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**Assessing the Effectiveness of Correctional Sanctions\***

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## **Assessing the Effectiveness of Correctional Sanctions**

### **ABSTRACT**

Despite the dramatic expansion of the U.S. correctional system in recent decades, little is known about the relative effectiveness of commonly used sanctions on recidivism. The goal of this paper is to address this research gap, and systematically examine the relative impacts on recidivism of four main types of sanctions: probation, intensive probation, jail, and prison. Data on convicted felons in Florida were analyzed and propensity score matching analyses were used to estimate relative effects of each sanction type on three-year reconviction rates. Estimated effects suggest that less severe sanctions are more likely to reduce recidivism. Implications of the findings are discussed.

**KEYWORDS:** sanctions effectiveness recidivism

## INTRODUCTION

Over the past thirty years, the United States has witnessed a dramatic expansion of its correctional system. The increased growth in probation, jail, and prison populations has been well documented (Glaze 2011) and has led to considerable scholarship aimed at understanding its causes and consequences (see, e.g., Garland 2001; Irwin 2005; Gottschalk 2006; Western 2007; Useem and Piehl 2008; Raphael and Stoll 2009; Blumstein 2011; Cullen et al. 2011). One of the central justifications policymakers have invoked for tougher sanctioning is a belief that it reduces offending, among those sanctioned, more so than less severe punishment. Recently, however, community-based, non-custodial sanctions have been promoted as a more effective approach, in part because their use may allow for the provision of rehabilitative services that facilitate reintegration into society and, ultimately, less offending (Petersilia 2003; Travis and Visser 2005; Nagin et al. 2009; Cullen et al. 2011).

Juxtaposed against these two perspectives—one arguing for tougher punishment and the other for a balance of punishment and rehabilitation—is a paucity of credible empirical evidence on the relative effectiveness of the central types of sanctions employed by most states: probation, intensive probation, jail, and prison. Recent reviews and meta-analyses have arrived at this same conclusion (Gendreau et al. 2000; Smith et al. 2002; McDougall et al. 2003; Villettaz et al. 2006; Nagin et al. 2009; Durlauf and Nagin 2011; Jonson 2011). Nagin et al.'s (2009) review of studies assessing the effect of imprisonment on reoffending is illustrative. The authors identified few rigorous quasi-experimental studies of the relative impact of custodial and non-custodial sanctions on offenders' likelihood to reoffend, and far fewer experimental studies. They also concluded that, among existing studies, mixed evidence exists for the relative effectiveness of non-custodial versus custodial sanctions. Specifically, some studies have identified criminogenic effects of custodial sanctions and some have not. More importantly, as the authors emphasized, these studies typically have suffered from methodological shortcomings, such as a failure to use matching designs and other more rigorous methodological approaches,

that render substantive conclusions questionable.

A concern arising from such reviews is not just that little is known about the effect of custodial versus non-custodial punishment. It also is that few studies have examined the relative effectiveness of shorter versus longer terms of incarceration, of probation versus intensive probation, and of these latter sanctions as alternatives to jail and prison sentences. This research gap assumes particular importance given recent calls for using less severe but potentially more certain sanctions because of the possibility that they can reduce recidivism more so than incarcerative sanctions (see, e.g., McDougall et al. 2003; MacKenzie 2006; Mears 2010; Nagin and Durlauf 2011; Cullen et al. 2011; Jonson 2011).

The purpose of this study is to respond to the calls by scholars for more rigorous assessments of sanction effects. Using Florida Department of Corrections data on a cohort of convicted felons, we use propensity score matching to examine the relative effectiveness of four types of correctional sanctions: probation, intensive probation, jail, and prison. We begin first by describing recent trends in corrections, what is known about the effectiveness of correctional system sanctions, and the critical questions that remain unaddressed. We then describe the data, methods, and findings, and conclude by discussing the study's implications.

## **BACKGROUND**

### **Correctional Expansion and “Get-Tough” Punishment**

Recent decades have been witness to historically unprecedented growth in the correctional system (Gottschalk 2011). The U.S. jail and prison population has grown from 501,886 in 1980 to over 2.2 million in 2010. Considerable scholarly and policy attention has been given to this growth, yet the larger growth, in absolute magnitude, has occurred among the population under non-custodial control. For example, during the same time period, the total population on probation or parole grew from 1.3 to 4.8 million (Glaze 2011). Of the 7 million individuals under correctional system control in 2010, approximately one-third was under some form of

custodial control (11% in jail and 21% in prison) and over half (57%) was on probation.

Scholars have attributed the expansion of the correctional system to several factors. They point, for example, to policymaker efforts to appear “tough on crime” and to be or appear proactive in the fight against drugs and violence (Beckett 1997; Davey 1998; Gottschalk 2006; Simon 2007; Mears 2010; Blumstein 2011). Others have suggested that the increased use of sanctioning stems from an indirect response to perceived threats among whites and the power elite from minorities, the poor, and other marginal groups (Garland 2001; Beckett and Western 2001; Bobo and Thompson 2006). In part, the growth may derive from disenchantment with rehabilitative approaches to crime control, as well as dissatisfaction with the record of intermediate sanctions in reducing recidivism (Tonry and Lynch 1996). This disenchantment in turn may have contributed to a belief that prison terms, lengthy prison terms in particular, constitute the only viable way to reduce offending. This belief—that more punitive sanctions reduce recidivism—provides the central justification for many of the get-tough changes that have arisen in U.S. sentencing policies (Spelman 2000; Cullen et al. 2011).

The historically unprecedented change in the correctional system policy landscape has led to considerable attention to investigating the effects of correctional system growth on crime rates (e.g., Sampson 1986; Marvell and Moody 1994; Levitt 1996; Spelman 2000; Kovandzic and Vieraitis 2005; Rosenfeld and Messner 2009). At the same time, there is, however, the question of whether more severe sanctions reduce recidivism. Although tougher punishment is motivated in part by retributive ideals, it also is motivated by a belief that more severe sanctioning generates a specific deterrent effect. The underlying theoretical premise is that such sanctions inspire a greater fear of further punishment and in turn a greater likelihood of refraining from criminal behavior (Nagin et al. 2009). In contrast to this view stands the theoretical argument that less severe sanctions can be more effective. Among other things, they may allow for more rehabilitative services to be provided and for ties to family and to the community to be maintained, thus not only facilitating prosocial behavior but also enabling social support that can allow for successful reentry (Braithwaite 1989; Lawrence 1991; Petersilia 1995; MacKenzie

2006; Pratt 2008; Mears 2010). At the least, according to this argument, non-incarcerative sanctions avoid the potentially criminogenic effects of incarceration (Nagin et al. 2009).

### **The Effectiveness of Correctional Sanctions**

Given the growth in the U.S. correctional system, the costs associated with such growth, the seemingly compelling arguments for and against tougher sanctioning, and policymaker calls for evidence-based policies (Welsh and Harris 2008; Mears 2010), it could reasonably be anticipated that a substantial body of rigorous empirical research has accumulated that adjudicates between these different perspectives. As noted at the outset, however, few studies exist that directly attend to this issue in ways that address a range of methodological concerns, such as selection bias. In addition, the available evidence supports no clear or consistent finding concerning the effectiveness of different types of sanctions, save to suggest that more severe sanctioning exerts a null or criminogenic effect.<sup>1</sup> That assessment, rendered most recently by Nagin et al. (2009) but also echoed by others (see, generally, Gendreau et al. 2000; Smith et al. 2002; McDougall et al. 2003; Villettaz et al. 2006; Mears 2010; Durlauf and Nagin 2011; Jonson 2011), underscores the need for studies that directly examine the relative effectiveness of different types of sanctions and that do so using more rigorous research designs. As prior scholarship has emphasized, there are two critical issues to address: identifying the relevant or appropriate counterfactual condition and arriving at credible estimates of sanction impacts.

The concern about identifying the appropriate counterfactual condition derives from the fact that any estimated impact of a sanction is relative to some other condition. In assessing the impact of a prison term, for example, the relevant counterfactual is some other type of sanction, such as jail or probation (Nagin et al. 2009:129; see, e.g., Smith and Akers 1993; Bales and Piquero 2012) or length of time served (see, e.g., Loughran et al. 2009; Snodgrass et al. 2011). Even so, a challenge here is that it is not always clear what sanction constitutes the counterfactual condition. Among convicted felons, a sanction of some type will occur.



However, for a given group of sanctioned felons, it may not always be clear what other sanction would have been administered. It is possible, for example, that prisoners would have been sent to jail, intensive probation, or even traditional probation. In a context where multiple possibilities exist, it is important, as we discuss below, to assess each of the different counterfactual conditions to arrive at estimates of the impacts of a given sanction relative to the range of alternatives that might otherwise have occurred.

The concern about methodology stems from the idea that any assessment of a sanction's impact must take into account the fact that individuals typically are not randomly selected into one type of sanction or another. In Nagin et al.'s (2009) review, the authors identified three categories of prison recidivism studies: experimental, matching, and regression-based. The authors noted that there were too few credible studies to draw firm conclusions about the relative effectiveness of different sanctions. For example, they identified only 5 experimental studies of prison effects and only 12 studies that employed matching to address selection biases (Caliendo and Kopeinig 2008; Guo and Fraser 2010). Finally, the authors identified 31 regression-based studies of prison effects, which would seem to hold promise for providing a robust estimate of sanction impacts. However, a critical limitation of such studies is that they typically do not address selection bias as well as experimental or matching designs (see, e.g., Chen and Shapiro 2007; Bales and Piquero 2012). In many of the studies, the flaw was more fundamental—for example, of the 31 studies, only 16 controlled for age, race, sex, prior record, and offense type (Nagin et al. 2009:155).

### **What Is the Relative Effectiveness of Correctional System Sanctions?**

The purpose of this paper is to respond to calls for more methodologically rigorous assessments of the relative effectiveness of correctional sanctions on recidivism (see, e.g., Smith et al. 2002; Chen and Shapiro 2007; Nagin et al. 2009; Bales and Piquero 2012). In so doing, the paper aims to build on prior studies to examine heterogeneity within non-custodial and custodial

sanctions. Non-custodial sanctions can consist, broadly, of either probation or intensive probation, while custodial sanctions can consist, broadly, of shorter-term confinement in jail or longer-term confinement in prison. Accordingly, this study examines the following question: What is the relative effectiveness of four different types of sanctions—probation, intensive probation, jail, and prison—in reducing recidivism? There can be, of course, heterogeneity within these broad categories of sanctioning. Such variation itself bears investigation, but at the same time and as Nagin et al. (2009) have highlighted, studies are needed that examine whether the general categories of sanctioning most commonly used by the courts influence sanctioning.

Although several prior studies have examined the impact of incarceration on recidivism, this study is, to our knowledge, the first to systematically investigate a series of different counterfactual conditions specific to each of these sanctions and to do so using a methodological approach, propensity score matching, called for in recent scholarship on sanction effects (see, generally, Nagin et al. 2009; Bales and Piquero 2012). In particular, we examine the following questions and the associated counterfactual conditions that they involve. First, what is the effect of probation? More precisely, what is the effect of probation as compared to what otherwise would have happened? The possibilities are that the individuals instead would have been placed on intensive probation or in jail or prison. Thus, to answer the question, we need to identify individuals from among each of the three counterfactual conditions who resemble those who received probation. Implicitly, then, the expectation is that there may be people in each of these three groups who have characteristics similar to those of probationers. If in fact no matches exist, then it is not possible to estimate a relative effect of probation. Second, if we view intensive probation as the treatment, we want to know what the effect of this treatment is as compared to what otherwise would have happened. Here, again, three possibilities present themselves—that is, the individuals otherwise would have been placed on traditional probation or in jail or prison. Third, for jail-as-treatment, the three possibilities are that the individuals otherwise would have been placed on traditional probation or intensive probation or in prison, respectively. Finally, if we view prison as the treatment, the three counterfactual conditions are

traditional probation, intensive probation, or jail, respectively.

A central implication that flows from identifying these different counterfactual conditions is that the impact of a given sanction may vary depending on which counterfactual is examined. From this perspective, there is no absolute effect of a given type of sanction. *Rather, its effect is always relative to the types of individuals who receive that sanction and concomitantly to the types of particular sanctions that the individuals otherwise would have received.* For example, a study that examines the effects of imprisonment versus intensive probation, in reality is assessing only one of several counterfactual conditions relevant for determining the impact of imprisonment (see, e.g., Bales and Piquero 2012).

To illustrate the policy relevance of these observations and the salience of answering the above questions, consider the case of a judge who must sentence a convicted felon. To simplify matters, let us focus only on the individuals who the judge typically sentences to prison. The judge may wonder if these individuals have lower levels of recidivism as compared to what otherwise would have happened—that is, as compared to the sanctions that he or she otherwise typically would have administered. If the judge discovers that the prison group had a higher level of recidivism as compared to what otherwise would have happened, then he or she might consider a different approach to sanctioning the types of individuals that, in the past, typically were sent to prison. Conversely, the judge may wonder what would have happened to the individuals that he or she typically sanctions to intensive probation. If the judge discovers that this group has a lower level of recidivism as compared to individuals in two of the other sanction groups—for example, those in jail or prison but not intensive probation—this finding might reinforce the judge’s view that it is the appropriate intervention for the types of individuals who he or she typically places on intensive probation instead of jail or prison. It also might raise questions about whether intensive probation, as a sanction for the types of individuals who he or she otherwise would have sanctioned to traditional probation, is worthwhile given that no difference in recidivism exists.

The implications of different counterfactual conditions associated with each of the four types

of sanctions bears emphasis—*there is no fixed or absolute effect of a given sanction*. Rather, the effect of a given sanction on recidivism is always relative to what otherwise would have happened. A central goal of this paper is to illustrate this point and, in particular, to show that a rigorous assessment of sanction effects requires systematically taking into account the sanctioning options that define the counterfactual universe of options. In the absence of an experimental design, a series of counterfactual, matching-based analyses provides one approach, among a range of approaches recommended by scholars (see, e.g., Chen and Shapiro 2007; Nagin et al. 2009; Bales and Piquero 2012), to address this complexity and arrive at more credible estimates of sanction effectiveness. A related line of investigation involves assessing whether the theoretical underpinnings of different sanctions contribute to identified effects. As Nagin et al. (2009) and Cullen et al. (2011) have emphasized, however, any such undertaking requires first developing credible assessments of sanction effects.

## **DATA AND METHODS**

The goal of the current study is to assess the relative effectiveness of four types of sanctions: probation, intensive probation, jail, and prison. Using data on convicted felons in Florida, the analyses employ propensity score matching to assess a series of counterfactual scenarios, each comparing the recidivism of one group of felons to the recidivism of other groups of felons who received a different sanction. Essentially, this approach asks the following question: What is the effect of a given sanction, or “treatment,” as compared to a particular counterfactual sanction? The data and methodology for answering this question are described below.

### **Data**

The data for this study came from the Florida Department of Corrections (FDOC) Sentencing Guidelines database, and consist of a cohort of sentenced individuals who were convicted of felonies in Florida and who were released between 1994 and 2002. All male offenders in the

guidelines database who were sentenced to probation, intensive probation, jail, or prison were included in the dataset.<sup>2</sup> Also included are a series of demographic characteristics and guidelines scores that describe each offender's most serious offense and whether or not an offender's total sentencing score allowed for a prison sentence. The guidelines records were then matched to information in the FDOC's Offender Based Information System to allow inclusion of prior record information, including prior prison commitments, supervision violations, and felony convictions, and also to acquire information on prison release dates. Recidivism was linked to each offender using the same databases and is here defined as a felony reconviction within a three-year follow-up period. For prison and jail, the recidivism "clock" starts upon release from incarceration; for either type of probation, it starts when the sentence begins. The dataset included 586,357 individuals in total.

### **Propensity Score Matching**

The analyses here employed a matching methodology using propensity scores to reduce the influence of sanction selection bias on estimated treatment effects (Rosenbaum and Rubin 1983; Becker and Ichino 2002; Apel and Sweeten 2010; Guo and Fraser 2010). As a treatment, sanction types are not randomly assigned, and offenders are likely to vary significantly on a number of characteristics related both to receiving a given sanction and to reoffending. For example, prisoners are likely to differ, on average, from individuals sentenced to probation, and in most cases this is by design. Prison is, for example, typically considered to be a more severe sanction. Accordingly, individuals who are imprisoned typically will differ from individuals who receive other sanctions. The record of prior convictions, prior prison commitments, and the severity of their offense, for example, all likely will be greater. That said, sentencing research highlights that there is considerable heterogeneity in sanctioning (see, e.g., Reitz 2011), thus creating a situation in which individuals who, as in this example, are sentenced to prison look similar in many respects to individuals placed in jail or on traditional or intensive probation. The

propensity score matching technique is useful for reducing selection bias by matching groups of individuals based on their propensity, or likelihood, of receiving various sanctions.

### **Analysis Plan**

In instances when an experimental design is not feasible and offenders have not been randomly selected into, in this case, one of the four sanction types, a matching procedure is useful because it attempts to simulate independent treatment assignment (see, generally, Rosenbaum and Rubin 1983; Apel and Sweeten 2010; Guo and Fraser 2010). Accordingly, for this study, we undertook a three-step process involving matching analyses to produce estimated average treatment effects on the treated (ATT). First, propensity scores were created using logistic regression to predict the likelihood of individuals receiving a given sanction relative to a given counterfactual condition, using matching information on demographic characteristics, offense, and prior record. Second, for each comparison, individuals from two different sanction groups—one designated to be the treatment and the other the control—were matched using the estimated likelihood score. The matching approach for all analyses involved 1-to-1 nearest neighbor matching without replacement using a .005 caliper setting.<sup>3</sup> Under the propensity score framework, two individuals with the same score have the same likelihood, based on the specified covariates, of receiving the designated treatment. Once matched, these groups are now comparable, given the assumption of no imbalance in unobserved confounders (Winship and Morgan 1999), and differences between them can be more confidently attributed to the effect of the specified treatment. Finally, the third step involved matching offenders from the designated treated and control groups and—after excluding unmatched cases that were, in the terminology of propensity score analysis, “off support”—comparing the respective rates of recidivism (for similar examples in the criminological literature, see King et al. 2007; Paternoster and Brame 2008; Loughran et al. 2009; Bales and Piquero 2012). These steps were followed for each of the four sanction groups and the respective counterfactual comparisons.

The accuracy of the comparisons is based on two considerations. The first is the quality of the matching variables. Nagin et al. (2009) suggest that, when assessing the effectiveness of sanctions, it is critical to match, at a minimum, on the following characteristics: race, gender, prior record, and offense information. Accordingly, we include measures that, if omitted, might potentially bias the results (see table 1). These consist of frequently identified factors associated with sanctioning, including criminal record (a count variable measuring number of prior convictions), age (continuous), race (Black, Hispanic, and White dummy variables), current offense information (a dummy variable for each type of offense, separated into 9 categories), and an indicator of the judicial circuit each offender was sanctioned in (a dummy variable for each Florida judicial circuit, numbered 1 through 20). In addition, we include the number of prior prison commitments (count), prior supervision violations (count), and two variables taken from the FDOC Sentencing Guidelines data: a measure of offense severity (values = 1-10, with 10 indicating the most serious offense), and a binary measure of prison eligibility based on the offender's total sentencing score in accordance with the Florida sentencing guidelines ("1" indicates an offender's sentencing score made them eligible for a prison sentence).

Insert table 1 about here

The second consideration is the ability to find matches to treatment group subjects. For matching analyses, one ideally has a sufficient pool of potential comparison subjects to ensure that individuals similar to those in the treatment group can be identified (Rosenbaum and Rubin 1983). Here, we have access to information on 318,073 individuals on traditional probation, 53,136 on intensive probation, 132,059 in jail, and 83,089 in prison. To create a larger pool of comparison subjects for each treatment-to-control group matching analysis, we created smaller treatment groups that nonetheless are substantially larger than those used in many prior studies. For each sanction group (probation, intensive probation, jail, and prison), we created treatment groups by randomly selecting 10,000 individuals. Then, we matched these individuals to

individuals from a given comparison pool of subjects. In each comparison, only the treated group is limited to 10,000 individuals. For example, 10,000 probationers were matched to individuals from the entire pool of intensive probation individuals; then these same 10,000 individuals were matched to individuals from the entire pool of individuals in jail; and, last, they were matched to individuals from the entire pool of individuals in prison.

This process was repeated for each of the other three treatment groups along with two additional comparisons when examining jail as a treatment alternative to prison. Because jail typically involves a relatively short term of incarceration, it may be that the more appropriate pool of subjects from which to make comparisons is not the full pool of prisoners but rather those who serve shorter prison sentences. For this reason, in addition to an analysis where jail inmates were matched to individuals from the general prison population, we conducted matching analyses that instead used individuals who served 1 year or less in prison and individuals who served 2 years or less in prison, respectively.<sup>4</sup> There were, then, a total of 14 comparisons.<sup>5</sup>

## **FINDINGS**

### **Post-Matching Balance on Covariates**

For each of the 14 total matching comparisons, we used logistic regression models to predict an offender's likelihood to receive the specified treatment (i.e., sanction) as compared to the alternative sanction under consideration. Per the protocol recommended in the propensity score literature (e.g., Rosenbaum and Rubin 1984; Becker and Ichino 2002), interactional and polynomial specifications were introduced in the models to achieve balance on the covariates.<sup>6</sup> The main goal of matching analyses is not these models but rather the generation of scores that result in covariate balance between the treatment and control groups and in turn the ability to draw more robust estimates of treatment effects. That is, the goal is to ensure that any confoundedness of the included variables is ruled out (Rosenbaum and Rubin 1983; Becker and Ichino 2002; Apel and Sweeten 2010). Accordingly, we present post-test balance, or adjustment,



statistics for each matching scenario. These are presented in tables 2a through 2d, respectively, and include the post-matching covariate means for the treated group and the matched group, the percent bias remaining, the percent bias reduction achieved by matching, and the *t*-test values.

Prior to making comparisons between treatment and control groups, it is important to eliminate any imbalance in the covariates. That is, after matching, there should be no remaining significant differences in covariate means between the treatment and the control group. Because of the large size of the samples, it is possible to identify statistically significant differences even when no substantively significant imbalance exists. Thus, we focus on both statistical significance and substantive significance when discussing post-matching covariate balance. Tables 2a through 2d present the balance statistics for all 14 comparisons. Table 2a focuses on probation as compared with intensive probation, jail, and prison, respectively. Table 2b focuses on intensive probation as compared with traditional probation, jail, and prison, respectively. Table 2c focuses on jail as compared with probation, intensive probation, prison, prison for 1 year or less, and prison for 2 years or less, respectively. Finally, table 2d focuses on prison as compared with probation, intensive probation, and jail, respectively.

Inspection of table 1 shows that substantial differences in many of the covariates existed prior to matching. By contrast, inspection of tables 2a through 2d shows that the propensity score matching process eliminated almost all statistically significant differences between treated and control groups in the covariates. To illustrate, prior to matching, individuals in the probation group had an average of .60 prior convictions and individuals in the intensive probation group had an average of 1.06 prior convictions (see table 1). By contrast, and as shown in the first panel of table 2a—the analysis in which traditional probation is the treatment and this group is matched to individuals from the intensive probation pool—there is no statistically significant difference; indeed, the mean prior record for both groups is almost identical (.610 vs. .613).

Insert tables 2a through 2d about here

The percent bias (%B) columns reinforce this assessment. For almost every covariate in every comparison across tables 2a through 2d, the percent bias remaining after matching typically is 2% or less. The percent bias reduction (%BR) column shows why. Across the different covariates, the matching process generally reduced the pre-matching imbalance by 80% or more. In the end, then, only a few covariate comparisons remain statistically significant. In these cases, the substantive differences are negligible. Consider, for example, table 2a. Of the three matching analyses—intensive probation in the first panel, jail in the second panel, and prison in the third—only six statistically significant post-matching differences, out of 114 total comparisons, emerge. Closer inspection of the mean values in each of these six cases identifies that the magnitude of the differences is trivial. In the panel 1 matching analysis, for example, the percentages of individuals in the treated and matched groups, respectively, who were convicted of murder are both less than 1%; the prison eligibility means for the two groups are within one percentage point of one another (.139 and .150, respectively); and the percentages of each group tried in the 15th circuit also are substantively similar (.044 and .052, respectively). Careful review of the other three statistically significant differences (in panel 3) identifies no appreciable substantive differences in the covariates after the matching analyses.

The same pattern can be seen in the other tables. In table 2b, only 9 of the 114 comparisons are statistically significant, and in each of these 9 cases, there are no substantively significant differences that remain. In table 2c, only 4 of 190 comparisons are statistically significant and in each instance the substantive differences are slight. Finally, in table 2d, only 3 of the 114 comparisons are statistically significant; here, again, the magnitude of difference in the mean values of the matching covariates is negligible. In short, then, the propensity score matching resulted in treated and matched control groups that are similar with respect to the matching covariates. This process thus enables the estimation of effects that we can be more confident reflect the relative effectiveness of the various sanctions rather than differences among the individuals who receive the different types of sanctions.

We also examined sensitivity analyses for all of the reported results (Becker and Caliendo

2007). These analyses provide *conservative* estimates of the degree to which the results might be sensitive to unobserved covariates (DiPrete and Gangl 2004:291). Specifically, they estimate how large the effect of unobserved confounders would have to be to alter the results. The size of this effect is expressed as gamma. For example, if gamma, expressed as an odds ratio, is 2, the result is sensitive to bias that would double the likelihood of receiving treatment. Across all 14 comparisons, the analyses yielded gamma scores ranging from 1.1 to 1.8. As would be expected, larger gamma scores were associated with substantively larger effects. In these cases, the effects were more robust and less vulnerable to unobserved confounding. The robustness of these larger effects, coupled with the consistent pattern in the estimated effects, discussed below, provides greater confidence in the assessment of the relative effectiveness of the different sanctions.

### **Traditional Probation**

We turn now to the results of the matching comparisons, presented in table 3. For the probation, intensive probation, jail, and prison “treatment” groups, it was possible to match substantial proportions of the individuals to counterparts in the control groups. We focus here on the first panel, which presents the matching analyses for the traditional probation group. Before discussing the results, there is the critical question of whether matches to this group can be found. Inspection of the last column in table 3 provides this information. Specifically, we were able to match 99% of the treatment group (n=10,000) to individuals in the intensive probation and jail populations, respectively. When matched to the prison population, approximately 36% of probationers fell off support; this indicates that it was more difficult to find comparable matches for the probationers among those sanctioned to prison. On the one hand, this loss of cases to off support limits the generalizability of an estimated effect of probation versus prison. On the other hand, it highlights that in fact large numbers of individuals in prison have counterparts with similar profiles who received probation instead of prison. *In short, the fact that probation counterparts can be found in prison suggests that prison is used as a sanction for*

*individuals who, in many respects, appear to be similar to individuals placed on probation.*

Insert table 3 about here

The question addressed in the first panel is as follows: Among individuals on probation, what is the effect of probation as compared to what would have happened had they not received this sanction and instead received any of three counterfactual sanctions? Focusing first on the comparison to intensive probation, the analyses show that, in absolute terms, 31% of the “treated” individuals—that is, those on probation—recidivated as compared to 34% for the matched individuals on intensive probation (.310 vs. .319 respectively, or a -.029 difference, statistically significant at the  $p < .001$  level, the level of statistical significance used for all of the analyses). This difference is only slightly more pronounced if being placed in jail defines the counterfactual. Here, again, those on probation are less likely to recidivate. Whereas 31% of individuals in the “treated” (probation) group recidivated, 35% of the jail group recidivated (.311 vs. .347, or a -.036 difference).

The more appreciable difference surfaces when prison is the counterfactual condition. Here, we see that 33% of probationers recidivated as compared to 47% of ex-prisoners (.328 vs. .469, or a -.141 difference). This effect reflects the expected difference in recidivism only among those prisoners who resembled individuals in the probation sample. Many prisoners had no counterparts on probation, which partially restricts the generalizability of the assessment. Notably, though, 64% of the probation sample could be matched to prisoners. For these individuals who do have counterparts in prison, placement on probation appears to be associated with a substantially lower likelihood of recidivism.

### **Intensive Probation**

What about when intensive probation is the treatment? Here, paralleling the steps taken above, we first need to find matches for each of three distinct counterfactual conditions. As with

the probation analyses above, matching was not a problem. Specifically, for each of the three comparisons, over 90% of the treatment group could be matched to the control group. Thus, it again appears that we have evidence that the policy question at hand is far from academic. That is, many individuals who receive traditional probation, jail, or prison sanctions in fact appear to be similar to individuals who receive intensive probation as a sanction.

We turn now to the second panel of table 3. When compared to traditional probation, what in many cases would be viewed as a less serious sanction, we find no significant difference in the effectiveness of intensive probation in reducing recidivism. This null effect is notable, given the greater supervision associated with intensive probation, and, in turn, the greater costs (Akers and Smith 1993; Piehl and LoBuglio 2005). A different pattern surfaces when we turn to custodial sanctions. Compared to jail or prison, intensive probation is associated with a reduced likelihood of recidivism, a finding that parallels the first set of analyses that centered on traditional probation. For individuals on intensive probation as compared to matched counterparts in jail, recidivism is slightly lower (.346 vs. .375, respectively, or a -.029 difference). As with the traditional probation analyses presented in the first panel, this recidivism-reducing effect is considerably more pronounced when the comparison is with prison. Specifically, the estimated recidivism for the probationers is 33% rather than 45%, what amounts to a 12% reduction in recidivism in absolute percentage terms (.332 vs. .453, respectively, or a -.121 difference).

## **Jail**

With the focus on jails, we now turn our attention to the effects of custodial sanctions. What, in particular, is the effect of jail? We begin first with examining the extent to which matches to the jail group could be obtained for four different counterfactual groups (traditional probation, intensive probation, prison, less than 1 year in prison, less than 2 years in prison). Although almost all individuals in the jail sample could be matched to individuals on traditional probation, approximately 17% of the sample was off support when matching to intensive probation. That

is, it was more possible to identify matches among individuals on traditional probation than it was among individuals on intensive probation. Finding matches to the prison population was more difficult. Among individuals in jail, 27% could not be matched to individuals from the prison population. Surprisingly, when the prison control group was limited to just those inmates who served 1 year or less, 38% of jailed offenders were off support. When we focused on individuals who served 2 years or less, 29% were off support. The fact that matches could be identified at all indicates that probation and prison terms are used for individuals who look, in many respects, comparable to individuals who received jail as a sanction. At the same time, the loss of some cases to off support limits the generalizability of the estimated effects, which apply only to comparisons between the jail population and the types of individuals in these other groups who could be matched on offense type, prior record, and the other measures.

What, then, is the relative effect of jail? Consistent with the previous two panels, a clear pattern is present—tougher sanctioning, jail in this instance, is associated with increased recidivism. Among individuals who received a jail sanction, as compared to those who were placed on traditional probation, the likelihood of recidivism is modestly increased, from 40% to 43% (.398 vs. .430, or a +.032 difference). As compared to matched individuals on intensive probation, the likelihood of recidivism among those who were placed in jail is also slightly increased from 40% to 42% (.395 vs. .422, or a +.027 difference). The pattern is evident, too, when prison, arguably a more severe sanction, serves as the counterfactual. Here, the recidivism of the treated jail sample is 45% compared to the 54% recidivism of the matched prison group (.447 vs. .538, respectively, or a - .091 difference). This difference essentially is the same when the comparison is to prisoners who served 1 year or less in prison or 2 years or less in prison.

## **Prison**

The effect of prison has perhaps received the most attention in the sanctions literature, but its impact has not been systematically compared to the full spectrum of alternative sanctions—for

example, probation, intensive probation, and jail. As with the preceding analyses, the initial question is how comparable the prisoners are to individuals in the other sanction groups. As can be seen in the table, almost all prisoners (99%) could be matched to individuals on traditional probation and 93% could be matched to individuals on intensive probation. Thus, few prisoners were off support. The matching was slightly more limited for the jail population. In that analysis, 84% of the prison sample could be matched to individuals in jail.

The comparisons in the final panel reinforce the notion that sanctions typically viewed as more severe are associated with increased recidivism. For example, placement in prison, as compared to traditional probation, is associated with an increase in recidivism, from 37% to 47% (.370 vs. .474, respectively, or a +.104 difference). A somewhat greater increase can be seen when the comparison is to intensive probation—here, the increase is from 33% to 47% (.329 vs. .467 respectively, or a +.138 difference). And prison appears, not least, to be a more criminogenic alternative to jail. Here, the effect is not as pronounced but nonetheless is notable. Specifically, when the counterfactual condition is jail, the estimated effect of a prison sanction is to increase recidivism from 44% to 50% (.441 vs. .498, respectively, or a +.058 difference).

## CONCLUSION

In the United States and in many other parts of the world, a dramatic increase in more punitive sanctioning occurred in recent decades, driven in no small part by the view that tougher sanctions “work”—that is, they reduce recidivism and they reduce crime rates. This “get-tough” trend has been challenged by critics who claim that tougher sanctioning does not produce these benefits and, at the same time, carries with it substantial costs in the form of increased recidivism and missed opportunities to invest in potentially more effective approaches to reducing crime (see, e.g., McDougall et al. 2003; Raphael and Stoll 2009; Mears 2010; Cullen et al. 2011). In support of such arguments are those who have argued that more certain sanctioning, coupled perhaps with a range of services and supports, may do more to reduce the offending of

individuals who enter the criminal justice system (see, e.g., MacKenzie 2006; Pratt 2008; Durlauf and Nagin 2011). Notably, however, as Nagin et al. (2009) and others have shown, few rigorous studies of the relative effectiveness of correctional system sanctions exist.

The goal of this study was to contribute to efforts to address this research gap. To this end, it is the first study of which we are aware to systematically compare the effectiveness of four commonly used types of sanctions—traditional probation, intensive probation, jail, and prison—relative to the unique counterfactual conditions specific to each. The latter emphasis is especially important because the effectiveness of a given sanction fundamentally depends on the basis of comparison. The effect of intensive probation, for example, may be different if the counterfactual condition is probation or jail or prison.

The matching analyses here proceeded from that premise and estimated the effects of a given sanction in comparison to several different counterfactual conditions. Two broad findings emerged. First, we found that across most comparisons, tougher sanctioning was consistently and positively associated with recidivism. Second, it was possible to find matches between groups, suggesting that, at least in Florida and presumably in other states, a naturally occurring experiment has been unfolding. That is, convicted felons who resemble one another with respect to offense type, prior record, and other such characteristics receive very different sanctions.

These findings are qualified by the fact that unobserved confounding might have influenced the estimated effects, a problem central to all quasi-experimental assessments of sanctioning impacts (Nagin et al. 2009), and by a loss of cases “off support” in some of the analyses. In this latter instance, the generalizability of the estimated effects from the analyses is limited to those cases for which matches could be identified. This limitation is, however, precisely what the matching analyses highlight more clearly than traditional regression analyses—that is, the effects of a given sanction, as compared to another sanction, should only be expected for individuals who resemble one another. Even so, the identified effects either are valid—that is, the pattern of results accurately assess the effectiveness of the different types of sanctions—or they are spurious. The research design here responded to calls for using matching analyses to estimate



more credibly the effectiveness of sanctions. More research will be needed, however, before a strong claim can be made concerning the effects of different sanctions on recidivism.

We turn now to several explanations for the finding that sanctions that typically are viewed as tougher are associated with more rather than less recidivism. First, it may be that tougher sanctions provide less support and fewer services. By contrast, opportunities for providing more support and services may be available with less severe sanctions. For example, being placed on probation or in a local jail may more readily allow for community-based reintegration and treatment. In turn, these effects may translate into reduced offending. Some research, for example, indicates that rehabilitative services, treatment, and community support and assistance can contribute to lower levels of offending (see, e.g., Lawrence 1991; Petersilia and Turner 1993; Petersilia 1995; Cullen and Gendreau 2000; MacKenzie 2006; Mears 2010; White et al. 2012).

Another possibility is that less severe sanctions reduce exposure to potentially criminogenic environments (Nagin et al. 2009). Jails and prisons, for example, are settings in which substantial deprivations can occur and in which cultures of violence and criminality may exist (Adams 1992; Bottoms 1999). Exposure to such conditions, and to the consequences that may attend to incarceration (e.g., an even greater reduction in the ability to find employment or housing), may increase the likelihood of recidivism. Conversely, a lack of exposure to them may reduce recidivism even in the absence of rehabilitative programming or various social supports that may be available while on probation.

Yet another possibility is that less severe sanctions are associated with increased perceptions of punishment certainty among convicted felons who experience them. That is, these individuals may perceive there to be a greater certainty that, if they commit an offense, they will be sanctioned. Such a possibility would generate a reduced likelihood of sanctioning if, as recent scholarship suggests, it is the certainty of punishment more so than the severity of punishment that exerts a specific deterrent effect (Durlauf and Nagin 2011).

Not least, an intriguing possibility is that sanctions, such as probation, that typically are viewed as less severe than other sanctions, such as prison, in fact may be perceived to be more

severe. Although seemingly counter-intuitive, this finding has emerged in several studies (see, e.g., Crouch 1993; Deschenes et al. 1995; Petersilia 1997). Research suggests, for example, that for some offenders a prison sentence may be preferable to a community sanction, primarily because of perceptions that prison is less severe than supervision and involves fewer restrictions upon release back into society (Spelman 1995; May et al. 2005).

In short, community sanctions, including jail, that provide access to the community and links to a variety of potential supports, may be more effective than prison in reducing recidivism because they may better allow both for more punishment and for more rehabilitation. That expectation certainly accords with the arguments made for reintegrative approaches to punishment (Braithwaite 1989; Lawrence 1991). What the precise balance of punishment and such services needs to be, under such arguments, remains largely unknown.

Given prior research and this study, there are, in our view, several implications for future scholarship and policy discussions. First, studies are needed that, as Nagin et al. (2009) and others have advocated, employ more rigorous approaches to estimating the effects of various sanctions on recidivism. In so doing, they will ideally want to examine a range of counterfactual conditions. Comparing incarcerative and non-incarcerative sanctions likely obscures important variation within these categories. Here, for example, there were clear differences in the effects of each of the four different types of sanctions as compared to the others. This study was not able to investigate further heterogeneity in sanctioning, but such a step would be justified. Traditional probation is illustrative. In some cases, traditional probation may consist of only a few contacts with an officer, while in others it might involve more contact and supervision and also a strong emphasis on facilitating access to social services and supports (Petersilia 2003; Piehl and LoBuglio 2005). In addition, community sanctions will not always provide better conditions than a prison setting, so what about instances when prisons provide higher quality services and features? This type of heterogeneity is typical of correctional system sanctions (see Chen and Shapiro 2007; Bonta et al. 2008; Jonson 2010; Cullen et al. 2011; Durlauf and Nagin 2011; Listwan et al. 2011) and so constitutes an important avenue of research.

In a related vein, it will be important for future research to investigate the extent to which incarceration effectiveness, relative to other types of sanctions, is moderated by the quantity and quality of post-release supervision. Such work will need to confront the challenge of specifying appropriate counterfactuals. For example, for ex-prisoners released to lengthy terms of intense post-release supervision, the appropriate matches from the probation pool might be those individuals on probation with comparable periods and amounts of supervision. However, to the extent that ex-prisoner post-release supervision derives in part from in-prison behavior, this approach would not necessarily result in equivalent groups.

Second, studies are needed that investigate the extent to which a given type of sanction may exert a differential effect for different groups of individuals. It is, for example, possible that the effects of a particular type of sanction may vary along such dimensions as age, race or ethnicity, gender, prior prison experience, offense type, and the community context from which individuals come or to which they return (Clear and Hardyman 1990; Spelman 1995; Bonta et al. 2000; Kubrin and Stewart 2006; Hipp et al. 2010).

Third, research will want to identify and assess empirically the theoretical mechanisms that would lead less severe sanctions to be associated with less recidivism. Is it, for example, the avoidance of criminogenic conditions in prison, the greater access to rehabilitative services and supports, the perception that community supervision sanctions and consequences are more certain or severe, or some other mechanism (Nagin et al. 2009)?

Fourth, future research ideally will continue to employ quasi-experimental designs aimed at estimating sanction effects. Experiments allow for greater internal validity—that is, we can trust more that a given sanction produced a particular effect. At the same time, they typically do not allow for investigating the more nuanced ways in which sanctioning occurs (Mears 2010; Sampson 2010). One example is the fact that a given sanction can serve two purposes. Intensive probation might be viewed as an alternative to traditional probation. Here, then, we need to devise a study—experimental or otherwise—that addresses this particular use. It may also be viewed, however, as an alternative to jail or prison. Once, again, this use requires its own study

design. What would not be appropriate, or sufficiently nuanced in an assessment of the effectiveness of intensive probation, would be to limit our focus to the one use or the other. As suggested by the present study, the effectiveness of a given sanction may well be relative to its particular use. In addition, it may be relative to the populations to which, and settings in which, the given sanctions occur. These are issues that future research ideally will investigate.

Fifth, studies should consider other effects of sanctions. The current study focused exclusively on recidivism. There are, however, other dimensions along which to evaluate the effectiveness of sanctions. One dimension consists of whether sanctions are used for the populations for which they are intended. As this study's results show, there are many individuals in each of the four major sanction groups who greatly resemble one another with respect to such dimensions as offense type and prior record. That does not mean that the sanctions have been used inappropriately. However, it does raise questions about whether they are. Given the calls for greater federal and state government accountability, these types of assessments may facilitate efforts to show that sanctions are used in the manner in which they are intended.

There is, of course, also the important task of identifying the effects of various sanctioning regimes on crime rates and other outcomes. For example, there is a need to identify the extent to which various sanctions result in the types and amount of retribution that are intrinsic to the sanctioning process, how these sanctions affect families and communities, how they affect racial or ethnic groups in a differential way, and how cost-effective different sanctions may be (Western 2007; Gottschalk 2011; Tonry 2011; Austin 2011). Future studies might explore, too, the differential impacts of sanctions on different types of recidivism, to determine, for example, whether certain sanctions are more effective in reducing violent, property, sex, or drug offending. Such assessments are difficult to make, but nonetheless are critical for balanced assessments of policy effectiveness.

Finally, a straight-forward policy implication stems from this study and that of several recent reviews (e.g., Nagin et al. 2009; Cullen et al. 2011)—specifically, greater reductions in recidivism may be obtained through the use of less severe sanctions. As the above discussion

highlights, there are many considerations other than recidivism to consider, and more research unequivocally is needed. Yet, as states deliberate on how best to allocate scarce resources, they may well want to revisit assumptions about the benefits of tougher sanctioning.

## ENDNOTES

<sup>1</sup> There is debate about what constitutes more severe punishment. Some scholarship, for example, suggests that offenders may perceive supervision-based sanctions as more severe than prison (Crouch 1993; Petersilia and Deschenes 1994; Deschenes, Turner, and Petersilia 1995; Spelman 1995; Petersilia 1997; May et al. 2005). In general, extant theory and research does not provide a clear answer (see, e.g., Paternoster 1987; Nagin 1998; Pratt et al. 2006). Here, we recognize that although incarceration typically is viewed as a tougher sanction, offenders' perceptions of severity may vary depending on the conditions of incarceration and supervision.

<sup>2</sup> In Florida, intensive probation is officially termed "community control." It typically includes house arrest, curfew, and contact restrictions greater than that of traditional probation.

<sup>3</sup> Ancillary analyses using replacement, 1-to-many matching, and various caliper specifications revealed substantively similar findings. These results are available upon request.

<sup>4</sup> Many inmates serve approximately a year in prison. For example, for Florida inmates released during the years covered in this study, approximately 15-30 percent of released inmates in a given year served a year or less (see Table 4c, Time Served in DC Custody, Florida Department of Corrections Inmate Release reports—<http://www.dc.state.fl.us/pub/index.html>). To illustrate, in 1994, over 28 percent of inmates served one year or less, and in 2002 almost 17 percent did so. Nationally, the same pattern holds; for example, the median time served among state prison inmates released in 2008 was 16 months (Bureau of Justice Statistics 2011).

<sup>5</sup> Ancillary analyses using the full samples (and thus 1-to-many matching analyses) and other treatment group sample sizes identified results that were substantively and statistically similar; these analyses are available upon request. Use of the sub-samples enables us to obtain estimates based on a more rigorous matching approach (e.g., 1-to-1 matching and narrow caliper settings).

<sup>6</sup> For all 14 models, the variables that typically predict sentences were statistically significant in the expected directions. Because each model had a slightly different specification, there is no parsimonious way to present the full set of regression results. They are available upon request.

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**Table 1. Descriptive Statistics**

	Probation		Intensive Prob.		Jail		Prison	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Reconviction, 3 years	0.31	0.46	0.33	0.47	0.43	0.50	0.47	0.50
Black	0.37	0.48	0.40	0.49	0.59	0.49	0.56	0.50
Hispanic	0.11	0.31	0.09	0.29	0.08	0.27	0.06	0.24
White	0.52	0.50	0.50	0.50	0.34	0.47	0.38	0.49
Age (years)	30.61	10.49	30.08	10.53	32.79	10.21	32.50	9.72
Offense–murder	0.00	0.04	0.01	0.08	0.00	0.02	0.02	0.12
Offense–sexual	0.02	0.13	0.05	0.21	0.00	0.05	0.04	0.20
Offense–robbery	0.02	0.12	0.04	0.20	0.01	0.12	0.09	0.28
Offense–other violent	0.15	0.36	0.18	0.38	0.08	0.27	0.16	0.37
Offense–burglary	0.11	0.31	0.15	0.35	0.08	0.27	0.19	0.39
Offense–property	0.23	0.42	0.15	0.36	0.18	0.39	0.13	0.34
Offense–weapons	0.04	0.19	0.04	0.19	0.03	0.16	0.04	0.19
Offense–drug	0.35	0.48	0.29	0.46	0.50	0.50	0.27	0.44
Offense–other	0.09	0.29	0.10	0.30	0.11	0.32	0.07	0.26
Prior convictions (#)	0.60	1.69	1.06	2.38	0.66	1.86	1.68	3.50
Prior prison com. (#)	0.23	0.74	0.43	0.99	0.57	1.16	1.28	1.61
Supervision viol. (#)	0.42	0.90	0.82	1.20	0.89	1.17	1.36	1.43
Off. seriousness (#)	3.75	1.79	4.67	2.02	3.46	1.51	5.36	1.92
Prison eligibility	0.14	0.34	0.40	0.49	0.15	0.35	0.78	0.41
Circuit 1	0.04	0.20	0.05	0.22	0.03	0.17	0.04	0.20
Circuit 2	0.02	0.14	0.02	0.12	0.01	0.07	0.03	0.16
Circuit 3	0.01	0.09	0.01	0.11	0.00	0.04	0.01	0.11
Circuit 4	0.04	0.20	0.04	0.20	0.12	0.32	0.07	0.25
Circuit 5	0.05	0.21	0.04	0.20	0.02	0.14	0.04	0.20
Circuit 6	0.10	0.29	0.12	0.32	0.07	0.26	0.10	0.30
Circuit 7	0.04	0.19	0.04	0.20	0.04	0.19	0.04	0.21
Circuit 8	0.03	0.16	0.02	0.15	0.01	0.12	0.03	0.16
Circuit 9	0.08	0.27	0.06	0.23	0.10	0.30	0.07	0.25
Circuit 10	0.04	0.20	0.03	0.17	0.02	0.13	0.05	0.22
Circuit 11	0.10	0.30	0.08	0.28	0.24	0.43	0.08	0.26
Circuit 12	0.03	0.18	0.04	0.19	0.03	0.18	0.03	0.17
Circuit 13	0.09	0.29	0.16	0.36	0.05	0.21	0.09	0.28
Circuit 14	0.02	0.15	0.04	0.19	0.00	0.06	0.03	0.17
Circuit 15	0.05	0.21	0.02	0.15	0.12	0.32	0.05	0.21
Circuit 16	0.01	0.11	0.01	0.09	0.00	0.06	0.01	0.09
Circuit 17	0.13	0.34	0.12	0.33	0.09	0.29	0.14	0.35
Circuit 18	0.04	0.20	0.04	0.20	0.01	0.12	0.03	0.18
Circuit 19	0.03	0.17	0.02	0.13	0.02	0.14	0.03	0.18
Circuit 20	0.05	0.22	0.04	0.19	0.02	0.13	0.03	0.17
N =	318,073		53,136		132,059		83,089	

**Table 2a. Adjustment Balance Statistics, Treatment = Probation**

	Matching Group 1 = Intensive Probation					Matching Group 2 = Jail					Matching Group 3 = Prison				
	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test
Black	0.371	0.376	-1.1	83.1	-0.81	0.374	0.372	0.6	98.7	0.41	0.444	0.431	2.8	92.8	1.54
Hispanic	0.106	0.106	-0.2	95.9	-0.14	0.108	0.100	2.7	71.7	1.84	0.085	0.090	-1.5	91.0	-0.85
White	0.524	0.518	1.2	65.9	0.87	0.518	0.529	-2.2	94.2	-1.52	0.470	0.479	-1.9	93.4	-1.05
Age	30.67	30.49	1.7	72.3	1.18	30.75	30.51	2.4	88.1	1.68	31.12	30.89	2.2	87.4	1.22
Offense–murder	0.001	0.002	-1.9	77.5	-1.98*	0.001	0.001	1.2	55.7	0.73	0.002	0.002	-0.7	95.6	-0.82
Offense–sexual	0.019	0.019	-0.1	99.3	-0.10	0.016	0.014	1.5	90.8	0.87	0.028	0.026	1.3	90.3	0.77
Offense–robbery	0.015	0.016	-0.4	97.5	-0.34	0.015	0.015	0.3	57.2	0.23	0.024	0.020	1.6	95.1	1.33
Offense–other viol	0.148	0.155	-1.9	77.0	-1.35	0.146	0.145	0.2	99.2	0.10	0.168	0.172	-1.3	63.4	-0.68
Offense–burglary	0.110	0.109	0.5	95.6	0.36	0.109	0.112	-1.1	89.1	-0.75	0.131	0.135	-1.2	94.5	-0.73
Offense–property	0.230	0.230	0.0	99.9	-0.02	0.235	0.237	-0.6	95.2	-0.42	0.219	0.214	1.4	95.0	0.71
Offense–weapons	0.037	0.037	-0.3	79.1	-0.19	0.037	0.035	0.9	82.6	0.61	0.040	0.037	1.8	-3967	0.97
Offense–drug	0.343	0.339	0.8	92.2	0.56	0.345	0.337	1.7	94.8	1.20	0.292	0.297	-1.2	92.7	-0.68
Offense–other	0.097	0.093	1.4	-40.6	0.97	0.097	0.104	-2.1	65.0	-1.54	0.098	0.097	0.3	95.6	0.18
Prior convictions	0.610	0.613	-0.2	99.2	-0.16	0.605	0.565	2.3	29.7	1.66	0.384	0.403	1.0	97.4	0.80
Prior prison comm.	0.247	0.249	-0.2	98.9	-0.19	0.246	0.241	0.5	98.5	0.45	0.639	0.636	-1.5	98.1	-1.14
Supervision violat.	0.433	0.424	0.9	97.7	0.75	0.431	0.414	1.6	96.4	1.38	4.207	4.170	0.2	99.7	0.14
Off. seriousness	3.743	3.764	-1.1	97.8	-0.81	3.708	3.668	2.4	84.7	1.62	0.216	0.241	2.0	97.7	1.13
Prison eligibility	0.139	0.150	-2.6	95.8	-2.21*	0.135	0.133	0.8	68.7	0.58	0.040	0.037	-6.5	96.1	-3.34*
Circuit 1	0.040	0.038	1.0	85.0	0.74	0.040	0.041	-0.8	84.7	-0.50	0.022	0.023	1.2	1.0	0.69
Circuit 2	0.019	0.020	-0.6	77.0	-0.41	0.019	0.019	0.5	96.3	0.26	0.011	0.010	-0.7	81.1	-0.42
Circuit 3	0.009	0.008	0.4	90.8	0.31	0.008	0.006	2.5	76.4	1.46	0.058	0.051	0.8	76.3	0.43
Circuit 4	0.038	0.039	-0.4	87.2	-0.26	0.038	0.041	-1.1	96.4	-1.02	0.044	0.043	3.3	76.8	1.84
Circuit 5	0.052	0.046	2.7	48.2	1.85	0.052	0.051	0.2	98.7	0.13	0.095	0.098	0.5	90.5	0.30
Circuit 6	0.097	0.099	-0.4	94.5	-0.29	0.097	0.096	0.1	98.3	0.10	0.054	0.049	-1.2	30.9	-0.69
Circuit 7	0.040	0.039	0.4	60.7	0.29	0.040	0.040	0.3	76.4	0.18	0.026	0.030	2.5	-10.8	1.28
Circuit 8	0.028	0.027	0.1	92.6	0.09	0.027	0.025	1.5	81.9	0.98	0.078	0.073	-2.4	-143.1	-1.28
Circuit 9	0.076	0.077	-0.6	92.7	-0.37	0.076	0.080	-1.4	82.1	-1.03	0.053	0.047	1.8	17.5	1.01
Circuit 10	0.042	0.041	0.3	95.7	0.21	0.043	0.043	-0.4	97.5	-0.21	0.067	0.079	3.2	-6.9	1.71
Circuit 11	0.095	0.101	-1.9	59.3	-1.32	0.098	0.093	1.4	96.4	1.23	0.030	0.027	-4.3	42.9	-2.62*
Circuit 12	0.031	0.028	1.4	66.4	1.05	0.031	0.030	0.2	91.0	0.12	0.081	0.091	2.0	-1194	1.17
Circuit 13	0.093	0.096	-1.0	94.9	-0.78	0.093	0.093	-0.3	98.4	-0.17	0.033	0.031	-3.6	-149.0	-2.09*
Circuit 14	0.026	0.026	-0.1	98.1	-0.09	0.022	0.021	0.8	95.5	0.49	0.044	0.045	1.1	58.6	0.61
Circuit 15	0.044	0.052	-4.2	71.2	-2.54*	0.049	0.052	-0.7	97.1	-0.65	0.010	0.010	-0.8	-47.2	-0.47
Circuit 16	0.012	0.013	-0.5	87.2	-0.32	0.012	0.012	0.3	96.8	0.19	0.142	0.145	0.8	85.1	0.45
Circuit 17	0.133	0.128	1.5	31.3	1.04	0.132	0.132	-0.2	98.7	-0.10	0.037	0.036	-1.0	76.2	-0.53
Circuit 18	0.041	0.036	2.4	-185	1.75	0.040	0.038	1.0	93.7	0.59	0.043	0.038	0.4	88.3	0.24
Circuit 19	0.031	0.032	-0.5	95.0	-0.28	0.032	0.032	0.1	99.2	0.04	0.033	0.037	2.7	-140.1	1.40
Circuit 20	0.053	0.053	0.0	99.3	0.03	0.053	0.055	-1.2	93.8	-0.69	0.098	0.097	-1.9	83.0	-1.16

%B = percent bias; %BR = percent bias reduction; \* p < .05

**Table 2b. Adjustment Balance Statistics, Treatment = Intensive Probation**

	Matching Group 1 = Probation					Matching Group 2 = Jail					Matching Group 3 = Prison				
	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test
Black	0.409	0.411	-0.5	94.3	-0.35	0.424	0.420	0.9	97.4	0.62	0.424	0.431	-1.4	95.3	-0.98
Hispanic	0.090	0.088	0.7	89.8	0.50	0.089	0.086	1.2	69.5	0.78	0.086	0.087	-0.4	96.7	-0.23
White	0.501	0.501	0.1	98.2	0.06	0.486	0.494	-1.6	95.3	-1.05	0.490	0.482	1.6	93.4	1.10
Age	30.01	29.97	0.3	93.8	0.25	30.32	30.23	0.9	96.5	0.64	30.35	30.14	2.1	91.4	1.41
Offense–murder	0.005	0.005	1.6	78.3	0.91	0.004	0.003	1.8	81.0	1.17	0.006	0.004	1.5	84.7	1.45
Offense–sexual	0.047	0.018	0.5	97.0	0.30	0.025	0.026	-0.7	97.6	-0.46	0.050	0.043	3.2	-17.5	2.16*
Offense–robbery	0.039	0.039	-0.4	96.9	-0.26	0.036	0.040	-2.5	83.3	-1.45	0.041	0.037	1.6	91.8	1.36
Offense–other viol	0.175	0.189	-3.9	45.1	-2.66*	0.172	0.176	-1.2	95.9	-0.70	0.176	0.179	-0.9	79.4	-0.59
Offense–burglary	0.143	0.145	-0.5	95.3	-0.34	0.138	0.132	1.9	91.0	1.16	0.148	0.149	-0.2	98.6	-0.12
Offense–property	0.150	0.147	0.6	97.0	0.50	0.159	0.161	-0.6	92.7	-0.44	0.150	0.152	-0.7	87.2	-0.47
Offense–weapons	0.041	0.039	0.9	54.4	0.61	0.043	0.043	-0.1	99.2	-0.04	0.040	0.040	-0.1	95.3	-0.07
Offense–drug	0.303	0.286	3.6	62.5	2.64*	0.321	0.301	4.2	89.8	2.94*	0.296	0.301	-1.2	84.2	-0.83
Offense–other	0.097	0.103	-2.1	-37.8	-1.44	0.102	0.117	-5.0	14.8	-3.33*	0.094	0.094	0.1	99.0	0.05
Prior convictions	1.021	0.984	1.9	91.2	1.21	1.023	0.998	1.2	93.1	0.76	1.062	1.058	0.1	99.4	0.10
Prior prison comm.	0.443	0.439	0.4	98.2	0.26	0.466	0.472	-0.5	95.9	-0.34	0.477	0.497	-1.5	97.5	-1.32
Supervision violat	0.849	0.858	-0.8	98.1	-0.47	0.890	0.910	-1.7	45.4	-1.18	0.885	0.916	-2.4	93.8	-1.73
Off. seriousness	4.632	4.670	-2.0	95.7	-1.33	4.461	4.434	1.6	97.6	1.02	4.730	4.694	1.9	95.0	1.25
Prison eligibility	0.391	0.388	0.6	99.0	0.38	0.354	0.350	1.0	98.2	0.63	0.420	0.420	0.0	100.0	0.01
Circuit 1	0.049	0.050	-0.6	82.3	-0.39	0.051	0.057	-3.3	65.4	-1.92	0.047	0.039	3.8	-16.8	2.63*
Circuit 2	0.014	0.015	-0.8	80.7	-0.60	0.013	0.014	-0.6	93.4	-0.32	0.015	0.015	-0.7	91.5	-0.54
Circuit 3	0.014	0.014	0.0	100.0	0.00	0.011	0.010	0.9	94.0	0.51	0.014	0.014	0.2	88.6	0.13
Circuit 4	0.043	0.046	-1.5	-0.5	-1.03	0.045	0.051	-2.0	92.6	-1.72	0.046	0.040	2.3	80.1	1.81
Circuit 5	0.044	0.043	0.7	66.0	0.49	0.045	0.044	0.6	95.3	0.35	0.045	0.043	1.1	42.9	0.75
Circuit 6	0.117	0.111	1.9	72.1	1.29	0.115	0.119	-1.4	90.8	-0.87	0.115	0.123	-2.7	46.1	-1.74
Circuit 7	0.043	0.044	-0.5	85.3	-0.31	0.046	0.045	0.4	84.5	0.28	0.046	0.047	-0.6	-13.0	-0.42
Circuit 8	0.023	0.026	-1.9	-39.2	-1.32	0.024	0.023	0.7	88.4	0.44	0.024	0.025	-0.3	81.7	-0.19
Circuit 9	0.055	0.060	-1.9	78.9	-1.46	0.058	0.059	-0.3	98.0	-0.25	0.057	0.055	0.8	86.5	0.57
Circuit 10	0.030	0.029	0.6	91.3	0.50	0.030	0.030	-0.4	94.7	-0.26	0.032	0.029	1.3	86.8	0.98
Circuit 11	0.082	0.083	-0.3	95.8	-0.21	0.088	0.077	3.1	92.9	2.78*	0.079	0.084	-2.0	16.1	-1.34
Circuit 12	0.038	0.037	0.7	80.9	0.44	0.040	0.042	-1.0	59.7	-0.63	0.038	0.036	0.6	85.5	0.43
Circuit 13	0.160	0.154	1.9	90.9	1.21	0.153	0.153	-0.3	99.3	-0.14	0.148	0.154	-1.9	91.5	-1.17
Circuit 14	0.038	0.038	0.2	97.1	0.15	0.031	0.030	0.3	98.8	0.17	0.038	0.036	1.2	72.9	0.78
Circuit 15	0.020	0.021	-0.5	96.6	-0.45	0.022	0.020	0.6	98.5	0.71	0.021	0.021	0.4	97.7	0.31
Circuit 16	0.009	0.009	-0.2	94.8	-0.15	0.009	0.009	-0.1	98.1	-0.08	0.009	0.008	0.6	61.5	0.39
Circuit 17	0.122	0.122	0.0	99.0	0.02	0.124	0.122	0.8	91.6	0.54	0.129	0.139	-3.0	54.9	-2.07*
Circuit 18	0.044	0.045	-0.4	70.4	-0.31	0.041	0.039	1.5	91.7	0.86	0.044	0.043	0.3	95.0	0.18
Circuit 19	0.017	0.018	-0.7	91.6	-0.59	0.018	0.019	-1.0	49.8	-0.65	0.018	0.017	0.8	92.9	0.61
Circuit 20	0.038	0.036	0.7	88.9	0.52	0.038	0.038	0.0	100.0	0.00	0.036	0.029	3.6	8.8	2.47*

%B = percent bias; %BR = percent bias reduction; \* p < .05



**Table 2c. Adjustment Balance Statistics, Treatment = Jail**

	Matching Group 1 = Probation					Matching Group 2 = Intensive Probation					Matching Group 3 = Prison				
	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test
Black	0.585	0.591	-1.4	97.0	-0.95	0.548	0.545	0.6	98.3	0.39	0.588	0.594	-1.2	75.2	-0.76
Hispanic	0.077	0.070	2.4	78.3	1.90	0.085	0.077	2.8	49.3	1.85	0.065	0.062	1.2	82.0	0.74
White	0.338	0.339	-0.1	99.8	-0.06	0.367	0.378	-2.2	93.4	-1.45	0.347	0.343	0.7	92.4	0.40
Age	32.85	32.83	0.2	99.2	0.12	32.31	32.32	0.0	99.8	-0.03	32.57	32.52	0.6	83.8	0.35
Offense–murder	0.000	0.000	0.4	91.3	1.00	0.000	0.000	0.0	100.0	0.00	0.000	0.001	-0.5	97.3	-1.34
Offense–sexual	0.003	0.004	-1.0	93.2	-1.23	0.003	0.003	0.2	99.5	0.27	0.004	0.004	-0.1	99.6	-0.14
Offense–robbery	0.016	0.014	1.4	-3865	1.05	0.019	0.019	0.2	98.5	0.17	0.021	0.021	0.1	99.8	0.06
Offense–other viol	0.073	0.069	1.2	95.2	0.99	0.087	0.091	-1.1	96.4	-0.85	0.088	0.088	0.0	99.8	-0.03
Offense–burglary	0.084	0.081	0.9	89.0	0.67	0.093	0.087	1.9	90.4	1.36	0.103	0.098	1.6	95.0	1.07
Offense–property	0.180	0.176	1.1	91.7	0.80	0.195	0.195	-0.1	98.7	-0.06	0.208	0.206	0.4	97.2	0.20
Offense–weapons	0.027	0.026	0.7	86.8	0.57	0.032	0.033	-0.3	94.8	-0.22	0.030	0.028	1.3	74.1	0.84
Offense–drug	0.504	0.518	-2.8	91.2	-1.95	0.451	0.448	0.8	98.2	0.48	0.432	0.435	-0.6	98.9	-0.32
Offense–other	0.113	0.112	0.4	94.7	0.25	0.119	0.124	-1.8	60.2	-1.12	0.114	0.120	-2.0	84.7	-1.11
Prior convictions	0.702	0.657	2.5	55.2	1.68	0.783	0.794	-0.5	96.9	-0.35	0.846	0.867	-0.7	97.9	-0.58
Prior prison comm.	0.557	0.535	2.2	93.4	1.32	0.564	0.539	2.3	81.4	1.38	0.717	0.753	-2.6	95.0	-1.81
Supervision violat	0.888	0.904	-1.5	96.7	-0.88	0.938	0.919	1.6	73.6	1.01	1.066	1.093	-2.1	94.1	-1.39
Off. seriousness	3.466	3.461	0.3	98.1	0.26	3.574	3.534	2.3	96.6	1.59	3.652	3.637	0.9	99.2	0.54
Prison eligibility	0.141	0.131	2.9	-187.1	2.05*	0.162	0.151	2.5	96.0	1.86	0.193	0.195	-0.4	99.7	-0.25
Circuit 1	0.030	0.026	2.3	63.3	1.84	0.036	0.034	1.1	90.0	0.80	0.038	0.036	0.8	87.2	0.48
Circuit 2	0.005	0.005	-0.3	97.8	-0.30	0.006	0.006	-0.1	98.9	-0.10	0.007	0.007	-0.6	96.6	-0.49
Circuit 3	0.001	0.002	-0.3	97.0	-0.37	0.002	0.002	-0.6	95.8	-0.71	0.002	0.001	0.8	93.4	1.04
Circuit 4	0.115	0.103	4.3	84.8	2.59	0.121	0.119	0.7	97.5	0.36	0.143	0.150	-2.3	85.4	-1.12
Circuit 5	0.022	0.024	-1.3	91.2	-1.08	0.026	0.027	-0.1	99.4	-0.05	0.029	0.028	0.6	94.7	0.35
Circuit 6	0.072	0.068	1.2	86.9	0.89	0.086	0.084	0.6	96.1	0.42	0.091	0.089	0.9	91.3	0.55
Circuit 7	0.037	0.038	-0.5	-919.2	-0.34	0.044	0.046	-0.6	75.2	-0.37	0.050	0.048	1.0	73.8	0.54
Circuit 8	0.013	0.012	0.7	92.2	0.65	0.015	0.014	0.6	92.9	0.45	0.017	0.015	1.3	86.3	0.85
Circuit 9	0.096	0.099	-1.3	79.8	-0.86	0.111	0.111	0.0	100.0	0.00	0.107	0.108	-0.3	96.9	-0.16
Circuit 10	0.016	0.017	-0.3	98.2	-0.28	0.019	0.018	0.6	93.2	0.40	0.022	0.021	0.5	97.5	0.34
Circuit 11	0.240	0.245	-1.4	96.4	-0.82	0.179	0.176	0.8	98.1	0.51	0.110	0.118	-2.2	95.4	-1.46
Circuit 12	0.034	0.034	0.5	60.7	0.35	0.041	0.043	-0.9	53.6	-0.54	0.040	0.039	0.5	77.0	0.30
Circuit 13	0.046	0.051	-2.0	89.1	-1.68	0.055	0.057	-0.4	98.8	-0.37	0.063	0.057	2.1	87.3	1.36
Circuit 14	0.004	0.004	0.2	99.0	0.23	0.005	0.005	-0.5	97.8	-0.66	0.005	0.006	-0.4	97.9	-0.44
Circuit 15	0.124	0.123	0.3	99.0	0.17	0.077	0.081	-1.4	96.4	-0.86	0.079	0.079	-0.2	99.1	-0.15
Circuit 16	0.004	0.005	-0.8	92.0	-0.77	0.005	0.004	0.6	90.0	0.47	0.005	0.005	0.6	88.3	0.35
Circuit 17	0.093	0.095	-0.7	94.7	-0.51	0.112	0.110	0.7	92.6	0.47	0.127	0.128	-0.5	97.1	-0.27
Circuit 18	0.014	0.014	-0.1	99.3	-0.12	0.017	0.022	-2.9	82.9	-2.19*	0.019	0.019	0.2	98.6	0.12
Circuit 19	0.020	0.021	-0.8	88.3	-0.60	0.024	0.022	1.8	-11.9	1.04	0.027	0.024	1.7	80.3	1.04
Circuit 20	0.015	0.014	0.2	99.2	0.18	0.017	0.019	-1.1	92.5	-0.87	0.019	0.020	-1.0	90.6	-0.66

%B = percent bias; %BR = percent bias reduction; \* p < .05

**Table 2c. Adjustment Balance Statistics, Treatment = Jail (cont.)**

	Matching Group 4 = Prison (≤12 Mos.)					Matching Group 5 = Prison (≤12-24 Mos.)				
	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test
Black	0.584	0.594	-2.2	19.8	-1.23	0.586	0.596	-2.0	49.8	-1.20
Hispanic	0.061	0.058	1.3	87.4	0.72	0.064	0.059	2.0	75.2	1.22
White	0.355	0.348	1.6	79.5	0.90	0.349	0.344	1.0	87.5	0.62
Age	32.64	32.54	1.0	82.8	0.55	32.47	32.51	-0.4	94.7	-0.22
Offense–murder	0.000	0.000	-0.4	93.6	-0.58	0.000	0.000	-0.6	93.3	-1.00
Offense–sexual	0.004	0.005	-0.5	97.1	-0.40	0.004	0.004	-0.2	98.8	-0.27
Offense–robbery	0.024	0.021	1.3	93.5	0.85	0.022	0.022	-0.3	98.6	-0.23
Offense–other viol	0.094	0.090	1.2	94.2	0.72	0.090	0.086	1.2	95.1	0.77
Offense–burglary	0.108	0.111	-0.9	96.0	-0.49	0.100	0.104	-1.4	94.8	-0.89
Offense–property	0.209	0.205	1.2	45.5	0.64	0.210	0.202	2.2	69.2	1.21
Offense–weapons	0.032	0.029	1.7	58.9	0.94	0.029	0.031	-0.9	83.6	-0.54
Offense–drug	0.416	0.429	-2.7	92.3	-1.46	0.432	0.433	-0.2	99.5	-0.12
Offense–other	0.112	0.109	0.9	84.9	0.49	0.114	0.118	-1.2	84.6	-0.68
Prior convictions	0.896	0.903	-0.3	98.9	-0.16	0.859	0.867	-0.3	99.0	-0.22
Prior prison comm.	0.786	0.811	-1.8	96.1	-1.11	0.732	0.758	-1.9	96.2	-1.25
Supervision violat	1.134	1.148	-1.1	97.3	-0.66	1.085	1.099	-1.1	97.3	-0.70
Off. seriousness	3.772	3.771	0.1	99.8	0.07	3.668	3.657	0.7	99.2	0.42
Prison eligibility	0.215	0.232	-4.1	95.7	-2.35*	0.199	0.203	-1.0	99.3	-0.61
Circuit 1	0.035	0.033	1.1	15.9	0.55	0.038	0.037	0.5	87.8	0.27
Circuit 2	0.008	0.008	0.1	99.4	0.10	0.007	0.005	1.6	90.4	1.53
Circuit 3	0.002	0.002	0.2	97.9	0.19	0.002	0.001	0.8	93.3	0.82
Circuit 4	0.142	0.150	-2.8	76.0	-1.30	0.142	0.153	-3.8	75.2	-1.83
Circuit 5	0.033	0.032	0.3	97.1	0.15	0.030	0.031	-0.8	92.3	-0.49
Circuit 6	0.098	0.093	1.8	86.2	0.95	0.091	0.091	0.2	97.9	0.15
Circuit 7	0.052	0.050	1.3	80.0	0.65	0.050	0.048	1.3	78.9	0.70
Circuit 8	0.019	0.020	-0.7	92.9	-0.39	0.017	0.016	0.8	91.5	0.53
Circuit 9	0.085	0.089	-1.7	88.7	-0.89	0.106	0.104	0.5	95.2	0.27
Circuit 10	0.026	0.025	0.6	97.0	0.40	0.023	0.024	-0.7	96.4	-0.50
Circuit 11	0.097	0.102	-1.4	97.3	-0.90	0.108	0.110	-0.8	98.5	-0.51
Circuit 12	0.035	0.036	-0.5	91.7	-0.24	0.040	0.037	1.9	42.0	1.01
Circuit 13	0.069	0.065	1.4	88.8	0.76	0.064	0.063	0.1	99.6	0.03
Circuit 14	0.006	0.005	1.4	90.7	1.09	0.006	0.006	-0.2	98.8	-0.22
Circuit 15	0.066	0.075	-3.3	87.9	-1.97*	0.074	0.079	-1.7	93.6	-1.05
Circuit 16	0.006	0.006	-0.7	85.1	-0.35	0.005	0.005	0.8	85.0	0.47
Circuit 17	0.148	0.141	2.0	93.3	1.10	0.131	0.128	0.9	95.9	0.50
Circuit 18	0.022	0.018	2.5	70.1	1.41	0.020	0.018	1.0	90.9	0.67
Circuit 19	0.032	0.028	2.2	78.4	1.22	0.028	0.024	2.5	73.8	1.53
Circuit 20	0.019	0.021	-1.0	86.4	-0.58	0.019	0.020	-0.3	97.0	-0.18

%B = percent bias; %BR = percent bias reduction; \* p < .05

**Table 2d. Adjustment Balance Statistics, Treatment = Prison**

	Matching Group 1 = Probation					Matching Group 2 = Intensive Probation					Matching Group 3 = Jail				
	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test	Treated	Matched	%B	%BR	t-test
Black	0.558	0.563	-0.9	97.7	-0.64	0.541	0.548	-1.4	95.6	-0.94	0.583	0.592	-1.7	67.8	-1.13
Hispanic	0.061	0.060	0.4	97.7	0.33	0.064	0.061	0.8	92.8	0.64	0.062	0.065	-1.2	83.5	-0.79
White	0.380	0.377	0.7	97.6	0.50	0.395	0.391	0.9	96.3	0.64	0.355	0.343	2.4	74.1	1.57
Age	32.49	32.60	-1.1	93.9	-0.83	32.11	32.16	-0.4	98.2	-0.30	32.59	32.51	0.8	72.4	0.53
Offense–murder	0.015	0.014	1.0	93.5	0.53	0.016	0.016	-0.7	91.8	-0.41	0.006	0.005	1.1	93.6	0.86
Offense–sexual	0.038	0.038	0.1	99.5	0.04	0.040	0.043	-1.4	71.2	-0.95	0.027	0.025	1.2	95.3	0.68
Offense–robbery	0.087	0.078	4.4	86.7	2.47*	0.082	0.075	2.9	85.7	1.77	0.066	0.064	0.5	98.4	0.31
Offense–other viol	0.152	0.161	-2.6	-160.4	-1.77	0.160	0.161	-0.5	92.9	-0.32	0.160	0.161	-0.4	98.0	-0.25
Offense–burglary	0.195	0.189	1.8	92.7	1.15	0.194	0.189	1.4	89.0	0.93	0.176	0.173	0.9	97.5	0.51
Offense–property	0.134	0.138	-0.9	96.4	-0.72	0.133	0.137	-1.2	76.7	-0.81	0.148	0.149	-0.4	97.3	-0.24
Offense–weapons	0.036	0.038	-1.0	-41.6	-0.68	0.037	0.043	-3.0	-98.2	-2.01*	0.039	0.039	0.1	98.6	0.04
Offense–drug	0.268	0.268	0.0	100.0	0.00	0.263	0.263	0.0	99.2	0.03	0.300	0.294	1.2	97.5	0.81
Offense–other	0.074	0.076	-0.9	86.4	-0.70	0.075	0.073	0.9	90.0	0.67	0.080	0.089	-3.4	76.3	-2.30*
Prior convictions	1.715	1.706	0.4	99.2	0.19	1.648	1.715	-2.3	89.8	-1.38	1.500	1.478	0.8	97.9	0.45
Prior prison comm.	1.264	1.233	2.5	97.0	1.33	1.102	1.098	0.3	99.5	0.19	1.275	1.302	-1.9	96.2	-1.08
Supervision violat	1.374	1.407	-2.8	96.5	-1.56	1.289	1.319	-2.3	94.6	-1.40	1.399	1.419	-1.5	96.0	-0.90
Off. seriousness	5.363	5.342	1.1	98.7	0.77	5.376	5.348	1.4	96.0	0.97	5.063	5.037	1.5	98.7	0.94
Prison eligibility	0.784	0.789	-1.3	99.2	-0.86	0.769	0.777	-1.6	98.1	-1.19	0.745	0.754	-2.5	98.5	-1.42
Circuit 1	0.040	0.040	0.2	85.9	0.11	0.042	0.038	1.7	72.7	1.27	0.042	0.042	-0.3	95.0	-0.15
Circuit 2	0.024	0.023	1.0	73.2	0.70	0.024	0.025	-0.3	95.1	-0.19	0.021	0.021	0.5	96.9	0.27
Circuit 3	0.013	0.015	-1.7	67.2	-1.02	0.013	0.014	-0.8	-109.8	-0.51	0.007	0.007	0.1	99.0	0.09
Circuit 4	0.067	0.060	3.1	74.5	2.01	0.063	0.062	0.7	93.6	0.42	0.077	0.084	-2.4	86.1	-1.61
Circuit 5	0.042	0.045	-1.5	48.4	-1.08	0.043	0.043	-0.1	92.6	-0.04	0.038	0.035	1.6	86.9	0.94
Circuit 6	0.099	0.095	1.1	-31.1	0.79	0.102	0.110	-2.5	61.2	-1.71	0.101	0.094	2.6	72.0	1.58
Circuit 7	0.048	0.050	-0.7	86.7	-0.49	0.047	0.046	0.6	80.3	0.42	0.050	0.050	0.1	98.9	0.04
Circuit 8	0.029	0.032	-2.0	11.3	-1.31	0.030	0.030	-0.1	95.3	-0.09	0.029	0.028	0.6	94.1	0.32
Circuit 9	0.071	0.072	-0.5	83.5	-0.33	0.070	0.064	2.1	66.2	1.38	0.078	0.084	-2.0	79.5	-1.30
Circuit 10	0.046	0.046	-0.1	84.4	-0.10	0.044	0.042	0.8	91.4	0.50	0.034	0.034	0.5	96.6	0.34
Circuit 11	0.074	0.069	2.0	79.6	1.54	0.078	0.072	2.3	34.6	1.59	0.086	0.086	0.0	99.9	0.03
Circuit 12	0.028	0.029	-0.3	87.5	-0.21	0.029	0.030	-0.2	96.8	-0.13	0.030	0.027	1.8	46.6	1.19
Circuit 13	0.092	0.087	1.5	-473.2	1.04	0.097	0.103	-2.0	89.5	-1.51	0.090	0.089	0.2	98.7	0.13
Circuit 14	0.031	0.031	-0.4	88.8	-0.29	0.032	0.034	-1.2	63.6	-0.82	0.020	0.019	0.5	97.8	0.28
Circuit 15	0.048	0.050	-1.0	-111.8	-0.72	0.042	0.040	0.8	94.1	0.52	0.056	0.057	-0.4	98.5	-0.30
Circuit 16	0.007	0.008	-0.6	88.8	-0.49	0.008	0.007	0.8	47.9	0.61	0.006	0.007	-1.0	82.4	-0.57
Circuit 17	0.143	0.145	-0.5	84.2	-0.34	0.142	0.143	-0.2	96.1	-0.15	0.141	0.143	-0.6	96.3	-0.35
Circuit 18	0.033	0.035	-0.9	80.6	-0.63	0.034	0.034	-0.3	94.0	-0.20	0.027	0.025	1.6	87.1	0.96
Circuit 19	0.036	0.037	-0.6	81.2	-0.38	0.030	0.030	0.1	99.4	0.04	0.035	0.037	-1.3	86.5	-0.74
Circuit 20	0.031	0.033	-1.3	87.1	-1.00	0.032	0.034	-0.9	78.1	-0.65	0.031	0.032	-0.5	94.1	-0.31

%B = percent bias; %BR = percent bias reduction; \* p < .05

**Table 3. Sanction Effects on Recidivism: An Assessment Using Propensity Score Matching and Average Effect on the Treated (ATT) Estimates**

	Treated	Matched	Difference	S.E.	<i>t</i> -test	% Off Support
<b>Treatment=Probation vs. . . .</b>						
Intensive Probation	0.310	0.339	-0.029*	0.007	-4.300	1.2
Jail	0.311	0.347	-0.036*	0.007	-5.340	0.8
Prison	0.328	0.469	-0.141*	0.009	-16.400	36.4
<b>Treatment=Intensive Probation vs. . . .</b>						
Probation	0.336	0.332	0.004	0.007	0.550	0.0
Jail	0.346	0.375	-0.029*	0.007	-4.180	6.8
Prison	0.332	0.453	-0.121*	0.007	-17.070	7.0
<b>Treatment=Jail vs. . . .</b>						
Probation	0.430	0.398	0.032*	0.007	4.550	0.1
Intensive Probation	0.422	0.395	0.027*	0.008	3.550	17.0
Prison (All)	0.447	0.538	-0.091*	0.008	-11.060	27.1
Prison (12 Mos. or Less)	0.450	0.551	-0.101*	0.009	-11.280	38.2
Prison (24 Mos. or Less)	0.447	0.550	-0.103*	0.008	-12.280	29.4
<b>Treatment=Prison vs. . . .</b>						
Probation	0.474	0.370	0.104*	0.007	15.000	0.3
Intensive Probation	0.467	0.329	0.138*	0.007	19.440	6.7
Jail	0.498	0.441	0.058*	0.008	7.510	15.6

\*  $p < .001$