SHOCK INCARCERATION AND POSITIVE ADJUSTMENT DURING COMMUNITY SUPERVISION: A MULTI-SITE EVALUATION

Final Report

To

THE NATIONAL INSTITUTE OF JUSTICE

December 1993

By:

Doris Layton MacKenzie

and

Robert Brame

University of Maryland

This investigation was supported in part by grant #90-DD-CX-0061 from the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice to the University of Maryland. Points of view in this document are those of the author(s) and do not necessarily represent the official position of the U.S. Department of Justice. Thanks are expressed to all those who have worked on the multi-site study. Requests for copies should be sent to the senior author at the University of Maryland, Department of Criminal Justice and Criminology, 2220 LeFrak Hall, College Park, MD. 20742.

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Research Team

Researchers from each state involved in the multi-site study of shock incarceration met in the summer of 1990 to plan the evaluation. The research design and instruments utilized are a result of this collaborative effort. State researchers were responsible for data collection, and in some states, data analysis. Multi-site researchers include the following:

Doris Layton MacKenzie, Principal Investigator
Claire Souryal, Research Associate
Robert Brame, Research Associate
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Shock Incarceration and Positive Adjustment
During Community Supervision:
A Multi-Site Evaluation

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Shock Incarceration and Positive Adjustment During Community Supervision: A Multi-Site Evaluation

Abstract

In recent years, shock incarceration programs, or "boot-camp prisons" have been a source of widespread attention. One of the oft-cited advantages of shock incarceration programs is that they provide offenders with a heightened sense of personal responsibility, confidence, and self-discipline as well as an increased capability to make a successful return to the community. In this paper, we examine the extent to which offenders emerging from shock incarceration programs in five states do, in fact, adjust more positively to the day-to-day requirements of living in the community. The results suggest that shock incarceration programs have limited impact on positive adjustment. Supervision intensity in the community, however, plays a key role in the explanation of community adjustment. More intensively supervised offenders tend to outperform offenders who are less intensively supervised.

I. Abstract

Shock Incarceration and Positive Adjustment During Community Supervision: A Multi-Site Evaluation

Administrative Summary

I. *Overview*

Recently, correctional officials and policy makers have been exploring and implementing alternatives to traditional punishment tactics. These efforts have largely been mounted in response to an emerging consensus that customary punishment mechanisms, such as probation and incarceration, are incapable of ensuring public safety and meeting offender needs within existing constraints. One alternative that has received significant attention within the past few years are the shock incarceration programs (often referred to as "boot-camp" prisons). In this study, data were collected on a range of measures of successful, or "positive adjustment" to life in the community in five states (see Tables 1a and 1b). The study's objective was to investigate whether otherwise similar groups of offenders from different correctional "treatments," including shock incarceration, were equally successful at adjusting to life in the community.

II. *Empirical Background*

Previous research on correctional options and their impact on positive adjustment to the community is limited but suggestive of several tentative conclusions:

- Shock incarceration has little independent impact on successful adjustment (MacKenzie et al., 1992).
- Intensive supervision, in conjunction with shock probation is positively associated with successful or "positive adjustment" (Latessa and Vito, 1988).
- Intensive supervision programs (ISP's) appear to be more effective than other tactics when criteria include securing employment and attending drug treatment/counseling (Petersilia and Turner, 1993). Participation in intensive supervision programs is also associated with a higher likelihood of revocation for technical violations of community supervision conditions.
- Neither shock incarceration nor intensive supervision programs appear to be particularly effective at reducing recidivism rates. But offenders emerging from these programs do not recidivate more frequently than their counterparts in other correctional programs (Souryal and MacKenzie, 1993; Petersilia and Turner, 1993).

This analysis focused on whether participation in shock incarceration programs was associated with positive adjustment in five states participating in the National Institute of Justice (NIJ) program to evaluate the effectiveness of U.S. shock incarceration programs.

III. Methodological Issues

The data for this study were collected as part of the National Institute of Justice research program on shock incarceration. The shock incarceration programs studied in the NIJ research program were all administered by state correctional officials. Participating states were selected so that several programming emphases, selection criteria, and regions of the U.S. would be represented. Not all of the participating states collected data on successful community adjustment. Four of the five states included in this analysis were located in the southeastern U.S. (sites included Florida, Georgia, Louisiana, New York, and South Carolina). Still, the regional homogeneity of the sites stood in contrast to differences in program emphases between them. All shock incarceration programs have a military atmosphere emphasizing drill, physical training, strict rules and discipline. But some programs in this study emphasized treatment, education and rehabilitation, while others emphasized punishment and deterrence. Exhibit 1 summarizes the major features of each state's program at the time the data for this study were collected. Importantly, some program features have changed since 1989-1990 (and 1987-1988, in Louisiana) when the data were collected. Table 2 presents the sample structure for each of the five states in the study.

An important focus of the analysis was whether offenders graduating from shock incarceration programs adjusted more positively than offenders from other programs (i.e., traditional probation, incarceration, or shock incarceration program dropouts). Although the study groups within each of the states were selected to be similar (i.e. comparison groups met legal eligibility criteria for entrance into their state shock programs), they were not completely equivalent. To the extent that the study groups differ on characteristics that are related to successful community adjustment, the results of an analysis of group differences in successful adjustment that fails to take the other differences into account will yield biased results.[1]

Demographic and offender characteristics data were, therefore, collected along with indicators measuring community supervision intensity. Our primary interest in these variables was their usefulness in controlling for pre-existing group differences. But supervision intensity is also interesting in its own right because intensive supervision programs (ISP's) are, in and of themselves, a viable correctional option.

Supervision intensity and positive adjustment data were recorded by the community supervision officer at three-month intervals (four

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[1]In short, such an analysis will lead to the incorrect conclusion in the average sample and the degree of inaccuracy will not improve with increasing sample size.

quarters or "waves") up to one-year. Positive adjustment items included the indicators presented in Tables 1a and 1b. Supervision intensity data were available for analysis in all states except New York. In all states except Louisiana, supervision intensity was measured by the number of combined face-to-face and telephone contacts with the offender during each month of community supervision.

For each three-month wave of the study, the number of monthly offender contacts were averaged over all three months (in Florida, Georgia, and South Carolina). In Louisiana, supervision intensity was measured through the use of three indexes: (1) knowledge of offender activities; (2) surveillance of offender activities and whereabouts; and (3) requirements for adequate progress during community supervision. Perhaps not surprisingly, our preliminary analysis revealed that supervision intensity indicators were positively skewed (the requirements index in Louisiana was the only exception). In each of these states, we worked with the supervision intensity indicators in their natural logarithm (log to base e) form rather than in their raw metric form (with the exception of the requirements index in Louisiana). Exhibit 2 summarizes demographic and offender characteristics and important group differences in those characteristics in each of the states. A more detailed comparison of these programs is described in MacKenzie and Souryal (1993).

For analysis purposes, our principal interests were (1) whether offenders from the different groups generally adjusted more or less positively over the course of the entire follow-up period (after adjusting for pre-existing differences); and (2) whether there were important changes and predictors of changes in the levels of successful adjustment (and supervision intensity) over the four quarters or "waves" of the study.

IV. Results

We began the analysis by testing the positive adjustment measures described in Table 1 for internal consistency. The primary concern in an assessment of internal consistency is whether the composite items measure a unified construct. Empirically, the question is how well the current composite correlates with all other composites that measure the same construct. Our analysis (using Cronbach's coefficient alpha) revealed that both the ten-item composite (Florida, Georgia, New York, and South Carolina) and the eighteen-item composite (Louisiana) achieved reasonable levels of internal consistency in each state. We concluded that positive adjustment, as measured in this analysis, represents a reliable construct.

Analysis of pre-existing group differences in demographic and offender characteristics indicated that: (1) some differences were statistically significant in each state; and (2) statistical controls for demographic and offender characteristics would be necessary before comparing the groups (i.e., prison, probation, shock incarceration program dropouts, and shock incarceration program graduates) on positive adjustment.

Analysis revealed that panel mortality, or subject attrition, was sufficiently pronounced to be a concern in this study. The greatest levels of subject mortality (mortality=dropping out of the study due to missing data, revocation, re-arrest, or release) were observed in Florida and Georgia while the problem was less pronounced in the other states. Where attrition sources could be identified, they were not usually positive events. In short, attrition was most likely to be the result of a revocation or a re-arrest -- not a release. Moreover, the analysis revealed that subject mortality and treatment sample (prison, probation, shock incarceration graduates, shock dropouts) were not independent in Florida, Georgia, and Louisiana. Thus, our preliminary analysis suggested the need to control for subject attrition patterns when positive adjustment is compared across groups.

Two types of models were constructed. First, a model that assessed whether positive adjustment scores averaged over the entire one-year follow-up period were different for different sample categories (shock, prison parolee, probationer, etc.) was estimated in each state. Put simply, this model largely ignores the over-time fluctuations in positive adjustment and supervision intensity and simply aggregates them for all available measurement waves for each subject. Second, we focused on whether there were important differences in the groups that would only be apparent in an over-time analysis. The over-time analysis consisted of estimating a series of longitudinal regression equations that utilized measurements in positive adjustment and supervision intensity at each wave within the one-year follow-up period.

Two important features of the longitudinal regression models are noteworthy. First, they control explicitly for prior values of the dependent variable in later waves (2, 3, and 4) of the study. Second, they allow the covariance matrix (the composite of predictor variables) to change over time. While the latter feature has no implications for variables such as type of correctional program, race, or age which remain constant over the course of the follow-up period, it has important implications for appropriately modeling the impact of supervision intensity which does change over time.

As an alternative to the longitudinal regression approach, we estimated repeated measures analysis of variance models that did not control for prior values of the dependent variable. The repeated measures approach allows for explicit tests of the hypothesis that the effect of a given predictor variable (e.g., type of correctional program) is constant over the course of the follow-up period but imposes the requirement that the covariance matrix be constant (i.e., the variables are not allowed to change their values) over the study period. Although the latter requirement was inconsistent with our supervision intensity data, this method provided a useful assessment of over-time variation in the impact of the other predictor variables.

Results of the analyses were, in many respects, consistent across the five states. Over-time and multivariate cross-sectional analysis revealed the following general conclusions:

- In Florida, the shock graduate sample had higher positive adjustment scores than either the prison parolee sample or the shock dropout sample. Over-time analysis revealed that among offenders who completed the full one-year follow-up period, there were no group differences in positive adjustment that were either substantively or statistically significant (Exhibit 3).

- In New York, the shock graduate sample had higher positive adjustment scores than the shock dropouts but not the prison parolees (Exhibit 6).

- In the longitudinal analyses in Louisiana, there was evidence that shock program graduates performed slightly worse than the other groups, although substantively, the differences were quite small. In the cross-sectional analysis, the shock sample performance was not significantly different from the performance of the other samples (Exhibit 5).[2]

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[2]Analysis of the effect of shock incarceration in the absence of control variables (particularly supervision intensity) indicated that the shock sample outperformed all of the other groups. The analysis indicated that the shock sample was also supervised significantly more intensively than the other groups. When

- In Georgia (Exhibit 4), and South Carolina (Exhibit 7), positive adjustment scores did not differ by the type of correctional sanction used.

- The effect of supervision intensity on positive adjustment varied in an interesting fashion. In Florida, Georgia, and South Carolina, where supervision intensity was measured by monthly offender contacts, positive adjustment was generally positively associated with successful adjustment. Beyond about 2.0 contacts per month, however, the relationship between the contact levels and successful adjustment leveled off. Although, there was only the most limited evidence that positive adjustment decreased as supervision intensity increased at these levels, there was consistent evidence that increases in supervision intensity beyond 2.0 contacts per month failed to lead to significant increases in positive adjustment until contacts reached extremely high levels (e.g., 15 to 20 monthly contacts). Taken at face value, these models imply a friction, of sorts, that occurs between two and very high levels of monthly contacts. The friction in the estimated regression curves were evident in all three states where contact data were used. Still, offenders with three and four contacts per month adjusted more positively than offenders with 0.5, 1.0, or 1.5 contacts per month on average.

  In Louisiana, where supervision intensity index scores were used, there was no evidence of friction or diminishing return/leveling-off relationships between supervision intensity and positive adjustment. Instead, the pattern was linear and suggestive of a positive relationship between supervision intensity and successful adjustment.

  In sum, the results pointed to the conclusion that offenders who were in more frequent contact with their supervising officer adjusted more positively than offenders who were in less frequent contact with their supervising officer.

- Over-time assessments of positive adjustment and supervision intensity suggested the following conclusions:

  - Both positive adjustment and supervision intensity tend to decline weakly over time. In Louisiana, positive adjustment and supervision intensity declined more strongly for the shock graduate sample than for the other groups. In the remaining states, the weak patterns of decline in both positive adjustment and supervision intensity were relatively uniform across the samples (In New York, only positive adjustment was observed to

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supervision intensity was held constant the difference between the shock sample and the other groups vanished.

decline weakly since supervision intensity information was not available).

- Within individual offenders, decreases in supervision intensity were associated with decreases in positive adjustment scores. Thus, positive adjustment and supervision intensity are not only linked between subjects. They are linked within subjects as well.

- Demographic and offender characteristics were also important predictors of positive adjustment during community supervision. Analysis of these data revealed the following general conclusions:

    - Nonwhite offenders tended not to adjust as positively as white offenders.

    - Offenders who were older (either at the beginning of community supervision or at their first arrest) adjusted more positively than younger offenders.

    - Property offenders (e.g., burglary, larceny, arson, etc.) did not adjust as well as offenders serving a sentence for a violent or drug-related offense.

    - Offenders with evidence of prior problematic behavior through either: (1) record indicates the presence of a prior arrest or conviction; or (2) record indicates that current sentence was served because of a revocation for a technical violation; are defined as having prior problems. Offenders with evidence of these prior problems did not adjust as well as offenders who had no evidence of prior problems.

Results varied somewhat from state to state but the above patterns were quite consistent across states. Exhibits 3-7, present summaries of the major findings within each of the states studied. Some of the features of these Exhibits require comment. First, the impact of demographic and offender characteristics are described on the far left-hand side of the diagram. Second, average positive adjustment scores <u>controlling for the other predictors</u> are presented for each sample within each state in the upper right-hand corner of the diagram. The displayed positive adjustment scores are cross-sectional (aggregated over the entire one-year period) but the over-time analyses lead to similar conclusions in each state.[3] The relationships between supervision intensity and

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[3]There is one possible exception to this rule. In Louisiana, the shock sample performs slightly worse than the other groups over time. The differences between the groups in the over-time analyses, however, are still quite small.

positive adjustment are also depicted in these Exhibits. Both the cross-sectional and longitudinal regression curves are presented. These curves display two important features: (1) the estimated relationships between supervision intensity and positive adjustment both cross-sectionally and over time are captured; and (2) the estimated (albeit weak) declines in both positive adjustment and supervision intensity are evident.

V. *Conclusions*

Our analysis objective was to assess whether there were significant between-group differences in positive adjustment. The study groups were comprised of shock incarceration program graduates and comparison groups of prison parolees, probationers, and shock program dropouts. Although the content of the programs, their structure, and their offender composition varied by state, the results largely did not. Both cross-sectionally and over time, there was little evidence that the shock program graduates in any state adjusted dramatically better than offenders from other groups. Although there was limited evidence that the shock graduates outperformed the prison parolees and the shock dropouts in Florida and that the shock graduates outperformed the shock dropouts in New York, these differences were not apparent in any of the other states.

The analysis did reveal that demographic and offender characteristics were related to positive adjustment as was the level of community supervision. Nonwhites, younger offenders, property offenders, subjects with prior arrest or conviction records, subjects whose current sentence resulted from a technical violation revocation, and less intensively supervised offenders tended to adjust less positively than offenders who did not share these attributes. Supervision intensity analysis, although not the primary focus of this research, yielded several particularly interesting results which merited exploration:

- Both supervision intensity and positive adjustment declined weakly over time and those declines were evident between subjects and even more definitively within subjects. At both the aggregate and the individual level declines in supervision intensity were generally associated with declines in positive adjustment.

- In Florida, Georgia, and South Carolina, there was evidence of a nonlinear relationship between monthly offender contact levels and positive adjustment. As monthly contact levels approached and exceeded an average of 2.0, gains in positive adjustment were more limited than when contact levels were below the 2.0 level. There was also some evidence that positive adjustment resumed its upward trajectory when monthly contacts reached very high levels. Although such a pattern is suggestive of a "friction" model of supervision intensity and positive adjustment, more research that relies on other instruments, other measures, and new data will have to be undertaken to validate this finding.

- Analysis in Louisiana revealed a generally linear and positive association between supervision intensity and successful adjustment during community supervision. Since the Louisiana analysis is based on the use of composite indicators of supervision intensity rather than raw contact levels, the absence of nonlinearity in this assessment suggests that something about the measurement of raw contact levels and positive adjustment leads to the findings of nonlinearity described above. Unfortunately with the available data we can do little more than speculate about what those factors might be.

We face a similar problem with the aberrant positive effect for the shock incarceration program observed in Florida. Given the available data it is difficult to develop an explanation that is data-driven. Instead, we have to rely on what we know about the Florida program. First, the Florida shock program has a relatively high level of in-sentence attrition. That is, offenders entering the Florida shock program over the 1987 to 1991 period only graduated at a rate of about 49%. Such a high attrition rate raises the possibility of a selection effect. The crucial issue is whether the large dropout rate acts as a filter that leaves those offenders who are most likely to succeed during community supervision in the program while those who are destined for lower levels of positive adjustment drop out. One empirical result that may support this result is the repeated measures analysis of variance test for subjects who completed the entire follow-up period. Sample comparisons for these offenders revealed no significant differences in positive adjustment at any of the four waves of the follow-up period. This result suggests that, among offenders with a propensity to stay out of serious trouble for the entire first year of community supervision, the shock program has no effect on positive adjustment.

Of course, there is also the possibility that offenders in the Florida program are performing better than their counterparts because the shock experience provides them with the special equipment they need to adjust successfully in the community. Other studies in the NIJ program on shock incarceration have revealed that both shock graduates and shock dropouts are less likely to recidivate than prison parolees in Florida. Anti-social attitudes decreased more dramatically for Florida shock graduates than they did for offenders in other groups. There is a commitment to counseling and education in the Florida program that clearly exceeds that of Georgia (which has a heavy emphasis on work, punishment, and deterrence). But the time spent in counseling and education in Florida is less than what an offender in New York or Louisiana would experience and is about the same as what an offender in South Carolina would experience.

Although the shock graduate group in Florida was disproportionately comprised of drug and property offenders this pattern does not seem to be dramatically at odds with what occurs in other states since shock programs often target these types of offenders. In Florida, the shock graduates were supervised much less intensively than their counterparts in the other Florida programs (i.e., prison parolees and shock dropouts) but the data did not indicate that the observed relationship between supervision intensity and positive adjustment was conditional on the type of correctional program.

One possibility that warrants further exploration is the impact of shock incarceration compared to other groups when all groups are comprised of relatively few offenders with prior arrest/conviction records. The samples in Florida all have relatively few offenders with prior records compared to what was observed in the other states. Although controlling for prior record failed to alter the results in our analysis of the Florida data, the homogeneity of the groups with respect to this variable could be responsible for the absence of an effect. This is certainly a research avenue that could provide useful insights into the potential effectiveness of shock incarceration on positive adjustment.

Marginal sample performance differences in New York are even less interpretable than those in Florida because the shock graduates do not perform better than both the shock dropouts and the prison parolees. Instead, they only outperform the shock dropouts. Moreover, we did not have access to supervision intensity indicators in New York. Since the New York program has a mandatory six-month intensive supervision program requirement for shock graduates, it seems reasonable to expect that the state's sample membership patterns are not independent of its supervision intensity indicators. In every state, our analysis revealed that supervision intensity and positive adjustment were significantly related to each other. Given the consistency of this result, across states, there is no reason to expect that these results would be markedly different in New York if they could be measured.

What do these results say about the efficacy of shock incarceration? The evidence indicates that despite a wide range of program emphases, selection criteria, dismissal/dropout mechanisms, and underlying correctional strategies (cf. MacKenzie and Souryal, 1993) there is little basis for concluding that offenders who emerge from any of the shock incarceration programs will adjust dramatically better or dramatically worse than offenders from other correctional programs. Thus, the efficacy of the shock incarceration program lies in the definition of success that is ascribed to it. Expectations for the program must be consistent with what a program that emphasizes short term incarceration and short-term treatment is capable of doing. These data indicate that no matter what type of program is used, the intensity with which an offender is supervised in the community is a critical success factor. That other research has discovered this same result is, in our view, a highly salient consideration.

Drug-dependent offenders are more likely to seek treatment and continue with treatment when they are required to do so as a condition of community supervision (Petersilia and Turner, 1993; Anglin and Hser, 1990). When offenders are contacted more often and experience more requirements, they are more likely to secure employment (Petersilia and Turner, 1993; Latessa and Vito, 1988). Offenders who are intensively supervised often do not find it to be a particularly pleasant experience (Petersilia, 1990). In fact, given the option, many offenders will choose prison over intensive supervision because of the oppressive nature of constrained life in the community compared to a relatively unburdensome often short term of incarceration. Although intensive supervision is not the cure for all correctional ills, it appears to be a viable mechanism for putting some offenders into the motions of conventional activities in the community.

Shock incarceration programs, as a group, do not appear to provide offenders with unique skills or abilities for adjusting more positively to life in the community, nor do they decrease the probability of a successful adjustment. But, to the extent that shock programs, either with or without intensive supervision, (1) facilitate a consistent and predictable system-level response to crime while (2) deemphasizing incarceration for offenders whose long-term incapacitation makes less sense, their use will continue to warrant serious consideration.

Table 1a
Items and Overall Means For Positive Adjustment Construct in Florida, Georgia, New York, and South Carolina

Positive Adjustment Items

<u>Procedure:</u> Increment the index by 1 for each applicable item, sum the items and divide by the total number of completed items (if at least eight items were evaluated).

During this period was the offender:

1. Employed, enrolled in school, or participating in a training program for more than 50% of the follow-up period.
2. Held any one job (or continued in educational or vocational program) for more than a three month-period during the follow-up.
3. Attained vertical (upward) mobility in employment, educational, or vocational program.
4. For the last half of follow-up period, individual was self-supporting and supported any immediate family.
5. Individual shows stability in residency. Either lived in the same residence for three months or moved at suggestion or with the agreement of supervising officer.
6. Individual has avoided any critical incidents that show instability, immaturity, or inability to solve problems acceptably.
7. Attainment of financial stability. This is indicated by the individual living within his means, opening bank accounts, or meeting debt payments.
8. Participation in self-improvement programs. These could be vocational, educational, group counseling, alcohol, or drug maintenance programs.
9. Individual making satisfactory progress during community supervision period. This could be moving downward in levels of supervision or obtaining final release within period.
10. No illegal activities on any available records during the follow-up period.

Descriptive Statistics

State	N=	Median	Mean	S.D.
Florida	280	0.35	0.38	0.27
Georgia	246	0.41	0.42	0.24
New York	237	0.55	0.51	0.30
South Carolina	326	0.50	0.46	0.29

Table 1b
Items and Descriptive Statistics For Overall Positive Adjustment Construct in Louisiana (N=278)

Positive Adjustment Items

Procedure: Increment the index by 1 for each applicable item, sum the items and divide by the total number of completed items (if at least fourteen items were evaluated).

1. Is subject working full-time or part-time?
2. Is employer's evaluation of subject favorable?
3. Subject required to attend Alcoholics' Anonymous and is is making satisfactory progress.
4. Subject required to attend drug treatment program and is making satisfactory progress.
5. No positive alco-sensor tests.
6. No positive drug screens.
7. Subject actively pursuing training or education and making satisfactory progress.
8. No difficulties in family relationships.
9. Subject is avoiding relationships with delinquent peer groups.
10. Attitude or appearance is satisfactory.
11. Subject is compliant and cooperative.
12. Subject has met curfews, provided information on whereabouts, has not missed appointments, has not lied to officer.
13. Subject accepts responsibility for actions.
14. Community supervisor evaluation is satisfactory.
15. Subject displays evidence of emotional stability.
16. Subject is making a successful adjustment.
17. Subject is doing better than officer evaluation might otherwise indicate.
18. Subject has not been arrested during this follow-up period.

Construct Descriptive Statistics

Median = 0.433
Mean = 0.438
S.D. = 0.146

Table 2
State Sample Frequency Distributions

State & Sample	Number of Cases	Percent of Total
Florida		
Shock Graduates	112	38.8%
Shock Dropouts	68	23.5%
Prison Parolees	109	37.7%
Total	289	100.0%
Georgia		
Shock Graduates	79	30.2%
Prison Parolees	98	37.4%
Probationers	85	32.4%
Total	262	100.0%
Louisiana		
Shock Graduates	77	27.7%
Prison Parolees	74	26.6%
Probationers	111	39.9%
Shock Dropouts	16	5.8%
Total	278	100.0%
New York		
Shock Graduates	94	32.9%
Shock Dropouts	97	33.9%
Prison Parolees	95	33.2%
Total	286	100.0%
South Carolina		
DPPPS Shock Graduates	85	26.1%
DOC Shock Graduates	84	25.8%
Prison Parolees	64	19.6%
Probationers	69	21.2%
Split-Probationers	24	7.4%
Total	326	100.0%

Exhibit 1
Summary of State Shock Incarceration Program Characteristics

Florida

One Shock Program
100 Inmates Per Class
Average Sentence: 3.3 Months (90 to 120 Days)
Regular Community Supervision Upon Release
Voluntary Dropouts Not Allowed
Dropout/Dismissal Rate: 51.1% (Oct. 1987-Jan. 1991)
Structured Activities: 9.8 Hrs/Day
Rehabilitative Activities: 1.84 Hrs./Day
Proportion of Time Spent in Rehabilitative Activities: 0.184

Louisiana

120 Inmates Per Class
Average Sentence: 120 Days (90 to 180 Days)
Minimum Six Months Intensive Supervision Upon Release
Voluntary Dropouts Allowed
Dropout/Dismissal Rate: 43.3% (Feb. 1987-Feb. 1989)
Structured Activities: 10.0 Hrs/Day
Rehabilitative Activities: 3.5 Hrs./Day
Proportion of Time Spent in Rehabilitative Activities: 0.35

Georgia

Two Shock Programs
Combined Program Capacity: 200 Inmates
Program Length: 90 Days
Community Supervision Status Is Reviewed at Program Completion
Voluntary Dropouts Not Allowed
Dropout/Dismissal Rate: 2.8% (1984-1989)
Structured Activities: 8.3 Hrs/Day
Rehabilitative Activities: 0.3 Hrs./Day
Proportion of Time Spent in Rehabilitative Activities: 0.036

South Carolina

Approximately 200 Male Inmates/Class
Program Length: 90 Days
Supervision Status Reviewed at Release
Dropout/Dismissal Rate: 16.0% (July 1989-June 1990)
Structured Activities: Approximately 11 Hours/Day
Rehabilitative Activities: Approximately 3 Hours/Day
Proportion of Time Spent in Rehabilitative Activities: 0.273

New York

Program Capacity: 1,500 Inmates
Program Length: Six Months
Six Months of Intensive Community Supervision Required
Voluntary Dropouts Allowed
Dropout/Dismissal Rate: 31.3% (Jan. 1988-Dec. 1988)
Structured Activities: 14.6 Hrs./Day
Rehabilitative Activities: 5.6 Hrs./Day
Proportion of Time Spent in Rehabilitative Activities: 0.384

Note: Multi-Site Community Supervision Data were collected between 1989 and 1991 in Florida, Georgia, and South Carolina. Louisiana data were collected between October 1987 and October 1988 and New York data were collected between Spring 1988 and Fall 1990. The descriptions of the programs in this exhibit are consistent with information available at the time of data collection although the programs may operate differently at the current time. For example, the Georgia program was undergoing significant change during 1991 and 1992: (1) the capacity of the program was scheduled to be expanded to "eight 90-day boot camp programs with a capacity of 1,844 beds [to] be used by the Georgia Board of Pardons and Paroles for potential parolees. Six more 90-day boot camps will be available to the state's trial judges for direct sentencing of probationers" (Bowen, 1991, p. 100). Moreover, Georgia officials were planning to increase the treatment and educational components of their programs while reducing the relatively heavy work requirements.

Exhibit 2
Study Variable Descriptive Statistics and Summary of Sample Differences In Each State

Note: Entries For Categorical Variables (e.g. Race, Offense Type) Represent The Percent of Offenders Possessing The Attribute. Entries For Continuous Variables Represent the Variable's Mean (and Median, for Contacts Only).

Florida

Samples Differ Significantly On:	Contrasts
Age @ CS	PP > SG & SD
Offense = Violent	PP > SG & SD
Offense = Drug	SG > SD & PP
Offense = Property	SG & SD > PP
Sentenced For New Crime	SD & PP > SG
Monthly Contacts	PP & SD > SG

Race = Nonwhite	56.8%
Age @ CS	19.4
Offense = Violent	32.2%
Offense = Drug-Rel.	14.2%
Offense = Property	53.6%
Sentenced For New Crime (vs. Technical Violation)	83.7%
Prior Arrest/Conviction Record	27.7%
Mean Monthly Contacts	6.62
Median Monthly Contacts	2.25

Louisiana

Samples Differ Significantly On:	Contrasts
Age @ CS	PP > SG & SD
Offense = Violent	PP > All Groups
Offense = Drug	PB > SG > PP & SD
Offense = Property	SD > SG & PP > PB
Sentenced For New Crime	PB > All Groups
Supv. Intensity Indexes	SG > All Groups

Race = Nonwhite	63.3%
Age @ CS	25.1
Age @ First Arrest	20.8
Offense = Violent	9.2%
Offense = Drug-Rel.	29.9%
Offense = Property	51.0%
Sentenced For New Crime (vs. Technical Violation)	74.6%
Prior Arrest/Conviction Record	73.7%
Knowledge Score	1.24
Surveillance Score	0.71
Requirements Score	2.72

Georgia

Samples Differ Significantly On:	Contrasts
Age @ CS	PP > SG & PB
Offense = Violent	SG & PP > PB
Offense = Drug	PP & PB > SG
Offense = Property	SG & PB > PP
Sentenced For New Crime	PB > SG > PP
Prior Arrests/Convictions	PP > SG > PB
Monthly Contacts	PP > SG > PB

Race = Nonwhite	61.5%
Age @ CS	21.8
Offense = Violent	15.3%
Offense = Drug-Rel.	27.1%
Offense = Property	57.6%
Sentenced For New Crime (vs. Technical Violation)	80.1%
Prior Arrest/Conviction Record	56.5%
Mean Monthly Contacts	2.55
Median Monthly Contacts	1.82

South Carolina

Samples Differ Significantly On:	Contrasts
% Nonwhite	S2 > PP & PB & SP > S1
Age @ CS	SP > All Groups
Age @ First Arrest	PB & SP > All Groups
Sentenced For New Crime	PB & S2 > All Groups
Prior Arrests/Convictions	S2 > PP & PB & SB > S1
Monthly Contacts	All Groups > PB

Race = Nonwhite	60.7%
Age @ CS	21.1
Age @ First Arrest	18.9
Offense = Violent	12.9%
Offense = Drug-Rel.	25.2%
Offense = Property	61.9%
Sentenced For New Crime (vs. Technical Violation)	90.2%
Prior Arrest/Conviction Record	67.2%
Mean Monthly Contacts	1.73
Median Monthly Contacts	1.50

New York

Samples Differ Significantly On:	Contrasts
Age @ First Arrest	SG > SD & PP
Offense = Drug	SG > SD & PP
Offense = Property	PP & SD > SG
Offense = Other/Violent	PP & SD > SG
Prior Arrests/Convictions	SG > SD & PP

Race = Nonwhite	82.9%
Age @ CS	22.0
Age @ First Arrest	18.0
Offense = Drug	53.5%
Offense = Property	22.7%
Offense = Other/Violent	23.8%
Prior Arrest/Conviction Record	90.6%

Note: For Ease of Presentation Contrasts Do Not Correspond Exactly to Duncan Post-Hoc Tests. They Do Provide an Approximate Representation.

Contrast Abbreviations

SG = Shock Graduates
SD = Shock Dropouts
PP = Prison Parolees
PB = Probationers
SP = Split-Probationers

Exhibit 3
Summary of Results in Florida

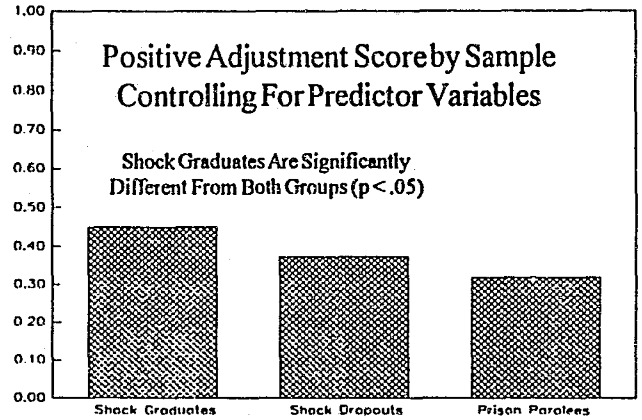
Analysis of Demographic & Offender Characteristics

Characteristics Associated With Higher Positive Adjustments Scores

Race=White
Older at Beginning of Community of Supervision
Current Offense Classified as Violent
Current Sentence Served For A New Crime

Characteristics Associated With Lower Positive Adjustments Scores

Race=Nonwhite
Younger at Beginning of Community of Supervision
Current Offense Classified as Property
Current Sentence Served For A Technical Violation



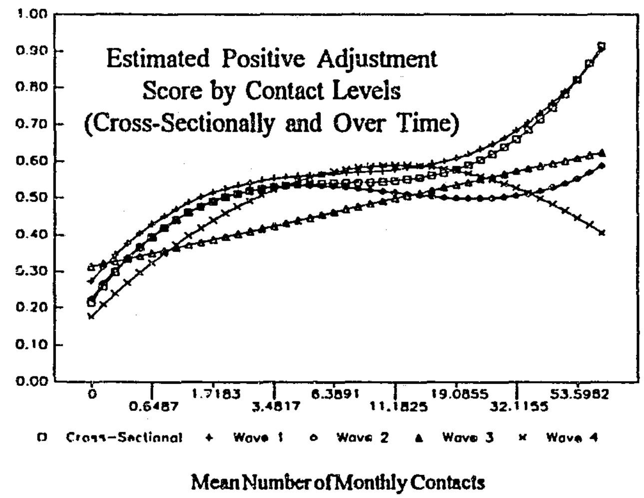


Exhibit 4
Summary of Results in Georgia

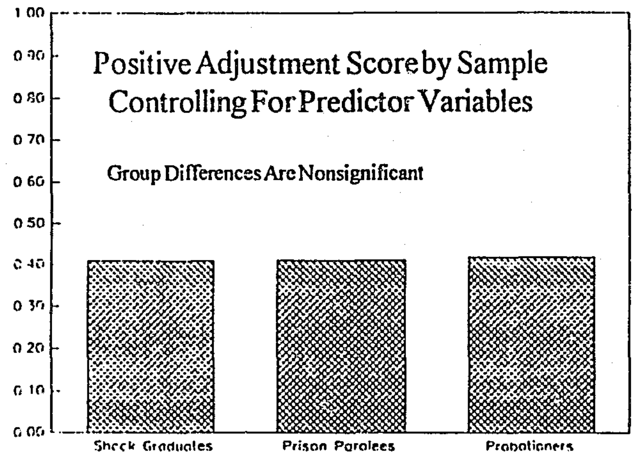
Analysis of Demographic & Offender Characteristics

Characteristics Associated With Higher Positive Adjustments Scores

Race=White
Older at Beginning of Community of Supervision
Current Offense Classified as Violent or Drug-Related
No Criminal History

Characteristics Associated With Lower Positive Adjustments Scores

Race=Nonwhite
Younger at Beginning of Community of Supervision
Current Offense Classified as Property
Criminal History Indicates Prior Arrest/Conviction Record



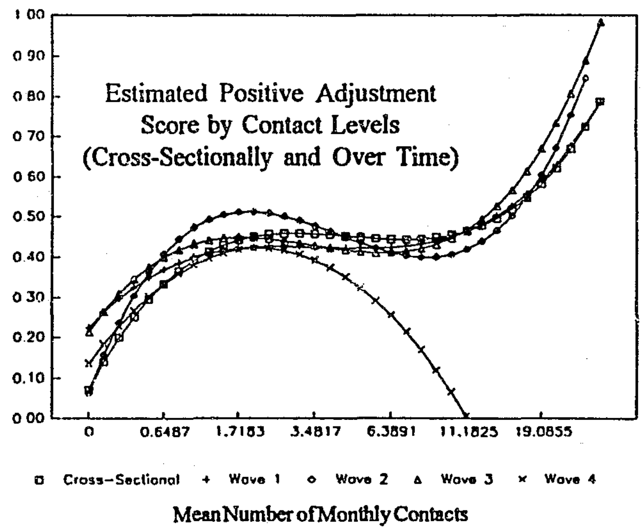


Exhibit 5
Summary of Results in Louisiana

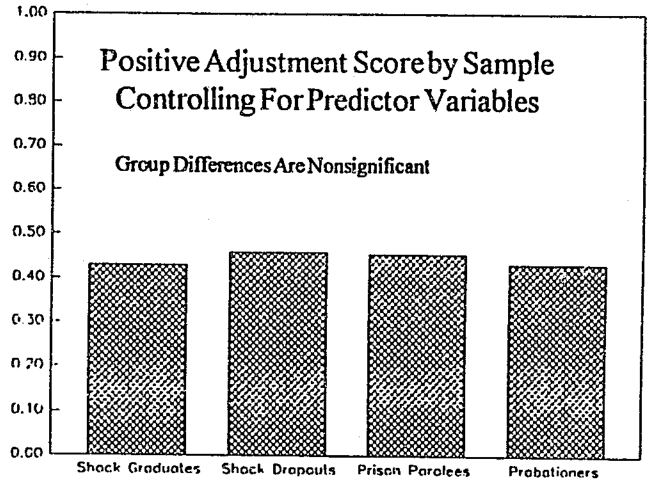
Analysis of Demographic & Offender Characteristics

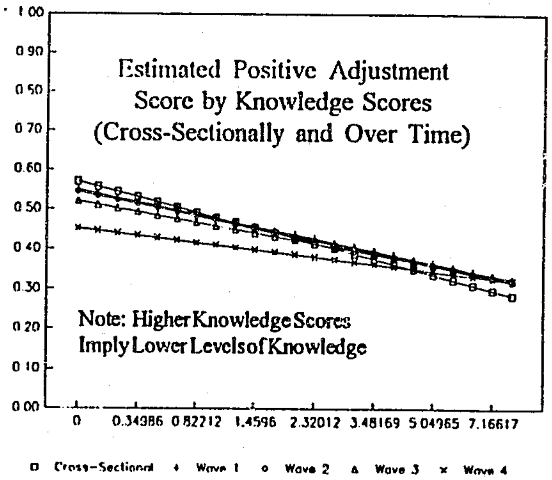
Characteristics Associated With Higher Positive Adjustments Scores

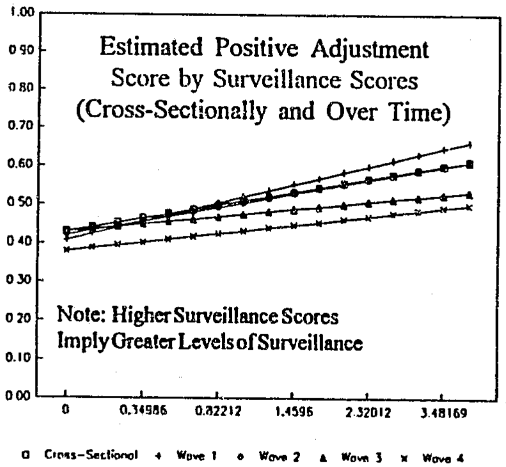
Race=White
Older at Beginning of Community of Supervision

Characteristics Associated With Lower Positive Adjustments Scores

Race=Nonwhite
Younger at Beginning of Community of Supervision







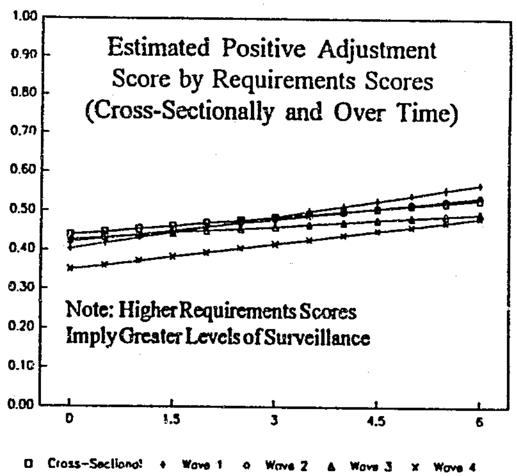


Exhibit 6
Summary of Results in New York

Analysis of Demographic & Offender Characteristics

Characteristics Associated With Higher Positive Adjustments Scores

- Race=White
- Older at First Arrest
- Current Offense Classified as Violent or Drug-Related
- No Criminal History

Characteristics Associated With Lower Positive Adjustments Scores

- Race=Nonwhite
- Younger at First Arrest
- Current Offense Classified as Property
- Criminal History Indicates Prior Arrest/Conviction Record

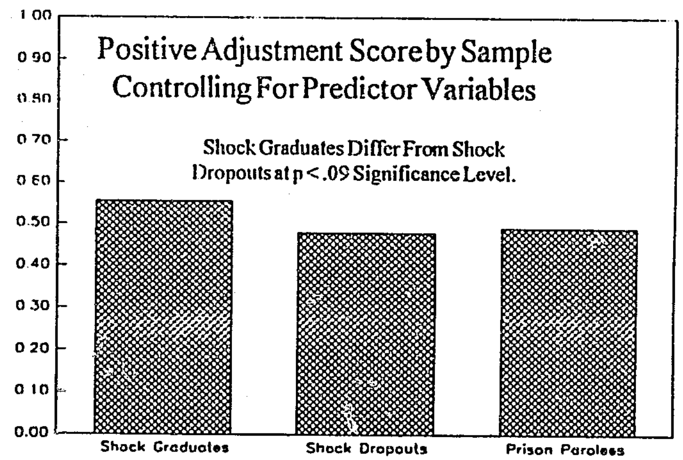


Exhibit 7
Summary of Results in South Carolina

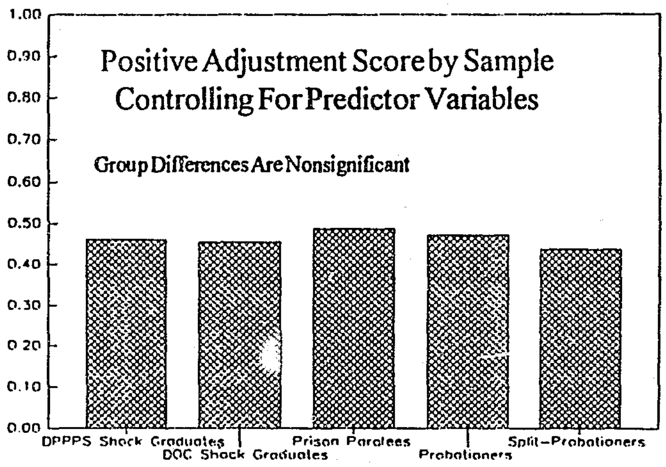
Analysis of Demographic & Offender Characteristics

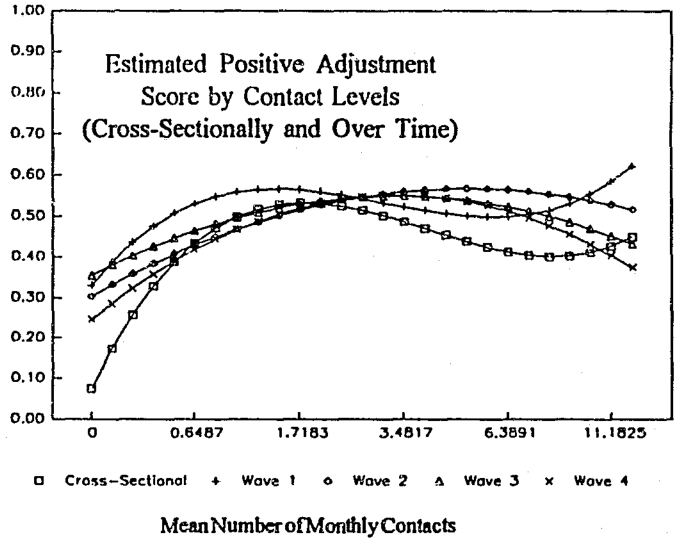
Characteristics Associated With Higher Positive Adjustments Scores

- Race=White
- Older at Beginning of Community of Supervision
- Current Offense Classified as Violent or Drug-Related
- Current Sentence Served For A New Crime
- No Criminal History

Characteristics Associated With Lower Positive Adjustments Scores

- Race=Nonwhite
- Younger at Beginning of Community of Supervision
- Current Offense Classified as Property
- Current Sentence Served For A Technical Violation
- Criminal History Indicates Prior Arrest/Conviction Record





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Shock Incarceration and Positive Adjustment During Community Supervision: A Multi-Site Evaluation

1. Overview

In response to persistent widespread prison overcrowding and public demand for more rational and consistent corrections policy, legislators, correctional officials, and applied researchers have been exploring a range of alternatives to more traditional correctional measures. The objectives of these alternatives are potentially far-reaching. Common themes include reducing the pressure on strained correctional facilities, inducing changes in offender behavior through the implementation of rehabilitation and deterrence measures, and integrating effective treatment for psychological and substance-abuse problems with punishment. An overarching concern is the public perception that offenders are often not punished sufficiently for their crimes and that public safety is compromised by an unresponsive correctional bureaucracy (Gowdy, 1993; Petersilia, 1990).

In the view of some policy analysts, a key ingredient of a rational corrections policy, is the presence of a "continuum of punishments" that bears some resemblance to a continuum of offense seriousness (Petersilia et al., 1985; Morris and Tonry, 1990). An integral component of such a continuum is a group of intermediate sanction programs that provide for a punitive response that falls somewhere between one of the two traditional extremes: probation and incarceration.

The term, "intermediate sanctions," is an umbrella phrase that encompasses measures such as intensive supervision, shock incarceration (boot camp prisons), and shock probation among others (Morris and Tonry, 1990; Gowdy, 1993). The underlying strategy for these programs is the development of a punishment "continuum." Within the framework of the punishment continuum, sanctions can be more closely tailored to fit the crimes to which they apply (Petersilia and Turner, 1993). Such an approach avoids many of the logical difficulties of an "all or nothing" system that emphasizes sanctioning extremes.

In addition to their attention to filling the punishment gap, intermediate sanctioning advocates have also focused on the merits of integrating these correctional options with treatment, counseling, and job training programs (MacKenzie and Shaw, 1993; Shaw and MacKenzie, 1992; Gowdy, 1993). Although much emphasis has been placed upon the ability of intermediate sanction programs to meet both individual and system level objectives (Souryal and MacKenzie, 1993), corrections policy and practice is ultimately a human enterprise. The success of offenders as they begin their transition to living in the community is a source of great interest and concern for policy makers and the public alike.

The use of terms such as adjustment, development, reform, rehabilitation, and deterrence, to describe offender experiences in the correctional system all convey the impression that criminal punishment, by design and implementation, is somehow equipped to change or modify behavior. In particular, a commonly held view is that the correctional system should somehow spur behavioral changes in offenders that will manifest themselves in the successful post-release adjustment of offenders. Successful adjustment is a complicated construct to be sure but it is inherently tied to some important benchmarks. These include the improvement of offender attitudes, an increase in offenders' motivation to change, and a modification of offender's activity structures (Cullen and Gendreau, 1990; Latessa and Vito, 1988; MacKenzie et al., 1992).

Researchers have begun to assess the extent to which existing intermediate sanction programs are achieving these objectives. Several themes appear to be emerging from these efforts. First, intermediate sanctions as a group and as they are currently operating do not hold much promise for creating dramatic reductions (or maybe even modest reductions) in recidivism.

Second, even though cost containment and reduction are prominent objectives associated with the development of intermediate sanction programs, there is some evidence that these programs will not necessarily create dramatic savings for correctional agencies and government budgets (Petersilia and Turner 1993; Gowdy, 1993). Shock incarceration programs still emphasize confinement albeit for shorter periods of time than would otherwise be the case. Integrating confinement with treatment leads to high costs. Either confinement or treatment by itself increases costs as well (MacKenzie, 1990). Intensive supervision is, by definition, a labor-intensive correctional option that has proven to be expensive in practice (Petersilia and Turner, 1993).[1]

Finally, many decisionmakers for intermediate sanction programs have experienced difficulty in arriving at a consensus about which offenders are most appropriately assigned to them. In a practice that has come to be known as "net-widening," officials have occasionally filled intermediate sanction programs at least partially with offenders who otherwise would have been probation-bound creating further cost increases (Petersilia, 1990; Petersilia and Turner, 1989). Apprehension often surrounds the decision of who not to incarcerate because the public generally views

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[1]Although, as Petersilia (1990) notes, the potential for significant cost reductions for these programs over the long term should not be minimized.

incarceration as a desirable means for punishing (and rehabilitating) offenders (Petersilia, 1990; Erickson and Gibbs, 1979; McClelland and Alpert, 1985).

But results also suggest that offenders who serve sentences that fall within the sweep of intermediate sanctions do not perform significantly worse than other offenders after release. Thus, the use of certain intermediate sanctions may be appropriate for certain types of offenders. To the extent that intermediate sanctions facilitate a consistent official response to criminal acts, the development of such programs could be a useful enterprise. Indeed, an important proposition in several prominent criminological theories[2] leads to the prediction that *ceteris paribus*, more certain punishments will lead to reductions in crime. It follows from this that "[i]ntermittent and/or inconsistent punishment, which is precisely the kind of punishment our criminal justice system provides at times, may actually increase the persistence of punished behavior" (Gendreau and Ross, 1981, p. 47). As Petersilia and Turner (1993) noted in their recent multi-site evaluation of intensive supervision, the development of intermediate sanctions for the sake of having a responsive set of intermediate punishments is of great importance to many correctional officials and policy makers.

Given the intuitive appeal of more certain punishments and public demand for rational corrections policy (perhaps even at the perceived expense of decarceration) it is not particularly surprising that intermediate sanctioning programs have become more popular in recent years. Evaluations of those programs as they develop have been and will continue to be forthcoming. Our paper continues this assessment effort. As part of a multi-state study of shock incarceration, data relating to the success of offenders during community supervision were collected along with offender demographic, current offense, and criminal history characteristics and intensity of community supervision indicators (MacKenzie and Souryal, 1992, 1993; Souryal and MacKenzie, 1993). Successful adjustment of offenders, as measured in this study, included but was not limited to items such as rearrest and recidivism. Along with these more traditional measures of success, offenders were evaluated on such items as their ability to gain employment, accept personal responsibility for their behavior and their obligations, seek and continue with appropriate treatments and self-improvement programs, and achieve emotional maturity and stability. Positive

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[2]These theories include but are not limited to the social control, rational choice and deterrence, and social learning perspectives (Gottfredson and Hirschi, 1991; Cohen and Felson, 1979; Hough, 1987; Paternoster, 1989; Burgess and Akers, 1966; Akers, 1985). The certainty of punishment appears to be an important component of offender decisionmaking calculus and the probability that subsequent negative behavior will be extinguished.

adjustment scores for groups sentenced to traditional correctional programs (prison and/or probation) were then compared to those of shock incarcerated offenders in five state correctional systems.

In Section 2, we review the extant literature on the effectiveness of two of the more prevalent forms of intermediate sanctions: intensive community supervision and shock incarceration. In particular, we assess the impact of these programs on community supervision performance and post-release adjustment.[3] Section 3 describes the research design for the current study and Section 4 presents a set of preliminary analyses. The evaluation results are presented in Section 5. We discuss the results and implications in Section 6.

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[3]Gowdy (1993) recently prepared an exhaustive review of National Institute of Justice (NIJ) sponsored research in the area of intermediate sanctions. In our discussion of the literature, emphasis is placed on the sanctions which bear directly on the research questions considered in this paper.

2. Intermediate Sanctions and Positive Adjustment

2.1 Community Adjustment and Shock Incarceration

2.1.1 Overview

Shock incarceration, (typified by the military-style boot camp prison), has received a substantial amount of attention from both the research and policy making communities (MacKenzie and Souryal, 1992, 1993; Gowdy, 1993; Souryal and MacKenzie, 1993). These programs apply an integrated schedule of military-type ceremony and drill, treatment and counseling, physical work, and exercise; usually to younger offenders who have been sentenced for less serious crimes. There is considerable program to program variation in the time and resources devoted to each of these areas (cf. MacKenzie and Souryal, 1992, Chapter II). Terms in shock incarceration facilities are usually shorter than they would be in a traditional prison but, in exchange for shorter incarceration periods, the time spent in shock facilities is considerably more occupied by structured activities.[4] The rigor and intensity of a shock program is also greater than that of a traditional prison sentence.[5]

Analysis of the efficacy of the shock incarceration experience has proceeded on three fronts. First, researchers have been concerned with the emotional and psychological benefits and consequences of participating in these programs (MacKenzie, 1990). Morash and

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[4]In some states (e.g., Georgia) enrollment represents an offender's commitment to finish the program. Withdrawal from the program is not usually allowed in these cases. In other states (e.g., Louisiana) enrollment in the shock program continued at the mutual discretion of offenders and correctional officials. That the attrition rate in shock programs where continued participation is discretionary can approach and exceed 50% in some states (MacKenzie and Souryal, 1992, p. 84), is testimony to the more difficult circumstances offenders encounter in shock facilities. Some research has used this result to support the contention that shock programs have "therapeutic integrity" (Shaw and MacKenzie, 1992).

[5]A recent multi-site process evaluation of shock incarceration programs suggests that where correctional officials have authority over the decision to retain or dismiss inmates, attrition rates are much higher than where those decisions are made by the sentencing judge (MacKenzie and Souryal, 1993). The authors identified Florida, New York, Louisiana, and the newer corrections department-controlled program in South Carolina as states where correctional officials have authority over sentencing decisions while the judge maintained control in Georgia, Texas, and in the old probation department-controlled program in South Carolina.

Rucker's (1990) concerns are typical. The military style nature of many (although not all) of the shock programs results in an emphasis on confrontation, personal responsibility, and toughness. These program qualities may have unanticipated and unintended yet obvious consequences: increased alienation and antisocial behavior. But supporters counter that shock programs can exert a deterrent effect and can represent a potentially useful means to rehabilitate offenders.[6]

Second, researchers have examined whether shock programs decrease negative behavior (i.e. recidivism and technical violations of supervision conditions). The key question for researchers in this area is not so much whether changes have occurred in attitudes and beliefs. Instead, researchers in this area seek to determine whether significant behavior modification has occurred that can be attributed to the effect of shock incarceration.

Finally, impact studies of the shock incarceration experience using broader measures of positive adjustment to life in the community, such as the one in this paper, have been undertaken. Utilization of treatment resources, acquisition of employment, a transition to personal and emotional stability, and the demonstration of consistently constructive behavior all represent outcomes, which shock incarceration programs, with their emphasis on personal responsibility, character-building, and hard work,[7] should be well-equipped to induce (MacKenzie, 1990).

2.1.2 Research

Recent research on shock programs has examined the effect of shock incarceration on attitudes in several states. (MacKenzie and Shaw, 1990; MacKenzie and Souryal, 1992). In these studies, attitudes were measured among shock inmates at the beginning of the program and were measured again at the program's conclusion. Within-subject changes in shock inmates were compared to within-subject attitude changes for prison parolees, probationers, and shock program dropouts who were also measured at the beginning and end of their programs. The analysis revealed that the shock experience, despite considerable program-to-program variation, had the effect of reducing antisocial attitudes. To the extent that antisocial attitudes explain variation in future adjustment and negative behavior, this could be an important finding. It speaks directly to the question of whether the shock experience heightens short-term feelings of alienation and antisocial attitudes. An important

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[6]This paradoxical reliance on two traditionally competing theoretical paradigms has not escaped notice on the part of many criminologists (Morash and Rucker, 1990; MacKenzie and Souryal, 1992).

[7]And, in some programs, drug treatment and counseling.

area for future research, however, is whether better attitudes and reduced alienation persist over time. Additionally, research will need to examine whether there are undesirable consequences associated with shock incarceration that manifest themselves after a more extended period of time. The available evidence suggests that shock incarceration programs appear to meet the threshold requirement that they do no harm.

Other studies, examining post-release offender adjustment, have arrived at less promising conclusions. MacKenzie (1991) and Souryal and MacKenzie (1993), employing survival-time regression analysis in four states, discovered that shock program graduates in Florida and Louisiana took longer, as a group, to have their community supervision status revoked for new crimes than did prison parolees. In Florida, however, shock graduates did not outperform shock program dropouts and, in Louisiana, shock graduates and probationers performed similarly.

Souryal and MacKenzie (1993) also focused on Georgia and South Carolina. Generally, the results in Georgia supported the finding of little or no effect of the shock program. Although initial differences in the performances of the shock incarceration, prison parolee, and probation groups might suggest otherwise[8], the use of statistical controls for demographic and offense-related variables reduced the between-group differences to non-significant levels.

In South Carolina, the data suggested that the effect of shock incarceration varied by the administering agency.[9] When the program was administered by the Department of Probation, Parole, and Pardon Services (DPPPS), shock graduates did not perform differently from comparison groups on failure criteria. When the

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[8]Shock incarcerated offenders failed at higher rates than other offenders in simple groupwise comparisons.

[9]Importantly, this has less to do with the effect of administering agency than it does the type of offenders that entered the program under the supervision of the different agencies. The criteria for entering the program were changed to enhance the program's ability to reduce prison overcrowding. In short, offenders under the old selection guidelines were sentenced directly to a term of shock incarceration followed by a period of community supervision. It, in fact, was originally referred to as a "shock probation" program. Offenders under the new guidelines could enter the shock program in one of two ways. They could continue to enter through a direct sentence or they could be diverted into the shock incarceration program by DOC officials. Presumably, the latter group would be comprised of more serious offenders since at least some of them were diverted from a prison-bound pool while the original program would have included probationers only (Souryal and MacKenzie, 1993).

program was administered by the Department of Corrections (DOC), however, the shock graduates performed significantly better than the probation and dropout groups. But the DOC shock sample's performance was not significantly different from that of the prison sample. Given that the DOC shock sample was comprised at least partially of prison-bound offenders, this finding is not particularly surprising. It does suggest that the shock program does not have the effect of improving performance on community supervision when the criteria include time to new arrest or new crime and technical revocations.

Other research, emphasizing the Louisiana shock program, (Shaw and MacKenzie, 1992; MacKenzie and Shaw, 1993) revealed that shock offenders have higher revocation rates for technical violations. Shock program graduates in Louisiana are required to undergo a minimum of six months of intensive community supervision upon completing the shock program. The policy implications of the supposed relationship between intensive supervision and technical revocations are significant. As Souryal and MacKenzie (1993) note, this relationship could be the beginning of a vicious cycle. If offenders go through a shock incarceration and/or an intensive supervision program that emphasizes heightened surveillance, there may be greater opportunities for a supervising officer to identify technical violations.

Thus, a key question is whether there is truly a higher violation rate among these offenders or whether greater violation rates merely reflect greater surveillance (MacKenzie, 1991). If the latter were true, the offender who was originally targeted for diversion is actually at substantial risk for ultimately returning to prison after utilizing other system resources as well. Paradoxically, a system that has among its stated objectives the goal of reducing costs and demand of prison resources could be operating in such a way that those costs and demands are increased.

Analysis of the Louisiana data also revealed that two cohorts of shock offenders in Louisiana had lower rates of revocations for new crimes than prison parolees, probationers, and shock program dropouts (MacKenzie and Shaw, 1993). Although the shock samples in Louisiana were more intensively supervised, attempts were made to control for this difference by comparing shock offenders with other offenders at the points in the community supervision process when all groups were expected to be supervised at regular levels. When such controls were employed, differences in new crime rates persisted. Nevertheless, the researchers were unable to rule out "residual" effects of supervision intensity at earlier points in the process or to control explicitly for supervision intensity at an individual level.[10] Since much research, however, finds no effect for shock incarceration or for supervision intensity alone, the Louisiana study suggests that a combination of the two treatments is a possible key. At a tactical level, this would imply the presence of an interaction effect. Basically, the hypothesis to be tested is whether the effect of supervision intensity is the same across treatments. The Louisiana research indicates that there may be important differences.

Another area of interest, and the primary focus of this paper, is overall adjustment to the requirements of living in the community. This construct broadly subsumes future criminal behavior and technical violations of supervision conditions. But it also includes components such as securing a job or attending school, accumulating legitimate income, and maintaining a stable living situation (cf. Latessa and Vito, 1988; MacKenzie et al., 1992).

In a recent evaluation of the Louisiana shock incarceration program, examination of the distributions of a positive adjustment index among a shock group, a prison parolee group, and a probation group over a twelve-month follow-up period revealed that shock inmates performed much more positively on community supervision in the short term (MacKenzie et al., 1992). By the end of the twelve-month period, the performance of shock inmates was still stronger than the performances of other groups.

But, two caveats are required. First, the over-time decline in performance was most pronounced for the shock group. Second, when supervision intensity was controlled, the association between shock incarceration and positive adjustment diminished considerably.
In Louisiana, shock incarcerated inmates are required to undergo intensive community supervision after release from the shock program and this gives them the appearance as a group of doing

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[10]Other studies which have not discovered a difference in new crime rates between intensively supervised offenders and other offenders may point us in the same direction as the findings from this study. Perhaps the shock experience coupled with intense supervision is able to achieve real reductions in new crime rates. As Petersilia and Turner (1990, 1993) note, however, these reductions may, in many cases, only be observable through offender behavior (versus official records of offender behavior). The fact that offenders are or were more intensively supervised may lead to greater scrutiny of their activities to be sure. But it is also possible that a residual effect of this intense scrutiny is that offenders' day to day habits, practices, and routines are well documented. Such data may enhance the "detectability" of offenders who are more intensively supervised. If this were true, it would be reasonable to expect a certain amount of new crime revocation rate inflation in the more intensively supervised group that would bias comparisons across levels of supervision intensity.

better.[11] The greater prevalence of intensively supervised offenders in the shock offender group accounts for that group's stronger performance on the positive adjustment index.

2.1.3 Summary

Several important findings have emerged in studies of shock incarceration that focus on attitudes and post-release criminal behavior. While attitudes become more positive and offenders become less antisocial in shock incarceration programs, these programs do not necessarily modify behavior as effectively. Thus, it does not follow that an improvement in attitudes and dispositions will inevitably result in better long-term behavior. Indeed, the most prominent finding emerging from these data is the lack of behavioral differences across offender groups (shock, probation, parole, and shock dropouts) and the general stability of this finding across sites.[12] To date, the evidence on shock incarceration programs, as with other intermediate sanctions, is that there is no large-scale improvement in the behavior of offenders that can be attributed to participation in these programs.

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[11]Whether this finding is net of the shock program or integrally connected to it, however, is unclear since other groups were not supervised at similar levels.

[12]The stable exceptions, are the findings of a reduction in new crime rates as a function of the shock experience in Louisiana and a difference between shock graduates and prison parolees in Florida (although shock graduates and dropouts performed about the same).

2.2 Intensive Community Supervision

2.2.1 Overview

One of the more commonly advocated sanction alternatives is the use of intensive supervision programs (ISP).[13] Intensive supervision is similar to more traditional forms of community supervision except for increased constraints on and monitoring of activities. There are often more demanding requirements for successful completion of probation as well (Gowdy, 1993). At a tactical level, the use of intensive supervision results in significantly greater contact between the supervising officer and the offender (Latessa and Vito, 1988; Petersilia and Turner, 1990).

Conrad (1985) suggests that increased contacts may serve double duty. First, increased contact leads to heightened surveillance. Thus, an offender who engages in activity that violates terms of community supervision has a higher probability of being detected in an intensive supervision program.[14] Second, argues Conrad, the development of a stronger relationship between the probationer and the supervising officer can result in little harm at worst and can do much good. Good rapport and mutual understanding between officer and offender can, by itself, have significant implications for community adjustment.[15]

2.2.2 Research

Several themes have emerged from findings of evaluation studies in this area. First, an artifact of increased supervision is a greater opportunity to observe technical violations of supervision conditions (Petersilia and Turner, 1990; Clear and Hardyman, 1990). Second, ISP's along with other intermediate sanction programs have

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[13]Intensive supervision is also a broad term that can refer to a range of supervision methods. In its simplest and most commonly used form, more intense supervision connotes a heightened level of contact between the supervising officer and offender. Variations of traditional intensive supervision include recent innovations such as home detention, electronic monitoring, and programs that emphasize working with offenders on a group rather than on an individual basis. The research to date on these alternative programs has not uncovered any strong differences in recidivism that can be attributed to their use (Gowdy, 1993; Corbett and Marx, 1991; Baumer and Mendelsohn, 1990; Erwin, 1990; Petersilia and Turner, 1990).

[14]Pearson and Harper (1990) also make this contention.

[15]If achieving stronger relationships and better rapport with offenders has little impact on behavior yet costs more to implement, however, the negative considerations increase.

often been advocated for reducing prison populations. Two methods for accomplishing this have received the most attention. Immediate crowding reductions are facilitated by moving prison-bound offenders into intermediate non-incarcerative or short-term incarcerative facilities (MacKenzie and Piquero, 1993). Moreover, sanctioning alternatives that lead to reductions in recidivism would also have an impact on prison capacity that is perhaps less immediate but no less real (Lattimore and Baker, 1992).

Early analysis of the Georgia Intensive Probation Supervision program revealed promising violation rates for otherwise prison-bound intensively supervised probationers (Conrad, 1985). Similar results emerged in a 1987 evaluation of the Georgia program (Erwin and Bennett, 1987). Recidivism rates were generally lower for intensively supervised offenders although they tended to be detected for violating probation conditions more frequently. The Georgia evaluations, however, suffer from a research design that utilizes nonequivalent comparison groups.[16]

A New Jersey evaluation revealed lower recidivism rates but higher drug violation rates for a group of intensively supervised offenders[17] compared to a matched prison parolee group (Pearson, 1985; Pearson and Harper, 1990; Gowdy, 1993). As was the case in Georgia, however, Petersilia and Turner (1990) questioned the inferential value of the data collected in the New Jersey program since the offenses committed by ISP participants tended to be less serious than offenses committed by comparison group offenders.

Using an experimental design, that focused on the effects of intensive supervision in three California counties, Petersilia and Turner (1990) found no strong differences in new crimes and arrests between intensively supervised probationers and a control group. In two of the three counties they studied, however, technical violation rates were higher among probationers who were more intensively supervised. Intensively supervised offenders tended to undergo counseling and treatment programs and secure employment at greater rates than the control group.

A recently published fourteen site evaluation of the effectiveness of intensive supervision programs as an intermediate sanctioning alternative revealed several key findings (Petersilia and Turner, 1993). First, the ISP's studied (at the county level) were

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[16]Petersilia and Turner (1990) make explicit note of this flaw in their California study: "[t]he ISP and prison comparison groups were not only not very comparable, they differed in characteristics that are known to affect recidivism (e.g., high risk)" (p. 15).

[17]These offenders served a minimum of sixty days in prison before being placed on intensive supervision in the community (Pearson, 1985).

objectively more restrictive and were perceived as such by offenders than more traditional forms of community supervision. Second, there were no significant differences in arrest rates during community supervision between intensively supervised offenders and a control group. As in the California evaluation, technical violation rates were significantly higher for ISP offenders. While some would interpret these data negatively, Petersilia and Turner offered an alternative view:

> The General Accounting Office, in its report on intermediate punishments, noted that if judged by a standard of zero risk, all [ISP] programs fail to protect public safety. However, what most of these programs try to achieve is a more stringent punishment for at least some of the serious offenders who now receive only nominal supervision. Judged by that criterion, virtually all of the sites succeeded. It is also possible that the closer surveillance imposed on [ISP] participants may increase the probability that they are caught for a larger percentage of the crimes they commit (p. 5).

Results of the multi-site study suggested that offenders in ISP programs were more likely to undergo treatment programs which, in many states, were required for at least some offenders as a condition of probation or parole.[18] In nearly all of the sites, employment rates for ISP participants either equaled or exceeded those of control subjects.[19] Both of these results lead to the expectation that supervision intensity will have a positive effect on at least some components of a successful community adjustment construct.

A variation of the pure intensive supervision approach involves sanction integration. Often, however, intensive supervision programs play a prominent role in these efforts. Shock probation/parole programs constitute one such effort. These

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[18]Legal coercion has been found to be a viable instigator of positive behavior on at least some broader measures of positive behavior (Anglin and Hser, 1990). Their study focused on the impact of requiring offenders to stay in a substance abuse treatment program as a condition of community supervision. The results suggested that the imposition of treatment requirements resulted in longer treatment participation. A number of other studies have uncovered similar results (Pearson, 1985; Petersilia and Turner, 1990; Pearson and Harper, 1990; Gendreau and Andrews, 1990). In the Petersilia and Turner (1993) study, the overall prevalence of counseling among ISP participants was 45% compared to 22% for control subjects (p. 8).

[19]Overall, the authors report a 56% employment rate for ISP participants and a 43% employment rate for control subjects (Petersilia and Turner, 1993, p. 8).

programs usually precede intensive or regular community supervision with a short term of incarceration. With novice offenders, the short prison term may deter recidivism while more intense community supervision (where it is imposed) may help the offender adjust to the community in a wide array of areas (Latessa and Vito, 1988; Vito and Allen, 1981; Vito, et al., 1985; Vito, 1984).

In a comparison of shock and regular probationers, Vito and Allen (1981) found that Ohio shock probationers were reincarcerated more frequently than regular probationers. Vito et al. (1985) discovered, in their multivariate analysis of rearrest in Jefferson County Kentucky, that the effect of the shock program on recidivism was nonsignificant. Another study comparing the effect of shock probation with regular supervision to shock probation with intensive supervision in Lucas County Ohio found little incremental change in positive community adjustment attributable to the use of intensive supervision (Latessa and Vito, 1988). In sum, the addition of intensive supervision to the shock probation regimen resulted in a greater number of contacts between supervising officers and offenders and a higher employment rate among those receiving intense supervision. But the study revealed no reductions in recidivism in the shock-plus-intensive supervision group.

2.2.3 Summary

The preponderance of the literature reports little empirical support for the contention that ISP's and their variants will lead to dramatic reductions in recidivism. Indeed, an important issue for future research to address is the relatively consistent finding of higher technical violation rates among ISP participants. Moreover, the labor intensive nature of ISP has in practice made it a costly correctional option. Whether it is more costly than incarceration depends on a host of conditions.[20] It does seem reasonable that in many jurisdictions ISP's hold the potential for reducing the load on physical facilities. But it seems just as probable that human resources will have to absorb the load. At best, it appears that ISP's as they are currently being implemented do not hold great promise for inducing noncriminal behavior or creating revolutionary cost-savings for correctional systems.

That ISP's do not achieve these ambitious objectives does not mean they should be abandoned. Research provides some insights into the potential strengths of ISP's as well. First, while ISP participants do not appear to behave significantly better, nor do they appear to behave significantly worse. Even though ISP offenders often have higher technical violation rates, the possibility of a surveillance effect is too plausible to be ruled

[20]In some instances, emphasis on treatment and counseling further ratchets up the cost of sanction delivery.

out.[21] Second, on some broader measures of post-release behavior, largely the behavior considered in this paper, ISP participants do appear to perform better. ISP participants, either by meeting required conditions of their sentence or through a motivation to change, tend to seek out treatment more aggressively and secure employment more frequently than their non-ISP counterparts. In this research effort, one of our central inquiries will focus on the relationship between indicators of supervision intensity and successful adjustment to the demands of life in the community in a wide array of areas.

2.3 Assessment

Although research designs in many studies on the efficacy of intermediate sanctions have not been as strong as researchers would like,[22] one cannot help but be struck by the consistent absence of strong behavioral differences for program participants in performance across the range of these studies. Such anticlimactic findings are not necessarily problematic for these programs.

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[21]Petersilia and Turner (1990, 1993) found this possibility so compelling that they recommended deemphasizing technical violations. Noting that the pursuit of technical violations, which are often relatively trivial events compared to serious new criminal activities, consumes a large proportion of ISP officer time, deemphasis could have significant effects on the costs and quality of community supervision.

[22]Several important threats to the validity of these studies have been identified in previous research efforts (cf. Souryal and MacKenzie, 1993; MacKenzie and Shaw, 1993). First, many studies to date have not incorporated a time dimension into their methodologies. To conclude simply at the end of a one- or two-year follow-up period that one group outperformed or did not outperform another may be throwing away important information (Schmidt and Witte, 1988). Studies that do incorporate a time dimension should also control for the number of subjects at risk for failure in a given time interval. Sole reliance on an "average time to failure" can yield misleading results when the number of subjects at risk at any given point is ignored. Another consistent problem in many evaluation efforts is the common finding of *a priori* differences between groups that are ultimately compared on failure criteria (see especially, Petersilia and Turner's (1990) criticism of the Georgia and New Jersey intensive supervision evaluations). Such differences can manifest themselves in findings of spurious between-group contrasts and selection effects that can be difficult to untangle. Although difficult to achieve in applied correctional settings, research is inevitably strengthened when random assignment to comparison groups is possible.

Although shock graduates and ISP offenders do not appear to perform dramatically better than other groups nor do they perform dramatically worse. Thus, the finding that intermediate sanction programs hold their own in comparison to other correctional options must be weighed in the balance. More generally, when intermediate sanction programs result in less rather than more confinement but yield similar aggregate behavior patterns, their use will continue to warrant serious consideration (MacKenzie, 1991).

While little research has been conducted on the relationship between shock incarceration and broad-based positive offender adjustment to the community, a significant amount of research suggests that supervision intensity plays a key role. In general, studies have found that offenders who are supervised more intensively tend to adjust better on measures such as seeking treatment and counseling, securing employment, and accepting responsibility than other probationers and parolees. Nevertheless many of these same studies have revealed that intensively supervised offenders are significantly more likely to be revoked for a technical violation. The policy maker is thus left to predict both a good and a bad outcome from the same set of exogenous circumstances.

Perhaps one explanation is that, in many instances, indicators of positive adjustment are simply tasks that offenders are required to perform as conditions of community supervision. Offenders may perform these tasks more frequently and regularly when they are monitored more closely. Since they are monitored more closely, violations are more likely to be detected. Thus, it may be too large a leap to conclude that performance of these tasks, which lead to higher positive adjustment scores, is indicative of anything other than the offender meeting minimum requirements to avoid revocation.

It is also possible that intensively supervised groups, with a higher technical violation rate, adjust quite positively in other ways. Alternatively put, even among those who fail on recidivism and probation violation indicators, the net effect of supervision intensity in other life areas may well be positive. This raises a difficult issue. If offenders are generally adjusting well but are then revoked for relatively minor technical infractions, researchers and policy makers have to decide whether limited resources are being put to the best use. In short, whether the cure is worse than the illness becomes the relevant question. This possibility has led some researchers, most notably, Petersilia and Turner (1993) to advocate the deemphasis of technical violation-based revocations in the intermediate sanctions realm.

A central focus for our research is identifying the effects of shock incarceration and intensive supervision on a relatively broad measure of adjustment to community supervision. Specifically, we focus on a range of positive behaviors that traditionally fall outside the sweep of legal scrutiny. Just as previous research on the effect of shock incarceration has focused on changes in attitudes and recidivist behaviors, this analysis examines the impact of correctional sanctions and community supervision on this wider spectrum of positive behaviors. It is to this task that we turn in the following sections.

3. Research Design

3.1 Overview

Data comprised of offender adjustment and background variables were collected in Louisiana between Spring 1988 and Spring 1989. In Florida, South Carolina, and Georgia, adjustment and background data were collected between 1989 and 1991. New York data were collected between Spring 1988 and Fall 1990. Table 1 presents the treatment sample distributions by state.[23] In this section, we review the method by which subjects were selected for the study. Subject selection methods varied by state and each state's selection process is, therefore, considered individually.[24] We then turn to a description of the information that was acquired for each offender along with the data collection procedure.

3.2 Subjects

3.2.1 Florida

In Florida, subsets of offenders in prison or the shock program at the time data collection commenced were followed on community supervision. Offenders in prison who were selected for the study met the legal eligibility criteria for the shock program. The Florida program is noteworthy for its relatively high attrition rate. In fact, the in-program attrition rate in the Florida shock program was the highest of any state in the study. Although voluntary withdrawals were not permitted, state officials reported attrition rates exceeding 50% for disciplinary and medical reasons (MacKenzie and Souryal, 1992). This high attrition rate within the Florida shock program has potential implications for the findings described later in this report.

3.2.2 Georgia

The subjects in the Georgia evaluation were randomly selected from populations of shock graduates, probationers, and parolees. Probationers and parolees were selected to be legally eligible for the shock program. The target sample size for each group was

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[23]The terms, "sample" and "treatment sample" are used interchangably to refer to the quasi-experimental group (shock, prison, probation, etc.) to which offenders were assigned within each state.

[24]Major differences in the programs are considered in the administrative summary preceding this document. A detailed description of the programming emphases for each state is provided in MacKenzie and Souryal (1993).

N=100. Final sample sizes did not equal 100 because some offenders transferred out of state and other offenders secured early release from supervision. Although dropping out was allowed for medical reasons and disciplinary terminations were possible, the prevalence of attrition in the Georgia study was virtually zero. We, therefore, do not consider this group separately.

3.2.3 Louisiana

In Louisiana, the original analysis plan called for collecting data on the first 100 offenders who met the legal, suitability, and acceptability requirements for the shock program and entered one of three correctional sanction programs: shock incarceration, prison, and probation. By the end of the data collection period, however, the full 100 cases had not been secured in either the shock incarceration or prison group. Slightly more than the target of 100 had been secured in the probation group. Additionally, a small group of offenders who entered the shock program but either dropped out or were terminated from the program during their sentence was followed.

3.2.4 New York

In New York, random samples of shock inmates, prison parolees, and shock program dropouts were selected. Selected prison parolees met the legal eligibility requirements for the shock program. Because the prison parolees entered prison prior to the institution of the shock program, however, they were not evaluated on suitability criteria.[25] Legal eligibility criteria that were formally evaluated for each subject in the prison parolee sample included criminal history (no prior service of an indeterminate sentence), offense type, and age. All offenders were released and followed on community supervision during the window described above.[26] Target sample sizes were 100 for each group. Fourteen of the subjects selected in this process were females and they are not analyzed in this study. The final sample size was N=286.

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[25]New York officials refer to this group as the "pre-shock" sample.

[26]Importantly, in New York, shock offenders are required to undergo intensive supervision for at least the first six months of their parole period. Disaggregated data containing the actual levels of supervision intensity in New York were not available for analysis.

3.2.5 South Carolina

Four groups were selected for the full community supervision follow-up study in South Carolina. The first group was comprised of a sample of shock graduates who completed the program in late 1989. This group completed the shock program when it was under the direction of the South Carolina Department of Probation, Parole, and Pardon Services.[27] Offenders in this group had been directly sentenced to the program by the judge hearing their case.

A second shock sample was comprised of the population of offenders who graduated from four consecutive shock programs. The shock program, for this group, was administered by the South Carolina Department of Corrections (DOC).[28] Offenders could enter this program via a direct sentence or at the discretion of officials in the Department of Corrections given offender compatibility with eligibility criteria. It seems reasonable to speculate that the pool of offenders from which the DOC group was drawn would have been more likely to serve prison time than the DPPPS group (Souryal and MacKenzie, 1993). Since positive adjustment data were not collected on this group over time, however, we were not able to include it in our over-time analysis. Instead, positive adjustment data were collected at the end of the one-year follow-up period for these offenders. Supervision intensity data (contacts) were only collected for this group during the first six months of community supervision. In our analysis of positive adjustment data for this group we will aggregate supervision intensity information over the entire six month period it was collected and assess its impact on positive adjustment scores for the entire year.

Groups of probationers and prison parolees were selected at random from the populations of probationers and prison parolees who met legal eligibility criteria for the shock incarceration program and began supervision in late 1989. A final follow-up group, the split-probation sample, was comprised of offenders originally selected for the probation sample. Upon close examination of these offenders' records it became apparent that they had served short prison sentences. They were, therefore, analyzed as a separate group (Souryal and MacKenzie, 1993).

3.3 Instruments

3.3.1 Overview

Data were collected from two sources. Demographic, current offense characteristics, and prior criminal history variables were

[27]We will refer to this group as the S.C. DPPPS sample.

[28]We refer to this sample as the S.C. DOC shock group.

available from offenders' official records. At the beginning of community supervision, a follow-up instrument was set up for each offender. The follow-up instrument was designed mainly to facilitate recording of contacts with and related to the offender, recidivist activities, and positive adjustment items. The primary responsibility for compiling information on positive adjustment to community supervision as well as the intensity of supervision rested with the community supervision officer. Offender contact information was collected on a monthly basis while positive adjustment items were completed on a quarterly basis (every three months).[29,30]

3.3.2 Positive Adjustment

3.3.2.1 Description

In all states except Louisiana, offenders' positive adjustment to community supervision was evaluated with a ten-item index[31] that measured such attributes as the offender's employment status, ability to support himself and his family, and financial and emotional stability. The index items and their overall means (averaged over the entire study period) are presented in Table 2. In Louisiana, an eighteen item index measuring similar attributes was used. These items along with their overall means (also, averaged over the entire study period) are provided in Table 3 while overall item means are summarized in Table 4.

Positive adjustment index items are scored with a zero if the offender is not performing well on the item or if the item is not applicable and a one if the offender is making satisfactory progress. We determine, for each offender, whether approximately 80% of the items (8 in all states except Louisiana and 14 in Louisiana) were completed at time *t* by the supervising officer. If the 80% criterion was met or exceeded, the available ones and

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[29]In Louisiana, positive adjustment data were compiled for each month. For our analysis, we aggregate these data into quarterly measurements.

[30]Quarter, measurement period, and time are used interchangably throughout the report. Thus quarter 1, the first measurement period, and time 1 all refer to the same follow-up period. When the term "month" is used it is constrained to values of 3, 6, 9, and 12. Month 3 refers to quarter 1 while months 6, 9, and 12 refer to quarters 2, 3, and 4, respectively.

[31]This index is based on the same set of questions that were used in Latessa and Vito's (1988) evaluation of the impact of intensive supervision on a sample of shock probationers.

zeroes were summed and divided by the number of valid entries for the offender.[32] In short, as the positive adjustment score approaches 1.0 the offender, according to the supervising officer, is adjusting positively in many areas and, as it approaches 0.0, there is little evidence that the offender is adjusting positively.

3.3.2.2 Reliability

Analysis of the scalability of these items revealed that they created an internally consistent composite. Cronbach's coefficient α and item-to-composite correlations were computed in each of the states at each of the follow-up points. These results indicated that the correlation between the positive adjustment index and all other 10-item (or, in the case of Louisiana, 18-item) indexes that measure the same construct approaches or exceeds 0.80 in every instance.[33] Item-to-total correlations provided no evidence of any individual item producing a large "drag" on the internal consistency of the index.[34] Table 5a presents a reliability analysis of the positive adjustment scores in each of the states except Louisiana. Reliability analyses for the Louisiana construct are depicted in Table 5b.

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[32]The calculation is: $\Sigma(PA_i)/N_i$, where PA_i refers to 1's and 0's indexed over items with valid responses and N_i indicates the number of items with valid responses.

[33]This interpretation of the inference associated with reliability measures such as Chronbach's alpha is supplied by Norusis (1989). The reliability analyses reported here were calculated by subprogram RELIABILITY in SPSS release 4.0.

[34]Positive adjustment item #8 (in all states except Louisiana) had a smaller item-to-total correlation in each state but its effect on Cronbach's α was negligible. Item 8 asks the officer to report whether the offender was participating in self-improvement programs (which could include vocational, educational, group counseling, or alcohol or drug maintenance programs). Given its small impact on the scale's internal consistency, we decided to retain it in our analysis of the positive adjustment construct.

3.3.3 Community Supervision Intensity

Supervising officers were asked to provide information measuring the intensity of their supervision of the offender. In all states except Louisiana, intensity of supervision is measured with a variable that indicates the number of contacts with the offender.[35] We refer to this variable as a "primary offender contacts" indicator. In Florida, complete data were collected recording the number of contacts with family, employer, and treatment delivery professionals. We refer to this variable as a "secondary contacts" indicator. Contact data were not available for New York subjects and data pertaining to secondary contacts were collected incompletely in South Carolina and Georgia.[36] Contact information was collected each month but positive adjustment data were collected each quarter. For this analysis, contact variables were aggregated over months within measurement periods so that they represent the mean number of contacts per month during each quarter of the study.[37] Table 6 presents overall descriptive statistics for contacts in Florida, Georgia, and South Carolina.

In Louisiana, intensity of supervision is measured through the use of three indexes measuring different dimensions of supervision intensity. A knowledge index measures how much the supervising officer knows about the offender's activities. Constraints on the offender's movements and formal requirements of community supervision were measured with a second index. A third index summarizes the level of surveillance that the supervising officer applies with respect to the offender. Increases in the

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[35]Originally, it was anticipated that primary contact variables would tap two dimensions: face-to-face and telephone contacts. Complete data were not collected on these two dimensions in Georgia and South Carolina, however. We, therefore, analyze the influence of aggregate primary contacts which is the simple sum of the two components in Florida, Georgia, and South Carolina.

[36]For the DOC shock sample in South Carolina, secondary contacts were collected only for the first six months of the study and not at all for the other samples. In Georgia, the quality of these data were better but data for family contacts at month 3 as well as data for contacts with employer and treatment officials at months 1-3 were missing for virtually all subjects. It is difficult to justify using this information in the absence of a good baseline measure of these scores.

[37]The calculation is: $\Sigma(C_i)/N_i$ where C_i refers to the number of reported contacts indexed over the months with valid contact information within measurement periods and N_i refers to the number of months within the three month measurement period for which contact data were reported.

requirements and surveillance indexes are associated with heightened supervision intensity while lower scores on the knowledge index connote higher levels of knowledge about offender activities. As in the other states, supervision intensity information, although collected on a monthly basis, is aggregated by quarter.[38] The components of these indexes and their overall means are presented in Table 7.[39]

3.4 Procedure

3.4.1 Official Records

Official-record information was collected for each offender at the beginning of the study. These data included the sample to which the offender belonged (shock, prison, probation, etc.), the offender's race/ethnicity, age at release, age at first arrest (in Louisiana, New York, and South Carolina), the type of offense committed (violent, property and "other", or drug-related),[40] whether the current sentence was the result of new criminal activity (regardless of whether the offender was in community supervision) or a technical violation of community supervision conditions,[41] and whether the offender had a record of prior arrests and/or convictions in his corrections file.[42] These

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[38]The calculation is the same for each index: $\Sigma(X_i)/N_i$ where X_i refers to the knowledge, requirements, or surveillance score indexed over months within measurements periods. N_i refers to the number of months within the three month measurement period for which supervision intensity data were reported.

[39]We note here that increases in the knowledge index are associated with decreases in knowledge about the offender's activities while increases in the requirements and surveillance indexes imply increased supervision intensity.

[40]In New York, offenses were classified as property, drug-related, and "other." The "other" category includes crimes against persons.

[41]No offenders in the New York study were serving a current sentence for a technical violation.

[42]Other variables measuring criminal history were available in the analysis files. The prior arrests and/or convictions indicator subsumes all of them except in some states where a count of the number of prior arrests and/or convictions was available. The separate indicators tended to be correlated with each other. After testing several preliminary models, we decided to use the broader indicator but the use of the more narrow indicators did not lead to

characteristics are, of course, fixed for the duration of the study.

3.4.2 *Follow-Up*

3.4.2.1 *Follow-Up Procedure*

Offenders were then followed for one year or until they exited from the study. During the follow-up period, community supervision officers were asked to compile recidivism, contact, and positive adjustment information at three-month intervals.[43] As noted earlier, primary interest here is in positive adjustment and supervision intensity information. Information on these variables was not relevant and generally not collected after offenders exited from the study.[44]

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different sets of conclusions.

[43]Although officers actually completed the forms once every three months, questions regarding contacts were specifically directed toward each month within each quarter. Positive adjustment data, however, were quarter-specific in all states except Louisiana.

[44]In some cases, offenders exited the study and then returned later. These subjects are identifiable because they have at least one interrupted measurement on the positive adjustment index. For these offenders, the exit may have been a "true" exit (i.e., the offender dropped out of the study). Alternatively, the supervising officer may have failed, for one reason or another, to compile an evaluation for an offender at a particular measurement period. Cases in both of these categories are treated as having exited the study for purposes of the longitudinal analysis since measurement continuity is required. As described later, however, this paper includes a cross-sectional analysis which will take the extra information associated with these offenders into account.

3.4.2.2 Follow-Up Attrition/Panel Mortality: Overview

We define attrition in this study as the point in the one-year follow-up period at which consecutive measurements on the dependent variable (positive adjustment) cease to exist in the data. Thus, a case that has measurements at quarter 1 and quarter 2 but not at quarter 3 or quarter 4 would be defined as completing two measurement periods.[45]

Using this definition of attrition, Figures 1 (Florida), 2 (Georgia), 3 (Louisiana), 4 (New York), and 5 (South Carolina) indicate the sources of attrition as best as they can be identified from the current data.[46] In Figures 1-5, we also introduce the "exit cohorts" which will be an important tool for avoiding specification bias in both cross-sectional and longitudinal models of supervision intensity and positive adjustment. In each state, cohort 0 (or the "missing" cohort) is comprised of offenders with no positive adjustment evaluation for the first three months of the study. The number of offenders in this cohort varies by state. In Georgia and New York approximately 50 offenders had no measurement

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[45]In some cases, measurements were interrupted. An example of this would be a case with measurements at quarters 1, 2, and 4 but no measurement for quarter 3. A case such as this, under our definition of attrition, would fall out of the over-time study at the end of the second quarter. As we shall describe later, we attempt to make use of this information in one of our analyses by aggregating all available positive adjustment data for the entire twelve month follow-up period for each case.

[46]Since the source of attrition was not a variable that was explicitly collected in these data sets, we have to make some inferences about the basis for a case's termination or interruption in measurements. Our approach to this problem is simple. Among those cases with a termination or interruption in measurement we interrogate fields that indicate whether the subject had experienced one of the following outcomes: (1) revocation for a new crime; (2) revocation for a technical violation; (3) revocation for an absconding incident; (4) jailed for any reason; (5) new case pending; (6) arrested; or (7) legally released (in Georgia, Louisiana, and New York only). In the case where more than one event occurred offenders were mapped to categories in the order presented above (except in states where legal release information was available it had the highest precedence). In Georgia, we could not assess arrest, jailing, or case pending terminations. If the case terminated or had interrupted measurements we associated that termination with the applicable disruption source. In New York, terminations or interruptions could be tied to a source in all of the cases. In other states, the reasons for termination or interruption were not always apparent from the data. We map those cases to an "unknown" attrition source category.

at the first period.[47] This cohort was comprised of fewer numbers of offenders in Florida, Louisiana, and South Carolina.

Exit cohort 1 is comprised of offenders who completed the first quarter but exited with no second quarter measurement. Similarly, exit cohorts 2 and 3 include offenders who were continuously measured through the second and third periods, respectively. Exit cohort 4 includes offenders with a complete set of four measurements on the positive adjustment scale. We note here that the exit cohort groups satisfy the formal definition of a variable insofar as their categories are mutually exclusive and exhaustive.

3.4.2.3 The Case For Using Exit Cohorts

The establishment of exit cohorts has several advantages. The major benefit is that it provides a useful mechanism for controlling panel mortality (and, in the process, sweeping up effects for which panel mortality proxies). In short, exit cohorts facilitate a division of the subjects into categories based on their unobserved propensity to complete the study. The cohort categories represent the observed manifestation of the propensity to complete the study but it seems reasonable to expect that true but unobserved propensity would be some continuous variable that underlies these categories. If this variable were observable, we might even be able to assign a name to it like the "A-trait" (for attrition), θ, or y^* (Lattimore and Linster, 1993; Nagin and Smith, 1991; Maddala, 1983). Since it is not observable (and we have no single behavior or instrument in which we are interested) in this context, however, the cohort categories must suffice.

An examination of Figures 1-5 reveals that most of the sources of attrition are not positive events (e.g., legal release would be an exception). To the extent that unobserved propensity to complete the study is related to both positive adjustment and other independent variables, statistical models of positive adjustment that omit this variable will be technically misspecified. Similarly, if supervision intensity is assumed to be at least partially stochastic, the part of supervision intensity that can be predicted from the data is of some interest. If this component of supervision intensity is a function of propensity to complete the study and other predictor variables yet these predictor variables and unobserved propensity to complete the study are correlated, models of supervision intensity will also be misspecified.

Another advantage of the use of discrete exit cohort categories is

[47] Although, as described below, the sources of this early attrition could be identified with a high level of confidence in New York. In Georgia, sources of attrition were more ambiguous.

that inclusion of k-1 (where k is the wave, or measurement period, of the panel model being estimated) cohort dummy variables into a statistical model of positive adjustment or supervision intensity does not impose a particular (say, linear) functional form on the model. Instead, each cohort category in such a model would be compared to the reference category. The cohort dummy variables would then be constrained only to have the impact of adjusting the intercept term up or down by a certain quantity (the parameter estimate) relative to the reference category controlling for other effects in the model. Since the substantive interpretation of a statistically significant exit cohort effect is inherently ambiguous in the first place, such a constraint would be appropriate.[48] Not surprisingly, the utility of the exit cohort effect is not in its theoretical implications. It is simply used in this analysis as an instrumental variable to proxy for other effects that would drive positive adjustment (or, supervision intensity) up or down, net of other predictor variables.

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[48]For example, it would be difficult to interpret the finding that in measurement period 3, offenders who were in cohort #3 (i.e., offenders who were about to exit the study) adjusted less positively than offenders in cohort #4. On its face, it seems plausible that offenders who are about to exit (usually for a negative rather than a positive reason) would also not be adjusting as positively to life in the community. However, in practice, it would be difficult to get at the true cause of lower positive adjustment. The unobservable propensity to complete the study simply proxies for an omitted variable that would represent the true cause of an offender's lower positive adjustment net of other effects in the model.

3.4.2.4 Attrition Patterns By Treatment Sample

Tables 8a through 8e break down the attrition patterns by treatment samples for each of the five states.[49] These data reveal that attrition rates as a percentage of the total sample size were greatest in Florida (79.9%) and Georgia (76.0%). As these data also reveal, identification of the sources of attrition in Florida and Georgia was more difficult than in the other states. In both Florida and Georgia, it seems reasonable to speculate that a significant portion of the unknown attrition sources were legal releases. This would not be inconsistent with the data insofar as the greatest drop in case follow-up (as a percent of potential cases at a given measurement period) occurred between the ninth and twelfth months of the follow-up periods in both states.

Attrition rates were significantly lower in Louisiana (36.0%), New York (53.5%), and in those cases followed over time in South Carolina (31.8%).[50] In Louisiana, legal release was the single most commonly identified source of attrition while in the other states, revocations for technical violations and new crimes were more prevalent sources. The shock samples studied were more likely to complete the entire follow-up period than other samples in New York and Georgia. In Florida, South Carolina, and Louisiana, the shock samples had slightly lower completion rates. Importantly, however, in no state did the shock sample complete the study at dramatically worse rates than other samples.

One way of analyzing the sample differences formally is to create a variable that ranges in value from zero to four.[51] Next, each case can be assigned a value on that variable that corresponds to the number of quarters completed. The average number of study periods completed may then be compared across the study groups. Table 9 presents these comparisons for each of the states.

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[49]Importantly, the rates reported in Tables 8a-8e should not be interpreted as the correct recidivism/failure rates for the states in this study. It was possible for offenders to fail via multiple paths. It was also possible for offenders to have valid positive adjustment scores after a failure event had occurred (suggesting the offender had not permanently exited the study). These tabulations, put simply, represent our best estimate of what factors may have been responsible for subject attrition. In the absence of a variable that explicitly measures this, however, our inferences about sources of attrition will necessarily be inexact.

[50]This, of course, excludes offenders in the S.C. DOC shock sample who were not followed over time. Data on these offenders were compiled at the end of the one-year follow-up period.

[51]This variable is the same as the exit cohort variable.

The results indicate that in Florida, the shock and prison samples had significantly longer follow-up periods than the dropout sample. In Louisiana, the probation and shock samples had longer follow-up periods than the prison and dropout samples. The contrast between the probation sample and the prison and dropout samples was statistically significant.

In New York, the shock sample was followed significantly longer on average than the shock dropout sample although the shock and prison samples were followed for about the same length of time. In Georgia and South Carolina, the samples were all followed for approximately equal periods of time. Even in the states where samples had different follow-up periods, though, the average follow-up period never differed by more than one quarter.[52] Moreover, in none of these analyses does sample membership explain more than seven percent of the variation in average number of quarters followed.

Table 10 presents joint sample by exit cohort proportion distributions which largely complement the information presented in Table 9. This analysis also suggests that relatively small yet statistically significant amounts of variance in cohort membership are explained by treatment sample membership.

In Florida and Louisiana, the exit cohort distributions differ by sample as they did in Table 9. As in Table 9, the New York and South Carolina data reveal that exit cohort distributions are independent of treatment sample.

Unlike Table 9, however, the joint distribution of proportions in Georgia indicates that treatment sample and exit cohort membership are not independent. Instead, the probation sample is over-represented in the third exit cohort while the shock graduate sample is overrepresented in the fourth exit cohort. The proximity of the third and fourth exit cohorts rendered the difference too small to be detected by the analysis of variance test in Table 9. We thus have further evidence that treatment sample membership and position in the exit cohort structure are not statistically independent of each other.

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[52]And only in Florida was there a difference that went much beyond a one-half of a unit. The dropout group in Florida had a mean follow-up period of 1.7 quarters while the prison parolee group was followed for 2.5 quarters on average. Smaller contrasts were observed in other states.

3.4.2.4 *The Impact of Attrition on Fixed Effect Distributions*

Table 11 describes the aggregate changes in the composition of the study sample at each measurement period. One column is reported for each follow-up period. Since the groups are neither completely independent nor dependent, the proper application of a test of significant differences is not clear.[53] Rather than pursue this issue, we adopt a simpler approach. The means of the study variables at month 3 represent the baseline score for each variable. These scores are incremented and decremented by 20% of their original value and then compared to the scores at months 6, 9, and 12. Asterisks indicate those scores that changed by more than 20% of their baseline value.[54]

The results of this analysis indicate that the composition of the cases with respect to the predictors do not vary by large magnitudes over the course of the study period. Some variation, however, is noteworthy and requires comment. When relatively large variation was evident it tended to be associated with the treatment sample indicators. In all states but Florida, these relatively large differences did not appear until the fourth quarter of the follow-up period.

In Florida, the shock dropout sample's prevalence in the dataset decreased by 53.6% over the four quarters. In fact, the shock dropout sample's presence in the dataset dropped most dramatically between the sixth and ninth months of the study period. While the shock sample in Florida maintained a relatively stable presence in the dataset, prison sample offender presence increased by 32.8%. There was also a spike in the presence of violent offenders in the dataset at the ninth month but patterns closely reflecting the baseline reemerged at month 12. In Florida, it is reasonable to conclude that cohort membership can be predicted with better than chance accuracy given knowledge of sample membership.

In Georgia, there is evidence of increasing strength of presence for the shock sample while the prevalence of the probation sample decreases over the study period. Moreover, the Georgia data reveal that violent offenders increased their presence in the dataset over

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[53]For example, the month 3 column includes subjects in all four measurement cohorts while the month 6 column only includes subjects in measurement cohorts 2, 3, and 4. Since analysis of variance and difference of proportions tests assume that the comparison groups are either independent or dependent, the validity of statistical significance tests in this context is questionable (Blalock, 1979).

[54]The choice of 20% change in the mean of the variable is clearly arbitrary but 20% change implies 80% stability which is a criterion that is often deemed to meet minimal standards for test-retest reliability.

the follow-up period while the presence of drug offenders decreased somewhat. These differences are probably not large enough to distort results, however, since the proportions were small at the outset.

The Louisiana data also indicate that sample prevalence changes over the duration of the follow-up period. While the shock graduate and shock dropout samples maintain a relatively stable presence over time, the presence of the probation sample grows and the presence of the prison sample diminishes. As in Florida and Georgia, the means for the other variables remained relatively stable over the follow-up period.

In New York and South Carolina, analysis of dataset composition at each follow-up period reveals little evidence of over-time change. The means for nearly all of the variables at each follow-up point are stable as are the treatment sample prevalences.

3.4.3 Summary

A major procedural issue with respect to this analysis is panel mortality. In every state, our analysis has to deal squarely with large numbers of subjects not completing the study. Matters are complicated by the absence of a concrete variable that defines the reason for an exit from the study. Our response to this problem was to ascertain from the data as best we could what the basis for subject attrition was in every case where it occurred. We were more successful at this task in Louisiana, New York, and South Carolina than in Florida and Georgia.

While it appears that source distributions do not differ dramatically by state, the high unknown rates in Florida and Georgia force us to make qualifications. We simply do not know why there are fewer cases available for analysis in these two states over the entire follow-up period or why their attrition rates were greater between the ninth and twelfth months. In future research efforts, the acquisition of more detailed information surrounding the attrition event will be required to improve our ability to make inferences about the way these processes operate.

We concluded that most of the identifiable reasons for panel mortality were not positive. Indeed, it seems reasonable to suspect that there is something qualitatively different about the subjects who did not complete the study. It may also be that there are differences between offenders who exit early and offenders who exit late. To the extent that these differences are related to treatment sample membership *and* other variables of interest (particularly positive adjustment), a model that omits this effect would be technically misspecified. While the substantive interpretation of an "exit cohort" effect is ambiguous at best, its potential value for avoiding specification bias appears to justify further exploration of its merits for use as an instrumental variable.

Our next task was to determine whether membership in particular "exit cohorts" was independent of treatment sample. The analysis revealed that in New York and South Carolina, the two variables were essentially independent but in the other states, there was clear evidence of a relatively weak but statistically significant association.

Despite the finding of some dependence between exit cohort and sample membership in three states, few other differences attributable to panel mortality emerged. In particular, we focused on changes in the composition of the analysis datasets at each wave of the follow-up period. The results of this effort revealed changes in treatment sample distributions in Florida, Georgia, and Louisiana but few other differences were evident. Thus, while some samples were overrepresented in early and late exit cohorts, other variables maintained their baseline means throughout the one-year follow-up period.

We leave the assessment of panel mortality with the tentative conclusion that it will be necessary to control for exit cohort membership in our subsequent analysis. We base this conclusion primarily on the *finding* that exit cohorts are not independent of treatment sample in three of the five states in the study. We base it secondarily on the *expectation* that whatever effects are swept up by exit cohort membership, an early exit from the study will not have positive implications for adjustment to the community. For now, enough evidence exists to carry the exit cohort effects forward to the preliminary analyses that we conduct in the next section. The evidence presented in this section also suggests that the composition of the datasets in each state remain relatively stable in terms of the other study variables for the duration of the follow-up period. Thus, while exit cohorts appear to be related somewhat to treatment sample, there appears to be little basis for pursuing other associations.

4. Preparatory Analysis

4.1 Overview

In this section, we turn to a detailed description of both the structure and content of the data. We also conduct a number of preliminary analyses that have special relevance for the multivariate models we develop in section 5.

In the terminology of Cook and Campbell (1979)[55] our analysis consists of multiple post-tests (repeated over time) with nonequivalent comparison groups. Diagramatically, the structure assumes the form presented in Figure 6 and it may be conceptualized as a multi-wave panel design, where the same subjects are followed over time and measurements are taken at several fixed points in the follow-up period.[56]

The problems associated with this type of design are well-documented in the research design literature but the most important of them bears repeating here. Group assignment in post-test only designs is a critical success factor. To the extent that group assignment is random, the conclusions one draws about the differences between the groups (as a function of their receiving different treatments) are strengthened considerably. To the extent that group assignment is not random, the analysis becomes an exercise in establishing differences between groups that are different at the outset (Cook and Campbell, 1979; Dunn and Clark, 1987). These sorts of differences are evident in the data studied in this analysis and we are necessarily concerned about the *a priori* differences between the treatment samples within states. Consequently, we begin this preliminary analysis by examining the extent to which sample membership can be predicted with better than chance accuracy from the other predictors within each of the states.

After examining the *a priori* or fixed-effect differences between the samples in each state, we turn to the question of whether the treatment samples experience different supervision levels in addition to different treatments. Supervision intensity indicators are distinct from the other predictor variables because they are collected at three month intervals over a maximum one-year follow-up period along with positive adjustment information. Although some states have policies governing supervision intensity levels

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[55]See Chapter 3 in their text for a discussion of this point.

[56]All of the predictor variables except supervision intensity are fixed at the beginning of the study and do not change. We routinely refer to these variables as fixed effects and explicitly distinguish them from supervision intensity which is not fixed.

for offenders in certain classes of correctional programs,[57] we will treat this issue as an empirical question.[58] Importantly, though, we will be unable to develop conclusive models of supervision intensity because important variables that determine supervision intensity levels are not available for analysis. In sum, our supervision intensity analyses will be descriptive rather than explanatory.

One of our concerns is whether supervision intensity can be predicted with better than chance accuracy given treatment sample membership information. Given some of the special requirements imposed on shock program graduates for intensive community supervision, some differences in supervision intensity levels is to be expected.[59] This assessment will be particularly important since the literature to date suggests that supervision intensity is a key predictor of positive adjustment.

Next, we turn to a descriptive analysis of the positive adjustment construct itself. Included in this phase is the distribution of positive adjustment scores in each of the states and descriptive bivariate analyses between positive adjustment scores and each of the predictor variables. Three topics receive special attention in this section: (1) the relationship between treatment sample and positive adjustment; (2) the between-subjects relationship between supervision intensity and positive adjustment; and (3) the within-subject association of supervision intensity and positive adjustment.

Our most pressing task at the moment is to provide an overview of some of the analytical methods that we employ to assess a priori group differences, univariate distributions, time-stability, and initial model specification. We turn to this issue next.

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[57]In Florida, shock graduates are supervised in the community at the same level as prison parolees, although from time to time the court will order shock offenders to receive intensive supervision. In Louisiana and New York, shock graduates are required to be intensively supervised in the community for at least six months after leaving the program. In Georgia, most shock graduates receive regular levels of community supervision and in South Carolina, supervision intensity levels are dictated by an offender's risk score and applicable court orders. A more detailed process evaluation of programs in these states and others has recently been compiled by MacKenzie and Souryal (1993).

[58]This empirical question can be squarely addressed in all states except New York where supervision intensity indicators were not available.

[59]See footnote 4, *supra*.

4.2 *Preliminary Analysis Methods*

4.2.1 *Fixed Effects*

The assessment of a priori group differences on the fixed-effect variables in the study pose no special analysis problems. Essentially, the objective is to compare the groups on the relevant criteria and test the null hypothesis that the groups are the same. Among the fixed effects in our analysis, only age at community supervision and age at first arrest are continuous predictors. The other predictor variables are categorical. For the categorical predictors, we simply compare the relative frequency distributions over the categories are the same across the treatment groups. This test is accomplished with a test statistic for independence that is distributed as χ^2 with (r-1)(c-1) degrees of freedom (where r and c refer to the number of rows and columns of an r x c contingency table) (Blalock, 1979).

With the age variables, we impose the assumption that the age values are normally distributed in the populations from which these data are drawn, that the variances of those distributions are approximately equal across treatment samples and that each of the *n* observations are independently sampled. We then test the null hypothesis that $\mu_1 = \mu_2 = \ldots = \mu_k$ where k = the number of samples within the state and μ_i = the mean age (at beginning of community supervision or first arrest) in the population from which these data are drawn indexed over sample categories. The test statistic is distributed as F with n-1 (numerator) and n-k-1 (denominator) degrees of freedom (Dunn and Clark, 1987).

4.2.2 *Variables Collected Over Time: Supervision Intensity and Positive Adjustment*

4.2.2.1 *Univariate Analyses: Naive Models*

While the analysis of fixed effect variables pose no special problems or difficulties, the same cannot be said for the variables that are collected over time. We begin our analyses of both supervision intensity and positive adjustment by exploring their univariate distributions both cross-sectionally and at each of the four study periods.[60]

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[60]The cross-sectional analysis of these variables involves summing all available scores for the ith subject and dividing by the number of available scores: $\Sigma X_{ij}/N_{ij}$ where i refers to the ith case and j refers to the number of available scores. Our analysis treats supervision intensity indicators and positive adjustment scores similarly in cross-sectional analyses.

These distributions are capable of helping us to reach some informal conclusions but more formal methods are available. When data are collected over time, it is often useful to construct naive models of the way the variables "behave" over time. The models are referred to as "naive" because they do not incorporate a vector of explanatory effects. They simply facilitate a formal expression for generating expected values of variables at various points in time. We adopt this approach by elaborating somewhat on the usual presentation of a repeated measures analysis of variance framework.

We begin by noting that the naive repeated measures analysis of variance model is simply comprised of a test of the null hypothesis that there is zero within-subject change over time. For the two time period case the model assumes the form

$$y_{it} = \tau + \beta^* y_{it-1} + \varepsilon^* \tag{1}$$

and

$$\varepsilon^* = v_{it} - v_{it-1} + w_{it} - w_{it-1} \tag{2}$$

where w_{it} is random measurement error at time t, v_{it} is random variation in y_{it} that is restricted to time t and τ is a parameter estimated from the data by the method of ordinary least squares when β^* is constrained to a value of 1.0 (Allison, 1990). The t-test for the statistical significance of τ, under these constraints, yields a p-value which is the same as that for the F-test effect for time, or within-subject change, in the repeated measures analysis of variance model. The estimated value of τ is the average change in y_i from time *t-1* to time t across the entire vector of cases. For each state, we estimate the τ values between each two adjacent time periods for all available cases and provide tests of whether these values are significantly different from zero.

The form of eq. (1) has not escaped controversy (cf. Kessler and Greenberg, 1981; Markus, 1980; Cronbach and Furby, 1970). In short, the constraint that the implied regression coefficient for y_{i1} be equal to 1.0 may or may not be a point of contention. The obvious alternative to (1) is:

$$y_{i2} = \alpha + \beta y_{i1} + \varepsilon^* \tag{3}$$

where β is now estimated via the method of ordinary least squares (given that requisite error term assumptions have been met) and α is the value of y_{i2} when y_{i1} is equal to zero. What does β say about change in y_{it}? In and of itself, it is just what it appears to be: a tool for predicting the value of a score at time 2 based on the score at time 1. As values of the coefficient approach 1.0 then the constraint that subjects perform exactly the same over the interval is justified.[61] As values of the estimated effect move away from 1.0, however, there is some interaction that estimates y_{i2} to be a function of the joint distribution of initial score and time.

If β is positive then subjects with relatively high scores at time 1 will be predicted to have relatively high scores at time 2. If β is negative, then subjects with relatively high scores at time 1 will have relatively low scores at time 2 (Kessler and Greenberg, 1981).[62] Since such a crossover effect in the context of within-subject change is quite rare, the expectation is usually that β will be bounded by zero and one (Allison, 1990).

Given these properties, the estimate of β is usually referred to as a "stability coefficient" that reveals the percent of variation in y_{i2} that is stable and, by consequent, the percent of variation due to instability (Liker et al., 1985). The stability coefficient, when properly estimated, has the property of sweeping up the time-stable effects of omitted variables and, therefore, controlling for persistent heterogeneity or state-dependence in the variable of interest (Allison, 1990; Liker et al., 1985).[63] This model has

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[61]But for an adjustment for time, α, which is equivalent to τ in the case where β=1. In this case, time is constrained to affect all cases uniformly.

[62]Following Kessler and Greenberg (1981), it is also possible to subtract 1.0 from the value of the estimated coefficient, β to yield an estimate of the effect of a score on y_{i1} on the change in y over the interval (represented by the vector, Δ**y**, over the dataset). Thus, β-1 is an estimate of the shift in Δy for a unit increase in y_{i1}. If $\beta = .50$ then $\beta - 1 = -.50$ and it can be concluded that a unit increase in values of y_{i1} are associated with a 0.5 unit decrease in y_{i1} to y_{i2} change. This particular property leads to the regression to the mean phenomenon which requires that very high and very low initial scores are differenced to a greater degree than scores that are closer to the mean at time 1.

[63]While the stability coefficient can be estimated by the method of ordinary least squares, the results are problematic. In short, there is usually a substantial stability component when a variable is measured on the same observation at two or more points in time. The error term (ε_t) in an equation with a lagged endogenous variable (say, y_{t-1}) is indirectly related to y_{t-1} through the component of y that is time-stable. The consequences of OLS estimation of the stability coefficient in this context are twofold: (1) the estimates will be biased, or incorrect; and (2) they will be inconsistent (bias persists, regardless of the sample size) (Markus, 1980; Liker et al., 1985). Two-stage least squares estimation, where an instrument for the lagged endogenous variable

been described in the psychometric literature as the analysis of covariance approach to studying within-subject change (Markus, 1980; Maxwell and Howard, 1981; Bock, 1975).

While this method has much to recommend it in the context of the issues described above, its utility seems to be most relevant in the multivariate context where controlling for the lagged endogenous variable has causal implications (Liker et al., 1985). For purposes of the descriptive analysis conducted in this section we will rely on the formulation presented in equation (1) and return to the latter approach when we estimate multivariate models.[64]

4.2.2.2 Analyses with Effects For Sample Membership

Next, we expand the naive repeated measures models above to include effects for treatment samples. The model is specified by

$$y_{it} = \tau + y_{it-1} + \delta x_i + \ldots + \delta_{k-1} x_{k-1i} + \varepsilon_i^{*} \qquad (4)$$

where k-1 = the number of treatment sample dummy variables (given that k is the number of samples within the state). In this context, the τ parameter estimate is not so useful since its

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is used instead of the lagged endogenous variable itself, can be used to provide biased, but consistent estimates of the stability coefficient (Markus, 1980).

[64]We did estimate simple stability coefficient models for supervision intensity and positive adjustment scores in each state via the method of ordinary least squares which yields biased but consistent results in the absence of serial correlation. In the presence of serial correlation, the consistency property is lost as well (Pindyck and Rubinfeld, 1991). The method of instrumental variables is used to correct for this when good instrumental variables can be constructed. Due to our inability to secure good instruments for these variables, however, we proceeded with OLS estimation. The estimates in these models revealed considerable evidence of stability over time and these estimates usually placed in the range of 0.4 to 0.8 for both sets of scores in each state and were virtually always statistically significant at any reasonable alpha error level. Estimates in this range indicate that (1) in most cases, at least half of the variation in the latter score is explained by a time-stable component; and (2) subjects who started at relatively high levels of supervision intensity and positive adjustment tended to maintain a relatively high score on these variables.

interpretation is not the same.[65] It now represents the average change over the interval of interest when all predictors are (at least in the hypothetical sense) set to zero and δ is a 1 x k-1 vector of treatment effects on supervision intensity or positive adjustment at the end of the interval controlling for the initial scores.[66] In this assessment, our use for the τ parameter estimate is much more limited. Put bluntly, in the words of Huck and McLean (1975), the results of analysis of this parameter in an explanatory context are "worthless" (p. 515). They continue:

> [t]o note that the entire group of subjects, averaged across treatment groups, significantly increased (or decreased their performance ... does not necessarily reveal anything at all about the treatments. The phenomena of testing, history, maturation, regression, and so on are all potentially confounded with the average influence of the various treatments, and thus it is impossible to know what causal factor was responsible for the change (or lack of change) between ... trials" (pp. 515-516).

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[65]A comparison of the estimate of τ to its standard error with the t-distribution, in this context, no longer yields the same p-values as the F-test for a within-subjects change (time) main effect in the repeated measures analysis of variance framework and it no longer estimates the mean of the change vector.

[66]These effects which appear in this model as main effects are actually equivalent to the time by predictor variable, say X_n, interaction terms that are routinely estimated in repeated measures analysis of variance models. In a pre-test/post-test design, this effect measures the change in the difference scores across the two measurement units as a function of scores on X_n (usually type of treatment). In a design such as this where there is no pre-test and there are multiple post-tests, it simply indicates whether there was an effect of X_n on the change in scores across times. Equivalently, this is a test for whether the effect of X_n on the dependent variable is constant over measurements. Put simply, the question is whether the observed effects are time-stable.

The between-subject effects, in the context of change score analysis simply reflect the average impact of X_n on the dependent variable (not the change score) over time. In the pre-test/post-test paradigm, this test is of little use because interest is usually centered on whether some factor induces differential change in scores over time. In the multiple post-test study, the absence of a time by X_n interaction effect implies that tests for overall between-subjects effects will yield approximately the same results as the between-subjects tests at each of the individual test points.

This is an important issue because the addition of factors and covariates to naive models often transforms what was a significant estimate of over-time change into a nonsignificant effect. What substantive importance might such a change have? The most plausible interpretation is that the patterns were not strong enough to withstand the segmentation of the study population into groups defined by the study factors. That is, they were weak trends at the outset and they became substantively unimportant when partialed with respect to other factors (Winer, 1971).[67] In some, although not all, of our data this phenomenon is evident. Where it occurs, we are inclined to ascribe less importance to the effect of unobserved variables that create a pattern of trend in the data. Where it does not occur we have not acquired any new information.

When significant interaction effects between time and sample occur,

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[67]Unfortunately, this is difficult to visualize because one of the two fundamental assumptions of the usual repeated measures analysis of variance model is that there be a common covariance matrix across the between-subject effects (Littell et al., 1991). The repeated measures analysis of variance test, as noted earlier, is primarily comprised of a regression of difference scores onto a vector X. When there is little variation in the dependent variable (i.e., little change over time), the introduction of a large number of variables will tend to decrease the relative efficiency of all of the parameter estimates. *Ceteris paribus*, statistically significant results are more probable when the variables of interest are more widely dispersed. We do not submit this to suggest that null effects for time are merely an artifact of reduced power. We do submit that if the over-time decline were sufficiently pronounced, this modest attack on statistical power would not convert a previously significant result into a null result.

There are two possible responses to this issue. One is to evaluate the type I, or incremental sum of squares (Draper and Smith, 1981, pp. 97-98), for the time effect. The other is to simply take as given that there is a weak to moderate decline pattern in the sample. Under this approach we would use changes in significance levels that accompany the use of partial sums of squares (controls for covariates) as evidence to support the claim. Since, our earlier analyses of supervision intensity will have already considered the univariate effect of within-subject change over time (the equivalent of examining the incremental sums of squares) we shall take the migration of a statistically significant F-test for time in the univariate context to a null F-test for time in the multivariate context as evidence of a weak effect for time. Since our principal concern is to explain changes in positive adjustment and main effects of time offer no substantively useful insights into this problem, no complications are introduced by pursuing this route.

however, new information has been acquired. The form of eq. (4) can provide insights into the structure of the interaction effect. As we shall see in the coming section there is little evidence of differential sample effects on either supervision intensity or positive adjustment over time. The exception to this will be in Louisiana where the shock graduate sample's decline on both supervision intensity and positive adjustment is significantly greater than what is observed in the other states.

To help put this type of interaction effect into perspective, we state a special case of eq. (4) and explicitly estimate the regression function:

$$y_{it} = \tau + y_{it-1} + \delta x_i + \varepsilon_{it}^{*} \qquad (5)$$

where x_{ii} = a dummy variable that is equal to 1 for the group of subjects in the shock sample and the coefficient for y_{it} is restricted to 1.0 and thus drops out of the equation. Thus the parameter vector for this function, β, is comprised of two elements:

$$\beta = [\ \tau \quad \delta] \qquad (6).$$

Both elements of β are key: (1) τ is the estimated change due time that applies to subjects in all groups; and (2) δ is the increase or decrease in Y_{i2} that can be attributed to membership in the shock sample over the interval. The significance test for the point estimate of change in the shock sample compared to all other subjects is the ratio of δ to its standard error. The two-tailed test is distributed as Student's t with n-2 degrees of freedom. We refer to the sum of the elements in β as scalar Δ' which denotes the estimated change for subjects in the shock sample (Δ_{shock}) and the estimated change for subjects in another sample (Δ_{other}).

4.2.2.3 Analyses With Effects For Exit Cohorts

In this section of the analysis we return to the exit cohorts presented in section 3. By analyzing levels and changes in supervision intensity across these cohorts we can respond to two important issues: (1) whether different exit cohorts experience different patterns of supervision intensity; and (2) whether, in fact, there is basis for the assumption that an early exit has negative consequences for positive adjustment. As analysis in section 3 revealed, treatment samples were not equally represented across exit cohorts in Florida, Georgia, and Louisiana. To the extent that this preliminary analysis reveals a relationship in reveals a relationship between exit cohort membership and either supervision intensity or positive adjustment, it will be necessary to adjust models of these variables for exit cohort membership in analyses presented later in the paper.

4.2.2.4 Analyses To Assess Proper Specification and Functional Form

4.2.2.4.1 Causal Ordering

Using cross-sectional measures of positive adjustment and supervision intensity, we attempt to identify the correct functional form of the relationship between these two variables. Our efforts here are centered on developing a reasonable specification of the process by which supervision intensity is expected to influence or impact positive adjustment.

This is not a trivial matter. Indeed, it amounts to imposing an important assumption on the data. We note here that it is certainly possible for supervision intensity and positive adjustment to have reciprocal effects on each other. This is a difficult analysis issue with these data since supervision intensity is, at best, only partially stochastic. That is, under ordinary circumstances where the two variables would influence each other, an analysis that takes simultaneity into account would be appropriate. It is questionable whether a set of sufficiently "ordinary" requirements exists. There are a myriad of legislatively imposed requirements at the macro level and judicially imposed requirements at the micro level that drive supervision intensity at least as much as positive adjustment does. The kaleidoscope of possibilities creates inherent omitted variable bias in any attempted specification of a simultaneous process with the data available for this analysis.

Thus, supervision intensity in many states behaves more like a manipulated variable than one which could be modeled simultaneously with positive adjustment. Since the behavior of this variable emulates a manipulated condition more than it emulates being a stochastic player with positive adjustment, it seems reasonable to cast analytical models of the process in a recursive sequence. Such a sequence would place exogenous variables on the far left hand side of the model and allow for them to influence supervision intensity which is cast as an intervening endogenous variable.[68] The exogenous variables and supervision intensity indicators then exert a causal influence on positive adjustment.[69]

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[68]At this juncture of the model, we would expect that variables such as sample membership, type of offense, and criminal history would exert an effect on supervision intensity although these effects would have to be taken as purely descriptive rather than causal for the reasons already indicated.

[69]We believe that when such a framework is transferred to the individual time points it is consistent with reasonable temporal ordering of the processes involved. First, positive adjustment data are essentially the supervising officer's evaluation of offender behavior at the end of *k*th three month follow-up period

4.2.2.4.2 Assessment of Relationship Between Longitudinal Changes In Positive Adjustment and Supervision Intensity

Another important issue that we address in this analysis is that of cross-sectional (sample-wide) effects compared to within-subject effects. It is possible for supervision intensity to impact positive adjustment in two dimensions. First, the joint distribution of positive adjustment and supervision intensity can be modeled in the population at any given point in time as:

$$y_{it} = \alpha + \beta x_{it} + \varepsilon_{it} \qquad (7)$$

where X_{it} = the supervision intensity level at time *t* and y = the positive adjustment score at time *t*. Next, it is possible for changes in positive adjustment within subjects to be related to changes in supervision intensity within subjects. We can represent this process as a special case of eq. (5) above:

$$y_{it} = \alpha + y_{it-1} + \gamma(x_{it} - x_{it-1}) + \varepsilon_{it} \qquad (8)$$

where α is an intercept term, the coefficient for y_{it-1} is restricted to unity and γ is a regression coefficient for the difference in supervision intensity over an interval *t*. The parameter estimate, γ, is the primary concern in eq. (8). The significance test for the point estimate is the ratio of γ to its standard error. The two-tailed test is distributed as Student's *t* with *n*-2 degrees of freedom. The estimated effect is the amount of change in y within subject *i* over the interval *t* for a unit increase in the change in x within subject *i* over the same interval (Allison, 1990; Liker et al., 1985; Kessler and Greenberg, 1981).[70] Estimation of this model by the method of ordinary least squares leads to unbiased estimation of the parameter γ.

We hypothesize a priori that within-subject changes in positive adjustment will be at least partially explained by within-subject changes in supervision intensity (indicating a positive relationship between Δx and Δy). Earlier analysis of positive adjustment data in Louisiana indicated that, for shock parolees, declines in supervision intensity were *accompanied* by declines in positive adjustment (MacKenzie et al., 1992). The authors suggest that this result would likely hold in a within-subjects analysis:

(up to *k*=4). Supervision intensity is occurring and is recorded throughout the follow-up period. Since supervision intensity is technically prior to the officer evaluation (which is actually the dependent variable) a recursive model such as the one described above seems to capture a reasonable temporal specification of the relevant events.

[70]A more intuitive representation is: $\Delta y_i = \alpha + \beta(\Delta x_i) + \Delta\varepsilon_i$.

> The performance of all groups declined over time but the decline in performance was greatest for the shock parolees. Although the shock parolees adjusted more positively, this appeared to be a result of the intensity of supervision, not self-directed choice. This significant deterioration in performance of shock parolees over time was most likely the result of a reduction in the intensity of their supervision (MacKenzie et al., 1992, p. 446).

Using the parameter estimates of γ described above within each state, we explicitly test the hypothesis that within-subject changes in positive adjustment are a function of within-subject changes in supervision intensity. For each state we execute tests for contrasts between each of the adjacent time periods and, then, we estimate a test for the differences between initial and ending values on these variables for all subjects who were measured at two or more consecutive points.

4.2.2.4.3 Analysis of Covariance Models of Cross-Sectional Positive Adjustment Including Effects For Supervision Intensity and Sample Membership

Finally, we turn to a slightly elaborated set of models where the relationship between treatment sample and cross-sectional positive adjustment is specified while adjusting for cross-sectional supervision intensity. These models are specified within the general analysis of covariance framework and amount to a regression of positive adjustment onto supervision intensity while allowing for individual treatment sample effects on the criterion:

$$Y = \alpha + \beta_1 x_1 + \delta_k s_k + \ldots + \delta_{k-1} s_{k-1} + \varepsilon \qquad (9)$$

where x_1 is a covariate (say, supervision intensity) and s_k = the kth sample (where k-1 sample dummy variables are included in the model). β and δ are parameters estimated from the data by the method of ordinary least squares (Dunn and Clark, 1987; Draper and Smith, 1981).

One of the areas we explore in detail in this section is the possibility that the effects of supervision intensity on positive adjustment are conditional on sample membership (i.e., a supervision intensity by treatment sample interaction effect). We assess these effects by generalizing eq. (9) to allow for multiplicative terms for sample categories and supervision intensity and we then explicitly test the significance of the parameter estimates. This process helps us to reach some tentative conclusions about the impact of shock incarceration and supervision intensity on positive adjustment to community supervision.

4.3 *A Priori Fixed-Effect Group Differences*

4.3.1 *Overview*

In the ideal case, offenders would be selected so that they were similar in all respects except for the "treatment" they received. Analysis of offender characteristics across the samples in each state, however, revealed noteworthy differences in offender characteristics in addition to the treatments they received. In this section, we explore the basic structure of each state's fixed effects and describe the by-sample differences that emerge.

At the outset, we note that there are some important differences between the states themselves. The programs in New York and Louisiana have strong rehabilitative and treatment components while the programs in Florida, Georgia, and South Carolina tend to emphasize work, drill, and physical training.[71] The program in Florida is comprised of a large proportion of first-time offenders while many offenders in the other programs have either a prior arrest or conviction. In-program attrition rates are highest in Florida followed by Louisiana and New York. Attrition rates in the Georgia and South Carolina shock incarceration programs are relatively low. A more complete overview of the different program characteristics is presented in the administrative summary preceding this document.

4.3.2 *Florida*

Three samples, including shock graduates, shock program dropouts, and prison parolees, were selected for follow-up in Florida. Violent offenders comprised almost half of the prison parolee sample while they represented less than a third of the offenders in the other samples. Drug offenders, on the other hand, were much more prevalent in the shock graduate group than in either the shock dropout group or the prison parolee group. Shock graduates were also more likely to be serving their current sentence for a technical violation than other offenders. A by-sample analysis of offender age suggests that shock dropouts are slightly younger than either shock graduates or prison parolees. The results of these comparisons are presented in detail in Table 12.

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[71]The program in Georgia stands out in particular for the small amount of time that is devoted to non-work activities. Although, as Bowen (1991) notes, this emphasis is shifting toward a stronger focus on education and treatment.

4.3.3 Georgia

The Georgia study followed three groups over a twelve-month period: shock graduates, prison parolees, and probationers. Table 13 conveys our analysis of *a priori* group differences on categorical predictors. The results indicated that shock graduates and prison parolees were more likely to be violent offenders than probationers. Shock graduates were less likely to be drug offenders than probationers and prison parolees while prison parolees were less likely to be property offenders than other treatment samples. Prison parolees were more likely to be serving time for a technical violation of community supervision (37.8%) than both shock graduates (17.7%) and probationers (1.2%). Not surprisingly, given the above result, prison parolees were more likely to have a prior offense history (75.5%) than shock graduates (39.2%) or probationers (10.6%). Table 13 also reveals that prison parolees were significantly older than offenders in other groups.

4.3.4 Louisiana

In Louisiana, four groups were followed over a one-year period on community supervision. The sample structure included a group of shock graduates, prison parolees, probationers, and shock program dropouts. Table 14 provides by-sample comparisons on the categorical predictors in the study.[72] These results indicate that prison parolees were more likely to be violent offenders than offenders in other groups. Probationers and shock graduates, on the other hand, were both over-represented in the drug-offender category. In general, probationers were more likely than other subjects to be serving a sentence for a new crime. As Table 14 also indicates, prison parolees tended to be older at the beginning of community supervision while shock entrants (both graduates and dropouts) were generally younger at the time of their first arrest.

4.3.5 New York

The New York data were comprised of three offender samples: shock graduates, shock program dropouts, and prison parolees. Table 15 presents our comparisons of categorical predictor distributions across samples. The results indicate that property offenses were more common in the shock sample while drug and other offenses were significantly more prevalent in the prison and dropout samples. Moreover, the prison parolee and dropout samples were significantly more likely than the shock sample to have a prior arrest or conviction in their correctional file. Table 15 reveals that shock

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[72] We note that the shock dropout sample, comprised of only sixteen offenders, is very small and findings associated with this group should be interpreted cautiously.

graduates, as a group, tended to be older at their first arrest than either prison parolees or shock dropouts.

4.3.6 South Carolina

The South Carolina study followed five samples over a one-year follow-up period. The study included samples of DPPPS shock graduates, DOC shock graduates, prison parolees, probationers, and a small group of split-probationers. Positive adjustment data were collected on the DOC shock sample once at the end of the twelve month follow-up period while data were collected in the other samples at three-month intervals throughout the one-year follow-up period.[73]

Tests for pre-treatment sample differences revealed several important findings. First, nonwhites were much more prevalent in the DOC shock sample than in other groups. Offenders in the probation and DOC shock samples were more likely to have committed a new crime (as opposed to a technical violation). Prior offenses were documented for over 90% of the DOC shock sample compared to a rate of 50% to 70% in the other samples. Table 16 presents the results of these tests. In addition, Table 16 indicates that split-probationers tended to be older than other offenders at the beginning of community supervision while the two shock samples and the prison parolee sample were younger, on average, at their first arrest than either the probation or the split-probation samples.

4.3.7 Summary

The results of these descriptive analyses establish the case, at least for the fixed effects in our study, that the study groups with which we are working are nonequivalent on factors that we expect to be related to the dependent variable. In particular, the analysis reveals statistically significant differences in offense distributions by sample in all states except South Carolina. Sample differences with respect to prior record or whether the current sentence was for a technical violation of community supervision or a new crime were also observed in every state. Finally, the results suggest that offenders across samples are not always about the same age. Moreover, in New York, Louisiana, and South Carolina, there were important by-sample differences in age at first arrest.

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[73]Secondary contact information was not available in South Carolina. Primary contact information was collected but was missing on many observations during the first three months. Primary contact data were only collected for the first six months for the DOC shock sample.

As a result of these differences, it will be necessary in our analyses to control for the effects of these potentially confounding variables.[74] Although we recognize that statistical control methodology is inferior to full-fledged case-control studies we also are prepared to be particularly confident of those findings that emerge in the presence of statistical controls when they persist across states.

4.4 Supervision Intensity

One of our principal areas of focus in this report is on supervision intensity. In this section, we examine three issues associated with this variable: (1) its distribution in each of the states where data were collected; (2) whether and how it changes over time; and (3) whether levels of supervision intensity are conditional on treatment sample.[75]

4.4.1 Univariate Distributions

One of our principal concerns early on in the analysis process was the skewness that was evident for supervision intensity measures in each of the states we studied. Particularly striking were the differences in the means and the medians for these distributions. Histograms for these distributions also concerned us. In particular, each distribution was characterized by a large group of cases at the lower end of the distribution with a few extremely large values at the upper end. Since our later analyses rely on regression functions that minimize the sum of the squared deviations between the actual and predicted values of positive adjustment, the presence of such extreme cases on a key variable such as supervision intensity is problematic.

To confront this problem we assessed each supervision intensity measure separately and concluded that it was reasonable to work with a natural log transform of the contact variables collected in

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[74]We note here that only South Carolina's data presented with differences in racial composition by sample. Nonwhites were significantly under-represented (49.4%) in the DPPPS shock sample while they were significantly over-represented in the DOC shock sample (73.8%). In the other samples nonwhites comprised about 60% of the cases.

[75]Again, we note that supervision intensity data were not collected in New York. Nor were they collected over time for the DOC shock sample in South Carolina. Secondary contact information was not available in South Carolina and Georgia. Three indexes, rather than contacts, were used to measure supervision intensity in Louisiana.

Florida, Georgia, and South Carolina. In Louisiana, the supervision intensity indicators were not based directly on contacts. Nevertheless, natural log transformations of the knowledge and surveillance indexes improved the skewness of those distributions as well. Our analysis did not reveal any evidence of a major skewness problem with the requirements index in Louisiana. This index was, therefore, retained in its original metric. Working with the average of the contact variables[76] averaged over all available follow-up periods for each subject, Table 17 presents the effects of transforming these indicators.[77]

Table 17 also provides some basis for comparing Florida, Georgia, and South Carolina on aggregate, or cross-sectional, levels of supervision intensity. The data suggest that offenders in Florida (median primary contacts = 2.25) tend to be contacted more frequently than offenders in Georgia (median = 1.82) and South Carolina (1.50).[78]

4.4.2 Change In Supervision Intensity Over Time

4.4.2.1 Overview

In this section, the analysis focuses on how supervision intensity levels change over the course of the follow-up period. To do this we estimate a set of naive repeated measures analysis of variance models within each state. The emphasis here is to observe whether there is a significant trend in supervision intensity across the entire dataset within each state.

4.4.2.2 Florida

In Florida, our examination of primary contacts over time reveals a general declining trend in supervision intensity over time (Table 18). Repeated measures analysis of variance tests reveal statistically significant effects for time in the six and nine month analyses. The F-test for time is not statistically

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[76]We will refer to this type of averaging of the data in later analyses as a "cross-sectional" analysis. We will be returning to this type of analysis often given subject attrition levels over the follow-up period.

[77]Since a number of cases in each state had mean contact levels that did not exceed 1.0 (and values of zero were observed in some cases) the log transformations used in this paper are calculated by taking: ln(raw score + 1.0).

[78]A comparison of the means is not particularly useful in this context given the positive skewness in the distributions.

significant in the twelve month analysis. The τ estimates provide some insight into the details. They reveal that time 1 to time 2 change in the natural log of primary contacts is negative and statistically significant ($\tau = -.195$; $p < .001$). Change between time 2 and time 3, however, is not statistically significant ($\tau = -.095$; $p < .210$). Time 3 to time 4 change is also not statistically significant ($\tau = -.082$; $p < .433$).

Figure 7 augments this presentation by showing the changes in the natural log transform of primary contact levels at each quarter. There are three legends in Figure 7. The first legend presents primary contact levels at the first and second quarters averaged over all subjects who completed testing at both times. The second legend displays primary contact levels over the first three quarters for all subjects who completed three measurements. The third legend shows contact levels at each of the four quarters for all subjects who completed four quarterly evaluations. For all groups, the largest change occurs between time 1 and time 2 with a "leveling-off" pattern thereafter. In short, the average number of monthly primary contacts during the first three months of the study was about 7.5. By the last three months of the study (for those remaining in the study), this figure declined to an average of about 5.8 monthly contacts.

Data for secondary contacts (contacts with family and employer and treatment officials) were also collected in Florida over time. Our analysis of these data, presented in Table 19, reveals significant over-time decline in these contact levels in each context. Figure 8 graphs the decay over the follow-up period for those who complete six, nine, and twelve months of testing, respectively. Again, the τ estimates are illuminating. Between time 1 and time 2 there was a statistically significant decline in the log-values of secondary contacts ($\tau = -.134$; $p < .002$). Neither changes between time 2 and time 3 ($\tau = -.066$; $p < .301$) nor between time 3 and time 4 ($\tau = -.114$; $p < .238$) were statistically significant.

The assessment is more clear with untransformed data. During the first three months of the follow-up period, offenders' family/employer/treatment associates were contacted an average of 3.1 times per month. By the end of the study period (final three months), average monthly secondary contacts declined to about 2.4. As with primary contacts, the declines for secondary contacts were statistically significant between times 1 and 2 but not thereafter.

4.4.2.3 Georgia

In each of the three measurement contexts for over-time changes, analysis of the log transform of primary contact levels as they evolve over the study reveals a pattern of decay. Table 20 indicates that repeated measures F-tests for a time effect are statistically significant in each case. The data are plotted in

Figure 9 and also suggest over-time decline in supervision intensity levels. Estimated τ values support these results. Change at each of the intervals was negative and statistically significant although the change at each interval was less than the change at the previous interval. Thus declines became more diffuse over the course of the follow-up period (time 1 to time 2 $\tau = -.204$, $p < .001$; time 2 to time 3 $\tau = -.146$, $p < .013$; time 3 to time 4 $\tau = -.119$, $p < .014$). Examination of the untransformed data reveals a total decrease of about 1 contact per month over the one year follow-up period. At the beginning of the study, offenders were contacted about 3.1 times per month on average. By the end of the follow-up, this level had dropped to about 2.2.

4.4.2.4 Louisiana

In Louisiana, we are concerned with the amount of change in the three supervision intensity index scores over time. Repeated measures analysis of variance tests for a time effect on the log transform of knowledge index scores reveal nonsignificant differences for each interval. Table 21 presents these analyses which are supported by the τ estimates and Figure 10.[79] The data do not provide strong support for significant changes in knowledge scores over the follow-up period.[80]

Table 22 presents the repeated measures analysis of variance tests for a trend in the log transform of surveillance scores. While the data provide little support for significant change in scores between times 1 and 2, change was strongly evident thereafter. The F-test for the T1-T2 difference was not statistically significant. The τ estimate for the T1-T2 contrast was also nonsignificant ($\tau = -.024$; $p < .288$). Later contrasts, however, revealed pronounced declines in these scores (Figure 11). The F-tests for time effects at nine and twelve months were statistically significant as were the τ estimates for the T2-T3 and T3-T4 contrasts (T2-T3 $\tau = -.135$, $p < .001$; T3-T4 $\tau = -.080$, $p < .002$). Raw surveillance scores also corroborate the trend. At the beginning of the follow-up, the average score was 0.87. This figure dropped to 0.37 by the end of one year of follow-up. After early stability, the evidence suggests a significant decline in surveillance activity after the six month follow-up point.

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[79]The estimated τ values were: T1-T2 $\tau = -.04$, $p < .184$; T2-T3 $\tau = +.018$, $p < .534$; T3-T4 $\tau = .028$, $p < .355$.

[80]The untransformed data suggest strong stability. The average monthly knowledge scores at time 1 was 1.24. At the end of the twelve month follow-up, the average score had increased to about 1.32. Since this is an increase of lack of knowledge, the data still imply decreasing knowledge of offender activities over time but the declines are not statistically significant.

The results of requirements index analysis in Table 23 follow the same pattern. Scores are stable at the first interval and decline at subsequent intervals. Figure 12, along with the τ estimates, confirm these trends.[81] Raw requirements scores averaged about 2.87 at the beginning of the follow-up period and declined to about 2.39 by the end of the study. In Louisiana, the results suggest that while knowledge levels are quite stable over the study period, surveillance and requirements levels both decline after the six month point.[82]

4.4.2.5 South Carolina

Analysis of supervision intensity over time in South Carolina is restricted to primary contact data and excludes the DOC shock sample. There is evidence of an over-time decline in primary contact levels. Repeated measures tests for a time effect (see Table 24) yield statistically significant results at times 2, 3, and 4. Figure 13 displays the trend which suggests a relatively uniform decline in primary contact levels over time. Estimated τ values at each interval indicate the presence of significant negative change.[83] However, the change is not as marked at the first interval as it is in later intervals. The data indicate that offenders were contacted about 2.4 times per month on average during the first three months. During the last three months offenders were contacted about 1.3 times per month. Consistent with what was observed in Louisiana, there is a pattern of early stability followed by an unmistakable pattern of decay.

4.4.2.6 Summary

The results of this analysis of trends in supervision intensity are reasonably clear. In none of the states was a pattern of increasing supervision intensity over time in evidence. In every state, supervision intensity was lower at the end of the twelve month period than at the beginning. The patterns of this decay

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[81]For the T1-T2 contrast, the τ estimate was not significantly different from zero (τ = -.037; p < .490). Both of the other contrasts, however, yielded negative τ values which were statistically significant (T2-T3 τ = -.269, p < .001; T3-T4 τ = -.329, p < .001).

[82]Perhaps levels of knowledge of offender activities, once established, are more easily maintained than surveillance (which is labor-intensive for the officer) and requirements (which are labor-intensive for the offender and officer).

[83]Estimated τ values were: T1-T2 τ = -.075; p < .001; T2-T3 τ = -.162, p < .001; T3-T4 τ = -.158, p < .001).

assumed different forms in different states, however. In Florida and Georgia, the steepest declines in supervision intensity occurred early in the follow-up period followed by more stable contact levels in later months. In Louisiana and South Carolina, supervision intensity tended to be stable at the beginning of the study period and declined after the six month point. Still, the overarching pattern is one of declining, not increasing, supervision intensity over the follow-up period.

4.4.3 Supervision Intensity By Sample

4.4.3.1 Overview

We begin this analysis by averaging contact information over the entire period that each subject was followed.[84] Thus, for an offender who was followed for two quarters we take the average supervision intensity over the two quarters and for an offender who was followed for four quarters we take the average number of contacts over all four quarters. Since such a step imposes a cross-sectional framework onto these longitudinal data, we refer to these aggregated values as "cross-sectional" scores.

Next, we compare these average cross-sectional scores by treatment sample. Our objective here is to determine whether cross-sectional supervision intensity can be predicted with better than chance accuracy from knowledge of treatment sample. A finding of significant differences across samples indicates further nonequivalence of the treatment samples that would have to be controlled in a statistical analysis of positive adjustment.

Finally, a repeated measures analysis of variance model is developed that specifies the over-time distribution of supervision intensity scores as a function of within-subject change, sample membership, and an interaction of the two terms. This analysis is designed to help us assess whether longitudinal supervision intensity patterns vary by treatment sample.

4.4.3.2 Florida

A test of the hypothesis that overall supervision intensity is equal across samples reveals that, in fact, the samples are different. Table 25 indicates that shock offenders are supervised less intensively than their prison parolee and shock dropout counterparts. Figure 14 portrays this result.

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[84]Again, we are working with the natural log transform of contact values and, in Louisiana, the knowledge and surveillance scores.

Repeated measures analysis of variance results generate themes that are consistent with what has already been noted. During the first nine months, significant changes in supervision intensity are evident over time and the shock sample is generally supervised less intensively than either the prison parolee or dropout groups. The largest decreases occurred between the first and second follow-up points. Among subjects who were followed for an entire year, neither a time nor a sample effect was evident. Table 26 presents the repeated measures analysis for primary contacts and Table 27 depicts the results for secondary contacts. In none of the comparisons was a treatment sample x time interaction effect evident. Thus, in Florida, we retain the null hypothesis that changes in contact levels over time are consistent across treatment samples.

4.4.3.3 Georgia

Based on results presented in Table 28, the null hypothesis of equal levels of cross-sectional contacts is rejected. The analysis indicates that probationers and parolees were supervised at significantly different levels while shock graduates were supervised at about the average level. Duncan post-hoc tests indicate that average log contact levels for the shock group were not significantly different from those of either of the other two groups.

Throughout the follow-up period, there is a consistent decline in supervision intensity and this decline is relatively stable across samples. Table 29 presents the results of the repeated measures analysis of variance tests.[85] The hypothesis tests suggest the presence of a stable between-group difference in level of contacts. As with the overall test described above, the shock sample occupies the middle position of the three groups with respect to contact levels.

4.4.3.4 Louisiana

Analysis of the cross-sectional knowledge, requirements, and surveillance indexes in Louisiana reveal strong between-group differences in supervision intensity. The important contrast in each case is the difference between shock offenders and other groups. In each comparison, the Duncan post-hoc tests indicate that shock offenders are supervised at significantly higher levels than the other groups (Table 30).

Over time, there is little evidence of a significant decrease in

[85]The time x sample interaction term was nonsignificant for each test but the main effects for time and sample were both significantly different from zero at each follow-up.

knowledge (Table 31). This is consistent with the results presented in the univariate analysis. Although there is some evidence that the shock and prison mean knowledge scores approach each other over time, the evidence is not strong enough to achieve statistical significance. We thus conclude that shock offenders have stronger knowledge scores than other groups and that the difference is time-stable. Figure 16 compares the overall knowledge scores for each of the samples.

Surveillance index scores are not as time-stable (see Table 32 and Figure 17). There is strong evidence of an over-time decline in surveillance levels after the six-month follow-up point (time effects are significant at nine and twelve months). There is also continued evidence that the shock sample is supervised more intensively than other samples (between-subject effects are consistently significant). Although the analysis suggests that by the end of the study, the shock sample is still supervised more intensively than other groups, the difference between the groups is not nearly as great as at the beginning of the study (time x sample effects were statistically significant at quarters 2, 3, and 4).

The statistical significance of these interaction effects require special attention. Our analysis suggests that decline is not uniformly distributed across the sample categories. At each of the intervals, surveillance of the shock sample declines at a greater rate than in the other groups. Returning to the model developed in eqs. (5) and (6), we estimate Δ values for the shock sample compared to the other groups. The results indicate that at the six month point, the shock sample's trajectory was significantly different than that of subjects in the other groups ($\Delta_{shock} = -.116$, $\Delta_{other} = .012$; $p < .010$). This pattern continued at month 9 ($\Delta_{shock} = -1.367$, $\Delta_{other} = -.329$; $p < .001$) and at month 12 ($\Delta_{shock} = -.255$, $\Delta_{other} = -.021$; $p < .001$).

As in the analysis of the knowledge index, the scores for the shock sample approach those of the prison sample more closely than the other groups. For this analysis, it seems reasonable to conclude that the shock sample has higher scores, that there is a general downward trend in those scores for all samples after six months, and that scores decay at a greater rate in the shock sample than in other groups.

Our analysis of the requirements index leads to similar conclusions. The results indicate again that there is over-time score decay at nine and twelve months and that decay is greatest for the shock sample.[86] The estimates of Δ confirm these

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[86]The between-subjects F-test was statistically significant for each analysis. Significant within-subject (time) effects were evident in the nine and twelve month analyses. Statistically

observations at six months where the shock sample did not differ from the other groups ($\Delta_{shock} = -.111$, $\Delta_{other} = -.008$; $p < .385$). As indicated by the significant time x sample interaction effects, however, the shock group did differ from the other groups at nine months ($\Delta_{shock} = -.730$, $\Delta_{other} = -.109$; $p < .001$) and at twelve months ($\Delta_{shock} = -.704$, $\Delta_{other} = -.202$; $p < .002$).

Table 33 along with Figure 18 suggest, however, that there is no single group toward which the shock sample moves. In the analyses of the knowledge and surveillance indexes the shock sample was on a convergent path with the prison sample but the other samples are not clearly distinguishable from each other in this analysis. Thus, there is a more general convergence toward common requirements levels among the samples than was apparent with the knowledge and surveillance indexes.

4.4.3.5 South Carolina

Analysis of cross-sectional log primary contact levels by sample in South Carolina yields a statistically significant F-test. Duncan post-hoc tests indicate that contacts are greatest for the DPPPS shock sample and the prison sample although the scores for these groups are only significantly different from those of the probation sample (Table 34). Supervision intensity levels for the DOC shock sample along with the split-probationers place these groups squarely in the middle of the distribution and they are not significantly different from any of the other samples (Figure 19).

Repeated measures analysis of variance tests tend to support the analysis of the aggregated primary contact variable above (Table 35).[87] The probation sample along with the split-probation sample tend to be supervised less intensively while the prison and shock groups are supervised more intensively.[88] There is a general decline in supervision intensity among all groups although the decline is more pronounced at months nine and twelve than at month six. Table 35 also indicates that there is a weak pattern of convergence in primary contact levels among the groups over time although the convergence is not statistically significant. In the

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significant time x sample effects were also evident beyond the six-month point.

[87]Note again that the DOC shock sample will be absent from this analysis.

[88]Between-subjects (sample) effects were statistically significant in each analysis. Within-subjects (time) effects were also statistically significant for each of the analyses and time x sample effects were not statistically significant.

way of formal conclusions, we surmise from these data that there is general decline in supervision intensity over time and that the rate of decline is approximately the same across samples.

4.4.4 Supervision Intensity and Exit Cohorts

4.4.4.1 Overview

As noted earlier, exit cohorts are defined according to the point in the follow-up period where subjects exited the study. The variable ranges from zero (for those who were not measured on positive adjustment at the first quarter) to four (for those who were measured at each quarter). In a manner similar to the previous analysis where the relationship between sample membership and supervision intensity was examined, this sub-section focuses on the relationship between exit cohort membership and supervision intensity. We examine each state in turn. Within each state we assess whether timing of attrition from the study was associated with patterns of cross-sectional supervision intensity.

4.4.4.2 Results

Cross-sectional analysis of both primary and secondary contact levels by exit cohorts revealed that all exit cohorts were supervised at approximately the same levels within each of the states.[89] Tables 36-39 present the single factor analysis of variance results in Florida, Georgia, Louisiana, and South Carolina, respectively. In sum, it seems reasonable to conclude that although samples are differentially represented in the exit cohorts, the exit cohorts are not comprised of differentially supervised offenders. Thus, at least in the cross-sectional context, supervision intensity and exit cohort membership are independent of each other.

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[89]In short, the null hypothesis of equal cohort means could not be rejected in any of the four states with supervision intensity data.

4.4.5 Summary of Preliminary Supervision Intensity Analysis

Our analysis in this section focused on several key questions. First, we examined univariate distributions of the supervision intensity indicators. Given the positively skewed distributions associated with most of these indicators, we concluded that there were reasonable grounds for working with a natural log transform of those indicators.[90]

Next, we considered the overall averages in supervision intensity and concluded that offenders in Florida tended to be supervised at higher levels, on average, than offenders in Georgia and South Carolina.[91] The median monthly primary contacts level for Florida offenders was 2.25. This compared to values of 1.82 and 1.50 in Georgia and South Carolina, respectively.[92]

Analysis of the movement of supervision intensity over time revealed evidence of longitudinal decay in contact levels in Florida and Georgia over the first two follow-up periods. Changes in supervision intensity levels tended not to decline as uniformly at later follow-up periods in these states. In Louisiana and South Carolina declines in supervision intensity were also evident but they did not become statistically significant until after six months.[93]

We found considerable evidence of variation in supervision intensity by sample. This result has important implications for our subsequent analyses. Since we hypothesize that supervision intensity and sample will both be related to positive adjustment, it will be important to control for their shared variance. In Florida, the analysis revealed that shock graduates were supervised less intensively than the other groups. In Georgia, shock graduates did not differ from either the probationers or the prison parolees but probationers and prison parolees did differ from each other. In Louisiana, there was a large difference in supervision intensity levels by sample. The results of our analyses indicated

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[90]In Louisiana, of course, we concluded that the requirements index did not need to be transformed since it was not nearly as positively skewed as other indicators in Louisiana or indicators in other states.

[91]We could not include Louisiana in this comparison because the supervision intensity indicators are not the same.

[92]Because of the positive skewness in the distributions, the means for the states provided a biased comparison of the differences.

[93]Knowledge of offender activities in Louisiana did not change significantly over time in any of the analyses that we conducted.

that shock offenders were supervised at significantly greater levels than offenders in the comparison groups. In South Carolina, probationers tended to be supervised at the lowest levels while DPPPS shock graduates and prison parolees were supervised at significantly higher levels. DOC shock graduates and split-probationers did not differ from any of the other groups. Repeated measures analysis of variance tests for the over-time stability of these patterns suggested little in the way of time-by-sample interaction effects.[94]

We also tested the hypothesis that exit cohort membership was independent of supervision intensity. The results of these analyses were entirely consistent across states. Supervision intensity did not vary by the timing of exit from the study. While this result has little substantive importance, it does suggest that the main complication of exit timing is associated with treatment sample. We will return to this issue again in the next section as we analyze positive adjustment scores.

In general, we conclude from this analysis that supervision intensity generally declines over time. The data we presented in this section suggest, however, that these declining patterns while evident are not particularly strong. In some follow-up periods declines were more evident than in others (patterns varied by state) and plots of the mean contact levels did not suggest particularly steep descents although downward trends were evident.

Moreover, we suggest that although particular levels of supervision intensity can be partially explained by membership in a particular sample within states, the downward shift in those levels over time is not attributable to membership in a particular sample. Finally, our analysis reveals that supervision intensity is not related in a meaningful way to membership in a particular exit cohort nor are changes in supervision intensity over time conditioned by cohort membership.

[94]A notable exception here is in Louisiana. The data suggest that over-time declines in requirements and surveillance were generally a little sharper for the shock sample than for the other groups. Still, the analysis revealed evidence for at least a weak declining pattern in the other samples as well.

4.5 *Positive Adjustment To Community Supervision*

4.5.1 *Overview*

The dependent variable in our analysis is positive adjustment to community supervision. In this section of the report, we present the univariate distributions for the positive adjustment construct in each state and describe how these distributions change over time. We then begin the process of explaining the variance in these distributions with three key predictor variables in our data.

The structure of this section roughly parallels what was presented in the supervision intensity analysis. We begin the inquiry by simply examining the cross-sectional and longitudinal distributions of positive adjustment in each state. Naive models estimating the τ parameter are presented followed by a description of cross-sectional and longitudinal (repeated measures) sample differences.

Next, we assess whether exit cohort membership is independent of cross-sectional positive adjustment levels as it was with supervision intensity. Our analysis will reveal that, in fact, timing of exit has statistically and substantively significant impacts on positive adjustment that will need to be controlled in a properly specified positive adjustment model.

The relationship between positive adjustment and our supervision intensity indicators is assessed in the next section. In this analysis, we pay particularly close attention to the functional form of the relationship in each state. We then examine the possibility that sample membership and supervision intensity interact with each other in predicting positive adjustment scores. Finally, we present a series of bivariate analyses that reveal the strength of association between each of our predictors and positive adjustment within each state.

4.5.2 *Univariate Positive Adjustment Distributions*

In this section, we present descriptive summaries of the positive adjustment construct in each state. The analysis is principally concerned with two different dimensions of the construct. First, we average all available positive adjustment scores ($k \leq 4$) for each subject to create cross-sectional positive adjustment scores.[95]

The second dimension with which we are concerned is the longitudinal distribution of the positive adjustment scores. Table

[95]This is analogous to the way we aggregated our supervision intensity indicators over time for each subject.

40 presents descriptive statistics for both dimensions within each state. Perhaps the most striking conclusion we can draw from this table is that the results are quite stable across states. Cross-sectional and longitudinal scores are highest in New York and lowest in Florida and Georgia. Louisiana and South Carolina tend to occupy the middle position throughout the one-year follow-up period. Although the standard deviations in Louisiana are significantly smaller than in other states, the state means tend to stay within about 0.20 points of each other in every comparison.

Figures 20-24 present cross-sectional positive adjustment histograms for Florida, Georgia, Louisiana, New York, and South Carolina, respectively. These histograms do not provide the same sense of uniformity that is implied by the descriptive statistics in Table 40 but they all suggest reasonable dispersion of cases along the entire length of the scale in every state except Louisiana. The histograms in Louisiana and Georgia stand out as being more different than the others. In both of these states, positive adjustment scores tend to cluster more closely at the mean. In Louisiana, the scores are never higher than about 0.8.[96] These results are consistent with the smaller standard deviations in these two states. In all states, there are significant clusters of cases with zero values and several spikes with relatively large numbers of cases at several points along the continuum. In short, these distributions seem to provide a reasonable target for our analysis efforts in this report.

4.5.3 Univariate Positive Adjustment Over Time

4.5.3.1 Overview

In this section, we estimate a series of repeated measures analysis of variance models along with the τ parameter to assess the within-subject change on the positive adjustment construct over the course of the follow-up period.

4.5.3.2 Florida

Results in Florida (Table 41) suggest that positive adjustment tends to decline over time. The analysis revealed that declines were statistically significant beyond the $p < .10$ level at each follow-up. Analysis of τ estimates, however, reveals some

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[96]Although, we note that maximum scores rounded off to about 0.9 in all but the final time period. In that period, the maximum score was about 0.78. The instrument in Louisiana, the reader will recall, is different than the one used in the other states and positive adjustment data were collected every month compared to every three months in other states.

variation. Between months 3 and 6, subjects declined by about .04 points on average ($\tau = -.044$; $p < .015$). During the 6 to 9 month period, the decline was more pronounced ($\tau = -.071$; $p < .002$). At the final follow-up, the change parameter decreased and was no longer statistically significant at the $\alpha = .05$ level ($\tau = -.06$; $p < .093$). Figure 25 plots the means which reveal a general declining pattern, although, as the τ parameter estimates suggest, the declines (relative to the means) are not particularly large.

4.5.3.3 Georgia

The repeated measures analysis presented in Table 42 conveys a less conclusive set of results. If the means are to be taken as an indicator, there is little variation in positive adjustment over time. Figure 26 tends to reinforce this view. Average within-subject changes also tend to support this conclusion as measured by the F-tests for within-subject effects and the change parameter estimates. Between months 3 and 6, the change estimate is negative but not statistically significant ($\tau = -.041$; $p < .171$). Between months six and nine, the change estimate is still negative but only marginally significant ($\tau = -.042$; $p < .077$). During the final three months, the analysis reveals even less evidence of change ($\tau = -.037$; $p < .232$). In Georgia, it is clear that the means are not increasing over time but the finding of very small levels of within-subject change over time seems to suggest that subjects are not adjusting significantly worse over time either. In short, there is virtually no evidence for significant over-time change in positive adjustment scores in Georgia.

4.5.3.4 Louisiana

The over-time distribution of positive adjustment scores in Louisiana suggests a stronger pattern of decline than what was observed in Georgia but not unlike what was observed in Florida. Repeated measures tests in Table 43 provide evidence for rejecting the null hypothesis (no within-subject change) for each analysis. Figure 27 plots the means. An examination of the means in both Table 43 and Figure 27, however, reveals that the declines, while apparently quite widespread among subjects, is not particularly dramatic. The τ estimates support this conclusion. During the month 3-month 6 interval, there was about a .04 point decline on average ($\tau = -.036$; $p < .001$). By month 9, the change over the previous three months was slightly greater but still not large by any standard ($\tau = -.052$; $p < .001$). At month twelve, the change estimate for the previous three months had decreased slightly again ($\tau = -.047$; $p < .001$). While these results clearly support the idea of widespread decline in positive adjustment scores it would be misleading to suggest that these declines were very large. Indeed, these average declines never amount to much more than 10% of the sample mean.

4.5.3.5 *New York*

Positive adjustment scores in New York tend to suggest a declining pattern but it is also weak and erratic. Table 44 presents the repeated measures analysis of variance results and Figure 28 plots the means. A perusal of Figure 28 suggests that the largest decrease occurs between months six and nine. The change estimates support this conclusion. At month six, the three month change estimate was negative but weak and nonsignificant ($\tau = -.026$; $p < .132$). At month nine, consistent with Figure 28, the change is more definitive ($\tau = -.062$; $p < .001$). By month twelve, positive adjustment score decay was still evident and statistically significant but much closer to zero ($\tau = -.029$; $p < .014$). As in the other states, there appears to be evidence of a decline, it is widespread enough to be statistically significant, and it is not particularly large.

4.5.3.6 *South Carolina*

The weak case for within-subject decay in South Carolina positive adjustment scores is rivaled only by Georgia. Table 45 presents the repeated measures tests and Figure 29 depicts the means. Decay between months 3 and 6 was evident but also weak and nonsignificant ($\tau = -.035$; $p < .087$). This lack of within-subject change was even more in evidence at month 9 ($\tau = -.024$; $p < .205$) and at month 12 ($\tau = -.031$; $p < .181$). There seems to be little evidence for solid inferences about declines in positive adjustment in South Carolina.

4.5.3.7 *Summary*

The evidence from this analysis of within-subject change with respect to positive adjustment to community supervision is that scores do not tend to increase over time. Indeed, in every state, the change parameters estimated from the data were negative in direction suggesting that the dominant pattern is one of decreasing scores. Particularly noteworthy, however, is the small magnitude of the change parameter estimates. In only two instances did these values imply a one period decrease of greater than .05 points that was statistically significant. The data in Georgia and South Carolina were noteworthy for their marked absence of statistically significant patterns. In Florida and New York, the patterns of change were erratic at best. Only in Louisiana were declines statistically significant at each interval. Based on the results of this section, we conclude that positive adjustment scores are more likely to decrease than increase over time but the expected magnitudes of these decreases are relatively small (especially compared to the mean scores presented in the previous section).

4.5.4 *Positive Adjustment Distributions By Sample*

4.5.4.1 *Overview*

In this section, we consider whether positive adjustment scores vary by treatment sample. We first consider the differences in cross-sectional positive adjustment scores across treatment samples. We then turn to an over-time assessment (repeated measures analysis) of the relationship between sample and positive adjustment. While the results of this analysis have implications for our conclusions, the fact that our study groups are nonequivalent on predictors that are related to positive adjustment renders it preliminary in scope.

4.5.4.2 *Florida*

In Florida, cross-sectional positive adjustment varies significantly by treatment sample. Table 46 presents the single-factor analysis of variance results. The F-test is statistically significant at the $\alpha=.05$ level and Duncan post-hoc tests indicate that the shock graduates outperform the shock dropouts but not prison parolees. The prison sample, which occupies the middle position, is not significantly different from either the shock or the dropout samples. Figure 30 plots the sample means.

Repeated measures analysis of the relationship between treatment sample and positive adjustment reveals that in any single time period, the samples are not significantly different from one another (Table 47). Between-subjects F-tests for each analysis yield null results and we would conclude from the longitudinal analysis that the samples do not differ significantly with respect to their positive adjustment scores. Indeed, an examination of the means suggests that the shock graduate sample outperforms the dropouts inconsistently.

The weak pattern of change in positive adjustment scores over time does not vary by sample. From this analysis, it seems reasonable to conclude that there are no striking sample differences in positive adjustment and that over-time changes in positive adjustment are not conditional on sample membership.

4.5.4.3 *Georgia*

Cross-sectional positive adjustment scores in Georgia do not differ significantly by sample. Table 48 presents the sample comparisons and Figure 31 plots the sample means. Repeated measures analysis of these data provide no additional insights. At every follow-up point, the samples perform at about the same levels and there is no evidence of differential change in positive adjustment levels over time across the treatment samples. Table 49 presents these tests.

4.5.4.4 Louisiana

Analysis of overall positive adjustment scores revealed that the sample of shock graduates outperformed the other samples which were not significantly different from each other. Table 50 presents this analysis and Figure 32 plots the sample means. A repeated measures analysis of these data (Table 51) suggested that the shock sample tended to perform better than the other samples at each time point although the decline in positive adjustment over time was more pronounced for the shock sample. The result of this is that shock positive adjustment scores, though significantly greater than those of the other groups at the outset of the study, converged toward the other groups by the end of the study.

The results of our assessment of the interaction effect (see eqs. (5) and (6)) suggest a strong "regression to the mean" pattern in the shock sample that is especially pronounced at months 9 and 12. At month six, however, the pattern is nonexistent: $\Delta_{shock} = -.048$, $\Delta_{other} = -.031$; $p < .388$. At month 9, however, the difference is more dramatic: $\Delta_{shock} = -.094$, $\Delta_{other} = -.037$; $p < .002$. And at month 12, the difference persists although it is slightly weaker: $\Delta_{shock} = -.076$, $\Delta_{other} = -.038$; $p < .073$. In short, it appears that the shock sample is responsible for generating the largest share of over-time change that is evident from the univariate repeated measures tests.

4.5.4.5 New York

The results of our analysis of overall positive adjustment in New York (see Table 52), as in Florida, suggest that the shock graduate offenders perform better than the shock dropout offenders but not significantly different than the prison parolees.[97] Figure 33 depicts the sample positive adjustment performances. Repeated measures analysis of variance (Table 53) indicates that the differences between the groups at different time points are inconsistent with respect to statistical significance.

In particular, some of the groupwise differences are statistically significant while others are not. Table 53 indicates that the shock sample tends to perform better than the other samples at each measurement period although in some cases dropouts perform better than parolees and vice-versa. The results of the sample by time interaction effect tests in Table 53 suggest that within-subject changes in positive adjustment are not conditional on sample membership.

[97]These inferences are based on Duncan post hoc tests displayed in Table 52.

From this analysis, we conclude that the shock sample generally adjusts more positively than the other groups. We also conclude that the weak pattern of change that we discovered in New York is approximately evenly distributed across the sample categories.

4.5.4.6 South Carolina

Overall positive adjustment analysis by sample in South Carolina indicates that there are no significant differences across the groups (Table 54).[98] Figure 34 shows that there is very little difference in overall sample performances. The over-time analysis yields results that are consistent with this overall finding. The data indicate that no sample has dominant positive adjustment scores at any of the four measurement periods and that there is no evidence of differential change rates in positive adjustment scores across sample categories. Table 55 presents the results of this assessment.

4.5.4.7 Summary

The results of this phase of the analysis varied somewhat by state. In Florida, New York, and Louisiana, there was evidence that the shock samples outperformed at least some of the other groups. The most striking and persistent finding was in Louisiana where the shock sample clearly dominated the other groups on positive adjustment at every follow-up point but also declined in positive adjustment over time at more than twice the rate of other samples after the six month point. Although there was at least weak evidence of decline in virtually all sample categories (including the shock sample) in each state, the shock samples in New York and Louisiana continued to outperform other groups through the end of the study. The shock sample in Florida outperformed the shock dropouts in the cross-sectional analysis but there was little evidence of a consistently stronger performance for the shock sample in the longitudinal analysis. In Georgia and South Carolina, there were no evident differences in performance or in change behavior across the sample categories.

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[98] In this analysis, we include the S.C. DOC shock sample although their data were collected only at the end of the one year follow-up period. In the over-time analysis we will not include the DOC sample in the comparisons.

4.5.5 Positive Adjustment Distributions By Exit Cohort

4.5.5.1 Overview

In this section, we return to the exit cohorts described earlier. As we noted earlier, a subject is assigned to a cohort category according to the number of consecutive measurements taken on the subject beginning at the first quarter. Cases that were not measured at the first quarter were assigned to cohort 0 while cases that were measured at the first quarter but not beyond were assigned to cohort 1. Subjects who were measured at the first and second quarters but not beyond were assigned to cohort 2 and subjects who completed only the first three measurements were assigned to cohort 3. Cohort 4 is comprised of subjects who completed all four measurements. As in the previous section, we assess cohort effect on cross-sectional positive adjustment scores.

4.5.5.2 Results

Table 56 presents the exit cohort analysis for each of the states. Figures 35-40 graph the means in Florida, Georgia, Louisiana, New York, and South Carolina, respectively. The data in Florida suggest that positive adjustment scores vary significantly by exit cohort. The Duncan post hoc test for Florida indicates that exit cohort 4 had the strongest performance while the other groups were not significantly different from each other.

In Louisiana and New York, the results indicate that subjects who continue follow-up until the end of one year had significantly higher positive adjustment scores than subjects who dropped out within the first six months. In South Carolina, the patterns observed in the other states were evident but not statistically significant. In Georgia, the above patterns were not evident and the cohort differences in successful adjustment were not statistically significant.

4.5.5.3 Summary

In Florida, Louisiana, and New York, the data reveal evidence of cohort effects on positive adjustment. In Georgia and South Carolina, there is no evidence that membership in a particular exit cohort is associated with a stronger or weaker cross-sectional positive adjustment score.[99] The nature of the effect also appears to be stable across states.

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[99]The patterns are not statistically significant, although the graphs in all states but Georgia are suggestive of a positive association between number of continuous follow-up periods and successful adjustment.

The importance of this cohort effect leads us to the question of what it means and what its importance for our analysis might be. The longevity of a subject's follow-up period is clearly most affected by detected acts of recidivism, technical revocations, and legal releases. Thus, we expect that the cohort variable sweeps up these effects. On balance, our preliminary analysis suggests that whatever this effect includes, when subjects fail to complete the study, they do not adjust as positively.

If we include the cohort effect in our multivariate analysis, we are, therefore, analyzing the effect of our predictors on positive adjustment controlling for whether the offender is on an early exit trajectory. To the extent that being on an early exit trajectory (e.g., an unobserved propensity to fail or be legally released) is related to our other predictor variables and is also related to positive adjustment, multivariate models of positive adjustment that do not include this variable will suffer from omitted variable bias (Neter et al., 1989; Draper and Smith, 1981). There are also good theoretical reasons for including this variable in a multivariate model of positive adjustment. It is reasonable to believe that subjects who are on an early exit trajectory will not adjust as positively as other offenders. In our analysis, by controlling for cohort effects on positive adjustment variables, we are simply incrementing or decrementing the value of the intercept term depending upon how long the offender was followed. From that point forward, our models formally impose the expectation that predictor variables will impact positive adjustment in the same way regardless of cohort.[100]

4.5.6 Assessment of the Relationship Between Supervision Intensity and Positive Adjustment

4.5.6.1 Overview

A key issue in our analysis is identifying the nature of the relationship between supervision intensity and positive adjustment to community supervision.[101] The literature to date suggests that this relationship should be positive. Our early analyses support

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[100]As a practical matter, the inclusion of the cohort effect had virtually no impact on the conclusions we drew from our models. Variables that were significantly related to positive adjustment without the cohort effect were also significantly related to positive adjustment when the cohort effect was added to the models. The addition of the cohort effect did improve the predictive power of our models somewhat although the improvement was not dramatic.

[101]Again, we note that supervision intensity data were not collected in New York. The New York data are not, consequently, considered in this analysis.

this expectation in every state that we studied but the evidence also suggests the need for some additional complexity.[102] In this section, for each state, we develop an estimate for the effect of cross-sectional supervision intensity on cross-sectional positive adjustment.[103]

4.5.6.2 Florida

The results of our initial assessment (a simple linear regression analysis) of the relationship between primary contact levels as well as secondary contact levels and positive adjustment in Florida are presented in Table 57. The data suggest that increases in supervision intensity are associated with increases in positive adjustment. Importantly, the analysis also reveals that secondary contacts have no effect on positive adjustment when primary supervision intensity is controlled. We attribute this to the strong positive correlation between these two indicators ($r = .846$; $p < .001$).

When both variables are included in the model, the variance inflation factors associated with their estimated effects exceed 3.5. Since these indicators are also highly correlated over time,[104] it appears reasonable to conclude that there is little benefit in retaining both of them for use in the same model. From this point forward, we work only with primary contacts in our

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[102]We are referring to the need for nonlinear specification. When fitting polynomial regression, it is customary to work with mean centered data on the polynomial terms (to minimize collinearity) (Draper and Smith, 1981). We adopt this practice in all nonlinear specifications reported in this paper.

[103]We believe that the evidence we have presented so far justifies the decision to work with cross-sectional rather than longitudinal data. In all of the states, both supervision intensity and positive adjustment decline weakly over time. Repeated measures analysis of variance tests also suggest that these declines are, with the exception of Louisiana shock graduates, not conditional on sample membership. Even in Louisiana, all groups tended to decline but the shock sample's decline was more dramatic. Given this uniformity both across states and within states over time, things are made much simpler by working tentatively with cross-sectional data. We will return to the longitudinal dimension of the data in a later section.

[104]Zero-order correlations between primary and secondary contact indicators for each of the four quarters are: $r_1 = .834$, $r_2 = .710$, $r_3 = .811$, and $r_4 = .783$. Each of the correlations is statistically significant beyond the $p < .001$ level.

analysis of positive adjustment.

Our diagnostics for the functional form of the relationship between supervision intensity and positive adjustment suggested that a linear function might not provide the most appropriate fit to the data. Our diagnostic effort consisted principally of smoothing out the supervision intensity distribution. This involved rank-ordering the subjects in each state on their overall supervision intensity scores. We then divided the cases into ten, twenty, and forty equally sized groups (ordered by supervision intensity) and assessed the distribution of overall positive adjustment scores across the ordered groups in each configuration.

Figure 40 presents the resulting graph for the twenty group assessment. There is a prominent nonlinear quality to the relationship although the relationship is generally positive. Still, a model that passes a curve through these data rather than a straight line would appear to provide a better fit. Continuing our diagnostic effort, we fit polynomials of degree 2 and 3 and found that the third degree (cubic) polynomial provided the best fit to the data. Table 58 presents the results of estimating the polynomial models and Figure 41A conveys the estimated regression functions.[105] Figure 41B presents some diagnostics for the fit of the model. The data in Figure 41B suggest that the curves in the positive adjustment distribution are captured most adequately by the cubic model.

In short, Figure 41A reveals that after about 1.8 monthly contacts, the return in positive adjustment is not as great. We note that key components of this curve are not fit on a small number of cases. The 50th and 90th percentiles of the primary contacts distributions are noted by vertical lines drawn through the graph. These data clearly suggest, whether polynomials of degree two or three are fit, that returns on positive adjustment are not strong above 1.8 contacts per month. The cubic model fits an upward curve at the right tail of the supervision intensity distribution but it is clear that this inference is based on a very small number of cases and we are hesitant to infer a great deal from it. The Florida data strongly suggest that our inferences about the relationship between supervision intensity and positive adjustment should be based on fitting a curve rather than a straight line.

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[105]The cubic term implies a reasonably strong positive linear trend after the second bend in the curve. We note, however, that this strong upward trend is based on a few cases with very large primary contact scores. The feature of the curve that is in the range of more cases is the relatively flat portion between curves 1 and 2.

4.5.6.3 Georgia

The results in Georgia lead us to basically the same conclusions that we reached in the Florida analysis. Figure 42 presents the smoothed joint distribution of supervision intensity and positive adjustment and again reveals a nonlinear pattern. Table 59 presents both the linear and polynomial models. There is a clear improvement in the fit of the model that we can attribute to the use of a third degree polynomial term. Figure 43A conveys the curves that are fit via our parameter estimates and Figure 43B assesses how well each function fits the data.[106] Interestingly, we note that dimininshing returns in positive adjustment as a function of increasing supervision intensity are evident once again at slightly fewer than 2 contacts per month. This result is quite consistent with what we observed in Florida. We conclude that an adequate accounting of the relationship between supervision intensity and positive adjustment will include a nonlinear component.

[106]Again, the strong increase in the third degree of the polynomial function is estimated from a relatively small group of extreme cases. We tend to emphasize the pattern implied by the second degree of the polynomial between curves 1 and 2 which is estimated on a much larger number of cases.

4.5.6.4 Louisiana

Nonlinear patterns are much less pronounced in Louisiana.[107] Figure 44 presents the smoothed joint distribution of the knowledge index and positive adjustment while Figure 45 presents a comparable graph for the surveillance index and Figure 46 depicts the requirements index. Tables 60, 61, and 62 present the linear, quadratic, and cubic models for knowledge, surveillance, and requirements scores, respectively. Figure 47 presents the estimated effects for the knowledge index, while Figures 48 and 49 present the estimated effects for the surveillance and requirements indexes, respectively.

Several conclusions are evident. First, the strongest case for curvilinearity is associated with the knowledge index. The problem with this effect is that is largely based on the influence of three observations which are represented as spikes at the right-hand side of the graphs in Figure 47B. When these two cases are removed from the analysis, the quadratic and cubic terms are no longer statistically significant. We conclude from this that while it would probably be unreasonable to tamper with the observations (since with supervision intensity there will always be extreme values), neither would it be desirable to make a solid inference about a curve beyond the 99th percentile of the distribution. We note that specification of the nonlinear model does not distort the relationship seriously before the 90th percentile of the knowledge index but between the 90th and 99th percentile, the function is seriously distorted. It appears that a nonlinear specification in

[107]Given the shared variance problems associated with our use of two supervision intensity indicators in Florida, we also assessed the potential for this problem in Louisiana. Simple zero-order correlations suggest that while the measures are certainly correlated, they do not proxy for each other. That is, they appear to tap different dimensions of community supervision (as they were intended to do). Variance inflation factors in models that we estimate did not suggest that any problems were created by entering all three variables in the model simultaneously. Zero-order correlations for these measures were as follows:

	Overall	Q1	Q2	Q3	Q4
Knowledge and Surveillance	-.42	-.38	-.34	-.34	-.20
Knowledge and Requirements	-.18	-.19	-.11	-.09	.01
Surveillance and Requirements	.67	.69	.64	.42	.30

All correlations greater than $|0.11|$ are statistically significant beyond the $p < .07$ level. On the basis of this evidence we retained all three measures of supervision intensity as separate indicators.

this context would not be particularly useful.

Second, a marginally significant quadratic effect for surveillance is also apparent (Table 61 and Figure 48A). Further analysis (Figure 48B) reveals, however, that the nonlinear specification is conditioned on the presence of one influential observation. When this observation is removed, the quadratic term becomes nonsignificant.[108] Distortions in the fit of the function in Figure 48B are strongly in evidence when nonlinear terms are included in the surveillance model. We conclude that the linear specification is most appropriate.

Finally, there is no evidence of a curvilinear effect of the requirements index on positive adjustment. We conclude tentatively here that limited support for nonlinear specification is evidenced by the estimated regression function but that the actual distribution of the data do not support our proceeding with these specifications. The linear function appears to provide the best fit to the available data.

4.5.6.5 South Carolina

As in Florida and Georgia, a strong case for nonlinearity is apparent in South Carolina. The smoothed joint distribution for primary contact levels and positive adjustment presents with nonlinear features (Figure 50) as does the estimation of models to capture these effects. Our estimated polynomial models yield statistically significant quadratic and cubic effects (Table 63). Figure 51A presents the curves that these models imply. Figure 51B assesses the fit of the functions to the data and reveals some ambiguity about whether the second or third degree model provides the best fit to the data. The ambiguity, however, is again in the extreme right tail of the supervision intensity distribution and either approach leads us to the same substantive conclusions about the bulk of the cases. As in Florida and Georgia, the model implies that more than about two contacts per month on average yields little return on positive adjustment. We conclude, once again, that the use of a nonlinear specification is appropriate.

4.5.6.6 Summary

In this section of the analysis we have considered the nature of the relationship between supervision intensity and positive adjustment. In so doing, it appears that, at least in three of the

[108] Note that we do not permanently remove these observations from the analysis. We only removed them for purposes of assessing their influence on the statistical significance of the polynomial terms.

four states where supervision intensity data were available, the effect of supervision intensity on positive adjustment is generally positive although the relationship is not linear. In particular, the data suggest that the relationship between supervision intensity and positive adjustment levels off significantly beyond a certain level and then begins to increase again. Interestingly we are able to state the leveling-off point across these three states with some degree of precision: it appears that incremental returns in positive adjustment as a function of supervision intensity diminish beyond 1.8 to 2.0 contacts per month. We are struck by the consistency of this effect across states.

The third degree of the function in each state is based on a very small number of extreme cases. Our attention is drawn more to the first and second degrees of the polynomial which show an initial strong positive relationship followed by a very weak relationship between supervision intensity and positive adjustment. The principal conclusion we draw from the available evidence is that in all states except Louisiana, an appropriate accounting of the relationship between positive adjustment and supervision intensity will include a nonlinear specification. In Louisiana, we conclude that there is little evidence to support a nonlinear specification but we also reiterate that supervision intensity in Louisiana is not measured by the number of contacts. Thus, we are not in a position to make useful comparisons about this apparent disparity.

4.5.7 Assessment of Within-Subject Change: Supervision Intensity and Positive Adjustment

In the previous section, we considered the between-subjects relationship between supervision intensity and positive adjustment. The analysis implies that, at any given point in time, there will be a curvilinear relationship between supervision intensity and positive adjustment across the population. However, this result has nothing to say about the impact of small short-term change (such as that observed in this study) on positive adjustment within subjects. In this section, we assess whether there is within-subject covariance between within-subject changes in supervision intensity and positive adjustment. The model described in eq. (8) is the equation of interest and γ is the parameter estimate.

Table 64 presents the analysis results for all states. Several comparisons are described within each state. First, the corresponding change within each of the adjacent time periods are estimated. Next, each subject's range of continuous measurements is determined. The time 1 measurement is then subtracted from the final measurement (either time 2, time 3, or time 4). For each subject, we then have the total change in positive adjustment and the total change in supervision intensity over their entire study period. A γ coefficient is then estimated on the total change throughout each subject's follow-up period. We note at the outset that this latter test is statistically significant in every state but Georgia (where $p < .165$).

The data in Table 64 provide some support for the hypothesis that the observed over-time changes in positive adjustment can be attributed, at least partially, to the observed over-time changes in supervision intensity. As expected, the estimates of γ are positive in all analyses (supervision intensity and positive adjustment tend to increase and decline together) and most of the estimates are statistically significant. We thus find evidence to reject the null hypothesis that within-subject supervision intensity and positive adjustment are unrelated in the population. *Ceteris paribus*, within-subject increases and decreases in supervision intensity are likely to lead to corresponding increases and decreases in positive adjustment.

4.5.8 Positive Adjustment, Treatment Sample, and Supervision Intensity

4.5.8.1 Overview

In this section, we turn to an assessment of the effect of sample on cross-sectional positive adjustment while adjusting for the cross-sectional effects of supervision intensity. The approach is based on the analysis of covariance model presented in eq. (9).

In addition to focusing on the additive effect we consider the possibility that the effect of supervision intensity on positive adjustment may not be similar across samples. We attend to this possibility by testing whether the data support a sample by supervision intensity interaction effect. Given that there are significant differences in supervision intensity levels across samples, we expect that this section will help us reach some more solid conclusions regarding both the effects of sample membership and supervision intensity in predicting outcomes on overall positive adjustment. As in previous sections, we consider each state's data in turn.

Finally, in this section we focus on somewhat of a tangential but still important issue. A point about which we were concerned was the "paradox" described in an earlier section where offenders with high levels of supervision intensity were predicted also to have high rates of technical violations and high levels of positive adjustment. The problem is that we expect offenders with technical violations during their term of community supervision to adjust significantly less positvely than their counterparts who did not have a technical violation. This puts us in the position of predicting polarized outcomes from the same set of exogenous circumstances. In this section we consider whether supervision intensity is generally positively associated with successful adjustment in the presence and absence of a technical violation.

4.5.8.2 Florida

We begin our assessment of the Florida data by testing several variants of interactions between treatment sample and primary contacts. Table 65 presents the results of these analyses. Using the simple linear specification of supervision intensity results and allowing it to interact with sample membership yields a model where the main effect for primary contacts is statistically significant but neither the main effect for sample nor the interaction term is statistically significant.

The expansion of this model to a quadratic specification yields statistically significant main effects for the quadratic supervision intensity term and the interaction effect. A plot of the function is presented in Figure 52 and reveals that the sample differences are indeed noteworthy. We note, however, that this curve depicts some potential for instability not unlike what we observed earlier in Louisiana. The differences in the supervision intensity distributions across samples is the culprit. The 90th percentile for supervision intensity in the shock graduate sample is 3.5 monthly contacts. The 90th percentile for shock dropouts and prison parolees is slightly more than 19 monthly contacts. The largest disparity in the functions is between the 3.5 and 19.0 monthly contacts levels. Indeed, if four influential shock graduate cases are removed from this region of the graph, quadratic and linear interaction terms are no longer statistically significant. Estimating a function with linear, quadratic, and cubic terms that interact with sample leads to the model described in Equation 3 of Table 66. Figure 53 plots the functions and reveals little evidence of important differences across samples.

Given this information, Table 66 presents what we believe to be a reasonable estimate of the effect of sample and supervision intensity on positive adjustment. The effect of sample is statistically significant and indicates that the shock sample performs significantly better on overall positive adjustment than the prison parolee and the shock dropout sample after adjusting for supervision intensity. The cubic effect of supervision intensity controlling for sample membership is presented in Figure 54. Two prominent features are evident in the curve: (1) the leveling-off effect occurs again at about 2.0 contacts per month and (2), the shock graduate sample performs significantly better than the shock dropout and the prison parolee sample adjusting for the effects of supervision intensity.[109]

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[109]In our single-factor analysis, we noted that the shock sample's performance differed from the dropouts but not from the prison parolees. In this analysis, the shock sample differs from both. The mechanics of the function suggest the following: (1) supervision intensity (particularly at the tail) is positively associated with successful adjustment; (2) prison parolees are

4.5.8.3 Georgia

In Georgia, there is no evidence of an interaction effect between the linear formulation of primary contacts and sample membership. Table 67 presents the assessments. The quadratic and cubic models yield a statistically significant interaction effect between sample and the quadratic and cubic effects of primary contacts, however, a plot of the function (Figure 55) reveals that this interaction term is due to three cases in the prison sample that force the positive adjustment curve downward dramatically at relatively high contact levels. The dramatic nature of this downward shift is not like anything we observe for prison parolees in other states. Consequently, we are inclined to view this as more an aberration than a meaningful estimate of what is occurring among prison parolees in Georgia.

Our final specification of the effect of sample adjusting for supervision intensity is presented in Table 68. The results of this analysis suggest that the effect of sample continues to be nonsignificant. Moreover, the polynomial specification of supervision intensity continues to be statistically significant. Figure 56 presents the effects of supervision intensity controlling for the effects of sample membership. The curve suggests the following two conclusions: (1) there is significantly less incremental change in positive adjustment attributable to changes in supervision intensity when monthly contacts exceed the 2.0 level; and (2) probationers have slightly higher positive adjustment scores than the other two groups but the differences are not statistically significant.

4.5.8.4 Louisiana

The data in Louisiana do not provide a great deal of support for the presence of substantively important interaction effects between sample and supervision intensity on overall positive adjustment. Table 69 presents the assessment of interaction terms for the linear specifications of knowledge, requirements, and surveillance. Product terms for surveillance and requirements are statistically significant. Figure 57 plots the surveillance functions and does not reveal any substantively important differences. Although the functions do diverge, the differences do not become significant until beyond the 90th percentile of the surveillance distribution. We, therefore, conclude that implementation of a product term for this effect would add unnecessary complexity to our models.

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supervised significantly more intensively than shock graduates; and (3) some of the higher positive adjustment scores in the prison parolee sample are attributable to supervision intensity rather than sample membership.

Things are not as clear for the significant requirements x sample interaction term. The regression functions are plotted in Figure 58A and reveal that the intercept terms for probationers and shock graduates are significantly higher than for the other groups.[110] Interestingly, the relationship between positive adjustment and requirements for probationers is slightly negative (although not significantly different from zero) while, for other groups, the relationship is clearly positive. By itself, this would constitute an important interaction effect. As Figure 58B reveals, however, when other measures of supervision intensity are controlled, the interaction effect vanishes and the regression functions for each of the samples are not significantly different. We, therefore, conclude that the estimation of this interaction effect would create unnecessary complexity.

After working through several preliminary models, we arrived at separate models for each of the supervision intensity variables adjusting for sample membership. Table 70 presents each of these three sets of tests and Figure 59 presents the specification for the knowledge effect. The results indicate that after controlling for knowledge, the shock graduates continue to adjust more successfully than other groups. Figure 60 presents the estimated effect of surveillance. The results of this analysis suggest that the samples perform about the same on positive adjustment when surveillance is controlled. Figure 61 depicts the linear effect of requirements as estimated by the model and indicates that shock graduates continue to perform better than the other groups. Thus, in two of the three specifications, shock graduates continue to outperform the other groups on the positive adjustment scale.

When these simple two variable models are expanded to include the other supervision intensity terms, however, things change (Table 71). In particular, we are concerned here with the partial regression coefficients for the sample categories after adjusting for all three supervision intensity scales simultaneously. The results provide evidence that the shock sample adjusts less well than the probation sample and the shock dropout sample although its performance is not significantly different from the prison parolee sample. Figures 62 (Knowledge), 63 (Surveillance), and 64 (Requirements), however, encourage us to use caution in reading too much into these differences since they appear to be relatively small. Nonetheless, it is interesting to note that the significantly positive effect of shock sample membership that was observed previously can be explained away by controlling

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[110]This is consistent with what we observed in the previous section when we noted that the shock and probation groups seemed to converge toward each other over time.

supervision intensity variables.[111] Thus, in Louisiana, the tentative conclusion must be that the shock sample does not outperform the other groups (and, indeed, may do worse) when indicators of supervision intensity are controlled.[112]

4.5.8.5 South Carolina

In South Carolina, assessments for interaction effects yielded no statistically significant results for any specification of primary contact levels. Table 72 presents the hypothesis tests for this analysis. Our final specification of a model for treatment sample adjusting for supervision intensity in South Carolina leads us to the results described in Table 73. The effect of sample continues to be nonsignificant while the third degree polynomial term for primary contacts continues to be statistically significant. Figure 65 presents the estimated effect of supervision intensity controlling for treatment sample effects. When average monthly contacts approach and exceed the 1.5 to 2.0 range, positive adjustment does not continue to increase (as it does with increases at the lower end of the distribution). Figure 65 also suggests that the samples perform quite similarly as groups.

4.5.9 *Assessment For Whether Supervision Intensity Effects Are Conditional on The Absence of Technical Violations*

4.5.9.1 Results

Analysis presented in Table 74a reveals the observed failure rates when failure is defined by: (1) arrest; (2) a new crime revocation; and (3) a technical violation within each state. Analysis results in Table 74b indicate, as expected, that successful adjustment is inversely related to failure on all three criteria. Table 74c suggests that intensity of supervision is positively associated with prevalence of technical revocations in

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[111]This result is consistent with an earlier analysis of these same data described by MacKenzie et al. (1992).

[112]Given the large difference in supervision intensity scores between the shock sample and other groups in the Louisiana study, this finding suggests that there may be a spurious effect for shock sample membership due supervision intensity. Since the groups are so nonequivalent on supervision intensity, however, the question of whether the result is spurious or whether there is an unanalyzable (in these data) interaction effect between sample membership and supervision intensity appears relevant. A test of this hypothesis would require a shock group followed on both low and high intensity and a comparison group followed on both low and high intensity.

Florida and Louisiana although there is no relationship between these variables in Georgia and South Carolina.

An interesting question is, therefore, whether the effect of supervision intensity on positive adjustment is still positive even for technical violators. In general, the data suggest that it is. The correlations between supervision intensity and positive adjustment for those who have technical violations and those who do not are presented in Table 74d. The results suggest that, in general, supervision intensity and positive adjustment are positively related regardless of whether the offender has a technical violation. In short, there appears to be little need for constructing an interaction term that would include this type of effect.

4.5.9.2 Summary

In Florida and Louisiana, adjustments for supervision intensity had implications for the single-factor results described earlier. In Florida, the shock sample outperforms both the prison parolees and the shock dropouts when supervision intensity is controlled. In the single-factor analysis we concluded that the shock graduates outperformed the shock dropouts but not the prison parolees. In Louisiana, adjusting for supervision intensity (particularly for surveillance) reduced what had been a significant positive effect for the shock program into a weak negative effect. We attributed this result to our earlier finding that shock graduates are supervised significantly more intensively than other groups.

The conclusions in all other states remain virtually the same. In Florida, the shock sample continued to perform slightly better than other groups[113] while, in Georgia and South Carolina, the performance of the shock sample was virtually indistinguishable from those of other groups. Effects of supervision intensity continued to display their curvilinear functional form in Florida, Georgia, and South Carolina. Our analysis for significant interaction effects between treatment sample and supervision intensity on positive adjustment provided little basis for the inclusion of product terms in our models. We thus concluded that supervision intensity operates similarly within different samples to effect positive adjustment outcomes and we impose this assumption on the remainder of our models.

Finally, we conducted a test to determine whether there was any

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[113]Although the shock graduate sample outperformed both the dropout and prison parolee samples after controlling for supervision intensity. Before controlling for supervision intensity, the shock graduates outperformed the shock dropouts but not the prison parolees.

justification for modeling the relationship between supervision intensity and positive adjustment separately depending on whether offenders had a technical violation. The results of this analysis suggested that supervision intensity and positive adjustment were positively related regardless of whether offenders were revoked for technical violations.

4.5.10 Differences in Positive Adjustment Across Fixed Effects

4.5.10.1 Overview

In this section, we turn to an assessment of the impact of our fixed effects on overall positive adjustment. First, we analyze the singular influences of each of the fixed effects on cross-sectional positive adjustment scores. This effort will provide us with an overview of which predictors are positively associated with successful adjustment.

4.5.10.2 Florida

The results of our single factor assessment in Florida are presented in Table 75. The data suggest that nonwhites have lower positive adjustment scores while offenders who were older at the beginning of community supervision had higher positive adjustment scores. Violent offenders tended to have slightly higher positive adjustment scores on average while scores for the drug offenders were not significantly different from the overall average. "Property and other" offenders tended to adjust less positively than violent offenders. If the offender was serving his current sentence as a result of a new crime (versus a technical revocation), his adjustment score tended to be higher. Positive adjustment scores were lower for those offenders with a prior criminal history.

4.5.10.3 Georgia

Table 76 presents the single factor analysis results for the Georgia data. The data suggest that nonwhites adjust less positively than whites and that property and "other" offenders adjust less positively than violent and drug offenders. Prior arrest and/or conviction histories tended to be associated with lower positive adjustment scores as well.

4.5.10.4 Louisiana

Analyses of the relationships between fixed effects and positive adjustment scores for Louisiana are presented in Table 77. The results of these analyses indicate that nonwhites adjust less positively than other offenders while drug offenders tended to adjust more positively than violent, and property and "other" offenders.

4.5.10.5 New York

Assessment of the zero-order impacts of fixed effects on positive adjustment are described in Table 78. The results indicate that nonwhites adjusted less positively than whites. Offenders who were older at the beginning of community supervision and older at their first arrest generally adjusted more positively than other subjects.

4.5.10.6 South Carolina

Table 79 presents the effects of the fixed predictors on cross-sectional positive adjustment scores in South Carolina. The distributions indicate that nonwhites adjust significantly less positively than whites. The data also suggest that offenders with a prior criminal history did not adjust as positively as other offenders.

4.5.10.7 Summary

The results presented in this section provided few surprises. Nonwhites, offenders who were younger at release and younger at their first arrest, and had demonstrated problem behavior before adjusted less positively on average than other offenders. The effect of type of offense tended to vary by state. In Florida and Georgia, violent offenders tended to do better than other groups while in Louisiana drug offenders outperformed the other groups.

4.6 Summary of Preliminary Analysis

This section completes our preliminary assessment of the data. Thus far, our examination of these data have revealed a number of important findings which we carry into the next section of the analysis. We review the most prominent of them here.

- The research design we employ is essentially a comparison of nonequivalent groups on multiple post-tests collected over time.

- The groups, or samples, that we compare in each state tend to differ on characteristics that we expect to be related to the dependent variable (e.g., age, type of offense committed, supervision intensity, etc.). Based on this we conclude that it will be necessary to control for these effects before making inferences about the relationship between sample membership and positive adjustment.

- Our analysis employs two types of predictor variables: fixed and time-varying effects. Fixed effects collected at the outset of the study include sample (shock, prison, probation, etc.), race, age at the beginning of community supervision, type of offense, arrest/conviction criminal history, an indicator for whether the current offense is the result of a technical violation or a new crime, and, in Louisiana, New York, and South Carolina, the offender's age at first arrest. Supervision intensity indicators and positive adjustment are both time-variant: they are both collected over the course of a one-year follow-up period.

- Univariate analysis of supervision intensity indicators, which measured the number of face-to-face and telephone offender contacts in Florida, Georgia, and South Carolina, indicated that extreme values in the distributions tended to distort the mean as a measure of central tendency. After examining the distributions in some detail, we decided to work with a natural log transformation of the contact variables in these states. In Louisiana, supervision intensity is measured by three scales which measure: (1) officer's knowledge of offender activities; (2) the level of surveillance of those activities; and (3) intensity of requirements imposed on the offender. Univariate analysis of these indexes suggested that working with natural log transforms of the knowledge and surveillance indexes would be a reasonable step as well. We left the requirements index in its original metric. Supervision intensity information was not available for analysis in New York.

- Positive adjustment and supervision intensity generally declined over time in all states. The magnitude of decline, however, was notably weak. We found little evidence that the rate of decline in these scores varied in the different samples in each state.

- As a matter of preliminary analysis we decided to test whether supervision intensity and positive adjustment were related to each other within subjects. This assessment was conducted by regressing over-time changes in positive adjustment scores onto over-time changes in supervision intensity indicators. In every state, the data provided support for the conclusion that supervision intensity and positive adjustment tend to move in the same direction within individuals (i.e., they are positively related).

- Assessment of "exit cohort" effects revealed that offenders who completed the study tended to adjust more positively than offenders who did not.

- Analysis of positive adjustment scores by sample membership categories showed that shock offenders adjusted significantly more positively than other samples in Florida, New York, and Louisiana. In Florida and New York, the shock sample outperformed the shock dropouts but not the prison parolee sample. In Louisiana, the shock sample outperformed each of the other samples. In Georgia and South Carolina, there were no between-sample differences on the positive adjustment index.

- Over-time analysis of the singular effect of sample membership on positive adjustment suggested that group differences remained relatively stable over time.[114] The most notable exception to this pattern was in Louisiana where the shock sample, although performing better, converged toward the other groups over time.

- The effect of supervision intensity on positive adjustment appears to be generally positive although there is considerable evidence that it is nonlinear. The most common finding (in our cross-sectional analyses) was a general "leveling off" of the effect of supervision intensity beyond about 1.8 to 2.0 offender contacts per month.

- Assessments for sample by supervision intensity interaction effects yielded null results. We conclude that supervision intensity tends to affect positive adjustment in similar ways across samples.

- We estimated analysis of covariance models of cross-sectional positive adjustment that compared sample performances adjusting for cross-sectional supervision intensity levels. In Georgia and South Carolina, the results of earlier sample comparisons remained unchanged after controlling supervision intensity. In Florida, the shock sample outperformed both the prison parolee and shock dropout groups. After adjusting for supervision intensity levels in Louisiana, the effect for the shock program was slightly negative and marginally significant. These analyses support the retention of nonlinear models to represent the relationship between supervision intensity and positive adjustment.

- Analysis of the relationship between fixed effects and positive adjustment indicated generally that nonwhites, younger offenders and subjects with prior records did not perform as well as others on the cross-sectional positive adjustment construct.

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[114] In Florida, however, the shock sample did not perform significantly differently from the other groups in the over-time analysis. Still, there was no time x sample interaction effect in these tests.

In the next section, our focus shifts to the development of multivariate models predicting positive adjustment. Our first concern is the specification of a full multivariate model of cross-sectional positive adjustment. Our focus then shifts to the specification of a multivariate positive adjustment model that utilizes the longitudinal information in our data.

5. Analysis Results

5.1 Overview

In this section, we present several multivariate models of positive adjustment to assess whether shock incarceration programs exert an effect net of other predictor variables. As in the previous sections, a significant amount of attention is focused on the effect of supervision intensity indicators as well.

Within each state, a model similar to those developed earlier will be estimated using indicators of cross-sectional supervision intensity and cross-sectional positive adjustment. For these analyses, positive adjustment and supervision intensity scores are simply averaged over all available measurement periods for each subject. These models are estimated with fixed effects treated as exogenous variables. Supervision intensity indicators are treated as intervening variables.[115] A diagram of this model, which we refer to hereafter as the cross-sectional model, is presented in Figure 66.

Next, we turn to the longitudinal structure of the data. There are obviously several ways that an analysis of these data could proceed. We will approach the problem with two different methods. First, we conduct a simple set of repeated measures analysis of covariance tests with two broad objectives: (1) to determine what variables are predictive of positive adjustment at different waves of the study (between-subject effects); and (2) to determine whether effects differ significantly across waves (time x between subject effect interactions). Second, we analyze the data as a series of three separate two-wave longitudinal regression models (see Figure 67). Both of these methods have their advantages and disadvantages which we review in the next section.

5.2 Analysis Methods

5.2.1 Repeated Measures Analysis

We begin the longitudinal component of the analysis in each state by specifying a simple set of repeated measures analysis of covariance models. These analyses are based on two important assumptions: (1) the dependent variables have a multivariate normal distribution; and (2) the dependent variables share a common covariance matrix (Littell et al., 1991). The first assumption is merely an extension of standard univariate ANOVA models which require that the dependent variable be normally distributed in the

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[115]Except, of course, in New York where supervision intensity indicators are not available.

population from which the sample is drawn (Draper and Smith, 1981; Dunn and Clark, 1987). The second assumption, however, is problematic for our purposes. First, we have to confront the problem of attrition. The decay in the sample size has implications for the solution to the normal equations:

$$\beta = (X'X)^{-1} (X'Y) \qquad (10).$$

In short, the dimensions of the X and Y matrices change over the course of the follow-up period because the sample size deteriorates. Cohort 4 subjects will be in every follow-up period and consequently will always be considered when the normal equations are solved but the same is not true for cohorts 1, 2, and 3. Consequently, within each state, to analyze data at four waves, ten vectors of coefficients have to be estimated along with three separate sets of between and within-subjects effects.

Adding to the problem is the lack of a common covariance matrix. For the fixed effects, the covariance matrix is, of course, common across the matrix of scores on the dependent variables (positive adjustment scores at the four different waves) and no problems are created. Our supervision intensity indicators, however, are not fixed. Instead, they vary along with positive adjustment over time and, by definition, violate the assumption of a common covariance matrix. There are two consequences associated with this for our purposes: (1) we have to force some sort of cross-sectional (averaged) indicator for supervision intensity into the covariance matrix (and hope that it is sufficiently correlated with what we should have entered that the effects are accurately estimated); and (2) since it is unlikely that we will be able to achieve this, the efficiency and unbiasedness of our estimates for the effect are highly questionable and the time by supervision intensity interaction tests could be biased and inconsistent.

Despite these problems, a repeated measures analysis of variance approach is a satisfactory way to address the two objectives described above, at least for the fixed effects. With respect to supervision intensity, this method is less useful. Consequently, we only include a cumulative average of raw contacts through the highest wave of each repeated measures analysis. For the repeated measures analysis of waves 1-3, then, the common covariance matrix would include the average number of monthly contacts (or supervision intensity scale scores in Louisiana) during the first nine months of the follow-up period.

For each repeated measures analysis, we present the vector β of parameter estimates for each wave, an F-test for the main effect of time (within-subject change), and F-tests for time x predictor interaction effects that are statistically significant. Only variables that made a statistically significant contribution to the model in at least one wave are presented. Time x predictor interaction effects that are not statistically significant are not presented. Finally, we present the mean positive adjustment scores for each sample adjusted for the average values of the covariates for each sample. These are usually referred to as "least-squares means" and they are calculated via the LSMEANS option of the GLM procedure in SAS (release 6) software (Littell et al., 1991).

5.2.2 Longitudinal Regression Models

An alternative approach that allows us to estimate four sets of models and include the appropriate indicators of supervision intensity at each time point is the analysis of covariance model that is estimated outside of the repeated measures analysis of variance framework. Schematically, such an analysis can be represented as a series of two-wave longitudinal regression models with diminishing sample sizes at each wave of the four-quarter follow-up period. In each model, subjects from exit cohorts that have valid measurements for that model are included.[116] Thus, each wave is based on a different number of cases. Using the exit cohort indicators developed earlier, however, the model for each wave statistically controls for exit cohort membership.[117] The fixed effects predictor variables described in the previous section along with the natural log transform of the contact variables are entered into the models.[118]

The positive adjustment model takes a generalization of the form presented earlier in eq. (3):

$$p_{it} = \alpha + \delta p_{it-1} + \gamma s_{it} + \beta x_i + \varepsilon^* \qquad (11)$$

and the supervision intensity model assumes the form:

$$s_{it} = \alpha + \lambda s_{it-1} + \gamma p_{it} + \beta x_i + \varepsilon^* \qquad (12).$$

Note from eqs. (11) and (12) that δ and λ are stability coefficients estimated by the method of ordinary least squares for positive adjustment scores (p) and supervision intensity scores

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[116]For example, cases in exit cohort 3 are included in the wave 1, 2, and 3 models but not in the wave 4 model.

[117]Except, of course, at wave 4, when only one exit cohort remains in the analysis.

[118]In Louisiana, the natural log transform of the knowledge and surveillance scores are entered. The requirements scores are retained in their original metric.

(s).[119] The x_i are fixed predictor variables that do not change over time.[120] The parameter estimates in B will be interpreted as usual. The stability coefficient has several useful interpretations: (1) it represents the percent of variation in the temporally prior value of the dependent variable that is stable over time; (2) when it is positive, it indicates that observations that scored relatively high at time *t-1* also scored relatively high at time t; and (3) when it is negative, it indicates that observations that scored relatively high at time *t-1* scored relatively low at time *t*.

In an effort to validate the longitudinal regression models that we develop, the observations are rank ordered by the predicted values of the dependent variables. We then divide the observations into five approximately equal-n groups cut at the 80th, 60th, 40th, and 20th percentile ranks. Within each of these "quintile" groups, the average predicted value of the dependent variable is compared to

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[119]Under this framework, the stability coefficients when estimated by the method of ordinary least squares are biased but consistent when there is no serial correlation in the errors. When serial correlation is present the estimates of the stability coefficients are biased and inconsistent (Pindyck and Rubinfeld, 1991). Any time temporal processes are studied, the risk of autocorrelation is high. Diagnostics for it are difficult to implement when there are only a handful of within-subject observations (for many of our cases, there were only two or three observations). The method of instrumental variables is usually invoked as a means to confront this issue (Liker et al., 1985; Markus, 1980). The categorical nature of the predictors hampered our ability to create an instrument for the lagged endogenous variable, however. The problem is that the introduction of the instrument into a two-stage least squares estimation procedure introduces linear dependencies into the **X** matrix. We also attempted to create an instrument by using prior values of the lagged endogenous variable but the stability coefficients looked less reasonable under this framework than they did when estimation by ordinary least squares was employed (i.e., occasionally, the estimates approached or exceeded 1.0 and sometimes the standard errors increased dramatically). In most of the models, these problems were not evident but they emerged in enough models that our comfort level with them was not high. Only in the worst cases of multicollinearity created by the instrument were the estimates between the OLS and 2SLS models for the other variables at variance with each other. In this paper, we will present the results of our OLS estimation efforts.

[120]Although it is possible to constrain these variables to have the same coefficients over time (Allison, 1990), we do not impose this constraint in this analysis given its exploratory nature.

the average actual value of the dependent variable. These analyses essentially help us to determine whether the models, as specified, do a reasonably good job of discriminating between low, medium, and high scorers on the dependent variable.

We also extend our assessment of the curvilinear effect of supervision intensity on positive adjustment scores to include an examination of these effects over the course of the follow-up period. Although we estimate these effects in the models, we complement this presentation by graphing the estimated regression function of this relationship cross-sectionally and longitudinally for a hypothetical subject in exit cohort 4. For this hypothetical subject, we assume that the other variables in the models are constrained at their mean values for the relevant time period.[121]

For each state, we compare the longitudinal regression models to the repeated measures analysis of variance approach and draw some conclusions about the processes that appear to be at work. Our preference, primarily because of the lack of a common covariance matrix, is the longitudinal regression approach and the majority of our interpretive effort is placed on the models developed in this framework. We turn next to the models that were estimated within each of the states.

5.3 Florida

5.3.1 Cross-Sectional Model

The analysis in Florida consists of a system of two equations (Figure 68). The first regresses primary contact levels on the fixed effects. The results of this model indicate that shock offenders are supervised less intensively than prison parolees and dropouts. Older offenders also tend to be supervised less

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[121]We note that even though the regression models are estimated with supervision intensity variables in log metric (as discussed earlier), these graphs exponentiate the log units back into their original metric. The graphs are still dispersed on a log scale because the skewness of the original distribution distorts the portion of the graphs where most of the cases are located. In addition, the graphs that we present for these analyses indicate the location of the 50th and 90th percentiles of the supervision intensity distribution. These indicators refer to the 50th and 90th percentiles of the cross-sectional (or overall) distribution. To calculate these percentiles, we average supervision intensity indicators across all periods for which each subject was followed. The median and 90th percentile of the derived distributions are the statistics indicated on the graph. We place these on the graphs in order to help the reader superimpose the frequency distribution on the regression functions.

intensively while violent offenders were generally supervised more intensively than either property or drug offenders.[122]

The analysis also reveals that the shock group performs significantly better than the prison parolee group controlling for the other predictors. The difference between the shock group and the dropouts, however, is not as large and is only marginally significant. Nonwhites tended to adjust less positively than whites and violent offenders generally outperformed property and "other" offenders. Offenders who had committed new crimes as opposed to technical violations of community supervision tended to adjust more positively as well. The analysis, as expected from our diagnostic efforts, suggests a curvilinear effect for supervision intensity. The exit cohort effects are also statistically significant and indicate generally that offenders who are followed longer tend to have higher positive adjustment scores. Model validation charts for supervision intensity and positive adjustment are depicted in Figure 69.

5.3.2 Repeated Measures Analysis

Tables 80-83 presents the analysis of covariance results at the first through the fourth waves, respectively. The analysis indicates that shock graduates performed significantly better than offenders in either of the other two samples. The least squares means portray the estimated magnitudes of those differences. Nonwhites, younger offenders, subjects who had committed a property offense and technical violators all performed significantly worse than their counterparts in other categories on the positive adjustment scale. These patterns emerged with relative consistency throughout the analysis.[123] The estimated linear effect of cumulative contacts is positive and statistically significant at virtually each wave and within each analysis.

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[122]In these analyses, we deleted the prior criminal history variable because it confounded the effect of the new crime indicator. When both variables were included in the model, neither was statistically significant. The effect of the new crime indicator was stronger than the prior criminal history indicator so we retained it for both the cross-sectional and the four-wave panel models.

[123]Although most of the coefficients were not statistically significant in the four-wave analysis, their signs and magnitudes were similar to those observed in the one-, two-, and three-wave studies. The major exception to this is the effect for the shock sample. The coefficients for the shock sample were much lower and not statistically significant at each of the four waves among the 58 subjects who were studied over the entire follow-up period.

The results for the positive effect of the shock sample over time are at odds with what was observed in the absence of control variables. Thus, the addition of important control variables in Florida has the effect of illuminating the differences between the groups.

The four-wave repeated measures analysis (Table 83) suggests a complete absence of this positive effect of shock incarceration. This finding suggests that, among offenders who are able to persist on community supervision for one year, the type of correctional program makes no difference in adjustment to the community.

The only statistically significant within-subjects interaction term was observed in the two-wave analysis. Table 81 indicates that the effect of having committed a violent offense on positive adjustment was significantly lower at the six-month point than at the three-month follow-up ($p < .072$). The repeated measures analysis revealed that, at each interval, the main effect for within-subject change was statistically nonsignificant. We take this as evidence of relatively weak and erratic patterns of change over time in the dataset which is consistent with our observations in Section 4.

5.3.2 Longitudinal Regression Model

Figure 70 presents the four-wave longitudinal model in Florida. We note at the outset that all stability coefficients are positive and significantly different from zero. Thus, subjects with high scores at any particular period are also likely to have high scores at the next period. For positive adjustment scores, at least 40% of the variation in scores at times 2, 3, and 4 was stable from the previous time period. For supervision intensity about 70% of the variation was reliable.

Supervision intensity at wave 1 is predicted by sample, age at the beginning of community supervision and type of offense. Positive adjustment at wave 1 is higher among shock offenders, subjects who were older at the beginning of community supervision, violent offenders, and offenders who were serving their current sentence for a new crime instead of a technical violation. Nonwhite subjects and subjects in the early exit cohorts tended to adjust significantly less positively than other offenders. The analysis also reveals that the effect for supervision intensity is curvilinear. Model validation charts are presented in Figure 71.

Wave 1 supervision intensity is the strongest predictor of supervision intensity at wave 2. At wave 2, offenders who were older tended to be supervised at slightly greater levels of intensity, controlling for positive adjustment at wave 1 which is marginally significant.[124] There is no direct effect of shock incarceration on positive adjustment at wave 2. Direct effects for age at the beginning of community supervision, the drug offense indicator (versus property and other offenses), and the new crime indicator (versus technical violation) were evident as was the curvilinear effect for supervision intensity. The cohort effects are also statistically significant in the expected direction. Subjects who persist in the study had higher positive adjustment scores than those who drop out earlier. The wave 1 positive adjustment was, as expected, the driving predictor and its effect indicates that about 48% of the value of wave 2 positive adjustment is directly attributable to positive adjustment at wave 1. Figure 72 presents model validation charts for the second wave equations.

At wave 3, supervision intensity is driven by a cohort effect (offenders who exit after wave 3 were supervised significantly less intensively than offenders who completed the study) and the level of supervision at wave 2. This result suggests that supervision intensity declined among those subjects that were more likely to exit the study. This same group, with lower supervision levels, goes on to perform worse on positive adjustment at wave 3 as well. Moreover, at wave 3, the shock and dropout samples both perform significantly better than the prison sample on positive adjustment. The data also reveal that older offenders perform better than younger offenders. The effect of supervision intensity on positive adjustment at wave 3 is both positive and linear. Figure 73 conveys the fit of the models at wave 3.

At wave 4, supervision intensity is predicted solely by the intensity of supervision at wave 3. Positive adjustment is predicted by a quadratic effect of supervision intensity and the positive adjustment score at wave 3. Thus, the impact of the fixed effects on positive adjustment at the end of the study was completely indirect under the longitudinal specification. Model validations for the fourth quarter are depicted in Figure 74.

In Figure 75, we graph the partialed effects of supervision intensity on positive adjustment both cross-sectionally and for each wave of the longitudinal model.[125] The graph represents a hypothetical subject in exit cohort 4 who scores the average value on each of the predictors. The results reveal some variation with respect to functional form although the first inflection point consistently occurs at about the 1.8 to 2.0 contacts per month point.

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[124]This is one of the few instances in any of our analyses where positive adjustment at one wave has a statistically significant impact on supervision intensity at a subsequent wave.

[125]By "partialed" we are referring to the effect implied by the estimated partial regression coefficient.

5.3.3 Summary of Florida Results

The models in Florida provide clear evidence of a lift in positive adjustment scores associated with membership in the shock incarceration sample. This effect is quite strong at the beginning and is generally indirect from that point forward. It is never negative, however. The repeated measures analysis of variance tests reveal no within-subject interaction terms for sample effects. This suggests that there are no great fluctuations in sample performance relative to other samples over time. Interestingly, however, the results for the four wave repeated measures model show no differences in sample performance. This result is important because it implies that, among those offenders who are able to continue on community supervision over the course of an entire year, there are no sample differences in positive adjustment. Although the sample size for the four-wave analysis is small to be sure (n=58), an examination of the adjusted means fails to reveal any hint of a positive effect for the shock graduate sample.

The longitudinal regression model at wave 4 provides some evidence that the dropout sample has moved closer to the shock sample but the initial effect of membership in the shock sample would, other things equal, lead us to the expectation that shock offenders adjust more positively than prison parolees and dropouts. The only evidence of a null effect for shock incarceration emerges in the four-wave repeated measures analysis of variance tests presented in Table 82. Still, these tests detect a slight (although not statistically significant) positive effect for membership in the shock sample. On balance, the evidence in Florida when the model is fully specified is that the shock sample outperforms the prison parolees and the shock dropouts.

Assessment of the impact of other variables revealed that the effect of supervision intensity is generally curvilinear and suggests a "diminishing returns" relationship between contacts and offender adjustment (Figure 75). The data suggest that returns begin to diminish, on average, when mean monthly contacts approach or exceed the 1.8 to 2.0 level. While the data also provide some support (at least in the earlier waves) for a subsequent lift in positive adjustment at extremely high contact levels, this end of the function is estimated on a very small number of cases and should be interpreted with caution.

Nonwhites tend to adjust less positively than white offenders initially and older offenders adjust more positively than younger offenders. There is evidence that violent and drug offenders tend to adjust slightly more positively than offenders who commit property and other types of offenses. Subjects serving a sentence for a new crime tended to adjust more positively than subjects who had been revoked for a technical violation. Finally, the results reveal that cohorts of offenders who remained in the follow-up period for more measurements outperformed offenders with fewer measurements.

5.4 *Georgia*

5.4.1 *Cross-Sectional Model*

Figure 76 presents the Georgia cross-sectional model. The data indicate that prison parolees are generally supervised more intensively than both probationers and shock graduates. The cross-sectional positive adjustment model reveals no direct effect for sample membership. The model does indicate that nonwhites and offenders with a prior criminal history perform worse while violent and drug offenders outperform property offenders. The effect of supervision intensity is curvilinear. Analysis of the impact of cohort membership yielded no important differences between the cohort groups. Validation charts for both the supervision intensity and positive adjustment equations are presented in Figure 77.

5.4.1 *Repeated Measures Analysis*

The repeated measures analysis reveals no statistically significant effects for sample membership at any of the waves (Tables 84-87). The least squares means reveal only trivial differences between the samples. The analysis also indicated that nonwhites, offenders with a prior record, and offenders who were contacted less frequently did not perform as well as other subjects. The effect of the nonwhite indicator did not emerge as being statistically significant until the second wave of the analysis. The result of this difference is that there is a statistically significant nonwhite x time interaction term in the two- and three-wave analyses. No other time interactions were statistically significant. The main effect for time is not statistically significant in these analyses which suggests that there were no substantively important within-subject differences in positive adjustment scores over time.

5.3.2 *Longitudinal Regression Model*

Figure 78 presents the longitudinal regression model for the Georgia analysis. The stability coefficients indicate positive and statistically significant prior time effects on the temporally current endogenous variables. The weak coefficient between time 1 and time 2 positive adjustment stands out, however. This result suggests that most of the variation at time 2 is not stable from time 1. There is evidence of more stability at subsequent time points for positive adjustment and at all time points for supervision intensity.

These data suggest that prison parolees and violent offenders are more intensively supervised than other groups at the outset of the study. Positive adjustment at the first wave is driven principally by supervision intensity at wave 1 and the presence of a prior criminal history. The regression coefficients indicate that the effect of supervision intensity on positive adjustment is generally positive but curvilinear (although achieving only marginal levels of statistical significance). Moreover, direct effects for exit cohort, race, age, and type of offense were not statistically significant at wave 1. Validation graphs for these models appear in Figure 79.

At wave 2, supervision intensity is predicted only by supervision intensity at the prior wave. Positive adjustment is directly affected by the nonwhite indicator, the type of offense, supervision intensity, and wave 1 positive adjustment (although the effect here is weak). The results indicate that nonwhites and property/other offenders adjust less positively than white offenders and subjects who committed violent or drug offenses. The effect of supervision intensity is curvilinear. Model validation charts are presented in Figure 80.

Supervision intensity at wave 3 is predicted largely by supervision intensity at wave 2. Evaluation of the positive adjustment equation suggests that nonwhites and offenders serving their sentence for a new crime (versus a technical revocation) adjusted less positively than other subjects. Violent and drug offenders continue to adjust more positively at wave 3 than their property offending counterparts even controlling for prior positive adjustment. The effect for supervision intensity continues to be curvilinear. Model validation charts are provided in Figure 81.

Wave 4 supervision intensity is predicted directly by prior supervision intensity although there is evidence for direct effects of the drug offender indicator (suggesting that drug offenders are supervised more intensively than property offenders) and the presence of a prior criminal history (offenders with priors are supervised more intensively than offenders without priors). Wave 4 positive adjustment is predicted by prior positive adjustment although at wave 4 older offenders appear to adjust slightly more positively net of prior positive adjustment. The model reveals a curvilinear effect of supervision intensity although the cubic term is no longer statistically significant. Model validation charts are provided in Figure 82.

The curves depicted in Figure 83 reveal the partialed effects of supervision intensity on positive adjustment for the cross-sectional and the longitudinal models. The results suggest that positive adjustment generally increases until an average of about 1.8 to 2.0 contacts per month. Beyond this point, the incremental gains in positive adjustment associated with increases in supervision intensity are much less evident. As was the case in Florida, there is evidence of a second inflection, both cross-sectionally and in waves 1, 2, and 3 but this point estimate is based on a very small number of cases and must be interpreted cautiously.

5.3.3 *Summary*

The results of the Georgia analysis suggest a marked absence of differences between the treatment samples with respect to positive adjustment. Other predictor variables, however, have effects on positive adjustment at various points in the follow-up period. Although the effect of being nonwhite on positive adjustment is negative overall, the longitudinal study does not reveal its emergence as a predictor of positive adjustment until wave 2.[126] Age at the beginning of community supervision, which is a relatively stable predictor of positive adjustment in other states, does not have a significant effect at all on positive adjustment in the cross-sectional model and does not become important until very late in the follow-up period (wave 4) in the longitudinal study. In general, the data suggest that "property/other" offenders do not adjust as well as violent and drug offenders and that the presence of a prior criminal history also detracts from positive adjustment. Interestingly, none of the cohort effects were statistically significant in the Georgia models but this is consistent with the results observed in our preliminary analysis of these data.

The consistent finding of a curvilinear relationship between supervision intensity and positive adjustment is a prominent feature of these models (Figure 83). As in the Florida study, the data suggest that positive adjustment increases with monthly contacts until the average number of monthly contacts reaches the 1.8 to 2.0 level. Beyond this point, there is considerably less evidence that contacts and positive adjustment are related to each other.

[126]This result was evident in both the repeated measures and panel regression studies.

5.4 *Louisiana*

5.4.1 *Cross-Sectional Model*

The results of the cross-sectional analysis in Louisiana are presented in Figure 84.[127] These data indicate that a significant portion of the variance in supervision intensity is explained by sample membership. Shock offenders were generally supervised much more intensively than offenders in other samples. Nonwhite offenders tended to be supervised significantly less intensively than other offenders. After controlling for the additional covariates that we include in these models, there was no evidence of an effect for sample on positive adjustment.

Nonwhite offenders tended to adjust less positively than white offenders while older offenders tended to adjust better than younger offenders. The cohort effects were statistically significant in this model and suggested that subjects remaining in the study to the end, adjusted significantly more positively than offenders in the three exit cohorts. Indeed, the effects for the exit cohorts dampen off in ordinal fashion suggesting that there is a stable positive relationship between the length of follow-up and positive adjustment during community supervision. Supervision intensity indicators were not curvilinearly related to positive adjustment in this model. Thus, we estimate the model with a linear term for each of these indicators. The linear terms suggest that supervision intensity and successful adjustment are positively related to each other. The validation charts for these equations are presented in Figure 85.

5.4.2 *Repeated Measures Analysis*

Tables 88-91 present the repeated measures analyses in Louisiana. The results provide some evidence that the shock sample does slightly worse as a group than the other three samples in the first six months of the follow-up period. After the six-month point, the shock sample continued to perform at a lower level than the other groups in most of the comparisons but the differences were no longer statistically significant. The least squares means provide the basis for these contrasts. Our conclusion from this assessment is that the shock sample tends not to perform quite as well as the other samples when other fixed effects and the aggregated

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[127]We excluded type of offense and age at first arrest variables from the models presented in this section. The missing data on these variables reduced the effective sample size from n=255 to n=221 and they added very little explanatory power to either the supervision intensity or positive adjustment models. Age at first arrest was especially problematic since its correlation with age at the beginning of community supervision was .71 ($p < .001$).

supervision intensity indicators are controlled.

The data also indicate that nonwhites and younger offenders generally performed more poorly than other offenders. Positive effects for the aggregated supervision intensity indicators were also evident. There was weak evidence of a slightly positive effect for the new crime (vs. probation violation) indicator. This result suggests that offenders who were serving a sentence for a new crime were performing slightly better than offenders who were serving time for technical violation.

Within-subject analysis in Louisiana revealed that there were no strong time x fixed predictor interaction effects. The main effect for time was also nonsignificant. This confirms what we have observed in other states, namely, that whatever purely within-subject changes over time were evident were not particularly strong. We conclude that there is no evidence of widespread within-subject variation in positive adjustment over the course of the follow-up period. Several time x supervision intensity interaction terms were evident. These effects, however, are difficult to interpret since they are cumulative. The interactions do indicate that the changes in the effects of supervision intensity indicators are changes of degree rather than changes of kind. We will return to the comparative magnitude of these effects in the next section.

5.4.3 Longitudinal Regression Model

Figure 86 presents the longitudinal regression model in Louisiana. All stability coefficients are positive and statistically significant indicating that subjects who score high on the endogenous variables at any particular point are likely to score high on those variables at subsequent points as well.

As expected from the cross-sectional model above, supervision intensity (all three indicators) at wave 1 is largely predicted by sample membership and the nonwhite indicator. Shock offenders are supervised more intensively while nonwhites are supervised less intensively. Positive adjustment at wave 1 is predicted by a negative effect for the shock sample compared to probationers, prison parolees, and shock dropouts. No effects in subsequent waves eliminate or cancel out this initial negative impact and we conclude, consistent with the repeated measures analysis, that the shock sample does not perform quite as well as the other groups in the study.

Wave 1 analysis also suggests that nonwhites adjust less positively than whites. Cohort effects were statistically significant and reveal that the longer subjects stay in the study the more positively they adjust to life in the community. Supervision intensity analysis indicates the presence of a linear effect for knowledge, requirements, and surveillance index scores. Figure 87 presents the validation charts for the models.

Analysis of supervision intensity at wave 2 reveals that, for all three indicators, the indicator at wave 1 was the driving predictor. On the knowledge index, the scores for the prison sample did not increase as much as they did for the other samples[128] and on the surveillance and requirements index, the scores for the shock sample, did not decrease as much as they did for other samples. Positive adjustment at wave 2 is predicted by a weak negative effect for the shock sample and the nonwhite indicator. Cohort effects were statistically significant in the expected direction as were the effects for the supervision intensity indicators. Validation charts are presented in Figure 88.

Supervision intensity at wave 3 is predicted most effectively by supervision intensity at wave 2. There was a significant direct effect of the nonwhite indicator on the knowledge index (officers knew less about the activities of nonwhites) and a significant direct effect of age at community supervision on the requirements index (older offenders were given fewer requirements) but these effects were not particularly important compared to past values of supervision intensity. The requirements index at wave 3 was significantly predicted by positive adjustment at wave 2 but this was not true of either of the other supervision intensity measures. Because of collinearity problems associated with this particular analysis, we deleted positive adjustment at the earlier wave from this model.[129]

Positive adjustment at wave 3 is directly predicted by supervision intensity, a significant cohort effect, and positive adjustment values at the second wave. All of the supervision intensity indicators are statistically significant and positively related to successful community adjustment. Analysis of cohort effects reveals that offenders who exit the study after wave 3 adjusted significantly less positively than offenders who completed wave 4

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[128]Since increasing scores on the knowledge index imply a decreasing level of knowledge of offender activities, this means that knowledge of activities for prison parolees did not drop off as much between wave 1 and wave 2 as it did for other groups.

[129]This appears to be a collinearity problem. Positive adjustment at wave 2 has a variance inflation factor exceeding 4.0 and is sharing significant variance with the knowledge and surveillance indexes at wave 2 (both of the variance inflation factors for these effects exceed 2.0). Removing positive adjustment from the equation causes the effect of the knowledge index to be statistically insignificant while the surveillance effect becomes positive and statistically significant.

measurement. The fit of these models is assessed with the validation charts in Figure 89.

Wave 4 supervision intensity is primarily a function of wave 3 supervision intensity. On the knowledge index, the analysis revealed that officers reported knowing more about older offenders while the surveillance index was predicted by positive adjustment at wave 3. This latter result appears to be a function of multicollinearity.[130] Consequently, we decided to discard positive adjustment from this equation. The dominant theme in these analyses is the effect of past supervision intensity. Analysis of the positive adjustment construct reveals that none of the treatment groups performs as well as the probation group. The shock group actually performs significantly worse than the probation group. The analysis also reveals that nonwhites do not perform as well as whites and older offenders perform better than younger offenders. Supervision intensity indicators at wave 4 are all positively associated with successful adjustment. Wave 4 validation charts are presented in Figure 90.

Figure 91 conveys the effects of the knowledge index over the cross-sectional and longitudinal models while Figures 92 and 93 present comparable comparisons for the surveillance and requirements indexes, respectively. A review of Figures 91-93 also suggests that there is no substantively different effects by time. Differences that appear in the graph are clearly differences in degree rather than differences in kind. In sum, the Louisiana data indicate that more intensive supervision generally leads to more positive adjustment.

5.4.3 Summary

The cross-sectional and longitudinal models in Louisiana lead to similar conclusions. There was a weak negative effect of shock incarceration relative to all of the other groups. This effect emerged in both the repeated measures and longitudinal regression models. Although the negative effects were not large, they were stable over time. It seems reasonable to conclude that net of the other effects, membership in the shock sample was neither particularly helpful nor harmful.

The effects of supervision intensity were generally positive. and the fixed effects influence positive adjustment in ways that would

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[130]This result appears to be a function of the collinearity between the knowledge index and positive adjustment at wave 3. When either variable is taken out of the surveillance equation, the other becomes nonsignificant. The variance inflation factor for the knowledge index is 1.97 and, for positive adjustment, the variance inflation factor is over 2.56.

be reasonably expected given our analysis so far. Nonwhites tended to adjust less positively while older offenders tended to adjust more positively. Cohort effects were also statistically significant and suggested that early attrition was associated with lower positive adjustment scores.

5.5 New York

5.5.1 Cross-Sectional Model

The New York cross-sectional model is presented in Figure 94.[131] The results indicate that positive adjustment to community supervision is not significantly associated with participation in the shock incarceration program. Although the estimated regression coefficient compares the shock group to the prison parolees and the coefficient for the dropouts is negative, the contrast between the shock group and the dropout group is only marginally significant ($p < .11$).

Nonwhite offenders tended to adjust less positively than white offenders while offenders who were older at the age of their first arrest tended to adjust more positively than offenders who were younger at their first arrest.[132] Offenders who committed violent

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[131]As noted previously, supervision intensity data were not available in New York. From what we have observed in other states, omitted variable bias is a possible source of error in this analysis that cannot be ruled out. To the extent that supervision intensity is related to other predictor variables *and* positive adjustment the results presented in this section will be biased (incorrect on the average sample) and inconsistent (they will not become more correct as the sample size increases).

[132]When age at the beginning of community supervision and age at first arrest are both included in the models simultaneously, neither effect is statistically significant. If we include age at the beginning of community supervision in the model, the effect for the shock sample is statistically significant. Since we consistently have information on age at community supervision in every state and age at first arrest was not strongly predictive of positive adjustment in Louisiana and South Carolina, our first inclination was to include age at community supervision in the New York model and eliminate the age at first arrest effect. Doing this, however, would have led us to different conclusions because age at first arrest, as we noted in the descriptive analysis section, is significantly larger in the shock sample than in the other groups. Since offenders who are younger at their first arrest tend not to adjust as well in New York, the statistically significant effect for the shock sample when age at community

offenses and "other" offenses tended to adjust significantly more positively than offenders who committed property offenses although their performance, on average, was not significantly different from that of the drug offender group. Cohort effects, as in Florida and Louisiana, suggest that offenders who persisted in the study tended to adjust more positively than offenders who exited early. Figure 95 presents the validation charts for this model.

5.5.2 *Repeated Measures Analysis*

Repeated measures analyses are presented in Tables 92-95. The results indicate that the shock graduate sample is marginally outperforming the shock dropout sample ($p < .05$) at the first wave but it is not performing differently than the prison parolee sample. At later waves, things become more ambiguous. The general theme in Tables 92-95, however, is that the shock sample is doing marginally better than the dropout sample and not significantly different than the prison parolees. The difference is statistically significant at some periods but not at others. These marginal changes in the value of the test statistic were not large enough to create a statistically significant time x sample interaction effect. It seems reasonable to conclude, therefore, that the shock graduates are certainly not doing worse than the other groups and, compared to the dropouts, they may be doing a little better.

Within-subjects analysis indicates that the nonwhite indicator and age at first arrest are quite unimportant factors at the beginning of the follow-up period. During the other follow-up periods, however, these effects are statistically significant.[133] The regression coefficients indicate that nonwhites and younger offenders tend not to perform as well as other subjects. Offense type and prior criminal history exerted no independent effects on positive adjustment. Within-subjects analysis revealed a stronger case for over-time change (i.e., a significant main effect for time in the six month analysis: Table 93) but these effects were substantially weaker in the nine and twelve month analyses.

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supervision is partially attributable to omitted variable bias when age at first arrest is not controlled. Controlling for age at first arrest reduces the effect of shock incarceration considerably.

[133]This result is confirmed by the presence of statistically significant time x nonwhite and time x age at first arrest interaction effects in the two-, three-, and four-wave analyses.

5.5.3 Longitudinal Regression Model

The four waves of data in New York (Figure 96) reveal relatively strong stability coefficients. This result again suggests most of the variation in positive adjustment at any given point is reliable from the previous point. Moreover, subjects who have high positive adjustment scores at time *t-1* are likely to have high scores at time *t*. Since supervision intensity is not available in New York, the results of the regression analyses in this section will parallel closely the repeated measures results described above. The longitudinal regression models, however, include the temporally prior versions of positive adjustment in the models at waves 2, 3, and 4 and the exit cohort dummy variables at waves 1, 2, and 3.

At the first wave, the shock sample does not significantly outperform the prison sample although it does outperform the shock dropout sample ($p < .05$). In remaining waves, the coefficients for the shock dropout group vary somewhat but they are never strong enough to put the dropouts at a higher average positive adjustment score than the shock graduates. There seems to be sufficient evidence to conclude that the shock graduates are marginally outperforming the shock dropouts although there is no evidence that they are outperforming the prison parolees.

In addition, the analysis reveals that the negative effect for the nonwhite indicator is strongest at waves 2 and 3.[134] The positive effect of age is strongest at wave 4 but also exerts a weak effect at the first wave. The effect of prior record is negative and statistically significant at wave 4 only. Cohort effects were strong and statistically significant at each of the first three waves of the analysis. Figure 97 portrays the validation charts for each model.

5.5.3 Summary

Initial analysis of the data in New York provide some support for the contention that shock incarceration is positively related to successful adjustment in the community. In particular, when age at first arrest is not controlled, the effect of shock incarceration on positive adjustment is statistically significant. Since shock offenders tend to be older at their first arrest, however, and offenders who are older at their first arrest adjust more positively, this initial effect of shock incarceration appears to be spurious. There is also evidence in New York, as in other states, that demographic characteristics (e.g., race and age) have a role to play in the explanation of positive adjustment. There is little evidence that offending characteristics such as offense type

[134]This is consistent with the repeated measures results described earlier.

and criminal history have effects on positive adjustment during community supervision.

5.6 South Carolina

5.6.1 Cross-Sectional Model

Figure 98 presents the results of the cross-sectional model in South Carolina.[135] The data indicate that there is no difference in treatment sample performance during community supervision net of the covariates. The analysis does reveal that nonwhites adjust less positively than whites and that property and "other" offenders adjust less positively than offenders serving sentences for violent crimes. The presence of a prior arrest and/or conviction also detracted from positive adjustment. Supervision intensity was curvilinearly related to cross-sectional positive adjustment but cohort effects were not evident. Figure 99 presents the validation results for these models.

5.6.2 Repeated Measures Model

The South Carolina repeated measures models are presented in Tables 96-99. The results suggest that treatment sample is unrelated to positive adjustment.[136] There is only limited support for effects of age and criminal history characteristics. The data reveal that nonwhites consistently adjust less positively than whites. Within-subjects analysis indicates a nonsignificant main effect for time and no substantively important time x predictor variable interaction effects. As in the other states, it seems reasonable to conclude that patterns of within-subject change are weak and inconsistent in South Carolina.

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[135]Age at first arrest was unrelated to positive adjustment controlling for age at the beginning of community supervision. Given that the correlation coefficient for these two variables is +.63 ($p < .001$), we omit age at first arrest from the analysis.

[136]The one exception to this conclusion is in the fourth wave of the analysis where prison parolees performed significantly better than shock sample and the probationers at the $p < .10$ level. This effect, not evident in earlier waves, generates a statistically significant time x sample interaction term in the four wave repeated measures model. When we move to the panel specification, however, no evidence of this effect is evident.

5.6.3 Longitudinal Regression Model

The results of the longitudinal analysis are depicted in Figure 100.[137] Stability coefficients are consistently positive and statistically significant. The analysis suggests that at the first wave, primary contact levels were significantly lower for the probation sample than for all other groups. Younger offenders also tended to be supervised more intensively at the outset of community supervision. Interestingly, offenders in cohort 1 were supervised at lower levels than offenders in the other cohorts. Perhaps not coincidentally, these offenders were on the verge of dropping out of the study at the first wave. This is similar to the finding we observed in some of the intermediate waves in Florida.

Positive adjustment at wave 1 is not predictable from treatment sample. It is predictable from race (nonwhites adjust less positively), age (older offenders adjust more positively) and offender characteristics (offenders with new crimes did better than offenders serving sentence for a technical violation; offenders with prior records did more poorly than offenders without prior records). Supervision intensity is curvilinearly related to positive adjustment at the first wave while cohort effects were not statistically significant. Figure 101 presents the validation charts for these models.

Supervision intensity at the second wave is largely predictable from supervision intensity at the first wave (83% of the variation in supervision intensity is stable over the first two time periods). As in the first wave, however, there is a cohort effect. Cohort 2 offenders, which is on the verge of dropping out of the study at wave 2, were supervised significantly less intensively at wave 2 than were their counterparts in cohorts 3 and 4. Positive adjustment at wave 1 also exerted a weak positive effect on supervision intensity at wave 2.

Positive adjustment at wave 2 continues to be negatively associated with the nonwhite indicator. Moreover, property and "other" offenders performed significantly worse than violent and drug offenders controlling for other predictors at wave 2. A relatively weak negative effect for membership in cohort 2 is also evident along with a quadratic effect for supervision intensity. Figure 102 depicts the validation assessment for the wave 2 models.

Wave 3 analysis indicated that supervision intensity at wave 2 was the sole statistically significant predictor of primary contact levels in the third wave. Analysis of positive adjustment at wave 3 continued to reveal a negative effect for the nonwhite indicator as well as a negative effect for membership in cohort 3. The

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[137]We reiterate here that longitudinal data were not available for the DOC shock sample in South Carolina.

quadratic effect for primary contact levels also continues to be statistically significant at the third wave. Figure 103 presents the validation work for the wave 3 models.

Analysis of the wave 4 supervision intensity data suggested a positive effect of an offender's having committed a violent offense on primary contact levels (although the result is only marginally significant). Supervision intensity at wave 3 was the dominant predictor in this model, however. Positive adjustment at wave 4 continued to be negatively associated with the nonwhite indicator. The data at wave 4 also suggest that violent and drug offenders outperformed property and "other" offenders while offenders with prior criminal histories tended not to do as well as offenders that had no prior arrest or conviction. Supervision intensity continued to be quadratically related to positive adjustment. Figure 105 reveals the validation assessment for the wave 4 models.

Figure 106 presents the partialed effects of supervision intensity for the cross-sectional analysis and the longitudinal study. These results indicate once again that there is less gain in positive adjustment per unit increase in contacts beyond 1.8 to 2.0 contacts per month. The curves suggest a diminishing returns function that is consistent with the results observed in Florida and Georgia.

5.6.4 Summary

The results of the analysis in South Carolina support two relatively consistent themes in the analyses across the states. First, the data suggest that there is no effect of shock incarceration on positive adjustment to community supervision. Second, the effect of supervision intensity on positive adjustment appears to be curvilinear. The inflection in the curves is evident at about 1.8 to 2.0 contacts per month and is very similar to the expected distributions observed in Florida and Georgia. The South Carolina data also reinforce other persistent findings: nonwhites tend not to adjust as well as whites while older offenders tend to adjust more positively than younger offenders. Moreover, the South Carolina analysis indicated that violent and drug offenders tend to outperform property offenders while subjects with prior records did not adjust as well as their counterparts who had never been arrested or convicted.

6. Discussion and Conclusions

In this analysis, we sought to determine whether shock incarceration programs impart unique qualities to offenders that help them adjust more positively to community supervision. Utilizing a quasi-experimental design that compared shock incarceration programs to other correctional programs, such as probation and prison, the provided no support for the contention that shock program graduates perform dramatically better. Instead, the common denominator appears to be that the shock programs do at least no worse than the other correctional options we examined.

Early in the analysis we made several observations about distributions of key variables in our analyses. Most prominent among these was the finding of a relatively uniform decrease in both positive adjustment scores and levels of supervision intensity over time. Further analysis revealed that these declines were evident in all states (to some degree) and that they tended to be relatively consistent across sample categories within states. The strength of these trends is questionable, however. Further analysis indicated that the raw within-subject change on these variables was relatively small. But another analysis of the linkages between within-subject change for supervision intensity and within-subject change for positive adjustment revealed that the two are indeed linked at the individual level as well as cross-sectionally. As supervision intensity scores declined or increased, positive adjustment scores tended to move in the same direction. Since our analysis finds overall evidence of decline in both variables over time, limited though it is, there is reassurance in the finding that changes in them are linked within subjects.

Analysis of predictor variable distributions across the sample categories reveals significant cause for concern. In every state, important differences in the samples were uncovered with the small number of study variables that were available to us. This leads us to wonder about important differences in the samples on study variables that were not available to us. To the extent that there are sample differences and positive adjustment differences on those omitted variables, the results of our analysis will be biased.

The results in Florida provide the only reasonably strong evidence for a positive effect of the shock program. Indeed, if the results in Florida had been observed in the other states, our conclusions regarding the effect of the shock incarceration experience would have been substantially altered. Shock graduates in Florida achieved significantly higher positive adjustment scores than both prison parolees and offenders who dropped out of the shock program. This pattern tends to take root at the outset of community supervision and it persists throughout the one-year follow-up period. Shock offenders also do better than other offenders regardless of whether key variables such as offense type and supervision intensity are controlled. Indeed, the shock offenders do better in the presence of these controls than in their absence.

Analysis of the data in Louisiana, Georgia, New York, and South Carolina, however, suggested a less impressive effect for shock incarceration. In Louisiana, what appeared at first to be a lift in positive adjustment scores that could be attributed to the shock program, disappeared when supervision intensity indicators were controlled. When demographic characteristics and supervision intensity were controlled in our over-time models, there was evidence that the shock program graduates did not perform quite as well as the other groups.

Similarly, higher than average positive adjustment scores among New York shock program graduates diminished considerably when demographic and criminal history characteristics were controlled. Although the shock graduates still appeared to do marginally better than a comparison group of shock dropouts, they did not do better than a comparison group of prison parolees. The absence of a control for supervision intensity in these data is problematic. In every state studied in this paper, supervision intensity and positive adjustment scores were related to each other. We have no reason to believe that these same relationships are not present in New York. Supervision intensity and sample membership are also related in New York since shock offenders are required to spend at least the first six months of their parole period in intensive supervision. The omission of this variable means that our equations in New York are inherently misspecified and we draw our conclusions from the analysis of the New York data with caution.

The data in Georgia and South Carolina are more definitive. Regardless of the analysis or the variables included in whatever model, the graduates of the shock programs in these two states did not have significantly higher positive adjustment scores.

Reconciling the divergent finding in Florida with these results is not an easy task. One possibility is that unique qualities associated with the Florida program are responsible for more positive offender behavior after graduation. Such a suspicion would not be at odds with the available evidence. The results of a study of attitude changes in shock programs indicated that shock graduates in Florida improved their scores on antisocial attitude scales to a greater degree than shock graduates in other states (MacKenzie and Souryal, 1993). Another study focusing on recidivism patterns found that shock graduates *and* dropouts in Florida outperformed prison parolees on failure rates and time to failure (Souryal and MacKenzie, 1993). The Florida program did not emphasize treatment as much as some programs (e.g., Louisiana and New York) but did devote more time to rehabilitative activities than others (e.g., Georgia).

The data also indicate that the Florida program (and the comparison groups in Florida) were comprised of offenders who were relatively unlikely to have a prior arrest or conviction. This stands in contrast to the analyses we conducted in the other states which indicated that the majority of offenders did have a prior arrest or conviction record. Perhaps the Florida shock program exerts unique effects when prior record is held constant in this fashion. Given the small number of offenders with prior records in this analysis, our controlling for prior record made no difference in any of the results. In fact, its effect was weaker than that of the new crime indicator and we, therefore, did not include it in our final Florida models. Whether there are important effects associated with the Florida program when first-time offenders are emphasized, however, seems to be a useful next question emerging from this research.

There are other possibilities, too. The Florida program, as we noted earlier, has the highest attrition rate of any shock program that was studied in this analysis. Indeed, termination rates have approached and exceeded 50% in the past (see administrative summary or MacKenzie and Souryal (1993)). Such a high termination rate compels the question of whether shock graduates in Florida represent an atypical group of offenders. The unobservable (in these data) propensity to successfully complete a high attrition program such as the one in Florida may actually be the variable or a good indicator of the variable that drives positive adjustment during community supervision. We have evidence from the repeated measures analysis in Florida that this result is plausible. When the analysis was restricted to offenders who completed the entire one-year follow-up period, no effects for sample membership were evident. Thus, among offenders with a propensity to persist in the community supervision program, positive adjustment does not vary by sample category.

The results in Florida notwithstanding, the preponderance of the data, in this set of analyses, clearly support the conclusion that shock incarceration has little if any effect on positive adjustment during community supervision.

Although not originally the focal point of our study, the analysis did reveal that there is a generally positive relationship between supervision intensity and successful adjustment during community supervision. The estimation of these effects, in Florida, Georgia, and South Carolina (where offender contacts were used to measure intensity), revealed that they were nonlinear. The shape of the partial regression functions were quite similar across these three states. The usual pattern was a monotonically increasing function up to a medium level of supervision (around 1.8 to 2.0 monthly contacts in Florida, Georgia, and South Carolina). There was usually a consistent "leveling off" effect up to very high levels of supervision intensity. The regression functions were erratic at these very high levels of supervision intensity. In some analyses, the curve resumed an upward trajectory while in others, the function curved downward. Since the third degree polynomial is a relatively complicated function and the third degree is based on a very small number of cases, we do not emphasize the specification in the upper regions of the function. The "leveling off" pattern, however, is pronounced and appears in some form or another in every state. The consistency with which the pattern appears is a strong indicator of the possibility that increasing supervision intensity does not lead to consistent corresponding increases in expected values of positive adjustment.

Finally, our analysis examined the effects of a number of other predictor variables on positive adjustment. The results indicated that nonwhites tended not to adjust as well as whites. Offenders who were older at the beginning of community supervision tended to adjust more positively than younger offenders. In most of the analyses, violent and drug offenders performed better than property offenders while offenders with evidence of prior trouble (including a confirmed criminal history or serving a current sentence for a technical violation of community supervision conditions) tended not to adjust as well as their counterparts with a less problematic past.

The common-denominator conclusion from these analyses thus appears to be that shock programs, by themselves, do not appear to instigate dramatic improvements in offender adjustment during community supervision. In fact, the analysis provides no solid evidence that any particular comparison group outperformed other comparison groups. Although increased supervision intensity is associated with positive adjustment, the relationship, in these data appears to be nonlinear. Since supervision is, by definition, labor-intensive, future research should examine whether there is truly a "diminishing-returns" effect.

If one of the criteria for shock incarceration programs is that they induce incremental improvement in offender behavior then these shock programs largely did not measure up. On the other hand, the true value of these programs may not rest in their ability or observed propensity to modify behavior over the short term. Rather, changes in policy that lead to consistent and predictable correctional responses to offender behavior may yield positive results over the long term. Such programs may, therefore, be beneficial for their ability to increase the certainty of punishment. This is distinct from enhancing the correctional system's ability to increase the therapeutic value of its programs or the severity of punishment (Petersilia and Turner, 1993; Gowdy, 1993). Moreover, expectations for what programs can accomplish should be tempered with a certain pessimism about the potential for modifying behavior in very short periods of time under tight fiscal and manpower constraints (Souryal and MacKenzie, 1993).

In previous analyses, researchers have found that the shock incarceration experience may reduce anti-social attitudes

(MacKenzie and Souryal, 1993). But other research suggests that shock incarceration, like other intermediate sanctions, does not appear to lead to strong stable reductions in recidivism and other negative activities. Thus, the short-term improvement in attitudes that these programs appear to induce do not appear to translate into improved offender behavior. Although, more research needs to be conducted, the data acquired to date have not revealed dramatic improvements on these criteria that can be attributed to the use of intermediate sanctions. This analysis tends to confirm, rather than contradict, the existing data.

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LIST OF TABLES

Table 1
State Sample Frequency Distributions

State & Sample	Number of Cases	Percent of Total
Florida		
Shock Graduates	112	38.8%
Shock Dropouts	68	23.5%
Prison Parolees	109	37.7%
Total	289	100.0%
Georgia		
Shock Graduates	79	30.2%
Prison Parolees	98	37.4%
Probationers	85	32.4%
Total	262	100.0%
Louisiana		
Shock Graduates	77	27.7%
Prison Parolees	74	26.6%
Probationers	111	39.9%
Shock Dropouts	16	5.8%
Total	278	100.0%
New York		
Shock Graduates	94	32.9%
Shock Dropouts	97	33.9%
Prison Parolees	95	33.2%
Total	286	100.0%
South Carolina		
DPPPS Shock Graduates	85	26.1%
DOC Shock Graduates	84	25.8%
Prison Parolees	64	19.6%
Probationers	69	21.2%
Split-Probationers	24	7.4%
Total	326	100.0%

Table 2
Items and Overall Means For Positive Adjustment Construct i
Florida, Georgia, New York, and South Carolina

Positive Adjustment Items

Procedure: Increment the index by 1 for each applicable item, sum the items and divide by the total number of completed items (if at least eight items were evaluated).

During this period was the offender:

1. Employed, enrolled in school, or participating in a training program for more than 50% of the follow-up period.
2. Held any one job (or continued in educational or vocational program) for more than a three month-period during the follow-up.
3. Attained vertical (upward) mobility in employment, educational, or vocational program.
4. For the last half of follow-up period, individual was self-supporting and supported any immediate family.
5. Individual shows stability in residency. Either lived in the same residence for three months or moved at suggestion or with the agreement of supervising officer.
6. Individual has avoided any critical incidents that sho instability, immaturity, or inability to solve problems acceptably.
7. Attainment of financial stability. This is indicated by the individual living within his means, opening bank accounts, or meeting debt payments.
8. Participation in self-improvement programs. These could be vocational, educational, group counseling, alcohol, or drug maintenance programs.
9. Individual making satisfactory progress during community supervision period. This could be moving downward in levels of supervision or obtaining final release within period.
10. No illegal activities on any available records during the follow-up period.

Descriptive Statistics

State	N=	Median	Mean	S.D.
Florida	280	0.35	0.38	0.27
Georgia	246	0.41	0.42	0.24
New York	237	0.55	0.51	0.30
South Carolina	326	0.50	0.46	0.29

Table 3
Items and Descriptive Statistics For Overall Positive Adjustment Construct in Louisiana (N=278)

Positive Adjustment Items

Procedure: Increment the index by 1 for each applicable item, sum the items and divide by the total number of completed items (if at least fourteen items were evaluated).

1. Is subject working full-time or part-time?
2. Is employer's evaluation of subject favorable?
3. Subject required to attend Alcoholics' Anonymous and is making satisfactory progress.
4. Subject required to attend drug treatment program and is making satisfactory progress.
5. No positive alco-sensor tests.
6. No positive drug screens.
7. Subject actively pursuing training or education and making satisfactory progress.
8. No difficulties in family relationships.
9. Subject is avoiding relationships with delinquent peer groups.
10. Attitude or appearance is satisfactory.
11. Subject is compliant and cooperative.
12. Subject has met curfews, provided information on whereabouts, has not missed appointments, has not lied to officer.
13. Subject accepts responsibility for actions.
14. Community supervisor evaluation is satisfactory.
15. Subject displays evidence of emotional stability.
16. Subject is making a successful adjustment.
17. Subject is doing better than officer evaluation might otherwise indicate.
18. Subject has not been arrested during this follow-up period.

Construct Descriptive Statistics

Median =	0.433
Mean =	0.438
S.D. =	0.146

Table 4: Positive Adjustment Item Means (Averaged Across All Available Measurement Periods For Each Subject) In Florida, Georgia, New York, South Carolina, and Louisiana[a]

Item #	FL	GA	NY	SC
Instrument 1 (Table 2)				
1	.415	.447	.589	.472
2	.362	.430	.357	.497
3	.181	.187	.094	.252
4	.354	.393	.480	.478
5	.648	.624	.801	.687
6	.438	.552	.572	.502
7	.278	.378	.447	.431
8	.203	.174	.524	.234
9	.381	.408	.558	.493
10	.516	.575	.675	.554
Range of Valid N=	279-282	245-246	237	325-326

Instrument 2 (Table 3; LA Subjects Only: N=278)

Item	Mean	Item	Mean
1	.567	10	.877
2	.188	11	.746
3	.117	12	.707
4	.144	13	.697
5	.130	14	.126
6	.143	15	.741
7	.032	16	.647
8	.629	17	.155
9	.397	18	.965

[a]Note: Items are described in Table 2 for FL, GA, NY, and SC. Table 3 describes items used in Louisiana. For the computation of these means, there was a maximum of four measurement periods in all states except Louisiana where there was a maximum of twelve measurements.

Table 5A
Reliability Analysis of Linear Positive Adjustment Scale by State: Florida, Georgia, New York, and South Carolina

Follow-Up Point	Mean Item-Total r	Cronbach's α
<u>Florida</u>		
Month 3	.5121	.8263
Month 6	.5539	.8527
Month 9	.6018	.8777
Month 12	.5246	.8340
<u>Georgia</u>		
Month 3	.5139	.8281
Month 6	.5571	.8564
Month 9	.5748	.8659
Month 12	.5887	.8720
<u>New York</u>		
Month 3	.5822	.8712
Month 6	.6410	.8972
Month 9	.6132	.8817
Month 12	.6610	.9048
<u>South Carolina</u>		
<u>DPPPS Shock, Prison, Probation and Split-Probation Samples</u>		
Month 3	.5391	.8401
Month 6	.5985	.8769
Month 9	.5788	.8666
Month 12	.6252	.8893
<u>DOC Shock Sample</u>		
Month 12	.5870	.8705

Table 5B
Reliability Analysis of Linear Positive Adjustment Scale
Lousisiana

Follow-Up Point	Mean Item-Total r	Cronbach's α
Month 1	.3795	.8059
Month 2	.3667	.7958
Month 3	.3693	.7929
Month 4	.4110	.8303
Month 5	.4069	.8237
Month 6	.3741	.8040
Month 7	.3862	.8172
Month 8	.3835	.8196
Month 9	.3609	.7917
Month 10	.3439	.7808
Month 11	.3601	.8015
Month 12	.3979	.8473

Table 6
Descriptive Statistics For Monthly Contact Variables In Florida, Georgia, and South Carolina[a]

State[b]	Descriptive Statistics
Florida	
Primary Contacts	
N=	276
Range	0.00 to 64.75
Median	2.25
Mean	6.62
Standard Deviation	9.53
Secondary Contacts	
N=	276
Range	0.00 to 17.78
Median	1.35
Mean	2.83
Standard Deviation	3.26
Georgia	
Primary Contacts	
N=	241
Range	0.00 to 30.00
Median	1.82
Mean	2.55
Standard Deviation	3.07
South Carolina	
Primary Contacts	
N=	310
Range	0.00 to 12.67
Median	1.50
Mean	1.73
Standard Deviation	1.40

[a]Note: Contact data are averaged over all available measurement periods for each offender.
[b]Note: Contact data were not available in New York.

Table 7
Supervision Intensity Indexes in Louisiana (N=278)

Items and Descriptive Statistics

Knowledge Index:

Each response of "don't know" causes the index to increment by one unit:

1. why subject isn't working (w8)
2. whether subject is going to school (e1)
3. whether having difficulty with family (i2)
4. whether spending time with other offenders (i7)
5. where subject is getting financial support (mis1)
6. whether subject has physical health problems (mis2)
7. whether subject shows signs of emotional instability (mis3)
8. whether subject shows signs of mental health problems (mis4)

Range=8; Median=1.0; Mean=1.24; Std. Dev.=1.13

Requirements Index:

Each imposed requirement causes the index to increment by one unit. Is subject required to:

1. attend AA (sa1)
2. attend drug treatment (sa2)
3. keep a curfew (pr4)
4. keep agent informed of activities (pr6)
5. make monthly appointments (pr8)
6. pay restitution (pr14)

Range=5.86; Median=2.61; Mean=2.72; Std. Dev.=1.11

Surveillance Index:

Each reported contact increments the index by one unit:

1. was employer contacted (w7)
2. was teacher/administrator contacted (e3)
3. was community service supervisor contacted (pr13)
4. was subject tested for alcohol (sa4)
5. was subject tested for drugs (sa6)

Range=4; Median=0.39; Mean=0.71; Std. Dev.=0.84

Table 8a
Study Attrition Sources by Treatment Sample in Florida

Treatment Sample & Source	Frequency	Percent
Total Number of Cases	289	100.0%[a]
Shock Sample	112	38.8%
Dropout Sample	68	23.5%
Prison Parolee Sample	109	37.7%
Completed 4 Measurement Periods	58	20.1%[b]
Shock Sample	21	18.8%
Dropout Sample	6	8.8%
Prison Parolee Sample	31	28.4%
Revoked For A New Crime	49	17.0%[b]
Shock Sample	15	13.4%
Dropout Sample	11	16.2%
Prison Parolee Sample	23	21.1%
Revoked For A Technical Violation	28	9.7%[b]
Shock Sample	6	5.4%
Dropout Sample	13	19.1%
Prison Parolee Sample	9	8.3%
Absconding, Jail, Case Pending, Or Arrest	83	28.7%[b]
Shock Sample	37	33.0%
Dropout Sample	17	25.0%
Prison Parolee Sample	29	26.6%
Other/Unknown	71	24.6%[b]
Shock Sample	33	29.5%
Dropout Sample	21	30.9%
Prison Parolee Sample	17	15.6%

[a]Percentages are the treatment samples' respective contributions to the total sample size.
[b]Percentages represent the share of the treatment sample that experiences the attrition event.

Table 8b
Study Attrition Sources by Treatment Sample in Georgia

Treatment Sample & Source	Frequency	Percent
Total Number of Cases	262	100.0%[a]
Shock Sample	79	30.2%
Prison Parolee Sample	98	37.4%
Probation Sample	85	32.4%
Completed 4 Measurement Periods	63	24.1%[b]
Shock Sample	26	32.9%
Prison Parolee Sample	25	25.5%
Probation Sample	12	14.1%
Legally Released	9	3.4%[b]
Shock Sample	0	0.0%
Prison Parolee Sample	7	7.1%
Probation Sample	2	2.4%
Revoked For New Crime	34	13.0%[b]
Shock Sample	15	19.0%
Prison Parolee Sample	14	14.3%
Probation Sample	5	5.9%
Revoked For Technical Violation	7	2.7%[b]
Shock Sample	4	5.1%
Prison Parolee Sample	0	0.0%
Probation Sample	3	3.5%
Absconding Violation	9	3.4%[b]
Shock Sample	2	2.5%
Prison Parolee Sample	1	1.0%
Probation Sample	6	7.1%
Unknown/Other	140	53.4%[b]
Shock Sample	32	40.5%
Prison Parolee Sample	51	52.0%
Probation Sample	57	67.1%

[a]Percentages are the treatment samples' respective contributions to the total sample size.
[b]Percentages represent the share of the treatment sample that experiences the attrition event.

Table 8c
Study Attrition Sources by Treatment Sample in Louisiana

Treatment Sample & Source	Frequency	Percent
Total Number of Cases	278	100.0%[a]
Shock Sample	77	27.7%
Prison Parolee Sample	74	26.6%
Probation Sample	111	39.9%
Shock Dropout Sample	16	5.8%
Completed 4 Measurement Periods	178	64.0%[b]
Shock Sample	45	58.4%
Prison Parolee Sample	38	51.4%
Probation Sample	86	77.5%
Shock Dropout Sample	9	56.3%
Legally Released	35	12.6%[b]
Shock Sample	10	13.0%
Prison Parolee Sample	20	27.0%
Probation Sample	3	2.7%
Shock Dropout Sample	2	12.5%
Revoked For Any Reason	15	5.0%[b]
Shock Sample	5	6.5%
Prison Parolee Sample	4	5.4%
Probation Sample	5	4.5%
Shock Dropout Sample	1	6.3%
Jailed	31	11.2%[b]
Shock Sample	11	14.3%
Prison Parolee Sample	8	10.8%
Probation Sample	9	8.1%
Shock Dropout Sample	3	18.8%
Absconding	12	4.3%[b]
Shock Sample	2	2.6%
Prison Parolee Sample	4	5.4%
Probation Sample	5	4.5%
Shock Dropout Sample	1	6.3%
Other/Unknown	7	2.5%[b]
Shock Sample	4	5.2%
Prison Parolee Sample	0	0.0%
Probation Sample	3	2.7%
Shock Dropout Sample	0	2.5%

[a]Percentages are the treatment samples' respective contributions to the total sample size.
[b]Percentages represent the share of the treatment sample that experiences the attrition event.

Table 8d
Study Attrition Sources by Treatment Sample in New York

Treatment Sample & Source	Frequency	Percent
Total Number of Cases	286	100.0%[a]
Shock Sample	94	32 9%
Dropout Sample	97	33.9%
Prison Parolee Sample	95	33.2%
Completed 4 Measurement Periods	133	46.5%[b]
Shock Sample	52	55.3%
Dropout Sample	35	36.1%
Prison Parolee Sample	46	48.4%
Revoked For A New Crime	26	9.1%[b]
Shock Sample	6	6.4%
Dropout Sample	10	10.3%
Prison Parolee Sample	10	10.5%
Revoked For A Technical Violation	40	14.0%[b]
Shock Sample	5	5.3%
Dropout Sample	22	22.7%
Prison Parolee Sample	13	13.7%
Absconding	43	15.0%[b]
Shock Sample	17	18.1%
Dropout Sample	14	14.4%
Prison Parolee Sample	12	12.6%
Arrest	42	14.7%[b]
Shock Sample	13	13.8%
Dropout Sample	15	15.5%
Prison Parolee Sample	14	14.7%
Legal Release	2	0.7%[b]
Shock Sample	1	1.1%
Dropout Sample	1	1.0%
Prison Parolee Sample	0	0.0%

[a]Percentages are the treatment samples' respective contributions to the total sample size.
[b]Percentages represent the share of the treatment sample that experiences the attrition event.

Table 8e
Study Attrition Sources by Treatment Sample in South Carolina

Treatment Sample & Source	Frequency	Percent
Total Number of Cases	326	100.0%[a]
DPPPS Shock Sample	85	26.1%
DOC Shock Sample	84	25.8%
Prison Parolee Sample	64	19.6%
Probation Sample	69	21.2%
Split-Probation Sample	24	7.4%
Completed 4 Measurement Periods	165	68.2%[b]
DPPPS Shock Sample	62	72.9%
Prison Parolee Sample	37	57.8%
Probation Sample	47	68.1%
Split-Probation Sample	19	79.2%
Revoked For A New Crime	16	6.6%[b]
DPPPS Shock Sample	4	4.7%
Prison Parolee Sample	4	6.3%
Probation Sample	6	8.7%
Split-Probation Sample	1	4.2%
Revoked For A Technical Violation	12	5.0%[b]
DPPPS Shock Sample	7	8.2%
Prison Parolee Sample	2	3.1%
Probation Sample	3	4.4%
Split-Probation Sample	1	4.2%
Absconding, Jail, Case Pending, or Arrest	20	8.3%[b]
DPPPS Shock Sample	3	3.5%
Prison Parolee Sample	11	17.2%
Probation Sample	5	7.2%
Split-Probation Sample	1	4.2%
Other/Unknown	29	12.0%[b]
DPPPS Shock Sample	9	10.6%
Prison Parolee Sample	10	15.6%
Probation Sample	8	11.6%
Split-Probation Sample	2	8.3%

[a]Percentages are the treatment samples' respective contributions to the total sample size.
[b]Percentages represent the share of the treatment sample that experiences the attrition event.

Table 9
Mean Number of Study Periods Completed By Sample For Each State

State and Sample	N=	Mean
Florida		
Shock Graduates[a]	112	2.277
Shock Dropouts[b]	68	1.691
Prison Parolees[a]	109	2.532
Total	289	2.235
$F_{(2,286)} = 10.44;\ p < .001$		
Georgia		
Shock Graduates[a]	79	2.418
Prison Parolees[a]	98	2.194
Probationers[a]	85	2.047
Total	262	2.214
$F_{(2,259)} = 1.39;\ p < .250$		
Louisiana		
Shock Graduates[a,b]	77	3.234
Prison Parolees[b]	74	3.081
Probationers[a]	111	3.622
Shock Dropouts[b]	16	3.000
Total	278	3.335
$F_{(3,274)} = 5.76;\ p < .001$		
New York		
Shock Graduates[a]	94	2.798
Shock Dropouts[b]	97	2.124
Prison Parolees[a,b]	95	2.442
Total	286	2.451
$F_{(2,283)} = 4.20;\ p < .016$		
South Carolina		
DPPPS Shock Graduates[a]	85	3.541
Prison Parolees[a]	64	3.344
Probationers[a]	69	3.420
Split-Probationers[a]	24	3.542
Total	242	3.455
$F_{(3,238)} = 0.67;\ p < .568$		

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 10
Joint Sample by Exit Cohort Distributions Within States[a]

State and Sample[c]	Exit Cohort[b] Missing	1	2	3	4
<u>Florida</u> $\phi^2 = .010;\ p < .001$					
Shock Graduates	.107	.179	.232	.295	.188
Shock Dropouts	.132	.338	.324	.118	.088
Prison Parolees	.087	.358	.368	.369	.535
Total	.080	.232	.263	.225	.201
<u>Georgia</u> $\phi^2 = .092;\ p < .002$					
Shock Graduates	.101	.190	.228	.152	.329
Prison Parolees	.245	.082	.163	.255	.255
Probationers	.177	.212	.141	.329	.141
Total	.179	.157	.178	.248	.241
<u>Louisiana</u> $\phi^2 = .113;\ p < .002$					
Shock Graduates	.000	.065	.221	.130	.584
Prison Parolees	.000	.081	.270	.135	.514
Probationers	.009	.036	.054	.126	.775
Shock Dropouts	.063	.125	.125	.125	.563
Total	.007	.061	.162	.130	.640
<u>New York</u> $\phi^2 = .046;\ p < .107$					
Shock Graduates	.117	.138	.128	.064	.553
Shock Dropouts	.237	.217	.093	.093	.361
Prison Parolees	.200	.158	.126	.032	.484
Total	.185	.171	.115	.063	.465
<u>South Carolina</u> $\phi^2 = .053;\ p < .174$					
DPPPS Shock Graduates	---	.035	.118	.118	.729
Prison Parolees	---	.063	.109	.250	.578
Probationers	---	.073	.116	.130	.681
Split Probationers	---	.125	.000	.083	.792
Total	---	.062	.103	.153	.682

[a]Note: ϕ^2 is a PRE measure of association calculated by χ^2/n (Blalock, 1979).
[b]Note: Exit cohort categories are constructed as described in Figures 1-5. The "Missing" cohort refers to subjects with no positive adjustment score for the first quarter of the follow-up period.
[c]Note: Entries are proportions of the treatment sample in each exit cohort. Proportions sum to ≈1.0 across columns.

Table 11
Changes in Composition of Analysis Files Over Time: Variation Fixed Effect Means at Each Follow-Up Wave ("M" denotes Month)

State and Predictor		Means M3	M6	M9	M12
Florida	N=	266	199	123	58
Shock Sample (0/1)		.376	.402	.439	.362
Dropout Sample (0/1)		.222	.181	.114*	.103*
Prison Sample (0/1)		.402	.417	.447	.534*
Nonwhite Indicator (0/1)		.571	.603	.593	.517
Age @ Comm. Supv.		19.4	19.4	19.7	19.7
Offense=Violent (0/1)		.335	.367	.415*	.362
Offense=Property (0/1)		.523	.492	.431	.466
Offense=Drug (0/1)		.143	.141	.154	.172
New Crime Indicator (0/1)		.838	.839	.846	.845
Georgia	N=	215	174	128	63
Shock Sample (0/1)		.330	.322	.297	.413*
Prison Sample (0/1)		.344	.379	.391	.39[illegible]
Probation Sample (0/1)		.326	.299	.313	.19[illegible]
Nonwhite Indicator (0/1)		.628	.632	.656	.683
Age @ Comm. Supv.		21.7	21.7	21.6	21.7
Offense=Violent (0/1)		.149	.149	.164	.175*
Offense=Property (0/1)		.581	.575	.617	.619
Offense=Drug (0/1)		.270	.276	.219	.206*
New Crime Indicator (0/1)		.813	.792	.781	.762
Prior Offense Indicator (0/1)		.428	.460	.453	.460
Louisiana	N=	2[illegible]6	259	214	178
Shock Sample (0/1)		.279	.278	.257	.253
Prison Sample (0/1)		.268	.263	.224	.213*
Probation Sample (0/1)		.399	.409	.467	.483*
Dropout Sample (0/1)		.054	.050	.051	.051
Nonwhite Indicator (0/1)		.638	.633	.620	.605
Age @ Comm. Supv.		25.1	25.3	25.2	25.6
New Crime Indicator (0/1)		.744	.751	.810	.800
Prior Offense Indicator (0/1)		.736	.733	.740	.762

(Continued)

*Denotes changes that deviate ± 20% from baseline score (Month 3).

Table 11 (Continued)
Changes in Composition of Analysis Files Over Time: Variation in Fixed Effect Means at Each Follow-Up Wave ("M" denotes Month)

State and Predictor	Means			
	M3	M6	M9	M12
New York N=	233	184	151	133
Shock Sample (0/1)	.356	.380	.384	.391
Dropout Sample (0/1)	.318	.288	.291	.263
Prison Sample (0/1)	.326	.332	.325	.346
Nonwhite Indicator (0/1)	.803	.783	.781	.774
Age @ First Arrest	18.1	18.3	18.4	18.5
Offense=Other/Violent (0/1)	.240	.245	.225	.226
Offense=Property (0/1)	.206	.196	.192	.180
Offense=Drug (0/1)	.554	.560	.583	.594
Prior Offense Indicator (0/1)	.893	.880	.881	.872
South Carolina N=	242	227	202	165
DPPPS Shock Sample (0/1)	.351	.361	.356	.376
Prison Sample (0/1)	.264	.264	.262	.224
Probation Sample (0/1)	.285	.282	.277	.285
Split-Probation Sample (0/1)	.099	.093	.104	.115
Nonwhite Indicator (0/1)	.562	.551	.530	.540
Age @ Comm. Supv.	21.1	21.1	21.1	21.2
Offense=Violent (0/1)	.140	.145	.119	.121
Offense=Property (0/1)	.640	.621	.639	.606
Offense=Drug (0/1)	.219	.233	.243	.273*
New Crime Indicator (0/1)	.884	.885	.886	.915
Prior Offense Indicator (0/1)	.591	.581	.594	.600

*Denotes changes that deviate ± 20% from baseline score (Month 3).

Table 12
Sample Comparisons on Categorical Predictor Variables in Flori

Sample		Study Variable Distributions		
Race/Ethnicity	N=	White	Nonwhite	
Shock Graduates	112	42.9%	57.1%	
Shock Dropouts	68	51.5%	48.5%	
Prison Parolees	109	38.5%	61.5%	
Total	289	43.3%	56.8%	

$\chi^2_{(2)} = 2.868; p < .238$

Current Offense	N=	Violent	Drug	Property/Other
Shock Graduates	112	24.1%	22.3%	53.6%
Shock Dropouts	68	29.4%	4.4%	66.2%
Prison Parolees	109	42.2%	11.9%	45.9%
Total	289	32.2%	14.2%	53.6%

$\chi^2_{(4)} = 19.248; p < .001$

Type of Offense	N=	New Crime	Technical Violation
Shock Graduates	112	77.7%	22.3%
Shock Dropouts	68	83.8%	16.2%
Prison Parolees	109	89.9%	10.1%
Total	289	83.7%	16.3%

$\chi^2_{(2)} = 6.067; p < .048$

Prior Offenses	N=	No Priors	Priors Indicated
Shock Graduates	112	69.6%	30.4%
Shock Dropouts	68	70.6%	29.4%
Prison Parolees	109	76.2%	23.9%
Total	289	72.3%	27.7%

$\chi^2_{(2)} = 1.3; p < .522$

(Continued)

Table 12 (Continued)
Sample Comparisons on Continuous Predictor Variables In Florida[1]

Sample	Study Variable Distributions		
Age at Community Supervision	N=	Mean	S.D.
Shock Graduates[a,b]	112	19.3	1.9
Shock Dropouts[b]	68	19.0	1.8
Prison Parolees[a]	109	19.7	1.9
Total	289	19.4	1.9

$F_{(2,286)} = 2.76;\ p < .065$

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 13
Sample Comparisons on Categorical Predictor Variables in Georgia

Sample		Study Variable Distributions		
Race/Ethnicity	N=	White	Nonwhite	
Shock Graduates	79	40.5%	59.5%	
Prison Parolees	98	32.7%	67.4%	
Probationers	85	43.5%	56.5%	
Total	262	38.6%	61.5%	

$\chi^2_{(2)} = 2.456; p < .293$

Current Offense	N=	Violent	Drug	Property/Other
Shock Graduates	79	17.7%	19.0%	63.3%
Prison Parolees	98	20.4%	29.6%	50.0%
Probationers	85	7.1%	31.8%	61.2%
Total	262	15.3%	27.1%	57.6%

$\chi^2_{(4)} = 10.2; p < .037$

Type of Offense	N=	New Crime	Technical Violation
Shock Graduates	79	82.3%	17.7%
Prison Parolees	98	62.2%	37.8%
Probationers	84	98.8%	1.2%
Total	261	80.1%	19.9%

$\chi^2_{(2)} = 38.248; p < .001$

Prior Offenses	N=	No Priors	Priors Indicated
Shock Graduates	79	60.8%	39.2%
Prison Parolees	98	24.5%	75.5%
Probationers	85	89.4%	10.6%
Total	262	56.5%	43.5%

$\chi^2_{(2)} = 78.897; p < .001$

(Continued)

Table 13 (Continued)
Sample Comparisons on Continuous Predictor Variables in Georgia[1]

Sample	Study Variable Distributions		
Age at Community Supervision	N=	Mean	S.D.
Shock Graduates[a]	76	20.5	1.9
Prison Parolees[b]	97	23.4	2.7
Probationers[a]	85	21.0	2.5
Total	258	21.8	2.8

$F_{(2,255)} = 37.5;\ p < .001$

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 14
Sample Comparisons on Categorical Predictor Variables in Louisiana

Sample		Study Variable Distributions		
Race/Ethnicity	N=	White	Nonwhite	
Shock Graduates	71	42.3%	57.8%	
Prison Parolees	74	29.7%	70.3%	
Probationers	110	35.5%	64.6%	
Shock Dropouts	15	53.3%	46.7%	
Total	270	36.7%	63.3%	

$\chi^2_{(3)} = 4.352; p < .226$

Current Offense	N=	Violent	Drug	Property/Other
Shock Graduates	69	5.8%	30.4%	63.8%
Prison Parolees	71	16.9%	22.5%	60.6%
Probationers	96	7.3%	36.5%	56.3%
Shock Dropouts	15	0.0%	20.0%	80.0%
Total	251	9.2%	29.9%	61.0%

$\chi^2_{(6)} = 11.736; p < .068$

Type of Offense	N=	New Crime	Technical Violation
Shock Graduates	71	66.2%	33.8%
Prison Parolees	73	63.0%	37.0%
Probationers	97	91.8%	8.3%
Shock Dropouts	15	60.0%	40.0%
Total	256	74.6%	25.4%

$\chi^2_{(3)} = 24.572; p < .001$

Prior Offenses	N=	No Priors	Priors Indicated
Shock Graduates	71	19.7%	80.3%
Prison Parolees	74	31.1%	68.9%
Probationers	110	30.0%	70.0%
Shock Dropouts	15	6.7%	93.3%
Total	270	26.3%	73.7%

$\chi^2_{(3)} = 6.22; p < .101$

(Continued)

Table 14 (Continued)
Sample Comparisons on Continuous Predictor Variables in Louisiana[1]

Sample	Study Variable Distributions		
Age at Community Supervision	N=	Mean	S.D.
Shock Graduates[a]	75	23.8	4.8
Prison Parolees[b]	74	27.0	5.7
Probationers[a]	110	24.5	5.3
Shock Dropouts[a,b]	16	25.8	4.8
Total	275	25.1	5.3
$F_{(3,271)} = 5.42;\ p < .002$			
Age at First Arrest	N=	Mean	S.D.
Shock Graduates[a]	60	19.5	3.8
Prison Parolees[b]	67	22.3	5.3
Probationers[a,b]	110	20.8	4.5
Shock Dropouts[a]	14	19.2	1.5
Total	251	20.8	4.6
$F_{(2,247)} = 4.74;\ p < .004$			

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 15
Sample Comparisons on Categorical Predictor Variables in New York

Sample		Study Variable Distributions		
Race/Ethnicity	N=	White	Nonwhite	
Shock Graduates	94	17.0%	83.0%	
Shock Dropouts	97	16.5%	83.5%	
Prison Parolees	95	17.9%	82.1%	
Total	286	17.1%	82.9%	

$\chi^2_{(2)} = 0.067; p < .967$

Current Offense	N=	Drug	Other/Violent	Property
Shock Graduates	94	68.1%	16.0%	16.0%
Shock Dropouts	97	48.5%	26.8%	24.7%
Prison Parolees	95	44.2%	28.4%	27.4%
Total	286	53.5%	23.8%	22.7%

$\chi^2_{(4)} = 12.354; p < .015$

Prior Offenses	N=	No Priors	Priors Indicated
Shock Graduates	94	18.1%	81.9%
Shock Dropouts	97	5.2%	94.9%
Prison Parolees	95	5.3%	94.7%
Total	286	9.4%	90.6%

$\chi^2_{(2)} = 12.240; p < .002$

(Continued)

Table 15 (Continued)
Sample Comparisons on Continuous Predictor Variables in New York[1]

Sample	Study Variable Distributions		
<u>Age at Community Supervision</u>	N=	Mean	S.D.
Shock Graduates[a]	94	22.1	2.6
Shock Dropouts[a]	97	22.1	2.2
Prison Parolees[a]	95	21.7	2.4
Total	286	22.0	2.4

$F_{(2,283)} = 1.00;\ p < .367$

<u>Age at First Arrest</u>	N=	Mean	S.D.
Shock Graduates[a]	94	19.0	[illegible]
Shock Dropouts[b]	97	17.[illegible]	1.7
Prison Parolees[b]	95	17.5	1.8
Total	286	18.0	2.2

$F_{(2,283)} = 16.01;\ p < .001$

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 16
Sample Comparisons on Categorical Predictor Variables in South Carolina

Sample	Study Variable Distributions			
<u>Race/Ethnicity</u>	N=	White	Nonwhite	
DPPPS Shock	85	50.6%	49.4%	
DOC Shock	84	26.2%	73.8%	
Prison Parolees	64	39.1%	60.9%	
Probationers	69	42.0%	58.0%	
Split-Probationers	24	37.5%	62.5%	
Total	326	39.3%	60.7%	
$\chi^2_{(4)} = 10.845; p < .028$				
<u>Current Offense</u>	N=	Violent	Drug	Property/Other
DPPPS Shock	85	14.1%	23.5%	62.4%
DOC Shock	83	9.6%	34.9%	55.4%
Prison Parolees	64	20.3%	17.2%	62.5%
Probationers	69	11.6%	20.3%	68.1%
Split-Probationers	24	4.2%	33.3%	62.5%
Total	325	12.9%	25.2%	61.9%
$\chi^2_{(8)} = 12.143; p < .145$				
<u>Type of Offense</u>	N=	New Crime		Technical Violation
DPPPS Shock	85	87.1%		12.9%
DOC Shock	84	95.2%		4.8%
Prison Parolees	64	81.3%		18.8%
Probationers	69	98.6%		1.5%
Split-Probationers	24	83.3%		16.7%
Total	326	90.2%		9.8%
$\chi^2_{(4)} = 15.861; p < .003$				
<u>Prior Offenses</u>	N=	No Priors		Priors Indicated
DPPPS Shock	85	47.1%		52.9%
DOC Shock	84	9.5%		90.5%
Prison Parolees	64	39.1%		60.9%
Probationers	69	37.7%		62.3%
Split-Probationers	24	33.3%		66.7%
Total	326	32.8%		67.2%
$\chi^2_{(4)} = 30.365; p < .001$				

(Continued)

Table 16 (Continued)
Sample Comparisons on Continuous Predictor Variables in South Carolina[1]

Sample	Study Variable Distributions		
<u>Age at Community Supervision</u>	N=	Mean	S.D.
DPPPS Shock[a]	85	20.6	2.1
DOC Shock[a]	84	21.1	2.3
Prison Parolees[a]	63	21.0	1.9
Probationers[a]	68	21.1	2.2
Split-Probationers[b]	23	23.4	2.0
Total	323	21.1	2.2
$F_{(4,318)} = 8.07; p < .001$			
<u>Age at First Arrest</u>	N=	Mean	S.D.
DPPPS Shock[a]	85	18.8	1.9
DOC Shock[a]	82	18.3	2.9
Prison Parolees[a]	64	18.7	1.7
Probationers[b]	69	19.7	2.3
Split-Probationers[b]	24	19.8	2.6
Total	324	18.9	2.4
$F_{(4,319)} = 4.69; p < .002$			

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 17
Effects of Natural Log Transformation of Overall Supervision Intensity Indicators In Florida, Georgia, Louisiana, and South Carolina[a]

State and Indicator	Median	Mean	S.D.	Skewness
<u>Florida</u>				
Primary Contacts	2.25	6.63	9.53	+2.25
Primary Contacts′	1.18	1.50	0.94	+0.91
Secondary Contacts	1.35	2.83	3.26	+1.99
Secondary Contacts′	0.98	1.12	0.71	+0.73
<u>Georgia</u>				
Primary Contacts	1.82	2.55	3.07	+4.60
Primary Contacts′	1.04	1.08	0.56	+0.90
<u>Louisiana</u>				
Knowledge Index	1.00	1.24	1.13	+1.65
Knowledge Index′	0.69	0.70	0.46	+0.32
Surveillance Index	0.39	0.71	0.84	+1.35
Surveillance Index′	0.33	0.43	0.43	+0.69
<u>South Carolina</u>				
Primary Contacts	1.50	1.73	1.40	+2.95
Primary Contacts′	0.92	0.90	0.43	+0.27

[a]Note: The prime symbol (′) denotes the log transform of the indicator.

Table 18
Repeated Measures Analysis of Variance Tests For Decay in Primary Contact Levels Over Study Period In Florida

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Primary Contacts @ Month 3	276	1.56	7.45
<u>Six Month Analysis</u>			
Primary Contacts @ Month 3	217	1.54	7.20
Primary Contacts @ Month 6	217	1.33	5.91
Ho: No Within Subject Change:	$F_{(1,216)}=17.40;\ p < .001$		
<u>Nine Month Analysis</u>			
Primary Contacts @ Month 3	141	1.55	7.52
Primary Contacts @ Month 6	141	1.33	5.86
Primary Contacts @ Month 9	141	1.25	5.21
Ho: No Within Subject Change:	$F_{(2,139)}=9.16;\ p < .001$		
<u>Twelve Month Analysis</u>			
Primary Contacts @ Month 3	73	1.63	8.48
Primary Contacts @ Month 6	73	1.49	6.53
Primary Contacts @ Month 9	73	1.45	5.99
Primary Contacts @ Month 12	73	1.38	5.79
Ho: No Within Subject Change:	$F_{(3,70)}=1.94;\ p < .131$		

Table 19
Repeated Measures Analysis of Variance Tests For Decay in Secondary Contact Levels Over Study Period In Florida

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Secondary Contacts @ Month 3	276	1.12	3.08
<u>Six Month Analysis</u>			
Secondary Contacts @ Month 3	217	1.09	2.93
Secondary Contacts @ Month 6	217	0.94	2.45
Ho: No Within Subject Change:	$F_{(1,216)}=15.821$; $p < .001$		
<u>Nine Month Analysis</u>			
Secondary Contacts @ Month 3	141	1.13	3.08
Secondary Contacts @ Month 6	141	0.99	2.67
Secondary Contacts @ Month 9	141	0.92	2.49
Ho: No Within Subject Change:	$F_{(2,139)}=6.655$; $p < .001$		
<u>Twelve Month Analysis</u>			
Secondary Contacts @ Month 3	73	1.15	3.29
Secondary Contacts @ Month 6	73	1.06	2.85
Secondary Contacts @ Month 9	73	1.03	2.73
Secondary Contacts @ Month 12	73	0.92	2.38
Ho: No Within Subject Change:	$F_{(3,70)}=3.484$; $p < .020$		

Table 20
Repeated Measures Analysis of Variance Tests For Decay in Primary Contact Levels Over Study Period In Georgia

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Primary Contacts @ Month 3	241	1.17	3.13
<u>Six Month Analysis</u>			
Primary Contacts @ Month 3	226	1.18	3.07
Primary Contacts @ Month 6	226	0.99	2.50
Ho: No Within Subject Change:	$F_{(1,225)}=17.78;\ p < .001$		
<u>Nine Month Analysis</u>			
Primary Contacts @ Month 3	175	1.23	3.29
Primary Contacts @ Month 6	175	1.07	2.83
Primary Contacts @ Month 9	175	0.94	2.23
Ho: No Within Subject Change:	$F_{(2,173)}=11.74;\ p < .001$		
<u>Twelve Month Analysis</u>			
Primary Contacts @ Month 3	91	1.27	3.35
Primary Contacts @ Month 6	91	1.07	2.88
Primary Contacts @ Month 9	91	1.00	2.47
Primary Contacts @ Month 12	91	0.88	2.16
Ho: No Within Subject Change:	$F_{(3,88)}=8.20;\ p < .001$		

Table 21
Repeated Measures Analysis of Variance Tests For Decay in Knowledge Index Scores Over Study Period In Louisiana

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Knowledge Index @ Month 3	278	0.67	1.24
<u>Six Month Analysis</u>			
Knowledge Index @ Month 3	262	0.67	1.25
Knowledge Index @ Month 6	262	0.63	1.15
Ho: No Within Subject Change:	$F_{(1,261)}=2.305;\ p < .130$		
<u>Nine Month Analysis</u>			
Knowledge Index @ Month 3	221	0.68	1.26
Knowledge Index @ Month 6	221	0.65	1.19
Knowledge Index @ Month 9	221	0.66	1.28
Ho: No Within Subject Change:	$F_{(2,219)}=0.476;\ p < .622$		
<u>Twelve Month Analysis</u>			
Knowledge Index @ Month 3	184	0.67	1.23
Knowledge Index @ Month 6	184	0.65	1.18
Knowledge Index @ Month 9	184	0.64	1.21
Knowledge Index @ Month 12	184	0.67	1.32
Ho: No Within Subject Change:	$F_{(3,181)}=0.411;\ p < .745$		

Table 22
Repeated Measures Analysis of Variance Tests For Decay in Surveillance Index Scores Over Study Period In Louisiana

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Surveillance Index @ Month 3	278	0.47	0.87
<u>Six Month Analysis</u>			
Surveillance Index @ Month 3	262	0.48	0.87
Surveillance Index @ Month 6	262	0.45	0.80
Ho: No Within Subject Change:	$F_{(1,261)}=1.134;\ p < .288$		
<u>Nine Month Analysis</u>			
Surveillance Index @ Month 3	221	0.46	0.83
Surveillance Index @ Month 6	221	0.46	0.81
Surveillance Index @ Month 9	221	0.32	0.50
Ho: No Within Subject Change:	$F_{(2,219)}=16.662;\ p < .001$		
<u>Twelve Month Analysis</u>			
Surveillance Index @ Month 3	184	0.47	0.84
Surveillance Index @ Month 6	184	0.45	0.82
Surveillance Index @ Month 9	184	0.33	0.51
Surveillance Index @ Month 12	184	0.25	0.37
Ho: No Within Subject Change:	$F_{(3,181)}=13.2;\ p < .001$		

Table 23
Repeated Measures Analysis of Variance Tests For Decay in Requirements Index Scores Over Study Period In Louisiana

Time Period	N=	Raw Mean
Three Month Analysis		
Requirements Index @ Month 3	278	2.87
Six Month Analysis		
Requirements Index @ Month 3	262	2.89
Requirements Index @ Month 6	262	2.84
Ho: No Within Subject Change:	$F_{(1,261)}=0.79$; $p < .375$	
Nine Month Analysis		
Requirements Index @ Month 3	221	2.90
Requirements Index @ Month 6	221	2.93
Requirements Index @ Month 9	221	2.63
Ho: No Within Subject Change:	$F_{(2,219)}=11.165$; $p < .001$	
Twelve Month Analysis		
Requirements Index @ Month 3	184	2.95
Requirements Index @ Month 6	184	2.96
Requirements Index @ Month 9	184	2.70
Requirements Index @ Month 12	184	2.39
Ho: No Within Subject Change:	$F_{(3,181)}=15.894$; $p < .001$	

Table 24
Repeated Measures Analysis of Variance Tests For Decay in Primary Contact Levels Over Study Period In South Carolina[a]

Time Period	N=	Log Mean	Raw Mean
<u>Three Month Analysis</u>			
Primary Contacts @ Month 3	231	1.05	2.35
<u>Six Month Analysis</u>			
Primary Contacts @ Month 3	229	1.05	2.35
Primary Contacts @ Month 6	229	0.98	2.12
Ho: No Within Subject Change:	$F_{(1,228)}=13.83;\ p < .001$		
<u>Nine Month Analysis</u>			
Primary Contacts @ Month 3	211	1.06	2.35
Primary Contacts @ Month 6	211	0.99	2.12
Primary Contacts @ Month 9	211	0.83	1.65
Ho: No Within Subject Change:	$F_{(2,209)}=17.47;\ p < .001$		
<u>Twelve Month Analysis</u>			
Primary Contacts @ Month 3	172	1.07	2.38
Primary Contacts @ Month 6	172	0.99	2.13
Primary Contacts @ Month 9	172	0.86	1.68
Primary Contacts @ Month 12	172	0.71	1.33
Ho: No Within Subject Change:	$F_{(3,169)}=19.55;\ p < .001$		

[a]Note: Analyses do not include the S.C. DOC shock sample.

Table 25
Overall Supervision Intensity by Treatment Sample In Florida[1]

Treatment Sample	N=	Mean[2]
Primary Contact Levels		
Shock Graduates[a]	106	1.254
Shock Dropouts[b]	65	1.583
Prison Parolees[b]	105	1.707
Total	276	1.504
$F_{(2,273)} = 6.70;\ p < .001$		
Secondary Contact Levels		
Shock Graduates[a]	106	0.933
Shock Dropouts[b]	65	1.184
Prison Parolees[b]	105	1.188
Total	276	1.088
$F_{(2,273)} = 4.91;\ p < .008$		

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are the natural log transforms of original variables.

Table 26
Repeated Measures Analysis of Variance Tests For Changes In Primary Contacts Over Time By Treatment Sample In Florida[a]

Treatment Sample	M3	M6	M9	M12
<u>Month 3 Analysis</u>				
Shock Graduates (N=106)	1.34			
Shock Dropouts (N=65)	1.61			
Prison Parolees (N=105)	1.75			
Sample: $F_{(2,273)} = 6.7$; $p < .001$				
<u>Month 6 Analysis</u>				
Shock Graduates (N=89)	1.29	1.09		
Shock Dropouts (N=43)	1.56	1.44		
Prison Parolees (N=85)	1.78	1.54		
Sample: $F_{(2,214)} = 6.58$; $p < .002$				
Within-Subject Change: $F_{(1,214)} = 13.614$; $p < .001$				
Within-Subject Change x Sample: $F_{(2,214)} = 0.387$; $p < .680$				
<u>Month 9 Analysis</u>				
Shock Graduates (N=60)	1.23	1.02	0.93	
Shock Dropouts (N=18)	1.50	1.49	1.32	
Prison Parolees (N=63)	1.88	1.59	1.54	
Sample: $F_{(2,138)} = 8.48$; $p < .001$				
Within-Subject Change: $F_{(2,137)} = 5.015$; $p < .008$				
Within-Subject Change x Sample: $F_{(4,274)} = 0.499$; $p < .737$				
<u>Month 12 Analysis</u>				
Shock Graduates (N=29)	1.32	1.22	1.21	1.07
Shock Dropouts (N=8)	1.39	1.44	1.45	1.43
Prison Parolees (N=36)	1.94	1.72	1.65	1.62
Sample: $F_{(2,70)} = 3.42$; $p < .038$				
Within-Subject Change: $F_{(3,68)} = 0.618$; $p < .606$				
Within-Subject Change x Sample: $F_{(6,136)} = 0.318$; $p < .927$				

[a]Note: M=Month and means are natural log transformations.

Table 27
Repeated Measures Analysis of Variance Tests For Changes Secondary Contacts Over Time By Treatment Sample In Florida[a]

Treatment Sample	M3	M6	M9	M12
Month 3 Analysis				
Shock Graduates (N=106)	0.98			
Shock Dropouts (N=65)	1.18			
Prison Parolees (N=105)	1.22			
Sample: $F_{(2,273)} = 4.9$; $p < .008$				
Month 6 Analysis				
Shock Graduates (N=89)	0.95	0.83		
Shock Dropouts (N=43)	1.12	0.94		
Prison Parolees (N=85)	1.23	1.05		
Sample: $F_{(2,214)} = 3.32$; $p < .038$				
Within-Subject Change: $F_{(1,214)} = 15.019$; $p < .001$				
Within-Subject Change x Sample: $F_{(2,214)} = 0.359$; $p < .699$				
Month 9 Analysis				
Shock Graduates (N=60)	0.95	0.85	0.76	
Shock Dropouts (N=18)	1.11	0.98	0.74	
Prison Parolees (N=63)	1.30	1.13	1.12	
Sample: $F_{(2,138)} = 4.69$; $p < .011$				
Within-Subject Change: $F_{(2,137)} = 6.242$; $p < .001$				
Within-Subject Change x Sample: $F_{(4,274)} = 0.536$; $p < .709$				
Month 12 Analysis				
Shock Graduates (N=29)	1.04	0.98	0.94	0.84
Shock Dropouts (N=8)	0.94	0.92	0.89	0.91
Prison Parolees (N=36)	1.29	1.16	1.14	0.99
Sample: $F_{(2,70)} = 1.02$; $p < .364$				
Within-Subject Change: $F_{(3,68)} = 1.315$; $p < .277$				
Within-Subject Change x Sample: $F_{(6,136)} = 0.306$; $p < .933$				

[a]Note: M=Month and means are natural log transformations.

Table 28
Overall Supervision Intensity by Treatment Sample In Georgia[1]

Treatment Sample	N=	Mean[2]
Primary Contact Levels		
Shock Graduates[a,b]	75	1.071
Prison Parolees[a]	89	1.213
Probationers[b]	77	0.936
Total	241	1.080

$F_{(2,238)} = 5.32;\ p < .005$

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are natural log transformations.

Table 29
Repeated Measures Analysis of Variance Tests For Changes In Primary Contacts Over Time By Treatment Sample In Georgia[a]

Treatment Sample	M3	M6	M9	M12
Month 3 Analysis				
Shock Graduates (N=75)	1.14			
Prison Parolees (N=89)	1.39			
Probationers (N=77)	0.96			
Sample: $F_{(2,238)} = 10.65$; $p < .001$				
Month 6 Analysis				
Shock Graduates (N=69)	1.17	0.99		
Prison Parolees (N=85)	1.39	1.10		
Probationers (N=72)	0.93	0.87		
Sample: $F_{(2,223)} = 8.52$; $p < .001$				
Within-Subject Change: $F_{(1,223)} = 16.617$; $p < .001$				
Within-Subject Change x Sample: $F_{(2,223)} = 2.314$; $p < .101$				
Month 9 Analysis				
Shock Graduates (N=51)	1.18	1.08	0.99	
Prison Parolees (N=69)	1.41	1.14	1.00	
Probationers (N=55)	1.06	0.97	0.80	
Sample: $F_{(2,172)} = 4.00$; $p < .020$				
Within-Subject Change: $F_{(2,171)} = 10.629$; $p < .001$				
Within-Subject Change x Sample: $F_{(4,342)} = 0.891$; $p < .470$				
Month 12 Analysis				
Shock Graduates (N=34)	1.24	1.21	1.18	1.10
Prison Parolees (N=34)	1.46	1.09	0.96	0.82
Probationers (N=23)	1.02	0.83	0.79	0.65
Sample: $F_{(2,88)} = 4.74$; $p < .011$				
Within-Subject Change: $F_{(3,86)} = 8.361$; $p < .001$				
Within-Subject Change x Sample: $F_{(6,172)} = 1.221$; $p < .298$				

[a]Note: M=Month and means are natural log transformations.

Table 30
Overall Supervision Intensity by Treatment Sample In Louisiana[1]

Treatment Sample	N=	Mean[2]
Knowledge Scores		
Shock Graduates[a]	77	0.417
Prison Parolees[b]	74	0.700
Probationers[b]	111	0.890
Shock Dropouts[b]	16	0.725
Total	278	0.699
$F_{(3,274)} = 19.55; p < .001$		
Surveillance Scores		
Shock Graduates[a]	77	0.948
Prison Parolees[b]	74	0.294
Probationers[b]	111	0.200
Shock Dropouts[b]	16	0.245
Total	278	0.435
$F_{(3,274)} = 113.73; p < .001$		
Requirements Scores		
Shock Graduates[a]	77	3.751
Prison Parolees[b]	74	2.203
Probationers[b]	111	2.382
Shock Dropouts[b]	16	2.538
Total	278	2.722
$F_{(3,274)} = 46.62; p < .001$		

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means for knowledge and surveillance scores are natural log transformations.

Table 31
Repeated Measures Analysis of Variance Tests For Changes In Knowledge Scores Over Time By Treatment Sample In Louisiana[a]

Treatment Sample	M3	M6	M9	M12
<u>Month 3 Analysis</u>				
Shock Graduates (N=77)	0.33			
Prison Parolees (N=74)	0.69			
Probationers (N=111)	0.89			
Shock Dropouts (N=16)	0.74			
Sample: $F_{(3,274)} = 22.84$; $p < .001$				
<u>Month 6 Analysis</u>				
Shock Graduates (N=72)	0.32	0.39		
Prison Parolees (N=69)	0.68	0.55		
Probationers (N=107)	0.89	0.84		
Shock Dropouts (N=14)	0.74	0.64		
Sample: $F_{(3,258)} = 22.84$; $p < .001$				
Within-Subject Change: $F_{(1,258)} = 2.105$; $p < .148$				
Within-Subject Change x Sample: $F_{(3,258)} = 2.336$; $p < .074$				
<u>Month 9 Analysis</u>				
Shock Graduates (N=58)	0.28	0.35	0.39	
Prison Parolees (N=49)	0.68	0.62	0.64	
Probationers (N=101)	0.90	0.83	0.83	
Shock Dropouts (N=13)	0.74	0.66	0.58	
Sample: $F_{(3,217)} = 19.50$; $p < .001$				
Within-Subject Change: $F_{(2,216)} = 0.453$; $p < .636$				
Within-Subject Change x Sample: $F_{(6,432)} = 0.964$; $p < .450$				
<u>Month 12 Analysis</u>				
Shock Graduates (N=48)	0.30	0.37	0.40	0.42
Prison Parolees (N=38)	0.62	0.60	0.66	0.70
Probationers (N=87)	0.88	0.81	0.77	0.80
Shock Dropouts (N=11)	0.78	0.71	0.58	0.60
Sample: $F_{(3,180)} = 11.56$; $p < .001$				
Within-Subject Change: $F_{(3,178)} = 0.453$; $p < .636$				
Within-Subject Change x Sample: $F_{(9,433)} = 0.952$; $p < .480$				

[a]Note: M=Month and means are natural log transformations.

Table 32
Repeated Measures Analysis of Variance Tests For Changes In Surveillance Scores Over Time By Treatment Sample In Louisiana[a]

Treatment Sample	M3	M6	M9	M12
<u>Month 3 Analysis</u>				
Shock Graduates (N=77)	1.10			
Prison Parolees (N=74)	0.31			
Probationers (N=111)	0.17			
Shock Dropouts (N=16)	0.30			
Sample: $F_{(3,274)} = 121.42; p < .001$				
<u>Month 6 Analysis</u>				
Shock Graduates (N=72)	1.09	0.98		
Prison Parolees (N=69)	0.33	0.32		
Probationers (N=107)	0.17	0.21		
Shock Dropouts (N=14)	0.33	0.23		
Sample: $F_{(3,258)} = 109.1; p < .001$				
Within-Subject Change: $F_{(1,258)} = 2.272; p < .133$				
Within-Subject Change x Sample: $F_{(3,258)} = 2.855; p < .038$				
<u>Month 9 Analysis</u>				
Shock Graduates (N=58)	1.07	1.00	0.63	
Prison Parolees (N=49)	0.38	0.39	0.28	
Probationers (N=101)	0.17	0.21	0.18	
Shock Dropouts (N=13)	0.30	0.18	0.16	
Sample: $F_{(3,217)} = 76.2; p < .0001$				
Within-Subject Change: $F_{(2,216)} = 13.37; p < .001$				
Within-Subject Change x Sample: $F_{(6,432)} = 8.732; p < .001$				
<u>Month 12 Analysis</u>				
Shock Graduates (N=48)	1.06	1.00	0.61	0.37
Prison Parolees (N=38)	0.43	0.39	0.32	0.25
Probationers (N=87)	0.17	0.21	0.19	0.20
Shock Dropouts (N=11)	0.32	0.22	0.19	0.10
Sample: $F_{(3,180)} = 44.98; p < .0001$				
Within-Subject Change: $F_{(3,178)} = 16.181; p < .001$				
Within-Subject Change x Sample: $F_{(9,433)} = 10.018; p < .001$				

[a]Note: M=Month and means are natural log transformations.

Table 33
Repeated Measures Analysis of Variance Tests For Changes In Requirements Scores Over Time By Treatment Sample In Louisiana[a]

Treatment Sample	M3	M6	M9	M12
Month 3 Analysis				
Shock Graduates (N=77)	4.16			
Prison Parolees (N=74)	2.34			
Probationers (N=111)	2.34			
Shock Dropouts (N=16)	2.73			
Sample: $F_{(3,274)} = 56.62;\ p < .001$				
Month 6 Analysis				
Shock Graduates (N=72)	4.15	4.04		
Prison Parolees (N=69)	2.42	2.20		
Probationers (N=107)	2.36	2.45		
Shock Dropouts (N=14)	2.79	2.87		
Sample: $F_{(3,258)} = 55.86;\ p < .001$				
Within-Subject Change: $F_{(1,258)} = 0.313;\ p < .576$				
Within-Subject Change x Sample: $F_{(3,258)} = 2.087;\ p < .102$				
Month 9 Analysis				
Shock Graduates (N=58)	4.13	4.13	3.29	
Prison Parolees (N=49)	2.67	2.55	2.33	
Probationers (N=101)	2.34	2.45	2.45	
Shock Dropouts (N=13)	2.59	2.71	2.13	
Sample: $F_{(3,217)} = 36.57;\ p < .001$				
Within-Subject Change: $F_{(2,216)} = 13.78;\ p < .001$				
Within-Subject Change x Sample: $F_{(6,432)} = 6.430;\ p < .001$				
Month 12 Analysis				
Shock Graduates (N=48)	4.19	4.26	3.39	2.73
Prison Parolees (N=38)	2.80	2.71	2.58	2.54
Probationers (N=87)	2.36	2.40	2.46	2.20
Shock Dropouts (N=11)	2.82	2.68	2.06	1.82
Sample: $F_{(3,180)} = 24.11;\ p < .001$				
Within-Subject Change: $F_{(3,178)} = 18.75;\ p < .001$				
Within-Subject Change x Sample: $F_{(9,433)} = 7.58;\ p < .001$				

[a]Note: M=Month

Table 34
Overall Supervision Intensity by Treatment Sample In South Carolina[1]

Treatment Sample	N=	Mean[2]
<u>Primary Contact Levels</u>		
DPPPS Shock Graduates[a]	81	1.00
DOC Shock Graduates[a,b]	79	0.83
Prison Parolees[a]	62	1.00
Probationers[b]	65	0.79
Split-Probationers[a,b]	23	0.92
Total	310	0.91

$F_{(4,305)} = 3.54;\ p < .008$

[1]Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are natural log transformations.

Table 35
Repeated Measures Analysis of Variance Tests For Changes In Primary Contacts Over Time By Treatment Sample In South Carolina[a,b]

Treatment Sample	M3	M6	M9	M12
Month 3 Analysis				
Shock Graduates (N=81)	1.14			
Prison Parolees (N=62)	1.15			
Probationers (N=65)	0.83			
Split Probationer (N=23)	1.08			
Sample: $F_{(3,227)} = 5.17; p < .002$				
Month 6 Analysis				
Shock Graduates (N=80)	1.14	1.11		
Prison Parolees (N=61)	1.15	1.03		
Probationers (N=65)	0.83	0.79		
Split Probationer (N=23)	1.08	0.92		
Sample: $F_{(3,225)} = 5.12; p < .002$				
Within-Subject Change: $F_{(1,225)} = 16.496; p < .001$				
Within-Subject Change x Sample: $F_{(3,225)} = 1.671; p < .174$				
Month 9 Analysis				
Shock Graduates (N=72)	1.15	1.13	0.94	
Prison Parolees (N=58)	1.16	1.05	0.82	
Probationers (N=58)	0.83	0.77	0.73	
Split Probationer (N=23)	1.08	0.92	0.78	
Sample: $F_{(3,207)} = 5.27; p < .002$				
Within-Subject Change: $F_{(2,206)} = 17.377; p < .001$				
Within-Subject Change x Sample: $F_{(6,412)} = 1.647; p < .133$				
Month 12 Analysis				
Shock Graduates (N=62)	1.15	1.13	0.95	0.69
Prison Parolees (N=40)	1.21	1.07	0.90	0.86
Probationers (N=49)	0.83	0.79	0.74	0.65
Split Probationer (N=21)	1.08	0.91	0.76	0.59
Sample: $F_{(3,168)} = 4.55; p < .004$				
Within-Subject Change: $F_{(3,166)} = 18.627; p < .001$				
Within-Subject Change x Sample: $F_{(9,404)} = 1.712; p < .084$				

[a]Note: M=Month and means are natural logs.
[b]Note: These analyses do not include the S.C. DOC shock sample.

Table 36
Overall Supervision Intensity by Exit Cohort in Florida[1]

Exit Cohort	N=	Mean[2]
Primary Contact Levels		
$Cohort_0{}^a$	16	1.228
$Cohort_1{}^a$	64	1.511
$Cohort_2{}^a$	73	1.541
$Cohort_3{}^a$	65	1.385
$Cohort_4{}^a$	58	1.659
$F_{(4,271)} = 1.03;\ p < .392$		
Secondary Contact Levels		
$Cohort_0{}^a$	16	0.840
$Cohort_1{}^a$	64	1.080
$Cohort_2{}^a$	73	1.100
$Cohort_3{}^a$	65	1.060
$Cohort_4{}^a$	58	1.177
$F_{(4,271)} = 0.82;\ p < .515$		

[1]Note: Cohorts with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are natural logs.

Table 37
Overall Supervision Intensity by Exit Cohort in Georgia[1]

Exit Cohort	N=	Mean[2]
Primary Contact Levels		
$Cohort_0$[a]	31	1.029
$Cohort_1$[a]	36	1.020
$Cohort_2$[a]	46	1.099
$Cohort_3$[a]	65	1.084
$Cohort_4$[a]	63	1.123

$F_{(4,236)} = 0.27; p < .895$

[1]Note: Cohorts with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are natural logs.

Table 38
Overall Supervision Intensity by Exit Cohort in Louisiana[1]

Exit Cohort	N=	Mean[2]
Knowledge Scores		
$Cohort_0^a$	2	1.099
$Cohort_1^a$	17	0.697
$Cohort_2^a$	45	0.660
$Cohort_3^a$	36	0.760
$Cohort_4^a$	178	0.692
$F_{(4,273)} = 0.64;\ p < .638$		
Surveillance Scores		
$Cohort_0^a$	2	0.000
$Cohort_1^a$	17	0.405
$Cohort_2^a$	45	0.532
$Cohort_3^a$	36	0.439
$Cohort_4^a$	178	0.417
$F_{(4,273)} = 1.19;\ p < .317$		
Requirements Scores		
$Cohort_0^a$	2	2.000
$Cohort_1^a$	17	2.490
$Cohort_2^a$	45	2.721
$Cohort_3^a$	36	2.581
$Cohort_4^a$	178	2.782
$F_{(4,273)} = 0.67;\ p < .612$		

[1]Note: Cohorts with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: Means are natural logs.

Table 39
Overall Supervision Intensity by Exit Cohort in South Carolina[1,2]

Exit Cohort	N=	Mean[3]
Primary Contact Levels		
$Cohort_0^a$	---	-----
$Cohort_1^a$	8	0.579
$Cohort_2^a$	22	0.928
$Cohort_3^a$	37	0.919
$Cohort_4^a$	164	0.951

$F_{(3,227)} = 1.75;\ p < .158$

[1]Note: Cohorts with different letters are significantly different at the .05 α error level using a Duncan multiple range test.
[2]Note: S.C. DOC shock sample is not included in these calculations.
[3]Note: Means are natural logs.

Table 40
Overall (Cross-Sectional) and Period-Specific Positive Adjustment Scores by State

State and Period	N=	Mean	Std. Dev.
Florida			
Overall	280	0.38	0.27
Month 3	266	0.42	0.29
Month 6	199	0.40	0.31
Month 9	123	0.42	0.34
Month 12	58	0.48	0.31
Georgia			
Overall	246	0.42	0.24
Month 3	215	0.45	0.30
Month 6	174	0.42	0.32
Month 9	128	0.38	0.32
Month 12	63	0.36	0.30
Louisiana			
Overall	278	0.44	0.15
Month 3	276	0.48	0.16
Month 6	259	0.46	0.18
Month 9	214	0.43	0.17
Month 12	178	0.42	0.17
New York			
Overall	237	0.51	0.30
Month 3	233	0.54	0.31
Month 6	184	0.57	0.33
Month 9	152	0.58	0.32
Month 12	133	0.58	0.34
South Carolina[a]			
Overall	326	0.46	0.29
Month 3	242	0.51	0.31
Month 6	227	0.48	0.33
Month 9	202	0.48	0.33
Month 12	165	0.47	0.34

[a]Note: The S.C. DOC shock sample is included in the overall measure but is excluded from the period-specific measures.

Table 41
Repeated Measures Analysis of Variance Tests For Decay in Positive Adjustment Scores Over Study Period In Florida

Time Period	N=	Mean
<u>Three Month Analysis</u>		
Positive Adjustment @ Month 3	266	0.42
<u>Six Month Analysis</u>		
Positive Adjustment @ Month 3	199	0.45
Positive Adjustment @ Month 6	199	0.40
Ho: No Within Subject Change:	$F_{(1,198)}=6.06$; $p < .015$	
<u>Nine Month Analysis</u>		
Positive Adjustment @ Month 3	123	0.53
Positive Adjustment @ Month 6	123	0.49
Positive Adjustment @ Month 9	123	0.42
Ho: No Within Subject Change:	$F_{(2,121)}=8.176$; $p < .001$	
<u>Twelve Month Analysis</u>		
Positive Adjustment @ Month 3	58	0.58
Positive Adjustment @ Month 6	58	0.57
Positive Adjustment @ Month 9	58	0.54
Positive Adjustment @ Month 12	58	0.48
Ho: No Within Subject Change:	$F_{(3,55)}=2.24$; $p < .094$	

Table 42
Repeated Measures Analysis of Variance Tests For Decay in Positive Adjustment Scores Over Study Period In Georgia

Time Period	N=	Mean
<u>Three Month Analysis</u>		
Positive Adjustment @ Month 3	215	0.45
<u>Six Month Analysis</u>		
Positive Adjustment @ Month 3	174	0.45
Positive Adjustment @ Month 6	174	0.41
Ho: No Within Subject Change:	$F_{(1,173)}=1.89$; $p < .171$	
<u>Nine Month Analysis</u>		
Positive Adjustment @ Month 3	128	0.45
Positive Adjustment @ Month 6	128	0.42
Positive Adjustment @ Month 9	128	0.38
Ho: No Within Subject Change:	$F_{(2,126)}=2.470$; $p < .089$	
<u>Twelve Month Analysis</u>		
Positive Adjustment @ Month 3	63	0.45
Positive Adjustment @ Month 6	63	0.38
Positive Adjustment @ Month 9	63	0.40
Positive Adjustment @ Month 12	63	0.36
Ho: No Within Subject Change:	$F_{(3,60)}=1.02$; $p < .390$	

Table 43
Repeated Measures Analysis of Variance Tests For Decay in Positive Adjustment Scores Over Study Period In Louisiana

Time Period	N=	Mean
Three Month Analysis		
Positive Adjustment @ Month 3	276	0.48
Six Month Analysis		
Positive Adjustment @ Month 3	259	0.49
Positive Adjustment @ Month 6	259	0.46
Ho: No Within Subject Change:	$F_{(1,258)}=17.63$; $p < .001$	
Nine Month Analysis		
Positive Adjustment @ Month 3	214	0.50
Positive Adjustment @ Month 6	214	0.49
Positive Adjustment @ Month 9	214	0.43
Ho: No Within Subject Change:	$F_{(2,212)}=21.817$; $p < .001$	
Twelve Month Analysis		
Positive Adjustment @ Month 3	178	0.50
Positive Adjustment @ Month 6	178	0.50
Positive Adjustment @ Month 9	178	0.46
Positive Adjustment @ Month 12	178	0.42
Ho: No Within Subject Change:	$F_{(3,175)}=17.480$; $p < .001$	

Table 44
Repeated Measures Analysis of Variance Tests For Decay in Positive Adjustment Scores Over Study Period In New York

Time Period	N=	Mean
<u>Three Month Analysis</u>		
Positive Adjustment @ Month 3	233	0.54
<u>Six Month Analysis</u>		
Positive Adjustment @ Month 3	184	0.60
Positive Adjustment @ Month 6	184	0.57
Ho: No Within Subject Change:	$F_{(1,183)}=2.288$; $p < .132$	
<u>Nine Month Analysis</u>		
Positive Adjustment @ Month 3	151	0.63
Positive Adjustment @ Month 6	151	0.64
Positive Adjustment @ Month 9	151	0.58
Ho: No Within Subject Change:	$F_{(2,149)}=6.305$; $p < .002$	
<u>Twelve Month Analysis</u>		
Positive Adjustment @ Month 3	133	0.64
Positive Adjustment @ Month 6	133	0.67
Positive Adjustment @ Month 9	133	0.61
Positive Adjustment @ Month 12	133	0.58
Ho: No Within Subject Change:	$F_{(3,130)}=6.882$; $p < .001$	

Table 45
Repeated Measures Analysis of Variance Tests For Decay in Positive Adjustment Scores Over Study Period In South Carolina

Time Period	N=	Mean
<u>Three Month Analysis</u>		
Positive Adjustment @ Month 3	242	0.51
<u>Six Month Analysis</u>		
Positive Adjustment @ Month 3	227	0.52
Positive Adjustment @ Month 6	227	0.48
Ho: No Within Subject Change:	$F_{(1,226)}=2.962;\ p < .087$	
<u>Nine Month Analysis</u>		
Positive Adjustment @ Month 3	202	0.53
Positive Adjustment @ Month 6	202	0.50
Positive Adjustment @ Month 9	202	0.48
Ho: No Within Subject Change:	$F_{(2,200)}=2.415;\ p < .092$	
<u>Twelve Month Analysis</u>		
Positive Adjustment @ Month 3	165	0.53
Positive Adjustment @ Month 6	165	0.52
Positive Adjustment @ Month 9	165	0.51
Positive Adjustment @ Month 12	165	0.47
Ho: No Within Subject Change:	$F_{(3,162)}=1.832;\ p < .144$	

Table 46
Overall (Cross-Sectional) Positive Adjustment
Among Florida Subjects

Sample	N=	Mean (S.D.)
Overall Adjustment	280	0.38 (0.27)
Shock Graduates[a]	108	0.43 (0.28)
Shock Dropouts[b]	65	0.33 (0.26)
Prison Parolees[a,b]	107	0.36 (0.26)

$F_{(2,277)}=3.15;\ p < .05$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 47a
Positive Adjustment Among Florida Subjects Completing Three Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	266	0.42
Shock Graduates[a]	100	0.47
Shock Dropouts[b]	59	0.33
Prison Parolees[a,b]	107	0.41

$F_{(2,263)}=4.07$; $p < .02$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 47b
Positive Adjustment Among Florida Subjects Completing Six Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	199	0.45
Shock Graduates[a]	80	0.47
Shock Dropouts[a]	36	0.37
Prison Parolees[a]	83	0.46
$F_{(2,196)}=1.84;\ p < .161$		
Adjustment by Month 6	199	0.40
Shock Graduates[a]	80	0.45
Shock Dropouts[a]	36	0.34
Prison Parolees[a]	83	0.39
$F_{(2,196)}=1.67;\ p < .191$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,196)}=2.00;\ p < .138$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,195)}=4.58;\ p < .034$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(2,196)}=0.58;\ p < .56$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 47c
Positive Adjustment Among Florida Subjects Completing Nine Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	123	0.53
Shock Graduates[a]	54	0.52
Shock Dropouts[a]	14	0.58
Prison Parolees[a]	55	0.53
$F_{(2,120)}=0.29;\ p < .746$		
Adjustment by Month 6	123	0.49
Shock Graduates[a]	54	0.52
Shock Dropouts[a]	14	0.43
Prison Parolees[a]	55	0.47
$F_{(2,120)}=0.71;\ p < .494$		
Adjustment by Month 9	123	0.42
Shock Graduates[a]	54	0.48
Shock Dropouts[a]	14	0.38
Prison Parolees[a]	55	0.37
$F_{(2,120)}=1.32;\ p < .271$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,120)}=0.49;\ p < .615$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,119)}=7.52;\ p < .001$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(4,238)}=1.54;\ p < .191$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 47d
Positive Adjustment Among Florida Subjects Completing Twelve Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	58	0.58
Shock Graduates[a]	21	0.55
Shock Dropouts[a]	6	0.58
Prison Parolees[a]	31	0.59
$F_{(2,55)}=0.19$; $p < .825$		
Adjustment by Month 6	58	0.57
Shock Graduates[a]	21	0.58
Shock Dropouts[a]	6	0.45
Prison Parolees[a]	31	0.59
$F_{(2,55)}=0.67$; $p < .517$		
Adjustment by Month 9	58	0.54
Shock Graduates[a]	21	0.55
Shock Dropouts[a]	6	0.52
Prison Parolees[a]	31	0.54
$F_{(2,55)}=0.02$; $p < .979$		
Adjustment by Month 12	58	0.48
Shock Graduates[a]	21	0.43
Shock Dropouts[a]	6	0.48
Prison Parolees[a]	31	0.51
$F_{(2,55)}=0.39$; $p < .678$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,55)}=0.18$; $p < .837$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,53)}=1.25$; $p < .301$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(6,106)}=0.63$; $p < .705$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 48
Overall (Cross-Sectional) Positive Adjustment
Among Georgia Subjects

Sample	N=	Mean (S.D.)
Overall Adjustment	246	0.42 (0.24)
Shock Graduates[a]	79	0.42 (0.24)
Prison Parolees[a]	89	0.42 (0.25)
Probationers[a]	78	0.42 (0.23)

$F_{(2,243)}=0.00;\ p \leq 1.0$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 49a
Positive Adjustment Among Georgia Subjects Completing Three Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	215	0.45
Shock Graduates[a]	71	0.48
Prison Parolees[a]	74	0.44
Probationers[a]	70	0.44
$F_{(2,212)}=0.42$; $p < .66$		

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 49b
Positive Adjustment Among Georgia Subjects Completing Six Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	174	0.45
Shock Graduates[a]	56	0.48
Prison Parolees[a]	66	0.44
Probationers[a]	52	0.43
$F_{(2,171)}=0.42$; $p < .655$		
Adjustment by Month 6	174	0.41
Shock Graduates[a]	56	0.38
Prison Parolees[a]	66	0.43
Probationers[a]	52	0.43
$F_{(2,171)}=0.55$; $p < .577$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,171)}=0.03$; $p < .968$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,171)}=1.96$; $p < .164$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(2,171)}=1.18$; $p < .311$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 49c
Positive Adjustment Among Georgia Subjects Completing Nine Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	128	0.45
Shock Graduates[a]	38	0.47
Prison Parolees[a]	50	0.47
Probationers[a]	40	0.41
$F_{(2,125)}=0.46$; $p < .633$		
Adjustment by Month 6	128	0.42
Shock Graduates[a]	38	0.41
Prison Parolees[a]	50	0.42
Probationers[a]	40	0.44
$F_{(2,125)}=0.08$; $p < .927$		
Adjustment by Month 9	128	0.38
Shock Graduates[a]	38	0.38
Prison Parolees[a]	50	0.38
Probationers[a]	40	0.39
$F_{(2,125)}=0.01$; $p < .985$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,125)}=0.02$; $p < .982$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,124)}=2.37$; $p < .098$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(4,248)}=0.27$; $p < .896$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 49d
Positive Adjustment Among Georgia Subjects Completing Twelve Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	63	0.45
Shock Graduates[a]	26	0.50
Prison Parolees[a]	25	0.46
Probationers[a]	12	0.33
$F_{(2,60)}=1.08;\ p < .346$		
Adjustment by Month 6	63	0.38
Shock Graduates[a]	26	0.36
Prison Parolees[a]	25	0.44
Probationers[a]	12	0.32
$F_{(2,60)}=0.81;\ p < .45$		
Adjustment by Month 9	63	0.40
Shock Graduates[a]	26	0.37
Prison Parolees[a]	25	0.42
Probationers[a]	12	0.39
$F_{(2,60)}=0.17;\ p < .845$		
Adjustment by Month 12	63	0.36
Shock Graduates[a]	26	0.37
Prison Parolees[a]	25	0.36
Probationers[a]	12	0.35
$F_{(2,60)}=0.02;\ p < .98$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,60)}=0.39;\ p < .678$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,58)}=0.69;\ p < .561$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(6,116)}=0.82;\ p < .558$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 50
Overall (Cross-Sectional) Positive Adjustment
Among Louisiana Subjects

Sample	N=	Mean (S.D.)
Overall Adjustment	278	0.44 (0.15)
Shock Graduates[a]	77	0.53 (0.15)
Prison Parolees[b]	74	0.42 (0.15)
Probationers[b]	111	0.39 (0.12)
Shock Dropouts[b]	16	0.43 (0.12)

$F_{(3,274)}=19.16;\ p < .001$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 51a
Positive Adjustment Among Louisiana Subjects Completing Three Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	276	0.48
Shock Graduates[a]	77	0.60
Prison Parolees[b]	74	0.47
Probationers[b]	110	0.41
Shock Dropouts[b]	15	0.47

$F_{(3,272)}=26.44;\ p < .001$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 51b
Positive Adjustment Among Louisiana Subjects Completing Six Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	259	0.49
Shock Graduates[a]	72	0.62
Prison Parolees[b]	68	0.48
Probationers[c]	106	0.42
Shock Dropouts[b]	13	0.50
$F_{(3,255)}=32.59;\ p < .001$		
Adjustment by Month 6	259	0.46
Shock Graduates[a]	72	0.57
Prison Parolees[b]	68	0.43
Probationers[b]	106	0.40
Shock Dropouts[b]	13	0.47
$F_{(3,255)}=16.93;\ p < .001$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,255)}=29.16;\ p < .001$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,255)}=10.03;\ p < .002$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(3,255)}=0.83;\ p < .477$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 51c
Positive Adjustment Among Louisiana Subjects Completing Nine Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	214	0.50
Shock Graduates[a]	55	0.63
Prison Parolees[b]	48	0.51
Probationers[c]	100	0.42
Shock Dropouts[b]	11	0.49
$F_{(3,210)}=30.87;\ p < .001$		
Adjustment by Month 6	214	0.49
Shock Graduates[a]	55	0.62
Prison Parolees[b]	48	0.50
Probationers[c]	100	0.40
Shock Dropouts[b]	11	0.50
$F_{(3,210)}=30.93;\ p < .001$		
Adjustment by Month 9	214	0.43
Shock Graduates[a]	55	0.53
Prison Parolees[a,b]	48	0.44
Probationers[b]	100	0.38
Shock Dropouts[a,b]	11	0.45
$F_{(3,210)}=10.52;\ p < .001$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,210)}=30.85;\ p < .001$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,209)}=14.46;\ p < .001$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(6,418)}=2.11;\ p < .051$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 51d
Positive Adjustment Among Louisiana Subjects Completing Twelve Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	178	0.50
Shock Graduates[a]	45	0.64
Prison Parolees[b]	38	0.53
Probationers[c]	86	0.43
Shock Dropouts[b]	9	0.51
$F_{(3,174)}=22.81;\ p < .001$		
Adjustment by Month 6	178	0.50
Shock Graduates[a]	45	0.63
Prison Parolees[b]	38	0.52
Probationers[c]	86	0.41
Shock Dropouts[b]	9	0.50
$F_{(3,174)}=27.62;\ p < .001$		
Adjustment by Month 9	178	0.46
Shock Graduates[a]	45	0.56
Prison Parolees[a,b]	38	0.49
Probationers[c]	86	0.40
Shock Dropouts[b,c]	9	0.46
$F_{(3,174)}=12.25;\ p < .001$		
Adjustment by Month 12	178	0.42
Shock Graduates[a]	45	0.48
Prison Parolees[a]	38	0.43
Probationers[a]	86	0.38
Shock Dropouts[a]	9	0.38
$F_{(3,174)}=3.80;\ p < .011$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,174)}=20.67;\ p < .001$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,172)}=15.64;\ p < .001$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(9,419)}=2.76;\ p < .004$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 52
Overall (Cross-Sectional) Positive Adjustment
Among New York Subjects

Sample	N=	Mean (S.D.)
Overall Adjustment	237	0.51 (0.30)
Shock Graduates[a]	85	0.58 (0.31)
Shock Dropouts[b]	75	0.45 (0.30)
Prison Parolees[a,b]	77	0.49 (0.28)

$F_{(2,234)}=4.08;\ p < .018$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 53a
Positive Adjustment Among New York Subjects Completing Three Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	233	0.54
Shock Graduates[a]	83	0.61
Shock Dropouts[b]	74	0.46
Prison Parolees[a,b]	76	0.54

$F_{(2,230)}=4.72;\ p < .01$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 53b
Positive Adjustment Among New York Subjects Completing Six Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	184	0.60
Shock Graduates[a]	70	0.66
Shock Dropouts[b]	53	0.53
Prison Parolees[a,b]	61	0.59
$F_{(2,181)}=2.83;\ p < .062$		
Adjustment by Month 6	184	0.57
Shock Graduates[a]	70	0.63
Shock Dropouts[a]	53	0.55
Prison Parolees[a]	61	0.52
$F_{(2,181)}=1.78;\ p < .171$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,181)}=2.28;\ p < .105$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,181)}=1.95;\ p < .164$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(2,181)}=1.87;\ p < .158$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 53c
Positive Adjustment Among New York Subjects Completing Nine Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	151	0.63
Shock Graduates[a]	54	0.69
Shock Dropouts[b]	14	0.56
Prison Parolees[a,b]	55	0.63
$F_{(2,148)}=3.05$; $p < .050$		
Adjustment by Month 6	151	0.64
Shock Graduates[a]	54	0.70
Shock Dropouts[a]	14	0.60
Prison Parolees[a]	55	0.61
$F_{(2,148)}=1.76$; $p < .175$		
Adjustment by Month 9	151	0.58
Shock Graduates[a]	54	0.67
Shock Dropouts[b]	14	0.51
Prison Parolees[b]	55	0.53
$F_{(2,148)}=4.10$; $p < .019$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,143)}=3.53$; $p < .032$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,147)}=6.87$; $p < .001$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(4,294)}=1.181$; $p < .319$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 53d
Positive Adjustment Among New York Subjects Completing Twelve Months of Positive Adjustment Evaluation

Time Period & Sample	N=	Mean
Adjustment by Month 3	133	0.64
Shock Graduates[a]	52	0.69
Shock Dropouts[a]	35	0.59
Prison Parolees[a]	46	0.63
$F_{(2,130)}=1.35;\ p < .262$		
Adjustment by Month 6	133	0.67
Shock Graduates[a]	52	0.71
Shock Dropouts[a]	35	0.68
Prison Parolees[a]	46	0.60
$F_{(2,130)}=1.71;\ p < .185$		
Adjustment by Month 9	133	0.61
Shock Graduates[a]	52	0.69
Shock Dropouts[a,b]	35	0.57
Prison Parolees[b]	46	0.54
$F_{(2,130)}=3.08;\ p < .049$		
Adjustment by Month 12	133	0.58
Shock Graduates[a]	52	0.67
Shock Dropouts[a,b]	35	0.54
Prison Parolees[b]	46	0.50
$F_{(2,130)}=3.54;\ p < .032$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(2,130)}=2.78;\ p < .066$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,128)}=7.986;\ p < .001$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(6,256)}=1.847;\ p < .091$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 54
Overall (Cross-Sectional) Positive Adjustment Among South Carolina Subjects

Sample	N=	Mean (S.D.)
Overall Adjustment	326	0.46 (0.29)
DPPPS Shock Graduates[a]	85	0.47 (0.25)
DOC Shock Graduates[a]	84	0.42 (0.33)
Prison Parolees[a]	64	0.50 (0.29)
Probationers[a]	69	0.46 (0.28)
Split-Probationers[a]	24	0.46 (0.31)

$F_{(4,321)}=0.83$; $p < .507$

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 55a
Positive Adjustment Among South Carolina Subjects Completing Three Months of Positive Adjustment Evaluation[1]

Time Period & Sample[2]	N=	Mean
Adjustment by Month 3	242	0.51
DPPPS Shock Graduates[a]	85	0.51
Prison Parolees[a]	64	0.53
Probationers[a]	69	0.51
Split-Probationers[a]	24	0.51

$F_{(3,238)}=0.11;\ p < .955$

[1]Note: Samples with different letters are significantly different at the .05 α level using a Duncan multiple range test.
[2]Note: Data were not collected for DOC shock sample over time.

Table 55b
Positive Adjustment Among South Carolina Subjects Completing Six Months of Positive Adjustment Evaluation[1]

Time Period & Sample[2]	N=	Mean
Adjustment by Month 3	227	0.52
DPPPS Shock Graduates[a]	82	0.51
Prison Parolees[a]	60	0.52
Probationers[a]	64	0.52
Split-Probationers[a]	21	0.54
$F_{(3,223)}=0.06;\ p < .981$		
Adjustment by Month 6	227	0.48
DPPPS Shock Graduates[a]	82	0.49
Prison Parolees[a]	60	0.47
Probationers[a]	64	0.46
Split-Probationers[a]	21	0.55
$F_{(3,223)}=0.38;\ p < .765$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,223)}=0.21;\ p < .887$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,223)}=1.61;\ p < .206$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(3,223)}=0.31;\ p < .815$

[1]Note: Samples with different letters are significantly different at the .05 α level using a Duncan multiple range test.
[2]Note: Data were not collected for DOC shock sample over time.

Table 55c
Positive Adjustment Among South Carolina Subjects Completing Nine Months of Positive Adjustment Evaluation[1]

Time Period & Sample[2]	N=	Mean
Adjustment by Month 3	202	0.53
DPPPS Shock Graduates[a]	72	0.54
Prison Parolees[a]	53	0.51
Probationers[a]	56	0.52
Split-Probationers[a]	21	0.54
$F_{(3,198)}=0.18;\ p < .912$		
Adjustment by Month 6	202	0.50
DPPPS Shock Graduates[a]	72	0.53
Prison Parolees[a]	53	0.49
Probationers[a]	56	0.46
Split-Probationers[a]	21	0.55
$F_{(3,198)}=0.69;\ p < .558$		
Adjustment by Month 9	202	0.48
DPPPS Shock Graduates[a]	72	0.50
Prison Parolees[a]	53	0.46
Probationers[a]	56	0.46
Split-Probationers[a]	21	0.46
$F_{(3,198)}=0.26;\ p < .857$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,198)}=0.39;\ p < .758$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,197)}=2.62;\ p < .075$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(6,394)}=0.43;\ p < .858$

[1]Note: Samples with different letters are significantly different at the .05 α level using a Duncan multiple range test.
[2]Note: Data were not collected for DOC shock sample over time.

Table 55d
Positive Adjustment Among South Carolina Subjects Completing Twelve Months of Positive Adjustment Evaluation[1]

Time Period & Sample[2]	N=	Mean
Adjustment by Month 3	165	0.53
DPPPS Shock Graduates[a]	62	0.53
Prison Parolees[a]	37	0.48
Probationers[a]	47	0.56
Split-Probationers[a]	19	0.56
$F_{(3,161)}=0.56;\ p < .643$		
Adjustment by Month 6	165	0.52
DPPPS Shock Graduates[a]	62	0.52
Prison Parolees[a]	37	0.56
Probationers[a]	47	0.48
Split-Probationers[a]	19	0.57
$F_{(3,161)}=0.59;\ p < .622$		
Adjustment by Month 9	165	0.51
DPPPS Shock Graduates[a]	62	0.50
Prison Parolees[a]	37	0.56
Probationers[a]	47	0.49
Split-Probationers[a]	19	0.45
$F_{(3,161)}=0.63;\ p < .595$		
Adjustment by Month 12	165	0.47
DPPPS Shock Graduates[a]	62	0.43
Prison Parolees[a]	37	0.59
Probationers[a]	47	0.45
Split-Probationers[a]	19	0.46
$F_{(3,161)}=1.86;\ p < .139$		

Effect of SAMPLE
Global Test of Effect of Group Membership: $F_{(3,161)}=0.37;\ p < .773$

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,159)}=1.65;\ p < .180$

Effect of TIME x SAMPLE
Test of Hypothesis That Between Sample Differences Are Time-Stable: $F_{(9,387)}=1.86;\ p < .056$

[1]Note: Samples with different letters are significantly different at the .05 α level using a Duncan multiple range test.
[2]Note: Data were not collected for DOC shock sample over time.

Table 56
Exit Cohort Membership and Overall (Cross-Sectional) Positive Adjustment Scores By State

Exit Cohort		N=	Mean
Florida	$F_{(4,275)}=10.07;\ p < .001$		
Cohort_0^a		14	0.393
Cohort_1^a		67	0.304
Cohort_2^a		76	0.288
Cohort_3^a		65	0.405
Cohort_4^b		58	0.542
Georgia	$F_{(4,241)}=0.23;\ p < .922$		
Cohort_0^a		31	0.441
Cohort_1^a		41	0.409
Cohort_2^a		46	0.416
Cohort_3^a		65	0.429
Cohort_4^a		63	0.398
Louisiana	$F_{(4,273)}=8.65;\ p < .001$		
Cohort_0^a		2	0.472
Cohort_1^b		17	0.298
$\text{Cohort}_2^{a,b}$		45	0.395
$\text{Cohort}_3^{a,b}$		36	0.397
Cohort_4^a		178	0.470
New York	$F_{(4,232)}=14.14;\ p < .001$		
$\text{Cohort}_0^{a,b}$		4	0.438
Cohort_1^b		49	0.332
Cohort_2^b		33	0.344
$\text{Cohort}_3^{a,b}$		18	0.474
Cohort_4^a		133	0.623
South Carolina	$F_{(3,238)}=2.59;\ p < .053$		
Cohort_1^a		15	0.386
Cohort_2^a		25	0.393
Cohort_3^a		37	0.423
Cohort_4^a		165	0.508

Note: Samples with different letters are significantly different at the .05 α error level using a Duncan multiple range test.

Table 57
Linear Regression of Overall Positive Adjustment on Supervision Intensity Indicators In Florida (N=272)

Variable	β	s.e.{β}	Sig.
<u>Equation 1</u>			
Intercept	.276	.030	$p < .001$
Mean Primary Contacts	.069	.017	$p < .001$
$R^2 = .059$			
<u>Equation 2</u>			
Intercept	.289	.031	$p < .001$
Mean Secondary Contacts	.083	.024	$p < .001$
$R^2 = .043$			
<u>Equation 3</u>			
Intercept	.276	.031	$p < .001$
Mean Primary Contacts	.067	.032	$p < .036$
Mean Secondary Contacts	.003	.045	$p < .939$
$R^2 = .059$			

Decision: Delete Secondary Contacts From The Models

Table 58
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Primary Contacts In Florida (N=273)

Variable	β	s.e.{β}	Sig.
<u>Equation 1</u>			
Intercept	.278	.030	$p < .001$
Mean Primary Contacts	.068	.017	$p < .001$
$r^2 = .058$			
<u>Equation 2</u>			
Intercept	.258	.031	$p < .001$
Mean Primary Contacts	.109	.023	$p < .001$
$Contacts^2$	-.047	.019	$p < .014$
$R^2 = .079$			
<u>Equation 3</u>			
Intercept	.394	.050	$p < .001$
Mean Primary Contacts	.023	.034	$p < .508$
$Contacts^2$	-.103	.025	$p < .001$
$Contacts^3$	.056	.017	$p < .001$
$R^2 = .116$			

Table 59
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Primary Contacts In Georgia (N=241)

Variable	ß	s.e.{ß}	Sig.
Equation 1			
Intercept	.286	.032	$p < .001$
Mean Primary Contacts	.119	.027	$p < .001$
$r^2 = .077$			
Equation 2			
Intercept	.275	.033	$p < .001$
Mean Primary Contacts	.142	.030	$p < .001$
$Contacts^2$	-.047	.027	$p < .081$
$R^2 = .089$			
Equation 3			
Intercept	.376	.046	$p < .001$
Mean Primary Contacts	.063	.039	$p < .105$
$Contacts^2$	-.140	.040	$p < .001$
$Contacts^3$	.083	.027	$p < .003$
$R^2 = .123$			

Table 60
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Knowledge Scores In Louisiana (N=278)

Variable	ß	s.e.{ß}	Sig.
<u>Equation 1</u>			
Intercept	.568	.013	$p < .001$
Knowledge Score	-.187	.016	$p < .001$
$r^2 = .342$			
<u>Equation 2</u>			
Intercept	.562	.013	$p < .001$
Knowledge Score	-.195	.016	$p < .001$
Knowledge $Score^2$	.056	.029	$p < .051$
$R^2 = .351$			
<u>Equation 3</u>			
Intercept	.595	.022	$p < .001$
Knowledge Score	-.233	.026	$p < .001$
Knowledge $Score^2$	.013	.037	$p < .718$
Knowledge $Score^3$	.086	.046	$p < .066$
$R^2 = .359$			

Table 61
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Surveillance Scores In Louisiana (N=278)

Variable	ß	s.e.{ß}	Sig.
<u>Equation 1</u>			
Intercept	.355	.010	$p < .001$
Surveillance Score	.190	.017	$p < .001$
$r^2 = .315$			
<u>Equation 2</u>			
Intercept	.359	.011	$p < .001$
Surveillance Score	.212	.021	$p < .001$
Surveillance Score^2	-.073	.043	$p < .093$
$R^2 = .322$			
<u>Equation 3</u>			
Intercept	.339	.020	$p < .001$
Surveillance Score	.239	.030	$p < .001$
Surveillance Score^2	.012	.082	$p < .884$
Surveillance Score^3	-.122	.099	$p < .219$
$R^2 = .326$			

Table 62
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Requirements Scores In Louisiana (N=278)

Variable	ß	s.e.{ß}	Sig.
Equation 1			
Intercept	.281	.021	$p < .001$
Requirements Score	.058	.007	$p < .001$
$r^2 = .192$			
Equation 2			
Intercept	.280	.021	$p < .001$
Requirements Score	.056	.007	$p < .001$
Requirements $Score^2$	.004	.004	$p < .402$
$R^2 = .194$			
Equation 3			
Intercept	.268	.035	$p < .001$
Requirements Score	.061	.013	$p < .001$
Requirements $Score^2$	.004	.005	$p < .361$
Requirements $Score^3$	-.001	.003	$p < .680$
$R^2 = .195$			

Table 63
Assessment Of Nonlinear Relationship Between Overall Positive Adjustment and Primary Contacts In South Carolina (N=310)

Variable	ß	s.e.{ß}	Sig.
<u>Equation 1</u>			
Intercept	.330	.037	$p < .001$
Mean Primary Contacts	.149	.036	$p < .001$
$r^2 = .052$			
<u>Equation 2</u>			
Intercept	.352	.035	$p < .001$
Mean Primary Contacts	.178	.035	$p < .001$
Contacts^2	-.255	.048	$p < .081$
$R^2 = .132$			
<u>Equation 3</u>			
Intercept	.449	.054	$p < .001$
Mean Primary Contacts	.083	.054	$p < .123$
Contacts^2	-.329	.057	$p < .001$
Contacts^3	.142	.061	$p < .020$
$R^2 = .147$			

Table 64
Estimated Impact of Within-Subject Changes in Supervision Intensity on Within-Subject Changes in Positive Adjustment

State and Interval	N =	γ	Sig.
<u>Florida</u>			
T2 - T1	186	.043	$p < .093$
T3 - T2	118	.077	$p < .005$
T4 - T3	63	.053	$p < .288$
End - T1	171	.112	$p < .001$
<u>Georgia</u>			
T2 - T1	174	.077	$p < .076$
T3 - T2	140	.109	$p < .002$
T4 - T3	77	.024	$p < .746$
End - T1	162	.056	$p < .165$
<u>South Carolina</u>			
T2 - T1	223	.056	$p < .395$
T3 - T2	201	.095	$p < .008$
T4 - T3	166	.165	$p < .001$
End - T1	223	.115	$p < .003$
<u>Louisiana</u>			
T2 - T1	259		
Knowledge		-.060	$p < .001$
Surveillance		.147	$p < .001$
Requirements		.059	$p < .001$
T3 - T2	214		
Knowledge		-.102	$p < .001$
Surveillance		.127	$p < .001$
Requirements		.036	$p < .001$
T4 - T3	178		
Knowledge		-.034	$p < .076$
Surveillance		.182	$p < .001$
Requirements		.032	$p < .001$
End - T1	259		
Knowledge		-.059	$p < .001$
Surveillance		.120	$p < .001$
Requirements		.043	$p < .001$

'Note: T=Time Period; γ = Parameter estimate for effect of a unit increase in Δ(supervision intensity) on Δ(positive adjustment).

Table 65
Tests For Sample x Supervision Intensity Interaction Terms in Florida (N=273)

Variable	F-test based on partial (Type III) SS	
Equation 1: Linear Model		
Sample	$F_{(2,267)} = 2.11$	$p < .123$
Contacts	$F_{(1,267)} = 17.97$	$p < .001$
Sample x Contacts	$F_{(2,267)} = 1.25$	$p < .290$
Equation 2: Quadratic Model		
Sample	$F_{(2,264)} = 0.56$	$p < .003$
Contacts	$F_{(1,264)} = 24.30$	$p < .001$
Contacts^2	$F_{(1,264)} = 5.71$	$p < .032$
Sample x Contacts	$F_{(2,264)} = 2.02$	$p < .135$
Sample x Contacts^2	$F_{(2,264)} = 3.41$	$p < .035$
Equation 3: Cubic Model		
Sample	$F_{(2,261)} = 0.82$	$p < .443$
Contacts	$F_{(1,261)} = 0.05$	$p < .816$
Contacts^2	$F_{(1,261)} = 19.75$	$p < .001$
Contacts^3	$F_{(1,261)} = 14.92$	$p < .001$
Sample x Contacts	$F_{(2,261)} = 1.28$	$p < .280$
Sample x Contacts^2	$F_{(2,261)} = 4.40$	$p < .013$
Sample x Contacts^3	$F_{(2,261)} = 0.38$	$p < .682$

Table 66
Specification of Effect of Sample and Supervision Intensity on Positive Adjustment in Florida (N=273)

Variable	ß	s.e.{ß}	Sig.
Intercept	.333	.061	$p < .001$
Shock Sample[a]	.076	.037	$p < .041$
Dropout Sample[a]	-.030	.040	$p < .452$
Contacts	.045	.035	$p < .203$
$Contacts^2$	-.086	.026	$p < .001$
$Contacts^3$	.045	.017	$p < .009$

$R^2 = .140$

[a]Reference category = prison parolees.

Table 67
Tests For Sample x Supervision Intensity Interaction Terms in Georgia (N=241)

Variable	F-test based on partial (Type III) SS	
<u>Equation 1: Linear Model</u>		
Sample	$F_{(2,235)} = 0.01$	$p < .999$
Contacts	$F_{(1,235)} = 13.90$	$p < .001$
Sample x Contacts	$F_{(2,235)} = 0.08$	$p < .927$
<u>Equation 2: Quadratic Model</u>		
Sample	$F_{(2,232)} = 0.07$	$p < .930$
Contacts	$F_{(1,232)} = 20.61$	$p < .001$
Contacts^2	$F_{(1,232)} = 18.66$	$p < .001$
Sample x Contacts	$F_{(2,232)} = 0.25$	$p < .777$
Sample x Contacts^2	$F_{(2,232)} = 6.57$	$p < .002$
<u>Equation 3: Cubic Model</u>		
Sample	$F_{(2,229)} = 3.21$	$p < .042$
Contacts	$F_{(1,229)} = 12.03$	$p < .001$
Contacts^2	$F_{(1,229)} = 30.67$	$p < .001$
Contacts^3	$F_{(1,229)} = 3.57$	$p < .060$
Sample x Contacts	$F_{(2,229)} = 3.70$	$p < .026$
Sample x Contacts^2	$F_{(2,229)} = 6.77$	$p < .001$
Sample x Contacts^3	$F_{(2,229)} = 5.44$	$p < .005$

Table 68
Specification of Effect of Sample and Supervision Intensity on Positive Adjustment in Georgia (N=241)

Variable	ß	s.e.{ß}	Sig.
Intercept	.393	.048	$p < .001$
Shock Sample[a]	-.045	.038	$p < .238$
Prison Parolees[a]	-.065	.039	$p < .102$
Contacts	.088	.042	$p < .036$
$Contacts^2$	-.156	.041	$p < .001$
$Contacts^3$	.079	.027	$p < .004$

$R^2 = .133$

[a]Reference category = probationers.

Table 69
Tests For Linear Sample x Supervision Intensity Interaction Terms in Louisiana (N=278)

Variable	F-test based on partial (Type III) SS	
Equation 1: Linear Model For Knowledge Scores		
Sample	$F_{(3,270)} = 3.51$	$p < .016$
Knowledge	$F_{(1,270)} = 40.71$	$p < .001$
Sample x Knowledge	$F_{(3,270)} = 0.39$	$p < .759$
Equation 2: Linear Model For Requirements Scores		
Sample	$F_{(3,270)} = 3.37$	$p < .019$
Requirements	$F_{(1,270)} = 15.34$	$p < .001$
Sample x Requirements	$F_{(3,270)} = 3.95$	$p < .009$
Equation 3: Linear Model For Surveillance Scores		
Sample	$F_{(3,270)} = 0.83$	$p < .476$
Surveillance	$F_{(1,270)} = 38.98$	$p < .001$
Sample x Surveillance	$F_{(3,270)} = 2.54$	$p < .057$

Table 70
Specification of Effect of Sample and Supervision Intensity on Positive Adjustment in Louisiana (N=278): Separate Effects Models

Variable	ß	s.e.{ß}	Sig.
<u>Equation 1: Knowledge Scores</u>			
Intercept	.530	.019	$p < .001$
Shock Sample[a]	.070	.019	$p < .001$
Prison Sample[a]	-.001	.018	$p < .954$
Dropout Sample[a]	.012	.031	$p < .694$
Knowledge	-.160	.017	$p < .001$
$R^2 = .380$			
<u>Equation 2: Surveillance Scores</u>			
Intercept	.348	.013	$p < .001$
Shock Sample[a]	.002	.026	$p < .835$
Prison Sample[a]	.011	.018	$p < .556$
Dropout Sample[a]	.030	.032	$p < .363$
Surveillance	.193	.025	$p < .001$
$R^2 = .315$			
<u>Equation 3: Requirements Scores</u>			
Intercept	.289	.024	$p < .001$
Shock Sample[a]	.089	.022	$p < .001$
Prison Sample[a]	.037	.019	$p < .058$
Dropout Sample[a]	.032	.034	$p < .350$
Requirements	.041	.009	$p < .001$
$R^2 = .239$			

[a]Reference category = probationers.

Table 71
Specification of Effect of Sample and Supervision Intensity on Positive Adjustment in Louisiana (N=278): Combined Effects Model

Variable	ß	s.e.{ß}	Sig.
Intercept	.430	.023	$p < .001$
Shock Sample[a]	-.044	.023	$p < .058$
Prison Sample[a]	-.004	.016	$p < .807$
Dropout Sample[a]	.005	.028	$p < .861$
Knowledge	-.148	.016	$p < .001$
Surveillance	.110	.026	$p < .001$
Requirements	.028	.008	$p < .001$

$R^2 = .491$

[a]Reference category = probationers.

Table 72
Tests For Sample x Supervision Intensity Interaction Terms in South Carolina (N=310)

Variable	F-test based on partial (Type III) SS	
Equation 1: Linear Model		
Sample	$F_{(4,300)} = 1.82$	$p < .125$
Contacts	$F_{(1,300)} = 19.64$	$p < .001$
Sample x Contacts	$F_{(4,300)} = 1.81$	$p < .127$
Equation 2: Quadratic Model		
Sample	$F_{(4,295)} = 1.49$	$p < .205$
Contacts	$F_{(1,295)} = 14.45$	$p < .001$
Contacts^2	$F_{(1,295)} = 19.38$	$p < .001$
Sample x Contacts	$F_{(4,295)} = 1.63$	$p < .166$
Sample x Contacts^2	$F_{(4,295)} = 1.16$	$p < .329$
Equation 3: Cubic Model		
Sample	$F_{(4,290)} = 1.39$	$p < .236$
Contacts	$F_{(1,290)} = 1.92$	$p < .167$
Contacts^2	$F_{(1,290)} = 12.79$	$p < .001$
Contacts^3	$F_{(1,290)} = 0.21$	$p < .644$
Sample x Contacts	$F_{(4,290)} = 1.62$	$p < .168$
Sample x Contacts^2	$F_{(4,290)} = 0.35$	$p < .844$
Sample x Contacts^3	$F_{(4,290)} = 0.79$	$p < .534$

Table 73
Specification of Effect of Sample and Supervision Intensity on Positive Adjustment in South Carolina (N=310)

Variable	ß	s.e.{ß}	Sig.
Intercept	.469	.059	$p < .001$
DPPPS Shock Sample[a]	.003	.045	$p < .944$
DOC Shock Sample[a]	-.054	.044	$p < .224$
Prison Parolees[a]	-.002	.048	$p < .972$
Split-Probationers[a]	-.017	.065	$p < .798$
Contacts	.079	.055	$p < .156$
$Contacts^2$	-.336	.058	$p < .001$
$Contacts^3$	.141	.061	$p < .022$

$R^2 = .154$

[a]Reference category = probationers.

Table 74a
Failure Criteria Distributions By State

State	Number of Cases	Percent Failing
<u>Failure by Arrest</u>		
Florida	289	52.6%
Georgia	262	----
Louisiana	278	45.3%
South Carolina	326	51.2%
<u>Revocation For New Crime</u>		
Florida	289	20.8%
Georgia	262	31.7%
Louisiana	278	11.5%
South Carolina	326	10.4%
<u>Revocation For Technical Violation</u>		
Florida	289	12.5%
Georgia	262	4.6%
Louisiana	278	10.4%
South Carolina	326	12.0%

Table 74b
Positive Adjustment Scores Across Failure Criteria

State	Mean Positive Adjustment Scores		
	Non-Failures	Failures	t-value[a]
Failure By Arrest			
Florida	0.513	0.257	8.87***
Louisiana	0.466	0.404	3.64***
South Carolina	0.571	0.354	7.30***
Revocation For New Crime			
Florida	0.426	0.188	8.06***
Georgia	0.459	0.328	4.51***
Louisiana	0.445	0.385	2.21*
South Carolina	0.489	0.210	7.22***
Revocation For Technical Violation			
Florida	0.396	0.249	3.82***
Georgia	0.425	0.249	2.52*
Louisiana	0.445	0.372	2.59**
South Carolina	0.487	0.261	5.98***

* $p < .05$
** $p < .01$
*** $p < .001$

[a]Note: Reported p-values are two-tailed.

Table 74c
Supervision Intensity Scores Across Failure Criteria[a]

State	Mean Supervision Intensity Scores: Non-Failures	Failures	t-value[b]
Failure By Arrest			
Florida	7.08	6.23	0.74
Louisiana (K)	1.15	1.35	-1.51
Louisiana (R)	2.56	2.91	-2.64**
Louisiana (S)	0.62	0.81	-1.92*
South Carolina	1.65	1.80	-1.01
Revocation For New Crime			
Florida	7.10	4.80	1.63
Georgia	2.82	1.98	2.70*
Louisiana (K)	1.22	1.40	-0.88
Louisiana (R)	2.69	2.93	-1.15
Louisiana (S)	0.68	0.91	-1.24
South Carolina	1.71	1.94	-0.88
Revocation For Technical Violation			
Florida	6.06	10.65	-2.66**
Georgia	2.56	2.39	0.19
Louisiana (K)	1.25	1.20	0.22
Louisiana (R)	2.65	3.28	-2.92**
Louisiana (S)	0.66	1.07	-2.52*
South Carolina	1.73	1.74	-0.03

* $p < .05$
** $p < .01$
*** $p < .001$

[a]Note: Supervision intensity scores represent mean number of monthly offender contacts (no log transformation) in Florida, Georgia, and South Carolina. In Louisiana, there are three measures. The (K) index represents lack of knowledge (ranges from 0 to 8) while the (R) index measures the level of requirements (ranges from 0 to 6) and the (S) index captures the level of offender surveillance (ranges from 0 to 5).
[b]Note: Reported p-values are two-tailed.

Table 74d
Zero-Order Correlation Coefficients Between Positive Adjustment and Supervision Intensity Stratified By Whether Offender Is Revoked For A Technical Violation During Follow-Up Period[a]

State	TV = No	TV = Yes	N=
Florida	+.317	+.075*	36
Georgia	+.258	+.488*	12
Louisiana (K)	-.609	-.484	29
Louisiana (S)	+.614	+.488	29
Louisiana (R)	+.483	+.442	29
South Carolina	+.209	+.331	38

* = correlation coefficient not significant at $p < .05$ level.

[a]Note: N refers to the number of subjects with technical violations and scores on both positive adjustment and supervision intensity measures. TV = Technical Violation.

Table 75
Assessment of Demographic and Offender Characteristics' Effects on Positive Adjustment in Florida

Variable	Mean Positive Adjustment/Correlation
Offender Race/Ethnicity	
Nonwhite	.340
White	.425
$t_{(278)} = 2.66;\ p < .008$	
Offender Age at Beginning of Community Supervision	$r = .192;\ p < .001$
Type of Offense	
Violent	.419
Drug Related	.387
Property and Other	.348
$F_{(2,277)}=2.01;\ p < .135$	
Offense For Current Sentence	
New Crime	.390
Technical Revocation	.310
$t_{(278)} = -1.82;\ p < .070$	
Offending History	
Prior Record	.337
No Prior Record	.392
$t_{(278)} = 1.51;\ p < .131$	

Table 76
Assessment of Demographic and Offender Characteristics' Effects on Positive Adjustment in Georgia

Variable	Mean Positive Adjustment/Correlation
Offender Race/Ethnicity	
Nonwhite	.378
White	.477
$t_{([illegible])} = 3.22;\ p < .002$	
Offender Age at Beginning of Community Supervision	$r = .032;\ p < .616$
Type of Offense	
Violent	.449
Drug Related	.448
Property and Other	.394
$F_{(2,243)} = 1.59;\ p < .206$	
Offense For Current Sentence	
New Crime	.421
Technical Revocation	.398
$t_{(243)} = -0.59;\ p < .556$	
Offending History	
Prior Record	.373
No Prior Record	.450
$t_{(244)} = 2.55;\ p < .011$	

Table 77
Assessment of Demographic and Offender Characteristics' Effects on Positive Adjustment in Louisiana

Variable	Mean Positive Adjustment/Correlation
Offender Race/Ethnicity	
Nonwhite	.405
White	.487
$t_{(268)} = 4.63;\ p < .001$	
Offender Age at Beginning of Community Supervision	$r = .067;\ p < .266$
Offender Age at First of Arrest	$r = -.016;\ p < .802$
Type of Offense	
Violent	.387
Drug Related	.483
Property and Other	.432
$F_{(2,248)}=5.37;\ p < .005$	
Offense For Current Sentence	
New Crime	.440
Technical Revocation	.439
$t_{(254)} = -0.04;\ p < .966$	
Offending History	
Prior Record	.439
No Prior Record	.424
$t_{(268)} = -0.79;\ p < .430$	

Table 78
Assessment of Demographic and Offender Characteristics' Effects on Positive Adjustment in New York

Variable	Mean Positive Adjustment/Correlation
Offender Race/Ethnicity	
Nonwhite	.489
White	.592
$t_{(235)} = 2.11;\ p < .036$	
Offender Age at Beginning of Community Supervision	$r = .156;\ p < .016$
Offender Age at First of Arrest	$r = .232;\ p < .001$
Type of Offense	
Drug Related	.526
Property	.450
Violent and Other	.522
$F_{(2,234)}=1.19;\ p < .306$	
Offending History	
Prior Record	.502
No Prior Record	.573
$t_{(235)} = 1.12;\ p < .265$	

Table 79
Assessment of Demographic and Offender Characteristics' Effects on Positive Adjustment in South Carolina

Variable	Mean Positive Adjustment/Correlation
Offender Race/Ethnicity	
Nonwhite	.402
White	.550
$t_{(324)} = 4.68;\ p < .001$	
Offender Age at Beginning of Community Supervision	$r = .048;\ p < .388$
Offender Age at First of Arrest	$r = .073;\ p < .192$
Type of Offense	
Violent	.511
Drug Related	.483
Property and Other	.440
$F_{(2,322)}=1.39;\ p < .252$	
Offense For Current Sentence	
New Crime	.463
Technical Revocation	.429
$t_{(324)} = -0.65;\ p < .519$	
Offending History	
Prior Record	.433
No Prior Record	.515
$t_{(324)} = 2.40;\ p < .017$	

Table 80
Analysis of Positive Adjustment For Subjects Completing Three Months of Evaluation In Florida (N=260)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	-0.43**			
Shock Sample[1]	0.13***			
Dropout Sample[1]	-0.03			
Nonwhite Indicator	-0.14***			
Age at Comm. Supv.	0.03***			
Violent Offense[2]	0.14***			
Drug Offense[2]	0.07			
New Crime Indicator	0.14***			
Contacts w/Offender	0.006***			
Overall Mean	0.422			
Adj. Shock Mean[3]	0.511[a]			
Adj. Dropout Mean[3]	0.352[b]			
Adj. Prison Mean[3]	0.379[b]			
R^2	.220			

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 81
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Six Months of Evaluation in Florida (N=196)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	-0.37	-0.69***		
Shock Sample[1]	0.10**	0.12**		
Dropout Sample[1]	-0.01	0.02		
Nonwhite Indicator	-0.15***	-0.15***		
Age at Comm. Supv.	0.03***	0.05***		
Violent Offense[2]	0.16***	0.08		
Drug Offense[2]	0.13**	0.18***		
New Crime Indicator	0.14**	0.19***		
Contacts w/Offender	0.007***	0.006***		
Overall Mean	0.450	0.407		
Adj. Shock Mean[3]	0.511[a]	0.476[a]		
Adj. Dropout Mean[3]	0.401[b]	0.374[a,b]		
Adj. Prison Mean[3]	0.414[b]	0.356[b]		
R^2	.220	.207		

Within-Subjects Analysis[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,187)}=1.87$; $p < .173$

Effect of TIME x Violent Offense Indicator
Test of Hypothesis That There is No Change in the Effect of the VIOLENT Crime Indicator Over Two Measurement Periods: $F_{(1,187)}=3.27$; $p < .072$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 82
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Nine Months of Evaluation in Florida (N=123)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.05	-0.32	-0.60*	
Shock Sample[1]	0.09*	0.12**	0.20***	
Dropout Sample[1]	0.08	-0.02	0.04	
Nonwhite Indicator	-0.21***	-0.16***	-0.17***	
Age at Comm. Supv.	0.02	0.03**	0.04**	
Violent Offense[2]	0.14**	0.06	0.09	
Drug Offense[2]	0.08	0.13	0.13	
New Crime Indicator	0.13*	0.12	0.10	
Contacts w/Offender	0.01***	0.009***	0.01***	
Overall Mean	0.528	0.490	0.419	
Adj. Shock Mean[3]	0.569[a]	0.560[a]	0.525[a]	
Adj. Dropout Mean[3]	0.557[a]	0.421[a,b]	0.368[a,b]	
Adj. Prison Mean[3]	0.480[a]	0.439[b]	0.329[b]	
R^2	.257	.164	.219	

Within-Subjects Analysis[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,228)}=1.95;\ p < .145$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 83
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Twelve Months of Evaluation in Florida (N=58)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.25	-0.18	-0.44	-0.46
Shock Sample[1]	0.01	0.04	0.06	-0.04
Dropout Sample[1]	0.05	-0.09	0.10	0.05
Nonwhite Indicator	-0.19***	-0.12	-0.09	-0.12
Age at Comm. Supv.	0.01	0.04*	0.05**	0.05**
Violent Offense[2]	0.12	0.08	0.11	-0.02
Drug Offense[2]	0.08	-0.02	0.08	0.03
New Crime Indicator	0.02	0.04	-0.10	-0.02
Contacts w/Offender	0.008**	0.005	0.01***	0.009**
Overall Mean	0.576[a]	0.571[a]	0.540[a]	0.480[a]
Adj. Shock Mean[3]	0.576[a]	0.605[a]	0.569[a]	0.448[a]
Adj. Dropout Mean[3]	0.618[a]	0.479[a]	0.604[a]	0.538[a]
Adj. Prison Mean[3]	0.568[a]	0.565[a]	0.507[a]	0.491[a]
R^2	.263	.190	.259	.266

Within-Subjects Analysis[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,147)}=1.04;\ p < .377$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 84
Analysis of Positive Adjustment For Subjects Completing Three Months of Evaluation In Georgia (N=210)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.40***			
Shock Sample[1]	0.06			
Prison Sample[1]	0.05			
Nonwhite Indicator	-0.01			
Violent Offense[2]	0.00			
Drug Offense[2]	0.05			
Priors Indicator	-0.08*			
Contacts w/Offender	0.01**			
Overall Mean	0.448			
Adj. Shock Mean[3]	0.468[a]			
Adj. Prison Mean[3]	0.466[a]			
Adj. Probation Mean[3]	0.411[a]			
R^2	.049			

* $p \le .10$
** $p \le .05$
*** $p \le .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 85
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Six Months of Evaluation in Georgia (N=174)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.39***	0.43***		
Shock Sample[1]	0.08	-0.04		
Prison Sample[1]	0.09	0.02		
Nonwhite Indicator	-0.01	-0.16***		
Violent Offense[2]	-0.02	0.08		
Drug Offense[2]	0.05	0.08		
Priors Indicator	-0.11*	-0.03		
Contacts w/Offender	0.02**	0.02***		
Overall Mean	0.452	0.411		
Adj. Shock Mean[3]	0.475[a]	0.376[a]		
Adj. Prison Mean[3]	0.480[a]	0.438[a]		
Adj. Probation Mean[3]	0.393[a]	0.417[a]		
R^2	.077	.154		

Within-Subjects Analysis[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,166)}=0.14$; $p < .709$

Effect of TIME x Nonwhite Indicator
Test of Hypothesis That There is No Change in the Effect of the Nonwhite Indicator Over Two Measurement Periods: $F_{(1,166)}=5.11$; $p < .025$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

Table 86
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Nine Months of Evaluation in Georgia (N=128)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.38***	0.49***	0.44***	
Shock Sample[1]	0.09	-0.03	0.01	
Prison Sample[1]	0.13*	-0.01	0.04	
Nonwhite Indicator	-0.02	-0.20***	-0.17***	
Violent Offense[2]	-0.00	0.08	0.19**	
Drug Offense[2]	0.06	0.08	0.10	
Priors Indicator	-0.11*	-0.03	-0.10	
Contacts w/Offender	0.02*	0.02**	0.01	
Overall Mean	0.452	0.424	0.382	
Adj. Shock Mean[3]	0.463[a]	0.408[a]	0.372[a]	
Adj. Prison Mean[3]	0.504[a]	0.425[a]	0.405[a]	
Adj. Probation Mean[3]	0.376[a]	0.439[a]	0.365[a]	
R^2	.072	.164	.157	

Within-Subjects Analysis[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,240)}=0.04$; $p < .958$

Effect of TIME x Nonwhite Indicator
Test of Hypothesis That There is No Change in the Effect of the Nonwhite Indicator Over Three Measurement Periods: $F_{(2,240)}=3.71$; $p < .026$

Effect of TIME x Violent Crime Indicator
Test of Hypothesis That There is No Change in the Effect of the Violent Offense Indicator Over Three Measurement Periods: $F_{(2,240)}=2.28$; $p < .105$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$
[1]Note: Reference category is comprised of probationers.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

Table 87
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Twelve Months of Evaluation in Georgia (N=63)

Variable	Effects on Positive Adjustment			
	M3	M6	M9	M12
Intercept	0.31**	0.41***	0.51***	0.40***
Shock Sample[1]	0.13	-0.00	-0.00	0.06
Prison Sample[1]	0.09	0.09	0.08	0.09
Nonwhite Indicator	-0.08	-0.28***	-0.30***	-0.24***
Violent Offense[2]	0.07	0.08	0.08	0.07
Drug Offense[2]	0.02	0.01	0.13	0.14
Priors Indicator	0.03	-0.01	-0.09	-0.13
Contacts w/Offender	0.03	0.05***	0.03	0.03**
Overall Mean	0.450	0.384	0.397	0.360
Adj. Shock Mean[3]	0.494[a]	0.347[a]	0.363[a]	0.358[a]
Adj. Prison Mean[3]	0.446[a]	0.441[a]	0.447[a]	0.392[a]
Adj. Probation Mean[3]	0.361[a]	0.347[a]	0.366[a]	0.299[a]
R^2	.098	.328	.278	.278

<u>Within-Subjects Analysis</u>[4]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,165)}=0.70$; $p < .550$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$
[1]Note: Reference category is comprised of probationers.
[2]Note: Reference category is comprised of all other offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.
[4]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

Table 88
Analysis of Positive Adjustment For Subjects Completing Three Months of Evaluation In Louisiana (N=253)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.34***			
Shock Sample[1]	-0.07***			
Prison Sample[1]	0.01			
Shock Dropout Sample[1]	0.00			
Nonwhite Indicator	-0.05***			
Age at Comm. Supv.	0.003***			
New Crime Indicator	0.01			
Knowledge Index	-0.05***			
Requirements Index	0.03***			
Surveillance Index	0.08***			
Overall Mean	0.484			
Adj. Shock Mean[2]	0.431[a]			
Adj. Prison Mean[2]	0.509[b]			
Adj. Probation Mean[2]	0.501[b]			
Adj. Dropout Mean[2]	0.503[b]			
R^2	.613			

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.

Table 89
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Six Months of Evaluation In Louisiana (N=236)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.42***	0.39***		
Shock Sample[1]	-0.04*	-0.10***		
Prison Sample[1]	0.00	-0.03		
Shock Dropout Sample[1]	0.03	0.02		
Nonwhite Indicator	-0.05***	-0.03**		
Age at Comm. Supv.	0.002**	0.002		
New Crime Indicator	-0.01	0.03*		
Knowledge Index	-0.05***	-0.08***		
Requirements Index	0.01*	0.02**		
Surveillance Index	0.09***	0.09***		
Overall Mean	0.496	0.461		
Adj. Shock Mean[2]	0.463[a]	0.393[a]		
Adj. Prison Mean[2]	0.510[b]	0.471[b]		
Adj. Probation Mean[2]	0.506[a,b]	0.496[b]		
Adj. Dropout Mean[2]	0.531[b]	0.516[b]		
R^2	.656	.549		

Within-Subjects Analysis[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,226)}=0.90$; $p < .343$
Effect of TIME x New Crime Indicator
Test of Hypothesis That The Effect of the New Crime Indicator Is Time-Stable Over Two Measurement Periods: $F_{(1,226)}=3.83$; $p < .052$
Effect of TIME x Knowledge Index
Test of Hypothesis That The Effect of the Knowledge Index Is Time-Stable Over Two Measurement Periods: $F_{(1,226)}=8.9$; $p < .003$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$
[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 90
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Nine Months of Evaluation In Louisiana (N=194)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.43***	0.38***	0.36***	
Shock Sample[1]	0.00	-0.02	-0.05	
Prison Sample[1]	0.03	0.02	-0.01	
Shock Dropout Sample[1]	0.02	0.05*	0.02	
Nonwhite Indicator	-0.04***	-0.03**	-0.03	
Age at Comm. Supv.	0.002**	0.004***	0.004***	
New Crime Indicator	-0.03*	-0.01	-0.01	
Knowledge Index	-0.04***	-0.07***	-0.08***	
Requirements Index	0.01	0.02*	0.03**	
Surveillance Index	0.10***	0.09***	0.04**	
Overall Mean	0.503	0.493	0.439	
Adj. Shock Mean[2]	0.498[a]	0.472[a]	0.401[a]	
Adj. Prison Mean[2]	0.521[a]	0.513[a,b]	0.446[a]	
Adj. Probation Mean[2]	0.494[a]	0.488[a,b]	0.454[a]	
Adj. Dropout Mean[2]	0.517[a]	0.543[b]	0.478[a]	
R^2	.626	.704	.481	

<u>Within-Subjects Analysis</u>[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,368)}=1.29$; $p < .278$
Effect of TIME x Knowledge Index
Test of Hypothesis That The Effect of the Knowledge Index Is Time-Stable Over Three Measurement Periods: $F_{(2,368)}=10.77$; $p < .001$
Effect of TIME x Surveillance Index
Test of Hypothesis That The Effect of the Surveillance Index Is Time-Stable Over Three Measurement Periods: $F_{(2,368)}=6.39$; $p < .002$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$
[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 91
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Twelve Months of Evaluation In Louisiana (N=159)

Variable	Effects on Positive Adjustment			
	M3	M6	M9	M12
Intercept	0.42***	0.42***	0.44***	0.39***
Shock Sample[1]	0.03	0.04	0.01	-0.02
Prison Sample[1]	0.04	0.05**	0.03	-0.00
Shock Dropout Sample[1]	0.03	0.06*	0.05	-0.00
Nonwhite Indicator	-0.05***	-0.04**	-0.02	-0.04**
Age at Comm. Supv.	0.003*	[illegible]*	0.003*	0.005***
New Crime Indicator	-0.02	-0.01	0.02	0.01
Knowledge Index	-0.04***	-0.06***	-0.08***	-0.09***
Requirements Index	0.01	0.[illegible]	-0.00	0.01
Surveillance Index	0.10***	0.09***	0.06***	0.00
Overall Mean	0.511	0.504	0.471	0.422
Adj. Shock Mean[2]	0.526[a]	0.521[a,b]	0.467[a]	0.408[a]
Adj. Prison Mean[2]	0.529[a]	0.528[b]	0.493[a]	0.424[a]
Adj. Probation Mean[2]	0.492[a]	0.479[a]	0.459[a]	0.429[a]
Adj. Dropout Mean[2]	0.523[a]	0.539[a,b]	0.510[a]	0.426[a]
R^2	.585	.661	.561	.413

<u>Within-Subjects Analysis</u>[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,447)}=0.72;\ p < .538$
Effect of TIME x Knowledge Index
Test of Hypothesis That The Effect of the Knowledge Index Is Time-Stable Over Four Measurement Periods: $F_{(3,447)}=9.28;\ p < .001$
Effect of TIME x Surveillance Index
Test of Hypothesis That The Effect of the Surveillance Index Is Time-Stable Over Four Measurement Periods: $F_{(3,447)}=8.63;\ p < .001$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$
[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

Table 92
Analysis of Positive Adjustment For Subjects Completing Three Months of Evaluation In New York (N=233)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.33			
Shock Sample[1]	0.05			
Dropout Sample[1]	-0.08			
Nonwhite Indicator	-0.07			
Age at First Arrest	0.01			
Other/Violent Offense[2]	0.07			
Drug Offense[2]	0.03			
Priors Indicator	-0.02			
Overall Mean	0.542			
Adj. Shock Mean[3]	0.596[a]			
Adj. Prison Mean[3]	0.550[a,b]			
Adj. Dropout Mean[3]	0.473[b]			
R^2	.064			

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of property offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 93
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Six Months of Evaluation In New York (N=184)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.34	-0.19		
Shock Sample[1]	0.04	0.05		
Dropout Sample[1]	-0.05	0.03		
Nonwhite Indicator	-0.05	-0.11*		
Age at First Arrest	0.02*	0.04***		
Other/Violent Offense[2]	0.04	0.12		
Drug Offense[2]	0.01	0.11		
Priors Indicator	-0.03	0.04		
Overall Mean	0.599	0.573		
Adj. Shock Mean[3]	0.596[a]	0.596[a]		
Adj. Prison Mean[3]	0.550[a,b]	0.572[a]		
Adj. Dropout Mean[3]	0.473[b]	0.547[a]		
R^2	.064	.111		

Within-Subjects Analysis[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,176)}=9.04$; $p < .003$

Effect of TIME x Age at First Arrest
Test of Hypothesis That The Effect of Age at First Arrest Is Time-Stable Over Two Measurement Periods: $F_{(1,176)}=7.14$; $p < .008$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of property offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 94
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Nine Months of Evaluation In New York (N=151)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.46^{**}	0.03	0.13	
Shock Sample[1]	0.05	0.05	0.11	
Dropout Sample[1]	-0.07	-0.00	-0.01	
Nonwhite Indicator	-0.05	-0.13^{**}	-0.18	
Age at First Arrest	0.01	0.04^{***}	0.03	
Other/Violent Offense[2]	0.01	0.08	0.05	
Drug Offense[2]	-0.01	0.03	0.05	
Priors Indicator	-0.02	-0.00	0.01	
Overall Mean	0.634	0.642	0.579	
Adj. Shock Mean[3]	0.688^{a}	0.675^{a}	0.652^{a}	
Adj. Prison Mean[3]	$0.633^{a,b}$	0.622^{a}	$0.540^{a,b}$	
Adj. Dropout Mean[3]	0.565^{b}	0.619^{a}	0.528^{b}	
R^2	.068	.111	.143	

<u>Within-Subjects Analysis</u>[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,142)}=2.97$; $p < .055$

Effect of TIME x Age at First Arrest
Test of Hypothesis That The Effect of Age at First Arrest Is Time-Stable Over Three Measurement Periods: $F_{(2,142)}=4.02$; $p < .020$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of property offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 95
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Twelve Months of Evaluation In New York (N=133)

Variable	Effects on Positive Adjustment M3	M6	M9	M12
Intercept	0.46**	0.06	0.19	0.03
Shock Sample[1]	0.04	0.06	0.11*	0.11
Dropout Sample[1]	-0.04	0.06	0.01	0.03
Nonwhite Indicator	-0.05	-0.13**	-0.20	-0.17**
Age at First Arrest	0.01	0.03***	0.02	0.03**
Other/Violent Offense[2]	0.01	0.04	0.05	0.05
Drug Offense[2]	-0.03	0.01	0.06	0.07
Priors Indicator	-0.02	0.01	0.04	-0.03
Overall Mean	0.641	0.666	0.607	0.579
Adj. Shock Mean[3]	0.679[a]	0.686[a]	0.673[a]	0.639[a]
Adj. Prison Mean[3]	0.635[a]	0.627[a]	0.561[a]	0.555[a]
Adj. Dropout Mean[3]	0.595[a]	0.688[a]	0.570[a]	0.526[a]
R^2	.042	.125	.137	.143

<u>Within-Subjects Analysis</u>[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,123)}=2.108$; $p < .103$

Effect of TIME x Nonwhite Indicator
Test of Hypothesis That The Effect of the Nonwhite Indicator Is Time-Stable Over Four Measurement Periods: $F_{(3,123)}=2.25$; $p < .086$

Effect of TIME x Age at First Arrest
Test of Hypothesis That The Effect of Age at First Arrest Is Time-Stable Over Four Measurement Periods: $F_{(3,123)}=3.25$; $p < .024$

* $p \le .10$
** $p \le .05$
*** $p \le .01$

[1]Note: Reference category is comprised of prison parolees.
[2]Note: Reference category is comprised of property offenders.
[3]Note: Samples with different letters are significantly different at the .05 α error level.

Table 96
Analysis of Positive Adjustment For Subjects Completing Three Months of Evaluation In South Carolina (N=228)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.20			
Shock Sample[1]	-0.03			
Prison Sample[1]	0.02			
Split-Probation Sample[1]	-0.04			
Nonwhite Indicator	-0.12***			
Age at Comm. Supv.	0.02*			
Priors Indicator	-0.05			
Contacts w/Offender	0.01			
Overall Mean	0.521			
Adj. Shock Mean[2]	0.501[a]			
Adj. Prison Mean[2]	0.546[a]			
Adj. Probation Mean[2]	0.530[a]			
Adj. Sp. Probation Mean[2]	0.492[a]			
R^2	.065			

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.

Table 97
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Six Months of Evaluation In South Carolina (N=220)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.22	0.28		
Shock Sample[1]	-0.03	-0.02		
Prison Sample[1]	-0.00	-0.00		
Split-Probation Sample[1]	-0.00	0.06		
Nonwhite Indicator	-0.14***	-0.16***		
Age at Comm. Supv.	0.02*	0.01		
Priors Indicator	-0.06	-0.06		
Contacts w/Offender	0.01	0.03**		
Overall Mean	0.519	0.483		
Adj. Shock Mean[2]	0.501[a]	0.464[a]		
Adj. Prison Mean[2]	0.527[a]	0.485[a]		
Adj. Probation Mean[2]	0.530[a]	0.485[a]		
Adj. Sp. Probation Mean[2]	0.526[a]	0.549[a]		
R^2	.078	.102		

Within-Subjects Analysis[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Two Measurement Periods: $F_{(1,212)}=0.10$; $p < .752$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant (p < .10) changes in the effects of predictors over values of TIME are reported.

Table 98
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Nine Months of Evaluation In South Carolina (N=198)

	Effects on Positive Adjustment			
Variable	M3	M6	M9	M12
Intercept	0.18	0.31	0.20	
Shock Sample[1]	-0.01	0.03	-0.01	
Prison Sample[1]	-0.02	0.02	-0.01	
Split-Probation Sample[1]	-0.01	0.08	-0.06	
Nonwhite Indicator	-0.14***	-0.14***	-0.18***	
Age at Comm. Supv.	0.02*	0.01	0.02	
Priors Indicator	-0.05	-0.05	-0.05	
Contacts w/Offender	0.03**	0.03**	0.03**	
Overall Mean	0.526	0.500	0.474	
Adj. Shock Mean[2]	0.523[a]	0.502[a]	0.479[a]	
Adj. Prison Mean[2]	0.518[a]	0.501[a]	0.475[a]	
Adj. Probation Mean[2]	0.537[a]	0.476[a]	0.484[a]	
Adj. Sp. Probation Mean[2]	0.524[a]	0.555[a]	0.423[a]	
R^2	.085	.083	.107	

Within-Subjects Analysis[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Three Measurement Periods: $F_{(2,380)}=0.35$; $p < .702$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

Table 99
Repeated Measures Analysis of Positive Adjustment For Subjects Completing Twelve Months of Evaluation In South Carolina (N=163)

Variable	Effects on Positive Adjustment			
	M3	M6	M9	M12
Intercept	0.36	0.25	0.21	0.41
Shock Sample[1]	-0.05	0.00	-0.02	-0.07
Prison Sample[1]	-0.08	0.06	0.06	0.13*
Split-Probation Sample[1]	-0.01	0.07	-0.06	0.02
Nonwhite Indicator	-0.15***	-0.13***	-0.16***	-0.19***
Age at Comm. Supv.	0.01	0.01	0.02	0.01
Priors Indicator	-0.03	-0.05	-0.03	-0.11**
Contacts w/Offender	0.02	0.05**	0.04*	0.02
Overall Mean	0.528	0.517	0.502	0.470
Adj. Shock Mean[2]	0.516[a]	0.497[a]	0.481[a]	0.397[a]
Adj. Prison Mean[2]	0.484[a]	0.555[a]	0.565[a]	0.598[b]
Adj. Probation Mean[2]	0.566[a]	0.495[a]	0.504[a]	0.464[a,b]
Adj. Sp. Probation Mean[2]	0.553[a]	0.566[a]	0.444[a]	0.481[a,b]
R^2	.078	.092	.097	.137

<u>Within-Subjects Analysis</u>[3]

Effect of TIME
Test of Hypothesis That There is No Within Subject Change Over Four Measurement Periods: $F_{(3,465)}=0.28$; $p < .840$

Effect of TIME x SAMPLE
Test of Hypothesis That The Effect of Sample Membership is Time-Stable Over Four Measurement Periods: $F_{(9,465)}=2.04$; $p < .033$

* $p \leq .10$
** $p \leq .05$
*** $p \leq .01$

[1]Note: Reference category is comprised of probationers.
[2]Note: Samples with different letters are significantly different at the .05 α error level.
[3]Note: Effect of TIME and statistically significant ($p < .10$) changes in the effects of predictors over values of TIME are reported.

LIST OF FIGURES

Figure 1
Sources of Subject Attrition in Florida

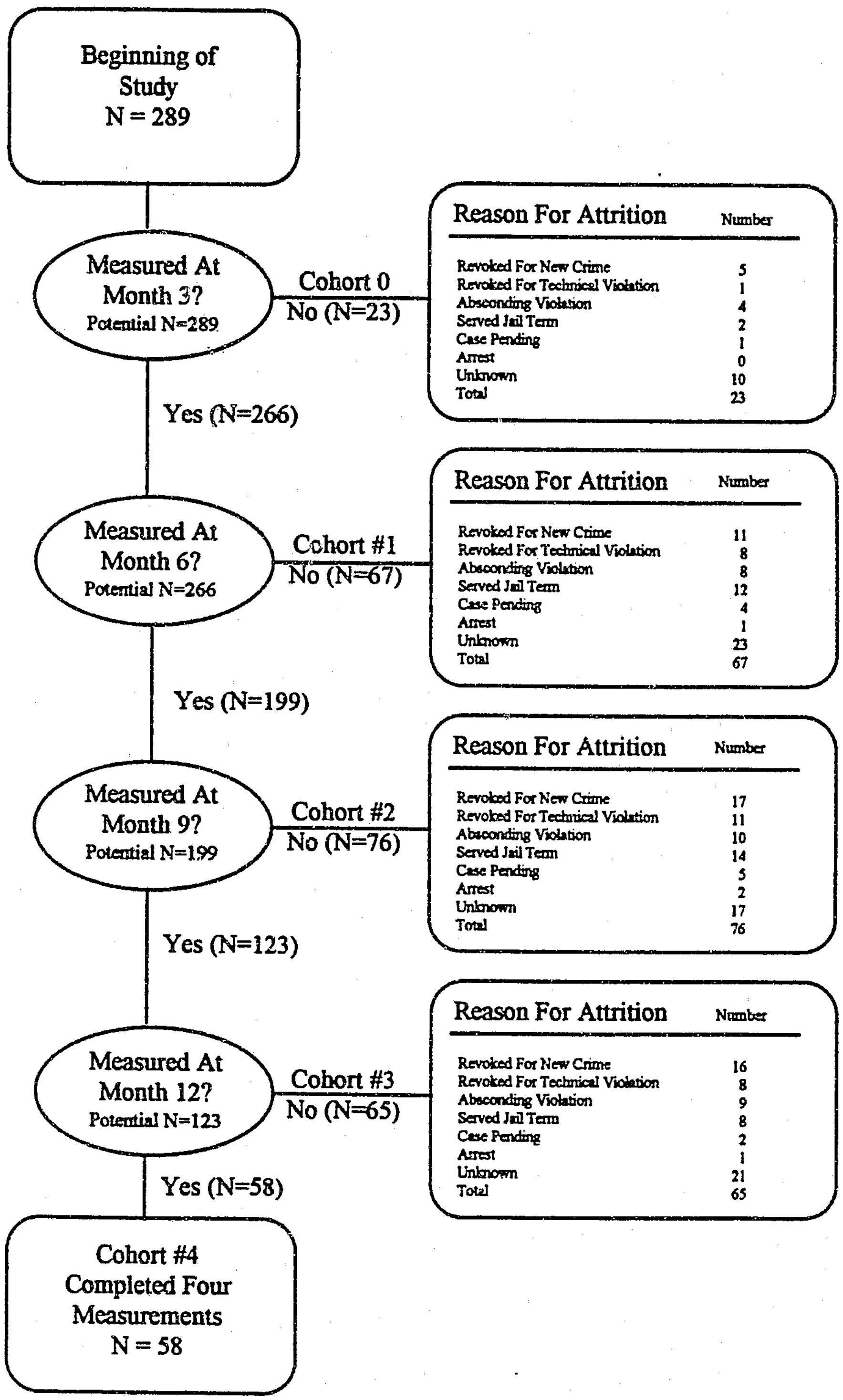


Figure 2
Sources of Subject Attrition in Georgia

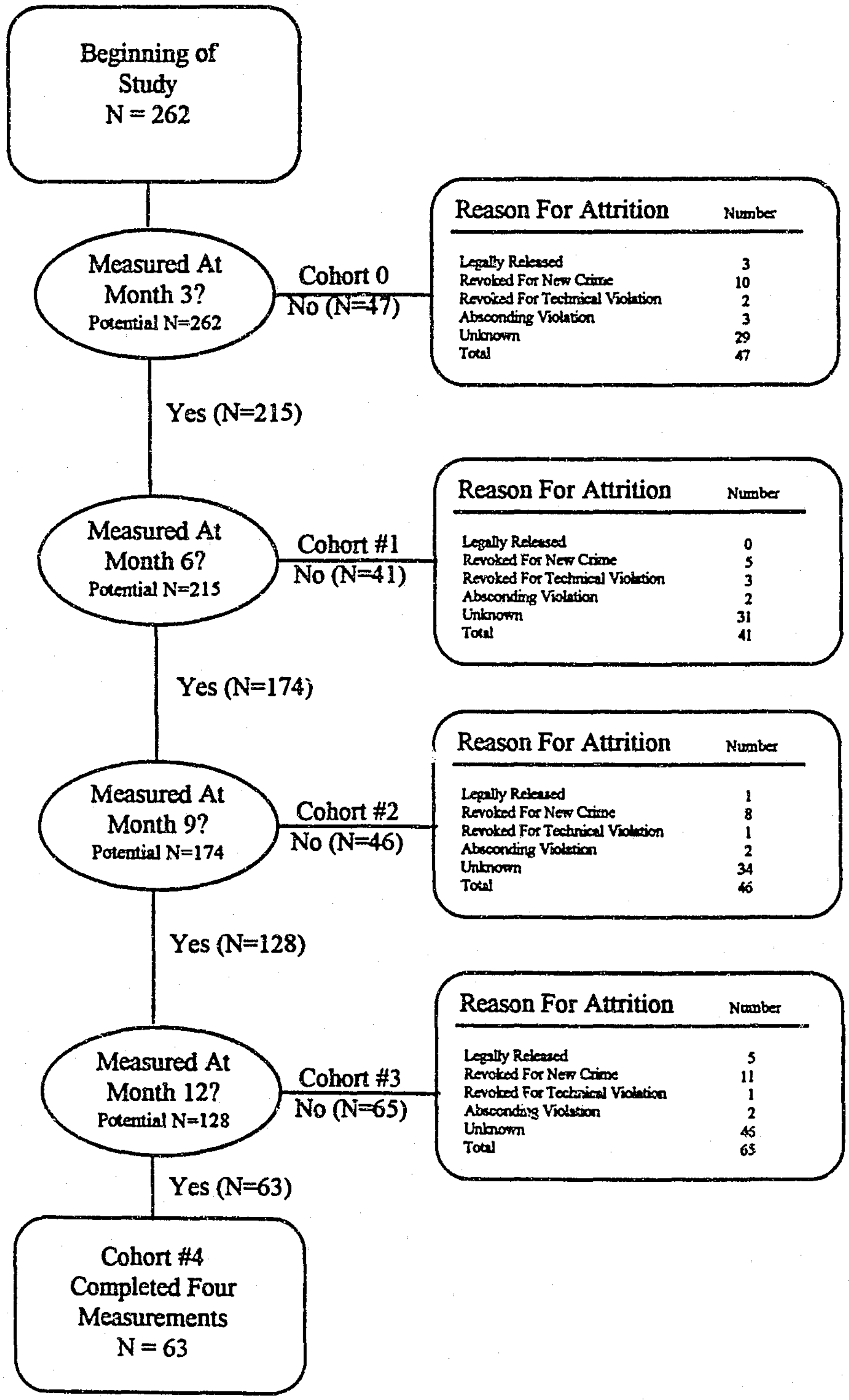


Figure 3
Sources of Subject Attrition in Louisiana

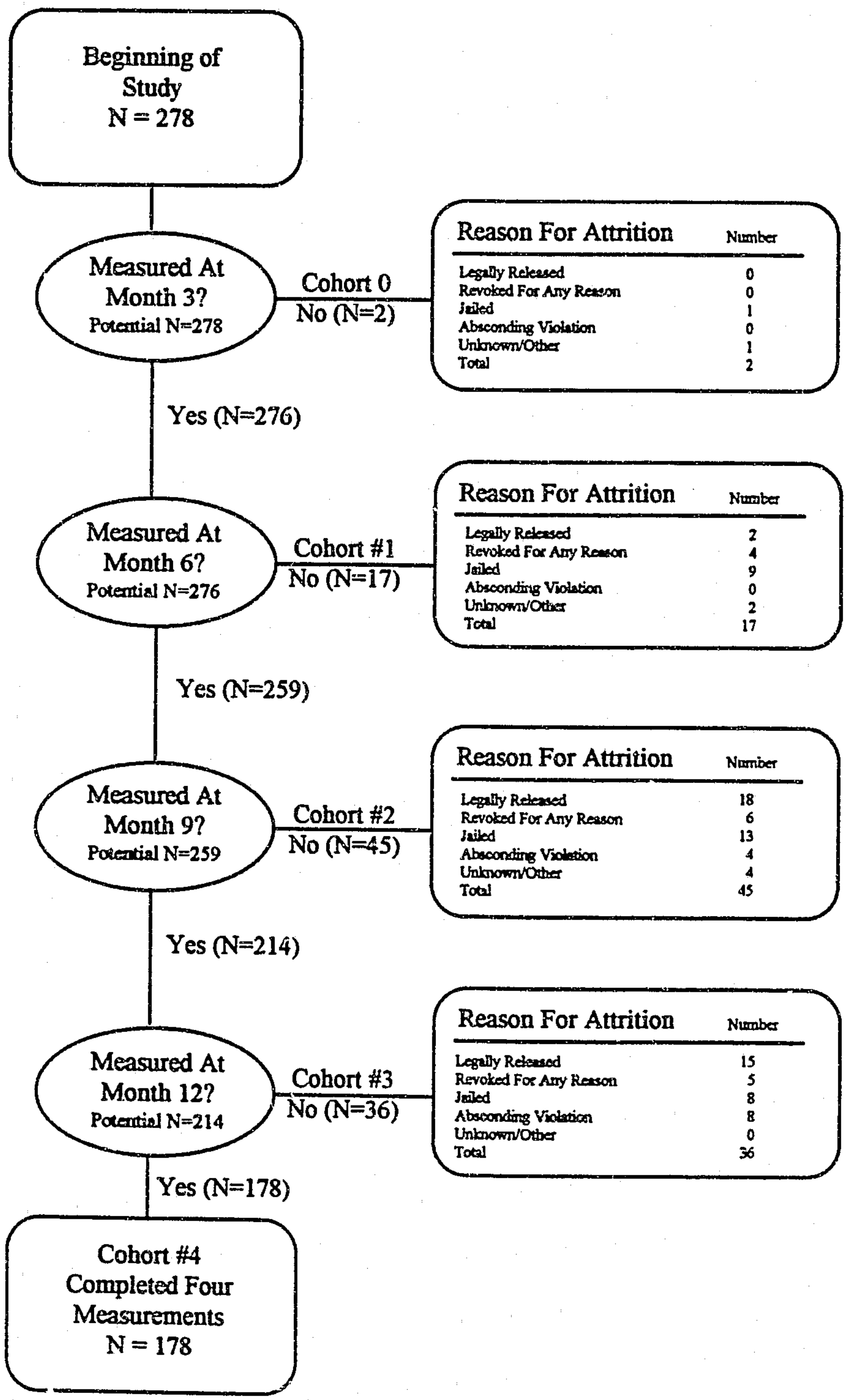


Figure 4
Sources of Subject Attrition in New York

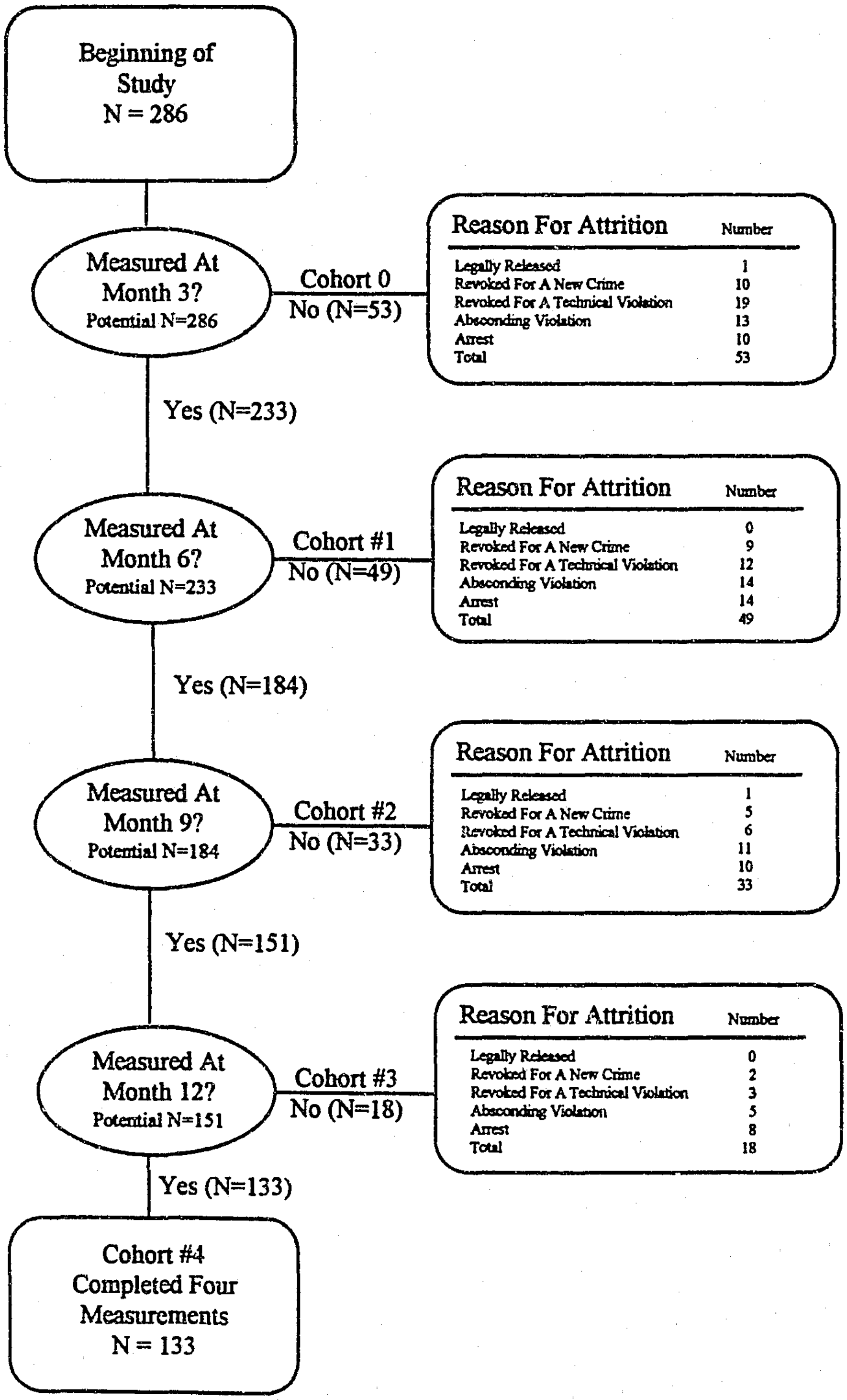


Figure 5
Sources of Subject Attrition in South Carolina

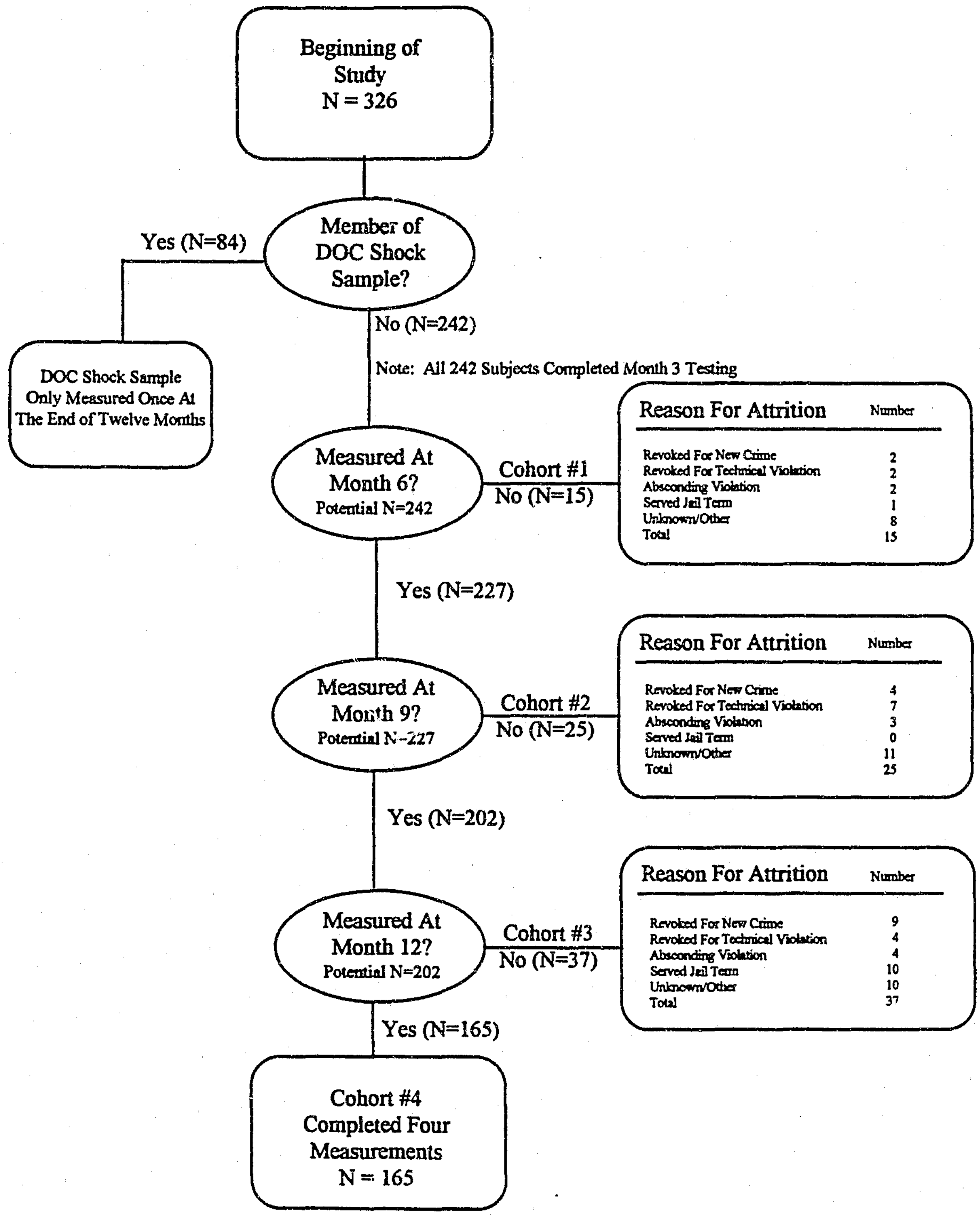


Figure 6
Posttest Design With Nonequivalent Groups

Nonequivalent Comparison Groups		Four Observation Points			
X_{T1}	——	O	O	O	O
X_{T2}	——	O	O	O	O
X_{T3}	——	O	O	O	O
•					
•					
•					
•					
X_{TK}	——	O	O	O	O

Note: X's denote groups whose members have not been randomly assigned and O's denote separate observations (Cook and Campbell, 1979).

Figure 7
Primary Contact Levels Over Follow-Up Period in Florida

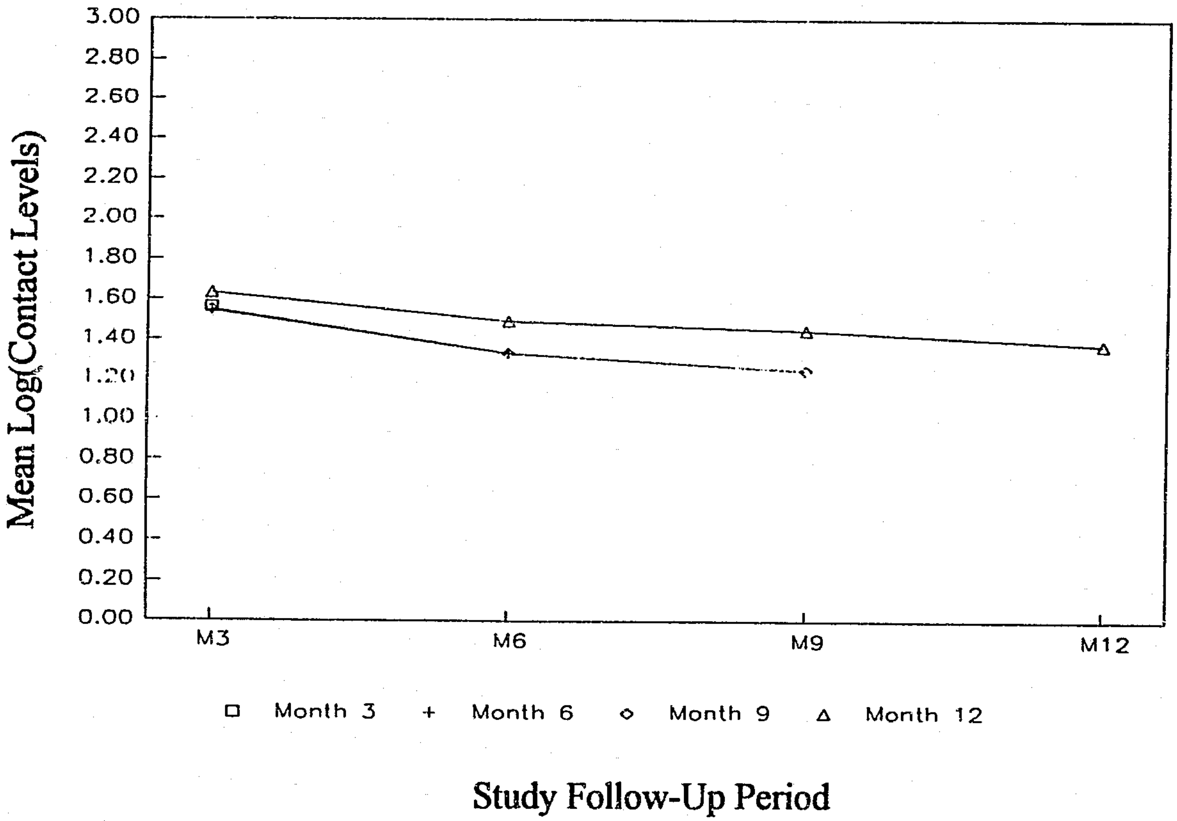


Figure 8
Secondary Contact Levels Over Follow-Up Period in Florida

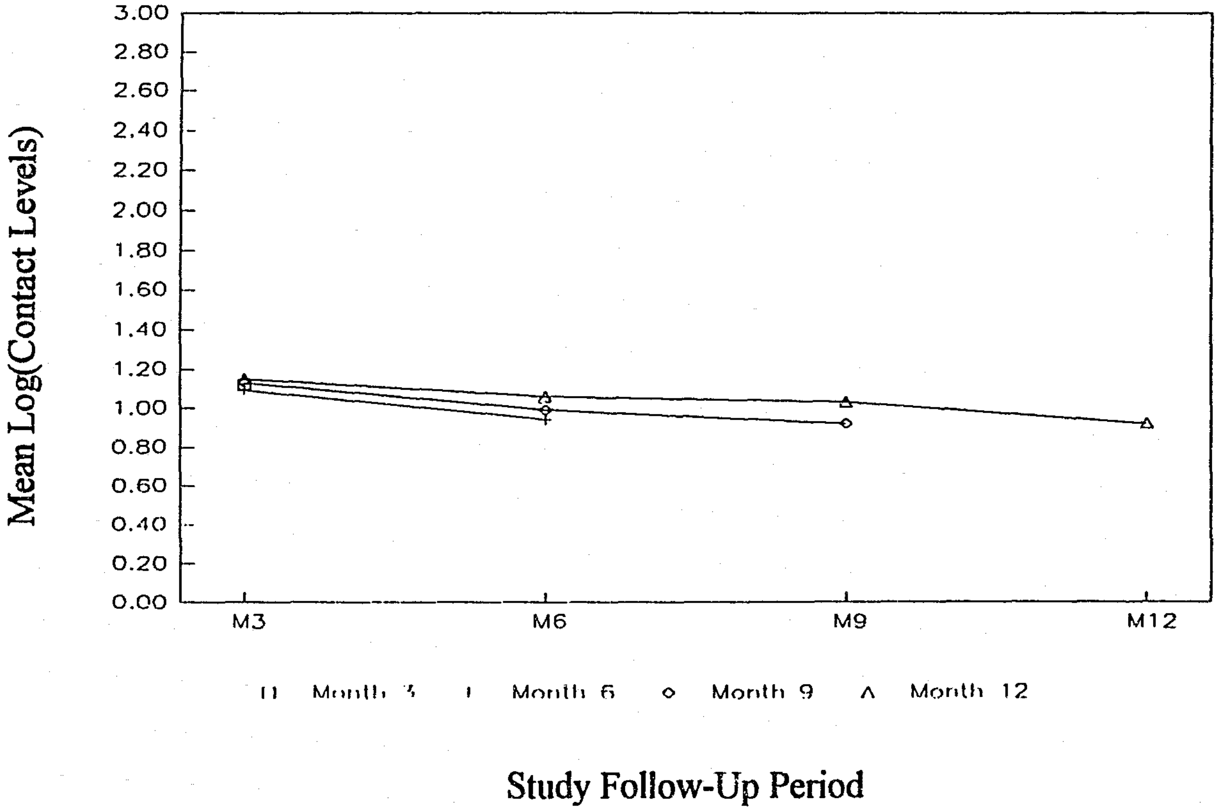


Figure 9
Primary Contact Levels Over Follow-Up Period in Georgia

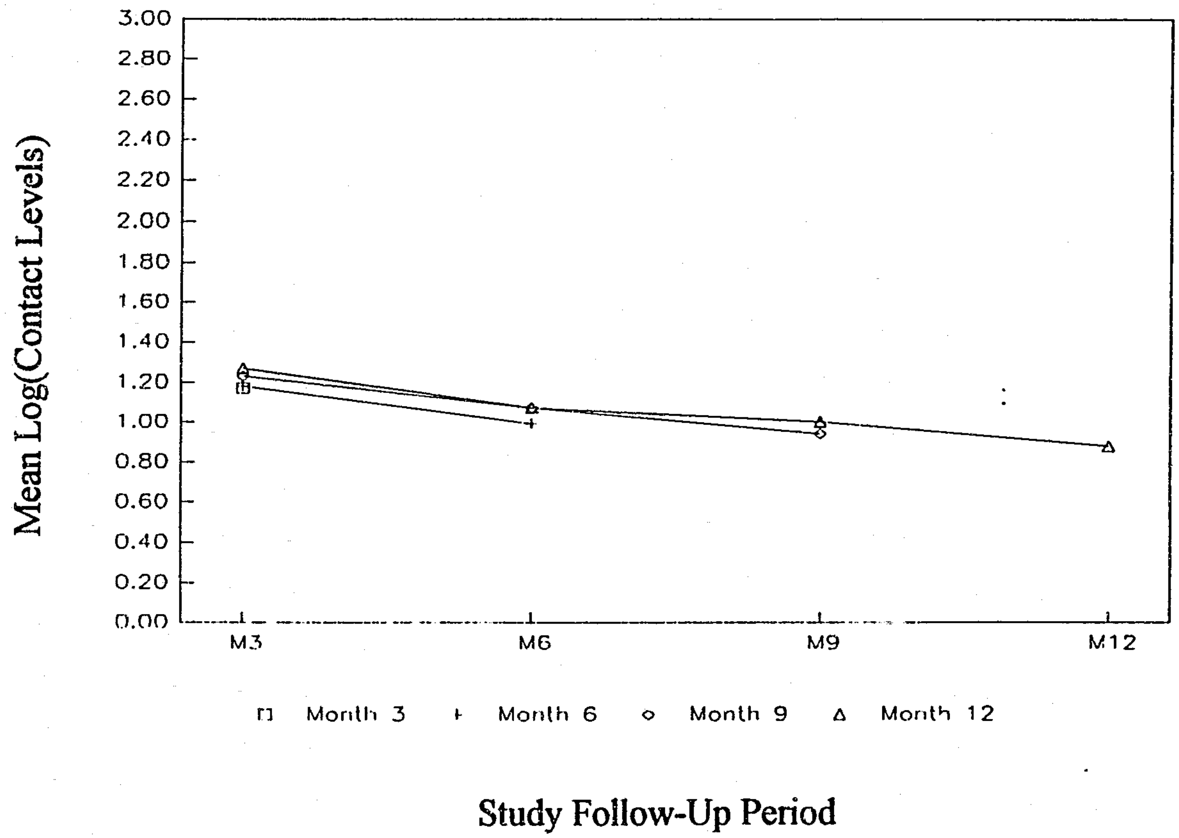


Figure 10
Knowledge Index Scores Over Follow-Up Period in Louisiana

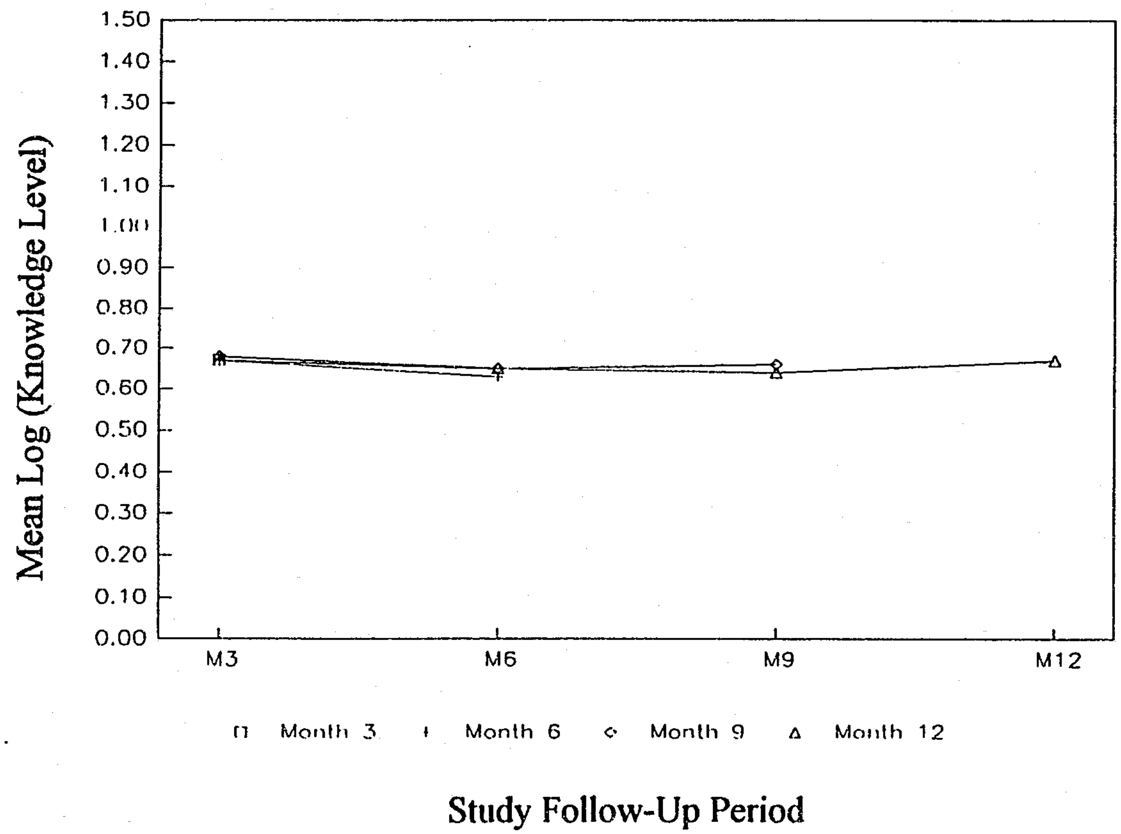


Figure 11
Surveillance Index Scores Over Follow-Up Period in Louisiana

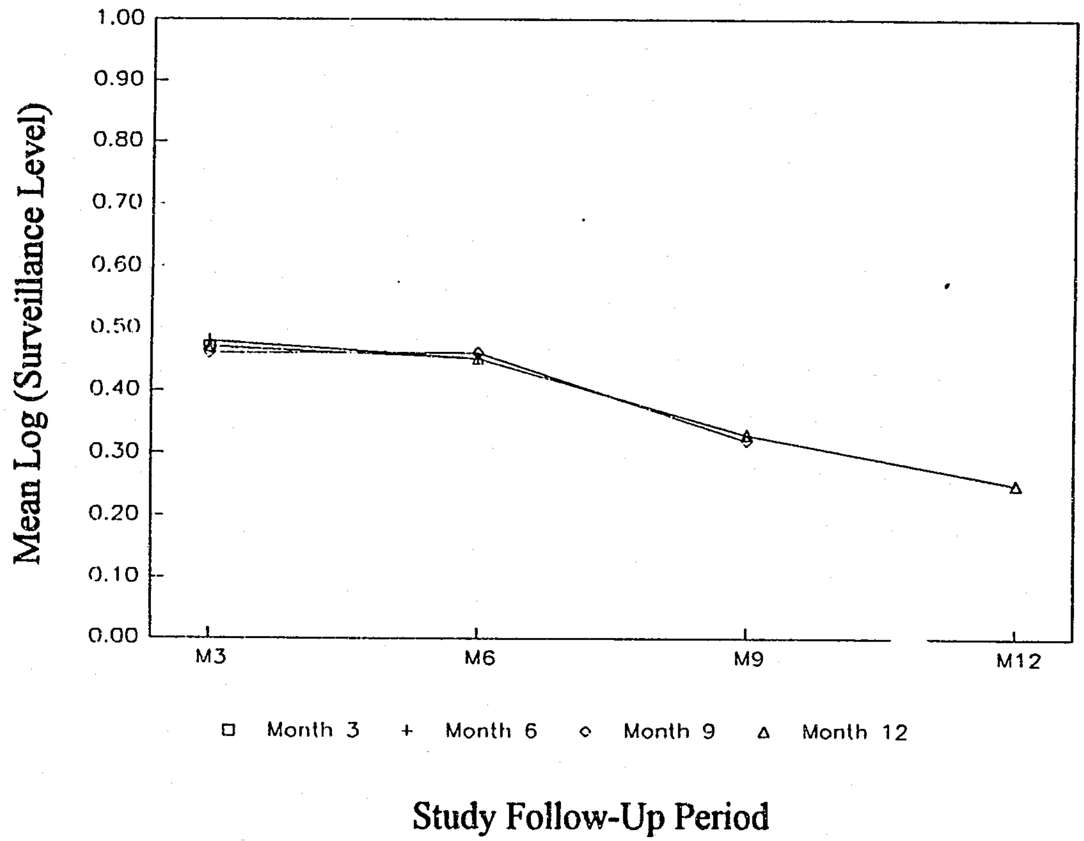


Figure 12
Requirements Index Scores Over Follow-Up Period in Louisiana

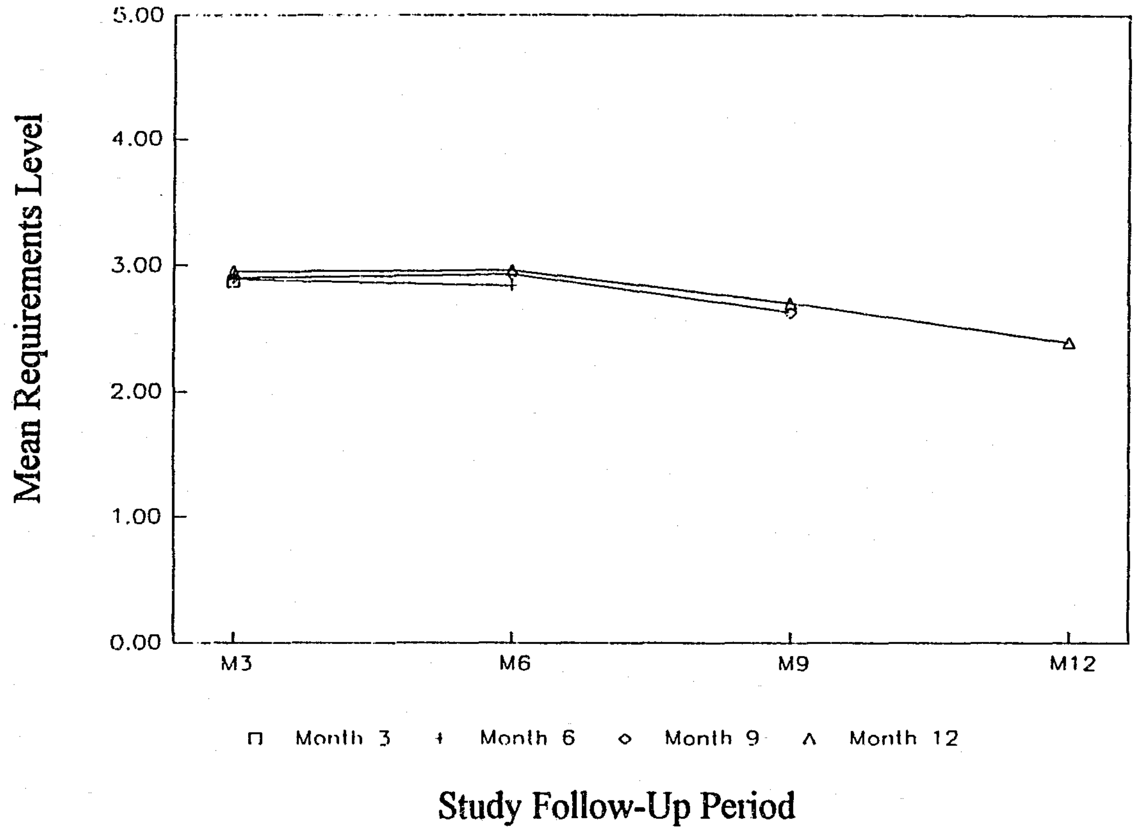


Figure 13
Primary Contact Levels Over Follow-Up Period in South Carolina

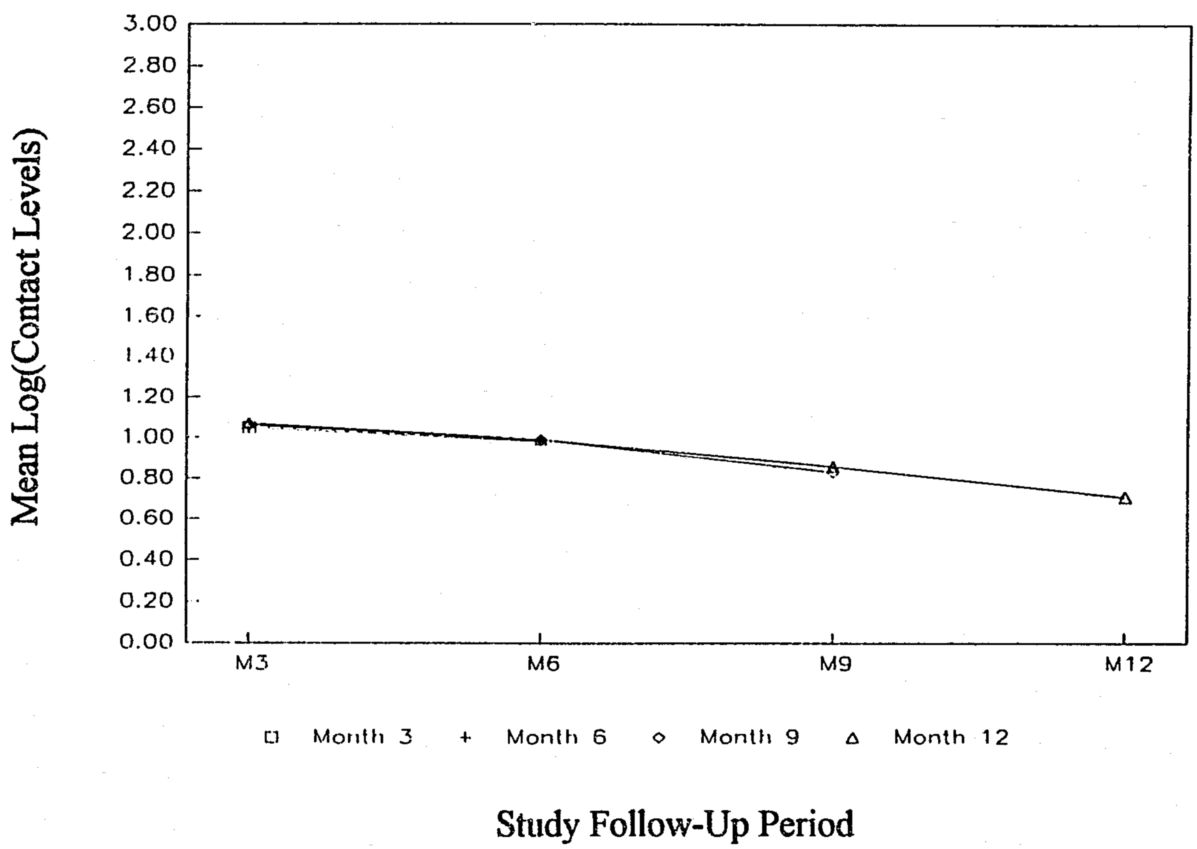


Figure 14
Supervision Intensity by Treatment Sample in Florida

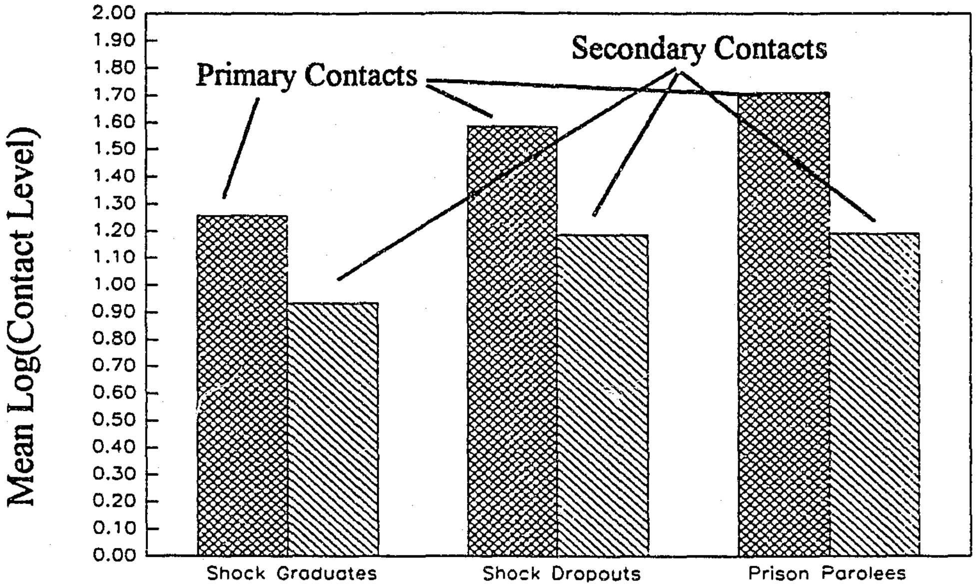


Figure 15
Primary Contact Levels by Treatment Sample in Georgia

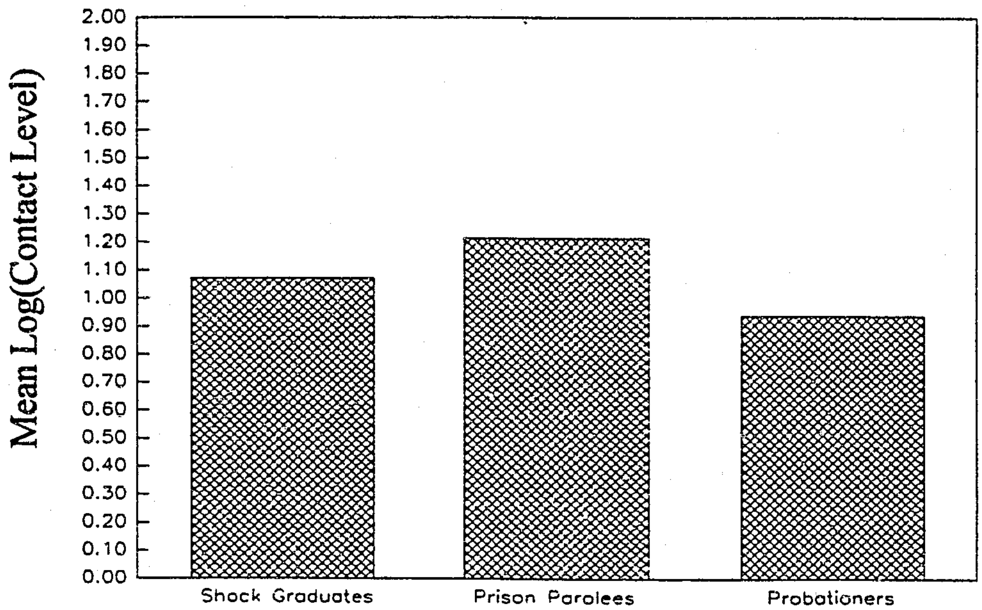
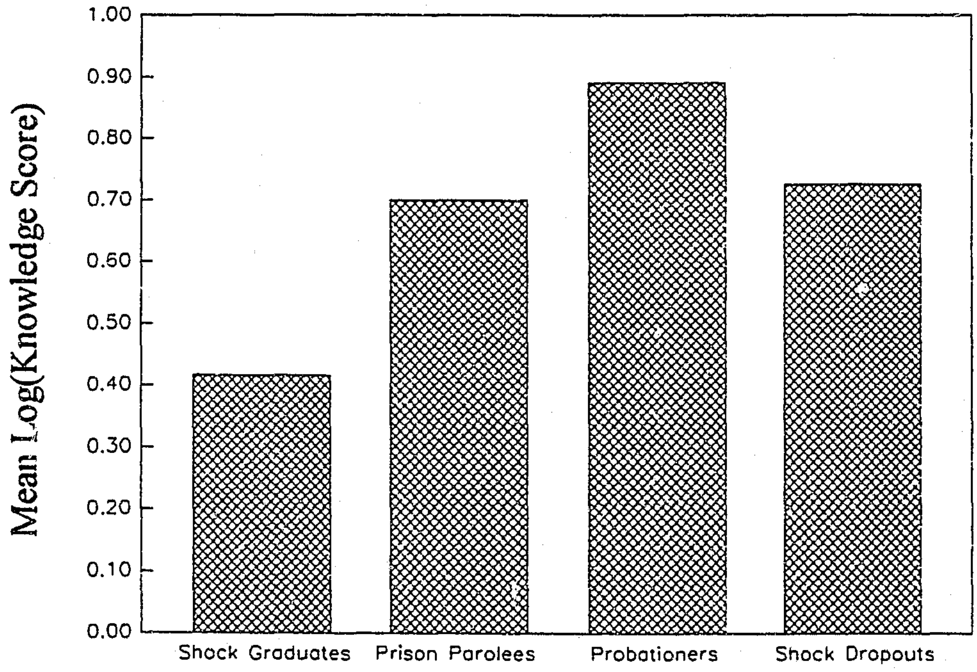
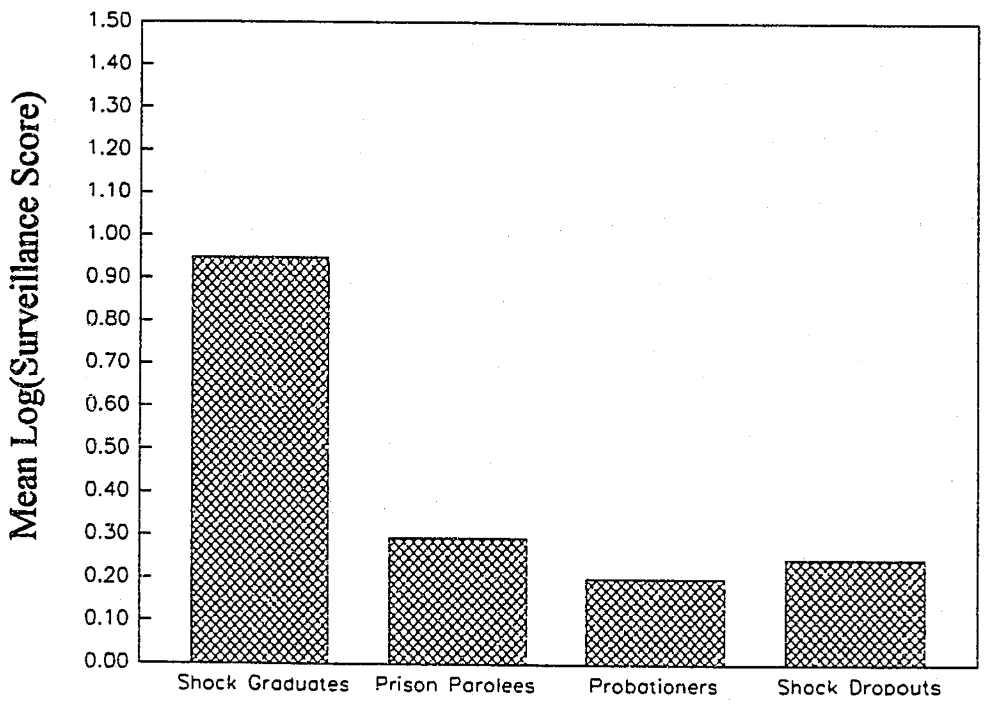


Figure 16
Knowledge Score Levels by Treatment Sample in Louisiana



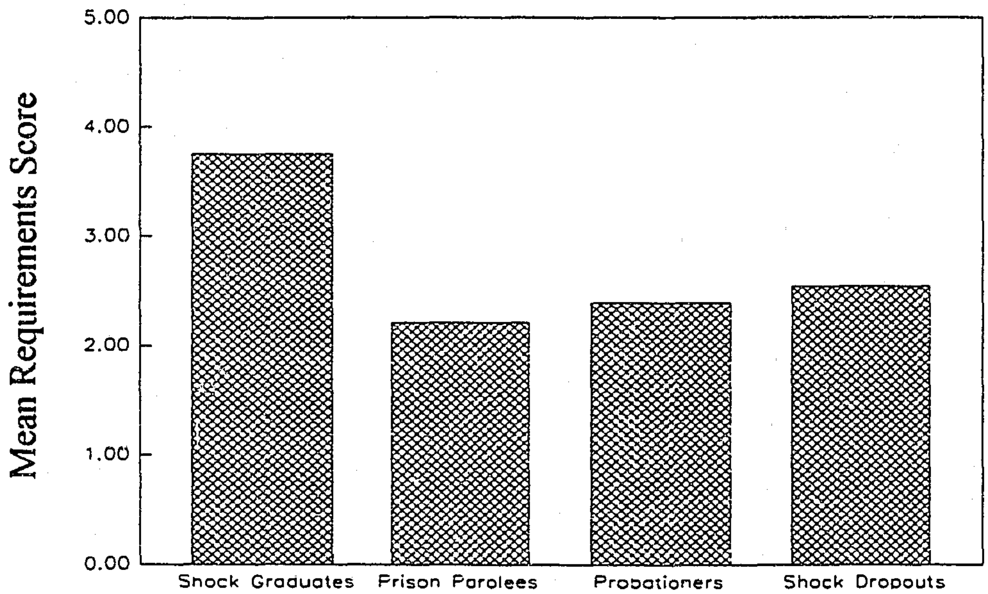
Note: Higher Knowledge Scores Imply Lower Levels of Knowledge of Offender Activities.

Figure 17
Surveillance Score Levels by Treatment Sample in Louisiana



Note: Higher Surveillance Scores Imply Higher Levels of Contact With Offender and Offender's Associates.

Figure 18
Requirement Score Levels by Treatment Sample in Louisiana



Note: Higher Requirements Scores Imply Greater Numbers of Required Activities.

Figure 19
Primary Contact Levels by Treatment Sample in South Carolina

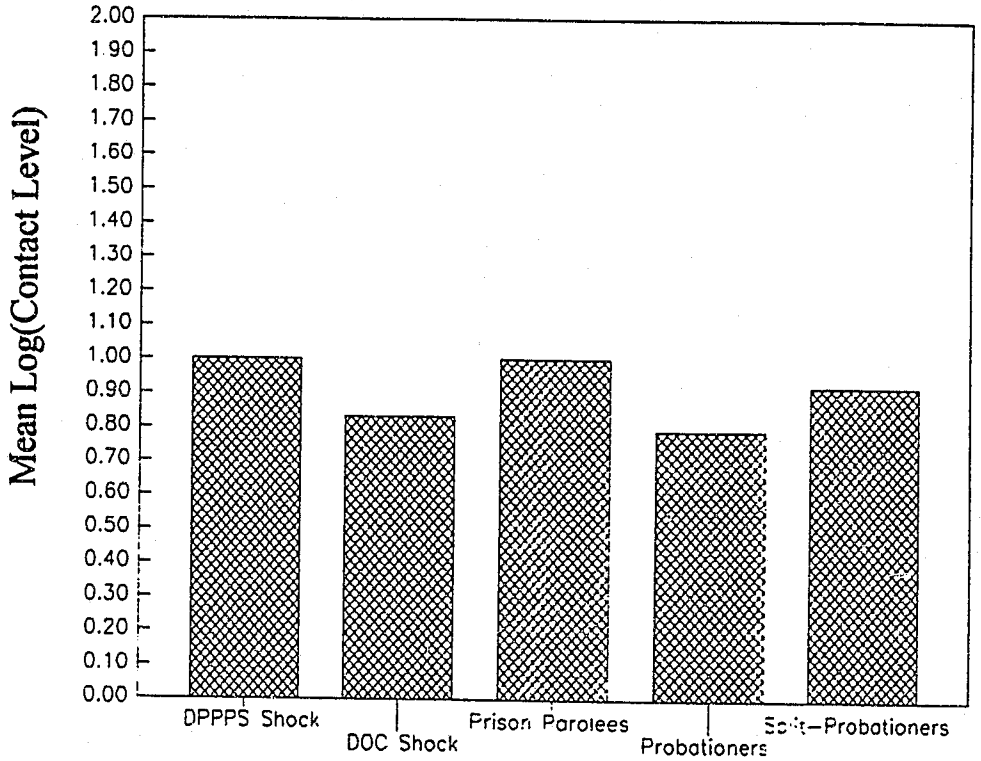
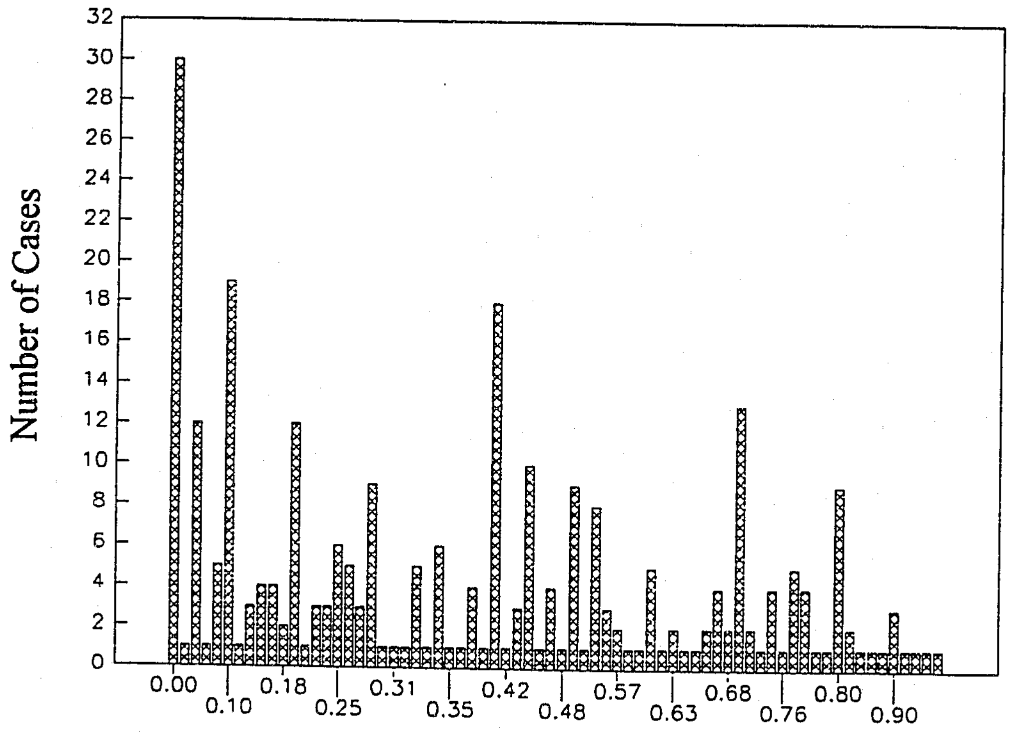
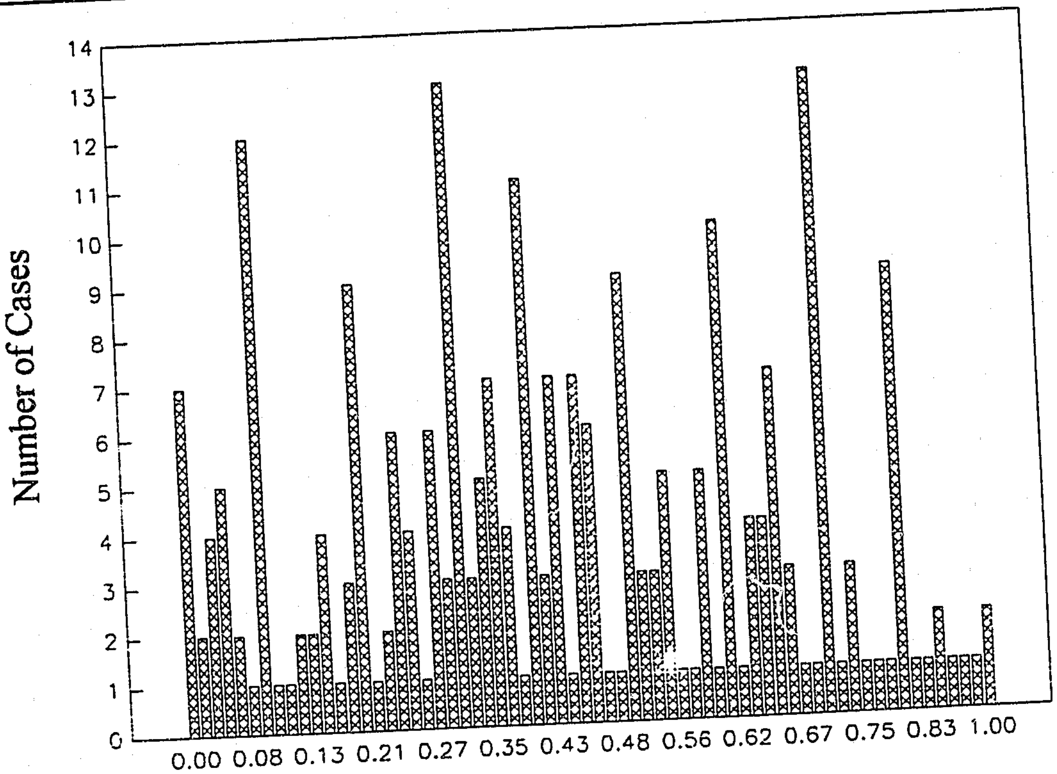


Figure 20
Distribution of Positive Adjustment Scores in Florida



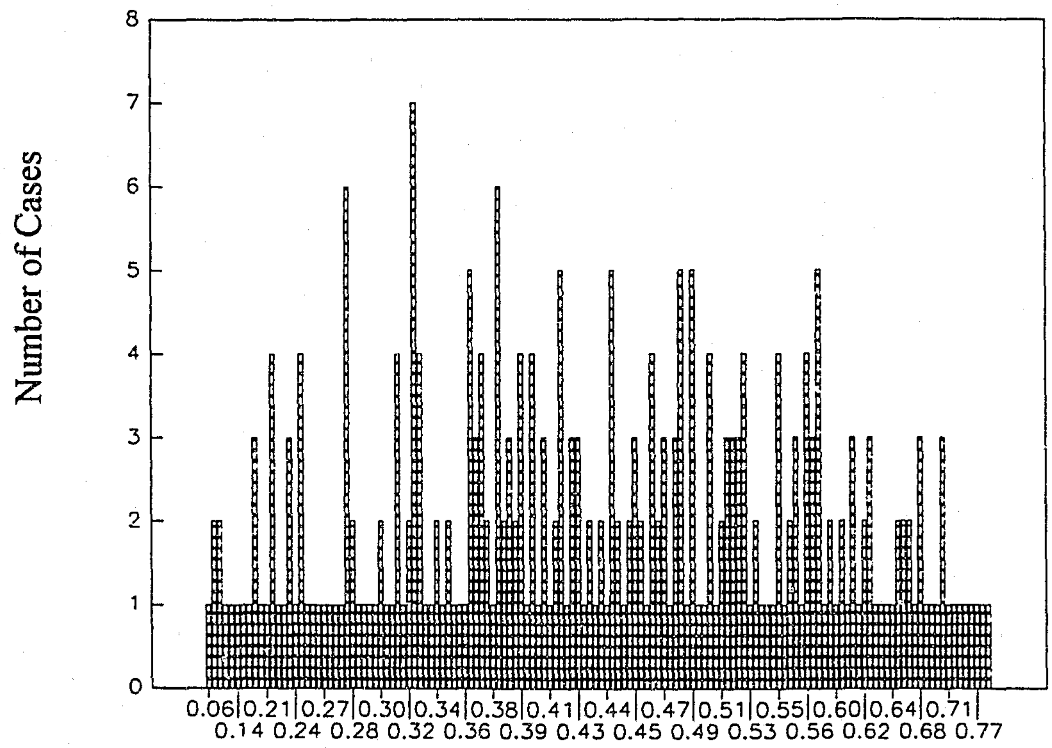
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 21
Distribution of Positive Adjustment Scores in Georgia



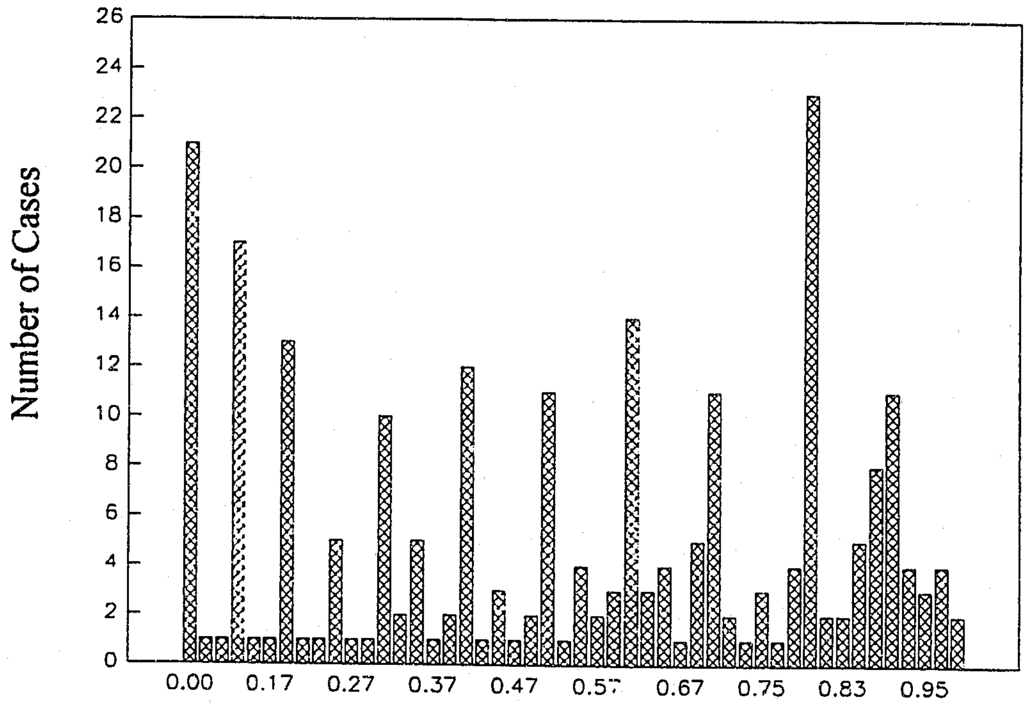
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 22
Distribution of Positive Adjustment Scores in Louisiana



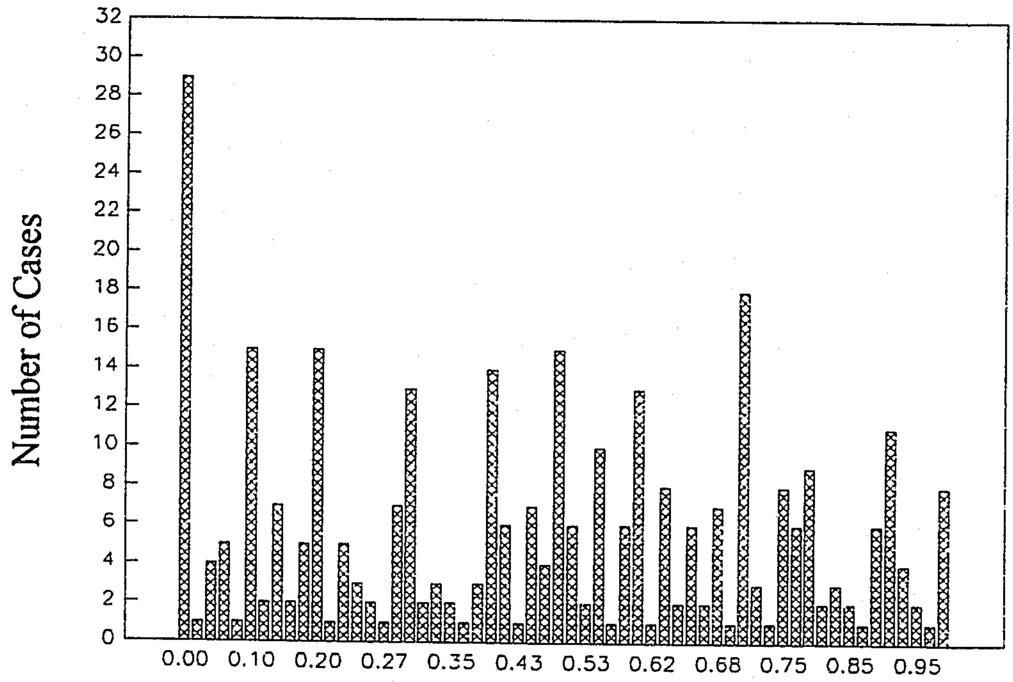
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 23
Distribution of Positive Adjustment Scores in New York



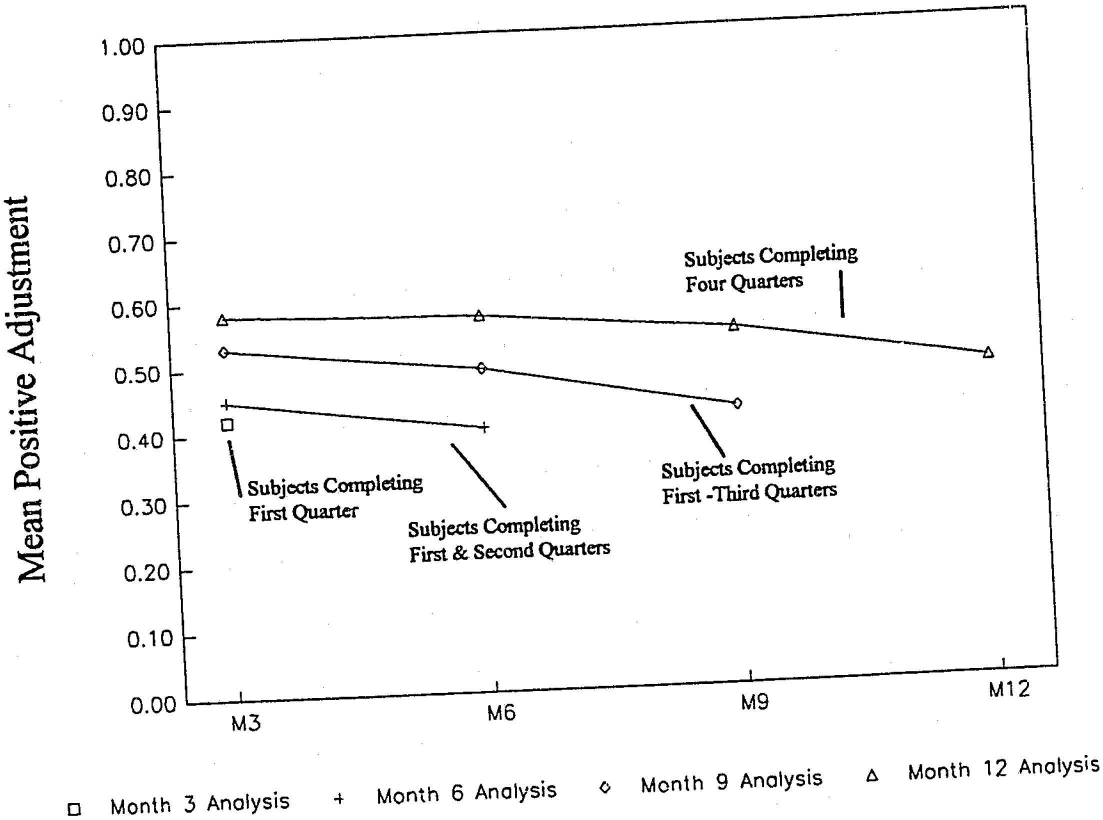
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 24
Distribution of Positive Adjustment Scores in South Carolina



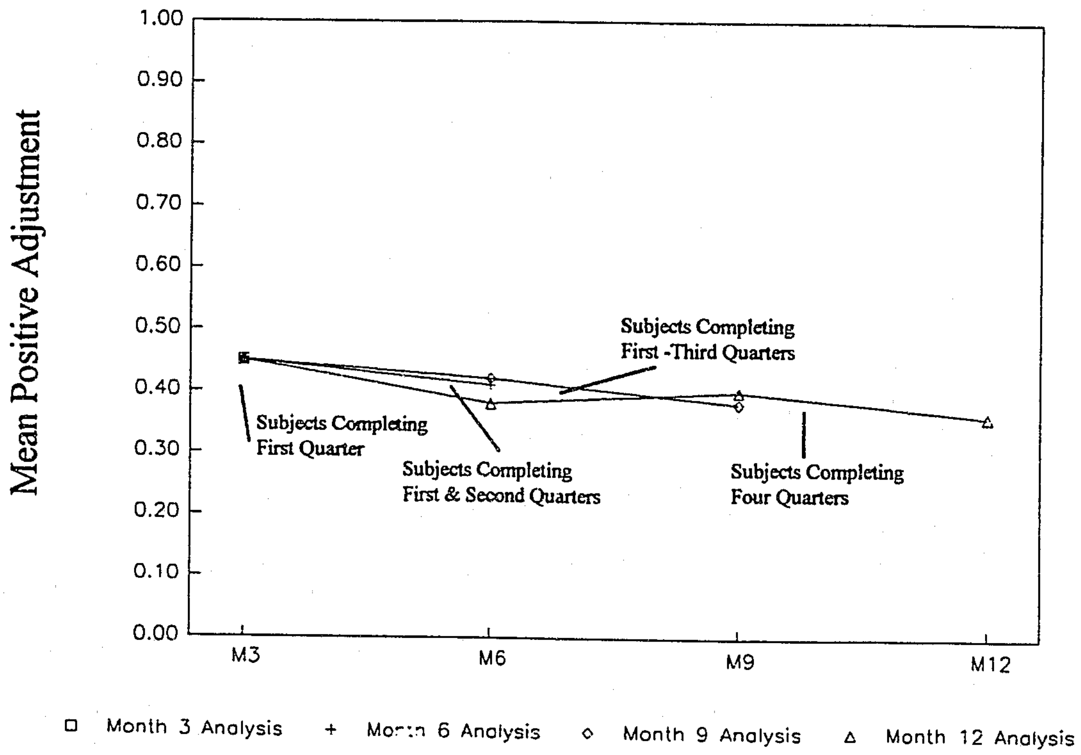
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 25
Mean Positive Adjustment Scores Over Time in Florida



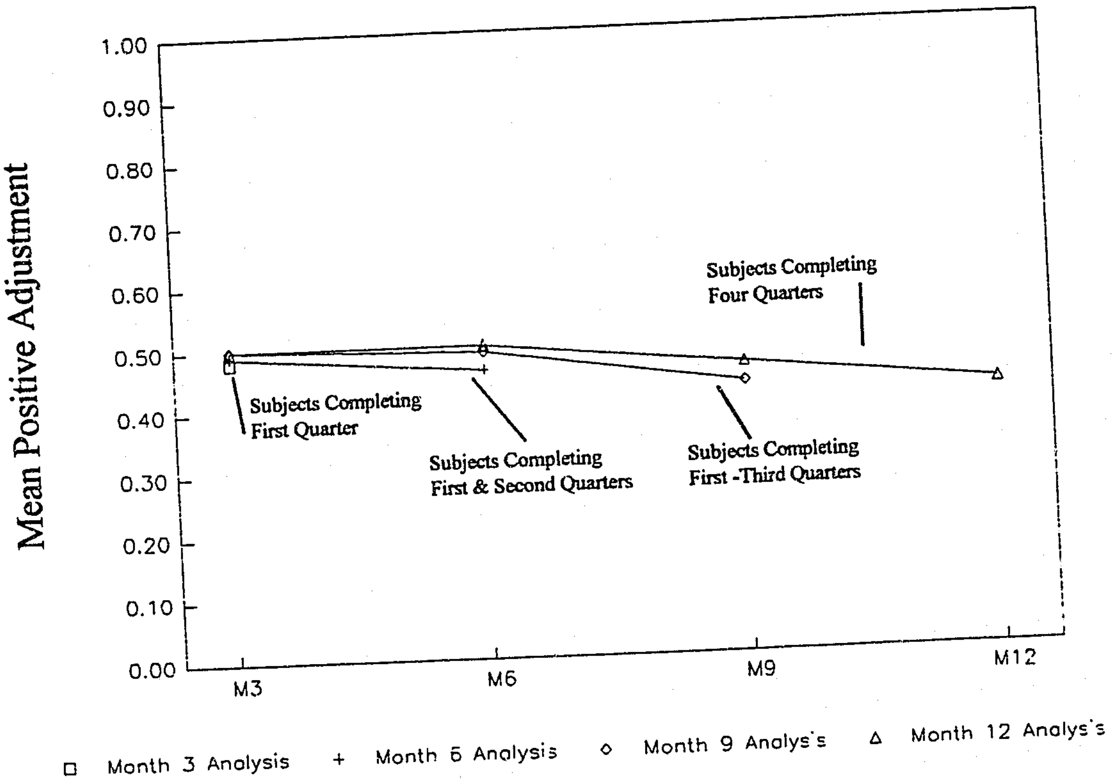
Note: Curves are not based on mutually exclusive groups of subjects. For example, all subjects who completed the first-third quarters are included in the three and six month analyses as well.

Figure 26
Mean Positive Adjustment Scores Over Time in Georgia



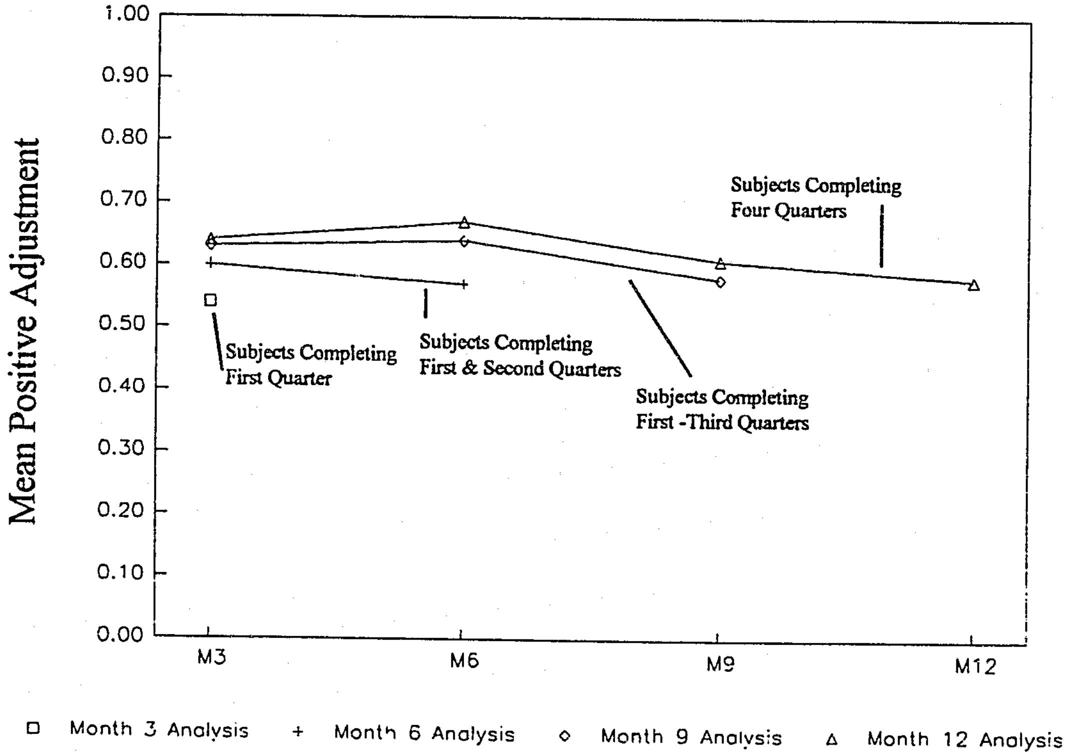
Note: Curves are not based on mutually exclusive groups of subjects. For example, all subjects who completed the first-third quarters are included in the three and six month analyses as well.

Figure 27
Mean Positive Adjustment Scores Over Time in Louisiana



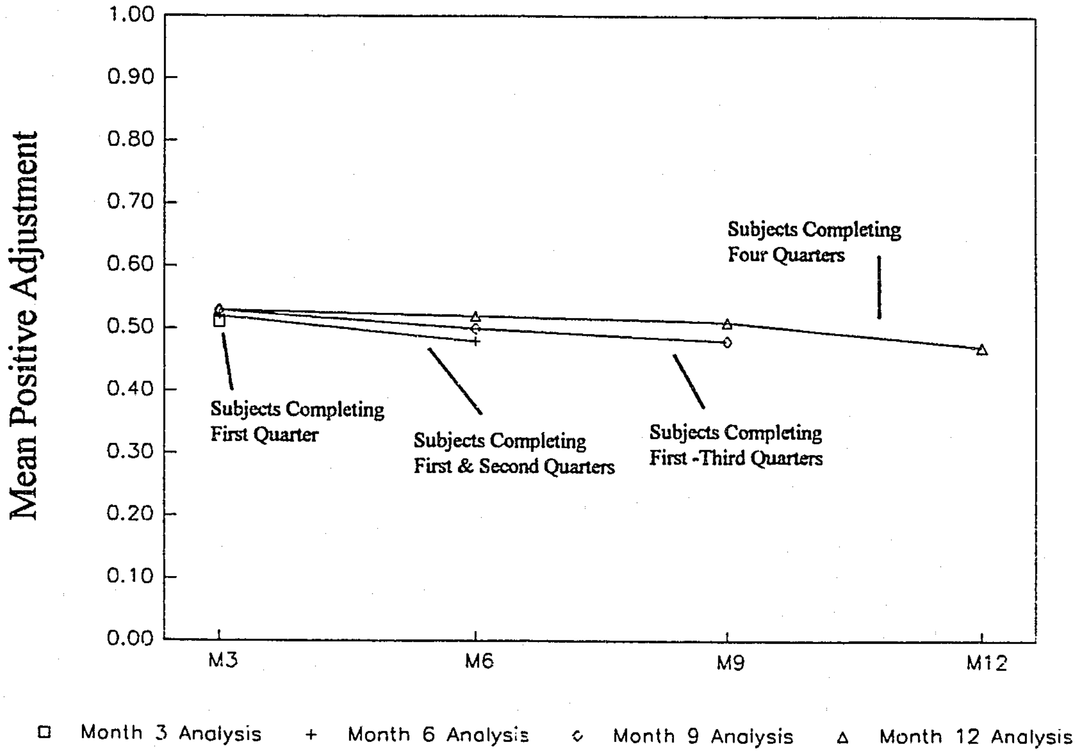
Note: Curves are not based on mutually exclusive groups of subjects. For example, all subjects who completed the first-third quarters are included in the three and six month analyses as well.

Figure 28
Mean Positive Adjustment Scores Over Time in New York



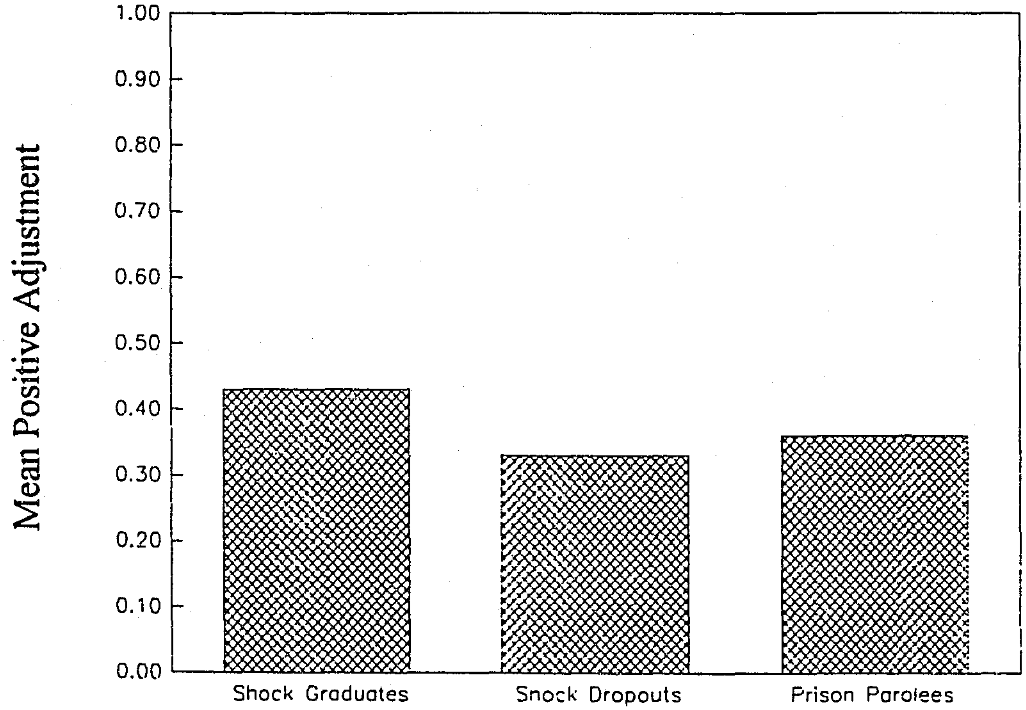
Note: Curves are not based on mutually exclusive groups of subjects. For example, all subjects who completed the first-third quarters are included in the three and six month analyses as well.

Figure 29
Mean Positive Adjustment Scores Over Time in South Carolina



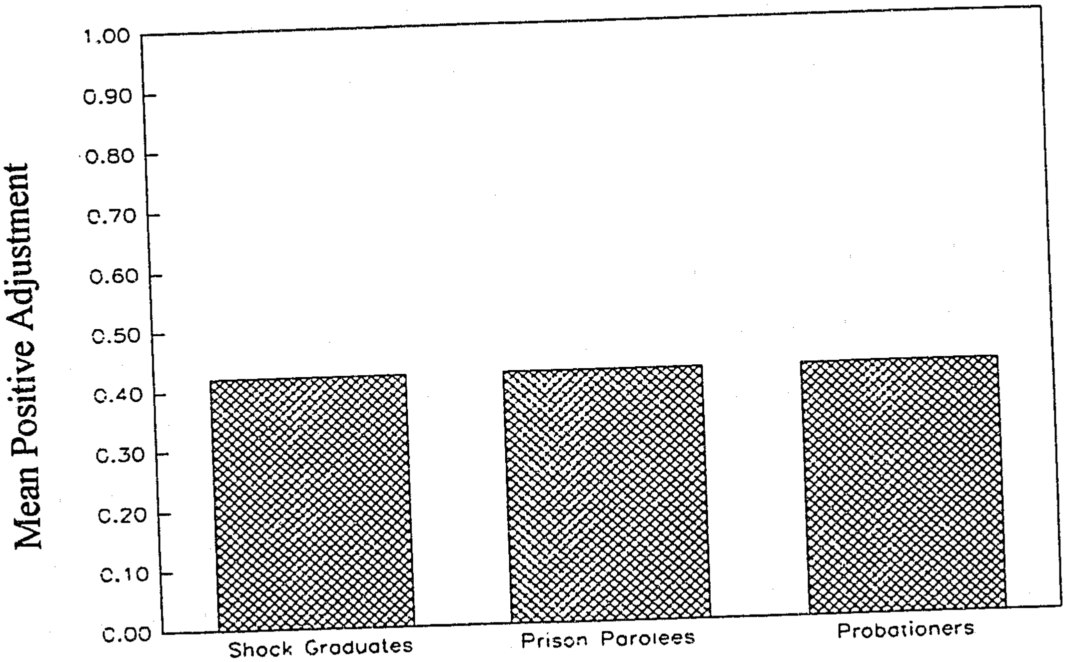
Note: Curves are not based on mutually exclusive groups of subjects. For example, all subjects who completed the first-third quarters are included in the three and six month analyses as well.

Figure 30
Mean Overall Positive Adjustment Score By Treatment Sample in Florida



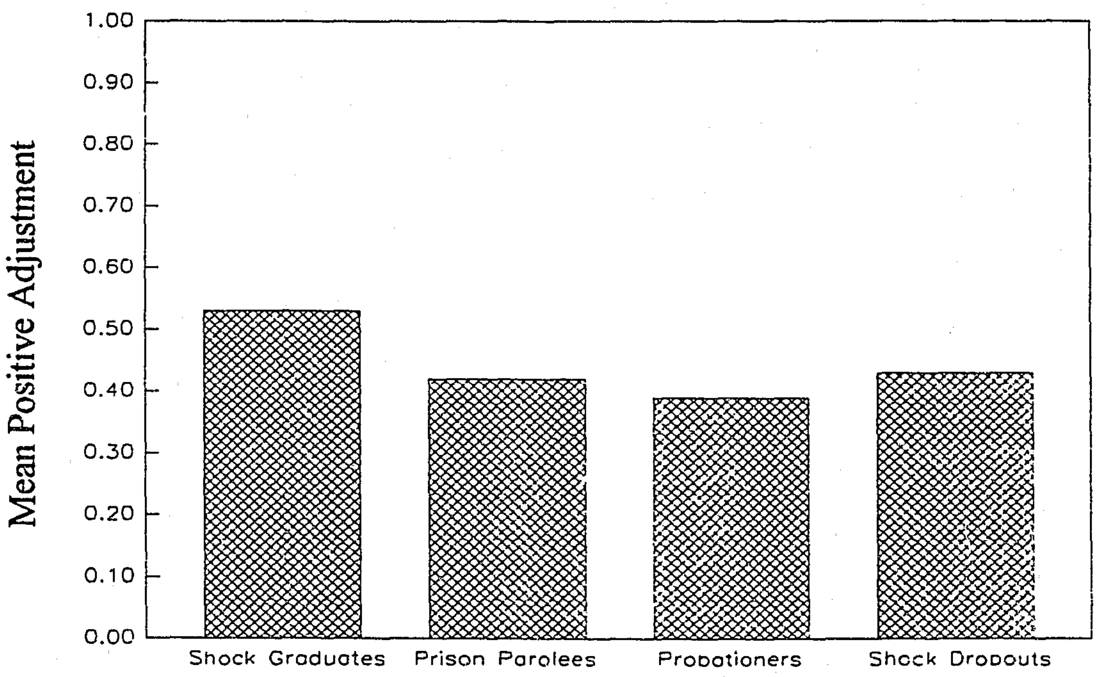
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 31
Mean Overall Positive Adjustment Score By Treatment Sample in Georgia



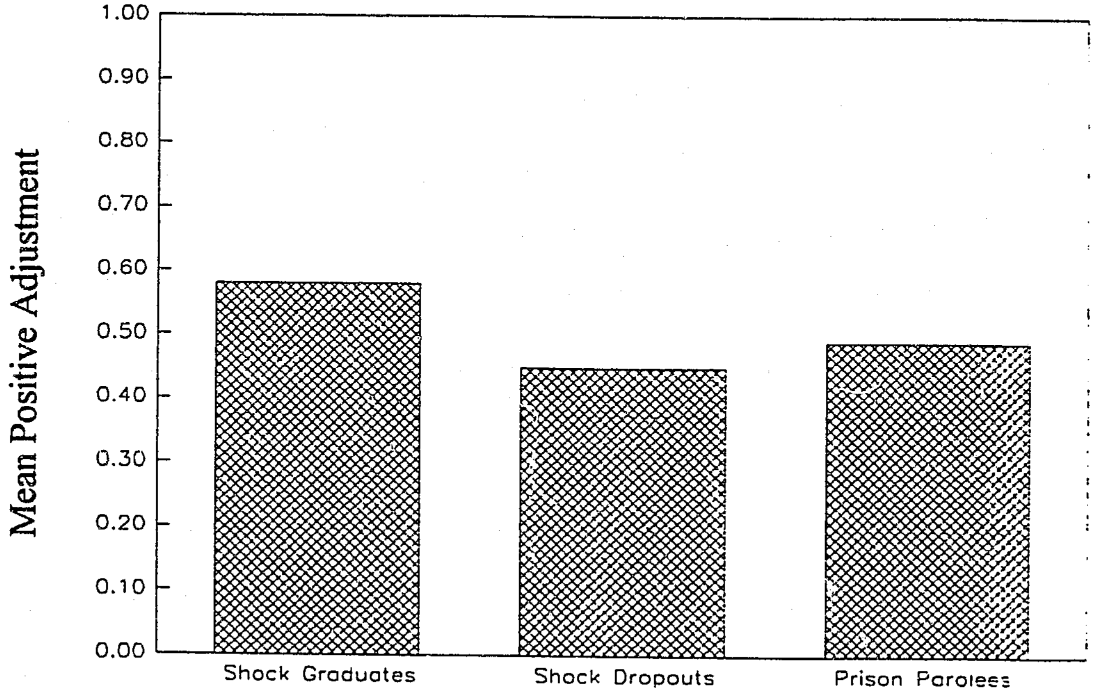
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 32
Mean Overall Positive Adjustment Score By Treatment Sample in Louisiana



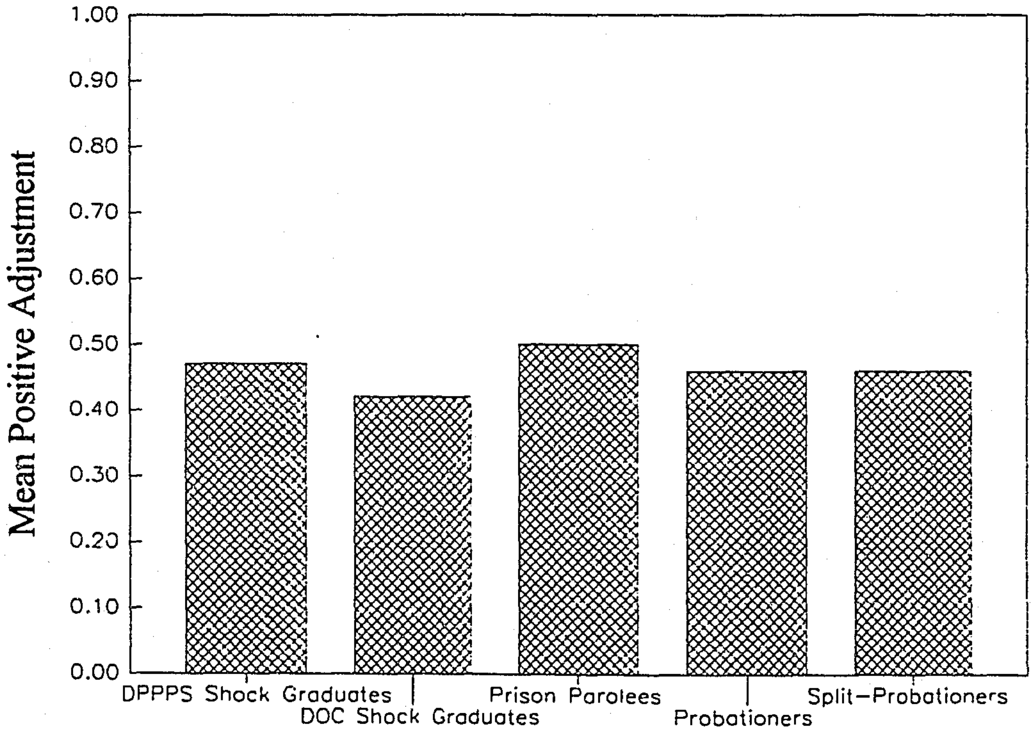
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 33
Mean Overall Positive Adjustment Score By Treatment Sample in New York



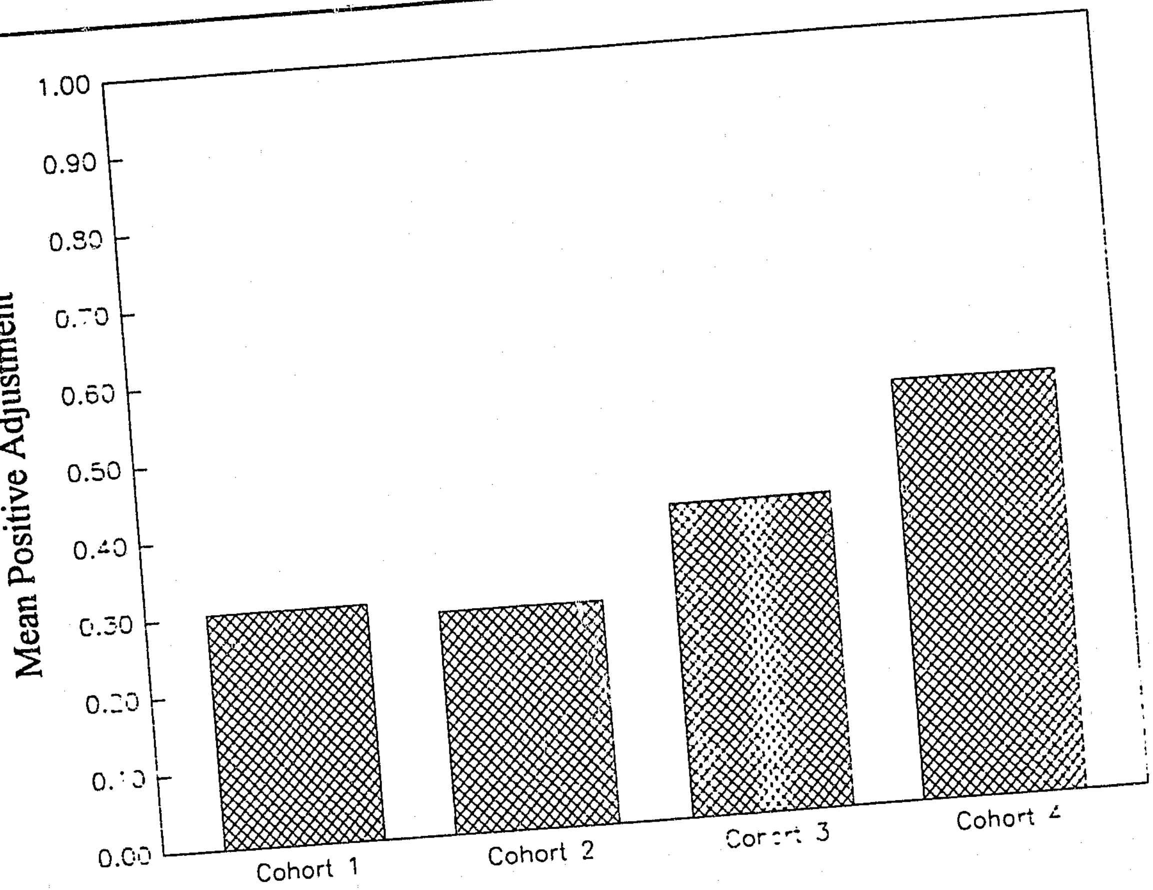
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 34
Mean Overall Positive Adjustment Score By Treatment Sample in South Carolina



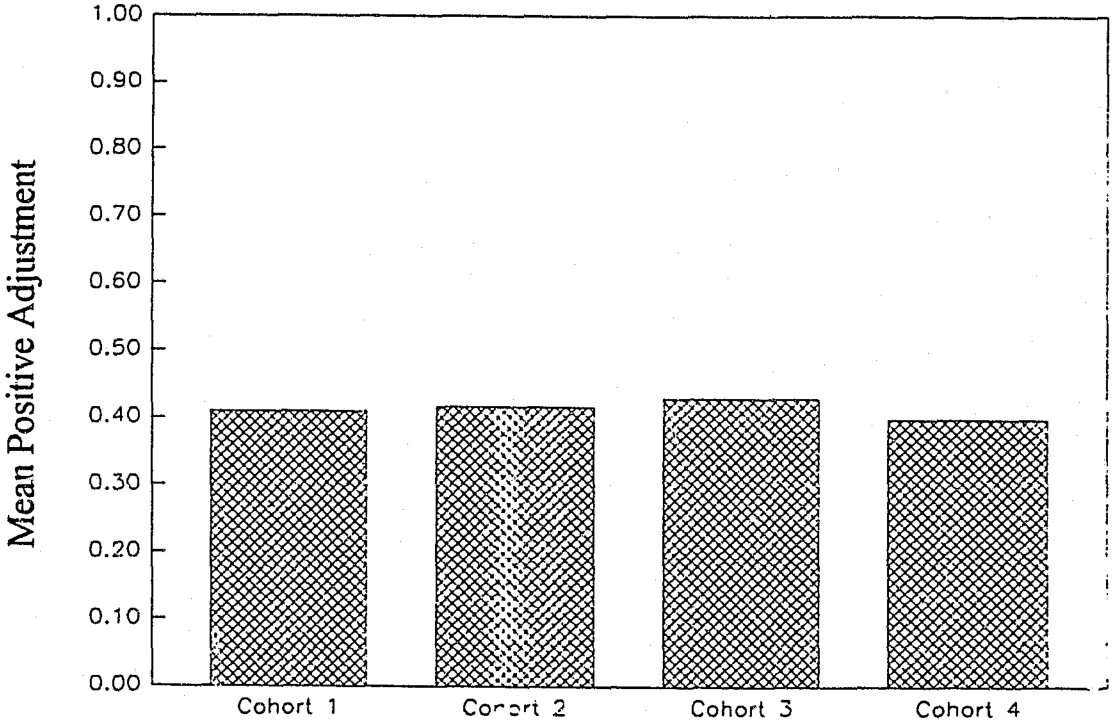
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 35
Mean Overall Positive Adjustment Score By
Exit Cohort Groups in Florida



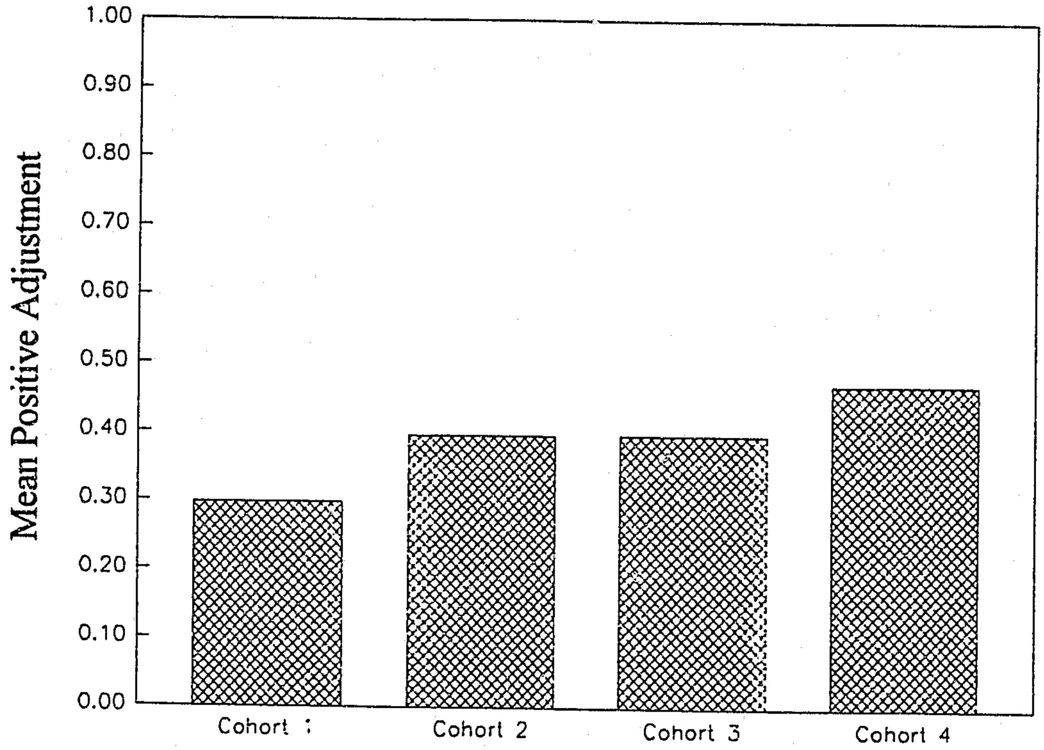
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 36
Mean Overall Positive Adjustment Score By Exit Cohort Groups in Georgia



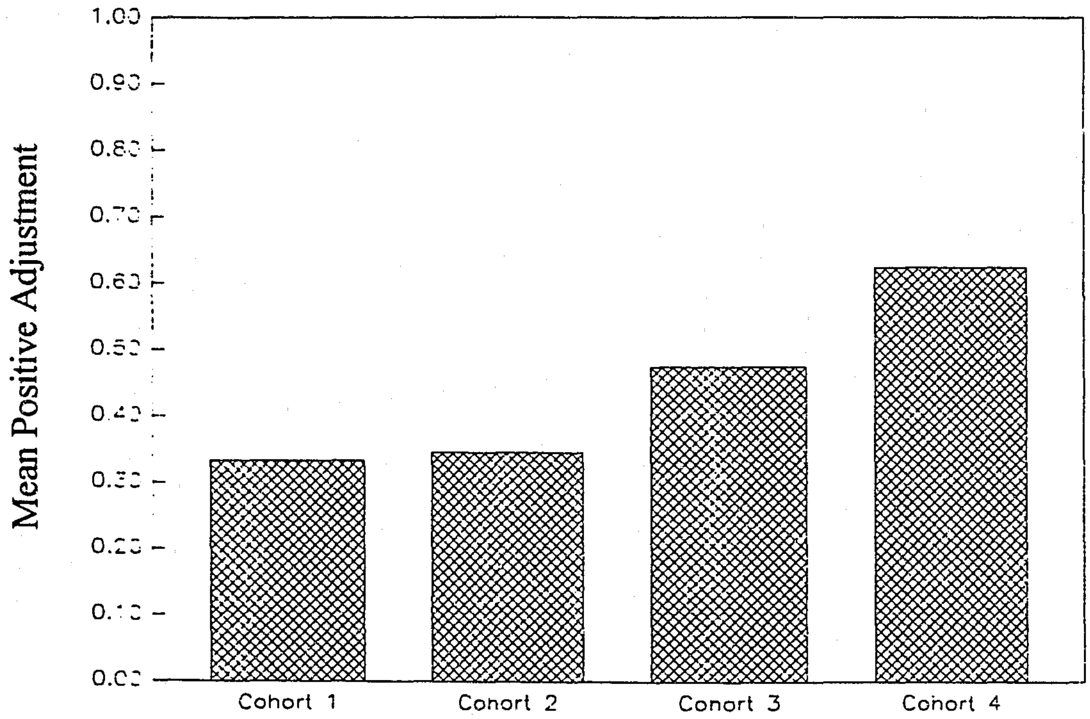
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 37
Mean Overall Positive Adjustment Score By Exit Cohort Groups in Louisiana



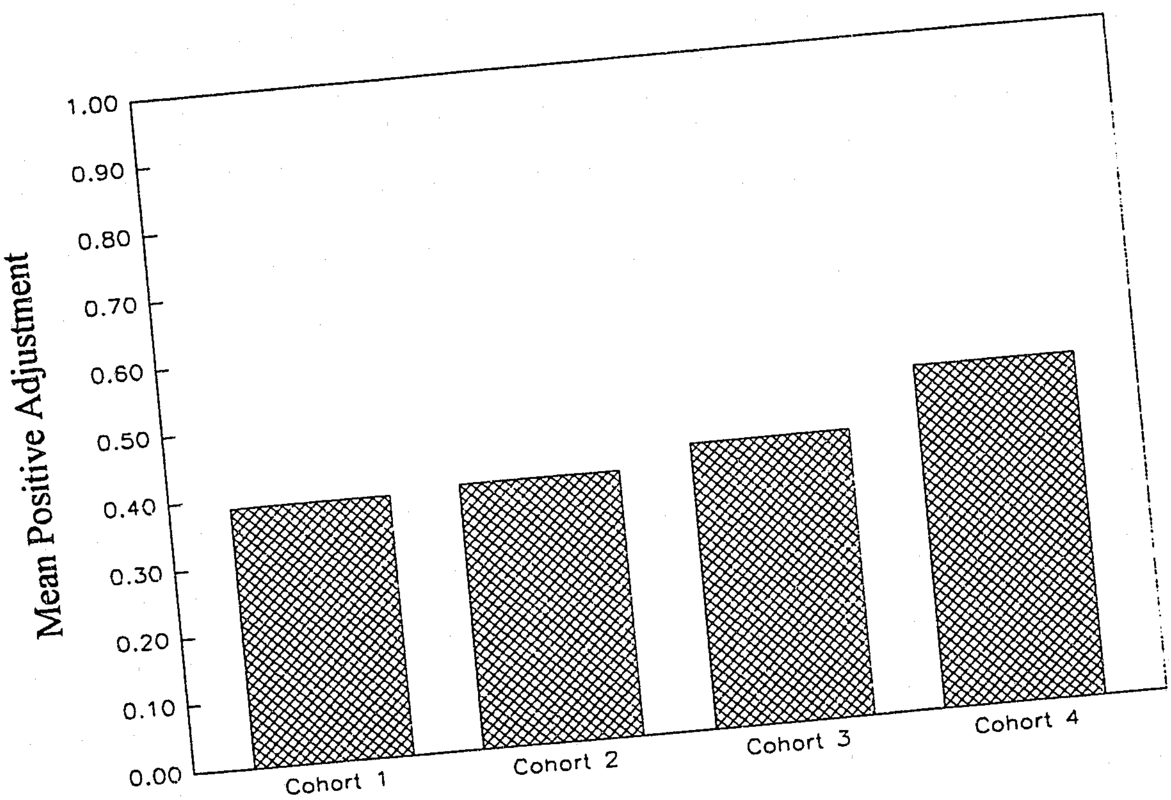
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 38
Mean Overall Positive Adjustment Score By
Exit Cohort Groups in New York



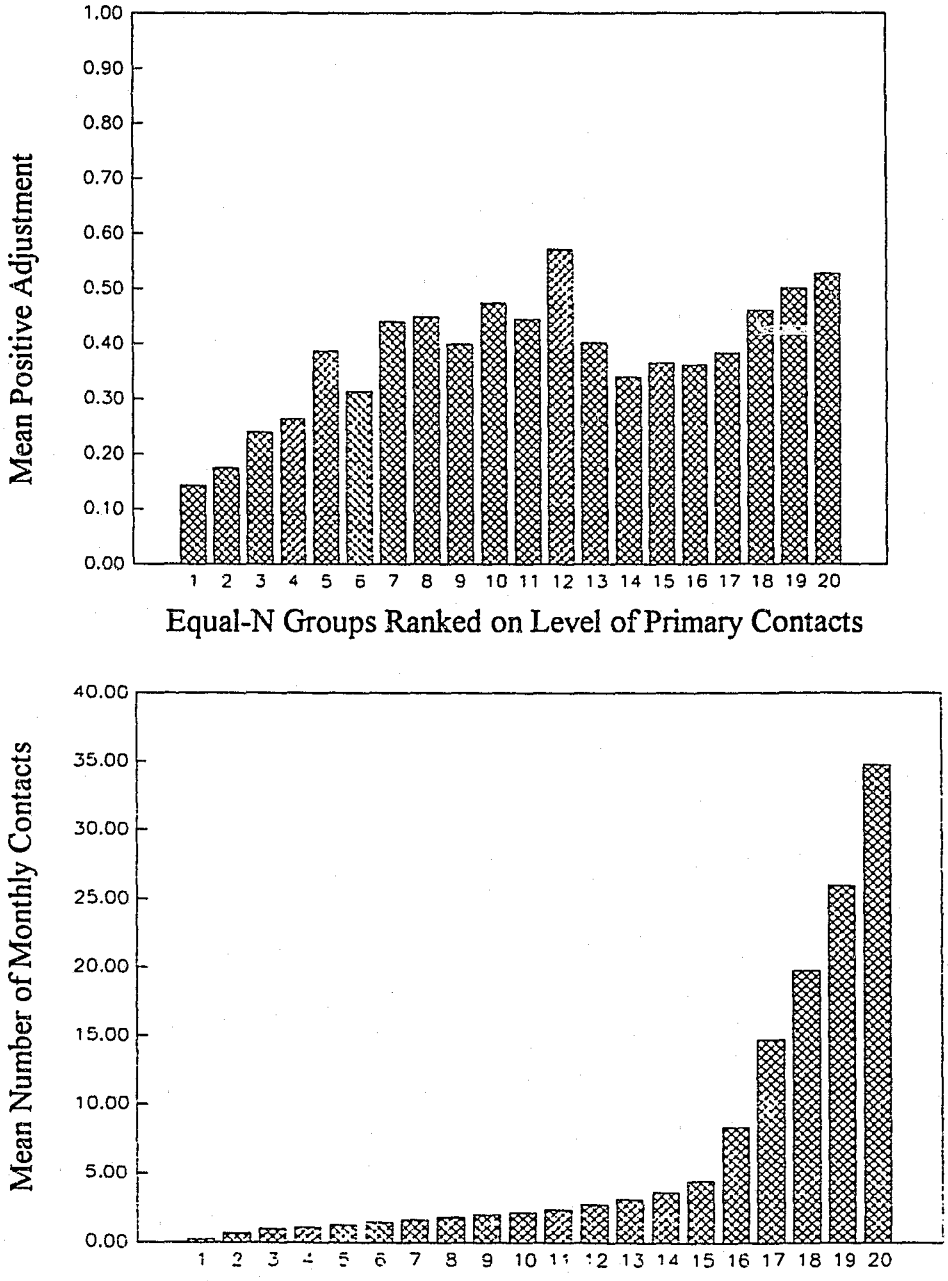
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 39
Mean Overall Positive Adjustment Score By
Exit Cohort Groups in South Carolina



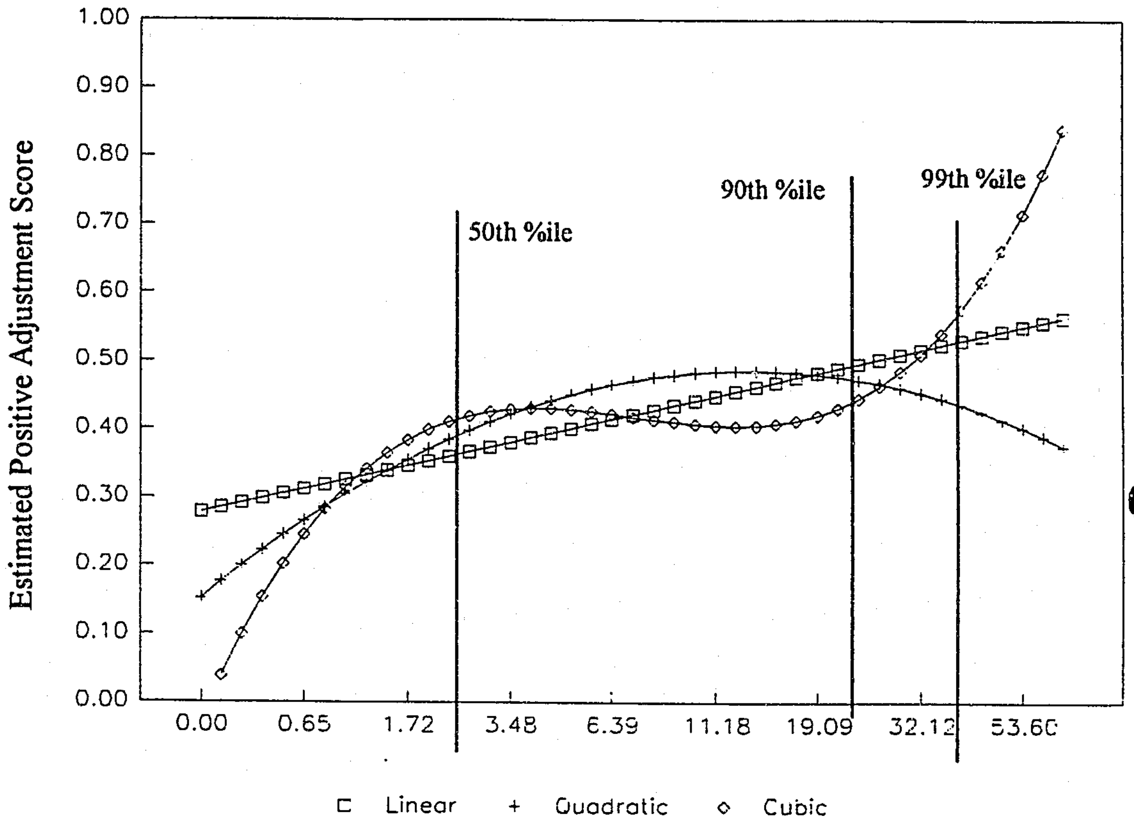
Note: Positive Adjustment Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 40
Mean Overall Positive Adjustment Score and Mean Number of Monthly Contacts By 20 Equal-N Groups Ranked on Contact Levels in Florida



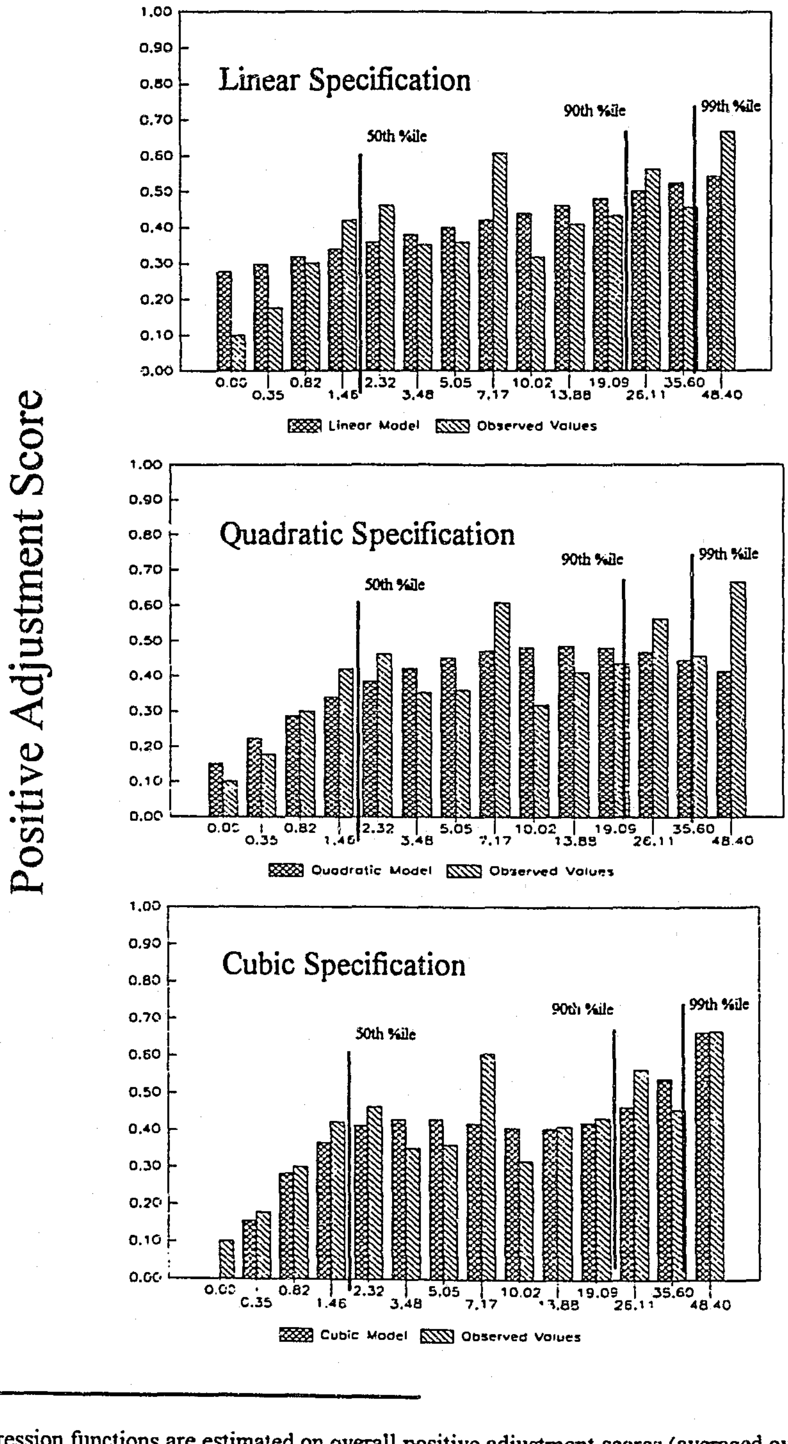
Note: Positive Adjustment Scores and Monthly Contact Rate are averaged over each subject's follow-up period (regardless of the length).

Figure 41A
Linear, Quadratic, and Cubic Regression Functions For Contact Levels and Estimated Overall Positive Adjustment Scores In Florida



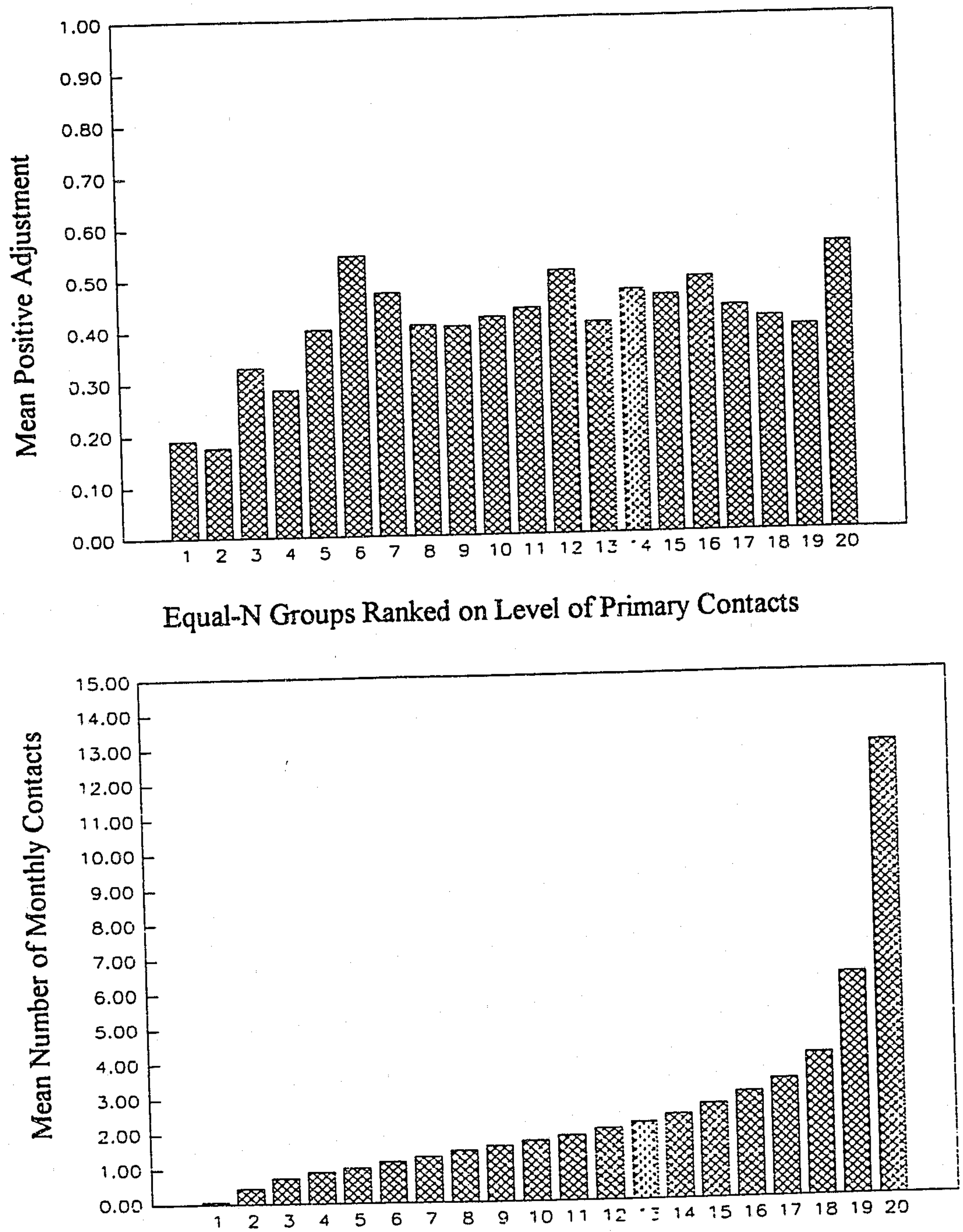
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates.

Figure 41B
Comparison of Alternative Model Specifications To Actual Mean Values of the Positive Adjustment Score Given Contact Levels in Florida.



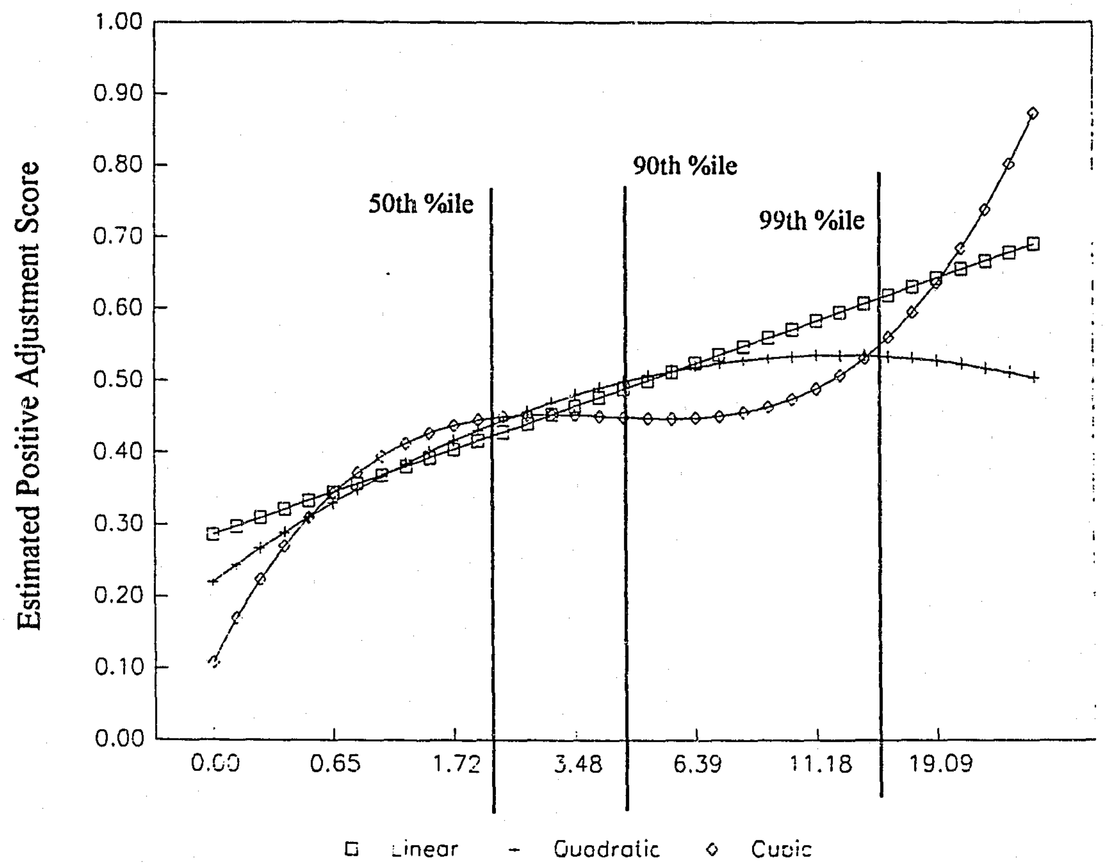
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates.

Figure 42
Mean Overall Positive Adjustment Score and Mean Number of Monthly Contacts By 20 Equal-N Groups Ranked on Contact Levels in Georgia



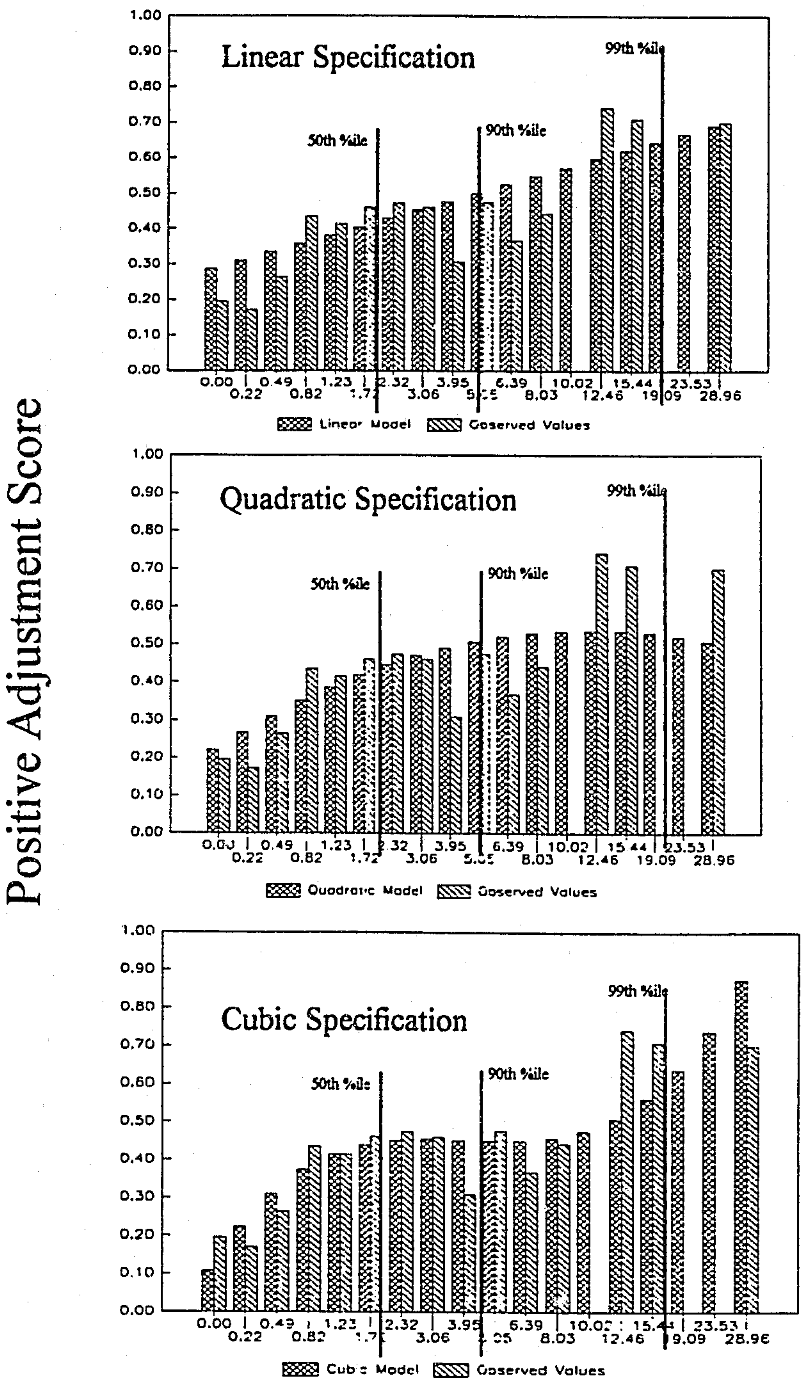
Note: Positive Adjustment Scores and Monthly Contact Rate are averaged over each subject's follow-up period (regardless of the length).

Figure 43A
Linear, Quadratic, and Cubic Regression Functions For Contact Levels and Estimated Overall Positive Adjustment Scores In Georgia



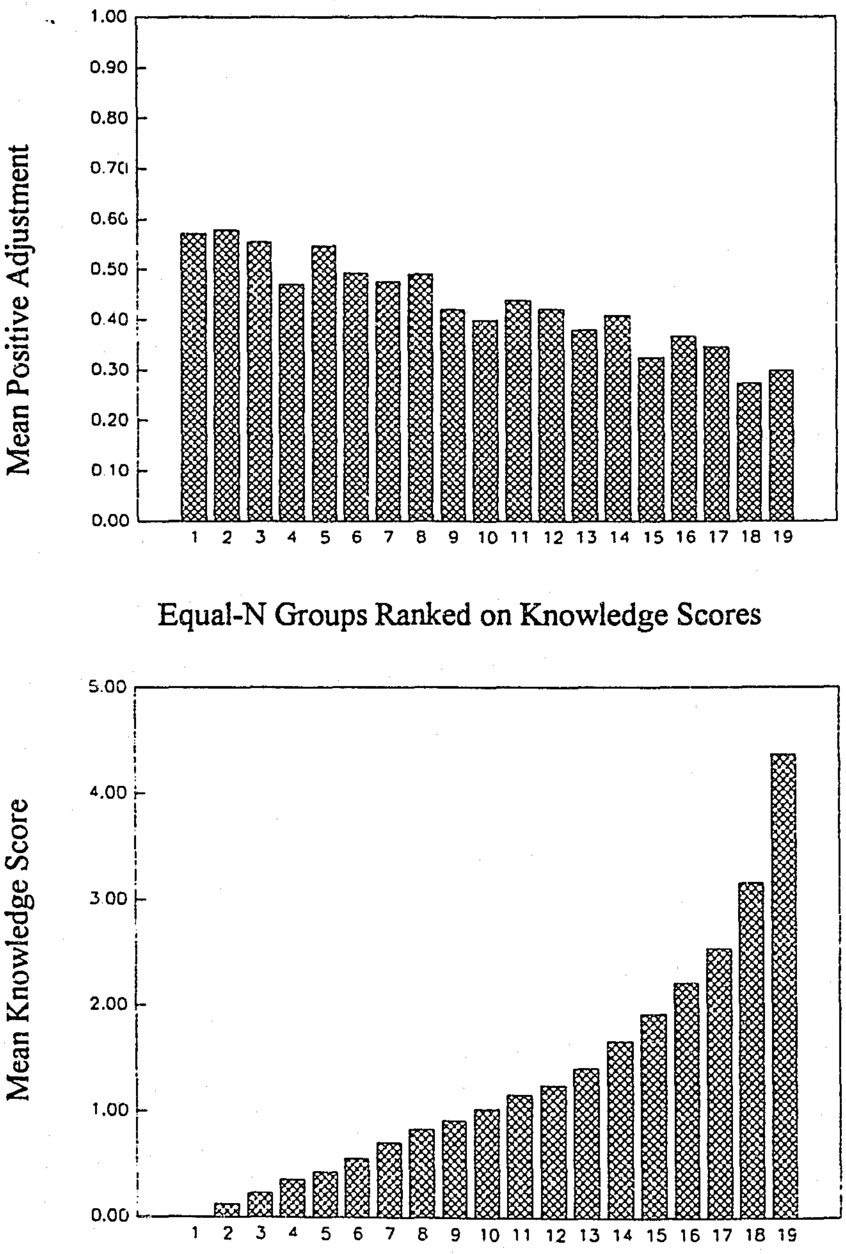
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 43B
Comparison of Alternative Model Specifications To Actual Mean Values of the Positive Adjustment Score Given Contact Levels in Georgia.



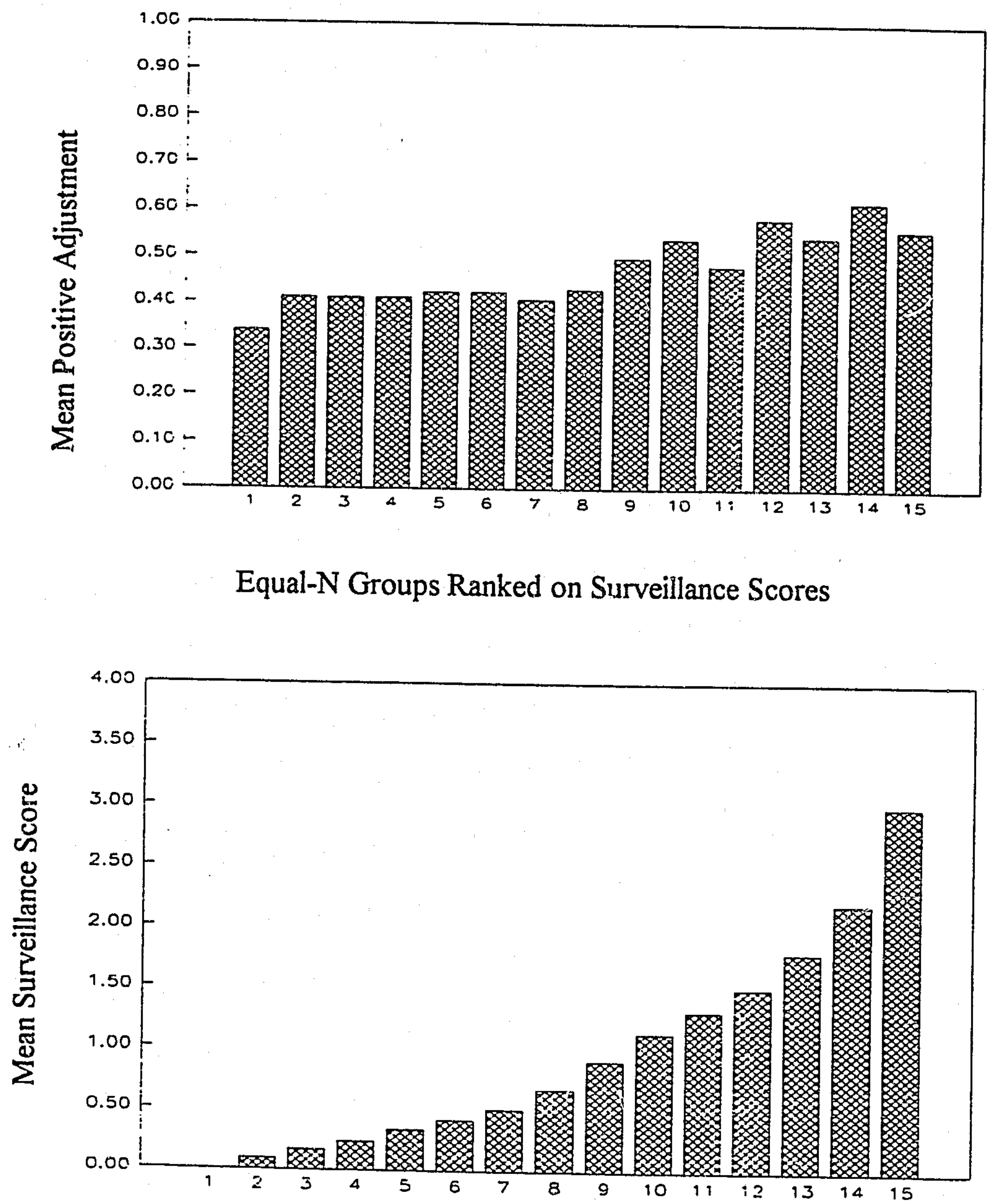
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates.

Figure 44
Mean Overall Positive Adjustment Score and Mean Knowledge Scores
By 19 Equal-N Groups Ranked on Knowledge Scores in Louisiana



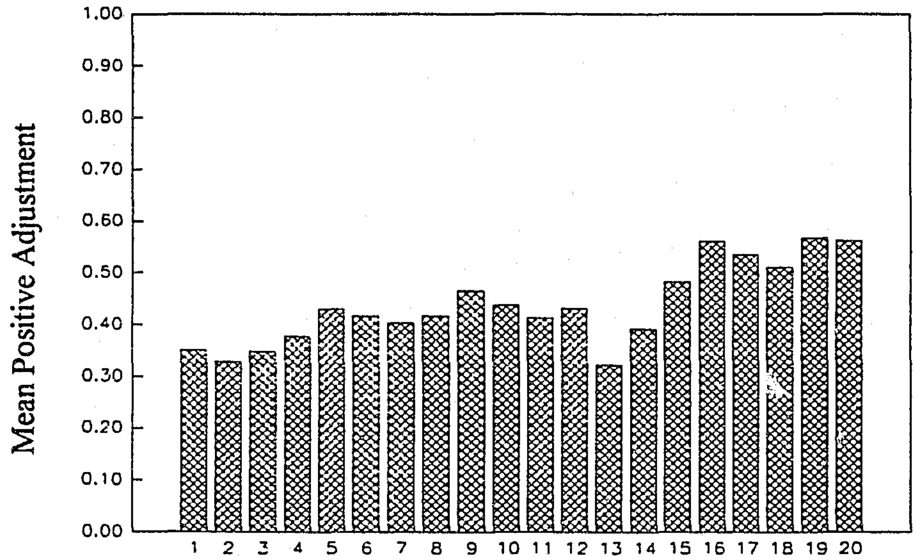
Note: Positive Adjustment Scores and Knowledge Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 45
Mean Overall Positive Adjustment Score and Mean Surveillance Scores By 15 Equal-N Groups Ranked on Surveillance Scores in Louisiana

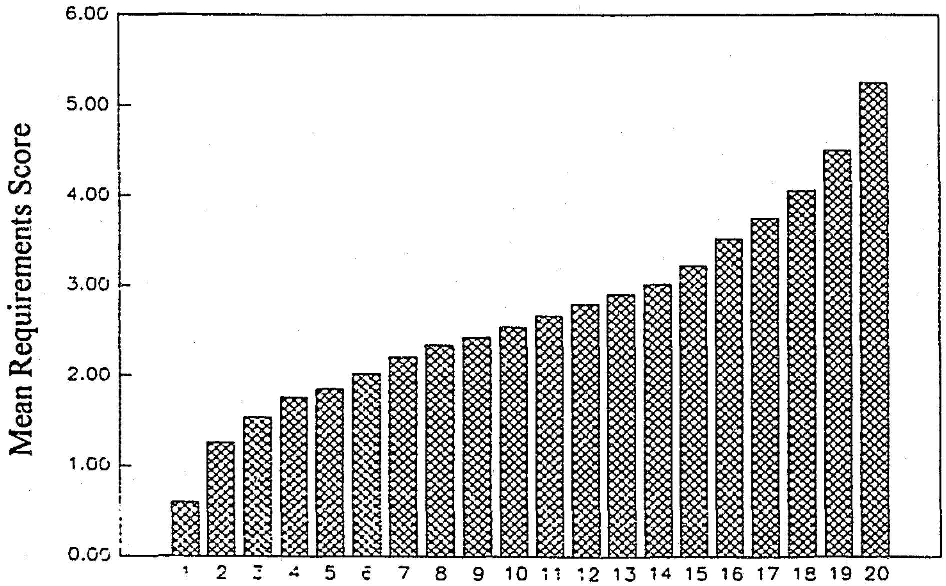


Note: Positive Adjustment Scores and Surveillance Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 46
Mean Overall Positive Adjustment Score and Mean Requirements Scores
By 20 Equal-N Groups Ranked on Requirements Scores in Louisiana

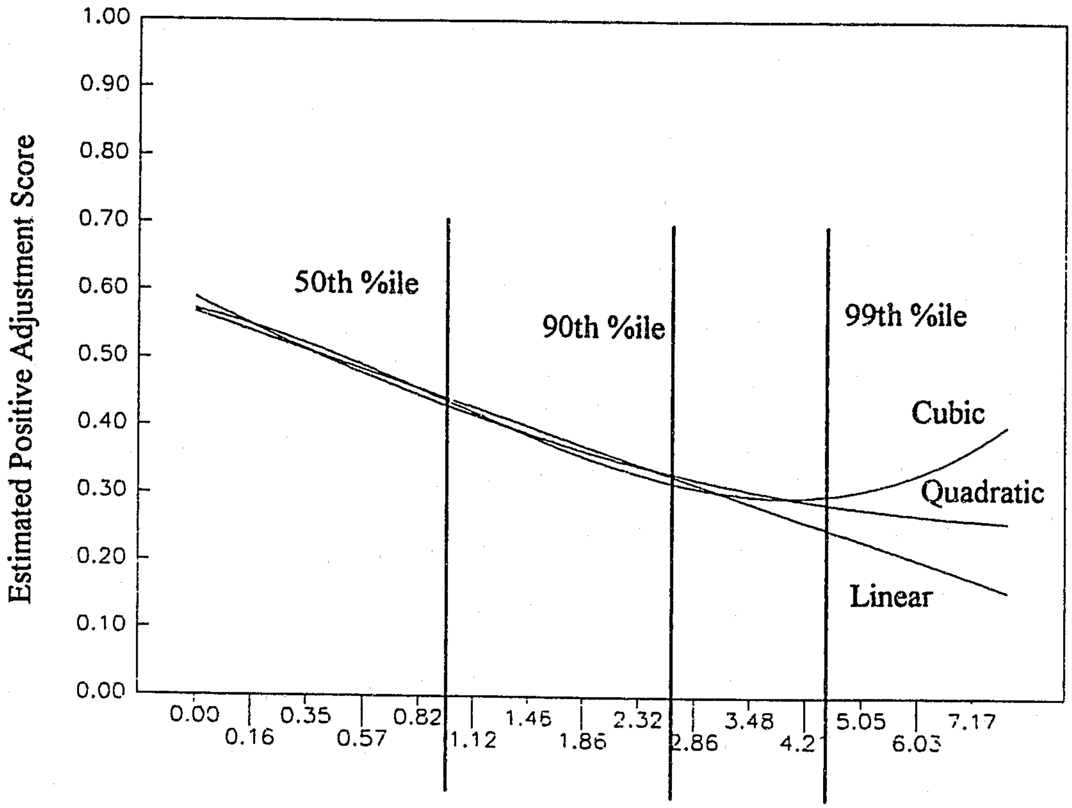


Equal-N Groups Ranked on Requirements Scores



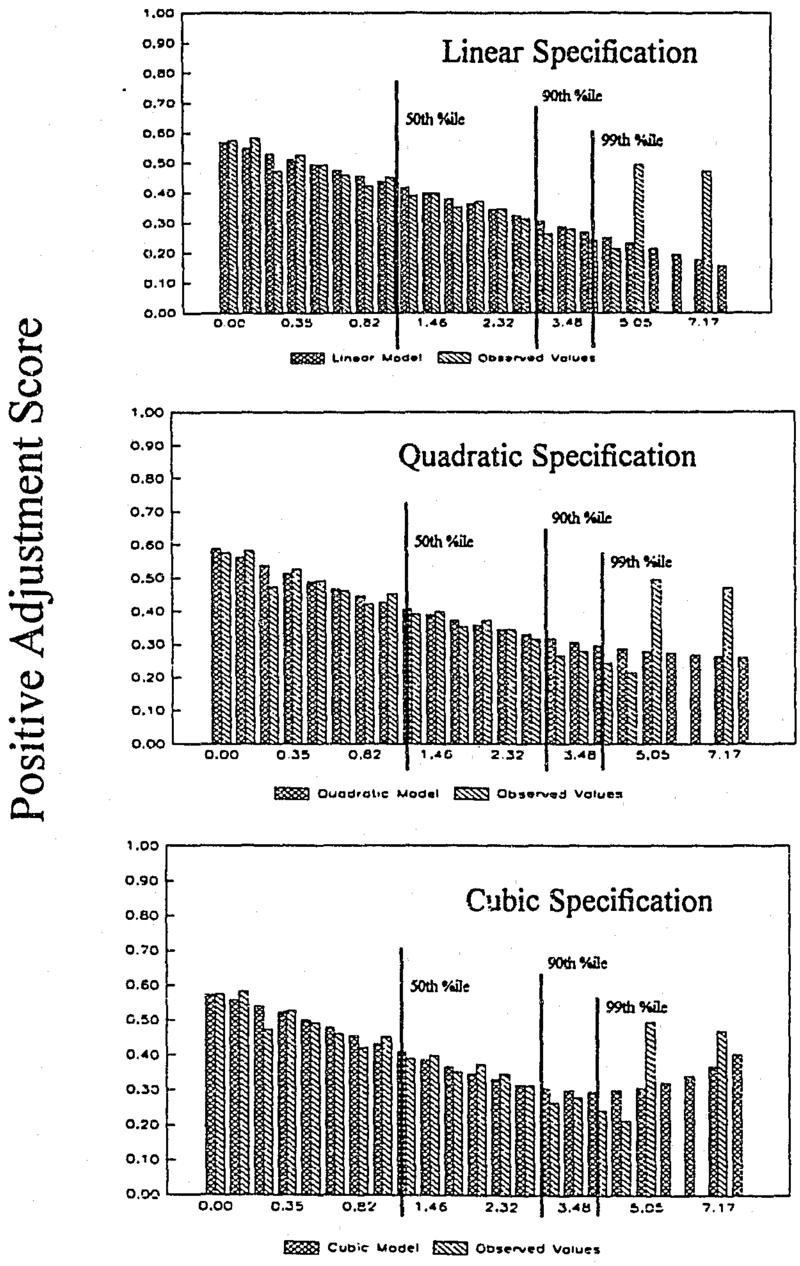
Note: Positive Adjustment Scores and Requirements Scores are averaged over each subject's follow-up period (regardless of the length).

Figure 47A
Linear, Quadratic, and Cubic Regression Functions For Knowledge Scores and Estimated Overall Positive Adjustment Scores In Louisiana



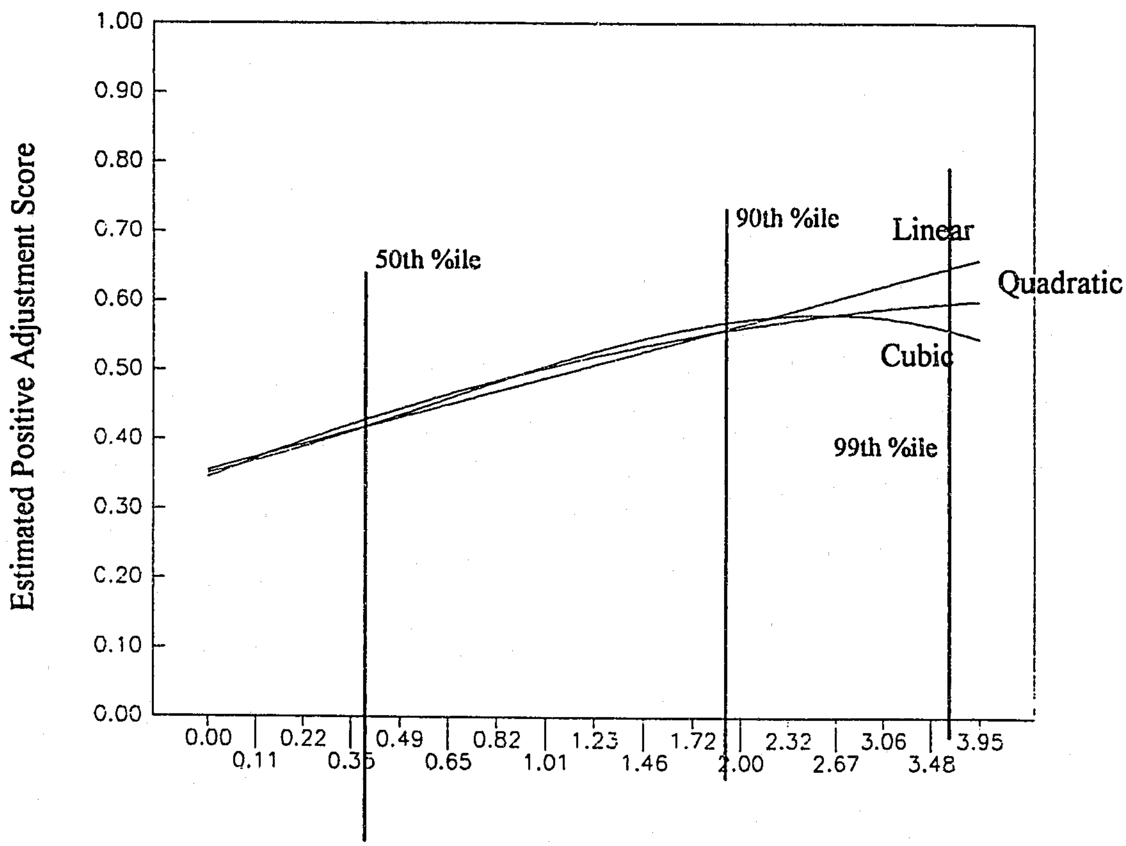
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of knowledge scores. The exponentiated log-values (raw knowledge scores) are presented here. Percentiles refer to relative standing on the knowledge index.

Figure 47B
Comparison of Alternative Model Specifications To Actual Mean Values of the Positive Adjustment Score Given Knowledge Scores.



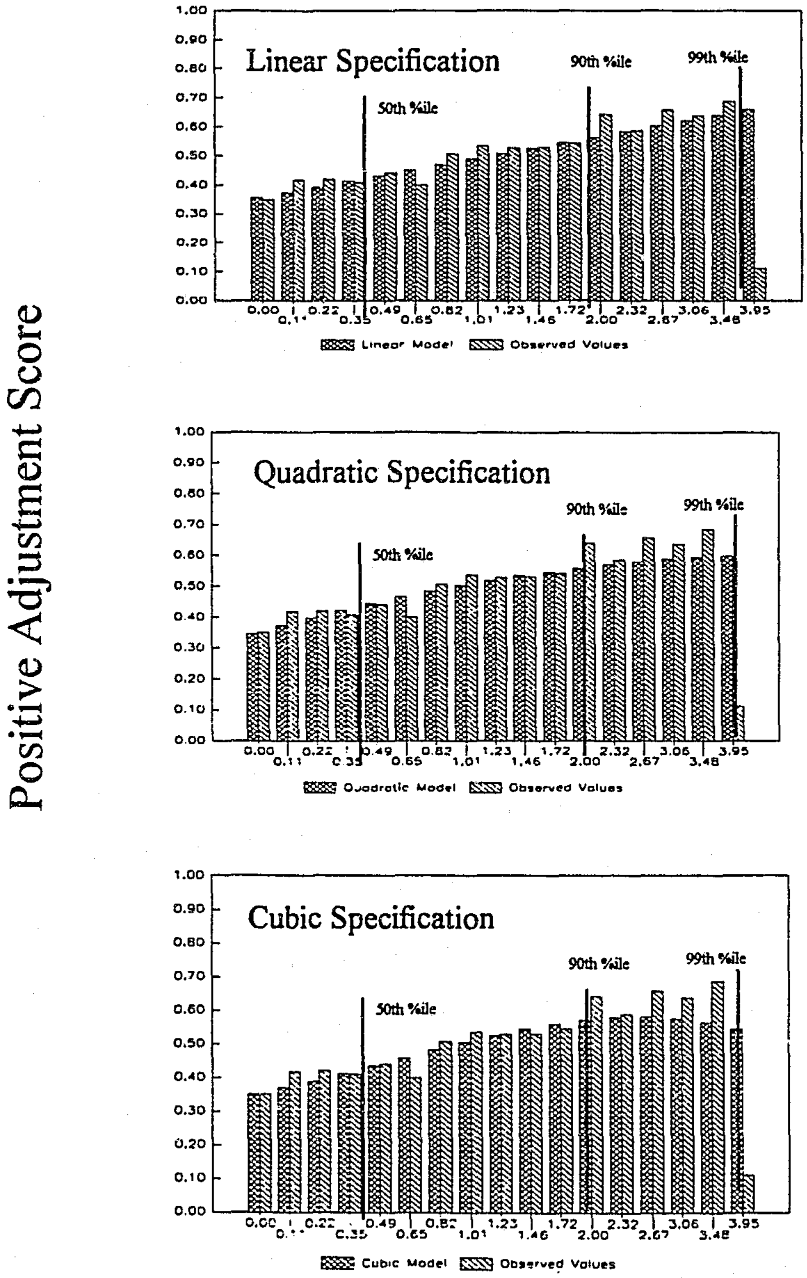
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of knowledge scores. The exponentiated log-values (raw knowledge scores) are presented here. Percentiles refer to relative standing on the knowledge index.

Figure 48A
Linear, Quadratic, and Cubic Regression Functions For Surveillance Scores and Estimated Overall Positive Adjustment Scores In Louisiana



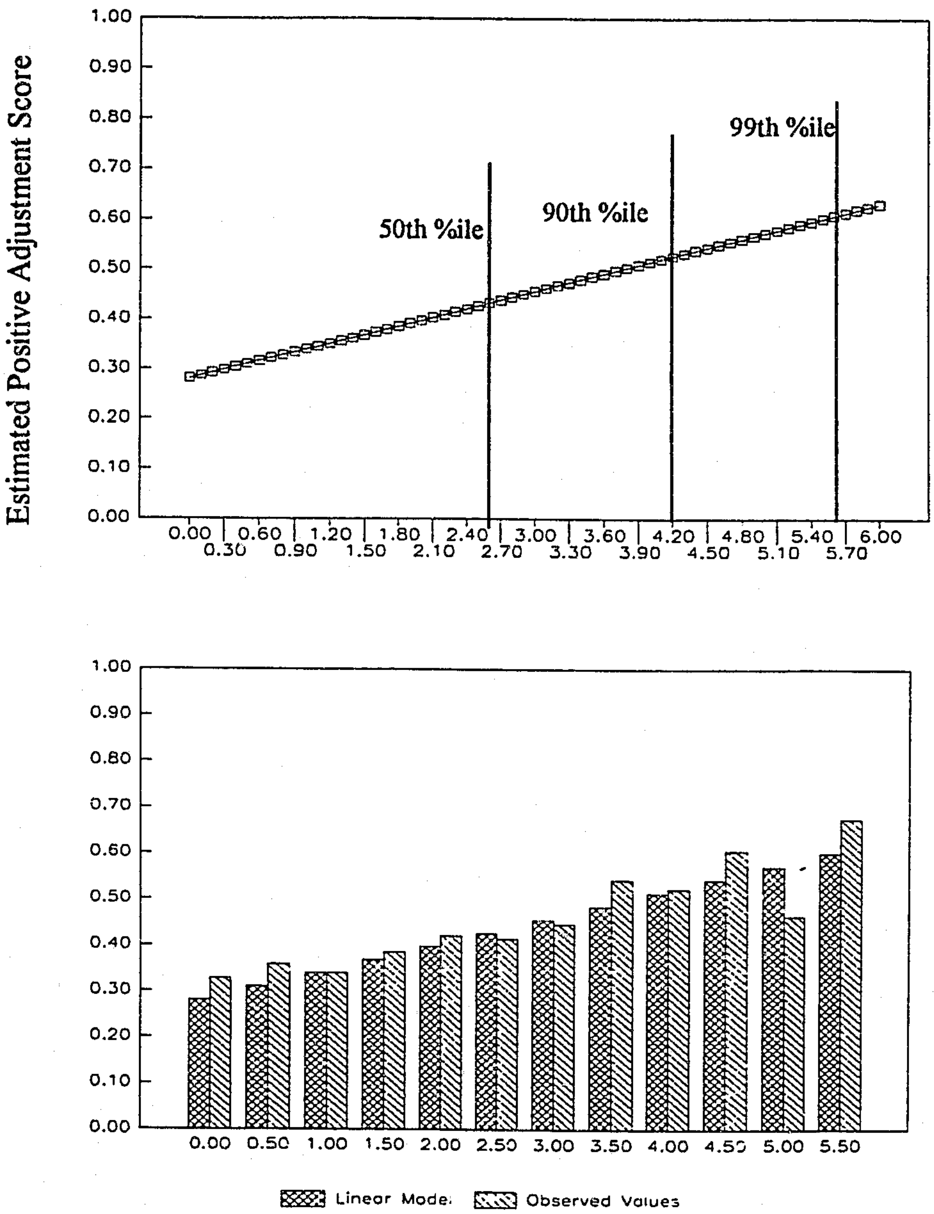
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of surveillance scores. The exponentiated log-values (raw surveillance scores) are presented here. Percentiles refer to relative standing on the surveillance index.

Figure 48B
Comparison of Alternative Model Specifications To Actual Mean Values of the Positive Adjustment Score Given Surveillance Scores.



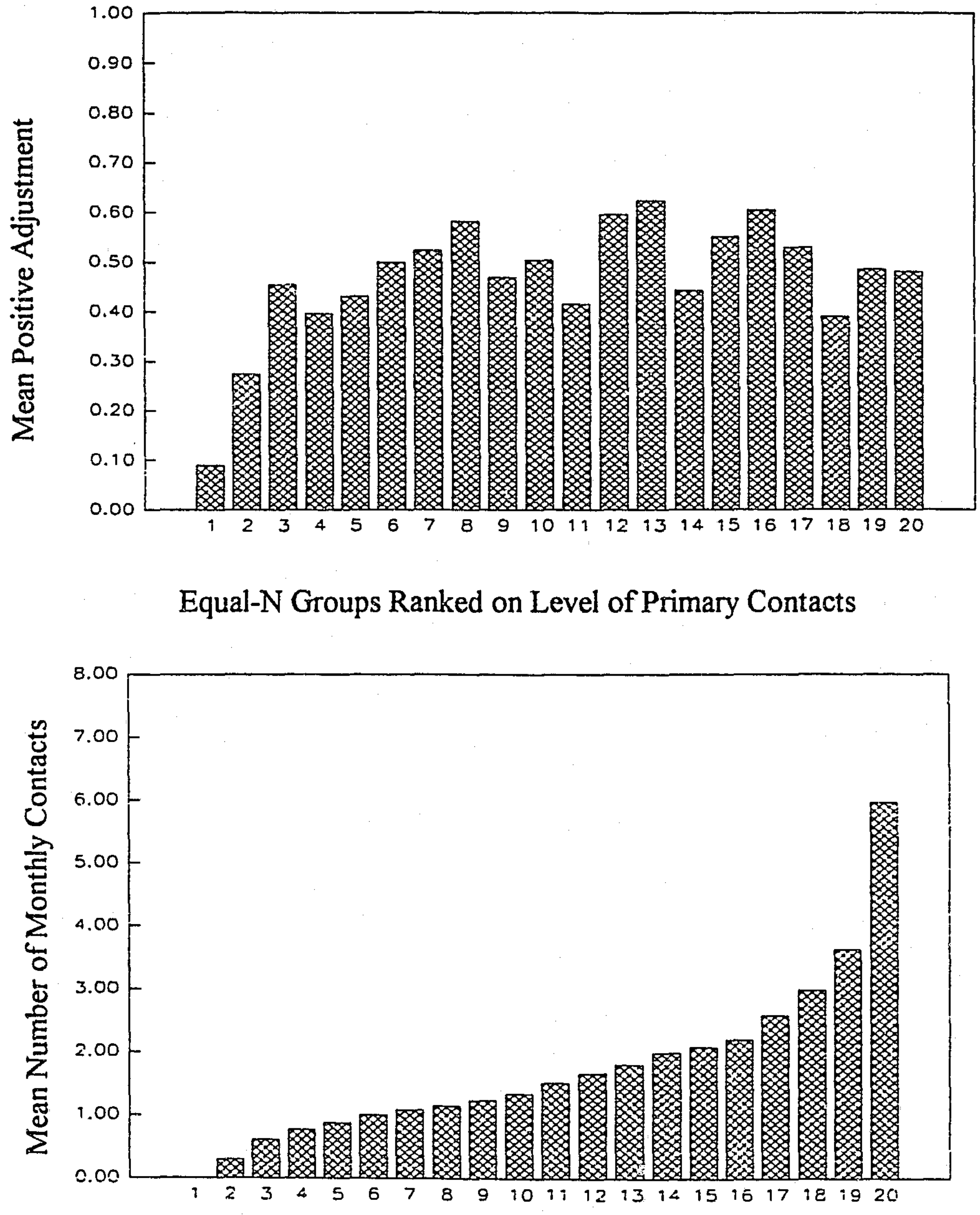
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of surveillance scores. The exponentiated log-values (raw surveillance scores) are presented here. Percentiles refer to relative standing on the surveillance index.

Figure 49
Linear Regression Function For Requirements' Scores, Estimated Overall Positive Adjustment Scores, & Actual Positive Adjustment Scores In Louisiana



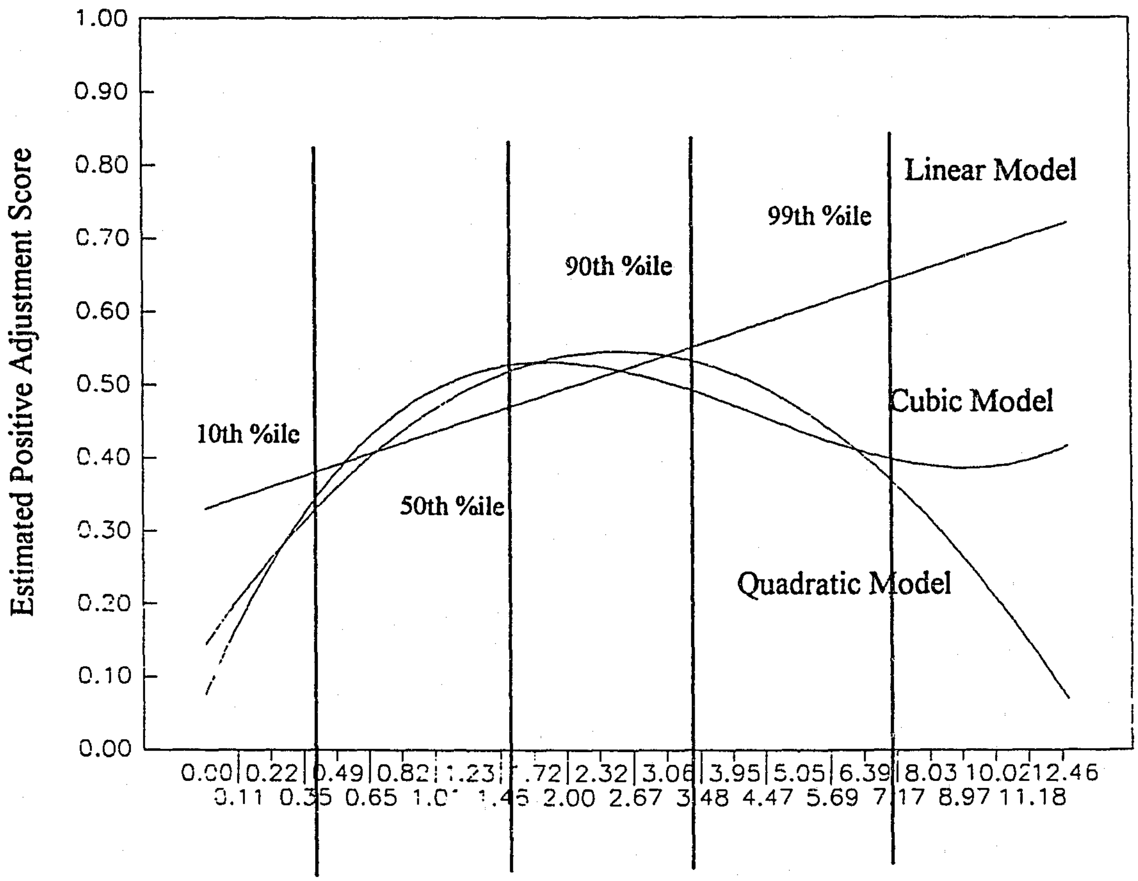
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Percentiles refer to relative standing on the requirements index.

Figure 50
Mean Overall Positive Adjustment Score and Mean Number of Monthly Contacts By 20 Equal-N Groups Ranked on Contact Levels in South Carolina



Note: Positive Adjustment Scores and Monthly Contact Rate are averaged over each subject's follow-up period (regardless of the length).

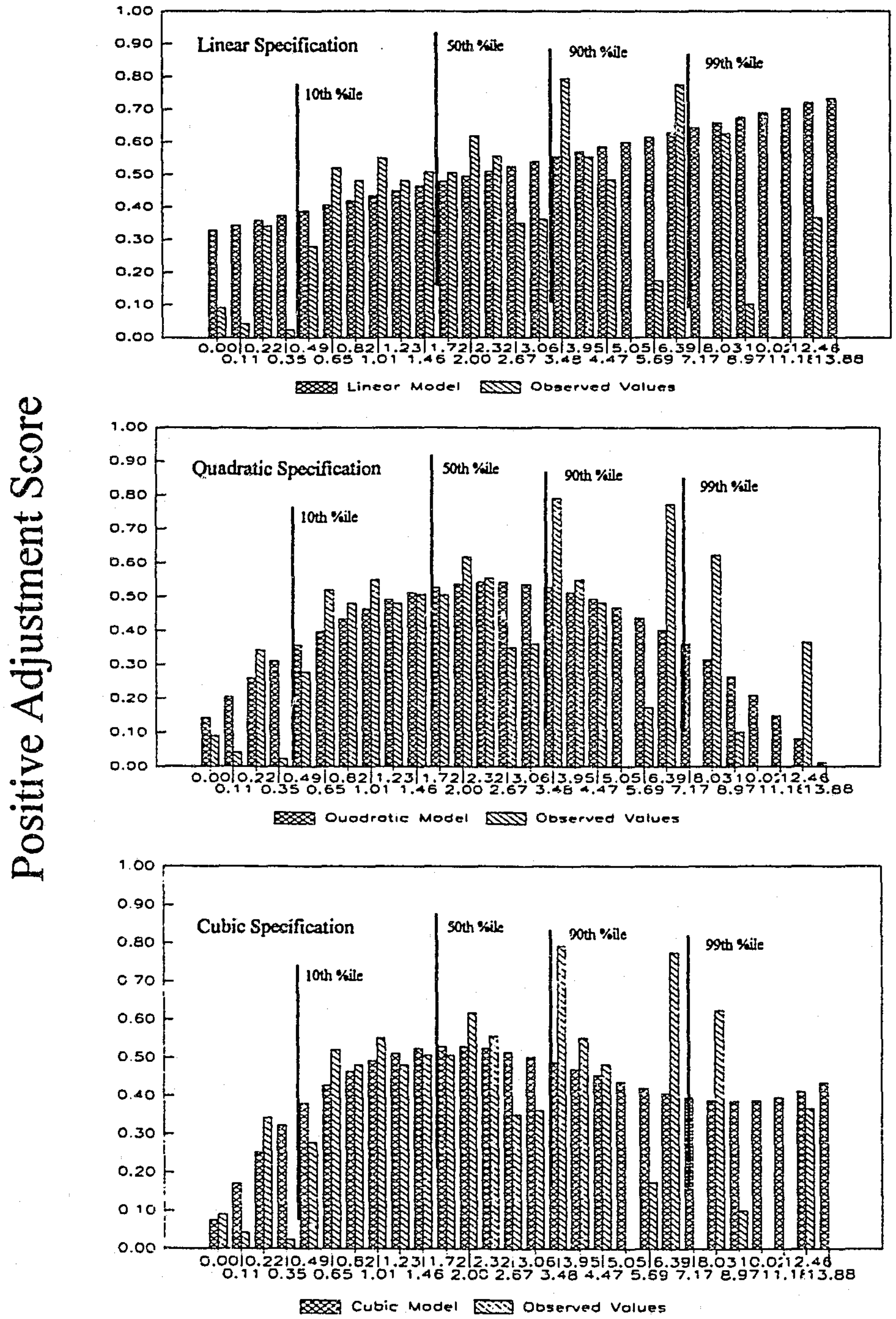
Figure 51A
Linear, Quadratic, and Cubic Regression Functions For Contact Levels and Estimated Overall Positive Adjustment Scores In South Carolina



Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates.

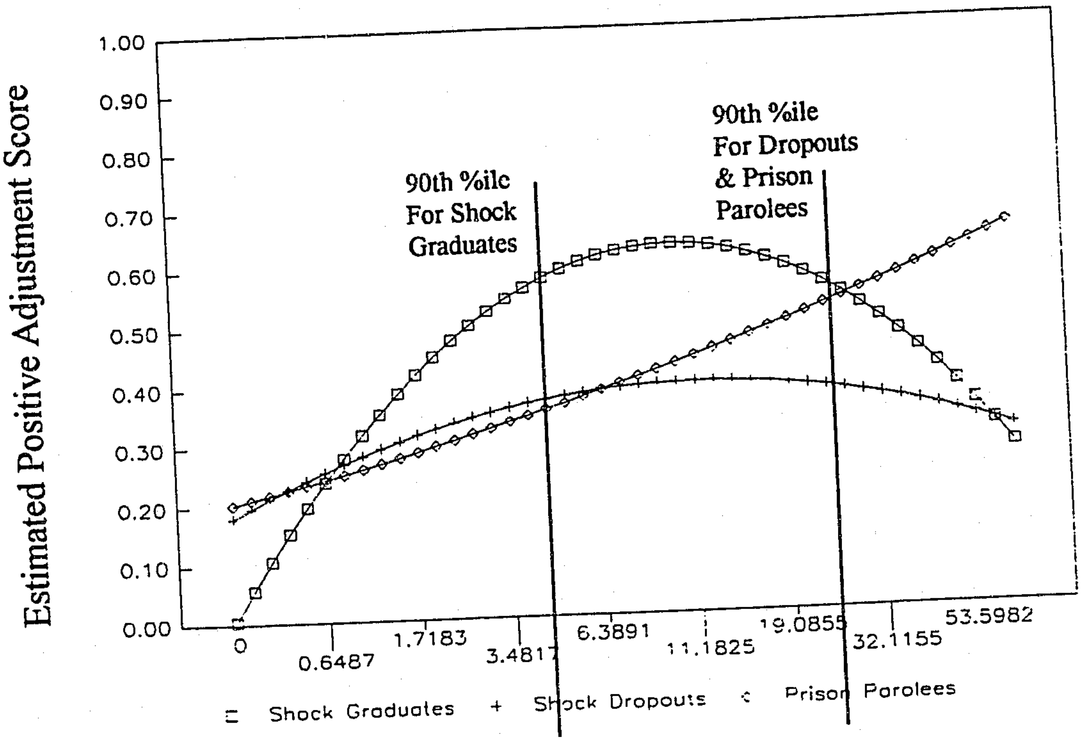
Figure 51B

Comparison of Alternative Model Specifications To Actual Mean Values of the Positive Adjustment Score Given Contact Levels in South Carolina.



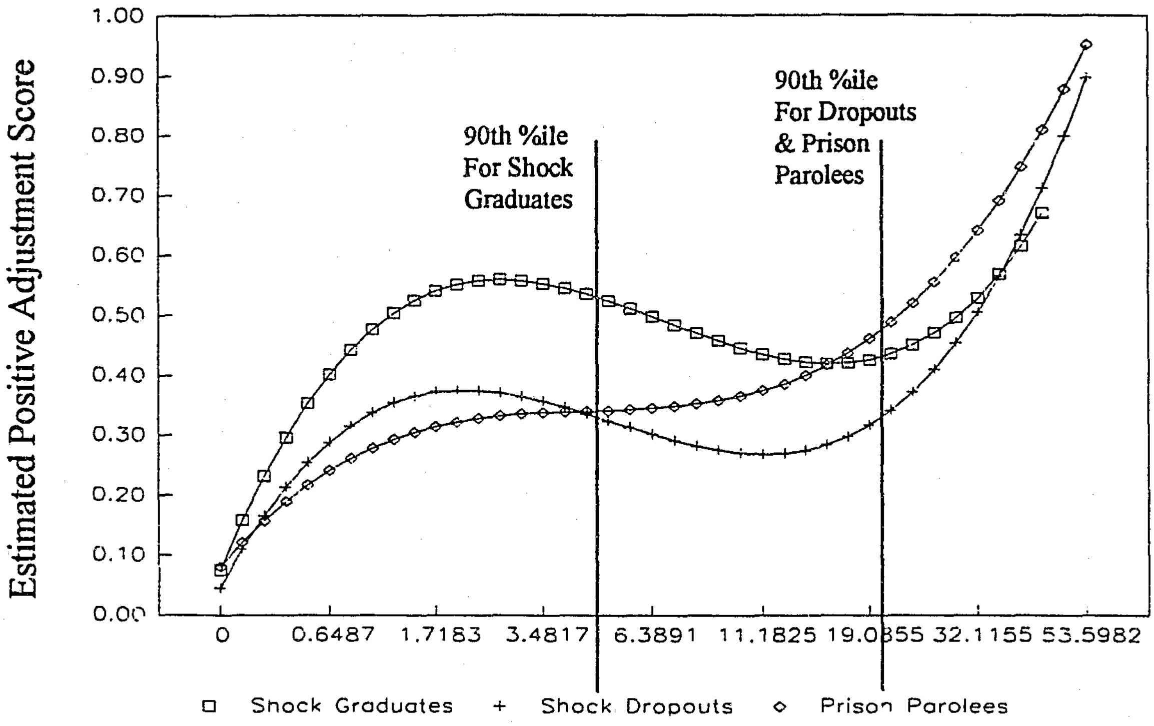
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates.

Figure 52
Assessment of Interaction Between Linear and Quadratic Primary Contacts Components and Treatment Sample in Florida



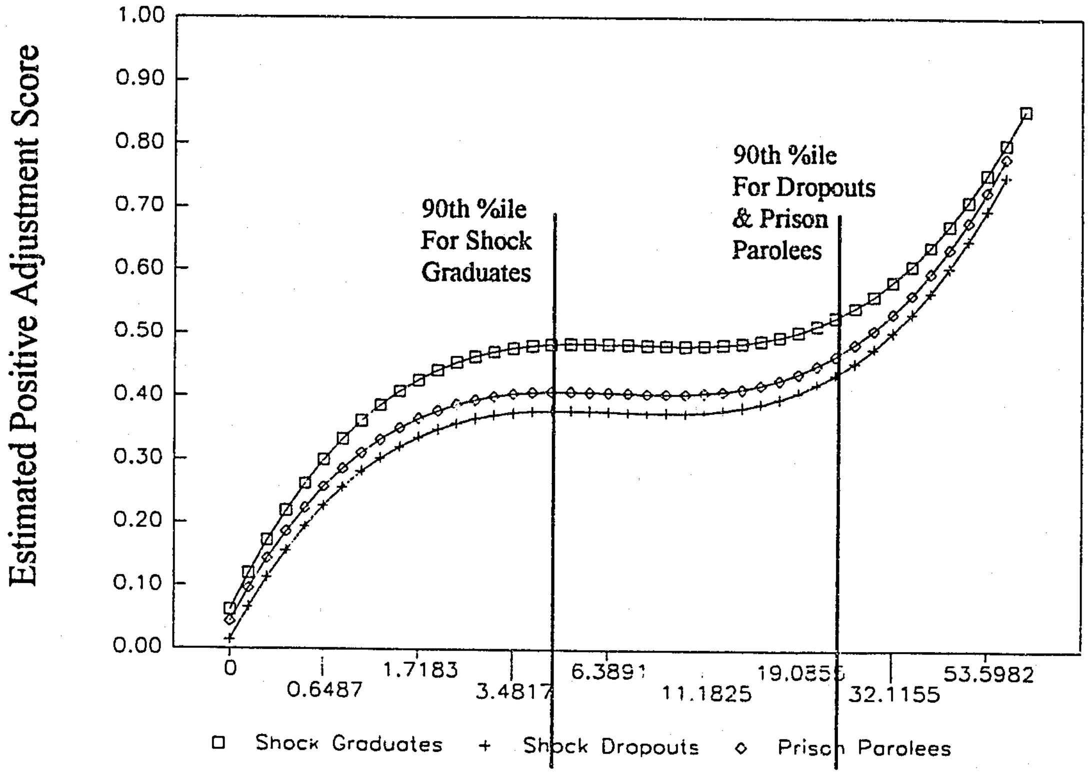
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 53
Assessment of Interaction Between Linear, Quadratic, and Cubic Primary Contacts Components and Treatment Sample in Florida



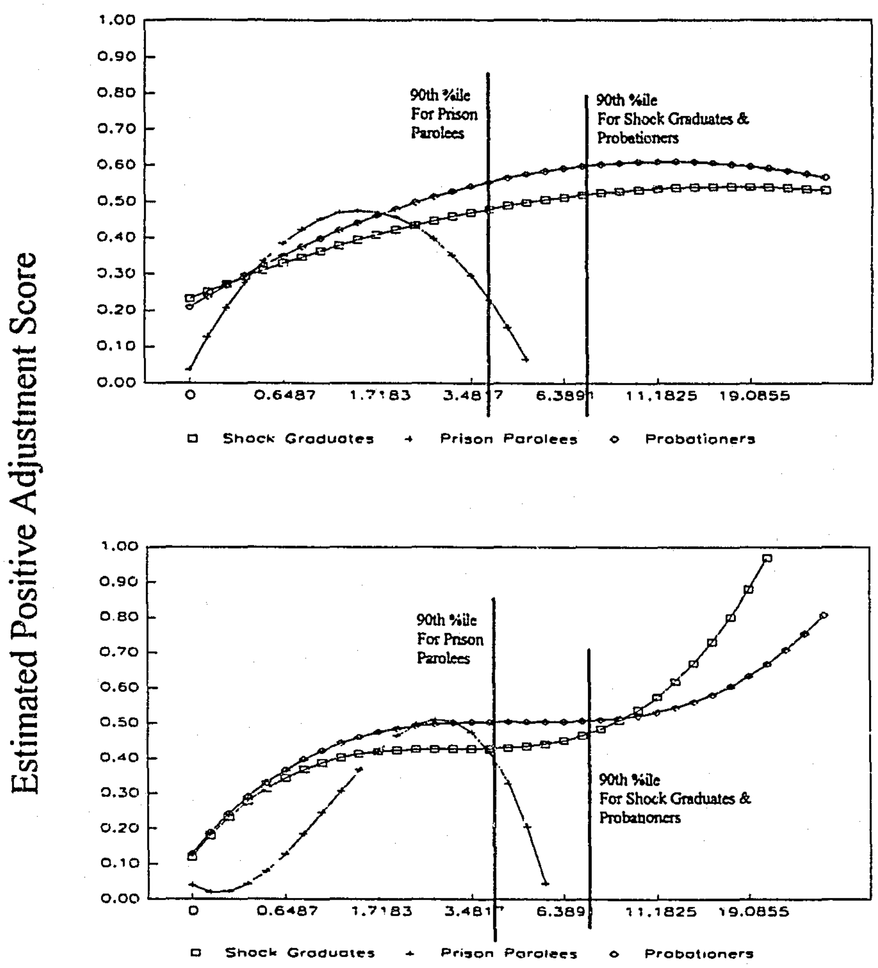
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 54
Estimated Effect of Primary Contacts and Treatment Sample Membership on Overall Positive Adjustment Scores in Florida



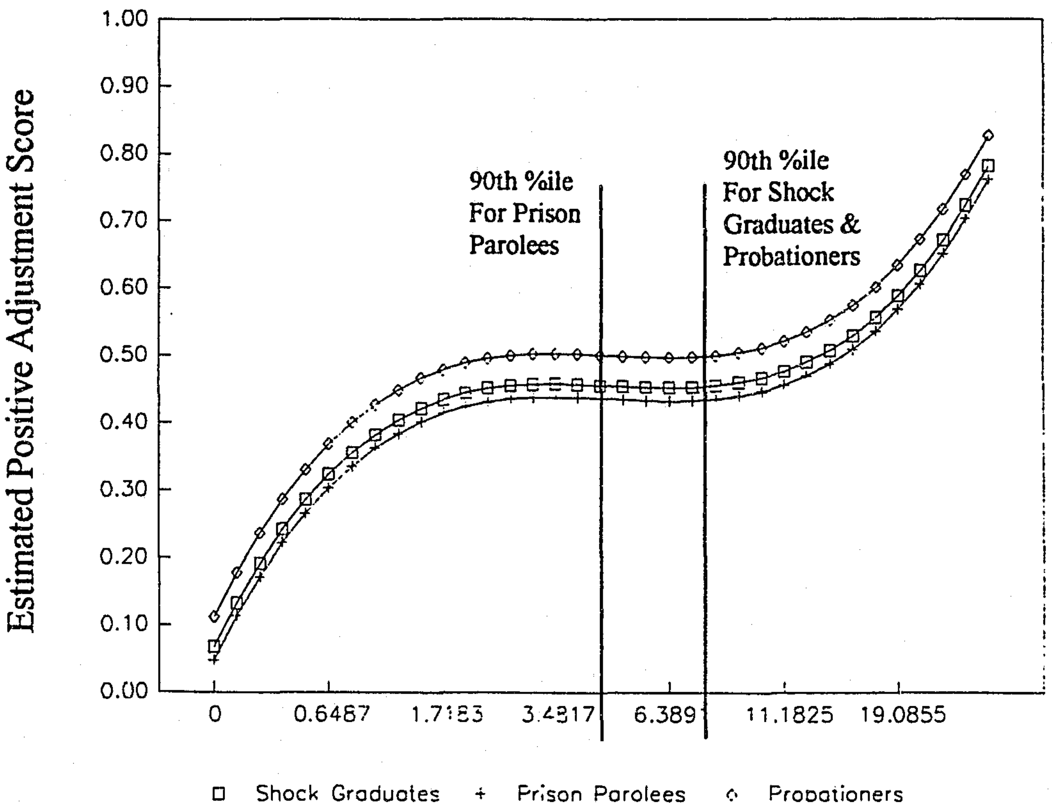
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 55A
Assessment of Interaction Between Linear and Quadratic Primary Contacts Components and Treatment Sample in Georgia



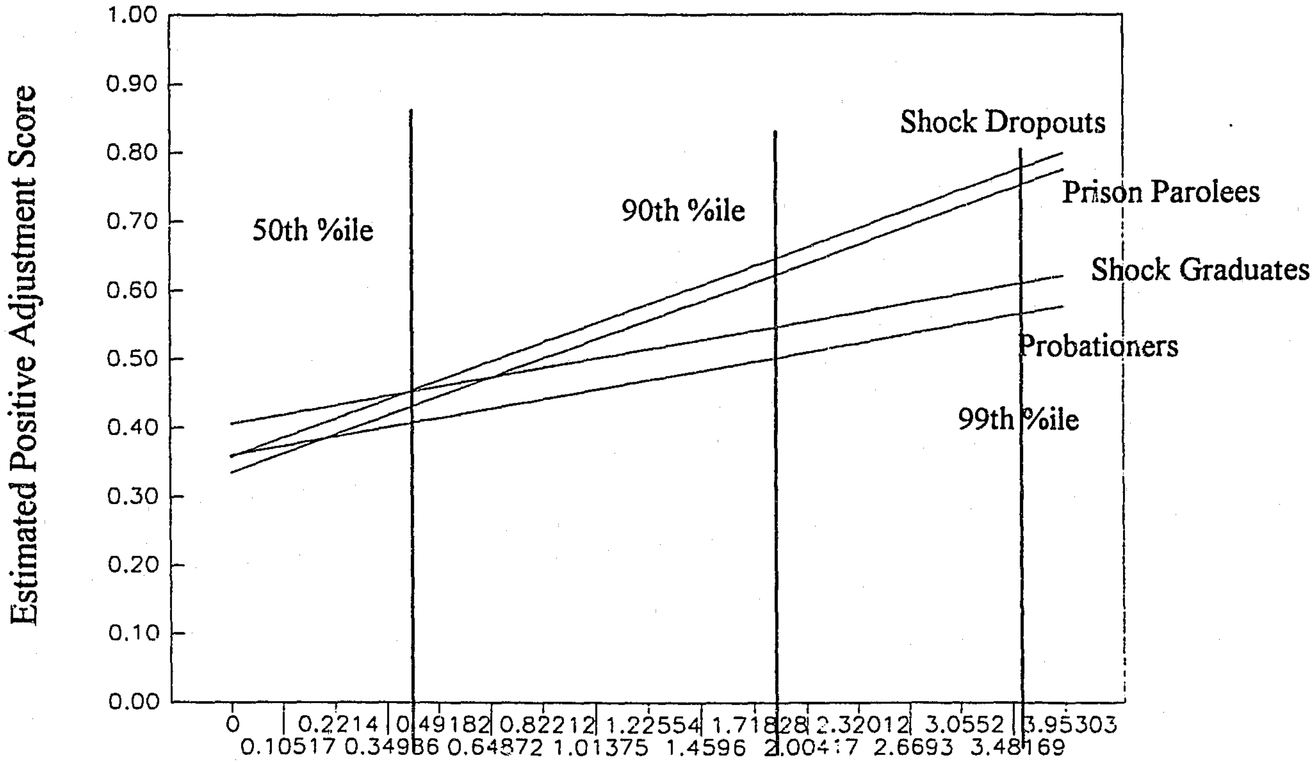
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 56
Estimated Effect of Primary Contacts and Treatment Sample Membership on Overall Positive Adjustment Scores In Georgia



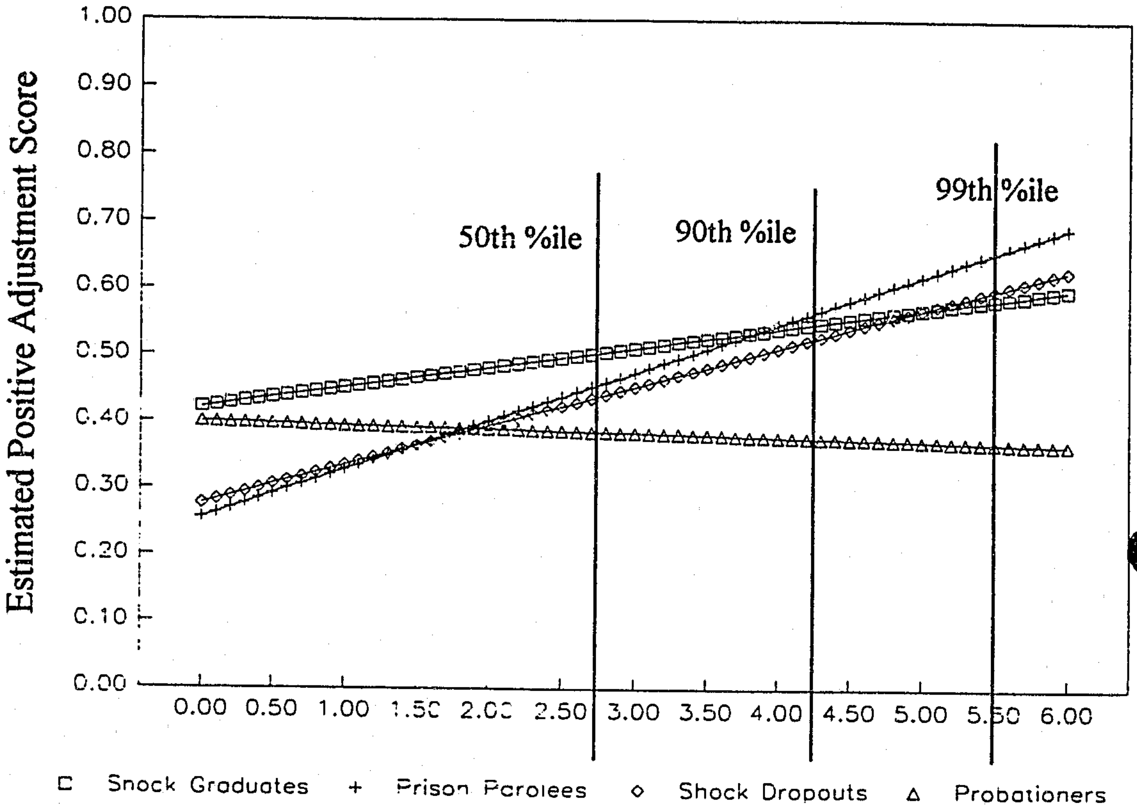
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 57
Assessment of Surveillance x Sample Interaction Effect on Positive Adjustment Scores In Louisiana



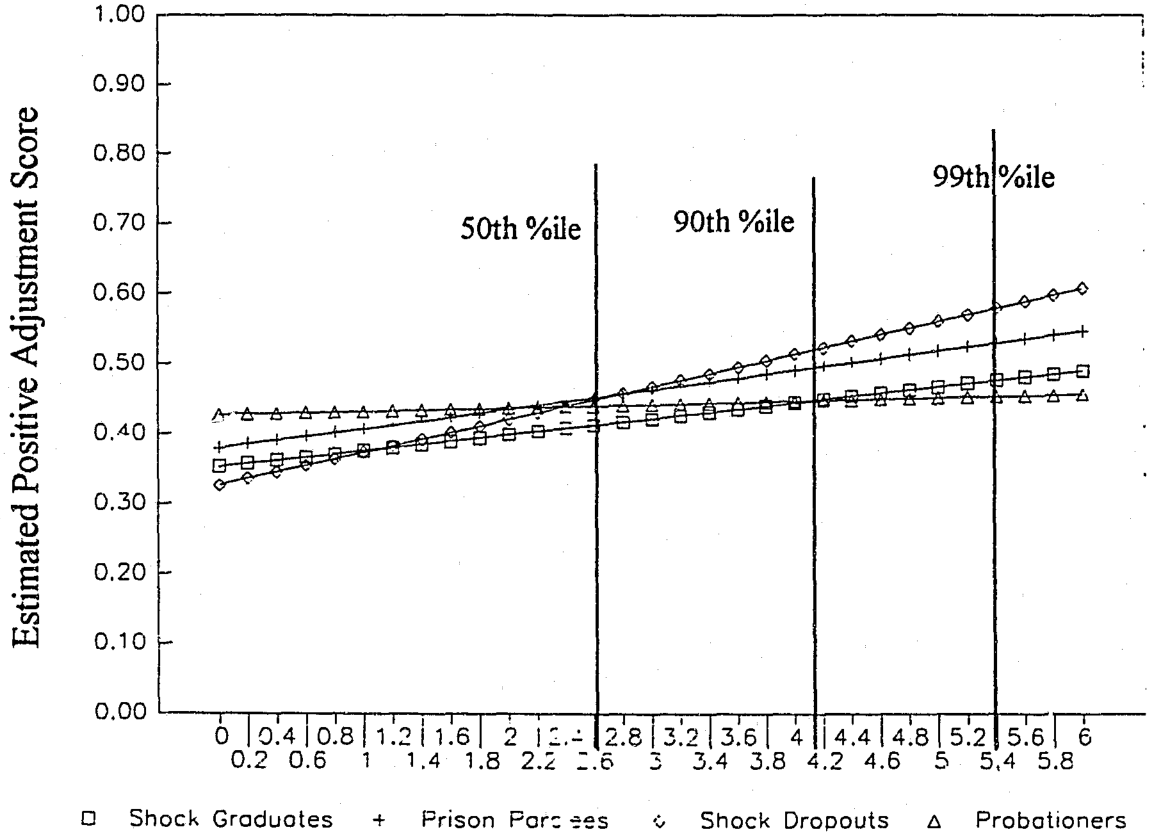
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). This model was estimated on a natural log transform of the surveillance index used in Louisiana. The exponentiated log-values (raw surveillance scores) are presented here. Percentiles represent relative standing on the surveillance index.

Figure 58A
Assessment of Requirements x Sample Interaction Effect on Positive Adjustment Scores In Louisiana



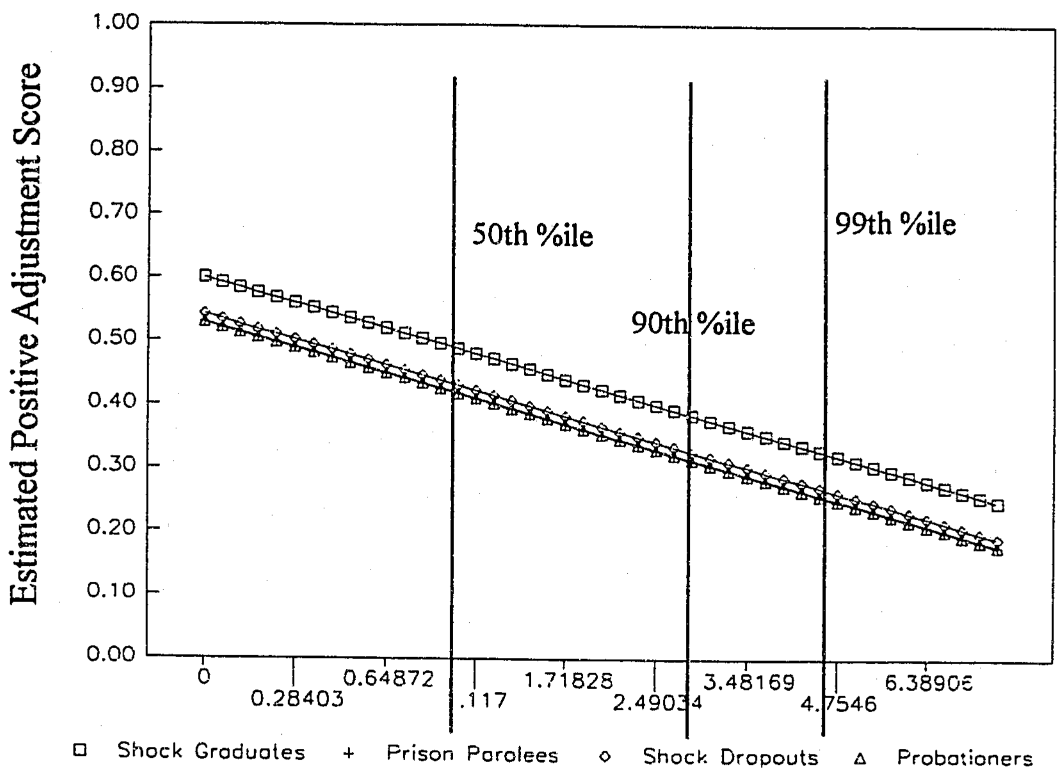
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Percentiles represent relative standing on the requirements index.

Figure 58B
Assessment of Requirements x Sample Interaction Effect on Positive Adjustment Scores In Louisiana After Controlling For Other Measures of Supervision Intensity (Knowledge and Surveillance Levels)



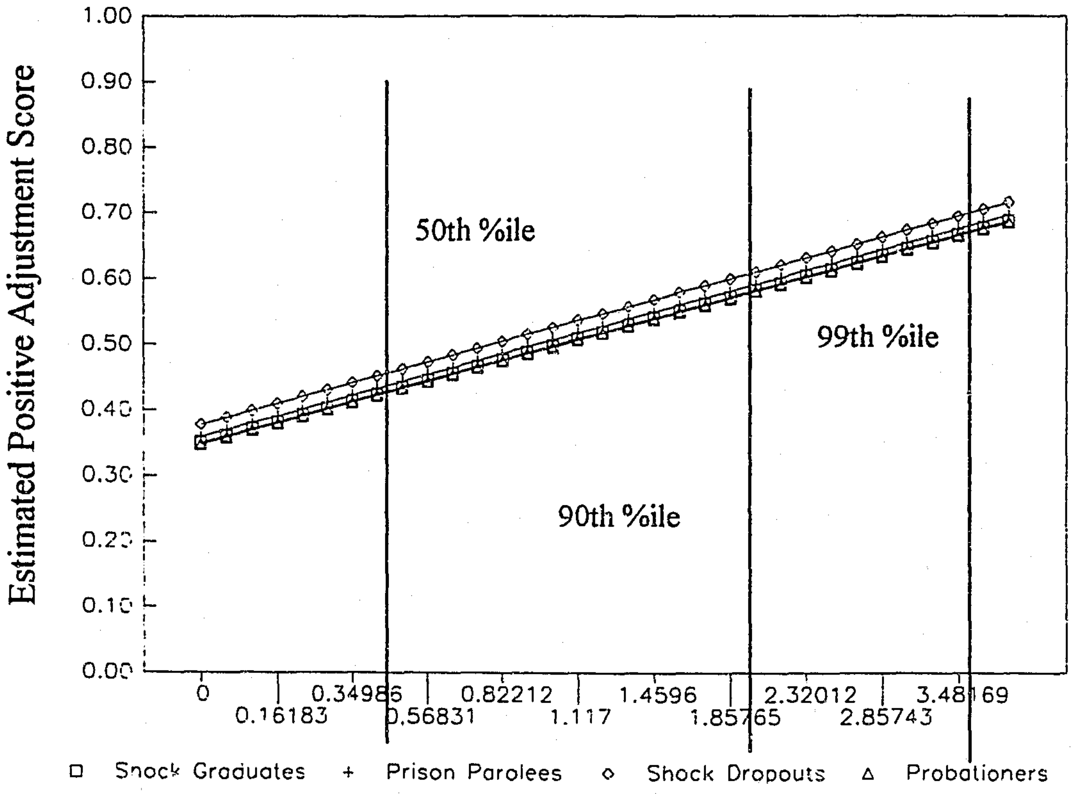
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). The effect of the requirements index is displayed here with knowledge and requirements scores fixed at their means (log knowledge=0.699 and log surveillance = 0.435). Percentiles represent relative standing on the requirements index.

Figure 59
Specification of Effect of Knowledge Scores on Positive Adjustment
By Treatment Sample in Louisiana



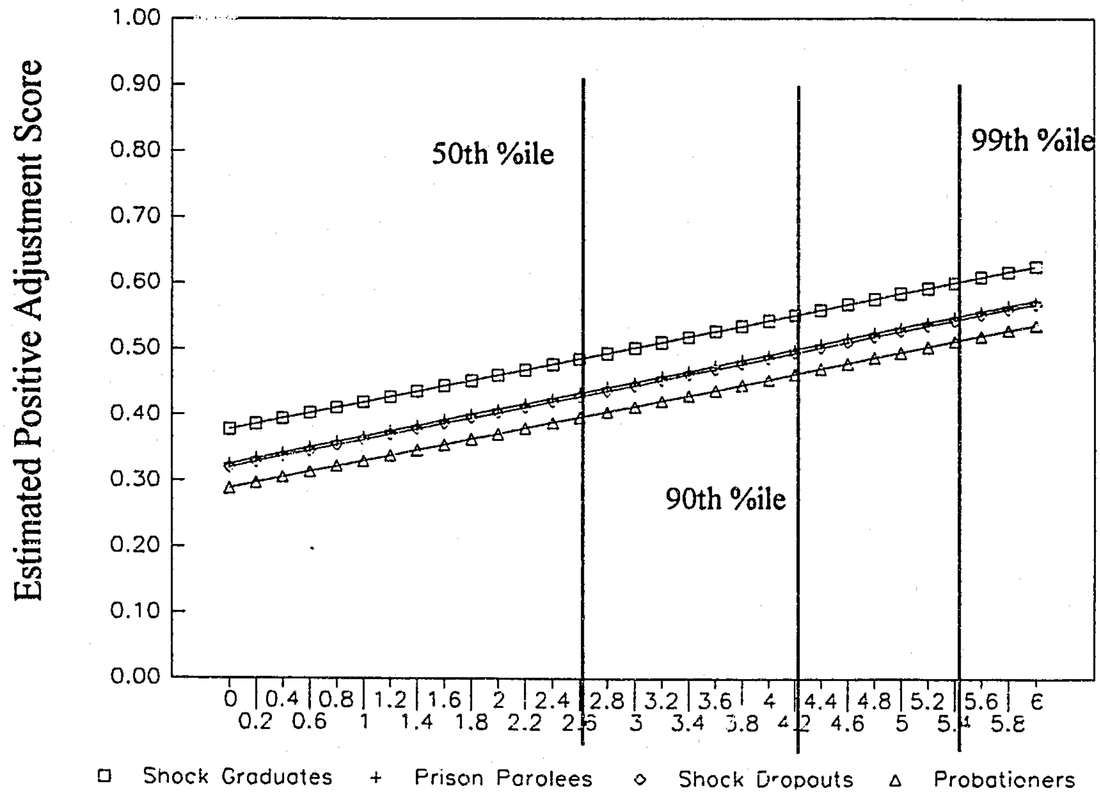
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of knowledge scores. The exponentiated log-values (raw knowledge scores) are presented here. Percentiles represent relative standing on knowledge scores.

Figure 60
Specification of Effect of Surveillance Scores on Positive Adjustment
By Treatment Sample in Louisiana



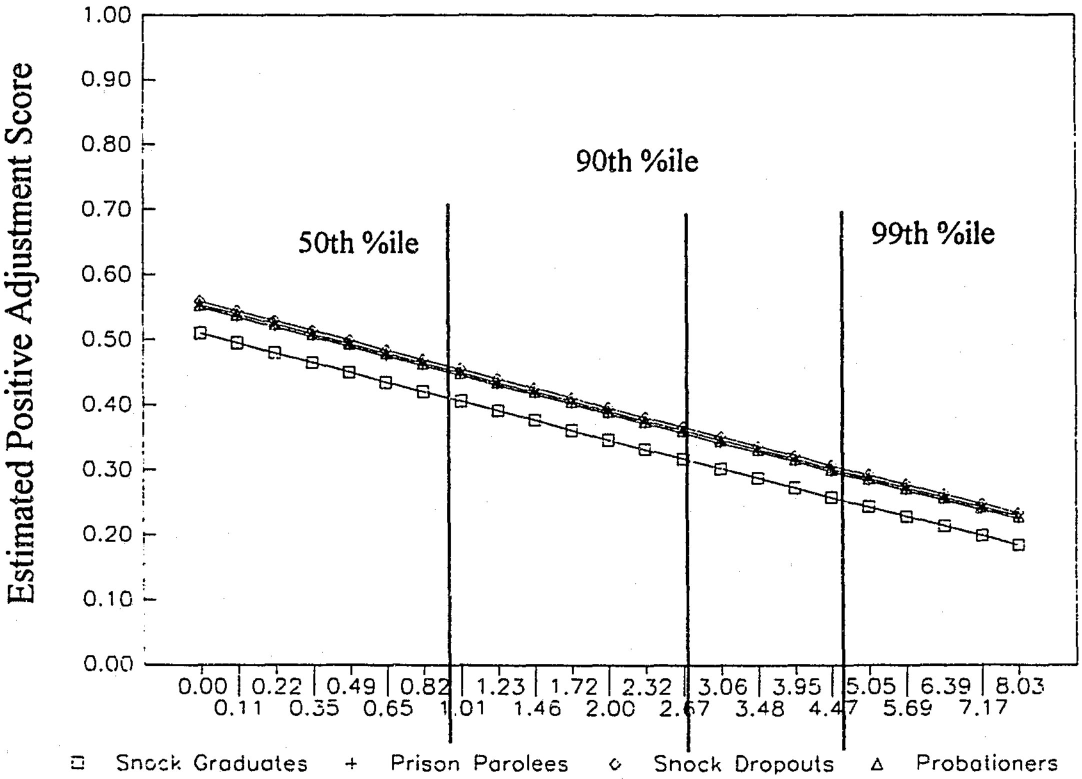
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of surveillance scores. The exponentiated log-values (raw surveillance scores) are presented here. Percentiles represent relative standing on surveillance scores.

Figure 61
Specification of Effect of Requirements' Scores on Positive Adjustment
By Treatment Sample in Louisiana



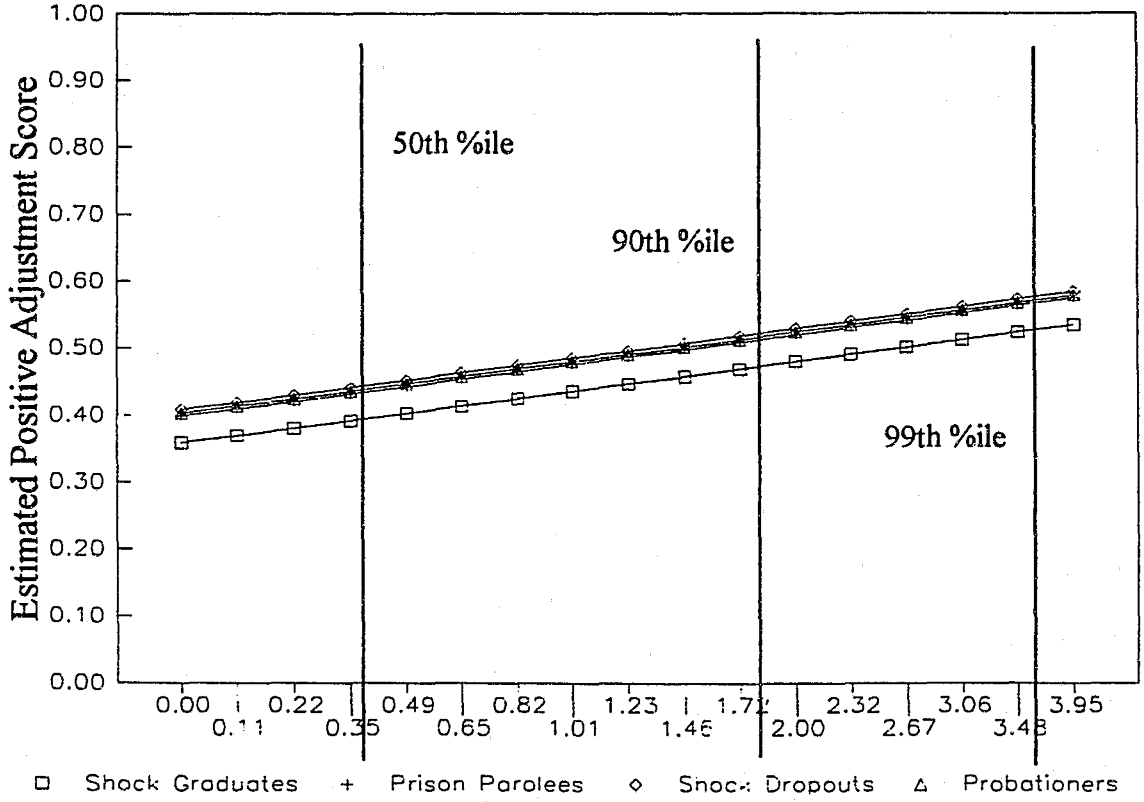
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Percentiles represent relative standing on requirements scores.

Figure 62
Estimated Effect of Knowledge Scores on Positive Adjustment
By Treatment Sample Controlling For Requirements' and Surveillance Scores
in Louisiana



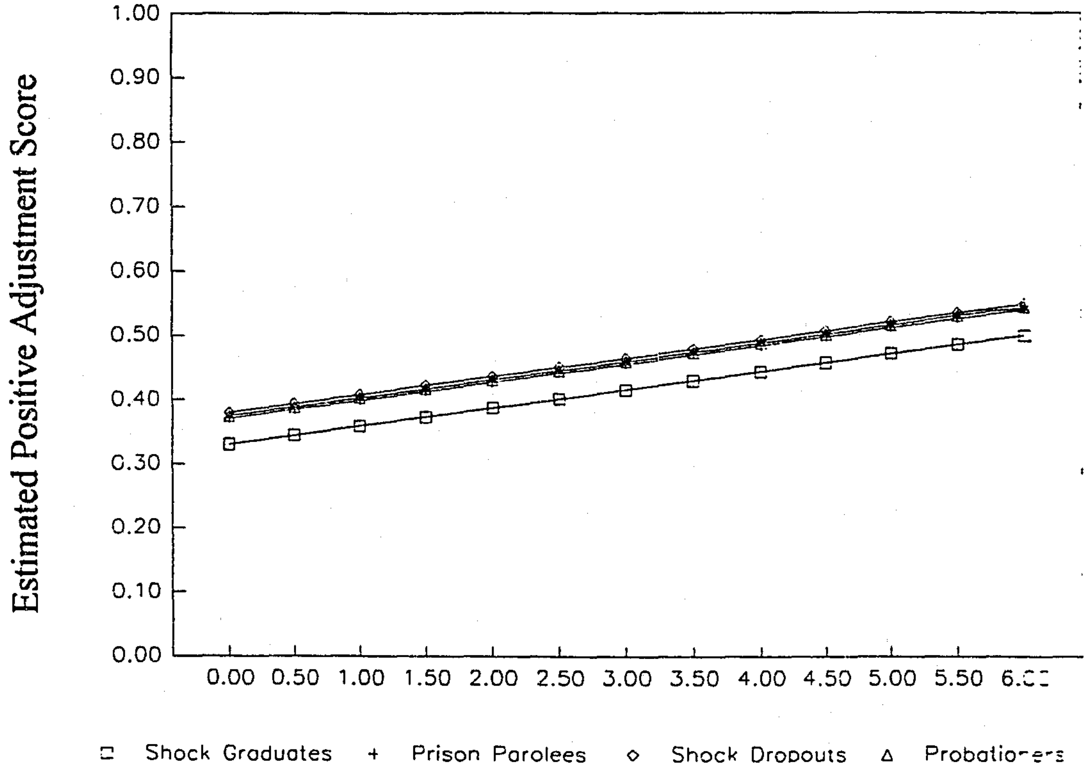
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). The model was estimated using the log of overall knowledge scores. Here, the logs have been exponentiated back to original knowledge index scores. The effect of the knowledge index is displayed here with surveillance and requirements scores fixed at their means (log surveillance = 0.435 and requirements = 2.722). Percentiles represent relative standing on the knowledge index.

Figure 63
Estimated Effect of Surveillance Scores on Positive Adjustment
By Treatment Sample Controlling For Knowledge and Requirements' Scores
in Louisiana



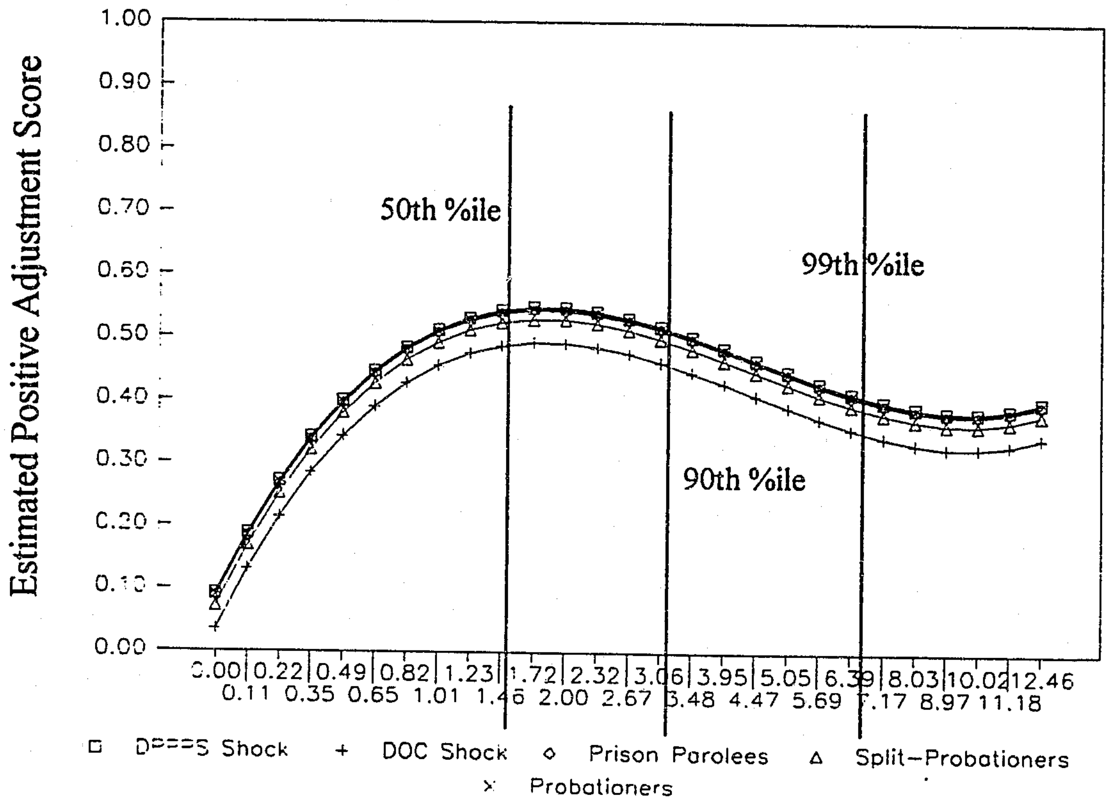
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). The model was estimated with overall surveillance scores measured in log units. Here, the log units have been exponentiated back into raw surveillance scores. The effect of the surveillance index is displayed here with knowledge and requirements scores fixed at their means (log knowledge = 0.699 and requirements = 2.722). Percentiles represent relative standing on the surveillance index.

Figure 64
Estimated Effect of Requirements Scores on Positive Adjustment
By Treatment Sample Controlling For Knowledge and Surveillance Scores
in Louisiana



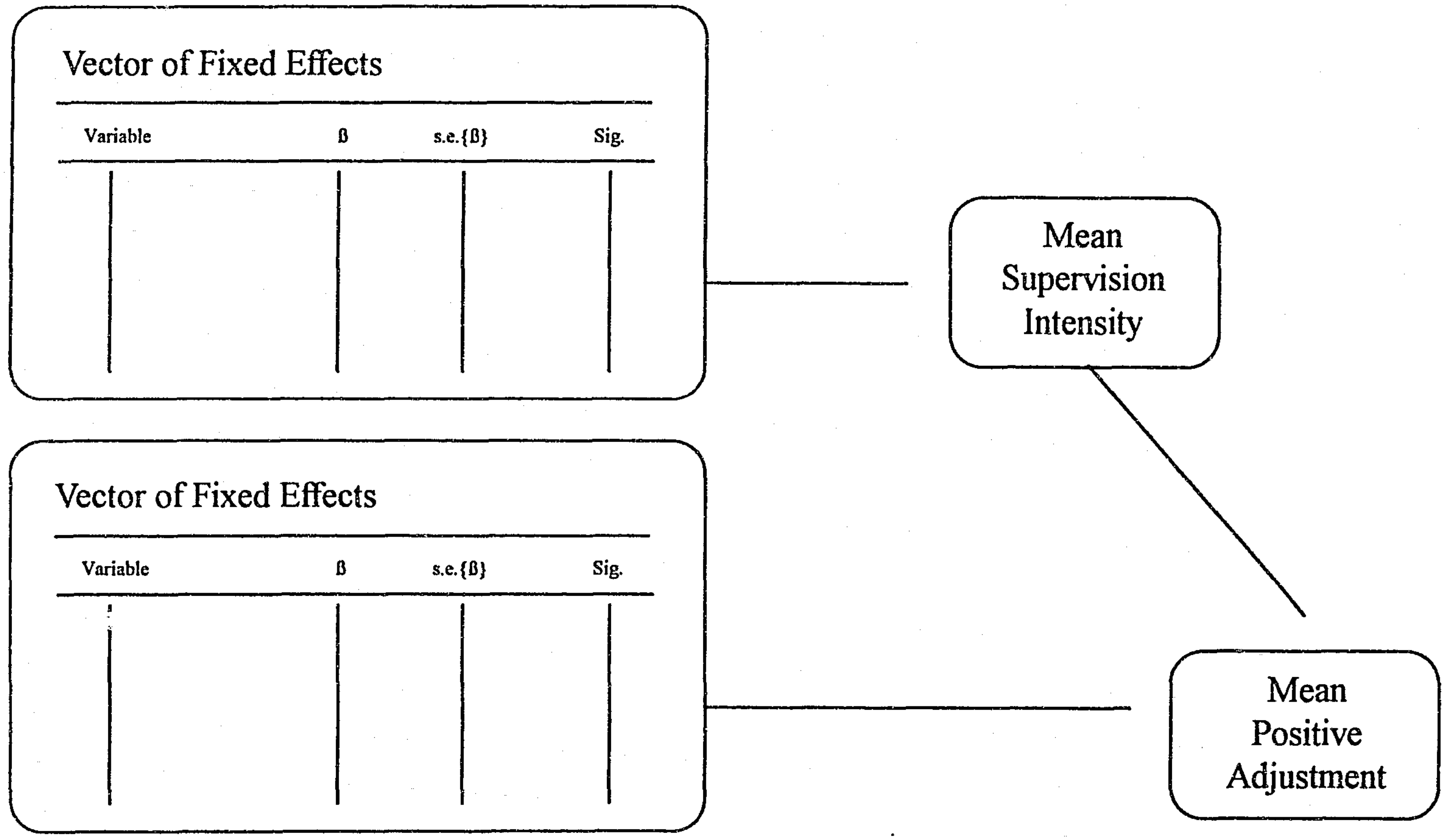
Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). The effect of the requirements index is displayed here with knowledge and surveillance scores fixed at their means (log knowledge = 0.699 and log surveillance = 0.435). Percentiles represent relative standing on the requirements' index.

Figure 65
Estimated Effect of Primary Contacts and Treatment Sample Membership on Overall Positive Adjustment Scores In South Carolina



Note: Regression functions are estimated on overall positive adjustment scores (averaged over each subject's complete follow-up period). Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles represent relative standing on primary contact rates.

Figure 66
Structure of Cross-Sectional Positive Adjustment Model



Note: All variables were not available or used in all states. Mean supervision intensity and positive adjustment are calculated for each offender by summing all available scores and dividing by the number of scores available.

Figure 67
Structure of Four-Wave Recursive Panel Model

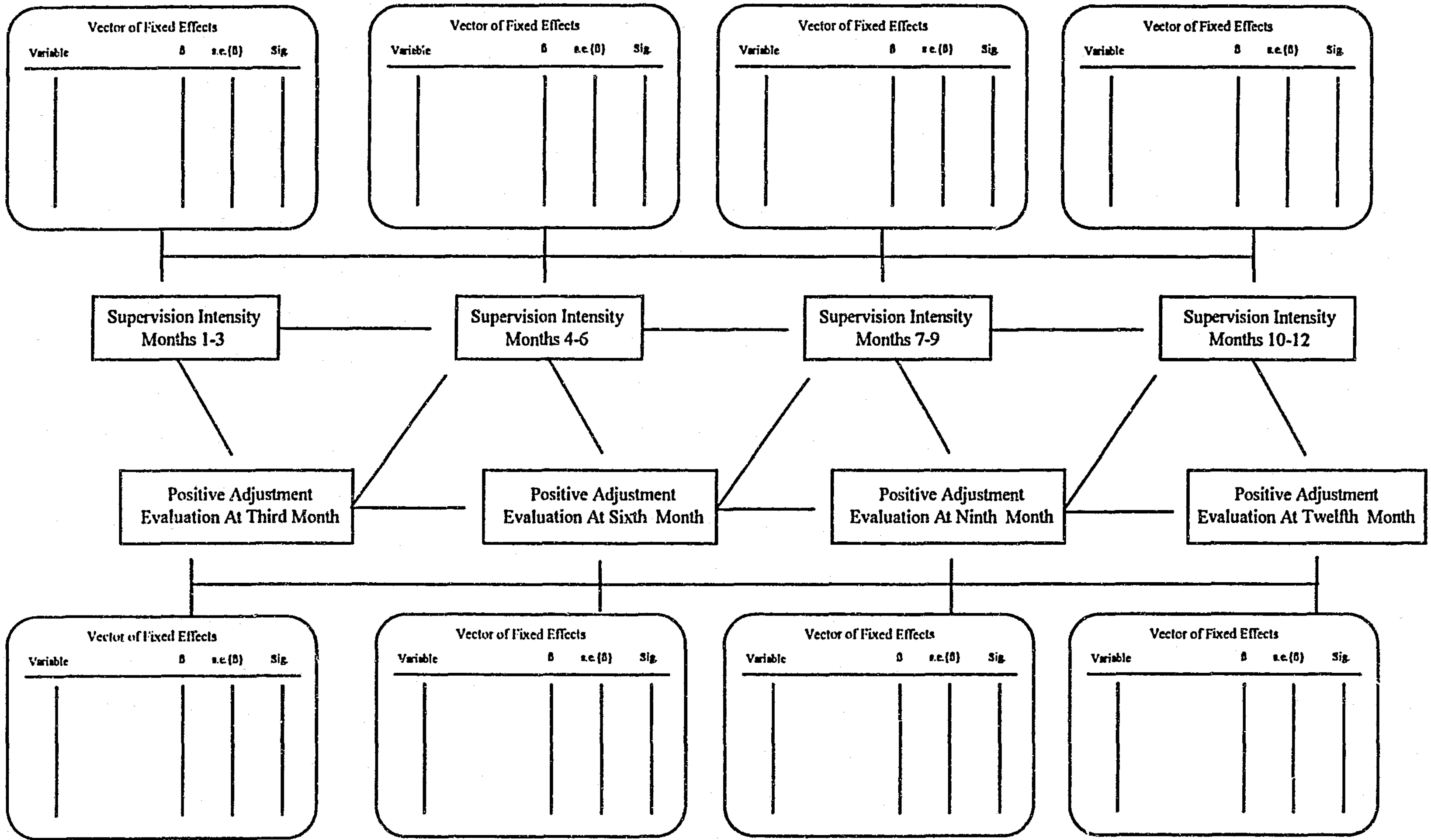


Figure 68
Florida Cross-Sectional Positive Adjustment Model

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	2.793	0.719	p<.001
Sample=Shock	-0.368	0.132	p<.006
Sample=Dropout	-0.111	0.153	p<.469
Nonwhite	-0.101	0.126	p<.422
Age @ Com. Supv.	-0.054	0.032	p<.098
Violent Offense	0.445	0.142	p<.002
Drug Offense	-0.001	0.171	p<.994
New Crime	-0.047	0.161	p<.771
Cohort=Missing	-0.256	0.265	p<.336
Cohort 1	-0.115	0.169	p<.495
Cohort 2	-0.097	0.165	p<.558
Cohort 3	-0.247	0.166	p<.139

Mean Log Primary Contacts (Valid N=276)

$R^2 = .108$

Effect	ß	s.e. {ß}	p <
Linear	.040	.031	.203
Quadratic	-.059	.023	.011
Cubic	.036	.015	.018

Vector of Fixed Effects

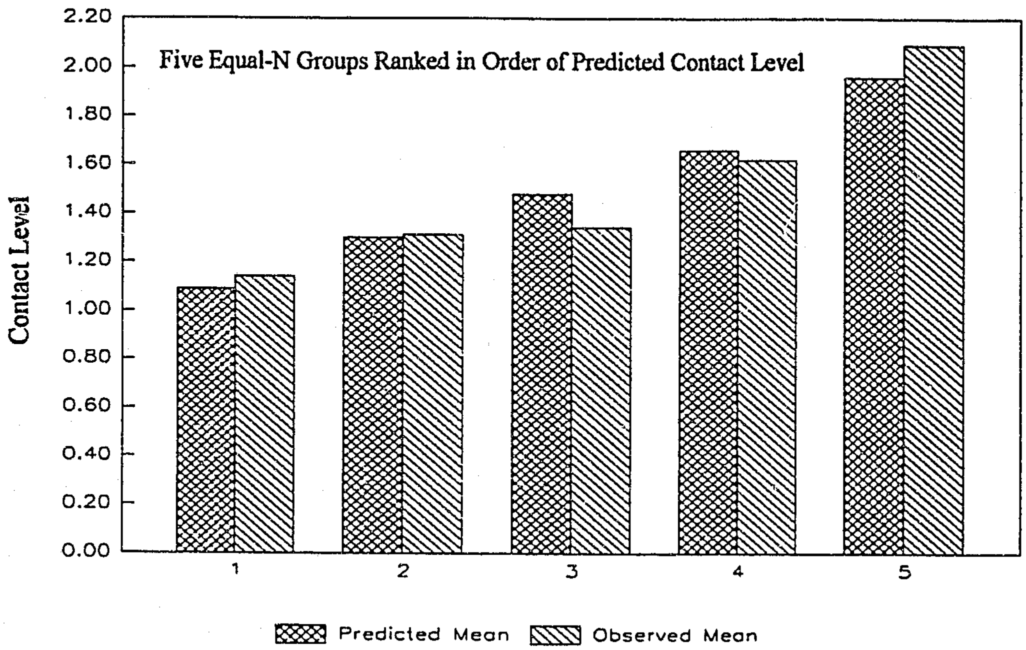
Variable	ß	s.e.{ß}	Sig.
Intercept	-0.469	0.185	p<.012
Sample=Shock	0.132	0.034	p<.001
Sample=Dropout	0.055	0.038	p<.148
Nonwhite	-0.109	0.032	p<.001
Age @ Com. Supv.	0.040	0.008	p<.001
Violent Offense	0.084	0.036	p<.019
Drug Offense	0.062	0.042	p<.144
New Crime	0.149	0.040	p<.002
Cohort=Missing	-0.196	0.070	p<.006
Cohort 1	-0.189	0.041	p<.001
Cohort 2	-0.189	0.040	p<.001
Cohort 3	-0.125	0.041	p<.003

Mean Positive Adjustment (Valid N=273)

$R^2 = .353$

Note Shock and Dropout offenders are compared to Prison Parolees. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements).

Figure 69
Validation of Cross-Sectional Models in Florida



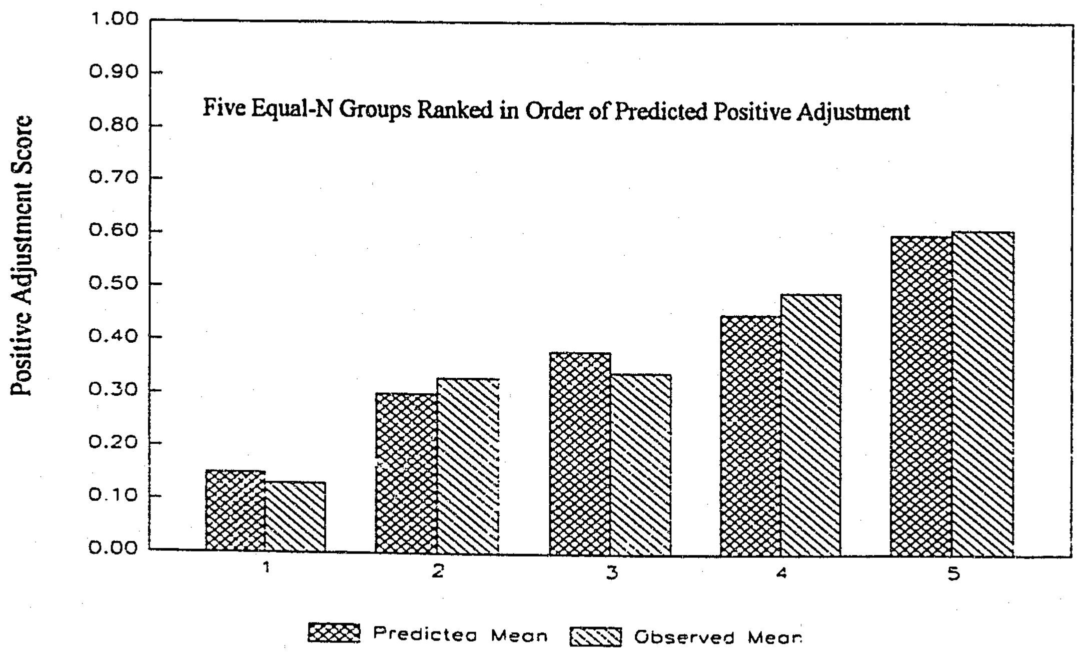


Figure 70
Florida Four-Wave Recursive Panel Model

Wave 1 Analysis (N = 266)

Variable	B	s.e.(B)	Sig.
Intercept	2.846	0.776	p<.001
Sample=Shock	-0.300	0.141	p<.034
Sample=Dropout	-0.149	0.165	p<.368
Nonwhite	-0.062	0.137	p<.650
Age @ Com. Supv.	-0.058	0.035	p<.099
Violent Offense	0.469	0.154	p<.003
Drug Offense	-0.037	0.186	p<.842
New Crime	-0.022	0.174	p<.901
Cohort 1	-0.075	0.178	p<.673
Cohort 2	-0.074	0.174	p<.672
Cohort 3	-0.237	0.175	p<.178

Wave 2 Analysis (N = 199)

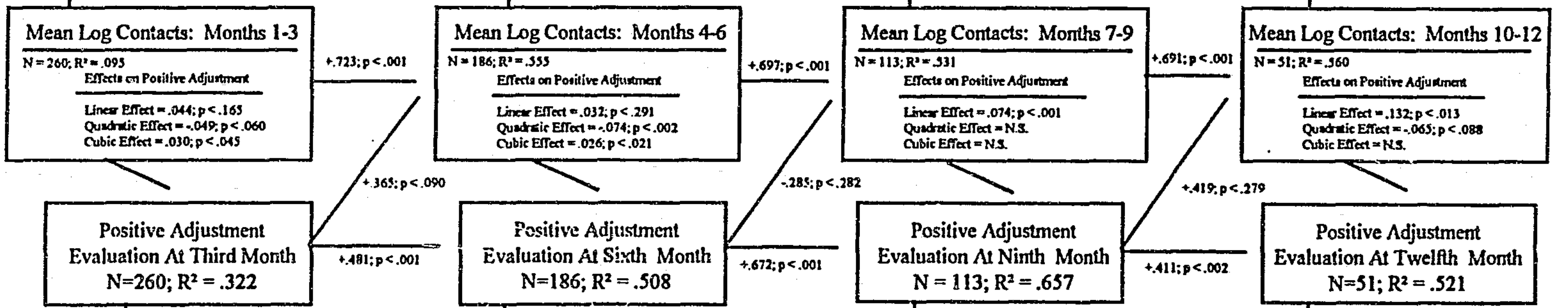
Variable	B	s.e.(B)	Sig.
Intercept	-1.166	0.680	p<.088
Sample=Shock	-0.130	0.123	p<.293
Sample=Dropout	0.113	0.151	p<.458
Nonwhite	0.031	0.120	p<.793
Age @ Com. Supv.	0.061	0.030	p<.045
Violent Offense	-0.104	0.135	p<.443
Drug Offense	-0.094	0.164	p<.565
New Crime	0.126	0.164	p<.443
Cohort 1	—	—	—
Cohort 2	[illegible]	0.137	p<.841
Cohort 3	0.055	0.130	p<.670

Wave 3 Analysis (N = 123)

Variable	B	s.e.(B)	Sig.
Intercept	1.166	0.872	p<.184
Sample=Shock	-0.172	0.167	p<.306
Sample=Dropout	-0.202	0.231	p<.384
Nonwhite	-0.215	0.172	p<.213
Age @ Com. Supv.	-0.024	0.040	p<.540
Violent Offense	0.153	0.182	p<.403
Drug Offense	0.218	0.218	p<.318
New Crime	0.120	0.237	p<.615
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	-0.416	0.149	p<.006

Wave 4 Analysis (N = 58)

Variable	B	s.e.(B)	Sig.
Intercept	0.933	1.288	p<.473
Sample=Shock	-0.376	0.266	p<.165
Sample=Dropout	-0.175	0.369	p<.637
Nonwhite	-0.144	0.238	p<.548
Age @ Com. Supv.	-0.023	0.058	p<.693
Violent Offense	-0.059	0.273	p<.829
Drug Offense	0.032	0.308	p<.918
New Crime	-0.106	0.333	p<.753
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	—	—	—



Variable	B	s.e.(B)	Sig.
Intercept	-0.224	0.206	p<.278
Sample=Shock	0.105	0.038	p<.006
Sample=Dropout	0.017	0.042	p<.697
Nonwhite	-0.115	0.035	p<.001
Age @ Com. Supv.	0.030	0.009	p<.001
Violent Offense	0.106	0.040	p<.009
Drug Offense	0.052	0.048	p<.274
New Crime	0.128	0.045	p<.005
Cohort 1	-0.197	0.046	p<.001
Cohort 2	-0.192	0.045	p<.001
Cohort 3	-0.078	0.045	p<.083

Variable	B	s.e.(B)	Sig.
Intercept	-0.187	0.224	p<.404
Sample=Shock	0.059	0.041	p<.149
Sample=Dropout	0.044	0.049	p<.374
Nonwhite	-0.061	0.039	p<.121
Age @ Com. Supv.	0.019	0.010	p<.056
Violent Offense	-0.000	0.045	p<.997
Drug Offense	0.108	0.054	p<.045
New Crime	0.104	0.054	p<.054
Cohort 1	—	—	—
Cohort 2	-0.128	0.045	p<.005
Cohort 3	-0.092	0.043	p<.031

Variable	B	s.e.(B)	Sig.
Intercept	-0.437	0.247	p<.080
Sample=Shock	0.105	0.047	p<.027
Sample=Dropout	0.113	0.065	p<.085
Nonwhite	-0.036	0.048	p<.457
Age @ Com. Supv.	0.023	0.011	p<.038
Violent Offense	0.083	0.051	p<.107
Drug Offense	0.043	0.061	p<.480
New Crime	-0.059	0.067	p<.381
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	-0.122	0.043	p<.005

Variable	B	s.e.(B)	Sig.
Intercept	-0.249	0.413	p<.551
Sample=Shock	-0.074	0.087	p<.401
Sample=Dropout	0.062	0.118	p<.602
Nonwhite	-0.077	0.076	p<.314
Age @ Com. Supv.	0.023	0.019	p<.229
Violent Offense	-0.041	0.087	p<.644
Drug Offense	-0.021	0.099	p<.833
New Crime	0.031	0.106	p<.773
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	—	—	—

Note: Shock and Dropout offenders are compared to Prison Parolees. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Dummy cohort effects adjust intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Analysis sample sizes do not equal total wave sample sizes because of missing data on some fixed effects.

Figure 71
Validation of Wave 1 Models in Florida

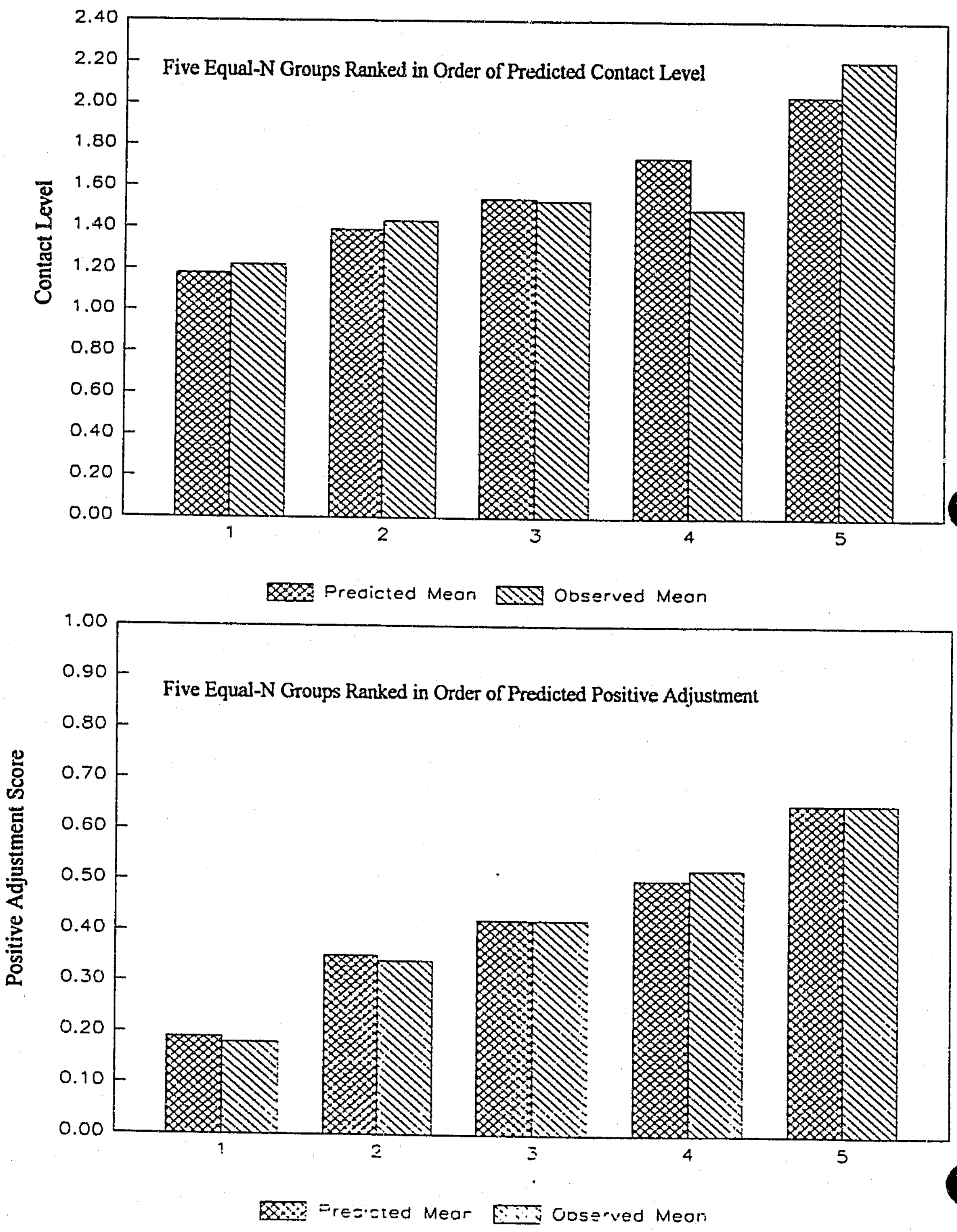
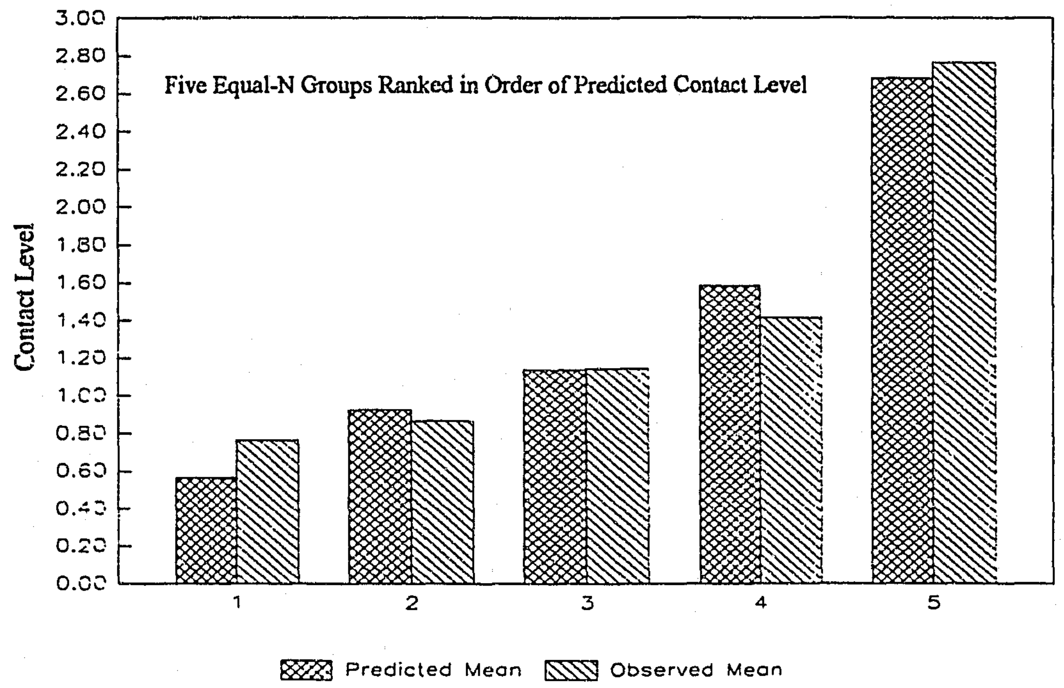


Figure 72
Validation of Wave 2 Models in Florida



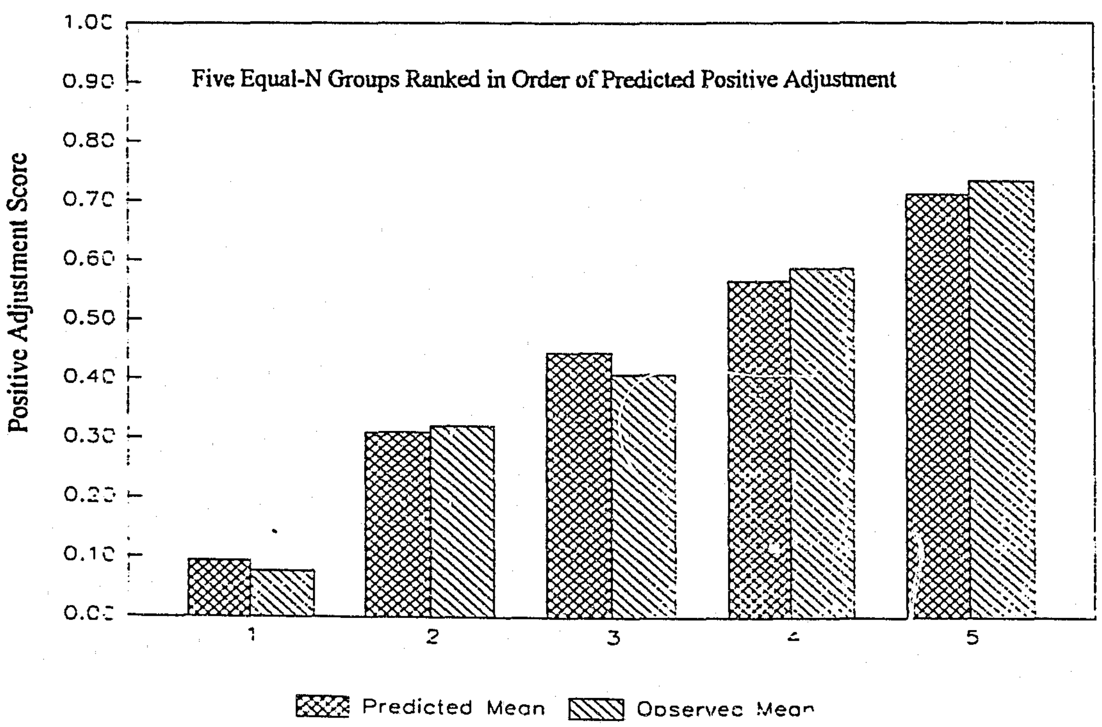
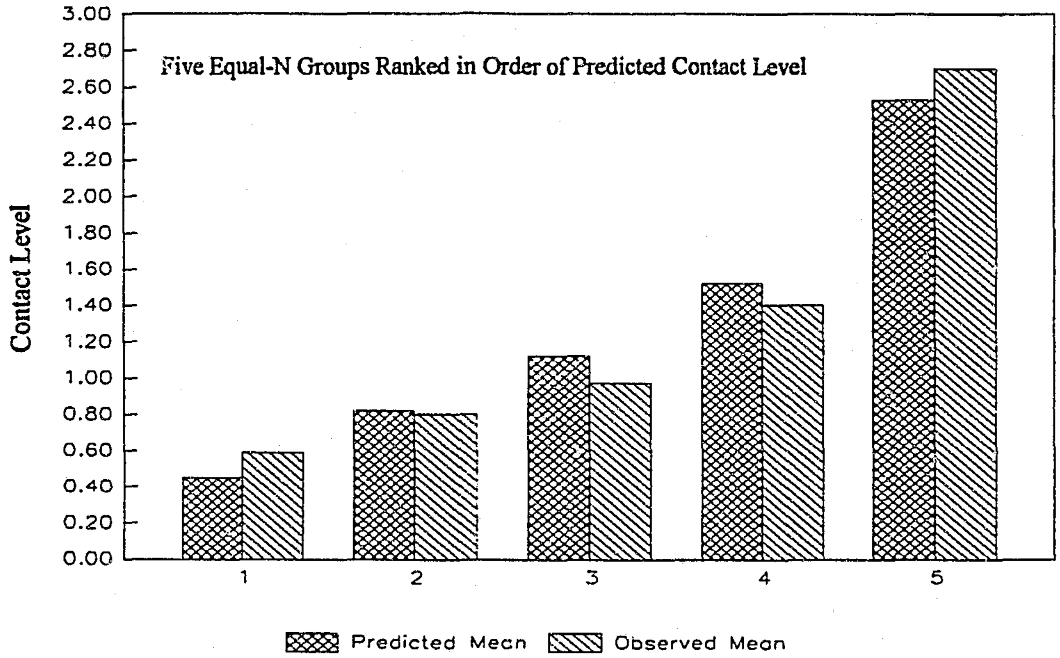


Figure 73
Validation of Wave 3 Models in Florida



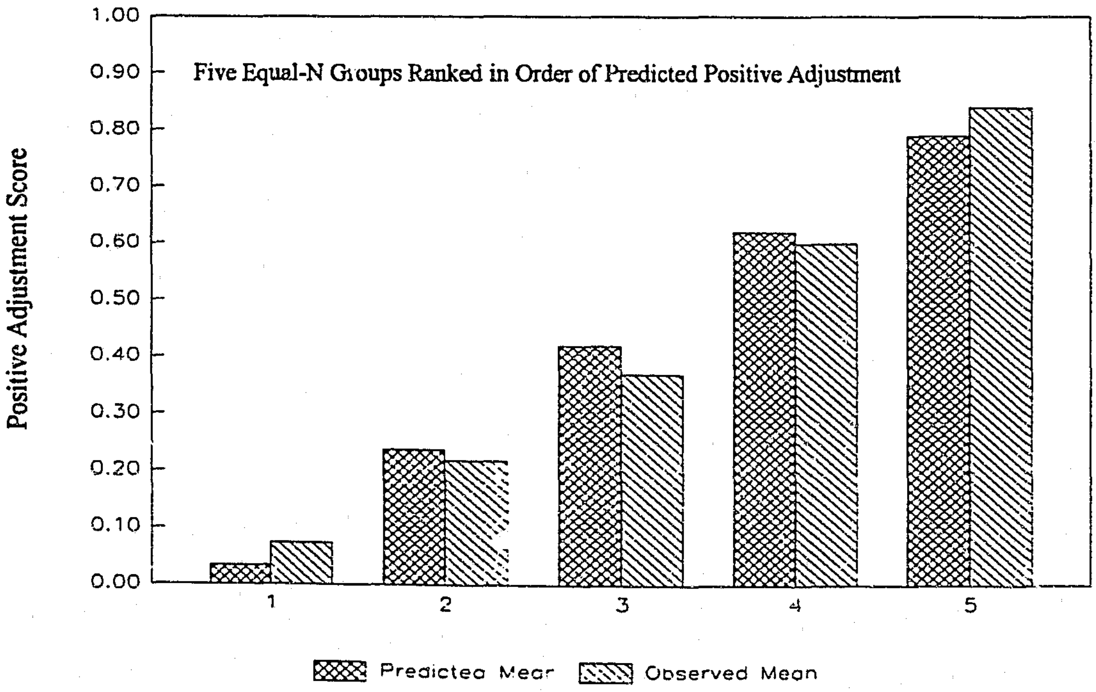


Figure 74
Validation of Wave 4 Models in Florida

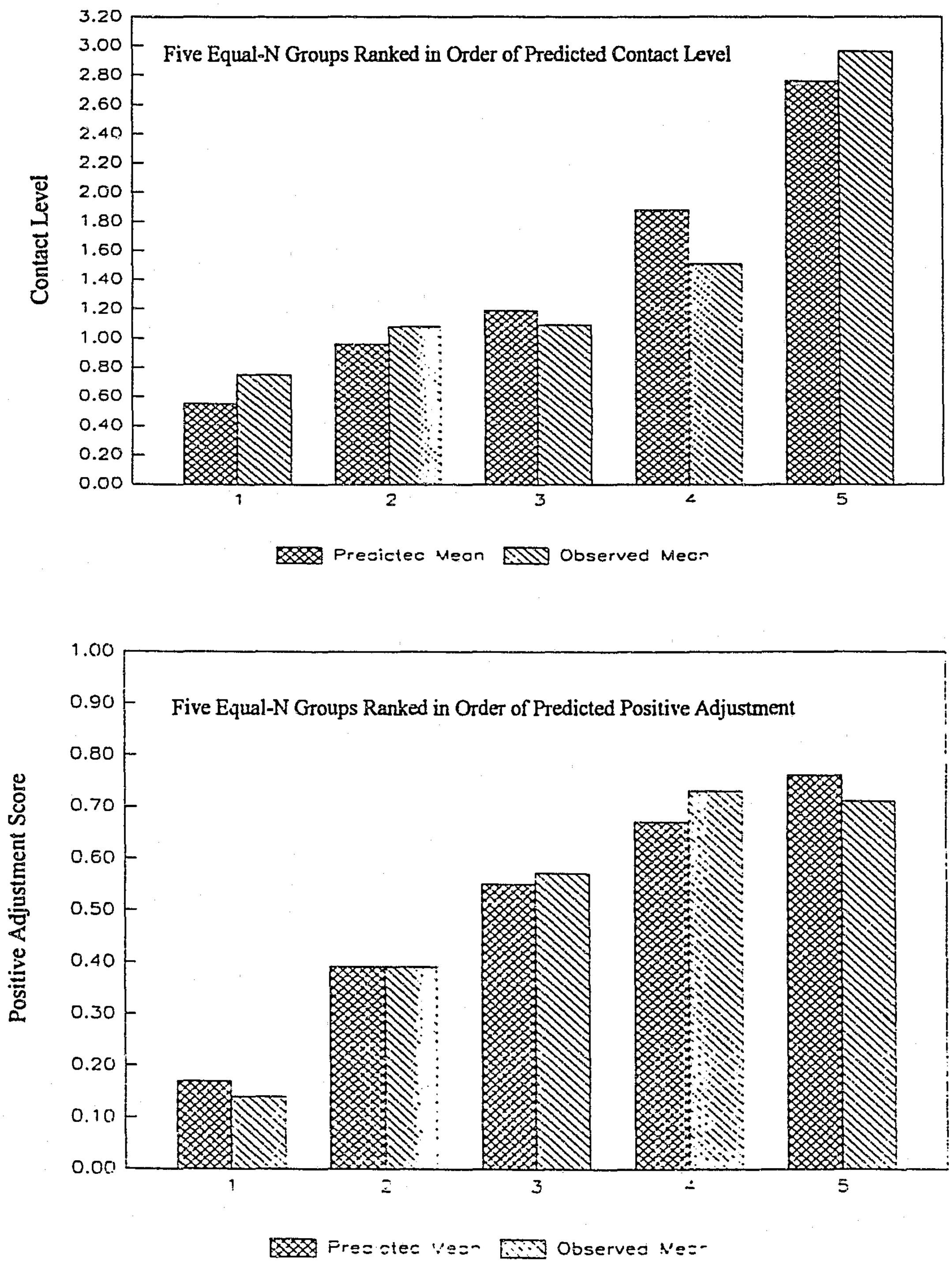
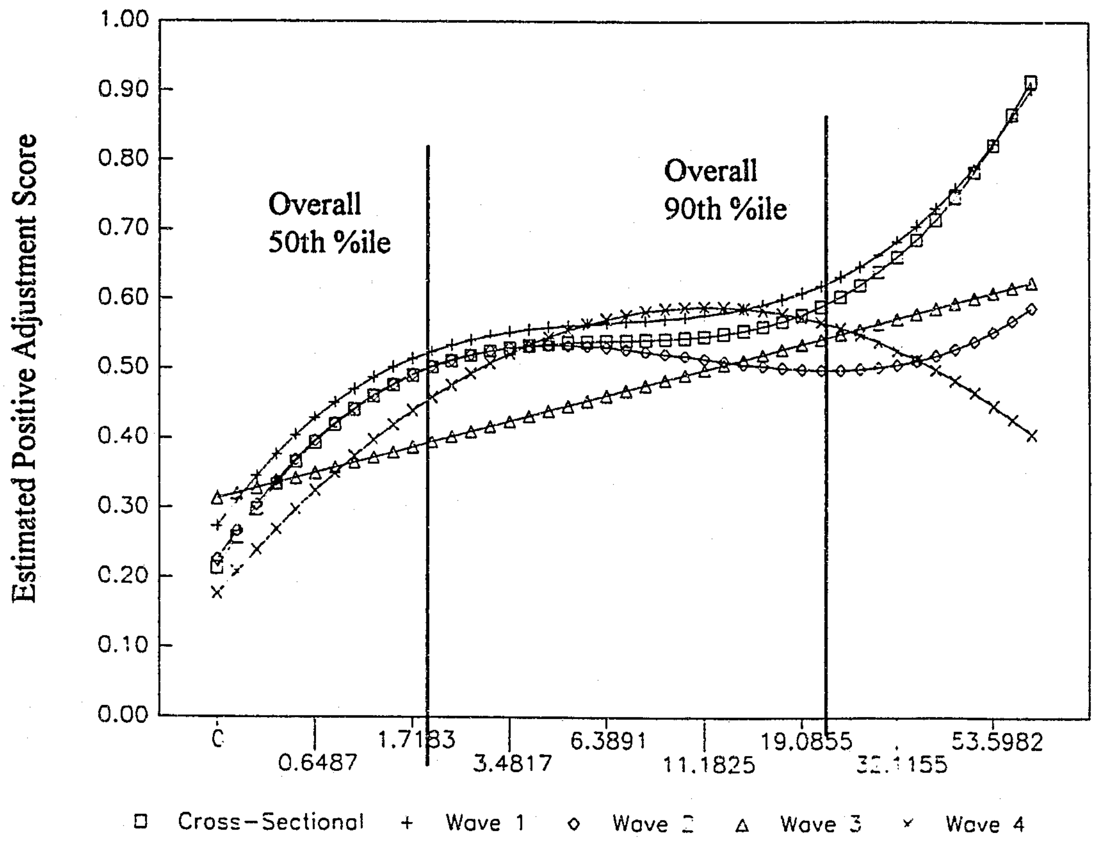


Figure 75
Estimated Effects of Primary Contacts For A Hypothetical Observation in Exit Cohort #4 in Florida



Note: Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates. For this presentation, all predictor variables were constrained to their time-period specific means.

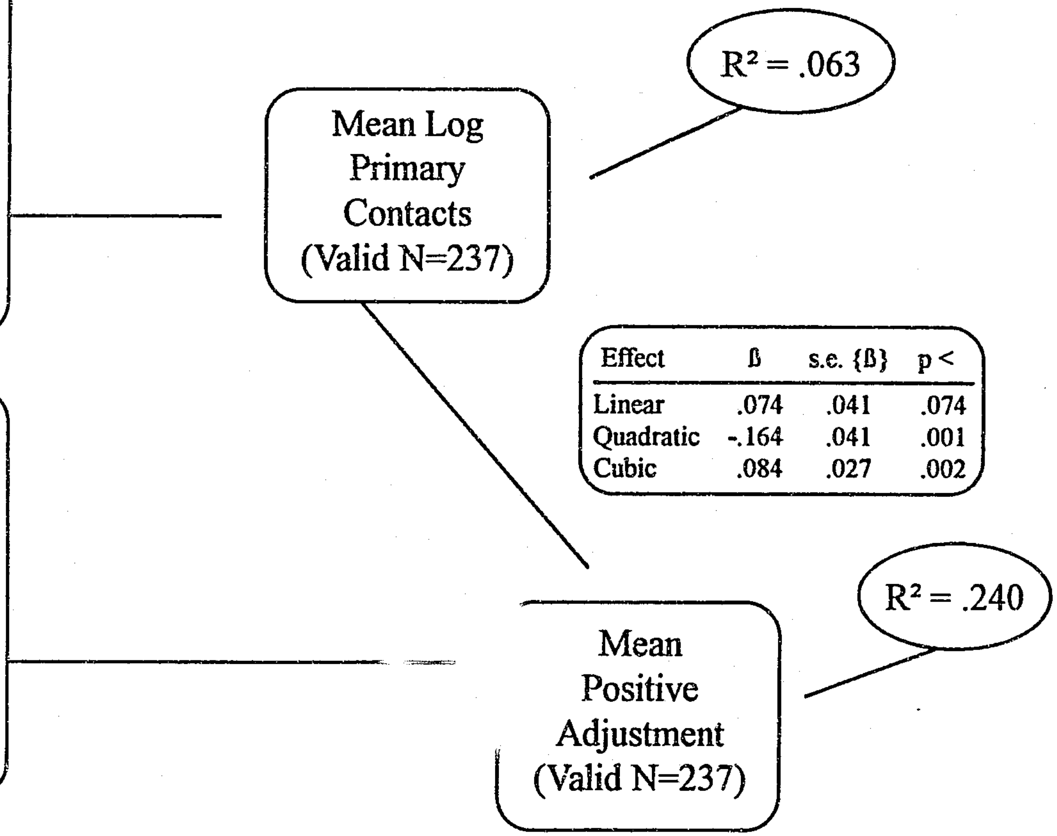
Figure 76
Georgia Cross-Sectional Positive Adjustment Model

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	1.193	0.353	p < .001
Sample=Shock	0.140	0.099	p < .160
Sample=Prison	0.380	0.117	p < .001
Nonwhite	-0.085	0.080	p < .290
Age @ Com. Supv.	-0.010	0.015	p < .501
Violent Offense	0.050	0.109	p < .645
Drug Offense	-0.011	0.092	p < .909
New Crime	0.048	0.100	p < .635
Prior Record	-0.095	0.087	p < .276
Cohort=Missing	-0.096	0.125	p < .442
Cohort 1	-0.048	0.118	p < .686
Cohort 2	-0.022	0.112	p < .844
Cohort 3	-0.002	0.101	p < .982

Vector of Fixed Effects

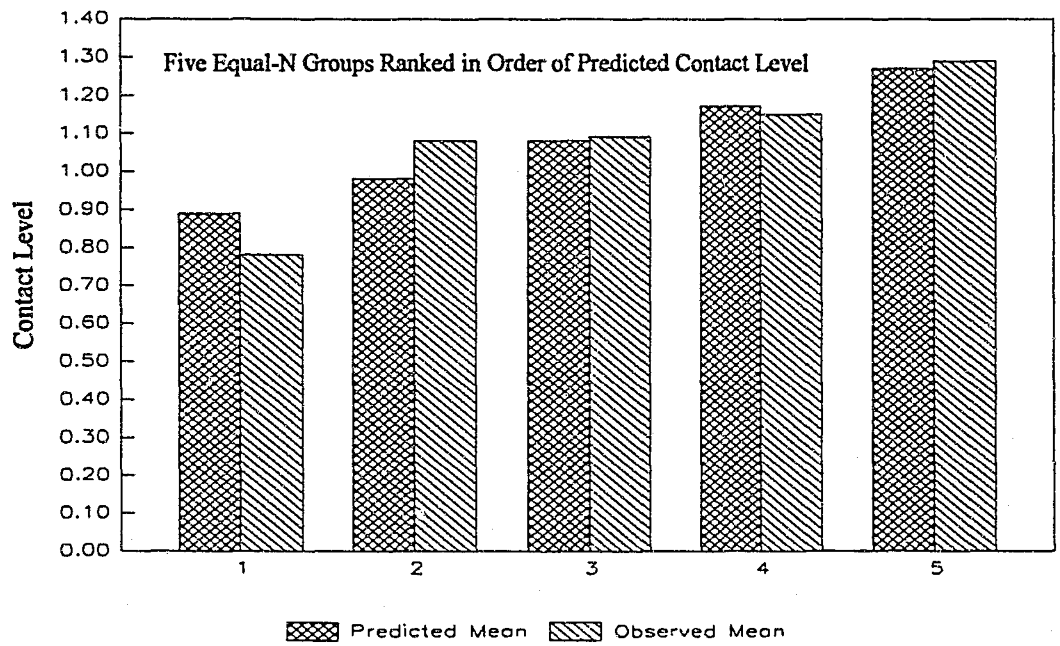
Variable	ß	s.e.{ß}	Sig.
Intercept	0.402	0.143	p < .005
Sample=Shock	-0.010	0.040	p < .807
Sample=Prison	-0.008	0.049	p < .873
Nonwhite	-0.109	0.031	p < .001
Age @ Com. Supv.	0.002	0.006	p < .785
Violent Offense	0.079	0.043	p < .063
Drug Offense	0.115	0.036	p < .002
New Crime	-0.009	0.039	p < .818
Prior Record	-0.090	0.034	p < .009
Cohort=Missing	0.033	0.049	p < .503
Cohort 1	-0.010	0.046	p < .838
Cohort 2	-0.002	0.044	p < .959
Cohort 3	0.027	0.039	p < .495



Effect	ß	s.e. {ß}	p <
Linear	.074	.041	.074
Quadratic	-.164	.041	.001
Cubic	.084	.027	.002

Note: Shock and Prison Parolee offenders are compared to Probationers. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements).

Figure 77
Validation of Cross-Sectional Models in Georgia



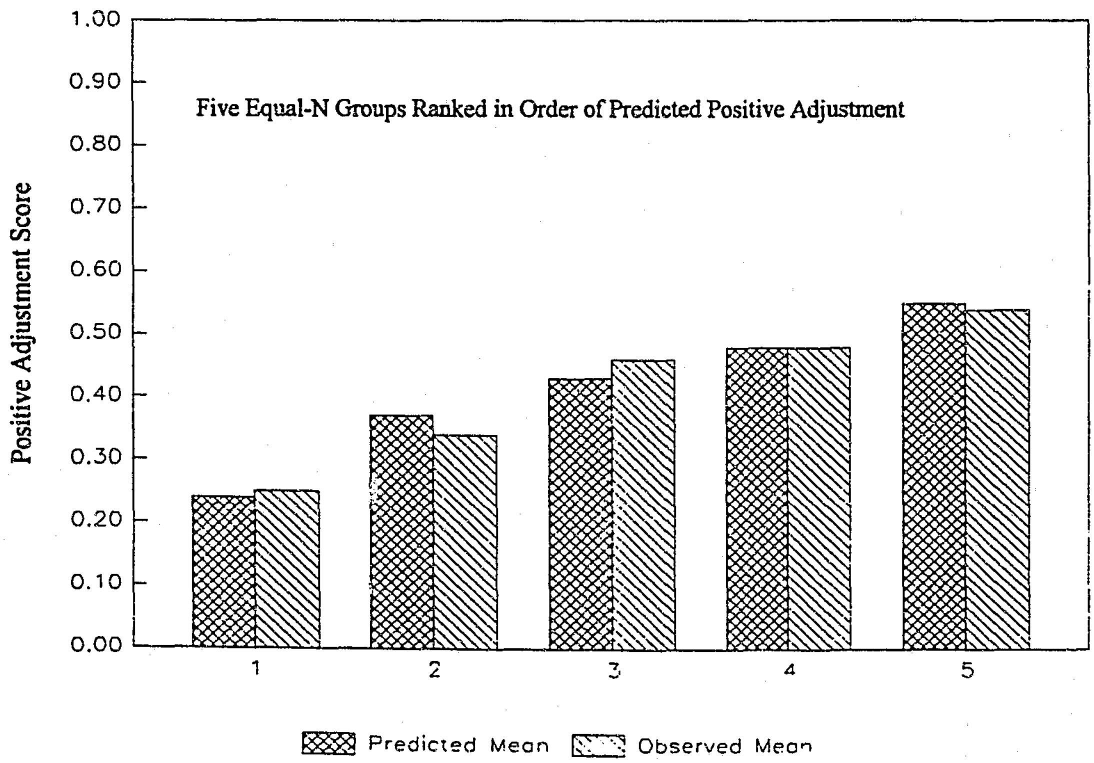


Figure 78
Georgia Four-Wave Recursive Panel Model

Wave 1 Analysis (N = 215)

Variable	ß	s.e.(ß)	Sig.
Intercept	1.589	0.411	p < .001
Sample=Shock	0.161	0.114	p < .160
Sample=Prison	0.614	0.139	p < .001
Nonwhite	-0.076	0.097	p < .431
Age @ Com. Supv.	-0.024	0.017	p < .169
Violent Offense	0.236	0.128	p < .066
Drug Offense	0.021	0.110	p < .846
New Crime	0.053	0.121	p < .664
Prior Record	-0.153	0.104	p < .143
Cohort 1	-0.170	0.130	p < .193
Cohort 2	-0.157	0.123	p < .205
Cohort 3	-0.180	0.111	p < .106

Mean Log Contacts: Months 1-3
N = 207; R^2 = .153
Effects on Positive Adjustment
Linear Effect = .025; p < .719
Quadratic Effect = -0.083; p < .105
Cubic Effect = 0.060; p < .110

Positive Adjustment Evaluation At Third Month
N=207; R^2 = .077

Variable	ß	s.e.(ß)	Sig.
Intercept	0.528	0.218	p < .016
Sample=Shock	0.037	0.058	p < .526
Sample=Prison	0.032	0.079	p < .685
Nonwhite	-0.001	0.047	p < .990
Age @ Com. Supv.	-0.004	0.009	p < .602
Violent Offense	0.004	0.063	p < .955
Drug Offense	0.062	0.054	p < .252
New Crime	-0.002	0.059	p < .976
Prior Record	-0.083	0.051	p < .106
Cohort 1	-0.017	0.065	p < .792
Cohort 2	0.002	0.061	p < .972
Cohort 3	0.014	0.055	p < .795

+.452; p < .001
+.204; p < .204
+.148; p < .037

Wave 2 Analysis (N = 174)

Variable	ß	s.e.(ß)	Sig.
Intercept	0.453	0.468	p < .335
Sample=Shock	-0.027	0.132	p < .839
Sample=Prison	0.032	0.163	p < .846
Nonwhite	-0.036	0.105	p < .729
Age @ Com. Supv.	0.001	0.019	p < .954
Violent Offense	0.065	0.140	p < .641
Drug Offense	-0.071	0.120	p < .557
New Crime	-0.114	0.128	p < .377
Prior Record	-0.068	0.115	p < .554
Cohort 1	—	—	—
Cohort 2	-0.032	0.123	p < .795
Cohort 3	0.106	0.112	p < .345

Mean Log Contacts: Months 4-6
N = 171; R^2 = .236
Effects on Positive Adjustment
Linear Effect = .043; p < .468
Quadratic Effect = -.282; p < .002
Cubic Effect = .140; p < .001

Positive Adjustment Evaluation At Sixth Month
N=171; R^2 = .365

Variable	ß	s.e.(ß)	Sig.
Intercept	0.300	0.203	p < .142
Sample=Shock	-0.041	0.058	p < .478
Sample=Prison	-0.026	0.071	p < .716
Nonwhite	-0.161	0.046	p < .001
Age @ Com. Supv.	0.008	0.008	p < .349
Violent Offense	0.094	0.061	p < .136
Drug Offense	0.130	0.053	p < .016
New Crime	0.016	0.057	p < .778
Prior Record	-0.035	0.051	p < .496
Cohort 1	—	—	—
Cohort 2	-0.052	0.055	p < .342
Cohort 3	0.016	0.050	p < .746

+.395; p < .001
+.181; p < .284
+.536; p < .001

Wave 3 Analysis (N = 128)

Variable	ß	s.e.(ß)	Sig.
Intercept	-0.183	0.466	p < .695
Sample=Shock	0.044	0.132	p < .742
Sample=Prison	-0.037	0.161	p < .819
Nonwhite	-0.021	0.110	p < .851
Age @ Com. Supv.	0.029	0.020	p < .146
Violent Offense	-0.183	0.132	p < .166
Drug Offense	0.125	0.130	p < .339
New Crime	-0.050	0.127	p < .694
Prior Record	0.170	0.117	p < .149
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	-0.072	0.100	p < .474

Mean Log Contacts: Months 7-9
N = 118; R^2 = .291
Effects on Positive Adjustment
Linear Effect = -.016; p < .821
Quadratic Effect = -.114; p < .079
Cubic Effect = .095; p < .052

Positive Adjustment Evaluation At Ninth Month
N = 118; R^2 = .509

Variable	ß	s.e.(ß)	Sig.
Intercept	0.412	0.224	p < .069
Sample=Shock	-0.017	0.062	p < .791
Sample=Prison	-0.007	0.074	p < .930
Nonwhite	-0.089	0.052	p < .089
Age @ Com. Supv.	-0.003	0.009	p < .725
Violent Offense	0.160	0.061	p < .010
Drug Offense	0.116	0.061	p < .071
New Crime	-0.090	0.060	p < .137
Prior Record	-0.048	0.055	p < .388
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	-0.059	0.046	p < .201

+.598; p < .001
+.060; p < .736
+.460; p < .001

Wave 4 Analysis (N = 63)

Variable	ß	s.e.(ß)	Sig.
Intercept	0.125	0.447	p < .780
Sample=Shock	0.168	0.130	p < .204
Sample=Prison	-0.012	0.156	p < .941
Nonwhite	-0.125	0.112	p < .270
Age @ Com. Supv.	0.005	0.020	p < .811
Violent Offense	-0.074	0.120	p < .540
Drug Offense	0.251	0.133	p < .066
New Crime	-0.164	0.113	p < .154
Prior Record	0.254	0.101	p < .016
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	—	—	—

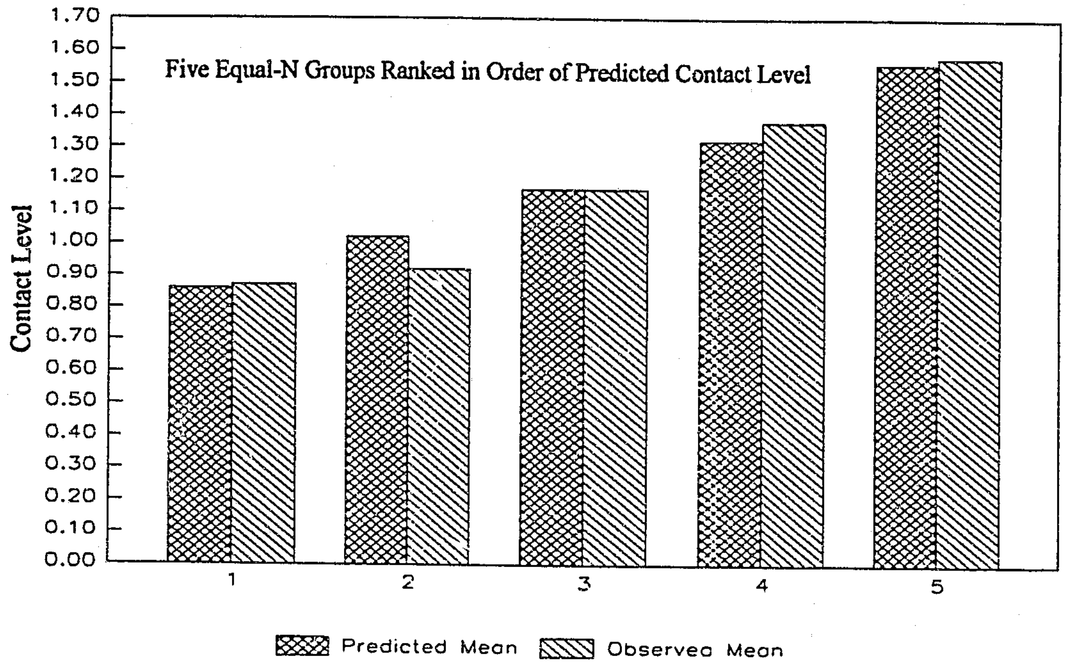
Mean Log Contacts: Months 10-12
N = 56; R^2 = .657
Effects on Positive Adjustment
Linear Effect = .024; p < .767
Quadratic Effect = -.223; p < .023
Cubic Effect = N.S.

Positive Adjustment Evaluation At Twelfth Month
N=56; R^2 = .565

Variable	ß	s.e.(ß)	Sig.
Intercept	-0.255	0.326	p < .439
Sample=Shock	0.093	0.096	p < .341
Sample=Prison	-0.008	0.113	p < .942
Nonwhite	-0.067	0.082	p < .413
Age @ Com. Supv.	0.026	0.014	p < .080
Violent Offense	0.033	0.087	p < .707
Drug Offense	0.071	0.100	p < .482
New Crime	-0.078	0.085	p < .367
Prior Record	-0.083	0.077	p < .287
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	—	—	—

Note: Shock and Prison Parolee offenders are compared to Probationers. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Analysis sample sizes do not equal total wave sample sizes because of missing data on some fixed effects.

Figure 79
Validation of Wave 1 Models in Georgia



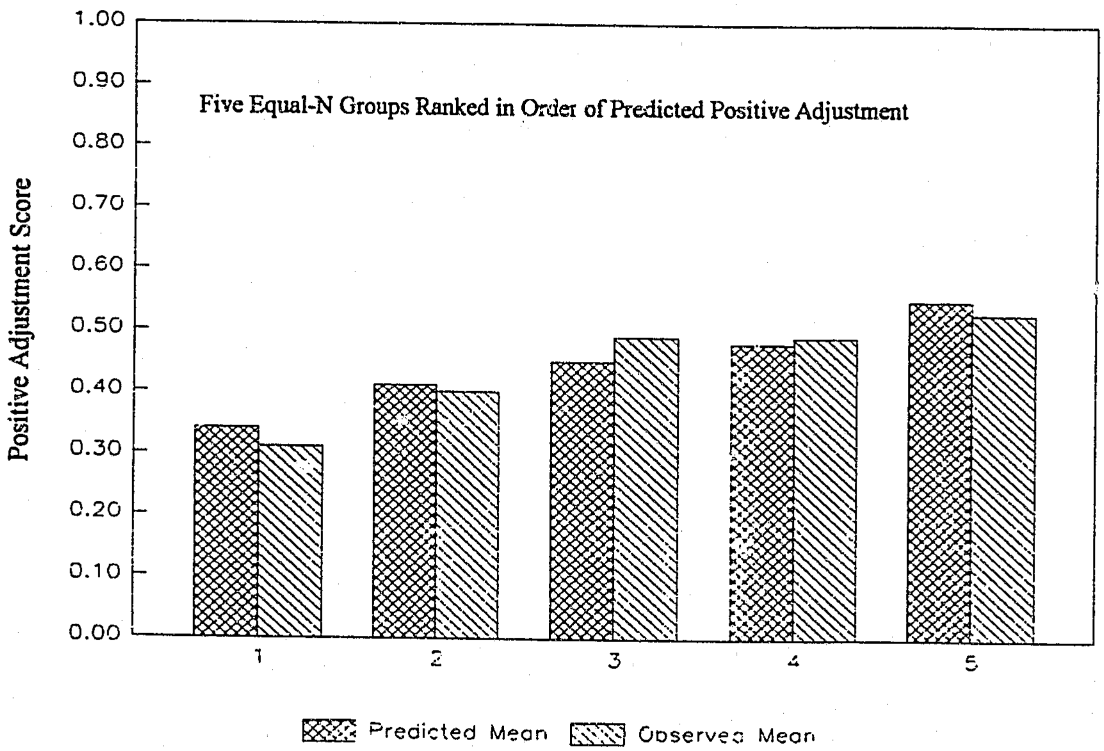
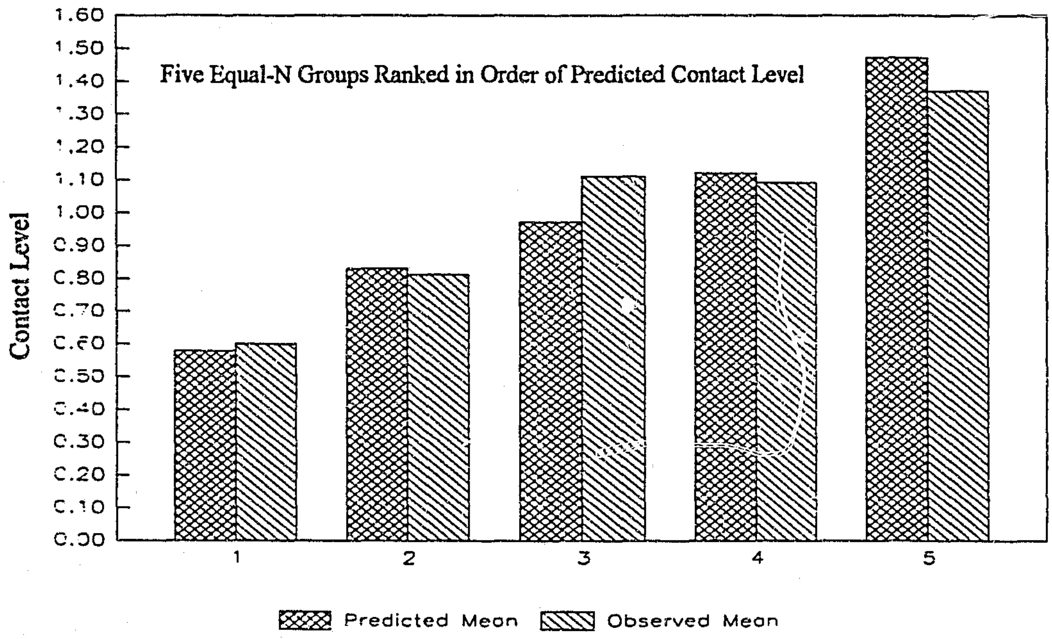


Figure 80
Validation of Wave 2 Models in Georgia



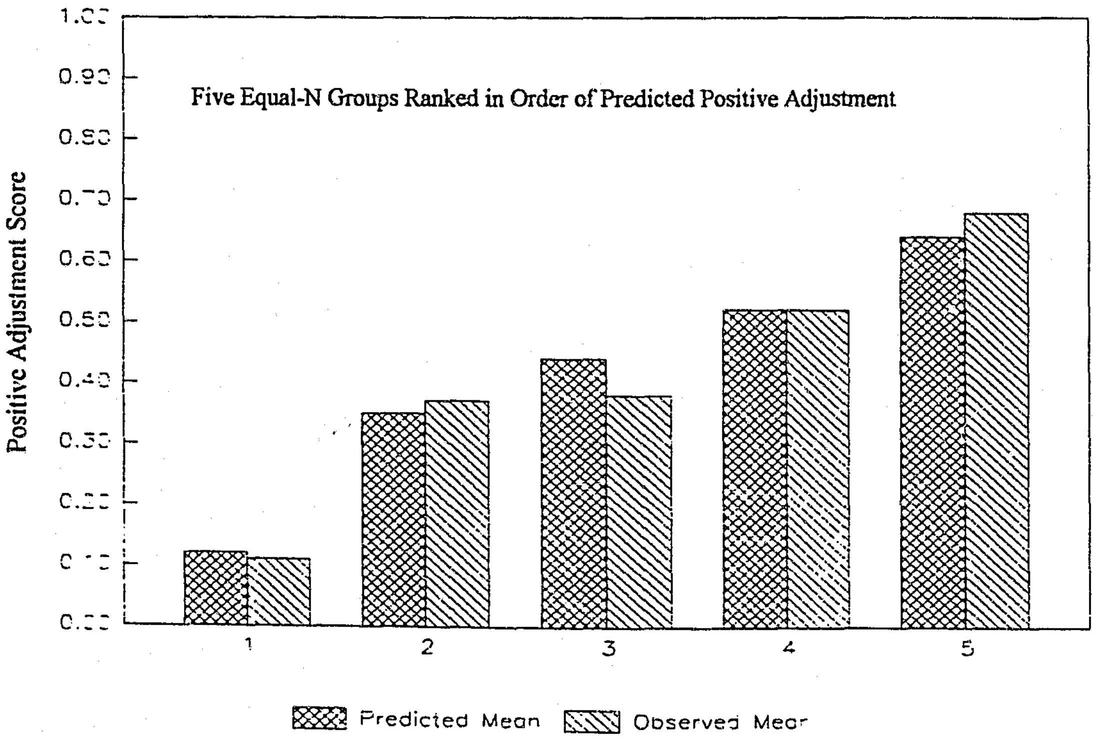
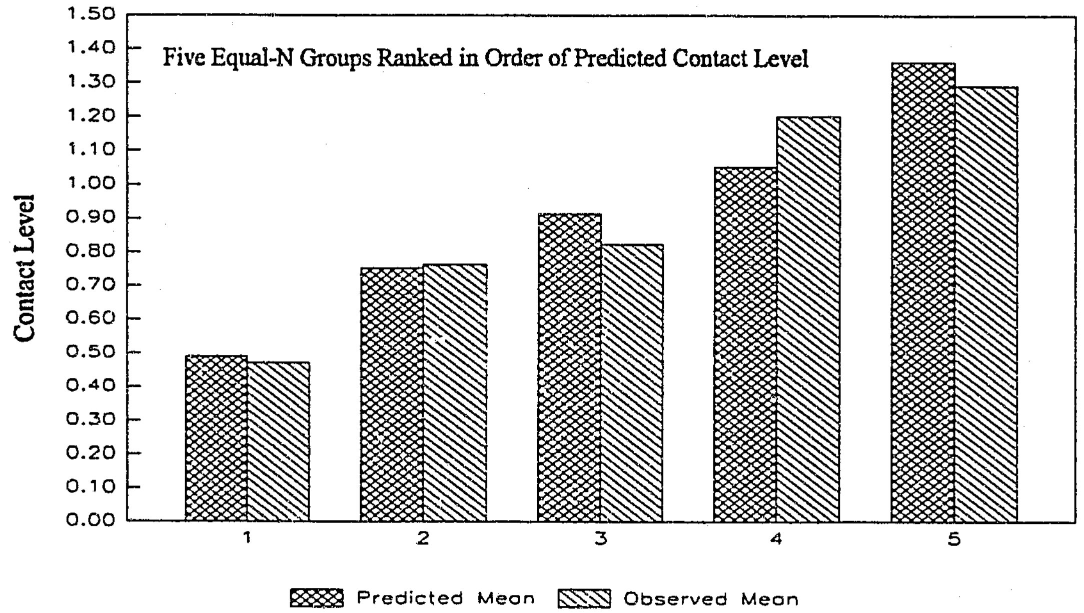


Figure 81
Validation of Wave 3 Models in Georgia



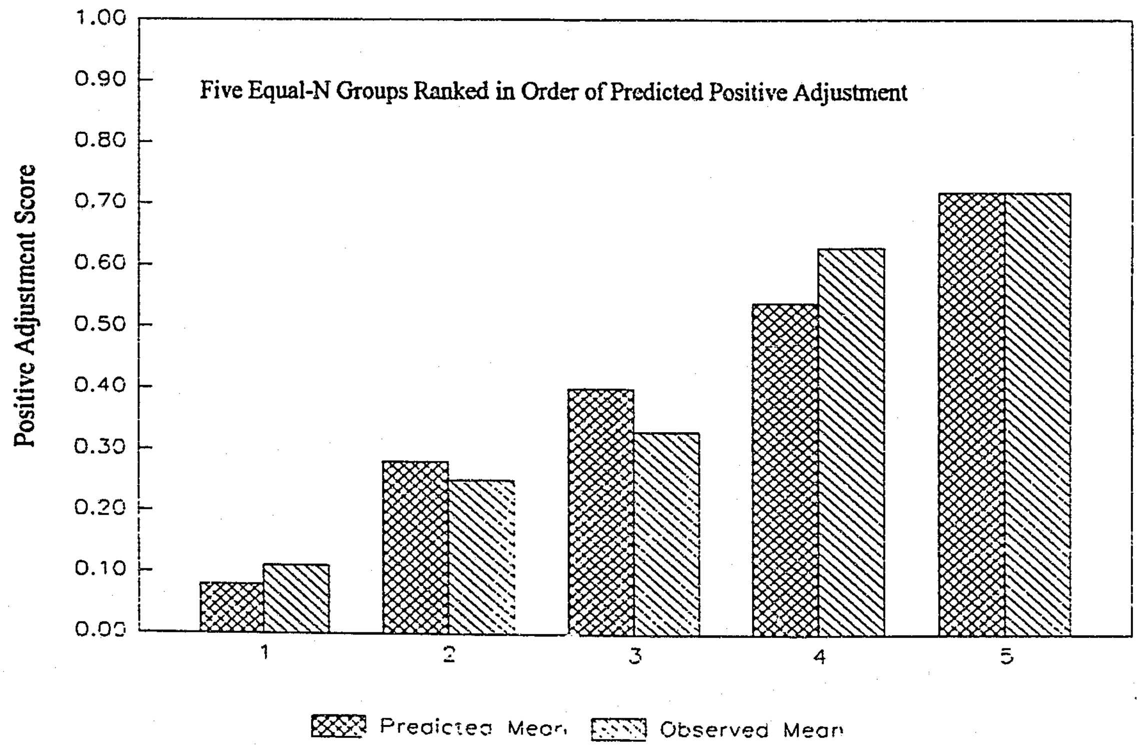
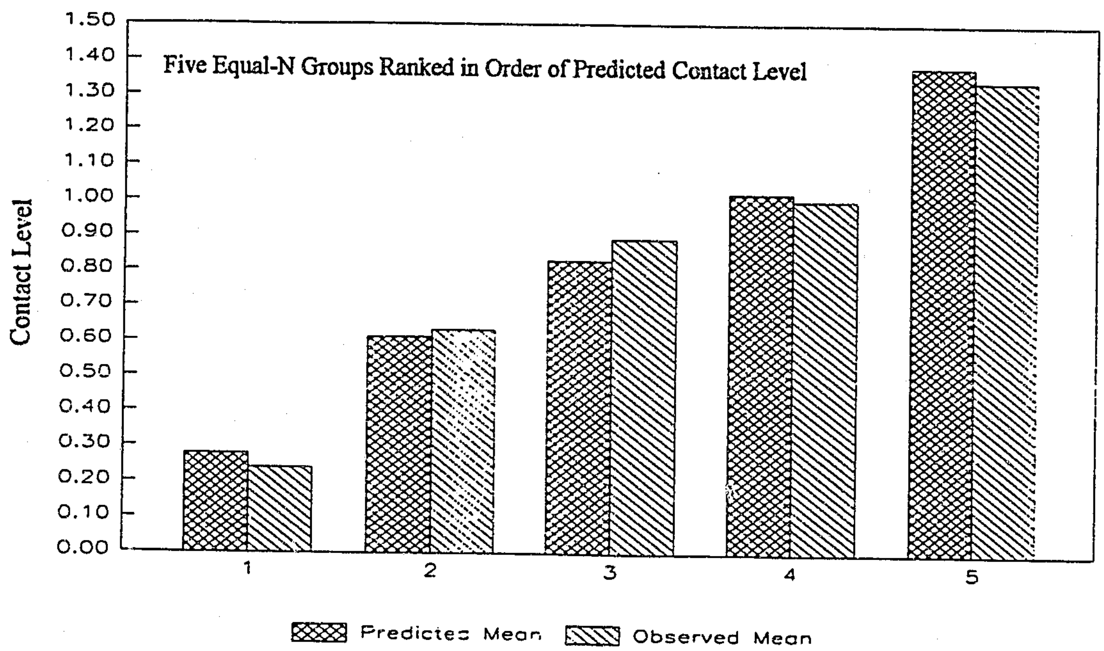


Figure 82
Validation of Wave 4 Models in Georgia



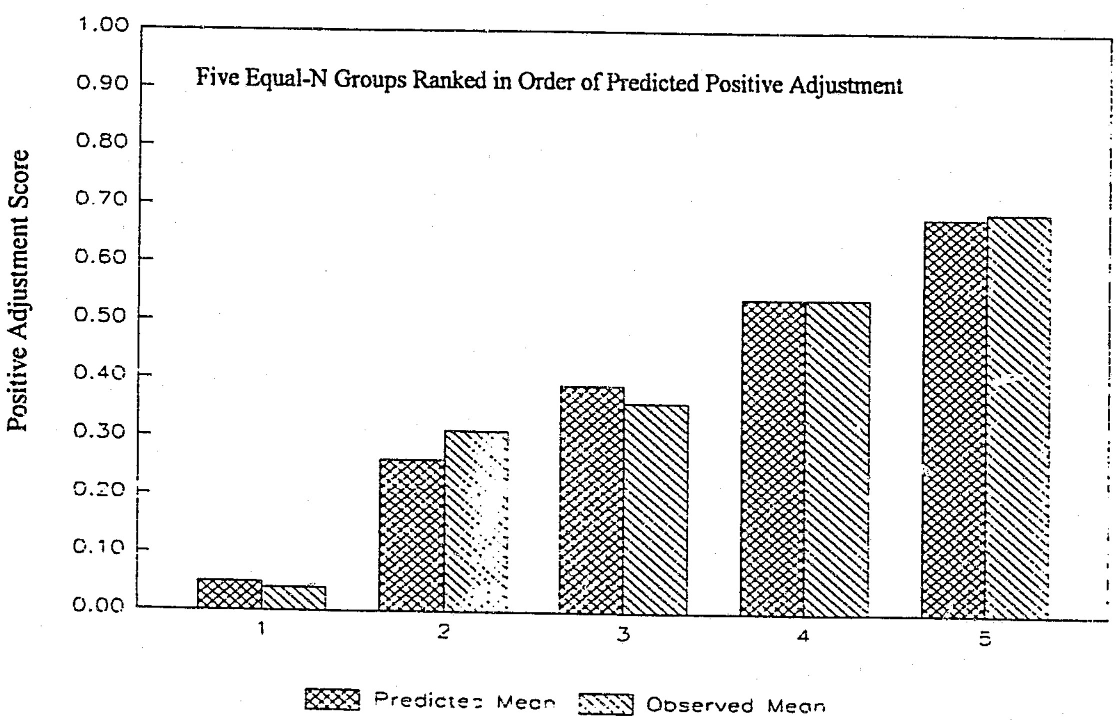
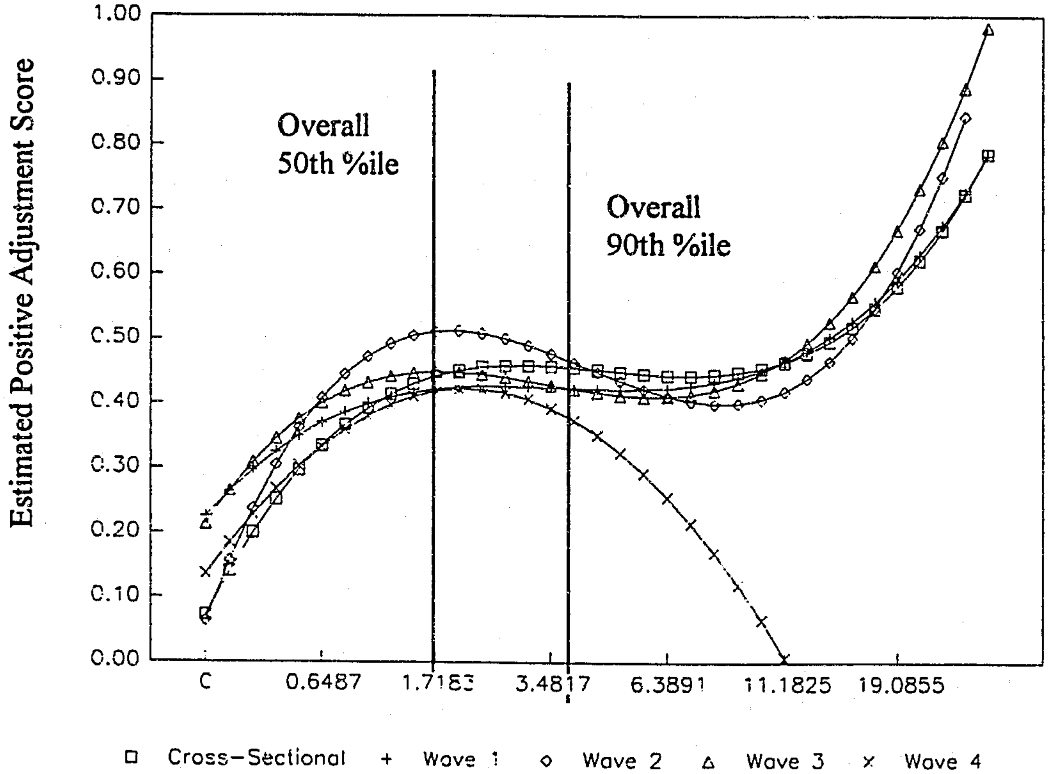


Figure 83
Estimated Effects of Primary Contacts For A Hypothetical Observation in Exit Cohort #4 in Georgia



Note: Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates. For this presentation, all predictor variables were constrained to their time-period specific means.

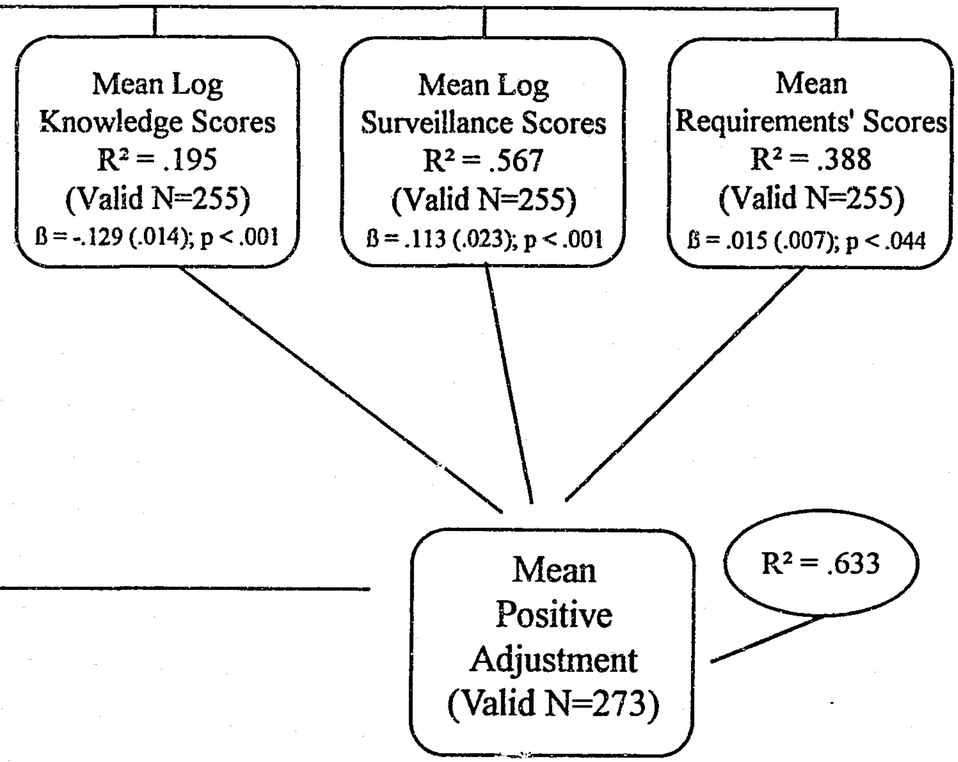
Figure 84
Louisiana Cross-Sectional Positive Adjustment Model

Vector of Fixed Effects

Variable	ß (Know.)	ß (Surv.)	ß (Req.)
Intercept	0.945 ***	0.341 ***	2.499 ***
Sample=Shock	-0.492 ***	0.739 ***	1.418 ***
Sample=Prison	-0.237 ***	0.093 *	-0.076
Sample=Dropout	-0.211 *	0.065	0.286
Nonwhite	0.107 *	-0.054	-0.233 *
Age @ Com. Supv.	-0.004	-0.003	-0.007
New Crime	-0.067	-0.011	0.190
Prior Record	0.021	0.004	0.181
Cohort=Missing	0.403	-0.278	-0.807
Cohort 1	0.035	-0.080	-0.365
Cohort 2	0.072	-0.018	-0.134
Cohort 3	0.075	-0.018	-0.248

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	0.433	0.039	p < .001
Sample=Shock	-0.002	0.022	p < .920
Sample=Prison	0.022	0.016	p < .165
Sample=Dropout	0.026	0.026	p < .325
Nonwhite	-0.046	0.012	p < .001
Age @ Com. Supv.	0.003	0.001	p < .017
New Crime	0.008	0.014	p < .571
Prior Record	-0.017	0.013	p < .207
Cohort=Missing	0.072	0.066	p < .281
Cohort 1	-0.016	0.024	p < .001
Cohort 2	-0.098	0.017	p < .001
Cohort 3	-0.061	0.017	p < .001

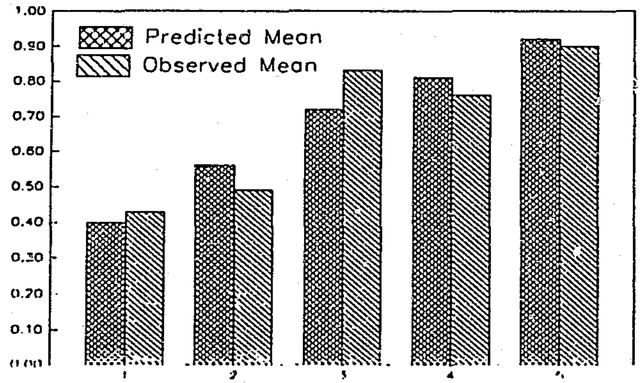


* p < .10
** p < .05
*** p < .01

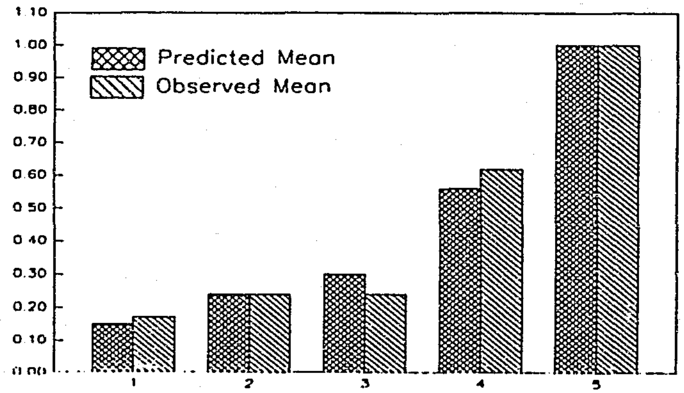
Note: Shock, Prison Parolee, and Dropout offenders are compared to Probationers. Type of offense and age at first arrest are excluded from the analysis because of excessive missing data and lack of predictive power. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions and offenders with a prior arrest and/or conviction record are compared to offenders without a record. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements).

Figure 85
Validation of Cross-Sectional Models in Louisiana

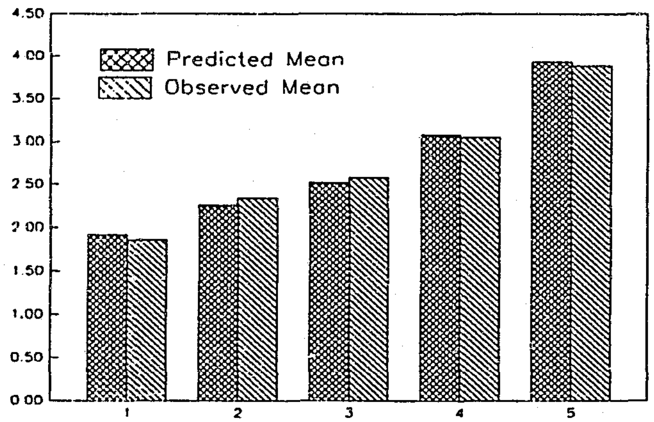
5 Equal-N Groups Ranked on Knowledge Scores



5 Equal-N Groups Ranked on Surveillance Scores



5 Equal-N Groups Ranked on Requirements' Scores



5 Equal-N Groups Ranked on Positive Adjustment Scores

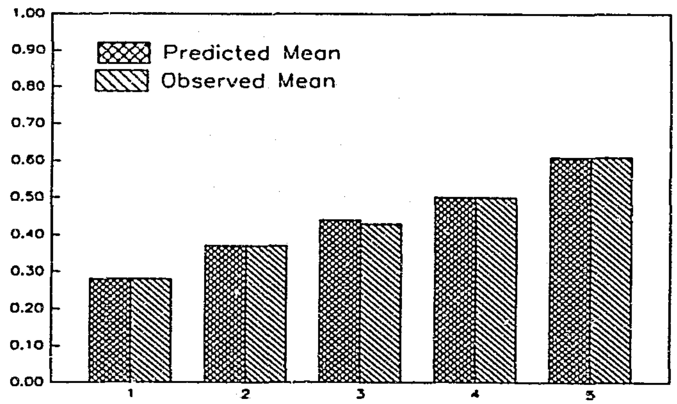


Figure 86
Louisiana Four-Wave Recursive Panel Model

Wave 1 Analysis (N = 276)

Variable	β (K)	β (S)	β (R)	β (PA)
Intercept	0.956***	0.378***	2.317***	0.393***
Sample=Shock	-0.545***	0.929***	1.867***	-0.052**
Sample=Prison	-0.199**	0.143**	0.073	0.028*
Sample=Dropout	-0.065	0.130	0.397	0.030
Nonwhite	0.128**	-0.093**	-0.318**	-0.040***
Age @ Com. Supv.	-0.008	-0.005	0.002	0.002**
New Crime	0.026	-0.029	0.166	0.004
Prior Offenses	-0.014	0.045	0.244*	-0.014
Cohort 1	0.080	-0.158*	-0.465*	-0.163***
Cohort 2	0.128	-0.048	-0.217	-0.047***
Cohort 3	0.031	-0.070	-0.399	-0.020

Log Knowledge
$N = 253; R^2 = .219$
Effect on Positive Adjustment @ T1 = -0.102; p < .001
Effect on Log Knowledge @ T2 = 0.549; p < .001
Effect on Log Surveillance @ T2 = -0.074; p < .118
Effect on Requirements @ T2 = 0.102; p < .181

Log Surveillance
$N = 253; R^2 = .600$
Effect on Positive Adjustment @ T1 = 0.159; p < .001
Effect on Log Knowledge @ T2 = -0.080; p < .348
Effect on Log Surveillance @ T2 = 0.500; p < .001
Effect on Requirements @ T2 = 0.177; p < .280

Requirements
$N = 253; R^2 = .454$
Effect on Positive Adjustment @ T1 = 0.027; p < .001
Effect on Log Knowledge @ T2 = -0.002; p < .949
Effect on Log Surveillance @ T2 = 0.082; p < .001
Effect on Requirements @ T2 = 0.688; p < .001

Average Positive Adjustment
Evaluation Over Months 1-3
$N = 253; R^2 = .679$

+0.342; p < .001

Wave 2 Analysis (N = 259)

Variable	β (K)	β (S)	β (R)	β (PA)
Intercept	0.365*	-0.005	0.646*	0.282***
Sample=Shock	-0.079	0.170**	0.359*	-0.035
Sample=Prison	-0.175**	0.073	-0.091	0.014
Sample=Dropout	-0.132	-0.031	0.383	0.039
Nonwhite	-0.085	0.027	-0.113	-0.025*
Age @ Com. Supv.	0.003	0.001	0.001	0.001
New Crime	-0.054	0.031	0.128	0.005
Prior Offenses	0.055	-0.088*	-0.067	-0.022
Cohort 1	—	—	—	—
Cohort 2	-0.027	0.129**	-0.401***	-0.159***
Cohort 3	0.043	0.029	0.113	-0.038**

Log Knowledge
$N = 236; R^2 = .385$
Effect on Positive Adjustment @ T2 = -0.102; p < .001
Effect on Log Knowledge @ T3 = 0.671; p < .001
Effect on Log Surveillance @ T3 = -0.115; p < .018
Effect on Requirements @ T3 = -0.142; p < .202

Log Surveillance
$N = 236; R^2 = .633$
Effect on Positive Adjustment @ T2 = 0.119; p < .001
Effect on Log Knowledge @ T3 = -0.109; p < .205
Effect on Log Surveillance @ T3 = 0.445; p < .001
Effect on Requirements @ T3 = -0.389; p < .021

Requirements
$N = 236; R^2 = .672$
Effect on Positive Adjustment @ T2 = 0.019; p < .001
Effect on Log Knowledge @ T3 = -0.005; p < .873
Effect on Log Surveillance @ T3 = 0.008; p < .765
Effect on Requirements @ T3 = 0.832; p < .001

Average Positive Adjustment
Evaluation Over Months 4-6
$N = 236; R^2 = .732$

+0.454; p < .001

Wave 3 Analysis (N = 214)

Variable	β (K)	β (S)	β (R)	β (PA)
Intercept	0.260	0.125	1.251***	0.225***
Sample=Shock	0.002	0.049	-0.218	-0.027
Sample=Prison	-0.035	-0.004	-0.080	0.001
Sample=Dropout	-0.087	0.001	-0.445*	0.020
Nonwhite	0.144*	0.019	0.159	-0.012
Age @ Com. Supv.	0.001	0.000	-0.027**	0.002
New Crime	-0.095	0.037	0.059	-0.002
Prior Offenses	0.000	0.001	-0.028	-0.009
Cohort 1	—	—	—	—
Cohort 2	—	—	—	—
Cohort 3	0.095	-0.045	-0.391***	-0.125***

Log Knowledge
$N = 194; R^2 = .490$
Effect on Positive Adjustment @ T3 = -0.088; p < .001
Effect on Log Knowledge @ T4 = 0.771; p < .001
Effect on Log Surveillance @ T4 = -0.047; p < .308
Effect on Requirements @ T4 = -0.159; p < .268

Log Surveillance
$N = 194; R^2 = .480$
Effect on Positive Adjustment @ T3 = 0.063; p < .005
Effect on Log Knowledge @ T4 = -0.042; p < .648
Effect on Log Surveillance @ T4 = 0.471; p < .001
Effect on Requirements @ T4 = 0.032; p < .875

Requirements
$N = 194; R^2 = .566$
Effect on Positive Adjustment @ T3 = 0.011; p < .116
Effect on Log Knowledge @ T4 = -0.024; p < .438
Effect on Log Surveillance @ T4 = -0.152; p < .501
Effect on Requirements @ T4 = 0.647; p < .001

Average Positive Adjustment
Evaluation Over Months 7-9
$N = 194; R^2 = .696$

+0.583; p < .001

Wave 4 Analysis (N= 178)

Variable	β (K)	β (S)	β (R)	β (PA)
Intercept	0.620**	0.200	0.477	0.086
Sample=Shock	-0.061	-0.059	-0.126	-0.052**
Sample=Prison	0.020	-0.027	0.242	-0.028
Sample=Dropout	0.144	-0.086	-0.068	-0.020
Nonwhite	0.023	0.001	-0.030	-0.033*
Age @ Com. Supv.	-0.012**	0.000	0.002	0.003**
New Crime	0.004	0.024	0.176	-0.009
Prior Offenses	-0.091	-0.046	0.116	-0.012
Cohort 1	—	—	—	—
Cohort 2	—	—	—	—
Cohort 3	—	—	—	—

Log Knowledge
$N = 159; R^2 = .577$
Effect on Positive Adjustment @ T4 = -0.059; p < .002

Log Surveillance
$N = 159; R^2 = .341$
Effect on Positive Adjustment @ T4 = 0.074; p < .012

Requirements
$N = 159; R^2 = .434$
Effect on Positive Adjustment @ T4 = 0.022; p < .013

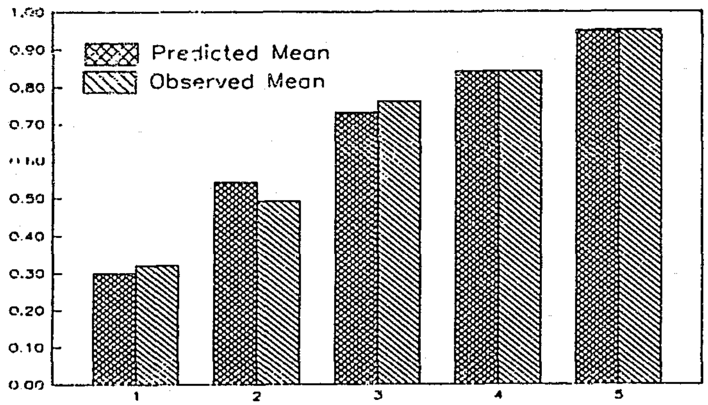
Average Positive Adjustment
Evaluation Over Months 10-12
$N = 159; R^2 = .563$

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

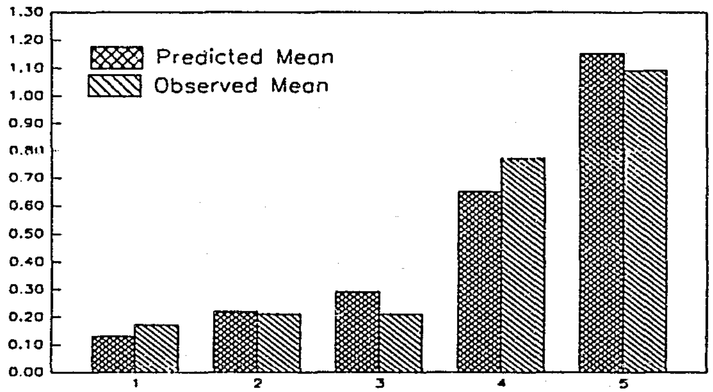
Note: Shock, Prison Parolee, and Dropout offenders are compared to Probationers. Type of offense and age at first arrest are excluded from the analysis because of excessive missing data and lack of predictive power. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions and offenders with a prior arrest and/or conviction record are compared to offenders without a record. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Analysis sample sizes do not equal total wave sample sizes because of missing data on some fixed effects

Figure 87
Validation of Wave 1 Models in Louisiana

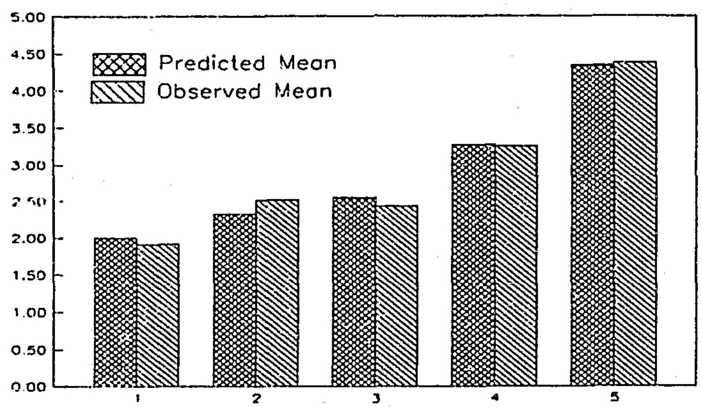
5 Equal-N Groups Ranked on Knowledge Scores



5 Equal-N Groups Ranked on Surveillance Scores



5 Equal-N Groups Ranked on Requirements' Scores



5 Equal-N Groups Ranked on Positive Adjustment Scores

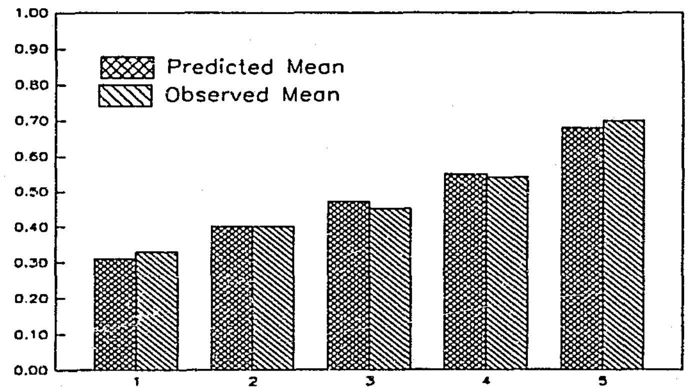


Figure 88
Validation of Wave 2 Models in Louisiana

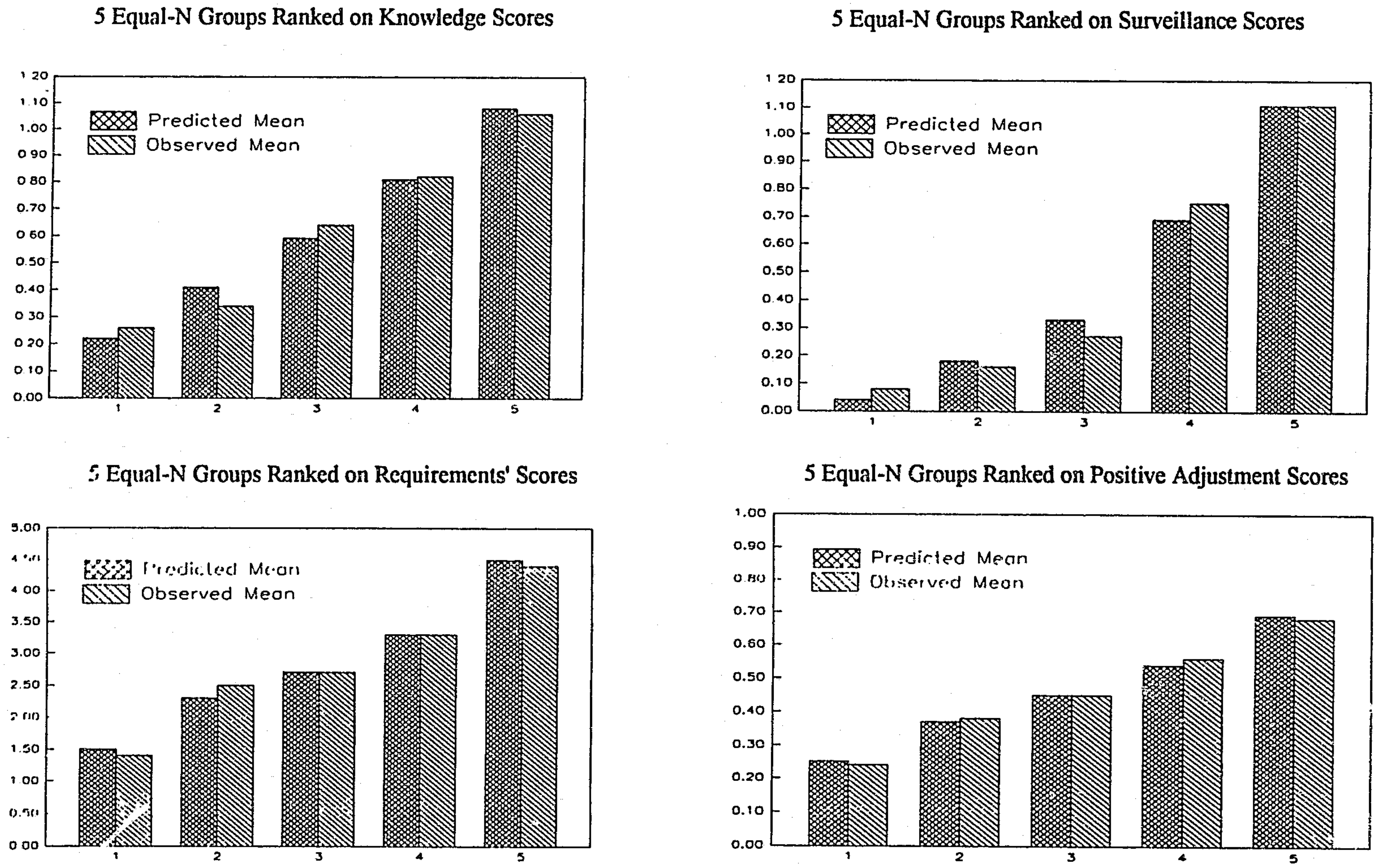


Figure 89
Validation of Wave 3 Models in Louisiana

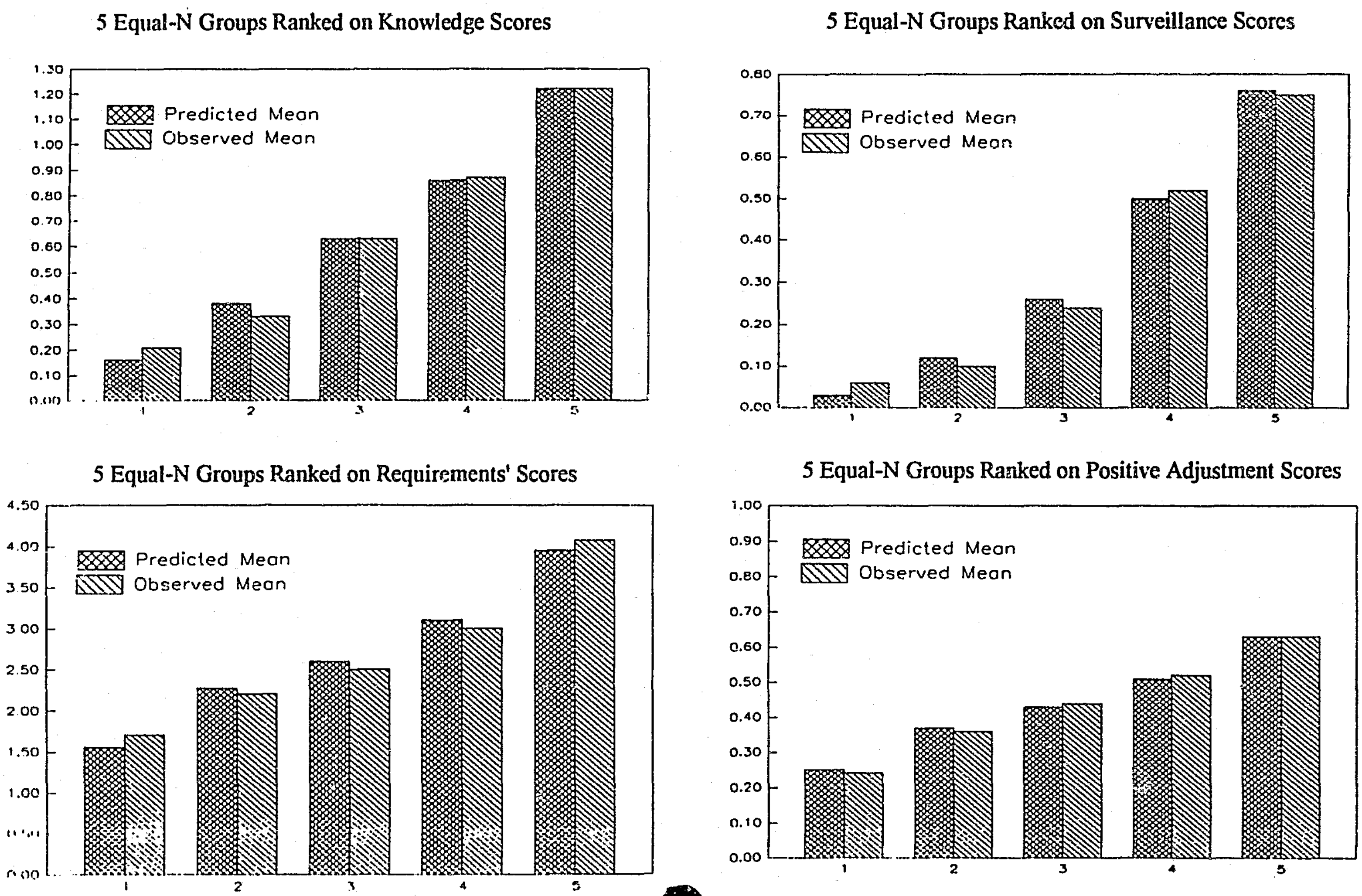


Figure 90
Validation of Wave 4 Models in Louisiana

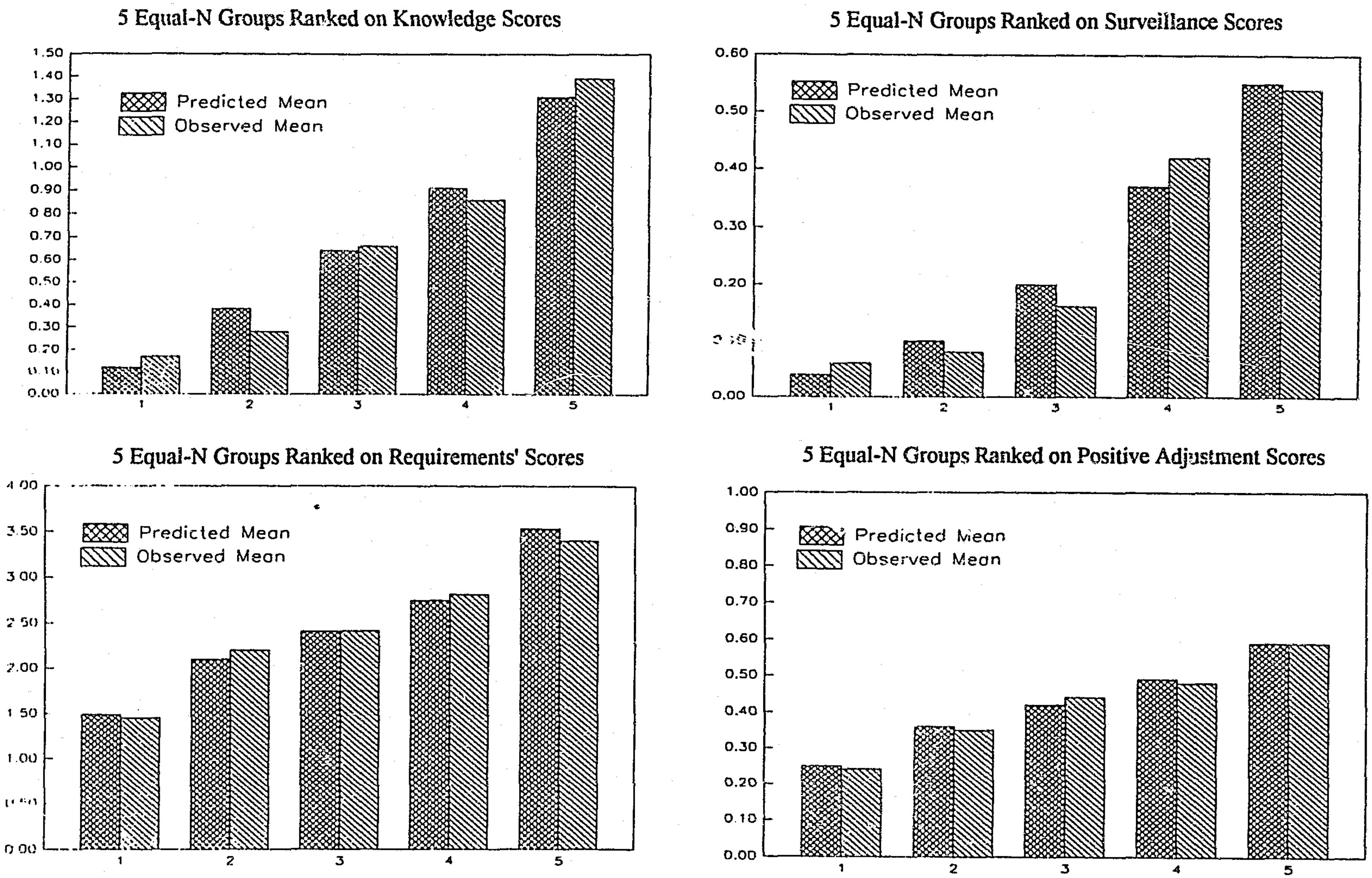
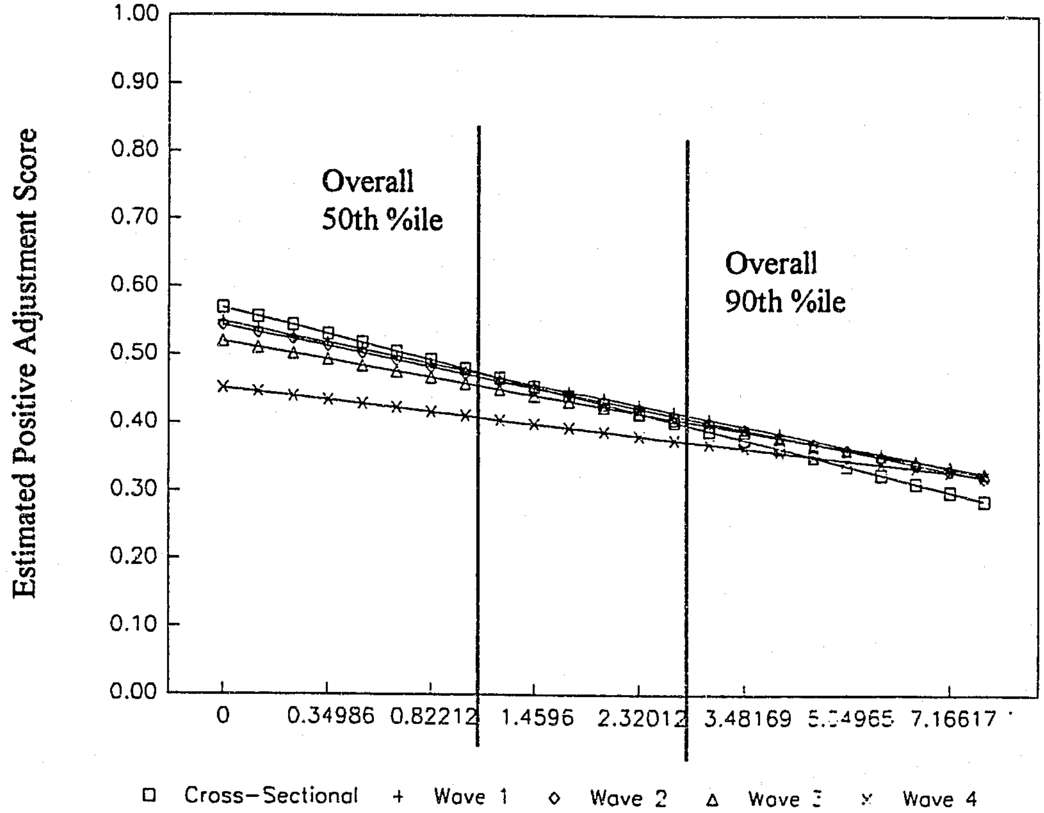
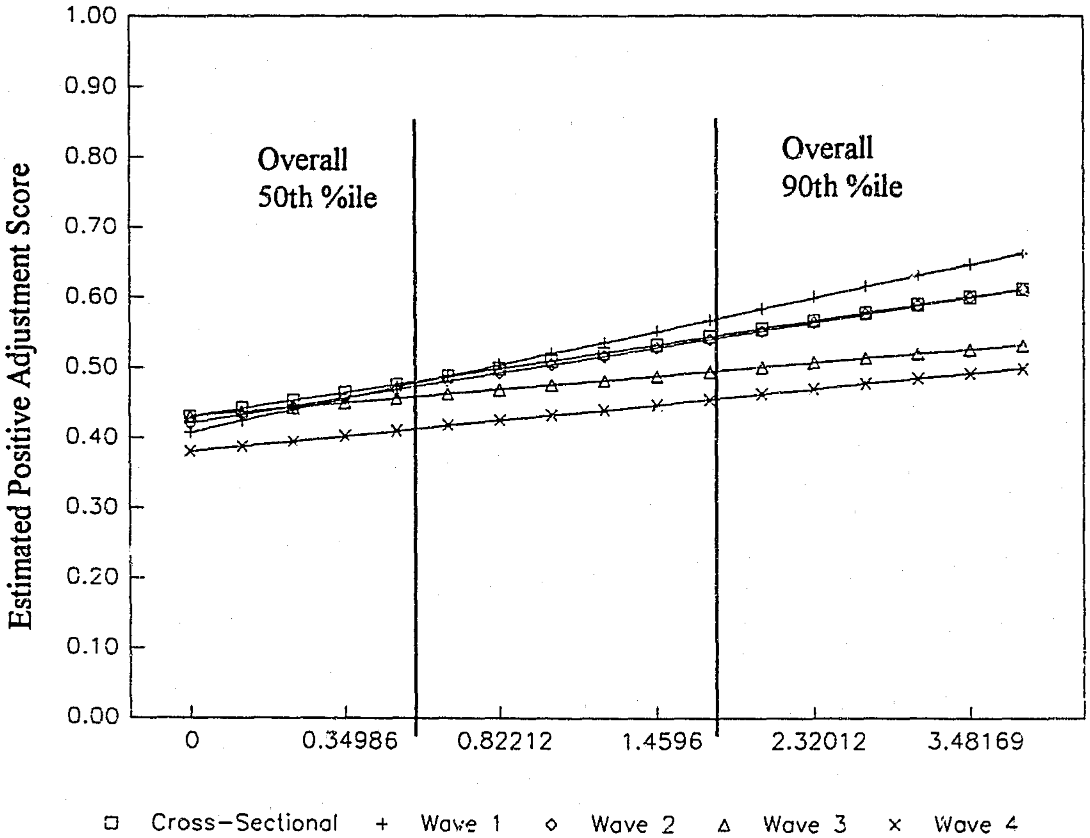


Figure 91
Estimated Effects of Knowledge Index For A Hypothetical Observation in Exit Cohort #4 in Louisiana



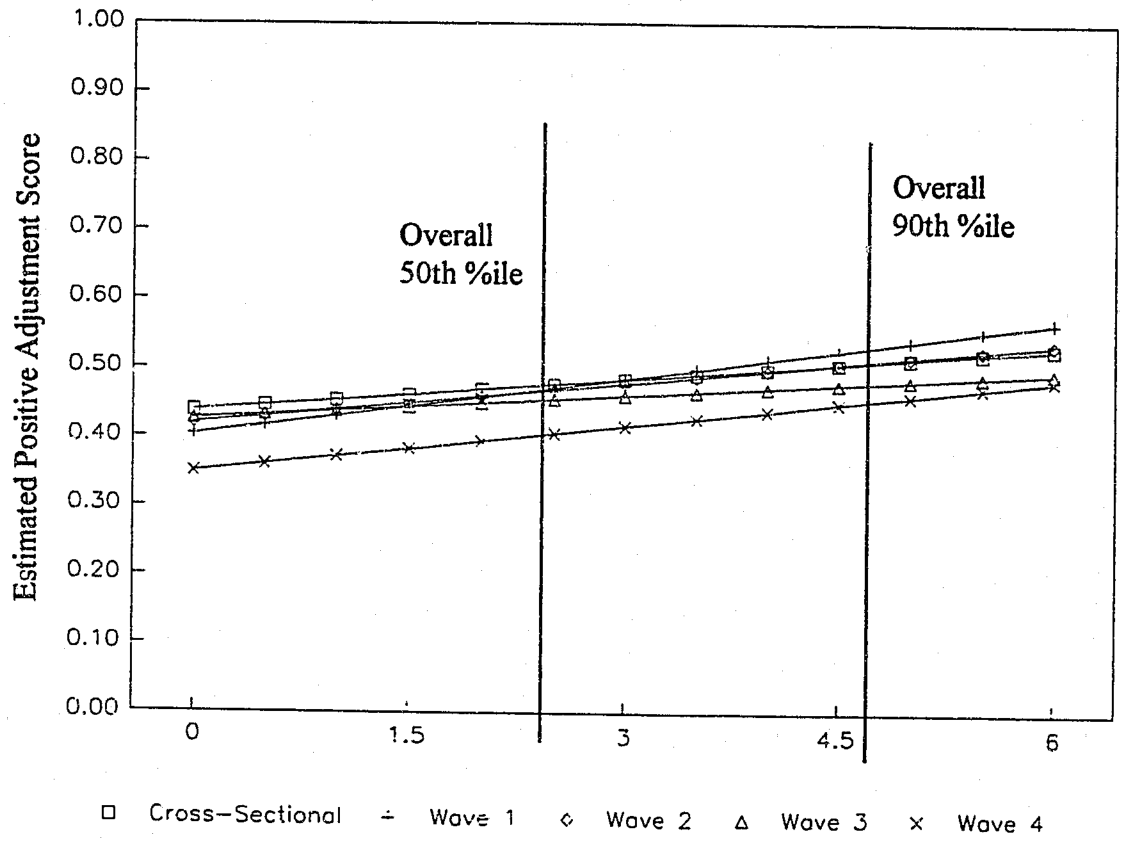
Note: Models were estimated on a natural log transform of the knowledge score. The exponentiated log-values (raw knowledge scores) are presented here. Percentiles refer to relative standing on the knowledge index. For this presentation, all predictor variables were constrained to their time-period specific means.

Figure 92
Estimated Effects of Surveillance Index For A Hypothetical Observation in Exit Cohort #4 in Louisiana



Note: Models were estimated on a natural log transform of the surveillance score. The exponentiated log-values (raw surveillance scores) are presented here. Percentiles refer to relative standing on the surveillance index. For this presentation, all predictor variables were constrained to their time-period specific means.

Figure 93
Estimated Effects of Requirements Index For A Hypothetical Observation in Exit Cohort #4 in Louisiana



Note: Percentiles refer to relative standing on the requirements index. For this presentation, all predictor variables were constrained to their time-period specific means.

Figure 94
New York Cross-Sectional Positive Adjustment Model

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	0.267	0.183	$p < .145$
Sample=Shock	0.063	0.046	$p < .170$
Sample=Dropout	-0.013	0.044	$p < .763$
Nonwhite	-0.091	0.046	$p < .049$
Age @ First Arrest	0.018	0.009	$p < .045$
Violent/Other Offenses	0.098	0.054	$p < .071$
Drug Offense	0.059	0.048	$p < .227$
Prior Record	0.027	0.060	$p < .654$
Cohort=Missing	-0.176	0.138	$p < .206$
Cohort 1	-0.254	0.046	$p < .001$
Cohort 2	-0.266	0.053	$p < .001$
Cohort 3	-0.109	0.069	$p < .115$

Mean Positive Adjustment (Valid N=237)

$R^2 = .251$

Note: Shock graduates and shock dropouts are compared to a group of prison parolees. Property offenders are the reference group for offense types. The model estimates comparison effects for drug offense and "other" offenses (which includes violent offenses). All offenders in the New York study were classified as having committed a new crime. The new crime indicator is, therefore, not included in these models. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Age at the beginning of community supervision was not included in the analysis because it is sufficiently collinear with age at first arrest that neither variable is significantly related to positive adjustment when both are included in the model. Age at first arrest and age at the beginning of community supervision are both predictive of positive adjustment but the shock sample is comprised of offenders who were significantly older at the age of their first arrest. Controlling for age at first arrest reduces the effect of the shock program on positive adjustment. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Supervision intensity indicators were not available for analysis in New York.

Figure 95
Validation of Cross-Sectional Positive Adjustment Model In New York

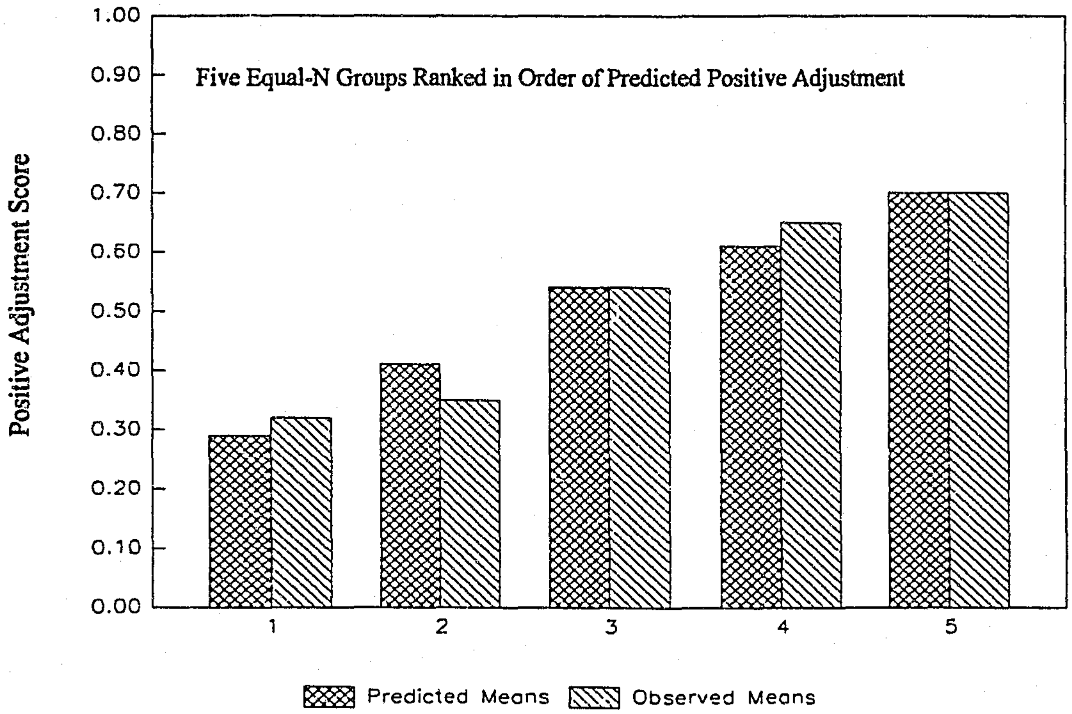


Figure 96
New York Four-Wave Recursive Panel Model

Wave 1 Analysis (N = 233)

Variable	ß	s.e.(ß)	Sig.
Intercept	0.510	0.191	$p < .008$
Sample=Shock	0.048	0.048	$p < .317$
Sample=Dropout	-0.057	0.046	$p < .222$
Nonwhite	-0.043	0.049	$p < .376$
Age @ First Arrest	0.008	0.010	$p < .366$
Violent/Other Offense	0.059	0.057	$p < .304$
Drug Offense	0.006	0.051	$p < .907$
Prior Record	-0.010	0.063	$p < .879$
Cohort 1	-0.287	0.048	$p < .001$
Cohort 2	-0.202	0.055	$p < .001$
Cohort 3	-0.027	0.072	$p < .710$

Wave 2 Analysis (N = 184)

Variable	ß	s.e.(ß)	Sig.
Intercept	-0.275	0.153	$p < .074$
Sample=Shock	0.034	0.038	$p < .369$
Sample=Dropout	0.079	0.039	$p < .042$
Nonwhite	-0.069	0.038	$p < .073$
Age @ First Arrest	0.019	0.007	$p < .009$
Violent/Other Offense	0.092	0.047	$p < .054$
Drug Offense	0.079	0.042	$p < .060$
Prior Record	0.072	0.048	$p < .140$
Cohort 1	—	—	—
Cohort 2	-0.242	0.041	$p < .001$
Cohort 3	-0.146	0.052	$p < .006$

Wave 3 Analysis (N = 151)

Variable	ß	s.e.(ß)	Sig.
Intercept	0.135	0.169	$p < .424$
Sample=Shock	0.076	0.043	$p < .081$
Sample=Dropout	-0.001	0.044	$p < .977$
Nonwhite	-0.075	0.043	$p < .085$
Age @ First Arrest	-0.002	0.008	$p < .803$
Violent/Other Offense	-0.015	0.055	$p < .786$
Drug Offense	0.024	0.047	$p < .602$
Prior Record	0.019	0.056	$p < .741$
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	-0.064	0.055	$p < .242$

Wave 4 Analysis (N = 133)

Variable	ß	s.e.(ß)	Sig.
Intercept	0.158	0.111	$p < .156$
Sample=Shock	0.003	0.029	$p < .932$
Sample=Dropout	0.020	0.030	$p < .506$
Nonwhite	0.027	0.030	$p < .367$
Age @ First Arrest	0.009	0.006	$p < .117$
Violent/Other Offense	-0.002	0.038	$p < .965$
Drug Offense	0.014	0.033	$p < .674$
Prior Record	-0.068	0.037	$p < .069$
Cohort 1	—	—	—
Cohort 2	—	—	—
Cohort 3	—	—	—

Positive Adjustment Evaluation At Third Month
N=233; $R^2 = .216$

+.733; $p < .001$

Positive Adjustment Evaluation At Sixth Month
N=184; $R^2 = .653$

+.768; $p < .001$

Positive Adjustment Evaluation At Ninth Month
N = 151; $R^2 = .613$

+.986; $p < .001$

Positive Adjustment Evaluation At Twelfth Month
N=133; $R^2 = .862$

Note: Shock graduates and shock dropouts are compared to a group of prison parolees. Property offenders are the reference group for offense types. The model estimates comparison effects for drug offense and "other" offenses (which includes violent offenses). All offenders in the New York study were classified as having committed a new crime. The new crime indicator is, therefore, not included in these models. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Age at the beginning of community supervision was not included in the analysis because it is sufficiently collinear with age at first arrest that neither variable is significantly related to positive adjustment when both are included in the model. Age at first arrest and age at the beginning of community supervision are both predictive of positive adjustment but the shock sample is comprised of offenders who were significantly older at the age of their first arrest. Controlling for age at first arrest reduces the effect of the shock program on positive adjustment. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Supervision intensity indicators were not available for analysis in New York.

Figure 97
Validation of Wave 1 - Wave 4 Positive Adjustment Models In New York

5 Equal-N Groups Ranked on Positive Adjustment Scores

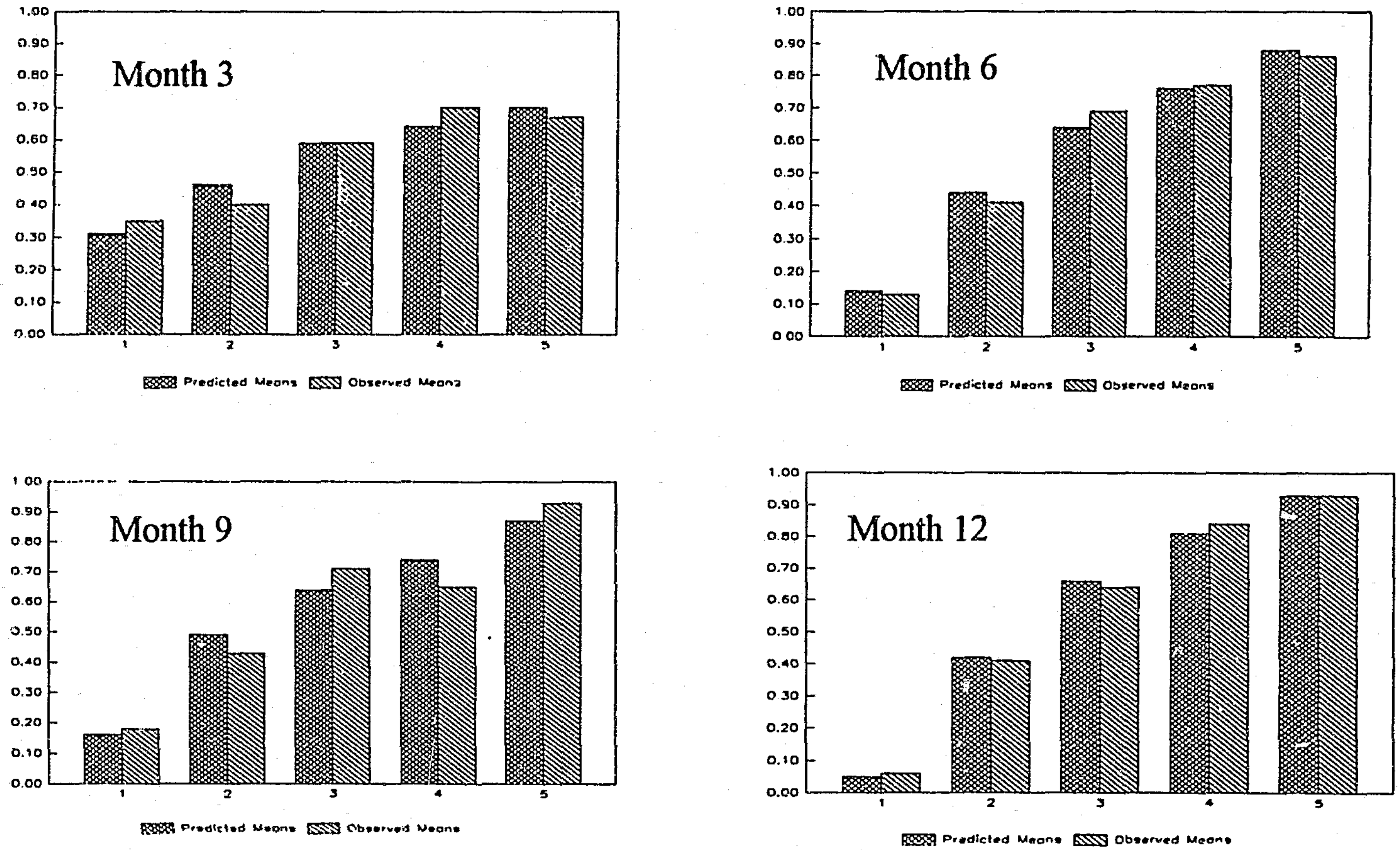


Figure 98
South Carolina Cross-Sectional Positive Adjustment Model

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	1.288	0.272	p<.001
Sample=DPPPS Shock	0.203	0.072	p<.005
Sample=DOC Shock	0.012	0.076	p<.877
Sample=Prison	0.242	0.078	p<.002
Sample=Split-Probation	0.192	0.111	p<.084
Nonwhite	-0.041	0.051	p<.429
Age @ Com. Supv.	-0.024	0.012	p<.043
Violent Offense	-0.073	0.078	p<.348
Drug Offense	-0.024	0.059	p<.686
New Crime	0.011	0.085	p<.895
Prior Record	0.072	0.055	p<.186
Cohort 1	-0.419	0.157	p<.008
Cohort 2	0.027	0.099	p<.786
Cohort 3	-0.078	0.082	p<.342

Mean Log Primary Contacts (Valid N=306)

$R^2 = .093$

Effect	ß	s.e. {ß}	p <
Linear	.073	..054	.180
Quadratic	-.333	.057	.001
Cubic	.155	.059	.009

Vector of Fixed Effects

Variable	ß	s.e.{ß}	Sig.
Intercept	0.346	0.176	p<.050
Sample=DPPPS Shock	-0.010	0.044	p<.823
Sample=DOC Shock	-0.016	0.045	p<.727
Sample=Prison	0.016	0.048	p<.743
Sample=Split-Probation	-0.033	0.066	p<.624
Nonwhite	-0.155	0.031	p<.001
Age @ Com. Supv.	0.009	0.007	p<.200
Violent Offense	0.120	0.046	p<.010
Drug Offense	0.033	0.035	p<.351
New Crime	0.056	0.050	p<.268
Prior Record	-0.072	0.033	p<.028
Cohort 1	0.071	0.095	p<.453
Cohort 2	-0.074	0.059	p<.213
Cohort 3	-0.026	0.050	p<.597

Mean Positive Adjustment (Valid N=306)

$R^2 = .262$

Note: DOC Shock, DPPPS Shock, Split-Probationer and Prison Parolee offenders are compared to Probationers. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Age at first arrest was not included in the analysis because it is sufficiently collinear with age at beginning of community supervision that neither variable is significantly related to positive adjustment when both are included in the model. By itself, age at first arrest is not predictive of positive adjustment although age at the beginning of community supervision is. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements).

Figure 99
Validation of Cross-Sectional Models in South Carolina

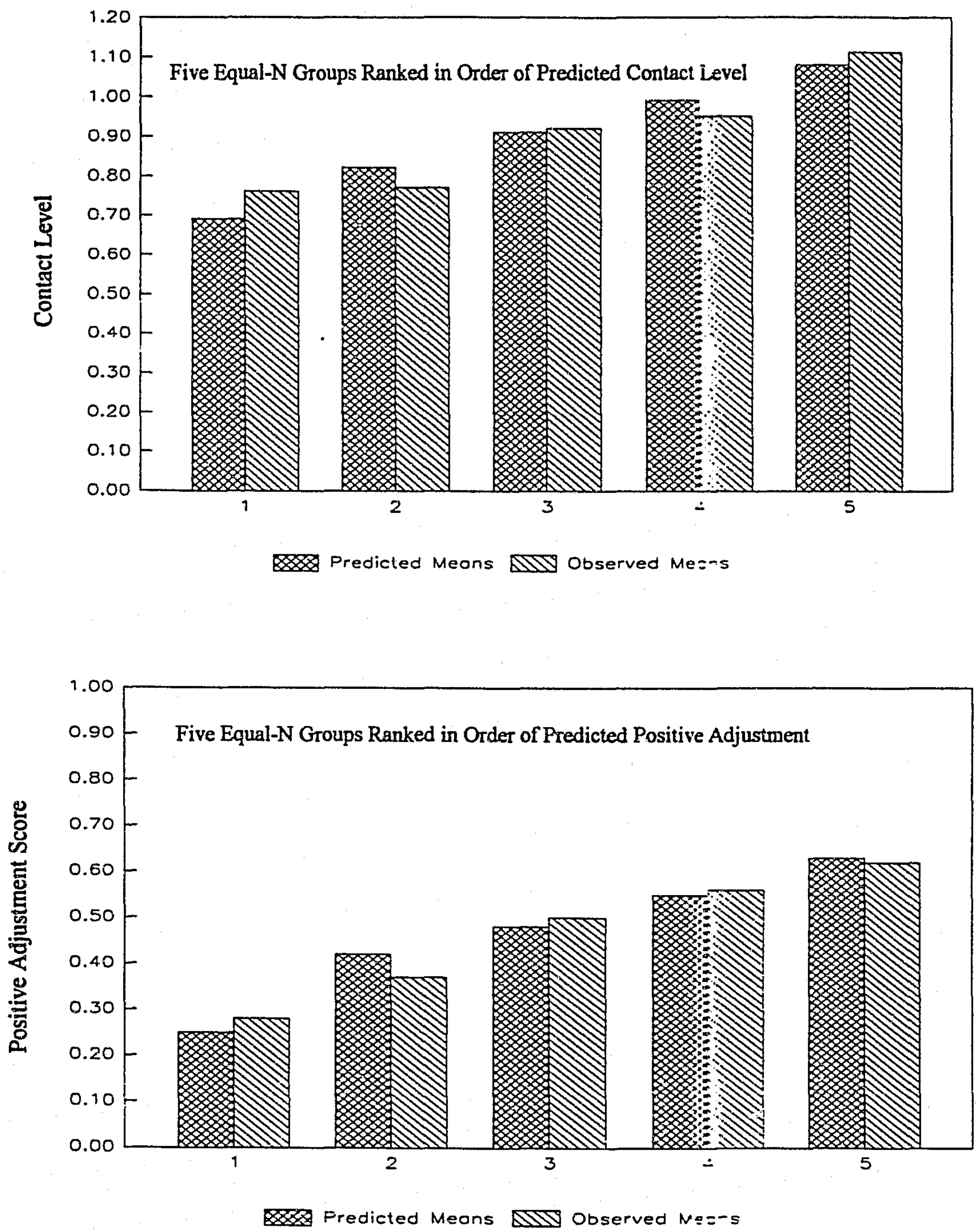


Figure 100
South Carolina Four-Wave Recursive Panel Model

Wave 1 Analysis (N = 242)

Variable	β	s.e.(β)	Sig.
Intercept	1.878	0.391	p<.001
Sample=DPPPS Shock	0.282	0.088	p<.002
Sample=Prison	0.368	0.096	p<.001
Sample=Split Probation	0.372	0.137	p<.007
Nonwhite	-0.063	0.070	p<.368
Age @ Com. Supv.	-0.050	0.017	p<.004
Violent Offense	-0.142	0.108	p<.189
Drug Offense	0.007	0.086	p<.934
New Crime	0.047	0.112	p<.673
Prior Record	0.078	0.071	p<.273
Cohort 1	-0.534	0.192	p<.006
Cohort 2	-0.003	0.121	p<.980
Cohort 3	-0.133	0.101	p<.188

Mean Log Contacts: Months 1-3
N = 228; R^2 = .153
Effects on Positive Adjustment
Linear Effect = -.006; p < .918
Quadratic Effect = -.184; p < .001
Cubic Effect = .122; p < .017

+.830; p < .001

+.106; p < .136

Positive Adjustment Evaluation At Third Month
N=228; R^2 = .148

+.485; p < .001

Variable	β	s.e.(β)	Sig.
Intercept	0.045	0.239	p<.852
Sample=DPPPS Shock	-0.005	0.052	p<.921
Sample=Prison	0.023	0.057	p<.690
Sample=Split Probation	-0.041	0.080	p<.610
Nonwhite	-0.129	0.040	p<.002
Age @ Com. Supv.	0.024	0.010	p<.019
Violent Offense	-0.006	0.063	p<.920
Drug Offense	0.022	0.049	p<.653
New Crime	0.141	0.063	p<.027
Prior Record	-0.067	0.041	p<.105
Cohort 1	0.121	0.112	p<.281
Cohort 2	-0.020	0.070	p<.779
Cohort 3	0.035	0.058	p<.543

Wave 2 Analysis (N = 227)

Variable	β	s.e.(β)	Sig.
Intercept	0.231	0.243	p<.302
Sample=DPPPS Shock	0.047	0.053	p<.375
Sample=Prison	-0.036	0.058	p<.543
Sample=Split Probation	-0.084	0.085	p<.327
Nonwhite	-0.018	0.043	p<.680
Age @ Com. Supv.	-0.007	0.011	p<.530
Violent Offense	0.045	0.063	p<.483
Drug Offense	-0.007	0.050	p<.893
New Crime	-0.047	0.068	p<.487
Prior Record	-0.002	0.043	p<.960
Cohort 1			
Cohort 2	-0.149	0.071	p<.037
Cohort 3	0.014	0.059	p<.810

Mean Log Contacts: Months 4-6
N = 220; R^2 = .716
Effects on Positive Adjustment
Linear Effect = .146; p < .001
Quadratic Effect = -.081; p < .048
Cubic Effect = N.S.

+.466; p < .001

+.120; p < .284

Positive Adjustment Evaluation At Sixth Month
N=220; R^2 = .402

+.354; p < .001

Variable	β	s.e.(β)	Sig.
Intercept	0.144	0.213	p<.499
Sample=DPPPS Shock	-0.010	0.047	p<.830
Sample=Prison	-0.015	0.051	p<.769
Sample=Split Probation	0.047	0.074	p<.530
Nonwhite	-0.082	0.038	p<.030
Age @ Com. Supv.	-0.001	0.009	p<.949
Violent Offense	0.110	0.056	p<.052
Drug Offense	0.114	0.044	p<.011
New Crime	0.031	0.059	p<.605
Prior Record	-0.040	0.038	p<.287
Cohort 1			
Cohort 2	-0.098	0.064	p<.126
Cohort 3	-0.051	0.052	p<.326

Wave 3 Analysis (N = 202)

Variable	β	s.e.(β)	Sig.
Intercept	-0.216	0.390	p<.580
Sample=DPPPS Shock	0.057	0.088	p<.517
Sample=Prison	0.017	0.093	p<.861
Sample=Split-Probation	-0.060	0.131	p<.646
Nonwhite	0.033	0.069	p<.627
Age @ Com. Supv.	0.019	0.017	p<.264
Violent Offense	-0.052	0.107	p<.627
Drug Offense	-0.018	0.082	p<.826
New Crime	0.160	0.109	p<.143
Prior Record	-0.019	0.070	p<.780
Cohort 1			
Cohort 2			
Cohort 3	-0.076	0.091	p<.407

Mean Log Contacts: Months 7-9
N = 198; R^2 = .265
Effects on Positive Adjustment
Linear Effect = .102; p < .003
Quadratic Effect = -.091; p < .058
Cubic Effect = N.S.

+.511; p < .001

-.014; p < .907

Positive Adjustment Evaluation At Ninth Month
N = 198; R^2 = .514

+.474; p < .001

Variable	β	s.e.(β)	Sig.
Intercept	0.120	0.194	p<.537
Sample=DPPPS Shock	-0.025	0.044	p<.568
Sample=Prison	-0.015	0.047	p<.756
Sample=Split-Probation	-0.110	0.066	p<.097
Nonwhite	-0.094	0.035	p<.008
Age @ Com. Supv.	0.008	0.008	p<.369
Violent Offense	-0.031	0.055	p<.565
Drug Offense	-0.003	0.042	p<.942
New Crime	-0.041	0.055	p<.456
Prior Record	-0.020	[illegible]	p<.562
Cohort 1			
Cohort 2			
Cohort 3	-0.103	0.046	p<.028

Wave 4 Analysis (N = 165)

Variable	β	s.e.(β)	Sig.
Intercept	0.125	0.447	p<.780
Sample=DPPPS Shock	0.168	0.130	p<.204
Sample=Prison	-0.012	0.156	p<.941
Sample=Split-Probation	-0.125	0.112	p<.270
Nonwhite	0.005	0.020	p<.811
Age @ Com. Supv.	-0.074	0.120	p<.540
Violent Offense	[illegible]	0.133	p<.066
Drug Offense	-0.164	0.113	p<.154
New Crime	0.254	0.101	p<.016
Prior Record			
Cohort 1			
Cohort 2			
Cohort 3			

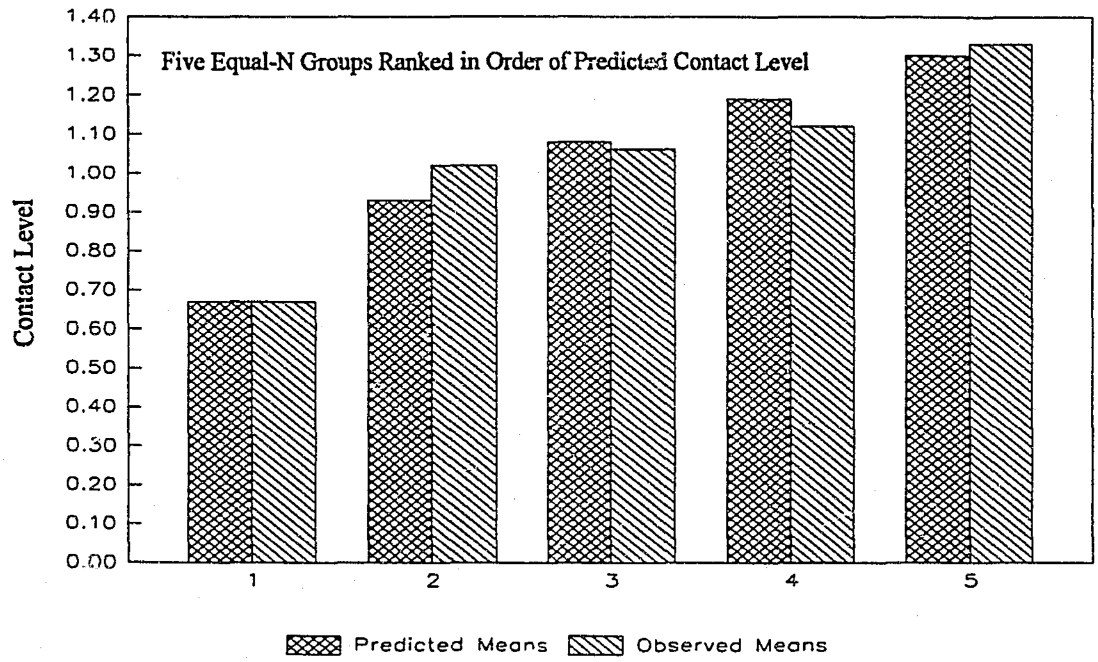
Mean Log Contacts: Months 10-12
N = 163; R^2 = .321
Effects on Positive Adjustment
Linear Effect = .160; p < .001
Quadratic Effect = -.140; p < .020
Cubic Effect = N.S.

Positive Adjustment Evaluation At Twelfth Month
N=163; R^2 = .453

Variable	β	s.e.(β)	Sig.
Intercept	0.332	0.231	p<.152
Sample=DPPPS Shock	-0.066	0.052	p<.210
Sample=Prison	0.043	0.061	p<.485
Sample=Split-Probation	0.059	0.075	p<.433
Nonwhite	-0.114	0.044	p<.010
Age @ Com. Supv.	-0.002	0.010	p<.851
Violent Offense	0.107	0.067	p<.113
Drug Offense	0.075	0.049	p<.127
New Crime	-0.058	0.075	p<.443
Prior Record	-0.081	0.043	p<.062
Cohort 1			
Cohort 2			
Cohort 3			

Note: DPPPS Shock, Split-Probationer and Prison Parolee offenders are compared to Probationers. The DOC Shock sample was not followed over time. Violent and Drug offenders are compared to Property and "Other" offenders. Offenders who are serving a sentence for a new crime are compared to offenders who are serving a sentence for a technical violation of community supervision conditions. Offenders with a prior arrest and/or conviction record are compared to offenders without a record. Age at first arrest was not included in the analysis because it is sufficiently collinear with age at beginning of community supervision that neither variable is significantly related to positive adjustment when both are included in the model. By itself, age at first arrest is not predictive of positive adjustment although age at the beginning of community supervision is. Dummy cohort effects adjust the intercept term up or down relative to membership in cohort 4 (the group completing all four measurements). Analysis sample sizes do not always equal wave sample sizes because some of the fixed effects have missing values.

Figure 101
Validation of Wave 1 Models in South Carolina



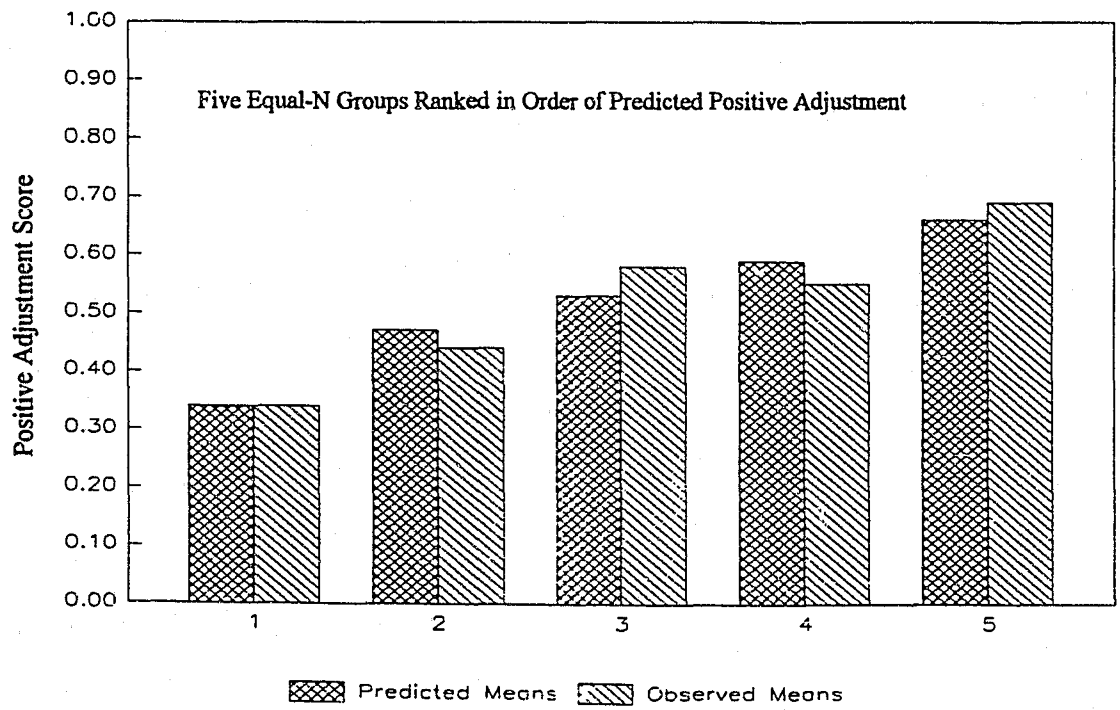
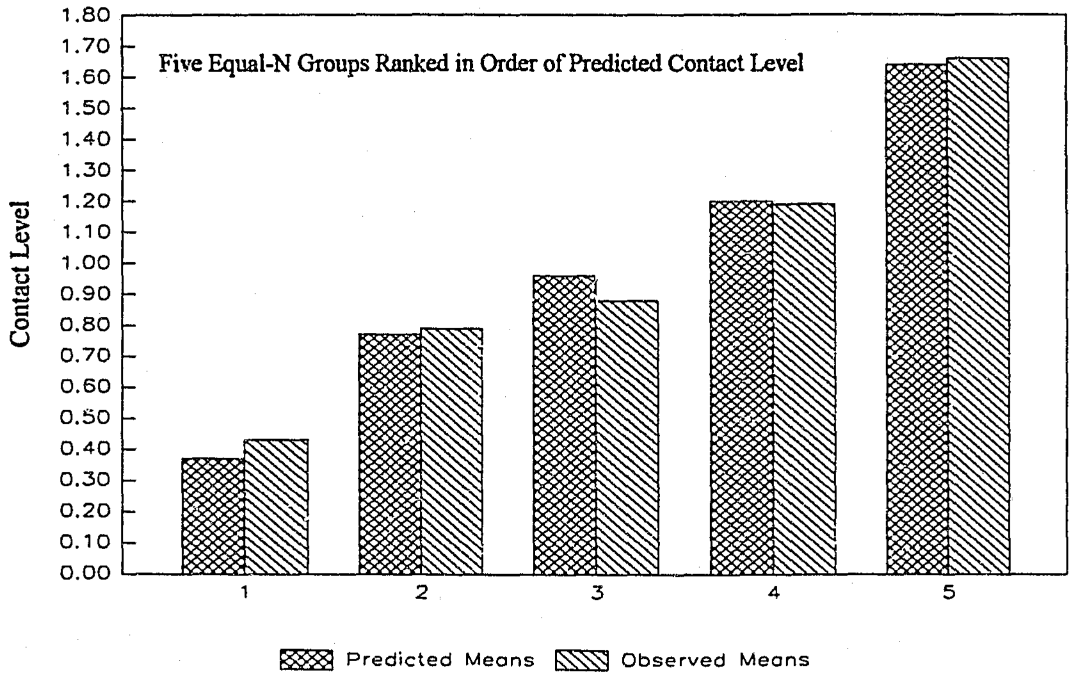


Figure 102
Validation of Wave 2 Models in South Carolina



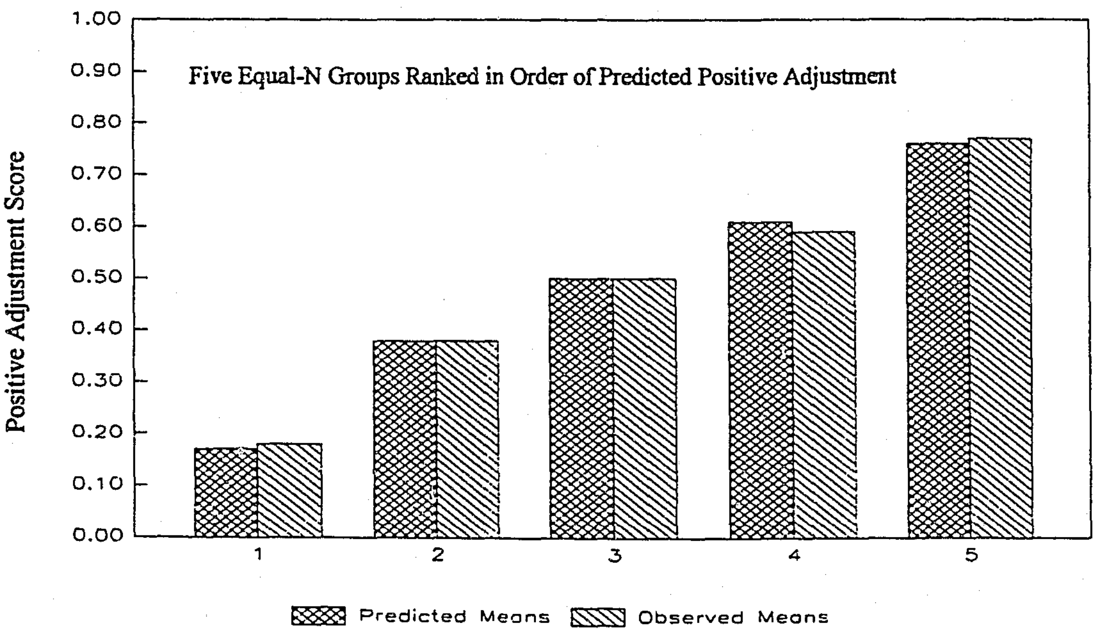
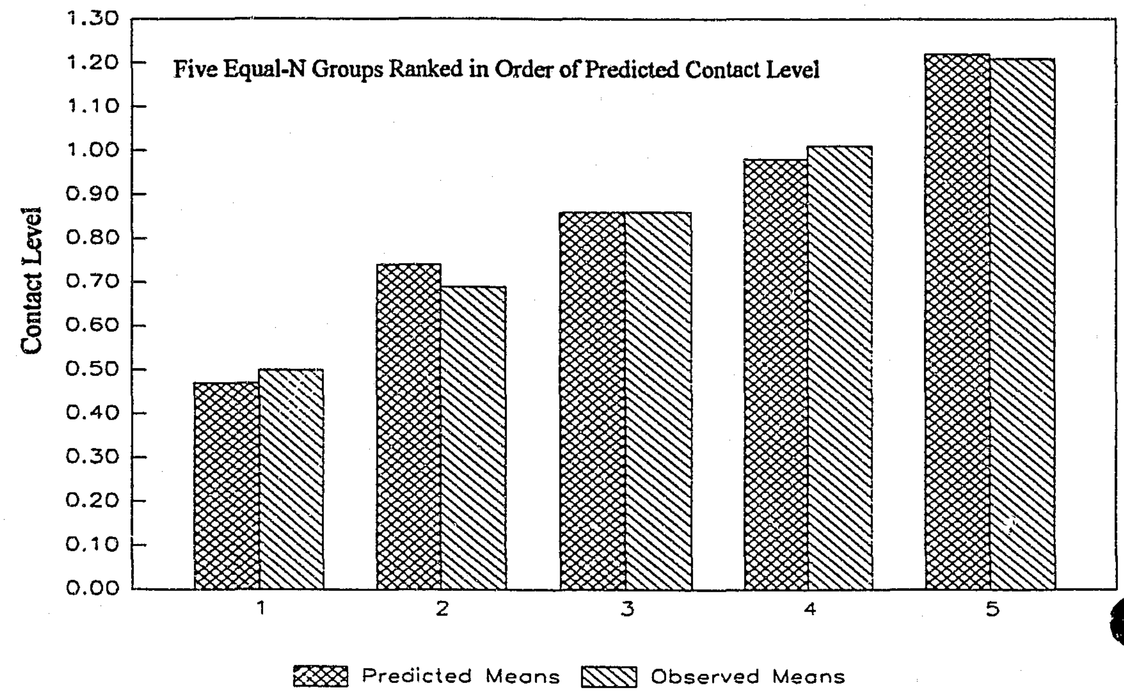


Figure 103
Validation of Wave 3 Models in South Carolina



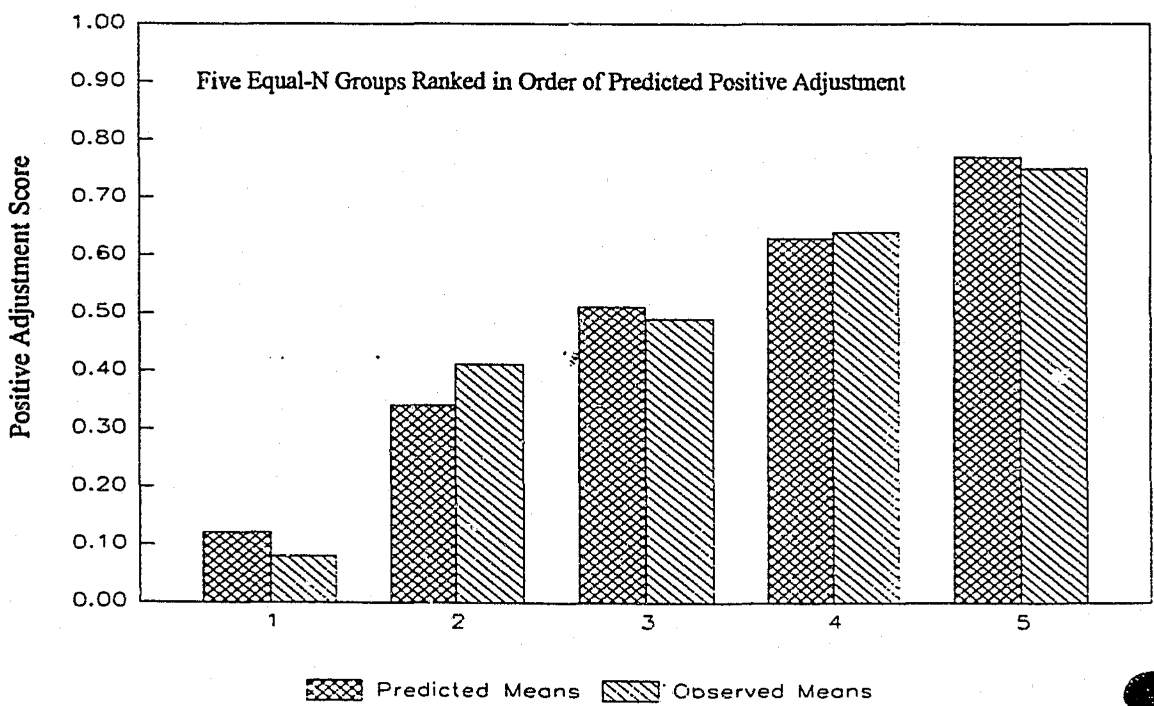
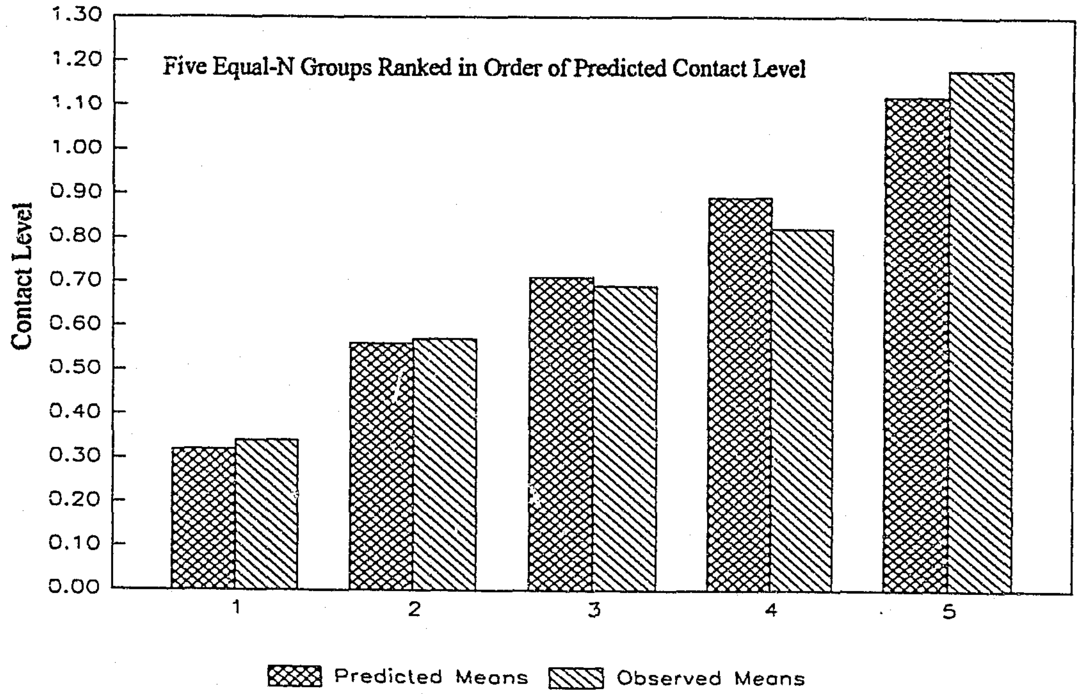


Figure 104
Validation of Wave 4 Models in South Carolina



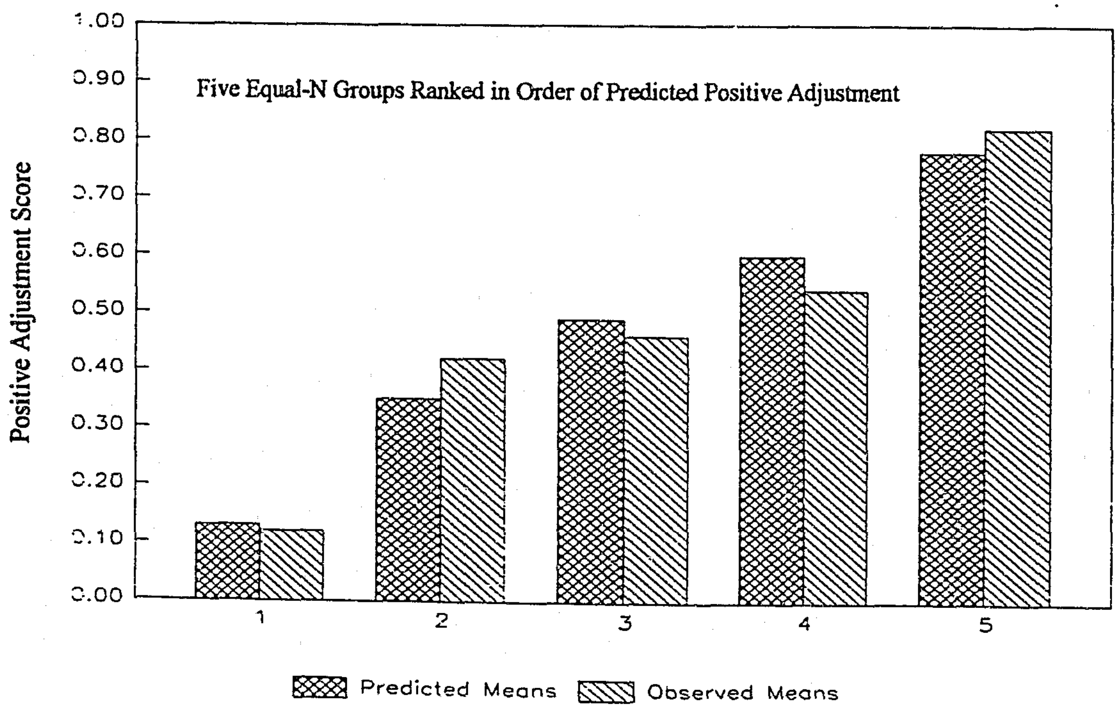
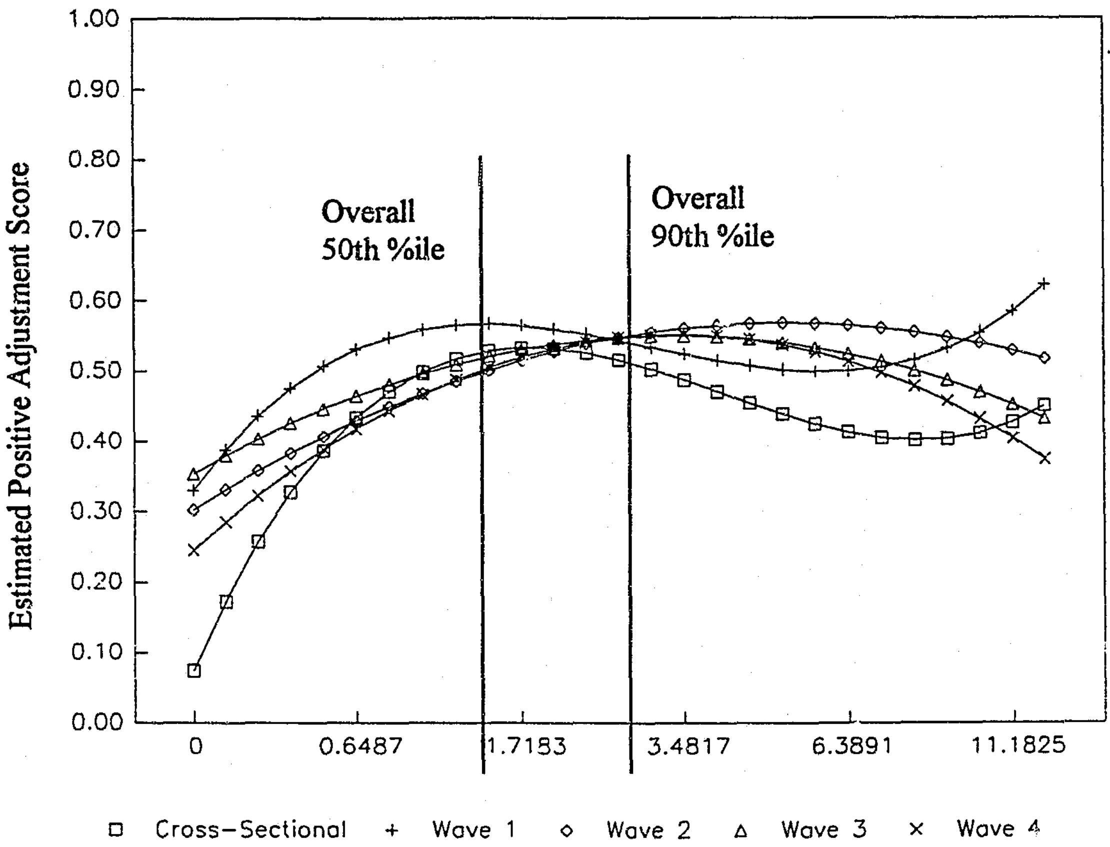


Figure 105
Estimated Effects of Primary Contacts For A Hypothetical Observation in Exit Cohort #4 in South Carolina



Note: Models were estimated on a natural log transform of contact levels. The exponentiated log-values (raw contact levels) are presented here. Percentiles refer to relative standing on primary contact rates. For this presentation, all predictor variables were constrained to their time-period specific means.