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ESTIMATING A DOSE-RESPONSE RELATIONSHIP BETWEEN LENGTH OF STAY AND FUTURE RECIDIVISM IN SERIOUS JUVENILE OFFENDERS*

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Abstract

The effect of sanctions on subsequent criminal activity is of central theoretical importance in criminology. A key question for juvenile justice policy is the degree to which serious juvenile offenders respond to sanctions and/or treatment administered by the juvenile court. The policy question germane to this debate is finding the level of confinement within the juvenile justice system that maximizes the public safety and therapeutic benefits of institutional confinement. Unfortunately, research on this issue has been limited with regard to serious juvenile offenders. We use longitudinal data from a large sample of serious juvenile offenders from two large cities to 1) estimate a causal treatment effect of institutional placement, as opposed to probation, on future rate of rearrest and 2) investigate the existence of a marginal effect (i.e., benefit) for longer length of stay once the institutional placement decision had been made. We accomplish the latter by determining a dose-response relationship between the length of stay and future rates of rearrest and self-reported offending. The results suggest that an overall null effect of placement exists on future rates of rearrest or self-reported offending for serious juvenile offenders. We also find that, for the group placed out of the community, it is apparent that little or no marginal benefit exists for longer lengths of stay. Theoretical, empirical, and policy issues are outlined.

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Recent policies have narrowed the jurisdiction of juvenile court by removing the most serious offenders through various mechanisms, which include waiver, statutory exclusion, and lowering the age of majority (Fagan, 2008; Feld, 1998, 1999; Griffin, 2003; Zimring, 1998). These changes have been occurring in an ongoing cycle for nearly three decades, capped by a frenzy in the 1990s, when virtually every state acted in some way to "toughen up" laws governing the processing of serious juvenile offenders. The removal of many serious offenders from juvenile court jurisdiction has raised basic questions about the viability of the juvenile court itself (Fagan, Kupchik, and Liberman, 2007). Given this political environment, demonstrating the effectiveness of the juvenile courts' treatment and confinement practices is essential for informed debate about future juvenile justice policy.

It is important to understand what prompted all this change in the first place. In reaction to increases in rates of youth violence in the 1980s (Blumstein and Wallman, 2000), both policy makers and the public lost confidence in the capacity of the juvenile court to fulfill its mission of effectively treating and/or deterring serious juvenile offenders from committing future crimes (Moon, Cullen, and Wright, 2003). Zimring (1998) suggests that the perception of leniency led to a related perception of increased public safety risks. Perceived short stays in juvenile facilities also offended the popular and political culture of proportionality. The lengths of placement in juvenile facilities were viewed as too short to be effective either in treating youth or in inflicting punishment that would have a residual deterrent effect once they were released.

The juvenile system responded by instituting policy changes aimed mainly at increasing public safety. These changes included mandatory placements in secure confinement for adolescents charged with serious crimes, mandatory minimums once placed, and longer overall stays (Fagan, 2008). Attempts were made to combine the treatment benefits of the juvenile system with the greater emphasis on punishment in the adult system (i.e., a "blended approach") once in placement (Redding and Howell, 2000). These policy shifts were designed with the expectation that the experience of institutional placement, as opposed to community placement, would reduce the incidence of criminality among juvenile offenders and that the reduction in criminality would be a function not just of the occurrence of placement but also of the duration of the placement. In other words, the probability of future criminality could decline as a function of either 1) the deterrent impact of institutional placement itself or 2) the length of exposure to institutional confinement. Longer stays in institutions provide increased treatment as well as a heightened cost for involvement in crime.

Thus, the policy question germane to this debate is finding the level of punishment and/or treatment within the juvenile justice system that maximizes the public safety benefits of confinement (Bhati and Piquero, 2008). A demonstration of capacity for effective punishment and the efficient use of resources are essential to the survival of the juvenile court (Schneider, 1990; Scott and Steinberg, 2008). If longer stays in institutional facilities are not producing gains in reduced offending, then it is questionable whether this use of resources is either justifiable or politically attractive (Blumstein and Piquero, 2007). The financial cost of placing individuals in institutional care for extended periods is substantial, and high levels of spending on this practice should produce some benefit in terms of increased public safety. Without a demonstration that increased or longer institutional stays provide such a benefit, the argument for incurring these costs is substantially weaker.

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This article addresses these questions using data from the Pathways to Desistance Study, which includes a longitudinal sample of serious juvenile offenders who can be thought of as the likely targets of competing policies (i.e., removal from court versus tougher punishment within court). We test two hypotheses. First, we estimate a causal treatment effect of placement into institutional care, as opposed to probation, on future rates of rearrest and self-reported offending among serious juvenile offenders to test whether placement reduces future crime. Second, using the group of youth sent to placement, we estimate the marginal effect on recidivism of longer stays in placement by identifying a dose-response relationship between the length of stay and future rates of both rearrest and self-reported offending, to test whether longer lengths of stay suppress recidivism rates. Our results suggest that an overall null effect of placement exists on future rates of recidivism for serious juvenile offenders. Furthermore, for those individuals receiving placement, it is apparent that little or no marginal benefit exists for longer lengths of stay, in terms of reducing rates of rearrest or self-reported offending, at least for being retained out of the community between 3 and 13 months.

BACKGROUND

At its core, deterrence assumes that sanctions delivered in a certain, swift, and severe manner will serve to increase sanction risk perceptions and subsequently reduce criminal activity (Beccaria, 1986 [1764]; Zimring and Hawkins, 1973). Criminologists long have been interested in the effects of sanctions on subsequent criminal activity, and much empirical research has been devoted to this line of work with regard to adult offenders (Nagin, 1998). Although some detectable effect is apparent regarding the certainty of punishment among certain offenders and certain crime types, this literature shows that increased sanctions do not substantially reduce future recidivism but instead produce only a small (Bhati and Piquero, 2008) or incapacitation effect (Piquero and Blumstein, 2007) on recidivism. In a series of studies, Gendreau and colleagues (Gendreau, Goggin, and Cullen, 1999; Gendreau, Goggin, and Fulton, 2000; Gendreau et al., 2001) conducted meta-analyses of 117 prior studies involving 442,471 offenders, which showed that increased sanctions did not suppress future criminality. Instead, this work found that increased sentence length or institutional, as opposed to community, sentences were often associated with increased recidivism. Similarly, evidence suggests (Austin, 1986) that prisoners who had shorter stays (because of early release caused by over-crowding) had lower rates of rearrest in the 1 year period after release than those individuals who served their full prison terms. This result, however, may be attributable to differences among those prisoners selected for early release as much as to any other factor (Austin, 1986).

THE UNIQUE FEATURES OF THE JUVENILE COURT

Two important caveats prevent us from mapping the above results to the context of juvenile response to sanctions. First, as mentioned, it is impossible to make a *causal* interpretation of the results above regarding the offenders' response to harsher sanctions. The observed associations between harsher sanctions and future recidivism could simply be the by-product of harsher sanctions being assigned to those criminals with a higher propensity to reoffend. Second, and more fundamentally, the juvenile system is, by design, qualitatively different from the adult system. As Allen (2000:*x*) notes, from its inception, the juvenile court was designed to be "wholly distinct from criminal courts in form, procedure and purpose." Even with the recent changes in favor of more strict sanctions in the juvenile court (Jenson and Howard, 1998), inherent differences between the respective missions of the more punitive-oriented adult court and the more rehabilitation-oriented juvenile court prevent a direct comparison (Cullen, Golden, and Cullen, 1983; Platt, 1977).

In contrast to the formalism of sentencing in the criminal courts, in which offense and offender characteristics often are translated into sentencing systems based on retribution and crime-

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control interests, dispositions in the juvenile court reflect a complex integration of theoretical, legal, and policy interests (Ainsworth, 1991; Feld, 1991; Stapleton, Aday, and Ito, 1982). In fashioning dispositions for juvenile offenders, juvenile court judges are challenged to balance the interests of rehabilitation and reintegration with both retributive and public safety concerns while managing systemic constraints and externalities in the local context.

These interests bear on decisions in which juvenile offenders are placed in nonsecure or secure confinement and for how long they will stay there. First, demands for penal proportionality create popular and political preferences for lengthy periods of secure placement when minors commit serious crimes (Zimring, 2000a, 2000b). Although this has led to an expansion of juvenile waiver activity (Fagan, 2008), the demand for punishment in the juvenile court also has led to dispositional regimes that include mandatory minimum sentences, sentencing guidelines developed by sentencing commissions or corrections authorities, and determinate or fixed sentences based on a sentencing "grid" (Feld, 1999; Torbet and Szymanski, 1998). Second, crime-control interests, which include deterrence and incapacitation, influence dispositional decisions by challenging judges to calibrate the lengths of placement that can maximize their impacts on future criminal activity (Feld, 1999).

But rehabilitative and therapeutic interests also are part of the calculus of dispositional decisions in the juvenile court and create a tension with punitive interests. The core philosophy of the juvenile justice system historically has been individualized justice, in which judges craft "custom-made" dispositions to youths based on a case-by-case consideration of each youth's distinct rehabilitative needs (Connell, 1980; Feld, 1991; Horwitz and Wasserman, 1980; Zimring, 2000c). Whether these extralegal factors, such as substance abuse, mental health, school attendance, or family dysfunction, play a primary or secondary role in dispositional decisions depends on their interplay with legal factors (Campbell and Schmidt, 2000; Cauffman et al., 2007; Niarhos and Routh, 1992). When offenses are severe, legal factors may have primacy in dispositional decisions. And judges also may attempt to balance these therapeutic concerns with the risk of future criminality. For example, the severity of a juvenile's sanctions for prior adjudicated offenses significantly influences the likelihood that a youth is placed in secure confinement (Cauffman et al., 2007).

Externalities, or systemic constraints, also may influence dispositional decisions, with the social context of the court being particularly influential (Britt, 2000). Courts located in communities with strong social control may be more willing to avoid placing individuals. Judges may wish to avoid placement for a youth with specific service needs but find that the absence of local services increases recidivism risks and, thus, skews dispositional decisions toward placement in either secure or nonsecure settings. The reverse also may be true; judges may elect placement precisely because intervention services are available that are not available locally or are inaccessible for administrative reasons.[1]

[1]All this information suggests that the juvenile system is more complex than the adult system in that it includes intricacies that affect the outcome of a case (i.e., placement decisions) beyond a verdict of guilt. An anonymous reviewer argued that rarely is it stated that a youth is sent to a residential facility to be deterred, in part because judges are hesitant to remove a youth from a family environment unless the environment itself is unhealthy or the youth can receive greater benefit from placement elsewhere. More often, factors that determine placement outside the home include an objective and subjective determination of the level of needs with respect to treatment, the availability of treatment or other resources in the youth's community, the status of the youth's family—which includes the criminal involvement and/or drug use of the parents—the ability of the youth to be transported to treatment programs if they are not placed outside the home, and so on. Thus, the presence or absence of an effect based on the length of stay may have to do more with the type and fidelity of treatment received in an institution rather than whether the youth perceived the placement to be "bad enough" to deter them from future delinquency. In short, the reviewer is correct in highlighting the importance of considering information on the treatment received when studying issues related to placement and length decisions in juvenile decision making, because many factors exist beyond experiencing a sanction that could produce any observed effect.

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ASSESSING THE EFFECTS OF INSTITUTIONAL PLACEMENT IN THE JUVENILE SYSTEM

Few studies have been performed that estimate the impact of sanctioning experiences in the juvenile system on the future behavior of youth. As Levitt (1998:1158) concedes, "in contrast to the well-developed literature on deterrence and incapacitation effects of the adult criminal justice system, there is remarkably little previous academic research on the response of juvenile crime to sanctions." The work to date on juvenile sanctions often has focused on changes in overall crime patterns concurrent with changes in transfer policies, and this work has sometimes produced conflicting results. Levitt (1998) posits some deterrent effect, as opposed to a mere incapacitation effect, to more serious sanctioning of juveniles, based on the decline in state-level crime associated with respective state differences in the age of majority. Similarly, Mocan and Rees (2005) attribute higher arrest rates across different locales with reducing the probability of engaging in serious crimes, such as assault, theft, and selling drugs. Analyses of longitudinal data involving individuals released from California Youth Authority institutions demonstrate that increased sanctions have had a crime reduction effect (Haapanen, 1990; Haapanen, Britton, and Croisdale, 2007). Conversely, others (Jensen and Howard, 1998; Lee and McCrary, 2005; Steiner, Hemmens, and Bell, 2006) find no significant changes in patterns of violent youth crime over time that correspond with policy changes, which shifts the juvenile court to a more punitive focus.

Few studies have been performed that estimate the impact on rearrest of varying lengths of institutional placement in the juvenile system. Several meta-analyses (Andrews et al., 1990; Garrett, 1985; Lipsey, 1999) have indicated that treatment in the juvenile system can lower the likelihood of rearrest, although institutional programs have only limited impact compared with community-based programs. In a more recent meta-analysis of interventions for serious juvenile offenders based on an analysis of 83 institutional outcome reports, Lipsey, Wilson, and Cothern (2000) found that the length of institutional treatment was related to the size of the treatment effect, with longer treatment (above an average of 25 weeks) producing larger decreases in rearrest. However, their findings also indicate that general program characteristics (e.g., how established the program is and the qualifications of those administering the treatment) were more related to the effect size than to the type or amount of treatment.

CONCEPTUAL ISSUES

The ability of the juvenile system to reduce criminal activity can rest on several possible mechanisms. As a result, any prediction about how increased sanctioning in the juvenile system might affect later criminal involvement is neither conceptually simple nor straightforward. Intervention in this system involves deterrence (rational choice) and developmental (rehabilitative) components, and the effects are likely more complex than a simple additive or multiplicative function. Different theoretical perspectives can lead to different predictions regarding the likely outcomes from increased juvenile justice sanctions (Sherman, 1993).

The rational choice framework would suggest that longer durations of confinement will reduce the probability of reoffending (Clarke and Cornish, 1985; Nagin, 1998), as placement out of the community is a price worth avoiding. Institutional confinement carries the costs of material restrictions and freedom restrictions (Fagan and Piquero, 2007) as well as possible victimization or labeling (Smith and Paternoster, 1990). Similarly, longer durations of exposure to rehabilitative services of the juvenile system should reduce the chances of subsequent crime (Andrews et al., 1990; Lipsey, 1999; Lipsey and Wilson, 1998), with these effects more pronounced in situations where more offenders are placed in private, instead of public, institutions (Pratt and Maahs, 1999; Shichor, 1998). Overall, it is not clear whether these effects interact, but they certainly both point in the same direction of more time in institutional placement lowering subsequent criminal involvement.

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However, one can also posit a theoretical argument that longer stays in correctional placement may increase the probability of reoffending. First, longer exposure to noxious environments may be criminogenic (Agnew, 1992), undermining any potential positive effects of treatment. Second, psychological and developmental factors, which are important factors related to the risk or amenability of an adolescent offender (Fagan and Piquero, 2007; Mulvey and Iselin, 2008), may change over an extended period of institutional care, reducing the impact of any intervention efforts. Third, the longer the length of stay, the more tenuous the link between the crime committed and the punishment becomes in the mind of the offender. If the offender loses this important connection, then the moral component of confinement is ultimately lost, which could potentially lead to perverse effects, such as defiance and resistance (Piquero, Langton, and Gomez-Smith, 2004). Without an incremental contingency relationship between action and punishment, the impact of deterrence is lost.

In sum, competing arguments can be mustered to posit either decreases or increases from more or longer institutional placements in juvenile justice. In addition, multiple forces are at work that could interact to produce a positive or negative outcome from longer institutional stays. As noted, the ambiguity produced by the competing arguments in place for prediction is exacerbated by the current lack of empirical results in this literature.

This article aims to advance our knowledge and offer some clarity to this issue. It provides a formal empirical assessment of the impact of both institutional placement itself and varied lengths of institutional placement on the rearrest rate of serious adolescent offenders. We do not provide an explicit test of the deterrence framework, but we do provide a first empirical step in how it may be tested. We consider the implications of our results for criminological theory, as well as the possible next steps for research in the Discussion section.

HYPOTHESES

The goal of the analyses presented here is to identify the following two related yet distinct treatment effects: 1) the *effect of placement*, as compared with probation, on subsequent rates of offending, and 2) the *marginal effect of length of stay in placement*. Specifically, we test whether recidivism rates are greater for those youths who receive institutional placement compared with those placed on probation and, among those placed in institutions, whether their subsequent rates of offending decline as a function of the length of stay.

DATA

This article examines a subset of the research participants recruited for the Pathways to Desistance study, which is an ongoing, longitudinal investigation of the transition from adolescence to young adulthood in serious adolescent offenders. Participants in the Pathways to Desistance study are adolescents who were found guilty of a serious offense (almost entirely felony offenses) in the juvenile or adult court systems in Maricopa County, AZ, or Philadelphia County, PA. These youth were ages 14 through 17 years at the time of the study index offense. A total of 1,354 adolescents are enrolled in the study, which represents approximately one in three adolescents adjudicated on the enumerated charges in each locale during the recruitment period (November 2000 through January 2003). The study participants completed follow-up interviews at 6-month intervals for the first 3 years of their study involvement and annually thereafter. This investigation considers information through the first 4 years of follow-up. Overall, the study sample comprises minority (44 percent African American and 29 percent Hispanic) males (86 percent) with an average of two [standard deviation (SD) = 2.4] prior petitions to court. More information regarding the rationale and overall design of the study can be found in Mulvey et al. (2004); details regarding recruitment, a description of the full sample, and the study methodology are discussed in Schubert et al. (2004).

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The current analyses exclude cases in which, as a result of the study index petition, either the petition was dismissed or the offender was transferred to adult court. This leaves a sample size of $N = 921$. We omit cases transferred to the adult system because, as noted, the adult and juvenile systems function differently, and experiences in these systems are qualitatively different. The purpose of the current study is to examine the marginal effect of institutional placement in the juvenile system. Of the 921 cases used for this analysis, 55 percent ($n_{prob} = 502$) were put on probation, and the remaining ($n_{place} = 419$, 45 percent) were sent to an institutional placement.

Most (62 percent) of the placed youth spent most of their initial disposition stay in a contracted residential setting (see Mulvey, Schubert, and Chung, 2007 for setting type definitions). The remaining youth were placed across a range of setting types, which include state-run secure facilities (19 percent), contracted residential settings with a mental health focus (14 percent), and others (5 percent). The mean and median lengths of stay for the 419 placement cases were 11.0 and 10.4 months, respectively, with a standard deviation of 6.7 months, which indicates a fair amount of variability.

MEASURES

OUTCOME VARIABLES

Rate of Rearrest: We use the rate of rearrest as a measure of subsequent engagement in criminal activity. Indicators of arrest prior to age 18 were based on reports of petitions to juvenile court recorded in the Juvenile Online Legal Tracking System used in Maricopa County and the juvenile court database in Philadelphia County. Probation violations before age 18 were excluded because they do not necessarily represent a new offense and may indicate local practices as much as adolescent behavior. Arrests after age 18 were based on merging the court record information from each jurisdiction with nationwide Federal Bureau of Investigation arrest records.

For adolescents given probation at disposition, the rate of rearrest is calculated as the total number of arrests divided by total time on the street for the period after disposition to the end of the 48-month follow-up period. For adolescents who were sent to an institution, the rate of rearrest is the total number of arrests divided by the total time on the street for the period after release from the institutional stay ordered at disposition until the end of the 48-month follow-up period. In each case, the rate was standardized such that the outcome would be in terms of yearly rate. The main benefit of this measure is that it controls for exposure time in the community, as opposed to either a binary marker for rearrest or the total number of arrests postdisposition, which may be confounded with exposure time. The importance of controlling for exposure time has been noted elsewhere (Piquero et al., 2001).

Rate of Self-Reported Offending: Because arrest data are an officially based measure of offending, they will be limited to the events that lead to official detection. Thus, we also investigate whether institutional placement is related to a self-reported offending (SRO) measure (Huizinga, Esbensen, Weiher, 1991). The SRO measure used in this study asks participants to report their involvement in 22 more serious antisocial and illegal behaviors. The information was collected at each follow-up interview to capture illegal activity reported for the recall period. The outcome used for these analyses reflects the rate of antisocial activity across the 48-month follow-up period. It is the summed value of the "variety scores" (Thornberry and Krohn, 2000) from the reports obtained from each time point interview through 4 years divided by the number of days in the community during that time. As with rearrest, this outcome includes only reported antisocial behavior that occurred in the months between the time of release from the initial disposition stay and the end of the 48-month follow-up period. The values were standardized to a yearly rate.

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We note the correlation between the SRO and the rearrest outcome is strong and positive ($\rho = .47$, $p < .01$ for all cases, and $\rho = .46$, $p < .01$ for only placement cases). This demonstrates that the two measures seem to be tapping the same underlying construct of involvement in illegal behavior, as they should given the strong concordance observed in the extant literature between self-report and official offending records (e.g., Brame et al., 2004). However, it also demonstrates that they are not perfect substitutes. Thus, to the extent that we arrive at substantively similar conclusions using these alternative measures, we can be more confident regarding the main findings of the article.

Covariates and Predictors: The main benefit of the Pathways data is it allows us to consider —and subsequently eliminate—a wide range of baseline variables as potential confounders related to selection bias. Specifically, we consider 66 covariates measured at baseline, which include demographic, familial, peer, legal, psychological, mental health, substance abuse, psychosocial maturity, and prior adjustment factors. Each of these was selected specifically to be ruled out as a potential confounder within the analysis, and because they have been found to be related to juvenile offending (Loeber and Farrington, 1998). We consider the potential for each to be a confounder in the next section. These covariates operationalize the influences of factors specified in an integrated theoretical framework for serious juvenile crime that incorporates individual, situational, developmental, and ecological influences on delinquency (Mulvey et al., 2004). More information on each of the 66 covariates, including descriptive statistics, can be found in appendix A.[2]

DESIGN ISSUES

Two factors complicate research on the question of estimating sanction effects in juvenile justice, and these have in all likelihood contributed to the lack of empirical results on this question. First, not much data are available for making solid estimates of these effects. Specifically, there is a relative dearth of panel data sets appropriate for analyzing serious juvenile crime at an individual level (Lee and McCrary, 2005; Levitt and Lochner, 2000). In addition, most existing longitudinal studies (e.g., National Longitudinal Survey of Youth) often do not fully characterize the intricate features of sanctions, such as type, length, and intensity, nor do they typically quantify the individual offender's response to them (Mulvey et al., 2004).

Second, even with appropriate data on individual sanctions, any attempt to examine the relations among sanctioning effects and future offending among adolescents in the justice system still faces complicated methodological issues. In particular, selection problems plague basic comparisons of outcomes between offenders with differing sanctions and limit the strength of any inferences that can be made. For instance, certain outcomes such as future recidivism may inherently depend on important, pre-existing characteristics of the individual, which may also be related to disposition decisions. Moreover, it is often difficult to address this selection problem because of the limited amount of information available regarding the case factors related to the type of sanction an individual receives (Mears and Field, 2000).

The Pathways to Desistance study provides a unique data set with which to circumvent both of these issues directly, as the data allow for the proper modeling of both sanctions and outcomes of individuals, as well as providing a wide range of background characteristics collected prior to the individual receiving sanctions. This richness allows us to incorporate propensity score methods (Rosenbaum and Rubin, 1983), as well as extensions to this methodology that allow for multiple treatments resembling a dosage (Lu et al., 2001; Zanutto, Lu, and Hornik, 2005). Using these approaches, we can rigorously control for selection biases,

[2]More information about the instruments and calculations used to generate these covariates can be obtained from the lead author.

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ruling out 66 potential confounders. Such an approach is rare in criminology generally and has not yet been applied to the theoretical and empirical issues addressed here regarding serious juvenile offenders. Nagin, Cullen, and Lero-Jonson (2009) have recently commented that such a dose-response approach is the proper way in which to model the relationship of longer sentence lengths on future recidivism to avert problems associated with traditional regression-based methods, but it has not yet been explored in most prior literature.

ANALYSES

The primary goal of this analysis is to identify two related yet distinct treatment effects—the *effect of placement* and, conditional on an individual being placed, the *marginal effect of length of stay* on future criminal offending. This section begins by exploring unadjusted differences in rates of rearrest and SRO between groups of individuals who receive different placements and lengths of stay, as well as preexisting differences in terms of baseline covariates between these same groups. We then introduce the propensity score methodology and relevant extensions necessary to adjust for these existing differences between the groups in calculating the effects for placement and increased length of stay.

UNADJUSTED COMPARISON OF OUTCOMES AND PLACEMENT

We begin by examining the mean outcome of rearrest conditional on placement status. For those individuals who were placed out of the community, the mean rate of rearrest was 1.20 arrests per year, postdisposition. In comparison, those who received probation had a mean rate of about .63 arrests per year. This unadjusted comparison of the probation group versus the placement group in terms of rate of rearrest shows that, on average, the placement group is rearrested at a higher rate than those who received probation, by approximately .57 arrests per year ($p < .001$). Similarly, for SRO, those individuals who are placed report, on average, about 2.5 more offenses per year of time in the community (10.9 vs 8.3 reported offenses per year) than those individuals who receive probation ($p < .05$). However, as we mentioned, preexisting differences likely exist between the placement and probation groups, which make a causal interpretation of this difference impossible. We consider these differences in more detail in the next section.

UNADJUSTED COMPARISON OF RATE OF LENGTH OF STAY AND REARREST

We also examine the unadjusted association between length of stay and future rate of rearrest. The plot of these two variables for only those 419 who were sent to institutional placement is shown in figure 1. Two things are apparent during visual inspection. First, there seems to be no strong relation between longer length of stay and future rate of rearrest. For verification, note that ordinary least squares (OLS), where the rate is the dependent variable and length of stay (in months) and its square are explanatory terms, yields neither linear nor quadratic slope terms significantly different from zero.[3] Second, figure 1 shows some interesting aspects of the distribution of length of stay, which may limit our ability to generalize any results. Dramatic heterogeneity is observed in the upper tail of the distribution, specifically beyond 15 or 16 months, where a few cases have long lengths compared with the rest of the group. Furthermore, many of these cases with lengths greater than 20 months have rates of rearrest that are zero. Given that we only observe a follow-up period of 48 months, it is likely that the outcome itself is confounded with length of stay for some of these extreme cases; offenders with longer lengths of stay simply cannot resume offending to the likely rate in the brief time they are exposed in

[3]Although we acknowledge that this simple specification is in no way intended to be an accurate portrayal of reality, we include it to highlight the basic relationship between dose and outcome. Thus, the insignificance of the linear and quadratic relationships of length of stay to rearrest are simply intended to demonstrate a lack of simple association but are not intended to test a de facto model of their relationship.

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the follow-up period. As we will discuss, we consider these data limitations a restriction on our ability to generalize the results on stays with lengths exceeding 15 months, which encompasses a small part of our data.

INITIAL IMBALANCES AMONG COVARIATES

The main problem in comparing the rearrest and self-reported offending outcomes across individuals who receive different sanctions centers on the idea that confounding factors may affect each offender's likelihood of receiving such sanctions. Furthermore, these factors may not only be associated with placement and/or length of stay but also with the individual's subsequent arrest or self-reported offending. Thus, the comparison of offenders who receive different sanctions involves a contrast of two groups that are dissimilar in important preexisting ways. In formal statistical parlance, because there is no random assignment to the treatment condition, the treatment and control groups are not directly comparable, unless these initial differences are taken into account.

We start by examining initial covariate *balance*, that is, differences between groups prior to entering any placement, to determine whether such observed differences must be corrected. To do this, we consider 66 covariates measured at baseline, each of which was selected to be ruled out specifically as a potential confounder within the analysis. We assess the imbalance in this set of covariates in two ways. First, we address imbalances in the placement versus probation groups by examining a standardized bias statistic, as advised by Rosenbaum and Rubin (1985), for a difference in means between these two groups for each of the covariates. The standardized bias statistic is the mean difference as a percentage of the average standard deviation, $100(\bar{x}_T - \bar{x}_C)/[(s^2_T + s^2_C)/2]^{1/2}$, where for each covariate, $\bar{x}_T$ and $\bar{x}_C$ are the sample means in the treated group and the control group, respectively, and s^2_T and s^2_C are the corresponding sample variances. Rosenbaum and Rubin indicate that a standardized difference percentage value greater than 20 for any covariate would suggest that the covariate is out of balance. Second, in this comparison of probation versus placement, we compare the differences in covariates between each group in terms of an ordinary difference in means test (*t*-statistic). A *t*-statistic exceeding an absolute size of 1.645 would correspond to a significant difference in the sample means at $\alpha = .10$, and for our purposes, it would be considered to be out of balance. Second, to assess initial imbalances among the length of stay, we consider a simple correlation coefficient ρ for each covariate with length of stay (conditional on the placement cases), as well as the associated *p*-value for a test of no association, where a *p*-value less than .10 would suggest an initial imbalance.

Table 1 reports these values for each of the 66 covariates considered. Prior to treatment (i.e., placement), 41 of the 66 covariates considered are initially out of balance in the probation versus placement comparison. Furthermore, the imbalance between placement and probation groups is on covariates that are likely related to future offending (e.g., number of priors, exposure to violence, personal cost of punishment, personal certainty of punishment, and psychopathy). In the length of stay comparison, 28 of 66 covariates are out of balance. Again, these preexisting differences are on covariates (e.g., offending history, exposure to violence, legal cynicism, and association with antisocial peers), which are arguably related to both length of stay and future offending. This seems to suggest a high degree of potential selection bias, which may contaminate basic comparisons of effects in both of these cases.

BALANCING TREATMENT AND CONTROL GROUPS—PROPENSITY SCORES

The two estimations for this study require different methods to compute propensity scores. In the case of evaluating the *effect of placement*, the treatment is binary (i.e., placement versus probation), and thus we can use traditional propensity score methods (Rosenbaum, 2002; Rosenbaum and Rubin, 1983) to eliminate bias caused by observable covariates. In the second

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case of evaluating a *marginal length of stay effect*, the treatment, length of stay, is no longer binary, but it can be thought of as resembling a dosage. Thus, we employ recent extensions to propensity score methodology (Lu et al., 2001; Zanutto, Lu, and Hornik, 2005) to account for selection biases in this case. We briefly examine both of these methods in more detail.

PROPENSITY SCORES FOR PLACEMENT—The propensity score represents the probability that an individual receives some treatment conditional on a vector of observed covariates (Rosenbaum, 2002). Rosenbaum and Rubin (1983) show that, conditional on two individuals, one treated and one control, with an identical propensity score, the difference in treatment status becomes independent of all observable characteristics, which is the assumption of a *strongly ignorable treatment assignment*. The idea is to estimate a propensity score for each individual and in turn to use this estimate as a method for creating balance on key covariates that may be confounding the treatment effect estimate. To estimate the propensity score, we employ a binary logit model, with the binary outcome of institutional placement (versus probation), and some combination of the 66 initial covariates, including squares and interactions, as explanatory terms. We treat model selection as an iterative process, where parsimony is unimportant as compared with achieving a covariate balance afterward (Heckman et al., 1998; Rosenbaum and Rubin, 1984). The predicted probability from the final model for each individual $\hat{e}$ (x) is thus that individual's estimated propensity score.

After estimating a propensity score for each individual, instead of conventional matching, we employ stratification, where subjects are divided into equal-sized subgroups based on the propensity score distribution. Stratification can be thought of as a special form of matching where subjects are grouped, rather than paired, with other individuals within a certain range of propensity scores. Rosenbaum and Rubin (1984) contend that stratification using quintiles (i.e., five equally sized subgroups) can remove approximately 90 percent of the initial imbalance in each baseline covariate. The subsequent average effect of treatment on the treated can be estimated as a weighted average of within-stratum probation-minus-placement mean differences in outcome *Y*:

$$ATT_{place} = \sum_{j=1}^{J} \frac{n_{place,j}}{n_{place}} \left[\overline{Y}_{place,j} - \overline{Y}_{prob,j} \right] \quad (1)$$

where there are $j = 1,\ldots, J$ number of strata, $n_{place,\,j}$ and n_{place} represent the total number of placed individuals in stratum j and overall, respectively, and $\overline{Y}_{place,\,j}$ and $\overline{Y}_{prob,\,j}$ denote the mean rate of reoffending for placed and probation individuals, respectively, in stratum j.

Dosages: Propensity Scores for the Length of Stay: Unlike the initial analysis of placement, in which treatment is binary (i.e., placement versus probation), estimating the effect of longer lengths of stay requires modeling treatment in a form that resembles a dosage. Thus, a modification of traditional propensity score methods becomes necessary. Lu et al. (2001) extend the basic propensity score framework described above to accommodate treatments with multiple doses. Zanutto, Lu, and Hornik (2005) extend this work to show how subclassification (i.e., stratification) can be used with treatment doses to estimate a dose-response curve. In the current application of length of stay, we restrict the analysis to the $n_{place} = 419$ cases who actually received placement as a sanction. This is done to differentiate those individuals who received probation, who technically have a length of stay equal to zero, as having received a unique sanction, and thus avoid the introduction of an additional and highly problematic selection problem into the analysis.

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When considering the length of stay, an individual receiving a longer length of stay can be thought of as being treated with a larger "dose." The distribution of dose propensity can be modeled using an ordinal logit model (McCullagh, 1980), generating a single balancing score, similar to the binary treatment case (Joffe and Rosenbaum, 1999). In this model, this distribution of doses for some individual i, D_i, conditional on a vector of observed covariates $\mathbf{x_i}$, can be written as follows:

$$\log\left[\frac{P(D_i \geq d)}{P(D_i<d)}\right] = \alpha_d + \boldsymbol{\beta}\mathbf{x_i}, \text{ for } d = 2, 3, 4, \ldots \tag{2}$$

In this model, the distribution of doses given covariates depends on the observed covariates only through $b(\mathbf{x}_k) = \beta\mathbf{x_i}$, so that the observed covariates $\mathbf{x}$ and the doses D are conditionally independent given the scalar $b(\mathbf{x}_i)$. Thus, $b(\mathbf{x}_i)$ is a balancing score and conditional on $b(\mathbf{x}_k)$; the dose D is assumed to be strongly ignorable. Therefore, after matching (as described in Lu et al., 2001) or subclassifying (as described by Zanutto, Lu, and Hornik, 2005), which we employ in the current study, we can balance the distribution of a large number of covariates simultaneously as we would in the case of binary treatment. Also, as in the binary case, we use an iterative approach for model selection to maximize covariate balance after stratification. A dose-response curve can then be estimated, as a stratum- weighted mean of rearrest conditional on receiving dose D. After subclassification on balancing score quintiles, the estimated mean response for individuals receiving dosage d can be written as follows:

$$\overline{Y}_d = \sum_{i=1}^{5} \frac{1}{5} \overline{Y}_{d,j} \tag{3}$$

where $\overline{Y}_{d,j}$ is the observed mean outcome among individuals receiving dosage level d in balancing score quintile i.

DEFINING TREATMENT DOSES

To apply the methods described by Lu et al. (2001) and Zanutto, Lu, and Hornik (2005) to the length of stay analysis, it is necessary to define discrete dosage categories of length of stay, or comparable segments for analysis. Figure 2 shows a histogram of length of stay in months. As an initial cut, we use rough adherence to the quartiles of this distribution, which in turn gives us four dosage categories of 1) 0–6 months, 2) 6–10 months, 3) 10–13 months, and 4) >13 months. However, given that the delineation of these categories is rooted in the data and not necessarily in practice, we also consider an alternative option, which defines doses in five 3-month intervals of 1) 0–3 months, 2) 3–6 months, 3) 6–9 months, 4) 9–12 months, and 5) >12 months. These categories, although more attractive in a practical sense, are not all of similar sizes. Lu et al. (2001), however, propose the use of five discrete dosage groups of unequal size membership, which would justify this choice. We estimate a response curve in each of these instances.

HETEROGENEITY IN PLACEMENT

An additional yet critical factor to consider when modeling juvenile sanctioning is the heterogeneity among institutional settings. For instance, placement in a state training school is likely a different experience from placement in a contracted residential mental health facility. These differences are likely confounded with both length of stay and outcome.

We cannot completely stratify by facility type and conduct unique analyses by each type because of sample size limitations. We can, however, examine how two broad differences in

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the characteristics of settings that may potentially affect our results—intensity of services provided and public versus private status of the institution. Intensity of service provision is a relevant consideration, because prior literature indicates that any type of sanctioning without the provision of rehabilitative services does not reduce future recidivism (Andrews et al., 1990; Lipsey, 1999; Lipsey and Wilson, 1998). In addition, there is some debate that economic motivation for profits may lead to quality differences between public and private institutions (Pratt and Maahs, 1999; Shichor, 1998). However, Armstrong and MacKenzie (2003) provide evidence that no such environmental differences exist between public and private institutions when assessing juvenile cognitive outcomes.

Given that exposure to services occurs *simultaneously* with the statistical treatment effect of interest, the length of stay, we cannot simply treat it as an ordinary covariate over which we may achieve balance and thus rule out as a confounder (e.g., number of prior convictions). Similarly, it could be argued that if material differences were found between environments in public and private facilities, then this would, in effect, constitute different treatments (in a statistical sense) altogether. As such, we consider these differences in a cruder manner than we do with length of stay.

First, for a subset of individuals, we can observe *intensity of service exposure* while in placement. To measure intensity of services, a modified version of the Child and Adolescent Services Assessment (CASA; Burns et al., 1992) was administered during each follow-up interview with the adolescent. The CASA is designed to assess the use of mental health and social services via self-report from youth ages 8–18 years. The instrument collects information regarding the occurrence of an institutional stay and the length of the institutional stay via self-report (Ascher and Farmer, 1996). If an institutional stay of longer than 7 days was reported, then information is also obtained about the types and frequency of services provided during the stay. A measure of the intensity of services provided during the institution stay ordered at disposition was calculated by dividing the total frequency of the services reported by the total number of days in that institutional stay. Because early versions of the interview did not contain the information needed to generate this score, a value for the intensity of services received was only available for a subset of study participants ($n = 289$). Second, for the entire placement sample ($n = 419$) we grouped our facilities into two broad types: public (i.e., jails, prisons, and state-run secure facilities) or private (i.e. contracted residential and drug and alcohol facilities).

Table 2, panel A, reports how each factor is associated with the future rate of rearrest using OLS. Column 1 shows no significant linear or quadratic association of length of stay to future rate of rearrest, as discussed.[4] Column 2 shows that after controlling for length of stay, there is similarly no significant association with intensity of service exposure. Columns 3 and 4, however, do yield an interesting result, with an intensity–length of stay interaction significant at $\alpha = .05$, when also controlling for both the linear and quadratic factors. The striking thing here is the *positive* sign of the interaction effect, which shows that continued higher exposure over time is actually associated with a *higher* rate of future rearrest, which suggests that a strong selection mechanism for sentence length must be considered when discussing service exposure. This result is reinforced in table 2, panel B, which reports similar analyses using rate of self-reported offending as the outcome. Again, in this case, we notice a marginally significant and positive effect of the length and intensity interaction. As such, in the context of the current analysis of length of stay, we cannot effectively rule it out as an explanatory term in the process. We consider the implications of this in the Discussion section.

[4]Again, we must point out the limitations of these simple OLS models in testing a proper relationship between service exposure and future recidivism. The statistical tests of the coefficients presented in table 2 are merely intended to show that a basic relationship between service exposure and future recidivism, conditional on length of stay, cannot be ruled out, and is not de facto evidence that such a statistically significant relationship actually exists. Indeed, we advocate for future research to develop a more formal, explanatory model of this relationship.

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Columns 5 and 6 include the entire sample stratified on the public or private delineation. In neither case is there a significant linear or quadratic relationship with length of stay and either future rate of rearrest or self-reported offending. We assume, then, that this distinction is not relevant in our interpretation of the results.

RESULTS

COVARIATE BALANCE AFTER STRATIFICATION

After stratification on the estimated propensity score, it is necessary to reassess covariate balance in both the placement group comparison and the group stratification by dose of length of stay. Essentially, we need to determine whether, after stratification, any covariates still can strongly predict placement instead of probation (or, in the length of stay case, predict who gets a longer length of stay). If so, there could still be selection bias attributable to out-of-balance confounders, and thus, the propensity score must be reestimated, in a model selection process described above, until balance is finally achieved. Again, it is important to remember that in each case, covariate balance, and not model parsimony, is the primary goal of estimating the propensity score (Rubin and Thomas, 1992).[5]

To reevaluate balance in the case of placement versus probation, we employ two-way analysis of variance (ANOVA) for each covariate, where binary exposure to treatment (i.e., placement) was one factor, the propensity score quintile was a second factor, and the covariate itself was the dependent variable. If balance is achieved, then there should neither be a statistically significant main effect of placement on the covariate nor a statistically significant interaction effect of exposure by quintile. Any F-statistic exceeding 3.84 would suggest a significant effect and, hence, that the covariate was still out of balance. If these two conditions are not met, the propensity score was reestimated by adding quadratic or interaction terms with those covariates that remained out of balance to the propensity score model specification. In the case of placement versus probation, after stratification on the final propensity score specification, 64 of the 66 covariates balanced, which means that we may reasonably rule these covariates out as confounders. Notice that this is actually *better* balance among observable covariates than one might expect from randomization, because, at $\alpha = .05$, we would expect about 3 of these 66 to be out of balance.

We also need to evaluate balance after stratification in the length of stay application. Again, we check this for each covariate by using a two-way ANOVA with dose and propensity score quintile as the two factors and the covariate as the dependent variable. Again, if balance is achieved, then there should neither be a statistically significant main effect of dose on the covariate nor a statistically significant interaction effect of dose by quintile. As with the placement application, the propensity score model was reestimated in an iterative manner until reasonable balance was achieved. Again using the $F > 3.84$ threshold to gauge balance, after stratification with the final model, 65 of the 66 covariates were balanced, meaning they can be reasonably ruled out as confounders. Once again, this is better balance among observable covariates than we would expect through randomization.

EFFECT OF PLACEMENT ON OUTCOMES

After accounting for potential selection biases in the 66 covariates, the magnitude of the difference in mean rate of rearrest between the placement and probation groups falls considerably, from .570 in the unadjusted comparison to .167, which is a reduction of about

[5]It is also important to note, however, that no statistical or econometric methodology can match the power of a pure experiment to identify effects; and although we can reasonably eliminate a wide range of observable covariates as confounders, there may still be hidden biases confounding our results (Rosenbaum, 2002). Yet, given the richness of pretreatment covariates, we are confident that we eliminate substantial amounts of likely bias.

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70 percent. Furthermore, the associated *p*-value for this effect is .115, which suggests that, although not significant at conventional levels, this effect of placement is actually still negative, indicating an *increased* effect on the future rate of rearrest for placement compared with probation. It is important to note that, even if this difference were statistically significant, the magnitude is so small (that is, we would expect the placed offenders to have an extra rearrest only about once every 6 years compared with offenders on probation) that it is also not significant in a policy sense. However, the more important implication from this null finding would seem to be that, even after accounting for selection effects, the results show no marginal *gain* from placement in terms of averting future offending. Similarly, the difference in mean rate of SRO falls from about 2.5 in the unadjusted comparison to .31 after stratification ($p = .81$). Again, we uncover no significant differences in SRO rates across the two groups, replicating the null effect that we observed using the rearrest outcome.

LENGTH OF STAY EFFECT ON OUTCOMES

After stratification, we estimate the conditional expected rate of rear-rest as a stratum-weighted average for each dosage level. Note that it is not straightforward how to conceptualize an average effect of treatment on the treated in the multiple dose case. Rather, a point on this curve can be thought of as a conditional expectation relative to the other dosage levels, and thus, this set of conditional expectations yields what we may interpret as a dose-response curve. Figure 3 reports response curves for both rearrest and self-reported offending outcomes for doses as defined by the quartiles. Notice that the shapes of these curves are both essentially flat, at least through 13 months. Not only are these differences not statistically significant, but also the very subtle magnitude of these differences suggest there is no practical benefit, even if they were statistically significant. [6] This suggests there is little or no marginal benefit, at least in terms of reducing future rate of offending, for retaining an individual in institutional placement longer. Although there does seem to be a slight reduction after 13 months, we are cautious about making an inference for this group, because, as noted, there is large heterogeneity in length as well as potential confounding with the outcome (because of a lack of reasonable exposure time) in this highest dose group.

Figure 4 reports response curves for the case where doses were defined in terms of 3-month intervals. Again, this more differentiated picture of the length of stay may be a more policy-relevant way in which to consider the dose delineation. As was the case when using the quartiles to define dose groups, there again seems to be flat response curves for both rearrest and SRO in terms of the intermediate lengths. However, notice in this case, there does seem to be somewhat of an effect for staying longer than 3 months, as both the rate of rearrest and SRO drop thereafter by a non-trivial amount. Although it is apparent that there may be some mechanism at work here, we are again restricted in our ability to interpret it fully as the number of observations in the 0–3-month dose group is too small to provide adequate statistical power with which to draw sound inferences.

In both these cases, it seems that, for lengths of stay between 3 and 13 months, there is no marginal benefit for retaining an offender in institutional care for longer periods of time. Although we cannot make solid inferences outside this range, this result is nonetheless dramatic.

[6]Zanutto, Lu, and Hornik (2005) note that although there is a manner in which to estimate corresponding standard errors for estimated points on the response curve, these estimates are inherently biased because of a dependency of responses based on the nature of the propensity score calculation. However, they may be reasonable approximations (Benjamin, 2003). Thus, we do not report standard errors for the points on the response curve given not only the above caution, but additionally, considering the small magnitude of the differences between point estimates, we believe that it would be highly unlikely to reject a null hypothesis of no difference.

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DISCUSSION

This article extends the discussion of how serious juvenile offenders respond to placement and longer stays out of the community. We show two distinct, yet related, results. First, we find that after rigorously controlling for selection, there is an overall null effect of placement on future rate of rearrest and future rate of self-reported offending. Furthermore, if any effect of placement is present, it is likely slightly negative, on average, raising the likelihood of rearrest. Second, conditional on an individual receiving placement, there appears to be no marginal benefit, in terms of reducing future rate of rearrest or rate of SRO, for additional length of stay; the conditional response curve is essentially flat, at least through 13 months. In addition, we find no significant variation in outcome, at least at a macro level, for public versus private facilities, in contrast to some earlier literature (Pratt and Maahs, 1999). These results certainly raise questions about whether the rehabilitative mission of the juvenile justice system is being met efficiently or effectively.

These results have ramifications on the direction of the juvenile court, which is tending toward a more punitive-oriented orientation (Jenson and Howard, 1998), with “get-tough” policies that are perhaps not as entirely effective as hoped for by proponents. Although this analysis is not presented as a formal test of the deterrence framework, it does align with the thesis that there is little benefit from increased deterrence connected with either placement out of the community or longer stays in institutions. It is still an open question whether this is the result of a belief among potential juvenile criminals that the sanctions they will likely face in the juvenile system are “easy,” as Levitt (1998) argues, but these results do nothing to dispel such a proposition. Moreover, if there is no deterrent effect of the juvenile system, it becomes even more imperative that a discussion of the effects of juvenile sanctioning must be framed in terms of the marginal rehabilitative benefit of the system. Namely, we would expect there to be an additional benefit in terms of normal reintegration into non-criminal activity for those serious juvenile offenders who are exposed to the system to a greater degree.

These findings also have implications for the broader issue of costs and benefits from the use of institutional care in juvenile justice. If there is no marginal rehabilitative effect for placement or longer exposure, it calls into question the need to expend resources on extended institutional care. A full cost–benefit analysis of these results, however, would be a complicated task. As Austin (1986) showed with data regarding the effects of early release in Illinois, a considerable economic gain was realized by downsizing the placement population. Even though this result was applied to the adult prison system, parallels can be drawn to the juvenile justice system in a very literal economic sense. As Austin points out, the huge economic savings are reduced by the cost of the crime attributable to these early release cases. If, as our results suggest, there is no evident gain from reduced crime, then one might argue there would be little cost to offset the economic-operating cost benefits. We urge readers to be cautious of such an interpretation, however, given that, even if there is no marginal rehabilitative benefit to reducing future crime, there is still an inherent incapacitation effect with placing serious adolescent offenders. This effect was not estimated in the current analyses, and any full assessment of the implications of the current findings would have to do so.

It is also important to remember that these analyses only considered outcomes related to reoffending. The effects of placement on many non-crime, developmental outcomes critical to positive adjustment in early adulthood are generally unknown (Scott and Steinberg, 2008). It may be that, even though this analysis attempts to balance over age at placement, there are substantive, underlying differences based on age and maturity that contribute in unique and offsetting ways to the overall null effect observed. Steinberg and Cauffman (2000) argue that, in the context of the boundaries of the juvenile court, developmental differences among offenders may lead to different responses, either positive or negative, to an otherwise uniform

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treatment. For example, placement may be problematic if it labels, and thus stigmatizes, less serious offenders and prevents their successful reintegration into roles connected with a successful transition into nonoffending adulthood. Furthermore, the prior point of an economic cost–benefit at some level must also consider these developmental consequences, as certain outcomes such as delayed development or labeling issues, which might affect items such as future labor market participation, must also be considered as potential economic costs or even possible dead-weight loss. If there are such developmental costs, then any economic analysis that does not include them is likely too muted an estimate of impact. An interesting extension to the current analysis would be to examine how certain developmental outcomes are affected by placement and longer lengths of stay.

An additional methodological note involves the choice to test the dose-response relation using discrete categories for the length of stay variable, as in the Lu et al. (2001) and Zanutto, Lu, and Hornik (2005) extensions to the propensity score literature. An alternative would be to treat the length of stay as a purely continuous treatment and, thus, to estimate a smooth function as the derivative of the response function, an approach taken by Hirano and Imbens (2004). We choose the discrete approach for two reasons. First, given its general similarity to the basic binary treatment case that we employ in the placement effect analysis, it is a relatively easily understood and straightforward extension. Second, although the idea of modeling a continuous treatment is conceptually appealing, it is unlikely that juvenile justice professionals making determinations about the need for longer institutional stays would be thinking in such a manner, as compared with more discrete chunks—like months. Review hearings regarding extended placements occur at regular intervals, and juvenile justice system professionals often use blunt assessments regarding the need for continued treatment involvement (Mulvey and Iselin, 2008). We must strongly emphasize, however, that given our results show a flat response curve, it is unlikely that a different modeling approach of the length of stay effect would have made a substantial, if any, difference in the substantive interpretation of the results.

One important caveat to our findings is that we cannot rule out intensity of exposure to rehabilitative services as a confounder. Given the contention that the provision of an adequate amount of rehabilitative services is perhaps the most important factor to reduce future recidivism (Andrews et al., 1990; Lipsey, 1999; Lipsey and Wilson, 1998), this is one potential limitation of these results. There is some prior evidence from this study that exposure to substance use services may reduce both crime and self-reported offending (Chassin et al., 2009; Mauricio et al., 2009). Our analyses of this issue, performed with only a subsample of the larger group and a gross measure of treatment involvement, indicate that longer stays with more intensive services seem to increase the risk of rearrest. It is possible that longer exposure to services of poor quality, or services that are poorly matched to the individualized needs of incarcerated offenders, may have iatrogenic effects. But these results could also be the result of a selection effect in which adolescents with a high propensity for future offending spend greater time in high-intensity environments, but to no avail. Considering the complexity of properly conducting an analysis of the effects of treatment intensity as well as its profound implications, the question of the true role of service exposure is a high-priority issue for future research.

We hope that this effort begins a course of research on the effects of institutional placement generally, and the dose of institutional confinement in particular, with respect to policy decisions regarding juvenile offenders. Our research shows a general lack of support for lengthy periods of placement and indirectly underscores the movement toward increased use of non-placement/community-based alternatives, especially for those offenders who do not evince the highest risk (Greenwood, 2005). Given the small increases in crime currently being observed among this population, the growth in the number of such individuals in the coming years, and the significant costs associated with (lengthy) incarceration decisions, a more accurate account

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of whether such policies lead to more or less subsequent criminal activity is of central theoretical, empirical, and policy significance.

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Biographies

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Jeffrey Fagan is a professor of law and public health at Columbia University and Director of the Center for Crime, Community and Law at Columbia Law School. His research focuses on crime, law, and social policy. His current research examines capital punishment, racial profiling, legal socialization of adolescents, the jurisprudence of adolescent crime, and the legitimacy of the criminal law. He has served on the Committee on Law and Justice of the National Academy of Science and the MacArthur Foundation's Research Network on Adolescent Development and Juvenile Justice. He is a fellow of the American Society of Criminology.

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Sandra H. Losoya is a research assistant professor of psychology at Arizona State University. She received her PhD at the University of Oregon and studies emotional development and sources of risk and resilience in children and adolescents. A recent publication is as follows: Losoya, S. H., Knight, G. P., Chassin, L., Little, M., Vargas-Chanes, D., Mauricio, A., and Piquero, A. 2008. Trajectories of acculturation and enculturation in relation to binge drinking and marijuana use in a sample of Mexican-American serious juvenile offenders.

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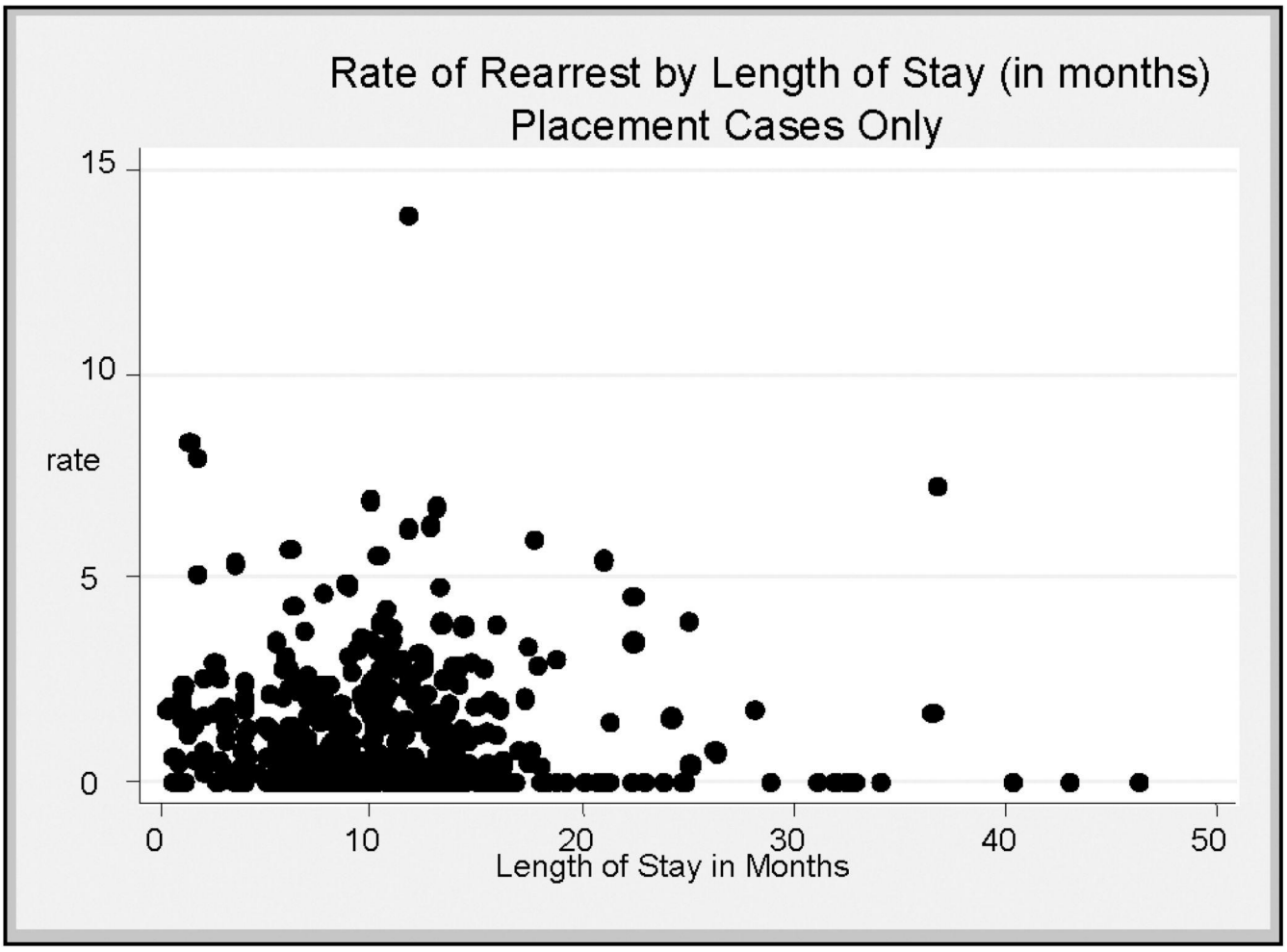


Figure 1.
Rate of Rearrest versus Length of Stay

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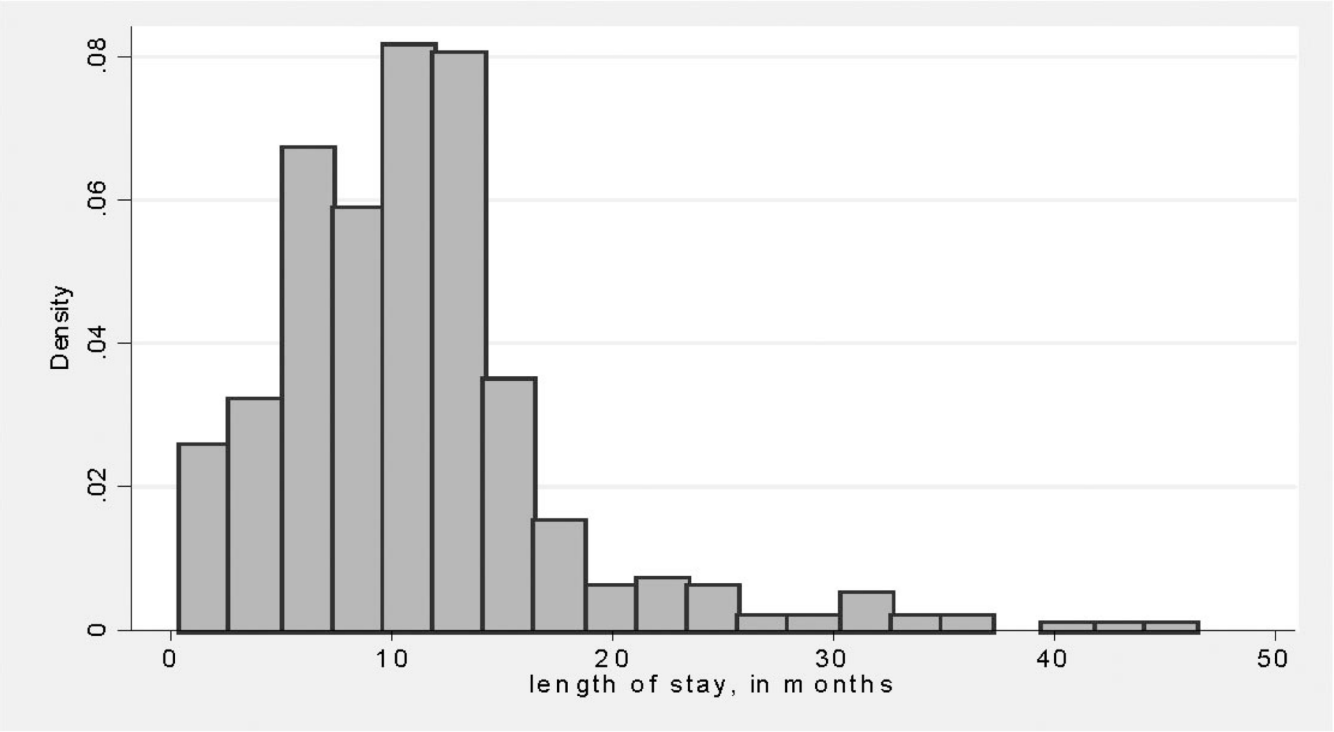


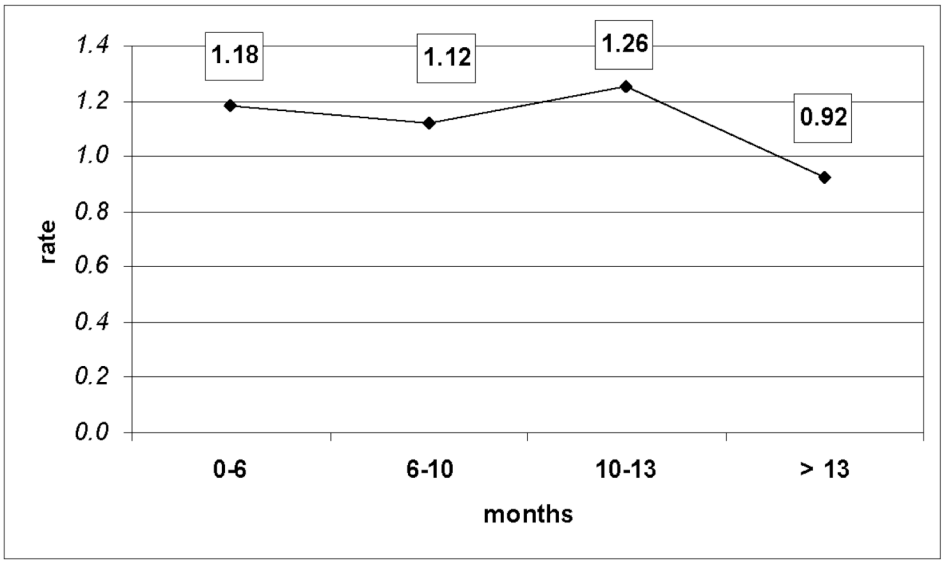
Figure 2.
Histogram of Length of Stay (in months)

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A. Expected Rate of Rearrest



B. Expected Rate of Self-Reported Offending

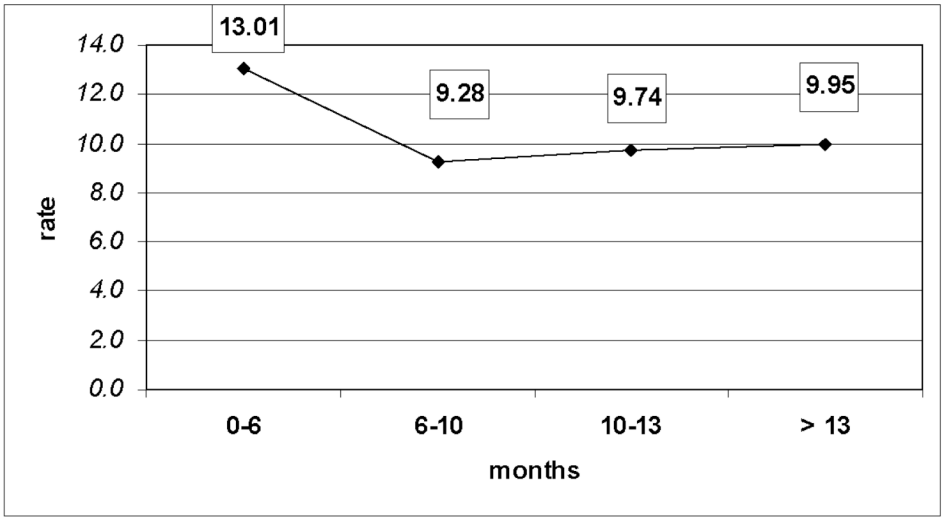


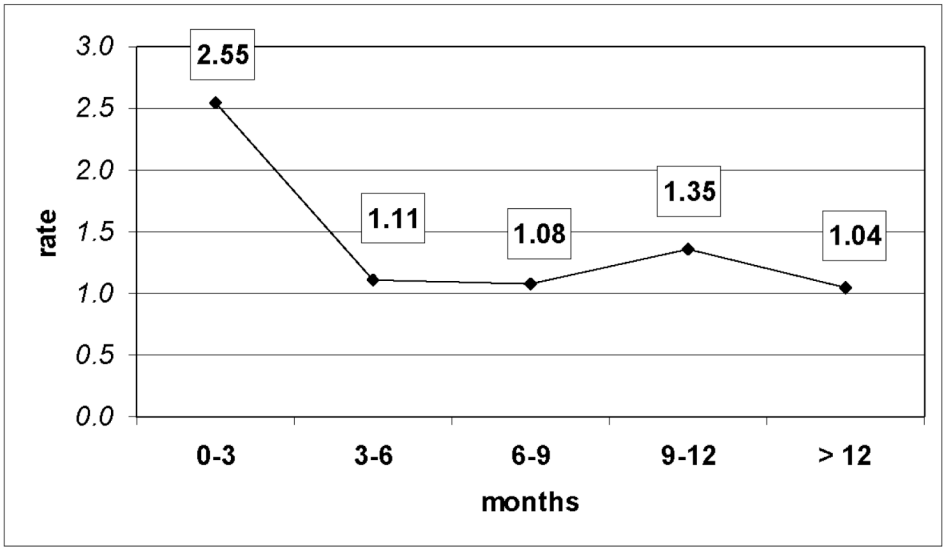
Figure 3.
Response Curves by Quartile Dose Category

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A. Expected Rate of Rearrest



B. Expected Rate of Self-Reported Offending

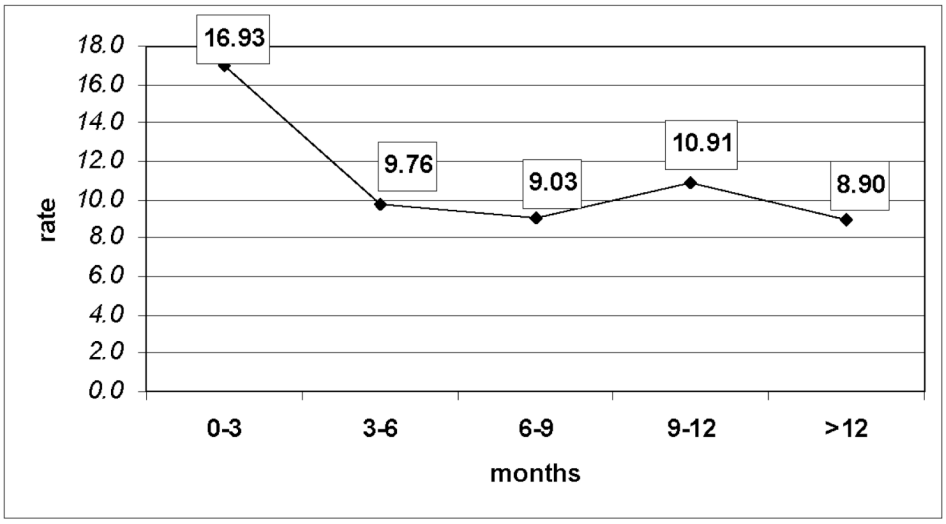


Figure 4.
Response Curves by 3-Month Dose Category

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Table 1

Baselines Covariates and Initial Balance

	Placement vs. Probation		Length of Stay	
Covariate	***t*-stat**	**sbs**	**ρ**	***p*-value**
Punishment cost—variety	21.46	389[a]	.45	.00[a]
Punishment cost— material issues	21.80	350[a]	.46	.00[a]
IQ	−5.52	171[a]	−.14	.00[a]
Punishment cost—freedom issues	12.93	110[a]	.28	.00[a]
PCL factor 2	6.71	100[a]	.00	.93
# of Priors—ever	9.55	97[a]	.21	.00[a]
Exposure to violence—total	6.93	85[a]	.18	.008
Certainty of punishment—yourself	−5.92	74[a]	−.14	.00[a]
Exposure to violence—witness	7.50	72[a]	.19	.00[a]
PCL factor 1	3.98	56[a]	.06	.21
Age of first prior	−6.11	53[a]	−.15	.00[a]
Certainty of punishment—others	−4.35	47[a]	−.07	.02[a]
Any overnight stay prior to study enrollment?	11.00	44[a]	.29	.00[a]
Antisocial—risk need score	8.71	42[a]	.00	.96
Black	9.43	40[a]	.24	.00[a]
# of Priors—past year	5.36	38[a]	.15	.00[a]
Unsupervised routine activities	6.03	35[a]	.15	.00[a]
Social cost of punishment	−5.59	33[a]	−.16	.00[a]
Exposure to violence—victim	3.99	32[a]	.11	.00[a]
Social capital—social integration	6.70	31[a]	.21	.00[a]
Parent's education	1.26	30[a]	−.01	.87
# of Priors—past 6 months	4.54	27[a]	.13	.00[a]
Social capital—closure and integration	5.80	26[a]	.17	.00[a]
Hispanic	−5.94	24[a]	−.15	.00[a]
Antisocial peer–risk need score	4.12	22[a]	−.03	.50
# of Biological parents present	−4.62	21[a]	−.13	.00[a]
Male	5.86	20[a]	.18	.00[a]
Personal rewards to crime	−1.78	20[a]	−.03	.36
Drug abuse or dependency	4.03	17[a]	.05	.10
White	−4.18	16[a]	−.10	.00[a]
Resistance to peer influence	3.11	15[a]	.07	.05[a]
Ethnic identity—identity achievement	3.01	13[a]	.08	.02[a]

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	Placement vs. Probation		Length of Stay	
Covariate	***t*-stat**	**sbs**	**ρ**	***p*-value**
Social capital—perceived opportunity for work	2.54	13[a]	.03	.43
Both biological parents present	−3.87	13[a]	−.09	.01[a]
Walden self-regulation	2.23	12[a]	.05	.12
Legal cynicism	2.25	11[a]	.07	.03[a]
Attitude risk need score	1.67	9[a]	−.01	.83
Age	1.30	9	−.03	.29
School–risk need score	3.34	9[a]	−.02	.70
Ethnic identity—overall	2.22	9[a]	.06	.08
Diagnosed prefrontal disorders	3.29	8[a]	.01	.83
Motivation to succeed	−1.58	8	−.05	.10
Substance abuse—risk need score	1.38	8	−.10	.03[a]
# of Early onset problems	1.09	8	.05	.15
PSMI	1.63	7	.06	.08
Diagnosed prefrontal pathology	2.90	7[a]	.00	.92
Two parents present	−1.54	7	−.01	.67
Single parent	1.23	5	−.02	.61
Ethnic identity—affirmation and belonging	1.22	5	.03	.34
Employed	1.20	5	.01	.83
Moral disengagement	1.19	4	.00	.96
Alcohol abuse or dependency	0.95	4	−.01	.71
Domains of social support	0.36	3	.02	.47
Mental health problems	.94	3	.03	.42
Involved in community activities—past 6 months	−.64	3	.00	.99
No adults present	1.23	3	.08	.02[a]
Total anxiety score—RCMAS	−0.14	3	.02	.52
Other race	−1.03	2	−.06	.08
Mental health—risk need score	.52	2	.04	.43
Weinberger—suppression of anger	.22	1	.00	.91
Parent—risk need score	.32	1	.03	.53
Weinberger—impulse control	.15	1	.00	.99
Weinberger—consideration of others	−.12	1	−.01	.81
Gang membership	.11	0	−.02	.47
Any involvement in community service	.07	0	−.03	.35
Weinberger—temperence	.01	0	.00	.92

ABBREVIATIONS: PCL = Psychopathy Checklist; PSMI = Psychosocial Maturity Index; RCMAS = Revised Children's Manifest Anxiety Scale; sbs = standardized bias statistic.

[a]Covariate is initially out of balance.

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Table 2

Difference Among Institutions Explaining Outcomes

					Private	Public
	1	2	3	4	5	6
Panel A: Rate of Rearrest						
Length (months)	−.013	−.006	−.071	−.067	−.017	−.009
	(.044)	(.045)	(.035)**	(.053)	(.030)	(.064)
Length^2	.000	.000		.000	.000	.000
	(.001)	(.001)		(.001)	(.001)	(.002)
Intensity of services during stay		.108	−.645	−.355		
		(.357)	(.241)**	(.414)		
Intensity^2		−.072		−.070		
		(.079)		(.079)		
Length × intensity			.044	.044		
			(.020)**	(.020)**		
n	288	288	288	288	267	152
Panel B: Rate of Self-Reported Offending						
Length (months)	−.482	−.436	−.586	−.731	−.371	−.308
	(.412)	(.423)	(.327)*	(.462)	(.658)	(.684)
Length^2	.013	.012		.005	.007	.007
	(.013)	(.013)		(.014)	(.027)	(.019)
Intensity of services during stay		1.253	−4.839	−1.877		
		(3.050)	(2.109)**	(3.631)		
Intensity^2		−.658		−.653		
		(.677)		(.676)		
Length × intensity			.318	.302		
			(.182)*	(.191)		
n	288	288	288	288	267	152

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NOTES: Standard errors are in parentheses.

* $p < .10$

** $p < .05$.

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APPENDIX A

DESCRIPTIONS OF COVARIATES

Construct	Covariate (Scale)	Mean	SD
Demographic	Age	16.35	1.10
	Male	.84	.37
	White	.21	.41
	Black	.45	.50
	Hispanic	.30	.46
	Other race	.04	.19
	Parent's education	5.77	8.19
Household composition	# of Biological parents present	.99	.55
	Both biological parents present (%)	.14	.35
	No adults (%)	.03	.17
	Two parents present (%)	.35	.48
	Single parent home (%)	.59	.49
Intelligence	IQ	84.13	13.39
Employment	Employed (%)	.25	.43
Official record information	# of Priors—ever	2.96	2.05
	# of Priors—past year	1.85	1.10
	# of Priors—past 6 months	1.35	.86
	Age of first prior petition	14.88	1.58
Gang involvement	Gang membership (%)	.20	.40
Early onset of behavior problems	# of Early onset behavioral problems	1.44	1.16
Services	Any overnight stays in a facility (%)	.67	.47
	Any involvement in community service (%)	.44	.50
Risk–need factors	Risk–need antisocial history	−.04	.61
	Risk–need antisocial attitudes	.00	.73
	Risk–need mood/anxiety problems	.20	.40
	Risk–need parental antisocial history	.24	.26
	Risk–need association with antisocial peers	−.03	.68
	Risk–need school difficulties	.40	.22
	Risk–need substance use problems	.70	.80
Trait anxiety	Total anxiety score—RCMAS	9.68	6.03
Substance use and mental health disorders	Alcohol abuse or dependency (%)	.20	.40
	Drug abuse or dependency (%)	.39	.49
	Presence of a selected mental health diagnosis (%)	.14	.35
Neurological functioning	Diagnosed prefrontal disorder (%)	.04	.20
	Diagnosed prefrontal pathology (%)	.03	.17
Psychopathy	PCL—factor 1	4.96	3.48
	PCL—factor 2	8.10	3.81

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Construct	Covariate (Scale)	Mean	SD
Acculturation	Multigroup measure of ethnic identity—Overall (1–4)	2.74	.44
	Multigroup measure of ethnic identity—affirmation and belonging (1–4)	2.96	.47
	Multigroup measure of ethnic identity—identity achievement (1–4)	2.43	.52
Exposure to violence	Exposure to violence—total	5.10	2.89
	Exposure to violence as a victim	1.44	1.40
	Exposure to violence as a witness	3.66	1.93
Psychological development	Consideration of others—Weinberger AI (1–5)	3.47	.90
	Impulse control—Weinberger AI (1–5)	2.95	.96
	Suppression of anger—Weinberger AI (1–5)	2.74	.97
	Temperament—Weinberger AI (1–5)	2.86	.86
	PSMI	3.03	.48
	Resistance to peer influence (1–4)	2.99	.57
Emotional reactivity	Walden—self-regulation (1–4)	2.74	.67
Social and personal costs and rewards of punishment	Certainty of punishment—yourself	5.63	2.26
	Certainty of punishment—others	5.38	2.88
	Punishment cost—variety	8.84	6.53
	Punishment cost—freedom issues	3.45	1.71
	Punishment cost—material issues	5.38	5.32
	Social costs of punishment (1–5)	2.71	.84
	Personal rewards to crime (0–10)	2.29	2.39
Perceptions of procedural justice	Legal cynicism (1–4)	2.02	.62
Social support	Domains of social support (#)	6.65	1.74
Academic motivation	Motivation to succeed (1–5)	3.29	.65
Moral disengagement	Moral disengagement (1–3)	1.61	.35
Community involvement	Involvement in community activities—past 6 months (%)	.27	.57
Routine activities	# of Unsupervised routine activities	3.85	.82
Personal capital and social ties	Social capital—closure and integration (1–4)	2.54	.52
	Social capital—perceived opportunity for work (1–5)	3.45	.69
	Social capital—social integration (1–4)	2.19	.55

ABBREVIATIONS: AI = Adjustment Index; PCL = Psychopathy Checklist; PSMI = Psychosocial Maturity Index; RCMAS = Revised Children's Manifest Anxiety Scale.