Do Harsher Prison Conditions Reduce Recidivism?
A Discontinuity-based Approach

M. Keith Chen*
Yale School of Management and Cowles Foundation

Jesse M. Shapiro
University of Chicago and NBER

April 16, 2007

Abstract

We estimate the causal effect of prison conditions on recidivism rates by exploiting a discontinuity in the assignment of federal prisoners to security levels. Inmates housed in higher security levels are no less likely to recidivate than those housed in minimum security; if anything, our estimates suggest that harsher prison conditions lead to more post-release crime. Though small sample sizes limit the precision of our estimates, we argue that our findings may have important implications for prison policy, and that our methodology is likely to be applicable beyond the particular context we study.

JEL classification: K42, Z13, J62

Keywords: recidivism, prison, regression discontinuity, deterrence

*e-mail: keith.chen@yale.edu, jmshapir@uchicago.edu. Earlier versions of this paper circulated under the title "Does Prison Harden Inmates? A Discontinuity-based Approach." We are extremely grateful for data and helpful conversations to Scott Camp, Gerry Gaes, Miles Harer, Neal Langan, Bo Saylor, and their colleagues at the Federal Bureau of Prisons, Office of Research and Evaluation. We thank Dan Benjamin, Judy Chevalier, John Donohue, Ray Fair, Matthew Gentzkow, Edward Glaeser, Claudia Goldin, Larry Katz, David Laibson, Steve Levitt, Ilyana Kuziemko, Emily Oster, Sharon Oster, Anne Piehl, Jim Ware and workshop participants at Harvard University, the University of Chicago, Ohio State University, the Yale School of Management, and Yale ISPS for feedback on earlier drafts. John Donohue, the editor, and two anonymous referees provided especially helpful comments.

1 Introduction

America's jails and prisons house roughly two million inmates (Bureau of Justice Statistics, 2002), nearly twice as many as in 1990 and more in per capita terms than any other OECD country (OECD, 2001). Current and former prisoners constitute an increasingly large share of the U.S. population, yet little is known about the effects that imprisonment and prison conditions have on the subsequent lives of inmates. This omission is unfortunate: each year roughly six-hundred thousand people are released from incarceration (Bureau of Justice Statistics, 2002), and roughly two-thirds of those released will be rearrested within three years (Langan and Levin, 2002). Because of this, crimes by former inmates alone account for a substantial share of current and future crime. Moreover, unlike many determinants of crime, prison conditions are already directly under the control of policymakers and the criminal justice system. Understanding whether confinement conditions affect post-release crime may therefore be essential to effective crime-control.[1]

Theory alone cannot tell us whether an increase in the severity of prison conditions will increase or decrease the propensity of inmates to commit crimes after release. Models of "specific deterrence" (Smith and Gartin, 1989), which posit that criminals learn from their own experiences about the severity of penalties, predict that harsher conditions will decrease the propensity to recidivate. Alternatively, if harsher prison conditions correspond to inferior labor market outcomes (Western, Kling, and Weiman, 2001), if prison life induces a taste for violence (Banister, Smith, Heskin and Bolston, 1973), or if encounters with peers while in prison can influence post-release crime (Glaeser, Sacerdote, and Scheinkman, 1996; Bayer, Pintoff, and Pozen, 2003), then harsher conditions may lead to more crime following release.

In this paper we exploit a feature of the federal inmate classification system to estimate the effect of moving a prisoner to a higher security level. Prior to incarceration, every federal inmate is assigned a score intended to reflect his need for supervision. An inmate is then assigned to a prison security level depending in part on where his score falls relative to certain predetermined cutoff values. By comparing inmates on either side of the boundaries between different security levels, we estimate the effect on recidivism of being assigned to a higher security level. Since both the physical and social conditions of confinement vary dramatically with security level, this setting provides a quasi-experiment for identifying the effect of prison conditions on post-release outcomes.

[1] For example, the literature on prison privatization has recently focused much of its attention on whether private prisons are likely to provide lower quality services than publicly managed prisons (Hart, Shleifer, and Vishny 1997; Camp and Gaes, 2001). If prison conditions affect rates of post-release crime commission, then providing quality-based incentives to private prison managers becomes an even higher priority.

Our approach avoids the obvious confounds inherent in simply comparing rearrest rates of prisoners in different security levels. Even with controls for demographics, such an estimation strategy would ignore the fact that prisoners are assigned to security levels based on characteristics such as crime severity that are themselves likely to predict recidivism. By taking careful account of the assignment mechanism, we can hope to avoid bias introduced by the endogeneity of security level.

We find no evidence that harsher confinement conditions reduce recidivism. If anything, our estimates suggest that moving an inmate over a cutoff that increases his assigned security level from minimum to above-minimum security tends to increase his likelihood of rearrest following release. Although small sample sizes mean that our estimates are not uniformly statistically significant, they are nevertheless difficult to reconcile with models of "specific deterrence," and seem more consistent with models of social interactions or psychological effects of incarceration.

We check our identifying assumptions by showing that discontinuities do not arise in a control population housed separately from other inmates. Though some predetermined correlates of recidivism do seem to change discretely around score cutoffs, our conclusions survive in a model that controls for a wide range of inmate characteristics.

This paper makes several contributions relative to the existing literature. Whereas most economic analyses of policy influences on crime focus on the deterrence or incentive effects of punishments (Levitt, 1998) or prison conditions (Katz, Levitt, and Shustorovich, 2003), we study how the conditions of incarceration influence *post-release* criminal behavior.[2] Our finding that harsher imprisonment conditions do not reduce recidivism stands in contrast to prior evidence of a specific deterrence effect (Sherman and Berk, 1984), in which punishing a criminal more severely reduces that individual's subsequent probability of recidivism.

Methodologically, the paper brings regression-discontinuity analysis (Campbell and Stanley, 1963; Rubin, 1977) to the study of post-release crime.[3] In the prior work most closely related to our own, Berk and De Leeuw (1999) use a regression-discontinuity design to evaluate the impact of confinement conditions on in-prison misconduct, but not on post-release criminal activity.[4] Moreover, although our research uses data from the federal corrections system, many state systems also

[2] Camp and Gaes (2005) study the effects of prison conditions on *in-prison* misconduct.

[3] In other contexts, economists have used regression discontinuity to estimate the effects of financial aid on college enrollment (van der Klaauw 2002), the effect of incumbency on election results (Lee, 2001), and the effects of class size on school performance (Hoxby, 2000).

[4] Berk and Rauma (1983) investigate the effects of transitional aid to prisoners on recidivism, exploiting a California policy which extends unemployment insurance to prisoners who work a certain number of hours prior to release.

employ scoring methods to assign inmates to confinement conditions. Our approach may therefore have wider applicability beyond the context we study.[5] In addition, though we focus primarily on the impact of prison conditions on post-release crime, a similar methodology could be employed to study effects on labor-market attachment, family structure, and other post-release outcomes, all of which might respond to the conditions of confinement.

Our paper also contributes to a growing economic literature on the importance of peer effects in general (Sacerdote, 2001), and on the role of peer effects in criminal behavior in particular (Glaeser, Sacerdote, and Scheinkman, 1996). Unlike Bayer, Pintoff, and Pozen (2003), we do not attempt to directly measure the effects of changes in peer group composition on prisoner outcomes. However, the composition of a prisoner's fellow inmates varies dramatically with the prisoner's security level, and is therefore likely to form part of the effect of security level on post-release recidivism.

Finally, our estimates suggest that the impact of prison conditions on recidivism could be an important factor in designing effective prison systems. As we discuss in section 4, the effect of harsher conditions on recidivism must be weighed against a broader deterrence effect in order to determine whether more or less harsh conditions are optimal from a crime-control perspective. However, our point estimates are relatively large compared to existing estimates of deterrence effects, suggesting the potential for significant gains from incorporating recidivism effects into policy analysis.

The remainder of the paper is organized as follows. Section 2 discusses the relationship between security level and conditions of confinement and describes the dataset. Section 3 presents our findings as well as some checks on the plausibility of our identifying assumptions. Section 4 discusses policy implications of our findings. Section 5 concludes.

2 Background and Data Description

2.1 Inmate Classification and Security Level

Upon entry to the federal prison system, an inmate is processed using an Inmate Load and Security Designation Form (see figure 1). The Security Designation Data recorded on this form are used to produce the individual's *security custody score*. The score is intended to predict prisoner misconduct

[5]Indeed, since the first version of our paper was circulated, at least two studies have used discontinuity-based designs to evaluate the effects of incarceration (Pintoff, 2005) and sentence length (Kuziemko, 2006) on recidivism. Lee and McCrary (2005) apply a discontinuity design to a study of the deterrent (as opposed to recidivism) effects of greater sentence length.

and therefore to measure the supervision needs of individuals. Over time, the score has been refined through continuing research into the predictors of in-prison misconduct (Harer and Langan, 2001).

In the score, each of seven items contributes points to an overall sum. For example, offenses are grouped into five categories, from lowest severity (such as "counterfeiting, under $2000") to greatest severity (such as homicide), and each inmate receives an associated offense severity score ranging from 0 (least severe) to 7 (most severe). The scoring is done by a Regional Designator at the Bureau of Prisons (BOP), and follows a procedure laid out in detail in the *Bureau of Prisons Security Designation and Custody Classification Manual* (Federal Bureau of Prisons, 1982). Important for our identifying assumption is that no aspect of the score relies on the Designator's personal judgment; all crimes, sentences, and judicial recommendations translate directly into a unique scoring. In the appendix we discuss in detail how the components of the score are determined, and appendix table 1 summarizes how those components sum to the overall score.

Once the score has been computed, it is compared to a set of cutoff values (see appendix table 2) to determine an inmate's security level.[6] Once a security level has been assigned to an inmate, a BOP employee assigns the inmate to an initial facility based primarily on location and on the availability of space.[7]

Some considerations may intervene to break the link between score and security level. For example, deportable aliens may not be housed in minimum security, nor can those who have been convicted of threats to government officials.[8] Issues of this type are recorded on the security designation form, and most have the effect of excluding an inmate from minimum security. Note, however, that our identification strategy does not use the variation in security level created by these exceptions. Rather, we identify the effects of security level using the discontinuities in the relationship between score and recidivism that occur at the cutoff values.

As first-hand accounts (such as Santos, 2006) make clear, an inmate's assigned security level has an enormous impact on his experiences in prison.[9] Table 1 compares self-reported conditions of confinement and in-prison misconduct across different security levels, using data from the Survey of Inmates of Federal Correctional Facilities (U.S. Department of Justice, 1991).[10] The data strongly

[6] In some cases security level can change during the incarceration period at the discretion of a BOP official, for example because of inmate misconduct. As such changes are endogenous, our analysis will focus (when possible) on security level upon entry to the federal prison system. See section 2.2 for details.

[7] Inmates who suffer from chronic medical conditions are also assigned scores, but are housed separately in a prison medical facility. We will use this subsample as a control group to check the plausibility of our identifying assumptions.

[8] Other such considerations include medical and mental health, aggressive sexual behavior, offense severity, organized crime, and gang membership.

[9] See Sykes (1958) and Conover (2001) for accounts of life in maximum security facilities.

[10] Camp (1999) has found that self-reports of this kind contain information helpful in making comparisons between

confirm the intuition that more secure facilities allow less contact with the community and less freedom of movement. While 14% of minimum security inmates report having been allowed furloughs during their current period of confinement, only 2.5% of low security inmates have had furloughs; for maximum security inmates the figure is below 1%. Similar trends show up in the percent of respondents who have been seriously injured during confinement. Moving from minimum to low security exposes an additional 2.7% to serious injury; moving from low to medium or medium to maximum increases the rate of injury by 1.2 and 1.8 percentage points, respectively. On the whole then, the available evidence strongly suggests that conditions of imprisonment differ dramatically by security level. Higher security prisons involve less contact with the outside world, allow less freedom, and subject inmates to far more violence.

2.2 Data on Inmate Classification and Recidivism

Our data are a representative sample of $1,205$ inmates released from federal prisons in the first six months of 1987 (Harer, 1994). Data on demographic characteristics and criminal histories were recorded for all inmates in the sample, as were the inmates' security custody scores and security levels on entry to the system, when available.[11]

Post-release criminal histories were obtained for all inmates for a three-year window following release, including both federal and state offenses. The database used to match inmates to histories takes advantage of automated criminal history systems, available for 21 states, supplemented by automated files maintained by the FBI. These automated databases produced criminal histories for all but 383 inmates; paper records were searched to identify histories for the remaining inmates. Although the data documentation reports that the follow-up records are complete, it is important to note that records may be more accurate for re-arrests in some states than in others, depending on the quality of the state's records in the late 1980s and early 1990s.[12]

Of the original sample of $1,205$ inmates, security level data are missing for 16, and 11 served short sentences in halfway houses that do not have a security designation. Another 216 were placed in administrative facilities for special medical needs; we will later use this sub-sample as a control

facilities.

[11] In many cases—usually inmates who entered the system prior to the introduction of modern computer records—data from the initial classification form was not available. In these cases score and security level were recorded from the earliest available reclassification form. The components of the score are unlikely to change significantly during confinement, so that data from these later records are likely to serve as a good proxy for the inmate's classification on entry to the prison system.

[12] Gaps in follow-up records could confound our analysis if higher-security facilities are located in jurisdictions with higher-quality arrest records. Comfortingly, a Pearson's χ^2 test shows no evidence of a relationship between an inmate's location relative to the score cutoff and his state of residence on release.

group in our analysis. Finally, 12 inmates have missing data on score and 1 has a miscoded rearrest date (with rearrest occurring prior to release), leaving a total of 949 inmates with usable data.

As is typical for administrative data on recidivism, our estimates measure the post-prison arrest rate, not necessarily the crime-commission rate. If the probability of arrest conditional on criminal behavior is unrelated to an inmate's conditions of confinement, this caveat will affect the scale, but not the validity, of our estimates. On the other hand, if an inmate's security level directly affected his rate of capture, say because of greater scrutiny by police, our results could provide misleading estimates of the effect of harsher conditions on post-release crime. While we cannot entirely rule out this explanation, we know of no federal parole policy that specifies a relationship between supervision intensity and the security level of an inmate's releasing facility.[13] Moreover, existing evidence suggests that even large differences in supervision intensity would not affect capture rates enough to change the sign of our estimates (Petersilia and Turner, 1993; see also Piehl and LoBuglio, forthcoming).[14]

3 Effects of Confinement Conditions on Recidivism

3.1 Identification Strategy

Table 2 presents a summary of the security level and recidivism patterns of the inmates in our sample, broken out according to the inmate's Security Custody Score. As the table makes clear, the score cutoff best covered by our data is that between scores 6 and 7. Inmates assigned a score of 6 or below are, by default, assigned to minimum security, whereas those assigned a score of 7, 8, or 9 are defaulted into low security facilities. Although (as we discuss in section 2.1 above) the prison system is not bound by these defaults, we would expect inmates with scores exceeding 6 to be more likely to be housed in above-minimum security facilities than inmates with scores of 6 or below.

The data in table 2 confirm this expectation: 48% of inmates with a score of 6 are housed in minimum-security facilities, compared with only 3% of inmates with a score of 7. Figure 2 shows

[13] Most state parole agencies use standardized risk assessment tools to map inmates into supervision levels (Jones et al, 1999). None of the instruments we examined take account of an inmate's former security level, nor look as if their cutoffs coincide with those in the security custody score. Furthermore, the variables these systems do take into account relate primarily to providing the appropriate services (drug users receive drug counselling) and limiting especially newsworthy crimes (convicted sex offenders are monitored very closely).

[14] As a final check on this point, we find that the differences in recidivism rates of inmates on either side of the score cutoffs we examine are only slightly smaller when we exclude from the sample inmates re-arrested for parole violations.

that the probability of assignment to an above-minimum-security facility jumps sharply as we pass the score cutoff.

The discrete change in the probability of being housed in above-minimum security depicted in figure 2 will be the key to our identification strategy. Provided that inmates close to the cutoff score are similar in all respects other than their conditions of confinement, comparing inmates just above and just below the cutoff will allow us to estimate the effect of being housed in above-minimum security on recidivism.

Formally, letting R_{it} be an indicator for whether inmate i has been rearrested t years after release, our goal will be to estimate a model of the form:

$$\Pr\left(R_{it}\right) = \beta_t above_i + g_t\left(score_i\right) \tag{1}$$

where $above_i$ is an indicator for whether the inmate was housed in above-minimum security, $score_i$ is the inmate's security custody score, and $g_t\left(\bullet\right)$ is a function relating the inmate's score to his probability of rearrest within t years. (For the time being, we assume that recidivism can be described by a linear probability model, an assumption that we relax in section 3.3 below.)

Because assignment to above-minimum security is not solely based on an inmate's security custody score, and may therefore respond to unmeasured characteristics of individuals, direct estimation of equation (1) is problematic.[15] We therefore take advantage of the use of cutoffs to assigned inmates to security levels, by instrumenting for an inmate's security level with a dummy for whether his score exceeds the cutoff. Formally, the first stage of the model we wish to estimate can be written as

$$\Pr\left(above_i\right) = \gamma cut_i + g^0\left(score_i\right) \tag{2}$$

where cut_i is a dummy for whether the inmate's score exceeds the cutoff of 6. (We focus for now on the cutoff at a score of 6, and incorporate the other cutoffs into our analysis in section 3.3 below.)

To use this approach to estimate the effect of being housed in an above-minimum security facility on recidivism, we need to know the effect of exceeding the score cutoff on both the likelihood of being housed in above-minimum security and the likelihood of rearrest. As equations (1) and (2) make clear, estimating the effect of exceeding the cutoff (and, hence, of security level) on recidivism requires an estimate of how recidivism would vary with the inmate's score if the security level itself had no impact on recidivism. More formally, it requires an estimate of the function $g_t\left(\bullet\right)$ (and

[15] As an exploratory exercise, we have estimated equation (1) directly (that is, not account for endogeneity in assignment to security level) for $t = 1, 2$, and 3. The point estimates are generally small, negative, and statistically significant. For $t = 3$, the point estimate is -0.08 and is marginally statistically significant.

the analogous function $g^0(\bullet)$) that governs the relationship between an inmate's score and his likelihood of rearrest, absent any effects of confinement conditions.

In the two subsections that follow, we will adopt two approaches to controlling for the relationship between score and recidivism. In section 3.2 below, we will compare the inmates on either side of the cutoff boundary, successively adopting tighter and tighter windows of comparison. This approach, in the spirit of border studies such as Holmes (1998) or Black (1999), amounts to assuming that the function $g_t(\bullet)$ is approximately flat in a neighborhood of the cutoff—that is, that close to the cutoff the score itself can be expected to have only a small effect on recidivism.

An alternative approach, which we develop in section 3.3 below, is to approximate the function $g_t(\bullet)$ using a polynomial. This regression-discontinuity approach (Campbell and Stanley, 1963; Rubin, 1977) assumes that, aside from the effect of confinement conditions, an inmate's score has a smooth relationship with his recidivism probability.

3.2 Difference Estimates Using Border Cases

In table 3, we compare the experiences of inmates on either side of the cutoff score of 6 in order to estimate the effect of being housed in above-minimum security on recidivism. In the first column of the table, we estimate the effect of exceeding the score cutoff on the likelihood of being housed in above-minimum security, the γ parameter in the first-stage model (2). When we compare inmates with scores of 7 through 9 to those with scores of 4 through 6, we find that, consistent with figure 2, inmates with scores above the cutoff are approximately 48 percentage points more likely to be housed in an above-minimum security facility than inmates with scores below the cutoff. This difference is highly statistically significant. As we tighten the band of comparison—moving first to inmates within two scores of the cutoff, and then to inmates just at the borders—this effect remains similar in magnitude and strongly statistically significant.

In the remaining three columns of table 3, we compare the proportion of inmates on either side of the cutoff who are rearrested after one, two, and three years following release. In each case, we also present the ratio of the effect of the cutoff on recidivism to its effect on security level. This ratio can be interpreted as an instrumental variables (IV) estimate of the effect of above-minimum security on recidivism, the β_t parameter in equation (1).

Comparing inmates within three scores of the border, we find that those above the cutoff are 10 to 13 percentage points more likely to be rearrested, depending on the follow-up window. These differences range from marginally statistically significant to statistically significant. Restricting attention to inmates within two scores of the border, these effects grow somewhat in magnitude and statistical significance. When we compare inmates just on the border—those with scores of 6 and 7—we find differences in the range of 10 to 15 percentage points. Though similar in magnitude to the estimates from wider comparison windows, smaller samples mean that these differences are not statistically significant.

Scaling these estimates by the effect of exceeding the score cutoff on the probability of being housed in above-minimum security produces IV estimates of the effect of above-minimum security on recidivism of about 21 to 42 percentage points, depending on the follow-up horizon and window of comparison. When we include inmates two or three scores from the border in our tests, the IV estimates are mostly marginally statistically significant; in a comparison of those just at the border they are statistically insignificant.

Comparisons of inmates on either side of the score cutoff show no evidence of a deterrent effect of harsher prison conditions on post-release crime. If anything, our point estimates are most consistent with the hypothesis that harsher confinement conditions lead to greater risk of recidivism, but inconsistent statistical significance means we cannot be definitive about this conclusion.[16]

Although focusing on inmates whose scores lie within a narrow band around the cutoff serves to minimize variation in unmeasured characteristics, it remains possible that inmates on opposite sides of the cutoff differ in their *ex ante* propensities to recidivate. If such differences were substantial, they could undermine our conclusions. One approach to testing for such a confound is to study a population of inmates whose scores are known but whose conditions of confinement are not determined by their scores. Inmates housed in "administrative" facilities, which are essentially prison hospitals, can serve this role, because they are housed apart from the general population and are therefore not exposed to the variation in conditions of confinement reflected in figure 2. Our dataset includes 216 inmates with known scores who were initially assigned to administrative facilities. Overall these inmates exhibit similar rates of recidivism to the general inmate population, and we find that similar demographic characteristics predict recidivism in both groups.

In panel A of table 4, we compare the recidivism rates of inmates on either side of the score cutoff who were housed in administrative facilities. If differences in the *ex ante* recidivism propensities of inmates with different scores were driving the results in table 3, we would expect them to show

[16] We focus here on effects on arrest rates, but our data also make it possible to examine the types of crimes into which inmates on either side of the score cutoffs recidivate. Relative to those below the cutoff, inmates with scores above 6 are more likely to be arrested for violent crimes (such as assault) and less likely to be arrested for "white-collar" crimes (such as forgery or fraud), conditional on being rearrested within three years. These differences are not statistically significant.

up here as well. By contrast, if the differences in table 3 were due to the effect of confinement conditions, such differences would not be present for the administrative group.

In fact, we find no evidence of differences in the recidivism rates of inmates on either side of the cutoff among this population. The observed differences are small, statistically insignificant, and inconsistently signed. Even the largest difference (of 6 percentage points), is less than half the comparable difference (of 15 percentage points) for inmates in the general population, and is of the opposite sign. The other differences, which do have the same sign as our results for the general population, are even smaller, at 3 and 4 percentage points, respectively. Small sample sizes mean that these results are imprecise and should be interpreted with caution; nevertheless, they provide some comfort that differences in criminal propensities of inmates with different scores may not be confounding our estimates.

In panel B of table 4, we turn to a different test of our identifying assumption, in which we compare the predetermined characteristics of inmates on either side of the score cutoff. If inmates on either side of the cutoff are comparable, we should see no significant differences in demographics between those with scores below and above the cutoff. Our findings are mixed. Of the 7 differences in demographic characteristics we examine, two are marginally statistically significant, and one is statistically significant. The others are statistically insignificant, but are consistent in sign, in the sense that inmates above the cutoff tend to have characteristics predictive of greater recidivism. Because this test does not rule out the possibility of a confound coming from variation in background characteristics, in the regression discontinuity analysis that follows we will control explicitly for inmates' characteristics.[17]

3.3 Regression Discontinuity Analysis

An alternative to the simple difference estimates of the previous subsection is to attempt to directly model the function $g_t(\bullet)$ that relates an inmate's score to his probability of recidivism. Because the parametric shape of that function is unknown, we will attempt to approximate it with a high-order polynomial.

Formally, we will estimate a probit model, in which the probability of recidivism depends on a polynomial in the inmate's score and dummies for whether the score exceeds the three cutoffs of 6, 9, and 13. To choose the order of the polynomial, we iteratively added higher-order terms

[17] Because the most serious lack of balance is on gender, we have also re-estimated our main specification excluding female inmates, and find similar results.

to the point at which the term of next highest order would no longer be statistically significant in determining rates of recidivism at any horizon. This approach suggests using a fourth-order polynomial in score.[18]

Figure 3 illustrates the fit of the polynomial model. The points on the plot show the share of inmates recidivating after one-year as a function of the score. The solid line shows the prediction of a probit model that includes a fourth-order polynomial and a dummy for whether the inmate's score exceeds the cutoff of 6. The dashed line shows the model's predictions when we counterfactually assume that no inmate is subject to the boost in recidivism attributed to exceeding the cutoff.

As the figure shows, the fitted polynomial is not monotonically increasing in score, and "expects" a small dip in recidivism in the vicinity of the score cutoff. Because the custody score is designed to predict in-prison misconduct and not post-release recidivism, and because higher-scoring inmates may recidivate into crimes with lower capture rates, it is not *a priori* obvious that the recidivism rate should be strictly increasing (or even monotonic) in score. Nevertheless, because we have so few points of support for estimating the polynomial, we cannot be certain of exactly what shape the function should take in the vicinity of the cutoff. If a flat, rather than decreasing, shape is more appropriate in this region, then the difference models of the previous subsection will provide more accurate estimates of the recidivism effects of confinement conditions.

With that caveat in mind, we turn in table 5 to a regression-discontinuity analysis of the effects of score cutoffs on recidivism. In the top half of the table, we show the reduced-form effects of the score cutoffs themselves; in the second half of the table we use an IV model to translate these estimates into the effect of being housed in above-minimum security. Because the analysis of the previous subsection suggests that some predetermined characteristics of inmates might vary over the score cutoff, we have included controls for a range of characteristics in all specifications.[19]

The first column of table 5 shows an OLS regression of a dummy for being housed in above-minimum security on score cutoffs (plus a polynomial in score and demographic controls). This specification can be thought of as an estimate of equation (2), and will serve as the first stage of our IV analysis. Consistent with prior expectations and our earlier analysis, we find that exceeding

[18] Adding a fifth-order term produces similar results. When we remove the fourth-order term, results are similar except at the one-year follow-up horizon, when we find smaller effects of the cutoff. A likelihood-ratio test of the goodness-of-fit of the polynomial model relative to a model with a complete set of score dummies (in the spirit of Lee and McCrary, 2005) fails to reject third- or higher-order polynomials at the five percent level.

[19] Specifically, we control for age, and dummies for high school graduation, prior convictions, married, white, male, and employed. These are the same variables we analyze in panel B of table 4. (We have also experimented with including a quadratic term in age. This term does not enter significantly and including it does not meaningfully affect our results.)

a score of 6 causes a significant increase in an inmate's probability of being housed in above-minimum security. The coefficient is highly statistically significant and large, though not as large as the difference estimates in table 3.

In columns (2) through (4) of table 5, we estimate probit models relating the probability of recidivism at various horizons to score cutoffs, with controls for a polynomial in score and predetermined inmate characteristics.[20] The point estimates imply that inmates whose scores exceed the cutoff of 6 are 14 to 21 percentage points more likely to recidivate following release. Two of these coefficients are marginally statistically significant.[21] The absence of consistently statistically significant point estimates prevents us from definitively concluding that harsher confinement conditions increase the likelihood of recidivism, but these estimates give us reasonable power to reject strong negative effects of the cutoffs on recidivism.

The second and third rows of the table report analogous estimates for the other cutoffs in our data, at scores of 9 and 13. (These coefficients can be interpreted as the marginal effect of each cutoff conditional on the value of the earlier cutoffs. For example, to estimate the effect of exceeding both a score of 6 and a score of 9, we would need to add the first two coefficients.) In general, we find no evidence of a negative effect of exceeding one of these cutoffs on recidivism, except for a (marginally statistically significant) negative effect of exceeding a score of 13 on the likelihood of recidivating within one year. In all other cases, the coefficients on these additional score cutoffs are positive and statistically insignificant, with standard errors that are too large to permit us to rule out positive effects on the order of those we find for the first cutoff.

In the second half of table 5, we use Newey's (1987) two-step procedure to obtain an IV estimate of the effect of above-minimum security on recidivism.[22] The model assumes a linear probability model for the likelihood of above-minimum security (as in column (1) of table 5) and a probit form for the probability of recidivism. In principle, the second and third cutoffs could be used to separate the effects of low, low/medium, and medium or higher security levels on recidivism; however, because our data include relatively few observations close to these cutoffs we choose to

[20] We have also estimated a Cox proportional hazard model of rearrest, which allows us to pool information from different follow-up horizons. We continue to find large (but not statistically significant) positive effects of score cutoff on recidivism probabilities.

[21] As Lee and Card (2006) have pointed out, regression discontinuity designs with discrete scores can suffer from specification error that induces score-level correlation in the error structure. This can cause an understatement of standard errors, which can sometimes be addressed by clustering at the score level. Experiments with clustering reveal, if anything, slight decreases in our standard errors, though we note that clustering may be unreliable in our context due to the small number of scores (Conley and Taber, 2005). Comfortingly, the difference estimates in table 3 tend to show similar levels of statistical significance to the regression discontinuity estimates we report in table 5.

[22] Two-stage least squares (2SLS) models produce very similar results. Estimating the IV probit model using maximum likelihood yields coefficients similar to those reported, but with smaller standard errors.

focus on the first score cutoff. We therefore treat a dummy for above-minimum security as the sole endogenous regressor, and instrument for it using a dummy for whether the inmate's score is greater than 6, leaving the other cutoffs in the model as controls.[23]

Consistent with the reduced-form findings, the IV estimates show no evidence that being housed in an above-minimum security facility decreases an inmate's likelihood of recidivating. If anything, the point estimates indicate positive effects of 36 to 52 percentage points on the probability of recidivism, with one coefficient marginally statistically significant. These point estimates are somewhat larger than those reported in table 3.

As a final interpretational point, we note that our sample is representative of the *released* population, not the incarcerated population. Although the released population is of greater interest for many policy questions, it is important to know whether the effects we identify are weaker for the average inmate than for the average released inmate. To check this, we have re-estimated our regression discontinuity models, weighting each observation by the inmate's total time in prison, which assigns more importance to inmates who had relatively low probabilities of entering our sample. In general, the estimated effect of the score cutoff is larger and statistically stronger in weighted than in unweighted models, indicating that if anything the effects of harsher conditions on recidivism may be larger (more positive) for inmates housed for a longer period.[24]

4 Policy Implications

Though not consistently statistically significant, our estimates suggest that harsher prison conditions may induce greater post-release recidivism among former federal inmates, an effect that would likely have important implications for prison policy. For several reasons however, such a finding by itself need not imply that prison conditions should be less harsh or restrictive. In this section, we discuss additional considerations that must be weighed against the parameters we estimate in determining the net effect on crime of harsher (or less harsh) confinement conditions.

The first consideration is that harsher prisons may deter crime among the non-incarcerated. Indeed, using the in-prison mortality rate as an index of a state's prison conditions, Katz, Levitt, and Shustorovich (2003; hereafter KLS) show that harsher prison conditions have a sizable contemporaneous deterrent effect. For a coarse comparison of the magnitude of their findings and our

[23] Models using all three cutoff dummies as instruments for housing in low, low/medium, and medium or higher security levels yield large but imprecisely estimated effects of higher security levels on recidivism.

[24] We have also checked whether the discontinuity we exploit appears to affect the time served by an inmate, and find no evidence for such an effect.

own, we note that according to the 1990 Census of State and Federal Adult Correctional Facilities (Bureau of Justice Statistics, 1990), federal facilities with a minimum-security custody designation had prisoner death rates of 0.66 per $1,000$ prisoners, whereas those with an above-minimum designation had death rates of 3.54 per $1,000$.

Scaling KLS's estimates by these differences in mortality rates implies that moving all inmates from minimum- to above-minimum-security facilities would decrease annual murders by 0.03 (per $100,000$ Americans), violent crimes by 18, and property crimes by 40, for a total reduction in the crime rate of about 58. These estimates, though, measure the contemporaneous effects of prison conditions on crime, and may therefore omit the effect of harsher confinement conditions on post-release recidivism.

Applying the same thought experiment, our RD estimates suggest that if all inmates were housed in above-minimum rather than minimum security facilities, they would be 41 percentage points more likely to be re-arrested in the year following release. (Since many crimes do not result in an arrest and individuals may be arrested multiple times, this almost surely undercounts the true increase in crime.) Given that the approximately $600,000$ inmates released annually account for roughly 200 of every $100,000$ Americans, our estimates predict an increase in the crimes committed by former convicts of approximately 82 per $100,000$ Americans. In other words, our point estimates are on the same order of magnitude as the deterrent effects estimated by KLS; indeed they appear large enough to outweigh deterrence and drive a net increase in crime should prison conditions worsen.

An important caveat to this conclusion is that KLS find that lagged changes in the in-prison death rate have negative effects on crime. If our estimated recidivism effect is indeed larger than the KLS deterrent effect, we would have expected the effect of prison conditions on crime to be positive when estimated with a long enough lag. One way to reconcile these findings is to suppose that the deterrent effect of a change in prison conditions becomes larger over time, because it takes time for information about the quality of prison life to diffuse through the population. Under this assumption, the true lagged deterrent effect of prison conditions is even larger than KLS's estimates suggest, but is partly counteracted by effects on recidivism.

An alternative way to reconcile our findings with those of KLS is to note that if the effects we estimate result largely from prisoners' effects on one another (say, through peer effects), then moving all inmates from minimum to above-minimum security might have no effect on aggregate recidivism rates. This is because the inmates in above-minimum security may experience a reduction in recidivism due to increased interactions with the less-hardened minimum-security inmates. (Note, however, that if peer effects are nonlinear, or if the magnitude of peer effects depends on individuals' *ex-ante* characteristics, then it may be possible to sort inmates so as to reduce overall levels of recidivism in the post-release population.)

A final issue regarding the consequences of changing prison conditions is that the security level of a facility is tailored to the inmate population in part to minimize the risk of in-prison misconduct, escape, and other undesirable outcomes. Indeed, Berk and de Leeuw (1999) show, using a discontinuity design, that inmates placed in higher security levels engage in less in-prison misconduct. Of course, these reductions in misconduct require the application of greater resources (such as staff). Nevertheless, if harsher conditions reduce in-prison violence and other misconduct, these effects must be made a part of any complete analysis of policy regarding confinement conditions.

5 Conclusion

With over two million inmates currently incarcerated and six hundred thousand inmates released per year, the demographic impact of American prisons can hardly be overstated. In this paper we have attempted to understand the impact of incarceration on inmates' subsequent criminal behavior.

By exploiting discontinuities in the assignment of inmates to different security levels, we attempt to isolate the causal impact of prison conditions on recidivism. Our findings suggest that harsher prison conditions do not reduce post-release criminal behavior, and may even increase it.

Turning to policy questions, while our estimates are imprecise, they are large in magnitude and appear larger than benchmark estimates of deterrence effects. Our results also highlight the potential importance of research aimed at determining which aspects of incarceration increase or reduce recidivism. A richer understanding of the ways inmates respond to both harsher prison conditions and exposure to more violent peers would likely allow policymakers to suppress socially costly recidivism by adjusting conditions and redesigning assignment systems, both between and within prisons. Additionally, because many prison systems utilize score cutoffs for inmate placement, our work highlights an empirical methodology with potentially wide scope and policy relevance.

A Appendix: Constructing the Security Custody Score

Here, we detail the process by which a prisoner is assigned a security custody score by the bureau of prisons. Upon entry to the federal prison system, an inmate is processed using an Inmate Load and Security Designation Form (see Figure 1). Seven separate items are evaluated by a regional designator for each inmate. Each item is governed by a procedure found in the Bureau of Prisons Security Designation and Custody Classification Manual (Federal Bureau of Prisons, 1982). Discussing each item in the order in which it is addressed on the Designation Form:

A.1 Type of Detainer

This category refers to the severity of charges for which the inmate has not yet been tried and sentenced. A pending charge under a state statute would fall under this category, for example. The severity of the worst such charge is ranked from 0 to 7 according to the severity of offense scale (discussed below), and this number becomes the inmate's type of detainer score, with the exception that 0 means no pending charges, and a score of 1 indicates a pending charge with a severity score of either 0 or 1.

A.2 Severity of Current Offense

All offenses are classified according to the Bureau of Prisons Severity of Offense Scale, which exhaustively partitions the penal code into 5 categories: 0 (lowest), 1 (low/moderate), 3 (moderate), 5 (high), and 7 (greatest). The severity of current offense score for an inmate is the severity of the *most severe documented behavior* associated with the crime for which the individual is currently serving a period of incarceration. For example, if an individual was involved in an armed robbery of a bank (which scores a 7), but plead down at trial to simple robbery (which scores a 5), he would score a 7.

A.3 Expected Length of Incarceration

To determine this value the regional designator first looks up the reference (standard) sentence length in months for the inmate, based only on the offense for which the inmate is serving time. These are found in the *Expected Length of Incarceration Scale* in the Sentencing Handbook. The minimum of this number and the months to which the inmate was *actually* sentenced is compared to a set of cutoffs, with 0-12 months receiving 0 points, 13-59 receiving 1, 60-83 receiving 3, and 84 or more months receiving 5 points.

A.4 Type of Prior Commitments

If an inmate has never been incarcerated before he receives a 0. Otherwise, the most severe offense he has been incarcerated for (as evaluated by the severity of current offense scale) is used. An inmate receives 1 point if his most serious prior offense is classified as either low or low-moderate. Any more serious offense leads to a score of 3.

A.5 History of Escape Attempts

This measure classifies the escape history of the individual. The history includes a individual's entire background of escapes or attempts to escape from confinement, excluding the current offense. This includes documented flight to escape prosecution, and if multiple escape attempts were made the most severe is used. The severity of the escape attempt is classified as either minor or serious. A minor attempt must have been from an open institution (work camp, work release, furlough, flight to avoid prosecution) and must not have involved a threat of violence. All other attempts are considered serious. As the security designation form details, this severity and the time elapsed since the attempt, combine to form this score component.

A.6 History of Violence

This classifies the violent acts history of the individual. This history comprises a individual's entire background of violent acts, excluding his current offense. Violent acts enter the history even if noted by a prison discipline committee but never prosecuted. If an inmate has multiple such acts, the most severe is used. The severity of each act is classified as either minor or serious. A minor act is a simple assault, fight, or domestic squabble. Aggravated assault or worse, arson, or any act involving a weapon, or explosives is considered serious. As the security designation form details, this severity and the time elapsed since the act combine to form this score component.

A.7 Pre-Commitment Status

An inmate scores 0 if prior to incarceration he was not out on his own recognizance and/or did not voluntarily surrender. He scores -3 if he was released on his own recognizance during his trial without posting bail to ensure appearance, but was incarcerated post-trial. An inmate scores -6 if he meets the previous criteria and surrendered voluntarily to confinement, i.e. was not escorted by a law official to the place of his confinement.

Appendix Table 1 *Computing the Security Custody Score*

Inmate characteristic	Score Range	
	From	To
Type of detainer (severity of outstanding charges)	0 (None)	7 (Greatest)
Severity of current offense	0 (Lowest)	7 (Greatest)
Expected length of incarceration	0 (0-12 Months)	5 (84+ Months)
Type of prior commitments	0 (None)	3 (Serious)
History of escapes or attempts	0 (None)	7 (Recent Escape)
History of violence	0 (None)	7 (Recent Serious)
Precommitment status (bail, bond, etc. set in trial)	-6 (Voluntary Surrender)	0 (None)
TOTAL	0	36

Appendix Table 2 *Determining the Appropriate Security Level*

Score Range	Assigned Security Level	Description	Example
0-6	1	Minimum	Danbury Camp
7-9	2	Low	La Tuna
10-13	3	Low/Medium	Otisville
14-22	4	Medium	Petersburg
23-29	5	High	Leavenworth
30-36	6	High	Marion

Source: Federal Bureau of Prisons (1985).

References

[1] P. A. Banister, F.V. Smith, K. J. Heskin, and N. Bolton. Psychological correlates of long-term imprisonment. *British Journal of Criminology*, 13(4):312–330, October 1973.

[2] Patrick Bayer, Randi Pintoff, and David E. Pozen. Building criminal capital behind bars: Social learning in juvenile corrections. *Yale University Mimeo*, October 2003.

[3] Richard A. Berk and J. de Leeuw. Evaluation of California's inmate classification system using a generalized discontinuity design. *Journal of the American Statistical Association*, 94(448):1045–1052, 1999.

[4] Richard A. Berk and David Rauma. Capitalizing on nonrandom assignment to treatments: A regression-discontinuity evaluation of a crime control program. *Journal of the American Statistical Association*, 78(381):21–27, March 1983.

[5] Sandra E. Black. Do better schools matter? parental valuation of elementary education. *Quarterly Journal of Economics*, pages 577–599, May 1999.

[6] Scott D. Camp. Do inmate survey data reflect prison conditions? using surveys to assess prison conditions of confinement. *The Prison Journal*, 79(2):250–268, June 1999.

[7] Scott D. Camp and Gerald G. Gaes. Private prisons: What do we really know and why don't we know more? In David Schichor and Michael Gilbert, editors, *Privatization of Criminal Justice: Past, Present and Future*, pages 283–298. Anderson, Cincinnati, OH, 2001.

[8] Scott D. Camp and Gerald G. Gaes. Criminogenic effects of the prison environment on inmate behavior: Some experimental evidence. *Crime and Delinquency*, 51(3), July 2005.

[9] D. T. Campbell and J. C. Stanley. *Experimental and quasi-experimental designs for research.* Rand McNally, Chicago, 1963.

[10] Timothy Conley and Christopher Taber. Inference with "difference in differences" with a small number of policy changes. *National Bureau of Economic Research Technical Working Paper*, 312, June 2005.

[11] Ted Conover. *Newjack: Guarding Sing Sing.* Vintage Books, New York, NY, 2001.

[12] Organization for Economic Co-operation and Development. *Society at a Glance: OECD Social Indicators.* OECD, Paris, 2001.

[13] Edward L. Glaeser, Bruce Sacerdote, and José A. Scheinkman. Crime and social interactions. *Quarterly Journal of Economics*, 111(2):507–548, May 1996.

[14] Miles D. Harer. *Recidivism among Federal Prisoners Released in 1987.* Federal Bureau of Prisons, Office of Research and Evaluation, Washington, DC, 1994.

[15] Miles D. Harer and Neal P. Langan. Gender differences in predictors of prison violent: Assessing the predictive validity of a risk classification system. *Crime and Delinquency*, 47(4):513–536, October 2001.

[16] Oliver Hart, Andrei Shleifer, and Robert Vishny. The proper scope of government: Theory and an application to prisons. *Quarterly Journal of Economics*, 112(4):1127–1161, November 1997.

[17] Thomas J. Holmes. The effect of state policies on the location of manufacturing: Evidence from state borders. *Journal of Political Economy*, 106(4):667–705, 1998.

[18] Caroline M. Hoxby. The effects of class size on student achievement: New evidence from population variation. *Quarterly Journal of Economics*, 115(4), 2000.

[19] Dana A. Jones, Shelley Johnson, Edward Latessa, and Lawrence F. Travis. Case classification in community corrections: Preliminary findings from a national survey. *Topics in community corrections*, 1999.

[20] Lawrence Katz, Stephen D. Levitt, and Ellen Shustorovich. Prison conditions, capital punishment, and deterrence. *American Law and Economic Review*, 5(2):318–343, 2003.

[21] Ilyana Kuziemko. Discretion versus rules in parole decisions: An empirical investigation. *Harvard University Mimeograph*, September 2006.

[22] Patrick A. Langan and David J. Levin. Recidivism of prisoners released in 1994. *Bureau of Justice Statistics Special Report*, June 2002.

[23] David S. Lee. The electoral advantage to incumbency and voters' valuation of politicians' experience: A regression discontinuity analysis of elections to the u.s. house. *National Bureau of Economic Research Working Paper*, 8441, 2001.

[24] David S. Lee and David Card. Regression discontinuity inference with specification error. *UC Berkeley Mimeograph*, February 2006.

[25] David S. Lee and Justin McCrary. Crime, punishment, and myopia. *National Bureau of Economic Research Working Paper*, 11491, July 2005.

[26] Stephen D. Levitt. Juvenile crime and punishment. *Journal of Political Economy*, 106(6):1156–1185, December 1998.

[27] Whitney K. Newey. Efficient estimation of limited dependent variable models with endogenous explanatory variables. *Journal of Econometrics*, 36(3):231–250, November 1987.

[28] Federal Bureau of Prisons. *Security Designation and Custody Classification Manual*. U.S. Department of Justice, Washington, D.C., October 1982.

[29] Federal Bureau of Prisons. *Security Designation and Custody Classification Manul*. U.S. Department of Justice, Washington, D.C., 1985.

[30] Joan Petersilia and Susan Turner. Intensive probation and parole. *Crime and Justice*, 17:281–335, 1993.

[31] Anne Morrison Piehl and Stefan F. LoBuglio. Does supervision matter? In Jeremy Travis and Christy Visher, editors, *Prisoner reentry and public safety in America*. Cambridge University Press, Cambridge, UK, 2005. forthcoming.

[32] Randi Pintoff. Juvenile jails: A path to the straight and narrow or hardened criminality? *University of Maryland Mimeograph*, November 2005.

[33] Donald B. Rubin. Assignment to treatment group on the basis of a covariate. *Journal of Educational Statistics*, 2:34–58, 1977.

[34] Bruce Sacerdote. Crime and social interactions. *Quarterly Journal of Economics*, 116(2):681–704, May 2001.

[35] Michael G. Santos. *Inside: Life Behind Bars in America*. St. Martin's Press, New York, 2006.

[36] Lawrence W. Sherman and Richard A. Berk. The specific deterrent effects of arrest for domestic assault. *American Sociological Review*, 49(2):261–272, April 1984.

[37] Douglas A. Smith and Patrick R. Gartin. Specifying specific deterrence: The influence of arrest on future criminal activity. *American Sociological Review*, 54(1):94–106, February 1989.

[38] Gresham M. Sykes. *The Society of Captives: A Study of A Maximum Security Prison*. Princeton University Press, Princeton, NJ, 1958.

[39] Bureau of Justice Statistics U.S. Department of Justice. *Census of State and Federal Adult Correctional Facilities*. Inter-University Consortium for Political and Social Research, Ann Arbor, MI, 1990.

[40] Bureau of Justice Statistics U.S. Department of Justice. Key facts at a glance. http://www.ojp.usdoj.gov/bjs/keytabs.htm, 2002.

[41] Bureau of Prisons U.S. Department of Justice. *Survey of Inmates of Federal Correctional Facilities, 1991*. Inter-University Consortium for Political and Social Research, Ann Arbor, MI, 1994.

[42] Wilbert van der Klauuw. Estimating the effect of financial aid offers on college enrollment: A regression-discontinuity approach. *International Economic Review*, 43(4), November 2002.

[43] Bruce Western, Jeffrey R. Kling, and David F. Weiman. The labor market consequences of incarceration. *Crime and Delinquency*, 47(3):410–427, July 2001.

Table 1 *Security Level and Prison Conditions*

Percent of Inmates	Security Level			
	Minimum	Low	Medium	Maximum
Receiving a furlough	14.20%	2.49%	1.60%	0.78%
In cell for > 8 hours per day	49.01	55.21	55.03	58.22
Seriously injured	16.54	19.21	20.45	22.19
Found guilty of prison rule violation for:				
Possession of drugs	0.45	2.02	3.59	15.78
Possession of alcohol	0.11	0.47	2.63	9.53
Possession of a weapon	0.00	0.12	0.99	7.66
Assaulting an inmate	1.07	3.32	5.05	9.38
Assaulting a correction officer	0.00	0.36	1.04	5.94
Number of observations	1782	843	2315	640

Source: Authors' calculations based on U.S. Department of Justice (1991).
Notes: In all cases, a Pearson χ^2 test rejects the null hypothesis of equal proportions across security levels at the one percent level.

Table 2 *Detailed Data Summary*

Score	Number of inmates	Percent of inmates in security level: Min.	Low	Low/Med	Medium	High	Percent rearrested within (years): One	Two	Three
Assigned security level based on score: Minimum									
0	411	78.35	6.33	2.43	4.87	8.03	4.62	9.98	17.27
1	46	63.04	17.39	6.52	8.70	4.35	17.39	28.26	41.30
2	45	77.78	17.78	0.00	4.44	0.00	26.67	40.00	51.11
3	55	63.64	25.45	1.82	5.45	3.64	20.00	30.91	34.55
4	79	58.23	21.52	10.13	5.06	5.06	24.05	34.18	44.30
5	47	57.45	27.66	0.00	10.64	4.26	17.02	31.91	44.68
6	44	47.73	36.36	6.82	4.55	4.55	22.73	40.91	52.27
Assigned security level based on score: Low									
7	32	3.13	56.25	25.00	9.38	6.25	34.38	56.25	62.50
8	20	10.00	65.00	25.00	0.00	0.00	35.00	55.00	65.00
9	33	9.09	63.64	18.18	6.06	3.03	27.27	36.36	48.48
Assigned security level based on score: Low/Medium									
10	26	3.85	26.92	53.85	15.38	0.00	34.62	61.54	69.23
11	17	11.76	5.88	70.59	5.88	5.88	23.53	23.53	52.94
12	31	3.23	3.23	61.29	29.03	3.23	29.03	45.16	58.06
13	11	0.00	18.18	18.18	54.55	9.09	36.36	45.45	72.73
Assigned security level based on score: Medium									
14+	52	0.00	0.00	11.53	61.54	26.92	30.77	61.54	73.08
ALL	949	55.32	17.39	10.22	10.12	6.85	16.44	27.50	36.99

Table 3 *Differences in Recidivism Among Border Cases*

Score range	N	Share in above-minimum security level	Share rearrested within		
			One year	Two years	Three years
4-6	170	0.4471	0.2176	0.3529	0.4647
7-9	85	0.9294	0.3176	0.4824	0.5765
Difference	255	*0.4824******	*0.1000**	*0.1294****	*0.1118**
IV Estimate	255	—	*0.2073**	*0.2683**	*0.2317*
5-6	91	0.4725	0.1978	0.3626	0.4835
7-8	52	0.9423	0.3462	0.5577	0.6346
Difference	143	*0.4698******	*0.1484****	*0.1951****	*0.1511**
IV Estimate	143	—	*0.3158**	*0.4152****	*0.3216**
6	44	0.5227	0.2273	0.4091	0.5227
7	32	0.9688	0.3438	0.5625	0.6250
Difference	76	*0.4460******	*0.1165*	*0.1534*	*0.1023*
IV Estimate	76	—	*0.2611*	*0.3439*	*0.2293*

Notes: Significance of differences evaluated using two-sample tests of differences in proportions. Significance of instrumental variables (IV) estimates evaluated using two-stage least squares.

* indicates statistically significant at the 10% level

** indicates statistically significant at the 5% level

*** indicates statistically significant at the 1% level

Table 4 *Falsification Tests of Identifying Assumptions*

Panel A: Estimates using administrative sample

Score range	N	Share rearrested within One year	Two years	Three years
5-6	29	0.2069	0.4483	0.5172
7-8	27	0.1481	0.4815	0.5556
Difference	56	*-0.0587*	*0.0332*	*0.0383*

Panel B: Using predetermined characteristics as dependent variables

Score range	N	Age	H.S. graduate	Prior convictions	Married	White	Male	Employed
5-6	91	36.34	0.4396	0.8022	0.3187	0.6154	0.8901	0.3516
7-8	52	35.77	0.2885	0.8846	0.1731	0.5577	1.0000	0.2692
Difference	*143*	*-0.5714*	*-0.1511**	*0.0824*	*-0.1456**	*-0.0577*	*0.1099***	*-0.0824*

Notes: Significance of differences evaluated using two-sample tests of differences in proportions for all characteristics except age. Significance of differences in mean age evaluated using two-sample t-test with unequal variances.

* indicates statistically significant at the 10% level

** indicates statistically significant at the 5% level

*** indicates statistically significant at the 1% level

Table 5 *Regression-Discontinuity Estimates of the Effect of Score Cutoffs on Rearrest*

	(1)	(2)	(3)	(4)
Dependent variable	Above-minimum security	Probability of rearrest within One year	Two years	Three years
Model	OLS	Probit	Probit	Probit
Score>6	0.3607	0.1577	0.2087	0.1373
	(0.0927)	(0.0925)	(0.1104)	(0.1188)
Score>9	-0.0045	0.0151	0.1208	0.1320
	(0.1023)	(0.0725)	(0.1176)	(0.1373)
Score>13	0.0668	-0.0862	0.2283	0.0267
	(0.1547)	(0.0452)	(0.1956)	(0.1937)
Security custody score	0.0418	0.1011	0.1202	0.0991
	(0.0340)	(0.0255)	(0.0356)	(0.0436)
Score^2	0.0023	-0.0239	-0.0245	-0.0181
	(0.0103)	(0.0074)	(0.0106)	(0.0135)
Score^3/100	-0.0420	0.1767	0.1473	0.0967
	(0.0874)	(0.0615)	(0.0885)	(0.1196)
Score^4/10000	0.1202	-0.4034	-0.2890	-0.1433
	(0.2249)	(0.1552)	(0.2261)	(0.3236)
Model	—	IV Probit	IV Probit	IV Probit
Above-minimum security	—	0.4103	0.5195	0.3602
	—	(0.2527)	(0.2653)	(0.3114)
Observations	949	949	949	949

Notes: Coefficients of probit and IV probit models reflect marginal effects evaluated at the mean of the independent variables. IV probit models estimated using Newey's (1987) two-step procedure, using the dummy for having a score over 6 as an instrument for being housed in an above-minimum security facility. All models include controls for inmate age, and dummies for high school graduation, prior convictions, married, white, male, and employed, all at time of intake to prison system.

Figure 1 *Inmate Load and Security Designation Form*

U.S. Department of Justice
Federal Bureau of Prisons

Inmate Load and Security Designation

Page 1
5100.2 CN-8
August 1, 1985

INMATE LOAD DATA

		1. REGISTER NO.	
2. LAST NAME	3. FIRST	4. MIDDLE	5. SUFFIX
6. RACE / 7. ETHNIC ORIGIN	8. SEX	9. DATE OF BIRTH	
10. OFFN/CHRG/SENT			
11. FBI NUMBER	12. HEIGHT FT IN	13. WEIGHT	
14. SOC. SEC. NO.	15. HAIR	16. EYES	
17. STATE OF BIRTH / 18. OR COUNTRY OF BIRTH	19. CITIZENSHIP		
20. ADDRESS – STREET			
21. ADDRESS – CITY			
22. ADDRESS – STATE	23. ZIP CODE	24. OR FOREIGN COUNTRY	
25. REMARKS			

SECURITY DESIGNATION DATA

IMPORTANT: Enter all CIM assignments in SENTRY before entering this portion of form.

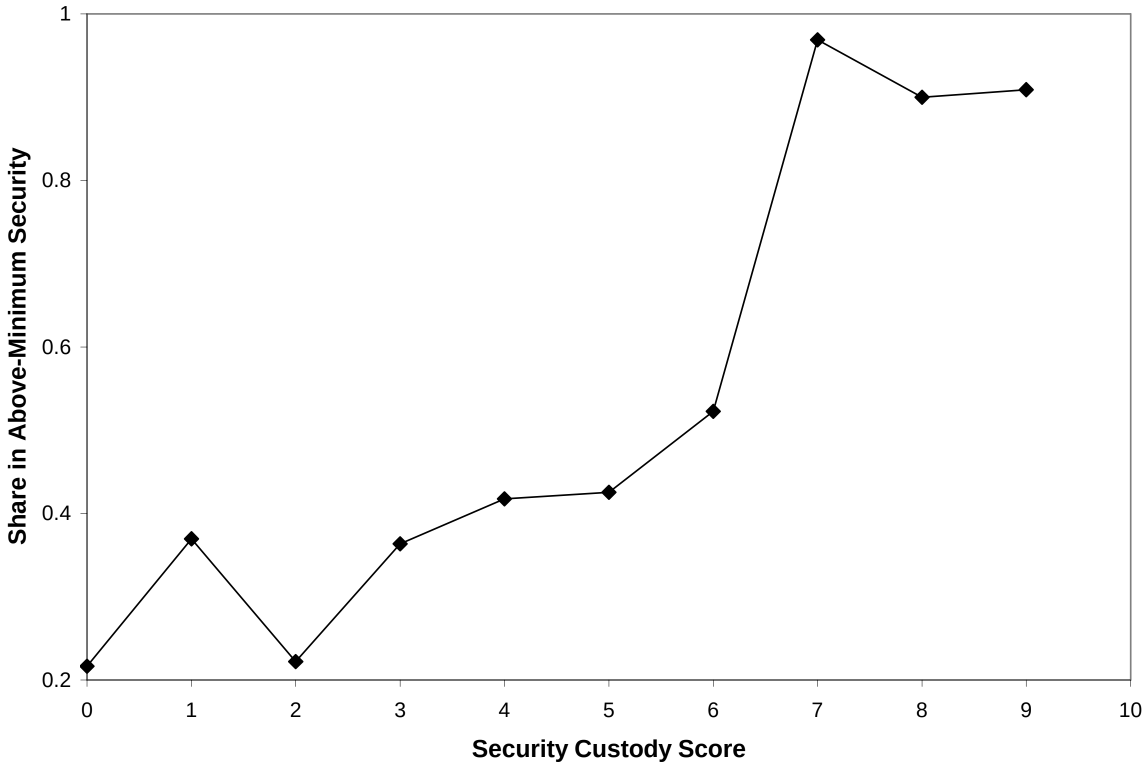
1. DESIGNATION LIMITATIONS — 0 – NONE; 1 – MISDEMEANOR; 2 – NARA; 3 – YCA; 4 – STUDY; 5 – SPLIT; 6 – PSYCH; 7 – MEDICAL
2. ADDITIONAL CONSIDERATIONS — 0 – NONE; 1 – MEDICAL HEALTH; 2 – MENTAL HEALTH; 3 – AGGRESS SEX BEHAVIOR; 4 – DEPORTABLE ALIEN
3. USM OFFICE — 4. JUDGE
5. RECOMMENDED FACILITY — 6. RECOMMENDED PROGRAM
7. TYPE OF DETAINER — 0 – NONE; 1 – LOWEST/LOW MODERATE; 3 – MODERATE; 5 – HIGH; 7 – GREATEST
8. SEVERITY OF CURRENT OFFENSE — 0 – LOWEST; 1 – LOW MODERATE; 3 – MODERATE; 5 – HIGH; 7 – GREATEST
9. EXPECTED LENGTH OF INCARCERATION — 0 – 0-12 MONTHS; 1 – 13-59 MONTHS; 3 – 60-83 MONTHS; 5 – 84 PLUS MONTHS; MONTHS
10. TYPE OF PRIOR COMMITMENTS — 0 – NONE; 1 – MINOR; 3 – SERIOUS

		NONE	>15 YRS	10-15 YRS	5-10 YRS	<5 YRS
11. HISTORY OF ESCAPES OR ATTEMPTS	MINOR	0	1	1	2	3
12. HISTORY OF VIOLENCE	SERIOUS	0	4	5	6	7

13. PRE-COMMITMENT STATUS — 0 – NOT APPLICABLE; 3 – OWN RECOGNIZANCE; 6 – VOLUNTARY SURRENDER
14. VOLUNTARY SURRENDER DATE (MM-DD-YYYY) — 15. VOLUNTARY SURRENDER LOCATION
16. IF ELIGIBLE FOR SL-1, IS THERE ANY MEDICAL REASON THAT WOULD PRECLUDE DESIGNATING A CAMP? — Y – YES; N – NO
17. REMARKS

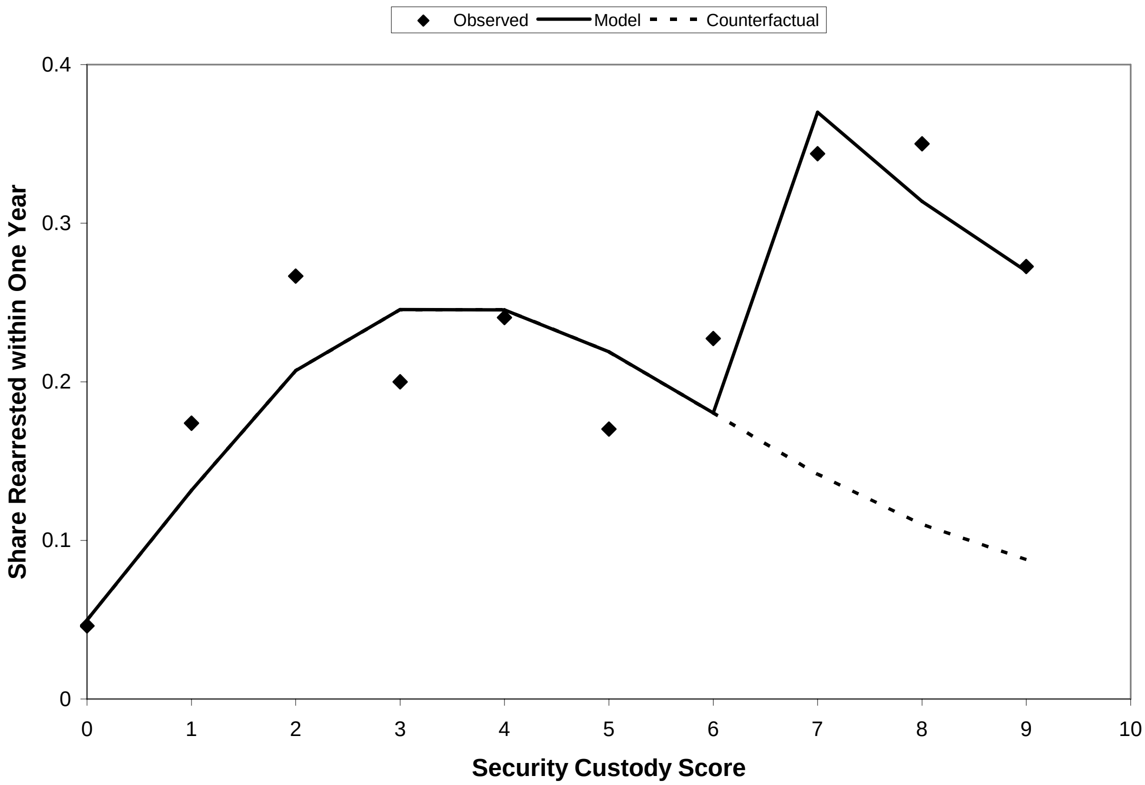
BP-14 (Manual)
March 1985

Figure 2 *Security Custody Score and Inmate Security Level*



Notes: Vertical axis measures share of inmates housed in an above-minimum security facility.

Figure 3 *Security Custody Score and Rearrest Rates*



Notes: Vertical axis measures share rearrested in one year. Points reflect empirically observed recidivism frequencies. Solid line reflects prediction of regression-discontinuity model (see section 3.3 for details). Dashed line reflects prediction of regression-discontinuity model with no discontinuity in recidivism between scores 6 and 7.