individual level may reveal more detailed patterns.

Note that the issue of when recidivism is predicted again becomes relevant. If the individual-level predictions of each domain are to be compared, the interest lies in the prediction of recidivism prior to the introduction of the sentence. This invalidates comparing domain-specific predictions with those of the full intervention model. Moreover, there is

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19 robust interactions identified in Chapter Five, all but one involve representatives of different domains. As these interactions are not domain exclusive, it is not possible to attribute their effects to only one domain.

little justification for an investigation of predicted success or failure based only on the use of the intervention independent variables of Chapter Seven. Our analytic strategy has consistently been one of looking for the possible effects of intervention variables only when the other control variables are in the equation.

In Table 9.3 results of a comparison of seven domain-specific models predicting rearrest at five years are presented. The intent of this table, and the seven that follow, is to compare the models' predictions of success and failure and to determine which domains are better than others when discrepant predictions are made. With seven different predictions of success and failure, there are 128 logically possible combinations of predictions across models. For some individuals the expectations across domains may all be the same. For other offenders, some domains may predict failure while the rest predict success. The empirical question is how frequent the 128 combinations are.

Succinctly summarizing the predictive overlap of the domain-specific models is not straightforward, especially when the accuracy of the predictions is also to be considered. It is easiest to key comparisons off the predictions of one model, and we use the hazard-based model, though any domain's model would be sufficient for this purpose. Thus, in Table 9.3, the left-most column under the heading "Hazard" is all "1's" as this model serves as the referent. When other domains make individual-level predictions that are <u>identical</u> to those of the hazard model, the agreement is represented by an additional "1" in the appropriate column. Hence, the first row of Table 9.3 summarizes those cases where all models make the same predictions, and this is represented by "1111111." The accuracy of the predictions can then be

assessed by determining the distribution of true negatives, false negatives, false positives, and true positives.[25]

When domain-specific models disagree on expectations for recidivism, some additional choices must be made. Again, the legend in Table 9.3 is keyed off the predictions of the referent hazard model. Thus, in the third row of the table, "1000000" represents those cases where the hazard model makes success and failure predictions that are at odds with those generated by all other domains. Similarly, "1110000" (the 26th row of Table 9.3) identifies those situations where the hazard, social structural, and presenting offense (PO) models yield the same predictions for recidivism, predictions that are the opposite of those found when the anamnestic, juvenile, prior CJS/offender action (CJS) or general control domains are used.

Given disagreements across domains, the accuracy of predictions becomes relative to the expectations of the referent model. Thus, for the predictive overlap identified by, say, "1001000" (the seventh row of Table 9.3), predictions that are "true" or "false" are defined relative to the recidivism

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[25] Following the findings from earlier in this chapter, we evaluate all the models' predictions using the cut-off points where the selection ratio equals the base rate. That is, percent selected to fail under each domain matches the percent that do fail. It will be remembered that this is a desirable strategy for positively skewed variables like those analyzed, as the choice of a cut-off equal to the mean simply results in many more false positives relative to true positives. By choosing cut-off points equal to the base rates of each of the dependent variables, we thus maximize the number of possible correct predictions for each domain. In addition, we investigated the use of the basic model (using all domains) as the referent for comparison with the predictions from each domain. It was generally the case that when most domains predicted failure or success for an individual, the basic model did as well. This is not surprising and the use of the more inclusive model as a referent would add little to demonstrating how the domains themselves make discrepant predictions of recidivism. Including a general model as an eighth model results in a slight reduction of the predictive overlap represented by the rows of these tables, leading to an increase in cases for the category we use as a residual. Thus some of the predictive consistency shown in these results tends to be lost if a model using the full set of independent variables is added to the analysis.

prognoses of the hazard and anamnestic domains. True negatives are then individuals who are predicted to succeed for these two models, and do so; false negatives are instances where both the anamnestic and hazard models predicted success for individuals who ultimately failed. The definitions of true and false positives are similarly geared to the predictions of the hazard and anamnestic models for this row. Note that implicit in this definition of accuracy for predictions is the converse where the predictions from the other five models are used to determine accuracy. Continuing to use the seventh row of Table 9.3 as an example, the implicit converse is "0110111." If either the social, presenting offense, juvenile, CJS, or control models are taken to define accuracy, the column headings for true negatives and false positives must be interchanged, as must those for false negatives and true positives. (This is true for all situations except that where all seven domains yield identical predictions. The column headings always apply when all seven domains agree.)

The notation using "1's" and "0's" provides a means of empirically demonstrating the consistency of individual-level recidivism predictions across the seven domains. Predictions overlap for models sharing a "1" in the legend of any row of Table 9.3, as they do for models having a "0" in common for a given row. However, there are still 64 logically possible combinations under this notation. We have chosen to further simplify the presentation by discussing only predictive overlap for which 100 (about .9% of the sample) or more individuals are involved. For rearrest at five years, this results in the 34 rows shown in Table 9.3. The other combinations of predictions have been combined into a residual category labeled "All Others."

The first row of Table 9.3 represents those predictions of the seven models for which there is unanimity regarding rearrest at five years. Twenty-one percent of the 11,749 cases are predicted to succeed (1,265) or to fail (1,215) by all the domain-specific models. This means that for 79% of these offenders, at least one model makes discrepant recidivism predictions and, more importantly, for these 79%, which domain is used matters for how their recidivism is predicted. By this general standard, there is little consistency across the seven domains as far more individual are not agreed upon than are similarly predicted by the seven domains. Yet, where there is complete agreement, predictions are quite accurate. Eighty four percent of the predictions for these 2480 individuals are correct. Predictions of success are slightly more accurate, as 85.5% are correct, compared to the 83% correct predictions of failure. Note too, that the percent of these cases correctly predicted is considerably higher than the 72.9% correctly predicted by the full model in Chapter Eight, (73% if selection bias is considered -- see Appendix B), or the 71% (72% adding hazards) correctly predicted when all the variables in the seven models presented here constitute one model as was developed in Chapter Five.

The residual "all other" category, line two of the table, represents all combinations of predictions with too few cases to warrant explicit discussion here. Empirically, these represent instances where the expectations of at least two models are at odds with the other. When aggregated together, these combinations yield rather poor predictions of rearrest at five years, when the expectations of the hazard model are used to define what is correct. Slightly less than 50% are correctly predicted (990 of 1992) across the 30 forms of overlap subsumed by this residual group.

Each combination of recidivism predictions is classified in the right-most column of Table 9.3 according to the number of models with a "1" or a "0" in each row. This facilitates the comparison of predictive overlap, as we can discuss all of those models of the same "type" together with a focus on the percentages of cases involved in various combinations. Note that we follow a convention of labeling the type of row with accuracy defined relative to the referent hazard model. Thus, for example, a type "2/5" row is one where the hazard model makes predictions similar to one other domain, and the true successes and failures are defined according to the predictions of those two models.

As noted above, for 21.1 percent of the cases there is complete agreement as to who will succeed or fail. When only one model departs from the predictions of all the others, some patterns emerge. In general, it is the rows with only one model at odds with the other six that involve the most cases. That is, those combinations of predictions where two or more models are discrepant from the others involve <u>fewer</u> cases than the rows where only one model is at odds with the rest.

For 9.8% of these individuals, only the PO model is in disagreement as can be seen in the "1101111" row. This form of type "6/1" overlap finds the presenting offense characteristics leading to predictions different from all other models and can be interpreted as indication of the "uniqueness" of the presenting offense model. For an additional 5.1 percent, it is the hazard model that is in disagreement from the others, and for 4.3 percent it is the general control model predictions that stand apart. Social structural variables yield predictions different from the other six models for 3,4% of the cases, while the anamnestic model makes unique predictions for 2.5 percent

of the cases. The expectations from the CJS model are atypical of all others 3.0% of the time and, basing recidivism predictions on only juvenile delinquency and onset variables yields distinct predictions for 1.2% of the sample.

Thus we find the following rank ordering of cases by domain when the predictions of one model depart from those of the others: presenting offense (1,157), hazard (603), general controls (510), social structural (399), CJS/offender action (357), anamnestic (298), and juvenile career/onset (137). This is an indication of the extent to which a particular domain is making different predictions relative to the other models' agreed-upon predictions. When the accuracy of these "solo" models decreases, (which we demonstrate below is true), this rank ordering is also an indication of the weakness of the model: the more cases predicted at odds with the other models, the weaker the model. The domains associated with the criminal career perspective are the better predictors, by this criterion, than are the other domains, such as presenting offense, social structural, or general control variables.[26]

The rows in which only one of the domains departs from the other six constitute about 29% of all cases with presenting offense and hazard models account for about half of them. Adding together this 29% with the 21% for whom there is unanimous agreement over recidivism prospects results in about 50% of the cases where there is little disagreement (i.e., one dissenting model). The predictions made for this half of the sample yield a relatively high rate of accuracy: 79.8% are either true positive or true negative which

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[26] Of course, this is partly circular, since the three domains constituting the criminal career perspective here are conceptually interrelated. Thus, it is not surprising to find that they yield predictions that are not as distinct from each other as they are from the predictions of other domains.

compares favorably to the base rate of 48% for all offenders. Thus, for half of the offenders, greater predictive success can be enjoyed when six or more domains yield the same expectations for recidivism. This high degree of overlap has identified a subgroup of offenders for whom quite accurate predictions of rearrest at five years can be made.

Also of interest in Table 9.3 is the extent to which a particular model does well or poorly relative to the other models, given the context of disagreement with the other models. That is, when there are different predictions across models, how accurate are the predictions? For example, in the "6/1" type rows in Table 9.3, when one domain is differentiated from the other six models, that domain is found to predict poorly. When the hazard model makes contrasting predictions, most of them (492 of 603) are wrong.[27] The social structural model, when in opposition to all of the other models, also does relatively poorly predicting failure: 82% of those predicted to fail by all other models save the social structural actually do fail. Given contrasting expectations from the other domains, the social structural model does slightly better in predicting success, though only 34% of those predicted to succeed by the social structural model actually do succeed.

The model using presenting offense variables, when at odds with the other models, does not fare well either. Row "1101111" shows that most (83%) of those predicted by the other models to fail do so, while the majority (81%) of the predicted successes are also accurate. The unique predictions of the anamnestic model ("1110111") do not fare quite so badly in that a relatively low 64% of the predictions of failure of the other models are correct, while

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[27] Note that the accuracy that is being assessed here is not a general one. These results are conditioned upon all other domains making opposite predictions.

72.4% of the predictions of success by the other models are correct when the anamnestic model predicts otherwise.

The juvenile career/onset model does somewhat better than the other models when its predictions are in opposition with the remaining six. Yet, here too there are more incorrect predictions than correct ones. While 53% of the "failure" predictions of the other models are correct, 71% of the "success" predictions of the other models are correct. Thus, there is some asymmetry in correct prediction of success or failure for those offenders whose juvenile career variables produce unique rearrest predictions. In the "1111101" line of Table 9.3, the predictions of the model of CJS/offender action differ from those of the other six, and again the result is poor predictive ability for the solo domain. About 70% of the other models' failure predictions are borne out, while about 82% of the success predictions involve no rearrest in five years. Finally, the general control domain, when its predictions are at odds with the other six models, (the last line of the table), also does poorly. About 70% of the failure predictions for the other six models are true, while 76% of the success predictions are not rearrested.

The accuracy found for the one domain that makes predictions discrepant from those of the other six is thus not very good.. Most of the predictions of each of the seven models, when they depart from the remaining six, are wrong, and these unique predictions result in little gain in predictive ability above the 48% base rate. Yet, for some domains, their discrepancy points to relatively poor predictions on the part of the other six. When the juvenile career/onset variables predict success when the other models predict failure, "only" 53% of the predictions of the other six models are correct. Similarly, when the anamnestic model predicts success and the other six models predict

failure, the other six models are correct in "only" 64% of the cases. This is one indication of the importance of the juvenile career/onset and anamnestic variables in predicting recidivism through rearrest by five years as these domains are able to identify those offenders for whom inaccurate predictions will be made by the other five. At the same time, it can be said that when six of the seven models agree in their predictions of success or failure, they are usually correct: aggregating the results from the six type "6/1" lines and the one type "1/6" line in the table reveals that 76.8% of the predictions are accurate.

Table 9.3 can also be used to analyze the extent to which pairs of models disagree with the remaining five models in the prediction of rearrest at five years. Five of the lines in Table 9.3 identify predictive overlaps of this form ("2/5") where the hazard model in conjunction with another domain yields discrepant predictions.[28] Once again, the predictions of the majority are superior to the predictions of the minority. The consistency of predictions from five models is less accurate, however, if either the anamnestic or social models pair with the hazard model to predict to the contrary. The accuracy achieved by the majority of domains is 54% and 58% in predicting failure in these instances. When the hazard and anamnestic models are odds with the remaining five, those five correctly predict success 66% of the time. Again, ignoring the discrepant predictions of the anamnestic and social structural models seems to lead to a reduction in the ability to predict rearrest at five years.

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[28] The hazard and juvenile models make predictions that contrast with those of the other five domains for fewer than 100 individuals.

Ten of the lines of Table 9.3 refer to individuals for whom two of the models (neither of which involve hazard variables) predict success when the other models predict failure. These lines are marked with a "5/2" in the model classification column of Table 9.3. Together, these constitute 13.5% of all cases, and thus this form of predictive communality is relatively frequent. In almost all cases, the predictions of the five models are superior to those of the two. The exceptions are combinations where social and general control models predict failure when the other models predict success, only 43% (35 out of 79) of the majority predictions are true negatives, or when the anamnestic and general control models both predict success while the other models predict failure. Here, 47% are true negatives. In all other combinations of this type, the majority predictions of recidivism are more often correct than incorrect. However, this accuracy rate is less than observed for combinations of models in which only one of the models is at odds in its predictions with the other six.

Only when juvenile career/onset and CJS models are in agreement with one another (and at odds with the other five domains) is the accuracy of the remaining five models above 70% for both predictions of rearrest and no rearrest. The social structural model in contrast along with the CJS model also results in relatively few accurate predictions (or 71% correct predictions of no rearrest for the other five models). The pair of models of presenting offense and CJS do poorly in predicting no rearrest when these expectations diverge from those of the other five domains, as 76% are correctly predicted by the remaining five models not to be rearrested. For all other combinations of this form of overlap, the percent correctly predicted falls below 70% for the five models that agree. Thus, having two

domains making predictions that contrast with those of the other five usually results in some attenuation in ability to predict on the part of the five models, relative to the accuracy rate when six or all seven agree. Yet, if the general control model is paired with either the anamnestic or social structural models in making a prediction at odds with the other five models, the correct prediction of failure or success is, respectively, 47% and 43%. Thus, a contrary prediction by some pairs of domains identifies subgroups for whom the other kinds of independent variables will not yield very accurate recidivism projections.

The remaining 10.6% of the cases in Table 9.3 pertain to situations where three domains make predictions that depart from the other four. Here too, the disagreement in recidivism expectations usually results in a relatively poor prediction of either success or failure. Where the majority of models are in agreement, however, they are more often correct than wrong in their predictions for 8 of 11 of the rows with these forms of overlap. There are three exceptions to this general pattern. One is that there are more true negatives than false negatives to the pattern of "1001001." Thus, when the hazard, anamnestic and general control models predict success at the same time as the other models predict failure, the threesome is usually correct (61% of the time). In the combination of "1100110," there are more false positives than true positives (32 versus 21) when hazard, social structural, juvenile career/onset and CJS/offender action models all predict failure and the other three models expect success. For the pattern of "1110001" (anamnestic, juvenile career/onset, and CJS/offender action predicting similarly), there are more false negatives than true negatives when these three domains are used to define correct predictions.

In general though, most of the predictions of the majority of the "3/4" or "4/3" overlaps are correct in the 50 to 65% range, and none of them exceeds 70% no matter which set of predictions is used to define accuracy. It is the anamnestic model, when in combination with two other models, that is involved in the exceptions to this general pattern. When either the anamnestic model alone or the anamnestic and general control models are involved in a "minority" prediction, the predictions of the majority of models proves less accurate. When the anamnestic and general control models are in agreement with those of two other domains, the percent correctly predicted is higher. This centrality for the anamnestic and general control domains can be seen in the fact that nine of the eleven combinations of the "3/4" or "4/3" types involve these two domains.

The analysis of the overlap in individual-level predictions for rearrest at five years has found more differences than similarities across the seven domains. For 79% of these offenders, the expectations of at least one domain depart from those of the others. In general, the greater the discrepancies (i.e., the higher the proportion of models not agreeing), the less accurate the expectations of success or failure. Faced with these competing predictions, some domains (e.g., anamnestic, general controls) seem to have more predictive utility, even when those expectations are in the minority. It is unclear, however, how these results might generalize to other forms of recidivism measured over other windows. Rearrest at five years has the highest base rate of the variables studied here, leading to the least skew for the predicted probabilities of recidivism. Consequently, we defer interpretations of these findings until other dependent variables have been considered.

Rearrest In One Year.

The analytic procedures just described are now applied to the dependent variable of rearrest at one year. It will be remembered that the base rate here is 23%, and thus there will be a proportionately higher percentage of "negatives" predicted, and correspondingly few "positives" relative to rearrest at five years. Indeed, 4,272 (36%) out of 11,714 individuals at risk for one year are predicted not to fail by all models and do not fail. Only 111 offenders are agreed upon by all models as likely to fail, and 70 (less than 1% of the entire sample) of them do for a 63% accuracy rate. Thus, unlike the situation with the dependent variable of rearrest a five years, where the base rate was 48%, relatively few true positives are identified, with a corresponding reduction in the accuracy rate, (although relative to the lower base rate of .23, the success rate is arguably higher here). At the same time, where all seven models agree that the offender is unlikely to be rearrested within one year, 90% of the predictions are borne out. This reaffirms the asymmetry in the accuracy of predicting success relative to failure for recidivism variables with low base rates: success is predicted more often, and with greater accuracy, than it is when the recidivism event is not as common as it was for the five-year follow-up measure.[29]

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[29] The percent correctly predicted to fail is about 83% when the base rate is 48% for rearrest at five years, while here the percent correctly predicted to fail is 63% when the base rate is only 23%. Arguably the latter result represents "better prediction" than the former as the absolute increase over the base rate is higher. RIOC, however, is higher for rearrest at five years than for rearrest at one year using only that subset of cases for which all domains make identical recidivism predictions, but note that this "base rate" is conditionally defined by the percentage of individuals in the row who fail. Furthermore, the prediction of success or failure must be evaluated relative to some criterion. If that criterion is simply the percent correctly predicted, then the conditional prediction of success when there is a low base rate is quite good here, while the conditional prediction of failure is quite poor. If the criterion is relative to the observed base rate, then the prediction of failure

One consequence of the lower base rate for rearrest at one year is that now all seven domains make identical predictions for 41.1% of the cases. This is nearly twice the level of unanimity seen for rearrest at five years. Overall, fewer kinds of predictive overlap involving 100 or more individuals are observed, and there is about a five percent increase in instances of those forms of communality involving relatively few offenders.

The residual category for Table 9.4 covers 23.4% of the cases. These represent pairs of combinations of models for which fewer than 100 individuals were involved. In general, the accuracy rate, again defined relative to the predictions of the hazard model, of these combinations is low. About 63% are predicted to fail and do not, while 62% of those predicted to succeed are true negatives. Thus, the success at predicting no rearrest is once again substantially lower for the combinations of predictions constituting the residual category, than is the case when all the models agree.

As with rearrest at five years, all seven combinations in which one of the domains is at odds with the other six involve more than 100 individuals, though the percentage of cases falling into each group is somewhat smaller. The rank ordering of domains by numbers of individuals uniquely predicted is: general controls (558), presenting offense (501), hazard (451), social structural (385), juvenile career/onset (302), CJS/offender action (268), and anamnestic (181). This follows the same general pattern found for rearrest at five years in that criminal career domains make predictions that are less frequently at odds with the predictions of all the other models.

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here is arguably better than the prediction of success. The percent correctly predicted is "only" 90% when 77% actually succeed, compared to 63 percent predicted accurately to fail against a base rate of 23%.

Almost sixty percent (about 59.5%) are now subsumed by unanimous predictions or the near unanimity found when only one domain makes discrepant predictions. This compares to about half of the cases for rearrest at five years. When all seven models agree, the accuracy rate is about 90% for identifying true negatives, while when six of the seven models predict no rearrest, the correct predictions fall into a range of from .63 to .84. The hazard model alone has a 63% false negative rate (34 out of 54), or, alternatively, the other six models enjoy a relatively modest true positive rate of 63% when only the hazard model predicts differently. All the other "6/1" combinations fare worse in their predictions of failure. That is, the combinations of six domains in agreement when predicting success are generally in the upper 70's to low 80's in terms of percent correctly predicted to be true negatives. Thus, the unique expectations of the hazard model may be less important in the prediction of rearrest at one year than at five years. The consistency of six models in agreement that best predicts success is the one that differs from the general control model. Here, 422 of 498 predicted to succeed do so for an 84% accuracy rate. Still, while the accuracy rate of predicting no rearrest at one year is relatively high, it is not as high as is the case when all seven models agree.

The prediction of rearrest at one year is not as successful in general as the prediction of no rearrest. The percent predicted to fail by a concurring six models is in the low 60's for most combinations. Thus, although prediction is more often correct than wrong in the case of one dissenting model, the successful prediction of rearrest at one year is well below the successful prediction of <u>no</u> rearrest within one year. On the other hand, relative to the base rate expectations of 77% succeeding and 23%

failing, the combinations of models predicting success are generally only a little better in predicting success than would be the case if all the individuals were predicted to succeed. Relative to the prediction that all individuals will fail, the prediction of rearrest within one year by the concurring six models is rather impressive. Thus, again we are reminded of the seductive appearance of predictive success associated with the modeling of relatively common events (no rearrest) to uncommon events (rearrest within a year). It should also be noted that relatively few individuals are predicted to fail by the "1/6" and "6/1" forms of predictive communality: 373 out of 11,714 or 3.2% of the entire sample. Stated another way, 373 individuals are predicted to fail when six models agree, and this is out of 2,694 who do actually do fail, or 13.8%. However, "only" 225 of the 373 (60%) predicted to fail do, or 8% of all those offenders rearrested in one year. Thus, these models identify few of the failures in the sample.[30]

Relatively few cases are involved in the forms types of overlap for rearrest at one year, and compared to the findings for rearrest at five years, not many combinations of predictions meet our inclusion criterion. Only 12 other rows involve more than 100 cases, and ten of them involve splits of "2/5" or "5/2". These types constitute 15.2% of the sample.

As before, it is found that, in general, the majority of five models predicts more successfully than the minority of two. There are several exceptions, however. In terms of predicting success, the "2/5" type involving the hazard and social models predicts success very well -- there are as many false negatives as true negatives when we would expect more false negatives

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[30] Remember that the greater success in predicting in concert with other models is attributable in part to the low base rate of the dependent variable: 77% would be predicted accurately if all models predicted only success.

than true negatives. There are three instances where the majority of five models does poorly in predicting failure: when PO and general controls, anamnestic and CJS/offender action, or juvenile career/onset and general controls are at odds with the other five models. Here, the majority of five models make more false positive than true positive prognostications. At the same time, the percent correctly predicted to fail is higher than the 23% base rate of failure for the full sample: 47%, 29%, and 43% respectively. Thus, relative to the base rate of failure for the full sample, each of these types does better. Still, this accuracy is not as high as when the majority predictions of the five models are used as accuracy increases to above 50%. Also, it is interesting to note that for these combinations of "2/5" and "5/2," it is when the general control model and one other model are at odds with the other five that two of the three lowest accuracy rates are observed for the majority of models. Thus, the importance of the general control variables for predicting rearrest at one year is suggested by the results here.

Only two of the combinations of models listed in Table 9.4 involve splits of "3/4" or "4/3" -- "1010010" and "1110000." In both cases there are far more false positives than true positives, by a 4 to 1 ratio. There are also slightly more false negatives than true negatives. The majority of models is relatively good at predicting successes (roughly 80% correct) and not as good in predicting failures, although these majority predictions of failure are still accurate slightly more than a 50% of the time. From these results, it seems clear that the hazard and presenting offense models are relatively weak, along with the social structural and CJS/offender action models, when their predictions are at odds with exactly four other domains.

In summary, the analysis of the models predicting rearrest at one year finds many results comparable to rearrest at five years. Which set of independent variables is used to make predictions has consequences for individual-level expectations: for the minority of the sample (41.1%), all models make the same predictions. Given contrasting expectations, it is usually the case that the predictions of the majority of models are more accurate than those of the minority. However, the relatively low base rate for rearrest at one year has led to fewer types of overlap, and to a greater overall accuracy for the predictions.

Arrest for Persons Offense in Five Years.

Recidivism through an arrest for persons offense within five years, with a base rate of .20, is analyzed in Table 9.5 In general, we see a pattern similar to that found for rearrest at one year. About 36% of the predictions are identical across all seven models, and thus most individuals can be predicted differently depending upon the domain used. Again, predictions of success (no rearrest for persons crimes) are more accurate than predictions of failure: 93% versus 52%. The residual category for forms of overlap involves 21.1% of the individuals. Here, the predictions defined by the hazard model are not very accurate, although they are more accurate for success than for failure. There about 2 false positives for every true positive.

Instances where the predictions of one model contrast with those of the other six again involve a higher percentage of the cases, now 20%. (Note that the anamnestic model predicting differently from all the others did not meet the criterion of involving more than 100 cases.) The discrepant model involving the most individuals is the CJS/offender action model. Its predictions are unique, and quite inaccurate, for 10.1% of the sample. The

remaining domains are rank ordered by percentage of unique predictions as general controls, hazard, social structural, and juvenile. Thus the results differ somewhat from what was observed earlier for the other dependent variables. Here, a "criminal career" domain, the CJS/offender action model, frequently makes predictions at odds with the other six models. At the same time, the anamnestic model is not frequently at odds with all of the other six models in that too few cases are involved to meet the inclusion criterion.

Once again, when only one model makes atypical predictions, they are usually incorrect. This is especially true when the expectations of the CJS model depart from those of the other six. The majority correctly predicts success 85.7% of the time in this instance. Also of note is the situation where the hazard model makes unique predictions. A disproportionately large number of failures are involved, about 11% of all those failing through rearrest via a persons crime within five years. When the hazard model stands apart, the accuracy rate of the majority is 88.1% for the positives that are predicted.

As before, the combinations involving two models at odds with the other five models, account for the next highest number of cases: all but three of the remaining rows in the table. Again, the majority of models is most often correct in its predictions of success: except when the hazard and social structural models make contrary predictions. There, false negatives and true negatives are about equal. The majority models are usually more often wrong than right in their predictions of failure, and correctly predict above the base rate of 20%. However, relatively few individuals are predicted to fail at all by the "5/2" and "2/5" forms of predictive communality. It is only when the hazard and CJS models are unique that we find many predictions of

failures.

Only two "3/4" or "4/3" combinations pertain to more than a hundred individuals. These differ in that agreement between the hazard, anamnestic and CJS/offender action models involves mainly predictions of failure: 104 individuals are predicted to fail by these three models while the other four predict success. This minority of models is usually wrong -- 78 individuals are false positives. Thus, when anamnestic, CJS/offender action, and hazard models are at odds with the other four models, relatively many offenders are predicted to fail, but with limited accuracy. In contrast, the other "4/3" type is when anamnestic, juvenile, and CJS models (the "criminal career" models) depart from the other four. At issue now are predictions of success, and the majority is more accurate. As before, when mixed combinations of "3/4" or "4/3" are found, the majority of models generally performs better than the minority, and the ability to predict is attenuated relative to the accuracy generally found for the majority domains when five or more are in agreement.

The analysis of overlap using the prediction of rearrest for person's offenses at five years reveals patterns similar to those found for other dependent variables. Most individuals are not predicted unanimously by all models, and combinations with one discrepant model involve a high percentage of cases. As was found with other skewed dependent variables, few individuals are successfully predicted to fail, relative to those predicted not to fail, as the number of true negatives far outweighs the number of true positives. While the accuracy of failure predictions across various forms of overlap is substantially higher than the base rate, these predictions are more often wrong than they are correct. Some nuances are seen, however, when recidivism

is measured by a persons arrest within five years. The criminal career models overlap in their predictions less than was the case earlier. Predictions of success and failure also seem to be more contrasted by the forms of communality as some divergent expectations center primarily around predictions of failure, while others are mainly disagreements over predicted success.

Rearrest for Persons Offenses at One Year.

In Table 9.6 the predictive overlap for rearrest for a persons offense at one year is presented. Here, the base rate is only 7% which is the next to lowest for recidivism measures considered in this chapter. Recidivism predictions are unanimous for 70% of the sample and, surprisingly, no individual is predicted to fail by all seven domains. All the unanimous predictions are for success, and 96% of the predictions are correct. The residual category of "all other" accounts for 12.2% of the cases. Here, there are a substantial number of failures predicted by the referent hazard model (521), but only 84 of them are "true positive" for a 16% success rate, which is still an improvement over the base rate guess of 7%.

Only types with one discrepant model involve 100 or more cases. Thus, there is unanimity or near unanimity in the recidivism predictions for about 88% of the sample. These predictions enjoy an accuracy rate of about 94.5% which is only slightly better than that which would be found if no one were predicted to fail. When one domain differs from the other six, accuracy rates for the majority are in the 80-90 percent range. The models making discrepant predictions can be rank ordered by frequency as follows: general controls, presenting offense, hazard, CJS/offender action, juvenile career/onset, social structural, and anamnestic, respectively. Once again, the criminal career models involve fewer unique individual-level predictions, although the social

structural model also leads to relatively few contrary predictions for rearrest for a persons crime within one year.

The extreme skew in this form of recidivism has reduced the extent to which the domains make divergent predictions. However, the general patterns from less skewed dependent variables are still evident. Accuracy is higher when the majority of domains agree, though it is still less than when unanimous predictions are made. Given a disagreement between domains, it is only when the hazard model is unique that expected failures are identified.

Reimprisonment at Five Years.

In Table 9.7, the comparison of individual-level predictions for reimprisonment at five years is presented. Here the base rate is 18.6%. About 48% of the individuals are predicted unanimously, and most of them (5502) are predicted not to be reimprisoned. These turn out to be true negatives 96% of the time. Of the 74 individuals predicted unanimously to fail, 72% do fail. The residual category accounts here for 25.6% of the cases, which is relatively large when compared to earlier tables. The same pattern of accuracy observed for previous dependent variables with low base rates is also found: modest accuracy in predicting no failure, and more false positives than true positives when failure is predicted by the referent hazard model.

Instances of near unanimity involve 21.9% of the sample. The domains are now ranked by frequency of unique predictions as follows: general controls, presenting offense, hazard, social structure, CJS/offender action, juvenile career/onset, and anamnestic. Thus, the rank ordering is generally similar to that observed before as criminal career domains involve the least discrepant predictions from the majority of models.

Across all these forms of predictive overlap, the majority predicts success rather well, and failure relatively poorly. The percentage of correct "no reimprisonment" predictions are in the 80-90% range. Relatively few individuals are predicted to be reimprisoned by the majority of domains given one discrepant model, though note the large percentage of cases where the hazard model alone predicts that the offender be reimprisoned. In most of these instances, there are more true positives than false positives. The one exception involves the anamnestic model being discrepant with the others. Here, only 10 individuals are predicted by the majority of models to fail. Aggregating those predicted unanimously, with those predicted by all but one of the domains results in 69% of the individuals being predicted to succeed or fail similarly across these combinations.

Relative to the "6/1" types, there are few cases where two domains predict differently from the other five. In all instances, the majority predictions are better than those of the minority of two, both for the predictions of success and failure. The majority does particularly well when the domains of either the hazard and presenting offense, or presenting offense and general controls, are in the minority. The more extreme forms of discrepant predictions are not found as there are no "3/4" or "4/3" types involving 100 or more individuals.

The predictive overlap for reimprisonment at five years has revealed little that is new. Most of the individuals are not predicted unanimously by all models. Prediction of the rare event, failure, is made less often and with less accuracy, than is prediction of success. The domains associated with the criminal career perspective tend to produce the least discrepant predictions. Finally, the hazard model, either by itself or in conjunction

with just the presenting offense model, produces particularly poor predictions of reimprisonment.

Reimprisonment at One Year.

In Table 9.8 the communalities for prediction of reimprisonment at one year are presented. This form of recidivism has the lowest base rate -- 5.6% -- of all those considered in this chapter. The resulting severe skew in the predicted probabilities has the, by now familiar, consequences for the manner in which the domain-specific predictions overlap. Virtually no one is predicted to be reimprisoned within one year. Of the 8808 unanimous predictions, all are for success, and most (97.5%) of them are correct. No one is unanimously predicted by all models to fail. The residual category contains a relatively low 11.1% of the cases, and here again success is predicted more frequently, and accurately, than failure.

The juvenile career/onset domain makes predictions that are unique from the other six for fewer than 100 individuals. For the remaining situations where the expectations of one domain contrast with those of the other six, between 85 and 93% of these prediction are "true negatives". The hazard models, when standing apart from the others, predicts only failures, and these expectations are quite inaccurate. Otherwise, there are too few predictions of failure by the majority of these combinations of models to warrant discussion. The number of individuals predicted uniquely by domain yields a rank ordering of general controls, presenting offense, social structural, hazard, CJS/offender action, and anamnestic models. Thus, once again, the models associated with the criminal career perspective involve the fewest discrepant individual-level predictions.

Only one "5/2" type involves at least .9% of the sample. Where general control and juvenile career/onset models predict differently from the majority, success is predicted quite well (85%) by the five majority models. Overall, the results for reimprisonment at one year are remarkably similar to those for a persons rearrest at one year. The conclusion to be reached is that how these domains compare in their predictions of recidivism is a function of the base rates of the recidivism variables.

High Rate Offenders.

The base rate for being a high rate offender has been defined, a priori, to be .10 for both the one and five-year windows. The overlap across domains found when this definition is used for the longer window is shown in Table 9.9. The models are unanimous in their predictions for 63.9% of the sample. Of the 7506 individuals predicted not to become such offenders by all the models, 7263 do not for a 96.8% rate of accuracy. Only 2 individuals are predicted to be high-rate offenders by all seven domains, and neither become such offenders. The residual category includes another 16.6% of the cases. Across these forms of communality there are more false positives than true positives, and more true negatives than false negatives when the predictions of the hazard model are used to define accuracy.

As was seen earlier, the criminal career domains are involved in the fewest unique predictions when the "1/6" and "6/1" types are rank ordered by frequency. In all instances, the majority of models is quite accurate in predicting success, ranging from 83% to 90% correct predictions. Again, too few cases of predictions of failure are observed to discuss. Only one "5/2" type involves more than 100 individuals, and it consists of the anamnestic and CJS/offender action models predicting at odds with the remaining five. The

percentage of correct "negative" predictions of the majority of models is "only" 70%, again suggesting some support for the argument that the criminal career models are important.

Overall, the prediction of high rate offenders at five years is dissimilar to the prediction of other dependent variables at five years: it is more skewed and, possibly for this reason, the results look more like the results obtained for some one-year follow-up dependent variables. Failure is seldom predicted by the combinations of models evaluated in the table. The prediction of success, however, is generally similar to that discussed above for other skewed dependent variables. Where most models agree, success can be correctly predicted 80 to 90 percent of the time.

In Table 9.10, the results for being a high rate offender after one year are presented. Given that the base rate has been defined to be identical to that of high rate offenders at five years, it is not surprising to find that Table 9.10 mirrors the previous one. In total, 62.5 percent of the individuals are predicted unanimously by the domains and all but three individuals are predicted to succeed. Of that number, about 96% do succeed. Another 16.9 percent fall into the residual "all other" category.

A small difference in the rank ordering of unique predictions exists, but it is still true that the criminal career domains yield the fewest discrepant predictions when one domain disagrees with the other six. The prediction of success by the majority of models is also similar to what was observed for high rate offenders at five years: 80 to 90% are correctly predicted. Only one "2/5" type is found in Table 9.10 and it differs from that identified in Table 9.9. Where the hazard and presenting offense models predict at odds with the other five models, the prediction of the majority of

models is quite good.

Thus, the prediction of high rate offenders at one year is not unlike the prediction of high rate offenders at five years. Most cases are predicted unanimously by all models to succeed. Success is predicted more often, and with greater accuracy, than is failure by the other predictive overlaps examined here. Criminal career domains are again less frequently involved in predictions that are discrepant from the majority of models. The predictions of the hazard model, when contrasted against those of the majority of other domains, tends to produce an inordinate amount of false positives.

The Predictive Communality of Domains.

We undertook this investigation into how the domains diverge in their expectations for two reasons. First, it provides a convenient way of studying how the choice of independent variables matters, both for any accuracy in the predictions and for the individuals whose subsequent criminal behavior is being predicted. Second, our analyses here has offered a look into the structure underlying individual-level predictions of recidivism. To the extent that the models representing the various domains of independent variables have made comparable predictions, some structure has been identified.

Any conclusions reached, and the generalizability of those claims, is clearly contingent on the base rates of the measures used to represent "recidivism." As the base rates of the recidivistic events decrease, predictions of success become likely, no matter how recidivism is defined. Lower base rates are accompanied by more accurate predictions, primarily because the percentage of true negatives increases dramatically. In the extreme case of very low base rates, predicting all individuals to succeed

will yield extremely accurate individual-level predictions, with little consequence for either the structure of predictions in common across domains or for the choice of domains to be used for those predictions.

Given this caveat, some consistencies are apparent from the results found in this section. The models based on the various domains do not always make identical predictions for the recidivism prospects of individuals. While it is true that the predictive communalities representing the unanimous agreement across domains constitute the most frequent form for all recidivism variables used here, these percentages are surprisingly low, and the higher the base rate, the fewer the number of individuals for whom all domains offer identical recidivism projections.[31] Table 9.11 summarizes this finding. Thus, for at least 25% of these offenders (using reimprisonment within one year), and up to 89% (rearrest for any crime at five years), which domain is used to predict their recidivism can matter greatly. The selective use of independent variables can lead to differing predictions of individual-level recidivism depending on the domain chosen.

Given the lack of unanimity in the likelihood that a particular individual will recidivate, there are several conclusions that can be made about the structure of divergent predictions. More often than not, it is the case that the predictions of only one domain will depart from those of the others. Thus, there is still a high degree of consistency in predictions across domains. The vast majority of the remaining competing predictions involve instances where the expectations under two domains differ from those found if the other five are used. It is only when recidivism is quite

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[31] Note that we have not analyzed any recidivism measures with base rates above .5. As the base rate rises above .5, agreement across domains would undoubtedly increase.

frequent (i.e, the base rate approaches .5), that we find these domains to have a considerable difficulty in sorting out the likely successes from the likely failures. Overlaps having the form of three domains predicting the opposite of the other four are empirically infrequent.

The more conceptually similar the domains, (e.g., the criminal career domains of anamnestic, CJS, juvenile career independent variables) the more likely these domains are to make comparable recidivism predictions and the less likely these variables are to make recidivism predictions that are discrepant from those of other domains. Conversely, the predictions of the sample selection bias hazard model, the model using characteristics of the presenting offense, and the general controls model tend to stand out in making contrary predictions for individual-level recidivism.

The consistency with which these domains predict individual-level recidivism is mirrored in the accuracy of those predictions. In general, the greater the proportion of domains agreeing on the success or failure of an offender, the more accurate those expectations are. The anamnestic set of independent variables stands out as one which is quite often accurate in the prediction of failure, particularly when consistent with one or more other domains. When discrepant predictions are found, the majority of domains usually make more accurate predictions than do the domains falling in the minority. Examples of the minority of domains providing better predictions of success and failure are empirically rare, though they have been noted in the discussions above.

The subgroup of individuals for whom all domains make identical predictions is intriguing. By definition, it does not matter which domain is used to predict for this group. Yet the most accurate recidivism predictions

are consistently found for these offenders. As can be seen in Table 9.11, when all domains are combined into our basic model of Chapter Five, or when selection bias hazards are included (Appendix B), recidivism is accurately predicted for between 73 and 94 percent of these offenders. The predictive ability of these combined models rises with the base rate, as does the accuracy for each domain. When the domains make identical predictions, much higher levels of accuracy are achieved. Thus, it may be better to predict success and failure with multiple models based upon different domains, and see for whom there is substantial agreement, than it is to predict success and failure using a single-equation model.

The accuracy found when domain-specific models make unanimous predictions suggests to us that there is a core group of offenders for whom recidivism prospects are relatively certain. These individuals are either quite likely to recidivate or quite likely to fail, and it does not matter which variables are used to make these predictions. The recommended interventions and treatments for this subset of offenders may be clearest. Moreover, the expectations for this core group set the upper limits for how accurate predictions of individual-level recidivism can be. As we have seen, when base rates are low, the expectations for this group may be only for success.

The contrasting predictions across domains can thus be taken as an indication of the uncertainty surrounding the future behavior for the remaining subgroup of individuals. Their recidivism prospects are less clear, though more certain when greater numbers of domains make comparable predictions. The accuracy of predictions for this group will be lower, but still higher than that suggested by the base rates. Of greater interest is

the fact that where domains diverge has some diagnostic utility. For those forms of recidivism with low base rates, we have seen that predictions of failure are uncommon, and the proportions of failures correctly targeted are relatively low. Yet, when the predictions of some domains diverge from the others, most notably when the processing of the case through the criminal justice system -- the hazard model -- suggests failure while the other domains predict success, the likely failures can be pinpointed more accurately.

To summarize, our study of predictive communality across domains has made more evident than was the case in the earlier chapters, the considerable difference it can make to select among the domains of predictor variables in generating recidivism predictions. Although this may seem to be an "obvious" point to some observers, it is one that seems to be discounted, minimized, or ignored in recidivism prediction research. Yet, as the use of individual-level predictions takes on greater importance for intervention decisions (see Chapter Ten), there are increased consequences for the selection of variables that are used to make such predictions.

ADJUSTING PREDICTIONS FOR RACE, ETHNICITY, AND GENDER

We have just seen that "it matters" which independent variables are used in making assessments of recidivism risk. We continue this line of inquiry here, now focussing on some of the variables representing the social structural domain. In particular, variables measuring the offender's race, ethnicity, and sex are explicitly considered. While we have interpreted the effects of these variables as indicative of other, unmeasured, aspects of the individual's position in the social structure, using them to predict the recidivism of offenders is viewed by some as objectionable. Thus, we examine

the extent to which it makes a difference for individual-level predictions to "leave in" or "remove" these attributes when predicting recidivism.

Our analysis follows the work of Goldkamp (1987) on the use of variables such as race, ethnicity and gender in models of recidivism. As was discussed in Chapter Two, the argument is that it is better to control statistically for these variables than it is to omit them. Omission allows some of their effects on recidivism to be reflected in other variables, thus possibly introducing bias into the parameters estimated for those other variables. This can also lead to "disguised" differential treatment when interventions are based on these other factors. Goldkamp suggests an alternative of leaving variables like race in the model, estimating their effects, and using the model's coefficients for all other variables to predict success or failure: when individual predictions are made, all individuals are "equated" on racial, ethnic, and gender characteristics by ignoring these variables.

Following this idea, we investigate the implications of treating everyone as if they were a white male when predicting the recidivism of individuals. For example, for rearrest at five years, we take the logistic regression coefficients for the full model, omit those coefficients involving race, ethnicity, gender, and any interaction terms involving these items, and re-calculate the probability of success or failure. The predictions for recidivism can then be compared with those obtained when such items are retained in the individual prediction model. Doing so removes the effects of these variables from the other variables' regression coefficients, while imposing the assumption that there is no variance in race, ethnicity, or

gender.[32]

The results for the prediction of recidivism at the individual level are presented in Table 9.12. The same eight forms of recidivism used in earlier analyses are utilized here. Across the top of the table are the eight model's predictions of success or failure when all individuals are treated as white males. We call these the adjusted models.[33] Down the side of the table are the predictions from the full model with black, Hispanic, and gender coefficients utilized in the calculation of the probability of success or failure. These are the same predictions used earlier. We call these the unadjusted models.

If the predictions of the adjusted model are to be compared to those from the unadjusted model, the issue of cut-off points is once again raised. We argued earlier that the choice of a cut-off such that the selection ratio equals the base rate is most appropriate. By extension, predicted success and failure for the adjusted model should be defined relative to the predicted probability distributions of that model. For example, whether or not an offender is predicted to be a high rate offender should be determined by whether he/she falls into the top ten percent of the adjusted model's predicted probabilities for that variable. Thus, the comparisons in Table

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[32] We could have used white females, following Goldkamp (1987) who argues that the categories chosen may be selected to reflect the least likelihood of intervention, or the smallest degree of intervention, on the part of the CJS.

[33] The adjusted models simply consist of the full models of Chapter Eight minus the terms for the variables and interactions involving black, Hispanic, and gender. Those variables are included in the full intervention model. Note that the parameters of the full model are somewhat different than would be the case if they were estimated without using any racial, ethnic, and gender variables. Thus, Table 9.12 informs us about how the adjustment influences predicted levels of recidivism and not about how adjusted predictions differ from those obtained when the variables in question are omitted entirely.

9.12 define success and failure relative to different cut-off points for the adjusted and unadjusted models.[34]

A quick glance at the numbers of individuals predicted to fail shows that relatively many are predicted differently across the two forms of models. In the upper left-most cells of Table 9.12 are the comparisons for rearrest at five years. The models agree on 10,850 (92.3%) of the 11,749 cases. For 7.7% of these individuals, the adjustment produces a difference in their designation. These changes are symmetric, though this is mainly a consequence of using different cut-off points for the two models. The adjustment produces a slightly higher conditional percentage of change for those individuals predicted to fail by the unadjusted model. The 450 offenders who are expected to fail under the full unadjusted model but are not predicted to fail when these adjustments are made represent 8.0% of the full model's predictions of failure.

The results found for rearrest at five years reveal general patterns that hold across all the recidivism forms presented in the table. The adjustments shift approximately equal numbers of individuals from designations of expected failure to predicted success and visa versa. While the black, female, and Hispanic variables add to the predicted probability of failure

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[34] It is also legitimate to use the procedures derived from Goldkamp (1987) to investigate how recidivism predictions change when the cut-off points from the unadjusted models are applied to the probabilities predicted under the adjusted model. We have done so and, in general, have found fewer individuals are shifted as a consequence of making the adjustment. The vast majority of these shifts take the expected form: individuals who are expected to fail under the full unadjusted model are predicted to succeed by the adjusted model. How many individuals are affected by the adjustment depends on the particular measure of recidivism and the window used to measure it. The most dramatic change was found for reimprisonment within five years, where 66.4% of those expected to fail by the unadjusted model were expected to succeed by the adjusted model. Other changes of this kind involved fewer cases, ranging from 6.8% for imprisonment within one year to 32% for rearrest for a persons offense over either window.

across indicators of recidivism, when failure and success are re-calibrated against the predicted probabilities found when these variables are ignored, as many "new" expected failures are found as are "new" successes.

How much of a difference the adjustment makes depends upon what is used to measure recidivism and when it is measured. The largest discrepancies are found for the five-year follow-up periods. When the five-year recidivism projections are adjusted for these variables, a greater percentage of cases are shifted. This is to be expected given the results of Chapters Five and Six where we saw an increase in the coefficients for social structural variables as the length of the post-sentence observation window increased. Thus, for rearrest we find 7.7% of all cases shifted at five years, compared to 4.8% at one year. When an arrest for persons crime indicates recidivism, 6.8% and 3.1% of the individuals are classified differently as a consequence of the adjustment at five and one year, respectively. The use of an imprisonment recidivism measure finds 4.3% (five years) and 1.7% (one year) being classified differently across the two models. The predicted success and failure for high rate offenders shift for 3.1% of the cases at five years and 2.2% at one year.

The differences observed across forms of recidivism tend to be tied to the underlying base rates of those measures. When base rates are high, more cases are classified differently as a consequence of the adjustment than are shifted when the recidivism measure has a low base rate. But, the changes seen for a persons rearrest at five years (6.8% of all predictions changed) with a base rate of .20 are greater than those found for rearrest for any crime at one year (4.8% changed) which has a base rate of .23. Rearrest for a persons crime and imprisonment at year both have a base rate of about .7, but

over twice as many individuals are changed by the adjustment for the former than for the latter (4.3% as opposed to 1.7%). Thus, the adjustment for race, ethnicity, and gender does not operate similarly on all types of recidivism, and affects more cases for some forms of recidivism than others.

The variables being adjusted for present legal and ethical problems when used to determine the degree of punishment for an individual. This makes shifts away from the unadjusted model's predictions of failure of arguably more import than those where offenders become expected failures as a consequence of the adjustment. Using this criterion, the conditional percentage changed, given an unadjusted prediction of failure, becomes relevant. Reading down the diagonal of Table 9.12, we find these conditional shifts to involve 8.0%, 10.6%, 17.0%, 21.2%, 11.6%, 15.2%, 15.3%, and 11.5% of the offenders predicted to fail by the full model. For the more serious forms of recidivism (i.e., rearrest for a violent persons crime, imprisonment), there is a more dramatic impact for the adjustment if only unadjusted predictions of failure are considered.

The results here reflect, of course, the relatively large coefficients observed in the regression results presented in earlier chapters for the variables of black, Hispanic, and female. They are among the strongest and most consistent predictors of recidivism across various models, and constitute a large part of the social structural domain effects. It is not surprising that "removing" those effects from the individual prediction models has the impact seen in Table 9.12. However, it should not be assumed that all of the shifts observed involve only minorities, and it is even less likely that the "new" predicted successes under the adjusted model are all minorities while the "new" predicted failures are all white males. While the coefficient for

being an Hispanic increases the predicted probability of recidivism, the coefficient for being a female leads to a lower probability, though the interaction between being female and prior Part I charges leads to a higher probability being predicted. Many of the effects removed by the adjustment are beneficial for the predicted level of recidivism for blacks. Three of the interaction terms deleted by the adjustment (two between main effects, one sanction interaction) lead to expectations of less recidivism for blacks. However, by looking at the numbers of individuals who are predicted differently, we can more clearly point to how many individuals would be affected by ignoring these kind of effects.

It should also be noted that the "difference it makes" is, in part, a function of the shape of the distribution of the probabilities of failure. If there are a large number of cases just above the cut-off probability defining failure for the unadjusted model, and many of these cases involve minorities, their probabilities of failure may be reduced enough to "drop" them below the cut-off threshold. We have effectively minimized the likelihood of this being a factor by selecting a cut-off point such that the selection ratio equals the base rate: the univariate distributions of the probabilities of failure for most of these dependent variables are all positively skewed, and the cut-off points used fall near the upper end of the distribution. Thus, relatively few cases are to be found near the cut-off points used here.[35] Lowering the cut-off point is likely to result in more cases being changed as a consequence of these kinds of adjustments.

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[35] This would not be the case if the mean of the dependent variable were to be used. Then there would be substantially more cases near the cut-off point.

The accuracy of the characterization that "it does not make much difference" as to whether or not one adjusts for race, ethnicity, and gender depends on how a big or small effect is defined. Although one can say that at most "only" 8% of all the cases are shifted, these represent a large number of cases if it is considered that, over time, and across jurisdictions, the use of the unadjusted model would result in many more individuals receiving a different classification. At the same time, such considerations of the "size of the difference it makes" ignores the conditional probabilities. Contingent upon how the individual would have been predicted using the unadjusted models, the use of the adjusted models has a relatively larger effect. Previous researchers have tended to interpret the effects of including these variables as having small effects, but we argue that the effects observed here are of considerable consequence: the percent of the total in the sample may seem "small", but large numbers of individuals are affected by the decision to ignore race, ethnicity, and gender. Moreover, this decision matters greatly to those individuals whose predictions of successful or failure would change depending upon whether the adjusted or unadjusted model were used.

In summary, the examination of the difference it makes to "adjust" for the effects of race, ethnicity, and gender suggests that the numbers of individuals predicted differently varies across dependent variables: as few as 201 individuals are predicted differently (imprisonment within one year) and as many as 899 (rearrest at five years). Again, how "big" is the "difference it makes" is a matter of interpretation. We contend that the difference is far too large to ignore: many individuals are predicted differently depending on whether the models are "adjusted" in the manner just described.

SUMMARY AND DISCUSSION

The models developed in the earlier chapters can be used to make predictions about the likelihood that individuals with various characteristics and prior experiences will recidivate. The knowledge that a given offender is likely to continue to commit crimes, or that he/she poses little threat to the community, is attractive as it can be used to help determine an appropriate intervention. However, many issues have to be resolved before these individual-level predictions can be helpful for effective CJS decision making. The analyses of this chapter addressed a wide variety of these concerns.

The first issue of individual prediction raised pertained to the choice of a cut-off point to define likely failures and successes. Three general strategies, based on statistical criteria, were introduced. The empirical behavior of these strategies was studied for four indicators of recidivism, measured over two post-sentence observation windows, with no completely satisfactory choice stemming from this analysis. Following the work of Blumstein et al., (1985), we then introduced the concepts of the civil-libertarian ratio and the relative disutility associated with the ratio of false positives to false negatives. Applying these to the possible choices for cutting-points, we found that the choice of a cut-off point equating the selection ratio (the proportion chosen to recidivate) to the base rate (the proportion actually recidivating) had the desirable feature of minimizing disutility within a range of civil-libertarian ratios from just below 1 to quite high values. For some skewed probability distributions, the choice of .5 as a cut-off point resulted in too few, or virtually no, cases selected to fail, while use of the mean resulted in an inordinately high percentage of expected failures.

In short, the analysis led us to adopt the cut-off criteria such that the selection ratio equals the base rate for the subsequent analysis. It should be recognized, however, that this choice is only reasonable in the context of equal weight being given to the disvalued outcomes of a false positive and a false negative. Various policy considerations may dictate the choice of a cut-off point that incorporates more extreme values on the civil-libertarian index of Blumstein and his colleagues. This choice was also of consequence in that all of the results in the remainder of the chapter were predicated upon it.

Four general issues about "the difference it makes" for the prediction of whether or not an individual will fail were then examined. First, the magnitude of discrepancies between models with and without intervention effects were explored. Although somewhat smaller than the differences later observed, a still sizeable number of individuals are predicted differently as a result of ignoring the effects of CJS intervention. Knowledge of the sentence received, and how those sanctions influence the recidivism, changed the success and failure expectations for between 2.3% and 5.8% of the sample. In this sense, the belief that "nothing makes a difference" is not found to be entirely true at the level of individual prediction.

To highlight the difference that choice of a recidivism indicator can make, the predictions of the full model were compared across the eight measures of recidivism. In general, the results showed that dependent variables cannot be equated, even when the same independent variables are used in the models, as was the case here. Whether or not a particular individual is expected to fail is quite contingent both upon which recidivistic event is predicted and the probability that the event will actually occur. Again, the

numbers of individuals who are predicted differently across dependent variables is substantial, and were among the largest differences reported in this chapter.

We then examined whether the choice of a domain of independent variable makes a difference. There, the results showed that many individuals are predicted differently across domains, depending, in part, on the base rate of the dependent variables. Where the base rates are relatively high, the proportion of the sample predicted differently across domains is higher than if the base rates are low. In the latter instance, there are many true negatives accounting for almost three-quarters of the agreed-upon predictions. Some domains also seemed more central in that their predictions were less often at odds with those of other domains, and were accurate more often. This was particularly true for the criminal career variables, such as those of the anamnestic domain. This analysis also demonstrated that the accuracy of individual-level predictions increased as more domains agreed in their predictions, leading us to posit the existence of a core group of individuals for whom the prediction of recidivism is relatively easy and accurate. In general, however, we reiterate that the choice of domain, or the omission of a domain, makes a difference in the prediction of recidivism outcomes as we have measured them.

We then focussed specifically on the issues of predictive models that were adjusted for race, ethnicity, and gender effects. We explored the predictions of recidivism by arbitrarily assuming that everyone was a white male, using the model developed from equations that included these variables as controls. The results again speak to the general theme that it makes a difference, and the magnitude of the difference is quite large, in our

opinion: many individuals are predicted differently based on this consideration.

In conclusion, in earlier chapters we put forth here the claim that "it makes a difference." This claim is reaffirmed by our analysis of individual-level predictions. Even seemingly small amounts of explained variance, if "real" in the sense of not being due to random sampling fluctuations, translate into large numbers of individuals predicted differently. "Large" is a matter of interpretation, but in the case of the CJS system and its goal of dealing with each individual fairly, a "couple of hundred" individuals a year in a state with over 10,000 convictions seems to be a substantial effect. Changes of this magnitude are found for considerations of race, ethnicity, gender, and CJS interventions. Even larger consequences are observed for the choice of a domain for independent variables and the choice of a dependent variable. To some extent, the values of R-square, and the "small" coefficients found in aggregate-level analyses mask the degree of impact that such considerations can have upon individuals. The results from earlier chapters seem much smaller than do those found in the examination of predicted success or failure for individuals. The small effects seen earlier are larger in terms of the number of individuals affected, and larger still over time.

It is often assumed, or "wished" in research applications, that the results are generally the same across various measures of recidivism. Thus, a model based on rearrest will work quite well with reimprisonment. Moreover, policy is often developed "as if" there is agreement as to an appropriate measure of recidivism, and that measures can be readily substituted with similar results. In that such claims or assumptions are made with regard to the prediction of individual behavior, it should be clear that these are

erroneous. Not only are there many individuals affected by the considerations of race, ethnicity, gender, and CJS intervention, but the effects vary across outcome measures. A general concept such as "recidivism" will not do in applications involving individual prediction. In this respect, the results from earlier chapters are supported by the study of individual-level prediction: the conclusions reached depend upon what is meant by "recidivism" and when it is measured.

The treatment of race, ethnicity, and gender in the analysis above was admittedly brief. The general subject of these variables and their relation to possible CJS interventions is quite complex and would require considerably more space and time than is available. However, a few considerations seem worth noting here. It is generally the case that researchers have tried to "shrink" this problem into small proportions, arguing that the direct effects of factors such as race in CJS decision making are small. Again, at risk of repeating ourselves too often, the effects observed here seem not to be small, and certainly cannot be ignored. Yet, they have largely been ignored in the research community and in numerous applications, such as parole release, and supervision risk assessment instruments. We do not think this is a problem of making an issue over just a few cases. Until researchers and practitioners address directly the issue of race, gender and ethnicity, particularly as it affects individual predictions, the effects of these variables will continue to be absorbed by other variables -- and translated into biased decision making on the part of those who implement policy.

Indeed, the policy implications of our findings in this, and previous, chapters are far-reaching. We have seen here that there are many choices to be made when individual-level predictions of recidivism are to be used. The

emphasis given to civil-libertarian and community safety concerns, which forms of recidivism are most appropriate, what independent variables are to be used for purposes of prediction, and how variables such as race and gender are to be treated are all decisions that must ultimately be made at the level of CJS policy. The options available and the consequences of these crucial decisions, as well as how the risk for recidivism can be incorporated into a more systematic sentencing policy, are the subjects of the next chapter.

Figure 9.1
Distribution of Predicted Values of Rearrest
at One Year (N=11,714) and Five Years (N=11,749)

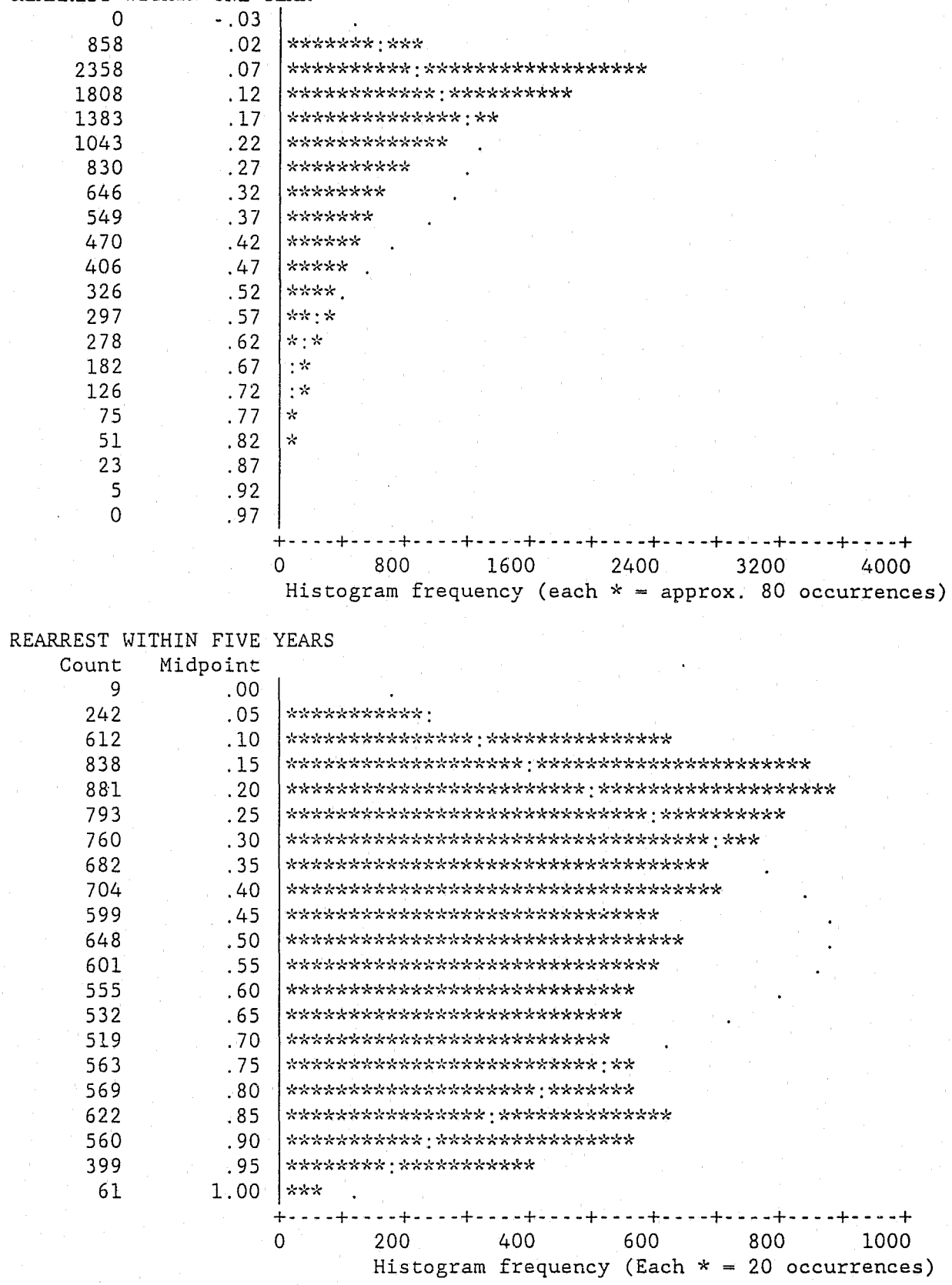


Figure 9.2
Rearrest Within One Year

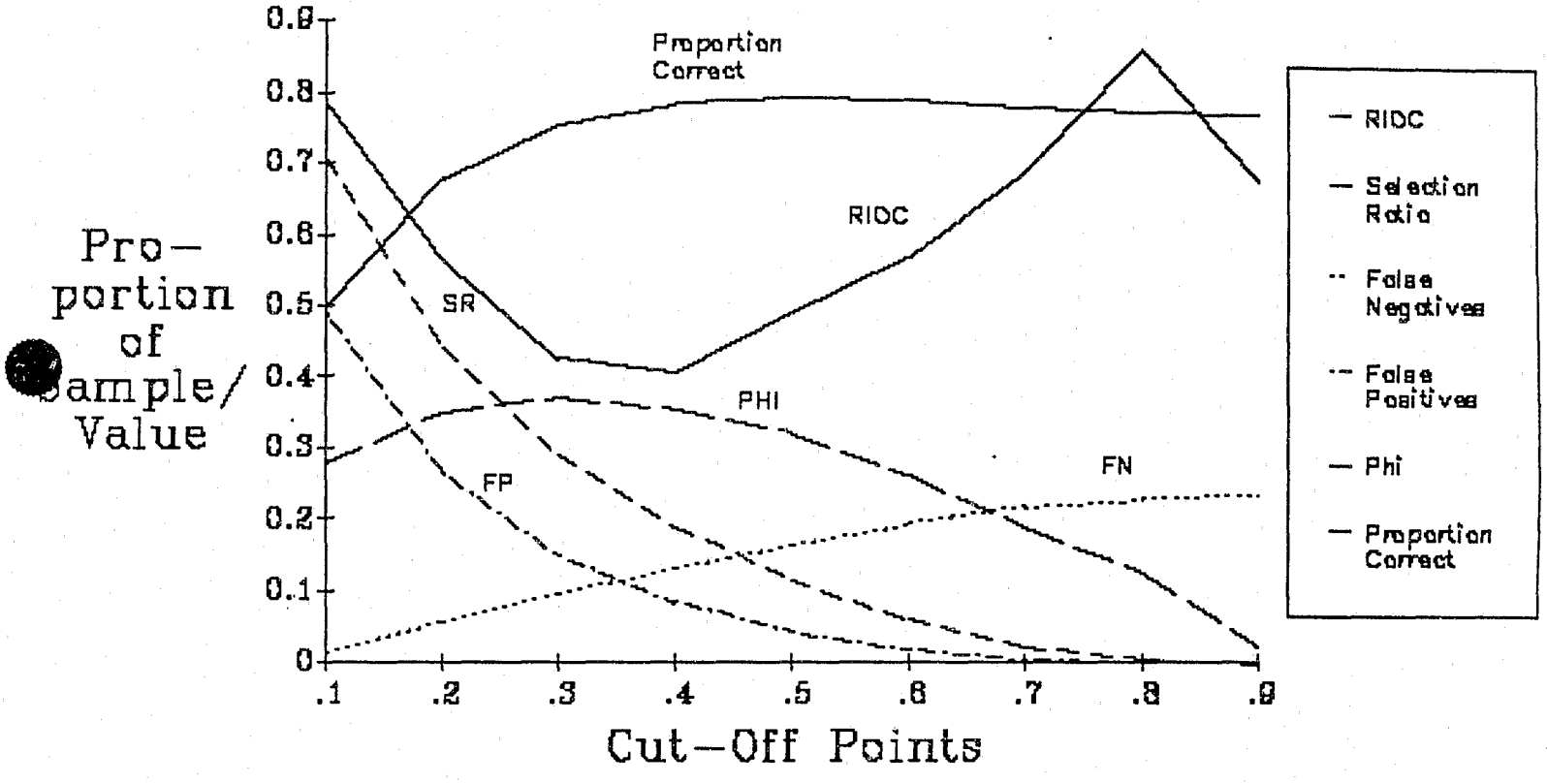


Figure 9.3
Rearrest Within Five Years

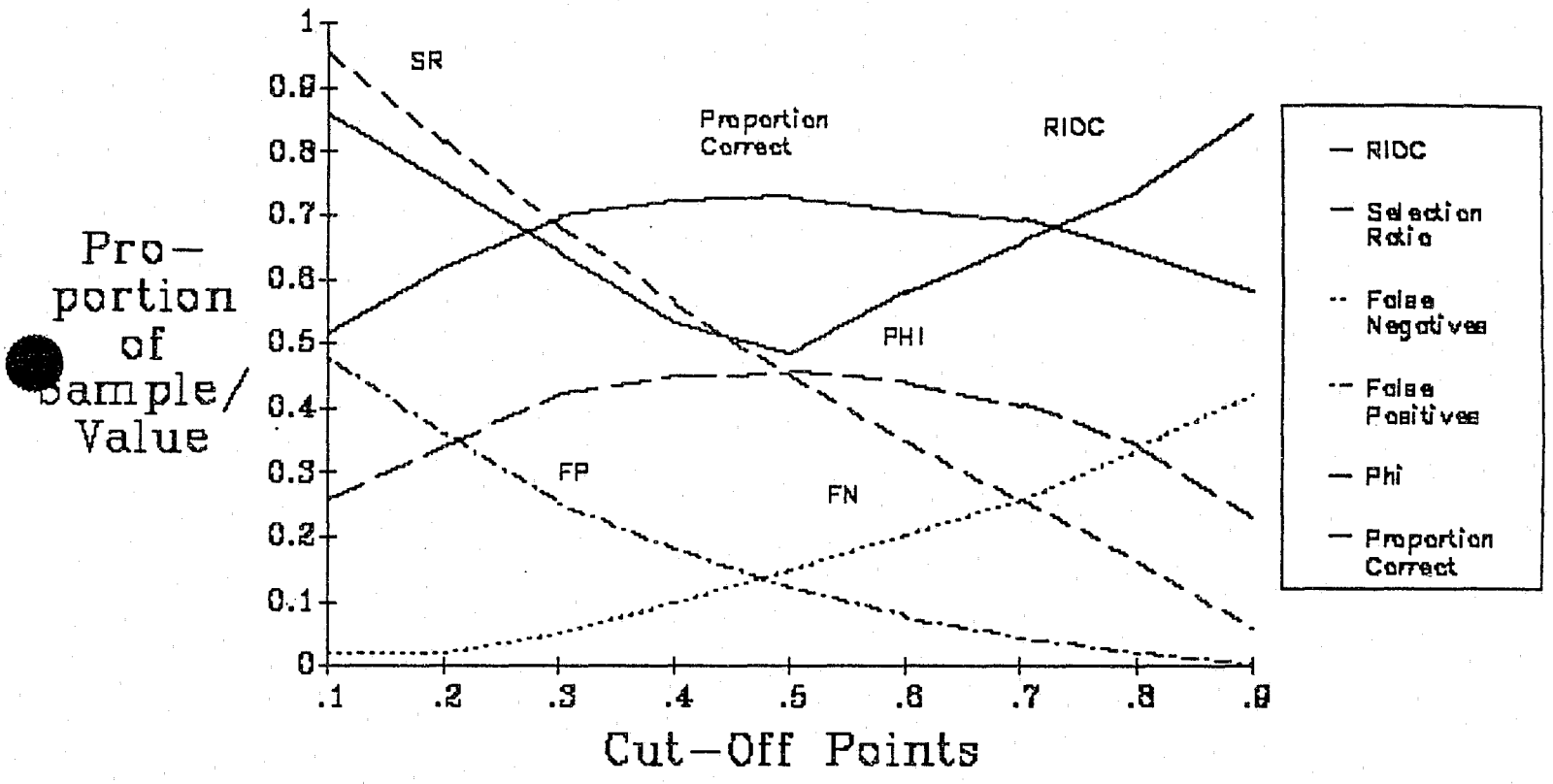
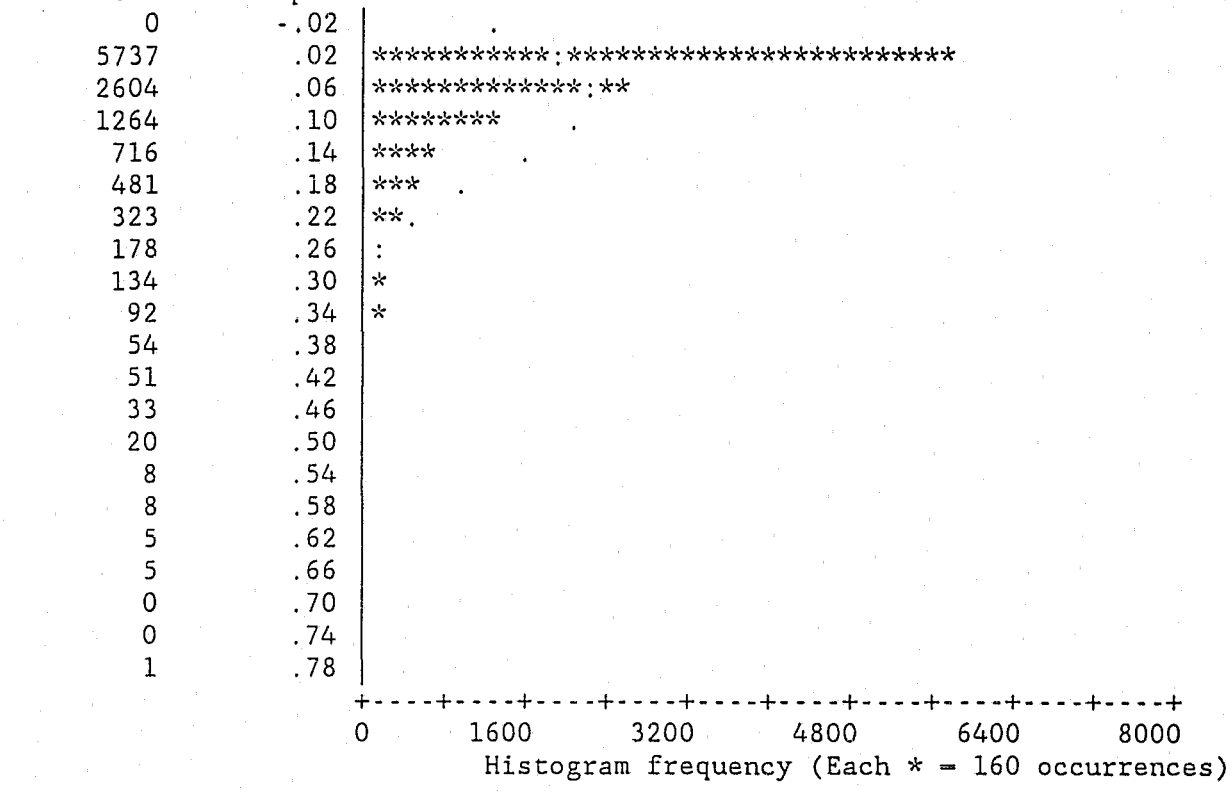


Figure 9.4
Predicted Values of Rearrest for Persons Offense
at One Year and Five Years

REARREST FOR PERSONS OFFENSE AT ONE YEAR



REARREST FOR PERSONS OFFENSE AT FIVE YEARS

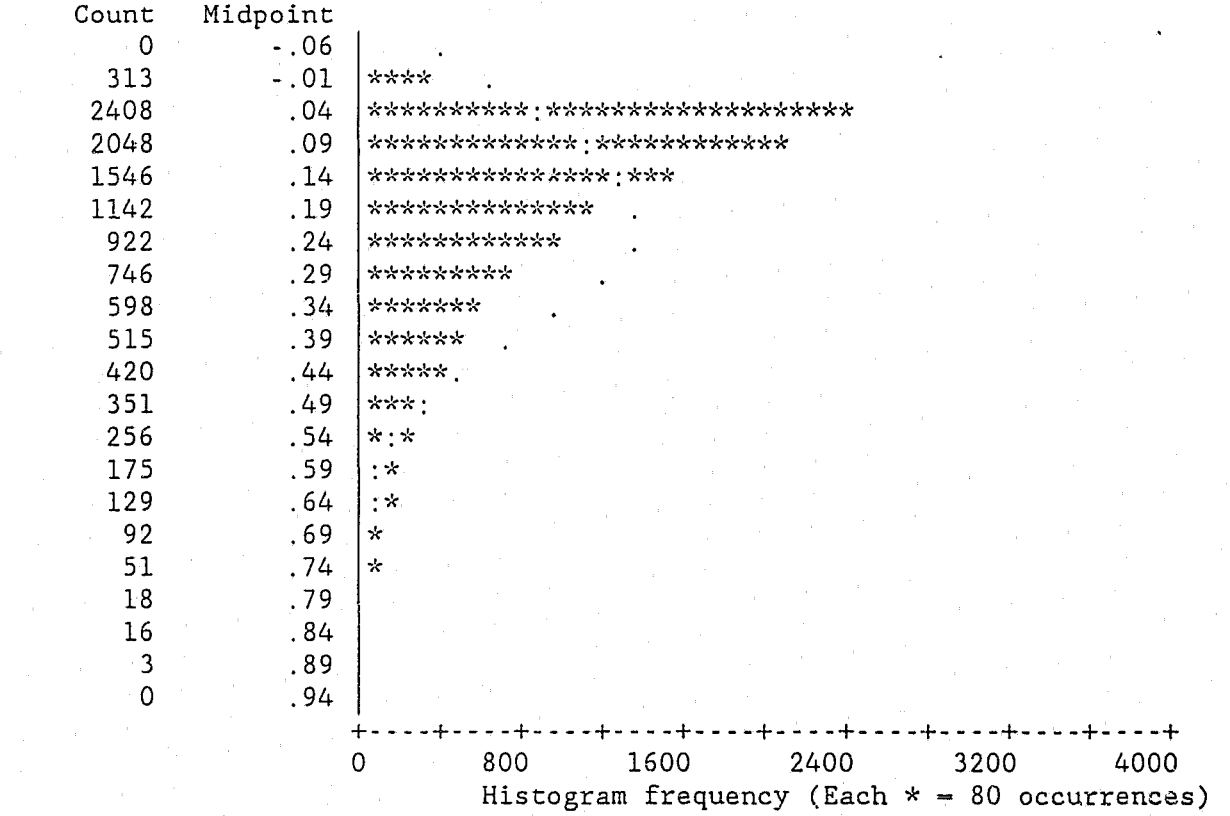
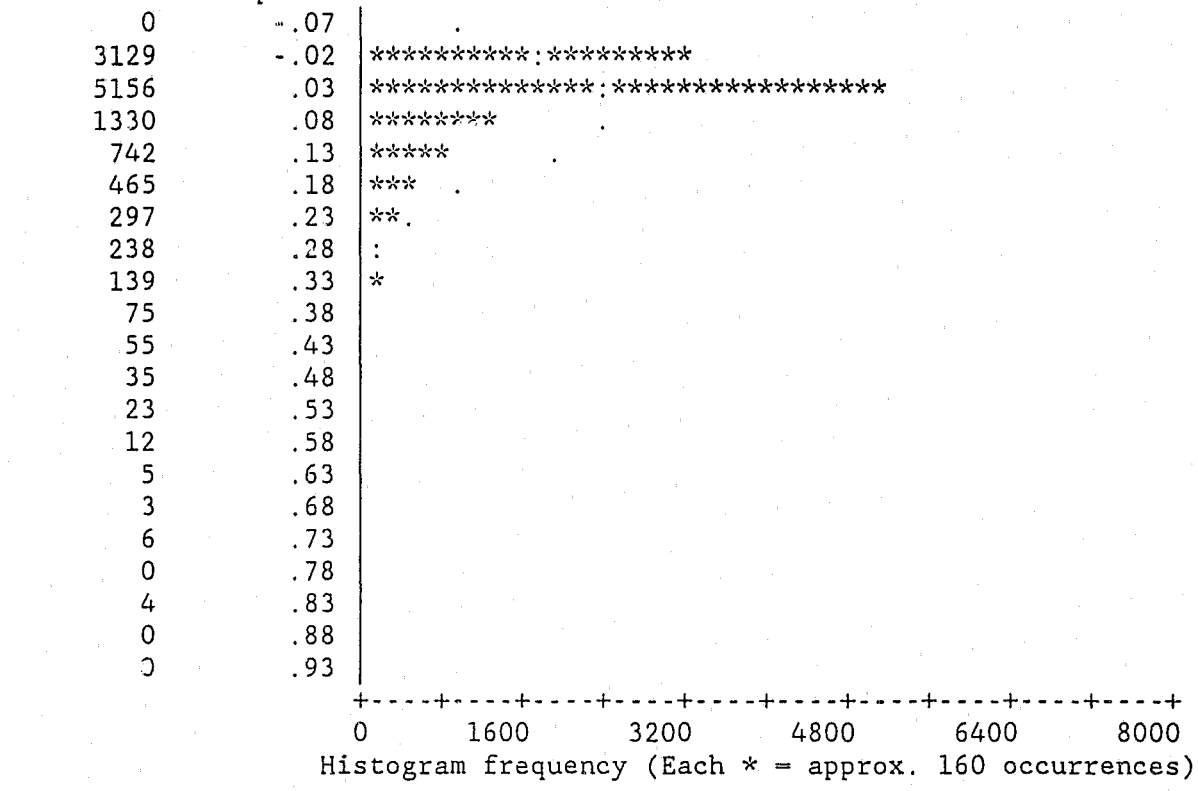


Figure 9.5
Predicted Values of Reimprisonment at One and Five Years

REIMPRISONMENT AT ONE YEAR



REIMPRISONMENT AT FIVE YEARS

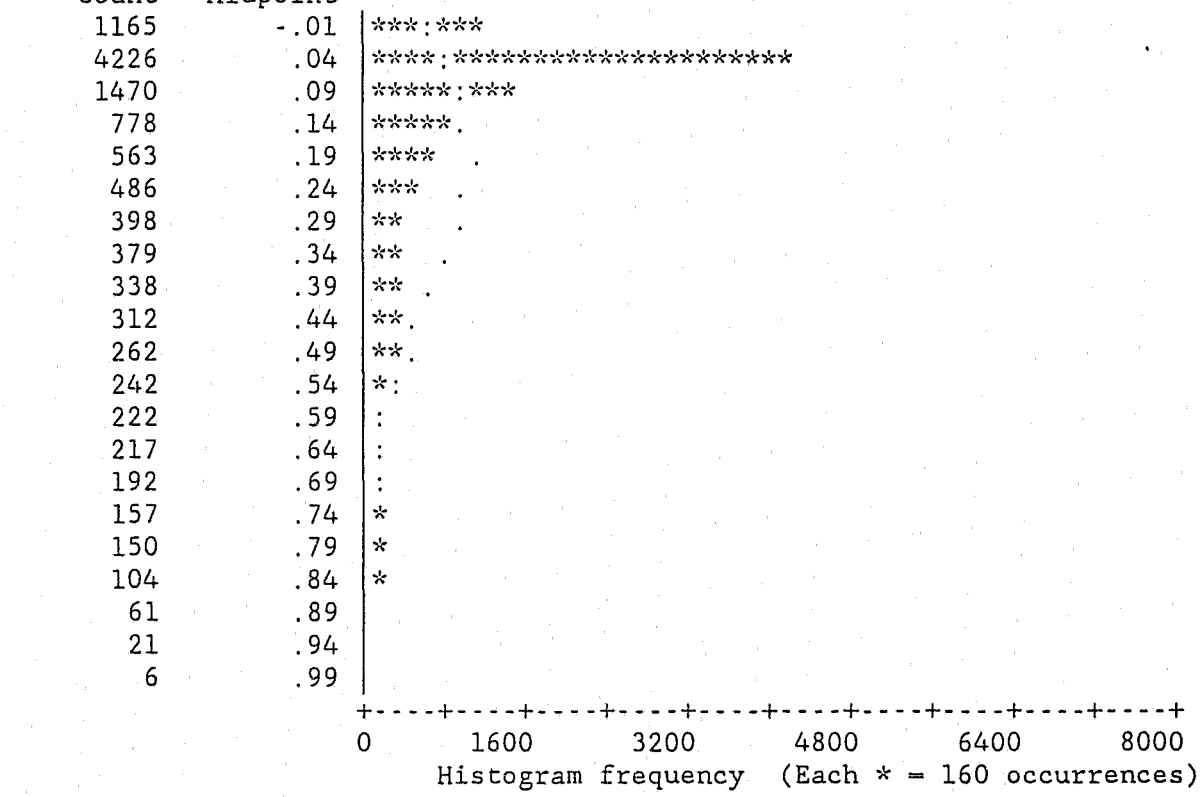
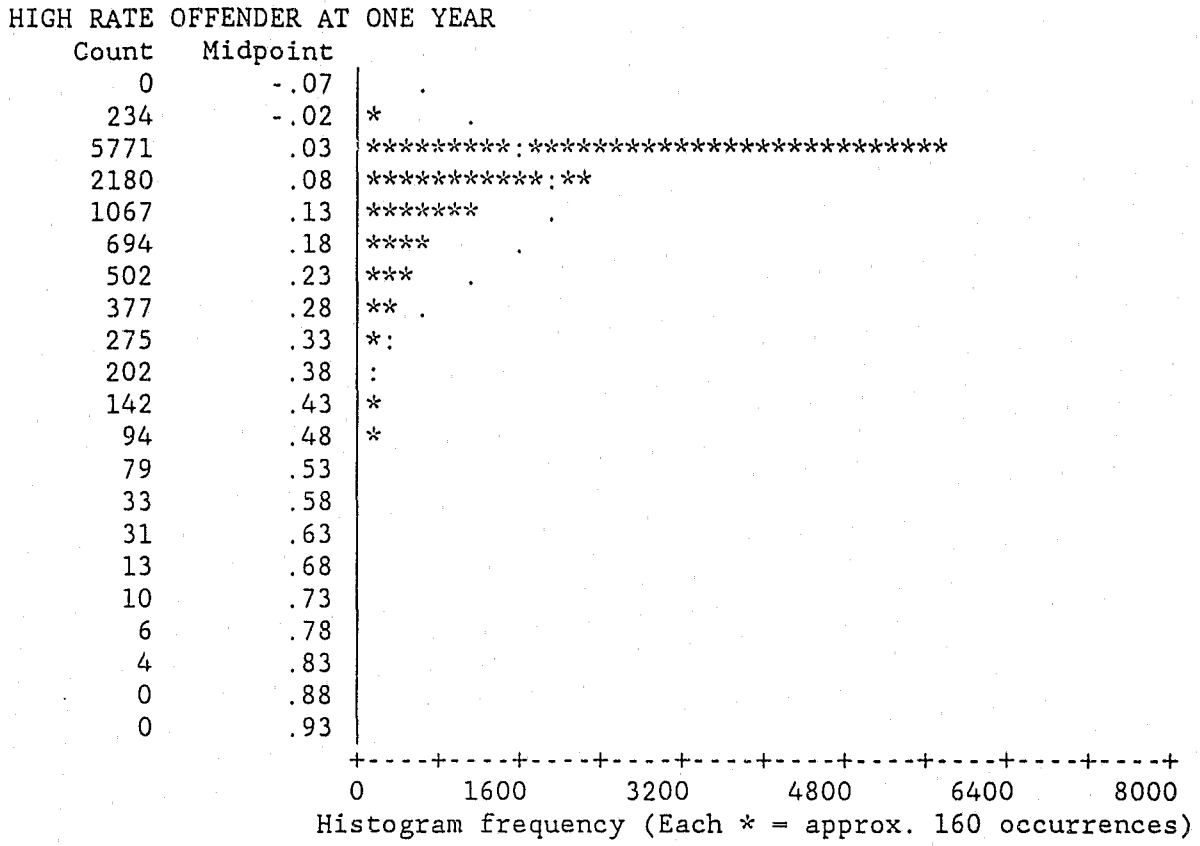


Figure 9.6
Predicted Values of High Rate Offender
at One Year and Five Years



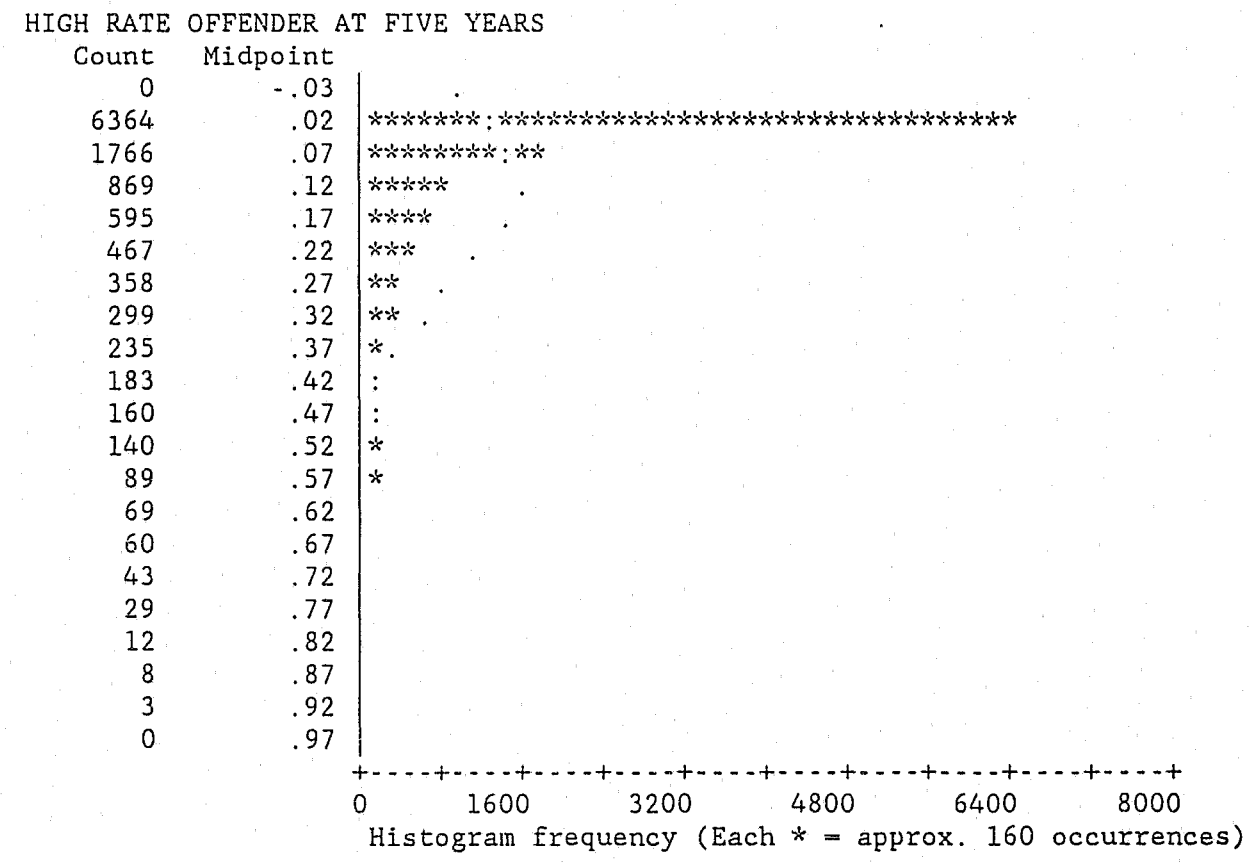


Figure 9.7
Rearrest for Persons Crime Within One Year

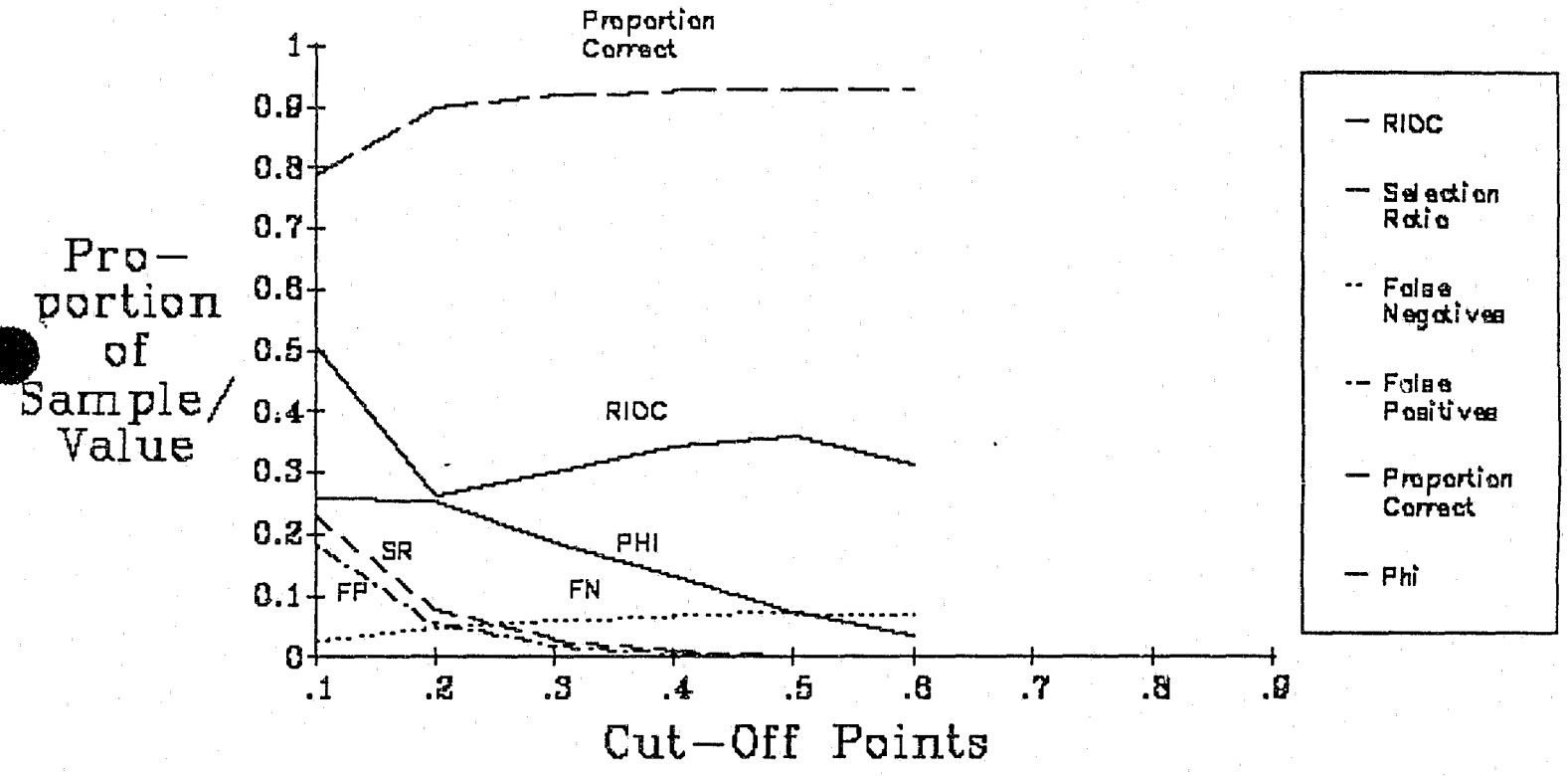


Figure 9.8
Rearrest for Persons Crime in Five Years

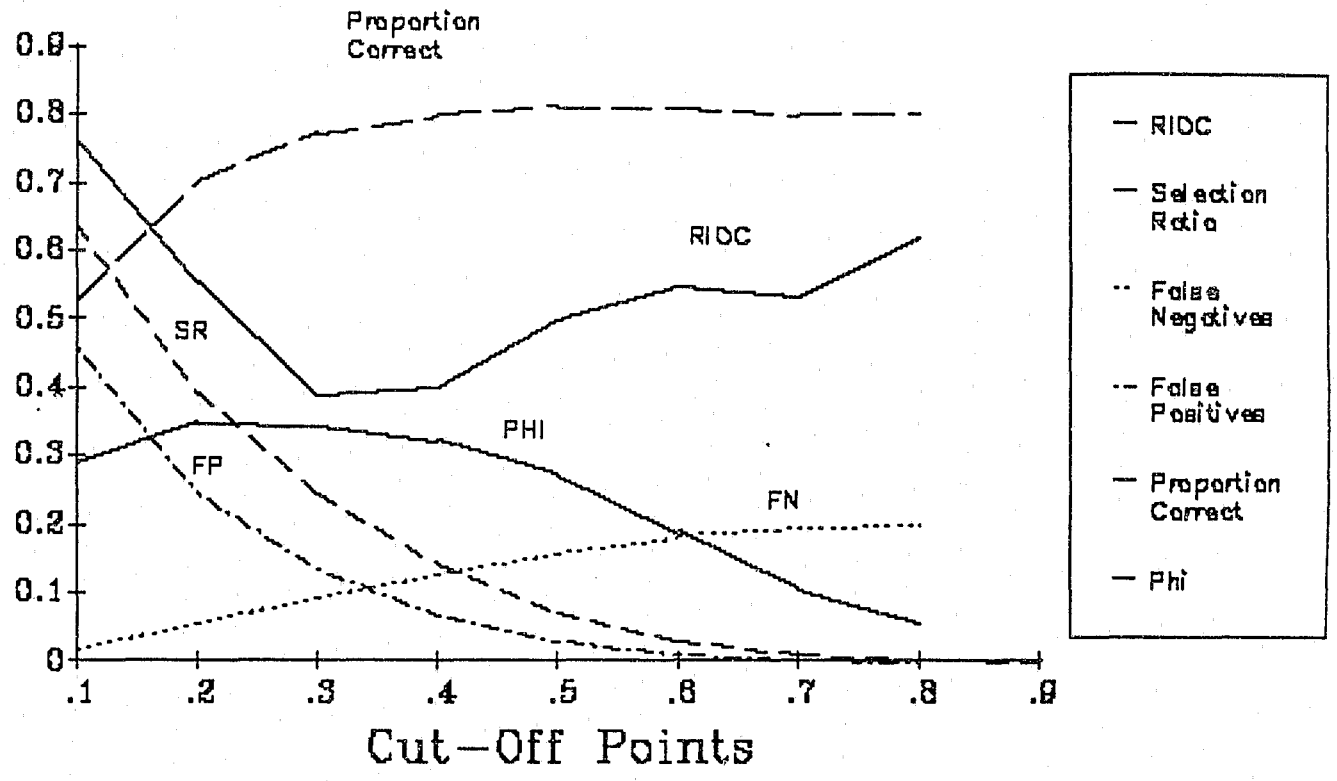


Figure 9.9
Reimprisonment Within One Year

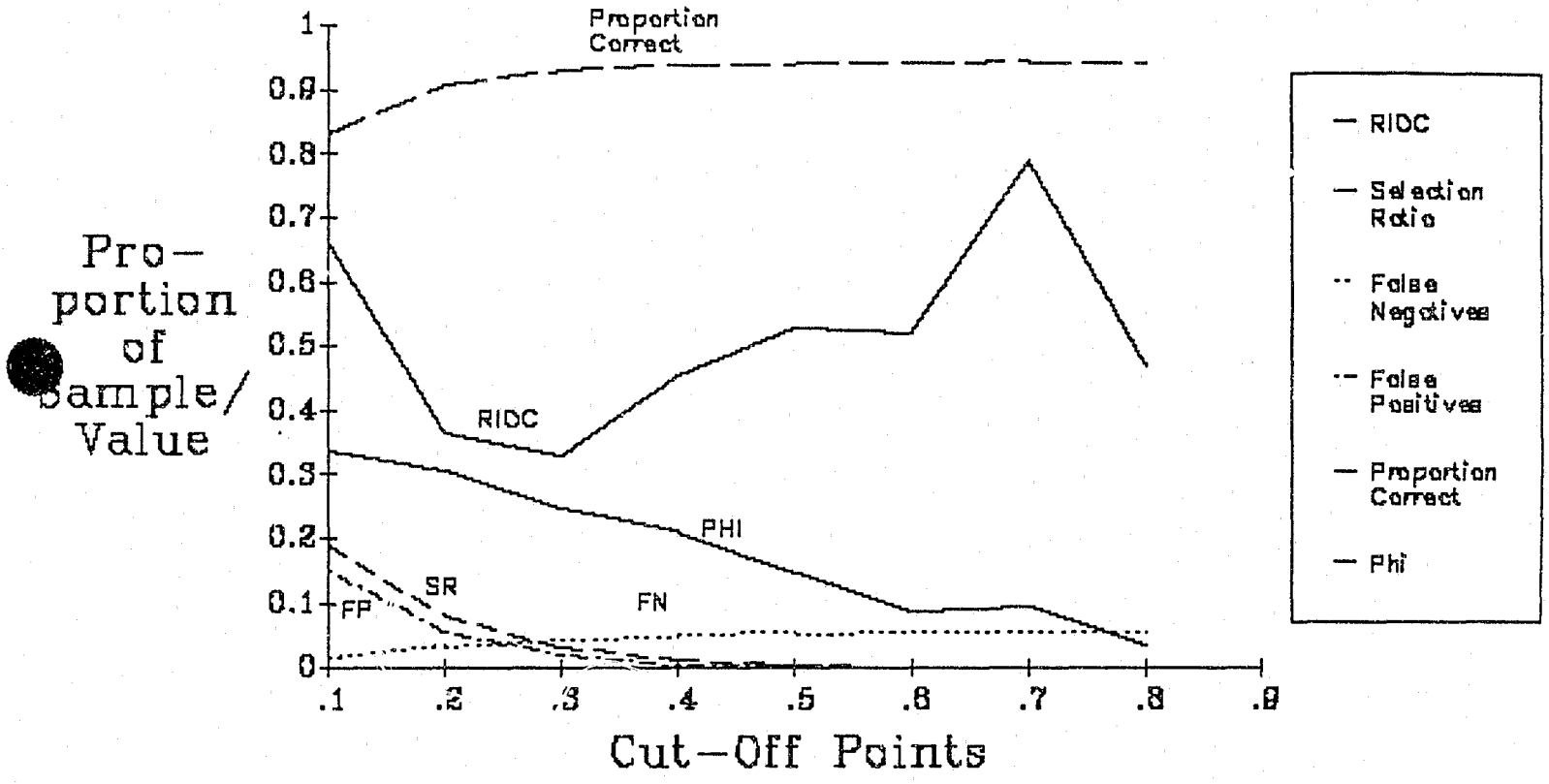


Figure 9.10
Reimprisonment Within Five Years

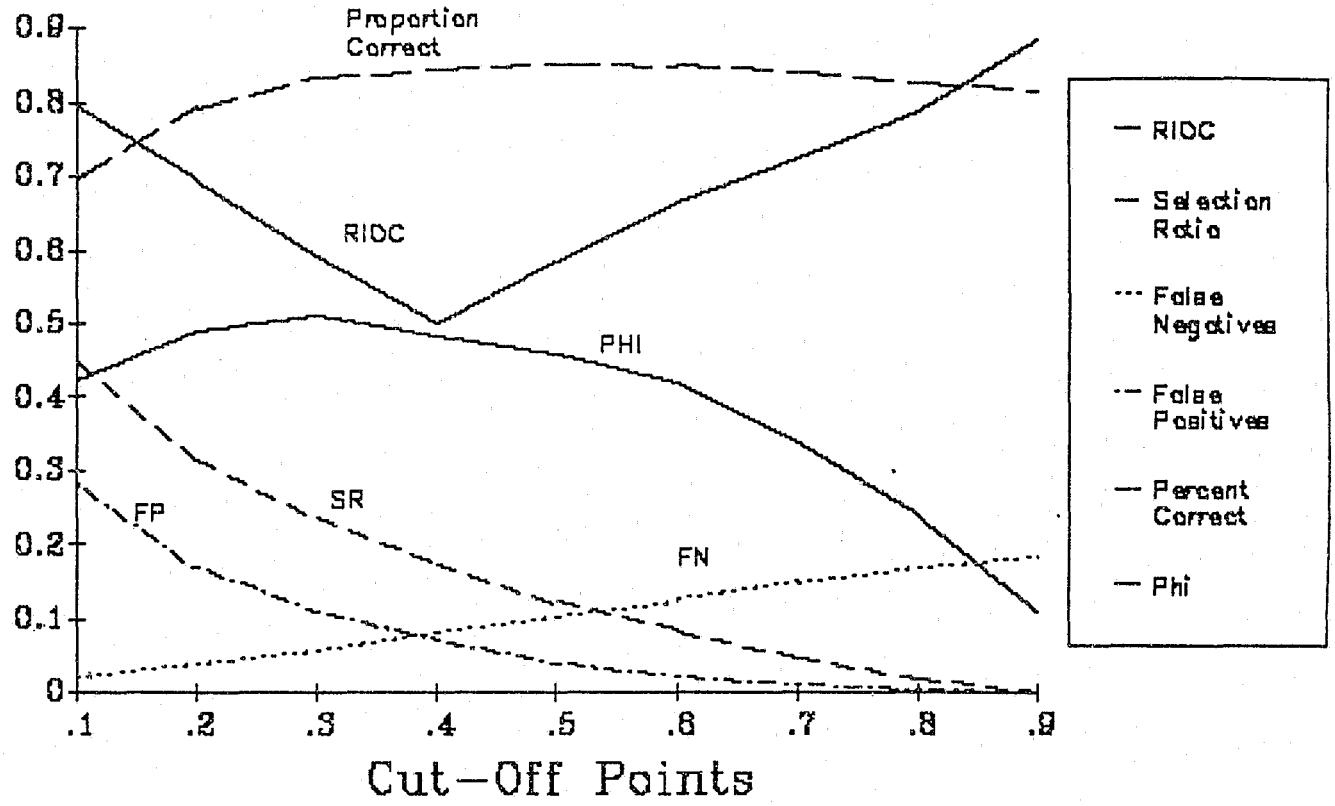


Figure 9.11
High Rate Offender Within One Year

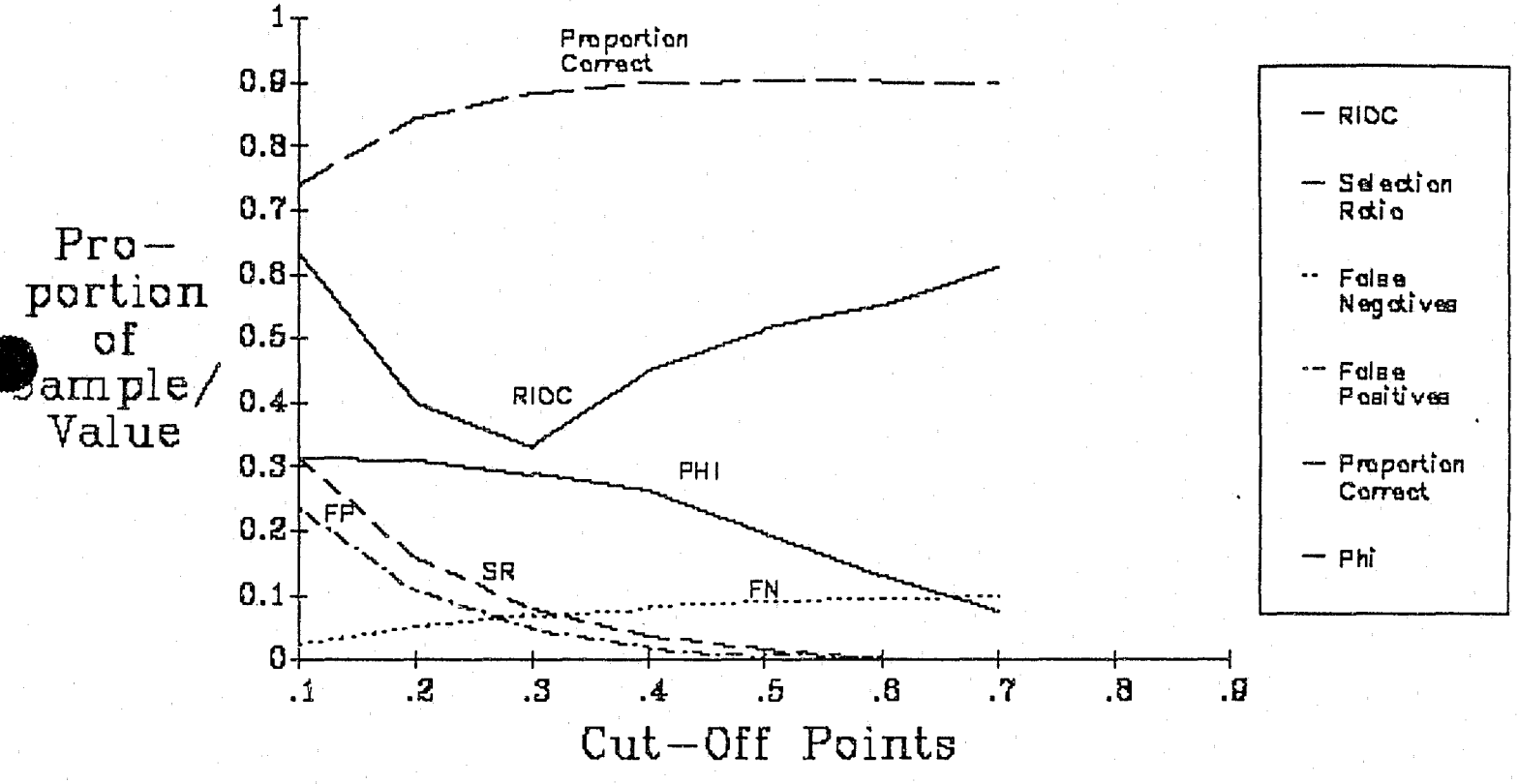


Figure 9.12
High Rate Offender Within Five Years

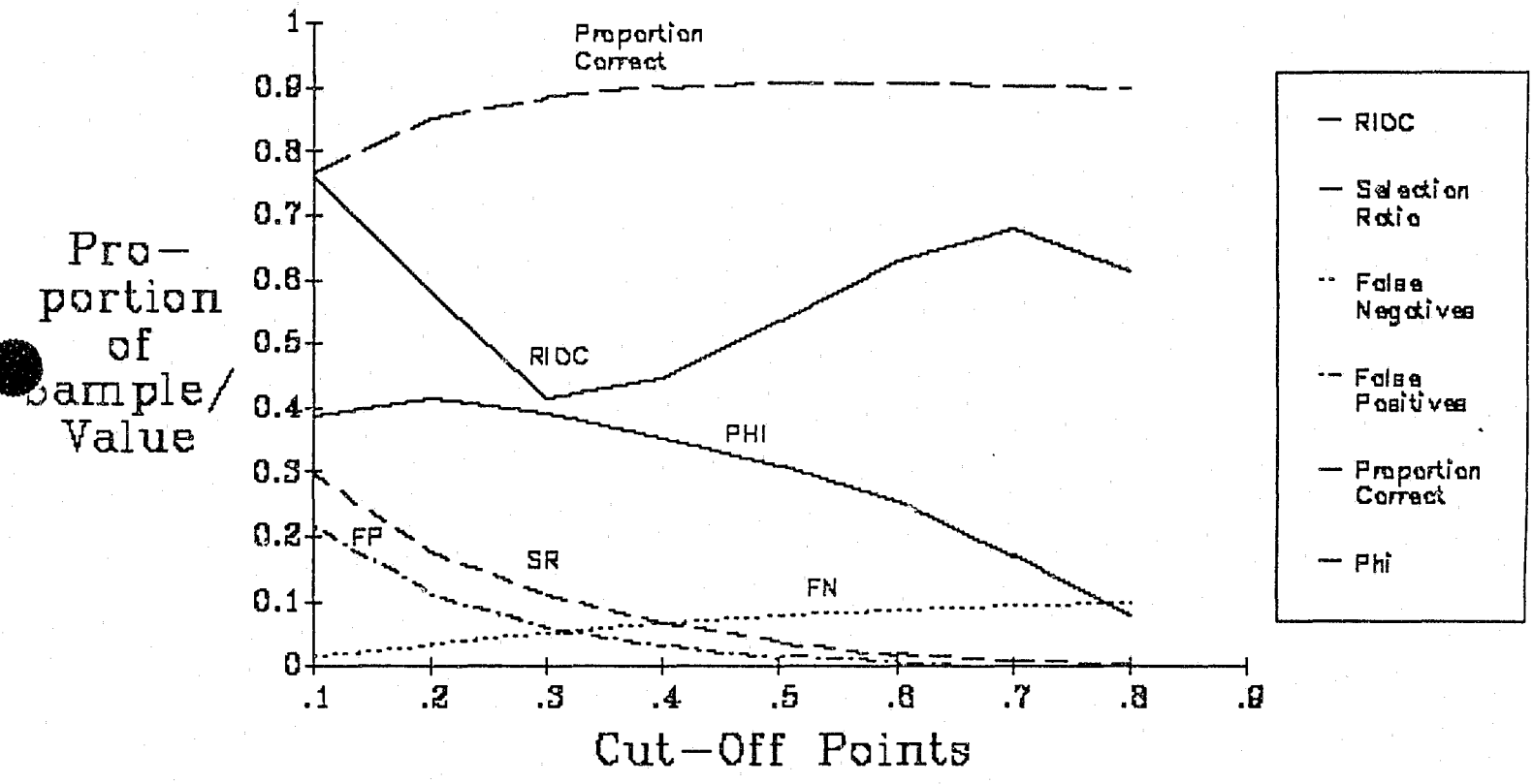


Figure 9.13
Rearrest at One Year:
Disutility Scores and Civil–Libertarian Ratios

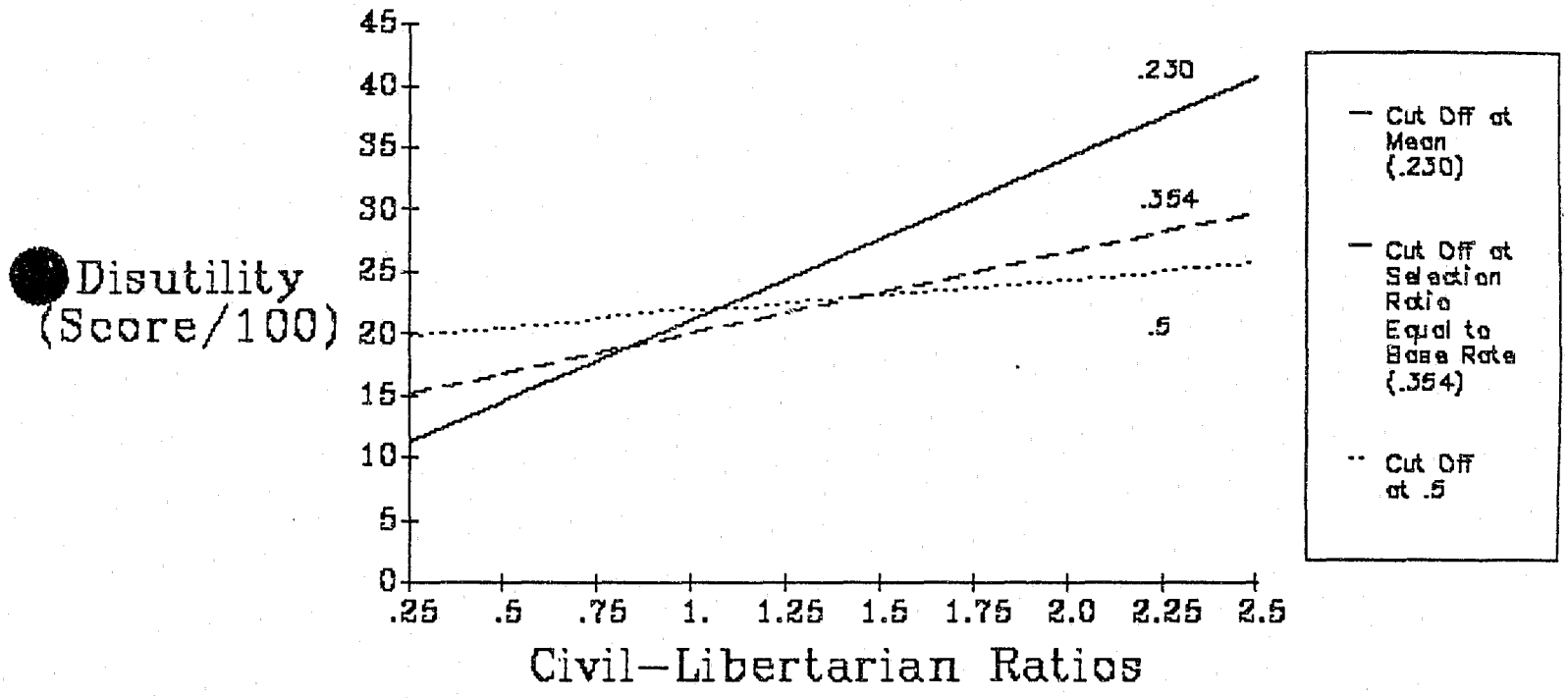


Figure 9.14
Rearrest at Five Years:
Disutility Scores and Civil–Libertarian Ratios

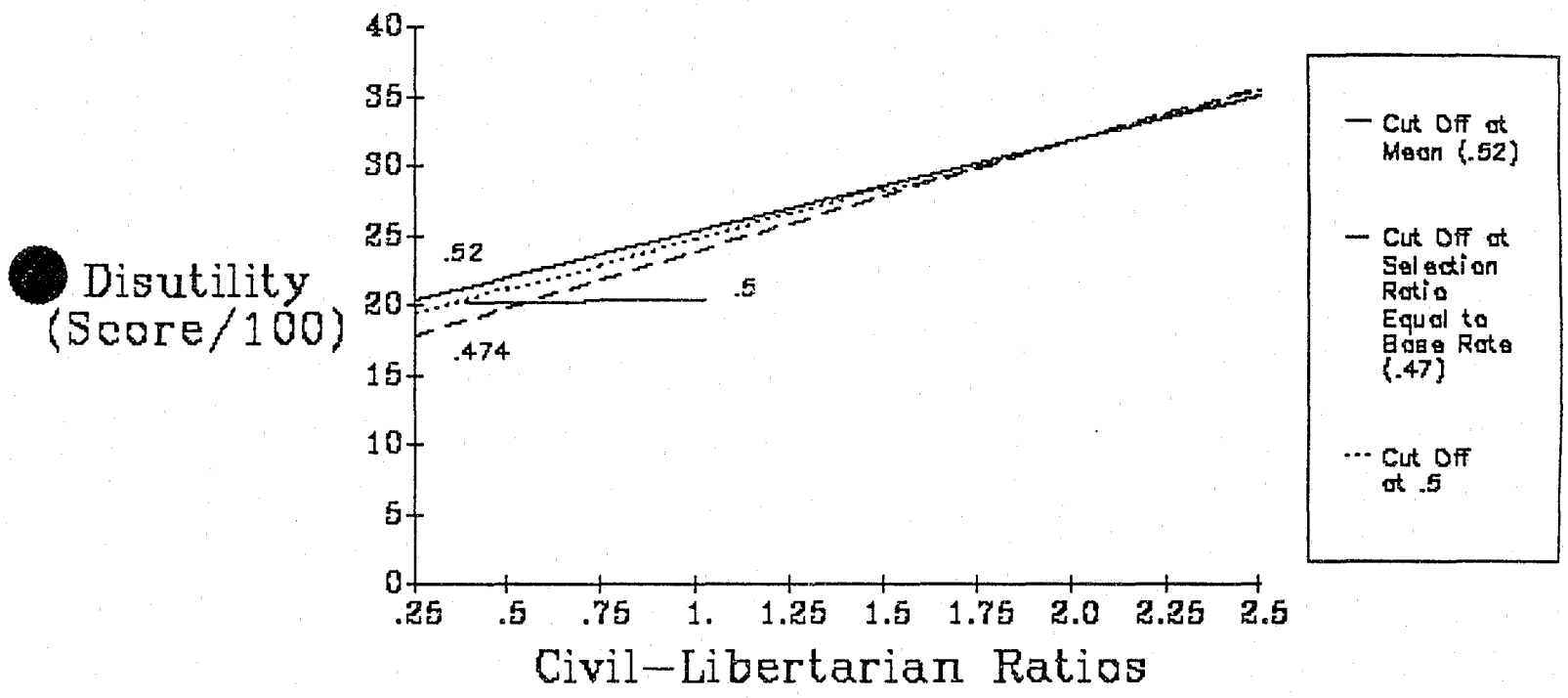


Figure 9.15
Rearrest for Persons Crime at One Year: Disutility Scores and Civil–Libertarian Ratios

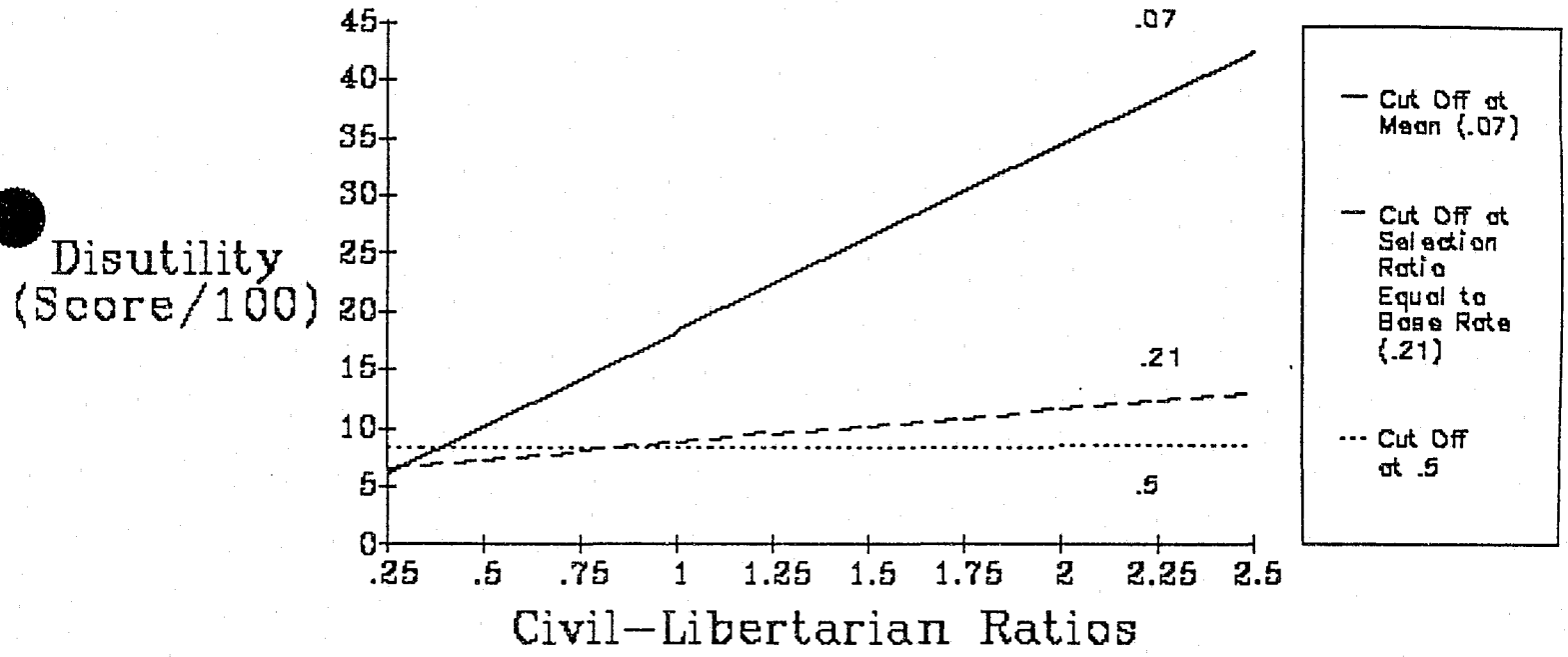


Figure 9.16
Rearrest for Persons Crime at Five Years:
Disutility Scores and Civil–Libertarian Ratios

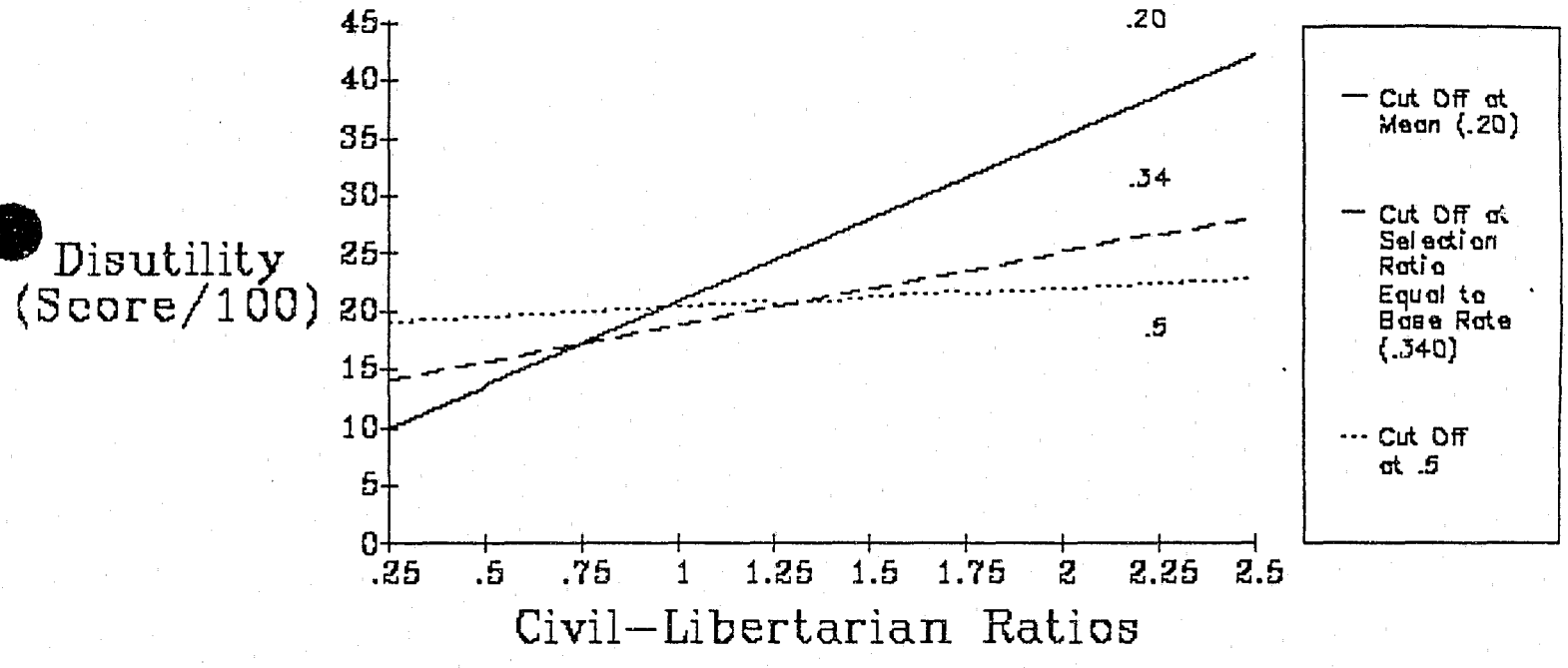


Figure 9.17
Reimprisonment at One Year: Disutility Scores and Civil–Libertarian Ratios

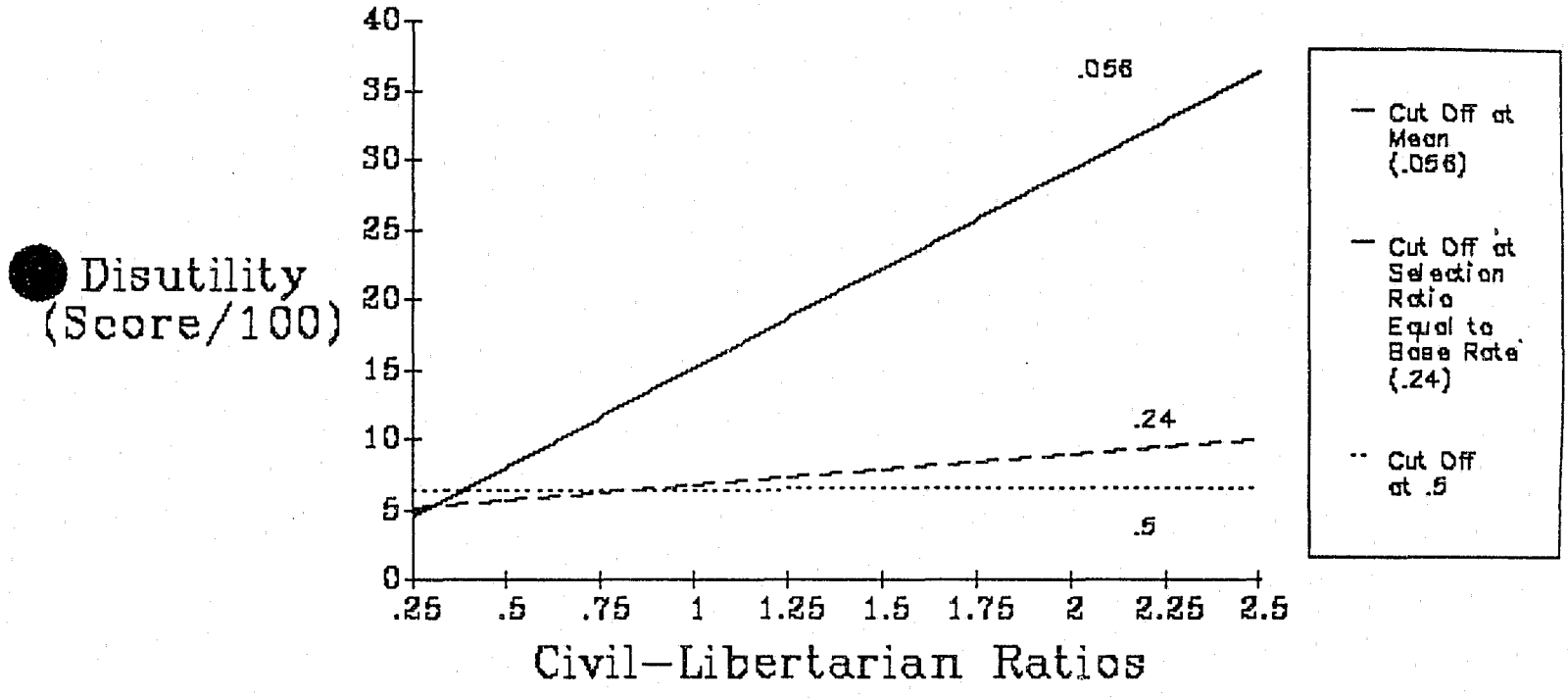


Figure 9.18
Reimprisonment at Five Years: Disutility Scores and Civil–Libertarian Ratios

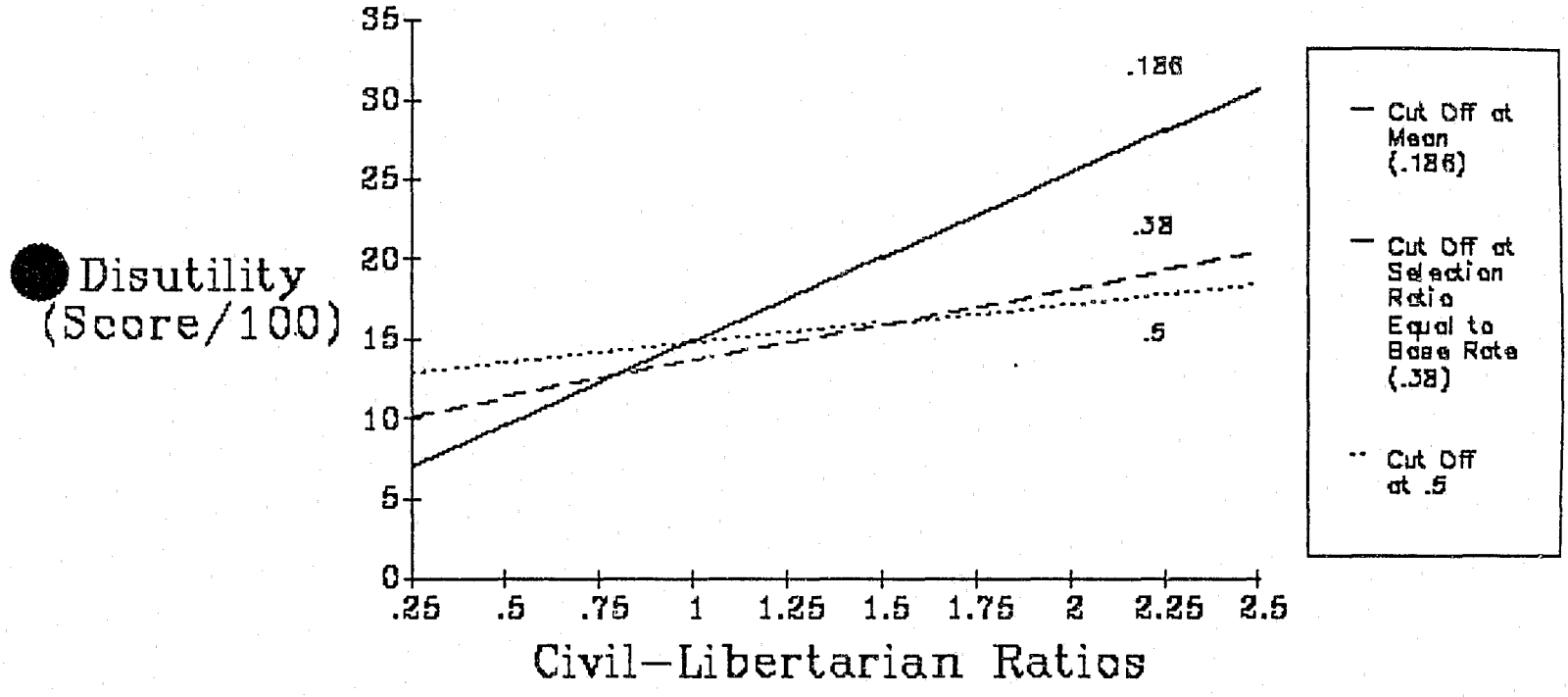


Figure 9.19
High Rate Offender at One Year:
Disutility Scores and Civil–Libertarian Ratios

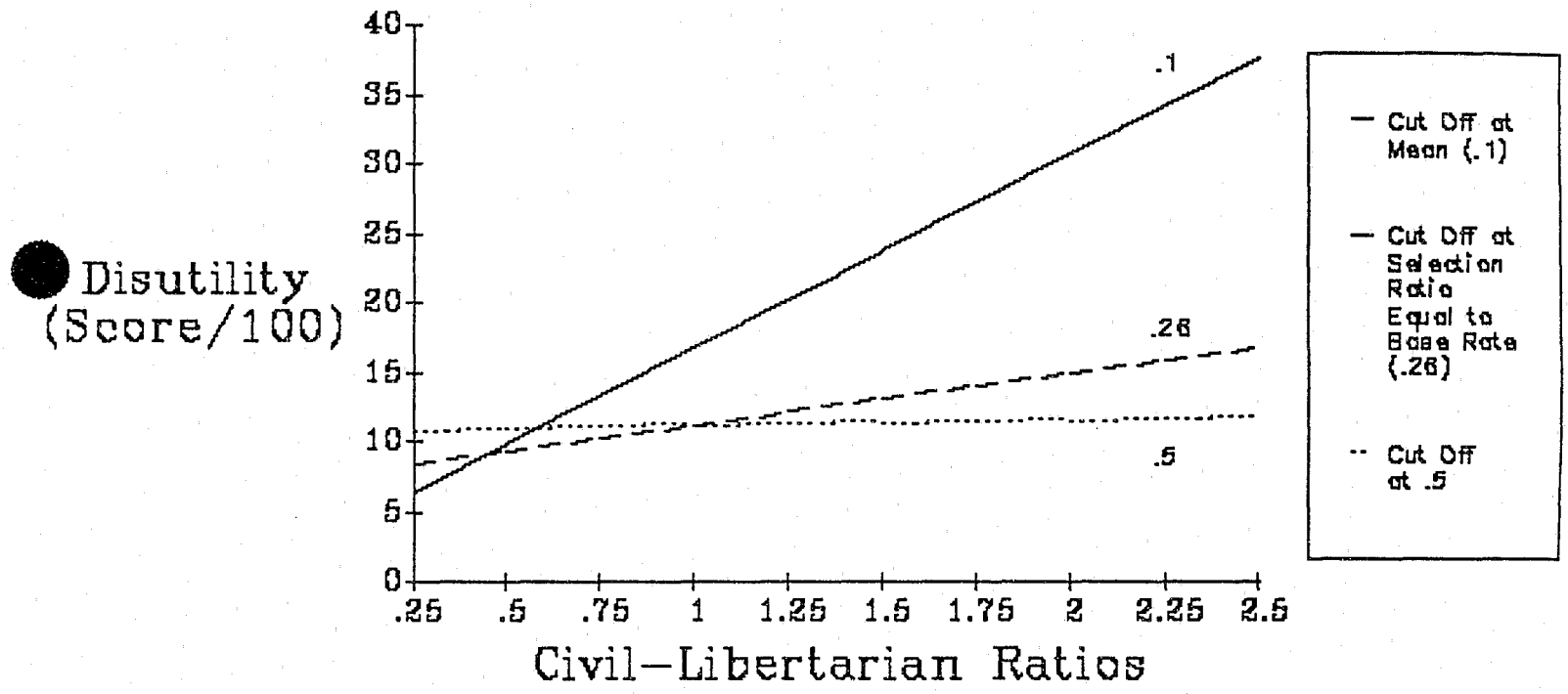


Figure 9.20
High Rate Offender at Five Years:
Disutility Scores and Civil–Libertarian Ratios

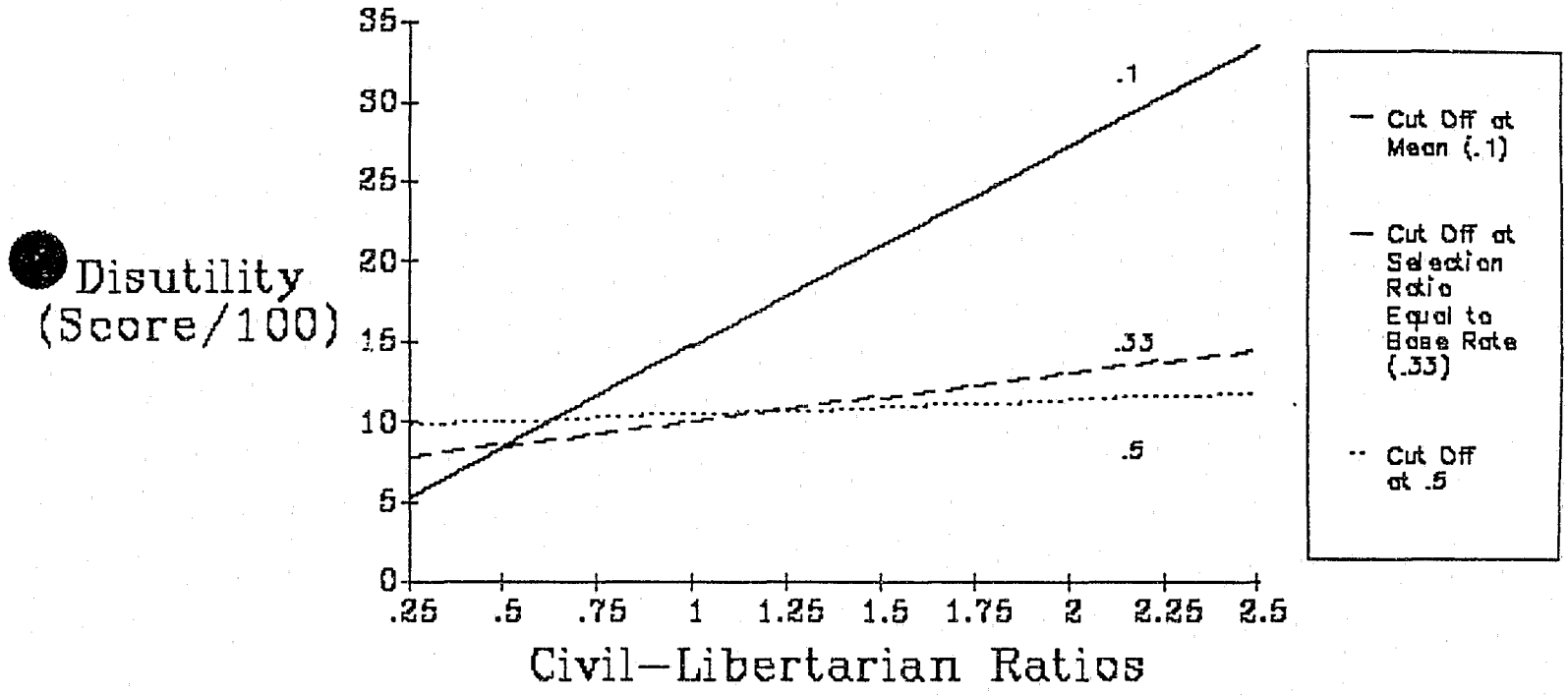


Table 9.1
Eight Measures of Recidivism: Comparison of Models With and Without Intervention Effects

Prediction If Intervention Variables Excluded:

Prediction of Full Model:		Rearrest Five Years		Rearrest One Year		Arrested Persons Off. in Five Yrs.		Arrested Persons Off. in One Year		Imprisoned Within Five Years		Imprisoned Within One Year		High Rate Offender Five Yrs.		High Rate Offender One Year	
		No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Rearrested Five Years	No	5763	340														
	Yes	341	5305														
Rearrested in One Year	No	-	-	8733	295												
	Yes	-	-	339	2347												
Arrested Persons in Five Years	No	-	-	-	-	9108	285										
	Yes	-	-	-	-	285	2071										
Arrested Persons in One Year	No	-	-	-	-	-	-	10710	187								
	Yes	-	-	-	-	-	-	185	632								
Imprisoned Five Years	No	-	-	-	-	-	-	-	-	9250	308						
	Yes	-	-	-	-	-	-	-	-	307	1884						
Imprisoned One Year	No	-	-	-	-	-	-	-	-	-	-	10903	161				
	Yes	-	-	-	-	-	-	-	-	-	-	160	490				
High Rate Offender Five Years	No	-	-	-	-	-	-	-	-	-	-	-		10502	66		
	Yes	-	-	-	-	-	-	-	-	-	-	-		199	982		
High Rate Offender One Year	No	-	-	-	-	-	-	-	-	-	-	-	-	-	-	10368	186
	Yes	-	-	-	-	-	-	-	-	-	-	-	-	-	-	180	996

Table 9.2
Eight Measures of Recidivism By Prediction of Success or Failure

Predicted To Be:		Rearrest One Year		Arrested Persons Off. in Five Yrs.		Arrested Persons Off. in One Year		Imprison-ed Within Five Years		Imprison-ed Within One Year		High Rate Offender Five Yrs.		High Rate Offender One Year	
		No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Rearrested Five Years	No	6098	0	6099	4	6096	2	6101	2	6096	2	6102	1	6097	1
	Yes	2930	2686	3294	2352	4801	815	3457	2189	4968	648	4466	1180	4451	1165
Rearrested in One Year	No	-	-	8603	425	8984	44	8708	320	9016	12	9024	4	9025	3
	Yes	-	-	774	1912	1913	773	837	1849	2048	638	1522	1164	1523	1163
Arrested Persons in Five Years	No	-	-	-	-	9372	5	8795	598	9255	122	9224	169	9190	187
	Yes	-	-	-	-	1525	812	763	1593	1809	528	1344	1012	1358	979
Arrested Persons in One Year	No	-	-	-	-	-	-	9398	1499	10551	346	10270	627	10252	645
	Yes	-	-	-	-	-	-	147	670	513	304	276	541	296	521
Imprisoned Five Years	No	-	-	-	-	-	-	-	-	9542	3	9517	41	9493	52
	Yes	-	-	-	-	-	-	-	-	1522	647	1051	1140	1055	1114
Imprisoned One Year	No	-	-	-	-	-	-	-	-	-	-	10443	621	10479	585
	Yes	-	-	-	-	-	-	-	-	-	-	103	547	69	581
High Rate Offender Five Years	No	-	-	-	-	-	-	-	-	-	-	-	-	10329	217
	Yes	-	-	-	-	-	-	-	-	-	-	-	-	219	949

Table 9.3

Comparison of Combinations of Models
Predicting Rearrest at Five Years
(Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success True Negative	Failure False Negative	Success False Positive	Failure True Positive	N	% of Total	Type
1	1	1	1	1	1	1	1082	183	210	1005	2480	21.1	7/0
ALL OTHERS							540	510	492	450	1992	16.9	-
1	0	0	0	0	0	0	57	202	290	54	603	5.1	1/6
1	0	0	0	0	0	1	21	48	56	24	149	1.3	2/5
1	0	0	0	0	1	0	16	39	48	11	114	1.0	2/5
1	0	0	0	1	1	0	16	36	35	18	105	.9	3/4
1	0	0	1	0	0	0	23	44	41	34	142	1.2	2/5
1	0	0	1	0	0	1	22	14	35	31	102	.9	3/4
1	0	0	1	1	0	1	32	17	22	29	100	.9	4/3
1	0	0	1	1	1	0	27	23	21	31	102	.9	4/3
1	0	0	1	1	1	1	58	34	30	56	178	1.5	5/2
1	0	1	0	0	0	0	44	128	141	46	359	3.1	2/5
1	0	1	0	0	0	1	19	32	29	27	107	.9	3/4
1	0	1	1	0	0	1	38	25	23	37	123	1.0	4/3
1	0	1	1	1	0	1	48	20	28	40	136	1.2	5/2
1	0	1	1	1	1	0	35	44	27	43	149	1.3	5/2
1	0	1	1	1	1	1	150	78	31	140	399	3.4	6/1
1	1	0	0	0	0	0	25	76	68	48	217	1.8	2/5
1	1	0	0	0	1	0	27	46	26	19	118	1.0	3/4
1	1	0	0	1	1	0	40	35	32	21	128	1.1	4/3
1	1	0	0	1	1	1	42	35	19	23	119	1.0	5/2
1	1	0	1	0	0	1	35	14	21	38	108	.9	4/3
1	1	0	1	1	0	1	58	18	27	59	162	1.4	5/2
1	1	0	1	1	1	0	80	43	37	83	243	2.1	5/2
1	1	0	1	1	1	1	584	137	76	360	1157	9.8	6/1
1	1	1	0	0	0	0	24	44	45	35	148	1.3	3/4
1	1	1	0	0	0	1	12	32	19	46	109	.9	4/3
1	1	1	0	1	0	1	22	16	32	44	114	1.0	5/2
1	1	1	0	1	1	0	72	41	41	37	191	1.6	5/2
1	1	1	0	1	1	1	121	46	47	84	298	2.5	6/1
1	1	1	1	0	0	1	63	26	21	61	171	1.5	5/2
1	1	1	1	0	1	0	42	23	24	33	122	1.0	5/2
1	1	1	1	0	1	1	61	25	24	27	137	1.2	6/1
1	1	1	1	1	0	1	110	25	67	155	357	3.0	6/1
1	1	1	1	1	1	0	213	69	68	160	510	4.3	6/1

Table 9.4

Comparison of Combinations of Models Predicting Rearrest at One Year (Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success True Negative	Failure False Negative	Success False Positive	Failure True Positive	N	% of Total	Type
1	1	1	1	1	1	1	4272	435	41	70	4818	41.1	7/0
All OTHER							934	567	783	456	2740	23.4	-
1	0	0	0	0	0	0	20	34	338	59	451	3.9	1/6
1	0	0	1	1	1	1	57	25	19	21	122	1.0	5/2
1	0	1	0	0	0	0	41	53	109	40	243	2.1	2/5
1	0	1	0	0	1	0	25	29	42	10	106	.9	3/4
1	0	1	1	1	1	0	62	26	16	19	123	1.1	5/2
1	0	1	1	1	1	1	250	80	18	37	385	3.3	6/1
1	1	0	0	0	0	0	22	22	67	18	129	1.1	2/5
1	1	0	1	1	0	1	97	32	21	26	176	1.5	5/2
1	1	0	1	1	1	0	48	27	31	28	134	1.1	5/2
1	1	0	1	1	1	1	312	97	33	59	501	4.3	5/2
1	1	1	0	0	0	0	30	33	45	11	119	1.0	3/4
1	1	1	0	1	0	1	62	26	17	7	112	1.0	5/2
1	1	1	0	1	1	1	112	45	9	15	181	1.5	6/1
1	1	1	1	0	0	1	68	19	9	14	110	.9	5/2
1	1	1	1	0	1	0	55	23	32	25	135	1.2	5/2
1	1	1	1	0	1	1	213	50	15	24	302	2.6	6/1
1	1	1	1	1	0	1	178	41	22	27	268	2.3	6/1
1	1	1	1	1	1	0	422	76	31	29	558	4.8	6/1

Table 9.5

Comparison of Combinations of Models
Predicting Rearrest for Person Offense at Five Years
(Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success True Negative	Failure False Negative	Success False Positive	Failure True Positive	N	% of Total	Type
1	1	1	1	1	1	1	3800	304	46	50	4200	35.8	7/0
ALL OTHER							910	480	720	367	2477	21.1	-
1	0	0	0	0	0	0	33	26	259	35	353	3.0	1/6
1	0	0	0	0	1	0	4	5	178	36	223	1.9	2/5
1	0	0	1	0	1	0	9	3	78	26	116	1.0	3/4
1	0	0	1	1	1	1	68	25	47	34	174	1.5	5/2
1	0	1	0	0	0	0	46	60	39	10	155	1.3	2/5
1	0	1	1	1	0	1	136	42	5	2	185	1.6	5/2
1	0	1	1	1	1	1	135	39	18	23	215	1.8	6/1
1	1	0	0	0	0	0	25	23	42	18	108	.9	2/5
1	1	0	1	1	0	1	181	60	1	3	245	2.1	5/2
1	1	0	1	1	1	0	50	21	44	27	142	1.2	5/2
1	1	0	1	1	1	1	347	64	35	48	494	4.2	5/2
1	1	1	0	0	0	0	66	36	17	7	126	1.1	3/4
1	1	1	0	0	0	1	78	45	5	2	130	1.1	4/3
1	1	1	0	1	0	1	164	59	10	10	243	2.1	5/2
1	1	1	1	0	0	1	163	43	3	5	214	1.8	5/2
1	1	1	1	0	1	1	78	12	11	12	113	1.0	6/1
1	1	1	1	1	0	0	126	32	1	1	160	1.4	5/2
1	1	1	1	1	0	1	1009	168	1	5	1183	10.1	6/1
1	1	1	1	1	1	0	364	54	34	39	491	4.2	6/1

Table 9.6

Comparison of Combinations of Models
Predicting Rearrest for Persons Offense at One Year
(Selection Ratio = Base Rate)

Models							Observed:						
Hazard	Social	POO	Anamnestic	Juv	JS	Control	Success	Failure	Success	Failure	N	% of Total	Type
							True Negative	False Negative	False Positive	True Positive			
1	1	1	1	1	1	1	7863	345	0	0	8208	70.1	7/0
ALL OTHER							717	193	437	84	1431	12.2	-
1	0	0	0	0	0	0	1	0	268	25	294	2.5	1/6
1	0	1	1	1	1	1	227	35	0	0	262	2.2	6/1
1	1	0	1	1	1	1	317	35	0	0	352	3.0	6/1
1	1	1	0	1	1	1	169	28	2	0	199	1.7	6/1
1	1	1	1	0	1	1	235	43	1	0	279	2.4	6/1
1	1	1	1	1	0	1	273	17	0	3	293	2.5	6/1
1	1	1	1	1	1	0	364	28	2	2	396	3.4	6/1

Table 9.7
Comparison of Combinations of Models
Predicting Reimprisonment at Five Years
(Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success — True Negative	Failure — False Negative	Success — False Positive	Failure — True Positive	N	% of Total	Type
1	1	1	1	1	1	1	5272	230	21	53	5576	47.5	7/0
ALL OTHERS							1041	674	779	516	3010	25.6	-
1	0	0	0	0	0	0	6	33	344	48	431	3.7	1/6
1	0	0	1	1	1	1	62	33	17	27	139	1.2	5/2
1	0	1	0	0	0	0	21	41	100	18	180	1.5	2/5
1	0	1	1	1	1	1	260	66	9	23	358	3.0	6/1
1	1	0	1	1	1	0	76	18	16	31	141	1.2	5/2
1	1	0	1	1	1	1	405	71	14	42	532	4.5	6/1
1	1	1	0	1	0	1	77	29	9	15	130	1.1	5/2
1	1	1	0	1	1	1	105	27	7	3	142	1.2	6/1
1	1	1	1	0	1	1	200	42	9	14	265	2.3	6/1
1	1	1	1	1	0	1	201	44	9	15	269	2.3	6/1
1	1	1	1	1	1	0	482	42	16	36	576	4.9	6/1

Table 9.8

Comparison of Combinations of Models
Predicting Reimprisonment at One Year
(Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success	Failure	Success	Failure			
							True Negative	False Negative	False Positive	True Positive	N	% of Total	Type
1	1	1	1	1	1	1	8590	218	0	0	8808	75.2	7/0
ALL OTHERS							712	195	309	88	1304	11.1	-
1	0	0	0	0	0	0	0	0	231	24	255	2.2	1/6
1	0	1	1	1	1	1	231	31	0	1	263	2.2	6/1
1	1	0	1	1	1	1	289	22	1	0	312	2.7	6/1
1	1	1	0	1	1	1	128	23	0	0	151	1.3	6/1
1	1	1	1	0	1	0	47	8	4	1	60	2.1	5/2
1	1	1	1	1	0	1	181	17	0	0	198	1.7	6/1
1	1	1	1	1	1	0	328	34	1	0	363	3.1	6/1

Table 9.9

Comparison of Combinations of Models
Predicting High Rate Offenders at Five Years
(Selection Ratio = Base Rate)

Models: Hazard	Social	PO	Anamnestic	Juv	CJS	Control	Observed: Success True Negative	Failure False Negative	Success False Positive	Failure True Positive	N	% of Total	Type
1	1	1	1	1	1	1	7258	248	2	0	7508	63.9	7/0
ALL OTHERS							763	393	548	247	1951	16.6	-
1	0	0	0	0	0	0	1	2	303	43	349	3.0	1/6
1	0	1	1	1	1	1	251	55	1	6	313	2.7	6/1
1	1	0	1	1	1	1	298	42	5	3	348	3.0	6/1
1	1	1	0	1	0	1	76	32	1	3	112	1.0	5/2
1	1	1	0	1	1	1	118	30	0	1	149	1.3	6/1
1	1	1	1	0	1	1	238	50	1	2	291	2.5	6/1
1	1	1	1	1	0	1	233	25	4	3	265	2.3	6/1
1	1	1	1	1	1	0	407	48	7	1	463	3.9	6/1

Table 9.10

Comparison of Combinations of Models
Predicting High Rate Offender at One Year
(Selection Ratio = Base Rate)

Models							Observed:						
Hazard	Social	POS	Anamnestic	Juv	CJS	Control	Success	Failure	Success	Failure			
							True Negative	False Negative	False Positive	True Positive	N	% of Total	Type
1	1	1	1	1	1	1	6999	325	3	0	7327	62.5	7/0
ALL OTHERS							913	352	524	188	1977	16.9	-
1	0	0	0	0	0	0	0	0	316	34	350	3.0	1/6
1	0	1	0	0	0	0	10	6	67	17	100	.9	2/5
1	0	1	1	1	1	1	242	38	1	5	286	2.4	6/1
1	1	0	1	1	1	1	495	64	4	5	568	4.8	6/1
1	1	1	0	1	1	1	132	33	0	0	165	1.4	6/1
1	1	1	1	0	1	1	236	41	0	2	279	2.4	6/1
1	1	1	1	1	0	1	190	18	3	2	213	1.8	6/1
1	1	1	1	1	1	0	399	47	3	0	449	3.8	6/1

Table 9.11

Summary of the Accuracy and Communality of Individual-Level Predictions

	Dependent Variable							
	Rearrest Five Years	Rearrest One Year	Arrested Persons Off. in Five Yrs.	Arrested Persons Off. in One Year	Imprison-ed Within Five Years	Imprison-ed Within One Year	High Rate Offender Five Yrs.	High Rate Offender One Year
Base Rate	48%	23%	20%	7%	19%	6%	10%	10%
Predictions Correct Using All Domains	73%	79%	81%	93%	85%	94%	n/a	n/a
Predictions Correct Using All Domains and Hazards	73%	79%	81%	93%	86%	94%	n/a	n/a
Percent Unanimous Predictions Across All Domains	21%	41%	36%	70%	48%	75%	64%	63%
Predictions Correct Within Unanimous Subgroup	84%	90%	92%	96%	96%	98%	97%	97%

"n/a" indicates that this form of the variable was not analyzed in previous chapters.

Table 9.12
Comparison of Eight Measures of Recidivism: Treating All as White Males

Prediction of Full Model:		Prediction If Treated as White Males															
		Rearrest Five Years		Rearrest One Year		Arrested Persons Off. in Five Yrs.		Arrested Persons Off. in One Year		Imprisoned Within Five Years		Imprisoned Within One Year		High Rate Offender Five Yrs.		High Rate Offender One Year	
		No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Rearrested Five Years	No	5654	449														
	Yes	450	5196														
Rearrested in One Year	No	-	-	8744	282												
	Yes	-	-	285	2403												
Arrested Persons in Five Years	No	-	-	-	-	8994	399										
	Yes	-	-	-	-	400	1956										
Arrested Persons in One Year	No	-	-	-	-	-	-	10717	180								
	Yes	-	-	-	-	-	-	178	639								
Imprisoned Five Years	No	-	-	-	-	-	-	-	-	9302	256						
	Yes	-	-	-	-	-	-	-	-	255	1936						
Imprisoned One Year	No	-	-	-	-	-	-	-	-	-	-	10962	102				
	Yes	-	-	-	-	-	-	-	-	-	-	99	551				
High Rate Offender Five Years	No	-	-	-	-	-	-	-	-	-	-	-	-	10387	181		
	Yes	-	-	-	-	-	-	-	-	-	-	-	-	181	1000		
High Rate Offender One Year	No	-	-	-	-	-	-	-	-	-	-	-	-	-	-	10413	135
	Yes	-	-	-	-	-	-	-	-	-	-	-	-	-	-	134	1032

CHAPTER TEN

RECIDIVISM AND SENTENCING POLICIES

This study has aimed at developing the linkage, at both the theoretical and applied level, between the concepts of sentencing and recidivism. Implications of the research extend at the theoretical level to those interested in assessing the probability of recidivism and in understanding what causes offenders to recidivate. At the policy level, our results are relevant to several policy directions that involve the use of recidivism prospects for decision making.

Although the empirical research conducted here has been concerned, in part, with the specific decision making of judges within an "indeterminate" system, we argue that the implications of the research extend not only to the judiciary in more "determinate" systems, but more generally to those who decide other components of the CJS intervention. These include probation, corrections, and parole officers who must routinely make many of the same evaluations that we will argue are appropriate for judges to make. Thus, the implications of our research are of a more general nature.

One of the strengths of the present study has been our ability to investigate both the full range of sanctions given to offenders and the wide variety of individuals who receive those sanctions. We have avoided analyzing subgroups of offenders (e.g., those convicted of only property crimes) as well as particular subsets of sanctions (e.g., those receiving State Prison sentences). In this sense our analyses have been true to what is most often done by the State Courts in New Jersey. Decisions must be made across a broad spectrum of offenders, and many options are available for a judge to consider.

Our study reflects this diversity.

The investigation into possible mediating influences as a consequence of statistical interactions lends support to a focus on the full range of offenders and sanctions. While there are important differences in what we have labeled as our "basic model," (and these will be discussed below), we find little to suggest that general processes of recidivism differ for specific subgroups of offenders or groups defined by those receiving different sanctions. While a broad spectrum of offenders may be serviced by the various parts of the CJS, this breadth does not translate to the need for different models of subsequent offending, either for variety of individuals who face sentencing, or for those who are sent to different parts of the CJS as a result of that sentencing. In retrospect, the decision to have one basic model for all individuals, and all forms of recidivism, was justified. Moreover, the general lack of robust interactions augments our claim that the current results speak to more than simply those CJS interventions represented by the sentencing of convicted offenders.[1]

It should also be remembered that all of the data used in our analyses come from official sources. No attitudinal measures were available to us and this has limited what could be studied, both in terms of what might be

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[1] An important caveat about our basic model deserves reiterating. Had we analyzed only one aspect of recidivism, or, by fiat, decided to investigate some subgroup of offenders, a substantially different model (i.e., set of predictor variables) might have resulted. However, as we argued in previous chapters, our variable selection strategy, coupled with the search for robust interactions, has found where the most important differences are across many subgroups defined by numerous characteristics. This allows us to compare recidivism processes across subgroups much more easily than would be the case if group-specific models had been developed. Moreover, as we have seen, a focus on only one definition of "recidivism" would have missed many of the complexities of the phenomenon. Our focus on the full range of offenders is most appropriate for informing as many parts of the CJS as possible.

predictive of recidivism, and the outcomes of the sentencing intervention. Yet, by focussing only upon official record data, we have used information that is (or should be) available to all parts of the criminal justice system. This too broadens the audience for our results.

Unfortunately, translating our findings into policy is not straightforward. We have seen that, at a very detailed level, "things matter." Which variable is taken to indicate recidivism, how long one looks for recidivistic behavior, which variables predict recidivism, and the magnitudes of variables' impacts on recidivism all have consequences for the conclusions reached. Basing social policy on such details is not feasible, and we are forced to back away from many of the specific details if policy implications are to be drawn from our research.

If, however, one views the development of policy as a series of <u>decisions</u> that must be made, our findings and conclusions can be most instructive. We can point to what we think are the important policy decisions that need to be made, and, at a more specific level, what any consequences of those decisions might be. This is the thrust of the current chapter. We highlight the findings from our many analyses with an eye towards their implications for future sentencing policy in particular, and, in general, the more effective use of CJS resources for intervention in offenders' criminal careers.

Like those that have preceded it, this chapter is broad in scope. We first revisit the relationship between recidivism and theories of criminal behavior, emphasizing that how risk for recidivism is modeled and used depends, in part, on decisions about the etiology of criminal behavior in general. We then overview how the sentence received has impacted on the

recidivism seen in this sample and on the magnitudes of the effects found in earlier chapters. Here too, the discussion focusses on the consequences of using our findings for the development of CJS policy.

Our major policy recommendation is that sentencing policy be (re)formulated to incorporate formally-assessed risk for recidivism at the time of sentencing. Such a policy raises several issues and would necessitate many important decisions. Among these are: the justification for risk-based sentencing strategies; how individual behavior is to be predicted; how recidivism is to be defined for policy decision making; how assessed risk may be used at sentencing; and, what a risk-based sentencing policy might look like. Sections are devoted to each of these issues. Finally, we close with an overview of what we see as the future of sentencing and recidivism research.

RECIDIVISM AND THEORIES OF CRIMINAL BEHAVIOR

We argued in Chapter Two that empirical studies of "recidivism" should not be narrowly conceived as separate, or autonomous from general theories of crime. We drew upon the literature of the etiology of crime in formulating a "general model" of recidivism in Chapter Five. Various "social structural" theories of crime causation were discussed as well as "criminal career" approaches to the prediction of future criminal behavior. A general model was derived from our theoretical and empirical considerations, reflecting our goal of including as many relevant theories of crime for which we had measures. After dropping from further consideration many variables that constituted weak predictors of recidivism as variously measured, the basic model (43 independent variables) covered a broad range of theoretical perspectives.

Social structural characteristics, such as the employment status of the offender, race, gender, area of residency, educational achievement, drug dependency, and so on, were included in the general model. Criminal career concerns were differentiated into four areas: attributes of the offender's presenting offense, prior adult arrests and convictions, offender's juvenile delinquency career and prior CJS interventions (and some of the offender's reactions to such interventions). In addition, general control variables (such as the offender's age) were included in the basic model developed in Chapter Five.

Although we do not claim that this model adequately reflects all theories of crime, we maintain that it does cover a wide range of commonly discussed theories and measures. Furthermore, it is a product of a variable selection process that is both unusual in its scope and in the diversity of predictor variables tested. Given the measures that were initially examined, by the selection and inclusion criteria discussed in Chapter Five, we feel that our basic model speaks an even a wider array of independent variables and theoretical perspectives. The model contains numerous and diverse independent variables.

The specific results of our models of recidivism show that each of the general domains of independent variables discussed is important for the explanation of recidivism. That is, at least one variable from each of the etiological theories was found to be predictive of most forms of recidivism. Particularly strong predictor variables include the offender's race or ethnic heritage, the nature of the presenting offense (property or persons), having pending charges, number of prior arrests, number of charges in the past five years, number of arrests as a juvenile, years since first incarceration,

number of prior incarcerations, as well as two general control variables, offender's age and offender born out of state. Thus, the social structural, presenting offense, anamnestic, juvenile delinquency career, and prior CJS intervention domains all represent perspectives that seem important to the explanation of recidivism.

In addition to these "main effects" of independent variables, a broad search was undertaken to find where the effects on recidivism were consistently mediated by other characteristics of the offender. The offender's race, age, presenting offense, and anamnestic characteristics were most frequently involved in interaction effects on measures of recidivism. Our contribution here is somewhat unique in focussing on such interaction effects.

It is the very same "generality" of the model that we developed in Chapter Five that leads us to consider some of the critiques of such models. Critics of general models have pointed out that there are empirical implications of simply choosing a general model of criminal behavior, and ignoring issues of the logical and temporal relations among the theories specifying those models. Specifically, some have expressed concern about the implications of including independent variables that vary in their "conceptual proximity" to the dependent variable. The attribution of the causal and predictive importance of any independent variable is a function of other variables included in the model. Thus, the researcher, by virtue of a decision to include or exclude variables varying in "conceptual proximity," or a decision to ignore causal order issues among the independent variables, effectively controls the designation of what is important or unimportant for the prediction and explanation of recidivism. The implications of making some

of these choices among the independent variables have been explored in some detail in Chapters Five and Nine, as well as to a more limited extent in Chapters Six through Eight.

Most research applications involve the use of some or all of the types of variables employed in our analyses, with little or no consideration given to the implications this rather "catholic" approach to crime explanation or recidivism prediction. Within the etiology literature, as discussed in Chapter Two, concern has been expressed for treating all general theories of crime as if they were logically equivalent, doubts over the value of "throwing everything into the hopper" have been expressed. Two general reasons for not doing so pertain to the question of temporal sequencing of causes of crime and to issues of conceptual proximity. The first refers to considerations as to which measured variables pertain to characteristics of the offender temporally and causally antecedent to other characteristics. Thus, race may be conceptualized as antecedent to not having a job; not having a high school diploma may be considered antecedent to number of arrests as an adult. If explanatory weight is given to the temporally prior variables a priori, then the variables that measure the subsequent characteristics of the offender may be conceptualized as products of the "earlier variables." That is, they may be conceptualized as "intervening variables" between the earlier ones and recidivism. In a regression equation with all independent variables controlled simultaneously, the antecedent variables must compete with the temporally subsequent variables on an equal basis. The result is that the effects of the temporally earlier variables are smaller (or non-existent) than it would be if the subsequent variables are excluded.

Unfortunately, there are few general rules or precedents to draw upon in making such choices or in giving weights to certain kinds of explanatory variables. For example, having relatively many prior convictions at the time of sentencing might be conceptualized as a factor causally subsequent to other factors such as educational achievement. As such, it can be conceptualized as a result of lack of educational achievement, as well as a cause of subsequent recidivism. Also, it can be seen as a "conceptually proximate" cause of recidivism. The latter consideration may lead one to drop the "conceptually proximate" cause in order to stress the importance of lack of educational attainment.

The results of the communality analyses for the six general domains of predictor variables points toward these general issues in the theoretical explanation of crime. For forecasting recidivism, the choice of a domain of independent variables has certain consequences for the degree of successful recidivism prediction (Chapter Nine); from the point of view of explaining recidivism, the implications of communality are potentially more profound. In the aggregate analyses of Chapter Six, the relatively large degree of communality among the independent variables suggests an interchangeability among the predictor variables that lessens the consequences of choosing one over the other. Yet, the understanding of the processes that predict recidivism may be greatly affected by such choices.

For example, recidivism can be explained fairly well by either anamnestic variables or by social structural variables. Choosing to omit anamnestic variables, for example, would result in some loss of explanatory power, but also a fundamentally different interpretation of what is causing recidivism to occur. Specifically, aspects of the social structure, such as

dropping out of high school, not holding a job, having a drug problem, living in an urban area, and the offender's race (which itself measures a host of social structural aspects not otherwise directly measured in our research) could be chosen to the exclusion of other domains of independent variables. Although there would be a reduction of about a third in explained variance of most measures of recidivism by such a choice (and this decision would also impact which specific individuals are predicted to recidivate), it would lead to a completely different explanation of why the offenders go on to recidivate than would be the case if anamnestic variables alone were entered into the regression models. Including anamnestic variables in addition to social structural variables in the models, for example, results in considerable communality between these two sets of predictor variables in the variance partitioning and individual-level analyses.

A sharing in the attribution of what explains recidivism in the regression analysis where individual coefficients are the focus also occurs. While it may be the case, for example, that all the social structural variables are statistically significant predictors when only social structural variables are entered in the analysis, this does not happen when anamnestic variables are also included in the regression model. Thus, as is often found in research applications involving both of these types of variables, number of prior arrests or convictions explain some proportion of recidivism "at the expense" of the effect of some social structural variables. In short, quite different views of the causal forces involved in generating crime are a consequence of the choice of independent variables.

Some of our results pertain more to factors to be considered in future recidivism studies than to the theoretical issues surrounding the study of

recidivism. The results pointing to at least three different dimensions of recidivism indicate that there is a degree of specialization in offending behavior, and are suggestive types of offenders. That is, the results show that persons offending (recidivistic acts of physical harm) are distinguishable from other types of behavior that also constitute recidivism. Specialization may occur in the sense that certain types of offenders are more likely to commit a subsequent persons crimes: having prior arrests or convictions for persons crimes is a good indicator of this. As well, the effects of independent variables for the type of presenting offense, coupled with some robust interactions involving the kind of presenting offense, add to the empirical support for some degree of offense specialization. Thus, our findings argue against claims made that specialization in offending is illusory (Gottfredson and Hirschi, 1990), and are supportive of a general typological approach (Chaiken and Chaiken, 1982; Gibbons, 1977).

We also find consistent evidence that recent chronicity is strongly related to levels of recidivism. Knowing how often the offender has been charged in the last five years, whether or not the individual had been incarcerated at some point two years prior to sentencing, and recent behavior and outcomes while on parole and probation are all predictive of recidivistic offending. This supports those who argue for differentiating among individuals who are in active or quiescent states (Maltz, 1984), as levels of recent activity appear predictive of recidivism and thus influence the likelihood that any sentence will effectively result in some form of crime control.

Conversely, we also find that more is involved than simply recent criminal behavior. Criminal behavior as a juvenile, including how long the

individual has been engaging crime, is also related to levels of recidivism. For the younger individuals in this sample, behavior as a juvenile is simply another measure of recent chronicity. Still, the mean age of the entire sample (approximately 28) is well beyond the juvenile years. Thus, studies of recidivism should also give consideration to prior behavior over longer periods of time.

One of the implications of our discussions on the use of theory for the study of recidivism is that, perhaps, the major policy decision revolves around what theory, or theories, of criminal behavior are to be used when fashioning policy. The ramifications of this choice are far reaching, touching on both the nature of CJS interventions and treatments, and which domains of variables are selected to model criminal behavior. If one adopts any of the variants of what we have labeled "social structural" theories, interventions that address structural inequalities are suggested as the most appropriate treatments. Thus, emphasis is given to programs to increase education, training, and employment opportunities for convicted offenders. Consistent with this are sentences that mandate participation in certain programs as part of the sanction.

Similarly, adopting social structural theories for the underlying model of criminal behavior leads to the use of a distinct set of predictor variables in any analyses. Independent variables capturing residential stability, education and employment histories, ties to the community, and indicators of social strain all become relevant. Even variables such as race, gender, and ethnicity become legitimate, if only as statistical controls for other variables in the models used to develop policy. And, if social structural theories are to guide policy and recidivism research, the variables suggested

by those theories may be given priority, even if they are less causally and conceptually proximate to the criminal behavior of the individual.

Conversely, the decision to adopt anamnestic theory, or the more general criminal career paradigm, for the purposes of developing policy and studying recidivism places the focus elsewhere. Sentencing policies giving greater emphasis to prior criminal behavior and recent chronicity are suggested, as are selective incapacitation strategies. Why people commit crime may have less import than how much crime is committed. Recidivism becomes more appropriately defined by volume and rate of offending and models of it incorporating more aspects of prior record, criminal behavior as a juvenile and, perhaps, offense specialization are suggested.

The various theories about criminal behavior need not be seen as mutually exclusive. In fact, our basic model for forms of recidivism draws upon aspects of all of these theories. Yet we emphasize that, for the purposes of developing sentencing and intervention policy, there are real consequences for the policies instituted. Which theories are to be used, or which parts of multiple theoretical perspectives are to be merged, is a crucial decision that needs to be made explicit.

THE EFFECTS OF SENTENCES ON RECIDIVISM

This study had as one of its goals the assessment of the impact of specific sentences on the subsequent recidivism of the offenders. We offered a much broader conceptualization of the sentence than is usually found in such research. In addition to the traditional aspects of "in versus out," "where," and "time," measures of how the sentence fit into the offenders overall history of sanctions were introduced.

Choices in sentencing decision making are usually conceptualized in terms of what types of interventions should be applied to what types of offenders. To an extent, the results from Chapter Eight suggest that the sentencing system has been relatively successful in identifying who the high risk offenders are, and in imposing more restrictive sentences on those offenders. Thus, even in the absence of guidelines and formal risk assessment instruments, the judiciary are generally sentencing in accordance with some general crime control principles. Our interpretation of other aspects of the effects of sentences leads to the additional conclusion that, as a whole, the system has "worked" in the subsequent monitoring of offenders. This too is in accordance with general crime control goals.[2]

No panacea for reducing recidivism by the implementation of particular types of sentences is, however, found from our research. While the nature of the CJS intervention does make some difference to the prediction and explanation of recidivism, both the ability of the sentence as a whole to increment explained variance, and the magnitudes of the effects for particular aspects of the sentence, are generally small. Many aspects of the sentence were found to be unrelated to subsequent recidivism.

After considering characteristics of the offender, prior record, and the nature of the presenting offense, we find that little impact can be attached to what we have labeled the continuous components of the sentence. Neither the dollar amount fined, the months sentence to jail, nor the months sentenced

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[2] We reiterate that the present study is not an exhaustive evaluation of what specific forms of intervention the offender actually received, nor does in address the effectiveness of various drug or other rehabilitative programs. The literature on programs and their effectiveness suggests that some of these programs have effects, although small and possibly short-lived. Our purpose here did not include such an evaluation component, nor were data available for such research.

to probation exhibit more than sporadic effects on the different definitions of recidivism used here. The subgroup of individuals convicted of property offenses does, however, seem to respond better to longer jail sentences than do those with other types of presenting offenses. Some promising effects are found for the length of time sentenced to State Prison, but these can easily be offset by increased numbers of previous property convictions. Overall, we find little to recommend that increased attention be paid to policy that focusses on the length of incarceration or probation supervision.

As well, we find results that are disappointing for those who would advocate a sentencing policy based on the use of increasingly severe sanctions. The dummy variable indicating that sentence followed a progressive pattern in the individual's sanction history was never found to be significant in Chapter Eight. This was true across all definitions of recidivism and all post-sentence observation windows.

The fact that the individual being sentenced is receiving his/her first sanction at the point of sentencing appears to be a crucial factor for recidivism prospects and how effective the sanction might be. In general, we find that those receiving their initial sanction are more likely to recidivate, however defined. Thus, the sentences given to this subgroup of offenders were not effective. Moreover, this lack of a desirable outcome can be exacerbated by increases in unsanctioned prior arrests: the greater the number of prior arrests that do not result in (officially reported) convictions, the higher the levels of recidivism seen after the initial sanction that is received.

A more positive finding surrounding the impact of initial sanctions pertains to how the blacks in this sample responded to them. A very robust

interaction effect was found for blacks receiving their initial sanction. For recidivism defined in terms of counts, adjusted rates, or time-to-failure, blacks receiving the first sanction of their career were significantly less likely to recidivate. Unfortunately, the exact mechanisms that produce this outcome are unclear.

There are several policy implications that follow from our results concerning the impact of the initial sanction on recidivism. Judges should be wary of "light" sentences simply because the individual has no (recorded) history of prior convictions or sentences. Those who are receiving their first sanction may be beginning their criminal career or be in an active state of offending, and future offending behavior may be likely. As well, both judges and prosecutors should pay particular attention to those individuals who have a history of unsanctioned prior arrests as these offenders are quite likely to respond poorly to any sentence that is administered.

More positive effects for these sentences are observed in that some interventions were found to result in lower recidivism rates than would be expected based on the background characteristics of offender. Specifically, sentences to State Prison, after statistical controls are introduced, result in measurable decreases in recidivism, although the general level of recidivism for these offenders is still quite high (e.g., 62% are rearrested within five years). Whether such effects can be linked to the ideas of specific deterrence or rehabilitation is not possible to determine with the data analyzed here.

We also find that the more severe sanctions of jail confinement, and probation with additional conditions are related to reduced levels of recidivism. Relative to the reference group of simple probationers,

significant effects for the indicators of various forms of jail sentence studied, and that for probation in conjunction with other sanctions, were found across most measures of recidivism. As with the State Prison "effect," however, high absolute levels of recidivism are observed, especially for those receiving jail sentences.

While there is no doubt that for some individuals the interventions represented by these sentences have resulted in the cessation of their criminal career, and thus rehabilitation or specific deterrence has been achieved, for most types of sanctions high absolute levels of recidivism are still observed. If the goal of sentencing is to stop future offending, these sanctions have not "worked." The high levels of recidivism, coupled with generally negative effects observed for most incarcerative sentences, lead us to interpret the effects of these sanctions more in terms of some form of incapacitative effect: levels of recidivism would have been higher still had not these individuals been in custody. As well, the more severe forms of probation sentences are associated with reduced recidivism which can be interpreted as successful monitoring of individuals while under probation supervision. Overall, these sanctions have led to crime being "controlled," but not eliminated.

An alternative explanation for the impacts of these sanctions is that sentences to incarceration delay recidivistic behavior. Periods of adjustment after release are associated with lowered recidivism, and it is not until after this adjustment that some individuals resume criminal behavior.[3] In fact, the only beneficial impact of sentences to the Youth Complex at

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[3] Others (e.g., Schmidt and Witte, 1984; Maltz, 1984) have found results suggesting that interventions may only slow down additional offending.

Yardville we have observed are associated with decreased rearrest within one year. After that point, this group of individuals produces the highest levels of recidivism across all sanction groups. Here too we see only a delay in subsequent offending. It would be appropriate to call these effects "temporary rehabilitation" or "temporary specific deterrence."

We see the policy implications of our interpretations of the effects of these sentences as hinging upon how "successful" intervention is to be defined. Our results are clearly consistent with general crime control goals, but not with more specific aspects of those goals. Some individuals cease offending, others enter a quiescent state for a period after release, and, for some, the intervention has no impact on criminal behavior. To strictly define a successful intervention only in terms of absolute specific deterrence or total rehabilitation ignores the fact that these goals may be achieved at least for short periods of time.

Observation Windows

Throughout we have been concerned with how the length of the post-sentence observation window may influence the results and conclusions reached. We have seen that the width of the observation window can matter greatly for what is a significant predictor of recidivism and for the magnitude of a variable's impact on recidivism. In addition, we have found that the longer one looks for recidivism behavior, the more of it one finds.[4] As the time from sentencing and release increases, greater proportions of individuals are rearrested and reimprisoned, and more arrests, charges, and convictions are accumulated.

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[4] The exception here is in rates of offending adjusted for time at risk. These measures steadily decrease with longer windows.

The width of the post-sentence window thus bears directly on conclusions about the effectiveness of sentencing. Few would argue that sentences should have a lasting effect upon individuals. The further removed the individual is from any incarcerative experience, or parole or probation supervision, the more likely that factors other than these interventions will produce any additional criminal behavior.[5] However, there are no firm guidelines as to when these sanctions should stop exerting their effects on subsequent behavior.[6]

Our findings point to, minimally, a distinction between short-run and long-term recidivism. Indeed, the one-year and five-year windows used in Chapter Nine appear quite serviceable for maintaining this distinction. However, to choose one over the other is yet another decision with important consequences. Given the current findings, we know that a short window will lead to the conclusion that many interventions are successful in producing reductions in crime, either through specific deterrence or rehabilitation. The selection of a longer window yields the conclusion that interventions have little impact on subsequent behavior, especially if only mean levels of recidivism are studied. Neither conclusion is correct.

We contend that, at the level of policy, considerations of the temporal aspects of any intervention effects should be made more explicit. In part, these decisions revolve around the stated purposes of any CJS intervention. If the goal is to produce total rehabilitation or absolute specific

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[5] In that we find social structural factors are better predictors of recidivism over longer observation windows, this claim is supported.

[6] One implication is that more sophisticated mathematical models of intervention effects should incorporate a decay parameter to allow for effects of diminishing magnitude. This is one strategy for identifying an "ideal" observation window.

deterrence, then program evaluation should cover longer periods of time -- with the recognition that strong positive outcomes are unlikely to be found. Conversely, if the intent is to produce a short-run crime control outcome, a shorter evaluation window is appropriate -- with the caveat that beneficial outcomes are likely to be overstated.

Judicial Discretion

The investigation into how subjective assessments can predict recidivism has produced mixed results. Subjective measures for general offender "badness", the seriousness of the presenting offense and the individual's prior record, and prosecutors recommendations for incarceration all failed to pass the variable inclusion criteria. Once the battery of objective measures, upon which these subjective assessments are presumably based, are controlled, such assessments have little predictive or explanatory utility. The notable exception is the probation officer's prognosis for recidivism. No matter how we have measured recidivism, this subjective measure has been found significantly related to continued criminal involvement.

We view the robust findings surrounding the prognosis measure as important for, of all subjective indicators available, it is the only prospective assessment, and, as we have seen, it is the only subjective measure that is related to subsequent recidivism. Those individuals who are thought unlikely to recidivate tend not to, while those who are assessed as likely to recidivate tend indeed to get rearrested, reimprisoned, and so forth. Consideration of the objective aspects of the offender and the presenting offense reduces the need for subjective evaluations of those factors, but there is still a place for human "guesses" at what someone might do in the future.

One implication for this finding is that an effective sentencing policy should allow for some judgement as to future recidivism. What judges and other CJS officials think about the prospects for rehabilitation, or the lack thereof, has some predictive utility beyond the more objective variables that are often used to determine a sentence. This necessarily involves allowing for judicial "discretion" and it is this discretion that has been criticized for leading to unequal treatment of "similar" offenders. Yet if "discretion" is defined in terms of expectations for recidivism, it can be helpful in achieving some forms of crime control.[7]

Both advocates and opponents of the various goals of sentencing (see Chapter One) can find some solace in our results surrounding the impact of sentences on recidivism. Custodial sanctions are associated with lower levels of recidivism. Nonnegligible numbers of individuals do not recidivate after sentencing. Yet, two aspects of these findings stand out. First, and most importantly, what the offender brings to the court room, both in terms of social structural and prior offending characteristics, is much more strongly related to levels of subsequent recidivism than what happens to him/her at sentencing. Second, as just noted, judgements as to future recidivism have some predictive ability. These, and in fact, the vast majority of our findings, direct the policy implications <u>away</u> from how the sentence received might influence recidivism. Before elaborating this point, however, we discuss the implications of how "big" the observed effects actually are.

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[7] As was reviewed in Chapter One, states with sentencing guidelines allow for departures from recommended sentences and the evidence suggests that rates of departures are quite high. Our point here is simply that beyond the practical need for departures, we have found empirical justification for the place of subjective assessments in the sentencing process.

THE MAGNITUDE OF EFFECTS

A fair characterization of our findings is that effects are exceedingly small, and this holds true for almost all of our results. Social structural factors augment the explanation of recidivism by between two and four percent after considering predictors from other domains. All aspects of the sentence account for an additional two percent at most. Statistically significant variables also have an impact that appears minimal. For example, having a job after sentencing decreases the probability of reimprisonment by about .04 and this is quickly offset if the offender had been incarcerated two years prior to sentencing. As seen in Chapter Nine, treating all defendants as white males results about a changed predictions for about 7% of the sample. The findings of the previous chapter also demonstrate that the choice of what is used to predict expected recidivism at the time of sentencing matters, but fairly comparable predictions will be made no matter which domain of variables is used for prediction.

In part, our findings reflect the complexity of the phenomena studied. The factors that lead an individual to commit a crime, and lead the authorities to detect, report, and prosecute that offense, are, at best, only partially understood. Our contention that recidivism is simply one aspect of criminal behavior in general seems supported by our findings as what can be explained about recidivistic behavior is tied more to the social structural position of the individual and prior criminal behavior than to the CJS intervention. But that contention also increases the difficulties in theoretically and empirically explaining recidivism. To find "only" small effects is not surprising, and if "big" effects did indeed exist, researchers would have observed them long ago.

The six general domains of independent variables are not autonomous in their empirical associations with one another, and thus we find considerable communality among the various types of independent variables that can explain and predict recidivism. Roughly half of the explained variance across all recidivism measures focussed on in our analyses is shared by three or more types of independent variables. Depending upon the recidivism measure, between about 20 and 75 percent of offenders will have their recidivism predicted the same way no matter which domain is used for predictive purposes. The implication of this is that recidivism (of all types) can be predicted and explained to about the same extent with virtually any one of the types of variables used in our research.

One could easily take such results to mean that there is little danger in dropping, potentially many, variables from future consideration. This can be particularly attractive for policy reasons. Variables like race, ethnicity, and gender are easily dismissed on moral or ethical grounds. We have used a myriad of measures for prior record: it is tempting to ultimately use only one or two of them for practical or policy reasons. Objections could be raised to the use of offender age as the prediction of recidivism is incorporated into CJS policies. Yet, as we "chip away" at the variables that might be used, there are consequences. No one group of recidivism predictors can account for more than about 80% of the variance that all independent variables combined (the "full model") can explain. Thus, an argument can be made for maximizing prediction by including more than one domain of independent variables, and as many variables as possible, in the prediction of recidivism. Minimally predictors from these two general types should be included as they uniquely account for the largest proportions of explained

variance across the thirteen measures of recidivism studied in Chapter Six.

Still, the advantage gained as the number and types of variables used to predict recidivism is increased would be incrementally small. Yet these small increments in explained variance, or predictive accuracy, should not be dismissed quickly as, in the aggregate, they add up. Given the volume of cases processed annually by the courts, even small specific deterrent impacts on offenders' behaviors will yield a considerable decrease in overall crime at the societal level. Even if employment leads to only a small reduction in the probability of recidivism, this reduction has greater import when summed across all offenders. To misclassify the potential risk for recidivism of one person is a tragedy for that individual. But the cumulative impact of misclassifications rapidly becomes a moral and practical problem for the system as a whole. Any of the findings of previous chapters, when viewed from the societal or systemic perspective, have greater import than is suggested by the magnitude of the effects seen in earlier chapters.

Ultimately, it is a policy decision as to which variables should be used to assess the risk that an individual poses to the community. Objections can be raised over the use of any of the predictor domains included in our models. A "just desserts" sentencing philosophy leads to using only aspects of the presenting offense at the expense of all other kinds of variables. Criminal behavior as a juvenile becomes irrelevant if those records are to be sealed once the individual turns age 18. Our group of social structural variables may be deemed as ethically and legally unacceptable (see below) as offender race and so forth are included in this domain.

To incorporate our findings into more formalized policy will thus necessitate choices about which sets of predictor variables are to be given

the greatest emphasis. We believe that our results clearly demonstrate what the consequences of those choices might be. Little will be lost if some independent variables are ignored. Yet the loses add up as policy decisions increase the number and types of independent variables that are deemed unacceptable for use.[8]

RISK ASSESSMENT AT SENTENCING

Our results throughout point to the fact that it is counterproductive to view the sentence (to be) administered in isolation from other factors. Information about the offender and the presenting offense are quite predictive of subsequent recidivism, much more so than the sanction received. To isolate any sentence from these characteristics ignores their impact on how effective a sanction might be. Similarly, we saw in Chapter Seven that nearly half of all sentences in this sample stood a chance of having their "effect" confounded with other CJS interventions that were, or were likely to be, in force simultaneously with the sentence under study. When the additional interventions associated with high levels of recidivism are added in, an even greater confounding of CJS interventions is seen. To isolate a sentence from other sentences and sanctions ignores the fact of interdependence among these multiple interventions.

Taken together, we see these results as arguing strongly against a purely retributionist approach to sentencing. The philosophical contention underlying a true "just deserts" approach to sentencing is that the majority

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[8] The findings of Chapter Nine suggest that, in the extreme, policy makers could use the likelihood of the individual appearing before the court for sentencing (i.e., the hazard variables) for predicting recidivistic behavior. Ignoring all other domains of variables would result in surprising little loss in predictive accuracy.

of factors, including the risk for recidivism, are a priori irrelevant to sentencing practices. While this is a decision that can be made at the level of policy, we see it as a decision that ignores the high rates of recidivism seen in Chapter Eight: when based on "pure principles," a retributionist sentencing philosophy may be said to be largely divorced from empirical reality. The lack of isolation of the sentence from other factors, and the predictability of recidivism, leads us to propose a sentencing policy that integrates as much information as possible: this is more consistent with our results. To the extent that more about the offender's previous behavior, and likelihood of recidivism, can be incorporated into fashioning the sentence, the sentence becomes less isolated from the factors that can influence how effective that sentence may ultimately be. Assessing risk for recidivism at the point of sentencing provides one means of incorporating such factors.

Use of risk assessments at sentencing has a long-standing legal basis. Morris and Miller (1985) have summarized this history, as well as reviewed some of the moral issues pertaining to risk assessment. Habitual offender laws, habitual petty offender laws, sexual psychopath laws, dangerous offender statutes, as well as the practice of sentencing itself, all suggest a strong existing basis for risk assessment at sentencing. It is only in the (relatively few) instances of some states where sentences are strictly mandatory[9] that some aspects of a risk-based sentencing policy may not be firmly established. Thus, neither the suggestion of risk assessment at sentencing, nor is the call for formal risk assessment at sentencing, is new:

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[9] We refer here to cases in which the judge's sentence is strictly determined by the statutes such that higher levels of punishment cannot be included in the sentence. In most instances "determinate sentencing" refers to a "required minimum," but these are not, strictly speaking, determinate sentences because the judge may sentence above the required minimum.

several states (e.g., Iowa, Maryland, and Michigan) already have risk assessment at sentencing that involves the use of formal instruments to predict subsequent recidivism . Rather, we argue that our results can help inform a number of theoretical, moral, and analytical issues surrounding the use of formal risk assessment at sentencing, and suggest ways in which risk assessment may be improved and expanded.

Two general aspects of our results support a further investigation into risk-based sentencing policies. First, what is known about the offender and his/her prior behavior is more predictive of subsequent recidivism than are the general parameters of the sentence received. The findings of Chapters Seven and Eight clearly demonstrate that, relative to information known prior to sentencing, the sanctions received only poorly account for differences in recidivistic behavior. Our "best guess" about the likelihood of recidivism revolves around aspects of the individual at sentencing, and these factors are relatively successful in predicting risk for recidivism.

Second, the interaction terms studied in Chapters Seven and Eight point to the fact that, in the absence of sentencing guidelines and grid sentencing schemes, judges have indeed implicitly assessed risk when fashioning the sentence.[10] The robust interactions between probation sentences and some measure of prior offending suggest that judges were correct (relative to various crime control goals) in giving probation terms to individuals whose prior record indicated otherwise. These decisions were successful in that offenders with longer prior records who were sentenced to some form of probation were significantly less likely to recidivate. Of course, risk

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[10] Some form of risk posed by the offender is also undoubtedly assessed by police officers in deciding to arrest a suspect, by prosecutors when deciding whether to forward a case for trial, and by judges when deciding bail.

assessment has been used both implicitly at sentencing, probably for most of this century. By formalizing risk assessment at sentencing, the implicit use of these predictions can be made more explicit and clear, especially as they relate to determining the sentence to be administered.

In addition, a risk-based system of sentencing can be justified as a better approximation of how judges actually sentence than either the sentencing grid approach or the pure retributionist or modified retributionist approaches proposed by von Hirsch (1975; 1987). Such approaches, because they are so far removed from the values that judges consider important at sentencing, result in high rates of "departures," in the first instance, and "no observed system" of intervention in the second instance. Thus, the Minnesota sentencing grid has been found to have rather high departure rates (Frase, 1991), and no one has ever tried to implement von Hirsch's proposed "just deserts" models of sentencing.

Formal assessment of offender risk can result in a more efficient utilization of recent innovations in the monitoring of some types of offenders (Huff et al., 1987; Petersilia, 1986). Of particular interest are intensive supervision programs (ISP). In New Jersey, eligibility for this program is strict, with offenders having to meet several requirements in order to participate (Pearson, 1987; Toby, 1987). Only those sentenced and committed to a State Prison are eligible to apply to the program, though individuals convicted of violent crimes, sex crimes, or some form of organized crime are excluded from consideration. Additional screening, using prior record and looking for evidence of heavy drug use, also determines eligibility for the ISP program. Note that these criteria lead to an initial assessment of risk for recidivism <u>after</u> a sentence to prison. Yet, this screening is quite

effective in that, by some accounts, participation in ISP leads to greatly reduced levels of recidivism. Using formal risk assessment at sentencing could lead to both a wider use of the ISP methods (frequent contacts with probationers, required work, curfews, drug testing, etc.) than is currently the case, and a more efficient use of these methods as those with the highest risk would be the focus of greater resources.

To date, explicit and formal CJS efforts in systematically evaluating individual risk have been largely limited to probation supervision and parole release instruments, although, as noted above, a few states use formal risk assessment at sentencing (Shane-DuBow, 1985). (The basic considerations of such instruments were covered in Chapter Two.) The precedent thus exists for formalized assessment of risk, but it is more commonly used after the offender has been sentenced to some sanction. Indeed, underlying our models is the assumption that risk assessment is a feasible alternative at the point of sentencing: much about the recidivistic behavior of the individual can be predicted before any sentence is administered.

These observations, as well as the empirical results from previous chapters, lead us to call for a more formalized use of risk assessment in the determination of an "appropriate" sentence for the convicted offender. The basic idea is that a score(s) representing assessed risk for recidivism be assigned to each person being sentenced, and, given this score the judge would have several sentencing options at his/her disposal: judges would have some latitude within a range of permissible sanctions. In this sense, we view a risk-based sentencing policy as similar in operation to that of sentencing guidelines (but not like most determinate sentencing schemes and mandatory sentencing policies). However, a risk-based sentencing policy would differ

from the majority of existing practices in that what determines the available options would be expected levels of risk for recidivism, and not options based upon retributionist principles.

Note that we are arguing for what is, in essence, another form of "funneling" within the criminal justice system. A nonrandom subset of offenders is to be chosen for selected treatment. Yet, it is precisely this form of funneling that is on the increase. As police standardize arrest decision making, as prosecutors formalize their decisions to prosecute, and as probation and parole risk assessment instruments gain wider acceptance, the selection of offenders into different types of intervention has increased. ISP clearly funnels certain types of individuals into the program through its screening procedures. We saw in Chapter Three that one consequence of the sentences administered to this sample was to create noncomparable groups of individuals. Even in states with more determinate sentencing practices, such as those defined by a cell of a grid, it is likely that groups defined by the different sanctions would be quite distinct.

We thus view a sentencing policy using assessed risk for recidivism as a way of making this implicit funneling more formalized -- for all types of offenders. The point of sentencing convicted offenders is a logical place in the CJS for making an <u>initial</u> assessment of risk and acting accordingly.[11] Knowing that an offender meets an acceptable level of risk makes him or her a reasonable candidate for some forms of intervention. Intensive supervision programs, assignment to levels of supervision, and the general search for

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[11] We stress that this assessment would only be the initial one. Additional evaluations would, of course, have to be made as the situation of the individual changes (e.g., good behavior in prison, or completion of a GED program while on probation).

intermediate forms of punishments (see below), are all ways in which risk assessment may be applied at the time of sentencing. In instances where unacceptable levels of risk were indicated, more traditional forms of incarceration could be employed.[12]

A formalized risk assessment at sentencing also represents a good use of system resources. As system resources get strained due to lack of bed space and personnel, more efficient use of these limited resources is needed. One of the rationales for using risk assessment to determine probation supervision is that it allows probation officers to use their limited time more wisely by giving the greatest supervision to those most in need. Similarly, it can be argued that what makes programs like ISP effective is the kinds of people in them: those who are most likely to succeed in the program are given the opportunity to participate. To the extent that sentences can be "matched" to offenders, or in the words of Andrews et al., (1990a) appropriate services can be delivered given a proper assessment of need, we see a risk-based sentencing policy as one way of achieving greater efficiency on a system-wide basis.

Using the assessed risk for recidivism is also consistent with most philosophies of sentencing. In Chapter One, we noted that crime control is the common goal underlying all but strictly retributionist sentencing practices. Thus, at the most general level, sentencing offenders on the expectations for future crime follows from most sentencing practices. But even at a more specific level, we see a logical consistency. Clearly a risk-based sentencing policy derives from the goal of selective incapacitation. Whether rehabilitation can be achieved depends, in part, on the ability to

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[12] What constitutes an "acceptable" or "unacceptable" level of risk is a crucial policy decision that must be made. We address this issue shortly.

place those most likely to be rehabilitated into the appropriate sanctions. The same can be said for specific deterrence. To the extent that assessed risk is indicative of the likelihood that some intervention will be successful, sentencing on the basis of risk for recidivism will be consistent with those specific goals.[13]

It is also possible to weigh the various goals of sentencing within the context of a risk-based sentencing policy. By definition, the assessment of risk for recidivism introduces considerations of crime control. However, formal risk assessment at sentencing must also be linked to retributionist concerns: it is doubtful that any sentencing policy that did not strongly differentiate between murderers and shoplifters would receive much political or public support. Therefore, both crime control and just deserts or retributionist concerns must be addressed.

Two ways of incorporating these goals within a risk-based sentencing policy are relatively straightforward. The first is to let the seriousness of the presenting offense determine the range of permissible sentences, within which crime control goals could be exercised. This could be achieved by having offenders within the same "level" of crime seriousness be eligible for greater or lesser sanctions based on recidivism projections. Offenders who are deemed to be high risk would be eligible for more time in prison (for example) than others within the same level of presenting offense seriousness. The second is to use a "modified" just deserts perspective and consider not

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[13] Even the more elusive goal of general deterrence may be met by a policy of sentencing using risk for recidivism. If it is known that more severe sanctions are to be given to individuals who are likely to commit crimes in the future, general deterrence might be achieved. Certainly such a policy would not result in any <u>less</u> general deterrence than that resulting from sentencing practices currently in use.

only the seriousness of the presenting offense, but also, say, the number of prior convictions. Each convicted offender defined to be "similar" using the level of seriousness and number of prior convictions, would be subject to the scrutiny of formal risk assessment. Thus, for example, the Minnesota Sentencing Guideline grid could be used to define categories of offenders, and within each cell offenders would be differentiated as to their risk of recidivism. Those with higher risk would warrant more punitive (and possibly more benign) interventions, than would the low risk offenders.

Other, more complicated forms of integrating retributionist and crime control goals within the framework of formalized risk ass ɔsment are also possible. Recall in Chapter One where we outlined the assignment of greater weight to certain goals over others, based on the severity of the presenting offense. In Figure 1.2 it was suggested that the more severe the presenting offense, the greater the weight that should be given to a retributive sentence aimed at punishment in proportion to the severity of the crime. As the severity of the crime lessens, however, greater consideration could be given to the likelihood of recidivism. Those offenders who are more likely to recidivate, somehow defined, would qualify for "more intervention" than offenders with less likelihood of recidivism. Choices could then be made among various crime control goals.

As summarized in Figure 1.3 of Chapter One, one offender may be judged likely to be a high recidivism risk under the criterion of being a high rate offender, leading to an appropriate intervention of incarceration so that the individual is incapacitated. Another may be judged a high risk offender via a rearrest but not as a high rate offender and may be sentenced to a number of community service hours with the hope of rehabilitation or specific

deterrence. Those who have committed less serious offenses, and are deemed unlikely to recidivate under any criterion, might receive no intervention or "administrative probation" (probation with no supervision). Note that this method of integrating the goals of sentencing with a formalized risk assessment is quite elaborate as the intervention is contingent upon both the seriousness of the presenting offense, the goal, and the recidivism criteria.[14]

The use of risk assessment at sentencing offers the opportunity for a different "system" of justice, several aspects of which warrant discussion. First, more than what seems to be the case in either actual sentencing decisions today, or in the various proposals for reforms of sentencing, the "system" proposed attempts to maximize the chances of equal treatment for all offenders. This can be achieved in the sense that everyone would be subject to the same formalized evaluation leading to a set of sentencing options. Most sentences today are carried out on the basis of a "judgment call" as to what is an appropriate sentence, with a great deal of discretion possible in the sentencing decision. Such discretion has been criticized as resulting in

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[14] The specific nature of the intervention given to an offender depends on the availability of programs, beds, supervisory personnel, and so forth, in a jurisdiction at a given point in time. In a relatively "progressive," well-financed system, options such as job tra .ng, adult education, drug and alcohol programs, and prison beds, would allow for more possible "interventions" for any offender. Conversely, a less progressive or well-financed jurisdiction would have fewer available options, perhaps limited to only the more punitive sanctions. Any specific implementations of what we are proposing here can only be made within the context of the resources available to the CJS at the time a convicted offender moves through the system. Because these resources vary considerably by jurisdiction, we will not prescribe specific sentences for various combinations of presenting offense seriousness and assessed risk under the different goals of sentencing. Rather, we only outline, in a general form, how decision making at sentencing might incorporate a formalized assessment of risk for recidivism.

bias, and in arbitrary sentences. While we view judicial discretion as desirable, we propose to limit it by a formal decision-making process where the severity of the presenting offense and the risk of recidivism determine the range of available sentencing options.

Unlike other attempts to limit discretion, such as the sentencing grids of Minnesota, Washington (Washington State Sentencing Guidelines Commission, 1987), or Pennsylvania (Kramer and Lubitz, 1985), our proposal incorporates the formal use of risk assessment as part of the sentence process. This recognizes the importance of considerations of community safety, as well as, the fact that judges actually consider recidivism at sentencing. Even those judges who use grid systems often depart from the prescribed sentence because of their belief that the offender is of relatively "low risk" of recidivism. By formalizing risk assessment, this component of potential bias and arbitrary sentencing is eliminated. Yet the importance of judicial discretion is also recognized as it would now be centered around choosing the particular intervention.

Second, the system that we propose also has the advantage of "solving" the problem of incompatibility between crime control and retributionist goals at sentencing. When retributionist considerations have priority over crime control goals, relatively few offenders have the specific form of their punishment defined by retributionist concerns. Only those convicted of the most serious crimes, a small percentage of all convicted offenders, are sentenced to imprisonment for a required minimum period of time. Crime control need not be a specific goal in the mind of the judge for these individuals. For the bulk of offenders, however, less serious crimes have led to a conviction. Here, the goals at sentencing for the judge are mostly those

of crime control (incapacitation, specific deterrence, rehabilitation).[15] Thus, by targeting recommended interventions to both the seriousness of the presenting offense and the assessed risk for recidivism, retributionist and crime control goals can be incorporated into sentencing decisions.

To formally implement a sentencing policy using assessed risk for recidivism will require several crucial decisions that will shape the ultimate form taken by such a policy. Among the most important decisions are how individual-level risk is to be predicted, including what independent variables are to be used and which are to be ignored. Also central is what aspect(s) of recidivism are to be incorporated into any assessment instrument. Given that risk can be assessed, there are still many ways in which the information may be used, especially as it pertains for suggested options for sentencing. In the sections below, we discuss how the results from previous chapters can help inform these decisions.

PREDICTING INDIVIDUAL-LEVEL RECIDIVISM

Developing a sentencing policy based upon the assessment of risk for recidivism necessarily takes us away from the aggregate models of Chapters Five through Eight. Rather, the emphasis shifts to recidivism expectations for individual offenders. Our aggregate results in Chapter Eight suggested that, in the absence of determinate sentencing practices, judges handed down sentences that, for at least some offenders, represented the successful identification of low risk individuals. It is precisely this kind of decision making that we contend should be formalized. To do so, however, requires more

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[15] Within the sphere of crime control goals, however, choices still have to be made among potentially competing goals, such choosing between incapacitation as opposed to specific deterrence. We address such choices below.

attention to the models and consequences of individual-level prediction.

The implications of choosing different types of predictor variables were explicitly developed in Chapter Nine, where models were used to generate predictions of recidivism at the individual level. Although there is a degree of correspondence across the different types of independent variables, the evidence suggests that between a quarter to a third of the predictions are different across the categories of independent variables tested (at least for selected dependent variables measured at one or five years). This difference is quite high relative to the base rates, and to the overall success in predicting recidivism when all the predictor variables of the basic model are used. Not only is it the case that choice of predictor makes a difference as to whom is predicted to succeed or fail, but, not surprisingly, some domains of independent variables make predictions more often at odds with the other domains. Thus, it was found that generally the anamnestic domain of predictor variables was least often at odds with the other predictor domains, and more generally, those domains associated with the criminal career perspective were also less likely to make predictions not in harmony with other specific domains.

Prediction of an individual's recidivism chances is also found to be affected by choice of dependent variable. No two indicators of recidivism are interchangeable. Whether or not offenders are evaluated as high rate offenders, or violent offenders, or offenders of any kind, all influence these predictions. Taking into consideration intervention effects, while resulting in relatively small contributions to explained variance, can result in many individuals being classified differently. Finally, the offender's race, ethnicity, and gender, if allowed in equations to predict recidivism for the

individual, would result in many being classified as positives or negatives who might not otherwise be so classified. The impact of using "adjusted" models to predict recidivism was shown to be considerable.

Thus, there are many arguments to be made that "it makes a difference" what is used to predict an individual's probability of recidivism. Yet, there are also commonalities in prediction of individual-level failure, as there were for the analyses in the aggregate. Thus, for example, the most extreme choice for predictor variables would be to ignore those from all of the six domains and simply use the hazard variables to predict the likelihood that the offender will recidivate. As we saw in Chapter Nine, this results in equivalent predictions for a surprisingly large proportion of individuals. While we do not advocate such a practice, we reiterate that many of the same variables (e.g., prior arrests, presenting offense) that are predictive of individual-level recidivism also predict how the case will be handled by the CJS: simply knowing the likelihood that an individual will appear before a superior court for sentencing can inform us as to levels of expected recidivism.[16]

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[16] The organization of the present chapter does not lend itself to a section containing summary observations about the impact of sample selection bias on our results. A few points should be made, however, about the performance of the hazard variables used in this research.

While it was never our purpose to compare the effects of the six hazard variables to each other, some regularities in the results have emerged. All individual hazard variables have been found to be significantly related to recidivism for at least some indicators of recidivism measured over some post-sentence observations windows. However, with the exceptions of the measures of the likelihood that the case will not proceed past arrest and will not result in a Superior Court conviction, the pattern of significant coefficients seems to be one of chance levels. This suggests to us that, prior to sentencing, the most crucial selection points in the CJS are in the hands of the prosecutors, both in continuing to forward the case through the system and in getting a conviction. Alternatively, these findings for the empirical behavior of the individual hazard measures supports the contention made in Appendix A that the selection bias measures are surrogates for variables (e.g., strength of evidence) for which we

have no direct measures. Note too, that the lack of any robust findings surrounding the hazard variable for the success in matching cases across data sources lends credence to the claim that we have not analyzed a very biased subset of the original Sentencing Guidelines data base.

At a more general level, we have seen little effect of these hazard variables on the magnitudes of the coefficients of our basic model, either in Chapter Six or Chapter Eight. (See Appendix B for the results that control for sample selection). Some changes are indeed found, but usually in the third decimal place of the estimated coefficient. Some variables cease to be significant and are replaced by others that become significant when the hazards are controlled, but this too is infrequent. We also found that some small suppression effects could be attributed to the impact of the sentence on recidivism. When the hazard variables were introduced, some indicators of the sentence have slightly larger coefficients.

The main motivation for using controls for sample selection is the potential for misestimating variables' effects on recidivism. Yet our results suggest that this misestimation will not be great, at least for the kinds of phenomena studied here. In fact, the impact of sample selection was much more dramatic for the hazard models of Appendix A than it was for the models of recidivism.

Several cautions should be considered before dismissing the need for controls for sample selection bias in future research. First, some of the statistical models that led to the creation of the hazard variables have a relatively poor ability to account for variation in the probability that a case proceeds past a particular CJS selection point. Pseudo R-squares never exceeded .169 for any of our models of a case proceeding through the CJS, and it tends to be true that those individual hazards which performed the poorest were measures taken from models with low explained variances. There is the expectation that, were we better able to model how a case moves past these decision points, the effects of these sample selection bias controls would have been greater.

Second, to our knowledge, this study represents the first time that multiple hazard variables have been simultaneously controlled. It could be the case that if only one were used, the effects on individual coefficients would have been greater. That is, selection bias effects may have been canceled by the competing hazard variables. More dramatic effects on the magnitude of the coefficients might have been seen if only one hazard variable were used, though we are hard-pressed to choose only one from that six that have been used.

Third, there is the caveat that sample selection bias may have a greater impact on individual level prediction. Granted that the coefficients change only minutely when these controls are introduced. But the results of Chapter Nine demonstrate that even small marginal changes can influence who is, and is not, predicted to recidivate. These are exactly the kinds of changes that might be expected if some variables (e.g., prior arrests) are given a slightly greater weight in the prediction. Exceedingly small changes in coefficients as a consequence of controlling for sample selection, may translate to larger changes in who is predicted to fail under some criteria.

Finally, the main conclusion, and one that was largely unanticipated, is that the processing of the case through the CJS is able to account for a relatively large proportion of variance across all definitions of recidivism. This conclusion is strongly supported by the results in Chapters Five and Six.

The fact that the choice of type of independent variable dramatically affects who is predicted to recidivate has been a largely overlooked aspect in recidivism prediction. Oddly, researchers have tended to focus on explained variance as an indication of predictive ability, and the contribution of individual predictor variables toward general predictive ability, and ignored the difference it can make to predict at the individual level. Yet, if a formalized risk assessment is to be incorporated into sentencing policies, it is precisely these kinds of issues that must be addressed. The implications of our results for assessing risk of individuals are discussed below.

The Trade-Offs in Assessing the Certainty of Predictions.

We have seen that predicting who will recidivate is not an exact science. Even in the "best case" scenario where all predictors lead to similar expectations of success and failure, some predictions will prove wrong. When interventions are tied to the designations of "expected recidivist" and "expected success," as they must be under a risk-based sentencing policy, the costs associated with inaccurate predictions increase.

How the uncertainty inherent in the prediction of recidivism is treated revolves around three factors. The first is the underlying base rate, or, more properly, the form of recidivism for which risk is being assessed. This sets limits on how accurate recidivism predictions can be as uncertainty increases as base rates approach .5. The second factor is the cut-off point chosen to define predictions of success and failure. These are easily manipulated so that the extent of false positives relative to false negatives can be controlled. Third is the nature of the interventions to be applied to those predicted to fail or succeed. The "costs" attached to inaccurate predictions vary directly with the severity of the punishments and treatments

prescribed. All of these factors are logically interrelated, and, ultimately all three decisions must be made at the level of CJS policy.

The implications for the choice of probability cut-off point to define expected success and failure may be assessed relative to "libertarian" versus "community safety" goals. A greater emphasis for the rights of the convicted offender, such that he/she is not punished unfairly or unjustly, can be viewed as a "libertarian" concern. At the opposite extreme is a concern for the community such that offenders are not free to inflict further harm on society through additional criminal behavior. In general, the research literature has not addressed the trade-offs associated with emphasizing one over the other (although see Blumstein et al., 1985). Policy, legal, and moral interests may dictate an emphasis and, consequently, the choice of a particular probability cut-off to define "high risk" offenders is linked to these interests. For example, if policy makers wish to limit the degree of "false positives" in CJS decision making, a higher cut-off probability is prescribed, with a net result of fewer individuals predicted to fail, but predicted with greater certainty.[17]

As demonstrated in Chapter Nine, choice of a cut-off point can be made with reference to the "civil-libertarian ratio." What is a desirable ratio, however, must be considered relative to the type of intervention implicated by the resulting classification of individuals. If the intervention is punitive in nature, then presumably higher "civil-libertarian ratios" would be chosen. If the intervention is relatively benign in nature (e.g., program

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[17] There are, however, limits to this line of reasoning. Our data indicate that the conditional probability of being a recidivist through rearrest involves over five percent error at a rather high cut-off of .9. Thus, it seems safe to conclude that some risk of false positive predictions will remain regardless of how expected success and failure are defined.

participation), then a lower ratio may be selected. The explicit linking of a civil-libertarian ratio to "punitive" or "benign" intervention decisions is an area of research that has not been examined fully, although Blumstein and colleagues (1985) have discussed these trade-offs in principle. By examining the implications of selecting one civil-libertarian ratio over another, across types of intervention, cut-off probabilities can be selected to as to minimize the undesirable decisions.

For example, if policy makers decide to give priority to the prediction of recidivism as measured by subsequent violent crimes, then it must be decided what cut-off point is acceptable relative to the dangers of false positives over false negatives. Some policy makers may argue for lower civil-libertarian ratios in that they are more concerned about false negatives than false positives for the prediction of violent offenses.[18] Presumably the likely punitive (incapacitative) interventions associated with being predicted to fail here would limit the lowering of the civil-libertarian ratio, as the number of "false positives" would be unacceptably high for such interventions.

In contrast, the assessment of risk for recidivism via rearrest may lead to a choice of a selection ratio made with a emphasis different from that for recidivism defined by violent crimes. If the recommended punishments are more benign, there may be less cost associated with an inaccurate prediction of failures, and false positives may be more easily tolerated. This suggests the use of lower civil-libertarian ratios to determine a cut-off point.

Clearly the trade-offs involved in inaccurate predictions of recidivism must be weighed by those wishing to implement a risk-based sentencing policy.

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[18] This is essentially what was decided in Minnesota, as the grid system itself represents a shift from earlier sentencing practices toward more punitive interventions being given to violent offenders rather than to property offenders.

A jurisdiction which is interested in "targeting" violent or high rate offenders may want to choose relatively low civil-libertarian ratios. Another jurisdiction, more concerned about the dangers of individuals being falsely predicted to fail, may choose higher civil-libertarian ratios. For any civil-libertarian ratio, however, the base rate of expected failure is crucial for the specific values selected. The consequences of a ratio, and the cut-off point implied, will vary with the size of the base rate and the shape of the predicted probability distribution. Thus, any policy that addresses these trade-offs can only be formulated in the context of data and information from researchers.

Ethical and Legal Issues in Recidivism Prediction.

In most of the analysis discussed in Chapters Five through Eight, blacks and Hispanics have been shown to be more likely to recidivate than others, regardless of the recidivism measure used. Other factors, such as being employed, and having a high school diploma, were also found to be predictive of recidivism. Concern may be expressed about the ethical and legal implications of using these kinds of variables to predict recidivism, especially if recidivism projections are to be used for sentencing decisions.

These concerns have long been part of the literature on recidivism prediction. Sparks (1983) and Goldkamp (1987), among others, have explicitly considered the implications of making particular kinds of choices among predictor variables. In brief, some have argued that all variables measuring race, or social attributes that are likely to be highly correlated with race, should not be studied at all, and certainly are not to be used to make any decision within the criminal justice system. Others have countered, however, that the omission of such variables from aggregate models of recidivism is a

form of specification error, resulting in a statistically biased assessment of the impact of other variables on recidivism. Thus, even if race is omitted from aggregate models of recidivism, race can still play an excessively large role in predicting (or explaining) recidivism via the other variables with which it is correlated. When models that do not include the variables in question are then used to make decisions for individuals, they remain "racist" or ethically reprehensible, as the consequence still may be greater punishment for blacks, or unemployed, now via the enhanced influence of, say, prior record variables that are correlated with these social characteristics. Thus, some have argued for keeping all theoretically relevant variables in the aggregate modeling component (the evaluation), and treating all offender's as if they were white, employed, and so forth, when predictions of the model are used to decide CJS interventions (Goldkamp, 1987). We argue that the latter is the preferred approach when a decision is made that is punitive in nature, and know of no moral grounds for disagreement with such an approach in this case.

Of course, the criminal justice system makes decisions affecting individuals that are somewhat more than just "punitive" and, furthermore, the use of factors such as the employment status of offenders has been, and remains, common -- even in punitive decisions. Although the use of some social attribute variables may be debatable for even a "benign" CJS decision, we do not think it appropriate to use race, ethnicity, or gender as explicit criteria for any CJS decisions. This includes the prediction of recidivism for purposes of risk assessment. Thus, the models that we have been evaluating include these variables <u>only</u> as a means to remove any explicit use of these variables at the individual level. To properly determine the

influence of other variables in the model, morally unacceptable variables need be used to provide "adjustments" to the models when decisions involving individuals are to be made. (See Chapter Nine.) We do not think it morally reprehensible, however, to include some social attributes (other than race, ethnicity, and gender) in some CJS decisions, such as those imposing community service, curfew hours, and so forth.[19]

Note that we have left unexamined the question of what "reference group" should be used to make the "adjustments" for the purposes of predicting individual behavior. Goldkamp suggests selecting characteristics that have received the least punitive interventions in the past. Here, that would suggest a reference group with the lowest probability of recidivism (controlling for all other variables in the model). Thus, white females may serve as a reference group: all offenders, regardless of race, ethnicity, and gender would be treated in the predictive equation as if they were white females.

It seems to us to be equally acceptable to select a different reference group, such as white males: this is what was done in Chapter Nine. Everyone except white, and possibly minority, females could receive a punitive sentencing decision based on the assumption that they were white males. When gender and race yield lower expectations of recidivism lower than those found with the adjustment for white males, these lower probabilities of recidivism could be used instead. Such a policy establishes a reference group that would only be used conditionally. If the individual's traits result in a lower

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[19] Nevertheless, we would be cautious about the use of some variables to make individual decisions if they were excessively correlated with race. As such, these variables (whatever they may be) would be race proxies, and would have to be excluded as a basis for individual-level decisions.

probability of recidivism than when those of the reference group were used, then the more favorable traits would be used. Note that for the "system" of punishment as a whole, this could result in more punishment for some groups than others based on the choice of the reference group. We do not think this is a morally reprehensible approach, however, as long at no one receives a more punitive sanction as a result of the choice of a reference group.

In some respects, the issue of a reference group may be moot, if the definitions of success and failure (i.e., the cut-off point used) are recalibrated to reflect the adjusted predicted probabilities. That is, if the same selection ratio is applied to both the adjusted and unadjusted probabilities, individuals will have their designations shifted about 7% of the time. With such adjustments, what determines the classification of "likely success" or "likely failure" is <u>not</u> the individual's race, ethnicity, or gender, and thus determining an intervention based on assessed risk would not be discriminatory. Alternatively, predictions could be adjusted to the greatest benefit of the individual. Thus, whichever reference group led to the lowest predicted probability of failure would be used. If the cut-off point of the unadjusted probabilities were then used on the new, adjusted expectations, the net effect would be to lower all probabilities, (save those for the reference group), as well as decreasing the expected number of failures. Here too, the result is a risk assessment that is purged of discriminatory variables. This alternative would, however, lead to higher percentages of false negatives.

To the extent that punitive sanctions are based on past criminal behavior and relatively "benign" sanctions are linked to other types of predictor variables, sanctioning offenders on the basis of risk for recidivism

seems, to us, to be morally acceptable. In general, we agree with the principle stated by Morris and Miller (1985:35):

> "Punishment should not be imposed, nor the term of punishment extended, by virtue of a prediction of dangerousness, beyond that which would be justified as a deserved punishment independently of that prediction."

The conception of "punishment" here is primarily one of incarceration, since that is the punishment "of last resort" to the criminal justice system. Thus, the minimal moral requirement of a sentence incorporating assessed risk is that offenders should not serve an "unjust sentence" if a "just" one is no greater than that prescribed within the statutory limits. Beyond this general statement, however, there is need for some consideration of several specific moral implications of such a principle in the context of risk-based sentencing (Moore, 1986). One is the realization that, since most offenders in most jurisdictions serve only a quarter to a third of their sentence, the "justice" principle of Morris and Miller is quite broad, possibly excessively so.

Take, for example, two offenders convicted of offenses of similar seriousness and possessing similar criminal histories. The first is sentenced to five years in prison, and serves two. The second is sentenced to five years and agrees to participate in an intensive supervision program after serving one year in prison. The program requires drug testing, work participation, and some standard restrictions associated with "intensive" parole release. The restrictions of the program prove to be too difficult for the second offender, who then violates some of the conditions of supervision, and is (re)incarcerated. As a result, the second offender serves an additional 18 months in prison (for a total of 30 months).

Has the second offender been treated justly by the CJS? If the second offender chose the second program knowing the risks associated with failure to

meet the demands of the program, we think he/she has unquestionably been treated in a just way. But what if he/she is sentenced to the intensive supervision without consent? We argue that such an offender has still been treated justly, if the decision to direct the individual to an intensive supervision program has been made based on a rational policy of risk assessment. Thus, if the second offender has a low risk of recidivism and the first offender a relatively high risk of recidivism, then the fact that the second offender's sentence did not result from the offender "approving" the sentence is irrelevant. The added requirements experienced by the second offender are justified by the lesser risk of recidivism he/she posed.[20]

The moral justification of using risk assessment at sentencing may be more readily granted if the basis for the predictions are variables associated with the anamnestic model. Past criminal participation (the anamnestic model) is both a good predictor of future criminal behavior and appropriate under the standards of "modified" just deserts perspective. The more prior convictions, arrests, charges, recent arrests, and so forth, the harsher the punishment. This "dual purpose" aspect of the anamnestic variables has been used to justify their use in risk assessment, particularly for parole release (Gottfredson et al., 1978).

The legitimacy of the use of the anamnestic variables can, itself, be extended to justify risk assessment based on other types of predictor

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[20] Note that it is hard to imagine a system in which an offender cannot "choose," in some sense, to comply with a community punishment given at sentencing. For example, an offender who receives as part of his/her sentence some form of community supervision, with requirements such as staying drug or alcohol free, and keeping curfew hours, can always refuse to cooperate with authorities, or violate the terms of the community supervision "immediately" if he/she chooses. It is likely that the community supervision components would be part of most plea bargaining arrangements, such that the offender would trade some of the more stringent requirements in return for a guilty plea.

variables. If it is morally acceptable to impose a harsher punishment for one offender over another because of the greater risk of recidivism for that offender as indicated by a model that uses only anamnestic variables, then it would seem equally valid to claim the moral acceptability of a sentence based on recidivism considerations as indicated by a model that uses other types of predictor variables. This includes social structural indicators, as long as the sentences are reasonably derived from an assessment of risk, and the resulting punishment is no worse than would have been inflicted based on the anamnestic model. In short, one can generalize the "not unjust" principle of Morris and Miller to provide a moral justification for the <u>basis</u> of risk assessment. If anamnestic variables result in a morally acceptable sentence, than predictions derived from other variables which result in a sentence which is not more punitive are also morally acceptable.

There is, however, the possibility of "net widening" as a result of failure to meet the requirements of a so-called benign program. If more offenders are sentenced to incarcerative sanctions, there is a greater possibility of recidivism through a violation of program conditions. We argue that as long as the offender has not received a punishment in excess of that which is "deserved", the punishment component of the intervention does not violate moral principles. That someone fails to meet the standards of a program (as is often the case, for example, in many intensive supervision programs) and is then required to go back to prison or jail does not seem to constitute a special grounds for concern, even when the initial assignment is based on a prediction of recidivism from a social structural model. "Net widening" it is not necessarily undesirable, as there are costs to the community of too narrow a net -- an argument that has been put forth by

Petersilia and colleagues (1985).

In summary, the morality of using formalized risk assessment to determine CJS interventions faces varying degrees of acceptability, depending on one's concern for the rights of the offender sentenced versus the community at large. Three general positions may be taken by those policy makers interested in our general proposal. The first accepts, in full, the relevancy of recidivism, and gives great weight to the community costs associated with releasing offenders into the community who should be incarcerated or who should receive community-based interventions. Such a position has been discussed in some detail by Morris, who has argued that the consideration of recidivism is morally acceptable as long as no receives an "undeserved punishment," that is, a punishment beyond that dictated by retributionist considerations. Those who are low risk offenders are subject to less intervention, but this does not make morally unacceptable the implementation of the "deserved" punishment for those with high risk. As such, this position could be used to justify all the variables of the basic model (all domains), but not variables such as race, ethnicity or gender.

A second position to take is that some types of predictor variables can be linked to a "modified just desert" perspective in which both crime control goals and retribution goals can be achieved simultaneously. This points to the use of only the anamnestic variables to predict recidivism. In that the anamnestic predictions correlate with desert, the offenders with the highest risk "by definition" are targeted for the greatest punishment. Such a model is appealing because it can serve as a basis against which the predictions of the basic model can be evaluated.

Finally, there is the moral viewpoint that allows for recidivism risk assessment using all domains to predict (again adjusting the model for the considerations of race, ethnicity, and gender). However, the use of that prediction is allowed only if the resulting sentence is no more punitive than the sentence that would have occurred by some other standard (such as that of the anamnestic model). Here, less punitive decisions may be made for some individuals based on social structural characteristics, but not if they result in more punitive sanctions than those of an alternative system. "Benign" interventions could possibly be based on any domain.

A risk-based sentencing policy raises a myriad of moral and ethical issues. In essence, none are new to the literature, and our overview here of the basic issues has been, admittedly, brief. However, their saliency is again raised by our call for a sentencing policy that explicitly ties the punishment to be received to expectations of future offending.

Prior Record and the Assessment of Risk.

Many of the moral and ethical issues revolve around the anamnestic domain of predictor variables. While the discussion of the previous section assumed that these variables are easily measured, this is not true. Implementation of a policy of risk assessment at sentencing, particularly one using anamnestic variables, will necessitate choices over how to measure the prior criminal record of the offender. These will not be simple choices as our investigation of the impact of "prior record" on recidivism has found many, often competing, complex relationships. Moreover, many of the main effects of prior record indicators on levels of recidivism are mediated by characteristics of the offender and the sentence received. Of the 33 robust interactions identified in Chapters Five and Seven, 19 involve some aspect of

the individual's prior record. Selection of only one or two indicators to use when assessing individual-level risk for recidivism could seriously impair the accuracy of these predictions.

Like the measurement of recidivism, we have found the measurement of prior record to vary considerably in the literature. There is no single, agreed upon way to operationalize levels of previous offending and, as was done for recidivism, we have offered multiple ways of characterizing the individual's prior criminal behavior. Basic distinctions were made between juvenile and adult prior record, with two measures used for the former and nine for the latter. In Chapter Three we argued for maintaining distinctions between distinct arrest days and the charges made on those days, as multiple charges, for potentially different kinds of crimes, could be made on a given arrest day. Similarly, distinctions can be made between conviction days in court and the number of convictions received in any given court appearance. Our prior record measures also tap chronicity (charges in the last five years), seriousness (prior Part I charges), and differences in types of previous crimes (charges convictions for property, persons, and weapons offenses). In retrospect, the decision to maintain all of these aspects of prior record has proved well-founded as we have seen empirical support for all of them.[21]

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[21] A few technical points about our prior record indicators deserve reiteration. First, two measures, number of prior adult charges for any offense, and prior adult property charges were dropped due to severe collinearity. However, this also means that those prior record indicators that were retained do <u>not</u> suffer from collinearity problems. Second, the variable selection strategy introduced in Chapter Five ensures that all measures retained in the full model account for statistically significant variation in "recidivism" beyond the other variables in the model. Thus, each of the prior record measures discussed here uniquely adds to the explanation, and prediction, of recidivism.

Our findings concerning prior record as a juvenile are relatively easy to summarize. As the number of arrests as a juvenile increases, so too does recidivism. However, this increase is even greater if the sentence received is one to a State Prison, but is less if the presenting offense is for a property crime. There is no direct relationship between prior charges as a juvenile across all offenders, but for blacks, increases in charges as a juvenile are associated with increased levels of recidivism. Clearly, knowing levels of prior record as a juvenile can improve the prediction of recidivism.

The results surrounding prior criminal behavior as an adult are not as straightforward. In general, as previous arrests or charges increase, all forms of recidivism also increase. In contrast, increases in both prior convictions and charge convictions are associated with <u>decreased</u> levels of subsequent recidivism. However, given the nature of how these generalized main effects interact with other variables, these statements represent oversimplifications.

The main effect of previous adult arrests is to increase recidivism. Each additional prior adult arrest increases the level of recidivism, and this holds across <u>all</u> the forms of recidivism we have studied in detail. However, for those sentenced to a State Prison, or those who receive their initial sanction, the increase in the impact of previous arrests as an adult is even greater. The positive relationship between adult arrests and recidivism is lessened if the offender is black or receives a sentence of probation with conditions in conjunction with a fine.

The number of charges accumulated in the five years prior to sentencing is also positively related to recidivism. This relationship is even stronger if the sanction involves the Youth Complex, but the effect is mitigated by

increases in the age of the offender: every year increase in the age of the individual decreases the impact of prior record chronicity on recidivism. Similarly, each previous Part I charge increases the likelihood that the individual with recidivate, and this increase is even greater for female offenders. This impact of prior Part I charges is lessened if the presenting offense is for a drug crime, and if the sentence is one to probation in conjunction with a fine.

Each previous conviction as an adult is associated with decreased levels in recidivism, and the decrease is even greater if the instant offense is a property crime or the sentence is to the Youth Complex. The crime control benefits of previous convictions can be offset by a presenting offense for drugs, as, for this subgroup of offenders, each previous conviction as an adult increases the chances of recidivism. Charge convictions as an adult are also associated with decreased recidivism, even more so for those sentenced to probation with conditions. Again, however, this main effect is modified by an interaction such that each month sentenced to a State Prison lessens the negative effect of adult charge convictions on recidivism. Prior convictions for weapons are positively related to levels of recidivism, as are prior property convictions for blacks.

This brief summary of the results for our array of prior record measures demonstrates the complexities that must be considered in using prior criminal offending to assess offender risk. At the most general level, we see that the effects of prior record on recidivism do not operate the same way for all types of offenders. To assume that, say, adult arrests or property convictions indicate the same level of risk across the full spectrum of individuals is inaccurate as for the former, it overstates expected risk for

blacks and, for the latter, expected risk is underestimated. Similarly, predictions of risk become less accurate if prior record is assumed to have the same effect across different types of presenting offense.

More troubling is the finding that prior levels of criminal behavior can mediate any effect of the sentence. In some cases, the intervention is less successful in achieving crime control if the individual has a longer prior record. In others, more positive outcomes are observed for those with more extensive prior records. Here too, the assumption an intervention will have the same impact across all types of offenders (as defined by prior record) is false.

If a formalized policy of risk assessment at sentencing is to be instituted, these complexities surrounding prior record must be addressed. Several possible alternatives present themselves. One is to use a single measure to represent all aspects of prior criminal behavior. This is what has been done in the Minnesota grid system, where previous convictions defines one of the dimensions of the grid. Yet, for purposes of risk assessment, prior convictions would seem to be the least desirable single indicator. Not only is it negatively related to recidivism (i.e., risk decreases with increases in prior convictions), but its effects are not uniform across different types of presenting offense.[22]

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[22] The argument in favor of using previous convictions as the measure of prior record is not without merit: convictions indicate a greater certainty that the individual committed the acts. However, we have seen that once other measures, perhaps with less certainty to them, are controlled, the "true" relationship of convictions to recidivism is negative. Note too that the fact that this relationship is negative is consistent with our interpretation of "temporary rehabilitation or specific deterrence" used to account for the sanction effects of the present study. There appears to be a beneficial effect for accumulated previous convictions, and it would seem unjust to ignore this benefit either when sentencing offenders or assessing their risk for recidivism.

Another alternative would be to employ a small number of prior record measures when assessing offender risk for recidivism. The use of multiple measures has the advantage of maintaining some of the complexities in the relationship of "prior record" to levels of recidivism while keeping some parsimony as well. Total charges as an adult, total charge convictions as an adult, and arrests as a juvenile, if available, would seem likely candidates. (Charges in the past five years could be substituted for juvenile arrests if juvenile records have been sealed.) These three aspects of prior record tend to capture some of the diversity of prior record and are easily computed from information available at the time of sentencing. And, as we saw in Chapter Six, these indicators of prior record have the most robust relationships with all definitions of recidivism.

Still another alternative follows from our basic model: use a wide variety of prior record indicators in assessing offender risk. This is a feasible alternative in that the measures constructed used information readily available to CJS agents. As well, our results demonstrate that each of these multiple aspects of prior record uniquely increase the accuracy of recidivism predictions. While the use of numerous prior record indicators complicates the mechanics of risk assessment -- the values for many variables must be computed prior to generating the summary risk score -- it is truer to how prior offending is actually related to recidivism.

The policy decisions surrounding the choice of prior record measure(s) will have consequences for how accurate risk assessments will ultimately be. Whatever the choice made, our results point strongly to the fact that interactions of prior record with other characteristics of the offender, presenting offense, and even the recommended sentence, will have to be

investigated. Failure to do so will further reduce the accuracy of predicted risk, underestimating it for some types of individuals and overestimating it for others. Moreover, any impact of a recommended sanction is likely to be mediated by the prior criminal behavior of the individual.[23]

CHOICE OF RECIDIVISM MEASURE

The formalized use of risk assessment at sentencing requires a policy choice over what aspect of recidivism is to be used. That is, before any programmatic changes can be implemented, it is first necessary to answer the question of "recidivism for what?" In this section, we sketch out what the possibilities are and how the choice of a recidivism measure can influence any resulting sentencing policy.

Empirical studies have varied considerably in their conceptualization and in their operationalization of the concept of recidivism. We classified the literature on recidivism (Chapter Two) into three general approaches to the conceptualization of recidivism: binary, criminal career, and time to failure approaches, and discussed some of the strengths and weaknesses of each. In Chapter Four we discussed the empirical relationships among numerous operational definitions of recidivism, and, in Chapter Six, argued that there are several dimensions to recidivism that limit their interchangeability as measures of a more general concept.

We found differences in the performance of our models across the three general approaches to measuring recidivism introduced in Chapter Two. Models

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[23] At a broader level, our results speak to the need for more research and conceptualization into what is meant by "prior record." As is often the case with indicators of recidivism, what is used to capture "prior record" is often a matter of convenience with little thought given to the consequences of using one measure over another.

for criminal career measures of recidivism were generally "better" in the sense of attaining higher levels of explained variance than the other types of dependent variables. At the same time it can be said that there were general similarities in the results for the binary and criminal career dependent variables. For example, the same independent variables that predicted rearrest for persons crime also tended to predict rates of rearrest for persons crimes. The results comparing the binary rearrest variable to rearrest rates were also quite similar, despite the difference in the amounts of variance explained across these dependent variables.

Our research on possible dimensions of recidivism reveals that there are at least three distinguishable dimensions that should be differentiated in recidivism studies. Whether the research chooses to focus on binary, criminal career or time-to-failure variables, there seem to be important distinctions between measures that consider any type of subsequent crime (whether an arrest, charge or conviction) or only crimes against persons. Yet another distinction is between the results obtained for dependent variables that involve either charges, arrests, or convictions, as opposed to reimprisonment. Models for the latter are quite different from those of the former, and to assume equivalency across the many forms of recidivism would be erroneous.

The general policy implications of the dimensions of recidivism and of the variations in general categories of crime are numerous. This claim itself needs some justification. Some have maintained that the choice of a recidivism measure may be made without any consideration of the differences that would arise with alternative measures.[24] If a state legislative body or

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[24] This point was made by an anonymous reviewer of early drafts of this work.

a judiciary system decides to select rearrest for any crime type as their criterion, that is a value or policy decision that need not be based on empirical or scientific considerations. While abstractly this is true, our experience with policy makers is that they are eager to know what "difference it makes" to choose one measure of recidivism over another. In practice, there are real consequences to such decisions that can only be assessed through empirical, scientific means. Choice of a particular measure of recidivism could have enormous implications relative to the system's capacity to handle offenders. For example, a policy based upon recidivism in terms of less frequent events (e.g., persons offending) is less apt to strain system resources than one based on all types of offending. (Witness the overcrowding resulting from mandatory incarcerations for some types of drug convictions.) Choice of a recidivism measure is not a decision made without real consequences, and the more informed such choices are, the more responsive the CJS can be in its decision making.

If policy makers are interested in using recidivism prospects to make an intervention decision, a decision needs to be made not only about how to measure recidivism, but also which specific <u>forms</u> of recidivism to consider. Should policy makers decide on one measure of recidivism as a criterion for making individual decisions, or more than one? To make those choices, knowledge about different recidivism measures is useful. If multiple recidivism measures are deemed to be relevant to decisions about individuals, policy makers will have to determine how to integrate or choose among the results when they differ in their predictions. Thus, for example, if an offender is at high risk in terms of rearrest for any crime, but not for subsequent persons (violent) crimes, what CJS decision should follow? While

have demonstrated empirically the extent of differences in prediction across recidivism measures, there is still the logical problem of making individual-level decisions using varying, and possibly contradictory, risk assessments.

One finding that is fairly consistent across our analyses is that three distinct forms of recidivism characterize the behavior of this sample of convicted offenders. The findings of the measurement of recidivism (Chapter Four), basic models of recidivism (Chapter Six), and the effects of sentences on recidivism (Chapter Eight) all point to useful distinctions between subsequent crimes against persons, reimprisonment, and recidivistic crimes for all types of offenses. Even at this most general level, a major decision is to which form of recidivism policy should be oriented. Intervention and sentencing policies based on any of these general forms of recidivism is desirable. Crimes against persons are viewed by society as the most abhorrent. Considerable policy is already directed toward persons-based crimes by sex-offender statutes, and the more severe sentences administered to those convicted of murder and homicide. Minimally, there is also a symbolic advantaged to be gained by formulating policy toward these forms of crimes.

Similar import can be attached to reimprisonment as a form of recidivism. Overcrowding of limited prison space necessitates some consideration of the strain on system resources when formulating policy. As well, the monetary costs of incarcerating an offender are quite salient given the current economic climate. The search for "intermediate punishments" can be seen as a response to limited bed space in both prisons and jails. Thus, the use of the likelihood of (re)imprisonment after sentencing and release to fashion sentencing policy is consistent with recent thinking about concerns of efficiency in sentencing practices. Moreover, we have seen that this aspect

of recidivism is amenable to prediction. Some of the highest explained variances, percentages of correct predictions, and relative improvements over chance have been found for the probability of reimprisonment and the days to reimprisonment.

A concern over the volume of crime argues for sentencing and intervention policies geared toward recidivism for any type of offense, preferably measured by counts or rates. Property and drug crimes dominate the presenting offenses of this sample, and, more generally, those found in society. Thus, attention must be paid to the types of crimes frequently faced by both the public and the authorities. That policy can be, and is, directed toward the frequency of offending is evident in habitual criminal statutes. As with the prison-based measures of recidivism, we have seen that some forms of recidivism using all types of crime can be predicted relatively well. Our models for all post-sentence charges, and the adjusted arrest and charge rates, resulted in among the highest explained variances found in our research.

While each of these general forms of recidivism is consistent with past and current sentencing practices, the choice of only one for the development of a risk-based sentencing policy is a decision with important consequences. Use of any one form of recidivism ignores the real advantages associated with the others. All of our results suggest that predicting persons-based recidivism is difficult at best -- few offenders recidivate through the commission of crimes against persons. While it is possible to identify, at the time of sentencing, those individuals who are likely to commit future serious persons crimes, they are few in number. The vast majority of convicted offenders are not expected to commit such offenses.

Use of the probability of (re)imprisonment (or the expected time to reimprisonment) as the sole criterion for a risk-based sentencing policy is similarly flawed. The results of Chapters Four, Eight, and Nine point strongly to the fact that the CJS response to recidivism is an integral part of this form of recidivistic behavior. This aspect recidivism is thus partially confounded by the behavior of individuals other than the offender. It would seem inappropriate to fashion a sentencing policy only on the basis of how the system, as a whole, might respond to what the individual may do in the future. Problems also arise if risk is assessed at sentencing only in terms of the expected volume of crime in the future. Here, the issue is mainly one of omission. To assess an individual's risk only on the basis of high levels of subsequent offending ignores the very real constraints of limited prison and jail space and society's concern over the more serious crimes against persons.

Given these consequences for developing policy based only on one form of recidivism, we contend that it is preferable to assess risk for all three general forms. That is, interventions should be formulated based upon how likely the offender is to recidivate across several criteria. Knowing the likelihood of recidivism for each of these general forms offers a broader assessment of offender "risk," one that maintains the advantages of assessing risk for each individual aspect of recidivism. This assessment could be then be used to decide upon an "appropriate" sentence.

For a risk-based sentencing policy to be effective, interventions and treatments must be determined using estimated risk levels. Some convicted offenders may be high risks, no matter how recidivism is conceptualized. In such circumstances, priority may have to be given to one (or more) aspects of

recidivism. For others, there is the possibility that expected risk across multiple forms of recidivism yields contradictory predictions. Here too, priority may have to be given to some forms of expected recidivism over others. Therefore, one possible avenue for policy development is to establish a hierarchy among recidivism criteria, as our results throughout argue strongly against using a single criterion to formulate CJS policy or make decisions about individuals. At the top of the hierarchy may be a criterion that is most difficult for an offender to meet, such as committing a persons offense, or one that policy makers think to be the most important or consequential outcome criterion, such as whether or not an offender is predicted to be subsequently imprisoned. In this respect, it seems more reasonable to give priority to recidivism expectations that involve the most risk to society, such as crimes against persons, crimes leading to reimprisonment, and high rate offenders.

For the sake of demonstrating how assessments for multiple forms of risk could be used for sentencing decisions, consider a "decision tree" approach. Our example presupposes a particular hierarchy of types of recidivism, though the approach would work for any hierarchy imposed on the various forms of recidivism. Note too, that by making use of assessed risk conditioned upon the expectations of other forms of recidivism, the problems arising when recidivism projections differ across multiple criteria are avoided. Figure 10.1 depicts a possible hierarchical "decision tree" approach that incorporates assessments for multiple forms of risk. In the figure, an offender is first evaluated in terms of the probability of a persons crime. If not predicted to be such an offender, the individual is then subject to an assessment of the likelihood of a subsequent reimprisonment. If the answer is

no, the offender is then subject to the question of likelihood of rearrest (or alternatively of being a high rate recidivist). When a positive prediction does occur, a decision is made to intervene in some way. For example, presumably the intervention would presumably be more intrusive for someone found likely to be reimprisoned chan with someone who is not likely to be reimprisoned or who is "only" likely to be rearrested for any type of crime.

Of course, other aspects of CJS intervention may also be part of the "decision tree" process. It may be decided to simply ignore recidivism prediction components for some offenders, or to do nothing about offenders with high recidivism potentials. Thus, those who either commit serious crimes or very trivial crimes, may, in the first instance, be subject to interventions based on retributionist goals, or in the second be dismissed for efficiency purposes. The specific hierarchy defining the tree approach may be quite different: high rate offenders may be placed at the top, for example. Also, it has been suggested here that the nature of the CJS intervention would be punitive or restrictive, but this need not be the case (a point to be taken up again below), as decisions involving more benign forms of intervention may frequently be involved. However these contingencies are evaluated, the decision tree approach may prove to be helpful to resolve the "choice problem" that multiple indicators of recidivism present.

POSSIBLE RISK-BASED SENTENCING POLICIES

As implied by the discussions above, there are many crucial decisions to be made that will shape the form of any risk-based sentencing policy. In the absence of the outcomes of these decisions, it is difficult to be specific as to exactly how the assessed risk for recidivism can be incorporated into

sentencing decisions. However, several options present themselves, and we sketch out the possibilities in this section.

The exposition is predicated on the assumption that both the seriousness of the presenting offense, and the risk for future recidivism, are the key elements defining the interventions (i.e., sentences) that are to be considered "appropriate." Given the combination of seriousness and assessed risk, a range of possible sanctions is to be suggested. Moreover, we see these sanctions as being able to balance considerations of "helping" the offender and achieving some retribution for the crime that led to the conviction. As such, relatively simple ideas about the nature of "treatment" and "punishment" need to be reevaluated. Specifically, our discussion follows from Morris and Tonry (1990), who argue for the breakdown of the perception that these two aspects of intervention are at odds. Some forms of intervention, such as intensive supervision in the community, should be conceptualized as representing a mixture of treatment and punitiveness.

Toward this end, we consider the "interchangeability" among forms of intervention or "intermediate punishments," as developed by Morris and Tonry (1990). They have argued, that the principle of interchangeability of punishments should apply to all individuals who otherwise would be sentenced to less than two years of imprisonment, though we see this as also appropriate within levels of risk. For all such offenders (the vast majority of offenders convicted in the criminal courts), there would be options other than simply probation or incarceration available to the judge. For example, in Delaware, factors such as the following have been considered as part of the comprehensive Du Pont proposal: fines, degree of supervision in the community, requirements for mobility in the community, extension of privileges

(such as choice of job, choice of residence, mobility within settings, driving, drinking, phone calls, curfew, mail, urine analysis, and choice of associates).

Morris and Tonry, who discuss the Delaware plan, also consider the concept of "punishment units," an idea developed by Paul Robinson, a Commissioner for the U.S. Sentencing Commission (1990:73). Here, judges would be guided in the degree of "punishment" by equivalencies to days of imprisonment. Thus, a month imprisonment might be deemed equivalent to two months of house confinement, or 200 hours of community service. Once such units are defined, judges would then have options for "mixing and matching" various combinations of such units when forming a sentence for an individual.

There are several appealing aspects to this approach to sentencing. First, it explicitly recognizes that "punishment," or what we have been calling "intervention," should involve something more than the simple "in" or "out" decision. By calling for intermediate punishments Morris and Tonry reveal both how anemic and cruel the CJS system can be: most offenders do not serve time for their crimes, and some offenders serve Draconian sentences. Their call for increased use of fines (see Hillsman, 1990), community service (Pease, 1985), intensive supervision (Clear et al., 1987), and other possible alternatives to imprisonment point to the need to develop criteria for matching offenders to interventions or punishments.

More than any other implication, the interchangeability concept of Morris and Tonry demonstrates the true complexity of a sentence decision that is aimed at something beyond deciding whether or not the offender serves time. Many possible factors are at work in decision making when the sentence is essentially the "in/out" decision, but decision making within the context of a

system with "interchangeable punishments" is much more complex. How is a judge to decide which combination of punishment units is appropriate for a given offender? Should a given offender be fined, made to work hours of community service, be under house arrest, have a curfew? Whether or not one endorses the interchangeabilty concept, it seems clear that guidance as to the ramifications of sentencing a given offender to one sentence as opposed to another would be useful toward the general goal of limiting individual discretion and possible bias in decision making. We argue here that more explicit consideration of recidivism will help toward that end.

Our criticism of the interchangeability concept as developed by Morris and Tonry (1990) is that, in the absence of principles to decide among the many options available to judges (e.g., fines, house confinement, work requirements, community service, drug and alcohol programs, group and individual therapy requirements), such decisions will be made in an ad hoc and possibly capricious manner. Moreover, within the context of systems of justice that rely heavily upon "plea bargaining" (part of which is the consideration of the possible "punishment" the judge will give), offenders will generally attempt to negotiate for the sentence with the least punitive components. This effectively restricts the range of interventions that the CJS will administer. In the absence of guiding principles, we think the interchangeability concept unlikely to succeed.

A risk-based sentencing policy can provide the guiding principles needed to implement a program of intermediate punishments. There are two general ways that risk of recidivism can help assign relatively complicated intermediate sanctions. The first is that resources may be more efficiently allocated based on the risk of recidivism. Since resources (e.g., programs,

beds, monitoring devices) are limited, in general, those offenders presenting the greatest risk to society could be given priority in the allocation of CJS interventions. It seems unrealistic to assume that all offenders can be "treated" equally relative to available resources, and the assessed risk for recidivism yields a possible allocation mechanism.

The second way that formal risk assessment can assist in deciding on the relatively complicated "designer" interventions discussed by Morris and Tonry is to increase the likelihood that such interventions will have a beneficial impact on the behavior of the individuals receiving them. There is some evidence of reduction in criminal activities as a result of some of these interventions (Pearson, 1991). Certain "types" of individuals may be more amenable to some combinations of interventions than other individuals, although more empirical research need be done on this matter. Still, we see the assessed level of risk as a viable means of matching individuals to all forms of punishments and treatments.

In that "intermediate punishments" may be better "designed" or allocated as a result of formal considerations of risk, several questions need be addressed about how to predict risk. Many of these (which predictors and measures of recidivism are to be used, what are the dangers of false positives) have been discussed earlier. Here, we presume some answers to these questions, focussing instead on ways in which assessed risk can help fashion an appropriate sentence. Several examples, based on which variables are used to assess risk, are presented. Note that these examples are not only in harmony with what many think is "fashionable" in current discussions of punishment, but may also provide avenues for a more reasonable implementation of such ideas.

Sentencing Based on a Single Domain Predicting Risk

Consider a sentencing schema such as that outlined in Figure 10.2. It is very similar to that adopted in Minnesota as their Sentencing Guidelines grid. Here, however, we have made several modifications. First, to simplify the discussion, we present only three broad levels of offense severity in Figure 10.2, borrowing some of the crime categories from the Minnesota Sentencing Commission Guidelines (MSCG) grid. (More categories of offense severity may be used, as in Figure 1.1 in Chapter One.) The second difference is that, in Figure 10.2, the dimension across the top does not represent the count of prior convictions as it is (essentially) in the MSCG grid. Instead, it consists of various levels of risk defined for an individual (after adjusting the model's predictions for characteristics such as race and gender) as predicted by the anamnestic characteristics of the offender. For the purpose of discussion, we employ three levels of assessed risk: low, a medium or intermediate level, and high.[25]

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[25] Note that, unlike in Chapter Nine where an offender could be classified as either "likely" to recidivate or not, here three possibilities are considered. For dichotomous recidivism criteria this corresponds to establishing two cut-off points to define low, medium, and high risk. (If the recidivism criterion were continuous, then the columns of Table 10.2 could be defined by the number of expected arrests, or convictions, and so forth.) Although we did not explicitly discuss trichotomous classifications in Chapter Nine, the logic involved in the choice of cut-off points is essentially the same: civil-libertarian ratios could be chosen to allow for the selection of cut-off points so as to define acceptable proportions of predicted low, medium, and high risk offenders. In the latter case, however, the importance of the civil-libertarian ratio for such definitions may be even more apparent. For example, it may be that for highly skewed predicted probabilities resulting from low base rate recidivism criteria, "high risk" would still be defined using the cut-off point such that the selection ratio equals the base rate. A low risk offender could then be defined by the selection of a cut-off point such that it is very unlikely that the offender would be a false negative (corresponding to a acceptable cut-off point where the civil-libertarian ratio is very low). Thus more false positives generally would be acceptable in defining the cut-off point between low and medium risk levels since there would presumably be less punishment of the medium risk offenders than the high risk offenders. For the designation of medium risk offenders, a

On the surface, Figure 10.2 appears to be quite similar to the MSCG grid itself, but actually it is quite different. Here, for example, many more factors go into the classification of "level" on the dimension across the top of the figure than simply the number of prior convictions. More importantly, the purpose of the classification is entirely different. Here, it is to define those who are likely to recidivate under some (unspecified) criterion. Thus, we define interventions -- the cells of the figure -- based on the likelihood of recidivism, and not necessarily on the extent of "culpability" attributable to the repeated failures that constitute the variables of the anamnestic model.

The cell values prescribe various punishments that increase in the degree of supervision as well as in the length of supervision. The degree of supervision is aimed at achieving the crime control goal of incapacitation when an incarcerative sentence is recommended and the goal of specific deterrence when greater supervision under probation is recommended. Note that the utility goals of this grid may result in quite different interventions for individuals than those found using the MSCG grid approach. Also, "more" is determined by the cells of Figure 10.2 than is in the MSCG grid. Degree of supervision, for example, is determined at sentencing, while in Minnesota, this is largely left to the probation supervisor, who uses a risk assessment instrument to determine level of supervision. Thus, the model here is suggestive of how a system of intervention may be made less diffuse, even less than in Minnesota, where it is arguably the least diffused of the fifty states.

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relatively high-cut off point, corresponding to a high civil-libertarian ratio, could be chosen.

Other aspects of Figure 10.2 deserves mention. As was discussed above, there is an explicit link of the probability of recidivism to the degree of supervision. Those with a high probability of recidivism are given more supervision (either in the community or in jail or prison). The "justification" for this is that the probability of recidivism is lessened by such interventions. That is, the community experiences less risk as a consequence of incapacitating or deterring would-be recidivists, as compared to the sentencing under the MSCG grid, or as compared to the sentencing in which non-statistical evaluations of risk determine the sentence.[26] In either of those systems, or in other systems of "determinate" sentencing, community safety considerations are of secondary importance.

What is "wrong" with the system proposed in Figure 10.2? Critics from a purely retributionist perspective will dislike the use of utility goals in establishing a recommended sentence. But, the use of the anamnestic model to yield predictions may make the classification of offenders across this dimension more true to "modified retributionist" goals than the simplistic use of only number of prior convictions. That is, the anamnestic model, which includes such predictor variables as the number of prior adult arrests, adult convictions, adult charge convictions, charges in the past five years, prior Part I charges, and prior property convictions, is arguably more in accordance

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[26] Note that although we have some empirical proof in our research findings that there is appreciably less risk of recidivism as a result of some of the interventions discussed in Chapters Seven and Eight, we do not attempt here to link any particular intervention to an offender based on the effects we observed. Rather, we simply assume that there is sufficient research evidence in the literature to claim that there are both specific deterrent associated with parole or probation supervision and incapacitative effects associated with incarceration. Research suggests that the greater the level of supervision, the less the recidivism, (see Baird et al., 1990). That imprisonment incapacitates is clearly suggested by the current results, as well as the findings of others (e.g., Visher, 1986).

with a "modified just deserts" perspective than the use of simply the number of prior convictions.[27] Offenders with "more" of most of these items receive the harsher punishment.

The purpose of using the anamnestic model in Figure 10.2 is, however, to punish offenders efficiently in accord with general retributionist principles. Incapacitation and specific deterrence goals may be met by the recommended sentences in the grid, as well as retributionist goals. Offenders incarcerated, or incarcerated for longer periods of time, are considered incapacitated by the experience. Offenders who qualify for greater degrees of supervision are probably somewhat deterred by the greater number of contacts of more intense supervision. For these reasons, we think that a system of punishment such as that proposed in Figure 10.2 is superior to that of the MSCG grid. It is more efficient and, arguably, as "retributionist" as the MSCG grid -- if retribution is one's primary policy goal.

Despite its advantages, we are suspect of the system proposed in Figure 10.2 as it does not go far enough in reconceptualizing the sentencing process. First, it limits discussion of recommended interventions to incarceration and probation, with varying degrees of supervision. No other interventions are recommended, nor do any others follow logically from the two dimensions of the figure: the options are limited to varying the degree and length of supervision, either in or out of the community. It assumes that the purposes of intervention are to monitor (in hopes of deterring) and incapacitate, without explicit consideration of the other forms of CJS interventions, such as various rehabilitation interventions, community service programs, and other

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[27] A "modified retributionist" might argue that all of the items in the anamnestic model should be based on convictions rather than charges or arrests. We have argued otherwise above, based on considerations of risk assessment.

forms for more "punitive" interventions (e.g., fines, in-house imprisonment, and restrictions like curfews, being alcohol-free, and having a job). These options are available, in varying degrees, across jurisdictions, yet are not incorporated into the sentencing grid of Figure 10.2, nor in the MSCG grid.

As such, these grids represent a limited range of goals and purposes, and lead to a system of intervention that essentially accomplishes little beyond the incapacitation and specific deterrence discussed above. For most offenders, there is only some specific deterrence effects, (assuming a jurisdiction can afford a relatively high number of contacts with probationers) and, for others, there is primarily custodial time, where they may or may not experience additional forms of intervention such as drug, job, or alcohol-related programs. We think it useful to consider a wider variety of sentencing interventions, many of which are linked to the "needs" associated with characteristics that measured in the social structural model developed in Chapter Five. Moreover, it is possible to take more advantage of intermediate forms of punishment when risk is assessed using other kinds of independent variables.

Consider, again for sake of pedagogy, Figure 10.3. Here the anamnestic model has been replaced by the social structural model. That model, includes variables such as the offender's employment, having a job to go to after the sentence, having been on welfare, living in an urban community, having a history of drug problems, having been treated for drug or alcohol problems, dropping out of high school, having needle marks, and committing the presenting offense in a group. Note that again we assume that the effects of race, ethnicity, and gender have been purged from the model's predictions. In Figure 10.3, the cells contain possible sentences associated with the

classification of offenders, again by the two dimensions of presenting offense severity and risk for recidivism. The sentences in Figure 10.3, however, are not meant to be the only examples of possible sanctions for an offender falling into a particular cell. Rather the actual sentence is to be based on the "need" associated with each predictor item. Thus, for example, those who have not completed high school, who do not have a job, who have needle marks, who committed crime with others, and who live in an urban area may have an intervention that involves getting a GED, participating in job training, participating in a drug treatment program, restrictions on associating with criminals, and curfew restrictions, respectively. That is, there is an attempt to tailor the intervention to the specific "needs" of the offender to maximize the chances of rehabilitation (see Wish and Johnson, 1986, for a discussion of the effects of drug treatment programs in particular).

Consider what Figure 10.3 represents relative to Figure 10.2. A polar opposite position has been taken. In the Figure 10.3, sentences are designed to require the offender to participate in those programs that are linked explicitly to the social structural "need" factors associated with that individual. Risk of recidivism is, itself, defined in terms of those factors. At higher levels of risk, increased amounts of intervention are recommended. Note too, that as one goes from the first to the third row of the figure, the nature of the intervention still increases in accordance with retributionist standards -- the more serious the offense, the more severe the punishment. As one compares across the columns, the amount of incarceration and degree of supervision increase, as does the amount and degree of "benign punishment," such as the programs and requirements listed above.

As such, Figure 10.3 would be grist for the mill of critics who would argue that the underclass would be unduly subject to punishment, relative to middle class offenders, by such an intervention strategy. The moral objections would center around the increases in length of time imprisoned across columns of the figure, as well as to the possibility of "net widening" as a result of failure of individuals to meet the requirements of the relatively "benign" programs in which participation would be required. Note that there can be relatively little complaint about the increase in punishment across the rows, as it is still in accord with retributionist principles, nor can there be concern for the increase in "benign" interventions per se, as offenders' needs are greater the more "deficiencies" they have, as measured by the variables in the social structural model.[28]

There are other aspects to sentencing in accordance with only the social structural model that warrant discussion. In Chapter Two we discussed the fact that it is frequently the case that the social structural variables occur in time prior to many of the anamnestic variables, and they are not as "logically proximate," as are the anamnestic variables, to measures of recidivism. Thus, there may be good reasons for using a predictive device based solely on the social structural model: this model might yield better

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[28] A comparison of the cells of Tables 10.2 and 10.3 illustrate more clearly Morris and Miller's principle that the prediction of dangerousness should not lead to a punishment beyond that justified as deserved. Some individuals are likely to be predicted to fail under the social structural model, but not the anamnestic one (and visa versa). This is especially likely if different recidivism criteria are used for the two tables. Moral objections can then be raised as the punishment of Table 10.3 exceeds that of Table 10.2. Even when expectations of success and failure coincide across these tables, it can be argued that more severe punishments may arise under the sanctions recommended when the social structural model is used. Earlier we discussed some possible resolutions to these moral objections. In the current instance, one might be using the sentences of Table 10.2 when more severe sanctions would result from Table 10.3.

estimates of the true influence of social structural variables than would be the case if the full model were used.[29] For example, in the analyses of Chapters Six and Eight were all domains were used, the effect of having a history of drug use on recidivism was small or negligible. However, in a model using only social structural variables, having a history of drug problems may have a larger effect, and thus a relatively large impact on the predicted probability of failure. Arguably this latter estimate is a better one of the true impact of "having a history of drug problems" than is the case when all the variables of the other domains that are more causally proximate are controlled. In that there is to be a correspondence between the "need" represented by these variables (need for a GED, need for drug or alcohol treatment, need for job training), and the intervention (finishing a high school degree, participating in such programs), then the importance of the social structural predictors is even greater. The so-called "benign" interventions discussed above may be best assigned to individuals when risk is based on only social structural predictors rather than on the full model.

Sentencing Based on Multiple Domains

There are, of course, no restrictions that risk-based sentencing strategies be limited to risk assessments using only one domain of predictor variables. Allowing for the predictions of multiple domains does, however, greatly increase the forms that such a strategy may take. Consider yet a third scheme where multiple domains are used to predict recidivism. We have

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[29] Note that we are not suggesting that the particulars of the social structural model developed here be used by any jurisdiction. Each jurisdiction should develop its own models, if the strategies proposed here are to be adopted. Moreover, the variable inclusion criteria that were discussed in Chapter Five resulted in many variables indicative of offender "need" being dropped for the purposes of our analyses. Those who would pursue our suggestions here would be well advised to develop their models without such inclusion criteria.

in mind here something like the basic model developed in Chapter Five where all the domains (social structural, anamnestic, juvenile career/onset, CJS/Offender action, presenting offense, and general controls) are used to make predictions of individual-level recidivism. Several aspects using such a model warrant discussion. The basic model represents our "best" attempt at predicting recidivism premised on what is known about the individual prior to sentencing itself. The amount of explained variance in any recidivism measure is higher than that found when a single domain is used, and thus predictions of recidivism should be more accurate. Second, the full model represents the effect of a variable from any domain with all the other variables controlled. As was discussed above with regard to the social structural domain, the effects of the variables are estimated differently than would be the case if only one alone were used to predict recidivism. This should lead to more precise effects being estimated for all independent variables.

Examples of recommended sentences when multiple domains are used to assess risk are shown in Figure 10.4. Arguably, these sentences maximize crime control goals by providing the best overall predictions of recidivism: fewer errors in prediction will result from the use of multiple domains relative to using only one or two domains. As recidivism projections are expected to be more accurate, the sanctions found in the cells are harsher than those of previous figures. To the extent that policy is concerned with those crime control goals, the use of a predictive model, such as the basic model of Chapter Five, would seem to be superior to that of either the anamnestic or social structural model. A second general advantage of a multiple-domain model over single-domain models is that it retains the advantages associated with the latter. A "modified" just deserts approach is

maintained in that anamnestic measures are used to assess risk. As well, social structural variables are part of the predictive model -- thus, specific interventions may still be linked to offender "needs."

Note that risk assessment in Figure 10.4 uses a multiple-domain model rather than one that incorporates effects of the sentence on subsequent criminal behavior. We do not consider such a model here, however, for two reasons. The first is that the types and range of interventions suggested in the figure far exceed the alternatives analyzed in Chapters Seven and Eight. The use of a full-intervention model requires estimation of the effects of sanctions, and those sanctions should match the ones that are recommended. Second, and more importantly, the purpose of sentencing strategies like that of Figure 10.4 is to suggest recommended sentences. Use of a full intervention model to assess risk presupposes that a sentence has been given.

This observation does, however, point to an interesting two-step decision making process that could be used if it were known that a sentence is likely to be effective. In the first step, specific intervention effects for the individual would be ignored (as they are in Figure 10.4 above), while recidivism potential is assessed using multiple domains. Upon arriving at an appropriate sentence, then a second-step would involve use of the effects that had previously been estimated for that specific intervention. Predicted recidivism using the second step could then be compared to that of the first. If the designation had changed from a prediction of failure to one of success, the prescribed sentence is likely to be effective. For example, if the sentence initially called for is that of imprisonment, and imprisonment would not lead to specific deterrent effects, while intensive probation would, then the intensive probation intervention might be chosen (assuming it is an

available option within the cell of the grid of the initial assessment).

Thus far, our examples of risk-based sentencing schemes have been ones where the seriousness of the presenting offense and assessed risk simultaneously lead to a range of recommended sentences. More elaborate strategies might make the assessment of risk conditional upon the seriousness of the presenting offense. Figure 10.5 outlines the possibilities associated with combining the recidivism expectations from multiple domains. The difference between this figure and previous ones is that now the choice of a domain to predict recidivism is represented as a function of the seriousness of the presenting offense. Where the offense is of least seriousness, the domain of choice is the social structural. Where the presenting offense is of intermediate seriousness both social structural and anamnestic domains are combined to make recidivism projections and, where the most serious presenting offenses are involved, only the anamnestic model is used.

There are several reasons why the system of recidivism prediction proposed in Figure 10.5 might be preferred to the earlier formulations. For one, multiple sentencing goals can be reached within a single policy framework. The beneficial aspects of both anamnestic and social structural predictive models can each be utilized. Thus, the most "serious" offenders are sentenced primarily based on anamnestic considerations, with the benefit of being in harmony with "modified just deserts" goals -- more punitive interventions are based on a higher number of past criminal behaviors. This achieves the greatest retribution. The least serious offenders, on the other hand, are subject to varying degrees of intervention on the part of the CJS as a consequence of an assessment of their likelihood of recidivism based on social structural attributes (as always, "adjusted" for race, ethnicity, and

gender). The advantage here is that the "need" component of the social structural variables can be linked directly to the intervention itself. In short, most of the advantages of single-domain prediction can be achieved without many of the costs.

Within intermediate levels of seriousness, there may be justification for giving greater weight to anamnestic considerations for purposes of incapacitation. These offenders, who are deemed higher risks for recidivism, might be those whom the CJS would want to incapacitate for a period of time and then release to the community with various community service requirements and other restrictions. Although such community service requirements would also generally be desirable for those offenders who have committed highly serious presenting offenses, the amount of time between sentencing and release into the community might be too long to warrant a "social structural" component to recidivism prediction at sentencing. Job prospects, family situation, and so forth, could change considerably by the time the release date approaches.[30]

It should also be noted that how the severity of the presenting offense is used as a determinant of the choice of domain depends upon the decisions surrounding "just deserts" considerations: the method of assessing risk is, itself, a function of what is "just" for the offender. Also, one of the consequences of a system such as that suggested by Figure 10.5 is that there would be more intervention to some offenders with low presenting offense seriousness than would typically be the case in most jurisdictions today.

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[30] We are not claiming here, however, that those offenders who fall into the intermediate level of crime seriousness should not be reassessed prior to release, rather we are suggesting which domains, and their associated goals, are probably most relevant at sentencing.

"Standard probation" would be the most likely intervention for all but the highest of risks in this category (as would be the case if the Minnesota grid were used).

Finally, consider one more multiple-domain strategy for incorporating risk of recidivism into the sentencing decision. Figure 10.6 represents the use of risk assessment within the cells of a Minnesota-like sentencing grid. Here the number of prior convictions, as well as the seriousness of the presenting offense, constitute the "just desert" component of sentencing. Within each of the cells, however, there are variations in the degree and amount of intervention based on risk considerations. Thus, unlike in Minnesota, where judges are free to sentence within the "acceptability" range prescribed for each cell of the grid, Figure 10.6 "limits" such decisions based on recidivism considerations. The nature of the suggested interventions may be defined in manner similar to that delineated in earlier figures where the recommended sanctions follow from the domains used to assess risk.

The advantage of the risk-modified Minnesota-type grid is that even more emphasis is given to the "modified just deserts" perspective than in earlier schemes. "Punishments" are explicitly defined by the offense severity and prior record dimensions. Yet, there is still the possibility of a wide variety of CJS interventions available to the judge, guided, in part, by a policy of risk assessment. That policy may involve single or multiple domains, as discussed in general for Figures 10.2 through 10.5, though the use of multiple domains is attractive for the reasons discussed earlier.

In summary, there is a wide variety of options for incorporating risk assessment into the sentencing decision. Strategies have been suggested that involve both the use of single and multiple domains to assess the likelihood

that an individual will recidivate. The considerations of Figures 10.2 through 10.6 reveal some of the advantages and disadvantages to predicting recidivism using single or multiple domains. The anamnestic model may be defined by some as most justifiably linked to punishment decisions, and especially decisions that have incapacitation or specific deterrence as a goal. The social structural model lends itself to the selection of more benign interventions, so-called "treatment" interventions, some of which may be coupled with incarceration and monitoring if the presenting offense is serious enough and the risk of recidivism is relatively high. As well, moral objections to the use of one domain over another may be moot to the extent that there is overlap in the predictions of the two types of models, or no one is predicted to fail according to the social structural model who is not predicted to fail by the anamnestic model. As long as no one receives an incarcerative sentence or a longer incarceration as a result of the use of a sentencing system that utilizes a social structural model to determine risk, moral objections are less relevant.

A multiple-domain model for risk assessment offers the distinct advantage of enhanced prediction over the predictions of single domains such as the anamnestic or social structural models. As such, crime control goals, especially incapacitation and specific deterrence receive the most legitimation compared to the alternative of trying to ignore such goals in sentencing (i.e., retributionist sentencing). One possible cost of using a broader set of predictors, (the basic model as opposed to, say, the social structural model), is that the assignment of more benign interventions to an offender might be different than would be the case if only the social structural model were used. In a situation where economy dictates that only

those with the highest risk of harming society receive the relatively "benign" interventions, the assessment of risk using a more inclusive model may result in some "needy" individuals not getting the benign interventions because they to not present a high enough risk for recidivism.

Should the "pedagogical" models represented above be seriously considered for sentencing purposes? We think that they should be seen in the context of a broader policy orientation, one that takes into consideration many of the other concepts and considerations discussed in our research. As such, our recommendations for the sentencing of offenders are centered around the consideration of several policy options, ones that should be considered and debated by jurisdictions interested in achieving more systematic sentencing policies. That is, we do not prescribe any particular sentencing strategy as the most sensible for all jurisdictions. Rather, policy should be derived based on a rational discussion of goals, resources, and the political and social environment in which criminal justice decisions are to be made. Below we summarize several basic questions that should be addressed in deriving policy for sentencing.

A SUMMARY OF POLICY CHOICES FOR SENTENCING

One of the themes of this chapter has been that many crucial policy decisions must be made in order to implement risk-based sentencing scheme such as those just described. These various policy choices are depicted in Figure 10.7. Six general policy decisions are discussed here, corresponding to the columns of the figure. The first is the severity of the presenting offense. It seems that whatever subsequent choices are to be made at sentencing, they must hinge on an assessment of the seriousness of the crime. While this may

be assessed in ways reflecting the public's view of crime seriousness, it is more likely to be based on the legal classification or "degree structure" of criminal codes. If there are multiple crimes, a decision must be made as to aggregating or selecting the most serious of the offenses. There are many issues involved, and we will not elaborate them here (but see Tonry, 1987; Morris and Tonry, 1990; Wolfgang et al., 1985). Although three general levels of seriousness have been presented in earlier figures, policy makers may prefer to choose more to achieve finer gradations of offense severity that are more consistent with statutory requirements.

One general point about the number of seriousness levels chosen deserves mention. Tonry (1987, 1988) and others have called for classifications of crime that allow for a more precise evaluation of the seriousness of an offense than is available from current legal categories. For example, "burglary" of a certain degree might be broken down into a more refined set of categories, each corresponding to differences in the seriousness level of the crime. Although such a refined system of classifying the offender's presenting offense may be desirable for research purposes, we argue here that there are limits to such classifications for sentencing applications: much of the variation in seriousness that can be "imposed" through a more precise designation of the nature of the offense probably should not be used for sentencing purposes. For example, if burglar A happens to victimize a residence in which more valuables are contained than burglar B, should burglar A serve more time in prison as a result to the greater seriousness of the crime, as measured by dollar value stolen? Many other examples could be made

of the "capricious" nature of such a sentencing schema.[31] We think it more reasonable, and more in harmony with just deserts principles, to conceive of rather broad categories of crime seriousness (possibly only three or four). Within each of the broad categories, sentencing would be determined by crime control principles.[32]

However presenting seriousness is defined, it seems crucial to link the assessment to the general goal to be achieved for offenders who are classified similarly by offense seriousness. Thus, those offenders who are at the high end of the continuum of offense seriousness may be eligible for a sentence that is geared to punishing the offender in accordance with either retributionist principles or incapacitative ones. Those offenders lower on the continuum may be sentenced with emphasis on various other crime control goals. At the level of policy directives, a jurisdiction may choose to establish a threshold level of offense seriousness above which "mostly retribution" is the explicit policy goal. For example, premeditated murder would presumably be an example of a presenting offense for which retribution receives primary consideration.[33]

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[31] We credit Daniel Glaser for this idea, which is based on his oral remarks at the 1990 American Society of Criminology meetings.

[32] For example, aggravated robbery, rape, and murder may be included in the most serious category of criminal behavior; robbery, aggravated assault in a second level of seriousness; burglary and other more serious property crimes in a third level; and lesser property crimes in a fourth. Such a classification may seem incredibly simplistic relative to the complex legal classifications that most states use, but, aside from specificity inherent in legislated mandatory sentences, most such classification, are, and have been, largely irrelevant to the actual punishments that the CJS administers.

[33] It is even possible to conceptualize the policy issue here as one in which the severity of the presenting offense is assigned a numerical weight (as in Wolfgang et al., 1985) and linked to varying "degrees" of emphasis on retribution versus crime control goals.

Crime control is often seen in opposition to retribution as a general goal for sentencing. Yet, it should be clear that when the seriousness of the presenting offense determines the classification of a case as receiving a "retributionist" or "crime control" sentence, the two can coexist, even if the sanctions are largely driven by retributionist considerations. Thus, entire systems of decisions can be based on premises grounded in retribution. Here, we depict the goals of crime control as being most relevant only to those cases that are less serious than the most serious offenses.[34] Whatever the general form of the link between offense severity and general goal, it should be established as a general standard to be applied for everyone who is sentenced.[35]

Once crime control goals are chosen are seen as desirable for sentencing policies, (and we think that for the vast majority of sentenced offenders, such goals would be relevant), three issues concerning the assessment of risk for recidivism are raised. The first is the aspect of crime that the sentence will attempt to control: it must be decided what outcome criteria should be used to determine the intervention. If a jurisdiction chooses to focus on violent crimes or on high rate offenders, then recidivism projections based on such factors must be made. If reimprisonment is the chosen criterion, then

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[34] For those offenders sentenced primarily in accordance with retributionist or incapacitative goals, the alternative goals of specific deterrence and rehabilitation may be more relevant at parole.

[35] At a practical level, we envision only a relatively small proportion of offenses as falling into explicit consideration for "mostly retributionist" goals: approximately 10% of those convicted for felony offenses. The remaining 90% would be sentenced primarily with some form of crime control goal as primary. It is also possible to conceive of the "top ten percent" as subject to retributionist goals through most of their sentence and crime control goals toward the end of their sentence. Thus, there are many specific variations to the general treatment of crime control versus retribution that can be formalized into a single system of sentencing for any jurisdiction.

recidivism probabilities should be calculated based on that outcome. Simple rearrest (e.g., for a felony) may also be used, in that policy is geared to limiting any form of subsequent offending. Multiple outcomes may also be considered. As discussed above, a decision tree may be used to arrive at the appropriate intervention for an offender when multiple forms of recidivism are to be considered. At the level of a general policy, jurisdictions need to decide what criteria are of greatest concern and, ideally, rank them accordingly.

The goal of crime control also necessitates decisions involving the choice of predictor domains(s) and choice of cut-off points to define levels of risk. The resolution of either of these decisions, and that for the recidivism criteria, has implications for the others. Thus, a choice of a cut-off point affects who is defined as "high risk," and a choice of who is chosen as "high risk" may determine what outcome criterion triggers an intervention. Choice of a cut-off point can only be made relative to the model used to predict recidivism, and such a model presupposes that decisions have been made regarding what domains of predictor variables should be used in assessing recidivism. In addition, the choice of a cut-off point based on considerations of a civil-libertarian ratio can only be made relative to some general form of intervention. Thus, it seems likely that a relatively low civil-libertarian ratio (more concern for false negatives than false positives) may be selected for "benign" interventions and a relatively high civil-libertarian ratio (more concern for false positives than false negatives) may be selected for more punitive interventions.

Finally, the policy options available at sentencing include the forms intervention. In Figure 10.7 we have listed several general categories of

interventions, though options will vary by jurisdiction, availability of intermediate sanctions and programs, and so forth. Policy could also dictate some combinations of interventions. For example, if community supervision is involved, then decisions need to be made as to the degree of supervision (frequency and nature of contacts by probation officers), what, if any, job and program participation should be required, and if some form of monitoring is feasible. Moreover, which types of offenders (i.e., levels of offense severity and risk) would be appropriate targets for any or all of these interventions must be decided.

All of the columns represented in Figure 10.7 are conceptually interrelated. The severity of the presenting offense and goal emphasis seemingly determine, as a policy matter, the extent to which crime control goals are relevant at sentencing. Considerations of recidivism criteria, domain, civil-libertarian ratio, and the intervention itself are contingent upon presenting offense severity and goal emphasis. Choice of domain has implications for which civil-libertarian ratio is selected, as does choice of recidivism criteria. Different recidivism criteria yield different base rates: the lower the base rate, the more likely that lower cut-off points will be selected, subject to considerations of civil-libertarian ratios. Choices must also be made for the width of the observation window. Short follow-up time periods have lower base rates than longer follow-up time periods. Thus "window length" affects what cut-off point is associated with a particular civil-libertarian ratio.

As the discussion above implies, there are natural affinities between some of the possible outcomes for these policy choices. Decisions to incapacitate (if that is a policy goal), lead to the anamnestic domain, or the

associated criminal career domains, if emphasis is to be given to the "retributionist" nature of these types of predictor variables. That is, these predictor domains also represent a "modified retributionist" concern for past failures. For the individual with relatively more prior arrests or imprisonments, there is a greater likelihood of recidivism and a greater weight may be given to an incapacitative sentence. The use social structural variables to assess recidivism chances invites treatments that address the "needs" identified by that domain. The choice of a domain may thus point to interventions that follow from that domain.

Imposing a hierarchy on multiple recidivism predictions suggests similar hierarchies for the decisions concerning the choice of domain, civil-libertarian ratios, and recommended interventions. Initially assessing risk for recidivism through more serious behaviors (i.e., persons crimes or imprisonment) suggests the use of anamnestic predictors, high civil-libertarian ratios, and more punitive interventions and follows from the incapacitative and specific deterrence goals of sentencing. Moving down a hierarchy to risk for lesser forms of recidivism points towards a greater use of social structural predictors, potentially low civil-libertarian ratios, more benign interventions, and a greater overall concern with a rehabilitative goal for the sentence. Thus, while Figure 10.7 identifies many distinct policy choices that must be made, once a few central ones have been decided, many more may follow readily.

One of the key aspects to the risk-based sentencing policies suggested here are their flexibility as a policy tool. Assuming the basic premise that the risk of recidivism should be formally incorporated into the sentencing decision, policy makers must decide on the numerous issues we have summarized

here. Each decision has consequences for the individuals sentenced, as well as for the society in which such individuals eventually are placed. Trade-offs between the various choices have been identified in the discussions above. Research must provide the policy makers, and judges, for any jurisdiction with the specific information necessary to make informed decisions for sentencing.

THE FUTURE OF SENTENCING AND RECIDIVISM

It has been a premise of our research that there is, and should be, a connection between the nature of the CJS intervention and the prospects that the offender will recidivate. Several forms of this connection have been discussed. The philosophical aspects of the relationship were addressed in terms of the goals of sentencing. Empirically, we investigated how sentences may impact subsequent recidivism. More practical considerations of the connection between sentencing and future offending were studied from the perspective of making predictions about individuals. At the level of CJS policy, we offered suggestions as to how the likelihood of recidivism can be incorporated into the sentencing decision. As such, the research here has little value to those who would exclude recidivism considerations in the determination of CJS interventions. We have argued that such a position is untenable: in practice, attention is given to recidivism throughout the system, and crime control is a justified goal of intervention.

How to better use recidivism prospects in intervention decisions has been the primary focus of the present chapter. The research community, with its use of formal models of recidivism, can be helpful to policy makers and practitioners wishing to incorporate risk assessment in their decisions. To

do so, however, requires asking questions about what constitutes recidivism, and choices need to be made about what predicts it. We have described some of the dimensions recidivism measurement and prediction that may provide answers to these questions. The various choices among independent and dependent variables that are made are not, however, without consequence. The discussions of this chapter have attempted to enumerate these choices and the consequences attached to each.

The implications of our research have led us to call for more sentencing policies which explicitly use the assessed risk for recidivism in the determination of an appropriate sentence for an individual offender. Several possible ways to accomplish this were suggested. In general, risk-based sentencing points to the importance of the ideas of "intermediate" punishments, as outlined by Morris and Tonry (1990). The traditional bifurcation of punishment into probation and imprisonment does not seem an adequate one for sentencing policies of the future. More explicit and controlled consideration of recidivism in making criminal justice decisions will allow for the successful implementation of strategies such as theirs.

Ultimately, we believe that incorporating individual risk into sentencing will achieve a greater "truth in sentencing" in two fundamental ways. The first is that what is stated as the form of intervention at sentencing will be closer to what actually happens to the individual than is generally the case in most jurisdictions today. Second, what is decided at sentencing will have an explicitly justified basis, as opposed to an implicit or possibly capricious one. Truth in sentencing seems to have these two meanings, and both can be accomplished through the use of risk-based sentencing policies.

The first meaning of "truth in sentencing" is contrasted with a highly diffuse decision making process in which what actually happens to an offender is a result of multiple decisions made by numerous CJS officials. Frequently, different standards are used to decide similar intervention decisions. After an individual is sentenced, a correctional classification board may decide when and where an inmate is placed, probation may decide the extent of supervision, parole may decide the time and form of release in the community. All such decisions may be made reasonably, and sometimes with explicit criteria, as in the case of risk assessment instruments determining the level of probation or parole supervision.

A diffused decision making system is not objectionable, but in practice becomes so in that different criteria are used across decision makers when the same criteria should be used. For example, the criteria for release of an imprisoned offender into the community under parole supervision may be made by a parole board who may use criteria involving an assessment of the possible risk to the community. Such criteria may involve a grid system in which an individual offender is assigned a score based on prior convictions, prior incarcerations, employment history, and so on. Another offender may be given a sentence of probation and be released into the community although he/she committed the same crime but is considered a low recidivism risk on the basis of some vague, unstated, criteria. Still another offender in the same jurisdiction is sentenced to jail and released after serving a presumptory proportion of the sentence without any consideration of his/her recidivism potential.

As we understand how most state systems work, there is little coordination across agencies and little correspondence in terms of the

operating assumptions of each decision maker (or their agency). In part, this arises because of the different tasks mandated for each agency.[36] Nevertheless, we do not see how the variation in criteria for release or for determining who gets what form of intervention can be justified within the same jurisdiction. What has happened over time is that organizations have achieved relative autonomy from each other, and each have established their own definition of "justice" as it pertains to the decisions they must make. We are not saying that any particular agency's definition of what is a reasonable decision is better or worse than any other organization, including that of the judge. Rather, we conceptualize decision-making in the CJS as one of relative agency autonomy, and that autonomy may be at odds with a consistent and justifiable system of intervention decisions.

One mechanism to control the bureaucratic diffusion of decision making, and the associated variability in standards, is to establish explicit criteria at sentencing using principles that can also be adopted by other decision makers who process and treat offenders. Thus, if risk assessment is a component of decision making, as we argue it is (at least implicitly) in all parts of the criminal justice system, then the formalized assessment of recidivism should be based on similar principles throughout the system. Risk-based sentencing can make the sentence the source for such formalization. It is the legal basis for the intervention, and the particulars of the sentence set the parameters for decisions to be made by other parts of the system.

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[36] Of course, not all decisions are "the same", and some discretion should be exercised by each decision maker. Thus, for example, the decision as to when to release an offender into the community when that offender has received an incarceration sentence cannot be made entirely at sentencing. Also, "people change" as do their attributes over time, so that reassessment of risk, for example, should be part of the responsibility of diffused decision makers.

Other agencies can only "do to" a convicted offender what is authorized by the court, yet somewhat oddly, the court has allowed these other agencies considerable latitude in defining the criteria, as well as in exercising discretion in making the decisions.

The second sense in which we advocate truth in sentencing is in making explicit the basis for the sentence that is given to an individual. In most jurisdictions in this country, the justification is quite mystified. It has something to do with the seriousness of the offense, with the perceived chances of recidivism and, in the worst case, with the race or ethnicity of the offender. The entire premise of using the risk for recidivism as part of the sentencing decision is to make more formal and explicit the criteria that determine a sentence, both in terms what leads a judge to consider a type of sanction and in terms of the decisions that led to a sentencing policy.

Consider the Minnesota sentencing guidelines in relation the goal of "truth in sentencing." To a great extent, the MSCG grid represents a successful attempt to achieve truth in sentencing (in the first sense) in that the judge establishes the nature of the sentence and determines, more so than in most other states, how long the offender serves in prison. In this regard, more truth in sentencing is achieved, although discretion is still exercised by probation, and by corrections in the determination of actual release dates. Truth in sentencing in also achieved in sense of being "explicitly justified," in that the criteria themselves are explicit. However, we question the justification. Critics have noted that one decision in the Minnesota sentencing grid is the most important: the decision to incarcerate or not. For the vast majority of offenders convicted in Minnesota, the decision is one of probation. Only after numerous failings, in the form of prior convictions,

does the system incarcerate an offender. Within level of presenting offense seriousness, it is the count of prior convictions that determines when, and for how long, such incarcerations occur.

The justification for using prior convictions is in a modified just deserts argument that "more is worse" -- the greater the number of prior convictions, the more punitive the intervention should be as the offender is more "culpable" or "deserving" of punishment. Yet, as formulated, traditional probation is the expected sentence for most crimes and most criminals. Although there are varying degrees or levels of supervision in Minnesota's probation department, for most offenders the sentence is essentially "the same." This hardly seems in accordance with just deserts principles, as their advocates have defined them. Moreover, to increase punishment as a strict function of the number of convictions and to justify that on just deserts grounds seems mystifying.[37] For this and other reasons, one can argue that the Minnesota Sentencing grid has moved in the right direction in limiting discretion, and centralizing decision making, but has not been particularly successful in establishing that the basis for the sentence is justified relative to any particular and explicit sentencing goals.

"Truth in sentencing," as the term has been used by Morris, describes what has never been the case in sentencing: because the actual treatment received is the result of decisions by other CJS personnel, offenders seldom receive the punishment given to them at sentencing. At this point in time,

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[37] The just deserts basis for the count of prior convictions as the reason for punishing an offender is elusive. Why the seventh-time shoplifter gets time in prison when the sixth-time receives probation, does not seem justifiable under just deserts theory. Rather, as we understand the derivation of the Minnesota sentencing guidelines, considerations of efficiency and crime control seem to have been implicitly instrumental in the acceptance of the dimension of counts of prior convictions.

the sentence only sets the general nature of the intervention. Every prison or department of corrections has control over when offenders are released, and every state has allowances for release from prison for earning "good time". This leads to the irony that offenders are usually given a longer sentence than they need serve, and thus the sentence is not "truthful."

Truth in sentencing has also been lacking due to the politicalization of the sentence. At least for the cases covered by the media, the sentence aims at appearing "tough" on offenders, so that others may be deterred from offending. Symbolically the sentence appears to be punitive, but there is almost always less punishment administered than is suggested by the sentence: the actual intention of the CJS is to treat the individual less harshly than is stated at sentencing. For example, individuals may be sentenced to fifteen years for robbery, but are eligible for parole in two and may actually serve three years. In part, this is because of department of corrections' need to retain discretion in release matters, but, it part, it is because the system never intended for the maximum time to be served by the offender (though, of course, some relatively small proportion of offenders will serve the maximum). This too results in the sentence being less truthful than it might otherwise be.

We see achieving "truth in sentencing" as a reasonable goal for future sentencing policies. Moreover, we believe that risk-based sentencing policies provide the opportunity for attaining this goal. If the sentence received is to be linked to levels of assessed risk and the severity of presenting offense, the factors entering into the judge's decision are made clearer. As more offenders are funneled into sanctions that are tailored to meet their needs, and those of society, the likelihood that what the sentence says should

happen will happen is increased. As well, our proposals should lessen the slippage between the amount of time a person is sentenced to and the amount of time served. All these increase truth in sentencing under the first sense of the term. Yet, in order for such policies to work, the justifications for sanctions have to be made explicit or else it is not possible to recommend sentences for various combinations of presenting offense seriousness and offender risk. Even the manner in which individuals are to be designated as a "risk" has to be justified explicitly with reference to policy goals before a risk-based sentencing policy can be implemented. In this sense, greater truth in sentencing will also result.

Arguably, there can never be complete truth in sentencing in that what happens to the individual can never be completely determined by the judge's pronouncement of the sentence. Still, there can be less discretion at sentencing, as well as afterwards, if risk assessment is formally incorporated, and the sentence itself is carried out. At the same time, for those serious presenting offenses, the emphasis on retribution goals should satisfy the need for symbolism in regards to "getting tough" on such offenders. For the remainder of offenders who are not now subject to any meaningful CJS intervention, more meaningful forms of interventions may result.

No matter what changes occur in sentencing practices, it will still be the case the CJS will be characterized by diffuse decision making: some autonomy is needed in all parts of the system. Police officers must be allowed to decide whether or not to proceed with an arrest, prosecutors must have the ability to determine whether to bring the case to court, judges must be allowed discretion as to the particulars of the sentence, prison officials

need control over parole release, and probation officers need to revise levels of supervision when the situation (e.g., employment) of the probationer changes. Few of our policy recommendations reduce this diffused decision making.

A common aspect underlying all of these decisions is implicit assumptions about the risk an individual poses for further criminal behavior. Officials are more likely to act in a certain way if they believe that the person poses a high risk. In this sense, the same decision must be made repeatedly throughout the system. It is a decision that we think can be more formalized, and more consistently made, on a system-wide basis.

The availability of more, and better, information is one way of formalizing the decision making process. Better measurement should yield more refined predictions for risk assessment at any stage of the CJS. Thus, we agree with those who have advocated and attempted to provide sources in which care has been taken in the collection and verification of the data elements (see the discussion of Gottfredson and Gottfredson, 1988). Data, such as that of PROMIS used by prosecutors making charging decisions (Institute for Law and Social Research, 1976) constitute "decision aids" that should be available throughout the CJS. At the systemic level better record keeping, and greater access to what is known about the person and his/her prior behavior, should lead to more consistent decision making over the treatment of individuals.

Figure 10.1

A Possible Decision Tree Approach
For Resolving Conflicting Predictions

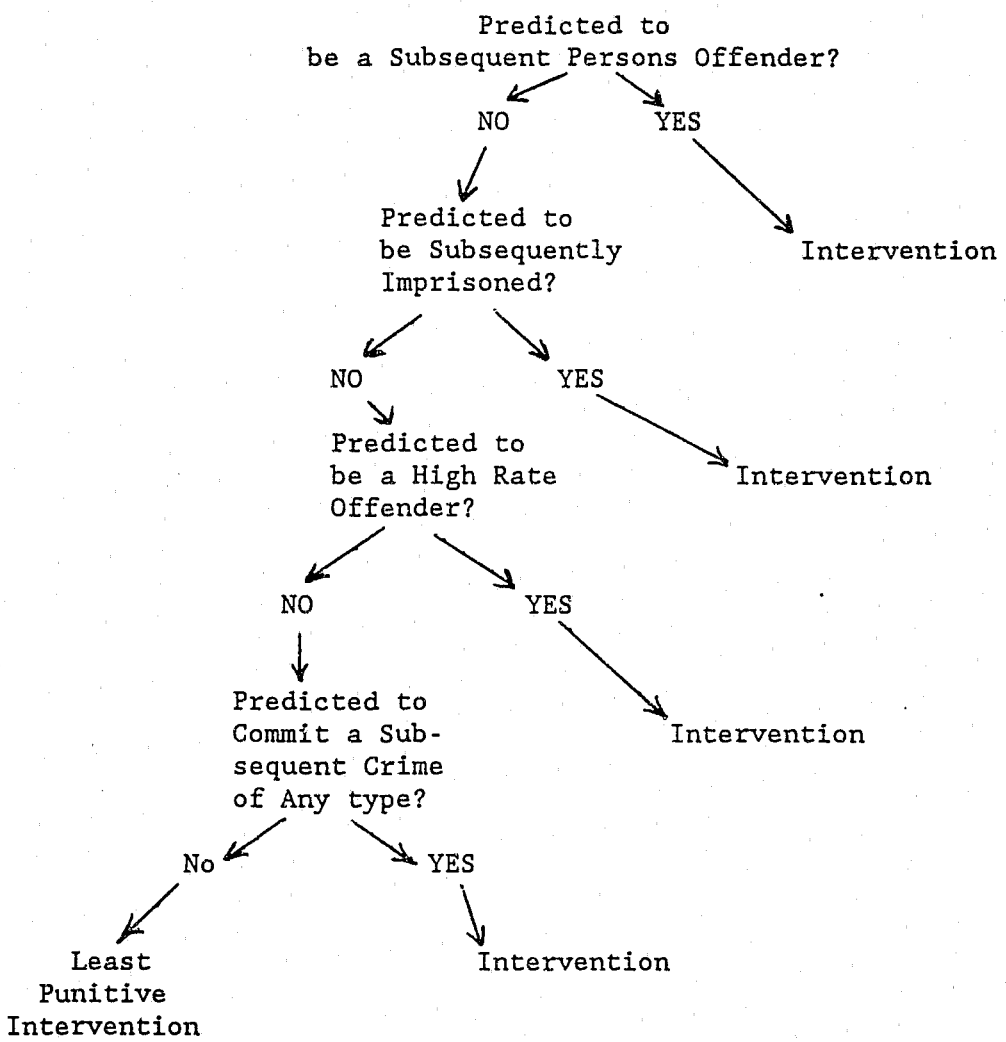

Figure 10.2

Example of Grid System Using Only Anamnestic Domain and Presenting Offense Seriousness
(Cells Include Intervention with Months of Supervision)

Severity of Presenting Offense	Predicted Probability of Recidivism Under Anamnestic Domain Model: Low	Medium	High
Low (Theft under $2500, Non-residential Burglary)	Probation 12	Probation 24	Jail 12
Medium (Residential Burglary, Simple Robbery	Probation 24	Jail 12	Prison 18
High (Aggravated Robbery, Murder)	Prison 24	Prison 36	Prison 72

Figure 10.3

Example of Grid System Based Only on the Social Structural Domain and Presenting Offense Seriousness
(Cells Include Intervention with Months of Supervision)

Severity of Presenting Offense	Predicted Probability of Recidivism Under the Social Structural Model: Low	Medium	High
Low (Theft under $2500, Non-residential Burglary)	Probation 12 No contacts	Probation 24 Job Requirements, Community Service, GED	Probation 36 Job and Leisure Curfews, Drug Testing, Monitoring, Program Participation
Medium (Residential Burglary, Simple Robbery	Probation 24 Job Requirements	Jail 12 Job and Leisure Curfews, GED, Drug Testing, Monitoring	Prison 18 Job and Leisure Curfews Drug Testing, Monitoring, Program Participation
High (Aggravated Robbery, Murder)	Prison 24 Job and Leisure Curfews (on release)	Prison 36 Job and Leisure Curfews, Community Service, GED, Drug Testing, Monitoring (on release)	Prison 72 Job and Leisure Curfews, Community Service, Monitoring, Program Participation (on release)

Figure 10.4

Example of Grid System Based on Basic Model
("Adjusted" for Race, Sex, etc.) and Offense Severity
(Cells Include Intervention with Months of Supervision)

Basic Model
Predicted Probability of Recidivism:

Severity of Presenting Offense	Low	Medium	High
Low (Theft under $2500, Non-residential Burglary)	Probation 12 No contacts	Probation 24 Community Service, Fine, Curfew	Split Jail 12 Program Participation, Fine, Curfew
Medium (Residential Burglary, Simple Robbery)	Probation 24 Community Service	Split Jail 12 Program Participation	Prison 18 Program Participation, Fine, Curfew
High (Aggravated Robbery, Murder)	Prison 36 Community Service Fine Curfew Program Participation	Prison 72 Community Service Fine Curfew Program Participation	Prison 144 Community Service Fine Curfew Program Participation

Figure 10.5

Using Severity of Offense to Determine Choice of a Domain for Predictive Models of Recidivism

		Basis for Recidivism Prediction	Recidivism Probability: Low	Medium	High
Severity of Presenting Case	Low	ST	Standard Probation or fine 12	More Intense Probation Supervision, Fines 24	Intense Probation, Require Job, Curfew 36
	Medium	ST & AN	Probation, with Restrictions re: curfew, driving, associates, etc. 18	More Intense Probation with Restrictions, and community service 40	Intense Supervision, drug testing, job, curfew, further restrictions 52
	High	AN	Incarcerate Low Security, Upon Release Restrictions 24	Incarcerate Medium Sec., Job Training, Education, Drug Program 48	Incarcerate Max. Sec., Job Training, Education, Drug Program 96

ST = Emphasis given to assessing risk via social structural independent variables.
AN = Emphasis given to assessing risk via anamnestic independent variables.

Numbers in each cell represent presumptive time sentenced, in months.

Figure 10.6

Assessing Risk Within Cells Defined by Offense Seriousness and Prior Convictions

Severity of Presenting Case	Number of Prior Convictions: One	Two	Three
Low	Probation 12-18 LR MR HR	Probation 18-24 LR MR HR	Probation 24-36 LR MR HR
Medium	Probation 18-24 LR MR HR	Probation 24-36 LR MR HR	Jail 6-12 LR MR HR
High	Prison 12-18 LR MR HR	Prison 18-24 LR MR HR	Prison 24-48 LR MR HR

LR = Range of sanctions recommended for those identified as low risks.

MR = Range of sanctions recommended for those identified as medium risks.

HR = Range of sanctions recommended for those identified as high risks.

Figure 10.7

Choices for Sentencing Policies

Presenting Offense Severity	Goal of Sentencing	Recidivism Criteria	Domain(s)	Civil-Libertarian Ratios	Interventions
Low (Theft of under $2500) Intermediate (Burglary) High (Aggravated Robbery)	Retribution Incapacitation Specific Deterrence Rehabilitation	Persons or Violent Crime Reimprisonment High Rate Offender Any Crime	Anamnestic Social Structural All domains Combinations of Domains	High to Minimize False Positives Intermediate to Equalize Concerns Low to Minimize False Negatives	Prison Jail Probation Fines Split Sentences Degrees of Supervision Drug Testing Program Participation Job Requirements Community Service Monitoring

APPENDIX A

SELECTION PROCESSES AND THE SENTENCING SAMPLE

Samples such as those described in Chapter Three are decidedly nonrandom. As offenders are processed through the criminal justice system filtering occurs: all police contacts do not result in arrests, all arrests do not lead to a court appearance, and all court appearances do not result in a conviction. At each stage of the process, cases are nonrandomly removed with a more selective subset passing on to the next stage. A sample of offenders convicted before a state court thus represents one of the most selective parts of the system.

While the winnowing of cases through the system is an inherent part of the process, it becomes problematic for analyses such as those conducted here. In general, the issue is how to disentangle the effects of variables upon recidivism from the influence of those variables upon decisions made earlier in the system. For example, when we find that prior record is correlated with the recidivism of offenders sentenced to state prison, are we able to accurately estimate the magnitude of the relationship? Prior record is also a good predictor of being sentenced to a term of incarceration. Is the impact of record on recidivism real or is it just reflecting the fact that the offender's record put him into a position of being a recidivist from a state prison?

More formally, the problem is one of "selection bias" (Heckman, 1976; 1979; Berk, 1983; Klepper et al., 1983). To date, the consideration of sample selection bias effects is most evident in sentencing research (e.g., Zatz and Hagan, 1985; Myers and Talarico, 1987). Such studies find that variables

representing the probability of being included in a sentencing data set (i.e., the likelihood of being filtered through the system to the point of being sentenced) exert consistent, significant effects. Failure to consider selection bias in such research will yield a misspecified analysis that could seriously distort the impact of commonly used variables such as length of prior record, seriousness of the presenting offense, age of offender, and so forth. If these common independent variables are related to the selection processes, as they usually are, then it is crucial to consider sample selection bias in the analysis.

The concern over sample selection bias is motivated by the observation that nonexperimental designs are less than ideal for the detection of "causal" relationships. Random assignment to treatments (e.g., a conviction, probation sentence, a prison sentence, and so forth) offers the best opportunity to assess how sentences might influence subsequent criminal behavior.[1] In the absence of randomization, the statistical control of important variables becomes necessary. Which variables are used as controls and how well they reflect the phenomena to be controlled will then determine the adequacy of "causal" relationships identified from nonexperimental designs.

Figure A.1 summarizes the problem from the perspective of assessing the impact of sentences upon recidivism. Certain variables influence the likelihood of an individual being convicted. Yet these variables are also likely to influence the sentence imposed, as well as the probability that the

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[1] As Berk (1983) has noted, there is a problem of infinite regress here. The true experimental design would randomly allocate individuals at the first point of the process (i.e., commission of a crime) and then randomly allocate them to outcomes at each stage (e.g., being arrested, the case being sent to the prosecutor, a conviction in court, etc.). This is no more feasible than a random allocation of individuals to a particular sentence.

offender recidivates after receiving that sentence. Failure to control for these variables is likely to misstate any deterrent or rehabilitative effects of the sentence.

By limiting the sample to only those convicted, two additional problems arise. First, the distributions of the independent variables to be controlled have been truncated. If, for example, offenders with longer prior records, or with a more serious presenting offense, are likely to be convicted, then when these variables are used in the analysis of recidivism, we no longer have the "true" relationship between prior record and criminal behavior or of offense severity and subsequent crimes. Thus, the efficacy of these statistical controls for studying recidivism has been reduced. Second, even if the use of a selected sample did not truncate the independent variables, we have, in effect "double counted" the impact of these variables, once in their determination of the sample and once in the analysis of sentencing effects.

Consider the use of presenting offense in the assessment of the impact of sentencing. Certain offenses (e.g., murder) are more likely to lead to a conviction, if for no other reason than the fact that the evidence must be strong for the case to be brought to court. In earlier analyses using a preliminary version of the sentencing data file (Smith and Smith, 1986) we found that robbers sentenced to a state prison were significantly less likely to recidivate than robbers receiving other sanctions. Further, this effect was not found for burglars receiving prison sentences. What is unclear is if the differential response to the imposed sentence by presenting offense (the statistical interaction) is truly an effect of the sentence on recidivistic tendencies. Alternatively, this could simply represent the influence that presenting offense exerts on the likelihood of being in the conviction sample.

One possible solution is to attempt to control for the selection process in the same manner as is done for substantively relevant variables. How we attempt to do this is described in the present Appendix. After briefly reviewing the correlates of selection in the criminal justice system, we describe approaches to modelling the selection process. Previous research using these techniques is also reviewed. We then detail the application to the sentencing data set, where variables representing the selection process at several steps of the criminal justice system are estimated for each case in the file. Finally we discuss some forms of selection that are not addressed by our procedures.

SELECTION PROCESSES IN THE CRIMINAL JUSTICE SYSTEM

The criminal justice system (CJS) has been viewed as a "funnel" (Lisefski and Manson, 1988) with relatively many individuals observed at the beginning of the CJS when crimes are reported to the police and suspects arrested, and relatively few individuals at prosecution and adjudication. At each stage cases are dropped, offenders jump bail, or are acquitted, or placed on probation -- each resulting in fewer cases advancing to the next decision point. The sequential loss of cases through the CJS (i.e., crimes going unreported, arrests not leading to prosecution, etc.), is an inherent part of the process. Often the same variables that are predictive of decisions at early stages in the CJS are used in studies of determinants of decisions at later stages. For example, arrest studies find that the seriousness of the behavior is a strong determinant of an arrest taking place. Seriousness of the offense may also effect the prosecutor's decision to prosecute, as well as the judges decision to incarcerate.

Depending on the research question of interest, samples are drawn at specific points in the CJS process. Studies of judicial decision making usually involve samples of cases appearing in court, or court convictions, and do not consider the fact that the existence of a case at that point in the CJS is a product of earlier decisions by police and prosecutors using some of the same factors relevant to the current CJS decision. In recent years, the failure to take into consideration factors affecting the process by which cases are funnelled through the CJS has been cited as a potentially major problem in a wide variety of analyses of the CJS and its effects (Berk, 1983; Zatz and Hagan, 1985). These studies have shown that "selection bias" may influence the parameter estimates for studies of the sentencing process. The essential problem is that determinants of continuing on to the next stage of the CJS are often used in the prediction equations of subsequent outcomes.

Figure A.2 presents a summary of the filtering process that leads to potential sample selection biases in sentencing research. (This Figure is necessarily simplified as all cases do not follow the same path through the criminal justice system.) The funnelling begins with the failure of individuals to report offenses to the police. We know from the National Crime Survey, for example, that approximately 58% of robberies, 59% of aggravated assaults, 52% of burglaries, 73% of motor vehicle thefts, 38% of larcenies with victim-offender contact, and 28% of larcenies without such contact are reported to the police (Sourcebook, 1987:215). Given that a crime comes to the attention of the police, there is a known probability that a crime will be "cleared by arrest." For example, the Uniform Crime Report suggests that 46.3% of violent crimes are cleared by an arrest, while only 17.5% of property crimes are cleared by an arrest.

Much is known about the determinants of whether or not a case will leave the system at any particular point in the CJS process. Table A.1 lists some of the identified correlates of the decision to continue a case past a particular selection point. Some variables, such as type of offense, are relevant for selection at each stage. Other factors (e.g., age of the offender, offender sex) have been found to be determinants of decisions at many points. Still other variables (e.g., victim preference) lose importance as cases get funnelled further through the system while some variables (e.g., prior record) become more predictive of outcomes.

The relatively systematic winnowing of cases from the system has implications for the study of recidivism. By definition, an offender has had to pass through all of the prior decision points to be in a position to recidivate. Given this, the literature has identified many factors (bottom of Table A.1) that are related to the likelihood of recidivism. However, because of sampling strategies, these conclusions are reached in isolation from the selection decisions of earlier stages. The possibility exists that the conclusions that certain variables are related to recidivism may be confounded by how those variables impact upon earlier decision points. Minimally, there is the expectation that failure to consider the correlates of selection can misestimate the impact of independent variables on recidivism.

ESTIMATING SELECTION PARAMETERS

Any time observations are systematically excluded from consideration at a point in the CJS, selection bias may occur. Suppose, for example, that we are interested in the relationship between the number of subsequent arrests and the value on some risk assessment scale. Following Berk's (1983)

exposition, the scatterplot representing this relationship might resemble Figure A.3. The true population regression line is depicted as "Line A" in Figure A.3. A positive slope exists -- the greater the value on the risk scale, the greater the number of subsequent arrests. Now suppose that our data come from offenders sentenced to a state prison. It is likely that a precondition for having received a state prison sentence is high values on the components of the risk scale. For example, offenders with heavy drug or alcohol use, lower ages, longer prior records, earlier age of first arrest, and those sentenced for a violent crime -- the components of the INSLAW scale (Rhodes et al., 1982) -- may have been disproportionately sentenced to state prison. If this assumption holds true, then offenders with low levels of risk would be relatively "unobservable" in our sample. The lower portion of the scattergram (shaded) represents the area with disproportionate missing data, resulting in a regression estimate (Line B) that is too flat.

The consequences for the generalizability of the results are apparent. We would not want to generalize to all offenders based on the sample of those leaving prison: the regression parameters may differ depending on the sample available. Frequently, researchers acknowledge this problem, and admit that the generalizability of the findings are limited to samples of the type used. However, the internal validity of the estimates is also suspect because there is a correlation between the error term for regression line B and the estimated level of risk. In Figure A.3 the correlation is positive: the lower the estimated value of "risk," the lower the value of the disturbance term. Furthermore, the variance of the errors is not constant across levels of the independent variable and this violates one of the basic assumptions of linear regression. Thus, the magnitude of the causal relationship attributed

to the risk assessment scale would be partly a product of random perturbations. Researchers cannot dismiss the problem by saying that they are interested only in cases that have been sentenced to prison. By excluding cases in a systematic manner, a specification error may be introduced that needs to be corrected (Heckman, 1979).

In Figure A.3, the type of selection bias introduced was due to values on the independent variable. This type of bias is called "incidental" or "indirect" and is widespread in social science research in general. In particular, incidental selection drives the filtering processes of the CJS summarized by Figure A.2. At any stage of the process, cases have been removed based upon the values of some set of independent variables and the removal of these cases is likely to affect subsequent outcomes in the criminal justice process. A second form of selection is "explicit" or "direct" selection. Here, cases are removed based upon some threshold value of the dependent variable. (For example, studies modelling the length of sentence might select only those cases whose sentence is greater than zero.) Either form of selection endangers external and internal validity.

Exactly how much bias is introduced by sample selection procedures is difficult to determine a priori. Under direct selection, with the added assumption that the exogenous variables come from a multivariate normal distribution, Goldberger (1981) demonstrates that the coefficients estimated in the sample are proportional to the true coefficients in the population. Moreover, the extent of distortion of the sample estimates is a function of the coefficient of determination in the population and the ratio of the sample and population variances for the dependent variable (Berk and Ray, 1982:359). The estimates for exogenous variables will be either inflated or attenuated,

depending upon the true population coefficient of determination and how the selection process has distorted the variance in the dependent variable. In principle, it is possible to adjust the sample estimates, after the fact, for the distortion produced by explicit selection (Berk and Ray, 1982:375).

In the presence of incidental or indirect selection, however, no such proportional distortion occurs (Goldberger, 1981; Berk and Ray, 1982). Thus it is not possible to adjust sample estimates for the effect of selection processes after the equations have been estimated. However, sample selection effects can be incorporated into the analysis in one of two ways. The first is to use one of the class of event history models (Tuma et al., 1979; Tuma and Hannan, 1984). These models allow for the consideration of the effect of independent variables on several discrete outcomes (the stages of the process) simultaneously and in continuous time with the potential for time variant independent variables as well.

Potential selection biases are reduced by event history models through the use of all cases, including those not making it to a given step of the filtering process, are used in the analysis. By treating cases as censored on the dependent variable (e.g., the case does not make it to court; a sentence does not result in incarceration), information on the full range of exogenous variables is maintained, thus appropriately adjusting the estimated parameters towards the true population values. While independence of error terms in the equations of the various stages is not ensured (Tuma, 1982), and thus parameter estimates may be biased, the use of all observations, including those censored on the dependent variable is preferable to the analysis of only the selected subsample (Zatz, 1985:174).

To anticipate the discussion below, while event history analysis has been successfully applied to transitions through the CJS (e.g, Zatz, 1985; Zatz and Hagan, 1985), the data with the richest information on the timing of events -- official arrest history information -- tends to offer a limited range of independent variables. For example, characteristics of the victim, offender drug use, and offender employment history are less likely to be found in official arrest histories. A potential consequence of this lack of certain independent variables is a misspecification of the equations for processing shifts through the system. The (official) data to be used in the present analysis offer a much greater range of independent variables for modelling movement through the later stages of the filtering process than in the earlier stages.

The second way to incorporate selection processes into the analysis is suggested by Heckman (1976; 1979). He distinguishes between a substantive equation, the parameters of which may be biased due to selection processes, and a selection equation which models the selection process itself (Berk, 1983). These two equations may share common independent variables, though this is not necessary under Heckman's formulation. However, by definition, the two equations share common observations, and thus the error terms are likely to be correlated across equations. Not considering this correlated error can lead to a misspecification of the substantive equation and consequently bias parameters which confound the selection process with the substantive phenomenon of interest.

The selection equation models a discrete outcome (e.g., a case going to court, a conviction leading to a sentence of incarceration) and thus logit or probit estimation procedures are used. For each observation, the estimation

produces an expected probability of the observation being included in a selected sample. In the present situation, these would represent the probability that a case passes the filter at a given stage of the process. The probability is then converted to a measure of likelihood of exclusion and can then be used to generate a hazard rate reflecting exclusion from the selected subsample. Berk (1983) presents empirical evidence suggesting that various estimation techniques (linear, logit, or probit) used to compute the hazard rate will produce similar findings, though he cautions that this may not be a general result.

The hazard rate estimated from the selection equation represents the selection process and yields a new variable that can then be used in the substantive equation for the subset of cases that pass the filter of selection. By including this hazard rate as a statistical control, the biases in the parameters of the exogenous variables of interest are reduced, adjusting them toward their true population values. Comparing substantive equations with and without the hazard rate controlled, the impact of sample selection bias on the parameters of the substantive equation can be assessed.

Heckman's two step approach to modelling sample selection has several advantages for understanding the filtering process of the CJS. First, it is possible to detect the cumulative effects of the sequence of selection at each stage. Cases that make it through the entire system from arrest to conviction to sanction will have hazard rates from each stage. The intercorrelations of these rates can be instructive, suggesting either a relative independence of the filters (i.e., low correlations) or a high interdependence of selection at each stage. Second, exactly which variables in the substantive equation are biased and how they are biased can be ascertained through the comparison of

estimated parameters where the hazard rate is controlled and when it is omitted.

Two cautions concerning the Heckman approach also deserve mention. First, as with any model, the efficacy of the selection equation depends on its specification. To the extent that important variables are omitted from this equation, the predicted probabilities of exclusion will be poor and thus, when the hazard rate is included in the selection equation, the control for selection processes will only be approximate. Second, the procedure can tend to produce high collinearity between the hazard rate and the independent variables of the substantive equation. Many of the same variables are likely to be important predictors of selection at each stage of the system (e.g., prior record, severity of the offense) as well as important exogenous variables for a particular substantive equation. Consequently, the hazard rate, which incorporates the impact of these variables in the selection process, can be collinear with these variables when used to model a substantive process.

THE IMPACT OF SAMPLE SELECTION BIAS

In the past five years a handful of studies on the CJS have been published that explicitly address the significance of selection bias for criminal justice research. In general, these studies focus on the sentencing process and correct for selection bias using one of the two procedures described above. Here, we highlight the known consequences of sample selection bias.

Peterson and Hagan (1984) use Heckman's approach (1976, 1979) in their analysis of race and sentencing. They compare equations corrected for

selection bias with uncorrected equations and find that the corrected equations lead to different conclusions concerning a number of variables. In contrast to the uncorrected equations, the corrected equations find that middle-class status, education, age, Nixon-appointed judges, judges with high severity scores, and pleading not guilty are significant predictors of imprisonment. For certain variables, such as race, the magnitude of the prediction increases when the hazard rate is included in the equation. Additionally, in the corrected equation for sentence length, they find that the hazard rate heightens the prediction made by the big dealer, drug user, middle-class status, and plea (not guilty) variables.

Hagan and Parker (1985) use hazard rates to model the transition from conviction to sentencing. Significant correlates of the correction factor include the strength of the evidence, prior record, and year of disposition. However, when the hazard rate is included in the equation analyzing sentence severity, the only difference between the corrected and uncorrected equation is that the type of charge is significant in the corrected equation.

Myers and Talarico (1986a, 1986b, 1987) and Myers (1987, 1988) also correct for selection biases in their analyses of the social contexts of sentencing. In a number of articles using the same data base, Myers (1987) and Myers and Talarico (1986a, 1986b) find that the hazard rates they compute are highly correlated with offense seriousness. Had they failed to include the hazard rate, their analysis would have overestimated the influence of offense seriousness on later stages of the criminal justice process. Additionally, in an article on judicial background and sentencing, Myers (1988:658) notes that "a comparison of additive models with and without correction for sample selection bias indicated that failure to correct

underestimated the effects of offender race and gender, as well as of judge's religion and local background."

In a series of articles using a longitudinal data base from the State of California, a number of studies have corrected for sample selection bias by using event history analysis. In general, these articles have focused on the issue of criminal justice processing, particularly on the timing of critical events (e.g. pleas) and the likelihood of making key transitions within the filtering process. Hagan and Zatz (1985) model the rate of transition from arrest to three different dispositions within the CJS: police release, prosecutor denial of complaint, and court. Although the analysis is conducted for five different samples (based on number of prior arrests) and is difficult to summarize concisely, overall their analysis suggests that certain biases, such as rural-urban differences and racial differences, are most evident at earlier stages of processing. In addition, they suggest that certain relationships, such as the impact of crime specialization on processing, become accentuated as individuals move through the system, and as they reappear following subsequent arrests.

Zatz and Lizotte (1985) analyze differences in the amount of time it takes offenders to move from arrest to either a guilty plea or a trial. According to their analysis, a wide range of variables affect the rate of change from arrest to disposition by plea for first arrests, including race, crime type, evidence, severity of offense, duration, and age. In addition, race, type of offense, use of firearm, duration, and sex significantly affect the rate of transition from arrest to disposition by trial for first arrests. For subsequent arrests, the findings are less conclusive, though pleas continue to heighten the rate of transition (as they did for first arrests).

Less serious offenses appear to increase the rate of transition from arrest to plea and offense specialization decreases the rate of change from arrest to trial.

In a similar article, Zatz (1985) focuses on the timing of transitions from arrest to three dispositions: acquittal or dismissal by the court, sentences not involving incarceration, and incarceration. Again, the findings are complex, but generally Zatz finds that a number of variables (pleading guilty, being white, black, or female) tend to accelerate processing for first arrests, and that this tendency is greater for sentences that do not involve incarceration.

Finally, Zatz and Hagan (1985) utilize event history analysis to model three transitions within the CJS -- from arrest to either prosecution, conviction, or prison. This article specifically addresses the degree of bias introduced when earlier transition points in the system are not taken into account. When they compare the three models, Zatz and Hagan (1985:15) point out that the prison sample yields biased estimates for violent offenses, "reflecting shorter sentences than do estimates from samples drawn at earlier processing stages." Additionally, some variables, such as age and mode of disposition, are significant only at earlier stages of processing. Other variables are significant only for later stages of processing for subsequent arrests.

In general then, virtually all of the studies that have considered the effect of sample selection bias on criminal justice research identify significant differences between samples drawn at later stages of the process and those drawn at earlier stages. Zatz and Hagan (1985:121) summarize their research this way:

> Findings indicate that bias is introduced when censored observations are excluded from the analysis. Moreover, the effects of certain exogenous variables on sentence length differ, depending upon the selection criteria. Some variables appear to be significant only in models with selection bias; others, only in models with less bias.

This variation from one model to another is typical of studies that have taken selection bias into account, and motivates the need to control for selection bias in the present research.

Though these studies have contributed greatly to our understanding of the consequences of selection bias, at least two shortcomings of previous research are worth noting. First, all of the studies that have corrected for selection biases in the CJS have used official records as maintained by a state-wide repository. Zatz (1985) and Zatz and Hagan (1985) make use of official arrest histories maintained by the State of California, while Myers and Talarico (1987) use arrest records provided by Georgia authorities. The use of such official arrest histories is increasingly common in sentencing research and research on criminal careers (e.g., Blumstein and Cohen, 1979). However, from the perspective of sample selection, such official data are also subject to selection bias since all local arrests are not reported to the central repository. This underreporting is yet another stage of the process that can introduce biases into analyses of later stages (see Figure A.2).

Accounting for this bias is particularly difficult since not much is known about the completeness of arrest history data. Cohen (1986), comparing crimes in the Washington, D.C. Annual Reports to that city's official arrest histories, finds that over 90% of arrests appear in both sources. Slight variation by crime type was also observed with 99% of auto thefts and 91% of aggravated assaults found in both sources. Much greater levels of underreporting were found in a comparison of the State of Michigan's Uniform

crime reports with the official arrest histories for the Detroit SMSA. There, from 40% (aggravated assault) to 47% (burglary) of the crimes known locally were reported to the official histories. In part, the low rates in Detroit were produced by a policy of forwarding only those arrests with known dispositions. In addition, Cohen speculates that "Other factors that may affect police reporting to central repositories are the extent to which local departments are linked routinely to the central repository. Lack of geographic proximity, absence of administrative ties, and heavy reliance on locally maintained records could all contribute to reduced reporting to a central repository" (1986:330-331).

Additional factors that can lead to underreporting in arrest histories have been noted by the Panel on Criminal Careers (1986). Some events might not meet reporting standards (e.g., fingerprint identification) and some jurisdictions may be more likely to forward arrest data than are others. Indeed, just as caseload considerations and severity of the offense can influence the decision to continue a case through the system, they may also be expected to influence a local department's decision to report an arrest to a repository. It is likely that arrests for minor violations will be underreported as will arrests that occur in jurisdictions with high levels of paperwork. The first stage of the analysis below attempts to model the selection biases introduced by underreporting.

The second shortcoming of previous research on selection bias is its failure to explicitly address the consequences of sample selection bias for studies of recidivism. All of the empirical applications to date have looked at processing through stages up to and including the sentencing decision. Thus, to our knowledge, the insights gained from the literature on sentencing

(Hagan, Zatz and colleagues) have yet to be extended to the study of recidivism. Yet, there is reason to believe that selection processes can influence the ability of certain variables to predict recidivism.

Research on recidivism has had a variety of objectives. Some studies are aimed at determining what predicts parole outcomes (e.g., Gottfredson et al., 1978) while others focus on which type of sentence has rehabilitative or specific deterrent effects on recidivism (e.g., Lipton et al., 1975). However, independent of a particular purpose or definition of the dependent variable, recidivism studies have pointed to a set of predictive factors. These include past criminal behavior, offender age, age of onset of criminal behavior, offender sex, marital status, race/ethnicity, commitment offense, drug and alcohol use, education, and institutional behavior. All, in varying degrees, influence recidivism. (See S. Gottfredson and D. Gottfredson, 1986, for a review.)

As we noted earlier, many of these variables influence the probability of a case being pushed through the CJS (See Table A.1). For example, the type of offense is related to decisions to arrest and prosecute, whether the defendant plea bargains, and to the sanction received. Prior record influences the chances that an offender is both convicted and receives a more severe sanction. What is clear is that many of the best predictors of recidivism are also related to incidental selection processes that occur prior to an individual's being in a position to recidivate. Consequently, sample selection bias may be affecting the parameter estimates in previous studies of recidivism.

Exactly what kinds of biases are introduced are difficult to determine in advance. By extending the findings of sentencing studies that control for

selection bias, some impacts may be anticipated. Peterson and Hagan (1984) find that controlling for the hazard rate increases the effect of being a drug user and a drug dealer on the length of sentence received. The findings of Myers and Talarico (1987) suggest that failure to control for sample selection will overestimate the effect of offense seriousness. Myers (1988) finds that race and gender effects are underestimated prior to the correction for selection processes. Zatz and Hagan find that offender age has a decreasing impact as cases are processed through the system. Thus we can expect that correlates of recidivism <u>will</u> be misestimated if sample selection is not statistically controlled. If these sentencing study results extend to recidivism, we should find that offender race, gender, and drug use are <u>better</u> predictors of recidivism than previously thought, while offender age and the seriousness of previous offenses are less predictive of recidivism.

The amount of variance explained in recidivism studies varies, but generally falls in the range of .15 to .20 (Gottfredson, 1987:33), and thus much of the variance in recidivism remains unexplained. The low proportions of variance explained in these models may be due to an inherent randomness in the phenomenon being explained, or to omitted variables such as the hazard rate. The statistical literature on selection effects (Goldberger, 1981; Berk and Ray, 1982) points to some known relationships between the variance explained and the effects on the coefficients under some forms of selection. In general, coefficients will be attenuated relative to their true population values. This suggests that stronger predictors of recidivism may emerge after controlling for selection processes.

The difficulty in speculating how the predictors of recidivism will change once sample selection biases are incorporated in the analyses is, in

part, due to the paucity of previous research on the topic. While the inclusion of hazard rates is likely to change the predictive ability of some variables, exactly which variables will be effected and how their coefficients will change is ultimately an empirical question. Furthermore, it is quite possible that variables previously found to be unrelated to levels of recidivism may emerge as significant factors after controlling for selection artifacts. In the following sections, we offer an analytic strategy for an initial assessment of the impact of sample selection biases on models of recidivism.

SELECTION INSTRUMENTS FOR THE SENTENCING DATA BASE

Overview.

At any stage of the sequential filtering process of the CJS, two equations are needed to model phenomena at that stage (Berk, 1983): a substantive equation for the decision being modeled (e.g., prosecute, incarcerate, etc.), and a selection equation representing the risk of making it to that point of the system. Thus, for example, studies of length of prison sentences need a selection equation for the likelihood of being sentenced to prison, studies of the arrest process need a selection equation for the risk of the offender coming in contact with the police, and studies of recidivism need a selection equation for the probability that an individual is sentenced to a given sanction.

The present analyses model the selection process at several stages of criminal justice processing. We use three sources of official data taken from various stages of the system. Because of the relatively limited range of independent variables available in the early stages of the CJS, we are better

able to specify models for both the substantive and selection equations as we move further through the selection process.

Our goal is to account for the nonrandom aspect of the Sentencing data base described in Chapter Three. This will be done by estimating several selection equations to model the likelihood that a case in the Sentencing file has passed a given selection point in the criminal justice system. To develop the appropriate selection equations for this sample, we "backtrack" through the process, modelling substantive and selection equations, as best as possible -- using official records. Several steps (e.g., selection into a particular sanction) can be modeled within the Sentencing data base. Other steps (e.g., an arrest leading to a court appearance, a court appearance in a state court leading to a conviction) will be modeled using state-wide official arrest histories. The selection processes that lead to an arrest appearing in an individual's arrest history will be modeled using state-wide data from the Uniform Crime Reports.

We choose 1975-76 as the time-frame for the analyses because descriptive statistics from the Sentencing data base indicate that, for 80% of the cases, the arrest that led to inclusion in the data set occurred in one of those years. That is, for the vast majority of cases in the Sentencing file, the arrest led to a conviction within one year. We collapse data for these two years into one time period, as there is no reason to believe that selection processes differed between 1975 and 1976.

Figure A.2 gives some indication of the extent of selection that occurred during this two-year period. The Uniform Crime Reports for New Jersey, (New Jersey Attorney General, 1975; 1976) indicate that 423,280 adults were arrested during this time. By comparison, a search of the State Police

computerized arrest histories found 165,667 arrests during 1975 or 1976.[2] Thus, about 60% of the arrests reported in the Uniform Crime reports fail to pass the filtering of either fingerprint identification of the suspect or reporting the arrest from the local authorities to the arrest history repository.

The arrest histories maintained by the State Police Department of Systems and Communications (SAC) also provide information of the disposition of those arrests that are reported.[3] Of the arrests reported in 1975-76, 20,261 (12.2%) had no further records, suggesting that the case had been dropped before going to court. Fifty three percent were ultimately disposed of in a municipal court, with about half of those arrests leading to a municipal court conviction.[4] Cases that made it to a Grand Jury, (35% of all reported arrests), stood about a 70% chance of proceeding to a Superior Court. If the arrest got that far in the system, the odds were slightly greater than 2 to 1 that the case would be disposed of through a conviction.

Clearly, there is a considerable drop off between an arrest and a conviction in a Superior Court. The current evidence suggests that

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[2] Approximately 2,000 arrests for which county of arrest was missing were dropped from the analysis.

[3] As with the reporting of arrests, not all dispositions are reported to SAC. Thus, the numbers in Figure A.3 are only approximations of the extent of selection through the system. Those interested in the extent of missing SAC records for individuals in the Sentencing data set are referred to Smith and Smith (1990).

[4] Where the arrest left the system was classified according to the ultimate disposition of the case. For example, a case that went to a Grand Jury, but was finally resolved in a municipal court was coded as only going through municipal court. Similarly, cases that were settled in a superior court were assumed to have gone through a grand jury. While this coding of the disposition of the arrest simplifies the complexities of the processing through the system, we feel it is sufficient to establish the crucial selection points of the system.

probability of an arrest proceeding to a Superior Court conviction (the basis for the Sentencing sample) is about .066.[5] That this filtering is non-random is quite likely and, indeed, raises the potential for a selection bias. Below, we attempt to identify the systematic aspects of selection for certain points in the filtering process.

Classifying Charges.

One factor likely to drive a case through the system is the offense itself. Certain crimes are more likely to be cleared by arrest and the evidence needed to get an arrest for some offenses may be stronger than for others. Consequently, it is imperative that information about the kind of offense be maintained in our analysis. However, the three sources to be used in modelling the selection process code the offense differently and this necessitates some decisions concerning how to map offense codes from one data source to another.

As was described in Chapter Three, charges in the Sentencing file were coded using a Master List Number based upon the state penal code, while charges in the SAC arrest histories were coded using the Uniform Offense Code. These different ways of classifying charges were equated under a twenty-two category system based upon the major headings of the Uniform Offense Code scheme. It is within this framework that the Uniform Crime Reports method of classifying charges was equated with the codings of the other data sources.

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[5] Note that the number of cases resulting in a Superior Court conviction, 28,109 arrests, compares favorably with the size of the Sentencing sample. The former covers a two year period while the Sentencing sample of 14,321 was collected over slightly more than 13 months.

The UCR scheme codes only the most serious offense, based upon an assumed hierarchy of seriousness.[6] Twenty-nine different offenses are distinguished, with the last two, curfew violations and runaway, being status offenses. These offenses, and their rank in the seriousness hierarchy, are shown in the left hand column of Table A.2. Also shown are the twenty-two offense categories derived from the Uniform Offense Codes. In order to compare the volume of offenses in the UCR data to that observed in the SAC arrests, it is necessary to collapse offense types.

Unfortunately, there is no simple correspondence between the coding schemes shown in Table A.2. In part, this is due to the fact that many of the crimes differentiated in the Uniform Offense Codes (UOC) are considered "all other offenses" under the UCR scheme. But there are also basic differences in the definitions of crimes that preclude a simple recoding of one classification system into another. Preliminary analyses that maintained the distinction between murder and manslaughter found that more murders were reported in the SAC data than in the UCR data. While this may reflect a real change in the charge from the time the arrest was reported to the UCR repository to when it was reported to SAC, it is more likely due to differing definitions of the offense. As our substantive concern is with the filtering of cases as they move through the criminal justice system, it is not desirable to have counts of offense types increase from one stage to another.

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[6] Strictly speaking this is not quite true. Arson crimes are counted if they occur with another crime that is considered more serious under the hierarchy. If arson occurs in conjunction with another offense that is considered a less serious crime, only the arson offense is counted. (The rationale here is that when arson is committed in conjunction with another offense, it is usually one considered more serious than arson. Failure to "double count" the charges would understate the number of arsons occurring in a particular year.) We have followed this rule when comparing the UCR crimes to the counts of offenses derived from the SAC data.

After comparing the counts of offenses in each source and discussion with officials responsible for compiling the UCR tables, a twenty-one category scheme that ranked offenses was derived. This compromise is given in the last column of Table A.2. Three aspects of this scheme deserve mention. First, the UCR offenses of murder, manslaughter, and atrocious assault are combined into one category which is equated with the UOC crime of homicide and this group of crimes is ranked third in the hierarchy. Our reasoning here is that neither source distinguishes between attempted crimes and completed crimes, and that many of the attempted homicides later get downgraded to atrocious assaults. During the processing of the Sentencing data file, we also noted that court charges of atrocious assault often appeared as a homicide charge in the SAC data base. Thus we are reasonably confident that no serious distortion of the offenses is produced by grouping these crimes. As atrocious assaults are much more numerous in the UCR tables, this composite group is ranked below the more serious offenses of rape and robbery.

Second, the fifth ranked crime in the mapped hierarchy combines the offenses of motor vehicle theft and larceny. This is a direct result of how UCR defines the crime of motor vehicle theft. It is more restrictive than that used by UOC, with some kinds of vehicle theft being considered larceny and other kinds deemed to be "other" offenses. As these two crimes are contiguous in the general UCR ranking, combining them should produce little distortion of that hierarchy. The final major compromise across offense definitions is the combining of forgery and fraud into one category. Again this was motivated by the likelihood that an offense of this type would be inconsistently classified across data sources. Here too, these crimes are contiguous in the UCR ranking.

Several of the broad groups defined by the UOC codes were broken down to make them correspond to the UCR categories. Using the detailed 4-digit UCR code, it was possible to differentiate prostitution and vice from other sex crimes, and pull out family and child support offenses, liquor law violations, disorderly conduct charges, and failure to give a good account charges from the residual "other" category used for the Sentencing data set. (These latter offenses are extremely unlikely to be found in the Sentencing data set.)

Arrest to Reporting Transitions.

Data indicating the volume of arrests in New Jersey for 1975 and 1976 are available in the form of tables produced by the State Police for the Uniform Crime Reports (New Jersey Attorney General, 1975; 1976). Our interest is in state-wide arrest totals contained in the following bivariate tables:[7]

Most serious charge by sex of offender
Most serious charge by race of offender
Most serious charge by juvenile or adult
Most serious charge by region of arrest

as well as the trivariate tables of:

Charge by region by sex of offender
Charge by region by race of offender
Charge by region by juvenile or adult
Charge by sex by juvenile or adult
Charge by race by juvenile or adult.

In these yearly tables, offense is categorized into 29 types, including the seven index offenses, as shown in Table A.2. Race is divided into white, black, indian, chinese, japanese, and all other. A preliminary analysis using a trichotomy of white, black, and other suggested that "other" as defined by UCR statistics was not comparable to "other" as defined in the SAC arrest

[7] Ideally, we would like a full cross-classification of arrests by the various variables contained in these tables. Conversations with the office responsible for this information confirm that such tables are never constructed.

histories. We therefore limited the categories of race to a simple black versus nonblack contrast. The UCR tables divide New Jersey into four regions (Northeast, Northwest, Southeast, and Southwest).

Our interest in the arrests tabulated for the New Jersey Uniform Crime Reports lies in their implications for the 928 cell crossclassification of charge by region by race of offender by sex of offender by offender age (juvenile or adult). Unfortunately, such a table is never compiled during the construction of the UCR tables. However, using algorithms developed for the analysis of multivariate contingency tables (Bishop et al., 1975) it is possible to estimate what such a table might look like after making several assumptions. Thus we can approximate the raw numbers of arrests in the 5-way table, even though these frequencies cannot be observed directly.

A multiplication process (iterative proportional fitting) that constrains the expected frequencies to sum to the known marginal distributions of the published bivariate tables (Bishop et al., 1975, pages 91-97), generates the desired table. This procedure approximates the desired table assuming that there is statistical independence in certain three-way tables (e.g., race by sex by offense; region by sex by race) as well as in some two-way tables (e.g., race by sex). By necessity, we are also forced to assume that all four-way interactions (e.g., charge by region by sex by race), as well as the 5-way interaction involving all variables, are not statistically significant.[8]

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[8] While these assumptions are needed to proceed with the analysis, they are quite likely to be false. The sheer volume of arrests (423,280 for adults alone) guarantees that even interactions of small magnitude will be statistically significant. Substantively, there is the expectation that certain combinations of variables may interact to lead to a volume of arrest higher than could be predicted by those variables alone. For example, in the urban Northeast region of the state, it is likely that black males arrested for property crimes would

Briefly, the steps taken are as follows. For each year of published data, the 928 cells in the full table for that year are estimated with reference to the marginal counts of the known 3-way tables listed above. For example, the number of white male adults arrested in the Northeast region for larceny is estimated subject to the constraint that when this table is collapsed across race and sex, the actual reported number of adults arrested in the Northeast for larceny is observed. If the estimated table were collapsed across all regions and sex, the number of reported arrests of adults in the Northeast for larceny would be found. Iterating across each marginal table for all 928 cells yields the desired table for each year.[9]

Any cells pertaining to the arrests of juveniles were then deleted as our concern is with the arrests of adults and the selection processes surrounding them. This leaves a 464 cell table for each year. These two tables were then added together to produce one table summarizing the frequency of adult arrests, by charge, region, race, and sex, for the time period under study. Finally, the combined table was collapsed across charge to produce estimated frequencies that would be comparable with the definitions of charge as shown in Table A.2. Thus, our best estimate of how arrests in 1975 and 1976 were distributed across offense, race, sex, and region are contained in a 336 cell table.

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define a cell with a significant interaction.

[9] Seven iterations were used, though the expected frequencies converged after the fifth iteration in both years. It should also be noted that one of the published tables in 1976 (charge by juvenile or adult by race) contained several errors so that the total number of arrests in the table was several thousand less than for the other tables of that year. Discussions with the agency responsible for compiling this information could not resolve the problem, so we used the published frequencies. Fortunately, the iterative proportional fitting algorithm is quite flexible and the estimated frequencies converged to a distribution that summed to the total arrests reported by UCR in 1976.

In order to establish how cases are filtered from the arrest process to the reporting in arrest histories, it is necessary to construct a comparable table from the individual-level arrest histories maintained by the New Jersey State Police Department of Systems and Communications (SAC). A subfile containing any offender with an arrest in either 1975 or 1976 was constructed from a search of the master SAC tapes. Using this subfile, it was possible to generate a simple crossclassification of charge by region by sex of offender by race of offender through recoding county of arrest into the one of the four regions and most serious charge into one of the groups shown in Table A.2.

This multivariate table of expected UCR arrests can then be compared to the table constructed from the individual-level arrest history subfile. On the basis of this comparison, bias parameters can be estimated. The probability that an arrest with a given configuration on the independent variables (e.g., a white male arrested in the Southwest region for a robbery) appears in the SAC data base can be computed by taking the number of those kinds of arrests appearing in SAC divided by the state-wide estimate of UCR arrests.[10] A better understanding of the process of selection at this point in the criminal justice system can, however, be obtained by modelling the set of these probabilities. This is done through a probit analysis for aggregate data.

The dependent variable for the model is the probability that a case moves from an arrest reported by UCR to a reported arrest in the SAC arrest histories. In addition to the independent variables of most serious charge,

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[10] Our collapsing of race into black versus non-black, and the mapping of most serious charge across the UCR and Uniform Offense Codes, has ensured that the estimated frequency of arrest in UCR, for any combination of variables, will always be greater than or equal to that observed in SAC.

region of the state, sex of offender, and race of offender (black versus nonblack), several contextual variables for characteristics of the region will be used. These variables, taken from the New Jersey Uniform Crime Reports (New Jersey Attorney General, 1975, 1976), are:

Regional crime rate per 100,000
Percentage of crimes cleared by arrest in the region
Regional violent crime rate per 100,000
Police employees per 1,000 in region.

These measures provide some control for both the volume of crime in a region and how much manpower is available to combat crime. Values on these variables were assigned to each combination of charge, region, sex, and race depending upon whether the information came from 1975 data or 1976 data.

Table A.3 shows the results of the probit model. The predicted value of a probit equation is a standard normal variable (z-score) with, in this case, higher values indicating that an arrest is more likely to proceed to being reported in an arrest history. Negative coefficients can thus be interpreted as reducing the probability that an arrest proceeds to the SAC files, while positive coefficients suggest the reverse. On this basis, several conclusions can be drawn from Table A.3.

Charge exerts a significant effect on the likelihood that an arrest gets reported to the SAC repository. In general, the less serious the charge, the more likely an arrest is to disappear from the system. The reporting process is also influenced by regional considerations with arrests from the Northwest portion of the state less likely to appear in SAC and arrests from the Northeastern and Southwestern regions more likely to pass the filter of fingerprint identification and reporting. (Arrests in the Southeast region are used as the referent category.) The volume of crime itself influences the reporting process as higher crime rates and greater clearance rates increase

the probability of an arrest being reported to SAC, with higher violent crime rates reducing the probability of an arrest proceeding to the SAC files.

Offender race and sex also have a significant effect on the reporting process. Arrests involving blacks and females are significantly less likely to be found in the SAC arrest histories than in the UCR tables. This finding must be tempered, however, by the identification of several interactions involving these variables. Arrests of black females are proportionally more likely to appear in the SAC records. Furthermore, race and sex interact with region in several important ways. Arrests of females in the Northeast regions of the state are more likely to be found in the SAC histories and arrests of females from the Southwest are significantly less likely to pass this filter. Arrests of blacks from the Southeastern region are significantly less likely to be found in SAC than in the UCR tables. Finally, region interacts with the race-sex interaction to produce proportionately more arrests in SAC of black females from the Northeast and Southwest parts of the state with a (marginally significant) decrease in the arrests of black females from the Northeastern region.

Exactly what can be concluded from this aggregate analysis is difficult to ascertain. Clearly the results establish that the process of an arrest going from being reported to the UCR to a fingerprint identification and the filing the report of this arrest to the SAC repository is nonrandom. It is just such a nonrandom filtering that motivates the concern over incidental selection. Beyond that, interpretation gets much more speculative. What we have modelled here is how the volume of arrests in one source differs from the volume of arrests in another. This could represent several, sometimes contradictory, processes.

It could be that our results indicate different policing practices as applied in the various regions of the state, with some regions more likely to arrest an individual and later drop the charges. As the most serious crime charged is an important factor in our results, we may be seeing the influence of how a particular offense is classified by the local authorities. Similarly, different reporting practices could be tapped here with some jurisdictions more likely to report an arrest to SAC, even if the charges are dropped, while jurisdictions in other regions report only those cases that go beyond the arrest stage (see the analyses below). The certainty of fingerprint identification, needed to forward an arrest to SAC, could well vary by region and by the kinds of offenders and offenses that characterize a region.

Alternatively, these findings could be interpreted as identifying bias in a discriminatory sense, given that the offender's race and sex have been found to exert significant effects on the likelihood of a case going from simply an arrest as reported by UCR to being part of an offender's rap sheet. If this is true, however, our results are quite complex. Unlike the usual findings that can be interpreted as racial discrimination, arrests of blacks are <u>less</u> likely to be found in the SAC files than are arrests of whites. (The finding that arrests of females are less likely to proceed through the system is consistent with other literature suggesting a more lenient treatment of females, at least under some circumstances - see, for example, Bernstein et al., 1979; Nagel and Hagan, 1982; Kruttschnitt and Green, 1984; Hagan, 1986). But, the patterning of interactions is not consistent with an across the board differential treatment of either blacks or females. In some regions arrests of females are more likely to proceed to SAC and in others, those arrests are

less likely to be forwarded. The same can be said for arrests of blacks and the three way interactions between region, race, and sex.

Regardless of how one wishes to interpret the results in Table A.3, the coefficients of the probit model can be used on the cases in the Sentencing data set. For each case of that file, the predicted value based on the selection equation of Table A.3 can be computed.[11] This predicted z-score can then be transformed into a hazard rate following the steps outlined in Berk (1983). The resulting indicator will represent the likelihood that the particular case would be excluded from the SAC arrest histories, given the most serious charge, offender race and sex, and the region of arrest, as well as the values for that region's contextual variables.[12]

Hazard rates computed in this fashion can be interpreted from the perspective of instrumental variables representing unmeasured aspects of the particular case. For example, a case that has a high likelihood of exclusion (not being reported to SAC), but continues on through the criminal justice system nonetheless, is apt to be one where the values on other, unmeasured, variables is quite high. Strength of evidence, for which there are no

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[11] The results described in the next section indicate that certain aspects of prior record (e.g., number of prior weapons charges) influence the likelihood of a case moving through the courts and leading to a conviction in a Superior Court. A consequence of these particular measures of prior record is that we must restrict our focus to only those cases in the Sentencing file that were matched with records in the SAC file, for it is only for those individuals that comparable measures of prior record can be computed. An advantage of using matched individuals is that most serious charge at arrest can be computed from their SAC records. This makes the computed hazard rate more comparable to the data used to generate the UCR to SAC selection equation as the differences in offense definition across data sources is minimized.

[12] The most serious charge was computed for the arrest that led to entry into the Sentencing data set (see Chapter Three). For those arrests occurring prior to 1975, contextual variables were computed using 1975 values. For the small number of arrests found after 1976, 1976 contextual variables were used.

indicators in either the UCR or SAC data sources, is a good example. An arrest that results in a conviction in a State Superior Court even though, based on the variables used here, there is the expectation that it will not pass the initial filter modeled, is likely to be one where the evidence is perceived to be sufficient to produce a conviction. Mitigating and aggravating circumstances surrounding an arrest may also be represented in this hazard rate. By controlling for the likelihood of selection we can, to some extent, control for variables not measured.

By necessity, the analysis to this point must subsume all selection biases that occur prior to this step. A crime had to occur, it had to have been reported to the police, the offender had to have had contact with the authorities, an arrest had to have been made and that arrest had to have appeared in the state-wide Uniform Crime Reports. As was mentioned earlier, many of the correlates of selection through these filters are the same. Furthermore, many of the determinants of the selection process to this step (e.g., preference of the complainant, policing practices) -- for which no indicators are available -- are known to be correlated with variables to which we have access (e.g., race of offender and county of arrest). To the extent that the offender characteristics of age, race, and sex, the type of offense, and the county of arrest are correlated with unmeasured legal and offense-specific factors, misspecification of the selection equation at this step will be slight.

Transitions Through the Courts.

Having gained some understanding of what influences the reporting of an arrest to the SAC repository, we now turn to modelling selection processes within the court system. We limit our attention to four decision points in

the system: a) a case proceeds past the reporting of an arrest, either to a municipal court or a grand jury; b) if it proceeds past arrest, does it go to a grand jury; c) does a case that has gone to a grand jury get disposed of in a Superior Court, and; d) if a case reaches a Superior Court, does it result in a conviction? (These decision points are shown in Figure A.2.)

The factors influencing selection past each of the stages will be modelled using data from the SAC file of arrests in either 1975 or 1976. SAC records are organized by arrest, adjudication, and custodial cycles with records available for the charges at each arrest, grand jury appearances, municipal or Superior court appearances, and court outcomes at either level.[13] Through the use of these records, it was possible to code each arrest by where it left the criminal justice system (Figure A.2) and dummy variables representing making it to a particular decision point were created.

The independent variables are all taken from the SAC file as well. The first record for each individual has information on date of birth (from which age was computed) and race and sex. Prior record, a variable missing from many previous studies of the selection process, was measured in several ways. Through counts of arrest and court records prior to the 1975/76 arrest, it was possible to compute:

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[13] We are at the mercy of the accuracy of these records. While the SAC files have been audited several times for accuracy and these data are constantly being updated, there is the suspicion that court records are missing from the SAC arrest histories. We know that certain kinds of custodial records are poorly reported to SAC (Smith and Smith, 1990). Given the limitations of the SAC data, were are apt to underestimate the probability that a case goes past arrest (no court records were reported), as well as the probability that the case resulted in a conviction (grand jury records were available, but no superior court outcomes were filed). In identifying the path that a given arrest took through the system, it was assumed that all cases that were disposed of in a Superior Court went through a Grand Jury. Consequently, we can compensate for some missing Grand Jury records.

Number of prior arrests
Number of prior charges at those arrests
Number of previous convictions on any charge
Number of prior weapons charges
Number of prior drug charges
Number of prior probation sentences
Number of prior jail terms
Number of prior prison terms.

The charges at the arrest in 1975/76 were coded using the twenty-two category system based on the Uniform Offense codes (Table A.2). These were originally calculated as the number of charges of each type. However, certain kinds of offenses were quite infrequent and their use resulted in problems of collinearity among the measures of instant offense type. We therefore deleted counts of charges for six groups of crimes (kidnapping, arson, extortion, embezzlement, bribery, and all other offenses). The majority of the other independent variables used in this aspect of our analysis are self-explanatory.

Table A.4 summarizes the selection models for each of the decision points under consideration. The left-most column shows the final equation for the probability that a case continues past the reporting of the arrest.[14] The vast majority of arrests (87.8%) reported to SAC in those years proceeded past

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[14] Software limitations forced several compromises on these analyses. First, the probability of a case going past arrest was modeled using a 25% random sample of all cases in SAC. A 50% random sample was used when analyzing the probability that a case went to the grand jury. Second, it was not possible to include the entire battery of independent variables (up to 53 different variables) in the first estimate of equations prior to model trimming. Four subsets of variables were first run -- all charge measures, half of the dummy variables for county, the remaining county indicators, and all measures of prior record. Variables that were significant in either the equation without a measure of prior hazard(s) or with prior hazard(s) were kept for the first estimated equation. (The exceptions were race and sex of offender which were retained regardless of the level of significance.) This equation was then trimmed by deleting variables with no significant coefficient in either equation. One consequence of this approach is that the referent (omitted) county changes from equation to equation.

the arrest stage. While the model does account for a significant proportion of variability in the likelihood of continuing on through the system, the pseudo R^2 statistic suggests that our ability to model this part of the selection process is slight.[15]

Variables from each of the three domains have statistically significant effects on the probability of the arrest proceeding to another part of the criminal justice system. Each fraud and escape charge lowers the chance that the case will continue, while the other significant charge variables all increase the likelihood of continuing on. Of these, charges for robbery and drugs have the largest coefficients. The county indicators show a similar pattern with cases from only one county (Essex) significantly less likely to go past arrest. Cases from Salem and Cape May counties are especially likely to continue through the system. Few offender variables are found to be important for transitions at this point. The only prior record indicator that is significant is number of prior weapons charges and each charge reduces the chance that the case proceeds past arrest. Black arrestees are less likely to have the arrest continue to a court or grand jury, as are older offenders.

When the hazard instrument reflecting the likelihood that the arrest was reported to SAC is included, these effects change more in magnitude than in substance.[16] Two additional charge variables now reach conventional levels of

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[15] There is no accepted summary statistic for probit models that is analogous to the proportion of variance explained in a regression model. We have used the pseudo measure suggested by Aldrich and Nelson (1984).

[16] Predicted values using the probit model for moving from UCR arrests to the SAC arrest histories (Table A.3) were computed in a fashion similar to that described for using this equation in the Sentencing data file. For all equations reported below, the model without prior hazards included was used to generate selection instruments in both the SAC file analyzed here and the Sentencing data file. It is these hazard rates, reflecting the selection at various stages of the criminal justice system, that are used in the analyses reported in all

significance: Each homicide charge increases the chance of continuing on, as does each assault charge.[17] What the inclusion of the prior hazard measure does produce is a change in the estimated magnitude of the parameters. In some instances, coefficients increase, suggesting that the selection process suppresses the "true" impact of the variable on the chance of going on to court or a grand jury. The number of robbery, larceny, and stolen property charges, are good examples here. Conversely, inclusion of the hazard for entry into the SAC files has reduced the estimated effect of some variables. The number of drug charges shows the greatest reduction, through there are smaller decreases for some county variables and the offender characteristic of being black. Note too, that the effect of the hazard instrument is positive. Those cases likely to be excluded from the SAC arrest histories are more likely to continue past arrest.

The models for the likelihood of a case proceeding past arrest clearly establish the utility for considering selection processes, especially as it pertains to misspecifying a substantive equation. Coefficients do change in one direction or another, and some variables become significant when previously they were not. In spite of the fact that we are not able to model transitions through this portion of the criminal justice system particularly well, use of a selection equation is still advantageous.

The next two columns of Table A.4 show the results of analyzing the probability that a case moves from arrest to a grand jury. Here we are able

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chapters.

[17] Caution is urged in using a strict interpretation of significance levels when models with a known selection bias are estimated. The selection process introduces heteroscedastic errors which make for inaccurate estimates of the standard errors used by conventional significance tests. We will say more about the mathematical foundations for our results below.

to do a better job in modelling the filtering process, in part because the dependent variable is less skewed: Approximately 40% of the cases that do go beyond just the reporting of an arrest pass through a grand jury. The pseudo R^2 measure for the model without the prior hazard measure indicates that we are accounting for about 17% of the variability in the selection process at this stage.

The types and number of charges are important for a case proceeding to this point of the criminal justice system.[18] Each serious crimes against persons charge (homicide, sexual assault, robbery) greatly increases the likelihood that a case will appear before a grand jury. Charges for burglary, forgery, gambling, weapons also have relatively large positive coefficients. Conversely, each charge for larceny and property damage decreases the chance that the case will go to a Grand Jury. County effects are more numerous and more variable than in the previous analysis. Cases from some counties (e.g., Bergen, Mercer, Salem, and Sussex) are more likely to go on through the system, while cases from other counties (e.g., Burlington, Cape May and Hudson) are less likely to continue on to a Grand Jury.

While smaller in magnitude than either charges or county effects, characteristics of the offender and his/her prior involvement with the criminal justice system influence the likelihood of an appearance before a

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[18] Charges were defined on the basis of information appearing in the Grand Jury records of the SAC files. If there was evidence that a case went to a Grand Jury, but the exact charges were missing, charges at arrest were substituted. A similar procedure was adopted for defining charges in subsequent analyses of the probability of a case going to Superior Court or being convicted in a superior court. If the court records indicated a charge (or charges), that information was used, if not, charges at the Grand Jury were substituted and, if that data was missing, the charges at arrest were employed. The goal here is to account for plea bargaining and the dropping of selected charges by only using those charges that defined the case at a particular point of the criminal justice system.

Grand Jury. Each prior prison term, prior jail term, prior charge, and prior weapons charge independently increases the chance of going on through the system. Every prior probation term and every prior arrest decreases the probability of a Grand Jury appearance for the current arrest. Blacks and older offenders are more likely to continue through the system.

Inclusion of the hazard variables representing selection into the SAC data set and the case proceeding past arrest alters the substance of the uncorrected equation. The coefficients for the hazard instruments themselves are instructive. The more likely a case is not to be reported to SAC, the more likely the case is to go before a grand jury. Cases that are likely to be excluded after the point of arrest are also likely to be routed away from a Grand Jury. (In the present application, this means that the case is disposed of in a Municipal Court.)

Once prior selection processes are controlled, several of the measured effects for the charge variables change. The most dramatic change is found for the number of presenting drug charges where the sign of the coefficient has changed, while remaining significant. Now, every drug charge decreases the chance that a case appears before a Grand Jury. For some offenses (burglary, gambling, weapons), the inclusion of prior hazards has reduced the magnitude of the associated coefficient. Stolen property charges no longer have a significant effect upon the chance of a case appearing before a Grand Jury. Yet the selection process also appears to suppress the influence of other types of charges (larceny, fraud, escape).

The impact of the county in which the arrest originated is also influenced by controlling for the selection process. Three county indicators (Monmouth, Salem, and Union) no longer have a significant impact on the

probability of an arrest appearing before a Grand Jury, while a fourth (Gloucester) is now seen as significant. As with the charge variables, there is both a reduction in the impact of the county of origin (e.g., Hudson) and a suppression (e.g., Middlesex, Union). Most notable among the latter is Essex county, where failing to consider the selection process greatly misstates how cases from this county proceed through the system. Before including the hazard instruments, cases from Essex county are estimated to have a small, negative, chance of going to a Grand Jury. Once the hazard variables are introduced, however, coming from Essex county has the largest positive effect on continuing on at this point.

Given the marked changes for the charge and county variables, the effect of controlling for selection processes on the estimates for the prior record and offender characteristics variables is relatively minor. With the exception of an increase in the magnitude of the number of prior weapons charges, the effects for the indicators of prior record remain unchanged. The effect of offender age doubles, though in relative terms, the coefficient is small when compared to those for charge and county variables. The selection process has, however, suppressed the effect of offender age and race on filtering at this point in the criminal justice system. Cases involving black offenders are even more likely to go on to a Grand Jury than was evident prior to the inclusion of the hazard instruments, and cases involving female offenders are now seen as significantly more likely to proceed through this part of the system.

The remaining panels of Table A.4 focus on what influences a case to move on to Superior Court and to result in a conviction at that level. Almost 72% of the cases that make it to a Grand Jury are found to continue on to

court. As before, the number of charges for various offenses remains an important determinant of making this transition. Charges for many offenses decrease the likelihood that a case will make it to this point of the criminal justice system. Various kinds of assaults, "minor" property offenses, and escape all have negative coefficients. The largest positive coefficients are found for charges of homicide, robbery, and burglary. The county variables are split in their effects, with cases from about half of the counties significantly less likely to continue to a Superior Court and the other half of the counties significantly increasing the chance that a cases with go on. Using an alias, prior prison terms, and prior drug charges all increase the chance of a case (that has made it to a Grand Jury) continuing on to a Superior Court. Black offenders are more likely to have their case go to a higher level court, while female and older offenders are less likely to have their case continue on.

As before, the introduction of measures of selection at prior points in the criminal justice system change these conclusions. The effects of three of the charge counts (homicide, burglary, and gambling) are no longer significant, and there have been sign reversals for four charge types. Once prior selection processes have been controlled, the impact of the number of charges for robbery, drugs, and weapons charges is estimated to be negative, while the effects of fraud charges is estimated to be positive. Some sign reversals are also found for the county variables, with cases for Hudson county now seen as more likely to proceed to a Superior Court and cases from Salem county seen as less likely to go on. For both the county and charge variables, there is the suggestion that the selection process has suppressed the "true" effects of some variables, while reducing the estimated impact of

others.

Fewer changes are observed for the prior record variables. Inclusion of the hazard variables has led to the identification of a positive effect of prior jail terms on the probability of going to Superior Court. Again a suppression for being black is found, with black offenders more likely to have their cases continue on to a Superior Court. Inclusion of the hazard rates has eliminated the effect of being female and reversed the impact of offender age: Controlling for prior selection processes finds that older offenders are more likely to have their case proceed to a Superior Court.

The effects of the hazard instruments themselves is similar to that found in the analysis of transitions to a Grand Jury. Cases likely to be excluded from the SAC files are more likely to continue to a Superior Court, while cases that are unlikely to go past the arrest stage are also unlikely to continue on to a Superior Court. The new available hazard indicator, representing the likelihood that a case goes to a Grand Jury, has a significant negative impact upon transitions to a Superior Court. If a case is likely not to make it to a Grand Jury, it is also less likely to go to a Superior Court.

The final model summarized in Table A.4 pertains to the probability that a case results in a conviction in a Superior Court. This represents the main criteria defining the Sentencing File*. Over two thirds of the cases that proceed as far as a Superior Court result in the defendant being convicted on at least one of the presenting charges. Our ability to account for the conviction process is limited given the available variables. The pseudo R^2 measure indicates that 11% of the variability in being convicted is accounted for by the battery of independent variables.

At this point in the criminal justice system, all charge variables, with the exceptions of sexual assault and stolen vehicle, have significant positive effects of the likelihood of a conviction. County effects are again varied, with cases from some counties likely to yield a conviction and cases from others, most notably Gloucester and Hunterdon counties, significantly less likely to result in a conviction. The prior record indicators suggest that use of an alias, prior probation terms, prior convictions, and prior drug charges all increase the chance of a conviction, while prior weapons charges decrease the probability of being convicted. Females making it to this point are significantly less likely to be convicted, as are older offenders.

Introduction of the prior hazard instruments does little to change these general conclusions. A sign reversal is found for the number of presenting drug charges, and the impacts of cases from Essex and Salem counties, as well as prior weapons charges and offender age, are no longer significant. The expected changes in the magnitude of coefficients is also found with the impact of some variables decreasing when the selection process is controlled, and the impact of others increasing. Among the hazard measures we find that if a case is likely to be reported to the SAC repository, it is significantly more likely to result in a conviction, while cases that are likely to be excluded after the initial arrest are significantly less likely to yield a conviction if the case comes before a Superior Court.

As a set, the models of selection at these points in the criminal justice system lead to several conclusions. First, from among the variables at our disposal, the numbers and types of charges leveled against an offender are clearly the most important factor driving a case through the system. This is to be expected. The evidence needed to bring some kinds of charges (e.g.,

homicide, robbery, weapons possession) is either stronger or more straight forward than that needed for other offenses. Second, the next most important factor in winnowing cases through the system is the county of arrest, and there is considerable variability in the ability of county actors to push a case through the system. It is striking that there is not one county that is significantly more likely to have its cases proceed at each stage modeled, nor is there a county that is significantly less likely to have its cases pass each filter. Rather, our results for the county indicators are characterized by less obvious patterns.

For example, cases from Burlington county are significantly more likely to proceed past arrest, but significantly less likely to make it to a Grand Jury. If cases from that county do, however, make it to a Grand Jury, they are significantly more likely both to go on to a Superior Court and to result in a conviction. Conversely, cases from Somerset county are significantly more likely to proceed past the arrest stage, but are neither more or less likely to go before a Grand Jury or to a Superior Court than are cases from the other counties. Cases from Somerset that do, however, make it to a Superior Court are significantly less likely to result in a conviction.

What is being captured by the county variables is unclear. It could represent differential reporting of the data at the various stages of the criminal justice system. Alternatively, these indicators could be capturing real differences in how vigorously prosecutors from the various counties pursue cases or the caution used in some counties before a case is brought before a Grand Jury. Differences in the abilities of prosecutors to get indictments or convictions, as well as differences in workload, may also be reflected in the county variables. What is evident from these results is that

cases from all counties are not equally likely to make it past the various filters in the criminal justice system.

While smaller, the influence of prior record on the processing of cases is also apparent. These effects are most notable once the case goes past simply an arrest. The use of an alias significantly increases the likelihood that a case will go to a Grand Jury, a Superior Court, and will result in a conviction. If the offender has prior incarcerations, the case is more likely to go to a Grand Jury and a Superior court. Prior convictions or charges are also likely to increase the chance that a case proceeds. In general, the results for the prior record variables suggest that repeat offenders or the more serious criminals are more likely to have their cases pass the filters of each stage.

We have retained information on offender race, sex, and age in each of the models of selection for their obvious implications on how individuals are treated at each stage. Across all the selection points under consideration, there is no consistent pattern in the coefficients. Black offenders are less likely to have their case proceed past arrest, more likely to go to a Grand Jury, and a Superior Court, but (essentially) equally likely to have the case result in a conviction. The impact of being female on the processing of a case is less straightforward as the existence of an effect is contingent upon whether prior selection steps have been controlled. Our findings suggest that, if anything, being female decreases the chance of the case proceeding through the system. While older offenders are significantly more likely to have their case proceed past arrest and significantly less likely to have it go to a Grand Jury, the influence of offender age after this point is contingent upon whether prior selection decisions have been considered. If

they have been controlled, the impact of age upon later decisions appears to be minimal.

Overall, it is difficult to interpret the results for the offender variables as evidence of any systematic differential treatment of individuals throughout the system. If differential treatment occurs at all, it appears to be limited to particular decisions at particular points in the filtering process. Such a conclusion must be tempered however, by the fact that our later models have not looked at any interactions between offender characteristics and offense or county and our initial model of the reporting process suggests that complex interactions between race, sex, and region may be operating. Similarly, interactions involving race, sex, age, and charge should also be considered.

Finally, and most importantly for the present concerns, the current models of the selection process point to the utility of the hazard measures themselves. At any of the decisions stages modeled here, failure to control for the selection process will lead to inaccurate conclusions in some respect. At times, the conclusion concerns only the magnitude of the estimated effect. Once prior selection is controlled, a given variable may have a slightly smaller, or larger, influence on the outcome. Other times, the effect of the selection process upon the substantive conclusions is more dramatic. We have found instances of variables no longer being significant once the hazard measures are included (the selection process "explains" the effect of the variable), as well as variables becoming significant after controlling for the hazard measures (the selection process suppresses the effect of the variable). We have also found occasions where the sign of the coefficient reverses after the selection process is considered.

These findings for the models which control for selection processes at earlier stages confirm the need to consider such processes: Failure to do so can lead to misspecified models and inaccurate conclusions. Consequently, analyses using the Sentencing Data Base must take into consideration the selection processes that lead a case to be included in that data set. How this is to be done is described in the next section.

INTERCORRELATIONS AMONG THE HAZARD MEASURES

An important consideration in how the results from the previous section are to be used to control for selection processes is the manner in which the hazard rates from each decision point are intercorrelated. These intercorrelations provide important clues about the information contained in the hazard rate for each decision point. High intercorrelations suggest a redundancy in the information captured by the hazard instruments, while low intercorrelations suggest a relative independence in the decisions at a particular stage.

Consider, for example, a pattern of uniformly high intercorrelations. We have modeled the transition of cases through five different points in the criminal justice system. If we were to find that the hazard rates for these five transitions are highly correlated, no matter which decision point were being modeled, this would have two implications. First, the likelihood of exclusion at any point in the system would be relatively <u>independent</u> of the stage at which the filter occurs. That is, our ability to predict the chance that a case proceeds to the next step (given the cluster of independent variables) would be the same no matter which part of the system was being modeled. Second, this pattern of correlations would suggest that only one

hazard indicator would be need to capture all the information about decisions at previous stages. A high degree of collinearity among the hazard rate from each stage would tell us that any one hazard instrument would be sufficient to capture previous selection processes.

On the other hand, a pattern of correlations that showed increasingly high intercorrelations at later points in the system would be indicative of a cumulative impact of earlier decisions. That is, the chance of being filtered out of the system early on is unrelated to the risk of exclusion at later points, but as one gets further through the filter process, the likelihood of exclusion becomes relatively constant. Such a pattern of correlations could occur, for example, if prosecutors put forward most cases early on in the system, but then selected out cases as the chance of getting a conviction became clearer. That could produce little correlation between the risk of exclusion at early and later points in the system, and higher correlations among decisions toward the end of the filter process. Note that pattern of increasingly high intercorrelations suggests that the hazard rate at any step of the process becomes a less perfect instrument for earlier selection decisions the further back in the system one goes. That is, a hazard measure for convictions in a Superior Court might serve as a good instrument for a case appearing before a Superior Court as well, but not be a good surrogate for decisions such as taking a case past arrest.

Other ideal type patterns of intercorrelations among the hazard rates could be postulated. It is unlikely, however, that any would mirror those actually found in the present data sets. Table A.5 gives the observed intercorrelations among the hazard rates, both for the SAC arrest histories used to estimate the selection models earlier, and these same hazard rates

estimated for the Sentencing data set. Consider first the interrelationships of selection decisions in the SAC data (the correlations above the diagonal in Table A.5).

One of the striking aspects of this table is the generally low intercorrelations among the likelihood of exclusion at the decision points modeled. The largest correlation is .414, indicating that if a case is likely not to go to a Grand Jury, but does, that case is also likely not to result in a conviction at the Superior Court level. Conversely, those cases that are likely to go to a Grand Jury are also likely to result in a conviction. Overall, however, the correlations of Table A.5 are suggestive of a relative independence of selection at each stage.[19]

Despite this, some patterns can be discerned from the correlations. The risk of exclusion at the reporting stage (the UCR to SAC transition) is relatively independent of the risk of the case going to a Grand Jury and decisions made in a Superior Co t, but is negatively related to the risk of exclusion after arrest and at the Grand Jury: cases that are more likely to be excluded at the reporting stage are more likely to continue on past arrest and to a Superior Court. While pushing a case on past arrest is related to the risk of exclusion in the reporting stage, the risk of continuing past arrest appears to have no relationship to risks at subsequent points in the system. The risk of exclusion by not forwarding a case to a Grand Jury is relatively independent of prior risks, but becomes increasingly related to decisions about going to a Superior Court and the outcome of the case at that

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[19] The large number of cases in both the SAC and Sentencing data sets ensures that even the smallest of correlations is statistically significant. Consequently, the interpretation of Table A.5 must be based solely on the magnitude of the estimated correlations.

court: cases that are not likely to be sent to a Grand Jury are also not likely to be sent to a Superior Court, and unlikely to result in a conviction if they do make it to that level court.

Decisions as to whether to continue a case past a Grand Jury (i.e., an indictment is presented) are most strongly related to the risk factors that get the case to the Grand Jury. If a case is unlikely to reach the Grand Jury, but does, it is also unlikely to result in an indictment. There is also a negative correlation between the risk of the case being reported to SAC and the likelihood of the case going to Superior Court. Those cases that are more likely to be excluded early in the system are less likely to be excluded by not sending the case to a Superior Court. Finally, cases that result in a conviction in a Superior Court are those which stood a good chance of going to a Grand Jury. However, the risk of a conviction at this level is relatively independent of the risk of proceeding past the other filters considered.

Below the diagonal of Table A.5 are the correlations observed when the equations discussed earlier are used to compute hazard rates for the cases in the Sentencing data file. Overall, these correlations are surprisingly similar to their counterparts computed using the SAC data and thus the general conclusions about the interrelationships among selection decisions at various stages remain the same. Two important differences exist, however, both involving sign reversals for the risk of exclusion at the reporting stage. For the cases in the Sentencing file, the risk of exclusion at the reporting stage is <u>positively</u> related to the risk of exclusion at both the decision to push the case past arrest and the decision to forward the case to a Superior

Court.[20]

The main conclusion to be drawn from the intercorrelations in Table A.5 is that, given the independent variables at our disposal, the risks of exclusion at any of the decision points modeled are relatively independent events. That is, knowledge of how likely a case is to be dropped (or, alternatively, to continue on) past a particular point in the criminal justice system does not inform us on the selection risks at other points of the system. How the characteristics of a case (the independent variables) combine to influence the decisions at each stage of the system are relatively unique to the particular decision. This observation has consequences for how the individual hazard rates are to be used in any analysis that attempts to control for selection processes.

USING THE HAZARD INSTRUMENTS

The main motivation behind the use of hazard rates is the desire to avoid inaccurate substantive conclusions. A failure to consider how certain processes led to the creation of a non-random subsample can lead to a distortion of the effects estimated using that subsample. To date, selection processes have been controlled either by using event history models (which are unavailable to us due to the nature of our data) or by modelling the selection process of the previous step and then controlling for that process in a

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[20] We are at a loss to account for this anomaly. The other eight correlations are close to the magnitudes estimated in the SAC data, and, aside from the sign, the two correlations in question are approximately the same magnitude across data sets. Our initial guess, a computational error, proved incorrect. It is also unlikely that this finding is due to the differing distributions of variables (e.g., numbers of charges for certain kinds of offenses as might be found if there were ,say, more homicide charges per case in the Sentencing file than in the SAC file), as such differences should have effected more than just two of the correlations.

substantive analytic equation.

We know a priori that the filtering that led a case to be included in the Sentencing data base involves a series of selection decisions. Moreover, the results from the previous section indicate that a knowledge of how likely a case is to be excluded at any part of the criminal justice system does little to inform us of the selection risks faced at other stages of the process. This puts us in uncharted statistical waters as little is known about the effects of including multiple hazard rates in a substantive equation. Some discussion of multistage models for event history analyses have appeared (e.g., Blossfeld and Hamerle, 1989), but we are unaware of any discussions of multiple applications of the Heckman correction for selection bias.

Additionally, there are several debated problems with using just a single hazard rate adjustment. Research methodologists do not agree on estimation procedures and the utility of the approach (Berk and Ray, 1982; Zatz and Hagan, 1985). While most agree that selection bias should be addressed, the best approach for identifying selection effects is unclear. The understanding of one-step selection processes has increased since Heckman's original articles. Thus, for example, some evidence (Manning et al., 1987) suggests that separate models for the selection and substantive equations are preferable to a model that incorporates both simultaneously. Dubin and Rivers (1989) have extended the procedure to logit and probit models for substantive equations of binary outcomes.[21]

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[21] While it would have been preferable to use the algorithms derived by Dubin and Rivers (1989) in the current selection models, specialized software is needed.

Some recent simulation results (Stolzenberg and Relles, 1990) indicate that a selection bias correction can actually overestimate the impact of independent variables. This suggests that controlling for a selection bias can do more harm than good. The finding that an overestimation can happen, even when the selection equation is highly accurate in it's prediction of the selection process is even more troublesome. Alternatively, these authors conclude that the Heckman correction may lower bias the most when the error terms of the two equations are highly correlated and the independent variables of the two equations are highly correlated. This is precisely the situation faced in the current modelling of selection through the criminal justice system where omitted variables (e.g., strength of evidence) are likely to produce correlated errors between the selection and substantive equations, and the same independent variables (e.g., presenting offense, prior record) appear in both equations.

It is important to recognize that in one sense the hazard rate represents an instrument subsuming the decision process at the previous stage. As such, it could be interpreted in the same manner as any other instrumental variable -- a surrogate for some other variable. In this sense the estimated hazard rates developed here serve the purpose of standing for decisions and variables we cannot measure directly. A case that stands a high probability of exclusion at some point of the criminal justice system, yet continues on through the system has some unmeasured property that pushes it through. (Strength of evidence and aggravating circumstances come to mind.) By not considering prior hazard instruments, we not only invite potential misestimates of substantive equations, we also lose valuable information about the characteristics of the case and its prior processing, information that

could be relevant to how a case is considered at the point of sentencing or to how the offender behaves after sentencing.

While there are good substantive reasons for considering multiple hazard instruments in any substantive equation, they are counterbalanced by the statistical uncertainties surrounding the use of a single hazard rate and the unknown properties of including multiple selection measures. Consequently, our analysis of recidivism and the impact of sentences on subsequent criminal behavior will proceed cautiously. We will present results that ignore prior selection processes (and thus may be suspect due to selection bias) as well as results that control for prior hazard rates (but are subject to unknown statistical properties).

SAMPLE SELECTION BIAS: ANOTHER DEFINITION

When some form of post-sentence <u>rate</u> is used as a measure of recidivism (e.g., arrest rate adjusted for time at risk, post-sentence convictions per year), additional aspects of selection bias become relevant. In many respects, the issues raised are similar to those discussed earlier. Non-random processes can create situations where it is difficult to ascertain the effects of independent variables. Furthermore, sample selection bias can seriously impair our ability to determine how various sentences (e.g., probation, prison, etc.) influence the subsequent criminal behavior of offenders.

The debate has its roots in the work of Murray and Cox (1979). Using a sample of adjudicated juveniles, they found that the rate of offending was significantly lower after serving the sentence imposed than it was prior to being sentenced. Furthermore, the harsher the sentence, the greater the drop

in offense rates. This "suppression effect" suggested that sentences had an effect upon subsequent criminal behavior and that tougher sentences had greater effects. Despite the acknowledged care in the analysis and exposition of their findings, the results and conclusions of the Murray-Cox study have been extensively criticized and cautions have been raised concerning their interpretations (McCleary, 1980; Maltz, 1980, 1984; Maltz et al., 1980; McCleary et al., 1979). Of primary concern are the methodological issues raised by these critics since they are pertinent to the kinds of analyses undertaken here.

If one graphs the yearly offense rates of some sample defined by a court appearance or conviction, the graph is likely to take the following form. Initially, offending rates will climb gradually. In the periods immediately prior to the sentence (and the arrest that defines entry into the sample), there is an abrupt peak in arrest rates. After the intervention of the criminal justice system, rates of offending are relatively low once again (see Murray and Cox, 1979; Figure 1.1, page 39).[22] The drop observed in such a graph is the suppression effect and its existence is what has been criticized. These criticisms can be grouped into three general areas: the steep rise prior to the intervention is an artifact, the sharp drop is not due to the intervention or sentence, and age or maturation accounts for the decline in arrest rates after intervention.

An artificial rise in rates of offending just prior to the intervention is cause for concern. It may be due to one or more of several different sources, all of which fall under the general rubric of "selection effects" as

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[22] In an early analysis of robbers in the Sentencing data file (Smith and Smith, 1986) we found aggregate patterns of charge rates that looked remarkably similar to the Murray and Cox's graph for juvenile delinquents.

applied to the problems of internal validity (Campbell and Stanley, 1966). First, any sample of offenders appearing in court or convicted in a court must, by definition, have at least one arrest. Including the arrest (or arrests) that led to entry into the sample will produce a rise in offending just prior to the intervention or sentence, and this can make it easier to observe a "suppression" of criminal behavior subsequent to the intervention.[23]

A second concern is what can be called the "multiple-arrest" argument. The contention is that the frequency of arrest in the months immediately prior to intervention influences the probability for inclusion into a conviction sample as judges may be more likely to convict those with recent multiple arrests. Thus, high rate offenders are more likely to be convicted and sentenced than low rate offenders. These high rate offenders "accumulate" offenses, which brings about the artificially high peak in arrest rates just prior to sentencing. This is clearly a "selection bias" argument as the contention is that the sample is determined by winnowing out those individuals with relatively low rates of offending. What makes this form of selection particularly troublesome is that samples selected on the bases of "extreme" values (i.e., high rates of offending) are more subject to regression to the mean effects (see below) and therefore can be expected to show a greater drop in offense rates regardless of the impact of the treatment.

A third methodological caution raised about suppression effects revolves around how individuals are assigned to treatments (e.g., probation, community-based placements, institutionalization). Clearly, offenders are sentenced to particular sanctions on the basis of systematic considerations. Judges assign

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[23] In all of our analyses, prior record measures are defined by using only those arrests and convictions that occurred before the arrest that led to entry into the sample.

sanctions on the basis of many factors (see any model of the sentencing process) and this results in treatment groups that are not identical in terms of the characteristics that are likely to be predictive of recidivism. Consequently, probationers tend to be relatively low rate offenders with short prior records, while those sentenced to prison tend to have much higher pre-sentence offense rates. To find the greatest suppression effects among those sentenced to the harshest treatments may simply be reflecting the differential composition of the treatment groups. For example, those with the highest prior offending rates (the artificial pre-sentence peak) may be most subject to regression to the mean effects.

This methodological caution is what is commonly referred to as "selection bias" in non-experimental designs (Campbell and Stanley, 1966). If the treatment groups differ significantly, it becomes extremely difficult to identify the exact influence of the treatment. This poses a problem for our interest in how the various sanctions might influence recidivism. As we have demonstrated in Chapter Three, the groups defined by the various sentences are composed of quite different mixes of offenders. These pre-existing differences might be producing the "effect" of the treatment and not the sanction itself.

Note that while this is called a "selection bias," it is substantively different from the selection biases discussed earlier in this appendix. The selection biases that can, to some extent, be addressed by a Heckman correction are biases in how the sample is defined and the systematic processes that lead to the formation of the sample. These kinds of biases occur <u>prior</u> to sampling. The selection bias that hinders our ability to determine the effect of sanctions on recidivism occurs <u>within</u> the sample that

has been defined. At this point nonequivalent treatment groups are created. Simply controlling for risk of entry into the sample does not alleviate the problem.[24]

The fourth methodological criticism that has been raised surrounding the analysis of suppression effects is that the sharp rise in offense rates just prior to the intervention is merely chance variation. In essence, some offenders are "unlucky" and are more frequently arrested during the preintervention period. As Maltz and Pollock (1980) have shown, the sharp rise in offending prior to the administration of the sanction can be modeled as the result of a stochastic process with offenders shifting between active and quiescent states. A spurt of activity leads to detection by the authorities (an arrest) and some form of intervention by the criminal justice system. The drop in offense rates subsequent to the treatment may then reflect either a return to a quiescent state or a return in "luck" and avoiding detection by the authorities.

Two other threats to our ability to ascertain the effects of sentences on recidivism deserve mention. While neither is strictly a selection effect, both can potentially interact with design selection biases to hamper the internal validity of our analyses. There is concern that the drop in the arrest rates after intervention (the suppression effect) is due to a regression to the mean. That is, if the rate of offending during the period

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[24] It is possible to conceptualize the differential mix of offenders across treatments in a way that is amenable to a Heckman-like correction. If selection equations modeling the probability that an offender was sentenced to a particular sanction were developed, it would be possible to compute hazard rates for the likelihood that an offender received a given type of sanction. The estimated hazards could then be used as statistical controls to lessen the impact of selection into the treatment groups. However, this raises the issue of using even more hazard instruments in the modeling of recidivism and is beyond the scope of the present analysis.

shortly before intervention is "unusually" high, it would be expected to drop, or "regress to the mean" regardless of why there is a peak before the intervention. Campbell and Ross (1968), for example, describe this danger for stochastic processes, such as yearly traffic fatalities in a geographic area over time.

Switching between active and quiescent offending patterns is one stochastic mechanism that could produce both a sharp rise in offending prior to the intervention and a drop subsequent to treatment. If periods of criminal activity get offenders into a conviction sample, but they return to inactive states after the treatment, a regression to the mean has occurred, independent of the exact nature of the treatment. The contention is that "they would have quit anyway" and consequently any measured effect of the treatment is an artifact.

Another, somewhat different, process can produce a regression to the mean effect as well. If arrests occur in a stochastic manner, then it can be demonstrated that the rate of arrest can accumulate over time as a type of "random walk." If intervention occurs at a point in time when arrest rates happen to be high, then the treatment would coincide with what is really just a randomly high level of criminal behavior. The level of arrest would then, stochastically drop to lower levels and what would appear to be the effect of the treatment (a suppression) would really just be the stochastic process returning to a mean level. Note that a selection bias could interact with the threat of regression to the mean to produce relatively large suppression effects that are not real. Those given the most severe sanctions, in part due to the highest preintervention arrest rates, may be most likely to have offending randomly drop to lower levels.

The final threat to the internal validity of any model of recidivism rates pertains to age (maturation, in the experimental design literature). Graphs of post-intervention arrest rates gradually taper off. While it is tempting to attribute this slower drop to the influence of the criminal justice sanction, offender age is potentially responsible for such a pattern. It is well-known that criminal activity varies by age (e.g., Wolfgang et al., 1972; Hirschi and Gottfredson, 1983) with participation and rate of offending declining after age 16 or 17. Indeed, it has been claimed that age alone can account for the behavior of chronic offenders (Gottfredson and Hirschi, 1986). Therefore, the detection of any suppression effect is confounded by age effects. Moreover, the differential age composition of treatment groups, which is quite evident in the different sentences administered in our sample, raises the potential for a selection-maturation interaction that furthers complicates our ability to identify the effects of sanctions on recidivism. It could well be that offenders in some sanction groups are more likely to show a decline in offense rates because of maturation than are offenders in other sanction groups.

SUMMARY

The issues raised in this Appendix have sobering consequences for any analysis of recidivism. Studies of the criminal justice system require nonrandom samples. The sentencing process itself nonrandomly allocates offenders into sanctions. This creates a myriad of methodological and statistical problems that make the study of recidivism difficult. The effect of seemingly straightforward variables such as prior record or presenting offense may be confounded by how these variables influence entry into the

sample. What effect, if any, a prison sentence has on the recidivism rates of offenders, is not independent of the kinds of offenders sent to prison and the fact that these individuals are quite different from those given other sanctions.

In the end, what makes any findings plausible is how these threats to internal validity are addressed. The analysis presented in this Appendix is a first step in determining the extent to which selection biases in the definition of the sample may influence our understanding of the correlates of recidivism. By examining results both with and without controls for prior selection, we can gain insight into how substantive conclusions are influenced by one form of sample selection bias.

The fact that the groups defined by particular sanctions are nonequivalent further complicates the determination of the effects of those sanctions. A careful investigation of how these groups differ and statistically controlling for the existing initial differences, provides some assurance that any observed impact of sanctions on recidivism is "real." In the end, however, an attempt to determine "causality" using nonexperimental data with known selection biases is at best instructive. Caution must be urged in interpreting the results.

Figure A.1

Form of the Selection Process

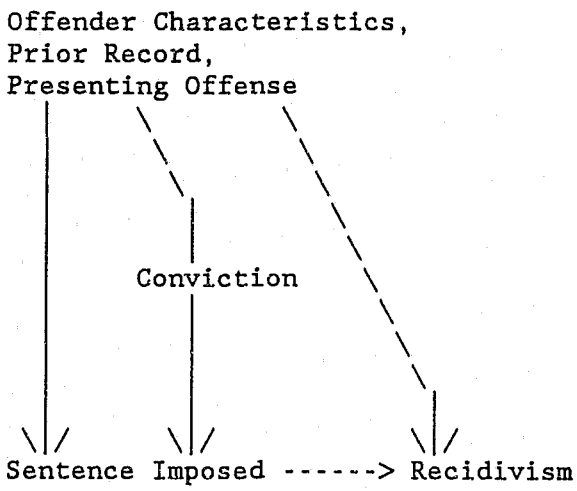

Figure A.2

Extent of Filtering in New Jersey - 1975 & 1976
(N in parentheses)

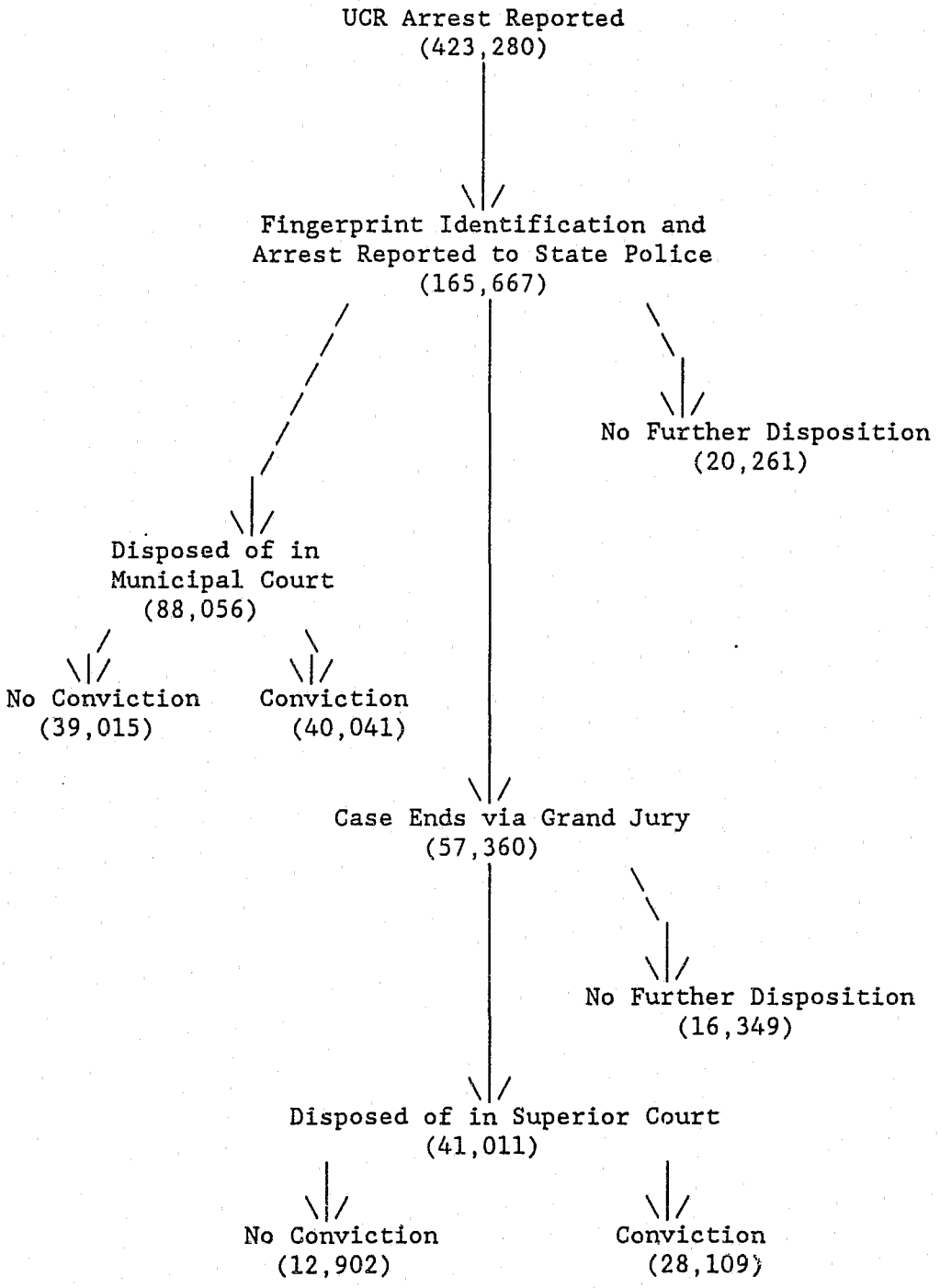

Figure A.3

An Example of "Incidental" Selection

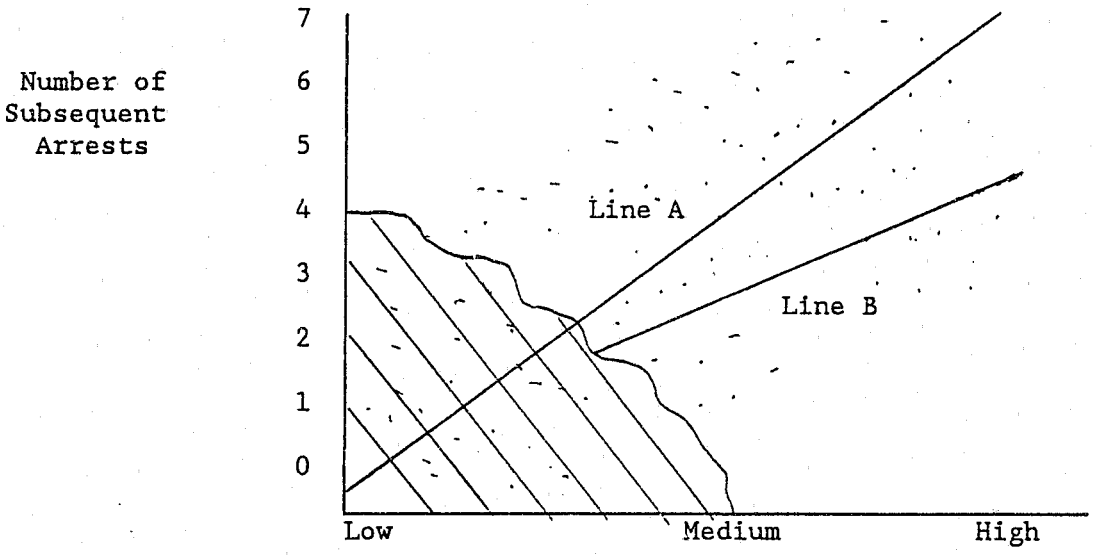

Table A.1

Correlates of Selection in the Criminal Justice System

Selection Point	Correlate	Sources
Offenses Reported to Police	Type of Offense; Victim fear; Powerlessness; Advice of peers;	Sourcebook, 1987; Kidd and Chayet, 1984; Ruback et al., 1984;
Decision to Arrest	Legal Seriousness of Behavior; Complainant Preference; Disrespect for officers; Sex of offender; Race of offender; Age of offender; Antagonistic suspect; Police supervisor present; Violent offense; Property offense; Poverty of neighborhood;	Black and Reiss, 1967; Black, 1971; Friedrich, 1977; Sykes et al., 1976; Lundman, 1974; Smith, 1984;
Arrest Reported to Repository	Jurisdiction; Offense;	Cohen, 1986;
Decision to Prosecute	Degree of evidence; Prior relationship between victim and offender: Prosecutor's office/ jurisdiction; Alcohol abuse; Victim provoked incident;	Forst et al.,1977; Vera 1977; Williams, 1978; Greenwood et al., 1973; Forst and Brosi, 1977; Brosi, 1979;
Decision to Convict	Type of charge; Degree of evidence; Jurisdiction; Prior record; Type of trial; Type of counsel;	Bernstein et al., 1977; Heumann, 1975; Friedman, 1979; Figueira-McDonough, 1985;

Table A.1 (continued)

Correlates of Selection in the Criminal Justice System

Selection Point	Correlate	Sources
Decision to Incarcerate	Presenting Offense; Number of indictments; Recommendation to court; Type of victim; Social Class; Age; Age at onset; Employment; Marital status; Alcohol or drug use; Dollar loss; Role in offense; Sex; Urbanization of area; Unemployment rate of area; Percent black in area;	Pope, 1975, 1976, 1978; Meyers and Talerico, 1986a; Sutton, 1978; Tiffany et al., 1975; Green, 1961; Lizotte, 1978; Roberts, 1983; Petersilia, 1983; Unnever and Hembroff, 1988; Bridges et al., 1987; Wheeler et al., 1982;
Decision Re: Length of Sentence	Conviction offense; Dollar loss; Social Class; Prior arrests; Most serious prior conviction; Region; Prior incarcerations; Sex; Age; Urbanization of area; Racial income inequality of area; Percent black of area; Caseload;	Sutton, 1978; Wheeler et al., 1982; Lizotte, 1978; Petersilia, 1983; Meyers and Talerico, 1986a;

Table A.1 (continued)

Correlates of Selection in the Criminal Justice System

Selection Point	Correlate	Sources
Recidivism	Prior convictions; Prior incarcerations; Age at first conviction; Presenting offense of auto theft; Parole revocation; Drug history; Education; Employment or full-time school Live with spouse/children Nondisreputable associates Property crime presenting offense Not a burglary or checks offense Not using aliases	Greenwood, 1982; Hoffman and Beck, 1976; Fisher, 1981; Chi, 1983; Rhodes et al., 1982; Babst and Mannering, 1965: Kusuda, 1966; Petersilia et al., 1985; Gottfredson and Gottfredson, 1988;

Table A.2

Crime Classifications and Mappings Across Data Sources

NJ UCR Reports		Major Uniform Offense Code Groupings		Mapping Used		
Rank	Offense	Group	Offense	Rank	UCR	Uniform
1.	Murder	A.	Homicide	1.	3	C
2.	Manslaughter	B.	Kidnapping	2.	4	D
3.	Forcible Rape	C.	Sexual Assault	3.	1,2,5	A
4.	Robbery	D.	Robbery	4.	6	H
5.	Atrocious Assault	E.	Assault	5.	7,8	I,J
6.	Breaking and Entering	F.	Arson	6.	9	E
7.	Larceny	G.	Extortion	7.	10	F
8.	Motor Vehicle Theft	H.	Burglary	8.	11,12	K,L
9.	Assault	I.	Larceny	9.	13	M
10.	Arson	J.	Stolen Vehicle	10.	14	N
11.	Forgery	K.	Forgery	11.	15	O
12.	Fraud	L.	Fraud	12.	16	U
13.	Embezzlement	M.	Embezzlement	13.	17	Q*
14.	Stolen Property	N.	Stolen Property	14.	18	Q*
15.	Malicious Mischief	O.	Damage Property	15.	19	P
16.	Weapons	P.	Drugs	16.	20	R
17.	Prostitution or Vice	Q.	Sex Offenses	17.	21	V*
18.	Other Sex Offenses	R.	Gambling	18.	23	V*
19.	Drug Violations	S.	Escape	19.	25	V*
20.	Gambling	T.	Bribery	20.	26	V*
21.	Family and Child Support	U.	Weapons	21.	22,24	B,G
22.	Driving While Intoxicated	V.	All Other		27,28	S,T
23.	Liquor Law Violations				29	V*
24.	Drunk					
25.	Disorderly Conduct					
26.	Failure to Give Good Account					
27.	All other (less traffic)					
28.	Curfew Violations					
29.	Runaway					

* Only selected four digit codes within Uniform Offense categories were used

Table A.3

Probit Model for an Arrest Proceeding from Uniform Crime Report to SAC Arrest History

Independent Variable	Coefficient	Standard Error	t-Statistic
OFFENSE			
Forcible Rape	-4.728	3.449	-1.371
Robbery	-4.936	3.449	-1.431
Serious Injury	-6.024	3.449	-1.747
Breaking and Entering	-4.698	3.449	-1.362
Larceny - MV Theft	-5.810	3.449	-1.685
Assault	-6.718	3.449	-1.948
Arson	-5.122	3.449	-1.485
Forgery - Fraud	-5.592	3.449	-1.622
Embezzlement	-4.750	3.449	-1.377
Stolen Property	-5.858	3.449	-1.670
Malicious Mischief	-6.738	3.449	-1.954
Weapons	-5.337	3.449	-1.547
Prostitution/Vice	-6.214	3.449	-1.802
Sex Offenses	-5.105	3.449	-1.480
Drug Violations	-4.850	3.449	-1.406
Gambling	-6.027	3.449	-1.748
Family/Child Offenses	-6.170	3.449	-1.789
Liquor Law Violations	-7.612	3.449	-2.208
Disorderly Conduct	-7.409	3.449	-2.148
Failure to Give Good Account	-9.118	3.452	-2.641
All Other Offenses	-7.361	3.449	-2.134
REGIONAL VARIABLES			
Northwest Region	-1.120	.204	-5.476
Northeast Region	1.502	.445	3.376
Southwest Region	.492	.107	4.619
Crime Rate per 100,000	.0005	.0002	2.539
Percent Crimes Cleared	.463	.133	3.477
Violent Crime Rate	-.001	.0003	-3.887
Police Employees per 1,000	.033	.028	1.168
OFFENDER CHARACTERISTICS			
Black	-.166	.013	-12.587
Female	-.343	.017	-19.877
INTERACTIONS			
Black by Female	.509	.032	15.734
Northwest by Female	-.046	.047	-.997
Northeast by Female	.063	.021	3.015
Southwest by Female	-.104	.025	-4.085
Northwest by Black	.543	.069	7.868
Northeast by Black	.343	.015	22.757
Southwest by Black	.131	.018	7.097
Northwest by Black by Female	.800	.239	3.352
Northeast by Black by Female	-.063	.037	-1.680
Southwest by Black by Female	.124	.045	2.757

NOTE: The constant is incorporated into the estimate of offense effects.

Table A.4

Probit Models of the Selection Process
(N in Parentheses)

Independent Variable	Case Goes Past Arrest (41,204)[1] Without Prior Hazards	Case Goes Past Arrest (41,204)[1] With Prior Hazards	Case Goes to Grand Jury (76,679)[2] Without Prior Hazards	Case Goes to Grand Jury (76,679)[2] With Prior Hazards
Constant	1.272	1.234	-1.009	-.633
NUMBER OF CHARGES				
Homicide	.161(ns)	.186	1.759	1.666
Sexual Assault			1.049	1.089
Robbery	.214	.236	1.177	1.042
Assault	.030(ns)	.050	.109	.150
Burglary	.072	.056	.838	.712
Larceny	.195	.213	-.394	-.517
Stolen Vechicle	.111	.128	.403	.360
Forgery			.777	.794
Fraud	-.062	-.050	.145	.295
Stolen Property	.139	.156	.094	.017(ns)
Damage Property	.120	.136	-.339	-.401
Drugs	.265	.239	.152	-.144
Sex Offenses			.376	.350
Gamble			.791	.686
Escape	-.216	-.184	.505	.898
Weapons	.188	.193	.731	.566
COUNTY				
Atlantic				
Bergen			.114	.326
Burlington	.206	.212	-.342	-.370
Camden	.140	.142	.112	.142
Cape May	.551	.544	-.229	-.525
Cumberland	.416	.416		
Essex	-.304	-.302	-.053	.638
Gloucester	.419	.424	-.020(ns)	-.250
Hudson			-.375	-.188
Hunterdon	.230	.225		
Mercer			.344	.547
Middlesex	.070	.073	.113	.229
Monmouth	.231	.229	.070	-.082(ns)
Morris	.128	.128	.122	.176
Ocean	.106	.110	-.279	-.197
Passaic	.115	.113	.156	.219
Salem	.636	.632	.351	-.057(ns)
Somerset	.182	.184		
Sussex			.357	.517
Union			-.194	.018(ns)
Warren				

[1] 25% random sample [2] 50% random sample

(continued)

Table A.4 (continued)

Probit Models of the Selection Process
(N in Parentheses)

Independent Variable	Case Goes Past Arrest (41,204)[1] Without Prior Hazards	Case Goes Past Arrest (41,204)[1] With Prior Hazards	Case Goes to Grand Jury (76,679)[2] Without Prior Hazards	Case Goes to Grand Jury (76,679)[2] With Prior Hazards
PRIOR RECORD				
Uses Alias			.094	.090
# Prison Terms			.037	.035
# Jail Terms			-.083	-.079
# Probation Terms				
# Arrests			-.015	-.014
# Charges			.023	.021
# Convictions				
# Weapons Charges	-.044	-.043	.042	.103
# Drug Charges				
OFFENDER				
Black	-.094	-.090	.188	.310
Female	-.023(ns)	-.022(ns)	.005(ns)	.042
Age	-.0001	-.009	.011	.025
HAZARDS				
UCR to SAC	na	.050	na	.122
Past Arrest			na	-4.136
To Grand Jury				
To Superior Court				
SUMMARY STATISTICS				
Mean of Dep. Var.	.878	.878	.394	.394
Likelihood Ratio	1687.4	1698.8	14763.9	15835.9
Degrees of Freedom	30	31	41	43
Significance	<.001	<.001	<.001	<.001
Psuedo R-squared	.039	.040	.169	.179

[1] 25% random sample [2] 50% random sample

(continued)

Table A.4 (continued)

Probit Models of the Selection Process
(N in Parentheses)

Independent Variable	Case Goes To Superior Court (57,339)		Conviction in Superior Court (40,995)	
	Without Prior Hazards	With Prior Hazards	Without Prior Hazards	With Prior Hazards
Constant	.574	1.462	.022(ns)	-.054(ns)
NUMBER OF CHARGES				
Homicide	.197	-.048(ns)	.718	.745
Sexual Assault	-.183	-.196		
Robbery	.190	-.077	.563	.580
Assault	-.282	-.213	.330	.324
Burglary	.175	-.029(ns)	.500	.481
Larceny	-.138	-.196	.693	.685
Stolen Vechicle	-.085	-.161		
Forgery			.545	.546
Fraud	-.082	.092	.530	.600
Stolen Property	-.194	-.277	.376	.361
Damage Property	-.217	-.199	.607	.607
Drugs	.075	-.207	.102	-.040
Sex Offenses			.608	.579
Gamble	.088	.022(ns)	.702	.624
Escape	-.567	-.171	.689	.721
Weapons	.041	-.206	.481	.460
COUNTY				
Atlantic	.102	.213		
Bergen	-.134	-.023(ns)		
Burlington	.242	.156	.156	.191
Camden	-.149	-.213		
Cape May	.055(ns)	-.353	.285	.232
Cumberland	-.078	-.462	.391	.321
Essex	.173	.934	-.245	-.072(ns)
Gloucester	-.087	-.457	-.551	-.621
Hudson	-.055	.120	-.194	-.166
Hunterdon			-.515	-.539
Mercer	-.225	-.114	.133	.126
Middlesex			-.142	-.129
Monmouth	.332	.146	.202	.228
Morris	-.503	-.578	-.113	-.213
Ocean			.233	.256
Passaic	-.104	-.135		
Salem	.159	-.452	.184	.105(ns)
Somerset			-.283	-.229
Sussex	-.289	-.262	-.228	-.283
Union	.102	.272	.227	.283
Warren	.294	.442	-.444	-.366

(continued)

Table A.4 (continued)

Probit Models of the Selection Process
(N in Parentheses)

Independent Variable	Case Goes To Superior Court (57,339)		Conviction in Superior Court (40,995)	
	Without Prior Hazards	With Prior Hazards	Without Prior Hazards	With Prior Hazards
PRIOR RECORD				
Uses Alias	.092	.069	.141	.152
# Prison Terms	.085	.075		
# Jail Terms	.015(ns)	.031		
# Probation Terms			.055	.053
# Arrests				
# Charges				
# Convictions			.018	.020
# Weapons Charges			-.033	-.021(ns)
# Drug Charges	.019	.001	.040	.039
OFFENDER				
Black	.123	.227	-.012(ns)	.027(ns)
Female	-.054	.012(ns)	-.168	-.169
Age	-.004	.000	-.003	-.000(ns)
HAZARDS				
UCR to SAC	na	.068	na	.075
Past Arrest	na	-4.870	na	-.879
To Grand Jury	na	-.286	na	-.052(ns)
To Superior Court			na	.326(ns)
SUMMARY STATISTICS				
Mean of Dep. Var.	.715	.715	.685	.685
Likelihood Ratio	3132.6	4736.6	5086.0	5168.2
Degrees of Freedom	38	41	39	43
Significance	<.001	<.001	<.001	<.001
Psuedo R-squared	.052	.076	.110	.112

Table A.5

Correlations Between Hazard Indicators
(N of Cases in Parentheses)[1]

	UCR to SAC	Past Arrest	To Grand Jury	To Superior Court	Superior Court Conviction
UCR to SAC	-	-.298 (145,361)	.029 (57,339)	-.158 (40,995)	.095 (28,089)
Past Arrest	.305 (12,321)	-	-.049 (57,339)	.027 (40,995)	.102 (28,089)
To Grand Jury	.030 (12,321)	-.090 (12,321)	-	.232 (40,995)	.414 (28,089)
To Superior Court	.203 (12,321)	.100 (12,321)	.243 (12,321)	-	.010 (28,089)
Superior Court Conviction	.025 (12,321)	.126 (12,321)	.491 (12,321)	.105 (12,321)	-

[1] Correlations using largest remaining case base in SAC data above the diagonal; correlations using measures computed in the Sentencing data base below the diagonal.

APPENDIX B

SAMPLE SELCTION BIAS ANALYSES

This appendix consists solely of tables supplementing results presented in Chapters Six and Eight. These tables incorporate the six hazard variables that were described in the previous appendix. Thus, the only difference between the tables here and those of Chapters Six and Eight are controls for sample selection bias.

The numbering of these tables mirrors that used for the companion chapters. For example, Table B6.1 is identical to Table 6.1 from Chapter Six, save for the inclusion of the hazard variables. Table B8.15 is the companion to Table 8.15 of Chapter Eight. Note that as a consequence of this parallelism, the tables of this appendix are not consecutively numbered.

No interpretations of results are offered in this appendix. Our conclusions concerning the contents of these tables are to be found in the associated chapters.

Table B6.1
Attribution of Explained Variance for Probability of Rearrest
- Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.174)	(.245)	(.261)	(.257)
Percent R2 Unattributed	33.99	33.79	33.40	32.59
	(.059)	(.083)	(.087)	(.084)
Attributed to Hazards	25.02	26.20	26.79	24.77
	(.044)	(.064)	(.070)	(.064)
Social Variables	5.04	6.79	7.93	9.60
	(.009)	(.017)	(.021)	(.025)
Presenting Offense	3.08	2.52	2.09	1.96
	(.005)	(.006)	(.005)	(.005)
Anamnestic Variables	8.23	8.51	7.79	8.71
	(.014)	(.021)	(.020)	(.022)
Delinquent Career/Onset	3.54	3.12	2.88	2.35
	(.006)	(.008)	(.008)	(.006)
Prior CJS/Offender Actions	.64	.24	.33	.69
	(.001)	(.001)	(.001)	(.002)
General Controls	4.19	5.16	5.51	6.29
	(.007)	(.013)	(.014)	(.016)
Social/Presenting Offense	.00*	.00*	.04	.03
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.87	.90	.94	1.07
	(.002)	(.002)	(.002)	(.003)
Social/Delinquent Career	1.39	1.53	1.80	1.88
	(.002)	(.004)	(.005)	(.005)
Social/Prior CJS Actions	.17	.02	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.71	1.40	1.18	1.29
	(.003)	(.003)	(.003)	(.003)
Presenting Offense/Delinquency	.00*	.00*	.01	.01
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.16	.33	.30	.37
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.10	.34	.38	.41
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	2.49	1.51	1.26	1.18
	(.004)	(.004)	(.003)	(.003)
Anamnestic/Prior CJS Actions	3.97	1.74	1.04	.07
	(.007)	(.004)	(.003)	(.000)
Anamnestic/General Controls	3.44	3.09	2.48	2.30
	(.006)	(.008)	(.006)	(.006)
Delinquency/Prior CJS Actions	.74	.15	.00*	.00*
	(.001)	(.000)	(.000)	(.000)
Delinquency/General Controls	1.90	3.07	4.52	5.78
	(.003)	(.008)	(.012)	(.015)
Prior CJS Actions/Controls	.11	.18	.18	.24
	(.000)	(.000)	(.000)	(.001)
All Interactions	4.04	2.63	2.40	2.41
	(.007)	(.006)	(.006)	(.006)

* Partitioned variance is negative due to suppressor effect

Table B6.2
Attribution of Explained Variance for Probability of Rearrest for Persons Crime - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.084)	(.133)	(.158)	(.179)
Percent R2 Unattributed	31.49	32.94	33.74	34.12
	(.026)	(.044)	(.053)	(.061)
Attributed to Hazards	19.49	20.44	20.65	20.65
	(.016)	(.027)	(.033)	(.037)
Social Variables	6.14	7.46	7.98	8.66
	(.005)	(.010)	(.013)	(.015)
Presenting Offense	1.28	1.70	2.02	1.59
	(.001)	(.002)	(.003)	(.003)
Anamnestic Variables	17.68	13.87	11.63	10.79
	(.015)	(.018)	(.018)	(.019)
Delinquent Career/Onset	5.56	5.07	4.89	4.77
	(.005)	(.007)	(.008)	(.009)
Prior CJS/Offender Actions	1.28	.65	.50	.58
	(.001)	(.001)	(.001)	(.001)
General Controls	1.60	3.34	3.97	4.57
	(.001)	(.004)	(.006)	(.008)
Social/Presenting Offense	.62	.84	1.13	1.01
	(.001)	(.001)	(.002)	(.002)
Social/Anamnestic Variables	1.33	1.32	1.56	1.34
	(.001)	(.002)	(.002)	(.002)
Social/Delinquent Career	1.75	1.98	2.31	2.41
	(.001)	(.003)	(.004)	(.004)
Social/Prior CJS Actions	.24	.14	.12	.09
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.62	.48	.60	.47
	(.001)	(.001)	(.001)	(.001)
Presenting Offense/Delinquency	.07	.23	.31	.29
	(.000)	(.000)	(.000)	(.001)
Presenting Offense/Prior CJS	.00*	.00*	.00*	.10
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.08	.25	.39	.40
	(.000)	(.000)	(.001)	(.001)
Anamnestic/Delinquent Career	4.32	2.95	2.56	2.31
	(.004)	(.004)	(.004)	(.004)
Anamnestic/Prior CJS Actions	2.36	2.16	1.55	1.32
	(.002)	(.003)	(.002)	(.002)
Anamnestic/General Controls	1.69	2.18	1.84	1.88
	(.001)	(.003)	(.003)	(.003)
Delinquency/Prior CJS Actions	1.26	.74	.60	.39
	(.001)	(.001)	(.001)	(.001)
Delinquency/General Controls	1.53	1.83	2.48	3.32
	(.001)	(.002)	(.004)	(.006)
Prior CJS Actions/Controls	.00*	.04	.08	.14
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.12	4.81	3.87	3.97
	(.004)	(.006)	(.006)	(.007)

* Partitioned variance is negative due to suppressor effect

Table B6.3
Attribution of Explained Variance for Probability
of Repeating Presenting Offense - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.067)	(.103)	(.130)	(.143)
Percent R2 Unattributed	34.56	33.84	32.31	31.79
	(.023)	(.035)	(.042)	(.046)
Attributed to Hazards	22.69	21.78	22.92	19.86
	(.015)	(.022)	(.030)	(.028)
Social Variables	7.52	8.14	8.64	9.59
	(.005)	(.008)	(.011)	(.014)
Presenting Offense	7.49	7.57	8.18	10.69
	(.005)	(.008)	(.011)	(.015)
Anamnestic Variables	8.10	9.99	10.22	9.64
	(.005)	(.010)	(.013)	(.014)
Delinquent Career/Onset	2.34	3.42	2.63	2.57
	(.002)	(.004)	(.003)	(.004)
Prior CJS/Offender Actions	1.83	.71	.67	.88
	(.001)	(.001)	(.001)	(.001)
General Controls	2.28	2.31	3.09	3.44
	(.002)	(.002)	(.004)	(.005)
Social/Presenting Offense	.00*	.00*	.00*	.08
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.96	1.33	1.29	1.13
	(.001)	(.001)	(.002)	(.002)
Social/Delinquent Career	.81	1.02	.96	1.11
	(.001)	(.001)	(.001)	(.002)
Social/Prior CJS Actions	.24	.09	.05	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.33	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.87	2.21	2.40	2.74
	(.001)	(.002)	(.003)	(.004)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.22	.25	.55
	(.000)	(.000)	(.000)	(.001)
Presenting Offense/Controls	.27	.44	.47	.59
	(.000)	(.000)	(.001)	(.001)
Anamnestic/Delinquent Career	3.13	2.49	1.85	1.53
	(.002)	(.003)	(.002)	(.002)
Anamnestic/Prior CJS Actions	3.55	1.58	1.77	.98
	(.002)	(.002)	(.002)	(.001)
Anamnestic/General Controls	2.67	2.56	2.67	2.60
	(.002)	(.003)	(.003)	(.004)
Delinquency/Prior CJS Actions	.57	.20	.06	.00*
	(.000)	(.000)	(.000)	(.000)
Delinquency/General Controls	.42	.35	.39	.75
	(.000)	(.000)	(.001)	(.001)
Prior CJS Actions/Controls	.00*	.09	.05	.10
	(.000)	(.000)	(.000)	(.000)
All Interactions	13.09	9.01	8.19	6.94
	(.009)	(.009)	(.011)	(.010)

* Partitioned variance is negative due to suppressor effect

Table B6.4
Attribution of Explained Variance for Probability of Reimprisonment Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.120)	(.242)	(.282)	(.292)
Percent R2 Unattributed	30.14	32.99	32.54	32.59
	(.036)	(.080)	(.092)	(.095)
Attributed to Hazards	25.55	28.39	30.77	31.76
	(.031)	(.069)	(.087)	(.093)
Social Variables	3.34	3.38	3.80	4.86
	(.004)	(.008)	(.011)	(.014)
Presenting Offense	1.08	1.31	1.17	1.47
	(.001)	(.003)	(.003)	(.004)
Anamnestic Variables	9.29	6.37	6.35	5.39
	(.011)	(.015)	(.018)	(.016)
Delinquent Career/Onset	6.59	6.68	7.14	7.31
	(.008)	(.016)	(.020)	(.021)
Prior CJS/Offender Actions	5.04	2.07	.77	.35
	(.006)	(.005)	(.002)	(.001)
General Controls	.35	1.19	1.42	2.01
	(.000)	(.003)	(.004)	(.006)
Social/Presenting Offense	.11	.28	.32	.18
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	.61	.61	.63	.58
	(.001)	(.001)	(.002)	(.002)
Social/Delinquent Career	.61	1.19	1.34	1.58
	(.001)	(.003)	(.004)	(.005)
Social/Prior CJS Actions	.61	.40	.26	.21
	(.001)	(.001)	(.001)	(.001)
Social/General Controls	.37	.59	.58	.44
	(.000)	(.001)	(.002)	(.001)
Presenting Offense/Anamnestic	.97	.84	.69	.77
	(.001)	(.002)	(.002)	(.002)
Presenting Offense/Delinquency	.06	.23	.27	.15
	(.000)	(.001)	(.001)	(.000)
Presenting Offense/Prior CJS	.00*	.07	.03	.03
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.05	.13	.18	.22
	(.000)	(.000)	(.001)	(.001)
Anamnestic/Delinquent Career	5.01	3.99	3.45	3.02
	(.006)	(.010)	(.010)	(.009)
Anamnestic/Prior CJS Actions	5.35	4.67	4.07	2.77
	(.006)	(.011)	(.011)	(.008)
Anamnestic/General Controls	2.49	2.75	2.64	2.60
	(.003)	(.007)	(.007)	(.008)
Delinquency/Prior CJS Actions	2.25	1.75	1.35	1.03
	(.003)	(.004)	(.004)	(.003)
Delinquency/General Controls	.13	.11	.20	.64
	(.000)	(.000)	(.001)	(.002)
Prior CJS Actions/Controls	.02	.01	.02	.02
	(.000)	(.000)	(.000)	(.000)
All Interactions	5.01	4.13	3.47	3.39
	(.006)	(.010)	(.010)	(.010)

* Partitioned variance is negative due to suppressor effect

Table B6.5

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	.0175	.0258*	.0309*	.0273*	.0023	.0094	.0180	.0126
Has job after sentence	-.0362***	-.0241*	-.0218	-.0186	-.0170**	-.0115	-.0163	-.0206*
Offender is on welfare	-.0213*	.0103	.0114	.0158	.0042	.0125	.0150	.0167
Offender is Black	.0701***	.1385***	.1507***	.1687***	.0463***	.1014***	.1081***	.1423***
Offender is Hispanic	.0681***	.1310***	.1447***	.1504***	.0411**	.1006***	.1136***	.1414***
Offender is female	-.0539**	-.0678**	-.0738**	-.0942***	-.0434***	-.0834***	-.1108***	-.1244***
Lives in urban area	.0137	.0322**	.0376**	.0323*	.0172**	.0251**	.0297**	.0278**
Years at current address	-.0013**	-.0014*	-.0014*	-.0014*	-.0004	-.0005	-.0005	-.0002
History of drug problems	.0031	.0241	.0207	.0263	.0047	-.0060	-.0050	-.0126
Treated for drugs/alch.	-.0131	-.0013	.0092	.0269	-.0119	-.0128	-.0085	-.0122
Has needle marks	-.0025	.0236	.0686**	.0766**	-.0074	-.0198	-.0194	-.0126
Not a school drop out	-.0326***	-.0484***	-.0514***	-.0540***	-.0019	-.0106	-.0272**	-.0271**
Doesn't live with family	-.0041	-.0145	-.0532***	-.0418**	-.0065	.0016	-.0059	-.0107
Commited PO with group	-.0186*	-.0350**	-.0370**	-.0376**	-.0058	.0001	-.0052	-.0082
Victim was a stranger	.0267*	.0422**	.0286*	.0347*	.0039	.0011	.0062	.0071
Presenting Offense								
PO property crime	.0082	.0271	.0370*	.0425*	-.0049	-.0211	-.0237	-.0119
PO crime against person	-.0321*	.0132	.0279	.0428*	.0245*	.0534***	.0716***	.0746***
PO drug offense	.0015	.0163	.0162	.0243	-.0010	-.0181	-.0289	-.0341
PO Wolfgang severity	-.0018**	-.0028***	-.0022**	-.0026**	-.0004	-.0009	-.0012*	-.0009
Has detainers at arrest	-.0214	-.0144	-.0168	-.0222	.0013	-.0118	-.0120	-.0080
Has pending charges	.0573***	.0867***	.0914***	.0771***	.0129*	.0161	.0271*	.0289*
On probation at PO	.0548***	.0701***	.0656***	.0698***	.0024	.0192	.0307*	.0440**
Anamnestic Theory								
N prior adult arrests	.0133***	.0288***	.0356***	.0364***	.0078***	.0141***	.0169***	.0184***
N prior adult conviction	.0019	.0005	.0044	.0057	-.0037*	-.0021	-.0024	-.0010
N prior adult chg. conv.	-.0079**	-.0131***	-.0158***	-.0154***	-.0019	-.0031	-.0064*	-.0075*
N charges past 5 years	.0064***	.0084***	.0089***	.0118***	.0019*	.0026*	.0024	.0036*
N prior Part 1 charges	.0095*	.0072	.0073	.0073	.0025	-.0039	.0004	-.0001
N prior property conv.	-.0006	.0006	-.0025	-.0022	-.0036*	-.0019	-.0023	-.0009
N prior persons conv.	.0022	.0046	.0102	.0055	.0083***	.0173***	.0233***	.0252***
N prior weapons conv.	.0298*	.0263	.0319	.0525**	.0198**	.0210*	.0374***	.0532***
Off street last 2 years	.0478***	.0965***	.1032***	.1004***	.0112	.0329***	.0340**	.0400***
Delinquent Career/Onset								
N arrests as juvenile	.0128***	.0140***	.0146**	.0185***	.0041**	.0072**	.0082**	.0088**
N charges as juvenile	.0074	.0190	.0209	.0318	.0077	.0013	.0108	.0177
Age at first arrest	-.0011	.0009	-.0002	-.0012	-.0019	-.0001	-.0006	-.0030
Yrs since first incarc.	.0011***	.0016***	.0016***	.0009*	.0005*	.0010***	.0010***	.0010**
Yrs since first drug use	-.0006	-.0009*	-.0007	-.0006	-.0002	-.0005	-.0005	-.0009*
Prior CJS-Offender Action								
N prior incarcerations	.0012	-.0065	-.0108	-.0226***	.0020	.0027	.0020	-.0009
N prior parole revokes	-.0001	.0053	-.0065	-.0022	-.0039	-.0154*	-.0173	-.0233*
Bad conduct last probat.	.0220	.0238	.0119	-.0042	.0113	.0140	.0187	.0232
Recent parole revoked	.0286	-.0148	-.0078	.0086	.0150	.0234	.0219	.0313
General Control Variables								
Offender age at sent.	-.0047***	-.0070***	-.0081***	-.0065***	-.0004	-.0035**	-.0052***	-.0050***
Off. born out of state	-.0450***	-.0872***	-.1052***	-.1239***	-.0142**	-.0273***	-.0406***	-.0589***
Coder prob. prognosis	-.0006**	-.0011***	-.0009***	-.0008***	-.0001	-.0005**	-.0004*	-.0006**

* p<.05 ** p<.01 *** p<.001

Table B6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0410*	-.0692**	-.0843**	-.0975***	-.0177	-.0370*	-.0484**	-.0554**
Black x prior adult arrs	-.0081**	-.0102*	-.0175***	-.0152**	-.0056***	-.0091***	-.0128***	-.0146***
Black x n prior prop cnv	.0064*	.0097*	.0202***	.0228***	.0034*	.0041	.0069**	.0071*
Black x n charges as juv	.0127	.0196	.0327	.0164	-.0115	.0036	-.0069	-.0127
Female x Part 1 charges	.0180	.0391*	.0588**	.0446*	-.0003	-.0007	.0085	.0182
Off. age x drug problem	-.0052*	-.0038	-.0029	-.0021	-.0014	-.0038*	-.0052*	-.0056*
Off. age x prior trtment	-.0038	-.0086**	-.0099***	-.0100***	-.0022	-.0029	-.0029	-.0045
Off. age x unemployed	-.0018	-.0019	-.0015	-.0016	-.0012	-.0013	-.0010	-.0014
Off. age x PO property	-.0060**	-.0063**	-.0042*	-.0059**	-.0025*	-.0027	-.0029	-.0053**
Off. age x chg pst 5 yrs	.0004**	.0001	.0000	.0001	.0000	-.0002	-.0002	-.0002
PO viol x has detainers	-.0490	-.1313**	-.1454**	-.1741***	-.0274	-.0448	-.0616*	-.0905**
PO prop x n adl.arrests	.0032	.0021	-.0026	.0063	.0018	.0023	.0048	.0100**
PO prop x prior prop con	-.0045	-.0037	-.0011	-.0073	-.0004	-.0022	-.0050	-.0079*
PO prop x n juv. arrests	-.0105**	-.0105*	-.0119*	-.0157*	-.0038*	-.0081**	-.0072*	-.0030
PO prop x age at 1st arr	.0026	-.0024	-.0038	-.0023	-.0012	-.0075**	-.0075**	-.0029
PO prop x yrs. 1st incar	.0001	.0001	.0002	.0005	-.0002	-.0003	-.0003	-.0006
PO drugs x n adl. convs.	.0131**	.0149*	.0151*	.0150*	.0017	.0081*	.0100*	.0123*
PO drugs x Part 1 chgs.	-.0236**	-.0201*	-.0157	-.0231	.0000	-.0093	-.0057	-.0098
PO drugs x last par. rev	.0650	.1297	.0614	.0765	-.0170	-.0142	-.0010	.0168
Selection Hazards								
UCR to SAC arrest histry	-.0409**	-.0283	-.0305	-.0109	-.0076	-.0236*	-.0333*	-.0469**
Case proceeds past arrst	.2781***	.1648*	.1357	.1098	.1124	.1688*	.2349**	.1620*
Case to Grand Jury	-.0014	-.0009	-.0099	.0017	.0071	.0174	.0307*	.0352*
Case to Superior Court	.0733*	.0690	.0440	.0619	.0273	.0576	.0249	.0388
Superior Court Convict.	-.0153	-.0220	-.0026	.0082	-.0171	-.0102	-.0005	.0312
Match over data sources	-.0136	.0175	.0257	.0306	-.0215	-.0519	-.0490	-.0287
Constant	-.1839***	-.2081***	-.1289***	-.0795*	-.0694***	-.1363***	-.1729***	-.2036***
Mean of Dep. Var.	.234	.399	.480	.549	.072	.148	.201	.256
N of cases	11,749	11,749	11,749	11,749	11,714	11,746	11,749	11.749
Pseudo R squared	.150	.211	.229	.229	.070	.116	.140	.159
Model Classifications:								
% Correct Predictions	78.95	73.04	72.44	72.97	92.79	85.39	81.10	77.24
% False Positives	4.14	10.14	12.14	13.50	.15	1.26	2.79	5.26
% False Negatives	16.91	16.83	15.42	13.53	7.07	13.35	16.11	17.50
RIOC	.491	.492	.478	.454	.322	.455	.484	.472

* p<.05 ** p<.01 *** p<.001

Table B6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	-.0063	.0092	.0125	.0194	.0005	.0181	.0148	.0131
Has job after sentence	-.0126*	-.0174*	-.0122	-.0080	-.0203***	-.0350***	-.0381***	-.0348**
Offender is on welfare	-.0115	-.0152	-.0125	-.0030	-.0082	-.0197*	-.0261*	-.0180
Offender is Black	.0104	.0312**	.0560***	.0895***	.0033	.0726***	.1072***	.1280***
Offender is Hispanic	.0169	.0415*	.0773***	.0908***	.0076	.0637**	.0880***	.1274***
Offender is female	-.0153	-.0461***	-.0571***	-.0787***	-.0011	-.0096	-.0217	-.0484*
Lives in urban area	.0049	.0178*	.0180	.0274**	-.0045	-.0052	.0049	.0238*
Years at current address	-.0005	-.0009*	-.0010*	-.0012*	-.0001	-.0009*	-.0009	-.0006
History of drug problems	.0003	.0074	-.0042	.0013	.0160*	.0221	.0415**	.0411**
Treated for drugs/alch.	-.0059	.0009	.0179	.0153	.0041	.0154	.0162	.0166
Has needle marks	.0267**	.0393**	.0335*	.0335	.0095	.0214	.0231	.0373*
Not a school drop out	-.0185***	-.0168*	-.0211*	-.0172	.0027	-.0116	-.0153	-.0182
Doesn't live with family	-.0064	-.0146	-.0318**	-.0371***	.0044	-.0028	-.0047	-.0118
Commited PO with group	-.0165***	-.0253***	-.0342***	-.0351***	-.0098*	-.0030	-.0034	-.0145
Victim was a stranger	.0097	.0247**	.0262*	.0321**	.0105	.0230*	.0331**	.0357**
Presenting Offense								
PO property crime	.0285**	.0393**	.0557***	.0458**	.0098	-.0007	.0168	.0383*
PO crime against person	.0253*	.0517**	.0637***	.0611***	-.0028	.0026	.0134	.0155
PO drug offense	.0719***	.1429***	.1841***	.2275***	-.0050	-.0108	.0172	.0382
PO Wolfgang severity	-.0022***	-.0031***	-.0036***	-.0046***	.0000	.0001	.0002	-.0008
Has detainers at arrest	-.0054	-.0026	-.0062	-.0101	-.0062	.0070	.0087	.0132
Has pending charges	.0176**	.0304**	.0491***	.0585***	.0221***	.0553***	.0620***	.0628***
On probation at PO	.0213*	.0288*	.0340*	.0470**	.0173*	.0468***	.0618***	.0519**
Anamnestic Theory								
N prior adult arrests	.0082***	.0153***	.0191***	.0203***	.0029	.0064*	.0093**	.0114**
N prior adult conviction	.0001	-.0001	.0002	.0050	-.0014	.0020	.0024	.0031
N prior adult chg. conv.	-.0036*	-.0066**	-.0074**	-.0065*	.0003	-.0024	-.0020	-.0035
N charges past 5 years	.0019*	.0024*	.0032*	.0034*	.0020**	.0031**	.0030*	.0041**
N prior Part 1 charges	.0060**	.0051	.0047	.0038	.0001	.0019	.0026	.0014
N prior property conv.	-.0066***	-.0067**	-.0084**	-.0093**	-.0009	.0002	.0011	.0019
N prior persons conv.	.0003	-.0001	-.0001	.0005	.0030	.0035	.0077*	.0111**
N prior weapons conv.	.0021	.0150	.0018	.0051	-.0011	.0090	.0041	.0071
Off street last 2 years	.0131	.0334***	.0446***	.0570***	.0204**	.0602***	.0927***	.0913***
Delinquent Career/Onset								
N arrests as juvenile	.0025	.0041	.0033	.0059	.0033*	.0054*	.0053	.0056
N charges as juvenile	.0016	-.0030	-.0035	.0074	.0039	.0141	.0243	.0187
Age at first arrest	-.0010	.0014	.0008	.0004	.0011	.0020	.0038*	.0024
Yrs since first incarc.	.0000	.0006*	.0007*	.0004	.0021***	.0029***	.0035***	.0036***
Yrs since first drug use	-.0001	-.0002	-.0002	-.0001	.0001	.0002	.0001	-.0003
Prior CJS-Offender Action								
N prior incarcerations	.0024	-.0020	-.0014	-.0071	.0033	.0049	.0047	.0040
N prior parole revokes	-.0055	-.0128	-.0222*	-.0228*	.0039	-.0020	-.0007	-.0039
Bad conduct last probat.	.0003	.0055	.0025	-.0015	.0027	.0102	.0073	.0076
Recent parole revoked	.0267*	.0246	.0418*	.0436	.0143	.0435**	.0178	.0174
General Control Variables								
Offender age at sent.	-.0003	-.0016	-.0015	-.0021	-.0015	-.0039**	-.0045**	-.0048**
Off. born out of state	-.0170**	-.0243**	-.0440***	-.0472***	-.0006	-.0166*	-.0246**	-.0397***
Coder prob. prognosis	-.0003*	-.0006***	-.0007***	-.0009***	-.0003**	-.0007***	-.0009***	-.0010***

* p<.05 ** p<.01 *** p<.001

Table B6.5 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0214*	-.0350*	-.0380*	-.0439*	-.0139	-.0442**	-.0547**	-.0353
Black x prior adult arrs	-.0038*	-.0067**	-.0082**	-.0099***	-.0049***	-.0078***	-.0104***	-.0141***
Black x n prior prop cnv	.0033*	.0052*	.0054*	.0063*	.0039***	.0046*	.0045	.0052
Black x n charges as juv	.0030	.0077	.0146	.0006	-.0025	.0087	.0210	.0308
Female x Part 1 charges	.0049	.0048	.0073	.0044	.0018	.0107	.0156	.0229*
Off. age x drug problem	-.0018	-.0010	-.0037*	-.0026	.0002	-.0017	-.0011	.0006
Off. age x prior trtment	-.0015	-.0031	-.0027	-.0047*	-.0022	-.0049*	-.0052*	-.0078**
Off. age x unemployed	-.0018*	-.0003	-.0009	-.0013	-.0002	-.0010	-.0029*	-.0047**
Off. age x PO property	-.0027*	-.0039*	-.0056**	-.0070***	-.0013	-.0060**	-.0066**	-.0076***
Off. age x chg pst 5 yrs	.0001	.0000	.0000	.0000	.0002**	.0003**	.0002	.0003
PO viol x has detainers	-.0252	-.0596*	-.0775**	-.0986**	.0001	-.0169	-.0252	-.0403
PO prop x n adl.arrests	-.0033	-.0041	-.0031	-.0024	.0001	.0032	.0049	.0071*
PO prop x prior prop con	.0059**	.0084**	.0109***	.0118***	-.0005	-.0013	-.0022	-.0039
PO prop x n juv. arrests	.0000	.0016	.0018	.0012	-.0002	.0012	.0029	.0038
PO prop x age at 1st arr	.0035*	.0010	.0022	.0034	.0008	.0022	.0019	.0032
PO prop x yrs. 1st incar	.0007*	.0005	.0006	.0011*	-.0001	.0004	.0003	.0004
PO drugs x n adl. convs.	.0039	.0077*	.0090*	.0079	.0042	.0113**	.0106*	.0082
PO drugs x Part 1 chgs.	-.0122**	-.0107	-.0010	-.0017	-.0078*	-.0096	-.0126*	-.0104
PO drugs x last par. rev	.0242	.0310	-.0046	.0382	.0176	.0221	.0792	.0565
Selection Hazards								
UCR to SAC arrest histry	-.0284**	-.0222	-.0342*	-.0323*	.0069	-.0025	-.0039	.0069
Case proceeds past arrst	.0823	.0576	.0394	.0120	-.0052	.0064	-.0035	-.0181
Case to Grand Jury	-.0062	.0071	.0159	.0273	-.0145*	-.0422***	-.0568***	-.0730***
Case to Superior Court	.0055	-.0211	-.0419	.0068	.0121	.1260***	.0965**	.0817*
Superior Court Convict.	-.0056	-.0153	.0062	.0183	-.0071	-.0330	-.0379	-.0352
Match over data sources	-.0113	.0078	-.0015	.0561	-.0349	-.0397	-.0520	-.0433
Constant	-.0739***	-.1357***	-.1673***	-.2081***	-.0552***	-.1279***	-.1651***	-.1893***
Mean of Dep. Var.	.079	.153	.199	.246	.057	.137	.186	.230
N of cases	11,714	11,746	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.057	.089	.112	.127	.093	.188	.202	.233
Model Classifications:								
% Correct Predictions	92.12	84.84	80.54	76.94	94.34	87.75	85.09	82.44
% False Positives	.07	.95	2.37	3.75	.20	2.50	3.99	5.41
% False Negatives	7.81	14.21	17.09	19.30	5.46	9.75	10.92	12.15
RIOC	.543	.434	.430	.451	.459	.553	.581	.569

* p<.05 ** p<.01 *** p<.001

Table B6.6
Attribution of Explained Variance for Log of
Total Post-Sentence Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.189)	(.294)	(.332)	(.344)
Percent R2 Unattributed	32.29	33.69	33.21	33.02
	(.061)	(.099)	(.110)	(.114)
Attributed to Hazards	25.94	26.69	27.78	25.97
	(.049)	(.078)	(.092)	(.089)
Social Variables	3.56	4.82	6.19	7.88
	(.007)	(.014)	(.021)	(.027)
Presenting Offense	2.94	2.21	2.01	2.33
	(.006)	(.006)	(.007)	(.008)
Anamnestic Variables	10.01	9.84	9.10	9.60
	(.019)	(.029)	(.030)	(.033)
Delinquent Career/Onset	3.86	3.87	3.50	2.97
	(.007)	(.011)	(.012)	(.010)
Prior CJS/Offender Actions	.82	.28	.21	.35
	(.002)	(.001)	(.001)	(.001)
General Controls	2.99	3.84	4.25	4.86
	(.006)	(.011)	(.014)	(.017)
Social/Presenting Offense	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.83	.94	1.08	1.10
	(.002)	(.003)	(.004)	(.004)
Social/Delinquent Career	1.17	1.25	1.33	1.44
	(.002)	(.004)	(.004)	(.005)
Social/Prior CJS Actions	.18	.12	.07	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.91	1.60	1.51	1.70
	(.004)	(.005)	(.005)	(.006)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.03	.20	.23	.27
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.10	.22	.24	.24
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.65	2.42	1.86	1.61
	(.007)	(.007)	(.006)	(.006)
Anamnestic/Prior CJS Actions	4.24	2.52	2.07	1.29
	(.008)	(.007)	(.007)	(.004)
Anamnestic/General Controls	4.05	3.60	3.18	3.05
	(.008)	(.011)	(.011)	(.010)
Delinquency/Prior CJS Actions	.85	.48	.22	.00*
	(.002)	(.001)	(.001)	(.000)
Delinquency/General Controls	1.07	1.53	2.04	2.85
	(.002)	(.004)	(.007)	(.010)
Prior CJS Actions/Controls	.03	.13	.14	.18
	(.000)	(.000)	(.000)	(.001)
All Interactions	4.99	3.91	3.04	3.02
	(.009)	(.011)	(.010)	(.010)

* Partitioned variance is negative due to suppressor effect

Table B6.7
Attribution of Explained Variance for Log of
Total Post-Sentence Convictions - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.149)	(.240)	(.277)	(.289)
Percent R2 Unattributed	32.23	33.35	32.92	32.33
	(.048)	(.080)	(.091)	(.093)
Attributed to Hazards	25.83	26.60	27.38	27.15
	(.039)	(.064)	(.076)	(.078)
Social Variables	3.65	4.43	5.63	6.96
	(.005)	(.011)	(.016)	(.020)
Presenting Offense	3.37	2.59	2.40	2.88
	(.005)	(.006)	(.007)	(.008)
Anamnestic Variables	8.09	9.65	9.37	9.45
	(.012)	(.023)	(.026)	(.027)
Delinquent Career/Onset	5.48	5.21	4.59	3.52
	(.008)	(.013)	(.013)	(.010)
Prior CJS/Offender Actions	.84	.33	.45	.69
	(.001)	(.001)	(.001)	(.002)
General Controls	3.39	3.69	4.15	4.84
	(.005)	(.009)	(.011)	(.014)
Social/Presenting Offense	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.72	.97	1.09	1.18
	(.001)	(.002)	(.003)	(.003)
Social/Delinquent Career	1.05	1.23	1.29	1.17
	(.002)	(.003)	(.004)	(.003)
Social/Prior CJS Actions	.20	.04	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.01	.14	.16	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	2.19	2.07	1.89	1.95
	(.003)	(.005)	(.005)	(.006)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.13	.34	.38	.44
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.10	.19	.16	.15
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	3.50	2.46	2.07	1.66
	(.005)	(.006)	(.006)	(.005)
Anamnestic/Prior CJS Actions	3.98	2.04	1.54	.93
	(.006)	(.005)	(.004)	(.003)
Anamnestic/General Controls	3.64	3.37	3.17	3.09
	(.005)	(.008)	(.009)	(.009)
Delinquency/Prior CJS Actions	1.00	.28	.07	.00*
	(.001)	(.001)	(.000)	(.000)
Delinquency/General Controls	1.22	1.21	1.38	1.93
	(.002)	(.003)	(.004)	(.006)
Prior CJS Actions/Controls	.06	.17	.22	.28
	(.000)	(.000)	(.001)	(.001)
All Interactions	5.78	4.79	3.94	4.18
	(.009)	(.011)	(.011)	(.012)

* Partitioned variance is negative due to suppressor effect

Table B6.8
Attribution of Explained Variance for Log of
Total Post-Sentence Persons Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.083)	(.140)	(.166)	(.189)
Percent R2 Unattributed	29.75	32.15	33.42	33.91
	(.025)	(.045)	(.056)	(.064)
Attributed to Hazards	18.19	19.16	19.00	18.59
	(.015)	(.027)	(.032)	(.035)
Social Variables	5.33	6.66	7.01	7.85
	(.004)	(.009)	(.012)	(.015)
Presenting Offense	1.43	1.60	1.53	1.18
	(.001)	(.002)	(.003)	(.002)
Anamnestic Variables	19.60	15.47	13.41	13.59
	(.016)	(.022)	(.022)	(.026)
Delinquent Career/Onset	5.39	5.42	5.94	5.41
	(.004)	(.008)	(.010)	(.010)
Prior CJS/Offender Actions	1.61	.80	.35	.34
	(.001)	(.001)	(.001)	(.001)
General Controls	1.99	3.32	4.10	4.34
	(.002)	(.005)	(.007)	(.008)
Social/Presenting Offense	.48	.78	.95	.93
	(.000)	(.001)	(.002)	(.002)
Social/Anamnestic Variables	1.40	1.30	1.56	1.56
	(.001)	(.002)	(.003)	(.003)
Social/Delinquent Career	1.57	1.88	2.17	2.28
	(.001)	(.003)	(.004)	(.004)
Social/Prior CJS Actions	.25	.16	.13	.11
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.71	.47	.56	.48
	(.001)	(.001)	(.001)	(.001)
Presenting Offense/Delinquency	.00*	.13	.22	.19
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.00*	.00*	.01
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.07	.14	.25	.26
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	5.11	3.90	3.50	3.26
	(.004)	(.005)	(.006)	(.006)
Anamnestic/Prior CJS Actions	3.00	2.25	1.71	1.30
	(.002)	(.003)	(.003)	(.002)
Anamnestic/General Controls	2.18	2.51	2.23	2.32
	(.002)	(.004)	(.004)	(.004)
Delinquency/Prior CJS Actions	1.39	1.00	.76	.45
	(.001)	(.001)	(.001)	(.001)
Delinquency/General Controls	1.18	1.52	1.90	2.50
	(.001)	(.002)	(.003)	(.005)
Prior CJS Actions/Controls	.00*	.02	.04	.07
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.37	5.13	4.30	4.70
	(.004)	(.007)	(.007)	(.009)

* Partitioned variance is negative due to suppressor effect

Table B6.9
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Arrest Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.197)	(.320)	(.354)	(.371)
Percent R2 Unattributed	32.91	33.23	33.80	33.50
	(.065)	(.106)	(.120)	(.124)
Attributed to Hazards	25.02	25.94	26.65	26.53
	(.049)	(.083)	(.094)	(.098)
Social Variables	3.42	3.62	4.83	5.48
	(.007)	(.012)	(.017)	(.020)
Presenting Offense	2.58	1.53	1.20	1.07
	(.005)	(.005)	(.004)	(.004)
Anamnestic Variables	9.81	10.83	10.16	10.31
	(.019)	(.035)	(.036)	(.038)
Delinquent Career/Onset	4.52	4.43	4.02	4.02
	(.009)	(.014)	(.014)	(.015)
Prior CJS/Offender Actions	.94	.52	.40	.49
	(.002)	(.002)	(.001)	(.002)
General Controls	2.98	3.27	3.39	3.40
	(.006)	(.010)	(.012)	(.013)
Social/Presenting Offense	.00*	.01	.08	.06
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.71	.78	.97	.92
	(.001)	(.002)	(.003)	(.003)
Social/Delinquent Career	1.12	1.10	1.11	1.06
	(.002)	(.003)	(.004)	(.004)
Social/Prior CJS Actions	.20	.15	.16	.13
	(.000)	(.000)	(.001)	(.000)
Social/General Controls	.04	.09	.12	.10
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.90	1.65	1.33	1.27
	(.004)	(.005)	(.005)	(.005)
Presenting Offense/Delinquency	.00*	.00	.06	.05
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.05	.08	.06
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.10	.19	.21	.20
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.59	3.23	2.77	2.85
	(.007)	(.010)	(.010)	(.011)
Anamnestic/Prior CJS Actions	4.87	3.95	3.62	3.40
	(.010)	(.013)	(.013)	(.013)
Anamnestic/General Controls	3.81	3.89	3.58	3.54
	(.007)	(.012)	(.013)	(.013)
Delinquency/Prior CJS Actions	.98	.74	.60	.53
	(.002)	(.002)	(.002)	(.002)
Delinquency/General Controls	.90	.75	.80	.95
	(.002)	(.002)	(.003)	(.004)
Prior CJS Actions/Controls	.01	.05	.07	.07
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.74	3.95	3.90	3.94
	(.009)	(.013)	(.014)	(.015)

* Partitioned variance is negative due to suppressor effect

Table B6.10
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Charge Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.196)	(.311)	(.346)	(.361)
Percent R2 Unattributed	32.35	32.99	33.05	33.09
	(.063)	(.103)	(.114)	(.119)
Attributed to Hazards	25.48	26.76	27.89	27.23
	(.050)	(.083)	(.097)	(.098)
Social Variables	3.28	3.50	4.46	5.20
	(.006)	(.011)	(.015)	(.019)
Presenting Offense	2.69	1.66	1.36	1.22
	(.005)	(.005)	(.005)	(.004)
Anamnestic Variables	10.11	10.56	9.98	10.38
	(.020)	(.033)	(.035)	(.037)
Delinquent Career/Onset	4.56	4.40	4.06	4.06
	(.009)	(.014)	(.014)	(.015)
Prior CJS/Offender Actions	1.04	.62	.43	.46
	(.002)	(.002)	(.001)	(.002)
General Controls	2.44	2.90	3.10	3.12
	(.005)	(.009)	(.011)	(.011)
Social/Presenting Offense	.00*	.01	.07	.05
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.81	.82	.94	.90
	(.002)	(.003)	(.003)	(.003)
Social/Delinquent Career	1.16	1.08	1.05	1.07
	(.002)	(.003)	(.004)	(.004)
Social/Prior CJS Actions	.20	.19	.18	.13
	(.000)	(.001)	(.001)	(.000)
Social/General Controls	.04	.08	.08	.11
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.86	1.56	1.32	1.30
	(.004)	(.005)	(.005)	(.005)
Presenting Offense/Delinquency	.00*	.00*	.04	.04
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.04	.07	.04
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.09	.18	.19	.20
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.99	3.33	2.83	2.86
	(.008)	(.010)	(.010)	(.010)
Anamnestic/Prior CJS Actions	4.50	3.72	3.70	3.37
	(.009)	(.012)	(.013)	(.012)
Anamnestic/General Controls	3.93	3.88	3.58	3.54
	(.008)	(.012)	(.012)	(.013)
Delinquency/Prior CJS Actions	1.00	.85	.68	.52
	(.002)	(.003)	(.002)	(.002)
Delinquency/General Controls	.88	.83	.87	1.04
	(.002)	(.003)	(.003)	(.004)
Prior CJS Actions/Controls	.00*	.04	.05	.06
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.95	4.37	3.92	3.86
	(.010)	(.014)	(.014)	(.014)

* Partitioned variance is negative due to suppressor effect

Table B6.11
Attribution of Explained Variance for Log of
Adjusted Post-Sentence Persons Charge Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.089)	(.148)	(.174)	(.194)
Percent R2 Unattributed	29.32	31.58	32.02	32.81
	(.026)	(.047)	(.056)	(.064)
Attributed to Hazards	18.27	19.18	19.20	19.01
	(.016)	(.028)	(.033)	(.037)
Social Variables	4.52	5.07	4.99	4.83
	(.004)	(.008)	(.009)	(.009)
Presenting Offense	1.28	1.40	1.37	1.19
	(.001)	(.002)	(.002)	(.002)
Anamnestic Variables	18.23	14.10	13.36	13.73
	(.016)	(.021)	(.023)	(.027)
Delinquent Career/Onset	6.83	6.69	6.89	6.97
	(.006)	(.010)	(.012)	(.014)
Prior CJS/Offender Actions	2.11	1.83	1.61	1.28
	(.002)	(.003)	(.003)	(.002)
General Controls	1.74	2.52	3.13	2.87
	(.002)	(.004)	(.005)	(.006)
Social/Presenting Offense	.40	.45	.67	.67
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	1.18	1.07	1.24	1.21
	(.001)	(.002)	(.002)	(.002)
Social/Delinquent Career	1.54	1.67	1.65	1.68
	(.001)	(.002)	(.003)	(.003)
Social/Prior CJS Actions	.25	.24	.22	.21
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.63	.38	.52	.51
	(.001)	(.001)	(.001)	(.001)
Presenting Offense/Delinquency	.01	.10	.20	.20
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.07	.14	.20	.23
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	6.07	5.44	5.00	5.17
	(.005)	(.008)	(.009)	(.010)
Anamnestic/Prior CJS Actions	3.16	3.14	3.05	2.72
	(.003)	(.005)	(.005)	(.005)
Anamnestic/General Controls	2.28	2.81	2.64	2.82
	(.002)	(.004)	(.005)	(.005)
Delinquency/Prior CJS Actions	1.61	1.61	1.54	1.16
	(.001)	(.002)	(.003)	(.002)
Delinquency/General Controls	1.01	1.01	1.03	1.10
	(.001)	(.002)	(.002)	(.002)
Prior CJS Actions/Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.36	5.60	4.95	5.40
	(.004)	(.008)	(.009)	(.011)

* Partitioned variance is negative due to suppressor effect

Table B6.12
Attribution of Explained Variance for Summed Seriousness
of All Post-Sentence Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.117)	(.203)	(.236)	(.243)
Percent R2 Unattributed	32.22	34.05	34.21	33.97
	(.038)	(.069)	(.081)	(.082)
Attributed to Hazards	24.52	25.46	25.95	24.92
	(.029)	(.052)	(.061)	(.061)
Social Variables	3.51	4.54	5.79	7.16
	(.004)	(.009)	(.014)	(.017)
Presenting Offense	1.83	.95	.91	1.23
	(.002)	(.002)	(.002)	(.003)
Anamnestic Variables	11.95	11.69	10.91	11.83
	(.014)	(.024)	(.026)	(.029)
Delinquent Career/Onset	4.60	4.61	4.66	3.79
	(.005)	(.009)	(.011)	(.009)
Prior CJS/Offender Actions	1.23	.45	.15	.24
	(.001)	(.001)	(.000)	(.001)
General Controls	2.29	3.24	3.36	3.52
	(.003)	(.007)	(.008)	(.009)
Social/Presenting Offense	.00*	.07	.05	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.75	.99	1.27	1.33
	(.001)	(.002)	(.003)	(.003)
Social/Delinquent Career	1.22	1.29	1.34	1.32
	(.001)	(.003)	(.003)	(.003)
Social/Prior CJS Actions	.21	.17	.11	.06
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	.93	.83	.81	1.10
	(.001)	(.002)	(.002)	(.003)
Presenting Offense/Delinquency	.00*	.01	.03	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.01	.05	.13
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.08	.14	.12	.11
	(.000)	(.000)	(.000)	(.000)
Anamnestic/Delinquent Career	4.71	3.55	3.06	2.78
	(.005)	(.007)	(.007)	(.007)
Anamnestic/Prior CJS Actions	4.55	2.98	2.40	1.61
	(.005)	(.006)	(.006)	(.004)
Anamnestic/General Controls	3.77	3.43	3.27	3.37
	(.004)	(.007)	(.008)	(.008)
Delinquency/Prior CJS Actions	1.27	.89	.52	.20
	(.001)	(.002)	(.001)	(.000)
Delinquency/General Controls	.87	.82	1.02	1.50
	(.001)	(.002)	(.002)	(.004)
Prior CJS Actions/Controls	.00*	.04	.08	.12
	(.000)	(.000)	(.000)	(.000)
All Interactions	6.29	5.78	5.07	5.19
	(.007)	(.012)	(.012)	(.013)

* Partitioned variance is negative due to suppressor effect

Table B6.13

Regression Coefficients for Log of Total Post-Sentence Charges
Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0280*	(.0240)	.0402**	(.0244)	.0474**	(.0250)	.0449*	(.0210)
Has job after sentence	-.0415***	(-.0371)	-.0432**	(-.0274)	-.0405*	(-.0223)	-.0393*	(-.0191)
Offender is on welfare	-.0316**	(-.0236)	-.0242	(-.0128)	-.0206	(-.0094)	-.0031	(-.0012)
Offender is Black	.0534***	(.0405)	.1349***	(.0760)	.1852***	(.0910)	.2661***	(.1175)
Offender is Hispanic	.0559**	(.0260)	.1446***	(.0477)	.1939***	(.0556)	.2685***	(.0682)
Offender is female	-.0396*	(-.0225)	-.0828***	(-.0334)	-.1032***	(-.0361)	-.1474***	(-.0457)
Lives in urban area	.0142	(.0126)	.0424**	(.0266)	.0613***	(.0335)	.0701***	(.0339)
Years at current address	-.0010*	(-.0189)	-.0014*	(-.0181)	-.0015*	(-.0167)	-.0013	(-.0130)
History of drug problems	.0032	(.0024)	.0305	(.0166)	.0504*	(.0237)	.0569*	(.0238)
Treated for drugs/alch.	-.0205	(-.0139)	.0056	(.0027)	.0424	(.0177)	.0756**	(.0280)
Has needle marks	.0164	(.0075)	.0575*	(.0186)	.1023***	(.0288)	.1201***	(.0299)
Not a school drop out	-.0254*	(-.0221)	-.0422**	(-.0260)	-.0569***	(-.0304)	-.0652***	(-.0309)
Doesn't live with family	-.0033	(-.0024)	-.0107	(-.0056)	-.0631***	(-.0285)	-.0636**	(-.0255)
Commited PO with group	-.0195*	(-.0174)	-.0246	(-.0156)	-.0225	(-.0124)	-.0290	(-.0142)
Victim was a stranger	.0145	(.0115)	.0493**	(.0277)	.0541**	(.0263)	.0749***	(.0323)
Presenting Offense								
PO property crime	.0156	(.0137)	.0450*	(.0281)	.0642**	(.0348)	.0891***	(.0428)
PO crime against person	-.0273	(-.0249)	.0017	(-.0074)	.0051	(-.0055)	.0030	(-.0072)
PO drug offense	.0097	(.0100)	.0058	(.0111)	.0060	(.0090)	.0239	(.0165)
PO Wolfgang severity	-.0018*	(-.0247)	-.0030**	(-.0300)	-.0032**	(-.0272)	-.0048***	(-.0365)
Has detainers at arrest	-.0050	(-.0124)	.0165	(-.0105)	.0155	(-.0104)	-.0068	(-.0175)
Has pending charges	.0719***	(.0515)	.1189***	(.0604)	.1388***	(.0612)	.1495***	(.0584)
On probation at PO	.0503**	(.0235)	.0773***	(.0232)	.0965***	(.0240)	.1158***	(.0247)
Anamnestic Theory								
N prior adult arrests	.0186***	(.1153)	.0400***	(.2002)	.0484***	(.2071)	.0534***	(.2155)
N prior adult conviction	-.0086*	(-.0239)	-.0092	(-.0096)	-.0008	(.0234)	.0038	(.0389)
N prior adult chg. conv.	-.0106**	(-.0754)	-.0189***	(-.0952)	-.0236***	(-.1032)	-.0229***	(-.0886)
N charges past 5 years	.0126***	(.1136)	.0153***	(.0977)	.0162***	(.0902)	.0198***	(.0975)
N prior Part 1 charges	.0212***	(.0576)	.0105	(.0074)	.0116	(.0224)	.0073	(.0133)
N prior property conv.	-.0005	(.0107)	.0058	(.0389)	.0043	(.0345)	.0060	(.0269)
N prior persons conv.	-.0028	(-.0075)	-.0015	(-.0029)	.0015	(.0025)	.0010	(.0015)
N prior weapons conv.	.0330*	(.0205)	.0500**	(.0220)	.0355	(.0136)	.0646**	(.0219)
Off street last 2 years	.0787***	(.0641)	.1600***	(.0924)	.1866***	(.0935)	.2039***	(.0905)
Delinquent Career/Onset								
N arrests as juvenile	.0178***	(.0707)	.0265***	(.0712)	.0279***	(.0632)	.0335***	(.0659)
N charges as juvenile	-.0085	(.0059)	-.0086	(.0139)	.0041	(.0213)	.0119	(.0205)
Age at first arrest	.0018	(.0486)	.0051**	(.0523)	.0051**	(.0444)	.0034	(.0271)
Yrs since first incarc.	.0008*	(.0565)	.0023***	(.0826)	.0030***	(.0899)	.0030***	(.0797)
Yrs since first drug use	-.0007	(-.0156)	-.0012*	(-.0195)	-.0013*	(-.0196)	-.0012	(-.0153)
Prior CJS-Offender Action								
N prior incarcerations	.0071	(.0279)	.0009	(.0025)	-.0067	(-.0163)	-.0228**	(-.0490)
N prior parole revokes	.0089	(.0075)	-.0017	(-.0010)	-.0144	(-.0076)	-.0185	(-.0086)
Bad conduct last probat.	.0295*	(.0206)	.0558**	(.0276)	.0497*	(.0214)	.0327	(.0125)
Recent parole revoked	.0595*	(.0309)	.0064	(.0196)	.0009	(.0138)	.0230	(.0200)
General Control Variables								
Offender age at sent.	-.0035**	(-.1623)	-.0083***	(-.2015)	-.0114***	(-.2132)	-.0134***	(-.2149)
Off. born out of state	-.0464***	(-.0415)	-.1009***	(-.0639)	-.1375***	(-.0756)	-.1800***	(-.0877)
Coder prob. prognosis	-.0004	(-.0193)	-.0012***	(-.0425)	-.0013***	(-.0405)	-.0016***	(-.0426)

*p<.05 **p<.01 ***p<.001

Table B6.13 (continued)

Regression Coefficients for Log of Total Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0400	(-.0142)	-.0720*	(-.0181)	-.0956**	(-.0209)	-.1199**	(-.0232)
Black x prior adult arrs	-.0137***	(-.0521)	-.0168***	(-.0454)	-.0205***	(-.0479)	-.0238***	(-.0494)
Black x n prior prop cnv	.0137***	(.0542)	.0152***	(.0426)	.0184***	(.0448)	.0192***	(.0415)
Black x n charges as juv	.0337	(.0157)	.0672**	(.0222)	.0748**	(.0215)	.0640*	(.0163)
Female x Part 1 charges	.0186	(.0242)	.0283*	(.0261)	.0466**	(.0373)	.0597***	(.0424)
Off. age x drug problem	-.0055*	(-.0388)	-.0073*	(-.0366)	-.0084**	(-.0367)	-.0072*	(-.0278)
Off. age x prior trtment	-.0077**	(-.0482)	-.0128***	(-.0571)	-.0128***	(-.0495)	-.0160***	(-.0550)
Off. age x unemployed	-.0028*	(-.0224)	-.0041**	(-.0233)	-.0043*	(-.0208)	-.0040*	(-.0172)
Off. age x PO property	-.0065***	(-.0527)	-.0083***	(-.0479)	-.0090***	(-.0454)	-.0110***	(-.0487)
Off. age x chg pst 5 yrs	-.0003	(-.0255)	-.0006**	(-.0361)	-.0006**	(-.0323)	-.0005	(-.0233)
PO viol x has detainers	-.1164**	(-.0205)	-.2535***	(-.0316)	-.2716***	(-.0294)	-.3328***	(-.0319)
PO prop x n adl.arrests	.0058	(.0218)	.0103	(.0274)	.0108	(.0251)	.0209**	(.0430)
PO prop x prior prop con	-.0101**	(-.0398)	-.0136**	(-.0379)	-.0130*	(-.0315)	-.0201**	(-.0430)
PO prop x n juv. arrests	-.0052	(-.0114)	-.0104	(-.0163)	-.0127*	(-.0174)	-.0165*	(-.0200)
PO prop x age at 1st arr	.0042	(.0282)	.0009	(.0041)	.0005	(.0022)	.0006	(.0020)
PO prop x yrs. 1st incar	.0015**	(.0297)	.0013*	(.0190)	.0013	(.0164)	.0015	(.0169)
PO drugs x n adl. convs.	.0204***	(.0536)	.0300***	(.0559)	.0294***	(.0476)	.0329***	(.0471)
PO drugs x Part 1 chgs.	-.0396***	(-.0690)	-.0476***	(-.0587)	-.0339**	(-.0364)	-.0347*	(-.0329)
PO drugs x last par. rev	.0593	(.0106)	.2525**	(.0320)	.2235*	(.0246)	.2720*	(.0265)
Selection Hazards								
UCR to SAC arrest histry	-.0360*	(-.0249)	-.0418*	(-.0205)	-.0545*	(-.0232)	-.0371	(-.0140)
Case proceeds past arrst	.2355***	(.0480)	.2268*	(.0327)	.2443*	(.0306)	.2267	(.0252)
Case to Grand Jury	-.0020	(-.0015)	.0069	(.0037)	-.0069	(-.0032)	-.0033	(-.0014)
Case to Superior Court	.0498	(.0142)	.0288	(.0058)	-.0054	(-.0009)	.0213	(.0033)
Superior Court Convict.	-.0353	(-.0152)	-.0492	(-.0150)	-.0243	(-.0064)	.0222	(.0052)
Match over data sources	-.0869	(-.0226)	-.0508	(-.0094)	-.0405	(-.0065)	-.0066	(-.0009)
Constant	.2241***	(-.0096)	.4075***	(-.0116)	.5670***	(-.0081)	.6680***	(-.0026)
R squared	.189		.294		.332		.344	
Adjusted R squared	.185		.289		.328		.341	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.14

Regression Coefficients for Log of Total Post-Sentence Convictions
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0113	(.0179)	.0196*	(.0203)	.0277**	(.0241)	.0254*	(.0194)
Has job after sentence	-.0215***	(-.0354)	-.0283**	(-.0306)	-.0310**	(-.0282)	-.0297*	(-.0237)
Offender is on welfare	-.0145*	(-.0199)	-.0103	(-.0093)	-.0070	(-.0053)	.0046	(.0031)
Offender is Black	.0177*	(.0236)	.0651***	(.0628)	.0971***	(.0786)	.1384***	(.0991)
Offender is Hispanic	.0225*	(.0194)	.0514**	(.0290)	.0729***	(.0346)	.0971***	(.0404)
Offender is female	-.0019	(-.0020)	-.0174	(-.0120)	-.0198	(-.0114)	-.0325	(-.0165)
Lives in urban area	.0047	(.0078)	.0203*	(.0218)	.0323**	(.0291)	.0374**	(.0296)
Years at current address	-.0004	(-.0142)	-.0003	(-.0075)	-.0004	(-.0076)	-.0004	(-.0073)
History of drug problems	-.0048	(-.0067)	.0038	(.0035)	.0178	(.0139)	.0221	(.0151)
Treated for drugs/alch.	-.0022	(-.0027)	.0132	(.0109)	.0202	(.0139)	.0512**	(.0311)
Has needle marks	.0049	(.0042)	.0370*	(.0205)	.0582**	(.0271)	.0634**	(.0259)
Not a school drop out	-.0102	(-.0164)	-.0196*	(-.0206)	-.0300**	(-.0265)	-.0332**	(-.0258)
Doesn't live with family	.0027	(.0037)	-.0047	(-.0041)	-.0303**	(-.0227)	-.0328**	(-.0215)
Commited PO with group	-.0221***	(-.0366)	-.0234**	(-.0253)	-.0224*	(-.0204)	-.0239*	(-.0191)
Victim was a stranger	.0166*	(.0242)	.0298**	(.0286)	.0391***	(.0315)	.0574***	(.0406)
Presenting Offense								
PO property crime	.0076	(.0124)	.0345**	(.0369)	.0343*	(.0308)	.0454**	(.0357)
PO crime against person	-.0127	(-.0197)	-.0050	(-.0098)	-.0108	(-.0130)	-.0215	(-.0193)
PO drug offense	.0072	(.0165)	.0204	(.0276)	.0174	(.0217)	.0150	(.0200)
PO Wolfgang severity	-.0013***	(-.0341)	-.0018**	(-.0307)	-.0026***	(-.0366)	-.0033***	(-.0407)
Has detainers at arrest	-.0138	(-.0167)	-.0044	(-.0127)	-.0131	(-.0156)	-.0309	(-.0228)
Has pending charges	.0254***	(.0337)	.0528***	(.0458)	.0688***	(.0502)	.0808***	(.0517)
On probation at PO	.0303**	(.0304)	.0473***	(.0280)	.0571***	(.0251)	.0645***	(.0219)
Anamnestic Theory								
N prior adult arrests	.0062**	(.0738)	.0190***	(.1769)	.0243***	(.1855)	.0287***	(.1952)
N prior adult conviction	-.0009	(.0211)	.0007	(.0343)	.0069*	(.0662)	.0091*	(.0716)
N prior adult chg. conv.	-.0021	(-.0276)	-.0065*	(-.0564)	-.0105**	(-.0760)	-.0119**	(-.0756)
N charges past 5 years	.0049***	(.0819)	.0074***	(.0810)	.0096***	(.0886)	.0114***	(.0923)
N prior Part 1 charges	.0098***	(.0471)	.0072	(.0301)	.0097*	(.0483)	.0085	(.0405)
N prior property conv.	.0000	(.0058)	.0006	(.0083)	-.0029	(-.0073)	-.0019	(-.0057)
N prior persons conv.	-.0077**	(-.0383)	-.0114**	(-.0373)	-.0111**	(-.0306)	-.0121*	(-.0293)
N prior weapons conv.	.0006	(.0007)	.0094	(.0071)	-.0032	(-.0020)	-.0023	(-.0013)
Off street last 2 years	.0374***	(.0563)	.0868***	(.0857)	.1080***	(.0895)	.1209***	(.0880)
Delinquent Career/Onset								
N arrests as juvenile	.0074***	(.0632)	.0135***	(.0725)	.0150***	(.0643)	.0156***	(.0566)
N charges as juvenile	.0050	(.0293)	-.0005	(.0269)	-.0076	(.0237)	-.0143	(.0198)
Age at first arrest	.0011	(.0541)	.0029**	(.0657)	.0036**	(.0643)	.0028*	(.0478)
Yrs since first incarc.	.0004	(.0543)	.0011***	(.0777)	.0017***	(.0903)	.0015***	(.0801)
Yrs since first drug use	-.0001	(-.0050)	-.0007*	(-.0190)	-.0009*	(-.0218)	-.0011**	(-.0234)
Prior CJS-Offender Action								
N prior incarcerations	.0014	(.0105)	-.0055	(-.0261)	-.0122**	(-.0489)	-.0191***	(-.0671)
N prior parole revokes	-.0014	(-.0022)	-.0061	(-.0063)	-.0043	(-.0037)	-.0088	(-.0067)
Bad conduct last probat.	.0136	(.0176)	.0216	(.0183)	.0271*	(.0193)	.0355*	(.0222)
Recent parole revoked	.0330*	(.0399)	.0020	(.0205)	-.0104	(.0138)	-.0105	(.0178)
General Control Variables								
Offender age at sent.	-.0014*	(-.1455)	-.0040***	(-.1987)	-.0062***	(-.2159)	-.0070***	(-.2148)
Off. born out of state	-.0258***	(-.0425)	-.0456***	(-.0494)	-.0648***	(-.0589)	-.0954***	(-.0762)
Coder prob. prognosis	-.0002	(-.0195)	-.0005**	(-.0315)	-.0006***	(-.0321)	-.0008***	(-.0341)

*p<.05 **p<.01 ***p<.001

Table B6.14 (continued)

Regression Coefficients for Log of Total Post-Sentence Convictions
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0167	(-.0109)	-.0342	(-.0147)	-.0517*	(-.0187)	-.0692**	(-.0219)
Black x prior adult arrs	-.0065**	(-.0459)	-.0088**	(-.0406)	-.0102**	(-.0395)	-.0129***	(-.0438)
Black x n prior prop cnv	.0064***	(.0471)	.0084**	(.0405)	.0112***	(.0451)	.0130***	(.0461)
Black x n charges as juv	.0271**	(.0234)	.0550***	(.0311)	.0738***	(.0351)	.0866***	(.0361)
Female x Part 1 charges	.0113	(.0271)	.0249**	(.0393)	.0326**	(.0432)	.0417***	(.0485)
Off. age x drug problem	-.0041***	(-.0531)	-.0061***	(-.0521)	-.0067***	(-.0485)	-.0077***	(-.0486)
Off. age x prior trtment	-.0026	(-.0300)	-.0055**	(-.0421)	-.0065**	(-.0413)	-.0076**	(-.0425)
Off. age x unemployed	-.0016*	(-.0227)	-.0029**	(-.0282)	-.0036**	(-.0287)	-.0038**	(-.0273)
Off. age x PO property	-.0035***	(-.0523)	-.0059***	(-.0578)	-.0064***	(-.0529)	-.0073***	(-.0531)
Off. age x chg pst 5 yrs	-.0001	(-.0106)	-.0003*	(-.0258)	-.0004*	(-.0303)	-.0003*	(-.0257)
PO viol x has detainers	-.0338	(-.0110)	-.0983**	(-.0209)	-.1093**	(-.0195)	-.1361**	(-.0214)
PO prop x n adl.arrests	.0045*	(.0316)	.0095**	(.0433)	.0097**	(.0372)	.0133**	(.0447)
PO prop x prior prop con	-.0060**	(-.0436)	-.0083**	(-.0395)	-.0071*	(-.0285)	-.0113**	(-.0395)
PO prop x n juv. arrests	.0004	(.0017)	-.0007	(-.0019)	-.0024	(-.0054)	-.0039	(-.0077)
PO prop x age at 1st arr	.0026*	(.0324)	.0026	(.0212)	.0025	(.0172)	.0027	(.0163)
PO prop x yrs. 1st incar	.0009**	(.0332)	.0012**	(.0302)	.0011*	(.0224)	.0017**	(.0309)
PO drugs x n adl. convs.	.0115***	(.0560)	.0166***	(.0529)	.0150**	(.0402)	.0162**	(.0381)
PO drugs x Part 1 chgs.	-.0208***	(-.0669)	-.0171*	(-.0360)	-.0079	(-.0140)	-.0095	(-.0147)
PO drugs x last par. rev	.0775*	(.0256)	.1627**	(.0352)	.1817**	(.0330)	.2462***	(.0393)
Selection Hazards								
UCR to SAC arrest histry	-.0172*	(-.0220)	-.0262*	(-.0220)	-.0394**	(-.0278)	-.0356*	(-.0220)
Case proceeds past arrst	.1709***	(.0643)	.1667**	(.0412)	.1846**	(.0383)	.1839*	(.0335)
Case to Grand Jury	-.0013	(-.0018)	.0143	(.0131)	.0111	(.0085)	.0212	(.0142)
Case to Superior Court	-.0008	(-.0004)	-.0026	(-.0009)	-.0024	(-.0007)	-.0143	(-.0037)
Superior Court Convict.	-.0017	(-.0013)	-.0232	(-.0121)	-.0237	(-.0103)	-.0132	(-.0051)
Match over data sources	-.0812**	(-.0390)	-.0376	(-.0119)	-.0489	(-.0130)	-.0730	(-.0170)
Constant	.1039***	(-.0093)	.1722***	(-.0126)	.2522***	(-.0127)	.3131***	(-.0095)
R squared	.149		.240		.277		.289	
Adjusted R squared	.144		.235		.273		.285	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.15

Regression Coefficients for Log of Total Post-Sentence Persons Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0070	(.0135)	.0163*	(.0216)	.0270**	(.0301)	.0223*	(.0211)
Has job after sentence	-.0145**	(-.0290)	-.0119	(-.0164)	-.0158	(-.0183)	-.0192	(-.0189)
Offender is on welfare	-.0016	(-.0027)	-.0011	(-.0012)	.0007	(.0007)	.0047	(.0039)
Offender is Black	.0247***	(.0445)	.0670***	(.0822)	.0781***	(.0802)	.1113***	(.0983)
Offender is Hispanic	.0203*	(.0213)	.0544***	(.0391)	.0692***	(.0419)	.1016***	(.0521)
Offender is female	-.0333***	(-.0424)	-.0636***	(-.0558)	-.0875***	(-.0646)	-.1114***	(-.0697)
Lives in urban area	.0115*	(.0229)	.0189**	(.0259)	.0255**	(.0294)	.0285**	(.0278)
Years at current address	-.0003	(-.0110)	-.0005	(-.0149)	-.0005	(-.0124)	-.0001	(-.0029)
History of drug problems	.0023	(.0040)	-.0013	(-.0015)	.0021	(.0021)	-.0163	(-.0138)
Treated for drugs/alch.	-.0106	(-.0162)	-.0126	(-.0132)	-.0086	(-.0076)	-.0078	(-.0058)
Has needle marks	-.0060	(-.0061)	-.0237	(-.0167)	-.0239	(-.0142)	-.0053	(-.0027)
Not a school drop out	.0009	(.0017)	-.0049	(-.0065)	-.0158	(-.0178)	-.0219*	(-.0210)
Doesn't live with family	-.0033	(-.0055)	-.0018	(-.0020)	-.0066	(-.0063)	-.0063	(-.0051)
Commited PO with group	-.0014	(-.0029)	.0060	(.0082)	.0013	(.0015)	.0015	(.0015)
Victim was a stranger	.0038	(.0067)	.0056	(.0068)	.0144	(.0148)	.0216	(.0188)
Presenting Offense								
PO property crime	-.0008	(-.0016)	-.0039	(-.0053)	-.0052	(-.0059)	-.0044	(-.0043)
PO crime against person	.0188*	(.0248)	.0437***	(.0417)	.0571***	(.0461)	.0608***	(.0385)
PO drug offense	.0050	(.0051)	-.0107	(-.0144)	-.0192	(-.0189)	-.0260	(-.0199)
PO Wolfgang severity	-.0001	(-.0039)	-.0005	(-.0111)	-.0008	(-.0144)	-.0009	(-.0136)
Has detainers at arrest	-.0051	(-.0152)	-.0173	(-.0238)	-.0168	(-.0225)	-.0031	(-.0191)
Has pending charges	.0137*	(.0220)	.0179*	(.0198)	.0274**	(.0256)	.0375**	(.0296)
On probation at PO	-.0074	(-.0204)	-.0004	(-.0176)	.0071	(-.0114)	.0234	(-.0006)
Anamnestic Theory								
N prior adult arrests	.0069***	(.0873)	.0132***	(.1268)	.0159***	(.1260)	.0192***	(.1386)
N prior adult conviction	-.0067***	(-.0904)	-.0072**	(-.0599)	-.0063*	(-.0415)	-.0061	(-.0290)
N prior adult chg. conv.	-.0012	(-.0189)	-.0024	(-.0263)	-.0060*	(-.0557)	-.0080*	(-.0626)
N charges past 5 years	.0042***	(.0844)	.0065***	(.0905)	.0062***	(.0726)	.0076***	(.0756)
N prior Part 1 charges	.0042	(.0248)	-.0029	(-.0428)	.0022	(-.0023)	.0000	(-.0120)
N prior property conv.	-.0042*	(-.0640)	-.0024	(-.0346)	-.0033	(-.0369)	-.0024	(-.0288)
N prior persons conv.	.0142***	(.0859)	.0249***	(.1037)	.0329***	(.1155)	.0398***	(.1183)
N prior weapons conv.	.0220**	(.0307)	.0211*	(.0202)	.0338**	(.0273)	.0566***	(.0387)
Off street last 2 years	.0164**	(.0299)	.0376***	(.0473)	.0443***	(.0469)	.0617***	(.0553)
Delinquent Career/Onset								
N arrests as juvenile	.0080***	(.0680)	.0112***	(.0672)	.0135***	(.0715)	.0165***	(.0790)
N charges as juvenile	-.0062	(.0038)	-.0171	(.0152)	-.0052	(.0255)	-.0054	(.0233)
Age at first arrest	.0005	(.0173)	.0021*	(.0335)	.0028**	(.0410)	.0019	(.0332)
Yrs since first incarc.	.0002	(.0189)	.0006**	(.0388)	.0009***	(.0509)	.0010**	(.0425)
Yrs since first drug use	-.0001	(-.0080)	-.0005*	(-.0184)	-.0007*	(-.0217)	-.0010**	(-.0260)
Prior CJS-Offender Action								
N prior incarcerations	.0047*	(.0413)	.0066*	(.0400)	.0053	(.0274)	.0028	(.0121)
N prior parole revokes	-.0044	(-.0084)	-.0212*	(-.0278)	-.0202	(-.0224)	-.0299*	(-.0280)
Bad conduct last probat.	.0140*	(.0220)	.0241**	(.0259)	.0196	(.0178)	.0225	(.0173)
Recent parole revoked	.0326*	(.0233)	.0330	(.0172)	.0292	(.0160)	.0341	(.0200)
General Control Variables								
Offender age at sent.	-.0010	(-.0724)	-.0032***	(-.1201)	-.0046***	(-.1474)	-.0053***	(-.1655)
Off. born out of state	-.0152**	(-.0304)	-.0308***	(-.0424)	-.0443***	(-.0515)	-.0594***	(-.0585)
Coder prob. prognosis	.0000	(-.0038)	-.0003*	(-.0260)	-.0004*	(-.0256)	-.0005**	(-.0279)

*p<.05 **p<.01 ***p<.001

Table B6.15 (continued)

Regression Coefficients for Log of Total Post-Sentence Persons Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0122	(-.0097)	-.0355*	(-.0194)	-.0442*	(-.0204)	-.0552*	(-.0216)
Black x prior adult arrs	-.0045**	(-.0386)	-.0077**	(-.0449)	-.0115***	(-.0571)	-.0143***	(-.0600)
Black x n prior prop cnv	.0025	(.0220)	.0039	(.0242)	.0063*	(.0325)	.0068*	(.0295)
Black x n charges as juv	.0183*	(.0191)	.0629***	(.0453)	.0595***	(.0361)	.0636***	(.0327)
Female x Part 1 charges	-.0077	(-.0225)	-.0086	(-.0172)	-.0048	(-.0081)	.0038	(.0054)
Off. age x drug problem	-.0006	(-.0101)	-.0015	(-.0170)	-.0028	(-.0254)	-.0041*	(-.0319)
Off. age x prior trtment	-.0014	(-.0192)	-.0021	(-.0208)	-.0027	(-.0220)	-.0049*	(-.0342)
Off. age x unemployed	-.0011	(-.0194)	-.0015	(-.0187)	-.0021*	(-.0212)	-.0022*	(-.0189)
Off. age x PO property	-.0005	(-.0094)	-.0006	(-.0071)	-.0010	(-.0110)	-.0029	(-.0260)
Off. age x chg pst 5 yrs	-.0002*	(-.0346)	-.0004***	(-.0491)	-.0004***	(-.0433)	-.0005**	(-.0440)
PO viol x has detainers	-.0516*	(-.0204)	-.0896**	(-.0243)	-.1132***	(-.0259)	-.1834***	(-.0355)
PO prop x n adl.arrests	.0004	(.0034)	.0020	(.0117)	.0043	(.0211)	.0085*	(.0353)
PO prop x prior prop con	-.0011	(-.0098)	-.0052*	(-.0316)	-.0075*	(-.0381)	-.0093**	(-.0401)
PO prop x n juv. arrests	-.0032	(-.0159)	-.0039	(-.0132)	-.0031	(-.0090)	-.0016	(-.0040)
PO prop x age at 1st arr	.0001	(.0020)	-.0012	(-.0128)	-.0011	(-.0095)	.0008	(.0057)
PO prop x yrs. 1st incar	.0001	(.0045)	.0000	(.0002)	.0001	(.0036)	-.0002	(-.0033)
PO drugs x n adl. convs.	.0008	(.0049)	.0038	(.0155)	.0047	(.0161)	.0080	(.0232)
PO drugs x Part 1 chgs.	-.0029	(-.0111)	-.0124*	(-.0334)	-.0090	(-.0203)	-.0130	(-.0249)
PO drugs x last par. rev	-.0338	(-.0136)	-.0277	(-.0076)	.0002	(.0001)	.0388	(.0076)
Selection Hazards								
UCR to SAC arrest histry	-.0016	(-.0025)	-.0109	(-.0116)	-.0176	(-.0158)	-.0297*	(-.0226)
Case proceeds past arrst	.0468	(.0214)	.0734	(.0231)	.0990	(.0262)	.0998	(.0224)
Case to Grand Jury	.0058	(.0098)	.0151	(.0175)	.0295*	(.0289)	.0422**	(.0349)
Case to Superior Court	.0252	(.0162)	.0411	(.0181)	.0202	(.0075)	.0129	(.0041)
Superior Court Convict.	-.0156	(-.0151)	-.0090	(-.0060)	-.0031	(-.0017)	.0166	(.0079)
Match over data sources	-.0254	(-.0149)	-.0512	(-.0206)	-.0360	(-.0122)	-.0197	(-.0057)
Constant	.0379**	(-.0096)	.0717***	(-.0137)	.1161***	(-.0096)	.1585***	(-.0089)
R squared	.083		.140		.166		.189	
Adjusted R squared	.078		.135		.162		.184	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.16

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0147	(.0154)	.0173*	(.0212)	.0200**	(.0265)	.0173**	(.0251)
Has job after sentence	-.0401***	(-.0435)	-.0277***	(-.0355)	-.0226***	(-.0313)	-.0192***	(-.0291)
Offender is on welfare	-.0267**	(-.0242)	-.0141	(-.0150)	-.0141*	(-.0163)	-.0097	(-.0122)
Offender is Black	.0444***	(.0382)	.0639***	(.0746)	.0723***	(.0915)	.0769***	(.1105)
Offender is Hispanic	.0442**	(.0250)	.0523***	(.0349)	.0557***	(.0401)	.0567***	(.0448)
Offender is female	-.0221	(-.0152)	-.0280*	(-.0228)	-.0251*	(-.0220)	-.0261**	(-.0251)
Lives in urban area	.0078	(.0084)	.0152*	(.0193)	.0200**	(.0274)	.0194***	(.0292)
Years at current address	-.0008*	(-.0183)	-.0007*	(-.0184)	-.0005	(-.0143)	-.0003	(-.0084)
History of drug problems	.0038	(.0035)	.0120	(.0132)	.0198*	(.0235)	.0150	(.0195)
Treated for drugs/alch.	-.0181	(-.0150)	-.0020	(-.0019)	.0128	(.0134)	.0212**	(.0244)
Has needle marks	.0240	(.0133)	.0261*	(.0171)	.0344**	(.0243)	.0327**	(.0253)
Not a school drop out	-.0194*	(-.0205)	-.0168*	(-.0209)	-.0153*	(-.0206)	-.0095	(-.0140)
Doesn't live with family	-.0074	(-.0066)	-.0135	(-.0142)	-.0204**	(-.0231)	-.0179**	(-.0223)
Commited PO with group	-.0216**	(-.0235)	-.0107	(-.0138)	-.0069	(-.0095)	-.0073	(-.0111)
Victim was a stranger	.0216*	(.0208)	.0212**	(.0240)	.0213**	(.0260)	.0208**	(.0280)
Presenting Offense								
PO property crime	.0139	(.0149)	.0208*	(.0263)	.0232**	(.0315)	.0239**	(.0357)
PO crime against person	-.0221	(-.0226)	.0006	(-.0067)	.0061	(-.0006)	.0062	(.0017)
PO drug offense	.0014	(.0058)	.0045	(.0114)	-.0012	(.0045)	.0041	(.0117)
PO Wolfgang severity	-.0014*	(-.0229)	-.0006	(-.0116)	-.0002	(-.0046)	-.0001	(-.0023)
Has detainers at arrest	-.0188	(-.0165)	.0143	(-.0049)	.0149	(-.0037)	.0033	(-.0086)
Has pending charges	.0527***	(.0459)	.0568***	(.0583)	.0487***	(.0539)	.0439***	(.0533)
On probation at PO	.0532***	(.0290)	.0339**	(.0226)	.0335***	(.0228)	.0239**	(.0193)
Anamnestic Theory								
N prior adult arrests	.0141***	(.1178)	.0169***	(.1863)	.0165***	(.1950)	.0147***	(.2001)
N prior adult conviction	-.0068*	(-.0221)	-.0066**	(-.0238)	-.0033	(-.0016)	-.0026	(-.0026)
N prior adult chg. conv.	-.0047	(-.0403)	-.0076***	(-.0778)	-.0091***	(-.0995)	-.0077***	(-.0925)
N charges past 5 years	.0109***	(.1195)	.0108***	(.1403)	.0090***	(.1258)	.0089***	(.1368)
N prior Part 1 charges	.0131***	(.0324)	.0087**	(.0250)	.0091***	(.0407)	.0095***	(.0523)
N prior property conv.	.0002	(.0032)	.0030	(.0284)	.0024	(.0308)	.0014	(.0146)
N prior persons conv.	-.0017	(-.0057)	-.0022	(-.0085)	.0002	(.0009)	-.0006	(-.0026)
N prior weapons conv.	.0247*	(.0187)	.0197*	(.0176)	.0197*	(.0190)	.0192*	(.0203)
Off street last 2 years	.0643***	(.0636)	.0736***	(.0859)	.0759***	(.0955)	.0693***	(.0957)
Delinquent Career/Onset								
N arrests as juvenile	.0168***	(.0801)	.0146***	(.0816)	.0112***	(.0704)	.0125***	(.0859)
N charges as juvenile	-.0050	(.0129)	-.0019	(.0253)	-.0065	(.0288)	-.0141	(.0222)
Age at first arrest	.0024*	(.0601)	.0034***	(.0796)	.0029***	(.0775)	.0024***	(.0732)
Yrs since first incarc.	.0008**	(.0636)	.0010***	(.0830)	.0010***	(.0848)	.0009***	(.0779)
Yrs since first drug use	-.0004	(-.0124)	-.0004	(-.0147)	-.0004	(-.0136)	-.0004*	(-.0173)
Prior CJS-Offender Action								
N prior incarcerations	.0062	(.0298)	.0040	(.0224)	.0014	(.0085)	-.0011	(-.0071)
N prior parole revokes	.0054	(.0056)	-.0014	(-.0018)	-.0054	(-.0071)	-.0005	(-.0007)
Bad conduct last probat.	.0223	(.0189)	.0198*	(.0198)	.0204*	(.0221)	.0136	(.0162)
Recent parole revoked	.0571*	(.0393)	.0471**	(.0428)	.0421**	(.0406)	.0461**	(.0473)
General Control Variables								
Offender age at sent.	-.0032**	(-.1635)	-.0043***	(-.2117)	-.0045***	(-.2227)	-.0041***	(-.2205)
Off. born out of state	-.0320***	(-.0356)	-.0402***	(-.0515)	-.0428***	(-.0590)	-.0412***	(-.0624)
Coder prob. prognosis	-.0003	(-.0158)	-.0005***	(-.0326)	-.0004***	(-.0305)	-.0004***	(-.0345)

*p<.05 **p<.01 ***p<.001

Table B6.16 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0455*	(-.0196)	-.0270	(-.0137)	-.0293*	(-.0161)	-.0184	(-.0111)
Black x prior adult arrs	-.0117***	(-.0539)	-.0092***	(-.0504)	-.0088***	(-.0517)	-.0091***	(-.0585)
Black x n prior prop cnv	.0106***	(.0512)	.0080***	(.0453)	.0087***	(.0535)	.0082***	(.0554)
Black x n charges as juv	.0371*	(.0210)	.0472***	(.0316)	.0600***	(.0432)	.0641***	(.0507)
Female x Part 1 charges	.0202*	(.0320)	.0184**	(.0342)	.0189**	(.0380)	.0160**	(.0353)
Off. age x drug problem	-.0052**	(-.0445)	-.0046***	(-.0465)	-.0042***	(-.0458)	-.0031**	(-.0377)
Off. age x prior trtment	-.0048*	(-.0369)	-.0053***	(-.0475)	-.0047***	(-.0460)	-.0051***	(-.0546)
Off. age x unemployed	-.0018	(-.0175)	-.0022**	(-.0253)	-.0020**	(-.0244)	-.0016**	(-.0216)
Off. age x PO property	-.0052***	(-.0520)	-.0044***	(-.0517)	-.0041***	(-.0520)	-.0038***	(-.0530)
Off. age x chg pst 5 yrs	-.0001	(-.0100)	-.0003**	(-.0345)	-.0004***	(-.0498)	-.0004***	(-.0504)
PO viol x has detainers	-.0606	(-.0129)	-.1115***	(-.0281)	-.1027***	(-.0279)	-.0748***	(-.0223)
PO prop x n adl.arrests	.0089**	(.0408)	.0099***	(.0536)	.0094***	(.0547)	.0113***	(.0721)
PO prop x prior prop con	-.0111***	(-.0528)	-.0099***	(-.0555)	-.0091***	(-.0552)	-.0095***	(-.0636)
PO prop x n juv. arrests	-.0054	(-.0146)	-.0049	(-.0156)	-.0028	(-.0096)	-.0033	(-.0123)
PO prop x age at 1st arr	.0031	(.0253)	.0018	(.0170)	.0019	(.0198)	.0019	(.0217)
PO prop x yrs. 1st incar	.0012**	(.0302)	.0009**	(.0271)	.0008*	(.0232)	.0007*	(.0224)
PO drugs x n adl. convs.	.0164***	(.0523)	.0165***	(.0622)	.0132***	(.0535)	.0099***	(.0442)
PO drugs x Part 1 chgs.	-.0378***	(-.0800)	-.0278***	(-.0694)	-.0203***	(-.0547)	-.0161***	(-.0476)
PO drugs x last par. rev	.0835	(.0181)	.1012*	(.0259)	.0861*	(.0238)	.0854*	(.0259)
Selection Hazards								
UCR to SAC arrest histry	-.0288*	(-.0242)	-.0181	(-.0180)	-.0165	(-.0176)	-.0118	(-.0139)
Case proceeds past arrst	.2093***	(.0518)	.1211**	(.0353)	.0925*	(.0291)	.0799*	(.0276)
Case to Grand Jury	.0001	(.0001)	.0056	(.0060)	.0024	(.0028)	-.0002	(-.0002)
Case to Superior Court	.0562	(.0195)	.0250	(.0102)	.0043	(.0019)	.0000	(.0000)
Superior Court Convict.	-.0235	(-.0123)	-.0359*	(-.0221)	-.0213	(-.0141)	-.0127	(-.0093)
Match over data sources	-.0431	(-.0136)	.0228	(.0085)	.0209	(.0084)	.0224	(.0099)
Constant	.1704***	(-.0058)	.1617***	(-.0118)	.1540***	(-.0149)	.1344***	(-.0141)
R squared	.197		.320		.354		.371	
Adjusted R squared	.192		.316		.350		.367	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.17

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0281*	(.0219)	.0289**	(.0252)	.0291**	(.0270)	.0266**	(.0266)
Has job after sentence	-.0492***	(-.0399)	-.0358***	(-.0325)	-.0273**	(-.0264)	-.0246**	(-.0256)
Offender is on welfare	-.0369**	(-.0249)	-.0234*	(-.0177)	-.0238*	(-.0192)	-.0195*	(-.0169)
Offender is Black	.0515***	(.0334)	.0800***	(.0657)	.0931***	(.0809)	.1040***	(.1008)
Offender is Hispanic	.0548*	(.0231)	.0767***	(.0363)	.0812***	(.0410)	.0864***	(.0469)
Offender is female	-.0356	(-.0183)	-.0471**	(-.0272)	-.0406**	(-.0250)	-.0466***	(-.0309)
Lives in urban area	.0159	(.0128)	.0227*	(.0204)	.0283**	(.0272)	.0262**	(.0271)
Years at current address	-.0011*	(-.0190)	-.0011*	(-.0205)	-.0009*	(-.0187)	-.0006	(-.0122)
History of drug problems	.0124	(.0086)	.0234	(.0182)	.0317*	(.0263)	.0238*	(.0212)
Treated for drugs/alch.	-.0243	(-.0150)	.0001	(.0001)	.0226	(.0166)	.0324**	(.0257)
Has needle marks	.0181	(.0075)	.0301	(.0140)	.0412*	(.0204)	.0464**	(.0247)
Not a school drop out	-.0224*	(-.0177)	-.0210*	(-.0186)	-.0191*	(-.0180)	-.0134	(-.0136)
Doesn't live with family	-.0097	(-.0065)	-.0129	(-.0096)	-.0297**	(-.0236)	-.0259**	(-.0222)
Commited PO with group	-.0240*	(-.0195)	-.0134	(-.0122)	-.0080	(-.0077)	-.0089	(-.0093)
Victim was a stranger	.0213	(.0153)	.0248*	(.0199)	.0284**	(.0243)	.0295**	(.0272)
Presenting Offense								
PO property crime	.0211	(.0169)	.0289*	(.0259)	.0342**	(.0326)	.0394***	(.0404)
PO crime against person	-.0283	(-.0231)	.0039	(-.0059)	.0067	(-.0032)	.0088	(-.0001)
PO drug offense	.0098	(.0101)	.0026	(.0086)	-.0011	(.0043)	.0108	(.0148)
PO Wolfgang severity	-.0017*	(-.0216)	-.0012	(-.0163)	-.0005	(-.0069)	-.0003	(-.0051)
Has detainers at arrest	-.0127	(-.0141)	.0326	(.0020)	.0323	(-.0003)	.0198	(-.0038)
Has pending charges	.0716***	(.0465)	.0811***	(.0590)	.0735***	(.0570)	.0663***	(.0554)
On probation at PO	.0668***	(.0290)	.0449**	(.0208)	.0447**	(.0191)	.0325*	(.0147)
Anamnestic Theory								
N prior adult arrests	.0203***	(.1120)	.0250***	(.1807)	.0235***	(.1834)	.0199***	(.1797)
N prior adult conviction	-.0126**	(-.0403)	-.0138***	(-.0515)	-.0082**	(-.0239)	-.0060*	(-.0176)
N prior adult chg. conv.	-.0084*	(-.0544)	-.0109***	(-.0788)	-.0116***	(-.0893)	-.0089***	(-.0737)
N charges past 5 years	.0148***	(.1214)	.0139***	(.1273)	.0117***	(.1143)	.0118***	(.1242)
N prior Part 1 charges	.0217***	(.0489)	.0126**	(.0202)	.0124**	(.0323)	.0116**	(.0370)
N prior property conv.	-.0008	(.0080)	.0045	(.0415)	.0039	(.0440)	.0035	(.0351)
N prior persons conv.	-.0020	(-.0050)	-.0016	(-.0043)	.0016	(.0046)	.0019	(.0059)
N prior weapons conv.	.0328*	(.0185)	.0350**	(.0221)	.0323**	(.0218)	.0369***	(.0267)
Off street last 2 years	.0884***	(.0653)	.1059***	(.0876)	.1099***	(.0970)	.1031***	(.0979)
Delinquent Career/Onset								
N arrests as juvenile	.0206***	(.0787)	.0185***	(.0782)	.0145***	(.0671)	.0170***	(.0829)
N charges as juvenile	-.0118	(.0064)	-.0061	(.0191)	-.0030	(.0298)	-.0126	(.0246)
Age at first arrest	.0022	(.0546)	.0038**	(.0697)	.0035**	(.0712)	.0030**	(.0683)
Yrs since first incarc.	.0010**	(.0659)	.0014***	(.0840)	.0014***	(.0855)	.0013***	(.0785)
Yrs since first drug use	-.0008	(-.0165)	-.0008*	(-.0193)	-.0007*	(-.0174)	-.0006*	(-.0169)
Prior CJS-Offender Action								
N prior incarcerations	.0099	(.0354)	.0089*	(.0354)	.0057	(.0242)	-.0002	(-.0010)
N prior parole revokes	.0128	(.0099)	-.0003	(-.0003)	-.0097	(-.0090)	-.0030	(-.0030)
Bad conduct last probat.	.0283	(.0179)	.0333**	(.0237)	.0307**	(.0232)	.0168	(.0137)
Recent parole revoked	.0772*	(.0370)	.0627*	(.0416)	.0607**	(.0392)	.0694***	(.0458)
General Control Variables								
Offender age at sent.	-.0034*	(-.1537)	-.0051***	(-.1978)	-.0056***	(-.2085)	-.0053***	(-.2073)
Off. born out of state	-.0421***	(-.0341)	-.0573***	(-.0520)	-.0635***	(-.0614)	-.0590***	(-.0614)
Coder prob. prognosis	-.0004	(-.0163)	-.0007***	(-.0356)	-.0006***	(-.0326)	-.0007***	(-.0392)

*p<.05 **p<.01 ***p<.001

Table B6.17 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0506	(-.0163)	-.0372	(-.0134)	-.0457*	(-.0176)	-.0338	(-.0140)
Black x prior adult arrs	-.0175***	(-.0603)	-.0147***	(-.0568)	-.0133***	(-.0547)	-.0136***	(-.0604)
Black x n prior prop cnv	.0158***	(.0570)	.0122***	(.0492)	.0121***	(.0520)	.0114***	(.0528)
Black x n charges as juv	.0442*	(.0187)	.0594***	(.0281)	.0738***	(.0373)	.0799***	(.0434)
Female x Part 1 charges	.0241*	(.0285)	.0178	(.0236)	.0204*	(.0288)	.0180*	(.0274)
Off. age x drug problem	-.0061*	(-.0391)	-.0055**	(-.0397)	-.0053**	(-.0406)	-.0036*	(-.0295)
Off. age x prior trtment	-.0074**	(-.0420)	-.0088***	(-.0560)	-.0075***	(-.0510)	-.0084***	(-.0612)
Off. age x unemployed	-.0030*	(-.0218)	-.0034**	(-.0275)	-.0028**	(-.0244)	-.0023*	(-.0212)
Off. age x PO property	-.0074***	(-.0548)	-.0063***	(-.0522)	-.0060***	(-.0530)	-.0057***	(-.0539)
Off. age x chg pst 5 yrs	-.0002	(-.0167)	-.0005***	(-.0430)	-.0006***	(-.0536)	-.0005***	(-.0522)
PO viol x has detainers	-.1138*	(-.0182)	-.1872***	(-.0334)	-.1671***	(-.0318)	-.1378***	(-.0282)
PO prop x n adl.arrests	.0084	(.0288)	.0114**	(.0437)	.0108**	(.0443)	.0151***	(.0665)
PO prop x prior prop con	-.0123**	(-.0438)	-.0117***	(-.0469)	-.0102**	(-.0432)	-.0117***	(-.0536)
PO prop x n juv. arrests	-.0033	(-.0066)	-.0036	(-.0082)	-.0019	(-.0045)	-.0031	(-.0081)
PO prop x age at 1st arr	.0055*	(.0332)	.0029	(.0199)	.0033	(.0242)	.0033*	(.0256)
PO prop x yrs. 1st incar	.0018**	(.0335)	.0015**	(.0296)	.0012**	(.0268)	.0010*	(.0226)
PO drugs x n adl. convs.	.0232***	(.0553)	.0240***	(.0640)	.0197***	(.0562)	.0153***	(.0468)
PO drugs x Part 1 chgs.	-.0484***	(-.0764)	-.0415***	(-.0735)	-.0315***	(-.0594)	-.0252***	(-.0511)
PO drugs x last par. rev	.0839	(.0136)	.1467*	(.0266)	.1068*	(.0207)	.1016*	(.0211)
Selection Hazards								
UCR to SAC arrest histry	-.0315	(-.0198)	-.0250	(-.0176)	-.0204	(-.0153)	-.0093	(-.0075)
Case proceeds past arrst	.2479**	(.0458)	.1263	(.0261)	.0856	(.0189)	.0573	(.0136)
Case to Grand Jury	-.0053	(-.0036)	.0003	(.0002)	-.0069	(-.0056)	-.0109	(-.0095)
Case to Superior Court	.0733	(.0190)	.0217	(.0063)	-.0064	(-.0020)	-.0075	(-.0025)
Superior Court Convict.	-.0448	(-.0175)	-.0540*	(-.0236)	-.0363	(-.0169)	-.0179	(-.0089)
Match over data sources	-.0867	(-.0205)	-.0035	(-.0009)	.0030	(.0009)	.0225	(.0068)
Constant	.2326***	(-.0076)	.2503***	(-.0151)	.2509***	(-.0174)	.2204***	(-.0154)
R squared	.196		.311		.346		.361	
Adjusted R squared	.191		.307		.342		.357	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.18

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0074	(.0127)	.0122*	(.0245)	.0135**	(.0292)	.0125**	(.0307)
Has job after sentence	-.0169**	(-.0303)	-.0117*	(-.0244)	-.0114**	(-.0257)	-.0094*	(-.0240)
Offender is on welfare	-.0016	(-.0024)	-.0031	(-.0054)	-.0045	(-.0084)	-.0057	(-.0120)
Offender is Black	.0255***	(.0405)	.0381***	(.0714)	.0361***	(.0723)	.0355***	(.0807)
Offender is Hispanic	.0210	(.0196)	.0288**	(.0313)	.0273***	(.0321)	.0251***	(.0335)
Offender is female	-.0346***	(-.0395)	-.0347***	(-.0459)	-.0340***	(-.0488)	-.0325***	(-.0528)
Lives in urban area	.0115*	(.0205)	.0100*	(.0206)	.0090*	(.0202)	.0083*	(.0209)
Years at current address	-.0003	(-.0125)	-.0004*	(-.0188)	-.0004*	(-.0190)	-.0002	(-.0119)
History of drug problems	.0047	(.0072)	.0022	(.0039)	.0062	(.0120)	-.0038	(-.0083)
Treated for drugs/alch.	-.0114	(-.0155)	-.0062	(-.0099)	-.0057	(-.0099)	-.0009	(-.0017)
Has needle marks	-.0100	(-.0092)	-.0192*	(-.0205)	-.0171*	(-.0197)	-.0053	(-.0069)
Not a school drop out	.0013	(.0023)	-.0017	(-.0035)	-.0053	(-.0115)	-.0044	(-.0108)
Doesn't live with family	-.0049	(-.0073)	-.0049	(-.0083)	-.0048	(-.0088)	-.0038	(-.0079)
Commited PO with group	-.0020	(-.0036)	.0027	(.0057)	.0019	(.0043)	.0017	(.0044)
Victim was a stranger	.0042	(.0066)	.0021	(.0038)	.0071	(.0142)	.0063	(.0142)
Presenting Offense								
PO property crime	.0003	(.0005)	.0003	(.0006)	.0012	(.0027)	.0032	(.0081)
PO crime against person	.0201*	(.0246)	.0249**	(.0362)	.0244***	(.0387)	.0204**	(.0365)
PO drug offense	.0089	(.0090)	.0016	(-.0024)	-.0019	(-.0082)	.0005	(-.0026)
PO Wolfgang severity	.0000	(.0011)	.0001	(.0030)	.0003	(.0118)	.0004	(.0144)
Has detainers at arrest	-.0110	(-.0177)	-.0205*	(-.0310)	-.0137	(-.0252)	-.0063	(-.0175)
Has pending charges	.0146*	(.0210)	.0128*	(.0214)	.0147**	(.0266)	.0169***	(.0346)
On probation at PO	-.0054	(-.0167)	-.0002	(-.0136)	.0029	(-.0105)	.0053	(-.0063)
Anamnestic Theory								
N prior adult arrests	.0067**	(.0739)	.0067***	(.0931)	.0059***	(.0842)	.0038**	(.0658)
N prior adult conviction	-.0080***	(-.0913)	-.0067***	(-.0833)	-.0059***	(-.0813)	-.0051***	(-.0773)
N prior adult chg. conv.	-.0009	(-.0133)	-.0009	(-.0152)	-.0013	(-.0241)	-.0003	(-.0057)
N charges past 5 years	.0050***	(.0899)	.0050***	(.1062)	.0040***	(.0906)	.0038***	(.0983)
N prior Part 1 charges	.0055*	(.0240)	.0013	(-.0165)	.0038*	(.0139)	.0029	(.0079)
N prior property conv.	-.0042*	(-.0575)	-.0021	(-.0382)	-.0023	(-.0439)	-.0015	(-.0390)
N prior persons conv.	.0162***	(.0874)	.0162***	(.1017)	.0173***	(.1176)	.0164***	(.1266)
N prior weapons conv.	.0216**	(.0269)	.0149*	(.0215)	.0196***	(.0307)	.0233***	(.0413)
Off street last 2 years	.0196**	(.0320)	.0249***	(.0472)	.0242***	(.0498)	.0254***	(.0591)
Delinquent Career/Onset								
N arrests as juvenile	.0097***	(.0790)	.0070***	(.0730)	.0068***	(.0760)	.0078***	(.0957)
N charges as juvenile	-.0071	(.0054)	-.0082	(.0262)	-.0011	(.0406)	-.0063	(.0322)
Age at first arrest	.0007	(.0243)	.0014*	(.0444)	.0016**	(.0574)	.0012**	(.0581)
Yrs since first incarc.	.0002	(.0225)	.0003*	(.0375)	.0003*	(.0411)	.0003*	(.0361)
Yrs since first drug use	-.0002	(-.0076)	-.0003	(-.0163)	-.0002	(-.0135)	-.0003	(-.0177)
Prior CJS-Offender Action								
N prior incarcerations	.0052*	(.0408)	.0066**	(.0608)	.0059**	(.0590)	.0037*	(.0420)
N prior parole revokes	-.0068	(-.0116)	-.0162**	(-.0322)	-.0146**	(-.0314)	-.0128**	(-.0310)
Bad conduct last probat.	.0128	(.0179)	.0138*	(.0224)	.0101	(.0179)	.0053	(.0106)
Recent parole revoked	.0536***	(.0351)	.0551***	(.0425)	.0538***	(.0470)	.0499***	(.0522)
General Control Variables								
Offender age at sent.	-.0011	(-.0706)	-.0019***	(-.1134)	-.0020***	(-.1253)	-.0017***	(-.1334)
Off. born out of state	-.0157**	(-.0281)	-.0176***	(-.0366)	-.0208***	(-.0470)	-.0169***	(-.0431)
Coder prob. prognosis	.0000	(-.0036)	-.0002	(-.0197)	-.0002*	(-.0207)	-.0002*	(-.0263)

*p<.05 **p<.01 ***p<.001

Table B6.18 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0143	(-.0102)	-.0182	(-.0151)	-.0199*	(-.0178)	-.0191*	(-.0194)
Black x prior adult arrs	-.0058**	(-.0445)	-.0061***	(-.0540)	-.0066***	(-.0631)	-.0064***	(-.0699)
Black x n prior prop cnv	.0037*	(.0293)	.0041**	(.0374)	.0046***	(.0460)	.0042***	(.0479)
Black x n charges as juv	.0228*	(.0213)	.0461***	(.0501)	.0415***	(.0489)	.0418***	(.0557)
Female x Part 1 charges	-.0095	(-.0249)	-.0068	(-.0207)	-.0057	(-.0188)	-.0044	(-.0163)
Off. age x drug problem	-.0007	(-.0092)	-.0006	(-.0105)	-.0007	(-.0128)	-.0007	(-.0134)
Off. age x prior trtment	-.0009	(-.0109)	-.0013	(-.0185)	-.0011	(-.0182)	-.0014	(-.0247)
Off. age x unemployed	-.0013*	(-.0202)	-.0013*	(-.0247)	-.0013**	(-.0258)	-.0011**	(-.0261)
Off. age x PO property	-.0006	(-.0102)	-.0004	(-.0072)	-.0003	(-.0068)	-.0007	(-.0156)
Off. age x chg pst 5 yrs	-.0002**	(-.0371)	-.0003***	(-.0556)	-.0002***	(-.0484)	-.0002***	(-.0485)
PO viol x has detainers	-.0458*	(-.0161)	-.0484*	(-.0198)	-.0448**	(-.0199)	-.0386**	(-.0194)
PO prop x n adl.arrests	.0016	(.0118)	.0028	(.0249)	.0033*	(.0315)	.0049***	(.0533)
PO prop x prior prop con	-.0024	(-.0190)	-.0043**	(-.0393)	-.0047**	(-.0470)	-.0051***	(-.0574)
PO prop x n juv. arrests	-.0023	(-.0103)	-.0002	(-.0013)	-.0003	(-.0016)	-.0010	(-.0061)
PO prop x age at 1st arr	.0004	(.0060)	.0001	(.0010)	.0002	(.0040)	.0007	(.0142)
PO prop x yrs. 1st incar	.0002	(.0064)	.0001	(.0069)	.0002	(.0086)	.0001	(.0047)
PO drugs x n adl. convs.	.0026	(.0140)	.0041	(.0250)	.0031	(.0206)	.0030	(.0225)
PO drugs x Part 1 chgs.	-.0063	(-.0220)	-.0094*	(-.0383)	-.0078*	(-.0341)	-.0074*	(-.0367)
PO drugs x last par. rev	-.0511	(-.0183)	-.0504	(-.0210)	-.0412	(-.0186)	-.0279	(-.0143)
Selection Hazards								
UCR to SAC arrest histry	-.0014	(-.0019)	-.0021	(-.0034)	-.0003	(-.0006)	-.0014	(-.0029)
Case proceeds past arrst	.0617	(.0252)	.0431	(.0205)	.0301	(.0155)	.0157	(.0091)
Case to Grand Jury	.0030	(.0045)	.0044	(.0078)	.0055	(.0105)	.0036	(.0078)
Case to Superior Court	.0338	(.0194)	.0390*	(.0260)	.0189	(.0136)	.0120	(.0098)
Superior Court Convict.	-.0194	(-.0167)	-.0164	(-.0165)	-.0118	(-.0128)	-.0059	(-.0073)
Match over data sources	-.0300	(-.0157)	-.0258	(-.0156)	-.0088	(-.0058)	.0082	(.0061)
Constant	.0393*	(-.0100)	.0374**	(-.0162)	.0451***	(-.0133)	.0395***	(-.0126)
R squared	.089		.148		.174		.194	
Adjusted R squared	.084		.143		.169		.190	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.19

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.5078*	(.0218)	1.1170**	(.0283)	1.7692***	(.0336)	1.9413**	(.0265)
Has job after sentence	-.6589**	(-.0296)	-.8658*	(-.0229)	-1.0901*	(-.0216)	-1.4623*	(-.0208)
Offender is on welfare	-.2752	(-.0103)	-.4795	(-.0106)	-.6046	(-.0100)	-.5883	(-.0070)
Offender is Black	.8379**	(.0335)	2.8945***	(.0678)	4.3086***	(.0771)	7.4170***	(.0985)
Offender is Hispanic	.8603*	(.0201)	2.6247***	(.0362)	4.1133***	(.0425)	7.3006***	(.0542)
Offender is female	-1.0847**	(-.0309)	-2.5776***	(-.0433)	-2.9988***	(-.0378)	-4.9647***	(-.0450)
Lives in urban area	.1969	(.0088)	.8259*	(.0217)	1.6117***	(.0317)	2.0454**	(.0289)
Years at current address	-.0210*	(-.0195)	-.0447**	(-.0245)	-.0545*	(-.0224)	-.0296	(-.0087)
History of drug problems	-.2309	(-.0089)	.1111	(.0025)	.8535	(.0145)	-.0718	(-.0009)
Treated for drugs/alch.	-.4982	(-.0170)	-.2186	(-.0044)	.6319	(.0095)	2.0895*	(.0226)
Has needle marks	.5049	(.0116)	1.0423	(.0141)	1.6045	(.0162)	2.8465*	(.0207)
Not a school drop out	-.2241	(-.0098)	-.4209	(-.0108)	-.6085	(-.0117)	-.8478	(-.0117)
Doesn't live with family	-.0391	(-.0014)	-.2540	(-.0055)	-1.3131*	(-.0214)	-1.6884*	(-.0198)
Commited PO with group	-.2031	(-.0091)	.1470	(.0039)	.1634	(.0032)	-.2351	(-.0034)
Victim was a stranger	.0074	(.0003)	.6072	(.0142)	.9594	(.0168)	1.8485*	(.0233)
Presenting Offense								
PO property crime	.1240	(.0055)	.2889	(.0075)	.4056	(.0079)	.9549	(.0134)
PO crime against person	-.0734	(-.0091)	.5913	(.0023)	.4981	(-.0024)	-.0829	(-.0119)
PO drug offense	.3924	(.0158)	-.1338	(.0022)	-.3967	(-.0007)	.1682	(.0089)
PO Wolfgang severity	.0017	(.0012)	-.0017	(-.0007)	-.0115	(-.0035)	-.0509	(-.0112)
Has detainers at arrest	.0361	(-.0114)	.8573	(-.0080)	1.6733	(-.0031)	1.4408	(-.0105)
Has pending charges	1.2312***	(.0443)	2.1614***	(.0458)	2.8890***	(.0459)	4.0785***	(.0466)
On probation at PO	.4125	(.0079)	.8079	(.0023)	1.0975	(.0035)	1.6239	(.0068)
Anamnestic Theory								
N prior adult arrests	.4413***	(.1245)	1.1016***	(.2162)	1.4903***	(.2237)	1.9161***	(.2265)
N prior adult conviction	-.2971***	(-.0714)	-.4718***	(-.0579)	-.3842*	(-.0285)	-.3796	(-.0113)
N prior adult chg. conv.	-.1759*	(-.0627)	-.4628***	(-.0972)	-.7744***	(-.1219)	-.9224***	(-.1044)
N charges past 5 years	.2004***	(.0908)	.4145***	(.1107)	.5192***	(.1039)	.8153***	(.1173)
N prior Part 1 charges	.5065***	(.0743)	.2584	(-.0052)	.5449**	(.0391)	.5183	(.0247)
N prior property conv.	-.1186	(-.0269)	-.0415	(.0030)	-.1564	(-.0065)	-.1752	(-.0172)
N prior persons conv.	.0827	(.0112)	.1353	(.0108)	.2535	(.0152)	.2549	(.0110)
N prior weapons conv.	.3951	(.0123)	1.1204*	(.0206)	.7921	(.0109)	2.1074*	(.0209)
Off street last 2 years	1.1925***	(.0487)	2.9207***	(.0704)	4.3109***	(.0778)	6.3370***	(.0822)
Delinquent Career/Onset								
N arrests as juvenile	.3067***	(.0620)	.5678***	(.0681)	.7297***	(.0618)	.9743***	(.0605)
N charges as juvenile	-.0827	(.0113)	-.5741	(.0129)	-.2369	(.0249)	-.3774	(.0229)
Age at first arrest	.0247	(.0386)	.1243**	(.0548)	.1584**	(.0567)	.1450	(.0421)
Yrs since first incarc.	.0045	(.0343)	.0380**	(.0600)	.0636***	(.0724)	.0742***	(.0585)
Yrs since first drug use	-.0029	(-.0034)	-.0232	(-.0164)	-.0476**	(-.0253)	-.0571*	(-.0218)
Prior CJS-Offender Action								
N prior incarcerations	.2032*	(.0401)	.3207*	(.0373)	.1602	(.0140)	-.2413	(-.0151)
N prior parole revokes	.2713	(.0116)	-.0256	(-.0006)	-.1539	(-.0029)	-1.0585	(-.0144)
Bad conduct last probat.	.4791	(.0168)	1.2606**	(.0260)	1.2209*	(.0189)	1.3885	(.0155)
Recent parole revoked	.8958	(.0208)	.1680	(.0138)	-.3590	(.0098)	.5903	(.0192)
General Control Variables								
Offender age at sent.	-.0469	(-.1126)	-.1402***	(-.1497)	-.2238***	(-.1718)	-.3304***	(-.1833)
Off. born out of state	-.6819**	(-.0306)	-2.0218***	(-.0534)	-2.8163***	(-.0557)	-3.8559***	(-.0549)
Coder prob. prognosis	-.0042	(-.0106)	-.0194**	(-.0289)	-.0263**	(-.0294)	-.0417***	(-.0335)

*p<.05 **p<.01 ***p<.001

Table B6.19 (continued)

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.4375	(-.0078)	-1.5959*	(-.0168)	-2.0088	(-.0158)	-2.3497	(-.0133)
Black x prior adult arrs	-.3114***	(-.0595)	-.5246***	(-.0591)	-.7276***	(-.0614)	-.9196***	(-.0558)
Black x n prior prop cnv	.2710***	(.0539)	.4891***	(.0573)	.6717***	(.0590)	.7884***	(.0498)
Black x n charges as juv	.7377	(.0173)	2.3693***	(.0327)	3.2711***	(.0338)	4.3539***	(.0324)
Female x Part 1 charges	.0050	(.0003)	.1187	(.0046)	.9906*	(.0286)	1.3486*	(.0280)
Off. age x drug problem	-.0574	(-.0205)	-.1302	(-.0273)	-.2342*	(-.0368)	-.3473**	(-.0393)
Off. age x prior trtment	-.1092*	(-.0345)	-.2132**	(-.0397)	-.2407*	(-.0336)	-.4571**	(-.0458)
Off. age x unemployed	-.0415	(-.0165)	-.0807*	(-.0190)	-.1134*	(-.0199)	-.1406*	(-.0178)
Off. age x PO property	-.1001**	(-.0410)	-.1662**	(-.0401)	-.2576***	(-.0466)	-.3677***	(-.0478)
Off. age x chg pst 5 yrs	-.0087**	(-.0364)	-.0205***	(-.0507)	-.0285***	(-.0529)	-.0384***	(-.0512)
PO viol x has detainers	-2.7786**	(-.0245)	-7.4235***	(-.0386)	-10.0532***	(-.0392)	-14.8448***	(-.0416)
PO prop x n adl.arrests	.0451	(.0085)	.1984	(.0221)	.3522*	(.0294)	.8310***	(.0499)
PO prop x prior prop con	-.1644*	(-.0324)	-.3922**	(-.0455)	-.4278**	(-.0372)	-.7485**	(-.0468)
PO prop x n juv. arrests	-.0814	(-.0091)	-.1412	(-.0093)	-.2754	(-.0135)	-.3289	(-.0116)
PO prop x age at 1st arr	.0778	(.0261)	.0291	(.0058)	.0722	(.0107)	.1200	(.0128)
PO prop x yrs. 1st incar	.0301**	(.0302)	.0292	(.0172)	.0409	(.0181)	.0394	(.0125)
PO drugs x n adl. convs.	.2882**	(.0380)	.6650***	(.0517)	.7517**	(.0438)	1.1276***	(.0473)
PO drugs x Part 1 chgs.	-.6229***	(-.0544)	-1.3384***	(-.0689)	-.9690*	(-.0374)	-1.2486*	(-.0346)
PO drugs x last par. rev	.3721	(.0033)	3.9817	(.0210)	6.0141*	(.0238)	9.6372*	(.0274)
Selection Hazards								
UCR to SAC arrest histry	-.4650	(-.0162)	-1.2313*	(-.0252)	-2.0125**	(-.0309)	-2.7982**	(-.0309)
Case proceeds past arrst	3.5284*	(.0361)	4.9331*	(.0297)	6.6492*	(.0300)	9.2927*	(.0302)
Case to Grand Jury	-.0976	(-.0037)	.5259	(.0117)	1.1586	(.0193)	1.6213	(.0194)
Case to Superior Court	.8952	(.0128)	.9481	(.0080)	-.1080	(-.0007)	-1.0913	(-.0050)
Superior Court Convict.	-.5681	(-.0122)	-.8827	(-.0112)	-.9472	(-.0090)	.4766	(.0033)
Match over data sources	-2.2827*	(-.0298)	-3.6323*	(-.0280)	-3.2320	(-.0186)	-2.3963	(-.0099)
Constant	3.1763***	(-.0086)	6.5333***	(-.0132)	9.7250***	(-.0107)	13.4224***	(-.0102)
R squared	.117		.203		.236		.243	
Adjusted R squared	.112		.198		.232		.238	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.20
Attribution of Explained Variance for
Days to Rearrest after Sentencing - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.148)	(.241)	(.275)	(.296)
Percent R2 Unattributed	34.88	34.50	34.24	33.89
	(.052)	(.083)	(.094)	(.100)
Attributed to Hazards	21.73	25.68	26.23	26.01
	(.032)	(.062)	(.072)	(.077)
Social Variables	5.17	5.57	6.22	7.55
	(.008)	(.013)	(.017)	(.022)
Presenting Offense	3.52	2.64	2.48	2.21
	(.005)	(.006)	(.007)	(.007)
Anamnestic Variables	7.97	7.89	7.91	8.07
	(.012)	(.019)	(.022)	(.024)
Delinquent Career/Onset	3.85	3.69	3.33	2.94
	(.006)	(.009)	(.009)	(.009)
Prior CJS/Offender Actions	.84	.31	.21	.31
	(.001)	(.001)	(.001)	(.001)
General Controls	4.78	4.59	4.97	5.45
	(.007)	(.011)	(.014)	(.016)
Social/Presenting Offense	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Social/Anamnestic Variables	.81	.87	.88	.96
	(.001)	(.002)	(.002)	(.003)
Social/Delinquent Career	1.30	1.52	1.59	1.75
	(.002)	(.004)	(.004)	(.005)
Social/Prior CJS Actions	.22	.11	.06	.00*
	(.000)	(.000)	(.000)	(.000)
Social/General Controls	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Anamnestic	1.60	1.47	1.38	1.32
	(.002)	(.004)	(.004)	(.004)
Presenting Offense/Delinquency	.00*	.00*	.00*	.00*
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Prior CJS	.00*	.24	.27	.32
	(.000)	(.001)	(.001)	(.001)
Presenting Offense/Controls	.11	.22	.29	.35
	(.000)	(.001)	(.001)	(.001)
Anamnestic/Delinquent Career	3.08	2.10	1.71	1.35
	(.005)	(.005)	(.005)	(.004)
Anamnestic/Prior CJS Actions	3.82	2.75	2.17	1.19
	(.006)	(.007)	(.006)	(.004)
Anamnestic/General Controls	4.22	3.28	3.06	2.62
	(.006)	(.008)	(.008)	(.008)
Delinquency/Prior CJS Actions	.98	.51	.29	.00*
	(.001)	(.001)	(.001)	(.000)
Delinquency/General Controls	1.95	2.58	3.18	4.31
	(.003)	(.006)	(.009)	(.013)
Prior CJS Actions/Controls	.03	.15	.16	.19
	(.000)	(.000)	(.000)	(.001)
All Interactions	4.65	3.34	2.84	2.47
	(.007)	(.008)	(.008)	(.007)

* Partitioned variance is negative due to suppressor effect

Table B6.21
Attribution of Explained Variance for
Days to Reimprisonment after Sentencing - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.086)	(.213)	(.269)	(.312)
Percent R2 Unattributed	30.11	31.97	32.49	32.67
	(.026)	(.068)	(.088)	(.102)
Attributed to Hazards	24.10	27.97	29.08	30.81
	(.021)	(.060)	(.078)	(.096)
Social Variables	4.38	3.29	3.39	3.92
	(.004)	(.007)	(.009)	(.012)
Presenting Offense	1.50	1.52	1.32	1.31
	(.001)	(.003)	(.004)	(.004)
Anamnestic Variables	10.16	7.52	6.80	6.02
	(.009)	(.016)	(.018)	(.019)
Delinquent Career/Onset	6.99	6.18	6.55	6.97
	(.006)	(.013)	(.018)	(.022)
Prior CJS/Offender Actions	4.85	2.86	1.93	.95
	(.004)	(.006)	(.005)	(.003)
General Controls	.36	.79	1.07	1.47
	(.000)	(.002)	(.003)	(.005)
Social/Presenting Offense	.22	.27	.28	.29
	(.000)	(.001)	(.001)	(.001)
Social/Anamnestic Variables	.65	.58	.59	.59
	(.001)	(.001)	(.002)	(.002)
Social/Delinquent Career	.86	1.01	1.15	1.35
	(.001)	(.002)	(.003)	(.004)
Social/Prior CJS Actions	.56	.47	.40	.30
	(.000)	(.001)	(.001)	(.001)
Social/General Controls	.44	.58	.61	.55
	(.000)	(.001)	(.002)	(.002)
Presenting Offense/Anamnestic	.99	.93	.79	.77
	(.001)	(.002)	(.002)	(.002)
Presenting Offense/Delinquency	.22	.20	.23	.22
	(.000)	(.000)	(.001)	(.001)
Presenting Offense/Prior CJS	.20	.14	.08	.03
	(.000)	(.000)	(.000)	(.000)
Presenting Offense/Controls	.07	.10	.13	.18
	(.000)	(.000)	(.000)	(.001)
Anamnestic/Delinquent Career	4.25	4.19	3.85	3.40
	(.004)	(.009)	(.010)	(.011)
Anamnestic/Prior CJS Actions	4.84	4.81	4.74	3.82
	(.004)	(.010)	(.013)	(.012)
Anamnestic/General Controls	2.22	2.69	2.70	2.65
	(.002)	(.006)	(.007)	(.008)
Delinquency/Prior CJS Actions	1.86	1.77	1.67	1.38
	(.002)	(.004)	(.005)	(.004)
Delinquency/General Controls	.11	.13	.12	.32
	(.000)	(.000)	(.000)	(.001)
Prior CJS Actions/Controls	.05	.02	.01	.02
	(.000)	(.000)	(.000)	(.000)
All Interactions	4.91	4.60	4.02	3.54
	(.004)	(.010)	(.011)	(.011)

* Partitioned variance is negative due to suppressor effect

Table B6.22

Regression Coefficients for Days to Post-Sentence Rearrest
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	-5.6415**	(-.0257)	-25.1959**	(-.0297)	-43.3762**	(-.0286)	-83.7245**	(-.0272)
Has job after sentence	6.5939**	(.0313)	25.9774***	(.0320)	42.0582**	(.0289)	71.0697**	(.0241)
Offender is on welfare	4.6613*	(.0184)	11.2691	(.0116)	8.9481	(.0051)	-7.3364	(-.0021)
Offender is Black	-10.9644***	(-.0480)	-64.0823***	(-.0704)	-142.062***	(-.0875)	-347.138***	(-.1043)
Offender is Hispanic	-11.8432**	(-.0293)	-63.4307***	(-.0407)	-141.528***	(-.0507)	-348.191***	(-.0615)
Offender is female	7.4523*	(.0225)	39.0317**	(.0305)	87.1262***	(.0381)	228.5834***	(.0493)
Lives in urban area	-1.9199	(-.0091)	-18.7468*	(-.0229)	-37.0558**	(-.0253)	-80.1129**	(-.0269)
Years at current address	.2595**	(.0255)	1.0811**	(.0275)	1.6942**	(.0241)	3.3137**	(.0232)
History of drug problems	-.0486	(-.0002)	-6.8260	(-.0072)	-19.0077	(-.0112)	-60.4497	(-.0176)
Treated for drugs/alch.	.3227	(.0012)	1.4222	(.0013)	-5.0411	(-.0026)	-47.1900	(-.0122)
Has needle marks	-2.1076	(-.0051)	-13.6755	(-.0086)	-37.4624	(-.0132)	-109.282*	(-.0189)
Not a school drop out	5.8209**	(.0269)	32.4169***	(.0388)	60.1405***	(.0402)	133.7790***	(.0441)
Doesn't live with family	-1.8702	(-.0073)	.1555	(.0002)	18.5225	(.0105)	77.0357**	(.0215)
Commited PO with group	4.7951*	(.0228)	18.9624**	(.0234)	38.1471**	(.0262)	73.8181**	(.0250)
Victim was a stranger	-7.0328**	(-.0296)	-30.3583***	(-.0331)	-51.7212***	(-.0315)	-97.9906**	(-.0294)
Presenting Offense								
PO property crime	-2.5681	(-.0120)	-20.1494*	(-.0244)	-43.6173*	(-.0296)	-115.879**	(-.0387)
PO crime against person	6.0142	(.0256)	6.7154	(.0121)	-3.4234	(.0039)	-55.3337	(-.0090)
PO drug offense	-2.0447	(-.0141)	-14.4410	(-.0204)	-19.2895	(-.0165)	-47.7180	(-.0177)
PO Wolfgang severity	.3733**	(.0275)	1.5976**	(.0305)	2.9953***	(.0320)	6.0506***	(.0318)
Has detainers at arrest	6.7742	(.0211)	19.6713	(.0224)	35.1625	(.0228)	71.8948	(.0233)
Has pending charges	-12.7875***	(-.0487)	-58.3141***	(-.0575)	-112.423***	(-.0620)	-220.974***	(-.0600)
On probation at PO	-7.1021*	(-.0202)	-40.7111***	(-.0257)	-81.3707***	(-.0276)	-188.198***	(-.0285)
Anamnestic Theory								
N prior adult arrests	-3.1129***	(-.0949)	-16.7623***	(-.1459)	-34.5656***	(-.1693)	-75.8073***	(-.1813)
N prior adult conviction	1.0356	(.0161)	.5265	(-.0185)	-1.0012	(-.0276)	-12.7030	(-.0514)
N prior adult chg. conv.	1.6965*	(.0641)	8.2988***	(.0812)	16.3416***	(.0893)	30.2494***	(.0815)
N charges past 5 years	-1.9122***	(-.0918)	-6.5678***	(-.0817)	-10.6785***	(-.0742)	-18.6335***	(-.0638)
N prior Part 1 charges	-2.2626*	(-.0212)	-7.3159*	(-.0255)	-10.7147	(-.0238)	-13.7283	(-.0155)
N prior property conv.	-.2914	(-.0215)	-.9704	(-.0227)	-1.9835	(-.0238)	-2.3900	(-.0223)
N prior persons conv.	-.6081	(-.0087)	-2.7392	(-.0102)	-7.7862	(-.0162)	-21.1156	(-.0216)
N prior weapons conv.	-4.8995	(-.0162)	-26.5333**	(-.0227)	-45.5471*	(-.0218)	-99.0949**	(-.0234)
Off street last 2 years	-11.7086***	(-.0507)	-67.5721***	(-.0758)	-129.954***	(-.0814)	-268.184***	(-.0828)
Delinquent Career/Onset								
N arrests as juvenile	-3.3803***	(-.0555)	-14.7040***	(-.0661)	-24.8048***	(-.0622)	-48.4402***	(-.0582)
N charges as juvenile	-2.8364	(-.0208)	-10.6243	(-.0292)	-25.8497	(-.0303)	-77.8312*	(-.0323)
Age at first arrest	-.5029*	(-.0420)	-2.0317*	(-.0352)	-3.3270*	(-.0262)	-2.9459	(-.0027)
Yrs since first incarc.	-.1938**	(-.0538)	-.9745***	(-.0661)	-2.0803***	(-.0730)	-4.2408***	(-.0716)
Yrs since first drug use	.1111	(.0142)	.5636*	(.0186)	.8735	(.0161)	1.6060	(.0146)
Prior CJS-Offender Action								
N prior incarcerations	-1.3017	(-.0272)	.1728	(.0009)	4.3069	(.0130)	30.2688**	(.0452)
N prior parole revokes	-3.0393	(-.0137)	.6801	(.0008)	.6900	(.0005)	12.0551	(.0039)
Bad conduct last probat.	-4.8429	(-.0180)	-26.8417**	(-.0258)	-40.2945*	(-.0217)	-40.4063	(-.0107)
Recent parole revoked	-6.2154	(-.0270)	-12.9309	(-.0192)	-.8779	(-.0119)	4.2060	(-.0081)
General Control Variables								
Offender age at sent.	.9322***	(.1619)	4.1111***	(.1856)	8.5622***	(.1959)	18.1948***	(.1902)
Off. born out of state	9.7576***	(.0463)	53.0751***	(.0653)	110.4694***	(.0759)	265.8978***	(.0901)
Coder prob. prognosis	.0823*	(.0220)	.4937***	(.0343)	1.0339***	(.0401)	2.2788***	(.0436)

*p<.05 **p<.01 ***p<.001

Table B6.22 (continued)

Regression Coefficients for Days to Post-Sentence Rearrest
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	4.0684	(.0077)	33.3958*	(.0163)	71.3252*	(.0195)	189.8363**	(.0256)
Black x prior adult arrs	2.1477**	(.0435)	8.3960***	(.0440)	15.2972***	(.0448)	33.6923***	(.0486)
Black x n prior prop cnv	-2.3588***	(-.0497)	-7.8716***	(-.0430)	-12.2579**	(-.0374)	-25.4528**	(-.0383)
Black x n charges as juv	-3.0434	(-.0075)	-27.2210*	(-.0175)	-36.7019	(-.0132)	-28.9352	(-.0051)
Female x Part 1 charges	-2.7555	(-.0191)	-17.6933*	(-.0317)	-36.6215**	(-.0367)	-76.2117**	(-.0376)
Off. age x drug problem	.9267*	(.0349)	3.6739*	(.0359)	5.7204*	(.0312)	7.9075	(.0213)
Off. age x prior trtment	1.0283*	(.0344)	5.4373**	(.0471)	10.2846***	(.0498)	21.3408***	(.0509)
Off. age x unemployed	.4827*	(.0204)	2.1604**	(.0236)	3.5187*	(.0215)	5.3800	(.0162)
Off. age x PO property	.8104*	(.0351)	3.4751**	(.0390)	5.8218**	(.0365)	11.0507**	(.0342)
Off. age x chg pst 5 yrs	.0414	(.0184)	.0770	(.0089)	.0868	(.0056)	-.1367	(-.0043)
PO viol x has detainers	11.3045	(.0106)	86.8081**	(.0210)	162.3025**	(.0219)	341.0630**	(.0227)
PO prop x n adl.arrests	-.3817	(-.0076)	-1.7607	(-.0091)	-1.9860	(-.0058)	-3.0655	(-.0044)
PO prop x prior prop con	2.0086**	(.0419)	5.7798*	(.0312)	8.5294	(.0258)	15.1894	(.0226)
PO prop x n juv. arrests	2.6219**	(.0309)	10.0097**	(.0306)	16.9311**	(.0289)	35.5094***	(.0299)
PO prop x age at 1st arr	-.1909	(-.0068)	.3832	(.0035)	2.0221	(.0104)	5.9479	(.0151)
PO prop x yrs. 1st incar	-.1356	(-.0144)	-.5031	(-.0138)	-.6203	(-.0095)	-.9854	(-.0075)
PO drugs x n adl. convs.	-2.3328*	(-.0326)	-11.4777**	(-.0416)	-20.3648**	(-.0412)	-39.0302**	(-.0389)
PO drugs x Part 1 chgs.	6.8247***	(.0631)	20.4818**	(.0491)	31.2350**	(.0418)	52.8136*	(.0349)
PO drugs x last par. rev	-24.7919*	(-.0235)	-85.8454	(-.0211)	-152.913	(-.0210)	-233.092	(-.0158)
Selection Hazards								
UCR to SAC arrest histry	3.8401	(.0141)	21.2756*	(.0203)	38.3699*	(.0204)	57.2509	(.0150)
Case proceeds past arrst	-41.1921**	(-.0446)	-169.404***	(-.0475)	-240.954**	(-.0377)	-405.027*	(-.0313)
Case to Grand Jury	-.8090	(-.0032)	1.5738	(.0016)	6.5299	(.0038)	18.9441	(.0054)
Case to Superior Court	-14.7962*	(-.0225)	-43.6490	(-.0172)	-72.3917	(-.0159)	-139.742	(-.0151)
Superior Court Convict.	.5740	(.0013)	15.7531	(.0093)	24.0308	(.0079)	18.2176	(.0030)
Match over data sources	4.6590	(.0064)	20.0962	(.0072)	4.9273	(.0010)	-30.7189	(-.0030)
Constant	333.7342***	(.0032)	885.1105***	(.0042)	1357.781***	(.0018)	2334.655***	(-.0030)
R squared	.148		.241		.275		.296	
Adjusted R squared	.144		.237		.271		.292	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B6.23

Regression Coefficients for Days to Post-Sentence Reimprisonment
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	1.2199	(.0125)	-7.4993	(-.0151)	-22.9192*	(-.0229)	-57.5560**	(-.0250)
Has job after sentence	3.6839***	(.0393)	21.6352***	(.0453)	40.6753***	(.0424)	87.9727***	(.0398)
Offender is on welfare	2.3990*	(.0213)	15.8823**	(.0277)	35.5067***	(.0309)	74.9015***	(.0283)
Offender is Black	.1258	(.0089)	-16.1786**	(-.0264)	-50.8164***	(-.0441)	-160.186***	(-.0659)
Offender is Hispanic	-2.4372	(-.0136)	-21.5055*	(-.0235)	-50.4313**	(-.0274)	-150.163***	(-.0354)
Offender is female	-1.2030	(-.0082)	-6.7276	(-.0090)	-9.2594	(-.0061)	14.0182	(.0040)
Lives in urban area	.2732	(.0029)	1.9216	(.0040)	2.5419	(.0026)	-18.5244	(-.0083)
Years at current address	.0135	(.0030)	.2202	(.0095)	.6588	(.0142)	1.4348	(.0134)
History of drug problems	-3.6470**	(-.0334)	-12.6429*	(-.0227)	-30.0987*	(-.0269)	-83.5062**	(-.0324)
Treated for drugs/alch.	-1.0514	(-.0085)	-10.9590	(-.0174)	-23.3061	(-.0185)	-45.9008	(-.0158)
Has needle marks	-.0988	(-.0005)	-9.7028	(-.0104)	-31.5308	(-.0168)	-98.1335**	(-.0227)
Not a school drop out	-.1172	(-.0012)	2.3230	(.0047)	9.1112	(.0092)	24.9634	(.0110)
Doesn't live with family	-.4752	(-.0042)	-.2527	(-.0004)	-.4881	(-.0004)	9.6083	(.0036)
Commited PO with group	1.2821	(.0137)	8.6725*	(.0182)	11.9878	(.0125)	27.8321	(.0126)
Victim was a stranger	-2.0335	(-.0192)	-15.2602**	(-.0283)	-30.6779**	(-.0283)	-79.3798***	(-.0318)
Presenting Offense								
PO property crime	-.2953	(-.0031)	-12.3588*	(-.0255)	-26.5823*	(-.0273)	-89.7617***	(-.0400)
PO crime against person	.6681	(.0094)	-2.2029	(-.0008)	-9.7909	(-.0058)	-44.9028	(-.0141)
PO drug offense	1.4412	(.0139)	-1.1491	(-.0020)	-3.1332	(-.0050)	-40.3029	(-.0197)
PO Wolfgang severity	-.0109	(-.0018)	.0000	(.0000)	.0313	(.0005)	1.0645	(.0075)
Has detainers at arrest	-4.6447*	(-.0174)	-24.7453*	(-.0202)	-41.6589*	(-.0169)	-71.3089	(-.0120)
Has pending charges	-3.2097**	(-.0275)	-26.8806***	(-.0451)	-60.0831***	(-.0502)	-142.298***	(-.0516)
On probation at PO	-1.6427	(-.0007)	-6.4653	(.0021)	-22.6225	(-.0035)	-63.3936*	(-.0118)
Anamnestic Theory								
N prior adult arrests	-.2856	(.0211)	-3.6814*	(-.0236)	-9.2206**	(-.0466)	-23.6122**	(-.0652)
N prior adult conviction	1.0724**	(.0697)	5.1751***	(.0568)	7.9542**	(.0378)	11.8422	(.0202)
N prior adult chg. conv.	-.8072**	(-.0685)	-1.4682	(-.0244)	.0740	(.0006)	6.4578	(.0232)
N charges past 5 years	-.7903***	(-.0852)	-4.4817***	(-.0948)	-8.0011***	(-.0843)	-16.7063***	(-.0764)
N prior Part 1 charges	.1271	(.0332)	-3.9103*	(-.0164)	-7.6869*	(-.0173)	-15.1286	(-.0197)
N prior property conv.	.0380	(-.0348)	.6961	(-.0273)	-.7425	(-.0378)	-6.2415	(-.0398)
N prior persons conv.	-.8711*	(-.0281)	-2.1611	(-.0137)	-6.3425	(-.0200)	-20.6290*	(-.0282)
N prior weapons conv.	.3489	(.0026)	-4.9339	(-.0072)	-10.0973	(-.0073)	-17.8978	(-.0056)
Off street last 2 years	-3.7738***	(-.0367)	-37.2619***	(-.0711)	-93.9761***	(-.0893)	-236.511***	(-.0976)
Delinquent Career/Onset								
N arrests as juvenile	-.4518	(-.0422)	-2.6255	(-.0520)	-6.0267*	(-.0559)	-15.6075*	(-.0555)
N charges as juvenile	.8257	(.0081)	-.8524	(-.0238)	-4.1676	(-.0351)	-5.8509	(-.0413)
Age at first arrest	-.1518	(-.0507)	-1.2181*	(-.0765)	-3.4543**	(-.0923)	-8.9686***	(-.0954)
Yrs since first incarc.	-.1401***	(-.0842)	-.6827***	(-.1039)	-1.6955***	(-.1185)	-4.9236***	(-.1370)
Yrs since first drug use	-.0264	(-.0076)	-.0809	(-.0045)	-.0977	(-.0027)	.2741	(.0033)
Prior CJS-Offender Action								
N prior incarcerations	-.8249*	(-.0388)	-5.5985**	(-.0516)	-12.0262**	(-.0552)	-24.7776**	(-.0494)
N prior parole revokes	-1.4477	(-.0147)	-5.2774	(-.0105)	-10.7562	(-.0107)	-14.7770	(-.0064)
Bad conduct last probat.	-1.7346	(-.0145)	-11.7928*	(-.0193)	-26.2095*	(-.0214)	-53.3788*	(-.0189)
Recent parole revoked	-10.5415***	(-.0513)	-66.8500***	(-.0658)	-110.741***	(-.0594)	-169.168***	(-.0455)
General Control Variables								
Offender age at sent.	.0142	(.0419)	.8081	(.1150)	2.5204*	(.1415)	7.6807***	(.1598)
Off. born out of state	-.4517	(-.0048)	3.0859	(.0065)	11.8796	(.0124)	53.8297**	(.0244)
Coder prob. prognosis	.0210	(.0126)	.2001*	(.0236)	.4930**	(.0290)	1.3429***	(.0343)

*p<.05 **p<.01 ***p<.001

Table B6.23 (continued)

Regression Coefficients for Days to Post-Sentence Reimprisonment
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	3.5680	(.0151)	17.5843	(.0146)	42.1194*	(.0174)	70.3179	(.0126)
Black x prior adult arrs	1.2932***	(.0588)	7.1103***	(.0634)	13.3148***	(.0592)	29.5185***	(.0569)
Black x n prior prop cnv	-1.2232***	(-.0579)	-6.4704***	(-.0601)	-11.3682***	(-.0526)	-20.5211***	(-.0412)
Black x n charges as juv	-.2305	(-.0013)	-22.7449**	(-.0249)	-63.5544***	(-.0346)	-184.523***	(-.0436)
Female x Part 1 charges	.1250	(.0019)	-2.9030	(-.0089)	-9.3102	(-.0141)	-30.3708	(-.0200)
Off. age x drug problem	.0700	(.0059)	1.0086	(.0168)	2.3771	(.0197)	3.6146	(.0130)
Off. age x prior trtment	.3353	(.0252)	4.0946***	(.0603)	8.7468***	(.0642)	20.9629***	(.0668)
Off. age x unemployed	.1444	(.0137)	1.1030*	(.0205)	2.6974**	(.0250)	7.6964***	(.0309)
Off. age x PO property	.1741	(.0170)	1.9745**	(.0377)	4.2611**	(.0406)	10.0378**	(.0415)
Off. age x chg pst 5 yrs	-.0061	(-.0061)	.1243	(.0243)	.3068*	(.0299)	.7883**	(.0334)
PO viol x has detainers	6.7646	(.0142)	26.2867	(.0108)	44.3816	(.0091)	89.6792	(.0080)
PO prop x n adl.arrests	-.1249	(-.0056)	-1.8274	(-.0161)	-4.4560	(-.0196)	-14.9494*	(-.0286)
PO prop x prior prop con	.3258	(.0153)	1.6638	(.0153)	4.1004	(.0188)	13.2466	(.0263)
PO prop x n juv. arrests	-.8113*	(-.0215)	-5.6234**	(-.0292)	-11.2295**	(-.0291)	-21.2812**	(-.0239)
PO prop x age at 1st arr	-.3932*	(-.0314)	-2.8950**	(-.0454)	-5.7836**	(-.0452)	-11.9191**	(-.0404)
PO prop x yrs. 1st incar	-.0817	(-.0195)	-.9996***	(-.0468)	-1.9630***	(-.0458)	-4.2304***	(-.0428)
PO drugs x n adl. convs.	-.5583	(-.0175)	-5.3768*	(-.0331)	-11.5866**	(-.0356)	-22.9595*	(-.0306)
PO drugs x Part 1 chgs.	2.2789**	(.0474)	10.7570**	(.0439)	21.8474**	(.0444)	38.7272*	(.0342)
PO drugs x last par. rev	1.4732	(.0031)	.6188	(.0003)	-42.9296	(-.0089)	-186.551	(-.0169)
Selection Hazards								
UCR to SAC arrest histry	-1.3208	(-.0109)	-2.8210	(-.0046)	-1.3744	(-.0011)	-7.6019	(-.0027)
Case proceeds past arrst	-.2335	(-.0006)	11.6887	(.0056)	31.2362	(.0074)	94.2607	(.0097)
Case to Grand Jury	2.3651	(.0212)	22.6385***	(.0399)	55.0017***	(.0483)	152.5792***	(.0581)
Case to Superior Court	-.5709	(-.0019)	-22.4632	(-.0150)	-61.5582*	(-.0205)	-120.536	(-.0174)
Superior Court Convict.	.8423	(.0043)	15.7197	(.0158)	35.2138	(.0177)	85.4826	(.0186)
Match over data sources	7.1308	(.0222)	27.2988	(.0166)	46.9466	(.0143)	75.7857	(.0100)
Constant	354.6281***	(.0028)	1021.898***	(.0172)	1662.280***	(.0193)	3046.951***	(.0175)
R squared	.086		.213		.269		.312	
Adjusted R squared	.080		.208		.265		.308	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.2
Variance Attributable to the Sentence for Probability of Rearrest
- Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.185)	(.255)	(.273)	(.271)
All Sanction Measures	6.12	4.12	4.44	5.34
	(.011)	(.011)	(.012)	(.014)
Sentence Variables	3.00	2.68	3.23	4.10
	(.006)	(.007)	(.009)	(.011)
Place Sentenced	1.82	1.14	1.13	1.04
	(.003)	(.003)	(.003)	(.003)
Time Sentenced	.09	.16	.16	.36
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.12	.70	1.08	1.46
	(.000)	(.002)	(.003)	(.004)
Interactions w/Ind. Vars.	3.12	1.45	1.21	1.24
	(.006)	(.004)	(.003)	(.003)

Table B8.3
Variance Attributable to the Sentence for Probability
of Rearrest for Persons Crime - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.092)	(.142)	(.165)	(.188)
All Sanction Measures	9.01	6.16	4.58	5.05
	(.008)	(.009)	(.008)	(.010)
Sentence Variables	4.02	3.41	2.98	4.03
	(.004)	(.005)	(.005)	(.008)
Place Sentenced	2.82	1.92	1.43	1.69
	(.003)	(.003)	(.002)	(.003)
Time Sentenced	.11	.07	.09	.32
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.25	.49	.80	.79
	(.000)	(.001)	(.001)	(.001)
Interactions w/Ind. Vars.	5.00	2.76	1.60	1.02
	(.005)	(.004)	(.003)	(.002)

Table B8.4
Variance Attributable to the Sentence for Probability
of Repeating Presenting Offense - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.072)	(.108)	(.135)	(.150)
All Sanction Measures	7.74	4.42	4.21	4.76
	(.006)	(.005)	(.006)	(.007)
Sentence Variables	2.71	1.80	2.02	3.12
	(.002)	(.002)	(.003)	(.005)
Place Sentenced	2.05	1.21	1.28	1.86
	(.001)	(.001)	(.002)	(.003)
Time Sentenced	.08	.10	.17	.21
	(.000)	(.000)	(.000)	(.000)
Sentence Pattern	.18	.05	.15	.44
	(.000)	(.000)	(.000)	(.001)
Interactions w/Ind. Vars.	5.03	2.62	2.18	1.64
	(.004)	(.003)	(.003)	(.002)

Table B8.5
Variance Attributable to the Sentence for Probability
of Reimprisonment - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.135)	(.267)	(.306)	(.314)
All Sanction Measures	11.08	9.43	7.96	6.90
	(.015)	(.025)	(.024)	(.022)
Sentence Variables	6.75	7.10	6.11	5.69
	(.009)	(.019)	(.019)	(.018)
Place Sentenced	3.57	4.58	3.81	3.08
	(.005)	(.012)	(.012)	(.010)
Time Sentenced	.07	.20	.39	.41
	(.000)	(.001)	(.001)	(.001)
Sentence Pattern	.22	.07	.05	.01
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.32	2.33	1.86	1.20
	(.006)	(.006)	(.006)	(.004)

Table B8.6

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	.0196	.0285*	.0336*	.0293*	.0035	.0109	.0200	.0149
Has job after sentence	-.0394***	-.0262*	-.0252*	-.0240	-.0176**	-.0105	-.0152	-.0224*
Offender is on welfare	-.0248*	.0042	.0048	.0083	.0037	.0099	.0119	.0114
Offender is Black	.0798***	.1518***	.1680***	.1841***	.0474***	.1016***	.1071***	.1341***
Offender is Hispanic	.0707***	.1303***	.1428***	.1483***	.0437**	.1024***	.1157***	.1426***
Offender is female	-.0490**	-.0634**	-.0688**	-.0906***	-.0416***	-.0806***	-.1072***	-.1192***
Lives in urban area	.0092	.0275*	.0328*	.0272*	.0166*	.0229**	.0272**	.0234*
Years at current address	-.0013**	-.0014*	-.0014*	-.0013*	-.0004	-.0005	-.0005	-.0002
History of drug problems	.0028	.0234	.0182	.0246	.0047	-.0063	-.0065	-.0128
Treated for drugs/alch.	-.0141	-.0067	.0009	.0170	-.0115	-.0130	-.0106	-.0168
Has needle marks	-.0043	.0225	.0685**	.0785**	-.0082	-.0226	-.0216	-.0127
Not a school drop out	-.0296**	-.0436***	-.0462***	-.0477***	.0000	-.0074	-.0237**	-.0220*
Doesn't live with family	-.0013	-.0131	-.0516***	-.0390**	-.0059	.0023	-.0048	-.0086
Commited PO with group	-.0165	-.0304**	-.0313**	-.0311**	-.0049	.0024	-.0023	-.0045
Victim was a stranger	.0267*	.0396**	.0247	.0313*	.0039	.0009	.0053	.0062
Presenting Offense								
PO property crime	.0050	.0243	.0336	.0400*	-.0059	-.0228	-.0274*	-.0138
PO crime against person	-.0212	.0248	.0363	.0544**	.0300**	.0590***	.0767***	.0871***
PO drug offense	.0058	.0180	.0163	.0255	.0007	-.0169	-.0291	-.0311
PO Wolfgang severity	-.0010	-.0016	-.0009	-.0011	-.0001	-.0003	-.0006	.0001
Has detainers at arrest	-.0189	-.0138	-.0136	-.0183	.0032	-.0123	-.0120	-.0068
Has pending charges	.0543***	.0844***	.0894***	.0757***	.0119	.0142	.0253*	.0270*
On probation at PO	.0523***	.0638***	.0595**	.0613**	.0018	.0143	.0234	.0320*
Anamnestic Theory								
N prior adult arrests	.0105**	.0276***	.0341***	.0343***	.0066**	.0127***	.0148***	.0155***
N prior adult conviction	.0014	-.0021	.0011	.0017	-.0042*	-.0038	-.0049	-.0037
N prior adult chg. conv.	-.0055	-.0113**	-.0138**	-.0137**	-.0010	-.0019	-.0053	-.0062
N charges past 5 years	.0052***	.0061**	.0062**	.0094***	.0013	.0015	.0012	.0021
N prior Part 1 charges	.0118**	.0090	.0100	.0101	.0030	-.0031	.0013	.0014
N prior property conv.	.0010	.0034	-.0002	.0000	-.0033	-.0008	-.0011	.0013
N prior persons conv.	.0032	.0061	.0118*	.0073	.0093***	.0180***	.0239***	.0266***
N prior weapons conv.	.0312*	.0242	.0294	.0510**	.0196**	.0201*	.0363**	.0532***
Off street last 2 years	.0478***	.1014***	.1074***	.1060***	.0099	.0340***	.0358**	.0463***
Delinquent Career/Onset								
N arrests as juvenile	.0122***	.0160***	.0152**	.0206***	.0040*	.0075**	.0086**	.0108**
N charges as juvenile	.0027	.0079	.0081	.0161	.0046	-.0048	.0018	.0038
Age at first arrest	.0008	.0041*	.0033	.0025	-.0011	.0016	.0017	.0000
Yrs since first incarc.	.0019***	.0022***	.0024***	.0020***	.0007**	.0010**	.0011**	.0012**
Yrs since first drug use	-.0006	-.0009*	-.0009	-.0006	.0000	-.0005	-.0004	-.0007*
Prior CJS-Offender Action								
N prior incarcerations	-.0002	-.0046	-.0077	-.0181**	.0018	.0033	.0032	.0014
N prior parole revokes	.0044	.0116	-.0003	.0047	-.0025	-.0122	-.0137	-.0190
Bad conduct last probat.	.0189	.0173	.0 3	-.0098	.0113	.0121	.0164	.0214
Recent parole revoked	.0316	-.0128	-.0067	.0130	.0174	.0261	.0242	.0375
General Control Variables								
Offender age at sent.	-.0049***	-.0081***	-.0094***	-.0081***	-.0005	-.0040***	-.0059***	-.0059***
Off. born out of state	-.0437***	-.0841***	-.1009***	-.1200***	-.0143**	-.0269***	-.0396***	-.0574***
Coder prob. prognosis	-.0006**	-.0011***	-.0008***	-.0008***	-.0001	-.0004*	-.0003	-.0005*

* p<.05 ** p<.01 *** p<.001

Table B8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	a) Rearrested — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years	b) Rearrested for Persons Crime — Post-Sentence Observation Window: 1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0444*	-.0752**	-.0963***	-.1104***	-.0192	-.0374*	-.0484**	-.0529**
Black x prior adult arrs	-.0087**	-.0109**	-.0191***	-.0162**	-.0056***	-.0086***	-.0120***	-.0127***
Black x n prior prop cnv	.0060*	.0091*	.0197***	.0220***	.0032*	.0036	.0063*	.0060*
Black x n charges as juv	.0134	.0186	.0254	.0126	-.0098	.0072	-.0019	.0007
Female x Part 1 charges	.0175	.0326*	.0503*	.0348	-.0005	-.0019	.0068	.0149
Off. age x drug problem	-.0047*	-.0031	-.0022	-.0013	-.0013	-.0034	-.0048*	-.0049*
Off. age x prior trtment	-.0035	-.0078**	-.0091**	-.0093**	-.0022	-.0025	-.0025	-.0042
Off. age x unemployed	-.0020	-.0021	-.0016	-.0018	-.0013	-.0013	-.0009	-.0013
Off. age x PO property	-.0058**	-.0060**	-.0041	-.0060**	-.0024*	-.0025	-.0028	-.0054**
Off. age x chg pst 5 yrs	.0005**	.0002	.0002	.0003	.0000	.0000	.0000	-.0001
PO viol x has detainers	-.0394	-.1089*	-.1205*	-.1433**	-.0266	-.0371	-.0541	-.0790*
PO prop x n adl.arrests	.0045	.0032	-.0022	.0060	.0025	.0032	.0057	.0108**
PO prop x prior prop con	-.0056	-.0050	-.0020	-.0076	-.0004	-.0028	-.0054	-.0085**
PO prop x n juv. arrests	-.0111**	-.0117*	-.0123*	-.0169**	-.0037	-.0083**	-.0075*	-.0044
PO prop x age at 1st arr	.0026	-.0025	-.0035	-.0017	-.0014	-.0078**	-.0078**	-.0029
PO prop x yrs. 1st incar	.0001	.0004	.0007	.0010	-.0003	-.0004	-.0002	-.0004
PO drugs x n adl. convs.	.0130**	.0138*	.0129*	.0121	.0018	.0077	.0095*	.0110*
PO drugs x Part 1 chgs.	-.0233**	-.0205*	-.0141	-.0203	.0004	-.0085	-.0047	-.0092
PO drugs x last par. rev	.0698	.1410	.0753	.0872	-.0135	-.0087	.0070	.0294
Sentence								
Prison	-.1146***	-.0993**	-.0987**	-.1033**	-.0395**	-.0360	-.0411	-.0603*
Youth complex	-.0456	-.0136	-.0013	-.0203	-.0173	.0000	.0045	-.0094
Jail, probation, fine	-.0671*	-.0834*	-.1383***	-.1379***	-.0339	-.0331	-.0524	-.0652
Jail, probation	.0020	.0468	.0087	.0047	-.0057	.0253	.0291	.0551
Jail only	-.0335	-.0462	-.0759*	-.0794*	-.0019	.0305	.0204	-.0025
Probation w/cond., fine	-.0232	-.0187	-.0018	-.0090	.0036	-.0165	-.0082	-.0234
Probation, fine	-.0279	-.0197	-.0107	-.0015	-.0065	-.0095	-.0067	-.0077
Probation w/conditions	.0027	.0126	.0195	.0094	.0004	.0160	.0262	.0187
Fined only	-.0321	-.0542	-.0618	-.1063*	.0117	.0141	.0106	-.0298
Other sanction	-.0927*	-.0966	-.0901	-.0976	-.0034	-.0075	.0046	-.0214
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	-.0001	-.0003	.0000	.0002	-.0002	.0002	.0000	-.0001
Months to jail	.0001	.0001	.0027	.0012	-.0001	-.0004	-.0004	-.0010
Months to prison	-.0002	-.0004*	-.0006**	-.0008***	.0000	-.0001	-.0001	-.0004**
First sanction of career	.0550	.0525	.0459	.0454	.0099	.0072	.0050	.0012
Progressive sanction	.0015	.0113	.0088	-.0020	.0075	.0146	.0224	.0109
Sentence Interactions								
Prison x n adult arrests	.0068*	.0018	.0009	-.0005	.0012	.0006	.0011	-.0010
Prison x n arrsts as juv	.0036	-.0019	.0032	-.0013	.0009	.0011	.0011	-.0035
Yth. comp x n adlt convs	-.0102*	.0062	.0060	.0089	-.0030	.0001	-.0013	-.0018
Yth. comp x chgs in 5 yr	.0036	-.0029	-.0024	-.0061	.0016	.0013	.0012	-.0003
Yth. comp x prior n incs	.0218**	.0252	.0206	.0099	.0069	.0113	.0142	.0130
Jl & prob x inc lst 2 yr	.0720	.0507	.0557	.0119	.0688**	.0664*	.0636	.0184
Jail x yrs using drugs	.0003	-.0001	.0005	-.0003	-.0015*	-.0005	-.0008	-.0012
Prb w/cnd, fn x adlt arr	-.0144**	-.0160**	-.0073	-.0029	-.0020	-.0020	.0013	.0012
Prb & fn x Part 1 chgs.	-.0263*	-.0128	-.0208	-.0125	-.0104	-.0018	.0049	.0052
Prb w/cnd x adlt chg cnv	-.0087**	-.0104*	-.0118*	-.0116*	-.0033	-.0063*	-.0049	-.0053
Mths to jail x PO prop.	-.0001*	-.0002**	-.0002***	-.0002**	.0000	-.0001	-.0001*	-.0001*
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0302	-.0409	-.0576*	-.0537	-.0046	.0011	.0040	.0288
Init sanc x n adult arrs	.0400***	.0513***	.0493***	.0490***	.0170**	.0296***	.0369***	.0436***

* p<.05 ** p<.01 *** p<.001

Table B8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	a) Rearrested — Post-Sentence Observation Window				b) Rearrested for Persons Crime — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Selection Hazards								
UCR to SAC arrest histry	-.0362*	-.0210	-.0225	-.0020	-.0049	-.0197	-.0286*	-.0396**
Case proceeds past arrst	.2429**	.1286	.1049	.0765	.0995	.1431	.2171**	.1287
Case to Grand Jury	-.0069	-.0047	-.0147	-.0078	.0037	.0142	.0289*	.0293
Case to Superior Court	.0528	.0395	.0157	.0337	.0192	.0401	.0068	.0092
Superior Court Convict.	-.0356	-.0472	-.0260	-.0139	-.0237	-.0258	-.0182	.0034
Match over data sources	.0202	.0757	.0758	.0882	-.0162	-.0304	-.0236	.0192
Constant	-.1658***	-.1664***	-.0659	-.0018	-.0686***	-.1340***	-.1678***	-.1875***
Mean of Dep. Var.	.234	.399	.480	.549	.072	.148	.201	.256
N of cases	11,749	11,749	11,749	11,749	11,714	11,746	11,749	11.749
Pseudo R squared	.158	.220	.237	.238	.075	.123	.146	.167
Model Classifications:								
% Correct Predictions	79.16	73.55	73.16	73.59	92.79	85.60	81.43	77.44
% False Positives	4.42	10.07	12.00	13.52	.18	1.30	2.84	5.39
% False Negatives	16.42	16.38	14.84	12.89	7.03	13.09	15.73	17.18
RIOC	.493	.501	.489	.472	.372	.493	.507	.476

* p<.05 ** p<.01 *** p<.001

Table 88.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Structural Variables								
Offender is unemployed	-.0062	.0094	.0126	.0196	.0015	.0208*	.0176	.0157
Has job after sentence	-.0141*	-.0178*	-.0109	-.0087	-.0190***	-.0314***	-.0328**	-.0313**
Offender is on welfare	-.0123	-.0162	-.0143	-.0061	-.0100	-.0232*	-.0307**	-.0246*
Offender is Black	.0108	.0340**	.0602***	.0873***	.0049	.0788***	.1220***	.1402***
Offender is Hispanic	.0180	.0415*	.0751***	.0890***	.0038	.0540**	.0773***	.1161***
Offender is female	-.0126	-.0422**	-.0519***	-.0725***	-.0009	-.0090	-.0200	-.0475*
Lives in urban area	.0037	.0156	.0143	.0227*	-.0074	-.0125	-.0039	.0133
Years at current address	-.0005	-.0009*	-.0010*	-.0011*	.0000	-.0007	-.0007	-.0004
History of drug problems	-.0012	.0055	-.0072	-.0015	.0147	.0183	.0373**	.0350*
Treated for drugs/alch.	-.0073	.0000	.0153	.0103	.0021	.0106	.0100	.0078
Has needle marks	.0267**	.0380**	.0317*	.0333	.0083	.0222	.0239	.0405*
Not a school drop out	-.0177**	-.0153*	-.0190*	-.0141	.0036	-.0108	-.0134	-.0149
Doesn't live with family	-.0054	-.0137	-.0308**	-.0357***	.0059	-.0004	-.0018	-.0073
Commited PO with group	-.0157**	-.0236***	-.0324***	-.0325***	-.0101*	-.0037	-.0033	-.0136
Victim was a stranger	.0101	.0247**	.0253*	.0318**	.0097	.0207*	.0302**	.0329**
Presenting Offense								
PO property crime	.0260*	.0363**	.0521***	.0412**	.0101	-.0026	.0145	.0379*
PO crime against person	.0301*	.0574***	.0674***	.0705***	-.0024	-.0022	.0093	.0177
PO drug offense	.0781***	.1493***	.1861***	.2336***	-.0063	-.0143	.0139	.0362
PO Wolfgang severity	-.0021***	-.0028***	-.0031***	-.0038***	.0001	.0005	.0008	.0003
Has detainers at arrest	-.0032	-.0021	-.0054	-.0074	-.0057	.0113	.0113	.0190
Has pending charges	.0176**	.0292**	.0471***	.0569***	.0213***	.0546***	.0612***	.0607***
On probation at PO	.0198*	.0272*	.0313*	.0409**	.0165	.0443**	.0572***	.0470**
Anamnestic Theory								
N prior adult arrests	.0072**	.0138***	.0180***	.0183***	.0019	.0050	.0080*	.0094*
N prior adult conviction	.0005	-.0002	-.0004	.0044	-.0014	.0017	.0013	.0027
N prior adult chg. conv.	-.0021	-.0050*	-.0052	-.0046	.0013	.0003	.0011	-.0007
N charges past 5 years	.0013	.0015	.0020	.0020	0015*	.0019	.0015	.0026
N prior Part 1 charges	.0062**	.0056	.0057	.0054	005	.0029	.0039	.0036
N prior property conv.	-.0063***	-.0064*	-.0085**	-.0090**	-.0001	.0014	.0027	.0035
N prior persons conv.	.0006	.0002	.0006	.0016	.0031	.0041	.0082*	.0119**
N prior weapons conv.	.0025	.0154	.0025	.0075	-.0013	.0081	.0019	.0057
Off street last 2 years	.0142	.0364***	.0462***	.0591***	.0174**	.0518***	.0831***	.0842***
Delinquent Career/Onset								
N arrests as juvenile	.0006	.0027	.0021	.0046	.0033*	.0068**	.0066*	.0075*
N charges as juvenile	.0015	-.0044	-.0071	-.0001	.0043	.0134	.0231	.0159
Age at first arrest	-.0004	.0026	.0023	.0023	.0017	.0034	.0058**	.0046*
Yrs since first incarc.	.0004	.0008*	.0009*	.0008	.0027***	.0038***	.0044***	.0050***
Yrs since first drug use	-.0002	-.0002	-.0002	.0000	.0001	-.0001	-.0003	-.0006
Prior CJS-Offender Action								
N prior incarcerations	.0018	-.0023	-.0014	-.0071	.0020	.0022	.0010	.0006
N prior parole revokes	-.0046	-.0115	-.0205*	-.0206	.0054	.0003	.0035	-.0002
Bad conduct last probat.	-.0007	.0044	.0013	-.0021	.0016	.0079	.0032	.0039
Recent parole revoked	.0246	.0233	.0411	.0441	.0151	.0462**	.0159	.0178
General Control Variables								
Offender age at sent.	-.0004	-.0021	-.0021	-.0027*	-.0016	-.0041**	-.0049**	-.0049**
Off. born out of state	-.0162**	-.0228**	-.0420***	-.0445***	.0020	-.0111	-.0192*	-.0341***
Coder prob. prognosis	-.0003*	-.0007***	-.0007***	-.0009***	-.0003*	-.0006***	-.0008***	-.0009***

* p<.05 ** p<.01 *** p<.001

Table B8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Interactions								
Black x on prob. at PO	-.0211*	-.0363*	-.0405*	-.0441*	-.0140	-.0455**	-.0578***	-.0381
Black x prior adult arrs	-.0038*	-.0068**	-.0083**	-.0095**	-.0049***	-.0076***	-.0105***	-.0139***
Black x n prior prop cnv	.0031*	.0051*	.0052*	.0061*	.0038***	.0043*	.0042	.0047
Black x n charges as juv	.0022	.0075	.0165	.0074	-.0037	.0087	.0177	.0327
Female x Part 1 charges	.0050	.0047	.0068	.0042	.0012	.0094	.0135	.0203
Off. age x drug problem	-.0018	-.0011	-.0037*	-.0027	.0006	-.0012	-.0005	.0013
Off. age x prior trtment	-.0013	-.0028	-.0023	-.0045*	-.0019	-.0043*	-.0046*	-.0073**
Off. age x unemployed	-.0018*	-.0003	-.0009	-.0014	.0000	-.0007	-.0025	-.0043**
Off. age x PO property	-.0028*	-.0041**	-.0058***	-.0072***	-.0015	-.0065***	-.0071***	-.0082***
Off. age x chg pst 5 yrs	.0001	.0000	.0000	.0001	.0002**	.0004**	.0003**	.0004**
PO viol x has detainers	-.0255	-.0565*	-.0748*	-.0948**	.0019	-.0146	-.0201	-.0325
PO prop x n adl.arrests	-.0026	-.0034	-.0027	-.0021	.0003	.0028	.0044	.0065
PO prop x prior prop con	.0056**	.0080**	.0109***	.0115***	-.0009	-.0015	-.0027	-.0044
PO prop x n juv. arrests	.0006	.0018	.0016	.0009	-.0006	-.0012	.0003	.0004
PO prop x age at 1st arr	.0037*	.0011	.0024	.0037	.0009	.0020	.0014	.0028
PO prop x yrs. 1st incar	.0007*	.0007	.0008	.0014**	-.0001	.0005	.0005	.0006
PO drugs x n adl. convs.	.0030	.0069*	.0080*	.0063	.0039	.0108**	.0096*	.0070
PO drugs x Part 1 chgs.	-.0106*	-.0095	.0007	.0000	-.0077*	-.0089	-.0121	-.0099
PO drugs x last par. rev	.0276	.0353	.0033	.0512	.0160	.0173	.0760	.0609
Sentence								
Prison	-.0424**	-.0468*	-.0468	-.0867***	-.0168	-.0052	.0173	-.0256
Youth complex	-.0143	-.0220	-.0228	-.0630**	-.0037	.0448	.0597*	.0300
Jail, probation, fine	-.0321	-.0604*	-.0895***	-.1102***	-.0504***	-.0954***	-.1263***	-.1354***
Jail, probation	-.0056	.0132	-.0068	-.0254	-.0482***	-.1017***	-.1237***	-.1168***
Jail only	-.0099	-.0093	-.0288	-.0525*	-.0254*	-.0621***	-.0714**	-.0949***
Probation w/cond., fine	.0035	-.0078	-.0298	-.0358*	-.0198	-.0253	-.0336	-.0525*
Probation, fine	-.0101	-.0307*	-.0439**	-.0687***	-.0133	-.0330	-.0367	-.0263
Probation w/conditions	.0050	-.0066	-.0017	-.0170	.0120	.0202	.0208	.0372
Fined only	.0306	.0282	-.0082	-.0496	-.0121	.0200	-.0206	-.0980
Other sanction	-.0189	-.0128	-.0001	-.0466	-.0234	-.0482	-.0516	-.0559
Dollars fined	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Months on probation	.0001	.0001	.0001	.0003	.0003	.0012**	.0013**	.0010*
Months to jail	-.0001	.0002	.0014	.0013	.0000	.0008	.0012	.0004
Months to prison	.0000	-.0002	-.0004*	-.0005**	.0000	-.0002	-.0005***	-.0006***
First sanction of career	.0273	.0607*	.0511	.0429	.0437	.0514	.0264	.0270
Progressive sanction	-.0078	-.0037	-.0046	-.0228	.0090	.0155	.0215	.0088
Sentence Interactions								
Prison x n adult arrests	.0014	.0020	.0008	.0009	.0011	.0014	.0020	.0020
Prison x n arrsts as juv	.0065**	.0049	.0041	.0050	-.0001	-.0036	-.0031	-.0050
Yth. comp x n adlt convs	-.0085**	-.0081*	-.0105*	-.0109*	-.0021	-.0091**	-.0129**	-.0136*
Yth. comp x chgs in 5 yr	.0029	.0031	.0039	.0044	.0006	.0018	.0033	.0042
Yth. comp x prior n incs	.0038	.0051	.0044	.0040	.0058	.0154**	.0261***	.0252**
Jl & prob x inc lst 2 yr	.0063	-.0117	-.0035	.0017	.0787	.1118*	.1128*	.0539
Jail x yrs using drugs	.0008	.0004	.0001	-.0008	.0000	.0005	.0012	.0005
Prb w/cnd, fn x adlt arr	-.0055*	-.0045	-.0017	.0008	-.0030	-.0047	-.0056	-.0023
Prb & fn x Part 1 chgs.	-.0117	-.0108	-.0151	-.0168	-.0022	-.0146	-.0066	-.0159
Prb w/cnd x adlt chg cnv	-.0021	-.0028	-.0051	-.0036	-.0035*	-.0077**	-.0081**	-.0113***
Mths to jail x PO prop.	.0000	-.0001*	-.0001**	-.0001**	.0000	.0000	.0000	-.0001
Mths to pris x prop cnvs	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
Init sanc x black	-.0059	-.0142	-.0172	-.0009	-.0136	-.0169	-.0375	-.0249
Init sanc x n adult arrs	.0171**	.0335***	.0344***	.0346***	.0155**	.0313**	.0269*	.0239*

* p<.05 ** p<.01 *** p<.001

Table B8.6 (continued)

Logit Regression Coefficients for Selected Binomial Measures -
Expressed as Change at the Mean
(Controlling for Sample Selection)

Independent Variable	c) Repeat of Presenting Offense — Post-Sentence Observation Window				d) Reimprisoned — Post-Sentence Observation Window			
	1 Year	3 Years	5 Years	9 Years	1 Year	3 Years	5 Years	9 Years
Selection Hazards								
UCR to SAC arrest histry	-.0275**	-.0190	-.0295*	-.0256	.0105	.0053	.0058	.0196
Case proceeds past arrst	.0861	.0547	.0296	-.0078	-.0120	-.0030	-.0205	-.0516
Case to Grand Jury	-.0074	.0060	.0162	.0236	-.0121	-.0366***	-.0507***	-.0700***
Case to Superior Court	.0031	-.0298	-.0543	-.0135	.0030	.1003**	.0689	.0499
Superior Court Convict.	-.0105	-.0268	-.0108	-.0056	-.0084	-.0355	-.0421	-.0471
Match over data sources	-.0113	.0193	.0148	.0791	-.0267	-.0048	-.0109	.0158
Constant	-.0725***	-.1315***	-.1573***	-.1876***	-.0546***	-.1261***	-.1610***	-.1777***
Mean of Dep. Var.	.079	.153	.199	.246	.057	.137	.186	.230
N of cases	11,714	11,746	11,749	11,749	11,714	11,746	11,749	11,749
Pseudo R squared	.061	.093	.118	.134	.101	.200	.232	.245
Model Classifications:								
% Correct Predictions	92.14	84.68	80.68	77.11	94.36	87.98	85.60	82.87
% False Positives	.09	1.10	2.44	4.00	.26	2.80	4.04	5.31
% False Negatives	7.77	14.23	16.88	18.89	5.39	9.22	10.36	11.82
RIOC	.558	.394	.443	.455	.479	.556	.597	.582

* p<.05 ** p<.01 *** p<.001

Table B8.8
Variance Attributable to the Sentence for Log of
Total Post-Sentence Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.202)	(.305)	(.344)	(.360)
All Sanction Measures	6.12	3.72	3.60	4.30
	(.012)	(.011)	(.012)	(.015)
Sentence Variables	2.32	1.90	2.26	3.29
	(.005)	(.006)	(.008)	(.012)
Place Sentenced	1.65	.97	1.00	1.18
	(.003)	(.003)	(.003)	(.004)
Time Sentenced	.05	.11	.15	.28
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.01	.22	.32	.46
	(.000)	(.001)	(.001)	(.002)
Interactions w/Ind. Vars.	3.80	1.82	1.34	1.01
	(.008)	(.006)	(.005)	(.004)

Table B8.9
Variance Attributable to the Sentence for Log of
Total Post-Sentence Convictions - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.164)	(.254)	(.295)	(.310)
All Sanction Measures	8.84	5.54	6.16	6.90
	(.014)	(.014)	(.018)	(.021)
Sentence Variables	3.08	2.52	3.23	4.39
	(.005)	(.006)	(.010)	(.014)
Place Sentenced	1.57	1.12	1.45	1.83
	(.003)	(.003)	(.004)	(.006)
Time Sentenced	.19	.28	.27	.35
	(.000)	(.001)	(.001)	(.001)
Sentence Pattern	.05	.02	.06	.14
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	5.76	3.02	2.94	2.50
	(.009)	(.008)	(.009)	(.008)

Table B8.10
Variance Attributable to the Sentence for Log of
Total Post-Sentence Persons Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.091)	(.148)	(.174)	(.199)
All Sanction Measures	8.58	5.79	4.49	5.31
	(.008)	(.009)	(.008)	(.011)
Sentence Variables	4.28	3.25	2.85	4.22
	(.004)	(.005)	(.005)	(.008)
Place Sentenced	2.98	1.78	1.33	1.71
	(.003)	(.003)	(.002)	(.003)
Time Sentenced	.10	.09	.28	.47
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.06	.19	.48	.58
	(.000)	(.000)	(.001)	(.001)
Interactions w/Ind. Vars.	4.30	2.54	1.64	1.09
	(.004)	(.004)	(.003)	(.002)

Table B8.11
Variance Attributable to the Sentence for Log of
Adjusted Post-Sentence Arrest Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.210)	(.331)	(.367)	(.381)
All Sanction Measures	6.32	3.60	3.51	2.78
	(.013)	(.012)	(.013)	(.011)
Sentence Variables	2.18	1.55	1.72	1.48
	(.005)	(.005)	(.006)	(.006)
Place Sentenced	1.36	.81	.94	.67
	(.003)	(.003)	(.003)	(.003)
Time Sentenced	.08	.08	.07	.10
	(.000)	(.000)	(.000)	(.000)
Sentence Pattern	.00	.02	.02	.04
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.14	2.04	1.79	1.30
	(.009)	(.007)	(.007)	(.005)

Table B8.12
Variance Attributable to the Sentence for Log of
Adjusted Post-Sentence Charge Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.209)	(.323)	(.358)	(.371)
All Sanction Measures	6.25	3.58	3.25	2.73
	(.013)	(.012)	(.012)	(.010)
Sentence Variables	2.20	1.48	1.56	1.42
	(.005)	(.005)	(.006)	(.005)
Place Sentenced	1.49	.80	.84	.66
	(.003)	(.003)	(.003)	(.002)
Time Sentenced	.06	.09	.10	.14
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.00	.03	.03	.03
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.06	2.09	1.69	1.31
	(.008)	(.007)	(.006)	(.005)

Table B8.13
Variance Attributable to the Sentence for Log of
Adjusted Post-Sentence Persons Charge Rate - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.097)	(.156)	(.180)	(.201)
All Sanction Measures	8.34	5.14	3.53	3.08
	(.008)	(.008)	(.006)	(.006)
Sentence Variables	3.95	2.18	1.34	1.43
	(.004)	(.003)	(.002)	(.003)
Place Sentenced	2.60	1.20	.64	.61
	(.003)	(.002)	(.001)	(.001)
Time Sentenced	.13	.11	.27	.38
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.06	.02	.06	.08
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	4.39	2.97	2.18	1.65
	(.004)	(.005)	(.004)	(.003)

Table B8.14
Variance Attributable to the Sentence for Summed Seriousness
of All Post-Sentence Charges - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.126)	(.212)	(.246)	(.256)
All Sanction Measures	7.63	4.57	4.05	5.30
	(.010)	(.010)	(.010)	(.014)
Sentence Variables	2.74	1.85	2.11	3.80
	(.003)	(.004)	(.005)	(.010)
Place Sentenced	1.97	.93	.91	1.56
	(.002)	(.002)	(.002)	(.004)
Time Sentenced	.13	.16	.34	.47
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.01	.04	.13	.20
	(.000)	(.000)	(.000)	(.001)
Interactions w/Ind. Vars.	4.89	2.72	1.94	1.50
	(.006)	(.006)	(.005)	(.004)

Table B8.15

Regression Coefficients for Log of Total Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0281*	(.0241)	.0403**	(.0245)	.0473**	(.0250)	.0452*	(.0211)
Has job after sentence	-.0454***	(-.0406)	-.0458**	(-.0290)	-.0445**	(-.0245)	-.0491**	(-.0239)
Offender is on welfare	-.0341**	(-.0254)	-.0298	(-.0157)	-.0289	(-.0133)	-.0160	(-.0065)
Offender is Black	.0675***	(.0373)	.1663***	(.0729)	.2327***	(.0875)	.3093***	(.1139)
Offender is Hispanic	.0553**	(.0258)	.1403***	(.0463)	.1861***	(.0533)	.2580***	(.0655)
Offender is female	-.0370*	(-.0210)	-.0746**	(-.0300)	-.0905***	(-.0317)	-.1312***	(-.0407)
Lives in urban area	.0115	(.0102)	.0370*	(.0233)	.0532**	(.0291)	.0590**	(.0285)
Years at current address	-.0010*	(-.0183)	-.0013*	(-.0168)	-.0013	(-.0150)	-.0011	(-.0112)
History of drug problems	.0046	(.0035)	.0331	(.0180)	.0499*	(.0235)	.0575*	(.0240)
Treated for drugs/alch.	-.0194	(-.0132)	.0018	(.0009)	.0308	(.0129)	.0559*	(.0207)
Has needle marks	.0137	(.0063)	.0545*	(.0177)	.0997***	(.0280)	.1216***	(.0303)
Not a school drop out	-.0229*	(-.0199)	-.0365**	(-.0225)	-.0495**	(-.0265)	-.0536**	(-.0254)
Doesn't live with family	-.0012	(-.0009)	-.0086	(-.0045)	-.0587***	(-.0265)	-.0559**	(-.0224)
Commited PO with group	-.0170	(-.0152)	-.0206	(-.0130)	-.0172	(-.0095)	-.0212	(-.0103)
Victim was a stranger	.0144	(.0114)	.0470**	(.0264)	.0502**	(.0245)	.0717***	(.0309)
Presenting Offense								
PO property crime	.0134	(.0118)	.0422*	(.0263)	.0590**	(.0320)	.0821***	(.0394)
PO crime against person	-.0176	(-.0171)	.0157	(.0006)	.0217	(.0027)	.0303	(.0044)
PO drug offense	.0165	(.0149)	.0109	(.0139)	.0087	(.0104)	.0283	(.0188)
PO Wolfgang severity	-.0010	(-.0133)	-.0017	(-.0169)	-.0015	(-.0128)	-.0024*	(-.0180)
Has detainers at arrest	-.0052	(-.0108)	.0170	(-.0084)	.0221	(-.0069)	.0075	(-.0121)
Has pending charges	.0696***	(.0499)	.1145***	(.0581)	.1324***	(.0583)	.1423***	(.0556)
On probation at PO	.0556**	(.0231)	.0792***	(.0190)	.0979***	(.0183)	.1090***	(.0166)
Anamnestic Theory								
N prior adult arrests	.0158***	(.1465)	.0377***	(.2575)	.0455***	(.2575)	.0488***	(.2741)
N prior adult conviction	-.0070	(-.0134)	-.0102*	(-.0141)	-.0034	(.0122)	-.0006	(.0219)
N prior adult chg. conv.	-.0065	(-.0763)	-.0152**	(-.0936)	-.0200***	(-.1022)	-.0194***	(-.0883)
N charges past 5 years	.0108***	(.1067)	.0124***	(.0839)	.0138***	(.0780)	.0174***	(.0847)
N prior Part 1 charges	.0248***	(.0439)	.0149*	(.0003)	.0170*	(.0185)	.0146	(.0160)
N prior property conv.	.0011	(.0175)	.0094	(.0528)	.0083	(.0497)	.0119	(.0469)
N prior persons conv.	-.0019	(-.0052)	.0000	(.0000)	.0035	(.0059)	.0045	(.0066)
N prior weapons conv.	.0319*	(.0198)	.0474*	(.0209)	.0340	(.0130)	.0669**	(.0227)
Off street last 2 years	.0703***	(.0636)	.1513***	(.0924)	.1758***	(.0929)	.1963***	(.0907)
Delinquent Career/Onset								
N arrests as juvenile	.0188***	(.0672)	.0299***	(.0710)	.0307***	(.0608)	.0374***	(.0639)
N charges as juvenile	-.0075	(.0040)	-.0081	(.0103)	.0060	(.0169)	.0066	(.0154)
Age at first arrest	.0028*	(.0584)	.0080***	(.0774)	.0090***	(.0757)	.0088***	(.0672)
Yrs since first incarc.	.0021***	(.1093)	.0036***	(.1213)	.0052***	(.1455)	.0054***	(.1370)
Yrs since first drug use	-.0008*	(-.0131)	-.0012*	(-.0184)	-.0015*	(-.0193)	-.0012	(-.0151)
Prior CJS-Offender Action								
N prior incarcerations	.0014	(.0203)	-.0004	(.0055)	-.0070	(-.0109)	-.0194*	(-.0375)
N prior parole revokes	.0147	(.0125)	.0077	(.0047)	-.0040	(-.0021)	-.0057	(-.0027)
Bad conduct last probat.	.0251	(.0175)	.0481**	(.0238)	.0402*	(.0173)	.0243	(.0093)
Recent parole revoked	.0623*	(.0314)	.0141	(.0219)	.0116	(.0171)	.0433	(.0257)
General Control Variables								
Offender age at sent.	-.0029*	(-.1475)	-.0088***	(-.2016)	-.0123***	(-.2172)	-.0148***	(-.2245)
Off. born out of state	-.0429***	(-.0384)	-.0952***	(-.0603)	-.1289***	(-.0708)	-.1688***	(-.0822)
Coder prob. prognosis	-.0005*	(-.0240)	-.0012***	(-.0432)	-.0013***	(-.0411)	-.0016***	(-.0439)

*p<.05 **p<.01 ***p<.001

Table B8.15 (continued)

Regression Coefficients for Log of Total Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0533*	(-.0189)	-.0953**	(-.0240)	-.1288***	(-.0281)	-.1517***	(-.0294)
Black x prior adult arrs	-.0153***	(-.0582)	-.0198***	(-.0535)	-.0248***	(-.0582)	-.0265***	(-.0550)
Black x n prior prop cnv	.0129***	(.0512)	.0148***	(.0415)	.0183***	(.0447)	.0188***	(.0406)
Black x n charges as juv	.0270	(.0126)	.0537*	(.0177)	.0529	(.0152)	.0533	(.0136)
Female x Part 1 charges	.0186	(.0243)	.0231	(.0213)	.0394*	(.0316)	.0492**	(.0349)
Off. age x drug problem	-.0050*	(-.0358)	-.0065*	(-.0328)	-.0075*	(-.0329)	-.0058	(-.0224)
Off. age x prior trtment	-.0070**	(-.0441)	-.0120***	(-.0534)	-.0120***	(-.0464)	-.0154***	(-.0528)
Off. age x unemployed	-.0031**	(-.0247)	-.0044**	(-.0249)	-.0045**	(-.0220)	-.0042*	(-.0184)
Off. age x PO property	-.0060***	(-.0490)	-.0076***	(-.0439)	-.0086***	(-.0430)	-.0108***	(-.0479)
Off. age x chg pst 5 yrs	-.0001	(-.0095)	-.0004	(-.0233)	-.0004	(-.0202)	-.0003	(-.0123)
PO viol x has detainers	-.0972*	(-.0171)	-.2222***	(-.0277)	-.2409***	(-.0261)	-.2923***	(-.0280)
PO prop x n adl.arrests	.0070	(.0264)	.0119*	(.0317)	.0120*	(.0279)	.0220***	(.0453)
PO prop x prior prop con	-.0112**	(-.0439)	-.0161**	(-.0448)	-.0153**	(-.0369)	-.0229***	(-.0491)
PO prop x n juv. arrests	-.0076	(-.0169)	-.0145*	(-.0228)	-.0171**	(-.0233)	-.0220**	(-.0266)
PO prop x age at 1st arr	.0036	(.0240)	.0001	(.0005)	.0001	(.0002)	.0006	(.0020)
PO prop x yrs. 1st incar	.0015**	(.0296)	.0015*	(.0215)	.0016	(.0192)	.0019*	(.0210)
PO drugs x n adl. convs.	.0207***	(.0544)	.0298***	(.0555)	.0282***	(.0456)	.0302***	(.0434)
PO drugs x Part 1 chgs.	-.0407***	(-.0709)	-.0497***	(-.0614)	-.0350**	(-.0375)	-.0361*	(-.0343)
PO drugs x last par. rev	.0524	(.0094)	.2525**	(.0320)	.2307*	(.0254)	.2922**	(.0285)
Sentence								
Prison	-.1779***	(-.1070)	-.2044***	(-.0871)	-.2458***	(-.0909)	-.3160***	(-.1036)
Youth complex	-.0483	(-.0257)	-.0430	(-.0162)	-.0580	(-.0190)	-.0878	(-.0254)
Jail, probation, fine	-.1112**	(-.0378)	-.1579***	(-.0380)	-.2531***	(-.0530)	-.2746***	(-.0509)
Jail, probation	-.0602	(-.0089)	-.0581	(-.0041)	-.1282*	(-.0208)	-.1217*	(-.0186)
Jail only	-.0525	(-.0296)	-.0798*	(-.0319)	-.1400***	(-.0486)	-.1617***	(-.0497)
Probation w/cond., fine	-.0332	(-.0195)	-.0671*	(-.0279)	-.0777*	(-.0280)	-.0844*	(-.0270)
Probation, fine	-.0443*	(-.0285)	-.0589*	(-.0268)	-.0588*	(-.0233)	-.0576	(-.0202)
Probation w/conditions	-.0216	(-.0131)	-.0249	(-.0107)	-.0067	(-.0025)	-.0080	(-.0026)
Fined only	-.0062	(-.0018)	-.0198	(-.0040)	-.0399	(-.0069)	-.0995	(-.0153)
Other sanction	-.0725	(-.0138)	-.1158	(-.0156)	-.1024	(-.0120)	-.1110	(-.0115)
Dollars fined	.0000	(.0042)	.0000	(.0034)	.0000	(.0083)	.0000	(.0086)
Months on probation	-.0001	(-.0044)	-.0003	(-.0074)	.0001	(.0011)	.0002	(.0040)
Months to jail	.0011	(-.0049)	.0007	(-.0140)	.0023	(-.0052)	.0011	(-.0163)
Months to prison	.0000	(-.0022)	-.0003	(-.0209)	-.0005**	(-.0283)	-.0009***	(-.0416)
First sanction of career	.0734*	(.0429)	.1014*	(.0328)	.0990*	(.0160)	.0967	(.0151)
Progressive sanction	.0002	(.0001)	.0043	(.0022)	.0115	(.0053)	.0017	(.0007)
Sentence Interactions								
Prison x n adult arrests	.0092**	(.0237)	.0085	(.0155)	.0084	(.0133)	.0041	(.0058)
Prison x n arrsts as juv	-.0062	(-.0094)	-.0144*	(-.0154)	-.0142	(-.0132)	-.0195*	(-.0161)
Yth. comp x n adlt convs	-.0234***	(-.0494)	-.0112	(-.0167)	-.0126	(-.0164)	-.0118	(-.0135)
Yth. comp x chgs in 5 yr	.0101**	(.0272)	.0068	(.0129)	.0029	(.0048)	-.0023	(-.0034)
Yth. comp x prior n incs	.0390***	(.0455)	.0245	(.0203)	.0256	(.0184)	.0198	(.0126)
Jl & prob x inc lst 2 yr	.1337**	(.0255)	.1518**	(.0205)	.1629*	(.0191)	.1390	(.0144)
Jail x yrs using drugs	.0021	(.0155)	.0011	(.0058)	.0016	(.0074)	.0008	(.0032)
Prb w/cnd, fn x adlt arr	-.0211***	(-.0529)	-.0270***	(-.0481)	-.0221**	(-.0341)	-.0135	(-.0184)
Prb & fn x Part 1 chgs.	-.0438***	(-.0645)	-.0378**	(-.0394)	-.0388*	(-.0351)	-.0298	(-.0239)
Prb w/cnd x adlt chg cnv	-.0147***	(-.0352)	-.0174**	(-.0295)	-.0168**	(-.0248)	-.0169*	(-.0221)
Mths to jail x PO prop.	-.0001*	(-.0215)	-.0002**	(-.0236)	-.0002**	(-.0255)	-.0003***	(-.0281)
Mths to pris x prop cnvs	.0000	(-.0195)	.0000**	(-.0257)	.0000*	(-.0237)	.0000**	(-.0253)
Init sanc x black	-.0491*	(-.0201)	-.1037**	(-.0301)	-.1541***	(-.0389)	-.1443***	(-.0323)
Init sanc x n adult arrs	.0277**	(.0981)	.0513***	(.1285)	.0542***	(.1179)	.0670***	(.1291)

*p<.05 **p<.01 ***p<.001

Table B8.15 (continued)

Regression Coefficients for Log of Total Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-.0296*	(-.0205)	-.0308	(-.0151)	-.0398	(-.0170)	-.0177	(-.0067)
Case proceeds past arrst	.2276**	(.0464)	.2052*	(.0296)	.2127*	(.0267)	.1673	(.0186)
Case to Grand Jury	-.0089	(-.0067)	.0005	(.0003)	-.0147	(-.0068)	-.0206	(-.0085)
Case to Superior Court	.0373	(.0107)	-.0004	(-.0001)	-.0464	(-.0081)	-.0375	(-.0058)
Superior Court Convict.	-.0531	(-.0228)	-.0780*	(-.0237)	-.0588	(-.0156)	-.0237	(-.0056)
Match over data sources	-.0717	(-.0187)	-.0009	(-.0002)	.0261	(.0042)	.0909	(.0129)
Constant	.2815***	(.0113)	.4964***	(.0296)	.6887***	(.0317)	.8427***	(.0486)
R squared	.202		.305		.344		.360	
Adjusted R squared	.195		.299		.339		.354	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 88.16

Regression Coefficients for Log of Total Post-Sentence Convictions
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0114	(.0181)	.0194*	(.0202)	.0274**	(.0239)	.0255*	(.0195)
Has job after sentence	-.0243***	(-.0401)	-.0312***	(-.0338)	-.0341***	(-.0310)	-.0363**	(-.0290)
Offender is on welfare	-.0164*	(-.0226)	-.0138	(-.0125)	-.0130	(-.0099)	-.0042	(-.0028)
Offender is Black	.0232**	(.0218)	.0824***	(.0604)	.1252***	(.0757)	.1668***	(.0962)
Offender is Hispanic	.0214	(.0184)	.0483**	(.0273)	.0672***	(.0319)	.0897***	(.0374)
Offender is female	-.0026	(-.0027)	-.0148	(-.0102)	-.0167	(-.0096)	-.0272	(-.0138)
Lives in urban area	.0026	(.0042)	.0161	(.0174)	.0256*	(.0231)	.0285*	(.0226)
Years at current address	-.0004	(-.0140)	-.0003	(-.0068)	-.0003	(-.0062)	-.0003	(-.0056)
History of drug problems	-.0027	(-.0038)	.0068	(.0063)	.0200	(.0156)	.0248	(.0169)
Treated for drugs/alch.	-.0017	(-.0022)	.0112	(.0092)	.0128	(.0088)	.0388*	(.0236)
Has needle marks	.0035	(.0029)	.0348*	(.0193)	.0551**	(.0256)	.0622**	(.0254)
Not a school drop out	-.0090	(-.0145)	-.0170*	(-.0179)	-.0258**	(-.0229)	-.0269*	(-.0209)
Doesn't live with family	.0043	(.0059)	-.0028	(-.0025)	-.0278*	(-.0208)	-.0287*	(-.0188)
Commited PO with group	-.0208***	(-.0343)	-.0213**	(-.0231)	-.0192*	(-.0174)	-.0192	(-.0153)
Victim was a stranger.	.0172*	(.0251)	.0297**	(.0285)	.0384***	(.0309)	.0568***	(.0402)
Presenting Offense								
PO property crime	.0065	(.0105)	.0331**	(.0354)	.0336*	(.0301)	.0441**	(.0347)
PO crime against person	-.0055	(-.0087)	.0065	(.0014)	.0069	(.0013)	.0037	(-.0018)
PO drug offense	.0109	(.0211)	.0234	(.0301)	.0208	(.0242)	.0197	(.0233)
PO Wolfgang severity	-.0008	(-.0200)	-.0008	(-.0142)	-.0013	(-.0184)	-.0016*	(-.0192)
Has detainers at arrest	-.0152	(-.0152)	-.0049	(-.0105)	-.0129	(-.0128)	-.0249	(-.0176)
Has pending charges	.0242***	(.0320)	.0498***	(.0433)	.0648***	(.0473)	.0759***	(.0486)
On probation at PO	.0332***	(.0309)	.0504***	(.0253)	.0609***	(.0214)	.0649***	(.0162)
Anamnestic Theory								
N prior adult arrests	.0039	(.1005)	.0164***	(.2156)	.0217***	(.2162)	.0249***	(.2310)
N prior adult conviction	.0005	(.0385)	.0015	(.0417)	.0074*	(.0688)	.0087*	(.0688)
N prior adult chg. conv.	.0000	(-.0295)	-.0042	(-.0583)	-.0080*	(-.0768)	-.0091*	(-.0767)
N charges past 5 years	.0042***	(.0784)	.0062***	(.0724)	.0084***	(.0777)	.0101***	(.0807)
N prior Part 1 charges	.0123***	(.0337)	.0110**	(.0164)	.0147***	(.0344)	.0151**	(.0330)
N prior property conv.	.0019	(.0224)	.0033	(.0259)	.0010	(.0149)	.0032	(.0207)
N prior persons conv.	-.0071**	(-.0354)	-.0103**	(-.0336)	-.0096*	(-.0263)	-.0095*	(-.0228)
N prior weapons conv.	.0010	(.0011)	.0096	(.0072)	-.0030	(-.0019)	.0003	(.0001)
Off street last 2 years	.0327***	(.0544)	.0793***	(.0836)	.0986***	(.0873)	.1105***	(.0867)
Delinquent Career/Onset								
N arrests as juvenile	.0092***	(.0616)	.0167***	(.0723)	.0192***	(.0646)	.0211***	(.0567)
N charges as juvenile	.0070	(.0320)	.0027	(.0275)	-.0029	(.0228)	-.0124	(.0187)
Age at first arrest	.0013	(.0564)	.0040***	(.0798)	.0052***	(.0832)	.0052***	(.0747)
Yrs since first incarc.	.0010***	(.1084)	.0021***	(.1296)	.0035***	(.1634)	.0036***	(.1558)
Yrs since first drug use	-.0003	(-.0013)	-.0008*	(-.0166)	-.0011**	(-.0202)	-.0013**	(-.0220)
Prior CJS-Offender Action								
N prior incarcerations	-.0019	(.0003)	-.0076	(-.0295)	-.0163***	(-.0528)	-.0218***	(-.0644)
N prior parole revokes	.0023	(.0037)	.0004	(.0004)	.0040	(.0034)	.0011	(.0009)
Bad conduct last probat.	.0116	(.0150)	.0178	(.0150)	.0210	(.0149)	.0297*	(.0186)
Recent parole revoked	.0371*	(.0419)	.0099	(.0240)	.0018	(.0188)	.0089	(.0254)
General Control Variables								
Offender age at sent.	-.0009	(-.1254)	-.0038***	(-.1877)	-.0059***	(-.2031)	-.0069***	(-.2068)
Off. born out of state	-.0235***	(-.0388)	-.0418***	(-.0453)	-.0592***	(-.0538)	-.0878***	(-.0701)
Coder prob. prognosis	-.0003*	(-.0253)	-.0006***	(-.0349)	-.0007***	(-.0358)	-.0009***	(-.0392)

*p<.05 **p<.01 ***p<.001

Table B8.16 (continued)

Regression Coefficients for Log of Total Post-Sentence Convictions
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0224	(-.0147)	-.0484*	(-.0208)	-.0720**	(-.0260)	-.0904***	(-.0287)
Black x prior adult arrs	-.0069***	(-.0485)	-.0104***	(-.0480)	-.0125***	(-.0483)	-.0145***	(-.0493)
Black x n prior prop cnv	.0060**	(.0437)	.0082**	(.0392)	.0107***	(.0432)	.0122***	(.0432)
Black x n charges as juv	.0260*	(.0224)	.0489**	(.0276)	.0609***	(.0289)	.0791***	(.0330)
Female x Part 1 charges	.0110	(.0265)	.0225**	(.0355)	.0285**	(.0377)	.0363**	(.0422)
Off. age x drug problem	-.0038**	(-.0501)	-.0056**	(-.0479)	-.0060**	(-.0430)	-.0066**	(-.0421)
Off. age x prior trtment	-.0022	(-.0257)	-.0050**	(-.0382)	-.0057**	(-.0366)	-.0069**	(-.0391)
Off. age x unemployed	-.0017**	(-.0249)	-.0031***	(-.0298)	-.0037***	(-.0300)	-.0040**	(-.0284)
Off. age x PO property	-.0032***	(-.0484)	-.0055***	(-.0543)	-.0059***	(-.0494)	-.0069***	(-.0505)
Off. age x chg pst 5 yrs	.0000	(.0031)	-.0001	(-.0141)	-.0002	(-.0154)	-.0001	(-.0110)
PO viol x has detainers	-.0177	(-.0057)	-.0744*	(-.0159)	-.0784*	(-.0140)	-.0996*	(-.0156)
PO prop x n adl.arrests	.0051*	(.0352)	.0104**	(.0475)	.0104**	(.0399)	.0140***	(.0472)
PO prop x prior prop con	-.0074***	(-.0536)	-.0104***	(-.0495)	-.0094**	(-.0374)	-.0138***	(-.0485)
PO prop x n juv. arrests	-.0020	(-.0081)	-.0043	(-.0116)	-.0071	(-.0161)	-.0098*	(-.0195)
PO prop x age at 1st arr	.0022	(.0274)	.0021	(.0171)	.0019	(.0128)	.0023	(.0136)
PO prop x yrs. 1st incar	.0010***	(.0371)	.0013**	(.0305)	.0010*	(.0210)	.0016**	(.0293)
PO drugs x n adl. convs.	.0120***	(.0582)	.0171***	(.0546)	.0150**	(.0402)	.0156**	(.0368)
PO drugs x Part 1 chgs.	-.0230***	(-.0741)	-.0201**	(-.0424)	-.0109	(-.0194)	-.0133	(-.0206)
PO drugs x last par. rev	.0711*	(.0235)	.1576**	(.0341)	.1791**	(.0325)	.2495***	(.0398)
Sentence								
Prison	-.0861***	(-.0956)	-.1351***	(-.0985)	-.1803***	(-.1103)	-.2394***	(-.1286)
Youth complex	-.0346*	(-.0340)	-.0437	(-.0282)	-.0505	(-.0273)	-.0798**	(-.0379)
Jail, probation, fine	-.0573**	(-.0360)	-.1022***	(-.0421)	-.1399***	(-.0484)	-.1523***	(-.0463)
Jail, probation	-.0348	(-.0136)	-.0777**	(-.0258)	-.1258***	(-.0392)	-.1394***	(-.0358)
Jail only	-.0272	(-.0283)	-.0594**	(-.0406)	-.0898***	(-.0516)	-.1182***	(-.0596)
Probation w/cond., fine	-.0257*	(-.0278)	-.0416*	(-.0295)	-.0633***	(-.0377)	-.0703**	(-.0368)
Probation, fine	-.0317**	(-.0376)	-.0508**	(-.0396)	-.0612**	(-.0400)	-.0717***	(-.0412)
Probation w/conditions	-.0207*	(-.0231)	-.0210	(-.0154)	-.0042	(-.0026)	-.0045	(-.0024)
Fined only	-.0089	(-.0046)	-.0250	(-.0085)	-.0300	(-.0086)	-.0568	(-.0143)
Other sanction	-.0419	(-.0147)	-.0716	(-.0164)	-.0608	(-.0117)	-.0938	(-.0159)
Dollars fined	.0000	(.0051)	.0000	(.0069)	.0000	(.0065)	.0000	(.0059)
Months on probation	.0001	(.0049)	-.0001	(-.0047)	.0001	(.0020)	.0000	(.0011)
Months to jail	.0007	(-.0079)	.0012	(.0002)	.0004	(-.0093)	-.0002	(-.0167)
Months to prison	-.0002*	(-.0247)	-.0003**	(-.0363)	-.0005***	(-.0417)	-.0006***	(-.0488)
First sanction of career	.0521**	(.0675)	.0727**	(.0488)	.0795**	(.0340)	.0812*	(.0306)
Progressive sanction	.0033	(.0046)	.0046	(.0042)	.0049	(.0037)	.0056	(.0037)
Sentence Interactions								
Prison x n adult arrests	.0056**	(.0266)	.0076**	(.0236)	.0072*	(.0188)	.0050	(.0116)
Prison x n arrsts as juv	-.0081**	(-.0228)	-.0136**	(-.0250)	-.0174***	(-.0269)	-.0238***	(-.0322)
Yth. comp x n adlt convs	-.0120***	(-.0468)	-.0083	(-.0212)	-.0069	(-.0148)	-.0105	(-.0198)
Yth. comp x chgs in 5 yr	.0047*	(.0233)	.0045	(.0146)	.0005	(.0013)	-.0007	(-.0018)
Yth. comp x prior n incs	.0194***	(.0418)	.0148	(.0209)	.0320***	(.0381)	.0361***	(.0377)
Jl & prob x inc lst 2 yr	.0595*	(.0209)	.0925**	(.0214)	.1166**	(.0226)	.1505**	(.0256)
Jail x yrs using drugs	.0021**	(.0298)	.0018	(.0167)	.0024*	(.0182)	.0023	(.0154)
Prb w/cnd, fn x adlt arr	-.0120***	(-.0558)	-.0146***	(-.0443)	-.0191***	(-.0488)	-.0153**	(-.0343)
Prb & fn x Part 1 chgs.	-.0244***	(-.0663)	-.0366***	(-.0652)	-.0473***	(-.0709)	-.0469***	(-.0617)
Prb w/cnd x adlt chg cnv	-.0078***	(-.0347)	-.0132***	(-.0383)	-.0149***	(-.0363)	-.0150***	(-.0322)
Mths to jail x PO prop.	-.0001**	(-.0272)	-.0001*	(-.0198)	-.0001*	(-.0166)	-.0001*	(-.0174)
Mths to pris x prop cnvs	.0000***	(-.0483)	.0000***	(-.0451)	.0000***	(-.0474)	.0000***	(-.0476)
Init sanc x black	-.0179	(-.0135)	-.0547**	(-.0272)	-.0892***	(-.0372)	-.0907***	(-.0332)
Init sanc x n adult arrs	.0159**	(.1037)	.0257**	(.1104)	.0282**	(.1015)	.0348**	(.1100)

*p<.05 **p<.01 ***p<.001

Table B8.16 (continued)

Regression Coefficients for Log of Total Post-Sentence Convictions
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-.0133	(-.0170)	-.0187	(-.0157)	-.0289*	(-.0204)	-.0214	(-.0132)
Case proceeds past arrst	.1553***	(.0584)	.1385*	(.0342)	.1441*	(.0299)	.1258	(.0229)
Case to Grand Jury	-.0064	(-.0088)	.0075	(.0068)	.0028	(.0021)	.0074	(.0049)
Case to Superior Court	-.0118	(-.0062)	-.0234	(-.0081)	-.0353	(-.0103)	-.0589	(-.0150)
Superior Court Convict.	-.0129	(-.0102)	-.0412	(-.0215)	-.0476	(-.0208)	-.0452	(-.0174)
Match over data sources	-.0679*	(-.0326)	-.0095	(-.0030)	.0026	(.0007)	-.0032	(-.0007)
Constant	.1397***	(.0182)	.2354***	(.0195)	.3366***	(.0178)	.4336***	(.0303)
R squared	.164		.254		.295		.310	
Adjusted R squared	.157		.248		.289		.304	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.17

Regression Coefficients for Log of Total Post-Sentence Persons Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0071	(.0137)	.0164*	(.0217)	.0270**	(.0301)	.0225*	(.0212)
Has job after sentence	-.0149**	(-.0299)	-.0121	(-.0167)	-.0167	(-.0194)	-.0233*	(-.0229)
Offender is on welfare	-.0020	(-.0033)	-.0031	(-.0035)	-.0021	(-.0020)	-.0007	(-.0006)
Offender is Black	.0299***	(.0419)	.0792***	(.0801)	.0940***	(.0782)	.1239***	(.0966)
Offender is Hispanic	.0205*	(.0215)	.0535***	(.0385)	.0678***	(.0410)	.0987***	(.0507)
Offender is female	-.0315***	(-.0402)	-.0606***	(-.0531)	-.0828***	(-.0611)	-.1053***	(-.0659)
Lives in urban area	.0114*	(.0226)	.0179*	(.0245)	.0243**	(.0281)	.0256*	(.0251)
Years at current address	-.0003	(-.0110)	-.0005	(-.0149)	-.0005	(-.0121)	-.0001	(-.0022)
History of drug problems	.0032	(.0055)	-.0004	(-.0005)	.0021	(.0021)	-.0155	(-.0131)
Treated for drugs/alch.	-.0098	(-.0150)	-.0133	(-.0139)	-.0114	(-.0101)	-.0144	(-.0108)
Has needle marks	-.0073	(-.0074)	-.0271*	(-.0191)	-.0262	(-.0156)	-.0050	(-.0025)
Not a school drop out	.0016	(.0030)	-.0028	(-.0038)	-.0129	(-.0146)	-.0171	(-.0164)
Doesn't live with family	-.0034	(-.0056)	-.0016	(-.0018)	-.0053	(-.0051)	-.0036	(-.0029)
Commited PO with group	-.0006	(-.0011)	.0079	(.0109)	.0040	(.0046)	.0053	(.0052)
Victim was a stranger	.0041	(.0073)	.0056	(.0068)	.0136	(.0140)	.0209	(.0182)
Presenting Offense								
PO property crime	-.0006	(-.0012)	-.0033	(-.0044)	-.0067	(-.0076)	-.0060	(-.0058)
PO crime against person	.0211*	(.0291)	.0486***	(.0482)	.0627***	(.0522)	.0718***	(.0481)
PO drug offense	.0072	(.0089)	-.0078	(-.0109)	-.0179	(-.0174)	-.0220	(-.0161)
PO Wolfgang severity	.0001	(.0043)	.0000	(-.0002)	-.0002	(-.0029)	.0002	(.0028)
Has detainers at arrest	-.0046	(-.0134)	-.0188	(-.0228)	-.0160	(-.0206)	.0008	(-.0154)
Has pending charges	.0129*	(.0207)	.0165*	(.0182)	.0255**	(.0237)	.0353**	(.0279)
On probation at PO	-.0052	(-.0206)	.0013	(-.0201)	.0077	(-.0155)	.0198	(-.0072)
Anamnestic Theory								
N prior adult arrests	.0065**	(.1158)	.0134***	(.1629)	.0151***	(.1662)	.0182***	(.1766)
N prior adult conviction	-.0064***	(-.0844)	-.0077**	(-.0644)	-.0076**	(-.0526)	-.0083*	(-.0453)
N prior adult chg. conv.	.0001	(-.0196)	-.0012	(-.0268)	-.0051	(-.0580)	-.0070*	(-.0656)
N charges past 5 years	.0031***	(.0753)	.0051***	(.0772)	.0047**	(.0610)	.0062***	(.0636)
N prior Part 1 charges	.0057*	(.0164)	-.0011	(-.0444)	.0038	(.0067)	.0022	(.0019)
N prior property conv.	-.0037*	(-.0614)	-.0014	(-.0283)	-.0024	(-.0314)	-.0006	(-.0173)
N prior persons conv.	.0147***	(.0893)	.0254***	(.1058)	.0334***	(.1171)	.0413***	(.1229)
N prior weapons conv.	.0216**	(.0301)	.0199*	(.0190)	.0322**	(.0260)	.0569***	(.0390)
Off street last 2 years	.0131*	(.0317)	.0342***	(.0495)	.0406***	(.0484)	.0618***	(.0571)
Delinquent Career/Onset								
N arrests as juvenile	.0080***	(.0643)	.0115***	(.0664)	.0138***	(.0697)	.0190***	(.0792)
N charges as juvenile	-.0051	(.0035)	-.0162	(.0121)	-.0050	(.0219)	-.0098	(.0193)
Age at first arrest	.0008	(.0245)	.0028**	(.0477)	.0041***	(.0631)	.0039**	(.0633)
Yrs since first incarc.	.0003	(.0279)	.0007*	(.0457)	.0012***	(.0677)	.0014**	(.0632)
Yrs since first drug use	.0000	(-.0071)	-.0004	(-.0171)	-.0007*	(-.0206)	-.0009*	(-.0250)
Prior CJS-Offender Action								
N prior incarcerations	.0041	(.0436)	.0061	(.0462)	.0063	(.0381)	.0056	(.0280)
N prior parole revokes	-.0021	(-.0041)	-.0172	(-.0226)	-.0161	(-.0178)	-.0247*	(-.0231)
Bad conduct last probat.	.0131*	(.0205)	.0213*	(.0229)	.0166	(.0150)	.0200	(.0154)
Recent parole revoked	.0339**	(.0248)	.0345	(.0187)	.0314	(.0176)	.0433	(.0252)
General Control Variables								
Offender age at sent.	-.0008	(-.0650)	-.0032***	(-.1167)	-.0049***	(-.1516)	-.0059***	(-.1740)
Off. born out of state	-.0146**	(-.0293)	-.0297***	(-.0410)	-.0425***	(-.0493)	-.0563***	(-.0554)
Coder prob. prognosis	-.0001	(-.0064)	-.0003*	(-.0270)	-.0004*	(-.0254)	-.0005**	(-.0284)

*p<.05 **p<.01 ***p<.001

Table B8.17 (continued)

Regression Coefficients for Log of Total Post-Sentence Persons Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0175	(-.0140)	-.0445**	(-.0244)	-.0556**	(-.0257)	-.0662**	(-.0259)
Black x prior adult arrs	-.0052**	(-.0443)	-.0089***	(-.0522)	-.0131***	(-.0646)	-.0147***	(-.0616)
Black x n prior prop cnv	.0022	(.0200)	.0037	(.0226)	.0063*	(.0325)	.0064*	(.0278)
Black x n charges as juv	.0155	(.0162)	.0561***	(.0403)	.0522***	(.0317)	.0651***	(.0335)
Female x Part 1 charges	-.0080	(-.0233)	-.0101	(-.0204)	-.0072	(-.0122)	-.0005	(-.0007)
Off. age x drug problem	-.0004	(-.0070)	-.0012	(-.0136)	-.0024	(-.0221)	-.0034	(-.0267)
Off. age x prior trtment	-.0014	(-.0198)	-.0019	(-.0187)	-.0024	(-.0198)	-.0047*	(-.0323)
Off. age x unemployed	-.0012*	(-.0207)	-.0016*	(-.0197)	-.0022*	(-.0222)	-.0023*	(-.0198)
Off. age x PO property	-.0004	(-.0075)	-.0003	(-.0038)	-.0009	(-.0095)	-.0028	(-.0252)
Off. age x chg pst 5 yrs	-.0001	(-.0213)	-.0003**	(-.0359)	-.0003*	(-.0324)	-.0004*	(-.0341)
PO viol x has detainers	-.0455*	(-.0179)	-.0745**	(-.0202)	-.1007**	(-.0230)	-.1645***	(-.0318)
PO prop x n adl.arrests	.0011	(.0097)	.0029	(.0166)	.0053	(.0259)	.0093*	(.0385)
PO prop x prior prop con	-.0016	(-.0145)	-.0062*	(-.0374)	-.0083**	(-.0425)	-.0103**	(-.0444)
PO prop x n juv. arrests	-.0036	(-.0177)	-.0044	(-.0151)	-.0039	(-.0113)	-.0042	(-.0101)
PO prop x age at 1st arr	.0000	(.0002)	-.0013	(-.0139)	-.0012	(-.0104)	.0008	(.0059)
PO prop x yrs. 1st incar	.0000	(-.0020)	.0000	(-.0014)	.0001	(.0034)	.0000	(.0001)
PO drugs x n adl. convs.	.0012	(.0071)	.0040	(.0164)	.0047	(.0162)	.0073	(.0212)
PO drugs x Part 1 chgs.	-.0040	(-.0155)	-.0137*	(-.0367)	-.0093	(-.0210)	-.0134	(-.0256)
PO drugs x last par. rev	-.0327	(-.0131)	-.0242	(-.0067)	.0034	(.0008)	.0472	(.0093)
Sentence								
Prison	-.0449**	(-.0606)	-.0530**	(-.0492)	-.0702**	(-.0549)	-.1076***	(-.0713)
Youth complex	-.0026	(-.0031)	.0034	(.0028)	-.0045	(-.0031)	-.0203	(-.0119)
Jail, probation, fine	-.0244	(-.0186)	-.0339	(-.0178)	-.0584*	(-.0258)	-.0788*	(-.0295)
Jail, probation	-.0022	(.0177)	.0061	(.0209)	-.0001	(.0140)	.0221	(.0144)
Jail only	.0027	(.0034)	.0194	(.0169)	.0009	(.0007)	-.0193	(-.0120)
Probation w/cond., fine	-.0013	(-.0017)	-.0187	(-.0169)	-.0148	(-.0113)	-.0301	(-.0195)
Probation, fine	-.0081	(-.0117)	-.0104	(-.0103)	.0001	(.0001)	-.0004	(-.0003)
Probation w/conditions	-.0031	(-.0041)	.0063	(.0058)	.0175	(.0138)	.0123	(.0082)
Fined only	.0028	(.0018)	.0124	(.0054)	.0019	(.0007)	-.0160	(-.0050)
Other sanction	-.0049	(-.0021)	-.0025	(-.0007)	.0047	(.0012)	-.0010	(-.0002)
Dollars fined	.0000	(-.0004)	.0000	(-.0015)	.0000	(.0173)	.0000*	(.0193)
Months on probation	-.0002	(-.0104)	.0000	(.0007)	-.0001	(-.0057)	-.0001	(-.0032)
Months to jail	.0002	(.0006)	.0005	(-.0071)	.0008	(-.0051)	.0006	(-.0143)
Months to prison	.0000	(-.0002)	-.0001	(-.0156)	-.0002	(-.0170)	-.0003**	(-.0317)
First sanction of career	.0209	(.0244)	.0261	(.0115)	.0239	(.0021)	.0122	(-.0048)
Progressive sanction	.0036	(.0059)	.0041	(.0047)	.0105	(.0101)	.0033	(.0027)
Sentence Interactions								
Prison x n adult arrests	.0007	(.0040)	.0010	(.0041)	.0035	(.0117)	-.0009	(-.0026)
Prison x n arrsts as juv	-.0014	(-.0047)	-.0011	(-.0026)	-.0025	(-.0049)	-.0111*	(-.0185)
Yth. comp x n adlt convs	-.0083**	(-.0392)	-.0032	(-.0105)	-.0032	(-.0087)	-.0039	(-.0090)
Yth. comp x chgs in 5 yr	.0062***	(.0370)	.0049	(.0202)	.0050	(.0173)	.0021	(.0063)
Yth. comp x prior n incs	.0089	(.0232)	.0153*	(.0275)	.0117	(.0177)	.0087	(.0112)
Jl & prob x inc lst 2 yr	.0721***	(.0308)	.0900**	(.0265)	.0883*	(.0219)	.0309	(.0065)
Jail x yrs using drugs	-.0010	(-.0171)	-.0001	(-.0015)	.0000	(-.0005)	-.0005	(-.0044)
Prb w/cnd, fn x adlt arr	-.0036	(-.0204)	-.0067*	(-.0261)	-.0034	(-.0109)	-.0029	(-.0081)
Prb & fn x Part 1 chgs.	-.0135*	(-.0446)	-.0101	(-.0230)	.0033	(.0063)	.0095	(.0154)
Prb w/cnd x adlt chg cnv	-.0039*	(-.0211)	-.0069*	(-.0254)	-.0065*	(-.0201)	-.0079*	(-.0207)
Mths to jail x PO prop.	.0000	(-.0048)	-.0001*	(-.0182)	-.0001*	(-.0203)	-.0001**	(-.0263)
Mths to pris x prop cnvs	.0000	(-.0056)	.0000	(-.0085)	.0000	(-.0071)	.0000	(-.0058)
Init sanc x black	-.0177	(-.0163)	-.0389*	(-.0246)	-.0501**	(-.0267)	-.0398	(-.0180)
Init sanc x n adult arrs	.0077	(.0608)	.0122	(.0666)	.0165*	(.0760)	.0191*	(.0742)

*p<.05 **p<.01 ***p<.001

Table B8.17 (continued)

Regression Coefficients for Log of Total Post-Sentence Persons Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	.0011	(.0017)	-.0072	(-.0077)	-.0126	(-.0113)	-.0217	(-.0166)
Case proceeds past arrst	.0456	(.0208)	.0655	(.0206)	.0906	(.0240)	.0804	(.0180)
Case to Grand Jury	.0028	(.0047)	.0109	(.0126)	.0253*	(.0248)	.0337*	(.0279)
Case to Superior Court	.0212	(.0136)	.0302	(.0133)	.0074	(.0027)	-.0110	(-.0035)
Superior Court Convict.	-.0206	(-.0199)	-.0209	(-.0139)	-.0178	(-.0099)	-.0069	(-.0033)
Match over data sources	-.0206	(-.0120)	-.0324	(-.0130)	-.0160	(-.0054)	.0172	(.0049)
Constant	.0447**	(.0029)	.0842***	(.0055)	.1380***	(.0164)	.2113***	(.0249)
R squared	.091		.148		.174		.199	
Adjusted R squared	.083		.141		.167		.192	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.18

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0149	(.0156)	.0169*	(.0208)	.0193**	(.0256)	.0164**	(.0239)
Has job after sentence	-.0427***	(-.0464)	-.0290***	(-.0371)	-.0230***	(-.0318)	-.0190**	(-.0288)
Offender is on welfare	-.0296**	(-.0267)	-.0164*	(-.0176)	-.0163*	(-.0188)	-.0113	(-.0143)
Offender is Black	.0568***	(.0351)	.0788***	(.0715)	.0881***	(.0880)	.0877***	(.1071)
Offender is Hispanic	.0430**	(.0244)	.0500***	(.0334)	.0527***	(.0380)	.0541***	(.0427)
Offender is female	-.0211	(-.0146)	-.0269*	(-.0219)	-.0243*	(-.0213)	-.0257**	(-.0248)
Lives in urban area	.0047	(.0050)	.0121	(.0154)	.0166**	(.0228)	.0167**	(.0251)
Years at current address	-.0008	(-.0176)	-.0007*	(-.0172)	-.0004	(-.0127)	-.0002	(-.0070)
History of drug problems	.0049	(.0046)	.0127	(.0139)	.0197*	(.0233)	.0142	(.0184)
Treated for drugs/alch.	-.0179	(-.0148)	-.0035	(-.0034)	.0106	(.0111)	.0194*	(.0223)
Has needle marks	.0208	(.0116)	.0242	(.0158)	.0319**	(.0226)	.0310**	(.0240)
Not a school drop out	-.0172*	(-.0182)	-.0151*	(-.0188)	-.0140*	(-.0188)	-.0084	(-.0124)
Doesn't live with family	-.0058	(-.0052)	-.0119	(-.0125)	-.0186**	(-.0211)	-.0162**	(-.0202)
Commited PO with group	-.0191*	(-.0208)	-.0090	(-.0115)	-.0054	(-.0075)	-.0061	(-.0093)
Victim was a stranger	.0213*	(.0205)	.0203**	(.0231)	.0201**	(.0246)	.0197**	(.0264)
Presenting Offense								
PO property crime	.0140	(.0150)	.0206*	(.0260)	.0230**	(.0313)	.0241**	(.0360)
PO crime against person	-.0125	(-.0133)	.0079	(.0016)	.0111	(.0058)	.0100	(.0069)
PO drug offense	.0061	(.0099)	.0075	(.0145)	-.0001	(.0058)	.0042	(.0119)
PO Wolfgang severity	-.0006	(-.0104)	.0000	(.0009)	.0003	(.0071)	.0003	(.0079)
Has detainers at arrest	-.0195	(-.0149)	.0145	(-.0030)	.0151	(-.0021)	.0037	(-.0072)
Has pending charges	.0506***	(.0440)	.0547***	(.0561)	.0466***	(.0516)	.0425***	(.0516)
On probation at PO	.0588***	(.0296)	.0381***	(.0217)	.0378***	(.0220)	.0270**	(.0190)
Anamnestic Theory								
N prior adult arrests	.0122***	(.1418)	.0153***	(.2050)	.0153***	(.2058)	.0134***	(.2061)
N prior adult conviction	-.0053	(-.0104)	-.0059*	(-.0170)	-.0026	(.0049)	-.0021	(.0026)
N prior adult chg. conv.	-.0012	(-.0412)	-.0052*	(-.0790)	-.0070***	(-.1023)	-.0059**	(-.0953)
N charges past 5 years	.0100***	(.1137)	.0101***	(.1352)	.0081***	(.1198)	.0083***	(.1350)
N prior Part 1 charges	.0160***	(.0165)	.0113***	(.0128)	.0115***	(.0283)	.0116***	(.0405)
N prior property conv.	.0016	(.0114)	.0046	(.0405)	.0037	(.0410)	.0024	(.0226)
N prior persons conv.	-.0013	(-.0042)	-.0017	(-.0067)	.0004	(.0016)	-.0006	(-.0028)
N prior weapons conv.	.0229	(.0173)	.0183*	(.0163)	.0180*	(.0173)	.0178*	(.0188)
Off street last 2 years	.0560***	(.0624)	.0664***	(.0836)	.0694***	(.0931)	.0645***	(.0934)
Delinquent Career/Onset								
N arrests as juvenile	.0173***	(.0774)	.0153***	(.0780)	.0108***	(.0642)	.0110***	(.0771)
N charges as juvenile	-.0037	(.0100)	.0013	(.0238)	-.0020	(.0271)	-.0099	(.0213)
Age at first arrest	.0031**	(.0683)	.0041***	(.0907)	.0036***	(.0881)	.0029***	(.0816)
Yrs since first incarc.	.0021***	(.1309)	.0021***	(.1469)	.0023***	(.1615)	.0019***	(.1492)
Yrs since first drug use	-.0006	(-.0100)	-.0005	(-.0141)	-.0004	(-.0135)	-.0005*	(-.0180)
Prior CJS-Offender Action								
N prior incarcerations	-.0002	(.0177)	.0002	(.0143)	-.0026	(-.0019)	-.0040	(-.0159)
N prior parole revokes	.0109	(.0112)	.0026	(.0032)	-.0019	(-.0025)	.0015	(.0022)
Bad conduct last probat.	.0180	(.0153)	.0163	(.0164)	.0165*	(.0178)	.0108	(.0128)
Recent parole revoked	.0577*	(.0392)	.0493**	(.0439)	.0420**	(.0405)	.0447**	(.0464)
General Control Variables								
Offender age at sent.	-.0026*	(-.1435)	-.0039***	(-.1970)	-.0040***	(-.2060)	-.0036***	(-.2049)
Off. born out of state	-.0295***	(-.0320)	-.0373***	(-.0477)	-.0396***	(-.0546)	-.0386***	(-.0585)
Coder prob. prognosis	-.0003*	(-.0206)	-.0005***	(-.0359)	-.0004***	(-.0336)	-.0004***	(-.0371)

*p<.05 **p<.01 ***p<.001

Table 88.18 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0566**	(-.0244)	-.0386*	(-.0196)	-.0409**	(-.0225)	-.0260*	(-.0157)
Black x prior adult arrs	-.0130***	(-.0601)	-.0107***	(-.0582)	-.0104***	(-.0611)	-.0101***	(-.0651)
Black x n prior prop cnv	.0100***	(.0483)	.0076***	(.0432)	.0084***	(.0518)	.0079***	(.0533)
Black x n charges as juv	.0284	(.0161)	.0374**	(.0250)	.0471***	(.0339)	.0531***	(.0420)
Female x Part 1 charges	.0195*	(.0308)	.0168*	(.0314)	.0176**	(.0353)	.0151**	(.0333)
Off. age x drug problem	-.0047**	(-.0407)	-.0042**	(-.0430)	-.0039**	(-.0422)	-.0029**	(-.0351)
Off. age x prior trtment	-.0041*	(-.0315)	-.0049**	(-.0441)	-.0043**	(-.0423)	-.0049***	(-.0527)
Off. age x unemployed	-.0020*	(-.0192)	-.0024**	(-.0269)	-.0021**	(-.0255)	-.0017**	(-.0229)
Off. age x PO property	-.0048***	(-.0476)	-.0041***	(-.0480)	-.0039***	(-.0493)	-.0037***	(-.0511)
Off. age x chg pst 5 yrs	.0001	(.0061)	-.0002	(-.0194)	-.0002**	(-.0317)	-.0002**	(-.0340)
PO viol x has detainers	-.0424	(-.0091)	-.0975***	(-.0246)	-.0916***	(-.0249)	-.0674**	(-.0201)
PO prop x n adl.arrests	.0095**	(.0434)	.0103***	(.0558)	.0098***	(.0568)	.0116***	(.0743)
PO prop x prior prop con	-.0118***	(-.0563)	-.0109***	(-.0611)	-.0100***	(-.0606)	-.0103***	(-.0685)
PO prop x n juv. arrests	-.0072*	(-.0195)	-.0068*	(-.0215)	-.0040	(-.0138)	-.0037	(-.0138)
PO prop x age at 1st arr	.0026	(.0213)	.0013	(.0124)	.0015	(.0159)	.0016	(.0183)
PO prop x yrs. 1st incar	.0013**	(.0304)	.0010**	(.0272)	.0008*	(.0233)	.0007*	(.0222)
PO drugs x n adl. convs.	.0166***	(.0532)	.0168***	(.0633)	.0134***	(.0543)	.0101***	(.0452)
PO drugs x Part 1 chgs.	-.0390***	(-.0826)	-.0292***	(-.0728)	-.0211***	(-.0568)	-.0166***	(-.0490)
PO drugs x last par. rev	.0801	(.0174)	.0993*	(.0254)	.0860*	(.0237)	.0859**	(.0260)
Sentence								
Prison	-.1405***	(-.1027)	-.1026***	(-.0884)	-.0962***	(-.0894)	-.0753***	(-.0768)
Youth complex	-.0390	(-.0252)	-.0218	(-.0166)	-.0193	(-.0159)	-.0150	(-.0135)
Jail, probation, fine	-.1000***	(-.0413)	-.0927***	(-.0452)	-.1068***	(-.0561)	-.0830***	(-.0479)
Jail, probation	-.0674*	(-.0166)	-.0637**	(-.0225)	-.0864***	(-.0412)	-.0635***	(-.0341)
Jail only	-.0578*	(-.0396)	-.0536**	(-.0433)	-.0643***	(-.0560)	-.0552***	(-.0528)
Probation w/cond., fine	-.0346*	(-.0246)	-.0363**	(-.0304)	-.0413***	(-.0374)	-.0367***	(-.0365)
Probation, fine	-.0344*	(-.0269)	-.0305*	(-.0281)	-.0312**	(-.0310)	-.0261*	(-.0285)
Probation w/conditions	-.0100	(-.0074)	-.0114	(-.0098)	-.0072	(-.0068)	-.0050	(-.0052)
Fined only	.0003	(.0001)	-.0015	(-.0006)	-.0046	(-.0020)	-.0130	(-.0062)
Other sanction	-.0605	(-.0139)	-.0466	(-.0127)	-.0342	(-.0100)	-.0182	(-.0058)
Dollars fined	.0000	(.0008)	.0000	(.0027)	.0000	(.0067)	.0000	(.0089)
Months on probation	-.0001	(-.0045)	.0001	(.0048)	.0002	(.0102)	.0002	(.0094)
Months to jail	.0008	(-.0083)	.0003	(-.0118)	.0008	(-.0049)	.0001	(-.0146)
Months to prison	-.0001	(-.0136)	-.0002*	(-.0225)	-.0002*	(-.0211)	-.0002*	(-.0225)
First sanction of career	.0588*	(.0401)	.0463*	(.0293)	.0366*	(.0176)	.0261	(.0135)
Progressive sanction	.0007	(.0006)	.0061	(.0065)	.0071	(.0082)	.0117	(.0147)
Sentence Interactions								
Prison x n adult arrests	.0072**	(.0226)	.0064**	(.0235)	.0053**	(.0210)	.0050**	(.0217)
Prison x n arrsts as juv	-.0022	(-.0040)	-.0039	(-.0085)	.0002	(.0005)	.0042	(.0107)
Yth. comp x n adlt convs	-.0148**	(-.0380)	-.0123**	(-.0372)	-.0110**	(-.0357)	-.0128***	(-.0459)
Yth. comp x chgs in 5 yr	.0037	(.0121)	.0038	(.0147)	.0048*	(.0197)	.0052**	(.0234)
Yth. comp x prior n incs	.0401***	(.0569)	.0239***	(.0400)	.0233***	(.0420)	.0166**	(.0328)
Jl & prob x inc lst 2 yr	.1198***	(.0277)	.0905**	(.0247)	.0784**	(.0231)	.0535*	(.0173)
Jail x yrs using drugs	.0019*	(.0179)	.0004	(.0039)	.0007	(.0083)	.0002	(.0028)
Prb w/cnd, fn x adlt arr	-.0186***	(-.0566)	-.0150***	(-.0540)	-.0123***	(-.0477)	-.0104***	(-.0444)
Prb & fn x Part 1 chgs.	-.0378***	(-.0677)	-.0276***	(-.0581)	-.0269***	(-.0611)	-.0235***	(-.0587)
Prb w/cnd x adlt chg cnv	-.0163***	(-.0475)	-.0100***	(-.0344)	-.0093***	(-.0343)	-.0056**	(-.0228)
Mths to jail x PO prop.	-.0001**	(-.0238)	-.0001*	(-.0200)	-.0001**	(-.0227)	-.0001*	(-.0199)
Mths to pris x prop cnvs	.0000*	(-.0243)	.0000***	(-.0341)	.0000**	(-.0289)	.0000**	(-.0285)
Init sanc x black	-.0430*	(-.0214)	-.0492**	(-.0289)	-.0520***	(-.0330)	-.0377**	(-.0262)
Init sanc x n adult arrs	.0200*	(.0862)	.0159*	(.0805)	.0113	(.0616)	.0089	(.0533)

*p<.05 **p<.01 ***p<.001

Table 88.18 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Arrest Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-.0237	(-.0200)	-.0127	(-.0126)	-.0114	(-.0123)	-.0075	(-.0088)
Case proceeds past arrst	.2000***	(.0495)	.1106*	(.0323)	.0847*	(.0267)	.0737*	(.0255)
Case to Grand Jury	-.0046	(-.0042)	.0027	(.0029)	.0013	(.0015)	-.0005	(-.0007)
Case to Superior Court	.0430	(.0149)	.0111	(.0045)	-.0077	(-.0034)	-.0096	(-.0047)
Superior Court Convict.	-.0391	(-.0204)	-.0480**	(-.0296)	-.0317*	(-.0211)	-.0204	(-.0149)
Match over data sources	-.0257	(-.0081)	.0458	(.0171)	.0424	(.0171)	.0416	(.0184)
Constant	.2182***	(.0120)	.2004***	(.0053)	.1926***	(-.0061)	.1631***	(-.0095)
R squared	.210		.331		.367		.381	
Adjusted R squared	.203		.326		.361		.376	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.19

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0279*	(.0217)	.0282**	(.0245)	.0277**	(.0257)	.0251**	(.0251)
Has job after sentence	-.0530***	(-.0429)	-.0373***	(-.0339)	-.0279**	(-.0270)	-.0243**	(-.0253)
Offender is on welfare	-.0396**	(-.0268)	-.0260*	(-.0197)	-.0263**	(-.0212)	-.0214*	(-.0185)
Offender is Black	.0686***	(.0301)	.1024***	(.0623)	.1184***	(.0776)	.1213***	(.0976)
Offender is Hispanic	.0538*	(.0227)	.0737***	(.0349)	.0770***	(.0389)	.0823***	(.0447)
Offender is female	-.0335	(-.0173)	-.0445**	(-.0257)	-.0383*	(-.0236)	-.0453**	(-.0300)
Lives in urban area	.0124	(.0100)	.0190	(.0172)	.0243**	(.0233)	.0227**	(.0235)
Years at current address	-.0011*	(-.0184)	-.0010*	(.0194)	-.0009*	(-.0175)	-.0005	(-.0111)
History of drug problems	.0142	(.0099)	.0251	(.0195)	.0313*	(.0260)	.0229*	(.0204)
Treated for drugs/alch.	-.0230	(-.0141)	-.0011	(-.0007)	.0194	(.0143)	.0295*	(.0234)
Has needle marks	.0148	(.0061)	.0278	(.0129)	.0384*	(.0190)	.0446**	(.0238)
Not a school drop out	-.0201	(-.0159)	-.0188*	(-.0166)	-.0173*	(-.0163)	-.0116	(-.0117)
Doesn't live with family	-.0073	(-.0048)	-.0107	(-.0079)	-.0268**	(-.0213)	-.0231**	(-.0197)
Commited PO with group	-.0213*	(-.0173)	-.0113	(-.0103)	-.0063	(-.0061)	-.0074	(-.0077)
Victim was a stranger	.0210	(.0151)	.0238*	(.0191)	.0266**	(.0228)	.0279**	(.0257)
Presenting Offense								
PO property crime	.0200	(.0160)	.0281*	(.0252)	.0330**	(.0315)	.0386***	(.0396)
PO crime against person	-.0172	(-.0150)	.0131	(.0016)	.0131	(.0024)	.0133	(.0042)
PO drug offense	.0171	(.0148)	.0069	(.0117)	.0006	(.0054)	.0107	(.0147)
PO Wolfgang severity	-.0008	(-.0097)	-.0003	(-.0045)	.0003	(.0041)	.0003	(.0051)
Has detainers at arrest	-.0137	(-.0128)	.0319	(-.0006)	.0328	(.0012)	.0206	(-.0024)
Has pending charges	.0689***	(.0448)	.0786***	(.0572)	.0707***	(.0548)	.0643***	(.0537)
On probation at PO	.0740***	(.0292)	.0502**	(.0192)	.0504***	(.0174)	.0362**	(.0133)
Anamnestic Theory								
N prior adult arrests	.0167***	(.1388)	.0224***	(.2143)	.0212***	(.2029)	.0175***	(.1967)
N prior adult conviction	-.0103*	(-.0269)	-.0128***	(-.0446)	-.0074*	(-.0185)	-.0054	(-.0127)
N prior adult chg. conv.	-.0041	(-.0570)	-.0078*	(-.0807)	-.0092**	(-.0930)	-.0066*	(-.0781)
N charges past 5 years	.0130***	(.1151)	.0121***	(.1201)	.0103***	(.1097)	.0108***	(.1229)
N prior Part 1 charges	.0257***	(.0351)	.0162***	(.0096)	.0155***	(.0238)	.0145***	(.0289)
N prior property conv.	.0009	(.0146)	.0069*	(.0528)	.0056	(.0527)	.0050	(.0426)
N prior persons conv.	-.0017	(-.0041)	-.0013	(-.0035)	.0016	(.0047)	.0017	(.0053)
N prior weapons conv.	.0304	(.0171)	.0328*	(.0207)	.0302*	(.0204)	.0352**	(.0255)
Off street last 2 years	.0783***	(.0638)	.0970***	(.0854)	.1008***	(.0939)	.0955***	(.0947)
Delinquent Career/Onset								
N arrests as juvenile	.0210***	(.0745)	.0192***	(.0735)	.0138***	(.0595)	.0149***	(.0732)
N charges as juvenile	-.0091	(.0044)	-.0001	(.0184)	.0051	(.0297)	-.0058	(.0248)
Age at first arrest	.0032*	(.0630)	.0051***	(.0832)	.0045***	(.0834)	.0038***	(.0786)
Yrs since first incarc.	.0028***	(.1304)	.0028***	(.1399)	.0030***	(.1535)	.0025***	(.1397)
Yrs since first drug use	-.0009*	(-.0141)	-.0009*	(-.0184)	-.0008**	(-.0169)	-.0007*	(-.0172)
Prior CJS-Offender Action								
N prior incarcerations	.0020	(.0232)	.0048	(.0280)	.0019	(.0158)	-.0029	(-.0085)
N prior parole revokes	.0196	(.0151)	.0055	(.0048)	-.0054	(-.0049)	.0001	(.0001)
Bad conduct last probat.	.0227	(.0144)	.0282*	(.0200)	.0253*	(.0191)	.0127	(.0104)
Recent parole revoked	.0772*	(.0361)	.0644**	(.0417)	.0607**	(.0386)	.0677**	(.0447)
General Control Variables								
Offender age at sent.	-.0026	(-.1356)	-.0048***	(-.1875)	-.0051***	(-.1974)	-.0048***	(-.1966)
Off. born out of state	-.0379***	(-.0307)	-.0533***	(-.0484)	-.0593***	(-.0573)	-.0554***	(-.0577)
Coder prob. prognosis	-.0005*	(-.0211)	-.0007***	(-.0382)	-.0006***	(-.0351)	-.0007***	(-.0408)

*p<.05 **p<.01 ***p<.001

Table B8.19 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0663*	(-.0213)	-.0543*	(-.0196)	-.0637**	(-.0245)	-.0461*	(-.0191)
Black x prior adult arrs	-.0194***	(-.0670)	-.0169***	(-.0656)	-.0159***	(-.0656)	-.0152***	(-.0676)
Black x n prior prop cnv	.0151***	(.0544)	.0119***	(.0478)	.0120***	(.0514)	.0112***	(.0516)
Black x n charges as juv	.0324	(.0137)	.0443*	(.0210)	.0550***	(.0278)	.0648***	(.0352)
Female x Part 1 charges	.0237*	(.0280)	.0155	(.0205)	.0187*	(.0263)	.0163*	(.0248)
Off. age x drug problem	-.0055*	(-.0357)	-.0051**	(-.0367)	-.0050**	(-.0382)	-.0033*	(-.0273)
Off. age x prior trtment	-.0065*	(-.0370)	-.0082***	(-.0527)	-.0070***	(-.0480)	-.0081***	(-.0597)
Off. age x unemployed	-.0033*	(-.0238)	-.0036***	(-.0292)	-.0030**	(-.0260)	-.0024**	(-.0227)
Off. age x PO property	-.0069***	(-.0513)	-.0059***	(-.0489)	-.0058***	(-.0509)	-.0056***	(-.0528)
Off. age x chg pst 5 yrs	.0000	(.0004)	-.0003*	(-.0276)	-.0004**	(-.0376)	-.0004**	(-.0379)
PO viol x has detainers	-.0920	(-.0147)	-.1682***	(-.0300)	-.1536***	(-.0292)	-.1280***	(-.0262)
PO prop x n adl.arrests	.0096*	(.0329)	.0124***	(.0475)	.0116***	(.0474)	.0157***	(.0692)
PO prop x prior prop con	-.0136**	(-.0484)	-.0136***	(-.0543)	-.0116***	(-.0494)	-.0130***	(-.0596)
PO prop x n juv. arrests	-.0058	(-.0116)	-.0062	(-.0139)	-.0037	(-.0088)	-.0040	(-.0103)
PO prop x age at 1st arr	.0048*	(.0290)	.0023	(.0154)	.0028	(.0203)	.0029	(.0224)
PO prop x yrs. 1st incar	.0018**	(.0331)	.0015**	(.0307)	.0013**	(.0277)	.0010*	(.0244)
PO drugs x n adl. convs.	.0236***	(.0563)	.0244***	(.0650)	.0200***	(.0569)	.0155***	(.0475)
PO drugs x Part 1 chgs.	-.0497***	(-.0785)	-.0434***	(-.0768)	-.0323***	(-.0610)	-.0259***	(-.0525)
PO drugs x last par. rev	.0741	(.0120)	.1408*	(.0255)	.1018	(.0197)	.0997*	(.0208)
Sentence								
Prison	-.1929***	(-.1052)	-.1414***	(-.0864)	-.1303***	(-.0848)	-.1055***	(-.0739)
Youth complex	-.0502	(-.0242)	-.0312	(-.0169)	-.0277	(-.0160)	-.0164	(-.0102)
Jail, probation, fine	-.1430***	(-.0441)	-.1240***	(-.0428)	-.1484***	(-.0546)	-.1180***	(-.0468)
Jail, probation	-.0927*	(-.0198)	-.0832**	(-.0220)	-.1141***	(-.0388)	-.0865***	(-.0316)
Jail only	-.0747*	(-.0382)	-.0694**	(-.0397)	-.0842***	(-.0514)	-.0744***	(-.0489)
Probation w/cond., fine	-.0385	(-.0205)	-.0480**	(-.0286)	-.0557**	(-.0353)	-.0510**	(-.0348)
Probation, fine	-.0444*	(-.0259)	-.0446*	(-.0291)	-.0412*	(-.0287)	-.0370*	(-.0277)
Probation w/conditions	-.0221	(-.0122)	-.0234	(-.0144)	-.0118	(-.0077)	-.0086	(-.0061)
Fined only	.0029	(.0007)	-.0001	(.0000)	-.0051	(-.0015)	-.0199	(-.0065)
Other sanction	-.0819	(-.0141)	-.0642	(-.0124)	-.0387	(-.0080)	-.0086	(-.0019)
Dollars fined	.0000	(.0033)	.0000	(.0036)	.0000	(.0104)	.0000	(.0130)
Months on probation	-.0001	(-.0041)	.0000	(.0001)	.0003	(.0083)	.0002	(.0072)
Months to jail	.0014	(-.0044)	.0004	(-.0130)	.0012	(-.0037)	.0005	(-.0127)
Months to prison	-.0001	(-.0084)	-.0002	(-.0207)	-.0002*	(-.0204)	-.0002*	(-.0236)
First sanction of career	.0834*	(.0432)	.0773**	(.0377)	.0620*	(.0233)	.0473*	(.0208)
Progressive sanction	-.0015	(-.0010)	.0048	(.0037)	.0084	(.0068)	.0105	(.0091)
Sentence Interactions								
Prison x n adult arrests	.0129***	(.0300)	.0101***	(.0265)	.0099***	(.0277)	.0089***	(.0268)
Prison x n arrsts as juv	-.0038	(-.0052)	-.0053	(-.0082)	-.0009	(-.0014)	.0047	(.0083)
Yth. comp x n adlt convs	-.0236**	(-.0451)	-.0181**	(-.0387)	-.0169**	(-.0386)	-.0188***	(-.0462)
Yth. comp x chgs in 5 yr	.0110**	(.0268)	.0099**	(.0270)	.0096**	(.0280)	.0092**	(.0288)
Yth. comp x prior n incs	.0460***	(.0487)	.0227*	(.0269)	.0187*	(.0236)	.0111	(.0152)
Jl & prob x inc lst 2 yr	.1395**	(.0241)	.1081**	(.0209)	.0983**	(.0203)	.0750*	(.0167)
Jail x yrs using drugs	.0025	(.0170)	.0012	(.0092)	.0018	(.0145)	.0012	(.0104)
Prb w/cnd, fn x adlt arr	-.0225***	(-.0512)	-.0199***	(-.0508)	-.0159***	(-.0432)	-.0129***	(-.0377)
Prb & fn x Part 1 chgs.	-.0482***	(-.0644)	-.0360***	(-.0539)	-.0309***	(-.0492)	-.0281***	(-.0482)
Prb w/cnd x adlt chg cnv	-.0190***	(-.0413)	-.0126***	(-.0306)	-.0097**	(-.0252)	-.0073*	(-.0204)
Mths to jail x PO prop.	-.0001*	(-.0226)	-.0001**	(-.0215)	-.0001**	(-.0230)	-.0001**	(-.0232)
Mths to pris x prop cnvs	.0000*	(-.0253)	.0000***	(-.0356)	.0000***	(-.0306)	.0000***	(-.0314)
Init sanc x black	-.0590*	(-.0219)	-.0740**	(-.0308)	-.0822***	(-.0365)	-.0587**	(-.0280)
Init sanc x n adult arrs	.0289*	(.0927)	.0280**	(.1007)	.0199*	(.0761)	.0168*	(.0691)

*p<.05 **p<.01 ***p<.001

Table B8.19 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-.0246	(-.0155)	-.0178	(-.0126)	-.0134	(-.0100)	-.0031	(-.0025)
Case proceeds past arrst	.2419**	(.0447)	.1162	(.0240)	.0781	(.0172)	.0494	(.0117)
Case to Grand Jury	-.0116	(-.0079)	-.0033	(-.0025)	-.0082	(-.0067)	-.0110	(-.0097)
Case to Superior Court	.0599	(.0155)	.0060	(.0018)	-.0208	(-.0064)	-.0197	(-.0066)
Superior Court Convict.	-.0637*	(-.0248)	-.0699**	(-.0305)	-.0505*	(-.0235)	-.0292	(-.0146)
Match over data sources	-.0693	(-.0164)	.0245	(.0065)	.0291	(.0082)	.0464	(.0141)
Constant	.2940***	(.0099)	.3025***	(.0089)	.3018***	(-.0037)	.2620***	(-.0040)
R squared	.209		.323		.358		.371	
Adjusted R squared	.202		.317		.352		.366	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.20

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.0075	(.0129)	.0121*	(.0241)	.0131**	(.0283)	.0122**	(.0299)
Has job after sentence	-.0173**	(-.0309)	-.0118*	(-.0245)	-.0116**	(-.0262)	-.0098*	(-.0250)
Offender is on welfare	-.0019	(-.0028)	-.0038	(-.0067)	-.0050	(-.0093)	-.0060	(-.0128)
Offender is Black	.0317***	(.0377)	.0451***	(.0693)	.0432***	(.0706)	.0394***	(.0784)
Offender is Hispanic	.0212*	(.0198)	.0283**	(.0307)	.0266**	(.0313)	.0244***	(.0325)
Offender is female	-.0329***	(-.0375)	-.0340***	(-.0450)	-.0335***	(-.0481)	-.0324***	(-.0526)
Lives in urban area	.0113	(.0201)	.0093	(.0192)	.0086	(.0193)	.0079*	(.0200)
Years at current address	-.0003	(-.0126)	-.0004*	(-.0189)	-.0004*	(-.0190)	-.0002	(-.0118)
History of drug problems	.0062	(.0095)	.0032	(.0057)	.0064	(.0125)	-.0034	(-.0074)
Treated for drugs/alch.	-.0101	(-.0138)	-.0059	(-.0094)	-.0061	(-.0105)	-.0011	(-.0022)
Has needle marks	-.0117	(-.0107)	-.0215*	(-.0229)	-.0188*	(-.0216)	-.0062	(-.0081)
Not a school drop out	.0018	(.0031)	-.0011	(-.0022)	-.0048	(-.0106)	-.0040	(-.0099)
Doesn't live with family	-.0051	(-.0074)	-.0046	(-.0078)	-.0040	(-.0075)	-.0028	(-.0059)
Commited PO with group	-.0010	(-.0017)	.0038	(.0079)	.0028	(.0064)	.0026	(.0065)
Victim was a stranger	.0047	(.0075)	.0023	(.0043)	.0069	(.0139)	.0062	(.0140)
Presenting Offense								
PO property crime	.0009	(.0016)	.0010	(.0020)	.0009	(.0020)	.0029	(.0074)
PO crime against person	.0230*	(.0294)	.0282***	(.0427)	.0269***	(.0438)	.0218***	(.0399)
PO drug offense	.0110	(.0123)	.0032	(.0005)	-.0015	(-.0077)	.0002	(-.0030)
PO Wolfgang severity	.0004	(.0100)	.0004	(.0130)	.0006*	(.0203)	.0006*	(.0228)
Has detainers at arrest	-.0108	(-.0162)	-.0219*	(-.0304)	-.0139	(-.0244)	-.0062	(-.0167)
Has pending charges	.0138*	(.0198)	.0120*	(.0201)	.0140**	(.0253)	.0166***	(.0341)
On probation at PO	-.0024	(-.0162)	.0021	(-.0138)	.0046	(-.0111)	.0063	(-.0069)
Anamnestic Theory								
N prior adult arrests	.0062**	(.1033)	.0061**	(.1287)	.0049**	(.1148)	.0031*	(.0861)
N prior adult conviction	-.0074***	(-.0828)	-.0063***	(-.0753)	-.0056***	(-.0759)	-.0049***	(-.0737)
N prior adult chg. conv.	.0004	(-.0146)	-.0001	(-.0185)	-.0008	(-.0295)	.0005	(-.0116)
N charges past 5 years	.0037***	(.0799)	.0040***	(.0962)	.0032***	(.0855)	.0034***	(.0985)
N prior Part 1 charges	.0073**	(.0166)	.0027	(-.0207)	.0047*	(.0176)	.0037*	(.0119)
N prior property conv.	-.0035	(-.0534)	-.0013	(-.0319)	-.0018	(-.0414)	-.0011	(-.0360)
N prior persons conv.	.0166***	(.0899)	.0161***	(.1015)	.0169***	(.1154)	.0161***	(.1245)
N prior weapons conv.	.0207**	(.0258)	.0138*	(.0200)	.0184**	(.0289)	.0224***	(.0398)
Off street last 2 years	.0166*	(.0341)	.0226***	(.0482)	.0220***	(.0496)	.0247***	(.0590)
Delinquent Career/Onset								
N arrests as juvenile	.0091***	(.0745)	.0063***	(.0683)	.0053***	(.0680)	.0058***	(.0846)
N charges as juvenile	-.0049	(.0051)	-.0053	(.0263)	.0020	(.0408)	-.0040	(.0318)
Age at first arrest	.0010	(.0301)	.0016**	(.0506)	.0019***	(.0657)	.0014**	(.0655)
Yrs since first incarc.	.0004	(.0333)	.0006**	(.0557)	.0006***	(.0696)	.0005**	(.0627)
Yrs since first drug use	.0000	(-.0069)	-.0003	(-.0151)	-.0003	(-.0125)	-.0003*	(-.0167)
Prior CJS-Offender Action								
N prior incarcerations	.0041	(.0414)	.0056**	(.0585)	.0054**	(.0568)	.0034*	(.0408)
N prior parole revokes	-.0040	(-.0069)	-.0138*	(-.0274)	-.0129*	(-.0278)	-.0115*	(-.0279)
Bad conduct last probat.	.0115	(.0161)	.0119	(.0194)	.0086	(.0151)	.0042	(.0084)
Recent parole revoked	.0536***	(.0354)	.0540***	(.0414)	.0513***	(.0440)	.0468***	(.0489)
General Control Variables								
Offender age at sent.	-.0009	(-.0608)	-.0018**	(-.1031)	-.0019***	(-.1197)	-.0016***	(-.1267)
Off. born out of state	-.0150**	(-.0268)	-.0168***	(-.0349)	-.0200***	(-.0452)	-.0161***	(-.0412)
Coder prob. prognosis	-.0001	(-.0065)	-.0002*	(-.0222)	-.0002*	(-.0224)	-.0002**	(-.0272)

*p<.05 **p<.01 ***p<.001

Table 88.20 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.0204	(-.0145)	-.0235*	(-.0195)	-.0245*	(-.0219)	-.0220*	(-.0223)
Black x prior adult arrs	-.0066***	(-.0507)	-.0069***	(-.0608)	-.0074***	(-.0710)	-.0068***	(-.0738)
Black x n prior prop cnv	.0035	(.0277)	.0039**	(.0363)	.0047***	(.0468)	.0042***	(.0476)
Black x n charges as juv	.0174	(.0163)	.0395***	(.0430)	.0346***	(.0408)	.0362***	(.0483)
Female x Part 1 charges	-.0101	(-.0262)	-.0076	(-.0231)	-.0064	(-.0209)	-.0051	(-.0191)
Off. age x drug problem	-.0004	(-.0055)	-.0004	(-.0072)	-.0006	(-.0104)	-.0005	(-.0104)
Off. age x prior trtment	-.0009	(-.0115)	-.0011	(-.0168)	-.0010	(-.0166)	-.0013	(-.0240)
Off. age x unemployed	-.0013*	(-.0209)	-.0014**	(-.0257)	-.0014**	(-.0271)	-.0012**	(-.0269)
Off. age x PO property	-.0005	(-.0086)	-.0002	(-.0046)	-.0003	(-.0056)	-.0007	(-.0154)
Off. age x chg pst 5 yrs	-.0001	(-.0224)	-.0002**	(-.0405)	-.0002**	(-.0360)	-.0002**	(-.0367)
PO viol x has detainers	-.0380	(-.0134)	-.0390*	(-.0160)	-.0402*	(-.0178)	-.0356*	(-.0179)
PO prop x n adl.arrests	.0025	(.0187)	.0036*	(.0316)	.0041*	(.0386)	.0055***	(.0590)
PO prop x prior prop con	-.0033	(-.0259)	-.0053**	(-.0489)	-.0055***	(-.0549)	-.0057***	(-.0644)
PO prop x n juv. arrests	-.0023	(-.0103)	-.0002	(-.0012)	.0002	(.0008)	-.0002	(-.0013)
PO prop x age at 1st arr	.0003	(.0043)	-.0001	(-.0011)	.0001	(.0015)	.0006	(.0123)
PO prop x yrs. 1st incar	.0000	(-.0013)	.0001	(.0033)	.0001	(.0041)	.0001	(.0038)
PO drugs x n adl. convs.	.0032	(.0167)	.0045	(.0273)	.0034	(.0223)	.0031	(.0234)
PO drugs x Part 1 chgs.	-.0080	(-.0278)	-.0107**	(-.0434)	-.0082*	(-.0359)	-.0077*	(-.0381)
PO drugs x last par. rev	-.0494	(-.0177)	-.0502	(-.0209)	-.0423	(-.0191)	-.0264	(-.0135)
Sentence								
Prison	-.0456**	(-.0549)	-.0345**	(-.0484)	-.0306**	(-.0465)	-.0253*	(-.0435)
Youth complex	-.0012	(-.0013)	-.0001	(-.0001)	-.0046	(-.0062)	-.0005	(-.0008)
Jail, probation, fine	-.0276	(-.0188)	-.0237	(-.0188)	-.0279*	(-.0239)	-.0229	(-.0223)
Jail, probation	-.0050	(.0138)	-.0047	(.0090)	-.0103	(.0001)	-.0011	(.0026)
Jail only	.0011	(.0012)	.0037	(.0048)	-.0065	(-.0092)	-.0100	(-.0162)
Probation w/cond., fine	.0013	(.0016)	-.0109	(-.0149)	-.0085	(-.0126)	-.0089	(-.0148)
Probation, fine	-.0081	(-.0104)	-.[illegible]	(-.0136)	-.0024	(-.0039)	-.0016	(-.0029)
Probation w/conditions	-.0047	(-.0057)	.[illegible]	(.0000)	.0056	(.0086)	.0018	(.0031)
Fined only	.0034	(.0019)	.0092	(.0060)	.0049	(.0035)	-.0009	(-.0007)
Other sanction	-.0062	(-.0023)	-.0010	(-.0005)	.0023	(.0011)	.0127	(.0069)
Dollars fined	.0000	(-.0002)	.0000	(-.0004)	.0000*	(.0189)	.0000**	(.0252)
Months on probation	-.0002	(-.0125)	.0000	(.0006)	.0000	(-.0010)	.0000	(-.0018)
Months to jail	.0001	(.0007)	.0001	(-.0075)	.0002	(-.0032)	.0001	(-.0085)
Months to prison	.0000	(-.0074)	-.0001	(-.0192)	-.0001	(-.0180)	-.0001	(-.0212)
First sanction of career	.0258	(.0274)	.0281*	(.0348)	.0220	(.0252)	.0113	(.0125)
Progressive sanction	.0049	(.0074)	.0022	(.0038)	.0030	(.0057)	.0043	(.0091)
Sentence Interactions								
Prison x n adult arrests	.0013	(.0066)	.0028	(.0171)	.0044**	(.0287)	.0025*	(.0186)
Prison x n arrsts as juv	.0008	(.0024)	.0023	(.0080)	.0044*	(.0170)	.0068***	(.0296)
Yth. comp x n adlt convs	-.0084*	(-.0354)	-.0040	(-.0196)	-.0037	(-.0199)	-.0064**	(-.0388)
Yth. comp x chgs in 5 yr	.0076***	(.0405)	.0061***	(.0379)	.0057***	(.0388)	.0045***	(.0345)
Yth. comp x prior n incs	.0116*	(.0272)	.0080	(.0218)	.0033	(.0097)	.0021	(.0070)
Jl & prob x inc lst 2 yr	.0735**	(.0281)	.0476*	(.0211)	.0359*	(.0173)	.0112	(.0061)
Jail x yrs using drugs	-.0013*	(-.0198)	.0001	(.0018)	.0005	(.0099)	.0006	(.0127)
Prb w/cnd, fn x adlt arr	-.0026	(-.0133)	-.0043*	(-.0251)	-.0022	(-.0141)	-.0022	(-.0156)
Prb & fn x Part 1 chgs.	-.0147*	(-.0434)	-.0097*	(-.0334)	-.0025	(-.0091)	-.0021	(-.0088)
Prb w/cnd x adlt chg cnv	-.0044*	(-.0214)	-.0046**	(-.0257)	-.0034*	(-.0208)	-.0036*	(-.0246)
Mths to jail x PO prop.	.0000	(-.0026)	.0000	(-.0134)	.0000	(-.0099)	.0000	(-.0159)
Mths to pris x prop cnvs	.0000	(-.0136)	.0000*	(-.0258)	.0000*	(-.0246)	.0000*	(-.0248)
Init sanc x black	-.0212	(-.0174)	-.0229*	(-.0219)	-.0225*	(-.0233)	-.0136	(-.0159)
Init sanc x n adult arrs	.0085	(.0601)	.0090	(.0739)	.0077	(.0687)	.0049	(.0490)

*p<.05 **p<.01 ***p<.001

Table 88.20 (continued)

Regression Coefficients for Log of Adjusted Post-Sentence Persons Charge Rate
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	.0015	(.0020)	.0001	(.0001)	.0014	(.0025)	-.0001	(-.0002)
Case proceeds past arrst	.0595	(.0243)	.0378	(.0179)	.0268	(.0138)	.0134	(.0078)
Case to Grand Jury	-.0004	(-.0006)	.0022	(.0039)	.0044	(.0083)	.0028	(.0061)
Case to Superior Court	.0292	(.0167)	.0335*	(.0223)	.0155	(.0112)	.0093	(.0076)
Superior Court Convict.	-.0240	(-.0207)	-.0224	(-.0224)	-.0164	(-.0178)	-.0095	(-.0116)
Match over data sources	-.0241	(-.0126)	-.0157	(-.0095)	-.0030	(-.0020)	.0120	(.0089)
Constant	.0446*	(.0021)	.0429**	(.0004)	.0509***	(.0009)	.0458***	(-.0030)
R squared	.097		.156		.180		.201	
Adjusted R squared	.090		.149		.173		.194	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 88.21

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	.4979*	(.0214)	1.0886**	(.0276)	1.7239***	(.0327)	1.9002**	(.0260)
Has job after sentence	-.7079**	(-.0317)	-.9039*	(-.0239)	-1.2027*	(-.0238)	-1.8323**	(-.0261)
Offender is on welfare	-.2868	(-.0107)	-.5465	(-.0120)	-.7776	(-.0128)	-.9837	(-.0117)
Offender is Black	1.0253**	(.0307)	3.6281***	(.0656)	5.5499***	(.0747)	8.5177***	(.0959)
Offender is Hispanic	.8640*	(.0202)	2.5593***	(.0353)	3.9417***	(.0407)	7.0019***	(.0520)
Offender is female	-1.0220**	(-.0291)	-2.4661***	(-.0415)	-2.8154***	(-.0355)	-4.6521***	(-.0421)
Lives in urban area	.1782	(.0079)	.7649*	(.0201)	1.4786**	(.0291)	1.7780**	(.0251)
Years at current address	-.0212*	(-.0196)	-.0450**	(-.0246)	-.0537*	(-.0220)	-.0283	(-.0083)
History of drug problems	-.1854	(-.0071)	.2242	(.0051)	.9253	(.0157)	.1032	(.0013)
Treated for drugs/alch.	-.4469	(-.0152)	-.1850	(-.0037)	.4405	(.0066)	1.5822	(.0171)
Has needle marks	.4822	(.0111)	.9459	(.0128)	1.4926	(.0151)	2.8443*	(.0207)
Not a school drop out	-.1918	(-.0084)	-.3481	(-.0089)	-.4790	(-.0092)	-.5352	(-.0074)
Doesn't live with family	-.0075	(-.0003)	-.2148	(-.0047)	-1.1911*	(-.0194)	-1.4500*	(-.0170)
Commited PO with group	-.1707	(-.0077)	.2029	(.0054)	.2760	(.0055)	-.0081	(-.0001)
Victim was a stranger	.0255	(.0010)	.6099	(.0143)	.9358	(.0164)	1.8732*	(.0236)
Presenting Offense								
PO property crime	.0883	(.0039)	.2994	(.0078)	.3544	(.0069)	.9242	(.0130)
PO crime against person	.0423	(-.0042)	.8782	(.0094)	1.0204	(.0067)	1.0461	(.0021)
PO drug offense	.5275	(.0203)	.0903	(.0066)	-.2054	(.0021)	.5733	(.0138)
PO Wolfgang severity	.0133	(.0092)	.0251	(.0103)	.0292	(.0090)	.0287	(.0064)
Has detainers at arrest	-.0018	(-.0109)	.7058	(-.0080)	1.6439	(-.0017)	1.5991	(-.0072)
Has pending charges	1.2211***	(.0439)	2.1164***	(.0449)	2.7977***	(.0444)	3.9514***	(.0451)
On probation at PO	.4933	(.0074)	.9473	(-.0001)	1.2515	(-.0003)	1.4997	(.0006)
Anamnestic Theory								
N prior adult arrests	.3929***	(.1404)	1.0820***	(.2340)	1.4507***	(.2290)	1.8424***	(.2392)
N prior adult conviction	-.2678***	(-.0615)	-.4620***	(-.0551)	-.4233*	(-.0338)	-.4761*	(-.0221)
N prior adult chg. conv.	-.0839	(-.0650)	-.3670**	(-.1009)	-.6746***	(-.1261)	-.8102***	(-.1080)
N charges past 5 years	.1709***	(.0904)	.3465***	(.1041)	.4603***	(.1000)	.7666***	(.1124)
N prior Part 1 charges	.5747***	(.0619)	.3508*	(-.0120)	.6728**	(.0390)	.7504**	(.0275)
N prior property conv.	-.0965	(-.0241)	.0152	(.0093)	-.0707	(.0033)	.0349	(.0015)
N prior persons conv.	.0959	(.0130)	.1519	(.0121)	.2785	(.0167)	.3760	(.0162)
N prior weapons conv.	.4061	(.0127)	1.0753*	(.0198)	.7618	(.0105)	2.2555*	(.0223)
Off street last 2 years	1.0624***	(.0474)	2.6982***	(.0687)	3.9663***	(.0757)	6.0838***	(.0816)
Delinquent Career/Onset								
N arrests as juvenile	.3252***	(.0565)	.6631***	(.0661)	.8308***	(.0579)	1.2199***	(.0588)
N charges as juvenile	-.0144	(.0134)	-.3766	(.0146)	.0631	(.0260)	-.3256	(.0236)
Age at first arrest	.0331	(.0414)	.1559***	(.0638)	.2197***	(.0719)	.2580**	(.0651)
Yrs since first incarc.	.0165	(.0573)	.0553***	(.0799)	.1135***	(.1172)	.1455***	(.1070)
Yrs since first drug use	-.0081	(-.0001)	-.0298*	(-.0136)	-.0579***	(-.0229)	-.0650**	(-.0195)
Prior CJS-Offender Action								
N prior incarcerations	.1376	(.0352)	.2824	(.0358)	.1275	(.0146)	-.1418	(-.0069)
N prior parole revokes	.3782	(.0162)	.1902	(.0048)	.1085	(.0020)	-.6652	(-.0090)
Bad conduct last probat.	.4300	(.0151)	1.1185*	(.0231)	1.0219	(.0158)	1.2007	(.0134)
Recent parole revoked	.9845	(.0213)	.3598	(.0147)	-.0143	(.0121)	1.6024	(.0261)
General Control Variables								
Offender age at sent.	-.0338	(-.1002)	-.1341**	(-.1428)	-.2219***	(-.1685)	-.3369***	(-.1826)
Off. born out of state	-.6481**	(-.0291)	-1.9664***	(-.0519)	-2.6834***	(-.0531)	-3.5995***	(-.0512)
Coder prob. prognosis	-.0054	(-.0135)	-.0201**	(-.0299)	-.0277**	(-.0309)	-.0456***	(-.0366)

*p<.05 **p<.01 ***p<.001

Table 88.21 (continued)

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	-.6561	(-.0117)	-2.1768**	(-.0229)	-2.9054**	(-.0229)	-3.2950*	(-.0186)
Black x prior adult arrs	-.3296***	(-.0630)	-.5978***	(-.0673)	-.8422***	(-.0711)	-.9595***	(-.0582)
Black x n prior prop cnv	.2531***	(.0503)	.4724***	(.0554)	.6611***	(.0581)	.7423***	(.0469)
Black x n charges as juv	.6810	(.0159)	2.0580**	(.0284)	2.7031**	(.0279)	4.3341***	(.0322)
Female x Part 1 charges	.0312	(.0020)	.0621	(.0024)	.8735	(.0252)	1.0674	(.0221)
Off. age x drug problem	-.0530	(-.0189)	-.1191	(-.0250)	-.2198*	(-.0346)	-.3095*	(-.0350)
Off. age x prior trtment	-.1043*	(-.0329)	-.1992*	(-.0371)	-.2259*	(-.0315)	-.4432**	(-.0444)
Off. age x unemployed	-.0467	(-.0186)	-.0900*	(-.0211)	-.1263*	(-.0222)	-.1584*	(-.0200)
Off. age x PO property	-.0959**	(-.0393)	-.1510**	(-.0364)	-.2437***	(-.0441)	-.3584***	(-.0466)
Off. age x chg pst 5 yrs	-.0060	(-.0251)	-.0155**	(-.0384)	-.0221**	(-.0409)	-.0302**	(-.0402)
PO viol x has detainers	-2.4767**	(-.0218)	-6.6657***	(-.0347)	-9.2014***	(-.0358)	-13.2558***	(-.0371)
PO prop x n adl.arrests	.0716	(.0135)	.2471	(.0276)	.3970*	(.0332)	.8729***	(.0525)
PO prop x prior prop con	-.1825*	(-.0360)	-.4480***	(-.0520)	-.4913**	(-.0427)	-.8551***	(-.0535)
PO prop x n juv. arrests	-.1318	(-.0147)	-.2579	(-.0169)	-.4259*	(-.0209)	-.6082*	(-.0215)
PO prop x age at 1st arr	.0676	(.0227)	.0059	(.0012)	.0457	(.0068)	.1030	(.0110)
PO prop x yrs. 1st incar	.0284*	(.0284)	.0271	(.0160)	.0405	(.0179)	.0479	(.0152)
PO drugs x n adl. convs.	.2980**	(.0393)	.6863***	(.0534)	.7552**	(.0440)	1.0737***	(.0450)
PO drugs x Part 1 chgs.	-.6514***	(-.0569)	-1.4032***	(-.0723)	-1.0219**	(-.0394)	-1.3695*	(-.0380)
PO drugs x last par. rev	.0915	(.0008)	3.5006	(.0185)	5.5970*	(.0222)	9.6942*	(.0276)
Sentence								
Prison	-2.7056***	(-.0816)	-3.6804***	(-.0655)	-5.6083***	(-.0747)	-10.1040***	(-.0968)
Youth complex	-.5735	(-.0153)	-.7425	(-.0117)	-2.3573	(-.0278)	-4.2161*	(-.0357)
Jail, probation, fine	-1.2693	(-.0217)	-2.5214*	(-.0254)	-5.6350***	(-.0425)	-7.6224***	(-.0413)
Jail, probation	-.4367	(.0012)	-.8446	(-.0009)	-3.1444*	(-.0185)	-4.0843*	(-.0205)
Jail only	-.1045	(-.0030)	-.1664	(-.0028)	-1.9038	(-.0238)	-3.2331	(-.0291)
Probation w/cond., fine	-.1907	(-.0056)	-1.4622*	(-.0253)	-2.4261**	(-.0315)	-3.7451**	(-.0350)
Probation, fine	-.8058	(-.0260)	-1.0646	(-.0202)	-1.1033	(-.0157)	-1.7701	(-.0181)
Probation w/conditions	-.4904	(-.0149)	-.6656	(-.0119)	-.1846	(-.0025)	-.5991	(-.0058)
Fined only	-.3520	(-.0050)	-.0055	(.0000)	-.7582	(-.0047)	-2.2659	(-.0102)
Other sanction	-.7902	(-.0075)	-1.0696	(-.0060)	-.8638	(-.0036)	.1708	(.0005)
Dollars fined	.0000	(.0034)	.0000	(.0044)	.0003*	(.0180)	.0003	(.0164)
Months on probation	-.0082	(-.0125)	-.0097	(-.0087)	-.0067	(-.0045)	-.0081	(-.0039)
Months to jail	.0026	(-.0071)	.0058	(-.0089)	.0278	(-.0072)	.0168	(-.0165)
Months to prison	.0002	(.0007)	-.0076	(-.0195)	-.0154**	(-.0296)	-.0310***	(-.0431)
First sanction of career	1.0308	(.0304)	1.6072	(.0147)	1.4209	(-.0050)	1.4388	(-.0017)
Progressive sanction	.1324	(.0049)	.1419	(.0031)	.5666	(.0094)	.6814	(.0081)
Sentence Interactions								
Prison x n adult arrests	.1382*	(.0179)	.2131	(.0162)	.2474	(.0141)	-.0485	(-.0020)
Prison x n arrsts as juv	-.1728	(-.0131)	-.4894**	(-.0219)	-.6117**	(-.0205)	-1.1971***	(-.0289)
Yth. comp x n adlt convs	-.7101***	(-.0751)	-.6038**	(-.0376)	-.7463**	(-.0349)	-.8564*	(-.0288)
Yth. comp x chgs in 5 yr	.2922***	(.0393)	.4433***	(.0351)	.4026*	(.0239)	.1453	(.0062)
Yth. comp x prior n incs	.4186*	(.0245)	.2564	(.0089)	.4100	(.0106)	.3294	(.0061)
Jl & prob x inc lst 2 yr	1.6923	(.0162)	2.6622	(.0150)	3.9465*	(.0167)	3.4729	(.0105)
Jail x yrs using drugs	.0722**	(.0273)	.0949*	(.0212)	.1332*	(.0223)	.1263	(.0152)
Prb w/cnd, fn x adlt arr	-.3183**	(-.0401)	-.6968***	(-.0517)	-.7641***	(-.0425)	-.9421***	(-.0377)
Prb & fn x Part 1 chgs.	-.8230***	(-.0608)	-.8370*	(-.0364)	-.6780	(-.0221)	-.8563	(-.0201)
Prb w/cnd x adlt chg cnv	-.2224**	(-.0267)	-.4158**	(-.0295)	-.4111*	(-.0218)	-.4602	(-.0176)
Mths to jail x PO prop.	-.0012	(-.0103)	-.0026	(-.0132)	-.0046*	(-.0172)	-.0088**	(-.0236)
Mths to pris x prop cnvs	.0000	(-.0170)	-.0001*	(-.0221)	-.0001*	(-.0250)	-.0002**	(-.0285)
Init sanc x black	-.6805	(-.0140)	-2.2935**	(-.0278)	-3.8680***	(-.0351)	-3.5847*	(-.0234)
Init sanc x n adult arrs	.3521	(.0625)	.5465	(.0572)	.5357	(.0420)	.9755	(.0550)

*p<.05 **p<.01 ***p<.001

Table B8.21 (continued)

Regression Coefficients for Summed Seriousness of All Post-Sentence Charges
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period							
	1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-.3361	(-.0117)	-.9955*	(-.0204)	-1.6213*	(-.0249)	-2.0906*	(-.0231)
Case proceeds past arrst	3.4974*	(.0357)	4.6986*	(.0283)	6.0009	(.0271)	7.4362	(.0241)
Case to Grand Jury	-.2479	(-.0094)	.3107	(.0069)	.8286	(.0138)	.8193	(.0098)
Case to Superior Court	.7952	(.0114)	.5222	(.0044)	-.9773	(-.0062)	-2.9913	(-.0136)
Superior Court Convict.	-.8152	(-.0176)	-1.4664	(-.0186)	-1.8471	(-.0176)	-1.1770	(-.0081)
Match over data sources	-2.2550*	(-.0295)	-2.9061	(-.0224)	-1.8874	(-.0109)	.3304	(.0014)
Constant	3.8176***	(.0001)	7.6434***	(-.0036)	12.1224***	(-.0027)	18.5915***	(.0125)
R squared	.126		.212		.246		.256	
Adjusted R squared	.119		.206		.240		.250	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table B8.23
Variance Attributable to the Sentence for
Days to Rearrest after Sentencing - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.161)	(.254)	(.288)	(.310)
All Sanction Measures	7.50	5.03	4.47	4.48
	(.012)	(.013)	(.013)	(.014)
Sentence Variables	3.98	2.93	2.80	3.23
	(.006)	(.007)	(.008)	(.010)
Place Sentenced	2.55	1.68	1.32	1.15
	(.004)	(.004)	(.004)	(.004)
Time Sentenced	.12	.10	.13	.19
	(.000)	(.000)	(.000)	(.001)
Sentence Pattern	.01	.31	.58	.97
	(.000)	(.001)	(.002)	(.003)
Interactions w/Ind. Vars.	3.52	2.10	1.67	1.26
	(.006)	(.005)	(.005)	(.004)

Table B8.24
Variance Attributable to the Sentence for
Days to Reimprisonment after Sentencing - Controlling for Sample Selection

	Follow-up Window			
	1 Year	3 Years	5 Years	9 Years
Total Explainable Variance	100.00	100.00	100.00	100.00
	(.095)	(.235)	(.296)	(.339)
All Sanction Measures	9.79	9.58	9.05	7.85
	(.009)	(.023)	(.027)	(.027)
Sentence Variables	6.76	6.73	6.70	6.17
	(.006)	(.016)	(.020)	(.021)
Place Sentenced	3.28	3.90	4.08	3.63
	(.003)	(.009)	(.012)	(.012)
Time Sentenced	.23	.16	.22	.33
	(.000)	(.000)	(.001)	(.001)
Sentence Pattern	.26	.09	.05	.02
	(.000)	(.000)	(.000)	(.000)
Interactions w/Ind. Vars.	3.04	2.86	2.35	1.68
	(.003)	(.007)	(.007)	(.006)

Table 88.25

Regression Coefficients for Days to Post-Sentence Rearrest
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	-5.8678**	(-.0267)	-25.9911***	(-.0307)	-45.3674***	(-.0299)	-88.1757**	(-.0287)
Has job after sentence	7.6372***	(.0363)	28.1057***	(.0346)	45.0824**	(.0310)	77.0656**	(.0261)
Offender is on welfare	5.3859*	(.0213)	14.8783	(.0153)	16.0248	(.0092)	8.9755	(.0025)
Offender is Black	-13.3505***	(-.0460)	-78.3157***	(-.0676)	-171.970***	(-.0845)	-409.369***	(-.1012)
Offender is Hispanic	-11.6816**	(-.0289)	-61.9374***	(-.0397)	-138.005***	(-.0495)	-337.975***	(-.0597)
Offender is female	7.2044*	(.0218)	34.0661**	(.0267)	75.1058***	(.0328)	196.5114***	(.0423)
Lives in urban area	-1.3410	(-.0063)	-15.7597*	(-.0193)	-31.3281*	(-.0214)	-66.4633*	(-.0224)
Years at current address	.2570**	(.0253)	1.0296**	(.0262)	1.5685**	(.0223)	3.0082*	(.0211)
History of drug problems	-.6916	(-.0028)	-7.0551	(-.0074)	-17.9470	(-.0106)	-55.1193	(-.0160)
Treated for drugs/alch.	.3282	(.0012)	4.2659	(.0040)	2.1593	(.0011)	-24.9510	(-.0064)
Has needle marks	-1.7396	(-.0042)	-11.4539	(-.0072)	-34.5000	(-.0121)	-106.420*	(-.0184)
Not a school drop out	5.2423**	(.0242)	28.8826***	(.0346)	52.7809***	(.0353)	116.2010***	(.0383)
Doesn't live with family	-2.1912	(-.0086)	-.7166	(-.0007)	16.9278	(.0096)	71.7153*	(.0200)
Commited PO with group	4.2482*	(.0202)	16.3091*	(.0201)	32.9020**	(.0226)	61.9207*	(.0210)
Victim was a stranger	-7.1780**	(-.0302)	-28.9306***	(-.0315)	-48.1745**	(-.0293)	-89.0230**	(-.0267)
Presenting Offense								
PO property crime	-2.6375	(-.0123)	-18.6902	(-.0227)	-39.0373*	(-.0265)	-101.714**	(-.0340)
PO crime against person	3.4930	(.0149)	-1.2085	(.0032)	-15.4583	(-.0037)	-75.7299	(-.0154)
PO drug offense	-3.2054	(-.0189)	-16.8544	(-.0233)	-20.8480	(-.0180)	-44.8074	(-.0176)
PO Wolfgang severity	.1852	(.0136)	.8704	(.0166)	1.6974	(.0181)	3.2986	(.0173)
Has detainers at arrest	6.3828	(.0180)	17.8186	(.0192)	30.2882	(.0192)	55.2795	(.0188)
Has pending charges	-12.3741***	(-.0471)	-55.7841***	(-.0550)	-107.560***	(-.0593)	-209.982***	(-.0570)
On probation at PO	-8.1109*	(-.0202)	-41.3273***	(-.0220)	-79.9720***	(-.0222)	-175.310***	(-.0205)
Anamnestic Theory								
N prior adult arrests	-2.7342**	(-.1302)	-15.4962***	(-.2074)	-32.7586***	(-.2454)	-71.6300***	(-.2740)
N prior adult conviction	.7521	(.0056)	1.1971	(-.0122)	1.8692	(-.0125)	-3.0232	(-.0262)
N prior adult chg. conv.	.9415	(.0633)	6.3632*	(.0771)	13.3690**	(.0835)	24.7531**	(.0744)
N charges past 5 years	-1.6742***	(-.0848)	-5.4747***	(-.0678)	-8.7096***	(-.0575)	-14.4985**	(-.0448)
N prior Part 1 charges	-3.0005**	(-.0121)	-9.5681**	(-.0177)	-14.4626*	(-.0172)	-20.7022	(-.0145)
N prior property conv.	-.7353	(-.0332)	-2.8497	(-.0372)	-5.2804	(-.0393)	-8.5780	(-.0380)
N prior persons conv.	-.9441	(-.0135)	-3.8153	(-.0142)	-9.5854	(-.0199)	-24.6788*	(-.0253)
N prior weapons conv.	-4.9444	(-.0163)	-25.4508*	(-.0218)	-43.3796*	(-.0207)	-95.9199**	(-.0226)
Off street last 2 years	-10.6606***	(-.0517)	-65.1258***	(-.0778)	-127.556***	(-.0836)	-268.677***	(-.0854)
Delinquent Career/Onset								
N arrests as juvenile	-3.7846***	(-.0549)	-15.6223***	(-.0665)	-26.3591***	(-.0633)	-50.0493***	(-.0589)
N charges as juvenile	-3.0966	(-.0207)	-8.4702	(-.0231)	-19.3260	(-.0217)	-56.8820	(-.0213)
Age at first arrest	-.6675**	(-.0513)	-3.5291***	(-.0613)	-6.8077***	(-.0616)	-11.9722***	(-.0500)
Yrs since first incarc.	-.3407***	(-.0846)	-1.5658***	(-.1017)	-3.0411***	(-.1074)	-6.1181***	(-.1054)
Yrs since first drug use	.1136	(.0114)	.5751*	(.0175)	.9117	(.0158)	1.7112	(.0152)
Prior CJS-Offender Action								
N prior incarcerations	-.5083	(-.0272)	1.1341	(-.0062)	3.7476	(.0018)	23.3709	(.0289)
N prior parole revokes	-4.2700	(-.0193)	-3.7189	(-.0044)	-7.4556	(-.0049)	-5.3990	(-.0017)
Bad conduct last probat.	-4.3128	(-.0160)	-22.8488*	(-.0220)	-32.4443	(-.0174)	-25.0290	(-.0066)
Recent parole revoked	-7.5907	(-.0302)	-16.4787	(-.0222)	-6.0204	(-.0149)	-4.5206	(-.0112)
General Control Variables								
Offender age at sent.	.8547***	(.1481)	4.3486***	(.1835)	9.6539***	(.2026)	21.9778***	(.2080)
Off. born out of state	9.1866***	(.0436)	49.9815***	(.0615)	104.1229***	(.0715)	251.3596***	(.0851)
Coder prob. prognosis	.1028*	(.0275)	.5268***	(.0366)	1.0344***	(.0401)	2.1447***	(.0410)

*p<.05 **p<.01 ***p<.001

Table B8.25 (continued)

Regression Coefficients for Days to Post-Sentence Rearrest
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	6.3739	(.0120)	43.3298*	(.0212)	90.5776**	(.0247)	227.6265***	(.0306)
Black x prior adult arrs	2.3373**	(.0473)	9.6836***	(.0508)	17.9876***	(.0527)	38.7715***	(.0560)
Black x n prior prop cnv	-2.1987***	(-.0463)	-7.5492**	(-.0412)	-12.0070**	(-.0366)	-25.6276**	(-.0385)
Black x n charges as juv	-2.3738	(-.0059)	-21.3248	(-.0137)	-24.4011	(-.0088)	-6.6129	(-.0012)
Female x Part 1 charges	-2.5266	(-.0175)	-15.4647*	(-.0277)	-30.9396*	(-.0310)	-61.5726*	(-.0304)
Off. age x drug problem	.7998	(.0302)	3.2262*	(.0315)	4.9291	(.0269)	6.1872	(.0166)
Off. age x prior trtment	.9523*	(.0318)	4.9913**	(.0432)	9.4792**	(.0459)	19.9170***	(.0475)
Off. age x unemployed	.5120*	(.0216)	2.2721**	(.0248)	3.6466*	(.0223)	5.4865	(.0165)
Off. age x PO property	.6994*	(.0303)	3.0208*	(.0339)	5.1327*	(.0322)	10.2483*	(.0317)
Off. age x chg pst 5 yrs	.0119	(.0053)	-.0279	(-.0032)	-.0694	(-.0045)	-.3826	(-.0121)
PO viol x has detainers	6.5333	(.0061)	69.3930*	(.0168)	133.5544*	(.0181)	288.7821**	(.0192)
PO prop x n adl.arrests	-.5698	(-.0114)	-2.4643	(-.0128)	-3.0432	(-.0088)	-5.0536	(-.0072)
PO prop x prior prop con	2.2491**	(.0469)	6.7989*	(.0368)	10.1519*	(.0307)	17.8265	(.0265)
PO prop x n juv. arrests	3.1226***	(.0368)	11.2345***	(.0343)	18.7234***	(.0320)	37.5476***	(.0316)
PO prop x age at 1st arr	-.1019	(-.0036)	.6317	(.0058)	2.2784	(.0117)	5.6063	(.0142)
PO prop x yrs. 1st incar	-.1240	(-.0132)	-.6129	(-.0169)	-.9634	(-.0148)	-1.7563	(-.0133)
PO drugs x n adl. convs.	-2.4724*	(-.0345)	-11.1718**	(-.0405)	-19.0703**	(-.0386)	-34.5160**	(-.0344)
PO drugs x Part 1 chgs.	7.4143***	(.0686)	21.3242***	(.0511)	31.8174**	(.0426)	51.1265*	(.0337)
PO drugs x last par. rev	-25.1698*	(-.0239)	-92.8158*	(-.0228)	-170.266*	(-.0234)	-278.563	(-.0189)
Sentence								
Prison	30.2104***	(.0965)	115.5927***	(.0957)	178.6345***	(.0826)	312.1233***	(.0712)
Youth complex	7.8349	(.0221)	19.7574	(.0145)	22.1221	(.0090)	28.6530	(.0058)
Jail, probation, fine	8.3265	(.0150)	70.1778**	(.0329)	135.1607**	(.0353)	320.7153***	(.0413)
Jail, probation	-4.7081	(-.0250)	-9.2556	(-.0178)	-27.0929	(-.0184)	-46.6790	(-.0137)
Jail only	6.3050	(.0189)	39.6969*	(.0308)	77.3857*	(.0336)	163.3406*	(.0349)
Probation w/cond., fine	7.6264	(.0237)	28.4365*	(.0229)	34.1917	(.0154)	44.1888	(.0098)
Probation, fine	6.3522	(.0217)	25.5526	(.0226)	35.1756	(.0174)	39.5011	(.0096)
Probation w/conditions	3.0564	(.0098)	-1.0118	(-.0008)	-8.3785	(-.0039)	-33.7306	(-.0077)
Fined only	1.8866	(.0028)	16.1923	(.0063)	45.5722	(.0099)	157.3916	(.0168)
Other sanction	13.6464	(.0138)	70.3089*	(.0184)	119.6705*	(.0175)	250.5296*	(.0180)
Dollars fined	-.0001	(-.0018)	.0000	(.0002)	.0008	(.0020)	.0027	(.0031)
Months on probation	.0340	(.0054)	.2348	(.0098)	.3704	(.0086)	.2521	(.0029)
Months to jail	.0672	(.0185)	-.6101	(.0135)	-1.2838	(.0139)	-2.8178	(.0121)
Months to prison	.0251	(.0116)	.1338	(.0160)	.3282*	(.0220)	.9473**	(.0313)
First sanction of career	-14.6207*	(-.0494)	-51.2527*	(-.0347)	-85.7770*	(-.0268)	-152.323*	(-.0190)
Progressive sanction	-.3393	(-.0013)	-3.1239	(-.0032)	-7.0562	(-.0040)	-5.7518	(-.0016)
Sentence Interactions								
Prison x n adult arrests	-.5664	(-.0077)	-3.3906	(-.0120)	-4.5360	(-.0090)	-4.9443	(-.0048)
Prison x n arrsts as juv	1.7414	(.0140)	2.7394	(.0057)	3.8359	(.0045)	2.7370	(.0016)
Yth. comp x n adlt convs	3.6817**	(.0412)	4.2009	(.0122)	1.2548	(.0020)	-5.3893	(-.0043)
Yth. comp x chgs in 5 yr	-.9415	(-.0134)	.2411	(.0009)	4.3793	(.0090)	14.4960	(.0147)
Yth. comp x prior n incs	-8.1114***	(-.0503)	-23.3082**	(-.0375)	-32.4728**	(-.0292)	-41.1615	(-.0182)
Jl & prob x inc lst 2 yr	-22.3223**	(-.0226)	-73.7959*	(-.0194)	-102.933	(-.0151)	-136.131	(-.0098)
Jail x yrs using drugs	-.2150	(-.0086)	-.4102	(-.0043)	-.4743	(-.0028)	-.3327	(-.0010)
Prb w/cnd, fn x adlt arr	3.2229***	(.0430)	13.3274***	(.0460)	19.7816***	(.0382)	20.1660	(.0192)
Prb & fn x Part 1 chgs.	6.5371**	(.0511)	20.9292**	(.0424)	33.2090*	(.0376)	41.9825	(.0234)
Prb w/cnd x adlt chg cnv	2.8762***	(.0366)	8.4785**	(.0280)	13.6882**	(.0252)	25.7684**	(.0234)
Mths to jail x PO prop.	.0194	(.0174)	.1201**	(.0278)	.2349***	(.0303)	.4592***	(.0293)
Mths to pris x prop cnvs	.0004*	(.0232)	.0013*	(.0202)	.0019	(.0172)	.0025	(.0112)
Init sanc x black	7.6984	(.0168)	47.3431**	(.0267)	99.4495**	(.0314)	209.0504***	(.0325)
Init sanc x n adult arrs	-5.1277*	(-.0964)	-27.9391***	(-.1361)	-56.4539***	(-.1536)	-128.259***	(-.1720)

*p<.05 **p<.01 ***p<.001

Table 88.25 (continued)

Regression Coefficients for Days to Post-Sentence Rearrest
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	2.8260	(.0104)	16.2289	(.0155)	29.8014	(.0159)	38.6554	(.0102)
Case proceeds past arrst	-36.8582**	(-.0399)	-153.454**	(-.0430)	-211.761*	(-.0332)	-337.767	(-.0261)
Case to Grand Jury	.9869	(.0039)	5.5038	(.0057)	12.2345	(.0071)	30.6574	(.0087)
Case to Superior Court	-10.7839	(-.0164)	-25.5237	(-.0100)	-39.0759	(-.0086)	-69.1780	(-.0075)
Superior Court Convict.	4.6150	(.0105)	32.9262	(.0195)	52.4307	(.0173)	72.8233	(.0119)
Match over data sources	-1.0781	(-.0015)	-8.8773	(-.0032)	-51.7721	(-.0104)	-160.510	(-.0158)
Constant	324.0839***	(-.0267)	839.2760***	(-.0414)	1276.744***	(-.0532)	2154.327***	(-.0699)
R squared	.161		.254		.288		.310	
Adjusted R squared	.153		.248		.282		.304	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

Table 88.26

Regression Coefficients for Days to Post-Sentence Reimprisonment
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Structural Variables								
Offender is unemployed	1.3905	(.0142)	-6.7500	(-.0136)	-21.4595*	(-.0215)	-54.4941**	(-.0237)
Has job after sentence	3.5978***	(.0384)	19.3103***	(.0404)	34.5926***	(.0361)	74.0238***	(.0335)
Offender is on welfare	2.6041*	(.0232)	16.2482**	(.0283)	36.2307***	(.0315)	79.0470***	(.0298)
Offender is Black	-.3654	(.0104)	-22.0975***	(-.0237)	-64.3917***	(-.0414)	-197.858***	(-.0636)
Offender is Hispanic	-2.1133	(-.0118)	-18.9118*	(-.0206)	-44.2542**	(-.0241)	-133.282***	(-.0315)
Offender is female	-1.0373	(-.0070)	-5.6380	(-.0075)	-6.9407	(-.0046)	17.7791	(.0051)
Lives in urban area	.6481	(.0069)	4.4165	(.0092)	8.3107	(.0086)	-3.6098	(-.0016)
Years at current address	.0068	(.0015)	.1551	(.0067)	.5052	(.0109)	1.0690	(.0100)
History of drug problems	-3.5659**	(-.0326)	-10.7251	(-.0192)	-25.2056*	(-.0225)	-71.4797**	(-.0277)
Treated for drugs/alch.	-.8027	(-.0065)	-9.5839	(-.0153)	-20.1257	(-.0160)	-33.2883	(-.0115)
Has needle marks	.0576	(.0003)	-8.8651	(-.0095)	-29.8095	(-.0159)	-94.9478**	(-.0220)
Not a school drop out	-.1251	(-.0013)	2.3186	(.0047)	9.1402	(.0093)	23.2403	(.0102)
Doesn't live with family	-.6963	(-.0061)	-1.7154	(-.0030)	-3.5091	(-.0030)	1.9538	(.0007)
Commited PO with group	1.1879	(.0127)	8.3737*	(.0175)	11.4533	(.0120)	25.6972	(.0116)
Victim was a stranger	-1.9565	(-.0185)	-13.6677**	(-.0253)	-26.3587**	(-.0243)	-68.3256**	(-.0274)
Presenting Offense								
PO property crime	-.4726	(-.0050)	-12.6380*	(-.0261)	-26.6966*	(-.0274)	-89.7017***	(-.0400)
PO crime against person	.2493	(.0052)	-.1759	(.0021)	-3.0248	(-.0005)	-31.7341	(-.0098)
PO drug offense	1.1509	(.0118)	-1.1021	(-.0015)	-1.4991	(-.0032)	-33.9225	(-.0171)
PO Wolfgang severity	-.0508	(-.0084)	-.2373	(-.0077)	-.4730	(-.0077)	-.4033	(-.0028)
Has detainers at arrest	-4.4316*	(-.0175)	-23.9637*	(-.0202)	-40.2623*	(-.0169)	-71.9567	(-.0129)
Has pending charges	-3.0373**	(-.0260)	-25.8552***	(-.0434)	-57.6433***	(-.0482)	-135.786***	(-.0493)
On probation at PO	-2.0314	(-.0020)	-9.7976	(-.0004)	-29.1529*	(-.0055)	-75.6585*	(-.0123)
Anamnestic Theory								
N prior adult arrests	-.0451	(.0196)	-1.7075	(-.0412)	-5.4926	(-.0562)	-16.2997*	(-.0632)
N prior adult conviction	.8416*	(.0524)	3.4831*	(.0328)	4.8435	(.0162)	6.6861	(.0053)
N prior adult chg. conv.	-.9288**	(-.0652)	-2.5432	(-.0228)	-2.1292	(.0029)	1.8052	(.0265)
N charges past 5 years	-.7033***	(-.0823)	-3.7138***	(-.0890)	-6.5522***	(-.0784)	-14.2415***	(-.0718)
N prior Part 1 charges	-.0506	(.0407)	-5.2662**	(-.0069)	-10.3173**	(-.0066)	-21.4170*	(-.0116)
N prior property conv.	-.0885	(-.0411)	.1262	(-.0313)	-1.7890	(-.0414)	-9.2365	(-.0460)
N prior persons conv.	-.8617*	(-.0278)	-1.8901	(-.0119)	-5.7175	(-.0180)	-19.7534*	(-.0270)
N prior weapons conv.	.5146	(.0038)	-2.5983	(-.0038)	-4.6871	(-.0034)	-6.3009	(-.0020)
Off street last 2 years	-3.4505**	(-.0333)	-33.1977***	(-.0659)	-83.8335***	(-.0829)	-209.448***	(-.0905)
Delinquent Career/Onset								
N arrests as juvenile	-.5983	(-.0393)	-1.9998	(-.0434)	-4.6419	(-.0468)	-13.2622	(-.0464)
N charges as juvenile	.2315	(.0049)	-4.9058	(-.0237)	-11.5918	(-.0335)	-20.8252	(-.0389)
Age at first arrest	-.1577	(-.0498)	-1.1998*	(-.0719)	-3.4530**	(-.0878)	-9.4730***	(-.0944)
Yrs since first incarc.	-.3029***	(-.1635)	-1.5649***	(-.1906)	-3.5141***	(-.2085)	-9.0080***	(-.2258)
Yrs since first drug use	-.0197	(-.0073)	.0331	(-.0013)	.1666	(.0015)	.8800	(.0078)
Prior CJS-Offender Action								
N prior incarcerations	-.2525	(-.0209)	-.7810	(-.0280)	-2.1778	(-.0308)	-5.1458	(-.0283)
N prior parole revokes	-1.6305	(-.0166)	-5.7624	(-.0115)	-11.9057	(-.0118)	-19.1452	(-.0083)
Bad conduct last probat.	-1.4311	(-.0119)	-8.8542	(-.0145)	-19.5505	(-.0159)	-37.7266	(-.0134)
Recent parole revoked	-10.6763***	(-.0508)	-63.5066***	(-.0616)	-102.381***	(-.0545)	-154.305**	(-.0418)
General Control Variables								
Offender age at sent.	-.0734	(.0217)	.1575	(.0854)	1.1910	(.1109)	4.9883*	(.1322)
Off. born out of state	-.8071	(-.0086)	.3671	(.0008)	5.8198	(.0061)	39.3645*	(.0178)
Coder prob. prognosis	.0268	(.0161)	.2055*	(.0243)	.4741**	(.0279)	1.2617***	(.0322)

*p<.05 **p<.01 ***p<.001

Table B8.26 (continued)

Regression Coefficients for Days to Post-Sentence Reimprisonment
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Interactions								
Black x on prob. at PO	4.0975	(.0174)	21.8701*	(.0182)	51.4687**	(.0213)	95.1750*	(.0171)
Black x prior adult arrs	1.3181***	(.0599)	7.4936***	(.0668)	14.2265***	(.0632)	31.7850***	(.0613)
Black x n prior prop cnv	-1.1836***	(-.0560)	-6.1826***	(-.0574)	-10.8109***	(-.0500)	-19.3284**	(-.0388)
Black x n charges as juv	.4633	(.0026)	-13.3659	(-.0146)	-43.1694**	(-.0235)	-138.758***	(-.0328)
Female x Part 1 charges	.1212	(.0019)	-2.9628	(-.0090)	-8.9556	(-.0136)	-27.0520	(-.0178)
Off. age x drug problem	.0371	(.0031)	.8085	(.0134)	1.9598	(.0162)	2.4966	(.0090)
Off. age x prior trtment	.2794	(.0210)	3.7209***	(.0548)	7.9279***	(.0582)	19.3267***	(.0616)
Off. age x unemployed	.1444	(.0137)	1.0010*	(.0186)	2.4399**	(.0226)	7.0868***	(.0285)
Off. age x PO property	.1821	(.0177)	2.0675**	(.0395)	4.4695***	(.0425)	10.4692***	(.0433)
Off. age x chg pst 5 yrs	-.0200	(-.0200)	.0068	(.0013)	.0663	(.0065)	.2756	(.0117)
PO viol x has detainers	5.6365	(.0118)	22.3741	(.0092)	37.8036	(.0078)	73.0318	(.0065)
PO prop x n adl.arrests	-.1343	(-.0061)	-1.7333	(-.0153)	-4.0273	(-.0177)	-13.3717	(-.0255)
PO prop x prior prop con	.4313	(.0202)	2.2294	(.0205)	5.1171	(.0234)	15.5527*	(.0309)
PO prop x n juv. arrests	-.5397	(-.0143)	-4.5533*	(-.0237)	-8.9164*	(-.0231)	-15.1109	(-.0170)
PO prop x age at 1st arr	-.3646	(-.0291)	-2.5883**	(-.0406)	-5.0899**	(-.0397)	-10.3482**	(-.0351)
PO prop x yrs. 1st incar	-.0824	(-.0197)	-1.0682***	(-.0500)	-2.1562***	(-.0503)	-4.7856***	(-.0485)
PO drugs x n adl. convs.	-.5717	(-.0180)	-5.2116*	(-.0321)	-11.0961**	(-.0341)	-21.3164*	(-.0284)
PO drugs x Part 1 chgs.	2.3340**	(.0485)	10.6226**	(.0433)	21.3401**	(.0434)	38.1427*	(.0336)
PO drugs x last par. rev	2.4785	(.0053)	4.4197	(.0018)	-35.7855	(-.0075)	-177.132	(-.0160)
Sentence								
Prison	7.2702**	(.0522)	28.4772*	(.0401)	40.2894	(.0283)	81.6043	(.0249)
Youth complex	1.6979	(.0108)	-36.4115**	(-.0453)	-96.6310***	(-.0599)	-228.239***	(-.0615)
Jail, probation, fine	12.9895***	(.0527)	75.0282***	(.0597)	167.6462***	(.0665)	407.8279***	(.0702)
Jail, probation	13.1681***	(.0667)	84.0170***	(.0759)	189.4707***	(.0844)	433.7017***	(.0816)
Jail only	6.9832**	(.0470)	52.4853***	(.0693)	121.6580***	(.0801)	298.3777***	(.0853)
Probation w/cond., fine	3.4190	(.0239)	14.4361	(.0198)	29.8758	(.0204)	84.1541*	(.0250)
Probation, fine	2.1030	(.0161)	15.0744	(.0227)	33.1078*	(.0248)	70.7038	(.0230)
Probation w/conditions	.1766	(.0013)	-2.3138	(-.0033)	-6.1235	(-.0043)	-31.9401	(-.0098)
Fined only	-1.0643	(-.0036)	-12.8416	(-.0085)	-26.7193	(-.0088)	-29.7584	(-.0042)
Other sanction	5.4228	(.0123)	6.9168	(.0031)	10.2758	(.0023)	23.5000	(.0023)
Dollars fined	.0000	(-.0001)	-.0005	(-.0032)	-.0012	(-.0044)	-.0064	(-.0099)
Months on probation	-.0349	(-.0126)	-.3852*	(-.0272)	-.9315**	(-.0328)	-2.0781**	(-.0317)
Months to jail	-.0331	(.0059)	-.5621	(.0029)	-1.7982	(-.0042)	-4.4136	(-.0036)
Months to prison	.0187	(.0195)	.0870	(.0178)	.2399*	(.0244)	.8502***	(.0375)
First sanction of career	-4.9766	(-.0417)	-38.7309**	(-.0573)	-65.5542**	(-.0435)	-105.957	(-.0216)
Progressive sanction	-.5504	(-.0049)	-4.5825	(-.0080)	-12.3131	(-.0107)	-27.4818	(-.0104)
Sentence Interactions								
Prison x n adult arrests	-.5809	(-.0179)	-5.8586***	(-.0353)	-12.3374***	(-.0371)	-24.8542***	(-.0324)
Prison x n arrsts as juv	.6985	(.0126)	-1.9543	(-.0069)	-4.7221	(-.0083)	-6.8149	(-.0052)
Yth. comp x n adlt convs	.7497	(.0189)	9.8649***	(.0487)	19.3098***	(.0475)	40.2362***	(.0430)
Yth. comp x chgs in 5 yr	-.6118	(-.0196)	-5.0647**	(-.0318)	-9.1469**	(-.0286)	-14.7989*	(-.0201)
Yth. comp x prior n incs	-1.9602*	(-.0273)	-23.1516***	(-.0633)	-46.5477***	(-.0634)	-92.9715***	(-.0550)
Jl & prob x inc lst 2 yr	.5005	(.0011)	-23.1350	(-.0103)	-58.4222	(-.0130)	-170.487*	(-.0165)
Jail x yrs using drugs	-.0534	(-.0048)	-.5103	(-.0090)	-1.0270	(-.0090)	-2.1612	(-.0083)
Prb w/cnd, fn x adlt arr	.8688*	(.0260)	4.6056*	(.0271)	9.9382**	(.0291)	21.2515*	(.0270)
Prb & fn x Part 1 chgs.	2.0970*	(.0369)	15.7414***	(.0543)	32.5517***	(.0559)	65.6859***	(.0490)
Prb w/cnd x adlt chg cnv	.6707	(.0192)	1.6197	(.0091)	4.5279	(.0127)	12.5708	(.0153)
Mths to jail x PO prop.	.0062	(.0125)	.0548*	(.0216)	.1211**	(.0237)	.3102**	(.0264)
Mths to pris x prop cnvs	.0002*	(.0224)	.0009*	(.0255)	.0018**	(.0251)	.0046**	(.0272)
Init sanc x black	1.7210	(.0084)	20.7644*	(.0199)	46.6250*	(.0223)	123.8358**	(.0257)
Init sanc x n adult arrs	-.9610	(-.0406)	-9.6786*	(-.0802)	-16.2893	(-.0672)	-25.4759	(-.0456)

*p<.05 **p<.01 ***p<.001

Table B8.26 (continued)

Regression Coefficients for Days to Post-Sentence Reimprisonment
- Controlling for Sample Selection
(Standardized Coefficients in Parentheses)

Independent Variable	Post-Sentence Observation Period: 1 Year Window		3 Year Window		5 Year Window		9 Year Window	
Selection Hazards								
UCR to SAC arrest histry	-1.9679	(-.0163)	-5.6078	(-.0091)	-7.2998	(-.0059)	-23.5976	(-.0083)
Case proceeds past arrst	-.0849	(-.0002)	7.8596	(.0037)	22.7933	(.0054)	90.0297	(.0093)
Case to Grand Jury	2.2804	(.0205)	18.1908**	(.0320)	43.4332***	(.0381)	127.0015***	(.0484)
Case to Superior Court	.1561	(.0005)	-19.8631	(-.0133)	-55.6429	(-.0186)	-95.6728	(-.0138)
Superior Court Convict.	1.5032	(.0077)	16.1300	(.0162)	34.7393	(.0174)	89.4772	(.0195)
Match over data sources	5.2811	(.0164)	16.4468	(.0100)	23.8427	(.0072)	5.6887	(.0008)
Constant	352.0411***	(-.0048)	1018.846***	(.0028)	1657.615***	(.0108)	3019.530***	(.0141)
R squared	.095		.235		.296		.339	
Adjusted R squared	.087		.229		.290		.333	
N of cases	11,714		11,746		11,749		11,749	

*p<.05 **p<.01 ***p<.001

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