"FIXED" SENTENCING: THE EFFECTS ON IMPRISONMENT RATES OVER TIME

by

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Beginning in the 1970s, states adopted sentencing reforms as a response to a growing number of concerns in the criminal justice system. These reforms included sentencing guidelines, statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws. Each reform has become an important part of the judicial system. These "fixed" reforms shifted sentencing from the indeterminate-rehabilitation sentencing model to a more predetermined-deterrence model. The reforms' main purpose is to limit judicial discretion by insuring convicted felons receive a reasonably standard sentence depending on the crime they committed.

Few studies have attempted to systematically answer the question of whether these reforms produced the outcomes stated by their supporters. This analysis utilizes a social chain theory, which suggests the socio-political context of the law and order movement interacted with structural-procedural changes in the justice system that led to

unintended consequences. The study assesses the effects of sentencing reforms on shifts in year-to-year changes in general incarceration rates, changes in the racial/ethnic composition of imprisonment, and changes in the gender composition of imprisonment. It also assesses the social, political, and demographic characteristics of states that change the rate of adoption of sentencing reforms across all 50 states from the years 1965 to 2008 on the aggregate state level.

This study finds, counter to most previous findings, that sentencing reforms are associated with higher rates of imprisonment. The results further suggest mechanisms are at work that unintentionally "target" historically disadvantaged groups, perpetuating inequalities within the criminal justice system instead of easing them. This result is counter to some of the policies' stated goals. Conversely, the results suggest that drug arrest rates and not sentencing reforms are associated with the narrowing gender gap in imprisonment. Finally, the results indicate that state-level characteristics are important in predicting which states will adopt sentencing reforms. From a policy perspective, rapid changes in the composition of imprisonment can be a logistical and financial burden, and these results shed light onto the specific mechanisms causing a portion of the change.

This dissertation includes previously unpublished co-authored material.

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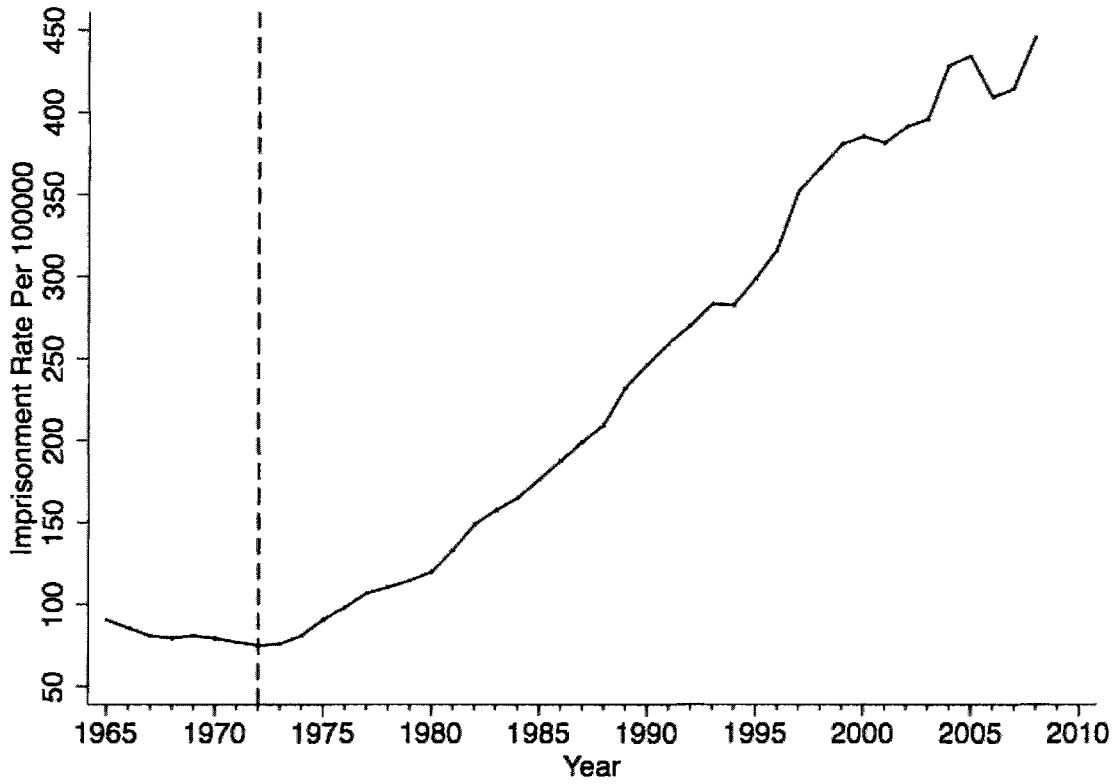
CHAPTER I

INTRODUCTION

Beginning in 1972 imprisonment rates in the United States began what would become the largest growth in inmate population per-capita in the history of the nation. In the four decades that followed, the U.S. would witness an unprecedented steady and rapid growth in imprisonment. At the start of the rise, the imprisonment rate was about 100 inmates per 100,000 U.S. residents and by 2007 there were 506 inmates per 100,000 (see Graph 1.1). This 500% increase drastically changed the way the prison system and the justice system operated (DeFina and Arvanites 2002; Greenberg and West 2001).

At the same time imprisonment rates were rising, the U.S. embarked on a new and vigorous "war on crime" that was characterized by a general "get tough" mentality. The "get tough on crime" mentality was part of a larger movement some researchers have come to call the law and order movement. The movement was born out of a counter culture movement against the 1960s where civil unrest resulting in riots, radical black power and youth movements, assassinations of top political figures, increased crime, and increased drug use drove public pressure to "do something about it" (Roberts 1996). This public perception interacted with partisan politics and media coverage to create a "perfect storm" for the law and order movement to flourish (Abramsky 2007; Beckett 1997; Scheingold 1991).

Graph 1.1. Average State Imprisonment Rate Over Time from 1965 to 2008



Note: The dotted reference line indicates 1972, which was the last year imprisonment remained relatively low before the rapid and steady increase of the "imprisonment binge" began.

Sentencing reforms were a part of the law and order movement and have drastically shifted the function and goals of sentencing to a system that is relatively more predetermined and calculable. The main purpose of sentencing reforms is to remove judicial discretion by insuring that convicted felons receive relatively similar sentence depending on the crime committed. Prior to sentencing reforms, the U.S. was dominated by an indeterminate sentencing system. This system allowed considerable latitude to the judge in rendering the type and degree of sentence. Parole boards acting independently of other parts of the judicial system and largely independent of legislators then

determined if the prisoner had been properly rehabilitated and was ready for release. This model represented the rehabilitation goal of sentencing. It was believed that offenders could be reformed within prisons (Blumstein, Cohen, Martin, and Tonry 1983; Frase 2005). Sentencing was meant to meet the offender's needs and patterned to allow the process of reform to play out. Thus, offenders were sentenced using rather loose sentencing ranges (Stemen, Rengifo, and Wilson 2006; Tonry 1995).

The sentencing discretion was granted to the judge (and in a few jurisdictions, the jury) largely by legislators at both the state and federal levels who established rather loose sentencing ranges, often allowing combinations of confinement and probation. This allowed the judge latitude to select a sentence within the relatively wide prescribed range when establishing minimum and maximum amounts of time-served (Roberts 1996). Relatively few set laws or procedures defined how the judge was to determine the sentence. Upon release, the convict would be reintroduced as a rehabilitated individual who was socially functional and law abiding (Albonetti 1997; Kempf-Leonard and Sample 2001; Marvell 1995).

The dominance of the indeterminate sentencing model came to an end during the law and order movement. The criticisms began during the 1970's, reached new heights during the 1980's, and continue today on multiple fronts (Hebert 1997; Reitz 1995). Critics advanced a number of purported problems, including complaints about the arbitrariness of the sentencing procedure, ineffectiveness of treatment and reform programs, lack of tough time, and claims of rampant repeat offenders. Critics focusing on sentencing charged that it allowed far too much disparity in types of sentencing and

time served. As a result, a push to limit the variability in sentencing occurred (Griset 1995; Hebert 1997; Miethe and Moore 1985).

Whereas the indeterminate-rehabilitation model of sentencing is offender-centered, the sentencing reform policies of the law and order movement are crime centered. Their goal is not to reform offenders, but to punish than. The model serves to create uniformity among similarly situated crimes. Judges no longer set sentences based on a loose range, but instead pass along a pre-determined fixed sentence to the offender based almost solely on their prior record and crime committed (Spohn 2000; Tonry 1995). Often, parole boards were then significantly limited or removed as the decision making body in determining if the offender was ready to return to society. The model pushes towards strict deterrence theory where the punishment is ideally, swift, certain, and severe. While the due-process revolution has made the swift component difficult to achieve[1], sentencing reforms were successful in implementing the certain and severe components (Blumstein, Cohen, Martin, and Tonry 1983; Von Hirsch 1985).

In this analysis we examine the effects of the six main sentencing reforms passed after 1972, which include sentencing guidelines (divided into two different types), statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws (see Appendix A for detailed discussion of the structure of each reform). Each has become an important part of the criminal justice system (see Table 1.1). Appendix A describes in detail the differences between the various reforms, but the key

[1] This is not necessarily a bad thing and many researchers have shown the benefits of such limits (Albonetti 1997; Saltzburg 2001).

is that they all generally make a shift away from the rehabilitation model and towards a deterrence model with more predictability.

Three of the reforms operate on the front-end, focusing on judicial discretion. They can be ranked in accordance of their level of severity and by the power they possess to limit discretion. The first is *presumptive sentencing guidelines.* It consists of a table of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) running across the top of the table and the severity of the current offense running down the side of the table. These two main determinants are cross-referenced to determine the sentencing range. The reform is strict in its application. Allowing only very limited deviations (*e.g.*, judge decides to hand down a longer than prescribed sentence).

The second most severe reform is *voluntary sentencing guidelines.* They treat guidelines as a formal recommendation, but do not *legally mandate* they be followed. The difference is in their legal mechanisms. States with presumptive sentencing guidelines have "legally-binding" sentencing matrixes enforceable by appellate review, allowing both the prosecution and defense to appeal sentences that do not follow the guidelines. Conversely, in states with the voluntary guidelines, there is no appellate review. First passed in the mid 1980s, sentencing guidelines are the most rational and predictable of the front-end reforms.

The third and final front-end reform considered is *statutory presumptive sentencing.* Like sentencing guidelines, it represents an attempt to create uniformity within similarly situated crimes, but acts less like a sentencing rubric. It specifies an

appropriate or "normal" sentence for each offense, creating more uniformity within similarly situated crimes, but ultimately leaves a large degree of discretion to judges (Spelman 2009; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009).

Presumptive sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing are considered independent and mutually exclusive "front-end" "fixed" sentencing reforms and may not, at any given time, coexist with each other or indeterminate sentencing. Their main focus is in the actions of the judge. Conversely, the "back-end" limiting (truth in sentencing) or elimination (determinate sentencing) of discretionary release is considered separate because their focus on release allows operation alongside the "front-end" reforms. These two reforms act to limit the ability of prison officials to shorten the original sentence (Blumstein, Cohen, Martin, and Tonry 1983; Tonry 1995).

Truth in sentencing legislation was first instituted in Illinois in 1978 and twenty-four states now have such laws. These laws require that those convicted serve a statutorily defined minimum amount of time, limiting the power of parole boards or other release mechanisms.[2] The other major back-end reform is often called *determinate sentencing* and, in general, is used to refer to a system *without* discretionary parole boards.

Finally we consider three strikes laws. This reform is unique and independent from the other reforms because it focuses on a specific offender; the habitual repeat offender. Three strikes can be considered alongside both the front-end and back-end

[2] In our coding of this variable we included only states meeting the 1994 federal omnibus crime bill minimum 85% time served of the original sentence.

reforms. They work to sentence three-time felony offenders to long sentences, often 25 years to life. Washington was the first state to pass a three strikes law in 1993 and due to a couple of high profile cases that created a media frenzy, 23 more states adopted the reform in a short three-year period. Because of the unique nature of the reform and the way the law was passed, the effect of this reform is likely to be less than the previous reforms, but any analysis of sentencing reforms would be remiss if it did not consider them (Boerner 2001; Dickey and Hollenhorst 1999; Kovandzic, Sloan, and Vieraitis 2004).

Table 1.1. Distribution of Sentencing Types Across the United States as of 2008

	Presum. Guide.	Vol Guide.	Stat. Presum.	Deter. Sent.	Truth in Sent.	Three Strikes
Alabama	-	-	-	-	-	-
Alaska	-	-	1980	-	-	-
Arizona	-	-	1978	1994	1994	-
Arkansas	-	1994	-	-	-	1995
California	-	-	1976	-	1994	1994
Colorado	-	-	1979	-	-	1994
Connecticut	-	-	-	-	1995	1994
Delaware	1987	-	-	1990	1990	-
Florida	1983	-	-	1983	1995	1995
Georgia	-	-	-	-	1995	1995
Hawaii	-	-	-	-	-	-
Idaho	-	-	-	-	-	-
Illinois	-	-	-	1978	-	-
Indiana	-	-	1977	-	-	1994
Iowa	-	-	-	-	1996	-
Kansas	1983	-	-	-	1993	1994
Kentucky	-	-	-	-	-	-
Louisiana	-	1992	-	-	-	1994
Maine	-	-	-	1976	1995	-
Maryland	-	1983	-	-	-	1994
Massachusetts	-	-	-	-	-	-
Michigan	-	-	-	-	1994	-

Table 1.1. (continue)

	Presum. Guide.	Vol Guide.	Stat. Presum.	Deter. Sent.	Truth in Sent.	Three Strikes
Minnesota	1980	-	-	1982	1993	-
Mississippi	-	-	-	1995	1995	-
Missouri	-	1997	-	-	1994	-
Montana	-	-	-	-	-	1995
Nebraska	-	-	-	-	-	-
Nevada	-	-	-	-	-	1995
New Hampshire	-	-	-	-	-	-
New Jersey	-	-	1977	-	-	1995
New Mexico	-	-	1977	-	-	1994
New York	-	-	-	-	1995	-
North Carolina	1994	-	-	1994	1994	1994
North Dakota	-	-	-	-	1995	1995
Ohio	1996	-	-	1996	1996	-
Oklahoma	-	-	-	-	-	-
Oregon	1989	-	-	1989	1995	-
Pennsylvania	1982	-	-	-	1991	1995
Rhode Is.	-	-	1980	-	-	-
South Carolina	-	-	-	-	-	1995
South Dakota	-	-	-	-	1996	-
Tennessee	1989	-	-	-	1995	1995
Texas	-	-	-	-	-	-
Utah	-	1993	-	-	1985	1995
Vermont	-	-	-	-	-	1995
Virginia	-	-	-	1995	1995	1994
Washington	1983	-	-	1984	1984	1993
West Virginia	-	-	-	-	-	-
Wisconsin	-	1985	-	-	-	1994
Wyoming	-	-	-	-	-	-
Total	10	7	8	12	24	24

Note: Table 1.1 represents the current sentencing type used by each state as of 2008. Presum. Guide. refer stands for presumptive sentencing guidelines. Vol. guide. Stands for voluntary sentencing guidelines. Stat. Presum. Sent. Stands for statutory presumptive sentencing. Deter. Sent. stands for determinate sentencing. Truth in sent. stands for truth in sentencing. Three strikes refers to three strikes laws. All other states utilize indeterminate sentencing.

While states adopting reforms often adopted numerous types (*e.g.,* Oregon which adopted presumptive sentencing guidelines in 1989, abolished parole in 1989, and adopted truth in sentencing in 1995), their coexistence is not absolute and they were often instituted in different years (see Table 1.1 for complete list of reforms) (Frase 2005; Stemen, Rengifo, and Wilson 2006). There are three key points about the reforms that

should be noted. First, all six reforms represent a shift from the indeterminate-rehabilitation model to a more punitive model with a discretion-limiting sentencing structure. Second, because the dependent variables in the analyses presented in chapters 2-5 do not include county or local jail populations, only individuals sentenced to more than one year are considered. Because it takes time from the passage of these reforms until their implementation, we use a one-year lag (impact one year after inception) in our analyses. Third, while in the year following the reform there was likely an instantaneous effect on some portion of the imprisonment rate, not all individuals imprisoned during that year were "caught up" by the reform. Some of the individuals contributing to the imprisonment rate (the dependent variables) were individuals who were already imprisoned and sentenced under the old sentencing procedure. Over time, a greater proportion of prisoners were likely to be "caught up" by the reform. At some point a plateau at nearly full effect would have occurred with only a small portion of the rate attributable to those sentenced to long terms under indeterminate sentencing. To address both the lagged effect and capture the expected "logged growth curve" like effect, a new measure of reforms was implemented. Using a logarithmic measure to model the effect of sentencing reforms represents an important divergence of this study from previous studies, which relied heavily on dummy variables to code the reforms effects on imprisonment rates in an "instantaneous" manner.[3]

[3] Coding the reforms as dummy variables for adopted reforms suggests that imprisonment rates would be affected 100% by the new policy in the first year it was adopted. A logarithmic measure represents a more appropriate theoretically expected effect and should lead to more robust results.

Few studies have attempted to systematically answer the question of whether these new sentencing procedures or parole board's limitations produced the outcomes stated by their supporters. The current study assessed the effects on shifts in incarceration rates within states (all 50 states) from the years 1965 to 2008.[4] The study has implications for both theory and policy. Substantively, the results may indicate that reforms significantly increased imprisonment. Functionally, rapid prison growth can be a logistical and financial burden on a state and these results could help to shed light onto the specific mechanisms associated with this growth. Furthermore, the results may indicate that socio-political mechanisms are at work that "target" historically disadvantaged groups, which may aid in the continuation of inequalities within the criminal justice system. This is of particular concern given that many states have suggested that racial and ethnic disparities should decrease with the limiting of judicial discretion. If results indicate no change or even an increase in disparities despite intentions to reduce, then states may want to reevaluate their sentencing procedures.

Primarily the research aims to assess "changes over time" within state imprisonment rates due to sentencing reforms. The full project assessed the effects of sentencing reforms on (1) the total state imprisonment rate; (2) imprisonment rates of people of color; (3) the female to male composition of state imprisonment rates; and (4) sociopolitical climates within states that led to adoption of sentencing reforms. The goal of the full project (all 4 sections) is to shed light on the effects of "fixed" sentencing reforms on these key areas of imprisonment rates by building theoretical understandings

[4] Some of the individual analyses incorporate fewer years or include only 49 states.

of the adoption of reforms and the policy implications resulting from unforeseen consequences. States have committed large amounts of time and money in the process of adopting and implementing these reforms and research is needed to assess their impact.

Given the context of the time (*e.g.*, the law and order movement) and the increased bureaucratization of the system (*e.g.*, Weberian formal rationalization[5]) it was expected that states passing reforms would have shifted their imprisonment rates and composition of those imprisoned as a direct result of the structure of the reform compared to non-reform states. The project suggests that formal rationality theory gives us the needed structure to understand *how* the reforms affected sentencing, while a sociopolitical interaction theory allows us to understand *why* it happened. Why it happened is found in the law and order movement. The sociopolitical interaction theory suggests the policy shifts represent the highly connected push-pull between the media, politicians, and a public demand for punishment that characterized the law and order movement. Politicians championed the reform movement where "getting tough on crime" became a political hot button issue and policy makers, both liberal and conservative, risked political suicide if they appeared even remotely soft on crime and justice (Gottschalk 2006; Jacobs and Carmichael 2001; Smith 2004). The theory suggests U.S. states began adopting neoclassical or what we have come to call "fixed" sentencing[6] procedures (see appendix A for descriptions) when both political rhetoric and

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[5] This concept is presented in more detail in subsequent chapters

[6] The name "fixed" sentencing is designed to illustrate the more determined nature of the reforms in comparison to indeterminate sentencing. This is not to suggest reforms are "fixed" with no discretion or departures. The name is only intended to highlight the more structured and more assured nature of the new sentencing procedure. "Fixed" reforms refers to sentencing reforms in the form of sentencing guidelines,

public outcry combined to lead to a demand for highly punitive sanctions, the result being a focus on few things other than punishment (Frase 2005; Stemen, Rengifo, and Wilson 2006). We further suggest that the sociopolitical movement to get tough on crime produced a shift from a substantive rational system to a formal rational system. This structural shift gives the reforms the needed mechanisms to have a causal impact on imprisonment. In the end, this paper suggests the "fixing" of sentencing represents both the goal of creating more punitive sanctions (ensuring the move towards the deterrence model) and the shift in structure to ensure it.

As Scheingold (1991) and Beckett (1997) suggest, the shift from indeterminate sentencing to "fixed" sentencing represented the concerted effort by policy makers to advance what has been called the "justice model" of crime and punishment that focuses on *making criminals pay* and *deterring* those who might commit crimes in the future. The model creates a system that is often referred to as "just deserts" in which the punishment is designed to fit the crime not the offender (Doob 2000; Frase 2005; Simon 2007). "Fixed" sentencing may serve as a concrete policy that represents a cultural climate in which both political rhetoric and public outcry resulted in highly punitive sanctions that focus little attention on any goal other than punishment.

While considerable criminology research focused on the structural causes (*e.g.,* poverty, racial segregation, and class conflict) of crime and imprisonment and the social disparities embedded in them (Engen and Gainey 2000; Engen and Steen 2000; Irwin and

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statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws which shifts the system from a indeterminate rehabilitation model of sentencing to a more predetermined punitive structure.

Austin 1997; Steffensmeier and Demuth 2000; Tayler 2000), research also suggests policy makers largely operate and make decisions outside the structural approach. Therefore a focus on the structures will not inform our understanding of why the policy was adopted (Beckett 1997; Doob 2000; Simon 2007). Scheingold (1991) suggests, instead, we need to focus on the public perceptions of crime. He argues that the public largely constructs its view on the interpersonal level, focusing on the offender-victim paradigm. That is, crime is an issue of underlying offender pathology and the personal-societal effect it has on the victim: focusing on personal harm and the social stigma attached to it. To the average citizen, the state's purpose is in *limiting* the actions of those who are likely to "perform" deviant acts and *punishing* those who have wronged others. Understanding the crime and justice schemas of the average citizen suggests why there is such an intense focus on sentencing. To the average citizen, the sentence is both the loci of *punishment* and the loci of *protection* (Beckett 1997; Jacobs and Carmichael 2001; Scheingold 1991).

But as Scheingold (1991) further suggests, the mechanisms by which policy are adopted is complex and interactive taking into consideration policy; policy makers at national, state, and local levels; political elites; public opinions; citizen activism; and media responses and representations. "[P]olicy changes are mostly likely to occur when political leaders, in part by taking their cues from the media, choose to play upon public anxieties that are themselves inflamed by media imagery and vicarious victimization rather than by crime as such (Scheingold 1991:44)".

An examination of the justice model under the law and order movement[7] lends support to this argument. Expansion of the justice model was often used to further political agendas by playing on public fears of crime. Both Republicans and Democrats championed this agenda and benefited from the movement. "Getting tough on crime" became a political hot topic, and policy makers, both liberals and conservatives, pushed an individualistic view of crime as a function of societal deviants that should be addressed by punitive measures (Abramsky 2007; Hunter 1991; Simon 2007).

Meanwhile, the media added to the movement by continually displaying images of crime that were largely symbolic of individual deviant actors preying upon random isolated individuals. Once the three components, the public, the media, and politicians, became full integrated and the fear of crime became a social issue, politicians then seized upon the fear to create law and justice platforms focused on *getting tough on crime*. This complex circle of interactions served to promote and sustain the politicization of crime that reinforces the deterrence model of justice (Beckett 1997; Simon 2007).

Savelsberg (1992) argues sentencing reforms are an ideal representation of what Weber calls formal rationality; while indeterminate sentencing, conversely, is more closely aligned with substantive rationality. Both sentencing procedures are rational as defined by Weber (1978) as they are based in law, giving the procedures "legalized" validity, but they differ in the application of the laws. Substantive decision-making is subject to values, appeals to ethical norms, and historical precedent with less focus on uniformity of the outcomes. Formal decisions, on the other hand, focus on limiting

[7] See Abramsky (2007), Hart (2005) or Hunter (1991) for a more detailed discussion of the "law and order movement."

subjective decision-making by stressing rationalized structural outcomes that removed subjective values by emphasizing a technical orientation to procedural process grounded in well-defined criteria designed to elicit a specific outcome. In the end, "fixed" sentencing reforms serve to bureaucratize the process with the specific goal of removing subjective interpretations and outcomes.

Although Weber's idea of a formal rational system advanced a theory highlighting separations between law and politics, bureaucratizing of sentencing into "fixed" procedures through political agendas may actually bridge the gap between law and politics. This suggests the Weberian argument may be informative in understanding the limiting of discretion: but it is an incomplete theoretical frame (Beckett 1997; Hunter 1991). Sheingold (1991) theorizes that until recently, application of law historically operated somewhat independently from political battles. National political policy and even state and local political policy specific to crime and punishment served more as a symbolic gesture that enhanced political rhetoric, but had less operational effect on how the justice system acted. But under the law and order movement, the separation between politics and law (or at least operational practices) may have been blurred with considerably more power placed into the hands of policy makers (Doob 2000; Scheingold 1991). For example, with "fixed" sentencing, the judge no longer "fits" the sentence and instead is reduced to the "automation" of the paragraphs written by policy makers or their designee (*e.g.*, sentencing commissions) (Boerner 2001; Tonry 1995).

Reform legislation was largely passed without the support of the courts, particularly judges, who immediately disapproved feeling they had lost their power to fit

the offense with appropriate sentences based on the contexts of the crime and offender (Savelsberg 1992). Now in most cases a judge can only deviate from this prescription by providing a written explanation of the reasons why she or he would do so (recent Supreme Court decisions have called this procedure into question). Research does indicate, on average, judges comply with guidelines 85% of the time, indicating the separation of law and politics may have been bridged (Marvell 1995; Scheingold 1991). This suggests a more appropriate theoretical frame may be a blending of the pure Weberian notion of shifting towards a rationally defined system where the judge now mindlessly acts out the prescribed structures with Sheingold's "blurring" of legal action and politics into a system susceptible to social and cultural pressures. The net result is a system that both limits variability and increases the average sentence. It should be noted that Weber's notion of formal rationality did not rule out a strong interaction of the political and legal spheres, but Sheingold's argument offers a more historically relevant theoretical explanation of the causes of the intersection and suggests a tighter weave.

Given that "fixed" sentencing reforms not only bureaucratized sentencing, but were also established under the politics of fear associated with the law and order crusade, it is likely the interaction of the various components discussed above have served to increase imprisonment beyond indeterminate sentencing. While some policy makers have argued that "fixed" sentencing reforms would not increase imprisonment rates (Brewer, Beckett, and Holt 1981; Engen and Gainey 2000), the sociopolitical pressures to "get tough on crime" are likely to manifest themselves in the policies stressing punitive punishment (Abramsky 2007; Beckett 1997; Hunter 1991; Simon 2007). Though states

not adopting "fixed" sentencing are also likely to be influenced by the law and order movement as well, "fixed" reforms are likely to increase imprisonment more by "formalizing" the political into law. For example, by limiting the low end of the sentence range, the judge has no ability to select lenient sentences for at least some felons (Savelsberg 1992; Weber 1978).

Chapter 2, herein, "Fixed" Sentencing: The Effects on the Total Imprisonment Rates Over Time, highlights the various sentencing reforms, both front- and back-end. It sets the context for these reforms in the law and order movement and the relation of these reforms to formal rationalization theory. It develops rationales for the hypothesized expected effect of reforms on changes in state imprisonment rates. The chapter is primarily concerned with the shifts in state imprisonment rates due to the adoption of the six sentencing reforms included in this analysis. The analysis also includes a number of new methodological innovations making the analysis significantly more adept at assessing the true outcomes. Chief among them was the adoption of a conditional change score panel model and the new "logarithmic growth curve" measure for sentencing. These innovations are one of the primary differences between this analysis and prior research on this subject.

While Chapter 2 deals with the effects of reforms on total state imprisonment rates, which is an area of reform research analyzed by other scholars, the effects of sentencing reforms on the racial/ethnic composition of imprisonment is an area, to date, that has not been assessed on an aggregate state level. The racial/ethnic composition of

those incarcerated is the focus of Chapter 3. The analysis in Chapter 3 assesses changes in the odds of Black to White imprisonment and Hispanic to White imprisonment.

Chapter 3 borrows arguments from Chapter 2, namely that the social context of the law and order movement interacted with the structural change that resulted from sentencing reform adoption, but takes it a step further by adding an additional link to the chain. Unfortunately, in the context of the racial composition of those incarcerated, the Weberian formal rationalization process described by Savelsberg (1992) may also serve to fortify the institutionalization of racist policies (or at a minimum policies that create racial disparities). The technical-rational apparatus serves to replace discretion (*i.e.,* individual) based racism with institutional racism in which the byproduct serves to create more "legitimized" justification for the higher rates of imprisonment among people of color (Ulmer and Kramer 1998; Ulmer and Johnson 2004). At first view, the sentencing process seems to be justified due to its "race neutral apparatus" that focused on crime and criminal history rather than an individualistic characteristic of the offenders. In actuality, it worked to conceal and possibly amplify the racial and ethnic disparities (Abramsky 2007; Beckett 1997; Scheingold 1991).

Drawing on work by Gary Fine (2006), Chapter 3's complete theoretical frame is presented as a series of chained social actions connected through a cascading series of events that ends with an increase in racial disparities (see Figure 1.1). While each link in the chain possesses a distinct theoretical frame, the entire chain is needed to understand why and how the changes in the racial and ethnic composition of imprisonment were

manifested. The analysis ultimately focused the expected outcome on Merton's theory of the unanticipated or unintended consequences of social action; the unintended consequences framework alone is insufficient to explain the policy outcomes (Fine 2006). Without the context of the structural shifts, the outcome that occurred might well have been quite different than what was observed and without the specifics of the entire chain of actions different outcomes might be anticipated.

Figure 1.1. Social Chain Theory Used in Chapter III

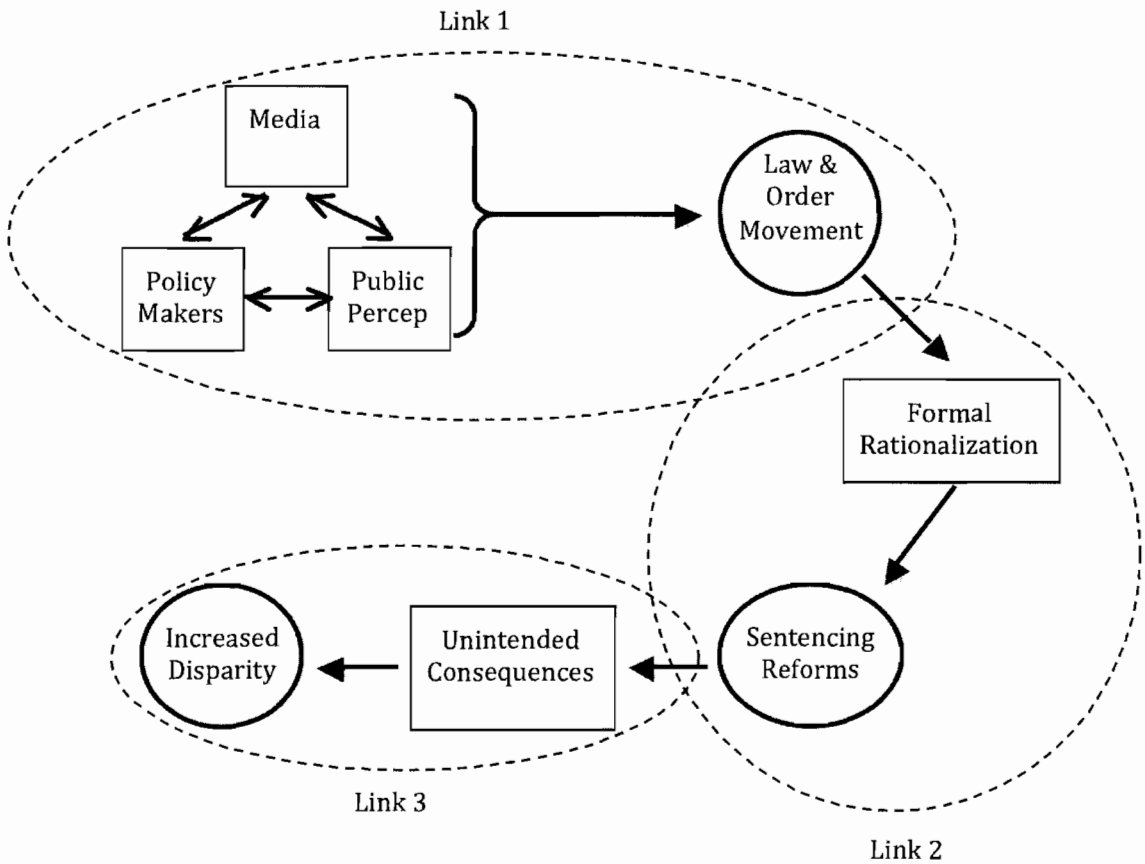

Figure 1.1 outlines the general framework of the social chain. The three ovals represent an event or tangible social action that is preceded by a square (or a series of

squares in the case of the law and order movement) representing the distinct theoretical frame for each link in the chain. While each link is important in its own right, the entire chain is ultimately more important in explaining the outcome than any one part. For example, unintended consequences as a result of the adoption of sentencing reforms are both a social phenomenon, characterized by the interaction of the media, public, and politics found in the law and order movement, and a structural phenomenon, found in the formal rationalization of the sentencing structure. While Merton's theory may suggest a rise in disparities among people of color is a likely result given the power of unintended consequences, the origins of the determinism are rooted elsewhere. Without both previous links of the chain, the final link, the rise in racial disparities in sentencing as an unintended consequence, is insufficient in understand the expected outcome (Wooldredge, Griffin, and Rauschenberg 2005).

Steffenmeier and Demuth (2000) argue "fixed" sentencing reforms also led to a *widening of the net* of those who are sentenced to prison in three possible ways. First, some convicted criminals of relatively small crimes may not have gone to prison prior to sentencing reforms. Generally inmates only go to prison if they are sentenced to more than one year; it is plausible under "fixed" sentencing relatively minor crimes requiring a sentence of less than a year would, under the reforms, carry a sentenced of more than a year, leading to a direct impact on new commitments to prison. Second, sentencing reforms may lead to more criminals being sentenced to prison time rather than probation, resulting in a widening of the net of those who go to prison as new commitments. Third, sentencing reforms often mandate that prisoners released from prison be placed under

supervised parole, rather than unconditional release or other more passive forms of parole, resulting in a direct increase in parole violators returned to prison.

There is particular concern that "net-widening" is disproportionately affecting females more than males. This concern is examined in Chapter 4 (includes previously unpublished co-authored material with Robert M. O'Brien), which tests the effects of sentencing reforms on the odds of female to male imprisonment. Previous research indicates the gender gap in imprisonment is narrowing with the female imprisonment rates growing faster than the male imprisonment rate. Furthermore, research also indicates the single fastest growing demographic group in prisons is females of color, suggesting the possibility that this group is being affected significantly more (Western 2005). The resulting growth in the female imprisonment rate may be associated with changing social expectations and roles of women in society and a resulting "equalizing" of the punitive treatment of female to male criminality (Doob 2000; Griset 1995).

Research into judicial attitudes suggest judges view females as the primary caregivers and often refrained from long sentences for mothers, but with more "fixed" sentencing judges may now see themselves as having few options except sentencing women to longer sentences than desired (Ulmer and Kramer 1998). Steffensmeier et al. (1998) research indicates that race is a stronger main effect on female imprisonment than male imprisonment. With "fixed" sentencing focused on drug and violent crimes, which have become associated with people of color, particularly blacks; race, gender, and crime may interact to increase female rates higher in states adopting reforms. This assertion is bolstered by research indicating violent and drug crime arrests are rising faster among

women than men and these increase may play into the politics of fear, further pushing for more consistent sentencing targeted at females specifically (Abramsky 2007; Ulmer and Johnson 2004).

Chapters 2-4 deal with the effects of sentencing on three key components of imprisonment. Chapter 5, on the other hand, shifts the analysis from an outcome model, concerned with changes in imprisonment, to an analysis of the social, political, and demographic environments in which reforms are more likely to be adopted. This Chapter utilizes event-history analysis to assess the relative likelihood a reform is adopted depending the level of state variation (Blossfeld and Rohwer 2002; Carroll 1982; Wu 2008). For example, in this analysis, I tested whether reforms were more likely to be adopted if states had high unemployment, high percent under the poverty line, Republican control of the legislator or governorship, or high percent black. These models have the ability to suggest important state demographics that can increase the likelihood a reform is adopted. In the end, the analysis indicates that reform adoption does not happen in a vacuum and which state characteristics are driving reform adoption (Xie 1994).

While the previous three substantive analyses worked within a broad theory of the law and order movement, which created an environment where reforms are likely to be adopted, and their specific structural impact, the event-history analysis focuses on identifying the relative impact of the components of the state environment. Therefore the analysis does not draw from the same broad theoretical approach of the previous analyses. Instead, it focused on smaller arguments used in previous research to suggest

state-level variables associated with sentencing reform adoption. The analysis helps identify the state structural components that influence criminal justice policy decisions (Wu 2008).

The general aim of the entire research project is to bring attention to the affects "fixed" sentencing reforms have on state-level imprisonment rates, how it changed the composition of imprisonment, and what state-level characteristics increase the likelihood that a state adopts a specific reform. The analysis begins with a discussion of the effects of sentencing reforms on the overall imprisonment rate. It is then followed by a discussion of the effects on people of color, specifically Blacks and Hispanics; did reforms increase the racial/ethnic disparities in imprisonment? Third, the analysis examines the role reforms played in the growing female imprisonment rate as compared to males. Finally the analysis shifts slightly and looks at how state social, political, and demographic characteristics affected reform adoption.

CHAPTER II

"FIXED" SENTENCING: THE EFFECT ON IMPRISONMENT RATES OVER TIME

Introduction

Prior to the mid 1970s indeterminate sentencing dominated the United States criminal justice system. This model of justice focused on rehabilitating offenders within prison through work and education programs (Roberts 1996). Sentencing by judges (and in a few jurisdictions, the jury) reflected the rehabilitation goal. In most cases judges were given wide discretion to facilitate the *reform* process through sentencing offenders to loose ranges (*e.g.,* 5 to 25 years). Parole boards acting independently of other parts of the judicial system and largely independent of legislators then determined if the prisoner had been properly rehabilitated and was ready for release (Albonetti 1997; Kempf-Leonard and Sample 2001; Marvell 1995). The sentencing model reflected a criminal-centered approached designed to pattern punishment to the offender's rehabilitation need. But in the late 1960s and early 1970s, critics advanced a number of purported problems, including complaints about the arbitrariness of the sentencing procedure, ineffectiveness of treatment and reform programs, cozy correctional facilities, and rampant repeat offenders (Hebert 1997; Reitz 1995). Critics focusing on sentencing charged that it

allowed far too much disparity in sentencing types and time served. In response, legislators (or voters) passed discretion limiting sentencing reforms with more pre-determined structures that limited the range and the ability of officials to deviate from them (Clarkson and Morgan 1995; Griset 1995; Hebert 1997).

The current study analyzes shifts in imprisonment rates due to the adoption of sentencing reforms on the state level over time. I focus on six main sentencing reforms passed after 1972, which include sentencing guidelines (divided into presumptive and voluntary), statutory presumptive sentencing, truth in sentencing, determinate sentencing (abolishment of discretionary parole boards), and three strikes laws. Given the context of the time (*e.g.,* the law and order movement) and the increased bureaucratization of the system (*e.g.*, Weberian formal rationalization) it was hypothesized states passing reforms would increase imprisonment rates more than non-reform states. While formal rationality gives us the needed structure to understand *how* the reforms affect sentencing, it does not by itself explain *why* it happened.

Why it happened is found in the law and order movement characterized by a politics of fear. The sociopolitical interaction theory suggests the policy shifts represent the highly connected push-pull between the media, politicians, and a public demand for punishment. Politicians championed the reform movement where "getting tough on crime" became a political hot button issue and policy makers, both liberal and conservative, risked political suicide if they appeared remotely soft on crime and justice (Gottschalk 2006; Jacobs and Carmichael 2001; Smith 2004). The theory suggests U.S.

states began adopting neoclassical or what I have come to call "fixed" sentencing[8] procedures (see appendix A for descriptions) when both political rhetoric and public outcry combined to lead to a demand for highly punitive sanctions, the result being a focus on few things other than punishment (Frase 2005; Stemen, Rengifo, and Wilson 2006).

In the end, this paper suggests the "fixing" of sentencing represents both the goal of creating more punitive sanctions and the shift in structure to ensure it. I suggest the reforms were driven by a sociopolitical movement to get tough on crime that produced a shift from a substantive rational system to a formal rational system. The effect of this shift is tested through a 50 state panel analysis covering the years 1967 to 2007. The models incorporate a number of controls including fixed-effects for states and a first-differenced dependent variable, which allows for the measurement of the average change over-time within states controlling for other state-level time-variant covariates. This model represents a significant divergence from previous research and should supply more robust and theoretically appropriate results. In an additional divergence from previous research, this analysis incorporates a new measure of reforms as logged growth curves that is more theoretically appropriate and should lead to more meaningful results.

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[8] The name "fixed" sentencing is designed to illustrate the more determined nature of the reforms in comparison to indeterminate sentencing. This is not to suggest reforms are "fixed" with no discretion or departures. The name is only intended to highlight the more structured and more assured nature of the new sentencing procedure. "Fixed" reforms refers to sentencing reforms in the form of sentencing guidelines, statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws which shifts the system from a indeterminate rehabilitation model of sentencing to a more predetermined punitive structure.

Literature Review

The previous research assessing "fixed" reforms falls largely into two categories, with notable exceptions. The first focuses on extra-legal effects like the influence of race or gender on judicial departures from baseline sentence recommendations (e.g. Griffin and Wooldredge 2006; Johnson 2006; Kramer and Steffensmeier 1993; Kramer and Ulmer 1996; Steffensmeier, Ulmer, and Kramer 1998; Ulmer and Kramer 1998; Wooldredge and Griffin 2005; Wooldredge, Griffin, and Rauschenberg 2005; Wooldredge 2009), while the second focuses on the influence of policy changes on sentencing outcomes (e.g. Arvanites and Asher 1998; Brewer, Beckett, and Holt 1981; Dalessio and Stolzenberg 1995; Frase 1995; Frase 2005; Griset 1996; Johnson 2006; Tonry 1995). In both cases the body of literature largely utilized data on individual states, counties, or cities. While providing important insights, they are limited to the specific context and sentencing structures of the particular jurisdiction and lack broad cross-sectional applicability. While a few studies analyzed year-to-year changes over time, these studies have also largely been limited to a single state,[9] calling into question their generalizability beyond the single case.[10] This paper follows an even smaller subset of research, which analyzes aggregate state level data over time (*e.g.,* Stemen et al (2006), Spelman (2009), and Zhang (2009). This prior research suggests the front-end sentencing reforms (*e.g.,* sentencing guidelines and statutory presumptive sentencing) do

[9] A few studies have also assessed the federal system (*e.g.,* Albonetti 1997).

[10] Preliminary analysis in this study indicated that considerable state-to-state variation in imprisonment rates, which further calls into question the generalizability of the single state approach.

not directly lead to changes in imprisonment, while the elimination of discretionary parole release (*e.g.,* determinate sentencing) is more likely to do so.

I build on theories advanced by Scheingold (1991) and Beckett (1997) suggesting that the shift in sentencing policy represents the concerted effort by policy makers to advance the "justice model" of crime and punishment through the law and order movement. This framework focuses on *making criminals pay* and *deterring* those who might commit crimes in the future,[11] with punishment tailored to fit the crime rather than the offender (Doob 2000; Simon 2007). To understand why this shift occurs, Scheingold (1991) suggests we must recognize the complex and interactive mechanisms that influence policy change. The interaction includes policy makers at national, state, and local levels, political elites, public opinion, citizen activism, and media responses and representations. In this interaction, the crucial element is the public's fear of crime. "[P]olicy changes are mostly likely to occur when political leaders, in part by taking their cues from the media, choose to play upon public anxieties that are themselves inflamed by media imagery and vicarious victimization rather than by crime as such" (Scheingold 1991:44). Gottschalk (2006; 2009) notes that prior to the late 1960s crime rarely registered as a top public issue and not until the law and order movement did the public begin to demand substantial change. Used to further political agendas by playing on public fears of crime (often misrepresenting actual aggregate crime rates), politicians

[11] Some authors like Von Hirsch (1985) would argue that reforms do not represent such a cynical approach, but instead take a more measure "proportional" structure designed to meet the crime and take on no real deterrence effect (or at least are not explicitly designed to be a deterrent). The authors advocating a Von Hirsch-type of view of reforms often refer to them as a proportional model (as opposed to the justice model).

advanced an individualistic view of crime as a function of societal deviants. A lack of effective sanctions was seen as part of the problem (Abramsky 2007; Simon 2007). Meanwhile, the media continually displayed images of crime, especially images of isolated victims of random violent crime, fueling public fears (Glassner 1999). In completing the interactive cycle, politicians then seized upon the fears to create platforms focused on *getting tough on crime*. This complex circle of interactions served to promote and sustain the politicization of crime characterized by the "politics of fear" that stressed "getting tough on crime," resulting in the model of punishment featuring among other things, "fixed" sentencing reforms (Beckett 1997; Simon 2007).

Furthermore, while it is true that crime increased in the late 1970s and 1980s, both the political response and the public perception of increasing crime far outstripped reality (Abramsky 2007). Thus, Scheingold (1991) suggests that researchers understanding of the political process of justice should begin with an understanding of the public *perceptions* of crime (not to be confused with actual crime rates which may or may not be related to perceptions). He argues that the public largely constructs its understanding of crime on the interpersonal level, focusing on the offender-victim paradigm characterized by a fear of victimization, especially of violent crime. To the general public, crime is seen as an issue of underlying offender pathology and the personal effect it has on their victim. Thus to the average citizen, the state's purpose is in *limiting* the actions of those who are likely to commit deviant acts and *punishing* those who have wronged others. To the average citizen, the sentence is both the loci of *punishment* and the loci of *protection,*

suggesting why there is such an intense public focus on sentencing (Beckett 1997; Jacobs and Carmichael 2001; Scheingold 1991)

These historical underpinnings provide some important contexts when considering why reforms were passed and what goals they may have had, but unless reforms are significant in altering the structure, shifts in imprisonment are not likely. For example, if judicial discretion is maintained, then reforms are unlikely to effect change. Savelsberg (1992) argues sentencing guidelines (and likely other sentencing reforms as well) are an ideal representation of Weber's formal rationality. A number of prior studies have employed the formal rationality theory, which creates a distinctive theoretical lens for understanding how sentencing policy has been "fixed" or, in other words, how the structure was altered to become more predetermined and allowing less discretion. The theory suggests sentencing reforms represent formal rationality, while indeterminate sentencing is more closely aligned with substantive rationality.

Both sentencing procedures are *rational* as defined by Weber (1978) because they are based in law and thus have "legalized" validity, but differ in their application (i.e. formal vs. substantive) of the laws. Substantive decision-making is subject to values, appeals to ethical norms, and historical precedent with less focus on uniformity of the outcomes. Formal decisions, on the other hand, focus on limiting subjective decision-making by stressing rationalized structural outcomes that remove personal values by emphasizing a technical orientation to procedural process grounded in well-defined criteria designed to elicit a specific outcome. "The judge . . . is more or less an automaton of paragraphs: the legal documents, together with the costs and fees, are

dropped in at the top with the expectation that the judgment will emerge at the bottom, together with more or less sound arguments - an apparatus, accordingly, whose functioning is by and large calculable or predictable (Weber 1978: 17)." In the end, social policies such as "fixed" sentencing reforms serve to bureaucratize the process, with the specific goal of removing subjective interpretations and outcomes (Engen and Gainey 2000; Zhang, Maxwell, and Vaughn 2009). Highlighting the need to place the structural changes within social contexts, formal rationalization is the link that brought the political rhetoric to fruition by codifying the sociopolitical goals of the law and order movement into structural-operational changes.

Under the formalization of the system driven by the law and order movement, the separation between politics and law (or at least operational practices) may have been blurred with considerably more power placed into the hands of policy makers (Doob 2000; Scheingold 1991). For example, with "fixed" sentencing, the judge no longer fits the sentence to the offender. Instead, the judge merely applies the laws passed by policy makers or their designee, *e.g.*, sentencing commissions.[12] The net result is a system more susceptible to social and cultural pressures that limits variability and possibly increases

[12] Several researchers point out that while the reforms removed discretion from the judge's hands, some discretion may remain in the process (Albonetti1997; Griset 1996; Kempf-Leonard and Sample 2001; Ulmer, Kurlychek and Kramer 2007). Instead, discretion is shifted to the hands of the prosecutor; inadvertently increasing their power by allowing them to choose a sentence category when charging a suspect. With this choice, they know if convicted, the type and length of sentence the suspect will serve. A few studies have found that prosecutors are more likely to consider factors outside the offender's crime and prior record in determining prosecution of violent and drug crimes, but are less likely for property and other non-violent felony crime (Engen et al. 2003). In the end this analysis is not concerned with the loci of the effect. Instead it is interested in the general effect of reforms on total change in state imprisonment over time and thus while researching discretion placement is important in general; it is not the focus of this analysis.

imprisonment (Boerner 2001; Tonry 1995). It should be noted that Weber's notion of formal rationality did not rule out an interaction of the political and legal spheres, but Beckett's and Sheingold's arguments about the influences of the law and order movement offers a historically relevant theoretical explanation of what caused the current intersection and suggested a tightening of the weave between the two spheres.

Hypotheses:

Given that "fixed" sentencing reforms represent a structural change born under the politics of fear associated with the law and order movement, it is possible the interaction served to increase imprisonment in states with the sentencing reforms at a greater rate than imprisonment in states with indeterminate sentencing. Though states not adopting "fixed" sentencing are also likely to be influenced by the sociopolitics of the time, "fixed" reforms are likely to increase imprisonment more by formalizing the sociopolitical environment into practice through their technical rational bureaucratic structure brought on by their formal rationalization. For example, by limiting the low end of the sentence range, the judge has restricted ability to select lenient sentences for at least some felons (Johnson, Ulmer, and Kramer 2008; Savelsberg 1992). Research of sentencing guidelines indicate, on average, judges comply 85% of the time, suggesting the separation of law and politics has been bridged and that the reforms (at least sentencing guidelines) do influence the way judges act (Marvell 1995; Scheingold 1991).

The Hypotheses are consistent with some previous research suggesting reforms will increase imprisonment. It should be noted, however, a number of studies argue against this conclusion (Dalessio and Stolzenberg 1995; Greenberg and West 2001;

Jacobs and Carmichael 2001; Marvell 1995; Smith 2004; Spelman 2009; Tonry and Hatlestad 1997). Additionally, this hypothesis is drawn from a theory that is likely to be specific to the U.S. justice system. As Von Hirsch (1985) has pointed out, while many western industrial societies also experienced significant prison growth, the U.S. maintains a unique justice system, both structurally and culturally, suggesting the effects are not generalizable to other countries.

Hypothesis 1 suggests that states with "front-end" sentencing reforms (distinctions between various reforms may be found in Appendix A) in the form of presumptive sentencing guidelines, voluntary guidelines, and statutory presumptive sentencing will increase imprisonment more than states that still utilize indeterminate sentencing. These three reforms are mutually exclusive with only one of the possible types of reforms being in operation at once in any given state.

> *Hypothesis 1: States implementing "fixed" sentencing reforms in the form of presumptive sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing had greater increases in rates of imprisonment than states not implementing them.*

These reforms are distinct from the other three reforms because their focus is on sentencing, while the other three reforms focus on release. To illustrate the difference, it is important to recognize that changes can come from one of three different avenues. First, imprisonment rates increase due to initial longer sentences. Second, rates increase because the system is sentencing more individuals to prison. Or third, felons serve longer terms because they are serving a longer portion of their original sentence, which is most likely due to the elimination or reduction in the use of early parole. In this analysis, point one, longer initial sentences is most likely the result of changes in sentencing on the

front-end, while point 3, serving longer portions of their original sentences, is likely the result of truth in sentencing laws, the abolishment of parole boards, or a combination of both (Abramsky 2007; Frase 2005; Reitz 1995; Tonry 1995). Therefore, to assess point 3; hypothesis 2 states:

> *Hypothesis 2: States adopting truth in sentencing laws or abolishing parole boards (determinate sentencing) had greater positive changes in rates of imprisonment than states not implementing them.*

It should be noted that abolishment of parole boards does not necessarily mean the abolishment of parole. In fact, states with "fixed" sentencing still utilize parole, but research indicates that as discretionary parole boards are removed, with early release limited to good behavior and a few other limited programs, imprisonment rates increased[13] (Stemen, Rengifo, and Wilson 2006). Furthermore, the "get tough on crime" climate along with increased budgetary limitation are likely to lead to other mechanisms of early release (*e.g.*, job training programs, GED, etc.) being curtailed or eliminated (Brewer, Beckett, and Holt 1981; Frase 2005; Stemen, Rengifo, and Wilson 2006).

While the five reforms in the previous two hypotheses deal with either a wholesale restructuring of the sentencing on the front end (presumptive sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing) or in the back end with the limiting or abolishment of parole (truth in sentencing and determinate sentencing), three-strikes laws are unique because they focus on a subset of offenders – the habitual offender – and not the system as a whole. Like the previous

[13] Of note, a number of studies have found evidence that truth in sentencing or determinate sentencing had no significant effect or actually reduced imprisonment (Mauer 2002b; Nicholson-Crotty 2004; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009).

reforms, three strikes laws are hypothesized to increase imprisonment (Lotke, Colburn, and Schiraldi 2004).

Hypothesis 3: States adopting three strikes laws will have greater increases in the rates of imprisonment than states that have not adopted them.

While hypotheses 1, 2, and 3 deal with general effects of reforms on changes in total imprisonment rates within states and assess two (longer initial sentences and longer portion of their sentence) of the three areas by which reforms can have an effect, Steffenmeier and Demuth (2000) argue that reforms can also lead to a *widening of the net* (*e.g.,* the third point previously discussed, reforms are bringing in individuals who would otherwise not have gone to prison) in three ways. First, some convicted criminals of relatively minor crimes may not have gone to prison under indeterminate sentencing. Generally convicts only go to prison if they are sentenced to more than one year; thus, it is plausible that some offenders would have received a sentence of less than a year under indeterminate sentencing, but were sentenced to more than a year under "fixed" sentencing. This change would directly increase new commitments to prison. Second, under sentencing reforms more criminals were sentenced to prison time rather than probation, resulting in a "widening of the net" of those who go to prison as new commitments. Third, direct increase in parole violators being returned to prison due to reforms could result from released prisoners being placed under supervised parole, rather than unconditional released or other more passive forms of parole. To assess the possibility that reforms both affect time-served and possibly widen the net, hypotheses 4 and 5 state:

Hypothesis 4: States implementing "fixed" sentencing reforms will have greater increases in time-served than states that have not adopted "fixed" sentencing reforms.

Hypothesis 5: States implementing "fixed" sentencing reforms will have greater increases in commitments than states that have not adopted "fixed" sentencing reforms.

Data

Before 1972, imprisonment rates remained relatively stable over time, but since that point, steady and rapid increases have occurred. The analysis begins in 1967 and covers each year to 2007[14]. The study analyzes state-level imprisonment data for all 50[15] states, allowing for assessment of the impacts of sentencing reforms on changes in imprisonment rates over time across 50 states. With 50 states and 41 years of observations, we have a total of 2050 observations. This "pooling" of the time-series and cross-sectional data greatly improves the statistical power of the models (Hsiao 2003; Wooldridge 1997).

In this analysis, presumptive sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing are considered independent and mutually exclusive "front-end" "fixed" sentencing reforms and may not, at any given time, coexist with each other or indeterminate sentencing. Conversely, the "back-end" limiting (truth in sentencing) or elimination (determinate sentencing) of discretionary

[14] Data actually covers the years 1965 to 2008, but the moving average calculated to alleviate the issues of spikes in changes in imprisonment truncated the data by one year at the beginning and the end. Furthermore the lagged variables further reduced the measured effects at the beginning of the data reducing the actual measured years to 72 to 07.

[15] Washington DC is often included in analysis of this type (treated as a "fifty-first state"), but was excluded because data for the entire time period in question was not complete. In addition to incomplete data, Washington DC stopped housing its own prisoners in 2001.

release is considered separate because their focus on release allows operation alongside the "front-end" reforms. Furthermore, the unique and independent nature of three strikes laws also allows them to be considered alongside both the front-end and back-end reforms. While states adopting reforms often adopted numerous types (*e.g.,* Oregon which adopted presumptive sentencing guidelines in 1989, abolished parole in 1989, and adopted truth in sentencing in 1995), their coexistence is not absolute and were often instituted in different years (See Table 1.1 on Page 7 for complete list of reforms) (Frase 2005; Stemen, Rengifo, and Wilson 2006).

Table 2.1 outlines the six sentencing reform variables. They were compiled from numerous sources including the Bureau of Justice Assistance (1996), Frase (2005), Zhang, Maxwell, and Vaughn (2009), and a report by the Vera institute (Stemen, Rengifo, and Wilson 2006). While most research is in agreement on the general goals of the reforms, considerable variation in the design and application of the reforms exists and there is little consensus about how to classify them into appropriate groups (Frase 2005; Marvell 1995; Stemen, Rengifo, and Wilson 2006; Tonry 1995). Appendix A provides a detailed discussion of the general function of the various reforms and how they are delineated in this study.

Table 2.1. The Six Reforms Included in Analysis

Reform	Measured	Description
Presumptive Sentencing Guidelines[1]	Logged Growth Curve	Consist of a matrix of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) and offense severity. Judges generally must follow the matrix. They are strict in their application; only allowing very limited deviations.
Voluntary Sentencing Guidelines[1]	Logged Growth Curve	Treats guidelines as formal recommendation, but does not *legally mandate* they be followed by the judge.
Statutory Presumptive Sentencing[1]	Logged Growth Curve	Represents an attempt to create uniformity within similarly situated crimes, but acts less like a sentencing rubric. It specifies an appropriate or "normal" sentence for each offense as a baseline for a judge.
Determinate Sentencing[2]	Logged Growth Curve	Refers to a system *without* discretionary parole boards.
Truth in Sentencing[2]	Logged Growth Curve	Requires offenders serve a statutorily defined minimum amount of time. Only states meeting the 1994 federal omnibus crime bill minimum of 85% time-served of original sentence are considered.
Three Strikes Laws[3]	Logged Growth Curve	A habitual offender law focused on three-time felony offenders. Generally the law suggests a severe sentence (25 to life or life) for a third felony offense.

[1] Front-End Reforms [2] Back-End Reforms [3] Sentencing Enhancement

There are three key points about the reforms that should be noted. First, all six reforms represent a shift from the indeterminate-rehabilitation model to a punitive model with a discretion-limiting sentencing structure. Second, because the dependent variable does not include county or local jail populations, only individuals sentenced to more than one year are considered, suggesting a one-year lag (impact one year after inception). Third, while in the year following the reform there will be an instantaneous effect on some portion of the imprisonment rate, not all individuals in any given year will be "caught up" by the reform. Some of the rates (the race specific rates used to construct the odds ratios) will be individuals who were already imprisoned and sentenced under the old sentencing procedure. Over time, a greater proportion of prisoners will be "caught up"

by the reform. At some point a plateau at nearly full effect will occur and only a small portion of the rate will be individuals who were sentenced to long terms under indeterminate sentencing. To address both the lagged effect and capture the expected "logged growth curve" like effect, a new measure of the reform's impact on total imprisonment rates was developed. Using a logarithmic measure to model the effect of sentencing reforms represents an important divergence of this study from previous analyses, which relied heavily on dummy variables.[16]

To create the new sentencing measures, each state was coded as 1.00 the first year of implementation and as *e* (2.71828) for year five through the last year of observation. The intervening years between one and five were set at an equal distance between 1.00 and 2.71828. Finally, the absolute log of each score was taken. This procedure created a "logarithmic growth variable" with a one-year lag (the log of 1 is 0) that suggests full effect at five years. The measure represents an effect that approaches 1 (the full effect) at an increasingly diminished rate (increases fast at first and then slows). States with indeterminate sentencing were coded to zero and represent the reference group and, of course, the log was not taken for those years before adoption of the sentencing reform.

The dependent variable, total prison population per 100,000, is measured on the state level and was observed over time for each state. Data for the dependent variable and two of the independent variables, parole violators and new commitments to prison per 100,000, were obtained from the Bureau of Justice Statistics (U.S. 1965-1983; U.S.

[16] Coding the reforms as dummy variables (zero before adoption and 1 after adoption) suggests imprisonment rates would be affected 100% by the new policy in the first year it was adopted. A logarithmic measure represents a more appropriate theoretically expected effect and should lead to more robust results.

1984-1998; U.S. 1999-2008)[17]. The FBI's Uniform Crime Report (UCR) (1965-2008b)[18] supplied the data on violent crime[19] and drug crime arrests per 100,000.[20] The crime variables will be lagged by one year (See Appendix E for descriptive statistics of variables used in this analysis).

It is important to note that the variables represent a theory that crime rates will have *direct* effects on changes in state prison populations. Simply put, states with more crime will have larger increases in imprisonment over time. This theory necessitates that the crime variables be lagged, as crime rates are not likely to have an "instantaneous" effect on imprisonment as those arrested will undoubtedly take time to be processed through the system. The models could have included un-lagged crime controls, but this would suggest crime had a more "simultaneous" effect within the same year. While possible, for example, if one theorized that higher crime creates social pressure to "do something about it," the former theory is tested in this analysis.

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[17] Data for the years of 1972 to 1983 is available in the yearly publication: Prisoners in State and Federal Institutions on December 31, 1972 (through 1983). Data for the years of 1984 to 1998 is available in the yearly publication: Correctional Populations in the United States, 1984 (through 1998). Data for the years of 1999 to 2008 is available in the yearly publication: Prisoners, 1999 (through 2008). All three publications are produced and printed by the U.S. Bureau of Justice Statistics (see Citations). Rates per 100,000 for each variable was computed by taking the raw number of prisoners for each state by year that was provided by the Bureau of Justice Statistics and dividing by state population per 100,000 as supplied by the Bureau of the Census (see citation).

[18] Data from the Uniform Crime Report (UCR) is available in print and online from the U.S. Dept. of Justice, Federal Bureau of Investigation and is produced and printed yearly (see Citations).

[19] Violent crime arrests represent the Uniform Crime Reports indexed crimes and include the offenses of murder, forcible rape, robbery, and aggravated assault.

[20] In this project "*violent crime*" and "*drug crime*" rates refers to violent crime and drug crime *arrests rates* recorded by the Uniform Crime Reports for each year (see Citations).

The U.S. Census (U.S. 1965-69, 1971-1979, 1981-1989, 1991-1999, 2001-2008; U.S. 1970, 1980, 1990, 2000)[21] supplied the data for percent black, percent Hispanic, percent urban, and population density and also supplied total population for each state which served as the denominator to construct all rate variables. The U.S. Bureau of Labor Statistics (1965-2008a) supplied information to construct the data for unemployment rates, while the U.S. Bureau of the Census (2006) provided the data for poverty rates.

Dubin (2007) supplied the needed data for political party affiliation of each state senate and house (or assembly).[22] The dummy code for the governor's political party was obtained from Hershey (2007). The political variables were also lagged in this analysis, suggesting that it takes two years for political control to manifest in the operations of these states. Research indicates the controls included in this study represent key areas of association with prison population, warranting their inclusion in this analysis (Albonetti 1997; Arvanites and Asher 1998; DeFina and Arvanites 2002; Engen and Gainey 2000; Steffensmeier and Demuth 2000; Western 2001; Western 2005; Zhang, Maxwell, and Vaughn 2009).

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[21] Data from the census is available online at census.gov maintained by the U.S. Bureau of the Census and was compiled from both census data and population estimates (see Citations).

[22] State house and senate variables were constructed by centering the percent Republican around 50% so that Republican control represents positive deviations from 50%, while Democratic control represents negative deviations. The absolute value of the deviations were then logged, with the negative sign return for the Democratic control, to create a logarithmic scale with positive and negative deviation from zero to represent the diminishing returns of political party concentrations.

To facilitate the testing of a possible "net-widening" effect, a *general* "time-served"[23] measure was modeled by including controls for prison entries. The measure *deduces* "time served" by isolating the unknown variable (time-served) by controlling for known contributing variables (new commitments and parole violators returned). Furthermore, *general* net-widening can also be deduced by looking for significant reductions in coefficients for the reforms when commitments to prison are added as covariates.

While not ideal, the measure does allow for rudimentary proxy testing of both of the main loci of reforms: time served and "net-widening." This is made possible because the dependent variables as a change in rates of imprisonment per 100,000 can be broken down into three components: (1) new individuals entering the system since the last count, plus (2) those who are still in the system from before the last count, minus (3) those who have left the system since last count, which when combined produces each year by state count. Taking this into consideration, new commitments and parole violators returned should have a significant positive impact on incarceration rates and their inclusion creates a more stringent test of sentencing reforms as a quasi-measure of "time served."[24]

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[23] Ideally the study would include time-served as a direct measure, but unfortunately this data is not available on the aggregate state level across all 50 states for the time period under investigation. The "quasi-measure" is the best approximation considering the data available.

[24] The quasi-measure of time served represents the rate of those who stay in prison minus those who have left. This measure *approximates* "time-served" across all prisoners while not distinguishing the percentage of prisoners sentenced under specific sentencing types. The measure serves to assess the *general* "time-served" for a specific state by year data point by controlling for those entering the system, *deducing* change due to "time-served" by isolating the unknown variable by controlling for a known contributing variable.

Methods

A Time-Series Cross-Section (TSCS) regression technique employing panel corrected standard errors (PCSE) was used to analyze changes in state imprisonment rates due to the adoption of sentencing reforms. This method is a variant of ordinary least squared (OLS) regression and while OLS is not particularly useful in TSCS (England, Kilbourne, Farkas, and Dou 1988), it can be correctly implemented when used in conjunction with PCSE and corrections for autoregression that greatly improves the reliability of the standard errors produced by the models (Beck and Katz 1995; Halaby 2004; Hsiao 2003; Wooldridge 1995; Wooldridge 2000).[25] In the end, the models measure a shift in sentencing from the reference group of states still incorporating indeterminate sentencing to states with one or more sentencing reforms.

While a PCSE model corrects some problems associated with TSCS models, it does not solve all issues associated with complicated TSCS models. Omitted state level variables that are stable over time (omitted variable bias), but correlated with the error term for the dependent variable can make point estimates biased. Therefore, a unit fixed effect model (instead of the random effects model), which holds the unexplained time-invariant variation constant, controlling for the unobserved state level effects, was incorporated.[26] While the unit fixed-effects model is statistically necessary, it also sacrifices little in substantive interpretation because the primary concern of this analysis is *not* with panel-to-panel variation. Instead the interest lies in aggregate change over

[25] For a more detailed discussion of the statistical models and their validity see Appendix B.

[26] In this analysis, both the Hausman and Mundlak tests of omitted variable bias suggest a bias is present in the random effects models.

time within states. In fact the panel—specific fixed-effects approach leaves us with a model that measures exactly that-the change in the dependent variable within each panel where the coefficients give us the aggregate within-state change over time for the average within state change in the independent variable (England, Kilbourne, Farkas, and Dou 1988; Halaby 2004; Wooldridge 1997; Wooldridge 2005). Substantively, this is where we would expect to find the effect of reforms that are state (panel) specific.

After implementing panel-specific fixed-effects, the Dickey Fuller and Phillips-Perron unit-root tests suggested the presence of a unit-root. Furthermore, there are substantive reasons to believe that a previous year's imprisonment is highly associated with the current year's level except for a new shock, given that the dependent variable is the total imprisonment rate. Because of the presence of a unit root we first differenced the dependent variable. Because theory suggests it is desirable to preserve the measure of change over time, I decided *not* to include fixed-effects for years[27] (*e.g.,* period or time effects) and instead use a series of common time-series procedures proven effective at de-trending data, addressing issues of random walks, and rectifying time specific shocks (Allison 2009; Finkel 1995; Halaby 2004).[28]

It is important to note that once fixed-effects for units is implemented the analysis no longer represents a pooling of the state (panel) by year (time) observations and now

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[27] Additionally, each state has almost complete autonomy over their imprisonment rates and its changes will be a reflection of change over time (within panels), thus "explaining" the uncontrolled variation over time may result in a misspecification of state specific effects (Halaby 2004).

[28] While fixed-effects for time is on the list of appropriate tools correcting for most time variant issues (including serial correlation), unfortunately they change the interpretation of the coefficients from change over time by controlling for unexplained change over time that is stable over the panels.

becomes a series of simultaneous time-series analyses that measure the aggregate (averaged across panels) change over-time within panels (Frees 2004; Hsiao 2003; Spelman 2009). As noted, between 1967 and 2007 imprisonment trended up almost 550%. This created the common time-series specification problem where reforms would likely increase total imprisonment simply because they are clustered on the more recent end of the time-series where the rate of imprisonment is the highest. This is a specification error that must be corrected for, thus the first-difference transformation was applied, changing the dependent variable to year-to-year change (sometimes referred to as the unconditional change score) (Finkel 1995; Halaby 2004). Even in a change scores analysis a shock or spike in the series can undermine the model's ability to measure change over time effectively. To remedy the apparent shocks observed, a three-year moving average was instituted (Frees 2004). Unlike the unchanged score model (total imprisonment), the change score model (first differenced) no longer fails the Dickey Fuller or Phillips-Perron unit-root test.[29]

Statistically, change scores are not without their limitations. A phenomenon known as regression to the mean suggests that the unconditional change score model can lead to biased results because the explanatory variables are related to the initial values of the dependent variables. To correct for this, a lagged term of imprisonment rate was included as a control variable. This model is often referred to as the static-score or conditional change-score model and "frames the analysis in the following fashion: do the independent X variables [both X or ΔX] influence changes in Y for fixed levels of Y_{t-1}, that

[29] This is true for both the first differenced and moving averaged variables.

is, taking into account the negative effect of initial values of Y on subsequent change?" (Finkel 1995: 9). In effect, this analysis can be viewed as 50 simultaneous first-differenced time–series regressions analyzed with aggregated estimates that produce coefficients representing the average effect of reforms on the average state (Finkel 1995; Frees 2004).

Results

Table 2.2 presents the results for four models testing the five hypotheses in this analysis. The models measure the increase or decrease in the change in the average imprisonment rate due to the move from indeterminate sentencing (the reference group) to one or more of the sentencing reforms. Model 1 serves as the base model, analyzing the six sentencing reforms and first-level interaction terms (as noted most states adopt more than one reform suggesting an interaction effect is likely). Keeping with convention, insignificant interaction coefficients were left in the models because significant interactions were expected (Jaccard and Turrisi 2003). The model indicates just one of the front end reforms, statutory presumptive sentencing and just one of the back-end reforms, determinate sentencing are significantly associated with imprisonment rates. Both suggest a conditional main effect that increases the change in imprisonment rates by 15.297[30] and 15.090 per 100,000 respectively. This suggests that when these two reforms are fully implemented they increase the rate of change by roughly 55% over indeterminate sentencing (the rate of change for indeterminate sentencing states is, on

[30] The average year-to-year changes for sentencing reforms were calculated by setting the control variables (when applicable) to their means and then a reform to 1 (indicating full effect at 5 years). This provides the average effect of the reform for the average state. For the interaction of more than one reform, the conditional main effects and the interaction terms are all set to 1.

average, 9.650). When statutory presumptive sentencing is paired with determinate sentencing and treated as interacting with each other, the interaction term, serving as a mitigating effect, actually reduces the combined conditional main effect resulting in a 17.702 increase. But it should be noted that this combination of reforms is only found in Arizona, suggesting Arizona is an outlier from the other states adopting statutory presumptive sentencing. The base model explains roughly 40% of the variation.

Table 2.2. Conditional Main Effects of Sentencing Reform and Their First Level Interactions.

Variables	**Total Imprisonment**		**"Time-Served"**	
	Model 1	Model 2	Model 3	Model 4
Reforms				
Pres. Sent. Guidelines	1.636	1.359	1.536	1.374
	(1.170)	(1.392)	(1.177)	(1.444)
Voluntary Sent. Guidelines	0.0514	-1.677**	-0.141	-1.864**
	(0.929)	(0.704)	(0.921)	(0.731)
Statutory Presumptive Sent.	5.647***	3.642***	4.814***	3.033***
	(1.113)	(1.029)	(1.156)	(1.132)
Determinate Sentencing	5.440***	2.675***	4.980***	2.330**
	(1.303)	(0.962)	(1.377)	(1.048)
Truth in Sentencing	-0.745	0.611	-0.717	0.831
	(0.894)	(0.774)	(0.960)	(0.861)
Three Strikes	1.119	0.411	1.089	0.512
	(1.002)	(0.920)	(1.043)	(0.912)
Interaction Terms				
Pres. Guide * Determ. Sent.	-6.281***	-6.603***	-6.303***	-6.805***
	(1.630)	(1.686)	(1.706)	(1.870)
Pres. Guide * Truth in Sent.	2.199**	2.655***	2.163*	2.693**
	(1.096)	(1.017)	(1.171)	(1.173)
Pres. Guide * Three Strikes	-3.207**	-3.890***	-3.191**	-3.775***
	(1.281)	(1.228)	(1.343)	(1.353)
Vol. Guide * Determ. Sent.	-6.819***	-6.791***	-6.377***	-6.769***
	(2.231)	(1.907)	(2.433)	(1.992)
Vol. Guide * Truth in Sent.	-0.267	1.566	-0.503	1.391
	(1.246)	(0.956)	(1.332)	(1.116)
Vol. Guide * Three Strikes	0.566	1.581**	0.459	1.711**
	(0.872)	(0.775)	(0.882)	(0.809)
Stat. Pres. * Determ. Sent.	-3.035	-1.554	-0.812	0.784
	(2.424)	(1.765)	(2.942)	(2.198)
Stat. Pres. * Truth in Sent.	-2.932*	-2.589	-4.491**	-4.321**
	(1.607)	(1.581)	(1.953)	(1.770)
Stat. Pres. * Three Strikes	-1.425	-0.761	-1.553	-0.865
	(1.013)	(1.183)	(1.006)	(1.268)
Det. Sent. * Truth in Sent.	-2.348**	-1.827	-2.303*	-1.791
	(1.166)	(1.177)	(1.226)	(1.332)
Det. Sent. * Three Strikes	2.850*	3.898**	3.288*	4.145**
	(1.655)	(1.522)	(1.733)	(1.708)
Truth * Three Strikes	-0.644	-1.045	-0.267	-0.72
	(1.135)	(0.818)	(1.223)	(0.963)
Observations	2024	2024	1924	1924
R-squared	0.398	0.493	0.418	0.508
Lagged Dependent Variable	-0.0077***	-0.020***	-0.014***	-0.025***
(For Reference Only)	(0.003)	(0.002)	(0.003)	(0.003)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Models present analysis incorporating Prais-Winsten Regression with Panel Corrected Standard Errors of imprisonment rates per 100,000 state population for the years 1967 to 2007 with a lagged dependent variable term and fixed-effects for states.

Models 2 adds crime, demographic, and political control variables (See Table 2.3) to the reform variables in model 1 increasing the explained variation by about 10%. Historical and contemporary research has shown high rates of violent crime increase state imprisonment (Irwin and Austin 1997; Mauer 2002a; Raphael 2009; Steffensmeier and Demuth 2000). The effect of violent crime is particularly strong in the South where the rates of violent crime and imprisonment are generally higher, likely the result of sociohistorical factors creating a subculture of violence particular to the South (Clarke 1998; Crow and Gertz 2008; Karnig and Mcclain 1985). Drug crimes on the other hand are essential to understanding the effect of reforms. The war on drugs was a key component of the law and order movement and the criminal justice system increased criminalization of various drug crimes, especially possession. As resulted, one fifth of the prison population has been convicted of a drug crime, a 20-fold increase from 1980 (Baum 1997; Beckett, Nyrop, Pfingst, and Bowen 2005; McDonald and Carlson 1994; McShane and Williams 1997; Western 2001). As expected, the lagged crime variables were significantly related to increases in imprisonment, suggesting increases in crime do increase the change in imprisonment within states.

Three of the demographic variables, including percent urban, percent unemployment, and percent Hispanic significantly increased the change in imprisonment, while percent poor of a state significantly reduced imprisonment. Finally, in Model 2, two of the political variables, Republican state senate control and Republican governor significantly increased changes in imprisonment. This suggests that while it is likely that the social pressure to get tough on crime cuts across political parties at least for these two

institutions, states with Republican control have larger average changes in imprisonment over time.

Table 2.3. Results from Control Variables Incorporated in Models from Table 2.2.

Variables	**Total Imprisonment**		**"Time-Served"**	
	Model 1	Model 2	Model 3	Model 4
Crime Controls				
Violent Crime (Lagged)	-	0.964***	-	.855***
	-	(0.001)	-	(0.002)
Drug Crime (Lagged)	-	5.11***	-	4.24***
	-	(0.001)	-	(0.000)
Demographic Controls				
Percent Black	-	0.0791	-	0.0857
	-	(0.076)	-	(0.077)
Percent Hispanic	-	0.196***	-	0.220***
	-	(0.061)	-	(0.066)
Percent Unemployment	-	0.381***	-	0.426***
	-	(0.113)	-	(0.116)
Percent Poor	-	-0.127*	-	-0.115*
	-	(0.066)	-	(0.069)
Population Density	-	0.00393	-	0.00689
	-	(0.007)	-	(0.007)
Percent Urban	-	0.0467***	-	0.0454***
	-	(0.007)	-	(0.007)
Political Controls				
Rep. St. House (Lagged)	-	0.0771	-	0.0916
(Two Year Lag)	-	(0.139)	-	(0.146)
Rep. St. Senate (Lagged)	-	0.199*	-	0.230*
(Two Year Lag)	-	(0.120)	-	(0.119)
Rep. Governor (Lagged)	-	1.058***	-	1.206***
	-	(0.231)	-	(0.239)
"Time-Served" Variables				
New Commitments	-	-	0.0204***	0.0183***
	-	-	(0.006)	(0.006)
Parole Violators Returned	-	-	0.0174**	0.0123*
	-	-	(0.008)	(0.007)
Observations	2024	2024	1924	1924
R-squared	0.398	0.493	0.418	0.508
Lagged Dependent Var.	-0.0077***	-0.020***	-0.014***	-0.025***
(For Reference Only)	(0.003)	(0.002)	(0.003)	(0.003)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Notes: Models present analysis incorporating Prais-Winsten Regression with Panel Corrected Standard Errors of imprisonment rates per 100,000 state population for the years 1967 to 2007 with a lagged dependent variable term and fixed-effects for states. This table presents the control variables incorporated in Table 2.2.

Model 2 in Table 2.2 outlines the effects of sentencing reforms on changes in imprisonment rates controlling for state-level influences listed in Model 2 of Table 2.3. The results are similar to those found in Model 1, but with three notable changes. First, in Model 2, voluntary sentencing guidelines now significantly *reduce* changes in imprisonment. This marks the only reform (the conditional main effect) to significantly reduce imprisonment changes. This suggests that while Hypothesis 1 is supported for statutory presumptive sentencing, it is not supported for voluntary sentencing guidelines. Additionally, two changes in the interaction terms are present. First, the interaction between determinate sentencing and truth in sentencing, which was significant, no longer significantly reduces changes in imprisonment. Second, an interaction between voluntary guidelines and three strikes is now significant, suggesting that while voluntary guidelines on their own reduce imprisonment changes, when taken into combination with three strikes laws they actually increase changes in imprisonment to 9.965 over the average change in indeterminate sentencing of 9.650 (a 3.26% increase). It is important to note that no states adopted voluntary sentencing guidelines by themselves and were always accompanied by an additional reform. For example, in the case of three strikes laws, Arkansas, Louisiana, and Wyoming all adopted voluntary guidelines and then chose to add three strikes.

To illustrate the average effect of sentencing reforms on aggregate changes in imprisonment over-time for states adopting the six reforms singularly or in combination, Table 2.4 outlines the predicted scores for the combinations of reforms. The predicted change scores represent the *average* change in imprisonment rates for the *average* state

adopting some combination of the reforms. A combination in which at least one of the reforms was significant was flagged. In some of the cases the reform (*e.g.,* voluntary sentencing guidelines) or some combinations of the reforms (*e.g.,* presumptive sentencing guidelines in combination with truth in sentencing and determinate sentencing) reduced the changes in imprisonment, but these cases were rarer. For the majority of the cases the reforms increased changes in imprisonment, both on the front-end and on the back-end, lending some support to hypothesis 1 (sentencing guidelines and statutory presumptive will increase changes in imprisonment on the front-end) and 2 (determinate sentencing and truth in sentencing will increase changes in sentencing on the back-end). Three strikes on the other hand did not significantly increase imprisonment changes on their own, but did when taken in combination with other reforms. The analysis of three strikes laws suggests support, but with caveats, for hypothesis 3 (three strikes laws will increase changes in imprisonment).

Table 2.4. Estimated Aggregate Change Over Time of Reforms by Themselves or in Combination with Other Reforms.

		Truth in Sentencing	Abolishment of Parole	Three Strikes	Truth in Sentencing & Abolishment of Parole	Truth in Sentencing & Three Strikes	Truth in Semt., Abolishment of Parole, and Three Strikes
Presumptive Sent. Guidelines	11.009	14.275	-	-	*8.52**	9.751	*7.894**
Voluntary Sent. Guidelines	*7.973**	10.15*	-	9.965*	*4.207**	11.097*	-
Statutory Presumptive Sent.	13.292*	-	-	12.942*	10.608*	9.919*	-
Truth in Sentencing	10.261	-	11.109*	9.627	-	-	-
Abolishment of Parole	12.325*	11.109*	-	-	-	14.373*	-
Three Strikes	10.061	9.627	-	-	14.373*	-	-
Indeterminate Sentencing	**9.650**	-	-	-	-	-	-
	Mean	St Dev.	Max	Min			
Average Imprisonment Rate Per 100,000	240.813	163.941	870	20			
Average Change in Imprisonment Rate	9.118	13.463	50.880	-19.727			

Note: The coefficients represent the average (of the aggregate state) change over time in imprisonment rate per 100,000 state populations controlling for the variables presented in Model 2 of Table 2.2. This table only presents combinations that existed in the data set. Combinations not observed are indicated with a "-" symbol. All other control variables were set to their means for calculation. Intercepts for the unit fixed-effects were not included in this table. Indeterminate sentencing was calculated by setting each of the reform variables to zero. Imprisonment rate descriptives give a base by which to compare the calculated coefficients. * Represents reform combinations where at least one of the reforms in the calculation were significant. Italicized reforms indicate reforms or combinations in which change in sentencing is reduced.

Models 3 and 4 of Table 2.2, on the other hand, presents the same analysis as Model 1 and 2, but includes controls for prison entries in the form of new commitments and parole violators returned. The Models' (3 and 4) purpose is to assess the effects on time served vs. total imprisonment (facilitating testing of hypotheses 4 and 5). Including

controls for new commitments and parole violators returned is designed to turn the dependent variable into a "quasi-measure" of time served. While not the ideal measure of time-served, the analysis indicates that while the coefficients have been reduced (albeit, not significantly), statutory presumptive sentencing and three strike laws remained significant as well as voluntary sentencing guidelines in the full model (Model 2), suggesting an increase in time-served (or "quasi-time-served"). A reduction in the coefficients may have also suggested an effect on the number of individuals entering the system.

To illustrate, if guidelines had both an effect on those entering the prison system (net widening) and sentencing lengths, it is logical to conclude that the effects of guidelines would be reduced, but remain significant when controls for new commitments to prison were introduced. A comparison of the related coefficients in the matching models (1 and 2) indicates that this argument is not supported by statistically significant reductions.[31] These results should be taken with caution as the "quasi-measure" of 'time served," is only a general proxy at best. These reservations notwithstanding, the results lend support for hypotheses 4, but not for hypothesis 5, suggesting overall reforms have a significant effect on how long individuals spend in prison (time-served), but do not significantly affect the number of person going to prison (net-widening). This of course does not suggest that net-widening, or the concerted effort to send more individuals to

[31] Significant difference between coefficients was determined by building confidence intervals around the two coefficients in question at $p<.05$.

prison did not occur during this time, only that reforms were not significantly associated with an even larger widening of the net.

The central finding in all the models of this investigation is the "fixed" sentencing reforms significantly impact changes in imprisonment rates, indicating they led to larger aggregate changes in rates of incarceration over time under some conditions, while reducing them in other combinations. While Boerner and Lieb (2001) make note of a few states that had not seen specific direct prison growth, this analysis suggests, on average, across all states, some "fixed" sentencing reforms are associated with changes in imprisonment rates. Furthermore, support for the argument that reforms will increase lengths of sentences is found while a net-widening effect is not suggested.

Discussion

In general, this analysis has some significant advantages over previous research. First, the analysis includes a long time period of historically relevant data that is current to 2008 (analysis limited to 2007 due to the moving average). The "imprisonment binge" began roughly in the early 1970's with 1972 often cited as the "start date." With data beginning in 1965, the analysis can provide *every* year from 1967 (limited due to the lagged terms and moving average) covering the effects more accurately than analysis limited to 3 or 5-year gaps that has been previously used. Second, this analysis includes almost all sentencing reforms of the period, a marked advantage over previous research. Previous research has generally assessed one reform at a time (with notable exceptions) limiting the analysis to that exact reform. As seen in this analysis, considerable change in the effects of reforms can be observed when different combinations of the reforms are

considered and failure to account for this may have mis-specified these effects in previous research. Third, the analysis includes both cross-sectional and time-series data giving it marked advantages over analysis that lacked one of these dimensions. Fourth, the modeling technique employed takes a marked step away from previous research and supplies a unique, yet arguably better, analysis, including the incorporation of a conditional change score, fixed-effects for states, and the logged growth curve variable. While the results do not allow for the complete state-by-state analysis (*e.g.,* we can not definitely calculate the change over time for Oregon vs. Washington), the aggregate change scores do have substantive interpretation, outlining the average change over time within states. Furthermore, while the base imprisonment rate often differed from state-to-state (states had different rates of imprisonment in 1967 and thus they had different intercepts), their individual growth rate (change over time) was more similar.

Table 2.4 may be the best way to illustrate the complete interaction of the reforms. While this table is useful for understanding the average effects, the estimates should be viewed with a few qualifications. First, this analysis represents the average or aggregate change in imprisonment within states over time. While not useful in all situations, it is useful for those seeking to draw large generalizable conclusions. Some previous researchers, however, have argued that such analysis is undermined by the variation in reforms from state-to-state, both in what they adopted and in actual functional practice. For example, not all presumptive sentencing guidelines are the same with some having tight ranges and some having wide ranges. This variation from policy-to-policy alone is not enough to discount the results. After all, variation is inherent in

measurement and almost never will two policies enacted by different states be exactly the same. Typically such measurement error reduces the likelihood of finding significant relationships. Policy variation may make the models less efficient, meaning larger standard errors, they do not make them less accurate—as the point estimates are still consistent. That being said, this analysis does not allow us to conclude that *all* states or even *one* particular state observed the effects, it does on the other hand allow us to conclude that, on the *aggregate* or on average, the reform is (or is not) significantly associated with larger changes in imprisonment within states (Finkel 1995). Therefore, the coefficients represent the *average* treatment effect across states. Finally, while it might seem logical to advocate for more delineation among the six reform variables, such a move is not without its statistical costs. Disaggregating the variables into smaller and smaller groups reduces the statistical power of the analysis and the "pooling" of states and their policy changes over time succumbs to an increased "slicing" of the data. This is the delicate balance of variable specification and statistical power (Wooldridge 2000). In this case the results presented in Table 2.4 indicate reforms do impact changes in sentencing rates.

The analysis points to broad causal connections in support of the general theoretical arguments of Beckett (1997), Scheingold (1991), and Simon (2007). They suggest the law and order movement of the mid 1970's to today was, in part, driving the shift to the "justice model" of imprisonment including "fixed" sentencing. The reforms served to change the process, through the bureaucratization of the system as described by Weber and others, from a system giving significant leeway to judges and in house prison

officials to a system largely placing power into hands of elected political officials: ultimately creating a system that is susceptible to political and social pressures. The final result is a general increase in punitive incarceration in which "fixed" sentencing represents a way to *codify* the punitiveness. These increases are likely regardless of the stated political purposes or goals of the reforms (*e.g.,* desire to be growth neutral). Adding to the body of research in the field, but also suggesting a number of important considerations for future research, this analysis outlines important broad causal impacts of reforms.

Because many of these reforms were created in the "justice model" framework, they are likely to have many consequences (*e.g.,* rapid prison growth). This analysis gives an important general picture of the structural frameworks of the reforms and allows for the beginnings of an incremental assessment of a number of more specific policy and theoretical implications (*e.g.,* the growth in the racial and ethnic disparities in imprisonment). This important first step has been largely overlooked or mis-specified in the current literature. While some research has attempted to address reforms with a panel model, this study presents results that should raise questions with past studies.

For instance, Stemen and colleagues (2006) study of the same reforms over a similar time period suggests "front-end" sentencing reforms are not driving additional prison growth, but that the "back-end" reform of determinate sentencing has helped to drive this growth. While their study was long overdue, it also has some limitations. First, it includes time intervals of every three years, which may limit the results by leaving out important shifts in imprisonment between observations. Second, the results

include dummy variables for shifts in sentencing types; these variables generally misrepresent the "growth like" effects of the reforms. Third, they use trending imprisonment rates to analyze their data. This study took a different approach and, in addition to some other methodological advantages, includes fixed effects for panels, a logarithmic growth curve to represent reforms, and conditional change scores, which better represents the true nexus of the reform effects. Stemen et al.'s (2006) findings that determinate sentencing is the chief reform driving higher rates of imprisonment runs counter to the results of this investigation. It is likely the difference in finding is related to the differences in model specifications.

The results presented here have useful policy and theoretical implications; an important area for future research is the assessment of key contexts and social structures that are likely to influence the adoption and implementation of these reforms. The current project suggests general arguments for the particular theoretical mechanisms at work, but more direct assessment is needed. In general, the effects of guidelines were consistent across models (3 and 4) that included controls for prison entries and across models that did not (1 and 2), though the declines in the coefficients representing these guidelines were not statistically significant. This suggested that reforms may have affected *time-served*, but did not *net-widening*.

After controlling for violent crime rates, drug crime rates, percent black, percent Hispanic, percent urban, population density, poverty rates, unemployment rates, log curves for state senate and state house, state governor, new commitments to prison, and parole violators returned to prison; the three front end "fixed" sentencing reforms remain

positively associated with imprisonment rates and directly related to a portion of the nearly 600% increase in imprisonment over the length of this study. Legislators should consider this when designing sentencing procedures. The costs of locking criminals up is increasingly expensive and "prison beds" are currently at critical levels in a number of states (Jacobs and Carmichael 2001; Shane-DuBow 1998). Rapid increases in prison populations can be devastating to a system that is often slow to respond (Griset 1995; Kruttschnitt 2005).

The overall costs to individual states can be high. For example, Oregon, a state that has implemented sentencing guidelines, spent $23,389 per inmate in 2003. When the logarithmic growth curve variable is calculated out for seven years, the effect is about a 1-prisoner increase per 1000 for that year, indicating that guidelines have a relatively large effect (considering the overall imprisonment rates are about 4 inmates per 1000). The Oregon guidelines could cost the state 81.84 million dollars a year (based on 2003's total state population).

The costs are likely to be even higher considering that these estimates do not include capital investments such as construction costs of new prisons and increased transportation. Additionally, increases in imprisonment places resource pressures on public defenders and courts. While considerable variation from state to state exists, the average cost per inmate across all states in 2003 was $22,650 suggesting costs are likely to be high in all states (Boerner 2001). The problem is exacerbated by other recent sentencing policies including truth in sentencing and three strikes laws that further increase prison populations.

While this chapter deals with the effects of sentencing reforms on shifts in the total imprisonment rates, the next chapter deals with a component of imprisonment; the effect of sentencing reforms on the odds of Black and Hispanic imprisonment. As with this study, it assesses the effects of sentencing reforms on shifts in imprisonment within U.S. states, but due to limitations in available data it only assesses the impacts from 1978 to 2005. The study has implications for both theory and policy. Substantively, the results indicate reforms significantly increase disparities in imprisonment for people of color, which is counter to some public policy's state goals. Theoretically, the analysis builds on the theory presented in this chapter to formulate a social chain theory. The social chain suggests the socio-political context of the law and order movement interacted with structural-procedural changes in the justice system that led to an unintended consequence of rising racial disparities. The results suggest mechanisms are at work that unintentionally "target" historically disadvantaged groups, perpetuating inequalities within criminal justice system instead of easing them.

CHAPTER III

THE IMPRISONMENT RACE: UNINTENDED CONSEQUENCES OF "FIXED" SENTENCING ON PEOPLE OF COLOR OVER TIME

Introduction

Prior to 1975, imprisonment philosophy in the United States was largely based on the rehabilitation model. It was believed offenders could be reformed by the prison system. Upon release, the convict would reenter society as a socially functional and law abiding citizen. Through this philosophy, sentencing structure allowed considerable judicial discretion (Roberts 1996; U.S. 1996). Given a wide sentencing range by state legislatures, the judge was bound by few set laws or procedures in rendering both the type and degree of sentence. Usually parole boards, acting independently of other parts of the judicial system and largely independent of legislators, determined if the prisoner had been properly rehabilitated and was ready for release (Albonetti 1997; Kempf-Leonard and Sample 2001).

Beginning in the 1970s, as part of a larger criticism of the criminal justice system as a whole, indeterminate sentencing policies came under scrutiny. Critics who focused on sentencing argued it allowed far too much disparity in types of sentencing and time

served. Of chief concern was the belief that the system was too lenient and was aiding (or at least not stopping) a perceived rise in criminal activity. As a result, a push to limit the perceived variability occurred (Griset 1995; Hebert 1997; Miethe and Moore 1985). In response, some states and the federal government adopted a number of new sentencing procedures including sentencing guidelines, statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws (Frase 2005; Stemen, Rengifo, and Wilson 2006). The movement reached a climax in 1994 when congress allocated billions of dollars in federal aid for reforms. Placing focus on the actions of judges and parole boards, the reforms set out to create greater uniformity in punishment (Reitz 1995; Spohn 2000).

Advocates cited a possible reduction in racial imprisonment disparities as one of the benefits of reforms. The argument was twofold. First, on the "front-end", it was believed that judges, taking cues from the general social setting and from personal biases, sentenced people of color, particularly Blacks and Hispanics, to significantly longer sentences. While it may be conventional to think of a judge as an orator of impartial law, their decisions are "impacted by popular passions shaped by historical and contemporary developments within democratic society (Crow and Gertz 2008: 363)." Second, but less emphasized, was the belief that under the indeterminate-rehabilitation model, parole boards, on the "back-end," covertly or unwittingly denied probation to minorities and the poor, believing they had unsupportive families and meager job prospects and were likely to reoffend. Thus, it was assumed that following sentencing reforms, which limit

discretion, racial disparities would at least be significantly reduced, if not nearly eliminated (Tonry 1994; Western 2001).

The current study assesses the effects of various sentencing reforms on the racial and ethnic composition of imprisonment across 49 states[32] and over time. As Crow and Gertz (2008) point out, sentencing research on the micro level flourished under a number of competing theoretical perspectives. Focusing on court communities and environmental contexts, some micro-level research suggested, in some circumstances, sentencing reforms have increased racial disparities. Conversely, macro-level policy studies largely under-utilized theoretical explanations. These studies have produced more mixed results and in some cases suggested reforms actually reduce imprisonment. Additionally, the aggregated macro-level studies incorporating an analysis both over time and across states have analyzed largely total imprisonment and have not assessed the impact on the racial/ethnic composition of imprisonment (Spelman 2009; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009). This paper diverges from the prior macro research by drawing on social chain theory (Fine 2006; Merton 1936) to suggest unintended consequence resulted in an increase in racial disparities in imprisonment.

Literature Review

Prior sentencing reform research has been conducted on both the micro and macro-level. The macro research, while informative in our understanding of the changes in total imprisonment rates and what may have been some of the prior determinates of

[32] Vermont was excluded from the analysis, as race specific imprisonment data was not reported for most years.

adoption of reforms, has, to date, not assessed aggregated changes in the racial/ethnic composition of imprisonment. The micro-level research, while some have assessed the effects of/on race, has typically not had broad generalizability due to a lack of comparisons across jurisdictions and/or a lack of analysis over time. Cross comparisons and observations over time are both essential for a robust analysis of the effects of reforms on people of color. This project utilizes a social chain theory to suggest the law and order movement created a context for the adoption of specific structural changes in sentencing that, while they may have had a stated goal of reducing racial/ethnic disparities, actually had an unintended consequence of increasing them. Before outlining the specifics of the social chain, I will briefly outline some of the important contributions and some of the weaknesses of prior reform research.

Focusing on judicial departures (*e.g.,* judges deviating from the average or suggested sentence) affecting people of color, some micro-level research built on a theoretical perspective drawn largely from symbolic interactionism has gained some traction (Albonetti 1997; Boerner 2001; Dalessio and Stolzenberg 1995; Frase 1995; Frase 2005; Griset 1996; Hebert 1997; Marvell 1995; Shane-DuBow 1998). In general, researchers either applied *focal concerns theory* or a more structural perspective focusing on the *indirect effect of race on legal factors*. For example, Albonetti (1997) and Steffensmeier, Ulmer, and Kramer (1998) used focal concerns theory to argue that when judges are presented with limited information for assessing a defendant's likelihood of re-offending, they may incorporate "perceptual shorthand" built on their own preconceptions of risk. Research has shown racial disparities resulted in part because the

defendants were seen as possessing certain "high-risk" extralegal characteristics, such as lower socioeconomic status or if they fit certain stereotypes of criminals, and received harsher sentences because of their "risky status". These extralegal factors in turn are also related to their racial or ethnic group status.

Furthermore, while it was argued that, under sentencing reforms, the emphasis on legal factors will serve to equalize the direct effect of race (*e.g.,* racist judges) (Von Hirsch 1985), the legal factors (*e.g.,* crime committed or prior record) are often directly influenced by racialized extralegal factors. The final result may actually be a more pronounced effect of race connected through extralegal factors that on appearance make the system seem race neutral, but in practice conceal and enhance disparities (Albonetti 1997; Kramer and Steffensmeier 1993; Kramer and Ulmer 1996; LaFree 1984; Nelson 1992; Spohn 2000; Steffensmeier and Demuth 2000; Steffensmeier, Ulmer, and Kramer 1998; Wooldredge 2007). While informative, because of small sample sizes, a general lack of observations over time, and a focus on contextual effects; most of the prior micro level research lacked broad generalizability.

Limited to the specific context and sentencing structures of the particular state, some single state research has assessed sentencing reforms over time, though often limited to relatively short time periods (Johnson 2006; Marvell 1995; Ulmer and Johnson 2004). While incorporating change in sentences over-time and highlighting the need for such measures, they lack broad cross-sectional applicability. Three studies to date (i.e. Spelman 2009; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009)

are notable exceptions and did attempt cross-sectional time-series[33] analysis of similar reforms on an aggregate state level across a fairly long time span. But while these studies do add to the literature on the effects of reforms on total imprisonment, they do not address possible changes in the racial and ethnic composition of imprisonment. As Johnson (2005) has argued, "additional studies are needed that assess the consequences of changing guideline systems across time and place (791)."

Broadly, this analysis is designed to assess the consequences of changes in sentencing policies across states and over time on the odds of imprisonment of people of color. Drawing on work by Gary Fine (2006) the complete theoretical frame incorporated in this analysis is presented as a series of chained social actions connected through a cascading series of events that ends with an increase in racial disparities. While each link in the chain possesses a distinct theoretical frame, the entire chain is needed to understand why and how the changes in the racial and ethnic composition of imprisonment were manifested. While this analysis ultimately focuses on unanticipated or unintended consequences of policy shifts in sentencing, the unintended consequences framework alone is insufficient in explaining the policy outcomes. Without the context or the structural shifts the outcomes are likely to be quite different than what was observed and without an understanding of the entire chain of actions different outcomes may be anticipated.

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[33] The cross-sectional time series analyses referred to here are studies that compare states (all 50) over a large section of time. These studies, like this study utilize the state as the unit of analysis and analyze changes in imprisonment rate.

Figure 3.1. Social Chain Theory Outlining the Frame Used in This Analysis

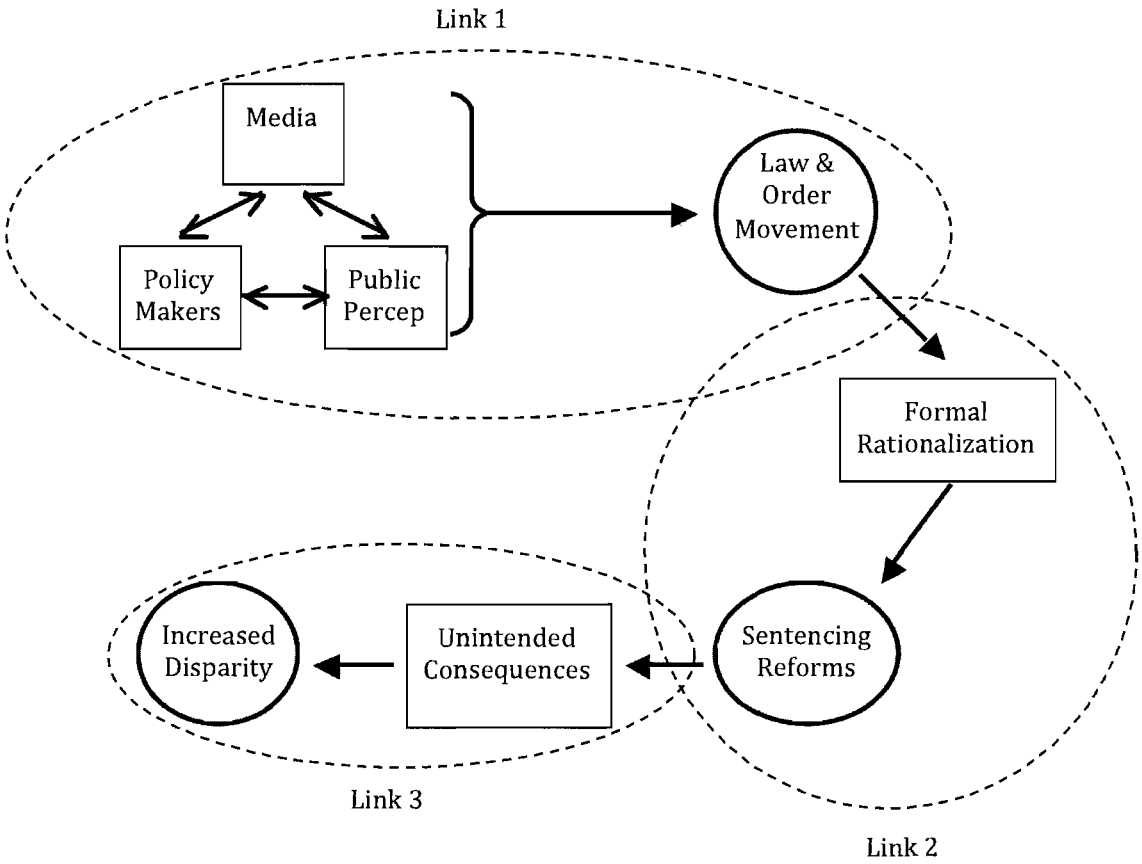

Figure 3.1 outlines the general framework of the social chain. The three circles represent an event or tangible social action that is preceded by a square (or a series of squares in the case of the law and order movement) representing the distinct theoretical frame for each link in the chain. While each link is important in its own right, the entire chain is ultimately more important in explaining the outcome than any one part. To follow is a brief outline of each component and what part in the chain it played. In the end the theoretical model suggests the politics of fear created a context (link 1: law and order movement) that led to a structural shift through formal rationalization (link 2:

sentencing reforms) that then led to an unintended outcome (link 3: increased racial disparities).

Politics of Crime and Fear:

Critics of the rehabilitation model of imprisonment advanced a number of purported problems, including complaints about the arbitrariness of the sentencing procedure, ineffectiveness of treatment and reform programs, lack of tough time, and claims of rampant repeat offenders (Hebert 1997; Reitz 1995). "Getting tough on crime" became a hot political topic and policy makers, both liberal and conservative, risked political suicide if they appeared soft on crime and justice (Glassner 1999; Hart 2005; Hunter 1991; Trend 2007). As Scheingold (1991) and Beckett (1997) suggest, the shift away from indeterminate sentencing represented the concerted effort by policy makers to advance the "justice model" of crime and punishment that focuses on *making criminals pay* and *deterring* those who might commit future crimes (Doob 2000; Frase 2005; Simon 2007). Socio-politically, the policies of the movement represented the highly connected push-pull between the media, politicians, and the public demand for punishment fitted to the crime not the offender. The interaction resulted in the law and order movement (illustrated in figure 3.1 by the three squares at the beginning of the chain) (Abramsky 2007; Clarkson and Morgan 1995; Engen and Steen 2000; Gottschalk 2006; Spohn and Holleran 2000; Tonry 1995).

Scheingold (1991) further suggests, "policy changes are mostly likely to occur when political leaders, in part by taking their cues from the media, choose to play upon public anxieties that are themselves inflamed by media imagery and vicarious

victimization rather than by crime as such (Scheingold 1991:44)". During the law and order movement, an expansion of the justice model was often used to further political agendas by playing on public fears of violent crime, resulting in advancement of an individualistic view of crime as a function of societal deviants (Abramsky 2007; Glassner 1999; Hunter 1991; Simon 2007). Meanwhile, the media flamed the fear by continually portraying images of crime, largely stressing violent offenders preying upon random isolated individuals (Trend 2007). Completing the interaction, politicians then seized upon the public fears to create law and order platforms focused on *getting tough on crime*. This complex circle of interactions served to promote and sustain the politicization of crime that reinforces punitive punishment (Beckett 1997; Scheingold 1991; Simon 2007).

Formal Rationality:

While the law and order movement provides an important historical backdrop that highlights the cultural underpinnings of the reform movement, this part of the social chain alone does not explain how reforms affected the racial/ethnic composition of imprisonment. A focus on only the law and order movement might obscure the impact that structural changes had and how they are undeniably connected to the context in which they were adopted. To understand how reforms served to restructure sentencing and possibly increase disparities, we must examine the second link in the social action chain. While under indeterminate sentencing the judge was allowed considerable discretion, under reforms the sentence is largely predetermined (or at least more predetermined). The "fixing" of sentencing into a formalized predictable formula was a

direct result of the law and order movement and is the driving force behind how reforms are likely to result in increased disparities.

To explain the restructuring, Savelsberg (1992) argues that sentencing reforms[34] are an ideal representation of what Weber (1978) called formal rationality; while indeterminate sentencing, conversely, is more closely aligned with substantive rationality. Both sentencing procedures are rational because they have "legalized" validity due to their basis in codified law, but differ in the application of the law. Substantive decision-making is subject to values, appeals to ethical norms, and historical precedent, focusing less on uniformity of outcomes. Formal decisions, on the other hand, focus on limiting subjective values-based decisions by stressing rationalized structural outcomes through an emphasis on a technical procedural process. The technical procedure is grounded in well-defined predetermined criteria designed to elicit a specific outcome. "The judge . . . is more or less an automaton of paragraphs: the legal documents, together with the costs and fees, are dropped in at the top with the expectation that the judgment will emerge at the bottom, together with more or less sound arguments - an apparatus, accordingly, whose functioning is by and large calculable or predictable (Weber 1978: 17)." In the end, sentencing reforms serve to bureaucratize the process with the specific goal of removing subjective interpretations and outcomes (Bohm 2006).

Ultimately the suggestion that sentencing reforms would reduce disparities relies heavily on the traditional notion of racism and suggests the new "color-blind" system would cut into the racist actions of individuals holding the discretionary power to wield

[34] Savelsberg (1992) only assessed the effects of sentencing guidelines. In this study, his theoretical frame is extended to all reforms considered in this analysis.

it. Unfortunately, the Weberian process described by Savelsberg (1992) may also serve to fortify the institutionalization of racists policies (or at a minimum policies that create racial disparities) into a form of legitimized bureaucracy, creating a "legitimized" justification for the higher rates of imprisonment among people of color (Ulmer and Kramer 1998; Ulmer and Johnson 2004). The sentencing process can appear to be justified due to its "race neutral apparatus" that focuses on crime and criminal history rather than individual characteristics of the offenders. The apparent "color-blind" system then suggests racial/ethnic disparities are a result of differential offending and not a result of a racist system at the structural level. In actuality, it works to conceal and possibly amplify the racial and ethnic disparities (Johnson 2003; Spohn and Holleran 2000; Spohn, Welch, and Gruhl 1985).

To illustrate, the rhetoric (this is not to suggest that some of the advocates did not believe reforms would have positive impacts) suggested a reduction in disparities among people of color (Savelsberg 1992) and even some prior research by Tonry (1995) and others assert racial and other extralegal (any factor not related directly to the commission of the crime) disparities should be less likely under schemes utilizing "race neutral" legal factors (Wooldredge 2007). But when considering how race and ethnicity is connected indirectly to legal factors through extra-legal factors it becomes apparent that an increase in disparities is possible, if not likely (Spohn 1990; Steffensmeier and Demuth 2000; Tayler 2000). On a structural level, reforms include many components shown through prior research to, both directly and indirectly, increase disparities (Spohn, Gruhl, and Welch 1982). For example, a number of the reforms included stated goals of reducing

repeat offenders. People of color (especially Blacks) are more likely to carry a criminal record due, in part, to guilty pleas to a relatively minor first offense. This first offense, while it may not result in prison time, will most likely result in a more severe sentence for the second offense. Furthermore, some crimes, for example crack cocaine possession, which is more common among Blacks, were seen as more "dangerous" than other drug possessions and received longer sentences[35] (Engen and Steen 2000; Hebert 1997; Spohn and Holleran 2000).

It is important to remember that the structural changes did not happen in a vacuum. The reforms were a result of the law and order movement that possessed specific characteristics. Without the context of the law and order movement and the demand for change that reflected the justice model, the structural changes the reforms enacted would likely have been different. The law and order movement and the formal rationalization of sentencing were undeniably connected and resulted in a contextually and structurally specific change.

Unintended Consequences:

While the formal rational structural theory gives us the framework for understanding how reforms changed the structure of the system and suggests a mode by which reforms can increase disparities, the final link of the social action chain is built on unintended consequences theory.[36] The study of "unanticipated consequences of

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[35] . Prior research has shown the connection between race and crack cocaine and that it was one of the driving forces behind the rise in young black males entering prison during the 1980s.

[36] In this study second order, unanticipated, unintended or unrecognized consequences, and latent effects will all be considered representing the general ideal of the laws of unintended consequences as defined by Merton (1936).

purposeful social action" (Merton 1936), has a long history, as far back as Adam Smith's "invisible hand" or Herbert Spencer's discussion of unhappy guesses or even Karl Marx, in Das Kapital, wrote about the likelihood of unanticipated outcomes (Fine 2006; Symonds and Pudsey 2008). Max Weber took it even further by naming it his "paradox" of social action in which he stated "it is undeniably true, indeed a fundamental truth of all history that the final result of political activity often, nay, regularly, bears very little relation to the original intention: often, indeed, it is quite the opposite of what is intended" (Weber 1978). More recently, contemporary authors Charles Tilly, Robert Rosenthal, Lenore Jacobson, Raymond Boudon, and Gary Alan Fine have expanded and applied the principal to a wide variety of social and political actions. While many have written on the general principle, the single most influential theorist was Robert Merton, who published his first of many important advancements of the theory in 1936 (Fine 2006). "Merton was, by no means, either the first or the last to recognize the importance of 'second order consequences', his general theory, however, is still the most influential" (Fine 2006: 5).

Unintended consequences can come in many forms and many sizes, from Lillian Rubin's (2007) study of public policy response to homelessness in San Francisco, Ganapati and Frank's (2008) study of subsidized housing in Miami-Dade county, or to Kovandzic, Sloan, and Vieraitis (2004) study of the three strikes law in California. These studies and others show that the effect can be local and intense or can be widespread and life changing (*e.g.,* as in the passing of prohibition leading to the rise of organized crime) (Fine 2006). In all of these situations, the attempt to address a specific social problem

(*e.g.,* homelessness, creation of affordable housing for racial minorities, the problem of repeat offenders, or the social ills of alcohol consumption), while they may have been well intentioned, also resulted in some unintended consequence. These consequences may, at least in some of the situations, have actually outweighed the social benefits (Fine 2006; Ganapati and Frank 2008). No matter how well intended, the possibility of unintended consequences raises a caveat for any social policy, not only may the policy not solve the problem it was designed to alleviate, it could exacerbate the problem, and finally it may lead to a new unforeseen problem. The current study suggests an unintended increase in racial and ethnic disparities in imprisonment occurred, which is counter to the reforms' stated goals.

In general, unintended consequences can be broken down into 5 specific "types"; incomplete information, error in appraisal and implementation, narrow focus on immediate future and ignoring long term effects, value based decisions operating beyond the specifics of the policy (*e.g.,* belief that felons deserved to be punished harshly regardless of the effectiveness of the policy), and the self-defeating prophecy (Merton 1936). Under this typology, each of the five unintended consequences encompasses varying degrees of likelihood with the specific "fixed" sentencing policies discussed in this paper. Table 3.1 briefly outlines the typology and illustrates potential disparity increasing outcomes that may or may not be present in any given state.[37]

It should be noted that this frame is not intended to suggest that outcomes, even the rise in the rate of imprisonment for people of color, from sentencing reform adoption

[37] The table does not represent an exhaustive list. Instead the table is designed to illustrate the likelihood of Merton-like unintended outcomes of sentencing reform adoption.

is always unintended. In fact it is likely the same pitfalls and biased views of people of color that may have affected judges, also affected legislators when drafting the reforms. If for example, legislators or sentencing commissions view specific crimes known to be prevalent among people of color as more dangerous and thus deserving of a harsher sentence, they may knowingly increase the length of sentence for that particular crime. While I do not intend to suggest that sentencing reforms' designs were completely void of racist intentions, the choice to focus on unintended consequences as the final link in the chain was intended to concentrate on those who seemed to be both well intended and optimistic that racial/ethnic disparities would be improved by sentencing reforms (Blumstein, Cohen, Martin, and Tonry 1983; Crow and Gertz 2008; McDonald and Carlson 1994).

Table 3.1. Unintended Consequences Typology

Type	General Pattern	Specific to Reforms
Incomplete Information:	It is nearly impossible for policy makers to consider all possible variables and construct changes with full and complete knowledge of the outcomes.[1]	In an attempt to make quick and sweeping change, the policy makers failed to adequately consider all the possible complexities of changing the sentencing structure.
Error:	Policy makers fail to fully comprehend the information.	Failure to recognize racial/ethnic disparities are tied to the legal factors used to equalize sentencing.[2]
Focus on Immediate Future:	A narrow focus on the immediate future while ignoring long term effect of the policy reform. Often a result of an intense public pressure to "to something."	Because of the intense focus on crime and justice in the 1970s and 80s, sentencing reforms often represented a narrow push for more punitive sanctions. These "fixed" sentencing reforms met the short-term goal of increasing punishment, pacifying both the media and the general public, but unfortunately they may have also had lasting long-term effects.[3]
Value Based Decisions:	When policy is driven by moral entrepreneurs, the policy can often be pushed and molded, not by rationally constructed policy, but by emotional and value based appeals to moral beliefs (Saltzburg 2001).	The sentencing reform movement was driven by what Becker (1963) called moral entrepreneurs, who saw sentencing as one of the sources of the social problem of crime. They played upon public fears and media depictions to push a social agenda that included a focus on punitive sanctions. The result was a swift move away from a rehabilitation model that used indeterminate sentencing to a justice model focused on warehousing and punishing.[4]
Self-Defeating Prophecy:	The policy reform defeats itself by focusing on the very individuals who are likely to resist the most. Judges feeling they had lost the power to mold sentences to the offender may actively work against the "fixing" of their decisions and again deviate from the prescribed ranges.	The judge may wittingly or unwittingly either downward deviates from average sentences for White defendants and/or upward deviate for people of color, resulting in a racial disparity in lengths of sentences.

Notes: [1] Of course, this particular type of unintended consequence suggests it is possible to know the information, but they failed to consider it due to limitations. This differs from the second type that suggests information was considered, but an error was made in assessing the current or future state of affairs or they failed to implement a needed course of action

[2] Under this possible outcome the reform could actually increase racial disparities rather than reduce them.

[3] At the minimum, they are likely to increase total imprisonment placing an undue burden on the justice system. The reforms could also compound the racial disparity by ratcheting up punishment on repeat offenders. Research has shown that racial minorities are at increased risk of being convicted of relatively minor first offenses. With this conviction, which may have not resulted in a prison sentencing, they now carry a criminal record that increases the punishment for future crimes

[4] Moral entrepreneurs also focused on second offenders and drug crimes which are two areas more highly associated with people of color. The second order effect being a rise in racial disparities in imprisonment resulting from the adoption of the "fixed" sentencing reform.

[5] Major obstacles to the limiting of judicial discretion are the judges themselves. Reform legislation was largely passed without the support of the courts, particularly judges, who immediately disapproved, feeling they had lost their power to fit the offense with appropriate sentence based on the contexts of the crime and offender (Albonetti 1997; Miethe and Moore 1988; Savelsberg 1992).

Hypotheses:

Unintended consequences as a result of the adoption of sentencing reforms is both a social phenomenon, characterized by the interaction of the media, public, and politics found in the law and order movement, and structural phenomenon, found in the formal rationalization of the sentencing structure. While the rise in disparities among people of color may be expected and unavoidable due to the structural determinism suggested by Merton and others, the origins of the determinism is rooted elsewhere: in this case the social context preceding the structure. Without both previous links of the chain, the final link, the rise in racial disparities in sentencing as an unintended consequence, is insufficient to hypothesize a possible divergence from a stated goal of reduced disparities (Wooldredge, Griffin, and Rauschenberg 2005). Furthermore, the social understanding (the law and order movement) only "exists" outside the structure (formal rationalization) because the structure exists regardless of the social schema creating it, but they are undeniably intertwined. As Fine (2006) has suggested:

> "Social problem solutions are path dependent, but they simultaneously change those paths, producing unintended consequences. Policies are imbedded in relations among actors, who also change those relations as they act. Because of the reflexive features of social systems-latent as well as manifest relations-few problem solutions are so narrowly targeted that they alter only the behaviors that the moral entrepreneurs [or policy makers] desire. This recognition provides an opportunity for sociologists [or social scientists in general] to strip away the confident rhetoric that surround proposed changes and to appreciate Robert Merton's claim that behavior is interdependent, situated in a world that is tightly coiled (p. 15)."

The general aim of this research was not to identify the specific unintended mechanisms (*e.g.,* rational concealment) or to test the interconnectedness of links as

presented in the social chain, but to test the existence of the *overall* unintended effect of sentencing reforms on people of color in direct contrast to one of the stated goals put forth by policy makers. Eventually the identification of the specific mechanisms causing the racial disparity and the testing of the social chain theory at the individual state level should be fully addressed. The analysis here is not intended to test the connections between the links in the social chain or even the possibility of their existence. Instead the theoretical chain is designed to highlight the likely causes of an unintended rise in racial/ethnic disparities as a (unintended) consequence of sentencing reforms. The substantive analysis is then designed to test if there was indeed a rise in the racial/ethnic disparity in imprisonment suggesting the existence of an unintended consequence.

> *Hypothesis 1: States implementing sentencing reforms in the form of presumptive sentencing guidelines, voluntary sentencing guidelines, statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws will increase the odds of Black rates to White rates of imprisonment more than states that have not implemented sentencing reforms.*
>
> *Hypothesis 2: States implementing sentencing reforms in the form of presumptive sentencing guidelines, voluntary sentencing guidelines, statutory presumptive sentencing, determinate sentencing, truth in sentencing, and three strikes laws will increase the odds of Hispanic rates to White rates of imprisonment more than states that have not implemented sentencing reforms.*

While the two hypotheses listed here represent the pooling of the six main sentencing reforms under study, each hypothesis can also be seen as having six sub-hypotheses testing each reform separately. Table 1.1 (Page 7) illustrates the use of sentencing reforms, which now represent a majority of sentencing procedures in the U.S. While considerable variation within the "types" of sentencing procedures exist,

considerable variation in the overlap of sentencing reforms also exists (these distinctions are discussed later). In the end, this investigation aims to test the general effects of sentencing reforms on the odds of imprisonment for Blacks and Hispanics on the aggregate state level over time.

Data

This study utilized state level data covering each year between 1978 and 2005 (1981 to 2005 for Hispanics) with data available for 49 states. This panel data has the benefit of incorporating both cross-sections, in the form of states, and observations over time, where changes in state odds of imprisonment of people of color due to sentencing reforms can be assessed in comparison to states not adopting reforms. The study begins in 1978 because prior to that year race and ethnic group specific imprisonment rates were not recorded at the state level. It would have been ideal to include groups in addition to Whites, Blacks, and Hispanics, however small sample sizes and inconsistent recording of groups over time and across states made this problematic.[38] Additionally, Washington DC[39] and Vermont were excluded from the analysis, as race specific data were not reported for most years.

Obtained from the Bureau of Justice Statistics[40], the dependent variables represent the odds of Black to White and Hispanic to White imprisonment.[41] While three separate

[38] First, early in the data series, states did not record any racial groups beyond Whites, Blacks, and others and used a separate question to assess Hispanic origin. Second, in some states, other racial groups represent relatively small percents in the general public, often resulting in very few and sometimes zero individuals in prison. This makes the analysis difficult, as the coefficients are unstable.

[39] Along with incomplete data, Washington DC stopped housing its own prisoners in 2001.

[40] Data for the years of 1978 to 1983 is available in the yearly publication: Prisoners in State and Federal Institutions on December 31, 1972 (through 1983). Data for the years of 1984 to 1998 is available in the

analyses of Black, Hispanic, and White imprisonment rates as individual rates would allow for the assessment of individual effects, the results from each group are not comparable to each other, as each dependent variable would measure a unique rate and would not allow for direct comparison (Frost 2008).[42] Therefore, the models incorporate odds ratios of Black imprisonment rates per state Black population to White imprisonment rates per state White population and Hispanic imprisonment rates per state Hispanic population to White Imprisonment rates per state White population. These two measures allow for testing of the increased likelihood (as odds ratios) of people of color are going to prison, as compared to whites, resulting from adoption of reforms. On average over the years under investigation, Blacks are 7.01 times more likely to go to prison, while Hispanics are 1.98 times more likely.

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yearly publication: Correctional Populations in the United States, 1984 (through 1998). Data for the years of 1999 to 2005 is available in the yearly publication: Prisoners, 1999 (through 2005). All three publications are produced and printed by the U.S. Bureau of Justice Statistics (see Citations). Rates per 1000 for each variable were computed by taking the raw number of prisoners for each state by year that was provided by the Bureau of Justice Statistics and dividing by state population per 1000 as supplied by the Bureau of the Census (see citation).

[41] Unfortunately, early in the time period of this analysis, states only collected data on white, Black, and other with a separate question assessing Hispanic heritage. This, of course, limits the analysis in those groups and a number of researchers have pointed to the limitation of the "other" category as a justifiable measure. Furthermore, when disaggregated data did become available many states had little to no individuals from a specific racial/ethnic group in prison (largely a result of small numbers in the general population of that state), making analysis unstable (Beck and Katz1995; Halaby 2004).

[42] For example, larger coefficients for Blacks (for each of the variables) over both Whites and Hispanics and the larger coefficients for Hispanics over Whites were observed. This is at least partly a result of differences in the dependent variables. For example, Blacks are incarcerated at higher rates; if therefore, voluntary guidelines resulted in a 10 percent increase in imprisonment rates for each racial group, the increase in the "effect" on Blacks would be greater than for the White rates and the coefficient associated with voluntary guidelines in the Black equation would be greater than for the coefficient in the analysis of the White imprisonment rates. This limitation necessitates the analysis presented in this paper, which presents logged odds ratios of Black to White and Hispanic to White imprisonment. In addition, the analysis has the added benefit of getting at the heart of the theoretical argument that reforms will have an "unintended" consequence of increasing imprisonment rates for people of color more than Whites.

While it is relatively straightforward to calculate the Black and White imprisonment rates over the entire data series, the construction of the Hispanic imprisonment rate data took more care. In contrast to our social schema of what it means to be Black or White, which has been generally stable in the years under investigation, other racial and ethnic categories are more fluid and influenced the consistency of the Hispanic imprisonment data (Karnig and Mcclain 1985; Quillian 1995). Collected over three decades, the Hispanic data, similar to the way the census changed, has taken on three distinct categorizations. To account for these changes, the Hispanic rates were "standardized" to the most recent categorical definition, where individuals are first classified by a racial category which includes a relatively long list like White, Black, Native American, Asian or Pacific Islander, and so on and then asked if they are of Hispanic Origin.[43] Additionally, in some states, especially early in the time series, the population of Hispanic prisoners was small, making the analysis less stable. Even with these complications with the Hispanic data, the data will allow the testing of both hypothesis 1 and 2.

Table 3.2 outlines the six key independent variables representing sentencing reforms. They were compiled from numerous sources including the Bureau of Justice

[43] The "standardization" of percent Hispanic involved three steps. First, the most recent categorization system began in 1990 and these percents were left unchanged and used as the "base" to adjust the remaining years. Second, because the rates between 1980 and 1989 were collected under a different definition and coding scheme, these rates were adjusted. The 1980 to 1989 rates were first converted to state-specific change-scores. These change-scores were then matched to their respective state-specific 1990 percent by using the average change-score from 1990 to 1992 as an estimate of the change from 1989 to 1990. Then taking the 1990 percent and multiplying it by the three-year averaged change-score from 1990-1992, a percent for 1989 was estimated. Third, the estimated 1989 state-specific percent was then used as the base rate to move backwards in time to generate percents based on the actual 1980 to 1989 change-scores. The same procedure was then implemented for data prior to 1980 using the estimated 1980 percent as the base.

Assistance (1996), Frase (2005), Zhang, Maxwell, and Vaughn (2009), and a report by the Vera Institute (Stemen, Rengifo, and Wilson 2006). While most research is in agreement on the general goals of the reforms, considerable variation in the design and application of the reforms exists and there is little consensus about how to classify them into appropriate groups (Frase 2005; Marvell 1995; Stemen, Rengifo, and Wilson 2006; Tonry 1995). Appendix A contains a detailed discussion of the general function of the various reforms and how they are delineated in this study.

Table 3.2. The Six Reforms Included in Analysis

Reform	Description
Presumptive Sentencing Guidelines[1]	They consist of a matrix of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) and offense severity. Judges generally must follow the matrix. They are strict in their application; only allowing very limited deviations,
Voluntary Sentencing Guidelines[1]	Treats guidelines as formal recommendation, but does not *legally mandate* they be followed by the judge.
Statutory Presumptive Sentencing[1]	They represent an attempt to create uniformity within similarly situated crimes, but act less like a sentencing rubric. They specify an appropriate or "normal" sentence for each offense as a baseline for a judge.
Determinate Sentencing[2]	Determinate sentencing is used to refer to a system *without* discretionary parole boards.
Truth in Sentencing[2]	They require offenders serve a statutorily defined minimum amount of time. Only states meeting the 1994 federal omnibus crime bill minimum 85% time-served of the original sentence are considered.
Three Strikes Laws[3]	They are a habitual offender law focused on three time felony offenders. Generally the law suggests a severe sentence (25 to life or life) for a third felony offense.

Notes: [1] Front-End Reforms, [2] Back-End Reforms, [3]Sentencing Enhancement

There are two key points about the reforms that should be noted. First, all six reforms represent a shift from the indeterminate-rehabilitation model to a punitive model with a discretion-limiting sentencing or release structure. Second, presumptive

sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing are considered independent and mutually exclusive "front-end" reforms and may not, at any given time, coexist with either each other or indeterminate sentencing. Third, the "back-end" limiting or elimination of discretionary release (truth in sentencing or determinate sentencing) is considered separate because their focus on release allows operation alongside the "front-end" reforms. Finally, the unique and independent nature of three strikes laws allows it to occur alongside both the front-end and back-end reforms. While states adopting reforms often adopted numerous types, their coexistence is not absolute and different reforms were often instituted in different years (See Table 1.1 on Page 7; *e.g.,* Oregon adopted presumptive sentencing guidelines in 1989, abolished parole in 1989, and adopted truth in sentencing in 1995) (Frase 2005; Stemen, Rengifo, and Wilson 2006).

Additionally, because the dependent variable does not include county or local jail populations, only individuals sentenced to more than one year are considered. To account for this lag effect, reforms were coded as beginning one year after their actual establishment. Second, while in the year following the reform there will be an instantaneous effect on some portion of the imprisonment rate, not all individuals in any given year will be "caught up" by the reform. Some of the rates (the race specific rates used to construct the odds ratios) will be individuals who were already imprisoned and sentenced under the old sentencing procedure. Over time, a greater proportion of prisoners will be "caught up" by the reform. At some point a plateau at nearly full effect will occur and only a small portion of the rate will be individuals' who were sentenced to

long terms under indeterminate sentencing. To address this issue and capture the expected "logged growth curve" like effect, a new measure of reforms was implemented. Using a logarithmic measure to model the effect of sentencing reforms represents an important divergence of this study from previous analyses, which relied heavily on dummy variables.[44]

To create the new sentencing measures, each state was coded as 1.00 the first year of implementation and as *e* (2.71828) for year five through the last year of observation. The intervening years between one and five were set at an equal distance between 1.00 and 2.71828. Finally, the absolute[45] natural log of each score was taken. This procedure created a "logarithmic growth variable" with a one-year lag (the log of 1 is 0) that suggests full effect at five years. The measure represents an effect that approaches 1 (the full effect) at an increasingly diminished rate (increases fast at first and then slows). States with indeterminate sentencing were coded to zero and represent the reference group. Finally, because in most states more than one reform has been adopted, interaction terms are included to assess the combined effect of multiple reforms.

The independent variables for the analysis were assembled from a number of different sources. The FBI's Uniform Crime Report (UCR)[46] supplied the data on violent

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[44] Coding the reforms as dummy variables for adopted or not suggests imprisonment rates would be affected 100% by the new policy in the first year it was adopted. A logarithmic measure represents a more appropriate theoretically expected effect and should lead to more robust results.

[45] Removes the potential for negative scores.

[46] Data from the Uniform Crime Report (UCR) is available in print and online from the U.S. Dept. of Justice, Federal Bureau of Investigation and is produced and printed yearly (see Citations).

crime[47] and drug crime arrests per 1000[48] and data from the U.S. Census[49] was used to construct the variables for percent Black, percent Hispanic, percent urban, and population density. It also supplied total population for each state, which served as the denominator to construct all rate variables. The U.S. Bureau of Labor Statistics (1965-2008a) supplied information to construct the data for unemployment rates, while the U.S. Bureau of the Census (U.S. 2006) provided the data for poverty rates. Dubin (2007) supplied the needed political party affiliation of state senate and house (or assembly)[50], While the dummy code for the governor's political party was obtained from Hershey (2007). Research indicates the controls presented in Table 3.3 (See Appendix E for descriptive statistics of variables used in this analysis) represent key variables associated with prison population making their inclusion warranted (Albonetti 1997; Arvanites and Asher 1998; DeFina and Arvanites 2002; Engen and Gainey 2000; Steffensmeier and Demuth 2000; Tonry 1994; Western 2001; Western 2005).

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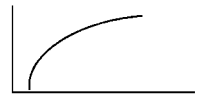
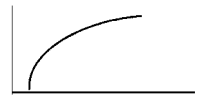
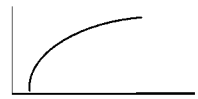
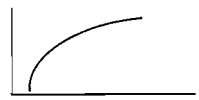
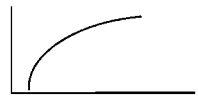
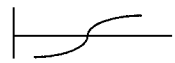
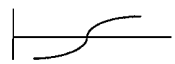
[47] Violent crime arrests represent the Uniform Crime Reports indexed crimes, which include the offenses of murder, forcible rape, robbery, and aggravated assault.

[48] In this project "*violent crime*" and "*drug crime*" rates refers to violent crime and drug crime arrests rates recorded by the Uniform Crime Reports for each year (see Citations).

[49] Data from the census is available online at census.gov maintained by the U.S. Bureau of the Census and was compiled from both census data and population estimates (see Citations).

[50] State house and senate variables were constructed by centering the percent Republican around 50% so that Republican control represents positive deviations from 50%, while Democratic control represents negative deviations. The absolute values of the deviations were then logged with the negative sign returned to the Democrats to create a logarithmic scale with positive and negative deviation from zero to represent the diminishing returns of political party concentrations.

Table 3.3. Independent Variables Included in Analysis

Variable Name	Data Type	Model Specification^	Graphical Representation and Notes
Presumptive Sentencing Guidelines (Key)	Logged growth curve	All Models	
Voluntary Sentencing Guidelines (Key)	Logged growth Curve	All Models	
Statutory Presumptive Sentencing (Key)	Logged growth Curve	All Models	
Determinate Sentencing (Key)	Logged growth Curve	All Model	
Truth in Sentencing (Key)	Logged growth Curve	All Models	
Three Strikes Laws (Key)	Logged growth Curve	All Models	
Violent Crime Rate	Per 100,000 state pop.	Model 2	Violent crime arrests
Drug Crime Rate	Per 100,000 state pop.	Model 2	Drug crime arrests
Unemployment Rate	Per 100,000 state pop.	Model 2	
Poverty Rate	Per 100,000 state pop.	Model 2	
Population Density	State population per mile	Model 2	
Percent Urban	Percent living in cities over 100,000	Model 2	
Percent Black	Percent state population	Model 2	
Percent Hispanic	Percent state population	Model 2	
Republican State Senate Political Power	Absolute logged percent deviation centered around 50%	Model 2	
Republican State Assembly Political Power	Absolute logged percent deviation from 50%	Model 2	
Republican Governor	Dummy Code	Model 2	

^ Indicates which model the variables appeared in.

For this analysis, it would have been advantageous to have violent crime rates, drug crime rates, unemployment rates, and poverty rates for each racial group (*e.g.*, Hispanic violent crime rate for the analysis of Hispanic imprisonment rates), but some issues made such analyses impossible.[51] While crime-specific rates for separate racial groups would have been valuable, variables incorporating total population (cutting across racial and ethnic groups) and variables specific to people of color test two different substantive things. First, data specific to each racial group would control the race specific change in each variable. For example, some researchers have suggested (Albonetti 1997; Spohn, Gruhl, and Welch 1982; Steffensmeier, Ulmer, and Kramer 1998), that unemployment among Blacks will increase Black imprisonment; thus, controlling Black unemployment in models of black imprisonment rates would be ideal. Conversely, critical race theory suggests Blacks can be viewed as a "threat" to the White majority regardless of race specific crime rates and this can translate into imprisonment disparities (Jacobs and Carmichael 2001; Jacobs and O'Brien 1998; Webster 2007).[52] In this sense, total rates may represent a more appropriate proxy of the "threat" because high

[51] First, a number of states were missing this data across a number of years. Second, a set standard of racial categorization was not used by individual states, making comparisons difficult; this is particularly true for Hispanics where states have counted individuals as both an ethnic and/or racial group. Third, data on drug crimes for separate racial groups is only available in the 1990s and is not available for violent crime on a state level across all states. As a result, violent crime rates, drug crime rates, unemployment, and poverty rates could only be calculated using total population per 100,000.

[52] The theory suggests people of color are seen (overtly or contextually) as "criminals," particularly as "violent criminals" and as more people of color enter a community, the perception of physical and social threat increases. This perceived threat translates into more arrests, charges, convictions, and longer sentences for minorities and this will translate into higher imprisonment rates for people of color. The perceived threat does not need to be related to a real threat, and thus minorities may be imprisoned at higher rates simply because they make up a larger portion of the population. The perceived "threat" can take the form of either a threat of higher criminal activity and thus minorities will be targeted as "the criminals" or a threat to social, political and/or economic dominance and again minorities could be targeted by the criminal justice system as a form of social and economic control.

rates of, crime will be attributed to people of color regardless of actual rates among the separate racial groups (Quillian 1995; Spohn and Holleran 2000).

Methods

The panel data, with its 49 states observed over time from 1978 to 2005, are analyzed using time-series cross-section (TSCS) regression (also known as panel regression).[53] Models were run using Prais-Winsten regression with panel corrected standard errors developed by Beck and Katz (1995). Each model incorporated fixed effects[54] for states to correct for time-invariant omitted variable bias and an AR1 term to correct for autocorrelation. This model specification has been shown to be effective in measuring within panel (*e.g.,* states) changes over time (Halaby 2004; Wooldridge 2000; Wooldridge 2005). When focusing on changes over time, fixed effects can be implemented by including dummy variables for each state.[55] This model controls the

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[53] Several problems can arise in panel analysis. Parks (1967) developed a generalized least squared (GLS) regression procedure to solve some of the issues and numerous studies have adopted the method (Beck and Katz 1995). Park's method, however, may understate the standard errors of regression coefficients by as much as 50 to 300 percent, seriously calling into question the use of this estimator. To counter this problem, Beck and Katz (1995) recommend an approach that uses the Prais-Winsten regression with panel corrected standard errors. Their method is a variant of ordinary least squared (OLS) regression and while regular OLS is not particularly useful in TSCS. It can be correctly implemented when used in conjunction with PCSE (an error correction that takes into consideration the tendency for panels (in this case the states) to be correlated) and corrections for autoregression (the tendency for year to year observations to be correlated with each other).

[54] The Hausman test can be used to determine whether the random effects model, a model that allows for both tome variant and time invariant error to vary freely, is likely to suffer from omitted variable bias. Omitted variable bias is a likely and potentially critical miss-specification of the models where unobserved unit specific (in this case state-specific) variation is not accounted for. If this bias is present and unaccounted for, the coefficient will be unstable and biased. In this case, the Hausman test indicates omitted variable bias is likely in this analysis. In this case, because no additional variables can be added, the fixed effect models should be used.

[55] In effect, fixed effects for panels exploits within group variation by holding constant unexplained between group variations. The estimates achieve an unbiased state even when the random effects assumptions are violated. In this analysis the unit fixed-effects model offer significant advantages over the random effects model (Halaby 2004)

unobserved state level effects that are stable over time by holding the unexplained time-invariant variation constant. The fixed effects model has the advantage of correcting for any state-level omitted variables that can cause bias. This can substantially improve the results and has the added benefit of controlling for any time-invariant state differences that may be associated with sentencing reform adoption but not measured (England, Kilbourne, Farkas, and Dou 1988; Halaby 2004).[56] Additionally, if the study is not primarily concerned with panel-to-panel variation, as in this analysis where the interest lies in the patterns of change over time, then the panel specific fixed-effects approach sacrifices little (Halaby 2004).[57]

The Hausman test can be used to indicate the need for fixed-effects for units and in this analysis the test indicates that the random effects model is not warranted. Conversely, if the Hausman test indicates the necessity of fixed effects for units, it does not necessarily dictate the absolute need for fixed effects for time. The choice of fixed effects for time is one largely based on theoretical interest (Halaby 2004; Wooldridge 2005).[58] This is because when fixed effects for units is incorporated, most time variant

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[56] An important advantage in this analysis is that fixed effects for states will control for any regional differences that may be present. For example, research has shown that the south has higher rates of imprisonment and the analysis will control for this difference and remove its effect from the results (England 1988). Though if someone were interested in state-to-state differences this model would not be appropriate.

[57] As long as the analysis includes controls for time variant bias, in which a significant number of controls can be included in the analysis (*e.g.*, AR controls for autocorrelation or moving averages).

[58] It is not wise to use a random effects model when the Hausman test of the random effects vs. fixed effects model for panels indicates that the fixed effect model is preferred. In most time-series cross-sectional analysis issues of confounded errors that are likely in a full random effects model can often be addressed with the inclusion of fixed effects for panels and controls for serial correlation (*e.g.*, auto-regressive controls). With the inclusion of fixed effects, the remaining variation becomes a measure of within panel variation from each panel's mean. As this may be a measure that is of theoretical interest, fixed effects for time would not be warranted. To illustrate, when fixed effects for both units and time are

issues (including time-specific omitted variable bias) can be assessed with a large number of proven procedures and while fixed effects for time is on the list of appropriate tools to assess models suffering from unexplained time variant co-variation, their inclusion changes the interpretation of the coefficients from change over time. Because theory suggests it is desirable to leave the year-to-year variation intact in this analysis and preserving the measure as change over time, it was decided *not* to include fixed effects for years (*e.g.,* period or time effects) and instead use AR(1) controls for serial autocorrelation. The fixed effects for units models with an AR(1) have been shown to be effective (Beck and Katz 1995; Halaby 2004).[59]

Results

Following the theoretical model that suggests an unintended consequence would result due to a cascading chain of events, it was hypothesized that sentencing reforms would affect the Black imprisonment rates more than White imprisonment rates. Table 3.4[60] presents two models testing this hypothesis. The first, Model 1, present the base model with only the conditional main effects of reforms and the first level interactions

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included, the substantive interpretation of the coefficients become a measure of the panels' deviation from time by unit specific mean that is stable over time, completely removing any unexplained time varying *and* panel varying effects. This of course could be a measure of theoretical interest, but is substantively different from the "change over time" measure of the panel-only fixed effects model. Whether a researcher chooses to include fixed effects for time is one of substantive interest informed by the research question, while the choice of the inclusion of fixed effects for panels is one of availability of appropriate controls irrelevant of the research question (Halaby 2004).

[59] Both a Mundlak and Hausman test of the time and unit specific fixed effects vs. the unit only fixed effects model suggest the AR(1) term is sufficient in addressing serial correlation and other time variant model concerns. Additionally, a Augmented Dickey-Fuller unit-root test suggests that the AR(1) model is sufficient in correcting any possible autoregressive process. Models ran without an AR(1) term failed the test, while models with the term passed.

[60] Control variable coefficients were not presented here, but can be found in Table 3.5.

between reforms. The model serves as a baseline to compare the results of the full model: Model 2. Model 2 on the other hand includes the control variables presented in Table 3.3. In Table 3.4 the coefficients represent the change in the odds of Black imprisonment when shifting from the reference group of indeterminate sentencing (represented as a 0 for all reform variables including interaction terms) to a state with at least one reform being fully implemented (represented by a 1[61]) or combination thereof. In Model 1, only presumptive sentencing guidelines and an interaction of statutory presumptive sentencing and determinate sentencing are significant.

While Model 1 serves as the base, Model 2 is the model of most theoretical interest. The model controls for state level crime, demographic and political effects. Of note is the large change in R-squared between Models 1 and 2. While in Model 1 the reforms only account for about 23% of the variation, Model 2 accounts for a robust 85.6% of the variation, suggesting that these state level controls that change over time are important in explaining differences between states. Model 2 suggests the conditional main effects of both types of sentencing guidelines, presumptive and voluntary, as well as truth in sentencing significantly increased the logged odds of Black to White imprisonment. Considering that the average odds of Black imprisonment under indeterminate sentencing were about 7.01 times that of White imprisonment, the conditional main effects are relatively large (though smaller than the effect on the Hispanic odds). For example, when truth in sentencing is fully implemented, it leads to an increase in the odds of Black imprisonment of about .308 or a 4.4% increase.

[61] Or the natural log of *e*, which is 1.

Additionally, both presumptive and voluntary guidelines increase the odds by .184 and .228 respectively.

It should be noted that few states implemented only one reform. This fact necessitated the inclusion of interaction terms. These terms simulate the simultaneous effects of two reforms "co-existing" in one state. In general the interaction terms served as a mitigating effect, reducing the additive conditional main effects of reforms. As expected, because of their significant main effect, significant interactions between both types of sentencing reforms and truth in sentencing are observed. So while the conditional main effects of presumptive sentencing guidelines and truth in sentencing increased the odds of Black imprisonment by .184 and .308 respectively, the interactive effect is .0616 or only a 0.9% increase for the average state. In addition to the expected interaction terms, an interaction between determinate sentencing and truth in sentencing was also observed. While the conditional main effects of truth in sentencing were significant, the conditional main effects of determinate sentencing were not. This may be a result of the strong main effects of truth in sentencing making the interaction significant despite determinate sentencing not being significant. Some previous research suggests that sentencing reforms would have a greater effect on Black imprisonment rates than on White imprisonment rates (Arvanites and Asher 1998; Engen, Gainey, Crutchfield, and Weis 2003). The analysis here supports this prior research for at least some of the reforms.

Table 3.4. The Odds of Black Imprisonment Compared to White Imprisonment

	Model 1	Model 2
Reforms		
Presumptive Sentencing Guidelines	0.268*	0.184*
	(0.194)	(0.098)
Voluntary Sentencing Guidelines	-0.000402	0.228*
	(0.106)	(0.118)
Statutory Presumptive Sentencing	-0.180	0.167
	(0.300)	(0.240)
Determinate Sentencing	0.138	0.115
	(0.232)	(0.168)
Truth in Sentencing	0.238	0.308***
	(0.356)	(0.106)
Three Strikes	0.464	0.133
	(0.466)	(0.142)
Interaction Terms		
Presumptive Guide * Determ. Sent.	-0.0846	-0.282
	(0.426)	(0.228)
Presumptive Guide * Truth in Sent.	-0.526	-0.416*
	(0.382)	(0.214)
Presumptive Guide * Three Strikes	-0.400	-0.0791
	(0.624)	(0.208)
Voluntary Guide * Determ. Sent.	-0.788	-0.332
	(0.944)	(0.510)
Voluntary Guide * Truth in Sent.	0.0456	-0.348**
	(0.330)	(0.152)
Voluntary Guide * Three Strikes	-0.300	0.129
	(0.272)	(0.122)
Statutory Presum. * Determ. Sent.	-1.186**	-1.142
	(0.594)	(0.722)
Statutory Presum. * Truth in Sent.	0.376	0.0944
	(0.302)	(0.251)
Statutory Presum. * Three Strikes	0.0582	0.311
	(0.452)	(0.204)
Determ. Sent. * Truth in Sent.	0.330	0.596**
	(0.358)	(0.268)
Determ Sent. * Three Strikes	0.158	-0.390
	(0.338)	(0.280)
Truth in Sent. * Three Strikes	-0.101	0.0958
	(0.276)	(0.146)
Observations	1321	1287
R-squared	0.227	0.856
Rho	0.764	0.638

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Control variables are presented in table 3.6
This table presents OLS regression with panel corrected standard errors of the log odds ratio of Black imprisonment rate per 1000 Black state population to White imprisonment rate per 1000 White state population for the years 1978 to 2004 with AR1 correction for autocorrelation and fixed effects for states for the six reforms and their interactions.

Table 3.5. Control Variables presented in Table 3.4 of the Odds of Black Imprisonment

Crime Variables	Model 1	Model 2
Violent Crimes	-	0.105***
	-	(0.037)
Drug Crimes	-	0.013
	-	(0.055)
Demographics		
Percent Black	-	-0.620***
	-	(0.010)
Percent Hispanic	-	0.202***
	-	(0.053)
Unemployment Rate	-	-0.200***
	-	(0.042)
Percent Poverty	-	-0.040
	-	-0.026
Population Density	-	-0.00546**
	-	(0.003)
Percent Urban	-	0.00527*
	-	(0.003)
Political Variables		
State House Republicans	-	0.092
	-	(0.058)
State Senate Republicans	-	-0.035
	-	(0.055)
Republican Governor	-	0.121
	-	(0.111)
Observations	1321	1287
R-squared	0.227	0.856
Rho	0.764	0.638

*** p<0.01, ** p<0.05, * p<0.1
Standard errors in parentheses

Also based on the social chain theory outlined above, it was hypothesized that the same increase observed in the odds of Black imprisonment would be observed on the odds of Hispanic imprisonment (Hypothesis 2). Tables 3.6 outlines the effects of reforms on the odds of Hispanic to White imprisonment. The models are structured the same way as in the analysis of the odds of Black imprisonment, but the results suggest important differences.

First, in the *base model* (Model 1), only determinate sentencing and three strikes laws are significantly associated with increases in the odds of Hispanic imprisonment. Four of the interaction terms were also significant and served as a mitigating effect, reducing the overall conditional main effects. The model (Model 1) explained more of the variation in the analysis of Hispanic imprisonment odds than the Black imprisonment odds, but not in the full model (Model 2). The Black imprisonment models explained 31% more of the variation and this difference was likely the result of two factors. First, due to less available data, the Hispanic models include three less years than the Black imprisonment models leading to fewer observations and, thus, less statistical power. Second, while the Black imprisonment data has generally indicated consistent high rates of disparities compared to whites, the Hispanic data has been more mixed, with some states showing low levels of disparities, while others show high levels. Additionally, while the Black imprisonment odds rose steadily throughout years, the Hispanic imprisonment odds remained relatively stable until the late 1980s and then rose steadily until the early 1990 before leveling back off and rising at a much slower rate than Black imprisonment.

As with the analysis of the odds of Black imprisonment the conditional effects should be interpreted with care as few states adopt only one reform and the interaction of the reforms can significantly change the outcome. For example, three strikes laws by themselves increase the odds of Hispanic imprisonment by 16.4%, but when taken in combination with voluntary guidelines the combo only increased the odds by 6.3% over

the average effect of indeterminate sentencing of 1.85. It should be noted that only Vermont, a state dropped from the analysis, adopted only a three strikes law.

In Model 2, *the full model,* again the conditional main effects of three strikes and determinate sentencing significantly increased the odds of Hispanic imprisonment, but additionally truth in sentencing was significant. Additionally, presumptive sentencing guidelines, while not significant by themselves, did show signs of interaction with all three of the back-end reforms. Voluntary guidelines also significantly interacted with three strikes laws, while statutory presumptive sentencing interacted with none of the back-end reforms. Again as with the Black imprisonment models, the non-significant conditional main effects may be a result of the strong interaction effects that are masking any main effect. In general, the effects of reforms are larger for the Hispanic imprisonment rates than the Black imprisonment rates. For example, while the average state adopting voluntary sentencing guidelines and truth in sentencing resulted in a 2.7% increase in the odds of black imprisonment, the same combination of reforms resulted in a 10.8% increase in the Hispanic odds. Of note is the significant interaction of statutory presumptive sentencing and determinate sentencing. This finding is consistent with this paper's hypothesis, but is counter to the expectations of some prior research. Both Stemen et al. (2006) and Zhang et al. (2009), while their research only focused on total state imprisonment, suggests the abolishment of parole may actually decrease

imprisonment. The results in this analysis suggest differently, indicating that the presence of determinate sentencing actually increased racial/ethnic disparities.[62]

In the analysis of both racial/ethnic groups the only reform that significantly increased both odds of imprisonment was truth in sentencing. This back-end reform mandates an offender serve 85% of the original sentence. This reform also received significant federal support in the mid 1990s and was adopted by a number of states. The strong effect may be a result of increased political pressure to cut into perceived rising recidivism rates. Additionally, for both analyses, statutory presumptive sentencing was not significant (though it sometimes significantly interacted with other reforms). Finally while both types of sentencing guidelines did increase the odds of Black imprisonment and determinate sentencing and three strikes did not, the opposite was true for the Hispanic odds, suggesting important differences exist. It should also be noted that the increase in the odds of both Black imprisonment and Hispanic imprisonment is not a result of a reduction in White imprisonment. In an analysis of each racial/ethnic group separately (not shown), reforms increase White imprisonment as well; suggesting the increase in the odds of imprisonment for people of color is related to an increase in imprisonment beyond the increase in White imprisonment.

[62] It should be noted that Stemen et al. (2006) and Zhang et al. (2009) did not actually test the racial composition of imprisonment and their research was intended to only highlight the effect on the total imprisonment rate.

Table 3.6. The Odds of Hispanic Imprisonment Compared to White Imprisonment

	Model 1	Model 2^
Reforms		
Presumptive Sentencing Guidelines	0.112	-0.154
	(0.183)	(0.166)
Voluntary Sentencing Guidelines	0.109	0.114
	(0.230)	(0.159)
Statutory Presumptive Sentencing	0.0971	0.168
	(0.218)	(0.179)
Determinate Sentencing	0.229***	0.240***
	(0.0202)	(0.0201)
Truth in Sentencing	0.166	0.198*
	(0.141)	(0.119)
Three Strikes	0.413***	0.356***
	(0.144)	(0.126)
Interaction Terms		
Presumptive Guide * Determ. Sent.	-0.661**	-0.513*
	(0.328)	(0.305)
Presumptive Guide * Truth in Sent.	-0.532*	-0.871***
	(0.299)	(0.295)
Presumptive Guide * Three Strikes	-0.365	-0.581*
	(0.318)	(0.325)
Voluntary Guide * Determ. Sent.	-0.423	-0.277
	(0.474)	(0.389)
Voluntary Guide * Truth in Sent.	-0.115	-0.184
	(0.252)	(0.175)
Voluntary Guide * Three Strikes	-0.386**	-0.396***
	(0.178)	(0.139)
Statutory Presum. * Determ. Sent.	-0.627*	-0.525
	(0.340)	(0.341)
Statutory Presum. * Truth in Sent.	-0.249	-0.361*
	(0.223)	(0.216)
Statutory Presum. * Three Strikes	-0.0965	-0.0476
	(0.167)	(0.176)
Determ. Sent. * Truth in Sent.	-0.410	-0.627**
	(0.283)	(0.260)
Determ Sent. * Three Strikes	0.117	0.178
	(0.379)	(0.376)
Truth in Sent. * Three Strikes	-0.0812	-0.279*
	(0.208)	(0.180)
Observations	1209	1179
R-squared	0.342	0.549
Rho	0.704	0.601

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: ^ The control variables are not presented but will be made available upon request. This table presents OLS regression with panel corrected standard errors of the log odds ratio of Hispanic imprisonment rate per Hispanic state population to White imprisonment rate per White state population for the years 1981 to 2004 with AR1 correction for autocorrelation and fixed effects for states for the six reforms and their interactions.

Table 3.7. Control Variables presented in Table 3.6 of the Odds of Hispanic Imprisonment

Crime Variables	Model 1	Model 2
Violent Crimes	-	-0.024
	-	(0.060)
Drug Crimes	-	0.223***
	-	(0.053)
Demographics		
Percent Black	-	-0.137***
	-	(0.025)
Percent Hispanic	-	0.185***
	-	(0.026)
Unemployment Rate	-	-0.211***
	-	(0.065)
Percent Poverty	-	0.129***
	-	(0.032)
Population Density	-	-0.00817***
	-	(0.001)
Percent Urban	-	0.006
	-	(0.004)
Political Variables		
State House Republicans	-	0.030
	-	(0.060)
State Senate Republicans	-	0.110**
	-	(0.049)
Republican Governor	-	0.584***
	-	(0.093)
Observations	1209	1179
R-squared	0.342	0.549
Rho	0.704	0.601

*** p<0.01, ** p<0.05, * p<0.1
Standard errors in parentheses

Discussion

Previous research indicates that sentencing reforms have increased total incarceration rates in a number of states (Albonetti 1997; Dalessio and Stolzenberg 1995; Frase 1995; Frase 2005). It should be noted that the intensity or even the presence of the unintended consequence suggested by this research is not likely to be the same from state to state and will depend on a number of social and structural components specific to each state (Fine 2006; Frase 2005). The structural components are important in understanding

the complexities of an individual state and in a sense researchers who have focused on the individual states are not wrong. Their individual state analyses allow for the inclusion and analysis of state level contexts and state specific legal and political subcultures that are key to their specific outcomes. While these analyses are justified and needed, they lack the ability to be generalized across states. This study, on the other hand, draws its power by exploiting within state variation across 49 states to make statistically broad inferences about possible unintended consequences of policy shifts on sentencing at the aggregate state level. The results here further prior research by indicating reforms are also associated with increased racial disparities on average across 49 states. The results are more generalizable than much of the earlier single-state studies and suggest a number of important considerations for future research. This analysis brings a more theoretically driven analysis than much of the earlier aggregated studies. Much of the earlier policy analysis tended to focus purely on outcomes with less attention to what might be driving the changes (Tonry and Hatlestad 1997; Webster 2007; Zhang, Maxwell, and Vaughn 2009). The theoretical model presented here helps to ground the analysis in contemporary understandings of the criminal justice system.

The central findings indicate some sentencing reforms have had a significant impact on the odds of imprisonment for Blacks and Hispanics, leading to a widening of the disparity between Whites and people of color. A finding that is counter to most stated goals (or rhetoric) surrounding the reforms and suggests a significant unintended consequence. Notably, the results occur in an analysis that includes fixed effects for states. Such models examine changes over time controlling for state-level characteristics

that are constant over time. In addition, the logarithmic growth curve measure for the effects of reforms should be a more theoretically appropriate indicator of the effects than measures used in previous studies. In general, the findings support Hypothesis 1 (increase in the odds of Black imprisonment) for the front-end sentencing guidelines and truth in sentencing on the back-end for the odds of Black imprisonment. However for the odds of Hispanic imprisonment, only the back-end reforms increase the odds, suggesting the loci of the unintended consequences for them is in the changes to the release mechanism and not the changes to the sentencing procedure (supporting Hypothesis 2). The divergent effect is curious and why this difference exists is not fully explained by this analysis. Further research is warranted and may be able to shed light on the causes. In the end there is support for *both* hypotheses 1 and 2, suggesting sentencing reforms led to an unintended consequence of increasing imprisonment of people of color more than Whites.

While this study does indicate a disproportionate unintended effect at the aggregate state level, why this disparity is exacerbated cannot be fully explained in this study. Previous research has indicated that while the discretion previously held by judges under indeterminate sentencing may have been removed from the judge, it has not been removed from the process. It is possible reforms have inadvertently given more discretionary power to prosecutors (who determine the severity of charges filed), which might reproduce or exacerbate the racial bias the reforms were intended to address (Kempf-Leonard and Sample 2001). Analysis with inclusion of racially disaggregated data for crime rates, poverty rates, and unemployment rates may be able to shed some

light onto other possible effects as well. A few studies have found that prosecutors are more likely to take into consideration factors outside of the offender's crime and prior record (directly or indirectly associated with race and/or other factors) in determining prosecution of violent and drug crimes, but less likely for property and other non-violent felony cases (Engen, Gainey, Crutchfield, and Weis 2003; Ulmer, Kurlychek, and Kramer 2007). In the end, the discretion has not been removed or reduced, but has been "displaced" from one group to another (Wooldredge and Griffin 2005).

Other research has indicated that Blacks are more likely to be convicted of a relatively minor crime (Shane-DuBow 1998). If true, an increase in the odds of Black imprisonment (and possibly Hispanics) may be linked to sentencing reforms through their greater emphasis on prior record (particularly for drug crimes) as a determinant of sentencing length (Inciardi, McBride, and Rivers 1996; McShane and Williams 1997). When this information is combined with the political desire to ratchet up the penalties on drug crimes, the connection between race and drug crimes as a driving force of a disparity in sentencing becomes more plausible. Future research should focus on determining why reforms have affected people of color more than Whites and if it is connected to prior record, specific crimes, or displaced discretion (Griset 1995; Wooldredge, Griffin, and Rauschenberg 2005).

While each of the unintended consequences outlined in Table 3.1 could represent a type at play in any given state, the focuses of this paper instead is on general unintended effects across states. While considerable variation from state to state exists in the degree and type of reform (there is *no* one size fits all). It is also likely the policy shift,

regardless of the actual structure of the policy itself, is likely to result in a latent effect, in this case, the rise in the disparity of imprisonment rates for people of color. The indication that sentencing reforms increase imprisonment disparities is not one that should be considered positive or minor (Fine 2006; Merton 1936). Finding an unintended consequence does not suggest a policy failure *per se*. After all, it is important to remember that these reforms are embedded within complex, dynamic, and interconnected social systems that can lead to what Fine (2006) called "chained social problems". Policy reforms are culturally path dependent, tied to what we can imagine, network embedded, connected to our social understandings and stereotypes, constrained by our social institutions, and related heavily to the power dynamics that surround us. What has failed here is not the desire to reform the system (a value judgment lying beyond this analysis), but the failure to adequately assess the impacts of the reforms. As Merton's theory tells us, the consequences of public actions often significantly deviate from the desired outcome (Merton 1936). This deviation was not foreseen and intended and it is important to not rely solely on the rhetoric of what is supposed to happen. This study suggests policy makers need to seriously assess and possibly reconsider sentencing reforms to alleviate the increased imprisonment disparities.

The next chapter focuses on the changes in the female imprisonment rates compared to males due to the adoption of sentencing reforms or changes in criminal arrest rates, as a compositional trend that extends over the period from 1970 through 2008. Research has shown that the gender gap in imprisonment has been narrowing over

the last four decades (Jaccard and Turrisi 2003; Steffensmeier 1993). We[63] focus our investigation on two potential explanations for part of this narrowing: the sentencing reform movements that occurred from the 1970s through the beginning of the 21st century and changes in the gender ratio of female to male for serious crimes. We hypothesize the law and order movement along with a shift in women's' roles in society combined to create an environment where female imprisonment rates rose faster than male rates. We suggest that both sentencing reforms and differential arrest rates were part of the reason the female rates rose faster.

The analysis of the next chapter draws on similar data as this chapter and the previous chapter, but here we take a slightly different approach to the modeling. First, this analysis includes a slightly different panel model; a cross-sectional time-series regression with clustered errors as opposed to the panel corrected standard errors incorporated in the previous models. Second, these models utilize period fixed effects in addition to unit fixed effects. Third, the models include some additional control variables not presented in earlier chapters.

[63] Chapter 4 was co-authored with Dr. Robert M. O'Brien

CHAPTER IV

"FIXING" THE GENDER GAP IN SENTENCING: THE EFFECT OF SENTENCING REFORMS ON THE ODDS OF FEMALE IMPRISONMENT OVER TIME

The material in this chapter will be published with Robert O'Brien as a coauthor. I collected and analyzed the data, and Dr. O'Brien prepared the methods and results sections. Both of us contributed to the introduction and discussion sections and a drafting of the final paper.

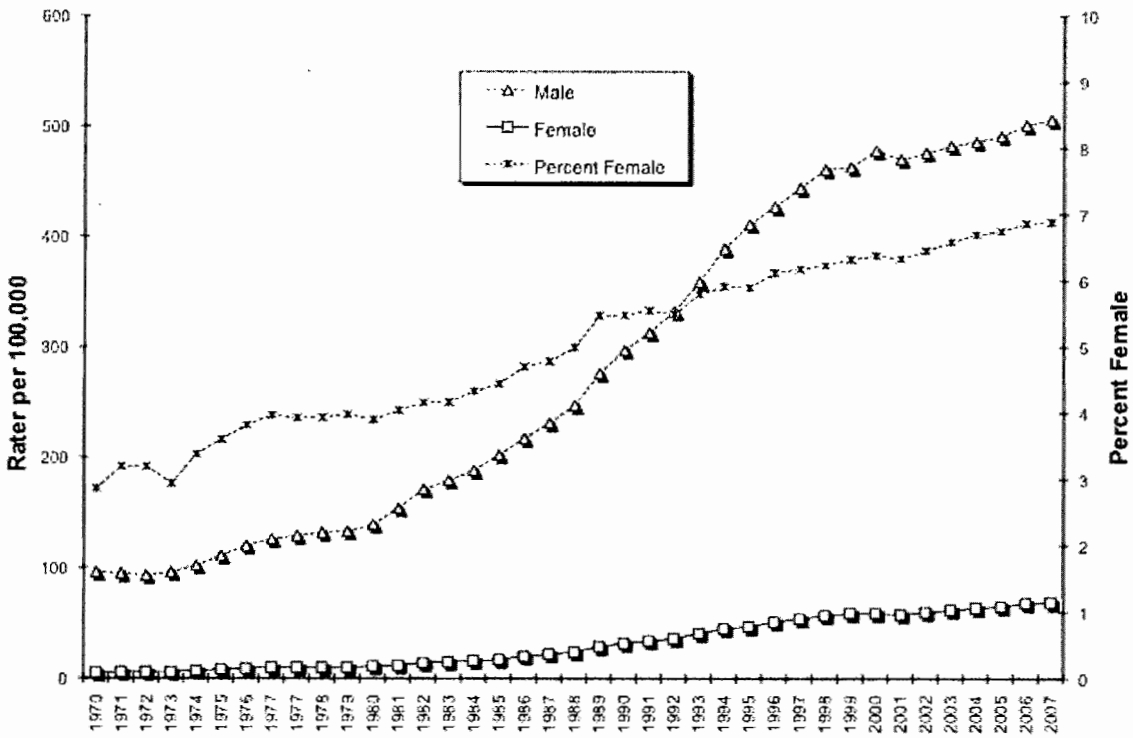
Introduction

When referring to the epidemic of youth homicide that occurred in the mid- to late-1980s, criminologists do so with a sense of astonishment at the doubling of homicide rates for those 15 to 19 years of age in less than a decade. An even more astonishing "imprisonment epidemic" occurred in the United States beginning in the 1970s. At the start of the 1970s the rates of incarceration per 100,000 U.S. residents hovered at slightly below 100 inmates per 100,000 U.S residents, but by 1980 had moved upward to 139 per 100,000. This upward movement increased to 297 in 1990 and by 2007 stood at 506 per 100,000. This amounted to an astonishing five-fold increase in the rate of incarceration over a forty-year period (Pastore and Maguire 2007).

The increased rates of imprisonment involved both men and women. The rate for men rose from 191 per 100,000 men in the 1970 to 955 per 100,000 by 2007. The rate for

women rose from 5 per 100,000 in 1970 to 69 per 100,000 by 2007. Despite the higher rise for women, throughout this period men continued to constitute well over 90% of those imprisoned. These increases are plotted in Figure 4.1 for both men and women (rates for men are represented by the triangles and rates for women by the squares and these rates correspond to the left hand axis (Pastore and Maguire 2007).

Graph 4.1. Imprisonment Rates for Males and Females and the Percentage of Those Incarcerated Who Are Females: 1970 to 2007



Over this period the percentage of those imprisoned who are women has risen markedly faster than males (represented by the stars on the stippled line in Figure 4.1). In 1970 women constituted 2.87% of those incarcerated, but by 1980 this percentage had risen to 3.90%. By 1990 the percentage of those incarcerated who were women had

grown to 5.48% and by 2007 it stood at 6.88%. This represents a more than doubling of the percentage of prisoners who were women from 1970 to 2007 (Pastore and Maguire 2007).

Although the upward trend in incarceration rates has been the focus of much literature in both scholarly venues (Garland 2001; LaFree 1984; Zimring 2001) and in the popular press. Much less attention has been paid to the female-male compositional trend in U.S. prison populations (Kruttschnitt and Gartner 2003). Our interest focuses on this compositional trend that extends over the period from 1970 through 2008. Specifically, we focus our investigation on two potential explanations for part of this increase: the sentencing reform movements that occurred from the 1970s through the beginning of the 21st century and changes in the ratio of female to male serious crimes.

The War on Crime and the Sentencing Reform Movement

The origins of the "get tough on crime" movement[64] are often linked to the decade of the 1960s and increasing urban unrest that resulted in riots, radical youth and black power movements, assassinations of top political figures, and increasing crime rates (Beckett 1997; LaFree 1998; O'Brien 2003). These factors interacted with and were enhanced by media coverage (Scheingold 1991), public concerns (Warr 1995), and the political responses to these problems. In a sense, these interactions created a perfect storm that fostered the law and order movement. In response, the federal government set up the President's Commission on Law Enforcement and the Administration of Justice in

[64] In this paper the "get tough on crime" movement and the law and order movement are treated synonymously and refer to the same general period and movement aimed at increasing the punitive nature of the criminal justice system in response to a perceived rise in criminal activity.

1969 and passed legislation setting up the Law Enforcement Assistance Administration – the LEAA – (part of the Omnibus Crime Control and Safe Streets Act of 1968). The LEAA supplied funds to law enforcement agencies to professionalize their operations and improve their record keeping (Marvell 1995).

The growing concern with crime as a social problem, helped to generate concerns about the effectiveness of the criminal justice system in general. Issues such as the amount of discretion at all levels of law enforcement, concerns that some offenders were not receiving stiff enough sentences or being released too soon, and general issues of fairness for victims and offenders in the criminal justice system took on a new salience. These issues formed some of the key elements of a sentencing reform movement, which was part of the larger law and order movement, which eventually resulted in a restructuring of sentencing laws and a more determinate sentencing structure (Blumstein, Cohen, Martin, and Tonry 1983).

Until the 1970s judges retained much discretion in the sentencing of convicted offenders and parole boards were accorded much leeway in the determination of the release of offenders. There were, of course, maximum sentences for most crimes, but often there were no minimum sentences and in almost all cases there was a relatively wide range of sentences that could be imposed by judges. Parole boards, acting independently of judges, also had limits on their discretion in terms of paroling prisoners, but these limits were often wide and typically their decisions could not be appealed. This model of sentencing reflected the rehabilitation model of justice, where the focus centered on rehabilitating offenders within prisons through work and educational

programs. The sentencing model reflected a criminal-centered approach designed to pattern punishment to each offender's rehabilitation needs to facilitate the reform process (Blumstein, Cohen, Martin, and Tonry 1983).

Blumstein et al. (1983) suggest several reasons for the sentencing reform movement, including prison uprisings (raising questions about rehabilitation), concern about individual rights and the control of discretion, demand for accountability, disillusionment with rehabilitation, disparity and discrimination in sentencing and parole, and crime control. As they note (Blumstein, Cohen, Martin, and Tonry 1983: 3), "[t]hese factors, among others, coalesced into a compelling case against indeterminate sentencing. The indeterminate sentencing system that was all but universally supported in the 1950s had few defenders by the late 1970s."

The shift away from indeterminate sentencing represented a movement toward a "justice model" of crime and punishment that was in line with the get tough on crime movement that focused on making criminals pay and deterring those who might commit future crime. In a sense, the movement took to heart the three tenets of deterrence theory: to be effective punishment must be swift, certain, and severe. In terms of policy change they were most successful in instituting the latter two tenets. The demand was for the punishment to fit the crime (be certain) and be severe enough (at least more severe than before the reforms) to deter crime and/or at least incapacitate the offender. The legal emphasis on "due process" makes it difficult to increase the swiftness of punishment. By the mid-1970s the sentencing reform movement began to have concrete effects on

legislation with the adoption of several sentencing reforms (Bohm 2006; Garland 2001; Tonry 1995).

The sentencing reforms passed by state legislatures can be categorized as: (1) reforms that focused on the front-end (those that occur at the initial sentencing of convicted offenders) and (2) reforms that occurred at the back-end (those that focus on how much of a sentence that a judge makes must be served (when and if parole can be considered) (Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009). Three major types of front-end reforms are evaluated in our research.

Presumptive Sentencing Guidelines: Table 1.1 (Page 7) shows when states passed various types of sentencing reform. In 1982 Pennsylvania passed presumptive sentencing guidelines and their lead was followed by nine others states so that by 1996 ten states had presumptive sentencing guidelines on the books. They constitute the most rigid and severe of the front-end reforms. Presumptive sentencing guidelines consist of a matrix of possible sentences with a very narrow range within each sentencing category that is defined by the offender's criminal history (prior offenses) and offense severity. Judges generally must follow the matrix, since the presumptive sentences are strict in their applications, allowing only very limited deviations (Frase 1995; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009).

Voluntary sentencing guidelines and statutory presumptive sentencing: These reforms are not as rigid as presumptive sentencing guidelines.[65] In Table 1.1, we see that California was the first state to institute statutory presumptive sentencing in 1976 and

[65] There is some discretion in categorizing these reforms, which will be discussed in the methods section of this paper.

was followed by seven other states for a total of eight states having instituted these guidelines by 1980. Voluntary sentencing guidelines were first instituted by Maryland in 1983 and by 1997 seven states had instituted these guidelines. Voluntary sentencing guidelines are not as binding as presumptive sentencing guidelines. The guidelines are treated as a formal recommendation, but the judge is not legally mandated to follow them. Even milder, in terms of rigidity, are statutory presumptive sentencing reforms – these reforms represent an attempt to create uniformity within similarly situated crimes. They specify an appropriate or "normal" sentence for each offense as a baseline for a judge (Frase 1995; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009).

A second set of reforms concentrates on how much of a sentence made by a judge must be served and, thus, they are sometimes referred to as back-end reforms. *Truth in sentencing* legislation was instituted in Illinois in 1978 and twenty-four states now have such laws. These laws require that those convicted serve a statutorily defined minimum amount of time.[66] The second major back-end reform now includes 12 states and involves the elimination of discretionary parole boards. We refer to this reform as *determinant sentencing* (Frase 1995; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009).

The final sentencing reform that we consider involves an enhanced sentencing procedure called *three strikes-laws*. Although California may be most famous for this law, there are 24 states with some form of a three-strikes law. These laws focus on

[66] In our coding of this variable we included only states meeting the 1994 federal omnibus crime bill minimum 85% time served of the original sentence.

habitual offenders who are convicted of a third felony. Typically the law suggests severe sentences (25 years to life or a life sentence) for the third felony offense (Frase 1995; Kovandzic, Sloan, and Vieraitis 2004; Lotke, Colburn, and Schiraldi 2004; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009). For a fuller explanation of these reforms see Appendix A.

Why the Increasing Proportion of Women Prisoners

There are many potential explanations for the increasing proportion of women prisoners. The two traditional explanations involve changes in the behavior of women and changes in the attitudes toward and treatment of women with regards to criminal behavior (O'Brien 2001). A more recently developed explanation involves changes to laws themselves. Sentencing reforms fit most easily into this later category because they represent a shift in the structure and application of sentencing based on changes in the law. While it is conceivable that some of the underlying reasons for sentencing reforms may result from changing attitudes towards women's behavior, the discussion here focuses on how the law impacts the odds of female to male imprisonment and not on the *why* the laws were changed.

One explanation of changes in the odds ratio of female to male involvement in the criminal justice system focuses on changes in women's societal roles and suggests that as these changes occur women's rates of crime became more similar to those of men (Adler 1975; Simon 1975; Sutherland 1947). In criminology this idea dates back, at least, to the work of Clarence Darrow (1922: 78) who wrote that as women enter "the fields of industry formerly occupied by men . . . she will be more and more judged as men are

judged, and will commit the crimes that men commit, and furnish her fair quota of the penitentiaries and jails." This theory contains elements from both of the two traditional perspectives referred to above; first, that as women's roles change their behavior will change and they will commit the crimes that men commit. Part of any change in the proportion of women's involvement in the criminal justice system that resulted from an increase in the proportion of women committing crimes may be referred to as an *offender-generated* explanation (O'Brien 2003; Schwartz and Rookey 2008; Schwartz, Steffensmeier, and Feldmeyer 2009).

Second, Darrow notes that as women's roles change they will be increasingly judged as men are judged. That is, changes in attitudes toward women offenders means that they will be less likely to be treated differently in terms of culpability or to receive "chivalrous" treatment (Visher 1983). Confounded with this change is the professionalization or bureaucratization in law enforcement that emphasizes treating (ideally) all people the same. O'Brien (1999) refers to these changes as *recording-generated changes* and Schwartz et al. (2009) refer to these changes as *changes in the law-in-action*. These are changes in policing, reporting, and tolerance of behavior. To the extent that these two types of changes differentially affect males and females, they should suspect the gender composition of those arrested and that should lead to changes in the composition of those incarcerated. In our analyses we will include the ratio of females to males arrested, but we will be unable to disentangle the reasons for changes in this odds ratio over time within states. Any changes could be due to behavioral changes or to

changes in enforcement. They could also be due, as we will discuss below, to the changes in laws-on-the-books.

A rather different explanation for changes in the composition of those arrested is in *changes in law-on-the-books*. Such changes in laws will likely mean changes in the number and, often, the characteristics of people who are caught up in the criminal justice system. When changes catch up more individuals in the criminal justice system, it is often referred to as net-widening (Cohen 1985; McMahon 1990; Steffensmeier, Schwartz, Zhong, and Ackerman 2005). When discussing net-widening Cohen (1985: 44) state: "(1) there is an increase in the total number of deviants getting into the system in the first place and many of these are new deviants who would not have been processed previously (wider nets); (2) there is an increase in the overall intensity of intervention, with old and new deviants being subject to levels of intervention (including traditional institutionalization) which they might not have previously received (denser nets); (3) new agencies and services are supplementing rather than replacing the original set of control mechanisms (different nets)."

To illustrate how net-widening can occur, a paradigmatic example can be given. Schwartz and Rookey (2008) describe a net-widening event due to changes in the law and how it affected the composition of those involved in the criminal justice system. They report that in many states the Blood Alcohol Content that defined the level of legal impairment was reduced from ".10 percent to .08 percent, or even to .02 percent for those under 21 years of age," and they note that "[p]olicies that redefine the legal criteria used to determine driver intoxication may inadvertently increase women's representation in

official statistics, without any underlying change in women's actual drunk-driving behavior" (Schwartz and Rookey 2008: 646). In this case, and in several others in the area of criminal justice, women are likely to commit less serious offenses. Thus, if one widens the net to include more minor forms of deviant behavior one is likely to catch a relatively larger portion of women and, in the process decrease the gender gap. They provide a number of pieces of evidence to show that this is the case for driving under the influence.

Net widening occurs in many ways. In the example above it is in response to a change in laws. It can also be in response to a change in how laws are enforced. Is the response of law enforcement agents chivalrous? What is the style of law enforcement: service oriented, legalistic, or as "watchmen" (Wilson 1978)? Each of these styles has implications for how many arrests occur and how serious the crime must be to warrant an arrest.

In this paper we will not be able to determine the specific mechanisms by which net-widening might change the gender composition of those incarcerated. We will not even be able to separate the effects of net-widening from the effects of differential changes in the criminal behavior of men and women: changes in the gender composition of criminal behavior. But we do expect that changes in the law and the enforcement of the laws that catch-up more people will be associated with a change in the characteristics of people who are caught up in the criminal justice system. To the extent that these changes catch up people who typically commit less serious offenses and sentence them to

longer punishment, we expect that they will have a differentially greater impact on women than on men.

When we examine the effects of the odds ratio of female to male violent crimes, property crimes, and drug crimes, we expect that the greater these ratios the greater the odds of female to male imprisonment. In particular the literature has suggested that the US drug policy "the war on drugs," has been a major influence on the increase in prison populations (Blumstein, Cohen, Martin, and Tonry 1983; Payon 2006). This war escalated after the term was coined by Richard Nixon in 1969 (Baum 1997; Inciardi, McBride, and Rivers 1996; Payon 2006). Nixon's drug wars were implemented as part of the Comprehensive Drug Abuse Prevention and Control Act of 1970 and have continued under different administrations during the period covered in this paper. Although much has been written about the differential impact of the war on drugs and the imprisonment of blacks and whites (especially with respect to differential sentencing for crack and powder cocaine), little research has examined its differential impacts on women and men. But some scholars have drawn attention to this differential (Belknap 2002; Chesney-Lind 1997; Owen 2000).

Hypotheses Relating to the Ratio of Female to Male Arrestees:

We expect that the greater the ratio of female to male arrests for violent crimes within states, the greater the odds of incarceration of females to males within states; the greater the ratio of female to male arrest for property crimes within states the greater the odds of incarceration of females to males within states; and the greater the ratio of female to male arrests for drug crimes within states the greater the odds of incarceration of

females to males within states. Given the previous literature, we expect this relationship to be especially salient for drug crimes (Chesney-Lind 1997; DPA 2003).

We also expect that sentencing reforms have contributed to the increased ratio of women to men incarcerated in the United States. As discussed above, these changes in the law are likely to widen the net of those caught up in the criminal justice system and are likely to affect women more than men, both in the number of women who are imprisoned and longer sentences (both of which would increase the rate of female imprisonment). What are the effects when discretion is taken away from judges; when mandatory minimum sentences are imposed for crimes that at one time might have led to probation; when sentences that might have at one time led to early release on parole are no longer eligible for such forms of release? To the extent that these reforms have led to harsher treatment for more minor crimes and have made it more difficult to fit sentences to extenuating circumstances (childcare, perceived probability of repeat offending) – there is good reason to expect that the reforms have differentially impacted women whose criminal behaviors are likely to be less serious than men's (Jacobs 2003; Steffensmeier and Demuth 2000).

Hypotheses Relating to Sentencing Reforms:

Given the discussion above we expect that sentencing reforms will lead to increases in the rates of incarceration of women relative to those of men. Specifically, we expect that the adoption of presumptive sentencing guidelines, voluntary sentencing guidelines, statutory presumptive sentencing, truth in sentencing, and determinant sentencing will be associated with an increase in the ratio (odds) of females to males

incarcerated in states controlling for other relevant variables (Jacobs 2003). We also examine the effects of the adoption of three strikes legislation – but are not sure how it will affect the relative rates of incarceration of females and males. Our hesitance in making a prediction in this case is, in part, due to the tendency of men to commit more serious offenses and the specific targeting of these laws to repeat felony offenders.[67] Additionally, while five of the reforms were passed over an extended period of time and involved considerable legislative debate, three strikes laws were passed in a short intense flurry of social and political pressure. Some prior research has also indicated that because of the relatively few criminals, outside of California and Georgia, to be sentenced under the law, it has had little actual substantive impact (Dickey and Hollenhorst 1999; Kovandzic, Sloan, and Vieraitis 2004).

Analytic Strategy

To test our hypothesis, we need a method that will allow us to compare rates before and after sentencing reform has been instituted. Ideally such comparisons would be made with other cases where sentencing reforms have not been instituted so that we could compare changes in rates for cases where reform occurred to changes in rates for cases where reform did not occur.

The best method available for conducting such comparisons for a large number of cases over a period of time are methods designed to analyze cross-sectional time-series or

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[67] Our research addresses most directly whether sentencing reforms are associated with changes in the ratio of male to female incarceration. The reasons for this change are more difficult to pin down. It may be simply that more women are incarcerated as sentencing targets less serious behaviors and mandates harsher sentences for less serious behavior, but it may involve other factors (*e.g.,* differences by gender in terms of plea bargaining or charges filed by district attorneys) that are more indirectly tied to sentencing reform.

what are labeled in the econometric literature as panel models (see Baltagi 1995; Wooldridge 2002). Since sentencing reforms have been enacted at the state-level, we will use states as our units (cases) and we have obtained data on imprisonment rates for men and women separately from 1970 to 2008. This gives us 1950 state-years of data for our analysis.

We are not the first to recognize the advantages of using panel analysis to study the effects of sentencing reform. Nicholson-Crotty (2004); Spelman (2009); Stemen, Rengifo, and Wilson (2006); and Zhang, Maxwell, and Vaughn (2009) have used this form of analysis to investigate the effects of sentencing reform on the total rate of imprisonment. But we know of no research, to date, that has used this technique to investigate the potential effects of sentencing reforms on the relative rates of female to male incarceration.

Since our focus is on the effects of female to male crime rates and sentencing reform changes on the odds ratio of females to males incarcerated, we use a fixed effects model that will control for all variables at the state level that are constant over time. We also control for a number of variables within states that vary over time and theoretically may be related to the ratio of female to male incarceration within states; including unemployment rates, poverty rates, population density, percent urban, percent black, percent Hispanic, Republican state senate power, Republican state assembly power, Republican governor, state revenues per capita, and state correctional spending per capita. These controls allow for testing the conditional association of the key independent variables (sentencing reforms and the ratios of females to males arrested

within states) with the gender composition of the incarcerated state populations over time. As a final control we used fixed effects for periods which controls for the association of exogenous shocks (*e.g.*, the passage of the Omnibus Crime Bill Legislation of 1994 or the national election of a Republican president or a Republican majority in the Senate) with state level passage of sentencing reforms or law enforcement in action and state-level incarceration rates. The analysis allows for the modeling of the fluctuations in the composition of incarceration within states that are greater than in other states over time. The regression coefficients represent the average effects in the fluctuation of the independent variables on the dependent variable for the average state (Finkel 1995; Halaby 2004).

Data

Dependent Variable:

Data on the number of women and men incarcerated in the 50 states for each year from 1970 to 2008 were obtained from the Bureau of Justice Statistics (U.S. 1965-1983; U.S. 1984-1998; U.S. 1999-2008).[68] We used Bureau of the Census (U.S. 1965-69, 1971-1979, 1981-1989, 1991-1999, 2001-2008; U.S. 1970, 1980, 1990, 2000) population estimates to convert the state incarceration figures for males and females into rates per 100,000 resident males and females.[69] To measure shifts in the relative size of the female

[68] Data for the years of 1978 to 1983 is available in the yearly publication: Prisoners in State and Federal Institutions on December 31, 1972 (through 1983). Data for the years of 1984 to 1998 is available in the yearly publication: Correctional Populations in the United States, 1984 (through 1998). Data for the years of 1999 to 2005 is available in the yearly publication: Prisoners, 1999 (through 2005). All three publications are produced and printed by the U.S. Bureau of Justice Statistics (see Citations).

[69] Rates per 100,000 for each variable were computed by taking the raw number of prisoners for each state by year that was provided by the Bureau of Justice Statistics and dividing by each state's female or male population per 100,000, respectively, as supplied by the Bureau of the Census (see citation).

to male population of those incarcerated, we divided[70] the female imprisonment rate by the male imprisonment rate and then logged this ratio. Logging the ratio of female to male imprisonment rates insures that our results are not dependent upon whether we take the ratio of male to female rates rather than female to male rates (the only difference would be to change the signs of the coefficients for the independent variables in our analyses.

Sentencing Reform Variables:

These variables are probably the most difficult to measure as there is no commonly agreed upon measurement (and thus their measurement warrants an extended discussion). They were drawn from several sources including the U.S. Bureau of Justice Assistance (1996), Frase (1995), Zhang, Maxwell, and Vaughn (2009), and a report by the Vera Institute (Stemen, Rengifo, and Wilson 2006). While most research is in agreement on the general outlines of the reforms discussed in this paper, there is not consensus on the exact classification of each state into particular sentencing reform categories. Thus researchers would likely disagree about whether a particular reform should be classified as a "presumptive sentencing guideline" or as a "voluntary sentencing guideline" or as something in between with a different name (Frase 1995; Marvell 1995; Stemen, Rengifo, and Wilson 2006; Tonry 1995). There is consensus, however, that within any category of sentencing reform there are differences in the specific reforms from state to state. Appendix A contains a discussion of the general function of the various reforms as delineated in this study. Because of this heterogeneity

[70] Dividing the two rates creates an odds ratio that measure the odds of female imprisonment per 100,000 to male imprisonment per 100,000.

within and between our categories of sentencing reform, we will group various sets of reforms to see if there is a relationship between the entire set of reforms and the odds of females to males incarcerated and subsets of these reforms as well as the reforms individually (as categorized by us).

All six reforms in Appendix A represent a shift from the indeterminate model to a more punitive model with a discretion-limiting sentencing or release structure. We have coded presumptive sentencing guidelines, voluntary sentencing guidelines, and statutory presumptive sentencing as mutually exclusive "front-end" reforms that may not coexist with each other or indeterminate sentencing at any given time. They are ordered from the most rigid of the front-end sentencing reforms to the least rigid. The next two reforms in Appendix A are back-end reforms that eliminate or limit discretionary release (determinate sentencing and truth in sentencing). These reforms can operate in conjunction with any of the front-end reforms. The final reform in Appendix A, three strikes laws, is a sentencing enhancement law and can go with any of the reforms above. In our analyses we will interact some of these variables to investigate whether different combinations (*e.g.*, presumptive sentencing guidelines *and* determinate sentencing) have an amplified effect on the ratio of female to male incarceration. Additionally it should be noted that these reforms were often made at different times both across and within states. For example, Oregon adopted presumptive sentencing guidelines in 1989, abolished parole in 1989, and adopted truth in sentencing in 1995 (U.S. 1996; Zhang, Maxwell, and Vaughn 2009).

Traditionally dummy variable coding has been used in most prior research to represent the period without the reform (coded with zeros) and after the reform is instituted (coded with ones). This assumes that the effect of these reforms on (in our case) the ratio of females to males imprisoned is somehow instantaneous even though most of those incarcerated were in prison before the legislation. To ameliorate this problem, we created a new sentencing measure where each state was coded as 1.00 for the first year of implementation as well as for the years before implementation and as *e* (2.71828) for years five through the last year of observation. The intervening years between one and five[71] were coded in an equal intervals manner between 1.00 and 2.71828 (an increase of .42957 per year). We then calculated the natural log of these numbers to create a variable that was zero for the years before implementation, a logged growth curve during the first five years of implementation, and then a constant of one (the natural log of 2.71828) thereafter. The rationale was that the first year of implementation would not result in a change in the gender composition of inmates, since there would not be enough time to have the new inmates incarcerated. The logarithmic growth from year one to five seems more realistic than the instantaneous full effect in gender composition implied by the use of dummy variables to code sentencing reforms.

The other key independent variables are more straightforward to code. We code the three variables representing the gender composition of arrests as the natural log of the ratio of female arrest rates to male arrest rates. Specifically, the three arrest ratios are

[71] Year five was chosen as the last year of the logged increase because the average (medium) offender is sentenced to a little less than 2.5 years. Very few offenders actually serve a sentence longer than five years making year five seem an appropriate stopping point of the logarithmic growth curve.

based on arrest rates for females and males for violent crimes (murder, forcible rape, robbery, and aggravated assault), property crimes (burglary, larceny, and motor vehicle theft), and drug crimes (distribution and possession) over time. Data on the number of female and male arrestees were obtained from the FBI (U.S. 1965-2008b) while population data for the states were obtained from the U.S. Census (U.S. 1965-69, 1971-1979, 1981-1989, 1991-1999, 2001-2008; U.S. 1970, 1980, 1990, 2000). These data allowed us to construct yearly arrest rates for females and males and their ratios.

Control Variables:

In order to determine whether the sentencing reforms had an effect on the ratio of females to males incarcerated in states (an average treatment effect), it is important to control for other variables within states that might have increased or decreased this ratio (especially those that might be correlated with our key independent variables). The Bureau of Labor Statistics (U.S. 1965-2008a) supplied information to construct data on unemployment rates, while the Census (U.S. 2006) provided data for poverty rates. Dubin (2007) supplied data on political party affiliation for state senate and house (or assembly) from which we constructed a Republican control index by centering the percent Republican around 50% so that Republican control represents positive deviations from 50% and Democratic control represents negative deviations. We then logged the absolute value of the deviations and returned the negative signs to the Democratic deviations and a zero to an evenly split body. This made the transition from Democratic to Republican control the most important part of the scale. A dummy coded variable for governor's political party was generated from data supplied by Hershey (2007). Finally,

we controlled for state revenues per capita and state correctional spending per capita (Stemen 2007). These control variables represent variables that have been shown to affect prison populations in past research (See Appendix E for descriptive statistics of variables used in this analysis) (Albonetti 1997; Arvanites and Asher 1998; DeFina and Arvanites 2002; Engen and Gainey 2000; Steffensmeier and Demuth 2000; Tonry 1994; Western 2001; Western 2005; Zhang, Maxwell, and Vaughn 2009).

Results

We first tested our panel data to see if they contained a unit root. This is accomplished by running a unit root test on the dependent variable across all of 50 panels of state data. If the panels contain a unit root this suggests that the data should be first differenced. A unit root occurs when a random shock at one period is perfectly transmitted to the next period ("perfect memory") and is particularly likely to occur when the dependent variable is based on a "stock" rather than a "flow." In our case the proportion of females to males imprisoned in a given year is largely based on the composition of the stock of prisoners the year before with a small flow component based on those who leave prison and those who enter prison. We used both the Dickey-Fuller and Phillips-Perron tests as modified for STATA, which uses a Fisher based meta-analysis that combines p-values from the independent panels to obtain an overall statistic for testing the results over all 50 panels. We used lags of two and three periods, which should be enough to model any autocorrelation that would remain after first differencing. As long as we demeaned the data as suggested by Levin, Lin, and Chu (2002) or included a drift or trend term (which is also appropriate) in all cases we reject the null hypothesis

that the panels contain unit roots at greater than the p = .001 level. Therefore, we did not first difference this panel data.

We used STATA's xtreg program in our analyses because it allows us to estimate panel models for random or fixed effects and to consistently estimate the standard errors of our coefficients even with serial correlation within panels and heteroscedasticity between panels by using the cluster option. In our analyses we use fixed effects for units (states) to control for all characteristics associated with states that do not vary over time. This is crucial because our analysis focuses on changes in the gender composition within states over time.[72] In addition the Hausman test[73] indicates that we should use fixed effects for states. We also include dummy variables for periods, because the F-test indicates significant variation over time in the predicted values of the dependent variable across periods (indicated by the F-values in the last row of Table 4.1) and because the Hausman test indicates a need to include the fixed effects for periods.[74]

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[72] Fixed effects for panels exploits within group variation by holding constant unexplained between group variations. The fixed effects estimates are unbiased even if some time invariant state level characteristics are correlated with other independent variables and the dependent variable. This is not the case in random effect models when these correlations are present. In our analyses the unit (state) fixed-effects model offers significant advantages over the random effects model (Halaby 2004)

[73] The Hausman test can be used to assess the likelihood that a key assumption of the random effects model is correct: that no unobserved time invariant state characteristics are correlated with the independent variables in the model *and* the dependent varaible. If this assumption is not correct, then the coefficient estimates based on the random effects model will be biased. Our results, based on the Hausman test, indicate that using the random effects model would result in biased coefficients and, thus, we use the fixed effect model for states in all of our analyses (Baltagi 1995).

[74] This Hausman test assesses whether period characteristics are correlated with the independent variables in the model *and* the dependent variable. If they are, not including fixed effects (dummy variables) for periods would result in biased coefficient estimates. This Hausman test compares the fixed effects for states model with the fixed effects for states and periods model (with both models including other time varying independent variables), the test indicates that we should include fixed effects for periods.

Table 4.1 contains the results of our analyses. We begin with a baseline model that contains the log of the ratio of female to male arrest rates for violent crimes, the log of female to male arrest rates for property crimes, and the log of female to male arrest rates for drug crimes. Each of these has been suggested as a potential reason for the increasing incarceration rates of females relative to males. Perhaps somewhat surprisingly, only the logged ratio of female to male arrest rates for the drug crimes is significantly related to the logged ratio of female to male incarceration rates for these 50 states across the period 1970 to 2008. Since both the dependent and independent variables are logged, we can interpret the coefficient associated with the gender composition of drug crime arrests as indicating that a 1% change in this ratio is associated with a .341% change in the ratio of females to male incarcerated.

Table 4.1. The Change in the Odds of Female to Male Imprisonment Over Time

Crime Variables	Model 1	Model 2	Model 3	Model 4
Violent Crime	0.071	0.071	0.069	0.040
	(0.062)	(0.067)	(0.060)	(0.058)
Property Crimes	0.007	0.011	-0.015	-0.119
	(0.116)	(0.112)	(0.118)	(0.089)
Drug Crime	0.341***	0.306***	0.283**	0.184*
	(0.097)	(0.099)	(0.095)	(0.089)
Reforms Variables				
Pres. Sent. Guidelines	-	-0.153	-0.172	-0.184^
	-	(0.121)	(0.114)	(0.107)
Voluntary Sent. Guidelines	-	-0.115	-0.131	-0.145
	-	(0.087)	(0.090)	(0.093)
Statutory Presumptive Sent.	-	0.149	0.167	0.181
	-	(0.129)	(0.129)	(0.125)
Determinate Sentencing	-	0.078	0.088	0.090
	-	(0.125)	(0.119)	(0.115)
Truth in Sentencing	-	-0.117^	-0.114*	-0.119*
	-	(0.065)	(0.058)	(0.054)
Three Strikes	-	-0.021	0.013	0.074
	-	(0.083)	(0.085)	(0.074)
Control Variables				
Percent Black	-	-	-0.006	-0.005
	-	-	(0.006)	(0.005)
Percent Hispanic	-	-	-0.007^	-0.011**
	-	-	(0.004)	(0.003)
Percent Unemployment	-	-	-0.008	-0.003
	-	-	(0.010)	(0.005)
Percent Poor	-	-	0.001	-0.001
	-	-	(0.007)	(0.005)
Population Density	-	-	-0.001	-0.001
	-	-	(0.001)	(0.001)
Percent Urban	-	-	0.001	-0.001
	-	-	(0.001)	(0.001)
Republican. State House	-	-	0.009	0.012
	-	-	(0.012)	(0.012)
Republican State Senate	-	-	0.0012	-0.001
	-	-	(0.012)	(0.011)
Rep Governor	-	-	-0.002	-0.001
	-	-	(0.026)	(0.024)
State Revenues	-	-	0.000	-0.000
	-	-	(0.000)	(0.000)
Linear Period Effect	-	-		0.034***
	-	-		(0.004)
Intercept	-2.744***	-2.799***	-2.715***	-69.72***
	(0.232)	(0.224)	(0.291)	(7.883)
Observations	1880	1880	1880	1880
R-squared	0.666	0.680	0.686	0.672
F(38, 49)	46.10***	50.83***	29.28***	-

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05 ^ p<0.10

Model 2 adds the sentencing reform variables to the analysis. None of them is statistically significantly associated with the gender composition of prisoners at the .05 level, but the adoption of truth-in-sentencing laws is marginally significant at the .10 level for a two-tailed test. We note that this coefficient is negative (not the hypothesized direction). Controlling for these sentencing reforms changes the relationships for the variables in Model 1 only slightly. Only the ratio of females to male drug crime arrestees is statistically significant (here at the .01 level) and it relationship is just slightly less strong than in Model 1: a one percent change in this variable is associated with a .306% change in the gender composition of the incarcerated population.

Model 3 adds a long list of control variables that have been found to be associated with imprisonment in past studies and all of them are measured for each state yearly from 1970 to 2008: percent black, percent Hispanic, unemployment rate, percent living below the poverty level, population density, percent urban, Republican control of the assembly/house, Republican control of the senate, Republican governor, state revenues per capita, and correctional spending per capita. None of these variables are significantly related to the gender composition of those incarcerated in the states, although percent Hispanic is significant at the .10 level and negatively related to the ratio of females to males incarcerated. Again truth-in-sentencing is marginally significant at the .10 level and its coefficient is nearly identical to Model 2. The logged ratio of female to male drug arrests remains the only statistically significant variable in predicting the logged ratio of rates of females to males incarcerated at the state level.

We tried several other auxiliary analyses. We interacted each of the front end sentencing reform variables (presumptive sentencing, voluntary sentencing, and statutory sentencing) with each type of back end reform (determinate sentencing and truth-in-sentencing) – for a total of six interaction terms. None of these interactions were even marginally statistically significant. We use an F-test to see if the front end, back end, or all of the reform variables together were statistically significant and found that they were not. For the time period covered in this analysis, we find only one of the variables that we examined to be significantly related to the gender composition of those incarcerated; the gender composition of those arrested for drug crimes. None of the sentencing reform variables has a statistically significant relationship with the gender composition of those incarcerated over this period.

Some authors (Beck and Katz 1995; Halaby 2004; Wooldridge 1995) worry about controlling for differences between periods using dummy variables for each period (with the exception of the reference period). We know from the F-test at the bottom of Table 4.1 that in each of our models the 38 dummy variables for periods account for a statistically significant amount of variability in the dependent variable controlling for all of the other independent variables in each model. We also know from Hausman tests that fixed effects for time should be used rather than using no control for time trends. Model 4 represents a more modest approach to modeling the trends across periods: it contains a linear trend for periods. We settled on this modeling of period variation because when we added quadratic and cubic period effects they did not significantly improve the fit of the model (they were not statistically significant). Additionally, we found that with no

dummy variables for period Model 3 has an R^2 within panels of .585 when these dummy variables are added to the model the R^2 within panels is .686 (as reported at the bottom of Table 4.1). When we run Model 3 with no dummy variables for periods, but with the linear trend for periods, we find that the R^2 within panels is .672 (as reported in Model 4). Model 4 seems to do almost as well as including all of the dummy variables for period in terms of modeling the trends within panels. Finally, when we use a Hausman test to compare Model 3 to Model 4 we find that both models are appropriate in terms of providing consistent estimates. While we might still prefer the model with dummy variables for periods (Model 3) because of its rigorous controls, Model 4 may be more appropriate because it does not "wash out" as much variation and is likely more efficient. When we interpreted the results from Model 4, we still find that the effects of drug crimes (ratio of female to male) were significant, but now the effects of truth-in-sentencing and percent Hispanic are both statistically significant. And, of course, the linear effect of period is positive and statistically significant at the .001 level in this no period dummy variables model. Each increase of one year in time is associated with a 3.458 percent increase in the ratio of females to males incarcerated – the annual rate of increase [(exp(0.034) – 1) × 100].

Discussion

Net-widening, for us, involves an increase in the proportion of people who are caught up in the criminal justice system. It is often accompanied by a shift in the demographic composition of the people caught up in that system. In terms of arrest rates there was a trend upward in both violent crimes from 1970 to the early 1990s with a

substantial drop after that date and for property crime rates there was an increase from 1970 to the early 1980s followed by a period of fluctuating rates with a definite downturn after the early 1990s. In the case of arrests there has been an active literature on changes in the gender gap for serious violent and property crimes (for example, O'Brien 1999; Schwartz, Steffensmeier, and Feldmeyer 2009; Steffensmeier 1980; Steffensmeier 1993). Overall the literature suggests a slight narrowing of the arrest rates gap between males and females for some of these crimes.

Drug crime arrests have increased for both males and females over the period covered by this study, but have been growing at a significantly higher rate for women. America's 25-year war on drugs has had a massive impact on the criminal justice system. Drug arrests have tripled since 1980; as a result, the number of all inmates imprisoned for drug offenses in 2000 equals the total number of *all* inmates in U.S. prisons *and* jails in 1975. Research has shown that the rates of women going to prison due to drug offenses have increased more rapidly than for men (DPA 2003; Greenfeld and Snell 1999). Though men still represent the majority of drug arrests, women now make up the fastest growing sub-group of the population going to prison because of drug crimes. In the mid-1990s the proportion of women in prison for drug crimes was double that of men (Greenfeld and Snell 1999).

In our analyses, we find that the gender composition of those arrested for serious violent and property crimes in states is not significantly associated with the gender composition of those incarcerated in the states, but the gender composition of those arrested for drug crimes is associated with the gender composition of those incarcerated.

The effect is strong and consistent across all of the models used in our analysis including Models 3 and 4, which should provide consistent estimates in the face of both unit and period time invariant variables. This finding reinforces the perspective that the war on drugs is in part a war (even if unintentionally) on women (Belknap 2002; Owen 2000). Of note is research that has shown that the war on drugs was also racialized and affected blacks and Hispanics more than whites. It is likely that the rise in female imprisonment has not been the same across racial/ethnic groups; in fact research has shown that black women's imprisonment rates are the single fastest growing group (Griffin and Wooldredge 2006; Owen 2000; Steffensmeier 1993). With Black women more likely to be involved in drug crime arrests (Spohn, Welch, and Gruhl 1985), future research should focus on testing the possible connection between the war on drugs and the very rapid increase in the imprisonment of women of color.

We did not find, however, that the sentencing reforms examined in this study are associated with the ratio of females to males incarcerated in states over this time period. This finding contradicts our own expectations and those of others. For example, Jacobs (2003:3) states that the increase in women's imprisonment "corresponds directly to the mandatory minimum sentencing laws in effect since the early 1970s. Since more women are convicted on non-violent, drug related crimes than for any other, these sentencing policies have had a particularly profound effect on women."

The contradictory patterns may be a result of the choice of our dependent and key independent variables. Our dependent variable does not assess directly the ratio of female to male incarcerations for drug crimes. If it did, we might well find that the

sentencing reforms are related to differences in this ratio. We also have not coded our key independent variables on sentencing reforms with an eye towards their treatment of drug offenses. Such a coding of reforms might well find that reforms specifically and harshly targeting drug crimes were related to the ratio of female to male incarcerations. Such an investigation is a topic for future research.

Net-widening has certainly occurred in the area of incarceration and that increase is graphically portrayed in Figure 4.1. Also portrayed is the differential increase of the proportion of women incarcerated over time that has accompanied this increase. One reason that some have cited for the increase in imprisonment is the institution of sentencing reforms that have accompanied the war on crime (Steffensmeier and Demuth 2000). To the extent that these reforms have contributed to net-widening, we might also expect that they have contributed to changes in the gender composition of those incarcerated (Greenfeld and Snell 1999). An important finding of this research is that we find no evidence of this for any of the front-end reforms, the back-end reforms, or for three strikes laws. We also examined whether combinations of these front-end and back-end reforms were associated with the changes in the gender composition of offenders at the state level. The results were negative in each case. What this research does find is strong evidence that the war on drugs has contributed to a narrowing of the gender gap in incarcerations.

The next chapter addresses the relative "risk" that a state will adopt a sentencing reform. While this chapter and the previous two substantive chapters deal with the effects of sentencing reforms on a component of imprisonment, the next chapter shifts the

focus to which state-level environmental factors are associated with the rate of reform adoption. The analysis used a number of social, criminal, demographic, and political variables to model the change in reform adoption rates across all 50 states from 1972 to 2008. Through event-history modeling, the analysis assesses the change in the relative risk of reform adoption due to changes in the state-level variables.

The next chapter diverges from the previous chapters on two fronts. First, instead of relying on a larger meta-theory to explain the expected outcome, the analysis focuses on building a number of small arguments for why a number of factors may simultaneously impact reform adoption. The smaller theoretical postulates are tested through a series of variables that are expected to impact the rate of reform adoption. Second, this chapter's analysis employs event-history analysis. This technique has the advantage of being able to model the risk of the shift from the indeterminate-rehabilitation model to the more predetermined discretion-limiting-determinate model, while being able to assess the change in the risk rate when state-level characteristics are controlled for.

CHAPTER V

INCREASING THE REFORM RISK: MODELING SOCIAL, DEMOGRAPHIC, AND POLITICAL CHARACTERISTICS THAT INCREASE THE RATE OF REFORM ADOPTION

Introduction

In the late 1960s and early 1970s, critics began to advance a number of critiques of the justice system, including complaints about ineffectiveness of treatment and reform programs, cozy correctional facilities, and rampant repeat offending (Hebert 1997; Reitz 1995). Critics focusing on sentencing charged that it allowed far too much disparity in sentencing types and time served. In response, beginning in the 1970s, legislators (or voters) passed a series of sentencing reforms with more pre-determined structures that limited officials' discretion during sentencing or release (Clarkson and Morgan 1995; Griset 1995; Hebert 1997). Prior to these reforms, indeterminate sentencing dominated the United States criminal justice system. This model of justice focused on rehabilitating offenders within prison through work and education programs (Frase 1995; Roberts 1996). Reflective of the rehabilitation goal, judges (and in a few jurisdictions, the jury) were given wide discretion, sentencing offenders to loose ranges (*e.g.,* 5 to 25 years) to

facilitate the rehabilitation process. Parole boards acting independently of other parts of the judicial system and largely independent of legislators then determined if the prisoner had been properly rehabilitated and was ready for release (Albonetti 1997; Kempf-Leonard and Sample 2001; Marvell 1995). The sentencing model reflected a criminal-centered approach designed to pattern punishment to the offender's rehabilitation needs. But under sentencing reforms, the model became a crime-centered approach where the judge is reduced to handing down pre-determined sentences based on limited information like the offense type and prior record. The end result was a shift to a crime-centered warehousing model of punishment with significant reduction in the rehabilitation focus.

This paper assesses the relative likelihood that a state adopted one of four sentencing reforms passed after 1972 when controlling for state-level characteristics. I considered two "front-end" reforms, sentencing guidelines and statutory presumptive sentencing, which are considered independent and mutually exclusive reforms that focus on limiting the actions of judges. These may not, at any given time, coexist with each other or indeterminate sentencing (the previous sentencing system). Conversely, on the "back-end", the analysis also considered two reforms that either limit (truth in sentencing) or eliminate (determinate sentencing) the use of discretionary release. The back-end reforms can operate alongside the "front-end" reforms. While states adopting reforms often incorporated numerous types (*e.g.,* Oregon which adopted presumptive sentencing guidelines in 1989, abolished parole in 1989, and then adopted truth in sentencing in 1995), their coexistence is not absolute and were often instituted in

different years (See Table 5.3 for complete list of reforms). I also assess the over-all rate of reform adoption cutting across the four reforms in an *all-reform* model. This later analysis is designed to give the general effect across all reforms and will serve as a base with which to compare the analysis of the separate reforms (Frase 2005; Stemen, Rengifo, and Wilson 2006; U.S. 1996).

The aim of the study is to determine which social, political, and demographic state characteristics are likely to increase the rate of sentencing reform adoption. Using event-history analysis, the impact of state-level characteristics can be modeled on the rate of sentencing reform adoption over time.[75] The numerous crime and justice, demographic, and political variables have been theorized in prior research to impact the criminal justice system. The goal of this study is to shed light onto the broad patterns of state-level variation, measured through the state-level variables, had on the adoption of reforms. The analysis will further allow for the comparison of the relative magnitude of the impact and how it changed depending on the reform being adopted.

Given certain characteristics, it is possible that a state is more likely to adopt a reform and these different characteristics are likely to affect reforms in different ways. In the end, the results suggest that the adoption of sentencing reforms are impacted by a number of key crime, demographic, and political characteristics. Chief among them, the analysis suggests that high rates of drug crime arrests, percent black, unemployment rate, and Republican control of state politics significantly increase the rate of reform adoption.

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[75] Or said another way, the analysis can model the "risk" a state will adopt a sentencing reform and what effect state-level characteristics have on the rate (*e.g.,* how the variables may increase or decrease the relative risk).

Literature Review

With a few notable exceptions, the previous research assessing sentencing reforms largely fall into two categories. The first focuses on extra-legal effects like the influence of race or gender on judicial departures from baseline sentence recommendations (e.g. Griffin and Wooldredge 2006; Johnson 2006; Kramer and Steffensmeier 1993; Kramer and Ulmer 1996; Steffensmeier, Ulmer, and Kramer 1998; Ulmer and Kramer 1998; Wooldredge and Griffin 2005; Wooldredge, Griffin, and Rauschenberg 2005; Wooldredge 2009), while the second focuses on the influence of policy changes on sentencing outcomes (e.g. Arvanites and Asher 1998; Brewer, Beckett, and Holt 1981; Dalessio and Stolzenberg 1995; Frase 1995; Frase 2005; Griset 1996; Johnson 2006; Tonry 1995). In both cases the body of literature largely utilized data on individual states, counties, or cities. While providing important insights, they are limited to the specific context and sentencing structures of the particular jurisdiction and lack broad cross-sectional applicability. While a few studies analyzed year-to-year changes over time, these studies have also largely been limited to a single state,[76] calling into question their generalizability beyond the single case.[77]

Smaller subsets of studies have focused on policy assessment. These studies are less concerned with the social or political impact of reforms and are more concerned with describing the structural impact of reforms (Nicholson-Crotty 2004; Spelman 2009;

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[76] A few studies have also assessed the federal system (*e.g.,* Albonetti 1997).

[77] Previous analysis in this study indicated that considerable state-to-state variation in imprisonment rates, which further calls into question the generalizability of the single state approach.

Stemen, Rengifo, and Wilson 2006). An even smaller subset focused on explaining the social contexts that influenced the adoption of reforms. This study follows this later group of policy papers. Some of these studies, (for example work by Spohn and Holleran (2000) and Steffensmeier, Ulmer, and Kramer (1998)) found that race, ethnicity, gender, and age are all important covariates with sentencing outcome and sentencing type. These micro-level studies show the power these demographic factors have on sentencing outcomes. I shift the focus from the micro-level onto a larger aggregate state-level analysis. Following a study by Spelman (2009), which incorporates similar data and suggests that state revenues are the primary driving factor in policy adoption, our research suggest reforms are not adopted in response to this factor alone and state context is highly influential in the adoption of reforms.

This paper does not draw on one all encompassing theoretical perspective to build an expected outcome. Instead, it uses a series of variables grouped into broad categories that are likely to be associated with the adoption of sentencing reforms. Within each group of variables a brief discussion of the prior research is provided and an expected outcome is hypothesized. The discussion does not advocate one particular theoretical frame over another. Instead it outlines possible effects and the relative significance of their impacts. The end product is an exploration of the various possible impacts, finding whether they are significantly associated with sentencing reform adoption, and their relative impact in comparison to the other variables included in the analyses. The paper begins with a discussion of the state characteristics modeled in the analysis and why they

are expected to influence the adoption rate, followed by an outline of the models included, and finally a discussion of the findings.

Independent Variables and Hypotheses

The independent variables in this analysis represent the social, political, and demographic characteristics of a state that may have influenced the adoption of one or more of the sentencing reforms. Each characteristic or group of characteristics is outlined below and brief discussions of the possible impacts are given (See Appendix E for descriptive statistics of variables used in this analysis).

Imprisonment:

Beginning in 1972 the America imprisonment rate began an unprecedented and steady increase. State governments responded to the perception that crime was on the rise and the general public was unsafe[78] and began to devise various policies to combat it, among them sentencing reforms. Across most political perspectives and clearly across the political ideology of the two dominant political parties, a "get tough on crime" culture prevailed (Griset 1995). Specific criminal and public policy legislation has increased the overall punitiveness of sentences, causing a significant portion of the nearly 550% increase in imprisonment over the period of time covered in this study (Bohm 2006). Imprisonment has been tied to sentencing with some prior research showing that states may be able to better control staggering climbs in overcrowding by adopting sentencing reforms (Dalessio and Stolzenberg 1995).

[78]Gottschalk (2006; 2009) notes that prior to the late 1960s crime rarely registered as a top public issue and not until the law and order movement did the public begin to demand substantial change.

Others have argued that imprisonment is tied to sentencing through socio-political connections that exploit public fears of crime to fuel a drive towards a repressive and punitive justice system (Abramsky 2007; Beckett 1997; Simon 2007). Scheingold (1991) suggests "policy changes are mostly likely to occur when political leaders, in part by taking their cues from the media, choose to play upon public anxieties that are themselves inflamed by media imagery and vicarious victimization rather than by crime as such" (Scheingold 1991:44). This complex circle of interactions served to promote and sustain the politicization of crime characterized by the "politics of fear" that stressed "getting tough on crime," resulting in the model of punishment featuring among other things, sentencing reforms (Beckett 1997; Simon 2007). Given the politics of the law and order movement characterized by the politics of fear, Hypothesis 1 states:

> *States with high rates of imprisonment will be more likely to adopt sentencing reforms.*

The hypothesis draws from the assumption that states that are already punitive in their imprisonment practices will be more likely to adopt a reform. This variable represents state imprisonment rates per 100,000 and was obtained from the Bureau of Justice Statistics (U.S. 1965-1983; U.S. 1984-1998; U.S. 1999-2008)[79]. This data allows for the measurement of the impact that state imprisonment has on the likelihood a state adopts a sentencing reform. Additionally, a number of prior studies point to concerted

[79] Data for the years of 1965 to 1983 is available in the yearly publication: *Prisoners in State and Federal Institutions* on December 31, 1972 (through 1983). Data for the years of 1984 to 1998 is available in the yearly publication: Correctional *Populations in the United States*, 1984 (through 1998). Data for the years of 1999 to 2008 is available in the yearly publication: *Prisoners*, 1999 (through 2008). All three publications are produced and printed by the U.S. Bureau of Justice Statistics (see Citations). Rates per 100,000 for each variable were computed by taking the raw number of prisoners for each state by year that was provided by the Bureau of Justice Statistics and dividing by state population per 100,000 as supplied by the Bureau of the Census (see citation).

efforts by policy makers to connect overcrowding and reforms. They suggested that sentencing reforms were adopted after the spike in sentencing and would be effective in curtailing a continued rise (Boerner 2001; Tonry and Hatlestad 1997; Von Hirsch 1985). Thus high rates of imprisonment would be associated with higher rates of reform adoption.

Arrests for Violent and Drug Crimes:

The violent crime arrest rates have been shown to be an important contributor to both the rise in imprisonment rates (particularly in the 1980s) and the adoption of sentencing reforms. One of the primary justifications for reforms given by public policy makers was that they were needed to counter a perceived growth in violent crime in America (Abramsky 2007; U.S. 1996; Von Hirsch 1985). Of course this argument is not unique to sentencing reforms, but is particularly relevant in this case. Drug crimes are also important to consider. During the late 1970s and early 1980s the U.S. stepped up legislation and enforcement against possession and use of drugs. Part of the push for sentencing reforms was to increase the focus on drug crimes (Inciardi, McBride, and Rivers 1996; McShane and Williams 1997). The FBI's Uniform Crime Report (UCR)[80] supplied the data on violent crime[81] and drug crime arrests per 100,000 used in this analysis. Because of the important impact these two crime variables have on the justice system and their likely influence in the adoption of reforms, Hypothesis 2: states:

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[80] Data from the Uniform Crime Report (UCR) is available in print and online from the U.S. Dept. of Justice, Federal Bureau of Investigation and is produced and printed yearly (see Citations).

[81] Violent crime arrests represent the Uniform Crime Reports indexed crimes, which include the offenses of murder, forcible rape, robbery, and aggravated assault.

States with high violent crime and drug crime arrest rates will be more likely to adopt sentencing reforms.

The South:

The South occupies a unique place in the study of the criminal justice system. Research has consistently shown that the South is more violent and more punitive than the majority of the rest of the nation (Clarke 1998). The South[82] has a long history of both direct and indirect racism (Clarke 1998; Jacobs and O'Brien 1998; Karnig and Mcclain 1985; Messner, Baller, and Zevenbergen 2005). Studies of the former Confederate states in the period after the Civil War reveal a time dominated by direct civil violence targeted at Blacks. The violence was not limited to, but often took the form of lynching. Lynching was highly prevalent and nearly exclusive to the South until the 1930s, when less overt forms of violence began to replace it. From the 1930s to the 1960s a steady decline in lynching occurred, and by the mid-1960s lynching became practically non-existent (Jacobs and O'Brien 1998; Karnig and Mcclain 1985; Messner, Baller, and Zevenbergen 2005). Some researchers have theorized that the history of violence in the South targeted at Blacks has led to cultural, political, and social conditions that make the South unique in the study of criminal justice. Racism and violence are not limited to the South, but found its most savage and enduring forms in the South (Crow and Gertz 2008; Engen and Steen 2000; Griset 1995).

Some researchers have argued that informal social control in the form of violence targeted at minorities in the South (*i.e.* lynching) has simply been replaced by formal

[82] The South includes the states of Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia; these states represent the south as defined by the Bureau of Justice Statistics (see Citations).

state sanctioned controls. As overt civil violence in the South steadily declined in the 20th century, the use of the criminal justice system as a form of social control of Blacks in the South grew (Crow and Gertz 2008). The highest per capita rates of execution occur in the South with Blacks' having disproportionately higher rates; black males are particularly more likely to be executed (Clarke 1998; Harries 1988; Messner, Baller, and Zevenbergen 2005).

A number of competing theories have attempted to explain this disparity. Popular in early research, Durkheim's theory of legal mechanisms of social control gained considerable initial support. This theory suggests that as legal forms of social control gain acceptance, extra-legal controls such as lynching will decrease. This may help to explain why the South has significantly higher rates of imprisonment. But empirical testing of this theory has produced mixed results, offering both support and considerable doubt concerning its validity (Clarke 1998; Jacobs and Carmichael 2001). Wolfgang and Ferracuti (1967) suggest an alternative theory that the South's higher rates of imprisonment are a result of a subculture of violence in the south.[83] Because of the punitive nature of sentencing reforms and the history of violence and repression in the South, Hypothesis 3 states:

States in the South will be more likely to adopt sentencing reforms.

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[83] Wolfgang and Ferracuti's (1967) theory is often referred to as the Subculture of Violence. They argue that the South has a cultural and societal structure that supports violence. They suggest that this pervasive ethos cuts across generations, age, class and sex in a way that violence becomes a part of being a Southern woman or man and that the South has come to accept violence as a natural part of life. It can be shown that homicide and violent crime rates are significantly higher in the South (Harries 1988; Messner et al. 2005), suggesting that high imprisonment rates and execution in the South are a direct result of significantly higher rates of violence.

Race and Ethnicity:

Research focusing on Blacks indicated that until the middle of the 1970s being Black had a direct and significant effect on sentence outcomes; this was particularly true for violent crimes (Hebert 1997; Johnson 2003; Walker, Spohn, and DeLone 2004). In the South, it was found that when a rape victim was white and the offender was black, the sentence was three times more likely to be death (when a sentence of death was legal) (Clarke 1998). A more recent examination has produced different results. Direct racial discrimination that dominated findings by researchers in the first half of the twentieth-century has been largely replaced by a focus on indirect and contextual evidence of discrimination (Doob 2000; Quillian 1995). Research indicates that race is significantly associated with educational level, type of crime committed, likelihood of being in poverty, and likelihood of being unemployed. Inciardi, McBride, and Rivers (1996) point out that during the increased criminalization of drugs, crack users received longer sentences than powder cocaine users when the amounts were relatively equal and that race is directly linked to type of cocaine used (crack cocaine is more prevalent among black users and powder cocaine is more prevalent among white users). In these situations, while race is not directly linked to type and length of sentence, race is linked indirectly through sentencing differentials in crime classifications and demographic considerations (Arvanites and Asher 1998; Pettit and Western 2004; Ulmer and Kramer 1998; Western 2001).

> Hypothesis 4 states: *States with high percentages of Blacks or Hispanics will be more likely to adopt sentencing reforms.*

Hypothesis 4 finds considerable support in the racial threat literature. The theory suggests that minorities are perceived as a "threat" to the majority population (Jacobs and Carmichael 2001; McDonald and Carlson 1994). Research has shown that Blacks are seen (overtly or contextually) as "criminals," particularly as "violent criminals" and as more Blacks enter a community, the perception of physical and social threat increases (Plumper and Troeger 2007; Spohn and Holleran 2000; Spohn, Welch, and Gruhl 1985). The perceived threat does not need to be related to a real threat, and thus minorities may be at increased risk of imprisonment simply because they make up a larger portion of the population (Berndt 2003; Jacobs and Carmichael 2001; Quillian 1995). The perceived "threat" can take the form of either a threat of higher criminal activity and thus minorities will be targeted as "the criminals" or a threat to social, political and/or economic dominance. Minorities could be targeted by the criminal justice system as a form of social and economic control that sentencing reforms help to make possible (Kramer and Steffensmeier 1993). To test the relative impact that the concentration of people of color in a state may have on reform adoption percent black and percent Hispanic variables were constructed. The data used for the variables were obtained for each state over time from the U.S. Census (U.S. 1965-69, 1971-1979, 1981-1989, 1991-1999, 2001-2008; U.S. 1970, 1980, 1990, 2000).[84]

Unemployment and Poverty:

While a large number of studies have pointed to the connection between criminal activity, race, and the increased punitiveness of the criminal justice system, a few studies

[84] Data from the census is available online at census.gov maintained by the U.S. Bureau of the Census and was compiled from both census data and population estimates (see Citations).

have found that poverty and unemployment are also important driving forces for the law and order movement (Western and Pettit 2000; Worrall 2008), suggesting they may be significantly associated with the likelihood a reform is adopted. This prior research suggests that class and the social pressure that poverty and unemployment place on individuals make them more likely to participate in criminal activity. This argument would then suggest that states with high rates of unemployment and poverty are more likely to adopt sentencing reforms in response and thus Hypothesis 5 states:

> *States with high unemployment or poverty rates will be more likely to adopt sentencing reforms.*

Robert Agnew (1992) has argued that individuals will feel strain due to the actual or anticipated failure to achieve positively valued goals, actual or anticipated removal of positively valued stimuli, and actual or anticipated presentation of negative stimuli leading to interpersonal strain. The theory is referred to as Social Strain Theory. In essence the theory suggests strain emerges from negative feeling towards others because individuals are not expecting to be treated well or they are not being treated the way that they want to and thus they will lose their positive belief in others (Rosenfeld and Messner 1994). Therefore in their anger and frustration with things like unemployment and poverty (or seeing those people unemployed or in poverty as problems) the public will be more likely to support sentencing reforms as a punitive measure that punishes and deters. To measure the unemployment rate and poverty rates for each state over time data was collected from two different sources; U.S. Bureau of Labor Statistics (1965-2008a) supplied information to construct unemployment rates, while the U.S. Bureau of the Census (2006) provided the data for poverty rates.

Urban and Population Density:

Also drawing from Social Strain Theory, Burton, Cullen, Evans, and Dunaway (1994) argue that in addition to unemployment and poverty, being placed in an environment that is crowded, sometimes overcrowded, will create strain. The strain may come from actual or perceived struggles for resources and opportunities or the strain can be a more general feeling of social pressure (*e.g.*, feeling lost in the crowd or boxed in). The strain will in turn, in addition to leading to more criminal activity in places with high population centers (*e.g.*, urban settings), will also lead to increased demands to do something about the system (Boerner 2001; DeFina and Arvanites 2002; Rosenfeld and Messner 1994). Thus similar to unemployment and poverty, hypothesis 6 states:

> *States with high population density or percent urban will be more likely to adopt sentencing reforms.*[85]

Politics:

Both liberals and conservatives championed the reform agenda; conservatives saw this as an opportunity to limit the powers of what they called "activist judges" who used their power to be lenient. Liberals used the argument as a perceived opportunity to limit the discretion of judges who might be using their power to be overly arbitrary and punitive (Bohm 2006; Clarkson and Morgan 1995; Doob 2000; Griset 1996). Across most political perspectives and clearly across the political ideology of the two dominant political parties, a "get tough on crime" perspective prevailed (Dalessio and Stolzenberg 1995; Kovandzic 2001). While research has shown that being tough on crime and

[85] The U.S. Census supplied the data for percent urban and population density and is available online at census.gov maintained by the U.S. Bureau of the Census and was compiled from both census data and population estimates (see Citations).

supporting tough sentencing reforms cuts across political perspectives, Republicans have generally been more stalwart in their pushing of reforms (Abramsky 2007; Barker 2006; Gottschalk 2009; Griset 1995; Scheingold 1991). Therefore Hypothesis 7 states:

> *States with more Republican state legislative power and/or have a Republican governor will be more likely to adopt sentencing reforms.*

Republican governor was measured with a simple dummy code and was supplied by Hershey (2007). The political influence of Republican control of the state legislators was measured by a unique variable constructed specifically for this analysis. With data supplied by Dubin (2007), both the variables for state house and state senate were constructed by centering the percent Republican around 50% so that Republican control represents positive deviations from 50%, while Democratic control represents negative deviations. The absolute value of the deviations were then logged, with the negative sign returned for the democratic control, to create a logarithmic scale with positive and negative deviation from zero to represent the diminishing returns of political party concentrations. Positive scores represent Republican control, while negative scores represent Democratic control.

Financial Resources:

A recent debate has emerged in response to an article written by William Spelman (2009) in which he argues that state revenues are the single most important driving factor determining why imprisonment rose more in certain states. His argument suggests that as state's tax-bases increased they were afforded the opportunity to spend money in areas they could not do so in the past and they directed more money to law and order. While

his argument has drawn criticism (Gottschalk 2009), this article suggests that states with higher revenues will put more resources into the criminal justice system and presumably sentencing reforms, thus Hypothesis 8 states:

States with high state revenues will be more likely to adopt sentencing reforms.[86]

Prior Reforms:

This analysis will ultimately test the likelihood of adopting one of four sentencing reforms. Additionally, a general assessment of whether a state implemented any reform was performed. For the four independent reforms, it should be noted that a state could only adopt only one of the front-end reforms, either sentencing guidelines or statutory presumptive sentencing. They can, however, additionally adopt one or both of the back-end reforms (determinate sentencing and/or truth in sentencing) in addition to the front-end reforms. While states adopting reforms often implement numerous types, their coexistence is not absolute and different reforms were often instituted in different years (See Table 5.1) (Frase 2005; Stemen, Rengifo, and Wilson 2006). Because states are likely to incorporate more than one reform and because states may be more likely to adopt a reform if they have previously done so, Hypothesis 9 states:

States that have already adopted a sentencing reform will be more likely to adopt an additional sentencing reform.

Table 5.1 outlines a few of the key descriptive statistics for the independent variables included in this analysis. The percentile scores will become important in the

[86] Data used to construct the state revenue variable was supplied by Stemen (2007) and was found online through the Inter-university Consortium for Political and Social Research (see Citation).

analysis as they can highlight the relative effect the change in the independent variable on the rate of reform adoption.

Table 5.1. Descriptive Statistics for the Independent Variables

	Mean	St Dev.	25th Per	50th Per	75th Per
Crime & Justice Controls					
Imprisonment Rate	223.622	162.612	92	173	328
Violent Crime Arrest Rate	156.8338	115.292	77.507	129.029	207.716
Drug Crime Arrest Rate	272.446	209.335	109.622	238.034	388.369
Demographic Controls					
South*	-	-	-	-	-
Percent Black	10.288	9.304	3.130	7.801	15.042
Percent Hispanic	5.685	7.868	1.067	2.457	6.679
Percent Unemployment	5.059	2.417	3.300	5.000	6.400
Percent Under Poverty	12.895	4.330	9.700	12.00	15.328
State Population Density	139.427	183.367	30.833	74.781	149.990
Percent Urban	66.922	8.837	55.772	69.318	81.233
Political Controls					
Republican State House	-.853	2.434	-2.971	-1.882	1.792
Republican State Senate	-.760	2.499	-3.037	-1.609	1.998
Republican Governor*	-	-	-	-	-
State Revenues	359079.7	239502.1	219517.1	321616.3	420762.6

* Indicates a dummy variable

Methods

This study tests nine different hypotheses. Some of these hypotheses include more than one variable and they should be seen as having multiple sub-hypotheses, one for each of the variables. Utilizing state-level data (data available for 50 states) covering each year between 1972 and 2008, this study tests the relative rate of adopting sentencing reforms over time through event-history analysis. This technique has the advantage of being able to model the relative "risk" a state has of adopting sentencing reforms over time. To this end, the dependent variables are a series of dummy codes representing the adoption of sentencing reforms. Because I am primarily interested in the social, demographic, and political environment that leads to increased risk of adoption, I code

the first year a state adopted a particular reform the year the law was passed, even if the new law did not have a substantive effect until a few years after the reform was adopted (Xie 1994). Table 5.2 outlines the four key dependent variables representing sentencing reforms. In addition to these four reforms, a general assessment of the risk of adopting any reform (collapsing across these 4 reforms) will also be assessed, resulting in five different substantive analyses to test the nine hypotheses.

Table 5.2. Reforms Included in Chapter V

Reform	Description
Sentencing Guidelines*	They consist of a matrix of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) and offense severity.
Statutory Presumptive Sentencing	They represent an attempt to create uniformity within similarly situated crimes, but act less like a sentencing rubric. It specifies an appropriate or "normal" sentence for each offense as a baseline for a judge.
Determinate Sentencing	Determinate sentencing is used to refer to a system *without* discretionary parole boards.
Truth in Sentencing	They require offenders serve a statutorily defined minimum amount of time. Only states meeting the 1994 federal omnibus crime bill minimum 85% time-served of original sentence are considered.

* The analysis includes only a general variable for all sentencing guideline states and not two separate variables for presumptive and voluntary guidelines. The decisions to only include the combined variable lies mostly in the lack of observations when the variable is separated and the general consensus that the same general effects of both types are likely present.

Another important sentencing reform passed during the period covered in this analysis was three strikes laws. While the reform was passed with much fanfare and early support, research has shown three strikes has had little impact beyond a handful of states that included Florida, Georgia, and California (Dickey and Hollenhorst 1999; Kovandzic, Sloan, and Vieraitis 2004). But even in these states the impact was limited to a small subset of individuals making the effect on the general population small. In this analysis it was decided not to include three strikes as the reform was passed under unique

social and political pressures not likely affecting the other reforms in the same way. First passed in 1992 by the state of Washington, three strikes gained considerable traction due to a handful of highly publicized criminal cases that fueled intense pressure to adopt it. In three short years 23 more states would adopt three strikes laws (Kovandzic, Sloan, and Vieraitis 2004). This short intense period of policy shifts likely operated outside the social and political environmental factors included in this analysis and differentiated it from the analyses of the other reforms. Thus three strikes laws were not included.

The list of reforms was compiled from numerous sources including the Bureau of Justice Assistance (1996), Frase (2005), Zhang, Maxwell, and Vaughn (2009), and a report by the Vera Institute (Stemen, Rengifo, and Wilson 2006). While most research is in agreement on the general goals of the reforms, considerable variation in the design and application of the reforms are present and little consensus on how to classify them into appropriate groups exists (Frase 2005; Marvell 1995; Stemen, Rengifo, and Wilson 2006; Tonry 1995). Appendix C is a detailed discussion of the general function of the various reforms and how they are delineated in this chapter. Table 5.3 on the other hand outlines when the various reforms were adopted and by which state, highlighting the considerable variation present in both the year and the combinations in which a reform was adopted.

Table 5.3. Distribution of Sentencing Reforms Across the United States as of 2008 for Chapter V

State	Sent. Guide.	Stat. Presum.	Det. Sent.	Truth In Sent.
Alabama	-	-	-	-
Alaska	-	1980	-	-
Arizona	-	1978	1994	1994
Arkansas	1994	-	-	-
California	-	1976	-	1994
Colorado	-	1979	-	-
Connecticut	-	-	-	1995
Delaware	1987	-	1990	1990
Florida	1983	-	1983	1995
Georgia	-	-	-	1995
Hawaii	-	-	-	-
Idaho	-	-	-	-
Illinois	-	-	1978	-
Indiana	-	1977	-	-
Iowa	-	-	-	1996
Kansas	1983	-	-	1993
Kentucky	-	-	-	-
Louisiana	1992	-	-	-
Maine	-	-	1976	1995
Maryland	1983	-	-	-
Massachusetts	-	-	-	-
Michigan	-	-	-	1994
Minnesota	1980	-	1982	1993
Mississippi	-	-	1995	1995
Missouri	1997	-	-	1994
Montana	-	-	-	-
Nebraska	-	-	-	-
Nevada	-	-	-	-
New Hampshire	-	-	-	-
New Jersey	-	1977	-	-
New Mexico	-	1977	-	-
New York	-	-	-	1995
North Carolina	1994	-	1994	1994
North Dakota	-	-	-	1995
Ohio	1996	-	1996	1996
Oklahoma	-	-	-	-
Oregon	1989	-	1989	1995
Pennsylvania	1982	-	-	1991
Rhode Is.	-	1980	-	-
South Carolina	-	-	-	-
South Dakota	-	-	-	1996
Tennessee	1989	-	-	1995

Table 5.3. (continued)

State	Sent. Guide.	Stat. Presum.	Det. Sent.	Truth In Sent.
Texas	-	-	-	-
Utah	1993	-	-	1985
Vermont	-	-	-	-
Virginia	-	-	1995	1995
Washington	1983	-	1984	1984
West Virginia	-	-	-	-
Wisconsin	1985	-	-	-
Wyoming	-	-	-	-
Total	17	8	12	24

Note: Table 5.3 represents the current sentencing type used by each state as of 2008. Presum. Guide. stands for presumptive sentencing guidelines. Vol. guide. Stands for voluntary sentencing guidelines. Stat. Presum. Sent. Stands for statutory presumptive sentencing. Deter. Sent. stands for determinate dentencing. Truth in sent. stands for truth in sentencing. Three strikes refers to three strikes laws. All other states utilize indeterminate sentencing.

All four reforms represent a shift from the indeterminate-rehabilitation model to a more punitive model with a discretion-limiting sentencing or release structure, along with the "all reform analysis," they are then modeled against a series of independent variables to assess the change in risk status associated with a specific state characteristics (independent variables). In this analysis it is assumed the pressure to adopt a sentencing reform is continuous (though not necessarily at the same rate) over the entire period of study. Public perceptions of a failing criminal justice system first became an important social issue in the early 1970s. Therefore, while particular social, political, or demographic characteristics may alter the risk that a state will adopt a reform; states were assumed to be continually "at risk" of reform adoption. I use a piecewise constant exponential event-history analysis technique that models reform adoption as a continuous-time (within the three separate time periods) discrete-state stochastic process. That is, a model in which the probability of the event (reform adoption) is continuous over time and where each state occupies only one of a finite number of discrete

conditions (two possible conditions; reform or no reform) at all times (Eldelman 1990). In event-history analysis, one models the rate at which states adopted sentencing reforms (also known as the "instantaneous transition rate"), which is defined as "the transition probability over a unit of time where the unit is infinitesimal" (Carroll 1982). The rate is not directly observable, but it can be calculated from observations of the timing of events.

In the analysis of policy shifts, event-history analysis has several advantages over conventional statistical analysis techniques. First, because I am interested in explaining the environment in which a state is more likely to adopt the reform rather than explaining the current distribution of sentencing reforms or how those sentencing reforms may impact the environment, event-history analysis supplies a technique particularly appropriate for measuring such an outcome. Thus the technique models reform adoption as a dynamic process, allowing specification of state characteristics that change over time and affect adoption rates. Second, event-history analysis allows for changing values of exogenous variables (*e.g.*, the state unemployment rate or Republican control of the Senate) over time and for changing impacts of the exogenous variables over time. Third, in modeling the rate of reform adoption, event-history analysis takes into account right censoring in the data; that is, the waiting time since first observed or since the last event is taken into account in establishing the instantaneous transition rate (Eldelman 1990; Wu 2008; Xie 1994).

In the end, event-history analysis is appropriate for the study of sentencing reform adoption, because sentencing reform is a discrete state dependent variable. That

is the risk of reform adoption is continuous over time, but is intertwined with changing political, social, and demographic characteristics of the states resulting in differential adoption rates. While, in general, event-history analysis is appropriate for this study, not every event-history model is appropriate. First, while models included data recorded only once per year over 37 years, the data measurement is assumed to be continuous over time. This assumption is made because it is assumed that each variable will be relatively stable within years. With this assumption it is possible to use exponential event-history modeling, but the exponential model also assumes the rate is proportional year-to-year across the data (Xie 1994). That is, the relative risk that a state will adopt a reform will be the same each year. If this assumption cannot be met, the simple exponential model cannot be used (Wu 2008).

Fortunately, in the exponential model, if the differential risk rates are era dependent, a relatively simple procedure can be instituted. Called the piecewise method, the differential risk proportions are corrected for by including dummy codes for the appropriate eras (Carroll 1982). In this analysis, it is likely the risk of reform adoption will vary over three distinct time periods. The first runs from 1972 to 1985. This constitutes the early years of reforms and is marked by both high political pressure for the adoption of reforms and rising crime rates. The second period runs from 1986 to 1993. This period marks the height of the crack cocaine epidemic and also was a period of high public demand for reforms. The third period runs from 1994 to 2008. This is the post omnibus crime bill period and is a time where high public demand for reforms and high public perceptions of crime began to subside. These dummy codes for the eras in

question allow the exponential event-history model to be correctly specified (Reitz 1995; Stemen, Rengifo, and Wilson 2006).

Results

The analysis below includes five separate dependent variables; each designed to test a different configuration of sentencing reforms. The first analysis consists of *all reforms* adopted during the period of study, including sentencing guidelines, statutory presumptive sentencing, determinate sentencing, and truth in sentencing. Three strikes laws are often included in analyses of this type, but were excluded as the reform has both a unique structure (deals with habitual offenders) and a unique social and political history revolving around their adoption.[87] Therefore, it was decided not to include them in the overall analysis or as a separate reform. Following the all reforms analysis, four separate event-history models were constructed to test, separately, the effects of the four reforms considered in this analysis.

All Reforms:

For the all reforms analysis, four models are presented. The first three models are designed to test three main categories of expected effects, which were the crime and justice characteristics model, the state demographic characteristics model, and the

[87] Due to their focus on habitual offenders, three strikes laws are considered an additional reform beyond the previous more structured reforms affecting sentencing or release. They too fall under the umbrella of sentencing reforms because of their design to limit discretion through an apparent push away from the rehabilitation model. But unlike the other reforms, three strikes laws were passed in a relatively short and intense political movement in response to a few highly publicized criminal cases. The intense pressure resulted in an unprecedented passage of three strikes laws in a very short two-year period of time starting in 1992 (Dickey and Hollenhorst 1999; Lotke, Colburn, and Schiraldi 2004). Research has generally found that three strikes laws have had little substantive impact (Boerner and Lieb 2001; Griset 1995; Kovandzic, Sloan, and Vieraitis 2004, Spohn 2000).

political characteristics model, respectively. Model 1 in Table 5.4 outlines the change in the base rate of reform adoption when imprisonment rate, violent crime arrests, and drug crime arrests are controlled for. As hypothesized (Hypothesis 2), it was expected that drug crime arrests would be an important contributor to the rate of reform adoption and in Model 1 drug crime arrests do significantly increase the rate of adoption. Conversely, both violent crime arrests and imprisonment rates were not significantly associated with reform adoption. The lack of significant impacts of imprisonment rates and violent crime rates on reform adoptions runs counter to Hypothesis 1. The models also included two dummy variables, one for 1986-1993 and one for 1994-2008 and were included because it was expected that the base rates for the adoption of reforms would vary by era (DeFina and Arvanites 2002; Greenberg and West 2001). As indicated in the model, the two variables are significant, suggesting that something about the risk of adoption is higher in these two periods than in the period of 1972-1985.

Table 5.4. All Reforms

	Model 1	Model 2	Model 3	Model 4
Crime & Justice Controls				
Imprisonment Rate	0.0128			0.0182
	(0.010)			(0.014)
Violent Crime Arrest Rate	0.00141			-0.00031
	(0.001)			(0.001)
Drug Crime Arrest Rate	0.00145**			0.00331***
	(0.001)			(0.001)
Demographic Controls				
South		-1.040*		-1.011
		(0.590)		(0.738)
Percent Black		0.0829***		0.0656*
		(0.026)		(0.039)
Percent Hispanic		-0.00342		-0.0315
		(0.028)		(0.040)
Percent Unemployment		0.685***		0.669***
		(0.065)		(0.075)
Percent Under Poverty Line		-0.184***		-0.164**
		(0.066)		(0.071)
State Population Density		-0.000933		-0.000114
		(0.001)		(0.001)
Percent Urban		0.00755		0.0117
		(0.008)		(0.009)
Political Controls				
Republican State House			0.138	0.427***
			(0.116)	(0.126)
Republican State Senate			0.123	0.313**
			(0.118)	(0.128)
Republican Governor			0.0237	-0.525
			(0.338)	(0.366)
State Revenues			0.0479	-0.117
			(0.080)	(0.161)
Time Controls				
Years 1986-1993	4.287***	2.778***	5.092***	2.206***
	(0.558)	(0.503)	(0.478)	(0.593)
Years 1994-2008	5.410***	5.438***	6.279***	4.265***
	(0.486)	(0.365)	(0.414)	(0.657)
Constant	-9.468***	-9.710***	-8.856***	-9.744***
	(0.376)	(0.840)	(0.367)	(1.054)
Log Likelihood	63.89	18.77	32.62	97.39
Observations	1705	1718	1690	1690

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Model 2 tests the effects of the demographic controls. A number of the controls significantly changed the rate of adoption. As expect (hypothesis 4), percent black significantly increased the rate. This may suggest that high percentages of Blacks represent a threat to the white majority and thus reforms are being adopted as a way to control this perceived threat (Johnson 2003; Quillian 1995). This argument is not supported for Hispanics, as their percentage of the population did not significantly increase the rate. Percent unemployment also is associated with the rate of reform adoption, lending support for the social strain argument. Conversely, percent under the poverty level actually significantly decreased the rate of reform adoption. This finding was unexpected and runs counter to social strain theory. Another curious finding was the significant reduction in the rate of adoption the South had. This again was unexpected given the socio-historical evidence that the south has a tendency to be more violent and more punitive (Clarke 1998; Karnig and Mcclain 1985).

One interesting finding in this analysis was the 35% reduction in the rate of reform adoption for the 1986-1993-era control when state demographics are controlled for. This reduction may be explained by the strong expected effects the demographics of a state for this era had over the other two periods. One of the key driving forces for the era was the perceived rise in a dangerous underclass and the specter of the crack cocaine epidemic. Many of the indirect effects of these two constructs (the underclass and crack cocaine) are encapsulated in the state demographics. For example, research has shown that Blacks, particularly young black men were highly connected to the "crack cocaine epidemic" and were often targeted under the reform movement (Baum 1997; Beckett,

Nyrop, Pfingst, and Bowen 2005; Berndt 2003; Engen and Steen 2000; Mosher and Akins 2007).

Model 3 includes only the four political controls. Oddly, on their own, none of the four variables increased the rate of reform adoption. This, of course, runs counter to Hypotheses 7 and 8 as it was expected that Republicans would be more likely than democrats to adopt sentencing reforms. This analysis may suggest that the social pressure to adopt reforms as a tool to combat a perceived rise in criminal activity cuts across political perspectives. The fact that state revenues also were not significantly associated with the rate of adoption runs counter to the prior findings by Spelman (2009), who suggested high state revenues were the chief driving force behind the imprisonment binge.

Model 4 is the full model, including all of the crime, demographic and political controls. This model is the most appropriate theoretical model as it allows for the simultaneous testing of the various theories and their hypothesized effects. The results for Model 4 are also presented in Table 5.5 as the full model, but this table also includes three reference points to give the coefficients some context. With exception of the two dummy codes for the eras and Republican governor, a predicted change in the rate of reform adoption for each coefficient was calculated using their 25^{th}, 50^{th}, and 75^{th} percentile score. These scores can be used to give the average change in the rate of adoption for each percentile for the average state. While some of the variables significantly increase or decrease the rate, their impacts vary greatly across the percentiles. For example, unemployment at the 25^{th} percentile increased the rate of

adoption by a factor of 9.095, but the rate increased by a factor of 72.356 at the 75^{th} percentile or about an eight-fold increase.

Table 5.5. All Reforms with Percentile Ranks for Model 4

	Full Model	**25 Percentile**	**50 Percentile**	**75 Percentile**
Crime & Justice Controls				
Imprisonment Rate	0.0182	1.048	1.157	1.316
	(0.014)			
Violent Crime Arrest Rate	-0.00031	.933	.893	.840
	(0.001)			
Drug Crime Arrest Rate	0.00331***	1.36	2.133	3.609
	(0.001)			
Demographic Controls				
South	-1.011	-1.011	-1.011	-1.011
	(0.738)			
Percent Black	0.0656*	1.228	1.668	2.682
	(0.039)			
Percent Hispanic	-0.0315	.967	.926	.810
	(0.040)			
Percent Unemployment	0.669***	9.095	28.361	72.356
	(0.075)			
Percent Under Poverty Line	-0.164**	0.204	0.140	0.081
	(0.071)			
State Population Density	-0.000114	.996	.991	.982
	(0.001)			
Percent Urban	0.0117	1.920	2.250	2.587
	(0.009)			
Political Controls				
Republican State House	0.427***	0.281	0.448	2.149
	(0.126)			
Republican State Senate	0.313**	0.387	0.604	1.869
	(0.128)			
Republican Governor	-0.525	.592	.592	.592
	(0.366)			
State Revenues	-0.117	.733	.664	.599
	(0.161)			
Time Controls		**Rate Change**		
Years 1986-1993	2.206***	9.079		
	(0.593)			
Years 1994-2008	4.265***	71.164		
	(0.657)			
		Base Rate		
Constant	-9.744***	0.000		
	(1.054)			
Log Likelihood	97.39			
Observations	1390			

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

In the Full Model as with Model 1 in Table 5.4, drug crime arrests significantly increase the rate of adoption, again supporting Hypothesis 2. But as in Model 1, violent crime arrests and imprisonment rates were not significantly associated with an increase in the rate of reform adoption. Percent Black and percent unemployed continued to significantly increase the rate of adoption, while percent under the poverty line significantly reduced the rate. Differing from Model 1, in the full model, when crime and justice variables and political variables are controlled for, the South is no longer significant. Also in the full model, Republican control of the state house and state senate now significantly increase the rate of adoption, suggesting that while the social and political pressure to adopt reforms may cut across political parties, Republicans are more likely to adopt reforms. The results for the political controls in the full model lend some support for Hypothesis 7, but not Hypothesis 8.

One conspicuous observation worth noting is the large effect (a full 71.164 increase in the rate) the 1994 to 2008 dummy code has in the model. This may be a result of the strong effect the 1994 omnibus crime bill had on the adoption of truth in sentencing laws. The law allocated billions of dollars to state governments as long as they adopted truth in sentencing with a mandated 85% of the original sentence being served.

Sentencing Guidelines:

While the *all reform* analysis gives a general picture of how state characteristics affected reform adoption across all four reforms included in the analysis, it is possible the characteristics that affect one reform differ from the characteristics that affect another.

The differential effects may be particularly likely between the front-end and the back-end reforms as they affect different areas of the process. I will begin the discussion with the analysis of sentencing guidelines adoption. This reform is generally referred to as the most "radical" of reforms as it has the most structurally deterministic design and has led to the largest of substantial change in the process of sentencing (U.S. 1996; Zhang, Maxwell, and Vaughn 2009).

Only the Full Model, the same model as Model 4 in Table 5.4 and Full Model in Table 5.5, is presented in Table 5.6 for each of the separate reforms. An additional control for prior reforms is also included in this analysis. The prior reform control is designed to indicate if and when a state adopted a reform prior to sentencing guidelines. This dummy code is expected to be highly influential with the potential of significantly increasing the rate of sentencing reform adoption (Frase 1995). As seen in Model 1 of Table 5.6, the base rate is increased 4.702 by the presence of a prior reform, lending support for Hypothesis 9 (see Table D.2 in Appendix D for a more detailed table of the analysis presented here).

As with the all reform analysis, drug crime arrests significantly increase the rate of sentencing guideline adoption, suggesting that states with a large number of drug arrests will adopt reforms. For example, when controlling for other state characteristics, states at the 75 percentile are 3.12 times more likely to adopt sentencing guidelines. The analysis may suggest the war on drugs was a key component in the push for sentencing guideline adoption (Gottschalk 2006; Mosher and Akins 2007). Percent Black does not significantly increase the rate of adoption, while percent Hispanic significantly reduces

the rate. This finding is counter to the expected outcome and suggests an outcome counter to the racial threat argument (Hypothesis 2). As with the all reform analysis, percent unemployed increased the rate of adoption, while percent under the poverty line decreased the rate.

Percent unemployment's relative strength is a notable finding. For sentencing guidelines, the analysis suggests that unemployment, across all three reported percentiles is the single largest influence on the rate of adoption. Unemployment's influence is even more notable when considering that the controls for the era will take into account differential rates of adoption for 70s and early 80s when unemployment nationally was relatively high (England, Kilbourne, Farkas, and Dou 1988; U.S. 1965-2008a).

Again, Republican control of the state legislatures, both the house and the senate, significantly increased the rate of sentencing reform adoption. This suggests that while the political pressure to adopt reforms is likely to be high for both major parties, the political and cultural identity of Republicans increased the rate of adoption more; again, supporting Hypothesis 7.

Table 5.6. Individual Reforms

	Model 1	Model 2	Model 3	Model 4
	Sent. Guide.	**Stat. Presum.**	**Deter. Sent.**	**Truth in Sent.**
Crime & Justice Controls				
Imprisonment Rate	0.00477	-0.00207	0.0161	0.0098
	(0.018)	(0.108)	(0.021)	(0.016)
Violent Crime Arrest Rate	0.00305	0.00666*	-0.00328	-0.000518
	(0.002)	(0.004)	(0.002)	(0.001)
Drug Crime Arrest Rate	0.00388***	0.000978	0.00457*	0.00398***
	(0.002)	(0.003)	(0.002)	(0.001)
Demographic Controls				
South	0.567	-10.94	-0.512	-2.837*
	(1.047)	(33.270)	(1.389)	(1.149)
Percent Black	-0.0267	-0.337	0.0602	0.0741
	(0.061)	(0.217)	(0.072)	(0.050)
Percent Hispanic	-0.351***	0.224**	-0.208*	-0.185***
	(0.131)	(0.095)	(0.114)	(0.062)
Percent Unemployment	0.523***	0.873***	0.480***	0.181
	(0.119)	(0.239)	(0.148)	(0.213)
Percent Under Poverty Line	-0.230**	-0.320	-0.0579	0.0885
	(0.113)	(0.255)	(0.134)	(0.083)
State Population Density	-0.00166	0.00891**	-0.00563*	-0.00228
	(0.002)	(0.004)	(0.003)	(0.001)
Percent Urban	0.00983	-0.00751	0.0191*	0.012
	(0.011)	(0.026)	(0.011)	(0.013)
Political Controls				
Republican State House	0.439***	0.982***	0.540**	0.293**
	(0.190)	(0.324)	(0.236)	(0.143)
Republican State Senate	0.427***	-0.0122	0.659***	0.109
	(0.192)	(0.335)	(0.224)	(0.156)
Republican Governor	-0.692	-3.287*	0.651	0.787
	(0.551)	(1.977)	(0.719)	(0.510)
State Revenues	-0.133	1.444	-0.095	-0.555***
	(0.151)	(0.820)	(0.254)	(0.217)
Prior Reform	1.548*	-11.82	4.967 ***	1.897***
	(0.836)	(48.670)	(1.162)	(0.578)
Time Controls				
Years 1986-1993	2.399***	-21.56	-0.850	0.505***
	(0.848)	149.880	(1.264)	0.147
Years 1994-2008	1.463	-27.34	-0.922	3.935***
	(1.227)	47.990	(1.419)	(0.139)
Constant	-8.522***	-15.52***	-11.130	-12.46
	(1.498)	(3.799)	(1.960)	(1.752)
Log Likelihood	29.770	21.492	15.337	76.135
Observations	1715	1769	1767	1754

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Note: Sent. Guide. stands for sentencing guidelines. Stat presum. Stands for statutory presumptive sentencing. Truth in sent. stands for truth in sentencing. Det. Sent. stands for the elimination of parole boards.

Statutory Presumptive Sentencing:

The analysis of statutory presumptive sentencing differs substantially from the analysis of sentencing guidelines. A finding that is curious considering both reforms represent changes on the front-end of sentencing (Stemen, Rengifo, and Wilson 2006; U.S. 1996). It was expected that the findings would be more similar than they are. While both sentencing guidelines and statutory presumptive sentencing are front-end reforms, shifting sentencing to a more predetermined structured system, they are substantially different in their functional application. Statutory presumptive sentencing served as more of a baseline recommendation and had less actual restructuring of sentencing than sentencing guidelines, where the grid makes the reforms very rational and bureaucratic (Nicholson-Crotty 2004; Stemen, Rengifo, and Wilson 2006; U.S. 1996).

The differences begin in the crime and justice variables. For statutory presumptive sentencing violent crime significantly increased the rate of reform. In fact, the impact of violent crime on the rate of statutory presumptive sentencing adoption is profound where the rate increase was 42.229 at the 75^{th} percentile (see Table D.3 in Appendix D for a more detailed table of the analysis presented here), almost a 10 fold increase over the rate increase for the 25^{th} percentile. The impact of violent crime arrests on statutory presumptive sentencing adoption may be a result of the time in which this reforms adoption rate was the highest, the late 1970s and early 1980s. This was a period of time when crime rates were actually on the rise and this reform may have been influenced more heavily by the rising crime rate than the other reforms.

Differences in the demographic variables can also be seen between sentencing guidelines and statutory presumptive sentencing. While the South and percent Black are not significant for statutory presumptive sentencing, and percent Hispanic decreased the rate for sentencing guidelines, for statutory presumptive sentencing, percent Hispanic significantly increased the rate. Additionally, percent unemployment was significant, with a relatively strong effect. While percent under the poverty line did not affect the rate.

State population density was significant, increasing the rate. This finding supports Hypothesis 6 for statutory presumptive sentencing, but in no other models does population density increase the rate of reform adoption. This is the only analysis in which population density is positive and statistically significant. This is the only analysis in which the variable Republican Governor is significant, and the analysis suggests the rate of the reform adoption was significantly reduced when a Republican governor was in office. This analysis may suggest that highly populated states, which also happen to have more Democratic governors, are adopting statutory presumptive sentencing at a higher rate (Barker 2006). Furthermore, Republican control of the state house (or assembly) significantly increased the rate of statutory presumptive sentencing adoption, which was similar to the previous reforms, while Republican control of the senate was not significant.

Another important distinction between sentencing guidelines and statutory presumptive sentencing lies in the era and prior reform variables. For prior reforms, it was hypothesized (Hypothesis 9) that states adopting a prior sentencing reform would be

more likely to adopt statutory presumptive sentencing, but in this analysis (Model 2 of Table 5.6) prior reform does not increase the rate. This finding may reflect the relatively early implementation of this reform and the relatively few states that actually had a reform prior to statutory presumptive sentencing. In fact, in most cases statutory presumptive sentencing preceded other reforms (Spohn 2000; Stemen, Rengifo, and Wilson 2006; U.S. 1996). Finally, the era dummy codes are not significant, suggesting the rate is proportional across the time periods.

Determinate Sentencing:

In the first of the two separate analyzes of the back-end reforms, the determinate sentencing model (Model 3 of Table 5.6) revealed results similar to those observed in the sentencing guidelines analysis. Determinate sentencing represented the first attempt by lawmakers to limit the indeterminateness of sentencing, but unlike sentencing guidelines or statutory presumptive sentencing, the focus of determinate sentencing is not on the sentence, but in the process of release. In general, it refers to a system without discretionary parole boards (Frase 2005; U.S. 1996).

The result from the full model suggests, like some of the previous models, drug arrests significantly increased the rate of determinate sentencing adoption (see Table D.4 in Appendix D for a more detailed table of the analysis presented here). The effect was strong for higher drug arrest rate states, but the effect was observed across all levels. Again percent Black and the South were not significant predictors of the rate of reform adoption, while percent Hispanic significantly reduced the rate. For determinate sentencing, the analysis lends no support for Hypotheses 3 and 4.

Conversely support for Hypotheses 5 and 6 are present. Unemployment, like sentencing guidelines and statutory presumptive sentencing, significantly increased the rate of adoption with large increases in effect size as unemployment rates go up. Hypothesis 3 is not supported for percent under the poverty line. While population density unexpectedly lowered the rate of reform adoption (running counter to the social strain argument), percent urban significantly increased the rate, giving mixed support for hypothesis 4. In general the analysis of determinate sentencing only weakly supports the social strain argument across the four variables testing strain effects.

Politically, the analysis again suggested Republicans are more likely to adopt sentencing reforms with both the house and senate variables increasing the rate of determinate sentencing adoption. This model again suggested support for Hypothesis 7 for state legislators, but not for state governor. As expected (hypothesis 9), states with a prior sentencing reform were more likely to adopt determinate sentencing (Spohn 2000; Stemen, Rengifo, and Wilson 2006; U.S. 1996). Somewhat surprisingly, the model does not suggest the era matters (or at least the rates do not very across the time periods).

Truth in Sentencing:

Truth in sentencing is the most popular sentencing reform to date, with 24 states adopting the reform (Barker 2006; Boerner 2001). For the crime and justice controls, drug crime arrests, again, were the only significant variable; increasing the rate of adoption. This again lends support for Hypothesis 2 for drug arrests, but not violent crime arrests. As with all previous models (Models 1-3 of Table 5.6), imprisonment rates were not significantly associated with truth in sentencing adoption rate. This finding was

consistent across the four separate reforms and the full model of the all reforms analysis and runs counter to a number of prior studies, which have pointed to concerted efforts by policy makers to connect overcrowding and reforms (See Model 4 of Table 5.6). They suggested that sentencing reforms were adopted after the spike in sentencing and would be effective in curtailing a continued rise (Boerner 2001; Tonry and Hatlestad 1997; Von Hirsch 1985). The models here suggests otherwise (see Table D.5 in Appendix D for a more detailed table of the analysis presented here).

While in no other model was Southern location significant, in the truth in sentencing analysis, the South significantly reduced the rate of adoption. This finding is curious as it runs counter to the expected finding (Hypothesis 3), suggesting the South may not be more punitive in this respect (Clarke 1998; Wolfgang 1967). As with a number of the previous models, percent Hispanic significantly reduced the rate of truth in sentencing adoption, running counter to hypothesis 4. But unlike other models, the South and percent Hispanic were the only demographic controls to significantly change the rate of adoption, suggesting no support for any of the four social strain variables.

Republicans, again, were more likely to adopt a sentencing reform, this time truth in sentencing. The effect is only observed for the state house (or assembly) and not for the state senate or governorship. Thus, as with the previous models, the results here support hypothesis 7, albeit with more limitations for truth in sentencing.

One of the most interesting findings in the entire analysis (including all previous models) was the significant reduction in the truth in sentencing adoption rate for state revenues. While this was the only model where a negative effect was observed, the rest

of the models suggested state revenues were not significantly associated with adoption of reforms. Perhaps states with higher revenues were not as swayed by the money offered in the 1994 Omnibus Crime Bill for states that adopted truth in sentencing reforms. In any case, these findings run counter to the expected outcomes (hypothesis 9) and reject the argument by Spelman (2009) that state revenues were the chief driving force behind reform adoption.

As expected (Hypothesis 9), the results here suggest the presence of a prior reform significantly increased the rate of truth in sentencing adoption. Additionally, the era dummy codes were also significant, but an interesting phenomenon with the time periods was observed. First, while the rate of adoption is higher from 1986 to 1993 compared to 1972-1985, the rate is much higher for 1994-2008. In fact, 31 times greater. This larger effect is likely the result of the 1994 omnibus crime bill, which, among other things, set aside federal subsidies tied to truth in sentencing adoption at the state level. The effect of this legislation was profound as 18 of the 24 states to adopt truth in sentencing laws did so in the three years immediately following the federal bill (Bohm 2006; Tonry 1995).

Discussion

This analysis has several significant advantages over previous research. First, the analysis includes data covering an extended time period from 1972 to 2008. The "imprisonment binge" began roughly in the early 1970's with 1972 often cited as the "start date" (Abramsky 2007; Gottschalk 2006). With data beginning in 1972, the analysis includes *every* year from the start of the binge, covering the effects more

accurately than analysis limited to 3 or 5-year gaps (Hsiao 2003; Wu 2008). Second, this analysis includes almost all sentencing reforms of the period, a marked advantage over previous research (Spelman 2009; Stemen, Rengifo, and Wilson 2006). Previous research has generally assessed one reform at a time (with notable exceptions) limiting the analysis to that exact reform (e.g. Griffin and Wooldredge 2006; Johnson 2006; Kramer and Steffensmeier 1993; Kramer and Ulmer 1996; Steffensmeier, Ulmer, and Kramer 1998; Ulmer and Kramer 1998; Wooldredge and Griffin 2005; Wooldredge, Griffin, and Rauschenberg 2005; Wooldredge 2009). As seen in this analysis, considerable change in the effects on reform adoption is observed when different reforms are modeled. Failure to account for the differential effects may miss-specify the results (Stimson 1985). Third, the analysis includes both cross-sectional and time-series data giving it a marked advantage over analysis that lack one of the dimensions. Fourth, the modeling technique, event-history analysis, has the advantage of predicting the relative influence state-level characteristics have on the rate of reform adoption.[88] Unlike other regression models where reforms can be used to show the effect of covariates like racial disparities, imprisonment rates, or judicial departures, in this analysis the models can show how the environment, socially, politically, and demographically affect the likelihood that a state adopts a reform (or the increase in the rate of adoption) (Blossfeld and Rohwer 2002; Carroll 1982; Wu 2008; Xie 1994).

The models assume *all* states are "at risk" of adopting a sentencing reform. Or said another way, the model assumes that a state will eventually adopt a reform, though

[88] No research to date could be found that systematically analyzed all sentencing reforms across all states over time using event-history analysis.

the model takes into account right censuring where a state is never "observed" adopting a reform (Blossfeld and Rohwer 2002). Of course it is possible not all states will adopt a sentencing reform and while the model assumes a state will, at some time (though the rate of adoption can be so functionally small that it has no real effect), adopt a reform, the important consideration in this analysis is the effect the various controls have on the overall rate of adoption. After all, the main purpose of this analysis is not to predict when a state will adopt a reform, but under what conditions a state is more likely to adopt a reform. This later focus has important substantive implications as it may point to state-level characteristics that knowingly or unknowingly influenced state policy decisions (Carroll 1982). These state-level characteristics operate at the structural level and identifying them may help in understanding the role social stratification, demographic shifts, and/or political affiliation has on social policy choices.

It was hypothesized that high percentages of ethnic/racial minorities would increase the rate of reform adoption. The argument follows that high percentages of Blacks or Hispanics would represent a perceived threat to the white majority and reforms would be used as a tool of social control of the underclass (Hypothesis 4) (Engen, Gainey, Crutchfield, and Weis 2003; Hebert 1997; Johnson 2003; Quillian 1995). This analysis offers only scant support for such a hypothesis with percent Black significantly increasing the rate of the all reform models and sentencing guideline adoption models. Additionally, when percent Hispanic was significant, the models suggest the rate of adoption was reduced (counter to the racial threat argument).

In general, the analysis suggests that the unemployment rate was the most powerful influence on the rate of reform adoption across most models, supporting hypothesis 6. Thus the results suggest that high unemployment placed a social strain on states increasing the desire of the state to "do something about the problem" (Kruttschnitt 2005; Nicholson-Crotty 2004; Quillian 1995; Ulmer and Johnson 2004). This "do something" pressure resulted in, among other things, the adoption of sentencing reforms as a form of social control to alleviate the strain.

As expected, with the exception of statutory presumptive sentencing, drug crime arrests increased the rate of reform adoption. This finding was consistent with previous research, which had suggested that drug crimes or at least the increased criminalization of drugs were highly connected to reform adoption. A number of prior studies have shown the deep connection between the law and order movement, which sentencing reforms were a part of, and the rise of drug crimes as a perceived social threat (Hamid 1998; Radosh 2008; Spohn, Gruhl, and Welch 1982; Wooldredge and Griffin 2005). Conversely, violent crime arrests were not generally associated with the increased rate of reform adoption. Some prior research had suggested reforms were at least partly a response to rising violent crime rates, especially in the late 70s and 80s (Tonry 1995; Von Hirsch 1985). But in this analysis this hypothesis was not supported.

Another consistent finding across all of the models was the significant impact Republican control of the state legislatures had on the rate of reform adoption. This finding was expected as Republicans have historically supported a more strict crime and justice platform (Gottschalk 2009). While the law and order movement was a key

driving force behind the reform movement and Democrats certainly participated in the movement, Republicans seem to have a significantly larger impact on reform adoption (Abramsky 2007; Barker 2006).

Future research should focus on the various characteristics that may have worked together to create certain environment "types" where reforms are the most likely to emerge. It is likely the reform movement was influenced by a number of competing and complementary effects acting simultaneously. In the end, the analysis suggests reform adoption did not happen in a vacuum. This analysis sheds some light onto the socio-political context by which reforms were adopted and which state characteristics influenced the rate of adoption suggesting a number of important forces are acting upon the system.

CHAPTER VI

CONCLUSION

The analyses contained in this dissertation have some significant advantages, both methodologically and theoretically, over previous research studying the effects of sentencing reforms on changes in imprisonment. First, the analyses include data from 1965 to 2008[89], which represents a relatively long period of historically relevant data that includes the beginning of the "imprisonment binge" that began roughly in 1972. The analysis also includes data for *every* year, providing finer grained coverage than analysis limited to 3 or 5-year gaps. Second, previous research has generally assessed one reform at a time (with notable exceptions) limiting the analysis to that exact reform. We included almost all sentencing reforms of the period and found considerable change in the effects of reforms when different combinations of the reforms were taken into account. Accounting for these differential affects provides a more complete picture of the effects of sentencing reforms. Third, the different model specifications employed diverged from previous research and supplied arguably better modeling, including the incorporation of a conditional change score, fixed-effects for states, a new growth curve variable coding of the effects of sentencing reforms, odds ratios, and an event-history analysis. Fourth, the analysis included *all* 50 states allowing comparisons across states,

[89] Some of the models include fewer years.

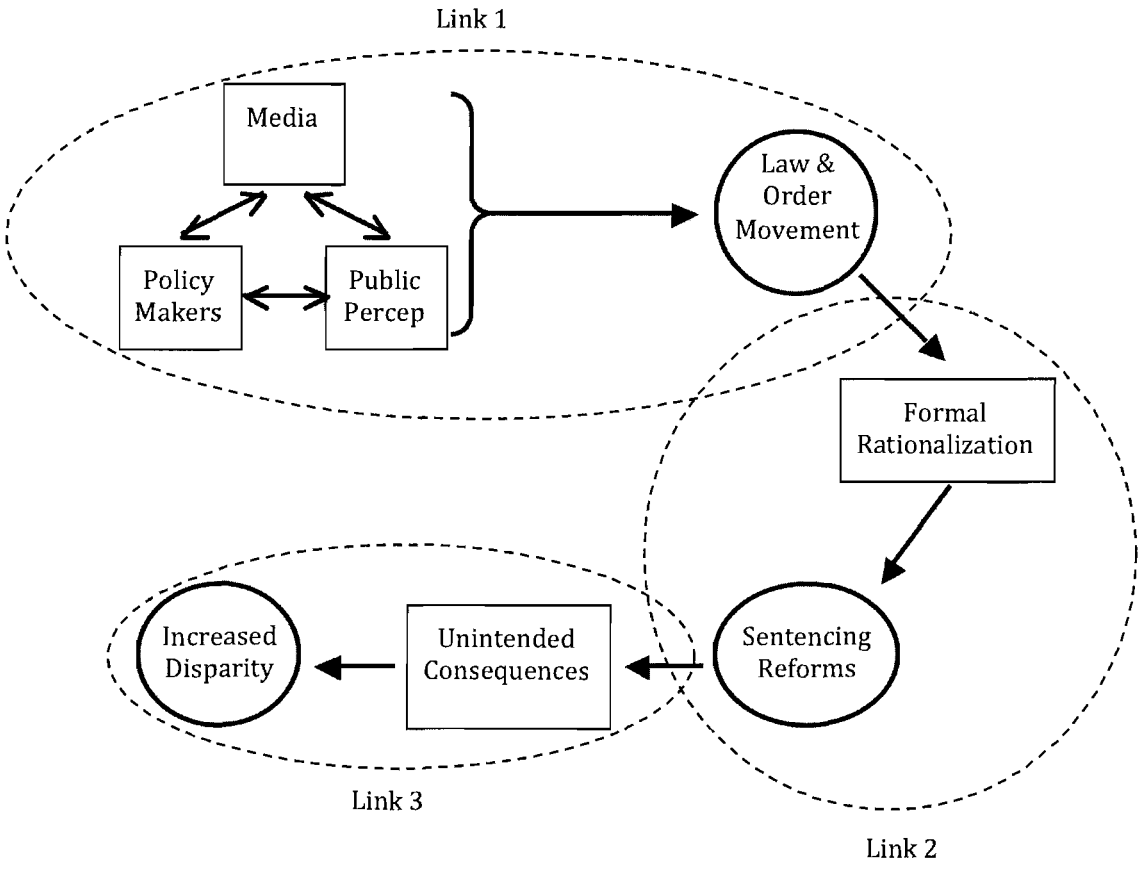
comparing states that have adopted reforms to states that have not, and allowing for greater statistical power and greater generalizability than much of the previous research. While the results do not allow for the complete state-by-state analysis (*e.g.*, we can not definitely calculate the change over time for Oregon vs. Washington), the results do have substantive interpretation, outlining the average changes over time within states in imprisonment and imprisonment composition by race and gender. Fifth, the analyses included not just the assessment of changes in total imprisonment, work conducted in prior research, but also the effects of reforms on the racial/ethnic and gender[90] composition of those imprisoned. Sixth, one of the chapters examines the environmental and social demographic characteristics of states that influence the rate of reform adoption for states using an event history analysis.

Much of the earlier policy analysis tended to focus purely on outcomes with less attention to what might be driving changes in imprisonment (Tonry and Hatlestad 1997; Webster 2007; Zhang, Maxwell, and Vaughn 2009). The theoretical model incorporated in this analysis helps to ground the analysis in contemporary understandings of the criminal justice system. This framework supplied the analysis with strong expected outcomes that are generated from a theoretical model. The social chain theoretical model may best be applied to the analysis of the racial/ethnic composition of imprisonment found in Chapter 3. Flowing from a social chain theory presented in Figure 6.1, where the context of the law and order movement (Abramsky 2007; Beckett 1997; Scheingold 1991) interacted with the structural shifts characterized by the formal rationalization

[90] The gender composition is a slight misnomer as prisons truly divide the population on sex not gender.

theory described by Weber (Weber 1978) to cause an untended consequence (Fine 2006; Merton 1936). In Chapter 3, it was theorized to result in the rise in the racial and ethnic disparities in imprisonment as the final link in the chain. The findings, which supported the expected outcome for some of the reforms, ran counter to the stated goals of policy makers, who had suggested reforms would actually reduce the disparity (U.S. 1996; Von Hirsch 1985). These findings should not be taken lightly as the growth in imprisonment disparities among people of color should be a primary concern of criminal justice reforms. This analysis raises an alarm for both policy makers and social science researchers (Hebert 1997; McDonald and Carlson 1994; Tonry 1995).

Figure 6.1. Social Chain Theory Outlining the Frame Used in Chapter III



The focus of Chapter 3 was not to explain which of the specific unintended outcomes were at play. Instead the focus was on the general unintended effects across all states. It was expected that the policy shifts, regardless of the actual structure the policy took on, resulted in a latent effect: in this case, the rise in the disparity of imprisonment rates for people of color (Fine 2006; Merton 1936). At the same time, a finding that an unintended consequence was likely present does not suggest a policy failure *per se*. After all, as stated previously, it is important to remember that these reforms are embedded within complex, dynamic, and interconnected social systems that can lead to what Fine (2006) called "chained social problems". What has failed here is not the desire to reform

the system, which clearly has faults that should be addressed, but the failure to adequately predict the likelihood reforms would result in an unintended effect and to assess the actual outcomes (Merton 1936). The unintended effect may have not been foreseen and intended, but the results should not be ignored simply because it was not expected by policy makers. After all, the lack of the expectation of an unexpected outcome does not guarantee the absence of one (Kleck 1998). This only underscores the importance of not relying solely on the rhetoric of what is supposed to happen and focusing on the observed effects, like those presented in this analysis.

This study suggests policy makers need to seriously assess, and possibly reconsider, sentencing reforms to alleviate the increased disparities in imprisonment. Previous research has indicated that while the discretion previously held by judges under indeterminate sentencing may have been limited under reforms, it has not been removed from the process altogether. It is possible reforms have inadvertently shifted the discretion to prosecutors (who determine the severity of charges filed), where the bias has simply been moved to another set of hands (Kempf-Leonard and Sample 2001). A few studies have found that prosecutors are more likely to take into consideration factors outside of the offender's crime and prior record (directly or indirectly associated with race and/or other factors) in determining prosecution of violent and drug crimes, but less likely for property and other non-violent felony cases (Engen, Gainey, Crutchfield, and Weis 2003; Ulmer, Kurlychek, and Kramer 2007). In the end, the discretion has probably not been removed or reduced, but has been "displaced" from one group to another (Wooldredge and Griffin 2005).

Other research has indicated that Blacks and possibly Hispanics are more likely to be convicted of a relatively minor first offenses (Shane-DuBow 1998). If true, an increase in the odds of Black imprisonment may be linked to sentencing reforms through their greater emphasis on prior record (particularly for drug crimes) as a determinate of sentencing length (Inciardi, McBride, and Rivers 1996; McShane and Williams 1997). When this information is combined with the political desire to ratchet up the penalties on drug crimes, the connection between race and drug crimes as a driving force of disparity in sentencing becomes more plausible (Griset 1995; Wooldredge, Griffin, and Rauschenberg 2005). This connection may underscore the possibility that an unintended consequence of sentencing reforms is present.

Chapter 4 focuses on the shifts in the female imprisonment rates compared to males due to the adoption of sentencing reforms or changes in criminal arrest rates. Our interest focuses on this compositional trend as it extends over the period from 1970 through 2008. Specifically, we focus our investigation on two potential explanations for part of this increase: the sentencing reform movements that occurred from the 1970s through the beginning of the 21st century and changes in the ratio of female to male rates for serious crimes. Similar to the previous two chapters, the analysis suggests that the law and order movement combined with a shift in women's roles in society (unique perspective for this chapter) to create an environment where female imprisonment rates rose faster than males rates. We hypothesize that both sentencing reforms and differential arrest rates were part of the reason the female rates rose faster (See Graph 4.1 on Page 107).

The analysis of the odds of female to male imprisonment draws on the net-widening literature. For us, net-widening, involves an increase in the proportion of people that are caught up in the criminal justice system. It is often accompanied by shifts in the composition of those "caught up" in the system. For the female offenders, it was suggested that the imprisonment net caught up women at a higher rate than previously because they are more likely to be involved in relatively minor crimes and reforms moved these relatively minor crimes from non-prison sentences to prison sentences.

But research also indicates that a narrowing of the gap in both arrests *and* imprisonment has occurred between females and males. Both of these phenomena were theorized to at least be partially a result of the law and order movement (Beckett 1997; Blumstein, Cohen, Martin, and Tonry 1983; Scheingold 1991; Simon 2007). The analysis points to the particularly strong effects of drug crimes. This finding confirms prior research that has pointed to drug crimes as perhaps the most important factor in the narrowing of the gender gap (Jacobs 2003; Schwartz and Rookey 2008). The finding that sentencing reforms were not significantly associated with the narrowing of the gender gap was counter to our expectations. We expected, as in the other areas of imprisonment studied, that the law and order movement was driving shifts in sentencing that created a more predetermined and less discretion based system that would have the effect of making sentencing in general more punitive and with a greater reach. As this happened women would be caught up more by an expanding sentencing net and thus the gender gap would be reduced. But in this analysis, we found no direct effect of reforms on the odds of female imprisonment. It may be possible that while there was a direct effect on total

imprisonment and a direct effect on the racial/ethnic composition of imprisonment, the law and order movement is pushing a narrowing of arrest rates that results in narrowing of the imprisonment rates between women and men.

Table 2.5 from Chapter 2 (Page 53) may be the best way to illustrate the complete interaction of the reforms and how they affected the change in total state imprisonment rates. While this analysis does not allow us to conclude that *all* states or even *one* particular state conforms to the pattern suggested in the table, it does allow us to conclude that, on the *aggregate* or on average, the reform is (or is not) significantly associated with larger changes in imprisonment within states (Finkel 1995). The predicted change scores represent the *average* change in imprisonment rates for the *average* state adopting some combination of the reforms. A combination in which at least one of the reforms was significant was flagged. In some of the cases the reform (e.g., voluntary sentencing guidelines) or some combinations of the reforms (e.g., presumptive sentencing guidelines in combination with truth in sentencing and determinate sentencing) reduced the changes in imprisonment, but these cases were rarer. For the majority of the cases, the reforms increased changes in imprisonment, both on the front-end and on the back-end. Three strikes laws, on the other hand, did not significantly increase imprisonment rate changes on their own, but did when taken in combination with other reforms. Overall this analysis suggested sentencing reforms are associated with changes in state imprisonment rates when controlling for important co-variates. They are associated with at least some of the rise in imprisonment (often referred to as the "imprisonment binge") presented in Graph 1.1 (Page 2).

The event-history analysis presented in Chapter 5 has the advantage of predicting the relative influence state-level characteristics have on the rate of reform adoption. Unlike the other regression models presented in Chapter 2-4, where the models can be used to show the effect reforms have on imprisonment, the event-history models illustrated the effect the state environments; socially, politically, and demographically had on the likelihood a state adopted a reform (*e.g.,* increased the rate of adoption) (Blossfeld and Rohwer 2002; Carroll 1982; Wu 2008; Xie 1994). This analysis assumed *all* states were "at risk" of adopting a sentencing reform at some time (Blossfeld and Rohwer 2002). Of course it is possible that not all states will adopt a sentencing reform and while the models assumes a state, at some time, will, this analysis was primarily concerned with the effect of the various controls on adoption rates. After all, the main purpose of the analysis in Chapter 5 is not to predict when a state will adopt a reform, but under what conditions a state is more likely to adopt one, possibly pointing to state-level characteristics that knowingly or unknowingly influenced state policy decisions. These state-level characteristics operate at the structural level and identifying them may help in understanding the role social stratification, demographic shifts, and/or political affiliation has on social policy choices.

In the end, the analysis, as a whole, points to broad connections that support the general theoretical arguments of Beckett (1997), Scheingold (1991), and Simon (2007) for total imprisonment and the odds of imprisonment for people of color, but not for the narrowing of the gender gap. They suggest the law and order movement of the mid 1970's to the present was, in part, driving the shift to the "justice model" of

imprisonment of which "fixed" sentencing is one part. The reforms served to change the process, through the bureaucratization of the system as described by Weber and others, from a system giving significant leeway to judges and in house prison officials to a system largely placing power into hands of elected political officials: ultimately creating a system that is susceptible to political and social pressures. The final result is a general increase in punitive incarceration in which "fixed" sentencing represents a way to *codify* the punitiveness. These increases are likely regardless of the stated political purposes or goals of the reforms (*e.g.*, desire to be growth neutral). Adding to the body of research in the field, this analysis outlines the broad impacts of reforms, but also suggesting a number of important considerations for future research.

Because many of these reforms were created in the multifaceted "justice model" framework, they are likely to have many consequences (*e.g.*, rapid prison growth and changes in the composition of imprisonment). My analyses shed light on these important impacts, an area of research that has been largely overlooked or mis-specified in the previous literature. While some research has attempted to investigate reforms using panel models, these studies present results that should be viewed with considerable reservations due to limits in both their application of theory and/or their model specifications.

After controlling for violent crime rates, drug crime rates, property crimes rates, percent black, percent Hispanic, percent urban, population density, poverty rates, unemployment rates, log curves for state senate and state house, state governor, state revenues, new commitments to prison, and parole violators returned to prison; some of

the "fixed" sentencing reforms remain positively associated with changes in imprisonment rates and directly related to a portion of the over 500% increase in the imprisonment rate over the length of this study and to increases in the racial/ethic disparities. Legislators should consider this when designing sentencing procedures. The costs of locking criminals up is increasingly expensive and "prison beds" are currently at critical levels in a number of states (Jacobs and Carmichael 2001; Shane-DuBow 1998).

Rapid increases in prison populations can be devastating to a system that is often slow to respond (Griset 1995; Kruttschnitt 2005). The overall costs to individual states can be expensive. For example, Oregon, a state that has implemented sentencing guidelines, spent $23,389 per inmate in 2003. When the logarithmic growth curve variable is calculated out for seven years, the effect is about a 1-prisoner increase per 1000 state residents for that year, indicating that guidelines have a relatively large effect (considering the overall imprisonment rates are about 4 inmates per 1000). The Oregon guidelines could cost the state 81.84 million dollars a year.[91] The costs are likely to be even higher considering that these estimates do not include capital investments such as construction costs of new prisons and increased transportation. Additionally, increases in imprisonment places resource pressures on public defenders and courts. While considerable variation from state to state exists, the average cost per inmate across all states in 2003 was $22,650 suggesting costs are likely to be high in all states (Boerner 2001). The problem is exacerbated by other recent sentencing policies including truth in sentencing and three strikes laws that further increase prison populations.

[91] Calculations based on 2003 dollars

The finding that sentencing reforms are associated with a rise, not a decline, in the racial/ethnic disparities of imprisonment is a finding that should heighten the concerns to both policy makers and social science researchers. I can find no study to date that has assessed the impacts of reforms on the odds of imprisonment of blacks or Hispanics to whites on the state-level across all 50 states. Many policy and some social science researchers assumed that sentencing reforms would have a positive impact on (reducing) the large racial disparity in the American imprisonment system. Some researchers have held steadfast in their defense of this claim despite no evidence to support such a conclusion. They assume that the reforms, because of their increased emphasis on apparent race neutral characteristics (crime committed and prior record), cannot be the reason for a continued rise in the racial disparities.[92] Some social science researchers and/or policy makers cling to the notion that sentencing reforms were designed not to increase racial disparities and thus must not be responsible for the rise (Kramer and Ulmer 1996; Von Hirsch 1985). My analysis calls into question the assumption that sentencing reforms have not increased the racial disparity and researchers should now endeavor to disentangle exactly what particular parts of sentencing reforms are responsible for this rise (Kleck 1998). Another area needing more research involves the characteristics of states that are associated with the adoption of various sentencing reforms. Chapter 5 highlights some of the broad state level structural characteristics that may be of influence, but future research should venture deep into roots of reforms to find the origins of the effects.

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[92] This statement is not designed to insinuate that no policy maker has questioned sentencing reforms.

APPENDIX A

DETAILED DISCUSSION OF

SENTENCING REFORMS FOR CHAPTERS II-IV

Sentencing guidelines consist of a matrix of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) and offense severity. These two main determinates make up the technical apparatus informing the judge's "automation of paragraphs" suggested by Weber (1978). It is important to note that while sentencing guidelines were established in each state under the same general principles, guidelines' design and implementation vary considerably from state to state (Frase 2005; Stemen, Rengifo, and Wilson 2006). One classification[93] of guidelines incorporates two loose *legal categories*. The first, labeled *presumptive sentencing guidelines*, are strict in their application. Only allowing very limited deviations, Judges generally must follow the matrix (Engen and Steen 2000; Frase 2005; Stemen, Rengifo, and Wilson 2006; Zhang, Maxwell, and Vaughn 2009). The second format, *voluntary sentencing guidelines*, treats guidelines as a formal recommendation, but does not *legally mandate* they be followed. The difference here is

[93] The general census is not uniformly agreed upon and considerable disagreement of the proper way to categorize them still exists. The two "types" discussed here represent a division largely discussed and advocated by Frase (1995) and Stemen (2006) and is the most likely division to be informative in this analysis.

their legal mechanisms. States with presumptive sentencing guidelines have "legally binding" sentencing matrixes enforceable by appellate review, allowing both the prosecution and defense to appeal sentences not following the guidelines. Conversely, in states with the voluntary guidelines, there is no appellate review. While judges may be required to give a written justification to deviate from them, they are not legally obligated to follow them and are largely free to render any decision they want. Research does indicate judges follow the recommendations of voluntary guidelines 85% of the time, suggesting they do have impact (Miethe and Moore 1988). The bifurcated classification of sentencing guidelines into *presumptive* or *voluntary* is unrelated to how the guidelines were created (by legislative statute, initiative process, or sentencing commission) or whether an active commission monitors them.

It is important to note that while it is relatively simple to classify guidelines by their binding legal status, considerable variation exists in design and application, both across and within the two types (Frase 1995; Frase 2005; Reitz 1995).[94] As Frase (2005) and Tonry (1995) argue it may be more appropriate to consider guidelines as falling on a continuum from structured to loose. Additionally, researchers rightfully acknowledged the "fixedness" of sentencing guidelines vary considerably from state-to-state. Some

[94] Furthermore, the factors considered during initial adoption varied greatly: for example, stated attempts to be growth neutral, designed to be proscriptive vs. prescriptive, and explicit consideration of one or more of the following issues: race, ethnicity, and gender. These state-to-state variations in "design factors" will not be directly assessed in this study, but do represent an area of analysis worthy of investigation. They are not directly assessed in this study for three reasons. First, the analysis in concerned with aggregate data and any further slicing of the independent variables further reduces the predictive power of the analysis. Second, theoretically it is likely that while states have discussed publicly their desires, outcomes will likely vary considerable from their stated goals. Third, individual discussion of isolated states sociopolitical landscape during adoption is hampered by a lack of available aggregated data.

voluntary systems like Missouri have very limited ranges giving little leeway to judges who choose to follow them, while a state like Pennsylvania, which is categorized as presumptive, has rather large ranges allowing judges some level of discretion. Because explaining the general effects is our chief concern and not the variation within guidelines, we have chosen to leave further discussion of guidelines diversity to another analysis.

Also, this study will assess *statutory presumptive sentencing*, another attempt to shift the correctional system from a rehabilitation model to a crime control model. Statutory presumptive sentencing, like sentencing guidelines, represents an attempt to create uniformity within similarly situated crimes, but acts less like a sentencing rubric. It specifies an appropriate or "normal" sentence for each offense as a baseline for a judge. It can be considered along with other relevant factors (aggravating or mitigating circumstances), but its intent is to "fit the punishment to the crime" rather than "fit the punishment to the offender'" (Brewer, Beckett, and Holt 1981; Frase 1995; Savelsberg 1992).[95]

Additionally, this study will incorporate an assessment of *determinate sentencing (abolishment of parole)* and *truth in sentencing laws.* They represent the first attempt to limit the "indeterminateness" of sentencing. The reforms, like the other "fixed" sentencing reforms, shifts the role of the correctional system from a rehabilitation model to a crime control model. In contrast to the three previously mentioned reforms, the

[95] As with other sentencing reforms, considerable variation between states adopting statutory presumptive sentencing exists. For this analysis, the important point is that this sentencing procedure again represents a shift from the indeterminate model to a model focused on equalizing the variation in sentencing across similar crimes by "fixing" the sentence to the crime.

focus lies in the "back-end" mechanism of release as opposed to the "front-end" of sentencing. In general, determinate sentencing is used to refer to a system *without* discretionary parole boards. While ultimately determinate sentencing operates independently from the type of sentencing procedure used (indeterminate, guidelines, or statutory presumptive), the reform fits into the "fixed" sentencing category because the amount of time served is more assured (Griset 1995; Kempf-Leonard and Sample 2001; Stemen, Rengifo, and Wilson 2006). Truth in sentencing on the other hand does not necessarily dictate the elimination of parole boards, but does require offenders serve a statutorily defined minimum amount of time. Only states meeting the 1994 federal omnibus crime bill minimum 85% time-served of original sentence are considered in this analysis. Finally, we considered *three strikes laws*. Due to their focus on habitual offenders, they are considered an additional reform beyond the previous five more structured reforms affecting sentencing or release. They too fall under the umbrella of "fixed" reforms because of their design to limit discretion through an apparent push away from the rehabilitation model.

APPENDIX B

DETAILED DISCUSSION OF METHODS FROM CHAPTER II

This appendix is designed to give a more detail illustration of the statistical procedures used. The analysis utilizes panel data or what some in the social sciences call time-series cross-section[96] (TSCS) data. In essence, panel data is simply data that include both cross-sections and time or better stated, it includes some number of cases observed repeatedly over time. The data in this study include relatively large equal-spaced time periods ($t = 41$ years) and a relatively large number of cases ($i = 50$ states).[97] It should be noted that the large i in this analysis is only considered large because t is relatively large. To illustrate, the number of observations (n), the metric by which the model draws its statistical power is determined by $t \times i$, thus having a large number of time points makes it possible to have fewer cases. In this case the analysis has the potential for 2,050

[96] The analysis incorporates both cross-sections (states) as the unit of analysis and measures over time and in the social science literature the analysis techniques are often called time-series cross-sectional analysis or panel analysis, with the later language more likely found in the econometric literature.

[97] Some researchers like to distinguish between the two "types" of data, but qualitatively these two "types" are not really different data, but possess different quantities of units or time. In this analysis, I do not see the necessity to distinguish the two, but you may find prior research that goes into great lengths discussing the difference. In general panel data is sometimes referred to data with a large number of cases (i), but a small number of time points (t) and cross-sectional time-series data refers to data with small i and large t. Though careful attention should be paid to the exact number of cases or time points stated as the definitions are loose and many researchers, including myself see the distinction as moot. This is not to say that a researcher should not be concerned with the number of cases in relation to the number of times points, but on the contrary, they are highly important to the specific statistical procedure used (Wooldridge 1995; Wooldridge 2000).

observations (n) determined by multiplying 41 (t) years by 50 (i) states and is generally considered a large n for Prais-Winsten regression.

Considering the number of cases in relation to the number of time points helps to select the proper modeling technique utilized. For example, with a large number of cases but a small number of time points, Feasible Generalized Least Squares, Arellano-Bond, or Blundell-Bond models are appropriate, while Prais-Winsten or Hausman-Taylor models may be more appropriate for large t. The modeling techniques just discussed and the analysis found in this paper should not be confused with other analysis that may be used on panel or panel like data including hierarchical linear modeling (HLM) or structural equation modeling (SEM). It is especially important to not confuse HML with panel models as the language for fixed vs. random effects are used in both techniques but represent different substantive effects and interpretations. This last point has caused considerable confusion among scholars (Wooldredge, Griffin, and Pratt 2001).

Specifically in this analysis, I use Prais-Winsten regression with panel corrected standard errors (PCSE), an approach recommended by Beck and Katz (1995). While it is a variant of Ordinary Least Squares regression, which has several known problems when analyzing panel data (Beck and Katz 1995; England, Kilbourne, Farkas, and Dou 1988), it incorporates a number of corrections that make the analysis justified (Beck and Katz 1995; England, Kilbourne, Farkas, and Dou 1988). Beck and Katz (1995) found that most panel analysis suffers from issues of serially correlated errors that may understate the standard errors of regression coefficients by as much as 50 to 300 percent. PCSE

corrects the underestimation and can lead to consistent and efficient point estimates when models are properly specified (Beck and Katz 1995; Halaby 2004).

While PCSE corrects for serially correlated errors, it does not address all issues that are likely to arise. Due largely to the issue of omitted variable bias, the first issue often needing consideration is the choice between random and fixed-effects. To illustrate, Prais-Winsten modeling (or other commensurate modeling of panel data) without the use of fixed-effects is what researchers have come to call pooled panel analysis or the random effects model. In a random effects model the panel-by-year specific observations are "pooled" and the errors are assumed to be exogenous (the panels are not related to each other) and are not serially correlated (points in time are not related to each other), thus yielding;

$$Y = \beta_o + \beta_1 X_{1it} ... \beta_n X_{nit} + \upsilon_i + \varsigma_t + \varepsilon_{it}$$

- Where υ_i is the unobserved time-invariant variation, ς_t is the unobserved case-invariant variation and ε_{it} is the idiosyncratic error term.
- ε_{it} is assumed to be uncorrelated with $X_{1it} ... X_{nit}$ and with υ_i and ς_t.
- And where υ_i and ς_t are not correlated with $X_{1it} ... X_{nit}$.

Both the Hausman and the Mundlak test can be used to determine whether the random effects model meets the assumption that υ_i and ς_t are uncorrelated with $X_{1it} ... X_{nit}$. If the data fails the tests it is likely to suffer from what researchers have come to call omitted variable bias (unobserved, variables not included in the analysis, time-invariant variables that are correlated with the X_{it}'s) and it would not be wise to use a random effects model. The analysis in this paper failed the both the Hausman and Mundlak tests suggesting the presence of omitted variable bias. In essence, this suggests that the model

must include more independent variables that explain the correlation between the panels (time-invariant variable) or remove the omitted variation, otherwise the point estimates will be inconsistent (biased) and inefficient, seriously calling into question the analysis (Wooldridge 2005). Fortunately, with Prais-Winsten regression with PCSE (and with other forms of panel analysis), the time-invariant omitted variable bias can be corrected for by utilizing a panel specific fixed effect model (Halaby 2004). In the simplest approach, including $i-1$ dummy variables for each state (except the reference state) achieves this. In this analysis, 50-1 (*e.g.*, $i_i..i_{i-1}$) dummy variables were included, yielding;

$$Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + i_1..i_{n-1} + \upsilon_i + \varsigma_t + \varepsilon_{it}$$

- Where ς_t is not correlated with $X_{1it} ... X_{nit}$.
- ε_{it} is assumed to be uncorrelated with $X_{1it} ... X_{nit}$.
- Where $i_i..i_{i-1}$ represent the dummy variables for each panel.
- Because $i_i..i_{i-1}$ and β_0 are perfectly correlated with υ_i, υ_i drops from the analysis and we are left with: $Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + i_1..i_{n-1} + \varsigma_t + \varepsilon_{it}$.

This model controls the unobserved state level effects (panel specific omitted variable bias) that are stable over time by holding the unexplained time-invariant variation constant. In effect, the panel specific fixed-effects model exploits within group variation by holding constant unexplained between state variation. As long as there is no serial correlation (ς_t is not correlated with $X_{1it} ... X_{nit}$), estimates achieve an unbiased state even when the random effects assumptions are violated. Because the random effects assumptions cannot be met in this analysis the unit fixed-effects model must be incorporated (Halaby 2004).

The fixed effects model offers some other significant advantages over the random effects model. For example, an important advantage in this analysis is that fixed-effects for states will control for any regional differences that may be present. Research has shown that the South has higher rates of imprisonment and the analysis will control for this difference and remove its effect from the results (England 1998). While this can substantially improve the reliability of the results, it also removes any cross-sectional effects that might be the focus of a study concerned with differences between states rather than changes within states (England, Kilbourne, Farkas, and Dou 1988; Halaby 2004; Wooldridge 1995). In this analysis, however, the primary concern lies not with panel-to-panel variation, but in the patterns of aggregate change over time, thus making the fixed effects model theoretically advantageous in addition to being statistically necessary.

In this analysis the panel specific fixed-effects approach sacrifices little. In practice the panel specific fixed-effects model creates a simultaneous time-series analysis where the coefficients represent the average, across panels, *within* panel change over-time of the average *within* panel level changes of the independent variables. In fact, early practitioners called it *simultaneous time-series modeling* for this exact reason (Finkel 1995). It has the advantage of increasing the statistical power through the "pooling" of the observations, just like in the random effects model, but it corrects for the critical flaw of unobserved omitted variable bias. A disadvantage of such a technique is the increased number of estimates created by the dummy variables for panels and the subsequent loss of degrees of freedom, but the relative advantage of correcting for omitted variable bias outweighs any argument for the advantage of the random effects model. Furthermore, in

an analysis with large *t* the "cost" of statistical power due to more covariates is likely to be at a minimum (Wooldridge 2005).

A change in the interpretation of the coefficients does occur when fixed effects for units is implemented, as they now no longer represent the average effect of the independent variables (time-variant and time-invariant) on the dependent variable (time-variant and time-invariant) found in pooled random effects model (Hsiao 2003). While this may be important to many, especially those concerned with differences between panels, the desire for between unit effects is not sufficient to justify a random effects model. While some researchers have pointed to the large variability between (state-to-state) the states adopting reforms (state level differences as well as differences between the reforms – *e.g.,* not all sentencing guidelines are the same[98]), in the unit-specific fixed-effects model, where the interpretation lies in the average effect over-time, such variation only makes the model less efficient (larger standard errors) affecting the ability to reach significant results.

Fixed effects for units may be statistically required and incorporated in the analysis, but it does not address any issues that are time-variant, that is, issues that are unit-invariant (does not change over the units) but change over time. Like the issues of omitted unit-specific variable bias, this time-specific issue revolves around omitted variables. If suspected (the Mundlak and the Hausman test can be used test for time-specific omitted variation), the choice of fixed-effects for time is not as cut and dry as the

[98] This should not be interpreted to mean that it is not important to correctly specify the data. Miss-specified data will result is biased results in any analysis.

choice of unit-specific fixed effects because there are a laundry list of proven procedures that may remedy the time-specific concerns (Halaby 2004).

Fixed-effects for time is on the top of the list of appropriate tools to assess models suffering from unexplained omitted variables that are constant across units at a given time period. Like fixed effects for units, fixed effects for time completely corrects for any time-variant omitted variation. But their inclusion is not without cost. First it changes the interpretation of the coefficients from change over-time. Because the theory suggests the effects of reforms will be observed in the changes in imprisonment over-time within a state, it is desirable to leave the year-to-year variation intact. To illustrate further, when fixed-effects for both units and time are included, the substantive interpretation of the coefficients become a measure of the panels' deviation from time by unit specific mean that is stable over time, completely removing any unexplained time varying *and* panel varying unobserved variation. This of course could be a measure of theoretical interest, but is substantively different then the "change-over-time" measure of the panel-only fixed-effects model. Second, and possibly more important, fixed effects for time, especially when coupled with fixed effects for units, are likely to "suck up" most of the variation in the analysis. When most of the variation is removed from the analysis the models can become highly inefficient and statistically significant results can be hard to come by.

Therefore, because most time variant issues can be assess with a large number of other proven procedures (*e.g.,* AR controls, first differencing, or the within transformation) that are less likely to make the analysis difficult by "sucking up" all of

the variance. That is, the analysis becomes difficult because when fixed effects for years are included, the time dummies are perfectly correlated with the error caused by the omitted time-variant variables. This "perfect correlation" is what gives the fixed effects approach its statistical utility, but it is also the reason it can be difficult (statistically speaking) to get significant results as the analysis becomes less efficient. Instead of fixed-effects for time, this analysis used a series of procedures common and proven effective to time series analysis that control for time-specific confounds. If these controls are capable of controlling for *enough* of the time-specific omitted variation that may be present in the analysis then it is appropriate to run the analysis without time-specific fixed effects (Finkel 1995; Hsiao 2003).[99]

The procedures can also be used to de-trend the data, address issues of the random walk, and to address time-specific shocks (Beck and Katz 1995; Halaby 2004). First, it is important to remember that once fixed-effects for units is implemented the analysis no longer represents a pooling of the state (panel) by year (time) specific observations and now becomes a series of simultaneous time-series analyses that measure the aggregate (averaged across panels) change over-time within panels (Frees 2004; Hsiao 2003). As noted, between 1972 and 2007, imprisonment increased by some 550%. This trend is problematic and should be corrected for. In general a researcher can choose to first-difference or perform the within mean transformation on the dependent variable. Because the within transformation is prone to serial correlation and is further from the substantive interpretation of "change-over-time" this analysis was interested in, first

[99] Of course you can use a Hausman or Mundlak test to assess the presence of time-specific omitted variables.

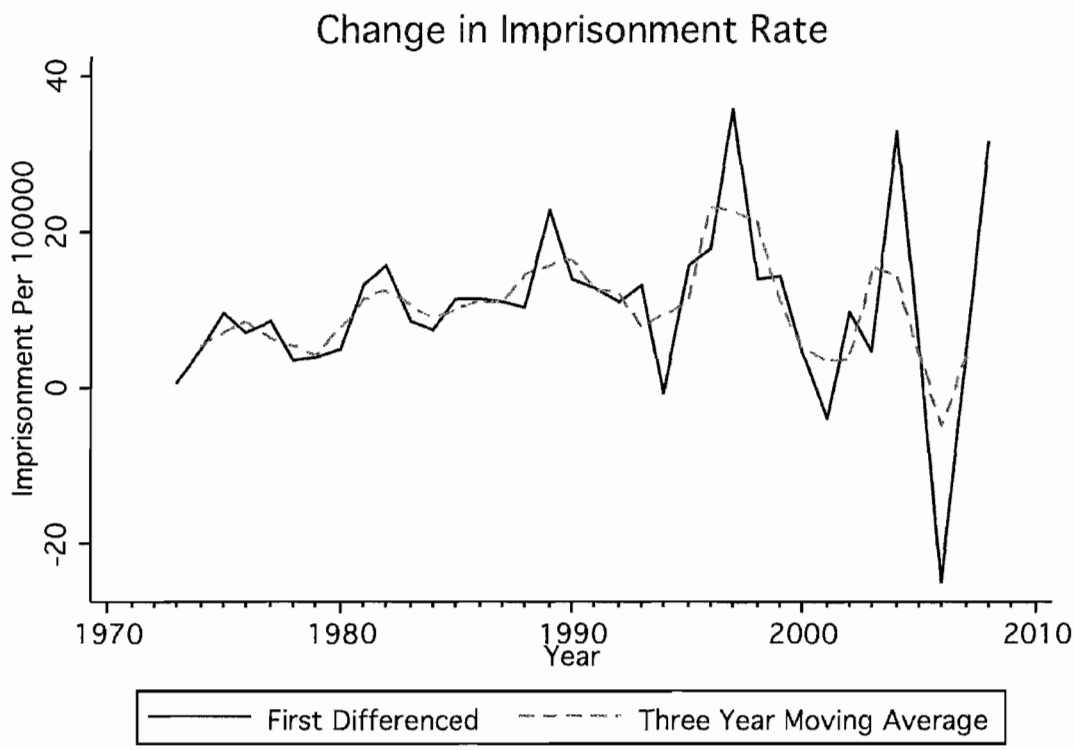
differencing was utilized. The approach has its advantage as it now represents year-to-year change in imprisonment for each i (Finkel 1995; Halaby 2004). The model now becomes what some call the unconditional change score model:

$$\Delta Y = \beta_o + \beta_1 X_{1it} ... \beta_n X_{nit} + i_1 .. i_{n-1} + \varsigma_t + \varepsilon_{it}$$

- Where ς_t is assumed uncorrelated with $X_{1it} ... X_{nit}$.
- ε_{it} is assumed to be uncorrelated with $X_{1it} ... X_{nit}$.
- Where $\Delta Y = Y_{it} - Y_{it-1}$ and $i_1 .. i_{i-1}$ creates individual intercepts for each ΔY
- Note: the β may be either β or $\Delta\beta$

In time-series analysis a shock or spike in the series can have a profound effect on the analysis and undermine the model's ability to measure change-over-time effectively. To remedy the apparent "shocks" observed in the first-differenced imprisonment variable, another common time-series transformation, a three-year moving average was instituted (Frees 2004). As you can see in Graph B.1 below the first differenced change scores suffer from some spikes, while the three-year moving average "smoothes" the spikes. It should be noted that moving averages are most appropriate when the analysis includes large *t*. Time specific fixed-effects, when coupled with unit specific fixed-effects, also largely controls for shocks to the series. As noted, a concern that needs to be addressed, but in this analysis the three-year moving average is more appropriate (see Graph B.1).

Graph B.1. The Change in Imprisonment Rates and the Three-Year Moving Average



Even when the panel specific fixed-effects model with PCSE and a three-year moving average of a first-differenced dependent variable was included, time related issues are still likely present (a Mundlak test of the unit fixed-effects v. the unit and time fixed-effects model confirms this assertion). A phenomenon known as regression to the mean is often found in this type of analysis and it suggests that the unconditional change score model, $\Delta Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + i_1 .. i_{n-1} + \varsigma_t + \varepsilon_{it}$ (where ς_t is left unchanged or unconditioned) leads to biased results because the effect size of the explanatory variables on the change score dependent variable is related to the preceding value of the dependent variable (Frees 2004). For example, an imprisonment rate going from 10 to 20 would

have a change score of 10, while a change form 100 to 110 would also be a change of 10. The former would actually be a doubling of imprisonment and arguably a larger "effect" while the later would be just a 10% increase. To correct for the this, a lagged term of imprisonment rate, $\beta_n Y_{it-1}$, was included in the analysis, yielding;

$$\Delta Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + \beta_{n+1} Y_{it-1} + i_1 .. i_{n-1} + \varsigma_t + \varepsilon_{it}$$

- ε_{it} is assumed to be uncorrelated with $X_{1it} ... X_{nit}$.
- Where, when Y takes the form of ΔY, $\beta_{n+1} Y_{it-1}$ is correlated with the portion of ς_t that takes on a first-order serial correlation structure, then ς_t becomes $\Delta\varsigma_t$
- Therefore we are left with: $\Delta Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + \beta_{n+1} Y_{it-1} + i_1 .. i_{n-1} + \Delta\varsigma_t + \varepsilon_{it}$
- Note: The lagged dependent variable term is *not* ΔY_{it-1} but is instead Y_{it-1}.

This model is often referred to as a static-score or conditional change score model and frames the analysis in the following fashion: "do the independent X variables [both X or ΔX] influence changes in Y (*e.g.,* ΔY) for fixed levels of $\beta_n Y_{it-1}$, that is, taking into account the negative effect of initial values of Y on subsequent change (Finkel 1995: 9)?" Additionally, because time-series analysis assumes that social systems are progressing towards *stability*, where the change in the dependent variable (Y or ΔY) becomes constant and stable until it is altered by some exogenous disturbance, a negative relationship between the lagged dependent variable on the right-hand side of the equation and the dependent change-score variable (ΔY) on the left hand side is likely to be observed. If the negative coefficient is found, it can be said that a causal relationship between Y_{it-1} and Y is present. That is, there is evidence of serial correlation. Furthermore, a general-proxy of the size of the serial correlation can be observed because as Y_{it-1} approaches zero the model is also approaching equilibrium (an absence of serial

correlation). Large values of β_n for Y_{it-1} indicate large correlation, similar to large values of rho (ρ) for an AR(1) term (Finkel 1995; Frees 2004).

Finally, a good measure to the overall need (or lack of need) of the need to difference the dependent variable is the unit root test. The model is said to possess a unit root when the error is non-stationary in the time-series. That is, the moments of the stochastic process depend on *t*. In other words, *Y* is correlated with the error in some proceeding value of Y_i in time or Y_i "remembers" the error of Y_{it-1} perfectly (at least the time before, but it can take on a more complicated structure than Y_{it-1}) (O'Brien 2001). A unit root occurs when a random shock at one period is perfectly transmitted to the next period ("perfect memory") and is particularly likely to occur when the dependent variable is based on a "stock" rather than a "flow." In this analysis imprisonment rates in a given year are largely based on the composition of the stock of prisoners the year before with a small flow component based on those who leave prison and those who enter prison (Levin, Lin, and Chu 2002).

The augmented Dickey-Fuller (ADF) test is likely the most appropriate unit root test for this analysis because of the complicated error structure.[100] It tests the null hypothesis that a time series is serially correlated assuming that the data dynamics have an ARMA structure (Levin, Lin, and Chu 2002; Ng and Perron 2001). In this analysis the unchanged dependent variable model fails the ADF test while with the conditioned change-score model the null is rejected suggesting the conditional moving average differenced model was successful in correcting the autoregressive error structure at a lag

[100] See Ng and Perron 2001 for a more detailed discussion of why the ADF test is the most appropriate

of 1 (for additional discussion of the importance of choosing the correct lag see Ng and Perron (2001).

It is believed that the conditional change model used in this analysis, $\Delta Y = \beta_0 + \beta_1 X_{1it} ... \beta_n X_{nit} + \beta_{n+1} Y_{it-1} + i_1 .. i_{n-1} + \Delta\varsigma_t + \varepsilon_{it}$, supplies considerable advantages, yielding consistent and efficient point estimates. The coefficient's interpretations are both theoretically desirable and statistically robust and produce results that can be interpreted as the average effect of the aggregate within panel change over time as a result of the implementation of reforms. Furthermore, in addition to passing the Dickey-Fuller (ADF) unit-root test, this model passes both the Hausman and Munlak tests, suggesting not unit-specific *and* time-specific omitted variable bias is present.

APPENDIX C

DETAILED DISCUSSION OF

SENTENCING REFORMS FOR CHAPTER V

Sentencing guidelines consist of a table of possible sentences with a very narrow range within a sentencing category that is defined by an offender's criminal history (prior offenses) running across the top of the table and the severity of the current offense running down the side of the table. These are the two main determinates that are cross-referenced to determine the box in which the judge will find the sentencing range. The table makes up the technical apparatus informing the judge's "automation of paragraphs" suggested by Weber (1978). It is important to note that while sentencing guidelines were established in each state under the same general principles, guidelines' design and implementation vary considerably from state to state (Frase 2005; Stemen, Rengifo, and Wilson 2006; U.S. 1996).

Tonry (1995) argue, it may be more appropriate to consider guidelines as falling on a continuum from structured to loose. Additionally, researchers rightfully acknowledged the "fixedness" of sentencing guidelines vary considerably from state-to-state. Some voluntary systems like Missouri have very limited ranges giving little leeway to judges who choose to follow them, while a state like Pennsylvania, which is categorized as presumptive, has rather large ranges allowing judges some level of

discretion. I have chosen to leave further discussion of guidelines diversity to another analysis.

Also, this study will assess *statutory presumptive sentencing*, another attempt to shift the correctional system from a rehabilitation model to a crime control model. Statutory presumptive sentencing, like sentencing guidelines, represents an attempt to create uniformity within similarly situated crimes, but acts less like a sentencing rubric. It specifies an appropriate or "normal" sentence for each offense. It can be considered along with other relevant factors (aggravating or mitigating circumstances), but its intent is to "fit the punishment to the crime" rather than "fit the punishment to the offender'" (Brewer, Beckett, and Holt 1981; Frase 1995; Savelsberg 1992). [101]

Additionally, this study will incorporate an assessment of *determinate sentencing* (abolishment of parole) and *truth in sentencing laws*. They represent the first attempt to limit the variation in imprisonment. The reforms, like the other "fixed" sentencing reforms, shifts the role of the correctional system from a rehabilitation model to a crime control model. In contrast to the two previously mentioned reforms, the focus lies in the "back-end" mechanism of prisoner release as opposed to the "front-end" of sentencing. In general, determinate sentencing is used to refer to a system *without* discretionary parole boards. While ultimately determinate sentencing operates independently from the type of sentencing procedure used (indeterminate, guidelines, or statutory presumptive), the reform fits into the "fixed" sentencing category because the amount of time served is

[101] As with other sentencing reforms, considerable variation between states adopting statutory presumptive sentencing exists. For this analysis, the important point is that this sentencing procedure again represents a shift from the indeterminate model to a model focused on equalizing the variation in sentencing across similar crimes by "fixing" the sentence to the crime.

more assured (Griset 1995; Kempf-Leonard and Sample 2001; Stemen, Rengifo, and Wilson 2006). Truth in sentencing on the other hand does not necessarily dictate the elimination of parole boards, but does require that offenders serve a statutorily defined minimum amount of time limiting the power of parole boards or other release mechanisms. Only states meeting the 1994 federal omnibus crime bill[102] minimum 85% time-served of original sentence are considered in this analysis.

[102] The 1994 Federal Omnibus Crime Bill refers to the Violent Crime Control and Law Enforcement Act of 1994 which allocated 30 billion dollars to law enforcement and included a truth in sentencing provision.

APPENDIX D

DETAILED TABLES FOR CHAPTER V

Appendix D offers detailed tables of the analysis presented in Table 5.6. The tables here are designed to give an overview of the relative effects, how much the variable increased or decreased the adoption rates (Xie 1994). The effects are presented at the 25th, 50th, and 75th percentiles. Each of the four models presented in Tables D.2-D.5 are identical to the models in Table 5.6 and they are numbered sequentially as they appeared in Table 5.6. Table D.1 is a replication of Table 5.1 outlining the descriptive statistics of the controls variables from the event-history analysis and supplies the percentiles used to calculate their relative effects.

Table D.1. Descriptive Statistics for the Independent Variables

	Mean	**St Dev.**	**25th Per**	**50th Per**	**75th Per**
Crime & Justice Controls					
Imprisonment Rate	223.622	162.612	92	173	328
Violent Crime Arrest Rate	156.8338	115.292	77.507	129.029	207.716
Drug Crime Arrest Rate	272.446	209.335	109.622	238.034	388.369
Demographic Controls					
South*	-	-	-	-	-
Percent Black	10.288	9.304	3.130	7.801	15.042
Percent Hispanic	5.685	7.868	1.067	2.457	6.679
Percent Unemployment	5.059	2.417	3.300	5.000	6.400
Percent Under Poverty	12.895	4.330	9.700	12.00	15.328
State Population Density	139.427	183.367	30.833	74.781	149.990
Percent Urban	66.922	8.837	55.772	69.318	81.233
Political Controls					
Republican State House	-.853	2.434	-2.971	-1.882	1.792
Republican State Senate	-.760	2.499	-3.037	-1.609	1.998
Republican Governor*	-	-	-	-	-
State Revenues	359079.7	239502.1	219517.1	321616.3	420762.6

* Indicates a dummy variable

Table D.2. Sentencing Guidelines

	Model 1	25 Percentile	50 Percentile	75 Percentile
Crime & Justice Controls				
Imprisonment Rate	0.00477	1.012	1.039	1.075
	(0.018)			
Violent Crime Arrest Rate	0.00305	1.968	3.044	5.552
	(0.002)			
Drug Crime Arrest Rate	0.00388***	1.439	2.430	4.501
	(0.002)			
Demographic Controls				
South	0.567	1.763	1.763	1.763
	(1.047)			
Percent Black	-0.0267	0.920	0.812	0.669
	(0.061)			
Percent Hispanic	-0.351***	0.688	0.422	0.096
	(0.131)			
Percent Unemployment	0.523***	5.618	13.667	28.423
	(0.119)			
Percent Under Poverty Line	-0.230**	0.107	0.063	0.029
	(0.113)			
State Population Density	-0.00166	0.948	0.881	0.773
	(0.002)			
Percent Urban	0.00983	1.730	1.977	2.222
	(0.011)			
Political Controls				
Republican State House	0.439***	0.271	0.438	2.196
	(0.190)			
Republican State Senate	0.427***	3.657	1.988	0.426
	(0.192)			
Republican Governor	-0.692	0.501	0.501	0.501
	(0.551)			
State Revenues	-0.133	0.703	0.627	0.558
	(0.151)			
Prior Reform	1.548*	4.702		
	(0.836)			
Time Controls		**Rate Change**		
Years 1986-1993	2.399***	11.012		
	(0.848)			
Years 1994-2008	1.463	4.319		
	(1.227)			
		Base Rate		
Constant	-8.522***	0.000		
	(1.498)			
Log Likelihood	29.770			
Observations	1715			

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Note: Model 1 of Table 5.6.

Table D.3. Statutory Presumptive Sentencing

	Model 2	25 Percentile	50 Percentile	75 Percentile
Crime & Justice Controls				
Imprisonment Rate	-0.00207	0.995	0.984	0.969
	(0.108)			
Violent Crime Arrest Rate	0.00666*	4.386	11.369	42.229
	(0.004)			
Drug Crime Arrest Rate	0.000978	1.096	1.251	1.461
	(0.003)			
Demographic Controls				
South	-10.94	0.000	0.000	0.000
	(33.270)			
Percent Black	-0.337	0.348	0.072	0.006
	(0.217)			
Percent Hispanic	0.224**	1.270	1.734	4.464
	(0.095)			
Percent Unemployment	0.873***	17.830	78.649	266.987
	(0.239)			
Percent Under Poverty Line	-0.320	0.045	0.021	0.007
	(0.255)			
State Population Density	0.00891**	1.332	1.980	3.987
	(0.004)			
Percent Urban	-0.00751	0.658	0.594	0.543
	(0.026)			
Political Controls				
Republican State House	0.982***	0.054	0.158	5.810
	(0.324)			
Republican State Senate	-0.0122	1.038	1.020	0.976
	(0.335)			
Republican Governor	-3.287*	0.037	0.037	0.037
	(1.977)			
State Revenues	1.444	45.973	157.783	558.201
	(0.820)			
Prior Reform	-11.82	0.000	0.000	0.000
	(48.670)			
Time Controls		**Rate Change**		
Years 1986-1993	-21.56	0.000		
	149.880			
Years 1994-2008	-27.34	0.000		
	47.990			
		Base Rate		
Constant	-15.52***	0.000		
	(3.799)			
Log Likelihood	21.492			
Observations	1769			

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Note: Model 2 of Table 5.6.

Table D.4. Determinate Sentencing

	Model 3	25 Percentile	50 Percentile	75 Percentile
Crime & Justice Controls				
Imprisonment Rate	0.0161	1.042	1.137	1.275
	(0.021)			
Violent Crime Arrest Rate	-0.00328	0.483	0.302	0.158
	(0.002)			
Drug Crime Arrest Rate	0.00457*	1.535	2.846	5.882
	(0.002)			
Demographic Controls				
South	-0.512	0.599	0.599	0.599
	(1.389)			
Percent Black	0.0602	1.207	1.599	2.473
	(0.072)			
Percent Hispanic	-0.208*	0.801	0.600	0.249
	(0.114)			
Percent Unemployment	0.480***	4.874	11.023	21.585
	(0.148)			
Percent Under Poverty Line	-0.0579	0.570	0.499	0.412
	(0.134)			
State Population Density	-0.00563*	0.834	0.649	0.417
	(0.003)			
Percent Urban	0.0191*	2.902	3.758	4.719
	(0.011)			
Political Controls				
Republican State House	0.540**	0.201	0.362	2.631
	(0.236)			
Republican State Senate	0.659***	7.397	2.888	0.268
	(0.224)			
Republican Governor	0.651	1.917	1.917	1.917
	(0.719)			
State Revenues	-0.095	0.778	0.717	0.660
	(0.254)			
Prior Reform	4.967 ***	143.595	143.595	143.595
	(1.162)			
Time Controls		**Rate Change**		
Years 1986-1993	-0.850	0.427		
	(1.264)			
Years 1994-2008	-0.922	0.398		
	(1.419)			
		Base Rate		
Constant	-11.130	0.000		
	(1.960)			
Log Likelihood	15.337			
Observations	1767			

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Note: Model 3 of Table 5.6.

Table D.5. Truth in Sentencing

	Model 4	25 Percentile	50 Percentile	75 Percentile
Crime & Justice Controls				
Imprisonment Rate	0.0098	1.025	1.082	1.159
	(0.016)			
Violent Crime Arrest Rate	-0.000518	0.891	0.828	0.747
	(0.001)			
Drug Crime Arrest Rate	0.00398***	1.452	2.486	4.679
	(0.001)			
Demographic Controls				
South	-2.837*	0.059	0.059	0.059
	(1.149)			
Percent Black	0.0741	1.261	1.783	3.048
	(0.050)			
Percent Hispanic	-0.185***	0.821	0.635	0.291
	(0.062)			
Percent Unemployment	0.181	1.817	2.472	3.185
	(0.213)			
Percent Under Poverty Line	0.0885	2.360	2.892	3.883
	(0.083)			
State Population Density	-0.00228	0.929	0.840	0.702
	(0.001)			
Percent Urban	0.012	1.953	2.297	2.651
	(0.013)			
Political Controls				
Republican State House	0.293**	0.419	0.576	1.690
	(0.143)			
Republican State Senate	0.109	1.392	1.192	0.804
	(0.156)			
Republican Governor	0.787	2.197	2.197	2.197
	(0.510)			
State Revenues	-0.555***	0.230	0.143	0.088
	(0.217)			
Prior Reform	1.897***	6.666	6.666	6.666
	(0.578)			
Time Controls		**Rate Change**		
Years 1986-1993	0.505***	1.657		
	0.147			
Years 1994-2008	3.935***	51.162		
	(0.139)			
		Base Rate		
Constant	-12.46	0.000		
	(1.752)			
Log Likelihood	76.135			
Observations	1754			

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Note: Model 4 of Table 5.6.

APPENDIX E

DESCRIPTIVE STATISTICS

Table E.1. Descriptive Statistics

	Mean	Std. Dev.	Min	Max
Dependent Variables (DV)				
Change in Imprisonment Rate*	8.344	13.332	-116.138	131.602
Black to White Impris. Rate**	6.992	4.159	0.035	28.177
Hispanic to White Impr. Rate**	1.856	2.222	0.001	13.771
Female to White Impris.***	0.057	0.030	0.054	0.248
Crime Variables				
Violent Crime Rate	156.834	115.292	58.996	725.512
Drug Crime Rate	272.446	209.335	13.221	1337.498
Female to Male Violent Crime	0.139	0.057	0.055	0.455
Female to Male Drug Crime	0.191	0.074	0.078	0.957
Female to Male Property Crime	0.308	0.115	0.056	1.382
Demographic Variables				
Percent Black	10.288	9.304	0.020	97.274
Percent Hispanic	5.685	7.868	0.247	65.433
Percent Unemployment	5.056	2.417	0.000	18.000
Percent Under the Poverty Line	12.895	4.330	2.900	31.500
Population Density	139.427	183.367	0.385	997.349
Percent Urban	66.922	18.837	0.000	182.590
South^	-	-	0.000	1.000
Political Variables				
Republican House	-0.853	2.434	-4.074	3.689
Republican Senate	-0.760	2.499	-3.912	3.734
Republican Governor^	-	-	0.000	1.000
State Revenues	359079.700	239502.100	127068.000	3700724.000
Prior Reform^	-	-	0.000	1.000
Imprisonment Variables				
New Commitment	86.318	150.139	7.0529	3237.291
Parole Violators Returned	25.282	33.865	0	267.844

^ Indicates Dummy Variable, * DV Chapter 1, ** DV Chapter 2, *** DV Chapter 3

Note: Table E.1. gives the mean, standard deviation (Std. Dev.) and the range (Min and Max) for all variables used in this analysis with the exception of the sentencing reform variables.

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