Specific Deterrence in a Sample of Offenders Convicted of White Collar Crimes*

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Abstract

It is generally argued that white collar criminals will be particularly influenced by punishment policies. White collar crime is seen as a highly rational form of criminality, in which the risks and rewards are carefully evaluated by potential offenders, and white collar criminals are assumed to have much more to lose through sanctions than more common law violators. In this paper we examine the impact of sanctions on the criminal careers of almost 1000 offenders convicted of white collar crimes in seven United States District Courts between 1976 and 1978. Utilizing detailed data on offender backgrounds and the nature of court imposed sanctions originally compiled by Wheeler, Weisburd and Bode (1988), as well as information on subsequent criminal behavior provided by the Identification Bureau of the FBI, we assess the effect of imprisonment upon recorded criminal behavior of people convicted of white collar crimes over a ten year follow-up period. Our findings do not provide evidence of a specific deterrent effect of imprisonment on white collar criminals. Using a quasi-experimental research design in which we compare groups of offenders that are alike in terms of factors that led to their receipt of an imprisonment sanction, those sentenced to prison and those not are found to fit similar models of recidivism. In concluding we argue that the measure of specific deterrence gained from a prison sanction for these relatively established offenders appears to be no greater than that found among their common crime counterparts.

Although scholars have focussed increasing attention upon the criminal careers of street criminals, they have largely overlooked those of white collar offenders. Behind this neglect lies a common assumption about the nature of white collar criminality. In contrast to street criminals who are assumed to be highly likely to recidivate, those convicted of white collar crimes are generally thought to be "one shot" offenders unlikely to be processed in the justice system after their initial brush with the law (see Edelhertz and Overcast, 1982; Benson, 1985; Wheeler, Mann and Sarat, 1988).

Studies of people who are prosecuted for white collar crimes contradict this common assumption about white collar criminals. In two major investigations examining offenders convicted under white collar crime statutes in the United States federal courts in the 1970s a substantial number of defendants were found to have prior arrests. Benson and Moore (1992), for example, report that almost forty percent of their sample, which included those convicted of bribery, bank embezzlement, income tax evasion, false claims and mail fraud, had at least one prior arrest. Weisburd, Chayet and Waring (1990) studying these crimes and securities violations, anti-trust violations, and credit fraud found that more than forty percent of their sample evidenced at least one prior arrest and more than a quarter had two reported prior arrests.

The fact that convicted white collar criminals often have

histories of prior criminal conduct raises significant research and policy questions. Perhaps most important of these concern the impact of sanctions on such offenders. In recent years there has been a growing concern that white collar criminals have avoided the most serious penalties in the justice system. This has led in the federal judiciary, for example, to increased severity in the sanctions for white collar crimes (U.S. Sentencing Commission, 1987) and to a larger number of such offenders being sentenced to imprisonment (U.S. Sentencing Commission, 1991). In good part because of the assumption that white collar criminals are unlikely to have multiple contacts with the criminal justice system, such policies have been developed without investigation of their impact on future criminal conduct among sanctioned white collar criminals.

Examination of the effects of sanctions upon convicted white collar offenders can also provide insight for our understanding of theories of specific deterrence. The failure of sanctions to provide specific deterrent effects in studies of street criminals is often attributed to the fact that such offenders have so little to lose through contact with the justice system (Mann, Wheeler, Sarat, 1980; see also Piliavin, Gartner, Thornton and Matsueda, 1986). Convicted white collar criminals are drawn from a population that is presumed to have much more to lose as a result of the criminal process than do more common offenders (see Weisburd et al., 1991), and thus provide a special opportunity to critically examine assumptions underlying specific deterrence theory.

In this paper we examine the impact of sanctions on the

criminal careers of almost 1000 offenders convicted of white collar crimes in seven United States District Courts between 1976 and 1978. Utilizing detailed data on offender backgrounds and the nature of court imposed sanctions originally compiled by Wheeler, Weisburd and Bode (1988), as well as information on subsequent criminal behavior provided by the Identification Bureau of the FBI, we assess the effect of imprisonment upon recorded criminal behavior of people convicted of white collar crimes over a ten year follow-up period. Our findings do not provide evidence of a specific deterrent effect of imprisonment on white collar criminals. Using a quasi-experimental research design in which we compare groups of offenders that are alike in terms of factors that led to their receipt of an imprisonment sanction, those sentenced to prison and those not are found to fit similar models of recidivism.

Prison, Specific Deterrence and White Collar Crime

Both in the criminal law and in the public consciousness prison sanctions are perceived as an effective deterrent to continued criminality among offenders (Farrington, Ohlin and Wilson, 1986). However, there is little empirical evidence to support this assumption (Lab, 1988). At least since the 1970s criminologists have consistently shown that those who are sentenced to prison have at best about the same rates of recidivism as non-imprisoned offenders, and in some cases a much higher rate (e.g. see Beck and Hoffman, 1976; Hopkins, 1976; Bartell and Winfree,

1977; Cohen, Eden and Lazar, 1991). Findings regarding the relationship between sentence length and recidivism evidence a similar pattern (e.g. see Babst et al., 1972; Gottfredson et al., 1977), though isolated specific deterrent effects have been noted in specific circumstances (e.g. see Gottfredson et al., 1977 in the case of very long prison sentences).

It is generally argued that white collar criminals will be particularly influenced by punishment policies (e.g. see Zimring and Hawkins, 1973; Geis, 1982; Braithwaite and Geis, 1982; Braithwaite, 1985). White collar crime is seen as a highly rational form of criminality, in which the risks and rewards are carefully evaluated by potential offenders, and white collar criminals are assumed to have much more to lose through sanctions than more common law violators (Braithwaite and Geis, 1982; Geis, 1982). Zimring and Hawkins note, in this regard, that "success determines the amount of investment in society an individual puts at risk when committing a threatened behavior" (1973: 128). In contrast to street criminals who have little to lose, white collar criminals, would seem especially susceptible to the threat of punishment.

While the relatively established economic and social positions of those convicted of white collar crimes lead a number of scholars to the conclusion that these offenders should be responsive to punishment policies, most discussion of deterrence for individual white collar criminals centers on the problem of general rather than specific effects (Wheeler et al., 1988). In turn, research

has generally focused on the corporate, rather than the individual offender, and frequently examines general deterrence resulting from new legislation, changes in prosecution, or the introduction of regulations (e.g. see Hopkins, 1980; Stotland et al., 1980; Geis and Clay, 1982). Studies of corporate sanctioning provide some support for a specific deterrent effect of sanctions in white collar crime (e.g. see Simpson and Koper, 1992). Nevertheless, it is difficult to generalize from corporate to individual offenders.

Evidence that sanctions may backfire and lead offenders to more serious or frequent offending (Farrington et al., 1986; Sherman et al., 1986; Petersilia and Turner, 1986; Bridges and Stone, 1986) also has implications for understanding the impact of sanctions on white collar criminals. The experience of punishment might be expected to reinforce the costs of criminality for the white collar offender (see Benson and Cullen, 1988, for a discussion of this view). Nevertheless, arrest, prosecution, conviction, and incarceration may also produce changes in the offenders present and future job opportunities, thus altering, and sometimes increasing his or her likelihood of reoffending (see Waring, Weisburd and Chayet, 1994). A stock trader, for example, who is restricted from future employment in the securities industry as a result of a conviction, may be more likely to look to illegal opportunities for financial gain.

The stigma of the criminal label may, in turn, serve to render the deterrent threat of punishment less serious. Once prestige and status are lost, they may be perceived as difficult to regain.

Once the cost of illicit behavior has been minimized, recidivism may be more likely. In some sense, the model of a spiraling process of deviance set into play by a labelling experience (see Wilkins, 1965) may be more appropriate for white collar criminals than for the common criminals for which the concept was initially developed.

The Sample

Our sample is drawn from a study of white collar criminals conducted by Wheeler, Weisburd and Bode (1988; see also Weisburd, Wheeler, Waring and Bode, 1991). They define white collar crime as "economic offenses committed through the use of some combination of fraud, deception, or collusion" (Wheeler et al., 1982:642; see also Shapiro, 1980). Following this they examine a stratified random sample of eight such crimes drawn from seven districts in the federal judicial system during fiscal years 1976-1978: antitrust offenses, securities fraud, mail and wire fraud, false claims and statements, credit and lending institution fraud, bank embezzlement, income tax fraud, and bribery.[1] While Wheeler et al. argue that their sample includes those offenses "that would most frequently be identified by persons as 'presumptively' white collar" (1982:643) and that most of the crimes identified in their sample fit one or another definition of white collar crime, they acknowledge that they cast a larger net for white collar criminals

[1]For a detailed description of sampling methods used by Wheeler et al. see Weisburd et al., 1991, chapter 1.

than most prior studies (see Weisburd et al., 1991).

The Wheeler et al. sample is drawn from a different population than most street crime samples (see Table 1). For example, only eight percent of the sample as a whole were unemployed at the time they committed their offense. This is a stark contrast to street criminals, most of whom are not employed in legitimate occupations (Sviridoff and McElroy, 1985). The large majority of those working (78%) were employed in white collar jobs (as defined by the Census Bureau), and almost one-third of the sample were officers or owners of businesses.

While the sample is clearly more "white collar" than a sample of street criminals would be, it is important to recognize that it also departs in some ways from accepted images of white collar criminals. The common portrait of white collar crime focuses on people of elite social status who use their established social and economic positions to commit crimes and avoid criminal justice punishment (Sutherland, 1949, Clarke, 1978; Geis and Stotland, 1980; Braithwaite, 1992; Geis, 1992). In contrast, the Wheeler et al., (1982) sample identifies offenders that are very similar to average or middle class Americans (Weisburd et al., 1991). As Weisburd et al. explain:

> Some of those examined were indeed located far above middle class status and use resources to commit their crimes and avoid punishment that are unavailable to all but the most privileged. And indeed these criminals are as alien to middle class citizens as are the poor who are popularly associated with most street crimes. But the majority occupy positions in society that are neither far above nor far below the middle, and their crimes do not necessitate nor do their defenses rely upon elite social status. Opportunities to commit these crimes are

available to average Americans. (Weisburd et al., 1991:3)

Identifying Criminal Histories

As our investigation began more than ten years after the criterion offense of the offenders we studied, we were able to begin with a follow-up period considerably longer than that available to many other investigators. We decided to focus on arrests[2] as our major measure of criminal conduct for two main reasons. First, though we cannot determine when actual criminal behavior occurs, the best measure is one which comes closest in time to offending (Maltz, 1984). Second, although all measures of recidivism include a substantial degree of error, that of a false positive (including some events as recidivism that are not instances of reoffending), is considered to be less serious than that of a false negative (excluding some events as recidivism because of attrition in criminal justice processing from arrest to conviction) (Maltz, 1984; Blumstein et al., 1986). Arrests are less likely to include this latter error than are other measures of recidivism.

We recognize at the outset that the meaning of an arrest for a white collar crime may sometimes be different than that for a street crime. Prosecutors, not the police, are usually the primary investigators of white collar crime (Katz, 1979). White collar criminals may, in turn, be "arrested" much later in the

[2] Like others we include here parole and probation violations which lead to a finger print being sent to the FBI and thus the appearance of an "event" in the FBI record.

investigative process than are street criminals (Braithwaite and Geis, 1982). Such offenders may not be arrested at all if prosecutors decide to use civil actions instead of a criminal prosecution (Mann, 1992). The fact that white collar crimes are often of longer duration than are street crimes (see Weisburd et al., 1991:44) provides added potential for misunderstanding criminal careers in a sample of white collar offenders.

Of course, this assumes that repeat white collar criminals specialize to some degree in white collar crime, a view that is challenged by recent research (see Weisburd, Chayet and Waring, 1990; Benson and Moore, 1992). Moreover, white collar crimes prosecuted in the federal courts seldom approximate the complex long term offenses reported in the popular press (Weisburd et. al., 1991). We suspect that the degree of bias in examining criminal history in this sample is not as different from that in other criminal populations as has been assumed. Nonetheless, the potential bias represented here is one that the reader should keep in mind when interpreting our study results.

Data Collection

Once we had identified arrest as our primary measure of criminal history it was natural that we attempt to gain access to Federal Bureau of Investigation "rap sheets" which are the most comprehensive single source of information on an individual's

arrest history.[3] Despite the fact that project staff submitted second and sometimes third requests for sample members for whom rap sheets could not be found, our final sample does not include three of every ten individuals found in the original sample.[4] A major reasons for non-receipt of rap sheets is that a number of the offenders in the sample did not receive FBI identification numbers prior to sentencing.[5] As a result we had to use other identifiers, such as name, date of birth, and social security number to capture FBI files.

The fact that the sample was on average much older than common criminals led us to add additional data to our study which

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[3] It is interesting to note that the rap sheets provide a somewhat different picture of official offending than do the PSIs used in Wheeler et al.'s original study. While forty-three percent of these offenders are identified as having a prior arrest in the PSI for the criterion offense, only thirty-two percent are so identified by the FBI. We are not surprised by the difference here, since probation officers routinely contact local police agencies and receive information that might not be sent on to the FBI. It is important to note that in examining offenses omitted on the rap sheets we found that they were usually less serious events such as traffic violations, bad checks and failure to pay child support.

[4]While we suspect that the validity of our research design is not greatly impacted by this case attrition, we did examine at the outset the sources of potential sample selection bias in our study. We were less likely to receive rap sheets for the oldest defendants in our sample, reflecting, at least in part, the purging practices of the FBI Identification Bureau. Those with more prior arrests in the original study were more likely to appear in our sample as were those who received an imprisonment sanction for the criterion offense. While the criterion offense category is not significantly related to receipt of a rap sheet, district of conviction is. Finally, women have a lower probability of having a rap sheet that we could identify, even controlling for the seriousness of the criterion offense and the number of prior arrests.

[5] Forty-two percent of offenders in this sample did not have FBI identification numbers in the PSIs.

would identify sample members who had died during the follow-up period. Such consistent compilation of deaths was available in the National Death Index (NDI).[6] The importance of identifying who in the sample had died and when death occurred was confirmed when we examined the NDI data. In total, fourteen percent of the sample had died between date of sentencing for the criterion offense and 1990 when we began data collection for the study.

The Effects of Imprisonment: Methodological Concerns

In the analyses that follow we focus on the impact that the presence or absence of a prison sanction has upon subsequent criminality. Our decision not to examine the effect of length of sentence was due in part to the difficulty we encountered in accurately defining time served. For us, as for other researchers who examine the federal system before imposition of the United States Sentencing Commission guidelines, neither the courts nor other federal agencies provide a precise method for tracking offenders through the criminal justice system (Criminal Justice

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[6]The NDI is a centralized listing of a set of identifying information on all decedents whose deaths have been registered with the states since 1979 (Department of Health and Human Services, 1981). The agency has developed a matching program which uses such information as name (using both exact and phonetic spellings), social security number, and date of birth which identifies individual decedents who may be sample members. Evaluation of this program indicates that it is successful at finding true matches and that its success rate improves with the quality of the identifiers submitted (Patterson and Bilgrad, 1985). Although the NDI has primarily been used for health research, the Index permits inter-agency requests from the Federal government for fact and date of death information

Information Policy, 1988).[7] Because of possible reductions in prison sentence through either good time credits or parole release, imposed sentences cannot provide an accurate estimate of time served.

Irrespective of the difficulty of gaining information on the length of served prison terms in our sample, our decision not to examine the impact of length of prison upon recidivism is consistent with other major criminal career studies (e.g. see Blumstein et al., 1986; Blumstein et al., 1988). In our sample, as in most street crime samples, relatively few offenders are sentenced to very long prison terms and thus there is little basis upon which to make comparisons of the experiences of the offenders examined. In our sample about half of those sentenced received a prison term (see Table 2). Of these, less than half were sentenced to more than six months imprisonment and less than 40% to a prison term of more than a year. Given the fact that prisoners in the federal system were unlikely to serve more than one third of their

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[7] When we drew information from the FBI, the Bureau of Prisons, and the Federal Parole Bureau, we were not able to establish with any degree of certainty the time in prison offenders had actually served. In the case of the rap sheets we found that time of release was seldom reported, though it was much more likely for an entry to be made when an offender entered a federal correctional facility. While the Bureau of Prisons has more accurate information on prison stays, during the period of time we studied computerization was just beginning and information on imprisonment is often missing for our offenders. Moreover, because different identifiers were used by different federal agencies, and some of our offenders served special sentences in local institutions, even those offenders who could be tracked are not accurately identified by the Bureau. Finally, parole records are accurate, but they fail to provide information on those offenders who did not come under the Parole Commission's jurisdiction.

imposed sentence before the imposition of the U.S. Sentencing Guidelines,[8] we believe that the basis for any comparisons of length of sentence are even more constrained in our sample than in the case of more general criminal career studies (e.g. see Schmidt and Witte, 1988).

The fact that we could not identify how long those sentenced to prison actually served meant that we also could not take into account how such prison penalties affected the time that offenders were "at risk" to recidivate in our sample. Even recognizing that white collar crimes, such as mail and wire fraud, can be committed by an offender in state custody,[9] it is clear that the risk of reoffending is different for those in prison and those not.

We believe that the actual biases that develop from the absence of accurate information on time served for the criterion offense in our sample is likely to be small. As is illustrated in Table 3 it takes, on average, a very long time for offenders who will reoffend in our sample to gain a subsequent rap sheet entry. Only twenty-seven percent of those in the sample who reoffend do so within the first year of follow up. Of the 297 individuals in the sample who fail in the follow-up period, almost half take more than three years to gain a subsequent rap sheet entry. This may be

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[8] This estimate was developed by the U.S. Sentencing Commission (U.S.Sentencing Commission, 1991, Volume II).

[9] In a remarkable case from the study, for example, one false claims swindler submitted fraudulent tax statements for himself in the name of Michael Rodent (also known as Mickey Mouse) and seven dependents: "this offender submitted as many as eight-five false claims to the IRS for as much as $77 thousand per year". (Weisburd et al., 1991:34).

compared with studies of street criminals which often find reoffending likely within a year of follow-up (Visher and Linster, 1990). Overall, for this sample, time served for the criterion offense accounts for a very small proportion of the overall time at risk for offenders who do reoffend.

Comparing Recidivism For Similar Offenders

In the following analysis we use a quasi-experimental design in which we compare groups of offenders that are alike in terms of factors that led to their receipt of an imprisonment sanction.[10] In order to identify similar defendants we began with a multivariate regression model that identified the factors influencing whether defendants were sentenced to prison. We then use this model to calculate, for each offender we study, the predicted probability of going to prison, irrespective of whether that particular offender was actually sentenced to a term of imprisonment. This provided us with a method for identifying similar offenders who were sentenced differently (i.e. to prison or not) by the judges they faced.

In developing our estimates of the predicted probabilities of imprisonment for offenders in the sample, we drew from a model of

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[10] In analyses in which we attempt to identify the specific factors that influence recidivism we use a multivariate statistical design (Weisburd et al., 1993). Our findings concerning the impact of imprisonment in these multivariate analyses are similar to those reported here. Nonetheless, in examining the specific question of the impact of imprisonment sanctions on sample members, we agree with Farrington, Ohlin and Wilson, that "quasi-experiments are far more convincing than correlational analyses" (1986:91).

sentencing behavior developed for this data set by Wheeler, Weisburd and Bode (1982; see also Weisburd, Waring and Wheeler 1990). Their model took into account twenty-one variables including such legally relevant indicators as prior record, type of conviction, statutory category of the offense, the district of conviction, and obvious social dimensions: sex, race, age, education and social status. Going beyond prior sentencing studies, they also controlled for both "act-related" (e.g. amount of victimization, geographic spread, type and number of victims, and offense complexity) and "actor-related" (role in the offense, cooperation with prosecution, remorse over the crime, and social record) variables often mentioned by federal judges (see Wheeler et al., 1988). We estimate a reduced logistic regression model including only the significant parameters ($p<.05$) for our sample cases (see Appendix A). This model, which categorized seventy-two percent of the cases correctly (an increase of thirty-three percent over the base rate (54%)),[11] was then used to develop the individual predicted imprisonment scores for our offenders.

Examining the distribution of these scores for those who had a prison sanction imposed and those who did not, we found that treatment and comparison groups closest in their mean probability estimates were gained by dividing our sample into three sub-samples (see Table 4). The first (A) includes offenders with a relatively low probability of imprisonment ($p\leq.40$). The second (B) includes

[11] For this analysis offenders were placed in the "predicted prison category" if their probability estimate of imprisonment was greater than fifty percent.

only those offenders with a relatively high predicted likelihood of receiving an imprisonment sanction ($p>.60$). The final group (C) represents a moderate probability category ($.40<p\leq.60$).

As Table 4 illustrates, dividing the sample up in this manner provides treatment and comparison groups with a fairly large number of cases that are relatively close in their overall mean estimates of probability of imprisonment.[12] Of the three sub-samples, the "moderate" category has the closest estimates, with both treatment and comparison groups showing an average probability of fifty percent. The "low probability" category, with a difference of .07 between the treatment and comparison groups, has the largest difference in mean probability estimates.

When we examine specific variables that might impact upon subsequent criminality we find strong support for this basic approach to the creation of equivalent groups. Looking at gender, race, class, drug use, prior arrests, marital status, type of residence, district and type of conviction for the criterion offense, and employment history across the three probability sub-samples we find that the selection procedure we employed created very similar prison and no prison comparison samples. Indeed in only one of the thirty comparisons that were examined (gender in

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[12] This issue of the size of the groups was important in part because we wanted our three comparisons to provide powerful statistical tests of the questions we examined (see Weisburd, 1993). Using Cohen's (1977, 1988) definition of moderate effects and a .05 two tailed significance test, the size of our sub-samples would provide a statistical power level above .80--a level that both Gelber and Zelen (1985) and Cohen (1988) suggest for experimentation.

the low probability sub-sample) was there a statistically significant difference between the prison and no prison samples.[13]

The Impact of Imprisonment on Recidivism

We begin our analysis with a simple comparison of how the prison and no prison groups differed in terms of their likelihood of "failure" during the follow-up period. Failure is defined in our analysis as any subsequent rap sheet entry for a new event, usually coded as an arrest, but sometimes evidenced in our data by a prison or jail entry (with no arrest noted on the rap sheet) or a probation or parole violation (in which a finger print record was transferred to the FBI).

As is apparent from Table 5 there is very little evidence of deterrence for the prison sample in terms of the simple likelihood of failure in the follow-up period. In the group that was defined by a high probability of imprisonment, the prison sample had a failure rate of about forty percent and the no prison sample had a rate of thirty seven percent. In the low prison group, the results are very similar, though the base rate of failure for both samples is much lower. Thirty-one percent of the prison sample recidivated in the follow-up period, as opposed to twenty-seven percent of the no prison sample. In the moderate probability of imprisonment

[13] We do not believe that this single difference warrants any change in the weights of our analysis, as suggested by Berk (1987). Our decision here derives in part from the fact that the gender difference is relatively small. Also, the result is gained from a series of thirty significance tests which would be expected, on average, to yield one such significant result just by chance.

category, there are larger differences between the samples. But the direction of this relationship does not suggest a deterrent effect for imprisonment. Forty-one percent of the prison sample failed in the follow-up period contrasted with twenty-eight percent of the no-prison sample.

Turning to "time to failure" for those who did recidivate during the follow-up period, we find a slight improvement for those in the high probability prison sample (Table 6). Nevertheless, in the low probability grouping, prison seems to speed-up rather than slow-down failure. Almost half of those in the prison sample who recidivated in the follow-up period did so within one year. Less than a third in the no prison sample did so. In the moderate probability category, the rates of failure are similar for both the prison and no prison samples.

One problem in interpreting these data is that we have assumed so far that everyone in our sample is free to fail during the entire follow-up period. As discussed earlier, however, a number of the offenders in our sample died during the follow-up period and thus cannot be seen at risk of failure after their deaths.[14] Conversely, our discussion so far assumes that those who have not failed in the follow-up period will never fail. They are deemed successes. However, it is possible, and even likely, that some of

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[14] A somewhat similar problem is reflected in the problem of prison or jail sentences in the follow-up period. As we noted earlier, most scholars assume that such reduction in risk time is relatively small for most offenders. We think this particularly relevant in the case of our sample, where individuals seldom commit crimes that would lead to long stays of imprisonment.

these offenders will fail after the "censoring" date of the study (i.e. the last date for which data was collected).

One technique that allows us to correct for these assumptions, while providing a general estimate of the differences between the models of failure for the prison and no prison samples we study, is what has come to be called event history analysis (Allison, 1984). Event history analysis treats those individuals who have not failed by the end of the follow-up period as "censored." That is, it recognizes the fact that they may fail in the period subsequent to the data collection period. It also permits censoring of individuals in our sample who died before the end of the follow-up period. The estimates in table 7 are developed using models provided in Surfit, software developed by Michael Maltz of the University of Illinois (1989).

Because there are a number of distributions that might be used to estimate the form of reoffending over time in such models, we provide parameter estimates from two distributions that appear to provide a good fit to our data. The first, the lognormal distribution, is commonly employed in recidivism research (e.g. see Schmidt and Witte, 1988). It assumes that everyone in the sample would eventually recidivate given a follow-up period of infinite length. The Gompertz distribution has been used less often in research on criminal careers. However, it has the advantage that under certain conditions it behaves as an incomplete distribution, that is, one that does not have an implicit assumption that all

offenders fail in the long run (Maltz, 1989).[15] We believe that this latter assumption is more appropriate for our sample.

Our major concern is whether the estimates gained here confirm our earlier findings. Looking at the lognormal distributions for the high, moderate and low groups, we do not find statistically significant differences between the prison and non-prison sub-samples.[16] The results for the Gompertz models also suggest that prison does not significantly impact recidivism. In both cases the contours of the likelihood functions for each of the prison and no prison comparisons in our analysis are not found to be significantly different at the five percent level.[17]

In Figure 1 we provide a graphic representation of our findings by plotting the survival distributions for each of the three sub-samples. Overall, these figures reinforce our basic

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[15] A number of recidivism studies have found that models which do not constrain all offenders to failure often provide a better fit than those that do (Schmidt and Witte, 1984; Maltz and McCleary, 1977; 1978, Maltz, et al., 1979, Maltz and Pollock, 1980). The incomplete models first used by Maltz and McCleary, however, are less tractable than those based on the Gompertz distribution.

[16] Some caution should be exercised in interpreting significance in our analysis. Our sample is a stratified one and thus does not represent the true population of offenders convicted of the crimes we examined. At the same time, because of our use of a quasi-experimental analysis strategy our offenders are divided into like groups that represent in a broad way those with low, moderate and high risks of imprisonment. These are the populations to which are inferences are made.

[17] Using Surfit we developed contours for each of the six distributions based on a ninety-five percent confidence interval. In each of the pairs of prison and no-prison samples the contours were found to overlap. We want to thank Michael Maltz for his assistance in constructing and interpreting these analyses.

conclusion that the prison and no prison samples are similar. There are relatively small differences in the curves represented in the plots of each sub-sample. However, in both the moderate and low probability comparisons there is a consistent backfire effect across the length of the distribution. Indeed, in the moderate probability comparison sub-sample this effect appears to be growing over time. In the high probability group there appears a very slight deterrence effect after 90 months, though it is important to note that the differences noted here, as those above, are small and not statistically significant.

Prison and White Collar Criminals

We find little evidence of any deterrent effect of imprisonment for our sample of offenders convicted under white collar crime statutes. When differences are found between the groups it is generally in the direction of "backfire" rather than deterrence. Nonetheless, in our main analyses these differences are not found to be statistically significant. But having concluded this, it is important to raise the question of why a sanction that is looked at as so serious within the criminal justice system has so little impact upon those who receive it.

One fact to note is that prison may have very important impacts on other aspects of the lives of these criminals that are not assessed in our study. For example, we believe it is likely that imprisonment would affect the occupational or personal histories of offenders (see Waring, Weisburd and Chayet, 1994),

though, of course, criminal history information provides little evidence of these very central features of their lives. Other studies suggest that criminal interventions that are deemed as failures in terms of their influence on recidivism, may have significant impacts on the quality of life, as measured by employment or personal stability, of those studied (Rossi, Berk and Lenihan, 1980; Berk, Lenihan, and Rossi, 1980).

Moreover, though policy makers often assume that imprisonment influences the future conduct of prisoners (e.g. see Schlegel, 1990), as we noted earlier, there is little evidence of specific deterrent effects in previous studies. The focus on incapacitation, or the crime control benefits gained through dangerous offenders being isolated from the community, has developed in part because so little evidence exists that imprisonment deters those sanctioned from future offending (Clarke and Weisburd, 1990). Nonetheless, it is often noted that there is not a specific deterrent effect for street criminals either because they have so little to lose from contact with the criminal justice system (Mann et al., 1980; Pollack and Smith, 1983) or they are at the outset unlikely to act rationally in their decisions about criminality (Braithwaite and Geis, 1982). The white collar criminals we examine, provide an important case study precisely because they address this concern. The fact that deterrence fares no better for a white collar sample than for more common offenders, provides a strong challenge to those that posit any specific deterrent effect of imprisonment.

These data provide some evidence of backfire effects of sanctioning. However, they also challenge those who claim that such unintended impacts will be very large for those convicted of white collar crimes. It has long been argued that prison may provide a training ground for criminality (Goldfarb, 1975). The labelling effects of imprisonment have been assumed, as well, to restrict the legitimate opportunities of offenders in the community (Gove, 1980; Lemert, 1984; Tittle, 1988), a factor which may be particularly significant for those who work in white collar occupations (see Waring, et al., 1994). Nonetheless, as Michael Benson (1985) suggests, the prison experience may only provide a marginal impact on such offenders, whose experience with the criminal justice system up until time of sentencing may provide the major deterrent effect of the criminal justice process.[18] We believe that a closer understanding of the offenders who fall in our sample and the nature of their criminal careers can provide important insight into why imprisonment does not have either a consistent deterrence or backfire effect.

A number of the offenders in our sample do not fit common stereotypes of criminality. They are often conventional people who confront some special crisis or opportunity that leads them to

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[18] One judge cited in Wheeler, Mann and Sarat (1988), Sitting in Judgement indicates:

> There is no doubt about the fact that in most white-collar crimes as such the return of the indictment is much more traumatic than even the sentence...There is no question about the fact that that is much more severe on the white-collar criminal than it is on the blue collar defendant (145-146).

temporarily cross the line and commit crime (see Weisburd, Waring and Chayet, 1994). These people, evidence a high degree of stability both in their professional and personal lives. Such offenders may be likely to be strongly impacted by the process of punishment (Feeley, 1979; Benson, 1982; Wheeler et al., 1988). And there are many cases in the pre-sentence investigations where such people appear to be shocked at what has befallen them. Take for example the following perjury and FIDC offender as described in his pre-sentence report:

> Regretfully, I did not tell the Grand Jury the complete truth of the matter. Under the stress and panic I was under I could not remember the details and facts as I ordinarily would. Even to the fact that immediately after leaving the Grand Jury, I called my wife at her place of employment and asked if I could speak to Mrs. ---, her previously married name....I will regret this action for the rest of my life. These past six months have been a living hell not only for me but for my wife and those closely associated with me.

We agree with Benson that a short prison stay, the main type of prison sanction evidenced in our sample, is not likely to provide more than a marginal impact beyond the experience of prosecution, conviction and sentencing itself. Whatever specific deterrence is gained may be produced before the imprisonment sanction is imposed.

For members of our sample who are more committed to criminality we again think it understandable that short prison stays have relatively little impact on future reoffending. Overall the time to failure for those in our sample is relatively long. More than half of those who did fail during the follow-up period go more than three years without a subsequent rap sheet arrest. It seems to us unreasonable to expect that a prison sentence of a few

months would guard against future crimes that occur years later.

It may be that white collar offenders who approach crime in a calculating fashion would be influenced by particularly long prison experiences. This assumes, of course, that for these offenders a long prison stay has a special impact beyond the stigma of criminal justice processing and prison punishment. While we cannot examine this question with our data, we believe it reasonable that such offenders might decide that the rewards of continued criminal behavior are offset by the experience of a long prison stay. However, it is important to note that it is very rare for white collar offenders, or indeed any offenders in the federal system not convicted of violent or drug crimes,[19] to be sentenced to prison terms of even a few years in length.

In regard to those people who evidence significant personal and occupational instability, such an effect would not be likely. Their life experiences generally are consistent with those described by Gottfredson and Hirschi (1990) in their portrait of criminality. These offenders evidence low self-control and an inability to delay gratification. There is no reason to expect that imprisonment years in their past would prevent them from seeking short term gratification in the present.

Conclusions

Only a true experimental design would allow researchers to make a clear and unambiguous connection between imprisonment

[19] U.S. Sentencing Commission, 1991, Volume 2.

sanctions and recidivism (Farrington, 1983). In this study we relied upon a quasi-experimental method which placed convicted white collar offenders in like comparison groups. While the ethical and practical constraints surrounding random allocation of prison sanctions have generally prevented true experimental studies of imprisonment (see Farrington, 1983; Weisburd et al., 1990; Weisburd, 1993), it is useful to speculate before concluding on the specific limitations of our methods.

If judges are giving significant weight to variables that are not assessed in the model that forms the basis of our allocation procedure we would expect systematic biases in our results. This problem is also relevant to correlational designs, and is likely to be more serious in cases where the overall model used is not well specified. While the Wheeler et al. model of imprisonment takes into account a large series of factors which influence the imprisonment decision, we recognize that biases that develop from excluded factors cannot be ruled out in our study. The appearance of small backfire effects in our analyses, for example, might reflect differences in the types of offenders likely to reoffend in the prison comparison samples rather than any specific influence of prison sanctions on recidivism. Nevertheless, we think it unlikely that such biases are large enough in our analyses to alter our basic findings, a conclusion that is supported by the fact that we could find little difference between the prison and no prison samples when examining a series of relevant background and criterion offense characteristics.

It has often been assumed by scholars and policy makers that white collar criminals will be particularly affected by imprisonment. Our findings provide evidence that this assumption is wrong, at least as regards reoffending among those convicted of white collar crimes in the federal courts. The fact that the prison and no prison samples we study fit similar models of recidivism leads us to conclude that prison does not have a significant impact on the likelihood of future criminal justice contacts for offenders convicted of white collar crimes. The measure of specific deterrence gained for these relatively established offenders appears to be no greater than that found among their common crime counterparts.

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Table 1 Employment Characteristics of Basic Wheeler et. al. (1982) Sample

Social and Demographic Characteristics	Percentage
Unemployed	8
Owners or officers	30
Employed in white-collar occupations*	78
N**	1090

* The definition of white-collar occupation is that used by the U.S. Census Bureau in their occupational classification system. See U.S. Bureau of the Census (1977, p. 152-155).

** This is the maximum number of cases used. Specific statistics are calculated using at least 90% of the cases.

Table 2 Prison Sentence Imposed for Criterion Offense

Percent of Offenders Receiving a Prison Sentence for the Criterion Offense	49.4%
Base n	993
Lengh of Sentence for Those Sentenced to Prison	
6 months or less	53.0%
6 months and 1 day to 1 year	8.8
1 year and 1 day to 3 years	23.7
3 years and 1 day to 5 years	7.2
More than 5 years	7.2
Base n[1]	430

[1] Length of sentence was not available for 48 offenders.

Table 5 Failure (Based on Arrest) Following the Criterion Offense by Offender Groups and Prison Sentence Imposed

Offender Group	Low		Moderate		High	
Sentence	Prison	No Prison	Prison	No Prison	Prison	No Prison
Failure Recorded	25.3%	17.6%	33.3%	20.8%	29.6%	24.4%
No Failure Recorded	74.7	82.4	66.7	78.2	70.4	75.6
Total At Risk	91	364	120	96	240	78

Table 6 Time Until First Failure for Recidivists by Prison Sentence Imposed and Offender Group

Offender Group	Low		Moderate		High	
Sentence	Prison	No Prison	Prison	No Prison	Prison	No Prison
Time Since Criterion Offense	Cumulative Percent of Failures	Cumulative Percent of Failures	Cumulative Percent of Failures	Cumulative Percent of Failures	Cumulative Percent of Failures	Cumulative Percent of Failures
Less than 1 month	4.3	3.1	5.0	5.0	6.0	0.0
1 month to 6 months	34.9	14.0	10.0	15.0	14.1	10.5
6 months and 1 day to 1 year	47.7	28.1	22.5	25.0	25.4	31.6
1 year and 1 day to 2 years	60.7	46.9	45.0	40.0	35.2	42.1
2 years and 1 day to 3 years	69.4	57.8	55.0	60.0	42.3	47.4
3 years and 1 day to 5 years	82.4	76.6	77.5	75.0	66.6	63.2
More than 5 years	100.0	100.0	100.0	100.0	100.0	100.0
Total Failures	23	64	40	20	71	19
Total At Risk	91	364	120	96	240	78

Table 7 Survival Models by Prison Sentence Imposed and Offender Group

Offender Group	Low		Moderate		High	
Sentence	Prison	No Prison	Prison	No Prison	Prison	No Prison
Log normal Distribution [1]						
Log Likelihood	-154.57	-449.56	-241.11	-134.23	-478.46	-130.6
Percent Surviving	0	0	0	0	0	0
μ	6.36	6.78	5.54	6.35	5.65	5.78
σ	3.05	2.78	2.02	2.70	2.29	2.40
Gompertz Distribution [2]						
Log Likelihood	-154.18	-446.57	-240.09	-133.23	-478.84	-130.6
Percent Surviving	68.1	73.3	58.6	68.8	50.8	59.8
θ	.010	.006	.008	.007	.006	.008
δ	-.026	-.019	-.015	-.020	-.009	-.010

[1] $S(t)=1-\Phi[(\ln(t)-\mu)/\sigma]$ where Φ is the standard normal cumulative distribution function.

[2] $S(t)=e^{\theta(1-e^{\delta t})/\delta}$

Figure 1a

Survival Distribution for the High Probablity Group

Prison and Non Prison Groups, Observed and Predicted

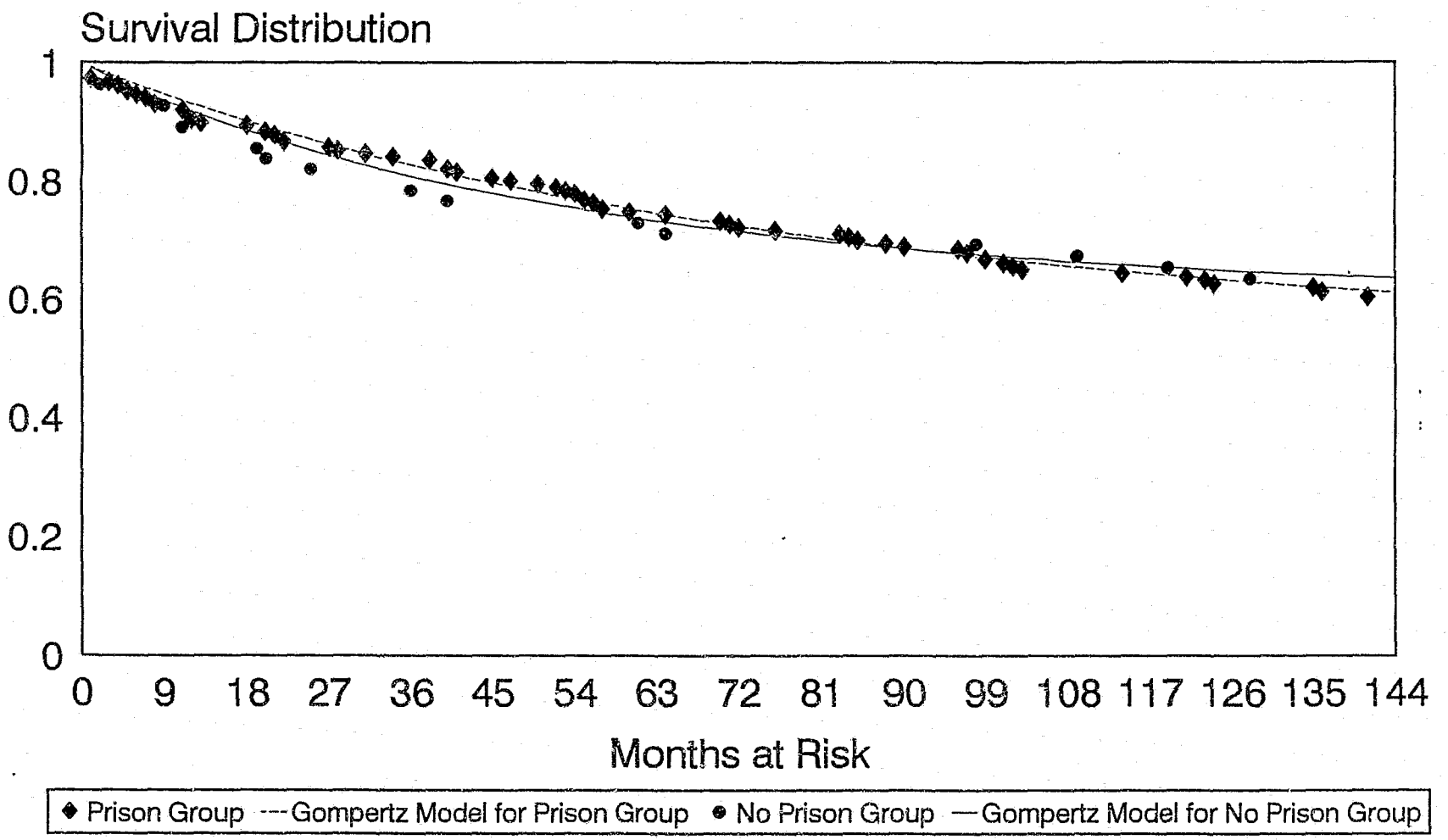


Figure 1b

Survival Distribution for the Middle Probablity Group

Prison and Non Prison Groups, Observed and Predicted

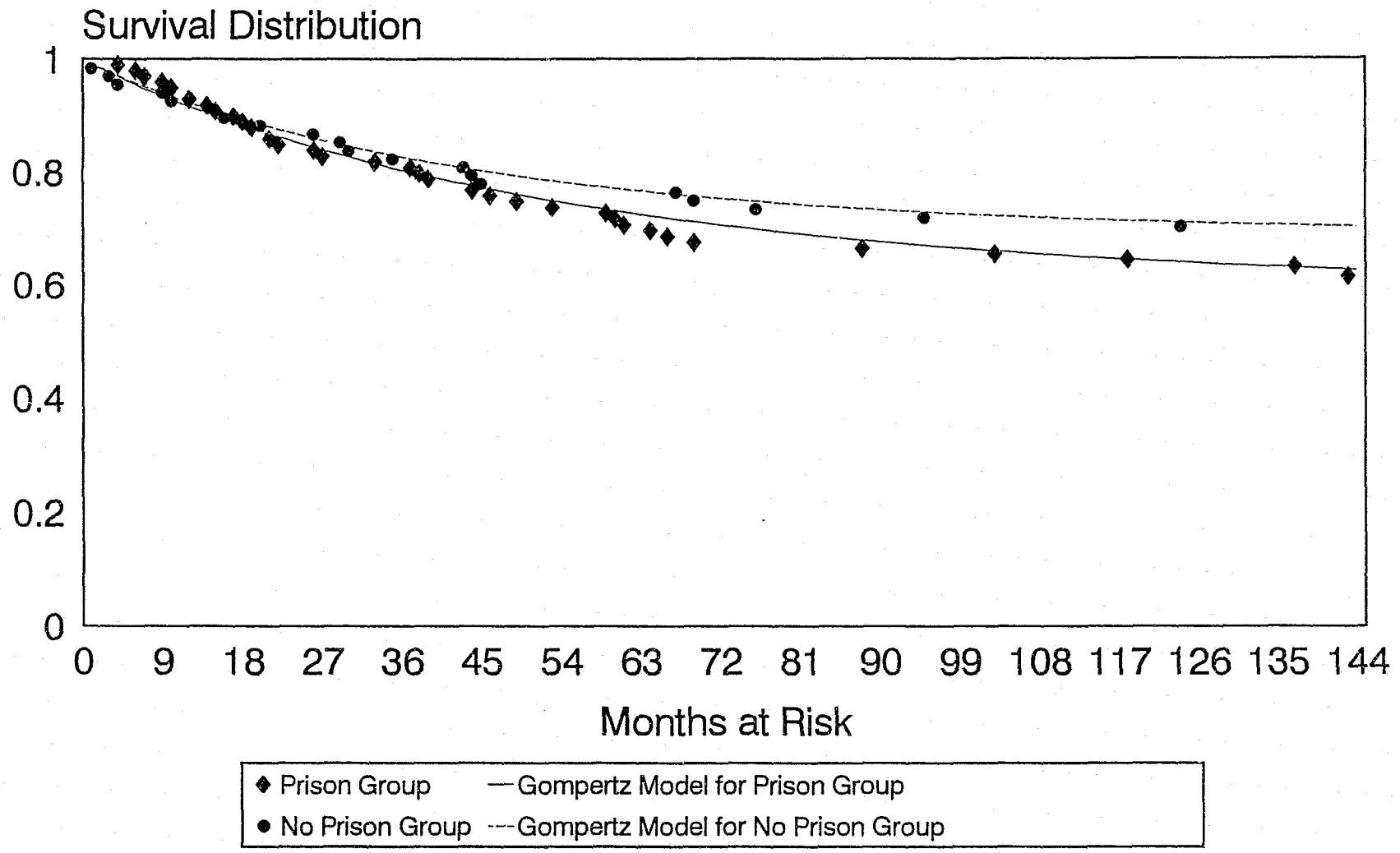
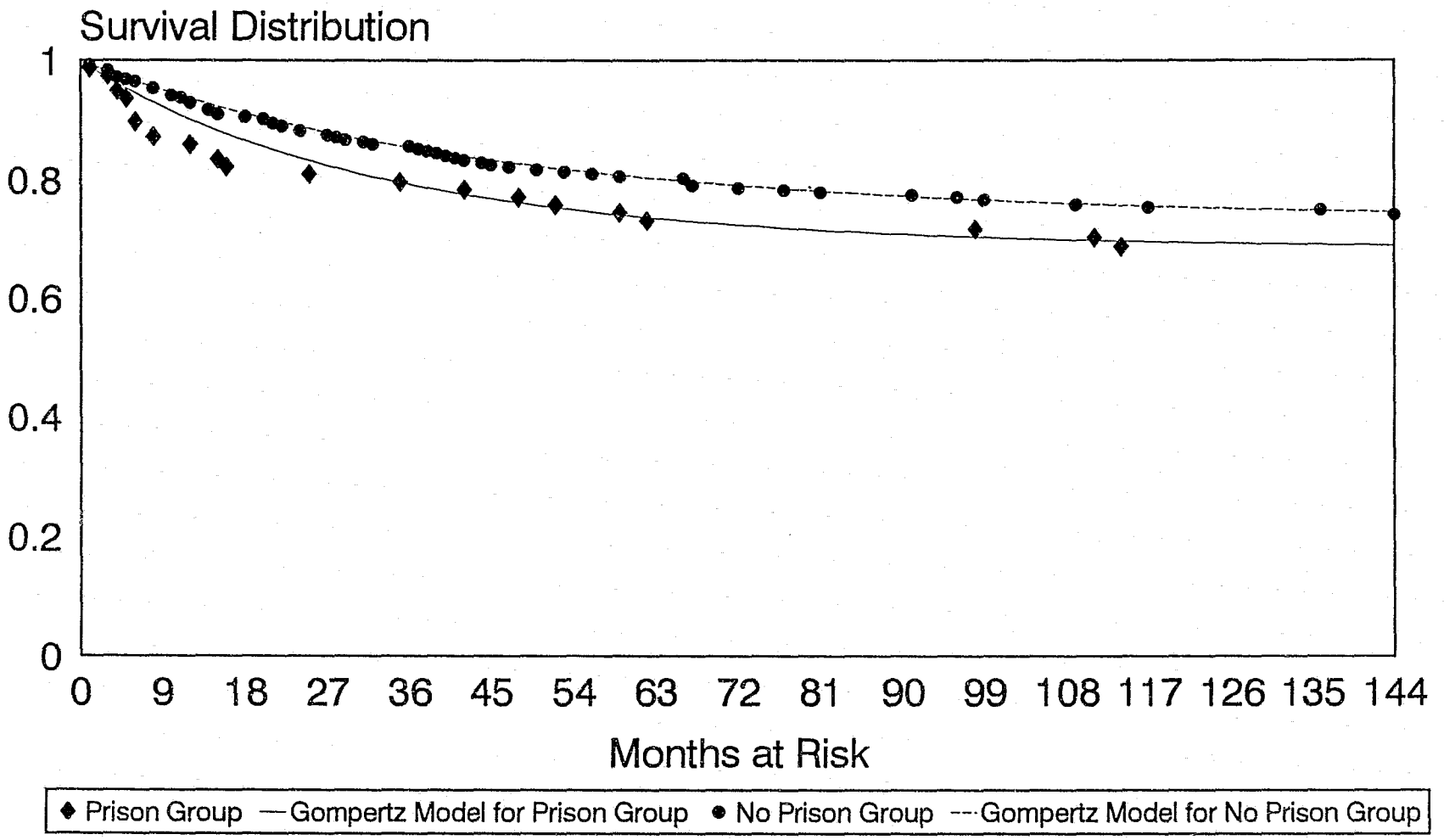


Figure 1c
Survival Distribution for the Low Probablity Group
Prison and Non Prison Groups, Observed and Predicted



Appendix A

Reduced Logistic Regression Model Used to Predict the Likelihood of Imprisonment of the Offenders in the Wheeler et al. Sample

Variable	Beta	Chi Square
Intercept	-5.2	26.57
Act Related Variables		
Dollar Victimization	0.17	22.41
Offense Complexity	0.10	4.96
Geographic Spread of Illegality	0.25	6.62
Maximum Exposure to Imprisonment	0.15	34.28
Actor Related Variables		
Duncan Socioeconomic Index	0.01	10.69
Impeccability	-0.13	4.87
Number of Prior Arrests	0.09	9.64
Most Serious Prior Arrest	0.24	5.92
Role In Offense[1]		
Middle	-1.03	5.13
Minor	-0.90	11.19
Missing	-0.41	3.58
Legal Process Variables		
Statutory Offense[2]		
Bank Embezzlement	-0.34	1.49
Tax Violations	0.82	9.15
Mail Fraud	-0.39	1.81
Securities Violations	0.12	0.07
False Claims and Statements	-0.60	4.38
Bribery	-0.78	3.71
Antitrust	-0.94	2.00
Other Variables		
Sex	-1.13	21.39
Age	0.08	3.19
Age Squared	-.001	5.01
Judicial District[3]		
Central California	0.33	1.26
Maryland	0.65	4.00
Southern New York	-0.07	0.07
Northern Texas	1.05	11.87
Northern Illinois	0.67	4.82
Western Washington	0.41	1.69

N of Cases=989
Model Chi-Square=305.08 with 27 degrees of freedom.
-2 log likelihood=1058.30, $p < .001$
Note: All variables are statistically significant at the .05 letters.

[1] Major role is the excluded category.

[2] Credit Fraud is the excluded category.

[3] Northern Goergia is the excluded category.