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2015

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Stéphane Mechoulan and Nicolas Sahuguet

ABSTRACT
In a rational choice model of parole release, a color-blind parole board seeking to minimize violations would release all prisoners below a certain risk threshold. To test this prediction, we extend the outcome-test methodology used in assessing discrimination in police searches. We overcome the inframarginality critique by taking advantage of strategic timing of release: within each racial group, violation rates are equalized for a given sentence length. We use the National Corrections Reporting Program data, which record all parole-release decisions in the United States. We find that violation rates are consistently higher for African American parolees, a result not consistent with a parole board bias against African Americans. This conclusion is robust to a variety of tests, including ruling out postrelease discrimination. Evidence on the timing of release suggests a policy aimed at limiting racial disparities in time served rather than in violation rates, which favors fairness over efficiency.

1. INTRODUCTION
The overrepresentation of minorities at all stages of the criminal justice process is striking, and the proportion of African Americans in prison

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or jail more than doubled over the 1980s and 1990s. One in 10 African American males ages 25–29 was behind bars in 2009. In contrast, one in 23 Hispanic males and one in 66 non-Hispanic white males in the same age group were incarcerated (West 2009). It is important to know to what extent this gap reflects real differences in criminal behavior as opposed to disparities in the treatment of minorities at the prosecution, sentencing, and parole stages. In this paper, we address the question of race 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, as opposed to arrests or sentencing, is a purely administrative decision contributes to this neglect. We address whether parole boards use information about race in their decision making and, if so, whether African American prisoners are treated unfairly as a result.

Studying the role of race in public decision making is politically sensitive. Further, producing compelling statistical evidence for that purpose is difficult. Most studies dealing with racial discrimination are susceptible to the omitted-variable-bias critique that plagues conventional tests based on regression analysis. An apparent significant effect of race may 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, and so forth. Against that, we build on the outcome-test methodology used to assess racial profiling in police search decisions (see Knowles, Persico, and Todd 2001). The basic idea of the methodology is to compare the outcome of the decision maker’s action (for example, in the police context, finding drugs after a search) and analyze whether that outcome is systematically different for minorities and nonminorities (for a good summary of the advantages of outcome tests, see Ayres 2002).

Consider the argument that police searches focus on young African Americans and, as a consequence, impose a disproportionate cost on this group. Differences in search rates and the associated disparate impact of police behavior have been interpreted as discrimination and even as racial animus on the part of the police. However, reporting hit rates (for example, successfully finding drugs during a search) is now common in racial profiling reports. Equality of hit rates across groups is interpreted as evidence that searches are conducted with an efficiency rationale. The

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1. Even critics of racial profiling now view hit rates as an interesting statistic to better understand police behavior. See, for instance, Leadership Conference on Human and Civil Rights (2011).
debate (from both academic and legal circles) has thus evolved toward the analysis of a possible trade-off between efficiency (maximizing hits) and fairness (minimizing disparate costs on groups).

As reported in Harcourt (2007), studies of parole have a long history of using statistical methods. Starting with Burgess (1928), parole-prediction instruments have been developed to forecast the success of any inmate eligible for parole. While success may be measured in different ways, the literature suggests that parole boards are mostly concerned with avoiding parole violations of released inmates. Most parole boards currently use sophisticated risk-assessment instruments to minimize the risk of releasing the wrong kind of prisoners, namely, those who are likely to violate their parole conditions and reoffend. At the same time, parole boards presumably strive to provide the same opportunities to prisoners exhibiting similar sentencing profiles, regardless of race. Thus, to make the analogy with police searches, the timing of release corresponds to police search rates, while parole violations correspond to hit rates. We believe that outcome testing is therefore a useful tool in assessing a similar efficiency-fairness trade-off in the parole decision-making process.

However, outcome tests also have some issues, and in particular, one must deal with the inframarginality critique. The drawback of the method becomes apparent when researchers are able to measure only the average outcome and not the outcome associated with marginal decisions. In our case, we observe only average rates of violation and not the marginal ex ante probabilities of violation. By construction, the average takes into account not only marginal but also inframarginal individuals. Given that the composition of the groups of interest can differ in terms of such inframarginal individuals, differences in average rates of violation could be observed even when the marginal individuals (on whom we should focus) are identical. Our approach to dealing with inframarginality parallels the method used in Ayres and Waldfogel (1994), which studies discrimination in bail bond settings. In our context, the fact that

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2. Success in other contexts (see, for instance, Wilson, Gallagher, and MacKenzie 2000) is measured by rates of recidivism, defined as returning to prison or jail within 3 years of prison release. We prefer to use violation as the main variable of interest. For short sentences, what happens after the end of the parole period (but within the 3-year window) is not within the parole board’s purview. Conversely, for longer sentences, we do not see why the parole board would restrict its attention to a 3-year window.

3. In Ayres and Waldfogel (1994), judges use the amount of bail to deter flight. This should lead to equal fail-to-appear (FTA) rates. Since they do not have data on FTA rates, they implement the outcome test with data on the bond rates used by professional bond setters, which also should be equal across groups in the absence of discrimination.
a parole board chooses 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. Kuziemko (2013) confirms this view: parole boards set prison time strategically to reduce recidivism. Thus, a prisoner is released when the probability that he will violate parole has just become acceptable. The theoretical implication of strategic timing is that all prisoners may be considered marginal when released: this 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 would occur within each racial group, but there may still be a systematic difference across groups. Thus, from a methodological standpoint, one contribution of our paper is to develop a new way to deal with the challenge of inframarginality in outcome tests.

The relevant testable implication of the strategic-timing hypothesis is that the amount of time served in custody should not affect violation rates within each group. The validation of this hypothesis would make us confident that the outcome-test approach will deliver a clear assessment of racial disparities in the parole process.

To address the problem formally, we develop a simple rational choice model of parole release whereby we assume that a parole board aims to minimize violation rates. We show that a strategic, efficiency-driven parole board would set the same threshold rate of violation for white and African American prisoners. From there, differences in observed violation rates would indicate different standards in release decisions.

Our data provide an adequate testing ground for this inquiry. We take advantage of a unique data set that reports a large sample of prisoners released from parole from 1983 to 2003 and is collected by the National Corrections Reporting Program (NCRP). Release from parole can be summarily described as either a successful discharge or a parole violation. Given our interest in parole board decision making, we focus on states with indeterminate sentencing. Indeterminate sentencing is a system in which a legislature establishes minimum and maximum prison terms for each crime. In the sentencing phase, the judge then selects a range of im-

4. A weaker version of that proposition is that for a given sentence the amount of time served in custody should not affect violation rates within each group. It is conceivable that sentence length conveys a signal about the nature of the possible parole violation. For example, a parole board may tolerate a higher fraction of violations among low-danger criminals (who are serving short sentences).
prisonment duration. Subsequently, a parole board determines within that range the amount of time each inmate must serve. Conversely, determinate sentences correspond to a fixed period of confinement, with possible time reduction for good behavior and little if any discretion left to parole boards.

To test the strategic-timing-of-release hypothesis, we run regressions taking the probability of violation as our dependent variable and the amount of sentence served as our predictor of interest. We show that for both African Americans and white parolees, the length of sentence served has little, that is, statistically insignificant and substantially small, explanatory power for the violation rate of parolees in a discretionary parole regime.5

To implement the outcome test, we compare the rates of parole violation for African Americans and whites in states that have a discretionary regime of parole release. The null hypothesis is that of equal violation rates across races. Yet we find that in almost every state African American parolees are about 10 percentage points more likely than white parolees to violate parole.6 At first blush, this result is consistent with parole boards in discretionary parole regimes being more lenient in their release decisions when facing African American prisoners. To explore the robustness of our outcome-test results, we perform additional tests to check whether parole boards could have different objectives for different sentence lengths or types of offenses that African Americans and whites do not commit in the same proportions. Our results hold for different sentence lengths and types of crime.

We also find 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 release exist and in Wisconsin after it adopted determinate sentencing in 1999 (the only switching state with exploitable data).7 Hence, this evidence casts doubt on the hypothe-

5. We also find that the amount of sentence served significantly and substantially affects the probability of violation when prisoners are released through 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 National Corrections Reporting Program (NCRP) data.

6. This confirms on a national scale some localized observations, such as that for Michigan presented in Citizens Alliance on Prisons and Public Spending (2003).

7. That is, after 1998, 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.
sis that postrelease discrimination (for instance, by parole officers) could account for the higher parole violation differential observed under indeterminate sentencing.

Our finding that the rate of parole violation among African Americans is systematically higher than among whites may suggest that to avoid discrimination of African Americans, parole boards want to make sure that African American prisoners are treated like white prisoners in terms of timing of release rather than in terms of risk of violation. This hypothesis would reflect a concern for procedural fairness.

We observe evidence of this in the data, even though African Americans spend slightly more time than whites in prison on average for a given maximum sentence length in each regime. In states where the two regimes coexist, parole boards do not release African Americans relatively later than they are released under determinate sentencing. However, the crucial difference is that all inmates spend significantly more time behind bars under a determinate-sentencing regime.

These results highlight some noteworthy legal and ethical issues. By releasing inmates earlier in a quasi-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, such as more frequent interactions with non-law-abiding peers or poorer labor market prospects, are more prevalent in some predominantly African American communities. In other words, average success rates across parolees of different races would be equal only if parole boards were using race in their release decisions, which could be interpreted as discrimination. Conversely, the consequence of parole boards not using race as a factor in parole decisions is higher violation rates for African American parolees. Paradoxically, parole boards thus appear to hold white prisoners to a higher standard than African American prisoners in terms of their expected rate of parole success.

In a sense, by failing to keep African American offenders longer in prison at the aggregate level, parole boards are not fulfilling their mission, namely, minimizing parole violation. However, given the uncertainty of predicting parole violation at the individual level, denying parole to African American prisoners and having them spend more time behind bars would be unconscionable. This trade-off between efficiency and fairness is at the heart of the debate on racial profiling (see Risse and Zeckhauser 2004; Durlauf 2006).
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, and some of it addresses arrests and prosecution (including denial of bail and probation). 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 and finds mixed evidence of discrimination. Many of these studies have been criticized for methodological deficiencies, including sample selection, small-sample issues, and omitted-variable bias. Recent studies have tried to move beyond the omitted-variable-bias critique by using, for example, exogenous variations between judges (Abrams, Bertrand, and Mullainathan 2012), variations between judicial characteristics (Schanzenbach 2005), or the random timing of events (Shayo and Zussman 2011).

2.2. Paroling Decisions

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.

There exists a small theoretical literature on parole decisions. Lewis (1979) was the first to cast the parole decision in a rational choice model. The main prediction is that criminals with higher risk of recidivism should be released at later periods. Fabel and Meier (1999) extend his analysis. Bernhardt, Mongrain, and Roberts (2012) explore the effect of sentence length and discretion on the timing of paroling decisions and insist on the incentive role of such decisions.

There is, however, little connection between theoretical models and empirical research. An important exception is Kuziemko (2013). Exploiting quasi-natural experiments in the state of Georgia, she shows that more time in prison reduces recidivism risk and that parole boards use the information at their disposal and their discretion in the timing of release in an efficient way. Still, little is known empirically about the parole process and the variables that affect parole-release decisions. Noting that most of the research over the last 30 years has focused on states that moved away from parole board decision making, Reitz (2012, p. 271)

8. Danziger, Levav, and Avnaim-Pessoa (2011) also document the importance of discretion in parole release in the context of parole boards in Israel.
argues that “what is needed is nothing less than a new field of ‘indeterminate sentencing studies.’”

We concur with Morgan and Smith (2008) in the assessment that, in general, the research that specifically examines racial bias in parole decision making is dated and that its findings are ambiguous. For example, Steen and Opsal (2007) look at whether race has a direct effect on parole revocation. Their contribution 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 and interpret their results indecisively in our view. A general concern with this literature is its narrow focus in terms of time period and geographical area and its corresponding small sample sizes. In turn, regardless of methodological considerations, most studies lack external validity.

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 suggests that if banks discriminate against minorities, we should expect minorities to exhibit lower default rates. Since then, outcome tests have been used to study, for example, bail-bond-setting decisions (Ayres and Waldfogel 1994) and 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 searches for contraband is a good way to test whether the police require less probable cause when searching minorities (see also Anwar and Fang 2006; Persico and Todd 2006; Sanga 2009). Persico and Todd (2005) use a similar test to analyze discrimination by customs officers during airport searches. Alesina and La Ferrara (2011) analyze discrimination in capital sentencing using an outcome test and find evidence of bias against minority defendants who killed white victims.

Our paper contributes to this literature in two ways. First, parole-release decisions constitute a novel application of the outcome-test methodology to the pervasive topic of discrimination in the criminal justice system. Second, it contributes to the debate over the inframarginality critique often associated with outcome tests. The idea that a continuous control variable affecting the outcome of interest may help deal with inframarginality is introduced in Ayres and Waldfogel (1994) in the context
of bond setting and is further discussed in Ayres (2002). More recently, Anwar and Fang (2012b) use that idea when investigating racial prejudice in the behavior of emergency department physicians. In their model, doctors want to avoid bounce backs, that is, situations in which a patient is discharged from the emergency room on account of suffering a minor ailment only to come back within 3 days with a major condition. As in our model, the optimal policy is characterized by an optimal threshold of acceptable risk. Compared with Ayres and Waldfogel (1994) and Anwar and Fang (2012b), we do not only provide a theoretical solution to the inframarginality problem. Our data also provide us with the opportunity to directly test our model empirically. This empirical confirmation gives added weight to the plausibility of our approach.

Finally, Anwar and Fang (2012a) is a closely related paper that studies recidivism in the Pennsylvania Department of Corrections. Like us, they use an outcome test, and their solution to the inframarginality problem comes from the use of strategic timing of release. The key idea is that time spent in prison affects the outcome of interest, namely, recidivism. There are two main differences between the papers. The first difference stems from the data used. Our paper uses the NCRP data set (US Department of Justice 2005). It is a much larger data set that covers most US states for a period of over 20 years. Their data are from the Pennsylvania Board of Probation and Parole and cover the period 1996–2001. The main advantage of their data is more detailed information about the timing of release. In particular, their data allow them to identify precisely prisoners who are released immediately after serving the minimum sentence. Another advantage of their data is better information on the reason behind parole revocation, for example, whether it is a technical violation, a new crime, or a new violent crime. The second difference is that the model is based on a different objective function for the parole board. They assume that parole boards balance the cost of holding a prisoner for a particular time period with the probability of recidivism. While we compare the success rate of different racial groups over the entire duration of parole, they compare recidivism rates measured per unit of time. They measure recidivism in an 18-month window after release from prison (irrespective of the length of parole). In terms of results, neither paper finds evidence of racial bias against African Americans in the parole-release process. However, some of their empirical results differ from ours. In particular, when violation is defined as a new violent crime, they find no evidence of differential treatment by the parole board (that is, violation rates for African Americans and whites are the same).
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 time for good behavior. 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. . . . 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, Rengifo, and Wilson 2005, p. 10)

Slightly more than 40 percent of released inmates are granted a mandatory release. Notably, determinate sentencing was instituted partly to address the suspicion that parole boards’ broad discretion would lead to discrimination (Reitz 2001). Finally, unconditional release, that is, release with no further correctional supervision, applies to any inmate who has completed a sentence in full, received a pardon, or had a sentence 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.

Rehabilitation is the main stated objective of the paroling process. Yet criminologists have argued that the economic and political environment, not inmate or offense characteristics, significantly affect parole decisions (see Branham 1983; DeGostin and Hoffman 1974; Gottfredson and Ballard 1966; Parsons-Lewis 1972; Pogrebin, Poole, and Regoli 1986). Simon (1993) notes that the transformation of the parole process has been affected by the political and economic landscape, which has caused rehabilitation to be replaced by management concerns, namely, efforts to
control prison population overcrowding.\textsuperscript{9} Finally, parole may be used to remedy sentencing disparities for inmates who are perceived to have been sentenced unfairly because of race or ethnicity, gender, or social class (Hofer, Blackwell, and Ruback 1999). Regardless of the ideology underlying parole, we need to make sense of how it operates in practice.\textsuperscript{10} Maltz (1984, p. 10) explains:

As von Hirsch . . . 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. . . . 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.” . . . 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.”

Since failure (reoffending) is easier to measure than success (parolees finding a job, being reattached to the labor force for a long period of time, and so forth), evaluations are thus failure based. Feeley and Simon (1992) characterize the shift in objective from 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.\textsuperscript{11}

Paroling guidelines identify the three variables that should take precedence in parole decisions: time served, offense seriousness, and risk of recidivism. Since the first two are perfectly observable by the parole board, only the latter is subject to question. While we do not observe

\textsuperscript{9} Although systematic studies of parole as a mechanism to regulate overcrowding 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 corrections officials.

\textsuperscript{10} Rhine (2012) provides a thorough discussion of the many considerations at play across parole boards. Our presentation is necessary simplified.

\textsuperscript{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. sec. 17-2-100.2)” (Reitz 2012, p. 227).
offense seriousness per se, the NCRP data contain the type of offense (possibly for several counts), detailed at a three-digit classification level, as well as maximum sentence length. We make the assumption that parole boards are concerned with the risk of recidivism during parole and not after the end of the prisoner’s sentence. 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 for them to adjust and increases their likelihood of lifelong recidivism. However, parole mandates explicitly mention the accountability of the parole board to the public for the inmates’ actions until the expiration of their sentences only. The Texas Board of Pardons and Paroles (2013, p. 3), for instance, is quite clear on the subject: “The Texas Board of Pardons and Paroles Guidelines combine a research-based risk assessment of the offender with a measurement of the severity of the offense. The risk assessment measures the likelihood of an offender to have a successful parole.” Success of parole is clearly defined as no violation of parole. “Parole Guidelines Scores range from one, for an individual with the poorest probability for success, to seven for an offender with the greatest probability for successfully discharging their sentence on parole without returning to prison” (Texas Board of Pardons and Paroles 2013, p. 7).

4. A MODEL OF DISCRETIONARY PAROLE RELEASE

We now present a simple model of parole-release decisions. Consider prisoners eligible for parole with race \( r \) and other characteristics. These characteristics encompass all information available to the parole board when it decides whether to release a prisoner. This information includes gender, age, type of crime, behavior in prison, and personal circumstances of the prisoner on release. Some of these variables are observed by the researcher, but other variables used by the parole board may not be accessible. Parole boards’ decisions come in two stages. They first determine a global risk threshold for release. The determination of this threshold takes into account the whole population of prisoners eligible for parole. Second, they determine for each individual prisoner the time of release, if any.

12. Incidentally, Norval Morris argues that the parole board’s information set for a given inmate is no greater than at the beginning of time served (Reitz 2004).
4.1. Setting a Global Level of Acceptable Risk

Consistent with Kuziemko (2013), we assume that the parole board uses all the information available to estimate the risk that a prisoner would violate the conditions of his parole. We create a single-dimensional index, $x$, to denote the parole board’s information about the characteristics of the prisoner. 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 corresponding probability distribution function $f(\cdot)$. We denote by $F_r(\cdot)$ the cumulative distribution function of $x$ among prisoners of race $r$.

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 who 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 error $C_1$ (keeping a prisoner incarcerated who would not violate his parole) and of type II error $C_2$ (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 cost 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

$$\min_{x_r} \int_0^{x_r^*} \lambda[1 - \pi(r, x)]f_r(x)dx + \int_{x_r^*}^1 (1 - \lambda)\pi(r, x)f_r(x)dx.$$  

The first-order conditions yield

$$\lambda[1 - \pi(r, x_r^*)(x)]f_r(x_r^*) - (1 - \lambda)\pi(r, x_r^*)f_r(x_r^*) = 0$$

and

$$\pi(r, x_r^*) = \lambda.$$  

The critical value $x_r^*$ represents the standard applied by the parole board in its release decisions. If the information about the prisoner is sufficiently favorable, which means that the expected violation is below the threshold
λ, the parole board decides to release the prisoner. The threshold \( \pi(r, x^*_r) \) is directly linked to the relative concern for type I error \( \lambda \): the greater the concern about keeping prisoners who are unlikely to violate their parole incarcerated, the more prisoners will be released on parole.

However, the model shows that the critical value \( x^*_r \) can potentially depend on race. The parole board could form different assessments of violation risk for prisoners with similar characteristics \( x \) but different races.\(^{13}\) Different critical values would thus be the result of the parole board using statistical discrimination to impose a common risk threshold across races.\(^{14}\) The main implication of our model is that the threshold does not depend on the distribution of risk characteristics but only on the relative concern for type I and type II errors. This means that parole boards would use the same threshold for all races if their concerns for releasing prisoners were independent of race. If the parole board set different thresholds for, say, different maximum sentence lengths or categories of crime, the main results of the analysis would not be affected.

Note that we could cast our model in a more standard format, that is, capturing the economic rationale of the optimal allocation of resources under a budget constraint. Framed in those terms, the parole board’s objective function would be to minimize the number of parole violations under the constraint of a number of prisoners being released. The idea that the parole board needs to release a given number of prisoners is consistent with the shortage in prison capacity coupled with an ever-increasing entry of new prisoners.\(^{15}\) This alternative formulation yields a similar cut-off rule.

To summarize, in the presence of a parole board with a race-neutral objective function, the risk threshold should not depend on race, and the acceptable risk for released prisoners should be the same across races. We should thus observe similar rates of violation. Otherwise, the parole board is using different parameters \( \lambda_r \) that depend on race \( r \). Racial bias in assessing recidivism risk would result in the parole board using different

\(^{13}\) It is a common argument that certain institutional factors lead to a higher propensity to commit crimes for African Americans: higher-crime neighborhoods, more interactions with criminal peers, and fewer labor market opportunities. These institutional factors would also be relevant to former inmates during their parole time and could make African Americans more likely to reoffend than whites coming out of prison.

\(^{14}\) We are not able to assess whether the board is using statistical discrimination (see, for instance, Curry and Klumpp [2009] for a discussion of this issue); we are able to identify only racial bias.

\(^{15}\) In Texas, during the mid- to late 1990s, the abrupt increase in prison capacity within a few years coincides with a sharp reversal in the trend of the stock of parolees (see Mechoulan 2011).
objectives for different races, which therefore leads to different expected violation rates for different races.

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

If the parole board is biased against African American prisoners, it would incur a larger cost associated with violation by an African American parolee than by a white parolee. That is, the parole board would set a more stringent standard of release for African American prisoners. The consequence of the model is that a parole board biased against group $r'$ would set a higher risk threshold, that is, $\pi(r, x^o_r) > \pi(r', x^o_{r'})$, for that group.

4.2. Strategic Timing of Release

Given a risk threshold, the parole board must decide at which time $t$ to release a prisoner. The parole board thus chooses the optimal time $t$ possible under the legal constraints of a minimum time served and a mandatory-release rule, $t \leq t \leq \tilde{t}$.

The dynamic nature of the parole process implies that the distribution of risk of violation at time of release is endogenous. By keeping a prisoner longer in prison, the parole board may get additional information about the prisoner, and 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, $\frac{\partial \pi(r, x, t)}{\partial t} < 0$.

Therefore, 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. Otherwise, the parole board would not grant parole, and the prisoner would be released at the end of his sentence. The consequence of the strategic timing of release is that the parole board releases prisoners when their probability of violation is exactly equal to the threshold $\lambda_r$.

This strategic-timing, or efficiency, hypothesis leads to sharp predictions in terms of what we should observe in the data. Except for those who are released as soon as they become eligible for parole because their risk is already below the threshold, within each group all prisoners re-

16. Postponing parole may frustrate and alienate prisoners. In turn, this may increase the chance of parole violation for some, which possibly offsets the mechanical effect, over a range of sentence lengths. This complication would lead to a discontinuity in the timing of parole violation but would not affect our fundamental result.
leased by the parole board should have the same expected rate of violation. Therefore, subject to the possible caveat of minimum time served for a given sentence length, we make the following first prediction:

Hypothesis 1. When a parole board chooses the time of release strategically, violation rates will not depend on the timing of release.\(^{17}\)

The intuition is the same as that in Kuziemko (2007, p. 4): “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.” 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 of various parole spells. 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.

This reasoning described so far applies within a group (race) for which the parole board uses a common violation threshold. Strategic timing means that, within a group, prisoners should have the same probability of violation at the time they are released. 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. 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. Going back to the observed violation rates, we see that if the parole board is unbiased \((\lambda_W = \lambda_A)\) and thus uses the same threshold for prisoners of different groups, the observed success rates should be the same. Differences in observed success rates across groups could be explained only by differences in the objective of the boards (different values of \(\lambda_r\)) and may be evidence of concerns other than minimizing parole violations, including possible racial bias.

4.3. Parole Boards’ Commitment to Efficiency

The model leads to a simple outcome test in the spirit of Knowles, Persico, and Todd (2001): success of parole should be equal across groups under the null hypothesis of no bias in the parole board’s decision-making

\(^{17}\) We assume for simplicity that a prisoner is potentially up for parole at any time. In practice, hearings are set at various intervals, so the exact time at which the prisoner reaches the risk threshold that should trigger his release may not coincide with the hearing date.
process. To test this hypothesis, we need to infer from the data the $\lambda_i$ values for different groups. We want to compare the values of $\lambda_i$ for white parolees and for African American parolees. For those two groups we assume that the parole board treats all members of the group in a similar way; that is, the value of $\lambda_i$ is constant for all members in each group. An unbiased parole board would release prisoners in such a way that marginal success is also equalized across groups. Thus, we would like to compare these marginal rates using the data at our disposal. We observe only average rates of violation and not the marginal violation risk used by the parole board. By construction, the average takes into account not only marginal but also inframarginal individuals. Composition of the groups of interest can differ in terms of such inframarginal individuals. This can lead to differences in averages even when the marginal individuals (on whom we should focus) are identical.

In the context of parole release, the fact that a parole board can choose the time of release alleviates the inframarginality issue. As mentioned above, the key insight is that the parole board decides not only whether to release a prisoner but also when to release him. A prisoner is released when the probability that he will violate parole is exactly equal to the threshold value of $\lambda_i$. All released prisoners become 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 following proposition summarizes the outcome test:

Proposition 1. If a parole board uses all information about prisoners in its release decision, the average rate of violation for a group represents the risk threshold used by the parole board for that group.

Thus, our outcome test simply consists of comparing violation rates between white prisoners and African American prisoners. The logic is the same as in the hit-rate method developed by Knowles, Persico, and Todd (2001) that compares the average success of police searches between groups.

Hypothesis 2. When parole boards use all information available (including race) to predict the risk of parole violation, observed violation rates will not depend on race under the null hypothesis of efficient minimization of parole violation.

One caveat arises because of the constraints imposed on parole boards in terms of the minimum time a prisoner must serve before being eligible for parole. If an inmate’s risk of violation is lower than the threshold at
the completion of his minimum sentence, he will be released immediately. These prisoners for whom the minimum-sentence constraint is binding have a risk of violation that is lower than average. If there are more white inmates than African American inmates in that situation, the inframarginality issue arises. To deal with this additional problem, our test must be restricted to inmates who were not released just after completion of their minimum sentences.

4.4. Parole Boards’ Commitment to Procedural Fairness

In general, the optimal timing of release depends on the distribution of risk in a given population. If the parole board uses race in its decisions (statistical discrimination), the timing of release will differ across races for individuals with otherwise similar characteristics. At the same time, the parole board’s decision-making process must be balanced by fairness considerations. In particular, the parole board may care about the amount of time each prisoner spends in prison for a given set of objective characteristics, especially sentence length. If parole boards are not using race in their parole decisions (there is no statistical discrimination, or release is color-blind), so the concern for fairness trumps efficiency, we would see similar prisoners spending the same amount of time in prison regardless of race (and likelihood of recidivism, if recidivism is linked to race).

Hypothesis 3. Under the fairness hypothesis, the timing of release will not depend on race. Consequently, if the propensity to violate parole depends on race in a systematic way, observed violation rates will differ across races.

5. DESCRIPTION OF THE DATA

Our data are from the publicly available information from the NCRP (US Department of Justice 2005). In 1983, the National Prisoner 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 prisoners entering and leaving the custody or supervision of state and federal authorities. In addition to state prisons, the federal prison system and the California Youth Authority also began reporting in 1984. The advantages of the NCRP data are mul-
The data are for 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 release-from-parole 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 offenses (a reasonable proxy for crime severity)—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, most important, the type of release from parole supervision: whether a discharge from parole (that is, a success) or a revocation (for violation of parole conditions or because of a new offense). Note that, by definition, the type of violations we examine represent offenses that are sufficiently serious to result in a return to prison or jail and hence correspond to the most pertinent measure of recidivism within the parole board’s control. The data distinguish between parole board releases and mandatory-parole releases (as well as other, rarer types of parole release). By construction, we do not observe prisoners who have maxed out, that is, those for whom parole is unnecessary because they have completed their sentences in full. To determine if this censoring is significant, we look at the distribution of sentence completion as a percentage of maximum sentence. Across all samples considered, it is always the case that at least 90 percent of prisoners paroled by a parole board have completed less than 80 percent of their maximum sentences. This strongly suggests that the percentage of prisoners who complete 100 percent of their sentences and hence disappear from the parole statistics is in fact negligible.

By the Bureau of Justice Statistics’ own admission, the NCRP data set is not exempt from problems: we found a large proportion of missing observations for 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, and parole releases. Of the 50 states and the District of Columbia, the federal prison system, and the California Youth Authority, only 13 have the complete parole-release series across all years. However, several states have only 1 year or a few years missing. After this first screening, our sample com-
prises 31 states (plus the federal prison system and the California Youth Authority).

Next, we selected states 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 (CPUS) data series for 1985–2002 (Bureau of Justice Statistics 1985–2002) and Probation and Parole Bulletins (PPB) for 1983–84 (Bureau of Justice Statistics 1983–84). Data availability refers to states participating in the NCRP, with all necessary variables present and meaningfully coded, that is, with few 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 1 year to the next and within different parole regimes in states with more than one concurrent regime. We also checked the consistency in the proportion of parolees entering each parole regime over time. Large jumps in the absence of institutional changes point to data-reporting errors: even if the violation rates are consistent, they are still meaningless if they are consistently wrong. That is why we also screened out states that had obviously misclassified some data, for example, states with implausibly low violation rates and those where parolees who entered prison under a determinate-sentencing regime were reported as parole board releases (a mistake that some states make systematically). To provide external validation, we then compared proportions for violation and type of parole entry with the figures provided by the CPUS and PPB. We retained states for which the NCRP data are substantially similar to those of the CPUS and PPB. Erring on the side of caution, we found that

18. The tables for Correctional Populations in the United States from 1993 to 1998 are accessible online. We thank Tom Bonczar from the Bureau of Justice Statistics for providing us with scanned copies of the rest of the series. Those data were collected alongside and separately from the NCRP data, hence their genuine comparative value.

19. 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 we decided not to impute a mandatory release when the coding indicates a parole board release. 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.

20. We also called state parole boards to check whether the data seemed correct on the basis of 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.
this process left us with Michigan, North Dakota, Utah, Colorado, Wisconsin, Texas, Missouri, Arkansas, New York, and California.

6. EMPIRICAL RESULTS

6.1. Inframarginality and the Strategic Timing of Release

Our approach tests the proposition that parole boards use the timing of release strategically. It could be the case that parole boards do not act as the theory predicts either because they have other objectives or because in practice they are unable to equalize risks of violation using a strategic-timing-of-release approach. The testable implication of the theory is that on average, in 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.

To implement the test, our dependent variable is the parole outcome (violation or no violation), and the main independent variable is the amount of time served at time of prison release. Note that the incarceration history of some parolees creates challenges for capturing the relevant amount of time served in prison. Some current parolees have already violated one or more earlier parole spells. Hence, it may be problematic to lump together former inmates who were paroled after a prison entry corresponding to a court commitment and former inmates who violated parole and have reentered prison. We present specifications for court commitments only as well as without restriction on prison entry type.

Our theoretical model predicts that violation rates are equalized across time served subject to the caveat that prisoners paroled at the minimum eligible time may represent a group of low-risk prisoners and hence are expected to violate less than everyone else. In other words, it is plausible that those prisoners would have been paroled earlier save for the constraints imposed on their sentences. For the small fraction of observations for which such information is available, the proportion of parolees who allegedly complete less than 100 percent of their minimum sentences can represent up to a quarter of prisoners in the NCRP. We therefore consider minimum sentence an unreliable measure and choose not to use it. Instead, we focus on time served as a percentage of the maximum sentence and look at whether our equalization prediction also applies to those who complete the smallest percentages. 21

21. The NCRP measures total prison time served, including any time served under remand.
To avoid censoring, we restrict parole release to occur no later than 1997, so by 2003 (the last year of data) over 95 percent of parolees have exited parole, one way or another. We select only male parolees released from prison (as opposed to jail, halfway houses, and other less common types of facilities). When considering parole board operations, we restrict the analysis to those that do not operate under a Truth in Sentencing Act regime (which mandates minimum percentages of sentence completion) because by nature that regime type restricts the ability of parole boards to time releases strategically. We similarly exclude other institutional restrictions on discretion (see Sabol et al. 2002).

Beyond pure indeterminate-sentencing states, we consider 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 is 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 for states that switched to a determinate-sentencing regime within the NCRP time frame—or at least not too long before the collection started—makes impractical a within-state comparative study of the effect of time served on parole violation between regimes.22 There may also be inherent difficulties stemming from the way judges may modify 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 prerelease centers, work centers, and other facilities as opposed to the almost exclusive use of state prison under indeterminate sentencing. The number of probation releases also grew to a quarter of the total of prison releases from almost 0 before determinate sentencing. If judges change their sentencing to mitigate what they perceive to be perverse effects of determinate sentencing, then the same prisoner may experience a different maximum initial sentence on the basis of the paroling regime. This would cripple the analysis.

We also looked at time served in indeterminate states that have both parole boards and mandatory parole as mechanisms of parole release (Missouri, New York, Texas, and Wisconsin before 1999). However, prisoners may not be randomly assigned to one regime or another, which creates a potential endogeneity issue. Parole-regime-dependent sentenc-

22. California is the only state for which we observe nearly 100 percent mandatory parole for prisoners admitted to prison following the regime change. Because California switched to determinate sentencing in 1976, the only parolees whom we observe having been paroled through a parole board are a small number of inmates originally sentenced to a maximum of life in prison, which is hence a nonrepresentative sample of offenders.
RACIAL DISPARITIES IN PAROLE RELEASE

ing is also a possible threat. Therefore, while we did not find prisoner or offense characteristics that specifically attach to one route or the other, we do not provide a presentation of the impact of time served on violation rates in these mixed-regime states, although, subject to the caveats outlined above, the comparison lends support for the strategic-timing hypothesis.\(^23\) That is, we found that in those mixed-regime states, the amount of time served in prison does not influence the likelihood of violation when the parolee has gone through a parole board, whereas it does when the parolee was paroled through a mandatory-parole regime. We also found that in California (a pure mandatory-regime state), the effect of time served on parole violation is small yet statistically significant. Table 1 shows our summary statistics.

We estimate the probability of violation using the following linear probability regression model:\(^24\)

\[
\text{VIOL}_i = \beta_0 + \beta_1 \text{TimeServed}_i + \beta_2 \text{TimeServed}_i \times \text{Black}_i \\
+ \beta_3 \text{Black}_i + \sum \beta_j X_j + \varepsilon,
\]

where \(X\) is a vector of individual control variables: age at prison release, maximum possible sentence length (to account for the possibility of various risk thresholds corresponding to offenders’ perceived dangerous-

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23. Details of this analysis are available on request.
24. Linear probability models are preferred in the presence of interaction terms; see Ai and Norton (2003).
ness),\textsuperscript{25} state dummies, year-of-release dummies, and state-specific time trends.\textsuperscript{26}

Table 2 presents the results of the regression analysis. To establish whether white or African American prisoners’ likelihood of parole violation was independent of time served (measured in years), we consider the $p$-value of the tests $\beta_1 = 0$ and $\beta_1 + \beta_2 = 0$, and the joint test of $\beta_1 + \beta_2 = 0$ and $\beta_1 = 0$. In column 1 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 result is consistent with parole boards using the time of release strategically. In column 2 we perform the same analysis for parolees who have completed less than a quarter of their maximum sentences—incidentally, the percentage of African Americans paroled that early is nearly the same as in the rest of the population. This group includes a number of prisoners who have completed more prison time than necessary, given their risk level, yet the parole board could not release them earlier because of minimum-sentence constraints. In that specification, time served in prison affects the violation rate for white prisoners and for both groups combined. As expected, we also find that parolees in that subsample violate less than the rest, even though a large racial differential remains. Conversely, in specification (3), when performing the analysis for parolees who have completed more than a quarter of their maximum sentences, time spent in prison does not affect the violation rate in either group. Specification (4) adds the interaction term Maximum Possible Sentence Length $\times$ African American to account for the possible differential effect of risk thresholds in each group. The results do not change qualitatively. Results are also robust regardless of prison entry type and excluding time served in prison prior to the date of admission and credited to prison service for the current sentence (results are available on request). We conclude that the data support the idea that parole boards strive to equalize the probability of violation within each group and that inframarginality concerns should not cloud the results of the outcome tests.

\textbf{6.2. The Efficiency Hypothesis}

Our outcome test compares the probability of parole violation across races. The theoretical model leads to the strong implication that viola-

\textsuperscript{25} Note that since we control for time spent in prison and sentence length, we implicitly control for maximum parole length as well.

\textsuperscript{26} For some states, education is also provided. We do not use it in the baseline regression so as to include as many states as possible. The analysis produces similar results when we restrict attention to states with data for education.
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<tr>
<td>Time Served × African American</td>
<td>-.002 (.001)</td>
<td>-.005 (.003)</td>
<td>-6 × 10⁻⁴</td>
<td>-.002 (.002)</td>
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<td>.01** (.007)</td>
<td>-.002 (.005)</td>
<td>.005 (.005)</td>
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<td>.124** (.006)</td>
<td>.133** (.008)</td>
<td>.115** (.01)</td>
<td>.125** (.009)</td>
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<tr>
<td>Adjusted R²</td>
<td>.068</td>
<td>.074</td>
<td>.067</td>
<td>.067</td>
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<tr>
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<td>30,594</td>
<td>11,832</td>
<td>18,762</td>
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**Note.** The dependent variable is whether the prisoner violated parole. The states included have accurate reporting and a discretionary parole board as the sole mechanism of release. The year of prison release is earlier than 1998 to limit censoring. All models control for age at prison release, sentence length, year of prison release fixed effects, state fixed effects, and state time trends. Robust standard errors clustered by state are in parentheses. The base specification, specification (1), includes only male parolees admitted to prison under court commitment (for example, for the first time on at least one new sentence and not readmitted on any previous sentences still in effect). Specification (2) also includes inmates who have served less than 25 percent of their time. Specification 3 adds to the base specification inmates who have served 25 percent or more of their time. Specification 4 adds to the base specification Sentence Length × African American as a control.

**p < .01.**
tion rates should be equal if parole boards are racially neutral in their
decisions. The outcome-test results correspond to the coefficient $\beta_3$. Being
African American is associated with an average increase of 12 percentage
points in the probability of parole violation (see Table 2). African Amer-
ican parolees have an average violation rate of 44 percent, while white
parolees have an average violation rate of 32 percent in indeterminate-
sentencing states.\footnote{This difference is calculated for prisoners who enter prison following a court com-
mittal. Although violation rates for prisoners who enter prison following an initial parole
violation are significantly higher, the African American–white differential (about 11 percent-
age points) is similar.} The same result follows if we use a nonparametric
Pearson $\chi^2$-test, which compares the observed violation count in each
group against the count that is expected under the null hypothesis of
equal thresholds, $\lambda_W = \lambda_{AA}$.

We also conducted a number of further checks (available on request)
to confirm the robustness of those results. For states with a mixed regime,
the results are similar when parole is granted through a parole board,
which provides an external validation of the outcome-test results found
in the indeterminate-sentencing states. Despite stark differences in aver-
age rates of violation across states (of no direct concern since we focus on
the within-state African American–white violation differential), in every
state this differential is significant both statistically and substantively and
remarkably consistent in magnitude. The indeterminate-sentencing vi-
olation rate differential is smaller than 10 percentage points only in Utah.

It could be that parole boards are not equally concerned by the vi-
olations of prisoners convicted for different types of crime that African
Americans and whites do not commit in the same proportions. Yet our
results are robust to a breakdown of groups according to sentence length
and category of crime, subject to a couple of caveats.\footnote{Within crime categories, we limit our analysis to the first count of the charge (when there is more than one).} With regard to
categories of crime, in mixed regimes parolees convicted for robbery and
released by a parole board violate their parole more frequently than other
categories of prisoners, who otherwise violate in similar proportions.
Second, violation rates are similar within racial groups and across sen-
tence lengths for the group of indeterminate-sentencing states but not for
parolees who have gone before a parole board in mixed-regime states.

We were also able to reject the hypothesis that parole boards may
be less concerned with African American offenders because they mostly
commit crimes in the African American community (one rationale be-
ing that these receive less publicity, so the parole board may not care as much):\(^29\) for crimes against the state, that is, those that do not involve individual victims, the results are qualitatively consistent although smaller in magnitude. Similarly, we can reject the hypothesis that because African American offenders are on average (slightly) younger, parole boards display leniency on account of age.

Overall, our results seem to suggest that the parole system systematically underestimates African American parolees’ violation risk. These empirical findings are somewhat surprising and quite at odds with the conventional wisdom that African Americans are discriminated against in the US criminal justice system. In the notation of the model, the empirical results means that \(\lambda_{AA} > \lambda_W\). Given the definition \(\lambda = C_1/(C_1 + C_2)\), a difference in thresholds may be explained by a difference in type I or type II costs, which leads to two possible interpretations.

The first explanation for the evidence could be that the observed differential in violation rates is not the result of parole boards’ 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 after prison release would be to compare the reasons for parole violation across groups. Intuitively, parole officers’ bias against African Americans, if any, 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 crime is committed. The NCRP data contain only crude information about the cause of parole violation (namely, whether there is a new sentence or charge pending) and whether parole is revoked, which indicates a violation of the conditions of parole; obviously we would like more details on the characteristics of each. A problem is that prisoners who commit a technical violation and return to prison do not have the opportunity to commit a new crime; hence, there is some endogenous censoring. Yet if there were discrimination against African American parolees, marginal parolees would arguably be removed from society quickly because of technical parole violations and would return to prison without committing new crimes. By the same logic, if there is a positive correlation between the likelihood of committing a technical violation and committing a new crime, the remaining pool of parolees would be less likely, on average, to commit new

\(^{29}\) This argument can also be framed as parole boards being more concerned with crimes in white communities and thus applying a stricter policy for white prisoners.
crimes. Yet we confirmed that African American parolees are still relatively more likely to return to prison on account of new charges when released by a parole board. This finding also addresses the criticism that perhaps the objective of the parole board is not avoiding violations but deterring new crimes. Hence, even if this were the case, the same bias, though smaller in magnitude, would 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. So when African American and white parolees released under either discretionary or mandatory parole coexist in one jurisdiction, we find that both groups violate parole significantly more often in absolute terms when they have been released through discretionary parole. Further, the violation differential is smaller when parole is granted through a mandatory parole mechanism, by about 7 percentage points. One explanation is that determinate sentencing leads to much shorter time on parole. Whites and African Americans are released in a similar pattern, but with a shorter parole duration they have fewer opportunities to violate parole. Under indeterminate sentencing, parole lengths are longer, and the African American–white differential in violation is thus significantly larger. California, a pure determinate-sentencing state, provides an extreme illustration of this finding, with a differential of just 2 percentage points. To provide more detail on that line of reasoning, we examine Wisconsin’s institutional changes, which provide a more robust testing ground (that does not suffer from any selection effects that may be present in mixed-regime states). In 1999, Wisconsin switched to a system of determinate sentencing. Notwithstanding censoring, we 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 when those two groups coexisted. It is most unlikely that parole officers or the police would discrim-

30. Still, if certain offenses for which African Americans are overrepresented result in a higher likelihood of postrelease discrimination, one may be misled to conclude that parole boards are biased in favor of African American prisoners. As already mentioned, applying the test for the most common types of crime, however, shows that this critique fails.

31. For prisoners admitted after 1998, parole board releases do not disappear entirely after 1999 yet decline every year and are reduced to 0 in the small sample of the last year of data.

32. Censoring could be a problem if, conditional on violating parole, whites tend to violate later in their parole spell than African Americans. Nothing in the data supports that objection.
inate on the basis of sentencing date. For violation rates occurring after 1998, among male prisoners admitted before 1999 and granted parole by a discretionary parole board after 1998, African American parolees are 15 percentage points more likely than whites to violate parole. In contrast, for parole releases among male prisoners admitted to prison after 1998 and granted parole through mandatory parole, the difference in violation is 12 percentage points—again, with the caveat of censoring.

6.3. The Fairness Hypothesis

An alternative explanation is that parole boards refuse to consider race to be a valid characteristic to use in their decisions. Indeed, if parole boards strive for an equalization of violation rates, this would be consistent with a racially neutral objective but also with potential racial profiling. We find some support for that hypothesis in Table 3, which measures the extent to which African American parolees spend more time in prison by the time they are paroled. For a given maximum sentence length, parole boards grant parole only marginally (although statistically significantly) later on average for African Americans relative to whites (1 extra month of time served for an average of 39 days in specification [1]). The difference in time served is no greater than that found in mandatory regimes in mixed jurisdictions. However, the key difference is that all prisoners spend substantially more time in prison (for example, 9 months in specification [2]) under a mandatory regime. Hence, it appears that parole boards do not react to higher average violation rates in the African American parolee population by keeping these prisoners in prison for a substantially longer amount of time. Yet by releasing inmates earlier in a quasi-uniform way, parole boards appear to create 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.

We do not take a position on 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 it. However, the stakes are worth spelling out: by refraining from keeping African American prisoners longer in prison, parole boards con-

33. The apparent higher violation rate among whites under mandatory parole after 1999 can be explained by the fact that there are fewer successful parole exits closer to the last year of prison exits; that is, most parolees are still under supervision and have not completed their parole and hence are not included in the data.
## Table 3. Testing the Strategic Timing of Release: Ordinary Least Squares Regressions

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<td>1.00**</td>
<td>.74**</td>
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<tr>
<td><strong>Parole Board</strong></td>
<td>−8.9**</td>
<td>−7.53**</td>
<td>−9.09**</td>
<td>−.06</td>
<td>(1.34)</td>
</tr>
<tr>
<td></td>
<td>(1.81)</td>
<td>(1.95)</td>
<td>(1.54)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>African American × Parole Board</strong></td>
<td>−.34</td>
<td>.20</td>
<td>.06</td>
<td>.06</td>
<td>.46</td>
</tr>
<tr>
<td></td>
<td>(1.45)</td>
<td>(.62)</td>
<td>(.56)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>.39</td>
<td>.43</td>
<td>.40</td>
<td>.43</td>
<td>.46</td>
</tr>
<tr>
<td>$N$</td>
<td>30,594</td>
<td>57,520</td>
<td>102,765</td>
<td>74,983</td>
<td>103,718</td>
</tr>
</tbody>
</table>


**Note.** The dependent variable is Time Served. The states included have accurate reporting and a discretionary parole board as the sole mechanism of release. The year of prison release is earlier than 1998 to limit censored. All models control for age at prison release, total maximum sentence length (all offenses), year of prison release, state fixed effects, and state time trends. Robust standard errors are clustered by state. Only male parolees admitted to prison under court commitment (and not under a prior parole revocation) are included, except in specification (4). Time served includes prior time served, except in specification (3). Specification (1) includes states in which the discretionary parole board is the sole mechanism of release (Michigan, North Dakota, Utah, and Colorado). Specifications (2)–(4) include states with discretionary parole only (Michigan, North Dakota, Utah, and Colorado) and states with both discretionary and mandatory parole (Wisconsin, Texas, Missouri, and Arkansas). Specification (5) includes only California (no parole board).

**$p < .01.$**
travene their stated objective of minimizing parole violations. In other words, the behavior of parole boards leads to a loss of efficiency. However, to the extent that it is difficult to accurately predict parole violation for individual prisoners, adopting a decision rule that makes African Americans spend more time in prison might seem unfair from the perspective of an individual African American prisoner, as he may become a postrelease model citizen (although many of his fellow African American prisoners will not). Thus, the choice between the two rules reveals a trade-off between efficiency and equity that parole boards have chosen to address by favoring the latter.

7. CONCLUSION

We develop a test to uncover possible discrimination in discretionary parole-release decisions. We argue that a simple, inframarginality-critique-proof outcome test that compares the average success of parole of different groups is sufficient for that purpose. A lower rate of success in a given group suggests that the parole board is too lenient in its decisions concerning that group. We show that in all US states for which we have reliable data, the outcome test leads to the conclusion that white parolees have a higher success rate, which suggests, at a minimum, no discrimination against African American parolees. The alternative interpretation that parole boards might be biased against deserving African American parole candidates exclusively, which thus results in both discrimination and higher violation rates by African Americans, is not supported by the evidence.34 We also find that in determinate-sentencing regimes, the difference in recidivism rates between African Americans and whites is smaller than under discretionary parole regimes both in absolute and relative terms. This suggests that determinate sentencing, despite not taking race into account, better succeeds (relative to parole boards) in attaining the efficiency objective. That determinate sentencing would do better than discretionary parole with respect to efficiency may seem counterintuitive, given that a determinate-sentencing system presumably releases prisoners in a color-blind way. We believe that this apparent puzzle can be explained by the fact that prisoners spend more time in prison in determinate regimes. Hence, both African Americans and whites have

34. This scenario could happen only if good risks among African American candidates were systematically denied parole and therefore did not appear in the data; otherwise, we would observe that African American prisoners paroled close to the end of their sentences would violate less, which is not the case.
shorter parole spells and, correspondingly, fewer chances of violating parole. Further, African Americans’ higher average propensity to violate parole leads mechanically to a higher violation probability differential in a discretionary regime given that parole duration increases.

Our conclusion puts into question the conventional wisdom that parole-release decisions in US states are biased against African Americans. We verified that it is unlikely that our results could be accounted for by discrimination by parole officers or the police. We suggest instead that parole boards’ apparent bias in favor of African American prisoners stems from them approximately equalizing prison time between African American and white inmates, 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. Also, it could be that the forecasting models used to probe risk of recidivism are not accurate for certain offenses committed relatively more often by minorities.\(^{35}\) 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 bias in discretion (see Dharmapala, Garoupa, and Shepherd 2006). Our results, in somewhat unexpected ways, provide a new justification for such changes.

Finally, it is also worth comparing the findings of this paper with recent studies of racial profiling in traffic stops, beginning with Knowles, Persico, and Todd (2001). For traffic stops, there is evidence of disparate impact, with African American drivers being searched at a higher rate than white drivers, but no evidence of disparate treatment: the success rate of searches is equal across groups. These results are interpreted as statistical discrimination in which police officers use race as a potentially valid characteristic to increase the efficiency of searches. In the case of parole release, our evidence points toward a near absence of disparate impact since the timing of release appears similar across groups for a given sentence length. Parole boards behave as if they do not take into consideration that African Americans have a higher propensity to violate parole.

\(^{35}\) “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 2012, p. 271).
This, in turn, leads to a significantly lower violation rate for white prisoners that in another context would be interpreted as disparate treatment according to the legal terminology. A possible reason for this difference is that parole boards follow guidelines and that these guidelines cannot lead to race-conscious policies that would be unconstitutional.

REFERENCES


