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*The Law and Economics Workshop*

*Presents*

**THE MYTHS AND REALITIES OF CORRECTIONAL  
SEVERITY: EVIDENCE FROM THE NATIONAL  
CORRECTIONS REPORTING PROGRAM ON  
SENTENCING PRACTICES**

by

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THURSDAY, September 25, 2008

3:40-5:30

Room 236 Hutchins Hall

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September 18, 2008

Dear University of Michigan seminar participants,

Thank you for taking the time to read my paper—I appreciate it. The basic goals of this paper are two-fold. First, it looks at trends in time served by state prisoners in a sample of states over the 1980s, 1990s, and 2000s to see how these trends have changed over the past 15 or so years. In general, I find, somewhat surprisingly, that many measures of time served appear to be constant or declining. These results, however, are complicated by the fact that we appear to be incarcerating increasingly low-level offenders: do declining times serve thus reflect truly shorter sentences (a less severe system) or more aggressive incarceration of minor offenders for short time periods (a more severe system)?

Second, the paper shifts its focus from macro-level trends to micro-level ones and examines how various offender-level traits—race, sex, age, and offense—influence time served. Youthfulness and violence appear to lead to a lower likelihood of release in any given year (even when accounting for the sentence imposed) and elderliness to a greater likelihood. No other factor, including whether the inmate is black, seems to affect release probabilities in any meaningful way.

I am looking forward to hearing your comments.

Thanks again,

John Pfaff

## The Myths and Realities of Correctional Severity: Evidence from the National Corrections Reporting Program on Sentencing Practices

John F. Pfaff<sup>\*</sup>

The scope of incarceration in the United States is hard to underestimate. Approximately 1.5 million people are currently in prison, an incarceration rate of approximately 700 per 100,000, or nearly 1 out of every 130 Americans.<sup>1</sup> For black men, the number is even more striking: about 1 out of 20 are in prison. State governments, strapped for cash, are spending nearly \$40 billion per year to house them. While Americans have traditionally been more punitive than citizens of other western democracies (Whitman 2005), the extent to which our incarceration rate outstrips rates in Europe and beyond is still remarkable. And it is all the more striking given that the U.S. incarceration rate was a steady 100 per 100,000 people for nearly 50 years, from 1920 (when the statistics are first gathered) through 1970, at which time there were only 300,000 people in prison. The subsequent quintupling in prison populations is an event unseen here or abroad.

Yet equally remarkable is how little we actually understand about the forces that have driven this buildup. Researchers have posited a wide array of theories, many of which shed valuable light on the issue. And in some cases, they have mustered anecdotal or historical evidence in cor-

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<sup>\*</sup> Associate Professor of Law, Fordham Law School. Thanks to Shawn Bushway, Jeffrey Fagan, Anup Malani, Tom Miles, JJ Prescott, Dan Richman, an anonymous referee, and workshop participants at the Conference on Empirical Legal Studies and Fordham Law School for helpful comments. Thanks also to Christopher Pece of the US Census Bureau for providing me with access to Annual Survey of State and Local Government Finances, and to Damien Lyons and Christina Schweikert of Fordham's Computer Science department for access to a sufficiently powerful computer. Ryan Kaupelis and Satinder Singh provided valuable research assistance. All errors remain my own.

<sup>1</sup> The recent report by the Pew Center on the States' Public Safety Performance Project (2008) puts the incarceration rate at 1 out of every 100 adults. The rate is 1 out of 130 for Americans of all ages.

roboration. But the causes of prison population growth have so far defied efforts at more rigorous empirical examination (Pfaff 2008a). A central failing of almost all efforts has been conceptual: they have sought primarily to explain the forces shaping total prison population, not admissions or releases. But total population is a complex stock variable, shaped by a host of concurrent and lagged factors that cannot be easily modeled. It is ultimately unclear what, if anything, these empirical efforts have demonstrated.

The flow variables of admissions and releases are substantially more conducive to rigorous empirical analysis. A few empirical studies have looked at admissions,<sup>2</sup> but none has examined releases. This paper starts to fill this gap, as part of a broader empirical project examining trends in American prison populations over the past thirty years. Despite the claims and anecdotes, in media and academic writings alike, about the severity of contemporary sentencing policy, we have at best only a weak understanding of what patterns in time served actually look like. This paper provides both a rich descriptive account of time served in prison as well as a more rigorous econometric examination of how trends in time served are shaped by inmate characteristics such as race, sex, age, and conviction offense. A companion article (Pfaff 2008b) explores how macro-level forces such as crime rates, economic conditions, demographic shifts, and politics have influenced these trends.

To date, investigators have made only rough forays into release policy, looking at releases only through crude approximations of time served, such as the number of prisoners admitted in a given year divided by that year's total population (see, e.g., Blumstein & Beck 1999).<sup>3</sup> Taking advantage of a highly detailed—and underused—dataset, the National Correc-

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<sup>2</sup> See Stuckey et al. (2005), Nicholson-Crotty (2004), Listokin (2003), Sorensen & Stemen (2002), Marvell & Moody (1997, 1996), Jacobs & Helms (1996) and Marvell (1995). As shown in Pfaff (2008a), however, all but Listokin (2003) suffer from substantial identification problems.

<sup>3</sup> Thus if 10,000 people were admitted in 1990 and the total prison population were 100,000, this formula would return an average sentence length of 10 years. This would be an accurate estimate if the prison population were in equilibrium: if each year 10,000 people were admitted and served on average ten years, then the total prison population would be 100,000. But during periods of change this estimate is inaccurate. Moreover, as I discuss below, it seems like the median and other quantiles, not the mean, are the more relevant measures.

tions Reporting Program (NCRP), I am able to map how the time served for individual inmates changes (to the day) over a ten- to twenty-year period in a sample of eleven states. As a result, I am able to paint a much more detailed and nuanced picture of the time spent in prison, as well as to start providing some insight into the factors that have shaped it.

The results stand in stark contrast to conventional wisdom. The standard view of release policy can be described easily: over the past thirty years, we are often told, legislatures have consistently passed tougher and tougher sentencing laws—mandatory minimums, truth-in-sentencing laws, two- and three-strike laws, parole abolitions—leading to more prisoners serving longer sentences. Even as crime rates entered their long decline in the 1990s, government officials felt the need to maintain a “tough on crime” appearance. Media accounts of sentencing focus almost exclusively on shockingly long sentences for relatively minor crimes (often federal sentences for drug offenses).

And it is not just the media that tells this story. Sophisticated observers of the criminal justice system, such as Alfred Blumstein, have likewise argued that longer sentence lengths have contributed significantly to the growth of prison populations. In a 1999 article, for example, he argued that 57.7% of the growth in prison population between 1980 and 1996 was due to longer sentences (Blumstein & Beck 1999). Although he noted the importance of increased commitments (at 41.5%), sentence length was nonetheless the most important driver in his results.

The results from the NCRP tell a much different story. In many states, the median time served has declined over much of the 1990s; so too has the 75th percentile of time served, and even the 90th in some cases. As I demonstrate below, it is undoubtedly clear that the United States has become more punitive over the past thirty years, but rarely with respect to sentence length *actually served*. It is our willingness to incarcerate in the first place, not to keep people in prison once admitted, that appears to be the fundamental engine of prison growth.

It is important, however, not to overstate this point. First, the rise in admissions has been driven at least in part by the incarceration of increasingly minor offenders, who likely serve shorter sentences; after all, prison populations grew during the longest sustained crime drop in recorded American history. Comparing time served by those admitted in 1988 to that served by those admitted in 1998 is thus challenging, since the two groups are not identical—a greater fraction of those admitted in 1998

ought to be serving shorter sentences even if sentencing policy remained constant (or grew more severe). Controlling for the shifting distributions of offenders less dramatic results, but these results still suggest that in many places for most prisoners time served still remained relatively constant or even declined.

Second, other countervailing trends may mask the effect of increasingly punitive practices. Some states, for example, parole prisoners quickly but violate them back aggressively—the median prisoner in California, for example, serves only about 180 days, but California violates nearly 30% of its parolees back to prison in any given year (accounting for approximately 43% of all parolees violated back in the United States).<sup>4</sup> If a large number of inmates return to prison at least once, is the median time 180 days or 360 days? Also, capacity could limit time served, so were admission rates to decline, total population may decline more slowly—with more capacity available, states may force prisoners to serve longer fractions of their (longer) sentences.

Moreover, aggregate state-level quantiles can mask important variation in how trends are distributed. Trends in time served need not be constant with respect to race, sex, and other offender characteristics. To explore this issue more carefully, I develop offender-level survival models to examine the effect of race, sex, age, and (broadly-defined) offense type on time actually served.<sup>5</sup> In general, the only traits consistently correlated with time served are whether the offender is over 40 years old, under 21 years old, or is serving time for a violent offense; the first trait leads to a higher hazard ratio (i.e., an increased probability of release in any given year), the latter two to lower hazard ratios. Interestingly, race, in particular whether the offender is black, appears to have very little or no influence on time served.

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<sup>4</sup> This is an approximation—180 days comes from the 2002 NCRP, while the parole data come from the 2006 BJS survey of parole (Glaze & Bonczar 2007).

<sup>5</sup> It is easy to think of other offender traits that could matter—employment status at time of conviction, education, and so on—but the four traits listed here are the only four consistently reported in the NCRP. A particular disappointment is that the NCRP provides no information on prior criminal history. It officially contains a variable indicating whether the inmate has served at least one prior prison sentence, but it is blank for every inmate in every state in every year.

The results in this paper shed new, and important, light on US penal policy. First, they indicate that our attention to sentencing<sup>6</sup> matters is at least partially misplaced. While institutions such as Families Against Mandatory Minimums and the Kennedy Commission (2004) focus on sentencing regimes such as mandatory minimums, most of the growth in prison population has come through admissions, an area that receives substantially less attention; and the one facet of admissions that receives the most attention—the incarceration of low-level drug offenders—is relatively unimportant.<sup>7</sup> Tough-on-crime activists likewise focus on trying to pass increasingly draconian sentences that often seem to simply not be imposed or, if imposed, not served. Reformers on both side thus appear to be looking in the wrong place.

Second, these results suggest that despite its great size the US prison population need not be particularly stable. Rising admissions drive the current growth in prisons, and admission rates—unlike release rates—can change immediately.<sup>8</sup> To take an extreme example, were California to maintain its current release policies but admit no new prisoners, its total population would drop by 75% in one year.

Third, these results further confirm prior empirical findings about how various offender characteristics affect sentencing outcomes, and do so using data not used in other studies, thus acting as an independent source of evidence. The negligible effect of race on time served, for example, corroborates Sampson and Lauritsen's (1997:362) claim that the current empirical literature finds "little evidence that racial disparities reflect systematic, overt bias on the part of criminal justice decision mak-

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<sup>6</sup> To be clear, in general I use "sentencing" to refer to choices about how long to incarcerate someone already heading to prison. Excluded from this definition is the decision whether to incarcerate in the first place; that falls under "admissions" policy. The line, however, is not always clear—admission can thought of as raising time served from zero to something greater than zero—and at times I will take this broader view.

<sup>7</sup> Low-level drug offenders makes up a small fraction of total prisoners. All drug offenders constitute only 20% of prisoners, and low-level dealers comprise only a small share of this fraction (see, e.g., Pfaff 2008a).

<sup>8</sup> Changes in sentencing policy generally operate with a lag. Assuming 100% of any sentence is served, reducing the sentence for burglary from ten years to six years will have no effect for six years, while changing the number of people admitted to prison for burglary reduces the prison population instantly.

ers.” And the aggravating nature of an offender’s youthful status supports recent sociological findings along these lines (as shown in Bushway and Piehl 2007).

The findings here also implicate broader issues of criminal justice. For example, one argument in favor of tougher sentencing practices is that they are needed to confine repeat offenders and other “predators” for longer periods of time. That prison growth is in fact driven by more admissions, not longer sentences, seems to suggest that practices do not live up to the rhetoric. But the actual picture provided here is more ambiguous. In fact, it could be that our sentencing (as opposed, perhaps, to our admissions) policies are working roughly as desired: serious offenders receive longer sentences, but those longer sentences do not trickle down to the more-minor inmates we increasingly incapacitate (at least for those we would have still locked up in prior years).

Another issue these results touch on is replacement. One concern with incapacitation is that its crime-reducing effect is undermined by replacement—if each offender taken off the street is replaced by another one committing the same acts, then incapacitation has no net effect on crime (Miles and Ludwig 2007). Incarcerating many people for short periods of time many mitigate the replacement concern, especially if the elasticity of response is high. Long sentences in the presence of high potential replacement may lead to something close to a one-for-one replacement and minimal reductions in crime. But short sentences still remove the offender while still discouraging much replacement (since the incarcerated offender is soon to return).

This paper proceeds as follows. Part 1 takes a detailed look at time served by inmates in a subset of states providing data to the NCRP. This part not only provides a rare picture of trends in actual time served, but it develops rough estimates of how those trends have been shaped by changes in the distribution of conviction offenses and probabilities of release. It also demonstrates the substantially larger role admissions have played in driving overall prison population sizes. Part 2 then constructs a more rigorous empirical model to estimate the extent to which the general trends given in Part 1 vary across different types of inmates. The Appendix discusses the data and the technical aspects of the empirical models in more detail.

## I An Overview of Release Trends

Our understanding of sentence lengths and release policy is driven more by anecdote and crude empiricism than extensive examination of the data. Furthermore, much of the focus is misplaced—far more attention, especially in the media, is given to the sentences imposed at trial, not on the actual time served. In this section I provide a descriptive, but quantitative, account of actual release policy in a sample of states.

In general, precise release behavior is not readily observable in much of the data available on the correctional system. The Bureau of Justice Statistics reports the number of inmates released each year in every state, but these aggregate values provide no information on the time served by those released. Some have attempted to estimate average time served by dividing the number of prisoners admitted in a given year by the total number of inmates, but such approaches rest on problematic assumptions (such as that prison populations are in equilibrium) and are incapable of disaggregating time served along many relevant margins. More important, the average is likely not the most useful number to consider. Time served clearly skews heavily to the left (towards short sentences), with a long thin right tail, so values such as the median and 75th-percentile of time served may be more meaningful; the ratio approach is of little help for calculating quantiles.

This paper examines data gathered by the National Corrections Reporting Program to develop substantially more precise measures of time served. The NCRP is an offender-level dataset, in which participating states submit data on each offender as he enters and leaves prison. As a result, I can calculate the exact time served (to the day) of each inmate released from prison, as well as the number of offenders admitted in a particular year  $t$  who are still serving time in a future year  $t + \tau$ . The NCRP also provides demographic information on each inmate and data on his crimes and sentences imposed. The NCRP is not without its limitations—the Appendix discusses in more detail the concerns with the data and how I confront them—but it provides the clearest picture of releases available.

The NCRP began gathering data in 1983 and continues through to today; this paper uses data through 2002, the most current release available at its start. During this period, twenty-one states contributed data of sufficient reliability for sufficiently long periods of time. Yet even these states differ in the apparent reliability of their reporting. The most reli-

able states, what I call the “Tier 1” states, are California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. These states are responsible for about 30% of the total prison population in United States. Slightly less reliable are the results from the “Tier 2” states of Mississippi, Nevada, New Hampshire, New York, North Dakota, Pennsylvania, South Carolina, Utah, and Wisconsin. Collectively, Tier 1 and Tier 2 states hold nearly half of all inmates in the United States. I focus on Tier 1 states here.

This section proceeds in three steps. First, I present the trends median, 75th- and 90th-percentile time to release.<sup>9</sup> The dominant feature of these trends—the stability or even decline in time served, especially for the median prisoner—is noteworthy. I also look at trends in the composition of each entering cohort. While the focus here is on release decisions, admissions trends partially drive them: if violent criminals make up a greater share of prisoners entering prison in 1990 than in 1989, then we should expect the average 1990 prisoner to spend more time in prison even if there is no change in sentencing policy. There is some shift in cohort composition, but not a substantial amount (partly because many of the observations here start after the shifts towards drug incarcerations were well underway). Nonetheless, a rough control for changing distributions of offense severity does lead weaker, but often roughly similar, results.

Second, I develop two counterfactuals to demonstrate more clearly the importance, or lack thereof, of changes in time served. The first considers the implications of holding release rates constant over the sample period for each state (frozen at that in effect in the state’s first year of data); the second, the implications of fixing the size of the admissions cohort at that of the first year of data. The two counterfactuals produce strikingly different conclusions. Prison populations would not look too much different today had sentencing policies remained unchanged; but if we had frozen the number of admittees as recently as five or ten years

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<sup>9</sup> A referee, noting that some of the change in time served could be for the most serious offenders, suggested that the 90th percentile is not high enough and that I should consider the 99th percentile as well. The point is valid, but unfortunately in all but one state in my sample the 99th-percentile prisoner from any entering cohort has yet to be released. This is not surprising, given that sentences of life (with or without parole) and death comprise slightly more than 3% of all violent sentences, and violent offenders make up approximately 50% of all inmates.

ago, many states would face constant or declining prison populations by this point.

And third, I briefly consider the possible importance of parole and probation violations. Much attention has been given to rising rates of parole violations, especially for technical violations. If a large number of those released after short sentences return to prison soon thereafter, then the median may not provide an accurate description of actual time served. The evidence below, however, suggests that parole violations play some, but perhaps not a major, role, and that probation violations have almost no effect whatsoever on prison populations. Due to limitations in the NCRP, however, these results must be taken as tentative.

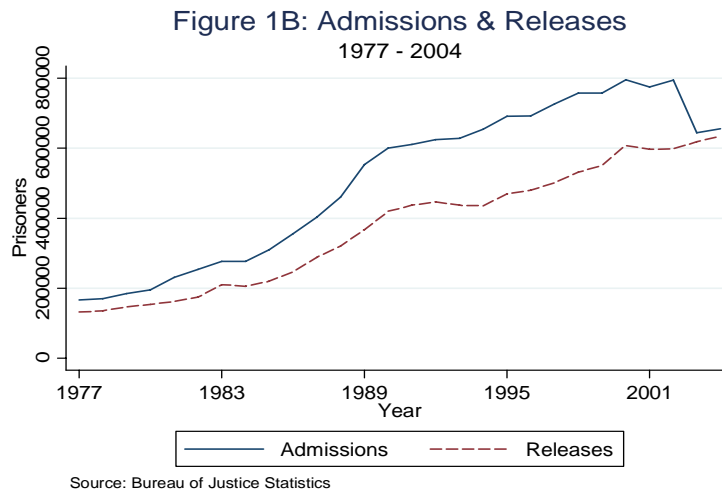
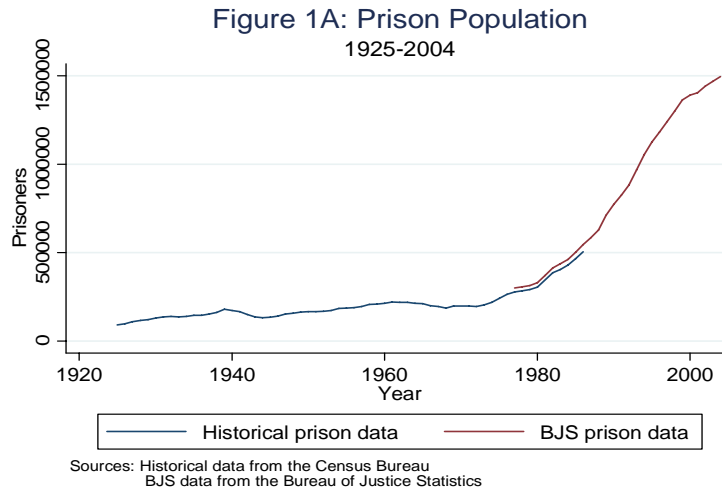
### 1.1 Patterns in Time Served

Even before turning to detailed release data, it is easy to demonstrate that the conventional wisdom about increasing sentencing severity faces some challenges. Figure 1A is a startling image, the placid flatness of the 1920s to 1970s giving way suddenly to the steady and unrelenting growth of the past thirty years. It is somewhat disappointing that Figure 1B does not have the same fame. Figure 1B decomposes the surging part of Figure 1A into its two primary pieces: admissions and releases.<sup>10</sup> If our sentencing practices were becoming increasingly punitive over the sample period, we should expect the gap between admissions and releases to widen. But with the exception of a period in the late 1980s, we simply do not see that. Thus, even before delving into the more detailed NCRP data, we should not expect to see a profound shift in sentencing policy: effective sentencing has remained relatively stable for thirty years.<sup>11</sup>

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<sup>10</sup> For any year, the gap between the two lines in Figure 1B equals the amount by which the line in Figure 1A rises.

<sup>11</sup> For example, it appears that many sentencing reforms may have adopted for purely symbolic reasons. Dharmapala et al. (2006), for example, point out that many states that adopted truth-in-sentencing laws had already abolished or restricted parole. The TIS law thus appears to be at least somewhat redundant.



But we can do much better than Figure 1B. Figure 2A provides the median and 75th-percentile time to release (in days) for the Tier 1 states; Figure 2B adds in the 90th-percentile time to release as well.<sup>12</sup> Two features stand out in Figure 2. First, the standard inmate does not spend a

<sup>12</sup> I omit the 90th-percentile line in Figure 2 solely because it flattens much of the variation in the median and 75th-percentile lines (since it is substantially higher).

significant amount of time behind bars.<sup>13</sup> Median<sup>14</sup> times to release are on the order of about one to two years, whether in states such as Minnesota, which has a relatively liberal view of criminal sentencing, or in Michigan, which is considered to be particularly punitive.<sup>15</sup> In the big states, such as California and Illinois, median time to release is well under a year, reaching lows of 179 days in California and 168 days in Illinois.

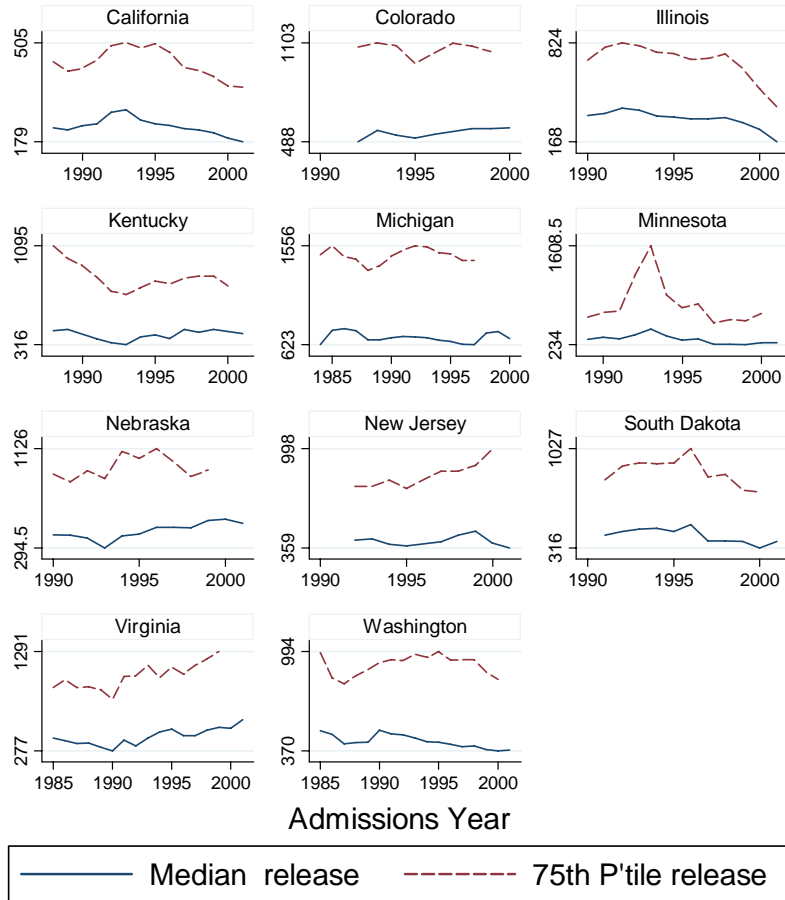
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<sup>13</sup> Those familiar with this literature will notice that my numbers are substantially lower than those often reported by the Bureau of Justice Statistics, despite the fact that I am using the same data the Bureau is. I believe the difference arises from different frames of reference. For any given year, the BJS computes the mean or average time served by those released in that year (regardless of the year admitted), while I focus on the time served by those admitted in that year (regardless of the year released). This is why the BJS can calculate average time served, a term that is effectively impossible under my approach.

<sup>14</sup> The Appendix explains how I calculated the quantiles. Since the admissions files do not contain information on releases, and the releases files contain information only on those released, determining how many prisoners remained unreleased at a given point requires some manipulations.

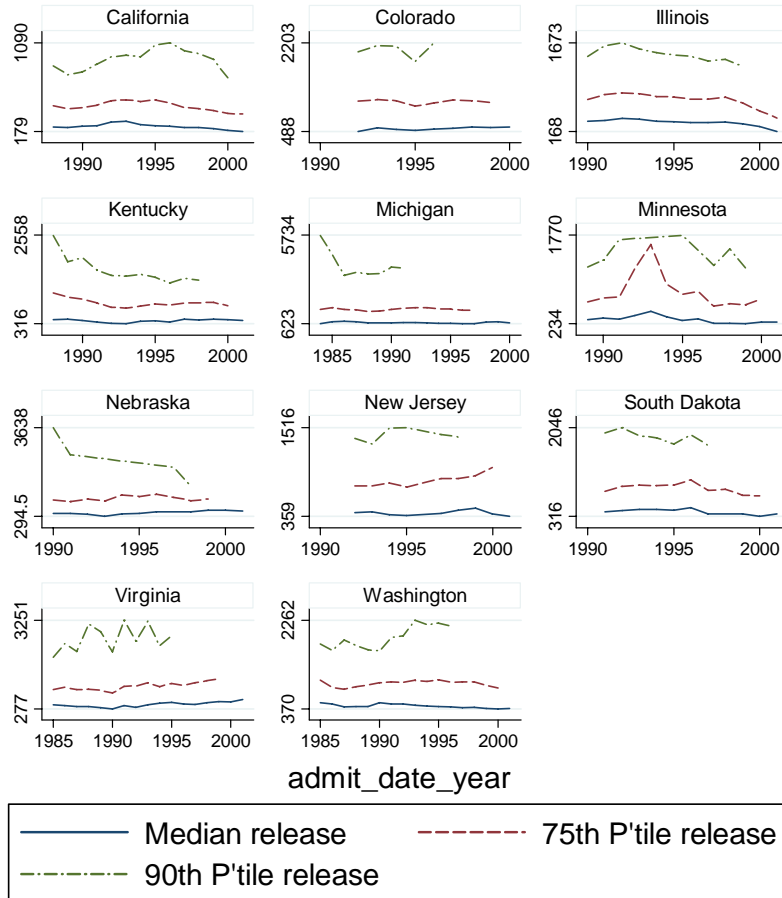
<sup>15</sup> Prison length can only be measured in quantiles, since the average is generally undefined—life sentences and death sentences cannot be converted meaningfully into averages, and the time actually to be served by as-yet unreleased offenders is unknown. Moreover, quantiles should be used since the distribution has a very long, but very thin, right tail (a very small number of very long sentences), which may, at least for certain serious offenses, cause any estimate of the average to be “deceptively” high. Lesser crimes release close to 100% of those admitted for them in only a few years, and averages could conceivably be calculated, though again the average would not reflect the experience of the “typical” (i.e., median) inmate.

## Figure 2A: Days to Release Median and 75th Percentiles



Source: NCRP. See Appendix

Figure 2B: Days to Release  
Median, 75th, and 90th Percentiles



Source: NCRP. See Appendix.

Seventy-fifth-percentile times to release are often substantially longer, but perhaps not as long as one might have initially thought. In most states they are on the order of four to five years, though in some cases they are substantially lower—around two years in Minnesota and Illinois, and about one year in California. In most states in the sample, the seventy-fifth percentile time to release has remained relatively constant,

though Virginia and New Jersey have seen it rise; Virginia is one of the few states in the sample to see its median time to release grow as well.

When the results in Figure 2 are broken down by broad type of major offenses—violent crimes, property crimes, and drug offenses—the same patterns roughly hold across states. And when crimes are further disaggregated within broad classes of crimes, again the same trends generally emerge. The aggregate nature of Figure 2 thus does not mask substantial differences in trends within states.

At one level, these figures suggest that the conventional wisdom about punitive penal policy on the release side is in error. However, due to two seemingly contradictory patterns in the 1990s—steadily declining crime rates and constantly rising prison admission rates—the median is a tricky number to interpret. After all, if more and more people are going to prison as fewer and fewer people commit crimes, it must be that increasingly marginal offenders have been entering our prisons at increasing rates. Thus even if states aggressively applied their tougher sentencing laws the median could fall.<sup>16</sup> Yet even the 75th- and 90th- percentile times remain relatively constant, and they drop in many jurisdictions.

It is thus essential to examine how admissions patterns have changed. Figure 3 plots the distribution of primary offenses across four broad categories of offenses. Where the fractions do not sum to 1 it is because the offenses for the residual amount were coded as blank or unknown.<sup>17</sup> Again, what is most striking about Figure 3 is its constancy. Not surprisingly, many states see a growing share of their admissions going towards drug crimes, but due in part to the sample periods here—usually in the 1990s—the shift towards drugs is not that significant. For those states

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<sup>16</sup> A simple thought experiment makes this clear. Imagine that in  $t_0$  the state locks up only person per year, a serious offender who serves a ten year sentence. In the long run, the median time served is ten years. Later, in some future  $t_1$ , the state continues to incarcerate the one serious offender, but raises his sentence to twenty years. At the same time, it starts to incarcerate two minor offenders every year for one-year terms each. The median sentence length will fall to one year. Note that the median falls despite the fact that the total equilibrium prison population rises from 10 to 22, with 83% (10/12) of that growth due to the longer sentences for the serious offenders.

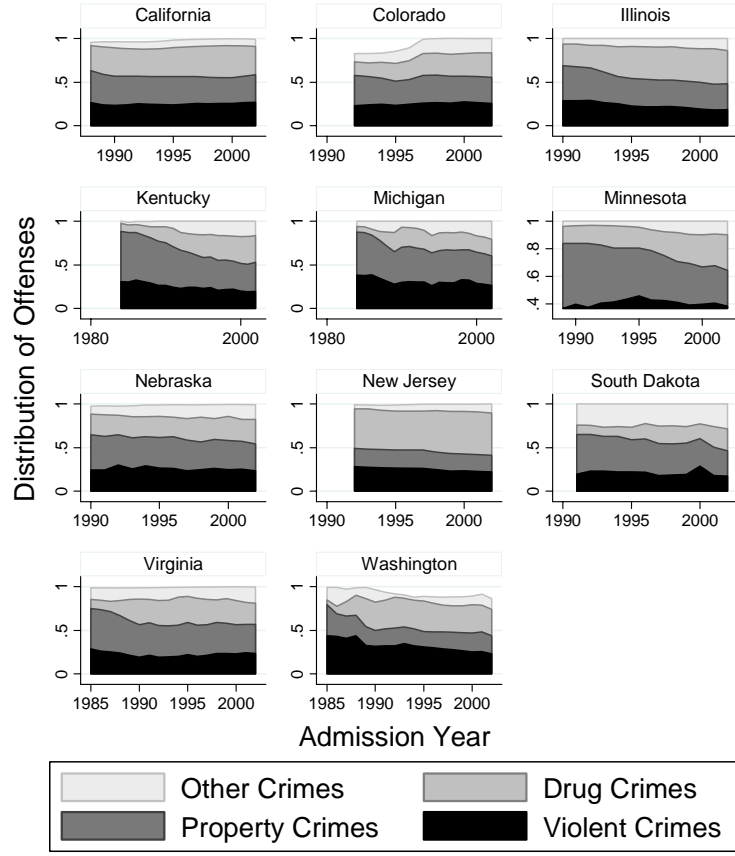
<sup>17</sup> In general, the four shares consistently sum almost to 1. As a result, even if the missing values are not randomly chosen from the four classes, their omission likely does not bias the findings by much.

with data going back into the 1980s, the share of drug crimes more than doubles, primarily at the expense of property crimes (although admissions in both categories rise in absolute terms). Importantly, the share of violent crimes, the crimes that have the longest prison sentences, remains relatively constant across almost all states and years. The distributions are more or less constant within the violent and property categories as well, although California, Illinois, Kentucky, and Washington see theft take up a larger fraction of property crimes at the expense of burglary. As I have discussed elsewhere (Pfaff 2006), changing offense classifications in the NCRP for drug crimes make it hard to draw clear inferences within that category.

However, the crime categories used here are quite broad and thus perhaps mask important variation. For violent crimes, for example, there are only six offenses: all killing crimes, all kidnapping crimes, all sex offenses, all arsons, all assaults, and all otherwise unclassified violent crimes. For property there are four (burglary, fraud, theft, and other) and for drugs five (heroin, cocaine, marijuana, other, and unknown). As discussed in more detail in the Appendix, I employ these coarse classifications as a concession to limitations in the data. Thus the longer-sentences/lesser-offenders hypothesis could remain true within each category: though the overall share of assaults remains roughly constant in many states, for example, it possible that lower-level assaults are making up a greater fraction of that share.

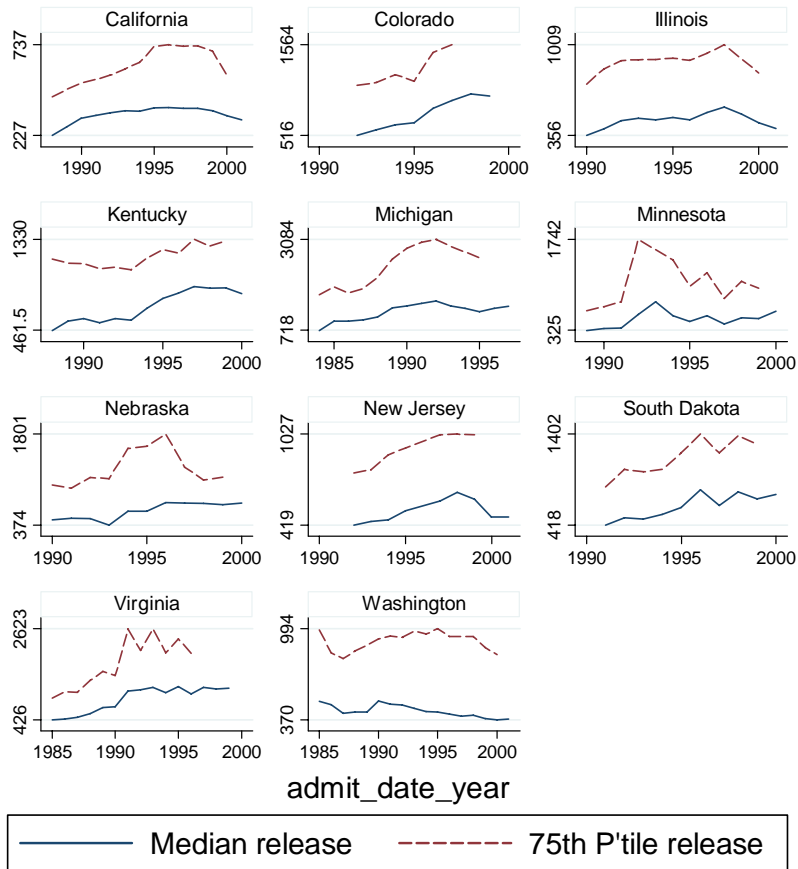
Figure 4 therefore provides a different way of examining the importance of incarcerating increasingly minor offenders. Figures 4a and 4b recalculate the median, 75th percentile, and 90th percentile times to release given in Figure 2 above, but with one major difference. For each state, I assume that the size of each entering cohort is fixed at the size of that in the first year of data, and only those who serve the longest sentences—a proxy for the severity of their acts—are admitted. In other words, the data for California start in 1988, and in that year it admitted 74,142 prisoners. I thus assume that each subsequent entering cohort consists of only 74,142 prisoners, and I choose the 74,142 prisoners from each cohort who served the 74,142 longest sentences (including not having been released by the end of the data). This attempts to control for the problem of evermore minor offenders in way that is least favorable to the claim that sentence length is growing shorter. Figure 4 presents the results.

Figure 3: Distribution of Admissions



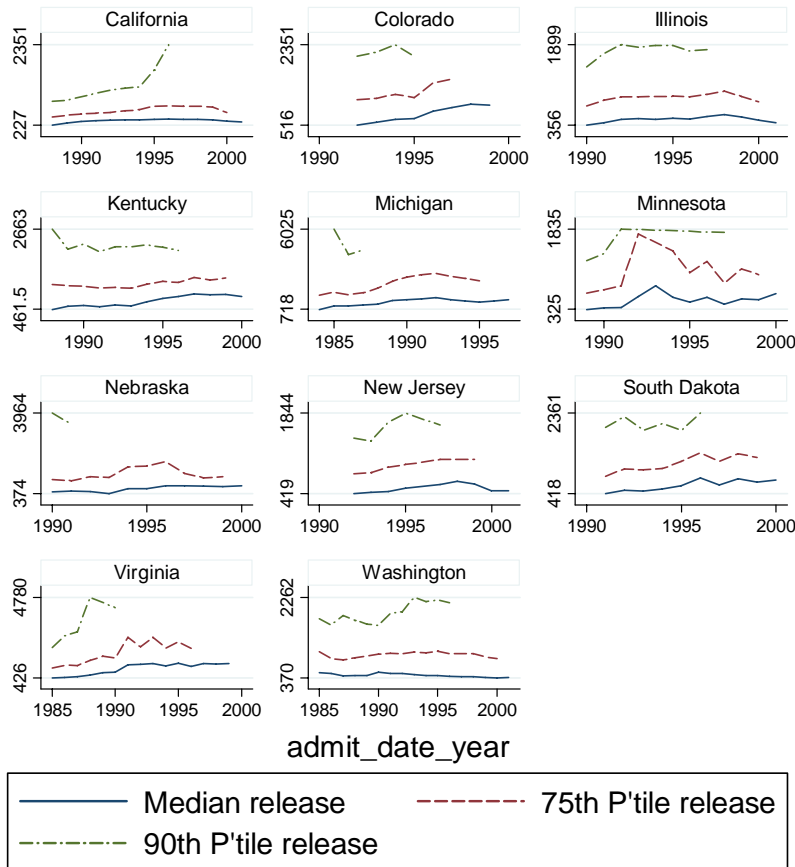
Source: NCRP. See Appendix.  
 Values do not sum to 1 if some offenses are unclassified.

**Figure 4A: Days to Release**  
**Median and 75th Percentiles**  
**Fixed Admission Cohort Size**



Source: NCRP. See Appendix

**Figure 4B: Days to Release**  
 Median, 75th, and 90th Percentiles  
 Fixed Admission Cohort Size



Source: NCRP. See Appendix

The results in Figure 4 suggest that the incarceration of increasingly minor offenders is partially responsible for the observed decline in time served. California's steep decline is now substantially flatter with a higher endpoint, and likewise for Illinois (with a median twice that in Figure 2). Most different is South Dakota, whose decline becomes a rise, and Colorado's and Kentucky's meager growth in Figure 2 becomes larger in Figure 4. Nonetheless, the general patterns seen in Figure 2 are still preserved in

several states, such as Michigan, Minnesota, Nebraska, Washington, and to a lesser extent Virginia. For the 90th percentile the patterns are harder to discern, though, since for many more entering cohorts the 90th-percentile prisoner has yet to be released in Figure 4. Thus some of the decline in time served seen in Figure 2 is likely due to the short terms served by prisoners who in earlier years may have avoided incarceration altogether. Nonetheless, even in Figure 4 we see several spells of declining, or at least relatively constant, median and even 75th-percentile time served.

## 1.2 Two Counterfactual Experiments

To better measure the importance of changes in sentencing and admissions policies to release rates, I develop two interesting counterfactuals in this section. Moving beyond simple quantile calculations, I can calculate what percent of the prisoners admitted in, say, California in 1988 are released in 1988, 1989, 1990, ..., all the way to 2002. And I can track how these year-by-year distributions move over time. With these numbers, I can derive two informative comparisons. First, I can compare the actual net increase in prisoners in each state from the first year of available data to 2002 to the hypothetical net increase that would have occurred had the state released prisoners at the same rate every year as it did in the first year. Call this the “fixed release” counterfactual. Second, I can compare the actual net increase to the hypothetical net increase that would have occurred had the state admitted the same number of prisoners in each subsequent year as it did in the first year. Call this the “fixed admissions” counterfactual.<sup>18</sup>

The values in these counterfactuals require some explanation. My goal is to understand what prison populations would look like had the rates of releases or the number of admissions not changed over the sample period. To do this, I first calculate how many extra prisoners each entering cohort contributes to each subsequent year: how many prisoners

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<sup>18</sup> In an earlier version of this paper, I computed several other possible counterfactuals, such as fixing the distribution of offenses (but allowing the number of admissions to grow), fixing the distribution of offenses and release rates, and fixing the release rates as well as the offense distributions while keeping the size of the admissions class constant as well. These additional counterfactuals do not provide much additional insight beyond that captured in the two I present here.

admitted in 1988 are still in prison at the end of 1988, of 1989, of 1990, etc. I then recalculate these numbers in one of two ways—by assuming that the distribution of release rates is constant for each cohort, or that the entering size is constant for each cohort.

In other words, any given point on the “actual data” curve is equal to:

$$pop_{t_1} = \sum_t (1 - \sum_{\tau} p_t(\tau)) admit_t \quad (1)$$

Here,  $p_t(\tau)$  is the fraction of prisoners<sup>19</sup> admitted in year  $t$  released  $\tau$  years after admission. The quantity is summed over all the years between the  $t_o$ , the first year for which data are available for that state, and the observation year  $t_1$ .  $pop$  thus measures the net increase in prison population between  $t_o$  and  $t_1$ . The fixed releases counterfactual sets  $p_t(\tau) = p_{t_o}(\tau)$  for all  $t$  (not  $\tau$ ), and the fixed admissions counterfactual freezes  $admit_t$  at  $admit_{t_o}$  for all  $t$ .

A concrete example can make this clearer. Assume that of all the prisoners admitted in year  $t$ , 50% are released that year, 30% in year  $t + 1$ , 10% in year  $t + 2$ , and 10% in year  $t + 3$  (so no prisoner serves more than three years). Then for each admission year  $t$ ,  $p_t(0) = 0.5$ ,  $p_t(1) = 0.3$ ,  $p_t(2) = 0.1$ ,  $p_t(3) = 0.1$ , and  $p_t(\tau) = 0$  for all  $\tau \geq 4$ . Table 1 below provides a hypothetical example in this state over five years. The value of 200 in the cell labeled (A) indicates that by the end of 1991, only 200 of the 1000 prisoners admitted in 1990 remain in prison (since 500 are released in 1990 and 300 in 1991). The value of 950 in the cell labeled (B) implies that by the end of 1991 prison admissions since 1990 have added a *net* 950 inmates to the system. In other words,  $950 = (1 - 0.5 - 0.2)(1000) + (1 - 0.5)(1500)$ . This is equation (1).

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<sup>19</sup> I allow the fraction to vary across five broad crime categories (violent, property, drug, miscellaneous, and unknown) and sum across categories to generate a single total population number.

Table 1: Hypothetical Releases

Year	Admits	1990	1991	1992	1993	1994
1990	1000	500	200 (A)	100	0	0
1991	1500		750	300	150	0
1992	2000			1000	400	200
1993	2500				1250	500
1994	3000					1500
Total		500	950 (B)	1400	1800	2200

Note what Table 1 does *not* tell us. Cell (B) does not give the actual prison population in 1991, nor does it provide the change in the actual prison population since the start of time series (here, 1990). Instead, it indicates how admissions and releases since 1990 have changed the prison population. The prison population in 1991 is surely larger than 950, since some prisoners admitted in 1989 are still serving time that year, given our hypothetical release schedule. And part of the change in 1991 is due to prisoners from 1989 and 1988 getting released. The “Total” row picks up none of these forces.

In effect, the “Total” row assumes that the prison population equals zero when the data start and calculates what the prison population would be in subsequent years. This is done solely as a concession to limitations in the NCRP data,<sup>20</sup> but it nonetheless yields informative numbers. This is particularly true in later years, where—given the relatively short terms actually served by prisoners in the United States—the “total” values converge on the actual sizes of prison population: by 2002, for example, very few pre-1990 prisoners are in the system.<sup>21</sup>

The fixed release counterfactual, then, compares two calculations of equation (1). The first, “true” calculation uses the values of  $p_t(\tau)$  in the actual data. The second, “counterfactual” calculation assumes that the release patterns never changes after the first year. For example, assume that

<sup>20</sup> By definition, data before the first available year are unreliable, so it is impossible to accurately estimate the release percentages  $p_t(\tau)$  for those years.

<sup>21</sup> In fact, for the Tier 1 states, the number of prisoners added between the first year of data and 2002 range from 88% and a bit over 100% of the total BJS prison counts for 2002 (values over 100% clearly reflect counting differences between the BJS and the NCRP). This demonstrates again the relatively short sentences served by a vast majority of prisoners.

the data start in 1988 and, in the real data, 45% of all prisoners admitted in 1988 are released in 1988 and an additional 30% in 1989. However, for those admitted in 1989, sentencing is tougher, and only 40% are released in 1989 and 25% in 1990. Table 2 compares then real and counterfactual versions.

**Table 2: Fixed Release Counterfactual Example**

	Real Data		Counterfactual Data	
	$t = 1988$	$t = 1989$	$t = 1988$	$t = 1989$
$p_i(0)$	0.45	0.40	0.45	0.45
$p_i(1)$	0.30	0.25	0.30	0.30

The fixed admissions counterfactual in turn allows the release rates to always take on their real-data values, but fixes the size of each entering cohort at the size obtained in the first year of available data. It does, however, allow the distribution of offense types to change with each cohort. In other words, assume that a state's real data starts in 1990. In that year, it admits 10,000 people to prison, 45% of whom are violent offenders and 55% of whom are property offenders; in 1991, it admits 15,000 people, 48% of whom are violent offenders and 52% of whom are property offenders. In the fixed admissions counterfactual, the fictitious entering cohort in 1991 numbers only 10,000 (fixed at 1990's level), but 48% of these are violent offenders and 52% property offenders (taking on the actual 1991 values).<sup>22</sup>

Figures 5 and 6 compare the actual and counterfactual outcomes. Two features immediately stand out—that fixing release policies has almost no effect on the net change in prison population, and that fixing admission rates leads many prison systems to steady if not shrinking populations.

Start with Figure 5. If punishments have become more severe over the sample period, the solid “actual” curve (the real data) should rise above the dashed “fixed releases” curve (the counterfactual data). And while that pattern generally holds for the Tier 1 states, the effects are slight: actual prison populations are about 1% to 8% larger than the coun-

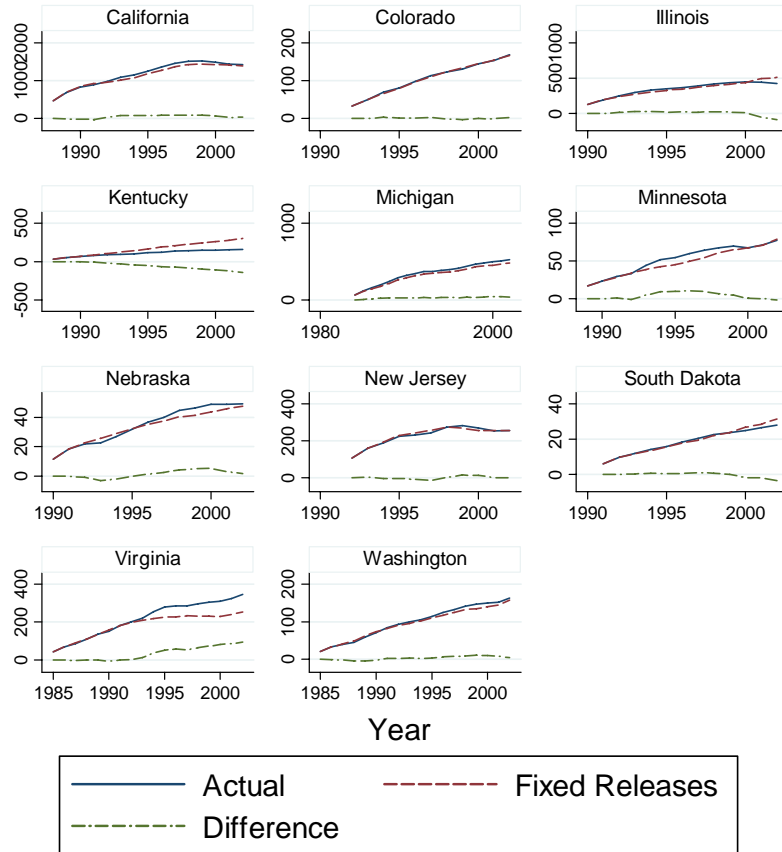
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<sup>22</sup> As shown by Figure 3, little turns on the changes in admissions composition. But again, those categories are by necessity quite coarse and may mask important intra-category changes.

terfactual populations in many states; in New Jersey, the two differ by exactly six prisoners (out of a population of over 25,000). Only Virginia witnesses a dramatic divergence, with the real population growing 28% faster, seemingly tied to Virginia's sentencing guidelines (which were adopted in 1992 and heavily amended in 1995). In a few states, the real populations are *smaller* than the counterfactual ones. Illinois, for example, added 20% fewer prisoners than had it kept its 1990 policies in place.

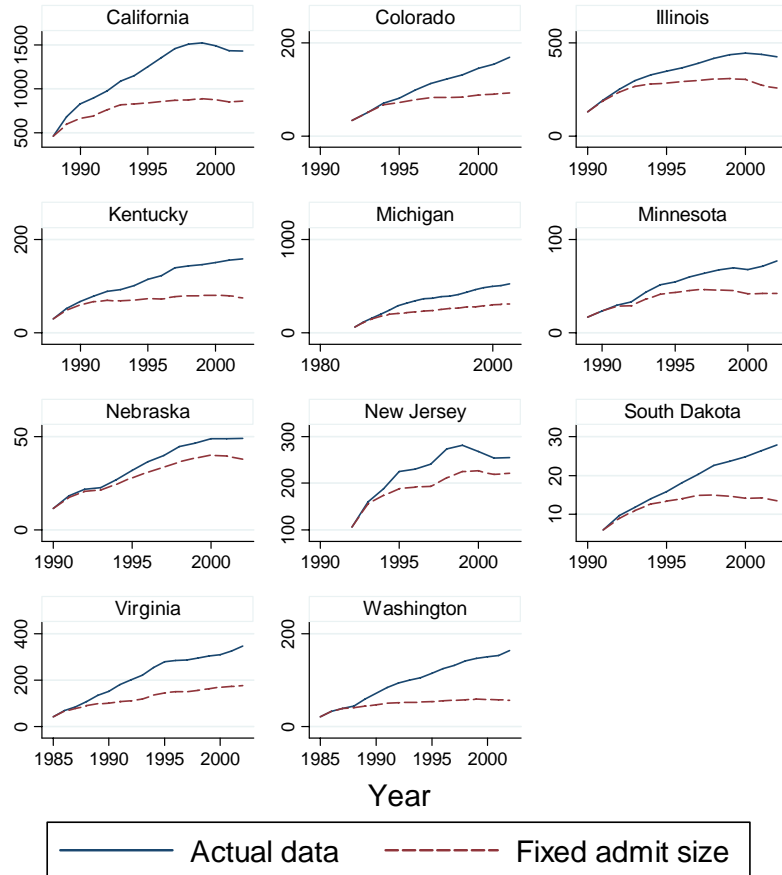
Figure 6 presents a fundamentally different picture. In almost all the states under consideration, had admission rates remained constant, prison population would have leveled off quickly. In fact, Figure 6 understates the extent to which growth would have slowed, if not reversed. Recall that the "fixed admissions" line for, say, California, is the net increase in prisoners due to admissions since 1988. During the period between 1988 and 2002, prisoners admitted prior to 1988 continue to be released, but these reductions are not reflected in the graphs. Central to our discussion here, Figure 6 demonstrates that release rates in most states are such that prison populations would quickly reach equilibrium. To the extent that prison populations are growing, the pressure appears to come from the admissions, not release, side of the ledger. Amongst the Tier 1 states, the only exceptions to this rule are Nebraska, Virginia and, to a (much) lesser degree, Michigan.

### Figure 5: Growth Rates Fixed Percent Released



Source: NCRP. See Appendix.  
Fixed releases fix the annual release rate per offense at the rate of first available year.

**Figure 6: Growth Rates**  
Fixed Admissions Size



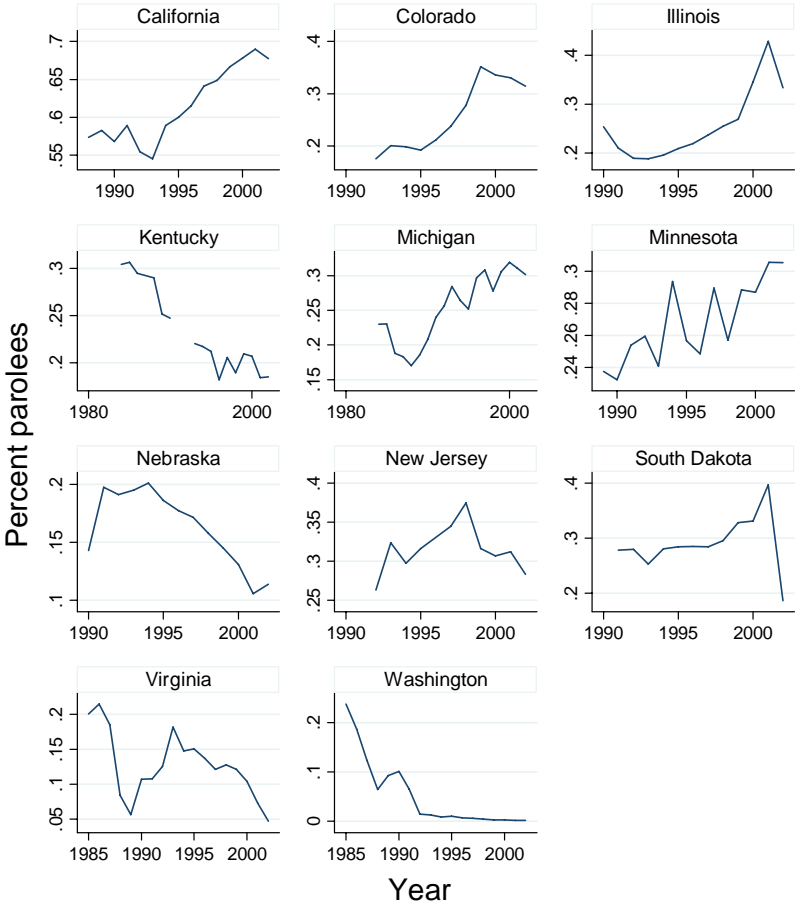
Source: NCRP. See Appendix.  
Admissions fixed at size of first available year.

### 1.3 The Problem of Violations

One common explanation for the growth in prisons is increasingly aggressive violations of parolees and probationers. If this is true, then the seemingly short times served seen above may be deceptive: if a large number of prisoners return quickly to prison due to (often technical) vio-

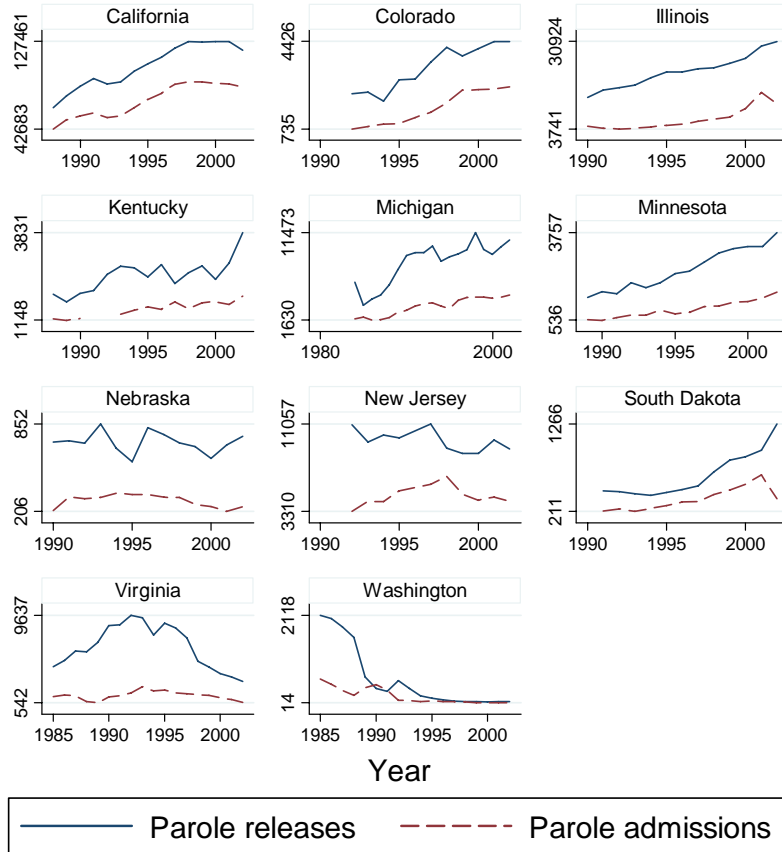
lations, then their effective sentence is much longer than the individual stints they serve. Figures 7 and 8 shed light on this issue.

**Figure 7: Parolees as Percent of Admits**



Note: Data from NCRP. See Appendix.  
 For Kentucky, percent in 1991 was 58% and percent in 1992 was 75%.  
 These appear to be errors.

### Figure 8: Parolee Movements Releases and Returns



Note: Data from NCRP. See Appendix.  
 Parole admissions for Kentucky in 1991 and 1992 were exceptionally high--see notes for Figure 7. These appear to be errors.

Figure 7 plots the fraction of each admission cohort entering prison due to either a voluntary or mandatory revocation of parole.<sup>23</sup> It suggests that while increased parole violations may explain some of the decline in apparent time served, they do not necessarily explain much of it. With

<sup>23</sup> Each state relies almost exclusively on either voluntary revocations or mandatory ones, but never both. I am unclear whether this reflects a reporting issue or genuine differences in state policy choices.

the exception of California, returning parolees make up a relatively small fraction of admittees, rarely comprising more than 30% of an entering cohort (in California the rate neared 70% by 2002). In several states, though, the fraction of parolees in each entering cohort rose over the sample period, usually from around 20% to around 30%. In a handful of states, especially Virginia and Washington, the fraction of parolees plummeted; these declines, however, are not due to dramatic improvements in re-entry programs but rather to restrictions on, or the abolition of, parole (so parolees simply no longer exist).

Probation violations are even less important for prison growth. In many of the states, no entering prisoner was classified as a probation violator, and outside of South Dakota (where the fraction peaked at 6% in 1994 before crashing to and remaining at 0% thereafter) no admission cohort consisted of more than about 1% or 1.5% violated probations. This should not be surprising, given that probation is generally given for less serious offenses.

Although somewhat of an aside, Figure 8 poses an even more serious problem for the argument that parole violations drive prison growth. With one exception for one year in Washington (and possibly two years in Kentucky, though these may be errors), the number of returning parolees is never larger than the number of parolees leaving prison. Moreover, the trends in returning parolees roughly track those in parole releases, and in many states the gap between releases and returns remains constant, or even grows slightly. In fact, Figure 8 suggests that the parole-driving claim may have inverted the direction of causality. As the prison population grows, the number of parolees grows with it, and a roughly-constant fraction of those paroled violate back to prison. It is not clear that it is good accounting to then attribute their return to the growth of prison populations.<sup>24</sup> Admittedly, if we did not violate parolees back our prison population would be smaller, but the rising number of parolees seems to be as much epiphenomenal of much deeper forces shaping prison populations as phenomenal in and of itself.

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<sup>24</sup> For example, assume that 20% of all parolees are violated back to prison. If the number of parolees rises from 1000 to 2000, the number of returning parolees in rises from 200 to 400, but the net effect of parole is a 800-prisoner *reduction* in the prison population (from  $1000 - 200 = 800$  to  $2000 - 400 = 1600$  prisoners on parole).

Some caution, however, is needed when considering these results. First, the NCRP only records the primary reason for admission. If an offender violates his parole and, in the process, is liable for a new substantive crime and is admitted for that new crime, he does not show up in the NCRP statistics as a parole readmission. If courts are more willing to incarcerate parole violators than otherwise-similarly situated offenders, then parole violations influence prison growth in a manner invisible to the NCRP. Offsetting this effect, however, is the possibility that parolees may be violated back—even on technical grounds—when the triggering act is actually a separate crime. Violations are easier than new charges for police and prosecutors to process, so they may use violations as procedural shortcuts for dealing with new offenses. In this case, the simple number of returning parolees overstates the importance of violations.

Also, bear in mind that much of the sample period witnessed a decline in the severity of the typical offender. This could imply that the constancy of parole violation is somewhat surprising: perhaps we should have expected the rate of violations to fall, if we expect lesser offenders to violate their conditions less often. Thus the constant ratio in Figure 8 may understate the importance of changing parole practices.

NCRP statistics therefore indicate that parole violations play a role—parole violators comprise about one-third of all admission—but not a dominant role in driving up prison populations. For our purposes here, however, the relatively mild *changes* in parole violators' share of prison admissions suggest that much of the apparent stability or decline in time served has not come from, say, breaking one long sentence into two shorter terms divided by an unsuccessful stint on parole. Furthermore, Figure 8 makes it clear that any story about parole violation's role in prison growth must address trends in parole releases as well. It is impossible, however, to say *a priori* whether the NCRP's limitations lead to over- or under-estimates of parole's role in prison growth. Regardless,

## 2 The Determinants of Sentence Length

The results in Part 1 provide a highly aggregated view of trends in time served. In general, prisoners serve short sentences, and the sentences they do serve in many cases appear to be growing shorter still. Yet these broad patterns may mask important underlying variation in how different categories of inmates are treated: black prisoners, for example, need not

face the same probability of release as otherwise-identical non-black inmates. This section considers the importance of several inmate-specific characteristics—race, sex, age, and offense—on release times. Elsewhere, I examine the causal effect of more macro-level factors, such as state economic health and state politics, on the observed trends in time served (Pfaff 2008b).

The basic model used here is the Cox proportional hazard model, which in its most general form is:

$$h_{is}(t) = h_o(t|s) \exp(\beta \text{trait}_i + \Gamma X_i + \Phi Z_{st}) \quad (2)$$

Here,  $h_{is}(t)$  is the hazard rate, the probability that offender  $i$  in state  $s$  is released at time  $t$ , conditional on having stayed in prison until  $t$  in the first place.  $h_o(t|s)$  is the baseline hazard, which may be conditioned on the state in question either through stratification or a shared frailty (random effects) parameter.<sup>25</sup>  $\text{Trait}_i$  is the particular individual-level characteristic under observation for inmate  $i$ ;  $X_i$  the vector of all other individual-level traits (which are fixed for each inmate); and  $Z_{st}$  a vector of (generally) time-varying, state-level controls.

In particular, *trait* represents one of eight possible dichotomous relationships: black/non-black, male/female, age-below-21/age-21-and-above, age-between-21-and-40/age-below-21-or-above-40, age-above-40/age-40-and-below, and violent-, property-, or drug-offender/all other offenses. For reasons explained below, I run the model in equation (2) separately for each pairing. The vector  $X$  reports the offender's race, sex, age, offense category, and controls for sentence imposed and credit for time served prior to admission,<sup>26</sup> though it excludes whatever factor is being used for *trait*.<sup>27</sup> The vector  $Z$  consists of controls for several potential state-level confounders: citizen- and politician-ideology, the poverty

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<sup>25</sup> Under stratification,  $h_o(t|s) = h_{os}(t)$ ; under shared frailty,  $h_o(t|s) = h_o(t)\alpha_s$ , where  $\alpha_s$  is a state-level random effect.

<sup>26</sup> Black and sex are dichotomous. Age, offense, sentence imposed, and credited time are all discrete polychotomous variables with six (for age) to seventeen (for offense) categorical values. As explained in the Appendix, clustering was essential to calculate missing values, and it facilitated the matching technique discussed below.

<sup>27</sup> In other words, when *trait* is black/non-black,  $X$  does not include race; when *trait* is under-21/21-and-over,  $X$  excludes age; and so on.

rate, per capita rates of black men under the age of 35, total per capita crime, the real per capita shortfall in state revenue, the prison admission rate, and whether the state possesses a truth-in-sentencing law or sentencing guidelines.<sup>28</sup> The Appendix discusses the particulars of the data in more detail.

The variable *trait* is broken up into eight dichotomous versions to limit the effect of model dependence. As Ho et al. (2007), Abadie et al. (2007) and others have demonstrated, the results returned from regression models on observational data can be very sensitive to functional form. One way to minimize this model dependence is to match the data, ensuring that each “treated” observation has a parallel “control” observation that looks sufficiently similar to it. In this paper I employ Ho et al.’s (2007) non-parametric matching protocol, MatchIt.

The intuition behind non-parametric matching is straightforward. The goal is to convert observational data into something that looks more experimental, by ensuring that the “treated” and “control” groups look as similar as possible, thus minimizing the importance of confounding factors (and thus the importance of how they are included in the econometric model). Here, for example, when *trait* is the black/non-black pairing, the matching algorithm takes each black inmate and looks for a non-black inmate admitted in the same state and the same year with identical values for sex, age, offense, sentence, and time credit. If no such non-black inmate can be found, the black inmate is dropped. As Ho et al. (2007) demonstrate, this approach leads to substantially more stable, less model-dependent results.<sup>29</sup>

Matching dictates several features of my models here. First, it requires that I use dichotomous treatment variables, which I why I employ multiple binary age and offense breakdowns rather than a single multivariate age or offense treatment. Second, as a result of dichotomization, I examine each trait in a separate regression, rather than testing several

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<sup>28</sup> These latter two are not time-varying but take on the value at the time of the prisoner’s admission, since prisoners are subject to the sentencing laws in place at the time of admission—more accurately at the time of the relevant offense, but that is invisible in the data—not the time of potential release.

<sup>29</sup> It should be pointed out that this matching approach is *not* equivalent to using a propensity score. As Ho et al. (2007) make clear, the non-parametric nature of their matching technique confronts directly the core limitations of propensity scores.

causal relationships in a single regression. And third, because matching is only done on four traits, it is only on those four that I focus my analysis. Thus I will not discuss in any depth the results for the state-level factors; I leave that analysis for Pfaff (2008b).<sup>30</sup>

Equation (2) presents the basic form of the model. I ultimately run eleven different versions of equation (2), varying the exact set of control variables, assumptions about how to model state-level heterogeneity, and whether to include year-interaction terms to allow the effect of the particular trait to vary with cohort year.<sup>31</sup> In all cases standard errors are clustered at the state level. Table 3 lists the basic elements of the eleven specifications, and the Appendix discusses them in more depth. In the Figures below, the number under each result refers to the model number given in the first column of Table 3.

The NCRP is an extensive dataset: the core dataset for the eleven Tier 1 states contains over three million offender observations and over eight million offender-year observations.<sup>32</sup> In order to make estimation computationally feasible, I took a 1% sample of my data within each state cohort-year pair, leaving me with something on the order of 80,000 offender-year observations. To avoid cluttering the body of the paper, I provide the summary statistics for the data in the Appendix—this requires eight tables, since each matched dataset requires its own set of summary statistics.

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<sup>30</sup> Furthermore, matching within state-year pairs should minimize the importance of these factors, since my state-level controls are annual and thus only vary across state-year pairs.

<sup>31</sup> I report the results for the coefficient on *trait* for all these results, but for ease of exposition not those on the interaction terms. In every case they are statistically insignificant, and including them seemed only to cause my standard errors to grow substantially.

<sup>32</sup> In other words, I treated an offender admitted on August 1, 1988 and released on June 3, 1990 as three observations: one from August 1, 1988 to December 31, 1988; one from January 1, 1989 to December 31, 1989; and one from January 1, 1990 to June 3, 1990. As discussed in the Appendix, this is done to accommodate time-varying state controls.

**Table 3: Basic Model Specifications**

Model	Individual Controls	State Controls	Other variables	Heterogeneity controls
1	Yes	No	No	No
2	Yes	Yes	No	No
3	Yes	No	No	Stratify
4	Yes	No	No	Shared frailty
5	Yes	No	Cohort Year	No
6	Yes	Yes	Cohort Year	No
7	Yes	No	Cohort 95	No
8	Yes	No	Start Year	No
9	Yes	Yes	Start Year	No
10	Yes	No	Year 5	No
11	Yes	No	Year 10	No

Note: Individual controls are black, sex, age, offense, sentence, and credit for time served. State controls are per capita crime rates, percent young black male, percent below the poverty line, citizen and state ideologies, government revenue shortfall, per capita prison admissions, and whether the state had a truth-in-sentencing law or sentencing guidelines in the cohort admission year. “Cohort year” refers to an annual interaction effect allowing the effect of *trait* on the hazard to vary with the calendar year of entry, and “Cohort 95” replaces the annual interaction with a single interaction looking at changes before and after 1995. “Start Year” is akin to Cohort Year, but rather than using the calendar year uses the number of years since a particular state starting contributing viable data to the NCRP. “Year 5” replaces the annual Start Year interaction with a single term looking at changes before and after the fifth year of data, and “Year 10” does the same with the inflection point at the tenth year. Stratify and shared frailty are defined above at note 25.

Figures 9a and 9b provide Forest plots of the hazard ratios for each of the eight traits, where the “hazard” is release from prison—prisoners, in other words, *want* to experience the hazard, so a ratio greater than one implies a greater likelihood of release in a given period. Since these are ratios, the critical question is whether the results differ from one, not zero: a hazard ratio of one implies that the two groups in question face the same hazard (i.e., that the particular trait has no apparent effect). These graphs provide both the point estimate and the upper and lower bounds on the 95% confidence interval. The results are defined such that

the hazard ratio refers to the demographic given in the title of the figure: for Figure 9a:1, for example, a value greater than one implies that black prisoners are more likely to be released in any given period than otherwise identical non-black prisoners.

Figure 9a: Relative Hazard Ratios

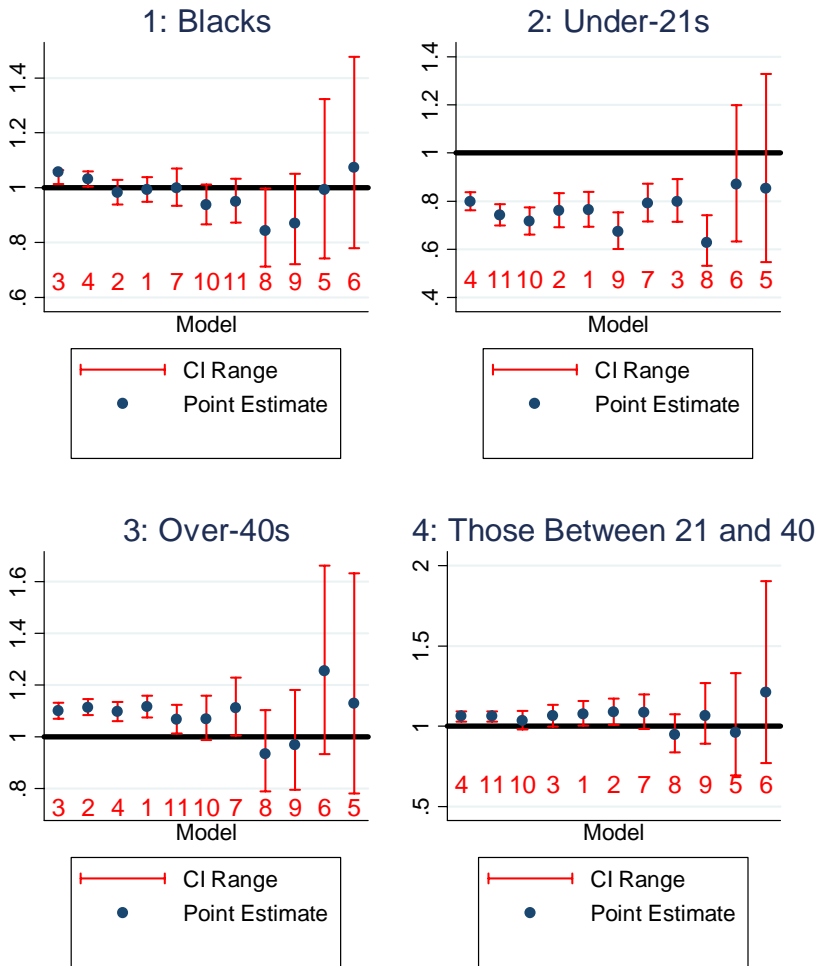
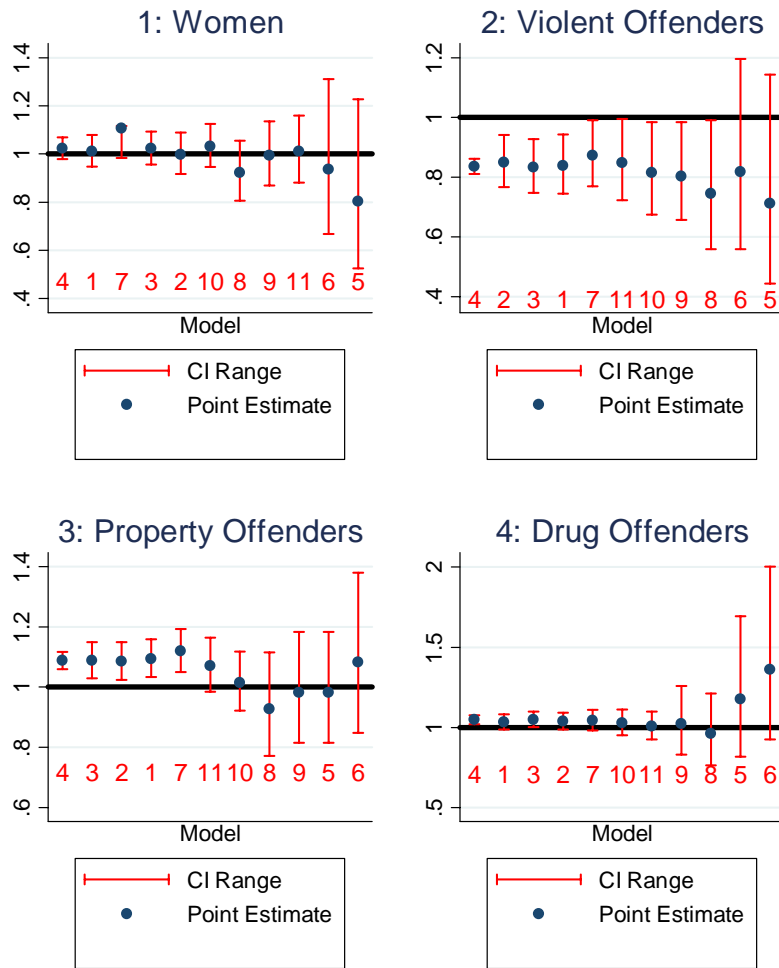


Figure 9b: Relative Hazard Ratios



Several interesting patterns emerge from these figures. First, race does not seem to explain much difference in sentencing outcomes—the effect is always statistically insignificant, and the point estimate jumps back and forth across the 1-line. Though contrary to conventional wisdom about criminal sentencing, it is consistent with the ambiguous results concerning race and the criminal justice system returned in much recent work (see, e.g., Sampson and Lauritsen 1997).

What is perhaps more interesting is that, due to limitations in the NCRP, I cannot control for prior criminal history. Thus blacks and whites seem to serve similar sentences for similar crimes, despite the generally longer prior histories possessed by black inmates. Some have argued that while blacks and whites may serve similar sentences conditional on the crime and the offender's prior criminal history, that latter conditioning is problematic: because of racial differentials in arrest and conviction, blacks have disproportionately longer criminal histories, and thus controlling for prior history masks the true source of racial bias at sentencing. These results at the very least complicate that argument.

The potential problem of increasingly marginal offenders rears its head here as well. Blacks could be serving roughly the same number of days as whites because blacks make up a disproportionate number of the relatively minor offenders who would not serve any time in prison under a more lenient system; this is analogous to the problem illustrated in Figure 4 above. For two reasons, however, this issue is not as relevant here. First, I condition on offense and sentence length. And while the offense definitions are broad, in conjunction with sentence length they should control for this effect to some degree. In other words, my results show that blacks sentenced for the same offense for the same number of years, a rough proxy for similar types of crimes, serve roughly the same number of days.

Nonetheless, we may remain concerned that blacks are disproportionately receiving an incarceratory sentence within these cells (i.e., blacks and whites sent to prison serve similar sentences, but disproportionately more blacks are sent to prison in the first place). But—and this is the second reason why the increase in marginal offenders may not undermine the causal story being proposed here—in most cases the share of each cohort that is black does not jump much over the sample period, usually on the order of plus or minus five percentage points. This suggests that the ever-broader net cast by the criminal justice system is sweeping up blacks and whites in roughly constant proportions.

Like the effect of being black, being female, between the ages of 21 and 40, or a property or drug offender does not appear to influence sentence length in any statistically, or certainly numerically, significant way. (The property and drug effects may simply reflect the fact that violent offenders serve substantially longer sentences than the other two groups.) The result for age should not be surprising, since under-21s are treated

relatively worse and over-40s relatively better,<sup>33</sup> so almost by default those between 21 and 40 should be treated roughly in the middle.

The two groups that clearly face harsher treatment are those under the age of 41 and violent offenders. The latter result should come as no surprise. The former is perhaps less expected, but is in fact consistent with a growing sociological literature that suggests courts impose harsher sentences on younger—and particularly younger black male—defendants (Steffensmeier et al. 1998, Spohn and Holleran 2000, Ulmer and Johnson 2004, Kurlychek and Johnson 2004). As Bushway and Piehl (2007) point out, at least one state, Virginia, makes the youthfulness of an offender an *aggravating* factor in its sentencing guidelines.

The normative implications of this “age penalty” are difficult to unravel, however. On the one hand, we want to provide young offenders with a second chance, and it is harder with a younger defendant, at least when a first- or second-time offender, to determine whether the bad conduct reflects the start of a life of crime or adolescent acting-out. On the other hand, a youthful offender with an identically long criminal record as an older criminal has committed more bad acts in a shorter period of time, and the young are more likely to commit crimes in the future than the old. Bushway and Piehl (2007) discuss these challenging policy tradeoffs in depth.

Two possible limitations with the results in Figure 9 deserve attention. First, California represents a disproportionate share of the data, on the order of 40%, raising the possibility that its behavior overly influences the results. To examine this possibility, I rerun Models 1 and 2 without California in the data (labeling them Models 12 and 13, respectively). I present the specific results in the Appendix, but the basic outcome is that the relative hazard for blacks rises and that for women falls, but neither the new coefficients nor the difference between the coefficients with and without California are statistically significant. All other results remain roughly the same. Thus California does not appear to be driving the result here.

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<sup>33</sup> Note that the share of the prison population has risen steeply in several states during the 1990s, sometimes by as much as ten-, fifteen-, or (in California) twenty percentage points. Thus the possibility that the improved treatment stems in part from increasingly marginal offenders is greater, although again, these would have to be increasingly marginal offenders within a given offense-class/sentence-length category.

Second, there is a potential source of post-treatment bias lurking in these models. One of the variables in the models is sentence length. These models thus ask if, for example, a black 25-year old male arsonist sentenced to ten years with no credit for time served spends as many days in prison as a white 25-year male arsonist sentenced to ten years with no credit for time served. This is certainly a valid question. But perhaps we are concerned that the real source of the problem is that a black 25-year old male arsonist is more likely to get a ten year sentence than a similarly-situated white defendant. Conditioning on sentence length masks this effect. Thus I rerun Models 1, 2, 4, 12, and 13 without the sentence length term (and named the Models 14 through 18, respectively). Figure 10 provides the comparison, with the sentence-length-excluded models on the right. Except for violent offenders, there is no meaningful difference between the two sets of results.

Figure 10a: Role of Sentence Length

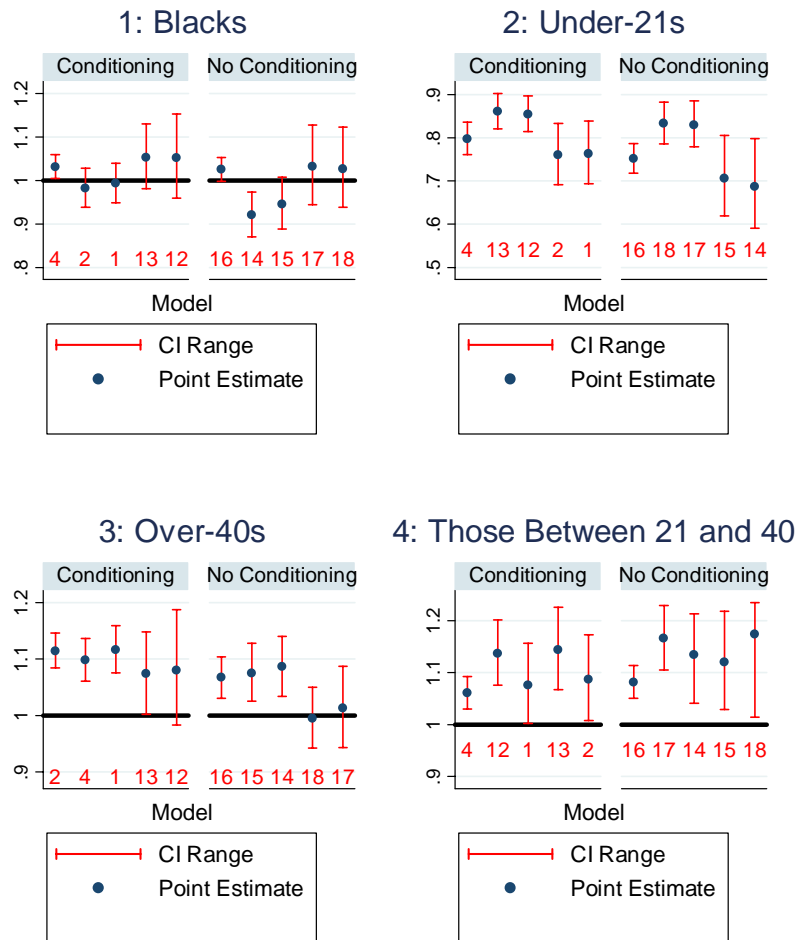
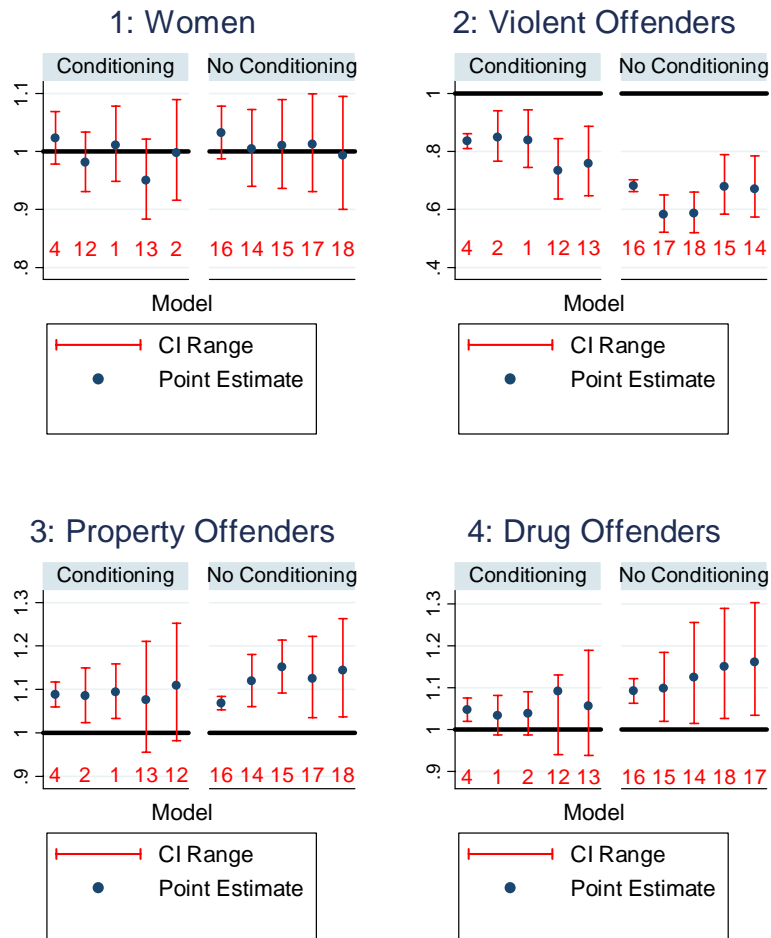


Figure 10b: Role of Sentence Length



Thus the demographic results can be quickly summarized. Blacks and women appear to be treated no differently than their complements, the young and the violent worse, and the old better. These findings do not seem to be driven by California, and they do not change substantially when we remove conditions for sentence length imposed, suggesting that the factors here (other than violent offending) do not meaningfully influ-

ence the sentence actually imposed at sentencing (as compared to the time actually served).

### 3 Conclusion

The dramatic boom in American prison populations over the past thirty years is a striking and oft-mentioned fact, but our understanding of its causes and distributional effects remains surprisingly weak. This paper has sought to shed at least some light on the issue by probing the role of changes in time served. Increasingly severe sentencing regimes are often cited as important forces in the growth of US prison populations, but there have been no efforts to examine this claim empirically.

The results in this paper indicate that the story is quite complex. In most states in my sample the median time served has been either relatively flat or declining. At least some of this leveling or decline, however, appears to be due to increasingly marginal offenders who would likely have received no term of incarceration in earlier years: by serving short sentences they pull down the median. Yet even when controlling for this effect in a way unfavorable to the declining-median hypothesis, we still see that in many states median-, and even 75th percentile-, times to release often leveled out or fell during the sample period, though certainly not as dramatically.

These results make clear that the primary engine driving prison growth has been changes in admissions, not time served. These two concepts are not entirely distinct, since the decision to incarcerate can be thought of at some level as a decision to not release immediately at time zero. But it is nonetheless useful to separate out whether prison growth is being driven by (1) locking up those who otherwise would have gone to prison for much longer terms, (2) locking up those who otherwise would not have gone to prison for long terms, (3) locking up those who otherwise would not have gone to prison for short terms, or some combination of (1) and (2) or (1) and (3). The results from this paper suggest that reason (3) is the key force.

This points to an important avenue of future research: understanding what has shaped changing admission patterns over time. While there has already been some work done along these lines, with the exception of that by Listokin (2003) all suffer from substantial statistical and conceptual flaws (Pfaff 2008a). Moreover, all work to date has focused solely on total

aggregate admissions, rather than examining how the distribution of admittees has changed over time and what forces have shaped those changes.

The distributional findings here are perhaps less counter-intuitive than the durational ones, but they provide important support for current theories about the role of race and age at sentencing. For example, much of the work on age and sentencing length cited above (Steffensmeier et al. 1998, Ulmer and Johnson 2004, Kurlychek and Johnson 2004) relies on data from a single state, Pennsylvania; Spohn and Halleran (2000) use data from three cities. The results here, derived from data rarely used to address the issue, indicate that the localized findings in these studies hold across a range of states over many years. And the results for race, similarly developed from data not used in most other studies, provide more evidence that race's role in the criminal justice system is clearly highly contextual and that simple stories of bald racial discrimination are simply not supported by the data, at least at the sentencing level.

## 4 Appendix

This appendix discusses in more detail the data and models used in this paper. The first part of the appendix provides a close look at the National Corrections Reporting Program data and the steps taken to clean it. It also explores the external consistency of the NCRP and our ability to extrapolate from the states included in this study to the United States as a whole. The second part describes the empirical models used in Part 2 above in more detail.

### 4.1 The National Corrections Reporting Program

#### 4.1.1 Basic Structure of the Data

The Bureau of Justice Statistics began gathering data for the NCRP in 1983, with data available through 2002 when I started work on this paper. Participation in the Program is voluntary, and the number of states contributing data has fluctuated over the years, though nearly forty participate now (up from thirteen in 1983). Participating states fill out an information card for every inmate entering and leaving prison. As a result, the NCRP provides offender-level data on not only the exact date of entry into and exit from prison but, among other things, on each inmate's race, sex, age, educational background, offenses of conviction, sentences imposed, nature of incarceration (new commitment, parole violation, etc.), subsequent malfeasance while in prison (and its sentencing implications), and type of release (completion of sentence, parole, etc.). It is the most detailed centralized source of information on prison inmates available.<sup>34</sup>

Unfortunately, the NCRP suffers from several noticeable shortcomings. Most immediately troubling, there are significant discrepancies between the NCRP and other sources of data. For many states in many years, the total number of admission or release observations in the NCRP

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<sup>34</sup> Some entries theoretically available prove not to be. For example, the NCRP includes a binary variable indicating whether the inmate has a prior felony conviction, but it is blank for every observation in every state in every year of data. Other variables, such as education, are not always left blank but are reported with enough infrequency to make them hard to use. Despite these omissions, the NCRP remains the most detailed dataset of its kind.

do not match those given in the annual Bureau of Justice Statistics prison data reports. In some cases, the NCRP numbers differ by as much as 75% or 100%. As a result, I classify the states into three categories: Tier 1 states, Tier 2 states, and excluded states. Tier 1 states are those that have a sufficiently long string of consecutive years (usually close to at least ten) that do not deviate too much (generally by less than 10%) from the corresponding BJS data. Tier 2 states either have shorter runs of reliable data or have a handful of years that deviate more substantially (but still generally less than 25%) from the BJS data.<sup>35</sup> The rest of the states are wholly excluded. For the purposes of this paper, I focus on Tier 1 states. Table A.1 lists the states in each Tier and the years that are included. In the end, I have 164 state-year pairs in Tier 1, containing information on over three million inmates.

The second significant shortcoming of the NCRP is that the admission and release datasets are wholly separate. This does not restrict my ability to calculate time served by those released, since the release file contains both admission and release dates for each inmate. But in order to calculate either the time-served quantiles or the hazard ratio, I need to know both how much time each released prisoner in fact served and how many members of each entering cohort remain in prison at the end of my sample period. The separate admission and release files complicate this latter calculation.

In effect, I must create phantom observations for the right-censored, unreleased prisoners. To do this, I use the admissions files to determine how many inmates of a certain type were admitted in a particular year, all the subsequent release files to calculate how many of those prisoners were ultimately released, and then create dummy entries to make up the difference. The ideal approach is the following: Assume that in 1990 California admits 100 25-year-old black, male inmates for second-degree assault with sentences of 10 years and no credit for prior time served. The release files for 1990 through 2002 indicate that 95 inmates meeting these criteria—admitted in 1990, 25-years old, black, etc.—have been released. I then create five dummy inmates with these traits, giving me the complete set: for 95 inmates I know the exact day of entry and release, and

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<sup>35</sup> Georgia, though listed as a Tier 2 state, should really be thought of as the sole occupant of Tier 3. Its data are even less in line with BJS data, and were it not for a paucity of Southern states, it would have been in the excluded category. I treat Georgia carefully.

for five of them I create fake admission dates (January 1, 1990, for simplicity) and know that they have not been released as of December 31, 2002. With these dummy inmates, I effectively created an integrated admissions and release file, allowing me to easily compute quantiles and hazards.

**Table A.1**  
**Years Available for Reporting States**

Tier	State	Start Year	End Year
1	California	1988	2002
1	Colorado	1992	2002
1	Illinois	1990	2002
1	Kentucky	1984	2002
1	Michigan	1984	2002
1	Minnesota	1989	2002
1	Nebraska	1990	2002
1	New Jersey	1992	2002
1	South Dakota	1991	2002
1	Virginia	1985	2002
1	Washington	1984	2002
2	Louisiana	1992	2000
2	Mississippi	1988	2002
2	Nevada	1990	1994
2	New Hampshire	1984	1995
2	New York	1995	2002
2	North Dakota	1984	2002
2	Pennsylvania	1992	2002
2	South Carolina	1985	2002
2	Utah	1993	2002
2	Wisconsin	1984	1994
2	Georgia	1992	2002

In theory, one could fully reconstruct the exact admittees not yet released by conditioning on every characteristic provided in the NCRP. As will be immediately clear, however, reporting errors in the NCRP render this approach impossible. Ultimately, I condition on six variables: race, sex, age, most serious offense, sentence imposed, and credit for time

served. But I had to adjust these categories to account for flaws in the NCRP.

First, in many cases it appears that over 100% of inmates of a particular type in a particular cohort are subsequently released. A state may admit 100 inmates of a particular type in 1990 but appear to release 120 such prisoners from 1990 over the next 13 years. In numerous cases, prisoners who do not exist in the admissions file appear in the releases file—for example, no prisoner is admitted in California in 1988 for crime code 13 (homicide, distinct from murder (10), non-negligent manslaughter (15), vehicular manslaughter (20) and involuntary manslaughter (40)), yet several prisoners released in subsequent years from the class of 1988 are listed as having committed code-13 crimes.

When looking at (unconditional) aggregate admissions and releases during the years listed in Table A.1, the over-releases were not significantly large, often less than 1%. In these cases, I simply rounded the releases down to get 100%.<sup>36</sup> More detailed measures—such as those conditioning on all six factors listed above—led to more substantial problems. To circumvent this problem, I clustered many of the measures. I narrowed the NCRP's 186 offense codes down to sixteen broad categories,<sup>37</sup> its continuous measure of age into six categories,<sup>38</sup> its continuous measure of sentence length imposed into fifteen categories,<sup>39</sup> and its continuous

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<sup>36</sup> In effect, I zeroed out all prisoners released after the 100th-percentile prisoner was released. When focusing on quantiles rather than averages, eliminating the few longest-serving inmates does not alter the basic findings. Were I focusing on averages, however, this may have been a contestable assumption, since averages are much more sensitive to outliers—one reason for focusing on quantiles.

<sup>37</sup> The sixteen are: murder and other killing offenses (including assault with intent to kill), kidnapping, sex offenses, robbery, assault (including hit-and-run and child abuse), burglary, arson, theft and associated offenses (including trafficking, distributing, or receiving stolen goods), drug trafficking, drug possession, persistent felon violators, unknown offenses, and four “other” categories (other violent, other property, other drugs, and other (lesser) crimes that do not fit easily into the violent/property/drug categories).

<sup>38</sup> The six are under sixteen, sixteen to under eighteen, eighteen to under twenty-one, twenty-one to under thirty-one, thirty-one to under forty-one, and forty-one and over.

<sup>39</sup> The fifteen are less than a year; one year ranges from one to two years through nine to ten years; ten to fifteen years, fifteen to twenty years; twenty to twenty-five years; over twenty-five years but not life; all life and death sentences. In gen-

measure of credit for time served into five categories.<sup>40</sup> I kept the NCRP's binary classification of sex but reduced its five-fold classification of race to a binary black/not-black measure. As a result, I have potentially 28,800 ( $16 \times 6 \times 15 \times 5 \times 2 \times 2$ ) different "types" of offenders (and dummy offenders) per state per year. With these broader categories, the over-release problem was substantially mitigated.

#### 4.1.2 External Validity

My sample here consists of eleven Tier 1 states, which house approximately 33% of the nation's inmates. Note that this implies that California makes up approximately one-third of my sample, since California contains approximately 10% of the nation's prisoners, second only to the combined Federal/DC prison system. It is thus essential to understand how generalizable the findings here are to non-reporting states. Since the NCRP is a voluntary program, there is no reason to assume that the reporting states are merely a random sample of the entire nation, particularly since some states have opted out after opting in.

Figure A.1 provides quantile-quantile (QQ) plots comparing key demographic, economic, and political traits of Tier 1 states with non-Tier 1 ("excluded," which here includes Tier 2 states as well) states. These plots provide an effective way to compare the distribution of values across two datasets. Each axis measures the values of one of the two datasets being compared, and each point on the plot represents a quantile. If each point presents a decile, then if the first point on the plot is at (3,5) the 10th percentile value of the dataset on the  $x$ -axis is 3, and the 10th percentile value of that on the  $y$ -axis is 5. If the datasets have identical distributions, all the points will fall on the 45-degree line. If the points are above the 45-degree line then the dataset on the  $y$ -axis has larger values than that on the  $x$ -axis for that quantile.

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eral, at least 80% of all sentences for all categories of crimes are below ten years, and 90% are below twenty.

<sup>40</sup> The five categories are no credit, less than one year, one year to three years, three years to five years, and more than five years.

Figure A.1: Quantile-Quantile Plots  
Tier 1 vs. Excluded States



The results in Figure A.1 are mixed. In general, Tier 1 states have lower property crime rates, larger populations, more young men, lower unemployment and poverty rates, higher per capita incomes, and more conservative citizens and lower houses. Yet there are several categories where the two sets of states are fairly similar, including violent crime, percent young black (for most of the observations), and Democratic governors. And even for many categories where there are systematic differ-

ences, the differences do not appear to be large. Even incarceration rates, which seem substantially lower in the Tier 1 states, are actually fairly similar but for one outlier.

Ultimately, there is no metric to tell us whether extrapolation is acceptable. Figure A.1 indicate that some care should be taken when extrapolating from Tier 1 states to the rest of the country, but also that the Tier 1 states do not differ radically from the rest of the country. These results thus suggest that cautious extrapolation may not be out of place with these results.

## 4.2 Empirical Models

Part 2 above sets forth the basic structure of the regressions it develops. This part of the Appendix covers four issues in more detail: the specifics of the models listed in Table 3, the results for the excluding-California models, the sources of the data used in this paper, and the summary statistics for the models.

### 4.2.1 Model Specifics

Table 3 lists the eleven basic models presented in Figure 9. I also ran seven additional models—two (Models 12 and 13) excluding California, and five (Models 14 through 18) excluding the control for sentence length. Each model was a Cox proportional hazard model. The specifics of each are the following:

**Model 1.** This model contains the core individual traits: black, sex, age, offense, sentence length, and credit for time served. Part 4.1 provides the definitions of those variables. Standard errors are clustered at the state level.

**Model 2.** This model contains all the variables in Model 1, along with nine state-level controls: per capita crime rates, percent young black male, percent unemployed, citizen and state ideologies, government revenue shortfall, per capita prison admissions, and whether the state had a truth-in-sentencing law or sentencing guidelines in the cohort admission year. All but the guideline and TIS variables are time-varying (annually), which requires me to replace inmate observations with inmate-year observations. Standard errors are again clustered at the state level.

**Model 3.** This model contains the variables in Model 1 and stratifies by state. In other words, it assumes that the baseline hazard can vary across states:  $h_o(t|s) = h_{os}(t)$ . Standard errors are again clustered at the state level.

**Model 4.** This model also contains the variables in Model 1, but adopts a shared frailty/random effects structure. Here,  $h_o(t|s) = h_o(t)\alpha_s$ , where  $\alpha_s$  is a state-level random effect.

**Model 5.** This model uses the variables in Model 1, but also includes a year interaction effect. In particular, it adds to equation (2) two terms,  $trait_i \times t$  and  $trait_i \times t^2$ , where  $t$  is an index of the calendar year ( $t = year - 1984$ , since 1984 is the first year for which any state has data).  $t$  is defined as the year in which offender  $i$  enters prison, so  $trait_i \times t$  and  $trait_i \times t^2$  are *not* time-varying for a particular offender (in other words,  $t = 2$  for a prisoner admitted in 1986, no matter how many years he serves in prison). Rather, they ask if the overall effect of *trait* varies across cohorts. Errors are again clustered at the state level.

**Model 6.** This is Model 5 with the nine state-level controls added in as well.

**Model 7.** This is a simplification of Model 5. Rather than including an indicator  $t$  that varies every year, I use a binary indicator  $t_{95}$  that equals zero for every year before 1995 and one for every year from 1995 on. This simpler model considers whether a more dramatic break can be seen between older and newer time periods. Again, errors are clustered at the state level.

**Model 8.** This model is similar to Model 5. However, instead of tying  $t$  to the calendar year, the model ties it to a state-specific calendar. In other words, data for California start in 1988 and for Illinois in 1990. Thus  $t_{CA} = 0$  in 1988,  $t_{CA} = 1$  in 1989, and so on, while  $t_{IL} = 0$  in 1990,  $t_{IL} = 1$  in 1991, and so on. This allows states to follow slightly different time paths. Errors are clustered at the state level.

**Model 9.** This is Model 8 with the nine state-level controls included.

**Model 10.** This is a simpler version of Model 8, replacing the annual indicator  $t_s$  with a binary indicator  $t_{5s}$ , which equals zero for the first four years the state provides data and one for the fifth year on. Like with Model 7, the goal is to look for a general shift over time that may be hard to see when considering annual changes.

**Model 11.** This is a variant of Model 10, but instead of using the binary  $t_{55}$  I use  $t_{510}$ , which equals zero for the first nine years of data and one for the tenth year on.

**Model 12.** This is Model 1 without California.

**Model 13.** This is Model 2 without California.

**Model 14.** This is Model 1 without the sentence length control.

**Model 15.** This is Model 2 without the sentence length control.

**Model 16.** This is Model 4 (shared frailty) without the sentence length control.

**Model 17.** This is Model 12 without the sentence length control, or Model 1 without either California or the sentence length control.

**Model 18.** This is Model 13 without the sentence length control, or Model 2 without either California or the sentence length control.

#### 4.2.2 Excluding California

Throughout most of the period considered in this paper, California had the largest prison system in the nation in terms of total prisoners, with about 10% of the nation's inmates. As a result, it comprises a large fraction of the data here, on the order of 40%. To make sure that my results were not simply reflecting California's behavior, I reran Models 1 and 2 without California.

Figure A2 provides the results. Without California, blacks appear to be treated slightly better and women slightly worse, but neither the absolute effects nor the differences are statistically significant. All other results change only slightly. It thus does not appear that California exerts undue influence on the results.

Figure A2a: Role of California

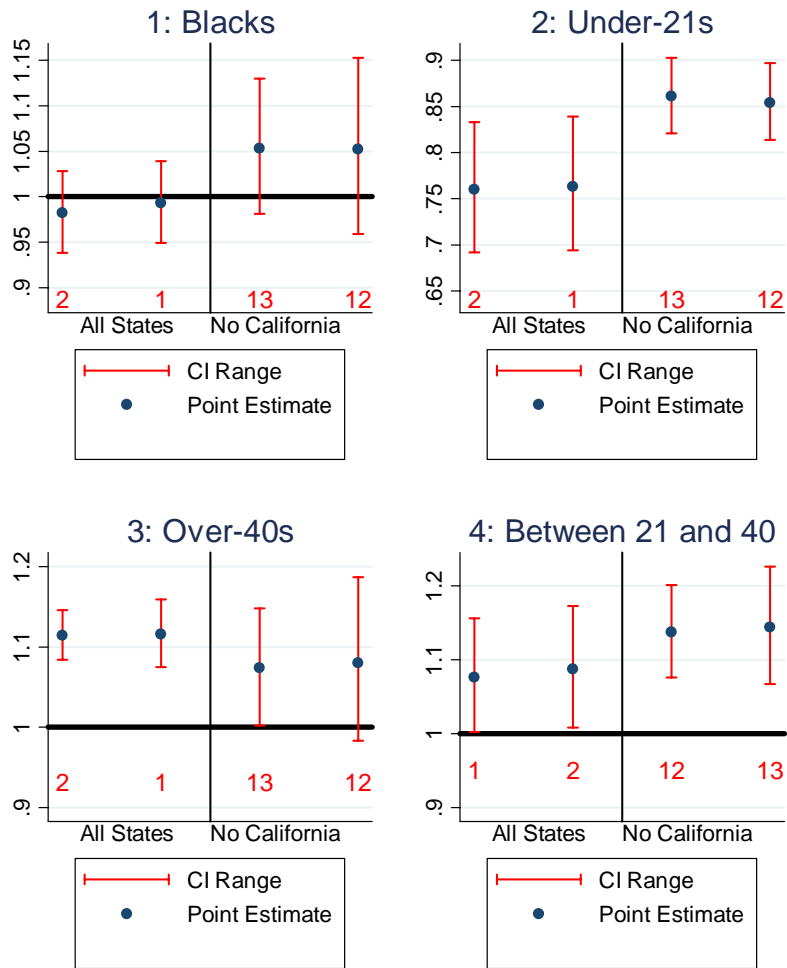
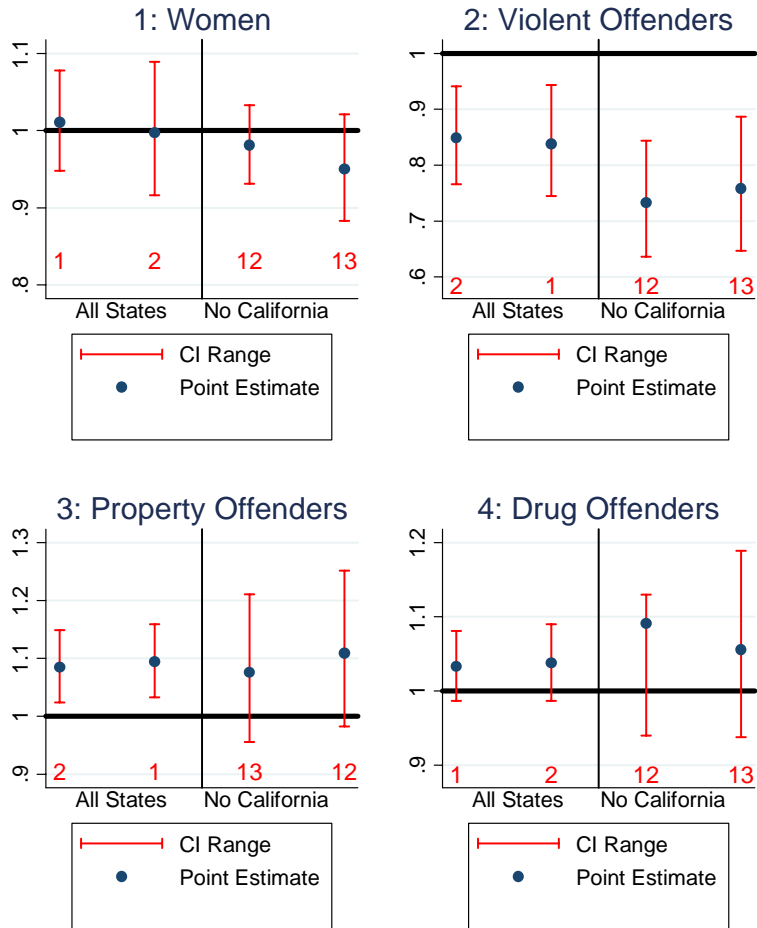


Figure A2b: Role of California



#### 4.2.3 Sources of Data

All offender-level data come from the NCRP between 1984 and 2003. Population data are from the US Census Bureau's Population Estimates, available on-line at <<http://www.census.gov/popest/archives/>>, and poverty data are from the Census Bureau's Historical Poverty Tables, available on-line at <<http://www.census.gov/hhes/www/poverty/>>

histpov/perindex.html>. Crime data are from Uniform Crime Reports, and prison admission data are from the Bureau of Justice Statistics. Citizen and State ideologies are from Berry et al. (date). Government revenue shortfall comes from the Annual Survey of State and Local Government Finances, and it is computed as the difference between a state's total revenue and total expenditure in a given year. Truth-in-sentencing laws and guidelines come from the sources given in Pfaff (2006).

#### 4.2.4 Summary Statistics

[To do: Create tables]

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