DISCUSSION PAPER SERIES

No. 8506

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PUBLIC POLICY

Centre for Economic Policy Research

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ABSTRACT

Assessing Racial Discrimination in Parole Release*

We investigate possible racial discrimination in the context of discretionary parole release. We develop a rational choice model of release whereby a parole board must balance parolees' risk of violation with the cost of not releasing prisoners who may not violate their parole. A color-blind parole board would release all individuals below a certain risk threshold. To test this prediction, we take advantage of a unique data set that reports all prisoners released on parole between 1983 and 2003 in the U.S. We apply the outcome test methodology recently used to assess racial profiling in police search decisions. Here, a higher rate of parole violation within a group suggests that the parole board used a less restrictive paroling criterion, and is thus biased in favor of that group. To overcome the concern of inframarginality that traditionally plagues outcome tests we provide evidence that parole boards strategically time the release of parolees. In turn, both minority and White prisoners become marginal from the perspective of their probability of parole violation. Parole boards operating under an indeterminate sentencing regime appear biased against White prisoners whose violation rate is significantly smaller than that of African Americans. In contrast, this gap is smaller or null when there is no discretion in the paroling system. Further evidence rules out post-release discrimination. We propose different hypotheses to account for the evidence.

JEL Classification: j15 and k40
Keywords: discrimination, outcome test, parole release and race

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1. Introduction

There is a large literature examining the hypothesis of racial discrimination in the U.S. criminal justice system. The motivation of this interest is straightforward. The overrepresentation of minorities at all stages of the criminal justice process is striking, and the proportion of African Americans in prison or jail has more than doubled over the 1980s and 1990s. One in eight African American males age 25-29 was behind bars in 2004. In contrast, 1 in 28 Hispanic males and 1 in 59 non-Hispanic White males were incarcerated in the same age group.[1] It is important to know whether this racial gap in incarceration reflects real differences in criminal behavior or rather disparities in the treatment of minorities at the prosecution, sentencing and parole stages. In this paper, we address the question of disparate treatment by the legal system in the context of parole release. Although parole decisions significantly contribute to incarceration rates, there are fewer studies on parole than on arrests or sentencing. The fact that paroling is a purely administrative decision, as opposed to arrests or sentencing, contributes to this neglect. While this study focuses on the behavior of discretionary parole boards, a corollary question that this research considers is whether different parole regimes are conducive to more or less racial discrimination, if any.

Most studies dealing with racial discrimination are susceptible to the omitted variable bias critique that plagues traditional tests of discrimination based on regression analysis. An apparent significant effect of race on outcomes may actually be accounted for by variables (unobserved by the researcher) that are omitted from the regression and that correlate with race, such as education, disability, socioeconomic status, lawyer quality, etc. Against that, we apply the outcome test methodology recently used to assess racial profiling in police search decisions (see Knowles, Persico and Todd, 2001). The basic idea for outcome tests is to analyze whether the outcomes (which the decision-maker cares about) are systematically different for minorities and non-minorities. In our context, parole boards care about the success of the parole decision. While success may be measured in different ways, the literature suggests that parole boards are mostly concerned with avoiding parole violations of released inmates.[2] Outcome testing is thus a useful tool in assessing allegation of racial bias in the parole decision-making process.

We develop a simple rational choice model of parole release. We assume that a parole board aims at minimizing violation rates, but would also like to avoid denying parole to eligible prisoners whom they believe would not violate parole. We show that to solve this trade-off between type I and type II errors parole boards set a threshold rate of violation and release prisoners whose evaluated risk of violation is lower than the threshold.

A color-blind parole board would use the same threshold for White and African American prisoners. Differences across race in the risk threshold used by the parole board would indicate

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[1] Source: Bureau of Justice Statistics Prison and Jail Inmates at Midyear report, 2004.

[2] Manski and Nagin (1998) use the expression "outcome optimization" in the context of sentencing.

different standards in release decisions. The aggregate ex post rates of parole violation are a valid indicator of the average level of risk that the parole board tolerates before releasing an inmate in a given group. Hence a finding that the rate of parole violation among African-Americans is systematically *lower* than among Whites would suggest that the parole board is engaged in disparate treatment *against* minorities. It would mean that released minority prisoners represent a better risk profile; in other words, that the parole board applies a more stringent criterion when evaluating minorities' possible parole violations.[3]

A major advantage of such outcome tests is that they do not suffer from the omitted variable bias critique.[4] Researchers do not need to observe and control for all the information used by the parole board as long as they can observe the outcome of their decision making. However, the outcome test method may also be deficient when researchers are only able to measure the average outcome and not the outcome associated with marginal decisions. In our case, we only observe average rates of violation and not the marginal ex-ante probabilities of violation. The fact that outcome tests compare average outcomes and not marginal ones is known as the inframarginality problem. By construction, the average takes into account not only marginal individuals but also inframarginal individuals. The composition of the groups of interest can differ in terms of such inframarginal individuals. That can lead to differences of averages even when the marginal individuals (whom we should focus on) are identical.[5]

In our context, the fact that a parole board can choose the time of release alleviates the inframarginality critique. Given that the risk of violation increases mechanically with the amount of time spent on parole, we hypothesize that the board will pick the time of release strategically. In other words, a prisoner would be released when the probability that he violates parole has just become acceptable. The strategic timing of releases would lead to an equalization of the expected probability of violation conditional on release across all parolees. If the board has ulterior, race-based motives, this equalization may occur within each racial group but there may still be a systematic difference across groups. For our purpose, the relevant testable hypothesis is that for a given sentence, the amount of time served in custody should not affect violation rates within each group. From the researcher's perspective, the marginal and the average prisoners released within each group will have similar expected rates of violation. The validation of this hypothesis would make us confident that the outcome test approach to test average parole violation across groups will deliver a clear view of discrimination, if any, in the parole process.

To test our theoretical model, we take advantage of a unique data set that reports a large

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[3]Because parole boards can observe the outcome of their decisions, statistical discrimination, grounded in a lack of information, would be less applicable in this context; rather, if any, discrimination would be more likely to be so-called "taste-based."

[4]See Ayres (2002) for a good summary of the advantages of outcomes tests.

[5]The inframarginality critique is usually mitigated in practice so that evidence of average discrimination is typically considered consistent with discrimination at the margin (see Ayres, 2002).

sample of prisoners released *from parole* from 1983 to 2003: the National Corrections Reporting Program (NCRP). Release from parole can be summarily described as either a successful discharge or a parole violation. Variables include incarceration history, type of offenses, total time served, and whether parole has been violated. Background information on individuals includes year of birth, sex, age, race, Hispanic origin, and in some cases educational attainment. A large number of states are excluded, however, on the basis of missing or miscoded data.

We first provide evidence that parole boards' decisions are consistent with the hypothesis of strategic timing. To that effect, we run regressions taking the probability of violation as our dependent variable and the amount of sentence served as our predictor of interest controlling for maximum sentence length and other available relevant factors. We show that for both African Americans and Whites the amount of sentence served has little explanatory power on the violation rate (statistically insignificant and substantively small) when parolees have gone through a discretionary parole board (so-called indeterminate sentencing). By contrast, the amount of sentence served significantly affects the probability of violation when prisoners are released through a non discretionary, mandatory release regime (so-called determinate sentencing). This is the case in states where both a parole board and a mandatory release regime coexist and, to a lesser degree, in California, which is our only exploitable example of a pure mandatory release state in the NCRP data.

We then proceed to implement the outcome tests. We compare parole violation rates between African Americans and Whites in states that have a pure discretionary regime of parole release and in those where mandatory and discretionary regimes coexist. In almost every state African American parolees are more likely to violate parole than White parolees by about ten percentage points when they have gone through a discretionary parole board.[6] This result is consistent with parole boards being more lenient in their releasing decisions when they face African American prisoners. Therefore, far from confirming discrimination against minorities, we find evidence that parole boards are using a more stringent criterion when dealing with White parole applicants. We explore the robustness of our results by performing additional tests. Parole boards could have different objectives for different sentence lengths or types of offenses, which African Americans and Whites do not commit in the same proportions. Yet, our results hold for different sentence lengths and types of crime.

We refrain from concluding that discretionary parole boards are purposely discriminating against Whites. That interpretation would be implausible if we consider the usual definition of racial animus used in studies of discrimination or racial profiling, *i.e.*, a higher psychological cost for the parole board associated with the violation of parole by a White prisoner. However, our model predicts that the parole board uses a threshold of acceptable risk that is the consequence of a trade-off between type I and type II errors. A higher violation rate for

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[6]This confirms on a national scale some localized observations, such as that for Michigan presented in www.capps-mi.org/pdfdocs/fulldatareport.pdf

African Americans could thus reflect a higher concern for type I errors or a lower concern for type II errors.

A lower concern for violations by African American parolees could explain our finding. This concern could come from parole boards anticipating that a higher rate of violation could in fact reflect a higher rate of revocation and that parole revocation is not racially-neutral. This would be the case for instance if parole boards believed in discrimination against African Americans by the police or by parole officers later on. Yet, both parole and police officers should be indifferent as to how parolees were granted parole. We show that there is a larger discrepancy in violations between African Americans and Whites for parolees who have been released through discretionary parole relative to those released through mandatory parole in states where both types of parole release coexist, and in Wisconsin (our only exploitable switching state) after it adopted determinate sentencing in 1999. That is, after 1999, the African American/White violation differential is larger for those paroled before 1999 under discretionary parole than for those paroled after 1998 under mandatory parole. To the extent that there may be post-release discrimination this evidence casts doubt on the hypothesis that such post release discrimination would account for the higher parole violation differential observed under indeterminate sentencing.

Alternatively, a higher concern for releasing African Americans could be the consequence of parole boards ensuring that African American prisoners are treated like White prisoners in terms of time of release rather than in terms of risk of violation. We observe evidence of this concern in the data: African Americans spend roughly the same amount of time as Whites in prison for a given sentence length in each regime – except for relatively slightly longer times for African American prisoners in California. However, the crucial difference is that inmates spend significantly more time behind bars under a mandatory release regime. Therefore, by releasing inmates earlier on in a uniform way, it appears that parole boards create the conditions for a higher chance of recidivism among African Americans, which could be the product of a network effect if circumstances leading to parole revocation or criminal activity are more prevalent in some predominantly African American communities.

Either way, we can conclude that the behavior of parole boards leads to release decisions that are more favorable to African American parolees in terms of violation rates. Therefore, discrimination against African Americans in the discretionary parole process can be ruled out a possible cause of their higher rate of incarceration.

2. Literature review

2.1. Discrimination in the criminal justice system

Most of the literature on racial discrimination in the criminal justice system deals with the sentencing stage, some of it with arrests and prosecution (including bail or probation denial).

Overall, this research has yielded contradictory findings, with most suggesting that there is no systematic bias in the system. Spohn (2000) summarizes more than 40 studies on the role of race on sentencing finding mixed evidence of discrimination. Many of these studies have been criticized for methodological deficiencies, including sample selection, small sample issues and omitted variable bias (see Klepper, Nagin and Tierney (1983)). Recent studies have tried to move beyond the omitted variable bias critique by using exogenous variations between judges (Abrams *et al.*, 2011), variations between judicial characteristics (Schanzenbach, 2005), or the random timing of events (Shayo, 2000). A recent paper (Alesina and La Ferrara, 2011) is closer to our approach. They use an outcome test strategy to study discrimination in capital sentencing and find evidence of bias against minority defendants who killed white victims.

2.2. Paroling decisions[7]

There exists a small theoretical literature on parole decisions. A classic reference is Lewis (1979) that was the first to cast the parole decision in a rational choice model. Fabel and Meier (1999) extend the analysis. One of the predictions of their model is that criminals with higher risk of recidivism will be released at later periods. Recent work by Bernhardt, Mongrain and Roberts (2011) explore the effect of sentence length and discretion on the timing of paroling decisions. The literature insists on the incentive role of parole decisions that we do not consider in this paper. There is however little connection between theoretical models and empirical research.

In turn, little is known empirically about the parole process and the variables that affect parole release decisions, although the number of parole violators readmitted to prison in 1999 was greater than the total number offenders admitted in 1980 (Travis & Lawrence, 2002). For an excellent review of what is known, see Petersilia (2003). Noting that most of the research over the last thirty years has focused on states that moved away from parole board decision-making Reitz argues that "what is needed is nothing less than a new field of indeterminate sentencing studies" (Reitz, 2011).

We concur with Morgan and Smith (2008) in the assessment that the research that specifically examines racial bias in parole decision-making is dated, and that its findings are ambiguous. Previous studies typically fall into one of two categories. The first type assesses the importance of certain variables in parole decisions (possibly telling apart selection for parole consideration and actual parole release decision-making) and whether or not these variables have equal importance for African American and White inmates (Carroll & Mondrick, 1976; Elion & Megargee, 1978; Scott, 1974). This is often referred to as "contextualization"; implicitly these studies look at the differential effect of race on certain characteristics, which is typically handled by a dummy variable interaction term in standard regression methodology.

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[7]Morgan and Smith (2008) include a useful survey of the literature in criminology, as well as a description of the parole system: we refer the reader to their article and summarize some of the key points here.

The second type looks at time served and whether minority inmates serve a greater percentage of their sentence before being paroled than their White counterparts (Johnson, 1968; Wolfgang and Cohen, 1970; Carroll & Mondrick, 1976; Elion & Megargee, 1978; Petersilia, 1985; Kassebaum, 1999; Kassebaum & Davidson-Corondo, 2001). They generally conclude that there is no race impact even though other studies of recidivism (not necessarily among parolees) show that predictors of re-offending include being a minority (Clarke *et al.*, 1988; Irish, 1989).

Recently, Steen and Opsal (2007) look at whether race has a direct effect on parole revocation. They also investigate whether race matters more under certain circumstances than others – recall "contextualization" of race is a leitmotiv in the criminology literature (race important for certain offences, for certain age/gender groups, but not *per se* necessarily). The contribution by Steen and Opsal (2007) is of particular interest to us because they too use NCRP data. They observe in their subsample of four states from the 2000 NCRP wave that African Americans are more likely to have their parole revoked. They fall short of concluding that a higher percentage of revocation is reflective of bias against African Americans. However, they interpret this result as "the product of the relatively large amount of discretion available in decisions about whether to file for a revocation when an offender violates parole and about whether to revoke such an offender," while conceding that "it is also possible, however, that African American offenders are more likely to violate the conditions of their parole (by committing a new offense or technical violation) or to be detected in such violations." A general concern with this literature is its narrow focus in terms of time period, geographical area and corresponding small sample sizes. In turn, regardless of methodological considerations, these previous studies lack external validity. More importantly, none, to our knowledge, examines violation in the framework of an outcome test.

2.3. Outcome tests

There is now a large body of knowledge on discrimination using outcome tests. The original idea goes back to Becker (1993) who suggested that if banks discriminate against minorities we should expect minorities to exhibit lower default rates. Since then, outcomes tests have been used to study *e.g.*, bail-bond setting decisions (Ayres and Waldfogel 1994), or editorial acceptance decisions (Ayres and Vars 2000). Recently, outcome tests have become a standard tool to analyze racial profiling by law enforcement. Knowles, Persico and Todd (2001) argue that comparing the productivity of police search for contraband is a good way to test whether police require less probable cause when searching minorities – see also Anwar and Fang (2006), Persico and Todd (2006). Persico and Todd have used a similar test to analyze discrimination by customs officers during airport searches (Persico and Todd, 2005). Alesina and La Ferrara (2011) analyze discrimination in capital sentencing using an outcome test.

The present paper contributes to this literature in two ways. First, parole release decisions are a new application of the outcome tests methodology to the pervasive topic of discrimi-

nation in the criminal justice system. It also provides a new response to the debate on the inframarginality critique often associated with outcome tests. We show that in the context of parole release, the evidence that the parole board chooses the time of release to equalize the success rates of parole can significantly alleviate the inframarginality critique[8].

3. The parole system in the United States

There are three mechanisms of release from prison: discretionary parole release, mandatory release, and unconditional release. Discretionary parole release is a conditional release granted by a parole board. Mandatory release may be granted after an inmate has served his full sentence minus good time. It is used by the federal government and by states operating under a determinate sentencing regime, but also, as will become clear, by certain states operating under indeterminate sentencing. Here, we should emphasize that determinate sentencing is less about sentencing and more about release decisions.

> "Although the term determinate sentencing has been applied to several types of sentencing and corrections schemes, it essentially refers to a system without discretionary parole release as a mechanism for releasing offenders from prison (...). Under determinate sentencing systems, the sentencing judge imposes a prison term expressed as a number of years of imprisonment. Without discretionary parole release, offenders are then automatically released from prison after serving a statutorily-determined portion of the term imposed. The "determinacy" in the system refers to the effort to ensure that time served by offenders is primarily determined by the length of the sentence imposed by the judge rather than by the discretionary release decision-making of the parole board" (Stemen *et al.*, 2005)

Slightly more than 40 percent of the inmates released annually are granted a mandatory release. Notably, determinate sentencing was partly instituted to answer the suspicion that parole board broad discretion would lead to discrimination (Reitz, 2001b). Finally, unconditional release, *i.e.*, release with no further correctional supervision, applies to inmates who have completed their sentence in full, received a pardon, or had their sentences commuted. There are two main reasons why we expect more discretion at the parole board stage than in any other stage: this is an administrative decision and not a legal one, and hearings are relatively invisible. Next, we investigate the objective of parole boards.

Rehabilitation is the main stated objective of the paroling process. Yet, criminologists have argued that the economic and political environment, not the inmate or offense characteristics, significantly affect parole decisions (Branham, 1983; DeGostin and Hoffman, 1974; Gottfredson and Ballard, 1966; Parsons, 1972; Pogrebin, Poole, and Regoli, 1986). Simon (1993)

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[8]We discuss in more details the issue of inframarginality and our solution to it in section 4.

notes that the transformation of the parole process has been related to the political and economic changes of society, thus causing rehabilitation issues to be replaced by management concerns, *i.e.*, efforts to control prison population overcrowding.[9] Finally, parole may be used to remedy sentencing disparities for inmates who are perceived to have been sentenced unfairly because of race/ethnicity, gender, and social class (Hofer, 1999). Regardless of the ideology underlying parole, we need to make sense of how it operates in practice.[10]

> "As von Hirsch (1976:11) points out, "In the literature of rehabilitation, there is often considerable ambiguity whether the aim is to reduce recidivism (a form of crime prevention) or to help the offender with his own problems (a paternalistic goal). But treatment programs have generally been tested by measuring their effects on recidivism –suggesting that the goal of reducing recidivism is actually the primary one." This is the stance taken by the National Academy of Sciences' Panel on Research on Rehabilitative Techniques (Sechrest et al., 1979). The panel not only points out that recidivism is the "traditional measure" for evaluating rehabilitation programs, but also states that it is "the sole criterion against which rehabilitation ultimately must be measured" (Sechrest et al., 1979:21, emphasis added: see also Martin et al., 1981 :8). Considering the difficulties inherent in doing a process evaluation, this stance is understandable; a virtue is made of necessity. Counting the number of people rearrested is much easier than doing a process evaluation. But this stance is also troublesome." (Maltz, 2001)

In other words, failure is easier to measure than success (parolees finding a job, being reattached to the labor force for a long period of time etc.) for a variety of reasons: "It is too difficult to collect such data," "It violates privacy rights," "It is not part of our mission," and "The cost of data collection is too high for measures that have little bearing on policy." In turn, the evaluations are failure-based. Feeley and Simon (1992) characterize the shift from the objective of rehabilitation to incapacitation as the coming of age of a "new penology" in the criminal justice system. They emphasize that since the early 1980s risk assessment has dominated rehabilitation. This confirms our view that minimizing violations has been the central objective of parole boards.[11]

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[9]Although systematic studies of parole as an overcrowding regulation mechanism are scarce (see Champion, 2002) there is evidence in the data to support this hypothesis, and it appears to be part of the culture of department of corrections officials.

[10]See Rhine (2011) for a thorough discussion of the many considerations at play across parole boards. Our presentation is necessary simplified.

[11]"The risk-incapacitative policy is often set out as an overarching principle of the parole board's function. The Colorado code, for example, says bluntly that "the primary consideration for any decision to grant parole shall be the public safety" (Colo. Rev. Stat. § 17-2-100.2)." (Reitz, 2011)

Paroling guidelines identify the three variables that should take precedence in parole decisions: time served, offense seriousness, and risk of recidivism.[12] Since the first two are perfectly observable by the parole board, only the latter is subject to question. While we do not observe offence seriousness *per se* the NCRP contains the type of offense (possibly for several counts), detailed at a three digit classification level as well as the length of the sentence.

Parole boards may use forecasting models in the assessment of recidivism (Harcourt, 2007). Although critical to decision-making, risk assessment using forecasting models is prone to type one and type two errors (Champion, 1994). Neither the parole board nor the researcher can observe the error coming from erroneously classifying a low risk inmate as high risk and subjecting them to further imprisonment. Instead, like the parole board, we focus on the observable propensity to commit an offense conditional on release. We leave it to further research to determine whether violation rates increase or decrease when parole boards adopt those models and in turn, whether discrimination is affected.

4. A model of discretionary parole release

4.1. Setting an optimal threshold of acceptable risk

We now present a simple model of parole release decisions. The implications of the model allow us to devise a test of racial prejudice of parole boards. The test is based on the outcome of the parole and is not subject to the inframarginality critique.

Consider prisoners eligible for parole with race r and other characteristics. These characteristics encompass all information available to the parole board when it decides to release or not a prisoner. This information includes gender, age as well as the type of crime, the behavior in prison and the personal situation of the prisoner in case he is released.[13] Some of these variables are observed by the researcher, but other variables used by the parole board may not be accessible.

The parole board uses all the information to estimate the risk that a prisoner will violate the conditions of his parole. We assume that the parole board summarizes all the information about the characteristics of the prisoner in a single dimension index which we denote x.

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[12]We make the assumption that parole boards are not concerned with risk of recidivism once parolees are done with their parole time. It could be that the more time prisoners spend in prison, the more alienated from society they feel, the fewer friends and family they have when they are released, and the more criminal human capital they acquire behind bars, which makes it harder to adjust and increases their likelihood of lifelong recidivism. However, it can be checked that parole mandates explicitly mention the accountability of the parole board to the public for the inmates' actions until the expiration of their sentence only.

[13]Incidentally, Morris has argued (Reitz, 2004) that the parole board's information set on a given inmate is no greater what it was at the beginning of time served.

We normalize the support of x so that $x \in [0, 1]$. The distribution of information x in the prisoners' population is denoted by the cumulative distribution function $F(\cdot)$ and probability distribution function $f(\cdot)$. We denote $F_r(\cdot)$ the cumulative distribution function of x among race-r prisoners.

From the information contained in x, the Parole Board infers the risk of violation associated with a possible release. Let $\pi(r, x)$ be the violation risk associated with a prisoner of race r with characteristics x. We assume that $\frac{\partial \pi}{\partial x} < 0$, that is a higher index x is associated with a lower risk of violation.

The parole board wants to avoid releasing prisoners who are likely to violate their parole, but would also like to avoid denying parole to eligible prisoners that are unlikely to violate it. To model this trade-off, we assume that the parole board minimizes a weighted sum of the cost of type I errors, C_1 (keeping a prisoner incarcerated who would not violate his parole) and of type II errors C_2 (releasing a prisoner who violates his parole). Let $\lambda = C_1 / (C_1 + C_2)$ denote the relative concern of the parole board between the costs of type I and type II errors. This concern may reflect the preferences of the parole board but also some outside pressure such as prison overcrowding or public outrage after a particularly odious crime has been committed by a parolee.

Therefore the parole board chooses the optimal x_r^* that solves:

$$Min_{x_r^*} \int_0^{x_r^*} \lambda \left(1 - \pi(r, x)\right) f_r(x) \, dx + \int_{x_r^*}^1 (1 - \lambda) \pi(r, x) f_r(x) \, dx$$

The first-order conditions yield:

$$\begin{aligned} \lambda \left(1 - \pi(r, x_r^*)\right) f(x_r^*) - (1 - \lambda) \pi(r, x_r^*) f(x_r^*) &= 0 \\ \pi(r, x_r^*) &= \lambda \end{aligned}$$

The critical value x_r^* represents the standard applied by the parole board in its release decisions. When the information about the prisoner is sufficiently favorable, which means that the expected violation is lower than the threshold λ, the parole board decides to release the prisoner. We see that the optimal decision leads to a risk threshold $\pi(r, x_r^*)$ that does not depend on race if the parameter λ is the same across races. Racial prejudice would be the result of the parole board using different objectives for different races, and thus leads to different expected violation for different races. Our empirical methodology is meant to test whether the objective function of the parole board is race neutral and it is based on the parole outcome.

Our model shows that the critical value x_r^* can potentially depend on race. The parole board could form different assessments in terms of risk of violation of prisoners with similar characteristics x but different races. Different critical values would thus be the result of the

parole board using statistical discrimination to impose a common risk threshold across races. Our outcome test will not try assess whether the board is using statistical discrimination (see *e.g.*, Curry and Klump, 2009) and is designed to only identify racial prejudice.

4.2. A test of racial bias

The objective of the parole board is summarized by the parameter λ that represents the relative concern between not releasing rehabilitated prisoners and the risk of violation. In the presence of a parole board with a race neutral objective function, this parameter should not depend on race, and the acceptable risk for released prisoners should be the same across races. Otherwise the parole board is using different parameters λ_r that depend on race r.

To analyze the possibility of an illegitimate discriminatory bias against a group, we follow the literature on racial profiling (see Persico, 2009) and model this bias as a group-specific coefficient β. This coefficient captures a psychic cost for the parole board associated with the violation of parole by an individual with specific characteristics.

Definition 1. *The parole board is said to be prejudiced if $\lambda_r \neq \lambda_{r'}$. It is prejudiced against prisoners of race r' if $\lambda_r > \lambda_{r'}$.*

This coefficient is interpreted as a taste for discrimination. If the parole board is biased against African American prisoners, it would incur a larger psychological cost associated with violation by an African American parolee (βC_2) than from a White parolee (C_2), with $\beta > 1$. In that case, we would have

$$\lambda_w = C_1/\left(C_1 + C_2\right) > \lambda_{AA} = C_1/\left(C_1 + \beta C_2\right).$$

The parole board would set a more stringent standard of release for African American prisoners.

Proposition 1. *If the parole board is prejudiced against group r', $\lambda_r > \lambda_{r'}$ it will set a more stringent risk threshold $(\pi\left(r, x_r^*\right) > \pi\left(r', x_{r'}^*\right))$ for that group.*

Note that if the bias enters the objective of the parole board through the cost of type I error C_1 as a coefficient γ, for instance because of the social pressure put on parole boards to release more prisoners of a given group, this would also lead to parameters λ that differ across races. If there is more pressure to release African American prisoners, that is $\gamma > 1$, we would get:

$$\lambda_w = C_1/\left(C_1 + C_2\right) < \lambda_{AA} = \gamma C_1/\left(\gamma C_1 + C_2\right).$$

In that case, the parole board would set a more stringent standard of release for White prisoners.

To establish the presence of discriminatory behavior, we need to infer from the data the λ_r for different groups. Specifically, we want to compare the value of the λ_r for White parolees and African American parolees. Ideally, we would observe the risk thresholds $\pi\left(r, x_r^*\right)$ used by the parole board and we would compare these thresholds for the groups we have. Under the assumption that the parole board is unbiased, we should observe similar thresholds for each group.

From the data available, we can only observe cruder statistics. In particular, we observe the average success of parole for a given group r, $\displaystyle\int_0^{x_r^*} \pi\left(r, x\right) \cdot f_r\left(x\right) dx$, and the number of parolees in that group under the release policy x_r^* used by the parole board, $\displaystyle\int_0^{x_r^*} f_r\left(x\right) dx$.

4.3. The inframarginality problem and the proposed solution

The researcher is also only able to observe coarse partitions of the characteristics x used by the parole board *i.e.*, whether a given parolee's characteristics belong to a partition of the set of characteristics. To illustrate this difference in information, note that we do not have data on the behavior of the inmate while he serves his sentence, or his personal environment after release. For two groups r_1 and r_2, we assume that the parole board treats all members of the group in a similar way that is λ_{r_i} $i = 1, 2$ is constant for all members of group i. This means that the ratio of the group-specific discrimination factors is proportional to theoretical risk thresholds $\left(\pi\left(r, x_r^*\right)\right)$.

An unbiased parole board would release prisoners in such a way that the marginal success is equalized across groups. To test for discrimination between Whites and African Americans, we would like to compare these marginal rates using the empirical data at our disposal. Unfortunately, we observe the average success of parole in a group and not the success of the marginal individual released. The identification strategy that uses average success to approximate marginal success suffers from the well-known inframarginality problem. The parole board releases not only individuals whose risk of violation is exactly equal to $\pi\left(r, x_r^*\right)$, but also all prisoners with a lower risk, so-called inframarginal inframarginal types. What we observe is then the average outcome of released inmates which plausibly depends on the composition of their group, that is the specific $F_r\left(\cdot\right)$ that may differ from group to group. However, the inframarginality problem is alleviated in the context of parole release. The key insight is that the parole board not only decides *whether* to release or not a prisoner but also *when* to release him.

The main implication of our model is that the decision of the parole board can be summarized as a risk threshold $\pi^*\left(x\right)$. Given that threshold, the parole board must decide on the time t to release the prisoner. The parole board thus chooses the optimal time t possibly under
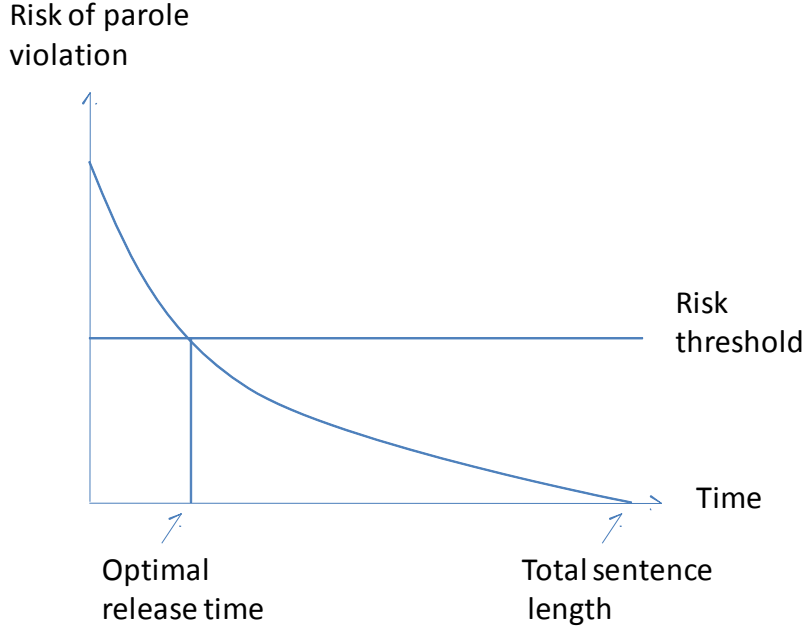
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Figure 4.1: Strategic timing within a group

legal constraints of a minimum time served and a mandatory release rule, $\underline{t} \leq t \leq \bar{t}$. The dynamic nature of the parole process implies that the distribution of risk of violation at the time of release is endogenous. By keeping a prisoner longer in prison, the parole board may get additional information on the prisoner and also by shortening the time spent on parole the board can mechanically decrease the risk of a violation. We thus make the natural assumption that the probability of violation $\pi(r, x, t)$ depends on the time of release and is decreasing with the amount of sentence served:

$$\frac{\partial \pi(r, x, t)}{\partial t} < 0.$$

We see that if there exists a time t^* after which the risk of violation is below the risk threshold, the parole board would choose the exact time t^* to release the prisoner. (See figure 1 an illustration of this logic for a given race.)

The result of the strategic timing of release is that the parole board releases prisoners when their probability of violation is exactly equal to the threshold λ_r. All released prisoners are thus marginal and for a given group the average parole success is a good indicator of the threshold of risk used by the parole board.

The strategic timing hypothesis leads to sharp predictions in terms of what we should observe in the data. All prisoners released by the parole board (except for some who are released

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as soon as they become eligible for parole, but we do not know this criterion) should have the same expected rate of violation. Therefore, subject to the possible caveat of minimum time served for a given sentence length, parole success should be equalized regardless of time actually served.[14] Applied to each group, the intuition is the same as in Kuziemko (2007): " I find that parole boards do indeed assign longer terms to those with higher initial risk, and do so in a manner that exactly offsets variation in inmates' initial risk, so that inmates are released when their expected recidivism falls below a certain threshold."[15] Note that this result also implies that the expected violation rates should be equalized across different lengths of parole. It is therefore legitimate to compare success rates across various parole terms. In other words the logic of strategic timing implies that the fact that some prisoners spend more time on parole than others does not make them automatically more prone to parole violation because they would have more opportunities to violate parole so to speak.

This logic described so far applies within a group (race) for which the parole board uses a common violation threshold. The logic of strategic timing extends to any group. As figure 2 illustrates, strategic timing means that white prisoners should have the same violation risk at the time they are releases and that African American prisoners should all have the same violation risk (but potentially different from the one of White prisoners) when they are released.
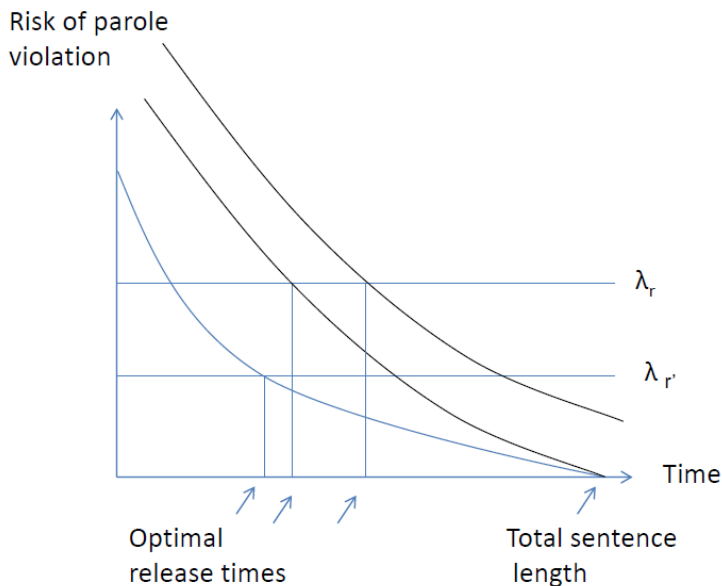

Figure 2: Strategic timing with two groups

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[14]We assume for simplicity that a prisoner is potentially up for parole at any time. In practice, hearings are set a various intervals so that the exact time when the prisoner reaches the risk threshold that should trigger his release may not coincide with the hearing date.
[15]Kuziemko (2007), p. 4.

Going back to the observed violation rates, we see that if the Parole Board is unbiased $(\lambda_W = \lambda_{AA})$ and thus uses the same threshold for prisoners of different groups, the observed success rates should be the same.

$$
\lambda_w = \int_0^{x_w^*} \pi\left(r, x_w^*\right) \cdot f_w\left(x\right) dx / \int_0^{x_w^*} f_w\left(x\right) dx
$$

$$
\lambda_{AA} = \int_0^{x_{AA}^*} \pi\left(r, x_{AA}^*\right) \cdot f_{AA}\left(x\right) dx / \int_0^{x_{AA}^*} f_{AA}\left(x\right) dx
$$

Differences in observed success rates across groups should only be explained by differences in the objective of the boards (different λ_r) and would be evidence of racial prejudice.

Proposition 2. *All prisoners released by the parole board (subject to minimum sentence requirements) should have the same risk of violation. All released prisoners are thus marginal prisoners in terms of risk of violation, and the observed average violation of a group is a good indicator of the risk threshold used by the parole board for that group.*

To summarize, the solution to the inframarginality problem comes from the use of strategic timing of release. The main idea is that time spent in prison plays the role of a continuous control variable that affects the outcome of interest, here the violation of parole. The idea that a continuous control variable may help to deal with the inframarginality has been introduced in Ayres (1994) in the context of bond setting and further discussed in Ayres (2002) and Ayres (2005). Anwar and Fang (2010) also uses this that idea in a different context. They study racial prejudice in the behavior of emergency department physicians. In their model, doctors wants to avoid bouncebacks that occur when a patient discharged from the emergency room as having only a minor ailment has to come back within three days with in fact a major condition. As in our model, the optimal policy is characterized by an optimal threshold of acceptable risk. Anwar and Fang shows that when doctors have a access to a continuous battery of diagnostic tests, they will administrate tests until they are sufficiently confident that the patient is an acceptable risk that is, discharged patients are marginal patients.

5. Data description

Our data come from the publicly available National Corrections Reporting Program (NCRP). In 1983, the National Prisoners Statistics program, which compiled data on prisoner admissions and releases, and the Uniform Parole Reports were combined into one reporting system. The NCRP evolved from the need to improve and consolidate data on corrections at the national-level. Its objective is to provide a consistent and comprehensive description of

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prisoners entering and leaving the custody or supervision of state and federal authorities. In addition to the state prisons, the Federal Prison System and the California Youth Authority also began reporting in 1984. The advantages of the NCRP are multiple. It allows for a large exploitable sample, since it is derived from a comprehensive (in theory) census.

Data refer to prisoners who were admitted to prison, released from prison, or released from parole. There are no identifiers that would allow us to follow the same individuals throughout the admission to prison, release from prison and release from parole files for cross validation purposes. However, the parole release file contains key information on prison history and we therefore rely on it exclusively. Variables include maximum sentence length – defined as the maximum total sentence length for all offences, incarceration history and parole history. Background information on individuals includes year of birth, sex, age, race, Hispanic origin, and in some cases educational attainment. In particular, the data on parole release give us the exact time of release and the type of release from parole supervision: most importantly, whether a discharge from parole (i.e., a success) or a revocation (for violation of parole conditions or because of a new offence). The data distinguish between parole board releases and mandatory parole releases (as well as other, more rare types of parole release).

By the BJS own admission, the NCRP data set is not exempt from problems: we found a large proportion of missing observations on key variables concentrated in some states. Not all states participate in the NCRP in any given year. In addition, not all states participate in all three phases - admissions, prison releases, parole releases. Out of the 51 states + federal prison + California youth authority, only 13 have the complete parole releases series across years. However, several states have only one or few years altogether missing. After this first screening, the sample ends up comprising 31 states (plus the Federal Prison System and the California Youth Authority).

Next, states were selected on the basis of data availability, internal consistency in reporting and external validation with aggregate data reported in the Correctional Populations in the United States data series (CPUS) 1985-2002 & Probation and Parole Bulletins (PPB) 1983-1984.[16] Data availability refers to states participating in the NCRP, and for those that do, whether all the necessary variables are present, and whether the variables were coded meaningfully, *i.e.*, with few enough observations coded as Unknown. Similarly we purged the data of logically inconsistent observations (for example, date of release predating date of admission). Internal consistency refers to the consistency of violation rates from one year to the next, and within different parole regimes in the case of states exhibiting more than one concurrently. We also checked the consistency over time in the proportion of parolees entering parole through one regime as opposed to another. Large jumps that have no justification in terms of institutional changes point to data reporting errors. Even if the

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[16]These tables are accessible online from 1993 to 1998. We thank Tom Bonczar from the BJS for providing us scanned copies of the rest of the series. Those data were collected alongside and separately from the NCRP hence their genuine comparative value.

violation rates are consistent, they are still meaningless if they are consistently implausible. That is why we also screened out states that had obviously misclassified some data, *e.g.*, states with violation rates systematically under 5%, and whether parolees who entered prison under a determinate sentencing regime were reported as parole board releases (some states make that mistake systematically). In those states the parole board still exists but it is stripped of its discretionary authority for prisoners sentenced under determinate sentencing (there is grandfathering for prisoners admitted under indeterminate sentencing). Retaining a parole board release classification under those circumstances does not make sense from the perspective of this investigation, yet imputing a mandatory release when the coding actually indicates parole board release was a leap that we were not ready to make. Notably, California, by far the largest contributor to the NCRP data, correctly codes all its releases as mandatory parole release - save some prisoners sentenced to life in prison. We then compared proportions for violation and type of parole entry to the figures provided by the CPUS & PPB. We kept those states where the NCRP data come "close enough" to those of the CPUS & PPB[17]. Erring on the side of caution, this process left us with Michigan, North Dakota, Utah, Colorado, Wisconsin, Texas, Missouri, Arkansas, New York, and California. Within that list, Colorado, Missouri and Arkansas present some notable discrepancies in the parole entry types between the NCRP and the CPUS. Arkansas reported a large number of releases as unspecified "Other Conditional Releases" in the NCRP. See Table 1 for summary statistics of the preferred sample for our empirical analysis.

6. Empirical results

Our empirical strategy is in two steps. First we test the behavior of parole boards to confirm the assumption of our theoretical model. The validity of our approach to test discrimination by comparing the success of African American parolees to that of White parolees indirectly relies on the assumption that the parole board uses the time of release strategically. It could be the case that the parole board was not behaving as the theory predicts either because it has other objectives, or because in practice it is unable to equalize risk of violation using a strategic timing of release approach.

Second, we develop a test for prejudice that compares the probability of violating parole across ethnic groups. The model in section 4 delivers the strong prediction that violation rates should be equal across groups. Therefore, under the null hypothesis that the parole system is racially neutral, we should observe similar violation rates for Whites and African Americans.

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[17]We also called state parole boards to check whether the data seemed correct based on their experience: all states selected passed this first impression test. We tested the robustness of our specifications with the inclusion (exclusion) of states where discrepancies were significant but seemingly still plausible.

6.1. Inframarginality and the strategic timing of release

In this subsection, we provide evidence supporting the argument that parole boards use strategic timing in their parole decision and that the risk of parole violation is indeed equalized across parolees within each group. The testable implication of the theory is that on average within a discretionary regime, the amount of time served at time of release, for a given sentence, should have no discernible influence on the violation rate as illustrated in Figure 1. The parole board decides to release a prisoner when his risk of violation has crossed the threshold of acceptable risk. To test this implication, our dependent variable is the parole outcome (violation/ no violation) and the main independent variable the group-specific amount of time served at time of prison release, controlling for total sentence length. Note again that strategic timing is consistent with bias: if the parole board favours one group over another, it paroles inmates from that group "too early" relative to the average acceptable odds of violation it has set (leading to a higher than average rate of violation for that group), and conversely paroles the inmates of the other group "too late" relative to that same threshold (leading to a lower than average rate of violation for that group).

We paid attention to pre judgment custody or "prior time served" (typically in jail). As it turns out, this variable is only present for a fraction of observations, corresponding to certain types of prison admission. For inmates serving long sentences time served prior "current admission" may be negligible, but since most prisoners serve short sentences this could be problematic. We thus present specifications where time served includes prior time served and with time served on current admission only. Further, the Census Bureau confirmed to us that prisoners who leave prison after a parole violation and a subsequent spell of prison pose a challenge for determining sentence length. Consider the case of an inmate who violated parole by re-offending and a new sentence is imposed. When he appears in front of a parole board for a second time, it is not clear whether the decision is based on the newest sentence (in case the violation was caused by a new offense), the sum of the current and prior sentences, or some other combination. Hence, it may be problematic to lump together former inmates who were paroled after a prison entry corresponding to a new court commitment and former inmates who violated parole and have re-entered prison, possibly several times, and for different reasons (technical revocation vs. new charge). Parole boards use more information for the latter case than what we observe. No doubt the fact that such inmates violated parole before influences the parole board decision whether to grant parole beyond what their total sentence length, however it may be determined, can capture. Therefore, likewise, because the variable on prison admission type is missing for many observations, we present specifications for new court commitments only as well as with no restriction on prison entry type. Finally we also present a specification where both restrictions are lifted.

To avoid censoring, we restricted parole release to occur no later than 1997, so that by 2003 (the last year of data), we checked that over 95% of parolees must have exited parole, one way or another. We selected only male parolees who had been released from prison (as opposed

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to jail, halfway houses and other less common types of facilities). When considering parole board operations, we restricted the analysis to those that do not operate under a Truth in Sentencing regime (which mandates minimum percentages of sentence completion) because by nature they would restrict the ability of the parole boards to time releases strategically. We similarly excluded other institutional restrictions on discretion (see Sabol *et al.*, 2002).

For a given sentence length, a finding of no impact of time served on violation rates in states where a parole board is the only method of parole release, supports the hypothesis of strategic timing but is not conclusive because of the absence of a comparison group. It could be that for a given sentence time served bears no influence on violation regardless of the paroling regime, in which case we would be hard pressed to conclude that parole boards are being strategic. A logical step is to focus on states that switched from indeterminate to determinate sentencing. Since there is no reason that parole officers would distinguish on the basis of paroling regime it would be an intuitive way to assess whether time served significantly influences violation for the latter and not for the former. However, the poor quality of the data on states that switched to a determinate sentencing regime within the NCRP time frame – or at least not too long before the collection started – made impractical a within state comparative study of the effect of time served on parole violation between regimes. California is the only state where we observe nearly 100% mandatory parole for prisoners admitted to prison following the regime change. Because California (actually featuring a majority of mandatory release observations within the NCRP overall) switched to determinate sentencing in 1976 the only parolees that we observe as having been paroled through a parole board are a small number of inmates originally sentenced to a maximum of life in prison, hence a non representative sample of offenders.

There may also be inherent difficulties stemming from the way judges may modify their sentences to get around the rigor of determinate sentencing: in Oregon for example, after determinate sentencing was implemented in 1989, two thirds of sentences involved pre release centers, work centers and other facilities, as opposed to the almost exclusive use of state prison under indeterminate sentencing. Probation releases also grew to a quarter of the total of prison releases from almost zero before determinate sentencing. If judges change their sentencing to mitigate perceived perverse effects of determinate sentencing, then the same prisoner may experience a different maximum initial sentence based simply on the paroling regime. This would cripple the analysis.

Therefore, as a second best, we compare the impact of time served within indeterminate states that have both parole boards and mandatory parole as mechanisms of parole release (Missouri, New York, Texas, and Wisconsin before 1999). Prisoners are not randomly assigned to one regime or another however, and it is important to understand who goes one route v. the other. There are three main possibilities, with different implications for violation rates. The first one is that inmates paroled under the mandatory regime failed their parole board hearing and therefore serve their full sentence minus good time. Although these prisoners have served a larger fraction of their sentence and therefore will spend less time on parole, they

are expected to be worse risks and should violate their parole more often, *ceteris paribus*.[18] Another hypothesis is that inmates released under mandatory parole served so much of their sentence before judgment that they are eligible for parole before their first parole board hearing. Indeed, in the data, mandatory parolees have served, on average, a significantly larger fraction of their sentence in pre trial custody. Those inmates are expected to violate parole less often. Another possibility is that certain types of offenses make offenders ineligible for a parole board hearing – thereby incorporating a dose of determinate sentencing in an otherwise indeterminate regime. For example, this appears to be the case in New York.[19] Yet, in practice, the system is more complex: when comparing the types of offense for each category of parolees we do not find any that is attached to mandatory parole exclusively: still, there are patterns *e.g.*, parolees released under mandatory parole are more likely to have committed a drug related offense.

To summarize, within states operating under a mixed paroling regime, the effect of being paroled through a parole board on the violation rate, captured by a parole board dummy variable, is ambiguous *a priori* and does not lend itself to a simple explanation. In turn, within the mandatory parole category, the effect of time served on the violation rate is ambiguous and depends on which effect dominates. It could be that these effects offset each other and that on the net, there is no significant effect of time served on the violation rate for mandatory release parolees. However, if one effect dominates, resulting in a significant coefficient, evidence of no effect under the parole board regime, by contrast, would provide support for the strategic timing theory. This support would be strengthened if states operating under determinate sentencing exclusively showed similar significant results as for mandatory releases under the mixed regime. Hence we report the effect of prison served within California as an illustrative comparison.

To find out, we estimate the probability of violation ($VIOL$) using the following linear probability regression model[20]

$$
\begin{aligned}
(VIOL_i) \;=\; & \beta_0 + \beta_1 TimeServed_i + \beta_2 PBOARD_i + \beta_3 Black_i + \beta_4 TimeServed_i * PBOARD_i + \\
& \beta_5 Black_i * PBOARD_i + \beta_6 TimeServed_i * Black_i + \beta_7 TimeServed_i * PBOARD_i * Black_i \\
& + \beta_8 \mathbf{X}_i + \epsilon
\end{aligned}
$$

where $\mathbf{X}$ is a vector of individual control variables: age at prison release, total length of

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[18]However, we were attuned to the possibility that for some types of crimes some parole boards are more likely to deny parole for fear of public opinion – especially in case of violation – even if inmates present a good risk profile.

[19]The Future of Sentencing in New York State: Recommendations for Reform, New York State Commission on Sentencing Reform, available on http://criminaljustice.state.ny.us/pio/csr_report2-2009.pdf

[20]Linear Probability models are to be preferred in the presence of interaction terms, see Ai and Norton (2003).

sentence as well as state dummies and year of release and their interactions.[21]

$PBOARD$ is a dummy variable that takes value 1 if the release was made through a parole board, 0 if made through mandatory parole. $TimeServed$ is the amount of time served, and different specifications include or exclude time served in pre adjudication custody. The interaction term $Black * PBOARD$ capturing the relative impact of being African American on violation within a parole board regime can be considered the reduced form test of discrimination in states with a mixed paroling regime, keeping in mind the caveats mentioned earlier on the limitations of such a test in a regression framework. Still, a positive coefficient β_5 is suggestive that the parole board systematically underestimates African American inmates' violation risk or discriminates in favour of African American inmates by releasing them too early.

Table 2 presents the results of the regression analysis. In column (1), showing results for 'parole board only' states, we observe that the amount of sentence served has a statistically and substantially insignificant effect on the violation rate and the effect is similar across racial groups. This first result is consistent with parole boards using the time of release strategically. Further, being African American is associated with an average of twelve percentage points increase in the probability of parole violation. In Columns (2)-(5), where we add states with a mixed paroling regime, the effect of time served for parole board releases is the sum of the coefficients β_1 and β_4 among Whites and the sum of the coefficients β_1 , β_4 , β_6 and β_7 among African Americans. Across the different specifications, the p-value associated with those sums reaches the 5% significance level only once (for Whites, in a specification that mixes court commitments and parole violations reentries) and the coefficient is always substantively small. The interpretation is that an extra month in prison leads to an absolute increase in the probability of violation of at most 0.001 in the parole board regime, *ceteris paribus.* F-tests on the joint impact of sentence served within African Americans and Whites never reach the 5% level (and reach the 10% level twice). By contrast, the effect of time served within the mandatory parole stream is always significant at the 5% level and is between twice and five times larger across specifications. With one exception across all specifications, there is no statistically significant difference between African Americans and Whites in terms of the effect of time served on violation in either regime (coefficients β_6 and β_7).

The large positive effect on violations associated with the parole board itself measured by β_2– ten to fifteen percentage points – is puzzling, and to our knowledge new in the literature. The interpretation can only be speculative given the different effects mentioned. However, this result alone lends indirect support to the ideology that led to the determinate sentencing reform in half the United States. Parole boards' decisions were perceived not only as too discretionary and perhaps unpredictable, but also as too lenient compared to what was

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[21]For some states, education is also provided. We do not use it in the baseline regression to include as many states as possible. The analysis brings similar results when we restrict attention to states with data on education.

achievable under a mandatory parole regime. The coefficient β_5 measuring the differential effect of being African American in the parole board stream is troublesome and provides further justification for the implementation of an outcome test. It points directly to a positive parole board bias towards African American parole candidates, even though there is a positive effect within mandatory release in the first place, captured by β_3. In California (Columns 6-8), the effect of time served appears somewhat smaller than for mandatory releases in the mixed regime states but is significant at the 1% level across groups in all specifications. So is the effect of being African American on violation, albeit the effect is substantively small.

We conclude that there is substantial support for the idea that parole boards strive to equalize the probability of violation within each group. However, testing the strategic timing hypothesis is only an intermediate step. The important point is that our analysis so far provides evidence that inframarginality should not bias the result of the outcome test.

6.2. Outcome Test

6.2.1. Statistical test of discrimination

Our test for prejudice compares the probability of parole violation across races. The theoretical model leads to the strong implication that violation rates should be equal if parole boards are racially neutral in their decisions. We therefore test the null hypothesis that the parole system is racially neutral:

$$VIOL_{AA} = VIOL_W$$

We use a simple nonparametric test, the Pearson χ^2 test, which compares the observed violation count in each group against the count that is expected under the null hypothesis of no prejudice of the parole board. That is, the statistic for testing the hypothesis that the parole board is treating two different ethnic groups differentially is:

$$\sum_r \frac{\left(\widehat{VIOL}_r - VIOL_r\right)^2}{VIOL_r} \sim \chi^2(1)$$

where $VIOL_r$ is the expected violation count of group r under the null and $\widehat{VIOL}_r$ is the observed violation count .

6.2.2. Test Results

Table 3a reports violation rates for Whites and African-Americans following the samples selected for specifications 1, 3 and 7 in Table 2.[22] As expected the outcome test results confirm their linear regression counterparts (coefficient β_5). African-American parolees have an average violation rate of 43% while White parolees have an average violation rate of 32%.[23] For states with a mixed regime, the figures are sensibly the same when parole is granted through a parole board, which provides an external validation of the results found in the indeterminate sentencing states. However, both violation rates (in each group) and, most notably, the differential are smaller when parole is granted through a mandatory parole mechanism. California, a pure determinate sentencing state, provides an extreme illustration of this finding.

Table 3b reports state by state violation rates of Whites and African-Americans who have gone through a parole board, in states with indeterminate sentencing and in mixed regime states. The data, and this is confirmed by the CPUS, indicate stark differences in average rates of violation across states. However, those differences are of no direct concern since we focus on the African American - White violation differential. We see that in every state this differential is significant both statistically and economically, and remarkably consistent in magnitude. The violation rate differential is smaller than 10 percentage points only in Utah.

Our results are robust to a breakdown of groups according to sentence length and category of crime (Table 3c).[24] Clearly even if parole boards take into consideration the type of crime and the dangerousness of the criminal, as measured by sentence length, when they take their decisions, the violation rate differential remains. In other words, the difference of violation rates in the different ethnic groups we consider cannot be explained by differences in the composition of crimes. Note that from Table 3c we can also reject the hypothesis that parole boards may be less concerned with African American offenders because they mostly commit crimes within the African American community (the rationale being that these get less publicity, so the parole board may not care as much): for crimes against the state, *i.e.*, that do not involve individual victims, the results are qualitatively consistent, though smaller in magnitude. Similarly we can reject the hypothesis that because African American offenders are on average (slightly) younger parole boards would display leniency on account of age.

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[22] Although the states we consider are the same as those used for the study of strategic timing our results are similar when we apply the outcome tests to states that were excluded for one reason or another from the strategic timing analysis.

[23] This difference is calculated for prisoners who entered prison following a court commitment – although violation rates for those prisoners who entered prison following an initial parole violation are significantly higher, the African American - White differential (about eleven percentage points) is similar.

[24] We limit our analysis to the first count of the charge.

6.3. Interpretation of the results

Overall, Tables 3a, b, c suggest that the parole system systematically underestimates African American parolees' violation risk. If there is any discrimination, it is against White prisoners who are not released as early as they should be given their risk of violation. These empirical findings are somewhat surprising and quite at odds with the conventional wisdom that African Americans are discriminated against in the U.S. criminal justice system. According to our model, the differential in average violations is to be interpreted as prejudice towards White parole applicants and the acceptable odds of violation are higher for African Americans. In the notation of the model, the empirical results means that $\lambda_{AA} > \lambda_W$. Given the definition of $\lambda = \frac{C_1}{C_1 + C_2}$, a difference in thresholds may be explained by a difference in type I or type II costs leading to two possible interpretations.

The first possible explanation for the evidence could be that the observed differential in violation rates is not the result of parole board's policy at the release stage but the result of discrimination at the supervision stage. This would correspond to the parole board having a lower cost of type II errors for African American parolees. One way to test whether discrimination happens post prison release would compare the reasons for parole violation across groups. Intuitively, parole officers's bias against African Americans should lead to a higher proportion of technical violations leading to revocation as opposed to new sentences for African American violators relative to Whites. There is plausibly less room, if any, for parole officers to exercise discretion when a new offence is committed. The NCRP contains only crude data on the cause of parole violation (*i.e.*, only whether there is a new sentence (or charge pending), or whether the parole is revoked, which means a violation of the conditions of parole; obviously we would like more information on the characteristics of each). Nevertheless, we checked that African American violators are still relatively more likely to return to prison on account of new charges when released by a parole board, compared to when released through mandatory parole (Table 4a). This finding also addresses the possible objection that perhaps the objective of the parole board is not avoiding violations but avoiding that parolees commit new crimes. Hence, even if this were the case, the same bias, though smaller in magnitude, would still remain.

In addition, we can isolate the effect of the parole board by making the assumption that any discriminatory behavior by parole officers and the police is immune to the way a prisoner is paroled. When African-American and White parolees released under both discretionary and mandatory parole coexist within one jurisdiction, Table 3a shows that African-Americans violate parole significantly more often when they have gone through discretionary parole suggesting that there is no post release discrimination.[25] Further on that line of reason-

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[25]Still, if certain offenses, for which African Americans are over represented, led to a higher likelihood of post release discrimination, one may be misled to conclude that parole boards are biased in favour of African-American prisoners. As already mentioned, applying the test for the most common types of crime, however, shows that this critique fails.

ing, Wisconsin's institutional changes provide a natural testing ground. In 1999, Wisconsin switched to a system of determinate sentencing. For prisoners admitted after 1999, parole board releases do not disappear entirely after 1999 yet decline every year, down to zero in the small sample of the last year of data. Notwithstanding censoring,[26] we can compare the differential violation rates between those paroled by a parole board under indeterminate sentencing and those paroled though mandatory parole, after the move to determinate sentencing in 1999, during the period where those two groups coexist. It is most unlikely that parole officers or the police would discriminate based on sentencing date.[27] Table 4b reports parole releases occurring after 1999, among male prisoners admitted before 1999 and granted parole by a discretionary parole board. We see that African American parolees violate parole by almost twenty two percentage points over Whites. For parole releases occurring after 1999, among male prisoners admitted after 1998 and granted parole through mandatory parole that same difference shrinks to twelve percentage points ($p<0.01$). The higher violation rate among both African Americans and Whites under mandatory parole after 1999 can be explained by the fact that we observe fewer successful parole exits as we move closer to the last year of prison exits, i.e., most parolees are still under supervision and have not violated their parole hence are not included in the data.

An alternative explanation is that parole boards refuse to consider race as a valid characteristic to use in their parole decisions. Indeed, if parole boards strived for an equalization of violation rates, this would be consistent with a racially neutral objective, but also with potential racial profiling. We do not want to take position on the debate about the legal or ethical nature of statistical discrimination, especially since it is impossible to disentangle the use of race from the use of other observable characteristics that are correlated with race. There is support for that hypothesis in Table 5. Table 5 shows that for a given maximum sentence length, parole is granted at the same time on average for African Americans and Whites, resulting in equal parole terms. Hence it appears that parole boards do not react to higher violation rates in the African-American parolee population by keeping these prisoners for a longer time in prison. Therefore, both time spent behind bars and violation rates are not consistent with a bias of the parole system against African-Americans. Further, it could be that in order to appear as racially neutral as possible, parole boards may feel compelled to equalize time spent in prison for a given maximum sentence length, and in any case, to keep the differential not larger than in mandatory regimes. Indeed, in the data, the differential is insignificant between the two regimes – except in California. However, the major difference is that paroled inmates spend significantly more time behind bars under a mandatory regime. Therefore, by releasing inmates earlier on in a uniform way, parole boards appear to create

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[26]Censoring could be a problem if conditional on violating parole, Whites tend to violate later than African Americans. Nothing in the data supports that objection.

[27]Note that after 1999 both African Americans and Whites appear to violate more often, but this just stems from censoring: we logically observe a disproportionate amount of parole failures since by 2003 future parole successes are by definition still under parole and not counted in the NCRP yet. What matters is the difference in violation rates.

the conditions for a higher chance of recidivism among African Americans, which could be the product of a network effect if the circumstances leading to parole revocation and criminal activity are more prevalent in some predominantly African American communities.

7. Conclusion

We develop a simple test to uncover possible discrimination in discretionary parole releases. We argue that a simple outcome test that compares the average success of parole of different groups is sufficient to uncover evidence of bias in the parole board's behavior. A lower rate of success means that the parole board is too lenient in its decisions concerning that group. We show that in all U.S. states where we have good data the outcome test leads to the conclusion that White parolees have a higher success rate, suggesting discrimination against Whites. This results put into question the conventional wisdom that the U.S. criminal justice system is biased against African Americans. We do not have a definitive explanation for what appears to be a case of reverse discrimination and our test cannot assist in that regard. We verified that it is unlikely that our results could be accounted for by discrimination of parole officers who are in charge of parole supervision or by the police. We suggest that parole boards appear to be biased in favour of African American prisoners because parole boards equalize time spent in prison between African American and White prisoners, possibly to ensure *ex ante* fairness, yet this results in higher violation rates among African American parolees *ex post*. Alternatively it could be that parole boards feel pressure to be more lenient with African American prisoners to compensate for some perceived discrimination at other stages in the judicial system. One way to address that conjecture would be to examine data on the stock of prisoners who are denied parole: yet we only have data about parolees (who end their parole either by a success or a violation). Also, it could be that the forecasting models used to probe risk of recidivism are not working for certain offences committed relatively more often by minorities.[28] In any event, the widespread curtailing or abolition of discretionary parole since the late 1970s was motivated by concerns of not merely uncertainty and disparity but also by concerns of bias in discretion – including allegation of discrimination (see Dharmapala, Garoupa, and Shepherd, 2006). Our results, in somewhat unexpected ways, provide a new justification for such changes.

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[28]"Nearly all American parole release agencies have adopted incapacitation through actuarial risk assessment as a major component of their decision making process. This is a low-visibility policy built on imperfect risk-prediction technology that is not subject to meaningful challenge by prisoners in their own cases." (Reitz, 2011)

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Table 1
SUMMARY STATISTICS FOR SPECIFICATION 3

Variable	Obs	Mean	Std. Dev.	Min	Max
Violation	102,765	.38	-	0	1
Time served	102,765	27.56	25.48	.1	421.3
Parole board	102,765	.80	-	0	1
Black	102,765	.49	-	0	1
Max. sentence length	102,765	106.70	107.25	1	7,764
Age at prison release	102,765	31.49	9.17	15.8	86.4
Year of prison release	102,765	1991.44	4.40	1961	1997

Table 2
TESTING STRATEGIC TIMING

<u>OLS Regressions with Dependent Variable: "whether violated parole"</u>

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Time served × Parole Board× Black		≈0	-0.001	-0.001	≈0			
		(≈0)	(0.001)	(≈0)*	(≈0)			
Total Time served × Parole Board		-0.003	-0.004	-0.003	-0.003			
		(0.001)+	(0.001)**	(0.001)*	(0.001)*			
Total Time served × Black	≈0	≈0	≈0	≈0	≈0	≈0	≈0	≈0
	(≈0)	(≈0)	(≈0)	(≈0)	(≈0)	(≈0)*	(≈0)**	(≈0)*
Black × Parole Board		0.066	0.095	0.072	0.078			
		(0.012)**	(0.01)**	(0.013)**	(0.01)**			
Time served	≈0	0.004	0.004	0.004	0.002	≈0	0.001	0.002
	(≈0)	(0.002)*	(0.001)**	(0.001)*	(0.001)**	(≈0)**	(≈0)**	(≈0)**
Parole board		0.1	0.153	0.11	0.103			
		(0.07)	(0.048)*	(0.05)+	(0.05)*			
Black	0.126	0.056	0.028	0.043	0.031	0.013	0.012	0.028
	(0.006)**	(0.013)**	(0.003)**	(0.001)**	(0.006)**	(0.003)**	(0.003)**	(0.003)**
State dummies ×Year of prison release	Yes	Yes	Yes	Yes	Yes			
Adjusted R2	0.056	0.066	0.11	0.057	0.076	0.025	0.026	0.08
# Observations	30,594	57,520	102,765	74,983	254,594	103,718	103,758	107,819

SOURCE: National Corrections Reporting Program: Parole Release files (1983-2003). States selected on the basis of data availability, internal consistency in reporting and external validation with

Correctional Populations in the United States data 1985-2002 & Probation and Parole Bulletins 1983-1984.

NOTE: All models control for age at prison release, total maximum sentence length (all offenses), year of prison release and state fixed effects.

ADDITIONAL RESTRICTIONS: Only male parolees admitted to prison under court commitment (*e.g.*, no parolees admitted to prison under a prior parole revocation) were included – except in specification (3), (5), (7), (9). Year of prison release < 1998 to limit censoring (by 2003 over 95% of parolees have either been released from parole successfully or have violated parole (either through technical violation or new offense)). Time served includes prior time served except in specifications (4), (5), (8), (10)

SPECIFICATIONS: Columns (1)-(5) parolees who entered prison under indeterminate sentencing, with no Truth-in-Sentencing program or other parole restriction reform (see Sabol *et al.*, 2002). Column (1): states with discretionary parole board as sole mechanism of release within the restricted sample (Michigan, North Dakota, Utah, and Colorado). Column (2)-(5): states with discretionary parole only and states where discretionary and mandatory parole coexist (Michigan, North Dakota, Utah, and Colorado; Wisconsin, Texas, Missouri, Arkansas).

Column (1): F-test for {prison served + prison served * black}: p-value = 0.4

F-test for {prison served + prison served * black} and {prison served}: p-value = 0.5

Column (2):

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black}: p-value = 0.11

F-test for {prison served + prison served * parole} p-value = 0.054

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black} and {prison served + prison served * parole}: p-value = 0.091

Columns (3): same as Columns (2) but time served not counting prior jail time:

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black}: p-value ≈ 1

F-test for {prison served + prison served * parole}: p-value = 0.71

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black} and {prison served + prison served * parole} p-value = 0.78

Columns (4): same as Columns (2) but without restriction on type of admission (now includes New York):

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black}: p-value = 0.1

F-test for {prison served + prison served * parole}: p-value = 0.04

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black} and {prison served + prison served * parole}: p-value = 0.072

Column (5): same as Columns (2) but time served not counting prior jail time and without restriction on type of admission (now includes New York):

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black}: p-value = 0.6

F-test for {prison served + prison served * parole}: p-value = 0.36

F-test for {prison served + prison served * parole+ prison served * black + prison served * parole * black} and {prison served + prison served * parole}: p-value = 0.58

Columns (6): mandatory releases in California only [note: this restricted sample of California prisoners serves a much higher fraction of their maximum offense length than other prisoners in our samples of indeterminate states];

F-test for {prison served + prison served * black}: p-value = 0.03

F-test for {prison served + prison served * black} and {prison served}: p-value ≈ 0

Columns (7): same as Columns (6) but time served not counting prior jail time

F-test for {prison served + prison served * black}: p-value = 0.002

F-test for {prison served + prison served * black} and {prison served}: p-value ≈ 0

Columns (8): same as Columns (6) but without restriction on type of admission

F-test for {prison served + prison served * black}: p-value ≈ 0

F-test for {prison served + prison served * black} and {prison served}: p-value ≈ 0

Table 3a

Rate of violation of parole by race

	Indeterminate sentencing states	Mixed regime states (Parole Board)	Mixed regime states (Mandatory Parole)	California
African American	0.441	0.471	0.354	0.0611
White	0.316	0.329	0.286	0.0463
P-value on Pearson Chi-squared Test	*<0.001*	*<0.001*	*<0.001*	*<0.001*

Table 3b

Rate of violation of parole by race and state (indeterminate sentencing)

	Colorado Violation rate	Michigan Violation rate	North Dakota Violation rate
African American	.603	0.427	0.226
White	.45	0.30	0.142
P-values	*<0.001*	*<0.001*	*<0.001*

	Arkansas Violation rate	Missouri Violation rate	Texas Violation rate
African American	.427	0.502	0.585
White	.0263	0.376	0.467
P-values	*<0.001*	*<0.001*	*<0.001*

	New York Violation rate	Utah Violation rate	Wisconsin Violation rate
African American	.501	0.576	0.341
White	.379	0.538	0.207
P-values	*<0.001*	*<0.001*	*<0.001*

Table 3c

Rate of violation by type of crime categories and length of sentence

(indeterminate sentencing)

	Drug related crimes	Sexual crimes	Murders and homicides	Assault
African American	.401	0.371	0.387	0.422
White	.214	0.185	0.22	0.33
P-values	*<0.001*	*<0.001*	*<0.001*	*<0.001*

Crimes against the state (no individual victim)	Robbery	Sentence less than 24 months	Sentence of 24 to 120 months	Sentence longer than 120 months
0.375	.531	0.16	0.397	0.569
0.322	.411	0.117	0.294	0.412
<0.001	*<0.001*	*<0.001*	*<0.001*	*<0.001*

Age <25 at prison release	Age >24 at prison release
0.485	0.465
0.376	0.312
<0.001	*<0.001*

Table 4a

Rate of violation of parole from new sentence or charge pending only, by race

	Indeterminate sentencing states	Mixed regime states (Parole Board)	Mixed regime states (Mandatory Parole)	California
African American	0.164	0.096	0.035	0.007
White	0.097	0.066	0.03	0.003
P-value on Pearson Chi-squared Test	*<0.001*	*<0.001*	*<0.01*	*<0.001*

Table 4b

Wisconsin (Parole release after 1999 after switch to determinate sentencing)

	Admitted to prison before 1999, released through parole board	Admitted to prison after 1998, released through mandatory parole
African American	.54	0.459
White	.325	0.335
P-values	*<0.001*	*<0.001*

Table 5

Sentence served at parole release as a function of race

OLS Regressions with Dependent Variable: "sentence served"

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	-.673	.167	-.008	.0091	.34566	2.05	1.317	2.099
	(.133)*	(.284)	(.200)	(.218)	(.239)	(.160)**	(.146)**	(.144)**
Parole board	-	-9.903	-9.809	-10.093	-8.908			
		(1.936)**	(.687)**	(1.688)**	(2.247)**			
Black × Parole Board	-	-.217	.421	.211	-.0312			
		(.557)	(.721)	(.7493)	(.852)			
State dummies ×Year of prison release	Yes	Yes	Yes	Yes	Yes			
Adjusted R2	0.3	0.4	0.36	0.4	0.3	0.44	0.44	0.44
# Observations	30,594	57,520	102,765	74,983	254,594	103,907	103,758	107,819

SOURCE: National Corrections Reporting Program: Parole Release files (1983-2003). States selected on the basis of data availability, internal consistency in reporting and external validation with Correctional Populations in the United States data 1985-2002 & Probation and Parole Bulletins 1983-1984.

NOTE: All models control for age at admission, total maximum sentence length (all offenses), year of prison release and state fixed effects.

ADDITIONAL RESTRICTIONS: Only male parolees admitted to prison under court commitment (*e.g.*, no parolees admitted to prison under a prior parole revocation) were included – except in specification (3), (5), (7), (9). Year of prison release < 1998 to limit censoring (by 2003 over 95% of parolees have either been released from parole successfully or have violated parole (either through technical violation or new offense)). Time served includes prior time served except in specifications (4), (5), (8), (10)

SPECIFICATIONS: Columns (1)-(5) parolees who entered prison under indeterminate sentencing, with no Truth-in-Sentencing program or other parole restriction reform (see Sabol *et al.*, 2002). Column (1): states with discretionary parole board as sole mechanism of release within the restricted sample (Michigan, North Dakota, Utah, and Colorado). Column (2)-(3): states with discretionary parole only

and states with discretionary and mandatory parole (Michigan, North Dakota, Utah, and Colorado; Wisconsin, Texas, Missouri, Arkansas).

Columns (3): same as Columns (2) but time served not counting prior jail or prison time.

Columns (4): same as Columns (2) but without restriction on type of admission (now includes New York).

Column (5): same as Columns (2) but time served not counting prior jail or prison time and without restriction on type of admission (now includes New York.

Columns (6): mandatory releases in California only [note: this restricted sample of California prisoners serves a much higher fraction of their maximum offense length than other prisoners in our samples of indeterminate states];

Columns (7): same as Columns (6) but time served not counting prior jail or prison time

Columns (8): same as Columns (6) but without restriction on type of admission