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IMPACTS OF SENTENCING REFORMS ON PRISONS AND CRIME RATES

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IMPACTS OF SENTENCING REFORMS AND PRISONS AND CRIME RATES

ABSTRACT

The need for certainty of punishment has long been a rallying cry for those seeking criminal court reform. In the 1970s and early 1980s, twenty states abandoned indeterminate sentencing and adopted presumptive sentencing, determinate sentencing, or sentencing guidelines designed to reduce punishment uncertainty. Research concerning these reforms has concentrated on their immediate goals, reducing sentence disparity; the purpose of the present research, in contrast, is to explore the bottom line implications for the criminal justice system, the impact on the prisons and on crime rates. We use a pooled time series-cross section design with data for each state during the 1970s and 1980s. In most instances the sentencing reforms had little impact on prison population, the number of prison admissions, or prison terms. A few laws, however, were followed by significant changes, especially reduced prison population growth in sentencing guideline states. We find even less evidence that the sentencing laws affected reported crime rates, and there is little sign that the laws had a significant deterrent impact.

1. INTRODUCTION

During the 1970s and 1980s twenty states enacted major sentencing reform legislation (Table 1). Although law makers' motives often are not clear (Casper and Brereton 1984), it is apparent that a major goal was to replace rehabilitation with punishment as the dominate justification for sentencing (see, e.g., summaries in Blumstein, et al. 1983:47-67; Travis 1982; Greenberg and Humphries 1980; Orland 1978; Twenty Century Fund 1976). Punishment, in turn, involves several different goals, especially retribution, incapacitation, and deterrence; and the reformers disagreed about their relative importance. The main goal for retribution is greater sentencing certainty for crimes that merit retribution and, to varying degrees, harsher sentences. The goal for incapacitation is crime reduction through the inability of criminals to commit crime while imprisoned. The goal for deterrence is reduction in crime, and the sentencing laws were designed to foster deterrence through more certainty in sentencing and often through longer sentences. Criminology research has, in the main, supported the conclusion that certainty of punishment does deter crime, but it has found little deterrence impact in the harshness of punishment (see the summaries in Blumstein 1985, and Paternoster 1987).

Research on sentencing reform has concentrated on the immediate goals and immediate impacts of the laws. The most common conclusions are that sentence disparity is reduced by determinate

sentencing laws (Clarke 1984; Clarke 1987; Casper, Brereton, and Neal 1982:149-158) and by presumptive sentence guidelines (Moore and Meithe 1986; Kramer and Lubitz 1986; Knapp 1984; von Hirsch, Tonry, and Knapp 1987), but not by voluntary guidelines (Rich et al. 1982; Carrow 1985). Other research has addressed the impacts on trial court operations. It has been found that sentencing reforms have little or no impact on guilty plea rates increase. Clarke (1984) concluded that the North Carolina determinate sentence law probably had no impact on court delay, but Heumann and Loftin (1979) suggest that Detroit's minimum sentence law reduced delay, a finding contested by Cohen and Tonry (1983:335-37).

In contrast to these issues, research on the more far reaching effects of sentencing reform has been meager. More specifically, did the reforms contribute to the decline in crime during the early 1980s or to the tremendous growth of prison population in the past decade?

1.1 Sentencing Laws.

The sentencing reforms evaluated in this research fall into three types: determinate and presumptive sentencing laws and sentencing guidelines.

Prior to the sentencing reforms the dominant mode of sentencing was indeterminate sentences, where the court gives only the maximum penalty, or sentence range, such that the parole board determines how long the sentence would be (see Hand and Singer 1974).

During the period covered by the research (1973-89), twenty states adopted broad new sentencing procedures (Table 1). Seven states -- Alabama, Arkansas, Colorado, Connecticut, Illinois, Maine, and Tennessee -- adopted determinate sentencing. Seven states -- Alaska, Arizona, California, Indiana, New Jersey, New Mexico, and North Carolina -- adopted presumptive sentencing. Six states -- Florida, Michigan, Minnesota, Pennsylvania, Washington, and Wyoming -- adopted sentencing guidelines.

Determinate sentencing statutes provide ranges for the various classifications of felonies, and the court sentences the defendant to a definite number of years falling within that range.

Presumptive sentencing statutes are similar to the determinate sentencing statutes, except that they give specific terms for each class of crime that are to be given unless the judge finds mitigating or aggravating circumstances.

Sentencing guidelines, which are elaborate versions of determinate and presumptive sentencing, give a suggested sentence or range based on several characteristics of the crime and defendant. Again, the judge typically can only depart from the suggested sentences if mitigating or aggravating circumstances are found. (This study does not include voluntary guidelines, which judges may or may not head, and it does not include guidelines applicable to only a few courts in the state.)

[Table 1 about here]

1.2 Prison population and commitments.

There are several reasons why one might argue that sentencing reforms increase prison commitments and prison population. Sentencing reform laws in some states call for stricter sentences, and in other states legislatures probably enacted laws with a similar, but unstated, purpose (Casper 1984:238; Casper and Brereton 1984:123-131; Foster, et al 1976:36-37; Holten and Handberg 1990:262). Judges may be more likely to give prison sentences if they are sure the prison term will be short (Casper, Brereton, and Neal 1984:111-118). Finally, it is likely that parole release under indeterminate sentence laws is used to limit prison overcrowding, an adjustment mechanism usually not available under determinate sentencing (Casper 1984:237-8).

Nevertheless, after reviewing the existing research, a National Academy of Science (NAS) panel concluded (Blumstein et al. 1983:32-33) that prison population growth appears to be only marginally related to sentence reforms. The major research effort in this area, and the apparent source for the conclusion reached by the NAS panel, is Casper, Brereton, and Neal (1982:111-148, 227-230). Imprisonment increased in two California counties studied, but declined in a third, during two years after the determinate sentencing law went into effect. Because imprisonment had also increased in the two years before the reform, leading the authors to conclude that evidence for an impact was weak: the increases in two counties may have been the continuation of pre-existing trends,

which in turn may be the result of a general climate of toughness towards crime that produced both the sentencing law and the increased prison rate (Casper, Brereton, and Neal 1982:147-49; Cohen and Tonry 1983:380-411). Other research has reached similar conclusions. Shane-DuBow, Brown, and Olsen (1985:313) concluded tentatively that determinate sentencing laws have but slight impact: 60% of states with determinate sentencing experienced more than a 12% increase in prison population between 1982 and 1983, as opposed to 47% of the states with indeterminate sentencing. Casper (1984:239-240) noted that Illinois prison admissions and population increased in the three years after a 1978 determinate sentencing law, but he concluded that this may be only a continuation of trends starting in the early 1970s. On the other hand, Clarke (1987) concluded that the North Carolina determinate sentencing law lead to shorter sentences and, thus, caused prison population growth to abate.

These studies do not use research designs adequate to test the impact of the sentencing laws. The before-and-after studies cannot distinguish the impact of the laws from other contemporaneous changes and from general trends. The cross section studies are not well suited for causal conclusions.

An important consideration is that the sentencing laws may not affect prison admissions and prison population in the same way. Judges may commit more defendants, but for shorter terms, or the prison authorities may accelerate releases in the face of rising court commitments.

1.3 Impact on crime.

A common argument for more determinate sentencing laws is the theory that more certainty in punishment provides a greater deterrent impact, although in most states deterrence was probably not a major factor behind the adoption of the laws because the deterrent impact of prison commitments is uncertain (Foster et al. 1976:38). There have apparently been no studies concerning the impact of these sentencing reforms on crime rates. In contrast, there has been a good deal of research concerning the effect on crime of other types of sentencing law changes, especially laws establishing mandatory minimum for crimes committed with deadly weapons (most found little or no impact, e.g., Loftin and McDowall 1981 and 1984; Loftin, Heumann and McDowall 1983).

2. RESEARCH DESIGN

Blumstein et al. (1983:31) concluded that almost all research into the impact of sentencing reforms suffer from major research design problems that limit interpretation of the findings. We address these problems by using the time series cross section design, pooling data from the states over a 20 year time period, 1970 to 1989. We use the standard economic procedure for analyzing pooled data, the fixed effect regression model (Mundlak 1978; Pindyck and Rubinfeld 1981; Hsiao 1986); this includes as independent variables separate dummy variables for each state and each year (except the first), and the coefficient associated with

the variable is an estimate of the influence of "fixed effects" unique to a state or year. Omission of these effects, if they are significant, causes the estimates of the other variables to be biased. The fixed effects, of course, reduce the degrees of freedom by nearly the number of states and years included (and an additional degree of freedom for each state is lost in the correction for autocorrelation), but the analysis still has over 500 degrees of freedom. In practice, the fixed effect model is a time-series analysis only; it combines the time series data from the several states into one regression, but ignores within-year, across-state variations.

Specifically, the form of the fixed effect model is as follows:

$$Y_{it} = a + bX_{it} + cY_{it} + g_2W_{2t} + g_3W_{3t} + \dots + g_NW_{Nt} \\ + d_2Z_{12} + d_3Z_{13} + \dots + d_TZ_{1T} + e_{it}$$

where X_{it} represents the discrete changes evaluated - that is, dummy variables representing the sentencing laws, and Y_{it} represents the control variables (such as age structure and economic trends). Also, $W_{it}=1$ for the i 'th state $i = 2 \dots N$, otherwise $W_{it}=0$; and $Z_{it}=1$ for the t 'th year, $t = 2 \dots T$, otherwise $Z_{it}=0$.

2.2 Advantages of the pooled design.

The pooled design has long been considered one of the best designs for studying social causation, probably second only to the pure random experiment (which would be prohibited by ethical

considerations). Campbell and Stanley (1967:55-57) called it "an excellent quasi-experimental design, perhaps the best of the more feasible designs" (a recommendation continued in Cook and Campbell 1979). Lempert (1966:130-131) called it "the research design par excellence" for research into the impact of legal changes, especially suitable in the United States where the states can be used as separate units for analysis.

In general, the pooled design has become particularly attractive in recent years because many time series, such as prison admission statistics, began in the early 1970's and have now been compiled long enough for pooled analysis, but not for single time series analysis.

The pooled design is basically a time series design in that it combines many separate time series, and time series designs are far superior to cross section designs when studying causation issues (e.g., Campbell and Stanley 1967; Farrington, Ohlin, and Wilson 1986). The pooled design provides a larger sample size than available for time-series analysis and for most cross section analyses. The main advantage, however, is the ability to enter control variables often lacking in time series research because there are not sufficient degrees of freedom or because multicollinearity problems are encountered. Especially important here is the use of fixed effects, which control for influences of omitted variables when constant for a state over time or a year over the states. The fixed effects variables are extremely

significant, such that single time series or single cross section analysis almost surely is subject to severe specification bias.

2.3 Form of Continuous Variables.

The research is conducted at the state level, which is the most appropriate for the topic. The basic rule is that the level of analysis should conform with the theoretical or policy issues underlying the research (e.g., Lieberman 1986:107-115). The present research is policy oriented, and it explores the impact of state-level sentencing policy on imprisonment and crime. Any attempt to analyze the issues by using aggregate data at the national level or using individual or city level data may not produce reliable information concerning state policy changes.

Because of the vast differences in state size, it is necessary to use ratio variables for all continuous variables. Otherwise, the variation in prison population, crime rates, and other continuous variables would be much greater for larger states, such that the regression results concerning the impact of the laws (represented by dummy variables) would be dominated by a few large states.

We use per capita variables, dividing the value of each continuous variable for each year in each state by the population of the state for that year. Population data change little from year to year and are less likely to cause spurious relationships that arise when error prone variables are used to construct ratio variables (see Gibbs and Firebaugh 1990).

The variables, except dummy variables, are in the form of natural logs to limit the impact of outliers.

2.4 Other statistical issues.

Autocorrelation was found in all analyses, and it was dealt with by using the standard correction with state-specific autocorrelation coefficients (Pindyck and Rubinfeld 1981:258-59). Heteroscedasticity problems were also found because the use of ratio variables lead to more variability in the small states (e.g., prison population and crime rates undergo greater year to year percentage changes in small states). This problem was corrected by using weighed regressions weighting by the square root of population.

3. VARIABLES

This section describes the dependent variables (prison commitments, prison population, prison release ratio, and crime rates), the exogenous control variables (such as economic and demographic variables), and the sentence reforms laws. The time span covered by the analysis is dictated by the availability of data. The starting year 1970 to 1973, and the final year is 1988 or 1989.

3.1 Prison variables.

The study uses three basic prison data time series: Prison admissions, prison population, and prison departures. The latter

are used to create the "prison release ratio," the number of departures divided by the prison population. All data are for prisoners sentenced to more than one year, although the states are not completely consistent in their application of this definition.

Prison admissions data start in 1972 because the definitions changed substantially that year (1972-3 Prisoners in State and Federal Institutions). Prison population data are used since 1971. Prison departure data starts in 1973.

The prison data are taken from Bureau of Justice Statistics (BJS) reports, mainly Prisoners and Prison Admissions and Releases. Admissions and release data for 1988 are from unpublished BJS statistics. In order to provide consistent time series we deleted much of the data provided by the Bureau of Justice Statistics, and we revised a substantial portion of the data used. These problems are discussed below with reference to specific categories of data.

3.1.1 Prison population.

Prison population is the number of prisoners in custody sentenced to more than a year, at the end of the year, because they are regularly revised (the revised versions are not yet available for 1989).

Prison population was counted by "custody" through 1976 and by "jurisdiction" starting in 1977. Nationwide, in that year, the number of prisoners counted by jurisdiction was 2.6 percent greater than the custody figure, but for some individual states the difference was much greater. The "custody" data are for the number

of prisoners actually in prison. The "jurisdiction" data are the number of prisoners under the jurisdiction of prison authorities. The major difference occurs when prisoners are placed in local jails because of overcrowding in state prisons. Also, jurisdiction figures include prisoners placed in federal prisons or prisons in other states; and they exclude prisoners in a state's prisons who are under federal jurisdiction or under the jurisdiction of another state.

Unless adjustments are possible, we exclude prison data that do not include prisoners kept in local jails because of overcrowding when the available information suggests that the number of such prisoners exceeds five percent of the total number of prisoners (an error of five percent or less would have little effect the regression results given the large year-to-year changes normally encountered by the prison systems). In most, but not all, states that used jails to relieve overcrowding before 1977, the published prison population data did not include such jail inmates. Even in later years several states did not include jail inmates in their prison data because they did not consider them under the jurisdiction of the prisons. Of the states that used local jails to relieve overcrowded prisons before 1977, seven did not count them in the prison population statistics: Alabama, Florida, Georgia, Louisiana, Mississippi, New Jersey, and Virginia. Data for these prisoners are available for 1976, and they are added to prison population counts when they amounted to more than 5 percent of the population figure for any year (the figures for Florida and

Georgia fell below that figure). Adjustments could not be made for earlier years; so prison data prior to 1976 are not used for Alabama, Louisiana, Mississippi, and Virginia (see Table 2). We similarly adjusted the data for states that did not include prisoners placed in jails because of overcrowding in their prison population statistics after 1977 - Arizona (1988), Arkansas (1985-88), Colorado (1982-1987), Idaho (1987-88), Kentucky (1984-86), Massachusetts (1986-88), New Jersey (1977-86), Tennessee (1983-88), and Virginia (1977-79).

We have not included prison capacity as a variable because of simultaneity problems and because adequate data are not available. The simultaneity problem is that prison capacity is probably influenced by prison population; more population growth, for example, prompts states to expand capacity. Capacity data, in addition, are not available before 1977 and are incomplete for years when available. Even if capacity data were available, they would not be a proper variable because they do not take account of the ability of states to use jails for overcrowding. As of 1987, at least seventeen states use jails to relieve overcrowding and five more have integrated systems, where prisons and jails are combined into a single organization (Bureau of Justice Statistics 1989:90, 109-115).

[Table 2 about here]

3.1.2 Prison admissions.

Prison admissions are defined as court commitments, parole or conditional release violators returned, and escapees returned. This measure is used rather than court commitments alone because data for the latter are not available before 1974 and because states often include parolees and escapees returned in the court commitment category.

The admission data are not useable for some or all years in several states. Data are not available in Indiana before 1973, North Carolina before 1975, Rhode Island before 1974, and Vermont before 1973. Whenever the data for prisoner population are adjusted for prisoners placed in jail (see above), the admission data are not useable because prisoners sent directly to jails are not counted. The years in which data are not available are listed in Table 2.

3.1.3 Prison departures and prison release ratio.

Prison departures, used to construct the prison release ratio, are the total prison releases, available since 1974. There are two widespread problems with these data. First, the number of releases for some years and some states the data include departures for authorized temporary absences (such as court appearances). Thus 1974 data for Connecticut, Indiana, Louisiana, Michigan, Texas, Virginia, and Washington are not used. Data for 1974 and later years are not used for California (before 1980), Florida (1978),

Kansas (1978), Massachusetts (1976), and Pennsylvania (1976). Second, the prison departure data exclude departures from jails when prisoners are placed in jail due to prison overcrowding and the state does not include these prisoners in the population count. Table 2 lists the states and years where the data are not available for these purposes.

The "prison release ratio" is the number of prisoners at year end divided by the number released that year. This variable is derived from the business inventory ratio (inventory divided by sales) and a common measure of court delay, the backlog index (pending cases divided by dispositions). It measures prison term length less precisely than average time served. But the latter measure can be biased by changes in release practices; for example, a policy to shorten prison terms can cause the average sentence length to increase if the authorities release more long term prisoners than usual. Hence, the release ratio is a better indicator of current practices and trends than prison term length.

3.2 Crime Rates.

When exploring the impact of sentence reform on crime, we use as a measure of crime the seven components crime index of the uniform crime reports, murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, property crime, burglary, larceny-theft, and motor vehicle theft.

The data are from Federal Bureau of Investigation (1972-1990), except that the 1971 larceny data are from unpublished statistics

supplied by the Bureau. (Larceny data before 1971 are not used because they exclude thefts involving property worth \$50 or less.) The crime data are the adjusted statistics published in the succeeding year Crime Reports.

The quality of crime data is always a major concern. Reported crime is the best data at the state level, and it is widely believed that for the period covered here, the data are reasonably adequate (e.g., Cohen and Land 1984; Gove, Hughes, and Geerken 1985; Myers 1980). Nevertheless, we took several steps to mitigate data quality problems. The year dummies in the pooled regression control for nation-wide changes in propensity of citizens to report crime, and the use of logged variables limits the impact of outliers. Finally, we used influence analysis (SAS Institute 1985, pp. 676; Belself, Kuh, and Welsh 1980) to uncover observations that may unduly affect the result. The only important problem uncovered were crime data in Illinois (the Chicago police seriously undercounted crime before 1984, Federal Bureau of Investigation 1986, pp. 4-5), but the results changed very little when Illinois was left out.

When exploring the impact of sentence reform on prisons, the crime rate is entered as a control variable, and here crime is limited to "major crimes" - murders, nonnegligent manslaughter, forcible rape, robbery, aggravated assault, and burglary (that is, the total crime index less larceny and motor vehicle theft). This measure is commonly used when comparing growth in crime and prison population. Convictions for these crimes result in more and longer

prison sentences than larceny and vehicle theft; the average sentence varies from 75 to 221 months for the former, and 46 months for the latter (Bureau of Justice Statistics 1990). Also, when we entered each of the seven crime types as independent variables, larceny and auto theft rates were far from significantly related to prison admissions, while all the remaining crimes except rape showed significant relationships.

The crime rates, as an independent variable, are entered for a lag of one year. The average time between arrest and sentencing is 194 days for all crimes (and it would be slightly greater for the crimes included here since the average is 178 days for larceny and vehicle theft) (Bureau of Justice Statistics 1990). There is also a potential simultaneity problem here, since changes in crime rates can prompt changes in crime control policy.

3.3 Other Variables.

The analysis includes a large number of control variables, the most important of which are the state and year dummies. Crime rates as control variables in the analysis of prison variables is described in Section 3.2. Other control variables are age structure and economic conditions.

Age structure is often said to be an important factor in crime and prison population trends because arrestees are disproportionately teenagers and young adults. Prison commitments and prison population are dominated by persons in their late teens through early thirties. The age structure variables entered are

determined largely by the availability of data. Year-by-year age structure data for states are available since 1970. The available age categories most related for our purposes are 15 to 17, 18 to 24, and 25 to 34 years old. The data, available from the U.S. Bureau of the Census, are estimates as of July 1 each year, except that the for 1970 and 1980 they are as of the April 1 census. We adjusted the 1980 data by adding one-third the difference between the 1980 and 1979 statistics. The 1970 data could not be so adjusted, but these data were not used in the final regressions because the first year dropped out in the autocorrelation correction.

The final variable, real personal income, controls for changes in economic conditions. It is often theorized, on the one hand, that crime increases as economic conditions worsen because there is less chance of legitimate employment, and on the other hand that crime increases as economic conditions improve because there are more activities and more lucrative crime targets. With respect to prison variables, it is likely that prison population increases as economic conditions permit states to afford more prison space.

Real personal income is the best available state-level measure of economic trends; adequate data for unemployment rates, for example, are not available for small states prior to 1976. The variable used is the total real personal income of the state controlled for inflation by dividing by the GNP price deflator (1972 = 1.0), and divided by the population of the state. The variable is lagged one year, because economic changes may not

affect motivation to commit crime immediately and because we wish to limit possible simultaneity problems due to the fact that crime may in turn affect economic activity. The personal income data were obtained from the Department of Commerce.

3.4 Sentencing Laws.

The main independent variables are the sentencing laws in the 20 states, which are listed in Table 1. Because there are no available complete survey of sentencing laws, locating these laws required extensive research into the state statutes, court rules, and court orders.

The sentencing laws are operationalized as variables having zeros in years before the laws went into effect and ones in years afterwards. For the year in which the law went into effect, the variable is coded as the portion of the year (taken to one decimal point) for which the law was in effect, e.g., 0.5 if the law went into effect on July 1st. With two exceptions, the effective dates of the laws pertain to the date the crime was committed (Table 1). The impact of the laws was analyzed both individually and in the aggregate. First, separate variables were entered for each of the twenty laws, and the laws were combined into three variables, one each for presumptive sentencing, determinate sentencing, and sentencing guidelines.

The proper lag for the sentencing law variables is an important and difficult issue. One cannot expect the laws to have immediate effects on prisons and crime rates (Casper and Brereton

(1984:132-138) and Cohen and Tonry (1983:442-444)). The impact on prisons could not occur until after the defendants were sentenced, typically several months after their crimes. It may take several more months, or even years, for the final change in attorneys' and judges' patterns of behavior to be settled. The impact on criminal behavior is also probably delayed for a period during which information about the law and its impact reached potential offenders. Therefore, we use a lag of one year, which tends to show a greater impact than using no lag or a longer lag, although the differences in results are not large.

Sentencing laws are included in the analysis only if there are at least two years of data before and after the law (in its lagged version). In the vast majority of instances there were at least four years before the law and eight afterwards.

[Tables 3, 4, and 5 about here]

4. RESULTS

4.1 Impact on Prisons.

Tables 3 through 5 present the results concerning the impacts on prison population, prison admissions, and prison release. Each table presents the results of two regressions, one with the sentencing laws aggregated into three law types and other with separate variables for each law. The three tables are not strictly comparable because differences in data availability mean that the time periods covered by the analysis and the number of laws included differ. Data are most complete for the prison population

study (Table 4) and least complete for the prison release ratio study.

In all the regressions the impact of the individual state sentence laws differ greatly, both in magnitude and direction. The aggregate law variables, therefore, are not strictly appropriate because their coefficients differ from state to state, but they do provide a rough estimate of the overall impact of the laws. Interpretation of the impact of individual state laws is hindered by the fact that with 20 laws results significant to the .05 level are likely to appear simply by chance. Therefore, we concentrate on laws whose coefficients are significant to the .01 level.

Most sentencing laws had little or no impact on prisons, and what impacts there are differ so much that overall conclusions are difficult. Among the seven states with presumptive sentencing, in Alaska the law led to higher prison populations, with the results suggesting weakly that both admissions and release ratios (which estimate prison terms) increased. The Indiana law produced more prison admissions and, possibly, greater prison population. In contrast, the New Mexico and North Carolina laws were followed by reduced prison populations, in New Mexico probably because prison terms declined and in North Carolina because growth of admissions declined.

Among the seven states with determinate sentencing, Tennessee and possibly Maine experienced declining growth rates for prison population. Neither state had sufficient data for the release

ratio regression; Maine could be included in the prison admission regression, where it showed a highly significant reduction in growth. Determinate sentencing led to higher prison release ratios (suggesting longer terms) in Arkansas, Connecticut, and possibly Alabama, but without significant impacts on prison population. A significant decline in the Illinois release ratio produced little if any change in population, probably because admissions increased substantially.

Sentencing guidelines show more consistent results than other sentencing laws. In three of the six states, Florida, Minnesota, and Washington, initiation of guidelines were followed by very significant declines in the growth of prison population. This was apparently caused by fewer admissions in Washington and lower release ratios in Florida. In Minnesota there is some evidence of both fewer admissions and smaller release ratios.

When the laws do have significant impacts, the absolute sizes of the impacts are often large. Because the dependent variables are logged, the coefficients on the dummy variables representing the laws estimate the percent change in the dependent variable resulting from the law. Thus, for example, we can estimate that Florida, Minnesota, and Washington sentencing guidelines reduced prison population by roughly a third.

4.1 Impact on Crime Rates.

Tables 6 through 10 suggest that the sentence reforms had very little impact on crime rates; again a possible exception is sentencing guidelines. When the laws are aggregated into the three

separate variables, there are few significant results and only a possible negative relationship between presumptive sentencing and burglary rates is significant to the .01 level.

Entering separate variables for individual state laws does not evidence a more definite pattern. Presumptive sentencing laws suggest little impact, with possible exceptions of higher murder and robbery rates in California, lower burglary, larceny and perhaps robbery rates in Indiana. Determinate sentencing laws in Alabama are associated with lower murder and rape rates, but higher larceny rates. The Arkansas law seems to be followed by higher property crime rates, Tennessee by higher auto theft, and Illinois by a drop in larceny.

Sentencing guidelines evidence more impact on crime. The Florida law was followed by generally higher crime rates, especially for robbery, burglary, and auto theft. The Michigan law is followed by higher rape rates, the Washington by higher murder, robbery and burglary rates, and the Wisconsin law by higher murder, assault, and possibly rape rates. In contrast, the Pennsylvania was followed by lower burglary rates. As suggested by tables 6 and 7, however, the tendency for sentencing guidelines to be followed by rising crime is not pervasive, and the overall evidence is that guidelines in any particular states have little or no impact on specific types of crime.

[Tables 6-9 about here]

5. DISCUSSION AND CONCLUSION

The major import of this analysis is the general absence of evidence that sentencing reforms affected prisons or crime rates. The occasional significant results are roughly balanced between positive and negative coefficients.

There is some evidence that sentence guidelines reduce prison populations, but the evidence is limited to only three of the six guidelines states, Florida, Minnesota, and Washington. A major impact was found in Minnesota, and it is tempting to attribute that impact to the fact that its law specifically lists prison overcrowding as a factor in sentencing, but the evidence is only suggestive because we do not know why other guideline laws did or did not affect prison populations.

The greatest impact on prison populations occurred in North Carolina, and this is consistent with the finding by Clarke (1987) that the guidelines resulted in shorter sentences (although we found that the impact was due mainly to fewer admissions, rather than a smaller prison release ratio).

Both deterrence and incapacitation theory suggest that states where prison populations declined should experience unusually growth in crime rates. Of the states where we have substantial evidence that sentencing laws reduced prison population -- Florida, Minnesota, New Mexico, North Carolina, Tennessee, Washington, and perhaps Maine -- roughly half are among the few states that encountered unusually large increases in crime rates after

sentencing laws were passed, but this association is not strong enough to provide more than suggestive evidence of an impact.

The overall conclusion is that differences between laws and their settings differ from state to state such that the impact of sentencing reforms varies greatly. The next logical step, one might argue, is to determine why the laws have impacts in some states but not in others. But it would be difficult to differentiate the effects of numerous factors that could influence the impacts of the laws. The prison terms for specific crimes are unique to each state, as are the prior laws and, thus, the changes affected by the sentencing reforms. Many other provisions of the laws might be relevant. For example, some sentencing reforms included mandatory minimums in addition to the determinate sentencing, while others did not (in the same year that the general sentencing reforms were enacted, six of the twenty states - Alaska, Arizona, Connecticut, Indiana, Maine, and Pennsylvania - also enacted mandatory minimums for offenses committed with deadly weapons). Finally, the actual implementation of the laws probably differed from state to state. The twenty laws provided a sample too small to differentiate the impact of all these and many other factors that affect the impact of the laws.

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Table 1
Sentencing Reforms 1969-1989

	Code Section	Law year & number	Effective Date	Type ¹
01 Alabama	13A-5-6	1977-607	1-1-80	D
02 Alaska	12.55.125	1978-166	1-1-80	P
03 Arizona	13.701	1977-142	10-1-78	P
04 Arkansas	5-4-401	1981-620	6-16-81	D
05 California	P 1170	1976-1139	7-1-77	P
06 Colorado	18-1-105	1977-216	7-1-79	D
07 Conn.	53a-35a	1980-442	7-1-81	D
10 Florida	Rule 3.701	--- ²	10-1-83	G
14 Illinois	1005-8-1	1978-1099	2-1-78	D
15 Indiana	35-50-2-1	1976-148	10-1-77	P
20 Maine	17A-1252	1975-499	5-1-76	D
23 Michigan	418 Mich lxxx	--- ²	3-1-84 ³	G
24 Minnesota	244.01	1978-723	5-1-80	G
31 New Jersey	2C:44-1	1978-95	9-1-79	P
32 New Mexico	31-18-15	1979-152	7-1-79	P
34 North Car.	15A-1340.4	1979-760	7-1-81	P
39 Penn.	202 Pa. Code	303-1 --- ²	7-22-82	G
43 Tennessee	40-35-101	1982-868	7-1-82	D
48 Washington	9.94A.905	1981-137	7-1-84	G
50 Wisconsin	973.012	1983-371	11-1-85 ³	G

1. D = Determinate (judge selects a specific term within a range established by statute); P = Presumptive (the statute gives a presumptive sentence, requiring special circumstances for deviation). S = Sentencing guidelines.

2. Nonstatutory: Florida, in Rules of Criminal Procedure; Michigan, Supreme Court order; Pennsylvania, Sentencing Commission Rule.

3. For sentences on or after that date.

Table 2

Years for Which Prison Data Are Not Available

	Prison Admission (1972-88)	Prison Population (1971-89)	Prison Departures (1974-88)
Alabama	before 77	before 76	before 77
Arizona	after 87		after 87
Arkansas	after 84		after 84
California			before 80
Colorado	after 81		after 81
Connecticut			before 75
Florida			before 78
Idaho	after 86		after 86
Indiana	before 73		before 75
Kansas			before 78
Kentucky	after 83		after 83
Louisiana	before 77	before 76	before 77
Massachusetts	after 85		before 76 after 85
Michigan			before 75
Mississippi	before 78	before 76	before 78
New Jersey	all		all
North Carolina	before 75		before 75
Pennsylvania			before 76
Rhode Island	before 74		
Tennessee	after 82		after 82
Texas			before 75
Vermont	before 73		before 73
Virginia	before 80	before 76	before 80
Washington			before 75

Table 3
Impact of Sentencing Law Types on Prison Population¹

	<u>Sentencing Laws Expressed as:</u>	
	Aggregate Law Types	Individual State Laws
<u>Presumptive Sentencing²</u>	.11 3.26**	
02 Alaska (1980)		.34 3.21**
03 Arizona (1978)		.08 1.01
05 California (1977)		.00 .06
15 Indiana (1977)		.14 2.15*
31 New Jersey (1979)		.02 .26
32 New Mexico (1979)		-.31 2.96**
34 North Car. (1981)		-.40 5.52***
<u>Determinate Sentencing²</u>	-.01 .29	
01 Alabama (1980)		.10 1.40
04 Arkansas (1981)		.00 .07
06 Colorado (1979)		-.08 .77
07 Conn. (1981)		.08 1.24
14 Illinois (1978)		.02 .46
20 Maine (1976)		-.24 2.19*
43 Tennessee (1982)		-.17 2.75**
<u>Sentencing Guidelines²</u>	-.07 2.27*	
10 Florida (1983)		-.30 6.88***
23 Michigan (1984)		.00 .01
24 Minnesota (1980)		-.35 6.82***
39 Penn. (1982)		.07 1.54
48 Washington (1984)		-.30 3.92***
50 Wisconsin (1985)		-.09 1.43
<u>Other Variables¹</u>		
Major crime (lagged)	-.02 .38	-.03 .73
Percent pop. 18-24	.23 .89	-.01 .02
Percent pop. 25-34	.11 .46	-.15 .64
Real per. inc. (lagged)	-.02 .14	.01 .09
<u>F Values</u>		
Year dummies	26.17***	25.62***
State dummies	31.58***	41.46***
Dependent var. mean	4.84	4.82
Degrees of freedom	805	788
Adjusted R-square	.99	.99
Durbin-Watson	1.65	1.65

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable, prison population, and the "other variables" are per capita variables and are in natural logs. For each regression, the two columns contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 4
Impact of Sentencing Law Types on Prison Admissions¹

	<u>Sentencing Laws Expressed as:</u>	
	Aggregate Law Types	Individual State Laws
<u>Presumptive Sentencing²</u>	.10 2.51*	
02 Alaska (1980)		.15 1.40
03 Arizona (1978)		.17 1.71
05 California (1977)		.13 1.31
15 Indiana (1977)		.20 3.03**
31 New Jersey (1979)		-- --
32 New Mexico (1979)		-.10 .84
34 North Car. (1981)		-.33 4.87***
<u>Determinate Sentencing²</u>	-.05 1.02	
01 Alabama (1980)		.19 1.57
04 Arkansas (1981)		-.19 2.30*
06 Colorado (1979)		-.18 1.39
07 Conn. (1981)		-.05 .48
14 Illinois (1978)		.15 1.97*
20 Maine (1976)		-.57 4.69***
43 Tennessee (1982)		-- --
<u>Sentencing Guidelines²</u>	-.06 1.50	
10 Florida (1983)		.05 .58
23 Michigan (1984)		-.03 .36
24 Minnesota (1980)		-.23 2.50*
39 Penn. (1982)		.07 .89
48 Washington (1984)		-.42 4.24***
50 Wisconsin (1985)		-.08 1.15
<u>Other Variables¹</u>		
Major crime (lagged)	.26 3.91***	.31 4.56***
Percent pop. 18-24	-1.51 4.37***	-1.69 4.81***
Percent pop. 25-34	-.84 2.77**	-.96 2.84**
Real per. inc. (lagged)	.21 1.26	.24 1.46
<u>F Values</u>		
Year dummies	8.69***	8.20***
State dummies	43.47***	50.87***
Dependent var. mean	4.25	4.25
Degrees of freedom	654	639
Adjusted R-square	.99	.99
Durbin-Watson	1.97	1.90

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable, prison admissions, and the "other variables" are per capita variables and are in natural logs. The two columns for each regression contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 5
Impact of Sentencing Law Types on the Prison Release Ratio¹

	Sentencing Laws Expressed as:			
	Aggregate Law Types		Individual State Laws	
<u>Presumptive Sentencing²</u>	.03	.61		
02 Alaska (1980)			.21	1.31
03 Arizona (1978)			.10	.68
05 California (1977)			--	--
15 Indiana (1977)			.19	1.41
31 New Jersey (1979)			--	--
32 New Mexico (1979)			-.35	3.18**
34 North Car. (1981)			-.12	1.69
<u>Determinate Sentencing²</u>	.23	3.93***		
01 Alabama (1980)			.28	2.29*
04 Arkansas (1981)			.36	4.02***
06 Colorado (1979)			.03	.17
07 Conn. (1981)			.28	2.84**
14 Illinois (1978)			-.42	3.30***
20 Maine (1976)			--	--
43 Tennessee (1982)			--	--
<u>Sentencing Guidelines²</u>	.01	.10		
10 Florida (1983)			-.35	5.58***
23 Michigan (1984)			.33	3.22**
24 Minnesota (1980)			-.14	2.10*
39 Penn. (1982)			.15	1.11
48 Washington (1984)			-.19	1.16
50 Wisconsin (1985)			-.06	.54
<u>Other Variables¹</u>				
Major crime (lagged)	-.16	1.59	-.11	1.17
Percent pop. 18-24	1.32	2.43*	.78	1.48
Percent pop. 25-34	.94	1.73	.80	1.64
Real per. inc. (lagged)	.03	.11	.22	.81
F Values				
Year dummies	3.71		3.74	
State dummies	12.70		10.83	
Dependent var. mean	.67		.67	
Degrees of freedom	549		536	
Adjusted R-square	.73		.76	
Durbin-Watson	1.93		1.99	

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable is the prison release ratio. The "other variables" are per capita variables and are in natural logs. For each regression, the two columns contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 6
Impact of Sentencing Law Types on Violent Crime Rates¹

	Murder		Rape		Assault		Robbery	
<u>Sentencing Law Types²</u>								
Presumptive Sentencing	.05	1.20	-.06	1.59	.01	.33	.06	1.54
Determinate Sentencing	-.01	.40	-.03	.97	.02	.55	-.03	.65
Sentencing Guidelines	.08	1.81	.05	1.32	-.03	.76	.08	2.10 ^a
<u>Other Variables¹</u>								
Percent pop. 15-17	-.63	3.00***	.15	.89	.22	1.01	1.30	6.12**
Percent pop. 18-24	.53	2.00*	.88	3.85***	.55	2.28*	-.13	.49
Real per. inc. (lagged)	.54	3.51***	.40	3.15**	.35	2.42*	.87	5.70**
F Values								
Year dummies	6.31***		10.92***		7.05***		15.34***	
State dummies	145.99		73.71***		73.03***		155.14***	
Dependent var. mean	1.94		3.28		5.37		4.86	
Degrees of freedom	876		876		876		876	
Adjusted R-square	.99		.99		.99		.99	
Durbin-Watson	2.06		2.07		1.73		1.77	

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable, crime rates, and the "other variables" are per capita variables and are in natural logs. For each regression, the two columns contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 7

Impact of Sentencing Law Types on Property Crime Rates¹

	<u>Burglary</u>		<u>Larceny</u>		<u>Auto Theft</u>	
<u>Sentencing Law Types²</u>						
Presumptive Sentencing	-.06	2.81**	-.04	2.20*	.01	.19
Determinate Sentencing	.02	.82	-.01	.69	.02	.51
Sentencing Guidelines	.02	.91	.00	.27	.04	1.25
<u>Other Variables¹</u>						
Percent pop. 15-17	.63	4.77***	.40	3.84***	.33	1.65
Percent pop. 18-24	.00	.01	.18	1.33	.12	.54
Real per. inc. (lagged)	.02	.25	-.02	.32	.60	4.41***
<u>F Values</u>						
Year dummies	44.16***		97.42***		12.02***	
State dummies	59.15***		57.31***		27.75***	
Dependent var. mean	7.09		7.87		5.92	
Degrees of freedom	876		826		876	
Adjusted R-square	.99		.99		.99	
Durbin-Watson	1.71		1.69		1.68	

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variables, crime rates, and "other" variables are per capita variables and are in natural logs. For each regression, there are two columns containing coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 8
Impact of Individual State Sentencing Laws on Violent Crime Rates¹

	<u>Murder</u>		<u>Rape</u>		<u>Assault</u>		<u>Robbery</u>	
<u>Presumptive Sentencing²</u>								
02 Alaska (1980)	-.06	.38	-.02	.17	-.02	.15	.02	.21
03 Arizona (1978)	.08	1.15	-.14	1.78	-.07	.81	.04	.58
05 California (1977)	.25	3.75***	.01	.15	.01	.17	.20	3.58**
15 Indiana (1977)	-.02	.23	-.11	1.25	.05	.67	-.20	2.04*
31 New Jersey (1979)	-.03	.23	.03	.51	.05	.71	.12	1.40
32 New Mexico (1979)	-.03	.28	-.15	1.56	.14	1.45	-.01	.07
34 North Car. (1981)	-.13	1.48	.07	.82	-.12	1.26	.14	1.23
<u>Determinate Sentencing²</u>								
01 Alabama (1980)	-.23	3.60***	-.14	2.71**	.01	.16	.08	.84
04 Arkansas (1981)	-.03	.28	-.04	.42	-.08	.78	.07	.56
06 Colorado (1979)	-.05	.74	-.21	2.03*	.01	.12	-.26	2.44*
07 Conn. (1981)	.27	1.80	.09	1.34	.03	.44	.03	.19
14 Illinois (1978)	.00	.09	-.03	.48	.03	.41	-.05	-.60
20 Maine (1976)	-.03	.34	.09	1.13	-.06	.31	.03	.19
43 Tennessee (1982)	-.03	.27	.10	2.09*	-.01	.08	.12	1.02
<u>Sentencing Guidelines²</u>								
10 Florida (1983)	.01	1.12	-.11	1.30	-.01	.14	.37	4.91**
23 Michigan (1984)	.19	2.53*	.20	3.26**	-.01	.10	.10	1.10
24 Minnesota (1980)	-.06	.56	.07	.79	.01	.10	.04	.49
39 Penn. (1982)	-.03	.35	-.07	1.06	-.06	1.03	.02	.36
48 Washington (1984)	.27	3.37***	.12	1.04	-.11	1.11	.28	4.49**
50 Wisconsin (1985)	.21	3.39***	.15	2.32*	.27	2.65**	.10	.91
<u>Other Variables¹</u>								
Percent pop. 15-17	-.55	2.57*	.13	.76	.32	1.44	1.10	5.06**
Percent pop. 18-24	.44	1.51	.63	2.78**	.49	1.89	-.27	1.01
Real per. in. (lagged)	.58	3.66***	.33	2.67**	.34	2.35*	.88	5.76**
 F Values								
Year dummies	6.21***		12.40***		6.76***		15.42***	
State dummies	133.41***		82.71***		66.07***		149.86***	
Dependent var. mean	1.94		3.28		5.37		4.86	
Degrees of freedom	859		859		859		859	
Adjusted R-square	.95		.99		.99		.99	
Durbin-Watson	2.05		2.04		1.71		1.77	

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable, crime rates, and the "other variables" and per capita variables are in natural logs. For each regression, the two columns contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.

Table 9
Impact of Individual Sentencing Laws on Property Crime Rates¹

	<u>Burglary</u>		<u>Larceny</u>		<u>Auto Theft</u>	
<u>Presumptive Sentencing²</u>						
02 Alaska (1980)	-.07	.95	-.09	1.50	-.08	.72
03 Arizona (1978)	-.12	2.53*	-.06	1.21	-.11	2.38*
05 California (1977)	-.01	.22	-.05	1.32	.02	.21
15 Indiana (1977)	-.11	3.07**	-.13	3.41***	-.05	.95
31 New Jersey (1979)	-.07	1.30	-.06	1.62	.16	2.27*
32 New Mexico (1979)	.00	.04	.08	1.24	.06	.52
34 North Car. (1981)	.04	.53	.07	1.37	.05	.72
<u>Determinate Sentencing²</u>						
01 Alabama (1980)	.08	1.46	.13	2.94**	.01	.08
04 Arkansas (1981)	.17	2.74**	.10	2.40*	.18	2.10*
06 Colorado (1979)	-.07	1.11	-.01	.09	-.17	1.67
07 Conn. (1981)	.00	.06	-.04	1.43	-.17	1.83
14 Illinois (1978)	.04	1.02	-.09	3.59***	.00	.03
20 Maine (1976)	-.14	1.41	.03	.52	.16	1.02
43 Tennessee (1982)	.08	1.57	.04	1.14	.24	3.30***
<u>Sentencing Guidelines²</u>						
10 Florida (1983)	.18	4.19***	.05	1.49	.34	6.82***
23 Michigan (1984)	.00	.02	-.01	.22	.12	1.64
24 Minnesota (1980)	.02	.46	.01	.38	-.17	1.61
39 Penn. (1982)	-.08	3.29**	-.03	.84	.01	.13
48 Washington (1984)	.18	4.11***	.07	1.56	.08	.79
50 Wisconsin (1985)	-.02	.43	-.06	1.39	.09	1.47
<u>Other Variables¹</u>						
Percent pop. 15-17	.64	4.80***	.36	3.49***	.21	1.06
Percent pop. 18-24	-.20	1.26	.17	1.28	-.06	.28
Real per. inc. (lagged)	.03	.38	-.05	.70	.71	5.30***
<u>F Values</u>						
Year dummies	44.47***		99.52***		12.21***	
State dummies	61.65***		47.42***		29.10***	
Dependent var. mean	7.09		7.87		5.92	
Degrees of freedom	859		809		859	
Adjusted R-square	.99		.99		.99	
Durbin-Watson	1.70		1.69		1.67	

* = significant to .05 level; ** to .01 level; *** to .001 level.

1) The dependent variable, crime rates, and the "other variables" are per capita variables and are in natural logs. For each regression, the two columns contain coefficients and absolute values of the T Ratios.

2) The sentencing reform laws are lagged one year.