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2012

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Daniel P. Mears, Joshua C. Cochran and William D. Bales



PRINT VERSION CITATION: Mears, Daniel P., Joshua C. Cochran, and William D. Bales.
2012. "Gender Differences in the Effects of Prison on Recidivism." *Journal of Criminal Justice*
40(5):370-378.

PRE-PRINT VERSION

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Daniel P. Mears, Joshua C. Cochran, and William D. Bales

* Direct correspondence to Daniel P. Mears, Mark C. Stafford Professor of Criminology, Florida State University, College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, e-mail (dmears@fsu.edu), phone (850-644-7376), fax (850-644-9614). Joshua C. Cochran, M.S., Florida State University, College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, email (jcochran@fsu.edu), phone (850-644-4050), fax (850-644-9614). William D. Bales, Ph.D., Professor, Florida State University, College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, e-mail (wbales@fsu.edu), phone (850-644-7113), fax (850-644-9614). We thank the Florida Department of Corrections for permission to use their data. The views expressed here are those of the authors and do not reflect those of the Department of Corrections.

BIOGRAPHICAL SKETCHES

Daniel P. Mears, Ph.D., is the Mark C. Stafford Professor of Criminology at Florida State University's College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, phone (850-644-7376), fax (850-644-9614), e-mail (dmears@fsu.edu). He conducts basic and applied research, and his work has appeared in leading crime and policy journals and a book, *American Criminal Justice Policy* (Cambridge University Press 2010).

Joshua C. Cochran, M.S., is a doctoral candidate at Florida State University's College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, phone (850-644-4050), fax (850-644-9614), e-mail (jcochran@fsu.edu). His research interests include theory, international comparative analyses of criminal justice systems, and prisoner reentry. He has published in *Justice Quarterly*, *Crime and Delinquency*, and other crime and justice journals.

William D. Bales, Ph.D., is a Professor at Florida State University's College of Criminology and Criminal Justice, 634 West Call Street, Tallahassee, FL 32306-1127, phone (850-644-7113), fax (850-644-9614), e-mail (wbales@fsu.edu). He focuses on a range of crime and policy topics, including the effectiveness of electronic monitoring and tests of labeling theory. He has published in *Criminology*, *Criminology and Public Policy*, and other crime and policy journals.

Gender Differences in the Effects of Prison on Recidivism

ABSTRACT

Purpose: This study examines gender differences in the effectiveness of prison in reducing recidivism. Methods: Using data on released male and female prisoners, we apply a propensity score matching methodology to compare the effects of prison on recidivism versus three counterfactual conditions—jail, intensive probation, and probation. Results: The analyses indicated that a prison term, as compared to placement on intensive probation or traditional probation, is associated with a greater likelihood of property and drug recidivism. There was little evidence that recidivism was greater when compared to jail, that prison increased the likelihood of violent or other recidivism, or that the criminogenic effect of prison is appreciably greater for females or males. Conclusions: The findings do not support arguments that prison is an effective alternative to non-incarcerative punishments or that it exerts a differential effect on females or males. Further research is needed on what features of the prison experience contribute to the observed effects.

KEYWORDS: prison gender effectiveness recidivism

Gender Differences in the Effects of Prison on Recidivism

HIGHLIGHTS

A prison term is associated with an increased likelihood of recidivism.

Prison appears to increase property and drug, and not violent or other, recidivism.

This effect arises primarily in comparison to probation, not jail.

The criminogenic effect of prison is similar for females and males.

Prison is more likely to increase drug recidivism among males.

Gender Differences in the Effects of Prison on Recidivism

INTRODUCTION

In recent years, the large-scale growth in American prison populations has led to increased attention to understanding the effectiveness of incarceration in reducing recidivism. Even so, the body of research that has investigated this topic is limited. Recently, for example, Nagin et al. (2009) reviewed rigorous empirical studies that assessed the impacts of imprisonment on recidivism. They found that few such studies existed, that existing studies suffered from significant methodological flaws, and that the available evidence pointed to conflicting findings, with some suggesting that prison decreases recidivism, others finding that it has no effect, and still others finding that it increases recidivism. They concluded that, on balance, the weight of the evidence indicated that, when compared to non-custodial sanctions, “incarceration appears to have a null or mildly criminogenic effect on future criminal behavior” (p. 115). Notably, none of the reviewed studies examined the relative effects of imprisonment for males and females, respectively, or, by extension, compared gender differences in the effects of prison as compared to different types of alternative sanctions.

This gap in research is notable for several reasons. First, given the costs of incarceration, we would expect, as Cullen et al. (2011) have argued, a greater body of empirical evidence to support investing in more of it. There are, of course, other goals of incarceration beyond reduced recidivism. For example, reduced crime rates, retribution, and justice all are goals that may be achieved, to some extent, through the use of prisons (Mears & Barnes 2010). Regardless, reduced recidivism constitutes a central goal that, if achieved through incarceration, would bolster arguments for greater investments in prisons and that, if not achieved would undermine such arguments. Second, a wide range of sanctioning alternatives exist (MacKenzie 2006). Accordingly, if prisons do not reduce recidivism, or if they worsen it, then there are options available to the courts for punishing offenders. By extension, these options may merit more

attention, especially if they achieve other criminal justice goals equally well. Third, a large literature suggests that the causes of offending may differ for females and that traditional correctional interventions may have different effects for them. Reviews of this literature suggest that, in general, more similarities than differences exist along these dimensions (see, e.g., Gartner 2011). In contrast to these assessments, however, are those that argue that extant research is not sufficiently well-developed to justify this claim that, in fact, substantial differences do exist (see, e.g., Van Voorhis 2012). Lipsey and Cullen (2007:312), for example, have noted that gender differences in the effects of correctional rehabilitation programs have not been well-examined “in part because most treatment studies use all male or nearly all male samples.”

The goal of this study is to contribute to scholarship aimed at understanding whether prison exerts a beneficial or criminogenic effect on recidivism and whether the effect varies among males and females. More specifically, the study is the first, to our knowledge, to examine if imprisonment increases or decreases violent, property, drug, and other recidivism among males and females, respectively, as compared to traditional probation, intensive probation, and jail. Drawing on data on correctional populations in Florida, we employ propensity score matching analyses to estimate imprisonment effects. Below, we situate the study relative to the broader context of mass incarceration, research on imprisonment and recidivism, and gender differences in offending, sanctioning, and sanction effects. We then describe the data, methods, and findings, and discuss the implications of the study for research and policy.

BACKGROUND

Mass Incarceration

During the past three decades, there has been historically unprecedented growth in the correctional system, including the United States probation, jail, and prison populations, respectively (Mears 2010). This growth has, if anything, been more pronounced for females

(Davidson 2011). For example, between 2000 and 2010, the number of males under the jurisdiction of state or federal correctional authorities increased from 1,298,027 to 1,402,624, for an average annual percentage increase of 1.7 percent (Guerino et al. 2011:2). The number of females under such jurisdiction increased from 93,234 to 112,822, for an average annual percentage increase of 2.2 percent. In absolute terms, females still comprise a much smaller percentage of the correctional system population than do males, but the gap is shrinking.

Considerable scholarship has been aimed at understanding the causes and consequences of mass incarceration (see, e.g., Beckett & Western 2001; Garland 2001; Gottschalk 2006, 2011; Spelman 2006; Simon 2007; Blumstein 2011). Among the causes that have been suggested has been the notion that “softer” sanctions, such as probation and a range of intermediate sanctions, which typically involve some combination of surveillance and treatment approaches (Tonry & Lynch 1996), were viewed as ineffective (Petersilia 1995, 1997), even though some studies have suggested that these sanctions may be viewed by offenders as, in some cases, tougher than a prison term (see, e.g., Petersilia & Deschenes 1994; Spelman 1995; Wood & Grasmick 1999). A greater reliance on prison thus derives in part from the perception that imprisonment is better than the options, including traditional probation, intensive probation, and short stays in jail, that typically have been and would be used to punish offenders. The logical question that thus arises is whether imprisonment reduces recidivism as compared to these counterfactual conditions.

Imprisonment and Recidivism

Calls for evidence-based policy have increased in recent decades (Welsh & Harris 2008). Logically, then, given the large-scale increase in prison populations in the United States during this same period, we would anticipate that a credible foundation of empirical research established that prisons reduce recidivism more so than non-incarcerative sanctions do. There is, however, little clear evidence that increased reliance on imprisonment has reduced crime rates (see, however, Spelman 2006). There also is little evidence that imprisonment reduces recidivism.

Indeed, most reviews (e.g., Gendreau et al. 1999; McDougall et al. 2003; Killias et al. 2006), including a recent examination of studies with strong methodological designs (Nagin et al. 2009; see also Cullen et al. 2011), have found that the evidence points either to imprisonment having no effect on recidivism or modestly increasing it.

The broader finding, however, has been that most prior studies have suffered from methodological limitations that rendered the conclusions from them questionable. As Nagin et al. (2009:177) concluded in their review: “rigorous investigations of the effect of incarceration on reoffending are in short supply.” Few studies, for example, employed matching designs to ensure that such factors as age, race, prior record, and type of offense were comparable for prison and non-incarceration sanction groups. Such factors are, as the authors emphasized, critical because they influence the types of sanctions that individuals receive and are related to recidivism (p. 152). Prior studies also have not systematically compared imprisonment to the main counterfactual conditions that confront the courts—that is, traditional probation, intensive probation, and jail. Studies typically combine these alternative sanctions or focus only on one or the other type of alternative. In addition, the studies have not compared males and females. Indeed, most rigorous studies of imprisonment effects have not included females at all, and among those that have the focus was on controlling for or matching on sex not examining differences in the effects of prison on males and females, respectively.

Gender Differences in Offending, Sanctioning, and Sanction Effects

The literature on gender, offending, and sanctions is vast, with numerous theoretical and empirical accounts that argue for and against different explanations for the criminal behavior, sentencing, and rehabilitative treatment of males and females, respectively. A substantial body of this work endeavors to show how societal views shape what is meant by “being” a “man” or “woman” or a “boy” or “girl.” This work examines how these views shape the ways in which males and females perceive themselves and how they act, and also how punishments and

rehabilitative treatment may affect each group differently. Not least, it also examines biological differences that may contribute to differences in offending. In general, however, this literature, including meta-analyses, the developmental criminological literature, and correctional intervention studies, has identified several common themes, as Gartner's recent review (2011) highlights (see also Zahn et al. 2010; Andrews et al. 2012; Baxendale et al. 2012; cf. van der Put et al. 2012; Van Voorhis 2012). We briefly describe several of these themes and do so with the goal of underscoring that substantial scholarship exists to suggest that, on the one hand, imprisonment may have different effects for males and females, and, on the other hand, that it more likely exerts a similar effect.

First, empirical studies typically have found that males engage in more, and more violent, offending. Both males and females engage in a wide range of offenses, but some differences in types, or sub-types, of offending exist. Females, for example, are more likely to kill intimate partners (Gartner 2011:352) and to engage in embezzlement and prostitution (p. 354). Similarly, the trajectories and causes of crime appear to be more similar than not. As Gartner (2011) has emphasized, for example, "while there are sex differences in the trajectories into and out of offending—for example, females' involvement in crime tends to begin, peak, and end earlier—these are differences more of degree than kind" (p. 358). Similarly, despite the "considerable debate over whether females' and males' motivations for crime are distinctly different," "research suggests that on the whole they are not" (p. 358). For example, exposure to delinquent peers, weak informal social control, and residing in resource-deprived areas predict offending among both males and females and typically do so equally well. Echoing this view, Andrews et al. (2012:127) have reported that "gender neutrality appears to be the rule in regard to the predictive validity of risk/need factors in the analysis of criminal recidivism. If a factor is predictive with female offenders, it will be predictive with male offenders, and vice versa."

That said, sex differences in offending do exist, and are thought to result from a variety of factors (see, e.g., Benda 2005; van der Put et al. 2012; Van Voorhis 2012). As Gartner (2011:359) has noted, girls are socialized differently than boys—they are, for example, more

likely to be discouraged from engaging in risky behavior, to empathize with others, and to value relationships with family, friends, and others. Biological differences also may account for some differences in offending among males and females. For example, research tends to find that females have more impulse control and better appreciate the consequences of what they do. They also may be less vulnerable to delinquent peer influence and inadequate parental supervision (p. 360). At the same time, females are more likely to experience abuse and victimization and to engage in drug abuse as a response. In addition, mental illness, trauma, and perceptions of self-efficacy may affect females differently than males and in ways that contribute differently to offending (Van Voorhis 2012). Thus, while there are similarities in the causes of offending among males and females, there may be significant differences in the magnitude and nature of these causes, and there may be causal factors that are unique to male offending and female offending, respectively. In addition, there may be differences in the extent to which various causal factors that are similar for males and females (e.g., delinquent peer association) are moderated by other factors (e.g., perceptions of the wrongfulness of criminal behavior) (see, e.g., Mears et al. 1998).

When we turn to sanctioning, similarities and differences again surface. For example, males are more punitively sanctioned even after controlling for legal factors, including the severity of the offense. Even so, in recent decades, females increasingly have received tougher treatment from the criminal justice system, and for both groups factors such as prior record and type of offense predict sentencing severity. Historically, female offenders have been less likely to be arrested or charged or, if convicted, to be incarcerated (Gartner 2011:369; see also Chesney-Lind & Pasko 2004). Three explanations typically have been offered for this historical pattern. The first, termed the chivalry hypothesis, is that the actors in the criminal justice system, who typically are males, view females in a gendered way—that is, they view females as moral beacons who, by extension, warrant greater courtesy and generosity in how they are treated. The second argues that criminal justice actors tend to act paternalistically toward females, viewing them as the “weaker sex” and thus in need of special care and treatment, and, concomitantly,

viewing males as more aggressive and dangerous and thus as warranting a tougher response. The third argues that the courts may view tougher treatment of females as having a more adverse effect on children and families and so sentence females less harshly. Notably, studies suggest that males and females tend to view the severity of different criminal justice sanctions in a similar way, although females tend to embrace alternative sanctions more so than males and also are more likely to view prison as a punitive sanction relative to non-incarcerative alternatives (see, e.g., Wood & Grasmick 1999).

Before turning to a focus on sanction effects, there is the question of whether there are gender differences in the effectiveness of rehabilitative programming. The effectiveness of correctional interventions has been well-examined in the literature (see, e.g., MacKenzie 2006; Lipsey & Cullen 2007). However, with rare exception (see, e.g., Pelissier et al. 2003), few studies have systematically compared the relative effectiveness of various programs on males versus females. The challenge, in part, is that programs may target females rather than males, or vice versa, rendering direct comparisons of these programs of questionable validity. Even so, reviews typically conclude that offender rehabilitation programs can be effective in reducing female recidivism (see, e.g., Tripodi et al. 2011). Notably, the programs identified in such reviews typically adhere to principles of effective correctional interventions (Cullen & Gendreau 2000). These principles, such as the targeting of criminogenic needs and treatment responsivity, appear logically to apply equally well to males and females. At the same time, scholars have persuasively argued that gender differences should guide programming both because the causes of offending may vary for males and females (see, e.g., Steffensmeier & Allan 1996; Uggen & Kruttschnitt 1998) and also because each group may respond better to programming that reflects their unique risks, such as differences in childhood sexual abuse and in how such abuse is perceived, and responsivity to certain treatment modalities (see, e.g., Tripodi et al. 2011; Van Voorhis 2012). In addition, the family and economic circumstances of females may differ from those of males, which in turn may influence the effectiveness of different sanctions, including prison (see, e.g., Cobbina et al. 2012).

As emphasized above, little is known about the effects of prison on recidivism. By extension, there is even less known about gender differences in these effects. There is, to be sure, a considerable body of work on various interventions that may reduce female recidivism. However, this work typically focuses on a particular population (e.g., females in prison) or a particular program (e.g., a drug treatment program for individuals on probation). It does not systematically compare different sanctions and their relative effectiveness among males and females. Indeed, to the best of our knowledge, such an assessment has not been undertaken in the literature despite the wealth of research examining gender differences in offending and treatment responsiveness. For example, in Nagin et al.'s (2009) systematic review of rigorous empirical assessments of the impact of imprisonment on recidivism, no study was identified that compared males and females with respect to the impact of prison, as compared to different types of non-incarceration sanctions, on subsequent types of criminal offending.

THIS STUDY

Overview

The goal of this study is to investigate how imprisonment affects recidivism among males and females. To this end, it uses data on convicted felons in Florida, drawing on measures from the Florida Department of Corrections Sentencing Guidelines database and the Offender-Based Information System, and propensity score matching. The matching analyses enable us to answer this question: What is the effect of imprisonment as compared to three counterfactual conditions—traditional probation, intensive probation, and jail—on violent, property, drug, and other recidivism, and do these effects vary by gender? To the best of our knowledge, this study is the first to employ a matching design that systematically compares the relative effectiveness of imprisonment for males versus females and for the three above-specified sets of counterfactual conditions. The data and the matching approached used here are discussed below.

Data

We use data on a random sample of 10,000 males and the full population of 7,550 females released from Florida prisons between 1994 and 2002 and reconvicted of a felony offense within three years of release. These individuals then, as described further below, were matched to convicted felons who received alternative sanctions, including traditional probation (N = 318,073 males, N = 91,516 females), intensive probation (N = 53,136 males, N = 12,258 females), and jail (N = 132,059 males, N = 22,775 females). Intensive probation in Florida is termed “community control,” but refers to conditions, such as house arrest, curfew, and contact restrictions, similar to what other states use when describing intensive probation. Collectively, these options constitute the three main categories of sanctioning, along with imprisonment, typically employed by states. Their use here thus enhances the policy relevance of the study and increases the ability of future studies to draw meaningful comparisons to the present one.

Measures

Our focus is on recidivism and the main dependent variable is reconviction for a felony within three years of release from prison or jail or, for individuals on traditional probation or intensive probation, from sentencing. The use of a three-year window and felony reconviction ensures that our focus is on individuals who are most likely to fail within a year or two of release and who commit serious offenses (Langan & Levin 2002; Killias et al. 2006; Kurlychek et al. 2006). Although the bulk of research to date suggests, broadly, that males and females engage in similar kinds of offending and that the causes of offending are similar (Gartner 2011; Andrews et al. 2012), some scholarship suggests otherwise (see, e.g., Van Voorhis 2012). Accordingly, we examine four different types of recidivism: violent, property, drug, and other.

The credibility of the matching analyses ultimately rests on the availability and quality of the

observed covariates to address bias that might influence both selection into “treatment” (i.e., imprisonment) and recidivism. Accordingly, for the male and female samples, respectively, we use measures that prior research has identified as robust predictors of sentencing and recidivism (see, e.g., Engen & Gainey 2000; Ulmer & Johnson 2004; Kubrin & Stewart 2006; Nagin et al. 2009; Doerner & Demuth 2010; Wang et al. 2010; Wooldredge 2012). The measures include: race and ethnicity (black, white, Hispanic), age, offense type (e.g., murder, sexual assault, robbery), counts of prior felony convictions in general and by type of offense (violent, sexual assault, property, drug, and other), prior prison commitments, prior supervision violations, an offense severity guidelines score (1= least serious type of offense and 10=most serious type of offense), a measure from the guidelines database that identifies whether an offender’s total sentencing score allowed, or made them “eligible” for, a prison sentence, and a series of dummy variables that identify the court circuit, based on the location of the offense, to address contextual factors that might contribute to sentencing and recidivism.

Methods

To examine the effects of prison on recidivism, we employ propensity score matching analyses. Specifically, we estimate average treatment effects on the treated (ATT), where the counterfactual question is: “What would the likelihood of violent, property, drug, or other recidivism have been among males and females had individuals who were imprisoned instead not been imprisoned?” Three conditions logically define the set of counterfactual conditions for what otherwise would have happened to these individuals. Specifically, they could have been placed on traditional probation or intensive probation or in jail.

The matching methodology constitutes a quasi-experimental approach to estimating an imprisonment effect. The challenge for this and related approaches, such as traditional multivariate regression modeling, lies in the fact that individuals assigned to prison likely differ from individuals assigned to traditional or intensive probation or jail, and they also likely differ

in turn in their likelihood of recidivating. Accordingly, an approach is needed that addresses the selection effect problem. Matching analyses fulfill this objective by first creating estimated probabilities, for all individuals, whether in prison or in any of the other sanction categories, of receiving the treatment (here, imprisonment), and then matching individuals based on these probabilities. The matching methodology has several advantages over regression-based estimating procedures, including the ability to ensure that individuals in the treatment group have actual counterparts in the counterfactual condition (for general discussions and examples from criminological research, see, generally, Rosenbaum & Rubin 1983; Becker & Ichino 2002; Apel & Sweeten 2010; Guo & Fraser 2010). Given variation in sentencing (Reitz 2011)—for example, convicted felons who are similar in many respects, including the type of offense committed and prior record, may be sentenced differently both within and across jurisdictions—the logic of a quasi-experimental design is further justified, especially in a context in which true random assignment is unlikely or not possible (Nagin et al. 2009). Ultimately, however, and as with any similar type of assessment, the accuracy of the estimated effects depends on the extent to which unobserved confounding is limited or does not greatly affect the estimates. That is, matching analyses simply reduce the likelihood that confounding may bias estimated treatment effects, but do not eliminate it (Smith 1997; Winship & Morgan 1999).

For the analyses, we created two samples, one consisting of convicted males and the other of convicted females. For each group, the first step consisted of estimating the likelihood of imprisonment relative to a given counterfactual sanction. These propensity scores, which provide estimates of the conditional probability of imprisonment, were created using logistic regression and the above-identified matching covariates. As recommended in the propensity score matching literature, we created polynomial terms where necessary to increase the ability of the models to create balance on the covariates. The latter goal constitutes the next step, and is the main focus of the analyses presented here. That is, we identify, using post-matching balance statistics, that balance on the covariates could be achieved. We then turn to the logical next step—identifying matches between individuals in the imprisonment “treatment” group and

individuals in each of the three counterfactual groups. To this end, we used Stata 12's PSMATCH 2 command, and applied a .005 caliper setting and 1-to-1 nearest neighbor matching without replacement. The narrow caliper setting, as well as the use of only 1-to-1 matching and not allowing replacements, provides a conservative foundation on which to identify an imprisonment effect (DiPrete & Gangl 2004; Apel & Sweeten 2010). After matching, if there is no imbalance with respect to unobserved confounders, differences in the recidivism of ex-prisoners and the comparison groups can be viewed as reflecting the effects of prison (Winship & Morgan 1999). In the findings below, we discuss the results of the post-matching balance assessment for males and females, respectively, the estimated imprisonment effect, and the salience of off-support cases—that is, individuals in prison who could not be matched to counterparts in the three counterfactual conditions—to interpreting the results.

FINDINGS

We begin first by examining whether the matching analyses resulted in treated and matched groups that were similar with respect to the covariates. Table 1 presents the results of several post-matching balance statistics for each of the three matched groups—that is, prison-to-probation, prison-to-intensive probation, and prison-to-jail—for the male sample. Inspection of the *t*-test columns highlight that after matching there was balance across almost all covariates. The only exceptions were in the probation matching analysis, and the differences, while statistically significant, were substantively trivial. For example, in the probation match comparison, there was a statistically significant difference between the two groups with respect to the instant offense measure, and, in particular, commission of robbery or some other violent act. Similarly, there was a statistically significant difference with respect to prior felony drug convictions. Forty-eight percent of individuals in the matched probation group had a prior drug conviction, as compared to 44 percent of the individuals in the treated group. Substantively, this difference is small and, as discussed above, likely leads to a modestly elevated increase in the

likelihood of recidivism for those on probation.

The bias reduction statistics provide evidence that the matching substantially reduced potential observed confounding that would have biased comparisons of the recidivism of male prisoners and the recidivism of their counterparts who received other sanctions. For example, the percent bias reduction is typically 90 percent or greater, indicating that pre-matching differences were substantially reduced or eliminated. The result is that the remaining observed bias is typically three percent or less. That does not mean that differences between the two groups in unobserved confounders do not exist, only that confounding due to some of the central predictor variables used in sentencing and recidivism studies, such as prior record, is addressed.

Insert table 1 about here

A similar pattern of results surfaces in table 2, which presents the post-matching covariate balance statistics for the female sample for the same three sets of comparisons. Here, again, inspection of the *t*-tests and bias reduction statistics indicates that pre-matching imbalances have been eliminated across all covariates. The two exceptions are, again, for the comparison to traditional probation. As in the male sample the differences, although statistically significant, are not substantively large. In short, for the two treatment groups—male ex-prisoners and female ex-prisoners—the matching analyses generated comparison groups of males and females, respectively, on traditional probation or intensive probation or in jail who resembled, with respect to the matching covariates, the two groups of ex-prisoners. For some comparisons, the prison-vs.-jail comparison in particular, some ex-prisoners could not be matched to individuals in the counterfactual conditions. That can be seen in the reduction in sample sizes shown at the bottom of table 1 and the bottom of table 2. Below, we return to this issue.

Insert table 2 about here

We turn now to our main focus—assessment of whether imprisonment increases or decreases recidivism and, in turn, whether its effect varies between males and females. Table 3 presents the post-matching comparisons. Panel A shows the comparisons for males and panel B shows the comparisons for females. In each panel, four types of recidivism are compared—violent, property, drug, and other. For each type of recidivism, we compare the recidivism of the “treated” group—that is, the imprisonment group—with the recidivism of the three counterfactual groups (traditional probation, intensive probation, and jail). Several patterns can be identified in the table.

First, prison appears to exert a modest criminogenic effect. That is, it slightly increases the likelihood of all types of recidivism for males and two types (property and drug) for females. There is no indication that imprisonment decreases recidivism. Of interest is the fact that the estimated effects in prison are greater when no matching analyses are undertaken, as evidenced in the pre-matching differences shown in the final column.

Second, there are some differences in the effect of prison on types of offending. Among males, the effect is more pronounced for property and drug recidivism. Among females, it is more pronounced for property recidivism. In the other comparisons in which statistically significant differences emerge, there is only a 1-2 percent increase in recidivism. In studies with smaller sample sizes, these effects likely would not emerge as statistically significant.

Although few of the effects are especially striking in magnitude, the effects in some instances are noteworthy. For example, among males, the largest difference in recidivism is in the comparison between prison and the intensive probation counterfactual condition for drug offending. Whereas 17.8 percent of ex-prisoners recidivated for a drug offense, 12.1 percent of matched individuals on intensive probation did so. In absolute terms, there is a 5.7 percentage point increase in the recidivism that occurred relative to what otherwise could have been expected under this counterfactual. In relative terms, there is a 47 percent increase in the proportion who recidivate. For this particular comparison, the difference is more striking given that the matched male probation sample was more likely to have a prior drug conviction as

compared to the prison sample, as shown in table 1. Given this difference, the traditional probation sample should have been slightly more at risk of recidivating for a drug offense. Instead, however, prisoners were more likely to be reconvicted of drug offending.

Third, when we examine the different effects of prison across the three counterfactual conditions, it is evident that the effect of prison is greater when the comparison is to probation and, in particular, to intensive probation. The differences across the three counterfactual conditions are not substantial, but they imply that the intensive probation counterfactual would have constituted the more effective type of punishment, or blend of punishment and rehabilitation, for the types of individuals who were imprisoned.

Fourth, there is evidence of both similarity and difference in the effects of prison on the recidivism of males and females, respectively. Among males, imprisonment appears to exert a greater criminogenic effect on drug recidivism and, to a lesser extent, property recidivism; its effect on increasing violent or other recidivism is slight. The greatest effects occur in comparison to traditional or intensive probation, not jail. By contrast, among females, prison modestly increases property recidivism and does so to a largely similar degree regardless of the counterfactual condition.

Insert table 3 about here

Before concluding, the loss of cases off-support bears discussion. When the focus was on males, fewer than 1 percent of cases were lost off-support when the comparison was prison versus traditional probation, 6.3 percent of cases were lost off-support when the comparison was prison versus intensive probation, and 14.9 percent of cases were lost off-support when the comparison was prison versus jail. The large number of cases allowed for greater precision in matching (e.g., the use of narrow caliper settings) than frequently is found in the literature. What is especially notable about the matching is the fact that matches could be identified at all. The ability to identify matches reflects the observation found in the sentencing literature that similar

cases receive dissimilar treatment, with variation stemming from differences in how judges and courts sentence cases (see, e.g., Feld 1999; Engen & Gainey 2000; Ulmer & Johnson 2004; Wooldredge 2012). Here, after matching on legal and extra-legal factors, we see that it is possible to identify individuals in each of these counterfactual conditions who appear similar to individuals who received a prison sanction. This fact in turn provides warrant for viewing sanctioning analyses that use propensity score matching as a method for identifying the extent to which variation in sanctioning “like cases” occurs within and across courts.

When the focus was on females, more cases were off-support. Specifically, 21.5 percent of cases were lost off-support when the comparison was prison versus traditional probation, 33.0 percent of cases were lost off-support when the comparison was prison versus intensive probation, and 47.9 percent of cases were lost off-support when the comparison was prison versus jail. Why the greater loss of cases? One reason is the smaller pool of subjects from which to obtain matches. However, given that there were a considerable number of females in the probation, intensive probation, and jail samples, it is unlikely that this factor alone accounts for the lesser ability to find matches. A different possibility is that the courts are more selective about sending women to prison. For example, and consistent with the chivalry and paternalism hypotheses (Gartner 2011), they may be more concerned about the harmful effects of prison on women, their families, and, in particular, their children. From this perspective, then, the courts may screen females for prison more carefully than they do males. As a result, the females who enter prison may be far more homogenous and have fewer counterparts on probation or in jail. By contrast, the courts may be less selective, or screen less carefully, when they sentence males to prison. That, in turn, would result in a greater ability to obtain matches.

The loss of cases off-support partially limits the generalizability of the findings. More specifically, the results generalize to those individuals in prison who have counterparts among the three counterfactual populations. However, even with no loss of cases, the generalizability of the results would extend only to those groups who resemble prisoners. Thus, we would conclude, in a situation with almost no loss in cases, that, for males, prison appears to produce

more property and drug recidivism than what otherwise would have happened if the counterfactual condition for them were traditional or intensive probation. What the effect would be if prison were the treatment for individuals who otherwise typically would not be placed in prison remains unknown. The same holds true for experimental designs that employ random assignment. In these cases, the results generalize to the treatment-eligible population. The generalizability to ineligible or non-targeted populations then remains as a separate question to be investigated using similar experiments with these populations.

Given the loss of cases off-support in some of the analyses, especially those involving females, we undertook a series of analyses to assess the robustness of the results. Specifically, we replicated the analyses using wider caliper settings, 2-to-1 matching, matching with replacement, and all of these options combined. The substantive and statistical significance of the results were largely the same, with the main difference being that fewer cases were, as would be anticipated, lost off-support. We also conducted sensitivity analyses by estimating Rosenbaum bounds, which provide an estimate of how much unobserved bias would have to exist to change the estimated results (DiPrete & Gangl 2004; Morgan & Harding 2006). The analyses identified gamma scores, or odds ratios for an unobserved confounder, of 1.4 to 1.5, with some as low as 1.1 (for the female prison vs. jail comparison) and as high as 1.7 (for the male prison vs. intensive probation comparison). Thus, for example, when gamma is 1.5, the estimated effects no longer would be statistically significant, suggesting some need for caution in interpreting the results. That same interpretation would hold for virtually any value of gamma; it is simply that with higher gamma scores, one can be more confident that larger amounts of unobserved confounding would have to exist before we would question the results. However, as Becker and Caliendo (2007:81) have emphasized, the Rosenbaum bounds assume a worst-case scenario. For example, it assumes that an omitted confounder would strongly affect the outcome; if it only weakly affected the outcome, the estimated effect would not likely change (DiPrete & Gangl 2004:291). Put differently, a gamma of 1.5, or of any other level, “does not mean that unobserved heterogeneity exists and that there is no effect of treatment on the outcome

variable. This result only states that the confidence interval for the effect would include zero if an unobserved variable caused the odds ratio of treatment assignment to differ between the treatment and comparison groups by $[\gamma]$.” In short, the results of the analyses remain consistent across a range of matching approaches, but they should also be interpreted with caution. (The ancillary analyses are available upon request.)

CONCLUSION

This paper was motivated by the need for studies that investigate the effects of imprisonment on recidivism and, in particular, that examine gender differences in the effects of imprisonment. Few rigorous studies of prison effects on recidivism exist (Nagin et al. 2009; Cullen et al. 2011), and, to our knowledge, none exist that systematically examine the effects of prison as compared to traditional probation, intensive probation, and jail for males and females, respectively. Prior scholarship provides a varied theoretical and empirical foundation for anticipating sex differences in the effects of imprisonment. The bulk of extant scholarship suggests that more similarity than difference exists when studies compare the causes of crime and the effectiveness of interventions for males and females (Gartner 2011; Andrews et al. 2012). At the same time, some scholars suggest that differences are more pronounced than what extant scholarship implies and that the theoretical and empirical foundation for asserting whether more similarity or more difference exists is not at present sufficient (see, e.g., Van Voorhis 2012).

Against that backdrop, this study sought to examine the extent to which imprisonment increases or decreases violent, property, drug, or other recidivism and whether differences in its effectiveness vary with respect to three counterfactual conditions—traditional probation, intensive probation, and jail—and to gender. The findings can be summarized briefly. First, based on this study’s findings, prison is criminogenic. This absence of a beneficial effect, for any comparison and for any type of recidivism, is notable because it suggests that, whatever the merits of incarceration—such as the potential to create general deterrent effects or achieve

greater retribution—reduced recidivism may not be one of them (see Cullen et al. 2011). Indeed, to the contrary, it appears to contribute to more rather than less or no offending. Second, the effect appears to be greater for property and drug recidivism; the effect of imprisonment on violent and other recidivism is nominal. Third, prison was more criminogenic when the comparison was to traditional probation and especially intensive probation; it was less criminogenic when the comparison was to jail. Fourth, consistent with the literature on sex differences in offending (Gartner 2011; Andrews et al. 2012), similarities and differences surfaced in the analyses. For both males and females, prison was criminogenic and was most likely to increase property and drug recidivism. However, for females, the main effect was to increase property recidivism, and this effect emerged regardless of whether the comparison was to traditional or intensive probation or to jail. This pattern, with a criminogenic effect surfacing only for property offending among women, suggests indirect support for arguments, such as those advanced by Wood and Grasmick (1999) and others (e.g., Cobbina et al. 2012), that prison may be more of a deterrent for females because they may be the primary caregivers in their families and are more focused on or committed to family unification.

These findings raise several intriguing implications for research and policy. First, replication studies are needed. The findings here are consistent with those suggested in recent reviews (e.g., Nagin et al. 2009; Cullen et al. 2011). However, it is possible that unobserved confounding in this study may have biased the estimated effects of imprisonment. Even if true, however, it appears unlikely that the estimated effects would be reversed. More plausible, given the consistently identified criminogenic effects across the comparisons, is the likelihood that prison at best exerts no effect on recidivism. That would be an improvement over a criminogenic effect but still would fall short of the intended goal of reducing offending. There are, of course, other penal goals, such as general deterrence and retribution. But recidivism is clearly a central criterion of effectiveness when evaluating criminal justice interventions, prison in particular. Thus, if future research consistently identifies a criminogenic effect of imprisonment (see, e.g., Nagin et al. 2009; Nieuwbeerta et al. 2009; Cullen et al. 2011), questions arise about the extent

to which the benefits of these other goals—however much they are achieved—balance the costs that stem from greater levels of recidivism or, conversely, missed opportunities to obtain lower rates of recidivism through more cost-efficient means (Raphael & Stoll 2009; Mears 2010).

Second, studies are needed that systematically compare ways in which the conditions of various correctional sanctions, such as intensive probation and prison, vary among males and females and how these conditions may affect recidivism. Some studies indicate that imprisonment may have variable effects on recidivism depending on the type of offender and whether rehabilitation is provided. For example, some studies suggest that prison terms increase recidivism among low-risk and drug offenders and that inmates who do not receive rehabilitative services are more likely to recidivate when compared to counterparts who do receive such services (Cullen et al. 2011). Research to date, however, has not systematically examined how a diverse range of types of offenders (e.g., the mentally ill, younger offenders, chronic offenders) and a diverse range of prison experiences (e.g., victimization, misconduct, various types of programming and services, residing in facilities with different inmate or officer cultures) may moderate the effect of prison on recidivism.

Research is needed as well on how prison effects, including effects that are moderated by various inmate experiences, may depend on the counterfactual condition—that is, what would have happened to these individuals had they not been imprisoned. The counterfactual condition is not obvious. Some inmates, for example, may have been placed in jail while others might have been placed on probation. In addition, what the counterfactual conditions are for males and females may well vary, especially in contexts where a wide range of intermediate sanctions exist.

More generally, an overarching question remains largely unaddressed: How do the experiences of male and female prisoners differ? Are the experiences, for example, “gendered” in ways that reflect differences in how males and females both view themselves and are treated while in prison or after release? What are the differences in the counterfactual conditions that apply to them, and how do these differences contribute to recidivism? Research on these questions would be responsive to calls for developing a more robust foundation for developing

theories of offending and correctional interventions that are informed by an understanding of sex and gender differences (Gartner 2011; Van Voorhis 2012). At the same time, it would be responsive to calls by policymakers, practitioners, and the public for evidence-based policy (Mears 2010). To be certain, no clear implication for gender-specific programming emerged from these analyses. At most, the results suggest that prison may be somewhat more criminogenic for males than for females. However, any such claims must remain tentative until a larger body of studies emerges that examines sex differences in prison effects.

Third, the link between in-prison and post-release experiences needs to be better understood. As a general matter, prison may be criminogenic because of disruptions to the life chances of individuals (Sampson & Laub 1993). Indirect evidence of this idea stems from the fact that the observed criminogenic effects of prison were primarily for property and drug recidivism. These constitute the types of crimes that more clearly reflect an instrumental response to impeded opportunities to “make it” in the marketplace (Maruna 2001, 2012; Mears 2010; Wang et al. 2010). Yet, the specific types of experiences inmates have in prison clearly may influence the extent to which such disruptions occur. The provision of rehabilitative services alone, for example, may reduce such effects (Cullen et al. 2011). At present, however, there remains little empirical research that systematically ties together in-prison and post-release experiences or that does so in ways that credibly address concerns about selection effects.

Fourth, investigations are needed into differences in the inconsistency of sentencing of males and females to prison. Sentencing studies provide one direct foothold to examine this issue. Another approach, however, is to use matching methodologies to assess the extent to which prison, or any other sanction, is used more selectively for one group or another. In this study, it was possible to identify matches to male and female prisoners from among the traditional probation, intensive probation, and jail populations. That alone provides indication that inconsistency in sentencing exists. However, in the analyses, it was substantially easier to identify matches to the male prisoner sample. By contrast, fewer female prisoners could be matched to the other sanction groups, suggesting that prison may be assigned in a more selective

way for females as compared to males. Studies that explore these possibilities hold the potential to inform the sentencing literature and to provide information that may prove useful to policymakers in determining how best to structure sentencing decisions. Ultimately, inconsistent sanctioning is relevant for discussions of justice, not least because imprisonment carries with it not only a wide range of deprivations (Adams 1992) but also leads to many challenges that impede the likelihood of a successful reentry (Visher & Travis 2011).

If, as this study and others suggest, prison increases recidivism, the general challenge that is clearly highlighted is the need to create sanctioning policies that better achieve societal goals. That is no simple task. Even if prison is criminogenic, its use may produce general deterrent effects whose benefits offset the costs associated with higher recidivism rates. Little convincing evidence exists to support that argument, but it nonetheless constitutes a compelling argument for incarceration. In addition, incarceration may be more effective than other types of sanctions for achieving retribution or other criminal justice goals. Here, again, there is little evidence to support that view but there is little to support arguments for other types of sentencing regimes. What we can say here simply is that a criminogenic effect of prison highlights the need for other sanctions, or for other ways of structuring the prison experience, that do not contribute to recidivism and that still achieve other goals, such as retribution and justice (Cullen et al. 2011). It also highlights the potential salience of investing in crime prevention and early intervention to avoid the need for sanctioning in the first place (Welsh & Farrington 2011).

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Table 1. Prison Group vs. Three Counterfactual Groups, Post-Matching Balance Statistics: Male Sample

	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	0.56	0.57	-2.1	94.7	-1.46	0.54	0.55	-1.2	96.1	-0.84	0.58	0.59	-2.7	50.3	-1.74
Hispanic	0.06	0.06	1.5	91.2	1.26	0.06	0.06	-0.1	99.3	-0.06	0.06	0.06	0.3	95.4	0.22
White	0.38	0.37	1.2	95.8	0.88	0.39	0.39	1.3	94.9	0.88	0.36	0.34	2.6	72.2	1.69
Age	32.49	32.71	-2.2	88.1	-1.58	32.15	32.16	0.0	99.9	-0.01	32.62	32.65	-0.2	91.8	-0.16
Offense—murder	0.02	0.01	0.3	97.8	0.18	0.02	0.02	-0.5	94.2	-0.29	0.01	0.00	2.4	85.7	1.82
Offense—sexual	0.04	0.04	0.7	94.5	0.41	0.04	0.04	-0.7	84.6	-0.52	0.03	0.03	0.3	99.0	0.14
Offense—robbery	0.09	0.08	4.7	85.8	2.65*	0.08	0.08	2.1	89.7	1.26	0.07	0.07	-0.9	97.3	-0.52
Offense—other viol	0.15	0.16	-2.9	<-100	-2.02*	0.16	0.16	-1.3	79.7	-0.92	0.16	0.17	-1.7	92.5	-0.93
Offense—burglary	0.20	0.19	2.1	91.3	1.37	0.19	0.18	2.8	78.8	1.81	0.18	0.17	1.4	96.1	0.79
Offense—property	0.13	0.13	-0.1	99.8	-0.04	0.13	0.13	0.1	98.8	0.04	0.14	0.15	-0.5	95.8	-0.37
Offense—weapons	0.04	0.04	-1.1	-57.3	-0.75	0.04	0.04	-2.1	-35.3	-1.40	0.04	0.04	-1.3	74.5	-0.75
Offense—drug	0.07	0.08	-1.3	80.6	-0.99	0.07	0.07	1.4	85.0	1.01	0.08	0.09	-2.1	85.6	-1.42
Offense—other	0.27	0.27	-0.7	96.2	-0.48	0.26	0.27	-1.9	68.0	-1.32	0.30	0.29	2.2	95.5	1.50
Prior convictions	1.72	1.80	-2.8	93.3	-1.51	1.67	1.65	0.7	96.7	0.45	1.52	1.49	1.2	96.9	0.67
Prior viol. conv.	0.27	0.29	-2.3	92.2	-1.28	0.26	0.27	-1.0	94.3	-0.57	0.25	0.25	-0.2	99.4	-0.09
Prior sex conv.	0.03	0.03	-0.6	94.6	-0.34	0.03	0.03	-1.1	80.1	-0.68	0.03	0.03	0.4	96.9	0.20
Prior prop. conv.	0.82	0.83	-0.4	98.8	-0.19	0.79	0.75	1.9	89.9	1.12	0.71	0.67	1.7	94.0	0.98
Prior drug conv.	0.44	0.48	-3.8	83.7	-2.08*	0.43	0.43	-0.3	96.8	-0.20	0.40	0.40	0.4	98.2	0.21
Prior other conv.	0.16	0.17	-2.1	88.4	-1.21	0.16	0.16	-0.5	91.8	-0.32	0.14	0.15	-0.8	95.1	-0.49
Prior prison commit	1.26	1.22	3.2	96.2	1.72	1.12	1.11	0.5	99.2	0.32	1.27	1.31	-2.8	94.5	-1.57
Prior superv. viol.	1.37	1.40	-2.0	97.5	-1.14	1.30	1.32	-1.6	96.3	-0.97	1.39	1.43	-3.3	91.3	-1.95
Offense severity	5.36	5.35	0.6	99.4	0.38	5.37	5.37	-0.2	99.5	-0.12	5.09	5.05	2.1	98.1	1.37
Eligible for prison	0.78	0.79	-0.7	99.6	-0.48	0.77	0.78	-1.7	98.0	-1.26	0.75	0.75	-1.8	98.9	-1.06
Circuit fixed effects	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
N	9,974	9,974				9,366	9,366				8,510	8,510			

%B = percent bias; %BR = percent bias reduction; * p < .05 (two-tailed).

Table 2. Prison Group vs. Three Counterfactual Groups, Post-Matching Balance Statistics: Female Sample

	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	0.51	0.51	-1.7	93.8	-0.90	0.49	0.48	0.8	96.5	0.40	0.54	0.55	-1.3	<-100	-0.57
Hispanic	0.05	0.05	1.0	89.8	0.61	0.05	0.05	1.9	70.2	0.94	0.04	0.04	0.5	86.9	0.22
White	0.45	0.44	1.2	94.6	0.65	0.46	0.47	-1.6	91.7	-0.82	0.41	0.41	1.1	31.7	0.48
Age	33.52	33.71	-2.2	92.5	-1.23	33.10	32.96	1.6	93.5	0.77	33.93	33.95	-0.2	86.3	-0.09
Offense—murder	0.02	0.02	-0.4	98.0	-0.21	0.02	0.02	0.3	97.9	0.15	0.00	0.00	0.9	96.1	1.16
Offense—sexual	0.01	0.01	-1.7	79.7	-0.68	0.01	0.01	-1.2	22.0	-0.53	0.00	0.00	0.4	96.4	0.22
Offense—robbery	0.05	0.04	1.8	93.7	0.85	0.04	0.05	-1.9	88.7	-0.93	0.03	0.03	1.4	95.0	0.67
Offense—other viol	0.18	0.19	-2.7	45.7	-1.39	0.18	0.19	-2.0	58.0	-1.00	0.12	0.13	-2.1	92.8	-0.88
Offense—burglary	0.08	0.08	-0.3	98.7	-0.13	0.08	0.08	-1.1	89.9	-0.50	0.05	0.05	-0.4	98.4	-0.20
Offense—property	0.23	0.23	1.3	97.3	0.74	0.24	0.23	1.8	87.2	0.94	0.27	0.28	-2.0	85.6	-0.86
Offense—weapons	0.01	0.01	-0.1	94.7	-0.08	0.01	0.01	0.5	73.5	0.25	0.01	0.01	-0.2	95.6	-0.10
Offense—drug	0.06	0.06	0.4	96.8	0.19	0.06	0.06	1.6	52.8	0.84	0.07	0.07	0.8	70.4	0.35
Offense—other	0.36	0.36	0.5	91.1	0.29	0.36	0.36	0.5	77.8	0.25	0.43	0.42	2.2	94.4	0.96
Prior convictions	1.76	1.75	0.4	99.1	0.20	1.63	1.57	1.5	94.1	0.82	1.48	1.44	1.1	97.4	0.53
Prior viol. conv.	0.21	0.20	1.7	94.8	0.78	0.18	0.18	1.2	94.1	0.60	0.15	0.14	1.6	94.6	0.75
Prior sex conv.	0.01	0.00	0.5	86.8	1.05	0.01	0.00	0.2	93.6	0.45	0.02	0.00	4.1	-15.1	1.34
Prior prop. conv.	0.83	0.83	-0.1	99.6	-0.07	0.76	0.72	1.2	93.2	0.74	0.70	0.67	0.9	96.6	0.50
Prior drug conv.	0.60	0.59	0.7	98.0	0.31	0.57	0.56	0.9	94.3	0.39	0.51	0.52	-0.9	96.7	-0.42
Prior other conv.	0.11	0.12	-1.1	94.7	-0.51	0.10	0.11	-0.6	93.6	-0.29	0.10	0.10	0.0	100.0	0.00
Prior prison commit	0.81	0.79	2.0	97.7	0.89	0.65	0.62	3.0	94.9	1.57	0.87	0.90	-2.9	94.8	-1.14
Prior superv. viol.	1.41	1.46	-4.5	95.4	-2.04*	1.34	1.32	1.3	96.6	0.66	1.59	1.66	-4.6	89.7	-1.90
Offense severity	4.76	4.80	-2.0	97.7	-0.97	4.74	4.81	-3.3	91.7	-1.60	4.09	4.07	1.2	98.8	0.51
Eligible for prison	0.56	0.54	6.0	95.9	2.55*	0.52	0.53	-2.0	97.4	-0.96	0.36	0.34	5.2	96.5	1.86
Circuit fixed effects	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
N	5,926	5,926				5,058	5,058				3,934	3,934			

%B = percent bias; %BR = percent bias reduction; * p < .05 (two-tailed).

Table 3. Estimated Average Treatment of Prison (ATT) on Recidivism, Compared to Three Counterfactual Conditions, Using Propensity Score Matching, 1:1 Nearest Neighbor without Replacement (.005 Caliper): Male and Female Samples

	Treated Group	Matched Group	Post-Matching Difference	S.E.	t-test	Percent Off-Support	Pre-Matching Difference
Panel A. Males							
<i>Violent Recidivism</i>							
Probation	0.065	0.055	0.010*	0.003	3.040	0.3	0.020
Intensive Probation	0.066	0.054	0.012*	0.003	3.570	6.3	0.014
Jail	0.067	0.067	0.000	0.004	-0.120	14.9	0.013
<i>Property Recidivism</i>							
Probation	0.157	0.122	0.035*	0.005	7.140	0.3	0.060
Intensive Probation	0.154	0.110	0.044*	0.005	8.850	6.3	0.053
Jail	0.161	0.142	0.020*	0.005	3.550	14.9	0.041
<i>Drug Recidivism</i>							
Probation	0.182	0.134	0.049*	0.005	9.490	0.3	0.073
Intensive Probation	0.178	0.121	0.057*	0.005	10.950	6.3	0.067
Jail	0.196	0.181	0.015*	0.006	2.470	14.9	-0.011
<i>Other Recidivism</i>							
Probation	0.063	0.054	0.009*	0.003	2.650	0.3	0.014
Intensive Probation	0.063	0.048	0.015*	0.003	4.470	6.3	0.010
Jail	0.067	0.054	0.013*	0.004	3.540	14.9	-0.004
Panel A. Females							
<i>Violent Recidivism</i>							
Probation	0.030	0.028	0.002	0.003	0.490	21.5	0.008
Intensive Probation	0.030	0.026	0.004	0.003	1.270	33.0	0.004
Jail	0.034	0.038	-0.004	0.004	-0.850	47.9	0.002
<i>Property Recidivism</i>							
Probation	0.153	0.105	0.049*	0.006	7.940	21.5	0.061
Intensive Probation	0.158	0.112	0.047*	0.007	6.890	33.0	0.031
Jail	0.176	0.138	0.037*	0.008	4.560	47.9	0.038
<i>Drug Recidivism</i>							
Probation	0.167	0.149	0.018*	0.007	2.700	21.5	0.078
Intensive Probation	0.166	0.127	0.039*	0.007	5.550	33.0	0.048
Jail	0.200	0.208	-0.008	0.009	-0.840	47.9	-0.030
<i>Other Recidivism</i>							
Probation	0.028	0.026	0.002	0.003	0.790	21.5	0.010
Intensive Probation	0.030	0.025	0.005	0.003	1.400	33.0	0.001
Jail	0.034	0.031	0.004	0.004	0.950	47.9	-0.005

* p < .05 (two-tailed).