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ABSTRACT

Title of Dissertation: VIOLATIONS OF PROBATION RELEASE CONDITIONS AND CRIMINAL RECIDIVISM

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The primary intent of the study was to examine whether violations of release conditions "signal" criminal recidivism during probation supervision. Two statistical tests were developed to test such a "signalling" hypothesis using data from ten sites involved in the RAND Corporation evaluation of intensive supervision programs.

Logistic regression models were estimated first to assess whether technical violation charges predicted arrest during community supervision, adjusting for demographic, criminal history, supervision intensity, and community factors related to recidivism as well as the possibility of confinement during the one-year followup period. By and large, technical violation charges (regardless of technical violation charge type) were associated with a decrease in the probability of arrest rather than an increase in the probability of arrest as the signalling hypothesis would suggest. A series of multinomial logistic regression models were estimated next to explore

whether technical violation charges and arrests appeared to be manifestations of the same underlying propensity to offend consistent with a generality of deviance understanding of the signalling hypothesis. Results of the analyses revealed that technical violation charges and arrests did not appear to be generated by the same underlying process.

In short, probationers who violated release conditions did not appear to have a higher probability of arrest during community supervision, nor did technical violations and arrest appear to spring from the same underlying tendency to offend. The inverse relationship between technical violation charges and arrests was more consistent with either a deterrent or specialization effect.

Probation policies grounded in the signalling hypothesis (e.g., policies that seek to protect public safety by revoking the community supervision status of probationers who violate release conditions) should therefore be carefully evaluated. In view of the substantial cost of revocation to corrections systems and the questionable impact of such policy on public safety, the results of this study support the increased use of intermediate sanctions (short of revocation) to respond to probationer noncompliance.

VIOLATIONS OF PROBATION RELEASE CONDITIONS

AND CRIMINAL RECIDIVISM

by

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DEDICATION

I would like to dedicate this dissertation to my family.

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TABLE OF CONTENTS

List of Tables	vi
List of Figures	ix
Introduction	1
Chapter	
1. Probation Supervision	5
Probation Defined	5
History and Development of Probation: An Overview	6
Prevalence of Release Conditions	13
Rationale for Release Conditions	15
Violations of Release Conditions	16
Variation in Technical Violation Rates	18
Probation Revocation and Institutional Crowding	20
Technical Violations as "Signals" of Criminal Recidivism	22
Policy Implications of the Signalling Hypothesis	24
2. Theoretical and Empirical Foundation of the Signalling Hypothesis	27
Empirical Evidence Related to the Signalling Hypothesis	37
3. Statement of the Problem	45
4. Method	50
Subjects	50
Procedure	54
Measures Used in Test 1	57
Treatment of Missing Data	68
Measures Used in Test 2	73
Data-Pooling Strategy	74
Statistical Models	75
General Model-Building Approach	77
Summary	81

TABLE OF CONTENTS

Chapter

5. Results 83

Test 1
- Total Sample Analysis 83
- Total Sample Results 87
- California Subsample Analysis 92
- California Results 94
- Georgia Subsample Analysis 96
- Georgia Results 98
- Miscellaneous Subsample Analysis 100
- Miscellaneous Results 101
- Summary of Test 1 106

Test 2 110
- Test 2 Results 113

6. Discussion 116

Tables 126

Figures 165

Appendix 186

References 195

INTRODUCTION

The mission of probation supervision has changed notably since the days of John Augustus. Disillusionment with offender rehabilitation has given rise to a more singular focus on offender incapacitation, control, and punishment during probation supervision. Decisionmakers have increased both the number of release conditions imposed and the level of supervision intensity (Clear & Hardyman, 1990:71; Fulton et al., 1995:25; Parent et al., 1994:4-5; Petersilia & Deschenes, 1994:322).

While the imposition of increasing numbers of conditions in combination with more intense supervision may indeed influence probationer behavior (e.g., deter probationer misconduct), many scholars observe that these two methods of exacting offender control and retribution simply result in higher rates of technical violations (i.e., violations of release conditions) (Blomberg, 1995:49; Buck, 1989:69; Cullen, 1996:114; Fulton et al., 1995:25; Parent et al., 1994:4-5; Petersilia & Turner 1993:311-312, 319; Tonry, 1990:178). The imposition of greater numbers of conditions increases the opportunity for failure while heightened surveillance increases the ability of supervisory officers to detect violations. While such changes are usually associated with the intermediate sanction movement and the reemergence of intensive supervision programs (ISP), the guiding philosophy has trickled down to routine probation caseloads as well (Byrne & Taxman, 1994:230; Gordon, 1991:133; Parent et al., 1994:4).

Given the sheer size of the probation population relative to institutional populations, changes in probation violation and hence revocation rates may have a substantial impact on prison crowding and costs (Parent et al., 1994:2; Rhine & Humphries, 1993:101). Probation and parole revocations represent an increasing proportion of new prison admissions nationwide. The percentage of state prison admissions due to probation and parole revocation, for example, has increased from 17% in 1980 to 30% in 1992 (Snell, 1995:56).

Increasing revocation rates raise two very important criminal justice system policy questions: (1) What does it mean when probationers violate release conditions? and (2) How should probation systems respond to evidence of noncompliance?

The present research explores the first question with regard to criminal recidivism. The intent of the study is to subject to empirical test the common (though largely untested) assumption that technical violations proxy for new crimes or "signal" that probationers are "going bad" in the community (Petersilia & Turner, 1993:312; 1991:635). The assumption that technical violations "signal" new crimes -- called the "signalling" hypothesis for purposes of this research -- lies at the heart of policies that seek to increase public safety by revoking the supervision status of probationers who violate release conditions. In this view, probationers who violate release conditions are presumed to be more likely to commit new crimes during probation supervision than probationers who comply with release conditions. It is also possible, however, that individuals who are charged with a technical violation may be deterred from further misconduct during community supervision.

The "signalling" hypothesis is most readily understood from a generality of deviance perspective, where both criminal and deviant (noncriminal) behavior is posited to emanate from a single underlying propensity or syndrome. Insofar as violations of release conditions are also manifestations of this underlying propensity to offend or deviate, such violations should be useful in distinguishing probationers who present a greater risk of recidivism. A technical violation would therefore serve as a signal by providing criminal justice practitioners with additional information regarding the likelihood of recidivism.

Probation office record data collected as part of an evaluation of intensive supervision programs (ISP) in 10 jurisdictions were analyzed. Approximately 1,000 offenders (one-half of whom were randomly assigned to participate in an ISP program) were followed for one year of community supervision. Two statistical tests were developed to examine the signalling hypothesis.

Test 1 assesses whether knowledge of a technical violation charge predicted arrest with better than chance accuracy during one year of community supervision. Multivariate logistic regression models were used that adjusted for demographic, criminal history, risk and need assessment, supervision intensity, and community activity variables as well as the possibility of confinement during community supervision. Test 2 explores whether technical violations and criminal recidivism are manifestations of the same underlying propensity to offend consistent with a generality of deviance understanding of the signalling hypothesis. To that end, multinomial logistic regression models subject to varying sets of equality restrictions are estimated and compared by means of likelihood ratio tests.

CHAPTER I
PROBATION SUPERVISION

Probation Defined

Probation is the most commonly imposed penal sanction in the United States. Fifty-nine percent of the 4.8 million U.S. adults under some form of criminal justice system control in 1992 were serving a sentence of probation (Snell, 1995:iii). In fact, 1.5% of the entire U.S. adult population was serving a sentence of probation in 1992 (Snell, 1995:iii). Between 1980 and 1992, the probation population grew by 152% (Snell, 1995:5). The growth in the probation population approximates similar increases in jail (142%), prison (166%), and parole (199%) populations over the same period of time (Snell, 1995:iii).

Probation has been defined as "[a] sentence not involving confinement which imposes conditions and retains authority in the sentencing court to modify conditions of sentence or to resentence the offender if the offender violates the conditions" (Allen et al., 1985:36). A sentence of probation typically requires individuals to comply with a set of release conditions during a period of community supervision. Compliance with release conditions (along with lawful behavior) ensures successful discharge from probation supervision.

Failure to comply with a condition of release is defined as a technical violation. Technical violations include all violations of release conditions except criminal recidivism, although strictly speaking criminal recidivism is clearly a violation of probation supervision. Both technical violations and criminal recidivism are grounds for the revocation of probation status. Probation status is revoked by means of a probation revocation hearing. The revocation decision is the responsibility of the sentencing judge (McCarthy & McCarthy, 1991:117). Due to the possibility of substantial loss of liberty, the U.S. Supreme Court in Gagnon v. Scarpelli, 411 U.S. 778, 790 (1973) guaranteed probationers procedural due process rights including a preliminary and final revocation hearing, advanced, written notice of the charges, the opportunity to present evidence and challenge witnesses, and to have legal counsel available as deemed necessary on a case-by-case basis (Allen, 1985: 54-55). The burden of proof in probation revocation hearings is a preponderance of the evidence (Simon, 1993:117).

History and Development of Probation: An Overview

The first probation statute was enacted in 1878 in Massachusetts as an alternative to prison (Champion, 1988:3). Not coincidentally, Massachusetts was the home of John Augustus, a Boston shoemaker and philanthropist, often considered the "father of probation" in the United States. For a period of nearly twenty years beginning in 1841, John Augustus posted bail for individuals he regarded "reformable" (Augustus, 1972:3). If they performed well under his guidance and supervision, they were spared incarceration in the House of

Correction at the time of sentencing.

Foreshadowing modern probation, Augustus writes that " [i]t became generally known that my labors were upon the ground of reform, that I confined my efforts mainly to those who were indicted for their first offence, and whose hearts were not wholly depraved, but gave promise of better things" (Augustus, 1972:19). Other precursors to modern probation included judicial reprieve, the practice of recognizance, and the practice of filing cases (Allen et al., 1985:38-39). By 1938, probation statutes had been enacted in 38 states, the District of Columbia, and the federal system (Champion, 1988:3). A survey of adult probation in 30 states conducted in 1935 revealed that by 1935, 30% of convicted adults were sentenced to probation (Dickey, 1993:138).

The passage of probation statutes largely coincided with the Progressive period in U.S. history (1900-1920) (Rothman, 1983:637). Progressive reformers active in the corrections arena understood crime to be the product of a set of discernable factors (e.g., environmental, psychological) unique to each individual. By and large, the mission of probation practice (and corrections generally) was to identify the source of deviant behavior (via the pre-sentence report) and to "treat" individuals in such a way as to affect a "cure" (O'Leary, 1987:8; Rothman, 1983:638). The "treatment" process necessarily focused on the offender rather than the offense and relied on indeterminate sentencing, discretion, and the promise of positivist science. Dickey (1993:140) contends that surveyors of crime "showed a touching, if naive faith in the efficacy of 'science' -- that is, psychology, psychiatry, and the social sciences -- to solve the crime problem." The theoretical model of rehabilitation held sway through the 1960s (Thomson, 1987:102).

The rehabilitation model was challenged on several fronts in the early 1970s. Neo-classical reformers (proponents of the justice model or "just deserts" models of sentencing) objected to the discretion, disparity, and disproportionality between crime and punishment resulting from individualized sentencing (Rothman, 1988:642). The rehabilitative model was questioned on empirical grounds as well (Rothman, 1983:641-642; Thomson, 1987:102-103). Martinson and his colleagues' widely disseminated meta-analysis of the effect of correctional treatment programs on recidivism first published in 1974 led to the infamous catch-phrase that "nothing works."

Justice model proponents advocated determinate sentencing schemes to achieve more equitable (and shorter) sentences (Rothman, 1983:642). Conservative commentators joined justice model proponents in the call for determinate sentencing. However, conservatives advocated determinate sentencing for the purposes of restraint, invoking the utilitarian strategies of deterrence and incapacitation (Rothman, 1983:644; Thomson, 1987:102). Because neo-classical reformers failed to address crime control concerns, deterrent/incapacitative policies mandating even longer prison sentences dominated sentencing reform (O'Leary, 1987:9). As Rothman (1983:643) writes:

> Theirs was an argument based on equity, not crime control -- and however sympathetic one might wish to be to such a position, it was simply not as appealing as a banner that paraded the prospect of crime reduction.

Thus, witness the emergence of "get tough" sentencing policies of the late 1970s and 1980s (e.g., longer mandatory minimum sentences, more mandatory penalties, multiple "wars" on drugs) (Harland & Rosen, 1987:39-40; Ohlin, 1993:1).

As a consequence of the shift in correctional philosophy (in addition to serious institutional crowding), probation systems adopted incapacitation as a competing strategy of crime control in the 1980s. The "[i]f you can't change people, you certainly can control them" mentality (O'Leary, 1987:9) prompted probation systems to "intensify" routine probation with the development of ISPs and to develop intermediate sanctions administered during community supervision (e.g., electronic monitoring, drug testing, house arrest programs). Such programs focused on short-term risk control "via primarily the incapacitative and specific deterrent means of intensive regulation and monitoring of their whereabouts and conduct, and the corresponding increased threat of detection and strict enforcement of consequences in event of violation" (Harland & Rosen, 1987:34).

While the less than promising results of the early ISP movement (in the early 1960s) should have provided reason for pause (Clear & Hardyman, 1990:44), the prospect of extending the incapacitative effect of institutional confinement to the community <u>at a lesser cost</u> proved irresistible (particularly given the dire need to alleviate prison crowding). According to Tonry (1990:174), the rapid development of ISPs in the 1980s was due to the fact that ISPs served "bureaucratic and organizational goals by enabling probation administrators to be 'tough on crime' and thereby increase the institutional and political credibility of probation."

The advent of the intermediate sanction movement increased the number of release conditions imposed and the intensity of supervision as measured by the number of probation officer contacts, drug tests, record checks, and so forth. While the emphasis on control/incapacitation and punishment is most visible within the ISP context, it has extended to routine probation caseloads as well. According to a 1988 National Institute of Justice Crime File Study Guide:

> probation officers are now directed to be less concerned with the provision of services for offenders (e.g., counseling, employment assistance) and more concerned with drug testing, curfew violations, employment verifications, arrest checks, surveillance, and revocation procedures. (as quoted in Gordon, 1991:143)

A study of probation officer attitude change additionally suggests that probation officers have grown more concerned with control (Harris et al., 1989:245).[1] The authors attributed the increased focus on control to "[r]ising caseloads, increased public awareness of crime (and concern for public protection), concern over prison crowding, and the demise of treatment as an officially mandated objective of probation" (Harris et al., 1989:245).

The interest in control and incapacitation notwithstanding, probation systems have yet to regain their sense of mission (McWilliams, 1987:114).

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[1]Probation officer attitudes were measured using two instruments: (1) the Authority/Assistance Questionnaire; and (2) the Correctional Policy Inventory. The Authority/Assistance Questionnaire was administered in 1974 and then again in 1983. The Correctional Policy Inventory was administered between 1968 and 1970 and then again in 1983.

Control has certainly not replaced rehabilitation. The extant literature is replete with discussion over what the appropriate guiding mission of probation should be (e.g., Rumgay, 1989:177; Petersilia, 1993:65). Further, some question the ability of probation to affect control in the community (Fielding, 1986:183; Thomson, 1987:114). Such skepticism has been borne out empirically by evaluations of ISP programs that reveal that they do not reduce criminal recidivism when compared to routine probation (Petersilia & Turner, 1993).

Documenting the history of probation in England, McWilliams (1987:98) demonstrates the growing role of policy in probation practice. He contends that "the reduction of belief in the treatment model was accompanied by the emergence and speedy development of the idea of policy" (emphasis in the original) (McWilliams, 1987:103). The "organizational machine" created by means of policy was not abandoned with the demise of the rehabilitative ideal; rather it was just "directed towards another purpose," namely the diversion of individuals from imprisonment (McWilliams, 1987:105). Individuals thus came to be viewed only from within the framework of policy and "[t]he 'new,' policy-pursuing probation service was no longer missionary, no longer scientific, and no longer unified" (McWilliams, 1987:105).

The rise of official probation policy in England seems to parallel developments in the U.S. Takagi (1973:315), for example, describes the advent of a managerial school of corrections in California in the 1960s using parole as an example. He writes (1973:315):

> Tasks were routinized and work performances governed by administratively defined criteria called "minimum standards." Supervisors and superiors evaluated subordinates on the basis of these standards, and in the process, organizational <u>efficiency</u> became confused with organizational <u>effectiveness</u>. (emphasis in the original)

Takagi attributes the organization's reliance on rules and regulations to the visibility of corrections (due to the mass media) and the unpredictability of human behavior, which spurs employees to follow rules in order to avoid personal blame.

The emphasis on policy directives and the concomitant deemphasis of the individual is consistent with what Feeley and Simon call the New Penology (Feeley & Simon, 1992:449-450). Feeley and Simon (1992:449) coined the term to describe a fundamental shift away from penal strategies that relate to the transformation of the individual to penal strategies that seek to manage classes of individuals. The new penology largely discards individual-level concepts such as "responsibility, fault, moral sensibility, diagnosis, or intervention and treatment of the individual offender" (characteristic of the old penology) (Feeley & Simon, 1992:452). In its stead, the new penology "seeks to sort and classify, to separate the less from the more dangerous, and to deploy control strategies rationally" (Feeley & Simon, 1992:452).

The discourse has thus changed from "clinical diagnosis" or "retributive judgment" to "probability and risk"; the primary system objectives have changed from recidivism reduction to internal measures of performance under the control of the agency; and more cost-effective techniques to identify, classify, and control have been developed (Feeley & Simon, 1993:450).

To some extent, the new penology coincides with the structuring of discretionary decisionmaking that necessarily comes at the expense of individualization. As Dickey (1993:164) writes: "In dealing with the behavior, the factors and values taken into account are fewer, since individualization and effectiveness have given way to other objectives, such as uniformity, certainty, predictability, and punishment."

In summary, despite the seeming inability of probation to achieve crime control by means of the utilitarian strategies of rehabilitation or incapacitation, it continues to serve an extraordinarily important penological function by providing an alternative to incarceration. Given the importance of this function, the emergence of policy and the bureaucratization of the probation organization has permitted probation practice to continue (likely without substantial changes) in spite of the lack of a guiding mission.

Prevalence of Release Conditions

Routine probation supervision requires compliance with a set of standard release conditions (e.g., steady employment or participation in an educational program, avoidance of certain places/people, mandatory probation office visits, restrictions on travel, and so forth). Research suggests that the imposition of special release conditions (conditions imposed above and beyond the routine set of release conditions) is becoming increasingly common (Byrne & Taxman, 1994:230;

Langan, 1994:791; Parent et al., 1994:5; Petersilia & Deschenes, 1994:322). As Parent et al. (1994:5) write:

> Many practitioners believe that as judges get more sentencing options, they increase the number of conditions they attach to probation terms. Instead of facing a year of standard probation, an offender might now have to perform 200 hours of community service, participate in an outpatient drug-treatment program, and pay $500 in restitution during the year of supervision.

The imposition of an increasing number of conditions is due in large part to the intermediate sanction movement. In order to increase control over offenders, judges and probation departments impose greater behavioral restrictions. In addition, conditions are imposed to increase the punishment value of the probation sanction. According to Byrne and Taxman (1994:230):

> The tendency is to pile on the conditions to make the offenders more accountable. Even traditional probation has been altered in the current "mood swing" as judges embrace "split sentencing" and impose a larger number of special conditions.

A large-scale, three-year followup evaluation of 79,000 felony probationers sentenced to probation in 1986 revealed that 91% of the sample were required to meet at least one special or financial condition (Langan, 1994:791). Supervision fees (32%) were most commonly imposed followed by drug testing (31%), victim restitution (29%), drug treatment (23%), alcohol treatment (14%), community service (12%), counseling (10%), residential placement (5%), intensive supervision (10%), house arrest (1%), and day reporting (1%) (Langan, 1994:791).

Rationale for Release Conditions

Individual conditions are imposed largely in the name of rehabilitation, deterrence, incapacitation and control, and retribution. The goal of the three utilitarian strategies is to reduce the risk of criminal recidivism. Rehabilitation or service-oriented conditions such as drug abuse treatment and education, anger management counseling, literacy training, or educational programs generally seek to engender long-term behavioral change, thereby altering the propensity to recidivate well beyond the intervention period (Harland & Rosen, 1987:39).

Given that control and surveillance is limited to the period of supervision, control or surveillance-oriented conditions necessarily have a more short-term focus. Control-oriented conditions limit the opportunity for offenders to commit crime (by, e.g., curfew and firearm ownership prohibition), decrease probationers' privacy thereby increasing the probability of detecting crime (by, e.g., drug tests, home visits), and simply consume a great deal of probationers' time while under some form of supervision (by, e.g., employment) (Clarke, 1979:417-418). Release conditions are also expected to serve a specific deterrent function by increasing the certainty of detection.

An additional rationale that has come to the fore in recent years with the intermediate sanction movement is retribution or punishment. Punishment-oriented conditions typically involve economic sanctions such as supervision fees, court costs, or restitution. Conditions are also imposed to further the goals of reparation or restoration (by, e.g., victim restitution or community service).

Attaching labels to individual conditions is somewhat misleading, however. First, the imposition of virtually any supervision condition may serve multiple goals. For example, attending an educational program may serve both a rehabilitation and control function. Moreover, such an approach obscures the fact that supervision conditions regardless of their intent constitute punishment. Probation conditions restrict liberty and mandate behavior. As Brilliant (1989:1359) writes: "A fundamental flaw in both scholarly and judicial evaluations of probation conditions is the accepted premise: It's probation, therefore it's not punishment." Clearly, common perceptions of the punitive value of probation stem from a comparison of probation to lengthy prison sentences (Clear & O'Leary, 1983:13).

Offender rankings of community-based sanctions relative to institutional sanctions suggest that many offenders consider selected community-based sanctions to be as punishing as short terms of institutional confinement (Petersilia, 1990:24; Petersilia & Deschenes, 1994:322; Spelman, 1994:121). As one individual who preferred a short period of incarceration to an intermediate sanction stated: "Probation has too many conditions. If you can't meet them, you end up in jail anyway. I'd rather just do the time and pay off my debt to society that way" (Spelman, 1994:126).

Violations of Release Conditions

As corrections systems impose greater numbers of conditions, they increase the opportunity for probationers to violate conditions of release (Petersilia & Turner, 1993:319). Several large-scale evaluations point to significant rates of noncompliance. For example, Langan and Cunnif's three-year follow-up of 79,000 felony probationers revealed that over the course of the three-year followup period, 49% of the sample had a disciplinary hearing for violating a condition of supervision (Langan & Cunnif, 1992:5). Thirty-five percent of those probationers who were eventually incarcerated were confined for committing a technical violation only (Langan & Cunnif, 1992:5).[2]

Clear and his colleagues (1992) examined the records of roughly 7,500 felony and misdemeanant probationers terminated from six probation agencies in five states. Approximately 25% of the sample of 7,500 probationers had committed either a technical violation or new crime during community supervision, for a total of 3,114 infractions. Roughly 50% of the infractions were technical violations (Clear et al., 1992:4).

As with any measure of recidivism that relies on official data, technical violation rates reflect both individual behavior and the behavior of social control agencies. Evaluations of ISP have consistently established that intensive supervision (e.g., more frequent supervisory officer contacts, drug testing)

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[2]Even among those probationers who were discharged from supervision (22%), 49% had not fully complied with special or financial conditions (Langan, 1994:791). Discharged probationers failed most commonly to pay supervision fees (69%) or make restitution payments (40%). Langan attributes this finding to less than rigorous enforcement of such conditions due to inadequate probation resources.

increases the probability of detecting noncompliance (e.g., Petersilia & Turner, 1993:311-312; Tonry, 1990:182; Clear & Hardyman, 1990:44; Cullen, 1996:114).

Petersilia and Turner's (1993) evaluation of ISP in 14 sites is most persuasive given its experimental design (which involved random assignment to ISP or routine supervision). During the course of the one-year followup, 65% of the ISP participants across sites violated release conditions as compared to 38% of the control samples supervised under routine supervision. As a consequence, ISP participants were more likely to be committed to jail or prison during the followup period (27% of the ISP participants as compared to 19% of those individuals placed on routine supervision caseloads).

Variation in Technical Violation Rates

Probation and parole evaluations reveal considerable variation in violation rates across jurisdictions (Clear et al., 1992:4; Maltz, 1984:53; Petersilia & Turner, 1992:19). The Clear et al. (1992) evaluation of probation in six agencies located in five states, for example, provides evidence that violation rates (technical violations and new offenses) vary substantially. The authors attributed such variation to differences in reporting practices (Clear et al., 1992:4). In one jurisdiction, for example, "officers customarily documented violations when they were seriously contemplating revocation, but no regularity of reporting prevailed at other times," while in another jurisdiction, the practice was "to document all known violations in an effort to deter future client misbehaviors" (Clear et al., 1992:4).

The extant literature on probation officer decisionmaking additionally suggests that supervisory officer discretion is considerable and varies within jurisdictions (Clear et al., 1992:2,6). Supervisory officer response to noncompliance is influenced by: (1) organizational policy; and (2) organizational tradition (Clear et al., 1992:5).

Organizational policy refers to the formal, stated policy of an organization while organizational tradition refers to the "workability" of the formal policy, available resources, and relationship with the judiciary (Clear et al., 1992:5). The way in which the "workability" of formal policy relates to individual officer discretion is illustrated below using supervisory review as an example (Clear et al., 1992:6):

> Some officers indicated that they often did not ask for supervisory review when circumstances technically required review because they believed that certain probationers would respond more favorably (i.e., they would be less likely to have additional problems) if responses were limited to interactions between the line officer and the client. Because it was difficult to avoid supervisory review once a violation had been documented in the probationer's file, early exercise of discretion meant the nonreporting of misbehaviors.

In summary, official rates of violation may vary considerably across jurisdictions due to differences in formal organizational policy and tradition and within jurisdictions due to probation officer discretion regardless of the dictates of formal policy.

Probation Revocation and Institutional Crowding

Violations of release conditions affect probation and parole revocation rates which in turn affect institutional crowding (Rhine & Humphries, 1993:101). Probation and parole revocations (due to either technical violations or criminal recidivism) made up nearly 30% of state prison admissions nationwide in 1992 -- up from 17% in 1980 (Snell, 1995:56). In several states, approximately two-thirds of prison admissions were due to probation and parole revocations (during either 1988 or 1989) (Parent et al., 1994:1). An increase in the absolute number of probation revocations springs from two sources: (1) the increasing size of the probation population; and (2) the increasing rate of revocation in some states. A national survey of probation administrators revealed that in about one-third of the states probation administrators believed that revocation rates were increasing (Parent et al., 1994:3).

Probation administrators attributed increasing revocation rates to the following factors: (1) a shift in the mission of probation to punishment and control; (2) an increase in probation caseloads (and the consequent focus on rule violations due to lack of time to do otherwise); (3) an increase in the number of conditions imposed; (4) improved technology for detecting violations; (5) the supervision of more serious offenders (a view not shared by all practitioners); and (6) a shift in educational background of probation and parole officers from social work to criminal justice studies (Parent, 1993:8-9; Parent et al., 1994:4-5).

In addition, within the intermediate sanctions framework, system credibility plays a role in explaining increasing revocation rates. Since intensive supervision is associated with higher technical violations rates, Tonry (1995:396) asserts that "once the discovery [of a technical violation] is made, many program operators believe they must take punitive action -- typically revocation and resentencing to prison -- to maintain the program's credibility in the eyes of judges, the media, and the community."

The extent to which the increase in the revocation rate has been driven by technical violations (as opposed to new crime) has not been systematically documented. Even if thorough records were kept, however, it would still be difficult to accurately distinguish between technical violation and new crime revocations. Revocations for technical violations often mask new criminal activity because practitioners resort to revocation procedures as a "less costly and more effective substitute for criminal prosecution" (Petersilia & Turner 1993:322; see also Gottfredson et al., 1982:280).

Nevertheless, evidence from several jurisdictions indicates that the proportion of revocations due to technical violations has increased in some jurisdictions. For example, the Missouri department of corrections evaluated institutional commitment patterns over a period of ten years[3]. The evaluation revealed that the proportion of probation and parole revocations due to technical violations had increased over the years, accounting for over 50% of the revocations

[3]Although the dates of the evaluation are not documented, the evaluation presumably took place between the early 1980s and 1990s.

(Herman, 1993:45-46). In Cook County, Illinois, the results of three studies completed between 1986 and 1991 suggested that the proportion of technical violators has increased (Lurigio, 1993:90-91). The Mississippi Department of Corrections analyzed revocation patterns between 1983 and 1991. Their review indicated that although the revocation rate had not changed, the percent of probation revocations resulting from technical violations increased from 35% in 1983 to 62% in 1991 (Grubbs, 1993:64). Heuristic review of the six factors cited to explain the increasing revocation rate additionally suggests that an increasing proportion of revocations may be due to technical violations as opposed to the commission of new crimes.

Technical Violations as "Signals" of Criminal Recidivism

> We usually tolerate drug use to a certain extent, then we run out of options....After a certain number of dirty tests we are going to pick up other delinquencies. The guy may be selling his mom's TV, stealing a carton of cigarettes or a bottle of gin from the liquor store. We come under a lot of pressure to do something about that guy. Up until now that has meant sending them back to prison. (Simon, 1993:184)

Criminal justice system practitioners commonly assume that technical violations "signal" that an offender is "going bad" in the community (Petersilia & Turner, 1993:312). Technical violations are hypothesized to be a proxy for the commission of new crimes or serve as a warning that new crimes are imminent (Allen, 1985:90). Assuming that technical violations are indicators of some underlying propensity to reoffend, revocation is often justified in the name of public safety or crime control (Fulton, 1995:25; Petersilia & Turner, 1991:635; 1993:312,322; Stone & Fulton, 1995:124). Such policy is grounded in the "signalling" hypothesis and presumably serves to remove offenders of higher recidivism-risk from community supervision caseloads.

The study of "signalling" as a means of communication has roots in economics. The signalling concept has been applied to the study of job markets and employment decisions (Spence, 1974). Educational level, for example, serves as an important "signal" of productivity in the job market.

"Signals" are observable individual-level characteristics that can be changed (as opposed to unalterable characteristics such as sex or race)[4] (Spence, 1974:107). According to Spence (1974:107), in order for a signal to provide information the following two conditions must be met:

> First, something about the signaler must be unobservable to the receiver of the signals. Whatever this something is, it must affect the way the receiver would prefer to reward or respond to the signaler. And the costs of the signaling activity must be negatively correlated with the unobservable attribute which the receiver values.

The use of signalling in the criminal justice system is not uncommon. Individuals signal to parole boards that they are good risks for release from prison by exhibiting good behavior while in prison and by participating in work or educational programs (Toborg et al., 1991:376). Similarly, by complying with the requirements of a pre-release drug-testing program, individuals signal that they are

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[4]An unalterable, observable characteristic is called an "index" rather than a "signal" (Spence, 1974:10).

willing (and able) to reduce their drug consumption and are therefore less prone to pretrial misconduct than their counterparts who fail to comply with the drug-testing program (Toborg et al., 1991:379). In a similar vein, individuals under probation or parole supervision may signal that they are committed to the community supervision process by complying with the mandates of conditional release. Consider Simon's (1993:188) observations regarding the role of drug testing in parole supervision:

> With little else to go on, many agents recognize adherence to procedure as the best available <u>sign</u> that the parolee is trying to "make his parole" and thus is a worthwhile risk to remain in the community even though a drug test may have been positive. Drug testing provides a regular system of cooperation points, where the parolee can either <u>show</u> his good faith (often phrased as "taking care of business") or demonstrate his lack of commitment to parole compliance. (emphasis added)

According to Spence's definition of an effective signal, signals provide information about an <u>unobservable</u> characteristic of the signaler. Within the criminal justice system context, this unobservable characteristic is invariably related to recidivism-risk. As such, the pertinent question here becomes whether community supervision performance (i.e., compliance with release conditions) provides criminal justice decisionmakers (i.e., receivers of signals) with valuable information regarding an individual's propensity to recidivate.

<u>Policy Implications of the Signalling Hypothesis</u>

The signalling hypothesis is sometimes used to justify probation revocation policies. The revocation of probation status as a result of a technical violation is grounded in part in public safety concerns. However, if probationers who violate the conditions of probation are no more likely to commit new crimes than those in full compliance with conditions of probation, the crime control rationale for incarcerating probation violators must be called into question (at least from an incapacitation perspective). Of course, crime control (by means of incapacitation) is not the only justification for probation revocations. Revocation is justifiable from a retributive perspective particularly within the ISP context where the goal is to provide a more punitive sanction than routine probation. As Tonry (1990:178) writes: "Low tolerance of violation of conditions is the best way to show probationers, prosecutors, and judges that they <u>are</u> tough" (emphasis in original)." As mentioned earlier, revocation in response to technical violations also serves to bolster system credibility. Practitioners additionally observe that probation revocation (in addition to other probation system sanctions) is necessary to deter future violations both specifically and generally and to "encourage" participation in treatment programs (Parent, 1993:10).

Nevertheless, monitoring and enforcing technical conditions takes a great deal of supervisory officers time (Parent et al., 1994:10; Petersilia & Turner, 1993:331). Moreover, as revocation rates increase, the requisite paperwork and procedure consumes even more probation officer time (Parent, 1994:4). Thus, although technical violation revocations are considered less serious than new crime revocations, they "impose a considerable strain on probation and court resources"

(Lurigio et al., 1993, p.91). As Petersilia & Deschenes (1994:324-325) explain:

> Policymakers often stack probation conditions and/or make probation terms longer for the purposes of creating a tough, credible punishment. Courts and the public often perceive that adding such court-imposed conditions (e.g., drug testing) to the sanctions' cost is rather minimal. In truth, each added condition is quite costly, both in terms of monitoring compliance and responding to violations. Generally speaking, the more conditions imposed and monitored, the higher the revocation rates and associated correctional costs. (Petersilia & Turner, 1993)

In short, the revocation of probation status for technical violations is clearly a costly enterprise. Revocation as a response to noncompliance is based in part on assumptions about the crime control value of revocation (i.e., the signalling hypothesis). Such assumptions have not been critically evaluated from either a theoretical or empirical standpoint. As Petersilia and Turner (1993:331) assert: "Despite the policy significance of technical violations, no serious research has focused on this issue." Research that speaks to the relationship between technical violations and criminal recidivism is pertinent to policymakers in developing the most efficacious responses to probationer noncompliance. Due to the cost of revocation and prison crowding, many jurisdictions are in fact currently restructuring their revocation policies (Parent et al., 1994:11-13; see generally Rhine, 1993).

CHAPTER II
THEORETICAL AND EMPIRICAL FOUNDATION OF THE SIGNALLING HYPOTHESIS

Given the broad scope of the signalling thesis -- where the violation of <u>any</u> type of release condition is expected to signal <u>any</u> type of criminal behavior -- the signalling thesis is most consistent with general theories of crime or deviance. By and large, general theories implicate a single causal mechanism or set of causal mechanisms in explaining criminal activity.[5] Given the heterogeneity of criminal behavior and its definitional ambiguity, some commentators doubt the ability of any single theory to account for all types of crime and call instead for more narrowly focused theories (Gibbons, 1994:196). Proponents of more general explanations of crime argue that while criminal acts are characterized by considerable behavioral heterogeneity, the simple fact that they constitute violations of law makes a common explanation possible. As Braithwaite (1989:3) writes:

> The homogeneity presumed between disparate behaviors such as rape and embezzlement in this theory is that they are choices made by the criminal actor in the knowledge that he is defying a criminal proscription which is mutually intelligible to actors in the society as criminal.

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[5]According to Tittle (1985:94), a general theory is "a scheme of highly abstract generality designed to account for an *entire domain* of phenomena such as all individual criminal (or socially disapproved) behavior or all variations in the content of criminal laws (or patterns of social disapproval)" (emphasis in original).

Some "general" theories are more general in scope than others (Tittle, 1985:94). For example, some general theories of crime explicitly call attention to the similarity of criminal behavior to other types of socially problematic, high-risk behavior (i.e., the generality of deviance). Further, while some general theories implicate a series of causal mechanisms in explaining crime (e.g., Braithwaite, 1989), other general theories set forth a more homogeneous explanation (Rowe & Flannery, 1994:375). Theories of the latter genre (e.g., Donovan & Jessor, 1985:891; Gottfredson & Hirschi, 1990; Jessor & Jessor, 1977) advance a generality of deviance approach by hypothesizing that criminal and other problematic or unconventional behavior springs from a single underlying propensity or syndrome[6,7].

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[6]For Gottfredson and Hirschi (1990), the underlying trait driving criminal behavior is low self-control. According to their theory, individuals who possess low self-control (an individual-level characteristic acquired early in life as a result of ineffective parental childrearing practices) are more likely to engage in criminal behavior and "analogous" behaviors (e.g., behaviors that are similarly deviant, exciting, or dangerous such as drug use or reckless driving). The common thread running through all such behaviors is that they provide short-term, immediate pleasure or gratification.

Jessor and Jessor (1977) propose problem-behavior theory to account for behaviors that have been "socially defined as a problem, a source of concern, or as undesirable by the norms of conventional society" among adolescents (e.g., drug use, problem-drinking, delinquency) (Jessor & Jessor, 1977:33). Problem-behavior theory is a social-psychological theory that explains problem-behavior as a function of the personality system, the perceived environment system, and the behavior system (Jessor & Jessor, 1977:19).

Empirical examination of problem-behavior theory led to the hypothesis that problem-behaviors may be manifestations of an underlying problem-behavior "syndrome" (Donovan & Jessor, 1985:891). The problem-behavior syndrome is hypothesized to be related to a dimension of the personality called conventionality-

The signalling hypothesis is most consistent with general theories of deviance, which hypothesize that the same underlying propensity accounts for all violations of the law (as well other types of unconventional behavior) (i.e., the generality of deviance hypothesis). Accordingly, any infraction (regardless of its nature) is viewed as a manifestation of this underlying propensity to offend. Hence, individuals who commit relatively fewer infractions presumably rank lower on a hypothetical continuum measuring the underlying propensity than individuals who commit infractions at higher rates (Britt et al., 1992:63; Goldkamp et al.,

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unconventionality (Jessor & Jessor, 1977:109) as well as to characteristics of the perceived environment (Jessor & Jessor, 1977:125).

[7]Empirical tests of such a generality of deviance hypothesis suggest that while there is considerable support for the thesis that different types of deviance share a common cause, the role of special or unique causes cannot be ruled out. Donovan and Jessor (1985) and Donovan et al. (1988) used maximum likelihood factor analytic methods to study the relationships among several different types of adolescent problem behavior (e.g., drug use, delinquency, sexual promiscuity). Comparison of the observed correlation matrix to a correlation matrix predicted by a one-factor model revealed that a one-factor model adequately accounted for the correlations among the different types of problem behavior. They also found that the study results generalized to a sample of young adults (middle to late 20s) (Donovan & Jessor, 1985:901).

Osgood et al. (1988:82) used structural equation models to examine the covariance of five different types of deviant behavior (criminal behavior, heavy alcohol use, marijuana use, other illicit drug use, and dangerous driving). The researchers used data collected from a nationally representative sample of young people ranging in age from 18 to 22 years (Osgood et al., 1988:85). Because a general, latent variable was not able to explain the totality of reliable variance, Oswood et al. (1988:91) concluded: "Each behavior is, in part, a manifestation of a more general tendency and, in part, a unique phenomenon." Dembo et al. (1992:213) replicated the Osgood et al. (1988) study using data from a sample of youths involved in the juvenile justice system and came to essentially the identical conclusion.

1990:593). Thus, consistent with the signalling hypothesis, knowledge of any type of infraction (e.g., a technical violation) may help to distinguish individuals who are of greater recidivism-risk from individuals who are of lesser recidivism-risk because all such infractions are simply indicators of the same underlying propensity.

Generality of Deviance and a Technical Violation-Specific Approach. For a general theory of deviance such as Gottfredson and Hirschi's or Jessor and Jessor's to provide the theoretical foundation of the signalling thesis, it must first be demonstrated that technical violations are in fact manifestations of the underlying propensity or syndrome to offend. In other words, how similar are technical violations to other risky, unconventional, pleasure-producing behaviors such as crime?

Clearly, all technical violations are violations of court-imposed rules. Technical violations are similar insofar as the violation of such rules jeopardizes community supervision status. Generally speaking, release conditions impose behavioral constraints (e.g., curfews, travel restrictions, prohibition of drug use and alcohol abuse) and mandate employment and participation in counseling, community service or other treatment-related programs.

Given the nature of low self-control (e.g., a "here and now" orientation, difficulty in persisting in a course of action, low toleration for frustration) (Gottfredson & Hirschi, 1990:89-90), persons with low self-control may have more difficulty meeting the requirements of community supervision. First, it may be a more onerous task for individuals characterized by low self-control or an unconventional personality pattern, for example, to forego the pleasurable activities proscribed by release conditions (e.g., drug use). And second, in order to meet release requirements, individuals must exercise a certain amount of forethought or planning. Reporting to the probation office at a specific time, for example, may take hours on public transportation requiring multiple transfers (Simon, 1993:188).

Nonetheless, many technical violations are not intrinsically deviant behaviors in that they do not provoke social censure or disapprobation. Most affect behaviors that persons not under criminal justice system supervision take for granted (e.g., alcohol use, freedom to travel, or possession of a driver's license). Other technical violations, however, are more directly related to the deviant or unconventional behaviors encompassed by such general theories (e.g., drug use, excessive drinking). Thus, insofar as some types of technical violations more closely resemble the deviant acts implicated by such general theories of crime and deviance, they may be more likely to serve as signals of criminal recidivism.

Social Control Theory and the Signalling Hypothesis. The signalling hypothesis is also consistent with a theory of social control (e.g., Hirschi, 1969; Toby, 1957; Reiss, 1951). Social control theorists do not attempt to explain motivation to commit crime (for they assume humans to be naturally deviant), but seek to explain conformity (Hirschi, 1969:10). In a relatively recent formulation of social control theory (Hirschi, 1969), conformity is explained by the strength of an individual's social bond.[8] Individuals who possess a strong social bond (i.e., who have a greater stake in conformity) are hypothesized to be less likely to violate the law.

Such informal social control processes have been linked to formal social control processes (e.g., Briar & Piliavin, 1965:39,42; Sherman, 1990:159; Williams & Hawkins, 1986:562). Formal controls (e.g., perceptions of the threat of legal punishments) are hypothesized to affect behavior through the anticipated impact on informal social controls (e.g., attachment to significant others). If formal controls interact with informal controls in this manner, individuals who possess stronger informal ties to conventional society may be more deterred by the prospect of criminal punishment than individuals whose ties to conventional society are relatively weak (Sherman, 1990:161).

Technical violations may distinguish individuals who possess a relatively stronger social bond from individuals who are more weakly bonded to conventional society (Petersilia, 1993:322). Technical violations -- though not necessarily deviant in and of themselves -- are violations of court-imposed rules. Supervisory officers respond to technical violations with a range of sanctions including the revocation of the supervision term. Accordingly, individuals who are more strongly bonded to society (or more firmly embedded in a conventional life-style)

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[8]Hirschi (1969:16-27) delineates four elements of the social bond: (1) emotional attachment to significant others; (2) strength of moral beliefs; (3) involvement in conventional activities; and (4) commitment to (or investment in) a conventional life-style.

relative to other probationers may be more likely to be deterred by the threat of sanctions and thus less likely to commit both technical violations and violations of law. Thus, consistent with the "signalling" hypothesis, individuals who comply with release conditions may signal that they are more committed to a conventional life-style (as well as the supervision process) and thereby more restrained from committing other criminal offenses.

Alternatives to the Signalling Hypothesis. While the signalling hypothesis may seem to resonate with general explanations of crime and deviance, it is but one possible explanation of the nature of the relationship between technical violations and criminal recidivism. It is certainly plausible, for example, that rather than being driven by the same underlying causal process, the factors that explain technical violations differ from the factors that explain criminal recidivism. In this event, separate theories would be required to explain each recidivism outcome and the presence of either event would not necessarily have ramifications for the other. Some individuals may simply be more inclined to violate release conditions while other individuals may be more inclined to violate the law. Hence, as Petersilia (1994:171) notes, the commission of technical violations need not necessarily imply the commission of criminal offenses.

Such a specialization argument[9] is consistent with the rational choice perspective (e.g., Cornish & Clarke, 1986:2), which adopts a crime-specific

[9]Offense specialization refers to the tendency to repeat a specific criminal act or pattern of criminal acts.

approach[10] and the criminal career perspective (Blumstein et al., 1986:13), which focuses attention on elements of the criminal career (e.g., frequency, participation, onset, career length, specialization, escalation). As Blumstein et al. (1988a:4) contend:

> different sets of "causes" may influence individuals' initiation of criminal activity, the frequency with which they commit crimes, the types of crimes they commit, and their termination of criminal activity.

Evidence regarding offender specialization is mixed. While the extant literature generally does not suggest that individuals (particularly juveniles) specialize in particular types of crime (i.e., robber as opposed to burglar) (e.g., Blumstein & Cohen, 1979:581; Klein, 1984:191; Petersilia, 1980:352; Wolfgang et al., 1972:254), there is some evidence to suggest that offenders tend to specialize in specific classes or "clusters" of crime (e.g., violent versus property offenses) (Blumstein et al., 1988b:341-342; Brennan et al., 1989:449; Cohen, 1986:397; Kempf, 1986:198; Spelman, 1994:109; Stander et al., 1989:329). Lattimore et al. (1994:293) describe offense clusters as follows:

> Offense clusters exist when there is a significantly greater preference to switch among offense types <u>within</u> a cluster (say, theft and burglary) and a decreased preference to switch to offenses <u>outside</u> the cluster (assault and weapons). (emphasis in original)

Blumstein et al.'s (1988b:341-342) study of adult arrest patterns, for example, revealed some evidence of specialization in drug, fraud, and auto theft

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[10]Note however that the rational choice perspective does not require offender specialization (Cornish & Clarke, 1986:12).

offenses.[11] In addition, the study identified two "clusters" of offense types (violent offenses and property offenses)[12] suggesting that offenders tended to commit either violent or property offenses. Similarly, Spelman's (1994:109) reanalysis of the RAND self-report inmate survey indicated that two-thirds of active adult offenders committed either exclusively property offenses or exclusively personal offenses. Recently, Lattimore et al. (1994:315) extended such findings to a sample of youth paroled by the California Youth Authority using official arrest data.[13] The authors concluded that offense patterns were not entirely random and hence more generally consistent with a tendency to specialize. Knowledge of prior and current offense type, for example, helped to predict the subsequent offense type (Lattimore et al., 1994:314-315).

Offender specialization has clear theoretical implications -- general theories

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[11]The results varied slightly by race such that white offenders tended to specialize more in drug and fraud offenses while black offenders tended to specialize more in auto theft.

[12]The violent offense cluster consisted of murder, rape, aggravated assault, and weapons offenses. The property offense cluster included burglary, larceny, auto theft, and fraud.

[13]The researchers used arrests that occurred prior to the commitment offense and arrests that occurred subsequent to release thereby including both juvenile and adult arrests in the analyses. Offense type was categorized as follows: (1) violence (homicide, assault, rape, weapons, and kidnapping); (2) robbery (including attempted robbery); (3) burglary (including attempted burglary); (4) other property (grand theft, auto theft, possession and sale of drugs, and arson); and (5) delinquency (miscellaneous assault, petty theft, receiving stolen property, statutory rape, contributing to delinquency of a minor, under the influence of drugs or alcohol, escape, miscellaneous felonies or misdemeanors, and welfare and institutional offenses).

of crime suggest versatility in offending patterns while more specific theories tend to implicate offender specialization. As Stander et al. (1989:325) write:

> If there were specialization, offenders would have different and relatively stable potentials on more than one theoretical construct; for example, one person might have a consistently high potential on construct A and a consistently low potential on construct B, while another might have a consistently low potential on construct A and a consistently high potential on construct B.

The tension between general and specific theories (and between versatility and specialization) has implications for the signalling hypothesis as well. If a crime-specific approach is required to explain crime (disparate acts proscribed by law), it follows that no one single explanation of crime will be able to simultaneously explain technical violations (acts proscribed by authority of the court). Some types of crime may be more or less similar to some types of technical violations (e.g., drug-related crimes and drug-related violations), but given their largely disparate causes, knowledge that an individual violated a release condition will likely not be particularly useful in predicting whether the same individual will commit a crime.

Summary. The signalling hypothesis suggests that individuals who violate the conditions of release distinguish themselves from "compliant" individuals by communicating that they pose a greater risk of criminal recidivism. Technical violations thereby provide community supervision practitioners with information regarding an unobservable characteristic of the individual -- the propensity to recidivate during community supervision.

The signalling hypothesis is consistent with general theories of crime or deviance that posit that a single underlying propensity or syndrome drives criminal behavior and other high-risk, unconventional behaviors. To the extent that technical violations spring from the same underlying propensity to offend, they serve as a measure of this underlying construct. Individuals who commit deviant acts at higher frequencies rank higher on a hypothetical continuum measuring recidivism propensity (i.e., the underlying propensity or syndrome) and thus are more likely to reoffend than individuals who rank lower on such a continuum. Since both technical violations and criminal behavior are manifestations of the underlying propensity to offend (and are hence essentially interchangeable), technical violations may be used as evidence that probationers present a greater risk of criminal recidivism. Alternatively, within the social control rubric, technical violations may help to identify individuals whose bond to conventional society is relatively more fragile and who, as a consequence, may be more likely to offend. On the other hand, it is also possible that distinct causal mechanisms drive specific types of technical violations and criminal offending thereby raising questions about the efficacy of the signalling hypothesis.

<u>Empirical Evidence Related to the Signalling Hypothesis</u>

Petersilia and Turner (1993:312) investigated the relationship between technical violations and criminal recidivism using data from a 14-site evaluation of ISP. First, the researchers computed a correlation coefficient between the total number of technical violations and the total number of arrests observed for each study participant in five sites (three probation jurisdictions in California and two parole jurisdictions in Texas) during the one-year followup period. Petersilia and Turner's (1991:635-636; 1993:312; 1990:73-75) examination was premised on the dual assumptions that technical violations signal the commission of new crimes and that aggressive sanctions in response to technical violations (namely, incarceration) therefore increase public safety. Hence, Petersilia and Turner anticipated negative correlations between technical violation charges and new arrests because they expected criminal justice sanctions (in response to technical violations) to suppress arrests.[14]

The absence of significant negative correlations led them to conclude that there was "no support for the argument that violating offenders on technical conditions suppressed new criminal arrests" (Petersilia & Turner, 1993:312). It is important to note that Petersilia and Turner did not take the temporal order of recidivism events (i.e., technical violation charges and arrests) into account. In other words, technical violation charges did not necessarily precede arrests.

Petersilia and Turner also attempted to ascertain whether drug-related technical violations were related to arrests during probation supervision in the three California sites involved in the ISP evaluation. Cross tabulations suggested that offenders who had drug-related violations were no more likely to be arrested than

[14]Petersilia and Turner assessed whether criminal justice system responses to technical violation incidents suppressed new arrests without regard to the specific causal mechanism involved, e.g., incapacitation, specific deterrence.

offenders who did not have drug-related violations (Petersilia & Turner, 1991:636; Petersilia & Turner, 1990:74-75).

In an analogous line of research in Washington, D.C., the relationship between compliance to the requirements of a pretrial urine testing program and performance on pretrial release was investigated (Toborg et al., 1989:12; Toborg et al., 1991:369; Visher, 1990:323).[15] Specifically, the researchers assessed whether compliance with the requirements of the drug monitoring program "signalled" success during pretrial release. The results of the Toborg et al. evaluation suggested that individuals who met the requirements of the program were significantly less likely to be arrested or to fail to appear for court, controlling for traditional risk-factors (Toborg et al., 1991:378).[16]

Visher (1990:329-330) reanalyzed the same data using a different measure of program compliance and came to essentially the same conclusion.[17] Visher's (1990:329) analysis revealed the following:

Those who did comply by showing up for weekly urine tests had fewer

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[15]The major differences between pretrial release supervision and probation/parole supervision include the following: (1) individuals involved in the pretrial release programs have merely been accused of committing a crime (rather than convicted); and (2) decisions regarding participation in pretrial release programs must be made in a short period of time with little information available upon which to base decisions (Wish et al., 1988:10).

[16]Toborg et al. (1991) measured program compliance as follows: (1) submitting to 3 or more drug tests subsequent to arrest; and (2) testing negative or "clean."

[17]Visher (1990) used the first drug test result administered as part of the drug monitoring program as a measure of program compliance.

rearrests and FTAs [failure-to-appears] than those who did not: about 56 percent of those who failed to show up for the first post-release urine test were either rearrested or missed court appearances compared to about 22 percent of those who showed up and tested negative and about 39 percent of those who tested positive. (emphasis in original)

In addition, multivariate analyses predicting arrest suggested that knowledge of program compliance predicted arrest, after adjusting for the effects of other risk factors (e.g., criminal history).[18]

Performance in the drug monitoring program therefore proved to be an effective post-arrest "signal" of misconduct (i.e., either arrest or failure-to-appear) during pretrial release (among individuals who had tested positive for drugs at the time of arrest).[19] As Toborg et al. (1991:379) write: "By continuing to appear for urine testing, defendants signal that they pose low risks of pretrial misconduct and by testing clean they demonstrate even lower probability of failure-to-

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[18]Models predicting failure-to-appear were not estimated due to the lack of theoretically relevant variables (i.e., community ties, number of prior failure-to-appears).

[19]It is possible that individuals who complied with program requirements were deterred from using drugs (and concomitantly other forms of pretrial misconduct) by the threat of program sanctions. Research bearing directly on this point does not support this assertion, however. Britt et al. (1992) randomly assigned pretrial releasees to either an experimental group involving a drug monitoring program or a control group that did not participate in the drug monitoring program in two jurisdictions in Arizona. The research revealed that participation in the drug monitoring program during pretrial release did not reduce the level of pretrial misconduct (Britt et al., 1992:76). Similarly, Goldkamp and Jones (1992:459) found that drug-monitoring did not reduce failure-to-appear or arrest rates using an experimental design in two jurisdictions.

appear."[20] However, as Wish (1988:20) points out it is not possible to establish whether the signalling effect stems from the actual urine testing itself, the reporting requirements, or both. Hence, it is possible that other types of pretrial requirements (e.g., electronic monitoring) may serve as equally effective signalling devices.

Drug use at the time of arrest (as opposed to <u>during</u> pretrial supervision) may also be considered a potential signal of pretrial misconduct. Related research investigated the relationship between positive drug tests taken at arrest and pretrial misconduct (e.g., Goldkamp et al., 1990; Smith et al., 1989; Toborg et al., 1991). The primary intent of such research was to determine whether knowledge of drug test results had the potential to improve pretrial release decisions. Multivariate analyses were conducted to assess whether individuals who tested positive for drugs at arrest were more likely to be involved in pretrial misconduct (i.e., arrest or failure-to-appear) over and above the effect of traditional predictors of risk (e.g., criminal history, community ties).

The Smith et al. (1989) evaluation of pretrial release performance in New York City suggested that individuals who tested positive for cocaine or heroin had a higher probability of failure-to-appear (controlling for other risk-factors) while

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[20]Individuals who reported for urine testing (regardless of whether they tested clean) were significantly less likely to be arrested or to fail-to-appear. Individuals who reported for urine testing *and* tested clean were even less likely to fail-to-appear. However, testing clean did not significantly influence the probability of arrest.

individuals who tested positive for PCP had a higher probability of arrest.[21] However, the authors cautioned that while the effects of positive drug tests on pretrial misconduct were statistically significant, the substantive import of the effects was debatable. Findings from the Toborg et al. (1991) evaluation of a urine testing program in Washington, D.C. were similar to the Smith (1989) results. Testing positive for PCP or a combination of three or more drugs at arrest increased the probability of arrest while a positive test for either cocaine or an opiate, or a combination of cocaine and an opiate increased the probability of failure-to-appear (Toborg et al., 1991:374). Goldkamp et al. (1990) also examined the incremental effect of drug testing results in predicting pretrial misconduct using data collected in Dade County, Florida. In contrast to the New York City and Washington, D.C. findings, positive drug tests did not predict failure-to-appear, in multivariate analyses controlling for other relevant risk factors. However, testing positive for marijuana or cocaine increased the probability of arrest during pretrial release.

In order to synthesize the results from individual studies that relied on different analytic techniques, Rhodes et al. (1996) reanalyzed eight data sets that contain drug test and pretrial misconduct information. Rhodes et al. (1996:319) standardized the analyses by using a common statistical model and by introducing a

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[21]Smith et al. (1989) controlled for selection bias by estimating a bivariate probit model. Selection bias occurs at the pretrial release stage because individuals who have the highest probability of pretrial misconduct are not released prior to trial.

common set of control variables. Arrest and failure-to-appear served as measures of pretrial misconduct. The results of their analyses cast doubt on the ability of urine tests in general to distinguish individuals who are more likely to engage in either form of pretrial misconduct. The research revealed that a positive test for heroin was the only drug that consistently predicted arrest across jurisdictions, while a positive drug test for cocaine consistently predicted failure-to-appear (Rhodes et al., 1996:333). Criminal history was the most important predictor of pretrial misconduct. Rhodes et al. (1996:341) suggested that urine testing may be inadequate to identify defendants at high-risk of pretrial misconduct because such tests fail to provide a measure of the intensity of prior drug use.

Summary. The absence of a significant bivariate relationship between technical violation charges and criminal recidivism led Petersilia and Turner (1993:312) to conclude that technical violations as a class do not signal criminal recidivism during post-conviction supervised release. Examination of compliance to a drug monitoring program during pretrial release, however, revealed that compliance with the mandates of a pretrial drug monitoring program was significantly related to failure-to-appear and arrest during pretrial release (Toborg et al., 1989:13; Toborg et al., 1991:376; Visher, 1990:329). Thus, performance in the drug monitoring program appeared to "signal" misconduct during pretrial release.

While findings from individual studies investigating the significance of positive drug tests at arrest (Goldkamp, 1990; Smith, 1989; Toborg et al., 1991) suggested that knowledge of a positive drug test helped to predict the probability of failure-to-appear and arrest (net the effect of other risk factors)[22], reanalysis of the results suggests that the use of drug testing to predict pretrial misconduct may be more limited (Rhodes et al., 1996:340). Performance in a drug monitoring program may be distinguished from a positive drug test at arrest because performance in a drug monitoring program may provide more information about sustained drug use patterns (i.e., the intensity of drug use) (Rhodes et al., 1996:344). However, it is not clear whether this finding extends to non-drug-related violations as well.

[22]Note that the Goldkamp et al. (1990) study finds that while knowledge of a positive drug test does increase the probability of arrest, it does not increase the probability of failure-to-appear.

CHAPTER III
STATEMENT OF THE PROBLEM

The relationship between technical violation charges and criminal recidivism during probation supervision has not been the subject of extensive study. Conventional wisdom holds that technical violations "signal" the commission of new crimes during supervised release. The signalling hypothesis in turn informs probation revocation guidelines. The critical question from a policy perspective then becomes whether revocation policies protect public safety as intended by revoking the community supervision status of probationers who violate release conditions.

The intent of the present research is to examine whether technical violation charges do in fact signal criminal recidivism. The signalling hypothesis is assessed directly by testing whether technical violation charges predict arrest during community supervision. Since the signalling hypothesis is most easily understood from a generality of deviance framework, the research also explores whether technical violation charges and criminal recidivism are manifestations of the same underlying propensity or syndrome. An overview of the two empirical tests devised to evaluate the signalling hypothesis follows.

Test 1. In order to test whether technical violation charges signal arrest, multivariate logistic regression models predicting arrest are estimated using indicators of technical violation charges as explanatory variables. Other variables of theoretical relevance to the explanation of recidivism are also included in the models. The technical violation indicator(s) would be expected to exert a positive and significant effect on arrest if the signalling hypothesis is correct, suggesting that individuals who are charged with a technical violation have a higher probability of arrest. Otherwise, the empirical validity of the signalling hypothesis would be called into question.

Other variables included in the multivariate recidivism analysis (e.g., demographic, criminal history, education, employment, drug and alcohol abuse measures) presumably tap the underlying propensity to deviate as well. Because a range of different variables are included in the analysis, the effect of technical violation charges on arrest is likely to be moderate. Nevertheless, in order to serve as an effective "signal," technical violation charges should exert a positive and statistically significant effect on arrest (even after adjusting for the effects of other explanatory variables).

A technical-violation specific approach is also adopted by disaggregating the general technical violation measure into either four or five categories of technical violation charge types. This analysis examines whether certain types of technical violation charges are more likely to "signal" arrest than other types of technical violation charges. Some types of technical violation charges may be more similar to the deviant or unconventional behaviors implicated by theories that advance a generality of deviance hypothesis (e.g., Gottfredson & Hirschi, 1990). Multinomial logistic regression models are also estimated to examine whether the effect of technical violation charges on arrest varies by arrest type.

The present approach extends Petersilia and Turner's examination of the relationship between technical violation charges and arrest using the same data in several ways. First, instead of allowing criminal justice system sanctions to mediate the relationship between technical violation charges and arrests, the present analysis controls for the criminal justice sanction expected to protect public safety (incarceration) by controlling for confinement (as a response to a technical violation charge). Attention is therefore focused squarely on the base assumption: whether technical violations do in fact signal new crimes or serve as indicators of an underlying propensity to offend. Second, the present research expands Petersilia and Turner's work by explicitly taking the temporal order of recidivism events into account. That is, in order for technical violation charges to signal arrest, they must precede the arrest. Third, the research adjusts for offender, supervision, and site characteristics in ten of the fourteen sites involved in the ISP evaluation (seven of which have not been previously used by Petersilia & Turner to examine the relationship between technical violation charges and arrest).

<u>Test 2</u>. Test 2 assesses whether technical violation charges and new arrests appear to be manifestations of the same underlying propensity or syndrome as a generality of deviance explanation of the signalling hypothesis suggests. The

underlying propensity has been dubbed Recidivism Propensity for purposes of the analysis.

Multinomial logistic regression models predicting a four-category dependent variable consisting of mutually exclusive recidivism outcomes are estimated. The fit of multinomial regression models subject to different combinations of equality restrictions is systematically compared. The critical test involves the comparison of an unrestricted model (where all explanatory variables are permitted to vary across recidivism categories) to a restricted model (where all explanatory variables related to Recidivism Propensity are restricted to be equal across recidivism categories).[23] The test assumes that if different types of recidivism outcomes are manifestations of the same underlying propensity (i.e., Recidivism Propensity), one set of parameter estimates related to Recidivism Propensity should suffice in explaining recidivism.

A likelihood ratio test is computed to determine whether the restricted model differs significantly from the unrestricted model. A significant test statistic would suggest that technical violation charges and arrest are not indicators of the same underlying propensity because the explanatory variables in the model exert different effects across categories of the outcome variable. The global test is

[23]Equality restrictions constrain the coefficients of a particular explanatory variable to be the same across categories of the dependent variable. For example, an explanatory variable such as age would be constrained to exert the same effect on each category of the dependent variable (e.g., arrest only, technical violation only).

supplemented with an exploratory analysis investigating whether individual explanatory variables exert differential effects across categories of the outcome variable.

In summary, the research evaluates a common assumption regarding the relationship between technical violations and criminal recidivism. The signalling hypothesis is put to test directly by examining whether technical violations predict arrest during one year of community supervision. In addition, an indirect exploration of the signalling hypothesis assesses whether technical violation charges and arrests appear to be indicators of the same underlying propensity or trait. Empirical examination of the signalling hypothesis is critical to the development of informed probation revocation policies.

CHAPTER IV
METHOD

Subjects

The present research analyzed data collected as part of the Bureau of Justice Assistance (BJA)/National Institute of Justice (NIJ) sponsored evaluation of ISP programs conducted by the RAND Corporation (and directed by Joan Petersilia and Susan Turner). Approximately 2,000 offenders participated in the project. Offenders were randomly assigned to either the experimental group (ISP program) or the control group (routine probation/parole) and followed for one-year of community supervision.

The data set was selected because it provided measures of the key constructs necessary to test the "signalling" hypothesis (e.g., technical violation charges and arrest incidents) during community supervision. Additionally, the data permitted examination of the "signalling" hypothesis in a diverse collection of jurisdictions throughout the nation.

RAND ISP Evaluation. In 1986, BJA selected fourteen jurisdictions to participate in the evaluation. Each jurisdiction (with two exceptions) received between $100,000 and $150,000 in funding from BJA to run an ISP program for 18 to 24 months (Petersilia & Turner, 1993:292). NIJ sponsored the ISP evaluation conducted by the RAND Corporation in all sites. Participating sites began data collection in February, 1987. Data collection continued through January, 1990 (Petersilia & Turner, 1990:5).

In exchange for the BJA funding, participating jurisdictions agreed to develop an ISP program for adult offenders modeled after Georgia's ISP[24] that excluded offenders convicted of serious violent offenses such as homicide, robbery, or rape. Further, program administrators were required to attend technical assistance and training programs and to participate in the RAND evaluation of ISP. As part of the RAND evaluation, program administrators were required to collect data and assist RAND researchers in executing the random assignment of cases to either ISP or routine supervision. In each jurisdiction, all incoming probationers and parolees who met the eligibility requirements of the ISP program were randomly assigned to either the ISP sample or the control sample. The control sample was supervised on routine probation or parole caseloads.

The fourteen jurisdictions selected to participate in the demonstration project spanned the nation. Jurisdictions included three jurisdictions in California (Contra Costa, Los Angeles, and Ventura); three jurisdictions in Georgia (Atlanta, Macon, and Waycross); two jurisdictions in Texas (Dallas and Houston); Seattle, Washington; Marion County, Oregon; Des Moines, Iowa; Milwaukee, Wisconsin;

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[24]Georgia's ISP program incorporated small caseloads, weekly contacts, drug testing, curfews, community service, and employment training (Petersilia, 1993:292).

Winchester, Virginia; and Santa Fe, New Mexico. The three California jurisdictions and the Oregon jurisdiction were operated at the county-level, while the other jurisdictions were operated at the state-level.

Although each jurisdiction was required to meet the general BJA specifications, jurisdictions were encouraged to design programs to meet the particular needs of their own jurisdiction (e.g., the risk and needs of offenders, financial resources, organizational and political contexts) (Petersilia & Turner, 1990:vi). Accordingly, three variations of ISP that incorporated the core components of ISP programs stipulated by BJA were implemented. Two sites (Marion County, Oregon and Milwaukee, Wisconsin) implemented prison diversion ISP programs. Two sites (Dallas and Houston, Texas) implemented parole enhancement programs. Three sites (Santa Fe, New Mexico, Des Moines, Iowa, and Winchester, Virginia) implemented a combination probation and parole enhancement program. The remaining 7 sites developed ISP programs designed exclusively for probation caseloads.

Site Selection. Ten of the fourteen sites were included in the present study. Sites that operated probation enhancement ISPs or a combination of probation/parole enhancement ISPs were selected. The two sites that operated as prison diversion ISP programs (Marion County, Oregon and Milwaukee, Wisconsin) and the two sites that operated exclusively as parole enhancement ISP programs (Dallas and Houston, Texas) were excluded from the study. The study was limited to probation enhancement (and probation/parole enhancement) ISP programs because they shared a similar purpose (i.e., providing a more stringent form of community supervision to probationers/parolees of higher recidivism risk). In addition, limiting participation to primarily probation enhancement programs was intended to maximize the similarity of offender populations across sites.[25]

Site Characteristics. The ten ISP programs selected for inclusion in the present study were designed to target similar types of offenders. Eight jurisdictions specifically targeted individuals who had been convicted of drug-related offenses or had a history of drug use or dependency (Petersilia & Turner, 1993:293). Two jurisdictions designed programs for high-risk probationers. Seven of these sites collected data related to drug testing. See Tables 1 through 4 for an overview of program characteristics. Petersilia and Turner (1990) provide a more extensive discussion of the three California sites.

Participant Characteristics. The demographic characteristics of probationers selected to participate in the ISP evaluation across sites were similar with a few exceptions (see Tables 1-4). The majority of study participants in each jurisdiction were males between the ages of 26 and 30 years. Between 28% and 48% of the study participants had graduated from high school or had earned their GED. The percentage of participants who were married ranged from 9% to 28%. The race and ethnicity of participants varied across jurisdictions. In three jurisdictions, for

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[25]Note that in two of the three combined probation/parole ISP programs, the sample consisted primarily of probationers. In Winchester, Virginia, 74% of the sample were probationers and in Santa Fe, New Mexico, 84% were probationers. However, in Des Moines, Iowa, 73% of the sample consisted of parolees.

example, over three-quarters of the participants were Black, in three jurisdictions approximately two-thirds of the participants were White, and in one jurisdiction the vast majority of participants were Hispanic.

Participants in the study were convicted most commonly of either a drug-related offense or a property offense. Between 4% and 27% of the participants across sites had been convicted of a violent offense (defined as homicide, rape, assault, and robbery). In the three Georgia sites and the Virginia and New Mexico sites, a substantial percentage of participants had been classified as committing an offense falling in the "other" category. The overwhelming majority of these individuals had had their probation or parole status revoked.

Study participants had been arrested between three and nine times on average. Over three-quarters of the individuals in eight of the ten sites had not previously served a prison term. A greater percentage of participants in Des Moines and Santa Fe (two of the sites that operated a joint probation and parole enhancement ISP) had served time in prison.

Procedure

Data were collected from probation and parole files by agency staff employed at each of the sites. Senior staff participated in training sessions intended to familiarize them with the research design and data collection instruments (Petersilia & Turner, 1990:36).

While RAND researchers would have preferred to have outside researchers collect the data due to the possibility of bias, limited resources forced them to rely on agency personnel. RAND researchers conducted validity checks at many of the sites and concluded that the data were correctly and consistently coded (Petersilia & Turner, 1993:300).

Data Collection Instruments. RAND researchers developed standard data collection forms to facilitate comparisons across sites. Probation and parole files (e.g., presentence investigation reports or chronological supervisory officer notes) were the primary data source. Three data collection instruments were completed per study participant (Background Assessment Instrument, Six-Month Followup Instrument, and Twelve-Month Followup Instrument). Each instrument took approximately one hour to complete. The data collection instruments were completed on-site and mailed to the RAND Corporation for analysis.

The Background Assessment Instrument was completed at the beginning of the probation term. It contained demographic data (e.g., age, race and ethnicity, sex, education, marital status, number of dependents); criminal history data (e.g., number of prior arrests, date of first arrest, data of first conviction, number of prior misdemeanor convictions, number of prior felony convictions); current offense information; and a risk and need assessment. Seven sites (Atlanta, Macon, Waycross, Des Moines, Santa Fe, Winchester, and Seattle) additionally collected data on drug abuse (e.g., evidence of drug dependency by drug type, age at first use).

The Six- and Twelve-Month Followup instruments measured the intensity of probation/parole supervision and the degree to which individuals complied with the conditions of release. The Six-Month Followup was completed at the end of six months of community supervision and the Twelve-Month Followup was completed at the end of twelve months of community supervision. Each study participant was followed for one year of community supervision.

Supervision intensity was measured on a monthly basis. Measures of supervision intensity included the number of personal contacts, the number of telephone contacts, the number of monitoring checks, and the number of drug and alcohol tests taken. Measures of supervision compliance included whether an individual was arrested, convicted, and/or charged with a technical violation during each six-month followup period. The date of each such infraction was recorded along with an identifying code and resulting sanction or sentence.[26] The Six- and Twelve-Month Followup instruments also recorded how many days per month an individual had engaged in paid employment or training, how many counseling sessions an individual had attended per month, how many hours of community service had been performed, and how many dollars of restitution, fines and court costs, and probation fees had been paid.

Seven sites (Atlanta, Macon, Waycross, Des Moines, Santa Fe, Winchester,

[26]In order to protect the confidentiality of study participants, dates of technical violation charges and arrests were not released in the ICPSR public data set. Instead RAND researchers calculated the number of days between the start of probation supervision and the date of each type of recidivism incident and released the resulting duration.

and Seattle) also collected more detailed drug testing data as part of the Six-and Twelve-Month Followup instruments. The number of drug tests ordered, the number of drug tests taken, and the number of positive drug tests were collected on a monthly basis.

A Status Calendar was completed as part of the Six- and Twelve-Month Followup instruments. The status calendar was intended to keep track of changes in an individual's community supervision status. The calendar was used to record the date and type of community supervision status change (i.e., whether an individual was jailed, assigned to a different supervision level, transferred, or had absconded, etc.).[27]

Measures Used in Test 1

The purpose of Test 1 was to provide a direct examination of the "signalling" hypothesis. Measures of technical violation charges and criminal recidivism during community supervision were therefore critical to the analysis. In order to test whether technical violation charges "signaled" criminal recidivism, it was also vital to establish the temporal order of technical violation charges and

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[27]Due to confidentiality concerns, the dates of changes in supervision status were also not available in the ICPSR public data set. RAND researchers released the total number of days spent in each possible supervision outcome during each followup period. Thus, it was possible to determine whether an individual was detained in jail and for how many days during each 6-month followup period. It was not possible, however, to determine exactly when the individual was detained relative to other community supervision events.

arrest. Clearly, in order for a technical violation to signal arrest, it must precede arrest. Since study participants were sometimes arrested multiple times and charged with several technical violations during the one-year followup period, the present study focused exclusively on the first arrest incident and the first technical violation charge.

Criminal Recidivism. Criminal recidivism during the one-year followup was operationalized using a binary indicator of arrest (where one indicates that an individual was arrested at least one time during the follow-up period) (see Table 5). Following Maltz (1984:58), arrest (as opposed to conviction) was selected as a more valid indicator of criminal activity during community supervision. Generally speaking, arrest is considered a better indicator of actual behavior than conviction. In view of the relatively short follow-up period, it would have also been difficult to use the conviction indicator because the conviction status of a substantial percentage of cases was still pending at the end of the data collection period.

Arrest was also represented as a five-category measure of arrest type (where zero equals no arrest). Non-zero values represent four different categories of arrest, including: (1) person arrests; (2) property arrests; (3) drug arrests; and (4) "other" arrests.[28] Due to the crude categorization of arrest incidents, offense

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[28]Person arrests include: homicide, forcible rape, robbery (armed and strong arm), aggravated assault, other assault, and other sex offenses. Property arrests include: burglary, larceny-theft, motor-vehicle theft, arson, forgery/counterfeiting/fraud/embezzlement, receiving stolen property, carrying and possessing weapons, vandalism, and other property offenses. Drug arrests include: possession of narcotics and controlled non-narcotics, sale/transportation of

type categories were not ordered according to offense seriousness. The five-category arrest variable corresponded to the first arrest during probation supervision.[29]

Technical Violation Charges. Technical violation charges were measured as a binary variable (where one equals the presence of one or more technical violation charges) (see Table 5). In order to construct the technical violation charge measure, it was necessary to determine whether individuals were charged with a technical violation, whether individuals charged with technical violations were also arrested, and whether an individual's first technical violation preceded their first arrest (among the subsample of individuals who were charged with a technical violation and arrested).

Among individuals who were not charged with a technical violation, the technical violation indicator assumed a value of zero. Among individuals who were charged with a technical violation only, the technical violation indicator assumed a value of one. Among individuals who were charged with a technical violation and arrested, the technical violation indicator only assumed a value of one

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narcotics and controlled non-narcotics, possession of marijuana for sale, sale/transportation of marijuana, and other felony and misdemeanor drug offenses. Other arrests include: prostitution and commercial vice, gambling, driving under the influence, and all other offenses.

[29]Three percent (3%) of the first arrest incidents involved more than one charge. In this event, the more serious charge was used to categorize the arrest. Person arrests were considered most serious, followed by property arrests, drug arrests, and "other" arrests.

if the first technical violation charge occurred prior to the first arrest. Figure 1 illustrates the breakdown of recidivism incidents among individuals in the ten jurisdictions pooled. Among those who experienced both recidivism events, 61% of the first technical violation charges took place before the first arrest.

Care was taken to accurately distinguish between technical violations (defined as rules violations) and criminal recidivism. The technical violation code attached to each technical violation in the data set was used to ensure that the technical violation was in fact a rules violation. Illegal behavior that resulted in a technical but not a new arrest was not counted as a technical violation (the data contained a code for technical violations that stemmed from illegal behavior). In addition, technical violations that occurred on the same day as a new arrest and were coded ambiguously (e.g., Violation of Probation/Parole) were not counted as a technical violation charge because it was likely that the technical violation resulted from the new arrest.

The technical violation codes attached to each technical violation charge were also used to classify the general measure of technical violation charges into five dummy-coded indicators of technical violation type (see Table 5). The five categories were created based on the relative frequency of technical violation charges and their substantive similarity. The categories were: (1) failure-to-report; (2) drug/alcohol related violation; (3) abscond; (4) curfew violation; and (5) all other violations.[30]

Exposure Risk. Exposure risk was intended to measure the number of days an individual was free in the community and hence "at-risk" to commit an offense. Variables measuring exposure risk are used to control for unequal follow-up times in recidivism research (e.g., Gottfredsòn & Taylor, 1986:141; Smith et al., 1989:115). When operationalized correctly, measures of exposure risk generally have a positive relationship with the recidivism outcome. Such a relationship implies that the longer individuals are observed in the community, the more likely they are to recidivate.

Preliminary analyses revealed that when exposure risk (measured as the number of days free in the community) was entered into a regression model predicting arrest, it had a strong, statistically significant negative effect on arrest. The inverse relationship between exposure risk and arrest was counter-intuitive, suggesting that those individuals who were "at-risk" in the community for the shortest period of time were more likely to be arrested. The inverse relationship between exposure risk and arrest suggested that the exposure risk variable was not completely exogenous to the arrest process. In other words, the number of days

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[30]Two percent (2%) of the individuals charged with technical violations were charged with two types of violations on the same day. Since there was not an obvious strategy to rank technical violation charges with regard to seriousness, the first technical violation charge was used to categorize the incident. Statistical models were estimated using the second technical violation charge to categorize the technical violation incident as well and led to virtually the identical result as when the first technical violation incident was used.

that individuals were followed in the community was tied directly to their community supervision performance (i.e., recidivism), the construct being modelled.

As a consequence, the exposure risk measure was reconceptualized to purge it of the component that was directly related to community supervision performance. Instead of subtracting all confinement time from the total followup period (365 days) to determine the number of days "at-risk" in the community, only confinement ordered as part the original probation sentence was subtracted. Time-served as part of the original sentence was clearly independent of community supervision performance upon release. Measured in this way, exposure risk was no longer negative and statistically significant. Instead of representing the total number of days an individual had been "at-risk" in the community, it represented the maximum number of days an individual could have been "at-risk" in the community absent any misbehavior (e.g., technical violation or arrest).

<u>Confinement as a Result of a Technical Violation Charge</u>. Since exposure risk was not sufficient to control for the possibility of an incapacitative effect springing from technical violation sanctions, a binary variable was created to measure whether the sanction imposed in response to a technical violation charge involved any type of total confinement (e.g., jail, prison, detention center, shock incarceration program).[31] Values of the confinement variable were conditional on

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[31]Technical violation sanctions that involved confinement included: (1) continued current program + new conditions + jail; (2) jail; (3) prison; (4)

whether an individual was charged with a technical violation.

Among individuals who experienced a technical violation charge only, the confinement variable assumed a value of one if any technical violation charge (not just the first technical violation charge) resulted in confinement. Among individuals who were both arrested and charged with a technical violation, the confinement variable assumed a value of one if any technical violation charge (not just the first technical violation charge) prior to the first arrest resulted in a period of confinement.[32] Among individuals who were not charged with a technical violation, it was not possible to be confined prior to the first arrest. In such cases, the confinement variable equaled zero.

Supervision Intensity Measures. Supervision intensity was measured on a monthly basis. The data contained the raw number of supervision-related events each month. Four types of supervision activities were available in the data and

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detention center; and (5) shock incarceration. It was unclear whether probation revocation should be included in the measure of confinement since it didn't necessarily result in confinement. It is possible to simply reinstate probation subsequent to revocation. Since probation revocation formed a distinct, mutually exclusive category in the codebook, it was not possible for probationers to be confined (i.e., a jail or prison code) and have their supervision status revoked. Therefore, probation revocation was not included in the confinement measure. Statistical models were estimated with revocation included in the confinement measure using the total pooled sample. The inclusion of revocation in the confinement measure resulted in an additional 22 cases that were considered to be confined, but did not substantively affect the results.

[32]Since the purpose of the confinement variable was to control the possibility of an incapacitative effect, confinement resulting from any technical violation sanction that occurred prior to the first arrest was deemed relevant.

used in the present study: (1) the number of personal (i.e., face-to-face) contacts between the supervisory officer and the study participant; (2) the number of telephone contacts between the supervisory officer and the study participant; (3) the number of monitoring checks performed by the supervisory officer[33]; and (4) the number of drug and alcohol tests taken (see Table 5). In seven sites, the number of positive drug tests was also available on a monthly basis. In addition, the data contained a variable that measured whether study participants were members of the experimental ISP subsample or the control non-ISP subsample.

Four measures of supervision intensity were constructed by calculating the average monthly rate of each type of supervision activity. Among individuals who were arrested, the average number of supervision-related activities that occurred <u>prior</u> to and during the month of arrest was calculated. Since the focus of the study was to examine the possibility that technical violation charges signalled criminal recidivism (as measured by <u>first</u> arrest), supervision patterns that occurred prior to the first arrest were central to the investigation. Further, it did not make sense to use supervision activities that took place after first arrest to predict arrest. Among individuals who were not arrested during the course of the followup period, supervision intensity variables were averaged across the total followup

[33]Monitoring checks included criminal record checks, other law enforcement checks, employment or school verifications, collateral contacts at the home, school, or place of employment, and community service location.

period.[34]

Community Activities. Two measures of community activities were constructed: (1) the number of days worked each month (20 days equals full-time employment); and (2) the number of counseling sessions attended each month (see Table 5). Although the data collection instrument distinguished between different types of counseling sessions (e.g., psychological, family/marital, alcohol/drug counseling), the low frequency of each type of counseling required the construction of a general counseling measure. Both the employment and counseling variables were collected on a monthly basis. The raw number of days worked and the raw number of counseling sessions attended were documented each month by agency staff.

Monthly rates of days worked and counseling sessions attended were computed. Among individuals who were arrested, a monthly rate of days worked and counseling sessions was computed prior to and including the month of first arrest. Among individuals who were not arrested, a monthly rate of days worked and counseling sessions attended was computed across the entire followup period.

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[34]An obvious problem with this approach was that supervision intensity was likely to diminish naturally over the course of the supervision period. Thus, supervision intensity may appear to be positively related to arrest simply because the measurement of supervision intensity was cut short for persons who were arrested when supervision levels were more likely to be higher. Comparison of the average number of face-to-face contacts, phone contacts, monitoring checks, and drug tests taken between the group of individuals who were arrested and the group of individuals who were not arrested did not reveal statistically significant differences in supervision intensity across groups. *t*-tests were calculated on data pooled across the 10 jurisdictions.

Demographic Variables. Basic demographic characteristics included sex, age, race and ethnicity, educational background, and marital status (see Table 5). For the purposes of the present study, race/ethnicity was collapsed into a binary indicator where one equals Nonwhite and zero equals White. Educational background was collapsed into a binary indicator where one represents a high school diploma or greater and zero represents less than a high school education. Marital status was collapsed into a binary indicator where one represents individuals who are married (including common law marriages) and zero represents all other possibilities.

Criminal History and Offense-Related Variables. Measures of criminal history used in the present study included the number of prior arrests, the number of prior felony convictions, the number of prior misdemeanor convictions, and the number of state/federal prison terms served (see Table 5). Age at first arrest was collected by RAND; however, high percentages of missing data in several sites precluded its use.

The current conviction offense was measured using four dummy-coded indicators of offense type: (1) person offense; (2) property offense; (3) drug-related offense (sale or possession); and (4) "other" offense.[35] The "other" offense category included probation/parole revocations. When a study participant was convicted on multiple counts, the most serious offense type was used. Person

[35]See footnote 30.

offenses were deemed most serious, followed by property offenses, drug offenses, and "other" offenses.

Risk and Need Assessment Variables. As part of the Background Assessment form a risk and need assessment similar to the National Institute of Corrections risk and need assessment instrument was completed by probation/parole department staff. The assessment consisted of fourteen items.[36] Missing data were endemic in nearly half of the jurisdictions. In one jurisdiction (Macon, Georgia), the answers to the entire risk and need assessment were virtually missing. Missing data appeared to be most problematic among items that supervisory officers were less likely to have objective information about at the beginning of the supervision period (e.g., marital/family counseling needs, mental ability, health, companions, attitudes, emotional stability, sexual behavior).

Initially, the construction of a risk and need assessment scale was attempted using 9 of the 14 items (percent of time employed, vocational needs, employment needs, financial assistance needs, marital/family counseling needs, number of address changes, attitude, alcohol treatment needs, and drug treatment needs). However, due to the extent of the missing data in some jurisdictions, the use of the

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[36]The items included: (1) the number of address changes in the last 12 months; (2) the percent of time employed, in training, or in school; (3) attitude towards personal change; (4) academic/vocational training needs; (5) employment assistance; (6) financial management assistance; (7) alcohol treatment needs; (8) other drug treatment needs; (9) marital/family counseling needs; (10) mental ability; (11) health; (12) companions; (13) emotional stability; (14) sexual behavior.

scale was ultimately rejected in favor of an individual variable approach. Five variables were selected (alcohol treatment needs, drug treatment needs, employment needs, vocational needs, and number of address changes) (see Table 5).

In addition to the risk and need assessment, evidence of drug dependency by drug type at the time of arrest was collected in 7 sites.[37] A binary variable measuring whether individuals were dependent on any drug (except marijuana) was created, where one indicates evidence of drug dependency (see Table 5). Marijuana was excluded because in several sites (Macon, Des Moines, Santa Fe, and Winchester) virtually the entire sample was coded as being drug dependent if marijuana was included in the measure.

Treatment of Missing Data

Several approaches exist for handling missing-data values within a regression framework (Blackhurst & Schluchter, 1989:164; Little, 1992:1229; Vach & Schumacher, 1993:353). The choice of methodology depends on the mechanism assumed to underlie the missing data process. Most approaches are grounded in the assumption that the missing values of a particular variable are "missing at random." Missing-data values are assumed to be missing at random if

[37]Evidence of dependency on the following drugs at the time of arrest was documented: marijuana/hashish, LSD/hallucinogens, PCP, uppers, downers, quaaludes, cocaine, heroin, alcohol, prescription drugs, methadone, pain pills, morphine.

the probability that they are missing is independent of the true value of the incompletely observed variable (Little, 1992:1229; Vach & Schumacher, 1993:353). A variable with missing-data values may be missing at random even if the probability that data values are missing is dependent on other observed covariates or the dependent variable (Little, 1992:1229). However, if the probability that a variable is missing is dependent on the true values of the incompletely observed variable (e.g., individuals with higher incomes are less likely to report their income), missing-data values are not missing at random (Little, 1992:1229).

A common means of dealing with missing-data values is to delete the cases from analysis that contain missing data (i.e., complete-case analysis). This approach is easily implemented and yields valid inferences in situations where missing-data values are not assumed to be missing at random (Little, 1992:1229; Vach & Schumacher, 1993:361). A major disadvantage of course is the loss of information due to the loss of cases.

Another common approach to dealing with missing-data values is to impute estimates of the missing-data values and then estimate regression models using the "filled-in" data set. Such imputation procedures rely on the following estimates of missing data-values: (1) the unconditional sample mean; (2) the conditional sample mean (obtained by regressing the variable with missing-data values on the observed covariates); and (3) the conditional sample mean with random error added (Blackhurst & Schluchter, 1989:165). Due to the potential for bias, unconditional sample mean imputation, however, is generally not recommended (Blackhurst & Schluchter, 1989:164; Little, 1992:1231). More sophisticated approaches relying on maximum likelihood estimation and multiple imputation have also been developed (Little, 1992:1229).

Missing Demographic and Criminal History Data. Missing demographic data did not present a serious problem. In four sites, there were no missing demographic data. The percentage of missing data exceeded 2% in only five of the fifty variables measured across sites. In two of those instances, the percentage of missing data exceeded 5%. In Seattle, 5% of the cases were missing the marital status indicator and in Des Moines, 7% of the sample were missing the educational attainment indicator. When the data from the ten jurisdictions were pooled, the percentage of missing-data values did not exceed 2% for any variable.

By and large, missing criminal history data also did not pose a serious problem. The percentage of missing data exceeded 5% in 9 of the 50 variables measured across sites. In only one instance did missing data exceed 10% (the measure of prior arrests, in Winchester, Virginia). When the data from the ten jurisdictions were pooled, the percentage of missing-data values did not exceed 3%.

Demographic and criminal history missing-data values were assumed to be missing at random. Due to the very small percentage of missing-data values for each variable and the ease of implementation, missing-data values were replaced with the unconditional site sample mean.[38]

Missing Risk and Needs Assessment Variables. Among the five items selected for analysis, the percentage of missing-data values in several jurisdictions was nontrivial. Excluding Macon, Georgia (where virtually the entire risk and need assessment was missing), the percentage of missing data in the other nine sites exceeded 10% on eight of the 45 items measured. Three of the eight variables were excluded from the analyses because the percentage of missing data approached 50% (vocational and employment needs in Los Angeles) and 30% (employment needs in Waycross, Georgia). Otherwise, the percentage of missing data ranged from 1% to 20%. Among the risk assessment items chosen for analysis, it was assumed that missing data were missing at random. The unconditional sample means was then imputed.

Missing Supervision Intensity Variables. In eight of the ten jurisdictions there was virtually no missing supervision intensity data. In two sites (Los Angeles, California and Ventura, California), 27% and 34% of the cases, respectively, were missing one or more months of supervision intensity data. The balance of missing supervision intensity data in those sites was presumably due to

[38]The complete-cases analysis approach was also used as a comparison. Logistic regression models were estimated using only cases with complete demographic data (10 sites pooled). Logistic regression models were also estimated using only cases with complete criminal history data (10 sites pooled). The substantive results did not vary based on the missing-data approach selected.

the confinement or absconding behavior of persons under supervision.[39] When such behavior occurred in the other jurisdictions, however, in place of coding it as missing, it was presumably coded as zero. In order to maintain consistency across jurisdictions, all missing supervision intensity data values were recoded to zero. The average number of personal and telephone contacts, monitoring checks, and drug tests (prior to the first arrest, if arrested) was then calculated.

Missing Community Activity Variables. The percentage of study participants missing at least one month of employment data ranged from 8% to 74%. In five sites, there were no missing counseling data. In each of the other five sites, the number of study participants who were missing at least one month of counseling data ranged from 1.7% to 34%.

Employment and counseling data are likely missing for the same reasons that supervision intensity data (i.e., confinement or absconding) were missing. In addition, it was also possible that supervisory officers simply did not know how many days an individual had worked each month and hence it was not recorded. Due to the inability to distinguish missing-data values stemming from confinement from missing-data values stemming from all other processes (e.g., lack of knowledge), missing values were simply recoded to zero. Monthly rates of

[39]It was not possible to determine whether an individual had been confined or had absconded during a particular month.

employment and counseling (prior to first arrest, if arrested) were then computed.[40]

Measures Used in Test 2

The purpose of Test 2 was to assess whether technical violation charges and criminal recidivism (i.e., arrest) during community supervision were manifestations of the same underlying propensity to offend, consistent with the signalling hypothesis. To that end, a four-category general recidivism outcome variable was created for use in multinomial logistic regression models (see Figure 2). The four categories included: (1) individuals who had not experienced either recidivism event (technical violation charge or arrest) during the one-year followup; (2) individuals who experienced one or more arrests only; (3) individuals who committed one or more technical violations only; and (4) individuals who were both arrested and charged with a technical violation.

Other measures used in Test 2 were identical to those used in Test 1 with the exception of the time-varying community activity and supervision intensity variables. In Test 1, monthly rates were calculated prior to the first arrest if an individual had been arrested. Otherwise, the monthly rate was averaged over the

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[40]An alternative strategy for dealing with missing data in this situation may have been to calculate the average rate among the number of nonmissing months. Thus, if 11 of 12 months of employment data were available, an average rate could be computed over the 11 nonmissing months.

Monthly rates of employment and counseling were computed using this method. Use of either strategy for dealing with missing data yielded substantively identical results.

entire one-year followup period. In Test 2, since the outcome measure consisted of any recidivism event (not just arrest), monthly rates were calculated prior to and including the month of the first recidivism event (either technical violation charge or arrest). As in Test 1, if an individual had not experienced either recidivism event, the monthly rate was averaged over twelve months.

Data-Pooling Strategy

Ten of the fourteen sites that participated in the ISP demonstration project were selected for participation in this research (see Chapter 2). Test 1 analyses were estimated first using a pooled sample of the ten jurisdictions. Due to the contextual and organizational differences reported across sites (Petersilia & Turner, 1990:292), dummy variables representing site membership were included as control variables in each analysis. In order to control for the possibility that the impact of explanatory variables on the dependent variable varied by site, site-by-explanatory variable interaction terms were systematically introduced into the models following Hosmer and Lemeshow (1989:91). Statistically significant interaction effects indicated that the effects of some explanatory variables were not constant across sites.

When the interaction effect involved certain binary explanatory variables (e.g., marital status, technical violation charge indicator), however, the logistic regression models sometimes failed to converge due to a zero cell count resulting from the addition of the product term (Hosmer & Lemeshow, 1989:128). The problem stemmed from the sparse number of arrests in two of the Georgia jurisdictions.

Because it would not have been possible to generalize results from the pooled model to individual sites without testing for site-by-explanatory variable interaction effects, separate subsample analyses were also conducted. Three clusters of more homogeneous sites were identified primarily by virtue of their common geographic location. Sites located within the same state were presumed to be guided by more similar correctional philosophies and policies and as a consequence to operate more similar programs.

The three California sites formed one subsample (Contra Costa County, Los Angeles, and Ventura). The three Georgia sites formed a second subsample (Atlanta, Macon, and Waycross). The third subsample consisted of 4 geographically diverse sites (Des Moines, Iowa; Santa Fe, New Mexico; Winchester, Virginia; and Seattle, Washington). Three of the four "miscellaneous" sites operated probation/parole enhancement programs (as opposed to exclusively probation programs) and all four of these sites collected drug-abuse information.

Results from the ten-site analysis and the three subsample analyses are presented as part of Test 1. Due to sample size constraints, only the total pooled sample is used in Test 2.

Statistical Models

Logistic regression and multinomial (or polytomous) logistic regression models were estimated in Test 1. Test 2 relied exclusively on multinomial logistic regression models. A negative binomial panel model was additionally estimated to examine the effect of technical violation charges on measures of supervision intensity.

Logistic regression is commonly used to analyze the relationship between a set of explanatory variables and a binary outcome. Logistic regression is based on the cumulative logistic probability function. This function relates probabilities of the dependent variable to the explanatory variables (Hanushek & Jackson, 1977:187). Subsequent to the logistic transformation, the dependent variable represents the logarithm of the odds of an event occurring (Pindyck & Rubinfeld, 1991:259). The logistic regression model is shown below:

$$\text{logit}(\text{pr}(Y=1 \mid X_1, X_2, \ldots X_i)) = \alpha_0 + \alpha_1 X_1 + \alpha_2 X_2 + \ldots \alpha_i X_i + e,$$

where $\text{logit}(p) = \ln(p/(1-p))$.

A major advantage of the logistic model over the linear probability model is that probability estimates are bounded between zero and one. A major disadvantage is that the parameter estimates are more difficult to interpret. Logistic regression models were estimated using SAS software (version 6.04) (SAS Institute, 1990).

The multinomial logistic regression model is an extension or generalization of the simple logistic model (Hosmer & Lemeshow, 1989:216). Instead of modeling a binary outcome, the multinomial outcome consists of three or more mutually exclusive categories. While binary logistic regression involves the estimation of one logit function (i.e., $Y=1$ versus $Y=0$), multinomial logistic regression involves the estimation of more than one logit function (equal to the number of categories less the reference category outcome) (DeMaris, 1992:61; Hosmer & Lemeshow, 1989:217). Thus, as Hosmer and Lemeshow illustrate (1989:217), a three-category outcome variable ($Y=0, 1, 2$) is characterized by two logit functions ($Y=1$ versus $Y=0$ and $Y=2$ versus $Y=0$). These two logit functions can then be used to calculate the logit comparing $Y=2$ to $Y=1$ (Hanushek & Jackson, 1977:212; Hosmer & Lemeshow, 1989:217). Estimation of a multinomial logistic model involves the estimation of separate equations for each logit function (DeMaris, 1992:66). Multinomial logistic regression models were estimated using LIMDEP (version 7.0) (Greene, 1995).

General Model-Building Approach

The model-building approach described by Hosmer and Lemeshow (1989) guided the analysis. Rather than detailing the model-building process for each logistic regression model estimated, an overview of the approach is presented here.

Each analysis began with an evaluation of the bivariate relationship between each explanatory variable and the outcome variable. Contingency table and chi-square analysis was used to assess the bivariate relationship between nominal- and ordinal-level variables. Univariate logistic regression models were estimated to assess the relationship between the continuous variables and the arrest outcome.

Explanatory variables that were associated with the outcome variable and exhibited p-values of roughly $p < .25$ were selected for inclusion in preliminary logistic models. Variables of particular theoretical interest were also included in the preliminary models regardless of the p-value.

Preliminary multivariate logistic models were then fit and the effects of each explanatory variable examined. Changes in the direction and/or magnitude of effects from the bivariate to multivariate analysis were noted. Variables that were clearly of little relevance in explaining arrest in these particular models were successively excluded from the model. Likelihood ratio tests were computed comparing the log-likelihood value of the full model to the log-likelihood value of the reduced model.[41] A nonsignificant test statistic was used as evidence that the excluded variables did not contribute significantly to the model.

After identifying important explanatory variables, the assumption of linearity in the logit was verified following Hosmer and Lemeshow (1989:88). To that end, design variables were constructed to assess whether the relationship between each explanatory variable and the logit was linear. Design variables were constructed by grouping a continuous variable into quartiles based on its frequency distribution and then treating it as categorical in the logistic regression model. The set of design (or dummy-coded) variables was then used in place of the continuous variable in the logistic model with the dummy variable representing the lowest

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[41]The test statistic is distributed as χ^2 and is computed as follows:

$$\chi^2 = -2[\log(\text{unrestricted likelihood}) - \log(\text{restricted likelihood})],$$

where the degrees of freedom equals the number of equality restrictions imposed.

quartile serving as the referent group. Linearity in the logit is evidenced by an increasing or decreasing trend in the coefficients (Hosmer & Lemeshow, 1989:96).

Once it was established that the relationship between the logit and each continuous variable appeared to be linear, tests for interaction effects were conducted. Interactions between the main site effects (dummy-coded variables capturing site membership) and the individual explanatory variables were of principal interest. Each set of explanatory-variable-by-site interactions (i.e., a total of 9 product terms for each explanatory variable in the total sample) was systematically entered into the model. A likelihood ratio test comparing the log-likelihood value of the main-effects only model to the log-likelihood value of the model containing the main effects and the set of interaction effects was computed. A significant test statistic indicated that the interaction terms were important and that constraining the explanatory variable to have the same effect across sites was unjustified. Each set of statistically significant explanatory-variable-by-site interaction effects was retained in the final model.

The appropriateness of the logistic response function was then examined by means of a "goodness-of-fit" test (sometimes called the Hosmer-Lemeshow test) (Hosmer & Lemeshow, 1989:140-141; Neter et al., 1989:613). The test statistic provides a summary means of assessing the overall fit of the logistic model. Calculation of the statistic involved first grouping the sample into deciles based on the predicted probability of arrest obtained from the model. The actual and predicted number of arrests observed in each decile across both outcome types were then used to calculate the test statistic. Hosmer and Lemeshow (1989:141) and Neter et al. (1989:614) present the formula.

While the goodness-of-fit statistic provides a summary measure of the fit of the model, the test is not able to detect small departures from a logistic response function (Neter et al., 1989:613). Therefore, diagnostics aimed at detecting individual departures from fit were examined.

Standardized Pearson and deviance residuals were examined to detect outliers. Values of the deviance residual will usually lie between -2 and 2 (Collett, 1991:126). Leverage values derived from the Hat Matrix Diagonal were examined to identify particularly influential cases in the model. Influential cases are those that when omitted from the model substantially alter the fit of the model (Collett, 1991:146). Observations with high leverage values possess combinations of the values of the explanatory variables that are far removed from the other observations in the covariate space (Collet, 1991:148; Hosmer & Lemeshow, 1989:153). Outliers are not necessarily influential just as influential cases are not necessarily outliers. An influential case may distort the estimates to such an extent that its residual may be indeed small (Collet, 1991:146).

Leverages are considered large if their value is two times as large as the average leverage value (Collet, 1991:149). Observations with high leverage values were deleted from the analysis in order to assess their impact on the parameter estimates.

Summary

Data from the RAND Corporation evaluation of ISP programs are used to test the signalling hypothesis. Ten of the fourteen sites (involving a total of approximately 1,000 offenders) were selected for participation in the study. Eight of the ten sites specifically targeted probationers convicted of drug-related offenses or probationers who had a history of drug-involvement.

Data were collected from probation/parole office records by agency staff. A Background Assessment form provided demographic, criminal history, current offense information, a risk and need assessment, and drug abuse information (in seven sites). Data related to community supervision included measures of supervision intensity, recidivism (i.e., technical violation charges and arrests), and community activities such as employment record and counseling attendance.

Logistic regression and multinomial logistic regression models were estimated in the balance of the analyses. The model-building strategy outlined by Hosmer and Lemeshow (1989) was adopted. Analyses were conducted using the total pooled sample of ten jurisdictions and three separate subsamples. Subsample analyses were deemed necessary due to the heterogeneity of the participating sites and the inability in some instances to control for site-by-explanatory variable interactions.

Two statistical tests were proposed to examine the "signalling" hypothesis. Test 1 assesses whether technical violation charges exert a positive and statistically significant effect on arrest in multivariate logistic regression models. Test 1 is estimated using the total pooled sample (ten sites) and the three subsamples (i.e., California sites, Georgia sites, and "miscellaneous" sites). It also takes a technical-violation specific approach by examining whether certain types of technical violations are more likely to serve as "signals." Two additional analyses include an examination of whether the effect of technical violation charges is constant across different types of arrests and an examination of whether supervision intensity increases as a result of a technical violation charge.

Test 2 examines whether a single underlying propensity generates both technical violation charges and arrests consistent with a generality of deviance explanation of the signalling hypothesis. The test formally compares multinomial logistic models subject to different sets of equality restrictions in order to determine whether different types of recidivism outcomes are indicators of the same underlying propensity (here called Recidivism Propensity). Due to sample size constraints, Test 2 models are estimated using the total pooled sample only.

CHAPTER V
RESULTS

TEST 1

Total Sample Analysis

Model Construction. Multivariate logistic regression models were estimated to predict the binary indicator of arrest. Attention focused primarily on the effect of the technical violation charge indicator on arrest. The bivariate relationship between the indicators of arrests and technical violations is shown in Table 6. A Pearson correlation coefficient revealed a negative and statistically significant relationship between technical violation charges and arrests. (See Table 7 for a detailed presentation of technical violation charge and arrest types.)

Demographic, criminal history, supervision intensity, and community activity variables were included in the preliminary logistic regression model (see Table 8 for descriptive statistics).[42,43] In addition, the preliminary model

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[42]Due to the positively skewed distribution of the variables measuring time-varying community characteristics (e.g., supervision intensity and community activities), the natural logarithm of each of these variables was used in place of the raw value (Collett, 1989:84).

[43]Risk and need assessment variables were not included in the model due to failure of one site to collect such data. In addition, variables related to drug dependency and drug testing were not available in the three California sites.

contained three explicit control variables: (1) a series of dummy variables intended to measure site membership; (2) the "exposure risk" variable; and (3) the dichotomous indicator measuring whether technical violation sanctions resulted in confinement.

Two missing-data value indicators were included in the models (i.e., a missing-data value indicator for days worked and a missing-data value indicator for the supervision intensity measures combined). Missing-data value indicators were included in the model when more than 10% of the sample possessed one or more months of missing-data values on a time-varying variable. Missing-data value indicators were included to examine whether the missing-data value pattern was systematically related to arrest (Cohen & Cohen, 1983:289; Raymond, 1986:401).

Due to the conditional relationship between the indicator of technical violation charges and the confinement indicator (i.e., confinement in response to a technical violation), preliminary analyses revealed that the resulting coefficients were difficult to interpret. Therefore, the technical violation charge indicator and confinement indicator were combined into two dummy-coded variables: (1) technical violation charge resulting in confinement; and (2) technical violation charge not resulting in confinement (see Figure 3).[44] The combined indicators

[44]The technical violation charge indicator exerted a negative and statistically significant effect on arrest when the confinement indicator was not included in the model (Model 1). When the confinement indicator was included in the model (Model 2), the effect of technical violation charges on arrest was still negative and statistically significant. However, the magnitude of the effect was cut roughly in half. However, the confinement indicator did not have a statistically significant

were used throughout the analyses.

Following the estimation of the preliminary model, variables that did not contribute substantially to the model were eliminated. Variables that were retained in the reduced model in addition to the control variables included: (1) the technical violation/sanction indicators; (2) age; (3) the average number of personal contacts, telephone contacts, and monitoring checks; (4) the average number of days worked per month; (5) the average number of counseling sessions attended; (6) ISP sample membership; (7) the number of prior misdemeanor convictions; and (8) the number of prior prison terms served.

The scale of continuous variables in the reduced model was then examined. One variable, the average number of telephone contacts, appeared to exhibit a nonlinear relationship with the logit. A quadratic and cubic term were added to the model to account for the apparent nonlinear relationship. Both terms were statistically significant at the $p < .05$ level. Further examination, however, revealed that the appearance of nonlinearity was due to the effect of an outlier. When one particular case was excluded from the analysis, the statistically

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effect on arrest and the log-likelihood value of the two models did not change.

The technical violation charge and confinement variables were then combined as described in the text. Use of the dual technical violation charge/sanction indicators revealed that the effect of the technical violation charge indicator (controlling for the confinement variable) in Model 2 represented the effect of technical violation charges on arrest among the subsample of individuals who had not been confined in response to the technical violation charge. The combined technical violation charge/technical violation sanction indicators were therefore used throughout the analyses because they provided a more straightforward interpretation.

significant effect of the cubic term disappeared. In order to avoid eliminating cases, the natural logarithm transformation of telephone contacts was then used in place of the raw number to minimize the disproportionate effect of the outlier. Subsequent to the log transformation, both the quadratic and cubic terms were no longer statistically significant and were dropped from the model.

Product terms were created in order to test for interaction effects. Site-by-explanatory-variable interaction effects were of primary concern. Interaction effects contributed significantly to the model for two supervision intensity measures, ISP sample membership, and the counseling measure.[45] Site-by-explanatory-variable interactions that focused on individual-level characteristics, on the other hand, were not significant. Significant interaction effects were retained in the model.

Hosmer and Lemeshow's "goodness-of-fit" statistic was calculated on the model containing the main effects and significant interaction effects. The test statistic, $\chi^2(8, \underline{N}=1{,}037) = 8.36$, $\underline{p}=0.399$, was not significant, indicating that the overall fit of the model was satisfactory. Figure 4 illustrates the fit of the model by providing a comparison of the observed and predicted number of arrests in each decile (ranked by the predicted probability of arrest).

The summary goodness-of-fit examination was followed by an examination

[45]When the technical violation charge-by-site interaction effects were entered into the model, the model failed to converge. This failure was due to a zero-cell count resulting from one of the site-by-technical violation product terms.

of standardized deviance residuals and leverage values. Deviance residuals that exceeded $|2|$ occurred in roughly 2% of the cases. In the vast majority of these cases, the model predicted a low probability of arrest (e.g., $\pi < .10$) for individuals who had in fact been arrested. Omission of these cases from the analysis did not affect the substantive conclusions. Approximately six cases with high leverage values were identified. When eliminated from the analysis, their absence did not have a large effect on the parameter estimates.

Total Sample Results

Results from the reduced model are shown in Table 9.[46] Both technical violation charge and sanction indicators exerted a negative and statistically significant effect on arrest. Figure 5 illustrates the predicted probability of arrest among individuals who were charged with a technical violation and among individuals who were not charged with a technical violation. The logistic regression function was used to calculate the predicted probability of arrest with all explanatory variables (except the technical violation indicators) held constant at their mean or median value (King, 1989:104-105).[47] The predicted probability of

[46]A correlation matrix of the explanatory variables in the model is shown in Appendix 1. The determinant of the correlation matrix reveals a high degree of multicollinearity.

[47]Predicted probabilities are calculated with individual-level characteristics held constant at their mean, and supervision- and community-related characteristics held constant at their median level. When all variables (including the measures of technical violation charges) are held constant, the predicted probability of arrest is

arrest therefore refers to a hypothetical individual characterized by average levels of all explanatory variables in the model except whether they had been charged with a technical violation. As shown in Figure 5, a technical violation charge (regardless of whether it results in confinement) decreased the predicted probability of arrest by roughly 50% (from $\pi = .44$ to $\pi = .22$). Thus, contrary to the signalling hypothesis (which anticipated a positive and significant effect), the model suggested that being charged with a technical violation decreased the probability of arrest regardless of whether it resulted in a sanction of confinement.

Individual-level demographic and criminal history variables suggested that individuals who were older were significantly less likely to be arrested while individuals with more extensive criminal histories (e.g., a greater number of prior misdemeanor convictions and prior prison terms served) were more likely to be arrested during the one-year followup period.

The effect of three supervision intensity measures (average number of personal contacts, monitoring checks, and ISP sample membership) varied across sites. The conditional effects (i.e., the effect of supervision intensity on arrest in a particular site) are shown in Table 9.[48] Examination of the supervision intensity

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identical to the actual percentage of arrests observed in the sample (32%).

[48]Conditional effects and standard errors were calculated following Friedrich (1981:804-805, 828). The calculations are shown below using the following regression equation as an example (where X1 = a continuous variable and D = a dummy variable) (Friedrich, 1981:804):

$$Y = b_0 + b_1X_1 + b_2D + b_3DX_1 + e.$$

coefficients revealed the following statistically significant relationships: (1) the average number of personal contacts was positively related to arrest in two sites (Santa Fe, New Mexico and Winchester, Virginia); (2) the average number of monitoring checks was inversely related to arrest in one site (Atlanta, Georgia) and positively related to arrest in another site (Contra Costa, California); (3) ISP sample membership was inversely related to arrest in one site (Santa Fe, New Mexico); and (4) the average number of telephone contacts was positively related to arrest in all sites.

The number of days worked per month exerted a strong, statistically significant negative effect on arrest in all sites. The effect of the average number of counseling sessions attended per month varied across jurisdictions. In two sites (Contra Costa, California and Santa Fe, New Mexico), the number of counseling sessions was inversely related to the probability of arrest.

Missing-data indicators for employment and supervision intensity were not statistically significant, suggesting that the missing-data value pattern was not systematically related to the probability of arrest.

The conditional slope coefficient was computed by taking the sum of the main effect (b1) (e.g., supervision intensity measure) and the main effect-by-site interaction effect (b3). This coefficient represents the effect of the explanatory variable on the dependent variable for individuals in a particular site. The standard error of the conditional effect and *t* test were calculated using the formulas shown below:

$$s_{(b1 + b3)} = [\mathrm{var}(b1) + \mathrm{var}(b3) + 2\,\mathrm{cov}(b1, b3)]^{1/2}$$

$$t = (b1 + b3)/s_{(b1+b3)}.$$

Technical Violation-Specific Model. A technical violation-specific approach was adopted next, where the technical violation indicator was disaggregated by technical violation type. Five dummy variables measuring the following types of technical violations replaced the technical violation charge/sanction measures in the model: (1) failure-to-report violation; (2) drug/alcohol violation; (3) abscond violation; (4) curfew violation; and (5) all other violations (see Table 7 and Figure 6).

The results of the technical violation-specific model are shown in Table 10. Each type of technical violation charge with the exception of the absconding violation had a negative and statistically significant effect on arrest. The predicted probability of arrest among individuals who were charged with each type of technical violation is shown in Figure 7. The predicted probability of arrest among individuals who were not charged with a technical violation was $\pi = .45$. Predicted probabilities of arrest among those charged with different types of technical violations ranged from $\pi = .13$ (drug/alcohol violation) to $\pi = .35$ (absconding violation). Thus, the technical-violation specific analysis suggested that the effect of different types of technical violation charges on arrest was similar across technical violation types (i.e., negative and statistically significant) with the exception of the absconding violation (which was negative, but not statistically significant).

Arrest-Specific Model. A multinomial logistic regression model was estimated in order to determine whether the effect of technical violation charges on arrest was the same across four different categories of arrest. The dependent variable was categorized as follows: (1) no arrest; (2) person arrest; (3) property arrest; (4) drug arrest; and (5) "other" arrest (see Figure 8). The model was estimated using LIMDEP (version 7.0) (Greene, 1995). The set of explanatory variables used in the binary logistic regression model was used in this analysis. Technical violation charges were measured using the dual technical violation charge and sanction indicators.

A fully unrestricted model was estimated first (Model 1). Model 1 allowed parameter estimates to vary across categories of the outcome variable. The results of Model 1 are shown in Table 11.[49] A second model was estimated restricting the effects of the two technical violation charge indicators to be equal across categories of the dependent variable (see Table 12). All other explanatory variables in the model were allowed to vary. A likelihood ratio test comparing the log-likelihood value of Model 1 to the log-likelihood value of Model 2 was then computed (see Table 13). The degrees of freedom equaled the number of equality restrictions imposed. The test statistic, $\chi^2(6, \underline{N}=1{,}035) = 7.15$, $\underline{p} > .05$, was not

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[49]In Macon, Georgia and Santa Fe, New Mexico, individuals did not experience one or more of the arrest categories. As a result of the zero cell counts, parameter estimates for the two variables representing site membership on the missing arrest category tended toward negative infinity. In order to produce reasonable estimates, a two-step procedure was used. First, one individual in each site was randomly assigned to experience the arrest type that was missing. The parameter estimate from this model was recorded. A second model was then estimated using the original data. The parameter estimates with zero cell counts were "fixed" to equal the estimate yielded when one case was randomly assigned to the missing arrest outcome.

significant, indicating that the effect of technical violation charges did not appear to vary across arrest types.

California Subsample Analysis

Model Construction. Preliminary logistic regression models were estimated using the demographic, criminal history, supervision intensity, community activity, and control variables available in the 10-site analysis.[50] In addition, three variables from the risk and need assessment were available: (1) alcohol treatment needs; (2) drug treatment needs; and (3) the number of address changes within the past year. Descriptive statistics are shown in Table 14. The dual technical violation and sanction indicator and the technical-violation specific indicators were used to measure technical violation charges. The number and type of technical violation charges and arrests are shown in Table 7. The bivariate relationship between technical violations and arrests is shown in Table 6. Since over 10% of the sample were missing one or more months of employment data and/or supervision intensity data, two missing-data value indicators representing cases with missing employment and supervision intensity data were included in the models.

Statistically significant variables retained in the reduced model included: (1)

[50]Due to the positively skewed distribution of the variables measuring time-varying community characteristics (e.g., supervision intensity and community activities), the natural logarithm of each of these variables was used in place of the raw value (Collett, 1989:84).

technical violation charge and sanction; (2) age; (3) marital status; (4) average number of monthly telephone and personal contacts; (5) average number of monthly drug and alcohol tests; (6) average number of days worked per month; (7) average number of counseling sessions attended; and (8) number of prior arrests.

The introduction of site-by-explanatory-variable interaction terms revealed several significant interactions. In contrast to the ten-site analysis, site interactions involving measures of supervision intensity were not significant. However, site interaction effects involving the number of days worked per month, the number of counseling sessions attended per month, and marital status contributed significantly to the model.

The Hosmer and Lemeshow goodness-of-fit test on the reduced model yielded a nonsignificant chi-square statistic, $\chi^2(8, \underline{N}=488) = 4.62$, $\underline{p}=0.797$. The fit of the model to the data was therefore deemed satisfactory. Figure 9 depicts the fit of the model by graphing the number of observed and expected arrests in deciles (obtained by ranking individuals according to their predicted probability of arrest).

Residual and influence diagnostics were examined next. Residual analysis uncovered approximately 21 outlying cases. The outlying cases consisted primarily of individuals who had been arrested during the course of the study, but for whom the predicted probability of arrest was low. When deleted from the analysis, changes in the parameter estimates were inconsequential. Five influential cases were detected. Deletion of these cases did not meaningfully change the results.

California Results

The results of the reduced model are shown in Table 15.[51] Again, contrary to the "signalling" hypothesis, both indicators of technical violation charges exerted a negative and statistically significant effect on arrest. The magnitude of the effect appeared to be slightly larger among persons charged with technical violations and sanctioned with confinement.

Figure 10 compares the predicted probability of arrest among individuals who were not charged with technical violations to the predicted probability of arrest among individuals who were charged with technical violations.[52] The predicted probability of arrest among individuals who were not charged with a technical violation was $\pi = .55$. In comparison, the predicted probability of arrest among those who were charged with a technical violation and confined was $\pi = .16$, while the predicted probability of arrest among those who were charged with a technical violation and not confined was $\pi = .21$.

Two supervision intensity variables were positively related to arrest

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[51]Note that the determinant of the correlation matrix of explanatory variables was less than .01, suggesting a high degree of multicollinearity.

[52]The predicted probability of arrest was computed holding all other explanatory variables constant at either their mean or median value. Individual-level variables were held constant at their mean value while time-varying variables were held constant at the median value. Note that when all explanatory variables (including the dual technical violation charge indicators) were held constant at either the mean or median value, the predicted probability of arrest was $\pi = .35$. Thirty-five percent of the sample were in fact arrested.

(average number of telephone contacts and average number of drug/alcohol tests). The average number of personal contacts on the other hand was inversely related to arrest. The conditional effect of the average number of counseling sessions attended was negative and statistically significant in Los Angeles, California. The conditional effects of employment were not statistically significant.

Individual-level characteristics revealed that older individuals were less likely to be arrested while individuals with a greater number of prior arrests were more likely to be arrested. The missing supervision intensity data indicators were not significant, suggesting that the pattern of missing-data values was not systematically related to arrest.

Technical-Violation Specific Model. The identical model was estimated again using the technical-violation specific indicators. Due to the very small number of curfew violations in the California jurisdictions, technical violation incidents were categorized as follows: (1) failure-to-report violation; (2) drug/alcohol violation; (3) absconding violation; and (4) all "other" violations. Figure 11 illustrates the breakdown of technical violation charge types.

The results of the model are shown in Table 16. Each type of technical violation incident exerted a negative and statistically significant effect on the predicted probability of arrest. Figure 12 compares the predicted probability of arrest among individuals who were not charged with a technical violation with the predicted probability of arrest among individuals who were charged with different types of violations. The predicted probability of arrest among individuals not charged with a technical violation equaled $\pi = .55$. In comparison, the predicted probability of arrest among individuals who were charged with a technical violation ranged from $\pi = .05$ (absconding violation) to $\pi = .21$ (failure-to-report). Thus, regardless of technical violation type, individuals charged with a technical violation were less likely to be arrested.

Georgia Subsample Analysis

Model Construction. Logistic regression models were estimated using the core set of demographic, criminal history, supervision intensity, and community activity variables.[53] In addition, more extensive data related to drug abuse and drug testing were available in the Georgia sites. One variable captured the average number of positive drug tests per month and the other variable measured whether an individual was judged to be drug-dependent. Macon, Georgia did not collect risk and need assessment variables; therefore those variables could not be used in the combined analysis. Descriptive statistics are shown in Table 17.[54] The bivariate relationship between technical violation charges and arrests is shown in Table 6. Technical violation charge and arrest types are shown in Table 7.

Preliminary analyses revealed that the following variables contributed

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[53]Note that the raw values of the time-varying variables were used in the Georgia analyses because the distributions of those variables were not as positively skewed.

[54]Note the small number of arrests in Atlanta, Georgia ($n = 4$) and Waycross, Georgia ($n = 7$).

significantly to the explanation of arrest: (1) the dual indicators of technical violation charge and sanction; (2) race and ethnicity; (3) the average number of drug and alcohol tests taken per month; (4) the average number of days worked per month; (5) drug dependency determination; and (6) the number of prior prison terms served. None of the site-by-explanatory-variable interaction effects introduced in the model were statistically significant.[55] Thus, the reduced model consisted of the variables listed above in addition to the site control variables.[56]

The Hosmer and Lemeshow goodness-of-fit test yielded a borderline significant chi-square statistic, $\chi^2(8, \underline{N}=150) = 13.73, \underline{p}=.09$). Deviance residual analysis detected three observations with deviance residual values of greater than $|2|$. In these three cases, the predicted probability of arrest was low, yet the individuals had been arrested. This departure from fit is illustrated in Figure 13.

When the three cases with high deviance residuals were deleted from the analysis, the log-likelihood value dropped substantially. Several influential observations were detected as well. Deletion from the model did not substantively affect the results other than to slightly increase the magnitude of the effect of some of the explanatory variables. Since the deviations from fit did not result in differences in the substantive conclusions (and due to the obvious problem of

[55]The model containing the technical-violation indicator-by-site interaction terms did not converge due to a zero cell count.

[56]Due to the lack of variation in the exposure risk variable, it was removed from the Georgia model.

omitting cases from analysis), results from the complete-case analysis are presented.

Georgia Results

The results of the reduced model are shown in Table 18. Both the technical violation charge and sanction indicators were negative and statistically significant. Technical violation charges that did not result in confinement were borderline significant with a p-value of .06.

The predicted probabilities of arrest among those charged with a technical violation and those not charged with a technical violation are shown in Figure 14.[57] According to the model, the predicted probability of arrest among individuals who were not charged with a technical violation was $\pi = .28$. In comparison, the predicted probability of arrest among individuals who were charged with a technical violation and confined was $\pi = .05$, while the predicted probability of arrest among individuals who were charged with a technical violation and not confined was $\pi = .08$.

Individuals who were nonwhite or were judged to be drug-dependent were more likely to be arrested. The number of drug and alcohol tests taken per month was positively related to arrest. The number of days worked per month was

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[57]All explanatory variables in the model were held constant at their mean value. When all variables (including the dual technical violation indicators) were held constant at their mean value, the predicted probability of arrest was 12%. Twenty-one percent of the sample was in fact arrested.

inversely related to arrest ($p < .10$).

Technical Violation-Specific Model. The reduced model was estimated again replacing the dual indicator of technical violation charges and sanctions with four indicators of technical-violation type. In Georgia, there were four categories of technical violations: (1) failure-to-report; (2) drug- or alcohol-related violation; (3) curfew violation; and (4) all "other" violation types. The Georgia sites were unique because of the relatively high percentage of curfew violations. The categorization of technical violation types is shown in Figure 15.

Results of the analysis revealed that technical violation charges were generally associated with a decrease in the probability of arrest (see Table 19). Two of the four technical violation indicators (failure-to-report and drug and alcohol violation) were negative and statistically significant at $p < .10$. The "other" technical violation category was negative and statistically significant at $p < .05$. The curfew violation indicator was not statistically significant.

The predicted probability of arrest among individuals charged with different types of technical violation charges is shown in Figure 16. By and large, the predicted probability of arrest among individuals who were charged with a technical violation was less than half the size of the predicted probability of arrest among individuals who had not been charged with a technical violation. Thus, the technical violation-specific analysis revealed that regardless of the type of technical violation charge, persons charged with a technical violation appeared to be less likely to be arrested.

Miscellaneous Subsample Analysis

Model Construction. The full-range of variables were available in the miscellaneous sites including the risk and need assessment and the more specific drug-abuse and drug testing variables. Five risk and need assessment variables were considered in the analyses: (1) the number of address changes in the last twelve months; (2) academic or vocational training needs; (3) employment assistance needs; (4) alcohol treatment needs; and (5) drug treatment needs. Evidence of drug abuse, in addition to the number of positive drug tests per month, were available as well. Due to the skewed distribution of variables that were measured on a monthly basis (i.e., supervision intensity measures and community activities), the natural logarithm transformation was used in place of the raw value in the analyses. Descriptive statistics are presented in Table 20. The bivariate relationship between technical violation charges and arrests is shown in Table 6. Technical violation charge and arrest types are shown in Table 7.

Preliminary analyses revealed that the following variables proved important (i.e., they contributed significantly to the likelihood) in predicting arrest in this sample: (1) age; (2) the average number of telephone and personal contacts per month; (3) the average number of drug and alcohol tests taken per month; (4) the average number of days worked per month; (5) the number of prior misdemeanor convictions; (6) the number of prior prison convictions; and (7) ISP sample membership. Tests for site-by-explanatory variable interaction terms (including the technical violation indicators) were nonsignificant, indicating that values of the explanatory variables were relatively constant across jurisdictions.

The Hosmer and Lemeshow goodness-of-fit test suggested that the model fit the data satisfactorily, $\chi^2(8, \underline{N}=399) = 6.10, \underline{p}=.636)$. Figure 17 illustrates the fit of the model in each decile (ranked according to the predicted probability of arrest). Residual analysis uncovered eleven outlying observations (i.e., deviance residuals of greater than $|2|$). Deletion of these cases did not change the results other than to increase the order of magnitude of the explanatory variables included in the model. Three influential observations were detected as well by means of their leverage value. Deletion of these cases resulted in only trivial changes to the parameter estimates.

Miscellaneous Results

Results of the reduced model are shown in Table 21. In contrast to the other subsamples, the dual indicators of technical violation charges and sanctions were not significantly related to arrest. Two measures of supervision intensity were positively related to arrest (the average number of telephone and personal contacts per month), while one measure of supervision intensity (the number of drug and alcohol tests taken per month) was inversely related to arrest. The number of days worked per month had a strong, negative effect on arrest. The missing-data indicator for the employment variable was not significant, indicating that the missing-data value pattern was not systematically related to arrest.

Older individuals and individuals with less extensive criminal records (as measured by the number of prior misdemeanor convictions and prior prison terms served) were less likely to be arrested. Lastly, ISP sample participation was inversely related to arrest. Figure 18 shows the effect of ISP sample membership on the probability of arrest.

Technical Violation-Specific Model. Four indicators of technical violation charges were substituted for the dual indicator of technical violation charges and sanctions in the second analysis. The four technical-violation charge measures included: (1) failure-to-report violation; (2) drug- or alcohol-related violation; (3) absconding violation; and (4) all "other" types of violations (see Figure 19).

Notably, the technical violation-specific analysis suggested that different types of technical violation charges exerted different effects on the probability of arrest (see Table 22). Drug- and alcohol-related violations were associated with a significant decrease in the probability of arrest while absconding violations were associated with a significant increase in the probability of arrest. The indicator representing failure-to-report was positive and statistically insignificant while the indicator for "other" types of violations was negative and statistically insignificant.

The predicted probability of arrest for each type of technical violation charge is shown in Figure 20.[58] The predicted probability of arrest among

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[58]Other explanatory variables in the model were held constant at either their mean or median value. Due to the skewed distribution of the time-varying variables (e.g., supervision intensity and community activities), they were held constant at the median. All other variables were held constant at the mean. Holding all variables constant (including the technical violation indicators), the predicted probability of arrest was 43%. Thirty-three percent of the sample was

individuals who had been charged with absconding was .24 <u>greater</u> than similarly-situated individuals who had not been charged with a technical violation. On the other hand, the predicted probability of arrest among individuals who had been charged with a drug/alcohol-related violation was .24 <u>less</u> than similarly-situated individuals who had not been charged with a technical violation.

The technical-violation specific analysis suggested that the effect of technical violations on arrest varied by technical violation type. Because the effects of technical-violation specific indicators were of roughly the same magnitude but in opposite directions, the general measure of technical violations did not appear to be related to criminal recidivism.

<u>Supervision-Intensity Test</u>. In order to further explore the inverse relationship between technical violation charges and arrest, an additional test was conducted using the total pooled sample to assess whether supervision intensity increased as the result of a technical violation charge. An increase in supervision intensity in response to a technical violation charge may serve to deter individuals from committing crime, thereby explaining the inverse relationship between technical violation charges and arrest. For example, if individuals who had been charged with a technical violation perceived that they were being watched more closely and were therefore more likely to be apprehended upon commission of a crime, such perceptions might lead to a reduction in criminal recidivism.

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actually arrested.

The hypothesis that a technical violation charge leads to an increase in supervision intensity was assessed by examining whether a monthly technical violation charge indicator predicted a monthly count of supervision activities (e.g., number of monthly personal contacts). The dependent variable was therefore a count of the number of supervision activities experienced each month. Since supervision intensity data were collected on a monthly basis, there were 12 supervision intensity observations per person. Four types of supervision intensity were examined: (1) number of personal contacts; (2) number of telephone contacts; (3) number of monitoring checks; and (4) number of drug tests taken. The mean number of each type of supervision-related activity is shown in Table 23. Note that if individuals were confined or had absconded during a particular month, the supervision intensity measure for that month equaled zero.

A negative binomial model was estimated to model the relationship between technical violation charges and supervision intensity measures. Because the supervision intensity measures consisted of event counts (i.e., integers greater than or equal to zero), a nonlinear regression model such as a Poisson or negative binomial model was preferred to ordinary least squares (OLS) regression. The OLS assumption of a homoscedastic error term is violated when a count is used as the dependent variable (Gardner et al., 1995:393-394). OLS may also yield nonsensical predicted values (i.e., a negative value) when used to model count data (Gardner et al., 1995:393).

The assumptions of Poisson regression (i.e., a constant rate of supervision intensity across individuals and the independence of supervision intensity events) are restrictive, however. They imply that the conditional mean and variance of the outcome variable are equal. Preliminary analyses revealed that the data were overdispersed (i.e., the conditional variance was larger than the conditional mean), suggesting that the assumptions of Poisson regression were not met.

A random effects negative binomial model was therefore estimated (Hausman, 1984:926-928). Such a specification controls for the possibility of unobserved, time-stable individual effects through the inclusion of a random disturbance term. Thus, in contrast to the Poisson model, it allows for unexplained variation in supervision intensity across individuals (Gardner et al., 1995:399). The random disturbance term is assumed to be drawn from a two-parameter Gamma distribution, where the parameters of the Gamma distribution are in turn assumed to be drawn from a two-parameter beta distribution (Hausman, 1984:927). The models were estimated using LIMDEP (version 7.0) (Greene, 1995).

Results of the analyses using the total sample are shown in Table 24. The analyses suggested that a technical violation charge was associated with a statistically significant increase in supervision intensity across the four different measures of supervision intensity. Thus, subsequent to a technical violation charge, probationers appeared to have been subjected to more frequent contact with supervisory officers (personal and telephone) as well as more frequent monitoring checks and drug tests. Significant values of a and b (parameters of the beta distribution estimated from the data) confirmed the presence of significant unobserved individual differences.

Summary of Test 1

Logistic regression models predicting arrest during one-year of community supervision were estimated to test the signalling hypothesis. Technical violation charge indicators were expected to exert a positive and statistically significant effect on arrest if the results were to be consistent with the signalling hypothesis. Models were estimated using data from the total pooled sample of 10 jurisdictions and the three subsamples of more homogeneous sites. First, a combined technical violation and sanction indicator was used to control for the incapacitative effect of confinement prior to the first arrest. Second, indicators of either four or five different types of technical-violation charges (depending on the frequency distribution of different types of technical violations in the subsample) were used in place of the general technical violation charge/sanction measures. A multinomial logistic model also assessed whether the effect of technical violation charges varied across four types of arrests using data from the total pooled sample.

Contrary to the signalling hypothesis, technical violation charges appeared to be associated with a decrease (rather than increase) in the probability of arrest in the total sample and in the pooled California and Georgia subsamples. Because the effect was not limited to technical violation charges that resulted in a period of confinement, the relationship could not be simply explained by an incapacitation effect (i.e., the lack of opportunity to offend). In the "miscellaneous" site subsample, however, the effects of the dual indicators of technical violation charges and sanctions were not significant. Thus, the signalling hypothesis failed to receive empirical support in the analyses using the dual technical violation charge and sanction indicators.[59]

When indicators of technical-violation type replaced the dual indicator of technical violation charges/sanctions, the total sample and California and Georgia subsample analyses suggested that a technical-violation specific approach was largely unnecessary. With the exception of the absconding violation in the ten-site sample and curfew violations in the Georgia subsample, each type of technical

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[59]Preliminary analyses revealed that ISP probationers were more likely to be violated for a technical violation and more likely to be confined in response to a technical violation prior to their first arrest. Given that roughly fifty percent of the sample were participants in the intensive supervision program, the generalizability of the results to routine probation was therefore investigated. The total sample and the three subsamples were therefore divided into two subsamples each: an ISP subsample and a non-ISP subsample. The Test 1 models were run separately in each subsample in order to examine whether the relationship between technical violation charges and arrests was the same in the ISP subsamples and the non-ISP subsamples. Regression coefficients were compared following Clogg et al. (1995:1276):

$$z = \frac{b_1 - b_2}{[b_1(var) + b_2(var)]^{1/2}}.$$

Comparison of regression coefficients across the ISP and non-ISP subsamples revealed that technical violation charge estimates did not differ significantly across subsamples. The results suggested that the combination of ISP and non-ISP participants was justified and that the total sample results appear to be generalizable to either intensive or routine probation supervision.

violation was associated with a significant decrease in the probability of arrest. In the California subsample each type of technical violation charge was associated with a decrease in the probability of arrest.

However, when indicators of technical-violation type replaced the general measure of technical violation charges in the "miscellaneous" subsample, a technical-violation specific result emerged. In these four sites, drug/alcohol-related violations significantly decreased the probability of arrest consistent with the other six jurisdictions. Absconding violations, on the other hand, appeared to significantly increase the probability of arrest. Thus, in this one instance, the technical violation charge "signaled" arrest. The other two technical violation charge types were not statistically significant. Because the indicators of drug/alcohol-related violations and absconding violations were of approximately the same magnitude but of opposite sign, the effects appeared to have offset each other in the analysis using the technical violation charge and sanction indicators.

Absconding violations can be distinguished from other types of violations. Individuals who abscond abandon any pretense of community supervision. When it is determined that an individual has absconded, most probation/parole agencies issue arrest warrants. Although law enforcement agencies generally do not assign such warrants high priority, these individuals may be picked up as a result of a traffic stop or on suspicion of a criminal offense (Parent et al., 1992:xiii). The enhanced cooperation between some ISP programs and law enforcement agencies (Petersilia & Turner, 1990:30) may increase the probability of apprehending absconders.

Thus, an absconding violation may represent an anomaly to the signalling hypothesis. Instead of being arrested for the commission of new crimes, these individuals may have been arrested as a result of their absconder status. Investigation of this hypothesis revealed, however, that roughly 80% of the individuals in the miscellaneous sample who were violated for absconding and were subsequently arrested had been arrested for a property (46%), person (15%), or drug (19%) offense. Twenty percent (20%) of the sample was arrested for an offense classified as "other." Therefore, absconder status alone did not explain the increased probability of arrest.

In addition, arrest-specific analyses revealed that the effect of technical violation charges on arrest did not vary by arrest type (using the total sample). The log-likelihood value of a model restricting the effect of the technical violation charge/sanction indicators to be equal across the categories of the dependent variable did not differ significantly from the log-likelihood value of the model where the technical violation/charge indicators were permitted to vary across arrest types. Supervision intensity analyses additionally suggested that the intensity of four measures of supervision-related activities increased as a result of a technical violation charge.

In summary, the "signalling" hypothesis did not receive empirical support. Although the analyses established a strong, substantively important relationship between technical violation charges and arrest, the relationship was not in the predicted direction. As a potentially important exception to the rule, however, individuals who absconded from community supervision were more likely to be arrested in the pooled "miscellaneous" sample. This was not true, however, in the California subsample, where absconding violations were also associated with a decrease in the probability of arrest.

TEST 2

The purpose of Test 2 was to assess whether technical violation charges and arrests appear to emanate from the same underlying propensity or trait. Evidence that the same underlying propensity or trait drives both technical violation charges and arrests is consistent with a generality of deviance explanation of the signalling hypothesis. For purposes of this project, the underlying propensity is called Recidivism Propensity. Due to sample size limitations, Test 2 was conducted on the total pooled sample only.

Test 2 compared two multinomial logistic regression equations predicting a four-category recidivism outcome variable. A likelihood ratio test was used to determine whether a completely unrestricted model differed significantly from a restricted model, in which explanatory variables related to Recidivism Propensity were constrained to be equal across categories of the dependent variable. If the generality of deviance hypothesis is correct, the restricted model should not differ significantly from the unrestricted model because the underlying process driving each type of recidivism event is posited to be the same. Exploratory analyses also examined whether particular explanatory variables included in the recidivism model varied significantly across recidivism categories.

Model Construction. A four-category dependent variable capturing mutually exclusive recidivism outcomes was created (see Figure 2). The four categories included: (1) no recidivism; (2) technical violation charge only; (3) arrest only; and (4) technical violation charge and arrest. Variables related to recidivism were used as explanatory variables (e.g., demographic, criminal history, supervision intensity, and community activity variables). Descriptive statistics are shown in Table 25. Site control variables, exposure risk, and missing data-value indicators were also included as control variables. The risk and need assessment variables and drug-abuse related variables were not available in the total pooled sample.

The critical test involved the comparison of two multinomial logistic regression models. The first model allowed each explanatory variable to vary across categories of the dependent variable. The second model restricted the coefficients of variables related to Recidivism Propensity to be equal across categories of the dependent variable. All demographic, criminal history, and community activity variables were used as indicators of Recidivism Propensity. Equality restrictions were placed on the following variables: (1) race and ethnicity; (2) age; (3) marital status; (4) average number of days worked per month; (5) average number of counseling sessions attended per month; (6) number of prior misdemeanor convictions; (7) number of prior prison terms served; and (8) offense type. Site control variables, supervision intensity variables, and missing data-value indicators were allowed to vary because they did not represent stable individual-level characteristics or activities (e.g., employment) that were influenced at least in part by a stable individual characteristic.

Assessing the goodness-of-fit of a multinomial logistic regression model is more difficult than assessing the fit of a binary logistic regression model. Hosmer and Lemeshow (1989:232-233) recommend estimating the fit of separate binary logistic regression models for each logit function implied by the multinomial model. If the separate logistic regression equations yield satisfactory goodness-of-fit statistics, then these results taken together imply that the multinomial model fit is satisfactory.

Here, such a goodness-of-fit test would involve the estimation of the following separate models: (1) the technical violation charge-only outcome ($Y=1$) to no recidivism event ($Y=0$); (2) the arrest-only outcome ($Y=2$) to no recidivism event ($Y=0$); and (3) the technical violation and arrest outcome ($Y=3$) to no recidivism event ($Y=0$). However, the technical violation charge-only outcome and the arrest-only outcome failed to converge, due to zero cell counts in two of the Georgia sites. The Hosmer and Lemeshow test statistic computed on the technical violation charge and arrest outcome suggested that the model fit was satisfactory, $\chi^2(8, \underline{N}=562) = 4.61$, $\underline{p}=0.798$. The summary goodness-of-fit test was therefore inconclusive.[60]

Test 2 Results

Results of Model 1 and Model 2 are shown in Tables 26 and 27.[61] In order to test whether the equality restrictions were consistent with the data, the log-likelihood values of Model 1 and Model 2 were compared by means of the likelihood ratio test (with degrees of freedom equal to the number of equality restrictions imposed). The test statistic was statistically significant, $\chi^2(20, \underline{N}=1{,}037) = 66.10$, $\underline{p} < .001$ (see Table 28). The significant test statistic suggests that the unrestricted model was more consistent with the process that generated the data than the restricted model. Since the unrestricted model implies that the variables related to Recidivism Propensity exert different effects across categories

[60]Hosmer & Lemeshow "goodness-of-fit" statistics were computed for each model with the site control variables omitted. The results indicated that two of the separate models (arrest only and technical violation and arrest combined) fit satisfactorily ($\chi^2(8, \underline{N}=423) = 10.56$, $\underline{p}=0.228$ and χ^2 $(8, \underline{N}=562) = 7.18$, $\underline{p}=0.517$). The test statistic for the technical violation only model, however, was statistically significant ($\chi^2(8, \underline{N}=704) = 16.19$, $\underline{p}=0.040$).

[61]Note that in two sites (Macon and Waycross, Georgia), individuals did not experience one of the dependent variable outcomes. As a result, preliminary analyses revealed that the resulting estimated coefficients and standard errors for the site control variable on the missing recidivism outcome were extremely inflated. One individual in each site was therefore randomly assigned to the missing dependent variable outcome. (An individual in Macon, Georgia was randomly assigned to the 'No Recidivism Event' outcome and an individual in Waycross, Georgia was randomly assigned to the 'Arrest-only' outcome.) The random assignment of each case to the missing outcome measure did not materially impact the final results.

of the dependent variable, the results of the analyses were inconsistent with the generality of deviance hypothesis.

Exploratory analyses were conducted next to identify which particular variables (or combination of variables) accounted for the difference in the overall log-likelihood values. Three additional models subject to different combinations of equality restrictions were estimated. The three sets of restrictions included: (1) equality restrictions placed on the three demographic variables (age, marital status, and nonwhite) and the two community activity variables (employment and counseling); (2) equality restrictions placed on the two criminal history measures (the number of prior misdemeanor convictions and the number prison terms served); and (3) equality restrictions placed on three dummy variables representing offense type (i.e., person offense, property offense, drug offense, and "other" offense). An overview of the equality restrictions imposed and the resulting χ^2 test statistic values are shown in Table 28.

Comparison of the model subjected to five restrictions (the three demographic characteristics and the two community activity variables) and the unrestricted model indicated that the model containing equality restrictions did not differ significantly from the unrestricted model (see Table 28). The test statistic was not significant at the $p < .05$ level. On the other hand, comparison of the models containing the criminal history and offense type equality restrictions revealed that both restricted models differed significantly from the unrestricted model (see Table 28).

Examination of the criminal history and offense type coefficients from the unrestricted model revealed that the effects of those variables varied across categories of the dependent variable. Broadly speaking, both criminal history measures were associated with an increase in the probability of arrest for both the technical violation-only and technical violation and arrest categories, but not the arrest-only category. Similarly, each type of conviction offense was associated with a decrease in the probability of arrest (in comparison to the "other" reference category) for both the technical violation-only and the technical violation and arrest categories, but not the arrest-only category.

In summary, the premise that technical violation charges and arrests are manifestations of the same underlying propensity or syndrome (i.e., Recidivism Propensity) was formally rejected. Likelihood ratio tests comparing multinomial logistic regression models revealed that a model that allowed explanatory variables related to Recidivism Propensity to vary across categories of the dependent variable was more consistent with the data than a model where the explanatory variables were restricted to be equal. Exploratory analyses suggested that while demographic characteristics (i.e., age, nonwhite, and marital status) as well as community-related activities (i.e., employment and counseling sessions attended) appeared to be relatively constant across categories of the dependent variable, the effect of criminal history and offense type varied across recidivism outcomes.

CHAPTER VI
DISCUSSION

Does the first technical violation charge "signal" the first arrest during community supervision? This research suggests that it does not. The results of two statistical tests intended to examine the relationship between technical violation charges and arrest were inconsistent with predictions derived from the signalling hypothesis.

The first test examined whether technical violation charges predict criminal recidivism (i.e., arrest) with better than chance accuracy. Rather than exerting a positive and statistically significant effect on arrest as the signalling hypothesis would anticipate, technical violation charges were associated with a <u>decrease</u> in the probability of arrest in most analyses.[62] In other words, probationers who were charged with a technical violation appeared to be less likely to be arrested during the course of the one-year followup period, regardless of whether the technical violation charge resulted in confinement. Joint technical violation charge/sanction indicators were used to control for the possibility that incapacitation in response to

[62]In the total pooled sample and the California and Georgia subsamples, the relationship between technical violation charges and arrests was negative and statistically significant. The technical violation charge and sanction measures were not statistically significant in the pooled miscellaneous sample, however.

a technical violation limited the opportunity to recidivate (which would also result in an inverse relationship between technical violation charges and arrests).

Further analyses suggested that the impact of technical violation charges on different types of arrest was quite similar. That is, technical violation charges did not appear to be any more or less likely to signal a particular type of arrest (e.g., a drug-related arrest or a property arrest).

Likewise, in the total pooled sample and the California and Georgia subsamples, different types of technical violation charges exerted similar effects on arrest. Generally speaking, each category of technical violation charge was inversely related to arrest. A technical violation-specific pattern emerged, however, in the analysis of the pooled miscellaneous subsample. Alcohol and drug-related violations were associated with a decrease in the probability of arrest whereas absconding violations were associated with an increase in the probability of arrest. Thus, in this one instance absconding violations appeared to signal arrest. While it is possible to distinguish absconding violations from other types of violations because they trigger enhanced law enforcement action (i.e., arrest warrants), the majority of individuals who were charged with an absconding violation in the pooled miscellaneous sample were arrested for crimes that were unrelated to their absconder status.

The signalling hypothesis was also examined indirectly by assessing whether technical violation charges and arrests appeared to be indicators of the same underlying propensity to offend or deviate consistent with a generality of deviance understanding of the signalling hypothesis. Formal comparison of multinomial logistic regression models suggested that a model that allowed the explanatory variables related to Recidivism Propensity to exert different effects across categories of the dependent variable (i.e., technical violation charge only, arrest only, technical violation charge and arrest) was more consistent with the data than a model that restricted the effects of the explanatory variables to be equal across categories of recidivism. Exploratory analyses further revealed that the effects of criminal history and offense type varied significantly across categories of the dependent variable, while demographic and community activity variables were relatively constant across categories of the dependent variable. Thus, the generality of deviance hypothesis did not appear to be consistent with the process that generated the data.

The findings of the present study are not easily integrated with prior research. For example, while Petersilia and Turner reported that technical violation charges and arrests were not significantly related (using data from the three California jurisdictions), the present research uncovered an inverse and statistically significant relationship. The present research, however, extended Petersilia and Turner's research by accounting for the temporal order of technical violations and arrests (i.e., technical violations were required to precede arrests in the analyses). It also adjusted for other factors related to recidivism, including the possibility of confinement subsequent to a technical violation charge.[63]

With regard to the pretrial drug testing literature, the findings of such research generally suggest that pretrial releasees who fail to comply with the mandates of a pretrial drug testing program (i.e., fail to appear for a scheduled drug test and/or test positive) are <u>more</u> likely to be arrested or fail to appear for a court proceeding (adjusting for other risk factors). Compliance with pretrial drug testing program requirements has therefore been interpreted as a "signal" that individuals are less likely to engage in pretrial misconduct.

In contrast, this research found that individuals who fail to comply with release conditions are <u>less</u> likely to be arrested. Of course, the emphasis in the pretrial drug testing literature was on drug-related violations. However, when technical violation charges were disaggregated by technical violation charge type, alcohol and drug-related violation remained inversely related to arrest (even in the pooled miscellaneous sample). Thus, the findings from pretrial drug testing programs do not appear to extend to post-conviction probation supervision -- even when attention focuses exclusively on alcohol or drug-related violations.

The discrepant results may arise from differences in the population studied

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[63]It should be noted that Petersilia and Turner excluded technical violations classified as "Violation of Probation/Parole" from their published analyses. Per personal communication, the researchers decided not to include them in the analyses because they were deemed "garbage" technicals or duplicates. According to the technical violation sanction code attached to this class of technical violations, however, a substantial portion of these technical violations were sanctioned with a period of confinement. Therefore, absent a compelling reason to exclude this category of technical violations, they were included in the present analyses.

or the nature of the criminal justice program (e.g., more or less stringent sanctioning policies in response to violations). Both the present study and the pretrial drug-testing research relied on official record recidivism data and adjusted for other relevant factors related to recidivism.

Petersilia and Turner concluded on the basis of their research that technical violation charges did not appear to suppress arrests. Petersilia (1994:171) later postulated on the basis of the same findings that the commission of technical violations may not necessarily imply the commission of new crimes. That is, some probationers may tend to commit technical violations while other probationers may tend to commit violations of the law. As such, Petersilia set forth a specialization argument. The inverse and statistically significant relationship between technical violation charges and arrests discovered here is certainly consistent with probationer specialization. A specialization argument is also consistent with the results of Test 2 in that different types of recidivism did not appear to be generated by the same underlying process. In other words, the factors that were influential in explaining one category of recidivism (e.g., arrest) were not necessarily influential in explaining another category of recidivism (e.g., technical violation charge), thereby suggesting specialization.

Probationer specialization is not the only explanation for the inverse relationship between technical violation charges and arrests, however. It is also possible that the inverse relationship springs at least in part from a deterrent process. Probationers who are charged with a technical violation may perceive that they are being watched more closely and as a consequence refrain from committing criminal offenses for fear of getting caught. Analyses examining the effect of a technical violation charge on the intensity of supervision suggested that supervision intensity did in fact appear to increase subsequent to a technical violation as measured by the monthly frequency of personal and telephone contacts, monitoring checks, and drug and alcohol tests taken.

In addition, roughly fifty percent of the individuals in the sample were supervised in the ISP program. Deterrence is of course a major goal of ISP programs. A survey of 22 ISP probationers and 22 routine probationers supervised in Contra Costa, California revealed that ISP probationers generally believed that they were supervised more strictly, were more likely to be caught for a technical violation, and would receive more punitive treatment in response to a violation than routine probationers (Buck, 1989:72). There is no evidence to suggest, however, that such perceptions affect behavior. Petersilia and Turner's evaluation of the effectiveness of ISP programs using official record data clearly demonstrated that ISP participation does not affect criminal recidivism. Similarly, evaluations of pretrial drug testing programs that relied on random assignment to treatment (i.e., drug testing program) and control conditions failed to provide evidence of a deterrent effect.

In short, while the supervision intensity analyses provide preliminary evidence consistent with a deterrence explanation, they are obviously far from conclusive. The data available in the present study are simply inadequate to provide further empirical insight into the process driving the relationship between technical violation charges and arrest.

Viewed from a criminal justice policy perspective, however, the finding that technical violation charges do not appear to signal criminal recidivism during probation supervision is instructive. The result suggests that probation system policies that remove probationers from community supervision upon evidence of a technical violation may not achieve their desired effect: an increase in public safety. Individuals who are charged with a technical violation do not have a higher probability of arrest. Such policies (probation revocation in conjunction with a period of confinement) also come at a substantial expense to corrections systems. Not only are they costly financially (e.g., the cost of incapacitation), but they are also costly in terms of staff resources (e.g., supervisory staff forced to spend increasing amounts of time on revocation-related paperwork and procedure).

While tough sanctions in response to noncompliance are clearly intended to achieve other correctional goals (i.e., retribution, system credibility), the efficacy of such a policy is debatable given the strain on correctional resources and the questionable impact on public safety. The growing costs associated with probation and parole revocation have in fact prompted many jurisdictions to reexamine their revocation policies and to implement intermediate sanction systems for probation/parole violators (Rhine & Humphries, 1993:101). Examples of intermediate sanctions (short of revocation) include home confinement or curfew, community service, short-term confinement, day reporting centers, and electronic monitoring.

Due to the vast amount of discretion typically involved in responding to probationer noncompliance, some correctional scholars call for the development of formal policy to govern sanctioning behavior. Formal policy would classify the severity of different types of violations (i.e., minor versus major violations), specify a range of appropriate responses, and detail the rationale underlying those responses (Rhine & Humphries, 1993:102).

In summary, the signalling hypothesis failed to receive empirical support during probation supervision in this study. The inverse and statistically significant relationship between technical violation charges and arrests may be attributed to probationer specialization patterns or to a deterrence process. The data do not permit more than informed speculation about the nature of the process driving the relationship. Given the results of both Test 1 and Test 2, however, it is unlikely that technical violation charges and criminal recidivism are manifestations of the same underlying propensity or syndrome as suggested by a generality of deviance hypothesis.

The present research was limited in several respects. First, the research relied on official records of technical violation charges and criminal recidivism. Use of official record data confounds probationer behavior with the behavior of law enforcement agencies such as the police or probation departments. Supervision intensity, for example, may effect the probability of detecting rule and law violations. Alternatively, probation officer discretion may effect whether an individual is charged with a violation.

The collection of self-report data would circumvent this problem by inquiring about criminal offending and rules violations independent of the reactions of law enforcement agencies. The relationship between self-report technical violations and self-report offending could then be compared to analyses using official record data. Self-report data would also enable researchers to assess the effect of sanctions imposed in response to technical violations. For example, does the imposition of a sanction in response to a technical violation decrease the probability of further violations (either rules violations or criminal offending)? Such an approach would provide a means to further investigate the inverse relationship between technical violation charges and arrest (and the possibility of a deterrent effect) discovered here. The effect of different types of sanctions on the probability of noncompliance could also be investigated.

In order to examine the relationship between technical violation charges and arrest, the possibility of an incapacitative effect must be taken into account. In other words, analyses must adjust for the opportunity to recidivate subsequent to a technical violation. Data limitations in this study resulted in a rather crude control for the possibility of an incapacitation effect (a dichotomous indicator of confinement prior to the first arrest). In order to adjust more precisely for the possibility of an incapacitative effect, it would be necessary to record exactly when and for how long an individual was confined during the followup period (i.e., the specific dates).

A third limitation related to the temporal order of recidivism events. Establishing the correct temporal order is critical to a test of the signalling hypothesis. The present study made use of only the first technical violation charge and first arrest in order to ensure that technical violation charges preceded arrests. However, some probationers committed multiple technical violations and were arrested more than one time over the course of the one-year followup. In order to sort the temporal order of multiple technical violation charges and arrests, it would be necessary to collect data that established the precise timing of recidivism events in relation to each other and importantly to any time spent incarcerated.

Tables

Table 1. Site and Sample Descriptive Statistics in Three Georgia Jurisdictions (Atlanta, Macon, Waycross).

Variable	Atlanta N=50	Macon N=50	Waycross N=50
Site Characteristics••			
Type of ISP•	Probation Enhancement	Probation Enhancement	Probation Enhancement
Target Group	High-need/low-risk felons with history of drugs	High-need/low-risk felons with history of drugs	High-need/low-risk felons with history of drugs
ISP Emphasis	Passive electronic monitoring	Active electronic monitoring	Treatment referrals
Sample Characteristics			
Age X (SD)	27.92 (7.85)	27.03 (6.12)	26.34 (4.99)
Sex (% Male)	41 (82.0)	42 (84.0)	45 (90.0)
Race/ethnicity N (%)			
White	12 (24.0)	20 (40.0)	33 (66.0)
Black	38 (76.0)	30 (60.0)	17 (34.0)
Hispanic	--	--	--
Other	--	--	--
Education N (%)			
< High school degree	32 (64.0)	25 (50.0)	30 (60.0)
>= High school degree	18 (36.0)	25 (50.0)	20 (40.0)
Married N (%)	9 (18.0)	6 (12.2)	14 (28.0)
# Prior Arrests X (SD)	3.33 (2.79)	5.12 (3.56)	6.30 (5.06)
# Prior Prison Terms N (%)			
0 Prior Prison	37 (77.1)	42 (84.0)	46 (93.9)
1 Prior Prison	7 (14.6)	3 (6.0)	1 (2.0)
>=2 Prior Prison	4 (8.3)	5 (10.0)	2 (4.1)
Conviction Offense			
Violent	5 (10.0)	2 (4.0)	0 (--)
Burglary/theft	4 (8.0)	2 (4.0)	2 (4.0)
Drug sale/possession	25 (50.0)	13 (26.0)	1 (2.0)
Other	16 (32.0)	33 (66.0)	47 (94.0)
Evidence of Drug Dependency (N, % Yes)	18 (36.0)	20 (40.0)	21 (42.0)

•• Source: Petersilia & Turner, 1993, pp. 294-296 • Intensive Supervision Program

Table 2. Site and Sample Descriptive Statistics in Three California Jurisdictions (Contra Costa, Los Angeles, Ventura).

Variable	Contra Costa N=170	Los Angeles N=152	Ventura N=166
Site Characteristics••			
Type of ISP•	Probation Enhancement	Probation Enhancement	Probation Enhancement
Target Group	Probationers convicted of felony or misd. drug offenses	High-risk probationers	High-risk probationers
ISP Emphasis	Drug testing	Active electronic monitoring	Police coordination, job training
Sample Characteristics			
Age X (SD)	26.51 (6.71)	29.10 (9.06)	29.67 (9.18)
Sex (% Male)	137 (80.6)	132 (87.4)	141 (85.5)
Race/ethnicity N (%)			
White	30 (17.6)	5 (3.3)	79 (47.9)
Black	135 (79.4)	130 (86.1)	24 (14.5)
Hispanic	5 (2.9)	16 (10.6)	59 (35.8)
Other	--	--	3 (1.8)
Education N (%)			
< High school degree	97 (57.1)	86 (57.7)	104 (63.4)
>= High school degree	73 (42.9)	63 (42.3)	60 (36.6)
Married N (%)	19 (11.2)	10 (6.6)	41 (25.0)
# Prior Arrests X (SD)	5.55 (6.79)	6.96 (7.99)	6.54 (6.94)
# Prior Prison Terms N (%)			
0 Prior Prison	161 (94.7)	115 (76.2)	136 (82.4)
1 Prior Prison	8 (4.7)	23 (15.2)	14 (8.5)
>=2 Prior Prison	1 (0.6)	13 (8.6)	15 (9.1)
Conviction Offense			
Violent	13 (7.7)	23 (15.3)	47 (28.5)
Burglary/theft	125 (74.0)	35 (23.3)	53 (32.1)
Drug sale/possession	30 (17.8)	88 (58.7)	60 (36.4)
Other	1 (0.6)	4 (2.7)	5 (3.0)
High Drug Treatment Needs•• (N, % Yes)	65 (41.9)	57 (40.7)	82 (52.6)

•• Source: Petersilia & Turner, 1993, pp. 294-296. • Intensive Supervision Program •• Drug dependency data were not collected in the California sites. Instead the percentage of probationers ranked in high need of treatment as part of a Risk/Needs Assessment is presented.

Table 3. Site and Sample Descriptive Statistics in Des Moines, Iowa and Santa Fe, New Mexico.

Variable	Des Moines, Iowa N=115	Santa Fe, New Mexico N=58
Site Characteristics••		
Type of ISP•	Probation/Parole Enhancement	Probation/Parole Enhancement
Target Group	Probationers and Parolees convicted of drug offenses or drug-involved burglars	Probationers and parolees with high-risk/needs and drug dependent
ISP Emphasis	Active electronic monitoring	Counseling, employment
Sample Characteristics		
Age X (SD)	30.07 (8.81)	30.44 (7.22)
Sex (% Male)	86 (74.8)	51 (87.9)
Race/ethnicity N (%)		
White	73 (64.6)	7 (12.1)
Black	40 (35.4)	--
Hispanic	--	51 (87.9)
Other	--	--
Education N (%)		
< High school degree	53 (49.5)	26 (44.8)
>= High school degree	54 (50.5)	32 (55.2)
Married N (%)	23 (20.5)	13 (22.4)
# Prior Arrests X (SD)	6.62 (7.80)	8.05 (7.20)
# Prior Prison Terms N (%)		
0 Prior Prison	57 (51.8)	38 (66.7)
1 Prior Prison	23 (20.9)	11 (19.3)
>=2 Prior Prison	30 (27.3)	8 (14.0)
Conviction Offense		
Violent	4 (3.5)	5 (8.6)
Burglary/theft	80 (70.2)	15 (25.9)
Drug sale/possession	21 (18.4)	9 (15.5)
Other	9 (7.9)	29 (50.0)
Evidence of Drug Dependency (N, % Yes)	108 (93.9)	54 (93.1)

•• Source: Petersilia & Turner, 1993, pp. 294-296

• Intensive Supervision Program

Table 4. Site and Sample Descriptive Statistics in Seattle, Washington and Winchester, Virginia.

Variable	Winchester, Virginia N=53	Seattle, Washington N=173
Site Characteristics••		
Type of ISP•	Probation/Parole Enhancement	Probation Enhancement
Target Group	Probationers and parolees with drug-related conviction and/or drug abuse history	Probationers convicted of drug-related offenses and drug dependent
ISP Emphasis	Substance abuse evaluation and outpatient treatment	Surveillance, treatment referrals
Sample Characteristics		
Age X (SD)	26.89 (6.69)	30.37 (8.75)
Sex (% Male)	43 (81.1)	127 (73.4)
Race/ethnicity N (%)		
White	51 (29.5)	35 (66.0)
Black	111 (64.2)	18 (34.0)
Hispanic	7 (4.0)	--
Other	4 (2.3)	--
Education N (%)		
< High school degree	96 (57.5)	34 (65.4)
>= High school degree	71 (42.5)	18 (34.6)
Married N (%)	15 (9.1)	9 (17.0)
# Prior Arrests X (SD)	9.04 (9.35)	9.14 (9.82)
# Prior Prison Terms N (%)		
0 Prior Prison	39 (76.5)	135 (78.5)
1 Prior Prison	6 (11.8)	23 (13.4)
>=2 Prior Prison	6 (11.8)	14 (8.1)
Conviction Offense		
Violent	2 (3.8)	12 (6.9)
Burglary/theft	7 (13.2)	43 (24.9)
Drug sale/possession	13 (24.5)	117 (67.6)
Other	31 (58.5)	1 (0.6)
Evidence of Drug Dependency (N, % Yes)	40 (75.5)	151 (87.3)

•• Source: Petersilia & Turner, 1993, pp. 294-296 • Intensive Supervision Program

Table 5. Research Variables and Variable Attributes.

Variable	Variable Attributes	
Recidivism Measures		
Arrest	1 = 1 or more arrests 0 = 0 arrests	
Arrest Type*	0 = no arrest 1 = "other" arrest 2 = person arrest 3 = property arrest 4 = drug arrest	
Technical Violation	1 = 1 or more technical violation charges** 0 = 0 technical violation charges	
Technical Violation Type:*		
Failure-to-Report	1 = failure-to-report charge;	0 = 0 failure-to-report charges
Drug/Alcohol Violation	1 = drug/alcohol charge;	0 = 0 drug/alcohol charges
Abscond	1 = abscond charge;	0 = 0 abscond charges
Curfew Violation	1 = curfew charge;	0 = 0 curfew charges
Other Violation	1 = "other" charge;	0 = 0 "other" charges
Test 2 Recidivism Measure	0 = no recidivism event 1 = technical violation charge only 2 = arrest only 3 = technical violation charge and arrest	
Time-at-Risk		
Exposure Risk	Maximum possible # days "at-risk" in community (1 year less time-served as part of current sentence)	
Confinement	1 = confined in response to technical violation 0 = not confined in response to technical violation	

* Arrest type corresponds to the first arrest. Technical violation type corresponds to the first technical violation charge.

** Among invividuals who are charged with a technical violation and arrested, technical violation only equals 1 if the technical violation charge preceded the arrest.

Table 5--Continued.

Variable	Variable Attributes	
Supervision Intensity Measures*		
Personal Contacts	Average # of monthly personal contacts	
Telephone Contacts	Average # of monthly telephone contacts	
Monitoring Checks	Average # of monthly monitoring checks	
Drug/Alcohol Tests Taken	Average # of monthly drug/alcohol tests taken	
Positive Drug Tests**	Average # of monthly positive drug tests	
Community Activity Measures*		
Employment	Average # of days worked per month (20 days=full-time)	
Counseling	Average # of counseling sessions attended per month	
Demographic Variables		
Sex	1 = male 0 = female	
Age	Age in years at onset of probation/parole term.	
Race/ethnicity	1 = nonwhite 0 = white	
Education	1 = high school/GED or greater 0 = less than high school diploma/GED	
Marital Status	1 = married (including common law) 0 = single, divorced, widowed, separated	
Drug Dependence**	1 = evidence of drug dependency (excluding marijuana/hashish) 0 = no evidence of drug dependency	
Offense Type		
Person Offense	1 = convicted person offense;	0 = otherwise
Property Offense	1 = convicted property offense;	0 = otherwise
Drug-Related Offense	1 = convicted drug-related offense;	0 = otherwise
"Other" Offense	1 = convicted "other" offense;	0 = otherwise

* Monthly rates were calculated prior to and including the month of first arrest (if arrested). Among individuals who were not arrested, monthly rates were calculated across the entire followup period.

** Available in seven sites that collected drug abuse data (Atlanta, Macon, Waycross, Des Moines, Santa Fe, Winchester, and Seattle).

Table 5--Continued

Variable	Variable Attributes
<u>Criminal History Variables</u>	
Prior Arrests	Number of prior arrests
Felony Convictions	Number of prior felony convictions
Misdemeanor Convictions	Number of prior misdemeanor convictions
Prison Terms Served	Number of prior prison terms served
Jail Terms Served	Number of jail terms served
<u>Risk and Need Assessment Variables</u>	
Number of Address Changes	1 = no address changes in last 12 months 2 = 1 address change in last 12 months 3 = 2 or more address changes in last 12 months
Academic/Vocation Training	1 = high school or above skill level (no need) 2 = adequate skills; able to handle every day requirements (low need) 3 = low skill level causing serious adjustment problems (moderate need) 4 = Minimal skill level causing serious adjustment problems (high need)
Employment Assistance	1 = Satisfactory employment for one year or longer (no need) 2 = Secure employment; no difficulties reported; homemaker, student, or retired (low need) 3 = Unsatisfactory employment; or unemployed but has adequate job skills (moderate need) 4 = Unemployed and virtually unemployable; needs training (high need)
Alcohol Treatment Needs	1 = No interference with functioning (no need) 2 = Occasional abuse; some disruption in functioning (low/moderate need) 3 = Frequent abuse; serious disruption; needs treatment (high need)
Other Drug Treatment Needs	1 = No interference with functioning (no need) 2 = Occasional abuse; some disruption in functioning (low/moderate need) 3 = Frequent abuse; serious disruption; needs treatment (high need)

Table 6. Bivariate Relationship Between Technical Violation Charges and Arrests in the Total Sample and Three Subsamples.

	Arrest		
	Yes N (%)	No N (%)	
Total Sample (N=1,037)			
Technical Violation:			
Yes	144 (27.59)	378 (72.41)	X^2_1 = 9.875***
No	189 (36.70)	326 (63.30)	
	r• = -0.098***		
California Sites (N=488)			
Technical Violation:			
Yes	55 (23.21)	182 (76.79)	X^2_1 = 26.566****
No	114 (45.52)	137 (54.48)	
	r = -0.233****		
Georgia Sites (N=150)			
Technical Violation:			
Yes	22 (24.44)	68 (75.56)	X^2_1 = 1.959
No	9 (15.00)	51 (85.00)	
	r = 0.114		
Miscellaneous Sites (N=399)			
Technical Violation:			
Yes	67 (34.36)	128 (65.64)	X^2_1 = 0.181
No	66 (32.35)	138 (67.65)	
	r = 0.021		

• Pearson correlation coefficient

* p<.10   **p<.05   ***p<.01   ****p<.001

Table 7. First Technical Violation Charge Type and First Arrest Type.

	Total Sample N=1037	California Subsample N=488	Georgia Subsample N=150	Misc. Subsample N=399
Technical Violation Type	N=522 (50.3)	N=237 (48.6)	N=90 (60.0)	N=195 (48.9)
Curfew Violation	26 (5.0)	3 (1.3)	22 (24.4)	1 (0.5)
Failure-to-Report	111 (21.3)	83 (35.0)	5 (5.6)	23 (11.8)
Dirty Drug Tests	96 (18.4)	39 (16.5)	18 (20.0)	39 (20.0)
Dirty Alcohol Tests	18 (3.4)	4 (1.7)	13 (14.4)	1 (0.5)
Other Drug Violation	13 (2.5)	8 (3.4)	3 (3.3)	2 (1.0)
Other Alcohol Violation	3 (0.6)	1 (0.4)	--	2 (1.0)
Employment/School	1 (0.2)	1 (0.4)	--	--
Community Service	2 (0.4)	--	2 (2.2)	--
Fines/Fees	5 (1.0)	--	5 (5.6)	--
Treatment Violation	11 (2.1)	2 (0.8)	--	9 (4.6)
Absconded	78 (14.9)	20 (8.4)	5 (5.6)	53 (27.2)
Other Violation	36 (6.9)	27 (11.4)	3 (3.3)	6 (3.1)
Probation/Parole Violation	114 (21.8)	44 (18.6)	14 (15.6)	56 (28.7)
Violation of Prob/Parole••	4 (0.8)	1 (0.4)	--	3 (1.5)
Failure-to-Report for Drug Tests	4 (0.8)	2 (1.7)	--	--
Arrest Type	N=333 (32.1)•	N=169 (34.6)•	N=31 (20.7)	N=133 (33.3)
Rape	1 (0.3)	--	--	1 (0.8)
Robbery	16 (4.8)	10 (6.0)	1 (3.2)	5 (3.8)
Assault	35 (10.6)	15 (9.0)	3 (9.7)	17 (12.8)
Other Sex Offense	1 (0.3)	--	--	1 (0.8)
Burglary	19 (5.7)	8 (4.8)	1 (3.2)	10 (7.5)
Theft	37 (11.2)	14 (8.4)	3 (9.7)	20 (15.0)
Forgery/counterfeiting/fraud	7 (2.1)	2 (1.2)	2 (6.5)	3 (2.3)
Receiving Stolen Property	9 (2.7)	3 (1.8)	2 (6.5)	4 (3.0)
Weapons (carrying, possession)	13 (3.9)	6 (3.6)	1 (3.2)	6 (4.5)
Vandalism	2 (0.6)	--	--	2 (1.5)
Other Property Offenses	15 (4.5)	6 (3.6)	2 (6.5)	7 (5.3)
Possession Narcotics or Controlled Substance	47 (14.2)	27 (16.2)	6 (19.4)	14 (10.5)
Sale/Transportation Narcotics	27 (8.2)	17 (10.2)	3 (9.7)	7 (5.3)
Other Drug Offenses	18 (5.4)	13 (7.8)	2 (6.5)	3 (2.3)
Prostitution/Commercial Vice	6 (1.8)	3 (1.8)	--	3 (2.3)
Driving Under Influence	22 (6.6)	10 (6.0)	--	12 (9.0)
All other Offenses	56 (16.9)	33 (19.8)	5 (16.1)	18 (13.5)

• Note: This figure represents the total number of arrests. Two arrest incidents are missing an arrest type code. •• Category includes probation revocation.

Table 8. Test 1 Descriptive Statistics in the Total Pooled Sample (N=1,037).

Variable	Mean (S.D.)	Median	Minimum	Maximum
Site 1 (Atlanta, GA)	0.048 (0.214)	0	0	1
Site 2 (Macon, GA)	0.048 (0.214)	0	0	1
Site 3 (Waycross, GA)	0.048 (0.214)	0	0	1
Site 4 (Contra Costa, CA)	0.164 (0.370)	0	0	1
Site 5 (Los Angeles, CA)	0.147 (0.354)	0	0	1
Site 6 (Ventura, CA)	0.160 (0.367)	0	0	1
Site 7 (Des Moines, IA)	0.111 (0.314)	0	0	1
Site 8 (Santa Fe, NM)	0.056 (0.230)	0	0	1
Site 9 (Winchester, VA)	0.051 (0.220)	0	0	1
Site 10 (Seattle, WA)	0.167 (0.373)	0	0	1
Exposure Risk	347.019 (36.805)	365	24	365
Arrest	0.321 (0.467)	0	0	1
Technical Violation	0.503 (0.500)	1	0	1
Tech. w/Confinement	0.313 (0.464)	0	0	1
Tech. w/o Confinement	0.190 (0.392)	0	0	1
Technical Violation Type:				
Failure-to-Report	0.107 (0.309)	0	0	1
Drug/Alcohol Violation	0.129 (0.336)	0	0	1
Abscond	0.075 (0.264)	0	0	1
Curfew	0.025 (0.156)	0	0	1
Other	0.167 (0.373)	0	0	1
Male	0.817 (0.387)	1	0	1
Nonwhite	0.667 (0.471)	1	0	1
Age	28.760 (8.208)	26.984	14.493	88.038
Married	0.155 (0.360)	0	0	1
High School degree	0.427 (0.490)	0	0	1
ISP Sample Membership	0.529 (0.499)	1	0	1
Personal Contacts*	3.583 (5.155)	1.750	0	35.25
(Log)	1.104 (0.863)	1.012	0	3.590
Phone Contacts*	0.853 (1.520)	0.250	0	17.667
(Log)	0.444 (0.517)	0.223	0	2.927
Monitoring Checks*	3.945 (6.096)	1.500	0	37.429
(Log)	1.128 (0.906)	0.916	0	3.649
Monthly Days Worked*	4.214 (6.245)	0	0	25
(Log)	0.954 (1.158)	0	0	3.258
Counseling Sessions*	1.350 (3.304)	0	0	33.500
(Log)	0.450 (0.750)	0	0	3.541
# Misd. Convictions	3.109 (4.528)	2	0	38
# Prison Terms Served	0.376 (0.891)	0	0	7
Supervision Missing Data	0.101 (0.302)	0	0	1
Work Missing Data Flag	0.385 (0.487)	0	0	1

* Due to the skewed distribution of the variable, the raw and natural logarithm transformation are presented. The variable represents the monthly average prior to the first arrest. If the individual was not arrested during the one-year followup period, the variable represents the monthly average over the followup period.

Table 9. Logistic Regression Model Predicting Arrest in the Total Pooled Sample.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	-0.425	0.856	-0.496
Exposure Risk	0.002	0.002	0.847
Site 1 (Atlanta, GA)	-2.140	2.190	-0.977
Site 2 (Macon, GA)	-2.247	1.973	-1.139
Site 3 (Waycross, GA)	-0.497	1.587	-0.313
Site 4 (Contra Costa, CA)	-0.753	0.463	-1.628
Site 5 (Los Angeles, CA)	0.310	0.472	0.656
Site 6 (Ventura, CA)	1.197	0.540	2.219**
Site 7 (Des Moines, IA)	-1.501	0.734	-2.046**
Site 8 (Santa Fe, NM)	1.315	1.886	0.698
Site 9 (Winchester, VA)	-5.322	2.343	-2.271**
Site 10 (Seattle, WA)	--	--	--
Technical w/Confinement	-1.003	0.208	-4.824****
Technical w/o Confinement	-1.042	0.236	-4.420****
Age	-0.040	0.011	-3.682****
Avg. # Phone Contacts[a]	0.898	0.302	2.974***
Personal contacts in Site 1[a]	1.063	1.277	0.832
Personal contacts in Site 2[a]	0.310	1.025	0.302
Personal contacts in Site 3[a]	-1.986	1.507	-1.318
Personal contacts in Site 4[a]	-0.177	1.053	-0.168
Personal contacts in Site 5[a]	-0.527	0.915	-0.576
Personal contacts in Site 6[a]	0.612	0.935	0.654
Personal contacts in Site 7[a]	0.996	1.190	0.837
Personal contacts in Site 8[a]	5.377	1.755	3.063****
Personal contacts in Site 9[a]	5.638	2.782	2.026**
Monitoring Checks in Site 1[a]	-3.598	2.042	-1.762**
Monitoring Checks in Site 2[a]	1.775	1.517	1.170
Monitoring Checks in Site 3[a]	1.637	1.724	0.949
Monitoring Checks in Site 4[a]	2.232	1.058	2.110**
Monitoring Checks in Site 5[a]	0.967	1.055	0.917
Monitoring Checks in Site 6[a]	0.199	0.937	0.212
Monitoring Checks in Site 7[a]	0.400	1.186	0.337
Monitoring Checks in Site 8[a]	-1.547	1.241	-1.247
Monitoring Checks in Site 9[a]	-0.666	1.843	-0.361
Supervision Missing Data Flag	-0.217	0.773	-0.280

(Continued on next page)

[a] Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 9--Continued.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Avg. # Days Worked Per Month•	-0.539	0.102	-5.296****
Days Worked Missing Data Flag	-0.276	0.195	-1.415
Counseling Sessions in Site 1•	-0.656	1.623	-0.404
Counseling Sessions in Site 2•	0.468	0.671	0.697
Counseling Sessions in Site 3•	1.473	1.333	1.105
Counseling Sessions in Site 4•	-4.209	1.655	-2.544**
Counseling Sessions in Site 5•	-15.563	11.042	-1.409
Counseling Sessions in Site 6•	-0.631	0.611	-1.032
Counseling Sessions in Site 7•	0.228	0.640	0.356
Counseling Sessions in Site 8•	-1.622	0.879	-1.844*
Counseling Sessions in Site 9•	0.570	0.775	0.735
Counseling Missing Data Flag	0.474	0.750	0.633
# Prior Misdemeanor Convictions	0.085	0.020	4.264****
# Prior Prison Terms Served	0.372	0.095	3.931****
ISP in Site 1	3.093	2.164	1.429
ISP in Site 2	0.106	0.969	0.110
ISP in Site 3	-0.508	1.168	-0.435
ISP in Site 4	-1.200	0.995	-1.206
ISP in Site 5	-0.143	0.905	-0.159
ISP in Site 6	-1.298	0.962	-1.350
ISP in Site 7	-1.035	0.935	-1.107
ISP in Site 8	-3.931	1.693	-2.322**
ISP in Site 9	-3.677	2.532	-1.452
Log-likelihood	-495.873		
N=1037			

• Natural logarithm transformation.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 10. Logistic Regression Model Predicting Arrest in the Total Pooled Sample Using Technical Violation Charge Type Indicators.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	-0.556	0.859	-0.647
Exposure Risk	0.002	0.002	0.760
Site 1 (Atlanta, GA)	-1.960	2.222	-0.882
Site 2 (Macon, GA)	-1.285	2.011	-0.639
Site 3 (Waycross, GA)	-0.258	1.592	-0.162
Site 4 (Contra Costa, CA)	-0.578	0.477	-1.211
Site 5 (Los Angeles, CA)	0.465	0.476	0.976
Site 6 (Ventura, CA)	1.551	0.564	2.752***
Site 7 (Des Moines, IA)	-1.263	0.746	-1.693*
Site 8 (Santa Fe, NM)	1.589	1.906	0.834
Site 9 (Winchester, VA)	-5.112	2.331	-2.193**
Site 10 (Seattle, WA)	--	--	--
Technical Violation Type			
Failure to Report	-0.979	0.293	-3.342****
Drug/Alcohol Violation	-1.655	0.309	-5.356****
Abscond	-0.426	0.326	-1.307
Curfew Violation	-1.625	0.680	-2.388**
Other Violation	-1.016	0.253	-4.013****
Age	-0.042	0.011	-3.779****
Avg. # Phone Contacts[a]	0.884	0.305	2.902***
Personal contacts in Site 1[a]	1.359	1.304	1.042
Personal contacts in Site 2[a]	0.309	1.087	0.284
Personal contacts in Site 3[a]	-1.999	1.300	-1.538
Personal contacts in Site 4[a]	-0.158	1.096	-0.144
Personal contacts in Site 5[a]	-0.422	0.946	-0.446
Personal contacts in Site 6[a]	0.521	0.991	0.525
Personal contacts in Site 7[a]	1.059	1.217	0.870
Personal contacts in Site 8[a]	5.051	1.790	3.076***
Personal contacts in Site 9[a]	5.580	2.767	2.017**
Monitoring Checks in Site 1[a]	-4.166	2.071	-2.011**
Monitoring Checks in Site 2[a]	1.623	1.532	1.059
Monitoring Checks in Site 3[a]	1.636	1.719	0.952
Monitoring Checks in Site 4[a]	2.322	1.055	2.201**
Monitoring Checks in Site 5[a]	0.946	1.050	0.901
Monitoring Checks in Site 6[a]	0.271	0.936	0.289
Monitoring Checks in Site 7[a]	0.345	1.180	0.292
Monitoring Checks in Site 8[a]	-1.602	1.241	-1.291
Monitoring Checks in Site 9[a]	-0.646	1.879	-0.344

(Continued on next page)

[a] Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 10--Continued

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Supervision Missing Data Flag	-0.138	0.802	-0.171
Avg. # Days Worked Per Month•	-0.531	0.103	-5.180****
Days Worked Missing Data Flag	-0.291	0.197	-1.482
Counseling Sessions in Site 1•	-0.761	1.650	-0.461
Counseling Sessions in Site 2•	0.608	0.677	0.898
Counseling Sessions in Site 3•	1.476	1.335	1.106
Counseling Sessions in Site 4•	-4.251	1.631	-2.607**
Counseling Sessions in Site 5•	-14.812	11.093	-1.335
Counseling Sessions in Site 6•	-0.546	0.617	-0.885
Counseling Sessions in Site 7•	0.220	0.642	0.342
Counseling Sessions in Site 8•	-1.601	0.886	-1.808
Counseling Sessions in Site 9•	0.719	0.788	0.913
# Prior Misdemeanor Convictions	0.090	0.021	4.396****
# Prior Prison Terms Served	0.377	0.095	3.971****
ISP in Site 1	3.443	2.240	1.537
ISP in Site 2	-0.095	0.991	-0.095
ISP in Site 3	-0.508	1.162	-0.437
ISP in Site 4	-1.157	0.993	-1.165
ISP in Site 5	-0.229	0.898	-0.254
ISP in Site 6	-1.407	0.960	-1.465
ISP in Site 7	-1.062	0.936	-1.135
ISP in Site 8	-4.049	1.701	-2.381**
ISP in Site 9	-3.564	2.494	-1.429
Log-likelihood	-491.264		
N=1037			

• Natural logarithm transformation.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 11. Multinomial Logistic Regression Model Predicting Five-Category Dependent Variable (No Arrest, Person Arrest, Property Arrest, Drug Arrest, and "Other" Arrest) in the Total Pooled Sample.

Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
	Person Offense		Property Offense	
Constant	-1.032 (1.520)	-0.679	0.324 (1.232)	0.263
Exposure Risk	-0.002 (0.004)	-0.453	-0.003 (0.003)	-0.808
Site 1 (Atlanta, GA)	-3.000 (fixed parameter)		-2.637 (0.925)	-2.850***
Site 2 (Macon, GA)	-0.266 (1.001)	-0.265	-0.123 (0.790)	-0.155
Site 3 (Waycross, GA)	-2.241 (1.334)	-1.680*	-0.124 (0.906)	-1.373
Site 4 (Contra Costa, CA)	-0.710 (0.529)	-1.342	-0.749 (0.406)	-1.843*
Site 5 (Los Angeles, CA)	-0.670 (0.572)	0.572	-1.328 (0.558)	-2.381**
Site 6 (Ventura, CA)	-0.501 (0.589)	-0.851	-0.697 (0.532)	-1.310
Site 7 (Des Moines, IA)	-1.375 (0.685)	-2.006**	-0.216 (0.468)	-0.462
Site 8 (Santa Fe, NM)	-1.486 (0.893)	-0.441	-1.192 (0.763)	-1.563
Site 9 (Winchester, VA)	-1.486 (0.886)	-1.678*	-1.515 (0.654)	-2.317**
Site 10 (Seattle, WA)	--	--	--	--
Technical w/Confinement	-0.647 (0.365)	-1.775*	-0.890 (0.297)	-3.026***
Technical w/o Confinement	-0.778 (0.452)	-1.720*	-0.391 (0.311)	-1.257
Age	-0.030 (0.020)	-1.547	-0.041 (0.016)	-2.471**
Avg. # Personal Contacts'	0.512 (0.389)	1.314	0.425 (0.309)	1.377
Avg. # Phone Contacts'	0.798 (0.395)	2.017**	0.596 (0.339)	1.757*
Avg. # Monitoring Checks'	0.400 (0.328)	1.222	0.941 (0.271)	3.473****
Supervision Missing Data Flag	0.616 (0.578)	1.066	1.487 (0.524)	2.839***
Avg. # Days Worked Per Month'	-0.391 (0.170)	-2.300**	-0.872 (0.151)	-5.792****
Work Missing Data Flag	-0.067 (0.341)	-0.197	-1.047 (0.313)	-3.345****
Avg. # Counseling Attended'	-0.252 (0.255)	-0.988	-0.426 (0.209)	-2.040**
# Misdemeanor Convictions	0.052 (0.035)	1.494	0.083 (0.024)	3.470****
# Prison Terms Served	0.270 (0.165)	1.636	0.314 (0.124)	2.534**
ISP Sample Membership	-0.390 (0.381)	-1.022	-0.600 (0.283)	-2.118**

(Continued on next page)

' Natural logarithm transformation.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 11--Continued.

Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
	Drug Offense		Other Offense	
Constant	0.015 (1.051)	0.014	-6.531 (2.071)	-3.154***
Exposure Risk	-0.004 (0.003)	-1.327	0.017 (0.006)	2.929***
Site 1 (Atlanta, GA)	-1.527 (0.899)	-1.698*	-3.00 (fixed parameter)	
Site 2 (Macon, GA)	0.956 (0.768)	1.245	0.209 (0.829)	0.252
Site 3 (Waycross, GA)	-0.835 (0.983)	-0.849	-2.330 (1.227)	-1.898*
Site 4 (Contra Costa, CA)	-0.416 (0.414)	-1.005	-0.803 (0.507)	-1.582
Site 5 (Los Angeles, CA)	0.113 (0.437)	0.258	-0.433 (0.505)	-0.858
Site 6 (Ventura, CA)	0.466 (0.438)	1.064	0.837 (0.436)	1.920*
Site 7 (Des Moines, IA)	-2.317 (0.819)	-2.830***	-0.165 (0.592)	-1.968*
Site 8 (Santa Fe, NM)	-2.00 (fixed parameter)		0.994 (0.709)	1.403
Site 9 (Winchester, VA)	-1.312 (0.729)	-1.799*	-2.275 (1.099)	-2.071**
Site 10 (Seattle, WA)	--	--	--	--
Technical w/Confinement	-0.860 (0.306)	-2.814***	-1.000 (0.302)	-3.313****
Technical w/o Confinement	-0.375 (0.323)	-1.161	-1.517 (0.468)	-3.239***
Age	-0.024 (0.318)	-1.571	-0.057 (0.018)	-3.224***
Avg. # Personal Contacts•	0.246 (0.318)	0.775	0.683 (0.328)	2.079**
Avg. # Phone Contacts•	0.432 (0.340)	1.270	0.663 (0.342)	1.940*
Avg. # Monitoring Checks•	0.317 (0.246)	1.288	-0.229 (0.257)	-0.891
Supervision Missing Data Flag	0.106 (0.445)	0.238	-0.668 (0.505)	-1.321
Avg. # Days Worked Per Month•	-0.455 (0.150)	-3.039***	-0.398 (0.140)	-2.841***
Work Missing Data Flag	-0.130 (0.285)	-0.457	0.526 (0.283)	1.860*
Avg. # Counseling Attended•	-0.434 (0.242)	-1.793*	-0.284 (0.216)	-1.318
# Misdemeanor Convictions	0.089 (0.025)	3.633****	0.107 (0.025)	4.203****
# Prison Terms Served	0.515 (0.125)	4.134****	0.172 (0.156)	1.098
ISP Sample Membership	-0.395 (0.300)	-1.316	0.115 (0.329)	0.350
Log-likelihood	-932.3178			
N=1037				

• Natural logarithm transformation.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 12. Multinomial Logistic Regression Model Predicting Five-Category Dependent Variable (No Arrest, Person Arrest, Property Arrest, Drug Arrest, and "Other" Arrest) in the Total Pooled Sample. Technical Violation Indicators are Restricted to be Equal Across Categories of the Dependent Variable.

Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
	Person Offense		Property Offense	
Constant	-0.994 (1.519)	-0.654	0.226 (1.228)	0.184
Exposure Risk	-0.002 (0.004)	-0.430	-0.003 (0.003)	-0.747
Site 1 (Atlanta, GA)	-3.00 (fixed parameter).		-2.518 (0.918)	-2.742***
Site 2 (Macon, GA)	-0.176 (0.981)	-0.179	-0.058 (0.781)	-0.074
Site 3 (Waycross, GA)	-2.268 (1.330)	-1.705*	-1.210 (0.905)	-1.337
Site 4 (Contra Costa, CA)	-0.788 (0.521)	-1.514	-0.665 (0.397)	-1.674*
Site 5 (Los Angeles, CA)	-0.705 (0.569)	-1.238	-1.266 (0.555)	-2.279**
Site 6 (Ventura, CA)	-0.508 (0.587)	-0.864	-0.635 (0.529)	-1.199
Site 7 (Des Moines, IA)	-1.418 (0.683)	-2.077**	-1.267 (0.555)	-2.279**
Site 8 (Santa Fe, NM)	-0.438 (0.894)	-0.490	-1.149 (0.758)	-1.517
Site 9 (Winchester, VA)	-1.530 (0.885)	-1.729*	-1.448 (0.649)	-2.229**
Site 10 (Seattle, WA)	--	--	--	--
Technical w/Confinement**	-0.871 (0.188)	-4.636****	-0.871 (0.188)	-4.636****
Technical w/o Confinement**	-0.650 (0.212)	-3.065***	-0.650 (0.212)	-3.065***
Age	-0.030 (0.019)	-1.565	-0.040 (0.016)	-2.450**
Avg. # Personal Contacts*	0.490 (0.385)	1.270	0.428 (0.309)	1.383
Avg. # Phone Contacts*	0.779 (0.392)	1.988**	0.598 (0.339)	1.764*
Avg. # Monitoring Checks*	0.418 (0.327)	1.281	0.927 (0.270)	3.437****
Supervision Missing Data Flag	0.623 (0.577)	1.080	1.455 (0.523)	2.782***
Avg. # Days Worked Per Month*	-0.396 (0.169)	-2.347**	-0.871 (0.150)	-5.814****
Work Missing Data Flag	-0.068 (0.341)	-0.198	-1.043 (0.313)	-3.335****
Avg. # Counseling Attended*	-0.253 (0.255)	-0.994	-0.430 (0.208)	-2.068**
# Misdemeanor Convictions	0.053 (0.035)	-0.198	0.084 (0.024)	3.521****
# Prison Terms Served	0.266 (0.166)	1.605	0.314 (0.123)	2.545**
ISP Sample Membership	-0.358 (0.375)	-0.955	-0.585 (0.280)	-2.089**

(Continued on next page)

* Natural logarithm transformation.

** Variable restricted to be equal across categories of the dependent variable.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 12--Continued

Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
	Drug Offense		Other Offense	
Constant	-0.057 (1.048)	-0.055	-6.592 (2.096)	-3.145***
Exposure Risk	-0.004 (0.003)	-1.272	0.017 (0.006)	2.911***
Site 1 (Atlanta, GA)	-1.399 (0.890)	-1.571	-3.00 (fixed parameter)	
Site 2 (Macon, GA)	1.041 (0.754)	1.380	0.049 (0.820)	0.060
Site 3 (Waycross, GA)	-0.814 (0.983)	-0.828	-2.286 (1.220)	-1.875*
Site 4 (Contra Costa, CA)	-0.344 (0.408)	-0.844	-0.884 (0.501)	-1.766*
Site 5 (Los Angeles, CA)	0.173 (0.435)	0.397	-0.481 (0.495)	-0.972
Site 6 (Ventura, CA)	0.525 (0.435)	1.206	0.783 (0.432)	1.809*
Site 7 (Des Moines, IA)	-2.249 (0.818)	-2.750***	-1.238 (0.587)	-2.109**
Site 8 (Santa Fe, NM)	-2.00 (fixed parameter)		1.030 (0.706)	1.458
Site 9 (Winchester, VA)	-1.249 (0.726)	-1.720*	-2.365 (1.096)	-2.157**
Site 10 (Seattle, WA)	--	--	--	--
Technical w/Confinement**	-0.871 (0.188)	-4.636****	-0.871 (0.188)	-4.636****
Technical w/o Confinement**	-0.650 (0.212)	-3.065***	-0.650 (0.212)	-3.065***
Age	-0.024 (0.015)	-1.560	-0.058 (0.018)	-3.233***
Avg. # Personal Contacts*	0.240 (0.317)	0.755	0.685 (0.324)	2.114**
Avg. # Phone Contacts*	0.424 (0.340)	1.248	0.649 (0.340)	1.909*
Avg. # Monitoring Checks*	0.317 (0.246)	1.290	-0.261 (0.255)	-1.024
Supervision Missing Data Flag	0.101 (0.445)	0.226	-0.733 (0.501)	-1.464
Avg. # Days Worked Per Month*	-0.460 (0.149)	-3.080***	-0.376 (0.139)	-2.699***
Work Missing Data Flag	-0.153 (0.284)	-0.537	0.573 (0.280)	2.047**
Avg. # Counseling Attended*	-0.428 (0.240)	-1.784*	-0.280 (0.216)	-1.293
# Misdemeanor Convictions	0.092 (0.024)	3.788***	0.103 (0.025)	4.103****
# Prison Terms Served	0.514 (0.124)	4.132****	0.162 (0.157)	1.033
ISP Sample Membership	-0.369 (0.297)	-1.243	0.102 (1.519)	-0.654
Log-likelihood	-935.8928			
N=1037				

* Natural logarithm transformation.

** Variable restricted to be equal across categories of the dependent variable.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 13. Comparison of Multinomial Logistic Regression Models Predicting Five-Category Dependent Variable.

Variable	Model 1 (Unrestricted)	Model 2 (Restricted)
	X=Equality Restriction	
Constant		
Exposure Risk		
Site 1 (Atlanta, GA)		
Site 2 (Macon, GA)		
Site 3 (Waycross, GA)		
Site 4 (Contra Costa, CA)		
Site 5 (Los Angeles, CA)		
Site 6 (Ventura, CA)		
Site 7 (Des Moines, IA)		
Site 8 (Santa Fe, NM)		
Site 9 (Winchester, VA)		
Site 10 (Seattle, WA)		
Technical w/Confinement		X
Technical w/o Confinement		X
Age		
Avg. # Personal Contacts•		
Avg. # Phone Contacts•		
Avg. # Monitoring Checks•		
Supervision Missing Data Flag		
Avg. # Days Worked Per Month•		
Work Missing Data Flag		
Avg. # Counseling Attended•		
# Misdemeanor Convictions		
# Prison Terms Served		
ISP Sample Membership		
Log-likelihood	-932.3178	-935.8928 X^2_6=7.15
N=1037		

Note: Entries that are blank represent unrestricted coefficients.

•Natural logarithm transformation.

*p < .10 ** p<.05 ***p<.01 ****p<.001

Table 14. Test 1 Descriptive Statistics in the Pooled California Sample (N=488).

Variable	Mean (S.D.)	Median	Minimum	Maximum
Site 4 (Contra Costa)	0.348 (0.477)	0	0	1
Site 5 (Los Angeles)	0.311 (0.464)	0	0	1
Site 6 (Ventura)	0.340 (0.474)	0	0	1
Exposure Risk	334.994 (44.423)	361	24	365
Arrest	0.346 (0.476)	0	0	1
Technical Violation	0.486 (0.500)	0	0	1
Tech. w/Confinement	0.268 (0.444)	0	0	1
Tech. w/o Confinement	0.217 (0.413)	0	0	1
Technical Violation Type:				
Failure-to-Report	0.170 (0.376)	0	0	1
Drug/Alcohol Violation	0.115 (0.319)	0	0	1
Abscond	0.041 (0.198)	0	0	1
Other **	0.160 (0.367)	0	0	1
Male	0.844 (0.363)	1	0	1
Nonwhite	0.765 (0.424)	1	0	1
Age	28.394 (8.461)	26.175	0	1
Married	0.145 (0.351)	0	0	1
Highschool degree	0.406 (0.489)	0	0	1
ISP Sample Membership	0.549 (0.498)	1	0	1
Personal Contacts*	1.850 (1.937)	1.222	0	8.333
(Log)	0.835 (0.645)	0.799	0	2.234
Phone Contacts*	0.831 (1.453)	0.250	0	12.273
(Log)	0.433 (0.517)	0.223	0	2.586
Monitoring Checks*	2.400 (3.599)	0.833	0	18.0
(Log)	0.850 (0.788)	0.606	0	2.944
Monthly Days Worked*	2.874 (5.163)	0	0	20.0
(Log)	0.692 (1.049)	0	0	3.045
Counseling Sessions*	0.775 (2.541)	0	0	30.0
(Log)	0.275 (0.598)	0	0	3.434
# Misd. Convictions	2.995 (4.095)	2	0	24
# Prison Terms Served	0.241 (0.683)	0	0	7
# Prior Arrests	6.325 (7.226)	4	0	62
Alcohol Trt. Need	1.613 (0.738)	1	1	3
Drug Trt. Need	2.203 (0.781)	2	1	3
Vocational Need	(50% missing in 1 site)			
Employment Need	(50% missing in 1 site)			
# Address Changes	1.447 (0.688)	1	1	3
Supervision Missing Data	0.199 (0.399)	0	0	1
Work Missing Data Flag	0.508 (0.500)	1	0	1

** Due to the small number of curfew violations, curfew violations are included in the "other category."

* Due to the skewed distribution of the variable, the raw and natural logarithm transformation are presented. The variable represents the monthly average prior to the first arrest. If the individual was not arrested during the one-year followup period, the variable represents the monthly average over the followup period.

Table 15. Logistic Regression Model Predicting Arrest in the Pooled California Sample.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	1.163	1.038	1.120
Exposure Risk	0.003	0.003	1.276
Site 1 (Los Angeles, CA)	-1.904	0.482	-3.950****
Site 2 (Ventura, CA)	-2.028	0.450	-4.503****
Site 3 (Contra Costa, CA)	--	--	--
Technical w/Confinement	-1.889	0.316	-5.971****
Technical w/o Confinement	-1.559	0.312	-5.003****
Married in Site 1	-0.784	1.111	-0.706
Married in Site 2	1.039	1.126	0.923
Age	-0.035	0.016	-2.236**
Avg. # Phone Contacts•	0.988	0.352	2.810***
Avg. # Personal Contacts•	-0.813	0.378	-2.152**
Supervision Missing Data Flag	-0.809	0.886	-0.913
Avg. # Drug & Alcohol Tests•	1.542	0.425	3.633****
Days Worked in Site 1	-0.523	0.486	-1.076
Days Worked in Site 2	-0.140	0.428	-0.327
Days Worked Missing Data Flag	-0.274	0.288	-0.952
Counseling Sessions in Site 1	-3.224	1.271	-2.536**
Counseling Sessions in Site 2	-6.142	9.731	-0.631
Counseling Missing Data Flag	1.148	0.912	1.260
Number of Prior Arrests	0.063	0.017	3.808****
Log-likelihood	-243.617		
N=488			

• Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 16. Logistic Regression Model Predicting Arrest in the Pooled California Sample Using Technical Violation Charge Type Indicators.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	0.959	1.039	0.923
Exposure Risk	0.003	0.003	1.266
Site 1 (Los Angeles, CA)	-1.782	0.486	-3.666****
Site 2 (Ventura, CA)	-1.850	0.461	-4.014****
Site 3 (Contra Costa, CA)	--	--	--
Technical Violation Charge Type			
Failure to Report	-1.508	0.340	-4.430****
Drug/Alcohol Violation	-1.884	0.411	-4.585****
Abscond	-3.174	1.091	-2.911***
Other	-1.641	0.365	-4.496****
Married in Site 1	-0.801	1.113	-0.719
Married in Site 2	1.208	1.168	1.035
Age	-0.032	0.016	-2.095**
Avg. # Phone Contacts•	0.978	0.355	2.756***
Avg. # Personal Contacts•	-0.802	0.386	-2.080**
Supervision Missing Data Flag	-0.799	0.873	-0.915
Avg. # Drug & Alcohol Tests•	1.551	0.434	3.576****
Days Worked in Site 1	-0.511	0.488	-1.047
Days Worked in Site 2	-0.167	0.433	-0.386
Days Worked Missing Data Flag	-0.282	0.287	-0.982
Counseling Sessions in Site 1	-3.184	1.252	-2.543**
Counseling Sessions in Site 2	-7.851	10.304	-0.762
Counseling Missing Data Flag	1.181	0.900	1.312
Number of Prior Arrests	0.066	0.017	3.874****
Log-likelihood	-242.273		
N=488			

• Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 17. Test 1 Descriptive Statistics in the Pooled Georgia Sample (N=150).

Variable	Mean (S.D.)	Median	Minimum	Maximum
Site 1 (Atlanta)	0.333 (0.473)	0	0	1
Site 2 (Macon)	0.333 (0.473)	0	0	1
Site 3 (Waycross)	0.333 (0.473)	0	0	1
Exposure Risk	364.093 (4.739)	365	326	365
Arrest	0.207 (0.406)	0	0	1
Technical Violation	0.600 (0.492)	1	0	1
Tech. w/ Confinement	0.380 (0.487)	0	0	1
Tech. w/o Confinement	0.220 (0.416)	0	0	1
Technical Violation Type:				
Failure-to-Report/ Abscond **	0.067 (0.250)	0	0	1
Drug/Alcohol Violation	0.227 (0.420)	0	0	1
Curfew	0.147 (0.355)	0	0	1
Other	0.160 (0.368)	0	0	1
Male	0.853 (0.355)	1	0	1
Nonwhite	0.567 (0.497)	1	0	1
Age	27.098 (6.418)	26.292	17.518	52.184
Married	0.194 (0.396)	0	0	1
Highschool degree	0.420 (0.495)	0	0	1
ISP Sample Membership	0.507 (0.502)	1	0	1
Personal Contacts*	12.424 (7.872)	11.822	0	35.250
Phone Contacts*	1.250 (2.541)	0.167	0	17.667
Monitoring Checks*	4.377 (2.452)	4.167	0	10.083
Drug/Alcohol Tests*	5.607 (5.662)	3.833	0	30.375
Positive Drug Tests*	0.399 (0.781)	0.167	0	6.0
Monthly Days Worked*	8.521 (7.519)	7.792	0	24.5
Counseling Sessions*	3.110 (4.351)	1.333	0	33.5
# Misd. Convictions	1.987 (2.417)	1	0	15
# Prison Terms Served	0.252 (0.674)	0	0	4
Drug Dependence	0.393 (0.490)	0	0	1
Supervision Missing Data	0.040 (0.197)	0	0	1
Work Missing Data Flag	0.140 (0.348)	0	0	1

** Due to the small number of absconding violations in Georgia, absconding violations and failure-to-report violations are combined in one category.

* The variable represents the monthly average prior to the first arrest. If the individual was not arrested during the one-year followup period, the variable represents the monthly average over the followup period.

* * Note Risk and Need Assessment Variables were not available in Macon, Georgia.

Table 18. Logistic Regression Model Predicting Arrest in the Pooled Georgia Sample.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	-2.728	0.836	-3.264***
Site 1 (Atlanta, GA)	-1.396	0.873	-1.599
Site 2 (Macon, GA)	1.887	0.912	2.070**
Site 3 (Waycross, GA)	--	--	--
Technical w/Confinement	-1.901	0.821	-2.315**
Technical w/o Confinement	-1.525	0.826	-1.847*
Nonwhite	1.326	0.588	2.255**
Avg. # Drug /Alcohol Tests	0.113	0.052	2.163**
Avg. # Days Worked Per Month	-0.066	0.041	-1.632
Days Worked Missing Data Flag	0.603	0.696	0.867
Drug Dependence	1.486	0.534	2.785***
# Prior Prison Terms Served	0.478	0.305	1.565
Log-likelihood	-54.529		
N=150			

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 19. Logistic Regression Model Predicting Arrest in the Pooled Georgia Sample Using Technical Violation Charge Type Indicators.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	-2.673	0.839	-3.187***
Site 1 (Atlanta, GA)	-1.384	0.909	-1.522
Site 2 (Macon, GA)	1.535	0.968	1.586
Site 3 (Waycross, GA)	--	--	--
Technical Violation Type			
Failure to Report/Abscond	-2.606	1.425	-1.829*
Drug/Alcohol Violation	-1.601	0.903	-1.772*
Curfew Violation	-1.281	0.942	-1.361
Other	-1.759	0.870	-2.022**
Nonwhite	1.262	0.584	2.162**
Avg. # Drug /Alcohol Tests	0.126	0.054	2.337**
Avg. # Days Worked Per Month	-0.072	0.042	-1.735*
Days Worked Missing Data Flag	0.471	0.706	0.667
Drug Dependence	1.520	0.535	2.839***
# Prior Prison Terms Served	0.525	0.312	1.681*
Log-likelihood	-54.158		
N=150			

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 20. Test 1 Descriptive Statistics in the Pooled Miscellaneous Sample (N=399).

Variable	Mean (S.D.)	Median	Minimum	Maximum
Site 7 (Des Moines, IA)	0.288 (0.454)	0	0	1
Site 8 (Santa Fe, NM)	0.145 (0.353)	0	0	1
Site 9 (Winchester, VA)	0.133 (0.340)	0	0	1
Site 10 (Seattle, WA)	0.434 (0.496)	0	0	1
Exposure Risk	355.308 (27.331)	365	160	365
Arrest	0.472 (0.710)	0	0	1
Technical Violation	0.489 (0.501)	0	0	1
Tech. w/ Confinement	0.343 (0.475)	0	0	1
Tech. w/o Confinement	0.145 (0.353)	0	0	1
Technical Violation Type:				
Failure-to-Report	0.058 (0.233)	0	0	1
Drug/Alcohol Violation	0.110 (0.314)	0	0	1
Abscond	0.133 (0.340)	0	0	1
Other **	0.188 (0.391)	0	0	1
Male	0.769 (0.422)	1	0	1
Nonwhite	0.584 (0.494)	1	0	1
Age	29.834 (8.366)	28.255	17.285	88.038
Married	0.154 (0.357)	0	0	1
Highschool degree	0.456 (0.489)	0	0	1
ISP Sample Membership	0.514 (0.500)	1	0	1
Personal Contacts*	2.380 (2.606)	1.417	0	12.0
(Log)	0.965 (0.698)	0.882	0	2.565
Phone Contacts*	0.732 (0.970)	0.333	0	6.333
(Log)	0.437 (0.442)	0.288	0	1.992
Monitoring Checks*	5.672 (8.523)	2.200	0	37.429
(Log)	1.308 (1.035)	1.163	0	3.649
Drug Tests*	1.058 (1.561)	0.250	0	8.333
(Log)	0.519 (0.593)	0.223	0	2.234
Monthly Days Worked*	4.235 (6.208)	0	0	25.0
(Log)	0.984 (1.142)	0	0	3.258
Counseling Sessions*	1.392 (3.444)	0	0	32.8
(Log)	0.471 (0.749)	0	0	3.520
# Misd. Convictions	3.669 (5.464)	2	0	38
# Prison Terms Served	0.589 (1.118)	0	0	7
Alcohol Trt. Need	1.845 (0.726)	2	1	3

(Continued on next page)

** Due to the small number of curfew violations, curfew violations are included in the "other" category.

* Due to the skewed distribution of the variable, the raw and natural logarithm transformation are presented. The variable represents the monthly average prior to the first arrest. If the individual was not arrested during the one-year followup period, the variable represents the monthly average over the followup period.

Table 20--Continued.

Variable	Mean (S.D.)	Median	Minimum	Maximum
Drug Trt. Need	2.592 (0.570)	3	1	3
Vocational Need	2.254 (0.887)	2	1	4
Employment Need	2.815 (0.696)	3	1	4
# Address Changes	1.976 (0.817)	2	1	3
Supervision Missing Data	0.005 (0.071)	0	0	1
Work Missing Data Flag	0.326 (0.469)	0	0	1

Table 21. Logistic Regression Model Predicting Arrest in the Pooled Miscellaneous Sample.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	0.532	1.690	0.315
Exposure Risk	-0.002	0.005	-0.398
Site 1 (Des Moines, IA)	-1.392	0.411	-3.383****
Site 2 (Santa Fe, NM)	-0.438	0.480	-0.912
Site 3 (Winchester, VA)	-2.182	0.533	-4.094****
Site 4 (Seattle, WA)	--	--	--
Technical w/Confinement	-0.160	0.288	-0.556
Technical w/o Confinement	0.297	0.379	0.782
Age	-0.044	0.017	-2.662***
Avg. # Phone Contacts•	0.641	0.394	1.626
Avg. # Personal Contacts•	2.311	0.437	5.286****
Avg. # Drug & Alcohol Tests•	-0.961	0.398	-2.414**
Avg. # Days Worked per Month•	-0.717	0.164	-4.379****
Days Worked Missing Data Flag	-0.364	0.284	-1.281
# Prior Misdemeanor Convictions	0.078	0.024	3.262***
# Prison Terms Served	0.346	0.112	3.091***
Intensive Supervision Membership	-0.785	0.342	-2.296**
Log-likelihood	-195.79		
N=399			

• Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 22. Logistic Regression Model Predicting Arrest in the Pooled Miscellaneous Sample Using Technical Violation Charge Type Indicators.

Variable	Estimated Coefficient	Estimated Standard Error	*t* ratio
Constant	1.212	1.708	0.710
Exposure Risk	-0.005	0.005	-1.072
Site 1 (Des Moines, IA)	-1.161	0.434	-2.674***
Site 2 (Santa Fe, NM)	-0.343	0.513	-0.668
Site 3 (Winchester, VA)	-2.148	0.582	-3.691****
Site 4 (Seattle, WA)	--	--	--
Technical Violation Type			
Failure to Report	0.629	0.577	1.091
Drug/Alcohol Violation	-1.168	0.496	-2.354**
Abscond	1.007	0.397	2.536**
Other Violation	-0.400	0.367	-1.091
Age	-0.042	0.017	-2.482**
Avg. # Phone Contacts•	0.436	0.410	1.065
Avg. # Personal Contacts•	2.740	0.472	5.800****
Avg. # Drug & Alcohol Tests•	-0.924	0.412	-2.243**
Avg. # Days Worked per Month•	-0.729	0.172	-4.244****
Days Worked Missing Data Flag	-0.371	0.288	-1.288
# Prior Misdemeanor Convictions	0.081	0.026	3.172***
# Prison Terms Served	0.378	0.115	3.276***
Intensive Supervision Membership	-0.917	0.356	-2.574**
Log-likelihood	-187.52		
N=399			

• Natural logarithm transformation.

*p<.10 **p<.05 ***p<.01 ****p<.001

Table 23. Monthly Measures of Supervision Intensity During One-Year of Probation Supervision.

Variable	Mean (S.D.)	Median	Minimum	Maximum
# Personal Contacts	3.246 (6.449)	1	0	92
# Phone Contacts	0.781 (2.626)	0	0	82
# Monitoring Checks	3.568 (6.795)	0	0	51
# Drug Tests Taken	0.937 (2.397)	0	0	59
Technical Violation	0.089 (0.284)	0	0	1

N=1,037 individuals
N=12,444 person-months

Table 24. Negative Binomial Panel Model Results Using Technical Violation Charges to Predict Monthly Measures of Supervision Intensity (Personal Contacts, Telephone Contacts, Monitoring Checks, and Drug Tests) over 12-Month Followup Period.

	Model 1 Personal Contacts		Model 2 Phone Contacts		Model 3 Monitoring Checks		Model 4 Drug Tests	
	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
Constant	-0.179 (0.011)	-16.930****	-0.613 (0.021)	-28.802****	-0.182 (0.010)	-17.281****	-0.471 (0.020)	-23.255****
Tech. Viol.	0.493 (0.026)	18.906****	0.456 (0.049)	9.274****	0.684 (0.027)	25.525****	0.518 (0.038)	13.637****
<u>a</u>	1.038 (0.064)	16.293****	1.480 (0.094)	15.787****	1.025 (0.063)	16.280****	1.109 (0.093)	11.934****
<u>b</u>	1.300 (0.067)	19.397****	0.899 (0.056)	16.057****	1.318 (0.068)	19.498****	0.624 (0.400)	15.580****
Log-likelihood	-23128.84		-11762.28		-23615.49		-12980.92	
N=1,037								

*p < .10 **p<.05 ***p<.01 ****p<.001

Table 25. Test 2 Descriptive Statistics in the Total Pooled Sample (N=1,037)

Variable	Mean (S.D.)	Median	Minimum	Maximum
Site 1 (Atlanta, GA)	0.048 (0.214)	0	0	1
Site 2 (Macon, GA)	0.048 (0.214)	0	0	1
Site 3 (Waycross, GA)	0.048 (0.214)	0	0	1
Site 4 (Contra Costa, CA)	0.164 (0.370)	0	0	1
Site 5 (Los Angeles, CA)	0.147 (0.354)	0	0	1
Site 6 (Ventura, CA)	0.160 (0.367)	0	0	1
Site 7 (Des Moines, IA)	0.111 (0.314)	0	0	1
Site 8 (Santa Fe, NM)	0.056 (0.230)	0	0	1
Site 9 (Winchester, VA)	0.051 (0.220)	0	0	1
Site 10 (Seattle, WA)	0.167 (0.373)	0	0	1
Exposure Risk	347.019 (36.805)	365	24	365
Recidivism Outcome				
No recidivism event	0.314 (0.464)	0	0	1
Technical only	0.365 (0.482)	0	0	1
Arrest only	0.094 (0.291)	0	0	1
Technical & arrest	0.228 (0.419)	0	0	1
Male	0.817 (0.387)	1	0	1
Nonwhite	0.667 (0.471)	1	0	1
Age	28.760 (8.208)	26.984	14.493	88.038
Married	0.155 (0.360)	0	0	1
Highschool degree	0.427 (0.490)	0	0	1
ISP Sample Membership	0.529 (0.499)	1	0	1
Arrest Type				
Person Offense	0.109 (0.312)	0	0	1
Property Offense	0.353 (0.478)	0	0	1
Drug Offense	0.364 (0.481)	0	0	1
Other Offense	0.170 (0.376)	0	0	1
Personal Contacts*	4.484 (6.478)	2	0	36.0
(Log)	1.204 (0.953)	1.099	0	3.611
Phone Contacts*	0.999 (1.748)	0.286	0	17.667
(Log)	0.484 (0.574)	0.251	0	2.927
Monitoring Checks*	4.532 (6.693)	2	0	41.0
(Log)	1.205 (0.963)	1.099	0	3.738
Drug/Alcohol Tests*	2.057 (4.017)	0.500	0	36.25
(Log)	0.707 (0.796)	0.405	0	3.618

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* Due to the skewed distribution of the variable, the raw and natural logarithm transformation are presented. The variable represents the monthly average prior to the first recidivism event (either first arrest or first technical violation). If the individual was not arrested or charged with a technical violation during the one-year followup period, the variable represents the monthly average over the followup period.

Table 25--Continued.

Variable	Mean (S.D.)	Median	Minimum	Maximum
Monthly Days Worked*	4.478 (6.742)	0	0	30.0
(Log)	0.940 (1.205)	0	0	3.434
Counseling Sessions*	1.422 (3.547)	0	0	33.5
(Log)	0.444 (0.776)	0	0	3.541
# Misd. Convictions	3.109 (4.528)	2	0	38
# Prison Terms Served	0.376 (0.891)	0	0	7
Supervision Missing Data	0.101 (0.302)	0	0	1
Work Missing Data Flag	0.385 (0.487)	0	0	1

* Due to the skewed distribution of the variable, the raw and natural logarithm transformation are presented. The variable represents the monthly average prior to the first recidivism event (either first arrest or first technical violation) . If the individual was not arrested or charged with a technical violation during the one-year followup period, the variable represents the monthly average over the followup period.

Table 26. Multinomial Logistic Regression Model Predicting Community Supervision Failure with Four-Category Dependent Variable (No Recidivism Event=0; Technical Violation Only=1; Arrest Only=2; Technical Violation and Arrest=3).

	Technical Violation		Arrest		Technical Violation and Arrest	
Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
Constant	-1.427 (1.010)	-1.413	-1.869 (1.377)	-1.357	-0.035 (1.185)	-0.029
Site 1 (Atlanta, GA)	-0.851 (0.535)	-1.592	-2.618 (1.002)	-2.613***	-3.316 (0.923)	-3.593****
Site 2 (Macon, GA)	2.126 (1.178)	1.804	-0.363 (1.671)	-0.217	2.454 (1.237)	1.984**
Site 3 (Waycross, GA)	-1.417 (0.675)	-2.098**	-2.648 (1.460)	-1.813	-1.627 (0.776)	-2.097**
Site 4 (Contra Costa, CA)	-0.573 (0.325)	-1.764	-0.353 (0.433)	-0.816	-1.268 (0.370)	-3.430****
Site 5 (Los Angeles, CA)	0.373 (0.358)	1.040	0.366 (0.466)	0.786	-0.575 (0.423)	-1.358
Site 6 (Ventura, CA)	0.705 (0.381)	1.850	-0.129 (0.537)	-0.241	1.056 (0.412)	2.565**
Site 7 (Des Moines, IA)	0.014 (0.386)	0.035	-0.929 (0.596)	-1.557	-0.952 (0.454)	-2.098**
Site 8 (Santa Fe, NM)	-1.891 (0.609)	-3.105***	-1.044 (0.887)	-1.177	-1.615 (0.664)	-2.433**
Site 9 (Winchester, VA)	-1.391 (0.523)	-2.659***	-1.793 (0.911)	-1.968**	-2.344 (0.647)	-3.625****
Site 10 (Seattle, WA)	--	--	--	--	--	--
Exposure Risk	0.006 (0.003)	2.404**	0.0003 (0.003)	0.097	0.005 (0.003)	1.532
Nonwhite	0.589 (0.208)	2.833***	0.124 (0.301)	0.412	0.760 (0.241)	3.159***
Age	-0.020 (0.011)	-1.797	-0.025 (0.015)	-1.647	-0.057 (0.014)	-4.002****
Marital	-0.552 (0.248)	-2.230**	-0.606 (0.396)	-1.532	-0.540 (0.296)	-1.826
Avg. # of Personal Contacts•	-0.042 (0.245)	-0.172	0.832 (0.379)	2.196**	-0.071 (0.281)	-0.251
Avg. # of Phone Contacts•	0.044 (0.216)	0.202	0.039 (0.317)	0.122	0.528 (0.260)	2.029**
Avg. # of Monitoring Checks•	0.912 (0.185)	4.942****	0.467 (0.265)	1.759	0.768 (0.206)	3.730****
Avg. # of Drug/Alcohol Tests•	0.555 (0.234)	2.367**	0.168 (0.349)	0.482	0.349 (0.268)	1.301
Supervision Intensity Missing Flag	1.102 (0.405)	2.720***	1.347 (0.513)	2.624***	0.663 (0.470)	1.412

(Continued on next page)

• Natural logarithm transformation **p<.05 ***p<.01 ****p<.001

Table 26--Continued.

Variable	Technical Violation b (s.e.)	Technical Violation t ratio	Arrest b (s.e.)	Arrest t ratio	Technical Violation and Arrest b (s.e.)	Technical Violation and Arrest t ratio
ISP Sample Membership	0.241 (0.206)	1.167	-0.608 (0.327)	-1.860	0.291 (0.242)	1.202
Avg. # of Days Worked•	-0.684 (0.099)	-6.928****	-0.516 (0.139)	-3.718****	-0.748 (0.116)	-6.454****
Days Worked Missing Data Flag	-0.218 (0.197)	-1.103	-0.339 (0.282)	-1.200	-0.238 (0.231)	-1.032
Avg. # Counseling Sessions•	-0.558 (0.145)	-3.841****	-0.361 (0.225)	-1.607	-0.484 (0.167)	-2.899***
# Prior Misdemeanor Convictions	0.065 (0.028)	2.353**	0.068 (0.037)	1.842	0.137 (0.028)	4.925****
# Prior Prison Terms Served	-0.134 (0.117)	-1.145	0.096 (0.160)	0.596	0.242 (0.114)	2.121**
Offense Type						
Person Offense	-0.851 (0.422)	-2.018**	0.849 (0.844)	1.005	-1.625 (0.507)	-3.204***
Property Offense	-0.794 (0.361)	-2.197**	1.272 (0.764)	1.666	-0.939 (0.418)	-2.246**
Drug Offense	-0.872 (0.358)	-2.436**	1.217 (0.764)	1.591	-1.330 (0.421)	-3.158***
Other Offense	--	--	--	--	--	--
Log-Likelihood	-1122.970					
N=1037						

• Natural logarithm transformation **p<.05 ***p<.01 ****p<.001

Table 27. Multinomial Logistic Regression Model Predicting Community Supervision Failure with Four-Category Dependent Variable. Equality Restrictions Have Been Imposed on Measures of "Recidivism Propensity" (Demographic, Criminal History, and Community Activity Variables)

	Technical Violation		Arrest		Technical Violation and Arrest	
Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
Constant	-1.477 (1.001)	-1.477	-0.493 (1.148)	-0.429	-0.450 (1.071)	-0.421
Site 1 (Atlanta, GA)	-0.669 (0.533)	-1.256	-3.164 (1.012)	-3.127***	-3.306 (0.892)	-3.707****
Site 2 (Macon, GA)	2.256 (1.178)	1.914	-1.323 (1.696)	-0.780	2.418 (1.211)	1.996**
Site 3 (Waycross, GA)	-1.377 (0.666)	-2.068**	-3.914 (1.326)	-2.951***	-1.318 (0.714)	-1.847
Site 4 (Contra Costa, CA)	-0.496 (0.321)	-1.547	-0.454 (0.415)	-1.093	-1.238 (0.347)	-3.565****
Site 5 (Los Angeles, CA)	0.490 (0.358)	1.367	0.323 (0.454)	0.710	-0.683 (0.406)	-1.685
Site 6 (Ventura, CA)	0.716 (0.380)	1.883	-0.066 (0.536)	-0.123	0.948 (0.388)	2.445**
Site 7 (Des Moines, IA)	-0.072 (0.382)	-0.190	-0.737 (0.566)	-1.302	-0.805 (0.423)	-1.902
Site 8 (Santa Fe, NM)	-1.783 (0.604)	-2.953***	-1.609 (0.871)	-1.848	-1.375 (0.623)	-2.208**
Site 9 (Winchester, VA)	-1.401 (0.515)	-2.718***	-2.657 (0.911)	-2.917***	-1.832 (0.571)	-3.207***
Site 10 (Seattle, WA)	--	--	--	--	--	--
Exposure Risk	0.006 (0.003)	2.482**	0.001 (0.003)	0.411	0.004 (0.003)	1.288
Nonwhite	0.551 (0.186)	2.958***	0.551 (0.186)	2.958***	0.551 (0.186)	2.958***
Age	-0.031 (0.010)	-3.089***	-0.031 (0.010)	-3.089***	-0.031 (0.010)	-3.089***
Marital	-0.562 (0.222)	-2.532**	-0.562 (0.222)	-2.532**	-0.562 (0.222)	-2.532**
Avg. # of Personal Contacts•	-0.020 (0.245)	-0.081	0.901 (0.374)	2.411**	-0.101 (0.272)	-0.372
Avg. # of Phone Contacts•	0.062 (0.216)	0.286	0.031 (0.322)	0.096	0.493 (0.251)	1.967**
Avg. # of Monitoring Checks•	0.895 (0.185)	4.850****	0.494 (0.261)	1.888	0.739 (0.197)	3.745****
Avg. # of Drug/Alcohol Tests•	0.498 (0.233)	2.141**	0.235 (0.341)	0.688	0.401 (0.256)	1.566
Supervision Intensity Missing Flag	1.082 (0.407)	2.659***	1.277 (0.517)	2.468**	0.827 (0.452)	1.830

(Continued on next page)

• Natural logarithm transformation **p<.05 ***p<.01 ****p<.001

Table 27--Continued.

	Technical Violation		Arrest		Technical Violation and Arrest	
Variable	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio	b (s.e.)	*t* ratio
ISP Sample Membership	0.220 (0.206)	1.067	-0.626 (0.325)	-1.924	0.283 (0.233)	1.211
Avg. # of Days Worked[a]	-0.673 (0.090)	-7.479****	-0.673 (0.090)	-7.479****	-0.673 (0.090)	-7.479****
Days Worked Missing Data Flag	-0.195 (0.198)	-0.987	-0.335 (0.286)	-1.170	-0.283 (0.221)	-1.279
Avg. # Counseling Sessions[a]	-0.496 (0.129)	-3.849****	-0.496 (0.129)	-3.849****	-0.496 (0.129)	-3.849****
# Prior Misdemeanor Convictions	0.092 (0.025)	3.664****	0.092 (0.025)	3.664****	0.092 (0.025)	3.664****
# Prior Prison Terms Served	0.041 (0.097)	0.426	0.041 (0.097)	0.426	0.041 (0.097)	0.426
Offense Type						
Person Offense	-0.901 (0.392)	-2.297**	-0.901 (0.392)	-2.297**	-0.901 (0.392)	-2.297**
Property Offense	-0.612 (0.332)	-1.844	-0.612 (0.332)	-1.844	-0.612 (0.332)	-1.844
Drug Offense	-0.767 (0.332)	-2.310**	-0.767 (0.332)	-2.310**	-0.767 (0.332)	-2.310**
Other Offense	--	--	--	--	--	--
Log-Likelihood	-1156.021					
N=1037						

[a] Natural logarithm transformation **p<.05 ***p<.01 ****p<.001

Table 28. Multinomial Logistic Models Predicting Community Supervision Failure Subject to Four Combinations of Equality Restrictions.

	X=Equality Restriction			
Variable	Model 1	Model 2	Model 3	Model 4
Constant				
Site 1 (Atlanta, GA)				
Site 2 (Macon, GA)				
Site 3 (Waycross, GA)				
Site 4 (Contra Costa, CA)				
Site 5 (Los Angeles, CA)				
Site 6 (Ventura, CA)				
Site 7 (Des Moines, IA)				
Site 8 (Santa Fe, NM)				
Site 9 (Winchester, VA)				
Exposure Risk				
Supervision Intensity:				
Personal Contacts				
Phone Contacts				
Monitoring Checks				
Drug/Alcohol Tests				
Supervision Missing Data Indicator				
ISP Sample Membership				
Nonwhite	X	X		
Age	X	X		
Married	X	X		
Days Worked	X	X		
Work Missing Data Indicator				
Counseling Sessions	X	X		
# Prior Misdemeanor Convictions	X		X	
# Prior Prison Terms Served	X		X	
Offense Type:				
Person Offense	X			X
Property Offense	X			X
Drug Offense	X			X
Other Offense	--			--
Log-Likelihood	-1156.021	-1131.134	-1139.807	-1131.895
	X^2_{20}=66.102****	X^2_{10}=16.328	X^2_{4}=33.674****	X^2_{6}=17.850**

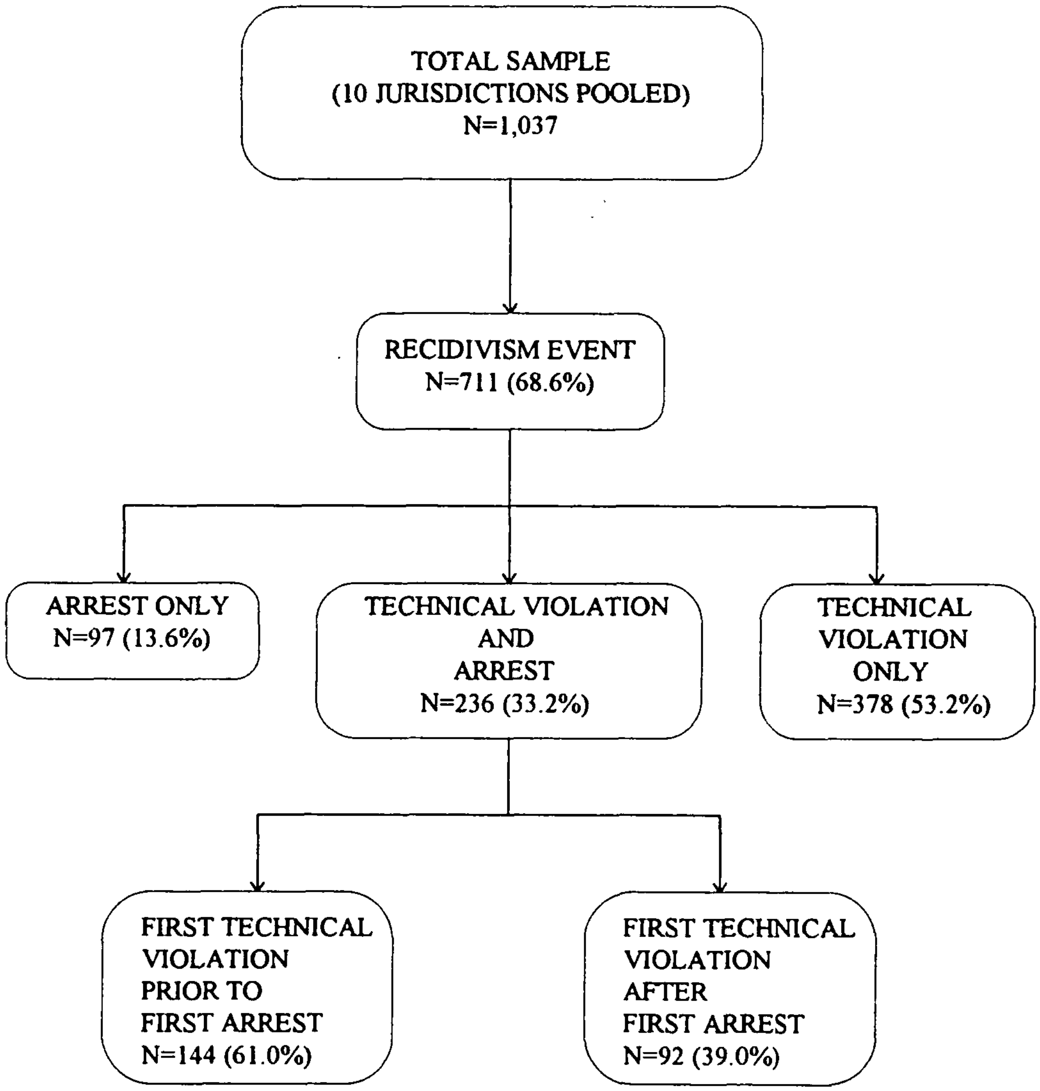
Note: The log-likelihood value of the unrestricted multinomial logistic model equals -1122.970 (see Table 26).

Note: Entries that are blank represent unrestricted coefficients.

*p<.05 **p<.01 ***p<.001 ****p<.0001

Figures

Figure 1. Recidivism Incidents (Technical Violation Charges and Arrest) in the Total Pooled Sample.



Technical Violation Measure:	N=522 technical violation charges
Arrest Measure:	N=333 arrests

Figure 2. Distribution of Recidivism Outcomes in Total Sample (N=1,037).

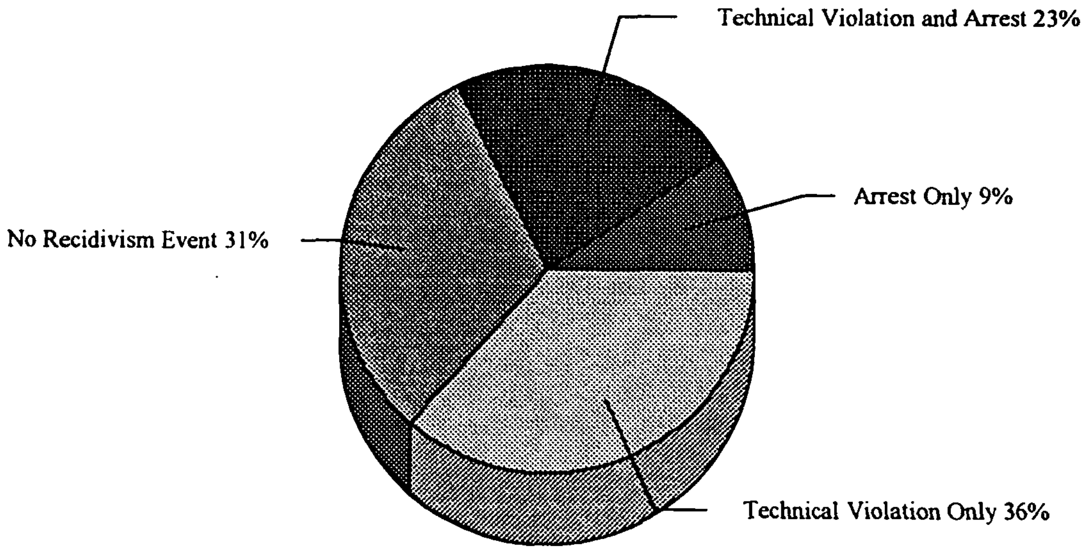


Figure 3. Technical Violation Charges by Whether the Technical Violation Sanction Resulted in Confinement in the Total Sample (N=1,037).

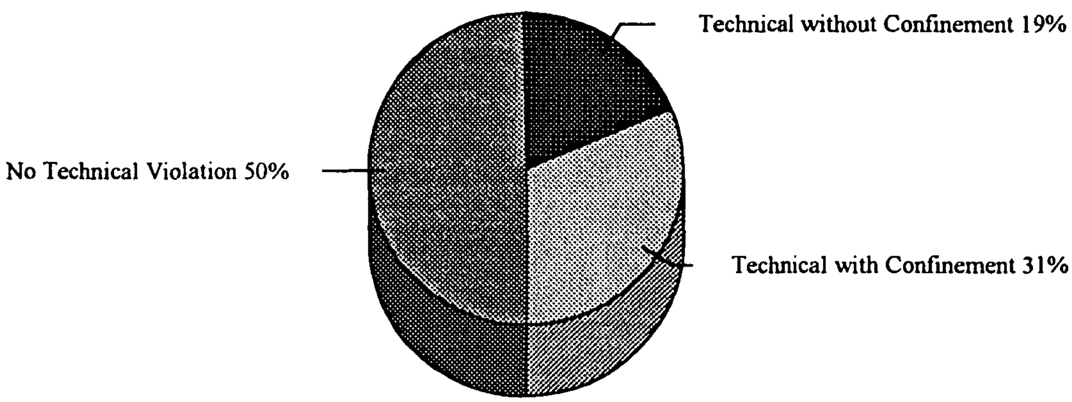


Figure 4. Predicted and Observed Number of Arrests in the Total Pooled Sample.

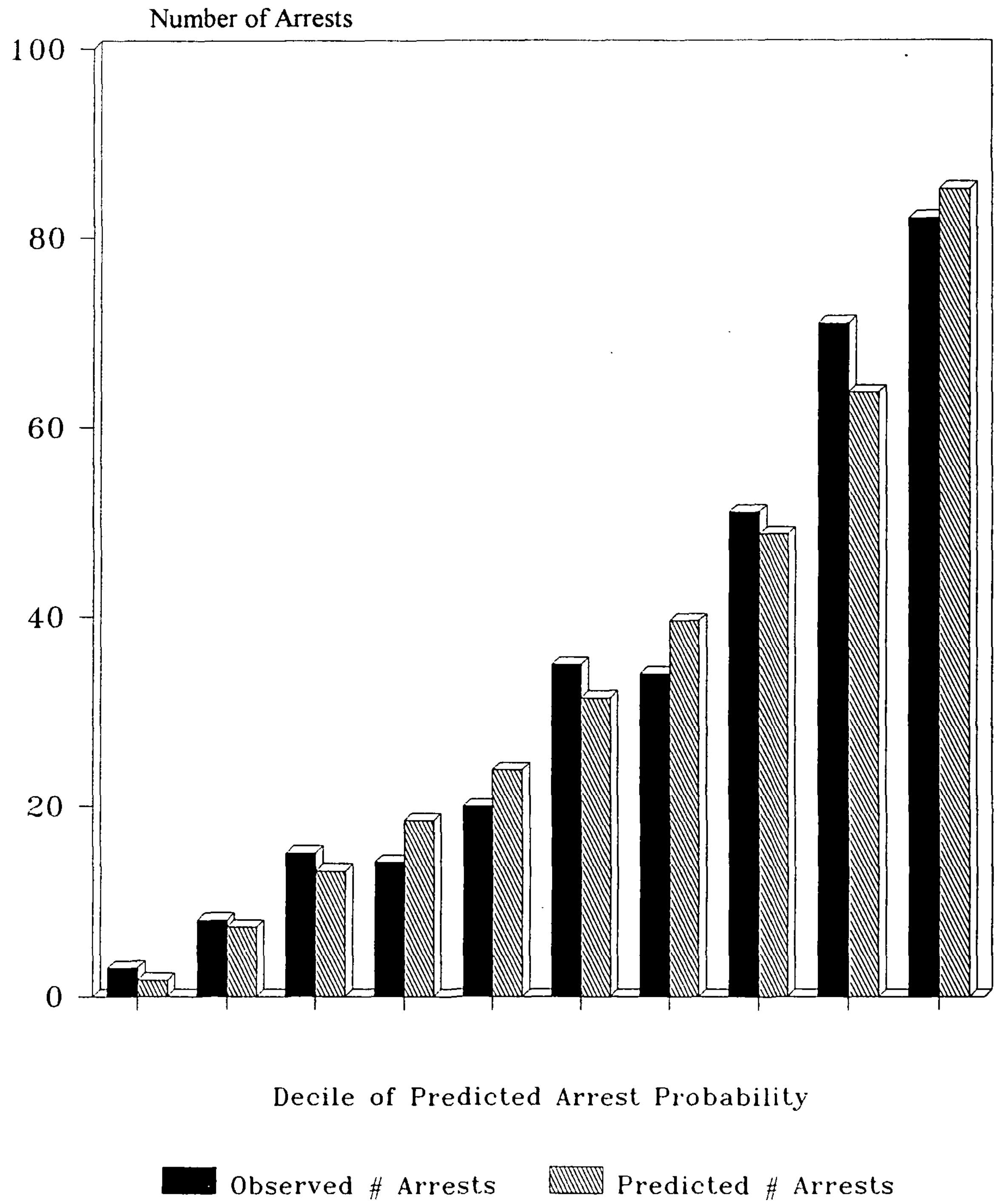


Figure 5. Predicted Probability of Arrest by Technical Violation (TV) in the Total Pooled Sample.

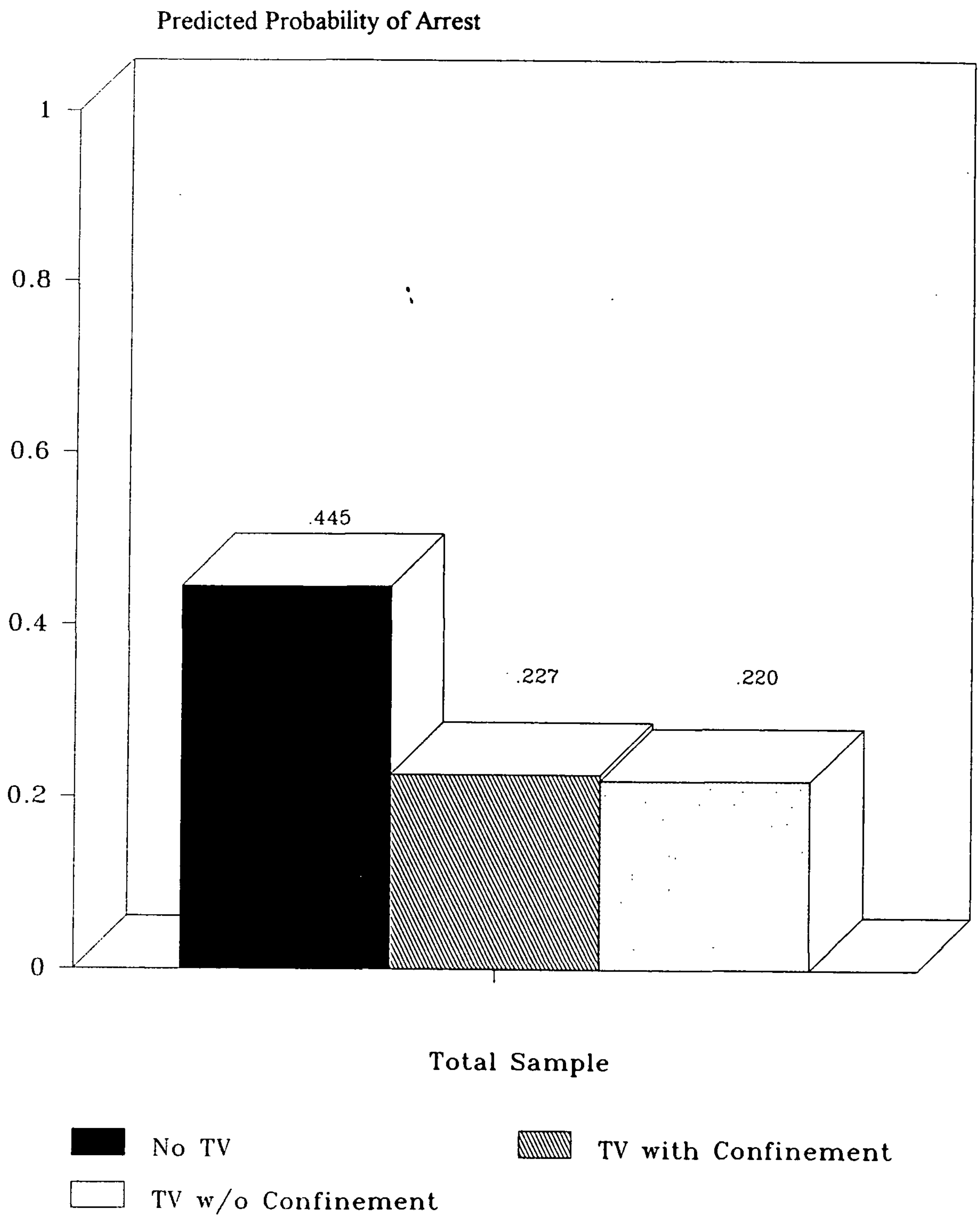


Figure 6. Technical Violation Charge Types in the Total Sample (N=1,037).

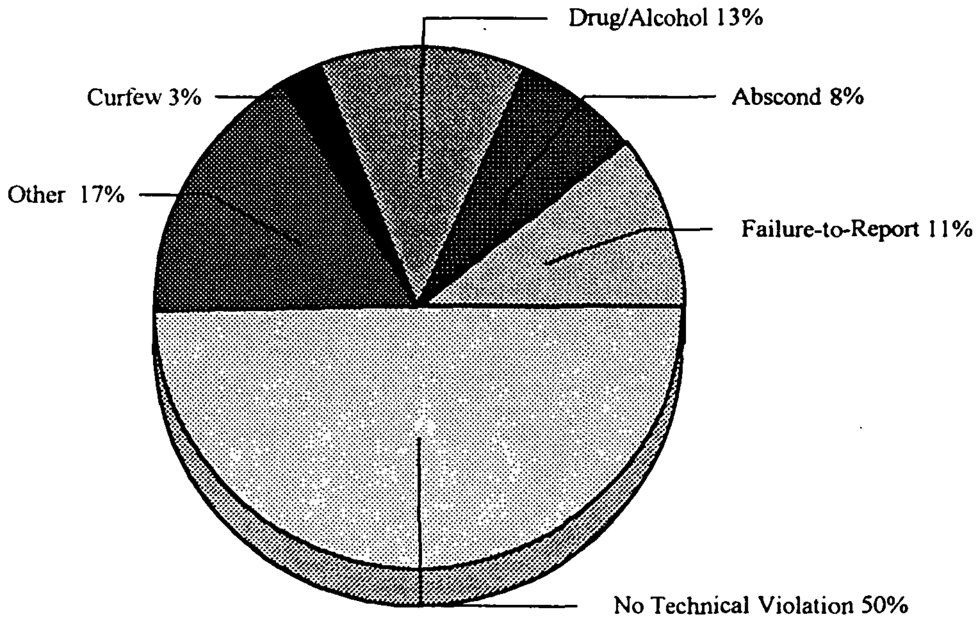


Figure 7. Predicted Probability of Arrest by Technical Violation Type in the Total Pooled Sample.

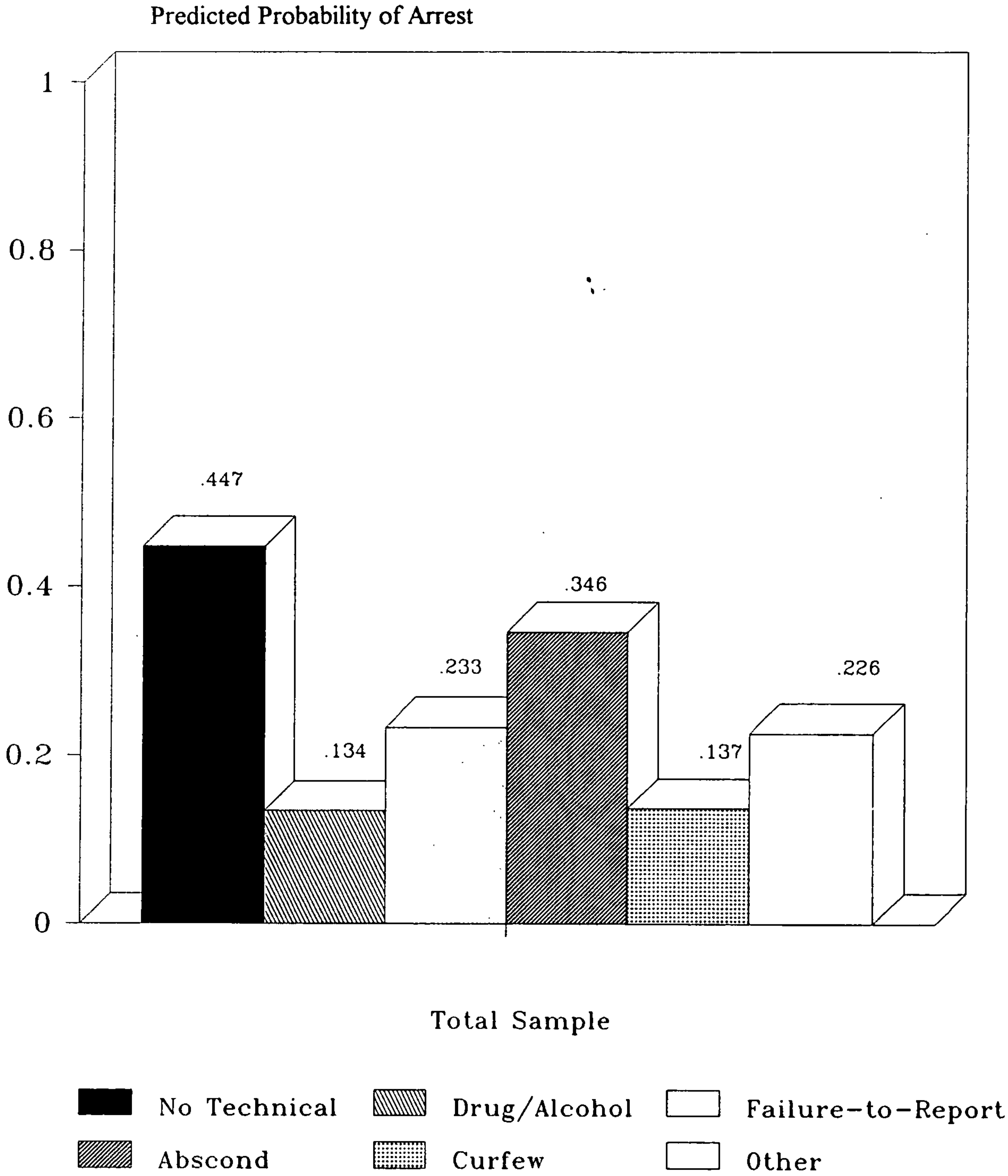


Figure 8. Arrest Type in the Total Sample (N=1,037).

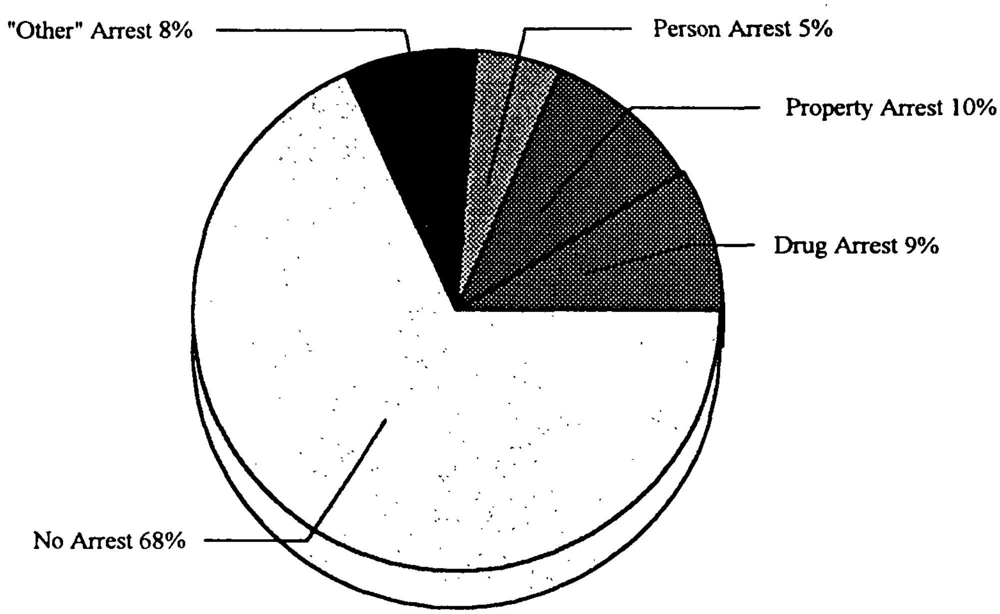


Figure 9. Predicted and Observed Number of Arrests in the Pooled California Sample.

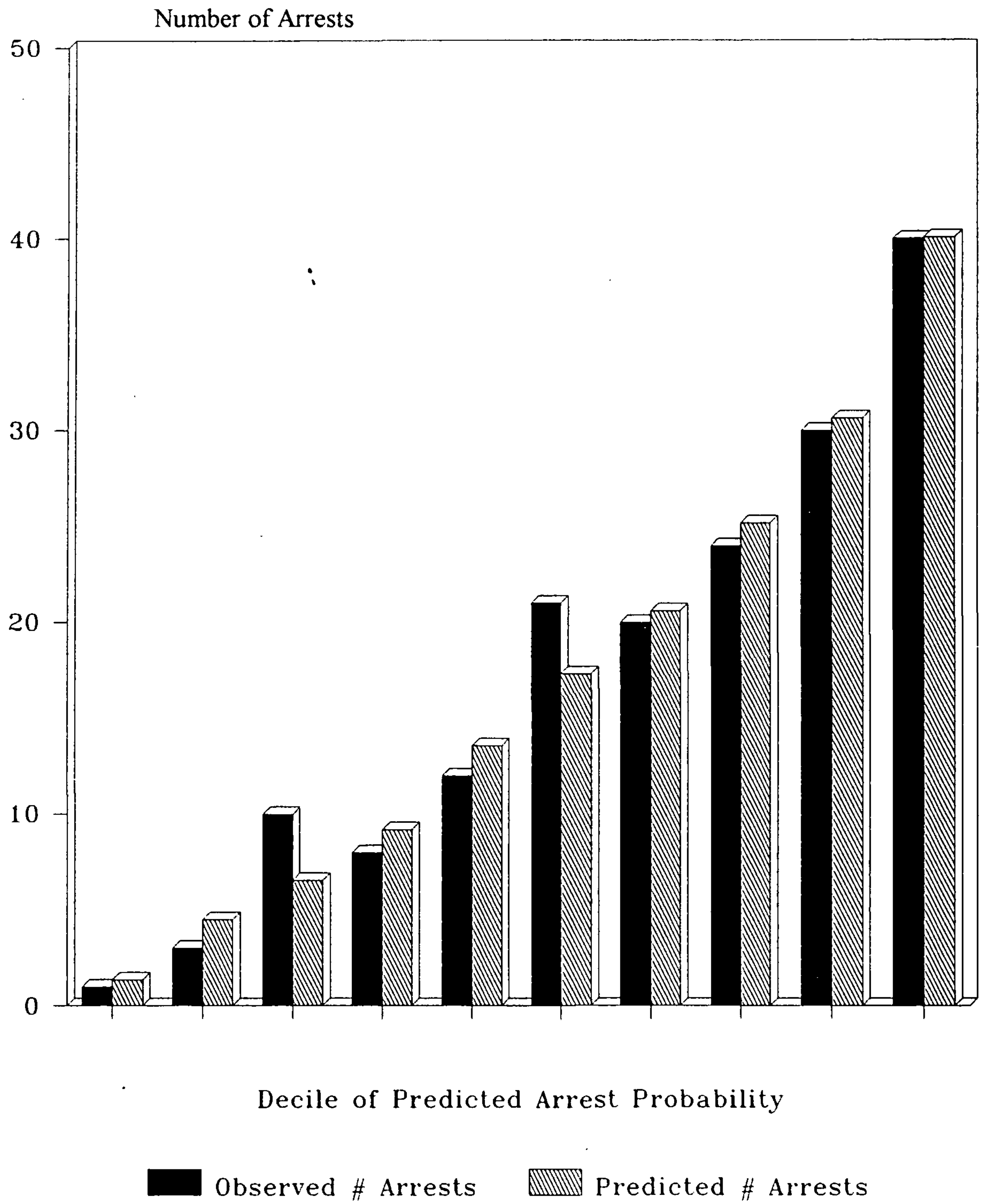


Figure 10. Predicted Probability of Arrest by Technical Violation (TV) in the Pooled California Sample.

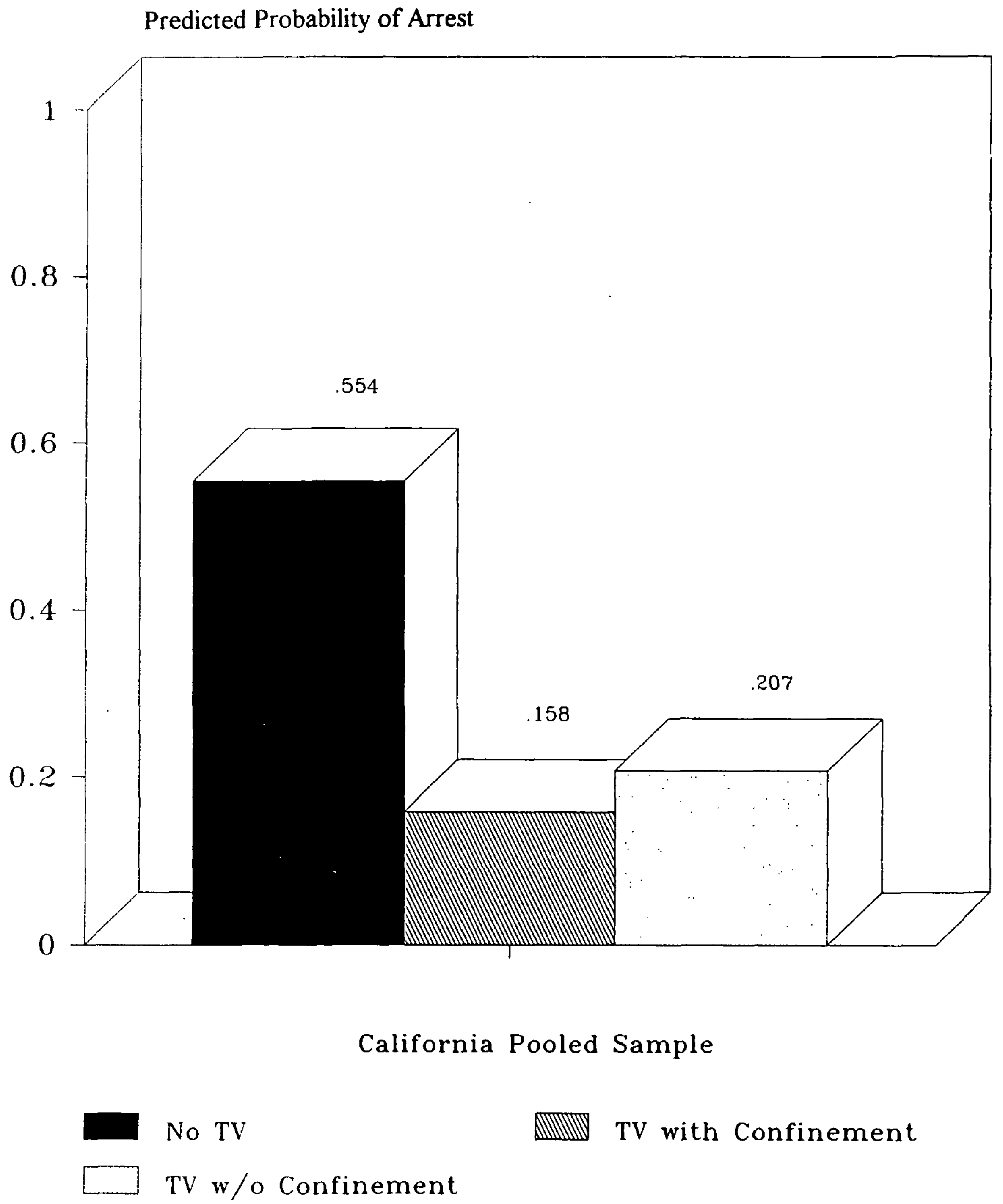


Figure 11. Technical Violation Charge Types in the Pooled California Sample (N=488).

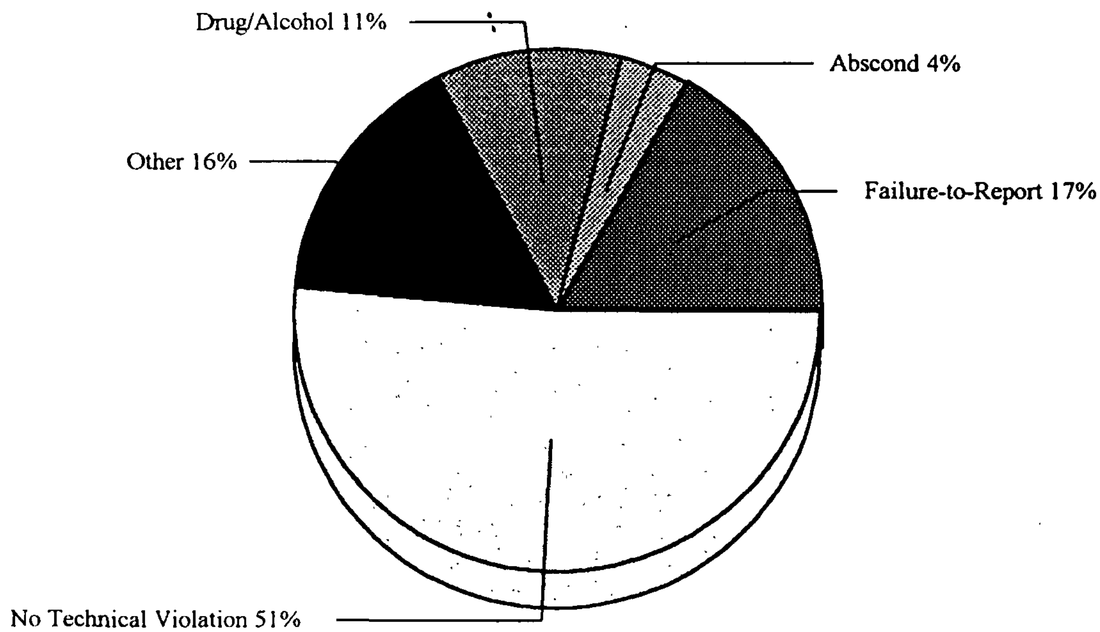


Figure 12. Predicted Probability of Arrest by Technical Violation Type in the Pooled California Sample.

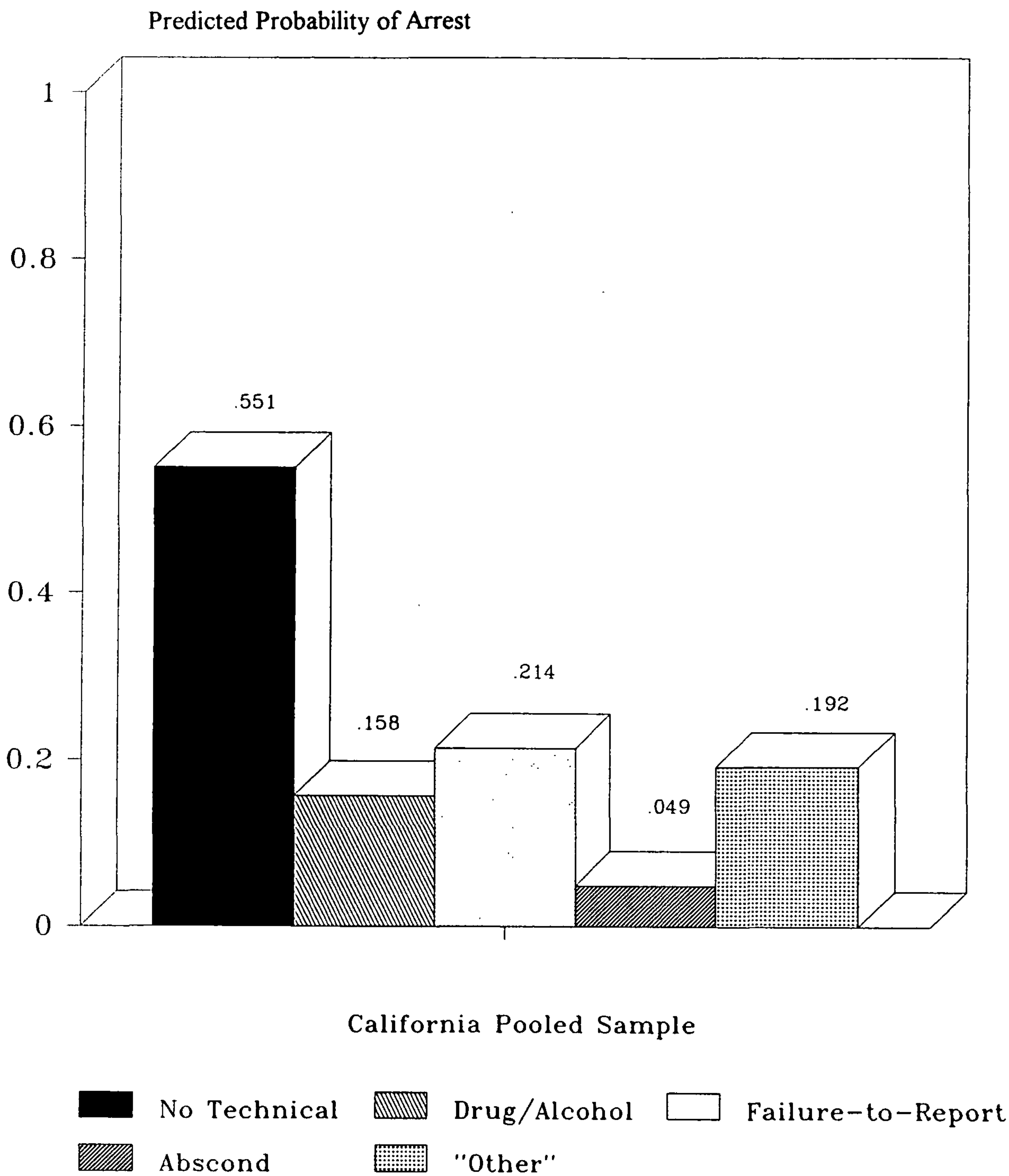


Figure 13. Predicted and Observed Number of Arrests in the Pooled Georgia Sample.

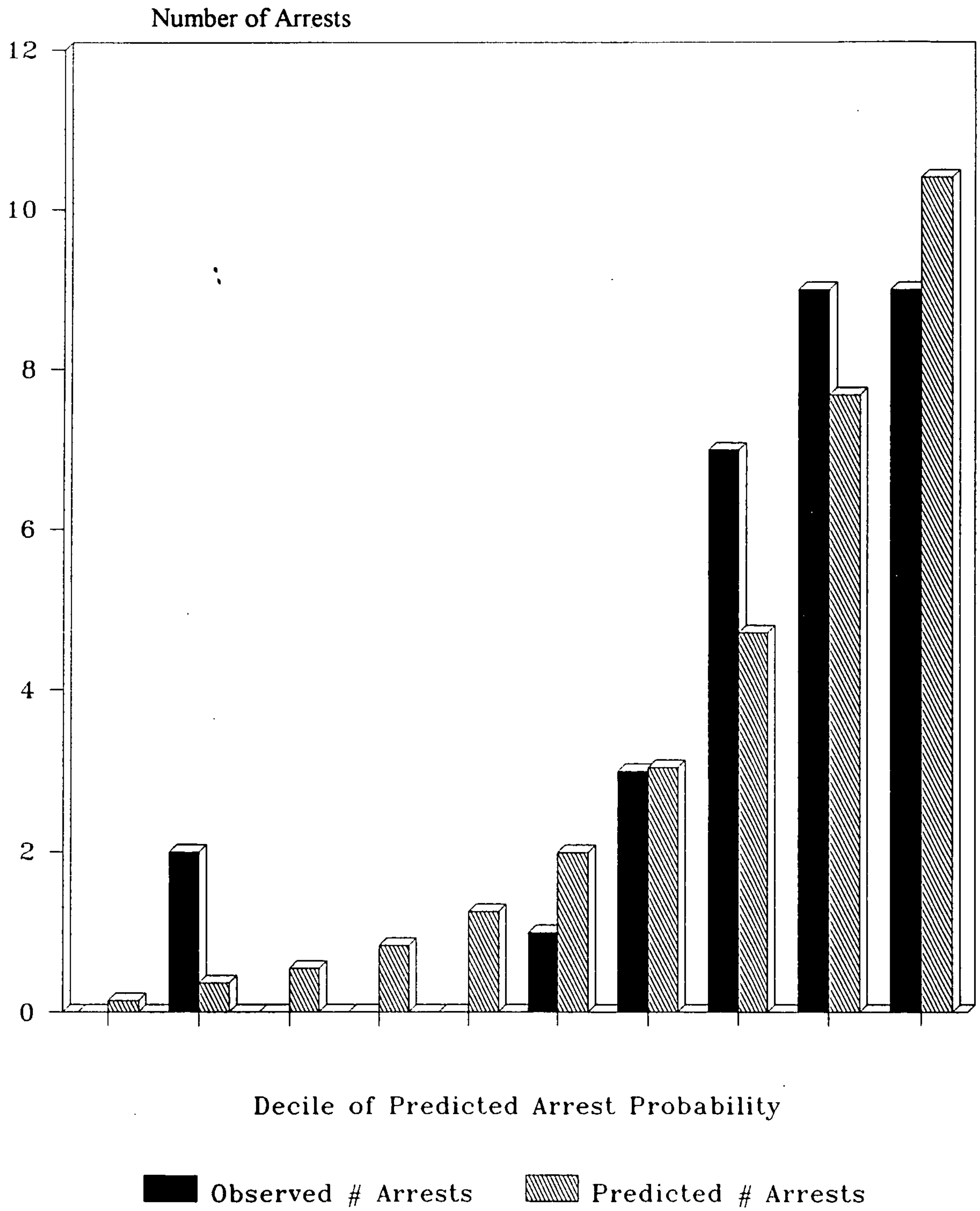


Figure 14. Predicted Probability of Arrest by Technical Violation (TV) in the Pooled Georgia Sample.

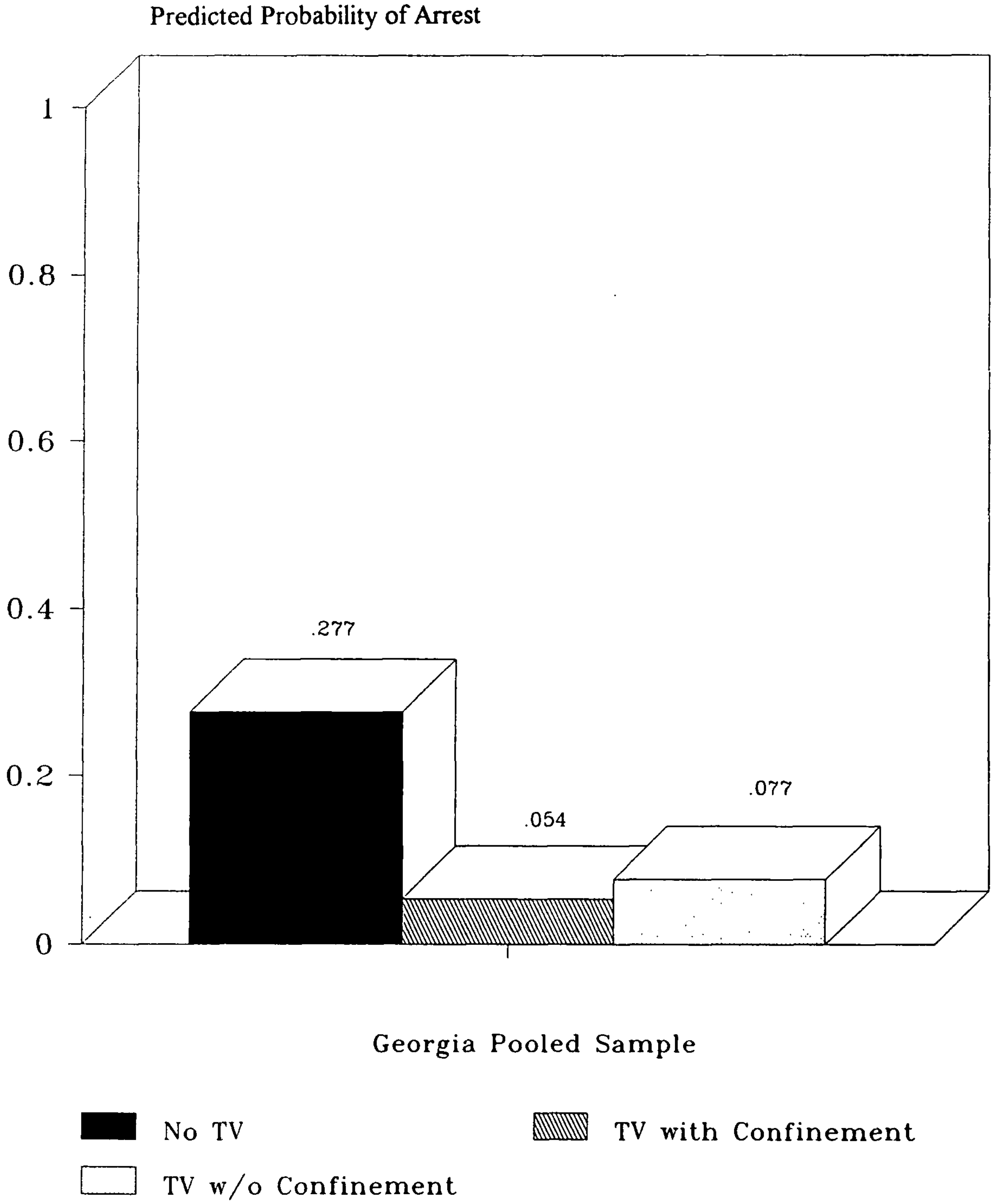


Figure 15. Technical Violation Charge Types in the Pooled Georgia Sample (N=150).

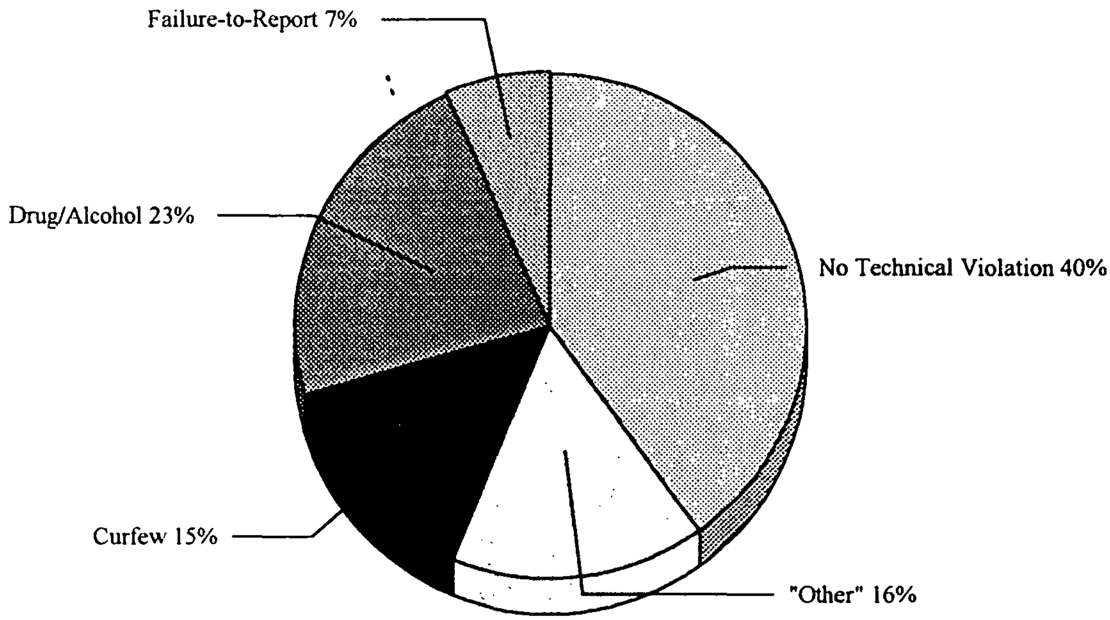


Figure 16. Predicted Probability of Arrest by Technical Violation Type in the Pooled Georgia Sample.

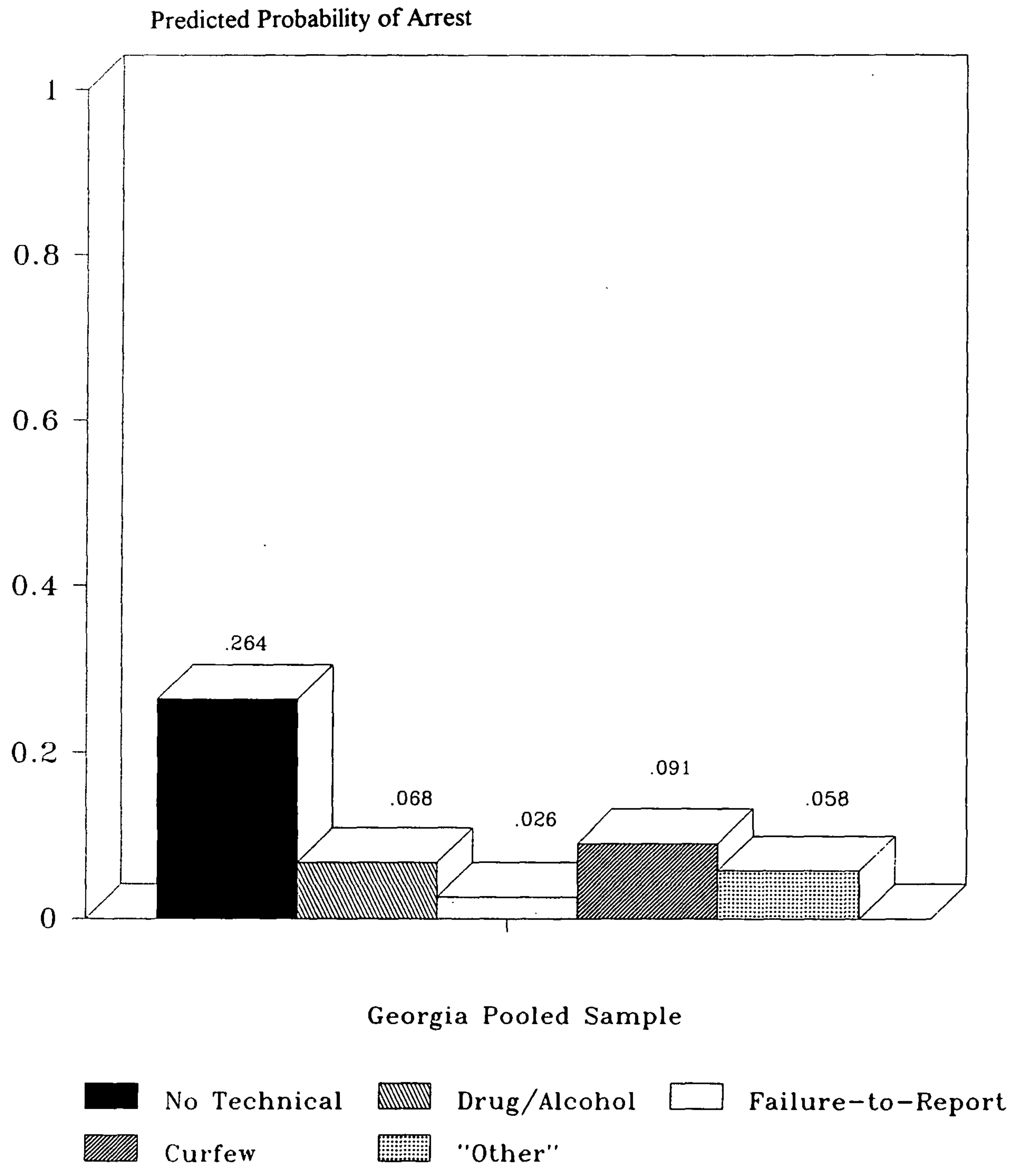


Figure 17. Predicted and Observed Number of Arrests in the Pooled Miscellaneous Sample.

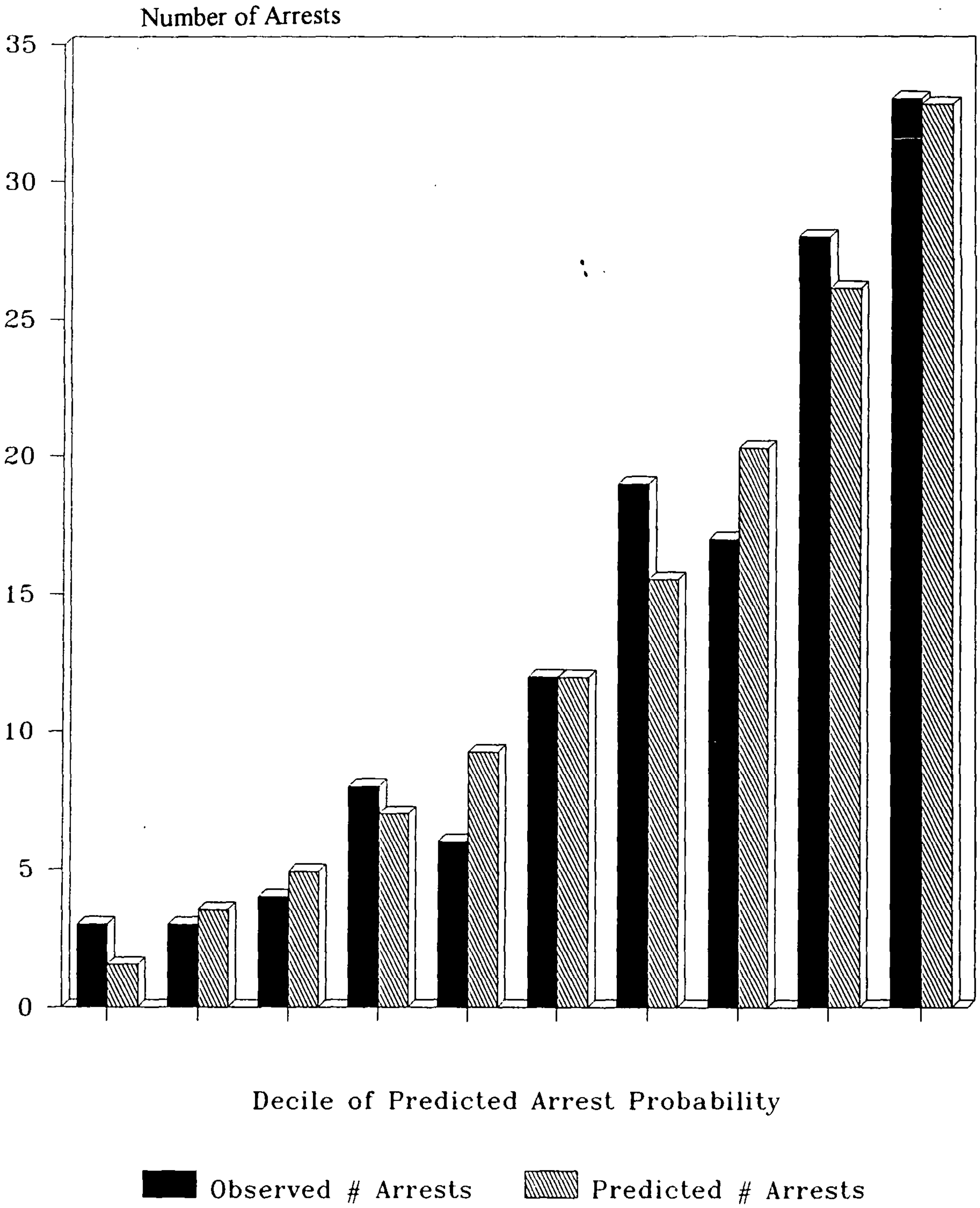


Figure 18. Predicted Probability of Arrest by ISP Sample Membership in the Pooled Miscellaneous Sample.

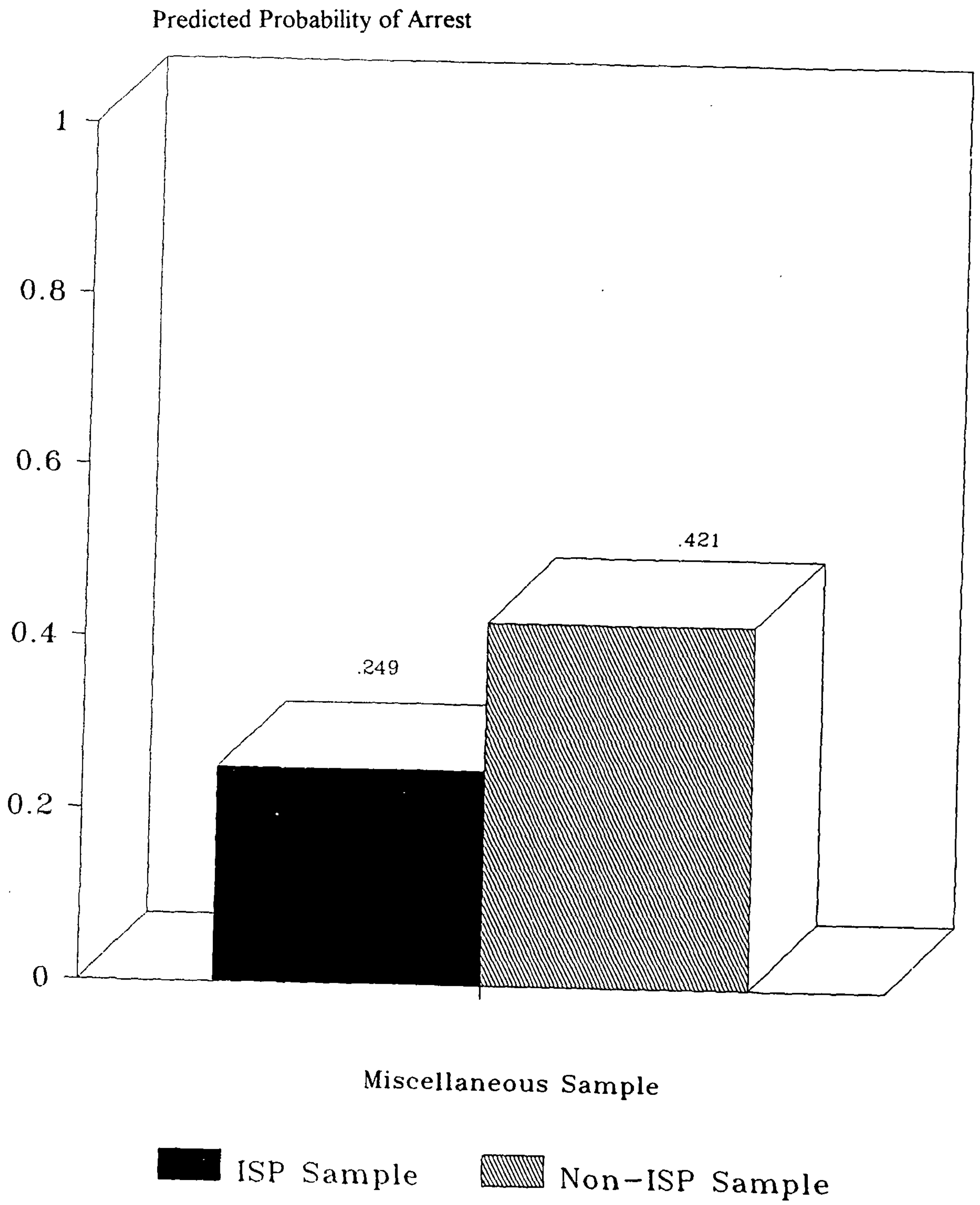


Figure 19. Technical Violation Charge Types in the Pooled Miscellaneous Sample (N=399).

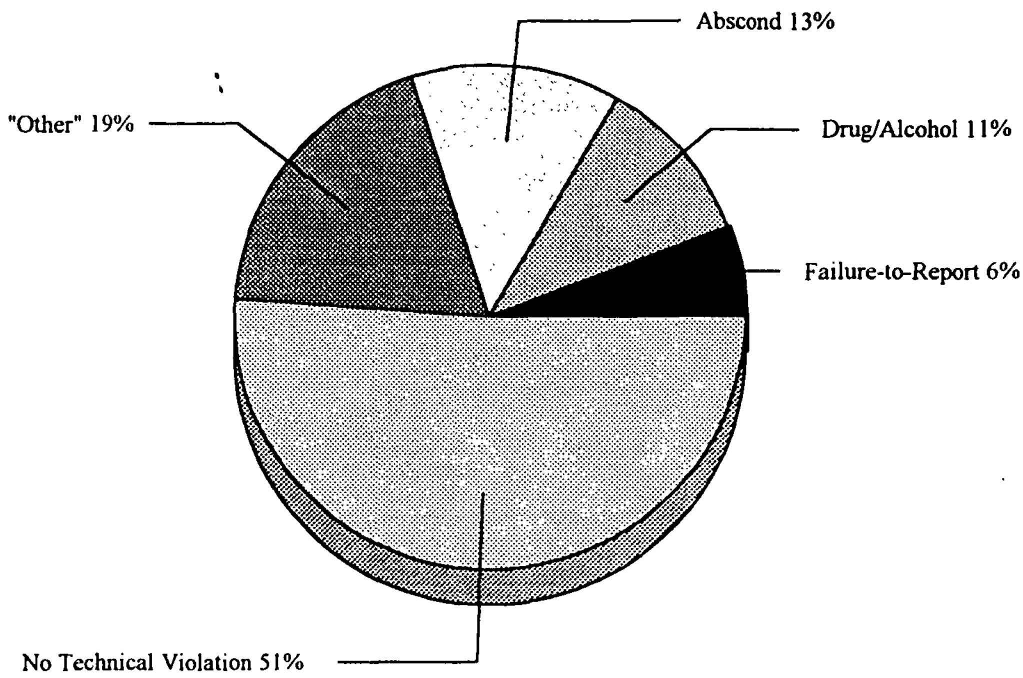
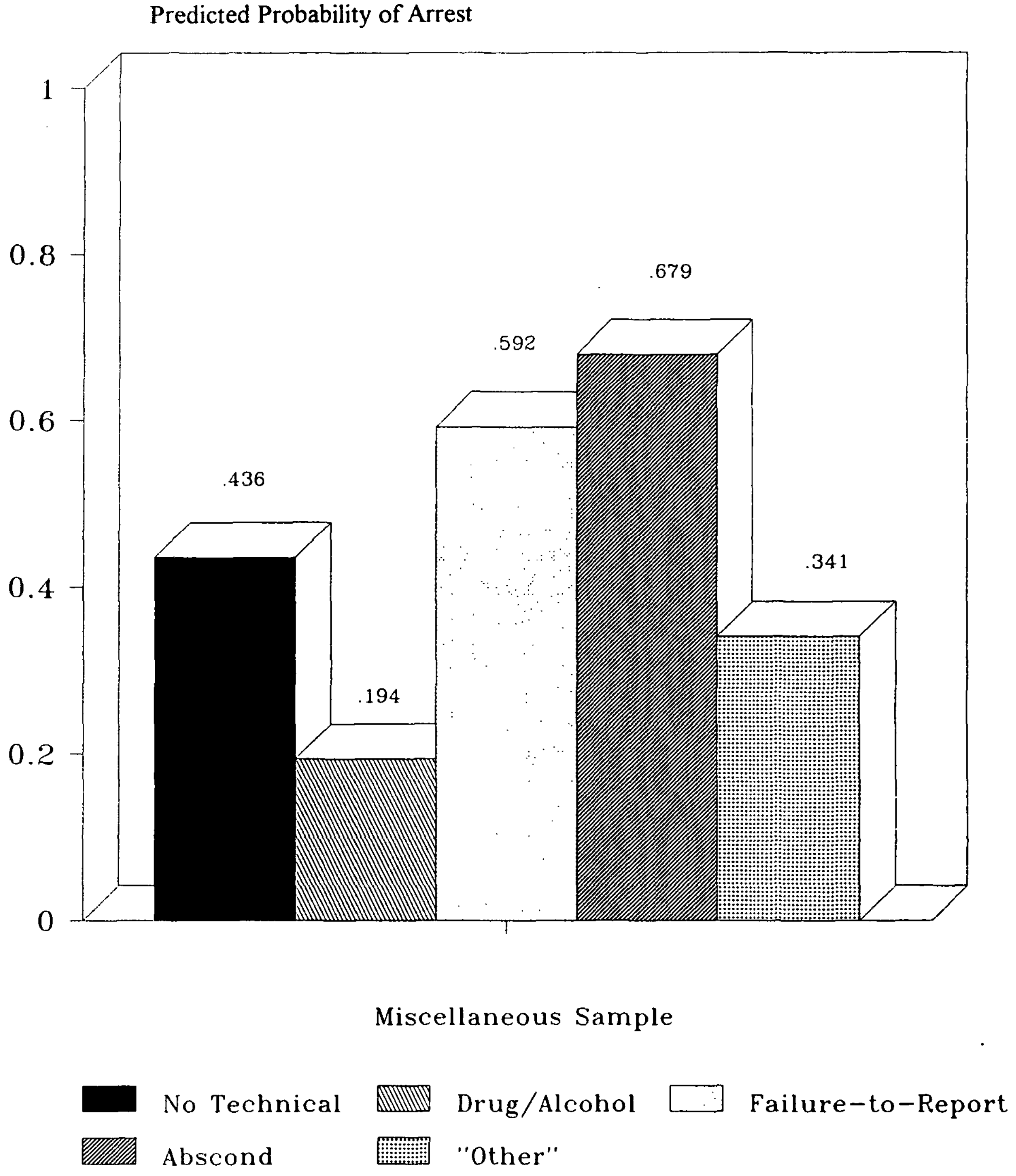


Figure 20. Predicted Probability of Arrest by Technical Violation Type in the Pooled Miscellaneous Sample.



Appendix

Appendix 1. Correlation Matrix of Explanatory Variables Used in the Test 1 Total Pooled Sample Analysis.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Exposure Risk	1.00000						
(2) Site 1	.11001	1.00000					
(3) Site 2	.11001	-.05066	1.00000				
(4) Site 3	.09337	-.05066	-.05066	1.00000			
(5) Site 4	-.23771	-.09966	-.09966	-.09966	1.00000		
(6) Site 5	-.13422	-.09328	-.09328	-.09328	-.18351	1.00000	
(7) Site 6	-.05006	-.09826	-.09826	-.09826	-.19331	-.18092	1.00000
(8) Site 7	.17262	-.07949	-.07949	-.07949	-.15639	-.14636	-.15418
(9) Site 8	.11566	-.05478	-.05478	-.05478	-.10778	-.10087	-.10626
(10) Site 9	.10855	-.05224	-.05224	-.05224	-.10277	-.09618	-.10132
(11) TV w/ Confinement	.07119	-.02591	.21669	-.09384	-.17002	-.03901	.08490
(12) TV w/o Confinement	.04766	.08608	.00575	-.04015	.11092	.02867	-.05052
(13) Age	.06905	-.02301	-.04739	-.06640	-.12122	.01715	.04848
(14) Phone Contacts (ln)	.08056	.30941	-.15135	-.08406	-.05602	.18633	-.15144
(15) Personal Contacts (ln)	.20599	.22105	.35799	.39384	-.28951	-.15860	.04552
(16) Personal Contacts x Site 1	.10013	.91023	-.04611	-.04611	-.09072	-.08490	-.08944
(17) Personal Contacts x Site 2	.10711	-.04932	.97362	-.04932	-.09704	-.09082	-.09567
(18) Personal Contacts x Site 3	.08821	-.04818	-.04818	.95115	-.09480	-.08872	-.09346
(19) Personal Contacts x Site 4	-.17183	-.07182	-.07182	-.07182	.72063	-.13224	-.13931
(20) Personal Contacts x Site 5	-.06416	-.06737	-.06737	-.06737	-.13255	.72227	-.13068
(21) Personal Contacts x Site 6	-.04430	-.08670	-.08670	-.08670	-.17057	-.15963	.88233
(22) Personal Contacts x Site 7	.14800	-.06815	-.06815	-.06815	-.13409	-.12549	-.13219
(23) Personal Contacts x Site 8	.10088	-.04871	-.04871	-.04871	-.09584	-.08970	-.09449
(24) Personal Contacts x Site 9	.09631	-.04549	-.04549	-.04549	-.08950	-.08376	-.08824
(25) Monitoring Checks (ln)	.20642	.01158	.14454	.16163	-.13761	-.35970	.09211
(26) Monitoring Checks x Site 1	.09998	.90880	-.04604	-.04604	-.09058	-.08477	-.08930
(27) Monitoring Checks x Site 2	.10789	-.04968	.98071	-.04968	-.09774	-.09148	-.09636
(28) Monitoring Checks x Site 3	.09018	-.04846	-.04846	.95656	-.09534	-.08923	-.09399
(29) Monitoring Checks x Site 4	-.19243	-.07809	-.07809	-.07809	.78351	-.14378	-.15146
(30) Monitoring Checks x Site 5	-.03434	-.06150	-.06150	-.06150	-.12099	.65928	-.11928
(31) Monitoring Checks x Site 6	-.03799	-.07798	-.07798	-.07798	-.15341	-.14358	.79359
(32) Monitoring Checks x Site 7	.15305	-.07048	-.07048	-.07048	-.13866	-.12977	-.13670
(33) Monitoring Checks x Site 8	.11326	-.05376	-.05376	-.05376	-.10576	-.09898	-.10427
(34) Monitoring Checks x Site 9	.09915	-.04702	-.04702	-.04702	-.09251	-.08658	-.09120
(35) Supervision Missing Flag	-.02103	.01399	-.07555	-.07555	-.14863	.33088	.24577
(36) Employment (ln)	.20588	.06119	.15177	.23747	-.23885	-.15909	.10382
(37) Employment Missing Flag	-.04836	-.06697	-.13173	-.14098	-.07179	.08695	.31421
(38) Counseling Sessions (ln)	.16488	.01460	.06204	.38673	-.13316	-.24358	.06941
(39) Counseling x Site 1	.05837	.53060	-.02688	-.02688	-.05288	-.04949	-.05214
(40) Counseling x Site 2	.07099	-.03269	.64531	-.03269	-.06431	-.06019	-.06341
(41) Counseling x Site 3	.08401	-.04661	-.04661	.92003	-.09169	-.08582	-.09040
(42) Counseling x Site 4	-.01462	-.03207	-.03207	-.03207	.32177	-.05905	-.06220

(Continued on next page)

Appendix 1--Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(43) Counseling x Site 5	.03227	-.01486	-.01486	-.01486	-.02924	.15931	-.02882
(44) Counseling x Site 6	-.01166	-.05771	-.05771	-.05771	-.11355	-.10627	.58737
(45) Counseling x Site 7	.09455	-.04354	-.04354	-.04354	-.08566	-.08017	-.08445
(46) Counseling x Site 8	.09255	-.04478	-.04478	-.04478	-.08810	-.08245	-.08686
(47) Counseling x Site 9	.04596	-.02116	-.02116	-.02116	-.04164	-.03897	-.04105
(48) Prior Misd. Convictions	-.04632	-.11545	-.06732	.01548	-.08185	-.10824	.15488
(49) Prior Prison Terms Served	.07710	-.01092	-.01934	-.06424	-.15511	-.01187	-.02713
(50) ISP Sample Membership	-.02366	-.00424	-.00424	-.02228	-.02609	.12307	-.04153
(51) ISP x Site 1	.07838	.71250	-.03609	-.03609	-.07101	-.06646	-.07001
(52) ISP x Site 2	.07838	-.03609	.71250	-.03609	-.07101	-.06646	-.07001
(53) ISP x Site 3	.06582	-.03464	-.03464	.68387	-.06816	-.06379	-.06720
(54) ISP x Site 4	-.14980	-.06725	-.06725	-.06725	.67480	-.12383	-.13045
(55) ISP x Site 5	-.09237	-.07474	-.07474	-.07474	-.14705	.80130	-.14497
(56) ISP x Site 6	-.05536	-.06508	-.06508	-.06508	-.12803	-.11982	.66228
(57) ISP x Site 7	.12005	-.05528	-.05528	-.05528	-.10876	-.10179	-.10723
(58) ISP x Site 8	.07829	-.03818	-.03818	-.03818	-.07511	-.07029	-.07405
(59) ISP x Site 9	.08077	-.03749	-.03749	-.03749	-.07376	-.06904	-.07272

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(8) Site 7	1.00000						
(9) Site 8	-.08596	1.00000					
(10) Site 9	-.08196	-.05649	1.00000				
(11) TV w/ Confinement	-.05986	.00744	-.03408	1.00000			
(12) TV w/o Confinement	.05600	-.05369	.03272	-.32719	1.00000		
(13) Age	.05645	.04983	-.05284	-.02439	-.02523	1.00000	
(14) Phone Contacts (ln)	.14358	.07007	-.02763	-.12852	.08337	-.00129	1.00000
(15) Personal Contacts (ln)	.00029	.10177	.02798	.00841	-.00716	-.01759	.41536
(16) Personal Contacts x Site 1	-.07235	-.04987	-.04755	-.05947	.08815	-.02856	.38044
(17) Personal Contacts x Site 2	-.07739	-.05334	-.05086	.21046	.01395	-.03489	-.14475
(18) Personal Contacts x Site 3	-.07561	-.05211	-.04968	-.10659	-.05291	-.05822	-.06732
(19) Personal Contacts x Site 4	-.11270	-.07767	-.07406	-.10963	.06630	-.10384	.11445
(20) Personal Contacts x Site 5	-.10571	-.07286	-.06947	-.05865	-.01314	.03678	.46527
(21) Personal Contacts x Site 6	-.13604	-.09376	-.08940	.05258	-.03144	.03502	-.10495
(22) Personal Contacts x Site 7	.85741	-.07370	-.07028	-.06522	.05585	.03858	.25028
(23) Personal Contacts x Site 8	-.07644	.88921	-.05023	-.01740	-.04117	.04192	.13978
(24) Personal Contacts x Site 9	-.07138	-.04920	.87090	-.01496	.05936	-.05806	.01510
(25) Monitoring Checks (ln)	.08492	.51485	.15175	.03689	-.00186	-.01284	.25162
(26) Monitoring Checks x Site 1	-.07224	-.04979	-.04747	-.05184	.06768	-.01559	.35346
(27) Monitoring Checks x Site 2	-.07796	-.05373	-.05123	.21639	.00215	-.04744	-.14669

(Continued on next page)

Appendix 1--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(28) Monitoring Checks x Site 3	-.07604	-.05240	-.04997	-.10313	-.05746	-.05886	-.07067
(29) Monitoring Checks x Site 4	-.12253	-.08445	-.08052	-.11516	.11686	-.10389	.09281
(30) Monitoring Checks x Site 5	.09649	-.06650	-.06341	-.03365	-.00633	.00691	.34936
(31) Monitoring Checks x Site 6	-.12236	-.08433	-.08040	.09102	-.02905	.03891	-.09144
(32) Monitoring Checks x Site 7	.88662	-.07622	-.07267	-.06687	.05584	.04008	.22698
(33) Monitoring Checks x Site 8	-.08435	.98128	-.05543	-.00117	-.04945	.05727	.08967
(34) Monitoring Checks x Site 9	-.07378	-.05085	.90016	-.02057	.04555	-.05351	.00641
(35) Supervision Missing Flag	-.11854	-.06779	-.07790	.08333	.00858	.00990	-.08332
(36) Employment (ln)	.12281	.18217	.07324	-.07176	-.05205	.03374	.21465
(37) Employment Missing Flag	.01737	-.05448	-.04853	.02543	-.02424	.09006	-.04579
(38) Counseling Sessions (ln)	.01435	.21791	-.02749	-.04020	-.05653	.06425	.12022
(39) Counseling x Site 1	-.04218	-.02907	-.02772	-.05267	.05108	.00969	.29622
(40) Counseling x Site 2	-.05130	-.03535	-.03371	.14241	.01548	-.01307	-.09679
(41) Counseling x Site 3	-.07313	-.05040	-.04806	-.09489	-.05498	-.05260	-.06358
(42) Counseling x Site 4	-.05032	-.03468	-.03307	-.03706	-.05108	.00200	.07142
(43) Counseling x Site 5	-.02332	-.01607	-.01532	.03064	.03739	.00884	.06192
(44) Counseling x Site 6	-.09056	-.06241	-.05951	.05200	-.02539	.08903	-.06215
(45) Counseling x Site 7	.54772	-.04708	-.04489	-.08380	.02330	.04465	.13644
(46) Counseling x Site 8	-.07026	.81739	-.04617	-.02467	-.03754	.05114	.15055
(47) Counseling x Site 9	-.03321	-.02289	.40515	.00662	.06811	.03595	.00885
(48) Prior Misd. Convictions	-.06632	-.04768	.09485	.06729	.01090	.07265	-.09738
(49) Prior Prison Terms Served	.25820	.06016	.01942	-.01555	.02295	.21857	.03960
(50) ISP Sample Membership	-.01158	-.01434	-.00052	.11221	.03303	-.00561	.30516
(51) ISP x Site 1	-.05664	-.03903	-.03722	.01132	.06385	-.02270	.18388
(52) ISP x Site 2	-.05664	-.03903	-.03722	.13099	.03240	-.01627	-.09767
(53) ISP x Site 3	-.05436	-.03746	-.03572	-.06252	-.00914	-.03165	-.05118
(54) ISP x Site 4	-.10553	-.07273	-.06935	-.07305	.11518	-.11587	.10568
(55) ISP x Site 5	-.11728	-.08083	-.07707	-.00195	.00356	.03302	.33447
(56) ISP x Site 6	-.10211	-.07037	-.06710	.07735	-.01103	.01934	-.09684
(57) ISP x Site 7	.69546	-.05978	-.05700	-.00440	-.00221	.02258	.14242
(58) ISP x Site 8	-.05990	.69686	-.03936	-.00112	-.03741	.01954	.14929
(59) ISP x Site 9	-.05883	-.04055	.71778	.00288	.05582	-.03966	.00968

	(15)	(16)	(17)	(18)	(19)	(20)	(21)
(15) Personal Contacts (ln)	1.00000						
(16) Personal Contacts x Site 1	.29249	1.00000					
(17) Personal Contacts x Site 2	.38143	-.04489	1.00000				
(18) Personal Contacts x Site 3	.43964	-.04386	-.04691	1.00000			

(Continued on next page)

Appendix 1--Continued.

	(15)	(16)	(17)	(18)	(19)	(20)	(21)
(19) Personal Contacts x Site 4	-.05405	-.06537	-.06993	-.06831	1.00000		
(20) Personal Contacts x Site 5	.09557	-.06132	-.06559	-.06408	-.09552	1.00000	
(21) Personal Contacts x Site 6	.16756	-.07891	-.08441	-.08246	-.12291	-.11530	1.00000
(22) Personal Contacts x Site 7	.12446	-.06204	-.06636	-.06483	-.09663	-.09064	-.11664
(23) Personal Contacts x Site 8	.18233	-.04434	-.04743	-.04633	-.06906	-.06479	-.08337
(24) Personal Contacts x Site 9	.10989	-.04141	-.04429	-.04327	-.06450	-.06050	-.07785
(25) Monitoring Checks (ln)	.64138	.04818	.15081	.18831	.04412	-.21011	.20987
(26) Monitoring Checks x Site 1	.26644	.95034	-.04482	-.04379	-.06527	-.06123	-.07879
(27) Monitoring Checks x Site 2	.36653	-.04522	.97929	-.04725	-.07044	-.06607	-.08502
(28) Monitoring Checks x Site 3	.43040	-.04411	-.04718	.98847	-.06870	-.06444	-.08293
(29) Monitoring Checks x Site 4	-.12249	-.07108	-.07603	-.07427	.88909	-.10385	-.13364
(30) Monitoring Checks x Site 5	.00325	-.05598	-.05987	-.05849	-.08719	.72160	-.10524
(31) Monitoring Checks x Site 6	.14604	-.07098	-.07592	-.07417	-.11055	-.10370	.89131
(32) Monitoring Checks x Site 7	.09883	-.06415	-.06862	-.06703	-.09992	-.09373	-.12061
(33) Monitoring Checks x Site 8	.11919	-.04893	-.05234	-.05113	-.07622	-.07149	-.09200
(34) Monitoring Checks x Site 9	.09367	-.04280	-.04578	-.04472	-.06666	-.06253	-.08047
(35) Supervision Missing Flag	-.16875	.02841	-.07355	-.07186	-.10711	.05119	.11011
(36) Employment (ln)	.58645	.08999	.17071	.26781	-.11109	-.00744	.18206
(37) Employment Missing Flag	-.09984	-.03919	-.13934	-.13194	-.00392	-.01676	.23262
(38) Counseling Sessions (ln)	.50923	.04987	.07357	.41983	-.03133	-.17472	.14841
(39) Counseling x Site 1	.18976	.61909	-.02617	-.02557	-.03811	-.03575	-.04600
(40) Counseling x Site 2	.25961	-.02976	.67357	-.03109	-.04635	-.04348	-.05595
(41) Counseling x Site 3	.42806	-.04242	-.04538	.97139	-.06608	-.06198	-.07976
(42) Counseling x Site 4	-.03293	-.02919	-.03122	-.03050	.41916	-.04265	-.05488
(43) Counseling x Site 5	-.00668	-.01353	-.01447	-.01413	-.02107	.15738	-.02543
(44) Counseling x Site 6	.13247	-.05253	-.05619	-.05489	-.08182	-.07675	.70208
(45) Counseling x Site 7	.04829	-.03963	-.04239	-.04141	-.06173	-.05790	-.07451
(46) Counseling x Site 8	.16699	-.04076	-.04360	-.04259	-.06349	-.05955	-.07663
(47) Counseling x Site 9	.03521	-.01926	-.02061	-.02013	-.03000	-.02815	-.03622
(48) Prior Misd. Convictions	-.01621	-.10515	-.07071	.02916	-.04312	-.09390	.14013
(49) Prior Prison Terms Served	-.00737	-.01741	-.02278	-.06168	-.11661	-.00793	-.04323
(50) ISP Sample Membership	.30984	-.01384	-.00130	-.02142	.17907	.21844	.04671
(51) ISP x Site 1	.14054	.61667	-.03514	-.03433	-.05117	-.04800	-.06177
(52) ISP x Site 2	.26077	-.03285	.70273	-.03433	-.05117	-.04800	-.06177
(53) ISP x Site 3	.26883	-.03153	-.03373	.64972	-.04912	-.04607	-.05929
(54) ISP x Site 4	-.07958	-.06122	-.06548	-.06397	.84633	-.08944	-.11510
(55) ISP x Site 5	-.03212	-.06803	-.07277	-.07109	-.10597	.79495	-.12791
(56) ISP x Site 6	.11984	-.05923	-.06336	-.06190	-.09226	-.08654	.74029
(57) ISP x Site 7	.05604	-.05032	-.05382	-.05258	-.07838	-.07352	-.09461
(58) ISP x Site 8	.18566	-.03475	-.03717	-.03631	-.05412	-.05077	-.06533
(59) ISP x Site 9	.11028	-.03413	-.03650	-.03566	-.05316	-.04986	-.06417

(Continued on next page)

Appendix 1--Continued.

	(22)	(23)	(24)	(25)	(26)	(27)	(28)
(22) Personal Contacts x Site 7	1.00000						
(23) Personal Contacts x Site 8	-.06554	1.00000					
(24) Personal Contacts x Site 9	-.06120	-.04375	1.00000				
(25) Monitoring Checks (ln)	.18341	.49248	.22202	1.00000			
(26) Monitoring Checks x Site 1	-.06194	-.04427	-.04134	.06374	1.00000		
(27) Monitoring Checks x Site 2	-.06684	-.04777	-.04461	.15751	-.04515	1.00000	
(28) Monitoring Checks x Site 3	-.06519	-.04660	-.04352	.19198	-.04404	-.04752	1.00000
(29) Monitoring Checks x Site 4	-.10506	-.07509	-.07012	.06269	-.07097	-.07658	-.07470
(30) Monitoring Checks x Site 5	-.08273	-.05914	-.05522	-.12266	-.05589	-.06031	-.05882
(31) Monitoring Checks x Site 6	-.10491	-.07498	-.07002	.32223	-.07087	-.07647	-.07459
(32) Monitoring Checks x Site 7	.97037	-.06777	-.06329	.18786	-.06405	-.06912	-.06742
(33) Monitoring Checks x Site 8	-.07232	.91662	-.04827	.53440	-.04886	-.05272	-.05142
(34) Monitoring Checks x Site 9	-.06326	-.04522	.97735	.22475	-.04273	-.04611	-.04498
(35) Supervision Missing Flag	-.10164	-.06612	-.06784	-.14334	.03477	-.07409	-.07226
(36) Employment (ln)	.17709	.19503	.09968	.51802	.11341	.16973	.26089
(37) Employment Missing Flag	.03878	-.06039	-.05282	-.03259	-.04995	-.13162	-.12738
(38) Counseling Sessions (ln)	.05003	.27409	-.00626	.47473	.04624	.06898	.41368
(39) Counseling x Site 1	-.03616	-.02585	-.02414	.04365	.60491	-.02636	-.02571
(40) Counseling x Site 2	-.04398	-.03144	-.02936	.10482	-.02971	.66086	-.03127
(41) Counseling x Site 3	-.06270	-.04482	-.04185	.18434	-.04236	-.04571	.96111
(42) Counseling x Site 4	-.04315	-.03084	-.02880	.04550	-.02914	-.03145	-.03068
(43) Counseling x Site 5	-.01999	-.01429	-.01334	-.04733	-.01350	-.01457	-.01421
(44) Counseling x Site 6	-.07765	-.05550	-.05183	.16615	-.05245	-.05660	-.05521
(45) Counseling x Site 7	.57224	-.04187	-.03910	.10361	-.03957	-.04270	-.04165
(46) Counseling x Site 8	-.06025	.91784	-.04021	.46178	-.04070	-.04392	-.04283
(47) Counseling x Site 9	-.02847	-.02035	.42028	.08807	-.01923	-.02075	-.02024
(48) Prior Misd. Convictions	-.06419	-.04728	.11494	.05204	-.10296	-.06857	.03077
(49) Prior Prison Terms Served	.21660	.06138	.00332	.06961	.00340	-.01958	-.05416
(50) ISP Sample Membership	.04533	.07362	.08227	.30511	.00350	-.00024	-.01934
(51) ISP x Site 1	-.04856	-.03471	-.03241	.01544	.67102	-.03540	-.03453
(52) ISP x Site 2	-.04856	-.03471	-.03241	.10815	-.03280	.71128	-.03453
(53) ISP x Site 3	-.04661	-.03331	-.03111	.11342	-.03148	-.03398	.66073
(54) ISP x Site 4	-.09048	-.06467	-.06039	.05540	-.06112	-.06596	-.06433
(55) ISP x Site 5	-.10056	-.07187	-.06712	-.24478	-.06793	-.07330	-.07150
(56) ISP x Site 6	-.08755	-.06258	-.05844	.29988	-.05914	-.06382	-.06225
(57) ISP x Site 7	.71536	-.05316	-.04964	.10453	-.05024	-.05422	-.05288
(58) ISP x Site 8	-.05136	.88116	-.03428	.36901	-.03469	-.03744	-.03652
(59) ISP x Site 9	-.05044	-.03605	.87984	.20559	-.03407	-.03677	-.03587

(Continued on next page)

Appendix 1--Continued.

	(29)	(30)	(31)	(32)	(33)	(34)	(35)
(29) Monitoring x Site 4	1.00000						
(30) Monitoring x Site 5	-.09479	1.00000					
(31) Monitoring x Site 6	-.12020	-.09466	1.00000				
(32) Monitoring x Site 7	-.10864	-.08555	-.10848	1.00000			
(33) Monitoring x Site 8	-.08287	-.06526	-.08275	-.07479	1.00000		
(34) Monitoring x Site 9	-.07248	-.05708	-.07238	-.06542	-.04990	1.00000	
(35) Supervision Missing Flag	-.11645	.15757	.15327	-.10510	-.06595	-.07012	1.00000
(36) Employment (ln)	-.14395	-.04515	.15003	.19251	.20302	.10238	-.18803
(37) Employment Missing Flag	-.03038	.03009	.22188	.04730	-.05330	-.04659	.39815
(38) Counseling Sessions (ln)	-.03418	-.15917	.13404	.05261	.23571	-.00974	-.14508
(39) Counseling x Site 1	-.04143	-.03263	-.04137	-.03739	-.02852	-.02495	.02140
(40) Counseling x Site 2	-.05039	-.03968	-.05032	-.04548	-.03469	-.03034	-.04875
(41) Counseling x Site 3	-.07184	-.05658	-.07174	-.06484	-.04946	-.04326	-.06950
(42) Counseling x Site 4	.45541	-.03893	-.04936	-.04462	-.03403	-.02977	-.04782
(43) Counseling x Site 5	-.02291	.15429	-.02287	-.02067	-.01577	-.01379	.12508
(44) Counseling x Site 6	-.08896	-.07006	.63263	-.08029	-.06125	-.05357	-.01474
(45) Counseling x Site 7	-.06711	-.05285	-.06702	.59412	-.04620	-.04041	-.06493
(46) Counseling x Site 8	-.06903	-.05436	-.06893	-.06230	.85412	-.04156	-.06212
(47) Counseling x Site 9	-.03262	-.02569	-.03258	-.02944	-.02246	.42193	-.03156
(48) Prior Misd. Convictions	-.05097	-.06763	.14026	-.06344	-.04802	.10477	.03144
(49) Prior Prison Terms Served	-.12378	.02543	-.01926	.22289	.05777	.00927	.01245
(50) ISP Sample Membership	.16407	.20970	.15680	.02983	-.00978	.06710	.07309
(51) ISP x Site 1	-.05564	-.04382	-.05556	-.05021	-.03830	-.03350	.06885
(52) ISP x Site 2	-.05564	-.04382	-.05556	-.05021	-.03830	-.03350	-.05383
(53) ISP x Site 3	-.05340	-.04206	-.05333	-.04820	-.03676	-.03216	-.05166
(54) ISP x Site 4	.86445	-.08164	-.10352	-.09356	-.07137	-.06242	-.10029
(55) ISP x Site 5	-.11521	.74283	-.11505	-.10398	-.07932	-.06938	.28398
(56) ISP x Site 6	-.10031	-.07900	.88055	-.09053	-.06906	-.06040	.19049
(57) ISP x Site 7	-.08522	-.06711	-.08509	.70302	-.05866	-.05131	-.08244
(58) ISP x Site 8	-.05885	-.04634	-.05876	-.05311	.69682	-.03543	-.05693
(59) ISP x Site 9	-.05780	-.04551	-.05771	-.05216	-.03979	.85417	-.05591

	(36)	(37)	(38)	(39)	(40)	(41)	(42)
(36) Employment (ln)	1.00000						
(37) Employment Missing Flag	-.08584	1.00000					
(38) Counseling Sessions (ln)	.40188	-.08332	1.00000				
(39) Counseling x Site 1	.07283	.01906	.20080	1.00000			
(40) Counseling x Site 2	.15234	-.10275	.20979	-.01735	1.00000		

(Continued on next page)

Appendix 1--Continued.

	(36)	(37)	(38)	(39)	(40)	(41)	(42)
(41) Counseling x Site 3	.25871	-.12417	.43870	-.02473	-.03008	1.00000	
(42) Counseling x Site 4	-.06343	-.02575	.26651	-.01702	-.02069	-.02950	1.00000
(43) Counseling x Site 5	.01958	.03908	-.01087	-.00788	-.00959	-.01367	-.00941
(44) Counseling x Site 6	.17597	.11003	.35138	-.03062	-.03724	-.05310	-.03654
(45) Counseling x Site 7	.11851	.02235	.26521	-.02310	-.02810	-.04006	-.02756
(46) Counseling x Site 8	.18272	-.05878	.31768	-.02376	-.02890	-.04120	-.02835
(47) Counseling x Site 9	.05845	-.06148	.20795	-.01123	-.01366	-.01947	-.01340
(48) Prior Misd. Convictions	-.06655	.03473	.00863	-.05910	-.03399	.03134	-.02149
(49) Prior Prison Terms Served	.04824	.00985	.01643	-.02470	-.00480	-.06220	-.05595
(50) ISP Sample Membership	.11435	.00701	.10348	.00450	.03183	-.00773	.04544
(51) ISP x Site 1	.02295	-.02540	.01620	.39960	-.02329	-.03321	-.02285
(52) ISP x Site 2	.13156	-.11414	.07630	-.01915	.57014	-.03321	-.02285
(53) ISP x Site 3	.15834	-.09536	.28735	-.01838	-.02236	.67156	-.02193
(54) ISP x Site 4	-.11316	-.01954	-.05606	-.03568	-.04340	-.06188	.31508
(55) ISP x Site 5	-.07811	.08859	-.19351	-.03966	-.04823	-.06877	-.04732
(56) ISP x Site 6	.09559	.17990	.10234	-.03453	-.04199	-.05987	-.04120
(57) ISP x Site 7	.10368	.01967	.02920	-.02933	-.03567	-.05086	-.03500
(58) ISP x Site 8	.15497	-.06201	.23641	-.02026	-.02464	-.03512	-.02417
(59) ISP x Site 9	.09204	-.05837	.01646	-.01989	-.02420	-.03450	-.02374

	(43)	(44)	(45)	(46)	(47)	(48)	(49)
(43) Counseling x Site 5	1.00000						
(44) Counseling x Site 6	-.01693	1.00000					
(45) Counseling x Site 7	-.01277	-.04960	1.00000				
(46) Counseling x Site 8	-.01314	-.05102	-.03849	1.00000			
(47) Counseling x Site 9	-.00621	-.02411	-.01819	-.01871	1.00000		
(48) Prior Misd. Convictions	.00475	.12060	-.04680	-.04051	.06314	1.00000	
(49) Prior Prison Terms Served	.02663	-.03537	.14267	.08060	.03849	.14297	1.00000
(50) ISP Sample Membership	.05442	.03915	.01791	.05469	.04462	.04713	.02070
(51) ISP x Site 1	-.01059	-.04112	-.03102	-.03191	-.01508	-.08377	.01994
(52) ISP x Site 2	-.01059	-.04112	-.03102	-.03191	-.01508	-.04746	-.00547
(53) ISP x Site 3	-.01016	-.03947	-.02977	-.03062	-.01447	.01331	-.04982
(54) ISP x Site 4	-.01973	-.07662	-.05780	-.05945	-.02810	-.04425	-.11454
(55) ISP x Site 5	.18575	-.08515	-.06424	-.06607	-.03122	-.08354	-.01007
(56) ISP x Site 6	-.01909	.50787	-.05593	-.05752	-.02719	.09031	-.01138
(57) ISP x Site 7	-.01622	-.06298	.43319	-.04887	-.02309	-.03234	.18646
(58) ISP x Site 8	-.01120	-.04349	-.03281	.77068	-.01595	-.03265	.05702
(59) ISP x Site 9	-.01100	-.04272	-.03222	-.03314	.42887	.09974	-.00729

(Continued on next page)

Appendix 1--Continued.

	(50)	(51)	(52)	(53)	(54)	(55)	(56)
(50) ISP Sample Membership	1.00000						
(51) ISP x Site 1	.15119	1.00000					
(52) ISP x Site 2	.15119	-.02572	1.00000				
(53) ISP x Site 3	.14512	-.02468	-.02468	1.00000			
(54) ISP x Site 4	.28172	-.04792	-.04792	-.04599	1.00000		
(55) ISP x Site 5	.31309	-.05325	-.05325	-.05111	-.09923	1.00000	
(56) ISP x Site 6	.27259	-.04637	-.04637	-.04450	-.08639	-.09601	1.00000
(57) ISP x Site 7	.23157	-.03939	-.03939	-.03781	-.07339	-.08156	-.07101
(58) ISP x Site 8	.15992	-.02720	-.02720	-.02611	-.05068	-.05633	-.04904
(59) ISP x Site 9	.15706	-.02671	-.02671	-.02564	-.04978	-.05532	-.04816

	(57)	(58)	(59)
(57) ISP x Site 7	1.00000		
(58) ISP x Site 8	-.04166	1.00000	
(59) ISP x Site 9	-.04092	-.02826	1.00000

Determinant of Correlation Matrix = .0000000

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