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Daniel P. Mears and Joshua C. Cochran

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**Progressively Tougher Sanctioning and Recidivism:
Assessing the Effects of Different Types of Sanctions**

Daniel P. Mears and Joshua C. Cochran*

* Daniel P. Mears, Ph.D., Florida State University, College of Criminology and Criminal Justice, Eppes Hall, 112 South Copeland Street, Tallahassee, FL 32306-1273, e-mail (dmears@fsu.edu), phone (850-644-7376). Joshua C. Cochran, Ph.D., is an Assistant Professor at the University of Cincinnati, School of Criminal Justice, P.O. Box 210389, Cincinnati, Ohio 45221, e-mail (joshua.cochran@uc.edu), phone (513-556-7688). We thank the Editor, reviewers, and Sonja Siennick for their helpful suggestions, and the Florida Department of Corrections for permission to use the data for this study. Points of view in this paper are those of the authors and do not represent the official views or policies 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, Eppes Hall, 112 South Copeland Street, Tallahassee, FL 32306-1273, e-mail (dmears@fsu.edu), phone (850-644-7376). He conducts research on crime and policy. His work has appeared in such journals as *Criminology* and *Justice Quarterly*, as well as several books, including *Out-of-Control Criminal Justice* (Cambridge University Press), *American Criminal Justice Policy* (Cambridge University Press), and *Prisoner Reentry in the Era of Mass Incarceration* (Sage).

Joshua C. Cochran, Ph.D., is an Assistant Professor at the University of Cincinnati, School of Criminal Justice, P.O. Box 210389, Cincinnati, Ohio 45221, e-mail (joshua.cochran@uc.edu), phone (513-556-7688). His research interests include criminological theory, imprisonment, prisoner reentry, and sentencing. His work has appeared in *Criminology*, the *Journal of Quantitative Criminology*, the *Journal of Research in Crime and Delinquency*, *Justice Quarterly*, and, with Daniel P. Mears, *Prisoner Reentry in the Era of Mass Incarceration* (Sage).

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Abstract

Objectives. The study tests two related hypotheses about recidivist sentencing premiums and the progressive sanctioning logic on which they rest: (1) Among first-time felons, punitive sanctions will more effectively reduce recidivism than will less severe sanctions, and (2) among second-time felons, progressively tougher sanctions will more effectively reduce recidivism than will progressions to comparable or less severe sanctions.

Methods. We use data on first-time and second-time felons and propensity score matching analyses to test these two hypotheses.

Results. Although tougher punishment, and increasingly tougher punishment among second-time offenders, may sometimes reduce recidivism, less severe punishment appears on average to be more effective.

Conclusions. The results raise questions about the effects of both tougher, and progressively tougher, types of sanctions in efforts to reduce recidivism.

Key words: sanctioning, prisons, incarceration, recidivism, punishment

Progressively Tougher Sanctioning and Recidivism: Assessing the Effects of Different Types of Sanctions

Introduction

The emergence of mass incarceration has spurred scholarship aimed at understanding the consequences of the “punitive turn” in American criminal justice (Clear and Frost 2014; Garland 2013; Gottschalk 2015; Travis and Visher 2005). The change entailed greater reliance on tougher sanctioning and less rehabilitative intervention (Austin et al. 2016; Lipsey and Cullen 2007). Among the varied goals motivating this “turn” was greater public safety through reduced recidivism; this benefit was anticipated to arise in part through specific deterrence processes. A corollary of this idea, one that undergirds modern sentencing regimes, is the notion not only that tougher sanctioning is more effective than less severe sanctioning but also that *progressively* tougher sanctioning—that is, a “recidivist sentencing premium” (Roberts 2008:468)—for repeat offenders is more effective than is relying on “more of the same” punishment.

This approach may be justified on retributive and incapacitative grounds (von Hirsch and Ashworth 1992; cf. Tonry 2016). However, its utility in reducing recidivism remains debated (see, generally, Cochran et al. 2014; Cullen et al. 2011; Gaes 2016; Mears et al. 2015; Morris and Piquero 2013; Nagin et al. 2009; Villettaz et al. 2014). Theoretical accounts of specific deterrence, for example, do not ineluctably imply that more severe sanctions in general or for repeat offenders will reduce offending more so than will less severe sanctions (Jacobs 2010; Nagin 2013; Paternoster 2010; Pogarsky 2002; Raaijmakers et al. 2017; Stafford et al. 1986).

The goal of this paper, then, is to contribute to scholarship aimed at understanding punishment and, more specifically, the deterrence-based logic and effects of punitive sanctions. To this end, we address a research gap on the effects of more severe types of punishment among individuals who have been convicted of felonies for the first time as well as the effects of progressively tougher types of punishment for individuals who commit a second felony. In what

follows, we situate the study against a context of increasingly tougher sanctioning regimes over the last several decades. Second, we describe the theoretical logic of this shift in policy and the assumption that tougher sanctions, as well as recidivist sentencing premiums, reduce recidivism. Third, we discuss prior research and the theoretical foundation for anticipating beneficial and harmful effects of this get-tough approach to sanctioning. We then describe the current study and findings, and conclude by discussing its implications for theory, research, and policy.

Background

The Rise of Get-Tough Punishment

The get-tough era in corrections resulted from a wide variety of factors (Austin et al. 2016; Garland 2013; Gottschalk 2015; Kelly 2015; Phelps and Pager 2016). Regardless of the causes, the result has been dramatic growth in punishment and the correctional system. For example, from 1980 to 2015, prison populations grew from 329,000 inmates to 1,526,800 (or from 140 to 458 inmates per 100,000 residents) a more than 460 percent increase (Carson and Anderson 2016; Kalish 1981). That increase inevitably has contributed to financial implications: From 1982-2010, inflation-adjusted spending on prisons, which accounts for approximately 70 to 80 percent of all corrections expenditures, increased from \$10 billion to \$37 billion (Kyckelhahn 2012:2). Increased expenditures on corrections resulted as well from dramatic growth in probation populations during this period (Corbett 2015).

Motivation for a tough-on-crime approach can be attributed to policymakers placing a greater premium on retribution, but it can be attributed, too, to an interest in improving public safety (Kelly 2015; Pratt 2008). Even if other factors, such as political gain or perceived racial threat, motivated the shift to punitive sanctioning, reducing crime constitutes a central justification for it (Austin et al. 2016; Campbell et al. 2015; Mears and Cochran 2015). Viewed from this perspective, the greater use of tougher sanctioning has been premised in part on the idea that it would increase public safety through a variety of mechanisms, including reduced recidivism.

Recidivism and the Theory Underlying Tougher, and Progressively Tougher, Sanctioning

Incarceration emerged relatively recently as a means both of punishing criminals and of reducing their likelihood of future criminal activity (Foucault 1977). Reliance on it—like that of tougher sanctioning more generally—has been premised on several theoretical arguments. Tougher sanctions, for example, have been viewed as a pathway to moral reform and as a vehicle through which offenders, due to greater supervision, could be rehabilitated (Clear and Frost 2014; Friedman 1993; Hancock and Jewkes 2011; Morris and Rothman 1995).

They also traditionally have been viewed as capable of producing a specific deterrent effect (Cullen et al. 2009; Gaes 2016; Nagin 2013). Indeed, a central pillar of the rise of mass incarceration has been the assumption among policymakers that prison creates large specific deterrent effects beyond that of non-incarcerative sanctions (Blumstein 1997; Clear and Frost 2014; Kelly 2015; Paternoster 2010; Nagin et al. 2011). The logic, though, is not specific to incarceration. *Any* more severe sanction can be viewed from a deterrence perspective as providing a greater reduction in offending. For example, intensive probation, which allows for closer and more frequent supervision, may be viewed as a tougher sanction than probation, and so should result in more deterrence (Corbett 2015; Petersilia and Turner 1993; Spelman 1995).

A corollary to this logic is the notion that when less severe types of sanctions, such as probation, have failed, tougher sanctions will be more effective than doing more of the same or relying on less severe punishments (Currie 2013; Roberts 2008; Tonry 1995). The theoretical underpinnings of this argument vary slightly. An absolutist view of sanction severity sees it as exerting an effect regardless of what an individual has previously experienced. Accordingly, no implication for punishment arises from the fact that an individual was previously sanctioned: Tougher punishment will always be more effective. A relativist view views punishment severity in the context of prior sanctions. From this perspective, if an earlier sanction failed to deter, a more severe sanction is needed to obtain an appreciable deterrent effect. A particular sanction's

effectiveness for repeat offenders thus depends on what these individuals previously received.

Roberts (2008:468) has highlighted that this “recidivist sentencing premium” approach “may be considered a universal feature of sentencing” (see also Durlauf and Nagin 2011:21-22). Both logics—that of tougher sanctioning in general and that of progressively tougher sanctioning—are central to American criminal justice sentencing and have become more so in recent decades. The “recidivist sentencing premium” can be justified on other grounds, such as retribution or incapacitation; a specific deterrent effect is, however, a central expectation of tougher sentencing policies (see, generally, Gottschalk 2015; Heilbroner 1990; Mears et al. 2015; Nagin 2013; Steiner et al. 2012). When, for example, the courts face a repeat offender, the logic implies that previous, less severe sanctions employed with the offender must have failed. Accordingly, applying the same punishment again will simply result in further failure (Clear and Frost 2014; Friedman 1993; Gilligan 2000; Roberts 2008; Currie 2013).

Although the logic is grounded in deterrence theory and has an intuitive appeal, it assumes an implication that does not necessarily arise from deterrence theory (Akers et al. 2016; Pogarsky 2002; Jacobs 2010; Nagin 2013; Paternoster 2010). For example, deterrence scholarship does not identify on theoretical grounds a set threshold of severity for producing specific deterrent effects. In addition, severity constitutes but one of several elements, including the certainty and celerity of punishment, necessary for deterrence to occur (Stafford et al. 1986; Steiner et al. 2012). All elements contribute in some way to deterrence, and severity may be less relevant than other dimensions. For example, the certainty of punishment may be more consequential for deterrence (Nagin 2013). The logic of tougher, or progressively tougher, sanctioning is challenged as well by the possibility that some putatively more severe sanction types, such as prison, may not always be viewed as such by offenders (May and Wood 2010; Spelman 1995). And it is challenged not least by the fact that deterrence-based approaches to sanctioning do not in and of themselves address a range of factors that may contribute to offending (Andrew and Bonta 2010; Corbett 2015; Klingele 2013; Latessa et al. 2014; Lipsey and Cullen 2007).

Effects of Tougher Sanctioning on Recidivism

In a recent systematic review undertaken by Villettaz et al. (2014), the authors echoed the conclusion from Nagin et al.'s (2009) earlier review: Most studies on the effects of incarceration on recidivism have relied on weak methodological designs, thus raising questions about the validity of the estimates. Putting aside such questions, some studies identify modest beneficial effects of prison in reducing recidivism, but most typically have found that it exerts little to no beneficial effect in reducing recidivism and may even increase it (see, e.g., Bales and Piquero 2012; Cochran et al. 2014; Nieuwbeerta et al. 2009). Theoretical explanations for the latter effects vary. They include the possibility that incarceration may be more likely than other sanctions to weaken social bonds, increase strain, and contribute to being labeled as “criminal”; at the same time, it may make it more difficult to provide rehabilitative programming and services (Clear and Frost 2014; Kelly 2015; Listwan et al. 2013; Nagin et al. 2009).

Evidence on the effectiveness of probation paints a similar portrait. Reviews generally have identified mixed findings. Although some studies have found probation to be effective, the bulk of studies have reported null and sometimes harmful (recidivism-increasing) effects (Gill et al. 2010; Latessa et al. 2014; Petersilia and Turner 1993; Piehl and LoBuglio 2005; Tonry and Lynch 1996; see also Lipsey and Cullen 2007 and Solomon et al. 2005).

Several implications flow from the reviews and recent studies. First, as Nagin et al. (2009) and others (e.g., Piehl and LoBuglio 2005) have emphasized, there is a need for more methodologically rigorous studies that employ matching analyses to examine the effects of probation and prison on recidivism. Such analyses do not address omitted variable bias, but, given a set of observed confounding measures, they can help to improve the validity of estimates (Apel and Sweeten 2010b; Gill et al. 2010; Guo and Fraser 2010; Villettaz et al. 2014).

Second, studies are needed that examine multiple counterfactual comparisons (Gaes et al. 2016; Mears et al. 2015; Nagin et al. 2009; see also Ostermann and Hyatt 2016). A limitation of many studies is that they focus only on incarcerative versus non-incarcerative comparisons. This

approach assumes that only one counterfactual exists. For example, it assumes that individuals sentenced to prison would otherwise have been sentenced to probation. However, the individuals might have been sentenced instead to intensive probation, which typically entails more restrictions and supervision (Piehl and LoBuglio 2005), or to jail. Similarly, individuals placed on probation might otherwise have been sentenced to intensive probation, jail, or prison.

In short, multiple punishment counterfactuals exist and reflect the range of sanctions available to the courts. When considering the sanction to apply, courts may consider each option and, by extension, its severity. In general, the options can be viewed as falling along a severity continuum. For example, the greater restrictions and supervision associated with intensive probation make it potentially more punitive than regular probation. Similarly, relative to either type of probation, jail and prison involve substantially greater deprivations, including a loss of such freedoms as contact with family and friends. And the lengthier terms of confinement typically associated with imprisonment suggest that prison constitutes a more severe sanction than jail; in addition, prison stays can involve placements that may make visitation less likely.

To our knowledge, only two studies have examined these different counterfactuals. One is an analysis undertaken by Cochran et al. (2014) that examined the four categories of sanctions; they found that prison exerted a criminogenic effect relative to jail and each type of probation. A second, identified by Villettaz et al. (2014:26), is an unpublished study by Apel and Sweeten (2010a) that used propensity score matching (PSM) to analyze data from the National Longitudinal Survey of Youth; the authors showed that incarceration, primarily jail, was associated with a greater probability of reoffending among those convicted for the first time.

Collectively, the two studies point to the importance of examining *multiple* counterfactuals. They also highlight two important considerations. First, as the Apel and Sweeten (2010a) study suggests, the effects of sanctions among first-time offenders warrants attention in its own right. Virtually all convicted felons experience a first-time sanction, and it is possible that average sanction effects among all felons, as assessed in the Cochran et al. (2014) study, do not hold for first-time offenders. Recent studies by Nieuwebeerta et al. (2009) and Wermink et al. (2013),

both of which examined imprisonment effects among such individuals, reinforce this line of investigation. First-time adult felons, for example, have virtually no prior experience with any other type of formal adult court sanction. Second, there is a need to determine whether types of probation influence the estimation of sanction effects. For example, Cochran et al. (2014) found that differences in the recidivism of regular probationers and intensive probationers, both in comparison to one another and to other sanction groups, may exist.

Current Study and Hypotheses

There is, then, a dearth of research on the effects of tougher types of sanctions for first-time offenders. Perhaps more striking is the void that attends to progressive sanctioning. To our knowledge, there is no rigorous empirical study of whether, among second-time felons, tougher types of sanctions than what the individuals previously received results in the specific deterrent effect anticipated under contemporary sentencing schemes. Put differently, little evidence exists that a “recidivist sentencing premium” (Roberts 2008:468) reduces recidivism.

This study thus seeks to contribute to efforts to understand how more severe sanctioning influences recidivism and whether progressively tougher sanctioning produces larger reductions in recidivism than does continued use of the same sanctions that felons previously received. It is important to emphasize that severity might be operationalized in different ways. For example, it might be measured in terms of the duration of a given sanction or the range and intensity of restrictions that it entails. Research that examines such dimensions can shed light not only on whether these indicators of severity influence recidivism but also, by extension, how sentencing policies might be revised to improve outcomes (e.g., shortening or lengthening sentence length).

Here, we focus on types of sanctions as the gauge of severity. This approach reflects the fact that the logic of many sentencing regimes centers on the idea that if an individual recidivates after receiving one punishment, then a more severe type will be more effective. In addition, we focus on first-time and second-time felons to isolate the effects of particular sanctions and

sanction progressions. This approach has the virtue of creating a manageable set of comparisons and insight into two groups who are important to understand in their own right and may shed light on the effects of more severe sanctioning for individuals who repeatedly offend. Even so, and as we discuss in the conclusion, a broader approach to understanding the impact of sentencing severity will require studies that operationalize tougher sanctioning using a diverse range of measures and that examine individuals who range from first-time to chronic offenders.

We test, then, two related questions and hypotheses. *First, among first-time felons, what is the effect of more severe types of punishments on recidivism?* Drawing on the deterrence logic that in part undergirds contemporary sentencing, we hypothesize that more severe punishments should result in lower rates of recidivism. Conversely, we hypothesize that less severe punishments should result in higher rates of recidivism.

Although prior work has examined sanction effects, we know of no studies that have investigated the effects of a range of different sanctions when individuals have previously never experienced a criminal court punishment. Nieuwebeerta et al. (2009) and Wermink et al. (2013), for example, compared incarceration to non-incarceration among first-time felons, but did not examine imprisonment in comparison to jail, regular probation, or intensive probation respectively. First-time felons as a group may differ from those who have repeatedly been sanctioned. That includes experience and familiarity with the nature of the different sanctions (see, e.g., Conover 2000 and May and Wood 2010). The varied punishments thus may differ in their effects from those identified in prior work (e.g., Cochran et al. 2014).

Second, among second-time felons, does progressively tougher sanctioning, as compared to prior sanctions that individuals received, reduce recidivism? Drawing on the specific deterrence logic of recidivist sentencing premiums, we hypothesize that individuals who receive tougher punishments than they previously experienced (e.g., jail rather than intensive probation) will be less likely to recidivate. Following this logic, those who receive less severe sanctions (e.g., regular rather than intensive probation) will be more likely to recidivate.

One might speculate about what to expect from specific comparisons. For example, 12

possibilities exist for first-time felons and the second-time felons, respectively. Among first-time felons, the 12 comparisons reflect the fact that: (1) Probation can be considered to be a counterfactual to intensive probation, jail, or prison, (2) intensive probation can be considered a counterfactual to regular probation, jail, or prison, (3) jail can be considered a counterfactual to regular probation, intensive probation, or prison, and (4) prison can be considered a counterfactual to each of the others. Among second-time felons, 12 related counterfactuals exist. Here, the comparisons are to the sanctions that individuals previously received. In all instances, however, we see no clear theoretical basis for anticipating specific effects of unique first-time comparisons or progressions. Rather, the guiding theoretical logic is that more severe sanctions or sanction progressions should exert a greater specific deterrent effect and reduce recidivism and, conversely, that less severe sanctions or sanction progressions should produce the opposite.

A final observation: Criminal justice policy in recent decades has emphasized punitive sanctions for first-time and repeat offenders. It has done so despite a lack of consistent evidence that this approach more effectively reduces recidivism. Evidence about the effectiveness of this approach thus can help to shed light on its potential merits.

Data and Methods

Data

The data for this study come from the Florida Sentencing Guidelines database and include information for all felony convictions processed in Florida state courts between 1994 and 2008.¹ These data are well-suited for this study because they contain sentencing data for the entire state, a large number of cases to support the requirements necessary for matching analyses, individual-level information to address potential confounding that may bias estimates, and information about each individual's sanction and recidivism history.

Two sets of analyses are presented. For the first, we focus on all individuals who experienced a first-time felony conviction in Florida during the study period (N=549,746). For

the second, we focus on individuals from the first-time felon sample convicted of committing a new felony (N=89,809). For the second-time felony group, we examine sentencing progressions by partitioning the sample into subgroups determined by their first sanction (i.e., probation, intensive probation, jail, or prison) and their progression to a second sanction.

Descriptive statistics for all study variables are provided in table 1.1 and 1.2. Table 1.1 presents information for each of four groups in this first-time felon analysis—individuals who experienced their first felony conviction and then were sentenced to prison, jail, intensive probation, or regular probation. Table 1.2 includes descriptive statistics only for individuals convicted of a second felony conviction, divided into four groups based on the sanction that they received for their first-time conviction (prison, jail, intensive probation, or probation).

Insert table 1.1 about here

Insert table 1.2 about here

The dependent variable for this study is recidivism, which is measured as a new felony conviction within three years of sentencing, for individuals who were sentenced to intensive probation or regular probation, or within three years of release from incarceration, for individuals who were sentenced to jail or prison. Use of felony reconviction accords with the approach used in most prior work on recidivism and ensures that the focus centers on serious offending (Villettaz et al. 2014). In addition, the use of a three-year follow-up period allows the focus to center not only on those inmates most likely to fail within the first year of release (Cochran et al. 2014), and it accords with national studies of recidivism (Durose et al. 2014).

Each of the four sanction possibilities and progressions constitute independent variables.² Probation is by far the most common type of sanction that felony-convicted individuals receive (Corbett 2015; Kaebler and Glaze 2016). Intensive probation (termed “community control” in Florida) is less common and differs from regular probation in that it typically provides more frequent contact and supervision and may require house arrest and curfew (Piehl and LoBuglio

2005). As is the case nationally, jail and prison occur less frequently than probation. Debate exists about the severity of the various sanctions. Some studies, for example, indicate that offenders may sometimes view supervision as more severe than prison (May and Wood 2010; Petersilia and Turner 1993; Spelman 1995). In general, however, prior work and policy discussions contemplate that incarceration constitutes a more severe sanction (see, e.g., Latessa et al. 2014; Paternoster 2010; Pratt 2008; Villettaz et al. 2014).

A central task in assessing sanction effects on recidivism consists of addressing potential confounding that may arise from the likelihood that higher-risk individuals receive more severe punishments and, at the same time, may be more likely to offend (Nagin et al. 2009; Wermink et al. 2013). An important attribute of these data is that they permit matching on a measure of risk. This measure, total sentencing score, is created by the Florida Department of Corrections and the Office of the State Courts Administrator (2015). The score is comprised from measures across a variety of dimensions of risk, including offense severity and prior record; it is the primary information courts use to determine whether an individual should receive a prison term.

In addition to total sentencing score, the Florida data also allow inclusion of other measures that are used as part of the total score, but that may signal risk independently of their contribution to that score and that mirror covariates used in prior studies of incarceration effects (see, e.g., Cochran et al. 2014; Loughran et al. 2009; Nieuwbeerta et al. 2009; Wermink et al. 2013). Specifically, we match on offense severity score, which is determined by the Florida Sentencing Commission and ranges from 1 (low severity) to 10 (high severity). We also include a dummy variable measure that indicates whether the offense involved injury to a victim (1=yes, 0=no). The matching algorithm also includes three measures of prior record: (1) A count of the number of prior misdemeanors, (2) a count of the number of prior community supervision violations³, and (3) a total prior record score.⁴ This third measure, total prior record score, is only included as a matching variable in the analysis for second-time felons because first-time felony offenders do not have additional prior record score values apart from prior misdemeanors and community supervision violations, which are accounted for in the first two measures. Finally, we include,

too, a measure of whether a given conviction arose through a trial (1=yes, 0=no) and a measure of additional charges (1, 2, or 3) related to any given sentencing event. The additional charges measure was originally a count measure, but we truncated it at 3, the maximum value for more than 95 percent of all cases, to allow for exact matching (discussed next). Not least, we included demographic information about each individual (i.e., age, race, ethnicity, and sex).

Analytic Strategy

Recent reviews and studies have called for reliance on more rigorous methodologies, such as matching analyses, for assessing the impact of incarceration on recidivism (e.g., Nagin et al. 2009; Wermink et al. 2013; Villettaz et al. 2014). Matching approaches, like PSM and EM, can theoretically provide more accurate estimates of treatment effects when compared to conventional regression approaches when imbalances exist in the distributions of covariates between treated and control groups (MacDonald et al. 2007:2574; see also Apel and Sweeten 2010b). Such imbalances are likely to exist between groups receiving different correctional sanctions. As Nieuwbeerta et al. (2009:239) have emphasized, matching is not “a panacea for the problem of nonrandom assignment to treatment status”; it is, however, “a very useful device for ensuring that at least upon measured dimensions the treated and controls are comparable.”

Accordingly, we rely on matching methodologies to estimate the effects of sanctions (Apel and Sweeten 2010b; Guo and Fraser 2010).⁵ We rely on measures that Nagin et al. (2009) have recommended for matching analyses (see also Gaes et al. 2016). The large size of the sample allows us to utilize stringent matching criteria and the sentencing guidelines dimensions, which are unique to Florida and include detailed measures of risk and prior record, provide a rigorous set of matching covariates. Specifically, we conduct a series of PSM analyses, using Stata’s `psmatch2` command, to test the relative impact of sanction progressions for first- and second-time felony convictions on likelihoods of recidivism.⁶ For the first-time analyses, we use a .0001 caliper with replacement, and for the second-time felon analyses, we use a .001 caliper with

replacement. This latter analysis incorporates substantially smaller sample sizes, which required relaxing the caliper settings to incorporate sufficient matches. However, results from analyses using the .001 caliper were substantively identical to those using a .0001 caliper.

As a check on the robustness of the results, we also conduct ancillary analyses. These involve the use of exact matching and coarsened exact matching (CEM) approaches (Blackwell et al. 2009). Some accounts suggest that EM constitutes a more rigorous approach to creating matched groups that are identical, or nearly identical, along measured covariates except for the treatment variable of interest (see, e.g., Bales and Piquero 2012; Savolainen et al. 2013). Whereas PSM relies on matching treatment and control cases based on a score, EM or CEM matches individual treated cases to control cases with respect to each matching variable.

Exact matching is possible in this study because of the large number of cases in each of four sanction groups. Even in large samples, however, and when examining multiple groups, some “coarsening” is required (Desmond and Gershenson 2016). This process applies primarily to variables with a large range of values and entails creation of groups or few categories to improve matching success. For the analyses of the second-time felons, which involved a smaller sample, this approach was necessary. Specifically, we applied the coarsened-exact matching approach by using the Scott method provided within the “CEM” command in Stata (see Blackwell et al. 2009) to prior record points, prior misdemeanors, and total sentencing points and by using a five-category measure of age. The EM analyses employed a 1:k (or “1-to-many”) matching procedure to maximize the matched sample size. Thus, the matched sample includes all possible exactly matched control cases. Post-matching analyses then utilize weights to account for the oversampling from the control group (Desmond and Gershenson 2016; Guo and Fraser 2010).⁷ Our main focus, however, is on the PSM analyses because few cases are lost to off-support, whereas the EM analyses result in a larger percentage of cases that cannot be matched.

As a further check on the robustness of the results, we also conducted a series of matching analyses with alternative specifications, including separate EM analyses using 1-to-1 matching. For the second-time felons analyses, we employed different coarsening strategies (e.g., larger

value categories versus more fine-grained coarsening) and no coarsening at all. In each instance, the findings were statistically and substantively similar to those shown here. We conducted, too, ancillary analyses using a conventional logistic regression approach; the estimated effects were substantively similar to the results from the matching analyses.

The matching analyses proceed as follows. For the first-time felons, we examine each sanction type (e.g., probation, intensive probation, jail, and prison) as a separate treatment. For the second-time felons, we assess the effect of receiving more, as well as less, severe sanctions relative to receiving the same sanction again. After matching, we estimate regression models to estimate treatment effects while controlling for sentencing years and judicial circuits.

Findings

First-Time Felons: Sanction Comparisons

The analyses begin with a focus on first-time felons and the question of whether more severe types of sanctions decrease recidivism more so than those that are more lenient. The four punishments—probation, intensive probation, jail, and prison—give rise to 12 potential counterfactuals. These consist of probation as a “treatment” in comparison to intensive probation, jail, and prison, respectively (three separate counterfactuals), intensive probation in comparison to regular probation, jail, or prison, respectively (another three counterfactuals), jail in comparison to probation, intensive probation, or prison, respectively (still another three counterfactuals), and prison in comparison to each of the three other sanctions (three final counterfactuals). Collectively, there are six sanction comparisons involving more severe punishment and six that result in comparisons involving less severe punishment.

As is evident in table 1.1, prior to matching, these groups differ markedly from one another across a range of dimensions. For example, prisoners were more likely to be male, have had their sentence determined through a trial, injured a victim, and have a higher sentencing score, reflecting the commission of more serious crimes. Jail inmates were more likely to be older,

Latino, and to have more prior misdemeanors than other groups; across other dimensions, such as the probability of going to trial, they were more similar to prisoners, and across still others, such as sentencing points, were more similar to probationers. Similarly, across some dimensions the intensive probation group resembled that of probationers, while across others, such as offense severity, it more resembled that of prisoners. The overarching pattern is that the characteristics of each group as a whole are relatively distinct from those of the other groups.

Table 2.1 presents the results of the PSM analyses. For the treatment groups in each of the four sets of comparisons, the analyses seek to create, based on propensity scores, matched groups that resemble the treatment groups with respect to observed covariates.⁸ Panel A presents the matching results when probation serves as the treatment group; panel B presents the results when intensive probation is the treatment; panel C presents them when the treatment group consists of jail inmates; and panel D displays the prison-as-treatment results.

Insert table 2.1 about here

Across each of the three sets of comparisons in each of the four panels, we can see that the matching creates groups that are nearly identical across the observed covariates. For example, in panel A, the probation-as-treatment group and the intensive probation comparison group are similar with respect to their demographic characteristics, their past misdemeanor record, sentencing points, and other dimensions. The similarity is reflected in the percent bias column, which identifies the magnitude of difference for a given covariate between the treatment and comparison group after matching. Covariate averages for both groups are within 1 to 3 percentage points of one another, suggesting that the matching was successful. Similar patterns can be seen across the other two probation-as-treatment comparisons. The matched jail inmate and matched prisoner groups, for example, have a profile that mirrors that of the probation group. For the jail comparison, the bias statistics resemble those for the intensive probation comparison. However, for the prison comparison, the matching procedure did not fully eliminate

bias. Across the different covariates in this comparison, bias was reduced, but the post-matching covariate differences range from roughly 1 to 10 percent; even so, the magnitude of remaining post-matching differences across the covariates typically is small. In addition, we can see that almost all probation-as-treatment cases could be matched for each of these three comparisons. Specifically, fewer than half of one percent of treatment group cases could not be matched.

Similar results obtained for the comparisons in panels B (intensive probation as treatment), C (jails as treatment), and D (prison as treatment). In each instance, the comparison group profiles, as reflected in the covariates, strongly resemble those of the treatment groups. Inspection of the bias statistics, for example, indicates that the mean values for the covariates between treatment and comparison groups are typically within a few percentage points of one another. As with the panel A prison comparison, some modest covariate differences persist. The one notable exception consists of the prison-as-treatment versus jail comparison in panel D; although the two groups have similar profiles, including the sentencing point averages, the prisoner prior violation average was somewhat lower than that of the jail violation average. Overall, the matching procedure created largely similar groups with respect to observed covariates, with modest differences in some of the covariate comparisons that in turn call for caution in interpreting results for some of the estimated effects, such as the prison-vs.-jail comparison.

Table 2.2 builds on the matching analyses to estimate, using logistic regression, the effect of a given “treatment” sanction versus each of the counterfactual sanctions after controlling for clustering of individuals within judicial circuit and by year. Panels A through D present the four sets of counterfactual comparisons that parallel those from table 2.1. Sample sizes for each comparison reflect the sum of treated and matched (with replacement) cases. As inspection of the treatment rows in each panel highlights, a statistically significant effect of the treatment sanction arose across all comparisons. To the extent that the matching provides a credible estimate of sanction effects, the results suggest that punishment severity may affect recidivism.

Insert table 2.2 about here

To convey these results in a more intuitive manner, table 3 provides the predicted probabilities of recidivism for each separate comparison. The predicted probabilities were created by evaluating each of these models for each individual. We begin first with probation as the treatment sanction. As compared with intensive probation, it is associated with a greater probability of recidivism (17.6 vs. 17.0). The gray shading highlights that the result supports a specific deterrence logic—that is, less severe sanctioning should result in more offending. When compared with two other more severe punishments, jail and prison, probation is associated with lower rather than higher probabilities of offending. For probation-vs.-jail, the probabilities are 17.9 vs. 27.7, and for probation-vs.-prison, they are 17.6 vs. 24.8.

Insert table 3 about here

Next, we examine intensive probation and find support for the specific deterrence hypothesis when this more severe punishment is compared to probation. The probability of recidivism among intensive probationers is 16.9 and for matched probationers is 17.6. For the other two comparisons, however, where intensive probation serves as a sanction that is less severe, the results run counter to the hypothesis. In both cases, intensive probation appears to contribute to a lower probability of recidivism relative to jail and prison, respectively.

In the jail comparisons, we find that only one comparison points to a specific deterrent effect of a tougher sanction. Compared with regular and intensive probation, jail results in a higher probability of recidivism. As compared to prison, a more severe sanction, it is associated with a greater likelihood of recidivating, lending support to the deterrence hypothesis. A similar pattern surfaces for prison. It, too, appears to result in more rather than less recidivism when compared to matched groups of regular probationers and intensive probationers, respectively. However, in comparison to jail, it is associated with a reduced probability of recidivism.

In total, only 4 of the 12 statistically significant effects accord with the specific deterrence hypothesis that more severe punishment will reduce recidivism and, conversely, that less severe

punishment will increase it. However, in two of these comparisons (probation vs. intensive probation), the differences are less than 1 percent and so substantively trivial. Evidence, then, for tougher sanctions generating appreciable reductions in recidivism among first-time felons appears to be limited. That finding accords with Cochran et al.'s (2014) assessment of sanction effects among a general offender population. However, in contrast to that assessment, the findings here suggest that prison may be more effective than jail for those individuals who typically might receive a jail sentence. Also, the findings here suggest that probation and intensive probation are more effective than jail or prison, respectively, in reducing offending among first-time felons. For example, in 8 of the 12 comparisons, probation of either type is associated with less recidivism than the more severe sanction alternatives.

Second-Time Felons: Sanction Progressions

We turn now to a related question: Among individuals who committed a second felony, what is the effect of then receiving a more severe or a less severe type of sanction? The analyses focus on the 12 specific progression possibilities. For individuals who previously were sentenced to probation, three different sanctions, all more severe, may occur. For those sentenced to intensive probation, one of the three options (regular probation) is less severe and the two others (jail and prison) are more severe. Previously jailed individuals face two less severe options and one more severe option. And the three sanction possibilities for those who previously served time in prison are all less severe. In total, there are six more severe and six less severe sanction progressions. These allow us to test whether tougher sanction progressions produce less recidivism and, conversely, if more lenient ones increase it.

In table 1.2, we can see that the second-time felon groups sentenced to regular probation, intensive probation, jail, and prison, respectively, differed with respect to their demographic characteristics, prior criminal record, offense severity, the punishments that they previously received, and their rate of recidivism. Given these differences, PSM analyses were used to create

matched groups, based on the propensity scores, that resemble the treatment groups with respect to observed covariates.⁹ The resulting matched groups are shown in table 4.1, which presents panels for each of four groups: Panel A=previously on probation, B=previously on intensive probation, C=previously sentenced to jail, and D=previously sentenced to prison.

As with the analyses for the first-time felons, the matching here created groups that are largely similar across the observed covariates.¹⁰ Across the 12 comparisons, the remaining difference in covariate means is typically 3 percent or less and few cases are off-support. Some exceptions exist. For example, in panel B, the prison-as-treatment vs. intensive probation matching resulted in nearly similar groups; however, the average number of prior misdemeanors was slightly lower for the prison group. In addition, 14 percent of the prison cases could not be matched to individuals on intensive probation. Overall, though, the matching achieved balanced on covariates and retained 95 percent or more cases across almost all comparisons.

Insert table 4.1 about here

The next step in the analyses consists of adjusting for the clustering of individuals within judicial circuit and by year to provide estimates of the effect of each of the 12 progressions on recidivism. The logistic regression models are displayed in table 4.2. Except for the prison-as-treatment vs. probation comparison in panel A, all treatment effects are statistically significant and suggest that the progressions may influence recidivism.

Insert table 4.2 about here

To facilitate the discussion of the results, the predicted probabilities from the progression comparisons are displayed in table 5. We begin with those individuals whose first sanction was probation. Among this group, those who receive intensive probation rather than probation are less likely to recidivate (31.9 vs. 43.6 percent), which supports a specific deterrence view of punishment. However, first-time probationers who then were sentenced to jail or prison are

more, not less, likely to recidivate as compared to matched counterparts on probation.

Insert table 5 about here

Second, among those who first were sanctioned to intensive probation, a different pattern surfaced. Those who were placed on probation recidivate less than matched counterparts placed on intensive probation (40.5 vs. 65.7 percent). That may seem to contradict the comparison above of intensive probation-as-treatment vs. regular probation, but it does not. Different counterfactuals are involved. The focus here is on probation-as-treatment in comparison to intensive probationers who resemble this treatment group. We can see, too, that incarceration appears to reduce recidivism more so than receiving intensive punishment a second time.

The third set of comparisons involve those previously sent to jail as punishment. For this group, both types of probation are associated with a lower probability of recidivism as compared to matched counterparts sentenced to jail. This pattern runs counter to a specific deterrence view that less severe sanctions should increase recidivism. In support of the deterrence hypothesis, however, prison reduces recidivism more so than it does among matched jail inmates.

Finally, when we turn to individuals who were sentenced to prison for their first felony, we find no support for the deterrence hypothesis. In each matched comparison, individuals who received less severe subsequent sanctions recidivate less than did matched prison inmate groups.

The overarching pattern is clear—in only 4 of the 12 comparisons do we find support for the hypothesis that more severe sanctioning of second-time felons reduces offending more so than less severe sanctioning. However, in these four cases, the effects generally are appreciable. Among individuals who initially were placed on probation, being sanctioned to intensive probation for a second felony appears to be more effective than being sanctioned to another round of probation. Similarly, among individuals who previously were sentenced to intensive probation, prison resulted in a more than 20 percentage point reduction in recidivism as compared to those who were again placed on intensive probation; placement in jail exerted a

similar effect. And among those previously placed in jail, prison reduced recidivism by over 10 percentage points as compared to individuals who were placed in jail a second time.

For two-thirds of the sanction progressions, however, tougher sanctions appear to be criminogenic. For example, among individuals whose first felony led to imprisonment, recidivism was lower when, in response to a second felony, they were sentenced to less severe sanctions. Put differently, across 6 of the 7 other statistically significant sanction progression effects, regular and intensive probation typically were associated with lower rates of recidivism.

As a check on the robustness of the results from the PSM analyses, we conducted two sets of ancillary analyses. First, we employed exact matching (EM), as well as coarsened exact matching (CEM), and replicated all of the PSM analyses of first-time felons and second-time felons, respectively. Exact matching has been argued by some scholars to produce more accurate estimates of intervention impact (Bales and Piquero 2012; Savolainen et al. 2013; Gaes et al. 2016). It does not overcome potential problems that arise from unobserved confounding. It can, though, generate comparison groups that may more closely match those of treatment groups. Appendix A provides the results of the EM analyses for first-time felons; these results reveal a similar pattern to those identified in table 3. Appendix B provides the results of the EM analyses for second-time felons; here, the results largely parallel those from table 5. Second, we estimated the punishment comparisons and progressions using logistic regression. These analyses consisted of separate models for the first-time felon comparisons and separate models for the second-time felon progressions. The results were statistically and substantively similar to those from the matching analyses. Collectively, the two additional sets of analyses lend support to the PSM analyses. As we discuss below, however, there is a need for further research on the effects of tougher, and progressively tougher, sanctions on recidivism.¹¹

Conclusion

The shift toward more punitive sanctioning has sought to achieve general deterrent effects

and retribution and perhaps to achieve political goals (Garland 2013; Gottschalk 2015). Even so, recidivism reduction has constituted a core aim (Austin et al. 2016; Cullen et al. 2011). Indeed, a prominent justification has been that tougher punishment would reduce recidivism and that progressively tougher sanctioning, such as prison rather than probation, would more effectively reduce recidivism than would continuing to apply the same sanctions. This latter emphasis on a “recidivist sentencing premium” (Roberts 2008:468) is striking both for its ubiquity in local, state, and national sentencing regimes and for the near void in empirical research that directly examines this logic. Scholarship on deterrence does not uniformly anticipate that more severe sanctions will provide a greater specific deterrent, and some research suggests that they may worsen offending (Cochran et al. 2014; Jacobs 2010; Latessa et al. 2014; Lipsey and Cullen 2007; Nagin et al. 2009; Paternoster 2010; Stafford et al. 1986; Steiner et al. 2012).

Accordingly, this study sought to build on work (e.g., Nieuwebeerta et al. 2009; Wermink et al. 2013) aimed at understanding the effects of sanctions among first-time felons and first-time felons who recidivated. The first group allowed for investigating whether more severe sanctions of various types—ranging from probation to intensive probation, jail, and prison—exert a stronger effect in reducing recidivism. The latter group allowed for investigating the effects of progressively tougher sanctions than those that individuals previously received. In so doing, the study also sought to heed recommendations to use rigorous methodologies, such as matching analyses (Bales and Piquero 2012; Cochran et al. 2014; Nagin et al. 2009; Villettaz et al. 2014).

The results of the study can be summarized briefly. First, the analyses did not clearly support the deterrence-based hypothesis that tougher types of sanctioning reduce recidivism among first-time felons. It led to modest reductions in reoffending in only 2 of the 12 comparisons; in 2 other comparisons, there was no appreciable difference; and in the remaining 8, less severe, not more severe, sanctioning was associated with a lower probability of offending. Indeed, in these 8 comparisons, regular or intensive probation consistently were associated with less recidivism as compared to the more severe sanction alternatives of jail and prison. This pattern, which to our knowledge is the first that focuses on first-time felons and the effects of several categories of

probation and incarceration, echoes the finding in many prior studies that incarceration may exert a null or criminogenic influence on recidivism.

Second, analyses of second-time felons did not clearly support the hypothesis that progressions to more severe types of sanctions would decrease recidivism. They did so for 4 of 12 progressions and had no effect on one other; in the remaining 7, progressions to less severe sanctions—especially probation and intensive probation—were associated with a lower probability of recidivism. An exception was tougher punishment of individuals who previously had been placed on intensive probation. For this group, jail and prison were more effective. So, too, however, was probation. Stated differently, when individuals have had prior experience with intensive probation, a different sanction may be more effective than more of the same.

Before discussing implications of the study, several limitations warrant discussion. One centers on the modeling approach taken. Specifically, the study relied on a matching analysis approach to estimating sanction effects. In so doing, it used the types of observable confounders found in similar research and that accord with Nagin et al.'s (2009) recommendations for variables that should be employed (Gaes et al. 2016; Helland and Tabarrok 2007; Ostermann and Hyatt 2016). Even so, unobserved confounding nonetheless might have influenced the results. For example, differences in the social capital of individuals across the sanction groups might be relevant as might their juvenile court record, case file information about amenability to treatment during a prior sanction, or the extent to which an individual complied with conditions of probation or jail or prison rules; the latter might influence a pre-sentence investigation report and in turn court decisions about which sanctions are appropriate. Also, although the matching analyses resulted in retention of almost all treatment cases, some loss of support in several comparisons may have influenced the estimated effects. It will be important for future studies to extend this work through matching analyses with other data or reliance on alternative estimation strategies, such as instrumental variable analysis, where such is possible.

A second limitation involves a broader set of methodological considerations. For example, to the extent that the results are generalizable, they hold only for first-time felons who would be

likely to experience different sanctions or to second-time felons likely to experience the different sanction progressions examined here. In addition, the study operationalized severity by focusing on sanction types. There are, however, alternative ways of operationalizing severity. For example, studies could examine sanction durations or the conditions that attach to particular sanctions (e.g., the amount of deprivation associated with incarceration). Accordingly, the results here may not reflect what might be found if severity were operationalized in these other ways. Similarly, the study examined the severity of punishment, not other dimensions of it, such as the certainty or celerity of punishment, that may interact with severity in influencing recidivism outcomes. Not least, the study focused only on one state, and it relied on a measure of recidivism used in many studies of sanction effects, but it is possible that the effects might vary using other measures (see, generally, Kazemian 2007 and Laub and Sampson 2001).

Several implications for research warrant discussion. First, more research is needed that examines the effects of punishments of varying severity (e.g., type or duration) and whether the effects are relative to the severity of prior sanctions that individuals have received. In this study, some of the analyses lent support to the specific deterrence hypothesis that more severe sanctions can be effective. The most notable example of that possibility emerged in the analysis of individuals whose first felony conviction resulted in intensive probation. Members of this group who went on to commit a second felony were less likely to recidivate if placed in jail or prison. Similarly, those sentenced to jail for their first felony appeared to be more deterred by a stay in prison for a second felony. Even in these cases, however, less severe sanctions were effective. For example, probation, like prison, was more effective than jail among those second-time felons who previously were sentenced to jail. And, in general, less severe sanctions were more frequently associated with lower rates of recidivism.

The general axiom, then, seems to be that less severe sanctions *or* different sanctions than were previously received may be more effective than imposing the same sanction again. However, we echo calls by others for more research on the effects of tougher sanctions and on tougher sanction progressions (e.g., Durlauf and Nagin 2011; Mears et al. 2015; Roberts 2008).

There is also a need for research that examines whether these approaches exert heterogeneous effects for men and women as well as different age, racial and ethnic, and socioeconomic groups.

Second, the theoretical logic of both tougher sanctions for first-time felons and for tougher sanction progressions, respectively, warrants close investigation. The punitive-is-better logic has governed sentencing policy for several decades, and can be seen in the development of both juvenile and adult corrections (Clear and Frost 2014; Howell 1995). It has not, however, been systematically evaluated. That is understandable from a methodological standpoint: Assessing different progressions, and doing so while addressing selection effects, is difficult. In this study, for example, we began with a half-million individuals, but the number of cases declined quickly to the thousands when we focused on second-time felons. Observe, in this regard, that already the number of possible progressions involved in two successive punishments, when four types of sanctions can be imposed, is considerable. By the second felony, there exist four sets of four progressions, for a total of 16 possible progressions. By the third felony, there exist 64. Each of these progressions represents the equivalent of a potentially different treatment. From a prosecutorial or judicial perspective, such permutations are the stuff of everyday decisionmaking (Heilbroner 1990). Prosecutors and judges in criminal court regularly confront situations where individuals have been convicted and punished many times. They frequently are guided by a progressive sanctioning logic that indicates that a more severe system response is warranted. Yet, what that response should be, given the different possible progressions, is unclear.

Third, there is a related need for research that unpacks the nature of specific sanction experiences and how they may influence the effects of sanction progressions. Under some conditions, probation might be perceived to be more severe than prison (Corbett 2015; May and Wood 2010; Spelman 1995; see also May et al. 2014). Conversely, prison in some instances may be perceived to be more severe and to provide more opportunities for rehabilitation. More generally, the effects of sanctions may depend on the particular design and implementation of sanctions (Lipsey and Cullen 2007; Mears et al. 2015; Whitman 2003). In some cases, prison may entail short stays, little evidence-based programming, and simultaneously may serve as a

school for crime; in others, it may entail longer stays and reliance on well-evaluated programs. The nature of one's experience with a sanction, too, can be expected to influence how one might be affected by a second or third round of it or another sanction. It is possible as well that punishment experiences and their effects may vary across different groups of individuals, including variation by age, sex, race, ethnicity, socioeconomic status, and other dimensions (Ekland-Olson et al. 1983). Assessing such possibilities will require studies that examine different sanctions, how individuals perceive them, and particular sanction sequences.

Finally, two policy implications merit mention. One is that greater reliance on probation and other intermediate sanctions may warrant renewed consideration (Petersilia and Turner 1993; Piehl and LoBuglio 2005). Such sanctions may be more effective than incarceration at reducing recidivism, especially given that studies and reviews cast doubt on the prospects that incarceration markedly reduces it (e.g., Bales and Piquero 2012; Cochran et al. 2014; Nagin et al. 2009). Among other things, probation may allow for punishments that individuals sometimes may view as more severe than incarceration. Of course, incarceration may produce general deterrent effects and can be justified on retributive or incapacitative grounds; however, even here, no consensus exists about its superiority to intermediate sanctions (Blomberg et al. 2016; Nagin and Snodgrass 2013; Paternoster 2010; Rossi and Berk 1997; Spelman 1995, 2006).

A second implication is that the logic of the recidivist sentencing premium may warrant reconsideration, at least to the extent that it is justified as a means by which to reduce recidivism. Deterrence does not require on theoretical or logical grounds more severe punishment or increasingly more severe punishment. Its effect can depend on other conditions, such as punishment certainty or celerity (Nagin 2013; Paternoster 2010). It is possible, too, that initial sanctions need only modest reinforcement to induce deterrence; here, then, less severe progressive sanctions may suffice to activate specific deterrence processes. Not least, it is possible that community-based sanctions may be less criminogenic and provide more opportunities for rehabilitation (Austin et al. 2016; Latessa et al. 2014; Lipsey and Cullen 2007).

Endnotes

¹ The Guidelines went into effect on January 1, 1994. We thus cannot examine whether the Guidelines might have altered the effects of progressive sanctions or sanction sequences.

² The Florida Legislature abolished parole in 1983, hence the effect of prison generally reflects a prison, not a prison and parole, experience.

³ For first-time felons, the violations measure captures violations of supervision that resulted from commission of misdemeanors, and thus applies to all four sanction groups.

⁴ Prior record includes information only for criminal activity committed as an adult.

⁵ We considered such approaches as regression discontinuity (RD) designs (Berk et al. 2010; Worrall and Morris 2011), instrumental variable (IV) analyses (Bushway and Apel 2010), and random assignment of cases to judges (Green and Winik 2010). However, no scoring system for sentencing to sanctions or sanction progressions exists, which precludes use of RD. Similarly, there is no clear instrument that can be used for assignment to different sanctions or sanction progressions. And data on sentencing proclivities of judges are not available to undertake a study built around potential random assignment of cases to judges who may vary in their punitiveness. Although these different approaches can provide credible estimates of impacts, matching approaches, too, can do so as well. They all rely on different requirements, such as scoring systems or relevant instruments. Here, we employ matching variables recommended by and used in prior research (Nagin et al. 2009). The analyses thus should provide a credible basis for estimating sanction effects. However, as with any study that relies on observed covariates to address selection effects, unobserved confounding might still have biased the estimates.

⁶ We use the psmatch2 standard errors. Some work suggests that bootstrapping methods for producing errors may be indicated when using nearest neighbor matching without replacement (Austin 2014). Our study uses such matching but with replacement; in these instances, the use of bootstrapping is not advised (Abadie and Imbens 2008).

⁷ All exact matching analyses entailed three steps, which parallel the approach used for the

PSM analyses. First, we specified matching variables and identified for the treatment group and the three respective matched control groups. Second, we compared variable means across matching variables to assess whether the approach resulted in matched groups. Third, we then use the matched samples in a logistic regression equation that include dummy variable measures for sentencing year and judicial circuit to estimate prison effects after accounting for the clustering of sentencing events across time and place. Steps 2 and 3 also include weights to account for oversampling of the control group due to the 1:k matching.

⁸ For each of the 12 comparisons, logistic regression models were run to estimate the probability of treatment and, in turn, to create propensity scores to permit the matching. To conserve space, these models are not included but are available upon request.

⁹ For each progression, a logistic regression model was run to estimate the probability of treatment. The resulting propensity scores were used to create a matched treatment group and control group. This approach parallels that used for the first-time felon analyses; here, again, to conserve space, the progression models are not shown but are available upon request.

¹⁰ We used a caliper setting of .0001 for all analyses except panels B and D of table 4.1. For these two panels of comparisons, use of that caliper resulted in loss of approximately one-third of cases off support. By contrast, use of a still-stringent caliper of .001 allowed almost all treated cases to be matched. Comparison of the two sets of results using the two different caliper settings revealed statistically and substantively similar results.

¹¹ We thank one of the anonymous reviewers for drawing attention to the fact that age of the subjects might influence the estimates. For example, even though the study matched on age, some subjects were incarcerated and some placed on probation, which might create an age discrepancy during the observation period that could bias estimates. It is a possibility. However, the magnitude of many of the identified differences in recidivism well exceed what would be anticipated by any potential age discrepancy, especially given that the average age of the samples was approximately 30-years-old, well past the peak age of offending (Valentine et al. 2015).

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Table 1.1. Descriptive Statistics, First-Time Convicted Felons

	Prison (N=30,734)		Jail (N=63,213)		Intensive Probation (N=36,769)		Probation (N=419,030)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Age (years)	29.496	(11.168)	31.757	(10.863)	29.739	(11.479)	30.337	(10.756)
White (1/0)	0.478	(0.500)	0.531	(0.499)	0.566	(0.496)	0.560	(0.496)
Black (1/0)	0.361	(0.480)	0.401	(0.490)	0.297	(0.457)	0.307	(0.461)
Latino (1/0)	0.161	(0.368)	0.067	(0.251)	0.137	(0.344)	0.133	(0.339)
Male (1/0)	0.883	(0.321)	0.807	(0.394)	0.789	(0.408)	0.736	(0.441)
Trial (1/0)	0.052	(0.221)	0.007	(0.085)	0.011	(0.106)	0.009	(0.095)
Offense severity (1-10)	6.499	(1.945)	3.395	(1.606)	5.299	(2.107)	3.663	(1.830)
Charges (1-3+)	1.229	(1.196)	0.630	(0.903)	1.124	(1.152)	0.793	(0.990)
Victim injury (1/0)	0.258	(0.438)	0.027	(0.163)	0.180	(0.384)	0.062	(0.242)
Prior misdemean. (#)	0.785	(1.343)	0.806	(1.297)	0.758	(1.225)	0.693	(1.145)
Prior violation (1/0)	0.126	(0.332)	0.117	(0.321)	0.100	(0.300)	0.016	(0.124)
Sentencing points (#)	83.020	(62.645)	22.844	(17.129)	54.578	(55.638)	25.819	(26.219)
New conviction (1/0)	0.223	(0.416)	0.292	(0.454)	0.161	(0.367)	0.180	(0.384)

Table 1.2. Descriptive Statistics, Second-Time Convicted Felons

	Prison (N=13,818)		Jail (N=28,798)		Intensive Probation (N=9,096)		Probation (N=38,097)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Age (years)	27.762	(8.857)	29.774	(9.739)	28.615	(9.105)	29.508	(9.200)
White (1/0)	0.427	(0.495)	0.419	(0.493)	0.532	(0.499)	0.506	(0.500)
Black (1/0)	0.460	(0.498)	0.479	(0.500)	0.383	(0.486)	0.384	(0.486)
Latino (1/0)	0.113	(0.316)	0.102	(0.302)	0.085	(0.279)	0.109	(0.312)
Male (1/0)	0.883	(0.322)	0.822	(0.382)	0.764	(0.425)	0.771	(0.420)
Trial (1/0)	0.025	(0.155)	0.006	(0.074)	0.007	(0.081)	0.008	(0.091)
Prior record pts (#)	9.444	(13.570)	5.191	(8.405)	4.884	(7.995)	4.329	(6.836)
Offense severity (1-10)	5.154	(1.867)	3.317	(1.522)	3.895	(1.886)	3.368	(1.708)
Charges (1-3+)	1.425	(1.204)	0.774	(0.981)	1.265	(1.155)	0.927	(1.051)
Victim injury (1/0)	0.094	(0.292)	0.017	(0.129)	0.049	(0.216)	0.034	(0.182)
Prior misdemean. (#)	1.277	(1.560)	1.111	(1.452)	1.285	(1.480)	1.249	(1.521)
Prior violation (1/0)	0.338	(0.473)	0.206	(0.405)	0.362	(0.481)	0.111	(0.315)
Sentencing points (#)	61.055	(42.541)	27.932	(17.316)	39.067	(30.520)	28.157	(22.277)
1st sanction=prob.	0.647	(0.478)	0.613	(0.487)	0.770	(0.421)	0.789	(0.408)
1st sanction=int. prob.	0.086	(0.280)	0.041	(0.199)	0.120	(0.325)	0.042	(0.201)
1st sanction=jail	0.108	(0.311)	0.305	(0.460)	0.074	(0.262)	0.131	(0.337)
1st sanction=prison	0.158	(0.365)	0.041	(0.198)	0.035	(0.185)	0.038	(0.191)
New conviction (1/0)	0.442	(0.497)	0.507	(0.500)	0.368	(0.482)	0.430	(0.495)

Table 2.1. Covariate Comparisons after Propensity Score Matching (1st-Time Felons)

A. Treatment (tx) = Probation	Probation (Tx) vs. Intensive Probation			Probation (Tx) vs. Jail			Probation (Tx) vs. Prison		
	P (Tx)	IP (Control)		P (Tx)	Jail (Control)		P (Tx)	Prison (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	30.334	30.176	1.4	30.335	29.822	4.7	30.337	31.527	-10.9
White	0.560	0.575	-3.0	0.561	0.566	-0.9	0.560	0.573	-2.5
Black	0.307	0.303	1.0	0.308	0.304	0.7	0.307	0.314	-1.5
Latino	0.132	0.122	3.0	0.131	0.130	0.3	0.133	0.113	5.6
Male	0.736	0.744	-1.9	0.736	0.733	0.9	0.736	0.775	-10.3
Trial	0.009	0.009	0.0	0.009	0.009	0.5	0.009	0.010	-0.8
Offense severity	3.656	3.637	1.0	3.656	3.615	2.4	3.661	3.474	9.9
Additional charges	0.791	0.765	2.4	0.791	0.758	3.4	0.793	0.818	-2.3
Victim injury	0.062	0.058	1.1	0.061	0.061	0	0.062	0.070	-2.1
Prior misdemeanors	0.693	0.698	-0.4	0.693	0.655	3.2	0.693	0.806	-9.1
Prior violation	0.014	0.017	-1.2	0.015	0.015	-0.3	0.015	0.030	-5.8
Sentencing points	25.527	26.199	-1.5	25.512	24.897	2.8	24.674	27.224	-5.5
Matched tx/Total tx	417,995/419,030 (0.2% off support)			417,517/419,030 (0.3% off support)			418,806/419,030 (0.1% off support)		

B. Treatment (tx) = Int. Probation	Intensive Probation (Tx) vs. Probation			Intensive Probation (Tx) vs. Jail			Intensive Probation (Tx) vs. Prison		
	IP (Tx)	P (Control)		IP (Tx)	Jail (Control)		IP (Tx)	Prison (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	29.760	29.787	-0.2	29.902	29.574	2.9	29.700	29.655	0.4
White	0.567	0.565	0.3	0.570	0.556	2.8	0.562	0.565	-0.6
Black	0.297	0.296	0.2	0.305	0.322	-3.5	0.300	0.303	-0.7
Latino	0.136	0.139	-0.7	0.125	0.122	1.0	0.138	0.132	1.7
Male	0.787	0.794	-1.7	0.787	0.801	-3.5	0.801	0.815	-3.7
Trial	0.011	0.012	-0.7	0.011	0.012	-0.4	0.011	0.012	-0.5
Offense severity	5.262	5.333	-3.6	5.043	5.074	-1.6	5.392	5.364	1.4
Additional charges	1.110	1.046	5.9	1.085	1.071	1.4	1.133	1.102	2.6
Victim injury	0.175	0.183	-2.4	0.143	0.142	0.3	0.184	0.187	-0.7
Prior misdemeanors	0.758	0.775	-1.5	0.790	0.855	-5.2	0.758	0.732	2.0
Prior violation	0.092	0.086	2.6	0.107	0.141	-10.8	0.102	0.100	0.5
Sentencing points	52.393	51.977	1.0	48.012	48.189	-0.4	55.177	56.485	-2.2
Matched tx/Total tx	36,151/36,769 (0.1% off support)			32,625/36,769 (4.1% off support)			35,688/36,769 (1.6% off support)		

Table 2.1. Covariate Comparisons after Propensity Score Matching (1st -Time Felons) (cont.)

C. Treatment (tx) = Jail	Jail (Tx) vs. Probation			Jail (Tx) vs. Intensive Probation			Jail (Tx) vs. Prison		
	Jail (Tx)	P (Control)		Jail (Tx)	IP (Control)		Jail (Tx)	Prison (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	31.726	31.810	-0.8	31.682	31.018	5.9	31.731	31.705	0.2
White	0.533	0.543	-2.1	0.534	0.572	-7.7	0.531	0.556	-5.0
Black	0.400	0.394	1.3	0.398	0.359	8.3	0.402	0.384	3.8
Latino	0.067	0.063	1.5	0.068	0.069	-0.4	0.067	0.061	2.0
Male	0.807	0.813	-1.3	0.807	0.794	3.1	0.808	0.819	-3.1
Trial	0.007	0.006	1.1	0.007	0.009	-1.4	0.007	0.009	-0.9
Offense severity	3.397	3.394	0.2	3.406	3.319	4.7	3.380	3.186	10.9
Additional charges	0.629	0.632	-0.3	0.633	0.642	-1.0	0.615	0.673	-5.5
Victim injury	0.027	0.025	1.0	0.027	0.027	0.3	0.027	0.034	-2.0
Prior misdemeanors	0.800	0.770	2.5	0.807	0.812	-0.4	0.796	0.901	-7.9
Prior violation	0.110	0.107	1.0	0.117	0.137	-6.3	0.114	0.163	-15.1
Sentencing points	22.783	22.524	1.2	22.925	22.598	0.8	22.714	22.948	-0.5
Matched tx/Total tx	62,666/63,213 (0.1% off support)			62,808/63,213 (0.4% off support)			61,241/63,213 (2.1% off support)		

D. Treatment (tx) = Prison	Prison (Tx) vs. Probation			Prison (Tx) vs. Intensive Probation			Prison (Tx) vs. Jail		
	Prison (Tx)	P (Control)		Prison (Tx)	IP (Control)		Prison (Tx)	Jail (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	29.561	29.975	-3.8	29.466	29.745	-2.5	29.567	29.744	-1.6
White	0.481	0.477	0.9	0.485	0.482	0.6	0.479	0.477	0.5
Black	0.358	0.356	0.3	0.354	0.354	0.1	0.366	0.387	-4.3
Latino	0.161	0.167	-1.7	0.160	0.164	-0.9	0.155	0.136	5.8
Male	0.880	0.877	0.7	0.880	0.881	-0.4	0.880	0.873	1.9
Trial	0.045	0.038	4.1	0.031	0.029	1.3	0.050	0.033	10.0
Offense severity	6.412	6.442	-1.6	6.430	6.451	-1.0	6.353	6.312	2.3
Additional charges	1.195	1.159	3.3	1.215	1.183	2.7	1.207	1.229	-2.0
Victim injury	0.249	0.246	0.8	0.248	0.249	0.0	0.248	0.233	4.4
Prior misdemeanors	0.781	0.788	-0.6	0.773	0.748	2.0	0.780	0.810	-2.3
Prior violation	0.114	0.116	-0.9	0.124	0.121	0.8	0.130	0.214	-25.5
Sentencing points	78.717	78.294	0.9	78.756	79.267	-0.9	83.563	83.521	0.1
Matched tx/Total tx	29,053/30,734 (0.4% off support)			29,387/30,734 (2.0% off support)			25,107/30,734 (6.0% off support)		

Table 2.2. Post-PSM Logistic Regression of Recidivism on Progressively Tougher Sanctions, Propensity Score, and Judicial Circuit and Sentencing Year (1st-Time Felons)

A. Treatment (tx) = Probation	Probation (Tx) vs. Int. Probation		Probation (Tx) vs. Jail		Probation (Tx) vs. Prison	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	0.046**	0.016	-0.560***	0.011	-0.432***	0.018
Propensity score	1.136***	0.053	-0.913***	0.041	0.666***	0.036
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-3.161***	0.056	-0.686***	0.046	-2.249***	0.042
n (treated and matched) =	454,764		480,730		449,540	
Log Pseudolikelihood =	-211543.97		-233340.27		-212154.34	
Pseudo R-squared =	0.010		0.017		0.009	
B. Treatment (tx) = Int. Probation	Int. Probation (Tx) vs. Probation		Intensive Probation (Tx) vs. Jail		Int. Probation (Tx) vs. Prison	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	-0.054***	0.016	-0.643***	0.020	-0.502***	0.022
Propensity score	-1.115***	0.052	-0.421***	0.040	0.816***	0.065
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-1.978*	0.031	-1.468***	0.076	-2.271***	0.129
n (treated and matched) =	455,181		95,838		66,422	
Log Pseudolikelihood =	-211719.21		-52421.78		-31649.15	
Pseudo R-squared =	0.009		0.026		0.018	

* p < .05, ** p < .01, *** p < .001

Note: To conserve space, coefficients for dummy variables for judicial circuit and sentencing year dummy variables not shown. Sample sizes reflect the sum of treated and matched (with replacement) cases.

Table 2.2. Post-PSM Logistic Regression of Recidivism on Progressively Tougher Sanctions, Propensity Score, and Judicial Circuit and Sentencing Year (1st-Time Felons) (cont.)

C. Treatment (tx) = Jail	Jail (Tx) vs. Probation		Jail (Tx) vs. Intensive Probation		Jail (Tx) vs. Prison	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	0.562***	0.011	0.654***	0.020	0.190***	0.025
Propensity score	0.946***	0.041	0.522***	0.038	0.381***	0.035
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-2.167***	0.030	-2.626***	0.077	-2.083***	0.087
n (treated and matched) =	481,696		99,577		91,975	
Log Pseudolikelihood =	-233579.05		-53608.49		-52832.09	
Pseudo R-squared =	0.017		0.030		0.012	
D. Treatment (tx) = Prison	Prison (Tx) vs. Probation		Prison (Tx) vs. Int. Probation		Prison (Tx) vs. Jail	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	0.429***	0.018	0.502***	0.022	-0.205***	0.025
Propensity score	-0.727***	0.037	-0.826***	0.066	-0.339***	0.035
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-2.012***	0.031	-1.993***	0.126	-1.550***	0.085
n (treated and matched) =	448,083		66,156		88,320	
Log Pseudolikelihood =	-211323.85		-31476.63		-51192.31	
Pseudo R-squared =	0.009		0.018		0.011	

* p < .05, ** p < .01, *** p < .001

Note: To conserve space, coefficients for dummy variables for judicial circuit and sentencing year dummy variables not shown. Sample sizes reflect the sum of treated and matched (with replacement) cases.

Table 3. Post-matching (PSM) results for all matching combinations: Predicted probabilities, from logistic regression models, of recidivating (1st-Time Felons)

	<i>Treatment (tx)</i>		<i>Control</i>		
	<i>% Recidivism</i>		<i>% Recidivism</i>		
Probation (tx) vs...					
<i>Intensive probation (control)</i>	Prob	17.6	IP	17.0	**
<i>Jail (control)</i>	Prob	17.9	Jail	27.7	***
<i>Prison (control)</i>	Prob	17.6	Prison	24.8	***
Intensive probation (tx) vs...					
<i>Probation (control)</i>	IP	16.9	Prob	17.6	***
<i>Jail (control)</i>	IP	17.2	Jail	28.3	***
<i>Prison (control)</i>	IP	15.1	Prison	22.8	***
Jail (tx) vs...					
<i>Probation (control)</i>	Jail	27.7	Prob	17.9	***
<i>Intensive probation (control)</i>	Jail	28.0	IP	16.8	***
<i>Prison (control)</i>	Jail	27.7	Prison	24.1	***
Prison (tx) vs...					
<i>Probation (control)</i>	Prison	24.7	Prob	17.6	***
<i>Intensive probation (control)</i>	Prison	22.9	IP	15.2	***
<i>Jail (control)</i>	Prison	24.2	Jail	28.1	***

* $p < .05$, ** $p < .01$, *** $p < .001$, for treatment coefficients in regression models predicting recidivism (table 2.2).

Note: Gray cells indicate evidence for deterrence.

Table 4.1. Covariate Comparisons after Propensity Score Matching (2nd-Time Felons)

A. First sanction = Probation	Intensive Probation (Tx) vs. Probation			Jail (Tx) vs. Probation			Prison (Tx) vs. Probation		
	IP (Tx)	P (Control)		Jail (Tx)	P (Control)		Prison (Tx)	P (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	28.727	28.583	1.6	29.068	29.105	-0.4	28.034	27.890	1.6
White	0.542	0.541	0.3	0.431	0.433	-0.3	0.464	0.458	1.2
Black	0.373	0.377	-0.8	0.450	0.455	-1.0	0.424	0.427	-0.6
Latino	0.084	0.082	0.8	0.118	0.112	2.0	0.113	0.115	-0.9
Male	0.753	0.753	-0.1	0.802	0.807	-1.1	0.852	0.858	-1.5
Trial	0.007	0.009	-1.9	0.007	0.006	1.2	0.017	0.015	1.7
Prior record points	3.888	3.835	0.9	4.051	4.134	-1.4	6.073	5.741	4
Offense severity	3.763	3.796	-1.8	3.326	3.343	-1.0	4.871	4.853	1
Additional charges	1.251	1.241	0.9	0.884	0.869	1.4	1.419	1.442	-2
Victim injury	0.044	0.043	0.5	0.017	0.015	1.2	0.085	0.085	0.2
Prior misdemeanors	1.241	1.243	-0.2	1.129	1.128	0.1	1.280	1.302	-1.5
Prior violation	0.385	0.385	-0.1	0.266	0.261	1.1	0.352	0.381	-6.8
Sentencing points	35.780	35.373	1.6	27.703	28.023	-1.7	52.204	50.846	4.1
Matched tx/Total tx	6,611/7,008 (1.1% tx off support)			16,973/670 (1.4% tx off support)			6,670/2,276 (5.8% tx off support)		
B. First sanction = Intensive Probation	Probation (Tx) vs. Intensive Probation			Jail (Tx) vs. Intensive Probation			Prison (Tx) vs. Intensive Probation		
	P (Tx)	IP (Control)		Jail (Tx)	IP (Control)		Prison (Tx)	IP (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	28.520	29.465	-10.5	28.228	28.223	0.1	26.830	27.009	-2.0
White	0.532	0.539	-1.3	0.439	0.435	0.9	0.509	0.481	5.5
Black	0.366	0.372	-1.1	0.464	0.475	-2.2	0.391	0.400	-1.9
Latino	0.101	0.089	4.0	0.097	0.091	2.2	0.101	0.119	-6.1
Male	0.786	0.771	3.7	0.798	0.822	-5.8	0.862	0.861	0.3
Trial	0.007	0.005	3.2	0.003	0.002	1.5	0.010	0.008	2.0
Prior record points	6.910	6.393	5.9	6.821	6.632	2.1	9.589	9.711	-1.1
Offense severity	3.534	3.546	-0.7	3.426	3.467	-2.2	4.954	4.949	0.3
Additional charges	0.929	0.925	0.4	0.879	0.872	0.7	1.474	1.465	0.8
Victim injury	0.040	0.042	-1.0	0.029	0.015	7.1	0.085	0.074	3.8
Prior misdemeanors	1.348	1.366	-1.1	1.259	1.334	-5.1	1.254	1.392	-9.6
Prior violation	0.105	0.084	5.6	0.235	0.228	1.7	0.347	0.351	-0.7
Sentencing points	32.069	32.773	-2.3	31.653	31.786	-0.5	57.194	56.584	1.4
Matched tx/Total tx	1,510/1,609 (3.7% tx off support)			1,100/1,189 (3.9% tx off support)			875/1,188 (13.7% tx off support)		

Note: Caliper setting in panels A and C = .0001 and in panels B and D = .001.

Table 4.1. Covariate Comparisons after Propensity Score Matching (2nd-Time Felons) (cont.)

C. First sanction = Jail	Probation (Tx) vs. Jail			Intensive Probation (Tx) vs. Jail			Prison (Tx) vs. Jail		
	P (Tx)	Jail (Control)		IP (Tx)	Jail (Control)		Prison (Tx)	Jail (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	30.803	30.488	3.1	30.213	29.840	3.7	30.672	30.398	2.7
White	0.434	0.449	-2.9	0.423	0.428	-1.1	0.383	0.351	6.6
Black	0.475	0.464	2.3	0.502	0.477	4.9	0.555	0.585	-5.9
Latino	0.090	0.088	0.9	0.076	0.095	-7.3	0.062	0.064	-1.0
Male	0.813	0.800	3.3	0.836	0.812	6.6	0.860	0.839	5.9
Trial	0.006	0.004	2.3	0.004	0.007	-4.7	0.006	0.010	-3.2
Prior record points	5.068	5.193	-1.6	6.562	6.758	-2.0	9.046	7.556	12.0
Offense severity	3.326	3.352	-1.6	3.599	3.556	2.3	4.121	4.207	-4.9
Additional charges	0.721	0.742	-2.3	0.891	0.944	-5.2	0.807	0.891	-8.1
Victim injury	0.016	0.019	-1.8	0.019	0.026	-4.2	0.015	0.030	-6.5
Prior misdemeanors	1.286	1.250	2.2	1.477	1.433	2.6	1.342	1.291	3.1
Prior violation	0.014	0.020	-3.0	0.093	0.074	7.1	0.089	0.090	-0.5
Sentencing points	25.647	26.091	-2.3	31.148	30.066	3.6	37.292	36.073	4.2
Matched tx/Total tx	4,364/4,983 (4.5% tx off support)			568/673 (1.1% tx off support)			778/1,495 (7.0% tx off support)		
D. First sanction = Prison	Probation (Tx) vs. Prison			Intensive Probation (Tx) vs. Prison			Jail (Tx) vs. Prison		
	P (Tx)	Prison (Control)		IP (Tx)	Prison (Control)		Jail (Tx)	Prison (Control)	
	Mean	Mean	% bias	Mean	Mean	% bias	Mean	Mean	% bias
Age	29.270	28.814	5.2	28.846	29.108	-2.8	28.928	28.853	0.8
White	0.436	0.426	1.9	0.467	0.513	-9.2	0.326	0.350	-4.9
Black	0.465	0.469	-0.9	0.448	0.402	9.2	0.574	0.537	7.5
Latino	0.099	0.105	-1.7	0.085	0.085	0.0	0.099	0.113	-4.7
Male	0.889	0.872	5.8	0.879	0.876	1.1	0.919	0.913	2.1
Trial	0.008	0.009	-0.6	0.000	0.000	0.0	0.002	0.003	-0.8
Prior record points	16.176	16.762	-3.2	16.576	16.541	0.2	16.182	15.474	3.8
Offense severity	3.585	3.574	0.6	4.056	4.095	-2.0	3.371	3.356	0.9
Additional charges	0.842	0.815	2.5	1.147	1.144	0.3	0.617	0.588	2.8
Victim injury	0.044	0.055	-4.3	0.069	0.065	1.3	0.016	0.026	-5.0
Prior misdemeanors	1.475	1.405	4.1	1.444	1.467	-1.4	1.124	1.178	-3.5
Prior violation	0.052	0.057	-1.7	0.167	0.170	-0.9	0.075	0.071	1.2
Sentencing points	41.254	42.225	-2.7	50.142	52.753	-6.9	37.437	36.630	2.5
Matched tx/Total tx	1,357/1,444 (2.4% tx off support)			306/321 (0.6% tx off support)			1,069/1,183 (3.4% tx off support)		

Note: Caliper setting in panels A and C = .0001 and in panels B and D = .001.

Table 4.2. Post-PSM Logistic Regression of Recidivism on Progressively Tougher Sanctions, Propensity Score, and Judicial Circuit and Sentencing Year (2nd-Time Felons)

A. First sanction = Probation	Int. Probation (Tx) vs. Probation		Jail (Tx) vs. Probation		Prison (Tx) vs. Probation	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	-0.497***	0.031	0.149***	0.021	0.041	0.032
Propensity score	-1.584***	0.099	-0.526***	0.082	-0.964***	0.061
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-0.734***	0.086	-0.835***	0.083	-0.876***	0.089
n (treated and matched) =	36,672		47,034		36,731	
Log Pseudolikelihood =	-23960.69		-31633.43		-24333.96	
Pseudo R-squared =	0.038		0.023		0.031	
B. First sanction = Intensive Probation	Probation (Tx) vs. Int. Probation		Jail (Tx) vs. Intensive Probation		Prison (Tx) vs. Int. Probation	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	-1.040***	0.090	-0.748***	0.100	-0.844***	0.107
Propensity score	-0.078	0.319	-0.738*	0.314	-1.605***	0.261
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	0.354	0.419	0.048	0.604	1.300	0.775
n (treated and matched) =	2,604		2,194		1,969	
Log Pseudolikelihood =	-1671.88		-1408.95		-1232.29	
Pseudo R-squared =	0.074		0.062		0.092	

* p < .05, ** p < .01, *** p < .001

Note: To conserve space, coefficients for dummy variables for judicial circuit and sentencing year dummy variables not shown.

Sample sizes reflect the sum of treated and matched (with replacement) cases.

Table 4.2. Post-PSM Logistic Regression of Recidivism on Progressively Tougher Sanctions, Propensity Score, and Judicial Circuit and Sentencing Year (2nd-Time Felons) (cont.)

C. First sanction = Jail	Probation (Tx) vs. Jail		Intensive Probation (Tx) vs. Jail		Prison (Tx) vs. Jail	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	-0.764***	0.041	-0.801***	0.093	-0.449***	0.080
Propensity score	0.177	0.184	1.179**	0.427	0.050	0.169
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-0.698***	0.175	-0.895***	0.210	-0.798***	0.214
n (treated and matched) =	13,147		9,351		9,561	
Log Pseudolikelihood =	-8636.80		-6103.39		-6271.31	
Pseudo R-squared =	0.050		0.044		0.039	

D. First sanction = Prison	Probation (Tx) vs. Prison		Int. Probation (Tx) vs. Prison		Jail (Tx) vs. Prison	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Tx vs. control	-0.881***	0.084	-1.122***	0.140	-0.398***	0.093
Propensity score	-0.341	0.197	-0.835	0.660	-0.332	0.182
Judicial circuit dummies	—	—	—	—	—	—
Sentencing year dummies	—	—	—	—	—	—
Intercept	-0.590	0.364	-0.839	0.825	-0.436	0.577
n (treated and matched) =	3,546		2,439		3,258	
Log Pseudolikelihood =	-2223.69		-1520.55		-2115.56	
Pseudo R-squared =	0.093		0.075		0.039	

* p < .05, ** p < .01, *** p < .001

Note: To conserve space, coefficients for dummy variables for judicial circuit and sentencing year dummy variables not shown. Sample sizes reflect the sum of treated and matched (with replacement) cases.

Table 5. Post-matching (PSM) results for all matching combinations: Predicted probabilities, from logistic regression models, of recidivating (2nd-Time Felons)

	<i>Treatment (tx)</i>		<i>Control</i>		
	<i>% Recidivism</i>		<i>% Recidivism</i>		
First sanction = Probation					
Intensive probation (tx) vs. <i>Probation</i> (control)	IP	31.9	Prob	43.6	***
Jail (tx) vs. <i>Probation</i> (control)	Jail	47.4	Prob	43.7	***
Prison (tx) vs. <i>Probation</i> (control)	Prison	43.7	Prob	42.7	
First sanction = Intensive probation					
Probation (tx) vs. <i>Intensive probation</i> (control)	Prob	40.4	IP	65.7	***
Jail (tx) vs. <i>Intensive probation</i> (control)	Jail	47.9	IP	66.0	***
Prison (tx) vs. <i>Intensive probation</i> (control)	Prison	43.4	IP	64.1	***
First sanction = Jail					
Probation (tx) vs. <i>Jail</i> (control)	Prob	40.1	Jail	58.9	***
Intensive probation (tx) vs. <i>Jail</i> (control)	IP	39.1	Jail	58.9	***
Prison (tx) vs. <i>Jail</i> (control)	Prison	47.6	Jail	58.7	***
First sanction = Prison					
Probation (tx) vs. <i>Prison</i> (control)	Prob	40.0	Prison	61.6	***
Intensive probation (tx) vs. <i>Prison</i> (control)	IP	36.6	Prison	63.9	***
Jail (tx) vs. <i>Prison</i> (control)	Jail	53.5	Prison	63.1	***

* p < .05, ** p < .01, *** p < .001, for treatment coefficients in regression models predicting recidivism (table 4.2).

Note: Gray cells indicate evidence for deterrence.

Appendix A. Post-matching (exact) results for all matching combinations: Predicted probabilities from logistic regression models (1st-Time Felons)

	Total Tx Matched	% Tx Matched	Total Control Matched	% Control Matched	Treatment	Control	
Probation (tx) vs...							
<i>Intensive probation (control)</i>	213,671	51%	22,425	61%	17.7	17.7	
<i>Jail (control)</i>	259,211	62%	49,005	78%	18.1	26.7	***
<i>Prison (control)</i>	119,592	29%	12,188	40%	17.9	25.0	***
Intensive probation (tx) vs...							
<i>Probation (control)</i>	22,425	61%	213,671	51%	16.0	17.0	***
<i>Jail (control)</i>	14,458	39%	35,276	56%	18.4	29.2	***
<i>Prison (control)</i>	13,657	37%	10,685	35%	16.4	24.4	***
Jail (tx) vs...							
<i>Probation (control)</i>	49,005	78%	259,211	62%	27.9	19.5	***
<i>Intensive probation (control)</i>	35,276	56%	14,458	39%	28.2	19.3	***
<i>Prison (control)</i>	21,979	35%	6,688	22%	29.0	28.7	
Prison (tx) vs...							
<i>Probation (control)</i>	12,188	40%	119,592	29%	21.5	15.2	***
<i>Intensive probation (control)</i>	10,685	35%	13,657	37%	23.1	15.3	***
<i>Jail (control)</i>	6,688	22%	21,979	35%	26.9	29.8	***

* p < .05, ** p < .01, *** p < .001, for treatment coefficients in regression models predicting recidivism.

Note: Gray cells indicate evidence for deterrence.

Appendix B. Post-matching (exact) results from logistic regression models: Predicted probabilities from logistic regression models (2nd-Time Felons)

	Total Tx Matched	% Tx Matched	Total Control Matched	% Control Matched	Treatment	Control	
First sanction = Probation							
Int. probation (tx) vs. <i>Probation</i> (control)	2,701	39%	8,335	28%	31.2	42.2	***
Jail (tx) vs. <i>Probation</i> (control)	8,949	51%	14,267	48%	46.8	43.5	***
Prison (tx) vs. <i>Probation</i> (control)	2,121	24%	6,297	21%	43.2	39.9	*
First sanction = Intensive Probation†							
Probation (tx) vs. <i>Int. Probation</i> (control)	971	60%	653	60%	43.4	70.1	***
Jail (tx) vs. <i>Intensive Probation</i> (control)	712	60%	545	50%	48.7	70.4	***
Prison (tx) vs. <i>Intensive Probation</i> (control)	570	49%	564	52%	42.3	69.2	***
First sanction = Jail							
Probation (tx) vs. <i>Jail</i> (control)	2,519	51%	4,621	53%	39.9	56.7	***
Intensive probation (tx) vs. <i>Jail</i> (control)	247	37%	1,394	16%	41.4	56.1	***
Prison (tx) vs. <i>Jail</i> (control)	370	25%	1,685	19%	45.0	57.1	***
First sanction = Prison†							
Probation (tx) vs. <i>Prison</i> (control)	283	20%	329	15%	39.1	64.3	***
Int. probation (tx) vs. <i>Prison</i> (control)	200	62%	635	29%	38.2	59.0	***
Jail (tx) vs. <i>Prison</i> (control)	309	26%	302	14%	48.7	70.3	***

* p < .05, ** p < .01, *** p < .001, for treatment coefficients in regression models predicting recidivism.

† = Additional variable coarsening required on matching variables (age, prior record points, and total sentencing points).

Note: Gray cells indicate evidence for deterrence.