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## The Prison Boom & The Lack of Black Progress After Smith & Welch

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**Working Paper No. 248**

**“The Prison Boom & the Lack of Black Progress after  
Smith & Welch”**

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**AND**

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**THE PRISON BOOM & THE LACK OF BLACK PROGRESS  
AFTER SMITH & WELCH**

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*PRELIMINARY AND INCOMPLETE. PLEASE DO NOT CITE.*

ABSTRACT. More than two decades ago, Smith and Welch (1989) used the 1940 through 1980 census files to document important relative black progress, but this progress did not continue, at least among men. Since 1980, prison populations have grown tremendously in the United States. Here, we show that, at least for the eight states that provide fairly reliable National Corrections Reporting Program (NCRP) data, this growth was driven by a move toward more punitive treatment of those arrested in each major crime category. These changes have had a much larger impact on black communities than white because arrest rates have historically been much greater for blacks than whites. Further, the growth of incarceration rates among black men in recent decades combined with the sharp drop in black employment rates during the Great Recession have left most black men in a position relative to white men that is really no better than the position they occupied only a few years after the Civil Rights Act of 1965. In addition, significant numbers of men are no longer “aging out” of crime but experience high arrest rates well into their 40s.

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## INTRODUCTION

More than two decades ago, Smith and Welch (1989) published their seminal work on the evolution of black-white inequality during the 20th century. They were among the first researchers to gain access to electronic versions of census long form data, and using census data from 1940 through 1980 as well as Current Population Surveys (CPS) data from the 1980s, they traced the evolution of black-white differences in education, employment, and earnings for cohorts born from the late 19th century into the latter half of the 20th century.

They titled their paper “Black Economic Progress After Myrdal,” and they argued that, although the 1940 Census data supported Myrdal’s (1944) bleak assessment of economic life among American blacks during the first four decades of the 20th century, subsequent census data recorded noteworthy gains for blacks relative to whites in not only education levels and measures of occupational prestige but also in levels of earnings and family income.

For example, Smith and Welch (1989) report that the black-white gap in completed years of schooling among males ages 26-35 fell from 3.9 years of schooling in 1940 to 1.4 years in 1980. This decline in the black-white schooling gap reflects an enormous increase in black educational attainment. Over the 1940-1980 period, average schooling levels among black men increased from just below 6 years of schooling to more than 12 years, while schooling levels among white men increased from just under 10 years to 13.5 years. Given the strength of the statistical relationship between earnings and education, it is not surprising that they also documented enormous gains in the earnings of black men relative to white men.

Most labor economists remember that Smith and Welch (1989) documented the dramatic economic and social progress of blacks during much of the 20th century, but in the final paragraphs of their paper, they also offered this cautionary note, “... there are also reasons for concern about the future, especially for the still large black underclass.”<sup>1</sup> Smith and Welch (1989) argued that problems with the quality of schools that serve black children, especially in Northern cities, were a barrier to further progress.

Wilson (1987) also drew attention to the struggles of black youth in the inner-city, and Neal (2006) used data from the National Assessment of Educational Progress (NAEP) to show that reading and math scores for black students in urban areas did fall during the 1980s relative to scores for other youth.<sup>2</sup> Further, although aggregate black-white gaps in achievement and attainment continued to shrink for much of the 1980s, there is considerable evidence that overall black-white skill convergence had already stopped by the time Smith and Welch (1989) published their findings. Today, black-white gaps in math and reading scores among youth and black-white gaps in educational attainment among young adults are quite similar to the corresponding gaps observed in the late 1980s.<sup>3</sup> Further, there are indications

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<sup>1</sup>Smith and Welch (1989), pp. 561

<sup>2</sup>Flanagan and Grissmer (2002) show that by the mid 1990s, test scores for black children in Northern cities were lower than test scores for black children in the rural Southeast. This pattern is striking because migration to Northern cities was an important engine of black progress for much of the 20th century.

<sup>3</sup>The National Assessment of Education Progress: Long Term Trend Studies track math and reading performance for students ages 9, 13, and 17 using exams that are designed to be comparable over time. Among nine year olds in 2008, the black-white gap in reading scores was smaller than the corresponding gap in 1990. However, other measured gaps in reading and math for 2008

that, at least among men, the labor markets prospects enjoyed by black adults are worse now, both in real terms and relative to whites, than they were in 1990.

Below, we explore several different methods for measuring recent trends in black-white inequality in labor market outcomes. While different estimators and different imputation procedures produce varied results, three facts dominate these calculations. First, employment rates for men have fallen in recent decades. Second, this decline in male employment rates has been coincident with an unprecedented increase in incarceration rates. Third, since 1980, these declines in employment and increases in incarceration have been much more dramatic among black men than white men.

Tables 1 through 4 present employment rates and institutionalization rates for adult men in the United States. The data come from the Integrated Public Use Micro data Series (IPUMS) and cover six census years, 1960-2010.<sup>4</sup> Each diagonal row in these tables presents data from one census year. Each column presents data for one age group over different census years. Each horizontal row presents data from different census years for a five-year birth cohort. Appendix Tables A1-A4 present parallel results for women. Given the numerous data sets we employ in this paper and the numerous different coding schemes that we encounter regarding Hispanic origin, we were not able to come up with a clean strategy for identifying Hispanic populations consistently over time. Therefore, we typically present results for Whites, Blacks, or all races combined.<sup>5</sup>

Institutionalization involves more than incarceration. Recent census files also include residents of mental hospitals and nursing homes in the institutionalized population. However, few persons under age 50 are in nursing homes, and the populations housed in mental hospitals have declined greatly over the past three decades. So, the trends in institutionalization rates presented in these tables likely understate recent trends in incarceration rates, and the most recent institutionalization rates are reasonable proxies for the corresponding incarceration rates.<sup>6</sup>

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were comparable to or greater than the corresponding gaps in 1990. See Rampney, Dion, and Donahue (2009). See Neal (2006) for an extensive treatment of trends in black-white achievement gaps.

<sup>4</sup>The 1960-2000 data come from the Census Long Form. The 2010 data come from the American Community Survey.

<sup>5</sup>Some states and some data sets treat Hispanic origin as a different variable than race, while others treat Hispanics simply as a separate race category. In the latter case, it is not possible to distinguish white and black Hispanics. To make populations as comparable as possible across different states and data sets, we decided to exclude persons that report to be of Hispanic origin when we present results for blacks and whites separately.

<sup>6</sup>Before 1980, the Census Bureau produced more detailed codes that allow researchers to identify persons living in correctional institutions, but starting in 1990, the codes used in the census and American Community Survey (ACS) files simply identify persons as living in some type of institutional quarters. This broader definition of the institutionalized population includes those who reside in correctional institutions, facilities for the mentally ill, or facilities for the elderly and handicapped. For the sake of consistency, we use the post-1980 classification for all years. This means that the institutionalization rates in Tables 1 and 2 are larger than the corresponding incarceration rates for every year and demographic cell. Nonetheless, among prime age men, the fraction of institutionalized persons living in penal institutions has always been high and has increased since 1980 because rates of institutionalization in mental health facilities have fallen. See Figure 1 in Harcourt (2006)). In 1980, roughly one fourth of the persons who were either in prison or mental hospitals were mental patients. This figure was less than 10% by 2000.

Tables 1 and 2 present employment rates and rates of institutionalization for black and white men respectively. The employment rates presented in these tables are based on respondent activities during the survey weeks in question. Note that, for both black and white men, employment rates have fallen since 1970 within each age group. While the declines in the ages 20-24 group may, in part, reflect higher levels of college and graduate school attendance, the declines among men at older ages strongly suggest weaker attachment to market work. In contrast, the Appendix tables indicate that both black and white women became more involved in market work over the same time period.

While Tables 1 and 2 document employment declines among both black and white men, the declines are much more pronounced for blacks, e.g. the decline in employment rates among white men ages 25-29 was roughly 11 percentage points between 1970 and 2010 while the comparable change among black males was over twice as large. By 2010, more than one third of black males ages 25-49 were either unemployed or out of the labor force.

The employment patterns in these tables have received less attention than they deserve in the economics literature on race and inequality. Many analyses of trends in employment and income rely on data from the CPS,<sup>7</sup> but the CPS sampling frame does not include institutionalized persons, and the institutionalization rates in these tables show noteworthy increases among both black and white men at all ages. Further, current levels of institutionalization are particularly noteworthy among black men. On any given day in 2010, almost one in ten black men ages 20-39 were institutionalized, and rates of institutionalization were actually slightly higher among black men in 2000. Further, because turnover among prison populations is quite high, these results suggest that far more than ten percent of prime age black men will serve some time in prison or jail during a given calendar year.

Table 3 demonstrates that, while black men experience high rates of institutionalization overall, rates among less-educated black men have reached levels that were unthinkable prior to 1980. By 2010, the institutionalization rate among black, male high school dropouts ages 25-29 was almost one third and the employment rate for this group was less than one fourth. Institutionalization rates among black dropouts ages 39 or less actually fell slightly between 2000 and 2010, but rates among those over 39 continued to rise. Thus, among black men in 2010, more than 1 in 6 high school dropouts in their forties were institutionalized on any given day. These numbers do not simply reflect a significant number of men who are serving extremely long sentences. Admission rates are now noteworthy for this age group, and overall the data are consistent with the hypothesis that a non-trivial fraction of less-educated, black men are now engaged in active criminal careers well into middle age.

Table 4 presents results for white men with less than a high school education. Employment rates have fallen steadily among these men since 1970 across all age groups. Overall, rates of institutionalization among these men remained roughly

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<sup>7</sup>In a recent Handbook chapter, Acemoglu and Autor (2011) assess the literature on links between trends in technology and trends in earnings inequality. They discuss the rates of growth of employment in jobs classified by skill-type, but in keeping with much of the related literature, they devote scant attention to the stunning growth in the numbers of low-skilled persons who have no formal sector job or how many of these non-employed persons are in prison. Neal (2006), Western (2006), and Pettit (2012) discuss how the growing prison population affects the sampling frames used to create official labor market statistics.

constant during the 1970s but have risen steadily since 1980. As in Table 3, overall institutionalization rates increased only slightly during the 2000-2010 period.

In sum, among less educated men, rates of institutionalization have increased for both blacks and whites at all ages. Further, among both black and white dropouts, we see employment levels among men over 40 that are strikingly low by historical standards. Nonetheless, because the black samples contain proportionally more dropouts and even worse trends in outcomes among dropouts, these trends contribute to growing inequality between black and white men. During roughly the past three decades, incarceration rates among both black and white men in different age groups have increased by factors of two to three, but these changes have had a much larger impact on black communities. Because black male incarceration rates were much higher than corresponding white rates before the prison boom ever began, the impact on blacks communities of a more than two fold increase in incarceration rates has been dramatic.

While it is possible that trends in incarceration, trends in black-white inequality and trends in overall income inequality could have all been driven by a common set of economic forces, there is also the possibility that policy changes created the prison boom. Thus, we seek more precise answers to two sets of questions

- (1) How important are changes in criminal justice policies as determinants of the dramatic rise in US incarceration rates during the past three decades and have these changes had differential impacts on black men as opposed to white men?
- (2) How should proper treatment of incarceration affect our assessment of economic and social inequality in the US both overall and between blacks and whites, especially black and white men?

We conclude that, over the past three decades, a broad menu of changes in sentencing rules and parole policies created a much more punitive criminal justice system. In the 2000s, arrested offenders received much more severe punishment than their counterparts in the 1980s, and this is true for both black and white offenders regardless of the offenses that led to their arrests.

There is some evidence that the increased use of long sentences as punishment for violent crimes may have been even more pronounced among blacks, and as others have noted, the War on Drugs was not conducted in a color-blind manner. However, these factors are minor parts of our story. The key point is that, since black arrest rates are now and have always been much higher than white arrest rates, the move to much more punitive treatment for all arrested offenders has had much larger effect on black communities than white ones.

Because CPS data do not contain information about persons who live in prisons or other institutions, the prison boom has affected the degree to which persons who have low earnings prospects are counted in many official measures of employment and earnings, and this is especially true among black men. Here, we explore several imputation strategies that allow us to measure trends in income inequality between blacks and whites while accounting for race-specific trends in non-employment and institutionalization, and our results are somewhat shocking. Although black-white gaps in potential earnings appear to have shrank during the 1990s, these gaps grew during both the 1980s and the 2000s. The Great Recession period of 2008-2010 was quite bleak for black men, and current levels of labor market inequality

between black and white prime-age men are likely not materially different than those observed before the Civil Rights Act of 1965.

Finally, we present suggestive evidence that the criminal justice policies pursued over the past three decades may have important impacts on inequality in future generations. For the first time in United States history, arrest rates and admission rates into prison are significant among men in their forties, especially among black men. Men born in the 1970s who became involved in crime as young men appear to be the first generation that did not “age out” of crime, and this pattern may bode ill for the partners of these men and any children they have fathered.

In the following sections, we review the legal history of changes in criminal justice practice over the past three decades or more. We then review the empirical literature on links between changes in criminal justice policies and prison growth. Next, we present original work with data from the National Corrections Reporting Program (NCRP) that clarifies the role of policy changes as engines of growth in state prison populations and examine the extent to which certain changes may have disproportionately affected blacks. After describing what is known about growth in the populations of jails and federal prisons, we discuss trends in black-white labor market inequality over recent decades and how changes in incarceration rates complicate the measurement of these trends. We conclude with thoughts about what scholars should expect in the future given different assumptions about future trends in criminal justice policy.

## 1. RECENT HISTORY OF TRENDS IN CRIMINAL JUSTICE POLICY

For most of the twentieth 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 the sentences for those who enter prison. Further, holding constant the sentences that judges imposed, parole boards enjoyed considerable control over the time that specific inmates actually served.

The indeterminate sentencing model offered judges and parole boards the freedom to customize punishments to specific offenders rather than simply match specific punishments to specific offenses. Judges and parole officials were free to consider prospects for rehabilitation, the provision of incentives for good behavior and self-improvement, as well as expected impacts on public safety when making decisions that determined the punishments received by various offenders.

However, during the 1970s, indeterminacy came under attack from activists of many different political persuasions. Some on the left charged that indeterminacy gave judges and parole officials too much freedom to indulge their own racial prejudices when determining sanctions. Others on the right 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.<sup>8</sup>

Thus, in the late 1970s, legal changes began to move justice and corrections policies toward a determinate sentencing model. Table 5 summarizes a number of changes that different states made as they moved to more determinate sentencing

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<sup>8</sup>See Raphael and Stoll (2013) as well as Stemen and Rengifo (2011) for discussions of this literature. See Dansky (2008) for references specific to the debate in California.



practices.<sup>9</sup> California, Illinois, Indiana, and Maine eliminated or severely curtailed discretionary releases by parole boards in the late 1970s, 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, 17 other states have adopted commissions that vary greatly in terms of their missions and their powers.

In 1994, the Federal government passed the Violent Crime Control and Law Enforcement Act. 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. The rules determining whether or not specific states qualified for these grants were complex, but the nominal goal of the program was to induce states to adopt policies that require most prisoners, especially violent offenders, to serve 85% of their sentences. Delaware, Minnesota, Tennessee, Utah, and Washington adopted TIS laws prior to 1994, and by 1998, 22 additional states adopted policies that secured their eligibility for TIS grant money. In addition, Table 5 shows that the vast majority of states now have some TIS law on the books that limits discretionary release by parole boards.<sup>10</sup>

It is easy to see how each one of the policies described in Table 5 could be employed not only to make criminal justice systems more determinate but also to make them more punitive. Nonetheless, empirical work on the effects of these policies has not produced a consistent set of results that link prison growth to the adoption of specific policies, and this is not surprising given that different states often implement nominally similar laws in quite different ways.

Frase (2005) points out that some states use sentencing commissions as devices for matching corrections populations and corrections budgets.<sup>11</sup> In these states, restrictions on judicial latitude in sentencing and parole board discretion over release appear to function as vehicles for keeping prison populations within ranges that are in line with budgets allocated for corrections. However, some other states have used similar determinate sentencing practices as vehicles for making criminal justice policy more punitive. Nicholson Crotty (2004) conclude, based on a panel data model of state commitment and incarceration rates between 1975 and 1988, that mandatory sentencing guidelines are negatively associated with state-level growth in commitment rates when such guidelines are linked to corrections budgets but

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<sup>9</sup>There are many factors that contribute to determinacy. Sentencing guidelines, narrow presumptive sentencing ranges, mandatory minimum laws and other policies restrict the discretion that judges may exercise at sentencing, while restrictions of 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 punitiveness in many states.

<sup>10</sup>See Ditton and Wilson (1999) for a Bureau of Justice Statistics report on the implementation of Truth in Sentencing laws during the 1990s.

<sup>11</sup>See Reitz (2006) for more evidence that the existence of sentencing commissions and restrictions on parole are not necessarily drivers of prison growth.

positively associated with commitment rates and incarceration rates when they are developed separately from corrections budgets.

Moreover, many states have created more determinate systems without ever establishing sentencing commissions by simply legislating presumptive sentences for specific crimes. Stemen, Rengifo, and Wilson (2006) reports that “between 1975 and 2002, every state adopted some form of mandatory sentencing,”<sup>12</sup> but the number of crimes covered by such statutes and the harshness of these minimum sentences varies greatly among states and over time within states. California began legislating presumptive sentences in 1976 when they abolished discretionary parole, and the initial determinate sentencing law passed by the legislature would have likely had little impact on prison growth. However, the state legislature approved 43 amendments to the legislation during only the first year under determinate sentencing, and each of these 43 amendments plus many amendments in subsequent years increased the severity of sanctions.<sup>13</sup>

During the past two decades, the vast 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. Although California was not the first state to pass a law mandating enhanced penalties for repeat offenders, the California statute commonly known as “Three Strikes and You’re Out” has received considerable attention from scholars and journalists. The law was passed by both the legislature and voter initiative in 1994 following two well-publicized murders that were committed by offenders who had recently been released from prison. Many states have followed California’s lead since 1994, but few states have adopted laws that are as severe as the California “Three Strikes” policy. Auerhahn (2002) claims that the California law stands out because it mandates a doubling of the presumptive sentence following only the second conviction for a covered offense, i.e. “strike,” and because the “strike zone” that defines covered offenses is more inclusive than it is in other states.

In sum, states have pursued determinacy using many different approaches over the past three decades or more. However, the complexity of criminal justice systems at the state level creates almost insurmountable problems for scholars who conduct empirical investigations that seek to establish concrete links between prison growth and specific features of state-level criminal justice systems that promote determinacy. The presence of a specific “type” of law or system in a given state tells researchers little about the level of punitiveness in that state. State legislators who wish to adopt more punitive policies have so many policy levers at their disposal that scholars would find it difficult to construct a comprehensive index of punitiveness at the state level for even a single point in time, much less a set of indices that track the evolution of punitiveness within each state over time. In fact, we have found no studies that have even attempted these quixotic measurement tasks.<sup>14</sup>

Nonetheless, we argue below that researchers can build a strong case that changes in criminal justice policy must be a key reason that prison populations are so large

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<sup>12</sup>See p. 118.

<sup>13</sup>See BJA (1996), p. 16.

<sup>14</sup>Note that in order to create such an index, researchers would have to understand how a specific set of policies translated into a set of distributions of time-served that condition on conviction for specific offenses. Then, researchers would need to conduct simulations that generated sample distributions of simulated time-served for various populations of convicted offenders, and then collapse the information contained in these simulated distributions into an index of punitiveness.

now relative to those observed in the 1970s. The insight here is that during the late 1990s and the 2000s, crime rates and arrest rates for most serious crimes remained flat or fell, but prison populations continued to grow until at least 2008 and have fallen only slightly since. The numbers of people who entered prison during the late 1990s and served prison terms of short, medium, and long durations are typically more than double the numbers that one would expect based on the outcomes we observe for the cohorts arrested as late as the mid 1980s. The two decades of reforms that began in the 1970s as attempts to enhance determinacy created a system by the mid 1990s that was not only more determinate but also more punitive.

Before moving on to the empirical literature on the links between criminal justice policy and prison growth, it is important to note that overall prison populations have fallen slightly since 2009, and this development may also reflect recent policy changes.

In 2004 and 2005, two United States Supreme Court decisions made it more difficult for states and the federal government to build enhancements into determinate sentencing systems. In *Blakely v. Washington* (2004), the court held that any factors that judges rely upon to determine the sentence for a specific defendant must be proven beyond a reasonable doubt to a jury. Thus, if guidelines developed by a sentencing commission or provisions contained in statutes passed by a legislature dictate enhancements to base level sentences based on specific aggravating circumstances, e.g. “deliberate cruelty,” judges may not apply these enhancements unless the aggravating circumstances have been submitted and proved to a jury. In *United States v. Booker* (2005), the court held that the same rules apply to federal judges.

The impact of these decisions has been to weaken the importance of guidelines developed by sentencing commissions or rules contained in statutes that attempt to spell out more punitive sentences for various crimes when certain aggravating circumstances are present. Unless prosecutors prove the existence of these circumstances while proving guilt of the crime in question, such enhancements impose no constraints on judges. Judges can ignore the aggravating circumstance when imposing sentences, and their decisions are not subject to review.

Further, a more recent Supreme Court decision may have important implications for the costs of incarcerating large state prison populations. In *Brown v. Plata* (2011), the United States Supreme Court upheld the decision of a three-judge court that had been appointed by the Chief Judge of the United States Court of Appeals for the Ninth Circuit to deal with a class action lawsuit that alleged the state of California had violated the constitutional rights of prisoners by operating prisons that were so overcrowded as to deny them adequate health care. This three-judge court had ruled in favor of the plaintiffs and mandated the release of enough prisoners to bring the California prison population down to 137.5 percent of the design capacity for the system, which implied early releases for more than 35,000 prisoners.

California has long been an outlier in terms of prison over-crowding, so the recent reduction in California prison populations that followed this decision has been noteworthy. However, it is not clear what the immediate impact of this decision will be on prison populations in other states since it is not clear how many states are currently beyond the 137.5 percent threshold or whether these states have found some different but satisfactory way to address prisoner health and safety in prisons

with populations that exceed the 137.5 percent threshold. Still, for many states, this Supreme Court decision clearly raises the expected costs of policies that impose punitive sentences on offenders, and it is reasonable to expect these additional costs to influence future policies.

The final *Brown v. Plata* decision came after almost a decade of trials and hearings in lower courts, so the outcome did not come as a complete shock to officials in California or other states. Further, the budget problems that have plagued many states for much of the past five years have raised public awareness of the costs associated with running large prison systems. Thus, some states are exploring ways to punish non-violent offenders that do not involve prison.<sup>15</sup>

Still, not only incarceration rates but also admission rates into prison remain more than twice as high as they were in the 1970s, and both crime rates and arrest rates have been at historic lows for almost a decade. Our goal in the next two sections is to shed some light on why prison populations grew so rapidly from 1980 well into the 2000s as well as why they have remained roughly constant at such high levels for the better part of a decade.

## 2. EMPIRICAL LITERATURE ON THE EFFECTS OF SPECIFIC LAWS

The literature contains a number of papers that 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 recent and prototypical contribution to this literature. The authors attempt to explain variation in measures of admission rates, incarcerations rates, and expected time served among states and over time by regressing these outcomes on six different measures of policy plus a set of additional control variables that often include state and year fixed effects. They employ data from 1973 through 1998. Their policy variables are a set of indicators for the presence of the following policies: (1) voluntary sentencing guidelines (2) presumptive sentencing guidelines (3) habitual offender laws (4) abolition of discretionary release by parole boards (5) requirements that sentencing guidelines consider prison capacity (6) truth-in-sentencing laws.

On the whole, the results imply few statistically significant effects for these policy variables and even fewer that are of the expected sign. The authors conclude that sentencing policies associated with determinacy did not contribute much to prison growth over the period 1973 to 1998. However, it is hard to know exactly what the authors mean by this conclusion. For example, they note in their conclusion that the details of habitual offender laws, which are often known as “three-strikes” laws, vary greatly among states. While California, Georgia, and Florida handed down numerous enhanced sentences to many offenders under these laws, a significant number of states defined their “strike zones” so narrowly that the statutes are rarely used.<sup>16</sup> Thus, it is not clear what we learn from the fact that the adoption of a some habitual offender statute is not an important predictor of within-state growth in admission rates or prison populations.

<sup>15</sup>The Pew Center on the States recently released reports, Pew (2010) and Pew (2012), that, among other things, describe steps that several states are taking to reduce the punitiveness of their criminal justice systems and thereby reduce the size and cost of their state prison systems.

<sup>16</sup>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 also to predict the continued growth in the population during much of the 2000s.

In addition, it is not obvious what we should expect from such models even if the measures of sentencing policy were more precise. Imagine a homogenous change in sentencing policy adopted in a number of different states at varying times and assume that this new policy took a simple form. Assume that it simply added three years to every sentence of at least two years that would have been given under previous law. This change would have no effect on admissions rates in any period because it would only apply to those who were going to serve at least two years anyway. However, it would have important effects on steady-state incarceration rates and expected time-served among admitted prisoners.

Yet, it is not clear that regression models like those in Zhang, Maxwell, and Vaughn (2009) would accurately measure these effects on incarceration rates and time-served. Recall that these regressions project admission rates, incarceration rates, and proxies for time-served on contemporaneous measures of policy, and such a policy change would not have any effect on incarceration rates for at least two years. Further, it would take many years for the full effects of such a change to unfold. Finally, because the authors are using ratios of prison populations to admissions flows to estimate expected time-served, the measures of expected time-served in states affected by such a law would be biased down for years following adoption, and this bias would create a correlation between any indicator variable for the existence of such a law in a given state in a given year and the residual associated with the observation for that state-year combination.

Stemen, Rengifo, and Wilson (2006) and Stemen and Rengifo (2011) follow a similar research strategy, but they focus only on incarceration rates as outcomes. Taken as a whole, the results in these papers are similar to those in Zhang, Maxwell, and Vaughn (2009). Indicator variables for adoption of these specific policies are not strong predictors of future prison growth within states. Still, it is worth noting that all three of these studies found that states that abolished discretionary parole release experienced slower than average growth in prison populations.<sup>17</sup>

The major limitation of this literature is that these regressions do not directly address a precise counterfactual that informs policy. In the language of the program evaluation literature, these regressions are trying to identify treatment effects associated with specific policies, but some indicator variables for treatment do not capture the implementation of a homogenous policy, and therefore, the treatments are not precisely defined. Further, because other policies with similar effects but different names are often implemented in non-treated states, the implied control groups in these regressions are not valid control groups.

We have already noted that, since 1975, all states have adopted more mandatory minimum statutes, and it seems reasonable to conjecture that many also tightened standards for parole revocation, applied more public scrutiny to parole board decisions, etc. Thus, the results of these studies tell us little about whether or not aggregate trends in the punitiveness of criminal justice policies are responsible for the dramatic growth in prison populations that we have witnessed over the past

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<sup>17</sup>We noted above that both 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 corrections expenditures. Marvell (1995) reached a similar conclusion using earlier data.

three decades or more. The studies simply tell us that states associated with a specific set of readily identifiable policies do not typically stand out as having above average rates of prison growth.

To illustrate this point further, consider a unique aspect of the Stemen, Rengifo, and Wilson (2006) study. In a few of their models, these authors tried to code up variables that captured some of the variation over time and among states in mandatory sentencing practices. Given the enormous variation in mandatory sentencing rules among states at a point in time and within specific states over time, it is hard to imagine how researchers could create an accurate index that captured both geographic and temporal variation in mandatory minimum punitiveness, and the authors did not attempt to create such an index. Rather, they created counts of the different types of mandatory minimum statutes in several different categories: (i) weapons use (ii) serious harm to victims (iii) crimes committed under supervision or in custody and (iv) hate crimes.

With the exception of (ii), increases in the numbers of each of these statutes on the books are significantly correlated with growth in prison populations, and on average, when states add 10 new mandatory minimum statutes, the expected incarceration rate increases by 23 (out of 100,000). However, the authors write, “we do not believe that the mandatory sentencing laws considered here are necessarily directly contributing to increases in incarceration rates; rather, they are used here as proxies for states’ general approaches to mandatory sentencing laws and, in this sense, indicate the states’ general use of mandatory sentencing policies.”

The use of the term “proxies” in the quote above is important. The authors recognize that differences among states and over time within states in count variables for particular types of statutes do not map cleanly into precise policy counterfactuals. However, they are conjecturing that in times and places where legislatures are devoting considerable energy to passing these types of statutes, state officials are also engaged in other unmeasured activities that enhance the punitiveness of criminal justice policies. This is a reasonable conjecture, but it also highlights the limits of panel regression methods as tools for discovering whether or not policy changes drove prison growth. It is quixotic to hope that scholars can create complete and precise measures of all the relevant policy changes in different states at different time periods. However, when scholars code different policies as the same policy or omit controls for relevant but unmeasured policies that are changing contemporaneously with measured policies, they should not expect panel regressions to produce useful or even interpretable results.

### 3. DECOMPOSITION METHODS

Given the limitations of these regression methods, many scholars have adopted a more indirect approach. A large literature employs statistical decompositions derived from the observation that, in a steady-state, the fraction of the population incarcerated,  $i$ , can be expressed as:

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

where

- $c$  = the fraction of criminals in the population
- $\alpha$  = the probability of arrest given engagement in crime
- $\gamma$  = the probability of conviction given arrest
- $\delta$  = the probability of admission given conviction
- $\bar{s}$  = the expected time served given admission

This equation ignores parole and parole revocations.<sup>18</sup> So, new entrants to prison in this setting are always newly convicted offenders. One can derive this equation from many different models of criminal, police, and court behaviors, but the most straightforward approach is to assume that a fixed fraction of an infinitely-lived population exhibit complete persistence in crime, i.e. those who choose crime as a career engage in crime whenever they are not incarcerated. This framework also rests on the assumption that no innocent persons are ever arrested, convicted, or imprisoned. Further, we are implicitly assuming 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.<sup>19</sup>

The first two terms in the decomposition above remind us that even if prosecutors and judges do not change their behavior, changes in the prevalence of criminality or the effectiveness of policing may generate changes in prison populations, and it is worth considering these two forces before moving on to the role of criminal justice policy as a driver of prison growth.

A large literature argues that technical change and globalization have harmed the labor market opportunities of unskilled men, and a smaller literature suggests that less skilled men are more likely to choose crime as an occupation when their prospects for legal employment are diminished.<sup>20</sup> The fact that employment rates have fallen most rapidly over the past 30 years for the groups of men who have experienced the most rapid rise in incarceration rates suggests that we should entertain the hypothesis that prison populations grew because opportunities for legal employment among the less-skilled shrank.

The first six columns of Table 6 describe trends in crimes and arrests nationally over the period 1985-2011. The remaining columns describe trends in state prison admissions as well as population trends for jails, state prisons, and federal prisons. We conjecture that property crimes and drug crimes should be the categories most affected by trends in labor market opportunities because dealing in stolen goods and drugs are alternatives to legal employment. While we see increases in property crime during the late 1980s, reported property crimes have fallen, in almost every year, since 1991. In contrast, state prison admissions continued growing until 2007 and state prison populations grew until 2008 before leveling off and then experiencing a small decline in 2011.

Further, we see no evidence of changes in police behavior that generated large increases in the number of persons arrested for property crime holding crime levels constant. Although there are year to year fluctuations in the ratio of arrests to

<sup>18</sup>It is straightforward but cumbersome to add parole. See Raphael and Stoll (2013) for a steady-state analysis that includes parole and parole revocations.

<sup>19</sup>Alternatively, some researchers modify this formula to incorporate different rates and different average sentences for different types of crime.

<sup>20</sup>See Grogger (1998) and Gould, Weinberg, and Mustard (2002).

reported crimes, arrests for property crime follow the same basic trends as reported property crime.

Arrests for drug crime do increase through 2006, and we cannot rule out the possibility that, over time, declining (legal) labor market opportunities for less skilled men increased the number of men who tried to make a living by producing, transporting, or selling illegal drugs. On the other hand, these arrest trends alone can account for only a small portion of the prison growth we observe over this period.

Below, we conduct numerous simulations of prison growth over the period 1985 to 2005 using a sample of eight states where we have detailed information about admissions and releases from state prisons and their parole systems in each year. In these states, we see the same trends in arrests for drug offenses that are present in Table 6 for the nation as a whole. However, our simulations results indicate that, between 1985 and 2005, these arrest trends account for far less than half of the growth in the stock of persons imprisoned for drug crimes and less than 10 percent of the growth in total prison populations.<sup>21</sup>In addition, trends in drug arrests are more difficult to evaluate because there are no series of reports of drug crimes. It is possible that the increases in drug arrests we observe do not reflect changes in levels of criminal activity but changes in police behavior. This possibility is noteworthy because available evidence on drug use suggests that rates of illegal drug use likely fell after 1995 even though drug-related arrest rates continued to rise.<sup>22</sup>

Prison growth in the US cannot be explained simply as the result of more persons entering illegal work in response to changes in the wage structure that made less-skilled legal employment less rewarding or more difficult to secure. Drug arrests are up, but this increase is trivial when placed against the overall growth in prison populations, and these arrest trends are not even definitive proof that the number of person using or dealing illegal drugs actually increased.

For much of the past two decades, crimes and arrests have been falling in all non-drug crime categories. Further, even in the 1980s when arrests were rising, they were not rising fast enough to account for the concurrent growth in prison admissions and prison populations. Thus, recent prison growth in the US must be driven by changes in policies and procedures that affect the punishments that offenders receive after being arrested.

Because data on the probability of conviction given arrest,  $\gamma$ , are so scarce,<sup>23</sup> scholars often implicitly assume that  $\gamma$  does not vary over time and focus on admission rates given conviction ( $\delta$ ) and expected time served given conviction ( $\bar{s}$ ), as empirical proxies for the punitiveness of criminal justice systems. When movements in these two variables account for most of the movements in  $i$ , scholars typically conclude that changes in the criminal justice system rather than changes in criminal behavior are the key drivers of changes in prison populations.

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<sup>21</sup>Drug arrests did play an important role as a driver of federal prison growth, but the federal system is a small part of the overall prison system. Further, drug arrests per se are not noteworthy drivers of state prison-growth, even though it appears that changes in the sentencing of those arrested for drug charges played a modest role as a driver of state prison growth.

<sup>22</sup>See results for drug use among high school students over time in NIH (2012)

<sup>23</sup>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, e.g. charges dropped, acquittal, conviction, etc.



Further, most scholars that employ this method distinguish between movements in  $\delta$  and movements in  $\bar{s}$  when discussing the potential importance of changes in broad features of criminal justice systems. The following paragraph from Blumstein and Beck summarizes the prevailing paradigm concerning the interpretation of the results from these decompositions:

*“Growth in incarceration is typically a consequence of growth in one or more of the sequence of stages leading to an increased prison population. Those stages begin with commission of crime, which can then be followed by arrest, conviction, commitment, and time served in prison, including time served as a result of a parole violation. Changes in any or all of these stages can contribute to growth in the prison population. By examining each stage of the criminal justice process separately, the contribution of each to the total growth in incarceration can be isolated. Such a partition can also provide insights into the degree to which the growth is associated with greater criminality; greater police effectiveness in arresting offenders; some combination of increased effectiveness by prosecutors and punitiveness by judges in convicting arrested offenders and sending them to prison; increases in time served once sent to prison because of longer sentences (including mandatory minimum sentences), because the parole boards or other release policies are slower in offering release or because of more aggressive policies in recommitting parolees, either for a new offense or for a technical violation.”<sup>24</sup>*

Note how Blumstein and Beck (1999) discuss the last two terms,  $\delta$  and  $\bar{s}$ . Given that a person is convicted of a crime, the literature divides the punitiveness of criminal justice systems into two components: (i) how likely are judges to sentence convicted offenders to prison, and (ii) among those who receive a prison sentence, how long will they expect to stay in prison. Further, Blumstein and Beck (1999) implicitly assert that, if there are changes in policy that lead to more long prison spells, e.g. mandatory minimum sentence provisions, restrictions on release to parole, etc., then researchers can detect the effects of these changes in policy by measuring changes in time-served among those admitted to prison.<sup>25</sup>

This assertion is wrong, and it has created much confusion in this literature. Changes in average time-served or any other feature of the distribution time-served, *among those admitted to prison*, tell researchers nothing per se about changes in the severity of sentencing, changes in the severity of parole policies, or the contribution of such changes to prison growth. We explain this simple point in detail below, but first we review the empirical findings in the existing literature.

**3.1. Existing Literature.** All papers in this literature report dramatic growth in admissions rates during the 1980s and 1990s. However, the literature does not speak with one voice concerning trends in average or expected time served among admitted prisoners. Blumstein and Beck (1999), Western (2006), and Raphael and Stoll (2013) all report that time-served among admitted prisoners did increase over the 1980s and 1990s. Raphael and Stoll (2013) report large increases for those

<sup>24</sup>Blumstein and Beck (1999), pp. 26.

<sup>25</sup>Zhang, Maxwell, and Vaughn (2009) adopt the same approach. When reporting that changes in admission rates contribute more to prison growth than changes in expected time-served, they write, “Accordingly, sentencing reforms that resulted in increased prison admissions increased prison populations more than sentencing reforms that lengthened prison terms.” See p.197.

admitted following conviction for violent crimes but report small changes in time-served for inmates convicted of property crimes.

All three of these papers use data on stocks of prisoners and their relationship to admission or release levels to back out estimates of expected time-served among admitted prisoners. On the other hand, Pfaff (2011) examines individual admission and release records from the National Corrections Reporting Program and concludes that distributions of time-served prior to release remained roughly constant over the 1980s and 1990s. He examines different percentiles in state-specific time-to-release distributions for prisoners admitted in particular years, and he finds rather mixed results and no evidence of a clear trend toward longer prison spells conditional on admission.

We present our own NCRP results below for the period 1985 to 2011. Like many before us, we document large increases in admission rates. We document large increases in admissions that follow new convictions as well as large increases in admissions that result from parole revocations. The increases in admissions that follow new convictions are far greater than one would expect given the time-series patterns in arrest data combined with the sentencing and parole outcomes observed among those arrested in 1985. The increase in admissions due to parole revocations seems to largely reflect the fact that more punitive sentencing for newly convicted offenders created rapid prison growth that, in turn, created a large increase in the stock of persons on parole and thus at risk of having their parole revoked.

We show that distribution of time-served prior to first release shifted modestly to the right for persons entering prison following new convictions, and this shift was more noteworthy for those convicted of violent crimes, e.g. murder, rape, assault, and robbery. The length of prison spells that began due to parole revocations shrank over this period as the number of such prison admissions grew both in levels and as a fraction of all admissions.

Overall, the expected length of prison spells appears to be slightly shorter in recent years than in the 1980s, but we again stress that this result is irrelevant to assessments of the roles that sentencing and parole practices have played as engines of prison growth in recent decades. Although Blumstein and Beck (1999), Western (2006), and Pfaff (2011) all write as if changes in  $\bar{s}$  provide direct evidence concerning whether or not harsher sentencing and parole policies, e.g. elevated mandatory minimums, enhanced penalties for habitual violator, Truth in Sentencing laws, etc, are important drivers of prison growth, this is simply not true.<sup>26</sup>

Pfaff (2011) acknowledges that, if states made sentencing more harsh by dictating positive sentences for some offenders who previously received probation as well as longer sentences for offenders convicted of more serious crimes, then prison populations would grow even though average time-served among admitted prisoners could remain constant. However, Pfaff (2011) goes on to wrongly argue that, if various percentiles of the time-served distribution, e.g. 10th, 25th, 50th, 75th, and

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<sup>26</sup>In fact, Pfaff (2011) concludes that the wide-spread view that the increased use of long prison spells has been an important driver of prison growth is a “myth” and argues that changes in “admissions policies” drove prison growth. He argues that prisons grew because offenders were more likely to enter prison not because courts were more likely to hand down sentences that resulted in long prison spells. Raphael and Stoll (2013) do not explicitly draw the same direct links between these types of policies and properties of the distribution of time-served.

90, do not change over time, then policies that increase the use of long prison spells as punishments cannot be important drivers of prison growth.<sup>27</sup>

**3.2. The Wrong Denominator.** Pfaff’s analyses are off-point because time-served statistics based on the population of persons admitted to prison involve the wrong denominator. 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 terms of plea agreements, one can argue that researchers should take another step back and focus on the fraction of *arrested alleged offenders* who serve long sentences.<sup>28</sup>

**3.3. A Different Decomposition.** Instead of characterizing corrections policy as a choice of an admissions probability and an expected sentence length, one could characterize policy as a set of probability weights,  $\rho_s$ , where  $s = 0, 1, 2, \dots, S$  are the potential times that a convicted offender may serve, and  $\rho_s$  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.

Given this notation, it is straightforward to re-write our steady-state equation as

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

Now, consider a change in policy that involves uniformly harsher sentencing policies, i.e. let  $\rho' = [\rho'_0, \rho'_1, \dots, \rho'_S]$  describe the new sentencing regime, and let  $\rho'_s = k\rho_s \forall s > 0$  with  $k > 1$ . Further, let  $\rho'_0 = 1 - \sum_{s=1}^S \rho'_s$ . 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,  $i'$ , 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  $\rho'$  as it is under  $\rho$ .

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

<sup>27</sup>Langan (1991) makes a similar mistake when analyzing data from an earlier period. He argues that mandatory sentencing laws did not drive prison growth, and cites as evidence that “Prison 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 impacts of changes in sentencing policy.

<sup>28</sup>Below, we discuss Rehavi and Starr (2012) who demonstrate that, among those arrested for the same offenses, federal prosecutors have considerable discretion over how to charge offenders, and these charge decisions constrain the discretion of judges in sentencing.

prisoners. Further, it is easy to construct scenarios in which a move to more punitive sentencing increases prison populations while lowering the average time-served among admitted prisoners.

The framework we describe can easily be extended to include crime-specific sentencing weights. Let  $j = 1, 2, \dots, J$  denote an exhaustive and mutually exclusive list of crime categories. We can define  $\rho_{sj}$  as the baseline probability of receiving a sentence of  $s$  years given conviction for crime  $j$ , and then define  $\rho'_{sj} = k\rho_{sj} \forall j$ . It remains straightforward to show that the distribution of time-served among admitted prisoners under the corrections policy summarized by the  $S \times J$  matrices  $\rho'$  and  $\rho$  are identical. Further, we can create infinitely many new matrices of sentencing weights with elements,  $\rho'_{sj} = k_{sj}\rho_{sj}$ , such that the implied changes in corrections policies imply higher admission rates and larger steady-state prison populations but no changes in the distribution of time-served among admitted prisoners.

In the next section, we show that, compared to arrested offenders in 1985, those arrested in recent years faced much higher likelihoods of serving short, medium, and long prison spells, and this result holds for all offense categories. If we use the notation above to describe how policy has changed since 1985, we would conclude that  $k_{sj} > 1$  for almost every combination  $(s, j)$ . We do not document dramatic changes over time in distributions of time-served prior to first release. However, this fact should deter no one from concluding that harsher sentencing and parole policies are the key drivers of prison growth in recent decades. The relative stability of the time-served distribution simply reflects that fact that, given arrest, prisons spells of all lengths became more likely over time.<sup>29</sup>

#### 4. NEW APPROACH

In this section, we present results from our own analyses of data on arrests, admissions, releases, and prison populations. We use the National Corrections Reporting Program (NCRP) 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 in order to select a set of states that provide reliable data over a long period of time. However, our audits are more extensive than those performed by Pfaff (2011), and we take the additional step of using micro-data on arrests to create offense-specific measures of arrests for each state-year-race cell in our NCRP data.

We use agency level data from the Uniform Crime Reports to construct state level data on arrests by offense for different years, and we demonstrate that our ability to track co-movements 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 many offenders are convicted of crimes that differ from the charges associated with their arrests and because there are lags between arrest and conviction. However, we gain useful insights by tracking these ratios over time, and in some analyses below, we treat these ratios as proxies for the likelihoods of entering prison given arrest for various offenses.

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<sup>29</sup>Note that, in our framework above, the admissions rate for a given convicted offender is simply one minus the sum of the likelihood of receiving each of  $S$  possible sentences.

We also depart from the existing literature by creating offense-specific survivor functions for prisoners who enter prison in each year from 1985 to 2005. For each major offense category, we trace not only how the likelihood of imprisonment given arrest changes over time but also how the likelihoods of serving sentences of various lengths change over time, given admission to prison. Our key tables describe how likely the persons arrested for a specific crime in a given year were to serve prison spells of various lengths. These results establish that sentencing and parole policies became much more severe over the past several decades.

Finally, we attempt to quantify the importance of these changes using simulations that describe how prison populations would have evolved under different assumptions about sentencing and parole policies. Here, we are careful to distinguish between admissions that result from new court commitments and admissions that result primarily from parole revocations. Because the majority of prison spells that result from new court commitments are less than two years in length, any policies that increase the number of prison admissions soon increase the number of persons on parole, even if the standards for parole revocation remain fixed. Further, this rise in the stock of parolees increases the number of parole revocations that generate new prison admissions.

Our simulation results indicate that the vast majority of prison growth we observe from 1985 to 2005 resulted from changes in policies that govern the sentencing and time-served of arrested offenders. Trends in arrests account for only a small portion of the prison growth we observe, and changes in policies that govern standards for revoking parole appear to have had small impacts on the overall growth of prison populations.

**4.1. Reliable Data.** Many social scientists are aware that 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 on crimes and outcomes in the criminal justice system are not usually gathered through the types of field operations that are often used to collect information about labor market outcomes or education. Instead, these data sets are typically compilations of self-reports made by government agencies within the criminal justice system concerning their own activities. This data collection process generates many missing reports as well as data that are often internally inconsistent or transparently wrong.

Social scientists have typically responded to these data quality problems by either 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 where NCRP data pass a number of reliability tests. We then clean the Uniform Crime Reports (UCR) data on crimes and arrests for these states to make sure that we have reliable information on the evolution of arrests over time in these states.

*4.1.1. Cleaning the NCRP Data.* Appendix A provides details concerning our data cleaning and construction procedures. Here, we provide an overview. We began by auditing the NCRP data. 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.

The NCRP data provide detailed records of admissions and releases for many states in many years from 1983 to 2009 as well as stocks of prisoners in custody for years 2005 through 2009. 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 eliminated 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 on NCRP data quality, we examined the dates in the release and admission data in the NCRP to check for internal consistency in 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 on NCRP data, we used the admission and release flows from 1987 through the period 2005-2009 to determine whether or not 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 teenagers 15 and under 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 involved comparisons between the NCRP data on admissions, releases, and stocks of prisoners and the National Prison Statistics data on flows and stocks. These two data series should not match exactly because they do not define the prisoner populations of interest in exactly the same way. Further, the available NPS files on admissions and releases end in 1998. However, large deviations in reported flows or large deviations between the reported changes in NPS stocks and the implied NCRP stock changes are cause for concern.

These four checks allowed us to group the remaining 34 states into three main categories: (1) Eleven states where NCRP data contain only minor problems. (2) Five states where NCRP data appear to be internally consistent but discrepancies with NPS require further investigation. (3) Seventeen states where NCRP data cannot be trusted. In addition, one state, IL, provides reliable data but stops reporting in 2003.

The analyses presented here employ data from 8 of the 11 states in the first category. They are CA, CO, MI, ND, NJ, SC, WA, and WI. We currently do not use NE, NY, and UT because their release records contain high missing rates concerning the type of admission or year of admission to prison in several years. This information is essential to estimate time served distributions for offenders who were admitted in a particular year.

Figure 1 uses data from the NPS to display percentage growth in prison populations for three samples: (i) state prisons in the 8 states in our main sample, (ii) all state prisons and (iii) all state and federal prisons. The overall growth pattern for our 8 states is similar to the pattern observed for all states. However, growth is more rapid early and levels off earlier in our sample. The data for CA account for most of these discrepancies. CA is a large state that experienced rapid prison growth early, and in part because of capacity constraints, CA prison populations stopped growing while many others continued to grow.

4.1.2. *Reliable Data on Crime and Arrests.* The FBI Uniform Crime Reporting (UCR) system collects data on crimes and arrests through reports from local law

enforcement agencies. However, these data are filled with missing reports as well as reports that appear transparently wrong. Further, while the FBI uses these data to produce national estimates of annual crime and arrest rates, we are not aware of any efforts to produce comparable estimates at the state level. Since our goal is to trace co-movements in arrests and prison outcomes by offense category, we produce annual estimates of crime and arrest rates for the eight states in our NCRP sub-sample.

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 making any determination about the monthly frequency of crimes or arrests in any given agency over any specific period. Based on monthly averages of valid reports of crimes and arrests within agencies within specific calendar years, we make imputations for missing monthly reports that do not result from “backlog filing.” In cases where agencies do not report for entire years, we use interpolation and regression procedures described in Appendix B to fill in the missing data.

**4.2. NCRP Results.** Table 6 above demonstrates that national trends in prison admissions and total prison populations do not track national trends in crimes or arrests after the early 1990s. Before turning to NCRP results on prison admissions and time-served in our eight-state sample, we first document that, in broad terms, trends in crimes and arrests follow the same patterns in our subsample of states that we see in the national totals.

Figure 2a shows trends in UCR data on violent crimes for the nation and our eight-state sample. Each dot is an index number with the 1985 level normalized to 100. The basic patterns are the same in the two samples, and violent crime in our NCRP states has been roughly at or below the 1985 level since 1999. Figure 2b shows the same basic trends in arrests for violent crime but here the figures for our NCRP states diverge slightly from the national numbers because arrests rise to a higher peak and never fall all the way back to 1985 levels. Still, arrests for violent crime in our NCRP states have been falling, almost steadily, since 1995.

Figures 2c and 2d present parallel results for non-violent crimes and arrests. Here, the declines in crimes and arrests begin several years earlier and are more pronounced in our NCRP states. Figure 2e shows that drug arrests follow qualitatively similar patterns in our NCRP sample and the nation as a whole, although some noteworthy quantitative differences emerge after 1995. Drug arrests in our NCRP states remain more than 50 percent above 1985 levels throughout the 2000s, but for the nation as a whole, drug arrests were at least double their 1985 levels for most of the 2000s. Most of this gap reflects the fact that drug arrests in California remained roughly flat from 1994 to 2002, and California accounts for a significant fraction of the arrests in our eight-state sample.

Figures 2a-2e show that the patterns in crime and arrest data for our sample of NCRP states follow the same broad trajectories that we observe for the nation as a whole, just as Figure 1 demonstrated that prison growth in our NCRP states followed national trends. We have already argued based on the results in Table 6 that the national growth in prison populations since the 1980s cannot be readily explained by trends in arrest data. Below, we show, in detail, that the growth in prison admissions and prison populations in our NCRP states since 1985 is far

greater than one would have expected based on sentencing and parole policies in 1985 and observed trends in arrest data since 1985.

Table 7 presents data on the persons in our NCRP data who entered prison immediately after their convictions for new crimes or who entered prison because they violated the terms of probations they were serving as a result of recent convictions. We would like to analyze admissions associated with new convictions separately because we would like to match admissions samples as closely as possible to the samples of arrested offenders that generated the admissions. However, some states include probation revocations in their reports of new court commitments, so were are not able to analyze these admission types separately. Nonetheless, the vast majority of admissions recorded in Table 7 should result from recent arrests, i.e. arrests in the current or previous calendar year.

Table 7 presents the key facts about sentencing and time-served outcomes over time in our eight NCRP states. At the end of 1985, the stock of prisoners in these eight state prison systems was about 105 thousand, and there were 42.6 thousand admissions to these systems associated with recent court convictions. By the end of 2005, these state prison systems housed roughly 326 thousand inmates, and in 2005, these systems recorded 100.3 thousand admissions that resulted from recent convictions.

Total admissions to these state prisons grew from just under 67.6 thousand in 1985 to roughly 199 thousand in 2005 because these states also experienced tremendous growth in the number of prison admissions associated with parole revocations. Below, we show that, for seven of our states, this growth in admissions due to parole revocations is primarily the result of growth in the stock of persons on parole, and not changes in standards used to determine parole revocations. Further, what appear to be changes in the implementation of parole policies over time in California have only modest impacts on growth in California prison populations. Thus, we argue that the changes documented in Table 7 drove overall prison growth in these states.

Four features of Table 7 deserve special attention. First, total arrests in these states peaked in 1994 and were actually lower in 2005 than in 1985. Second, prison admissions associated with new court commitments and probation revocations grew steadily from 1985 to 1998, declined slightly from 1999 to 2001, and then grew steadily again from 2002 through 2005. Third, the ratio of admissions to arrests also grew fairly steadily over this period. Because there are lags between arrests and convictions, these ratios do not provide precise information about the fraction of arrested persons in any given year who entered prison. However, total arrests move slowly in percentage terms over this period, so the entries in the Admissions/Arrests column are reasonable proxies for the probabilities of admission given arrest in different years.

Fourth, the distributions of time-served prior to first release shifted modestly to the right over the cohorts entering these state prisons between 1985 and 2002. Further, although release rates during the first four years appear to increase slightly over the 2003 to 2005 cohorts, the fractions of admitted prisoners who serve at least three, four or five years, remain greater than they were for the 1985 cohort. Thus, it seems reasonable to conjecture that the average time-served among admitted prisoners in 2005 will be equal to or greater than the average-time served among those admitted in 1985.



Taken together, these four results point to more punitive sentencing practices as the engine of prison growth in these states. Between 1985 and 2005, admissions to arrests ratios rose by more than 144 percent (.0159 to .0389) while the distribution of time-served among admitted prisoners shifted mostly to the right. This implies that when we compare those arrested in 2005 to those arrested in 1985, we see that alleged offenders in 2005 are at least 140 percent more likely to have entered prison and served prison terms of two years or less, two to three years, three to four years, four to five years, etc. Further, a similar result holds if we compare outcomes for those arrested in 1985 to those of any cohort arrested in the 2000s.

Table 7 shows that something changed between the doors of police stations and the doors of prisons that made harsh punishment much more likely for arrested offenders, and there is no credible way to explain these data without positing some hypothesis that involves prosecutors, judges, parole boards, or some combination of these actors changing their behavior over time in ways that created more punitive treatment of alleged offenders.

Before proceeding to results for each offense category, it is important to note that we highlight the 1985 to 2000 comparison in our tables because the NCRP data do not allow us to be as confident about the five-year release rates for offenders that enter prison after 2000. After 2005, we do not have valid reports for all eight NCRP states. Thus, the entries in our tables that correspond to outcomes in 2006 through 2010 are based on the assumption that cohort hazard rates for any subsample of our states match the corresponding cohort hazard rates for these years in our full eight-state sample.

The three panels of Table 8 present results that parallel those in Table 7 except Table 8 presents results separately for different offense categories. The first thing to note about the patterns in Tables 8a-8c is that the admissions to arrests ratios increase for every offense category. The increase in admissions to arrests documented in Table 7 did not occur because ratios of admissions to arrests remained constant within offense categories while the composition of arrests shifted toward more serious crime. Table 8 documents more punitive outcomes across the board.

We see the most dramatic increases in admissions to arrests for a handful of crimes that have not historically been associated with high admissions to arrests ratios. The numbers of prison admissions following conviction for sex crimes other than rape, non-aggravated assaults, motor vehicle theft, and other crimes rose much faster than the numbers of arrests in these categories. In fact, the admissions to arrests ratios for these categories all increased by more than 200 percent between 1985 and 2000, and with the exception of the “Other Sex Crimes” category, these ratios all continued rising in the 2000s.

The distributions of time-served among prisoners admitted for these offenses exhibit mixed changes over time, but given such large increases in the likelihood of admission given arrest, those arrested for these offenses in 2000 faced higher probabilities of serving short, medium, and long prison sentences than those arrested for the same offenses in 1985.

The patterns for drug offenders in Table 8c are also striking. Ratios of admissions to arrests for drug trafficking and drug possession more than doubled between 1985 and the early 2000s. Further, the trend in admissions to arrests for drug possession is particularly noteworthy because arrests for drug possession almost

doubled between 1985 and 1996 before gradually falling to roughly 1.5 times the 1985 level during the early 2000s.

Among those entering prison for drug-trafficking, expected time-served appears to grow over time because the time-served distribution is shifting to the right. Among those entering prison for drug possession, there is a marked decline over time in the probability of serving more than 10 years in prison prior to first-release. However, this group has always represented less than three percent of each cohort entering prison for drug possession, and the probabilities of serving more than two years or more than five years, conditional on entering prison for drug possession, increased over time.

When we multiply the admissions to arrests ratios by the entries for various survivor function levels, it becomes clear that Table 8c implies a trend toward harsher treatment for those arrested on drug charges. For example, in 1985, we see 7.8 admissions to prison for every 1000 arrests for drug trafficking, and 651 of every 1,000 prisoners who entered prison for drug trafficking served at least two years prior to release. Thus, for every 1000 persons arrested for drug trafficking in 1985, roughly 51 served at least two years in prison, but the comparable figure for 2005 is 120. If we repeat these calculations for the outcome of serving at least five years in prison, the resulting numbers are 19.5 for 1985 and 75 for 2005.

Although we do not see the same dramatic increases in admissions to arrests ratios among those arrested for violent crimes, the results in Table 8a still imply a trend toward harsher punishment of violent offenders. For every 1000 persons arrested for homicide in 1985, roughly 170 persons remained in prison ten years later. For those arrested for homicide in 2000, the comparable figure is 385. The exact numbers differ, but the same patterns hold for Rape, Robbery, and Aggravated Assault. Compared to comparable alleged offenders in 1985, persons arrested for violent crime in the 2000s faced not only greater risks of entering prison but much greater risks of serving long prison terms prior to first release.

Table 9 presents these results in a more compact manner that closely matches the statistical model of prison populations that we presented above. Recall that, for a person who is arrested and convicted for crime  $j$ , we defined  $\rho_{sj}$  as the probability that the offender serves a prison spell of length  $s$  given some baseline set of corrections policies. We then noted that one can define a new set of corrections policies,  $\rho'_{sj}$ , using a matrix of constants  $k_{sj}$  that scale these punishment probabilities up or down, i.e.  $\rho'_{sj} = k_{sj}\rho_{sj}$ . Further, one can collect these elements in matrices  $\boldsymbol{\rho}$  and  $\boldsymbol{\rho}'$  that fully characterize the two corrections 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 arrest record that documents his arrest, i.e. the most serious charge against against the defender at the time of arrest. Next, assume that the probabilities of conviction given arrest for specific crimes are identical for both cohorts. Finally, assume that persons who enter prison in the same year they are arrested.

None of these assumptions are valid. However, they allow us to link data on arrests and admissions in a useful way, and we argue below that the conclusions we

draw by making these links are likely robust to corrections for existing departures from our assumptions.

We consider all possible combinations of offense categories  $j$  and prison spell length  $s$ . We can calculate, within each cohort of arrested persons, the ratios of the number of persons convicted of offense  $j$  who served  $s$  periods in prison to the number of persons that were originally arrested for offense  $j$ . Given our assumptions, the ratio associated with any pair  $(s, j)$  in our baseline cohort is a consistent estimator of the quantity  $(\gamma_j * \rho_{sj})$ , where  $\gamma_j$  is the probability of conviction given arrest for offense  $j$ . The corresponding ratio for the latter cohort is a consistent estimator for  $(\gamma_j * \rho'_{sj})$ .

Now, recall that  $k_{sj} = \frac{\rho'_{sj}}{\rho_{sj}}$  defines the severity of corrections policies for the latter cohort relative to the baseline cohort. Thus, for each pair  $(s, j)$ , we can form a consistent estimator of  $k_{sj}$  by forming ratios of the ratios we describe above, i.e.

$$\hat{k}_{sj} = \frac{\widehat{[\gamma_j * \rho'_{sj}]}}{\widehat{[\gamma_j * \rho_{sj}]}}$$

Since  $k_{sj}$  is the proportional change in the likelihood of receiving a prison term of length  $s$  given arrest or conviction for crime  $j$ , we can describe how corrections policies changed between the two cohorts using a  $(S \times J)$  matrix  $\hat{K}$  with elements  $\hat{k}_{sj}$ .

Table 9 presents these results for the 60  $(s, j)$  pairs that capture the intersections of 12 offense categories and five spells lengths. For example, consider the cell in Table 9 that corresponds to the row “Drug-Trafficking” and the column “2-3 years.” The first entry tells us that for every 1000 persons arrested for drug trafficking in 1985, there were 7.2 persons who entered prison in 1985 and served between two and three years for drug trafficking. The second entry implies that the corresponding figure for 2000 is 27.1 persons. The final entry, 3.77, is the ratio of these two ratios,  $\hat{k}_{sj}$ , which describes how corrections policies changed between 1985 and 2000. And, given our assumptions above, this value indicates that the probability of serving between two and three years in prison, conditional on being arrested for drug trafficking, increased by 277 percent between 1985 and 2000.

Table 9 is a compact way to demonstrate that the data imply a shift to uniformly harsher punishments for offenders in all crime categories. Note that, for all non-violent crimes, each  $k_{sj}$  entry is greater than one, and the vast majority are greater than two. In every category, arrested offenders in 2000 faced a higher likelihood of serving prison terms of 0-1 year, 1-2 years, 2-3 years, 3-4 years, 4-5 years, or more than five years. Since only tiny fractions of those arrested for non-violent offenses ever serve more than five years in prison, we lose little information by aggregating all prison spells longer than five years into one category, and these results clearly establish that those arrested for all non-violent offenses faced harsher expected punishments in 2000 than in 1985.

The results for violent crime may appear different at first glance, but they still imply a shift to harsher punishments. Several of the  $k_{sj}$  ratios that correspond to short prison spells for violent crimes are actually less than one, but this does not indicate that corrections policies became more moderate in some respects over time. Recall that Table 8a demonstrates that total admissions to arrests ratios increased over the 1985 to 2000 period within every violent crime category. Thus,

within each category, any declines in the likelihood of short prison spells simply imply that the shift toward more frequent use of long prison spells was even greater than the increases in overall admissions rates given arrests. Note that the likelihood of serving at least five years given arrest more than doubled in each violent offense category.

We note above that, beginning in 2006, we have less than full coverage of releases in our eight NCRP states. Thus, in order to get better information on changes in the likelihood of long prison spells for violent criminals over time, we made similar comparison between the 1985 and 1995 cohorts.

We compute  $\hat{k}_{sj}$  values that describe changes in criminal justice policies between 1985 and 1995 governing sentence lengths of 0-1 years, 1-2 years, 2-3 years, 3-4 years, 4-5 years, 5-6 years, 7-8 years, 8-9 years, 9-10 years, and more than 10 years given arrest for each violent crime. The results for spells less than five years resemble those in Table 9 and those that describe spells longer than five years indicate a clear shift to longer prison spells for violent offenders. All of these  $\hat{k}_{sj}$  values are greater than one. The smallest is 1.21, and more than half of the values are greater than 2. Compared to those arrested in 1985, persons arrested for violent crime in 1995 were much more likely to enter prison and serve long prison terms.<sup>30</sup>

Table 9 not only demonstrates that the shift to harsher punishment for arrested offenders applied to all crime categories, it also shows that the sizes of some shifts are stunning. Conditional on being arrested for drug possession, the likelihood of serving between two and five years in prison increased by more than 500%, and the shift toward harsher punishment for drug traffickers is also dramatic. Finally, compared to their counterparts in 1985, persons arrested for simple assault in 2000 were five times more likely to serve at least five years in prison.

**4.3. Simulation Results.** Next, we describe simulation results that attempt to assess the connection between prison growth over the past several decades and changes in policies that govern the punishments given to arrested offenders. Appendix C provides details concerning the methods used to create these simulations. However, the conceptual framework is simple.

Once again, we assume that each convicted offender is convicted of the most serious charge listed in his arrest file, i.e. the charge recorded in UCR arrest files, and that the offender enters prison in the year of his arrest. Given this assumption, we can use data from the 1985-2005 NCRP files as well as NPS data from 1982 through 1985 to calculate the following rates:

- the rate at which arrested offenders enter prison in 1985 as new court commitments
- the rate at which persons enter prison in 1985 given that they have been on parole for  $p$  years
- the rate at which persons exit prison to parole – among those who entered prison in 1985 due to new court commitments and have served  $s$  years
- the rate at which persons exit prison to parole – among those who entered prison in 1985 due to parole revocations and have served  $s$  years

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<sup>30</sup>We see qualitatively similar results for the 2000 cohort when we simply rely on the data that we have in each year after 2005.

- the rate at which persons exit prison without parole supervision – among those who entered prison in 1985 due to new court commitments and have served  $s$  years
- the rate at which persons exit prison without parole supervision – among those who entered prison in 1985 due to parole revocations and have served  $s$  years

Further, we calculate each of these rates separately for cells defined by the interaction of three race groups, 14 offense categories, and two geographies. The race groups are white, black, and other. The offense categories are those used in Tables 8a-8c, and we divide geography into California and our other seven 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 found in our other NCRP states.

We begin by focusing on the following question:

*If we assume that crime rates and resulting arrest rates evolve independently of sentencing or parole policies, how would prison populations in our NCRP states have evolved over time if the rates described above had been held fixed at their 1985 levels?*

To understand how we build the counterfactual answer to this question, note first that we can use NCRP data to estimate the initial stocks of persons in prison and on parole at the beginning of 1985,<sup>31</sup> and we can also track all subsequent releases from these initial stocks of inmates and parolees. In addition, because the list of rates above fully characterizes the dynamics of flows in and out of prison and parole, we can track the flows that we expect into prison, out to parole, back to prison from parole, etc out of each sample of arrested offenders for 1985 and subsequent years. Further, we can also track the movements we expect for persons on parole in 1985 who subsequently entered prison because they violated their parole conditions. Thus, we can build a counterfactual time path for prison populations under the assumption that the rates described above govern the movements into and out of prison and parole for all cohorts of arrested offenders and parolees from 1985 forward.

Figure 3 presents the results of this exercise and makes our main point clear. Prison populations in our NCRP states grew from 105,000 at the beginning of 1986 to 326,000 at the beginning of 2006. However, taking the arrest series since 1985 as given, our counterfactual simulation suggests that at the beginning of 2006, the stock of prisoners would have been 161,000 if the 1985 corrections regime had remained in effect. Put differently, just over  $3/4$  of the growth in prison populations in these states between the beginning of 1986 and the beginning of 2006 resulted from changes in corrections policy.

Some may view this figure as an upper bound because we have assumed away the possibility that arrest rates would have been higher in recent years if courts and parole boards had not become more punitive. The most straightforward way to address this concern is to allow for the possibility that, *ceteris paribus*, the number of arrests in each year is a decreasing function of the stock of incarcerated persons

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<sup>31</sup>The aggregate prison stocks implied by these estimates match the stocks reported for 1985 by the NPS fairly well. See Appendix A.

at the beginning of the year. However, the existing literature does not speak with one voice concerning the magnitude of this relationship, and there is no clearly optimal way to parameterize this adjustment.

Levitt (1996) reports the largest, in absolute value, estimated elasticities between prison stocks and crime rates. He uses court orders concerning prison overcrowding as instruments for changes in prison populations and concludes that, at the state level, the elasticity of crimes rates in year  $t$  with respect 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. Thus, if we adjust the simulation results in Figure 3 for incapacitation and deterrence effects using these elasticities, the resulting counterfactual prison populations for our NCRP states are likely upper bounds on the prison populations that would have been realized post-1985 if the 1985 corrections policies regime had remained in place. Here, we assume that, within 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 with respect to lagged prison stocks as estimates of the corresponding elasticities of arrests with respect to lagged prison stocks.

Figure 4 presents the results. In each year from 1987 forward, we inflate arrest rates to account for the fact that crime rates may have been higher under the 1985 corrections regime since prison populations would have been lower. The results indicate that prison populations in these states would have grown from 105,000 at the beginning of 1986 to 186,000 at the beginning of 2006 if 1985 corrections policies had remained in effect. Thus, even when we use Levitt's results to account for incapacitation and deterrence, our results indicate that almost 65% of the growth in prison populations between the beginning of 1986 and the beginning of 2006 is the result of changes in the policies that govern sentencing and time-served outcomes among arrested offenders.

Some may argue that the drops in crime rates and arrest rates observed in the late 1990s and 2000s occurred not only because large prison populations incapacitate potential offenders but also because the dramatic growth of prison populations during the 1990s signaled a long-term shift in punitiveness that also deterred potential criminals. This line of reasoning may cause some to conclude that, for the purpose of our simulation exercise, Levitt's elasticities are still too small in absolute value because he exploits only year-to-year variation in prison populations induced by court orders. These court orders may be less salient for potential criminals than sustained long-term shifts toward more punitive policies.

However, sustained shifts toward more punitive punishment may have many long term effects that are not captured by the yearly variation in prison populations that Levitt exploits. While a sustained commitment to punitive sentencing and release policies may deter some potential offenders, it may also harden others. Policy makers demonstrate such a commitment by putting offenders in prison and keeping them there, and lengthy prison spells may have criminogenic effects. Below, we review the existing literature and note that a number of recent studies provide evidence consistent with the proposition that longer prison spells make recidivism more likely, which may indicate that time in prison strengthens ties to other persons involved in crime and also degrades the skills and habits that facilitate legal employment.

For these reasons, we contend that the results in Figure 4 yield a conservative estimate of the portion of prison growth during the 1986-2006 period that one can attribute to changes in corrections policies. Further, for any reasonable alternative assumptions about the elasticities of crime to changes in prison populations, our simulation model delivers results that always identify changes in corrections policies as the key driver of prison growth during the period.<sup>32</sup>

This result should not be surprising given the results in Table 9. During the past several decades, the likelihoods of serving short, medium, and long prison terms increased greatly for almost all arrested offenders. In many cases, these likelihoods increased by factors of 2, 3, or more. Nothing in the literature on deterrence or incapacitation effects suggests that these forces could ever be more than a minor drag on the growth in prison populations that one should expect from such drastic changes in corrections policies.

In Figure 3, we are tracking total and simulated prison populations. We have 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 the number of prisoners associated with parole revocations. Further, at the beginning of 2006, 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 persons serving prison terms that began as the result of parole revocations is growth in the stock of persons on parole and therefore at risk for revocation, and this growth in the stock of parolees was primarily driven by growth in the number of persons serving time for new convictions.

Finally, we note that prison populations in these eight states would have grown by more than 12,600 or just less than six percent if the ratios of incarcerated persons to population had simply remained constant within cells defined by age, race, and gender. Thus, Figures 3 and 4 suggest that between 69 and 80 percent of the growth in prison populations not accounted for by changes in demography can be attributed to increases in the punitiveness of corrections policies.

*4.3.1. Race-Specific Results.* We began by noting the low employment rates and high incarceration rates that now exist among black men, especially less-educated black men. We have shown that changes in sentencing and parole policies drove overall prison growth during the past several decades. Thus, we now ask whether these changes in sentencing and parole policies were implemented in a way that targeted blacks. Tables 10 and 11 present race-specific versions of Table 9.

These tables do not support the idea that changes in sentencing and parole policies during the 1985 to 2000 period were more punitive in cases that involved black offenders. Most of the  $\hat{k}_{sj}$  entries are, in fact, larger in Table 11, which describes results for white offenders.

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<sup>32</sup>Even if we double Levitt's elasticities, i.e. use -.8 for violent crime and -.6 for non-violent crime, our results still indicate that, if the 1985 sentencing and parole policies had remained in place, the growth in state prison populations in our eight NCRP states would have been less than half of the growth we observe.

Figures 5 and 6 repeat the simulations in Figure 3 treating blacks and whites as separate populations. Here, we see dramatic growth in prison populations for both races that cannot be explained by trends in arrests. Further, the 2006 ratio of actual prisoners to the simulated stock based on 1985 corrections policies is greater for whites than blacks, 2.16 vs 1.81.

Thus, there is no evidence that the prison boom was driven by *changes* in sentencing and parole policies that were differentially punitive for blacks. However, policy changes that increase expected punishment for arrested offenders have a larger effect on blacks as a group because black arrest rates have been and remain much higher than white arrest rates. There is also some suggestive evidence that, even in our base year 1985, black offenders were more likely to receive significant prison time for some offenses, e.g. Robbery, Burglary, Motor Vehicle Theft and Larceny. However, this baseline differential is a minor part of the story. The policy change we describe above involved an across the board move to more punitive sentencing and parole policies for all offenders, and this change had more significant impacts on black communities than white communities because black arrest rates have been at least four times greater than white rates from 1980 forward.

Incarceration rates for both whites and blacks have more than doubled since 1980 primarily because public officials persistently pursued ever more punitive sentencing and parole policies for arrested offenders of all races. Yet, because the black incarceration rate in 1980 was more than four times the white rate, this doubling of incarceration rates has had a much larger impact on black communities.

## 5. FEDERAL PRISONS

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 growth in the stock of Federal prison inmates from other sources. Tables 12a, 12b, and 12c present prison populations, admissions, and releases for the Federal prison system from 1989 through 2010.

In percentage terms, the Federal prison population grew even more rapidly than state prison populations during the 1990s and 2000s. Between, 1989 and 2010, the stock of federal prisoners increased by more than 250 percent.

However, the reasons for this growth are different. The Federal prison population increased by about 150 thousand persons over this period, and increases for only three offense categories account for almost 90 percent of this growth. The number of persons imprisoned for drug offenses increased by almost 81 thousand, while the number imprisoned for weapons and immigration offenses increased by more than 29 thousand and 21 thousand respectively. The stock of prisoners in the other crime category increased by roughly 18 thousand as well, while the stock of prisoners serving time for traditional violent and property crimes remained roughly constant.<sup>33</sup>

We do not have federal arrest data that is comparable to the arrest data we employ in analyzing the growth of state prisons. However, even if we had data on federal agency arrests, we could not compare trends in arrests to trends in crime for the federal crime categories that drove federal prison growth. UCR crime data do

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<sup>33</sup>We see large percentage increases in the stocks of prisoners incarcerated for rape, but this is an increase of only hundreds of people, and it appears to be driven by a change in the classification of what sexual crimes count as rapes.



not include drugs, weapons, or immigration as offense categories. When an offender breaks into a home and steals jewelry, the owner of the jewelry may well file a police report. However, drug crimes, weapons crimes, and immigration offenses typically involve episodes where all parties involved are violating the law. The persons who just bought illegal drugs or illegal weapons are not likely to report the sales to police, and the persons who helped illegal immigrants enter the country or obtain illegal employment have no interest in reporting the related immigration law violations to police. In this light, it is interesting to note that Federal prison admissions for drug offenses continued rising long after the crack epidemic ended in most urban areas, and admissions for immigration violations are higher in the period 2008-2010 than any previous period, even though rates of illegal immigration allegedly fell during the Great Recession.

We also do not have time-served information for federal prisoners by offense because the NCRP records for the federal prison system are spotty. Thus, we cannot calculate proxies for the likelihood of serving various prison terms given arrest for specific federal offenses, and we cannot clearly assess the extent to which changes in federal corrections policies mirrored the changes we see at the state level. However, we can note one important difference. Recent growth in the stock of federal prisoners did not involve significant growth within all offense categories. In fact, there was almost no growth in the numbers of prisoners serving time for offenses other than drug, weapons, and immigration violations.

Further, the growth in Federal prison populations was not the result of a uniform shift toward more punitive sentencing that officials applied in a color blind manner. The Anti-Drug Abuse Act of 1986 was one of the major federal actions in the War on Drugs, and it established mandatory minimum sentences for drug possession that appeared to target blacks for particularly punitive punishment. For example, the act put in place mandatory prison sentences for drug possession, but the rules that mapped drugs amounts into sentences made huge distinctions between drugs that are chemically similar but used by different racial groups. The act specifies a five year minimum sentence for either possession of at least five grams of crack cocaine or possession at least 500 grams of powder cocaine. Because powder cocaine is the main ingredient in crack cocaine this 100 to 1 ratio has attracted much attention. Many legal scholars have noted that crack use is much higher in black communities than white, while the prevalence of powder cocaine use is relatively higher in white communities, and they have alleged that the sentencing provisions of the Anti-Drug Abuse Act of 1986 targeted blacks for particularly punitive treatment.

In 2010, President Obama signed the Fair Sentencing Act that greatly reduced but did not completely eliminate disparities in mandatory sentencing provisions among drugs that are chemically similar. However, the rules in the Anti-Drug Abuse Act of 1986 governed sentencing in federal drugs cases for more than two decades.

Also, Rehavi and Starr (2012) demonstrate that racial biases may exist in the Federal system that are not codified in any statutes or guidelines. These authors find evidence that black offenders receive almost 10% longer sentences than comparable white offenders arrested for the same crimes, but half of this gap can be explained by decisions made before defendants are convicted. Holding constant the

characteristics of arrested offenders and the charges in their initial arrest files, federal prosecutors are more than twice as likely to file charges against black defendants that carry mandatory minimum prison sentences.

In our analyses of state prison growth, we have no information about charges filed, plea bargains, acquittals, or convictions. We only have data on arrests and prison spells. As we note above, this limits our capacity to know which arrested offenders entered prison as convicted offenders found guilty of specific crimes. However, the Rehavi and Starr (2012) evidence points to an advantage of our approach. Their results demonstrate that judges and parole boards are not the only actors who influence the punitiveness of corrections policies. Prosecutorial decisions also matter, and thus, it may make sense to begin our analyses with the pool of arrested offenders rather than the pool of convicted offenders.

Further, while we could learn more with data on both arrests and convictions, the data we have are sufficient to demonstrate that an important shift toward a more punitive corrections regime did occur. In our NCRP states, we find that the number of arrested offenders in all major non-drug crime categories fell between 1995 and 2005. However, the number of prison admissions and the stock of prisoners associated with major crimes rose over this period while the distributions of time-served for entering prisoners remained roughly constant.

If one assumes that persons who enter prison for major violent crimes and property crimes did not enter jail with a drug charge as the most serious charge in their arrest file, then the trends in our NCRP states demonstrate that, over time, prosecutors or judges or both found ways to generate more prison years served for major crimes from smaller samples of at-risk arrested offenders. Further, no hypothesis concerning changes in the composition of arrests over time can account for these trends in the NCRP states because arrests fell between 1995 and 2005 in every major crime category.

More work remains before researchers can pin down the mechanisms involved, but the number of people entering prison to serve significant prison spells following conviction for a major crime rose for more than a decade as the numbers of persons arrested in each major crime category fell, and the Rehavi and Starr (2012) results raise the possibility that changes in sentencing and parole policies may not fully account for the patterns we observe. Changes in prosecutorial behavior may also have played a role.

## 6. JAILS

While Federal prison population grew much faster than state prison populations over the past three decades, jail populations grew at similar rates, at least over the long term. Table 6 shows that in 1985, there were 1.8 inmates in state prisons for every inmate in local jails. In 1990, 1995, 2000, 2005, and 2011, this ratio equaled 1.74, 2.02, 1.99, 1.78, and 1.87 respectively. Thus, jail populations follow roughly the same long-term trends as state prison populations.

With 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 more closely than arrest records. Some sentenced prisoners serve time in jail while awaiting transfer to state prisons, and other convicted prisoners with short sentences actually 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 growing jail populations over the past several decades.

## 7. MEASURING BLACK-WHITE INEQUALITY OVER TIME

Most of the Smith and Welch (1989) results came from 1940-1980 long-form census files, but they also provided results from the Current Population Surveys (CPS). Smith and Welch (1989) used CPS data from the 1980s to demonstrate that black-white wage inequality remained roughly constant during the period 1980-1986. They concluded that, although black men had not made further progress during the 1980s, they had not given back the gains made in previous decades. Thus, the almost half century period 1940-1986 contained a record of significant absolute and relative economic progress for blacks.<sup>34</sup>

The CPS program has long been the source of many government statistics on labor force participation, unemployment, and earnings, and CPS data were the best source of information about 1980 labor market trends available to Smith and Welch (1989). Yet, for more than a decade, subsequent scholars have argued that the prison boom, which began around 1980, has created an environment such that CPS data provide a grossly incomplete picture of labor market inequality among men. Because the CPS program draws samples from the non-institutionalized population, researchers who rely on CPS data to measure employment rates cannot count prisoners among the non-employed. Further, because prisoners tend to be less educated and from minority populations, standard measures of labor market inequality between racial and education groups may be significantly biased by the systematic omission of prisoners.

The literature on how race-specific trends in non-employment may contaminate measured trends in black-white gaps in earnings or wages begins with Butler and Heckman (1977). The literature is large, and we will not review it all here. Instead, we review a set of studies that came after the prison boom of the 1980s and devote specific attention to the growing number of incarcerated persons missing from the CPS sampling frame.

Chandra (2000) is among the first economists to raise the possibility that different trends in institutionalization rates by race are an important source of selection biases that contaminate standard measures of how black-white earnings differences have evolved over time. He uses data from the 1940-1990 census files, and he employs three different imputation rules for non-workers. The first follows the approach developed by Brown (1984). It assumes that all non-workers have wage offers from the bottom half of their race-specific unconditional offer distributions and that offers follow a log normal distribution. The results from this procedure imply that between 1970 and 1990, the black-white ratio of mean wage offers actually fell slightly between 1970 and 1990 even though the ratio of mean observed wages rose from .69 to .75. The other two methods involved within-cell imputations. Chandra (2000) defines cells using the intersection of age and education

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<sup>34</sup>A significant literature followed Smith and Welch (1989). The literature affirms that conclusion that black Americans made important relative economic progress during much of the 20th century. However, the literature contains serious debate over the relative importance of different forces as contributing factors to black progress. See Card and Krueger (1992) and Donohue and Heckman (1991).

groups. The imputations involve either the mean or minimum of observed wages in each cell. These imputation rules do not produce estimates of black-white ratios of mean wage offers that differ dramatically from black-white ratios of observed wages. However, the results still imply that black-white wage ratios of observed mean wages are larger than the corresponding ratios of mean wage offers and that the gap between these two ratios grew between 1970 and 1990.

Neal (2006) employs data from the 1960-2000 census files. He divides the data into cells defined by the intersection of schooling levels and potential experience levels. He assumes that the mean log of potential earnings for those employed in the previous calendar year is .4 greater than the corresponding mean for those who are either institutionalized or simply not employed. The basic methodology is similar to the approach that Smith and Welch (1989) used to address the possibility that falling black employment rates after 1970 bias measures of trends in black wage growth after 1970. Neal settled on the .4 after examining the wages of men in the National Longitudinal Survey of Youth 1979 (NLSY79) who worked in some years but also reported no earnings for a number of calendar years.

For all but the groups with the most education and potential work experience, the Neal (2006) imputation rules imply that the absolute values of black-white gaps in log potential earnings are larger in 2000 than in 1970, and in most cells, this pattern does not hold for gaps in observed log earnings among the employed. Further, by 2000, the differences between observed black-white earnings gaps and the implied gaps in potential earnings are often quite large among workers with a high-school education or less, i.e. these differences typically imply that the gap in potential earnings is from one fifth to one third larger.

Becky Pettit and Bruce Western, along with a several co-authors, have done the most extensive work on this topic. Here, we review the results from one of their more recent and comprehensive efforts. Pettit, Sykes, and Wester (2009) depart from the approaches above by employing different data. They do not use census files to capture the institutionalized population. Instead, they estimate the population characteristics of prisoners using several annual surveys of prisons and jails, and then assume that growth in the number of prisoners in various demographic groups follows the same trajectories as the trajectory of total prisoners. Then, they add estimates of the annual stocks of prisoners in different demographic cells to annual CPS samples and form adjusted wage measures by making different assumptions about the potential wages and earnings of incarcerated persons. The three imputation rules they employ are: (i) assign all non-workers the mean potential wages of workers who share the same race, age, gender, and education, (ii) follow assignment rule (i) for non-workers who are not institutionalized and assign mean potential wages for inmates based on several surveys of inmates that gathered information about labor market outcomes prior to imprisonment, and (iii) assign zero wages to all non-workers. They perform these analyses separately for ages 22-64 and ages 22-30. They produce results for years 1980-2008.

In all specifications, black-white gaps in mean log hourly wages are larger in absolute value in 2008 than in 1980. As a whole, the results clearly indicate that blacks fared relatively better during the 1990s than during the 1980s or the period 2000-2008. Still, it is hard to draw precise conclusions from these results. The implied amount of increase in black-white inequality is much larger given specification (iii) than specifications (i) or (ii). In fact, the black-white wage gap for 2008 based

on (iii) is roughly double the gap that results from method (ii) even though the gap based on (iii) is only about 50% larger in 1980.

The first two approaches are conservative adjustments for selection. Non-workers who are not in prison always receive the average wages of similar workers as their wage offers, and the information used to form imputed wages for prisoners comes from those prisoners who were employed prior to entering prison. At the same time, the authors acknowledge that assigning wages of zero to all non-workers is an extreme adjustment for selection.

In sum, these methods all produce results that suggest standard measures of black-white wage inequality overstate the relative economic success of blacks in recent decades by ignoring the selection bias created by the prison boom. However, these results do not pin down how large this bias likely is.

**7.1. Imputation Methods.** Here, we also pursue an imputation strategy, but we do not focus on gaps in mean wages or mean log wages. Instead, we focus on the ratio of the median potential wage for black males to the median potential wage for white males in various demographic groups.

We focus on medians because medians have important robustness properties with respect to imputation rules that researchers use to deal with missing data problems. Suppose that a researcher has data on a sample of size  $N$ , and for  $M$  observations in this sample, data on the variable  $W$  is missing. If  $\frac{N-M}{N} > .5$ , the researcher can recover the true median of  $W$  for the entire sample if the researcher knows only the fraction of the missing observations,  $\alpha$ , that fall above the true median. Given this information, the researcher can create an imputation sample by assigning the highest observed  $W$  to  $\alpha M$  of the records with missing values while assigning the lowest observed  $W$  to the remaining  $(1-\alpha)M$ , and the median  $W$  in this imputation sample will be the median of the true  $W$  values over all records in the full sample. The researcher does not require any additional information about the distribution of the missing values. As long as  $\frac{N-M}{N} > .5$ , knowledge of  $\alpha$  alone provides enough information to recover the median of  $W$  over the full sample.

In tables 13 and 14, we exploit this result to produce numerous estimates of ratios of black median potential weekly wages to white median potential weekly wages for samples of men defined by experience group and year. In these tables, we group men by potential experience categories, and we focus on men with between 6 and 25 years of potential work experience.

Our goal here is to describe economic inequality that reflects inequality in labor market opportunities for men. Thus, we want to consider men who are in what should be their prime working years. We want to focus on persons who are finished with schooling and persons who are not in a period of transition to retirement.

We use data from the ACS and Census that describe earnings and weeks worked in the previous year, and we calculate weekly earnings as our wage measure. We do not compute hourly wages because data on hours worked from these sources is quite noisy and because we want to accommodate the possibility that some workers are willing but not able to secure full-time jobs.

The thought experiment here is to describe black-white differences in potential wages for the median male. For men who are not working and not institutionalized, we focus on the likelihood that the weekly wage they would earn if they took the best job available to them is above the median potential wage for men of their same race and potential experience. For men who are institutionalized, we take two different

approaches. The first, presented in Table 13, treats institutionalized workers like any other non-employed persons. Here, we define “potential weekly wage” as the wage available to a given worker if the worker was not institutionalized, and we further assume that the distribution of potential wages among the institutionalized is the same as the distribution among other observationally similar men who are not employed.

Table 14 presents results from our second approach. Here, we define the “potential weekly wage” values among institutionalized men as the wages available at jobs they can claim within their institutions, e.g. prison jobs. Given the wages paid for jobs in prisons or homes for adults with disabilities, we feel confident that these potential wages are all below the relevant median potential wages for each race-experience cell.

Tables 13 and 14 follow the same format. Each panel presents results for samples of workers that share similar levels of potential work experience. Each row presents results for a different year. The data for 1960-2000 come from census long form files. The data for 2007 and 2010 come from the American Community Survey. We include the 2007 results to demonstrate that effects of the Great Recession on black-white inequality.

The columns describe different mixing rules. The column (10/25) presents results based on the assumption that 10 percent of black men in a given cell who do not work actually face wage offers above the median wage offer for the cell while the corresponding figure among white men who do not work is 25 percent. The column (25/10) reverses these imputation rules by race. The column (15/15) uses a common imputation rule for white and black men. The Raw column presents the ratio of median observed black earnings to median observed white earnings. These results are equivalent to those one would obtain by using a (50/50) imputation procedure. The final six columns give the fractions working, institutionalized and not working or institutionalized.

Several patterns stand out in these tables.

First, our 1960 and 1970 results are not terribly sensitive to the imputation rules we employ. Among workers with five or less years of potential experience, correcting for selection is important even in 1960, and our results vary depending on the rule we employ. However, in these first two census years, we see smaller corrections for selection and a narrower range of selection corrected ratios for more experienced workers.

Second, when assessing the relative progress of black men during the 1970s, the use of the selection corrected results often implies even greater black relative progress during the 1960s. This is clearly true for those with 11-25 years of experience. In these groups, institutionalization rates fell during the 1970s for both black and white men, but the declines were greater among black men. Further, among both white and black men, employment rates in these experience groups were roughly the same in 1970 as in 1960.

Third, after 1970, differences between the ratios of observed median wages and our selection corrected ratios grow over time. By 1990, we see noteworthy differences between the raw ratios of observed median wages by race and all of our selection corrected measures. Note that the corrected ratios in the (25/10) column likely overstate the relative wages offers of black men, but these ratios are less than the corresponding entries in the Raw column for all experiences groups in each

year from 1990 through 2010. Further, in Table 14, the absolute values of these differences are almost always .05 or more. If we consider the (15/15) results, the differences between our selection corrected ratios and the ratios of raw observed medians are almost always greater than .10 in absolute values.

Fourth, among workers with less than 15 years of potential experience, it is hard to escape the conclusion that black-white ratios of median potential wage remained constant or fell after 1970. Among workers in these first two experience groups, the entries in each of the first three columns of Table 14 are smaller in 2010 than in 1970, and the same pattern holds for four of six comparisons in Table 13. These results are particularly noteworthy since the entries in the Raw column are much larger for 2010 than for 1970 in both cases. Even though median wages among employed men suggest that black men of all experience levels made significant relative progress during the 1970-2010 period, it appears quite likely that black men with 6-10 or 11-15 years of potential experience lost ground relative to their white counterparts.

Fifth, among those with either 16-20 or 21-25 years of potential experience, the changes in observed medians again imply that black men made substantial gains during the 1970-2010 period, i.e. gains of .128 and .119 respectively, but our selection corrected ratios provide little support for the view that these measured gains reflect actual gains in relative potential wages for black men. Within five of the six columns of Table 14 that describe results for workers with 16-25 years of experience, the absolute differences between our selection corrected ratios for 1970 and 2010 are less than .02. Unless racial differences in patterns of selection changed quite dramatically between 1970 and 2010, it is unlikely that black men in these groups actually made important relative progress during these four decades.

Sixth, while our fourth and fifth points taken together point to the conclusion that black men likely made little relative economic progress between 1970 and 2010, it is important to note that there were ups and downs. For black men with less than 20 years of experience, the 1990s involved noteworthy relative gains, and both our selection corrected ratios and the simple comparisons of observed median wages point to this conclusion. However, our results suggest that black men suffered significant relative losses during the Great Recession, and this is particularly true of those with 6-15 years of experience.

Even though the numbers in the Raw column barely move between 2007 and 2010, the selection corrected results tell a different story. For all experience groups, the fraction of black men without employment jumped sharply, and these jumps were roughly twice as large as the corresponding increases in white non-employment rates. Further, in three of four experience categories, black institutionalization rates increased by more than half of a percentage point. Thus, our selection corrected results suggest that black men with 6-15 years of experience clearly lost ground between 2007 and 2010 and these losses appear to have been quite large among those with 6-10 years of experience. Further, black men with more than 16-25 years of experience held their own at best and likely lost some ground during this period as well.

Our results support the conventional wisdom that black men enjoyed significant relative gains during the 1960s, and in most cases, our selection corrections suggest that these gains were slightly larger than the results based on wages for employed men indicate. In addition, for black men with 6-20 years of experience, the 1990s were also a decade of noteworthy relative progress, and this result holds for all of

our measures, even though black men with 21-25 years of experience appear not to have gained ground on white men during this same period. However, our results are squarely in line with the view that black relative progress was anemic at best in the 1970s and 1980s, and during the Great Recession, black men lost the ground they gained during the 1990s. These broad conclusions are fairly obvious given our selection corrected results even though none of them square with conclusions drawn from simple comparisons of median wages among working men.

Finally, it seems that any sensible assessment of relative welfare would indicate that black men in 2010 were relative worse off than they were in 1970. There is little evidence that black men enjoyed relative gains in terms of labor market opportunities over this period, and they experienced much larger increases in institutionalization rates generally and incarceration rates in particular. In sum, the representative working-age black man (in terms of medians) made no gains relative to his white counterpart during the 1970-2010 period, while less-skilled black men saw their employment rates fall and their incarceration rates rise relative to their white counterparts. Unless black men at the top of the skill distribution enjoyed large gains, there is no way to escape the conclusion that black men lost ground relative to white men between 1970 and 2010.

In Table 15, we present results for the ratio of black wages to white wages at the 75th and 90th quantiles of the respective wage distributions. We present four sets of results for the 75th percentile ratios. The first, (0,5) imposes the assumption that five percent of the missing white wage observations are above the corresponding 75th percentile values in the white potential wage distributions. The (0,0) column imposes the assumption that none of the missing wages are above the race-specific 75th percentiles in the corresponding potential wage distributions. The (0,5) column reverses the rule used in the first column. The Raw column presents ratios of the 75th percentile observations in the distributions of observed wages. For the 90th percentile, we consider only the (0,0) imputation rule. In both cases, these imputation rules apply to missing wages among those who are not institutionalized. We restrict the potential wages of institutionalized persons to be below the 75th or 90th percentiles of the corresponding potential wage distributions, respectively.

The theme of Table 15 is quite clear. The 1970s were a time of significant progress for black men in the top quarter of the black potential wage distribution with more than 10 years of experience. However, for all experience groups, black men at the top of the potential wage distribution have lost ground relative to whites since 1980, and in many cases, these losses are noteworthy.

Table 15 also presents a clear warning about the dangers of discussing trends in inequality based on statistics drawn solely from the sample of employed workers. The results in the Raw column give a false impression that, overall, blacks at the top of their potential wage distributions held their own relative to whites during the Great Recession. However, most of the selection corrected results suggest that, even highly skilled blacks, suffered modest losses relative to whites during the 2007-2010 period.

We have shown that policy changes and not changes in the prevalence of criminal behavior drove the dramatic increase in prison populations during roughly the past three decades. We have also explained why these policy changes had much larger impacts on black communities than white communities. Finally, we have shown that if one properly accounts for the large number of black men in prison, it is



reasonable to conclude that, at least among men, the economic prospects for black men at the medians of distributions of potential wages given various experience levels have not improved since 1970, and there is no evidence of relative black progress in the upper quartiles of potential wage distributions since 1980.

The type of imputation methods we employ here are not the right vehicles for assessing movements in the lower quantiles of potential wages distributions. In 2010, non-employment rates were more than 25 percent in all four experience groups, so it is not even possible to recover black-white ratios at the 25 percentile of potential wage distribution using imputation procedures like ours.

However, Table 3 provide ample evidence that less skilled black men have lost ground relative to their white counterparts since 1970. Employment rates have fallen and institutionalization rates have risen for less skilled men of all races, but these trends have been truly dramatic among less skilled black men. Recent gaps in employment rates and institutionalization rates between black and white high school dropouts are so large that we cannot imagine a credible explanation for these outcomes that does not clearly imply that less skilled black men are now worse off relative to whites than they were in 1970.

**7.2. Incarceration and Subsequent Labor Market Outcomes.** At this point, we are not able to determine the degree to which policy changes that increased prison populations actually contributed to the economic struggles of less-skilled black men. The selection corrections in Tables 13 and 14 reflect adjustments for not only prison populations but also the large population of prime-aged black men who are not incarcerated and also not employed, and we would like to know whether or not the current high levels of non-employment among black men, especially among those with little education (see Table 3), are in part a result of sentencing and parole decisions made in recent decades.

The literature on this topic does not speak with one voice. Here, we discuss mechanisms that may link the punitiveness of sentencing and parole policies to the life-cycle employment and earnings outcomes of less-skilled men. We then describe results from some of the more important studies on this topic. We conclude by presenting some original results that provide suggestive evidence about the possible criminogenic effects of incarceration.

For several reasons, policy shifts that favor incarceration over probation as well as longer time-served for admitted prisoners may increase recidivism by harming prospects for gainful market employment, strengthening ties to criminal networks, or both. While convictions per se may harm market opportunities if employers are reluctant to hire or promote convicted offenders, prison time creates forced separations from existing jobs, prevents the accumulation of additional work experience and removes offenders from networks of employed people who may provide useful contacts.

Further, prison time may not only affect prospects for legal work but also impact the attachment of offenders to illegal activities. Prisons are filled with career criminals. Thus, young offenders who are given prison time instead of probation are exposed to persons who may strengthen their attachment to crime by teaching them how to be better criminals or by simply deepening their exposure to organized networks of criminals.

Finally, many prisoners have drug problems or mental health challenges that simultaneously make them less fit for market work and more prone to crime. While

it is possible that rehabilitation and counseling programs in prisons may help such prisoners, it is also possible that prison environments may make these problems worse.

The existing literature contains mixed evidence concerning the effects of harsher punishments for offenders on subsequent labor market opportunities. Although it seems obvious that criminal convictions can only hurt and never help prospects for legal employment, it is difficult to isolate additional costs that convicted offenders may incur from receiving harsher sentences. Kling (2006) finds that, given admission to prison, the length of time-served does not have important negative impacts on future labor market outcomes. Kling employs controls for earnings histories prior to prison entry, and he uses the identity of judges as a source of exogenous variation in sentence lengths.

However, a more important policy choice involves the margin between probation and imprisonment. Western (2002) argues based on life-cycle histories in the NLSY79 that incarceration significantly harms future wage growth. The Western (2002) results are not ideal because he does not possess samples of arrested persons that allow him to compare persons who are arrested and released, versus those who are arrested and convicted but not imprisoned, versus those who are arrested, convicted, and imprisoned. Without the capacity to make these comparisons, it is difficult to know exactly what the NLSY79 patterns mean.

Grogger (1995) reports that arrests have negative effects on employment and earnings. However, these effects are modest and short-lived. Grogger also reports negative effects on imprisonment and time-served given imprisonment. However, the panel he employs is rather short, and he cannot clearly determine the extent to which these penalties simply reflect breaks in current employment that result from the demands of court appearances, guilty verdicts, or prison entry because he is not able to determine how these events affect long term employment and earnings trajectories.

The Kling (2006) study is unique because few studies that address the links between punishments and future labor market outcomes actually have access to data on a plausibly exogenous source of variation in the severity of punishments. However, a number of recent recidivism studies use judge assignment to isolate exogenous variation in the severity of punishments given the severity of offenses.

Aizer and Doyle (2011) exploit random assignment of juvenile offenders to different judges to isolate exogenous variation in assignment to incarceration among observationally similar offenders. They find that the use of incarceration as a punishment for juveniles significantly increases the likelihood of adult incarceration. In contrast, Green and Winik (2010) follow over 1,000 drug defendants and conclude that variation in assignment to incarceration versus probation induced by random assignment to different judges did not impact future arrest rates. Di Tella and Schargrodsky (2013) employ a similar research design using data on adult offenders from Argentina. They conclude that the use of electronic monitoring as a substitute for incarceration significantly lowers rates of recidivism. Chen and Shapiro Jesse M. (2007) employ a regression discontinuity model to examine the effects of assignment to federal prisons of different security levels. They find some evidence that harsher prison conditions do not deter future offending and may actually increase recidivism.

Although the literature on criminogenic effects of incarceration contains mixed results, the results do raise the possibility that the decision to incarcerate convicted offenders does, in some cases, prolongs the criminal careers of these offenders. Thus, while the incapacitation effects associated with higher incarceration rates may lower contemporary crime rates, greater reliance on incarceration may increase recidivism rates in the future.

A complete assessment of this issue is beyond the scope of this paper, but the results in Tables 16a-16c may contain important warning signs about the long term impacts of the prison boom. These tables employ data on arrests and admissions from our eight NCRP states, and they follow the same format as Table 1 through 4. The diagonals contain results for five cross-sections: 1985, 1990, 1995, 2000, and 2005. The horizontal rows contains results for a cohort of persons as they age. The vertical columns track results for a common age group over time. The entries give the numbers of men per thousand who are arrested or admitted to prison.

The key patterns of interest involve comparisons of the columns for younger versus older offenders. We note above that arrest rates fell between the early 1990s and 2005. And, this pattern is obvious in the results for men under 40. However, for men ages 40-44, the decline in arrest rates after 1995 is quite modest, and for men ages 45-49, arrest rates rose slightly between 1995 and 2005. In addition, we see noteworthy percentage growth in admission rates during the 1995-2005 period for men over forty but not for younger men.

Tables 16b and 16c show that the same patterns hold if we isolate property or drug offenses. These offenses involve illegal activities that generate income and therefore may provide valuable information about changes over time in the length of criminal careers. Once again, we see that the composition of arrests and prison admissions is shifting over time toward older offenders. In fact, drug arrests among those 45-49 rose by more than 50% between 1995 and 2005 while corresponding arrests rates for men in their 20s and 30s remained roughly constant.

Tables 3 and 4 indicate the employment rates for men in their 40s with less than a high school education are now around 35 percent for black men and just over 65 percent for white men. Since high school graduation rates among men have been roughly constant for decades, one cannot attribute these results to composition effects. Less-educated men have withdrawn from market work in large numbers, and these withdrawals may be permanent, especially among less-educated black men.

Look across the rows of Table 3. Among the most recent birth cohorts, members of each cohort begin their potential working lives with lower employment rates than the previous cohorts, and these employment rates do not rise with age. It takes little imagination to identify causal mechanisms that potentially link these trends to the prison boom observed in recent decades, but more research is required to pin down the importance of these mechanisms.

Finally, it is even more difficult to know how the prison boom has affected the spouses of inmates as well as their children, and how these effects will impact future generations. Wildeman and Western (2010) review the literature on this topic, and while they highlight the challenges of isolating truly causal effects of incarceration on families, they review many findings that point to the possibility that spouses and children may suffer when changes in criminal justice policies increase incarceration

rates. This is particularly true of policies that increase incarceration rates for non-violent offenders.<sup>35</sup>

## CONCLUSION

More than two decades ago, Smith and Welch (1989) used the 1940 through 1980 census files to document important relative black progress, but this progress did not continue. Neal (2006) demonstrates that black-white convergence in educational attainment among young adults and in test scores among youth stopped before 1990. Our analyses of labor market outcomes for men suggest that men at the median of the black potential wage distribution are now no better off relative to their white counterparts than they were in 1970, and trends in non-employment and incarceration rates strongly suggest that less-skilled black men are now relatively worse off than they were in 1970. Finally, while highly-skilled black men enjoyed important relative gains in the 1970s, these men did not make further relative gains after 1980.

Since 1980, prison populations have grown tremendously in the United States. Here, we show that, at least for the eight states that provide fairly reliable NCRP data, this growth was driven by changes in policy that influenced the distribution of punishments that arrested offenders expect to receive. This move toward more punitive treatment of offenders involved a shift toward harsher punishment for those arrested in each major crime category. Further, these changes in policy increased expected punishments for arrested offenders of all races. Nonetheless, these changes have had a much larger impact on black communities than white because arrest rates have historically been much greater for blacks than whites.

More research is needed to determine the extent to which changes in corrections policy have harmed the employment and earnings prospects of less skilled men, and less-skilled black men in particular. However, it no longer makes sense to discuss racial inequality in labor market outcomes using data on wage inequality among employed persons or even data on earnings and employment outcomes among non-institutionalized populations. The growth of incarceration rates among black men in recent decades combined with the sharp drop in black employment rates during the Great Recession have left most black men in a position relative to white men that is really no better than the position they occupied only a few years after the Civil Rights Act of 1965.

It may be two more decades before social scientists can fully assess the life-cycle and intergenerational consequences of the policy shifts that created the prison boom. However, the results in Tables 16a-16c are not encouraging. Significant numbers of men are no longer “aging out” of crime, and future work is required to assess the consequence of having significant numbers of men engaged in crime as a long-lasting career, especially if these men are concentrated geographically in specific communities.

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<sup>35</sup>Johnson (2009) produces related findings using data from the Panel Survey of Income Dynamics.

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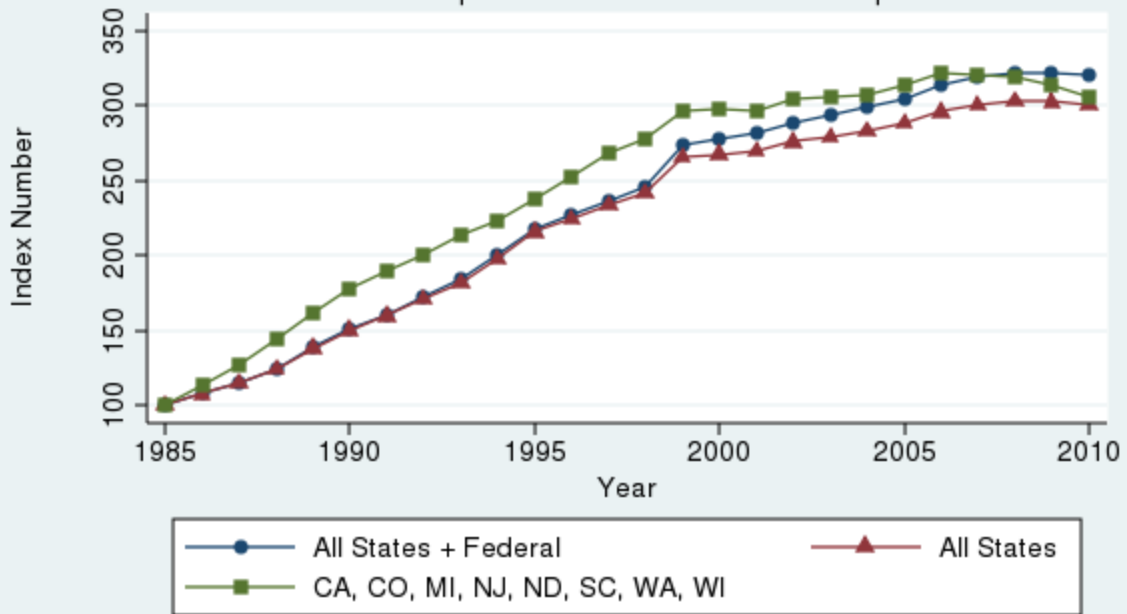
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Figure 1

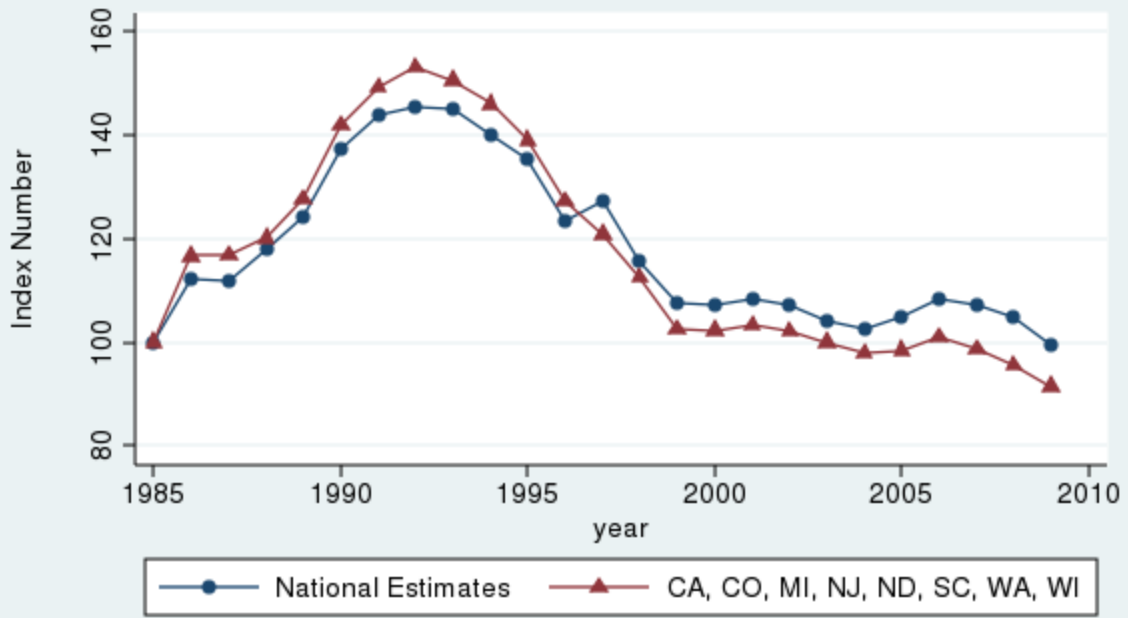
Prison Population Rates for Three Samples



This figure shows prison populations for 3 different scopes of the prison system.  
All data taken from the National Prison Statistics program.  
The vertical axis is indexed such that 1985 is 100.

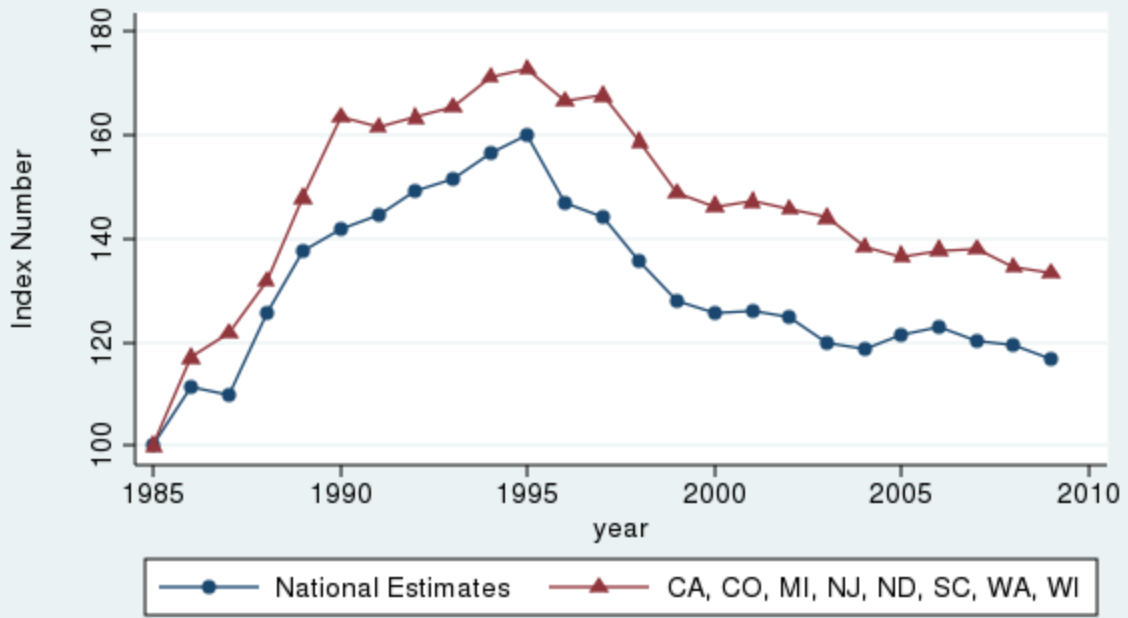


Figure 2a  
Violent Crimes



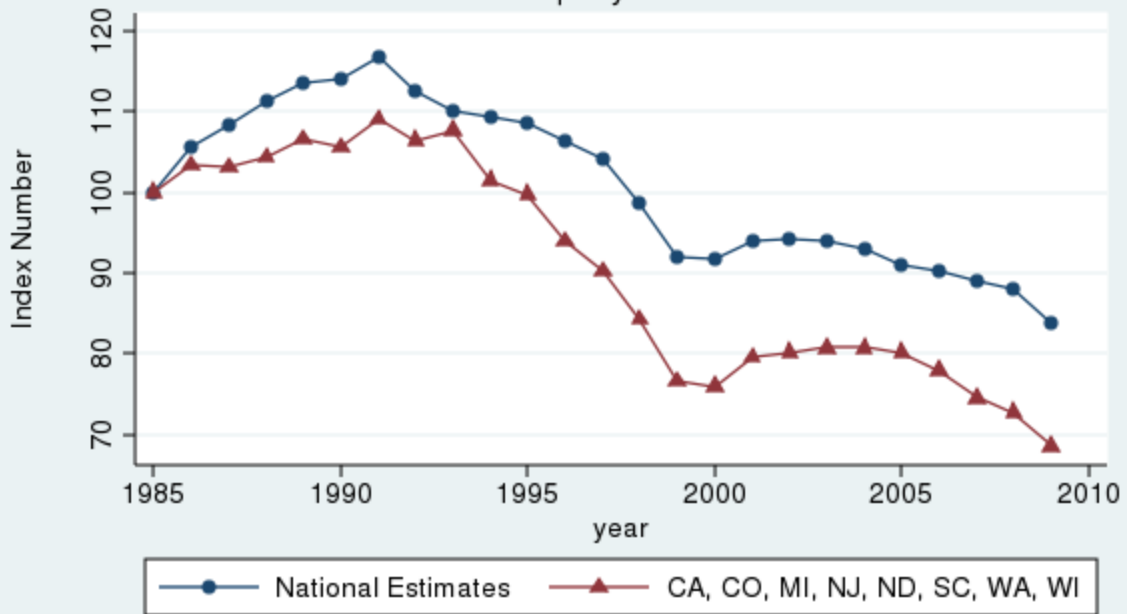
This figure plots reports of violent offenses (murder, rape, robbery, and assault) as calculated from the FBI's Uniform Crime Reporting program. The vertical axis is indexed such that 1985 is 100.

Figure 2b  
Violent Arrests



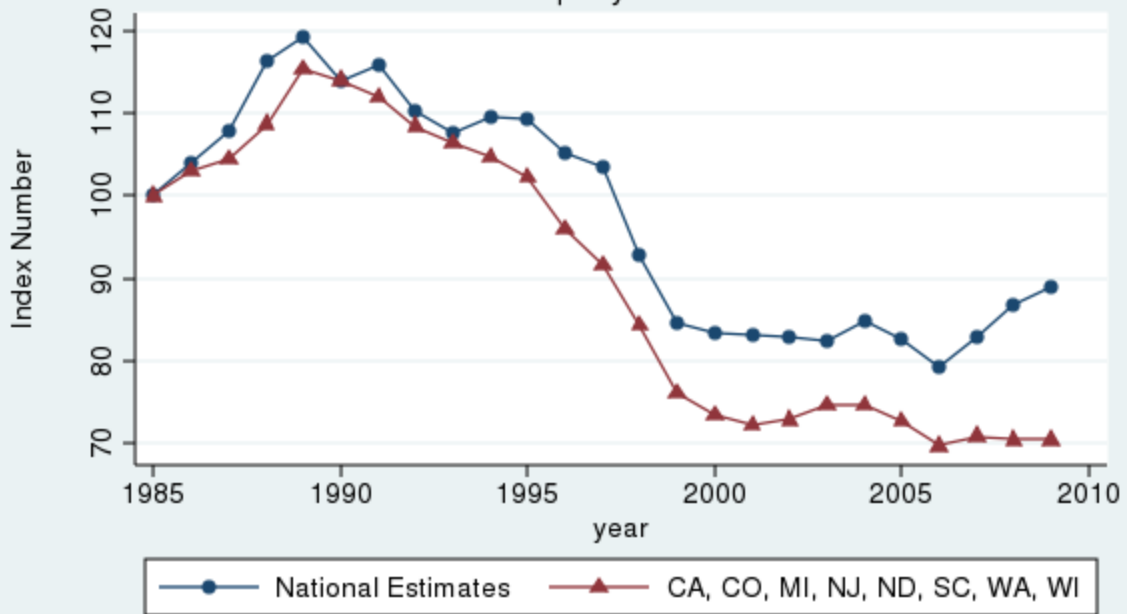
This figure plots arrests for violent offenses (murder, rape, robbery, and assault) as calculated from the FBI's Uniform Crime Reporting program. The vertical axis is indexed such that 1985 is 100.

Figure 2c  
Property Crimes



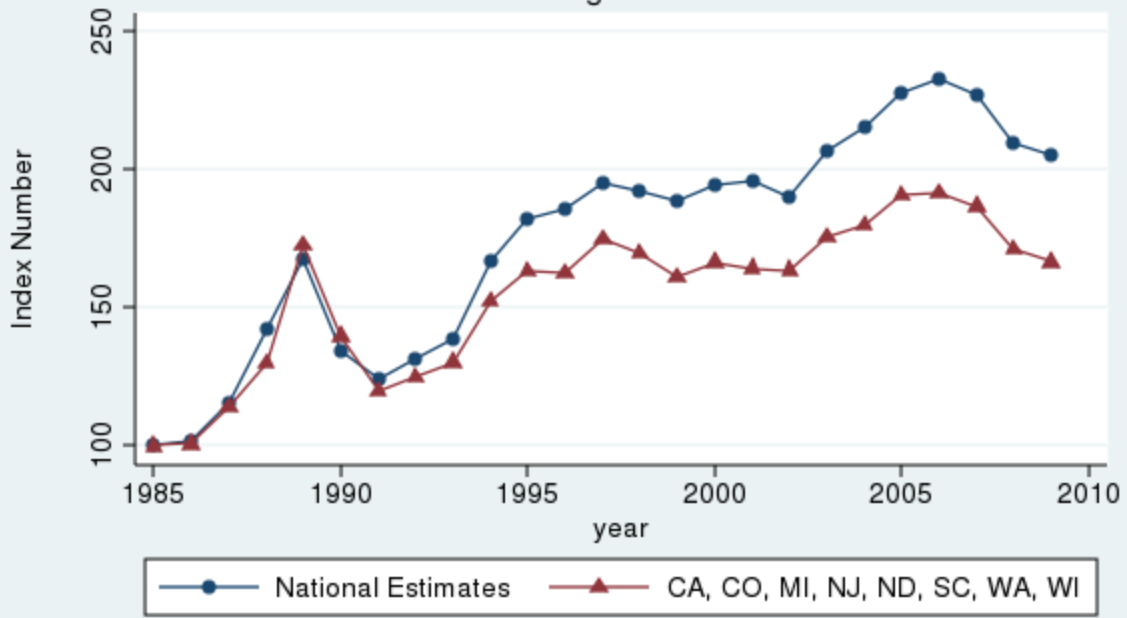
This figure plots reports of property offenses (burglary, larceny, and motor vehicle theft) as calculated from the FBI's Uniform Crime Reporting program. The vertical axis is indexed such that 1985 is 100.

Figure 2d  
Property Arrests



This figure plots arrests for property offenses (burglary, larceny, and motor vehicle theft) as calculated from the FBI's Uniform Crime Reporting program. The vertical axis is indexed such that 1985 is 100.

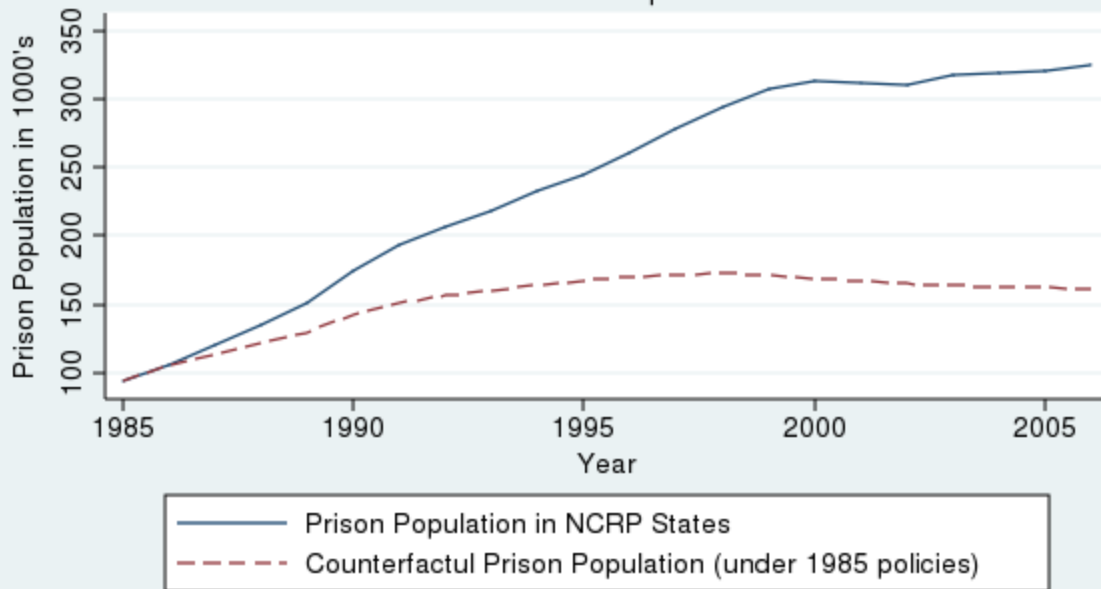
Figure 2e  
Drug Arrests



This figure plots arrests for drug offenses (trafficking and possession) as calculated from the FBI's Uniform Crime Reporting program. The vertical axis is indexed such that 1985 is 100.

### Figure 3

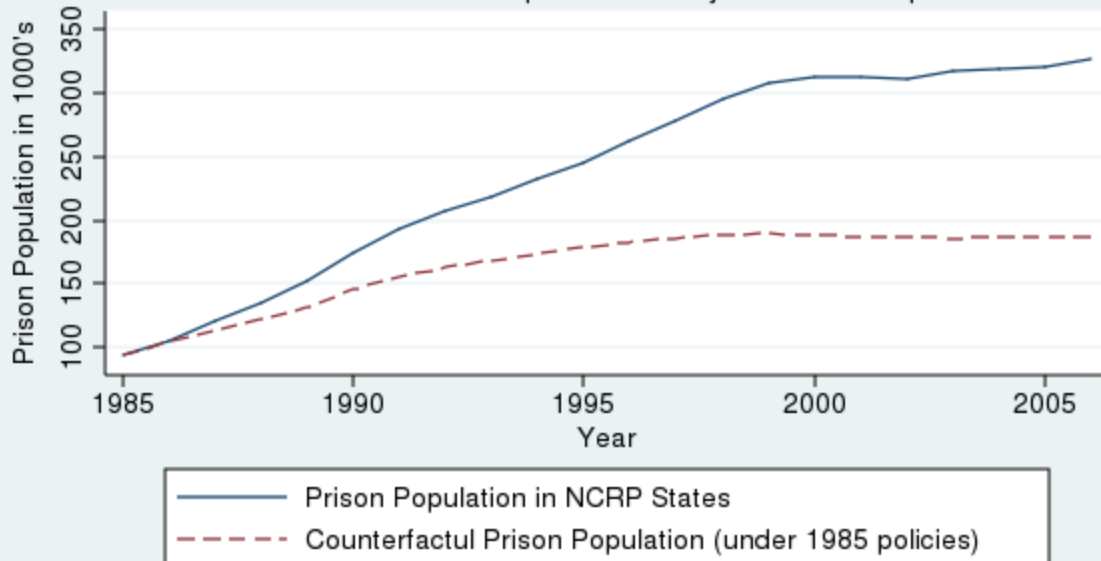
#### Counterfactual Prison Populations: Baseline



This figure shows the growth of prison populations over time in the 8 states we examine. The solid line plots the observed populations in the NCRP for those states while the dashed line plots the counterfactual growth under the assumption that incarceration policies do not change from their 1985 baselines.

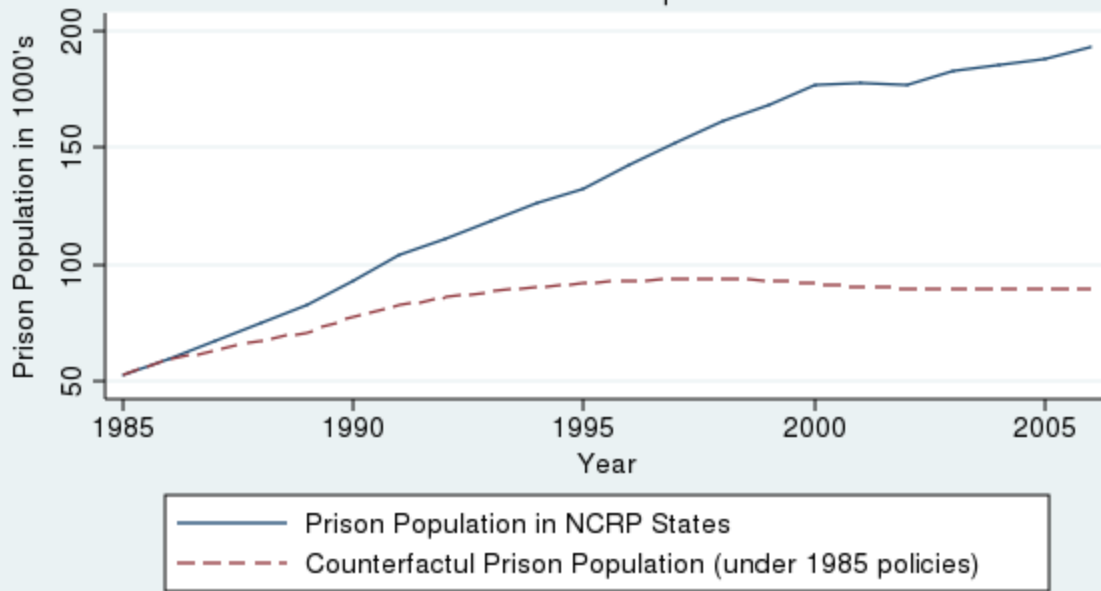
Figure 4

Counterfactual Prison Populations: Adjusted for Incapacitation



This figure shows the growth of prison populations over time in the 8 states we examine. The solid line plots the observed populations in the NCRP for those states while the dashed line plots the counterfactual growth under the assumption that incarceration policies do not change from their 1985 baselines. This figure also accounts for the incapacitation effects of imprisonment, whereby a smaller prison population implies a larger population of potential criminals.

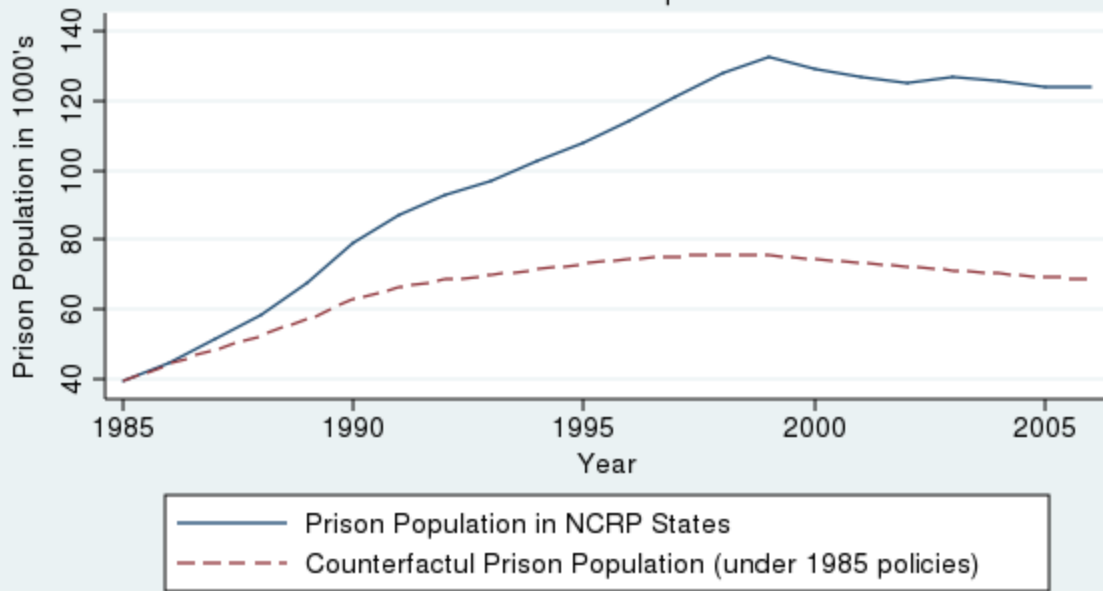
Figure 5  
Counterfactual Prison Populations: Whites



This figure shows the growth of prison populations by race over time in the 8 states we examine. The solid line plots the observed populations in the NCRP for those states while the dashed line plots the counterfactual growth under the assumptions that incarceration policies do not change from their 1985 baselines. See Table 2 for definition of 'white'.



Figure 6  
Counterfactual Prison Populations: Blacks



This figure shows the growth of prison populations by race over time in the 8 states we examine. The solid line plots the observed populations in the NCRP for those states while the dashed line plots the counterfactual growth under the assumptions that incarceration policies do not change from their 1985 baselines. See Table 1 for definition of 'black'.

**Table 1**  
**Employment and Institutionalization Rates**  
**Black Males**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.827 0.028
1915-1919					0.825 0.038	
1920-1924				0.828 0.044		0.823 0.025
1925-1929			0.818 0.054		0.842 0.028	
1930-1934		0.807 0.053		0.856 0.038		0.782 0.016
1935-1939	0.696 0.048		0.846 0.040		0.794 0.021	
1940-1944		0.811 0.054		0.792 0.031		0.748 0.029
1945-1949	0.647 0.055		0.761 0.044		0.748 0.044	
1950-1954		0.710 0.053		0.725 0.065		0.660 0.057
1955-1959	0.569 0.048		0.703 0.083		0.658 0.079	
1960-1964		0.679 0.095		0.661 0.100		0.640 0.066
1965-1969	0.574 0.085		0.657 0.115		0.650 0.079	
1970-1974		0.625 0.122		0.662 0.084		
1975-1979	0.509 0.116		0.607 0.108			
1980-1984		0.571 0.110				
1985-1989	0.445 0.086					

The top number is the employment rate and the bottom number is the institutionalization rate. This table was created using 1960-2000 census data and the 2007 and 2010 ACS from IPUMS. The IPUMS website is available at <http://usa.ipums.org/usa>. The IPUMS variables for employment and institutionalization were EMPSTAT (=1) and GQTYPE (=1 for 1990 and later; =2, 3, 4 for 1980 and earlier). After the 1980 census, the data no longer distinguish between different types of institutionalization. Thus, to be consistent we calculate total institutional rates for all years. All samples are weighted by the IPUMS variable PERWT. Black males are all those with race 'black'.

**Table 2**  
**Employment and Institutionalization Rates**  
**White Males**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.919 0.012
1915-1919					0.928 0.011	
1920-1924				0.936 0.011		0.923 0.008
1925-1929			0.932 0.011		0.934 0.009	
1930-1934		0.900 0.012		0.939 0.009		0.899 0.006
1935-1939	0.783 0.015		0.934 0.009		0.913 0.006	
1940-1944		0.901 0.011		0.918 0.007		0.890 0.007
1945-1949	0.738 0.013		0.906 0.008		0.900 0.009	
1950-1954		0.865 0.010		0.900 0.011		0.852 0.010
1955-1959	0.732 0.011		0.896 0.013		0.856 0.015	
1960-1964		0.873 0.016		0.853 0.019		0.811 0.014
1965-1969	0.757 0.015		0.851 0.020		0.826 0.016	
1970-1974		0.833 0.021		0.835 0.020		
1975-1979	0.735 0.022		0.824 0.022			
1980-1984		0.788 0.024				
1985-1989	0.654 0.020					

See notes for Table 1. White males are all those with race 'white' or Hispanics reporting 'other race' (not white, black, Asian and Pacific Islander, or Native American).

**Table 3**  
**Employment and Institutionalization Rates**  
**Black Males With Less Than HS**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.816 0.029
1915-1919					0.810 0.041	
1920-1924				0.808 0.048		0.799 0.029
1925-1929			0.799 0.061		0.820 0.031	
1930-1934		0.783 0.067		0.821 0.051		0.718 0.022
1935-1939	0.680 0.067		0.801 0.058		0.727 0.030	
1940-1944		0.752 0.086		0.700 0.051		0.617 0.044
1945-1949	0.580 0.095		0.640 0.078		0.598 0.069	
1950-1954		0.562 0.108		0.528 0.110		0.439 0.117
1955-1959	0.453 0.099		0.462 0.172		0.414 0.169	
1960-1964		0.394 0.214		0.379 0.242		0.369 0.174
1965-1969	0.329 0.212		0.337 0.323		0.333 0.218	
1970-1974		0.302 0.331		0.344 0.239		
1975-1979	0.278 0.298		0.290 0.278			
1980-1984		0.249 0.322				
1985-1989	0.192 0.264					

See notes for Table 1. This table was created using only data for black males whose value for the IPUMS variable EDUCD was either less than or equal to 50 (completed less than 12 years of schooling).

**Table 4**  
**Employment and Institutionalization Rates**  
**White Males With Less Than HS**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.892 0.016
1915-1919					0.893 0.018	
1920-1924				0.896 0.019		0.883 0.015
1925-1929			0.894 0.021		0.894 0.016	
1930-1934		0.861 0.026		0.890 0.019		0.816 0.013
1935-1939	0.782 0.034		0.879 0.023		0.822 0.014	
1940-1944		0.852 0.029		0.816 0.020		0.750 0.018
1945-1949	0.745 0.044		0.793 0.027		0.742 0.028	
1950-1954		0.756 0.036		0.746 0.032		0.629 0.029
1955-1959	0.693 0.036		0.747 0.039		0.649 0.041	
1960-1964		0.734 0.045		0.656 0.047		0.653 0.033
1965-1969	0.688 0.051		0.664 0.052		0.684 0.042	
1970-1974		0.677 0.052		0.715 0.048		
1975-1979	0.659 0.056		0.710 0.058			
1980-1984		0.674 0.067				
1985-1989	0.576 0.070					

See notes for Table 3.

**Table 5**

	<b>Abolish / Restrict Discretionary Parole</b>	<b>Sentencing Commission<sup>c</sup></b>	<b>Truth In Sentencing<sup>e</sup></b>
<b>AL</b>			
<b>AK</b>	1980 (partial) <sup>a</sup>	1980	Other
<b>AZ</b>	1994 <sup>b</sup>		85%
<b>AR</b>	1994 <sup>a</sup>	1994	Other
<b>CA</b>	1976 <sup>b</sup>		85%
<b>CO</b>			Other
<b>CT</b>			85%
<b>DE</b>	1990 <sup>a, b</sup>	1987	85%
<b>FL</b>	1983 <sup>a, b</sup>	1983 - 98	85%
<b>GA</b>			85%
<b>HI</b>			
<b>ID</b>			100%
<b>IL</b>	1978 <sup>b</sup>		85%
<b>IN</b>	1977 <sup>b</sup>		50%
<b>IA</b>			85%
<b>KS</b>	1993 <sup>a, b</sup>	1993	85%
<b>KY</b>			85%
<b>LA</b>			85%
<b>ME</b>	1976 <sup>b</sup>		85%
<b>MD</b>		1983 <b>1996<sup>d</sup></b>	50%
<b>MA</b>			Other
<b>MI</b>		1984	85%
<b>MN</b>	1980 <sup>a, b</sup>	1980	85%
<b>MS</b>	1995 <sup>b</sup>		85%
<b>MO</b>		1997	85%
<b>MT</b>			
<b>NE</b>			50%
<b>NV</b>			100%
<b>NH</b>			100%
<b>NJ</b>			85%
<b>NM</b>	1977 <sup>b</sup>		
<b>NY</b>			85%

	Abolish / Restrict Discretionary Parole	Sentencing Commission <sup>c</sup>	Truth In Sentencing <sup>e</sup>
<b>NC</b>	1994 <sup>a</sup>	1994	85%
<b>ND</b>			85%
<b>OH</b>	1996 <sup>a, b</sup>	1996	85%
<b>OK</b>			85%
<b>OR</b>	1989 <sup>a, b</sup>	1989	85%
<b>PA</b>		1982	85%
<b>RI</b>			
<b>SC</b>			85%
<b>SD</b>			
<b>TN</b>	1989 (partial) <sup>a</sup>	1989-95	85%
<b>TX</b>			50%
<b>UT</b>		1979 <b>1983</b>	85%
<b>VT</b>			
<b>VA</b>	1995 (partial) <sup>a, b</sup>	1991-95	85%
<b>WA</b>	1984 <sup>a, b</sup>	1984	85%
<b>WV</b>			
<b>WI</b>	1999 <sup>b</sup>	1985	Other
<b>WY</b>			

Notes:

<sup>a</sup>: Listed in [Frase, Richard S. "State Sentencing Guidelines: Diversity, Consensus, and Unresolved Policy Issues." *Columbia Law Review* 105:4, pp. 1190-1232, 2005], Table 1 as abolishing parole release in the listed year.

<sup>b</sup>: Listed in [Stemen et al. "Of Fragmentation and Ferment: The Impact of State Sentencing Policies on Incarceration Rates, 1975-2002, National Institute of Justice report, 2006], Table 1-3 as enacting determinate sentencing in the listed year.

<sup>c</sup>: Listed in (Frase, 2005), Table 1 as establishing a state sentencing commission in the listed year. Date ranges are used when a sentencing commission was abolished.

<sup>d</sup>: First year is date when commission first established. Bolded date is year when commission made permanent. In all other cases, commission was permanent when established.

<sup>e</sup>: Listed in [Ditton et al. "Truth in Sentencing in State Prisons." Bureau of Justice Statistics Special Report. 1999], Table 1 as requiring prisoners to serve the listed percentage of their minimum sentence.

**Table 6****Estimated National Crimes, Arrests, and Admissions in Thousands: 1985-2011**

Year	Crimes		Arrests				Admissions	Stocks			
	Violent	Property	Violent	Property	Drug	Other	State Prison	Jail	State Prison	Federal Prison	Total
1985	1328	11103	498	1945	811	8691	252	257	463	40	760
1986	1489	11723	554	2023	824	9086	285	274	502	44	821
1987	1484	12025	546	2100	937	9128	320	296	536	48	880
1988	1566	12357	626	2263	1155	9769	361	344	577	50	971
1989	1646	12605	686	2320	1362	9973	437	396	651	59	1106
1990	1820	12655	706	2218	1090	10182	474	405	706	66	1177
1991	1912	12961	719	2253	1010	10231	480	426	752	72	1250
1992	1932	12506	742	2146	1066	10121	496	445	803	80	1328
1993	1926	12219	754	2094	1126	10062	493	460	881	90	1430
1994	1858	12132	779	2132	1351	10387	514	486	960	95	1541
1995	1799	12064	796	2129	1476	10719	535	507	1026	100	1633
1996	1636	11805	730	2046	1506	10886	526	518	1077	106	1701
1997	1689	11558	718	2016	1584	10967	550	567	1129	113	1809
1998	1534	10952	676	1806	1559	10488	576	592	1179	123	1894
1999	1426	10208	636	1645	1532	10218	587	606	1228	135	1970
2000	1425	10183	625	1621	1580	10155	618	621	1236	145	2003
2001	1439	10437	627	1618	1587	9867	601	631	1249	157	2037
2002	1424	10455	621	1614	1539	9968	619	665	1277	164	2106
2003	1384	10443	597	1605	1678	9759	644	691	1297	173	2161
2004	1360	10319	590	1650	1746	10019	655	714	1316	180	2211
2005	1391	10117	604	1609	1846	10035	683	748	1338	188	2273
2006	1436	10031	612	1540	1890	10339	697	766	1378	193	2337
2007	1422	9873	597	1610	1842	10160	699	781	1399	200	2379
2008	1393	9775	595	1687	1703	10021	696	786	1409	201	2396
2009	1318	9321	582	1728	1664	9714	680	768	1406	208	2381
2010	1251	9113	552	1644	1639	9286	654	749	1403	210	2361
2011	1204	9063	535	1640	1531	8703	612	736	1382	216	2334

Crime and arrest data are FBI estimates of national totals as published in Crime in the United States (1985-2011). Admissions data are National Prison Statistics counts of admissions to state prisons for sentences of length greater than one year. Jail data are midyear counts taken from the Census of Jails and Annual Jail Survey. State and federal prison stock data are NPS counts of prisoners with any length of sentence.



**Table 7: Sentencing and Time Served Outcomes (All Offenses)**

**Admission Types:** Court Commitments and Probation Revocations  
**Races:** All  
**States:** CA, CO, MI, NJ, ND, SC, WA, WI  
**Prior Jail Time:** Excluded

Year	Arrests	Prs. Adm.	Adm. per 1000 Arr.	Remaining Prisoners t Years after Admission Year for every 1000 Admissions									
				1	2	3	4	5	6	7	8	9	10
1985	2,679	42.6	16	586	323	209	146	114	93	80	71	65	59
1986	2,764	48.3	17	594	321	208	149	116	94	82	73	67	61
1987	2,819	53.4	19	588	315	205	146	113	93	80	72	66	61
1988	2,873	60.3	21	578	307	195	137	107	90	78	71	65	60
1989	3,141	72.0	23	563	287	179	128	100	83	73	66	61	56
1990	3,163	78.7	25	561	279	175	126	100	84	73	66	61	55
1991	2,971	80.3	27	564	290	190	139	110	93	81	73	66	57
1992	2,857	83.7	29	565	297	198	146	116	98	86	76	66	58
1993	2,844	85.7	30	565	303	203	151	121	102	89	75	65	56
1994	2,899	84.2	29	565	308	203	150	120	100	85	72	63	54
1995	2,894	89.0	31	585	324	218	167	132	108	92	79	68	59
1996	2,895	91.7	32	586	326	224	173	135	111	94	81	70	61
1997	2,889	92.7	32	582	331	223	169	131	109	92	80	70	62
1998	2,780	94.6	34	593	335	221	167	129	107	90	79	70	61
1999	2,664	88.8	33	590	329	220	165	128	106	92	81	71	61
2000	2,628	86.3	33	583	336	224	168	130	110	96	83	71	66
2001	2,568	86.6	34	587	338	222	167	130	112	95	82	75	
2002	2,563	91.3	36	588	332	219	169	133	113	95	88		
2003	2,599	95.5	37	565	314	217	170	133	111	101			
2004	2,579	98.8	38	516	301	209	160	123	112				
2005	2,579	100.3	39	539	311	211	159	140					
Percentage Change 1985 - 2000	-2%	103%	107%	0%	4%	7%	15%	14%	18%	20%	18%	9%	11%

(1) Arrests and prison admissions reported in 1000's. Rates reported in percent. (2) If states stop reporting before 2009 (MI (2006), NJ (2005), WI (2007)), calculations are based on the assumption that the cohort hazard rates of the remaining states match the cohort hazard rates for our full eight-state sample. Arrest figures from FBI Uniform Crime Reporting; admissions figures from National Corrections Reporting Program.

**Table 8a: Sentencing and Time Served Outcomes (Violent Crime)**

**Admission Types:** Court Commitments and Probation Revocations

**Races:** All

**States:** CA, CO, MI, NJ, ND, SC, WA, WI

**Prior Jail Time:** Excluded

Year	Murder and Homicide						Forcible Rape						Robbery						Aggravated Assault						Other Assault					
				Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.								
	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10
1985	6.2	2.7	442	785	547	384	10.5	1.3	124	749	314	126	82.8	2.8	33	361	83	33	40.4	5.3	132	508	155	54	170.4	0.5	3	218	43	24
1986	6.3	2.9	457	801	557	388	10.7	1.3	118	768	355	153	106.2	3.1	29	358	86	35	40.4	5.4	135	507	169	64	192.4	0.6	3	325	69	43
1987	6.7	3.0	445	829	589	429	10.8	1.1	104	751	354	147	115.2	3.2	28	391	96	43	37.7	5.6	148	490	174	71	198.3	0.6	3	358	76	45
1988	7.0	3.2	456	824	594	440	11.0	1.0	92	766	378	181	127.8	3.5	27	385	97	46	38.5	5.4	142	502	172	75	212.6	0.6	3	316	83	57
1989	7.7	3.3	427	828	595	439	11.2	1.2	103	777	373	180	144.7	4.0	28	379	111	49	43.1	5.8	135	497	180	84	235.0	0.6	2	243	97	79
1990	8.1	3.6	439	835	611	441	11.6	1.3	113	718	352	158	159.1	4.5	28	367	100	46	49.9	6.6	131	491	175	79	249.8	0.7	3	203	83	52
1991	7.9	3.8	480	840	617	444	11.2	1.3	120	747	410	196	157.1	4.9	31	382	115	50	49.6	7.2	145	512	188	78	242.7	0.9	4	282	110	54
1992	7.3	4.0	553	868	658	474	10.8	1.4	130	751	423	192	161.5	5.1	32	372	118	48	48.8	7.6	157	523	198	77	249.4	1.0	4	265	93	47
1993	7.3	4.0	547	870	672	482	10.2	1.4	135	755	429	181	166.0	5.1	31	378	122	48	47.7	7.7	161	536	201	70	262.3	1.2	5	266	115	42
1994	6.8	3.7	540	873	684	498	9.7	1.3	131	741	410	183	176.3	5.5	31	401	126	46	46.6	7.2	154	570	240	82	279.4	1.5	5	255	93	41
1995	6.4	3.8	605	885	701	513	8.9	1.3	143	798	449	193	181.2	5.8	32	415	165	57	45.3	7.3	161	571	268	99	285.4	1.6	6	252	101	40
1996	5.9	3.9	657	889	738	555	8.9	1.4	159	844	513	248	175.6	6.4	36	426	171	56	42.6	7.5	175	584	294	111	281.5	1.8	6	256	100	29
1997	5.4	3.6	671	907	752	588	8.5	1.3	158	861	536	276	180.8	6.5	36	421	158	57	39.8	7.1	180	599	293	113	283.5	2.2	8	271	86	24
1998	5.4	3.5	646	898	760	584	8.3	1.3	157	853	528	281	171.3	6.7	39	426	162	55	36.1	7.0	193	583	303	135	275.9	2.4	9	274	83	22
1999	4.8	3.2	662	904	770	598	7.9	1.3	160	864	529	273	162.9	6.4	39	439	165	58	32.3	6.4	197	593	331	162	264.0	2.2	8	253	79	21
2000	4.7	3.0	638	891	765	603	7.5	1.2	156	838	522	295	161.8	6.2	39	431	175	63	30.5	6.1	201	635	351	183	263.3	2.4	9	267	74	16
2001	4.3	3.1	719	883	747		7.2	1.2	162	826	525		164.0	6.4	39	421	166		30.3	6.0	197	670	366		260.0	2.4	9	265	71	
2002	4.1	3.1	758	892	757		7.3	1.2	161	803	551		162.2	6.6	41	424	165		30.2	6.2	205	675	382		259.2	2.7	11	258	66	
2003	4.1	3.2	767	880	752		7.0	1.3	184	804	578		160.0	7.0	44	403	168		30.3	6.2	206	685	370		263.8	2.9	11	235	80	
2004	4.0	3.0	742	872	744		6.6	1.2	183	800	572		152.1	7.2	47	383	151		30.7	6.2	202	672	346		255.6	3.0	12	225	72	
2005	3.9	3.3	860	875	757		6.3	1.2	182	818	664		149.5	7.2	48	399	191		31.3	5.9	190	656	367		252.2	2.9	12	213	81	
Percentage Change 1985 - 2000	-24%	9%	44%	13%	40%	57%	-29%	-10%	26%	12%	66%	135%	95%	126%	16%	20%	112%	91%	-24%	15%	52%	25%	127%	236%	55%	363%	200%	23%	72%	-34%

See notes to Table 7.

**Table 8b: Sentencing and Time Served Outcomes (Property Crime)**

**Admission Types:** Court Commitments and Probation Revocations

**Races:** All

**States:** CA, CO, MI, NJ, ND, SC, WA, WI

**Prior Jail Time:** Excluded

Year	Burglary						Motor Vehicle Theft						Larceny/Theft						Other Property Crime					
				Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.		
	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10
1985	135.0	8.0	59	257	53	22	44.3	1.0	22	123	27	21	411.0	4.6	11	157	34	13	76.3	0.5	6	326	49	4
1986	131.5	8.8	67	289	73	29	51.3	1.1	22	112	27	17	424.8	4.9	12	171	35	20	77.5	0.6	7	271	39	7
1987	126.3	9.3	74	295	74	28	55.4	1.2	22	119	37	28	435.1	5.3	12	183	39	19	76.7	0.6	8	271	48	12
1988	127.6	9.3	73	293	73	27	63.6	1.4	22	116	32	19	449.9	5.9	13	170	35	20	80.0	0.6	7	291	66	26
1989	134.5	9.8	73	281	74	33	71.2	1.7	24	113	42	32	475.0	6.2	13	163	32	21	85.4	0.7	8	304	65	22
1990	129.9	9.5	73	271	74	34	69.4	1.9	28	144	42	30	473.9	6.5	14	147	34	18	90.3	0.7	8	319	69	14
1991	127.8	9.4	73	283	85	31	66.5	2.4	37	146	38	25	466.6	6.7	14	140	29	14	90.0	0.7	8	228	49	15
1992	126.4	9.7	77	276	83	29	65.6	2.7	41	138	37	18	448.1	6.7	15	144	41	15	90.4	0.7	8	265	58	16
1993	122.7	9.7	79	280	85	28	63.0	2.6	41	132	44	20	442.5	6.6	15	139	41	14	95.6	0.7	7	305	74	21
1994	115.4	9.3	80	301	96	30	60.5	2.7	45	135	36	10	442.1	6.7	15	146	30	9	96.0	0.7	7	291	85	21
1995	110.3	9.5	86	313	106	37	56.6	3.1	54	162	42	17	436.6	8.0	18	173	36	11	92.0	0.7	8	299	91	17
1996	105.8	9.5	90	318	110	40	48.3	2.8	58	182	36	10	412.0	8.3	20	171	35	10	88.3	0.7	7	331	116	35
1997	100.4	9.1	90	326	106	40	44.4	2.8	63	169	34	13	396.0	8.0	20	181	37	11	86.9	0.7	8	343	97	27
1998	94.1	9.1	97	337	108	45	39.0	2.9	75	181	38	11	359.3	7.9	22	178	31	7	82.7	0.7	9	353	112	32
1999	83.7	8.2	99	330	96	38	34.0	2.6	75	153	34	10	331.4	7.5	23	179	31	7	79.5	0.7	9	366	95	22
2000	80.8	8.0	99	340	99	39	35.7	2.5	71	144	25	9	316.8	7.2	23	180	31	8	78.7	0.7	9	334	105	36
2001	80.9	8.1	101	314	95		38.6	2.7	71	164	32		306.6	7.4	24	188	32		77.1	0.8	10	339	91	
2002	82.2	8.5	104	314	89		40.6	3.3	80	153	26		307.2	8.0	26	174	25		77.2	0.9	12	331	101	
2003	84.8	8.9	105	297	98		43.4	3.8	88	125	21		311.7	8.4	27	154	22		78.4	0.8	11	315	103	
2004	84.3	9.3	111	286	90		44.0	4.3	98	104	13		312.1	9.1	29	158	16		74.9	1.0	13	280	88	
2005	84.9	9.3	110	314	113		45.8	4.6	101	108	20		297.9	9.1	31	169	28		75.6	1.0	13	299	62	
Percentage Change 1985 - 2000	-40%	0%	66%	32%	86%	76%	-19%	164%	228%	17%	-7%	-55%	-23%	57%	104%	15%	-7%	-39%	3%	38%	34%	2%	114%	787%

See notes to Table 7.

**Table 8c: Sentencing and Time Served Outcomes (Drug-Related And Other Crime)**

**Admission Types:** Court Commitments and Probation Revocations

**Races:** All

**States:** CA, CO, MI, NJ, ND, SC, WA, WI

**Prior Jail Time:** Excluded

Year	Drug Trafficking						Drug Possession/Use						Other Sex Crime						White Collar Crime						Other Crime					
				Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.						Prisoners. left per 1000 adm.		
	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10	Arrests in 1000s	Prs. adm. in 1000s	Adm. per 1000 arr.	2	5	10
1985	34.1	2.7	78	651	252	105	57.0	4.2	73	189	47	29	212.3	2.2	10	108	44	34	82.0	1.9	23	127	17	5	1316.8	4.9	4	154	53	36
1986	34.3	2.9	85	644	262	131	59.8	5.8	98	187	41	23	211.2	3.0	14	91	39	32	86.6	2.2	26	131	18	5	1330.9	5.6	4	168	52	31
1987	34.1	2.9	85	640	260	132	72.5	8.0	110	209	44	21	233.9	4.3	18	92	35	24	87.2	2.2	25	148	17	3	1328.9	6.1	5	173	52	31
1988	32.7	3.2	97	660	267	138	89.9	10.9	121	230	40	20	259.5	6.1	23	100	30	18	90.6	2.3	26	154	16	6	1282.1	7.0	5	175	53	33
1989	33.6	3.1	94	640	280	138	135.2	15.5	114	251	44	22	329.2	8.2	25	96	25	16	93.4	2.6	28	136	16	5	1342.2	9.3	7	136	42	25
1990	36.2	3.5	96	649	305	155	121.0	17.9	148	239	43	20	255.1	8.0	31	101	28	16	96.8	2.6	27	135	18	6	1411.8	11.4	8	115	38	23
1991	34.3	3.6	105	684	335	157	105.9	17.9	169	237	48	20	216.5	7.0	33	99	32	18	101.1	2.7	26	127	18	6	1293.5	11.8	9	127	43	24
1992	34.0	3.9	113	676	329	155	103.1	18.6	180	247	52	20	233.1	7.3	31	106	36	15	101.5	2.9	29	136	23	8	1176.8	12.1	10	129	42	22
1993	32.5	4.2	128	709	343	151	99.6	19.0	191	261	58	16	250.9	7.7	30	96	37	15	107.7	2.9	27	132	27	7	1136.4	13.0	11	145	47	22
1994	30.8	3.8	125	721	345	154	103.0	18.5	180	264	55	14	307.8	8.0	26	94	27	10	113.1	2.8	25	125	23	5	1111.4	12.6	11	159	50	22
1995	27.9	3.8	135	761	385	175	106.9	19.8	185	281	66	16	332.3	9.9	30	119	27	7	112.6	2.6	23	168	32	7	1091.8	11.8	11	200	66	27
1996	29.1	3.9	134	737	376	175	107.4	20.1	187	273	59	14	330.5	10.3	31	124	28	10	116.6	3.2	28	145	29	6	1142.1	12.0	10	201	62	24
1997	30.5	4.2	138	734	398	196	105.3	19.5	185	290	57	12	365.6	11.9	32	125	26	8	112.1	3.7	33	161	30	5	1130.1	12.0	11	226	67	29
1998	29.4	4.3	147	738	388	187	103.3	20.3	196	310	58	12	354.3	12.3	35	131	27	7	102.4	3.8	37	151	20	4	1118.2	12.3	11	232	63	26
1999	28.4	4.1	145	726	384	188	86.9	18.6	213	302	53	10	346.9	12.1	35	131	26	6	97.6	3.5	36	152	19	3	1103.7	12.2	11	226	63	25
2000	27.5	4.5	165	719	384	210	94.7	16.9	179	308	53	11	353.9	11.7	33	138	26	6	97.4	3.5	36	150	18	6	1074.3	12.4	12	240	63	27
2001	27.6	4.4	160	701	378		92.2	16.6	180	310	55		350.6	9.8	28	151	26		96.2	4.0	42	146	20		1032.7	13.7	13	240	64	
2002	28.7	4.7	162	705	403		85.7	16.9	197	304	64		354.8	9.3	26	155	28		101.7	4.6	45	135	18		1022.3	15.2	15	232	69	
2003	27.4	4.7	170	669	387		88.9	17.0	192	280	68		384.7	10.6	27	148	25		95.3	4.9	51	131	19		1019.0	15.7	15	228	71	
2004	27.4	4.5	163	675	380		87.5	16.6	190	285	55		397.6	12.0	30	142	18		97.7	5.0	51	119	15		1004.3	16.4	16	222	70	
2005	26.1	4.7	180	667	417		87.6	16.6	190	269	66		426.8	12.6	30	126	20		96.1	4.9	51	117	15		994.9	16.9	17	258	86	
Percentage Change 1985 - 2000	-19%	70%	111%	10%	52%	100%	66%	306%	144%	63%	13%	-60%	67%	428%	217%	28%	-42%	-83%	19%	84%	55%	18%	8%	11%	-18%	152%	209%	55%	18%	-25%

**Table 9**  
**Number of Persons per 1000 Arrests Who Serve Prison Terms of t**  
**All Races**

<b>Violent Crime</b>		0-1 years	1-2 years	2-3 years	3-4 years	4-5 years	5+ years
Murder & Homicide	1985	38.84	56.06	45.99	36.07	23.24	241.96
	2000	32.02	37.60	30.30	26.00	24.28	487.97
	<b>Ratio</b>	<b>0.82</b>	<b>0.67</b>	<b>0.66</b>	<b>0.72</b>	<b>1.04</b>	<b>2.02</b>
Forcible Rape	1985	9.13	22.06	22.92	20.73	10.36	38.98
	2000	11.17	14.10	20.75	13.57	15.17	81.55
	<b>Ratio</b>	<b>1.22</b>	<b>0.64</b>	<b>0.91</b>	<b>0.65</b>	<b>1.46</b>	<b>2.09</b>
Robbery	1985	27.03	38.08	23.04	15.02	8.70	20.52
	2000	35.03	38.27	25.08	17.78	13.98	70.51
	<b>Ratio</b>	<b>1.30</b>	<b>1.00</b>	<b>1.09</b>	<b>1.18</b>	<b>1.61</b>	<b>3.44</b>
Aggravated Assault	1985	9.96	11.36	5.63	2.50	1.15	2.77
	2000	11.88	10.06	4.54	3.30	2.05	6.77
	<b>Ratio</b>	<b>1.19</b>	<b>0.89</b>	<b>0.81</b>	<b>1.32</b>	<b>1.79</b>	<b>2.45</b>
Other Assault	1985	1.27	1.06	0.31	0.13	0.08	0.13
	2000	3.48	3.07	0.92	0.49	0.33	0.66
	<b>Ratio</b>	<b>2.73</b>	<b>2.89</b>	<b>2.94</b>	<b>3.76</b>	<b>3.97</b>	<b>5.15</b>
<b>Property Crime</b>							
Burglary	1985	27.36	16.79	7.35	3.24	1.50	3.15
	2000	41.08	24.14	13.81	6.13	3.92	9.74
	<b>Ratio</b>	<b>1.50</b>	<b>1.44</b>	<b>1.88</b>	<b>1.89</b>	<b>2.62</b>	<b>3.09</b>
Motor Vehicle Theft	1985	13.66	5.26	1.47	0.45	0.16	0.59
	2000	42.11	18.47	5.63	1.82	0.98	1.79
	<b>Ratio</b>	<b>3.08</b>	<b>3.51</b>	<b>3.84</b>	<b>4.04</b>	<b>6.21</b>	<b>3.06</b>
Larceny/Theft	1985	6.63	2.74	0.83	0.40	0.14	0.37
	2000	12.95	5.64	2.12	0.81	0.45	0.71
	<b>Ratio</b>	<b>1.95</b>	<b>2.06</b>	<b>2.56</b>	<b>2.03</b>	<b>3.23</b>	<b>1.90</b>
Other Property Crime	1985	2.61	1.69	0.98	0.56	0.22	0.31
	2000	3.34	2.36	1.03	0.57	0.36	0.90
	<b>Ratio</b>	<b>1.28</b>	<b>1.40</b>	<b>1.05</b>	<b>1.01</b>	<b>1.60</b>	<b>2.87</b>
<b>Drug-Related Crime</b>							
Drug Trafficking	1985	29.76	29.62	7.20	2.02	1.19	3.46
	2000	63.49	60.16	27.11	12.04	6.46	9.50
	<b>Ratio</b>	<b>2.13</b>	<b>2.03</b>	<b>3.77</b>	<b>5.96</b>	<b>5.41</b>	<b>2.75</b>
Drug Possession/Use	1985	7.23	2.04	0.41	0.18	0.07	0.46
	2000	21.47	6.92	2.33	0.86	0.51	0.85
	<b>Ratio</b>	<b>2.97</b>	<b>3.39</b>	<b>5.62</b>	<b>4.80</b>	<b>7.80</b>	<b>1.84</b>
<b>Other</b>							
Other Sex Crime	1985	9.78	17.46	14.15	11.07	6.00	19.68
	2000	22.45	23.97	24.77	12.82	17.62	63.27
	<b>Ratio</b>	<b>2.29</b>	<b>1.37</b>	<b>1.75</b>	<b>1.16</b>	<b>2.93</b>	<b>3.21</b>
White Collar Crime	1985	14.60	5.78	1.68	0.68	0.22	0.39
	2000	22.70	8.10	3.08	1.16	0.55	0.66
	<b>Ratio</b>	<b>1.55</b>	<b>1.40</b>	<b>1.83</b>	<b>1.70</b>	<b>2.53</b>	<b>1.68</b>
Other Crime	1985	2.41	0.76	0.22	0.11	0.05	0.20
	2000	5.79	3.01	1.16	0.57	0.32	0.72
	<b>Ratio</b>	<b>2.40</b>	<b>3.97</b>	<b>5.23</b>	<b>5.32</b>	<b>6.35</b>	<b>3.65</b>

Underlying data given by National Corrections Reporting Program. Entries are, for every 1000 prisoners, the number arrested for a particular crime (row) who serve prison spells of particular lengths (column).

**Table 10**  
**Number of Persons per 1000 Arrests Who Serve Prison Terms of t**  
**White**

<b>Violent Crime</b>		0-1 years	1-2 years	2-3 years	3-4 years	4-5 years	5+ years
Murder & Homicide	1985	50.03	65.49	46.09	40.02	21.53	257.73
	2000	45.68	52.92	46.13	35.73	32.11	585.26
	<b>Ratio</b>	<b>0.91</b>	<b>0.81</b>	<b>1.00</b>	<b>0.89</b>	<b>1.49</b>	<b>2.27</b>
Forcible Rape	1985	11.51	28.07	28.25	27.72	11.33	41.33
	2000	13.97	16.00	23.69	14.58	16.20	91.92
	<b>Ratio</b>	<b>1.21</b>	<b>0.57</b>	<b>0.84</b>	<b>0.53</b>	<b>1.43</b>	<b>2.22</b>
Robbery	1985	30.77	44.24	27.01	17.47	9.36	18.78
	2000	39.81	42.08	27.12	17.37	13.33	58.54
	<b>Ratio</b>	<b>1.29</b>	<b>0.95</b>	<b>1.00</b>	<b>0.99</b>	<b>1.42</b>	<b>3.12</b>
Aggravated Assault	1985	8.35	10.35	4.95	2.31	0.74	1.70
	2000	10.44	9.43	3.94	2.63	1.89	5.80
	<b>Ratio</b>	<b>1.25</b>	<b>0.91</b>	<b>0.80</b>	<b>1.14</b>	<b>2.57</b>	<b>3.41</b>
Other Assault	1985	1.13	0.96	0.22	0.11	0.05	0.06
	2000	3.18	2.96	0.73	0.44	0.28	0.57
	<b>Ratio</b>	<b>2.82</b>	<b>3.09</b>	<b>3.30</b>	<b>3.85</b>	<b>5.63</b>	<b>9.93</b>
<b>Property Crime</b>							
Burglary	1985	23.85	15.46	6.65	2.96	1.23	2.36
	2000	37.03	21.96	12.29	5.45	3.09	8.31
	<b>Ratio</b>	<b>1.55</b>	<b>1.42</b>	<b>1.85</b>	<b>1.84</b>	<b>2.53</b>	<b>3.52</b>
Motor Vehicle Theft	1985	12.09	4.81	1.35	0.43	0.16	0.49
	2000	40.99	16.96	5.14	1.93	0.80	2.01
	<b>Ratio</b>	<b>3.39</b>	<b>3.52</b>	<b>3.81</b>	<b>4.50</b>	<b>4.88</b>	<b>4.06</b>
Larceny/Theft	1985	4.62	1.97	0.56	0.27	0.08	0.25
	2000	11.68	5.00	1.90	0.62	0.38	0.59
	<b>Ratio</b>	<b>2.53</b>	<b>2.54</b>	<b>3.38</b>	<b>2.30</b>	<b>4.51</b>	<b>2.43</b>
Other Property Crime	1985	2.03	1.50	0.80	0.47	0.18	0.23
	2000	2.84	1.98	0.84	0.55	0.32	0.73
	<b>Ratio</b>	<b>1.40</b>	<b>1.32</b>	<b>1.05</b>	<b>1.17</b>	<b>1.76</b>	<b>3.18</b>
<b>Drug-Related Crime</b>							
Drug Trafficking	1985	29.87	31.97	7.69	1.78	0.76	2.45
	2000	65.60	65.50	26.14	11.26	5.76	8.48
	<b>Ratio</b>	<b>2.20</b>	<b>2.05</b>	<b>3.40</b>	<b>6.32</b>	<b>7.58</b>	<b>3.45</b>
Drug Possession/Use	1985	5.32	1.60	0.22	0.07	0.03	0.36
	2000	17.50	5.49	1.66	0.53	0.31	0.56
	<b>Ratio</b>	<b>3.29</b>	<b>3.44</b>	<b>7.50</b>	<b>7.25</b>	<b>10.15</b>	<b>1.57</b>
<b>Other</b>							
Other Sex Crime	1985	9.45	16.73	13.92	11.14	5.73	17.95
	2000	20.08	23.16	24.88	12.24	17.09	62.01
	<b>Ratio</b>	<b>2.13</b>	<b>1.38</b>	<b>1.79</b>	<b>1.10</b>	<b>2.98</b>	<b>3.45</b>
White Collar Crime	1985	13.03	5.61	1.39	0.69	0.14	0.32
	2000	18.58	7.39	2.84	1.08	0.44	0.58
	<b>Ratio</b>	<b>1.43</b>	<b>1.32</b>	<b>2.04</b>	<b>1.55</b>	<b>3.09</b>	<b>1.80</b>
Other Crime	1985	1.81	0.63	0.17	0.08	0.04	0.13
	2000	4.60	2.54	0.93	0.45	0.22	0.51
	<b>Ratio</b>	<b>2.55</b>	<b>4.02</b>	<b>5.63</b>	<b>5.95</b>	<b>5.96</b>	<b>3.94</b>

See notes to Table 9. See notes to Table 2 for definition of 'white'.

**Table 11**  
**Number of Persons per 1000 Arrests Who Serve Prison Terms of t**  
**Blacks**

<b>Violent Crime</b>		0-1 years	1-2 years	2-3 years	3-4 years	4-5 years	5+ years
Murder & Homicide	1985	26.12	46.08	45.72	30.48	25.40	218.07
	2000	19.13	22.96	15.31	14.88	15.31	366.07
	<b>Ratio</b>	<b>0.73</b>	<b>0.50</b>	<b>0.33</b>	<b>0.49</b>	<b>0.60</b>	<b>1.68</b>
Forcible Rape	1985	5.76	14.51	16.64	12.37	8.53	35.63
	2000	5.82	7.48	14.96	9.56	11.22	59.02
	<b>Ratio</b>	<b>1.01</b>	<b>0.52</b>	<b>0.90</b>	<b>0.77</b>	<b>1.31</b>	<b>1.66</b>
Robbery	1985	24.26	33.60	20.31	13.21	8.26	21.77
	2000	29.34	33.08	22.53	17.77	14.43	81.69
	<b>Ratio</b>	<b>1.21</b>	<b>0.98</b>	<b>1.11</b>	<b>1.35</b>	<b>1.75</b>	<b>3.75</b>
Aggravated Assault	1985	12.93	13.39	7.08	2.99	1.87	4.83
	2000	15.52	11.16	5.88	4.78	2.43	9.00
	<b>Ratio</b>	<b>1.20</b>	<b>0.83</b>	<b>0.83</b>	<b>1.60</b>	<b>1.30</b>	<b>1.87</b>
Other Assault	1985	1.70	1.34	0.56	0.18	0.18	0.33
	2000	4.14	3.19	1.35	0.54	0.48	0.91
	<b>Ratio</b>	<b>2.44</b>	<b>2.38</b>	<b>2.42</b>	<b>3.04</b>	<b>2.72</b>	<b>2.72</b>
<b>Property Crime</b>							
Burglary	1985	38.01	20.90	9.51	4.08	2.31	5.52
	2000	53.16	29.82	18.65	7.83	6.03	13.91
	<b>Ratio</b>	<b>1.40</b>	<b>1.43</b>	<b>1.96</b>	<b>1.92</b>	<b>2.61</b>	<b>2.52</b>
Motor Vehicle Theft	1985	17.22	6.44	1.72	0.52	0.15	0.82
	2000	45.61	22.34	6.90	1.54	1.54	1.44
	<b>Ratio</b>	<b>2.65</b>	<b>3.47</b>	<b>4.01</b>	<b>2.95</b>	<b>10.31</b>	<b>1.75</b>
Larceny/Theft	1985	12.52	5.02	1.62	0.80	0.30	0.76
	2000	16.55	7.47	2.71	1.37	0.67	1.02
	<b>Ratio</b>	<b>1.32</b>	<b>1.49</b>	<b>1.67</b>	<b>1.72</b>	<b>2.20</b>	<b>1.35</b>
Other Property Crime	1985	5.18	2.48	1.77	0.99	0.43	0.71
	2000	5.74	4.21	1.82	0.73	0.58	1.74
	<b>Ratio</b>	<b>1.11</b>	<b>1.69</b>	<b>1.02</b>	<b>0.73</b>	<b>1.36</b>	<b>2.45</b>
<b>Drug-Related Crime</b>							
Drug Trafficking	1985	29.66	26.32	6.56	2.41	1.87	4.95
	2000	60.31	53.42	28.18	12.80	7.16	10.64
	<b>Ratio</b>	<b>2.03</b>	<b>2.03</b>	<b>4.30</b>	<b>5.32</b>	<b>3.82</b>	<b>2.15</b>
Drug Possession/Use	1985	13.78	3.61	1.09	0.54	0.19	0.84
	2000	34.85	11.49	4.55	1.97	1.23	1.82
	<b>Ratio</b>	<b>2.53</b>	<b>3.18</b>	<b>4.19</b>	<b>3.63</b>	<b>6.53</b>	<b>2.18</b>
<b>Other</b>							
Other Sex Crime	1985	12.05	21.05	14.94	11.03	7.30	28.01
	2000	31.42	26.46	23.77	15.50	20.26	67.18
	<b>Ratio</b>	<b>2.61</b>	<b>1.26</b>	<b>1.59</b>	<b>1.40</b>	<b>2.78</b>	<b>2.40</b>
White Collar Crime	1985	18.37	6.18	2.41	0.64	0.40	0.52
	2000	31.05	9.44	3.57	1.20	0.77	0.87
	<b>Ratio</b>	<b>1.69</b>	<b>1.53</b>	<b>1.48</b>	<b>1.87</b>	<b>1.91</b>	<b>1.66</b>
Other Crime	1985	6.23	1.58	0.55	0.30	0.13	0.63
	2000	11.82	5.17	2.24	1.14	0.79	1.74
	<b>Ratio</b>	<b>1.90</b>	<b>3.26</b>	<b>4.09</b>	<b>3.79</b>	<b>5.99</b>	<b>2.75</b>

See notes to Table 9. See notes to Table 1 for definition of 'black'.

## Table 12a

### Adjusted Federal Prison Stocks

Year	Total	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor	Drug	Weapon	Immigration	Other
1989	59171 (100)	1552 (100)	543 (100)	6108 (100)	2398 (100)	579 (100)	1869 (100)	507 (100)	28488 (100)	2802 (100)	2210 (100)	12118 (100)
1990	65526 (111)	1418 (91)	506 (93)	5931 (97)	2258 (94)	508 (88)	1597 (85)	427 (84)	35034 (123)	3533 (126)	1987 (90)	12327 (102)
1991*												
1992	80259 (136)	1145 (74)	219 (40)	6755 (111)	2509 (105)	391 (68)	1482 (79)	378 (75)	53553 (188)	6240 (223)	2407 (109)	2051 (17)
1993	89587 (151)	939 (61)	168 (31)	7108 (116)	2562 (107)	273 (47)	1110 (59)	292 (58)	53079 (186)	7058 (252)	2351 (106)	14647 (121)
1994	95035 (161)	1086 (70)	133 (25)	9413 (154)	702 (29)	204 (35)	946 (51)	242 (48)	56978 (200)	7631 (272)	2801 (127)	14899 (123)
1995	100250 (169)	1208 (78)	136 (25)	9472 (155)	711 (30)	200 (35)	847 (45)	224 (44)	59683 (210)	8420 (301)	3867 (175)	15482 (128)
1996	105544 (178)	1235 (80)	140 (26)	9492 (155)	735 (31)	206 (36)	823 (44)	200 (40)	62860 (221)	8765 (313)	5098 (231)	15990 (132)
1997	112973 (191)	1159 (75)	129 (24)	9664 (158)	765 (32)	207 (36)	798 (43)	242 (48)	66920 (235)	9228 (329)	6227 (282)	17634 (146)
1998	123041 (208)	1518 (98)	149 (27)	9910 (162)	948 (40)	281 (49)	866 (46)	232 (46)	71177 (250)	9875 (352)	8393 (380)	19692 (162)
1999**	135246 (229)	1700 (110)	972 (179)	10615 (174)	1017 (42)	354 (61)	827 (44)	202 (40)	77572 (272)	10773 (385)	11525 (522)	19689 (162)
2000	145416 (246)	1264 (81)	907 (167)	10625 (174)	959 (40)	315 (54)	1038 (56)	256 (51)	82518 (290)	11977 (427)	15377 (696)	20179 (167)
2001	156993 (265)	1426 (92)	1128 (208)	10906 (179)	1067 (45)	296 (51)	1099 (59)	269 (53)	88870 (312)	13985 (499)	17279 (782)	20668 (171)
2002	163528 (276)	1430 (92)	1135 (209)	10959 (179)	1092 (46)	263 (45)	1028 (55)	257 (51)	92667 (325)	15692 (560)	17802 (806)	21203 (175)
2003	173059 (292)	1427 (92)	1150 (212)	10843 (178)	1090 (45)	270 (47)	1035 (55)	249 (49)	97381 (342)	18178 (649)	19187 (868)	22251 (184)
2004	180328 (305)	1515 (98)	1267 (234)	10431 (171)	1051 (44)	207 (36)	826 (44)	189 (37)	99968 (351)	22114 (789)	20983 (950)	21775 (180)
2005	187618 (317)	1527 (98)	1350 (249)	10237 (168)	1089 (45)	220 (38)	777 (42)	166 (33)	101969 (358)	24986 (892)	22641 (1025)	22656 (187)
2006	193046 (326)	1551 (100)	1343 (248)	9926 (163)	1128 (47)	203 (35)	705 (38)	140 (28)	104949 (368)	27264 (973)	22228 (1006)	23611 (195)
2007	199618 (337)	1585 (102)	1326 (244)	9347 (153)	1082 (45)	200 (35)	681 (36)	121 (24)	108552 (381)	28999 (1035)	22586 (1022)	25139 (207)
2008	201280 (340)	1590 (102)	1316 (243)	9025 (148)	1118 (47)	202 (35)	673 (36)	114 (22)	107117 (376)	30381 (1084)	22494 (1018)	27250 (225)
2009	208118 (352)	1558 (100)	1282 (236)	8684 (142)	1131 (47)	184 (32)	749 (40)	124 (24)	109229 (383)	31487 (1124)	24863 (1125)	28826 (238)
2010	209771 (355)	1568 (101)	1251 (231)	8479 (139)	1205 (50)	167 (29)	775 (41)	117 (23)	109213 (383)	32239 (1151)	24197 (1095)	30560 (252)

This table presents estimates of prisoners under the jurisdiction of the federal system, detailed by offense. It is a combination of data from the Compendium of Federal Justice Statistics (which publishes details by offense but uses the narrower "in custody" criterion) and National Prison Statistics (which publishes figures for "jurisdiction" but does not detail by offense). The "Total" column lists NPS totals while the offense-specific columns present the CFJS data adjusted by a factor of (NPS total)/(CFJS total) for that year. \*There are currently no CFJS data available for 1991. \*\*In 1999, the 'Rape' category was replaced by 'Sexual Assault', which carries a broader definition.



**Table 12b****Federal Prison Admissions**

<b>Year</b>	<b>Murder</b>	<b>Rape</b>	<b>Robbery</b>	<b>Assault</b>	<b>Burglary</b>	<b>Larceny</b>	<b>Motor</b>	<b>Drug</b>	<b>Weapons</b>	<b>Immigration</b>	<b>Other</b>	<b>Total</b>
1989	250 (100)	90 (100)	1484 (100)	669 (100)	191 (100)	1750 (100)	224 (100)	13803 (100)	1736 (100)	7128 (100)	11144 (100)	38460 (100)
1990	203 (81)	54 (60)	1788 (120)	712 (106)	214 (112)	1798 (103)	269 (120)	16355 (118)	2338 (135)	7862 (110)	12019 (108)	43612 (113)
1991*												
1992	194 (77)	31 (34)	2271 (153)	848 (127)	212 (111)	1776 (101)	275 (113)	19193 (139)	3172 (183)	5528 (78)	10421 (94)	43316 (113)
1993	181 (72)	19 (21)	2618 (176)	938 (140)	169 (88)	1533 (88)	250 (112)	19900 (144)	3412 (197)	5499 (77)	12499 (112)	47832 (124)
1994	218 (87)	22 (24)	3387 (228)	538 (80)	184 (96)	1584 (91)	227 (101)	19098 (138)	3206 (185)	5514 (77)	12931 (116)	46909 (122)
1995	271 (108)	29 (32)	2933 (198)	551 (82)	158 (83)	1394 (80)	207 (92)	17611 (128)	3019 (174)	5873 (82)	13047 (117)	45093 (117)
1996	180 (72)	21 (23)	2851 (192)	605 (90)	134 (70)	1367 (78)	178 (79)	19362 (140)	2915 (168)	6216 (87)	13600 (122)	47609 (124)
1997	214 (87)	19 (21)	3139 (212)	664 (99)	204 (107)	1438 (82)	208 (93)	21403 (155)	2843 (164)	7300 (102)	14160 (127)	51592 (134)
1998	296 (118)	28 (31)	3356 (226)	792 (118)	308 (161)	1466 (84)	181 (81)	23422 (170)	3241 (187)	9762 (137)	15363 (138)	58215 (151)
1999**	351 (140)	426 (473)	3282 (221)	870 (130)	382 (200)	1394 (80)	175 (78)	26500 (192)	3544 (204)	11857 (166)	15205 (136)	63986 (166)
2000	254 (102)	438 (487)	2997 (202)	718 (107)	261 (137)	1359 (78)	170 (76)	26447 (191)	4032 (232)	13151 (184)	15091 (135)	64918 (169)
2001	274 (110)	449 (499)	2882 (194)	751 (112)	216 (113)	1367 (78)	179 (80)	27312 (198)	4846 (279)	13167 (185)	15211 (136)	66654 (173)
2002	277 (111)	488 (542)	3061 (206)	824 (123)	236 (124)	1410 (81)	160 (71)	28481 (206)	5540 (319)	11706 (164)	15694 (141)	67877 (176)
2003	257 (103)	499 (554)	3020 (203)	790 (118)	237 (124)	1419 (81)	143 (64)	29993 (217)	7033 (405)	13100 (184)	16184 (145)	72675 (189)
2004	303 (121)	546 (607)	2883 (194)	916 (137)	216 (113)	1369 (78)	119 (53)	28865 (209)	8509 (490)	15257 (214)	15868 (142)	74851 (195)
2005	263 (105)	579 (643)	2885 (194)	921 (138)	186 (97)	1198 (68)	113 (50)	28999 (210)	9440 (544)	17738 (249)	16119 (145)	78441 (204)
2006	267 (107)	446 (496)	2664 (180)	871 (130)	165 (86)	1067 (61)	77 (34)	29442 (213)	9539 (549)	18838 (264)	15871 (142)	79247 (187)
2007	247 (99)	321 (357)	1888 (127)	613 (92)	123 (64)	739 (42)	68 (30)	27210 (197)	8499 (490)	18806 (264)	13419 (120)	71933 (187)
2008	202 (81)	300 (333)	1728 (116)	617 (92)	113 (59)	711 (41)	102 (46)	25786 (187)	8495 (489)	19785 (278)	13824 (124)	71663 (186)
2009	172 (69)	295 (328)	1604 (108)	678 (101)	101 (53)	764 (44)	117 (52)	25708 (186)	8544 (492)	22563 (317)	13790 (124)	74336 (193)
2010	199 (80)	268 (298)	1619 (109)	660 (99)	85 (45)	744 (43)	84 (38)	24508 (178)	8336 (480)	21520 (302)	13949 (125)	71972 (187)

This table contains data on federal prison admissions from the annual Compendia of Federal Justice Statistics. The number in parentheses is the percent ratio of that year's observation with the observation for 1989. \*There are currently no data available for 1991. \*\*In 1999, the 'Rape' category was replaced by 'Sexual Assault', which carries a broader definition.

**Table 12c****Federal Prison Releases**

<b>Year</b>	<b>Murder</b>	<b>Rape</b>	<b>Robbery</b>	<b>Assault</b>	<b>Burglary</b>	<b>Larceny</b>	<b>Motor</b>	<b>Drug</b>	<b>Weapons</b>	<b>Immigration</b>	<b>Other</b>	<b>Total</b>
1989	150 (100)	35 (100)	1494 (100)	644 (100)	171 (100)	1826 (100)	79 (100)	9311 (100)	1278 (100)	6816 (100)	32606 (100)	32606 (100)
1990	349 (233)	96 (274)	2056 (138)	878 (136)	286 (167)	2069 (113)	348 (441)	11194 (120)	1754 (137)	8097 (119)	39191 (120)	39191 (120)
1991*												
1992	443 (295)	83 (237)	2272 (152)	940 (146)	283 (165)	1977 (108)	295 (373)	13096 (141)	2002 (157)	5589 (82)	39807 (122)	36170 (111)
1993	229 (153)	40 (114)	2205 (148)	860 (134)	208 (122)	1704 (93)	282 (357)	14425 (155)	2010 (157)	5003 (73)	39973 (123)	39973 (123)
1994	218 (145)	30 (86)	2731 (183)	524 (81)	197 (115)	1660 (91)	267 (338)	15370 (165)	2140 (167)	5051 (74)	40812 (125)	40812 (125)
1995	174 (116)	29 (83)	2743 (184)	547 (85)	161 (94)	1484 (81)	220 (278)	15308 (164)	2227 (174)	4889 (72)	40306 (124)	40306 (124)
1996	211 (141)	22 (63)	2901 (194)	580 (90)	140 (82)	1407 (77)	206 (261)	16722 (180)	2650 (207)	5092 (75)	42753 (131)	42964 (132)
1997	196 (131)	28 (80)	2919 (195)	630 (98)	181 (106)	1443 (79)	185 (234)	17535 (188)	2526 (198)	6355 (93)	45027 (138)	45027 (138)
1998	212 (141)	24 (69)	2954 (198)	626 (97)	204 (119)	1418 (78)	179 (227)	18575 (199)	2561 (200)	7792 (114)	48545 (149)	48545 (149)
1999**	207 (138)	374 (1069)	2968 (199)	728 (113)	258 (151)	1419 (78)	195 (247)	20578 (221)	2751 (215)	8851 (130)	52472 (161)	52472 (161)
2000	281 (187)	421 (1203)	2982 (200)	733 (114)	253 (148)	1373 (75)	171 (216)	22058 (237)	3082 (241)	10143 (149)	55876 (171)	55876 (171)
2001	279 (186)	428 (1223)	2934 (196)	1765 (274)	259 (151)	1352 (74)	167 (211)	23252 (250)	3253 (255)	11711 (172)	59321 (182)	59321 (182)
2002	261 (174)	462 (1320)	2901 (194)	795 (123)	254 (149)	1481 (81)	181 (229)	24370 (262)	3892 (305)	10994 (161)	60832 (187)	60832 (187)
2003	236 (157)	476 (1360)	3058 (205)	808 (125)	242 (142)	1389 (76)	156 (197)	25372 (272)	4761 (373)	11946 (175)	63732 (195)	63732 (195)
2004	250 (167)	443 (1266)	3006 (201)	806 (125)	205 (120)	1327 (73)	120 (152)	25697 (276)	5627 (440)	13769 (202)	66235 (203)	34381 (105)
2005	230 (153)	432 (1234)	2839 (190)	798 (124)	172 (101)	1219 (67)	124 (157)	26209 (281)	6594 (516)	16050 (235)	69558 (213)	69558 (213)
2006	232 (155)	466 (1331)	2702 (181)	765 (119)	179 (105)	1090 (60)	98 (124)	24971 (268)	7089 (555)	17526 (257)	69557 (213)	69557 (213)
2007	185 (123)	334 (954)	2284 (153)	590 (92)	123 (72)	783 (43)	89 (113)	23628 (254)	6900 (540)	18842 (276)	65626 (201)	65626 (201)
2008	203 (135)	317 (906)	2003 (134)	572 (89)	115 (67)	728 (40)	105 (133)	26556 (285)	7448 (583)	20564 (302)	70327 (216)	70327 (216)
2009	199 (133)	333 (951)	1849 (124)	610 (95)	114 (67)	712 (39)	111 (141)	23651 (254)	7638 (598)	21256 (312)	68767 (211)	68767 (211)
2010	213 (142)	296 (846)	1805 (121)	592 (92)	97 (57)	739 (40)	113 (143)	24831 (267)	8073 (632)	22752 (334)	72116 (221)	72116 (221)

This table contains data on federal prison releases from the annual Compendia of Federal Justice Statistics. The number in parentheses is the percent ratio of that year's observation with the observation for 1989. \*There are currently no data available for 1991. \*\*In 1999, the 'Rape' category was replaced by 'Sexual Assault', which carries a broader definition.

## Table 13. Ratio of Median Black and Median White Weekly Wages, Males Only

Mixing Over All Nonworkers

Years of Potential Experience	Year	10/25	15/15	25/10	Raw	Percent Workers		Percent Inst. Nonworkers		Percent Other Nonworkers	
						Black	White	Black	White	Black	White
6-10	1960	0.567	0.567	0.588	0.606	90.1%	96.7%	3.6%	0.9%	6.3%	2.4%
	1970	0.642	0.666	0.669	0.689	89.6%	96.4%	3.8%	0.7%	6.6%	2.8%
	1980	0.653	0.673	0.695	0.716	82.1%	95.2%	4.2%	0.8%	13.7%	4.1%
	1990	0.660	0.670	0.719	0.738	78.8%	94.0%	6.9%	1.0%	14.3%	5.0%
	2000	0.696	0.715	0.756	0.828	74.1%	92.0%	10.9%	1.7%	15.1%	6.3%
	2007	0.722	0.733	0.777	0.797	77.7%	92.5%	8.1%	1.5%	14.1%	6.1%
	2010	0.586	0.607	0.682	0.801	68.6%	88.3%	8.6%	1.6%	22.8%	10.1%
11-15	1960	0.578	0.578	0.585	0.602	91.1%	97.2%	3.3%	0.9%	5.6%	1.9%
	1970	0.665	0.683	0.684	0.669	91.1%	97.0%	2.9%	0.7%	6.0%	2.2%
	1980	0.642	0.666	0.697	0.712	83.7%	95.1%	3.7%	0.7%	12.5%	4.1%
	1990	0.615	0.631	0.680	0.712	79.5%	94.2%	6.4%	0.9%	14.1%	4.9%
	2000	0.697	0.720	0.770	0.785	76.2%	92.4%	10.6%	1.8%	13.2%	5.9%
	2007	0.673	0.711	0.737	0.750	79.6%	93.2%	8.3%	1.5%	12.1%	5.3%
	2010	0.593	0.651	0.694	0.750	71.0%	89.2%	9.0%	1.7%	20.0%	9.1%
16-20	1960	0.593	0.593	0.611	0.622	90.9%	96.8%	3.5%	1.0%	5.6%	2.3%
	1970	0.654	0.668	0.669	0.654	91.2%	96.9%	2.5%	0.7%	6.2%	2.4%
	1980	0.652	0.669	0.684	0.718	84.5%	94.9%	2.7%	0.7%	12.8%	4.5%
	1990	0.601	0.634	0.667	0.709	79.8%	93.7%	5.1%	0.8%	15.1%	5.5%
	2000	0.673	0.694	0.722	0.787	76.4%	91.7%	9.3%	1.7%	14.3%	6.6%
	2007	0.693	0.734	0.765	0.791	80.8%	92.5%	7.5%	1.3%	11.7%	6.1%
	2010	0.651	0.690	0.721	0.782	74.2%	89.0%	7.1%	1.5%	18.7%	9.5%
21-25	1960	0.575	0.575	0.589	0.619	90.4%	96.3%	3.0%	1.0%	6.6%	2.7%
	1970	0.640	0.647	0.661	0.641	89.8%	96.1%	2.5%	0.8%	7.8%	3.1%
	1980	0.653	0.663	0.668	0.705	84.1%	93.9%	1.8%	0.6%	14.1%	5.6%
	1990	0.638	0.658	0.685	0.750	79.9%	92.6%	3.9%	0.8%	16.2%	6.7%
	2000	0.608	0.643	0.684	0.750	75.2%	90.7%	7.6%	1.4%	17.2%	7.8%
	2007	0.652	0.680	0.711	0.742	79.7%	90.9%	6.4%	1.3%	13.9%	7.8%
	2010	0.633	0.667	0.689	0.760	71.8%	87.5%	7.0%	1.3%	21.2%	11.2%

This table was created using 1960-2000 census data and the 2007 and 2010 ACS from IPUMS. The IPUMS website is available at <http://usa.ipums.org/usa>. Column X/Y assumes that X% of black non-workers had latent wages above the median of the black wage offer distribution, and Y% of white non-workers had latent wages above the median of the white wage offer distribution. The Raw column presents the ratio of median observed black earnings to median observed white earnings. The results are equivalent those one would obtain by using a (50/50) imputation procedure. This table does not distinguish between institutionalized nonworkers and other nonworkers.

**Table 14. Ratio of Median Black and Median White Weekly Wages, Males Only**

Mixing Over Only Non-Institutionalized Nonworkers

Years of Potential Experience	Year	10/25	15/15	25/10	Raw	Percent Workers		Percent Inst. Nonworkers		Percent Other Nonworkers	
						Black	White	Black	White	Black	White
6-10	1960	0.553	0.558	0.567	0.606	90.1%	96.7%	3.6%	0.9%	6.3%	2.4%
	1970	0.643	0.650	0.666	0.689	89.6%	96.4%	3.8%	0.7%	6.6%	2.8%
	1980	0.646	0.661	0.688	0.716	82.1%	95.2%	4.2%	0.8%	13.7%	4.1%
	1990	0.644	0.668	0.675	0.738	78.8%	94.0%	6.9%	1.0%	14.3%	5.0%
	2000	0.679	0.706	0.733	0.828	74.1%	92.0%	10.9%	1.7%	15.1%	6.3%
	2007	0.701	0.729	0.733	0.797	77.7%	92.5%	8.1%	1.5%	14.1%	6.1%
	2010	0.567	0.592	0.652	0.801	68.6%	88.3%	8.6%	1.6%	22.8%	10.1%
11-15	1960	0.578	0.578	0.581	0.602	91.1%	97.2%	3.3%	0.9%	5.6%	1.9%
	1970	0.669	0.672	0.684	0.669	91.1%	97.0%	2.9%	0.7%	6.0%	2.2%
	1980	0.635	0.657	0.688	0.712	83.7%	95.1%	3.7%	0.7%	12.5%	4.1%
	1990	0.608	0.637	0.649	0.712	79.5%	94.2%	6.4%	0.9%	14.1%	4.9%
	2000	0.667	0.695	0.716	0.785	76.2%	92.4%	10.6%	1.8%	13.2%	5.9%
	2007	0.679	0.684	0.711	0.750	79.6%	93.2%	8.3%	1.5%	12.1%	5.3%
	2010	0.591	0.613	0.667	0.750	71.0%	89.2%	9.0%	1.7%	20.0%	9.1%
16-20	1960	0.591	0.593	0.593	0.622	90.9%	96.8%	3.5%	1.0%	5.6%	2.3%
	1970	0.654	0.654	0.669	0.654	91.2%	96.9%	2.5%	0.7%	6.2%	2.4%
	1980	0.644	0.657	0.684	0.718	84.5%	94.9%	2.7%	0.7%	12.8%	4.5%
	1990	0.600	0.618	0.652	0.709	79.8%	93.7%	5.1%	0.8%	15.1%	5.5%
	2000	0.659	0.683	0.694	0.787	76.4%	91.7%	9.3%	1.7%	14.3%	6.6%
	2007	0.698	0.713	0.738	0.791	80.8%	92.5%	7.5%	1.3%	11.7%	6.1%
	2010	0.635	0.667	0.731	0.782	74.2%	89.0%	7.1%	1.5%	18.7%	9.5%
21-25	1960	0.571	0.575	0.578	0.619	90.4%	96.3%	3.0%	1.0%	6.6%	2.7%
	1970	0.640	0.647	0.660	0.641	89.8%	96.1%	2.5%	0.8%	7.8%	3.1%
	1980	0.649	0.665	0.667	0.705	84.1%	93.9%	1.8%	0.6%	14.1%	5.6%
	1990	0.641	0.645	0.678	0.750	79.9%	92.6%	3.9%	0.8%	16.2%	6.7%
	2000	0.615	0.624	0.658	0.750	75.2%	90.7%	7.6%	1.4%	17.2%	7.8%
	2007	0.654	0.667	0.689	0.742	79.7%	90.9%	6.4%	1.3%	13.9%	7.8%
	2010	0.622	0.649	0.674	0.760	71.8%	87.5%	7.0%	1.3%	21.2%	11.2%

See notes to Table 13. This table assumes all institutionalized nonworkers have wages below the median of their respective potential wage offer distributions.

**Table 15. Ratio of Black and White Weekly Wages, Males Only**

Mixing Over Only Non-Institutionalized Nonworkers

Years of Potential Experience	Year	75th Percentile				90th Percentile	
		0/5	0/0	5/0	Raw	0/0	Raw
6-10	1960	0.669	0.669	0.669	0.675	0.684	0.696
	1970	0.721	0.721	0.727	0.761	0.716	0.732
	1980	0.715	0.715	0.731	0.795	0.796	0.820
	1990	0.684	0.692	0.707	0.764	0.721	0.773
	2000	0.736	0.748	0.748	0.826	0.738	0.801
	2007	0.729	0.740	0.740	0.800	0.730	0.800
	2010	0.660	0.667	0.688	0.800	0.694	0.787
11-15	1960	0.645	0.645	0.645	0.669	0.654	0.657
	1970	0.697	0.697	0.697	0.699	0.688	0.708
	1980	0.743	0.746	0.754	0.779	0.767	0.797
	1990	0.681	0.687	0.687	0.742	0.720	0.751
	2000	0.700	0.700	0.700	0.780	0.680	0.741
	2007	0.683	0.683	0.700	0.744	0.674	0.700
	2010	0.650	0.650	0.667	0.754	0.652	0.714
16-20	1960	0.669	0.673	0.673	0.693	0.647	0.646
	1970	0.668	0.668	0.668	0.668	0.626	0.657
	1980	0.749	0.749	0.760	0.784	0.756	0.773
	1990	0.701	0.709	0.731	0.762	0.707	0.741
	2000	0.689	0.689	0.691	0.727	0.661	0.704
	2007	0.704	0.716	0.732	0.771	0.714	0.727
	2010	0.671	0.671	0.686	0.743	0.698	0.742
21-25	1960	0.653	0.653	0.655	0.662	0.640	0.634
	1970	0.668	0.668	0.668	0.669	0.604	0.620
	1980	0.731	0.731	0.731	0.755	0.718	0.743
	1990	0.698	0.702	0.715	0.763	0.706	0.723
	2000	0.672	0.684	0.702	0.749	0.682	0.711
	2007	0.694	0.694	0.694	0.733	0.661	0.667
	2010	0.662	0.681	0.694	0.734	0.652	0.717

See notes to Table 13. The Raw column for the x percentile presents the ratio of the x percentile of observed black earnings to the x percentile of observed white earnings. The results are equivalent to those one would obtain by using a (x/x) imputation procedure. This table assumes all institutionalized nonworkers have wages below the median of their respective potential wage offer distributions.

**Table 16a**  
**Male Prison Admissions and Arrests per 1000 Persons**  
**All Offenses**  
**CA, CO, MI, NJ, ND, SC, WA, and WI**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1936-1940						0.83 46.93
1941-1945					1.15 59.54	1.23 45.54
1946-1950				1.73 76.06	2.01 62.39	1.51 40.63
1951-1955			2.41 99.32	3.15 90.34	2.52 60.92	1.77 41.36
1956-1960		3.23 131.49	4.44 120.02	3.81 83.01	2.86 58.18	2.40 43.48
1961-1965	4.16 175.75	5.50 152.02	4.81 103.12	3.80 71.15	3.42 56.27	
1966-1970	7.13 194.88	6.26 124.38	4.33 78.63	4.17 63.27		
1971-1975	7.87 161.42	4.82 94.10	4.75 71.92			
1976-1980	7.34 143.78	6.26 97.17				
1981-1985	7.55 138.28					

The top number is prison admission rate and bottom number is arrest rate for the given cohort. Population data acquired from 1960-2000 Census. Admission data are from National Corrections Reporting Program. Arrest data are from FBI Uniform Crime Reporting.

**Table 16b**  
**Male Prison Admissions and Arrests per 1000 Persons**  
**Property Offenses**  
**CA, CO, MI, NJ, ND, SC, WA, and WI**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1936-1940						0.16 5.10
1941-1945					0.24 7.28	0.20 5.84
1946-1950				0.46 11.19	0.38 8.85	0.33 5.48
1951-1955			0.73 16.09	0.71 14.04	0.59 9.35	0.32 4.68
1956-1960		1.07 21.80	1.01 18.74	0.92 13.20	0.57 7.17	0.52 5.72
1961-1965	1.72 34.61	1.34 24.45	1.21 17.14	0.80 9.19	0.78 8.08	
1966-1970	1.89 37.60	1.50 20.97	0.91 10.62	0.96 9.70		
1971-1975	1.87 29.48	0.93 12.10	1.06 10.67			
1976-1980	1.78 21.91	1.38 14.31				
1981-1985	2.06 22.35					

The top number is prison admission rate and bottom number is arrest rate for the given cohort. Population data acquired from 1960-2000 Census. Admission data are from National Corrections Reporting Program. Arrest data are from FBI Uniform Crime Reporting.

**Table 16c**  
**Male Prison Admissionw and Arrests per 1000**  
**Drug Offenses**  
**CA, CO, MI, NJ, ND, SC, WA, and WI**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1936-1940						0.15 2.22
1941-1945					0.21 3.76	0.38 3.98
1946-1950				0.30 6.57	0.66 7.22	0.53 5.39
1951-1955			0.42 10.74	1.05 12.16	0.89 9.62	0.66 6.85
1956-1960		0.56 17.01	1.48 17.42	1.31 14.59	1.01 10.71	0.79 9.43
1961-1965	0.46 24.29	1.84 22.73	1.65 18.57	1.29 13.73	1.05 12.73	
1966-1970	2.37 27.68	2.28 22.71	1.54 15.21	1.27 14.39		
1971-1975	2.43 29.85	1.74 18.52	1.53 16.22			
1976-1980	2.04 30.62	1.98 22.95				
1981-1985	1.83 33.08					

The top number is prison admission rate and bottom number is arrest rate for the given cohort. Population data acquired from 1960-2000 Census. Admission data are from National Corrections Reporting Program. Arrest data are from FBI Uniform Crime Reporting.



**Table A1**  
**Employment and Institutionalization Rates**  
**Black Females**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.541 0.013
1915-1919					0.535 0.008	
1920-1924				0.511 0.008		0.558 0.007
1925-1929			0.457 0.008		0.571 0.005	
1930-1934		0.409 0.008		0.575 0.007		0.620 0.004
1935-1939	0.381 0.006		0.548 0.005		0.653 0.003	
1940-1944		0.543 0.006		0.662 0.004		0.703 0.005
1945-1949	0.476 0.006		0.659 0.005		0.733 0.005	
1950-1954		0.606 0.005		0.711 0.008		0.671 0.007
1955-1959	0.465 0.004		0.668 0.009		0.677 0.009	
1960-1964		0.626 0.011		0.678 0.011		0.694 0.006
1965-1969	0.530 0.008		0.680 0.011		0.711 0.007	
1970-1974		0.665 0.009		0.716 0.006		
1975-1979	0.572 0.007		0.691 0.007			
1980-1984		0.652 0.008				
1985-1989	0.529 0.005					

The top number is the employment rate and the bottom number is the institutionalization rate. This table was created using 1960-2000 census data and the 2007 and 2010 ACS from IPUMS. The IPUMS website is available at <http://usa.ipums.org/usa>. The IPUMS variables for employment and institutionalization were EMPSTAT (=1) and GQTYPE (=1 for 1990 and later; =2, 3, 4 for 1980 and earlier). After the 1980 census, the data no longer distinguish between different types of institutionalization. Thus, to be consistent we calculate total institutional rates for all years. All samples are weighted by the IPUMS variable PERWT. Black females are all those with race 'black'.

**Table A2**  
**Employment and Institutionalization Rates**  
**White Females**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.444 0.008
1915-1919					0.414 0.006	
1920-1924				0.359 0.005		0.506 0.005
1925-1929			0.312 0.004		0.489 0.004	
1930-1934		0.322 0.004		0.443 0.003		0.590 0.003
1935-1939	0.439 0.006		0.399 0.003		0.615 0.003	
1940-1944		0.432 0.003		0.598 0.002		0.718 0.003
1945-1949	0.529 0.004		0.589 0.002		0.745 0.002	
1950-1954		0.630 0.002		0.723 0.002		0.743 0.002
1955-1959	0.627 0.002		0.697 0.002		0.731 0.002	
1960-1964		0.711 0.003		0.702 0.003		0.717 0.002
1965-1969	0.682 0.002		0.687 0.003		0.708 0.003	
1970-1974		0.705 0.002		0.693 0.003		
1975-1979	0.669 0.002		0.695 0.003			
1980-1984		0.706 0.004				
1985-1989	0.639 0.003					

See notes for Table A1. White females are all those with race 'white' or Hispanics reporting 'other race' (not white, black, Asian and Pacific Islander, or Native American).

**Table A3**  
**Employment and Institutionalization Rates**  
**Black Females With Less Than HS**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.514 0.013
1915-1919					0.500 0.007	
1920-1924				0.478 0.010		0.498 0.008
1925-1929			0.409 0.008		0.510 0.005	
1930-1934		0.358 0.010		0.497 0.010		0.509 0.006
1935-1939	0.315 0.007		0.459 0.006		0.523 0.005	
1940-1944		0.426 0.010		0.511 0.007		0.506 0.008
1945-1949	0.340 0.011		0.460 0.011		0.498 0.011	
1950-1954		0.380 0.013		0.439 0.020		0.403 0.015
1955-1959	0.283 0.011		0.377 0.025		0.402 0.026	
1960-1964		0.289 0.030		0.397 0.036		0.379 0.026
1965-1969	0.231 0.030		0.381 0.050		0.392 0.031	
1970-1974		0.356 0.038		0.414 0.030		
1975-1979	0.356 0.025		0.431 0.031			
1980-1984		0.325 0.034				
1985-1989	0.263 0.020					

See notes for Table A1. This table was created using only data for Black females whose value for the IPUMS variable EDUCD was either less than or equal to 50 (completed less than 12 years of schooling).

**Table A4**  
**Employment and Institutionalization Rates**  
**White Females With Less Than HS**

Birth Year	Age					
	20-24	25-29	30-34	35-39	40-44	45-49
1910-1914						0.388 0.011
1915-1919					0.372 0.009	
1920-1924				0.332 0.008		0.442 0.009
1925-1929			0.288 0.007		0.443 0.008	
1930-1934		0.262 0.008		0.411 0.006		0.469 0.006
1935-1939	0.286 0.011		0.360 0.008		0.480 0.007	
1940-1944		0.318 0.008		0.471 0.007		0.492 0.009
1945-1949	0.332 0.012		0.429 0.009		0.499 0.009	
1950-1954		0.392 0.010		0.484 0.011		0.437 0.008
1955-1959	0.386 0.009		0.462 0.010		0.461 0.008	
1960-1964		0.417 0.011		0.447 0.009		0.465 0.007
1965-1969	0.382 0.009		0.408 0.009		0.478 0.007	
1970-1974		0.383 0.007		0.446 0.010		
1975-1979	0.406 0.007		0.424 0.009			
1980-1984		0.373 0.015				
1985-1989	0.381 0.015					

See notes for Table A3.

## Table A5 - Internal and External Consistency Checks

	Internal Consistency Checks (NCRP)		External Consistency Checks (NCRP vs. NPS)	
	Adm. Vs. RIs	Flow vs. Stock	NCRP vs. NPS Flow	NCRP Flow vs. Change in NPS stock
AL	X	-	X	X
AK	X	X	X	X
<b>CA</b>	<b>√</b>	<b>(√)</b>	<b>√</b>	<b>√</b>
<b>CO</b>	<b>(√)</b>	<b>(√)</b>	<b>X</b>	<b>(√)</b>
FL	√	X	X	X
GA	(√)	X	X	√
HI	X	-	X	X
<b>IL</b>	<b>√</b>	-	<b>√</b>	<b>√</b>
IA	X	X	X	X
KY	(√)	-	X	X
MD	X	X	X	X
<b>MI</b>	<b>√</b>	<b>√</b>	<b>√</b>	<b>√</b>
MN	(√)	(√)	(√)	X
MS	X	-	X	X
MO	X	X	X	X
<b>NE</b>	<b>√</b>	-	<b>X</b>	<b>(√)</b>
NV	X	-	X	X
NH	√	-	X	X
<b>NJ</b>	<b>√</b>	<b>√</b>	<b>√</b>	<b>√</b>
<b>NY</b>	<b>√</b>	<b>√</b>	<b>√</b>	<b>√</b>
NC	X	(√)	X	(√)
<b>ND</b>	<b>√</b>	-	<b>X</b>	<b>(√)</b>
OH	√	-	X	X
OK	X	X	X	X
OR	X	√	X	(√)
PA	X	X	(√)	(√)
<b>SC</b>	<b>√</b>	<b>(√)</b>	<b>X</b>	<b>√</b>
TN	X	X	X	X
TX	X	(√)	X	X
<b>UT</b>	<b>√</b>	-	<b>√</b>	<b>√</b>
VA	√	-	(√)	X
<b>WA</b>	<b>√</b>	<b>(√)</b>	<b>X</b>	<b>√</b>
WV	X	-	X	(√)
<b>WI</b>	<b>√</b>	<b>(√)</b>	<b>X</b>	<b>(√)</b>

Notes:

"√" indicates that the state passes the test.

"(√)" indicates minor problems in only a few years.

"X" indicates major inconsistency problems in many years.

"-" indicates that the test could not be performed since the state did not report any stock data.

## Appendix

### A National Corrections Reporting Program

#### Data Description

The restricted-use version of the National Corrections Reporting Program (NCRP) provides detailed information on individual admissions to prison, releases from prison, and releases from parole from 1983 till 2009. From 2005 onwards, a subset of states also reports data on the population of prisoners currently in custody. These four different record types are reported separately in four different data sets. In most years, the data does not include the federal prison system.

The records contain information on basic demographics such as the prisoner's date of birth, gender, race, Hispanic status, and highest grade completed. It also contains the most recent admission and release date, the type of admission and/or release, and the most serious offense for which the individual was convicted. To make offense codes comparable to the FBI data on crimes and arrests, we collapse the offense codes to the following 14 categories: murder and manslaughter, forcible rape, robbery, aggravated assault, other assaults, burglary, larceny-theft (excluding motor vehicle theft), motor vehicle theft, other property crime, drug trafficking, drug possession or use, other sex crime, 'white collar' crime (such as forgery and counterfeiting, fraud, embezzlement, and dealing in stolen property), and other crime. Also, we collapse admission types into three different categories: new court commitments & probation revocations, parole revocations, and other admissions (such as returns from appeal or bond, returned escapees, or transfers). New court commitments and probation revocations are typically associated with criminal activity and recent arrests<sup>1</sup>, while parole revocations typically stem from violations of parole conditions. The number of other admissions is very small across states and years, and these admissions either do not affect the total prison population or are not affected by changes in the sentencing regime.

#### Data Quality and Selection of States with Reliable Data

A major problem with the NCRP is data availability and reliability. Only a subset of 34 states reports consistently for most of the period covered by the NCRP, and the data for some of these states exhibit severe internal inconsistencies or external inconsistencies with other data sets such as the NPS. To identify a set of states with reliable data, we conduct four tests for internal and external consistency.

First, we check the consistency of the NCRP admissions and release records: By construction, the number of admissions in year  $x$  according to the admission records should weakly exceed the number of releases that correspond to admissions in year  $x$  according to the release records. In particular, we are con-

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<sup>1</sup>Some states do not distinguish court commitments and probation revocations, so a consistent distinction of these two types of admissions is not possible.

cerned about data quality if admissions according to the release records exceed admissions according to the admission records by more than 10 %.

Second, we check whether age-specific stocks in the post-2005 NCRP files are consistent with NCRP admission and release flows from 1987 to the 2005-2009 period. In particular, assuming that teenagers aged 15 and under are not entering regular prisons, the difference between total admissions and total releases from 1987 onwards for any cohort that was 15 or younger in 1987 should be of roughly the same magnitude as the stock of prisoners for that cohort in 2005-2009. In particular, we question the reliability of the NCRP data if deviations exceed 10%. Note that 13 of the 34 states (AL, HI, IL, KY, MS, ND, NE, NH, NV, OH, UT, VA, WV) cannot be tested this way because they never report any stock data.

Finally, we check the external consistency of the NCRP admissions and release data by comparing it to the corresponding data from the NPS and changes in the NPS stock data. Unfortunately, the NCRP and the NPS are not perfectly comparable. The NPS admission and release data only include prisoners sentenced to more than one year and are only available until 1998. Until the same year, the NPS stock data covers all prisoners in custody (i.e., it also includes those sentenced to one year or less) and thus changes in the NPS stock should be comparable to net flows in the NCRP. From 1999 onwards, however, the NPS reports the number of prisoners by jurisdiction rather than custody such that NPS and NCRP stocks and flows may diverge for a given state. Thus, we compare the two data series for the 1983-1998 period only. In particular, we question the reliability of the reported data if (i) the NPS flows frequently exceed the NCRP flows by more than 10%<sup>2</sup> or (ii) the net NCRP flows deviate substantially from changes in the NCRP stock.

Appendix Table A5 summarizes the results of the four consistency checks we employ. Given the complications comparing the NPS and NCRP flow data, we currently select a state even if it fails the corresponding test as long as it does not exhibit severe inconsistencies in the other tests. This procedure identifies CA, CO, IL, MI, NE, NJ, NY, ND, SC, UT, WA, and WI as states with reliable data. Since IL stops reporting in 2003 while all the other selected states report at least until 2005, we currently do not include IL in our analyses. KY, MN, NH, OH, and VA exhibit only minor internal consistency problems but have somewhat more serious problems with external consistency.

This selection procedure is a lot more elaborate than the one in Pfaff (2011), who selects states that report for at least eleven consecutive years and for which the total number of admissions and releases does “not deviate too much (generally < 10%)” (Pfaff (2011), p. 520) from the corresponding NPS data. As mentioned above, the NCRP and NPS flow data are not perfectly comparable due to different definitions of the prison population. As a result, Pfaff (2011) uses data on KY, MN, SD, and VA in addition to CA, CO, IL, MI, NE, NJ, and WA. We do not use these four states because they either fail the more reli-

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<sup>2</sup>NCRP flows that are larger than the NPS flows are somewhat less of a concern since the NCRP flow data include prisoners sentenced to one year or less, while the NPS flow data do not.

able external consistency check concerning the change in NPS stocks (KY, MN, and VA) or do not consistently report throughout the late 1980s and the 1990s (SD). Instead, we identify NY, ND, SC, UT, and WI as additional states that report consistently over the period and perform relatively well on the internal and external consistency checks.

A final issue with the NCRP data is that within some state-year cells, key variables such as type of admission or year of admission to prison exhibit very high missing rates or are not reported at all. Due to this missing data problem, we exclude NE, NY and UT from our analysis. For the remaining 8 states, missing rates for all key variables are small within each state-year cell, and we impute missing information on gender, age, race, and type of offense committed using a hot-deck procedure that conditions on state, year, and the other key characteristics, treating missing values on other characteristics as a separate category.

## B Uniform Crime Reports

The FBI Uniform Crime Reports (UCR) provide agency-level monthly data on reported crimes and arrests for 43 offense categories by either (1) age and gender or (2) race. The data is available from 1980-83 and 1985-2009. We downloaded this data from the NACJD website<sup>3</sup> on August 3, 2012.

We use the agency-level data to create estimates for the total number of crimes and arrests for our 14 broad offense categories in each state with reliable NCRP data (see Appendix A). Since not each agency submits a report for each month, we use the following procedure to estimate state-wide totals of arrests (the procedure for crimes is analogous):

First, we impute missing monthly information for agencies that report at least one month in a given year. Our default procedure is to create annual estimates by ‘inflating’ the number of reported arrests, i.e., we take the average number of arrests across reported months and multiply it by 12. However, some agencies have so few arrests in reported months that it is likely that they only submit a report whenever they actually had an arrest to report. Other agencies tend to ‘backlog file’ to reduce the administrative burden of reporting to the UCR, i.e., they do not report for  $x$  months and then include the arrests for these  $x$  months in the report for month  $x + 1$ . These agencies report all arrests to the UCR even though they have missing monthly records, and the inflation procedure would thus overestimate the number of arrests for these agencies. Therefore, we take the simple sum across reported months as the number of total arrests for an agency-year cell if one of the following conditions is satisfied:

1. The agency reports not more than 20 arrests across offense categories in a given year.
2. The agency only reports in December.

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<sup>3</sup><http://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html>



3. There is a ‘spike’ in reported arrests after a period of non-reporting. More specifically, the maximum of total monthly arrests within a year is more than 1.5 times the average arrests in other months within this year and occurs after a period of non-reporting.
4. The agency reports for at most 6 months, the reported arrests are at least half the average across years in which the agency reports in each month, and the inflation procedure would produce an annual estimate that is more than twice as high as this average.

If none of these conditions is satisfied, we use the inflation procedure. At the end of this step, we have an estimate for the annual number of arrests by offense for any agency that reports at least one month of data in a given year. All other agency-year cells are missing.

Second, we impute non-reporting spells of 3 years or less by linear interpolation.<sup>4</sup> Third, we use regression imputation to fill in missing data for agencies that have at least ten years of annual data after steps 1 and 2. Since a substantial fraction of agencies reports zero arrests for some offenses, we use a Tobit. We run the regression separately for each state and offense category, using the following covariates: a dummy for non-zero population, a quartic in population, year dummies, and agency dummies.

The final step is to create state-wide annual estimates by dividing the total number of arrests by offense category in each state by the fraction of the state’s population that is covered by agencies for which we have (original or imputed) arrest data. Note that after steps 1-3, we tend to have annual estimates for a set of agencies that covers more than 98% of the state’s population.

## C Simulation of Counterfactual Prison Populations

This appendix describes our procedure to estimate counterfactual prison populations under the assumption that the sentencing policy after 1985 had been the same as the sentencing policy in 1985. We proceed in three steps: First, we formalize our definition of a ‘sentencing policy’. Second, we describe the estimation of policy parameters in the base year 1985 given the data available to us. Third, we discuss how we use the estimated policy parameters for 1985 and the available data to produce counterfactual prison populations from 1986 onwards.

### Our Definition of A Sentencing Policy

We define the sentencing policy in year  $y$  as a set of transition probabilities for two at-risk populations within each cell defined by a set of common characteristics (e.g., race, type of offense allegedly committed):

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<sup>4</sup>We do not extrapolate if the agency starts or ends with a non-reporting spell.

1. The sentencing policy for the population of arrested alleged offenders in year  $y$ , denoted by

$$SP_y^{arr} \equiv \left( \alpha_y^{cc}, \rho_{y,y}^{cc,p}, \dots, \rho_{y,y+T}^{cc,p}, \rho_{y,y}^{cc,np}, \dots, \rho_{y,y+T}^{cc,np} \right).$$

2. The sentencing policy for the population of parolees in year  $y$ , denoted by

$$SP_y^{prl} \equiv \left( \alpha_{y-P,y}^{p,pr}, \dots, \alpha_{y,y}^{p,pr}, \rho_{y,y}^{pr,p}, \dots, \rho_{y,y+T}^{pr,p}, \rho_{y,y}^{pr,np}, \dots, \rho_{y,y+T}^{pr,np} \right).$$

In the above,  $T$  is the maximum time served in prison among all admissions;  $P$  is the maximum time spend on parole among all parolees;  $\alpha_y^{cc}$  denotes the probability of admission to prison in year  $y$  by a court commitment<sup>5</sup> given arrest in year  $y$ ;  $\rho_{y,z}^{cc,p}$  ( $\rho_{y,z}^{cc,np}$ ) is the probability of release to parole (non-parole) in year  $z$  given admission to prison in year  $y$  by a court commitment;  $\alpha_{x,y}^{p,pr}$  is the probability of a parole revocation in year  $y$  given release from prison to parole in year  $x$ ; and  $\rho_{y,z}^{pr,p}$  ( $\rho_{y,z}^{pr,np}$ ) is the probability of release to parole (non-parole) in year  $z$  given a parole revocation in year  $y$ .

Note that this definition of a sentencing policy imposes several strong assumptions that are needed to identify the policy parameters with the available data. First, we assume that each convicted defendant is convicted of the offense listed in the UCR arrest records. Second, we assume that all offenders convicted to prison enter prison in the same year as they are arrested. Third, we assume that the time served distribution for parole revokees does not depend on time spend on parole or the number of previous parole revocations.

### Estimating The Sentencing Policy in the Base Year

We divide the sample into cells defined by the interaction of three race groups, 14 offense categories, and two geographies. The race groups are Whites & non-black Hispanics, Blacks, and Other; the offense categories are those used in Tables 8a-8c; and we divide geography into California and our other seven NCRP states given the peculiarities of the Californian justice system. Within each of these cells and for any year from 1985 onwards, the UCR arrest files and the NCRP records allow us to determine the number of arrests, the flows in and out of prison as well as the flows in and out of parole. Moreover, the NCRP also contains information on end-of-year prison populations within each cell after 2005. Thus, we can use the flow data for previous years to calculate actual end-of-year prison populations starting in 1984. Suppressing indices for race, offense, and geography to simplify notation, let

$$\begin{aligned} arr_y &= \# \text{ of arrests in year } y \\ adm_y^{cc} &= \# \text{ of admissions in year } y \text{ due to a court commitment} \\ &\quad \text{or probation revocation} \end{aligned}$$

<sup>5</sup>As in our previous analyses, we have to pool court commitments and probation revocations since some states do not distinguish between these two types of admission.

$$\begin{aligned}
adm_y^{pr} &= \# \text{ of admissions in year } y \text{ due to a parole revocation} \\
rls_{y,z}^{cc,np} &= \# \text{ of people released from prison to non-parole in year } z \\
&\quad \text{who have been admitted in year } y \text{ due to a court} \\
&\quad \text{commitment or probation revocation} \\
rls_{y,z}^{cc,p} &= \# \text{ of people released from prison to parole in year } z \\
&\quad \text{who have been admitted in year } y \text{ due to a} \\
&\quad \text{court commitment or probation revocation} \\
rls_{y,z}^{pr,np} &= \# \text{ of people released from prison to non-parole in year } z \\
&\quad \text{who have been admitted in year } y \text{ due to a parole revocation} \\
rls_{y,z}^{pr,p} &= \# \text{ of people released from prison to parole in year } z \\
&\quad \text{who have been admitted in year } y \text{ due to a parole revocation} \\
prl\_rev_{y,z}^{cc} &= \# \text{ of people released from parole to prison in year } z \\
&\quad \text{who have been released from prison to parole in year } y \text{ after} \\
&\quad \text{being admitted due to a court commitment or probation revocation} \\
prl\_rev_{y,z}^{pr} &= \# \text{ of people released from parole to prison in year } z \\
&\quad \text{who have been released from prison to parole in year } y \\
&\quad \text{after being admitted due to a parole revocation} \\
pop_y^{cc} &= \text{prison population at the end of year } y \text{ that has been admitted} \\
&\quad \text{due to a court commitment or probation revocation} \\
pop_y^{pr} &= \text{prison population at the end of year } y \text{ that has been admitted} \\
&\quad \text{due to a parole revocation}
\end{aligned}$$

Also, let

$$rls_z^p = \sum_{y \leq z} (rls_{y,z}^{pr,p} + rls_{y,z}^{cc,p})$$

be the total number of people released to parole in year  $z$ . Given the NCRP for our 8-state sample, we can construct all of the above quantities by race, offense, and geography for  $1985 \leq y \leq 2005$  and  $y \leq z \leq 2005$ . Given these data, we want to estimate the policy parameters of the sentencing regime in year  $y$  as follows:

$$\begin{aligned}
\hat{\alpha}_y^{cc} &= \frac{adm_y^{cc}}{arr_y} \\
\hat{\alpha}_{x,y}^{p,pr} &= \frac{prl\_rev_{x,y}^{cc} + prl\_rev_{x,y}^{pr}}{rls_x^p} \\
\hat{\rho}_{y,z}^{i,j} &= \frac{rls_{y,z}^{i,j}}{adm_y^i} \quad \forall i \in \{cc, pr\} \text{ and } j \in \{p, np\}
\end{aligned}$$

Estimating  $SP_{1985}^{cc}$  is straightforward given arrests in 1985, the flow of releases in 1985-2005 for court commitment admissions in 1985 and the remaining prison

population for these admissions at the end of 2005. Estimating  $SP_{1985}^{pr}$  is a bit more involved. First, we need releases to parole for 1982-1984 by race and offense, which are not available in the NCRP. We estimate these release counts as follows: We take the number of total releases for each of these three years and each of our 8 states from the NPS and assume that the composition of these releases with respect to release type, race and offense is as in 1985. The result becomes our estimator for  $rls_x^p$ ,  $x \in \{1982, 1983, 1984\}$ . Next, we use the parole release records to calculate the number of parole revocations in 1985 for people released from prison to parole in or before 1985. Since CO and WA do not provide parole release records throughout the 80's, we have to restrict our data set to 6 out of 8 states. Since very few people get their parole revoked after more than 3 years on parole, we assume  $P = 3$  and treat all people released from prison to parole before 1982 and readmitted to prison in 1985 as if they were released from prison in 1982. This yields estimates for  $prl\_rev_{x,1985}^{cc} + prl\_rev_{x,1985}^{pr}$ ,  $x \in \{1982, 1983, 1984, 1985\}$ . The resulting estimates for  $\alpha_{x,y}^{p:pr}$  can be cross-checked using the releases to parole 1982-1985 for all 8 states and admissions for parole revocations calculated from the prison release records 1985-2005 as well as remaining stocks at the end of 2005. It turns out that our estimation procedure works very well for California, but we tend to underestimate parole revocation admissions for the other 7 states (either because the parole release records underreport releases from parole to prison or because CO and WA have higher revocation rates than MI, NY, ND, SC and WI). To correct for this bias, we rescale  $\hat{\alpha}_{x,1985}^{p:pr}$  by the ratio of actual parole revocations in 1985 over estimated parole revocations in 1985 that result from releases to parole 1982-1985 and  $\hat{\alpha}_{x,1985}^{p:pr}$ . Finally, estimating  $\rho_{y,z}^{pr:p}$  is straightforward given the flow of releases 1985-2005 for parole revocations in 1985 and the remaining prison population for these admissions at the end of 2005.

### Simulation of Counterfactual Prison Populations

In what follows, we let a hat over a variable denote the counterfactual value of that variable. We assume that changes in sentencing policies after 1985 do not affect the time series of prison releases for admissions that occur until the end of 1985. Thus, we take these time series as given and let

$$\widehat{rls}_{y,z}^{i,j} = rls_{y,z}^{i,j} \quad \forall i \in \{cc, pr\}, j \in \{p, np\}, y \leq 1985, z \geq 1985.$$

Moreover, we start by ignoring potential second order effects of changes in sentencing policies on criminal activity and the probability of arrest given involvement in crime. Under this assumption, we can take the time series of arrests throughout the period as given and let

$$\widehat{arr}_y = arr_y.$$

From January 1, 1986, on, we impose  $SP_{1985}^{cc}$  on the actual time series of arrests to calculate counterfactual time series of court admissions and subsequent releases from these admissions to parole/non-parole from 1986 onwards. That

is, for  $y > 1985$  and  $z \geq y$ , we can calculate

$$\begin{aligned}\widehat{adm}_y^{cc} &= \alpha_{1985}^{cc} \times arr_y \\ \widehat{rls}_{y,z}^{cc,p} &= \widehat{adm}_y^{cc} \times \rho_{1985,1985+z-y}^{cc,p} \\ \widehat{rls}_{y,z}^{cc,np} &= \widehat{adm}_y^{cc} \times \rho_{1985,1985+z-y}^{cc,np}\end{aligned}$$

Calculating the corresponding counterfactual time series of parole revocation admissions and subsequent releases from these admissions to parole/non-parole from 1986 onwards is a bit more involved since it is based on the *counterfactual* time series of releases to parole, which is itself a result of the counterfactual policy in previous years. Thus, we need to apply the following procedure:

We initialize the time series of counterfactual releases to parole with

$$\widehat{rls}_x^p = rls_x^p \quad \forall x = 1983, \dots, 1985.$$

For  $y > 1985$ , we need to calculate

$$\widehat{adm}_y^{pr} = \alpha_{1985,1985}^{p,pr} \times \widehat{rls}_y^p + \alpha_{1984,1985}^{p,pr} \times \widehat{rls}_{y-1}^p + \alpha_{1983,1985}^{p,pr} \times \widehat{rls}_{y-2}^p + \alpha_{1982,1985}^{p,pr} \times \widehat{rls}_{y-3}^p. \quad (1)$$

At the end of year  $y - 1$ , we know all elements on the right hand side except for  $\widehat{rls}_y^p$ . However, note that

$$\begin{aligned}\widehat{rls}_y^p &= \widehat{rls}_{y,y}^{pr,p} + \widehat{rls}_{y,y}^{cc,p} + \sum_{x < y} \left( \widehat{rls}_{x,y}^{pr,p} + \widehat{rls}_{x,y}^{cc,p} \right) \\ &= \rho_{1985,1985}^{pr,p} \widehat{adm}_y^{pr} + \rho_{1985,1985}^{cc,p} \alpha_{1985}^{cc} arr_y + \sum_{x < y} \left( \widehat{rls}_{x,y}^{pr,p} + \widehat{rls}_{x,y}^{cc,p} \right)\end{aligned}$$

Substituting this expression into equation (1) yields

$$\begin{aligned}\widehat{adm}_y^{pr} &= \frac{1}{1 - \alpha_{1985,1985}^{p,pr} \rho_{1985,1985}^{pr,p}} \times \dots \\ &\dots \times \left( \alpha_{1985,1985}^{p,pr} \times \left( \rho_{1985,1985}^{cc,p} \alpha_{1985}^{cc} arr_y + \sum_{x < y} \left( \widehat{rls}_{x,y}^{pr,p} + \widehat{rls}_{x,y}^{cc,p} \right) \right) + \dots \right. \\ &\left. \dots + \alpha_{1984,1985}^{p,pr} \times \widehat{rls}_{y-1}^p + \alpha_{1983,1985}^{p,pr} \times \widehat{rls}_{y-2}^p + \alpha_{1982,1985}^{p,pr} \times \widehat{rls}_{y-3}^p \right).\end{aligned}$$

At the end of year  $y - 1$ , we know all elements on the right hand side of this expression. Moreover, given  $\widehat{adm}_y^{pr}$ , we can calculate

$$\begin{aligned}\widehat{rls}_{y,z}^{pr,p} &= \widehat{adm}_y^{pr} \times \rho_{1985,1985+z-y}^{pr,p} \\ \widehat{rls}_{y,z}^{pr,np} &= \widehat{adm}_y^{pr} \times \rho_{1985,1985+z-y}^{pr,np}.\end{aligned}$$

This way, we can iteratively calculate  $\widehat{adm}_y^{pr}$ ,  $\widehat{rls}_{y,z}^{pr,p}$ , and  $\widehat{rls}_{y,z}^{pr,np}$  for  $y > 1985$  and  $z \geq y$  and update the time series of counterfactual releases to parole with

$$\widehat{rls}_y^p = \sum_{x \leq 1985} \left( \widehat{rls}_{x,y}^{pr,p} + \widehat{rls}_{x,y}^{cc,p} \right) + \sum_{1985 < x \leq y} \left( \widehat{rls}_{x,y}^{pr,p} + \widehat{rls}_{x,y}^{cc,p} \right).$$

The calculation of the time series of counterfactual releases to non-parole,  $\widehat{rls}_y^{np}$ , is analogous.

Finally, given  $\left\{ \widehat{pop}_{1985}^i, \widehat{adm}_y^i, \widehat{rls}_{y,z}^{i,p}, \widehat{rls}_{y,z}^{i,np} \right\}_{i \in \{cc, pr\}}$ , we can calculate the counterfactual time series of prison populations within each cell as

$$\widehat{pop}_y^i = \widehat{pop}_{y-1}^i + \widehat{adm}_y^i - \sum_{x \leq 1985} (rls_{x,y}^{i,p} + rls_{x,y}^{i,np}) - \sum_{1985 < x \leq y} (\widehat{rls}_{y,z}^{i,p} + \widehat{rls}_{y,z}^{i,np}).$$

### Accounting for Incapacitation and Deterrence

We account for incapacitation and deterrence effects of changes in sentencing policies assuming that there is (1) a constant elasticity between the total prison population across all offenses in year  $y - 1$  and criminal activity in year  $y$  and (2) a constant ratio of arrests to reported crime over time. In particular, let  $\beta_j$  denote the elasticity for offense category  $j \in J$ ,  $arr_{y,j}$  the number of arrests in year  $y$  for offense category  $j$ , and  $pop_{y,j}$  the prison population in year  $y$  for inmates committed for offense category  $j$ . Then the two assumptions above imply the following relationship between the ratio of lagged counterfactual and actual total prison populations and the ratio of contemporaneous counterfactual and actual arrests:

$$\frac{\widehat{arr}_{y,j}}{arr_{y,j}} = \left[ \frac{\sum_{j \in J} (\widehat{pop}_{y-1,j})}{\sum_{j \in J} (pop_{y-1,j})} \right]^{\beta_j}$$

or

$$\widehat{arr}_{y,j} = arr_{y,j} \left[ \frac{\sum_{j \in J} (\widehat{pop}_{y-1,j})}{\sum_{j \in J} (pop_{y-1,j})} \right]^{\beta_j}$$

Thus, given the counterfactual end-of-year prison populations in year  $y - 1$ , we can calculate counterfactual arrests in year  $y$ . The remaining counterfactual stocks and flows for year  $y$  are then calculated just as in the simpler simulation procedure described above.