The Prison Boom and Sentencing Policy

Derek Neal and Armin Rick

ABSTRACT

The existing literature on the role of changes in sentencing policies as drivers of growth in prison populations contains findings that appear contradictory. We present a new method for characterizing changes in the severity of expected punishments for offenders and build a new simulation model based on this method. We provide clear evidence that changes in sentencing policy drove recent growth in prison populations in the United States, and our approach sheds light on the reasons that some previous studies did not reach this conclusion. The shift to more punitive sentencing policies had a disproportionate effect on black communities, even though, for the most part, this shift did not target blacks or crimes that blacks commit relatively more than whites.

1. INTRODUCTION

For most of the 20th century, policies that governed justice and corrections in the United States reflected a paradigm known as indeterminate sentencing. Judges enjoyed great discretion when deciding whether to sentence convicted offenders to probation or prison, and they enjoyed similar discretion when deciding sentences for those entering prison. Fur-

DEREK NEAL is Professor in the Department of Economics and the Committee on Education and the College at the University of Chicago and a research associate at the National Bureau of Economic Research. ARMIN RICK is Assistant Professor in the Johnson Graduate School of Management at Cornell University. We thank Joseph Altonji, Elizabeth Carson, Janet Currie, Steven Levitt, Jens Ludwig, Justin McCrary, Thomas Miles, Steven Raphael, Bruce Western, and Max Schanzenbach, and workshop participants at the Institute for Research on Poverty Summer Research Workshop, the National Bureau of Economic Research Summer Institute, Duke University, and the Becker Friedman Institute conference honoring Finis Welch for helpful comments. This work grew out of discussions during the October 2008 meeting of the Chicago Workshop on Black-White Inequality, which was funded by the Searle Freedom Trust. We thank the George J. Stigler Center for Study of the Economy and the State for research support. We thank Andrew Jordan, Ian Fillmore, Ezra Karger, Sarah Komisarow, and Joshua Shea for excellent research assistance.

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ther, holding constant the sentences that judges imposed, parole boards enjoyed considerable control over the time that inmates actually served.

The indeterminate-sentencing model offered judges and parole boards the freedom to consider prospects for rehabilitation, the provision of incentives for good behavior and self-improvement, and the expected effects on public safety when allocating punishments to offenders. However, during the 1970s, a diverse set of political groups mounted attacks on indeterminacy. Some charged that indeterminacy gave judges and parole officials too much freedom to indulge their own racial prejudices when determining sanctions. Others charged that indeterminacy allowed lenient judges and parole boards to undermine public safety by putting dangerous criminals back on the streets far sooner than legislators intended (for discussions of this literature, see Raphael and Stoll 2013; Stemen and Rengifo 2011; Dansky 2008). In response, state legislatures began passing laws in the late 1970s that constrained the discretion of judges and parole boards. Moreover, reviews of the literature suggest that state legislatures sought to make sentencing policies not only more determinate but also more punitive throughout the 1980s and 1990s (for more detailed treatments of the literature, see Raphael and Stoll 2013; Neal and Rick 2014). Here we define sentencing policies as the collection of laws and regulations that direct the actions of judges and parole boards concerning the punishment of convicted offenders. In practice, changes in sentencing policy may affect incarceration rates through a number of channels. Given conviction, sentencing policies influence whether offenders enter prison and how long they remain there. Further, the possible punishments associated with convictions for different crimes influence the bargaining power that prosecutors bring to negotiations over plea bargains. This is noteworthy since most convictions result from plea bargains. Our goal is to quantify the total contribution of changes in sentencing policies to recent growth in prison populations.

Figure 1 documents the dramatic rise in US incarceration rates. The 1980s and 1990s are the decades when incarceration rates grew most rapidly, but incarceration rates began rising in the early 1970s and did not peak until 2007. By then, the total incarceration rate was more than five times the 1970 rate, and although the rate fell slightly between 2007 and 2013, it remains roughly five times greater than the 1970 rate.

Figure 2 presents arrest rates for the same period. These series are Federal Bureau of Investigation (FBI) estimates of national arrest rates based on data from the Uniform Crime Reports (UCR). The UCR data come

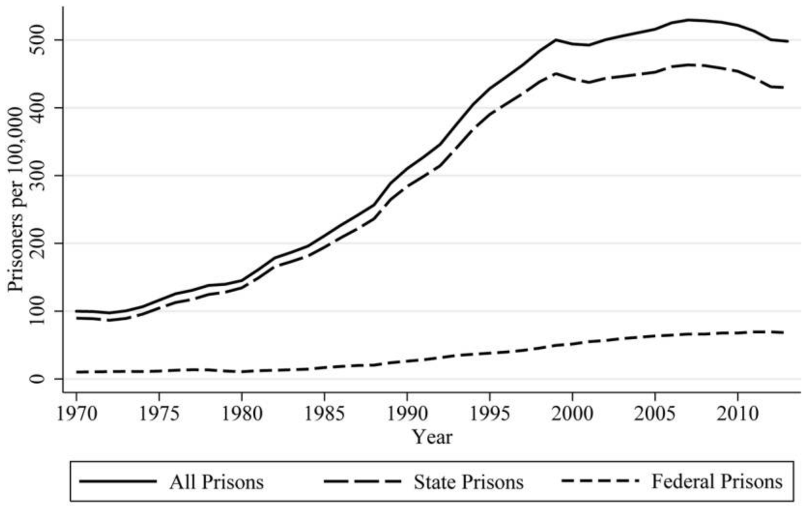

Figure 1. US incarceration rates, 1970–2013

from self-reports by police agencies and therefore suffer from errors and missing reports. The FBI attempts to correct common forms of reporting error when creating its estimates of annual arrests, but some of the remaining year-to-year variation likely reflects measurement error.

Nonetheless, several low-frequency patterns in these data are clear. Over all crime categories, arrest rates followed a mostly upward trend throughout the 1970s and 1980s, but since 1990, there have been noteworthy differences in the trends for different types of crime. Arrest rates for property crime peaked in the late 1980s and fell below 1970 levels during the 2000s. Arrest rates for violent crime rose until the mid-1990s before falling to levels that are below the 1980 rate but still above the 1970 rate. Drug arrest rates rose until 2005 before falling modestly, but the national arrest rate for drug crimes in 2010 remained roughly 2.5 times the corresponding rate for 1970.

Figures 1 and 2 motivate the existing literature that explores how changes in sentencing policies contributed to the prison boom. First, percentage increases in incarceration rates during the 1980s and 1990s are even more dramatic than contemporary increases in arrest rates. Second, incarceration rates during the 2000s remained at least five times greater than the 1970 incarceration rate, while arrest rates for most crimes fell sharply. Because persons incarcerated for drug crimes account for a small portion of prison populations at any point in time, the contrasts between

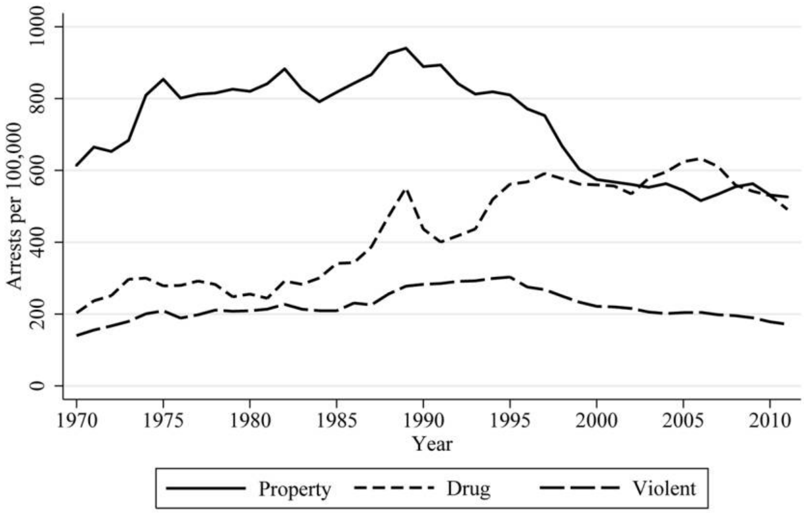

Figure 2. Arrest rates by crime category

the trends in Figures 1 and 2 suggest that changes in sentencing policies must have contributed to the growth in incarceration rates. Yet, as we explain below, the existing literature contains a collection of empirical results that appear to support a wide range of positions concerning the importance of changes in sentencing policies as drivers of growth in prison populations.

In this paper, we develop a new method for measuring the punitiveness of sentencing policies and present simulation models based on this method. These tools allow us to better isolate how changes in sentencing policies affect prison populations. Our results indicate that a broad shift toward more punitive sentences and parole practices was the key factor that drove the growth of prison populations in the United States. This shift did not affect just a few types of offenders but involved an increase in expected punishment for offenders in every major crime category.

We also explore how this shift affected black offenders relative to white offenders. Although we find no evidence that changes in state laws and regulations created greater increases in expected punishment for black offenders than for white offenders, we do find that the move to more punitive sentencing policies had a disproportionate effect on black communities. The reason is that arrest rates for blacks have been at least four times greater than arrest rates for whites for decades. Thus, the shift to more punitive treatment for offenders had a much larger effect on the levels of incarceration rates among blacks than among whites.

We begin by reviewing the literature on the role of changes in sentencing policies as drivers of growth in prison populations. We discuss the limitations of several commonly employed empirical methods and the apparent contradictions among existing findings. We then present a new method for characterizing changes in the severity of expected punishments, and we build a new simulation model based on this method. We argue that our methods and results resolve apparent conflicts in the literature and provide clear evidence that changes in sentencing policy drove the growth in prison populations in the United States. We are not the first to reach this conclusion, but our approach provides the best available evidence that this conclusion is correct. Finally, we show that the shift to more punitive sentencing policies had a disproportionate effect on black communities, even though, for the most part, this shift did not target blacks or crimes that blacks commit relatively more than whites.

2. LITERATURE REVIEW

Table 1 summarizes some of the changes that states made as they moved to more determinate sentencing practices.[1] Before 1980, California, Colorado, Illinois, Indiana, Maine, and New Mexico eliminated or severely curtailed discretionary releases by parole boards, and since that time, 16 other states have either eliminated or curtailed the discretionary powers of parole boards. Further, a number of these states eliminated discretionary parole as one component of a large set of reforms that also involved establishing independent sentencing commissions. These commissions developed sentencing guidelines that constrain the sentencing decisions of judges. Minnesota, in 1980, was the first state to establish an independent sentencing commission. Since then, 24 other states have adopted commissions that vary greatly in terms of their missions and powers.

In 1994, the federal government passed the Violent Crime Control

1. Many factors contribute to determinacy. Sentencing guidelines, narrow presumptive sentencing ranges, laws requiring mandatory minimum sentences, and other policies restrict the discretion that judges may exercise at sentencing, while restrictions on discretionary parole release limit the ability of parole boards to affect actual time served. We do not attempt to code some states as determinate and others as indeterminate at any point in time. Instead, we address a number of factors that influence both determinacy and severity in many states.

Table 1. The Move to Determinate Sentencing

State	Discretionary Parole	Sentencing Commission	Truth-in-Sentencing Law: Required %
Alabama		1998 [2000][a]	
Alaska	1980[b] (partial)	1980[c]	Other
Arizona	1994[d]		85
Arkansas	1994[b] (partial)	1994[e]	Other
California	1976[d]		85
Colorado	1979–85[d]		Other
Connecticut	1981–90[d]	2010[a]	85
Delaware	1990[b,d]	1987[c]	85
Florida	1983[b,d]	1983–98[c]	85
Georgia			85
Idaho			100
Illinois	1978[d]	2010[a]	85
Indiana	1977[d]		50
Iowa			85
Kansas	1993[b,d]	1993[c]	85
Kentucky			85
Louisiana		2010[a]	85
Maine	1976[d]		85
Maryland		1983[c] [1996]	50
Massachusetts		1994[a]	75
Michigan		1984[c] [1995–2002]	85
Minnesota	1980[b,d]	1980[c]	85
Mississippi	1995[d]		85
Missouri		1997[c]	85
Nebraska			50
Nevada			100
New Hampshire			100
New Jersey			85
New Mexico	1977[d]	1978[a]	
New York		2010[a]	85
North Carolina	1994[b]	1994[c]	85
North Dakota			85
Ohio	1996[b,d]	1996[c]	85
Oklahoma			85
Oregon	1989[b,d]	1989[c]	85
Pennsylvania		1982[c]	85
South Carolina			85
Tennessee	1989[b] (partial)	1989–95[c]	85
Texas			50
Utah		1979[c] [1983]	85
Virginia	1995[b] (partial)[d]	1991[c] [1995]	85

Table 1. *continued*

State	Discretionary Parole	Sentencing Commission	Truth-in-Sentencing Law: Required %
Washington	1984[b, d]	1984[c]	85
Wisconsin	1999[d]	1985–95,[c] 2002–7[a]	Other

Note. Years in square brackets indicate when a temporary sentencing commission was made permanent. Results for truth-in-sentencing laws are the percentages of their minimum sentence that prisoners are required to serve (Ditton 1999, table 1). "Other" means that the statute involved nonstandard provisions; for example, some excluded certain offenses. Some specified requirements in terms of combinations of time in prison and on parole.

[a] Data are from state sentencing commission and legislative websites. For more information, see Section D of the online appendix.

[b] Year in which parole release was abolished (Frase 2005, table 1).

[c] Commission established (Frase 2005, table 1); ranges indicate if it was later abolished.

[d] Determinate sentencing enacted (Stemen, Rengifo, and Wilson (2006, tables 1–3); ranges indicate that indeterminate sentencing was later reinstated. Mississippi reinstated indeterminate sentencing for first-time nonviolent offenses in 2000.

and Law Enforcement Act (Pub. L. No. 103-322, 108 Stat. 1796). This law established the Truth-in-Sentencing (TIS) Incentive Grants Program, which provided grants for prison construction and expansion to states that adopted policies requiring sentenced offenders to serve large portions of their sentences. Table 1 shows that the majority of states now have a TIS law that limits discretionary release by parole boards (on the implementation of TIS laws during the 1990s, see Ditton and Wilson 1999).

Moreover, many states that never established sentencing commissions have created more determinate systems by simply legislating presumptive sentences. Stemen, Rengifo, and Wilson (2006, p. 118) report that "between 1975 and 2002, every state adopted some form of mandatory sentencing," but the number of crimes covered by such statutes and the harshness of the minimum sentences vary greatly among states and over time in states. During the past 2 decades, a majority of states have also added laws that impose enhanced sentences for habitual offenders, but once again, the details of these habitual-violator laws differ greatly among states.

States have pursued determinacy using a variety of approaches over the past 3 decades or more. In most states, the push for determinacy also involved at least some efforts to make sentencing policies more punitive as well. Next we review the literature that attempts to establish links between sentencing reforms and subsequent growth in state prison populations.

2.1. Panel Regressions

One segment of the empirical literature on the growth in prison populations tries to determine whether particular changes in sentencing policy were key drivers of population growth in the 1980s and 1990s. These papers attempt to isolate the effects of specific types of legal reforms on the growth of prison populations by applying panel regression methods to data sets that track variation in outcomes and policies among states and over time.

Zhang, Maxwell, and Vaughn (2009) is a prototypical contribution to this literature. The authors attempt to explain variation in measures of admission rates, incarceration rates, and expected time served among states and over time by regressing these outcomes on six measures of policy and a set of additional control variables that often include state and year fixed effects. They employ data for 1973–98 and a set of indicators for the presence of the following policies: voluntary sentencing guidelines, presumptive sentencing guidelines, habitual-offender laws, abolition of discretionary release by parole boards, requirements that sentencing guidelines consider prison capacity, and TIS laws. On the whole, their results imply few statistically significant effects for these policy variables and even fewer that are of the expected sign. They conclude that sentencing policies associated with determinacy did not contribute much to prison growth over the period they examine. Stemen, Rengifo, and Wilson (2006) and Stemen and Rengifo (2011) follow a similar research strategy but focus only on incarceration rates as outcomes. Taken as a whole, their results are similar to those in Zhang, Maxwell, and Vaughn (2009). Indicator variables for adoption of these policies are not strong predictors of future growth in prison populations in states.[2]

These panel regression methods are limited in at least two important ways. To begin, they do not directly address a precise counterfactual. In the language of the literature on program evaluation, the regressions are an attempt to identify treatment effects associated with particular poli-

2. It is worth noting that all three of these studies find that states that abolished discretionary parole release experienced slower than average growth in prison populations. Frase (2005) and Nicholson-Crotty (2004) conclude that mandatory-sentencing guidelines reduce prison populations in states where the guidelines are used as a tool to manage expenditures on corrections. Marvell (1995) reaches a similar conclusion using earlier data.

cies, but the indicator variables for treatment do not capture the implementation of homogeneous policies, and therefore the treatments are not precisely defined.

For example, Zhang, Maxwell, and Vaughn (2009) note in their conclusion that the details of habitual-offender laws, which are often known as three-strikes laws, vary among states. While California, Georgia, and Florida handed down enhanced sentences to many offenders under these laws, a significant number of states define their strike zones so narrowly that the statutes are rarely used.[3] Researchers cannot correctly interpret estimated effects of habitual-offender laws without learning about how each habitual-offender law was written and implemented.

Further, because policies with similar effects but different names were implemented in nontreated states, the implied control groups in the regressions discussed above are not valid control groups. Because all states have adopted new mandatory minimum statutes since 1975, and it seems reasonable to conjecture that many also tightened standards for parole revocation, applied more public scrutiny to parole board decisions, and so forth, in ways that may not be reflected in coding schemes that seek to capture the adoption of a specific collection of statutes. The results from these panel regression studies tell us little about whether changes in the punitiveness of sentencing policies are responsible for the dramatic growth in prison populations over the past 3 decades or more. They simply tell us that states associated with a particular set of readily identifiable policies do not stand out as having above-average rates of growth in prison populations.

2.2. Decomposition Methods

Given the limitations of panel regression methods, some scholars adopt an indirect approach that treats prison growth as the product of changes in a number of factors. Here we derive a simple version of the steady-state equation that justifies this method.

Consider a fixed population of persons who live forever, and assume that at any point in time, t, there exists a fixed population of persistent criminals, C, who are always engaged in crime if they are not incarcerated. Let I_t denote the population of incarcerated persons in period t. Further, define the following probabilities: α is the probability of arrest given

3. Auerhahn (2002) uses a simulation model to demonstrate the large effect that these policies had on prison growth in California in the late 1990s and to predict the continued growth in the population during much of the 2000s.

engagement in crime, γ is the probability of conviction given arrest, and δ is the probability of admission to prison given conviction.

For now, let us ignore parole and parole revocations. In this thought experiment, new entrants to prison are always newly convicted offenders, and all prisoners serve their entire sentence.[4] Let $s = 0, 1, 2, \ldots, S$ denote the potential sentence lengths. Then, we can use the following equation to describe the prison population at time t:

$$I_t = \sum_{i=1}^{S}(C - I_{t-i}) \times \alpha \times \gamma \times \delta \times \Pr(s \geq i).$$

This equation indicates that, at any point in time, the criminals who are not already incarcerated are the ones at risk of being arrested, convicted, and sentenced to various possible prison terms. Further, the current prison population reflects the accumulation of past flows in and out of prison.

To make things simple, let us consider steady-state prison populations. If the prison population is in steady state in period t, then $I_t = I_{t+k}$ $\forall\ k > 0$. The steady-state incarceration rate is the fraction of the population that is in prison in steady state. Since s is nonnegative, we know that $\sum_{i=1}^{S} \Pr(s \geq i) = \mathbb{E}[s] \equiv \bar{s}$. Given this result, we can divide both sides of our equation by the size of the total population to yield

$$i = (c - i) \times \alpha \times \gamma \times \delta \times \bar{s}.$$

Here i is the fraction of the population incarcerated and c is the fraction of criminals in the population. In our derivation, we assume that a fixed fraction of an infinitely lived population exhibits complete persistence in crime, but there are several ways to derive the same equation that do not require infinitely lived persons or career criminals.[5]

Our steady-state equation is useful because it spells out the sequence of transitions that generate entry into and exit from prison. The first two terms in the decomposition indicate that even if prosecutors and judges do not change their behavior, changes in the prevalence of criminality or

4. Our key points are unchanged in a more cumbersome version that models transitions in and out of parole. See Raphael and Stoll (2013) for a steady-state analysis that includes parole and parole revocations.

5. This framework also rests on the assumption that no innocent persons are ever arrested, convicted, or imprisoned. Further, we implicitly assume that the composition of crimes does not vary over time so that it is meaningful to talk about single rates for crimes, arrests, convictions, and admissions. Alternatively, some researchers modify this formula to incorporate different rates and different average sentences for different types of crime.

the effectiveness of policing may generate changes in the size of prison populations. Because data on the probability of conviction given arrest, γ, are so scarce,[6] scholars often implicitly assume that γ does not vary over time, and given this assumption, they focus on admission rates given conviction (δ) and expected time served given admission ($\bar{s}$) as empirical proxies for the punitiveness of sentencing policies. Further, most papers in the literature implicitly assert that if there are changes in sentencing policies that lead to more long prison spells—for example, mandatory-minimum-sentence provisions, restrictions on release to parole, and so forth—then researchers can detect the effects of these changes in policy by measuring changes in time served among those admitted to prison (see Blumstein and Beck 1999; Zhang, Maxwell, and Vaughn 2009; Pfaff 2011; Raphael and Stoll 2013). However, the imposition of more severe sentencing rules does not always generate noteworthy changes in the distribution of time served among admitted prisoners because changes in sentencing policy also change the composition of the population of admitted prisoners.

Before addressing this issue in detail, we review the empirical findings in the existing literature and describe what appears to be an unresolved debate concerning the role of mandatory minimum sentences and other sentencing enhancements as drivers of growth in prison populations. We then show how a different framework for analyzing the data resolves this debate, and we confirm some key conclusions from the literature.

2.3. Decomposition Results

Blumstein and Beck (1999) examine national data for 1980–96 on offense rates, arrest rates per offense, numbers of prison admissions per arrest, expected time served given admission, and incarceration rates. They examine linear trends in each of these statistics for six crime categories and ask how much trends in the first four statistics contribute to trends in incarceration rates by offense. They conclude that 88 percent of growth in total incarceration rates for state prisons in 1980–96 was due to trends toward more punitive sanctions, which they decompose as follows: "the

6. Better data on convictions may be available in the future through the National Judicial Reporting Program, but the Bureau of Justice Statistics does not provide any data that allow researchers to trace offenders from the dates of their arrests to the dates of the dispositions of their cases—for example, documenting charges dropped, acquittal, conviction, and so forth.

decision to incarcerate (51 percent) and . . . (increases) in time served by those incarcerated (37 percent)" (Blumstein and Beck 1999, p. 43).

Raphael and Stoll (2013) develop a steady-state model of prison populations that is richer than the simple steady-state equation above because it includes parole. The model includes six states: not incarcerated or on parole, incarcerated for a violent felony, incarcerated for a property felony, incarcerated for a drug felony, incarcerated for a parole violation, and on parole. They employ data from the National Corrections Reporting Program (NCRP) in a few years around 2004 and a few more years around 1984 as well as data from the 1986 and 2004 Survey of Inmates in State and Federal Correctional Facilities and the FBI's Uniform Crime Reports. They calculate the transition probabilities between these six states and solve for the steady-state incarceration rates implied by these transition matrices under the assumption that crime rates in different categories determine the population at risk for entering the prison system.

Raphael and Stoll (2013) perform these steady-state calculations for the 1984 and 2004 parameter values and then calculate a counterfactual 2004 steady state assuming 2004 crime levels and 1984 policy parameters. They repeat the counterfactual calculation, making adjustments for the fact that 2004 crime rates would have been higher if incarceration rates had been lower. However, even with these adjustments, they conclude that more than 90 percent of the difference between the 1984 and 2004 steady states reflects differences in sentencing policies. When discussing the relative contribution of various components of policy to the growth in prison populations, they echo the conclusions of Blumstein and Beck (1999): "[O]ur enhanced tendency to sentence convicted felons to prison is particularly responsible for incarceration growth, though longer sentences are also a contributing factor" (Raphael and Stoll 2013, p. 80 and figure 3).[7] Both Raphael and Stoll (2013) and Blumstein and Beck (1999) conclude that expected time served given admission to prison rose sharply for violent criminals over time, and they identify this change as a noteworthy driver of growth in prison populations.

In contrast, Pfaff (2011) concludes that changes in expected time served for admitted prisoners played no role in the growth of prison populations over a similar time period. The analysis in Pfaff (2011) differs from much of the related literature in three ways. First, Pfaff does not rely

7. Raphael and Stoll also demonstrate that their 1984 and 2004 steady-state calculations are reasonable approximations for the 1984 and 2004 prison populations and effectively approximate the change in prison populations between the 2 years.

on steady-state methods but follows cohorts in and out of prison over time. Second, while Blumstein and Beck (1999) rely on national aggregates and Raphael and Stoll (2013) employ data from all states that filed reports with the NCRP, Pfaff (2011) relies on NCRP reports from 11 states that he identifies as providers of reliable data. Third, Pfaff (2011) does not calculate admission rates and time-served distributions that are specific to particular crime categories but instead uses aggregate flows.

Pfaff (2011, p. 495) finds that, among newly admitted prisoners, expected time served prior to release remained roughly constant over the 1980s and 1990s and thus concludes that "attention to sentencing matters is at least partially misplaced." He contends that growth in mandatory-minimum-sentence provisions and other policies that enhance sentences played little or no role in the growth of prison populations.[8]

In the process of making his argument, Pfaff (2011) acknowledges that if states made sentencing harsher by dictating positive sentences for some offenders who previously received probation and longer sentences for offenders convicted of more serious crimes, average time served among admitted prisoners could remain constant even though the adoption of more punitive sentencing rules happened to be the sole factor causing growth in the stock of prisoners. However, Pfaff then claims that because various percentiles of the time-served distribution—the 10th, 25th, 50th, 75th, and 90th—did not change much over time, one must conclude that changes in sentencing policies cannot be important drivers of growth in prison populations.

We demonstrate in Section 3 that a move to more punitive sentencing rules can create growth in prison populations without creating any noteworthy changes in the distribution of time served among admitted prisoners. Further, we argue in Section 5 that this is precisely how prison populations grew in the United States.

The relative stability of the distribution of time served among admitted prisoners during the 1980s and 1990s resulted from offsetting increases in various types of admissions. The number of persons serving short terms increased as the number of admissions associated with parole revocations and convictions for minor crimes increased, but the number serving medium and long terms also increased as offenders charged with more serious crimes began serving more time in prison. Prison popula-

8. In fact, Pfaff (2011) concludes that the widespread view that the increased use of long prison spells has been an important driver of prison growth is a myth. He argues that changes in admissions policies drove prison growth.

tions grew because sentencing policies became more punitive. The overall distribution of time served among admitted prisoners changed little because sentencing became more punitive for offenders in all crime categories.

Changes in expected time served given admission to prison are not, per se, germane to scholarly assessments of the effects of changes in rules that govern sentencing and parole on rates of growth in prison populations. Although the literature contains the repeated contention that changes in $\bar{s}$ provide direct evidence of whether harsher sentencing policies—such as elevated mandatory minimum sentences, enhanced penalties for habitual violators, TIS laws, and so forth—are important drivers of growth, this is simply not true.[9]

We contend that neither panel regression models that attempt to measure the links between specific laws and prison growth nor decomposition methods that attempt to measure changes in expected time served among admitted prisoners provide clean information about the effects of changes in sentencing policies on the growth in prison populations. In Section 3 we propose an alternative approach that characterizes sentencing policy as a collection of probabilities that describe the likelihoods that offenders convicted of various crimes will serve prison terms of various lengths.

3. NEW METHOD

If researchers want to know the extent to which policies that result in longer time served, given the severity of crimes committed, contribute to growth in prison populations, they do not want to measure changes in the fraction of admitted prisoners who serve long sentences. Instead, they want to measure changes in the fraction of convicted offenders who serve long sentences. Further, because prosecutors have some discretion over which charges to file and how to bargain the terms of plea agreements,

9. Langan (1991, p. 1570) makes a similar mistake when analyzing data from an earlier period. He argues that mandatory-sentencing laws did not drive growth in prison populations and cites as evidence that "[p]rison sentence lengths have not gotten longer since 1973, although mandatory sentencing laws commonly authorized or required longer sentences." Changes in the average lengths of sentences among admitted prisoners provide no information about the effects of changes in sentencing policy.

one can argue that researchers should take another step back and focus on the fraction of arrested offenders who serve long sentences.[10]

Instead of characterizing sentencing policy as a choice of an admissions probability and an expected sentence length, we characterize it as a set of probability weights, ρ_s, where $s = 0, 1, 2, \ldots, S$ are the potential sentence lengths that a convicted offender may serve and ρ_s is the probability of serving a sentence of s given conviction. Here $s = 0$ denotes being fined, sentenced to probation, or some other punishment that does not involve prison time. At the other extreme, $s = S$ denotes serving the maximum possible sentence.

Again, we deal with the simplest case and ignore parole. Given our new notation, it is straightforward to rewrite our steady-state equation as

$$i = (c - i) \times \alpha \times \gamma \times \sum_{s=1}^{S} s\rho_s.$$

Now consider a change in policy that involves uniformly harsher sentencing policies; that is, let $\boldsymbol{\rho}' = [\rho_0', \rho_1', \ldots, \rho_S']$ describe the new sentencing regime, let $\rho_s' = k\rho_s \;\; \forall s > 0$ with $k > 1$, and let $\rho_0' = 1 - \Sigma_{s=1}^{S}\rho_s'$. If we assume that criminal behavior, arrest rates, and conviction rates do not change in response to this change in sentencing policy, the new steady-state prison population is monotonically increasing in k. However, $\bar{s}$, the average time served among convicted offenders who enter prison does not change, since

$$\bar{s} = \frac{\sum_{s=1}^{S} s\rho_s'}{\sum_{s=1}^{S} \rho_s'} = \frac{\sum_{s=1}^{S} s\rho_s}{\sum_{s=1}^{S} \rho_s}.$$

Moreover, the entire distribution of time served among admitted prisoners is the same under $\boldsymbol{\rho}'$ as under $\boldsymbol{\rho}$. Changes in sentencing policy can create enormous growth in prison populations while having little or no effect on the distribution of time served among admitted prisoners. Further, it is easy to construct scenarios in which a move to more punitive sentencing increases prison populations while reducing the average time served among admitted prisoners.

The framework we describe can easily be extended to include crime-specific sentencing probabilities. Let $j = 1, 2, \ldots, J$ denote an exhaustive and mutually exclusive list of crime categories. We define ρ_{js} as

10. We argue below that prosecutors may bargain more aggressively when sentencing provisions allow them to file initial charges that will result in long prison terms given conviction.

the baseline probability of serving a sentence of s years given conviction for crime j and then, for each j and for all $s > 0$, define $\rho'_{js} = k_{js}\rho_{js}$.[11] Here $\boldsymbol{\rho}'$ and $\boldsymbol{\rho}$ are $J \times S$ matrices that describe sentencing policies, and given any flow of persons convicted for various crimes and an initial matrix $\boldsymbol{\rho}$, we can create an infinite number of $\boldsymbol{\rho}'$ matrices that imply higher admission rates and larger steady-state prison populations but no change in the distribution of time served among admitted prisoners.

Below we show that, compared with arrested offenders in 1985, those arrested in more recent years faced much higher likelihoods of serving short, medium, and long prison spells, and this result holds for almost all offense categories. In terms of the notation above, the best way to describe how policy has changed since 1985 is to state that $k_{js} > 1$ for every $s > 0$ for almost all j.[12]

4. DATA

In this section, we present results from our analyses of data on arrests, admissions, releases, and prison populations. We use NCRP data to construct measures of admissions, releases, and time served for different states and time periods. We follow Pfaff's (2011) approach of auditing the NCRP data to select a set of states that provide reliable data over a long period. However, our audits are more extensive, and we take the additional step of using microdata on arrests to create offense-specific measures of arrests for each state-year-race cell in our NCRP data.

We also use agency-level data from the UCR to construct state-level data on arrests by offense for various years, and we demonstrate that our ability to track comovements in arrests and admissions over time by offense is key to developing a more complete understanding of how prison populations grew over time. In particular, we show how ratios of admissions to arrests evolved for various offense categories. These ratios do not tell us everything we want to know about how the likelihood of imprisonment changed over time for persons arrested for specific offenses because some offenders are convicted of crimes that differ from the charges associated with their arrests and because there are lags between arrest

11. Because the sum over s of the ρ_{js} values for each j must equal 1, this transformation also implicitly defines ρ_{0j} for each j.

12. The one exception is that the likelihood of serving short prison terms fell among those arrested for some violent crimes. However, the likelihoods that these same arrested offenders serve medium or long terms increased by even more.

and conviction. However, we gain useful insights by tracking the ratios over time, and in some analyses below, we treat them as proxies for the likelihood of entering prison given arrest for a particular offense.

4.1. Data Quality

Available data on crimes, arrests, admissions to prison, releases from prison, and stocks of prisoners in the United States are usually of lower quality than well-known data series that track employment or education levels. Data sets on crimes and outcomes in the criminal justice system are typically compilations of self-reports from government agencies in the criminal justice system concerning their own activities. This reliance on self-reports results in missing reports and data that are internally inconsistent or transparently wrong.

Social scientists typically respond to these data-quality problems either by avoiding certain data series altogether or by hoping that at least the estimates of national aggregates derived from various series are somewhat reliable. We take a different approach. We analyze NCRP data from each state separately and then restrict our attention to a set of states for which NCRP data pass a number of reliability tests. We then clean the UCR data on arrests for those states to make sure that we have reliable information on the evolution of arrests over time.

4.1.1. Cleaning the National Corrections Reporting Program Data. We began by auditing the NCRP data.[13] Pfaff (2011) performed similar audits on the NCRP admission and release files, but his sample ended in 2002, and our cleaning and checking procedures are more involved and include consistency checks with data on prison populations that are available only starting in 2005.

The NCRP data provide detailed records of admissions and releases for many states and years for the period 1983–2009 as well as stocks of prisoners in custody for 2005–9. We begin our audit process by restricting our attention to states that filed NCRP reports on a fairly consistent basis, especially during the 1990s, when prison populations were rising quite rapidly. This requirement eliminates 16 states and the District of Columbia.

For the 34 states that remain, we conduct several checks for internal and external consistency. In our first check, we examine the dates in the release and admission data in the NCRP to ensure internal consistency in

13. See Section A of the online appendix for details of our data cleaning and construction procedures.

the following sense: for any given year *t*, the total number of prisoners in the release files with recorded admission dates in year *t* should not be greater than the number of prisoners recorded in the admissions files for year *t*.

In our second check, we use the admission and release flows from 1987 through 2009 to determine whether the age-specific stocks in the post-2005 NCRP files are consistent with the flow data on admissions and releases prior to 2005. For example, if we assume that offenders 15 and younger are not entering regular prisons, the difference between total admissions and total releases after 1987 among the cohorts who were 15 or younger in 1987 will tell us what the stocks of prisoners under age 35 should be in 2007.

Our third and fourth checks involve comparisons of NCRP data on admissions and releases of prisoners and National Prisoner Statistics (NPS) data on flows and stocks. These data series should not match exactly because they do not define the prisoner populations of interest in exactly the same way. However, large deviations in reported flows or between the reported changes in NPS stocks and the implied NCRP changes in stocks are cause for concern.[14]

These four checks allow us to identify eight states with continuous reports that appear to be of acceptable quality: California, Colorado, Michigan, New Jersey, New York, South Carolina, Washington, and Wisconsin.[15] We do not use New York because its release records contain no information on the type of admission or year of admission to prison for several years. Without these variables, we cannot determine how distributions of time served for different types of offenders evolved over time.

Figure 3 uses data from the NPS to display incarceration rates for 1985–2010 for three samples: state prisons in the seven states in our main sample, all state prisons, and all state and federal prisons. Our main analyses are of state prison populations, and the overall growth pattern for the total incarceration rate in our seven states is similar to that observed for all states. Growth is slightly more rapid early and levels off earlier in our sample, but the data for California account for most of these discrepancies. California is a large state that experienced rapid growth in prison

14. For years before 1999, the stock data for the National Corrections Reporting Program (NCRP) and National Prisoner Statistics are comparable: both contain counts of all prisoners in custody. However, stock data after 1998 and flow data are not comparable. See Section A of the online appendix for details.

15. Illinois also provides reliable data but stopped reporting in 2003.

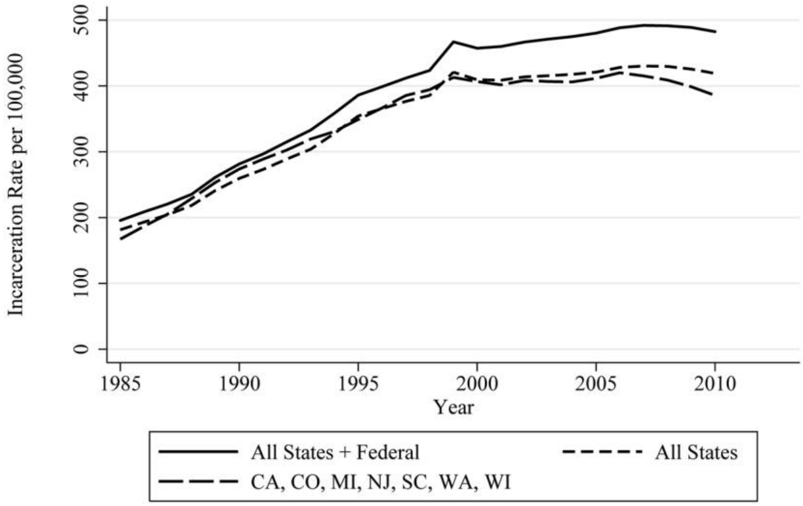

Figure 3. Prison populations for three samples

populations early in the sample period, and, in part because of capacity constraints, California prison populations stopped growing while many others continued to grow.

The federal prison population has always been much smaller than the total population in state prisons, but over this period the federal prison population grew more rapidly, in percentage terms, and by 2010 federal prisons housed more than 10 percent of all inmates. (We address the growth of federal prison populations in Section 7.) The patterns of growth in the federal system are quite different than those in our seven-state sample.

4.1.2. Reliable Data on Crime and Arrests. The FBI's UCR system collects data on crimes and arrests through reports from local law enforcement agencies. However, missing reports are also rife in these data. Further, while the FBI uses them to produce national estimates of annual crime and arrest rates by offense, we are not aware of any FBI efforts to produce comparable estimates at the state level. Here we describe how we clean the data in the UCR arrest files.[16]

16. For edited versions of the Uniform Crime Reports (UCR) crime files, see Justin McCrary, UCR and LEOKA Files, 1960–2005, from FBI (http://emlab.berkeley.edu/~jmccrary/UCR/index.html). We describe the construction of our arrest files in Section B of the online appendix. When we apply our cleaning procedures to the UCR crime files, we are able to create edited versions of the crime data that closely match McCrary's files.

As a first step, we examine the monthly reports to identify incidences of backlog filing. Some agencies periodically place the crimes and arrests for a several-month period in one monthly report, and it is necessary to identify these reports before determining the monthly frequency of crimes or arrests in an agency over a specific period. Using monthly averages of agencies' valid reports of arrests for specific calendar years, we make imputations for missing monthly reports that do not result from backlog filing.[17]

Figures 1a–c in the online appendix document arrest-rate trends for our seven states and the nation as a whole. Arrest rates are always higher in our NCRP sample, but the two series track each other quite closely.[18]

5. RESULTS FROM NATIONAL CORRECTIONS REPORTING PROGRAM DATA

Table 2 displays information about changes in the distribution of punishments that arrested offenders receive in the states with clean NCRP data. The results are for two cohorts of arrested offenders: those arrested in 1985 and those arrested in 2000. The organization of the table follows the statistical model of prison populations presented in Section 3.1.

Recall that, for a person who is arrested and convicted of crime j, we define ρ_{js} as the probability that the offender serves a prison spell of length s given some baseline set of sentencing policies. We then define a new set of sentencing policies, ρ'_{js}, using a matrix of constants k_{js} that scale these punishment probabilities up or down for all $s > 0$; that is, $\rho'_{js} = k_{js}\rho_{js}$. Further, we collect these probabilities in matrices $\boldsymbol{\rho}$ and $\boldsymbol{\rho}'$ that fully characterize the two sentencing policy regimes.

Now consider data on corrections outcomes for two cohorts of arrested offenders, where the first cohort faces $\boldsymbol{\rho}$ and the second cohort faces $\boldsymbol{\rho}'$. Further, make three assumptions about the charging and sentencing processes that govern both regimes. First, assume that each convicted defendant in both cohorts is convicted of the offense listed in the UCR record that documents his arrest—that is, the most serious charge against him at the time of arrest. Next, assume that the probabilities of conviction given arrest for specific crimes are identical for both cohorts.

17. When agencies do not report for entire years, we use the interpolation procedures described in Section B of the online appendix to fill in the missing data.

18. In Neal and Rick (2014), we show that crime rates track arrest rates in our NCRP sample. However, in those seven states, rates of violent crime fell even faster than arrest rates for violent crime in the late 1990s and 2000s.

Finally, assume that prisoners enter prison in the same year they are arrested.

None of these assumptions are strictly valid. However, they allow us to link data on arrests and admissions in a useful way. We consider combinations of offense categories $j = 1, 2, \ldots, J$ and prison-spell lengths $s = 1, 2, \ldots, S$. Then, in each cohort of arrested offenders, we calculate ratios of the total number of inmates convicted of offense j who served s periods in prison to the total number of persons originally arrested for offense j.[19] Given our assumptions, the ratio associated with any pair (j, s) in our baseline cohort is a consistent estimator of the quantity $(\gamma_j \times \rho_{js})$, where γ_j is the probability of conviction given arrest for offense j. The corresponding ratio for the latter cohort is a consistent estimator for $(\gamma_j \times \rho'_{js})$.

We seek to measure $k_{js} = \rho'_{js}/\rho_{js}$, the probability that an alleged offender arrested for crime j in the latter cohort serves a sentence of length s divided by the corresponding probability for the baseline cohort. For each pair (j, s), we form a consistent estimator of k_{js} by calculating the ratios we describe above; that is,

$$\hat{k}_{js} = \frac{\widehat{\gamma_j \times \rho'_{js}}}{\widehat{\gamma_j \times \rho_{js}}}.$$

The $J \times S$ matrix $\widehat{K}$ containing elements $\hat{k}_{js}$ describes how sentencing policies changed between the two cohorts. The 84 cells in the first 14 rows and six columns of Table 2 describe results for $J = 14$ offense categories and $S = 6$ sentence lengths. The cells contain time-served outcomes for both the 1985 and 2000 cohorts as well as the corresponding $(J \times S)$ elements of $\widehat{K}$.

For example, consider the cell in Table 2 that corresponds to the row "Drug Trafficking" and the column "2–3 Years." The first entry tells us that for every 1,000 persons arrested for drug trafficking in 1985, there were just over seven persons who entered prison in 1985 and served between 2 and 3 years for drug trafficking. The second entry implies that the corresponding figure for 2000 is almost 27 persons. The final entry, 3.68, is the ratio of these two ratios, $\hat{k}_{js}$. Given our assumptions above, this value indicates that the probability of serving between 2 and 3 years

19. We track persons who entered prison following a conviction for a specific offense. We include those who go directly to prison and those who enter prison after a court revokes their probation. Table 2 does not employ data on prison spells that result from parole violations.

Table 2. Number of Persons per 1,000 Arrests Who Serve Prison Terms of Length *s*

	0–1 Years	1–2 Years	2–3 Years	3–4 Years	4–5 Years	5+ Years	All Term Lengths
Violent crime:							
Murder and homicide:							
1985	37.84	55.55	45.56	35.58	23.02	239.74	437
2000	31.26	36.96	29.36	25.35	23.87	478.39	625
Ratio	**.83**	**.67**	**.64**	**.71**	**1.04**	**2.00**	**1.43**
Forcible rape:							
1985	9.01	21.72	22.77	20.68	10.34	38.80	123
2000	11.00	13.36	20.04	13.36	14.93	80.04	153
Ratio	**1.22**	**.62**	**.88**	**.65**	**1.44**	**2.06**	**1.24**
Robbery:							
1985	26.76	37.75	22.85	14.90	8.61	20.37	131
2000	34.62	37.67	24.78	17.49	13.73	69.76	198
Ratio	**1.29**	**1.00**	**1.08**	**1.17**	**1.60**	**3.43**	**1.51**
Aggravated assault:							
1985	9.76	11.24	5.59	2.48	1.14	2.75	33
2000	11.74	9.90	4.48	3.26	2.02	6.72	38
Ratio	**1.20**	**.88**	**.80**	**1.32**	**1.77**	**2.44**	**1.16**
Other assault:							
1985	1.22	1.06	.30	.13	.08	.13	2.9
2000	3.39	3.01	.90	.48	.32	.66	8.8
Ratio	**2.77**	**2.85**	**2.95**	**3.74**	**3.94**	**5.11**	**3.00**
Property crime:							
Burglary:							
1985	27.14	16.74	7.33	3.24	1.50	3.17	59
2000	40.34	23.49	13.54	6.06	3.89	9.67	97
Ratio	**1.49**	**1.40**	**1.85**	**1.87**	**2.59**	**3.05**	**1.64**
Motor vehicle theft:							
1985	13.37	5.18	1.46	.45	.16	.59	21
2000	41.74	18.32	5.59	1.81	.97	1.78	70
Ratio	**3.12**	**3.54**	**3.82**	**4.01**	**6.17**	**3.04**	**3.31**
Larceny or theft:							
1985	6.52	2.73	.82	.40	.14	.38	11
2000	12.74	5.55	2.07	.80	.45	.71	22
Ratio	**1.95**	**2.03**	**2.53**	**1.99**	**3.21**	**1.88**	**2.03**
Other property crime:							
1985	2.56	1.69	.97	.55	.22	.32	6.3
2000	3.29	2.33	1.00	.55	.35	.89	8.4
Ratio	**1.28**	**1.38**	**1.02**	**1.01**	**1.58**	**2.84**	**1.33**
Drug crime:							
Drug trafficking:							
1985	29.81	29.96	7.29	2.05	1.21	3.50	74
2000	62.36	59.44	26.84	11.91	6.42	9.45	176
Ratio	**2.09**	**1.98**	**3.68**	**5.82**	**5.31**	**2.70**	**2.39**

Table 2. *continued*

	0–1 Years	1–2 Years	2–3 Years	3–4 Years	4–5 Years	5+ Years	All Term Lengths
Drug possession or use:							
1985	7.23	2.04	.42	.18	.07	.46	10
2000	21.47	6.92	2.33	.86	.51	.85	33
Ratio	**2.97**	**3.39**	**5.60**	**4.80**	**7.76**	**1.84**	**3.17**
Other:							
Other sex crime:							
1985	9.71	17.29	13.98	11.00	6.00	19.57	78
2000	21.75	23.70	24.53	12.55	17.28	62.73	163
Ratio	**2.24**	**1.37**	**1.75**	**1.14**	**2.88**	**3.21**	**2.10**
White-collar crime:							
1985	14.95	5.95	1.74	.70	.23	.41	24
2000	23.07	8.19	3.12	1.17	.57	.68	37
Ratio	**1.54**	**1.38**	**1.79**	**1.67**	**2.49**	**1.66**	**1.54**
Other crime:							
1985	1.70	.54	.16	.07	.04	.14	2.7
2000	3.12	1.63	.63	.31	.17	.40	6.3
Ratio	**1.84**	**3.00**	**3.96**	**4.13**	**4.89**	**2.78**	**2.36**
All offenses:							
1985	5.45	3.49	1.52	.83	.43	1.53	13
2000	10.13	6.00	2.74	1.36	.92	3.19	24
Ratio	**1.86**	**1.72**	**1.81**	**1.63**	**2.16**	**2.09**	**1.84**

Sources. Arrest records are from Federal Bureau of Investigation (1980–2009); restricted-use data on prison releases and prison populations are from Bureau of Justice Statistics (1984–2009). Population data for generating incarceration rates are from Census Bureau historical population estimates.

Note. Results are based on data from California, Colorado, Michigan, New Jersey, South Carolina, Wisconsin, and Washington "Other" crimes include prostitution, gambling, and vice offenses, driving under the influence and drunkenness, and weapons charges.

in prison, conditional on being arrested for drug trafficking, increased by 268 percent between 1985 and 2000.

The last row of Table 2 presents aggregate results that map total arrests into total counts of prisoners who serve prison terms of different lengths. The last column of the table gives, for each crime category, the total number of persons who serve any prison time per 1,000 persons arrested. The ratios in the last column are ratios of admission probabilities for 2000 to the corresponding probabilities in 1985.

Table 2 documents a clear shift to more punitive treatment of arrested offenders between 1985 and 2000. To see this, let us start with the results in the last column. In both the 1985 and 2000 cohorts, only a small fraction of arrested alleged offenders ever serves prison time. Nonetheless, in percentage terms, the increase from 13 admitted prisoners in 1985 to 24 admitted prisoners in 2000 is noteworthy. Overall, the probability of entering prison given arrest increased by 84 percent, and this result is not driven by a change in the composition of arrests. Within every crime category, the fraction of arrested offenders admitted to prison rose between 1985 and 2000, and in many categories, the probability of admission given arrest more than doubled.

Next, if we focus on the last row, we see that the aggregate increase in prison admissions per arrest does not simply reflect an increase in the number of persons serving short prison terms. In 2000, arrested alleged offenders faced a higher likelihood of serving short, medium, and long prison terms. While the overall pattern likely implies a modest increase in expected time served among admitted prisoners, time served among persons who entered prison following parole revocations declined during this same period, and like Pfaff (2011), we find only minor changes in the overall distribution of time served among those admitted to prison. However, we contend that the overall stability of this distribution tells us nothing about changes in the severity of sentencing policies between 1985 and 2000. Arrested offenders faced higher risks of serving short, medium, and long prison terms in 2000 than in 1985, and this result is not driven by a change in the composition of arrests but by more punitive sentencing within each crime category.

Note that in all nonviolent crime categories, every value of $\hat{k}_{js}$ is greater than 1. This demonstrates that among those arrested for nonviolent crimes, prison terms in each of our $S = 6$ duration categories became more likely. In each nonviolent crime category, arrested offenders in 2000 faced significantly greater risks of serving 0–1, 1–2, 2–3, 3–4, 4–5, and 5+ years in prison. This pattern is clear evidence of a shift toward more punitive sentencing.

In our results for violent offenders, there are a few values that are less than 1. Some of those arrested for violent crimes did face lower probabilities of serving short prison terms in 2000, but overall, these offenders almost surely faced more punitive sentencing than their 1985 counterparts. The "Total" column shows that they faced higher probabilities of entering prison, and the "5+ Years" column shows that they faced much higher probabilities of serving long prison terms. We cannot examine composition changes in the 5+ years category, but our results strongly suggest that those arrested for violent crime in 2000 faced longer expected prison time. Between 1985 and 2000, the fraction of arrested offenders who served more than 5 years in prison increased by at least a factor of two in every violent crime category.

Table 2 contains three key results. First, within all crime categories, the likelihood that an arrested offender would serve prison time rose between 1985 and 2000. Second, for those charged with violent crimes, these increases in the likelihoods of prison admission disproportionately reflect increases in the number of offenders who serve long prison terms. Finally, among those charged with nonviolent crimes, Table 2 documents striking increases in the likelihoods of serving short, medium, and long prison terms.

If we assume that the probability of conviction given arrest was roughly constant between 1985 and 2000, the only way to explain these results is to recognize that these states greatly increased the severity of their sentencing policies between 1985 and 2000. Existing data sources do not provide measures of convictions per arrest at the state or national level. Thus, we cannot rule out the possibility that some portion of the changes that we are measuring over time reflects an increase in the rate of convictions per arrest. However, if such an increase occurred, it too may have been the result of changes in sentencing policies. Most convictions are the product of plea bargains, and worst-case scenarios for defendants shape the bargaining between defendants and prosecutors. We provide a more detailed discussion of this issue in Section 6.2.

6. SIMULATION RESULTS

Table 2 shows that, in our NCRP sample, arrested offenders faced much harsher expected punishments in the early 2000s than in 1985. Nonetheless, these results do not provide precise information about the total contribution of the implied changes in sentencing policies to the growth of prison populations after 1985. Thus, we next describe a simulation model that directly maps changes in arrest rates and policies that govern expected punishments for arrested offenders onto changes in prison populations.

Our simulation model fills a hole in the literature. To date, Raphael and Stoll (2013) provide the most detailed empirical model of prison populations. However, they employ data from only 2 years, 1984 and 2004, and they treat the prison populations in those years as steady-state populations. Further, they do not remove states that report inconsistent data but include all states that report NCRP data in 1984 and 2004.

As demonstrated above, the NCRP data from many states are not reliable, so we employ data only from states that report NCRP data that pass our basic quality checks. Given the upward trend in prison populations over much of our sample period, we do not impose steady-state restrictions but instead produce actual and counterfactual prison populations for each year between 1985 and 2005. This allows us to show that even during the late 1980s and early 1990s, when arrest rates were rising for many crime categories, changes in sentencing policy were still important drivers of growth in prison populations.

Previous studies employ simulation models that are similar in spirit to ours, but none involve the detailed measurements that we employ. McCrary (2010) develops a mathematically similar model with a single offense and no parole, and McCrary and Sanga (2012) use a parameterized version of this model to illustrate how deterrence elasticities shape the paths of prison populations following exogenous shifts in the severity of sentencing. Here we provide an overview of our methods.[20]

We assume, as before, that each convicted offender is convicted of the most serious charge listed in his arrest file—the charge recorded in UCR arrest files—and that he enters prison in the year of his arrest. Given these assumptions, we use our arrest data, data from the 1985–2009 NCRP files,[21] and NPS data for 1982–85 to estimate the following probabilities:

1. the probability that an offender arrested in 1985 enters prison as a new court commitment,
2. the probability that an offender who was paroled from prison in year $(1985 - p)$ enters prison in 1985 as the result of a parole revocation,
3. the probability that an offender exits prison to parole in year $(1985 + s)$ given that he entered prison in 1985 as the result of a new court commitment,
4. the probability that an offender exits prison to parole in year $(1985 + s)$ given that he entered prison in 1985 as the result of a parole revocation,

20. Section C of the online appendix provides details.

21. We do not employ the actual admissions data from the NCRP. Rather, we use the most recent NCRP stock data for a given state and the NCRP release files from the previous years to construct admissions files that are consistent with the stock and release data for 1985 and subsequent years. Given our rules for selecting states, the admissions series that we construct closely track the actual NCRP admissions series. By using these constructed admissions data, we ensure that the probabilities that we calculate for use in our simulation model are between 0 and 1.

5. the probability that an offender exits prison without parole supervision in year $(1985 + s)$ given that he entered prison in 1985 as the result of a new court commitment, and
6. the probability that an offender exits prison without parole supervision in year $(1985 + s)$ given that he entered prison in 1985 as the result of a parole revocation.

In the NCRP data, only trivial numbers of parolees face parole revocation more than 3 years after their release to parole. Thus, we let $p = 0$, 1, 2, 3, and we treat all revocations after 3 years as if they happened in $p = 3$. We do not need to keep track of releases from parole to freedom because they do not change the stock of prison inmates.

We let $s = 0, 1, 2, \ldots, 20$. Because we are examining the evolution of prison stocks between 1985 and 2005, we do not need to know anything about the release probabilities for $s > 20$.

We calculate the elements of items 1–6 separately for the interaction of three race groups (black, white, and other), the 14 offense categories used in Table 2, and two geographies, California and the other six NCRP states. California is a large state that, not only in recent years but also in 1985, followed corrections policies that are notably different than those of our other NCRP states.

Note that item 1 is a single probability but items 2–6 are vectors of probabilities. Thus, for each of the $14 \times 3 \times 2$ cells, we calculate a probability of commitment, a vector of probabilities of parole revocation, and four vectors of probabilities of prison release. Given any hypothetical cohort of arrested offenders for any crime in any year in any (race $\times$ geography) cell, we simulate the transitions that follow offenders' arrests—namely, transitions to prison, from prison to supervised parole release, from parole back to prison because of parole revocation, and from prison to release without parole supervision. We do not attempt to measure transitions from release without supervision back to prison because they begin as future arrests.

Thus, we can feed various sequences of hypothetical annual arrest levels through our simulation model and track the resulting counterfactual evolution of prison populations in each of the $14 \times 3 \times 2$ cells from 1985 through the end of 2005 under the assumption that the transition probabilities that describe the experiences of offenders arrested in 1985 apply to subsequent arrest cohorts.

Our goal is to address the following question: how would incarceration rates in our NCRP states have evolved over time if the transition probabilities described above had been held fixed at their 1985 levels? If we are willing to assume that crime rates and resulting arrest rates evolve independently of sentencing policies and prison populations, the transition probabilities we describe above provide all the information we need to address this counterfactual.[22] Although this is a natural starting point, some may argue that independence is not a reasonable assumption. The introduction of harsh sentencing policies may reduce arrest rates if severe expected punishments deter crime or if the imposition of longer sentences incapacitates criminals who are not replaced by new participants in criminal activity. On the other hand, harsh sentencing policies may increase arrest rates if time in prison hardens young criminals and extends their criminal careers.

It is quite difficult to produce credible measures of the extent to which harsh sentencing policies deter or harden criminals, but several studies directly investigate the importance of incapacitation by examining how much crime rates increase in the short term when amnesty programs or court decisions trigger sudden releases of large numbers of prisoners. Motivated by this literature, we build a second simulation model based on the assumption that, ceteris paribus, the number of arrests in each year is a decreasing function of the stock of incarcerated persons at the beginning of the year. Given this assumption, we can simply include additional arrests for 1987 and after to reflect the fact that arrest rates would have been higher in those years if the 1985 policies had been in place beginning in 1986.

We parameterize this adjustment with crime-specific elasticities of criminal activity to lagged prison populations, and we adopt parameter values that likely yield upper bounds on how much the move to more punitive sentencing policies affected arrest rates. Among the studies of incapacitation that we have reviewed, Levitt (1996) reports by far the largest, in absolute value, estimated elasticities of prison stocks to crime rates (see Johnson and Raphael 2012; Lofstrom and Raphael 2015; Marvell and Moody 1996; McCrary and Sanga 2012; Owens 2009). He uses court orders targeting prison overcrowding as instruments for changes in prison populations and concludes that, at the state level, the elasticity of crime rates in year t to incarceration rates in year $t - 1$ is $-.4$ for violent crimes and $-.3$ for property crimes. These estimates are more than double, in absolute value, most estimates in the related literature.

22. See Section C of the online appendix for details.

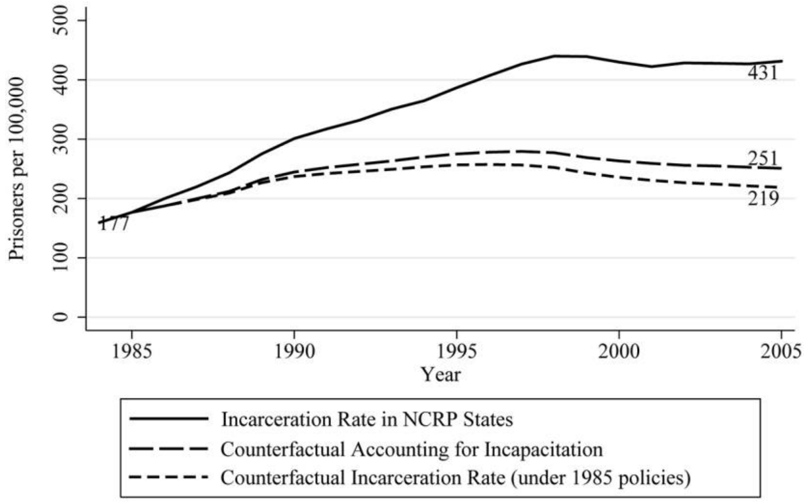

Figure 4. Actual and counterfactual incarceration rates

We assume that, in each crime category, there is a constant ratio of arrests to reported crime over time. This allows us to treat Levitt's estimated elasticities of crime to lagged prison stocks as estimates of the corresponding elasticities of arrests to lagged prison stocks.

Figure 4 presents our results. The solid line tracks actual end-of-year incarceration rates over time. The long-dashed line tracks simulated incarceration rates under the assumption that the 1985 sentencing policies remained in place and, as a result, arrest rates rose in 1987 and future years. The short-dashed line tracks simulated incarceration rates under the assumption that arrest rates evolved independently of sentencing policies.

To begin, note that the actual and counterfactual prison populations diverge quickly. Figure 2 documents positive trends in arrest rates across all crime categories during the late 1980s, and arrests for drug crimes and violent crimes continued to increase in the early 1990s. However, these trends in arrests cannot account for the rapid growth of prison populations during the late 1980s and early 1990s.

Now consider the sample end points. The incarceration rate at the end of 1985 is 177. The 2005 rate is 431. The simulated 2005 incarceration rates associated with holding time-served probabilities at their 1985 values are 251 and 219. The former number reflects our adjustments for incapacitation effects, and the latter reflects the assumption that arrest flows evolve independently of prison stocks. Our simulation results thus suggest that changes in the severity of sentencing policies generated between 71 and 83 percent of the growth in incarceration rates between 1985 and 2005 in our seven states.

We note above that Raphael and Stoll make similar calculations concerning changes in steady-state incarceration rates implied by the sentencing policies and arrest rates that prevailed in 1984 and 2004. In Raphael and Stoll (2013, figure 3.1), they report that 91 percent of the growth in these implied steady states is attributable to changes in policy. The key differences between our methods and those in Raphael and Stoll (2013) are that they decompose growth in implied steady states rather than actual populations, they do not restrict their analyses to states that report clean NCRP data, and they use smaller adjustment factors, in absolute value, to correct for incapacitation and deterrence.

It is not clear how the first two differences should affect Raphael and Stoll's results compared to ours.[23] However, it is clear that they attribute a larger role to changes in sentencing policies, at least in part because they assume that the effects of the growth in prison populations on arrest rates are significantly smaller than those reported by Levitt (1996). The methods they employ to create their estimates are defensible and produce results that are more in line with the rest of the literature. By using Levitt's estimates, we are most likely creating a conservative estimate of the effects of more severe sentencing and corrections policies on the growth in prison populations.

Yet some may challenge this characterization because Levitt exploits only year-to-year variation in prison populations induced by court orders. If court orders are less salient for potential criminals than sustained long-term shifts toward more punitive policies, Levitt's research design may not capture the full deterrence effects of recent growth in prison populations on crime rates. Nonetheless, Nagin (2013) presents a comprehensive review of the literature on deterrence and concludes that the deterrence effects of shifts to longer prison terms are modest at best.[24] In addition, while a sustained commitment to punitive policies may deter some potential offenders, it may also harden others. Several recent

23. However, there are reasons to believe that some of the data Raphael and Stoll (2013) employ are quite noisy. For example, their table 2.2 reports that in 2004 their data contain almost 40 percent more prison admissions for murder than actual arrests for murder.

24. The most relevant studies that Nagin (2013) reviews are Helland and Tabarrok (2007) and Lee and McCrary (2009).

studies suggest that putting more offenders in prison and keeping them there longer may have criminogenic effects (see Aizer and Doyle 2013; Bayer, Hjalmarsson, and Pozen 2009; Chen and Shapiro 2007; Di Tella and Schargrodsky 2013).

In Figure 4, we track total and simulated prison populations. We repeated the exercise while keeping separate track of the populations of prisoners who last entered prison following conviction for crime and the populations who last entered prison following parole revocations. The results show that, in most years, the number of prisoners associated with new court commitments is at least three times that associated with parole revocations. Further, at the end of 2005, both stocks of prisoners are roughly twice the levels implied by the simulations that impose the 1985 corrections policies throughout the sample period. Although there appear to have been some changes over time in how California made decisions concerning parole revocations, the key driver of growth in the number of prisoners serving terms that began as the result of parole revocations is the growth in the stock of persons on parole and therefore at risk for revocation, and this growth in the stock of parolees was driven primarily by growth in the number of persons serving time for new convictions.

6.1. Race-Specific Results

Figures 5 and 6 repeat the simulations in Figure 4 separately for whites and blacks, respectively. These figures highlight the striking racial difference in incarceration-rate levels that exist throughout the time frame we consider. Further, the increases in incarceration-rate levels observed among blacks are more than three times those observed among whites.

On the other hand, these results provide no evidence that the changes in sentencing policies that drove the prison boom created greater changes in the punitiveness of sentences applied to black offenders relative to white offenders. In fact, the percentage of the increase in incarceration rates that cannot be attributed to trends in arrests is greater among whites than blacks.[25] In Neal and Rick (2014, tables A6 and A7), we provide evidence that, even in our base year 1985, black offenders were already more likely to receive significant prison time for some offenses (for example, robbery, burglary, motor vehicle theft, and larceny), but this baseline

25. However, because we cannot identify Hispanics in a consistent way over time and because the Hispanic population is growing over time, we cannot rule out the possibility that punishments applied to both black and Hispanic defendants became more severe over time relative to those applied to non-Hispanic white defendants.

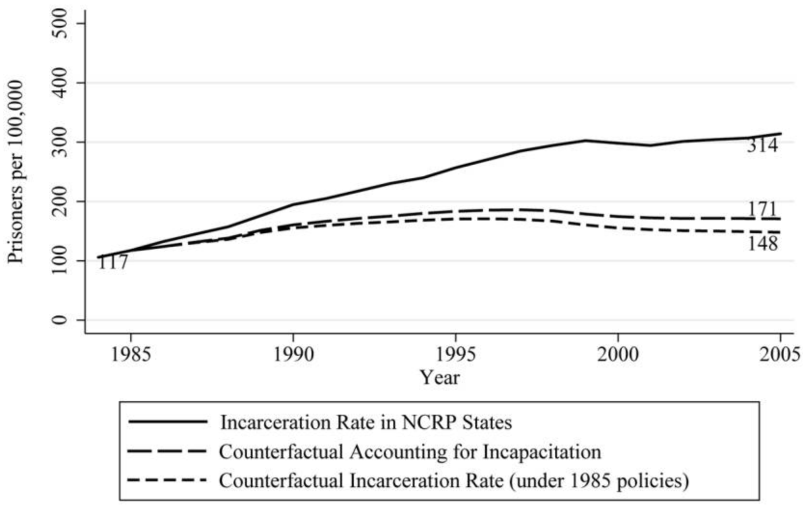

Figure 5. Actual and counterfactual incarceration rates: whites

differential is a minor part of the story. Our NCRP states pursued an across-the-board shift to more punitive sentencing and parole policies for all offenders, and this change had much more significant effects on black communities than white communities because arrest rates for blacks have been at least four times as large as arrest rates for whites since 1980.[26]

6.2. Conviction Rates

No existing sources provide representative data on conviction rates given arrest at either the state or national level. Thus, to create our simulation results, we follow the common practice of assuming that conviction rates given arrest do not vary over time in our sample.

At least three scenarios could have generated rising conviction rates for arrested offenders between 1985 and 2005. First, more severe sentencing policies may have dealt prosecutors a stronger hand in negotia-

26. We do not claim that racial disparities in charges filed against arrested offenders or sentences given to convicted defendants do not exist. Rehavi and Starr (2013) not only review the substantial literature on racial disparities in sentencing but also provide clear evidence of discriminatory behavior by federal prosecutors prior to the sentencing stage. When dealing with comparable arrested offenders, federal prosecutors are more like to file formal charges that bring mandatory-minimum-sentence provisions into play if the offenders are black, and we cannot rule out the possibility that state prosecutors engage in the same biased behaviors. Still, our results provide no evidence that these types of discriminatory behaviors have become worse over time.

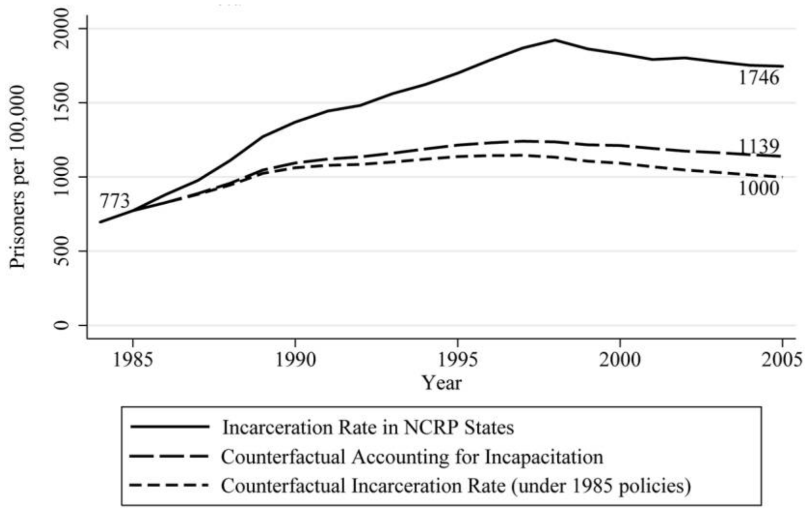

Figure 6. Actual and counterfactual incarceration rates: blacks

tions over plea bargains. If this was the case, increases in conviction rates were simply mechanisms through which changes in sentencing policies drove the growth in prison populations.

Second, state and local governments may have allocated more generous budgets to prosecutors over time. Such changes in budget would not qualify as changes in sentencing policy per se. However, the same political forces that influence sentencing policy also likely affect budgets for prosecutors and correctional institutions. Laws that mandate harsher punishment for offenders cannot accomplish their stated objectives if legislators do not allocate sufficient resources to both prosecute and confine offenders.

Finally, characteristics of arrested offenders may have changed over time in ways not captured by our NCRP data. If the composition of offenders shifted in adverse ways in crime categories, our simulation results may overstate the importance of changes in sentencing policies as drivers of growth in prison populations.

We cannot examine this possibility directly because no national or state data on defendants' characteristics and case outcomes exist. However, we have examined reports that summarize the State Court Processing Statistics (SCPS) collected by the Bureau of Justice Statistics during the month of May in 1990, 1992, 1994, 1996, 1998, 2000, 2002, and 2004 (Bureau of Justice Statistics 1990–2004). The SCPS samples are not representative of the nation or any particular state. The SCPS sampling scheme defines 75 large urban counties as the population of interest, and each SCPS survey draws cases from only about 40 counties.

Conviction rates rise and fall over time in the SCPS data.[27] While it is hard to know the extent to which these changes reflect sampling variation or secular trends in conviction rates, the SCPS data provide no evidence that movements in conviction rates are correlated with movements in offenders' characteristics. In each major crime category, the proportions of arrested offenders who have prior arrests, prior convictions, or ongoing court supervision do not covary with conviction rates.[28]

Although California is well represented in the SCPS data, the SCPS sample contains fewer than 10 counties that are in our other NCRP states. Thus, the SCPS data do not rule out the possibility that unmeasured trends in offenders' characteristics drove secular increases in conviction rates over time in some of our NCRP states. Nonetheless, we have found no data that support this possibility.

7. FEDERAL PRISONS AND JAILS

To this point, we have focused on growth in state prisons. The NCRP contains spotty data on federal prisons, so we have pieced together information on stocks of federal prison inmates from other sources. Table 3 presents incarceration rates in federal prisons for 1970–2010 and separate rates by offense category for 1989–2010. Between 1980 and 2010, federal incarceration rates increased by more than a factor of 6. During the 1990s and 2000s, the federal prison population grew at a significantly faster rate than the population of state prisons.

We do not have federal arrest data that are comparable to the arrest data we employ in analyzing the growth of state prison populations. Thus, we cannot clearly assess the extent to which changes in federal corrections policies mirrored the changes at the state level. Nonetheless, the available data highlight an important difference between federal and state prisons. The growth of federal prison populations does not reflect increases in incarceration rates for all federal crime categories. Three of-

27. The felony conviction rate for violent offenses ranges from 40 percent in 1990 to 52 percent in 2004, and there are ups and downs in between. The felony conviction rates for property offenses, drug offenses, and public-order offenses follow a similar pattern overall but tend to be 10–20 percent higher than those for violent offenses.

28. Details are available from the authors on request.

fense categories—drug crimes, weapons violations, and immigration offenses—account for the majority of the increase in the federal incarceration rate. The incarceration rate for "other" federal crimes also rose, but Table 3 shows that the federal incarceration rates for standard violent and property crimes were higher in 1989 than in 2010.

Further, the literature shows that, in contrast to our results for state prisons, growth in the federal prison population was not the result of a color-blind shift toward more punitive sentencing. The Anti-Drug Abuse Act of 1986 was one of the major federal actions in the War on Drugs, and it drew distinctions between crack and powder cocaine that appeared to target black drug offenders. Many black offenders received much longer sentences than comparable white offenders who possessed and distributed the same drug in a different form.[29]

While the federal prison population grew even faster than state prison populations over the past 3 decades, jail populations grew at similar rates, at least over the long term. Over the period 1983–2010, the ratio of inmates in state prisons to inmates in local jails rarely fell below 1.8 and never rose as high as 2.1. Jail populations follow the same long-term trends as state prison populations.

Without more detailed information on the movements of prisoners between jails and prisons, it is not possible to know exactly why jail stocks track prison stocks so closely. Some sentenced prisoners serve time in jail while awaiting transfer to state prisons, and other convicted prisoners with short sentences serve their entire sentences in jail without ever entering state prison. Thus, it seems reasonable to expect that the move to more punitive sentencing that caused prison populations to grow also increased jail populations. Nonetheless, more work is required to pin down the different sources of growth in jail populations over the past several decades.

29. See Alexander (2012) for an extensive discussion of this issue and related aspects of the War on Drugs. In 2010, President Barack Obama signed the Fair Sentencing Act, which greatly reduced but did not completely eliminate disparities in mandatory-sentencing provisions among drugs that are chemically similar. Finally, Rehavi and Starr (2013) find that black offenders receive sentences that are almost 10 percent longer than comparable white offenders arrested for the same crimes.

Table 3. Jail, State Prison, and Federal Prison Incarceration Rates per 100,000 Persons

			Federal Prison						
	Jail	State Prison	All Offenses	Violent Crimes	Property Crimes	Drug Crimes	Weapons Violations	Immigration Offenses	Other Crimes
1970		90	10						
1971		89	11						
1972	67	87	11						
1973		89	11						
1974		96	11						
1975		105	12						
1976		113	13						
1977		117	14						
1978	71	125	13						
1979		128	12						
1980		134	11						
1981		149	12						
1982		166	13						
1983	97	173	14						
1984	98	181	15						
1985	111	194	17						
1986	111	208	18						
1987	120	222	20						
1988	137	236	20						
1989	157	265	24	4.29	1.20	11.54	1.14	.90	4.91
1990	164	284	26	4.05	1.01	14.04	1.42	.80	4.94
1991	168	299	28						

1992	173	315	31	4.17	.88	21.00	2.45	.94	.80
1993	181	341	35	4.18	.65	20.59	2.74	.91	5.68
1994	184	369	37	4.35	.53	21.89	2.93	1.08	5.72
1995	194	390	38	4.39	.48	22.71	3.20	1.47	5.89
1996	194	406	40	4.37	.46	23.70	3.30	1.92	6.03
1997	208	421	42	4.38	.47	24.99	3.45	2.33	6.59
1998	220	438	46	4.63	.51	26.34	3.65	3.11	7.29
1999	223	450	50	5.25	.51	28.45	3.95	4.23	7.22
2000	219	443	52	4.87	.57	29.24	4.24	5.45	7.15
2001	220	437	55	5.10	.58	31.17	4.91	6.06	7.25
2002	227	444	57	5.08	.54	32.20	5.45	6.19	7.37
2003	234	446	60	5.00	.54	33.54	6.26	6.61	7.66
2004	241	449	62	4.87	.42	34.11	7.55	7.16	7.43
2005	248	453	63	4.80	.39	34.48	8.45	7.66	7.66
2006	253	461	65	4.67	.35	35.15	9.13	7.44	7.91
2007	257	463	66	4.42	.33	35.99	9.62	7.49	8.34
2008	255	462	66	4.29	.32	35.19	9.98	7.39	8.95
2009	250	458	68	4.12	.34	35.58	10.26	8.10	9.39
2010	242	454	68	4.04	.34	35.31	10.42	7.82	9.88

Source. Data on jail populations are from Bureau of Justice Statistics, Jail Inmates, Jail Inmates at Midyear, and Prison and Jail Inmates at Midyear (1983–2011) (http://www.bjs.gov/index.cfm?ty=pbse&sid=38), and Bureau of Justice Statistics, Census of Jails (1983, 1988, 1993, 1999, 2002, 2005, 2006) (http://www.bjs.gov/index.cfm?ty=dcdetail&iid=254). Data on state prison populations and aggregate federal prison populations are, for 1970-1997, Bureau of Justice Statistics, Historical Statistics on Prisoners in State and Federal institutions, Yearend 1925-1986 (https://www.icpsr.umich.edu/icpsrweb/NACJD/studies/8912/version/1), and for 1998-present, Bureau of Justice Statistics, National Prisoner Statistics, 1970-2010 (https://www.icpsr.umich.edu/icpsrweb/NACJD/studies/34540/version/1), and Langan et al. (1988). Data on federal prison populations by offense are from the Compendium of Federal Justice Statistics series (Bureau of Justice Statistics 1989–2010), adjusted to align with the aggregate NPS data. Population data for generating incarceration rates are from Census Bureau historical population estimates.

8. CONCLUSION

Over the past 40 years, state legislatures and the federal government have passed numerous laws and changed many regulations that affect the punishments that criminal offenders receive, and often these changes were intended to make punishments both more determinate and more severe. Over this same period, incarceration rates increased by more than a factor of 4 before declining slightly during the Great Recession as some states struggled to fund their expanded prison systems (see Pew Center on the States 2010).

Yet the literature contains conflicting claims concerning the contribution of more punitive sentencing to the growth of prison populations over this period. Here we focus on the period 1985–2005, and we use new methods and data-cleaning procedures to provide evidence that more punitive sentencing policies drove the majority of growth in prison populations. Our results cover only seven states, but crime and arrest trends in these states follow national trends, and we conjecture that our results are broadly applicable to the nation as a whole.

We make several important methodological choices. First, we clean and audit the data we use to characterize flows in and out of prisons. Second, we do not focus on the distribution of time served given admission to prison as a summary measure of sentencing policies. Instead, we focus on the distribution of time served given arrest. Third, we analyze these distributions separately by offense category and build simulation models that track the dynamics of various components of prison populations separately over time.

Taken together, these approaches allow us to demonstrate that no single set of statutes that affect particular groups of offenders is responsible for the prison boom, at least in our seven NCRP states. On the contrary, we document a broad shift toward more punitive treatment for offenders in every major crime category. In state courts, both black and white offenders faced similar changes in the overall severity of the punishments they received. However, because arrest rates for blacks are much higher than arrest rates for whites, this secular shift to more punitive sentencing policies was particularly devastating for black communities. In federal courts, the shift toward more punitive sentencing for drug crimes affected black offenders more than white offenders, and drug offenders account for a significant portion of growth in the federal prison population in recent decades.

Since the early 1980s, the real value of labor market opportunities for less-skilled men has declined significantly, and decades of education reform have done little to narrow the skill gaps among youth from different racial or economic backgrounds. Yet we know that these secular inequality trends did not drive the growth of prison populations. Only a small portion of the recent growth in prison populations can be attributed to higher crime and arrest rates. Changes in policies that govern the treatment of arrested offenders account for the majority of prison growth.

It may be decades before we understand the long-term consequences of the policy choices that created the prison boom. However, it is possible that the wide-ranging shift to more severe sentencing policies contributed to growing inequality by greatly increasing the stock of adult men whose time in prison cost them current and future opportunities to gain work experience in legal employment (for reviews of this literature, see Holzer 2009; Neal and Rick 2014; Raphael and Stoll 2013). If this is the case, future research must also explore how these losses of employment and income affected the families, and especially the children, of these men.

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