HOW SHOULD INMATES BE RELEASED FROM PRISON? AN ASSESSMENT OF PAROLE VERSUS FIXED-SENTENCE REGIMES*

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Abstract

Over the past thirty years, many states have abolished parole boards, which traditionally have had the discretion to release inmates before the expiration of their full sentence, in favor of fixed-sentence regimes in which the original sentence is binding. However, if prison time lowers recidivism risk and if parole boards can accurately estimate inmates' recidivism risk, then, relative to a fixed-sentence regime, parole can provide allocative-efficiency benefits (costly prison space is allocated to the highest-risk offenders) and incentive benefits (prisoners know they must reduce their recidivism risk to gain an early release, so invest in their own rehabilitation). Exploiting quasi-experiments from the state of Georgia, I show that prison time reduces recidivism risk and that parole boards set prison time in an allocatively efficient manner. Prisoners respond to these incentives; after a reform that eliminated parole for certain offenders, they accumulated a greater number of disciplinary infractions, completed fewer prison rehabilitative programs, and recidivated at higher rates than inmates unaffected by the reform. I estimate that eliminating parole for all prisoners would increase the prison population by ten percent while also increasing the crime rate through deleterious effects on recidivism.

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I. Introduction

The U.S. criminal justice system currently incarcerates over two million people and each year releases over 700,000. Each of these releases entails a trade-off between two significant social costs. On the one hand, keeping individuals in prison is very costly—the operating costs of the nation's prisons are nearly $60 billion annually.[1] On the other hand, the costs associated with overly lenient releases are also high given the vastly disproportionate share of crime committed by the newly released—for example, past work suggests that individuals released over the previous twelve months account for fourteen percent of all murders.[2]

Release policies thus have large consequences in terms of government expense and public safety, and these consequences are likely growing. After decades of unprecedented growth in the incarceration rate, states are attempting to shrink their prison populations in response to budget pressure, and in 2009 prison releases exceeded admissions for the first time since at least 1980.[3] Moreover, despite the well-documented decrease in the overall crime rate over the past two decades, recidivism rates among ex-inmates have remained stubbornly high. The increasing number of releases coupled with high recidivism rates means that ex-inmates are determining a growing share of the overall crime rate (Rosenfeld et al. 2005).[4]

Release policies that allocate costly prison space to inmates identified as posing the greatest recidivism risk or that incentivize prisoners to invest in reducing their future recidivism could improve public safety while potentially reducing criminal justice expenditures. Despite these potentially important welfare consequences, economists have paid limited attention to release policy, in contrast to their considerable work examining what deters criminal activity in the first place, even though the questions involved in designing an optimal release policy—how to allocate scarce resources, extract information from noisy signals, and incentivize human capital investments—are ones to which economists have traditionally made important contributions.[5]

The lack of attention paid to release policy is especially surprising because it has undergone sweeping changes over the past several decades, moving from a system in which parole boards had great discretionary power to one in which an inmate's original sentence is more binding. From the late nineteenth century until recently, a judge typically would assign a convicted offender an

[1]States are projected to have spent $51 billion on corrections in 2010—note that this number generally does not include the cost of local jails, which are run by cities and counties (Natonal Association of State Budget Officers 2010). The federal government spent $7.5 billion on prisons in 2010 (`http://www.justice.gov/jmd/2010factsheets/pdf/prisons-detention.pdf`).

[2]See Raphael and Stoll (2004), who also find that these individuals account for seven percent of all robberies. Bureau of Justice Statistics (2002) finds similar results.

[3]Given estimates of the growth of the prison population, admissions almost surely exceeded releases in the years before 1980, but the Bureau of Justice Statistics only began systematic collection of these data in 1980.

[4]See Petersilia (2012) on recidivism rate levels and trends.

[5]Researchers from other fields have focused on parole policy and prison release. See Petersilia (2003), Abadinsky (2003), and Western (2006).

indeterminate sentence (e.g., "ten years to life") and a parole board would have the discretion to eventually decide when, within that range, the prisoner would be released. However, parole boards began to fall out of favor in the 1970s—conservatives thought they were too lenient while liberals felt they were corrupt and discriminatory. While in 1980, state and federal parole boards still had the discretion to release the large majority of prisoners before the expiration of their full sentence, today less than one-quarter of prison inmates are released via a parole board's discretion.[6]

In this study I provide a simple framework for analyzing how different release policies balance the costs of incarceration versus future recidivism risk, and then use this framework to compare traditional parole and a fixed-sentence regime where the original sentence is binding. In the framework, there are two ways that the choice of release policy can affect total incarceration and recidivism costs. First, policies can be more or less allocatively efficient—roughly speaking, an allocatively efficient policy would release a prisoner at the point in time when the marginal cost of his incarceration equals the marginal expected cost of his recidivism risk, as depicted in Figure I.A. A key assumption of the framework—and one that I verify empirically—is that, all else equal, recidivism risk falls with time in prison, and thus allocative efficiency requires that lower-risk inmates leave prison sooner, as depicted in Figure I.B, given that higher-risk inmates require more time before their expected recidivism risk falls to the level of their incarceration cost. If parole boards can accurately estimate recidivism risk, then with respect to allocative efficiency, they would be preferred to a fixed-sentence regime because they can adjust prison time so as to assign costly prison space to those inmates with the greatest risk. Conversely, if parole boards' assessments are negatively correlated with actual expected risk, then a fixed-sentence regime is preferred.

While allocative efficiency is concerned with finding the point in time when the incarceration cost curve and recidivism cost curve meet, the second way that release policy can affect total costs is by shifting the recidivism curve itself. If parole boards assign longer prison terms to inmates with greater recidivism risk, then, relative to a regime where they have no hope of an early release, inmates will have an incentive to invest in their own rehabilitation while in prison so as to reduce their recidivism risk and thus gain an early release. As such, the recidivism cost curve should shift inward. By contrast, if parole board merely flip coins to make their decisions or release higher-risk inmates before lower-risk ones, then incentives would be non-existent or even perverse.

The empirical work examines the key assumptions and predictions of this simple framework, using individual-level data on all inmates incarcerated in Georgia state prisons over the past several decades. I benefit from a number of natural experiments in the state as well as discontinuities in

[6]In 2009, the most recent year of data, 729,295 individuals were released from prisons (Bureau of Justice Statistics 2010a), of which 146,696 were via the discretion of a parole board (Bureau of Justice Statistics 2010b). It is important to note that the demise of parole *boards* does not imply a demise in the role of parole *officers*. Inmates released under a fixed-sentence regime are almost all assigned to mandatory post-prison supervision and thus report to parole officers in much the same way as in traditional parole regimes (Petersilia 1999). See Petersilia (1999) for the opposition to parole among conservatives and Cullen (2004) for opposition among liberals.

its criminal justice process that create quasi-experimental variation along the key dimensions in the framework. Georgia has also experimented with limiting parole board discretion for certain offenders, allowing me to examine both a traditional parole model as well as a fixed-sentence model within the same state.

I begin by exploring whether time in prison actually reduces recidivism risk upon release, a basic assumption of the framework. To address the typical problem of the endogeneity of time served, I take advantage of sharp discontinuities in Georgia's parole-board guidelines between 1995 and 2006 and estimate that an extra month in prison reduces the probability that an inmate returns to prison within three years of his release by 1.3 percentage points.

An allocatively efficient parole board should assign longer prison terms to those with greater recidivism risk, so an obvious factor in evaluating parole boards versus fixed-sentence regimes is how well boards perform this task. But in trying to estimate the relationship between recidivism and recommended time served, mechanical endogeneity arises—because the parole board's recommended time served typically *equals* an inmate's actual time served, one cannot separately identify the relationship between recidivism and the parole board's recommended time served and also control for actual time served. However, a mass prison release in 1981 led Georgia to release hundreds of inmates before the parole board's recommended date of release, allowing me to estimate the relationship between recidivism and recommended time served while controlling for actual time served. I show that recommended time served is positively associated with recidivism—consistent with parole boards acting in an allocatively efficient manner—while actual time served is negatively associated with recidivism—consistent with the regression-discontinuity results above.

If parole boards indeed set release dates so that higher-risk inmates serve longer terms than do lower-risk inmates, then inmates should have an incentive to invest in lowering their recidivism risk so as to gain an early release. The final empirical application explores whether inmates indeed respond to such incentives. I analyze a 1998 policy reform in Georgia that required inmates convicted of certain crimes to serve at least ninety percent of their original sentence, effectively eliminating the possibility of an early parole. As predicted, inmates in this group experienced substantial increases in their recidivism rates after the reform, relative to inmates not subject to the policy. Moreover, their disciplinary infractions increased while their completion of rehabilitative prison programs fell, further evidence they invested less in their own rehabilitation after the reform.

This study does not try to perform a complete welfare analysis of the discretion-versus-rules decision, which would require making strong assumptions about the social cost of crime and the value of inmates' freedom. Nonetheless, the findings suggest that at least along some metrics, the move away from discretion appears quite costly. Assuming that other offenders would react similarly when their incentives to rehabilitate are diminished, I estimate that had Georgia's "ninety-percent" policy been instead applied to all crimes, it would have increased the state's prison population by roughly ten percent, while also increasing crime through its deleterious effects on the recidivism rate. This policy simulation is not an extreme hypothetical scenario—sixteen states and the federal justice system have completely eliminated parole for all crimes, not even allowing the residual discretion above ninety percent that the Georgia policy retained. And many more states have abolished parole for large groups of offenses or individuals. However, as I discuss later, it appears states are beginning to question their "get tough" attitude toward release policy, making research in this area relevant to policy-makers.

The paper is organized as follows. Section II presents a framework for analyzing how different release policies affect the costs associated with incarceration and future recidivism risk, and then uses it to compare parole versus fixed-sentence regimes. Section III describes the Georgia data. Section IV estimates the effect of prison time on recidivism rates. Section V examines whether parole boards assign longer prison terms to inmates with higher recidivism risk. Section VI explores whether inmates under parole authority react to these incentives by investing more in reducing their recidivism risk so as to obtain an early release than do prisoners subject to fixed-sentence regimes. Section VII offers directions for future work and concluding thoughts.

II. A simple framework for analyzing release decisions

This section presents a very basic, illustrative framework for analyzing prison release decisions. For the most part, the intuitions can be gained through variations on a simple graph, though a more formal treatment appears in the Online Appendix. Even the more formal treatment makes important simplifying assumptions—which I briefly revisit at the end of this section—and is thus better viewed as a way of organizing and motivating the empirical work than a complete model encompassing all aspects of the release decision.

II.A. The basic framework

My framework focuses on two central costs—the social cost of an individual's incarceration and the social cost related to his release, which I assume is mostly captured by his recidivism risk. Consider an individual who has committed an offense and is admitted to prison. Let the flow cost of his incarceration C be roughly constant across time and individuals.[7] The daily food, shelter, and supervision the state provides while he is in prison account for part of C, but any other net cost or benefit of incarceration that does not depend on the specific individual being punished can also be included. For example, if prison time generally deters crime, then such an ex-ante deterrence effect would lower C and suggest longer optimal prison terms.[8]

[7] While the daily cost of incarcerating an individual likely varies both across individuals (e.g., older inmates have higher health care costs) and within individuals (e.g., newly-arrived inmates might have greater disciplinary issues), I assume that this variation is small relative to its mean.

[8] As demonstrated in the Online Appendix, the main conclusion of the framework—that release regimes should assign longer prison terms to those with higher recidivism risk—holds regardless of ones view of ex-ante general

Let $r_i(t)$ be the flow social cost inmate i would impose if he were to be free at time t since his prison admission. I will typically assume this social cost is driven by recidivism risk and thus refer to $r_i(t)$ as such, but, in principle, it could encompass broader factors and even be negative, if through paying taxes or helping relatives he makes a net social contribution when free. I assume that, at least for sufficiently large t, $r'(t) < 0$. Figure I.A illustrates the key assumptions regarding C and $r(t)$.[9]

The assumption that costs do not vary with time I largely take as given, whereas much of the empirical work is dedicated to supporting the assumption that recidivism risk falls with time t since admission. There are several reasons that one might assume *a priori* that expected risk falls with time. First, criminal activity may be in part triggered by a heightened state of emotion or a difficult period in an individual's life, so recidivism risk might naturally fall as the person "cools off" or as their circumstances revert to their normal state. Second, criminal propensity is well known to fall with age (see, e.g., Hirschi and Gottfredson 1983). Note that if instead prison time generally *increased* recidivism risk, optimal punishment schemes would look very different from either their typical observed form or the form they take in this framework: to a first approximation, individuals below a certain risk threshold at the time of their sentencing would receive no prison time and those above it would be sentenced to life.[10]

Assuming that an individuals $r_i(t)$ and C curves are fixed, a release regime will minimize the sum total of incarceration and recidivism costs by releasing him at the point in time when his $r_i(t)$ curve meets the C curve, as in Figure I.A. At this point, the marginal cost of incarceration C is equal to its marginal benefit, the prevented recidivism, $r_i(t)$. Figure I.B shows the result more clearly. If individual i is released at $t = \bar{s}$ instead of at the optimal s_i^*, then he is free for the period $t \in (\bar{s}, s_i^*)$, but the social cost of his being free during this period is greater than the social cost of his being in prison. Similarly, keeping the lower-risk inmate j in prison until $t = \bar{s}$ involves incarcerating him during $t \in (s_j^*, \bar{s})$ when his recidivism risk falls *below* his incarceration cost. Figure I.B also suggests that the dead-weight loss of assigning every inmate $\bar{s}$ instead of his optimal s_i^* will increase with the variance of recidivism risk—the area of the dead-weight loss triangles grow the larger the range of initial recidivism risk levels. I show this result more formally in the Online Appendix.

II.B. Empirical implications

Figure I.B shows that if parole boards are setting their recommendations $\hat{s}$ in a roughly optimal manner, then, holding t constant, there should be a positive relationship between recommended

deterrence. While general deterrence affects the optimal average level of time served, it does not change the manner in which it is allocated.

[9] This basic set-up is similar in spirit to Shavell (1987).

[10] To see this, note that if a prisoner's risk at the moment of sentencing were higher than the incarceration cost, then it will be higher still after a day in prison, and higher still the next day, suggesting it would never be optimal to release him. Of course, this simple example abstracts from other considerations such as general deterrence.

time served $\hat{s}$ and recidivism risk $r(t)$—simply put, parole boards should assign more time to higher-risk inmates. But this prediction is difficult to test because recidivism risk cannot be observed at any given t and instead can only be observed when an individual is actually released. And in almost all cases, actual time served s_i is *equal* to recommended time served $\hat{s}_i$, so that when recidivism is observed at $t = s_i = \hat{s}_i$ it is impossible to separately identify the relationship of recommended time served and recidivism risk (which, holding actual time served constant, should be positive) and to also control for the effect of time served (which, holding recommended time served constant, should be negative).

Figure I.C illustrated the spirit of the identification strategy used in Section V to circumvent this problem. Assume instead that actual time served s_i can be set in a manner orthogonal to the parole board's recommended term $\hat{s}_i$. Without loss of generality, assume that it is set at some $\tilde{s}$ for two inmates i and j, as in Figure I.C.[11] At $t = \tilde{s}$, i has a higher recidivism rate than does j, and as both are released at this time, this difference $r_i(\tilde{s}) - r_j(\tilde{s})$ can be observed by the researcher. Thus, if the parole board indeed assigned a longer recommendation to higher-risk inmate i than to lower-risk inmate j, then both $\hat{s}_i > \hat{s}_j$ and $r_i(\tilde{s}) > r_j(\tilde{s})$ and thus observed recidivism risk and the recommended time served are positively correlated.

In fact, as shown in the Online Appendix, if one is willing to assume that recidivism is a roughly linear function of time served, then the framework yields a more specific prediction. If actual time served s_i is randomly assigned and a release regime is setting recommended time served $\hat{s}_i$ optimally, then $r_i(s_i) = C - \beta s_i + \beta \hat{s}_i$. That is, a regression of recidivism should yield a negative coefficient on actual time served s (simply the treatment effect of time served on recidivism) and a positive coefficient *of equal magnitude* on the recommended time served $\hat{s}$. Essentially, such a result would show that the release regime is exactly offsetting differences in initial recidivism risk with longer recommended prison terms. It is important to emphasize that the linearity assumption suggests one should not take this prediction too literally, but it is a useful benchmark when considering the later empirical results.

II.C. Incentive effects

So far, I have assumed that each individuals' recidivism and incarceration cost curves are fixed, and thus the only goal of a release regime is to find the specific point in time at which the curves meet for each inmate. However, the choice of the release regime might itself make inmates take actions that shift these curves. If inmates believe that parole boards positively condition time served on recidivism risk, then, assuming they prefer an earlier release to a later one, they will have a greater incentive to lower their actual recidivism risk so as to gain an early release relative to a

[11] As shown in the Online Appendix, the two inmates need not be released at the same time, but assuming they do makes the figure less cluttered.

regime in which sentences are fixed upon admission.

Such an effect is illustrated in Figure 1(d). While time in prison is assumed to lower recidivism risk *holding constant the choice of release regime*, the incentives created by the release regime itself can shift the negative recidivism-time-served relationship. As depicted in the figure, even though time in prison lowers recidivism under both regimes, a shorter prison term in a regime that creates incentives for rehabilitation could lead to lower recidivism upon release than a longer term in a regime without such incentives. The hope of early parole would seem to create such incentives, an empirical question taken up in Section VI.

II.D. Revisiting assumptions

Before proceeding to the empirical work, I briefly discuss some of the key simplifying assumptions made in the framework (additional assumptions are discussed in the Online Appendix). First, the framework considers only two types of release regimes—a traditional, discretionary parole regime and a fixed-sentence regime—as these are the two I observe in my data and most states have some form of these two policies. However, there are other ways of varying the initial sentence that do not involve discretion. For example, time served could be reduced by a certain amount for good behavior, or for completing drug treatment programs or GED courses.[12] As such, the framework and empirical results can speak at most to a policy of discretionary parole versus a particular rules-based regime—a fixed-sentence policy—but not to the discretion-verus-rules debate more generally.

Second, the figures have implicitly assumed that the relationship between recidivism risk and elapsed time does not change once an individual leaves prison. Put differently, an individual's recidivism begins to fall at some rate and I assume that process continues in a similar manner after he leaves prison. If instead the rate at which an inmate's recidivism rate falls with time is greater inside prison than out—perhaps due to access to vocational and drug treatment programs—then parole boards will want to release an inmate slightly *after* his recidivism risk hits the C threshold, as this enhanced rehabilitation effect lowers the social cost of incarceration and thus increases the optimal incarceration spell. Conversely, if prison retards rehabilitation, then the optimal spell is shorter. Because little if any research speaks to whether *time in prison* increases or decreases future criminal activity *relative to the mere passage of time*, I make the simplifying assumption that any difference is small.[13]

[12] Such a formulaic approach is generally referred to as "good" or "earned time." Georgia prisoners do not have good or earned time. In those states that do have good/earned time, the maximum sentence reductions possible are typically small relative to the ability the parole board has traditionally enjoyed to reduce sentences (Reitz 2012).

[13] The likely reason no research speaks to this question is that it is difficult to imagine a credible empirical strategy. Assume there are two identical individuals, and randomly assign individual i to a one-year prison sentence and set j free. What one would like to do is to compare their criminality in one year, after j has spent a year free and i a year in prison. However, j may not be observable at this point, as he may have committed a crime and been sentenced to prison over the course of the year. And comparing i only to those j's who are still observable (i.e., not in prison)

Third, I have generally taken the size of the original prison population as given and have thus assumed the decision between parole and a fixed-sentence regime has no independent effect on general deterrence. The little direct empirical work on this question finds no consistent effects.[14] On the one hand, a risk-averse individual would dislike the uncertainty of parole, so holding average punishment constant parole would have a greater deterrent effect than a fixed-sentence regime. On the other hand, if potential criminals are overly confident in their ability to manipulate the parole board, then a fixed-sentence regime is a more effective deterrent.

As these limitations demonstrate, the framework is not a full model of the release decision, but it is still useful in directing and motivating the empirical work. First, it highlights that a non-trivial assumption—that recidivism risk falls with time since prison admission—needs to be established empirically before moving forward to explore predictions of the framework. Second, it highlights the type of exogenous variation—actual time served randomly deviating from recommended time served—required to test whether parole boards indeed positively condition time served on recidivism risk. Finally, it also illustrates the possibility that this very conditioning itself shifts the relationship between recidivism risk and time served by creating an incentive for inmates to invest in their own rehabilitation.

III. Data

The empirical work relies on administrative records from the Georgia Department of Corrections (GDC), which cover every inmate who has served time in a Georgia state prison over the past several decades. Inmates in the Georgia data appear to be representative of the national prison population in many key ways. In 1997 (roughly the mid-point of the sample used in much of the analysis) offenders convicted of violent crimes composed 47.3 percent (48.5 percent), and offenders convicted of drug crimes composed 20.6 percent (18.6 percent), of the national (Georgia) prison population. Both populations in that year had the same average age at admission (31 years), although Georgia inmates released in 1997 served an average of 24 months—three months fewer than the national average.[15]

Recidivism is a key outcome of the study and I will typically define it as an inmate returning to prison within three years of his release, consistent with past literature.[16] Because my data terminate

after one year introduces obvious selection bias.

[14]Marvell and Moody (1996) find no deterrence effect of truth-in-sentencing laws using state panel data from the early 1970s to the early 1990s. Shepherd (2002) uses county panel data from 1984-1996 and finds that truth-in-sentencing laws have a large deterrence effect on violent crime. I find the Marvel and Moody study more convincing largely because of their inclusion of state-specific trends.

[15]National prison statistics are published annually by the U.S. Bureau of Justice Statistics. The numbers are based on summary statistics from the GDC data (without the sampling restrictions described later in this section, to be parallel to the published national numbers) and U.S. Bureau of Justice Statistics (1998).

[16]According to the Bureau of Justice Statistics, "recidivism is measured by criminal acts that resulted in the rearrest, reconviction, or return to prison with or without a new sentence during a three-year period following the

in Spring 2011, I generally sample individuals released before 2008 so that all observations have a well-defined three-year recidivism measure. To be consistent with past literature, I mostly use this binary measure of recidivism, though I will also show results that weight this binary measure by the seriousness of the crime to which a recidivating individual recidivates.[17]

I generally limit the sample to those individuals who are (1) convicted of a crime as an adult by a Georgia criminal court (that is, "new commitments," as opposed to parole or probation violators), (2) sentenced to between seven months and ten years, and (3) already released. The first condition is commonly used in the literature, and makes the sample match the assumptions of the framework in Section II, where the parole board evaluates an individual after he is sentenced by the court. I impose the second condition—which excludes less than ten percent of the sample— because the sources of variation that I am able to exploit operate mostly in this sentence range and I want to avoid creating comparison groups with much longer or shorter sentences.[18] The third condition is necessary to observe recidivism, though when I examine behavior while in prison I relax this condition.

The first column of Table I provides summary statistics from the resulting sample. The average inmate served about 32 months in prison, had an original sentence between four and five years and had been incarcerated about 0.8 times before his current spell in prison. While this exact sample is never used in any regression, it is the baseline sample from which almost all of the empirical applications draw. The next section begins the empirical analysis, examining the effect of the length of an incarceration spell on the probability an inmate returns to prison after his release.

IV. The effect of prison time on recidivism risk

One of the main assumptions of the framework in Section II is that the probability of recidivism, all else equal, falls with time in prison. Testing this assumption requires overcoming the endogeneity of time served. As the framework in Section II demonstrated, a criminal justice system that seeks

prisoner's release," and while the three year window is arbitrary, I generally will use it to remain consistent with the literature. Given that my data records admissions and releases from prison, as opposed to arrests that do not result in re-admission, I use the "return to prison" version of recidivism.

[17]For any individual who did not recidivate, the value of this weighted measure and the binary recidivism measure are both zero. For those that do recidivate, the value is equal to the average prison sentence for the crime to which they recidivate. For individuals who return to prison on parole violations and not new crimes, the value is equal to his remaining sentence. Evidence suggests that individuals who return to prison on a "technical" parole violation have in fact committed a new crime and law enforcement official have merely deemed a violation an easier route to guarantee the prisoner returns to incarceration. Austin and Lawson (1998) find that even in California, considered the strictest state in terms of "violating" recently released parolees, over eighty percent of inmates returned on technical violations in fact had an underlying criminal charge. For this reason, I believe that the "returned to prison" definition is the preferable measure of recidivism, though all the results in this paper hold when re-conviction for a new charge is instead the outcome variable.

[18]Another reason to exclude individuals with very short sentences is that individuals assigned sentences of less than a year typically serve their time in jail and not prison, so the few observations with sentences of six months or fewer would be highly unusual cases.

to be allocatively efficient will tend to assign longer prison terms to those with greater recidivism risk. Thus, it is likely that in a regression with recidivism as the outcome, the selection effect of longer terms is positive, leading an OLS estimate to be a positively biased measure of the treatment effect of time served.

Recent work has attempted to address the endogeneity problem using natural experiments, providing quasi-experimental estimates of the effect of incarceration spells on recidivism and other outcomes. In a much-imitated research design, Kling (2006) uses random assignment to judges with different sentencing propensities and finds no adverse affects of longer incarceration spells on future labor market outcomes in data from California and Florida. Other researchers have relied on discontinuities in sentencing or other punishment guidelines, a technique I employ later in this section. Hjalmarsson (2009), using discontinuities in Washington State's juvenile sentencing guidelines, finds that relative to a fine or probation, incarceration reduces juveniles' future recidivism propensity.

Finally, like I do in Section V, researchers have exploited variation arising from unexpected releases from prison. Many papers have made use of various mass clemencies in the Italian prison system. Drago et al. (2009) uses a 2006 Italian clemency and finds that an early release from prison combined with a greater expected sentence conditional on recidivism lowers future recidivism, consistent with predictions from general deterrence theory. Maurin and Ouss (2009) find that individuals released early due to a mass clemency in France on Bastille Day of 1996 recidivated at higher rates, suggesting that longer incarceration spells lower recidivism risk.

To the best of my knowledge, however, few papers have provided quasi-experimental estimates of the effect of incarceration length on recidivism for a large sample from the general adult U.S. state prison population, a group that in fact constitutes a large share of the world's incarcerated population. The rest of this section focuses on that task, exploiting discontinuities in Georgia's parole guidelines.

IV.A. Georgia parole guidelines

When an inmate arrives in a Georgia state prison, he typically receives a point designation from one to twenty based on pre-determined characteristics such as age and past record. As Appendix Table I shows, the point designation, along with the inmate's conviction charge, can be used to generate a recommended prison term. This matrix with rows based on the seriousness of the conviction charge and columns based on points is generally referred to by GDC officials as "the grid," and I adopt their terminology. In principle, the grid does not infringe on the parole board's discretion, as they are free to adjust its recommendation up or down, but, as I show below, in practice the grid has a considerable effect on the board's final decision.

The grid described by Appendix Table I was generally in effect between 1993 and 2008. However, the assignment of charges to severity levels changes substantially after 1994 and additional point levels were added in 2006.[19] I therefore sample individuals admitted to prison after 1994 and released before 2006. Because the parole board appears to follow the grid more closely for less serious crimes, I sample individuals convicted of offenses in the first four severity categories, which account for 88 percent of inmates.[20]

The grid does not use the exact point level but instead assigns inmates to high- (1 to 8 points), medium- (9 to 13 points) and low-risk (14 to 20 points) groups. Figure II plots the median time served across grid points without controlling for any other factors. Time served generally falls with points, as would be expected since individuals with more points are predicted to have lower recidivism risk and thus are likely to be treated more leniently by parole boards. However, the effect of crossing the threshold from high to medium risk is substantial—the typical inmate with eight points serves over three months longer than his counterpart with nine points—and easily separated from the underlying negative trend. The effect of the second cut-off is not as marked and cannot be distinguished from a general linear trend in points, and as such I focus on the first cut-off in most of the empirical work. To focus on an area with substantial density, I generally restrict the analysis to those who have between four and thirteen points. For this sample, the grid recommendation is followed exactly over 38 percent of the time, and the final parole board decision is within four months of the grid recommendation 64 percent of the time.[21] Summary statistics appear in the second column of Table I.

The discontinuous treatment of individuals above and below the thresholds in the grid suggests a regression discontinuity design; criminal propensity should vary in a roughly continuous manner across points, but the recommendation varies in a highly discontinuous manner at the threshold. Moreover, as the grid assigns higher-risk inmates to longer sentences, any bias would go against confirming the framework's assumption that longer terms reduce recidivism.

Importantly, the threshold between high and medium risk does not appear to be associated with discontinuous changes in characteristics associated with recidivism. In Online Appendix Table C.I, I regress several key background variables on a linear term for total grid points and a dummy variable for being above eight points, and for none is the coefficient on the dummy variable close to statistical significance. These results are echoed graphically in Online Appendix Figure C.I. Although I control for each of these factors in the regression analysis, it is reassuring to know that they do not appear to change discontinuously at the location of the threshold.

[19] Email correspondence with David Humphries, Guidelines Director, Georgia Board of Pardons and Parole.

[20] Including individuals from 1992 onward and all offense categories do not change the patterns or statistical significance of the main results, however.

[21] The corresponding percentages for individuals with offense severity levels above four are 18 and 26, which is why I chose to exclude them. For these more serious crimes, the parole board appears to rely more on their own discretion and less on the guidelines.

IV.B. Results

Before turning to regression analysis, I present graphical results on recidivism for individuals just above and below the cut-off between high and medium risk. Figure III plots the share of individuals who have returned to prison over various windows of time, separately for individuals with seven, eight, nine and ten points. All else equal, individuals with a higher point designation would be expected to have lower recidivism rates. However, as shown in Figure II, individuals with nine points are assigned a discontinuously lower prison term than those with eight points, and thus if prison time lowers recidivism risk, those with nine points should have higher recidivism rates relative to those with eight points than one would otherwise expect. Indeed, this pattern holds in Figure III. Those with seven (ten) points are predicted to be the highest (lowest) risk, and indeed they recidivate at the highest (lowest) rates. But the expected relationship between grid points and recidivism rates is reversed for those just above and below the threshold—individuals with nine points recidivate at higher rates than those with eight points.

Table II presents regressions results. Col. (1) presents "naive" OLS results, which ignore the potential endogeneity of time served. I include controls standard in the literature—demographic information, past incarcerations, as well as fixed effects for the year of release and controls for sentence length. The coefficient on months served is negative, but relatively small in magnitude—each month in prison lowers recidivism rates by 0.5 percentage points. Taking the linear specification literally, an individual entering prison with a sixty percent chance of recidivating would need to spend about five years in prison before his recidivism rate fell to thirty percent.

The rest of the table shows results when grid recommendations serve as an instrument for time served. Col. (2) shows the results from estimating the first-stage equation:

$$Months\ served_{ips} = \gamma Grid\ recommendation_{ps} + \lambda_p + \nu_s + \tau_{ps} + \epsilon_{ips},$$

where i denotes the individual, p his point designation, and s the severity level of the crime for which he was convicted, and τ_{ps} are additional controls that interact points and crime severity level (which I vary to determine how robust the results are to different assumptions). *Grid recommendation*, the instrument, is the corresponding entry from the grid, and λ and ν are fixed effect for points and severity level, respectively. The specification thus completely controls for variation in criminal background as reflected in grid points and variation in the seriousness of the current offense as reflected in the crime severity level. The identification comes only from the manner in which the grid interacts these two variables. Further, I sometimes allow τ_{ps} to include a linear trend in points for each crime severity level, in case the effect of points varies by offense level. In both specification of the first-stage equation, the F-statistic corresponding to the instrument is over ninety.

Col. (3) presents the first IV results. The local average treatment effect of an additional month of time served is to reduce the three-year recidivism by roughly 1.3 percentage points. The magnitude of the estimated effect is more than 2.5 times that in the OLS estimation in col. (1)—consistent with OLS estimates being positively biased, or, in this case, "less negative," than the true treatment effect—and the OLS point-estimate is not in the 95-percent confidence interval of the IV estimate.

Col. (4) adds a linear control for points that varies for each crime severity level. That is, I add the variable $\mathbb{1}(crime\,severity = s) \times Points$, where $\mathbb{1}$ is an indicator function, for each severity level s. As the coefficient is essentially unchanged, I return to the simpler specification for the rest of the table.[22]

In col. (5), instead of the binary measure of recidivism, I use the severity-weighted measure, as described in Section III, and the sign and significance of the result is unchanged. Finally, in col. (6), I estimate the col. (3) specification but include only those observations directly above and below the first cut-off, which has little effect on the point estimate.[23] Taken together, the results in cols. (3) through (6) suggest that the effect of time served on recidivism is robust to changes in the control variables, the weighting of the outcome variable, and the exact sampling criteria.

IV.C. Discussion

The coefficients suggest that an additional month in prison lowers the three-year recidivism rate by about 1.3 percentage points. Of course, a completely linear effect may be unrealistic, but interpreting the point estimate literally would be consistent with, say, the average inmate entering prison with a sixty percent chance of recidivating and leaving two years later with just under a thirty percent chance of recidivating. The results thus appear to be reasonable in magnitude.[24]

In the framework in Section II, the negative treatment effect of time served on recidivism means parole boards can potentially assign longer terms to higher-risk inmates so that initial differences in recidivism risk are offset by the time inmates are released. The next section explores how well parole boards actually meet this objective.

[22]Similarly, adding in a quadratic control for points for each severity level has no effect on the estimates (results available upon request). I also explore whether entering the main controls in a linear manner is overly restrictive. For this estimation, I replace *Age at admission* and *Prior incarcerations* with fixed effects for each age in years and each number of prior incarcerations, respectively. The coefficient barely moves (from -0.0130 to -0.0133).

[23]I also repeat this specification but use the second cut-off, which, as shown in Figure II, provides a weaker first stage. Perhaps for this reason, which would tend to bias the coefficient toward the positively biased OLS estimate, the coefficient (-0.0062) is substantially smaller in magnitude and not statistically significant, though is still negative and potentially economically meaningful.

[24]Estimating non-linearities in the recidivism-time-served relationship would appear to be a formidable challenge. Comparing, say, the treatment effect of individuals sentenced to longer sentences versus shorter sentences cannot speak to non-linearities in the recidivism-time-served relationship as the assignment to long versus short sentences is not random. The difference in the estimated local treatment effect may in part reflect a non-linearity, but it will also pick up the differential treatment effect across these two samples. One would need a two-stage randomization where an individual is first assigned to a long versus short sentence ($s^1 = \{s_S, s_L\}$) and then, in a second randomization, was randomly assigned some Δ more or less than s^1. Then, the local average treatment effect could be identified around both s_S and s_L, which would give a more detailed picture of the shape of the relationship between recidivism and time served than what I provide in Table II. However, a quasi-experiment that imitates such a randomization is difficult to imagine.

V. Do parole boards set release dates as a function of recidivism risk?

While the framework in Section II suggests that the relationship between recidivism risk and recommended time served should be positive if parole boards are acting in an allocatively efficient manner, as discussed earlier, this relationship is difficult to estimate empirically. Typically, the parole board's recommended time served *equals* actual time served, but actual time served has its own (presumably negative, given the results in the previous section) relationship with recidivism upon release. Thus, not controlling for actual time served will bias the relationship between recommended time served and recidivism risk toward zero, but in general the two variables cannot be separately identified because the former is determined by the latter.

Figure I.C showed that the positive association between recidivism risk and recommended time served can be recovered when actual time served is orthogonal to recommended time served. In fact, if the relationship between recidivism and actual time served is roughly linear, then a regression of recidivism risk on recommended time served and (randomly determined) actual time served should yield a negative coefficient on actual time served (simply the treatment effect of time served on recidivism) and a positive coefficient of equal magnitude on recommended time served. In this section, I explore a scenario in which actual time served randomly deviated from recommended time served, thus allowing me to test these implications of the framework.

V.A. Empirical strategy using the 1981 mass release

In March of 1981, the governor of Georgia ordered the GDC to free up several hundred beds so that over-crowded local jails could send some of their inmates to GDC state prisons. The GDC ranked its current non-violent inmates by day of prospective release—already set by the parole board—and on March 18 released them in that order until sufficient space had been created (roughly, those with prospective release dates set for the following twelve months were released). Those at the top of the list (with prospective release dates just days or weeks after March 18) served almost all of the time the parole board recommended, but those at the bottom of the list had served considerably less time than was originally recommended.

Most individuals in the mass-release sample were sentenced to relatively modest sentences—the mode and median is equal to 36 months—not surprising given that they were all non-violent offenders. I thus generally limit the sample to those sentenced to no more than six years, to ensure that the results are not driven by a small number of inmates in the mass release with much longer sentences, though I show that the main results are not sensitive to this cut-off.[25] For this sample of 519 inmates, the typical (average) inmate was sentenced to 36 (33.2) months, served about 12.1

[25] Six years is the 95$^{\text{th}}$ percentile among those on the mass release list. The main results are robust to including these longer sentences or picking a slightly different cut-off for the sentence maximum (see Online Appendix Table C.II).

(13.2) months, or about 5.2 (3.8) fewer months than the parole board had originally recommended. Additional summary statistics appear in the third column of Table I.

For inmates in this sample, how much time they serve is a function of how much time the parole board recommended—obviously, no one in the sample served *more* time than the parole board recommended. However, conditional on the parole board's recommendation, how much time they served is based on the date they began serving their sentence. Outside of potential seasonal effects (for which I can control), the actual date will have a large random component. Another attractive feature of this empirical strategy is that, unlike most past work using mass releases, the precipitating conditions leading to the releases occurred at *other* facilities (the local jails). As such, the prison inmates in my regression sample did not themselves experience unusually crowded or inhumane conditions and instead are more likely to have had a typical prison experience, outside of the early release.

V.B. Results

Because the mass release breaks the mechanical relationship between actual and recommended time served, I can now estimate the relationship between recommended time served and recidivism holding actual time served constant (and vice versa). Before turning to regression results, I explore these relationships graphically. I begin by considering the 68 inmates in the mass-release sample who have actual time served within one month of 6.2 months (the 25$^{\text{th}}$ percentile of time served for the mass-release sample) and in Figure IV.A consider how recidivism and recommended time served covary within this subsample.[26] Note that as inmates in the mass-release sample received an early release, the scatter plot points (the hollow circles in Figure IV.A) of recommended time served are all to the right of the actual time served of six months.

Figure IV.A shows that, as predicted by the framework, when actual time served is held constant a positive relationship emerges between recommended time served and recidivism. The figure also shows the same positive relationship when a parallel analysis is performed for the 58 inmates with actual time served within one month of 12.1 months (the median for this variable).[27]

Similarly, Figure IV.B shows how recidivism varies with time served when the *recommended time served* is held constant. Again, I show the results for the 25$^{\text{th}}$ and 50$^{\text{th}}$ percentiles of the recommended time served distribution, 11.3 and 17.4, respectively. Here, all the scatter points fall to the left of the recommendation, as each of these inmates serve less time than the parole board

[26] In practice, actual time served is a continuous variable (it is based on the actual date of release, so is measured in days, which I round to the nearest month by dividing by 30.4), so I cannot hold it perfectly constant or else I would only have a single or handful of observations for each value of actual time served, which is why I use the sample with time served within one month of 6.2 months.

[27] Similar results hold when I examine the 75$^{\text{percentile}}$ of time served, though because the distribution of time served is somewhat right-skewed and thus less dense at higher values, it is difficult to hold actual time served relatively close to the 75$^{\text{th}}$ percentile but also have a large enough sample size to perform a meaningful analysis.

had recommended. Consistent with the results in the previous section and with the assumption of the framework, the scatter plots show a negative relationship between recidivism and actual time served.

Table III presents regression results. Col. (1) regresses the three-year recidivism rate on the parole board's recommended time served and actual time served. The point-estimates suggest that a one-month increase in the parole board's recommendation is associated with a 3.6 percentage point increase in the three-year recidivism rate, whereas an extra month in prison reduces the three-year recidivism rate by roughly 3.3 percentage points. Thus, not only do the coefficients on actual and recommended time served have the predicted, opposite signs, they are of roughly equal magnitude, consistent with the prediction from Section II.

For this mass release sample, holding constant the parole board's recommendation, the variation in time served by March 1981 is driven by when the individual started his sentence. As such, I control in col. (2) for month-of-admission to see whether seasonal effects are potentially polluting the identification, and the point-estimates are essentially identical. Col. (3) replicates col. (1) but uses the weighted measure of recidivism and the predicted pattern of the coefficients on actual time served and recommended time served holds.

An important question in terms of the marginal contribution of the parole board to allocative efficiency is whether variation in inmates' original sentences would have captured much of the gains without the parole board altering them. In Georgia, courts have never been constrained by sentencing guidelines, so this freedom could allow them to vary punishments to offset individual risk, rendering the parole board redundant. Col. (4) investigates the marginal contributions of courts versus parole boards by including inmates' original sentence as a control. The coefficient on the original sentence variable is positive, suggesting that sentences do correlate with recidivism risk, but it is small and not statistically significant. While the coefficient on the parole board's recommendation decreases slightly, it is still significant at the ten-percent level. Col. (5) goes further and shows the col. (1) specification on the subsample of inmates with an original sentence of *exactly* 36 months (the median and mode for the mass-release sample). Even though the sample-size shrinks by nearly 80 percent, the coefficients on the parole board's recommendation is still positive and significant. It thus appears that most of the allocative efficiency gains associated with parole board recommendations come from their altering original sentence length.[28]

In the framework in Section II, if a certain group's expected risk falls faster with time, then parole boards should take that into account when making recommendations. Therefore, the result that the coefficients should be equal in magnitude and opposite in sign should hold for subgroups

[28]One possibility is that, in the absence of a parole board, courts would begin to vary punishments more at the individual level than they currently do. I do not find any evidence of such a reaction after Georgia drastically decreased parole discretion for certain crimes after 1997—the reform investigated in the next section. If anything, the standard deviation of sentences for these crimes shrinks slightly after parole is curtailed.

of the data as well. In col. (5), it held for the subgroup sentenced to exactly three years. Col. (6) shows that the result holds for those convicted of burglary (the most common conviction charge in the mass release sample). It also holds for other salient subsamples—inmates above versus below the sample's median age at admission (see Online Appendix Table C.II) and white versus black inmates (available upon request).

So far, I have been very parsimonious about control variables. The framework in Section II makes clear that a social planner seeks to release individuals when their *actual* recidivism risk, not recidivism risk after conditioning on, say, demographic controls or criminal history, falls below the cost of incarceration. Put differently, allocative efficiency is an unconditional, not a conditional, concept. If the relationship between recommended time served and recidivism risk appeared *only* after controlling for covariates, then it would suggest that the parole board is assigning longer sentences to higher-risk individuals *within certain categories* but is not necessarily assigning longer terms to higher-risk individuals more generally. As such, I have not controlled for covariates so far in these regressions.

However, controlling for individual characteristics sheds light on a different and also important question—how parole boards make their decisions. If adding, say, criminal history variables as controls, eliminates the relationship between the parole recommendation and recidivism, then the gains in allocative efficiency associated with parole decisions could be replicated by instead using a formula based on these variables. Col. (7) adds the standard controls used earlier in Table II, and shows that the coefficient on the parole recommendation barely falls from that in col. (1) and is still highly significant. Col. (8) adds a more exhaustive set of controls and even then the coefficient on the recommendation is only reduced by less than 25 percent and is still significant at the ten-percent level.[29] These results indicate that parole boards are making use of information beyond basic demographics, criminal history and the nature of the current offense.[30] Thus, at least with respect to these standard covariates commonly associated with future criminality, the contribution of the parole board's discretion toward allocative efficiency would not seem easily replicated by instead using a formula with these variables as inputs. Note also that despite varying sets of controls used, the coefficient on actual time served barely moves across cols. (1), (2), (7), and (8), supporting my identification assumption that, conditional on the parole boards recommendation, the variance in

[29] Additional controls include fixed-effects for offense category, fixed effects for age at admission rounded to the nearest five years (age rounded to the nearest year results in the probit regression dropping about forty observations, which is why I use the rounded measure, but the results are very similar), fixed effects for the number of prior incarcerations and fixed effects for employment status at the time of arrest (which has a large share of missing values, which received its own fixed effect). I also experimented with adding interactions between race, gender and age, but adding these additional controls did not affect the coefficients of interest.

[30] It would be useful to know how much factors such as disciplinary infractions and rehabilitative program completions (outcomes I investigate in the next section) could explain the positive correlation between parole recommendations and recidivism, but the GDC did not start recording these measures until well after the mass release took place.

time served caused by the mass release is as good as randomly assigned.

Finally, Online Appendix Table C.II shows that the main results are robust to changing the window for the recidivism definition from three years to four, using a hazard model instead of a probit, and varying the sentence lengths included in the sample.

While I have generally focused on the coefficient on the parole board's recommendation, note that the coefficient on actual time served corroborates the results in the previous section on the treatment effect of time served on recidivism. While the 3.3-percentage-point-per-month effect is larger than the treatment effect in the previous section, given its standard errors one cannot reject that they are equal. Moreover, compared to the sample in the grid experiment, the individuals in the mass-release sample are roughly five years younger, and based on the results from cols. (4) and (5) in Online Appendix Table C.II, the effect of prison time on recidivism upon release tends to be larger for younger inmates.

V.C. Do parole boards use all available information?

Another test of whether the parole board is making efficient use of information is to compare the informational value of observables in predicting recidivism when parole boards enjoy more or less discretion. If parole boards use their discretion to assign more time to those who continue to have high expected recidivism rates and less time to those who are no longer serious risks, then observable variables should have limited predictive power, as boards should have already used the information in setting release dates so as to limit the variance of expected recidivism upon release.

This test requires variation in how much freedom the parole board has to optimize an individual's sentence. I make use of the fact that in Georgia boards cannot generally release an inmate before one-third of his sentence has expired and cannot keep him in prison beyond the expiration of his sentence. Thus, at these lower and upper bounds, boards are highly constrained, and for inmates in these regions observable information should have greater power in predicting recidivism as boards have been forced to leave some information "on the table." By this logic, R^2 values should have a u-shaped relationship with the share of his sentence that the parole board recommends an inmate should serve.

To test this prediction, I divide individuals based on the percent of their sentence they are ordered to serve: zero to ten, ten to twenty, and so on, up to ninety to one-hundred. Because there are very few serving less than thirty percent, I combine the first three groups. For each group, I run a separate probit regression of the three-year recidivism rate on the standard set of covariates used in Tables II and III.

Figure V plots the pseudo-R^2 values for each of these regressions against the parole board's recommendation as a percent of the original sentence. The R^2 value for those who serve less than thirty percent of their sentence is roughly 0.09. It falls to roughly 0.06 for those serving between forty and eighty percent, and then climbs back to 0.09 for those serving between ninety and one-hundred percent. The second series in the figure runs the same set of regressions but uses the weighted version of recidivism, with the same pattern resulting.

In general, I choose the controls used so far in this figure as well as the earlier tables because they are commonly used in the literature. However, the Georgia data are very rich, and in the final series I re-estimate the probit regressions using a much richer set of covariates that the parole board would have reasonably had at their disposal.[31] The same pattern obtains, suggesting that the u-shaped relationship between the predictive power of observables and the percent of the sentence the parole board recommends is robust to the exact controls and measure of recidivism used.

V.D. Discussion

The results from the 1981 mass release suggest that parole boards' recommendations appear to offset inmates' recidivism risk, in accordance with the framework in Section II. Moreover, when parole boards are less constrained, information regarding an inmate's expected recidivism risk appears to be used more efficiently than when boards are more constrained.

The simple framework in Section II suggested that if the coefficient on actual and recommended time served are equal in magnitude but opposite in sign—as they essentially are in Table III—then parole boards are fully optimizing and the observed variation in recidivism risk upon release would be pure noise. However, R^2 values are not zero in Figure V even in regions where the parole boards are assumed to be unconstrained. Several explanations seem likely. First, recall that the framework makes a linearity assumption, so the prediction that the coefficients on actual and recommended time served be of exactly the same magnitude, while a useful benchmark, should not be taken too literally. Second, even if the parole board perfectly optimized on this small sample of non-violent offenders that benefited from the mass release, it is very likely that it typically tends to leave at least some information on the table. Indeed, given the stickiness of the grid recommendations demonstrated in Section IV, parole boards appear inclined to make use of heuristics instead of adjusting time served on a truly case-by-case basis.[32] Third, beyond not being able to release an inmate before the first hearing or after the sentence expires, parole boards' discretion is likely circumscribed in other ways that I cannot observe. For example, during periods when prison capacity is especially limited, they likely face pressure to release inmates earlier. Given that these factors seem likely to affect all inmates similarly, whereas the first-hearing and max-sentence constraints are only binding for some, I would still predict an increase in the R^2 value at these two end points

[31] These covariates include controls for disciplinary infractions and rehabilitative program completions (variables that are described in greater detail in the next section), fixed effects for age at admission, interactions between gender, race and age, time served, and controls for educational level and marital status upon admission.

[32] The 1981 mass release took place before the grid was implemented, and the stronger evidence of allocative efficiency from the results from that experiment may have been due to the parole board's greater freedom during that period.

of the percent-of-sentence served distribution. However, these factors suggest that one should not necessarily expect R^2 values to be zero even for intermediate values.

Whether or not they perfectly offset differences in recidivism, the evidence from this section suggests that parole boards are adjusting time served in the allocatively efficient direction and indeed assign more prison time to inmates with greater recidivism risk. Releasing inmates only after their risk falls below a certain threshold should provide incentives for prisoners to invest in their own rehabilitation so as to reach the threshold sooner. The next section investigates whether inmates actually react to such incentives.

VI. The effect of parole board discretion on recidivism and rehabilitation

It is not clear *a priori* that inmates would necessarily respond to the incentive to invest in rehabilitation that parole boards would seem to create. Offenders are thought to discount the future heavily (Lee and McCrary 2005, McCrary 2010), so the possibility of a sentence reduction several years in the future might have little effect on their contemporaneous decisions to invest in lowering their recidivism risk. Moreover, inmates may not believe that parole boards can actually observe their true recidivism risk, so may not feel any greater incentive to reform under a parole regime than under one in which their release date is fixed.[33] As such, it is an empirical question, to which I now turn.

VI.A. Georgia's "Ninety-percent" reform

In 1997, the GDC announced that all inmates convicted of certain offenses after December 31st must serve at least ninety percent of their sentence.[34] The parole board maintained the power to assign inmates convicted of a so-called "ninety-percent crime" to release dates between ninety and one-hundred percent of their original sentences and their original discretion over inmates convicted of other crimes. Although the specified offenses included such sensitive crimes as child molestation and incest, the majority of those affected by the policy were convicted of robbery and assault.[35] In 2006, the reform was declared unconstitutional by a state court and discontinued.

Figure VI plots the share of their sentence served by inmates convicted of ninety-percent offenses and who otherwise meet the criteria in Section III, as well as that of the control group, inmates

[33] The parole-versus-rules decision might have effects on recidivism rates outside of the mechanisms included in the framework in Section II. For example, discretionary parole might increase recidivism by demonstrating to early releasees that the criminal justice system has more "bark than bite," an interesting hypothesis explored by Bushway and Owens (2011) in the context of actual sentences set far below the maximum allowable level under sentencing guidelines. The results in Section VI, showing that eliminating parole increases recidivism, would be gross of this effect, so at least in the context of parole if this effect operates it is outweighed by other factors.

[34] According to discussions with Dr. Tim Carr (GDC), the legislature's threat to pass a law to this effect precipitated the department's decision.

[35] The crimes specified in the ninety-percent reform were child molestation, statutory rape, aggravated assault or battery, car-jacking, attempted murder, assault on a police officer, incest, attempted rape, manslaughter, or robbery. Assault and battery accounts for 35 percent of such cases and robbery for 22 percent.

who meet the Section III criteria but were convicted of offenses not affected by the reform. I plot these against the year the inmates were sentenced.[36] The figure shows that for the average "ninety percent" inmate, the reform had little effect, as inmates convicted for these offenses had already been serving nearly ninety percent of their sentence before 1998. However, the effect can be seen at lower percentiles—before 1998, the inmate at the 25$^{\text{th}}$ percentile served roughly three-quarters of his sentence, and after the reform served ninety percent. Similarly, the inmate at the tenth percentile originally served roughly fifty percent of his sentence, which increased to over eighty percent after the reform.[37] The mean for the control group shows no change after 1997, and, though not shown to avoid cluttering the figure, neither do the 25$^{\text{th}}$ or 10$^{\text{th}}$ percentiles. Perhaps because the policy was phased out in 2006, beginning with those sentenced in 2002 the 25$^{\text{th}}$ percentile serves slightly less than ninety percent of their sentence, so my regression sample will include those sentenced between 1993 and 2001.

Online Appendix Figure C.II shows that sentences for those convicted of ninety-percent crimes were slowly falling through the sample period, while those for inmates convicted of other crimes remained relatively flat.[38] As a result of the overall negative trend for sentence length as well as the fact that many were serving close to ninety-percent of their sentence in the pre-period, there is only a two month increase in time served for the ninety-percent inmates relative to control inmates after 1997.

Figure VI and Online Appendix Figure C.II suggest that the ninety-percent reform is probably best thought of as removing the hope prisoners might have had of being in the fortunate, say, twenty percent of inmates who in the original regime could have seen a twenty to thirty percent reduction in their prison sentence. As such, it offers a relatively rare chance to study the effect of a reduction in discretion that did not have a large effect in terms of average punishment length. And to the extent that the policy did increase time served, it would tend to decrease recidivism (based on the negative treatment effect of time served on recidivism documented in Sections IV and V), biasing the results against finding that greater discretion is associated with lower recidivism rates.

[36]While the reform actually applies to individuals *convicted* after 1997, the two dates are usually the same and the GDC recommends using the sentencing date, which has fewer missing values. From the GDC data documentation: "Date sentence began can be substituted for conviction date. Sentence began date is more stable than conviction date." Note also that for this graph (but not for later regressions), I include only those inmates sentenced to less than eight years in prison and thus I am calculating the percent of sentence served for a sample whose original sentences would have expired before my data terminate in Spring 2011. Not doing so would lead to some individuals to have their time served truncated, but in practice including them in the figure makes little difference.

[37]Some of the early releases after 1997 among the group of inmates at the tenth percentile and below are for out-of-state transfers, deaths or other unusual circumstances. However, roughly half of individuals below the tenth percentile appear to have been paroled before serving ninety percent of their sentence, which may reflect a slight incomplete enforcement of the policy or classification errors in the conviction-offense variable.

[38]In particular, it does not appear that judges began to lower sentences for ninety-percent offenders relative to other inmates in order to offset the effect of the policy, consistent with the findings in Owens (forthcoming) on truth-in-sentencing laws more generally.

VI.B. Empirical strategy and results on recidivism rates

The ninety-percent reform suggests a difference-in-difference strategy, and I thus compare how outcomes for those convicted of ninety-percent crimes change after 1997 relative to any change for the control group. The key outcome, as usual, is recidivism risk. Essentially, I wish to test the prediction in Figure I.D that when inmates are incentivized by the hope of an early release, they will reduce their recidivism risk.

Before turning to the results, I look for compositional changes that might have arisen at the same time the policy went into effect, though in the regressions I will also control for the standard background variables. Online Appendix Table C.III shows that the difference between the ninety-percent and control groups do not change after 1997 with respect to the main control variables.[39] Online Appendix Figure C.III explores compositional changes graphically. The only discernible pattern is that the black share of the ninety-percent group falls during the pre-period, and flattens out in the post period. Note, however, that because blacks in my data tend to have higher infraction and recidivism rates and lower program-completion rates, this demographic shift would make it less likely to find the results documented in this section.

Figure VII plots the recidivism rate for the ninety-percent inmates and the control group. I include only inmates who meet the usual sampling conditions and are also sentenced to no more than five years. This condition means that inmates who begin their sentence in the final year of my sample period (2001) will be released by 2006 and will thus have spent all or the large majority of their time in prison while the ninety-percent policy was in effect. Both before and after the reform, the ninety-percent group has lower recidivism rates. Such a result is consistent with their serving substantially longer prison terms, though, of course, prison time is not assigned randomly in this case so selection effects could contribute to this difference as well. More important for the question at hand, recidivism rates for ninety-percent inmates relative to the control group increase substantially in 1998—a relative increase of four percentage points realized immediately the year the reform is introduced. The figure also shows that this effect persists and is even more marked for the four-year recidivism rate.

As noted earlier, ninety-percent inmates serve substantially more time in prison both before and after the reform than do control group members, meaning the control group as currently constituted may not serve as the ideal comparison for the treatment group. As such, I will display some of the key results when a subsample of the control group—those with longer sentences—is compared to the treatment group. This conditioning removes some but not all of the difference in time served. Because the ninety-percent crimes were chosen because they are considered serious in nature, it is not easy to find a perfect control group that receives equal treatment. Online Appendix Figure C.IV

[39] As noted in the notes to the Online Appendix Table, this null result holds on both the smaller recidivism sample used in this subsection or the larger activities-while-in-prison sample used in the next subsection.

replicates the analysis in Figure VII but limits the control group to those with sentences of at least four years, which brings the time served of the control group to within ten months of the treatment group, and the sharp increase in recidivism after the reform is, if anything, more dramatic.

Table IV reports regression results. The coefficient on the interaction term $Ninety\ percent \times After$ 1997 is positive and significant without basic covariates (col. 1) and grows in magnitude by about twenty percent when basic covariates are added (col. 2). About half of the increase is driven by controlling for time served. As noted earlier, ninety-percent inmates serve slightly more time after the reform, which should reduce recidivism *all else equal*. But all else is not equal, because these inmates now have fewer incentives to reform, which shifts the recidivism-time-served relationship to the right, as in Figure I.D. As such, controlling for time served makes the estimated effect of the ninety-percent policy on recidivism even larger.

The remaining specifications perform robustness checks. Col. (3) drops control group inmates with sentences shorter than four years, to make the treatment and control groups more comparable. The coefficient of interest changes only slightly and still remains highly significant despite the substantially smaller sample. The result holds when, in col. (4), the weighted recidivism measure serves as the outcome variable. Finally, col. (5) includes a linear trend for ninety-percent inmates. Given, if anything, a slightly negative pre-trend for the treatment group in Figure VII, it is not surprising that adding this control slightly increases the coefficient on the interaction term.

VI.C. Results on investment while in prison

While the key result in terms of the framework in Section II is recidivism, I also examine how disciplinary infractions and rehabilitative program completions change for ninety-percent inmates relative to other inmates. These outcomes serve as further tests of the story that inmates respond to the hope of an early parole by taking steps to lower their recidivism risk. If, instead, the lower recidivism rates documented above were accompanied by *worse* behavior in prison and *less* participation in prison programs, then one might worry that the recidivism effects were spurious.

Of course, correctional officers have a myriad of methods for controlling individuals' behavior while in prison—from recommending solitary confinement to looking the other way when a prisoner needs protection from an abusive inmate—that could provide more immediate incentives, and these factors might swamp the incentives created by parole. Interestingly, however, the correctional officers union president voiced concern over the likely effects of the ninety-percent law when it was first announced: "If that [the hope of early parole] no longer exists, what carrot or stick is there for inmates? Parole has been a major factor to control inmates. In Georgia prisons, there are no longer weights and TV. What other privileges are there?"[40]

[40] *Savannah Morning News*, "Reaction mixed on plans for no parole," Lawrence Viele, January 4, 1998. This quote (from union executive director Tyrone Freeman) is especially telling because prison-guard unions are usually among the most vocal supporters of any "get tough" prison policy that increases time served, as such policies increase

Unlike recidivism, I can observe the infractions and prison program variables whether an individual has completed his term or is still in prison, and thus will generally not condition on an individual having completed his sentence for inclusion in these analyses (though I show that the results are robust to doing so). Because the policy was phased out in 2006 and one would like to exclude behavior measures from beyond that point, I calculate each inmates' annual rate of total infractions or program completions by March of 2006, regardless of whether they had completed their sentence by then.[41]

Figure VIII plots the rate of disciplinary infractions per twelve months for the ninety-percent and control groups. Because the infraction rate distribution is highly right-skewed—the median rate is 0.3 infractions per twelve months, with the 99^{th} percentile (7.8) over seven times greater than the 75^{th} percentile (1.04), and twice as large as the 95^{th} percentile (3.8)—I drop outliers above the 99^{th} percentile to make the figure more readable, though include all observations in some of the later regression analysis. The first two series show the resulting means for each group, by year of sentencing. Overall, ninety-percent inmates have higher rates of disciplinary infractions both before and after the reform. The third series subtracts the control-group means from the ninety-percent-group means, showing a marked increase after 1997 in the infraction rate of individuals convicted of ninety-percent crimes relative to the control group. The fourth series shows this difference after outliers above the 98^{th} percentile have been dropped, which makes the relative increase in infractions among the ninety-percent group even clearer. As with the recidivism sample, the ninety-percent group serves substantially more time than the control group. Online Appendix Figure C.V replicates Figure VIII but restricts the control group to individuals who have sentences of at least five years. This conditioning reduces the difference between the average time served in the treatment versus the control group from 25 to 13 months, thus making the two groups more comparable. As with recidivism, the relative increase in infractions among ninety-percent offenders after the reform appears even more marked.

Table V presents the regression analogues to Figure VIII. Cols. (1) and (2) shows that, with or without the basic controls, the coefficient on the interaction term is positive but not significant when all observations, including outliers, are included. Col. (3) excludes observations above the 98^{th} percentile, and the coefficient on the interaction term increases substantially from its level in col.

demand for their labor (see, e.g., Page 2011).

[41] Actually, I do not calculate these measures as I was not given infraction-level data. Instead, the GDC merely keeps track of the accumulated total infractions, which gets updated with each new version of the data. Rather luckily, the original version of the inmate data file given to me by the GDC had accumulated infractions through March of 2006, just as the ninety-percent policy was being phased out. So, by dint of good fortune, I have a measure of behavior recorded during a period when the ninety-percent policy was in effect. To account for the fact that inmates will have served different amounts of time by the time they are observed in March 2006, I generally use the rate of infractions per twelve months—that is, I divide the GDC's accumulated infractions by March 2006 by the total months served by March 2006. For individuals released before March 2006, I divide their total infractions by their total (completed) months served. I follow the same steps in calculating the annual rate or rehabilitative program completions.

(2) and is now highly significant.[42] I next investigate the effect of the policy on the extensive margin of committing any infractions versus the intensive margin. The effect on the extensive margin (col. 4) is positive and significant, and the effect on the intensive margin (not shown) is positive but with a p-value just over 0.2. This pattern is not surprising as it would seem likely that changes in parole policy would affect an individual who is in the right tail of this distribution less than someone with zero or few infractions, given that both before and after the reform the former inmate would be unlikely to see any leniency from the parole board.

The next two columns subject the result in col. (3) to robustness checks. The coefficient increases substantially when, in col. (5), I restrict the control group to have a sentence of at least five years, so that the control and treatment groups are more comparable with respect to time served (the result also holds on the recidivism sample used in Table IV). Finally, col. (6) allows the effect of being convicted of a ninety-percent crime to have a linear trend, which increases the coefficient on the interaction term.

Besides avoiding infractions, an inmate investing in his own rehabilitation might also take advantage of vocational courses or other rehabilitative programs offered in prison. Figure IX is the analogue to Figure VIII but the rate of program completions per twelve months is plotted. The most striking aspect of the figure is the strong upward trend among both the treatment and control groups throughout much of the sample period; the GDC informed me that this trend is likely not due to an actual increase in program completions but merely improved reporting from prisons throughout the early years of the sample period. Given the changing nature of reporting for this variable, I tend to view the program-completion results more tentatively than the recidivism or disciplinary-infraction results.

That being said, there is an obvious trend break for the ninety-percent group in 1998—before 1998, a strong, positive pre-trend prevails, but then abruptly flattens out for inmates convicted in 1998 and later. The control group, after a slight dip in 1998, regains a positive trend for the next few years. This temporary dip for the control group in 1998 means the relative drop in investment begins in earnest only in 1999, as the third series shows. Because of the strong reporting-based pre-trends for both groups, it is difficult to pinpoint when exactly investment activity among the two groups begins to diverge. Overall, however, after moving in near lock-step for several years, investment patterns for the ninety-percent group drop off relative to the control group at or shortly after the introduction of the reform. Online Appendix Figure C.VI shows similar results when the control group is limited to those with sentences of at least five years.

Table VI presents regression results. Without any controls beyond year fixed effects, the coefficient on the interaction term is negative and significant. Ninety-percent inmates after 1997 complete

[42] Though not shown in the table, estimating a median regression also gives a large, positive statistically significant coefficient on the interaction term (as does estimating other major quantiles).

about 0.13 fewer programs each year, relative to their control-group counterparts. Given that the average inmate completes just under 0.6 programs a year, that effect is sizable. Adding standard controls in col. (2) has little effect on the estimates. Cols. (3) and (4) shows that the effect can be observed at both the intensive and extensive margins.[43] As with infractions, the results are robust to restricting the control group to have longer sentences (col. 5) and to instead using the recidivism sample in Table IV (not shown). Unlike the results on disciplinary infractions, the result is not robust to including a linear trend for ninety-percent crimes. Figure IX and Online Appendix Figure C.VI suggest a linear trend may not be particularly appropriate—indeed, the result when this control is included is highly unstable when the end points of the sample period change slightly, perhaps not surprising given the shape of the raw differences graphed in these two figures. Instead, col. (7) focuses the analysis on the three years immediately before and after the reform, so that at the very least the large differences between treatment and control in the first two years of the sample period are excluded. The coefficient of interest remains negative and highly significant.

VI.D. Discussion of the ninety-percent results

Taken together, the results from this section suggest that the hope of an early parole release incentivizes inmates to invest in their own rehabilitation and when such incentives are removed investment falls and recidivism rises. It could be the case that the specific rehabilitative investments explored in this section—i.e., prison behavior and program completions—*cause* recidivism to fall—but such a relationship is not necessarily implied by the results. For example, the hope of parole could lead prisoners to change in ways I can observe (e.g., enrolling in prison programs and avoiding disciplinary infractions), as well as ways I cannot (e.g., committing themselves to religion or reconnecting with their families). If the observable changes have no effect but the unobservable changes in fact reduce recidivism, then the same pattern of results seen in this section could be produced without disciplinary infractions and program completions having any causal effect on recidivism.[44]

Of course, reducing disciplinary infractions and promoting prison education may be of interest in their own right, independent of any affect on recidivism. To return to the terminology of the framework in Section II, changes in the infraction rate will shift the incarceration cost curve C downward—for example, the compensating differential required to work with prisoners should fall if they are better behaved, reducing prison wage bills. Similarly, prison education might shift downward the net social costs associated with releasing a prisoner—for example, the skills he acquires

[43] Although the program-completion rate is not particularly skewed, for completeness I also follow the previous table and drop outliers above the 98^{th} percentile, and the coefficient of interest remains negative and significant. As with disciplinary infractions, the result is robust to estimating median regressions and other major quantiles. Results available upon request.

[44] Indeed, the mechanism by which rehabilitative investment in prison might affect future recidivism remains an open question. See Wilson et al. (2000) for a review.

could bring better employment opportunities and thus he might contribute more in tax revenue during times he is not in prison.

VII. Conclusion

This paper developed a simple framework for comparing two types of release regimes—a traditional parole model and a fixed-sentence regime—in their ability to minimize the costs associated with the incarceration and future recidivism of prisoners. I examine two key ways that release policies can affect these costs. First, policies can be more or less allocatively efficient—allocative efficiency requires that costly prison space be allocated to inmates with the greatest recidivism risk. Second, release policies can incentivize inmates to lower their incarceration and recidivism costs by investing in their own rehabilitation. For example, inmates who know that they must lower their recidivism risk to gain an early release may behave better in prison (lowering incarceration costs) or take steps to prepare themselves for a successful release (lowering recidivism costs).

I present evidence that in the state of Georgia, parole boards appear to perform better along these metrics than do policies in which inmates' original sentences are binding. Results from the mass release experiment suggest that parole boards indeed assign longer terms to inmates with greater recidivism risk. Evidence from Georgia's "ninety-percent" reform suggest that inmates respond as predicted to parole boards' conditioning of release dates on expected recidivism. Inmates who lost parole eligibility as a consequence of the reform accumulated more disciplinary infractions, completed fewer prison rehabilitative programs and recidivated at higher rates after the reform than did a control group not subject to it.

While a full welfare analysis is beyond the scope of this paper, the estimates presented in the previous sections allow at least a rough accounting along certain dimensions. Consider col. (2) of Table IV, which suggests the ninety-percent policy is associated with a ${}^{0.065}/{}_{0.219} = 29.7$ percent increase in the three-year recidivism rate, and assume that other inmates would react similarly had the policy been applied to all offenses. Raphael (2011) calculates the elasticity of the steady-state incarceration rate with respect to several transition probabilities, including the probability of returning to prison after release. Using his elasticities, I estimate that the policy would increase the incarceration rate by 9.4 percent; applied nationally, such an increase would entail roughly \$5.5 billion annually in additional incarceration costs.[45] Of course, an increase in the recidivism rate not only increases the prison population but also the overall crime rate. My back-of-the-envelope estimate is that a thirty-percent increase in the recidivism rate could lead to a three percent increase in the general crime rate, and a considerably higher increase for serious violent crime such

[45] For the estimate of \$5.5 billion, see footnote 1 for current prison spending of \$58.5 billion. This calculation assumes constant returns to scale. As I am not including the higher costs of prison administration due to greater disciplinary infractions, this estimate is likely a lower bound for the effect on prison costs.

as murder.[46] Given that the annual cost of crime may be on the order of \$1 trillion (Anderson 1999), the increase in social costs along this dimension would seem at least as large as those related to prison expenditure.

Estimating the benefits of allocative efficiency requires more restrictive assumptions than does the above calculation, so readers may wish consider the following calculation with greater caution. In the more formal version of the framework in Online Appendix A, I derive a formula for the benefits of allocative efficiency, and in Online Appendix B, I show how each element in the formula can be derived from the regression results already presented. My best approximation is that eliminating the allocative-efficiency benefits of parole discretion increases the costs associated with inmates' incarceration and recidivism by five to seven percent, or about \$5,000 per offender.

As noted earlier, the reform in Georgia that curtailed parole discretion is relatively mild compared to the reforms in many other states, sixteen of which have completely abolished parole. If inmates in other states respond similarly to those in Georgia when parole discretion is limited, then not only have millions of individuals been incarcerated over the past two decades as prison populations have ballooned, but the institutional incentives to which they have been exposed may have been more criminogenic than in the past. The decline of parole may thus help explain why recidivism rates have remained high while crime rates for the rest of the population have fallen over the past two decades.

The movement to limit parole and otherwise "get tough" with respect to release policy may have crested, at least temporarily, as tight budgets since the 2008 financial crisis have led states to search for ways to reduce their prison populations. Recent state-level reforms have included the expansion of parole eligibility for certain crimes, the re-introduction of good- or earned-time for the completion of GED, drug treatment or other rehabilitative programs, and significant changes to re-entry programs.[47]

While the current paper largely compares two specific policies—traditional discretionary parole versus fixed-sentences—future work could exploit this state-level experimentation and compare a larger variety of policy options, enriching our understanding of the rehabilitative process. For example, if the same programs tend to reduce recidivism for all inmates, then more formulaic,

[46] I have not found a paper that calculates the steady-state crime rate as a function of the recidivism rate of ex-inmates. As a static calculation, I use evidence from Rosenfeld et al. (2005) that in 2001 inmates released over the previous twelve months account for about ten percent of all arrests (though a higher share of violent crimes) , and assume, as they do, that the arrest rate is proportional to the crime rate. A thirty-percent increase in the recidivism rate would thus lead to a $0.10 * 0.3 = 3$ percent increase in the crime rate. As the ten percent figure only accounts for those released in the previous twelve months and recidivism rates two or three years after release are still considerable, I suspect this calculation is an underestimate.

[47] An August 2011 *New York Times* story began: "Fanned by the financial crisis, a wave of sentencing and parole reforms is gaining force as it sweeps across the United States, reversing a trend of 'tough on crime' policies that lasted for decades," and gives many examples of recent reforms. See also the annual report "The State of Sentencing," by the advocacy group The Sentencing Project (for the 2011 report, see `http://www.sentencingproject.org/doc/s_Model_Legislation_for_State_Sentencing_Reform.pdf`) for many additional examples.

earned-time policies that directly incentivize their use might be expected to reduce recidivism more effectively than parole. If instead inmates are highly heterogeneous and each knows best what works for him (for some, say, attending GED classes, while for others, studying the Bible) then giving them a more general goal—convincing the parole board they are reformed—might work better. Similarly, while this paper has focused on inmates' activities while in prison, future work exploiting changes in re-entry programs (expansions in some states and contractions in others) could examine how best to incentivize investment and rehabilitation outside of prison among the newly released. These and related research questions would appear to take on heightened importance as states try to maintain public safety while, for the first time in decades, reducing prison populations.

APPENDIX TABLE A.I
GEORGIA PAROLE GUIDELINES ("THE GRID"), 1993-2005

	Recommended time served (months)		
Offense severity level	Low risk (14-20 pts.)	Avg. risk (9-13 pts.)	High risk (1-8 pts.)
1	10	16	22
2	12	18	24
3	14	20	26
4	16	22	28
5	34	40	52
6	52	62	78
7	72	84	102

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TABLE I
SUMMARY STATISTICS, GEORGIA DEPARTMENT OF CORRECTIONS INMATE DATA FILE

	Empirical application					
	General	Grid	Mass release	R^2 test	Ninety-pct. reform	
	(1)	(2)	(3)	(4)	(5) Ninety-percent	(6) Control
Returned to prison within 36 months of release	0.250	0.344	0.360	0.273	0.267	0.177
Months served	32.86	24.80	13.15	26.56	28.04	54.25
Black	0.597	0.655	0.590	0.594	0.617	0.593
Male	0.888	0.901	0.927	0.873	0.878	0.926
Age at admission	32.19	32.80	27.26	33.19	33.07	30.27
Prior incarcerations	0.822	1.298	0.536	0.971	1.012	0.474
Observations	60,638	17,374	519	41,770	29,556	14,746

Notes: For more detailed sampling information, please see relevant sections of the text. In general, col. (1) includes all individuals entering prison directly from a sentencing ("new admits") after 1992, serving sentences between seven months and ten years, released before 2008, and at least 18 years old at the time of admission; col. (2) makes the same restrictions as col. (1) but in addition samples only individuals admitted after 1995 and released before 2006 (which corresponds to the period when "the grid" did not have any changes made to it), and convicted of a crime with a severity level of less than five (because the grid has the greatest predictive power in that region), and have grid points between four and thirteen; (3) includes all individuals released as part of the mass commutation in 1981 who otherwise meet the usual sampling restrictions in col. (1) and have sentences no greater than six years ; (4) makes the same restrictions as (1) but excludes individuals convicted of "ninety-percent" crimes as parole boards do not have full discretion over them; (5) and (6) make the same restrictions as (1) but sample only individuals sentenced between 1993 and 2001.

TABLE II
THE EFFECT OF TIME SERVED IN PRISON ON RECIDIVISM RISK UPON RELEASE

	Dept. vars: Three-year recid. rate (2nd stage), months served (1st stage)					
	Recid. (OLS)	Mos. served	Recidivism (IV estimates)			
	(1)	(2)	(3)	(4)	(5)	(6)
Months served	-0.00497***		-0.0130***	-0.0130***	-0.382**	-0.0150***
	[0.000342]		[0.00291]	[0.00291]	[0.146]	[0.00253]
Black	0.0363***	-0.0802	0.0316***	0.0317***	1.495***	0.0202
	[0.00725]	[0.202]	[0.00728]	[0.00733]	[0.297]	[0.0201]
Male	0.0677***	3.061***	0.0822***	0.0822***	3.236***	0.0717*
	[0.0128]	[0.246]	[0.0158]	[0.0158]	[0.689]	[0.0330]
Age at admission	-0.00752***	0.0214*	-0.00712***	-0.00713***	-0.321***	-0.00885***
	[0.000407]	[0.0107]	[0.000447]	[0.000448]	[0.0217]	[0.000761]
Prior incarcerations	0.0487*** [0.00211]	0.278*** [0.0543]	0.0399*** [0.00285]	0.0399*** [0.00285]	1.413*** [0.132]	0.0537*** [0.00451]
Grid recommendation		0.402*** [0.0444]				
Mean, dept. var.	0.344	24.80	0.344	0.344	15.99	0.358
Points sampled	4-13	4-13	4-13	4-13	4-13	8-9
Pt. x severity contols?	No	No	No	Yes	No	No
Add. fixed effects?	No	No	No	No	No	No
Weighted recidivism?	No	No	No	No	Yes	No
Observations	17,373	17,373	17,373	17,373	17,373	3,759

Notes: In addition to meeting all the sampling requirements in col. (1) of Table I, all individuals in this sample were admitted to prison after 1995 and released before 2006, have "grid" points between four and thirteen and were sentenced to crimes with severity levels under five. See Section IV for a discussion of these restrictions. Besides those variables reported, all regressions include dummy variables for sentence length rounded to the nearest year and crime type (violent, property, drug, other). Cols. (2) through (6) also include dummy variables for "grid" points and "grid" crime severity level. Col. (4) includes a vector of variables $\mathbb{1}(crime\,severity = s) \times Points$ for each grid severity level s, where $\mathbb{1}$ is an indicator function. Col. (5) weights the recidivism measure by the severity of the recidivating charge, as described in Section III. Standard errors, clustered by the entries of the grid matrix, are in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

TABLE III
THE RELATIONSHIP BETWEEN RECIDIVISM, RECOMMENDED TIME SERVED AND ACTUAL TIME SERVED

	Dependent variable: Returned to prison within 36 months							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parole board recommendation	0.0367***	0.0376***	1.418**	0.0294*	0.0617*	0.0435**	0.0340***	0.0276*
	[0.0124]	[0.0130]	[0.552]	[0.0152]	[0.0373]	[0.0204]	[0.0127]	[0.0167]
Months served	-0.0339***	-0.0349***	-1.143**	-0.0336***	-0.0691**	-0.0435**	-0.0337***	-0.0320**
	[0.0128]	[0.0133]	[0.572]	[0.0128]	[0.0315]	[0.0206]	[0.0131]	[0.0142]
Original sentence				0.00391				0.00199
				[0.00469]				[0.00516]
Black							0.0665	0.0697
							[0.0431]	[0.0472]
Male							0.0456	0.0840
							[0.0802]	[0.0865]
Age at admission ÷100							-1.013***	
							[0.284]	
Prior incarcerations							0.0939***	
							[0.0248]	
Estimation model	Probit	Probit	OLS	Probit	Probit	Probit	Probit	Probit
Sample	All	All	All	All	36-mo. sent.	Burglary	All	All
Month-of-adm. FE?	No	Yes	No	No	No	No	No	No
Weigted recidivism	No	No	Yes	No	No	No	No	No
Additional controls	No	No	No	No	No	No	No	Yes
Observations	519	519	519	519	106	199	519	509

Notes: All individuals were released on March 18, 1981 as part the mass release described in Section V. They otherwise meet the sampling restrictions in Section III and have original sentences no greater than six years. Except for col. (3), all results are reported as marginal effects from probit regressions. Col. (2) includes fixed effects for the month of admission to prison, to control for potential seasonal effects. Col. (3) weights the binary recidivism variable by the severity of the recidivating charge (see Section III for further explanation). Col. (5) includes only those inmates sentences to exactly 36 months, while col (6) includes only those convicted of burglary. The "additional controls" in col. (8) include fixed effects for conviction offense, fixed effects for age at admission (rounded to the closest five years), fixed effects for the number of prior incarcerations, and fixed effects for employment status (with a separate effect for "missing" as there are many missing values). Standard errors in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

TABLE IV
RECIDIVISM RATES BEFORE AND AFTER THE NINETY-PERCENT REFORM

	Dependent var: Returned to prison within 36 months				
	(1)	(2)	(3)	(4)	(5)
Ninety-percent crime x After 1997	0.0533** [0.0214]	0.0650*** [0.0148]	0.0595*** [0.0118]	1.747*** [0.495]	0.0713*** [0.0145]
Ninety-percent crime	-0.0936*** [0.0210]	0.0366*** [0.0134]	0.0334* [0.0180]	1.451*** [0.487]	0.0466 [0.0359]
Male		0.0519*** [0.0114]	0.0396** [0.0169]	2.033*** [0.363]	0.0519*** [0.0114]
Black		0.0344*** [0.00648]	0.0316*** [0.00603]	1.979*** [0.304]	0.0344*** [0.00648]
Age at admission ÷100		-0.711*** [0.0328]	-0.681*** [0.0399]	-26.48*** [1.437]	-0.710*** [0.0329]
Prior incarcerations		0.0500*** [0.00254]	0.0513*** [0.00412]	2.047*** [0.142]	0.0500*** [0.00254]
Months served		-0.00275*** [0.000446]	-0.00290*** [0.000451]	-0.0656*** [0.0184]	-0.00274*** [0.000442]
Mean of dept. var.	0.219	0.219	0.208	10.73	0.219
Estimation model	Probit	Probit	Probit	OLS	Probit
Ex. low-sent. controls	No	No	Yes	No	No
Weighted recidivism?	No	No	No	Yes	No
Ninety-percent trend	No	No	No	No	Yes
Observations	30,481	30,480	17,437	30,480	30,480

Notes: All individuals in this sample were sentenced between 1993 and 2001, received sentences no greater than five years, and otherwise meet all other conditions listed in Section III. Except for col. (4), reported coefficients are from probit regressions reported as marginal effects. Col. (1) has no additional controls except year fixed effects; besides those listed, cols. (2) through (5) include all the Table II covariates: fixed effects for sentence-length (rounded to the nearest year), fixed effects for offense severity level (from the "grid"), violent and drug offense dummies, and months served. Col. (5) also adds a linear trend for ninety-percent crimes. Col. (3) weights the binary recidivism variable by the severity of the recidivating charge (see Section III for further explanation). Standard errors, clustered by conviction charge, are in brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

TABLE V
DISCIPLINARY-INFRACTION RATES BEFORE AND AFTER NINETY-PERCENT REFORM

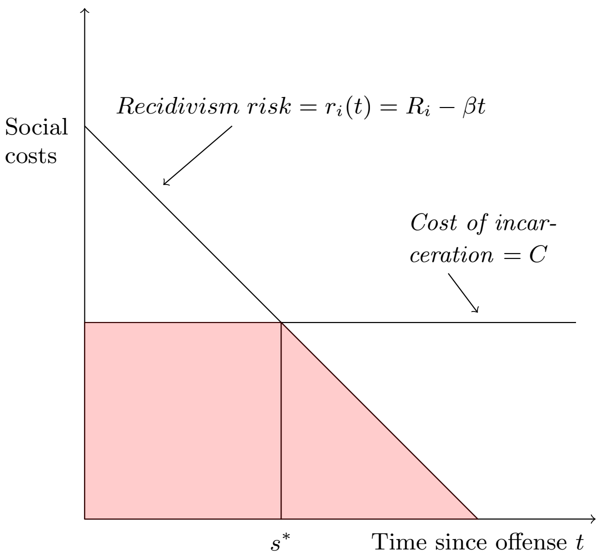
	Dependent variable: Measures of inmates' disciplinary infractions					
	Annual infraction rate			Any (0/1)	Annual rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Ninety-percent crime x After 1997	0.0812 [0.0999]	0.0539 [0.0697]	0.0980*** [0.0233]	0.0239** [0.0119]	0.0897*** [0.0300]	0.114*** [0.0374]
Ninety-percent crime	0.285** [0.142]	-0.0923 [0.0692]	-0.0691*** [0.0254]	-0.0248 [0.0194]	-0.134*** [0.0477]	-0.0503 [0.0743]
Male		0.0430 [0.0369]	0.0252 [0.0176]	0.00795 [0.00698]	0.0120 [0.0356]	0.0251 [0.0259]
Black		0.302*** [0.0270]	0.192*** [0.0114]	0.0680*** [0.00672]	0.197*** [0.0257]	0.192*** [0.0202]
Age at admission ÷100		-5.207*** [0.361]	-3.878*** [0.0576]	-1.469*** [0.0549]	-3.918*** [0.329]	-3.878*** [0.264]
Prior incarcerations		0.0196** [0.00811]	0.0196*** [0.00390]	0.0227*** [0.00271]	0.0273*** [0.00759]	0.0196*** [0.00593]
Months served		0.0181*** [0.00149]	0.0113*** [0.000380]	0.00725*** [0.000539]	0.0111*** [0.000976]	0.0113*** [0.000937]
Mean dept. var.	0.944	0.944	0.774	0.572	0.815	0.774
Outliers dropped	No	No	Yes	No	Yes	Yes
Ex. low-sent. controls	No	No	No	No	Yes	No
Ninety-percent trend	No	No	No	No	No	Yes
Observations	40,330	40,329	39,498	40,329	24,656	39,498

Notes: All individuals in this sample were sentenced between 1993 and 2001 and otherwise meet all the other conditions listed in Section III. Col. (1) has no controls except year fixed effects; cols. (2) through (6) include the addition controls listed in Table IV, with col. (6) adding a linear trend for the ninety-percent inmates. The dependent variable in col. (4) is an indicator variable for whether an inmate has incurred any disciplinary infractions during his prison term (an "extensive margin" measure). "Outliers dropped" indicates that outliers above the 98$^{\text{th}}$ percentile have been excluded. All regressions are estimated with OLS. Standard errors, clustered by conviction charge, in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

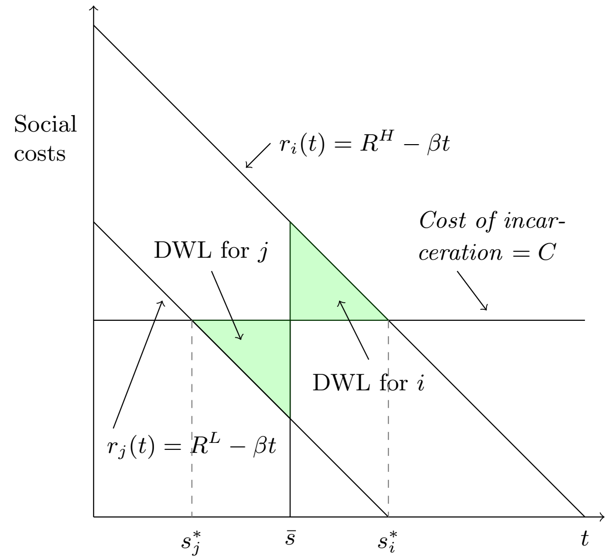
TABLE VI
PROGRAM-COMPLETION RATES BEFORE AND AFTER THE NINETY-PERCENT REFORM

	Dependent variable: Measures of inmates' completion of prison programs					
	Annual rate		Any (0/1)	Log of rate	Annual rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Ninety-percent crime x After 1997	-0.134***	-0.127***	-0.0325***	-0.158***	-0.0822***	-0.0619***
	[0.0309]	[0.0228]	[0.0100]	[0.0348]	[0.0222]	[0.0237]
Ninety-percent crime	0.0205	0.0555**	-0.00500	0.0962**	0.00384	0.0376
	[0.0236]	[0.0249]	[0.0137]	[0.0412]	[0.0257]	[0.0308]
Male		-0.238***	-0.0563***	-0.257***	-0.288***	-0.245***
		[0.0313]	[0.0124]	[0.0313]	[0.0459]	[0.0377]
Black		-0.0600***	-0.0219***	-0.0557***	-0.0556***	-0.0701***
		[0.00740]	[0.00504]	[0.0102]	[0.00875]	[0.00896]
Age at admission ÷100		0.260***	0.0544*	0.402***	0.257***	0.323***
		[0.0418]	[0.0321]	[0.0457]	[0.0529]	[0.0540]
Prior incarcerations		-0.0230***	-0.00347	-0.0410***	-0.0300***	-0.0268***
		[0.00248]	[0.00297]	[0.00339]	[0.00270]	[0.00339]
Months served		-0.000971**	0.00563***	-0.00926***	-0.0000453	-0.00227***
		[0.000420]	[0.000248]	[0.00111]	[0.000323]	[0.000472]
Mean dept. var.	0.581	0.581	0.664	-0.368	0.568	0.674
Ex. low-sent. controls	No	No	No	No	Yes	No
Years from 1993-2001	All	All	All	All	All	1995-2000
Observations	40,330	40,329	40,329	26,791	25,146	28,186

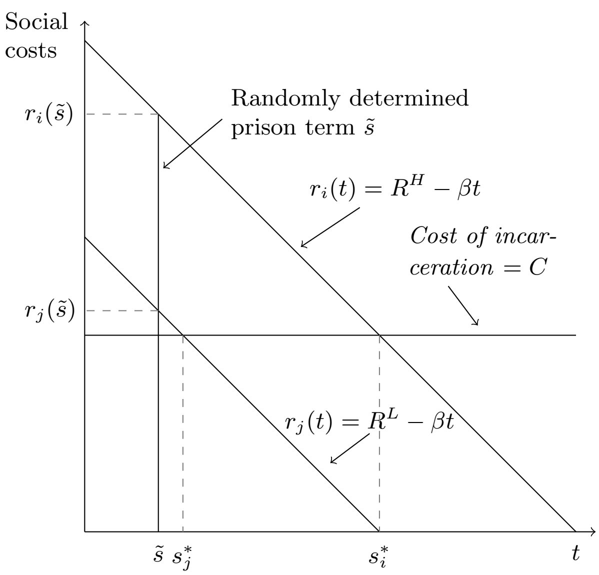
Notes: All individuals in this sample were sentenced between 1993 and 2001 and otherwise meet all the other conditions listed in Section III. Col. (1) has no controls except year fixed effects; cols. (2) through (6) include the addition controls listed in Table IV. The dependent variable in col. (3) is an indicator variable for whether an inmate has completed any programs during his prison term (an "extensive margin" measure) and the dependent variable in col. (4) is the natural log of the annual rate, conditional on having at least one infraction (an "intensive margin" measure). All regressions are estimated with OLS. Standard errors, clustered by conviction charge, in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$



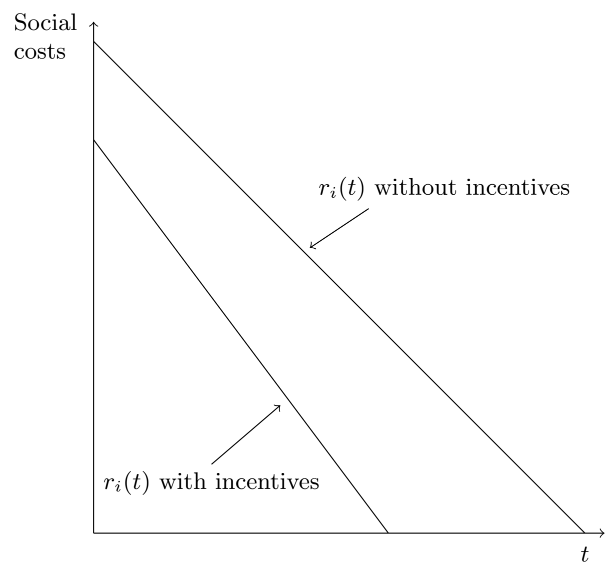
A Total social cost (incarceration costs plus future recidivism costs) at optimal time-served s^* for a linear recidivism-risk function $r_i(t) = R_i - \beta t$



B Dead-weight loss of a fixed release date $\bar{s}$ relative to optimal s_i^* for a high-risk ($R_i = R^H$) inmate and optimal s_j^* for low-risk ($R_j = R^L$) inmate



C Positive correlation between optimal time served s^* and observed recidivism $r(s)$ when actual time served s is fixed at $\tilde{s}$



D Possible effect of parole incentives on the relationship between time served and recidivism risk

Figure I

Social costs associated with optimal and fixed release dates

Notes: The flow expected social cost an individual i represents if free is denoted by $r_i(t)$. The shaded area in (A) represents total social cost (incarceration costs while in prison plus expected recidivism risk while free) and the shaded areas in (B) represent the social cost of a fixed release date $\bar{s}$ relative to setting $s_i = s_i^*$, the social-cost-minimizing sentence. See Section II and the Online Appendix for a fuller analysis.

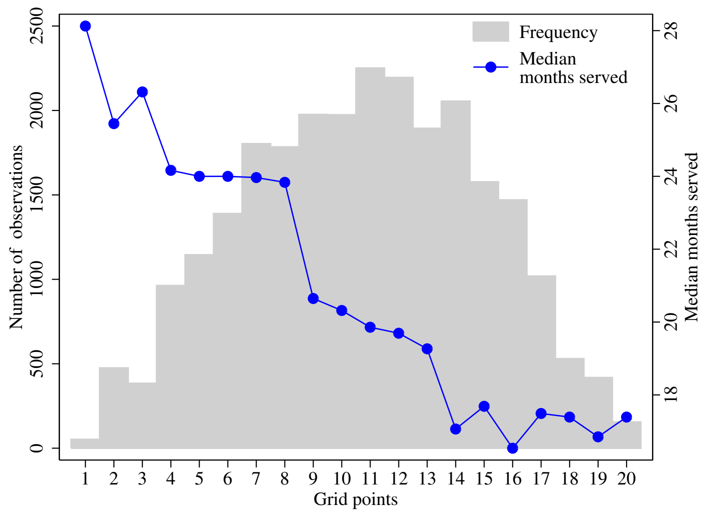


Figure II
Time served by point designations in the Georgia parole guidelines (the "grid")

Notes: All individuals in this sample were admitted to prison after 1995 and released before 2006, were convicted of offenses with a "grid" severity level below five, as well as meet all other sampling requirements in Section III.

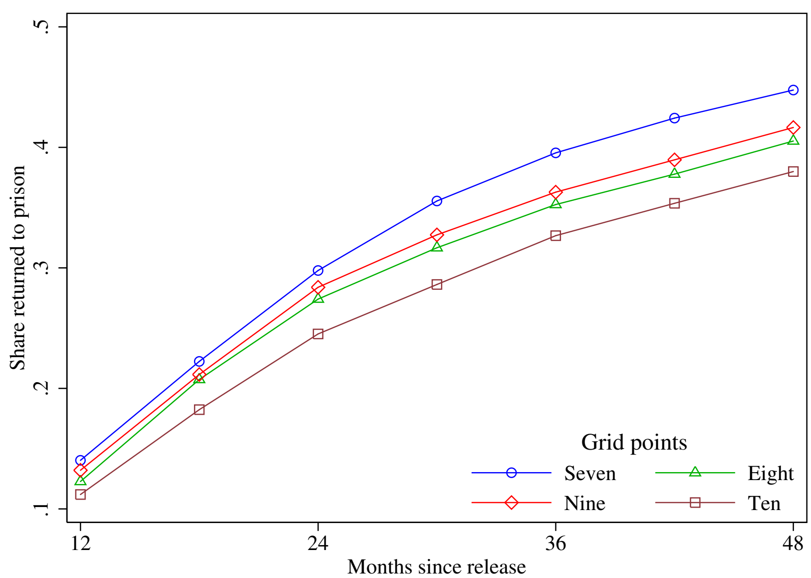
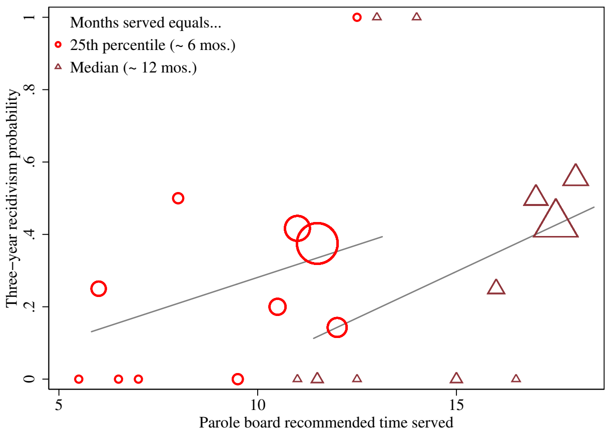
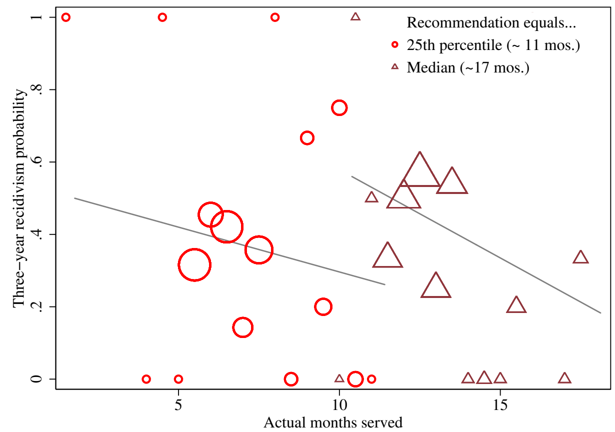


Figure III
Recidivism rates by "grid" points, above and below the first cut-off

Notes: All individuals in this sample were admitted to prison after 1995 and released before 2006, were convicted of offenses with a "grid" severity level below five, as well as meet all other sampling requirements in Section III. The first cut-off in the grid falls between eight and nine points.



A Relationship between recidivism and recommended time served, holding actual time served fixed



B Relationship between recidivism and actual time served, holding recommended time served fixed

Figure IV
Relationship between recidivism, recommended time served and actual time served

Notes: Two groups of inmates are sampled in Figure IV.A. Both meet the sampling criteria for the mass release experiment described in Section V, while the first group (68 observations) also served within one month of the 25th percentile of the actual months served distribution (6.2) and the second (58 observations) served within one month of the median of the distribution (12.1). For each of these groups, the scatter plot shows the recidivism rate averaged for each half-month bin of recommended time served (in order to avoid plotting all zeros and ones for the binary recidivism variable, though the fitted line is based on the raw, un-binned data). Similarly, the individuals included in Figure IV.B meet all the mass release sampling criteria and are either within one month of the 25th percentile of the recommended-time-served distribution (11.3) or within one month of the median (17.4). There are 95 observations with recommended time served within one month of 11.3 months and 98 observations within one month of 17.3 months. For each of these groups, the scatter plot shows the recidivism rate averaged for each half-month bin of actual time served (again, fitted lines are based on actual, unbinned observations). For both figures, the size of the marker is proportional to the number of observations in the bin.

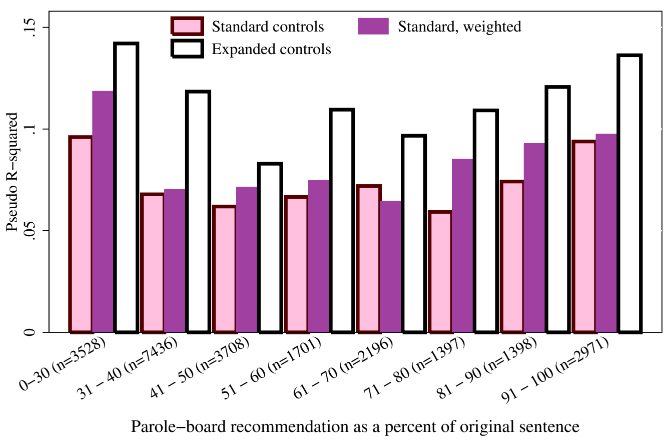


Figure V

Power of observables to predict recidivism

Notes: All observations meet the sampling restriction in Section III, though ninety-percent crimes are excluded as the parole board does not have full discretion over them. The R^2 values for the binary variables are "pseudo" R^2 values from probit regressions; for the weighted recidivism measure, standard R^2 values are plotted. "Standard controls" include gender, race, age at admission, prior incarcerations, and months served. "Expanded controls" include in addition interactions between gender, age and race, dummy variables for violent and drug crimes, "grid" crime severity level, controls for disciplinary infractions and program completions, and fixed effects for education and marital status.

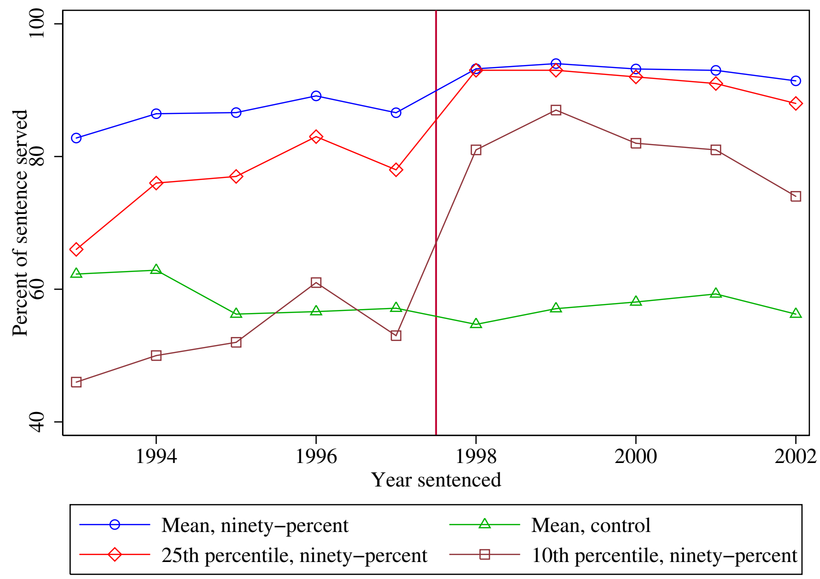
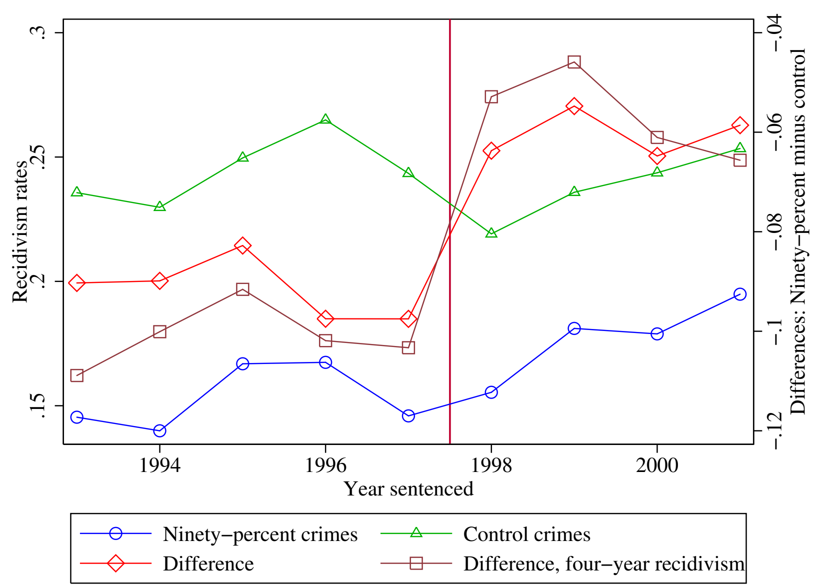


Figure VI
Percent of sentence served for "ninety-percent" and control crimes

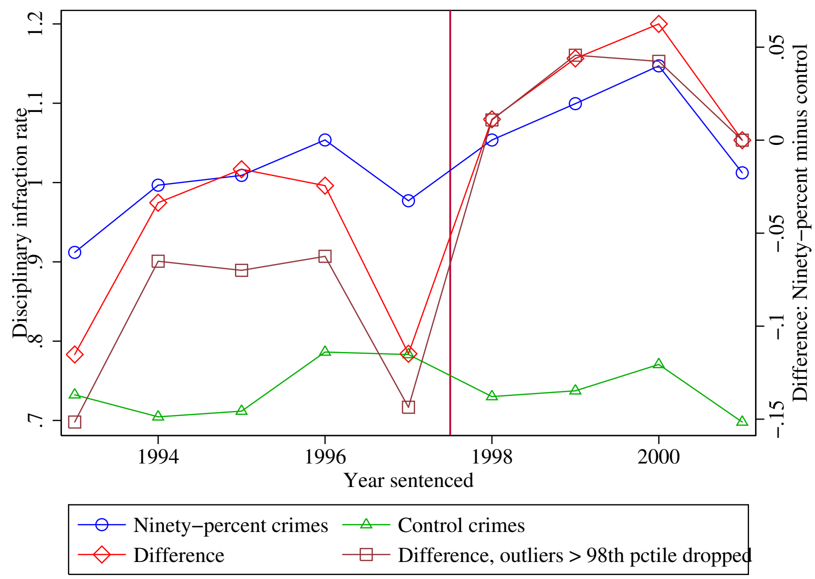
Notes: All individuals in this sample meet the sampling criteria in Section VI. In addition, for this graph (but not for the regression analysis) they were sentenced to no more than eight years, so that their sentence would have expired by the Spring of 2011 when my version of the Georgia data end and thus their share of sentence served is well-defined (though including those with longer sentences produces a very similar picture). Individuals convicted of ninety-percent crimes (child molestation, statutory rape, aggravated assault or battery, car-jacking, attempted murder, assault on police officer, incest, attempted rape, manslaughter, or robbery) after December 31st, 1997 were required serve at least ninety percent of their sentence. The control group consists of individuals convicted of other (generally less serious) crimes who otherwise meet the sampling criteria.

Figure VII
Three-year recidivism rates before and after the "ninety-percent" reform



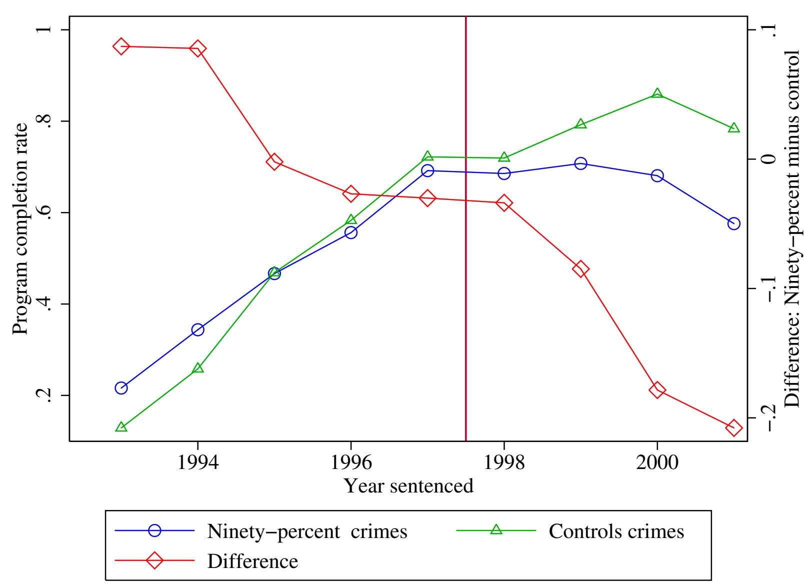
Notes: All individuals in this figure meet the sampling criteria in Section VI and in addition have sentences no longer than five years. See Figure VI for a list of the ninety-percent crimes.

Figure VIII
Disciplinary-infraction rates before and after the “ninety-percent” reform



Notes: All individuals meet the sampling criteria in Section VI. See Figure VI for a list of the ninety-percent crimes. Unlike the recidivism sample, inmates who have not yet completed their sentences are included and inmates with sentences of greater than five years and not excluded. In this figure, observations with infraction rates above the 99^{th} percentile are excluded, and the last series plotted excludes observations with infraction rates above the 98^{th} percentile.

Figure IX
Program-completion rates before and after the “ninety-percent” reform



Notes: All individuals meet the sampling criteria in Section VI. Unlike the recidivism sample, inmates who have not yet completed their sentences are included and inmates with sentences of greater than five years and not excluded. See Figure VI for a list of the ninety-percent crimes.

Online Appendix A: A more formal treatment of the framework in Section II

In this Appendix, I present a more formal version of the framework illustrated graphically in Section II. I write this section so that it can stand alone, and thus parts of Section II are repeated here (though the figures are not). Note that while this section is a more formal treatment of the framework, it still should be viewed more as illustrative than as a complete model encompassing all aspects of the release decision.

Overview

Consider an individual who has been convicted of a crime and sentenced to prison. The social cost of his trial and (if he is guilty) the crime itself are already sunk, but he may still impose future costs on society: first, the food, shelter, and supervision the state must provide while he is incarcerated, and second, the net social cost he generates (e.g., via future recidivism) after his release. While there are certainly other costs that release policy might affect—some of which I discuss at the end of this section—these are the two central costs on which I focus. When I refer later in this section to a "cost-minimizing" policy, for example, it is with respect to these two costs.

In this framework, release policies can affect these costs in two key ways. First, policies can be more or less *allocatively efficient*, that is, they can vary in how well they balance the two competing concerns of incarceration costs and recidivism risk. In the limit, keeping someone in prison forever will eliminate any cost associated with future recidivism but would itself be enormously expensive. If time in prison reduces recidivism risk upon release—a key assumption in the framework and one that I establish empirically in Section IV—then individuals with greater recidivism risk should remain in prison longer and those with limited risk should be granted early release. Much of this section is devoted to deriving an empirical test for determining whether release decisions are allocatively efficient.

While allocative efficiency is concerned with finding the point in time at which the incarceration cost curve meets the recidivism cost curve, the second way that release policies affect total costs is by shifting the curves themselves. For example, if a release regime incentivizes better conduct among inmates while in prison, it could lower incarceration costs; if it encouraged them to invest in reducing their own recidivism risk, it could lower future recidivism costs.

Notation and key assumptions

Let the flow social cost of incarceration equal C. While the daily cost of incarcerating an individual likely varies both across individuals (e.g., older inmates have higher health care costs) and within individuals (e.g., newly-arrived inmates might have greater disciplinary issues), I assume that this variation is small relative to its mean. Any cost or benefit of incarceration that does not depend on the individual can be included in the total social cost C. For example, if longer average sentences deter potential future criminals then the level of C will be lower.

While incarceration costs are assumed to be constant, the flow social cost that an offender, if free, would impose on society is allowed to vary both across individuals i and within individuals over time. A relationship that is central to the framework is $r_i(t)$, how the social cost imposed by individual i if free varies with time t since the beginning of his incarceration. Note that $r_i(t)$ could potentially be negative if, say, through the taxes he would pay or the assistance he would provide to family members, the individual's activities if free would provide positive net social surplus. Because I will generally think of the social cost an individual imposes if free as being driven by recidivism risk, I refer to $r_i(t)$ as an individual's "expected risk."

I make several baseline assumptions regarding $r_i(t)$, though discuss later how relaxing them affects the analysis. First, I assume that, at least for sufficiently large t, $r_i'(t) < 0$, so that individuals become less of a risk with time t since the beginning of their incarceration, a claim established empirically in Sections IV and V.[48] There are several reasons that one might assume *a priori* that expected risk falls with time. First, criminal activity may be in part triggered by a heightened state of emotion or an unusually difficult period in an individual's life, so recidivism risk might naturally fall as the person's circumstances revert to their usual state. Second, criminal propensity is known to fall with age (see, e.g., Hirschi and Gottfredson 1983). Note that if instead prison time generally *increased* recidivism risk, optimal punishment schemes would look very different from either their current form or the form they take in this framework: to a first approximation, individuals below a certain risk threshold at the time of their sentencing would receive no prison time and those above it would be sentenced to life.[49]

Second, $r_i(t)$ depends only on the amount of time t since the start of incarceration, regardless of whether that time is spent in prison or free. In other words, I assume that the rate of rehabilitation is the same inside and outside of prison. This assumption follows naturally if mean reversion or aging is the main driver of rehabilitation. I discuss relaxing this assumption at the end of the section.

Finally, for simplicity I assume no discounting—for example, a dollar in incarceration costs today is no more costly than a dollar tomorrow.

Analysis related to allocative efficiency

Given the notation and assumptions already laid out, the optimization problem can now be specified. Time served in prison s_i should be set to minimize the total cost (both in prison and out of prison) associated with offender i:

$$\min_{s_i} \quad \overbrace{Cs_i}^{\text{Incarceration costs}} \quad + \overbrace{\int_{s_i}^{\infty} r_i(t)\,dt}^{\text{Recidivism costs}}. \tag{1}$$

The first result demonstrates that cost-minimization requires that, outside of corner solutions, each individual i be released when his expected risk $r_i(t)$ equals C.

Proposition 1. For all $r_i(t)$ such that $r_i(0) \geq C$ and $lim_{t\to\infty} r_i(t) < C$, the sum cost of incarceration and future recidivism are minimized when the time served s_i by individual i satisfies $r_i(s_i) = C$.

Proof. This result follows directly from the cost-minimization problem. Parole boards seek to minimimize the expression in (1):

$$Cs_i + \int_{s_i}^{\infty} r_i(t)\,dt.$$

By the fundamental theorem of calculus, the first-order condition is:

$$C - r_i(s) = 0.$$

Thus, for all $r_i(t)$ such that $r_i(0) \geq C$ and $lim_{t\to\infty} r_i(t) < C$, the cost-minimizing solution is to set

[48] Note that if recidivism risk temporarily rises upon incarceration but then fall with time in prison, all results hold in this section still hold.

[49] To see this, note that if a prisoner's risk at the moment of sentencing were higher than the incarceration cost, then it will be higher still after a day in prison, and higher still the next day, suggesting it would never be optimal to release him. Of course, this simple example abstracts from other considerations such as general deterrence.

s_i so that $r_i(s_i) = C$. ∎

Intuitively, for all interior solutions to the minimization problem in (1), time served s_i is set to equate the marginal social benefit and marginal social cost of an extra day of incarceration.[50] The marginal cost of incarcerating person i is C, and the marginal benefit is equal to *the social cost that would be incurred had he been free*, that is, r_i. Therefore, the optimal s_i is the point at which marginal social benefit $r_i(s_i)$ equals marginal social cost C. Figure I.A illustrates this result.

To focus on the general intuition, I assume for the rest of the section that expected risk $r_i(t)$ can be approximated by a linear function $r_i(t) = R_i - \beta_i t$, where R_i, initial recidivism risk, and β_i, the rehabilitation rate, can vary for each individual. This simplification yields the following lemma:

Lemma 1. If expected risk $r_i(t) = R_i - \beta_i t$, the optimal time served s_i^* is a positive function of initial risk R_i and a negative function of the rehabilitation rate β_i.

Proof. From Proposition 1, the optimal s_i^* solves $r_i(s) = C$. Therefore, $R_i - \beta_i s = C$, which yields the following expression for s_i^*:

$$s_i^* = \frac{R_i - C}{\beta_i}.$$

Therefore, the optimal time served is a positive function of initial risk R_i and a negative function of the rehabilitation rate β_i. ∎

As cost-minimization requires that prison space be conserved for individuals with the greatest risk, individuals who either enter prison with very low initial risk or very quickly lower their expected risk below the threshold C will gain an earlier release. Note that this result holds regardless of the level of C. As noted in Section II, one might think that ex-ante general deterrence is a benefit of prison time and thus the net cost of incarceration would be lower than merely considering the costs of food, shelter and supervision. If C is lowered in such a manner, allocative efficiency still requires that longer terms are allocated to higher-risk inmates.

Empirical implications

Given that R_i, β_i, and C will be unobserved to the researcher, neither of these results readily lend themselves to empirical tests. In particular, it is important to note that expected risk $r_i(t)$ is not generally observed in the data—most obviously, all inmates in prison at time t have some expected probability of recidivism $r_i(t)$ were they free, but because they are incarcerated they are not at risk of recidivating and $r_i(t)$ is unobserved. As is typical in recidivism studies, the empirical work will focus on modeling the probability that an individual commits a crime within certain windows of time since his release. Therefore, the binary measure *Recidivate* used in almost all such studies can be thought of as following a latent model such as $P(Recidivate) = \Phi(r_i(s_i) + \epsilon_i > \kappa)$, where s_i is the length of individual i's incarceration. That is, the empirical measure of recidivism will be a function of an individual's expected risk the moment he leaves prison (when $t = s_i$). The next result yields a very specific prediction regarding the empirical relationship between the allocatively-efficient level of time served s_i^*, actual time served s_i and *observed* recidivism $r_i(s_i)$.

[50] The two conditions for an interior solution are that $r_i(0) \geq C$ and $lim_{t\to\infty} r_i(t) < C$. The first condition requires that at the moment of incarceration inmates' expected risk exceeds the cost of their incarceration. In practice, given that offenders whom judges deem as posing minimal risk are typically assigned to probation rather than incarceration, parole boards will typically only review cases of individuals who meet this initial standard. The second condition requires that individuals' risk eventually falls below C; given that very elderly offenders likely present minimal risk, this condition also appears reasonable.

Proposition 2. Assuming $r_i(t) = R_i - \beta_i t$, then $r_i(s_i) = C + \beta_i s_i^* - \beta_i s_i$, where s_i^* is the allocatively-efficient level of time served for individual i and s_i is *actual* time served by i.

Proof. When $r_i(t) = R_i - \beta_i t$, the social-cost-minimizing s_i, s_i^*, is given by solving $R_i - \beta_i s_i^* = C$. R_i can thus be written as $C + \beta_i s_i^*$. Substituting this expression into the expected risk function $r_i(t) = R_i - \beta_i t$ and setting $t = s_i$ yields $r_i(s_i) = C + \beta_i s_i^* - \beta s_i$. ■

This proposition is central to formulating some of the empirical tests used later in the paper, so it is worth further discussion. It gives a very specific condition consistent with perfect allocative efficiency, and thus a benchmark for comparing how close actual release policies come to allocative efficiency. Consider, for example, a parole board that is, essentially, flipping coins to make its recommendation $\hat{s}_i$. This decision will be uncorrelated with either initial or actual observed recidivism risk, and hence the coefficient β_1 on $\hat{s}_i$ in the regression equation $P(Recidivate) = \alpha + \beta_1 \hat{s}_i + \beta_2 s_i$ would be zero. By the same logic, if parole boards systematically recommend those with *greater* risk to *shorter* terms, then the coefficient on the recommendation $\hat{s}_i$ would be negative.

In contrast, when parole boards assign longer terms to those with greater recidivism risk, the coefficient on recommended time served $\hat{s}_i$ will be positive. Moreover, when they do so in a manner that perfectly offsets inmates' recidivism risk (that is, when $\hat{s}_i = s_i^*$), then Proposition 2 implies that $\beta_1 = -\beta_2$.

While Proposition 2 provides a condition for allocative efficiency, it does not give a sense for how important it is in reducing total incarceration and recidivism costs. The following result compares total incarceration and recidivism costs under an allocatively efficient discretionary regime and an optimal fixed-sentence regime (that is, a fixed-sentence regime that chooses the *fixed* sentence that minimizes total incarceration and recidivism costs). In fact, this estimation holds even when both the discretionary and fixed-sentence regime incorrectly estimate the level of incarceration costs.

Proposition 3. If $r_i(t) = R_i - \beta t$, then the difference in expected incarceration and recidivism costs between a fixed-sentence regime that sets an optimal fixed prison term and an allocatively-efficient discretionary regime is $\frac{Var(R_i)}{2\beta}$.

Proof. To prove the more general result assume that instead of observing C both regimes instead believe that incarceration costs are equal to $C' = C + \Delta C$. In this case, following the algebra in the Lemma, the parole board sets $s_i = \frac{R_i - (C + \Delta C)}{2\beta}$. The fixed-sentence regime seeks the fixed prison term $\bar{s}$ that minimizes expected costs given $R_i \sim f(R)$. As such, they seek to minimize:

$$(C + \Delta C)\bar{s} + \int\limits_{R_i \in R} \int\limits_{\bar{s}}^{T} (R_i - \beta t)\, f(R_i)\, dt\, dR_i.$$

The first-order condition with respect to $\bar{s}$ is given by:

$$
\begin{aligned}
&C + \Delta C + \frac{d}{d\bar{s}} \int\limits_{R_i \in R} \left(R_i T - \frac{\beta T^2}{2} - R_i \bar{s} + \frac{\beta \bar{s}^2}{2} \right) f(R_i)\, dR_i = 0 \Longleftrightarrow \\
&C + \Delta C + \frac{d}{d\bar{s}} \left(\bar{R} T - \frac{\beta T^2}{2} - \bar{R} \bar{s} + \frac{\beta \bar{s}^2}{2} \right) = 0 \Longleftrightarrow \\
&C + \Delta C - \bar{R} + \beta \bar{s} = 0 \Longleftrightarrow \\
&\bar{s} = \frac{\bar{R} - (C + \Delta C)}{\beta}
\end{aligned}
$$

Hence, one can write the difference between the costs under the fixed-sentence versus parole regimes as:

$$
\begin{aligned}
Cost_i(\bar{s}) - Cost_i(s_i) &= \overbrace{C\frac{\bar{R} - (C+\Delta C)}{\beta} - C\frac{R_i - (C+\Delta C)}{\beta}}^{\text{Difference in incarceration costs}} + \overbrace{\int\limits_{\frac{\bar{R}-(C+\Delta C)}{\beta}}^{T} (R_i - \beta t)\, dt - \int\limits_{\frac{R_i-(C+\Delta C)}{\beta}}^{T} (R_i - \beta t)\, dt}^{\text{Difference in recidivism costs}} \\
&= \frac{C(\bar{R} - R_i)}{\beta} + \int\limits_{\frac{\bar{R}-(C+\Delta C)}{\beta}}^{\frac{R_i-(C+\Delta C)}{\beta}} (R_i - \beta t)\, dt \\
&= \frac{C(\bar{R} - R_i)}{\beta} + R_i \left(\frac{R_i - (C+\Delta C)}{\beta} - \frac{\bar{R} - (C+\Delta C)}{\beta} \right) - \\
&\quad \frac{\beta}{2} \left(\left(\frac{R_i - (C+\Delta C)}{\beta} \right)^2 - \left(\frac{\bar{R} - (C+\Delta C)}{\beta} \right)^2 \right) \\
&= \frac{C(\bar{R} - R_i)}{\beta} + R_i \frac{R_i - \bar{R}}{\beta} - \frac{1}{2\beta} \left(R_i^2 - \bar{R}^2 - 2(R_i - \bar{R})(C + \Delta C) \right) \\
&= \frac{2C(\bar{R} - R_i) + 2R_i(R_i - \bar{R}) - R_i^2 + \bar{R}^2 - 2C(\bar{R} - R_i) + (R_i - \bar{R})\Delta C}{2\beta} \\
&= \frac{(R_i - \bar{R})^2 + \Delta C(R_i - \bar{R})}{2\beta}
\end{aligned}
$$

Thus,

$$
\begin{aligned}
\mathbb{E}[Cost_i(\bar{s}) - Cost_i(s_i)] &= \frac{\mathbb{E}[(R_i - \bar{R})^2] - \Delta C \mathbb{E}[R_i - \bar{R}]}{2\beta} \\
&= \frac{Var(R_i)}{2\beta} \qquad \blacksquare
\end{aligned}
$$

Figure I.B provides the basic intuition for the result and depicts the deadweight loss of a policy that assigns all inmates the same optimal fixed sentence relative to an allocatively-efficient discretionary regime. The greater the variance in R_i, the greater the height of the deadweight-loss triangle; the greater β, the smaller the difference between the discretionary terms s_i and the fixed term $\bar{s}$ and the smaller the base of the triangle. As Proposition 3 requires additional functional form assumptions, when I use it later in the paper to derive cost estimates I will emphasize the many caveats entailed. However, it allows at least a ballpark estimate of the importance of allocative efficiency in determining total incarceration and recidivism costs.

Analysis related to shifts in the cost curves

So far, I have assumed that individuals' recidivism and incarceration cost curves are fixed, and thus the only goal of a release regime is to find the point in time at which the curves meet. However, the choice of the release regime might itself make inmates take actions that shift these curves. If inmates believe that parole boards positively condition time served on recidivism risk, then, assuming they prefer an earlier release to a later one, they will have a greater incentive to lower their actual recidivism risk so as to gain an early release relative to a regime in which sentences are fixed upon admission.

Such an effect is illustrated in Figure 1(d). While time in prison is assumed to lower recidivism risk holding constant the choice of release regime, the incentives embodied in the release regime itself can shift the negative recidivism-time-served relationship. As depicted in the figure, even though time in prison lowers recidivism under both regimes, a shorter prison term in a regime that creates incentives for rehabilitation could lead to lower observed recidivism than a longer term in a regime without such incentives. The hope of early parole would seem to create such incentives, an empirical question I take up in Section VI.

Revisiting assumptions

This final subsection revisits some of the main simplifying assumptions of the model. First, I consider only two types of release regimes—a traditional, discretionary parole regime and a fixed-sentence regime—as these are the two I observe in my data and most states have some form of these two policies. However, there are other ways of varying the initial sentence that do not involve discretion. For example, time served could be reduced by a certain amount for good behavior, or for completing drug treatment programs or GED courses.[51] As such, the framework and empirical results can speak at most to a policy of discretionary parole versus a particular rules-based regime—a fixed-sentence policy—but not to the discretion-verus-rules debate more generally.

Second, as noted earlier, I assume that the relationship between recidivism risk and elapsed time does not change once an individual leaves prison. Put differently, an individual's recidivism begins to fall at some rate and I assume that process continues in a similar manner after he leaves prison. Of course, time in prison might either enhance or inhibit this rehabilitation process relative to spending that same time elsewhere. For example, prisoners may have the chance to enroll in vocational courses or other rehabilitative programs otherwise unavailable to them, so for some the negative effect of time on recidivism risk might be greater in prison than outside. In contrast, some inmates might gain "criminal capital," as in Bayer et al. (2009), so prison could diminish the rehabilitative process.

If the rate at which an inmate's recidivism rate falls with time is greater in prison, then parole boards will want to release an inmate slightly *after* his recidivism risk hits the C threshold, as this enhanced rehabilitation effect lowers the social cost of incarceration and thus increases the optimal incarceration spell. Conversely, if an inmate would rehabilitate more quickly if free, then the optimal spell is shorter. Because little if any research speaks to whether time in prison increases or decreases future criminal activity *relative to the mere passage of time*, I make the simplifying

[51] Such a formulaic approach is generally referred to as "good" or "earned time." Georgia prisoners do not have good or earned time. In those states that do have good/earned time, the maximum sentence reductions possible are typically small relative to the ability the parole board has traditionally enjoyed to reduce sentences (Reitz 2012).

assumption that any difference is small.[52]

Third, discretion could lead to discrimination. As noted earlier, while a new "get tough" attitude in the 1970s among the political right helped lead to the abolition of parole (Petersilia 1999), so too did suspicion among the political left that parole boards were discriminatory (Cullen 2004). The net effect of parole board discretion on discrimination is difficult to determine. If parole boards use their discretion in part to correct unjustified sentencing disparities across inmates, then eliminating parole boards' authority might only enhance the effective discretion of judges and other "up-stream" agents and the net effect would depend on whether discrimination among police, district attorneys and judges is worse than that of parole boards.[53] Moreover, if some of the disparities in punishment are due to statistical discrimination, then because parole boards have substantially more information than do earlier actors, they may be less prone to discrimination. At least in Georgia, parole files include not only information on the case gathered from the police and courts, but inmate interviews, prison diagnostic data, and reports on the prisoner's behavior and results from any rehabiliatitive programs.[54]

Fourth, I have assumed that while the discretion-versus-rules margin affects the variance of prison terms there is no systematic effect on mean sentence length, whereas one of the main arguments against parole boards was that they were too lenient and in general released prisoners too quickly. How this assumption affects the social-cost calculations depends on the optimal average prison term. If prison terms are generally longer than optimal, then "lenient" parole boards are actually beneficial, whereas the opposite is true if prison terms are on average too short. As there is no consensus on optimal average sentence length, it is difficult to assess the direction of any bias caused by this assumption.

Finally, I have generally taken the size of the original prison population as given and have thus assumed the decision between rules and discretion has no general deterrent effect. Given that the debate over whether marginal increases in sentence length deter crime remains largely unsettled, it seems unlikely that, holding constant average sentence length, the discretion-versus-rules margin would have a large effect.[55] Indeed, the little direct work that exists finds no consistent effects.[56] On

[52]The likely reason no research speaks to this question is that it is difficult to imagine a credible empirical strategy. Assume there are two identical individuals, and randomly assign individual i to a one-year prison sentence and set j free. What one would like to do is to compare their criminality in one year, after j has spent a year free and i a year in prison. However, j may not be observable at this point, as he may have committed a crime and been sentenced to prison over the course of the year. And comparing i only to those j's who are still observable after one year introduces obvious selection bias.

[53]See Abrams et al. (forthcoming) for recent work on racial disparities in sentencing among Illinois judges. Of course, parole boards could exacerbate excessive sentences instead of offsetting them. Bernhardt et al. (2009) present a model of parole with such a result even absent any discriminatory tendencies among judges or parole boards. In their model, if two otherwise identical individuals have different sentences, a parole board would release the one with the shorter sentence sooner since the option value related to learning about his type is lower.

[54]See `http://www.pap.state.ga.us/opencms/export/sites/default/index.html?page=index.html` for a full description of the items contained in an inmates' parole file. Of course, gathering this information requires effort and expense, a cost not included in the model. However, the annual budget for the entire parole agency in Georgia is \$45 million—compared to \$1.1 billion for corrections and probation—with the large majority going to parole *supervision*, which, as footnote 6 in the Introduction noted, exists in fixed-sentence regimes as well.

[55]For a general review of empirical estimates of the deterrent effect of expected punishment, see Levitt (2004). Kessler and Levitt (1999) use aggregate crime rates to show that criminals are indeed sensitive to expected punishment as they substitute away from crimes whose sentences have been raised. However, Lee and McCrary (2005) use individual panel data to demonstrate that the criminal activity of teenagers is not responsive to even large, salient increases in sanctions—their arrest probability falls by only two percent when they turn 18 despite a 230 percent increase in expected punishment conditional on arrest.

[56]Marvell and Moody (1996) find no deterrence effect of truth-in-sentencing laws using state panel data from the early 1970s to the early 1990s. Shepherd (2002) uses county panel data from 1984-1996 and finds that truth-in-sentencing laws have a large deterrence effect on violent crime. I find the Marvel and Moody study more convincing

the one hand, a risk-averse individual would dislike the uncertainty of parole, so holding average punishment constant a parole regime would have a greater deterrent effect than a rules-based regime. On the other hand, if potential criminals are overly confident in their ability to charm the parole board, then a rules-based regime is a better deterrent.

Online Appendix B: Allocative-efficiency calculation

Proposition 3 in Online Appendix A provides a formula for the expected difference in total costs between a fixed-sentence regime and an allocatively-efficient discretionary regime: $\frac{Var(R_i)}{2\beta}$. Recall that $r_i(t) = R_i - \beta t$ has always referred to expected *cost* of recidivism, whereas most of the empirical work has focused on predicting expected the *probability* of recidivism. I assume that one can approximate expected recidivism cost by $r_i(t) = \omega\gamma p_i(t)$, where γ is the social cost of the average crime committed by a recidivist, ω is a weight to account for the cost of unreported and unpunished crimes committed by a recidivist, and $p_i(t)$ is the expected recidivism probability for person i after serving time t in prison. This formulation is not innocuous—one might think, for example, that crimes with higher recidivism rates might have higher or lower social costs than the average crime. With some abuse of notation, I let $p_i(t) = P_i - \beta t$, where β now refers to the decay of recidivism probability (as opposed to expected cost of recidivism) with time.

These assumptions allow me to write the recidivism cost associated with releasing person i at time t as $\omega\gamma(P_i - \beta t)$. The parole board will set individual i's term equal to $\frac{(P_i - C/(\omega\gamma))}{\beta}$ and a fixed-sentence regime will set the fixed term at $\frac{(\bar{P} - C/(\omega\gamma))}{\beta}$. Via very similar algebra to that in Proposition 3, the expected difference in costs between the discretionary and fixed-sentence regime is equal to $\omega\gamma\frac{Var(P_i)}{2\beta}$.

I estimate the variance of initial recidivism probabilities using the results from the mass release experiment. I first estimate an OLS regression with the covariates included in col. (7) of Table III, which yields an estimate for β, the coefficient on time served t, of -0.0332. For each individual, I use these results to predict expected recidivism upon release, $\hat{p}_i(t_i)$. To estimate the *initial* probability of recidivism, I subtract $\hat{\beta}t_i$ from the predicted probability upon release $\hat{p}_i(t_i)$. As $\hat{\beta}$ is negative, this step just adds back the decrease in recidivism risk due to the time served t_i to obtain a rough estimate for the probability of recidivism for each individual at $t = 0$, that is, P_i.

One final step is required. In the model in Online Appendix A, all costs are flow costs. Thus, one needs a measure of recidivism risk over the same period of time in which t is measured, that is, per month. As t is measured in months and recidivism risk is measured over three years, I let $p_i^m(t) = \frac{p_i(t)}{36} = \frac{P_i - \beta t}{36} = P_i^m - \beta^m t$, where $P_i^m = P_i/36$ and $\beta^m = \beta/36$.[57] That is, P_i^m is the initial risk of recidivating over a one-month period and β^m is the effect of spending a month in prison on the one-month recidivism rate.

This procedure yields the following estimation:

$$\frac{Var(\hat{P}_i^m)}{2\hat{\beta}^m} = \frac{0.00009019}{\frac{2*.0332}{36}} = 0.0489. \tag{2}$$

largely because of their inclusion of state-specific trends. As Sabol et al. (2002) argues, truth-in-sentencing reforms are usually passed during periods of heavy legislative activity on other tough-on-crime reforms, making state-specific trends essential in teasing out the specific effect of truth-in-sentencing laws on crime rates.

[57]This approximation assumes that the recidivism rate is relatively constant across time during the first three years following release and thus that the probability of recidivism within x months of release is $\frac{x}{y}$ times the probability of recidivism within y months of release. Figure III suggests this approximation is reasonable—while risk diminishes over time in that figure, much of the flattening appears between 36 and 48 months. In the case of the mass release sample, the one-year recidivism rate is 0.111, very close to one-third the value of the three-year recidivism rate of 0.371.

Estimating ω and γ

Let O be the set of all offenses for which recidivists are charged, and S_j the share of these offenses accounted for by offense j. I can calculate values for each S_j (where the j's include each of the major FBI crime categories, along with "other") from my data. Let $Cost_j$ be the social cost associated with offense j, which I take from the literature. Following the literature, I assume the cost of all non-FBI index crimes is zero, which will underestimate γ.

When an inmate recidivates with offense j, two conditions are implied. First, he must have committed the offense (for simplicity, I assume that any recidivist charged with an offense did indeed commit that offense). Second, he must have been *found guilty by law enforcement.* He thus imposes social cost $Cost_j$ associated with the offense j for which he was "caught" (which is observed in the data), as well as the social costs associated with (unobserved) offenses he committed but for which he was not caught. To account for the social cost of unobserved offenses, I weight each observed offense of type j by the inverse of the product of: (1) the probability police are notified about offense j (known in the literature as the "notification rate") and the probability that police make an arrest given a report of offense j (known as the "clearance rate"). This weight, ω, should roughly scale up each observed offense by the inverse of the probability of being "caught." Thus, the average cost imposed by a recidivist can be written:

$$\omega\gamma = \sum_{j \in O} S_j \left(\frac{1}{Notify_j}\right)\left(\frac{1}{Clearance_j}\right) Cost_j.$$

Using values for $Cost_j$ from Cohen (1988) and Miller et al. (1993) (as tabulated in Levitt 1996 and adjusted to 2010 dollars) and for $Notify_j$ and $Clearance_j$ from, respectively, the National Crime Victimization Surveys (Bureau of Justice Statistics 2011) and the FBI's Uniform Crime reports (Federal Bureau of Investigation 2011), I find that $\omega\gamma \approx \$80,100$. Using the estimate in equation (2), the average cost of fixed-sentences versus discretion with respect to allocative efficiency is roughly $\$80,100 * 0.0489 \approx \$3,917$.

Finally, I compare this value to the average total cost associated with a prisoner—that is, his incarceration and expected recidivism costs. Based on the summary statistics in col. (1) of Table I, the average prisoner served 32 months and leaves prison with a three-year recidivism rate of 0.29. Assuming that the annual cost of incarceration is \$29,500 (Bureau of Justice Statistics 2004, adjusted to 2010 dollars) and excluding recidivism costs beyond the three-year window, the average prisoner imposes costs of $\$29,500 * ({}^{32}/{}_{12}) + 0.29 * \$80,100 \approx \$101,900$. Hence, forgoing the allocative-efficiency benefits of parole increases total per-prisoner costs by roughly ${}^{\$3,917}/{}_{\$101,900} =$ 3.8 percent.

Caveats

This calculation represents my best approximation of the benefits of allocative efficiency, but several caveats should be emphasized. First, compared to the cost estimate in Section VII of removing the incentive effects of discretionary parole, this calculation requires more restrictive functional form assumptions (in particular, that the relationship between recidivism risk and time served is roughly linear). Second, my estimate of $Var(R_i)$ is likely understated, which would tend to underestimate the gains from allocative efficiency. I control for a standard set of criminal and demographic variables, but it is likely that my model is not capturing all predictable variation. Third, I have been assuming in these calculations that all allocative efficiency comes from parole board decisions and not sentencing decisions, which would likely overstate the effect of parole boards on allocative efficiency. Given the results in Table III, showing that the relationship between sentences and actual recidivism risk is small, I am hopeful this bias is minimal.

Online Appendix C: Additional Tables and Figures

ONLINE APPENDIX TABLE C.I

Testing for discontinuous changes in demographic characteristics at the point of the "grid" threshold

	(1) Male	(2) Black	(3) Age at adm.	(4) Prior incarcerations
Above eight grid points	-0.00970	0.0102	0.223	-0.00707
	[0.00894]	[0.0143]	[0.288]	[0.0419]
p-value	.278	.475	.439	.866
Observations	17,373	17,373	17,373	17,373

Notes: This table examines whether demographics vary discontinuously at the grid threshold (between eight and nine points). Each column reports results from a regression with the given outcome variable on a dummy variable for having grid points greater than eight and a linear control for grid points. The p-value reported refers to the coefficient on the dummy variable. See Table II for sampling information. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

ONLINE APPENDIX TABLE C.II

The relationship between recidivism, parole-board decisions and time served (additional specifications)

	Dept. Vars: Returned-to-prison probabilities and hazard rates						
	Returned to prison within....						Hazard
	(1) 36 mos.	(2) 36 mos.	(3) 36 mos.	(4) 36 mos.	(5) 36 mos.	(6) 48 mos.	(7)
Parole board recommendation	0.0367***	0.0319***	0.0328***	0.0396**	0.0262	0.0353***	0.0735**
	[0.0124]	[0.0122]	[0.0122]	[0.0185]	[0.0165]	[0.0126]	[0.0349]
Months served	-0.0339***	-0.0277**	-0.0285**	-0.0385**	-0.0219	-0.0321**	-0.0636*
	[0.0128]	[0.0124]	[0.0124]	[0.0192]	[0.0168]	[0.0130]	[0.0362]
Max. sentence (years)	Six	Ten	Eight	Six	Six	Six	Six
Additional sampling criteria?	No	No	No	Age $\leq$ 26	Age$>$ 26	No	No
Observations	519	542	533	271	248	519	518

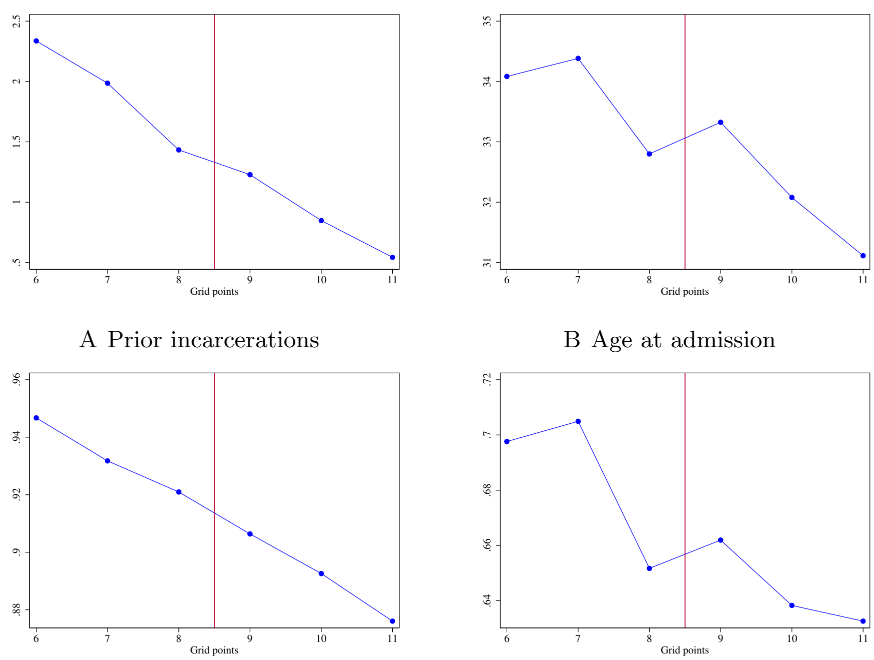
Notes: All individuals were released on March 18, 1981 as part the mass release described in Section V. Col. (1) replicated the main result from col. (1) of Table III. Cols. (2) and (3) shows that the result is robust to using different maximum-sentence sampling rules. Col. (7) reports results from a cox hazard regression, otherwise all coefficients are from probit regressions reported as marginal changes in probability. Standard errors in brackets. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

ONLINE APPENDIX TABLE C.III

Testing for compositional changes among ninety-percent offenders after 1997

	(1) Male	(2) Black	(3) Age at adm.	(4) Prior incarcerations
Ninety-percent crime x After 1997	-0.00240	-0.00731	-0.636	0.0410
	[0.00942]	[0.0356]	[0.996]	[0.0363]
p-value	.799	.837	.524	.259
Observations	30,793	30,792	30,793	30,793

Notes: This table examines whether compositional changes among ninety-percent offenders relative to control-groups offenders occur after 1997. Each regression estimates the given outcome variable on the interaction term $Ninety\ percent_i \times After\ 1997_t$, year fixed effects, and the $Ninety\ percent_i$ dummy variable and clusters standard errors by offense. The p-value refers to the coefficient on the interaction term in the reported regression. For the regressions reported in this table, I use the sample used in the recidivism analysis in Figure VII and Table IV. I also estimate the same regressions used in the activities-while-in-prison analysis (Figures VIII and IX and Tables V and VI). The corresponding p-values, in the same order as the columns in the table, are 0.753, 0.853, 0.339, and 0.296. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$



A Prior incarcerations

B Age at admission

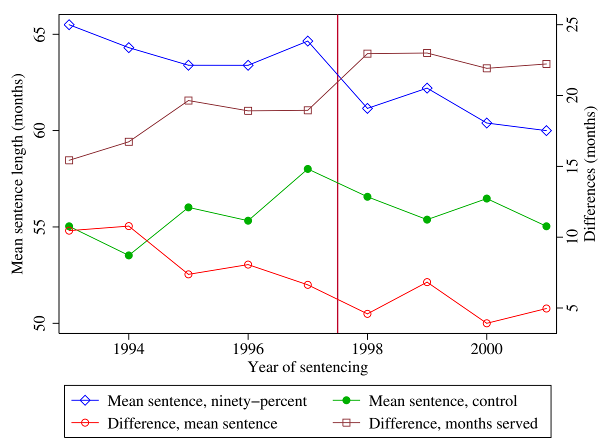
C Percent male

D Percent black

Online Appendix Figure C.I

Demographic characteristics by "grid" points near the first threshold

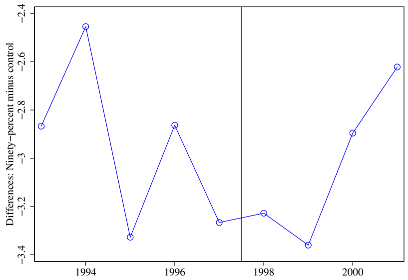
Notes: All individuals in each graph meet the selection criteria described in Section IV. For each grid point, the average value of the y-variable is plotted. The vertical line shows the cut-off between eight and nine points used to separate "high-" and "medium-risk" offenders in the grid.



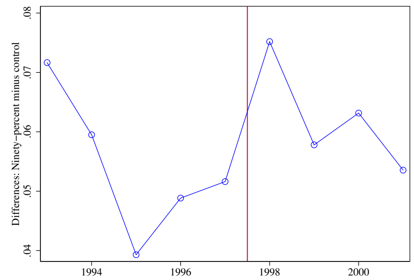
Online Appendix Figure C.II

Sentence length and time served for ninety-percent and control crimes

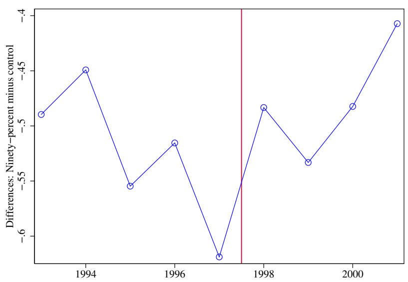
Notes: All individuals in this sample were sentenced between 1993 and 2001 and otherwise meet all the other conditions listed in Section VI. Individuals convicted of ninety-percent crimes (child molestation, statutory rape, aggravated assault or battery, car-jacking, attempted murder, assault on police officer, incest, attempted rape, manslaughter, or robbery) after December 31st, 1997 were required serve at least ninety percent of their sentence. The control group consists of individuals convicted of other crimes who otherwise meet the sampling criteria in Section VI.



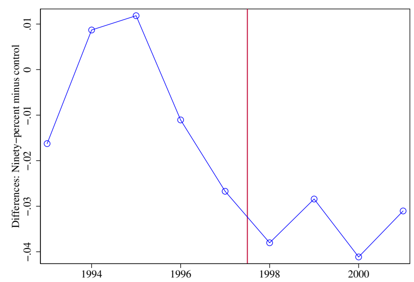
A Age at admission



B Share male



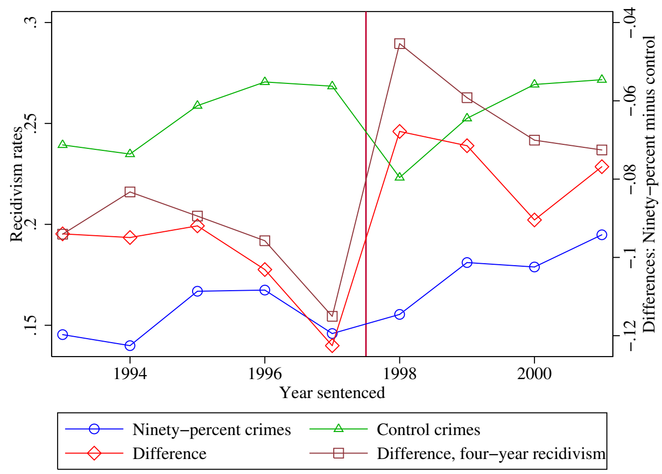
C Prior incarcerations



D Share black

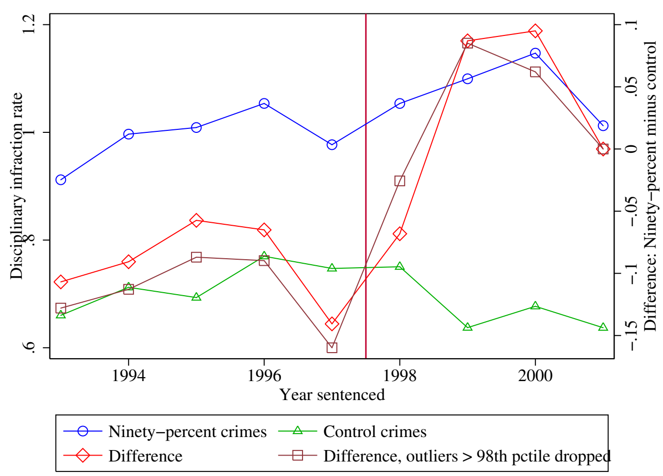
Online Appendix Figure C.III
Differences in demographic characteristics of individuals incarcerated for ninety-percent versus control crimes

Notes: See Figure VII for sampling and definitions. The difference (ninety-percent minus control) for each characteristic is plotted by year of sentencing.



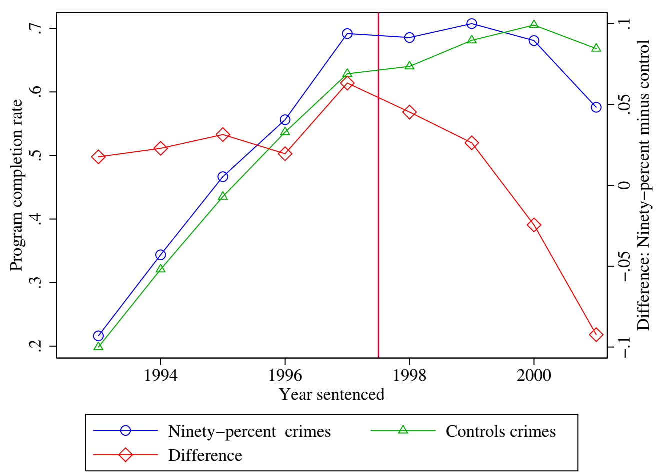
Online Appendix Figure C.IV
Three-year recidivism rates before and after the “ninety-percent” reform, excluding control group members with short sentences

Notes: See Figure VII for sampling and definitions. In addition, all control group inmates are sentenced to at least four years. This additional restriction makes the average difference in time served between the ninety-percent group and the control group fall from 15 to 11 months.



Online Appendix Figure C.V
Disciplinary-infraction rate before and after the “ninety-percent” reform, excluding control group members with short sentences

Notes: See Figure VIII for sampling and definitions. In addition, all control group inmates are sentenced to at least five years. This additional restriction makes the average difference in time served between the ninety-percent group and the control group fall from 25 to 13 months.



Online Appendix Figure C.VI

Program-completion rate before and after the "ninety-percent" reform, excluding control group members with short sentences

Notes: See Figure IX for sampling and definitions. In addition, all control group inmates are sentenced to at least five years. This additional restriction makes the average difference in time served between the ninety-percent group and the control group fall from 25 to 13 months.