OF FRAGMENTATION AND FERMENT
The Impact of State Sentencing Policies on Incarceration Rates, 1975-2002

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Overview

What accounts for the size of state incarceration rates in the United States? While no consensus has been achieved, many demographic, social, economic, political, and ideological factors have been associated with differences in the size and growth of state incarceration rates. As states struggle to balance public safety with the need to curtail growth in prison populations, many policymakers seek a better understanding of those factors affecting incarceration rates, particularly the impact of sentencing and corrections policies. Yet, while many analysts attribute the increases in state prison populations over the last 30 years to the sentencing and corrections policies enacted since the late 1970s, few studies have systematically assessed the impact of such policies on incarceration rates. Given the wide variation in the growth of state prison populations, understanding the impact of different policies on imprisonment is critical to understanding recent sentencing reform efforts in the states and the potential costs of future policies to the criminal justice system.

In 2002, the Vera Institute of Justice received funding from the National Institute of Justice to conduct a comprehensive survey of state-level sentencing and corrections policies implemented between 1975 and 2002 and to assess the impacts of those policies on state incarceration rates during the period. The project first built a conceptual framework for understanding the types of state-level sentencing and corrections policies in use between 1975 and 2002. Major characteristics of state sentencing systems were examined including indeterminate/determinate sentencing structures, sentencing guidelines and sentencing commissions, sentences for drug offenses, truth-in-sentencing laws, habitual offender laws, mandatory sentencing laws, and "good time" sentence reduction policies. This simple schematic was then expanded to focus the examination on the complex, internal characteristics of each policy that vary across states and over time. The ultimate goal was to produce an historical overview of the types of policies adopted over the last 30 years, the timing of adoption of each policy in each state, and the way each policy differs across states over time.

The project then identified and examined the ways in which various sentencing and corrections policies affected state prison populations between 1975 and 2002. Using a pooled time-series cross-sectional design, employing data for all 50 states from several government sources, and controlling for a host of demographic, economic, ideological, and crime-related variables, we sought to isolate the influence of sentencing and corrections policies on changes in state incarceration rates between 1975 and 2002. Prior studies analyzing variation in state prison populations have been limited to short time frames and have largely failed to consider the impact of different policies on imprisonment. This analysis extends previous work and overcomes these limitations by broadening the theoretical scope of the inquiry to include political, cultural, economic and policy variables and extending the time frame to cover a 28 year period from 1975 to 2002.

Findings

This report considers the impact of six sentencing and corrections policies on state incarceration rates between 1975 and 2002: determinate sentencing, sentencing guidelines, time served requirements, sentences for sale and possession of cocaine, habitual offender laws, and mandatory sentencing laws for weapons use, offenses against protected persons, and offenses committed while in state custody. Our primary findings include:

Policy Impacts

- States with the combination of determinate sentencing (i.e. the abolition of discretionary parole release) and presumptive sentencing guidelines had lower incarceration rates and smaller growth in incarceration rates than other states. Either policy alone was not related to the size or growth of incarceration rates.
- States with the combination of determinate sentencing and voluntary sentencing guidelines had larger growth in incarceration rates than other states; however, the combination of policies was not related to the size of incarceration rates. Again, either policy alone was not related to the size or growth of incarceration rates.
- States with separate time served requirements for violent offenders had higher incarceration rates than other states. However, higher time served requirements for all offenders was not related to incarceration rates.
- States with more provisions enhancing sentences for drug offenses – such as sale near a school, sale to a minor, or possession of a weapon during a drug offense – had higher incarceration rates than other states.
- States with higher statutory minimum sentences for cocaine possession had higher incarceration rates than other states. However, states with higher statutory maximum sentences for cocaine possession had lower incarceration rates than other states. Statutory sentence ranges for cocaine sale were not related to incarceration rates.
- States with more mandatory sentencing laws had higher incarceration rates than other states. However, habitual offender laws for second- or third-time offenders were not related to incarceration rates.

Non-Policy Impacts

- States with higher property crime rates experienced larger growth in incarceration rates than other states. However, neither violent crime rates nor increases in violent crime rates were related to the size or growth of state incarceration rates.
- States with larger minority populations had higher incarceration rates than other states. Further, the relationship between the size of the black population in a state and incarceration rates was increasingly stronger in the late 1990s than in other periods. However, the size of the minority population was not related to growth in incarceration rates.

- States with higher income per capita and more generous welfare benefits had lower incarceration rates than other states. In contrast, states with more revenue per capita had higher incarceration rates.
- Unemployment rates throughout the entire period were not associated with the size of state incarceration rates; however, higher unemployment rates in the 1990s were associated with higher incarceration rates. Further, states with higher unemployment rates and greater increases in unemployment rates had larger growth in incarceration rates.
- States in which a higher number of arrests were for drug offenses and states with more law enforcement personnel per capita had higher incarceration rates than other states.
- States with Republican governors and more religiously conservative citizens had higher incarceration rates than other states. Politically conservative citizens were also associated with higher incarceration rates in the late 1990s.

A society's approach to punishment is driven by a variety of objectives and determinants and, in the end, is "overdetermined" by a variety of forces (Garland, 1990). Our results confirm this. Differences in the size of racial and ethnic populations, the size of economically disadvantaged groups, wealth, and politics all influence incarceration rates in different ways and help explain the differences in the size and growth of state incarceration rates. However, they do not explain all of the differences. This report shows that the policies adopted by officials affect prison populations and that those policy impacts can be measured. The remainder of this report presents our analyses of the impact of six policies on incarceration rates in the states.

Chapter One: Policies and Imprisonment

The indeterminate sentencing structures that dominated state systems through the 1970s fragmented over the last 30 years, replaced by patchworks of determinate and structured sentencing, mandatory sentencing, habitual offender laws, and truth-in-sentencing laws. Through a series of progressions and regressions, states have adopted, abandoned, or altered various sentencing strategies at different points in time to address diverse and often conflicting objectives. After 30 years of experimentation and flux, the fragmentation in sentencing and corrections policies across states has created an array of approaches to the use of imprisonment as numerous as they are complex.

This fragmentation is clearly drawing notice. The American Law Institute, for example, is revisiting the sentencing provisions of the Model Penal Code in an effort to address the increasing variation in sentencing approaches in the United States (Reitz, 2001a). The American Bar Association reviewed state sentencing policies in the 1993 ABA standards and in the recent formation of the ABA Justice Kennedy Commission in an effort to examine change in the state and federal sentencing systems. The National Institute of Justice has produced several publications aimed at describing and cataloguing states' varied approaches to individual sentencing policies including sentencing guidelines (Lubitz and Ross, 2001; Parent, et. al., 1996b), mandatory sentencing laws (Parent et. al, 1996a), habitual offender laws (Henry, Austin, and Clark, 1997), and truth-in-sentencing laws (Sabol et. al., 2002). The Bureau of Justice Assistance (BJA) has also published two surveys that attempt to catalog the disparate state-level sentencing systems and corrections policies currently in force (BJA, 1996; 1998).

The manifold policy changes have accompanied a dramatic rise in state incarceration rates. The stability of incarceration rates once predicted in the United States has dissolved over the last 30 years, replaced by ever increasing state prison populations that exceeded 1.2 million persons by 2002 (Harrison and Beck, 2003). Nationally, incarceration rates quadrupled between 1970 and 2002; in some states, they increased as much as 1,000 percent during the same period. After 30 years of instability and expansion, the explosion in the number of persons incarcerated across states has created diverse variations in the size and rates of growth in state incarceration rates in the United States.

This variation in state incarceration rates is drawing equal interest. Scholars and practitioners continue to struggle to explain the rapid growth in incarceration rates in the United States and the variation in the use of imprisonment across states. Many point to the state-level policy changes adopted since the 1970s as the primary factor explaining this variation and growth (Blumstein, 1988; Casper, 1984; Jones and Austin, 1995; Joyce, 1992; Mauer, 2001); yet, few studies have systematically assessed the impact of state policies on incarceration rates. Further, few understand the range and variability of sentencing systems that exist in the United States, the state-level changes in those systems, or the connections between policies and clusters of policies.

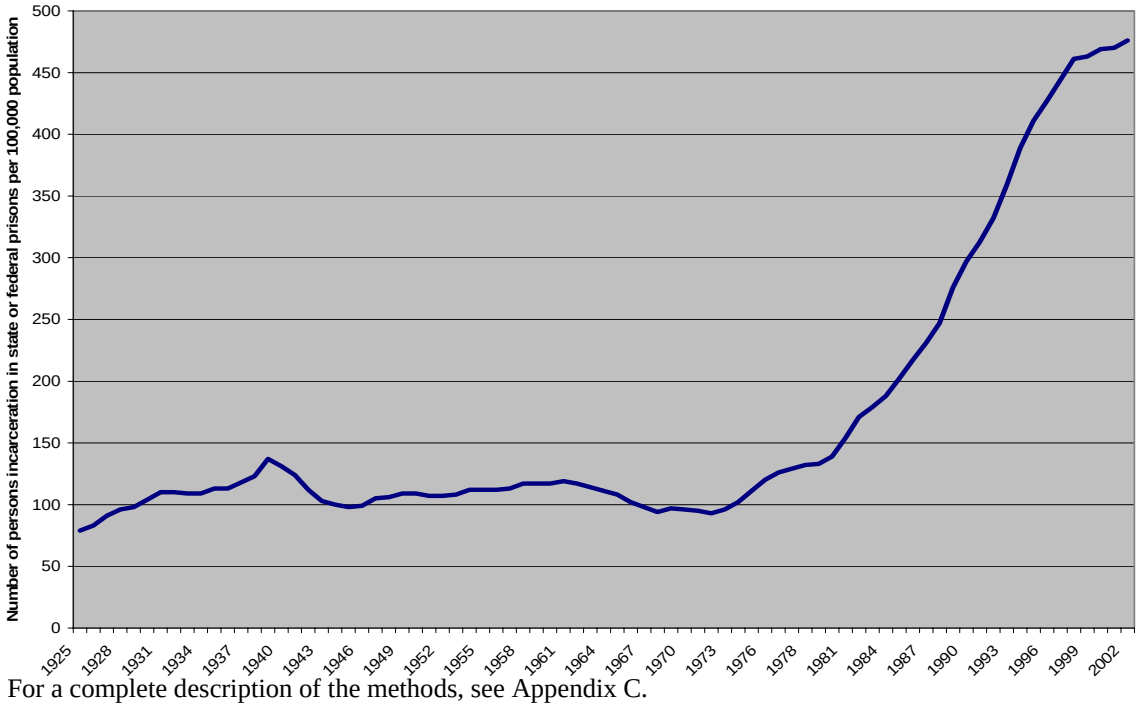
As a result, there is a shortage of comparative work critically examining the impact of policy reforms on state prison populations.

This chapter examines the growth in state incarceration rates and the changes in state sentencing and corrections policies over the last 30 years. The following chapter provides an initial examination of the factors affecting incarceration rates and a description of the methods employed in the analyses included in this report.[1] The remaining chapters then look at the impact of different policies and combinations of policies on incarceration rates across the states.

The Size and Growth in State Incarceration Rates

Through the early 1970s, several criminologists embraced the idea that societies maintain, in the long run, a stable level of imprisonment. This perspective – the "stability of punishment hypothesis" – was significantly developed by Alfred Blumstein and his colleagues (Blumstein and Cohen, 1973; Blumstein, Cohen and Nagin, 1976). Basing their empirical studies on Durkheim's postulates on the normality of crime and the need of deviance to foster social cohesion (1893 [1964]), Blumstein and Cohen (1973) claimed that shifts in the "behavior distribution," or the amount of crime in society, are followed by policy changes in order to maintain a constant incarceration rate. In short time periods, incarceration rates may display wide variation; but over long time periods, Blumstein and Cohen argued, incarceration rates displayed relative stability. Incarceration rates in the United States, indeed, displayed a relative stability at just over 100 persons per 100,000 through 1975 (see Exhibit 1-1).

Exhibit 1-1. U.S. Incarceration Rate, 1925-2002



[1] For a complete description of the methods, see Appendix C.

These equilibrium-based studies have been heavily criticized on both theoretical and methodological levels (Rauma, 1981; Berk et al., 1981). But the actual upward trend in incarceration rates in the United States at the time of Blumstein and Cohen's analyses ultimately resulted in a loss of interest in these postulates. The last three decades have seen increases in incarceration rates unparalleled in U.S. history. Between 1970 and 2002, the incarceration rate in the United States increased 390 percent, from 87 inmates per 100,000 residents to 427 inmates per 100,000 residents. While all states experienced significant growth in prison populations during this period, national figures hide the variation in the rates of growth across the states. In North Carolina, for example, the incarceration rate increased 125 percent between 1970 and 2002, while in Delaware it expanded 1,264 percent (see Table 1-1).

National figures also hide the variation in the rates of growth within individual states over time. In Colorado, for example, the incarceration rate grew just 20 percent between 1970 and 1985 but increased 303 percent in the next fifteen years; in contrast, the incarceration rate in Alaska increased 746 percent between 1970 and 1985 but rose just 61 percent in the next fifteen years (see Exhibit 1-2 and Table 1-2).

Exhibit 1-2. Percentage Increase in Incarceration Rates, 1970-1985 and 1985-2002, States with Largest Variation in Growth Between Periods

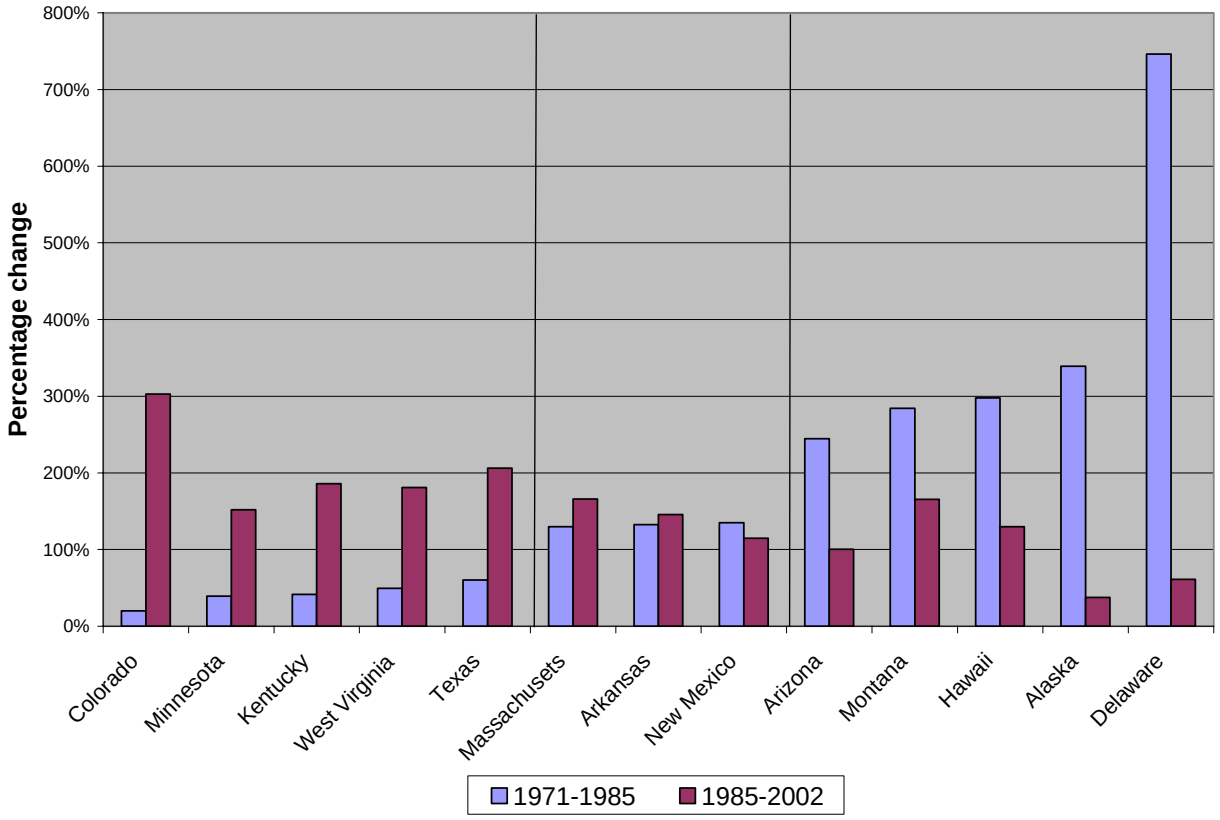


Table 1-1. State Incarceration Rates, 1970 and 2002

State	Incarceration Rate, 1970	Incarceration Rate, 2002	Percentage Change
Alabama	110	612	456
Alaska	66	396	504
Arizona	74	513	590
Arkansas	84	479	471
California	87	452	417
Colorado	86	415	383
Connecticut	63	405	540
Delaware	33	453	1,264
Florida	136	450	231
Georgia	146	552	278
Hawaii	34	308	814
Idaho	49	461	843
Illinois	52	336	541
Indiana	83	348	320
Iowa	54	248	363
Kansas	91	327	261
Kentucky	94	380	304
Louisiana	113	794	603
Maine	45	141	213
Maryland	125	425	240
Massachusetts	38	234	511
Michigan	106	501	371
Minnesota	40	141	251
Mississippi	83	743	798
Missouri	77	529	589
Montana	35	361	920
Nebraska	69	228	230
Nevada	124	483	290
New Hampshire	28	192	586
New Jersey	73	322	344
New Mexico	61	309	404
New York	65	346	432
North Carolina	153	345	125
North Dakota	21	161	656
Ohio	85	398	370
Oklahoma	144	667	363
Oregon	94	342	266
Pennsylvania	45	325	627
Rhode Island	41	191	372
South Carolina	118	555	369
South Dakota	58	378	554
Tennessee	86	430	399
Texas	141	692	391
Utah	53	233	337
Vermont	47	214	360
Virginia	109	460	322
Washington	82	261	217
West Virginia	60	250	319
Wisconsin	55	391	606
Wyoming	78	348	349
US	87	427	390

Table 1-2. Percentage Change in State Incarceration Rates, 1970-1985 and 1985-2002.

State	Percentage Change in Incarceration Rate, 1970-1985	Percentage Change in Incarceration Rate, 1985-2002
Alabama	143%	129%
Alaska	339%	38%
Arizona	245%	100%
Arkansas	132%	146%
California	107%	150%
Colorado	20%	303%
Connecticut	101%	219%
Delaware	746%	61%
Florida	82%	82%
Georgia	72%	120%
Hawaii	298%	130%
Idaho	172%	247%
Illinois	207%	109%
Indiana	111%	99%
Iowa	83%	153%
Kansas	112%	70%
Kentucky	41%	186%
Louisiana	173%	158%
Maine	84%	70%
Maryland	123%	52%
Massachusetts	130%	166%
Michigan	84%	156%
Minnesota	39%	152%
Mississippi	187%	214%
Missouri	153%	173%
Montana	284%	165%
Nebraska	56%	111%
Nevada	220%	22%
New Hampshire	143%	182%
New Jersey	106%	116%
New Mexico	135%	115%
New York	200%	77%
North Carolina	66%	36%
North Dakota	158%	193%
Ohio	129%	105%
Oklahoma	73%	167%
Oregon	76%	107%
Pennsylvania	166%	173%
Rhode Island	144%	93%
South Carolina	148%	89%
South Dakota	153%	159%
Tennessee	73%	189%
Texas	60%	206%
Utah	84%	138%
Vermont	76%	161%
Virginia	87%	125%
Washington	89%	67%
West Virginia	49%	181%
Wisconsin	104%	246%
Wyoming	91%	135%

Incarceration rates continue to vary substantially between states, ranging from 141 inmates per 100,000 residents in Maine and Minnesota to 794 inmates per 100,000 residents in Louisiana (see Table 1-1). Indeed, as the national and state incarceration rates increased, the difference between the highest and lowest state incarceration rates increased as well; incarceration became more fragmented as state incarceration rates increasingly differed in size. Exhibits 1-3 and 1-4 show the growth in the national incarceration rate between 1970 and 2002. The vertical lines in the graph represent the range of state incarceration rates for each year. For example, in 2002, the upper end of the vertical line represents those states with the highest incarceration rates while the lower end of the vertical line represents those states with the lowest incarceration rates. As the graph indicates, the ends of the vertical lines have continued to grow further apart between 1970 and 2002, indicating greater differences in incarceration rates between states over time.

Exhibit 1-3. Incarceration Rate 1971-2002, Mean and Standard Error

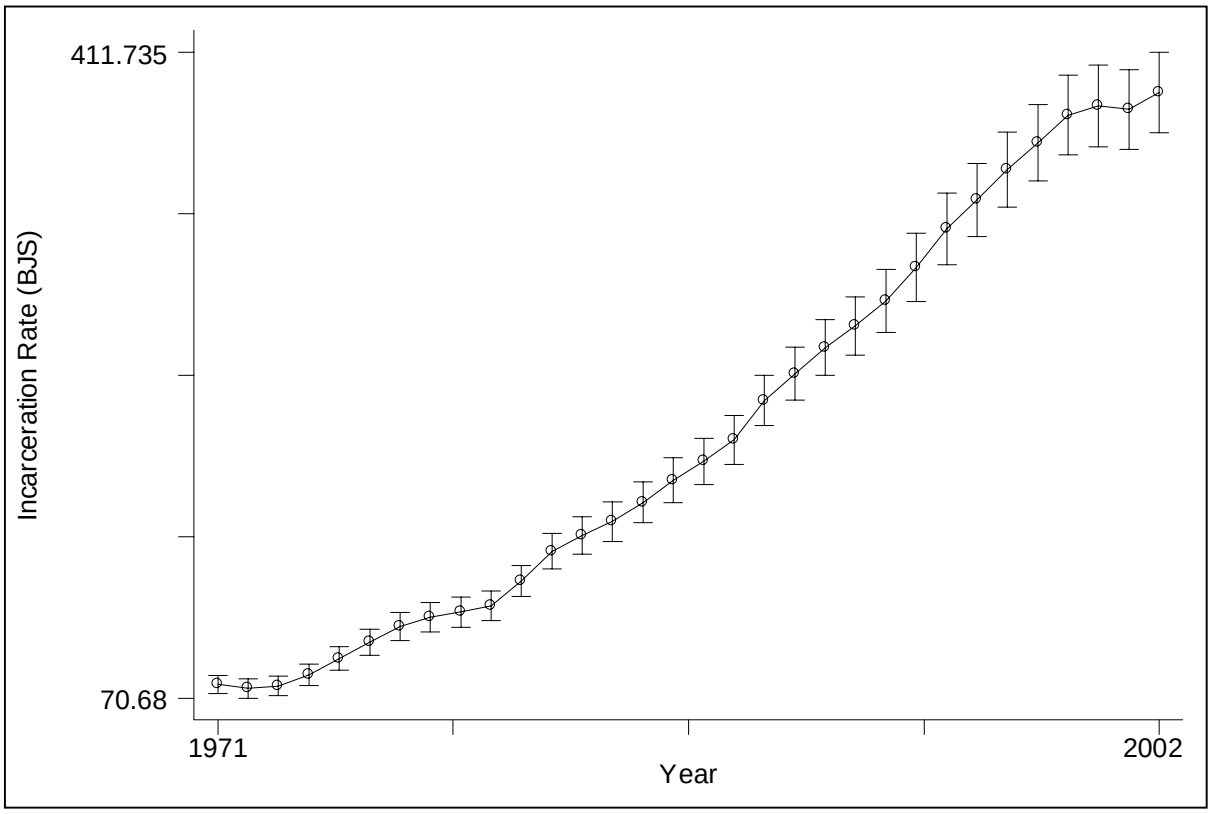
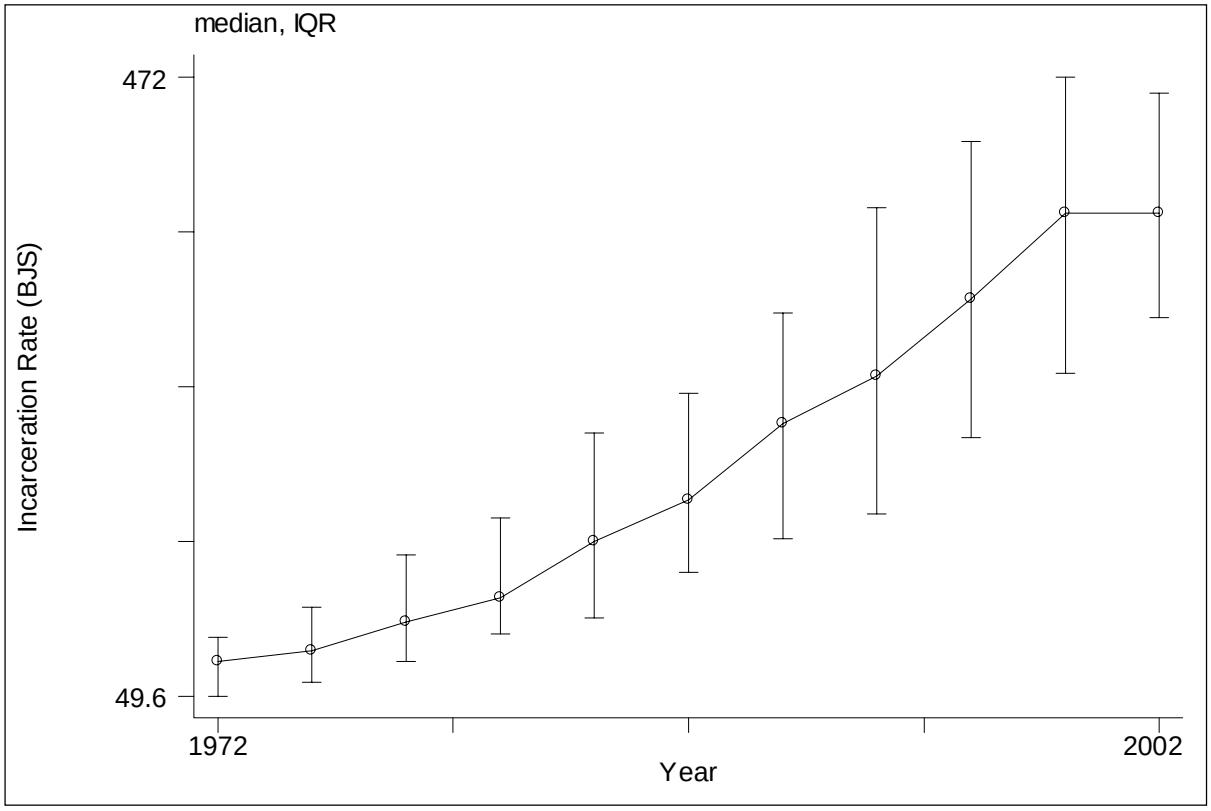


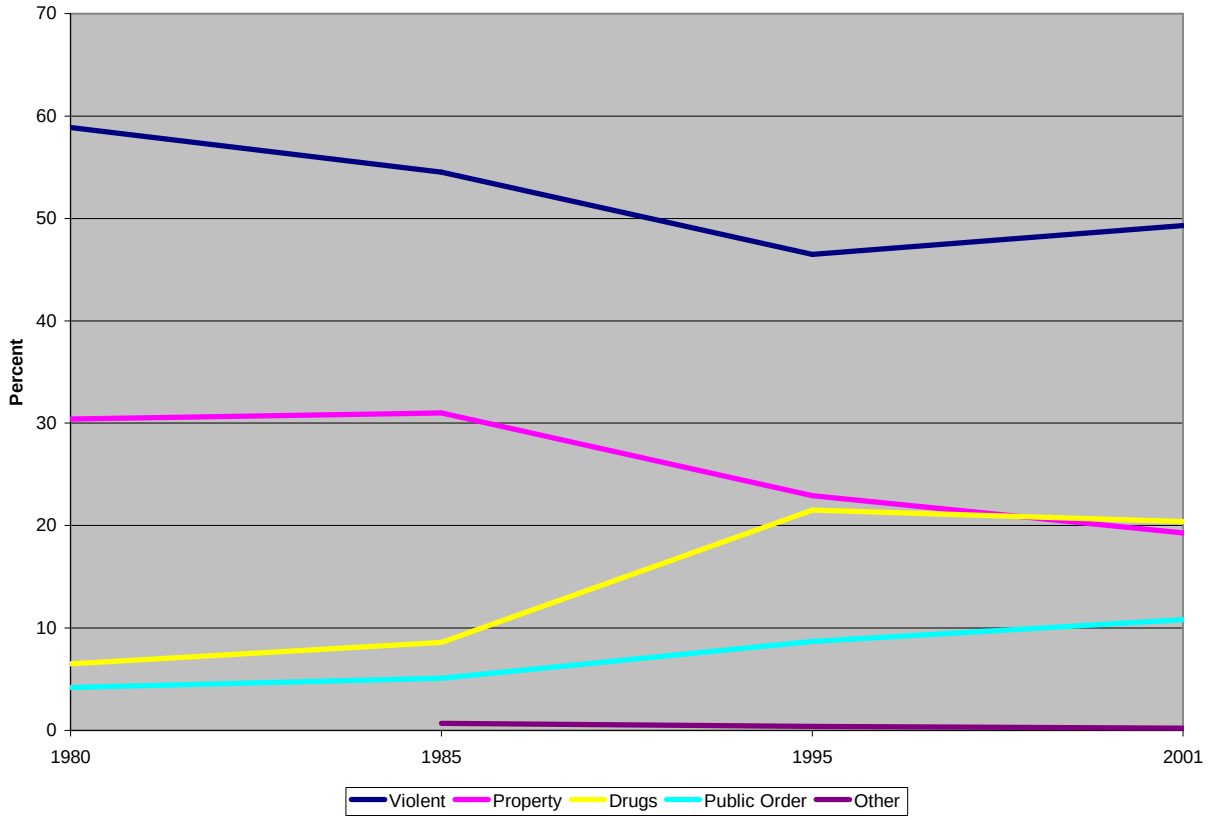
Exhibit 1-4. Incarceration Rates 1972-2002, Median and Inter-Quartile Range



The type of offender held in state prisons has also changed dramatically since the 1970s. Between 1975 and 2002, the proportion of state prisoners incarcerated for violent offenses steadily decreased while the percentage incarcerated for drug offenses and public order offenses steadily increased (see Exhibit 1-5).[2] The trend for drug offenses is consistent with the heightened sanctions associated with the "war-on-drugs," as well as the crack panics of the late 1980s.[3] The upward trend in public-order offenses has received less attention from criminal justice scholars and practitioners; while its growth is less pronounced than the trend for drug offenses, it does not appear to have reached a plateau by 2002. While the upward trend in the 1980s may be explained in part by increased sanctions for weapons violations, the continued increases in the 1990s may be due to the enforcement of "quality-of-life policing" directed at offenses such as driving under the influence.

[2] Public order offenses include: weapons offenses, driving under the influence, escape, court offenses, obstruction, commercialized vice, morals and decency charges, and liquor law violations.
[3] The impact of changes in drug sentencing on state incarceration rates will be discussed in Chapter Six.

Exhibit 1-5. Estimated Percent of State Prisoners by Most Serious Offense, 1980-2002



The racial composition of state prisons has also changed since the 1970s. In 1974, white offenders accounted for 51 percent of state prisoners; by 2001, they accounted for just 39 percent. White offenders were replaced partially by black offenders, who constituted 33 percent of state prison populations in 1974 and 39 percent in 2001. Most of the decrease in white offenders was replaced by an increase in Hispanic offenders; between 1974 and 2001, the percentage of state prisoners who were Hispanic increased from 6 percent to 18 percent. Again, these national estimates may hide the variation between states and within states over time. Across states, the proportion of black offenders in prison is between 2 and 13 times the proportion of black persons in the general population. Minority populations tend to be "more" overrepresented in state prisons in those states with a small minority population.[4]

The increases in the rate of incarceration across the states have had a significant impact on individuals and communities that will continue into the next decades. Between 1974 and 2001 the prevalence of imprisonment – the number of people that had ever served a sentence of

[4] While black offenders represented about two thirds of the prisoners in states such as Alabama, Georgia and Mississippi, these states also have the largest black populations. The same is true for the Hispanic population in the states of New Mexico, New York, Florida and California.

imprisonment in state prison – increased by 3.8 million; by 2001, over 5.6 million living Americans had ever been incarcerated in a state prison, nearly 2 percent of the U.S. population (Bonczar and Beck, 2003). According to the model developed by the Bureau of Justice Statistics, roughly 6.6 percent of all persons born in the U.S. in 2001 will be incarcerated during their lifetime; in 1974, only 1.9 percent of all persons born that year faced the same prospect.

Change in State Sentencing and Corrections Policies

The dramatic increases and increasing variation in state incarceration rates since the 1970s have accompanied equally striking changes in state sentencing and corrections policies. Through the 1970s, sentencing policy in all fifty states consisted of an indeterminate sentencing model, in which judges exercised broad discretion over the disposition and duration of sentences imposed and parole boards maintained authority over the duration of sentences served through discretionary release (Tonry, 1996; Griset, 1991; Rothman, 1983; Reitz, 2001b). The "indeterminacy" in the system referred to the relative disconnect between the length of sentence imposed by the sentencing court and the length of sentence actually served by an offender in prison prior to release on parole.

The broad discretion characteristic of the indeterminate system was based on the idea that individualization and rehabilitation should be the goals of sanctioning and could be achieved by tailoring a sentence to the unique characteristics of the offender (Blumstein, et. al. 1983; Frase, 1995; Griset, 1991). Under indeterminate sentencing, states generally set wide statutory sentence ranges for offenses which allowed a judge equally wide latitude to impose a sentence length anywhere within the range, based on his or her evaluation of the offense and the offender. Few restrictions were placed on a judge's ability to impose a particular sentence length or to suspend the sentence and place an offender on probation. Similarly, states set few restrictions on the time offenders were required to serve prior to release from prison and provided few criteria on which parole boards were to base release decisions; as a result, parole boards had wide discretion to release offenders at any time between some minimal time served and the statutory maximum for the offense.

The indeterminate system and its rehabilitative ideal, however, were attacked in the mid-1970s on two fronts (Reitz, 2001b). Many saw the potential for abuse and discrimination in the broad discretion available to judges and parole boards; others felt that penalties imposed by judges were too lenient and time served by offenders was too short (Griset, 1991). In the end, both sides supported sentencing and corrections systems that were 1) more "determinate" or guaranteed time served by offenders was primarily determined by the sentence imposed by the sentencing court or 2) more "structured" or ensured sentence lengths and dispositions were uniform and imposed according to a set of prescribed criteria (Shane-Dubow, 1998). The result was the initial adoption of sentencing systems in which discretionary release by a parole board was abolished or curtailed by parole guidelines and statutory sentence ranges available to judges

were constricted by statute. The goal was to control decision-making at sentencing and/or at release from prison.

The common goals underlying these initial reforms rested on the desire to equalize sentences, to fit punishments to crimes, and to abolish the variability of sentencing and release decisions (Reitz, 2001b; Shane-Dubow, 1998). Policy-makers realized that the goals of determinacy and structure could be combined with the goal of either increasing or decreasing the use of incarceration and sentence severity. Thus, while several states reformed policies in the late 1970s and early 1980s, the attacks on indeterminacy did not abate after these initial reforms (Reitz, 2001b). Critics continued to contend that more determinacy and structure were necessary to correct the system. Mandatory sentences, habitual offender laws, and truth-in-sentencing laws proliferated through the 1980s and 1990s as reformers sought ways to either fix the continued disparities in the system or impose harsher penalties. While the dissolution of a common approach to sentencing and corrections has not marked a wholesale rejection of the indeterminate ideal, the twin goals of determinacy and structure continue to appeal to policymakers and have shaped policy reforms through the 1990s (Tonry, 1999a; BJA, 1998). The following sections describe the policies most commonly adopted by states since the 1970s.

Determinate Sentencing

The initial turn from indeterminate sentencing was marked by a move to determinate sentencing. But the adoption of determinate sentencing was less about *sentencing* decisions and more about *release* decisions. Although the term determinate sentencing has been applied to several types of sentencing and corrections schemes, it essentially refers to a system *without* discretionary parole release as a mechanism for releasing offenders from prison (Reitz and Reitz, 1993; Tonry, 1987; BJA, 1996).[5] Under determinate sentencing systems, the sentencing judge imposes a prison term expressed as a number of years of imprisonment. Without discretionary parole release, offenders are then automatically released from prison after serving a statutorily-determined portion of the term imposed.[6] The "determinacy" in the system refers to the effort to ensure that time served by offenders is primarily determined by the length of the sentence imposed by the judge rather than by the discretionary release decision-making of the parole board (see Chapter Three).[7]

California and Maine were the first states to adopt a determinate sentencing system by abolishing discretionary parole release in 1976, followed by Indiana, New Mexico, Illinois, and

[5] Determinate sentencing has been used to describe 1) systems without discretionary parole release and 2) systems with "presumptive" recommended sentences for offenses. The former is the definition of determinate sentencing used here; the latter is the definition used to describe "structured sentencing" (see Chapter Four).

[6] This fixed term generally can be reduced only through sentence reduction credits (e.g. "good time" or "earned time"); in the absence of sentence reduction credits, offenders must serve 100 percent of the fixed term imposed by the court. See *Truth-in-Sentencing and Time Served Requirements* section for a description of the amount of time offenders must serve before release.

[7] This is not to say that, under determinate sentencing, states do require offenders to serve some form of supervision after release from prison (traditionally, referred to as parole). Many states without discretionary parole release, nonetheless, require offenders to serve a term of supervision after release from prison.

Minnesota. Between 1975 and 2002, 19 states adopted determinate sentencing systems for most offenses (see Table 1-3). Two additional states (Idaho and New York) adopted determinate sentencing for a significant number of offenses.[8]

Yet, while determinate sentencing is primarily about the absence of discretionary release from prison, the determinate systems ultimately adopted by the states varied widely in terms of the statutory sentence ranges set for offenses and the constraints placed on judges in setting sentences within those ranges. While many states, such as Maine, accompanied the adoption of determinate sentencing with revised and narrowed sentence ranges for offenses, several states, such as Illinois, retained the wide sentence ranges of the indeterminate model. Other states abandoning the indeterminate model, such as California and Minnesota, sought both determinacy and structure in their systems by prescribing "presumptive" sentences for judges to impose or by creating presumptive sentencing guidelines (see *Structured Sentencing* section below); the structures of these determinate systems were, and continue to be, diverse. Further, while many states have abandoned indeterminate sentencing by abolishing discretionary parole release, a majority of states continue to maintain indeterminate systems (see Table 1-4).

[8] Since 1986, judges in Idaho have had discretion to impose a determinate or indeterminate sentence for an individual offender. In 1996, New York established determinate sentences for all violent offenses by making offenders convicted of such offenses ineligible for parole; the state retained indeterminate sentences for all other offenses (see Chapter Three).

Table 1-3. States Adopting Determinate Sentencing Systems

State	Adoption Date
Arizona	1994
California	1976
Colorado	**1979 (indeterminate sentencing reinstated in 1985)**
Connecticut	**1981 (indeterminate sentencing reinstated in 1990)**
Delaware	1990
Florida	1983
Illinois	1978
Indiana	1977
Kansas	1993
Maine	1976
Minnesota	1980
Mississippi	**1995 (indeterminate sentencing reinstated in 2000 for first-time non-violent offenses only)**
New Mexico	1977
North Carolina	1981
Ohio	1996
Oregon	1989
Virginia	1995
Washington	1984
Wisconsin	1999

Table 1-4. States with Determinate or Indeterminate Sentencing for Most Offenses, 2002

State	Determinate Sentencing	Indeterminate Sentencing
Alabama		●
Alaska		●
Arizona	●	
Arkansas		●
California	●	
Colorado		●
Connecticut		●
Delaware	●	
Florida	●	
Georgia		●
Hawaii		●
Idaho		●
Illinois	●	
Indiana	●	
Iowa		●
Kansas	●	
Kentucky		●
Louisiana		●
Maine	●	
Maryland		●
Massachusetts		●
Michigan		●
Minnesota	●	
Mississippi	●	
Missouri		●
Montana		●
Nebraska		●
Nevada		●
New Hampshire		●
New Jersey		●
New Mexico	●	
New York		●
North Carolina	●	
North Dakota		●
Ohio	●	
Oklahoma		●
Oregon	●	
Pennsylvania		●
Rhode Island		●
South Carolina		●
South Dakota		●
Tennessee		●
Texas		●
Utah		●
Vermont		●
Virginia	●	
Washington	●	
West Virginia		●
Wisconsin	●	
Wyoming		●
Total	17	33

Structured Sentencing

While many states sought to increase the "determinacy" of their systems through the abolition of discretionary parole release, others sought more "structure" in their systems through the adoption of recommended or presumptive prison terms for offenses. States with structured sentencing seek to narrow or guide judicial discretion in determining the length of an imposed prison term by proscribing a recommended term within the wider statutory sentence range for an offense. Judges are expected to impose the recommended term; however, states generally allow a judge to impose a term of incarceration above or below this recommended term (up to the statutory maximum or down to the statutory minimum) based on aggravating or mitigating circumstances. The "structure" in the system refers to the effort to ensure that prison terms imposed for similar offenses or offenders are uniform and that the criteria for imposing sentences are consistent for all offenses and offenders. While determinate sentencing is about controlling release decisions and time served, structured sentencing is about controlling sentencing decisions and the length of prison terms imposed. Thus, in addition to distinguishing determinate and indeterminate systems, it is equally important to distinguish "structured" and "unstructured" systems (see Chapter Four)

States accomplished this structure through the creation of two similar, yet distinct, mechanisms. The first was "presumptive sentencing," or a system of single recommended prison terms or narrow sentence ranges within the wider statutory sentence range for each offense or offense class. The system is "presumptive" because it is presumed that the judge will impose the recommended prison term or a term from within the narrow recommended range; generally, a judge may impose a prison term that is longer or shorter than the recommended term or outside the recommended range *only* by a finding of aggravating or mitigating circumstances or by stating reasons for deviating from the recommended term. Between 1975 and 2002, nine states adopted some form of presumptive sentencing system (see Table 1-5).

Table 1-5. States Adopting Presumptive Sentencing

State	Adoption date
Alaska[9]	1980 (statutorily created for some first offenses and all second and third offenses) 1981 (judicially created "benchmarks" for all first offenses)
Arizona	1978
California	1976
Colorado	1979
Indiana	1977
New Jersey	1977
New Mexico	1977
Ohio	1996
Rhode Island[10]	1981 (judicially created "benchmarks" for all offenses) 1992 (statutorily created only for offenses comprising more than 5 percent of criminal caseloads)

While many states sought structure in their systems through the adoption of presumptive sentencing, others sought additional structure through the second mechanism – the adoption of "sentencing guidelines." At their base, sentencing guidelines are a system of multiple recommended sentences *and* dispositions and a set of procedures designed to guide judicial sentencing decisions and sentencing outcomes. Although recommended prison terms under presumptive sentencing systems are determined entirely by the severity of the current offense, under sentencing guidelines, recommended prison terms are generally determined according to the severity of the offense committed *and* the prior criminal history of the offender; thus, each offense or offense class will have multiple sentence recommendations under sentencing guidelines based on the prior criminal history of the offender. The intent is to ensure that all offenders committing similar offenses and with similar criminal histories receive nearly identical sentences under sentencing guidelines.

[9] In 1980, the Alaska legislature created presumptive sentences for the first-time commission of some felonies and the second- and third-time commission of all felonies. In 1981, the Alaska Court of Appeals developed a series of "benchmarks," or presumptive sentences, for the first-time commission of offenses without statutory presumptive sentences.

[10] In 1981, the Rhode Island Superior Court created a set of "sentencing benchmarks" that judges were advised to follow at sentencing (see R.I. Rules of Court, Superior Court Sentencing Benchmarks). According to the policy statement accompanying the benchmarks, "In order to eliminate, insofar as possible, disparity in the sentencing of defendants for crimes committed under the same or similar circumstances, the court may consider and utilize the sentencing benchmarks formulated by the Supreme Court Committee on Sentencing as guidelines."

At the most basic level, sentencing guidelines systems are divided into "presumptive sentencing guidelines" systems and "voluntary sentencing guidelines" systems. The degree to which states use formal legal authority to constrain judicial sentencing decisions distinguishes the two systems.[11] Presumptive sentencing guidelines require judges to impose the sentence recommended by the guidelines or provide written justification for imposing some other sentence; sentences that do not adhere to the sentence recommendations of the guidelines may be appealed by either the defendant or the prosecution. Thus, states with presumptive sentencing guidelines employ appellate review of sentences to ensure that sentences adhere to the sentencing guidelines. In contrast, states with voluntary sentencing guidelines do not require judges to impose the sentence recommended by the guidelines; while judges under voluntary sentencing guidelines systems may be required to provide reasons for not imposing the term recommended, sentences that do not adhere to the recommendations may not be appealed by either the defendant or the prosecution. Thus, states with voluntary sentencing guidelines lack any appellate review of sentences or other formal legal authority to ensure that sentences adhere to sentencing guidelines.

Minnesota was the first state to adopt presumptive sentencing guidelines in 1980, followed closely by Pennsylvania and Washington. Between 1980 and 2002, 17 states adopted some form of sentencing guidelines (see Table 1-6).[12] To date, there have been nine presumptive guidelines systems and ten voluntary guidelines systems adopted by states; two states – Florida and Michigan – originally adopted voluntary guidelines systems which were later repealed and replaced with presumptive guidelines. Further, Wisconsin originally adopted voluntary guidelines in 1985, which were repealed in 1994; in 1999, the state adopted a new version of voluntary guidelines.

[11] At a more complicated level, sentencing guidelines vary in their attempt to structure sentencing processes as well as sentencing outcomes. Thus, states may appear more presumptive or voluntary in terms of both sentencing outcome and process.

[12] Oklahoma also adopted voluntary sentencing guidelines in 1997. However, the state repealed the guidelines in 1999 before they became effective.

Table 1-6. States Adopting Sentencing Guidelines

	Adoption Date	
State	**Presumptive Guidelines**	**Voluntary Guidelines**
Arkansas		1994
Delaware		1987
Florida	1994	**1983 (converted to presumptive guidelines in 1994)**
Kansas	1993	
Louisiana		1987
Maryland		1983
Michigan	1999	**1985 (converted to presumptive guidelines in 1999)**
Minnesota	1980	
Missouri		1997
North Carolina	1995	
Oregon	1989	
Pennsylvania	1982	
Tennessee	1989	
Utah		1985
Virginia		1995
Washington	1984	
Wisconsin		**1985 (repealed in 1994)** **1999**

Thus, between 1975 and 2002, 26 states adopted some form of structured sentencing systems – nine presumptive sentencing systems and 17 sentencing guidelines systems. Each of these types of systems has been implemented with and without parole, creating different combinations of determinate/indeterminate and structured/unstructured sentencing systems (see Table 1-7).

Table 1-7. Determinate and Structured Sentencing, 2002

State	Determinacy		Structure		
	Determinate	Indeterminate	Presumptive Sentencing	Presumptive Guidelines	Voluntary guidelines
Alabama		●			
Alaska		●	●		
Arizona	●		●		
Arkansas		●			●
California	●		●		
Colorado		●	●		
Connecticut		●			
Delaware	●				●
Florida	●			●	
Georgia		●			
Hawaii		●			
Idaho		●			
Illinois	●				
Indiana	●		●		
Iowa		●			
Kansas	●			●	
Kentucky		●			
Louisiana		●			●
Maine	●				
Maryland		●			●
Massachusetts		●			
Michigan		●		●	
Minnesota	●			●	
Mississippi	●				
Missouri		●			●
Montana		●			
Nebraska		●			
Nevada		●			
New Hampshire		●			
New Jersey		●	●		
New Mexico	●		●		
New York		●			
North Carolina	●			●	
North Dakota		●			
Ohio	●		●		
Oklahoma		●			
Oregon	●			●	
Pennsylvania		●		●	
Rhode Island		●	●		
South Carolina		●			
South Dakota		●			
Tennessee		●		●	
Texas		●			
Utah		●			●
Vermont		●			
Virginia	●				●
Washington	●			●	
West Virginia		●			
Wisconsin	●				●
Wyoming		●			

Truth-in-Sentencing and Time Served Requirements

While a majority of states maintain the broad statutory sentence ranges and case-specific discretion characteristic of the indeterminate system, many states have sought greater determinacy through the adoption of Truth-in-Sentencing laws. Like determinate sentencing, Truth-in-Sentencing laws seek to ensure that time served by offenders is primarily determined by the length of the sentence imposed by the sentencing court rather than by the discretionary decision-making of a parole board. As such, Truth-in-Sentencing laws call for offenders to serve a large portion of the prison sentence imposed by the court before becoming eligible for release from prison. Such laws have been combined with both determinate and indeterminate sentencing systems.

In 1994, the federal government enacted legislation creating federal Violent Offender Incarceration and Truth-in-Sentencing (VOI/TIS) grants for states. Under the program, states requiring violent offenders to served 85 percent of the sentence imposed by the court could receive funding from the federal government to expand jail and prison capacity and to ensure that prison space was reserved for violent offenders.[13] While defined under federal guidelines as requiring violent offenders to serve 85 percent of their imposed sentences, the actual percentage of sentence required and the offenses subject to the policy vary by state. For example, while Arizona requires all violent offenders to serve 100 percent of the sentence imposed, Maine and Illinois require violent offenders to serve 85 percent of the sentence imposed and Oregon requires violent offenders to serve 70 percent. States had discretion to define "violent offenses" under their Truth-in-Sentencing legislation. By 2002, 28 states had adopted Truth-in-Sentencing laws requiring violent offenders to serve at least 85 percent of the sentence imposed by the sentencing court before becoming eligible for release from prison (Sabol, et al., 2002).[14]

While the federal Truth-in-Sentencing grant guidelines have been used generally to describe states' time served requirements for violent offenders, the notion of "truth in sentencing" may be expanded to include any restrictions placed on the release of offenders. Many states now require all offenders to serve high percentages of their imposed sentences. Kansas, for example, requires all offenders to serve 85 percent of the sentence imposed before release from prison; Ohio requires all offenders to serve nearly 100 percent of the sentence imposed. With this more expansive notion, states have exhibited a continued increase in the amount of time all offenders must serve in prison prior to release. However, comparing time served requirements across states raises several difficulties. Some states set time served requirements according to the maximum or fixed term imposed by the court (i.e. requiring offenders to serve a certain percentage of the maximum term before they are eligible for parole or release), while other states

[13] These grants are no longer available.

[14] These states are: Arizona, California, Connecticut, Delaware, Florida, Georgia, Illinois, Iowa, Kansas, Louisiana, Maine, Michigan, Minnesota, Mississippi, Missouri, New Jersey, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Utah, Virginia, and Washington.

set time served requirements according to the minimum term imposed by the court.[15] Nonetheless, under either process, time served requirements across the states have shown significant increases over the last 30 years (see Chapter Five).

Exhibit 1-6 shows changes in time served requirements across states that base time served on the *maximum or fixed term* imposed by the court. The middle line represents the average percentage of the term offenders are required to serve before release from prison. In 1975, offenders were required to serve an average of 28 percent of the term imposed before release from prison; by 2002, this had increased to 45 percent. As the lower line in Exhibit 1-6 indicates, some indeterminate sentencing states allow offenders to be released from prison any time after admission; thus, the minimum time served requirement in these states is 0 percent.[16] Conversely, by 2002, some determinate sentencing states required all offenders to serve 100 percent of the maximum or fixed term imposed by the court (as indicated by the upper line in the graph).

Exhibit 1-7 shows the changes in time served requirements across states that base time served on the *minimum term* imposed by the court. Again, the middle line represents the average percentage of the term offenders are required to serve before release from prison. In 1975, offenders were required to serve an average of 70 percent of the minimum term imposed before release from prison; by 2002, this had increased to 93 percent. As the lower line in Exhibit 1-7 indicates, in 1975 some states allowed offenders to be released from prison any time after admission; thus, time served in these states was 0 percent. However, by 1981, all states required offenders to serve some portion of the minimum term imposed; indeed, by 2002, no state allowed offenders to be released prior to serving at least 50 percent of the minimum term imposed. As the upper line indicates, some states require offenders to serve 100 percent of the minimum term imposed prior to release.

[15] Only indeterminate states base time served on the minimum term imposed. The term imposed in a determinate sentencing system always functions as a maximum term, representing the longest amount of time an offender could serve in prison in the absence of sentence reduction credits.

[16] For example, in Alabama and North Dakota, the parole board had, and continues to maintain, the authority to release an offender *at any time* after the sentence is imposed. In Alabama, all offenders must serve 1/3 of their sentence before becoming parole eligible; however, the Board of Pardons and Paroles may parole any offender prior to 1/3 of sentence served by unanimous vote (AL Code 15-22-28(e)); thus, any offender can be released immediately after entering prison. Similarly, in North Dakota, while offenders sentenced for violent offenses must serve 85% of their sentences before becoming parole eligible, all other offenders can be released on parole after a "reasonable period" (NCC 12-59-07).

Exhibit 1-6. Time Served Requirement, Based on Maximum or Fixed Term Imposed, 1975-2002

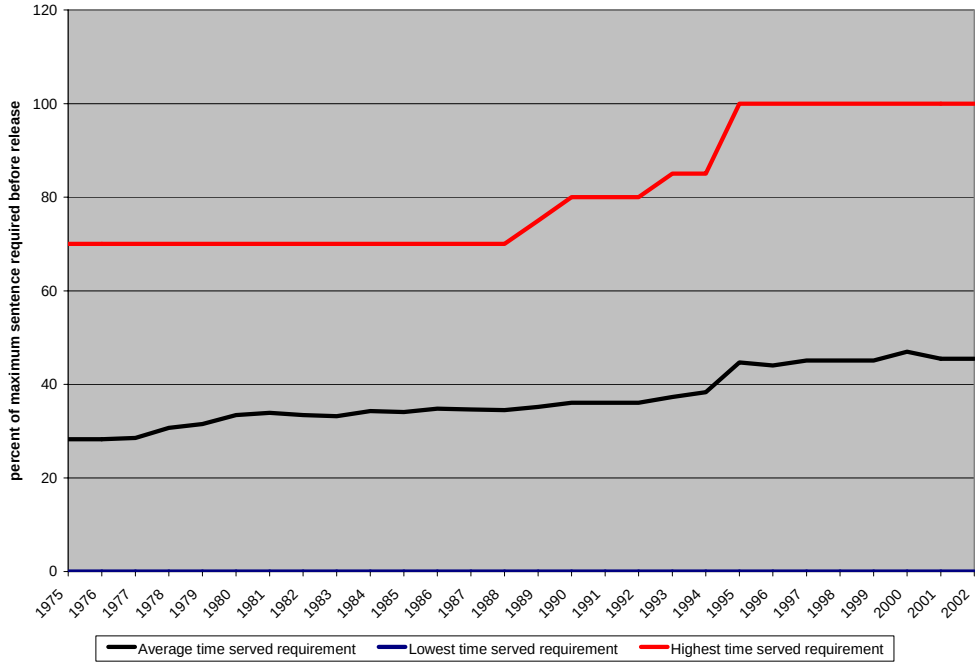
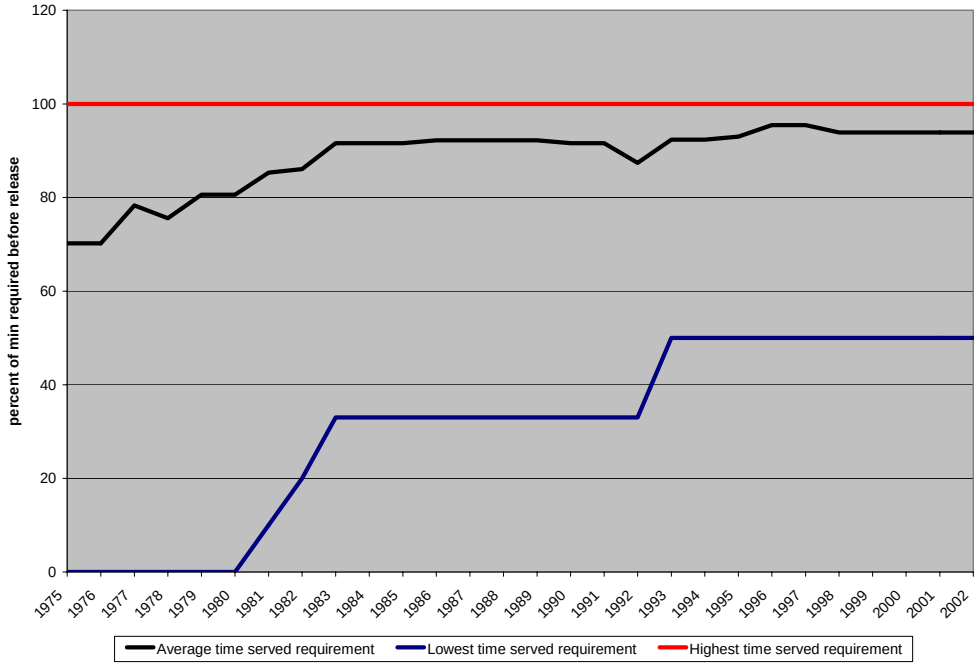


Exhibit 1-7. Time Served Requirement, Based on Minimum Term Imposed, 1975-2002



Drug Laws

Determinate sentencing, structured sentencing, and alterations in time served requirements represent significant *procedural* changes to states' sentencing and corrections policies. But states also made considerable changes in *substantive* areas as well. Drug laws and sentences for drug offenses have undergone significant alterations since the 1970s. States have introduced greater complexity into drug laws by grading drug offenses by the type of drug involved, the quantity of drugs involved, or the number of prior convictions for drug offenses. Several states, such as Kansas and Washington, have introduced greater structure into the sentencing of drug offenders by developing sentencing guidelines specifically for drug offenses; these two states now employ a separate sentencing guidelines grid for drug offenses. Other states have repealed or reduced high mandatory sentencing laws for drug offenses (e.g. Michigan and Delaware) or increased opportunities for alternatives to incarceration for low-level drug offenders or drug abusers (e.g. California, Indiana, and Texas). Yet, while several states have recently reduced sentences for some low-level drug offenses, most states significantly increased penalties for drug offenses since the 1970s, either through the creation of mandatory sentencing laws or through increases in statutory sentence ranges for offenses.

Changes to statutory sentence ranges for sale and simple possession of cocaine provide a good indication of overall changes to states' sentencing policies for drug offenses. As Exhibit 1-8 shows, the average statutory minimum sentences for first-offense simple possession and sale of 28 grams (approximately one ounce) of cocaine increased steadily between 1975 and 2002, although they plateau in the early 1990s (see also Table 1-8).[17] Statutory minimum sentences for simple possession increased from an average of 13 months in 1975 to 28 months in 2002 (a 115 percent increase); minimum sentences for sale increased from an average of 25 months to 41 months (a 64 percent increase) during the same period.[18] Statutory sentence ranges for all drugs experienced similar increases (see Chapter Six).

[17] It is important to note that the sentences described here do not necessarily reflect the actual sentences imposed by judges or the actual time served in these states. Rather, this approach simply provides a comparison of the overall drug sentencing structure and minimum possible sentences available across states. Further, these minimum sentences are not mandatory minimum sentences; rather they are the statutory minimum sentences for the offense.

[18] Since average values are vulnerable to outliers, we also examined median sentences. Using this approach we observe that the majority of states did not have statutory minimum sentences until the mid 1980s (i.e. the statutory minimum sentence available was 0 months). In terms of possession charges, the majority of states implemented a six-month statutory minimum in 1981 and then a 12-month minimum in 1987. For sale charges, the median in 2002 was about 30 months with a peak in 1990-93 of 36 months.

Exhibit 1-8. Average Statutory Minimum Sentence for Sale or Simple Possession of 28g Powder Cocaine, 1975-2002

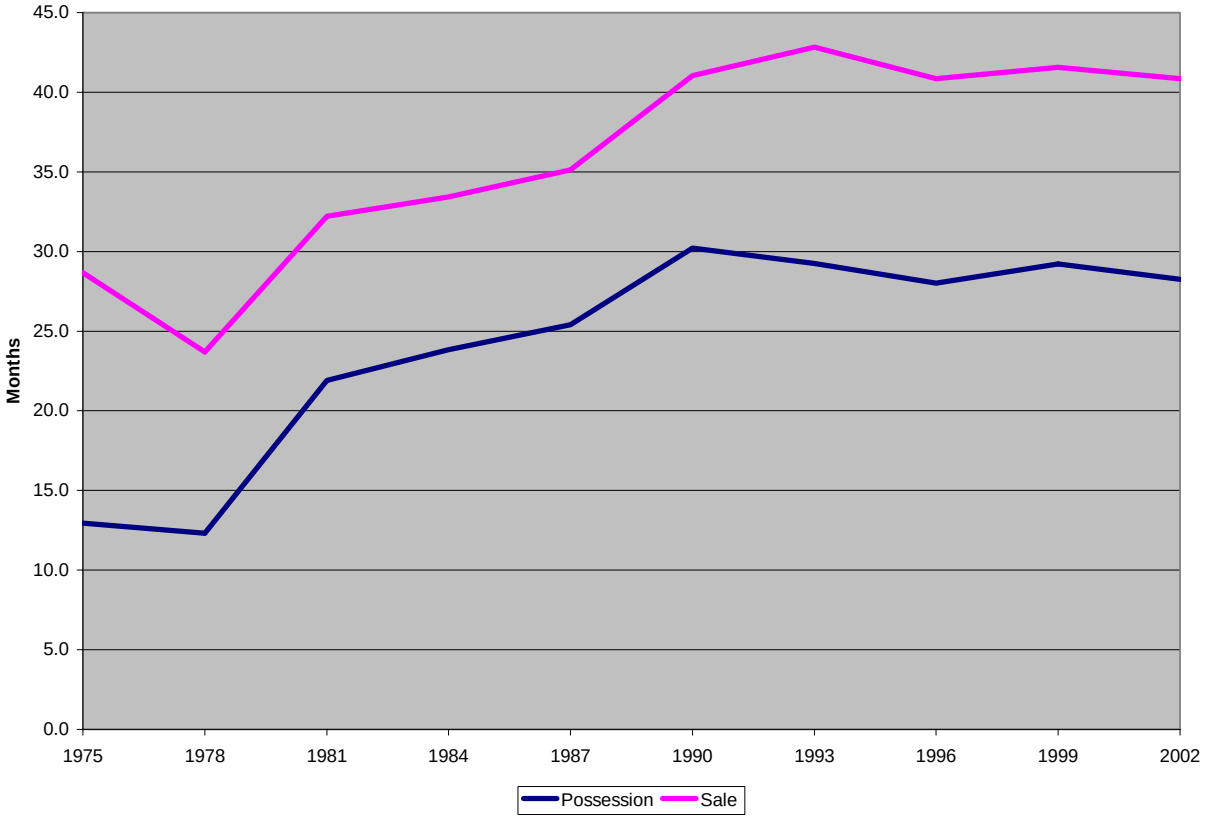


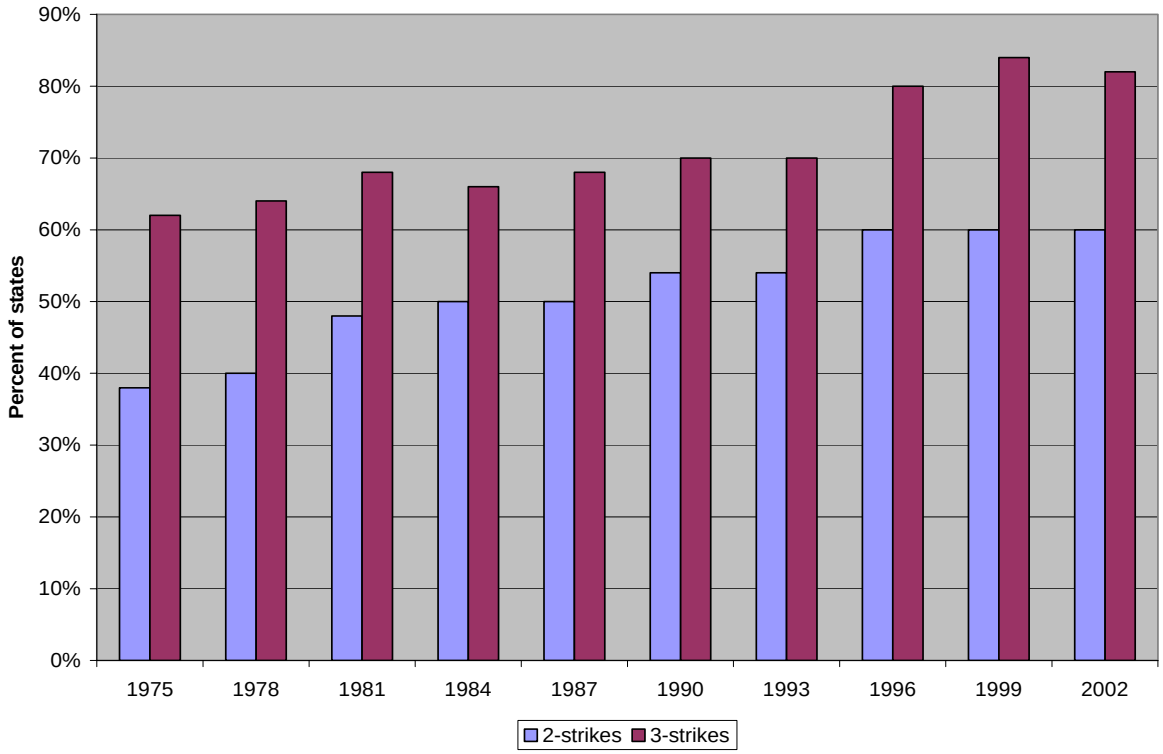
Table 1-8. Statutory Minimum Sentences for Sale of 28g of Powder Cocaine, 1975 and 2002

	Statutory Minimum Sentence (in months)	
State	**1975**	**2002**
Alabama	30	36
Alaska	24	0
Arizona	60	48
Arkansas	60	180
California	60	36
Colorado	48	48
Connecticut	60	12
Delaware	120	60
Florida	0	36
Georgia	0	120
Hawaii	60	240
Idaho	0	36
Illinois	0	6
Indiana	12	72
Iowa	60	0
Kansas	0	14
Kentucky	12	0
Louisiana	12	24
Maine	60	0
Maryland	0	0
Massachusetts	0	60
Michigan	0	12
Minnesota	0	0
Mississippi	0	0
Missouri	0	60
Montana	60	24
Nebraska	12	60
Nevada	12	12
New Hampshire	12	0
New Jersey	0	60
New Mexico	0	72
New York	180	36
North Carolina	0	35
North Dakota	0	0
Ohio	240	12
Oklahoma	60	120
Oregon	0	16
Pennsylvania	0	36
Rhode Island	0	120
South Carolina	0	84
South Dakota	0	12
Tennessee	48	96
Texas	60	60
Utah	0	12
Vermont	0	0
Virginia	60	60
Washington	0	0
West Virginia	12	12
Wisconsin	0	36
Wyoming	0	0

Habitual Offender Laws

In addition to drug offenders, "habitual offenders" were increasingly the focus of substantive policies during the last 30 years. States sought to ensure greater determinacy and structure in their systems through the creation and alteration of habitual offender laws. Unlike "repeat offender laws," which may be directed at offenders with prior convictions for the same or similar offenses, habitual offender laws are generally broad in their scope, targeted at offenders with prior convictions for any felony offense. Such laws impose longer sentences, mandatory sentences, or restrictions on release for offenders with previous convictions or terms of incarceration. While most states had habitual offender laws in place prior to the 1970s, many states increased the severity or scope of their habitual offender laws between 1970 and 2002 (see Exhibit 1-9) (see Chapter Seven).

Exhibit 1-9. Percent of States with Habitual Offender Laws



Beginning in 1994, many states adopted habitual offender laws under the label of "three strikes." Three strikes laws, patterned after those adopted in Washington and California, generally call for longer sentences than prior habitual offender laws and often apply only to serious or violent offenses. States vary in terms of the number and type of felony convictions necessary to trigger the laws and the sentences ultimately imposed under the laws (Clark, et. al., 1997). For example, California's "three strikes" law is triggered when an offender is convicted of any felony if

previously convicted of two prior felonies, one of which is a "violent" felony;[19] the law then requires the imposition of a mandatory life sentence with the possibility of parole after 25 years. In contrast, Pennsylvania's "three strikes" law is triggered only when an offender is convicted of one of eight specified felonies if previously convicted of two prior felonies, one of which is one of the same eight offenses; the law then gives the sentencing court discretion to increase the sentence for the underlying offense by up to 25 years. Between 1994 and 1996, 24 states adopted "three strikes" laws aimed at imposing substantially more severe mandatory prison sentences for repeat violent offenders.[20]

Mandatory Sentencing Laws

The policy change that has garnered the most attention since the 1970s is the adoption of mandatory sentencing laws across the states. While such laws may impact procedural aspects of a state's sentencing system (by constraining sentencing and release decisions for certain offenses), mandatory sentencing laws are substantively focused at particular offenses (e.g. drug offenses, violent offenses, or sex offenses) or specific triggering events (e.g. use of a firearm, against a minor, or in proximity to a school). Sentencing courts generally have discretion to control both the disposition and duration of the sentence imposed for a particular offense. In other words, the sentencing court has discretion to decide whether an offender will go to prison and, if so, for how long. Mandatory sentencing laws constrain both forms of discretion, requiring the court to impose a term of incarceration or requiring the court to impose a prison term of a certain length. For example, some laws increase the sentence range for an offense, but do not require the judge to alter the sentence ultimately imposed; other laws require the judge to impose a specific length of sentence or to impose incarceration.[21]

Between 1975 and 2002, every state adopted some form of mandatory sentencing law. The variation in these laws is dramatic, from the length of sentences mandated to the impact the laws have on judicial discretion and release from prison to the types of offenses and events that trigger the laws. Thus, a thorough examination of all types of mandatory sentencing laws adopted by states is not detailed here; Chapter Eight provides information on a limited set of mandatory sentencing laws across the states.

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[19] The state also has a "two strikes" provision, calling for increased penalties for those convicted of any felony with a prior violent felony.

[20] These states include: Arkansas, California, Colorado, Connecticut, Florida, Georgia, Indiana, Kansas, Louisiana, Maryland, Montana, Nevada, New Jersey, New Mexico, North Carolina, North Dakota, Pennsylvania, South Carolina, Tennessee, Utah, Vermont, Virginia, Washington, and Wisconsin.

[21] Initial analyses of mandatory sentencing laws across the states reveals three factors that affect mandatory sentencing provisions: 1) whether the law alters the duration of the sentence for the underlying offense, 2) whether the law requires the judge to alter the duration of the sentence imposed, and 3) whether the law requires the judge to impose incarceration. Based on these factors, several types of mandatory sentencing laws may be inferred: 1) *discretionary sentence enhancements*; 2) *mandatory sentence enhancements*; 3) *mandatory enhanced incarceration*; 4) *mandatory incarceration*; and 5) *enhanced mandatory incarceration* (see Chapter Eight).

The Impact of Policies on Imprisonment

After 30 years of policy adoption and change, there are now an array of approaches to sentencing and corrections in the United States. Several authors have documented the manifold changes that have occurred in the "American" approach to sentencing over the last three decades (Tonry, 1996, 1999b; Reitz, 2001; Clark, et. al., 1997; BJA 1996, 1998), but the absence of a comprehensive survey of policy change at the state level has created a shortage of comparative work on the impact of policy reforms on state prison populations. As state prison populations expanded over the last 30 years, scholars attributed much of the growth to changes in sentencing policies.[22] However, few studies have examined the impact of policies on incarceration rates (Nicholson-Crotty, 2004; Greenberg and West, 2001; Jacobs and Carmichael, 2001; Marvel and Moody, 1996; Marvel, 1995; Taggart and Winn, 1993). As a result, the overall impact of sentencing policies on prison populations remains unclear.

Given the wide variation in the size and growth of prison populations across states, understanding the impact of different policies on imprisonment is critical to understanding recent sentencing reform efforts in the states and the potential costs of future policies to the criminal justice system. This report considers the impact of six sentencing and corrections policies on state incarceration rates between 1975 and 2002: determinate sentencing, sentencing guidelines, time served requirements, sentences for drug offenses, habitual offender laws, and mandatory sentencing laws.

Overview of the Report

The remainder of this report presents our analyses of the impact of these policies on incarceration rates in the states. Chapter Two presents our initial analyses of different non-policy variables, including demographic, economic, political, and ideological factors found in previous studies to impact incarceration rates. Chapter Three presents our analyses of the impact of determinate sentencing on incarceration rates. Chapter Four considers the impact of structured sentencing – presumptive sentencing, presumptive sentencing guidelines, and voluntary sentencing guidelines. Chapter Five presents our analyses of time served requirements, including Truth-in-Sentencing laws. Chapter Six examines the relationship between sentences for drug offenses and incarceration rates, specifically looking at statutory minimum and maximum sentences available for cocaine sale and possession. Chapter Seven presents our analyses of second-time and third-time habitual offender laws. Chapter Eight analyzes the affect of mandatory sentencing laws for weapons use, offenses against protected persons, and offenses committed while in state custody. The final chapter, Chapter Nine,

[22] Langan (1991), for example, concludes that changes in sentencing practices explained 51 percent of the increase in national prison populations between 1973 and 1986, while demographic shifts accounted for 20 percent and the crime rate accounted for only 9 percent of growth during the same period. Blumstein and Beck (1999) similarly argue that 88 percent of the rise in national prison populations between 1980 and 1996 can be explained by changes in admissions to prison due to sentencing policies. Blumstein and Beck also conclude that changes in crime rates explain just 12 percent of growth during this period

presents our analyses of period-specific interactions between certain variables and incarceration rates, examining whether different factors have a stronger influence on incarceration at different points in time. Chapter Ten provides our analyses of factors affecting the growth in incarceration rates over the last 30 years. We then present our conclusions and suggestions for future research.

Chapter Two: Social Forces

What accounts for the size of state incarceration rates in the United States? While no consensus has been achieved, many demographic, economic, political, and ideological factors have been associated with differences in the size and growth of state prison populations. High crime rates, large minority and youth populations, large urban populations, high unemployment rates, high poverty rates, economic inequality, and political and religious conservatism have all been argued to be associated with higher incarceration rates. While no single factor will determine the level of imprisonment in a given state, the convergence or divergence of this multitude of factors may account for variations in prison populations both across states and over time. As policy makers seek to control prison populations through the adoption of different policies or combinations of policies, these "social" forces operating outside the influence of state actors may override any attempt to constrain prison populations through such policy mechanisms.

A large body of theoretical and empirical research has been devoted to determining those social, or non-policy, factors influencing the use of imprisonment. This chapter begins by discussing this literature and the implications for analyses of variation in state incarceration rates. It then provides our analyses of the non-policy factors impacting incarceration rates in the United States, expanding the list of explanatory variables used in prior analyses. The analyses contained in this chapter provide a baseline for analyses of sentencing and corrections policies in subsequent chapters.

Social Forces and Incarceration: A Review

A large body of literature has analyzed those factors associated with the use of punishment, particularly the factors influencing the size of state incarceration rates. These studies draw from a range of theoretical explanations of a state's use of punishment and have identified several demographic, economic, political, and ideological variables influencing state-level incarceration rates. It is clear from the findings that the forces driving incarceration rates, and punishment policies more generally, are complex. A full understanding of the variation in incarceration rates across the states requires the consideration of several theoretical explanations of a state's approach to punishment and the empirical testing of those theories.

Crime. The simplest functionalist theories of punishment argue that punishment is a direct governmental response to crime. As crime rates increase, government responds by increasing crime control mechanisms, which may include increasing prison admissions or imprisonment rates or adopting new sentencing and corrections policies. While such simplistic, direct causal explanations have been largely discounted, analysts maintain that crime does influence a state's criminal justice policy choices (Garland, 2001) and incarceration rates (Greenberg and West, 2001). Institutional responses to criminal violations, in the form of increased incarceration or policy adoption, may be partially determined by the volume of crime in a particular state; public pressure to deal with crime and political choices of policy makers are generally responsive to a

state's crime patterns (Greenberg and West, 2001). However, as Greenberg and West (2001) maintain, a state's responses to the crime rate may also be "conditioned by its ability to finance their cost, and by its political culture" (618). Indeed, while researchers have clearly established that rising crime rates in the United States during the past three decades did not lead to increases in incarceration rates (Blumstein and Beck, 1999; Chaiken, 2000; Greenberg and West, 2001) or the adoption of sentencing reforms (Link and Shover, 1986), studies have found a high degree of correlation between crime and incarceration rates across states (Carroll and Cornell, 1985; Marvell, 1995; Jacobs and Carmichael, 2001) and support for the argument that persistent high crime rates have contributed to prison population growth in the states (Greenberg and West, 2001; see also Garland, 2001). Jacobs and Helms (1999) also found a relationship between the crime rate and increases in corrections spending. Thus, while analysts discount the simplistic functionalist perspective, the relationship between crime and institutional responses to crime cannot be dismissed.

Michael Tonry (1999c) revises the functionalist theory somewhat, arguing that recent sentencing and corrections policies, and resultant increases in incarceration, are a response to moral panics resulting from high crime rates and drug use. However, Tonry's main point is that moral panics about crime and drug use tend to occur, paradoxically, at times when crime has already begun to decline. Specifically, he argues that "at times after crime rates and drug use have peaked and begun to decline, public attitudes harden, debate about crime and drug abuse narrows, and policies become harsher" (Tonry, 1999c: 1753).[23] However, downturns from high crime rates and drug use patterns alone did not create the moral panics of the last 30 years; rather, according to Tonry, "wrenching social changes" occurred in the United States (e.g. the success of the civil rights and feminist movements, mass immigration and increased diversity, and economic restructuring)[24] and the politicization of crime coincided with downturns in crime rates to lead states to implement current sentencing and corrections policies.

[23] Tonry (1999c) argues that in the last 30 years, "a series of moral panics about sexual and violent crime and about drugs have coincided with downturns in crime and drug use, which has meant that the short-term effects of moral panics have exacerbated the effects of long-term cyclical changes in attitudes associated with drugs (and probably crime). Current repressive policies are the result" (Tonry, 1999c: 1753). Tonry argues that a series of moral panics about crime problems occurred between 1985 and 1995. These included panics precipitated by: the 1986 cocaine-overdose death of basketball star Len Bias and the outbreak of the "crack cocaine epidemic;" the deaths of Megan Kanka and Polly Klaas; the generalized fear of stranger violence represented by candidate George Bush's use of Willie Horton as a campaign symbol (Tonry, 1999c: 1787). However, what Tonry does not note, is that many punitive policies were implemented well before 1985 and well before the peak in crime rates in 1995.

[24] While Tonry states that "discussing the major changes at length would turn [his work] into a historical work and require competences [he] lack[s]," (1999c: 1786), he goes on to list several social changes: "the overthrow of Jim Crow laws and the (as yet partial) realization of the civil rights movement; the Vietnam War and the long-lasting turmoil associated with it; the feminist movement, the mass entry of women into the paid labor market, and the (as yet partial) transformation in sexual roles and stereotypes; the mass immigration of the past quarter century and the increased diversity of the U.S. population; and the fundamental economic restructuring of the 1970s and 1980s" (Tonry, 1999c: 1786).

Wealth, Urbanization, and Industrialization. Legal evolution and political development theories are concerned with the evolution of legal institutions and social control mechanisms associated with the process of modernization. Such theories maintain that wealthier, more industrialized, and more urbanized states would be the first to adopt innovative policies or organizational reforms, largely because such states face particular problems and have sufficient financial and knowledge resources to adopt new policies. In the area of criminal justice reforms, scholars have argued that this leads to two conclusions. Some argue that more developed states are expected to utilize more innovative approaches to corrections, relying less heavily on traditional sanctions such as imprisonment (Carroll and Cornell, 1985; McGarrell and Duffee, 1995); others maintain that more developed states will exhibit higher degrees of social disorganization and, hence, a greater reliance on formal mechanisms of social control (Rose and Clear, 1998). Findings have confirmed both perspectives.

Unemployment. While some legal evolution/political development theories maintain that increasing wealth and modernization are associated with the adoption of less punitive criminal justice policies, the materialist perspective, in the tradition of Marxist theories, argues that changes in incarceration rates are a response to deteriorating economic conditions, worsening fiscal difficulties, and the need to control a potentially disruptive problem population. Sentencing policies are argued to be a response to these economic problems, allowing the state to reinforce the legitimacy of its social control processes. This produces three hypotheses: the higher the level of state economic crisis, the greater the use of incarceration; the higher the level of fiscal strain, the greater the use of incarceration; and the greater the proportionate size of the problem population, the greater the use of incarceration. However, the impact of increasing economic and fiscal crises may not be so direct; if incarceration is too expensive, deteriorating economic conditions may make it harder to use.

Most prior studies have relied on unemployment rates as a measure of fiscal crisis in a state. In this sense, Marxist theories, following the work of Rusche and Kirchheimer (1939), argue that punishment reflects the needs of the labor market (Greenberg, 1977), with the criminal justice system incarcerating fewer offenders when the market demand for labor is high and supply is short (Myers and Sabol, 1987; Speiglman, 1977). Scholars have found positive relationships between unemployment and incarceration rates (Greenberg 1977; Yeager, 1979; Cappell and Sykes, 1991; Jacobs and Carmichael, 2001) and between unemployment and the adoption of sentencing and corrections reforms (Link and Shover, 1986), but not between unemployment and corrections spending (Jacobs and Helms, 1999).

Joachim Savelsberg (1994) rejects the Marxist accounts of rises in incarceration rates, arguing that in the 1970s and 1980s incarceration rates increased while unemployment remained stable. However, Savelsberg qualifies this by noting that while unemployment remained stable, the size of the "imprisonable population" (i.e. extremely poor people completely detached from the labor market) increased. Greenberg and West (2001) make a similar argument in their analysis of state incarceration rates, arguing that the unemployment rate "is an imperfect measure

of the size of a 'dangerous class'" (621) that may be targeted by imprisonment policies. As the authors note, the unemployment rate includes persons who have savings and future prospects of employment or are voluntarily unemployed and excludes the most disadvantaged individuals with no or few employment prospects. While Greenberg and West find that it is not significant in predicting incarceration rates, the authors introduce percentage of families below the poverty line as a supplemental measure of the dangerous class that may be targeted by sentencing and corrections policies in times of fiscal crisis.

Inequality, Race, and Age. Other authors have argued that punishment is shaped by such "economic threats" and, like Greenberg and West (2001), have considered the size of the poor population as an indication of this threat posed to the state (Garland, 1990; Chambliss and Seidman, 1980). As Garland (1990) argues, punishment should not be seen "in the narrow terms of the 'crime problem' but instead as one of the mechanisms for managing the underclass" (134). Under this economic threat thesis, a growing economic underclass with an interest in redistributive violence and little to lose threatens economically influential groups, who respond by calling for enhanced repressive capacity. Thus, states with high levels of poverty may be expected to rely more on formal mechanisms of social control, including imprisonment, to control the underclass (Chiricos and DeLone, 1992; Liska et al., 1999). However, analysts have found no relationship between the number of families below the poverty line and incarceration rates (Greenberg and West, 2001).

Jacobs and Helms (1999), in their analysis of corrections spending, argue that this "menace of the economic underclass" should be thought of as a relational concept. In other words, increased gaps between the resources of the rich and the poor should heighten the economic threat, rather than simply the number of persons in poverty. Based on this construction, however, Jacobs and Helms (1999) find that economic inequality does not explain corrections spending. Similarly, others have found no relationship between economic inequality and incarceration rates (Greenberg and West, 2001). Jacobs and Helms (1999) also argue that there may be a racial/economic inequality interaction that may explain a state's corrections policies. The authors argue that a focus only on economic inequality ignores minorities and implies that economic differences between poor whites and the affluent and poor nonwhites and the affluent have identical effects. However, the authors find that the ratio of nonwhite to white median incomes is not a significant predictor of enhanced spending on incarceration.

Social theorists have also posited a link between welfare policies and penal policies. As Garland (1985, 2001) has shown, welfare benefits have historically been used as alternatives to penal sanctions as ways to control poor and marginalized groups. Many have drawn a distinction between inclusive regimes, which emphasize the need to improve and integrate the socially marginal and place more emphasis on the social causes of marginality, and exclusionary regimes, which emphasize the undeserving and unreformable nature of deviants and are more likely to feature less generous welfare benefits and more punitive anti-crime policies (Greenberg, 1999; Beckett and Western, 2001). According to many, social policies in the last 30 years have shifted

from inclusive policies, such as welfare, to exclusionary policies, such as imprisonment, as a means of controlling these marginalized groups (Young, 1999).

Beckett and Western (2001), for example, argue that shifts in welfare spending and incarceration rates in the United States since the 1970s reflect a change in the governance of social marginality. The argument holds that as welfare payments decrease, the use of incarceration or harsh sentencing and corrections policies will increase. In cross-national comparisons of incarceration rates, authors have found that inclusive countries (Greenberg, 1999) or countries that provide higher relative welfare payments to the poor (Sutton, 2000) also have lower incarceration rates. Similar results were found for the size of welfare payments and incarceration rates among the states (Greenberg and West, 2001; Beckett and Western, 2000); however, authors did not find that changes in welfare payments were associated with changes in incarceration rates over time (Greenberg and West, 2001). Beckett and Western (2001) conclude that the policy sectors of social policy and criminal justice policy have been sometimes loosely coupled and sometimes tightly coupled over the last 30 years (see also Greenberg and West, 2001). They conclude that following "the Reagan revolution, penal and welfare institutions have come to form a single policy regime aimed at the governance of social marginality." Thus, the influence of welfare payments on sentencing and corrections policy adoption may be time dependant. Greenberg (2001), however, questions this conclusion, arguing that welfare payments and incarceration rates remain fairly constant over time.

In addition to responding to economic threats, punishment is also argued to be a response to growing racial threats. Social structures characterized by racial or ethnic diversity are hypothesized to be more punitive toward crime (Black, 1976; Galliher and Cross, 1983; McGarrelll, 1993; Liska, 1992) and less tolerant of liberal correctional programming (Downs, 1976). Dominant groups are argued to be threatened by growth in minority populations (Blumer, 1958; Blalock, 1967) and fear of crime has been found to be associated with the percentage of African Americans in cities (Liska, Lawrence, and Sanchirico, 1982). Liska (1992), for example, argues that racial or ethnic diversity in a political system retards community-forming policies and enhances reliance on institutional innovations that remove the subjects of a policy from the community; specifically, the social controls adopted by the group in power become harsher as different cultures encroach on the traditions and power base of the dominant group.

Although this hypothesis has not been found to explain shifts in prison admissions (Jacobs and Helms, 1996), others have found that growth in the percent of the population that is nonwhite leads to greater spending on corrections (Jacobs and Helms, 1999),[25] police strength in cities (Liska, Lawrence, and Benson, 1981; Huff and Stahura, 1980; see also Jacobs and Helms,

[25] Jacobs and Helms do not compare across states, but consider the effects of independent variables on total corrections spending in the US for the years 1954 to 1990. Further, the authors assume that increased spending on corrections is associated only with increased punitiveness; they fail to consider whether increased spending is directed as treatment, release programs, or other correctional programs; further, they fail to differentiate between increased capital expenditures to upgrade facilities and increased capital or operational expenditures to seek greater control over more of the population.

1997, finding no relation between minority presence and increased police strength), higher incarceration rates (Carroll and Cornell, 1985; Marvell and Moody, 1996; Greenberg and West, 2001; Soreson and Stemen, 2002), and the imposition of more punitive sanctions (Black, 1976; Sorensen, Marquart, and Brock, 1993). In a study of the deinstitutionalization of juveniles, Downs (1976) found that race and class conflict reduced the likelihood that a state would adopt community programming for juveniles. Link and Shover (1986) also found that the size of the black population was not a significant predictor of the adoption of sentencing policies; however, when the authors considered the impact of the size of the black population in combination with unemployment rates, they found that the percent of the population that was black was a good predictor of policy adoption, but only in those states experiencing severely high unemployment.

The age structure of the population is another demographic characteristic linked to both crime and prison populations. The at-risk age category for criminal activity typically includes those in their late teens and early twenties. While crime rates peak for those in their early twenties, incarcerations typically lag a few years until a record of criminal justice contacts produces prison sentences. As such, incarceration rates peak for those in their late twenties and remain relatively stable for those in their early thirties, dropping significantly for those age 35 and over (Marvell and Moody, 1997). Fluctuations in admission rates may also be attributable to changes in the number of young males in the population (Blumstien, Cohen, and Miller, 1980).

Politics. Developments in political sociology and politics (Evans, Rueschemeyer, and Skocpol, 1984) suggest that state political arrangements are not completely determined by social and economic forces; rather, political processes act themselves as determinants of public policy. State officials act autonomously to pursue their own interests, and anti-crime agendas are often used strategically by politicians to gain wider political support (Beckett, 1997). Beckett (1997), for example, finds that national political officials are an important determinant of anxieties about crime; she shows that agenda setting by elected officials has important independent effects on criminal justice policies because political rhetoric about law and order makes street crime a more salient issue. Beckett and Sasson (2000) further argue that the politicization of street crime and law and order rhetoric was pursued by conservative politicians, beginning with Southern Republicans, in late the 1960s and early 1970s. Conservatives framed the problem of street crime in terms of immoral individuals and effectively redefined the poor as dangerous and undeserving. According to Beckett and Sasson, this politicization of street crime attracted more people to the Republican party and legitimized efforts to redirect state policy toward harsher crime control policies and away from welfare.

Several authors have found a relationship between Republican party strength and approaches to sentencing and corrections. Becket and Western (2001) find that states with more Republican-dominated legislatures have been more inclined to adopt harsh approaches to social marginality (i.e. reduced welfare and increased incarceration). Jacobs and Helms (1999) find that growth in Republican strength at the national, state, and local levels leads to increased corrections spending; they also find that expenditures accelerate through the term of Republican presidents.

Similarly, Caldiera and Cowart (1980) found that Republican presidents since 1935 increased spending on corrections and criminal justice programs, in contrast to Democrats. While some have found that national imprisonment rates grow faster during Republican presidencies (Jacobs and Helms, 1996) and that Republican strength in a state is related to incarceration rates (Jacobs and Carmichael, 2001), others have found no relation between Republican party and state incarceration rates (Greenberg and West, 2001). However, while early appeals to law and order were reserved to more conservative parties (Chambliss, 1994; Scheingold, 1991), many maintain that Democrats as well as Republicans now campaign on law and order platforms (Beckett and Sasson, 2001). Thus, over the last 30 years, there may have been a decline in the relative importance of Republican party in determining a state's approach to the use of imprisonment.

Culture. While Republican party strength in a state may influence the state's approach to crime, conservative political ideology may be more influential in determining the types of policies a state adopts and the use of incarceration. Researchers have found that people with conservative values are more likely to support punitive responses to crime (Van Dijk, and Steinmetz, 1988). Authors have asserted that the conservatism of the citizenry and government influence incarceration rates (Vaughn, 1993; Griset, 1999; Greenberg and West, 2001; Sorenson and Stemen, 2002; Jacobs and Carmichael, 2001), support for more severe sanctions (Tyler and Boeckmann, 1997), and the length of prison sentences for rape, robbery, and assault (Bowers and Waltman, 1993). Changes in ideology may also predict the timing of policy implementation and the types of policies adopted (Jacobs and Helms, 1996). For example, in his study of the deinstitutionalization of juveniles, Downs (1976) found that political culture was significantly related to policy adoption (states classified as liberal/moralist were more likely to adopt community-based policies).

Several scholars have also argued that religious fundamentalism may influence a state's sentencing and corrections policies. Religious views have historically influenced punishment philosophies (Ignatieff, 1978; McGowen, 1995); conservative Christian or fundamentalist Protestant values are also associated with support for harsher crime control responses (Grasmick, et. al, 1992; Curry, 1996). While no studies have examined the relationship between such views and the adoption of particular criminal justice policies, several studies have found a relationship between the size of the religious fundamentalist population in a state and the state's incarceration rate (Greenberg and West, 2001; Jacobs and Carmichael, 2001).

Law Enforcement. While studies often find a high degree of correlation between crime and incarceration rates across states, these studies have not accounted for the level of drug crime in a state. Uniform crime reports do not include drug crime in their measures of index crime, since drug offenses are counted only when an arrest is made. However, given the apparently large impact of the "war on drugs" on prison populations in the United States, the failure to include drug crime as a factor influencing incarceration rates seems problematic. Indeed, arrests for drug offenses in the United States nearly tripled from just 580,900 arrests in 1980 to 1,678,192 arrests in 2003 (Federal Bureau of Investigation, 2004). Between 1980 and 2001, the number of persons

held in state prisons for drug offenses increased 1,195 percent, from just 19,000 prisoners in 1980 to over 246,000 prisoners in 2002 (Harrison and Beck, 2003). Lacking a comparable measure for the occurrence of drug crimes, Greenberg and West (2001) find that the number of drug arrests is significantly related to the size of a state's incarceration rate.

Social Forces and Incarceration Rates: An Analysis Over Time

It is clear that the forces driving changes in prison populations are complex. Prior research on state differences in prison populations and incarceration rates, however, has focused on only a small number of explanatory variables. Most studies (Greenberg and West, 2001; Marvell, 1995; Marvell and Moody, 1996; Carroll and Cornell, 1985) have been further limited to a consideration of only a few years in their analyses, failing to consider incarceration rates beyond 1991. This analysis extends previous work and overcomes these limitations by broadening the theoretical scope of the inquiry to include 20 demographic, economic, political, and ideological variables and extending the time frame to cover a 33-year period from 1970 to 2002.

Data

We present here an abbreviated discussion of data and methods used in the analyses. For a detailed discussion of data used in the models see Appendix B; for a discussion of analytic methods see Appendix C. The methods discussed in Appendix C address those used in this and all subsequent chapters.

The dependant variable in the analysis is the state incarceration rate obtained from Bureau of Justice Statistics (BJS) sources. State incarceration rate refers to the number of sentenced prisoners serving one year or more under the jurisdiction of the state per 100,000 population.

A complement of controls, including demographic, economic, ideological, crime, and systemic variables, are used to predict trends in the dependant variable. To allow for the time needed to convict and sentence arrestees, all dependant variables are measured a year after other variables. Thus, analyses will be lagged by one year to assure that the policies and other factors were in full effect at the measurement of the dependent variables (Marvell and Moody, 1996).

Crime measures include violent crime rates (i.e. the number of violent crimes reported to police per 100,000 population) and property crime rates (i.e. the number of property crimes reported to police per 100,000 population) as reported in the Federal Bureau of Investigation's Uniform Crime Reports.[26] Lacking a comparable measure for the occurrence of drug crimes, we use a measure of drug arrests, calculated as the percent of total arrests that are for drug offenses.

[26] Crime rates measured by UCR reported crime statistics are often criticized, especially for underreporting. But for most crimes we are not aware of reasons why such errors would bias the results here (Gove et. al., 1985). (An exception is assault which grew unusually fast in recent years.) In any event, the fixed effects model mitigates such data problems because they control for nation-wide trends in underreporting and for consistent reporting biases in individual states.

We also use the number of full-time equivalent law enforcement officers per 100,000 population as a proxy for increased law enforcement practices.[27]

Demographic variables, derived from U.S. Census Bureau reports, include: the percent of the state population within the ages 18 through 34, the percent of the state population that is black, the percent of the state population that is Hispanic, and the percent of the state population residing in Standard Metropolitan Statistical Areas (as defined by the U.S. Office of Management and Budget). Economic measures, derived from U.S. Census Bureau reports, include: state per capita income, the state unemployment rate, the poverty rate (i.e. the percent of the population below the poverty line), a measure of economic inequality (GINI), state revenue per 100,000 population, and welfare payments per 100,000 population.

Ideological measures include the party of the governor and the states' level of both citizen and government liberalism (Berry et al., 1998). Berry et al. (1998) created and validated measures of citizen political ideology (updated through 2002), based on the interest group ratings of members of Congress for each state and the election returns for congressional races (which reflect ideological divisions in the electorate), and government political ideology, based on the party composition of state legislatures and the party affiliation of governors. Unlike measures of state level political ideology (Wright, Erikson, and Ivan, 1985) or political culture (Sharkansky, 1969) used in previous analyses of state incarceration rates (Taggart and Winn, 1993; Greenberg and West, 2001), the measures developed by Berry et al. (1988) are contemporary and account for variations in citizen and government political ideology over time.

Analyses

To assess the influence of these variables on changes in state incarceration rates, we employ a multiple time series or pooled time series cross-sectional design, which combines data from all 50 states over 33 years from 1970 to 2002. This has the benefit over time series or cross-sectional designs because it provides more degrees of freedom, permits evaluation of many separate changes in independent and dependant variables, reduces mutli-colinearity for some variables, and increases the precision of estimates by increasing the ratio of cases to variables. This research design allows us to account for trends over time within individual states and for the influence of nation-wide phenomena that may impact all states. We use data for every three years, accounting for the average length of time served by offenders,[28] which gives us 11 observations for each of the 50 states, for a total of 550 cases.

A pooled time series cross-sectional design also provides control groups – each state acts as a control for the other states – and allows control for missing variables that may cause differences

[27] This measure will include law enforcement directed at all offenses, not just drugs; however, we anticipate that the number of law enforcement officers per capita will have an increased effect in the late 1980s and 1990s, as more law enforcement efforts were directed at drug offenders.

[28] Analyses of time served data from the National Corrections Reporting Program indicate that the average time served for non-violent offenses was below the threshold of three years throughout the study period.

between states. There are two standard procedures used to analyze such pooled data: Fixed-Effects models and Random-Effects models (Hsiao, 1986; Mundlak, 1978; Pindyck and Rubinfeld, 1991). The main differences between these two approaches involve 1) the particular set of assumptions that each makes about the form of the covariance matrix produced by the analysis and 2) the treatment of omitted variables. In either approach, dummy variables are included for each state and each year. These dummy variables partly control for variables not entered in the analysis. Coefficients associated with state dummy variables estimate the influence of specific factors unique to that state and coefficients associated with year dummy variables estimate the influence of factors unique to each year but common across states.

In order to explore the appropriateness of a Random-Effects model, we first conducted a Breusch–Pagan test for overall significance of these effects. According to our results, we strongly rejected the null hypothesis that the random components are equal to zero ($chi2(1)=182.97$, $p<.001$). This test also provided support for the rejection of a pooled Ordinary Least Squares (OLS) estimation of the data over a Generalized Least Squares (GLM). Additional support for the Random-Effects model was further obtained from a Hausman test of model specification given that we failed to reject the null hypothesis of "no difference" between the coefficients of the Random- and the Fixed-Effects models ($Chi2=32.93$, $p=.063$). Given this result, we focused on the outcomes provided by the Random-Effects regression since they are more efficient and more robust. Additional specifications to this model are provided in the statistical appendix to this report (Appendix C).

The decision to focus our narrative on the Random-Effects results does not imply that the Fixed-Effects estimators are incorrect. On the contrary, regression coefficients in the Fixed-Effects model are unbiased; but given the relative size of the standard errors and the vulnerability of this estimation procedure to certain regression assumptions, there is a potential for type I error (i.e., a true null hypothesis is incorrectly rejected). The Fixed-Effects perspective is particularly relevant since the F-Test for the joint contribution of state dummies was highly significant ($F(49, 466)=6.19$, $p<.001$).

Table 2-1 presents the results of the analysis. Overall, both models explain approximately 85 percent of the within-variance of incarceration rates. Given that Random-Effects estimators weight the contribution of between and within estimators, the overall fit of the model is greater in this case (.823) than in the case of the Fixed-Effects model (.620). The year dummies are also highly significant ($p<.001$) suggesting that nation-wide trends are not accounted for by the variables in the model. Overall, differences in significance levels and the direction of the regression coefficients are very similar between the two estimation routines (this was the finding from the Hausman test).

Table 2-1. Fixed-Effects and Random Effects Models

	Fixed Effects		Random Effects	
Variable	b	SE	b	SE
Violent crime rate	0.108**	0.035	0.110***	0.031
Property crime rate	0.002	0.006	0.001	0.005
% population 18-24	1.887	4.911	2.038	4.409
% population 25-34	2.754	2.902	3.532	2.743
% population Black	11.822**	3.821	4.408***	1.009
% population Hispanic	8.101**	2.073	1.888*	0.952
% population in SMAs	-0.139	0.571	-0.241	0.324
% population religious fundamentalist	8.102**	2.970	1.607	0.907
Income per capita	-0.009***	0.002	-0.006**	0.002
Unemployment rate	1.246	2.097	2.683	1.985
Poverty rate	-4.906**	1.465	-5.067***	1.422
Gini	171.404	305.909	479.010	277.364
Revenues per 100k population (*1000)	0.063*	0.027	0.077**	0.024
Welfare per 100k population (*1000)	-0.774***	0.242	-1.110***	0.218
FTE Police per 100k population	0.166*	0.068	0.136*	0.064
Drug arrest rate	0.571***	0.161	0.577***	0.951
Governor (Republican)	13.959**	5.176	14.022**	5.134
Citizen political ideology	0.144	0.344	-0.089	0.300
1975	-3.396	16.195	-2.527	14.056
1978	23.612	18.635	20.285	15.965
1981	44.88	23.902	40.47*	19.72
1984	78.036**	23.47	72.10***	19.65
1987	114.52***	27.13	102.67***	22.89
1990	154.05***	30.61	145.14***	24.70
1993	215.75***	31.55	216.83***	24.92
1996	279.67***	34.39	283.34***	26.80
1999	339.77***	35.60	332.46***	28.33
2002	350.37***	37.85	350.60***	29.43
Constant	-163.51	131.23	-148.263	113.38
R2 Within	.852		.846	
R2 Overall	.620		.823	
N	544		544	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

Consistent with the functionalist approach, results suggest that states with higher levels of violent crime have higher incarceration rates. Similar reports have been found in the literature (Greenberg and West, 2001) although slightly different operationalizations have found no support (Jacobs and Carmichael, 2001). The coefficients for property crime rates are also positive, but non-significant. However, the results suggest a strong link between the level of

drug arrests and incarceration; states in which drug arrests comprise a larger part of total arrests have higher incarceration rates than other states. The same link was established for states with greater law enforcement capacity ($p<.05$).

Consistent with prior analyses, we also find a strong relationship between the size of a state's minority population (both black population and Hispanic population) and a state's incarceration rate. In the case of the age structure of the population, our results are in the expected direction, but they fail to be statistically significant.

We do not find a significant relationship between unemployment or economic inequality and incarceration rates. Prior analyses suggest a strong relationship between these factors (Rusche and Kircheimer; Wallace, 1981; Greenberg and West, 2001). It is possible that these relationships are not constant over time; thus, further analyses may require period interactions between these variables and certain years (see Jacobs and Carmichael, 2001; Greenberg and West, 2001). This perspective will be developed in Chapter Nine of this report. Other economic indicators, however, are significant. Wealthier states as well as states with greater welfare expenditures have lower incarceration rates ($p<.01$).

One surprising finding is the relationship between poverty rates and incarceration rates; the results suggest that states with higher poverty rates have lower incarceration rates. However, poverty rates may be measuring something other than true poverty in a given state (see Appendix C); thus, the results should be read with caution.

Both Random- and Fixed-Effects regressions provide strong support for the association between politics and incarceration rates. Consistent with prior analyses, states with Republican governors have higher incarceration rates (see also Jacobs and Carmichael, 2001). This result is somewhat unexpected given the non-significance of the citizen political ideology variable, which was found to be significant in other studies (Jacobs and Carmichael, 2001).

In terms of the percent religious fundamentalist, both models suggest that more fundamentally religious states have higher incarceration rates; however, this relationship is only significant in the Fixed-Effects model. Although our operationalization approach is different from the one employed in other similar studies (see Greenberg and West, 2001; Jacobs and Carmichael, 2001), the results presented here are consistent with previous findings.[29]

[29] See our operationalization strategy in Appendix B.

Conclusion

A society's approach to punishment is driven by a variety of objectives and determinants and, in the end, is "overdetermined" by a variety of forces (Garland, 1990). Our results confirm this, finding support for functionalist, political development, and racial and economic threat theories.

While a society's imprisonment practices may not be entirely determined by a functional response to crime rates, crime, nonetheless, matters. States with higher violent crime rates have higher incarceration rates than other states. Certainly, high levels of crime present a larger pool of "eligible" persons for incarceration; the larger number of criminals in a state will create higher incarceration rates even if crime rates do not lead states to adopt harsher penalties. However, the relationship may not be so direct. As Greenberg and West (2001) argue, it may be persistently high crime rates that shape public attitudes for increasingly harsh penalties and, in turn, lead to higher incarceration rates. This line of reasoning will also explain the fact that only violent crime rates are significantly associated with higher incarceration rates—property crime rates remain positive but non-significant. The impact of crime may also have a lag affect, impacting sentencing and corrections policies and incarceration rates only after crime rates have peaked (Tonry, 1999c). If that is the case, one would expect incarceration rates to decline after the peak in crime rates in the mid-1990s as public attitudes change.

Despite the potential effect declining crime rates may have on incarceration rates in the long run, the impact of drug arrests and other law enforcement initiatives may outstrip any such effects. Our findings show that states with more drug arrests and a larger commitment to law enforcement have higher incarceration rates than other states. While these are not direct measures of specific policies across the states, they indicate that a state's approach to substantive criminal law matters.

The problem with many functionalist theories is that they do not account for other factors in the state that similarly impact the use of imprisonment. As Greenberg and West (2001) maintain, a state's responses to the crime rate may be "conditioned by its ability to finance their cost" (618). Our analyses indicate that money, indeed, matters. Wealthier states – those with higher state revenues per capita – have higher incarceration rates than other states (see also Greenberg and West, 2001). Thus, the theory that wealthier states will invest in more innovative approaches to corrections, relying less heavily on traditional sanctions such as imprisonment, appears to be refuted by our findings; rather, wealthier states appear to rely more heavily on formal mechanisms of social control.

According to our findings, race and economics also matter. States with larger minority populations – larger black and Hispanic populations – have higher incarceration rates than other states. Similarly, states with lower welfare payments have higher incarceration rates. This is true even after controlling for other indicators of crime, wealth, poverty, and economic inequality. The research literature has provided significant empirical support for the association between economic and racial variables and state punishment policies (Wallace, 1981; Greenberg and West, 2001). This report contributes to this line of research suggesting that states may use

incarceration practices as an alternative approach to the control of marginal classes. Several theoretical works provide a substantive framework to explain the implications of this expression of government control (Young, 1999, Garland, 2001). In a recent work, Loic Wacquant (2005) has offered a historical perspective on the relationship between race, crime and the administration of justice. According to his work, the penal system has been employed as an instrument to manage disadvantaged populations—especially African Americans. Ethnographic work both inside the prison (Irwin, 1990) and outside (Clear et al., 2003) provides some support for this perspective.

Finally, our analyses indicate that politics matter. States with Republican governors have higher incarceration rates than other states. This is surprising given the fact that states with conservative citizens do not have higher incarceration rates. This lends strong support to the theory that state officials act autonomously to pursue their own interests and that the politicization of crime may be pursued largely by conservative politicians (Beckett, 1997). Coupled with the findings that higher incarceration rates are associated with lower welfare payments, this indicates that Republican parties tend to redirect state policy toward harsher crime control policies and away from welfare. While appeals to law and order may be taken up by both Democrats and Republicans, the relative importance of Republican party in determining a state's approach to the use of imprisonment may remain stronger.

As subsequent chapters show, the stability of these few social factors to predict the size of state incarceration rates is remarkable, particularly given the breadth of the time period considered and the number of control variables in our analyses.[30] As we show in the next eight chapters, even after controlling for a variety of sentencing and corrections policies and a host of non-policy variables, factors such as race, inequality, and political ideology continue to influence the size of state incarceration rates.

[30] The consistency of these findings relies on the statistical approach employed: while we have relied on partial regression coefficients to establish significant association, these coefficients are not constant over time. More importantly, several statistical corrections need to be put in place in order to fit the models properly. These issues are addressed in the Conclusion as well as in the Appendix C.

Chapter Three: Determinate Sentencing

As prison populations expanded over the last 30 years, scholars attributed much of the growth to changes in sentencing policies (Langan, 1991; Blumstein and Beck, 1999; Blumstein, 1988; Casper, 1984; Jones and Austin, 1995; Joyce, 1992; Mauer, 2001). Langan (1991), for example, concludes that changes in sentencing practices explain 51 percent of the increase in national prison populations between 1973 and 1986, while demographic shifts account for 20 percent and crime rates account for only 9 percent of growth. Blumstein and Beck (1999) similarly argue that 88 percent of the rise in national prison populations between 1980 and 1996 can be explained by changes in admissions to prison due to sentencing policies while changes in crime rates explain just 12 percent of the rise. However, these broad conclusions fail to consider the effects of specific sentencing policies on incarceration rates and are limited to aggregate data at the national level. As Chapter One shows sentencing policies and incarceration rates vary widely both across states and over time. Understanding the impact of sentencing policies on the size and variation in incarceration rates in the United States requires an examination of policy change and incarceration rates at the state level.

Few studies have systematically assessed the impact of specific state policies on incarceration rates (Taggart and Winn, 1993; Wooldredge, 1996; Marvell, 1995; Marvell and Moody, 1996; Greenberg and West, 2001; Jacobs and Carmichael, 2001; Nicholson-Crotty, 2004). Those studies that have assessed the impact of state policies have generally considered the presence or absence of only one policy – determinate sentencing – on variation in state incarceration rates (Greenberg and West, 2001; Jacobs and Carmicheal, 2001; Marvell and Moody, 1996; Taggart and Winn, 1993; Carroll and Cornell, 1985).[31] Despite the robustness of the research designs in these studies, there are certain difficulties associated with the classification scheme used to identify determinate sentencing states and the time-frame under study.

This chapter begins by clearly defining what is meant by determinate sentencing and by providing a clear classification of states as either determinate or indeterminate. It then provides a detailed examination and replication of two studies – by Greenberg and West (2001) and Jacobs and Carmichael (2001) – that represent the best analyses to date of the relationship between determinate sentencing and incarceration rates. Finally, it provides our analyses, which go beyond these prior studies by extending the time-frame of the study, expanding the list of explanatory variables used in the analyses, and providing greater theoretical grounding for the potential impact of determinate sentencing on incarceration rates.

[31] As the following chapters indicate, there are few studies assessing the impact of other policies, such as sentencing guidelines or mandatory sentencing laws, on incarceration rates across the states.

Determinate Sentencing in the States

Although the term "determinate sentencing" has been applied to several types of sentencing schemes, it essentially refers to a system *without* discretionary parole release as a mechanism for releasing offenders from prison (Reitz and Reitz, 1993; Tonry, 1987; BJA, 1996).[32] Under determinate sentencing systems, the sentencing judge imposes a prison term expressed as a number of years of imprisonment, often referred to as a "fixed" term of imprisonment. Without discretionary parole release, offenders are then automatically released from prison after serving a statutorily-determined portion of the term imposed. This term generally can be reduced only through sentence reduction credits (e.g. "good time" or "earned time"); in the absence of sentence reduction credits, offenders must serve 100 percent of the term imposed by the court.[33] The "determinacy" in the system refers to the effort to ensure that time served by offenders is primarily determined by the length of the sentence imposed by the judge rather than by the discretionary release decision-making of the parole board.

In contrast, indeterminate sentencing refers to a system *with* discretionary parole release. Under such systems, different states have required judges to impose a prison term in several different ways – by imposing the maximum prison term an offender *could* serve, the minimum prison term an offender *must* serve, or both the maximum and minimum prison terms. In all indeterminate systems the parole board determines when an offender will actually be released from prison – in other words, the actual amount of time an offender serves in prison – based loosely on the term imposed by the judge; the parole board may release an offender at any time after some set parole eligibility date up to the maximum term imposed by the judge or allowed by law (i.e. the statutory maximum sentence). The "indeterminacy" in the system refers to the relative disconnect between the length of sentence imposed and the actual length of time an offender serves in prison prior to release by the parole board.

Through the 1970s, all fifty states had indeterminate systems with discretionary parole release. California and Maine were the first states to adopt a determinate sentencing system in 1976 by abolishing discretionary parole release for all offenses, followed by Indiana, New Mexico, Illinois, and Colorado. Between 1975 and 2003, 19 states adopted determinate sentencing systems by abolishing discretionary parole release for most offenses (see Table 3-1 and Exhibit 3-1); however, two of these 19 states – Connecticut and Colorado – later reinstituted discretionary parole release for all offenses.[34] One of these 19 states – Mississippi – reinstated

[32] Determinate sentencing has been used to describe 1) systems without discretionary parole release and 2) systems with "presumptive" recommended sentences for offenses (see Shane-Dubow, Brown, and Olsen, 1985; Bureau of Justice Assistance, 1996, 1998). The former is the definition of determinate sentencing used here; the latter is the definition used to define "structured sentencing" (see Chapter Three).

[33] See Chapter Five for a discussion of the time served requirements across states.

[34] Two additional states – Idaho and New York – similarly operate largely mixed systems. In Idaho, judges have discretion to impose a determinate or indeterminate sentence. Under the "Unified Sentencing Act of 1986," Idaho judges are required to impose a minimum term of imprisonment in all cases and may impose a maximum "indeterminate" term of imprisonment at their discretion. This is known as a "unified sentence." The combination of the minimum period of confinement and the indeterminate period of custody cannot exceed the statutory

discretionary parole release in 2000 for all first-time non-violent offenders, and, thus, operates a mixed, indeterminate/determinate system (however, for the analyses that follow, Mississippi is still categorized as a determinate sentencing system after 2000). By 2002, 17 states operated under (primarily) determinate sentencing systems. Thus, while many states have abandoned the indeterminate model by abolishing discretionary parole release for all offenses, 33 states continue to maintain indeterminate systems (see Table 3-2).

Table 3-1. States with Determinate Sentencing Systems, 1975-2002

State	Dates of Operation
Arizona	1994 – 2002
California	1976 – 2002
Colorado	1979 – 1985
Connecticut	1981 – 1990
Delaware	1990 – 2002
Florida	1983 – 2002
Illinois	1978 – 2002
Indiana	1977 – 2002
Kansas	1993 – 2002
Maine	1976 – 2002
Minnesota	1980 – 2002
Mississippi[35]	1995 – 2002
New Mexico	1977 – 2002
North Carolina	1981 – 2002
Ohio	1996 – 2002
Oregon	1989 – 2002
Virginia	1995 – 2002
Washington	1984 – 2002
Wisconsin	1999 – 2002

maximum for the offense. During the minimum term of imprisonment an offender is ineligible for release on discretionary parole; the offender is then eligible for discretionary parole release at any time during the indeterminate period of the sentence. However, if the judge imposes only a minimum term of imprisonment without specifying a maximum term of imprisonment, the offender must be released from prison after serving the minimum term; in such cases, the minimum term functions essentially like a term of imprisonment imposed under a determinate system. If the judge imposes a minimum *and* a maximum term or imprisonment, the offender may be released after serving the minimum or the parole board may require the offender to serve the entire maximum imposed by the court; in such cases, the minimum term functions only as a parole eligibility date and the total sentence functions as under an indeterminate system. In 1996, New York abolished discretionary parole release for all violent offenses, but retained discretionary release for all other offenses. However, for the analyses that follow, both Idaho and New York are categorized as indeterminate sentencing systems

[35] In 2000, Mississippi reinstated discretionary parole release for first-time non-violent offenses.

Exhibit 3-1. Number of States with Determinate Sentencing for Most Offenses, 1975-2002

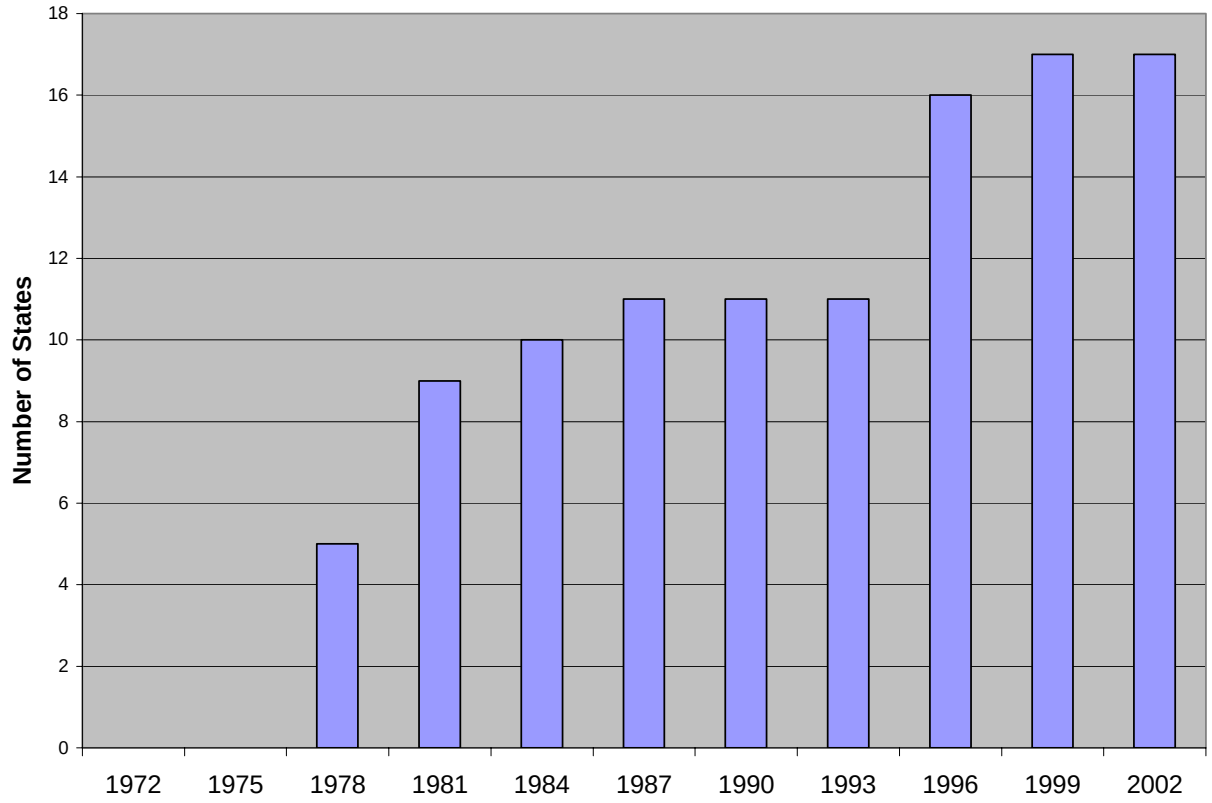


Table 3-2. States with Determinate or Indeterminate Sentencing for Most Offense, 2002

State	Determinate Sentencing	Indeterminate Sentencing
Alabama		●
Alaska		●
Arizona	●	
Arkansas		●
California	●	
Colorado		●
Connecticut		●
Delaware	●	
Florida	●	
Georgia		●
Hawaii		●
Idaho		●
Illinois	●	
Indiana	●	
Iowa		●
Kansas	●	
Kentucky		●
Louisiana		●
Maine	●	
Maryland		●
Massachusetts		●
Michigan		●
Minnesota	●	
Mississippi	●	
Missouri		●
Montana		●
Nebraska		●
Nevada		●
New Hampshire		●
New Jersey		●
New Mexico	●	
New York		●
North Carolina	●	
North Dakota		●
Ohio	●	
Oklahoma		●
Oregon	●	
Pennsylvania		●
Rhode Island		●
South Carolina		●
South Dakota		●
Tennessee		●
Texas		●
Utah		●
Vermont		●
Virginia	●	
Washington	●	
West Virginia		●
Wisconsin	●	
Wyoming		●
Total	17	33

Determinate Sentencing and Incarceration: A Review

Sentencing policies, designed to ensure the parity or certainty of court-imposed sentences, have been presumed to affect incarceration rates by altering the flow of inmates into the prison system or by changing the amount of time offenders actually serve in prison. Determinate sentencing has the potential to affect prison populations in both senses; however, the direction of that effect is debatable.

Following the initial adoption of determinate sentencing structures, pubic expectations of the long maximum sentences imposed under previous indeterminate systems could lead to initial increases in the length of prison terms imposed as judges seek to impose terms that more closely mirror those imposed under the old system. With the absence of discretionary parole release, time served under the new determinate structure could then increase as offenders are required to serve a pre-determined portion of the longer sentence; as a result, prison populations could initially increase as well.

However, the adoption of determinate sentencing is often accompanied by narrowing statutory sentence ranges for offenses or by creating some form of recommended sentence for offenses (see Chapter Four). These narrowed sentence ranges or recommended sentences are often intended to more accurately reflect the average time most offenders served in prison under the indeterminate system and are generally lower than the state's prior statutory sentence ranges and prior prison terms imposed by judges. These sentence ranges and recommended sentences may provide significant constraints on judicial discretion and ensure that terms imposed are significantly lower than under the prior indeterminate system. By eliminating the variation in the length of time some offenders serve due to discretionary parole release, states can reduce the number of offenders serving long sentences. Similarly, by narrowing the difference between the minimum and maximum statutory sentence for an offense, prison terms for repeat offenders – offenders that received both long imposed terms and long time served requirements by parole boards under indeterminate systems and who can account for large portions of prison populations – will likely be shorter under the determinate system. Thus, determinate sentencing may be expected to reduce prison populations

Early studies in individual states both supported and contradicted these predictions with results varying across and within states (*see* Tonry, 1988; Hewitt and Clear, 1983; Joyce, 1992). Hewitt and Clear (1983), for example, found that in Indiana admissions to prison remained unchanged but the lengths of prison terms imposed increased after the state's adoption of determinate sentencing. Subsequent research considering determinate sentencing across the states shows that determinate sentencing may have no effect on prison populations (Carroll and Cornell, 1985; Taggert and Winn, 1993) or a potential moderating effect, holding incarceration rates in check or reducing them somewhat (Marvell and Moody, 1996; Jacobs and Carmichael, 2001; Greenberg and West, 2001).

Two studies – by Greenberg and West (2001) and Jacobs and Carmichael (2001) – represent the most significant attempts to explain the impact of determinate sentencing on the variation of

state imprisonment levels.[36] These studies employ a large number of social and ideological controls and use more appropriate statistical modeling that, as a result, make their findings more compelling than prior cross-sectional or panel studies. Both sets of authors pooled secondary data from 1970, 1980, and 1990 in order to increase the explanatory ability of their models. Greenberg and West (2001) used OLS robust regressions whereas Jacobs and Carmichael (2001) developed a series of Fixed- and Random-Effects models taking advantage of the cross-sectional time series design of their data. In both cases, determinate sentencing was found to be negatively associated with incarceration rates. In other words, after controlling for a significant number of social, demographic, economic, political, and ideological predictors, states that abolished discretionary parole release had lower incarceration rates than states that maintained discretionary parole release.

Determinate Sentencing and Incarceration Rates: An Analysis Over Time

Replication of Prior Studies

Given the significance of the research by Greenberg and West (2001) and Jacobs and Carmichael (2001), we decided to replicate the findings of their research in order to generate a baseline model for our study. The idea was to begin our inquiry about the effects of sentencing policies by building upon previous examinations of determinate sentencing and creating the necessary framework for the analysis of more specific sentencing policies.

In order to replicate the studies by Greenberg and West (2001) and Jacobs and Carmichael (2001) we needed to modify our original data set to recreate these studies' original models. This process involved the inclusion of new variables not originally considered in our models (such as the variable "court orders" used in Greenberg and West) or the utilization of proxies for certain variables (such as "party of the governor" instead of "republican strength" as used in Jacobs and Carmichael). Despite these changes we were able to develop robust models with results that closely tracked the results published in these two studies.

In addition to this replication routine, we also included a fourth wave of data (2002) in order to assess the stability of the trends observed for the period between 1970 and 1990 by Greenberg and West and Jacobs and Carmichael. The last decade (1990-2002) was particularly relevant given a number of important systemic changes such as welfare reform, tighter state budgets, ongoing crime control issues (such as the "war-on-drugs"), and the expansion of determinate sentencing to six additional states. Overall, our results confirm the trends observed for the period 1970 to 1990. In particular, the abolition of discretionary parole release is consistently associated with lower incarceration rates for the model set up both by Greenberg and West (2001) and

[36] Other authors have considered the impact of determinate sentencing on incarceration rates; however, these other studies are limited by their research designs and, thus, not discussed in detail here. This is the case of studies such as Taggart and Winn (1993), Carroll and Cornell (1985), and Marvell and Moody (1996). In these cases, authors explored creative ways to look at sentencing policies but were seriously limited in their research designs, by the cross-sectional models, limited number of cases, or statistical modeling techniques.

Jacobs and Carmichael (2001). Patterns of association and significances of other social and systemic covariates also remain relatively stable.

Greenberg and West (2001)

We replicated "Model 1" of the pooled regression of incarceration rates as presented by Greenberg and West (2001: 632). Several adjustments to the data were made, most significantly the replacement of their variable measuring "black males as percent of the state's population" by a more general factor consisting of "percent of population that is black."[37] Table 3-3 compares selected descriptive statistics of our dataset (Replication) with those published in Greenberg and West (2001: 629, Table 1) (Original). Only variables that are strictly comparable were included in the analysis (proxies are omitted).[38]

Table 3-3. Arithmetic Means for Control Variables, Replication of Greenberg and West (2001)

	1970		1980		1990	
Variable	Original	Replication	Original	Replication	Original	Replication
Violent crime rate	261.3	259.3	454.8	454.7	542.3	534.1
Property crime rate	2088	2201	5020	5043	4672	4589
Unemployment rate	4.85	4.50	6.85	6.84	5.36	6.36
Gini coefficient	.36	.36	.36	3.6	.39	.39
% population Hispanic	3.39	3.39	4.26	4.28	5.47	5.26
% population urban	62.29	63.2	62.17	61.3	67.54	66.6
% population religious fundamentalist	10.93	11.11	11.53	11.28	11.19	11.28
Governor party	.41	.44	.43	.38	.47	.44
Determinate sentencing	**0**	**0**	**.12**	**.12**	**.20**	**.20**

The differences between the original study's descriptive statistics and those of our replication correspond to the use of different sources or variables or slightly different indicators (e.g. seasoned unemployment rate vs. unseasoned unemployment rate). As Table 3-3 indicates no states had a determinate sentencing system in 1970; by 1980, six states had determinate

[37] This should not be a problem since male and female populations tend to track each other. Data from the regression will confirm this.

[38] Some variables are expressed in different metrics such as state revenues and welfare payments. We use constant 2002 dollars while Greenberg uses actual dollars; we also relied on the Statistical Abstract for state revenue information as opposed to the State Government Finances series. It is also important to note these replication models do not include the state of Vermont due to issues of data integrity; however, all of our subsequent analyses include Vermont. For court orders we keep data constant between 1990 and 2002.

sentencing systems and, in 1990, 10 states had determinate sentencing systems.[39] Demographic and social variables also show some noticeable trends (for instance, the rise of the Hispanic population), but these changes tend to be relatively small as compared to policy or systemic changes.

Consistent with Greenberg and West's analyses, we conducted a standard OLS regression with robust standard errors as a measure to correct for heteroskedasticity in the data. The independent variable was the state incarceration rate and all independent variables were lagged one year with the exception of "state revenues" (lagged two years). In Table 3-4 we present the results of this routine. The first column reproduces the regression coefficients for "Model 1" as presented by Greenberg and West (2001: 629). In the second set of columns we attempt to replicate their findings with our own data. As noted, we used proxies for some variables or replaced original variables with similar ones. In the third set of columns, we expanded our replication to include a fourth wave of data (2002). Overall, the models explain about 85 percent of the variance on the outcome.

[39] Our research finds that between 1970 and 1990 a total of 12 states had actually adopted determinate sentencing; two states – Colorado and Connecticut – adopted and then repealed determinate sentencing between 1979 and 1990. A problem arises from the use of only census years in Greenberg and West's analyses, which fails to account for inter-censal determinate sentencing policies. Indeed, determinate sentencing in Connecticut existed from 1981 to 1990, the entire period under study by Greenberg and West, but was not categorized as having determinate sentencing in their analyses; similarly, Colorado had determinate sentencing from 1979 to 1985, but was not included in the 1980 count of determinate sentencing states in the analyses. We adjusted our dataset to replicate Greenberg and West's model, by categorizing Colorado and Connecticut as indeterminate sentencing systems during this period; however, for our final analyses at the end of this chapter, these states are specified as determinate sentencing states for relevant years.

Table 3-4. Results for Pooled Models, Replication of Greenberg and West (2001)

	Original	Replication	Extension 2002
Variable	b	b	b
Violent crime rate	.12***	.12***	.090*
Property crime rate	.006	.012	.021*
Drug arrest rate	.11**	.63**	394.62
State revenues	.05***	.058	.049
Gini coefficient	.22	-.13.5	.326
Unemployment rate	5.3*	2.58	6.81*
% population Black	42.2***	31.99***	48.13**
% population Hispanic	-1.73 #	-1.10	-.462
Region	-19.5	-12.3	-16.17
% population in SMAs	-.04	-.49	-.600
Welfare expenditures	-.63*	-.10 *	-.16***
Political ideology	2.911	.912 *	-.543
% population religious fundamentalist	1.06	1.05	1.095
Governor	-.71	5.52	8.96
Determinate Sentencing	**-28.76 #**	**-34.08****	**-47.59****
Court orders	-3.41	-2.18	2.63
1980	8.24	3.89	-19.07
1990	125.28 ###	130.30***	108.36***
2002			340.59***
R2	.866	.856	.898
N	147	150	200

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

Two-tail tests: # Significant p<.05 ## Significant p<.05 ### Significant p<.05

Greenberg and West found a negative, significant (p<.05 two-tail) association of determinate sentencing and incarceration rates. The direction and strength of this relationship was preserved after the inclusion of time-interactions. Our replication was consistent with this observation. We also found support for the majority of the findings reported in the original model, despite the fact that the two analyses are not identical. States with higher crime rates and higher drug arrest rates have higher incarceration rates. Wealthier states also have higher incarceration rates. In contrast, more liberal states have lower incarceration rates. As noted by Greenberg and West, the racial composition of the state plays a significant role in explaining differences in imprisonment rates, even after controlling for other demographics and economic variables.

These results tend to hold when we introduce a fourth wave of data into the analysis. Interestingly, unemployment rates become significant (p<.05) when the 1990-2002 period is included. Given the behavior of other variables such as percent black and welfare payments, there is enough room to suggest that during the nineties, social conditions deteriorate making

more evident the link between disadvantage and incarceration.[40] Results for out 2002 expansions were not altered once time-interactions were included in the model.

Jacobs and Carmichael (2001)

Jacobs and Carmichael (2001) use a dataset that is similar to the one constructed by Greenberg and West (2001). There are slight differences in the treatment of some variables (e.g. the Jacobs and Carmichael use the "log of percent Hispanic" while Greenberg and West use "percent Hispanic") and some more important distinctions in the operationalization of other factors (e.g. political ideology). Jacobs and Carmichael adhere to the classification of determinate and indeterminate states made by Marvell and Moody (1996). Data was pooled for 1970, 1980 and 1990. Analyses were conducted using Fixed- and Random-Effects models with and without time-invariant covariates.[41]

The models we attempted to replicate have as a dependent variable the logarithm of the state's incarceration rates (two-year averages). In terms of independent variables, we substituted the original "republican strength" variable for a dummy for "party of the governor" and replaced "tax base" with "median income." We were able to use the same source and metrics for the majority of the remaining variables in the original model. Table 3-5 presents descriptive statistics of the variables employed for our dataset (Replication) and those published in Jacobs and Carmichael (2001) (Original). Note that some of these variables were previously examined when replicating Greenberg and West's (2001) model.

[40] Paradoxically, this does not mean that the effect of disadvantage was represented exclusively by higher crime rates. Our models cannot test for this kind of relationship. However, there is some evidence pointing in this direction: more disadvantage may be associated with higher property crime rates but the change in the coefficients does not compensate enough.

[41] See Appendix C for a complete discussion on the estimation procedures employed when analyzing sentencing policies and incarceration rates.

Table 3-5. Arithmetic Means for Control Variables, Replication of Jacobs and Carmichael (2001)

	1970		1980		1990	
Variable	Original	Replication	Original	Replication	Original	Replication
Ln two-year incarceration rate	4.23	4.24	4.84	4.84	5.47	5.47
Ln Violent crime rate	5.34	5.38	5.94	5.97	6.11	6.11
Unemployment rate	4.87	4.50	6.78	6.84	6.36	6.36
Gini coefficient	.359	.359	.360	.360	.395	.395
% population Black	8.78	8.78	9.18	9.14	9.61	9.58
Ln % population Hispanic	.338	.336	.784	.779	.976	.973
% population urban	61.48	63.2	61.36	61.3	66.9	66.6
Citizen Political ideology scale	44.58	44.57	42.40	42.45	48.22	48.37
Ln Median income	10	9.61	10.21	9.87	10.36	10.05
Determinate sentencing	**0**	**0**	**.14**	**.14**	**.20**	**.20**

Examining the means of variables over time allows us to confirm that most of the social and political covariates tend to remain relatively constant. This assessment is conditioned by the fact that the table presents only means over time, without taking into account variation over space. This additional perspective will be employed toward the end of this chapter, once we introduce a completely specified model.

While Jacobs and Carmichael and Greenberg and West both rely on Marvell and Moody's (1996) classification of determinate and indeterminate sentencing states, the 1980 descriptives for determinate sentencing reported by each study differ. Greenberg and West count only six states with determinate sentencing in 1980, while Jacobs and Carmichael count seven states. While it is not clear from their analyses, Jacobs and Carmichael are likely including Colorado as a determinate sentencing state in 1980 (Colorado adopted determinate sentencing in 1979). However, Jacobs and Carmichael encounter the same problem as Greenberg and West in their use of census years in their analyses, failing to account for the inter-censal adoption and repeal of determinate sentencing provisions in Connecticut and Colorado (see footnote 15 above).

Following the procedure described by Jacobs and Carmichael (2001), we proceeded to conduct Fixed-Effects models with no time interactions. Results are presented in Table 3-6 for both the original and the replication regressions.

Table 3-6. Results for Fixed-Effects Models, Replication of Jacobs and Carmichael (2001)

	Original	Replication	Extension 2002
Variable	b	b	b
Governor (Republican)	.12	0.047	0.035
Citizen political ideology	-.005*	-0.005	-0.004*
% population Hispanic (Ln)	.07	0.076	-0.084
% population Black	.03	0.040	0.022
Violent crime rate (Ln)	.14	0.069	0.230**
Unemployment rate	.01	0.015	0.025
% population in SMAs	.003	0.003	0.004
Gini	2.91	2.592	0.447
Income per capita (Ln)	.44	0.153	0.084
1980	.49***	0.454***	0.430
1990	.94***	0.959***	1.030
2002			1.567
Determinate Sentencing	**-.21****	**-0.211***	**-.162****
R within	.81	.925	.943
R overall		.799	.844
N	150	150	200

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

In the first column of Table 3-6, we present the original results as they were published by Jacobs and Carmichael (2001). In the second column we replicate the study with our own data. Coefficients and significance levels are very similar in both cases. We believe that the few differences between the original study and our replication have to do with the fact that we use different years for some variables. The last set of columns represents our extension of Jacobs and Carmichael's model to 2002. As we observed with the replication of Greenberg and West, the models tend to be relatively stable; however, with the 2002 data pooled into the regression, we observe a significant relationship between violent crime and incarceration rates. The coefficient for determinate sentencing remains negative and significant.

The replication routine proved to be extremely helpful in creating a baseline for the study of sentencing policies and their impact on incarceration rates. As suggested in the literature, the two most extensive studies offered support for the association between the abolition of discretionary parole release and systematically lower incarceration rates. This result holds if based on longitudinal data and a large series of control variables. When expanding previous research to include the period from 1990 to 2002, our estimation proved to be consistent with the results by Greenberg and West (2001) and Jacobs and Carmichael (2001). In terms of social and economic variables, results suggest that the racial composition of the state, as well as state politics and ideology (party in power and citizen's political ideology) have significant effects on

incarceration rates, controlling for crime levels and other factors. However, there is more mixed support for the independent, significant effect of economic variables on state levels of imprisonment.

Our Findings

Despite the significance of their findings, the studies by Greenberg and West and Jacobs and Carmichael are intended to offer a structural account of change and variation in imprisonment levels and the association between an array of social and economic covariates and incarceration rates. They are not aimed at characterizing the relationship between sentencing policies and incarceration rates. This report addresses this gap in the literature and seeks to test more thoroughly the findings from previous studies. Further, while these prior studies created a very important baseline for the study of imprisonment variations over time and space, our models developed a more comprehensive specification of the models when taking into account not only state policies but also more implementation-related variables such as the size of police forces or drug arrest rates.

As described in Chapter Two, to assess the influence of demographic, social, economic, political, ideological, and policy variables on changes in state incarceration rates, we employ a multiple time series or pooled time series cross-sectional design, which combines data from all 50 states over 33 years from 1970 to 2002. Unlike pervious studies that relied on data only from Census years, we use data for every three years, which gives us 11 observations for each of the 50 states, for a total of 550 cases. Compared to previous studies of determinate sentencing, this project has more data points (550 state-year cases compared to just 150 in prior analyses) and a completely unexplored decade (1990-2002). By taking observations every three years we were able to look at short variation in determinate sentencing and other variables, impossible to capture when using only census years as in prior analyses. (As noted above, neither Greenberg and West's nor Jacobs and Carmichael's models include the determinate sentencing laws adopted by Colorado and Connecticut in the mid-1980s and repealed before 1990.) Table 3-1 lists the states adopting determinate sentencing systems and the years of operation of those systems.

Results of the Breush/Pagan and Hausman tests suggest fail to provide strong support for the statistical equivalence of the Fixed- and Random-Effects coefficients.[42] However, the Random-Effects coefficients are more efficient since they tend to produce smaller standard errors, tend to be more robust since their specification takes into account the possibility of measurement error, and are more flexible regarding the variables that can be included and the underlying

[42] As noted in Chapter Two, to assess whether the Fixed- or Random-Effects model should be used, we conducted a Breush/Pagan LM test and a Hausman specification test. Using the Breush/Pagan test, we rejected the null hypothesis suggesting that Random effects are highly significant (Chi2(1)=162.1 p<.0001), although the results may be influenced by the sample size (N*T=550). Using the Hausman test, we reject the null hypothesis of no systematic differences between the coefficients arising from both Random- and Fixed-Effects estimation techniques (chi2(23)=31.92 p= .107); therefore, the test suggests that differences in the models may be systematic.

assumptions of estimation.[43] As suggested by Jacobs and Carmichael (2001), both techniques offer somewhat different perspectives and, given the results of the Hausman test, it is relevant to present the outcome of both procedures. It is also important to note that a joint F-test for the contribution of the state dummies under Fixed-Effects was strongly significant ($F(49,465)=5.90$, $p<.001$), indicating that forces unique to states but not captured in our variables continue to exert a strong influence on variation in state incarceration rates.[44] Table 3-7 presents the results of the analysis without time-varying factors for both Fixed- and Random-Effects models.

[43] Random-effects models allow for the inclusion of variables that do not change over time or for which there are only a limited number of observations. For instance, for the "Gini" and the "percent fundamentalist" variables we only had observations for census years (1970, 1980, 1990 and 2000). Instead of being forced to introduce these variables as exclusive Random-Effects factors, we interpolated these variable's scores for between-census years. A more comprehensive discussion of the statistical modeling of the variables in this chapter is presented in Appendix C.

[44] This also implies that the use of pooled OLS estimation is not advisable.

Table 3-7. Results for Fixed- and Random-Effects with Determinate Sentencing

	Fixed Effects		Random Effects	
Variable	b	SE	b	SE
Violent crime rate	0.109**	0.035	0.112***	0.031
Property crime rate	0.003	0.006	0.002	0.005
% population 18-24	1.967	4.906	1.547	4.393
% population 25-34	2.909	2.901	3.704	2.735
% population Black	12.096**	3.822	4.489***	0.991
% population Hispanic	8.273***	2.075	2.081*	0.945
% population in SMAs	-0.121	0.571	-0.229	0.319
% population religious fundamentalist	7.458*	3.004	1.457	0.892
Income per capita	-0.009***	0.002	-0.006***	0.002
Unemployment rate	0.896	2.110	2.233	1.993
Poverty rate	-4.910***	1.464	-5.056***	1.419
Gini	162.548	305.678	467.610	276.115
Revenues per 100k population (*1000)	0.064*	0.027	0.078***	0.024
Welfare per 100k population (*1000)	-0.789***	0.242	-1.131***	0.217
FTE Police per 100k population	0.167*	0.068	0.132*	0.063
Drug arrest rate	564.299***	161.594	568.023***	151.434
Governor (Republican)	13.603**	5.177	13.706**	5.127
Citizen political ideology	0.144	0.343	-0.091	0.298
Determinate Sentencing	-12.967	9.385	-17.606*	8.573
1975	-4.603	16.203	-4.253	14.015
1978	24.235	18.623	21.351	15.909
1981	46.332	23.902	42.432*	19.665
1984	80.889***	23.543	75.991***	19.682
1987	117.413***	27.190	105.503***	22.860
1990	156.656***	30.643	147.310***	24.615
1993	219.493***	31.644	220.035***	24.848
1996	284.586***	34.543	287.569***	26.745
1999	344.858***	35.763	337.688***	28.302
2002	356.224***	38.051	357.234***	29.449
Constant	-155.422	131.242	-135.458	112.987
R2 Within	.853		.847	
R2 Overall	.628		.823	
N	544		544	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

In terms of the Fixed-Effects model, the overall fit of the model increased from 0.81 (within-R squareds) in Jacobs and Carmichael to 0.85 in our model. Given the significant increase in data points, the improvement of the overall goodness of fit seems modest. Rather, the important changes are observed for individual variables. As a whole, the model gained in explanatory power not in terms of within-state variations but in terms of between-state variations. These results are enhanced by the Random-Effects model, given its ability to maximize the between-state estimators (from .64 to .82). As suggested by the Hausman test, the set of coefficients in the Random-Effects model is not significantly different to the set obtained by the Fixed-Effects routine. In both cases, coefficients have the same direction and significance levels (with the exception of "percent religious fundamentalist").

Our models confirmed the results obtained by the replications of Greenberg and West (2001) and Jacobs and Carmichael (2001).[45] States with higher levels of violent crime and higher minority populations have higher levels of incarceration. Some systemic variables continue to be significant such as the size of the police force, the level of welfare payments in the state, and state revenues. In terms of determinate sentencing, both Fixed- and Random-Effects models suggest that states that abolished parole (determinate sentencing) exhibited consistently lower incarceration rates, however, the relationship was significant only in the Random-Effects model.

However, the observed significance of determinate sentencing is conditioned on the group of states categorized as determinate sentencing. Because several states appear to be mixed determinate/indeterminate systems (see above), we reran the analyses with different coding for the presence of determinate sentencing. As noted above, New York adopted determinate sentencing for violent offenses in 1996; coding New York as having determinate sentencing for 1996 to 2002 makes the determinate sentencing variable in the regression less "significant" (and not significant at all for the Random-Effects models); however, overall, coefficients are in the same direction and the model has the same goodness of fit. The same outcome is observed when changing Idaho to determinate sentencing between 1986 and 2002, the period in which judges were given the discretion to determine parole eligibility. In contrast, coding Mississippi as indeterminate for the period from 2000 to 2002 (recall, the state reinstated discretionary parole for first-time, non-violent offenders in 2000) makes the determinate sentencing variable more significant in the Fixed- and Random-Effects models. This underscores the importance of clear specifications of policy variables in such analyses.

Conclusion

While all states experienced increases in prison populations over the last 30 years, some states have managed to slow or even reverse the rate of growth in their prisons, often deliberately through the adoption of specific policies. The adoption of these policies may be influenced by

[45] We replicated our model following Greenberg and West (2001) estimation routine and results were similar to the ones reported in Table 3-8. In particular, the effect of determinate sentencing was significant ($p<.001$) and negatively associated with the outcome ($b=-31.03$).

the social forces operating in a given state, but our research shows that they also operate alongside those social forces to exert an independent influence on incarceration rates. As we show, the broad conclusions reached about the effects of sentencing policy changes on incarceration rates (Langan, 1991; Blumstein and Beck, 1999) do not hold for determinate sentencing – states with determinate sentencing have lower incarceration rates than other states.

This finding is not entirely predicted. Determinate sentencing can be combined with a cost-control agenda and potentially reduce incarceration rates or with a crime-control agenda and potentially increase incarceration rates. Thus, the potential impact of determinate sentencing depends on the reasons behind the policy's adoption. Even if states did not adopt determinate sentencing with the intent of controlling costs, determinate sentencing may, nonetheless, constrain those forces that would have otherwise increased incarceration rates in the absence of such a law. Such laws have likely reduced the lengths of terms imposed and actual time served. Only three of the 19 states – Connecticut, Illinois, and Mississippi – failed to narrow sentence ranges at the same time they adopted determinate sentencing; only four of the 19 states – Connecticut, Illinois, Maine, and Mississippi – failed to adopt some form of recommended sentences when they adopted determinate sentencing. Thus, most states adopting determinate sentencing constrained judicial discretion in sentencing by narrowing ranges and recommending sentences; combined with the uniformity in time served and release decisions, such laws may have placed limits on incarceration rates even if not intended by those adopting the laws. Chapter Four more thoroughly explores the impact of controlling both sentencing and release decisions on incarceration rates.

While policies matter, these findings also show the stability of social factors to predict the size of state incarceration rates. Race, welfare, wealth, politics, and the enforcement of drug crimes exert a strong influence on the size of a state's incarceration rate. After including determinate sentencing, we increased the explanatory power of our model very little; nonetheless, the strength of the association between determinate sentencing and incarceration rates remains important. Thus, while many forces driving increases in incarceration rates over the last 30 years were outside the control of policymakers, the policy choices made by states during that period mattered and continue to influence the size of state incarceration rates.

The next chapter begins to create greater distinctions between determinate and indeterminate sentencing systems, by exploring the impact of "structured sentencing" on incarceration rates. As noted above, it may be the particular structure of a determinate sentencing system that influences incarceration rates, rather than simply the abolition of discretionary parole release.

Chapter Four: Structured Sentencing

While many states sought to increase the "determinacy" of their systems through the abolition of discretionary parole release, others sought more "structure" in their systems through the adoption of "structured sentencing" – or the creation of recommended prison terms for offenses. States accomplished this structure through the creation of "presumptive sentencing" – systems of single recommended sentences for each offense or offense class – and "sentencing guidelines" – systems of multiple sentence recommendations for each offense or offense class.[46] Structured sentencing, unlike mandatory sentencing laws, does not eliminate judicial discretion; rather, it identifies the "typical" case and provides a recommended sentence for such a typical case; judges are then authorized to impose a sentence longer or shorter than that recommended after taking into account additional sentencing factors. While determinate sentencing is about controlling release decisions and time served, structured sentencing is about controlling sentencing decisions and the length of prison terms imposed. Thus, in addition to distinguishing determinate and indeterminate systems, it is equally important to distinguish "structured" and "unstructured" systems.

As noted in Chapter Three, most prior studies assessing the impact of policies on variation in state incarceration rates have only considered the presence or absence of determinate sentencing (Greenberg and West, 2001; Jacobs and Carmicheal, 2001; Marvell and Moody, 1996; Carroll and Cornell, 1985). To date, only a handful of studies have considered the impact of structured sentencing on incarceration rates across states (Marvell, 1995; Sorenson and Stemen, 2002; Nicholson-Crotty, 2004). None of these have attempted to evaluate the potential impact of presumptive sentencing systems; rather, these studies have focused exclusively on sentencing guidelines, and primarily on presumptive sentencing guidelines. And, like analyses of determinate sentencing, prior research on structured sentencing has failed to consider the possible impacts of other polices on incarceration rates. Of the 16 states that adopted some form of sentencing guidelines over the last 30 years, nine adopted determinate sentencing at the same time; of the 24 states that adopted structured sentencing (presumptive sentencing or sentencing guidelines), 16 also adopted determinate sentencing. Prior analyses have failed to account for the possibility that the findings regarding the impact of sentencing guidelines may be picking up the actual effects of determinate sentencing, or other state-level policies.

Like analyses of determinate sentencing, a further problem arises in the definition and specification of structured sentencing systems in prior studies, with authors often providing little

[46] While policies such as mandatory sentencing laws create similar "recommended sentences," the focus here is on policies that provide recommended sentences for all offenses. Mandatory sentencing laws are aimed at a small subset of offenses; in contrast, policies such as presumptive sentencing and sentencing guidelines apply to all (or nearly all) felony offenses sentenced in a state with structured sentencing. While several authors have used the terms "structured" or "determinate" to refer to all policies that provide recommended, mandatory, or presumptive sentences for limited numbers of offenses, we use the term "structured" here to refer only to those systems that seek to provide such sentences for all offenses.

explanation for their categorization of states as subscribing to a particular type of system. State guidelines systems have been categorized as "presumptive/mandatory" sentencing guidelines or "voluntary/advisory" sentencing guidelines; however, analyses of the impact of such systems on incarceration rates have been inconsistent in defining which states have adopted either of these two types of systems. As a result, prior findings concerning the impact of presumptive and voluntary sentencing guidelines on incarceration rates are often confusing and may be misleading.

This chapter begins by clearly defining what is meant by structured sentencing and providing a clear classification of states according to the type of structured sentencing policy in use. It then provides a detailed examination and replication of two studies – by Marvell (1995) and Nicholson-Crotty (2004) – that represent the best analyses to date of the relationship between sentencing guidelines and incarceration rates. Finally, it presents our analyses, which go beyond these prior studies by looking at both presumptive sentencing and sentencing guidelines, expanding the list of explanatory variables used in prior analyses, and considering the interactive impact of determinate sentencing and structured sentencing on state-level incarceration rates.

Structured Sentencing in the States

All state criminal codes establish the minimum and maximum prison terms available for an offense. Traditionally, states provided only statutory sentence ranges for offenses, which were generally fairly wide (e.g. 2-20 years) and represented the only legislative statement of appropriate prison sentences for criminal conduct. Judges typically had discretion to impose a prison term anywhere within the statutory range, but statutes provided no guidance on the particular term of incarceration to impose from within these ranges. Several states articulated general purposes of sentencing or mitigated and aggravated factors that judges could consider in setting a term of incarceration; however, judges were not instructed on how to evaluate or weigh these factors, nor did the factors guide judges in determining the length of the prison term to impose within the statutory sentence range.

Structured sentencing represents a marked departure from this approach. States with structured sentencing seek to narrow or guide judicial discretion in determining the length of an imposed prison term by proscribing a recommended term within the wider statutory sentence range. Judges are expected to impose the recommended term; however, states generally allow a judge to impose a term of incarceration above or below this recommended term (up to the statutory maximum or down to the statutory minimum) based on aggravating or mitigating circumstances. Thus, it is possible to distinguish "unstructured sentencing" systems (those that provide no system of specific recommended prison terms for offenses) and "structured sentencing" systems (those that provide a system of specific recommended prison terms for offenses). The "structure" in the system refers to the effort to ensure that prison terms imposed for similar offenses or similar offenders are uniform and that the criteria for imposing sentences are consistent for all offenses and offenders.

While states with structured sentencing share the common characteristic of proscribing recommended prison terms for offenses, states accomplished this structure in two ways: through the creation of "presumptive sentencing" systems – systems of single recommended sentences for each offense or offense class – or "sentencing guidelines" systems – systems of multiple sentence recommendations for each offense or offense class.

Presumptive Sentencing

Presumptive sentencing refers to a sentencing system that provides a single, recommended prison term or a narrow recommended sentence range for each felony class or offense within a wider statutory sentence range; the recommended sentence is based solely on the severity of the offense committed. The system is "presumptive" because it is presumed that the judge will impose the recommended prison term or a term from within the recommended range; generally, a judge may impose a prison term that is longer or shorter than the recommended term or outside the recommended sentence range *only* by a finding of aggravating or mitigating circumstances or by stating reasons for deviating from the recommended term.[47]

Between 1975 and 2002, nine states adopted some form of presumptive sentencing system (see Table 4-1). Six of these nine states combined presumptive sentencing with the adoption of determinate sentencing; three of these nine states combined presumptive sentencing with indeterminate sentencing (see Table 4-3).[48] However, even within the category of presumptive sentencing systems, state systems are constructed quite differently. For example, most states provide a single, recommended sentence within the wider statutory sentence range for each offense class. However, Colorado provides a narrow presumptive sentence range within the wider statutory sentencing range; California provides a single presumptive term and two alternate terms (a "lower" term and an "upper" term) that a judge may impose.[49]

[47] An example showing available sentences for robbery in one presumptive sentencing state may be illustrative. New Jersey created recommended prison terms for offenses in 1977 and is, thus, categorized as a structured sentencing state. In New Jersey, robbery is a crime of the second degree, which has a statutory sentence range of 5 to 10 years. According to New Jersey's criminal code "unless the preponderance of aggravating or mitigating factors…weighs in favor of a higher or lower term within the [statutory sentence range], when a court determines that a sentence of imprisonment is warranted, it shall impose…a term of seven years for a crime of the second degree" (NJSA 2C: 44-1(f)) Thus, in New Jersey, a judge should impose a prison term of seven years (the presumptive term) for robbery, and may impose a term as short as five years or as long as 10 years *only* by finding additional factors.

[48] Alaska, Arizona, California, Indiana, New Mexico, and Ohio have presumptive sentencing and determinate sentencing. Colorado, New Jersey, and Rhode Island have presumptive sentencing and indeterminate sentencing. Colorado originally combined presumptive sentencing with determinate sentencing; however, in 1985, Colorado reinstated discretionary parole release, but retained the presumptive sentences for offenses.

[49] California dispensed entirely with the sentence ranges found under the indeterminate model and replaced them with a set of three possible fixed sentences for each offense. Under the California system, the statute governing each offense prescribes a high, middle, and low term of imprisonment from which the judge selects a fixed sentence; the court must impose the middle term or provide a written statement justifying imposition of the upper or lower term.

Table 4-1 States with Presumptive Sentencing, 1975-2002

State	Dates of Operation	Presumptive Term
Alaska[50]	1980 – 2002	Single term for each offense class (statutorily created for some first offenses and all second and third offenses)
	1981 – 2002	Single term for each offense class (judicially created "benchmarks" for all first-offenses)
Arizona	1978 – 2002	Single term for each offense class
California	1976 – 2002	Single term for each offence
Colorado	1979 – 2002	Presumptive range for each offense class
Indiana	1977 – 2002	Single term for each offense class
New Jersey	1977 – 2002	Single term for each offense class
New Mexico	1977 – 2002	Single term for each offense class
Ohio	1996 – 2002	Single term for each offense class
Rhode Island[51]	1981 – 2002	Presumptive range for each offense class (judicially created "benchmarks" for all offenses)
	1992 – 2002	Single term for each offense class (statutorily created for offenses comprising more than 5 percent of criminal caseloads)

Sentencing Guidelines

Sentencing guidelines refer to a system of multiple recommended prison terms for each offense based on multiple types of prior criminal histories and a set of procedures designed to guide judicial sentencing decisions and sentencing outcomes. Sentencing guidelines differ from presumptive sentencing systems in one primary respect: sentences under presumptive sentencing systems are determined by the severity of the offense alone; in contrast, sentences under sentencing guidelines are generally determined by multiple factors, but primarily by the severity of the offense *and* the prior criminal history of the offender. The goal is to ensure that all offenders committing similar offenses *and* with similar criminal histories receive nearly identical sentences under the sentencing guidelines.

[50] In 1980, the Alaska legislature created presumptive sentences for the first-time commission of some felonies and the second- and third-time commission of all felonies. In 1981, the Alaska Court of Appeals developed a series of "benchmarks," or presumptive sentences, for the first-time commission of offenses without statutory presumptive sentences.

[51] In 1981, the Rhode Island Superior Court created a set of "sentencing benchmarks" that judges were advised to follow at sentencing (see R.I. Rules of Court, Superior Court Sentencing Benchmarks). According to the policy statement accompanying the benchmarks, "In order to eliminate, insofar as possible, disparity in the sentencing of defendants for crimes committed under the same or similar circumstances, the court may consider and utilize the sentencing benchmarks formulated by the Supreme Court Committee on Sentencing as guidelines."

Sentencing guidelines identify the "typical" case, given the severity of the offense and the criminal history of the offender, and provide a recommended sentence for such a typical case. All guidelines systems developed by the states then allow a judge to "depart" from the recommended sentence, or impose a sentence longer or shorter than that recommended. The restrictions placed on a judge's ability to depart, the limits placed on the length of sentence which may be imposed upon departure, and the procedures available for enforcing the guidelines' recommended sentence all distinguish one guidelines system from another.

At the most basic level, sentencing guidelines systems are divided into presumptive sentencing guidelines systems and voluntary sentencing guidelines systems. The degree to which states use formal legal authority to constrain judicial sentencing decisions distinguishes the two systems. Presumptive sentencing guidelines require judges to impose the sentence recommended by the guidelines or provide written justification for imposing some other sentence; sentences that do not adhere to the sentence recommendations of the guidelines may be appealed by either the defendant or the prosecution. Thus, presumptive sentencing guidelines states employ appellate review of sentences to ensure that sentences adhere to the sentencing guidelines.[52] In contrast, voluntary sentencing guidelines do not require judges to impose the sentence recommended by the guidelines; while judges under voluntary sentencing guidelines systems may be required to provide reasons for not imposing the sentence recommended by the guidelines, sentences that do not adhere to the sentence recommendations of the guidelines may not be appealed by either the defendant or the prosecution. Thus, voluntary sentencing guidelines states lack any appellate review of sentences or other formal legal authority to ensure that sentences adhere to the sentencing guidelines.

Minnesota was the first state to adopt presumptive sentencing guidelines in 1980, followed closely by Pennsylvania and Washington. Between 1980 and 2002, 17 states adopted some form of sentencing guidelines (see Table 1-6).[53] To date, there have been nine presumptive guidelines systems and ten voluntary guidelines systems adopted by states; two states – Florida and Michigan – originally adopted voluntary guidelines systems which were later repealed and replaced with presumptive guidelines. Further, Wisconsin originally adopted voluntary guidelines in 1985, which were repealed in 1994; in 1999, the state adopted a new version of voluntary guidelines.

[52] As Frase (1995) notes, such systems are presumptive, not mandatory. "The prescribed sentence is presumed to be correct," but the court may impose a sentence different from this recommendation if it finds that specific reasons exist to impose such a different sentence. In some states, these reasons must be "substantial and compelling" while in other states judges must simply give reasons for the sentence imposed.

[53] Oklahoma also adopted voluntary sentencing guidelines in 1997. However, the state repealed the guidelines in 1999 before they became effective.

Table 4-2 States with Sentencing Guidelines, 1975-2002

	Dates of Operation	
State	**Presumptive Guidelines**	**Voluntary Guidelines**
Arkansas		1994 – 2002
Delaware		1987 – 2002
Florida[54]	1994 – 1998	1983 – 1994
Kansas	1993 – 2002	
Louisiana		1987 – 2002
Maryland		1983 – 2002
Michigan[55]	1999 – 2002	1985 - 1999
Minnesota	1980 – 2002	
Missouri		1997 – 2002
North Carolina	1995 – 2002	
Oregon	1989 – 2002	
Pennsylvania	1982 – 2002	
Tennessee	1989 – 2002	
Utah		1985 – 2002
Virginia		1995 – 2002
Washington	1984 – 2002	
Wisconsin		1985 – 1994 1999 – 2002

Thus, between 1975 and 2002, 26 states adopted some form of structured sentencing system – nine presumptive sentencing systems and 17 sentencing guidelines systems. Each of these types of systems has been implemented with and without parole, creating several different combinations of determinate/indeterminate and structured/unstructured sentencing systems (see Table 4-3).

[54] In 1994, Florida converted its voluntary sentencing guidelines to presumptive sentencing guidelines. In 1998, the state repealed the presumptive sentencing guidelines of 1994 and replaced them with the "Criminal Punishment Code" which went into effect October 1, 1998 (adopted in 1997). The Criminal Punishment Code essentially exists only to determine the "lowest permissible sentence" that the trial court must impose without a departure. Once the court determines the lowest permissible sentence, the court may impose any sentence from this lowest permissible sentence up to the statutory maximum sentence for the offense. Thus, the Code is not really a set of sentencing guidelines used to determine a specific sentence, but functions to simply determine the minimum sentence that a judge must impose without a departure.

[55] In 1999, Michigan converted its voluntary sentencing guidelines to presumptive sentencing guidelines.

Table 4-3 Determinate and Structured Sentencing, 2002

State	Determinacy		Structure		
	Determinate	Indeterminate	Presumptive Sentencing	Presumptive Guidelines	Voluntary guidelines
Alabama		●			
Alaska		●	●		
Arizona	●		●		
Arkansas		●			●
California	●		●		
Colorado		●	●		
Connecticut		●			
Delaware	●				●
Florida	●			●	
Georgia		●			
Hawaii		●			
Idaho		●			
Illinois	●				
Indiana	●		●		
Iowa		●			
Kansas	●			●	
Kentucky		●			
Louisiana		●			●
Maine	●				
Maryland		●			●
Massachusetts		●			
Michigan		●		●	
Minnesota	●			●	
Mississippi	●				
Missouri		●			●
Montana		●			
Nebraska		●			
Nevada		●			
New Hampshire		●			
New Jersey		●	●		
New Mexico	●		●		
New York		●			
North Carolina	●			●	
North Dakota		●			
Ohio	●		●		
Oklahoma		●			
Oregon	●			●	
Pennsylvania		●		●	
Rhode Island		●	●		
South Carolina		●			
South Dakota		●			
Tennessee		●		●	
Texas		●			
Utah		●			●
Vermont		●			
Virginia	●				●
Washington	●			●	
West Virginia		●			
Wisconsin	●				●
Wyoming		●			

Structured Sentencing and Incarceration: A Review

Most structured sentencing systems were adopted to reduce sentencing disparity and bring greater uniformity to sentences imposed. By providing a recommended sentence for each offender or offense class, states with structured sentencing try to ensure that prison terms imposed are similar for similarly situated offenders. The sentence recommendations control the lengths of prison terms imposed and may be expected to lead to lower incarceration rates in those states adopting such reforms.

While no analyses have been devoted to states with presumptive sentencing systems, sentencing guidelines have received a fair amount of scholarly attention; however, most of this is directed at presumptive sentencing guidelines, with voluntary sentencing guidelines receiving very little attention. Generally regarded as having a minimal or no effect on judicial sentencing practices, voluntary guidelines have been largely dismissed by scholars and have not been subjected to the same analyses as those devoted to presumptive sentencing guidelines. In the end, most conclude that voluntary guidelines have no impact on admissions to prison or incarceration rates (Marvel, 1995).

In contrast, presumptive sentencing guidelines have been held out as a "balanced approach to critical issues of sentencing policy" and were initially heralded as a way to control rising prison populations (Frase 1995: 174). While some analysts tentatively considered such laws successful in their ability to hold prison populations in check (Alschuler, 1991; Tonry, 1991), others criticized individual state guidelines commissions for failing to keep populations below capacity (Savelsberg, 1992; Holten and Handberg, 1990).[56] Subsequent research has argued that presumptive sentencing guidelines have no effect on prison populations (Wooldredge, 1996) or can act as a mediating factor, slowing prison population growth and reducing prison populations, but only when such guidelines are sensitive to prison capacity (Marvell, 1995).

Yet, while many scholars have heralded the benefits of sentencing guidelines for controlling prison populations, little comparative work has been conducted examining the effects of sentencing guidelines across states. The work by Thomas Marvell (1995) and Sean Nicholson-Crotty (2004) represent the most significant attempts to explain the impact of sentencing guidelines on incarceration rates across states. Both authors used pooled time-series designs to

[56] According to Frase (1995), the impact of sentencing guidelines on prison populations is mixed. Delaware and Pennsylvania experienced increases in prison populations after implementation of guidelines. However, Minnesota, Oregon, and Washington were successful in limiting prison populations and avoiding overcrowding immediately after implementation of guidelines; however, Minnesota and Washington later experienced significant growth in prison populations. According to Frase, this latter growth was the result of sudden increases in felony caseloads, changes in prosecutorial charging patterns, and system responses to specific highly publicized violent crimes. Frase further argues that sentencing guidelines systems tend to experience "a statistically artificial 'grace period' of lowered inmate populations. This occurs because increased sentence durations, and charging changes which increase future criminal-history scores, take effect gradually, whereas presumptive probation terms for non-violent offenders have a large and immediate impact…In addition, prosecutors and judges can give immediate effect (through charge reductions and mitigated departures) to any disagreements that have with the increased severity proposed to be given to certain offenders" (Frase, 1995: 177).

examine incarceration rates across all 50 states over significant time periods; Marvell (1995) examined change in state-level incarceration rates between 1974 and 1990 and Nicholson-Crotty (2004) considered change in incarceration rates between 1975 and 1998. Further, both studies are primarily concerned with presumptive sentencing guidelines constructed specifically to control prison populations or corrections resources and the impact of such guidelines on incarceration rates. Marvell examined only presumptive sentencing guidelines systems and found that states with presumptive sentencing guidelines had lower incarceration rates than states without sentencing guidelines (1995:703-704).[57] However, this was particularly true for sentencing guidelines constructed specifically to control prison populations or resources. Nicholson-Crotty (2004) similarly found that presumptive sentencing guidelines constructed to control prison populations or resources were negatively associated with prison commitment rates; in other words, states with such sentencing guidelines had lower prison commitment rates than states without such guidelines.[58] However, Nicholson-Crotty was not able to confirm Marvell's finding that states with resource-linked presumptive sentencing guidelines had lower incarceration rates than other states. He did find that presumptive sentencing guidelines not linked to prison populations or resources were significantly associated with higher incarceration rates; a non-significant but positive relationship was also found for voluntary guidelines systems. Yet, while it is clear that both studies are important contributions to the field of penal policies, there are several caveats that undermine the validity of their empirical claims.

Structured Sentencing and Incarceration Rates: An Analysis Over Time

Replication of prior studies

Given the oft-cited findings of Marvell (1995) and the significance of the research by Nicholson-Crotty (2004) we decided to replicate the findings of their research. The idea was to begin our inquiry about the effect of structured sentencing by building upon previous examinations of presumptive sentencing guidelines. Overall, our results confirm the trends observed in the two prior studies. However, several methodological problems, discussed below, arose in trying to replicate the findings.

Marvell (1995)

Marvel examines the effect of sentencing guidelines on prison populations and admissions to prison between 1974 and 1990 using a group of nine states – Delaware, Florida, Michigan, Minnesota, Oregon, Pennsylvania, Tennessee, Washington, Wisconsin – defined by Marvell as "presumptive" guidelines states. Marvel examines only "presumptive" sentencing guidelines

[57] While Marvell distinguishes presumptive and voluntary sentencing guidelines systems, his categorization of states is unclear. Issues of classification of states in Marvell's work are described below.

[58] Nicholson-Crotty uses the term "mandatory" to refer to presumptive sentencing guideline. And, like Marvell, Nicholson-Crotty distinguishes these from voluntary sentencing guidelines systems, but provides unclear guidance for how states were categorized.

because he believes that "voluntary guidelines, which do not require judges to state reasons for departing from the guidelines,…are generally local and largely ignored by judges" (702). However, Marvel provides no clear definition of presumptive sentencing guidelines. The criteria that Marvel uses to designate states as having presumptive sentencing guidelines – that judges provide reasons for departing from the guidelines – would designate many self-proclaimed "voluntary" guidelines systems as presumptive as well. Delaware and Wisconsin, two states Marvel uses in his analysis as presumptive guidelines states, are, in fact, voluntary sentencing guidelines systems; while judges in both systems are required to provide reasons for not following the sentencing guidelines recommendations, the guidelines are described as "advisory" and there is no way to require a judge to sentence within the guidelines (i.e. there is no right to appeal a sentence that deviates from the guidelines).[59] Marvell further distinguishes between those guidelines systems that explicitly require the consideration of prison capacity in the drafting of guidelines and those that do not. Marvell lists six states –Florida, Delaware, Minnesota, Oregon, Tennessee, and Washington – as being constructed with some form of prison capacity constraint as a general purpose of guidelines creation and lists three states – Michigan, Pennsylvania, and Wisconsin – as being constructed without such a purpose.

Marvel considers two main dependent variables: prison admissions per capita and incarceration rates. The analysis was conducted using a pooled time-series analysis with fixed effects (state effects and year effects). Despite a very high r^2 in both models presented (0.99), very few independent variables reached significance levels in his analyses. State and year dummies were found to be significant; besides these controls, only percent of the population that was 25-34 was significant and positively associated with admissions and incarceration rates ($p<.05$). For those states with guidelines that were constructed to control for prison populations, guidelines were associated with lower prison populations; in contrast, in those states with guidelines that were not constructed to control prison populations, the direction was reversed.[60]

For some time, Marvell's analysis was the best and only cross-state comparison of the impact of sentencing guidelines. However, several methodological problems plague the analyses, rendering Marvell's findings suspect. First, there is an issue of independence of observations. Data is pooled for all years; however, since the incarceration rate is defined as those sentenced to prison for one year or more, Marvell is counting some people twice by including incarceration rates for every year as a dependant variable in his analyses. A better approach would have been to separate observations by taking data every three years to ensure observations are independent. Failure to account for these issues may have increased collinearity problems as well as biased

[59] As noted above, Wisconsin has operated under two different versions of sentencing guidelines, both of which were voluntary. The version of Wisconsin's guidelines used in Marvell's analysis exited from 1983-1994; while judges were required to "consider" the guidelines and state reasons for imposing a sentence outside the guidelines, there was no mechanism for enforcing judicial compliance with the guidelines recommendations. Thus, the system was voluntary. The new version of Wisconsin's voluntary guidelines were adopted in 1999.

[60] Results were even worse for admissions data. Only the "major" crime rate ended up positively associated with increases in state-level prison admissions controlling for all other variable sin the model.

estimation of regression coefficients. This issue is raised by Marvell, but never fully addressed.[61] No information about statistical tests is included in the paper to assess whether independence of observations is achieved.

A larger problem arises in the specification of guidelines variables, which is inappropriate for the research question driving the analysis. His usage of dummies for each state guideline system in a general model implies that there may be an association between these variables and the prison population in a different state. For example, the use a dummy variable for the presence of guidelines in, for example, Delaware in the general model implies that the presence of guidelines in Delaware could affect incarceration rates in Mississippi. It is not advisable to use state-specific covariates in general pooled time-series models because they generate these sort of unwelcome assumptions. In order to detect state-specific effects Marvell would have had to conduct separate time-series regressions for each state without pooling data from several states; by conducting state-specific time series analyses, Marvell could have made some statement about the specific impact of sentencing guidelines in, for example, Delaware on Delaware's incarceration rate. As such, Marvell's findings are inaccurate.

There are additional research design considerations omitted in Marvell's analyses, such as the need to develop a better specification for the models; for instance, Marvell includes very few control variables in his analyses, failing to include a crime control variable.[62] In addition to these procedural issues, selecting only states in which presumptive guidelines went into effect before 1990 may be a source of bias, excluding several states that adopted guidelines in the 1990s. Again, Marvell notes the additional states adopting guidelines in the 1990s, but does not fully explain why they were omitted from the analyses.

Despite the problems with the analyses, for some time this represented the most developed analyses of the impact of sentencing guidelines across states. Given the impressive goodness of fit statistic in Marvell's study (adjusted R2 =.99) we decided to replicate his findings. Our best approximation is presented in Table 4-4. (Results from Marvell's analyses are in the column Original; our replication of Marvell's analyses are in the column Replication).

[61] In footnote 41, Marvell (1995) notes that "there coefficients (sic) in Table 1 can be misleading due to possible collinearity;" however, this is the only statement on the possible co-linearity problem.

[62] There is a short footnote providing a very succinct explanation. See footnote 41 in Marvell (1995:703). The lack of other policy variables and its consequences in the specification of the models is acknowledged by Marvel (705).

Table 4-4 Results for Fixed-Effects Models, Replication of Marvell (1995)

	Original	Replication
% population 18-24	0.06	-0.261
% population 25-34	0.66**	0.253#
Unemployment Rate	-0.22	-0.001
Personal Income	0.08	0.273*
Guidelines		
Delaware	-0.17*	-0.06*
Florida	-0.23*	-0.14***
Michigan	0.02	0.02
Minnesota	-0.35**	-0.15***
Oregon	-0.13*	-0.15***
Pennsylvania	0.14**	0.09**
Tennessee	-0.21**	-0.09**
Washington	-0.33**	-0.17***
Wisconsin	-0.09	-0.03
R-square	.99	.87
N	842	850

Note: Year dummies omitted.
significant p<.1
* significant p<.05
** significant p<.01
*** significant p<.001

In order to produce this set of results we initially followed Marvell's description of his analytical procedure, using the same data sources (provided that the data employed in the original source was publicly available). Despite our best efforts we could not approach the published results. In order to produce the outcomes observed in Table 4-4, we dismissed any sort of autocorrelation correction, taking only the state's incarceration rate for every year and connecting it with actual values for control variables. No differencing was implemented in either side of the equation. No consideration for inertial trends was taken into account (an often-used technique when analyzing growth). Finally, there are major violations of pooled time series methodology, which should caution the validity of Marvell's analysis as a whole. While we present our replication findings here, we believe they *do not* represent valid findings and should be disregarded as a statement of the impact of sentencing guidelines on incarceration rates.

Nicholson-Crotty (2004)

A more careful research design is developed by Nicholson-Crotty (2004). Nicholson-Crotty examines the effect of sentencing guidelines on prison populations between 1975 and 1998. This study develops a better specification of the models tested and expands the analyses to include both presumptive and voluntary sentencing guidelines. The author is careful at correcting for panel heteroskedasticity given the nature of his data and also implements an order 1 serial autocorrelation correction.

Like Marvell, Nicholson-Crotty considers two main dependent variables: prison admissions per capita and incarceration rates. The analysis was conducted using a pooled time-series analysis with fixed effects (state effects and year effects). Nicholson-Crotty achieved a lower r^2 (0.61 for the analysis of commitment rates and 0.60 for the analysis of incarceration rates) than Marvel, and had very few independent variables that reached significance levels in his analyses. State and year dummies were found to be significant in both of Nicholson-Crotty's models. Besides these controls, only percent of the population that was black, percent of the population that was 25-34, and state population were significant and positively associated with admissions rates ($p<.05$); similarly, in addition to state and year controls, only percent of the population that was black and percent of the population that was 25-34 were significant and positively associated with incarceration rates ($p<.05$). For those states with presumptive guidelines that were constructed to control for prison populations, guidelines were significantly associated with lower prison admissions; guidelines constructed to control prison populations displayed a negative, although non-significant, association with incarceration rates as well. In contrast, guidelines that were not constructed to control prison populations, were significantly associated with higher admission rates and incarceration rates. Finally, voluntary guidelines displayed a positive, although non-significant, association with admission rates and incarceration rates.

Consistent with Nicholson-Crotty's analyses, we conducted a Fixed-Effects, pooled time-series analysis. While Nicholson-Crotty included both admission rates and incarceration rates as dependant variables, we consider only the state incarceration rate. In Table 4-5, we present the results of this routine. The first column (Original) reproduces the regression coefficients as reported by Nicholson-Crotty (2004:406) and the second column (Replication) presents our attempt to replicate these findings with our own data.

Table 4-5 Results for Fixed-Effects Models, Replication of Nicholson-Crotty (2004)

	Original	Replication
Mandatory resource linked Guidelines	-5.91	-25.07**
Mandatory non-resource linked Guidelines	32.01*	25.86*
Voluntary Guidelines	2.28	7.86
Unemployment rate	-.14	0.58
% population 18-24	.97	1.770
% population 25-34	3.78**	1.165
% population black	2.38*	5.878***
Crime rate	.00	0.001
State ideology	-.15	-0.06
Population	.01	0.002
% Urban	.59	-0.133
Personal Income	-471.02	0.002
Adj R-square	.60	.73
Chi2	1556***	1743***
N	1200	1200

Note: Year dummies omitted. The variable "% urban" does not correspond to the variable "population density" used in the original study. However, it can be used as a reasonable approximation.
* significant p<.05
** significant p<.01
*** significant p<.001

As table 4-5 indicates, we found general support for Nicholson-Crotty's models; however, our results were quite different. Most striking is our finding regarding sentencing guidelines constructed to control prison populations; while Nicholson-Crotty found a negative, although non-significant, association between such guidelines and incarceration rates, we found a very strong, significant negative association. Further, while Nicholson-Crotty's model explains about 60 percent of the variance in the outcome, our model explained 73 percent of the variance. While several factors could explain these discrepancies, we believe most of the differences arise from problems in operationalizing the sentencing guidelines variables.

The most significant problem when conducting a replication of this study centers on the classification of states as guidelines states; more specifically, we had great difficulty in determining which guidelines states were defined as "mandatory resource-linked" or "mandatory non-resource linked" systems. While the author provides an informative table listing states with sentencing guidelines (2004: 397), it is not clear for a number of states the coding procedure

followed. In particular, we had problems classifying Alaska, Florida, Ohio, Delaware and Louisiana. For example, while Louisiana and Delaware are voluntary guidelines states, their guidelines are, nonetheless, based on resource constraints; it is unclear from Nicholson-Crotty's table whether these states are classified as mandatory resource linked guidelines or voluntary guidelines. Similarly, Nicholson-Crotty lists Florida simply as "voluntary until 1994;" but, in 1994, Florida adopted a "presumptive" (or "mandatory" in Nicholson-Crotty's terminology) sentencing guidelines systems linked to available correctional resources. Yet, it is unclear if this distinction is made in the analyses. While Nicholson-Crotty improves on the prior analyses of sentencing guidelines, these specification problems, unfortunately, render the findings confusing.

Our Findings

Despite the contribution of their findings, the studies by Marvell and Nicholson-Crotty remain limited in their scope and the applicability of their findings. In addition to the specification problems noted above, neither author considers the potential impact of other policies on incarceration rates; nor do these studies attempt to evaluate the impact of presumptive sentencing systems on incarceration rates. Our analysis of the impact of structured sentencing on incarceration rates builds on the models developed in the previous chapter, considering the impact of determinate sentencing. They also expand our notion of structured sentencing to include all states that provide some form of recommended sentences for offenses – presumptive sentencing systems, presumptive sentencing guidelines systems, and voluntary guidelines systems. Tables 4-1 and 4-2 list the states adopting structured sentencing systems and the years of operation of those systems.

The models described below (see Table 4-6) follow the same analyses outlined in Chapter Three. Thus, we will not reiterate the specifics of the methodology here. In Model 1, we introduce the first structured sentencing policy variable; this variable includes all systems with some form of structured sentencing – presumptive sentencing, presumptive sentencing guidelines, and voluntary sentencing guidelines; we also look at the interaction between structured sentencing and determinate sentencing. In the second model, we consider only those systems that provide some form of presumptive structured sentencing – presumptive sentencing or presumptive sentencing guidelines; again, we consider the interaction of these systems with determinate sentencing. In Model 3 we divided the presumptive structured sentencing into presumptive guidelines and presumptive sentencing. Finally, in Model 4 we look specifically at sentencing guidelines, distinguishing between presumptive sentencing guidelines and voluntary sentencing guidelines.

Table 4-6. Results for Fixed and Random Effects, Structured Sentencing

Variable	Model 1	Model 2	Model 3	Model 4
Violent crime rate	0.108**	0.104**	0.092**	0.098**
Property crime rate	0.002	0.004	0.007	0.004
% population 18-24	2.006	2.076	2.484	1.677
% population 25-34	2.841	2.385	3.023	2.681
% population Black	11.531**	10.498**	10.189**	9.491*
% population Hispanic	8.316***	8.725***	8.286***	8.006***
% population in SMAs	-0.102	-0.077	-0.052	-0.148
% population religious fundamentalist	7.355*	6.781*	6.019*	6.534*
Income per capita	-0.009***	-0.009***	-0.009***	-0.009***
Unemployment rate	0.967	1.194	0.997	0.626
Poverty rate	-4.744**	-4.463**	-4.344**	-4.562**
Gini	151.372	227.072	224.868	254.213
Revenues per 100k population (*1000)	0.063*	0.058*	0.053	0.062*
Welfare per 100k population (*1000)	-0.802***	-0.797***	-0.793***	-0.745**
FTE Police per 100k population	0.169*	0.176**	0.154*	0.159*
Drug arrest rate	563.996***	556.928***	535.824***	550.693***
Governor (Republican)	13.536**	13.052*	11.256*	12.419*
Citizen political ideology	0.171	0.070	0.090	0.064
Determinate Sentencing	-9.649	9.030	6.413	-9.042
Structured Sentencing	7.534			
Determinate * Structured Sentencing	-10.689			
Presumptive Structured Sent.		21.131		
Det * Presump Structured Sent.		-61.890**		
Presumptive Guidelines			-5.891	-31.810*
Presumptive Sentencing			33.309	
Det * Presumptive sentencing			-33.048	
Det * Presumptive Guidelines			-41.454	
Voluntary Guidelines				20.320
1975	-3.655	-5.615	-10.545	-5.457
1978	24.502	21.831	12.744	22.380
1981	46.038	41.538	30.922	43.913
1984	79.429***	77.082***	68.895**	79.325***
1987	116.638***	112.616***	103.656***	110.996***
1990	156.042***	153.459***	147.137***	151.365***
1993	218.550***	214.853***	210.488***	214.188***
1996	284.180***	280.530***	278.166***	278.616***
1999	343.788***	338.193***	335.490***	335.296***
2002	354.819***	348.907***	347.948***	347.166***
Constant	-151.157	-166.022	-169.504	-152.856
R Within	.853	.856	.859	.856
R overall	.639	.653	.672	.675
N	544	544	544	544

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

As Table 4-6 indicates, the R "squared," or the measure of how well the model predicts state incarceration rates, does not change much with the inclusion of new policy variables; however, the progressive models present a slight increase in the overall fit (r^2 = .86 in the Fixed-Effects

final model as compared to $r^2 = .85$ in the first model), indicating that the models get better at predicting state incarceration rates.[63]

What is remarkable in the above models is the stability of the social variables after inclusion of additional policy variables. The social variables found to be significant in Model 1 remain significant and in the same direction in each of the subsequent models (with the exception of state revenues, which is not significant in Model 3). States with higher levels of crime and higher minority populations have higher levels of incarceration. Similarly, states with larger religious fundamentalist populations and Republican governors have higher incarceration rates. Some systemic variables continue to be significant such as the size of the police force which is positively related to incarceration rates and the level of welfare payments in the state which is negatively related. Economic indicators such as income per capita and the poverty rate continue to be negatively associated with incarceration rates while state revenues continue to be positively associated with incarceration rates. Finally, states with higher drug arrests continue to have higher incarceration rates.

Several variables were not significant in any of the models explored. For example, the property crime rate, the age structure of the population, and the percent of the population living in urban areas were not associated with incarceration rates in any of the models. Economic variables such as unemployment rates and income inequality were similarly not associated with incarceration rates. Finally, ideological variables, such as government and citizen conservatism, were not related to incarceration rates.

As Model 1 indicates, the inclusion of structured sentencing does not change the significance of any social variables found in the prior analyses. While the coefficient for the structured sentencing variable was positive, it was not significant. The same pattern is observed for the interaction term of structured sentencing and determinate sentencing. The inclusion of structured sentencing also changed the significance of determinate sentencing (from the Random-Effects model in Table 3-7), however, the influence remained negative.

Model 2 attempts to make a finer distinction between different types of structured sentencing systems, focusing on presumptive structured sentencing systems – those systems with presumptive sentencing or presumptive sentencing guidelines. Again, determinate sentencing is not significant; but, after inclusion of presumptive structured sentencing, the direction of the influence of determinate sentencing changes and becomes positive. While the coefficient for the presumptive structured sentencing variable was positive, it was not significant. However, the interaction term between presumptive structured sentencing and determinate sentencing was negative and significant ($p<.01$), indicating that states that control both sentencing and release decisions have lower incarceration rates than other states.

[63] The r^2 statistics reported by this procedure do not have all the properties of OLS R^2 (in fact, Stata calls them r "squareds": the ratio of the variances is not equal to the squared correlation + it can higher than 1).

Model 3 attempts to provide a more specific distinction in terms of the presumptive nature of the state's sentencing structure, by distinguishing between presumptive sentencing and presumptive sentencing guidelines. We observe that none of the policy variables or combinations of policies in Model 3 are significantly related to incarceration rates; however, again, there is come indication that states with the combination of determinate sentencing and either form of presumptive structured sentencing have lower incarceration rates (although the relationship is not significant). Similarly, presumptive guidelines alone is related to lower incarceration rates, although the effect is also not significant.

Finally, in Model 4, we distinguish between different types of sentencing guidelines. As it was already noted for other models, in Model 4, the effect of determinate sentencing remains not significant as we expand the number of sentencing structure variables; however, in Model 4, the coefficient for the determinate sentencing variable again becomes negative, indicating states with determinate sentencing may have lower incarceration rates than other states. In contrast, presumptive sentencing guidelines alone was associated with lower incarceration rates even when controlling for determinate sentencing. While the voluntary sentencing guidelines variable was not significant, it was positive. As Model 4 suggests, structuring sentencing decisions through sentencing guidelines matters more than regulating release decisions. However, the nature of the guidelines also matters: presumptive guidelines states have lower incarceration rates than other states while voluntary guidelines states have higher incarceration rates than other states (although the effect of voluntary sentencing guidelines is not significant). Yet, this model does not account for effects in states that have both sentencing guidelines and determinate sentencing.

Our most accurate models are presented in the Table 4-7. Here we include the separation between voluntary and presumptive guidelines as well as their interaction effects with the determinate sentencing variable (see Final Model). Before considering these models, we decided to include an alternate regression focusing on the link between presumptive guidelines and correctional resource constraints (see Resource Model); as it was already mentioned in this report, the distinction between resource-oriented and non-resource oriented legislation has been addressed in recent articles (Marvell, 1995; Nicholson-Crotty, 2004).

Once the definite set of variables were introduced in the models, we ran an initial number of tests to assess its correct specification. First we conducted a Breush-Pagan test for the significance of Random Effects. According to this test statistic ($Chi2(1)=146.78$) we were able to reject the null ($p<.001$) and therefore provide support for the relevance of the Random Effects. This result was confirmed via the more extensive Hausman test of model specification ($Chi2(27)=33.97$, $p=.167$). Given this result, we focused on the outcomes provided by the Random-Effects regression since they are more efficient (less chances of Type I error).

Table 4-7 Results for Fixed and Random Effects, Final Models Structured Sentencing

	Resource Model	Final Model			
	Fixed Effects	Fixed Effects		Random Effects	
	b	b	SE	b	SE
Violent crime rate	0.087*	0.082*	0.036	0.094**	0.031
Property crime rate	0.005	0.007	0.006	0.004	0.005
% population 18-24	1.415	1.696	4.862	0.732	4.362
% population 25-34	2.240	2.396	2.874	3.312	2.707
% population Black	9.276*	9.799*	3.919	4.037***	0.984
% population Hispanic	8.877***	8.950***	2.098	1.817	0.951
% population in SMAs	0.032	0.013	0.567	-0.130	0.315
% population religious fundamentalist	6.428*	6.551*	2.991	1.621	0.888
Income per capita	-0.009***	-0.009***	0.002	-0.006**	0.002
Unemployment rate	0.514	0.760	2.085	2.258	1.965
Poverty rate	-4.415**	-4.345**	1.454	-4.614***	1.406
Gini	230.973	229.642	309.898	549.443*	276.590
Revenues per 100k population (*1000)	0.064*	0.062*	0.027	0.075***	0.023
Welfare per 100k population (*1000)	-0.783**	-0.727**	0.241	-1.058***	0.217
FTE Police per 100k population	0.169*	0.160*	0.067	0.113	0.063
Drug arrest rate	550.147**	506.032**	160.268	515.337***	149.652
Governor (Republican)	10.470*	11.369*	5.134	11.832**	5.078
Citizen political ideology	0.060	0.004	0.341	-0.171	0.295
Determinate Sentencing	-14.112	-4.223	11.928	-7.796	10.774
Resource-oriented Presumptive Guidelines	-53.892**				
Non-resource oriented Presumptive Guidelines	-3.649				
Presumptive Guidelines		6.930	20.316	-8.179	18.917
Det * Presumptive Guidelines		-65.487*	27.254	-49.898*	25.200
Voluntary Guidelines	24.379*	16.591	14.032	15.340	12.877
Det * Voluntary Guidelines		15.185	24.946	14.104	23.069
1975	-7.517	-9.926	16.161	-8.070	13.895
1978	21.131	16.369	18.577	15.650	15.819
1981	42.270	36.292	23.799	34.873	19.547
1984	77.290**	71.677**	23.447	68.652***	19.561
1987	110.110***	104.615***	26.962	92.931***	22.684
1990	150.499***	144.815***	30.355	136.011***	24.400
1993	214.160***	207.328***	31.391	206.784***	24.711
1996	277.709***	272.294***	34.286	274.308***	26.617
1999	333.353***	327.601***	35.675	321.102***	28.292
2002	344.697***	338.720***	38.055	342.005***	29.484
Constant	-145.463	-156.629	132.149	-159.950	112.642
R2 Within	.858	.858		.853	
R2 Overall	.675	.666		.835	
N	544	544		544	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

According to the Fixed-Effects estimators of the Resource Model, we found support for the relevance of the classification used in prior analyses: only resource-linked guidelines are significantly associated with lower incarceration rates ($p<.01$). Non-resource linked guidelines are also negatively related, but failed to reach the threshold of statistical significance. In contrast, voluntary guidelines are significantly associated with higher incarceration rates ($p<.05$). The remaining policy and non-policy variables behave in the direction already observed in Table 4-6.

However, the Final Model, considering the interaction of presumptive and voluntary sentencing guidelines and determinate sentencing, explains more of the variance than the Resource Model. When interactions between presumptive and voluntary sentencing guidelines and determinate sentencing are included in the analyses, the effect of presumptive sentencing guidelines drops out and only the interaction between determinate sentencing and presumptive guidelines becomes significant ($p<.05$). In particular, results suggest that states with both determinate sentencing and presumptive sentencing guidelines have lower incarceration rates than other states; however, neither policy alone is significantly related to incarceration rates (although both are in the negative direction in the Random-Effects model, but not significant). Conversely, states with determinate sentencing and voluntary sentencing guidelines have higher incarceration rates than other states – although this relationship is not significant in our models

While there are slight disagreements between the Random-Effects and the Fixed-Effects models, it is interesting to note across models the evolution of the coefficients for the sentencing structure variables. We observe for instance that determinate sentencing is never significant when other policy variables are included in the models. In general, presumptive guidelines are negatively associated with incarceration rates while voluntary guidelines behave in the opposite direction. Finally, the interaction terms between sentencing guidelines and determinate sentencing were interesting. The coefficient for the interaction variable for presumptive sentencing and determinate sentencing was negative and significant.

Conclusion

Increasing the determinacy and structure of state sentencing and corrections systems has been the goal of many policymakers over the last 30 years. The impact of policies that make a state's system more determinate or more structured, however, is not entirely clear and has been subjected to few empirical studies. The results from Chapter Three suggested that increasing determinacy alone, through the abolition of discretionary parole release, could lead to lower incarceration rates. Our analyses confirmed the findings of prior research – states with determinate sentencing have lower incarceration rates than other states. However, we cautioned against a simple connection between determinate sentencing and incarceration rates; given the variation in the construction of determinate sentencing systems, we suggested that other policies may have a complimentary effect on prison populations.

Indeed, prior research showed that increasing structure alone, through the creation of presumptive sentencing guidelines, could also lead to lower incarceration rates. Our analyses confirmed these findings – states with presumptive sentencing guidelines have lower incarceration rates than other states. However, again, such a simple connection may not be warranted. There is a great deal of overlap between those states with presumptive sentencing guidelines and those with determinate sentencing; analyses looking only at the presence of one of these policies may fail to account for the possibility that findings may be picking up the actual effects of another policy.

Our final analyses show that it is only the introduction of both greater determinacy *and* greater structure that leads to lower incarceration rates. Thus, controlling sentencing *and* release decisions matters more than controlling either form of discretion alone. But how the state controls sentencing discretion also matters. Simply providing some form of recommended sentences – in the form of presumptive sentencing, presumptive sentencing guidelines, or voluntary sentencing guidelines – is not enough; states with structured sentencing or the combination of structured and determinate sentencing have neither higher nor lower incarceration rates than other states. However, making recommended sentences presumptive – in the form of presumptive sentencing or presumptive sentencing guidelines – does make a difference; states with presumptive recommended sentences *and* determinate sentencing have lower incarceration rates than other states. Further, states with presumptive sentencing and determinate sentencing have lower incarceration rates than other states; however, states with presumptive sentencing alone have higher incarceration rates.

Most of the discussion on structured sentencing in recent years has been focused on the impact of sentencing guidelines on incarceration rates. As noted above, our analyses show that the presence of sentencing guidelines matter; however, the impact depends on the interaction of sentencing guidelines with determinate sentencing. States with presumptive sentencing guidelines and determinate sentencing have lower incarceration rates than other states; conversely, states with voluntary sentencing guidelines and determinate sentencing have higher incarceration rates than other states (although the effect is not significant).

The way states control the determinacy and structure in their systems continues to influence the size of state incarceration rates. Yet, while determinacy and structure affect incarceration rates, our findings show the continued stability of social factors. Race, welfare, wealth, politics, and the enforcement of drug crimes exert a strong influence on the size of a state's incarceration rate. The next chapter adds another layer of complexity to the determinate/indeterminate and structured/unstructured sentencing systems, by exploring the impact of time served requirements on incarceration rates.

Chapter Five: Time Served Requirements

The desire to introduce greater determinacy into state systems may create a mediating effect on prison populations. As Chapter Three showed, states with determinate sentencing have lower incarceration rates than other states. However, as Chapter Four suggested, this effect is contingent on the adoption of presumptive sentencing guidelines. Indeed, it is only when states strictly control both sentencing *and* release decisions that incarceration rates are lower. But the abolition of parole is only one way that a state may control release decisions. Other procedural constraints can introduce greater "determinacy" in a state's system by ensuring time served in prison more closely reflects the term of imprisonment imposed by the court. Such restrictions on time served are present in both indeterminate sentencing systems (through parole eligibility requirements) and determinate sentencing systems (through release requirements) and may further mediate the ultimate impact of determinate sentencing on incarceration rates.

Time served requirements, in both determinate and indeterminate systems, have increased significantly over the last 30 years. In the 1990s, states placed additional restrictions on time served requirements for violent offenders under the federal Violent Offender Incarceration and Truth-in-Sentencing (VOI/TIS) grant program. By 2002, 28 states had adopted truth-in-sentencing laws requiring violent offenders to serve at least 85 percent of the sentence imposed by the sentencing court before becoming eligible for release from prison (Sabol, et al., 2002). However, while states adopted Truth-in-Sentencing requirements under the federal grant program, many already had specific time served requirements for violent offenders in their criminal codes.

While increased time served requirements have received a great deal of attention among policymakers, practitioners, and academics, no studies of which we are aware have attempted to examine the impact of such increases on incarceration rates across states and only a few studies have considered the impact of federally-funded Truth-in-Sentencing laws (Turner, et. al. 1999; Grimes and Rogers, 1999). This chapter presents an analysis of the impact of time served requirements. It begins by describing time served requirements in the United States and then presents our analyses, which look at the impact of increases in time served for all offenders and violent offenders and continue to consider the interactive impact of determinate sentencing, structured sentencing, and time served requirements on state-level incarceration rates.

Time Served Requirements in the States

While indeterminate sentencing was attacked in the 1970s for the relative disconnect between offenders' imposed sentences and their actual time served in prison, most states placed some constraints on when an offender could be released from prison. In 1975, however, nine states – Colorado, Hawaii, Idaho, Iowa, Kansas, Minnesota, North Dakota, Oregon, and Washington – had no time served requirements; most offenders in these states were eligible for release any time

after admission to prison;[64] in 2002, only three states retained such a provision – Hawaii, Iowa, and North Dakota. Comparing such time served requirements across states, however, raises several difficulties. Some states set time served requirements according to the maximum term (under some indeterminate systems) or the fixed term (under determinate systems) imposed by the court (i.e. requiring offenders to serve a certain percentage of the maximum or fixed term before eligibility for release), while other states set time served requirements according to the minimum term imposed by the court. Nonetheless, under either mechanism, time served requirements across the states have steadily increased sine the 1970s.

Exhibit 5-1 shows changes in time served requirements across states that base time served on the *maximum or fixed term* imposed by the court.[65] The time served requirements described here reflect states' general parole or release provisions and do not include specific time served requirements for certain sub-groups of offenders. The middle line represents the average percentage of the term offenders are required to serve before release from prison. In 1975, offenders were required to serve an average of 28 percent of the term imposed before release from prison; by 2002, this had increased to 45 percent. As the lower line in Exhibit 5-1 indicates, some states allow offenders to be released from prison any time after admission; thus, time served in these states is 0 percent. Conversely, in 1975, no states required offenders to serve the entire maximum term imposed by the court; in 2002, three determinate sentencing states – North Carolina, Ohio, and Wisconsin – required all offenders to serve 100 percent of the fixed term (as indicated by the upper line in the graph).

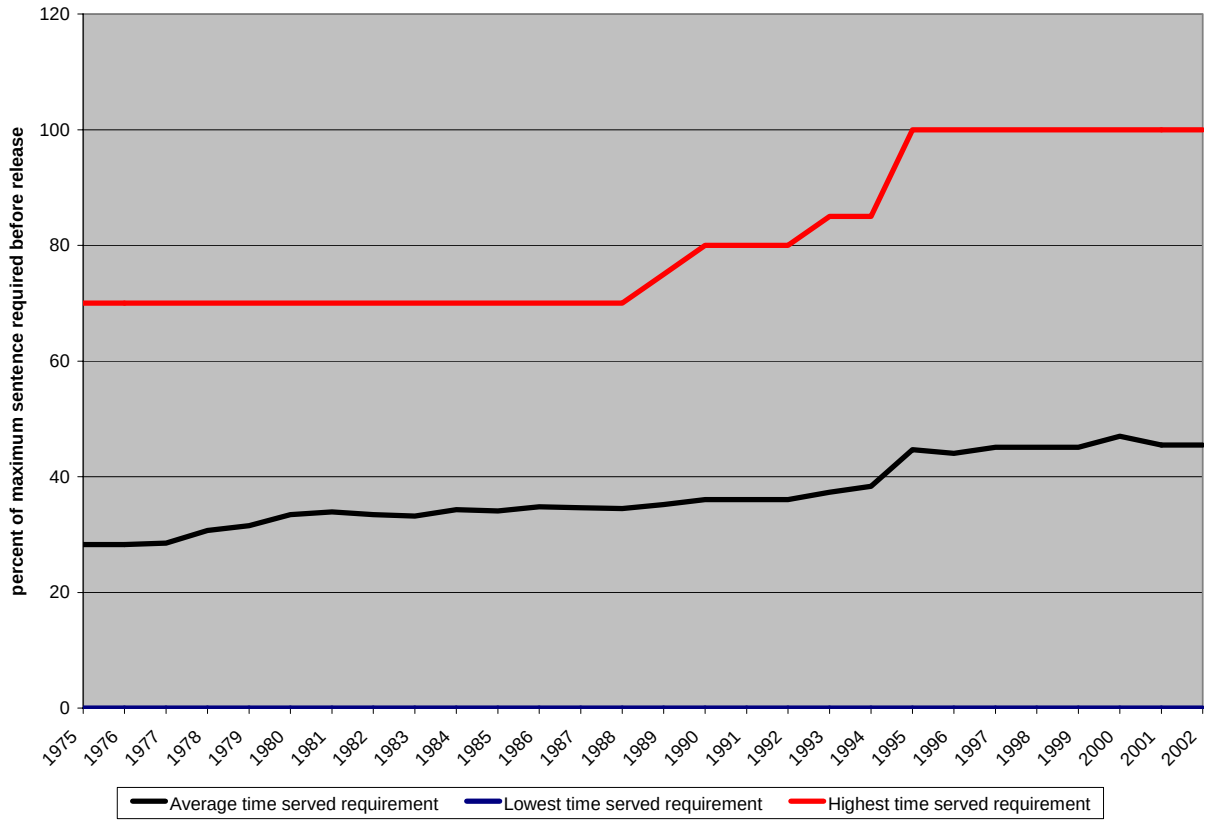
Exhibit 5-2 shows the changes in time served requirements across states that base time served on the *minimum* term imposed by the court. Again, the middle line represents the average percentage of the term offenders are required to serve before release from prison. In 1975, offenders were required to serve an average of 70 percent of the minimum term imposed before release from prison; by 2002, this had increased to 93 percent. As the lower line in Exhibit 5-2 indicates, in 1975 some states allowed offenders to be released from prison any time after admission; thus, time served in these states was 0 percent. However, by 1981, all states required offenders to serve some portion of the minimum term imposed; by 2002, no state allowed offenders to be released prior to serving at least 50 percent of the minimum term imposed. As the upper line indicates, several states have historically required offenders to serve 100 percent

[64] In Alabama, all offenders must serve 33 percent of their maximum sentence before becoming parole eligible; however, the Board of Pardons and Paroles may parole any offender prior to the 33 percent requirement by unanimous vote (AL Code 15-22-28(e)); thus, any offender can be released immediately after entering prison.
[65] These represent time served requirements in both indeterminate systems that base parole eligibility on the maximum term imposed by the court and determinate systems that base mandatory release dates on the fixed term imposed.

of the minimum term imposed prior to release. In 1975, 12 states required offenders to served 100 percent of the minimum term imposed; by 2002, nine states had similar requirements.[66]

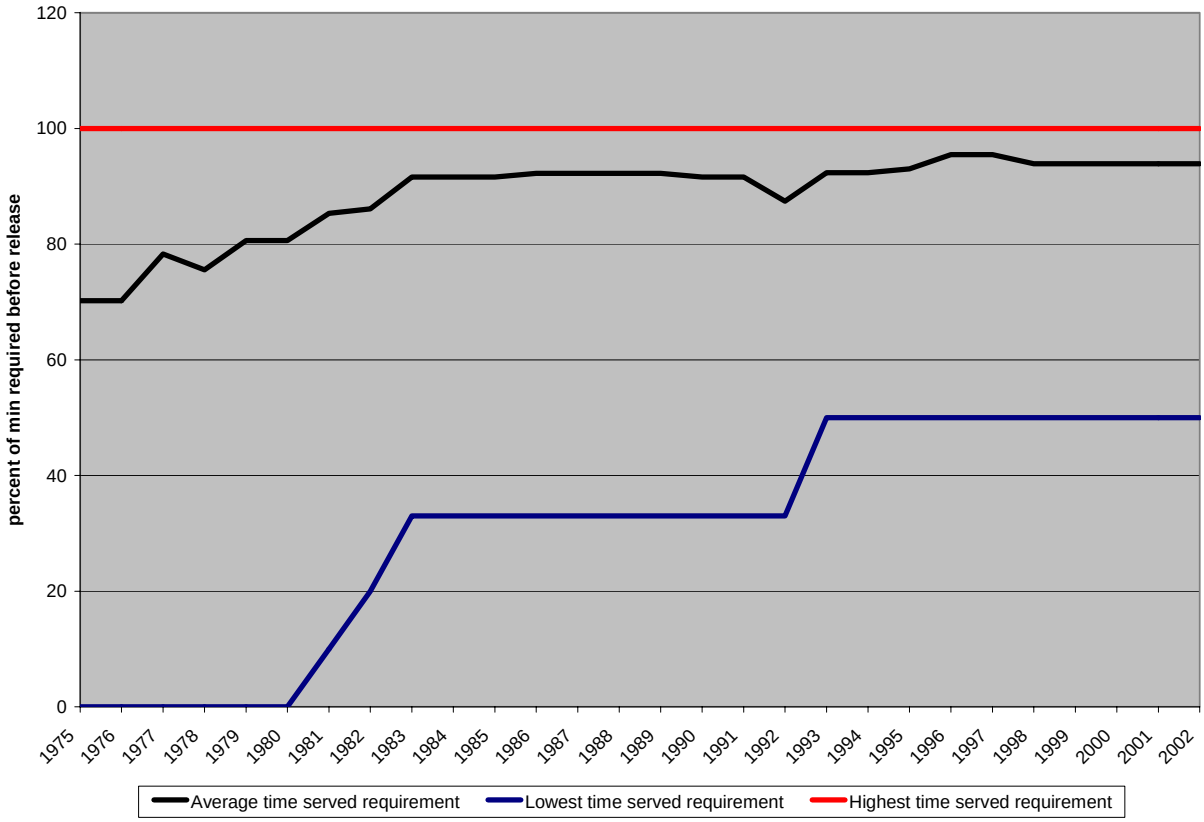
These increases are not part of the larger federally-funded Truth-in-Sentencing initiative, which targeted time served requirements for violent offenders. Rather, these increases reflect time served requirements directed at all offenders.

Exhibit 5-1 Time Served Requirement, Based on Maximum or Fixed Term Imposed, 1975-2002



[66] In 1975, these states were: Connecticut, Illinois, Indiana, Michigan, Nebraska, New Hampshire, New Mexico, New York, Pennsylvania, Utah, Vermont, and Wyoming. In 2002, these states were: Idaho, Massachusetts, Michigan, New Hampshire, Nevada, Pennsylvania, Utah, Vermont, and Wyoming.

Exhibit 5-2 Time Served Requirement, Based on Minimum Term Imposed, 1975-2002



Truth-in-Sentencing

While a majority of states maintain the broad statutory sentence ranges and case-specific discretion characteristic of the indeterminate system, both indeterminate and determinate sentencing states have sought greater determinacy through the adoption of federally-funded Truth-in-Sentencing laws. In 1994, the federal government enacted legislation creating federal Violent Offender Incarceration and Truth-in-Sentencing (VOI/TIS) grants for states. Under the program, states requiring violent offenders to served 85 percent of the sentence imposed by the court could receive funding from the federal government to expand jail and prison capacity and to ensure that prison space was reserved for violent offenders.[67] The grants were available to states that based time served on either the minimum term imposed or the maximum term imposed by the court; thus, there was no requirement that states alter how they procedurally determined eligibility for release from prison (see Sabol, et. al., 2002). By 2002, 28 states had

[67] These grants are no longer available.

adopted truth-in-sentencing laws requiring violent offenders to serve at least 85 percent of the sentence imposed by the sentencing court before becoming eligible for release from prison.[68]

While states adopted Truth-in-Sentencing requirements under the federal grant program, many already had separate time served requirements for violent offenders; although these requirements did not necessarily meet the federal definition of Truth-in-Sentencing, all such laws required violent offenders to serve longer portions of their imposed sentences than other offenders. Table 5-1 shows those states with a separate, longer violent offender time served requirement in 1975 and 2002. In 1975, just four states had separate time served requirements for violent offenders; by 2002, 26 states had such policies.

[68] Theses states include: Arizona, California, Connecticut, Delaware, Florida, Georgia, Illinois, Iowa, Kansas, Louisiana, Maine, Michigan, Minnesota, Mississippi, Missouri, New Jersey, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Utah, Virginia, and Washington.

Table 5-1 States with Separate Time Served Requirement for Violent Offenders, 1975 and 2002

State	1975	2002
Alabama		●
Alaska		●
Arizona		
Arkansas		●
California		●
Colorado	●	
Connecticut		●
Delaware		
Florida		
Georgia		●
Hawaii		
Idaho	●	
Illinois		●
Indiana		●
Iowa		●
Kansas		
Kentucky		●
Louisiana		●
Maine		
Maryland		●
Massachusetts	●	
Michigan		
Minnesota		●
Mississippi		●
Missouri		●
Montana	●	
Nebraska		
Nevada		
New Hampshire		
New Jersey		●
New Mexico		●
New York		●
North Carolina		
North Dakota		●
Ohio		
Oklahoma		●
Oregon		●
Pennsylvania		
Rhode Island		
South Carolina		●
South Dakota		●
Tennessee		●
Texas		●
Utah		
Vermont		
Virginia		
Washington		●
West Virginia		
Wisconsin		
Wyoming		

Time Served and Incarceration Rates: An Analysis Over Time

Increased time served requirements and Truth-in-Sentencing laws, with their express desire to make offenders serve nearly their entire sentence before release, may significantly increase incarceration rates by filling prison space with inmates serving longer sentences than previously enforced. While the impact of time served requirements on incarceration rates has not been examined in prior analyses, Truth-in-Sentencing laws have not been shown to increase incarceration rates in this way (Turner et al., 1999). In fact, Grimes and Rogers (1999) find that Truth-in-Sentencing laws requiring inmates to serve 85 percent of their sentences actually reduced prison admissions and prison population growth in Mississippi; however, clear explanations for this relationship were not apparent.

In order to assess the extent of the relationship between time served requirements and incarceration rates, we build on the cross-sectional time-series models already tested in previous chapters. In this chapter, we add two variables accounting for a state's time served requirements. The first variable – Time Served (all offenses) – is a continuous variable measuring the percent of the sentence imposed that *most* offenders are required to serve before release from prison; since we control for determinate sentencing, the time served requirement is coded the same for either determinate or indeterminate sentencing systems, measuring the minimum percent of sentence most offenders must serve before release. The second variable – Time Served (violent offenses) – is a dichotomous variable indicating whether the state has a separate time served requirement for violent offenses; since all states define "violent offense" differently and apply time served requirements to different numbers of offenses, we did not create a continuous variable similar to that above. Rather, for Time Served (violent offenses), states with a separate time served requirement targeted directly at violent offenders are coded 1; states that have no separate requirement or that require all offenders to serve the same percent of the sentence imposed are coded 0. While this is not a true measure of the presence of federally-defined Truth-in-Sentencing laws, it does indicate whether the state seeks to treat violent offenders differently in setting release dates and ensuring longer prison terms.

Table 5-2 presents the findings from three regression analyses. The models described below follow the same analyses outlined in previous chapters; thus, we will not reiterate the specifics of the methodology here (see Appendix C for a complete methodology). In Model 1 we introduce the first time served variable – Time Served (all offenses) – with determinate sentencing as the only other policy variable in the analyses. In the second model, we reintroduce presumptive and voluntary sentencing guidelines and the interactions of those policies with determinate sentencing.

Table 5-2 Results for Fixed Effects Models with Time Served Requirements

	Model 1		Model 2	
Variable	b	SE	b	SE
Violent crime rate	0.093*	0.038	0.067	0.039
Property crime rate	-0.002	0.007	0.002	0.007
% population 18-24	4.515	5.156	4.350	5.128
% population 25-34	6.049*	3.018	5.061	3.007
% population Black	11.595*	4.526	9.048	4.639
% population Hispanic	6.936**	2.296	7.600***	2.323
% population in SMAs	-0.925	0.691	-0.830	0.688
% population religious fundamentalist	6.633*	3.286	5.317	3.289
Income per capita	-0.011***	0.002	-0.010***	0.002
Unemployment rate	-0.571	2.310	-0.494	2.300
Poverty rate	-4.141*	1.620	-3.478*	1.613
Gini	108.006	323.958	174.476	326.682
Revenues per 100k population (*1000)	0.088**	0.028	0.086**	0.028
Welfare per 100k population (*1000)	-0.803**	0.254	-0.749**	0.253
FTE Police per 100k population	0.173*	0.069	0.169*	0.068
Drug arrest rate	581.760***	169.905	545.624***	169.782
Governor (Republican)	12.476*	5.464	10.078	5.458
Citizen political ideology	0.396	0.378	0.243	0.378
Determinate Sentencing	-11.383	10.470	-0.875	13.327
Presumptive Guidelines			4.961	21.287
Det * Presumptive Guidelines			-64.595*	29.507
Voluntary Guidelines			16.313	14.677
Det * Voluntary Guidelines			3.745	26.661
Time Served (all offenses)	-0.042	0.144	0.045	0.149
1978	26.580***	11.784	23.513*	11.746
1981	55.141***	15.699	49.582**	15.695
1984	83.851***	17.741	79.527***	17.962
1987	127.849***	20.998	120.792***	20.923
1990	170.649***	24.752	164.825***	24.711
1993	245.392***	27.933	238.013***	27.995
1996	317.797***	31.675	309.207***	31.791
1999	378.427***	34.077	364.528***	34.361
2002	391.910***	36.720	377.230***	37.162
Constant	-123.351	143.618	-127.082	144.823
R Within	.846		.851	
R overall	.634		.684	
N	494		494	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

As Table 5-2 indicates, the R "squared," or the measure of how well the model predicts state incarceration rates, actually decreased slightly with the inclusion of the time served variables; as Table 4-6 showed, we achieved an R-squared of 0.86 in the Fixed-Effects model considering presumptive and voluntary sentencing guidelines and achieved an R-squared of just 0.84 for our preliminary Fixed-Effects model considering a single time served variable, indicating that these models are not as good at predicting state incarceration rates.[69]

As the preliminary Model 1 in Table 5-2 indicates, the inclusion of time served requirements for most offenders does not change the significance of most variables found in the prior analyses (see e.g. Table 3-7); the only exception is the size of the population between the ages of 25 and 24 years of age, which significant and positively related to incarceration rates. In the Fixed-Effects model in Table 3-7, considering determinate sentencing as the only policy variable in the analyses, the coefficient for determinate sentencing was negative but not significant.[70] Including time served requirements does not change the sign of determinate sentencing, but it does change the significance. While the coefficient for the time served variable was negative, it too was not significant; while not significant, the negative coefficient seems odd, indicating a negative relationship between higher time served requirements and incarceration rates. This may be due to the higher time served requirements in determinate sentencing and sentencing guidelines states, states shown to have lower incarceration rates than other states.

Model 2 attempts to make a finer distinction between different types of sentencing systems, focusing on time served requirements in the context of structured and unstructured sentencing systems – those systems with presumptive sentencing or voluntary sentencing guidelines and those without any form of guidelines; as in the previous chapter, we also include interactions between the guidelines systems and determinate sentencing. Again, we find a significant and negative association between incarceration rates and the interaction between determinate sentencing and presumptive guidelines. The coefficients for presumptive guidelines alone, voluntary sentencing guidelines alone, and the combination of determinate sentencing and voluntary sentencing guidelines were all positive, although none of them was significant. Time served requirements and determinate sentencing are also negative but non-significant. In addition, with the inclusion of structured sentencing variables, several social variables drop out of the models, specifically, violent crime rates, age structure variables, the size of the black population, the size of the religious fundamentalist population, and the party of the governor.

Given this set of results, we decided to add the second time-served variable – a specific time served requirement targeted to violent offenses. Table 5-3 presents our results using Fixed-Effects and Random-Effects estimators. According to the Breusch and Pagan test for Random-

[69] The r^2 statistics reported by this procedure do not have all the properties of OLS R^2 (in fact, Stata calls them r "squareds:" the ratio of the variances is not equal to the squared correlation and can higher than 1).

[70] Recall that in the Random-Effects model in Chapter Three (Table 3-7), determinate sentencing alone was significant and negative, indicating that the abolition of discretionary parole release was associated with lower incarceration rates. However, once structured sentencing variables were included in the analyses, determinate sentencing alone was no longer significant, although the direction remained negative in most analyses.

Effects, we found that these effects were significant (chi2(1)=.157, p<.001) and therefore, the use of OLS coefficients would not produce consistent results. In order to assess the fit of the Random-Effects model we also compared its coefficients with the coefficients produced by the Fixed-Effects regression. The results provided by the Hausman test suggest that these two sets of coefficients are equivalent (Chi2(28)=15.37, p=0.97). At this point we decided to use Random-Effects coefficients as the basis of our narrative because they tend to be more efficient and robust than the Fixed-Effects estimators—even if statistically there are no differences between them. We also decided to present the Fixed-Effects estimators, given that we found that the state dummies made an overall significant contribution to the model (F(49, 410)=6.31, p<.001). Additional details on the specification of these models are presented in the statistical appendix.

Table 5-3 Final Models, Fixed and Random Effects with Time Served Requirements

	Fixed Effects		Random Effects	
Variable	b	SE	b	SE
Violent crime rate	0.069	0.039	0.078*	0.033
Property crime rate	0.001	0.007	0.002	0.005
% population 18-24	5.287	5.105	2.704	4.574
% population 25-34	4.726	2.989	5.051	2.804
% population Black	8.017	4.624	4.535***	1.089
% population Hispanic	7.077**	2.315	1.995	1.043
% population in SMAs	-0.804	0.683	-0.072	0.357
% population religious fundamentalist	5.455	3.266	1.432	1.000
Income per capita	-0.009***	0.002	-0.007*	0.002
Unemployment rate	-0.324	2.285	1.088	2.137
Poverty rate	-3.314*	1.603	-3.949*	1.557
Gini	282.284	326.989	569.044	294.265
Revenues per 100k population (*1000)	0.085**	0.028	0.000***	0.000
Welfare per 100k population (*1000)	-0.789**	0.252	-0.001***	0.000
FTE Police per 100k population	0.171*	0.067	0.143*	0.064
Drug arrest rate	489.887**	169.927	477.137**	158.292
Governor (Republican)	10.659*	5.424	11.077*	5.355
Citizen political ideology	0.260	0.375	-0.001	0.320
Determinate Sentencing	0.041	13.238	-5.876	11.683
Presumptive Guidelines	2.554	21.157	-7.324	19.636
Det * Presumptive Guidelines	-70.623*	29.390	-58.207*	26.511
Voluntary Guidelines	13.597	14.611	12.938	13.404
Det * Voluntary Guidelines	7.269	26.507	10.799	24.070
Time Served (all offenses)	0.202	0.160	0.182	0.137
Time Served (violent offenses)	21.201**	8.102	21.147**	7.800
1978	20.925	11.705	19.694	11.461
1981	47.162**	15.612	40.451**	14.320
1984	76.981***	17.862	70.173***	16.475
1987	115.534***	20.872	96.671***	18.652
1990	159.506***	24.621	137.893***	20.706
1993	232.529***	27.877	216.273***	22.429
1996	300.420***	31.745	283.812***	25.177
1999	351.751***	34.467	327.562***	27.931
2002	364.225***	37.234	347.846***	29.573
Constant	-177.169	145.072	-229.582	125.218
R Within	.853		.848	
R overall	.689		.820	
N	494		494	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

As the models indicate, the presence of a specific time served requirement for violent offenders is positively related to incarceration rates and significant; this indicates that states that seek to treat violent offenders differently have higher incarceration rates than other states. The effect of the combination of presumptive sentencing guidelines and determinate sentencing remains negative and significant in both models ($p<.05$). The combination of determinate sentencing and voluntary guidelines remains positive but non-significant. While there are slight disagreements between the final Fixed-Effects and Random-Effects models, most social factors remain fairly stable. The only exceptions are violent crime rates and percent of the population that is black – which are not significant in the final Fixed-Effects model but are significant in the Random-Effect model. As was already mentioned, coefficients from the Random-Effects models tend to be more consistent and robust than those produced by a Fixed-Effects estimation.

As in previous models, social variables remain fairly stable even after the inclusion of additional policy variables. The social variables found to be significant in Model 1 remain significant and in the same direction in each of the subsequent models, with the exception of the violent crime rate, the age structure, and religion. While the violent crime rate was significant only in the Random-Effects model, drug arrests and law enforcement remained significant and positively related to incarceration rates, indicating that states with larger police forces per capita and more drug arrests have higher incarceration rates. Race variables continue to be significant, indicating that states with higher minority populations have higher levels of incarceration. Similarly, economic indicators such as income per capita and the poverty rate continue to be negatively associated with incarceration rates while state revenues continue to be positively associated with incarceration rates. Finally, the level of welfare payments in the state remains significant and negatively related to incarceration rates; combined with the persistence of the variables measuring the size of minority populations in the state, this continues to lend strong support for the marginal population threat hypothesis.

As in the analyses in Chapters Three and Four, several variables were not significant in any of the models explored. For example, the property crime rate, the percent of the population living in urban areas, unemployment rates, and citizen political ideology were not related to incarceration rates.

Conclusion

Claims that increases in time served have fueled the growth in state prison populations are common in the literature (Blumstein and Beck, 1999), but little empirical work has been done on the issue. Our findings suggest that the growth is not due to higher *time served requirements* for all offenders – states with higher time served requirements do not have higher incarceration rates than other states. If increases in time served are, indeed, leading to growth in prison populations, these findings suggest that offenders may be serving longer time in prison because of longer sentences imposed, rather that because of time restrictions on release. This is partially confirmed by the findings regarding sentencing guidelines. States with presumptive sentencing guidelines

and determinate sentencing have lower incarceration rates than other states, even after controlling for the length of time offenders are required to serve under such systems; this implies that by controlling the lengths of sentences imposed through guidelines, a state can control the size of the prison population even with higher time served requirements. This is likely due to the narrowed sentence ranges and resultant lower sentences imposed under such systems.

However, our findings suggest that when states increase time served requirements for violent offenders they may, in turn, increase incarceration rates – states that set different time served requirements for violent offenders have higher incarceration rates than other states. This may be due to an actual increase in the length of time violent offenders are serving in prison. These findings also may be capturing the general punitiveness of the state; the singling out of violent offenders may be an indication of the state's approach to offenders in general

The impact of other policies on incarceration rates remain after controlling for time served requirements. As noted above states with presumptive sentencing guidelines and determinate sentencing have lower incarceration rates than other states. In contrast, states with voluntary sentencing guidelines and determinate sentencing tend to have higher incarceration rates (however, this association was not significant in either model) Again, tightly controlling sentencing and release decisions may lead to lower incarceration rates in the states; tightly controlling release decisions without placing tight controls on sentencing decisions may lead to higher incarceration rates.

Finally, while states may alter prison populations by enacting policies affecting procedural aspects of sentencing and corrections systems, social forces continue to impact prison populations. Even after controlling for several state policy choices, racial and economic differences continue to account for differences in incarceration rates across the states. Conservative political parties, even after controlling for the conservatism of citizens, also exert an independent influence on incarceration rates. In addition, enacting policies affecting sentencing and corrections may not be enough to reduce or stall the growth in incarceration rates; the number of drug arrests and the capacity of law enforcement in the state both continue to impose a strong influence on incarceration rates. The next chapter explores whether altering the sentences available for drug offenses may affect incarceration rates across the states.

Since many of these policy variables are dichotomous, we can also begin to see the relative influence of each policy on incarceration rates. For example, the influence of the combination of presumptive sentencing guidelines and determinate sentencing is more than three times as great as the influence of the time served requirement for violent offenders, which, in turn, is twice as great as the influence of the party of the governor.

Chapter Six: Sentences for Drug Offenses

Chapters Three, Four, and Five considered policies directed at altering *procedural* aspects of state sentencing and corrections systems – policies creating greater controls over release decisions, sentencing decisions, and time served. This chapter and those that follow consider policies directed as altering substantive criminal law – policies creating new penalties or significantly altering penalties for existing offenses. Increased penalties for drug offenses, stricter habitual offender laws, and mandatory sentencing laws are just a few examples of the substantive policy changes adopted by states since the 1970s. While these substantive policies are generally targeted at specific offenses or types of offenders, they may, nonetheless, have a significant impact on a state's prison population if their use and enforcement become a priority in the state. Given the scale of criminal justice responses to drug offenses since the 1970s, the impact of changes in policies surrounding drug offenses may be quite significant.

Arrests for drug offenses in the United States nearly tripled from just 580,900 arrests in 1980 to 1,678,192 arrests in 2003 (Federal Bureau of Investigation, 2004). In 2002, 340,330 persons were sentenced in state courts for drug crimes, accounting for over 32 percent of all felony convictions; of these, 39 percent resulted in a prison sentence with an average sentence length of 48 months (Durose and Langan, 2003). Between 1980 and 2001, the number of persons held in state prisons for drug offenses increased 1,195 percent, from just 19,000 prisoners in 1980 to over 246,000 prisoners in 2001. In 1980, drug offenders accounted for just 6.5 percent of states' prison populations; by 2001, they accounted for just over 20 percent (Harrison and Beck, 2003).

While the sanctioning of drug offenses has received a great deal of attention among policymakers, practitioners, and academics, no studies of which we are aware have attempted to examine the impact of drug sentences on incarceration rates across states. Analysts have argued that rising prison populations are the result of increased penalty ranges and mandatory sentencing laws for drugs, however, no cross-state analyses have tested this claim. As the previous chapters have shown, states with more drug arrests consistently have higher incarceration rates. While the inclusion of drugs arrests in prior analyses considers the impact of enforcement patterns for drug offenses on incarceration rates, it fails to capture state sentencing and corrections interventions affecting the use of imprisonment for such offenses.

This chapter presents an analysis of the impact of statutory sentences for sale and possession of controlled substances. It begins by describing sentences for drug offenses in the United States, focusing on statutory sentence ranges for sale and possession of heroin, cocaine, methamphetamine, and marijuana. It then presents our analyses, which look at the impact of these sentences and considers the interactive impact of determinate sentencing, structured sentencing, time served requirements, and drug sentences on state-level incarceration rates.

Sentences for Drug Offenses in the States

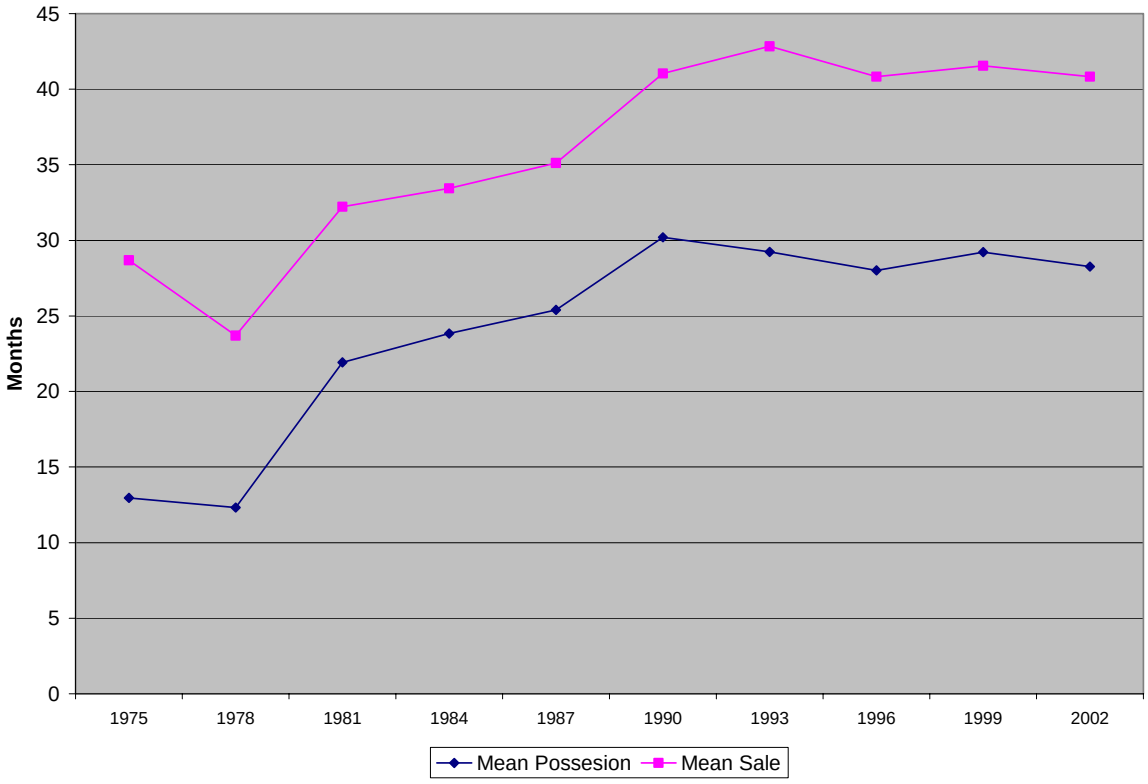
Statutory penalties for drug offenses vary dramatically across states, and comparing sentences for such offenses raises several difficulties. Every state categorizes and sentences drug offenses differently based on varying definitions of the criminal act committed (sale, delivery, distribution, manufacture, possession, etc.), the substance involved (powder cocaine, crack cocaine, methamphetamine, etc.), and the quantity of the substance involved. This project collected data on the sentencing structure for possession and sale of heroin, crack cocaine, powder cocaine, methamphetamine, and marijuana. Data included sentences for each substance by quantity and type of offense. We also collected data on twelve different sentencing enhancements (such proximity to a school, use of a firearm, or prior convictions) that may increase the underlying sentence for an offense; each one of these enhancements was classified by drug and scope of application (possession or sale charges). For the analyses that follow, we focused our initial examination on sentences for sale and possession of powder cocaine, heroin, and marijuana.

Our analyses showed that sentences for sale and possession of these three drugs showed similar trends over time. Regardless of the drug examined, we observed that statutory minimum sentences increased significantly during the period studied. In particular, data shows a general trend for higher penalties that reached its maximum level in the mid-1980s. After this peak, statutory minimum sentences attained a plateau for cocaine and marihuana and continued to increase for heroin, but at a slower rate. For the three drugs studied here, the ratio of penalties for sale as compared to possession was relatively stable. Further, while sentences for cocaine and heroin were similar in 1975, by 2002, sentences for heroin were longer than those for cocaine. In the case of marijuana, statutory minimum sentences for possession doubled between 1975 and 2002, although in absolute numbers the sentences remain fairly short at four months. Given the general similarities in states' approaches to sentencing heroin and cocaine, we decided to use only data on sentences for cocaine in the statistical analyses developed. Cocaine-related offenses are likely more significant than heroin-related offenses in terms of number of offenses and number of offenders incarcerated. We decided not to include this data on sentences for marijuana in the analyses. The description that follows, thus, provides information for sale and possession of cocaine only.

Exhibit 6-1 shows the evolution of average statutory minimum sentences for first-offense possession and sale of 28 grams (approximately one ounce) of cocaine. It is important to note that the sentences described here do not necessarily reflect the actual sentences imposed by judges or the actual time served in these states; nor do these sentences represent mandatory minimum sentences for the offense. Rather, this approach simply provides a comparison of the overall drug sentencing structure and statutory minimum sentences available across states. For both offense types, statutory minimum sentences increased steadily between 1975 and 2002, although they plateau in the early 1990s. Statutory minimum sentences for possession moved from an average of 13 months in 1975 to 28 months in 2002 (a 115% increase). Minimum

sentences for sale moved from an average of 25 months to 41 months (a 64% increase). Since average values are vulnerable to outliers, we decided to examine median sentences as well. Using this approach we observed that the majority of states did not have statutory minimum sentences until the mid-1980s (i.e. many states had statutory sentences ranges in the form of, for example, 0 to 10 years). For possession offenses, the majority of states implemented a six-month statutory minimum sentence in 1981 and then a 12-month minimum in 1987. For sale offenses, the median in 2002 was about 30 months with a peak in 1990-93 of 36 months.

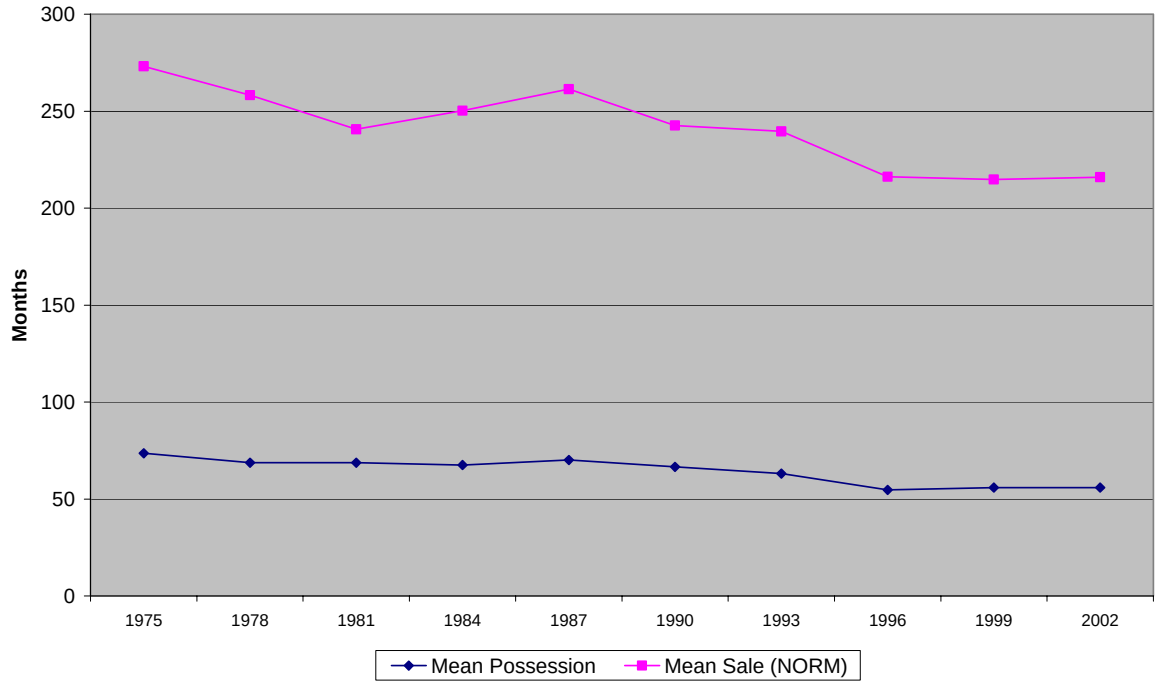
Exhibit 6-1 Average Statutory Minimum Sentences for Sale and Possession of 28g of Cocaine, 1975-2002



In terms of statutory maximum sentences for cocaine offenses, the picture is quite different. As Exhibit 6-2 shows, the average statutory maximum sentences for sale and possession of any small amount of cocaine (approximately 1 gram) have steadily decreased since 1975. The statutory maximum sentence for sale of cocaine decreased from 273 months in 1975 to 216 months in 2002. While not as sharp of a decrease, the statutory maximum sentence for possession of cocaine went from 73 months to 55 months during the same period. This reflects the general pattern of increased use of different quantity thresholds across the states; as states

created increased penalties for larger quantities of drugs, they decreased penalties for smaller quantities.

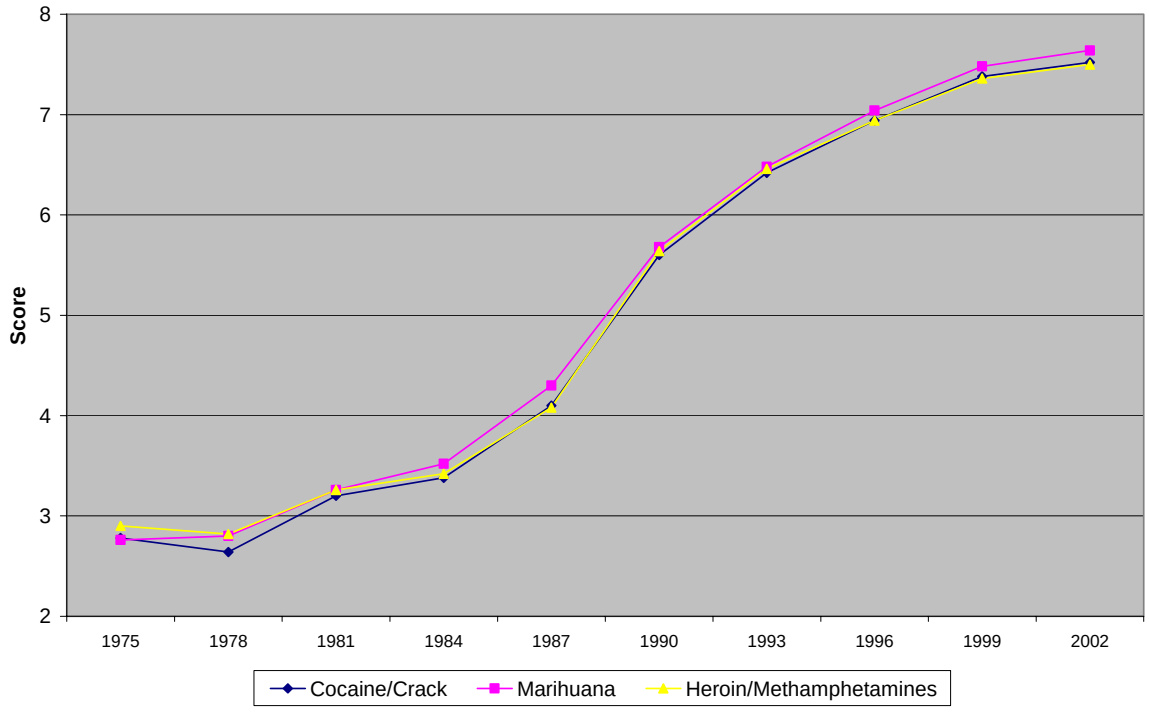
Exhibit 6-2 Average Statutory Maximum Sentences for Sale of 1g of Cocaine, 1975-2002



In order to confirm our preliminary finding of a general uniformity in the sentencing of drug possession and sale across states and over time, we decided to examine the evolution of sentencing enhancements for cocaine, heroin, and marihuana between 1975 and 2002 (Exhibit 6-3). Data on twelve different sentence enhancements was collected for each drug in each state. These enhancements represent factors that may increase a sentence for the underlying offense if found by the jury at trial or by the judge at sentencing. Among these specific considerations, we coded enhancements based on: location of the offense (selling/possessing drugs near a school, park, public housing complex, or church), excessive quantities of drugs involved, offenses involving minors, weapons use, and gang activity. In order to get a measure of "coverage" and "severity" we created a score for each state for each year by assigning a value of 1 to sale-related enhancements, a value of 2 to possession-related enhancements, and a value of 3 to those related to both sale and possession. A separate score was calculated for each of the three substances – cocaine, heroin, and marijuana.

Exhibit 6-3 displays the average scores across the states. As the exhibit indicates, in 1975, states had an average of just under three possession- or sale-related enhancements for each of the three substances; by 2002, states had an average of just under eight enhancements for each substance. What is interesting about Exhibit 6-3 is the fact that the three lines track each other almost perfectly regardless of the drug considered. In terms of median values, most states had a score of three in 1975 and seven in 2002. The uniformity of this data across drugs supports the preliminary finding described above on the common characteristics of drug policies. With the enhancements data we can also complement our earlier assessment of sentencing changes over time and confirm that sentences became harsher during the 1980s and, while they continued to increase, they increased at a slower rate.

Exhibit 6-3 Average Number of Sentence Enhancements for Sale or Possession of Cocaine, Heroin, or Marijuana, 1975-2002



There is enough evidence to suggest that in the mid-1980s there was a significant transformation of drug policies. This transformation impacted statutory minimum and maximum sentences as well as the structure of sentencing enhancements. In the mid-1980s, a significant number of states repealed the wide sentence ranges with low minimum sentences and high maximum sentences and replaced them with narrower sentence ranges with higher minimum

sentences and relatively low maximum sentences. For example, in Alabama, the maximum sentence for possession of 28 grams of cocaine decreased from 180 months in 1975 to 120 months in 2002; during the same period, the minimum sentence increased from 24 months to 36 months.

The transformation in penalties for drug offenses was also a phenomenon that encompassed all types of drugs (at least those analyzed in this study). There is a remarkable similarity in terms of the changes in sentences for cocaine and heroin. While this observation may be limited by the specificity of the offenses observed (sale or possession of 28 grams), it can still provide significant information about the general trends in drug policies. Unlike heroin or cocaine, the sentencing trends observed for marijuana follow a different trajectory: the peak in statutory minimum sentences seems to be reached earlier than in the other drugs studied, and its variation over time does not track the variation of cocaine or heroin. In the case of these drugs, changes over time are more pronounced and sustained for both possession and sale charges.

Sentences for Drug Offenses and Incarceration Rates: An Analysis Over Time

In order to assess the extent of the relationship between drug policies and incarceration rates we build on the cross-sectional time-series models already tested in previous chapters, adding additional policy variables in each subsequent chapter. In this chapter, we add several variables accounting for the state's approach to the sentencing of drug offenses. As noted above, given the similarities in states' approaches to sentencing heroin and cocaine, we decided to use only data on sentences for cocaine in the statistical analyses developed.

Table 6-1 presents the findings of the first two Fixed-Effects models tested. Model 1 includes two drug sentencing variables representing the statutory minimum sentences for sale and possession of 28 grams of cocaine. Model 2 expands the drug policy universe by including statutory minimum and maximum sentences for cocaine as well as the score for sentencing enhancements for this drug. In this model, we also included two variables measuring the number of quantity thresholds for possession and sale of cocaine. Our initial rationale was that increasing the number of severity levels for drug offenses, by creating greater distinctions in offenders based on quantity of drugs involved, may be related to decreased sentences for low-level offenses; since most offenders are likely convicted of these low-level offenses, such increased differentiation in penalties may lead to lower incarceration rates.

Table 6-1 Results for Fixed Effects Models with Variables for Sale and Possession of Cocaine

	Model 1	Model 2
Variable	b	b
Violent crime rate	0.066	0.045
Property crime rate	0.000	0.003
% population 18-24	5.767	6.409
% population 25-34	4.888	4.466
% population Black	5.982	4.468
% population Hispanic	7.403**	6.030**
% population in SMAs	-0.877	-0.418
% population religious fundamentalist	5.635	3.987
Income per capita	-0.008***	-0.004*
Unemployment rate	-0.247	0.595
Poverty rate	-3.528*	-2.603
Gini	87.564	14.762
Revenues per 100k population (*1000)	0.085**	0.057*
Welfare per 100k population (*1000)	-0.808**	-0.529*
FTE Police per 100k population	0.179**	0.147*
Drug arrest rate	454.439**	459.551**
Governor (Republican)	11.102*	7.501
Citizen political ideology	0.253	0.154
Determinate Sentencing	-2.589	-12.965
Presumptive Guidelines	4.702	26.971
Det * Presumptive Guidelines	-68.195*	-103.889***
Voluntary Guidelines	15.444	16.378
Det * Voluntary Guidelines	9.134	7.493
Time Served (all offenses)	0.178	0.198
Time Served (violent offenses)	19.016*	13.163
***Cocaine* enhancements**		3.977**
Cocaine possession severity lev.		14.718***
Cocaine sale severity lev.		-13.053***
***Cocaine* Possession Maximum**		-0.569***
***Cocaine* Sale Maximum**		0.016
***Cocaine* Possession Minimum**	0.375*	0.047
***Cocaine* Sale Minimum**	-0.374**	0.041
1978	19.188	15.271
1981	44.224**	31.226*
1984	73.508***	59.652***
1987	118.007***	101.615***
1990	161.656***	138.841***
1993	236.570***	197.402***
1996	304.600***	256.002***
1999	357.870***	303.126***
2002	367.918***	307.680***
Constant	-96.952	-122.973
R2 Within	0.856	.880
R2 Overall	0.71	.771
N	494	494

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

Models 1 and 2 Fixed-Effects are similar in terms of social and political variables. The percent Hispanic, the income per capita, and the state's revenues are significantly associated with variations in incarceration rates. States with higher income per capita have lower incarceration rates, as well as those states with higher welfare payments. Consistent with the models examined in previous chapters, drug arrests are positively related to higher incarceration rates. Overall, Models 1 and 2 explain about 86 percent of the variance in the dependent variable. The main differences between these two models arise from the all-inclusive specification of drug policies developed for Model 2. Basing our analysis in the evolution of cocaine sentences, we included in this regression measures of policy complexity (i.e. severity levels based on quantity) and severity (statutory minimums and maximums as well as sentence enhancement). Due to this specification, Model 2 has a slightly higher explanatory power and a different set of significant associations.

In terms of drug policies, sentence enhancements and severity levels become highly significant: states with a greater number of enhancements have higher incarcerations rates. In terms of severity levels, significant, opposite effects are found for possession and sale charges. The inclusion of this set of variables also decreased the effect size for other policy variables, such as the Time Served (violent offenses), and, perhaps more importantly, the variables measuring the minimum sentence for cocaine possession and the Governor's party affiliation. The opposite trend can be observed for the interaction term for determinate sentencing and presumptive guidelines: this variable becomes highly significant ($p<.001$) in Model 2 and increases in magnitude once we develop an exhaustive specification of the drug policies for cocaine.

Despite the benefits of having the severity variables, we decided to drop them from the analysis. Three reasons sustain this approach: first, the heightened significance reached by the severity variables may be eclipsing the significance of other drug policy variables. Second, Model 2 drops theoretically important variables. Third, and more importantly, severity is a very indefinite measure. While it seems to be that having more severity levels is associated with stiffer penalties (at least for cocaine offenses) the effects are very different by state and by type of offense.

In Table 6-2 we present what we think are the most comprehensive and theoretically sound models of drug policies. In the first set of columns we present Fixed-Effects results and in the second set we present Random-Effects coefficients. The Breush Pagan test suggests the use of a Random-Effects formulation of the model rather than an OLS regression ($Chi2\ (1)=162.78$, $p<.001$). When comparing the Fixed- and Random-Effects coefficients via a Hausman specification test, we fail to find additional support for the Random-Effects approach, rejecting the null hypothesis ($p<.05$) that differences between these two models were systematic ($Chi2(30)=50.15$, $p=.012$. Given the potential differences between the two estimation models and the fact that the state dummies are significant in the Fixed-Effects model ($F(49,405)=7.52$, $p<.001$), we report below the final estimates for both models. Additional specifications to the estimation models are presented in the statistical appendix (Appendix C).

Table 6-2 Results for Final Fixed- and Random-Effects Models with Variables for Sale and Possession of Cocaine

	Fixed Effects		Random Effects	
Variable	b	SE	b	SE
Violent crime rate	0.031	0.037	0.049	0.032
Property crime rate	0.001	0.007	0.004	0.005
% population 18-24	7.283	4.883	2.952	4.404
% population 25-34	4.535	2.808	4.807	2.676
% population Black	5.171	4.422	3.766**	1.143
% population Hispanic	5.546*	2.227	2.958**	1.090
% population in SMAs	-0.334	0.653	0.088	0.367
% population religious fundamentalist	4.097	3.278	3.273**	1.089
Income per capita	-0.007**	0.002	-0.005*	0.002
Unemployment rate	0.190	2.162	1.001	2.047
Poverty rate	-2.755	1.511	-3.534*	1.481
Gini	159.223	319.128	388.858	290.096
Revenues per 100k population (*1000)	0.066*	0.026	0.071**	0.023
Welfare per 100k population (*1000)	-0.427	0.250	-0.774**	0.227
FTE Police per 100k population	0.148*	0.064	0.128*	0.061
Drug arrest rate	470.771**	160.375	411.979**	152.312
Governor (Republican)	11.070*	5.089	11.092*	5.050
Citizen political ideology	0.218	0.352	-0.093	0.311
Determinate Sentencing	-10.282	12.739	-12.102	11.554
Presumptive Guidelines	13.601	19.912	0.482	18.781
Det * Presumptive Guidelines	-88.608**	28.006	-70.134**	25.568
Voluntary Guidelines	12.967	13.806	9.227	12.847
Det * Voluntary Guidelines	8.398	25.032	15.563	23.085
Time Served (all offenses)	0.293	0.151	0.208	0.134
Time Served (violent offenses)	16.649*	7.636	17.619*	7.439
***Cocaine* enhancements**	4.604**	1.362	4.527***	1.262
***Cocaine* Possession Maximum**	-0.581***	0.110	-0.445***	0.092
***Cocaine* Sale Maximum**	0.007	0.023	0.004	0.018
***Cocaine* Possession Minimum**	0.335*	0.154	0.353*	0.141
***Cocaine* Sale Minimum**	-0.199	0.138	-0.235	0.129
1978	15.680	10.998	16.012	10.815
1981	35.074*	14.746	29.412*	13.699
1984	62.733***	17.044	59.217***	15.790
1987	102.124***	20.254	84.078***	18.214
1990	136.710***	24.285	115.328***	20.633
1993	198.490***	28.469	181.876***	23.214
1996	256.315***	32.586	238.781***	26.333
1999	303.463***	35.788	283.193***	29.178
2002	309.186***	38.691	296.246***	30.988
Constant	-149.860	140.353	-193.686	122.862
R2 Within	.873		.869	
R2 Overall	.751		.812	
N	494		494	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

In the Fixed- and Random-Effects models, the sentencing enhancements variable for cocaine offenses continues to be significantly associated with higher incarceration rates. This means that controlling for all the other policy and non-policy variables, states with more sentencing enhancements for drug offenses have higher incarceration rates. In both models, neither minimum nor maximum statutory sentences for cocaine sale are significantly associated with incarceration rates. However, both minimum and maximum statutory sentences for cocaine possession are significantly associated with incarceration rates. States with higher statutory minimum sentences for cocaine possession have higher incarceration rates than other states. While non-significant, the coefficient for statutory minimum sentences for cocaine sale suggests an opposite effect. Conversely, states with higher statutory maximum sentences for cocaine possession have lower incarceration rates than other states; again, while this seems odd, this may be due to the narrowing of sentence ranges since the 1980s as states created additional penalty ranges based on quantity for cocaine sale.

The interaction between determinate sentencing and presumptive sentencing guidelines remains significant ($p<.05$) in both the Fixed- and Random-Effects models. Determinate sentencing or sentencing guidelines, if measured separately, and the interaction between determinate sentencing and voluntary sentencing guidelines remain not significantly associated with variations in incarceration rates despite the expected sign in the direction of the effects (Random-Effects model).

The social and political predictors do not change significantly when compared to the results presented in Table 6-1. However, the Fixed-Effects model does not capture small variations in the covariates used to explain the outcome variable. In Table 6-2 for instance, variations in the states' black population, the percent of religious fundamentalism, the poverty rate, and welfare per capita are all significant only in the Random-Effects model ($p<.01$). Overall, Fixed-Effects estimators are consistent and unbiased, but given their misspecification of standard errors, they may generate type I errors. In this case, we observe that the size of the coefficients is substantially greater for Fixed-Effects than for Random-Effects.

Conclusion

Drug offenders now account for 20 percent of state prison populations in the United States, up from just 6 percent of prison populations in 1980. Our findings suggest that much of this increase is due to increased law enforcement practices (see also Blumstein and Beck, 1999). Our findings have consistently shown that states with higher proportions of drug arrests and a greater capacity for law enforcement have higher incarceration rates; indeed, the emphasis of much of the war on drugs was the increased enforcement of existing drug laws. This is true even after controlling for sentences for drug offenses. States did increase penalties for drug offenses over the last 30 years, but the increases in arrests and the investment in police continued to affect prison populations.

Nonetheless, the substantive changes to drug laws that states enacted to control drug offending have affected incarceration rates. However, sentences for possession offenses have a greater impact than sentences for sale offenses. In fact, differences in minimum or maximum sentences for sale of cocaine do not account for differences in incarceration rates across states. Thus, states with higher minimum or maximum sentences for sale of cocaine do not have higher incarceration rates than other states. Rather, states with higher minimum sentences for cocaine possession have higher incarceration rates than other states. This may be because many drug offenders are entering prison for low-level drug offenses – either possession of small amounts of drugs or sale offenses pled down to possession. In states with higher statutory minimum sentences for possession, a large number of drug offenders may then be sentenced to longer prison terms than in other states. Our findings concerning statutory maximum sentences for cocaine possession are less clear – states with higher statutory maximum sentences for cocaine possession have lower incarceration rates than other states.

The availability of sentence enhancements also contributes to incarceration rates. States with more sentence enhancements available for drug offenses have higher incarceration rates than other states. Thus, while higher statutory maximum sentences for sale of cocaine may not lead to higher incarceration rates, offenders in states with more enhancements may, nonetheless, be receiving longer imposed sentences than offenders in other states based on factors, such as proximity to a school, sale to a minor, or offenses committed with a weapon. These factors may carry mandatory terms of incarceration or mandatory minimum terms, which could lead to higher incarceration rates through increased admissions to prison or through longer imposed terms.

While drug offenses have constituted much of the growth in prison populations since the 1970s, substantive changes in drug laws do not account for all of the growth. After controlling for changes in drug laws, other policies continue to impact incarceration rates. The combination of presumptive sentencing guidelines and determinate sentencing is associated with lower incarceration rates. Separate time served requirements for violent offenders are also associated with higher incarceration rates. Thus, many policies have impacted incarceration in complimentary and contradictory ways to changes in policies concerning drug offenses.

As in previous chapters, racial and economic differences and conservative politics and citizenry (through fundamentalist religious views) all continue to explain differences in state incarceration rates. The persistence of these forces after controlling for several policy differences in the states is remarkable. As the following chapters show, their stability remains throughout most of our analyses.

Chapter Seven: Habitual Offender Laws

Drug offenses were just one substantive area targeted by policy changes over the last 30 years. States focused reforms on "habitual offenders" as well, increasing penalties and creating greater controls over release decisions and time served requirements for offenders with prior felony convictions. While many states had some form of habitual offender law in place in 1975, many states sought to create stricter laws or created more specific laws targeted at sub-groups of offenders – habitual violent offenders, habitual drug offenders, or habitual sex offenders. In the 1990s, at least 24 states adopted specific "three strikes" laws, patterned after those passed in Washington and California, which imposed substantially higher penalties for repeat violent offenses. Again, while these policies, like those increasing penalties for drug offenses, were targeted at a specific group of offenders, they may, nonetheless, have a significant impact on a state's prison population if their use and enforcement become a priority in the state. Given the high recidivism rates reported among offenders released from prison – 47 percent of offenders reconvicted for a new offense and 25 percent re-incarcerated – the impact of habitual offender laws may be quite significant (Langan and Levin, 2002)

While the increased penalties available for habitual offenders – particularly, so-called "three strikes" laws – have received a great deal of attention, no studies of which we are aware have attempted to examine the impact of habitual offenders laws on incarceration rates across states. Several analysts have considered the impact of "three strikes" laws on incarceration rates in particular states (Austin, 1999; Zimring, Hawkins, and Kamin, 2003) or have provided descriptive accounts of the number of persons incarcerated under such laws across states (Schiraldi, Colburn, and Lotke, 2004); however, these analyses are focused exclusively on habitual offender laws adopted after 1994, specifically under the rubric of "three strikes." As such, researchers have not considered the impact of habitual offender laws on state incarceration rates over much of the last 30 years.

This chapter presents an analysis of the impact of general habitual offender laws on incarceration rates. It begins by describing habitual offender laws in the United States, focusing on a few characteristics that distinguish different approaches to habitual offenders across the states. It then presents our analyses, which look at the impact of these laws and considers the interactive impact of determinate sentencing, structured sentencing, time served requirements, and drug sentences on state-level incarceration rates.

Habitual Offender Laws in the States

Habitual offender laws existed in many states prior to the renewed interest in habitual offenders and "three strikes" in the 1990s (Clark, Austin, and Henry, 1997). Unlike "repeat offender laws," which may be directed at offenders with prior convictions for the same or similar offense (e.g. increased penalties for repeat theft), habitual offender laws are generally broad in their scope, targeted at offenders with prior convictions for any felony offense. Under such laws, offenders

convicted of a felony are generally eligible for an increased sentence if they have a prior conviction for a felony or number of felonies. At the most basic level, these laws are triggered by any type of current felony and by the presence of any type of prior felonies.

The difficulty in describing habitual laws comes from the heterogeneity of such laws across states. For example, states show wide variation in the numbers and types of prior and current offenses that trigger the law. Some habitual offender laws are triggered when an offender has one, two, or three or more prior offenses. For example, in North Dakota, an offender convicted of any felony if previously convicted of one prior felony is subject to the state's habitual offender law; in contrast, under Georgia's general habitual offender law, an offender is subject to increased penalties only upon a fourth felony conviction. In addition, many habitual offender laws adopted under the "three strikes" label, such as Pennsylvania's, are triggered only when an offender's current offense and a prior offense are for violent felonies; other states, such as California, require that only the prior offense be a violent offense, and apply the law to any current offense. In contrast, many traditional habitual offender laws, like that in Montana, are triggered by any type of current or prior felony.

States also differ on the types of prior adjudications that trigger the law. In most states, the habitual offender law applies when an offender has any prior felony *convictions*; in other states, such as Iowa and Maryland, the law is triggered only when an offender has served a *prior term of incarceration*. States also vary in the time limits placed on when prior adjudications must have occurred. In many states, prior offenses must have occurred within a certain number of years of the current offense. For example, in Colorado, an offender convicted of any felony may be sentenced under the habitual offender law if previously convicted of any two felonies within 10 years of the current offense. In other states, there is no time limit on when prior adjudications must have occurred. For example, in Delaware, an offender convicted of any felony if previously convicted at any time of any two felonies may be sentenced under the state's habitual offender law.

Finally, states vary in the actual sentences available under the laws. For example, under New Jersey's general habitual offender statute, an offender's sentence may be increased by one felony class if previously convicted of two felonies. In contrast, under Vermont's habitual offender statute, an offender convicted of any felony if previously convicted of any three felonies is eligible for a sentence of up to life imprisonment.

Beginning in 1994, many states adopted habitual offender laws under the label of "three strikes." Three strikes laws generally call for longer sentences than prior habitual offender laws and often apply only to serious or violent offenses. Again, states vary in terms of the number and type of felony convictions necessary to trigger the laws and the sentences ultimately imposed under the laws (Clark, Austin, and Henry, 1997). For example, California's "three strikes" law is triggered when an offender is convicted of any felony if previously convicted of a "violent" felony; the law then requires the imposition of a mandatory life sentence without the possibility of parole for 25 years. In contrast, Pennsylvania's "three strikes" law is triggered only when an

offender is convicted of one of eight specified offenses if previously convicted of one of the same eight offenses; the law then gives the sentencing court discretion to increase the sentence for the underlying offense by up to twenty-five years. Between 1994 and 1996, twenty-four states adopted "three strikes" laws aimed at imposing substantially more severe mandatory prison sentences for repeat offenders.[71] However, all 24 of these states already had some form of habitual offender law in place prior to the adoption of a "three strikes" provision.

This diversity in provisions across states makes comparisons of policies and statistical analysis of the habitual offender laws quite difficult. The mixture of number of prior offenses, types of prior offense, types of prior adjudications, time limits on prior adjudications, and sentences available under the laws make comparisons across states nearly impossible. In order to provide an accurate, although non-comprehensive assessment of habitual offender laws, we decided to focus our examination on general habitual offender laws – those broadly applicable to any type of current or prior adjudication (i.e. not restricted to violent offenses) – that are triggered by one or two prior adjudications. In other words, we focused on habitual offender laws that applied to: 1) second-time offenders, or offenders convicted of any felony if previously convicted of any felony and 2) third-time offenders, or offenders convicted of any felony if previously convicted of any two felonies.[72]

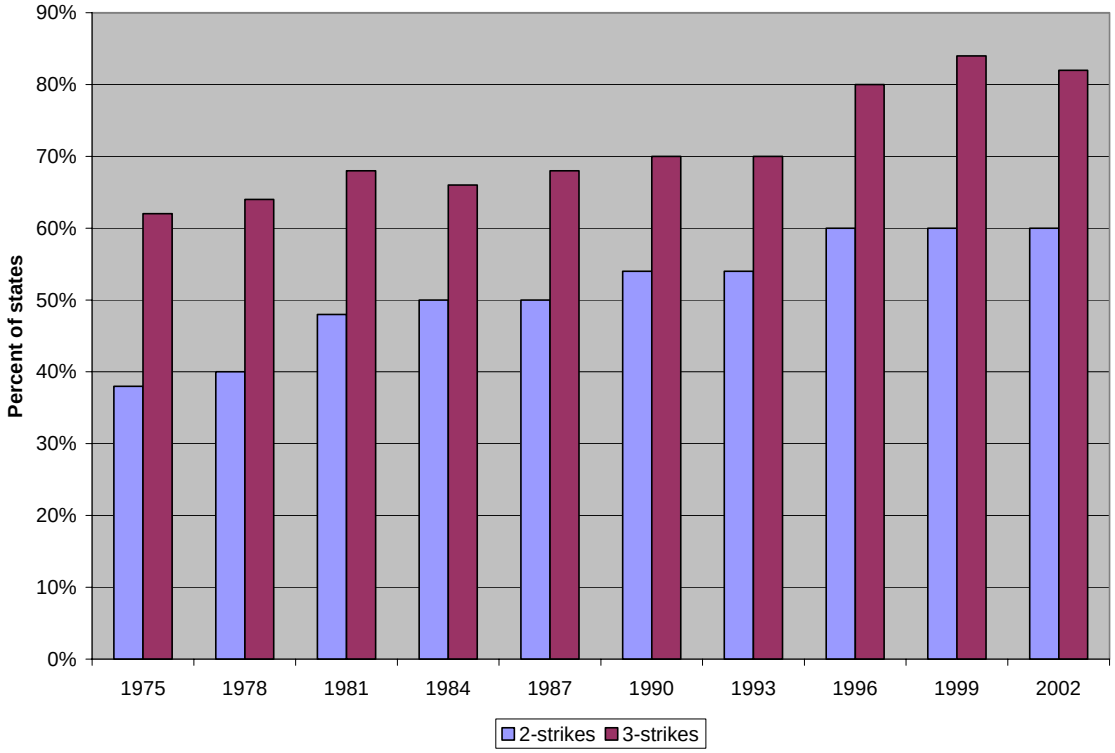
Exhibit 7-1 presents the evolution in the number of states with second-time and third-time offender provisions, showing an upward trend for both policies between 1975 and 2002. In 1975, 19 states had second-time offender laws; by 2002, 30 states had such laws. Similarly, by 2002, 41 states had third-time offender laws, up from 30 in 1975.

According to our data, habitual offender laws are seldom repealed once enacted (there are some important exceptions such as Kansas or Utah). Taking this into account, it may be the case that the impact of habitual offender laws on incarceration rates may be more cumulative and over the long run than the immediate result of enactment of a particular piece of legislation. Clearly, the association of habitual offender laws with imprisonment levels has to do also with the specificity of the enacted laws (coverage, severity, mandatory incarceration). While our database on sentencing policies provides this level of detail about the features of the habitual offender laws passed by the states between 1975 and 2002, we decided to limit our analysis here to an overview of the association between habitual offender laws and incarceration rates over time.

[71] These states include: Arkansas, California, Colorado, Connecticut, Florida, Georgia, Indiana, Kansas, Louisiana, Maryland, Montana, Nevada, New Jersey, New Mexico, North Carolina, North Dakota, Pennsylvania, South Carolina, Tennessee, Utah, Vermont, Virginia, Washington, and Wisconsin.

[72] This analysis did not include "repeat offender" statutes that focus on offenders convicted of repeat convictions of a small set of offenses.

Exhibit 7-1 Percent of States with Habitual Offender Laws



Consistent with this perspective we included in our analysis several offense-specific classifications for second-time and third-time offender laws. For instance, we coded habitual offender laws specifically addressing violent offenses, drug offenses, and sex offenses (regardless of the classification of these statutes as second-time or third-time offender laws). This approach also allowed us to account for multiple statutes addressing the same number of generic offenses. We often found states had general habitual offender laws (covering a wide array of felonies for both previous and current offenses) in place since 1975, which were later complemented with targeted statutes dealing with specific offenses. In some cases, the habitual offender laws in place were already restricted to a particular set of offenses (for example, the second-time offender law in Illinois). The general approach to the habitual offender laws offered in this report does not account for these specific features of the enacted laws.

Table 7-1 lists the adoption dates for habitual offender laws considering second-time and third-time offenders. In addition, it presents a list of the particular habitual offender laws that are particularly targeted for drug offenses or violent crimes. According to the information collected by this project only a handful of states had no specific provision dealing with habitual offenders (Alaska, Ohio and Vermont).

Table 7-1 Habitual Offender Laws

State	Date of Adoption			
	Second-Time Offender	Third-Time Offender	Violent Offender	Drug Offender
Alabama	1975-2002	1975-2002		
Alaska				
Arizona	1975-2002	1975-2002	1984-2002	1996-2002
Arkansas	1978-2002	1996-2002	1996-2002	
California	1981-2002		1996-2002	
Colorado		1975-2002	1993-2002	
Connecticut	1981-2002			
Delaware		1975-2002	1975-2002	1987-2002
Florida	1975-2002	1999-2002	1990-2002	
Georgia	1975-2002		1996-2002	
Hawaii	1981-2002	1975-2002	1981-2002	1981-2002
Idaho		1975-2002		
Illinois	1975-2002	1975-2002	1975-2002	
Indiana		1978-2002		1984-2002
Iowa	1990-2002	1978-2002	1990-2002	
Kansas	1975-1990	1975-1990		
Kentucky	1978-2002	1975-2002		
Louisiana	1975-2002	1975-2002	1996-2002	
Maine		1999-2002	1999-2002	
Maryland	1996-2002	1975-2002	1978-2002	
Massachusetts		1975-2002		
Michigan	1975-2002	1975-2002		
Minnesota	1975	1990-2002	1990-2002	
Mississippi		1975-2002		
Missouri	1975-2002	1975-2002	1981-2002	
Montana	1975-2002	1981-2002	1999-2002	
Nebraska		1975-2002	1996-2002	
Nevada		1987-2002		
New Hampshire		1975-2002		
New Jersey	1981-2002	1975-2002	1981-2002	
New Mexico	1975-2002	1975-2002	1996-2002	
New York	1975-2002	1975-2002	1978-2002	
North Carolina		1996-2002	1996-2002	
North Dakota		1975-2002		
Ohio				
Oklahoma	1975-2002	1978-2002		
Oregon	1990-2002			
Pennsylvania	1984-2002	1996-2002	1984-2002	
Rhode Island		1975-2002		
South Carolina	1996-2002	1975-2002	1996-2002	
South Dakota	1975-2002	1978-2002		
Tennessee		1975-1990; 1996-2002	1975-2002	
Texas	1975-2002	1975-2002		
Utah	1984-2002	1975-1993		
Vermont				
Virginia	1996-2002	1996-2002	1996-2002	
Washington	1975-1981; 1993-2002	1975-1981; 1993-2002	1993-2002	
West Virginia	1975-2002	1975-2002		
Wisconsin	1975-2002	1993-2002	1993-2002	
Wyoming		1975-2002		

Habitual Offender Laws and Incarceration Rates: An Analysis Over Time

Habitual offender laws, designed specifically to increase incarceration and sentence lengths for repeat offenders, have been predicted to increase incarceration rates through increased admissions to prison and through longer lengths of time served by offenders. Such laws have also been argued to increase the incidence of plea bargaining for many additional offenders; as a result, such laws may increase incarceration rates in a second way by increasing admissions for offenders who plead down to avoid the most severe sentence possible under the habitual offender law.

Few studies have assessed the impact of such laws on admissions or incarceration rates across states. Those studies that do exist have considered only the recent proliferation of "three strikes laws," similar to the oft-cited California provision, on individual state prison populations. These studies show that such laws increase incarceration for violent offenses only slightly (Turner et al., 1999), perhaps because of their infrequent use in most states (Schultz, 2000). Indeed, of the 24 states adopting a "three strikes" law in the 1990s, 14 states each had fewer than 100 people incarcerated under their provisions;[73] only California, Florida, and Georgia had more than 400 people incarcerated under a three strikes law (Schiraldi, Colburn, and Lotke, 2004).[74] As Austin (1999) notes, for most states the enactment of three strikes laws was "much ado about nothing." This is likely because such laws were not new to most states. All of the 24 states adopting a three strikes law in the 1990s already had some form of habitual offender law; the addition of a specific three strikes law changed most states criminal codes very little. Again, the only apparent exceptions are California, Florida, and Georgia.

We tested several models in order to account for the relationship between habitual offender laws and incarceration rates. Following the procedures presented in previous chapters, we added a series of general policy variables specifically related to habitual offender statutes to the baseline model. We focused on the statutes that apply to offenders with one prior offense and with two prior offenses. As noted above, habitual offender laws are very heterogeneous across states; states show wide variation in terms of the definitions of prior and current offenses that trigger the law, the types of prior adjudications that trigger the law (prior convictions versus prior terms of incarceration), time limits on when prior adjudications must have occurred, and the ultimate penalties available at sentencing. After several attempts to build a habitual offender "score" for each state's policy, the analyses, instead, relied on a dichotomous coding procedure by which we classified the presence or absence of a particular piece of legislation dealing with the sentencing of habitual offenders. Each state was coded "1" if it had a habitual offender law that increased penalties for offenders with one previous conviction and a "0" if it had no such provision (Second-time offender); similarly, each state was coded "1" if it had a habitual

[73] These states included: Arkansas, Colorado, Connecticut, Indiana, Montana, New Jersey, New Mexico, North Carolina, North Dakota, Pennsylvania, South Carolina, Tennessee Vermont and Wisconsin.

[74] According to the Justice Policy Institute report, Florida had 1,628 persons incarcerated under the law and Georgia had 7,631 persons incarcerated. California had 42,322 persons incarcerated under either the 2-strike or 3-provisions.

offender law that increased penalties for offenders with two previous convictions and a "0" if it had no such provision (Third-time offender).[75] We used a similar dichotomous coding scheme for the presence and absence of habitual offender laws directed specifically at habitual violent offenders (Violent offenses HOL) and habitual drug offenders (Drug offenses HOL).

Table 7-2 presents a summary of the results of our first set of pooled cross-sectional regressions using Fixed-Effects estimators. Models 1 and 2 account for the independent relationship between the enactment of second-time and third-time offender laws and variations in incarceration rates. Model 3 is an attempt to provide an offense-specific account of habitual offender laws, instead of following the general prior adjudications approach. For this model we employ the coding scheme based on the targeting of drug offenses or violent offenses.

[75] We have consciously avoided the use of the terms "2-strikes" or "3-strikes" to describe these laws. The strikes metaphor is too closely linked to the "three strikes and you're out" habitual offender laws passed by many states in the mis-1990s; these "strikes" laws were primarily directed at violent offenders. The habitual offender laws described in our analyses may be more general, applicable to all types of habitual offenders (not just those convicted of violent offenses).

Table 7-2 Results for Fixed Effects Models with Habitual Offender Laws

	Model 1	Model 2	Model 3
	b	b	B
Violent crime rate	0.040	0.045	0.030
Property crime rate	0.000	0.000	0.001
% population 18-24	6.914	8.902	7.250
% population 25-34	4.385	4.146	4.663
% population Black	5.378	6.620	5.271
% population Hispanic	5.544*	5.017*	5.548*
% population in SMAs	-0.281	-0.206	-0.315
% population religious fundamentalist	3.660	3.256	4.324
Income per capita	-0.007**	-0.007**	-0.007**
Unemployment rate	0.629	0.985	0.062
Poverty rate	-2.857	-3.003*	-2.669
Gini	193.883	187.392	138.879
Revenues per 100k population (*1000)	0.067*	0.065*	0.066*
Welfare per 100k population (*1000)	-0.474	-0.480	-0.420
FTE Police per 100k population	0.146*	0.148*	0.148*
Drug arrest rate	493.494**	522.941**	472.432**
Governor (Republican)	12.298*	12.262*	10.852*
Citizen political ideology	0.221	0.087	0.199
Determinate Sentencing	-2.667	-2.787	-9.693
Presumptive Guidelines	13.427	12.965	12.978
Det * Presumptive Guidelines	-92.179**	-89.886**	-89.556**
Voluntary Guidelines	12.882	10.528	12.200
Det * Voluntary Guidelines	2.746	-1.582	9.155
Time Served (all offenses)	0.216	0.209	0.293
Time Served (violent offenses)	16.083*	14.850	16.328*
Cocaine enhancements	4.572**	4.777***	4.661**
Cocaine Possession Maximum	-0.581***	-0.612***	-0.588***
Cocaine Sale Maximum	0.009	0.007	0.007
Cocaine Possession Minimum	0.343*	0.371*	0.344*
Cocaine Sale Minimum	-0.208	-0.207	-0.197
Second-time offender	-2.753		
Third-time offender		20.436*	
Drug offenses HOL			-7.409
Violent offenses HOL			4.427
1978	18.440	17.174	15.439
1981	38.062*	35.386*	34.669*
1984	64.427***	63.682***	62.232***
1987	103.637***	105.960***	102.223***
1990	138.817***	142.304***	136.412***
1993	199.868***	203.854***	198.189***
1996	258.500***	259.632***	255.139***
1999	304.849***	306.047***	302.812***
2002	310.711***	312.308***	308.620***
Constant	-151.717	-186.441	-146.016
R2 Within	.876	.875	.873
R2 Overall	.754	.742	.748
N	492	492	494

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

In terms of the independent assessment of second-time offender legislation, the results of Model 1 were unexpected: according to our regression coefficients, the association between habitual offender laws directed at offenders with 1 prior adjudication and incarceration rates is negative, yet non-significant, indicating that states with second-time habitual offenders laws have lower incarceration rates than other states. Despite the direction of the coefficient, its relative size is small when compared to effects of other sentencing policies. In terms of the control variables included in the model, we found additional support for the significant associations of several socio-demographic variables (such as percent Hispanic and income per capita) with variations in state incarceration rates. In terms of the additional policy variables, we found that controlling for such habitual offender legislation does not affect the patterns observed for any other policy variables. The consistency in the direction and significance of other control variables and policy variables has been already noted throughout this report.

In Model 2 we tested the same baseline model using only a third-time offender law. While the overall fit indicators of the model did not change (Within R^2=.87), this new policy variable was significantly related to higher incarceration rates ($p<.05$). In other words, controlling for all the variables in the model we found that states with habitual offender laws directed at offenders with two prior adjudications have consistently higher incarceration rates that states that do not have this particular policy. While this finding did not alter the overall significance patterns of most other variables in the model, it did make the poverty rate variable significant and the variable measuring time served requirements for violent offenders non-significant.

In Model 3 we tested an alternative version of habitual offender laws using an offense-specific classification scheme. We found that neither the drug offender habitual offender law nor the violent offender habitual offender law was significantly related to variations in incarceration rates according to the specification of the models. In fact, the coefficients for these two types of legislation were in opposite directions.

Table 7-3 presents both Fixed- and Random-Effects estimators for a model capturing both second-time and third-time offender laws. We again conducted a series of tests to assess the advantages and disadvantages of each estimation procedure we conducted a series of initial tests. The Breush Pagan test suggests the use of a Random-Effects formulation of the model rather than an OLS regression (Chi2 (1)=143.17, $p<.001$). When comparing the Fixed- and Random-Effects coefficients via a Hausman specification test, we find additional support for the Random-Effects approach, failing to reject the null hypothesis that differences between these two models were systematic (Chi2(33)=30.23, $p=.60$. The state dummies are significant in the Fixed-Effects model ($F(49,401)=7.57$, $p<.001$). Additional specifications to the estimation models are presented in the statistical appendix (Appendix C). While Random-Effects estimates were again found to be more efficient and robust than the Fixed-Effects estimates, the latter estimation procedure may add information about the models not provided by the Random-Effects. Thus, we report in Table 7-4 both Random- and Fixed-Effects models.

Table 7-3 Final Models for Fixed and Random Effects with Habitual Offender Laws

	Fixed Effects		Random Effects	
	B	SE	b	SE
Violent crime rate	0.045	0.037	0.054	0.033
Property crime rate	0.000	0.007	0.003	0.005
% population 18-24	8.751	4.933	3.713	4.447
% population 25-34	3.943	2.818	4.684	2.721
% population Black	6.559	4.430	3.726**	1.186
% population Hispanic	5.151*	2.235	2.787*	1.134
% population in SMAs	-0.239	0.656	0.082	0.376
% population religious fundamentalist	3.279	3.274	3.172**	1.125
Income per capita	-0.007**	0.002	-0.005**	0.002
Unemployment rate	0.987	2.172	1.361	2.065
Poverty rate	-3.001*	1.506	-3.684*	1.483
Gini	195.552	318.715	404.356	294.200
Revenues per 100k population (*1000)	0.065*	0.026	0.072**	0.024
Welfare per 100k population (*1000)	-0.487	0.251	-0.787**	0.229
FTE Police per 100k population	0.149*	0.063	0.126*	0.061
Drug arrest rate	524.940**	160.687	447.096**	153.366
Governor (Republican)	12.144*	5.106	11.979*	5.073
Citizen political ideology	0.090	0.354	-0.126	0.316
Determinate Sentencing	-2.215	13.294	-6.842	12.003
Presumptive Guidelines	14.180	19.902	1.094	18.902
Det * Presumptive Guidelines	-91.308**	27.994	-72.350**	25.723
Voluntary Guidelines	11.306	13.815	8.655	12.989
Det * Voluntary Guidelines	-1.396	25.153	9.496	23.282
Time Served (all offenses)	0.195	0.158	0.164	0.140
Time Served (violent offenses)	15.039*	7.628	16.662*	7.452
Cocaine enhancements	4.811***	1.359	4.598***	1.271
Cocaine Possession Maximum	-0.597***	0.113	-0.463***	0.095
Cocaine Sale Maximum	0.006	0.023	0.002	0.019
Cocaine Possession Minimum	0.372*	0.154	0.371**	0.142
Cocaine Sale Minimum	-0.203	0.138	-0.241	0.130
Second-time offender	-5.855	10.093	0.024	9.046
Third-time offender	21.139*	9.133	9.300	8.228
1978	17.547	11.045	17.598	10.925
1981	36.161*	14.812	30.958*	13.944
1984	64.579***	17.022	60.681***	15.930
1987	106.315***	20.185	86.981***	18.273
1990	142.490***	24.212	119.063***	20.751
1993	203.633***	28.365	185.718***	23.478
1996	259.503***	32.427	241.576***	26.689
1999	305.170***	35.652	285.600***	29.709
2002	311.042***	38.583	298.477***	31.645
Constant	-183.635	140.480	-205.170	123.769
R2 Within	0.875		.871	
R2 Overall	0.741		.805	
N	492		492	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

According to the results on the Fixed-Effects model, the effect of third-time offender laws is consistently related to higher incarceration rates ($p<.05$). In contrast, the effect of second-time offender laws is negative and non-significant. While this assessment holds for the Fixed-Effects models, in the Random-Effects model neither of the habitual offender laws is significant (although both have a positive sign). The discrepancy between the two estimation procedures explored is important because it may be related to the likelihood of type I errors (i.e. accepting as significant an otherwise not statistically significant relationship). Given that this type of warning should be mentioned before interpreting results in the Fixed-Effects model, we will take the results in the Random-Effects model as the best fit to the data. In this sense, we will conclude that both habitual offender laws have a positive association with incarceration rates, but this association is statistically indistinct from zero (this is, not significant) when controlling for other factors.

In terms of the drug policy variables, both models are consistent with the observations made in Chapter Six: more sentence enhancements and higher statutory minimums for cocaine possession are associated with higher incarceration rates, although this is not the case for statutory maximum sentences for possession. Separate time served requirements for violent offenders continue to be positively associated with higher incarceration rates ($p<.05$). In terms of sentencing structure, we continue to find support for the effects of the interaction between determinate sentencing and presumptive guidelines. Again, the independent effects of either of these policies on incarceration rates remained non-significant, indicating that it is only the combination of the two that impacts incarceration rates.

In terms of social, economic, and political variables, the models continue to provide support for the significant association of incarceration rates with the presence of minorities in a state's population, the level of welfare payments and revenues, and income per capita. For these variables, the Random-Effects model provides more consistent estimators that should be employed for the analysis, in order to provide a conservative account of the statistical analysis developed.

Conclusion

States have traditionally singled out habitual offenders for increased penalties. While many states sought to create even stiffer penalties for habitual violent offenders in the 1990s through the adoption of "three strikes" laws, most states already had some mechanism on the books for sentencing such offenders. Few studies have considered the impact of such laws on incarceration rates across the states. Our findings suggest that the mere presence of habitual offender laws is not associated with higher incarceration rates. However, the practical use of the laws likely matters more than their availability to prosecutors or judges. Indeed, while many states created "three strikes" laws patterned after those adopted in Washington and California, only three states – California, Florida, and Georgia – have made extensive use of such policies.

Rather, other policies continue to exert stronger influences on incarceration rates. The inclusion of controls for habitual offender laws does not change the significance of any other policies considered, except determinate sentencing. Nor does this inclusion of habitual offender laws change the significance of the non-policy variables considered. This suggests that the substantive focus on habitual offenders over the last 30 years was largely symbolic (Austin, 1999), in contrast to the substantive focus on drug offenders.

Chapter Eight: Mandatory Sentencing

Sentencing courts generally have discretion to control both the disposition and duration of the sentence imposed for a particular offense. The judge usually has discretion to decide whether an offender will go to prison and, if so, for how long. As Chapter Four noted, this discretion may be "guided" by structured sentencing, but it is not entirely abolished under such systems. In contrast, mandatory sentencing laws constrain both forms of discretion, requiring the court to impose a term of incarceration or requiring the court to impose a prison term of a certain length. Between 1975 and 2002, every state adopted some form of mandatory sentencing law. The variation in these laws is dramatic, from the types of offenses targeted to the lengths of sentences mandated to the impact the laws have on judicial discretion and release from prison.

While several critics maintain that mandatory sentencing laws have contributed to rapidly growing prison populations (see e.g. Beckett and Sasson, 2000), no studies of which we are aware have attempted to examine the impact of mandatory sentencing laws on incarceration rates across states. In fact, few empirical studies have examined the impact of mandatory sentencing laws on incarceration rates in individual states.

This chapter presents an analysis of the impact of mandatory sentencing laws on incarceration rates. It begins by describing selected mandatory sentencing laws in the United States, focusing on a few characteristics that distinguish different laws across the states. It then presents our analyses, which look at the impact of these laws and considers the interactive impact of determinate sentencing, structured sentencing, time served requirements, and drug sentences on state-level incarceration rates.

Mandatory Sentencing Laws in the States

The policy change that has garnered the most attention since the 1970s is the adoption of mandatory sentencing laws across the states. While such laws may impact procedural aspects of a state's sentencing system (by constraining sentencing and release decisions for certain offenses), mandatory sentencing laws are substantively focused at particular offenses (e.g. drug offenses, violent offenses, or sex offenses) or specific triggering events (offenses involving use of a firearm, against a minor, or in proximity to a school).

Our analyses of mandatory sentencing laws across the states reveals three factors that affect mandatory sentencing provisions: 1) whether the law alters the duration of the sentence for the underlying offense, 2) whether the law requires the judge to alter the duration of the sentence imposed, and 3) whether the law requires the judge to impose incarceration. Based on these factors, several types of mandatory sentencing laws may be inferred: 1) *discretionary sentence enhancements* in which the law alters the duration of the sentence for the underlying offense but allows the judge to impose the same length of sentence for the underlying offense that would otherwise be available by law and still allows the judge to impose a non-incarceration sanction for the underlying offense; 2) *mandatory sentence enhancements* in which the law alters the

duration of the sentence for the underlying offense and requires the judge to impose a different length of sentence than would otherwise be available or required by law, but still allows the judge to impose a non-incarceration sanction; 3) *mandatory enhanced incarceration* in which the law alters the duration of the sentence for the underlying offense, requires the judge to impose a different length of sentence than would otherwise be required or available by law, and requires the judge to impose incarceration; 4) *mandatory incarceration* in which the law requires the judge to impose incarceration, but does not alter the statutory term for the underlying offense and does not require a specific length of sentence be imposed; and 5) *enhanced mandatory incarceration* in which the law alters the duration of the sentence for the underlying offense and requires the court to impose incarceration, but does not require the judge to impose a different length of sentence than would otherwise be required or available by law

Mandatory Sentencing Laws and Incarceration Rates: An Analysis Over Time

Mandatory sentencing laws have been predicted to increase incarceration rates through increased admissions (imposing prison sentences for offenses that in the absence of the mandatory policy would not have resulted in a prison sentence) and through longer sentences imposed. Several critics maintain that mandatory sentencing laws have contributed to rapidly growing prison populations (Beckett and Sasson, 2000); however, few empirical studies have been conducted to support this claim. Indeed, several researchers have rejected these policies as a cause of increased prison populations (Carroll and Cornell, 1985) or admissions to prison (Marvell and Moody, 1995; Langan, 1991). As Langan (1991) points out, between 1973 and 1989, a period of marked increases in prison populations, admissions per arrest increased for all types of offenses, not just those targeted by mandatory sentencing laws. The difficulty in assessing the impacts of mandatory sentences on admissions and prison populations arises from the complexity of their potential influence. In the short run, if fully enforced, mandatory sentences may increase prison admissions and prison sentences; however, if not fully enforced over time, such laws may have no impact of incarceration rates.

Exhibit 8-1 shows four alternative models providing a preliminary approximation to the modeling of mandatory minimums at the state level. Building upon the results of previous analyses included in this report, we added to our baseline model a combination of variables indicating the number of specific policy provisions meeting a general classification criterion. Specifically, we counted the number of mandatory sentencing laws enacted by the states that consider a specific set of triggers, regardless of the actual underlying offenses. While during the course of our project we tested mandatory sentencing laws based on ten different triggers (i.e. all laws based on proximity, the number of offenders or victims, repeated offenses, etc.), Exhibit 8-1 focuses on only four provisions that were the most relevant in terms of the analysis. These included only mandatory sentencing laws that required the judge to impose a term of incarceration for any offense 1) involving use of a weapon, 2) causing significant harm to the victim, 3) committed while under any form of state supervision (bail, probation, parole, jail, or

prison), and 4) committed because of certain characteristics of the victims (e.g. based on race, religion, age, etc.). While we are aware that the coding approach just described creates several methodological challenges when comparing policies across states,[76] we believe that the models presented constitute a good general approximation to the topic of mandatory sentencing laws.

We follow here the same initial Fixed-Effects exploration of the relationship between incarceration rates and our set of social, political and policy-related variables. In terms of the latter set of variables, models in Exhibit 8-1 include the specification of sentencing structure, time served, drug policies and habitual offender laws. Mandatory sentencing laws constitute the last sentencing area covered by this report. By coding the number of mandatory sentencing laws enacted by the states meeting a specific set of triggers we wanted to measure the state's general approach to this type of sentencing policy. The four triggers presented in Table 8-1, cover a wide range of areas addressed generally by legislators and targeted by the criminal justice system.[77]

[76] Such as assuming that these policies have a similar scope between states and that the differences in combinations of triggers and underlying offenses is comparable between states.

[77] As in previous chapters, details on the specification of these models and tests performed are presented in the statistical appendix.

Table 8-1 Fixed-Effects Model, with Mandatory Sentencing Laws

	Model 1	Model 2	Model 3	Model 4
Violent crime rate	0.042	0.044	0.045	0.041
Property crime rate	0.002	0.001	0.000	0.000
% population 18-24	10.035*	10.953*	9.785*	10.322*
% population 25-34	4.886	4.979	4.026	4.878
% population Black	6.681	7.456	6.824	6.762
% population Hispanic	5.292*	4.533*	4.387	4.801*
% population in SMAs	-0.164	-0.079	-0.404	0.019
% population religious fundamentalist	3.112	3.474	3.133	4.257
Income per capita	-0.007**	-0.007**	-0.007**	-0.007**
Unemployment rate	0.687	0.647	1.328	1.323
Poverty rate	-3.144*	-3.033*	-3.050*	-3.195*
Gini	190.412	206.424	196.666	226.025
Revenues per 100k population (*1000)	0.060*	0.062*	0.069**	0.058*
Welfare per 100k population (*1000)	-0.499*	-0.479	-0.562*	-0.486
FTE Police per 100k population	0.146*	0.146*	0.153*	0.161*
Drug arrest rate	525.308**	524.123**	535.96**1	508.136**
Governor (Republican)	13.365**	12.383*	11.855*	11.507*
Citizen political ideology	0.124	0.104	0.115	0.152
Determinate Sentencing	-3.383	-0.849	-5.561	-1.050
Presumptive Guidelines	16.229	10.707	12.909	4.136
Det * Presumptive Guidelines	-89.824**	-84.217**	-84.731**	-78.933**
Voluntary Guidelines	8.455	9.515	11.308	8.486
Det * Voluntary Guidelines	7.443	1.972	2.659	2.261
Time Served (all offenses)	0.170	0.180	0.197	0.179
Time Served (violent offenses)	15.502*	16.806*	15.654*	16.909*
Cocaine enhancements	4.042**	4.296**	4.194**	4.335**
Cocaine Possession Maximum	-0.621***	-0.613***	-0.612***	-0.600***
Cocaine Sale Maximum	0.007	0.010	0.011	0.005
Cocaine Possession Minimum	0.377*	0.386*	0.384*	0.386*
Cocaine Sale Minimum	-0.186	-0.215	-0.203	-0.198
Second-time offender	-10.745	-6.794	-5.597	-7.800
Third-time offender	20.883*	21.397*	22.112*	19.010*
Weapons Mandatory	2.944*			
Harm Mandatory		3.784		
Supervision Mandatory			9.934*	
Victims Mandatory				3.060*
1978	15.104	14.030	16.387	14.690
1981	30.899*	30.586*	35.425*	30.340*
1984	61.255***	60.205***	62.899***	56.818**
1987	102.611***	101.080***	105.826***	97.154***
1990	140.162***	138.866***	144.262***	132.867***
1993	204.250***	203.664***	207.586***	195.222***
1996	261.655***	260.528***	265.336***	253.575***
1999	308.225***	306.948***	310.050***	298.511***
2002	314.754***	313.077***	315.794***	303.573***
Constant	-212.606	-247.319	-196.781	-258.537
R Within	.877	.876	.877	.877
R Overall	.744	.742	.747	.744
N	492	492	492	492

As Table 8-1 indicates, different scores for particular mandatory sentencing laws do not seem to have an impact on the fit of the models, as measured by the R-squared statistic. Caution should be taken when reading these figures since the estimators of overall fit for Fixed-Effects do not have the same properties as the traditional R-square estimator from OLS regressions. In addition, it is important to mention that for Models 1 through 4, both period and unit effects remain strongly significant. This situation has been systematically observed in other chapters of this report. The fact that year and state dummies remain significant after the full specification of our models suggests that there are state- and year-specific trends unaccounted for by our control variables. These trends may be artificially increasing the proxy estimators of overall goodness of fit. The conclusion section of this report will address the possibility that some of the associations tested in the models are time-specific and therefore, we may expect a decrease in the significance of period dummies.[78]

In terms of the social and political variables included in our models, results tend to be consistent with previously observed trends: states with larger proportions of Hispanic populations and larger state revenues have higher incarceration rates ($p<.05$); conversely, states with higher income per capita have lower incarceration rates ($p<.01$). In addition, we continue to observe that states with a greater number of law enforcement personnel and a higher rate of drug arrests also have higher incarceration rates ($p<.05$ and $p<.01$, respectively). Our dichotomous variable measuring the political party of the state's governor remains significant ($p<.05$) indicating that states with republican governors have systematically higher incarceration rates. Finally, it is worth noticing that in accordance with Chapter Seven, crime rates failed to achieve the standard for statistical significance when using Fixed-Effects.

In terms of the policy variables, results also remain consistent across models: states with determinate sentencing and presumptive sentencing guidelines have lower incarceration rates than other states ($p<.01$). Our models also suggest a consistent positive relationship between separate time served requirements for violent offenders and higher incarcerations rates ($p<.05$). The number of sentence enhancements for cocaine offenses and the statutory minimum and maximum sentences for cocaine possession are also significantly related to variations in incarceration rates.

When examining the results concerning the number of mandatory provisions at the state level, we found that all such provisions are positively related to higher incarceration rates, although the variable measuring mandatory penalties for offenses involving harm was not significant. The strongest partial regression coefficient was the one measuring the number of provisions targeting supervision violations ($b=9.934$, $p<.05$). Supervision was operationalized as bail, probation, parole, jail, or prison (see data management appendix). The inclusion of these

[78] This specification does not come free from caveats. Allowing time-specific interactions may increase problems of multicollinearity in our estimation. A further discussion about this topic is addressed in the conclusion chapter and in the statistical appendix.

mandatory provisions did not change observed trends among other policy variables or social/political covariates.

Despite the limitations of this general approach to mandatory sentencing laws, we think that scores for these policies included in Table 8-1 show a significant variation between and within states over time. These variations may be "representative" of more specific policy considerations that should be specified in additional models. In other words, we do not believe that the mandatory sentencing laws considered here are necessarily directly contributing to increases in incarceration rates; rather, they are used here as proxies for states' general approaches to mandatory sentencing laws and, in this sense, indicate the states' general use of mandatory sentencing policies. Based on the results, one may argue that states with more mandatory sentencing policies have higher incarceration rates than other states.

In this report we were interested in providing a general background for the analysis of sentencing policies—including mandatory sentencing laws—that would lay out the ground for more detailed analyses. Consistent with our idea of providing a summary account of policy changes, we decided to aggregate the scores for the mandatory sentencing laws presented in Table 8-1. In this way, we would provide a more balance approach to the sentencing considerations between states. This was important due to possible regional differences in the treatment of some of the triggering factors consider in this analysis (such as weapons or the definition of "significant harm"). We define the new aggregate score as a "Mandatory score" and we tested its relevance in our final, fully-specified model. Results are presented in Table 8-2.[79]

[79] The Breush-Pagan test for Random effects was highly significant ($Chi21(1)=150.6$, $p<.001$). The Hausman test lead to the rejection of the null hypothesis of no systematic difference between the Fixed-effects and the random-effects models ($Chi2(33)=53.70$, $p=.013$). State dummies in the Fixed-effects model are strongly significant ($F(49,400)=7.69$, $p<.001$). As in previous chapters, details on the specification of these models and tests performed are presented in the statistical appendix.

Table 8-2 Full Models with Mandatory Sentencing Laws

	Fixed Effects		Random Effects	
	b	SE	b	SE
Violent crime rate	0.040	0.036	0.049	0.032
Property crime rate	0.001	0.007	0.003	0.005
% population 18-24	11.184*	4.941	6.116	4.445
% population 25-34	5.408	2.827	5.775*	2.708
% population Black	6.870	4.384	3.810**	1.208
% population Hispanic	4.820*	2.213	2.531*	1.149
% population in SMAs	-0.023	0.652	0.082	0.379
% population religious fundamentalist	3.852	3.243	3.563**	1.146
Income per capita	-0.007**	0.002	-0.005**	0.002
Unemployment rate	1.085	2.149	1.424	2.039
Poverty rate	-3.272*	1.492	-3.880**	1.463
Gini	214.782	315.325	378.009	291.477
Revenues per 100k population (*1000)	0.056*	0.026	0.059*	0.024
Welfare per 100k population (*1000)	-0.513*	0.248	-0.803***	0.227
FTE Police per 100k population	0.157*	0.063	0.142*	0.061
Drug arrest rate	515.106**	158.979	466.451**	151.589
Governor (Republican)	12.553*	5.053	12.456*	5.002
Citizen political ideology	0.170	0.351	-0.025	0.314
Determinate Sentencing	-3.030	13.152	-7.268	11.914
Presumptive Guidelines	7.910	19.788	-4.902	18.806
Det * Presumptive Guidelines	-79.273**	27.956	-62.375*	25.680
Voluntary Guidelines	6.942	13.737	5.370	12.874
Det * Voluntary Guidelines	9.241	25.111	17.770	23.197
Time Served (all offenses)	0.164	0.156	0.162	0.139
Time Served (violent offenses)	16.956*	7.570	18.153*	7.370
Cocaine enhancements	3.705**	1.390	3.458**	1.306
Cocaine Possession Maximum	-0.621***	0.113	-0.502***	0.095
Cocaine Sale Maximum	0.008	0.023	0.007	0.019
Cocaine Possession Minimum	0.390*	0.152	0.392**	0.141
Cocaine Sale Minimum	-0.186	0.137	-0.238	0.128
Second-time offender	-11.099	10.123	-4.208	9.059
Third-time offender	19.558*	9.048	8.360	8.183
Mandatory score	2.310**	0.737	2.223**	0.669
1978	13.203	11.013	14.098	10.815
1981	27.467	14.912	25.628	13.871
1984	55.722**	17.073	55.077**	15.833
1987	96.380***	20.217	82.842***	18.142
1990	133.811***	24.109	117.336***	20.578
1993	198.687***	28.102	186.862***	23.313
1996	258.072***	32.080	245.955***	26.518
1999	303.675***	35.269	290.095***	29.520
2002	309.421***	38.169	302.393***	31.461
Constant	-265.960	141.420	-245.619	123.319
R Within	.878		.896	
R Overall	.747		.806	
N	492		492	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

We observe almost no change in terms of the overall goodness of fit of both models. The overall R-squares is higher on the Random-Effects model given its ability to produce a weighted estimation of the between and within regressions. The summary score for mandatory sentencing laws is highly significant ($p<.01$) and positively associated with our outcome variable. This means that controlling for all the other variables in the model, states that have a greater number of mandatory sentencing laws (using our operationalization of these provisions) also have higher rates of incarceration. This result holds for both Random- and Fixed-Effects models. Results for most of the additional policy variables are not impacted by the inclusion of this new variable; only the third-time offender variable drops out of the analysis when we develop a Random-Effects model (the same result was noted in Chapter Seven). The strong associations for the interactions between determinate sentencing and presumptive guidelines are still present in this last iteration of our analysis.

In terms of social variables, there is some evidence of the ability of the Random-Effects model to capture small variations in some of our predictors. As seen in other chapters, under the Random-Effects Model, percent of the population that is black and percent of the population that is religious fundamentalist become significant ($p<.01$).

Conclusion

The increased use of mandatory sentencing laws has been held out as a major cause of increases in prison populations over the last 30 years. Our findings show that states with more mandatory sentencing laws have higher incarceration rates than other states. States have imposed more prohibitions against the granting of probation and have proscribed more mandatory minimum sentences for offenses. In many cases, judges are now constrained in their abilities to set either the disposition or duration of many sentences. Our findings suggest that such constraints have led to higher incarceration rates across the states.

Our analyses considered the presence of only four types of mandatory sentencing laws; we do not believe that these specific laws are leading to increased incarceration rates. Rather, these laws likely act as a proxy for a state's overall use of mandatory sentencing policies. In other words, states with more mandatory sentencing laws addressing this limited number of offenses likely have more mandatory sentencing laws than other states addressing additional offenses. Combined with our previous finding concerning time served requirements for violent offenders, this suggests that states that single out particular sub-groups of offenses or offenders for increased punishment take a more punitive approach to all offenses or offenders than other states, which is reflected in higher incarceration rates.

The inclusion of mandatory sentencing laws does not change the influence of other policies. The findings concerning presumptive sentencing guidelines are informative here. Presumptive sentencing guidelines combined with determinate sentencing continues to be related to lower incarceration rates. Thus, increasing structure by *controlling* judicial discretion may decrease

prison populations; however, increasing structure by completely *eliminating* judicial discretion through mandatory sentencing laws may increase prison populations.

The inclusion of mandatory sentencing laws does not change the influence of social forces either. However, up to this point, we have assumed that the relationship between these forces and incarceration rates is constant over time. In the next chapter, we allow the relationship between these forces and incarceration to vary over time; in this way, we can examine whether different forces are stronger at different points in time.

The analyses so far have also considered only the relationship between different variables and the *size* of state incarceration rates. But, given the variation in the rates of growth in incarceration rates across states, it is important to understand the relationship between these variables and the *growth* in state incarceration rates. In the next chapter, we also examine this growth and the forces that explain variation in growth across states.

Chapter Nine: Policies and Imprisonment Over Time

While all states experienced significant growth in incarceration rates since the 1970s, growth has not been uniform over time. Incarceration rates in some states increased rapidly in the 1970s and early 1980s and then stalled or increased slowly through the 1990s. Other states experienced little growth in incarceration rates through the mid-1980s and then saw their prison populations explode in the 1990s. Table 9-1 shows differences in the rates of growth in state incarceration rates over time. In Colorado, for example, the incarceration rate grew just 20 percent between 1970 and 1985 but increased 303 percent in the next fifteen years; in contrast, the incarceration rate in Alaska increased 746 percent between 1970 and 1985 but rose just 61 percent in the next fifteen years (see Table 9-1).

As the previous chapters have shown, several variables consistently have strong relationships to incarceration rates. However, these prior analyses assume that the strength of these relationships is constant over time; in other words, we have assumed that the impact of, for example, the size of the minority population on state incarceration rates was the same in 1975 as it was in 2002. But the strength of this relationship may vary over time. In other words, the size of the minority population may have a greater impact on incarceration rates in the 1990s than it did in the 1980s. Given the varied rates of growth in individual state incarceration rates during short periods over the last 30 years, the impact of many variables may not be constant over time but may be quite variable.

Further, while certain variables may be associated with the *size* of state prison populations, it cannot be assumed that such variables are associated with the *growth* in state prison populations. This report, to this point, has looked only at the size of state incarceration rates, seeking to explain why certain states have higher incarceration rates than other states. However, while our analyses have shown, for example, that states with determinate sentencing and presumptive sentencing guidelines have lower incarceration rates than other states, this does not necessarily mean that states with this policy combination have had slower growth in incarceration rates; these states may have historically had lower incarceration rates and our findings may have simply confirmed this. What is needed is an analysis of growth in incarceration rates, trying to explain why incarceration rates grew faster or slower in certain states. Few prior analyses have considered those factors associated with growth in state incarceration rates (see e.g. Greenberg and West, 2001).

This chapter estimates the strength of the relationships between certain variables and incarceration rates at different points in time. This use of "period-specific" interactions (interactions between a variable and time) will show how relationships and the impact of different variables may shift over time – increasing or decreasing in importance at certain periods during the last 30 years. This chapter also estimates the relationships between certain variables and the growth in incarceration rates over the last 30 years.

Table 9-1 Percentage Change in State Incarceration Rates, 1970-1985 and 1985-2002.

State	Percentage Change in Incarceration Rate, 1970-1985	Percentage Change in Incarceration Rate, 1985-2002
Alabama	143%	129%
Alaska	339%	38%
Arizona	245%	100%
Arkansas	132%	146%
California	107%	150%
Colorado	20%	303%
Connecticut	101%	219%
Delaware	746%	61%
Florida	82%	82%
Georgia	72%	120%
Hawaii	298%	130%
Idaho	172%	247%
Illinois	207%	109%
Indiana	111%	99%
Iowa	83%	153%
Kansas	112%	70%
Kentucky	41%	186%
Louisiana	173%	158%
Maine	84%	70%
Maryland	123%	52%
Massachusetts	130%	166%
Michigan	84%	156%
Minnesota	39%	152%
Mississippi	187%	214%
Missouri	153%	173%
Montana	284%	165%
Nebraska	56%	111%
Nevada	220%	22%
New Hampshire	143%	182%
New Jersey	106%	116%
New Mexico	135%	115%
New York	200%	77%
North Carolina	66%	36%
North Dakota	158%	193%
Ohio	129%	105%
Oklahoma	73%	167%
Oregon	76%	107%
Pennsylvania	166%	173%
Rhode Island	144%	93%
South Carolina	148%	89%
South Dakota	153%	159%
Tennessee	73%	189%
Texas	60%	206%
Utah	84%	138%
Vermont	76%	161%
Virginia	87%	125%
Washington	89%	67%
West Virginia	49%	181%
Wisconsin	104%	246%
Wyoming	91%	135%

Time-Specific Variables and Incarceration Rates

Previous chapters have consistently shown the significance of year dummies in our analyses, indicating the presence of national trends associated with each year; these national trends are common to all states and not explained by our models. To account for these national trends, it is possible to test for variation in the relationships between different variables and incarceration rates at different points in time. This is commonly done by the use of period-specific interactions, or interactions between particular variables and time. Several authors have examined period-specific relationships in analyses of state incarceration rates. Greenberg and West (2001), for example, find that the size of the black population, unemployment, and welfare have more substantial relationships with incarceration rates in 1990 than in other periods. Jacobs and Carmichael (2001) present similar findings, showing that the relationship between incarceration rates and Republic strength, the size of the black population, and the size of the Hispanic population is stronger in 1990 than in prior years.

Building on the results of previous analyses included in this report, we added to our baseline model a combination of variables indicating an interaction between time and six variables: the size of the black population, the size of the Hispanic population, unemployment, welfare, the law enforcement capacity of the state, and citizen political ideology.[80] We created these time interactions by multiplying each of these variables with the year dummies; the interaction term (variable*year) was then included in the models with our original set of policy and non-policy variables. If an interaction term between a particular variable and time is significant, after holding all other effects constant, we can conclude that that variable had a different relationship with incarceration rates at different points in time.

Table 9-2 shows five alternative models examining period-specific relationships. We follow the same initial Fixed-Effects exploration of the relationship between incarceration rates and our full set of non-policy and policy-related variables. In terms of the latter set of variables, models in Table 9-2 include the specification of sentencing structure, time served, drug policies, habitual offender laws, and mandatory sentencing laws. Model 1 reproduces our results from the final Fixed-Effects model in Chapter Eight (Table 8-2) with the mandatory score variable; Model 1 is included here for comparison to the time-variant Models 2 through 5. Model 2 adds all six time interaction terms. Model 3 then reproduces the analysis but without the time interaction term for welfare. Models 4 and 5 present our final Fixed-Effects and Random-Effects models including all non-policy and policy variables as well as four time interaction terms for the size of the black population, unemployment, the law enforcement capacity of the state, and citizen political ideology.

[80] We tested our models using time interaction terms for all control variables. While not reported here, other variables showed no period-specific relationship to incarceration rates (unreported analyses are available upon request). However, the inclusion of all of these interaction terms created unnecessary confusion in our final models and lead to problems of multicollinearity. As such, we do not include these interaction terms in our models below. The six time interactions presented here displayed the most significant period-specific relationships with incarceration rates.

Table 9-2 Fixed-Effects and Random-Effects Models, with Time Interactions

	Model 1	Model 2	Model 3	Model 4	Model 5
	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects	Random Effects
Violent crime rate	0.040	-0.025	-0.002	0.035	0.052*
Property crime rate	0.001	-0.019**	-0.018**	-0.015*	0.013**
% population 18-24	11.184*	2.867	6.583	5.093	-12.417**
% population 25-34	5.408	1.171	2.258	1.576	8.119**
% population Black	6.870	-2.037	-1.751	1.399	2.469
% population Hispanic	4.820*	-4.398	-5.608	2.879	0.221
% population in SMAs	-0.023	-0.476	-0.496	-0.279	0.202
% population religious fundamentalist	3.852	10.297***	9.939**	9.553**	2.323***
Income per capita	-0.007**	-0.003	-0.003	-0.003	0.000
Unemployment rate	1.085	-7.852	-8.515	-8.911	-11.656
Poverty rate	-3.272*	-1.083	-1.369	-0.836	-1.755
Gini	214.782	330.400	339.458	504.883	503.336
Revenues per 100k population (*1000)	0.056*	0.057*	0.060*	0.067**	0.054*
Welfare per 100k population (*1000)	-0.513*	-1.192	-0.862	-0.577**	-1.308***
FTE Police per 100k population	0.157*	0.339*	0.107	0.111*	0.056
Drug arrest rate	515.106**	90.718	147.286	112.094	-21.382
Governor (Republican)	12.553*	10.504*	11.140*	12.139**	11.809*
Citizen political ideology	0.170	0.940	0.917	1.248*	0.517
Determinate Sentencing	-3.030	-15.535	-17.975	-18.510	-20.048*
Presumptive Guidelines	7.910	-4.963	-9.428	-6.830	-43.130**
Det * Presumptive Guidelines	-79.273**	-35.970	-25.586	-33.872	-35.317
Voluntary Guidelines	6.942	-3.566	-6.231	-4.992	-13.985
Det * Voluntary Guidelines	9.241	37.391	43.681*	40.991	41.843*
Time Served (all offenses)	0.164	-0.068	-0.086	-0.076	0.138
Time Served (violent offenses)	16.956*	15.111*	11.466	10.572	8.525
Cocaine enhancements	3.705**	2.163	1.908	1.769	2.286*
Cocaine Possession Maximum	-0.621***	-0.384***	-0.410***	-0.389***	-0.260***
Cocaine Sale Maximum	0.008	0.002	0.003	-0.016	0.027*
Cocaine Possession Minimum	0.390*	0.333*	0.353*	0.291*	0.249*
Cocaine Sale Minimum	-0.186	-0.129	-0.107	-0.104	-0.238*
Second-time offender	-11.099	-20.728*	-18.400*	-17.909*	11.473
Third-time offender	19.558*	11.166	14.119	9.420	-15.689*
Mandatory score	2.310**	2.196***	2.122**	2.143**	1.365**
1978	13.203	1.098	0.972	15.276	20.853
1981	27.467	6.346	27.911	35.342	-10.559
1984	55.722**	-26.045	30.019	47.321	-2.537
1987	96.380***	-2.017	89.092	118.534*	20.476
1990	133.811***	-10.036	103.672	117.644*	1.261
1993	198.687***	53.719	149.971**	138.959*	33.943
1996	258.072***	119.019	228.649***	225.616***	109.451
1999	303.675***	283.972***	308.298***	292.946***	181.026**
2002	309.421***	378.269***	313.667***	303.402***	186.323**
% population Black_1978		-0.024	-0.084	-0.431	-0.422

	Model 1	Model 2	Model 3	Model 4	Model 5
	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects	Random Effects
% population Black_1981		0.136	0.083	-0.100	-0.265
% population Black_1984		0.473	0.657	0.403	0.933
% population Black_1987		0.320	0.667	0.123	0.927
% population Black_1990		1.740	2.265	1.465	1.486
% population Black_1993		2.792*	3.239*	2.318	1.825
% population Black_1996		3.876**	4.722***	3.547**	2.443
% population Black_1999		5.349***	5.523***	4.716***	3.035
% population Black_2002		5.922***	5.639***	5.165***	3.457*
% population Hispanic_1978		3.407	3.588		
% population Hispanic_1981		1.987	2.516		
% population Hispanic_1984		2.025	3.189		
% population Hispanic_1987		2.654	3.912*		
% population Hispanic_1990		3.552	4.984*		
% population Hispanic_1993		4.157	5.101*		
% population Hispanic_1996		5.214*	6.864**		
% population Hispanic_1999		5.135*	5.607**		
% population Hispanic_2002		4.218	4.239*		
Unemployment rate_1978		7.241	7.749	10.156	7.235
Unemployment rate_1981		8.057	8.352	9.447	12.350
Unemployment rate_1984		10.089	9.964	10.564	11.677
Unemployment rate_1987		8.739	6.984	7.178	8.746
Unemployment rate_1990		7.684	5.560	9.059	13.318
Unemployment rate_1993		11.619	9.196	17.770**	23.694**
Unemployment rate_1996		17.502*	13.232	19.981*	35.948***
Unemployment rate_1999		22.244*	20.537*	23.228**	42.448***
Unemployment rate_2002		17.539	18.321	17.314	37.626**
Welfare per 100K pop._1978		0.000	0.000		
Welfare per 100K pop._1981		0.000	0.000		
Welfare per 100K pop._1984		0.000	0.000		
Welfare per 100K pop._1987		0.000	0.000		
Welfare per 100K pop._1990		0.000	0.000		
Welfare per 100K pop._1993		0.000*	0.000*		
Welfare per 100K pop._1996		0.000	0.000		
Welfare per 100K pop._1999		0.000	0.000		
Welfare per 100K pop._2002		0.000	0.000		
FTE police per 100K pop_1978		-0.025			
FTE police per 100K pop_1981		0.054			
FTE police per 100K pop_1984		0.192			
FTE police per 100K pop_1987		0.302			
FTE police per 100K pop_1990		0.245			
FTE police per 100K pop_1993		0.163			
FTE police per 100K pop_1996		0.364			
FTE police per 100K pop_1999		0.127			
FTE police per 100K pop_2002		-0.313			
Citizen political ideology_1978		-0.645	-0.758	-0.985	-0.742
Citizen political ideology_1981		-0.879	-0.992	-0.834	-0.912
Citizen political ideology_1984		-0.817	-0.900	-1.091	-0.573
Citizen political ideology_1987		-1.647*	-1.792*	-1.431*	-0.754

	Model 1	Model 2	Model 3	Model 4	Model 5
	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects	Random Effects
Citizen political ideology_1990		-1.069	-1.605*	-1.156	-0.506
Citizen political ideology_1993		-2.369**	-2.641**	-1.797*	-1.264
Citizen political ideology_1996		-2.147**	-2.424**	-2.764***	-2.726**
Citizen political ideology_1999		-2.794**	-2.850**	-3.627***	-3.241***
Citizen political ideology_2002		-2.922***	-2.920***	-3.367***	-2.484**
Constant	-265.960	-40.041	-64.411	-206.777	-110.393
R Within	0.878	0.931	0.927	0.917	0.889
R Overall	0.747	0.567	0.537	0.654	0.883
N	492	492	492	492	492

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

As Table 9-2 indicates, the inclusion of period-specific interactions significantly changes the fit of the Fixed-Effects models, as measured by the R-squared statistic, increasing from 0.87 in Model 1 to 0.93 in Model 2. Caution should be taken when reading these figures since the estimators of overall fit for Fixed-Effects do not have the same properties as the traditional R-square estimator from OLS regressions.

We also see a reduction in the significance of the year dummies from Model 1 to Model 2 (the first model with time interactions); after inclusion of time interactions for these six variables, only the year dummies for 1999 and 2002 remain significant. This is true for most of the other three Fixed-Effects models; and in the final Random-Effects model, only the year dummies for 1999 and 2002 remain significant. Thus, the time interactions are explaining much of the year-specific (national) trends impacting all states in the period 1975-1998 that were unaccounted for by our control variables in previous analyses. This specification, however, does not come free from caveats. Allowing time-specific interactions may increase problems of multicollinearity in our estimation. By testing policies that were often incorporated contemporaneously, our models are already exposed to this particular problem for the calculation of consistent estimators. Adding to this several time-varying factors inherently increases the influence of harmful correlations among our set of predictors. In order to deal with this issue we have separated cross-sectional and longitudinal sources of the variation of incarceration rates. A further discussion about this topic is addressed in the statistical appendix (Appendix C).

While several time interactions are significant in Model 2, the most interesting may be those for size of the black population and citizen political ideology. As noted in previous chapters, the size of the black population is consistently related to incarceration rates. As Model 2 indicates, however, the relationship between the size of the black population and incarceration is significant only in the late 1990s. While prior research found similar period-specific effects of the size of the black population in 1980 and 1990 (Jacobs and Carmichael, 2001; Greenberg and West, 2001), our analyses show that this interaction occurs only in the late 1990s. As the coefficients for the period-specific black population variable indicate, the magnitude of the impact of the size of the black population also grows between 1993 and 2002, indicating that the

relationship between the black population and incarceration gets stronger through 2002. With the inclusion of the periodized variable, the time-invariant black population variable changes sign, although it is non-significant.

Previous chapters found no relationship between citizen political ideology and incarceration rates. As Model 2 indicates, however, this relationship is very strong in the 1990s. Thus, while conservative governments (measured by Republican governor) influenced incarceration rates consistently throughout the period, we see substantial relationships between politically conservative citizens and incarceration only in recent years. While both liberal and conservative citizens supported punitive measures in the 1980s and 1990s, it may be that such support is now limited only to conservative citizens. Like the size of the black population, the magnitude of the relationship between citizen political ideology and incarceration rates is increasing through 2002. This is true after controlling for the party of the governor, which continues to be significantly related to incarceration.

Other period-specific effects are not as interesting. There is some support for a general punitive approach to "marginalized" populations. After the growth in the Hispanic population in the U.S. through the 1990s, the size of the Hispanic population displayed a stronger relationship to incarceration rates in the late 1990s, although only for the periods 1996 and 1999. In prior analyses, we found no relationship between unemployment and incarceration rates; the inclusion of the period-specific unemployment rates, however, shows a stronger relationship in late 1990s; although only for the periods 1996 and 1999. Like the size of the black population, the inclusion of the periodized variables for the Hispanic population and unemployment changes the sign of the time-invariant Hispanic population and unemployment variables, although they are non-significant. Model 2 showed no indication that the law enforcement capacity of the state was stronger in certain time periods.

The findings for the time interaction for welfare were surprising. Prior chapters have shown that welfare is consistently related to incarceration rates, finding that states with more generous welfare systems had lower incarceration rates. When allowing time-specific variations, (Model 2) results show that most of the period effects are positive but non-significant. The disruption observed for this trend around 1996 may be related to the welfare reform adopted at the federal level in that period. As argued by Greenberg (2001) the substitution of welfare for more punitive forms of social control is a nationwide phenomenon hardly captured by a pooled time-series design. The overall relationship between these two variables is more accurately measured by the time-invariant coefficient that remains negative and significant (Models 4 and 5).

In terms of the non-policy variables included in our models, results for only a few variables remain consistent with previously observed trends: wealthier states, states with larger law enforcement capacity, and states with Republican governors have significantly higher incarceration rates than other states ($p<.05$). Similarly, violent crime rates, urbanization, unemployment, and income inequality (Gini) continue to fail to achieve statistical significance. However, results for other variables are quite different.

As Model 1 indicates, without period-specific relationships the following variables were significant: the age structure of the population, size of the Hispanic population, income per capita, poverty rates, welfare per capita, and drug arrests. However, in Model 2 (with period-specific effects), none of these variables remain significant. Conversely, while the size of the religious population was not significant in Model 1, Model 2 indicates that states with a larger religious fundamentalist populations have higher incarceration rates ($p<.001$). Model 2 also shows that the property crime rate is significant, indicating that states with higher property crime rates have lower incarceration rates ($p<.01$).

In terms of the policy variables, results remain consistent for only a few policies after introduction of period-specific effects: separate time served requirements for violent offenders, minimum and maximum sentences for cocaine possession, and the number of mandatory sentencing laws. However, other variables, found to be significant in prior analyses, are not significant in Model 2: the interaction between determinate sentencing and presumptive sentencing guidelines, cocaine enhancements, and third-time offender laws. Finally, other policy variables become significant that were not significant in prior analyses: second-time offender laws.

Given the lack of effects for the period-specific law enforcement variables, we decided to drop these variables from the analyses in Model 3. Results for all variables remain the same, with the exception of the time-invariant law enforcement variable, which becomes non-significant. The only other exceptions are the policy variables: the interaction between determinate sentencing and voluntary sentencing guidelines which becomes significant and positively related to incarceration rates, and time served requirements for violent offenders which becomes non-significant.

The period-specific effects for other variables remain fairly constant as well. The relationships between the size of the black population, citizen political ideology, and unemployment rates and incarceration are all significant only in the late 1990s. The period-specific effects of these variables are also increasingly strong in the late 1990s. The relationship between the size of the Hispanic population and incarceration rates also becomes significant for the periods 1987 to 2002, a period of massive increases in Hispanic populations in the United States. Finally, the period-specific effects of the welfare variable are significant only for the period 1993.

Given the lack of effects for the period-specific welfare variables in Model 3, we decided to drop these variables from the analyses in Model 4, our final Fixed-Effects model. We were also interested in narrowing our focus on marginalized populations, so we maintained only one period-specific demographic variable – size of the black population – in our final Fixed-Effects model and dropped the period-specific Hispanic variables, despite their significance.

Again, results for most time-invariant variables remain the same, with the exception of welfare, law enforcement, and citizen political ideology variables, which become significant in

Model 4 and the interaction variable between determinate sentencing and voluntary sentencing guidelines, which become non-significant (although the sign remained the same).

The period-specific effects for other variables remain fairly constant as well. The relationships between the size of the black population and citizen political ideology and incarceration are both significant only in the late 1990s and increasing in magnitude. Also, the period-specific unemployment rate variable becomes significant for the periods 1993 to 1999 and is similarly increasing in magnitude through 1999.

While time-specific covariates constitute the basis of the "historical contingency" produced by a pooled design, this specification may lead to significant problems of estimations. As it was already mentioned in this chapter, collinearity problems may arise given strong correlations between our set of predictors and the period dummies. Given the significance of these issues, the results presented in this section should be interpreted with extreme caution. Results from the Hausman test and the Breusch-Pagan test suggest that the Random-Effects model provides more consistent estimators that the Fixed-Effects model. However, Fixed-Effects estimates may be less vulnerable to third-variable biases given its strong reliance on time and unit specific dummies. However, this characteristic of Fixed-Effects models can also lead to an incorrect estimation of particular time interactions via an under representation of standard errors (type I error). In this particular case, Random-Effects models provide a more robust framework for the analysis.

We observe almost no change in terms of the overall goodness of fit between Model 4 (our final Fixed-Effects model) and Model 5 (our final Random-Effects model). The overall R-squared is higher on the Random-Effects model given its ability to produce a weighted estimation of the between and within regressions. The period-specific effects for unemployment and citizen political ideology are highly significant ($p<.01$) in Model 5 and positively associated with our outcome variable for the late 1990s. Results for the period-specific relationship between the size of the black population and incarceration rates is not as significant under in the Random-Effects model (reaching significance only for the period 2002).

In terms of social variables, there is evidence of the ability of the Random-Effects model to capture small variations in some of our predictors. As seen in other chapters, we note that using this routine increases the significance of many variables not significant under the Fixed-Effects model. These include: the violent and property crime rates, both age structure variables, the size of the fundamentalist religious population, revenue per capita, welfare per capita, and party of the governor. Several other variables become non-significant in the Random-Effects model: law enforcement capacity and citizen political ideology.

Several of the policy variables gain significance under the Random-Effects model. Determinate sentencing and presumptive sentencing guidelines (as separate policies) are both significant, indicating that states with either of these policies have lower incarceration rates than other states; however, contrary to our prior analyses, the combination of these policies is not associated with incarceration rates (although the sign remains negative, the relationship is not

significant). States with the combination of determinate sentencing and voluntary guidelines, more sentence enhancements for cocaine, higher maximum sentences for cocaine sale, higher minimum sentences for cocaine possession, third-time offender laws, and more mandatory sentencing laws have higher incarceration rates than other states. Paradoxically, states with higher minimum sentences for cocaine sale and higher maximum sentences for cocaine possession have lower incarceration rates than other states.

Growth in Incarceration Rates

The analyses to this point have focused only on those factors explaining variation in the *size* of state incarceration rates. In these final analyses, we consider actual *change* in state incarceration rates by estimating models in which growth in the incarceration rate is the dependant variable. Few authors have examined such growth in state incarceration rates. Greenberg and West (2001), for example, look at change in state incarceration rates between 1970 and 1990 and find that faster growth in incarceration rates is related to higher violent crime rates, higher unemployment rates, larger black populations, political conservativeness, increases in drug arrests, increases in revenues, and increases in the size of the black population; they also find that increases in welfare spending and the adoption of determinate sentencing are associated with slower growth in incarceration rates.

Building on the results of previous analyses included in this report, the analysis of growth begins by re-examining the original baseline model (i.e. the model without policy variables) with a dependent variable of change in incarceration rates (rather than the static incarceration rate). The set of control variables includes a combination of lagged variables corresponding to the original set of control variables and change variables corresponding to changes in those control variables from one period to the next. Thus, the analyses examines the relationship between variables at time t_n and the changes in variables from time t_n to time t_{n+1} and changes in incarceration rates from time t_n to time t_{n+1}.

Table 9-3 shows five alternative models examining change in incarceration rates. We follow the same initial exploration of the relationship between incarceration rates and the full set of non-policy and policy-related variables. In terms of the latter set of variables, models in Table 9-3 include the specification of only determinate sentencing and sentencing guidelines.[81]

Initial models were conducted using Fixed- and Random Effects estimators. However, unlike the analyses in the rest of this report, the estimation procedure here is not simple. According to the Hausman test, we reject the hypothesis that the Fixed- and Random-Effects coefficients are the same (the null is that there is no difference between the two) ($Chi2= 73.39$, $p<.001$). Under normal circumstances, this would indicate the Fixed-Effects estimates are better indicators. However, the results of the Breusch-Pagan test for Random-Effects failed to reject the null

[81] Other policy variables were originally included in the models. However, given the difficulty in specifying the models, we decided to drop all policy variables except determinate sentencing and sentencing guidelines.

hypothesis of no independence of the residuals (Chi2=.45, p=.50). This suggests that for a given state, there are unaccounted for trends in the residuals. In other words, there is heteroskedasticity in the data which makes the estimators inefficient (although, still unbiased). The fact that we fail to reject the null in the Breusch-Pagan test suggests that panel data techniques (Fixed- and Random-Effects), should not be employed.

Using a different version of the same dataset we found that there is strong evidence of serial level 1 autocorrelation, despite the fact that our observations are taken every three years ((F(1,49)=59.7, p<.001). Since there are problems of both panel heteroskedasticity and level 1 autocorrelation in the data, we generate a new model with corrections for both by employing a Feasible Generalized Least Squares approach. Using this method, we can specify the model to have a common autocorrelation coefficient or one for every panel (state). The second option is more refined but uses many degrees of freedom (since it requires the estimation of an extra 50 parameters). Since our models already employ many variables, we use the common autocorrelation. This sort of estimation will be our final estimation procedure for all growth models that follow.

Model 1, in Table 9-3, shows a general model that includes the lags and differentials for all the non-policy variables included in our previous analyses and a lag of the state's incarceration rate which accounts for "self-correcting mechanisms" between low and high levels of incarceration at the state level (see e.g. Greenberg and West, 2001, page 635). Following Greenberg and West (2001), we decide to eliminate the lag for incarceration rates since it is not significant and may be creating conflicts with other variables (collinearity); Model 2 has the initial set of non-policy variables without the disturbance of this variable. Model 3 then adds variables for determinate sentencing and the adoption of determinate sentencing. Finally, Model 4 presents our final model including variables for presumptive and voluntary guidelines, interactions between determinate sentencing and guidelines, and the adoption of each of these policies.

Since the models use maximum likelihood estimation, the same goodness of fit parameters as seen in previous Fixed- and Random-Effects models are not available. Instead, we present the -2 log likelihood and the Wald ratio. Ideally, both parameters should decrease as we further specify our models.

Table 9-3. Feasible Generalized Least Squares Models for Growth in Incarceration Rates

	Model 1	Model 2	Model 3	Model 4
	FGLS AR1 Hetero	FGLS AR1 Hetero	FGLS AR1 Hetero	FGLS AR1 Hetero
Incarceration rate	-0.035			
Violent crime rate	0.007	0.002	0.009	0.008
Property crime rate	0.006***	0.005***	0.006***	0.007***
% population 18-24	4.099*	4.143*	3.836*	3.393
% population 25-34	-3.537**	-3.630**	-3.523**	-3.694**
% population Black	0.393	0.274	0.243	0.145
% population Hispanic	-0.320	-0.282	-0.163	-0.295
% population in SMAs	-0.173	-0.165	-0.163	-0.152
% population religious fundamentalist	0.393	0.397	0.364	0.508*
Income per capita	0.001	0.001	0.001	0.001
Unemployment rate	2.049*	2.005*	2.061*	1.780*
Poverty rate	-1.091	-0.838	-0.813	-0.629
Gini	271.337*	231.184*	211.611*	166.577
Revenues per 100k population (*1000)	0.000	0.000	-0.022	-0.014
Welfare per 100k population (*1000)	0.000	0.000	-0.028	0.011
FTE Police per 100k population	0.075*	0.082*	0.055	0.029
Drug arrest rate	-154.068*	-170.742**	-184.694**	-185.864**
Governor (Republican)	2.571	2.006	2.292	1.112
Citizen political ideology	0.139	0.121	0.101	0.122
Δ Violent crime rate	0.007	0.007	0.008	0.008
Δ Property crime rate	-0.002	-0.002	-0.002	-0.001
Δ % population 18-24	3.890	4.335	3.995	3.844
Δ % population 25-34	-0.092	-0.395	-0.299	-0.567
Δ % population Black	0.556	0.547	0.455	0.452
Δ % population Hispanic	-2.520*	-2.709*	-2.511*	-2.477
Δ % population in SMAs	0.067	0.078	0.087	0.111
Δ % population religious fundamentalist	0.352	0.629	0.519	0.610
Δ Income per capita	-0.006	-0.006	-0.004	-0.006
Δ Unemployment rate	3.396***	3.530***	3.571***	3.665***
Δ Poverty rate	-0.489	-0.355	-0.353	-0.339
Δ Gini	230.006	196.131	202.185	247.833
Δ Revenues per 100k population (*1000)	0.000	0.000	-0.082	-0.046
Δ Welfare per 100k population (*1000)	0.000	0.000	-0.079	0.012
Δ FTE Police per 100k population	0.044*	0.047*	0.047*	0.038
Δ Drug arrest rate	0.007	0.006	0.006	0.007
Δ Governor (Republican)	5.054*	4.927*	5.058*	4.200
Δ Citizen political ideology	0.095	0.075	0.069	0.091
Determinate Sentencing			-8.665**	-2.747
Presumptive Guidelines				10.940
Det * Presumptive Guidelines				-27.331**
Voluntary Guidelines				-0.578
Det * Voluntary Guidelines				-4.147
Δ Determinate Sentencing			-4.083	-7.217
Δ Presumptive Guidelines				3.262
Δ Det * Presumptive Guidelines				-0.187
Δ Voluntary Guidelines				-12.055
Δ Det * Voluntary Guidelines				40.723**

	Model 1	Model 2	Model 3	Model 4
	FGLS AR1 Hetero	FGLS AR1 Hetero	FGLS AR1 Hetero	FGLS AR1 Hetero
1978	-11.355	-11.215	-10.464	-11.968
1981	-7.961	-9.249	-8.637	-10.612
1984	-4.552	-5.426	-4.022	-6.902
1987	12.911	11.330	11.727	7.242
1990	32.336***	29.827**	30.203**	27.396**
1993	21.221	15.519	17.331	13.318
1996	37.270***	29.888**	30.845**	25.524*
1999	29.054*	20.514	21.261	14.575
2002	0.703	-10.245	-7.707	-13.162
Constant	-106.967*	-99.325*	-89.192*	-69.973
2 Log Likelihood	-2207.2	-2206.5	-2202.3	-2194.46
Wald Chi2 ratio	333.54	359.82	377.1***	409.7***
N	494	494	494	494

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

As Models 1 and 2 indicate, dropping the incarceration rate as a predictor of growth in incarceration rates does not change the direction or significance of any of the control variables. According to Model 2, sustained levels of property crime, but not violent crime, were associated with larger growth in incarceration rates (although the coefficient for violent crime is in the positive direction); however, change in crime rates was not associated with growth. This is consistent with previous analyses by Greenberg and West (2001), finding that high crime rates but not increases in crime rates lead to growth in incarceration rates. Larger youth populations were also associated with larger growth in incarceration rates; paradoxically, a larger population of 25 to 34 year olds was associated with smaller growth in incarceration rates.

Incarceration rates in states with higher levels of unemployment, greater increases in unemployment, and higher levels of income inequality grew faster than other states. Similarly, states with more law enforcement personnel per capita and larger increases in law enforcement personnel per capita saw larger growth in incarceration rates. Paradoxically, in states with lower levels of drugs arrests, the incarceration rate grew faster, probably as a by-product of the war-on-drugs combined with the catching up effect of low/high rates of incarceration between states. Again, paradoxically, larger increases in the size of Hispanic populations were associated with slower growth in incarceration rates. Finally, the election of a Republican governor was associated with larger growth in incarceration rates in the period immediately after election.

Models 4 and 5 replicate the same model and estimation. In Model 4 we include a variable for determinate sentencing; in Model 5 we include the additional variables for sentencing guidelines and the combination of determinate sentencing with sentencing guidelines.

As Model 4 indicates, the inclusion of determinate sentencing does not change the direction or significance of the control variables; the only exception is the number of law enforcement personnel, which becomes non-significant in Model 5. There is a significant effect of

determinate sentencing on growth. States with determinate sentencing have slower growth in incarceration rates than other states; this is a long-term effect, since the adoption of determinate sentencing is not associated with slower growth in the period just after adoption (although the direction is negative, it is not significant). The inclusion of determinate sentencing also slightly increases the explanatory power of our model.

After the inclusion of sentencing guidelines in Model 5, several additional control variables are no longer significant: percent of the population between the ages of 18 and 24, income inequality, change in the size of Hispanic population, change in the number of law enforcement personnel, and election of a Republican governor. Thus, in our final model, states with higher property crime rates, smaller percentages of their population between ages 25 to 34, higher unemployment rates, greater increases in unemployment rates, and fewer drug arrests have larger growth in incarceration rates than other states.

Two policy variables are also significant. Once sentencing guidelines are included in the model, determinate sentencing alone is no longer significant (although the sign remains negative). However, determinate sentencing combined with sentencing guidelines is strongly associated with growth. Specifically, states with the combination of determinate sentencing and presumptive sentencing guidelines have smaller growth in incarceration rates than other states; yet, conversely, states with the combination of determinate sentencing and voluntary sentencing guidelines have larger growth than other states. None of these policies alone is associated with growth.

Conclusion

Over the last 30 years, the size of state incarceration rates has varied substantially over time. Our analyses show that the relationship between incarceration rates and several social, economic, and political factors has also varied over time. Specifically, the relationship between the size of marginalized populations – minorities and the unemployed – and state incarceration rates is not constant; rather, the size of marginalized populations was related to incarceration rates only in the late 1990s and had an increasing impact during that period. This coincided with national welfare reforms that removed the social safety net for many minority and unemployed persons. Thus, we find support for the theory that, in the late 1990s, states started using incarceration practices as an alternative approach to the control of marginal classes (see e.g., Young, 1999; Garland, 2001; Wacquant, 2005). A politically conservative citizenry was also related to higher incarceration rates only in the late 1990s; like the size of marginalized populations, the impact of a conservative citizenry became increasingly strong through 2002. Thus, while it appears that both liberal and conservative citizens supported more punitive approaches to crime in the 1970s and 1980s when crime rates were increasing, as crime declined in the 1990s, liberal citizens decreased their support. In contrast, conservative citizens continued their backing of tough on crime measures and increased incarceration.

While certain variables may be associated with the *size* of state prison populations, it cannot be assumed that such variables are associated with the *growth* in state prison populations. Policymakers seeking to lower prison populations are interested in those variables that explain future changes in incarceration rates. As in our previous analyses, we found that the combination of determinate sentencing and presumptive sentencing guidelines were associated with slower growth in incarceration rates. In other words, while all states experienced increases in incarceration rates during the last 30 years, incarceration rates in those states that tightly controlled sentencing and release decisions increased at a slower rate. However, loosely controlling sentencing decisions had the opposite effect – the combination of determinate sentencing and voluntary sentencing guidelines were associated with faster growth in incarceration rates. In other words, loosely controlling sentencing decisions but tightly controlling release decisions led to faster growth in prison populations.

Conclusion

What accounts for the size of state incarceration rates in the United States? As we noted at the beginning of this report, a society's approach to punishment is driven by a variety of objectives and determinants and, in the end, is "overdetermined" by a variety of forces (Garland, 1990). Our results confirm this. Differences in crime rates, the size of racial and ethnic populations, the size of economically disadvantaged groups, wealth, and politics all influence incarceration rates in different ways and help explain the differences in the size of state incarceration rates. However, they do not explain all of the differences.

Implications

Policies and Imprisonment

Policymakers make choices that affect prison populations; this is clear from our research. The twin desires of determinacy and structure that have guided the creation of sentencing and corrections policies over the last 30 years have affected state incarceration rates. But how states adopt that determinacy and structure also matters.

Sentencing policies, designed to ensure the parity, certainty, or severity of court-imposed sentences, have been presumed to affect incarceration rates by altering the flow of inmates into the prison system or by increasing the amount of time offenders are required to serve. Determinate sentencing has the potential to affect prison populations in both senses; however, the direction of that effect is debatable. Early studies both supported and contradicted these predictions with results varying across and within states (*see* Tonry, 1988; Hewitt and Clear, 1983; Joyce, 1992). Subsequent research, however, showed that determinate sentencing may have no effect on prison populations (Carroll and Cornell, 1985; Taggert and Winn, 1993) or a potential moderating effect, holding incarceration rates in check or reducing them somewhat (Marvell and Moody, 1996; Jacobs and Carmichael, 2001; Greenberg and West, 2001).

In contrast, sentencing guidelines, particularly presumptive sentencing guidelines, have been held out as a "more balanced approach to critical issues of sentencing policy" (Frase 1995: 174). Generally designed with the explicit goal of predicting and avoiding prison overcrowding, guidelines were initially heralded as a way to control rising prison populations (Frase, 1995). While some analysts tentatively considered such laws successful in their ability to hold prison populations in check (Alschuler, 1991; Tonry, 1991), others criticized individual state guidelines commissions for failing to keep populations below capacity over time (Savelsberg, 1992; Holten and Handberg, 1990). Subsequent research shows that presumptive sentencing guidelines can act as a mediating factor, slowing prison population growth and reducing prison populations but only when such guidelines are sensitive to prison capacity (Marvell, 1995; Nicholson-Crotty, 2004). Indeed, guidelines can both effectively reduce the rate of incarceration in states where their formulation is explicitly linked to prison capacity or significantly increase the rate of incarceration in states where their utilization mixes with a crime control agenda (Griset, 1999).

Regardless of the policy objective sought, research has shown that states employing presumptive sentencing guidelines have been very successful in initially achieving their policy objectives of altering sentencing patterns and either increasing or decreasing prison populations (see e.g. Tonry, 1996).

Our research shows that controlling release decisions alone through the adoption of determinate sentencing is not enough; similarly, controlling sentencing decisions alone through the adoption of sentencing guidelines is not enough. It is only the combination of the two policies that impacts incarceration rates. We consistently found that states with the combination of determinate sentencing and presumptive sentencing guidelines have lower incarceration rates than other states. (Only in our final analyses of period-specific effects did we find presumptive sentencing guidelines alone associated with lower incarceration rates.) Further, the combination of the two policies was also associated with smaller growth in incarceration rates. The stability of the combined policies was noticeable in all analyses conducted, after controlling for all other policies and social variables. However, voluntary guidelines do not have the same effect. We consistently found that states with the combination of determinate sentencing and voluntary sentencing guidelines have higher incarceration rates and experienced larger growth in incarceration rates than other states. Thus, it is only by tightly controlling sentencing decisions and release decisions that states achieve lower incarceration rates and smaller growth.

Yet, completely eliminating sentencing discretion through mandatory sentencing laws can lead to higher incarceration rates. States with more mandatory sentencing laws have higher incarceration rates than other states. Mandatory sentencing laws have been predicted to increase incarceration rates through increased admissions (imposing prison sentences for offenses that in the absence of the mandatory policy would not have resulted in a prison sentence) and through longer sentences imposed. Several critics maintain that mandatory sentencing laws have contributed to rapidly growing prison populations (Beckett and Sasson, 2000); however, few empirical studies have been conducted to support this claim. Indeed, several researchers have rejected these policies as a cause of increased prison populations (Carroll and Cornell, 1985) or admissions (Marvell and Moody, 1995; Langan, 1991). As Langan (1991) points out, between 1973 and 1989, a period of marked increases in prison populations, admissions per arrest increased for all types of offenses, not just those targeted by mandatory sentencing laws. It is unclear from our analyses whether mandatory sentencing policies are leading to increased admissions or sentencing lengths. The mandatory sentencing laws considered here are likely not leading directly to increased incarceration rates. Rather, they are likely acting as a proxy for the state's general approach to sanctioning criminal offenders. Thus, Langan's observation may be correct and our findings suggest that more mandatory sentencing laws simply indicates greater punitiveness among policymakers and the electorate toward all offenders, which in turn leads to higher incarceration rates.

Other policies have received far less empirical study in prior analyses. While no examinations of state's general time served requirements exist, several studies have considered

the impact of Truth-in-Sentencing laws. Truth-in-Sentencing laws, with their express desire to make offenders serve nearly their entire sentence before release, may significantly increase incarceration rates by filling prison space with inmates serving longer sentences than previously enforced. However, such laws have not been shown to increase incarceration rates in this way (Turner et al., 1999). In fact, Grimes and Rogers (1999) find that truth in sentencing laws requiring inmates to serve 85 percent of their sentences reduced prison admissions and prison population growth in Mississippi; however, clear explanations for this relationship were not apparent. Our analyses suggest that states with separate time served requirements for violent offenders – not necessarily Truth-in-Sentencing laws under the federally defined criteria – have higher incarceration rates than other states. However, states with higher time served requirements for all offenders do not necessarily have higher incarceration rates than other states.

Habitual offender or "three strikes" laws, designed specifically to increase incarceration and sentence lengths, have also been predicted not only to increase sentence lengths for offenders and incarceration rates, but to increase the incidence of plea bargaining for many additional offenders. As a result, such laws may also increase admissions for offenders who plead down to avoid the most severe sentence possible under the habitual offender law. However, while only a few studies exist, three strikes laws have been found to increase incarceration for violent offenses only slightly (Turner et al., 1999), perhaps because of their infrequent use in most states (Schultz, 2000). Thus, their impact on incarceration rates remains debatable. We found support for the argument that third-time habitual offender laws are associated with higher incarceration rates. However, like mandatory sentencing laws, it is unclear if the presence of the habitual offender laws lead to more offenders entering prison under the provisions or if the law simply acts as a proxy for the state's general punitiveness.

Finally, while many analysts and practitioners claim that changes in sentences for drug offenses have led to large increases in prison populations across the states, no prior studies have systematically examined the impact of changes in drug laws on incarceration rates. Arrests for drug offenses in the United States nearly tripled between 1980 and 2001 and the number of persons held in prison for a drug offense increased by 1,195 percent during the same period (Harrison and Beck, 2003). With an absence of studies examining states' sentencing approaches to drug offenses, it has been unclear whether the increases in state incarceration rates can be attributed to changes in sentencing policies for drug offenses or changes in enforcement practices. We found support for both explanations. The number of drug arrests in a state and the capacity of law enforcement were consistently related to higher incarceration rates; it was only in our final period-specific analyses that these were not associated with incarceration rates. However, we also found that states with higher statutory minimum sentences for cocaine possession and more sentence enhancements for sale or possession of cocaine have higher incarceration rates. Thus, a state's approach to both the creation and enforcement of drug laws affects incarceration rates.

Crime

While policies may affect prison populations in the states, many social forces continue to operate, creating differences in the size of state incarceration rates. While a society's imprisonment practices may not be entirely determined by a functional response to crime rates, we found that states with higher violent crime rates have higher incarceration rates than other states. As noted in Chapter Two, high levels of crime present a larger pool of "eligible" persons for incarceration; but persistently high crime rates may also shape public attitudes for increasingly harsh penalties and, in turn, lead to higher incarceration rates. The impact of crime may also have a lag affect, impacting sentencing and corrections policies and incarceration rates only after crime rates have peaked (Tonry, 1999c). If that is the case, one would expect incarceration rates to decline after the peak in crime rates in the mid-1990s as public attitudes change.

There is some evidence for this in the latest public opinion polls. A 1994 survey by the Pew Research Center for the People and Press showed that 29 percent of respondents felt crime was the most important problem facing their community; by 2001, only 12 percent gave the same answer (Pew Research Center, 2001). Indeed, in national surveys, 36 percent of Americans felt that crime was one of the two most important issues for government to address in 1994; by 2003, only 5 percent felt the same way. Public attitudes toward incarceration also appear to be changing. Polls by Peter D. Hart Research Associates show that in 1994, 48 percent of Americans favored addressing the underlying causes of crime while 42 preferred deterrence through stricter sentencing; by 2001, 65 percent of respondents preferred to address the root causes of crime and only 32 percent opted for harsher sentencing. Growth in incarceration rates slowed after crime rates peaked in the 1990s (Beck and Karberg, 2001).

As crime rates continue to decline, if theories are correct, public attitudes toward punishing crime should become less harsh and incarceration rates should then decline. However, as our findings indicate, states with high crime rates continue to have higher incarceration rates. Additional research should address the question of whether changes in crime rates indeed affect changes in incarceration rates and whether high crime rates lead to the adoption of harsher sentencing policies; the analyses here only show that the size of the crime rate is, indeed, associated with the size of a state's incarceration rate. Additional studies should also consider non-lineal versions of the influence of crime rates on incarceration rates. Perhaps more importantly, our analysis assumes that imprisonment rates are influenced by crime rates without considering the possibility of a feedback loop. This issue has been considered previously by research on the social covariates of imprisonment and results seem to confirm that the direction of effects is more important when going from crime to incarceration (Greenberg and West, 2001). However, further work on this model specification is needed given the number of predictors that are assumed exogenous in this report (Greenberg, 2001).

The War on Drugs

Despite the potential effect declining crime rates may have on incarceration rates in the long run, the impact of drug arrests and other law enforcement initiatives may outstrip any such effects. Our findings show that states with more drug arrests and a larger commitment to law enforcement have higher incarceration rates than other states. Our analyses indicate that a reduced emphasis on enforcing drug offenses should reduce incarceration rates. States have begun to moderate their approaches to drug offenders by creating alternatives to incarceration for low-level drug offenders and offenders with drug abuse problems (Wool and Stemen, 2004). The adoption of mandatory treatment for drug possessors in Arizona, California, Kansas, and Texas indicates that states of diverse political backgrounds and with strikingly different incarceration rates are seeking to divert more drug offenders from state prisons. Polls indicate that the public strongly supports such alterative sanctions, with nearly 75 percent of respondents supporting treatment over incarceration for drug possessors and just over 70 percent supporting treatment for drug sellers. As public attitudes toward drug offenders continue to change, policymakers may continue to look for such treatment alternatives, reducing incarceration rates as more drug offenders receive non-incarcerative sanctions.

Social Cleavages

Social cleavages explain differences in incarceration rates across the states. Our results support theories that maintain that incarceration is one method the state uses to manage racial and economic cleavages and conflicts in the populace.

As noted above, under the functionalist account, as crime rates decline the public's desire for punitive sanctions should decrease as well. As we also noted, crime rates have been dropping in most states since the early 1990s and public attitudes toward crime and justice are become less punitive. However, the increasing racial and economic threat in the late 1990s may have continued to fuel increases in incarceration rates after these declines in crime and popular support for punitive sanctions.

Indeed, according to our findings, the relationship between race and incarceration is increasingly stronger in the 1990s. In other words, while we found that states with larger minority populations have higher incarceration rates than other states, we found that race had a stronger influence on incarceration rates in the late 1990s that in other periods. We also found that higher unemployment rates were related to higher incarceration rate, but only in the late 1990s. Combined with our finding that lower welfare payments are also associated with higher incarceration rates, this provides significant empirical support for the association between economic and racial threat and state punishment policies (Wallace, 1981; Greenberg and West, 2001). These findings suggest that states may use incarceration practices as an alternative approach to the control of marginal classes. Several theoretical works provide a substantive framework to explain the implications of this expression of government control (Young, 1999, Garland, 2001). In a recent work, Loic Wacquant (2005) has offered a historical perspective on

the relationship between race, crime and the administration of justice. According to his work, the penal system has been employed as an instrument to manage disadvantaged populations—especially African Americans. Ethnographic work both inside the prison (Irwin, 1990) and outside (Clear et al., 2004) provides some support for this perspective.

This management of disadvantaged populations may be contingent on a state's ability to pay for it. Our analyses indicate that wealthier states – those with higher state revenues per capita – have higher incarceration rates than other states (see also Greenberg and West, 2001). Thus, the theory that wealthier states will invest in more innovative approaches to corrections, relying less heavily on traditional sanctions such as imprisonment appears to be refuted by our findings; rather, wealthier states appear to rely more heavily on formal mechanisms of social control. Thus, like Greenberg and West (2001), our findings suggest that declining state revenues may lead to lower incarceration rates. The continued budget constraints placed on states in recent years may have opened a unique window in which states can begin to reduce large prison populations, particularly in light of the changing public attitudes about crime and incarceration (Jacobson, 2005). The budget crises experienced by most states have already led to significant changes in sentencing and corrections policies in several jurisdictions (Wool and Stemen, 2004). It is unclear when and if these policy changes will result in reductions in incarceration rates. Any reductions will likely be mediated by other social forces impacting the state, particularly race.

Politics

Conservative shifts in politics, evidenced through the influence of political party of the Governor and the influence of religious fundamentalism, also account for differences in incarceration rates across the states. While violent crime rates were found to influence incarceration rates, the strength of the Republican governors helps explain the continued increases in incarceration rates during a time in which crime rates stayed constant or fell. While both Democrats and Republicans campaigned on law and order rhetoric over the last 30 years, the Republican party exerted a strong influence over incarceration rates during the period (see also Jacobs and Carmichael, 2001). We also find support for the argument that the calls for law and order were driven by politicians and not by public sentiment (Beckett, 1997); the influence of the Republican party continues even after controlling for citizen political ideology and membership in fundamentalist religions is held constant. This lends support for the argument that issues such as law and order are used strategically to gain support from working and lower class voters who have greater reasons to fear street crime. This argument is strengthened by the fact that citizen political ideology did not explain incarceration rates for most of the period from 1975 to 2002. The conservatism of citizens only influenced incarceration rates in the late 1990s, after continued declines in crime rates. Thus, both liberal and conservative citizen may have supported punitive policies for much of the law 30 years when crime rates were increasing; but continued support for such policies existed only among conservative citizens after crime rates fell.

Future Research

While this report provides a complex view of the relationship between state-level policies and incarceration, it creates many new questions and directions for future research. This project considered the impact of state-level policies on state incarceration rates. However, this is just one way in which state policies may impact criminal justice systems in the United States. There are several ways we can expand the scope of the research begun under this project and look at these varied impacts.

Trajectories for incarceration by race, gender, and offense type.

Research should next consider the impact of state policies on incarceration rates for particular sub-groups of the population, particularly minorities, women, and drug offenders. Research should address how trajectories for incarceration vary over time and across states for these groups, what factors are associated with variations in these outcomes, and what impact different policies have on those trajectories.

Extending the analyses "horizontally."

While this project has considered incarceration rates, changes in state-level policies also impact patterns of law enforcement, case loads of courtroom actors, and local corrections populations. Research should address the impact of policies on different parts of the criminal justice systems across states. This may include examining the impact of policies on plea bargaining and trials rates, prosecution or defense case loads, prosecution practices, or pretrial detentions. In this way researcher can begin to connect the impact of policies across the entire criminal justice system. This could then be combined with the mapping of trajectories to consider the impact on court processes for specific offenses or groups of offenses.

Extending the analyses "vertically"

While criminal justice policy is generally set at the state-level, the effects are felt primarily at the local level. Changes in state-level policies impact different communities in different ways, increasing incarceration rates in certain neighborhoods and altering local social networks. The disparate impact of policy changes on communities is little studied and little understood. The analyses could, thus, be extended to examine policy impacts on different parts of the system within a given state, exploring the influence of state policies on county-specific incarceration, system, and crime variables.

Appendix A: Policy Data Collection

This Appendix describes the research methods and analytic techniques used in this project to compile, process, and analyze data on state-level sentencing and corrections policies. The objective of the project was to build a framework for examining the types of sentencing and corrections policies adopted by states between 1975 and 2002 and to use that framework to collect data on the timing and content of those policies in each of the 50 states during that period. The study consisted of two phases completed between November 2002 and March 2004.

Phase One: Building a Policy Framework

The first phase of the research involved building a framework for understanding the types of state-level sentencing and corrections policies in use between 1975 and 2002. The goal was to identify those policies or general areas of sentencing and corrections that had undergone the greatest change, displayed the greatest diversity of content across states, or garnered the most attention among practitioners, policymakers, and academics since the 1970s.

Prior analyses of policies developed by Shane-Dubow, Brown, and Olsen (1985), the Bureau of Justice Assistance (1996; 1998), and the National Institute of Justice[82] provided an initial reference point for the types of policies or general areas considered. The policies/areas broke down into nine generally categories: general sentencing structure, probation, time served requirements, sentence reduction credits, sentencing guidelines, habitual offender laws, drug sentences, discretionary parole release and post-release supervision, and mandatory sentencing laws.[83] Major characteristics of each of these policies/areas were then developed; the goal was to focus the examination on the complex, internal characteristics of each policy rather than the simple presence or absence of a particular policy across the states. Characteristics of each policy/area included:

> General Sentencing Structure: number of felony classes; statutory sentence ranges for each felony class; form of prison term imposed by the judge (minimum term, maximum/fixed term, or minimum and maximum terms); non-incarceration sanctions available to the judge at sentencing; limits on judicial discretion to modify sentence after imposition; statement of reasons required by judge at sentencing; general types of

[82] These included NIJ publications describing states' varied approaches to individual sentencing policies including sentencing guidelines (Lubitz and Ross, 2001; Parent, et. al., 1996b), mandatory sentencing laws (Parent et. al, 1996a), habitual offender laws (Henry, Austin, and Clark, 1997), and truth-in-sentencing laws (Sabol et. al., 2002).

[83] While these policies/areas overlapped other, equally salient features of criminal justice systems (such as community corrections or reentry), this study did not specifically target these other topics in the analysis. The study did capture information on the types of sentencing alternatives available to the court at sentencing. However, this did not consider all of those alternatives in the state that were available at the discretion of probation departments or departments of correction, nor did it look at the functioning and funding of such programs; rather, this study was concerned with those sanction available at sentencing. .

offenses eligible for life sentences; availability of victim statement at all sentencing hearings; presence of victim compensation fund.

Probation: probation sentence ranges for each felony class; presence and amount of probation supervision fees; level of government at which probation supervision is organized.

Time served requirements: time served requirements (percentage of sentence or number of years) for each offense class, specific offense, or group of offenses.

Sentence reduction credits: the types of sentence reduction credits available (statutory, earned, meritorious, emergency); the amounts of sentence reduction credits available; offenses to which sentence reduction credits may be applied; how sentence reduction credits affect sentence (reduce minimum term, reduce maximum/fixed term, expedite parole eligibility); any caps placed on the amount of sentence reduction credits that may be earned by different offense classes or groups of offenders.

Sentencing guidelines: the voluntary/presumptive nature of guidelines; the offenses covered by guidelines; the sentence ranges specified in guidelines cells or recommendations; the sentence dispositions specified by the guidelines.

Habitual offender laws: the number and type of prior offenses required for application of the law; time limits on when prior offenses may have occurred; the nature of the prior adjudication (conviction versus incarceration) required for application of the law; the type of current offense required to trigger the law; the impact of the law on the duration of the sentence for the underlying offense; the impact of the law on the judge's decision to alter the duration of the sentence imposed; the impact of the law on the judge's decision to impose incarceration; the impact of the law on release decisions.

Drug sentences: quantity thresholds that determine penalties for each offense/drug; minimum and maximum sentences for each quantity threshold; presumptive dispositions for each quantity threshold; circumstances that increase the penalties available for each offense/drug.

Discretionary parole release and post-release supervision: presence of discretionary parole release or post-release supervision for particular offenses; length of parole or post-release supervision term for each felony class; presence and amount of supervision fees; level of government at which supervision is organized; following supervision revocation,

available credit for time served on supervision; following supervision revocation, availability of second release on parole or post-release supervision.

Mandatory sentencing laws: the offense(s) targeted by mandatory sentencing law; the trigger events that trigger the law; the impact of the law on the duration of the sentence for the underlying offense; the impact of the law on the judge's decision to alter the duration of the sentence imposed; the impact of the law on the judge's decision to impose incarceration; the impact of the law on release decisions.

Once the initial outline of policies/areas and their characteristics was completed, members of the Vera Institute of Justice's National Associates Program on State Sentencing and Corrections (SSC), a group of practitioners, politicians, and policymakers who have significant experience in sentencing reform at the state level, reviewed the outline and suggested minor changes in the characteristics detailed. The construction of the initial outline and review by SSC associates were completed in November 2002.

The outline was then used to construct an initial data collection instrument (DCI). The outline was converted into a structured survey instrument with specific questions addressing each of the characteristics addressed above. This survey instrument was created as a series of six forms/tables in Microsoft Access, a desktop microdatabase that served as the temporary repository for data collected. This microdatabase was loaded onto a laptop computer and allowed researchers to enter data directly into the database through Access Form view during data collection.

After the completion of the initial DCI microdatabase, the DCI was pilot-tested by collecting data on three states – California, Illinois, and New York – by Don Stemen, one of the principal investigators. These states were chosen because of the complex structure of their criminal codes and the large amount of secondary literature detailing their sentencing and corrections practices. The data collection on these three states involved examining the bound versions of each state's criminal laws and criminal procedure laws as amended in 2002 using the DCI. State codes as amended in 2002 were used in the initial examination because it was assumed that these versions of each state's code represented the most complex mixture of sentencing and corrections policies to date, with state codes becoming less varied as one moved back chronologically. Each state code was searched for the presence of each sentencing policy/area and the relevant characteristics of each policy for each state were recorded into the database; while state code indexes provided some guidance, the search generally involved reading the entire criminal law and criminal procedure sections of each state's code, locating the relevant policy, and recording information about the provisions of the policy into the DCI. To ensure inclusion of all relevant statutes, secondary sources were reviewed, including law reviews, reports by state-level professional legal organizations, and state government reports; these were used to supplement

the initial data collected from state codes. Construction of the initial DCI and data collection on three states was completed between December 2002 and February 2003.

After this initial pilot-testing, the DCI was refined. This involved reviewing the coding for each of the three states, evaluating the classification scheme developed in the DCI and used in the initial examination, and making changes to the DCI to reflect finer points of policy variation/characteristics or policy areas missed by the initial schematic. The final DCI was completed in March 2003. Appendix D contains a description of and coding instructions for entering data for each variable included in the DCI (Appendix G also contains a copy of the final DCI as it appears in Access form view).

The final DCI is comprised of six forms/tables in Microsoft Access:

Sentencing Structure, TIS, Credits, and Probation form. This form captures information on a state's general sentencing structure (felony classes, alternative sanctions, sentence ranges, etc), time served requirements for offenses, sentence reduction credits, and probation policies. Each state/year is entered as a separate case in the database.

Drug Policies form. This form captures information on drug sentences for simple possession and sale of cocaine, crack cocaine, methamphetamine, marijuana, and heroin.[84] For these two offenses and drug five types, data include statutory minimum and maximum sentences available and presumptive dispositions (presumptive prison or presumptive probation, if specified). Since many states increase penalties for different threshold quantities of drugs, the form also collects data on penalties for the first three drug quantity thresholds the state sets (if applicable); for states with more than three quantity thresholds, increased penalties are captured in the mandatory sentencing form. Each state/year is entered as a separate case in the database.

Post-Incarceration Supervision form. This form captures information on a state's provision for supervision after release from prison, including organization of supervision services (local versus state), length of post-release supervision term, amount of supervision fees, and revocation provisions. This includes all periods of supervision a state may require of offenders after release from prison, under either an indeterminate sentencing system (i.e. with discretionary parole release) or a determinate sentencing system (without discretionary parole release). Each state/year is entered as a separate case in the database.

[84] Other drug offenses, including manufacture or possession with intent to sell, and other types of drugs (e.g. LSD, PCP) are captured in the mandatory sentencing and enhancements section, only if such offenses carry a mandatory or enhanced sentence.

Habitual Offender Laws 1-2 form: This form captures information on a state's habitual offender laws, including offenses that trigger the law, the sentence available under the law, and the effect of the law on a judge's discretion in altering the length of the sentence and in imposing a term of incarceration. Since many states have more than one habitual offender law or have habitual offender laws that impose different penalties for different combinations of offenses, this form captures information on two different laws. The form provides a list of possible current offenses and prior offenses that may trigger the imposition of a habitual offender law. Each state/year is entered as a separate case in the database.

Habitual Offender Laws 3-4 form: This form captures information identical to that included in Habitual Offender Laws 1-2 form. It is included only for those states with more than two habitual offender laws.

Mandatory Sentences and Enhancements form: This form captures information on every mandatory sentencing law or sentence enhancement law in a state. Mandatory sentencing laws stipulate a mandatory sentence (mandatory incarceration, a mandatory minimum or fixed term, or a mandatory term added to the underlying offense) for all offenders convicted of a specified offense or convicted of an offense that involves a specific trigger event (e.g. possession of a firearm). Sentence enhancement laws increase the sentence range available for a specified offense or an offense that involves a specific trigger event, but do not require the trial court to impose an incarcerative term and may not require the trial court to increase the actual sentence imposed. The form collects information on the offenses to which the law applies, the "triggering events" to which the law applies, the sentence available under the law, and the effect of the law on a judge's discretion in altering the length of the sentence and in imposing a term of incarceration. Each mandatory sentencing and sentence enhancement law created by the state is captured as a separate case in the DCI for each year that it is in existence. Thus, while the unit of analysis in other forms is state/year, the unit of analysis in the Mandatory Sentences and Enhancements form is policy/year (i.e. the individual mandatory sentencing law for each year in existence).

In addition to the characteristics for each policy area, the DCI captures references to the section of each state's code from which the relevant information is derived. This ensured that coders and the principle investigators could easily return to the state codes and check the accuracy of the data collected or to reconcile any problems in coding. This will also allow other researchers and policymakers to use the DCI to quickly access particular policies in the states for future reference.

As noted, habitual offender laws and mandatory sentencing/enhancement laws apply to specific offenses and trigger events. Since definitions of offenses are not always uniform across states, we also created clear definitions of specific crimes and directions for coding such statutes. Appendix E contains the definitions used in the analyses.

Phase Two: Data Collection

Phase two consisted of state-level data collection for all fifty states for all study years, 1975 to 2002. 1975 was chosen as the cut-off year since, according to most criminologists and practitioners, most of the dramatic changes in state-level sentencing and corrections policies have occurred post-1975.

Data collection was conducted by the Principal Investigators and six research assistants – three law school students and three political science students. In order to ensure the accuracy of the data collected, several levels of quality control were employed. To establish coding consistency, research assistants attended a one-day training session, at which each item of the DCI was reviewed and a glossary of terms was distributed. Each assistant was then assigned one state for which to code data. The 2002 data for each state were coded by Don Stemen, the Co-Principal Investigator; before a research assistant began collecting data for a state, Mr. Stemen reviewed this initial 2002 coding for the state with the research assistant. The research assistant then coded the 2002 data for that state on their own using Mr. Stemen's initial coding as a template. After the research assistant completed the 2002 code for a state, Mr. Stemen met with them again to evaluate their coding. Any differences in coding were reconciled and instructions given to all coders to improve reliability. Coders then proceeded to code data for all state years for that state. At the conclusion of the coders' analyses of all study years, all results were checked again by Mr. Stemen to assure that all state years were complete. To standardize the process, any inconsistencies were adjudged by Mr. Stemen, the senior researcher responsible for the project. Data collection for all fifty states was completed by May 2004.

The final Access microdatabase version of the DCI described above was loaded onto a laptop computer for each of the research assistants. Each research assistant then entered data directly into the database through Access Form view during data collection. A separate Access microdatabase was created for each state; thus, each state had six tables addressing the six policy areas described above (Sentencing Structure, Drug Policies, Mandatory Sentences and Enhancements, Habitual Offender Laws 1-2, Habitual Offender Laws 3-4, and Post-Incarceration Supervision). Data for each state-year represented a separate case in each of the Access tables; for example, all sentencing structure data for New York in 1975 was one case in the Sentencing Structure table and all sentencing structure data for New York in 1976 was a separate case in that table. Changes in policies were then reflected by changes in data from the case "New York-1975" to "New York-1976." Thus, the unit of analysis was state-year. This was true for all data except that collected on mandatory sentencing policies. Data for each mandatory sentencing policy represented a separate case in the Mandatory Sentences and Enhancements table; for

example, data for a mandatory minimum for armed robbery in New York in 1975 was one case and data for a mandatory minimum for armed robbery in New York in 1976 was a separate case. Changes in policies were then reflected by changes in data from the case "armed robbery-1975" to "armed robbery-1976." Thus, the unit of analysis for mandatory sentencing policies was policy-year.

The policy analysis involved an in-depth legal analysis of each state's sentencing and corrections statutes. Data collection began by analyzing microfiche versions of state codes as amended in 1975; microfiche versions of superseded state codes (including supplements) and state sessions laws were then used to collect data on changes to each state's code for each year between 1975 and 2002. Data collection generally involved reading the entire criminal law and criminal procedure sections of each state's 1975 code, locating the relevant policy, and recording information about the provisions of the policy into the DCI. Annual code supplements were then analyzed to note changes to each state's code; when a revised version of the entire code was published, data collection then involved reviewing the entire criminal law and criminal procedure sections of each state's code again. Where changes to policies were unclear from annual supplements, microfiche versions of state sessions laws were consulted, which provided the actual legislation altering the code. This process continued until data collection reached 2002, and analysis turned to the bound versions of state codes as amended in 2002. To ensure the inclusion of all relevant data, secondary sources, such as law review articles, reports by state-level professional legal organizations, and state reports, were reviewed at the completion of each state-level coding. All data collection occurred using the bound versions of state codes and microfiche versions of state codes and sessions laws archived at New York University School of Law.

Appendix B describes the transfer and conversion process of state-level policy variables from the Access databases into SPSS and STATA for data analysis.

Appendix B: Variables and Database Construction

This Appendix describes the process for the procurement of control variables and database construction used in analyses of the impact of state-level sentencing and corrections policies on incarceration rates. The objective of the project was to examine the impact of different policies adopted by states between 1975 and 2002 on incarceration rates in each of the 50 states during that period.

Database Construction

We created a total of 50 Access databases – one for each of the 50 states – that housed data on state-level sentencing and corrections policies (see Appendix A). Next, we developed an SPSS syntax file to transfer the data stored in Access into a setting suitable for conducting statistical analyses. Each data point contained in the Access state files was transferred as a variable identified with a particular state and a particular year. From each individual state database we created five different SPSS datasets, one for each Access form— Sentencing Structure, Drug Policies, Mandatory Sentences, Habitual Offender Laws,[85] and Post-Incarceration Supervision. These modules were designed to serve as stand-alone pieces of information: each module had columns for "state" and "year," as well as the substantive variables describing a particular policy area. In the case of drug policies, for example, our SPSS database contains 195 variables describing this substantive area of interest; these 195 variables are available for every state-year in our dataset. The same is true with the other modules created as part of our data collection effort.

When transferring the information from Access to SPSS, we dealt with three different types of variables: in the majority of the cases we developed dummy variables corresponding to either "check boxes" in Access, such as the presence of probation supervision fees, or multiple-response items, such as the actual offenses that trigger mandatory minimums. The check boxes function as "yes/no" responses, capturing the presence or absence of a particular characteristic of a policy. Second, we transferred numerical variables accounting for information such as sentence ranges, quantity thresholds for drug offenses, and number of prior convictions triggering habitual offender laws. Third, we had a number of string variables referring to policy descriptions that we could not capture in the pre-designed coding instrument. In this category we included variables like the designations of felony or notes written by coders describing the working principles for the sentencing structure of each state. Our syntax routine assured that during our data transfer from ACCESS to SPSS we preserved variable labels and value labels as they were originally designed.

[85] There were two Access forms used for collecting data on habitual offender laws in the states; we combined these two forms into a single file in SPSS.

Not all the information contained in the Access databases was transferred into SPSS. In some cases we omitted the transfer of redundant variables or information with no statistical interest (such as the actual code reference for a particular policy). A list of the variables collected in the Access forms is available in Appendix D. In SPSS we conducted specific descriptive statistics to look at changes over time in independent variables within particular states. Despite the benefits of using SPSS for the transfer of data from ACCESS and its capabilities to generate summery statistics, this software package is not ideal for conducting pooled time-series analysis.

Thus, we built the final database using the STATA (version 8) software package. This is a standard computer program that enables a more sophisticated treatment of the data, while providing flexible estimation techniques and an efficient use of digital storage space and computing time. The data for our control variables was directly imputed into STATA while the information for our substantive area of work was transferred from SPSS. In STATA we put together the information of all 50 states into a single file. In the case of the sentencing variables, we combined the existing modules (five in total) for every state into a single set of "policy variables." The information was centralized with rows representing state-years and columns representing our set of variables. Depending on the requirements of the project, the information contained in specific variables was lagged one year (crime), two years (state revenues) or three year (correctional expenditures). The need for the lagging of variables implied the expansion of our study period to cover the period 1971-1975. In the case of our outcome variables (incarceration rates) unless otherwise indicated, every state-year was aimed to represent its actual value for the current year.

As was the case in the transfer from ACCESS to SPSS, the transfer from SPSS to STATA also meant a data reduction process by which we decided to work with a selected number of variables. The body of this report contains specific narratives for each one of these policy areas justifying its choice over other significant approaches to sentencing policies (also contained in our original ACCESS data collection instrument). (This list of sentencing variables used for the STATA version of the project's database is discussed below.)

An analysis of data collected by the National Corrections Reporting Program indicated that offenders serve an average of three years of incarceration. Thus, in terms of the analysis of incarceration rates, the database was filtered in order to separate data points by three-year intervals. This was necessary in order to approximate some assumptions of pooled time-series analysis (such as the independence of observations). As a result, the final version of the STATA database used for most of the analyses presented in this report contains state-level information between 1975 and 2002 effectively including 10 data points per state (a total of 500 observations).[86] There are alternative ways to filter the information; thus, the main data file contains a complete account of all state-years for the entire period from 1975-2002.

[86] For the analyses of non-policy control variables, determinate sentencing, and sentencing guidelines we included an additional observation (1972).

Appendix C describes the statistical methods used in the analyses of factors impacting state incarceration rates.

Outcome and Control Variables

The project involved a large effort in terms of data collection given its scope over time (27 years) and space (50 states).[87] It was also a significant effort given the number of control variables in the study (over 20) and the number of variables describing the substantive policy areas examined.

Outcome Variables

This project focused on the change and growth in state incarceration rates between 1975 and 2002. The incarceration rate refers to the number of sentenced prisoners serving one year or more under the jurisdiction of the state per 100,000 residents. The National Prisoner Statistics (NPS) series was employed to collect state-level incarceration rates between 1972 and 1975. For the years 1975 to 2002 we relied on several reports published by the Bureau of Justice Statistics (BJS). For the period 1975 to 1998, we downloaded several publications available at the BJS website. This data corresponds also to the figures included in the Correctional Populations series. From 1999 onwards, we relied on the supporting documents accompanying the "Prisoners in [year]" reports by BJS.

Control variables

This project collected information for non-policy variables found in previous studies to be associated with changes in incarceration rates (Table 1). Some of these variables were discarded from final analyses (e.g. correctional expenditures, total population). Data was collected for every state and every year included in the analysis. In some cases the information was available only for census years (e.g. GINI coefficient, religious adherents) or only compiled for a specific time frame (e.g. drug arrest rates since 1985). We describe the each variables and data sources below; we also note which variables were lagged in the final analyses.

[87] As noted above, in some analyses, our time frame was expanded to cover a longer time period (1972-2002).

Table B-1. Description of Control Variables

Variable	Unit
Violent Crime Rate	Rate
Property Crime Rate	Rate
Percent population between ages of 18-24	Rate
Percent population between ages of 25-34	Rate
Percent population African American	Rate
Percent population of Hispanic origin	Rate
Percent population living in urban areas	Rate
Percent adherents to "fundamentalist" religion	Rate
Income per capita	2002 dollars
Unemployment rate	Rate
Percent population below poverty level	Rate
GINI Income distribution coefficient	Index for households
State revenues per 100,000 residents	2002 dollars
Public welfare per 100,000 residents	2002 dollars
Police officers per 100,000 residents	Rate
Drug arrest rate	Rate
Corrections Expenditures per 100,000 residents	2002 dollars
Citizen political ideology	Index
Government political ideology	Index
Governor's party affiliation	Dichotomous
Region	Dichotomous

Violent crime rate: Violent crime rates for the period 1972 to 1998 were derived from the Uniform Crime Reports as compiled by the Bureau of Justice Statistics. For 1999 to 2002, data was collected directly from the "Crime in the United States" series published by the Federal Bureau of Investigation. According to the UCR Handbook, violent crimes are defined as murder, forcible rape, robbery, and aggravated assault. **1 year lag.**

Property crime rate: Property crime rates for the period 1972 to 1998 were derived from the Uniform Crime Reports as compiled by the Bureau of Justice Statistics. For 1999 to 2002, data was collected directly from the "Crime in the United States" series published by the FBI. According to the UCR Handbook, property crimes are offenses of burglary, larceny-theft, and motor vehicle theft. **1 year lag.**

Percent population 18-24 and 25-34: Data on the raw number of persons in each age group was collected directly from the Census Bureau website for the period 1980 to 2000. Data for the period 1970 to 1979 was obtained from the Census Bureau via disk. For the period 2000-2002 data was extracted from the Census Bureau website. **1 year lag.**

Percent population African American: Data was gathered for the years 1970, 1973, 1975 and 1976 from the Statistical Abstract and the Current Population Survey; we interpolated the values for the missing years between 1970 and 1976. Estimates for 1977 to 2002 were collected from the Census Bureau. **1 year lag.**

Percent population of Hispanic origin: Hispanic population was systematically reported by the Census Bureau only after 1980. For the period 1980 to 2000, data was collected directly from the Census Bureau website. For the period 1970 to 1979, several data interpolations were conducted using the data reported by the Census in 1976 (based on CPS data). **1 year lag.**

Percent population living in urban areas: This variable corresponds to the percentage of the population living in metropolitan areas and was collected from the Statistical Abstracts. Given that data is only updated every two years, proxies from adjacent years were employed when needed. **1 year lag.**

Percent adherents to "fundamentalist" religion: Fundamentalist religions were classified using Table 1 of Bible Belt Denominations from Heatwole, Charles (1978). "The Bible Belt: A Problem in Regional Definition." *Journal of Geography* 50-55. This table lists church bodies that "profess literal interpretations of the Bible." The number of persons with membership in a fundamentalist religion was collected from the census of *Churches and Church Membership in the United States* series (1970, 1980, 1990, 2000). Data was interpolated for the years where no information was produced.

Income per capita: Data on "personal income" was collected from the Statistical Abstract series (various years). Data was adjusted to 2003 constant dollars using the Consumer-Production Index. **1 year lag.**

Unemployment rate: (non-seasonally adjusted[88]). Refers to the number of persons unemployed per 100,000 residents and was collected from the Bureau of Labor Statistics website for years 1978-2002. Data for 1970 to 1977 were collected from the Statistical Abstract. **1 year lag.**

Percent of population below the poverty level: This variable refers to the percent of the resident population below the established poverty level.[89] Data was collected form the

[88] The "adjustment" of an unemployment time series is done to eliminate the effect of intrayear variations which tend to occur during the same period on an annual basis.

[89] The definition of poverty is complex. The Census Bureau uses a set of money income thresholds that vary by family size and composition to detect who is poor. If a family's total income is less than that family's threshold, then

Census Bureau website for the period 1979 to 2000. Information for 2001 to 2002 was calculated using the data published in the Statistical Abstract. **1 year lag.**

GINI: GINI represents an income inequality coefficient for households available only for Census years and was collected from the Census Bureau website. Data was interpolated for the years where no information was produced.

State revenue per 100,000 residents: Data on "total general revenue" was collected from the Statistical Abstracts (various years). Data was adjusted to 2002 constant dollars using the Consumer-Production Index. The denominator (Population) is the same population count used for previous ratio variables. **2 year lag.**

Public welfare per 100,000 residents: This variable corresponds to state's general expenditures on "public welfare" and was derived from Statistical Abstracts (various years). Data was adjusted to 2002 constant dollars using the Consumer-Production Index. The denominator (Population) is the same population count used for previous ratio variables. **1 year lag.**

Police officers per 100,000 residents: This variable refers to the total number of Full Time Equivalent state and local employees in "Police protection" functions as published by the Sourcebook of Criminal Justice statistics and posted on the Bureau of Justice Statistics website. Population estimates are consistent with the ones used for previous ratio variables. **1 year lag.**

Drug arrest rate: Number of drug arrests and total arrests were derived from "Crime in the United States" series published by the Federal Bureau of Investigation. This ratio was calculated using the arrest data stored by the National Archive of Criminal Justice Data (NACJD). Disaggregated information at the state level per offense type was only available between 1985 until 2001. For the period 1977-1985 we relied on data sent directly by the FBI-UCR division. Observations between 1970 and 1976 were assumed constant and equal to the observed values in 1977. **1 year lag.**

Corrections Expenditures per 100,000 residents: Expenditures in corrections refers to "total direct expenditures" on "corrections" for state and local corrections agencies

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that family, and every individual in it, is considered poor. The poverty thresholds do not vary geographically, but they are updated annually for inflation with the Consumer Price Index (CPI-U). The official poverty definition counts money income before taxes and excludes capital gains and noncash benefits (such as public housing, medicaid, and food stamps). Data comes from the census website:http://www.census.gov/hhes/poverty/histpov/hstpov21.html

derived from the Sourcebook of Criminal Justice Statistic (various years). Data was adjusted to 2002 constant dollars using the Consumer-Production Index. The denominator (Population) is the same population count used for previous ratio variables. **3 year lag.**

Citizen political ideology: Citizen political ideology refers to the "conservativeness" of the state's citizens as reflected in voting patterns. The scale is 0 to 100, with 0 being the most "liberal" and 100 being the most "conservative." The variable was created by Berry, W.D., Ringquist, E.J., Fording, R.C., and Hanson, R.L. (1998). "Measuring citizen and government ideology in the American states, 1963-93." *American Journal of Political Science*, 41, 327-348. See also extensions. Data updated through 2002 as downloaded from the ICPSR website (March 2004). **1 year lag.**

Government political ideology: Government political ideology refers to the "conservativeness" of the state's government as reflected in parties of persons holding public office in the state. The scale is 0 to 100, with 0 being the most "liberal" and 100 being the most "conservative." The variable was created by Berry, W.D., Ringquist, E.J., Fording, R.C., and Hanson, R.L. (1998). Measuring citizen and government ideology in the American states, 1963-93. *American Journal of Political Scienc*e, 41, 327-348. See also extensions. Data updated through 2002 was downloaded from the ICPSR website (March 2004). **1 year lag.**

Governor's party affiliation: The party of the governor was classified only as Republican or Democrat. Information for the period 1975-2002 comes from the National Governors Association website (www.nga.org/governors/). Missing cases for some particular years were replaced by their actual values looking at specific governor's offices. **1 year lag.**

Region dummies: Region variables are dummies for each of four regions of the country. South: Alabama, Arkansas, Delaware, Florida, Georgia Kentucky, Louisiana, Maryland, Mississippi North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia. West: Alaska Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, Wyoming; East: Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont. Midwest: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin.

Policy variables

This project collected information on specific sentencing and corrections policies in each state between 1975 and 2002 (see Appendix A for a full description of policy data collected). For the

final analyses, we relied on only 14 policy variables: determinate sentencing, presumptive sentencing guidelines, voluntary sentencing guidelines, time served requirements, minimum and maximum sentences available for cocaine sale and possession, the number of factors that enhance sentences for cocaine sale, the presence of habitual offender laws triggered by one or two prior convictions, and mandatory sentencing laws for weapons use, offenses against protected persons, and offenses committed while in state custody. However, all of the policy variables transferred to our final STATA database are described below.

Table B-2. Description of Policy Variables

Variable	Unit
Determinate Sentencing	Dichotomous
Structured Sentencing	Dichotomous
Presumptive Sentencing Guidelines	Dichotomous
Voluntary Guidelines	Dichotomous
Presumptive Guidelines	Dichotomous
Time Served (all offenses)	Continuous
Time Served (violent offenses)	Dichotomous
Sentencing Enhancements	Continuous
Severity Levels	Continuous
Drug sentences	Continuous
Two-strikes Law	Dichotomous
Three Strikes Law	Dichotomous
Violent HOL	Dichotomous
Drugs HOL	Dichotomous
Mandatories with weapon trigger	Continuous
Mandatories with violence trigger	Continuous
Mandatories with victim trigger	Continuous
Mandatories with supervision trigger	Continuous
Mandatory Score	Continuous

Sentencing Structure

Determinate sentencing: Although the term determinate sentencing is applied to several types of sentencing schemes, we used the common definition of determinate sentencing to include only those systems without discretionary parole release (Reitz and Reitz, 1993; Tonry, 1987; BJA, 1996); in contrast, indeterminate sentencing include those systems with discretionary parole release. Determinate sentencing is a dichotomous variable coded "1" if a state has abolished discretionary parole release for most offenses and "0" if a state has not abolished discretionary parole for most offenses. **1 year lag.**

Structured Sentencing: Structured sentencing refers to a system providing some form of recommended sentences for offenses within a wider statutory sentence range. These systems include presumptive sentencing or sentencing guidelines. Structured sentencing is a dichotomous variable coded "1" if a state has some form of recommended sentences for most offenses and "0" if a state has no form of recommended sentences for most offenses. **1 year lag.**

Presumptive sentencing guidelines: Presumptive sentencing guidelines refers to a type of structured sentencing system consisting of procedures to guide sentencing decisions and a system of legally enforceable multiple, recommended sentences based generally on a calculation of the severity of the offense committed and the criminal history of the offender. The guidelines require a judge to impose the recommended (presumptive) sentence or one within a recommended range, or provide justification for imposing a different sentence. Presumptive sentencing guidelines is a dichotomous variable coded "1" if a state has presumptive guidelines for most offenses and "0" if a state has no presumptive guidelines for most offenses.

Voluntary sentencing guidelines: Voluntary sentencing guidelines refers to a type of structured sentencing system consisting of procedures to guide sentencing decisions and a system of non-legally enforceable multiple, recommended sentences based generally on a calculation of the severity of the offense committed and the criminal history of the offender. The guidelines may require a judge to provide justification for imposing a sentence different from the guidelines. Voluntary sentencing guidelines is a dichotomous variable coded "1" if a state has voluntary guidelines for most offenses and "0" if a state has no voluntary guidelines for most offenses.

Presumptive sentencing: Presumptive sentencing refers to a type of structured sentencing system consisting of a system of legally enforceable recommended sentences based solely on the severity of the offense committed; these are distinction from presumptive

sentencing guidelines. Statutes require a judge to impose the recommended (presumptive) sentence or one within a recommended range, or provide justification for imposing a different sentence. Presumptive sentencing is a dichotomous variable coded "1" if a state has presumptive recommended sentences for most offenses and "0" if a state has no presumptive recommended sentences for most offenses.

Drug Policy

Sentencing enhancement score (cocaine, heroin, marijuana): Data on twelve different sentence enhancements was collected for each drug in each state. These enhancements represent factors that may increase a sentence for the underlying offense if found by the jury at trial or by the judge at sentencing. Among these specific considerations, we coded enhancements based on: location of the offense (selling/possessing drugs near a school, park, public housing complex, or church), excessive quantities of drugs involved, offenses involving minors, weapons use, and gang activity. In order to get a measure of "coverage" and "severity" we created a score for each state for each year by assigning a value of 1 to sale-related enhancements, a value of 2 to possession-related enhancements, and a value of 3 to those related to both sale and possession. A separate score was calculated for each of the three substances – cocaine/crack, heroin, and marijuana. A score foe each substance was recorded separately in the dataset.

Severity levels for possession and sale (cocaine, heroin, marijuana): we included variables measuring the number of quantity thresholds for possession and sale of cocaine, heroin and marihuana. A score for each substance and type of offense (sale or possession) was recorded separately in the dataset.

Minimum sentence for 28 grams of cocaine (sale): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses). When no minimum was establish we score this variable as "zero".

Maximum sentence for the lowest quantity of cocaine (possession): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses)

Minimum sentence for 28 grams of heroin (sale): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses).

Maximum sentence for the lowest quantity of heroin (possession): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses).

Minimum sentence for 500 grams of marihuana (sale): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses).

Minimum sentence for the lowest quantity of marihuana (possession): In months. Life sentences, if any, were coded as being equivalent to a sentence of 600 months (using 900 months did not change the outcome of the analyses). When no minimum was establish we score this variable as "zero".

Time Served Requirements

Time served (all offenses): Continuous variable measuring the percent of the sentence imposed that *most* offenders are required to serve before release from prison; since we control for determinate sentencing, the time served requirement is coded the same for either determinate or indeterminate sentencing systems, measuring the minimum percent of sentence most offenders must serve before release

Time served (violent offenses): a dichotomous variable indicating whether the state has a separate time served requirement for violent offenses; since all states define "violent offense" differently and apply time served requirements to different numbers of offenses, we did not create a continuous variable similar to that above. Rather, for Time Served (violent offenses), states with a separate requirement targeted directly at violent offenders are coded 1; states that have no separate requirement or that require all offenders to serve the same percent of the sentence imposed are coded 0

Habitual Offender Laws (HOL)

Two-strikes law: dichotomous coding variable representing the presence or absence of a particular piece of legislation dealing with the sentencing of habitual offenders. Each state was coded "1" if it had a habitual offender law that increased penalties for offenders with one previous conviction or incarceration and a "0" if it had no such provision (second-time offender).

Three-strikes law: dichotomous coding variable representing the presence or absence of a particular piece of legislation dealing with the sentencing of habitual offenders. Each

state was coded "1" if it had a habitual offender law that increased penalties for offenders with two previous convictions or incarcerations and a "0" if it had no such provision (third-time offender)

HOL targeted for violent offenses: dichotomous coding scheme signaling the presence (1) or absence (0) of habitual offender laws directed specifically at habitual violent offenders (regardless of the number of strikes). Offenses listed by the statute were highlighted as "violent" if they appeared explicitly listed among the "current" or "previous" offenses contemplated by the general HOL. The definition of violent offenses was consistent with the Uniform Crime Reports definition.

HOL targeted for drug offenses: dichotomous coding scheme signaling the presence (1) or absence (0) of habitual offender laws directed specifically at drug offenders (regardless of the number of strikes). The following list of drug offenses was used to generate the dichotomous coding: drug/narcotic sale; drug/narcotic possession; drug/narcotic manufacturing.

Mandatory Sentences

Number of mandatory minimums for weapons use: Number of mandatory minimum laws using "weapon" as trigger. We define the triggers in this case to be: a) Armed with a firearm; b) Use of firearm; c) Armed with a deadly weapon; d) Armed with an assault weapon/automatic weapon; e) Use of an assault weapon/ automatic weapon.

Number of mandatory minimums for violent offenses: Number of mandatory minimum laws using "harm" as trigger: We define the triggers in this case to be: a) Infliction of great bodily harm/injury; b) By force, violence or threat.

Number of mandatory minimums for offenses against protected individuals: Number of mandatory minimum laws using specific characteristics of the victims as triggers (for instance, "hate crimes"). We define the triggers in this case to be: a) Against a person of a specific age group b) Against a person with disability c) against a person because of race/ethnicity d) Against a person because of religious affiliation e) Against a person because of sexual orientation.

Number of mandatory minimums for offenses committed while in state custody: Number of mandatory minimum laws using violation to state supervision as triggers. We define the triggers in this case to be: a) Offender was on bail b) Offender was on parole c) Offender was on probation d) Offender was in prison/jail.

Mandatory Score: Continuous variable resulting from the addition of a set of the mandatory minimums created for the project's database (mandatory minimums for weapons, against protected individuals and for offenses committed while being on state custody).

Appendix C: Statistical Analyses

This Appendix describes the statistical analyses used in this project to examine the impact of state-level sentencing and corrections policies on incarceration rates.

Pooled Time-Series Cross-Sectional Design

To assess the influence of the control variables on changes in the outcome variable (incarceration rates), we employed a multiple time series or pooled time series cross-sectional design, combining data from all 50 states over 33 years (1970-2002).[90] This method is based our research/data collection design which collected repeated observations (T=years) of the same spatial units (N=states). Pooling different waves of data allowed us to work with a dataset composed of N*T observations (33 observations*50 units = 1,650 total observations).

This procedure has several advantages over standard time series or cross-sectional designs: it provides more degrees of freedom, permits evaluation of many separate changes in independent and dependant variables, reduces multicollinearity for some variables, and increases the precision of estimates by increasing the ratio of cases to variables. Indeed, given its effect on the total number of observations, the pooled approach has been specifically employed by researchers to provide more degrees of freedom when testing the relationship between a large number of independent and dependent variables. The pooled time-series design is also "historically contingent" (allowing the analysis of trends over time and the influence of nation-wide phenomena) (Jacobs and Carmichael, 2001), provides control groups (in that for each state the others act as controls), and allows control for missing variables that may cause differences between states. By pooling data, analyses have the ability to simultaneously model time and space and generalize across both dimensions. Conceptually, this is a very important step when examining historical data for different ecological units (e.g. states).

Despite the advantages of such an approach, the interpretation of pooled regression coefficients is not straightforward. Specifically, often coefficients cannot be employed as those generated by a Ordinary-Least-Squares (OLS) estimation; results for cross-sectional analyses do not necessarily hold when pooling data for several years. In addition, pooled models often violate some of the OLS assumptions such as homoskedasticity and independence. As mentioned by Podesta (2002), OLS estimators are often biased, inefficient, or inconsistent when applied to pooled data.

In addition to estimation challenges, it is important to account for research design issues when describing the methods employed in this report. For instance, the models explored here

[90] Information on state policies was collected only for the period 1975-2002; all control variables were collected for the period 1970-2002. For the analyses of social forces (Chapter Two), determinate sentencing (Chapter Three), and structured sentencing (Chapter Four), the models include data for the entire period 1970-2002. For analyses of other policies (Chapters Five, Six, Seven, and Eight) and final time interactions (Chapter Nine), the models include data only for the period 1975-2002.

assume that all predictors are exogenous; in other words, the models assume that predictors may influence incarceration rates, but that incarceration rates cannot influence the predictors. The lagging of all our covariates was consistent with this approach. However, this model specification is far from ideal. While some research has shown that the estimation of the feedback loop between incarceration and its covariates does not change the outcomes of the analysis for certain variables (Greenberg and West, 2001), alternative estimation procedures should be explored. Given the characteristics of this project's dataset, additional analysis can be employed to model non-recursive associations between variables, as well as the examination of change in incarceration rates.

The common techniques for analyzing panel data are "fixed effects" and "random effects" models (Hsiao, 1986; Mundlak, 1978; Pindyck and Rubinfeld, 1991). The primarily differences between these two approaches are: 1) the particular set of assumptions that each one makes about the form of the covariance matrix produced by the analysis and 2) the treatment given to omitted variables. In either case, dummy variables are included for each state and each year in the general working database. These partly control for variables not entered in the analysis. Coefficients associated with state dummy variables estimate the influence of specific factors unique to that state and year coefficients estimate the influence of factors unique to each year but common across states.

The fixed effects model is an estimation procedure that maximizes the fit of the models over time within states (as opposed to maximizing the overall fit using a between-states estimator). Fixed-Effects models use OLS estimators calculated from the pooled set of observations. In fact, the parameters estimated are the same as the ones obtained from a purely OLS regression with only the space (state) dummies—this is, a standard analysis of variance. When including both time and space dummies, the model becomes a two-way ANCOVA. However, one of the major shortcomings with the Fixed-Effects approach involves the modeling of omitted variables as dummies. By following this procedure, Fixed-Effects models are limited in their ability to estimate the effect of predictors that are constant over time because these become perfectly collinear with the unit-dummies. Similarly, the estimation of parameters with small variance may tend to be inaccurate. Fixed-Effects models are also criticized because they use a significant number of degrees of freedom, which may undermine the increase in sample size achieved by pooling several waves of data.

Despite these characteristics, Fixed-Effects models yield generally accurate predictors. The inclusion of state and year dummies decreases the likelihood of specification bias in the estimations. The inclusion of these predictors (to control for omitted variables) is easy to explain to non-specialists and very frequently used in comparative research (Beck and Katz, 1995, 1998; Hicks, 1994). Since this project has no interest in generalizing its results, it is not a problem that Fixed-Effects models are dependent on the characteristics of the sample selected (Random-Effects models do not need to meet this limitation). As Jacobs and Carmichael (2001) note,

stronger claims can be made with this design, since year and state dummies are controlling for all unobserved patterns in the data.

We also explore Random-Effects models as an alternative to account for possible heterogeneity in the data. While Fixed-Effects models assume that the unobservable state-specific effects are fixed, Random-Effects models assume these effects as random and independent of the regression residuals. As mentioned by Johnston and DiNardo (1997), the underlying concept is that the unit-effects are uncorrelated with the set of predictors in the models. The random effects model uses a General Least Squares (GLS) estimator producing a weighted-average of the between and the within estimators[91]. The GLS estimates with homoskedastic panels and no auto-correlation are very similar to the OLS estimates.

One of the important advantages of this estimation procedure is its ability to account for time-invariant predictors (given that the unit-effects are modeled randomly and not as dummies that can be collinear with these predictors). Random-Effects models also assume that observations are drawn from a distribution and therefore measurement error may be present. Given this specification, Random-Effects estimates may be more efficient and more robust that Fixed-Effects when there is measurement error. In this project, however, our data points are not coming from a given distribution of state-years. Our units (states) are fixed and inferences are only conditional to this sample which is the same as the universe. Our number of time-invariant predictors is also fairly limited; instead of taking census years, for instance, we relied on inter-censal estimates. We also interpolate between estimates when no other approximation was possible (for instance, with the percent black between 1975 and 1980). In some other cases we assume a constant number for the missing years (drug arrest rate between 1975 and 1978). More details can be found on the data description in appendix B.

Model Specification

Data was collected for all variables between 1970 and 2002. However, the pooling of waves of data for the pooled analysis was done selecting observations every three years, due to the nature of our outcome variable: state incarceration rates. Incarceration rates are defined as the number of sentenced prisoners serving one year or more under the jurisdiction of the state per 100,000 residents. Thus, annual incarceration rates likely involve some overlap between years given that a significant number of prisoners remain under correctional supervision for more than a year. Failing to account for this would lead to the multiple counting of a portion of the prison population.[92] Analyses of time served data from the National Corrections Reporting Program (NCRP) indicated that the average time served for non-violent offenses was slightly less than three years throughout the study period. Selecting observations every three years guarantees the

[91] Actually, the RE approach takes both the between and within estimators and converges to Fixed effects at infinitum). There is a rather unexplored Maximum-Likelihood approach to the estimation of these models.

[92] In addition to the clear need in terms of design to separate observations, this is also a requirement often violated when research address pooled time-series (see e.g. Nicholson-Crotty, 2004).

independence of observations. As a result, our final dataset consists of N=50 and T=11, which makes it "cross-sectional dominant" with a total of 550 cases of "state-years." As mentioned in the data description appendix, incarceration rates where entered in the models for the actual year they were collected. Most of the predictors have one-year lags (although there are a few with two-year lags, such as state revenues).

The models were run using STATA. Fixed-Effects models were run using the XTREG, FE command and Random-Effects were run using the XTREG, RE command. Other, more specific procedures were developed using the STATA manuals as guidelines (specifically the handbook on cross-sectional time series). In a few circumstances, user-created commands were employed (such as XTGRAPH, XTSERIES or XTTEST3).

Dependent Variable

Our models use as the dependent variable the state's incarceration rate. We also examined the logged transformation of the incarceration rate. In both cases we conducted several tests in order to examine the distribution of the outcome. According to the Skewness-Kurtosis test [SKTEST] we reject the hypothesis that incarceration rates are normally distributed (Adj Chi2=54.62, $p<.001$). This result was confirmed using the Shapiro-Wilk test [SWILK] and the Shapiro-Francia tests [SFRANCIA].

Since the assumption of normality was violated and we did not have a large number of observations, we needed to use nonparametric methods that employ an empirically-based distribution (sometimes called a "distribution free" procedure). The Kolmogorov-Smirnov tests [KSMIRNOV] whether or not the distribution of an interval or ratio variable is the same across units or groups. A series of Kolmogorov-Smirnov tests showed that the residuals for our baseline model (chapter 1) presented a positively skewed distribution, although in the case of the natural log computations residuals were approximately normal. When using a fully-specified model (chapter 8) the differences between the log and the unlogged distribution of residuals tend to disappear. In fact, residuals from the logged version of incarceration rates presented a more serious skewness than those of the raw incarceration rate. A similar situation has been reported elsewhere (Greenberg and West, 2001). In consequence, we decided to use the results form the unlogged version of incarceration rates throughout this report.

Model estimation

In all our preliminary analyses of each of the separate policy variables, the baseline model employs a Fixed-Effects estimation procedure. This perspective is appropriate given our approach to the data (each section constitutes a new set of variables). For the final version of each section's models we presented both Fixed and Random-Effects estimators which were useful to confirm or debate the findings arising from the baseline models. In addition, given our utilization of inter-census estimates, we needed to consider the presence of measurement error for some variables (Jacobs and Carmichael, 2001).

In order to see the appropriateness of the Random-Effects models, two different tests were conducted: the Breusch-Pagan Lagrange Multiplier test for random effects [XTTEST0] and the Hausman specification test [HAUSMAN]. The Breusch-Pagan Lagrange Multiplier test is similar to an F test in terms of the influence exerted by large samples in the rejection of the null hypothesis. In this case, the null hypothesis is that the random effects are equal to zero (i.e. there is no random error component in the model). Failing to reject this hypothesis would suggest that the Random-Effects are not needed. For our baseline model in chapter 2 we strongly rejected the null ($chi2(1)=182.97$, $p<.001$.) This result was consistent throughout the study.

The Hausman specification test [HAUSMAN] tests the null hypothesis that Random-Effects coefficients and Fixed-Effects coefficients are the same. The Hausman test is also used to assess problems of misspecification in the models. This procedure is not free from shortcomings since Hausman tests with large samples may tend to offer support for rejecting the null hypothesis when rejection may be due to misspecification. For our baseline model (Chapter Two), an examination of the model yields a Chi2 statistic of 32.93 which indicated that we failed to reject the null ($p<.05$) and therefore, no statistical differences exist between the two models examined (see Exhibit C-1). Given the choice of no difference between procedures we focus on the interpretation of Random-Effects estimators through this report since they are more efficient than the Fixed-Effects estimators. This result was consistent through the study.

Exhibit C-1 Hausman Test for baseline model (Chapter 2)

Variable	Fixed-Effects	Random Effects	Difference	Square Diag. SE
Violent Crime	0.108	0.110	-0.002	0.018
Property Crime	0.002	0.001	0.001	0.003
% pop 18-24	1.887	2.038	-0.151	2.163
% pop 25-34	2.754	3.532	-0.777	0.946
% Black	11.822	4.408	7.414	3.685
% Hispanic	8.101	1.888	6.213	1.841
% in SMAs	-0.139	-0.241	0.102	0.471
% religious fundamentalist	8.102	1.607	6.496	2.828
Income per capita	-0.009	-0.006	-0.003	0.001
Unemployment rate	1.246	2.683	-1.437	0.674
Poverty rate	-4.906	-5.067	0.161	0.351
Gini	171.404	479.010	-307.606	129.032
Revenues per 100k pop (*1000)	0.000	0.000	0.000	0.000
Welfare per 100k pop (*1000)	-0.001	-0.001	0.000	0.000
FTE Police per 100k pop	0.166	0.136	0.029	0.024
Drug arrest rate	571.456	577.663	-6.208	55.206
Governor (Republican)	13.959	14.022	-0.063	0.653
Citizen political ideology	0.144	-0.089	0.234	0.167

Note: Year dummies omitted.

Analyses

This report focuses on the analysis of the partial regression coefficients produced by both Random-Effects and Fixed-Effects routines. Fixed-Effects models were included in the final version of each chapter's models because we consistently found that state effects were significant ($p<.001$). Modeling these effects as either random of fixed yields no statically different coefficients according to our Hausman test; however, substantively, results may be interpreted in different ways. It is also important to note that while our models provided high goodness of fit measures, there is a strong reliance on our year and state dummies (for the Fixed-Effects models). The fact that these dummies were significant for most of our static analyses (without time interactions) suggests that there are trends between and within states that we did not accounted for in our models and that remain to be explored.

In the following tables, we re-estimate our baseline policy model (Chapter Three) and full policy model (Chapter Nine) accounting for different ways to model the likely presence of heteroskedasticity and serial autocorrelation in our dataset. Initially, we use a population-averaged approach as employed elsewhere (Jacobs and Carmichael, 2001) [XTREG, i(.) PA]. This procedure yields robust estimates of variance that are translated into smaller standard errors. Compared to results for the Random-Effects model presented in Chapter Three, coefficients are practically the same (Wald $Chi2(29)=2931$, $p<.001$). This result holds once the complete model is specified (Chapter Nine). Table C-3 presents these results (Wald $Chi2(42)=3149$, $p<.001$).

Exhibit C-2 Population-Averaged Estimation of Baseline Policy Model (Chapter Three)

Variable	b	SE
Violent Crime	0.112***	0.030
Property Crime	0.002	0.005
% pop 18-24	1.487	4.261
% pop 25-34	3.709	2.654
% Black	4.474***	0.953
% Hispanic	2.043*	0.911
% in SMAs	-0.226	0.308
% religious fundamentalist	1.454	0.858
Income per capita	-0.006***	0.002
Unemployment rate	2.253	1.934
Poverty rate	-5.065***	1.378
Gini	472.626	267.670
Revenues per 100k pop (*1000)	0.078**	0.001
Welfare per 100k pop (*1000)	-1.1350***	0.021
FTE Police per 100k pop	0.131*	0.061
Drug arrest rate	566.433***	146.931
Governor (Republican)	13.712**	4.980
Citizen political ideology	-0.098	0.289
Determinate Sentencing	-17.680*	8.311
1975	-4.516	13.596
1978	21.063	15.427
1981	42.015*	19.060
1984	75.615***	19.079
1987	104.896***	22.162
1990	146.666***	23.853
1993	219.490***	24.072
1996	287.007***	25.905
1999	337.095***	27.409
2002	356.753***	28.512
Constant	-136.601	109.506

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

Exhibit C-3 Population-Averaged Estimation of Full Policy Model (Chapter Nine)

	b	SE
Violent Crime	0.049	0.031
Property Crime	0.003	0.005
% pop 18-24	6.120	4.274
% pop 25-34	5.775*	2.604
% Black	3.810**	1.162
% Hispanic	2.532*	1.105
% in SMAs	0.082	0.364
% religious fundamentalist	3.563**	1.103
Income per capita	-0.005**	0.002
Unemployment rate	1.424	1.961
Poverty rate	-3.880**	1.407
Gini	377.884	280.252
Revenues per 100k pop (*1000)	0.059*	0.022
Welfare per 100k pop (*1000)	-0.803***	0.218
FTE Police per 100k pop	0.142*	0.058
Drug arrest rate	466.509**	145.747
Governor (Republican)	12.456*	4.809
Citizen political ideology	-0.025	0.302
Determinate Sentencing	-7.264	11.456
Presumptive Guidelines	-4.892	18.082
Voluntary Guidelines	5.373	12.378
Determinate * Presumptive	-62.387*	24.691
Determinate * Voluntary	17.762	22.304
Time Served (all offenses)	0.162	0.133
Time Served (violent offenses)	18.153*	7.086
Cocaine enhancements	3.458**	1.256
Cocaine Possession Maximum	-0.502***	0.091
Cocaine Sale Maximum	0.007	0.018
Cocaine Possession Minimum	0.392**	0.135
Cocaine Sale Minimum	-0.238	0.123
2-strikes Law	-4.214	8.710
3-strikes Law	8.368	7.868
Mandatory score	2.223**	0.644
1978	14.097	10.398
1981	25.629	13.337
1984	55.077***	15.223
1987	82.852***	17.443
1990	117.347***	19.785
1993	186.871***	22.416
1996	245.964***	25.498
1999	290.104***	28.384
2002	302.396***	30.250
Constant	-245.622*	118.572

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

A general test for serial autocorrelation in panel data has been recently developed by Wooldridge (2002) (see also Drukker, 2003) [XT SERIES]. This procedure applies regardless of the Fixed-Effects or Random-Effects estimation procedure. The null hypothesis is that there is no

serial correlation in the data examined. The issue of serial autocorrelation in panel data is important because it can bias the computation of standard errors. We tested our models for serial autocorrelation using Wooldridge's procedure, assuming that our observations are consecutive over time (as opposed to collected in three-year intervals). When this procedure is implemented in our baseline non-policy model we strongly reject the null of no autocorrelation in our data ($F_{1,49}=144.26$, $P<.001$). Results hold for the full policy model (Chapter Nine) ($F_{1,49}=137.37$). Exhibit C-4 and C-5 present the results for the baseline Random-Effects model presented in Chapter 2 taking into account an auto-regressive process of order 1 (Baltagi and Wu, 1999) [XTREGAR, RE].

Exhibit C-4 Random-Effects Full Non-Policy Model (Chapter Two) with Auto-Correlation Correction

Variable	b	SE
Violent Crime	0.041	0.024
Property Crime	-0.001	0.004
% pop 18-24	2.372	3.440
% pop 25-34	3.057	2.049
% Black	5.011***	0.985
% Hispanic	1.999*	0.893
% in SMAs	0.064	0.288
% religious fundamentalist	2.277*	0.927
Income per capita	-0.002	0.002
Unemployment rate	2.640*	1.281
Poverty rate	-0.838	0.760
Gini	-107.176	217.321
Revenues per 100k pop (*1000)	0.024	0.014
Welfare per 100k pop (*1000)	-0.452*	0.184
FTE Police per 100k pop	0.079*	0.039
Drug arrest rate	0.264*	0.130
Governor (Republican)	6.379	3.151
Citizen political ideology	-0.040	0.212
1975	6.52	9.42
1978	23.47	12.49
1981	38.43*	16.35
1984	61.98***	16.52
1987	107.30***	19.92
1990	154.93***	21.81
1993	204.56***	22.31
1996	266.75***	24.16
1999	321.68***	26.22
2002	333.94***	28.00
Constant	-42.70	97.91
R within	.835	
R overall	.792	
N	544	

One-tail tests: * Significant p<.05 ** Significant p< .01 *** Significant p <.001

Results are somewhat similar with those presented in Chapter Two. However, there are some notable differences. When taking into account an auto-correlation disturbance of level 1, several variables become non-significant (violent crime, income per capita, and governor party). This may be due to the fact that the estimate of the auto-correlation is high (rho=.84) and the standard errors are larger than for the model without auto-correlation (Chapter Two). The model in Exhibit C-4 also shows a significant and positive relationship between higher unemployment rates and states with higher imprisonment levels. Other models studied in this report failed to find this relationship (in contrast, see Wallace (1981) and Greenberg and West (2001) finding positive and significant relationships). This approximation to the modeling of auto-correlation is far from ideal given its strict assumptions about the independence of Random-Effects from regression residuals and the set of covariates employed in the models.

An alternative way to fit cross-sectional time-series models when the disturbances are not assumed to be independent is to implement a regression with Panel-Corrected Standard Errors [XTPCSE]. According to the tests developed in this section, it is possible to model our dataset to fit the structure of its disturbances (heteroskedasticity and autocorrelation of level 1). This procedure can be implemented as a Prais-Winsten model in order to produce estimates that are conditional to the estimates of the correlation parameters. According to Beck and Katz (1995) this procedure yields smaller standard errors—and therefore more conservative estimates—than a more traditional Feasible Generalized Least Squares (FGLM) approach, especially when the dataset is cross-sectional dominant (i.e. more units than years). This particular approach provides an alternate estimation of pooled models when violations to the assumptions of residual independence and homoskedasticity are violated. For PCSE, STATA requires that panels be balanced (i.e. there are no gaps in the data).[93] Following Beck and Katz (1995), we estimate our full policy model (Chapter Nine) with a single auto-regressive parameter for all panels (see Exhibit C-4).[94]

Before presenting the results of this estimation procedure, it is important to note an additional set of tests that provided more information about the structure of the data we were analyzing. In particular, using our Fixed-Effects models, we ran several routines in order to check for cross-sectional independence of disturbances and groupwise homoskedasticity. These two characteristics need to be taken into account for the modeling of pooled data. In order to correctly specify the PCSE model, information from these two tests was required. For the model in chapter 9, for instance, we conducted the Breusch-Pagan LM test of cross-sectional independence (XTTEST2)[95]. This procedure examines whether the cross-state correlations of residuals are statistically different from zero. In the case of the fixed –effects model for chapter 9

[93] The PCSE routine presented here we assume that the data was collected for several consecutive years. The total number of state-years remains the same (550) from all the previous analyses.
[94] This procedure is different than then one employed by Nicholson-Crotty (2004). In his analysis, PCSE is used to estimated also auto-correlations for each individual panel. Here we estimate a single *rho* for all states.
[95] Model estimated here is Fixed-Effects with no dummies.

we were able to reject the null, meaning that there is cross-sectional correlation of residuals (chi2 = 2235 p<.001). Next we used a user-created STATA command that provides a modified Wald statistic that tests for the homoskedasticity of the spatial units in the models (i.e. whether or not the variance of residuals is the same for all states) [XTTEST3]. According to the results for model 9, this test revealed that we had groupwise heteroskedasticity in our data (Chi2(50)=582.02, p<.001). A similar routine was developed for each chapter and results were similar.

We use the information from the tests described above to specify the PCSE estimation of the full model presented in chapter 9. Model 1 presents the regression results with standard errors corrected by the fact that the disturbances for each observation are not independent. Specifically, the disturbances are set to be correlated between states over time. In model 2 we model the same disturbances as if they were specific to each state. Both models control for auto-correlation.

Exhibit C-4 Panel-Corrected Standard Errors for Full Policy Models (Chapter Nine) with Auto-Correlation Correction^

	Model 1		Model 2	
	b	SE	b	SE
Violent Crime	0.036	0.025	0.036	0.027
Property Crime	0.006	0.004	0.006	0.004
% pop 18-24	0.049	3.680	0.049	3.793
% pop 25-34	5.447*	2.232	5.447*	2.111
% Black	4.629***	0.758	4.629***	0.883
% Hispanic	1.809*	0.765	1.809#	1.031
% in SMAs	-0.067	0.266	-0.067	0.257
% religious fundamentalist	2.450**	0.759	2.450**	0.839
Income per capita	-0.002	0.002	-0.002	0.002
Unemployment rate	2.745#	1.512	2.745#	1.526
Poverty rate	-1.093	0.951	-1.093	0.948
Gini	57.867	224.523	57.867	222.739
Revenues per 100k pop (*1000)	0.000	0.000	0.024	0.019
Welfare per 100k pop (*1000)	-0.001***	0.000	-0.698***	0.196
FTE Police per 100k pop	0.105*	0.043	0.105**	0.038
Drug arrest rate	214.577	135.835	214.577#	125.501
Governor (Republican)	7.598*	3.776	7.598#	3.933
Citizen political ideology	-0.271	0.236	-0.271	0.232
Determinate Sentencing	-14.468	9.508	-14.468	10.121
Presumptive Guidelines	-10.602	18.010	-10.602	15.638
Voluntary Guidelines	-1.743	11.546	-1.743	11.474
Determinate * Presumptive	-11.812	23.427	-11.812	21.449
Determinate * Voluntary	41.893*	20.900	41.893*	19.243
Time Served (all offenses)	0.040	0.109	0.040	0.107
Time Served (violent offenses)	13.825*	6.773	13.825*	6.828
Cocaine enhancements	3.310**	1.118	3.310**	1.194
Cocaine Possession Maximum	-0.216**	0.077	-0.216*	0.089
Cocaine Sale Maximum	0.003	0.014	0.003	0.015
Cocaine Possession Minimum	0.119	0.123	0.119	0.123
Cocaine Sale Minimum	-0.070	0.105	-0.070	0.109
2-strikes Law	10.817	7.283	10.817	6.824
3-strikes Law	-7.096	6.717	-7.096	6.476
Mandatory score	1.320*	0.606	1.320*	0.616
1978	10.457	7.216	10.457	7.225
1981	15.738	11.235	15.738	11.362
1984	40.319**	13.296	40.319**	13.307
1987	71.982***	16.269	71.982***	16.121
1990	111.007***	18.268	111.007***	18.209
1993	165.870***	19.867	165.870***	19.958
1996	226.015***	22.425	226.015***	22.431
1999	280.588***	24.768	280.588***	24.736
2002	297.795***	26.345	297.795***	26.398
Constant	-99.646	104.893	-99.646	107.206
R2	.673		.674	
N	492		492	

One-tail tests: # Significant <.1 * Significant p<.05 ** Significant p< .01 *** Significant p <.001; ^Model 2 is not technically a PCSE model, but rather a "heteroskedasticity-corrected SE" model

As Exhibit C-4 shows, there is a drop in the R-square from 0.87 in Chapter Nine (Random-Effects) to 0.67 once we implement more stringent estimation procedures. The estimate of the autocorrelation parameter in both models was lower than in the case of the regression with controls for only the auto-regressive patterns in the data (*rho*=.68 vs. .84). Second, we note that the size of both regression coefficients and standard errors tend to be smaller. This is particularly noticeable in the case of the policy variables. Overall, results presented with this estimation procedure tend to confirm the general observations made in Chapter 9. However, there are some noticeable differences: we observe a positive, non-significant association between 2 strikes laws and higher incarceration rates (the coefficient for the 3 strikes dummy is negative and non-significant) as well as non-significant negative effects of our sentencing structure variables. Only the interaction term between voluntary guidelines and determinate sentencing remains significant ($p<.05$) once the PCSE approach is incorporated in the analysis. Lastly, among our stable social and political control variables, we observe that poverty rate, violent crime and state revenues, all drop out of the analysis, while keeping the direction of its association with the outcome.

Appendix D: Data Collection Instrument Coding Instructions

This Appendix describes the final data collection instrument (DCI) used to collect information on state-level sentencing and corrections policies. The data collection instrument was constructed as a series of six forms/tables in Microsoft Access – Sentencing Structure, Drug Policies, Mandatory Sentences and Enhancements, Habitual Offender Laws 1-2, Habitual Offender Laws 3-4, and Post-Incarceration Supervision. Data entry occurred through Access Form view, with a separate form for each of the six areas listed. Each form functioned as a structured survey instrument with specific questions addressing specific characteristics of each policy.

A separate Access microdatabase was created for each state; thus, each state had six tables addressing the six policy areas described above (Sentencing Structure, Drug Policies, Mandatory Sentences and Enhancements, Habitual Offender Laws 1-2, Habitual Offender Laws 3-4, and Post-Incarceration Supervision). Data for each state-year represented a separate case in each of the Access tables; for example, all sentencing structure data for New York in 1975 was one case in the Sentencing Structure table and all sentencing structure data for New York in 1976 was a separate case in that table. Changes in policies were then reflected by changes in data from the case "New York-1975" to "New York-1976." Thus, the unit of analysis was state-year. This was true for all data except that collected on mandatory sentencing policies. Data for each mandatory sentencing policy represented a separate case in the Mandatory Sentences and Enhancements table; for example, data for a mandatory minimum for armed robbery in New York in 1975 was one case and data for a mandatory minimum for armed robbery in New York in 1976 was a separate case. Changes in policies were then reflected by changes in data from the case "armed robbery-1975" to "armed robbery-1976." Thus, the unit of analysis for mandatory sentencing policies was policy-year.

The variables collected in each form and the instructions for coding data into the form are described below.

Sentencing Structure Form

State/Year

v001 ID – a unique ID must be entered for each state for each year of data. For example, if the state is New York and the data is for the year 1975, the ID would be NY1975, if the state is New York and the data is for 1990, the ID would be NY1990. [NOTE: This ID is the same on the following forms: Sentencing Structure Form, Drug Policies Form, Habitual Offender Laws Form, Post-Incarceration Supervision Form]

v002 and v003 State and Year – enter the state and the year for which data is being entered. These should correspond to the ID variable.

v003a1 – v003c Primary purposes of sentencing and Secondary purposes of sentencing – (Punishment, Rehabilitation, Deterrence, Incapacitation, Restitution, restoration, or reconciliation) Often states will clearly articulate the purposes of the criminal code or sentencing for violations of the code. For example, the code may state, "the purpose of incarceration is punishment;" if the state expresses a primary purpose or only one purpose of the criminal code or sentencing, check the appropriate box in v003a1 through v003a4. If the state lists one or several purposes without expressing them as the *primary*, check all appropriate boxes in v003a1 through v003a4; for example, the code may state, "the purposes of incarceration are punishment, incapacitation and rehabilitation." Use v003b1 through v003b4 only if the state expresses the purposes of sentencing or the criminal code in terms of primary and secondary purposes; for example, the code may state, "the primary purpose of sentencing is punishment. Other purposes include rehabilitation and deterrence." In v003c enter the section of the code listing the purposes of sentencing or the criminal code.

Sentencing Structure

V004 and v004a Sentencing scheme – (Determinate, Indeterminate, Determinate/Indeterminate Combination) the state may not clearly define what type of sentencing structure it employs; in such instances, you may have to wait until you determine if the state has discretionary parole release, the factor that differentiates determinate and indeterminate systems. [NOTE: see *Policy Definitions* section for definitions of determinate and indeterminate sentencing.]

a. *determinate sentencing* – a system without discretionary parole release
b. *indeterminate sentencing* – a system with discretionary parole release
c. *determinate/indeterminate combination* – a system with discretionary parole release for some offenders but not for others.

V004b If the state uses both determinate and indeterminate sentencing… – list the offenses that receive either determinate or indeterminate sentences; in most instances, most offenses in the state will receive one type of sentence and a small number of offenses will receive the other type of sentence; list the smaller number of offenses first and simply state, "all other offenses receive [determinate or indeterminate] sentences."

v005 Number of felony classes – the total number of felony classes into which offenses are statutorily divided (e.g. Class 1 felony, Class 2 felony…); if the state defines murder or another offense as its own felony class, count it as a separate felony class. Not all states divide offenses into felony classes; rather, some states may simply define each offense separately and not attempt to group offenses into classes; in such instances, enter a "0" in v005.

v006 Felony class designations – list felony classes in descending order of severity; if murder is a separate felony class, be sure to list it. (e.g. Class 1 Felony – Class 4 Felony; or First Degree Felony – Fourth Degree Felony; or first degree murder, Class A felony – Class F felony).

v007 – v0013b Possible felony dispositions trial court may impose – (Restitution, Community Service, Periodic Imprisonment, House Arrest, Electronic Monitoring, Intensive Supervision/Probation, Drug Treatment, Boot/Work Camp, Split Sentence, Other) list all possible types of sanctions that the trial court can impose at the time of sentencing; do not include here types of sanctions that may be imposed at some later date by a probation department or corrections department. This question is concerned only with sanctions that can be imposed by a trial court. If all possible dispositions are listed in the same section of the code, simply copy the same code reference into each applicable code reference variable. If the court may impose a sanction not listed here, check the box next to "other" and briefly describe the sanction in v013b.

v015 and v015a Sentence range for each offense set by – states will set incarcerative sentence ranges for offenses in one of two ways. For this variable, choose the response based on how the state sets the ranges for most, if not all, offenses. If the sentence range for each offense is set by the specific offense definition, leave v015a (code reference variable) blank.

a. *felony class* – the code sets the same sentence or sentence range for all offenses within a particular felony class (e.g. burglary is a Class A felony; all Class A felonies are subject to the same sentence range, 5 to 10 years).
b. *specific offense definition* – the statute defining each specific offense also sets the sentence or sentence range for that offense (e.g. the statute defining burglary sets the sentence for that offense at 5 to 10 years).

v016 – v017 Imprisonment term set by statute – For v016, choose the response based on how the state sets the ranges for most, if not all, offenses; for v016a, enter the code reference for that section of the code where sentence ranges are established. If the sentence ranges for some limited number of offenses follow a different form (i.e. for most offenses, the code sets the minimum and maximum term, but for murder the code sets only the minimum term), list the offenses receiving different sentences and the form of the different sentence in v017. The range actually established for any offense will be in one of the following forms:

a. *maximum term* – the code sets only the maximum number of years that may be imposed for the offense (e.g. the code states that for burglary or Class A felonies, "the term of imprisonment may be no more than 10 years").

b. *minimum and maximum term* – the code sets both the minimum number of years that must be imposed for the offense and the maximum number of years (e.g. the code states that for burglary or Class A felonies, "the term of imprisonment may be no less than 5 years and no more than 10 years").
c. *fixed term(s)* – the code sets a fixed number of years that must be imposed for the offense or a number of fixed terms that may be imposed (e.g. in California, the trial court must impose 2, 4, or 6 years for the offense of burglary).

v018- v018a Imprisonment term set by trial court – For v018, choose the response based on how the trial court sets the ranges for most, if not all, offenses; for v018a, enter the code reference for that section of the code that establishes the type of sentence the trial court imposes. When the trial court imposes a sentence of incarceration on an offender, it takes one of the following forms:

a. *minimum term* – trial court sets only the minimum number of years that an offender must serve; the maximum term is automatically set at the statutory maximum for the offense; the number of years actually served is determined by discretionary release or parole up to the maximum allowed by statute (e.g. the trial court imposes a sentence of not less than 5 years for burglary; the parole board may release the offender or require the offender to serve up to the maximum allowed by statute).
b. *Maximum term* – trial court sets only the maximum number of years that an offender could serve (e.g. the trial court imposes a sentence of not more than 10 years for burglary); the number of years actually served is determined by discretionary release up to the maximum.
c. *minimum and maximum term* – trial court sets both the minimum and maximum number of years that an offender may serve (e.g. the trial court imposes a sentence of not less than 5 years and not more than 10 years for burglary).
d. *fixed term* – trial court sets a fixed number of years of imprisonment; (e.g. trial court imposes a sentence of 5 years for burglary); release is determined by either discretionary release agency (in indeterminate system) or by expiration of term (in determinate system).

v019 – v019f1 Imprisonment sentence ranges for each felony class – if the state sets imprisonment sentence ranges by felony class, enter the minimum and/or maximum incarcerative sentence for each felony class (if the statute gives a range). [NOTE: if the state sets a number of fixed terms (e.g. 2, 4, or 6 years for the offense), enter the minimum and maximum fixed terms in this section, make a note of this in the notes section at the end of the structured sentencing section, and make a copy of the page of the code setting the fixed terms.] [NOTE: if sentence ranges are set by specific offense definitions, enter general sentence ranges for most offenses and make a note in the notes section at the end of the structured sentencing section stating that sentence ranges represent estimates]. Put the sentence ranges for the most severe felony class first (including murder if treated as a separate felony class) and then list in descending order of severity; if the state has more than 5 felony classes, put the remaining classes and sentence ranges in v019f1.

v019a4 and v019b4 Parole Eligible – (yes/no) often, the most severe felony classes are not eligible for parole or are not eligible until the offender has served the entire sentence imposed; if offenders are not eligible for parole enter 'NO' in v019a4 or v019b4.

v020 – v020f1 Probation sentence ranges for each felony class – if the state prescribes probation sentence ranges based on felony class (not all states do this), enter the minimum and maximum sentence for each felony class. If the state sets only fixed terms of probation (e.g. all Class C felonies receive 2 years probation), enter the fixed period in v020a2 – v020e2 (i.e. maximum sentence length) and note the fact that these are fixed periods in the notes section at the end of the Structured Sentencing Section. If the most severe felony classes are not eligible for probation, leave the minimum and maximum sentences blank and enter 'not applicable' in v020XX.

v021 – v021f1 Other disposition sentence ranges for each felony – [NOTE: use only if the state sets ranges for another dispositional sentence (e.g. periodic imprisonment); if the state sets ranges for more than one other disposition (e.g. periodic imprisonment and house arrest), enter one of the dispositions here, make a note of the other disposition in the notes section, and make a copy of the page of the code setting the ranges for the other disposition]. If the most severe felony classes are not eligible for the other disposition, leave the minimum and maximum sentences blank and enter 'not applicable' in v021XX.

v022 – v022b Trial court must give reason for sentence imposed – (Always, Sometimes, Never) often, by statute, the trial court must articulate the reasons for the sentence imposed. This may be required in all instances, when the court does not impose a presumptive term or a term recommended by sentencing guidelines, or when the court finds aggravating or mitigating circumstances. If court must give reasons only "sometimes," briefly note when the court must give reasons in v022b.

v024 – v024c Can the judge change the sentence after sentencing – (yes/no) (Percent of sentence, number of months) often states give trial courts discretion to change an offender's sentence of imprisonment after sentencing; in such cases, the court generally can reduce the incarceration sentence or impose probation in lieu of incarceration. If the trial court retains such power, there is generally a time frame in which they can make such a change; enter this time frame in v024a and v024b.

life01-life07a – Can a life sentence by imposed for the following offenses? – if a life sentence is available for any of the listed offenses without the presence of a triggering factor (e.g. by firearm, etc.), check the appropriate box next to that offense type. Include all offenses in which a life sentence is possible; do not limit data only to those offenses in which a life sentence is mandatory or the only available sentence.

vic001 – Does state have a victim compensation fund? – (yes/no) enter "yes" if the state has some form of victim compensation fund available.

vic002 – Is input solicited from victims at sentencing? – (yes/no) enter "yes" if the state allows victims to address the court at any sentencing hearing; do not check yes if the state only allows victims to testify at capital sentencing hearing.

v025a Notes – enter any notes on sentencing structure here, including all data that did not fit into the previous variables.

Truth in Sentencing

We are using a broad definition of truth in sentencing to refer to any requirement or restriction on the percentage of the imposed sentence an offender must serve before release from prison. Truth in sentencing generally refers to the requirement that offenders serve 85% of the sentencing imposed; here, we are using it for any such time served requirement, even if it only requires offenders to serve 30% of the sentence imposed. [NOTE: see *Policy Definitions* section for definition of truth in sentencing.]

v034 – v034a Truth in sentencing – this is simply a check and code reference to the section of the code that states the percent or length of sentence offenders must serve.

v036 – v036a Truth in sentence accomplished by – Ensuring that offenders serve a certain length of time or portion of the imposed sentence in prison can be accomplished in three ways:

a. *Limitations on sentence reduction credits* – generally, this occurs in determinate sentencing states; an offender can earn sentencing reduction credits but only up until those credits reduce his/her sentencing by a certain percent; for example, an offender may earn credits to reduce his/her sentence by 15% (i.e. the offender must serve 85% of the sentence imposed).
b. *Parole restrictions* – in indeterminate sentencing states, parole restrictions often prevent the parole authority from releasing an offender until that offender has served a certain number of years or percent of sentencing imposed; for example, an offender may not be eligible for parole until he/she has served one-third of the imposed sentence (i.e. the offender must serve 33% of the sentence imposed).
c. *Mandatory sentence* – in both determinate and indeterminate states, offenders may be required to serve a set mandatory number of years regardless of the actual sentence imposed; for example, an offender may not be eligible for release or parole until they have served 5 years.

[NOTE: if the state employs indeterminate sentencing (i.e. discretionary parole release) and uses both limitations on sentence reduction credits and parole restrictions to determine percent of sentence that must be served, check the box for parole restrictions, since this will likely be the dominant determinate of release date; parole restrictions will set the initial date and sentence reduction credits will only reduce this parole restriction date.]

v037 – v037a Truth in sentencing requirement – (Same for all felonies, Varies among felonies) the required portion of the sentence that offenders must serve may be the same for all offenders (i.e. all offenders must serve 50% of the sentence imposed or all offenders are eligible for parole after serving 75% of the sentence imposed) or it may vary by the type of offense or offender (i.e. violent offenders must serve 85% of the sentence imposed and all other offenders must serve 30% of the sentence imposed).

v038 Required percentage of sentence for each felony class/offense – as concisely as possible, note the required sentences that offenders must serve expressed as a percentage of the sentence imposed or a definite number of years (if applicable). If both parole restrictions and sentence reduction credits reduce the sentence, express the required sentence only as that required before eligibility for parole not further reduced by sentence reduction credits; note that the parole eligibility may be further reduced by sentence reduction (e.g. state the required percentage as "50% less good time"). If possible group by felony class or by designated group (e.g. violent felonies).

v038b – Notes on truth in sentencing – enter any notes on time served requirements here, including all data that did not fit into the previous variables.

Sentence Reduction Credits

v039 – v039a Sentence reduction credits – note if the state allows offenders to accrue sentence reduction credits (also known as good time credits, earned time credits, good conduct credits, etc.) and note the code reference.

v040a1 – v040d1f Sentence Reduction Credits Table – sentence reduction credits are a mechanism for reducing the amount of time an offender must serve in prison or under the supervision of correctional authorities; sentences are reduced a certain amount each month or year that an offender is incarcerated.

Credit type
Check boxes for all applicable types of sentence reduction credits available to offenders:

a. *statutory* – credits are given to offenders generally based on "good conduct;" offenders do not have to actively do anything to receive statutory credits; the only requirement for accruing statutory credits is avoidance of disciplinary infractions.
b. *earned* – credits given to offenders for participation in education, work, treatment, etc.
c. *meritorious* – credits given to offenders at the discretion of corrections staff for performing exceptional acts, such as exemplary behavior in emergencies, donating blood, acts of heroism, etc.; generally, these are one-time awards of credit (e.g. 180 days credit for some act)
d. *emergency* – credits given to offenders when prison populations reach a certain capacity to accelerate discharge dates or parole hearings.

Amount of credits available determined by – the amount of credits available to offenders may be determined in two ways:

a. *fixed/predetermined by statute* – the statute authorizing credits also states the amount of credits offenders must be given (e.g. "offenders receive 1 day credit for each day served")
b. *at discretion of DOC* – the statute authorizing credits states only the maximum amount of credits offenders may earn and allows the DOC discretion to determine the actual amount the offender may earn (e.g. "offenders may receive up to 4 days per 8 days served").

Amount of credits awarded determined by offender's: all offenders may not all be eligible for the same amount of credits; often violent offenders may not receive as many credits as other offenders (for example, a state may award violent offenders 5 days credits per 30 days served and award all non-violent offenders 10 days credits per 30 days served). States may determine these awards based on different offender characteristics: offense/felony class, sentence length, time served, or institutional placement (i.e. the DOC makes an evaluation of offenders while incarcerated and places them in some offender category based on behavior, psychological interview, etc.).

If most inmates receive the same amount, number of credits available – List the number of credits that most offenders may earn or that one particular group of offenders may earn; enter the amount of credits others may earn in the notes variable at the bottom of the page. Express available credits as a certain number of days, months, or years per a certain number of days, months, or years (as expressed in the statute).

v041 – What do credits reduce? – (Minimum, Maximum, Total Sentence, Minimum and Maximum, Parole Eligibility) Sentence reduction credits may impact a sentence in different ways, reducing the maximum sentence imposed, reducing the minimum sentence imposed, or expediting parole eligibility. Check the box of the manner in which credits generally affect sentences. If more than one applies (e.g. total sentence and parole eligibility), check one box and make a note in the notes section.

v041a – v041d If the state awards earned credits, an offender may earn credits for participation in – (Work, Educational Program, Vocational Training, Treatment) often states award credits for participation in a program or for completion of the program. Note here only those programs for which offenders earn credits for simply participating. Check all that apply.

v042a – v042d If the state awards earned credits, an offender may earn credits for completion of – (Work, Educational Program, Vocational Training, Treatment) Note here only those programs for which offenders earn credits only for completing a program. Check all that apply.

v044 – Notes on sentence reduction credits – enter any notes on sentence reduction credits here, including all data that did not fit into the previous variables.

Probation

prob01- prob01a – Who administers probation supervision services? – (Judiciary, Department of Corrections, Autonomous State-Level Probation Department, An autonomous state-level department supervising both probation and parole)

prob02 – At what level of government are probation supervision services organized? (Local Level/State Level)

prob04 – Does the state allow probation supervision fees? (Yes/No)

prob04a – If yes, are probation supervision fees mandatory? (Yes/No)

prob004b1 - prob004b2 -- If yes to prob004, what are the fees? – enter the amount of the fee offenders are required to pay; enter the amount using the same units as the state. Thus, if the state charges a one-time fee, enter the dollar amount in the first variable and choose "one-time fee" in the second variables; if the state charges a monthly fee, enter the dollar amount in the first variable and choose "month" in the second.

prob06 – Can an offender be released from probation before the expiration of the probation sentence imposed by the trial court? (Yes/No) – check yes if the judge or the probation department can release the offender prior to completing the term of probation.

prob06b - prob06b1 – How much of probation sentence must an offender serve before being eligible for release? (e.g. 5 years or 50 percent) –

prob07 – Notes on probation – enter any notes on sentence reduction credits here, including all data that did not fit into the previous variables.

Mandatory Sentences and Enhancements Form

Mandatory sentencing laws stipulate a mandatory sentence (mandatory incarceration, a mandatory minimum or fixed term, or a mandatory term added to the underlying offense) for all offenders convicted of a specified offense or convicted of an offense that involves a specific trigger event (e.g. possession of a firearm). Sentence enhancement laws increase the sentence range available for a specified offense or an offense that involves a specific trigger event, but do not require the trial court to impose an incarcerative term and may not require the trial court to increase the actual sentence imposed; a sentence enhancement law, for example, may increase the statutory minimum or maximum for an offense or may add an additional term to the underlying offense. Each mandatory sentencing and sentence enhancement law created by the state is captured as a separate case in the DCI.

The mandatory sentence form must be filled out for every mandatory sentence or enhancement that the state has in place in a given year. If several offenses are covered by the same mandatory sentencing law or same section of the code, the form is set up to allow you to enter all of these offenses at once as one entry. However, make certain that all the offenses have the same triggering factor(s) (e.g. use of a firearm); only those offenses with the same triggering factor(s) may be grouped together.

For example, if the statute reads, "5 years must be added to the underlying term if the offender used a firearm during the commission of murder, rape, or robbery, or if the offender used a firearm in the commission of burglary and had previously been convicted of burglary." Murder, rape, and robbery may be grouped together and entered as one case. However, burglary cannot since it has different triggers; in this case burglary has two triggers – use of a firearm and previous conviction for burglary – and must be entered separately.

Identification
man002 – man003 State and Year – enter the state and year identifies.

man004 Mandatory sentence or enhancement ID – do not enter anything in this space; the ID automatically increases by one number for each mandatory you enter; it will increase every time you hit the key with the "*" next to it at the bottom of the screen.

man005 Mandatory sentence or enhancement code ref. – enter the code reference for the mandatory or enhancement.

Underlying Offenses

man006a-man006z1 – This is a fairly exhaustive list of possible offenses. Check all offenses to which the mandatory or enhancement applies. If the offense(s) is not listed, enter it in the final variable marked other offenses. If the mandatory applies to all "violent offenses," list the violent offenses in the notes section for the state.

Triggering Factors

man007a-man007z1 – Again, this is a fairly exhaustive list of triggering factors. Check all triggering factors that apply to ALL the offenses checked on the prior page (remember to group only those offenses with the same trigger(s)). For example, if there is a mandatory for all offenses committed while armed with a handgun within 1,000 feet of a school, check both armed with a handgun and proximity to school. When entering data in the "proximity," "quantity," etc. variables, use the language or measurements used by the state.

Structure of Mandatory Sentence or Enhancement

man008 Type of mandatory sentence or enhancement –

a. *Presumptive imprisonment of unspecified length* – the statute requires or presumes imprisonment for the offense except in unusual circumstances in which the judge may impose probation; further, the statute does not indicate a specific length of time which must be imposed. For example, in CA such statutes read, "incarceration shall be imposed for the following offenses, unless, on the court's finding, such incarceration

would be a grave injustice." Such language indicates a presumption for incarceration but does not make it mandatory.

b. *Mandatory imprisonment of unspecified length* – the statute requires the trial court to imprison the offender but does not specify the length of sentence that must be imposed; for example, "incarceration must be imposed for the following offenses:"
c. *Mandatory fixed term* – the statute specifies the exact number of years that must be imposed if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details). For example, the statute may read, "Offenders sentenced for the following offenses will receive a sentence of 5 years."
d. *Mandatory minimum term* – the statute specifies only the minimum number of years an offender must serve if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details). For example, the statute may read, "Offenders sentenced for the following offenses must receive a sentence of not less than 5 years."
e. *Increased minimum term* – the statute increases the minimum term that an offender must serve for the underlying offense if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details). For example, if the statutory range for burglary is 5 to 15 years, the mandatory/enhancement statute may read, "Conviction for burglary while armed with a firearm increases the minimum range from 5 years to 10 years."
f. *Increased maximum term* – the statute increases the maximum term that an offender must serve for the underlying offense if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details).
g. *Increased minimum and maximum terms* -- the statute increases both the minimum and maximum terms that an offender must serve for the underlying offense if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details). For example, if the statutory range for burglary is 5 to 15 years, the mandatory/enhancement statute may read, "Conviction for burglary while armed with a firearm increases the statutory range from 5 years to 10 years to 10 years to 30 years."
h. *Additional fixed term added to underlying offense* – the statute adds additional years to whatever sentence is imposed by the court for the underlying offense if the offender is incarcerated; however, the statute may not require the trial court to impose incarceration (see man010 for details). For example, the mandatory/enhancement statute may read, "For conviction for burglary while armed with a firearm, 5 years will be added to the term of incarceration imposed by the court."
i. *Increased offense class* – the statute increases the offense class for the underlying offense; for example, the mandatory/enhancement statute may read, "Conviction for burglary while armed with a firearm, increases the offense from a Class C felony to a Class B felony."

man009 Mandatory sentence or enhancement – simply indicate the sentence that the statute calls for. For example, "5 years" (for c, d, e, f, and h above), "5-10 years to 10-15 years" (for g above) or "2 felony classes" (for i above). If the mandatory/enhancement is of type a or b above, do not enter anything in this space.

man010a – Does the statute alter the duration of the sentence for the underlying offense? – (yes/no) check "yes" if the law changes the sentence range for the underlying offense; for example, the statute may read, "upon a sentence of incarceration, the maximum term of incarceration is doubled."

man010b -- If yes to man010a, does the statute require the judge to alter the duration of the underlying sentence? – (yes/no) check "yes" if the law changes the sentence range for the underlying offense and requires the judge to impose a term of incarceration longer than he/she would have been able to in the absence of the mandatory sentence or enhancement; for example, the statute may read, "upon a sentence of incarceration, the minimum term of incarceration is doubled." In such a case, the judge could not impose the minimum term for the underlying offense, since the law essentially changes the sentence range for the offense by doubling it.

man010c – Does the statute require the judge to impose incarceration? – (yes/no) check "yes" if the law requires the judge to impose a term of incarceration.

man011 – Does mandatory sentence or enhancement affect release date? – (yes/no) check "yes" only if the offender must serve the entire mandatory or enhancement imposed before release or if the offender is ineligible for release while serving some portion of the mandatory or enhancement.

man012 – Notes on mandatory sentence or enhancement – enter any notes on the mandatory sentence or enhancement here, including all data that did not fit into the previous variables. If possible, note what the sentence for the offense was in the absence of the mandatory sentence or enhancement. For example, if the mandatory increases the maximum sentence for the offense, note "increases term from 5-10 years to 5-20 years."

Habitual Offender Laws 1-2 and Habitual Offender Laws 3-4 Form

Habitual offender laws stipulate a mandatory sentence (mandatory incarceration, a mandatory minimum or fixed term, or a mandatory term added to the underlying offense) or sentence enhancement (increased minimum or maximum term) for all offenders convicted of a specified offense if previously convicted of a certain number or type of offense. For example, a habitual offender law may state: an offender convicted of a felony, if previously convicted of any two felonies, shall be sentenced to a term of incarceration up to twice the maximum term for the underlying offense.

Habitual offender laws vary in two primary respects: the number of prior offenses that may trigger the law and the type of current or prior offenses that may trigger the law. For example, a law may be triggered by one prior offense or two prior offenses. Further, such laws may be very specific, targeted only at offenders convicted of particular offenses; for example, an offender may be subject to a habitual offender law only if the prior offenses were violent or only if the current offense is violent. Thus, habitual offender laws may contain several variations depending on combinations of current and prior offenses. As a result, states may appear to have several habitual offender laws. Habitual Offender Laws 1-2 form contains spaces to collect data on two habitual offender laws for the state; Habitual Offender Laws 3-4 form contains spaces to collect data on an additional two habitual offender laws for the state. All forms are identical.

In each case, include in the notes section 1) whether the habitual offender law is triggered by prior convictions or prior incarcerations and 2) whether there is a time limit on when prior convictions/incarcerations must have occurred. If there is no time limit on when prior offenses must have occurred, note "no time limitations" in the notes section.

Identification

v001 – v003 ID, State and Year – a unique ID must be entered for each state for each year of data. For example, if the state is New York and the data is for the year 1975, the ID would be NY1975, if the state is New York and the data is for 1990, the ID would be NY1990. [NOTE: This ID is the same on the following forms: Sentencing Structure Form, Drug Policies Form, Post-Incarceration Supervision Form]

v026 – v026a Habitual offender law– this is simply a check and code reference to the section of the code that provides the habitual offender provisions.

v027 – Number of offenses required to trigger law (including current offense) – enter the number prior offenses needed to trigger the law plus the current offense; thus, if the law is triggered if the offender had two prior convictions, enter a 3 (2 prior convictions plus 1 current conviction = 3).

Qualifying Prior Offenses

hab001a- hab001z1: This is a fairly exhaustive list of possible offenses. Check all types of prior offenses that trigger the habitual offender law. If the offense(s) is not listed, enter it in the final variable marked other offenses. If the habitual offender law applies to all "violent offenses," list the violent offenses in the notes section for the state.

Qualifying Current Offenses

hab002a- hab002z1: Again, this is a fairly exhaustive list of possible offenses. Check all types of current offenses that trigger the habitual offender law. If the offense(s) is not listed, enter it in the final variable marked other offenses. If the habitual offender law applies to all "violent offenses," list the violent offenses in the notes section for the state.

Structure of Habitual Offender Law Sentence

v102 Sentence for habitual offender law– simply indicate the sentence that the statute calls for. For example, "5 years," or "increases offense by one offense level." If the habitual offender law imposes a different sentence based on the felony class of the offense, enter information for each felony class in the space provided; for example, "Class A – 50 years; Class B – 35 years;" etc.

v103a – Does the statute alter the duration of the sentence for the underlying offense? – (yes/no) check "yes" if the law changes the sentence range for the underlying offense; for example, the statute may read, "upon a sentence of incarceration, the maximum term of incarceration is doubled."

v103b -- If yes to v103a, does the statute require the judge to alter the duration of the underlying sentence? – (yes/no) check "yes" if the law changes the sentence range for the underlying offense and requires the judge to impose a term of incarceration longer than he/she would have been able to in the absence of the habitual offender law; for example, the statute may read, "upon a sentence of incarceration, the minimum term of incarceration is doubled." In such a case, the judge could not impose the minimum term for the underlying offense, since the habitual offender law essentially changes the sentence range for the offense by doubling it.

v103c – Does the statute require the judge to impose incarceration? – (yes/no) check "yes" if the law requires the judge to impose a term of incarceration.

v110c – Does DA have to invoke habitual offender law 1? – (yes/no) check "yes" if the law is triggered ONLY when the DA files motion; in some states, sentences under habitual offender laws are available when either the judge or the prosecutor make a motion.

v033b – Notes on Habitual Offender Law – enter any notes on the habitual offender law here, including all data that did not fit into the previous variables. If possible, note what the sentence for the offense was in the absence of the habitual offender law. Also note whether the law is triggered by prior convictions or prior terms of incarceration and whether there is any time limit placed on when prior convictions/terms of incarceration must have occurred.

Appendix E: Offense Definitions and Coding Instructions for Mandatory Sentencing and Sentence Enhancement Laws

This Appendix describes the offense definitions and coding instructions used to collect data on mandatory sentencing laws.

Mandatory Sentencing and Sentencing Enhancements

Sentence enhancements are statutes that alter the duration of the incarceration term for an offense but DO NOT affect the disposition of the sentence for the offense. In other words, sentence enhancements increase the length of the prison sentence that the trial court may impose but DO NOT require the trial court to impose incarceration. For example, suppose the sentence for burglary is 2 to 5 years and the trial court has the discretion to impose probation or prison; then suppose the sentence for burglary by use of a deadly weapon is 5 to 10 years and, again, the trial court has the discretion to impose probation or prison. Then burglary by use of a deadly weapon carries a sentence enhancement; "burglary" is the underlying offense and by "use of a deadly weapon" is the triggering factor.

The coding of sentence enhancements depends, however, on the definitions of the offenses. The factors that trigger sentence enhancements for some offenses (e.g. by use of a deadly weapon) are, in fact, elements in the definitions of other offenses and, thus, cannot always be used as enhancement triggers. The easiest example is simple assault and aggravated assault – two distinct offenses that share a base definition. Simple assault is defined as the intentional causation of bodily injury and carries a sentence of 1-3 years; aggravated assault is defined as the intentional causation of *serious* bodily injury or the intentional causation of any bodily injury by *use of a deadly weapon* and carries a sentence of 5-10 years. At first glance, it may seem that simple assault by use of a deadly weapon carries a sentence enhancement with "simple assault" as the underlying offense and by "use of a deadly weapon" as the triggering factor (similar to burglary above). However, this is incorrect since this is the definition of aggravated assault – a distinct crime. In this case, the definitions of simple assault and aggravated assault are quite similar – both involve the infliction of injury to another. However, they are distinct offenses, distinguished by the use of a deadly weapon to inflict such injury. In contrast, "burglary by use of a deadly weapon" is not an offense distinct from "burglary;" thus, burglary by use of a deadly weapon carries a sentence enhancement.

Sentence enhancements occur only when the underlying offense plus the triggering factors DO NOT equal the definition of another, distinct crime. For example, "burglary" + "use of a deadly weapon" ≠ another distinct offense, therefore there is a sentence enhancement. However, in the case of simple assault and aggravated assault, "simple assault" + "use of a deadly weapon" = "aggravated assault," therefore there is no sentence enhancement. The definitions of the offenses below will provide guidance on when particular triggering factors cannot be used to enhance a sentence for an underlying offense.

This is further complicated by the grading of offenses in some states. For example, some states do not have the offenses of simple assault and aggravated assault; rather, they have first, second, and third degree assault. Each degree of assault is distinguished by the presence or absence of some factor – generally, those factors that distinguish simple assault from aggravated assault in other states. Third degree assault, the least severe, may be defined as the infliction of bodily injury on another; second degree assault may be define as the infliction of serious bodily injury on another; finally, first degree assault, the most severe degree, may be defined as the infliction of bodily injury on another by use of a deadly weapon. As we described above, each of these factors – infliction of serous bodily injury or use of a deadly weapon – that the state uses to grade the offense from second and first degree assault should not be used as triggers to enhance the sentence for third degree assault; the presence of serious bodily injury or use of a deadly weapon are part of what we have defined as aggravated assault. While a particular state may have three kinds of assault, we are recognizing only two kinds of assault. In this particular case, third degree assault would be considered simple assault (infliction of bodily injury) and both second and first degree assault would be considered as one offense of aggravated assault (infliction of serious bodily injury or infliction of bodily injury by use of a deadly weapon).

In other instances, state may grade offenses like burglary, but may do so by using factors that we define as enhancement triggers. For example, a state may define second degree burglary as entering a building with the intent to commit a crime (a standard definition of burglary) and may define first degree burglary as entering a building with the intent to commit a crime by use of a deadly weapon. While this may look a separate offense, under our definitions of burglary, burglary by use of a deadly weapon is NOT a separate offense; rather it is an enhanced form of burglary and, if it carries a more severe penalty than burglary, must be coded as a sentence enhancement.

In the following sections we provide definitions for the offenses or assault, rape, kidnapping, robbery, burglary, arson, and theft. These definitions are based on Model Penal Code definitions of offenses and Uniform Crime Reports definitions of offenses. Based on these definitions, we then list those factors that can and cannot be used to trigger an enhancement sentence for each offense.

Offense Definitions

Assault

Simple assault is the actual or threatened intentional infliction of any bodily injury on another.

Aggravated assault is 1) the actual or threatened intentional infliction of serious bodily injury on another OR 2) the actual or threatened intentional infliction of any bodily injury on another with a deadly weapon.

Thus, we make the distinction between just two types of assault – simple assault and aggravated assault. Based on these definitions, the following factors cannot be used as triggers to enhance the underlying term for simple or aggravated assault (since they are used here as part of the definitions):

- use of or armed with a firearm/deadly weapon/assault weapon;
- by force, violence, or threat;
- infliction of great bodily harm/injury.

However, assault involving the use of or while armed with a *firearm or assault weapon* may be used as a trigger for an enhancement ONLY IF the penalty imposed is different than that imposed for assault involving the use of or while armed with a deadly weapon.

All other factors may be used as triggers to enhance the penalty for simple or aggravated assault, including assaults against a person of a specific age group/disability/etc, against a peace officer, involving multiple offenders, etc.

If the state has a separate offense for domestic violence (i.e. assault against a family member, cohabitant, etc.) and the penalties for such an offense are greater than those imposed for assault or aggravated assault (or any degree of assault as the state may define it), this must be included as an enhancement in which "against a family member or partner" is the trigger.

If the state has a separate offense for assault of a peace officer or corrections officer or any other designated category of persons, and the penalties for such an offense are greater than those imposed for assault or aggravated assault, this must be included as an enhancement.

Rape

Rape is the carnal knowledge or a person 1) forcibly and/or against that person's will OR 2) not forcibly or against the person's will where the victim is incapable of giving consent because of his/her temporary or permanent mental or physical incapacity or because of his/her youth.

According to the commentaries of the Model Penal Code, most rape statutes in the states continue to treat intercourse with a very young person as an especially heinous event and punish it more severely than statutory rape (i.e. sexual intercourse with a person below the age of consent). According to the commentaries, "more commonly, carnal knowledge of a [person] under a specified age carries the same penalties authorized for forcible rape" (MPC, 1980: 324). The critical age for an offense of this grade varies across the states and ranges from roughly age 11 to age 18. Several states establish only a single criterion age, while others establish a two step grading scheme that differentiates between sexual intercourse with a very young child (rape) and sexual intercourse with an older child (statutory rape). For example, Maine punishes intercourse with a person under 14 as a more severe felony than intercourse with a person between the ages of 14 and 16.

We continue this approach by including the youth of the victim as an incapacitating factor in the definition of rape above. Thus, the age of the victim alone does not necessarily act as a trigger for rape. HOWEVER, several states punish sexual intercourse with a person of a very young age *more severely* than forcible rape of an adult; in such instances, the age of the victim should be coded as a trigger enhancing the punishment of the underlying offense of rape.

Based on this definition and explanation, the following factors cannot be used as triggers to enhance the sentence for rape:

- by force, violence, or threat;
- against a person of a specific age group.

However, "against a person of a specific age group" may be used if rape of a person OVER a certain age (e.g. 65) carries a different sentence than rape of any other adult; "against a person of a specific age group" should not be used as a trigger to enhance the sentence for rape of a person below a certain age UNLESS rape of a person below a certain age carries a sentence greater than that for forcible rape.

All other factors can be used as triggers, including: infliction of bodily harm/injury.

Kidnapping

Kidnapping is 1) the unlawful restraint/confinement of a person in circumstances exposing him to risk, 2) the holding of a person in a condition of involuntary servitude, 3) the removal of a person from one place to another against his/her will, or 4) the restraint/confinement of a person for the purpose of (a) holding the victim for ransom or reward, or as a shield or hostage, (b) facilitating commission of any felony or flight thereafter; (c) inflicting bodily injury on or to terrorize the victim or another; or (d) interfering with the performance of any governmental or political function.

Based on this definition, the following factors cannot be used as triggers to enhance the underlying term for kidnapping since they are part of the definition:

- for the purpose of holding the victim for ransom, etc.;
- for the purpose of committing rape, sexual assault, robbery, etc. (since these fall under (b) above);
- by force, violence, or threat;
- for the purpose of inflicting serious bodily injury (since this falls under (c) above).

HOWEVER, "infliction of great bodily harm/injury" may be used as a trigger ONLY IF the *actual* infliction of bodily injury increases the penalty. Many states enhance the penalty for kidnapping beyond the penalty generally given for kidnapping for the purpose of holding a victim for ransom, etc. when serious injury actually occurs. In other words, kidnapping for ransom in which the victim suffers serious physical injury often carries a sentence greater than kidnapping for ransom. In these cases, "infliction of great bodily harm/injury" may be a trigger. This is a subtle point – the definition of kidnapping involves only the *intent* to inflict bodily injury on the victim; some states may enhance the sentence if the kidnapping involves the *actual infliction* of bodily injury.

All other factors may be used as triggers to enhance the penalty for kidnapping, including: use of or while armed with a firearm/deadly weapon/etc.; against a person of a specific age group/disability/etc.; resulting in death; etc.

If the state has a separate offense for child kidnapping (i.e. kidnapping of a person under a certain age) and the penalties for such an offense are greater than those imposed for kidnapping of any adult, this must be included as an enhancement in which "against a person of a specific age group" is the trigger.

Robbery

Robbery is the taking, or attempting to take, anything of value under confrontational circumstances from the control, custody, or care of another person by force or threat of force or violence and/or by putting the victim in fear of immediate harm.

Based on this definition, the following factors cannot be used as triggers to enhance the underlying term for robbery (since they are used here as part of the definition):

- by force, violence, or threat.

All other factors may be used as triggers to enhance the penalty for robbery, including: use or armed with a firearm/deadly weapon/etc.; infliction of great bodily harm/injury; against a person of a specific age group/disability/etc, against a peace officer, involving multiple accomplices, use of an auto to escape.

If the state has separate offenses for robbery of particular structures (school, religious structures, government buildings, banks, train cars, etc.) and the penalties for such offenses are greater than those imposed for general robbery, this must be included as an enhancement.

Burglary

Residential burglary is the unlawful entry of any *dwelling house*, whether in the nighttime or daytime, with the purpose to commit a crime.

Commercial burglary (non-residential burglary) is the unlawful entry of any building, whether in the nighttime or daytime, with the purpose to commit a crime.

For the Fragmentation and Ferment study, we will restrict the definition of burglary to the "unlawful" entry into a building (thus, shoplifting would not be considered burglary) and will distinguish between two types of burglary – non-residential burglary (or commercial burglary) and residential burglary.

Based on these definitions, the following factors cannot be used as triggers to enhance the underlying term for burglary (since they are used here as part of the definitions):

- involves a dwelling house (or other building or vehicle used as a dwelling house);
- entering a dwelling or building at night;
- intent to commit a felony.

All other factors may be used as triggers to enhance the penalty for burglary, including infliction of great bodily harm/injury; assault of a person within; use of or while armed with a firearm/deadly weapon/etc.; or while a person is actually present in the building.

If the state has separate offenses for burglarizing particular structures (school, religious structure, government building, bank, train car, etc.) and the penalties for such offenses are greater than those imposed for general burglary, this must be included as an enhancement.

Arson

Arson is damage, or attempt to damage, any real or personal property by fire or incendiary device.

Based on this definition, the following factors cannot be used as triggers to enhance the underlying term for arson (since they are used here as part of the definitions):

- use of explosives.

The following factors may be used as enhancement triggers: involves a dwelling house (i.e. a building or vehicle used as accommodations); when a person is actually present in the building. All other factors may be used as triggers as well: infliction of great bodily harm/injury; results in death; involves property of a certain value.

If the state has separate offenses for the burning of particular structures (school, religious structure, government building, etc.) and the penalties for such offenses are greater than those imposed for arson, this must be included as an enhancement.

Theft

For the Fragmentation and Ferment study, we will not code enhancements based on the value of the property stolen. Most states grade theft in this way. For example, theft of less than $100 is third degree theft, theft of $100 to $500 is second degree theft, and theft of more than $500 is first degree theft.

Similar gradings based on dollar value of property stolen are common for fraud, criminal mischief, damage to property, etc. We will not include these in the enhancements section.

All other factors may be used as triggers to enhance the underlying term for theft.

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