

# The Causes of Growth in Prison Admissions and Populations

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## Abstract

The explosive growth in the US prison population is well documented, but its causes are poorly understood. In this paper I exploit previously-unused data to define precisely where the growth is occurring. In short, the growth in prison populations has been driven almost entirely by increases in felony filings per arrest. All other possible sites of growth—arrests, admissions per filing, convictions per filings and admissions per conviction, and even (perhaps most surprisingly) time served per admission—have barely changed over the past four decades. But the growth in filings tracks that of admissions almost perfectly. This paper demonstrates the importance of felony filings and considers some of the possible explanations for their growth.

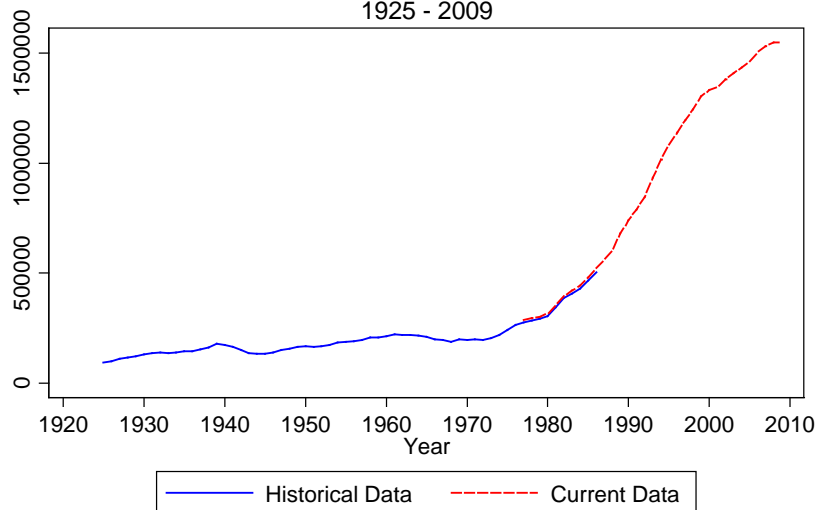
The past four decades have witnessed a breathtaking growth in the size of the US prison population. After hovering around 100 per 100,000 from the 1920s through the 1970s, the American incarceration rate skyrocketed to 502 per 100,000 by 2009; as Figure 1 demonstrates, the total number of state prison inmates soared from 174,379 in 1972 to 1,360,835 in 2009. It is a boom unseen here or abroad: the United States is home to 5% of the world's population but 33% of its prison and jail inmates (Walmsey 2007). The financial impact has been no less impressive. State governments spend over \$40 billion per year maintaining their prison systems; these expenditures account for approximately 2.6% of overall state budgets and 8.5% of the discretionary budgets (Pfaff 2010).

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Our understanding of what has fueled this growth is surprisingly weak. Numerous empirical studies have tried to trace out its causes, but as I point out in Pfaff (2008) all suffer from fundamental methodological flaws. More basically, they suffer from a significant conceptual flaw: they do not attempt to identify where in the criminal justice system this growth is taking place. These studies take either the stock of prisoners or the admission rate as the dependent variable, and then they regress that on a host of putative causal variables. But the criminal justice system is not a single, coordinated entity but a ramshackle amalgamation of various institutional actors—such as local police, county prosecutors and state legislators and parole boards—that often have dissimilar constituencies, incentives, and goals. Each causal factor may have a different effect at each stage of the process, and so it is essential to determine where the growth is occurring.

Fig. 1: United States Prison Population  
1925 - 2009



Historical data from Bureau of Justice Statistics (1997).  
Current data from Maguire (2009), Table 6.28.2009.

That is exactly what this paper does. It is not the first to do so—see, for example, Blumstein and Beck (1999, 2005), Boggess and Bound

(1997), and Langan (1991) (hereafter collectively referred to as BBL). But these studies suffer from a common analytical shortcoming: all point out that the primary “location” of prison growth is an increase in the rate of prison admissions per arrest, but due to limitations in their data they cannot disaggregate this finding further. Yet the path from arrest to admission passes through several institutions, particularly the prosecutors’ offices and the courts, and may implicate other actors as well (such as legislatures that pass mandatory minimum laws which force otherwise-disinclined judges to incarcerate defendants).

Using two datasets that BBL overlooked, I demonstrate that the growth in prison population, at least during the 1990s and 2000s, has been driven almost entirely by change in precisely one part of the criminal justice chain—the prosecutor’s decision to file a felony claim. Prison admissions grew by approximately 35% between 1994 and 2008,<sup>1</sup> even while the crime rate and the total number of arrests fell. But during that same time, felony filings rose by approximately 35% as well, but admissions per filing remained flat. During that time, convictions per filing and admissions per conviction appear to be relatively flat, suggesting that it is prosecutors’ increased willingness to file charges that are the primary engine of growth.

The claim that changes in case filings are driving prison admissions can be made stronger: these changes have driven the growth in overall prison *populations*. In other words, not only have arrests, convictions per filing, and admissions per conviction been flat, but so too has time served per admission. This claim runs contrary to the standard story told about prison growth. As Frost (2008) points out, much of the academic literature, not to mention the political and popular discourse, has emphasized the role of longer sentences. Blumstein and Beck (1999) attribute about a third of the growth in prison populations between 1980 and 1996 to longer sentences; Blumstein and Beck (2005) partition prison growth into two period, 1980 – 1992 and 1992 – 2001, and find that longer sentences explain only 15% of the growth between 1980 and 1992 but a remarkable 59.9% of the growth between 1992 and

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<sup>1</sup> As I explain below, these dates are dictated by limitations in the data.

2001.<sup>2</sup> Zimring (2001) classifies the time from the late-1980s onward as a “throw away the key” period.

Yet in two earlier papers (Pfaff 2011, 2010), I demonstrated that there was little evidence that longer sentence lengths explained much—if any—of the prison growth between the late 1980s and 2002 for a non-random sample of eleven states.<sup>3</sup> In this paper, I extend that analysis to cover all fifty states between the years 1977 and 2009. The results are striking. Sanctioning severity does not appear to have changed much at all between 1977 and the early 2000s; to the extent that there has been any change since then, it has been in the direction of *leniency*. There are a few caveats that I will flesh out, but in general my results extend my earlier findings and strongly indicate that the conventional wisdom about sentencing severity and prison growth is simply incorrect.

Part 1 of this paper establishes the decision to file as the primary engine behind the rise in prison admissions. It also briefly considers whether the growth is due to changing policies in prosecutors’ offices or the application of constant policies to a changing pool of offenders.<sup>4</sup> Of the factors considered, only shifts in the distributions of defendants’ prior felony records appear to play an important causal role. Part 2 then demonstrates that neither the level of sentencing severity nor changes in that level appears to explain the overall growth in prison populations. This serves to further emphasize the centrality of case filing as the driver of overall prison populations, not just admissions. These two parts

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<sup>2</sup> Langan (1991) points out that longer sentences played no role in prison growth during the period he analyzed, but his time frame was only 1973 to 1986.

<sup>3</sup> Specifically, there are eleven states that provided consistently reliable data to the Bureau of Justice Statistics’ National Corrections Reporting Program: California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. As is immediately clear, these states are disproportionately Democratic-leaning and non-Southern, both traits that one may expect to be correlated with penal severity.

<sup>4</sup> In other words, if prosecutors are always less likely to file felony charges against first-time offenders than against three-time offenders, then the number of cases filed could rise just because the number of offenders with longer records is rising.

thus provide the clearest identification to date of the institution most responsible for prison growth. Part 3 briefly considers the possible role of parole violations on prison growth; their effect appears to be slight, despite the attention they have recently received. Part 4 then concludes by discussing the implications of these findings for future empirical work. Prosecutors' offices are generally empirical black boxes—there is very little data available about them—which could have some important implications for examining the causes behind the increases in filings.

## **1 Decomposing Trends in Prison Admissions Growth**

By definition, prison growth arises from increases in crime rates, arrests per crime, felony filings per arrest, conviction per filing, admissions per conviction, and/or time served per admission. Earlier efforts to locate the source of prison growth, namely BBL, all relied on the same—and limited—sources of data that forced them to elide filings per arrest, convictions per filing, and admissions per conviction into admissions per arrest.<sup>5</sup> Table 1 presents their basic findings. As I show below, this elision is costly, as it masks the key primary source of growth in prison admissions, and in prison populations more generally.

The key point of distinction between this paper and BBL is that I exploit two datasets on felony filings they do not use. BBL employ data from the Uniform Crime Reports (which provides data on crimes and arrests), the National Prisoner Statistics (which provides aggregate state-level data on prison admissions and releases), and the National Corrections Reporting Program (which provides inmate-level data on prison admissions and releases).<sup>6</sup> None of these sources, however, pro-

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<sup>5</sup> Boggess and Bound (1997) and Langan (1991) further limit their analysis to admissions, and thus they do not consider the role of time served per admissions. Boggess and Bound do not explain their decision; Langan, like me, asserts that there is no evidence that sentence length has grown and thus that admissions are the sole source of growth.

<sup>6</sup> Langan (1991) and Boggess and Bound (1997) both use National Corrections Reporting Program data to measure admissions rates. (They use NCRP data rather than the more-commonly used National Prison Statistics data—which is what I use here—in order to disaggregate admissions by offense type, some-

vides any information on what takes place between arrest and admissions. The two additional datasets I use here do.

Table 1: Results from BBL

	Bl.Beck 2005	Bl.Beck 1999	Bog.Bd. 1997	Langan 1991
Years	1980 – 2001	1980 – 1996	1979 – 1992	1974 - 1986
Crime	Negligible	0.115	-	0.09
Arrest/Crime	Negligible	0.005	0.141	
Admits/Arr	0.528	0.514	0.859	0.51
Time Served	0.472	0.356	-	-
Notes – Langan does not sum to one since he separates out drug arrests (8%) and demographic shifts (20%). Both Langan and Boggess and Bound look only at admissions, not releases, and Boggess and Bound focuses only allocating the growth of admissions into arrests and commitment per arrests.				

The first is state-level data on felony filings provided by the National Center on State Courts as part of its Court Statistics Project.<sup>7</sup> The dataset covers the years 1994 to 2008,<sup>8</sup> and it provides reliable data for either twenty-one or thirty-four states; the results are similar for both sets of states.<sup>9</sup> I just use the results from the thirty-five state sample

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thing that is possible with the NCRP but not the NPS.) As I note in Pfaff (2010, 2011), NCRP data is reliable only for a subset of states that provide it. When looking at time served, only eleven states provide reliable data; for admissions data, that number rises to eighteen (although the first year of reliable data varies across these states, and several exhibit one or two years of less-reliable reporting). Thus their results should be taken with some caution.

<sup>7</sup> The data are available at [http://www.ncsconline.org/d\\_research/csp/SCCS-pastreports.html](http://www.ncsconline.org/d_research/csp/SCCS-pastreports.html).

<sup>8</sup> The Court Statistics Project has data running back through the mid-1980s. The NCSC changed its method of gathering data in 1994, and the Center discourages researchers from comparing data on either side of that year.

<sup>9</sup> For twenty-one states, the NCSC data contains observations for every single year between 1994 and 2008, and none of those observations is marked as being “overinclusive” or “incomplete.” For another fifteen states, the NCSC data contains observations for every years, but these years are marked as either being “overinclusive” or “incomplete.” For these fifteen states, every observation is either “overinclusive” or “incomplete” (with only one marker applying to each state’s observations), suggesting that interyear comparisons re-

here, and I provide a more detailed comparison of the two samples' results in the Appendix. The second is defendant-level case data from a sample of counties collected biannually by the Bureau of Justice Statistics as part of its State Court Processing Statistics project. The NCSC data allow me to partition admissions per arrest into filings per arrest and admissions per filing, while the SCPS data allow me to look at convictions per filing and admissions per conviction; since the SCPS starts tracking a defendant only after charges have been filed, it does not provide any insight into changes in filings per arrest. The NCSC and SCPS data differ somewhat, and so too do the results they produce. But taken together, they tell a fairly consistent story: as this section demonstrates, changing decisions in prosecutors' offices about when to file charges appear to be the primary—at times, seemingly almost the sole—driver of prison growth, at least since the mid- to late-1980s.

### **1.1 Stage 1: Trends in Arrests**

Unlike BBL, I start my analysis examining trends in the number of arrests, instead of looking at crime first and then at arrests per crime. If nothing else, this simplifies the analysis at little cost. As I show here, the volume of arrests does not move much during the 1980s and falls from the mid-1990s on; almost by definition, trends in arrests (and thus in arrests per crime) cannot be driving admissions growth. Also, there are (potentially) important sources of arrests—drug crimes in particular—for which there are no corresponding “crime” statistics. And finally, this is partly a concession to data reliability. Even at the state level the Uniform Crime Reports suffer from well-known flaws (see, for example,

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main valid even if the levels are not precise. I drop two of these fifteen states (Illinois and New York) from the sample because of unreliable arrest data. The Appendix lists the twenty-one and thirty-four states and considers the representativeness of this sample; it also demonstrates that dropping Illinois and New York—both large-population, large-prison, high-crime states—does not meaningfully affect the results.

Maltz and Targonski 2002), but systematic reporting flaws may be more pronounced for crimes than for arrests.<sup>10</sup>

It is easy to dispatch changes in arrests as driving prison growth. As Table 1 attests, BBL find that arrests (or arrests per crime) play only a small role in the growth of prison populations; my results reinforce their findings. Figure 2 plots the total volume of “serious” arrests for the thirty-four state sample between 1994 and 2008.<sup>11</sup> As that figure makes clear, arrests have been falling over almost the entire period. And this decline includes not just arrests for index crimes, but also those for drug offenses, where arrests are likely more discretionary. The comparison of trends in arrests and admissions is striking: between 1994 and 2008, arrests in the sample fell by 10.1% (from 3,713,266 to 3,336,982) while admissions rose by approximately 40% (from 359,359 to 504,715).<sup>12</sup> It is thus clear that arrests are not driving the growth in incarceration—and by extension neither are trends in crime levels, since their effect is wholly mediated by these arrest rates.

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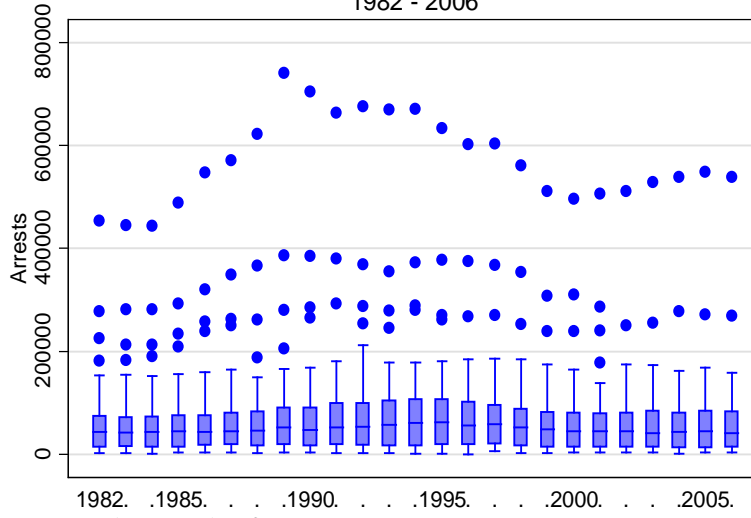
<sup>10</sup> It is perhaps easier, for example, to never report a crime than an arrest, since the latter likely triggers more paperwork. Moreover, changes in crime rates may partially reflect changes in the willingness to report crimes, independent of their incidences.

<sup>11</sup> I define “serious arrests” as those for violent and property index crimes and for non-marijuana drug arrests.

<sup>12</sup> Alaska did not report reliable admissions data in 1994, so the admissions comparison uses data from only thirty-three states in 1994 and thirty-four in 2008. Since Alaska admitted only 1,999 prisoners in 1995, dropping Alaska from the base year does not alter results significantly. (For example, had Alaska admitted 1,999 prisoners in 1994 as well, the sample’s percent change in admissions between 1994 and 2008 drops from 40.4% to 39.7%.)

Fig. 2A: Major Arrests

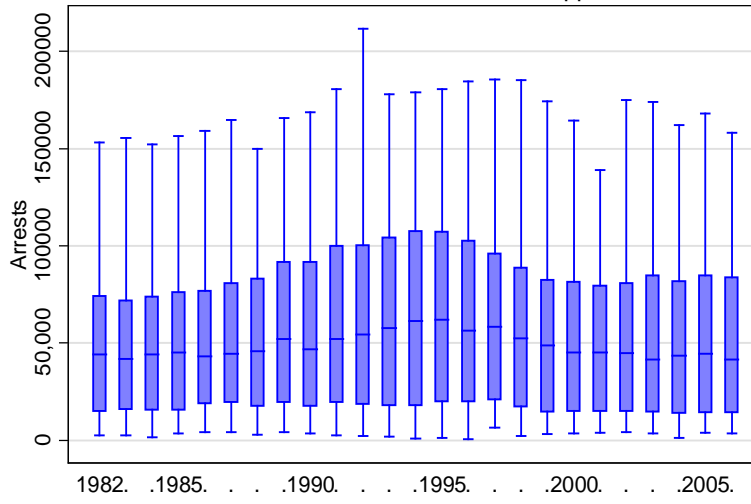
1982 - 2006



1982. .1985. . . .1990. . . .1995. . . .2000. . . .2005.  
Data from the Uniform Crime Reports  
Drug arrests exclude marijuana arrests. Please see Appendix for more details.

Fig. 2B: Major Arrests

1982 - 2006, Outside observations suppressed



1982. .1985. . . .1990. . . .1995. . . .2000. . . .2005.  
Data from the Uniform Crime Reports  
Drug arrests exclude marijuana arrests. Please see Appendix for more details.

## 1.2 Stage 2: Filings Per Arrest

Figure 3A tells the central story of this paper: unlike the volume of arrests, that of felony case filings tracks the number of admissions quite closely. In my thirty-four state sample, between 1994 and 2008 filings grow by 37.4% (from 1,392,418 to 1,913,405) while admissions grow by an almost-identical 40% (from 359,359 to 504,715). The decision to file charges thus appears to be at the heart of prison growth.

The importance of the filing decision can be highlighted even more sharply by separating out two trends: filings per serious arrest,<sup>13</sup> and admissions per filing. Figure 3B plots the aggregate results for these two trends, and the patterns are clear. The rate of filings per serious arrest rises almost every single year, from 0.375 in 1994 to 0.573 in 2008, an increase of about 1.6 percentage points (or about 3.2%) per year. Conversely, admissions per filing barely grow at all, from 0.258 in 1994 to 0.264 in 2008, peaking at 0.271 in 1999. In short, once a felony charge is filed, the risk that a defendant ends up in prison has remained roughly constant; but the risk of that filing occurring in the first place has trended upward strongly.

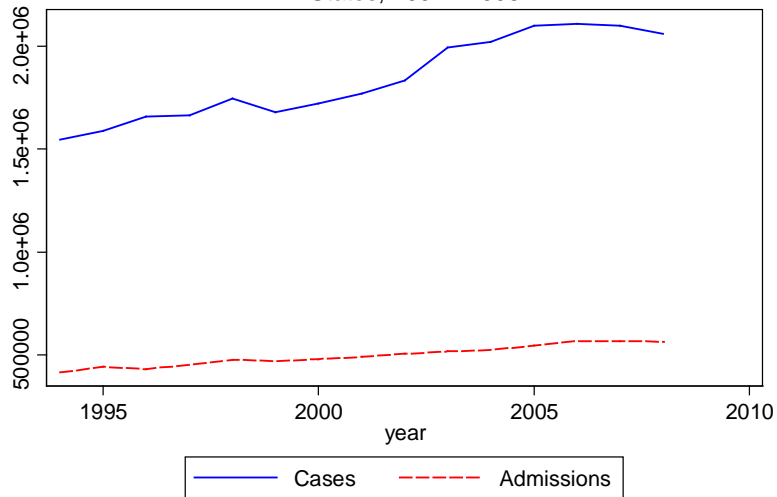
For completeness, Figures 3C and 3D provide state-level box plots in place of a single aggregate line. These latter figures tell essentially the same story as that in Figure 3B.<sup>14</sup> Figure 3C suggests that several states did not experience much of an increase in cases filed per serious arrest, but as Figure 3D makes clear, much of that is due almost solely to the behavior of Massachusetts. Without Massachusetts, state-level patterns track the aggregate results closely.

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<sup>13</sup> Recall that “serious arrests” refers to arrests for violent and property index crimes and non-marijuana drug crimes.

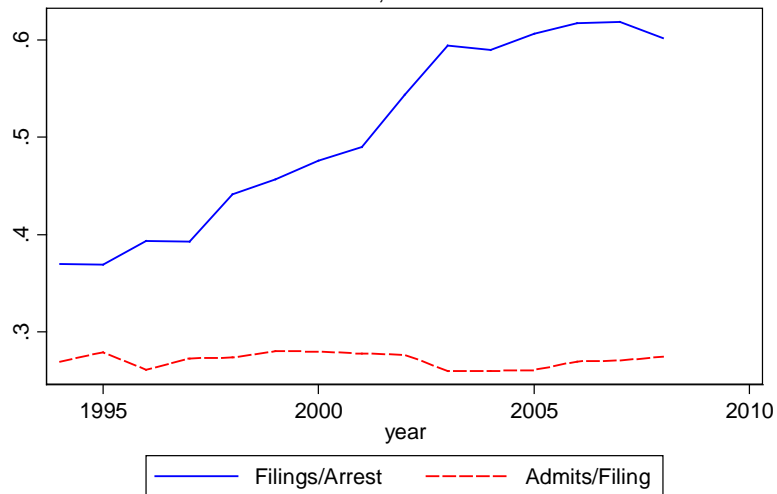
<sup>14</sup> The larger outliers in the early years for filings per arrest are Illinois (just in 1994), Tennessee, and Vermont.

Figure 3A: Filings and Admissions  
B States, 1994 - 2008



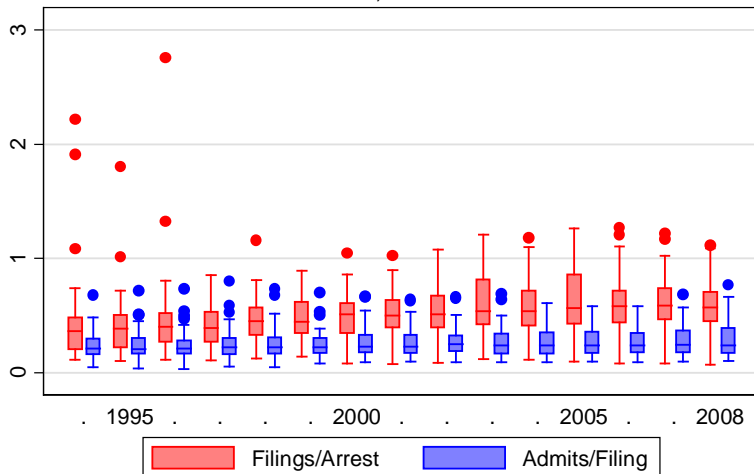
Data from the National Center on State Courts and National Prisoner Statistics.

Figure 3B: Filing and Admissions Rates  
B States, 1994 - 2008



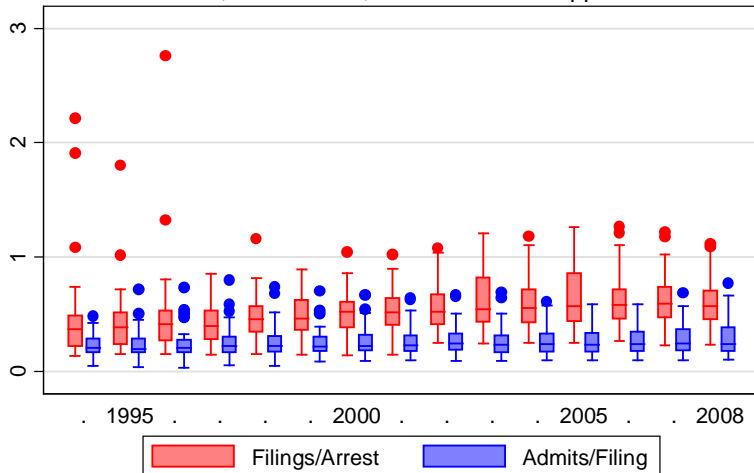
Data from the UCR, National Center on State Courts, and National Prisoner Statistics.

Fig. 3C: Trends in Filing and Incarceration Rates  
B States, 1994 - 2008



Filing data from the National Center on State Courts.  
Admissions data from BJS's National Prisoner Statistics.

Fig. 3D: Trends in Filing and Incarceration Rates  
B States, 1994 - 2008, Massachusetts suppressed



Filing data from the National Center on State Courts.  
Admissions data from BJS's National Prisoner Statistics.

Two simple counterfactuals can illuminate more clearly the central importance of changing filing behavior. The first demonstrates the importance of changing prosecutorial filing decisions, and the second the relatively minor impact of changes in post-filing outcomes (i.e., admissions per filing). The first counterfactual compares the real number of cases filed to a hypothetical history of filings which fixes the rate of filings per arrest at its 1994 level. In other words, the counterfactual number of filings in, say, 1999 is the product of the real number of serious arrests in 1999 and the filing-per-arrest rate from 1994. Figure 4A provides these results.<sup>15</sup>

Given the flatness of arrests seen in Figure 2, that the counterfactual filings are also relatively flat, as well as less than the real levels, is not surprising. The magnitude of the effect, though, is remarkable; while real filings rise by 37.4%, the counterfactual filings *fall* by 10.1%. Thus changes in arrests should have *slowed* case filings: output soared with a declining input. As a robustness check, I rerun the counterfactual experiment using the filing-per-arrest rates for 1995, 1996, and 1997, just to ensure that nothing peculiar about 1994 is driving my results. The results, given in Figure 4B, dispel such a concern.

The second counterfactual experiment, which is given in Figure 4C, with a similar robustness check in Figure 4D, compares the real number of admissions to two counterfactual admissions populations. The first, the “Incarceration Rate” counterfactual (IRC), allows filings to take on their real values but fixes the rate of incarcerations per filing at its 1994 level. The second, the “Filing Rate” counterfactual (FRC), fixes the rate of filings per arrest at its 1994 level (thus replicating the counterfactual filing data given in Figure 4A) but allows the rates of admissions per filing to take on their real values.<sup>16</sup>

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<sup>15</sup> While Figure 4A provides the aggregate result, the counterfactuals were computed at the state level and then added up.

<sup>16</sup> Thus the IRC admission size in, say, 1995 equals the real number of filings in 1995 times the 1987 admissions-per-filing rate. Conversely, the FRC admissions size in 1995 equals the real arrest levels in 1995 times the filings-per-arrest rate from 1987 times the admissions-per-filing rate in 1995.

Taken together, these two counterfactuals make clear that changes in filings, not in admissions per filing, have been the core driver of admissions growth. The IRC says that if there had been no change in admissions per filing, prison admissions would have roughly the same through the early 2000s, but then either slightly larger or no different throughout the rest of that decade; in 2008, the real and counterfactual admission levels differ by only 2.2% (504,715 to 493,817, respectively).

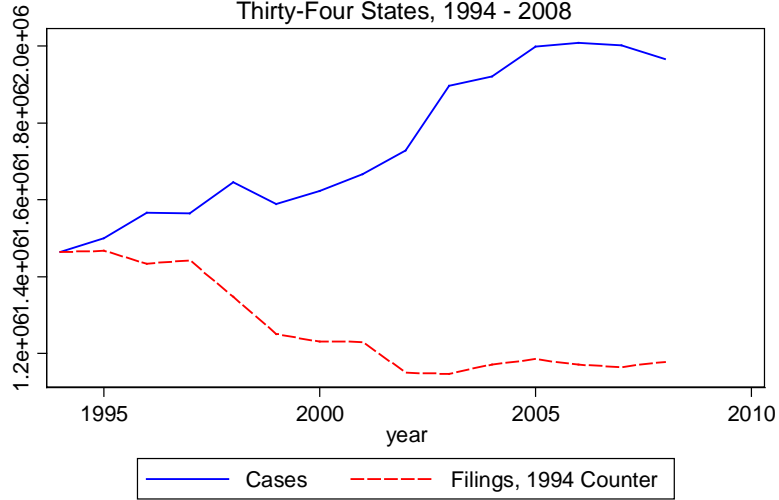
Conversely, the FRC indicates that had filings per arrest not changed, admissions would have declined throughout the 1990s and flattened in the 2000s. Between 1994 and 2002, actual admissions rise by 24.2% (from 359,359 to 446,374) while FRC admissions decline by 12.2% (from 359,359 to 315,237). Between 2002 and 2008, real admissions rise by a further 13.1% (from 446,374 to 504,715), while FRC admissions shift by merely 2.4% (from 315,237 to 322,943).

In short, the growth in prison populations has been driven primarily by the decision to file. Two important caveats, however, deserve attention. First, as noted above, changes in admissions per filing did play some role in prison growth during the early 1990s; given the generally short time inmates spend in prison (see Pfaff 2011), however, the long-run impact of this increased admission rate is likely small. Second, disaggregated results indicate that some states did experience respectable growth in the rate of admissions per filing, even if the aggregate effect is not substantial. This is shown in Figure 4E, which provides the box plots of the state-level percent differences between real admissions on the one hand and IRC and FRC admissions on the other.<sup>17</sup> It is thus useful to unpack a bit more what exactly happens between the decision to file and to incarcerate.

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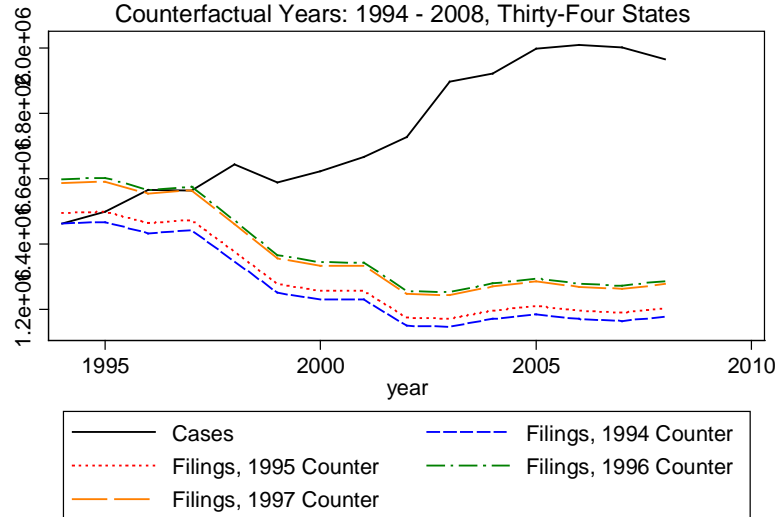
<sup>17</sup> In other words, a median value of 0.25 for “vs. Incarc. Counter” means that the median IRC admissions level is 25% smaller than the real admissions level.

Fig. 4A: Real and 1994-Counterfactual Felony Filings  
Thirty-Four States, 1994 - 2008



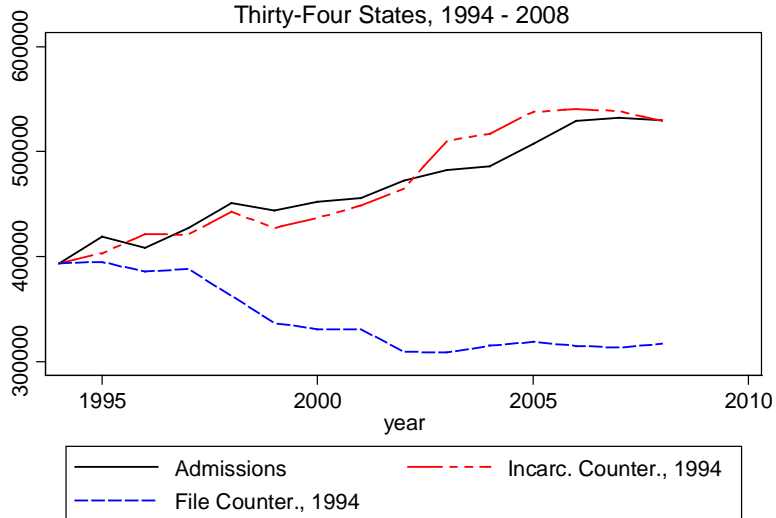
Data from the UCR and National Center on State Courts.  
Please see Appendix for a discussion of included states.

Fig. 4B: Robustness Check  
Counterfactual Years: 1994 - 2008, Thirty-Four States



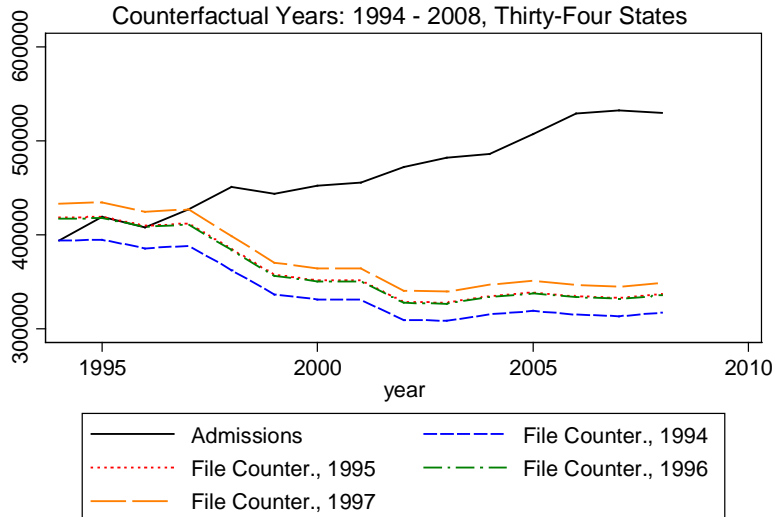
Data from the UCR and National Center on State Courts.

Fig. 4C: Real and Counterfactual Admissions  
Thirty-Four States, 1994 - 2008



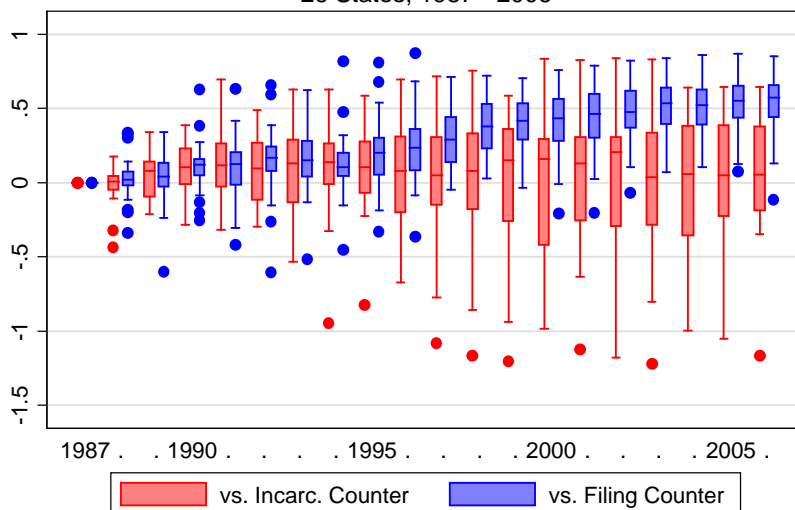
Data from the National Center on State Courts and National Prisoner Statistics.

Fig. 4D: Robustness Check  
Counterfactual Years: 1994 - 2008, Thirty-Four States



Data from the National Center on State Courts and National Prisoner Statistics.

Fig. 4E: Percent Diff.: Real vs. Counterfactual Admits  
26 States, 1987 - 2006



Data from the National Center on State Courts and National Prisoner Statistics.

There is at least one key step between filing charges and admitting someone to prison that we have not yet addressed: conviction. Trends in admissions per filing could be driven by changes in either convictions per filing or admissions per conviction. The aggregate-level data from the NCSC and NPS cannot shed light on the role of convictions, but case-level data from the State Court Processing Statistics can. Since 1990, the SCPS has gathered data every two years from a sample of county courts. These counties are chosen in such a way that their results can be aggregated to reflect overall outcomes in the seventy-five most populous counties in the United States; ten of these counties are included, by design, in every wave of data, and the remaining sixty-five are chosen more randomly each year.<sup>18</sup> The SCPS gathers a sample of cases in each chosen county in May and then follows those cases for an entire year,

<sup>18</sup> The ten “permanent” counties are Maricopa, AZ; Los Angeles, CA; San Bernardino, CA; Santa Clara, CA; Broward, FL; Dade, FL; Cook, IL; Wayne, MI; Bronx, NY; Kings, NY; Philadelphia, PA; Shelby, TN; Dallas, TX; and Harris, TX.

recording information on charges at arrest, indictment, and (if any) conviction, as well as a numerous demographic details about the defendant.

The SCPS and the NPS/NCSC datasets are not perfectly compatible. Most important, the SCPS does not start following a defendant until a case is filed, making it hard to link the SCPS data to relevant arrest levels; I cannot compute filings per arrest, but rather only take the level of filings as a given and then compute convictions per filing and admissions per conviction. Moreover, the trends in filings in the SCPS do not track those in the NCSC data as closely as one might wish. Between 1990 and 2004, filings in the SCPS rise by 1.7%, and by only 8.8% between 1994 and 2004 (the data's trough and peak for filings). Conversely, filings in the NCSC data rise by 34.2% between 1994 and 2004.

Note, though, that this latter discrepancy may actually be informative. *If* the SCPS data are generally comparable to the NCSC data,<sup>19</sup> then the faster growth in the NCSC data suggests that the surge in filings has been taking place in less-urban counties. If true, this insight helps us better pin down exactly where to focus attention going forward.

With these important limitations in mind, SCPS data indicate that convictions per filing play a more important role driving admissions growth than admissions per conviction. Figure 5A plots the percent of filings that result in convictions, and Figure 5B the percent of convictions that result in admissions.<sup>20</sup> Taken together, these graphs tentative-

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<sup>19</sup> The key assumption that has to hold for this to be true—and one that I cannot test with the data available—is that the intracounty sampling method in the SPCS provides an unbiased estimate of annual behavior in that county. In other words, I need May to be a representative month, and I need the days chosen within May to sample cases to be representative days for that (representative) month. If these conditions hold, the SCPS provides an unbiased estimate of filings in these counties.

<sup>20</sup> In results not provided here, I decompose each line into two pieces: one line for the ten counties that report every year, and one for the remaining counties used in each year. This was done as a robustness check, to make sure that changes over time did not reflect changes in the level effects in the sampled states (i.e., that the selection algorithm did not inadvertently choose states with high levels of, say, convictions per filing in one wave and with low levels in

ly point toward convictions per filing as the primary force, although there is significant year-to-year variation.

Simply looking at overall trends between 1990 and 2006, convictions per filing seem to be more dynamic than admissions per conviction. During that time, convictions per admission rise by 22%, from 42.4% to 51.9%, while admissions per filing are basically flat, rising from 72.7% to 72.8%. Figure 5C, however, isolates the relative importance of these two factors on a year-by-year basis, and in doing so tells a less coherent story. This figure separates the annual percent change in the rate of admissions per filing (the black bar) into its two constituent pieces: convictions per filing (the grey bar) and admissions per conviction (the white bar).<sup>21</sup> For several years—1992, 2000, and 2002—convictions per filing are the core driver of admissions per conviction. This reverses completely in 1998. Both seem to contribute relatively equally in 2004 and to offset each other almost completely in 1994 and 1996. Perhaps the most unified explanation of these results, then, is this: where changes in admissions per filing matter, convictions per filing are more important than admissions per conviction, but both have played important roles.

So, to summarize so far. Increases in filings have been the key driver of admissions growth; trends in arrests have had a negligible impact, and those in admissions per filing have had only sporadic effects. When admissions per filing have mattered, it appears that convictions per filing have been more important than admissions per conviction, but not overwhelmingly. All told, these results consistently identify the same institutional actor as the central engine of prison admission growth: the prosecutor.

My claim here, however, goes beyond arguing that increases in filings have driven prison admissions, but that they have driven prison *populations*. To do this, I need to show that there have not been signifi-

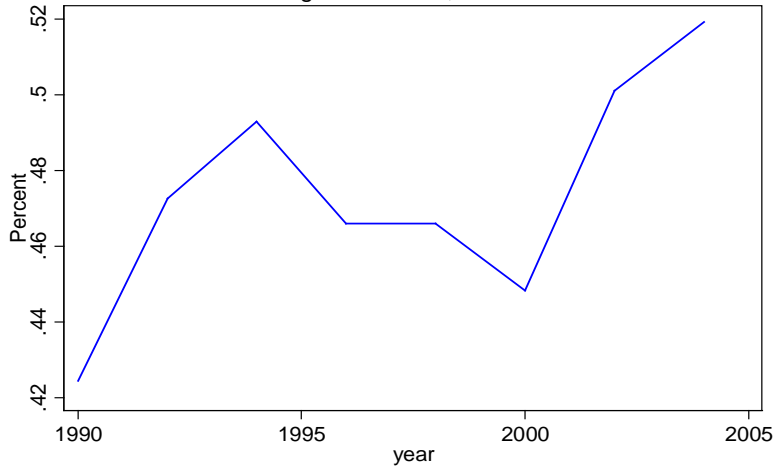
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the subsequent wave). Although the two lines did not overlap perfectly, their trends were strongly similar in both graphs, suggesting that such selection problems are not driving the variation seen in Figures 5A and 5B.

<sup>21</sup> By definition, the height of the black bar is equal to the sum of the white and grey bars.

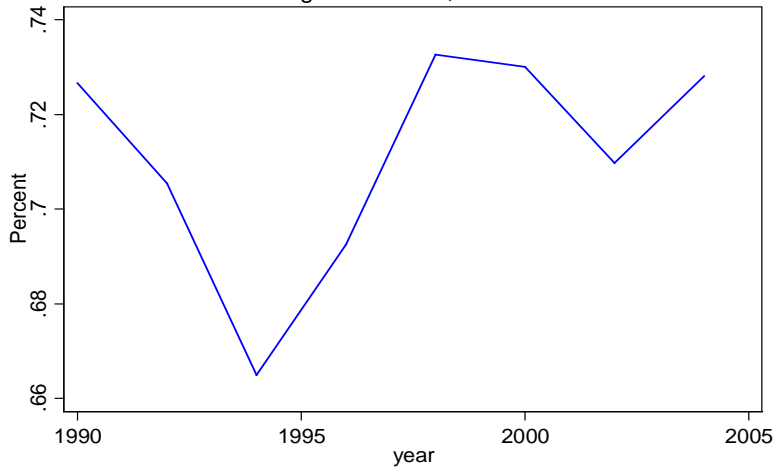
cant changes in time served. Before shifting attention from admissions to releases, however, I want to briefly touch on a few causal explanations for the growth in filings.

Fig. 5A: % Felony Filings Yielding Felony Convictions  
75 Largest Counties, 1990 - 2004



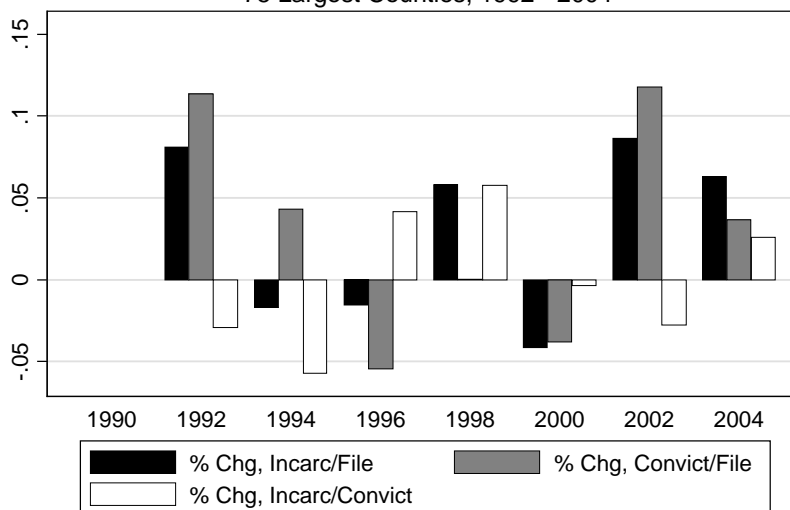
Data from the State Court Processing Statistics.

Fig. 5B: % Felony Convictions Yielding Incarcerations  
75 Largest Counties, 1990 - 2004



Data from the State Court Processing Statistics.

Fig. 5C: Decomposing Changes in Filings-to-Admits  
75 Largest Counties, 1992 - 2004



Data from the State Court Processing Statistics.

### 1.3 Why Have Filings Increased?

A full examination of the causal explanations for the rise in filings is beyond the scope of this paper. Here I want to touch just on one class of potential explanations: changes in the inputs. Filings can grow either because prosecutors change their policies on when to file charges against certain categories of offenders, or those policies remain fixed but the pool of offenders changes in important ways. As Figure 2 demonstrates, the *size* of the defendant pool has declined of late, but that tells us little about the *composition* of that pool. In this section, I want to consider three possible changes in its composition that could affect filing decision: changes in the number of drug offenders, changes in the distribution of offense types more generally, and changes in the prior records of offenders. The results here indicate that changes in drug arrests and the wider distribution of offenses do not appear to have had much influence on charging outcomes, but that changes in prior records may have a substantial effect.

First, consider the role of drug arrests. Drug arrests are relatively discretionary (since there are no first-party victim complaints), and so they could push up filings even when index crimes are dropping. But while this effect is conceptually possible, it does not appear to be so empirically. If nothing else, the flat-to-declining levels of arrests seen in Figure 2 already account for all non-marijuana drug arrests. But given the outsized attention the “War on Drugs” has received as a possible explanation for prison growth, it is useful to emphasize more strongly the relative unimportance of drug arrests to filing and admission trends.

To highlight the limited effect of drug arrests, I develop another simple counterfactual, which is given in Figure 6A. That figure compares the real number of case filings in the NCSC data to two counterfactual filing levels. The first nets out *all* non-marijuana drug arrests—it plots the number of non-drug felony filings that would occur under the preposterous assumption that *every* non-marijuana drug arrest results in a felony filing;<sup>22</sup> the second takes the slightly less extreme, but still (intentionally) unrealistic view that half of all non-marijuana drug arrests lead to felony filings. As is clear, even net of these hypothetical drug filings, total filings for violent and property offenses rise steadily in the presence of declining violent and property index crimes. Even if we assume that all non-marijuana drug arrests result in a felony filing, 1.37 million (or 71.7%) of the 1.91 million felony cases filed in my sample in 2004 would have stemmed from non-drug arrests, up from just under 858,000 (or 61.6%) of the 1.39 million cases filed in 1994. That is a

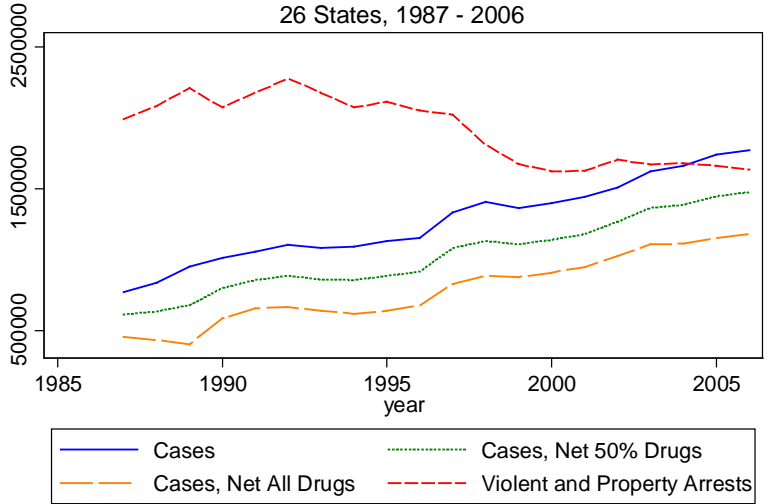
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<sup>22</sup> Recall that the NCSC filing data does not disaggregate its results by offense type, nor does the Center have access to such data. Thus I need to approximate the non-drug felony filings. Note that netting out all non-marijuana arrests leads to plausible results in the aggregate, but not always at the state level—in some states with particularly high levels of drug arrests, such as California, the netted-out filings can take on negative values. This likely reflects the fact that (1) states with high drug arrests may have above-average levels of marijuana arrests (and due to limitations in the available data I calculate non-marijuana drug arrests using the national average of arrests that are for marijuana offenses), and (2) states with high levels of drug arrests may be making more misdemeanor arrests in general or be more willing to plead out felony weights as misdemeanors.

growth rate of 59.9%, compared to 37.4% for all cases filed. In other words, even in extreme cases non-drug filings are doing a lion's-share of the work here; under more realistic assumptions their importance would be even greater. If the War on Drugs is fundamentally changing our prison admissions policy, it must be doing so indirectly; I will return to this point when I discuss the role of prior records.

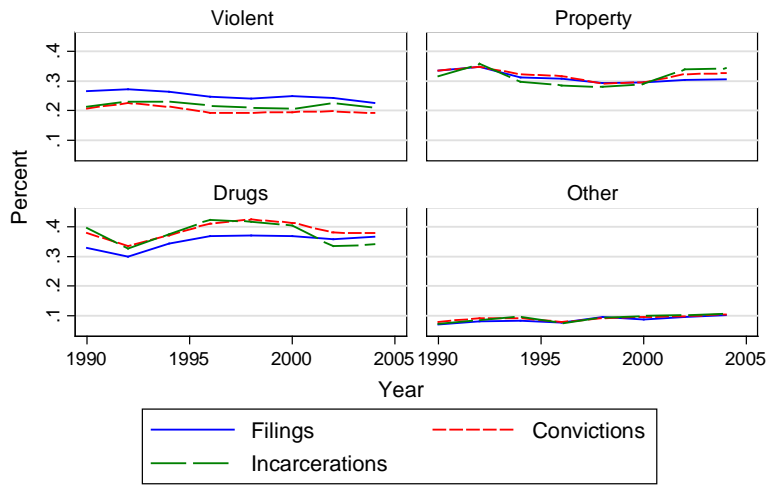
There has also been no real change in the distribution of offenses or how they are handled, at least in the counties providing data to the SCPS. As shown in Figure 6B, the fraction of filings in the SCPS that are violent or property crime have not shifted much—from 26.6% to 22.6% for violent crimes and from 33.5% to 30.6% for property crimes, with the rise in drug crimes from 32.8% to 36.7% almost perfectly explaining these decreases. There has been little change in the compositions of convictions or admissions as well. Such subtle shifts cannot explain the rise in filings. It is worth recalling, though, a key difference between SCPS and NCSC data, namely that the total number of filings is substantially flatter in the SCPS data. So it may not be surprising that the distribution of offenses is relatively flat in jurisdictions where the rate of filing growth appears to be somewhat flat; in faster-growing jurisdictions, the distributions of offenses within and across filings, convictions, and admissions may be shifting more.

**Fig. 6A: Trends in Cases, Net of Drug Arrests**  
26 States, 1987 - 2006



Cases data from National Center on State Courts. Arrest data from Uniform Crime Reports. Drugs are all drugs except marijuana.

**Fig. 6B: Outcomes by Offense Type**  
75 Largest Counties, 1990 - 2004



Data from the State Court Processing Statistics.

Finally, one factor that may explain some of the growth in filings is changes in the prior records of offenders.<sup>23</sup> The story is a simple one: as a result of the spike in crime from the 1960s to the early 1990s—and the attendant increases in arrests, convictions, and incarcerations—the distribution of criminal records in each year’s defendant pool may be shifting to the right. In other words, the fraction of defendants with multiple priors may have been smaller in the 1980s than in the 1990s or 2000s. If prosecutors are generally more likely to file charges the longer a defendant’s record, then filings could rise during a time of declining arrests if the average record length per arrestee is growing. Unfortunately, the SCPS does not tell us anything about the prior records of arrestees, but it does provide information on the records of those who have cases filed against them. And the distribution of prior records does shift to the right over the sample period, which is at least consistent with prosecutors applying constant policies to a toughening pool of defendants.<sup>24</sup>

Figure 6C plots the fraction of each year’s defendant pool that has no prior record, three prior felony convictions, four or more felony convictions, or no data on prior records.<sup>25</sup> As that figure makes clear, between 1990 and 2004 the share with no record falls, from 59.6% to 51.9%, while the percent with three priors rises from 3.9% to 5.9% and that with four or more prior felonies from 7.8% to 14.3%. Not shown on the graph are the relatively stable shares of those with one prior (13.3% to 14.0%) and with two priors (7.7% to 8.8%).

Some back-of-the envelope calculations can shed a bit of light on how significant these trends could be. In 1990, there were 1,012,350

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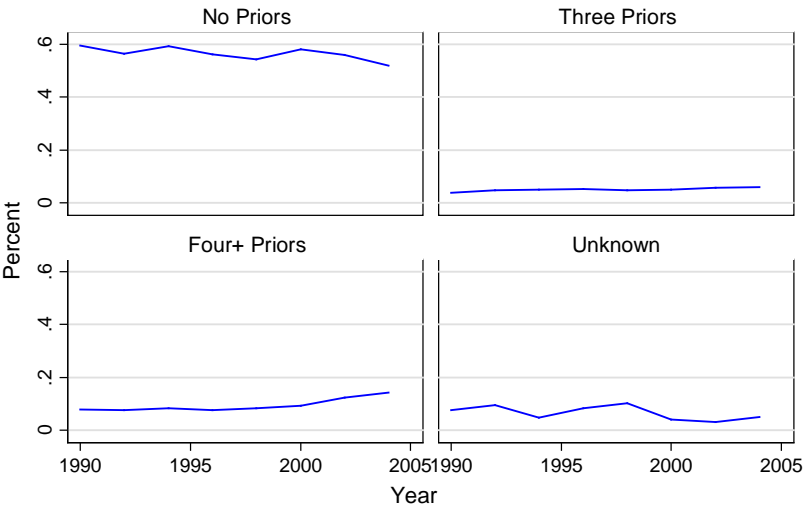
<sup>23</sup> This is an issue I intend to explore in more depth in future work, and I just touch on its implications here.

<sup>24</sup> It is also consistent with prosecutors changing their views on how to handle repeat offenders. Without data on the prior records of arrestees, I cannot compute filings per arrest conditional on prior record length, which is the actual variable of interest.

<sup>25</sup> The percent of unknown priors is also included given its relative size: at 7.7% in 1990 and 5.1% in 2004, it is on the same scale as the percent of cases filed against those with two, three, or four-or-more prior felonies. It thus points to an important source of noise in the data.

cases filed in the NSCS data; in 2004, 1,660,264. Combining NCSC and SCPS data—which, given the differences between the two datasets, should be done with some caution—these numbers imply that in 1990 there were 118,445 cases filed against defendants with three or more prior felony convictions; in 2004, 333,373 such cases, an increase of 181%. Conversely, the number of cases filed against those with no records rose by only 42.8% (from 603,361 to 861,677) and against those with no more than two prior felony convictions by 52.0% (from 815,954 to 1,240,217).

Fig. 6C: Distribution of Prior Records  
26 States, 1990 - 2004



Data from the State Court Processing Statistics.

The following comparison puts these numbers in perspective: in my thirty-four state sample, the number of prison *admissions* rose by 145,356 between 1994 and 2008; during roughly the same period of time (1990 to 2004), the number of cases filed against defendants with three or more priors rose by approximately 214,928 in the seventy-five most populous counties in the country. It is surely not the case that *all* the growth in admissions is just due to increased filings against defen-

dants with longer records—after all, during this time the number of cases filed against those with two or fewer prior convictions grew by 378,540. But filings against defendants with three or more prior convictions grew by 181%, compared to 52% (albeit from a much larger baseline) for those with two or fewer priors. These rough estimates at least suggest that longer criminal histories may be important to the growth of filings.

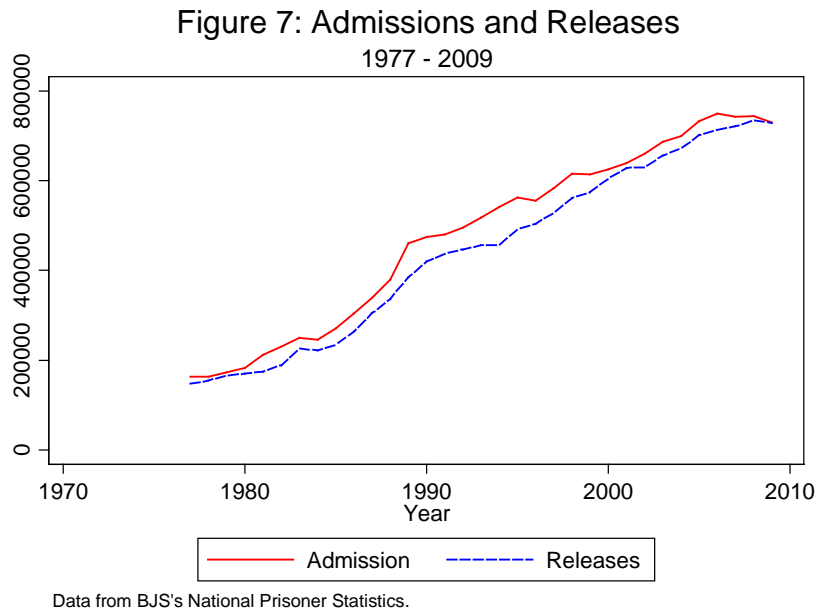
And it is here that the War on Drugs returns. The War on Drugs is not increasing admissions directly via the incarceration of drug offenders. But it may be increasing admissions indirectly, by lengthening the records defendants have, and thus the likelihood that prosecutors opt to file charges against them for non-drug crimes. These results may also point to an on-going collateral cost of the crime boom of the 1960s to the 1990s: by producing cohorts of offenders with longer records, it generated a pool of offenders that faced may face tougher sanctioning outcome even when all else is constant, thus helping prison populations trend upwards even as crime rates fall.

## **2 The Relative Unimportance of Sentencing Severity**

As legislatures have passed increasingly tough penal laws over the past few decades—such as truth-in-sentencing laws, two- and three-strike laws, and the abolition of parole—the impact of longer sentences on prison populations has received increased attention. Zimring (2001), for example, argues that in the 1990s we shifted from an era of “lock ‘em up” to one of “throw away the key.” Frost (2008) documents other academic pronouncements that stress the importance of sanctioning severity to the growth of incarceration.

The connection between longer prison terms and increased prison populations, however, is perhaps more tenuous than its plausibility might suggest. Zimring et al. (2001), for example, point out that while a majority of states have passed strike laws, only California uses such a law with any regularity; evidence in Dharmapala et al. (2010) suggests that states often pass truth-in-sentencing laws only after they have already abolished parole, implying that the former laws are primarily

symbolic; and Bowers (2008) claims that New York State prosecutors often divert low-level felony drug offenders to misdemeanor treatment courts and then more-serious drug felons to felony treatment courts, all in an effort to evade New York's notoriously tough Rockefeller drug laws. In other words, that states pass tough laws do not mean such laws are widely used on the ground. And Figure 7, which compares annual admissions to and releases from state prisons since 1977, suggests it is unlikely that time served has grown too dramatically. A significant lengthening in time served would cause the release line to pull away from the admissions line; while the gap between them does grow somewhat between the late-1980s and mid-1990s, it then narrows again through 2009, suggesting that any lengthening of sentences was not particularly long-lived.



In two recent papers (Pfaff 2011, 2010), I use data from the National Corrections Reporting Program to demonstrate more rigorously that changes in sentence length do not explain the growth in prison popula-

tions from the late 1980s to 2002 (the period for which NCRP data was then available). My results indicate that the time actually served, at least by the median and 75th-percentile prisoners, did not rise during that period and often fell; changes in time served do not appear to explain the growth in prison populations, with all the work being done by increases in the size of admissions cohorts.

There are two important limitations to those papers, however, that I confront here. First, only eleven states provided sufficiently reliable data to the NCRP, and these states are disproportionately more Northern and liberal states.<sup>26</sup> Extrapolating my results to more Southern and conservative (and thus perhaps more punitive) states is risky. Second, the NCRP does not begin to provide reliable data until approximately 1987, over ten years after the boom in prison populations began. In this paper I develop an alternative model, using the NPS data rather than the NCRP, that allows me to extend my analysis to all fifty states, and from 1977 to 2009. Limitations in the NPS force me to use an approach that is somewhat less rigorous than what I used in Pfaff (2011, 2010), but the results nonetheless appear to reconfirm and extend my claim in those papers that longer sentence lengths have not played a major role in prison growth.

The key problem with the NPS is one of aggregation: while the NCRP provides inmate-level admission and release data, the NPS reports only state-level aggregate numbers of admissions and releases. In Pfaff (2011, 2010), I was able to construct relatively precise release schedules for each entering inmate: for each state I could calculate how many prisoners admitted in, say, 1990 were released in 1990, in 1991, in 1992, etc., and then look at how these years-to-release rates changed across entering cohorts and across states. The aggregate nature of the NPS denies me this option.

To examine the effect of any putative changes in release policies on prison populations, then, I create an ersatz release dataset to compare to the real release data. To do this, I apply a hypothetical release pattern to the real admissions data; specifically, I assume that 40% of all

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<sup>26</sup> The states are listed in note 3 above.

inmates are released the year they are admitted, 10% each of the two following years, 5% each of the six following years, 4% the year after that, and 2.5% per year for the next two years.<sup>27</sup> In total, 99% of all prisoners are released within eleven years, and 1% are never released.<sup>28</sup> The high early release rates are consistent with what I found in Pfaff (2011), and the 1% never-being-released seems roughly consistent with the claim that 2.5% of prisoners (not admissions) are serving life-without-parole sentences (see, e.g., Nellis 2010). I impose this release policy on every state in every year.

Figure 8A plots the national aggregate of real prison releases (the solid line) against my ersatz releases (the dotted line). In the early years of the graph, ersatz releases are necessarily less than real releases: the only people counted as ersatz releases in 1977 are those who are released from the 1977 admissions cohort, while the real release data includes all those from pre-1977 admissions cohorts being released in 1977. Note, however, that the ersatz and real release populations rapidly converge, even before 1988—marked by the vertical line—which is the first year that my ersatz release population is “complete.”<sup>29</sup> Figures 8B and 8C demonstrate the speed of convergence between the ersatz

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<sup>27</sup> In other words, if 1000 prisoners are admitted in 1977, I assume that 400 are released in 1977, 100 in 1978 and 100 in 1979, 50 each year from 1980 through 1985, 40 in 1986, and 25 in 1987 and 1988. Thus 990 prisoners are released within eleven years, and 10 are not released at all. Furthermore, if 1100 prisoners are admitted in 1978, then my ersatz 1978 release population is 540: 100 (10%) from the 1977 admissions cohort, and 440 (40%) from the 1978 admissions cohort.

<sup>28</sup> Eventually such prisoners would die or be executed, but for the time frame here I assume they are never released.

<sup>29</sup> By complete, I mean it is the first year that all those who are going to be released from the first (1977) cohort have been released. In other words, assume that my ersatz release schedule were the true release schedule. In 1986, my ersatz releases would be too small, since some inmates admitted in 1976 (who are not in my data) would be released in 1987. But by 1988, according to my ersatz schedule, only inmates admitted in 1977 or later would be released, and so my ersatz releases would equal the real releases. Of course, in practice inmates admitted prior to 1977 continue to be released even today, but 1988 marks the start of the closest I can come to “completeness.”

and real populations a different way: these box plots graph the ratio of ersatz releases to real releases, and the rate of convergence toward equality (a ratio of one) is clearly quick, within just five or six years. In the Appendix, Figure A\_ provides individual state-level graphs comparing the ersatz and real release population.

Several important features of Figure 8 deserve comment. First, it is striking how closely the real and ersatz releases follow each other. By design, the ersatz curves will almost always be smoother, since the release pattern is invariant over time (this relative smoothness is most obvious in Figure A\_, but it can also be seen in Figure 8A). Yet despite this artificial constancy, for most states the ratio of ersatz to real releases remains quite close to one. In Figure 8A, ersatz and real releases are almost indistinguishable by the early 1990s, and in Figures 8B and 8C the mean ratio never drops below 0.9 after 1985; for almost every year after 1990 at least 75% of the states have a ratio over 0.9. That real releases remain close to my invariant ersatz releases suggests that there have been few meaningful changes in time served over the period of 1977 to 2009.

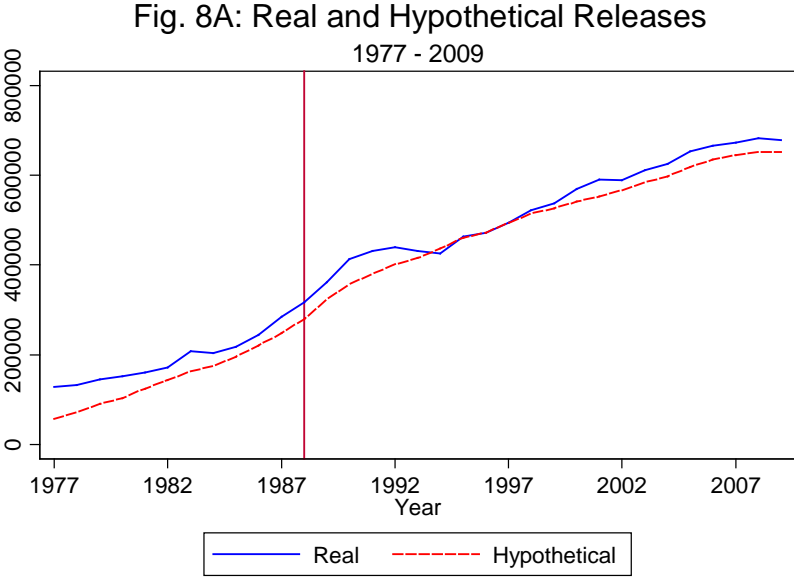


Fig. 8B: Ratio, Hypo and Real Releases  
1977 - 2009

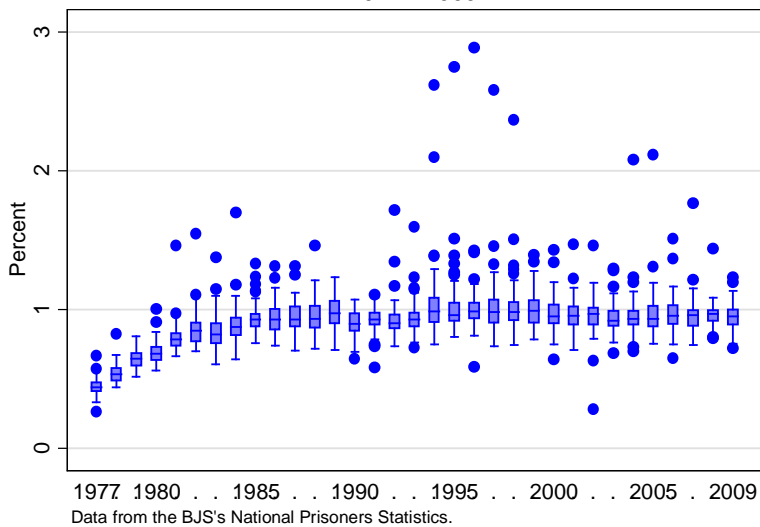
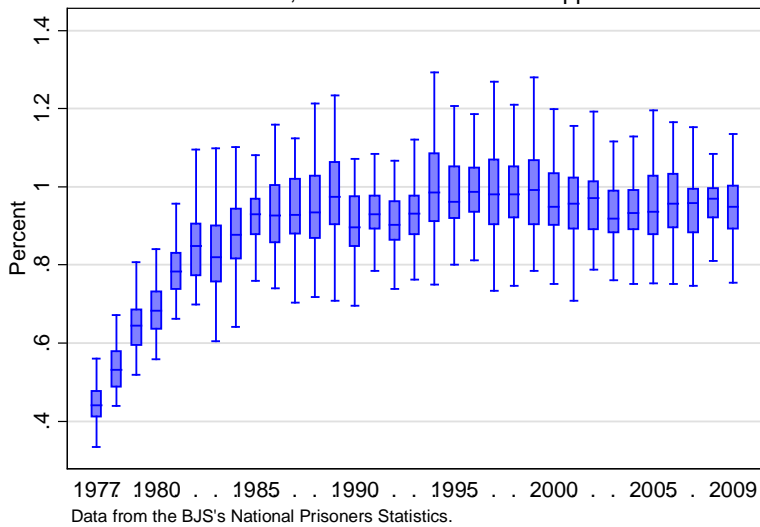


Fig. 8C: Ratio, Hypo and Real Releases  
1977 - 2009, Outside observations suppressed



Second, my release pattern actually *overstates* the punitiveness of sentencing policy in the United States. My ersatz release pattern understates true releases (and thus overstates punitiveness) in over 75% of all state-year pairs in the data; If I restrict the analysis to 1988 and beyond, that percentage drops only slightly, to 69%. In theory, this understatement could arise simply because my ersatz releases cannot account for the release of those admitted before 1977. But there is evidence that the understatement arises in part because real sentences are often less punitive than my hypothetical ones. To show this, I compare in 2009 the cumulative net effect all admissions and releases since 1977, using real and ersatz releases.<sup>30</sup> By 2009, my ersatz net contribution is greater than the real net contribution in all fifty states, and by over 100% in seventeen of them (including big states such as California and Illinois); in no case is the overage less than approximately 8%. It is thus unlikely that the failure to account for the release of pre-1977 admissions can explain all (or, in the case of the large overages, much) of the gap between the ersatz and real releases.

That my constructed release pattern is likely more punitive than the real release schedule is notable, since my release pattern is arguably not particularly severe: 40% of all inmates are released in less than a year, 50% within one year, 75% within five years, and 99% within eleven years. Only a small fraction serve particularly long sentences. Moreover, the divergence between ersatz and real releases in the early 2000s suggests that real release policies have become even *less* punitive in the 21st century.

It is true, as I point out in Pfaff (2010), that these short release times are not inconsistent with a small cadre of very-long serving inmates driving much of the observed prison growth.<sup>31</sup> In that paper, however, I

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<sup>30</sup> The cumulative net impact at 2009 since 1977 is simply the sum of the annual difference between admissions and releases over the period 1977 to 2009. Thus if a state admits 1000 prisoners each year and releases 950, it adds 50 prisoners each year, for a cumulative net impact between 1977 and 2009 of 1650 inmates (or 33 years times 50 prisoners per year).

<sup>31</sup> A simple thought experiment can demonstrate this problem. Consider a state that initially admits only one person to prison each year—for a serious crime—

reject that concern for the eleven states with reliable NCRP data. The similarity in outcomes here between those eleven states and the rest of the country suggests that the few-long-serving problem likely does not explain growth in the states that Pfaff (2010) did not consider.

Thus despite numerous legislative attempts to make sentences more severe, and despite all the academic attention penal severity has received, the evidence in this section—along with that in Pfaff (2011, 2010)—strongly suggests that sentence length has played at best a very minor role driving prison growth. And even if that role is nonzero, it clearly is far less important than that played by trends in admissions.

### **3 An Important Aside: Parole Violations**

A final factor that could explain the growth in prison populations is the rise in parole violations. Conceptually, parole violations fit awkwardly into the admissions/time-served divide: are they an admissions-side effect (since they cause more people to enter prison) or a time-served-side one (since they cause a violator to ultimately serve more time for the underlying felony)? Fortunately, for my purposes here it is unnecessary to resolve this debate. As I point out in Pfaff (2011), a strong argument can be made that it is poor accounting to attribute prison growth to parole releases; I just want to briefly summarize that argument here.

At first blush, parole violations appear to play an increasingly important role driving up prison admissions. In the eighteen states that provided reliable admissions data to NCRP between 1992 and 2002, the absolute number of parolees returning to prison rose from 86,430 to 134,110, an increase from 37.0% of all admissions to 44.1%. However, simply dropping California from the analysis immediately alters the sto-

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and that person serves a ten-year sentence. In the long-run, the equilibrium prison population in ten. After reaching this equilibrium, the state decides to change its policy: it increases the sentence for the serious offender from ten years to twenty, but it also starts to incarcerate two minor offenders each year, both for one-year terms. The equilibrium prison population rises to twenty-two. And while 83.3% of that growth is due to the longer sentence imposed on the serious offender, the average time served falls from ten years to 7.3, and the median time served from ten years to one.

ry: the absolute increase is now from 32,914 to 51,060, a shift from 24.0% of admissions to just 28.2% of admissions. California has the largest prison populations in the NCRP, but also one of the most idiosyncratic (particularly when it comes to parole), and so it can thus strongly influence aggregate results.<sup>32</sup>

But there is a more powerful reason to be cautious about assigning too much responsibility to parole violations: the number of violations has grown in close conjunction with the number of releases.<sup>33</sup> To the extent that violations are rising only because of the number of releases are rising, it seems like poor accounting to hold that these violations are contributing to prison growth. Think of it this way: if a boat is filling up with water, and if someone is bailing that boat out with a bucket that has a hole in it, is the water leaking out of the hole *causing* the boat to fill with water?

That said, it is still worthwhile to consider whether the metaphorical hole in the bucket is getting larger. To do this, I estimate how parole failure rates have shifted over time in the eleven states that provide reliable admissions and release data to the NCRP;<sup>34</sup> I calculate the failure rate as the number of parole admissions in year  $t$  divided by the number of parole releases in year  $t - 1$ . This is a rough but suggestive estimate, and the results are plotted in Figure 9. In general, the failure rate appears fairly flat in most states, although it does rise in Colorado, Illinois, and Kentucky. Overall, however, the “hole” does not appear to be getting much larger.

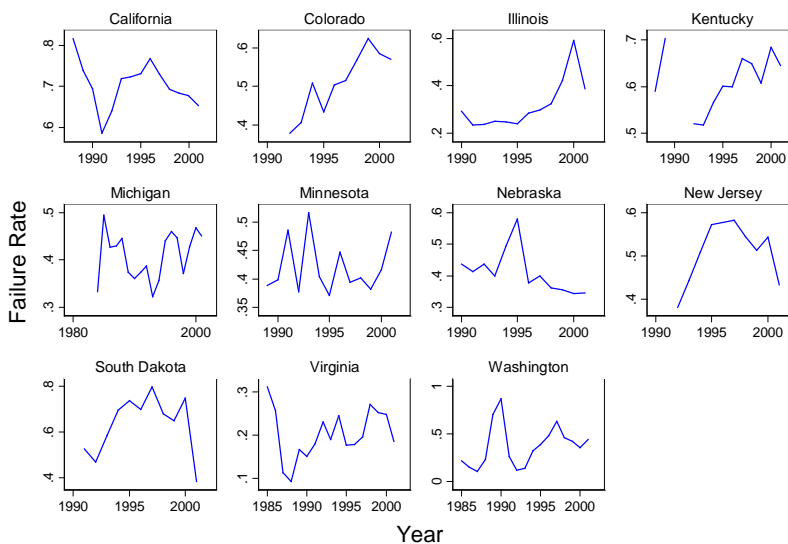
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<sup>32</sup> Not only does California have the largest prison population in the country outside of the Federal system, but since only a fraction of states provide data to the NCRP, California’s influence is even stronger there. Looking at the eighteen states considered here, California accounts for over 40% of all observations in 2002. Studies such as Blumstein and Beck (2005), which use the NCRP to estimate the effects of parole admissions and releases without carefully separating out California, should thus be viewed with some caution.

<sup>33</sup> Pfaff (2011) discusses this in a bit more detail; see Fig. 4B in particular.

<sup>34</sup> These states are given in Figure 9. The Appendix in Pfaff (2011) explains why these states are used and others are not.

Fig. 9: Parole Failure Rate



Data from the National Corrections Reporting Program.

Furthermore, it is hard to know what exactly a parole violation entails. Is it “violation” re-entry, in which the offender did nothing more than violate an otherwise non-criminal provision of parole, or committed a crime (such as smoking marijuana) that would never have resulted in a prison commitment but for his parole status? Or did the defendant commit another felony-level crime, but prosecutors opt to violate him back rather than incurring the costs of securing another felony conviction? The former feels like a “true” violation-causes-admission-growth case, while the latter does not; data from the NCRP (which is the BJS’s primary source of data on parole) simply does not provide enough detail to distinguish these cases. But these examples make it clear that the total number of parole-violation entries surely overstates the effect of “true” violations on admissions growth.

#### 4 Conclusion

The results in this paper clearly indicate that (1) prison admissions, rather than time served, have been the primary driver of prison growth,

and that (2) at least since the late 1980s, the main force behind rising admissions has been rising felony filings (and filings per arrest). These findings represent a significant advance over earlier efforts to understand the source of prison growth, and they provide a clear indication of where we need to focus our attention.<sup>35</sup>

Unfortunately, they imply that we need to focus our attention on the least transparent part of the criminal justice system. Prosecutors' offices are to a large extent empirical black boxes. There is, for example, no prosecutorial equivalent of the Uniform Crime Reports or the National Prisoner Statistics.<sup>36</sup> The data from the National Center on State Courts can be examined in more detail, but it will be hard to develop a clear causal story without case-level data. That from SCPS may yield some informative insights—as it does above highlighting the possible role of prior felony convictions—but since it does not gather data until after a charge is filed in court, it cannot tell us much directly about what is shaping the decision to file in the first place.

Thus a key challenge going forward will be to identify data that can help us better understand what has been influencing and altering prosecutorial behavior over the past several decades. While the BJS does not provide easily-comparable multi-state data on prosecutorial decision-making, individual offices and counties may have more detailed data that, with some work, can be unified into panel data on prosecu-

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<sup>35</sup> As shown in Pfaff (2008), earlier projects simply took prison admissions at the dependent variable without trying to locate where in the criminal justice system growth was taking place. Perhaps ironically, the results here suggest that admissions may not have been as problematic a dependent variable as it may appear at first. If various causal factors operated differently in different parts of the criminal justice system, then regression coefficients would return only the average effect across all stages, weighted by the unknown importance of each stage to prison growth. But since that growth appears to be concentrated in just one stage of the process, admissions in effect are just an imperfect proxy for filings. Going forward, though, it likely makes more sense to use the NCSC data than admissions data.

<sup>36</sup> The BJS does conduct a periodic National Prosecutors Survey, but it does not provide the sort of detailed case-level data needed to address this issue, and its sample weights are set to provide only national-level results.

torial decision-making. It may also be possible to attack the problem from the “outside.” There are many longitudinal datasets on at-risk populations that include information on criminal behavior and criminal justice outcomes (see Liberman 2008 for examples); by explaining why criminal justice experiences differ for various study participants, we may be able to indirectly measure what variables prosecutors are paying increasingly more or less attention to. And finally, this may be one of those issues where qualitative, ethnographic analysis can reach answers that more rigorous techniques simply lack the data to confront.

For years, academics have been unable to convincingly explain why prison populations have grown as much as they have in recent decades; there were numerous compelling theories, but little empirical support. While this paper does not resolve the causal question, it does tell us where precisely we need to look to find the correct causal answer(s).

## **5 Appendix**

This Appendix explains the NCSC data in more detail as well as what turns on the various exclusion decisions that I made. I also provide state-by-state version of Figure 8B for those interested in more detailed results. And I give the summary statistics here as well.

### **5.1 The NCSC Data**

The National Center on State Court’s State Court Caseload Statistics Program provides an in-depth look at the demands placed on state court resources. Among the variables it measures is the number of felony filing made in state court each year. Due to a change in data-gathering methods, data up through 1993 cannot be compared with data from 1994 onward, and so I restrict myself to the latter period. Between 1994 and 2008 (the last year of available data), twenty-six have provided reliable felony-filing data to the NCSC every year; another thirteen provided data every year, but for at least some or all of those years the data the NCSC classified the data as “incomplete,” “overinclusive,” or “incomplete and overinclusive.” A visual inspection of the data sug-

gests that there are few year-to-year changes in felony filings in these states that appear anomalous. Thus even if the levels are not entirely correct, the rates of change appear likely to be accurate most of the time.

I then cull these twenty-six and thirty-nine state samples down to twenty-one and thirty-four states, respectively. In both cases, I drop five states (Washington, DC, Florida, Illinois, Kansas, and New York) due to limitations in other sources of data, specifically prison admissions (for Washington, DC) and arrest data (for the remaining states). Table A1 lists the twenty-one and thirty-four states in the two samples. In the body of the paper, I present the results for the thirty-four state sample. In this part of the appendix, I simply want to show that the cleaner twenty-one state sample yields similar results. In other words, my results above do not appear to be driven by problems with the filing data in the larger sample, and using the larger sample size allows me to produce more externally-reliable results.

Consider the basic trends. In my thirty-four state sample, between 1994 and 2008 major arrests declined by 10.1%, cases filed rose by 37.4%, and prison admissions rose by 40%. For the twenty-one state sample, these values are -9.8%, 39.9%, and 34.6%. As is immediately clear, these values are quite similar in magnitude. The same pattern holds true for the rate of filings per arrest and admissions per filing. In the thirty-four state sample, between 1994 and 2008 filings per arrest rise from 0.374 to 0.573, and admissions per filing shift from 0.258 to 0.264. For the twenty-one state sample, filings per arrest rise from 0.380 to 0.590, and admissions per filing shift from 0.229 to 0.220.

And, not surprisingly, the counter-factual models yield similar results as well. In Figure 4A, the counterfactual level of filings for the thirty-four state sample in 2008 is 65.4% that of the true number of filings; for the twenty-one state sample, 64.8%. And in Figure 4C, for the thirty-four state sample the “incarceration rate counterfactual” level of admits is 97.8% that of the true number in 2008, and the “filing rate counterfactual” level is 64.0%. For the twenty-one state sample, these values of 103.9% and 67.0%, respectively. As these results indicate, the two samples track each other closely, suggesting that whatever reporting errors

are present in the thirty-four state sample are not driving the results given in the paper.

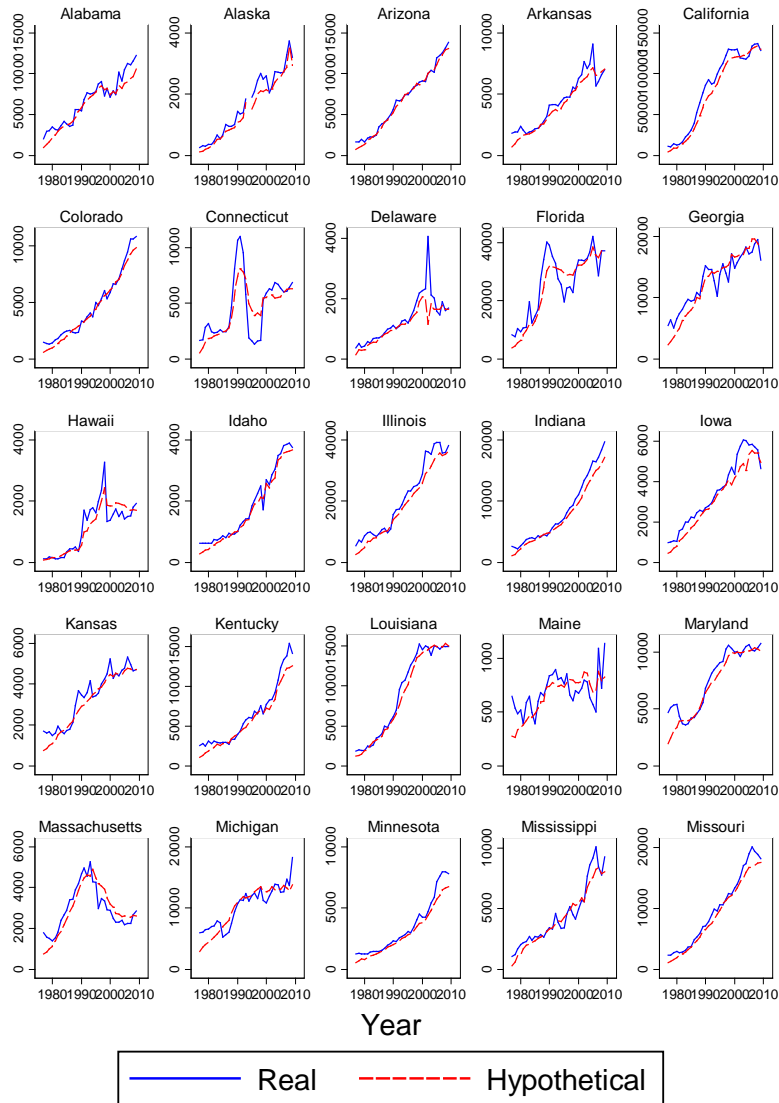
Table A1: States in the Sample

States in the 21-State Sample	Add'l States in 34-State Sample
Arizona	Alaska
Arkansas	California
Colorado	Connecticut
Georgia	Hawaii
Idaho	Louisiana
Indiana	Maryland
Iowa	Michigan
Massachusetts	Minnesota
Missouri	Nebraska
New Jersey	New Mexico
North Carolina	Tennessee
North Dakota	Texas
Ohio	Wisconsin
Oregon	
Rhode Island	
South Dakota	
Utah	
Vermont	
Virginia	
Washington	
West Virginia	

## 5.2 State-Level Results for Figure 8B

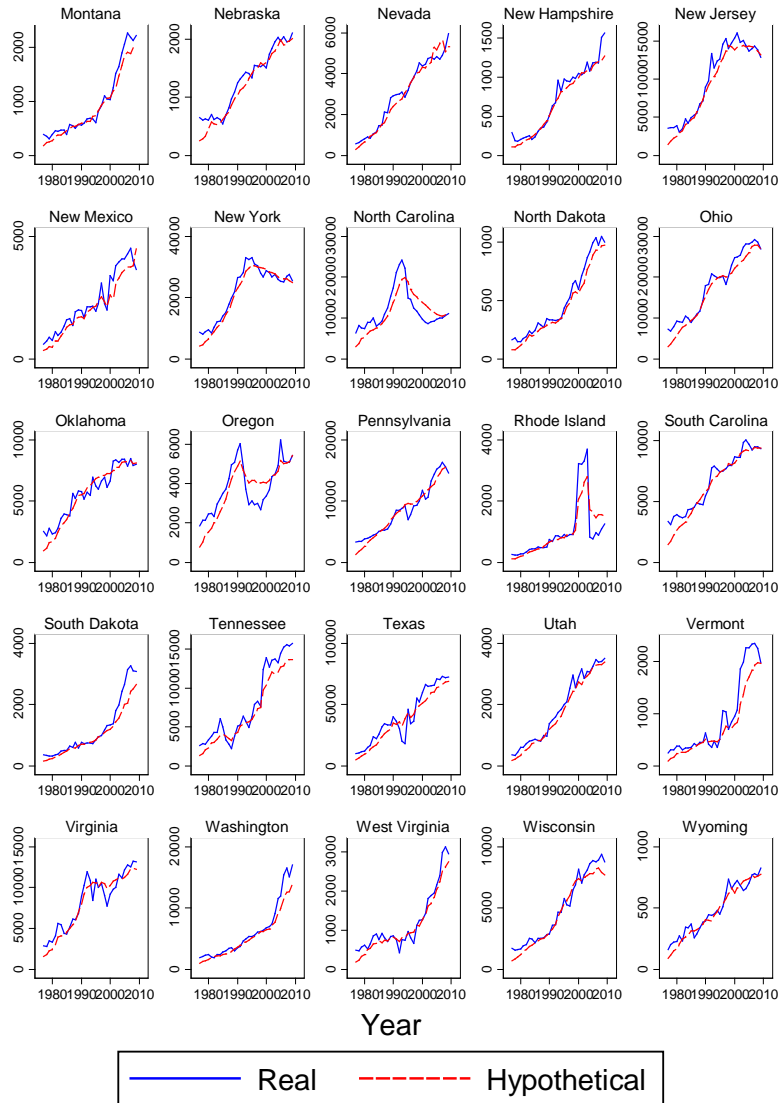
Figure A1 provides the state-by-state graphs that are presented in a more aggregate form in Figure 8B. Perhaps even more starkly than Figure 8B, Figure A1 makes it clear the extent to which my hypothetical release rate—which is fixed at the same level across all states and years—tracks each state’s actual release practices.

Fig A\_: Real and Hypothetical Releases  
1977 - 2009



Data from the BJS's National Prisoners Statistics.

Fig A\_: Real and Hypothetical Releases  
1977 - 2009



Data from the BJS's National Prisoners Statistics.

### 5.3 Summary Statistics

Table A2 provides the summary statistics for the thirty-four state sample.

Variable	Obs.	Mean	St. Dev.
Major arrests	505	100,246.6	118,207.6
Felony filings	510	49,545.27	55,348.82
Prison admissions <sup>37</sup>	509	12,912.22	22,916.9
Filings per arrest	15	0.502	0.077
Admits per filing	15	0.261	0.009
Prison admissions <sup>38</sup>	1648	8,948.143	16,029.02
Prison releases	1649	8,306.838	15,283.82

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<sup>37</sup> These are the statistics for the thirty-four state sample between 1994 and 2008.

<sup>38</sup> These are the statistics for the fifty-state sample between 1977 and 2009. Admission data for Delaware is missing in 2002, which is why there is one fewer admission observation than release.

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