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ULTIMATE IMPACTS OF SENTENCING REFORMS AND SPEEDY TRIAL LAWS

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ULTIMATE IMPACTS OF SENTENCING REFORMS AND SPEEDY TRIAL LAWS

EXECUTIVE SUMMARY

The need for certainty and promptness of punishment has long been a rallying cry for those seeking criminal court reform. Indefinite and distant threats of punishment, many theorize, are not potent deterrents. In the 1970's and early 1980's, these tenets resulted in widespread adoption of 1) sentencing reforms, to enhance certainty, and 2) speedy trial laws, to enhance promptness. Research to date concerning these reforms has concentrated on the immediate goals, such as reducing sentence disparity and court delay, finding that at times these goals are furthered and at times not. But these results have limited policy importance. The purpose of the present research is to explore the bottom line implications for the criminal justice system - that is, the impact on prisons and crime rates. Specifically, did reforms contribute to the decline in crime during the early 1980s or to the tremendous growth of prison population in the past decade?

Tonry suggests that the limited research on sentence reform does not permit conclusions: he can only guess that the reforms have contributed relatively little to prison population growth, and he concludes that the research presents mixed results concerning the impact on crime.<sup>1</sup> These uncertain conclusions are largely the product of inadequate research designs, a common complaint concerning sentencing impact research.<sup>2</sup> We use the time series-cross section design, long considered one of the best

procedures for studying causation, and the best for the issues addressed here. The design involves the use of data for each year in each state over the decades of the 1970's and 1980's, providing a sample size large enough for robust analysis. However, it requires complex regression procedures to control for various sources of bias, especially the likelihood that crime rates and prison populations differ systematically between states.

This report evaluates two types of reforms, broad sentencing reforms and laws that mandate prison terms for crimes committed with deadly weapons. The broad reforms, which have been adopted in 20 states, fall into three categories: determinate sentencing, presumptive sentencing, and sentencing guidelines. In these states prior to the reforms, and in almost all other states today, the dominant mode of sentencing was indeterminate sentencing, under which the court sets only the maximum term or a broad range of years. Seven states -- Alabama, Arkansas, Colorado, Connecticut, Illinois, Maine, and Tennessee -- adopted determinate sentencing statutes that specify ranges for various classifications of felonies, and the court sentences the defendant to a definite number of years falling within that range. Seven states -- Alaska, Arizona, California, Indiana, New Jersey, New Mexico, and North Carolina -- adopted presumptive sentencing statutes that specify specific penalties for each class of crime, and the judge must give that sentence unless mitigating or aggravating circumstances are found. Six states -- Florida, Michigan, Minnesota, Pennsylvania, Washington, and Wyoming -- adopted state-wide sentencing guidelines

that give a suggested sentence or range based on several characteristics of the crime and the defendant. Again, the judge typically cannot depart from the suggested sentences unless mitigating or aggravating circumstances are found.

During the past two decades nearly all states enacted laws that established prison terms for crimes committed with deadly weapons, and at present 47 states have such laws (the exceptions are Illinois, Mississippi, and North Carolina). Most of these laws require mandatory minimum sentences, typically one or two years for first offenders, if the court finds that the defendant used or possessed a deadly weapon during the commission of a felony. Most also provide for an enhancement of the sentence beyond that which would normally be given for the felony.

Our research explores the impact of the new sentencing laws on prisons and on crime rates. The prison variables used are 1) the total prison population in each state, 2) the number of admissions during each year, and 3) the "prison term index," which is the total population divided by the number of departures, and it provides an estimate of trends in prison terms. All pertain only to prisoners sentenced to more than one year. The crime rate variables are the seven types of crime that make up the Uniform Crime Reports crime index - that is, murder, rape, assault, robbery, burglary, larceny, and auto theft.

The regression analysis found little relationship between sentencing laws and the prison and crime variables. In the aggregate, neither the sentencing reforms nor the weapon laws show

statistically significant relationships with any of the three prison variables. Some of the sentence reform types, however, do show relationships: presumptive sentencing led to more admissions and higher prison population, and sentencing guidelines are followed by lower populations. But these impacts differ greatly from state to state, and their magnitudes are usually small, such that the laws on the average led to only about a ten percent change in prison admissions or population. On the other hand, the determinate sentencing laws, but not the presumptive sentencing laws or the sentencing guidelines, are followed by sizeable increases in the prison term index, indicating that the laws led to longer sentences.

The impacts on crime rates are also slight. The regression analysis found statistically significant increases in (1) murder and robbery rates after sentencing guidelines and (2) robbery rates after presumptive sentencing laws went into effect. On the other hand, the weapon laws were followed by statistically significant, but small, reductions in murder and robbery. The magnitude of these effects is small, rising or lowering crime rates by approximately five percent.

Similar patterns were found with respect to the impact of speedy trial laws. They were followed by slight increases in prison population, but they apparently had no lasting impact on prison admissions and crime rates.

This general picture of small impacts, if any, of the various laws studied stands in sharp contrast to regression results finding

that other factors greatly affect prison variables and crime rates. As one might expect, higher crime rates are followed by more prison admissions. The size of the high crime age groups, teenagers and young adults, had strong positive relationships with most types of crime. The portion of the populations in the young age group had no discernable impact on prison population, but it was negatively associated with admissions and positively associated with the prison term index; in other words, it appears that the recent growth in high risk age groups has led states to be less likely to imprison offenders committing the least important crimes.

Overall, therefore, our research confirms the prevailing views among researchers that sentencing laws cannot be held responsible for the growing prison population and that they do little to dampen growing crime rates.

1. Michael H. Tonry, Sentencing Reform Impacts 3-4 (Washington: National Institute of Justice, 1987).
2. See id. at 11-17; Jacqueline Cohen and Michael Tonry, "Sentencing reforms and their impacts," in Alfred Blumstein, et al. (eds.), Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates (Washington: National Academy of Sciences, 1983).



## Chapter 1. INTRODUCTION

### 1.1 Sentencing Reforms

Criticism of indeterminate and uneven sentences in the early 1970's caused many states to enact major sentencing reforms by the early 1980's (Shane-DuBow, Brown, and Olsen 1985; see Tables III-3 and III-4). The major reforms in the felony courts were determinate sentencing, presumptive sentencing, sentence guidelines, and mandatory minimum sentences for crimes conducted with deadly weapons. Although law makers' motives often are not clear (Casper and Brereton 1984),<sup>1</sup> it is apparent that a major goal was to replace rehabilitation with punishment as the dominate justification for sentencing<sup>2</sup> (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

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1. The references are to writings in one of the three bibliographies in section 8. The sentencing law and speedy trial bibliographies contain writings specifically on those topics. All other references are in the methodology bibliography.

2. Another important goal of sentencing reform, not directly relevant here, is to reduce unfairness involved in sentence discrepancy.

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 impacts of the laws, and not on the ultimate goals of the laws. Several studies have concluded that lawyers and judges often circumvent determinate and, especially, minimum sentence laws, but overall the laws result in more and longer prison sentences for less extreme crimes (Heumann and Loftin 1979; Loftin and McDowall 1981; Casper, Brereton, and Neal 1982). Research has found that sentence disparity is reduced by determinate sentencing laws (Clark 1984; Casper, Brereton, and Neal 1982:149-158), and by presumptive sentence guidelines (Moore and Meithe 1986; Kramer and Lubitz 1986; Knapp 1984; von Hirsh, Tonry, and Knapp 1987), but not by voluntary guidelines (Rich et al. 1982; Carrow 1985). Some researchers concluded that guilty plea rates increase (California Judicial Council 1987), but others found little or no impact (Clarke 1984; Casper, Brereton, and Neal 1982). Heumann and Loftin (1979) concluded that Detroit's minimum sentence law reduced delay, a

finding contested by Cohen and Tonry (1983:335-37). Clarke (1984) concluded that the North Carolina determinate sentence law probably had no impact on delay, whereas Marvell, Moody, and Luskin (1988) found that the North Carolina law, as well as the California determinate sentencing law, increased delay slightly.

These immediate impacts, however, have only modest policy implications. The truly important questions are whether the ultimate goals of sentence reform have been furthered - that is, the impacts on prisons and on crime rates. The research here has not been extensive.

#### 1.1.1 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 and Brereton 1984:123-131). Also, 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). Nevertheless, after reviewing the existing research, a National Academy of Science (NAS) panel concluded (Blumstein et al. 1983:32-33): "Prison populations increased steadily in the 1970's, and further increases are projected through the 1980's. This growth in prison populations appears to continue preexisting trends and is only marginally related to recent sentencing reforms." In

fact, looking at nation-wide trends, both the number of prison commitments and the size of prison population increased at a rather steady pace from the early 1970's to the mid-1980's. If anything, the growth rate declined in the late 1970's when most determinate and mandatory minimum statutes went into effect (Bureau of Justice Statistics 1985a; Bureau of Justice Statistics 1986a). But national trends may mask the effects of laws in individual states.

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. Imprisonment had also increased in the two years before the reform, leading the authors to conclude that the argument 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). Similarly, 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 12% increase in prison population between 1982 and 1983, as opposed to 47% of the states with indeterminate sentencing.

On the other hand, the NAS report contains an indirect, indication that sentencing reform has a major impact. Blumstein (1983) used regression procedures to forecast Pennsylvania trends in prison populations should proposed mandatory minimum sentencing legislation be passed, and he concluded that the law would cause an increase of approximately 50 percent. The forecast proved accurate: the graph in Blumstein (1983:483), adjusted three years later due to the fact that the law was passed in 1981, rather than 1978 as assumed in the study, is close to the actual prison populations of 11,767, 13,094, 14,227, and 15,201 in 1983 through 1986. The accuracy of this forecast provides strong evidence that the Pennsylvania law had a major impact on prison population.

The difference between findings of Casper, Brereton, and Neal (1982) and Blumstein (1983) may be caused by the fact that California enacted a determinate sentencing law and Pennsylvania a mandatory minimum law; still there is a striking contrast between Blumstein (1983) and the conclusion in Blumstein, et al. (1983), quoted above, that sentencing reform has little impact on prison populations.

Another 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.

### 1.1.2 Impact on crime.

The studies of the impact of sentence reforms on crime are city-wide time series evaluations of laws establishing mandatory minimum for crimes committed with deadly weapons. Most find little or no impact (Loftin and McDowall 1981 and 1984; Loftin, McDowall, and Heumann 1983), with apparently only one exception (Pierce and Bowers 1981).

The conclusion that sentencing reforms fail to meet their major aims is startling, especially in view of common finding that certainty in punishment is a deterrent. But, as often happens in evaluation research, the lack of results may simply be due to the use of research designs that are not able to distinguish the impact from the surrounding noise. First, the studies assume that any impact would occur as soon as the law goes into effect. But, as stressed by Casper and Brereton (1984:132-138) and Cohen and Tonry (1983:442-444), the effects of sentencing laws may not be felt for months or years. The inertia of standard operating procedures may delay putting the law into actual operation, and potential criminals may not comprehend the impact of the law until acquaintances are affected by it. Second, the research attempted few if any controls for other factors that may affect crime rates, such that the findings may simply reflect the researchers' inability to disentangle the effects of the sentencing laws from the other changes taking place at the same time. (A partial exception is

Loftin and McDowall [1984], who compared the impact on armed and unarmed crimes, finding little difference.)

### 1.2 Speedy Trial Laws.

Speedy trial laws are perhaps the most important response in the past several decades to felony court delay. Their announced goals were to aid both defendants and the public. Speedy trial shortens pretrial detention and the anxiety of awaiting trial. The public is protected by 1) reducing the loss of convictions because the state's case dissipates when, for example, witnesses become reluctant or their memories fade, 2) reducing the amount of crime by defendants on pretrial release, and 3) "maximiz[ing] the deterrent effect of prosecution and conviction" (American Bar Association 1968:10-11; see also Partridge 1980:11-13, and Garner 1987:29-30). That is, speedy trial laws are, in part, designed to increase the number of convictions and to reduce the amount of crime (the latter, again, through both incapacitation and deterrence).

During the past three decades, most states and the federal government enacted speedy trial laws that ostensibly require courts to bring defendants to trial within a specified period, usually some four to six months. At present 29 states have speedy trial statutes or court rules (Table III-5). The time limits vary greatly in strictness, especially due to differences in tolling provisions, which stop the clock for specified

reasons, such as the defendant's failure to appear (see Fort 1978; Poulos and Coleman 1976).

Speedy trial laws are typically evaluated with reference to the goal of reducing delay. In this regard, commentators and researchers initially greeted the laws with considerable skepticism. Feeley (1983) considered them among the "reforms that failed," largely because of provisions for tolling time limits. Several cross-section surveys of state laws support Feeley's contention. Church et al. (1978: 47-49) found no relationship between the extent of delay in ten metropolitan courts and the length of the speedy trial limit. A similar study of eighteen courts by Mahoney (1988:63) found that speedy trial laws are not "clearly correlated" with case processing times, but no correlation data are given. A student note found no relationship between delay (as measured by the ratio of dispositions to filings) and the structure of speedy trial laws in seven states (Comment 1972). These studies, however, are not persuasive because cross-section comparisons are a poor design for studying causation, and the sample sizes are much too small to rule out relationships, even substantial relationships.

The 1974 federal speedy trial law, like the state laws, was viewed skeptically largely because of the tolling provisions (e.g., Ames et al. 1980). Using median processing time statistics, Bridges (1982) argued that delay changed little from 1971 to 1981, but a single time series of eleven years is not sufficient for proper analysis. The common recommendation is



that a single time series should have at least 50 time periods (see Cook and Campbell 1979: 228). Garner (1987: 235), in addition, faulted Bridges for not considering other delay measures that suggest the federal law may have an impact.

Several studies, on the other hand, give partial evidence that speedy trial laws can reduce delay. Grau and Sheskin (1982a, 1982b) conducted individual case time series regressions, with a sample of 2,267 cases filed in 1967-1977 in three Ohio courts. They found that processing times were substantially less for cases filed in the years after the 1974 speedy trial law went into effect in two of the courts, even after controlling for numerous features of the cases such as pre-trial custody status.

Flemming, Nardulli, and Eisenstein (1987) studied 7,475 cases in nine courts - three each in Illinois (which has a 120 day speedy trial rule), Pennsylvania (180 day rule), and Michigan (180 day rule). The authors found no relationship between processing time and the length of the time limits, after controlling for features of the cases. They concluded, nevertheless, that speedy trial laws have an impact because cases subject to laws with less "bite" have longer processing times (this is really a comparison between court that are subject to and not subject to speedy trial laws, since Michigan, the state with the least "bite," really did not have a speedy trial law (see Section 3.5). This research, however, is essentially a cross section-study of three states. The greater

delay in states with weaker laws could be caused by numerous other differences between the three states not accounted for by other variables in the analysis.

Garner (1987) used nation-wide data for federal courts over 150 months to explore the impact of the federal speedy trial law on three delay measures. He found a significant reduction in mean and median times, but not in the time for the 90th percentile case (Garner 1987: 240-242).

Marvell and Luskin (1990) using a time series-cross section design found that the North Carolina speedy trial law had a major impact on delay reduction, but the Connecticut law had no effect.

Finally, several descriptive studies of the Philadelphia Court of Common Pleas found that case scheduling is constantly dictated by the need to comply with the state's strict speedy trial limits (Church et al. 1978: 42-43; Marvell 1982; Schulhofer 1984), but these studies provide no evidence concerning actual impact on case processing time.

Thus, the research to date does not present a clear picture of the impact of speedy trial laws on court delay. The varying results may be due to methodological differences; for example, all studies finding no relationship have very inadequate sample sizes. The best-designed studies, especially Garner (1987) and Grau and Sheskin (1982a, 1982b), do find impacts.

This is as far as the research has gone, however. Although the research suggests that the speedy trial laws can accomplish

their primary goal of reducing felony court delay, it has not addressed the more important ultimate goals. To the extent that the goals of reducing delay are accomplished, one can expect that more defendants are convicted and sentenced after the laws go into effect. This would be expected to result in a short term jump in prison admissions; the level of admissions would soon return to their prior levels as the new, more expeditious court processing times become established. The impact on prison populations, however, should be more permanent. Any jump in admissions when a speedy trial law goes into effect would be translated to higher prison populations for several year. The impact on crime rates, if any, is probably far more tenuous. The deterrent impact of "swift" and certain justice depends on potential offenders' knowledge concerning changes in court procedure, and it is far from clear that such information is well disseminated. Also, the differences on processing time may not be sufficient to change potential offenders' concerns about swiftness. Finally, the incapacitation impact of any increase in prison population is greatly dampened because many the additional inmates due to the speedy trial laws would have been in pre-trial detention otherwise.

## Chapter 2. RESEARCH DESIGN

The research deals with questions of causation, and causation in social science research involves numerous thorny issues of research strategy and statistical analysis. Perhaps the most detailed analysis of such problems encountered in this type of research is found in the NAS review of deterrence research (Blumstein, Cohen, and Nagin 1978). It stressed simultaneity problems, missing variable bias, ratio variable construction, and data uncertainty. Particularly common are spurious relationships when variables on both sides of the equation contain a common elements, for which the data are not completely trustworthy. The more recent NAS study on sentencing also concludes that research designs are usually inadequate: "[A]lmost all impact studies suffer from methodological problems that limit or ability to attribute these changes to the sentencing reforms" (Blumstein et al. 1983:31; see also Cohen and Tonry 1983).

In addition, regression analysis often involves numerous technical problems that must be overcome, including adequate sample size, autocorrelation, and heteroscedasticity. A final problem is the need for replications and robustness checks. To be given much weight, especially for the important policy issues

involved here, research must be replicated (e.g., Campbell 1984 and 1986).

This section describes the research design, the statistical techniques applied, and the form of the variables entered. The following chapter describes the specific variables in more detail.

### 2.1 Description of the pooled time series-cross section design.

The pooled design, of course, combines data from several units, states in this analysis, over a number of years. The more years and the more jurisdictions, the better. The design has long been used in economics, and the most common form of analysis, which we will use here, is the fixed effects model (Mundlak 1978; Pindyck and Rubinfeld 1981; Hsiao 1986). A second common model, the random effects model, is less useful in this application (Mundlak 1978): it assumes that state specific effects are not correlated with exogenous variables, it requires the same times series for each state, and correction for autocorrelation is difficult. In order to distinguish the two approaches, we write the error term as:  $u'_{kt} = V1_k + V2_t + V3_{kt}$ , here  $V1_k$  is a cross section component,  $V2_t$  is a time series component and  $V3_{kt}$  is a truly white noise component. If we assume that the cross section and time series components are constant, then we have the fixed effects model. If we assume that they are random, then we have the random effects model. Another model, Kmenta (1971), is rarely used, and will not be

applied here because it makes the unrealistic assumption that there are no state effects.

The fixed effect model, which is an analysis of covariance, creates a dummy variable for each state in the analysis (except the first) and for each year (except the first), and the coefficient associated with the variable is an estimate of the influence of specific factors ("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 all the analyses here still have 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:

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

where  $X_{1t}$  represents the discrete changes evaluated - that is, dummy variables representing the sentencing or speedy trial laws, and  $Y_{1t}$  represents the control variables (such as age structure and economic trends),

and  $W_{it}=1$  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 causation research in the law and society field, especially suitable in the United States where the states can be used as separate units for analysis. Blumstein, Cohen, and Nagin (1978:11,14) give as one of their major recommendations for future deterrence research the use of time series cross section data, decriing the lack of sufficient longitudinal data for single time series designs.

Nevertheless, the pooled design was largely ignored in criminal justice research until recently. Berk et al. (1979) are apparently the first to use it, followed by a rapidly increasing number of others (Wolpin 1980; Cook and Tauchen 1984; Moody and Marvell 1987). 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, of course, has a much larger sample size than a cross section study and, especially, a single time series. The main advantage is the ability to enter control variables, which are often lacking in time series research because there are not enough 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 (much more so than, for example, age structure variables), such that single time series analyses probably encounter severe specification bias.

The pooled design permits us to limit data to states and years where data are most accurate and most relevant to present day concerns. It permits time series analysis without using the less reliable FBI statistics of the 1930's to 1960's, and results based on data for recent years have clearer policy implications than results based on historical data.

### 2.3 Level of analysis

The research is conducted at the state level, using only state-level variables. This 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 policy changes, new speedy trial or sentencing laws, 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.

#### 2.4 Variable form.

This section discusses several issues concerning the form of the continuous variables in the analysis. The use of dummy variables for the laws is discussed in the next chapter.

It is necessary to use ratio variables for all continuous variables because of the vast differences in state size. Without ratio variables, the variation in prison population, crime rates, and other continuous variables would be much greater for larger states (while the variation of the dummy variables does not change), such that the regression results concerning the impact of the laws 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. While researchers typically use population as the denominator for crime variables,

they often use other denominators for prison variables, such as crime or court filings. In general, though, per capita data are preferred for the practical reason that population data change little from year to year and, thus, are less subject to error that would cause spurious relationships (Gibbs and Firebaugh 1990). The variables, except dummy variables, are in the form of natural logs to limit the impact of outliers.

A major issue is the time period in which the sentencing laws or speedy trial laws are likely to operate. As discussed earlier, the impact is likely to be delayed substantially, but there is no a priori reason to specify any particular lag. Hence, we explored reasonable lags, current year through the prior two years, and found that the lag of one year generally shows the most impact.

#### 2.5 Other statistical issues.

Autocorrelation, found in all analyses, was dealt with by using the standard correction with state-specific autocorrelation coefficients (Pindyck and Rubinfeld 1981:258-59), which means that we lost the first year of each analysis. The Durbin-Watson figures were in the .6 to 1.0 range; and the figures after correction are given in the tables. Heteroscedasticity problems occurred because ratio variables led to more variability in the small states (e.g., prison population and crime rates undergo greater percentage year to year changes in small states). This problem was corrected by using weighed regressions weighting by the square root of population.

### Chapter 3. VARIABLES

This chapter first describes the dependent variables (prison commitments, prison population, prison term index, and crime rates), then the target variables (sentence reforms and speedy trial), and finally the exogenous control variables (such as economic and demographic variables). The time span covered by the analysis is dictated by the availability of data. The earliest year for any analysis is 1970, primarily because demographic variables are not available for earlier years. The final year for the data is either 1988 or 1989. The following discussion describes the variables used, the variable labels, the data sources, and the procedures for deleting inadequate data.

The prison, crime, and personal income variables are all divided by 1,000 population (variable POP).

#### 3.1 Prison variables.

The study uses three basic prison variables: Prison admissions, prison population, and prison departures. The latter are used to create the "prison term index," the number of departures divided by the prison population. For all variables the figures 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. Before 1972 the data are for felonies, and afterwards they are for prisoners with sentences of over one year. Also, states were probably less consistent in applying the data categories before 1972. (1972-3 Prisoners in State and Federal Institutions). Prison population data are used since 1971. Prison departure data does not start until 1974.

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. Footnotes in the BJS publications indicate many problems that may hinder comparability of these data from year to year and state to state. These are discussed below with reference to specific categories of data.

The basic prison variables in the data set are listed below (with the years for which data are used). The variables actually used in the regressions presented here are indicated with an asterisk, and they are further described below.

PRCE      Prison population at year end (1976-89).

PRCYE\*    Prison population, prisoners sentenced to more than a year, at year end (1971-89) (PRCYE + PRADJ = total prisoners, including those in local jails due to overcrowding).

PRAT Total prison admissions, sentences of more than a year (1974-88).

PRAC\* Court commitments to prison, more than a year (1974-88).

PRA\* Court commitments (PRAC), plus escapees and AWOLs returned and parole or other conditional release violators returned (1972-88).

PRDT\* Total departures, prisoners sentenced to more than a year (1974-88).

PRDD Departures due to death, prisoners sentenced more than year (1974-88).

PRDJ Departures due to transfers to other jurisdictions, prisoners sentenced more than a year (1974-88).

PRADJ\* The number of prisoners in local jails due to overcrowding and not counted in the prison population (coded as missing data when the number of prisoners in local jails are not known) (1971-89).

The data for these variables often have problems, and considerable effort was made to mitigate the them. Table III-1 gives the problems that were mentioned in the Bureau of Justice Statistics reports, which are the source of the data. The first column, "estimates used," refer to BJS notations, otherwise unexplained, that the figures were estimates. The other columns are described in the following pages.

Table III-1

Prison Data Problems 1972-89<sup>1</sup>

	Estimates used	Includes some sentenced to one year or less	Excludes split sentence	Custody data <sup>2</sup>	Court commitment includes some returned from parole or prob.
01 Alabama	-	-	-	-	83+
02 Alaska	72-82	-	to 76	-	83+
03 Arizona	-	-	79	-	79
04 Arkansas	75	81,88	-	-	-
05 California	-	-	-	82+	-
06 Colorado	74	71-4,86+	-	-	77
07 Connecticut	75-6	-	-	-	77-83
08 Delaware	71-2	-	76+	-	76+
10 Florida	-	-	-	83+	83+
11 Georgia	71-4	-	77-8	83+	74,77
12 Hawaii	81	-	-	-	-
13 Idaho	-	-	-	-	75
14 Illinois	75-81	81	-	85+	-
15 Indiana	72-4,78-9	-	-	85+	-
16 Iowa	-	81-2	-	81+	-
17 Kansas	-	-	-	-	-
18 Kentucky	-	-	-	-	-
19 Louisiana	71-2,75	-	-	-	-
20 Maine	79,87	77-8	-	-	75
21 Maryland	-	71-81	-	-	77-81
22 Massachusetts	76,82	72-4,85-7	-	-	81+
23 Michigan	-	-	-	83+	-
24 Minnesota	72-4,78	-	-	-	-
25 Mississippi	-	74-6	-	-	82-3
26 Missouri	72-4	-	-	-	86+
27 Montana	-	-	-	-	-
28 Nebraska	-	76	-	-	-
29 Nevada	-	-	-	-	-
30 New Hampshire	-	-	-	-	83+

Table III-1 (cont.)

	Estimates used	Includes some sentenced to one year or less	Excludes split sentences	Custody data <sup>2</sup>	Court commitment includes some returned from parole or prob.
31 New Jersey	72-4	-	-	-	-
32 New Mexico	-	-	77-8	-	-
33 New York	-	-	-	-	85+
34 North Carolina	72-7	-	77+	86+	-
35 North Dakota	-	78	-	-	75,85+
36 Ohio	-	83+	-	-	75+
37 Oklahoma	72-4,83	83-4	-	-	75,82-3
38 Oregon	-	74	-	-	-
39 Pennsylvania	-	75	-	-	76
40 Rhode Island	73-4	-	77+	-	-
41 South Carolina	72-5	74	-	-	75,78
42 South Dakota	84	-	-	-	-
43 Tennessee	-	79-80	-	-	-
44 Texas	-	-	-	78+	82
45 Utah	-	-	-	86	-
46 Vermont	73,81-2	-	76-8	-	76,86
47 Virginia	73-4	74	-	-	74-5
48 Washington	-	-	-	-	-
49 West Virginia	75	84	-	86+	79,83+
50 Wisconsin	82	-	-	-	-
51 Wyoming	86-7	-	-	87	-

1. This information is derived from footnotes and other comments in the Bureau of Justice Statistics reports, Prisoners (1971-1983) and Prison Admissions and Releases (1983-87), and Correctional Populations in the United States (1986-87). Information about admissions and releases is not available for 1985 and 1988, and it is assumed that problems for the prior year continue. When year spreads are given, there may be years in between where there problem was not mentioned. It is likely that the problems exists for years not given but are not noted; especially the problems may exist for the early years even though only mentioned for the first time in later years.

2. Prisoners sentenced to more than one year, with part of the sentence suspended, such that the part not suspended is under one year.

Table III-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



### 3.1.1 Prison population.

The prison population (PRCYE) is the number of prisoners in custody sentenced to more than a year, taken at the end of the year. These data start in 1971 and the last year is 1989. They are the revised figures, taken from the next year's report (e.g., prison custody figures for 1978 are taken from the 1979 Prisoners), because they are regularly revised. The revised versions are not yet available for 1989, however. (It should be noted that even the prior year data in the Sourcebook of Criminal Justice Statistics are not the final versions.)

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 (we assumed that such an error of five percent or less would not affect the regression results). Most instances where the prison data did not include such jail inmates occurred before 1977 when the data were based on custody. However, even afterwards several states did not include these jail prisoners 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 such prisoners in jail 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 for the four states where the adjustments for 1976 were more than 5 percent of the number of prisoners sentenced to more than one year (Alabama, Louisiana, Mississippi, and Virginia) the data are considered missing prior to 1976 (see Table III-2).

After 1977 several states did not include prisoners placed in local jails because of overcrowding in the prison population data, but usually for only short periods. These states are 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). These prisoners were added to the prison populations if they were more than 5 percent of total prison population.

The variable PRADJ includes the number of prisoners in local jails due to overcrowding - that is, the data for 1976 in Alabama, Louisiana, Mississippi (1976 and 1977), and Virginia, plus the figures for the states and years listed in the above paragraph. PRADJ is scored as missing data before 1976 for Alabama, Louisiana, Mississippi, and Virginia, as well Vermont for 1971.

The prison population dependent variable in the regressions is the sum of prison population as given (PRCYE) and the adjustments for jail inmates (PRADJ).

Table III-1 includes other irregularities noted in the BJS prisoner reports for prison population data.

### 3.1.2 Prison admissions.

The data for prison admissions are for prisoners with maximum sentences of more than a year. Court commitments (PRAC) are those admitted with new sentences, excluding, in most states, those readmitted after parole or probation violation (but they do include prisoners admitted for the first time after probation violation).

The basic measure of admissions used in the regression analysis is PRA, includes court commitments (PRAC), parole or

conditional release violators returned, and escapees returned; the latter comprise approximately 30% of PRA. It is used as the major prison admission variable rather than PRAC because data are available for 1971-73, and because PRAC often includes these two additional categories in any event (see Table III-1).

Total admissions (PRAT) is PRA plus "transfers from other jurisdictions," "return from appeal bonds," and "other admissions." A major problem with this measure is that it includes "returns from authorized temporary absences" for some years for some states (see the discussion of total departures below). This measure is not used in the analysis.

In some states, all or some of the data for PRA are not useable. 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 admissions 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 III-2.

### 3.1.3 Prison departures.

Prison departures (PRDT) are the total prison departures ("total releases" in later BJS reports), available since 1974. There are two widespread problems with these data. First, the data for the number of releases for some years and some states include departures for authorized temporary absences (such as

court appearances). Thus for 1974 data for Connecticut, Indiana, Louisiana, Michigan, Texas, Virginia, and Washington are not used. Data for 1974 and later years are not used for several more states: California (before 1980), Florida (1978), Kansas (1978), Massachusetts (1976), and Pennsylvania (1976).

Second, the data exclude departures from jails when prisoners are placed in jail due to prison overcrowding and the state does not include these inmates within the data for prison population. This problem is discussed above concerning prison admissions and population, and whenever the admissions data are not usable for this reason, the departures data are also not useable. Table III-2 lists the states and years where the data are not available for prison departures.

#### 3.1.4 Prison term ratio.

The measure of prison term used here is the "prison term ratio," 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 the 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, average sentence length can increase when prisoners' terms are shortened because the authorities release more long term prisoners than

usual. Hence, the release ratio is better indicator of current practices.

### 3.2 Crime Rates.

The dependent variables, state crime rates, are the seven components crime index of the uniform crime reports:

- murder and non-negligent manslaughter (CRMUR)
- forcible rape (CRRAP)
- robbery (CRROB)
- aggravated assault (CRASS)
- property crime (CRPRO)
- burglary (CRBUR)
- larceny-theft (CRLAR)
- motor vehicle theft (CRMV)

Another measure of crime used is the "major crime" index which consists of murders, nonneglegint manslaughter, forcible rape, robbery, aggravated assault, and burglary (that is, the total crime index less larceny and motor vehicle theft). This is used as an independent variable in the analyses of prison population and as a dependent variable in the study of the impact of speedy trial laws on crime. There are several reasons why we used this measure, instead of the total crime index. First it is the measure that the Bureau of Justice Statistics used when comparing prison admission trends to crime rates. Second, convictions for the included crimes result in more and longer prison sentences than larceny and vehicle theft and the

average sentence varies from 75 to 221 months for the former, and 46 months for the latter. (Bureau of Justice Statistics 1990). Third, 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 larger relationships.

The crime rates, as independent variables, 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).

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. They are not comparable to later data because they exclude thefts involving property worth \$50 or less.) The crime data are the adjusted statistics published in the succeeding year Crime Reports (i.e., 1980 data were taken from the 1981 Crime Report, and so on).

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, that is after 1970, 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; Belsly, Kuh, and Welsch 1980) to uncover observations that may unduly affect the result. The only important problems uncovered were crime data in Illinois (the Chicago police seriously undercounted crime before 1984, Federal Bureau of Investigation 1986, pp. 4-5), and prison population in California in the early 1970's, but the results changed very little when these states were left out.

### 3.3 Other Variables.

The analysis includes a large number of control variables, the most important of which are the state and year dummies described in Chapter 2. The use of crime rates as control variables in the analysis of prison variables is described in Section 3.2. Other control variables, discussed below, are age structure and economic conditions.

#### 3.3.1 Age structure variables.

Age structure is often said to be an important factor in crime and prison population trends. Crime is disproportionately committed by 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 only since 1970, and the age categories relevant to our study are limited to 15 to 17 years old, 18 to 24, and 25 to 34. 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 was dropped in the autocorrelation correction.

The absolute numbers of persons, in 1000's, in the three age groups are found in the variables P15T17, P18T24, and P25T34. Each is divided by the total population in 1000's (PTOT).

### 3.3.2 Real personal income.

The final control 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 gainful 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. (That is, the final variable is TPI multiplied by DEFL, divided by POP, and divided by 100.) 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 sentencing reforms evaluated in this research fall into two distinct categories, broad sentencing reforms and laws that mandate enhanced sanctions for crimes committed with deadly weapons. The first category consists of three types: determinate and presumptive sentencing laws and sentencing guidelines.

We have not included several other types of sentencing reforms because they are not amenable to the research design employed here. The major example is laws pertaining to repeat

and habitual offenders; most states had such laws before the period covered by research (Hand and Singer 1974; Uniform Law Commissioners 1985: 255-257), and the laws are frequently revised such that it would be difficult to determine when the laws are expected to have an impact. Because the study is limited to sentencing laws, it does not include changes in parole procedure except to the extent that they are incorporated in determinate sentencing laws. We also leave out laws that were in effect for less than a year (e.g., New Jersey guidelines).

#### 3.4.1 Broad sentencing reforms.

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 III-3). 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 III-3

Sentencing Reforms 1969-1989

	Code Section	Law year & number	Effective Date	Type <sup>1</sup>
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
08 Delaware				
09 Dist. Col.				
10 Florida	Rule 3.701	--- <sup>2</sup>	10-1-83	G
11 Georgia				
12 Hawaii				
13 Idaho				
14 Illinois	1005-8-1	1978-1099	2-1-78	D
15 Indiana	35-50-2-1	1976-148	10-1-77	P
16 Iowa				
17 Kansas				
18 Kentucky				
19 Louisiana				
20 Maine	17A-1252	1975-499	5-1-76	D
21 Maryland				
22 Mass.				
23 Michigan	418 Mich lxxx	--- <sup>2</sup>	3-1-84 <sup>3</sup>	G
24 Minnesota	244.01	1978-723	5-1-80	G
25 Miss.				
26 Missouri				
27 Montana				
28 Nebraska				
29 Nevada	193.130	—	old	D

Table III-3 (cont.)

	Code Section	Law year & number	Effective Date	Type <sup>1</sup>
30 New Hamp.				
31 New Jersey	2C:44-1	1978-95	9-1-79	P
32 New Mexico	31-18-15	1979-152	7-1-79	P
33 New York				
34 North Car.	15A-1340.4	1979-760	7-1-81	P
35 North Dak.				
36 Ohio				
37 Oklahoma				
38 Oregon				
39 Penn.	202 Pa. Code 303-1	--- <sup>2</sup>	7-22-82	G
40 Rhode Is.				
41 South Car.				
42 South Dak.				
43 Tennessee	40-35-101	1982-868	7-1-82	D
44 Texas				
45 Utah				
46 Vermont				
47 Virginia				
48 Washington	9.94A.905	1981-137	7-1-84	G
49 West Va.				
50 Wisconsin	973.012	1983-371	11-1-85 <sup>3</sup>	G
51 Wyoming				

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.

### 3.4.2 Felonies committed with deadly weapons.

During the past two decades nearly all states enacted laws that increase penalties for crimes committed with deadly weapons, and at present 47 states have such laws for first time offenders. Approximately half apply to any deadly weapons, and half apply to firearms only (Table III-4). Thirteen laws broadly cover possession of a weapon while committing a crime, whereas most require that the defendant display or use the weapon in some manner.

For the present study we include only laws pertaining to broad classes of felonies and to first offenders. The laws are listed in Table III-4, along with their effective dates and summary descriptions of their contents. We exclude narrower laws, such as:

- laws that pertain to only specific felonies, such as burglary committed with a weapon, or narrow categories of felonies, such as those inflicting great bodily harm.

- laws pertaining to specific types of firearms, such as sawed-off shot guns and automatic rifles.

- laws dealing only with repeat offenders (and Table III-4 does not indicate the harsher penalties often given for defendants convicted of repeat crimes involving weapons).

We also do not include more general weapon laws, such as possession of an unregistered firearm or a concealed weapon,

that do not specifically give additional penalties for felonies committed with the weapons.

The weapons laws mandate either mandatory minimum sentences or enhanced sentences, or both. The mandatory minimums are sentences that the court must give if it finds that the defendant used or possessed a weapon as defined in the statute (but Hawaii, Oklahoma, and Wisconsin give judges discretion not to impose the mandatory minimum). The sentence cannot be suspended, and probation cannot be substituted for the prison term. In many states defendants can be released before the full term ends because of good behavior reductions.

These weapon laws also differ greatly in that some were enacted alone and some were part of legislation that made other changes to the sentencing system, often adding penalties for possession of weapons or for repeat offenders. The weapons laws went into effect at the same time, or with a few months of the time, that the state enacted broad sentencing laws in Alaska, Arizona, Connecticut, Indiana, Maine, and Pennsylvania (compare Table III-3 and Table III-4), and it is not feasible to distinguish the impact of the two types of laws.

We have categorized the laws for the purpose of the regression analysis according to three dimensions, which we hypothesize to have a greater deterrence impact. They are whether the law applies to any deadly weapon (not just firearms), whether possession is sufficient (use is not required), and whether the law has a flat mandatory minimum.



The first type is indicated in the "Instrument" column of Table III-4 by a "W," and the second is indicated in that column by the superscript "p". The third type consists of states with mandatory minimums, as indicated in the second-to-the left column in Table III-4, but excluding the states where judges have discretion not to apply the mandatory minimum (indicated by the superscript "6").

Variables representing sentencing laws and their aggregations are not entered into the data set; instead each regression program creates the variables.

#### 3.4.3 Research Procedures.

Finding the various sentencing laws and determining their effective dates was a difficult and time-consuming task. There are no extant surveys adequate for this purpose. The statutes of the various states are organized differently, the laws were often scattered in various parts of the statute books, and some of the reforms are in court orders or court rules, rather than statutes. The research was conducted by the project director, a lawyer, with limited help from law students. The results were then checked against law review articles and other secondary sources (listed in the bibliography in Section 8.1).

Table III-4

Sentencing Laws for Felonies Committed with Deadly Weapons, 1969-89

State	Code section	Law number	Effective date	Instrument <sup>1</sup>	Mandatory minimum	Enhancement
01 Alabama	13a-5-6	1981-840	5-27-81	W	10 yr. <sup>2</sup>	
02 Alaska	12.55.125	1978-166	1-1-80	WP	7 yr. <sup>2,3</sup>	
03 Arizona	13-604	1977-142	10-1-78	W	1/2 sent. <sup>2</sup>	oth. <sup>5</sup>
04 Arkansas	5-4-505	1975-280	1-1-76	F		15 yr.
05 California	P. 12022	---	old	W		1 yr. <sup>2</sup>
06 Colorado	16-11-309	1976-547	5-10-76	W	120 days	oth. <sup>5</sup>
07 Connecticut	53A-216	1981-342	10-1-81	F	5 yr.	
08 Delaware	11-1447	1973-203	7-1-73	WP	3 yr. <sup>3</sup>	3-30 yr.
10 Florida	775.087	1975-7	10-1-75	W	3 yr.	oth. <sup>5</sup>
11 Georgia	16-11-106	1976-1591	7-1-76	W		5 yr.
12 Hawaii	706-660.1	1976-204	6-7-76	FP	3 yr. <sup>6</sup>	
13 Idaho	19-2520	1977-10	7-1-77	W		15 yr.
14 Illinois	none					
15 Indiana	35-50-2-2	1976-148	10-1-77	W	NS <sup>4</sup>	
16 Iowa	902.7	1976-1245	1-1-78	FP, <sup>7</sup>	5 yr.	
17 Kansas	21-4618	1976-168	7-1-76	F	NS <sup>4</sup>	
18 Kentucky	533.060	1976-180	6-19-76	F	NS <sup>4</sup>	
19 Louisiana	893.3	1981-139	9-11-81	F	5 yr. <sup>3</sup>	
20 Maine	17A-1252	1975-499	5-1-76	W	1 yr. <sup>2</sup>	oth. <sup>5</sup>
21 Maryland	27-36b	1972-13	6-1-72	F	5 yr.	5-20 yr.
22 Mass.	265-18B	1974-830	11-12-74	F	2 yr.	2-5 yr.
23 Michigan	750.227b	1976-6	1-1-77	FP	2 yr.	2 yr.
24 Minnesota	609.11	1979-258	8-1-79	WP	1 yr. <sup>2</sup>	
25 Mississippi	none					
26 Missouri	571.015	1977-60	1-1-79	W	3 yr.	3 yr.
27 Montana	46-18-221	1977-584	1-1-78	W	2 yr.	2 yr.
28 Nebraska	28-1205	1977-38	7-1-78	WP		oth. <sup>5</sup>
29 Nevada	193.165	1973-759	7-1-73	W	NS <sup>4,7</sup>	oth. <sup>5</sup>
30 New Hamp.	651:2	1977-397	9-3-77	F	1 yr.	oth. <sup>5</sup>
31 New Jersey	2C:43-6	1981-31	2-12-81	FP	18 mo. <sup>2</sup>	

Table III-4 (cont.)

State	Code section	Law number	Effective date	Instrument <sup>1</sup>	Mandatory minimum	Enhancement
32 New Mexico	31-18-16	1977-216	6-17-77	F	1 yr.	
33 New York	Cr. 65.08	1980-233	6-13-80	F		oth. <sup>5</sup>
34 North Car.	none					
35 North Dak.	12.1-32-02.1	1977-12	7-1-77	W	2 yr. <sup>2</sup>	
36 Ohio	2929.71	1982-199	1-5-83	FP	3 yr.	3 yr.
37 Oklahoma	21-1287	1969-220	7-29-69	WP		2-10yr. <sup>3</sup>
38 Oregon	161.610	1979-779	10-2-79	F	5 yr. <sup>6</sup>	
39 Penn.	42-9712	1982-169	6-6-82	FP	5 yr.	
40 Rhode Is.	11-47-3	--	old	W	3 yr.	
41 South Car.	16-23-490	1986-462	6-3-86	WP	5 yr.	
42 South Dak.	22-14-12	1985-192	4-3-85	F	5 yr.	5 yr. <sup>3</sup>
43 Tennessee	39-6-1710	1976-768	7-1-76	F	5 yr.	5 yr.
44 Texas	Cr. 42.12	1977-347	8-29-77	W	NS <sup>4</sup>	
45 Utah	16-3-203	1976-9	4-1-76	F		1 yr. <sup>2</sup>
46 Vermont	13-4005	--	old	WP		oth. <sup>5</sup>
47 Virginia	18.2-53.1	1975-624	10-1-75	F	2 yr.	2 yr.
48 Washington	9.95.040	--	old	W		1-2 yr. <sup>3</sup>
49 West Va.	62-12-2	1979-87	6-8-79	F	NS <sup>4</sup>	
50 Wisconsin	939.63	1979-114	3-1-80	W	3 yr. <sup>6</sup>	
51 Wyoming	6-8-101	1982-75	6-1-82	F		oth. <sup>5</sup>

1. F = firearms. W = deadly weapon (or firearms and knives). P = Penalty for possession, in addition to use, of weapon (Minnesota, 3 year minimum for firearms).

2. Higher minimum or enhancement for more important crimes.

3. Penalties have changed: Alaska, 6 years for firearms to 1982; California, 5 years to 1977; Delaware, 5 years to 1981; Louisiana, court permitted to impose a lesser sentence after Sept. 9, 1988; Oklahoma, 2 to 5 years to 1982; South Dakota, from Oct. 1, 1977, to 1985, a class 2 felony; Washington, 5 year minimum to 1984.

4. NS = there can be no suspension of the minimum sentence.

5. Other: Arizona, up to double sentence; Colorado, up to twice maximum sentence; Florida, one crime class higher (1974 law); Maine, one class higher; Nebraska, a separate crime; Nevada, double sentence; New Hampshire, also a Class B Felony (650-A:1); New York, separate felony; Vermont, separate felony; Wyoming, up to 10 years.

6. The court has discretion not to apply the minimum.

7. Limited to major crimes.

#### 3.4.4 Effective Dates and Lags.

The effective dates of the laws, given in Tables III-3 and III-4, pertain to the date the crime was committed (except that a few sentencing guidelines apply to defendants sentenced as of the effective date). The variables are coded one for each full year in which the law was in effect, and zero for other years. For the year in which the law went into effect, the variable in the portion of the year (taken to one decimal place) in which the law was in effect, e.g., 0.5 if the law went into effect on July 1st.

One cannot expect that the impact of a law, if any, would occur immediately. The impact on prisons would not occur until the defendants were sentenced, typically several months after the crime was committed. 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 also probably would be delayed, since there is probably a significant delay before 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

the vast majority of instances where were at least four years before the law and eight afterwards.

### 3.5 State speedy trial laws.

Table III-5 lists the 29 state speedy trial rules that specify a definite time period within which the felony defendant must be brought to trial or else the case is dismissed, unless the time limit is tolled. We exclude less strict laws, including those that 1) pertain to misdemeanors rather than felonies (e.g., Montana), 2) require that the defendant be brought to trial within a specific number of court terms (e.g., West Virginia), 3) have extremely long time periods (Louisiana, with two years before 1981), 4) required that the defendant be released from pretrial custody if time limits are not met (e.g., Michigan and Rhode Island), 5) give the trial judge discretion concerning whether the case is to be dismissed (e.g., Nevada), or 6) pertain only to defendants incarcerated following conviction of another crime (e.g., Michigan).

Speedy trial laws, as defined above, do not exist in Alabama, Delaware, Georgia, Kentucky, Maine, Michigan, Minnesota, Montana, Nevada, New Hampshire, New Jersey, North Dakota, Oklahoma, Oregon, Rhode Island, South Carolina, Tennessee, Utah, Vermont, West Virginia, and Wyoming.

The research for state speedy trial laws was conducted in the same manner as the research for sentencing laws described earlier, that is through extensive research instate statutes

and court rules. The material was checked against the writings listed in the speedy trial bibliography (see Section 8.3). Especially, it was checked against prior surveys of speedy trial laws found in Poulos and Coleman (1976), Fort (1978), Misner (1983), Clifford and Roper (1986), and Boland and Sones (1986), although only the Misner work was at all complete and none provided information about the effective dates of the laws.

The regression analysis includes only laws in 15 states where the laws were effective after 1973, such that there is a sufficient period in the regression before the laws went into effect. These states are: Arizona, Arkansas, Connecticut, Hawaii, Idaho, Louisiana, Maryland, Massachusetts, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, South Dakota, and Texas.

Variables representing speedy trial laws and their aggregations are not entered into the data set, instead each regression program creates the variables.

Table III-5

State Speedy Trial Laws

State	Law <sup>1</sup>	Effective date	Time Limits	
			period <sup>2</sup>	begin point
02 Alaska	Rule 45	pre-72	120 days	charge/arrest
03 Arizona	Rule 8.2	9-1-73	120/150 days	arrest <sup>6</sup>
04 Arkansas	Rule 28	7-1-80	12 months <sup>3, 5</sup>	charge
05 California	Penal 1382	pre-72	60 days	indictment
06 Colorado	18-1-405 <sup>4</sup>	pre-72	6 months	plea
07 Conn.	Rule 956	7-1-83	8/12 mo. <sup>5</sup>	information
10 Florida	Rule 3.191	3-31-71	175 days	custody
12 Hawaii	Rule 48	1-1-77	6 months	charge/arrest
13 Idaho	19-3501	7-1-80	6 months <sup>3</sup>	indictment
14 Illinois	38:103-5	1-1-64	120/160 days	custody <sup>6</sup>
15 Indiana	Rule 4	pre-72	12 months	charge/arrest
16 Iowa	Rule 27	pre-72	90 days <sup>5</sup>	indictment
17 Kansas	22-3402	7-1-70	90/180 days <sup>3</sup>	arraignment
19 Louisiana	Crim. 701	9-11-81	120/180 days <sup>3</sup>	arraignment
21 Maryland	27-591	6-25-79 <sup>7</sup>	180 days	arraignment
22 Mass.	Rule 36	7-1-79	12 months	return date
25 Miss.	99-17-1	7-1-76	270 days	arraignment
26 Missouri	217.460	9-1-78	180 days	certificate
28 Nebraska	29-1207	4-30-71	6 months	indictment
32 New Mexico	Rule 5-604	7-1-72	6 months	arraignment
33 New York	Crim. 30.30	4-28-72	6 months	complaint
34 North Car.	15A-701	10-1-78	120 days	arrest
36 Ohio	2945.71	1-1-74	270 days	arrest
39 Penn.	Rule 1100	7/1/73	180 days <sup>5</sup>	complaint

Table III-5 (cont.)

state	Law <sup>1</sup>	Effective date	Time Limits period <sup>2</sup>	begin point
42 South Dak.	23A-44-5.1	7-1-85	180 days	1st appearance
44 Texas	Cr. 32A.02	7-1-78	180 days	commencement
47 Virginia	19.2-243	pre-72	6/9 months	probable cause
48 Washington	Rule 3.3 <sup>8</sup>	pre-72	60/90 days <sup>5</sup>	arraignment
50 Wisconsin	Rule 971.10	7-1-70	90 days	trial demand

1. The laws citation are either to the rule of criminal procedure or to statutory sections.

2. When two periods are given, the first is for defendants in pre-trial custody and the second is for those not in custody.

3. The speedy trial law superceded an earlier, less strict law. The former laws specified that the defendant must be brought to trial within a specified number of court terms, except in Louisiana the law had a limit of two years from filing to trial.

4. Also, Rule 48(b).

5. The laws have been amended to change the times: in Arkansas the times were 9 months if in jail and 3 terms if not to October 1, 1987; in Connecticut the times were 12 and 18 months from July 1, 1983, to June 30, 1985; in Iowa the time was 60 days until January 1, 1978; in Pennsylvania the time was 270 days until July 1, 1974; in Washington the time was 60 days for all defendants before November 17, 1978.

6. In Arizona the time runs from initial appearance for persons in custody, and in Illinois the time runs from when the defendant demands trial for persons not in custody.

7. Date of State v. Hicks 285 Md. 310, 403 A2d. 356 (1979) which held that violation of Section 591 results in dismissal.

8. Was Section 10.46.010.



#### Chapter 4 SENTENCING LAW IMPACT ON PRISONS

The first of three topics in this report is the impact of sentencing laws on prisons. To summarize what was said in Section 3.1, we look at three aspects of prisons: population, admissions, and prison term index. The laws are operationalized in three different manners, each used in separate regressions. They are, in descending levels of aggregation: 1) two aggregate variables, one each for the broad sentencing reforms and the weapon laws, 2) separate variables for three types of sentencing reform laws and three types of weapon laws, 3) separate variables for each law in each state.

The results for the three are presented in Table IV-1 through Table IV-3. The three sets of results, however, are not strictly comparable, because differences in data availability mean that the time periods covered by the analyses, and the number of laws included, are greater for prison population and less for the prison term index. As discussed in Section 3.1, the prison population data spanned the period 1971-1989, the prison admissions data 1972-1988, and prison term data 1974-88 (and the first year in each analysis is lost in the corrections for autocorrelation). The laws included in each analysis are indicated in Table IV-3.

#### 4.1 Prison variables.

When aggregated into single variables, neither sentencing reforms nor weapon laws show significant impacts on any of the prison variables, if one uses the standard .05 level to denote significance (Table IV-1). The coefficients for the prison term index, however, are significant to the .10 level; so there is a substantial chance that the sentencing laws, overall, do increase prison terms. In any event, the magnitude of the impact is small: the increase, if any, in prison terms resulting from sentencing laws is only in the order of 7 percent (because the continuous variables are logged, the coefficients on the law variables estimate the proportion of change due to the law). A different pattern emerges when the sentencing reforms are divided into the three principal components (Table IV-2). Presumptive sentencing was followed by significant increases in prison population and prison admissions, and sentencing guidelines by significant reductions. But the magnitude of these effects is tiny: presumptive sentencing produced only about twelve percent more prisoners, and sentencing guidelines only about eight percent fewer. This impact is tiny compared to the overall changes in prisons during the past two decades.

On the other hand, determinate sentencing (but not presumptive sentencing laws and sentencing guidelines) has a larger impact on the prison term index, resulting in approximately a 21 percent increase. This, however, is only based on laws in five states, Alabama, Arkansas, Colorado,

Connecticut, and Illinois (there are not sufficient data for the prison term index to study the impact in Maine and Tennessee); so this finding is only moderate evidence that the determinant sentencing laws have the impact, which was probably desired by their authors, of lengthening prison terms through reduction of prison officials' discretionary authority to release prisoners.

There is little evidence that any type of weapon law has a greater impact than suggested by the aggregate weapon law variable. Although laws covering deadly weapons, rather than just firearms, have a significant negative impact on prison populations, and laws that have a strict required minimum show positive impact, the significance levels are low and the coefficients are very small, suggesting only about a six percent change.

The analysis of individual laws (Table IV-3) again produces an overall pattern of little impact, although several laws do produce highly significant coefficients. It should be cautioned that these results are much less robust than the results for the aggregate variables. First, just as a matter of statistical odds, one could expect some laws - about one in twenty - to show statistically significant results either way. Second, there may be data quirks in individual states that we did not uncover and that produce spurious results. Third, in individual states, there may be other changes that were made around the time as the sentencing laws went into effect and that enhance or

sentencing laws went into effect and that enhance or counterbalance the impact of the sentencing laws. One example of the latter is the fact that the broad sentencing reforms and weapon laws occasionally occurred in the same year (see Table IV-3, footnote 2).

The aggregate impacts of sentencing laws on prison variables (see Tables IV-1 and IV-2) are largely the product of a few instances where there the laws were followed by large changes, either up or down, in the prison variables. There is no dominant pattern, either within the two broad categories of sentencing laws or within the six different subtypes. For example, the suggestion in Table IV-2 that determinate sentencing increases the prison term index is greatly influenced by the large coefficient in Arkansas, and the lack of impact of sentencing guidelines is largely a balancing between the large positive coefficient for Michigan and the large negative coefficient for Florida.

In all, Table IV-3 strongly suggests that the Arkansas determinate sentencing law lead to longer prison terms, which were somewhat counterbalanced by fewer admissions, and the Connecticut determinate sentencing law led to longer prison terms and probably more prisoners.

Presumptive sentencing in Alaska, Indiana, and perhaps Connecticut were followed by higher prison populations, due to various combinations of more admissions and longer prison terms. The California and New Jersey laws were followed by upswings in

prison populations, but we could not determine whether this was due to longer prison terms; on the other hand the presumptive sentencing law in North Carolina was followed by sharply fewer admissions and lower population.

Sentencing guidelines were followed by fewer prisoners in Florida and Washington, primarily the result of shorter prison terms in Florida and fewer admissions in Washington. On the other hand the guidelines were followed by higher prison populations in Michigan, the result of much greater prison terms.

The findings concerning individual state weapons laws are also erratic, but the results here suggest more strongly that there may actually be no impact. Only a small minority of the coefficients are statistically significant, and these are smaller and have lower significant levels than generally found for broad sentencing reform laws. Importantly, of the twenty states with weapons laws in the analysis of prison terms, only two, Idaho and New Hampshire, showed significant increases in the prison term index. It is probably safe to conclude, therefore, that the weapons laws had virtually no discernable impact on prison populations.

#### 4.2 Other Variables.

The results concerning the control variables are interesting, although they are not the focus of the study. The first such variable is major crime (UCR violent crimes plus

burglary), crimes that are most likely to result in prison sentences. The crime variable is lagged because, for one reason, it may take some months between the time the crime is committed and the offender is sentenced. Also, there are simultaneity problems in that more persons in prison may remove potential criminals, and more prison admissions and longer prison terms may deter prospective criminals. This problem is mitigated (but, because of serial correlation, not fully removed) by using a lagged value. The analyses in Tables IV-1 through IV-3 show that major crime rates are positively associated with prison admissions, but not with prison population. There is a suggestion that more crime leads to shorter prison terms, but the relationship is not statistically significant.

Because over 70 percent of state prisoners are between 18 and 34, we have included two age structure variables, the percent of population in the state between 18 and 24 and the percent between 25 and 34. These variables show strong negative relationships with prison admissions, such that even though the preponderance of prisoners fall in these age groups, their sizes are not associated with overall prison population size. A likely interpretation is that the criminal justice system responds to increases in high crime age groups by imprisoning a smaller percent so as not to overcrowd prisoners. The ones that are imprisoned, however, are likely to have received longer prison terms. In all, it seems that the criminal justice system

has responded to growth in high-risk age groups by sending to  
imprison only the most flagrant offenders, and it has responded  
to declines in high-risk age groups by passing out prison  
sentences more freely.

Although one might expect that better economic conditions  
might lead states to authorize more expansion of prison  
capacity, real personal income for the prior year has no  
significant impact on any of the prison variables.

Finally, the year and state effect are highly significant,  
although less so for the prison term index. That is, there are  
major factors associated with individual states and individual  
years that greatly influence prison population, prison  
admissions, and to a lesser extent prison terms, beyond the  
factors captured by the other independent variables in the  
analysis, such as the law changes and age structure.

Table IV-1

Impact of Sentencing Laws in the Aggregate on Prison Population

	Dependent Variable <sup>1</sup>					
	Population		Admissions		Term Index	
<u>Sentencing Laws (lagged)</u>						
Sentencing Reforms	.01	.43	.01	.52	.07	1.91
Weapon Laws	.02	1.22	-.01	.25	.05	1.76
<u>Other Variables</u>						
Major crime (lagged)	-.01	.23	.25	3.66***	-.16	1.56
Percent pop. 18-24	.13	.49	-1.75	5.01***	1.62	2.99**
Percent pop. 25-34	-.02	.09	-.94	2.90**	.93	1.70
Real per. inc. (lagged)	.01	.09	.25	1.46	.02	.09
<u>F Values</u>						
Year dummies	25.72***		8.82***		3.83***	
State dummies	28.38***		46.47***		11.85***	
Dependent var. mean	4.82		4.25		.67	
Degrees of freedom	806		655		550	
Adjusted R-square	.99		.99		.75	
Durbin-Watson	1.66		1.96		1.92	

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

1) The dependent variables and "other" variables are per capita variables (except the term index), and all are in natural logs. The two columns below each dependent variable are the coefficients and absolute values of the T Ratios.



Table IV-2

Impact of Sentencing Law Types on Prison Population

	<u>Dependent Variable<sup>1</sup></u>					
	<u>Population</u>		<u>Admissions</u>		<u>Term Index</u>	
<u>Sentencing Reforms (lagged)</u>						
Presumptive sent.	.11	3.18**	.12	2.69**	.01	.27
Determinate sent.	-.01	.31	-.05	1.01	.21	3.58***
Sent. guidelines	-.08	2.59**	-.05	1.30	-.03	.62
<u>Weapon Laws (lagged)</u>						
Possession laws	.02	.61	-.02	.49	.08	1.39
Any weapon	-.06	2.21*	-.06	1.77	.07	1.57
Required minim.	.06	2.34*	.03	1.00	.02	.55
<u>Other Variables</u>						
Major crime (lagged)	-.01	.25	.25	3.65***	-.14	1.35
Percent pop. 18-24	.15	.59	-1.55	4.40***	1.43	2.60**
Percent pop. 25-34	.12	.48	-.87	2.70**	.85	1.53
Per. inc. (lagged)	-.07	.59	.18	1.11	.02	.06
<u>F Values</u>						
Year dummies	25.05***		8.83***		3.83***	
State dummies	30.17***		43.25***		12.74***	
Dependent var. mean	4.82		4.25		.67	
Degrees of freedom	802		651		546	
Adjusted R-square	.99		.99		.73	
Durbin-Watson	1.66		1.96		1.95	

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

1) The dependent variables and "other" variables are per capita variables (except the term index), and all are in natural logs. The two columns below each dependent variable are the coefficients and absolute values of the T Ratios.

Table IV-3

Impact of Individual Sentencing Laws on Prison Population

	<u>Dependent Variable<sup>1</sup></u>							
	Population		Admissions		Court Commitments		Term Index	
<u>Sentencing Reforms<sup>3</sup> (lagged)</u>								
01 Alabama (1980) D	.04	.55	-.03	.24	.03	.24	.14	.99
02 Alaska <sup>2</sup> (1980) P	.32	2.93**	.14	1.36	.34	3.21**	.21	1.22
03 Arizona <sup>2</sup> (1978) P	.14	1.71	.16	1.42	.20	1.97*	.19	1.56
04 Arkansas (1981) D	.07	1.23	-.17	1.62	-.12	1.16	.43	4.50***
05 Calif. (1977) P	.17	2.58*	.09	.91	.35	4.10***	--	--
06 Colorado (1979) D	-.12	1.20	-.05	.43	-.07	.44	-.16	.92
07 Conn. <sup>2</sup> (1981) D	.12	1.92	.04	.36	.09	.90	.23	2.37*
10 Florida (1983) G	-.20	3.83***	.21	2.66**	.34	3.91***	-.36	5.29***
14 Illinois (1978) D	.06	1.06	.11	1.44	.12	2.17*	-.03	.27
15 Indiana <sup>2</sup> (1977) P	.23	3.89***	.19	2.99**	.30	3.54***	.30	2.77**
20 Maine <sup>2</sup> (1976) D	-.19	3.00**	-.67	9.77***	--	--	--	--
23 Michigan (1984) G	.16	2.52*	-.05	.56	.03	.34	.63	5.31***
24 Minn. (1980) G	-.24	1.57	.29	1.30	.66	2.50*	-.12	.48
31 New Jer. (1979) P	.22	2.92**	--	--	--	--	--	--
32 New Mex. (1979) P	-.24	2.10*	-.04	.29	-.24	1.35	-.25	1.38
34 N. Car. (1981) P	-.42	6.25***	-.31	4.08***	-.20	3.48***	-.13	1.87
39 Penn. <sup>2</sup> (1982) G	.11	2.31*	.09	1.11	.07	.62	.18	1.73
43 Tenn. (1982) D	-.24	6.11***	--	--	--	--	--	--
48 Wash. (1984) G	-.37	6.05***	-.30	2.54*	-.02	.14	-.26	1.65
50 Wisc. (1985) G	-.06	.98	-.17	2.18*	-.15	1.78	.18	1.60
<u>Weapon Laws<sup>4</sup> (lagged)</u>								
01 Alabama (1981)	.08	1.00	-.02	.20	-.10	.81	.18	1.35
04 Arkansas (1976)	-.02	.37	-.04	.42	.01	.11	--	--
06 Colorado (1976)	.00	.04	-.25	2.54*	--	--	--	--
10 Florida (1975)	-.05	.77	-.01	.08	--	--	--	--
11 Georgia (1976)	-.29	3.48***	-.06	.63	--	--	--	--
12 Hawaii (1976)	.27	1.84	.14	1.42	--	--	--	--

Table IV-3 (page 2)

	Population		Admissions		Court Commitments		Term Index	
13 Idaho (1977)	-.05	.51	-.27	1.88	-.26	1.02	.23	2.16*
16 Iowa (1978)	.12	1.55	.28	3.88***	.28	3.81***	-.13	.93
17 Kansas (1976)	.17	2.06*	-.06	.36	--	--	--	--
18 Kentucky (1976)	-.19	2.64**	-.09	1.63	--	--	--	--
19 Louisiana (1981)	.06	.91	.11	2.04*	.13	2.13*	-.12	1.37
23 Michigan (1977)	.05	.85	-.07	.82	-.16	1.53	--	--
24 Minnesota (1979)	-.12	.78	-.46	1.99*	-.74	2.72**	-.02	.09
26 Missouri (1979)	.04	.84	.12	1.51	.05	.56	-.07	.77
27 Montana (1978)	.05	.51	-.25	1.61	-.25	1.66	.28	1.84
28 Nebraska (1978)	-.22	3.11**	-.27	2.35*	-.18	1.43	.18	1.47
30 New Hamp. (1977)	.16	1.18	-.06	.34	-.07	.64	.64	3.69***
31 New Jersey (1981)	.11	1.69	--	--	--	--	--	--
32 New Mex. (1977)	-.04	.31	-.02	.15	.07	.33	-.14	.70
33 New York (1980)	.08	1.43	.20	3.62***	.17	1.80	-.04	.45
35 N. Dak. (1977)	-.13	1.13	.08	.53	.32	2.36*	.18	1.06
36 Ohio (1983)	.01	.23	-.06	.86	.02	.38	.12	1.32
38 Oregon (1979)	-.13	2.43*	-.08	.71	-.17	1.71	-.13	.83
41 S. Car. (1986)	-.02	.21	-.19	1.38	-.18	1.18	.24	1.74
42 S. Dak. (1985)	.07	.63	-.03	.19	-.08	.38	-.04	.27
43 Tenn. (1976)	.11	2.59**	.06	.73	--	--	--	--
44 Texas (1977)	.12	1.61	.11	1.90	-.08	.99	.12	.80
45 Utah (1976)	-.04	.44	.15	1.41	--	--	--	--
49 West Va. (1979)	-.12	1.19	-.03	.22	.18	1.58	-.05	.33
50 Wisconsin (1980)	-.05	.79	.06	1.03	.09	1.44	-.10	1.16
51 Wyoming (1982)	-.10	1.21	-.17	1.04	-.14	.78	.15	.92

Table IV-3 (page 3)

Other Variables	Population		Admissions		Court Commitments		Term Index	
Major crime (lagged)	-.07	1.55	.25	3.70***	.27	3.35***	-.02	.22
Percent pop. 18-24	-.18	.72	-1.79	4.74***	-1.53	3.71***	.31	.55
Percent pop. 25-34	.12	.48	-.94	2.58*	-.64	1.51	.40	.76
Per. inc. (lagged)	-.14	1.12	.32	1.89	.64	2.96**	.47	1.77

## F Values

Sentence reform	10.22***	9.26***		
Weapon laws	2.37***	2.55***		2.04**
Year dummies	23.61***	7.57***	5.87***	4.23***
State dummies	45.32***	36.71***	47.50***	11.29***
Dep. var. mean	4.82	4.25	4.05	.67
Deg. of freedom	757	609	536	516
Adjusted R-square	.99	.99	.99	.80
Durbin-Watson	1.61	1.81	1.84	1.94

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

1) The dependent variables and "other" variables are per capita variables (except the term index), and all are in natural logs. The two columns below each dependent variable are the coefficients and absolute values of the T Ratios.

2) The sentencing reform law went into effect on the same date, or nearly the same date, as a deadly weapon law.

3) The law types are: D - determinate sentencing, P - presumptive sentencing, G - sentencing guidelines.

4) The states not listed are 1) those with deadly weapon laws with effective dates corresponding to sentence reform laws (see note 2), 2) those without specific deadly weapon laws (Illinois, Mississippi, and North Carolina), and 3) those with laws effective before 1975 for prison population and admissions (California, Delaware, Maryland, Massachusetts, Nevada, Oklahoma, Rhode Island, Vermont, and Washington), and those effective before 1977 for the prison term index, and 4) states where the data for the independent variable are not useable for less than two years before or after the law.

## Chapter 5. SENTENCING LAW IMPACT ON CRIME RATES

### 5.1 Crime Rates.

The impact of the sentencing laws on crime rates follows a similar pattern. We conducted separate regressions with the seven UCR crime types as dependent variables, with data for 1972-89, after losing one year for autocorrelation correction (Tables V-1 to V-6). The results suggest an uneven pattern, but with little indication of a substantial impact.

Using aggregate variables for sentencing reforms and weapon laws (Tables V-1 and V-2) produced small coefficients that are usually not statistically significant. The weapon laws did have significant negative impacts on murder and robbery rates, a result in accord with probable aims of the lawmakers enacting the weapon laws, but the coefficients are barely significant, and they suggest that the size of the impacts are modest, about five percent for murder and six percent for robbery. These regressions with aggregate variables, it should be emphasized again, are crude summaries because they (incorrectly) assume that slopes are the same for each law.

In the regressions with law types (Tables V-3 and V-4), again there is no indication that sentencing reforms reduce crime, and there is even a moderately strong suggestion that sentencing guidelines have been followed by higher crime rates, especially for murder and robbery. As for the types of weapon laws, there is some suggestion that laws giving greater

sentences for possession of weapons during crimes (as opposed to weapon use only), and laws with required mandatory minimums are associated with reductions in some types of crime, especially murder, robbery, larceny, and perhaps burglary. But again the coefficients are small, indicating changes no higher than eight percent. The regressions for individual laws occasionally produce significant results (Tables V-5 and V-6), with both positive and negative coefficients. In general, however, such results are few and far between and are similar to what one would expect by chance given the unevenness of the data. One exception is that a sizeable number of weapon laws produced large negative coefficients for murder and robbery, especially laws in Kentucky, Michigan, Montana, Nebraska, Ohio, Tennessee, Virginia, and Wyoming. This is consistent with the results in Tables V-1 to V-4.

#### 5.1 Other Variables.

As expected, most crime rates are strongly influenced by age structure. Robbery, burglary, and larceny are sensitive to changes in the 15 to 17 age category, and rape and assault are sensitive to the older age group, 18 to 24. Murder is negatively associated with the 15-17 age group, which suggests that it is positively associated with other, older, age groups not included. The age groups included have no significant impact on auto theft rates, possibly because most auto thefts are conducted by older, professional thieves.

Real personal income has a sizeable positive association with violent crime rates and auto thefts in the next year. This is in accord with theory that crime rates are a product of the opportunities associated with the general amount of social activity. There is no evidence to support the common theory that poor economic conditions stimulate crime because potential criminals are less able to obtain gainful employment to satisfy their needs.

Again, the state and year dummies are highly significant, indicating that crime rates are mainly influenced by factors not entered in the regression here. The huge F Values for the state dummies mean that crime rates differ between states to such a large extent that cross-section analysis is unlikely to produce credible results.

Table V-1

Impact of Sentencing Laws in the Aggregate on Violent Crime Rates

	<u>Dependent Variable<sup>1</sup></u>							
	Murder		Rape		Assault		Robbery	
<u>Sentence Laws (lagged)</u>								
Sentencing Reforms	.03	1.30	-.02	.84	.00	.09	.05	2.20*
Weapon Laws	-.05	2.06*	.00	.07	-.01	.51	-.06	2.79**
<u>Other Variables</u>								
Percent pop. 15-17	-.71	3.41***	.13	.76	.23	1.08	1.24	5.89***
Percent pop. 18-24	.50	1.83	.79	3.50***	.57	2.38*	-.18	.68
Per. inc. (lagged)	.54	3.45***	.44	3.42***	.38	2.62**	.92	5.88***
<u>F Values</u>								
Year dummies	6.11***		9.17***		6.96***		14.92***	
State dummies	139.28***		71.31***		76.89***		157.69***	
Dep. var. mean	1.94		3.28		5.37		4.86	
Deg. of freedom	877		877		877		877	
Adjusted R-square	.93		.98		.99		.99	
Durbin-Watson	2.06		2.08		1.73		1.80	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.



Table V-2

Impact of Sentencing Laws in the Aggregate on Property Crime Rates

	Dependent Variable <sup>1</sup>					
	Burglary		Larceny		Auto Theft	
<u>Sentence Laws (lagged)</u>						
Sentencing Reforms	-.01	.36	-.01	1.08	.02	1.03
Weapon Laws	-.02	1.78	-.02	1.67	-.02	1.11
<u>Other Variables</u>						
Percent pop. 15-17	.60	4.49***	.32	3.05**	.29	1.49
Percent pop. 18-24	.00	.02	.10	.72	.11	.51
Real per. inc. (lagged)	.02	.24	-.04	.59	.60	4.39***
<u>F Values</u>						
Year dummies	43.65***		97.97***		11.73***	
State dummies	51.09***		47.62***		27.44***	
Dependent var. mean	7.09		7.87		5.92	
Degrees of freedom	877		828		877	
Adjusted R-square	.99		.99		.99	
Durbin-Watson	1.73		1.65		1.68	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

Table V-3

Impact of Sentencing Law Types on Violent Crime Rates

	Dependent Variable <sup>1</sup>							
	Murder		Rape		Assault		Robbery	
<u>Sentencing Reforms (lagged)</u>								
Presumptive sent.	.06	1.57	-.07	1.86	.01	.25	.11	2.90**
Determinate sent.	-.03	.84	-.03	1.03	.02	.46	-.03	.57
Sent. guidelines	.13	3.05**	.04	1.15	-.02	.54	.14	3.76***
<u>Weapon Laws (lagged)</u>								
Possession laws	-.08	2.17*	-.01	.49	.00	.08	-.04	1.24
Weapon laws	-.01	.37	-.01	.51	.04	1.15	.00	.13
Required minim.	-.04	1.58	.02	.80	-.02	.80	-.08	2.69**
<u>Other Variables</u>								
Percent pop. 15-17	-.61	2.91**	.15	.82	.26	1.17	1.25	5.82***
Percent pop. 18-24	.43	1.64	.82	3.51***	.57	2.33*	-.24	.91
Per. inc. (lagged)	.55	3.57***	.46	3.49***	.39	2.63**	.93	5.81***
<u>F Values</u>								
Year dummies	5.89***		9.70***		6.63***		14.82***	
State dummies	138.64***		67.71***		65.21***		141.01***	
Dep. var. mean	1.94		3.28		5.37		4.86	
Deg. of freedom	873		873		873		873	
Adjusted R-square	.95		.99		.99		.99	
Durbin-Watson	2.05		2.08		1.74		1.81	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

Table V-4

Impact of Sentencing Law Types on Property Crime Rates

	<u>Dependent Variable<sup>1</sup></u>					
	<u>Burglary</u>		<u>Larceny</u>		<u>Auto Theft</u>	
<u>Sentencing Reforms (lagged)</u>						
Presumptive sent.	-.07	2.78**	-.02	.94	.01	.26
Determinate sent.	.02	.84	-.01	.50	.02	.61
Sent. guidelines	.03	1.47	.01	.46	.05	1.52
<u>Weapon Laws (lagged)</u>						
Possession laws	-.04	1.81	.01	.41	-.02	.55
Weapon laws	.02	1.04	.02	1.29	-.02	.61
Required minim.	-.02	1.18	-.04	2.90**	-.02	.82
<u>Other Variables</u>						
Percent pop. 15-17	.64	4.82***	.34	3.22**	.31	1.58
Percent pop. 18-24	-.09	.57	.17	1.26	.07	.31
Real per. inc. (lagged)	.05	.58	-.04	.56	.61	4.48***
<u>F Values</u>						
Year dummies	43.38***		97.85***		11.67***	
State dummies	62.50***		49.54***		29.08***	
Dependent var. mean	7.09		7.87		5.92	
Degrees of freedom	873		824		873	
Adjusted R-square	.99		.99		.99	
Durbin-Watson	1.72		1.67		1.68	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

Table V-5

Impact of Individual Sentencing Laws on Violent Crime Rates

	Dependent Variable <sup>1</sup>							
	Murder		Rape		Assault		Robbery	
<u>Sentencing Reforms (lagged)</u>								
01 Alabama (1980)	-.14	1.03	-.12	1.33	.11	1.13	-.01	.05
02 Alaska <sup>2</sup> (1980)	.11	.76	.23	1.80	.07	.49	.05	.55
03 Arizona <sup>2</sup> (1978)	.08	1.18	-.16	2.25*	-.05	.60	.00	.06
04 Arkansas (1981)	.02	.18	.01	.12	-.03	.27	.08	.68
05 California (1977)	.18	2.22*	-.06	.67	.01	.20	.14	2.43*
06 Colorado (1979)	.09	.90	-.32	3.04**	-.04	.38	-.27	2.53*
07 Conn. <sup>2</sup> (1981)	.12	.87	.07	1.12	.04	.59	-.03	.22
10 Florida (1983)	.03	.23	-.11	2.14*	-.01	.27	.37	4.85***
14 Illinois (1978)	-.01	.27	.01	.12	.01	.13	-.09	1.01
15 Indiana <sup>2</sup> (1977)	.00	.06	-.10	1.26	.00	.01	-.24	3.01**
20 Maine <sup>2</sup> (1976)	-.08	.96	.09	1.14	-.23	1.28	-.25	1.54
23 Michigan (1984)	.21	2.68**	.13	1.78	-.06	.88	-.03	.37
24 Minnesota (1980)	.17	.58	.09	.46	.22	1.01	.25	1.11
31 New Jersey (1979)	.07	.53	.11	1.33	.12	1.38	.11	1.12
32 New Mexico (1979)	.00	.03	-.05	.40	.17	1.24	.14	.97
34 North Car. (1981)	-.12	1.21	.06	.74	-.13	1.26	.16	1.42
39 Penn. <sup>2</sup> (1982)	-.03	.38	.00	.03	-.06	1.13	-.04	.56
43 Tennessee (1982)	.07	1.00	.10	1.81	.13	1.44	.07	.66
48 Washington (1984)	.21	2.10*	.06	.53	-.15	1.45	.25	4.14***
50 Wisconsin (1985)	.19	2.51*	.08	1.14	.03	.39	.05	.46
<u>Weapon Laws<sup>3</sup> (lagged)</u>								
01 Alabama (1981)	-.12	.79	-.03	.27	-.06	.57	.02	.15
04 Arkansas (1976)	-.17	1.54	-.10	1.27	-.09	.98	.00	.02
06 Colorado (1976)	-.14	1.39	.09	.83	.04	.35	-.01	.13
10 Florida (1975)	-.11	.84	.11	1.87	.10	1.69	.09	1.02

Table V-5 (page 2)

	Murder		Rape		Assault		Robbery	
11 Georgia (1976)	-.20	2.12*	.14	2.78**	-.08	.91	.07	.55
12 Hawaii (1976)	-.12	1.15	.07	1.01	-.06	.54	.16	1.01
13 Idaho (1977)	-.16	1.88	-.12	2.16*	.05	.29	-.05	.23
16 Iowa (1978)	.04	.50	-.12	2.14*	.36	4.94***	.03	.58
17 Kansas (1976)	.00	.03	.07	1.14	.10	.98	.02	.21
18 Kentucky (1976)	-.19	2.17*	-.12	2.08*	-.20	1.47	-.19	3.26***
19 Louisiana (1981)	-.01	-.16	-.02	.29	.06	.71	.25	3.18**
22 Mass. (1974)	-.12	1.77	.15	4.07***	.30	4.46***	-.14	1.77
23 Michigan (1977)	-.09	1.28	-.01	.08	.02	.25	-.23	2.98**
24 Minnesota (1979)	-.23	.76	.01	.07	-.14	.63	-.21	.89
26 Missouri (1979)	-.03	.38	-.24	3.76***	.09	1.78	-.16	2.46*
27 Montana (1978)	-.10	1.14	-.05	.96	-.14	1.02	-.30	3.06**
28 Nebraska (1978)	-.03	.35	-.11	1.96*	-.01	.14	-.18	2.48*
30 New Hamp. (1977)	-.13	1.30	.40	4.81***	.11	1.56	-.02	.13
31 New Jersey (1981)	-.21	1.73	-.11	1.29	-.08	.93	-.05	.58
32 New Mexico (1977)	.04	.25	-.09	.65	-.04	.26	-.16	1.10
33 New York (1980)	.07	.93	-.21	3.28**	.00	.03	.00	.08
35 North Dak. (1977)	.29	3.91***	.11	1.64	-.29	3.02**	-.20	1.79
36 Ohio (1983)	-.15	2.48*	.07	1.05	-.06	.88	-.16	3.09**
38 Oregon (1979)	.21	2.87**	-.01	.13	-.18	1.71	.35	4.14***
41 South Car. (1986)	.06	.32	.09	1.28	-.02	.19	.14	1.35
42 South Dak. (1985)	.32	2.91**	.27	2.40*	-.33	1.90	-.30	2.74**
43 Tennessee (1976)	-.23	3.35***	.05	.89	-.25	2.55*	-.03	.27
44 Texas (1977)	.15	1.39	.13	2.86**	.12	1.40	.18	2.64**
45 Utah (1976)	.19	2.45*	-.15	1.63	.08	.57	-.02	.10
47 Virginia (1975)	-.19	2.52*	-.25	3.55***	-.43	5.27***	-.22	3.35***
49 West Va. (1979)	-.09	1.18	.19	2.71**	.00	.28	-.03	.33
50 Wisconsin (1980)	.08	1.33	.08	1.29	.30	4.25***	.15	1.57
51 Wyoming (1982)	-.35	3.43***	-.07	.81	-.08	.66	-.49	4.50***

Table V-5 (page 3)

	Murder		Rape		Assault		Robbery	
<u>Other Variables</u>								
Percent pop. 15-17	-.01	.06	-.02	.13	.30	1.33	1.08	4.63***
Percent pop. 18-24	.45	1.46	.57	2.39*	.28	.98	-.27	.89
Per. inc. (lagged)	.65	3.93***	.39	3.12**	.25	1.66	.90	5.88***
<u>F Values</u>								
Sentence reform	1.48		1.97**		.85		3.69***	
Weapon laws	3.40***		4.74***		4.11***		4.25***	
Year dummies	3.97***		10.58***		8.43***		14.61***	
State dummies	113.54***		63.07***		73.97***		118.57***	
Dependent var. mean	1.94		3.27		5.37		4.86	
Degrees of freedom	826		826		826		826	
Adjusted R-square	.95		.99		.99		.99	
Durbin-Watson	2.03		1.96		1.65		1.80	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

2) The sentencing reform law went into effect on the same date, or nearly the same date, as a weapon law.

3) The states not listed are 1) those with weapon laws with effective dates corresponding to sentence reform laws (see note 2), 2) those without specific deadly weapon laws (Illinois, Mississippi, and North Carolina), and 3) those with laws effective before 1975 (California, Delaware, Maryland, Massachusetts, Nevada, Oklahoma, Rhode Island, Vermont, and Washington).

Table V-6

Impact of Individual Sentencing Laws on Property Crime Rates

	Dependent Variable <sup>1</sup>					
	Burglary		Larceny		Auto Theft	
<u>Sentencing Reforms (lagged)</u>						
01 Alabama (1980)	.02	.30	.08	1.84	-.02	.26
02 Alaska <sup>2</sup> (1980)	-.05	.64	-.06	.98	-.01	.12
03 Arizona <sup>2</sup> (1978)	-.09	1.91	-.05	1.09	-.09	1.65
04 Arkansas (1981)	.16	3.20**	.11	2.58*	.12	1.45
05 California (1977)	-.08	1.54	-.05	1.21	-.02	.21
06 Colorado (1979)	-.02	.27	.03	.40	-.14	1.42
07 Conn. <sup>2</sup> (1981)	-.04	.75	-.05	1.84	-.17	1.97*
10 Florida (1983)	.25	7.02***	.11	4.99***	.37	6.11***
14 Illinois (1978)	.01	.28	-.11	3.72***	.03	.51
15 Indiana <sup>2</sup> (1977)	-.14	3.70***	-.14	3.88***	-.07	1.26
20 Maine <sup>2</sup> (1976)	-.23	2.47*	.01	.20	.07	.43
23 Michigan (1984)	-.12	2.29*	-.07	2.00*	-.09	1.27
24 Minnesota (1980)	.11	.85	-.06	.60	.13	.73
31 New Jersey (1979)	-.09	1.59	-.04	.85	.12	1.51
32 New Mexico (1979)	.22	2.49*	.14	2.19*	.04	.31
34 North Car. (1981)	.08	1.20	.07	1.27	.05	.76
39 Penn. <sup>2</sup> (1982)	-.11	3.48***	-.03	1.02	.03	.67
43 Tennessee (1982)	.07	1.46	.02	.61	.36	6.04***
48 Washington (1984)	.14	2.99**	.06	1.35	.12	1.16
50 Wisconsin (1985)	-.04	1.06	-.06	1.37	.08	1.09
<u>Weapon Laws<sup>3</sup> (lagged)</u>						
01 Alabama (1981)	-.01	.22	.04	.75	-.04	.39
04 Arkansas (1976)	.00	.09	-.02	.55	.19	2.33*
06 Colorado (1976)	-.05	.74	.07	1.00	-.07	.64
10 Florida (1975)	-.07	1.61	-.14	5.35***	-.02	.25

Table V-6 (page 2)

	Burglary		Larceny		Auto Theft	
11 Georgia (1976)	-.05	.59	.16	3.39***	.02	.15
12 Hawaii (1976)	-.07	.90	.06	1.13	-.08	.57
13 Idaho (1977)	.02	.22	-.12	1.80	-.15	1.07
16 Iowa (1978)	.20	4.68***	-.04	1.39	.00	.01
17 Kansas (1976)	.04	.63	-.05	1.37	.00	.05
18 Kentucky (1976)	.03	.57	-.02	.55	-.09	.89
19 Louisiana (1981)	.19	3.23**	.18	4.91***	.08	.88
22 Mass. (1974)	-.01	.22	-.01	.13	-.12	1.44
23 Michigan (1977)	-.07	1.51	-.09	2.69**	-.05	.82
24 Minnesota (1979)	-.09	.72	.06	.59	-.33	1.79
26 Missouri (1979)	-.02	.45	.00	.07	.00	.04
27 Montana (1978)	.05	.75	.05	1.13	-.13	1.26
28 Nebraska (1978)	.14	4.06***	.05	2.16*	-.30	3.54***
30 New Hamp. (1977)	-.02	.19	.02	.23	-.11	.78
31 New Jersey (1981)	-.17	3.25**	-.03	.76	.04	.57
32 New Mexico (1977)	-.07	.72	-.09	1.34	.03	.21
33 New York (1980)	-.18	4.90***	.03	1.24	.13	2.24*
35 North Dak. (1977)	.10	2.16*	.08	1.26	-.05	.32
36 Ohio (1983)	-.08	2.79**	-.07	2.21**	-.04	.70
38 Oregon (1979)	.11	1.64	.01	.28	-.01	.12
41 South Car. (1986)	.04	.61	.05	.84	.03	.43
42 South Dak. (1985)	-.01	.15	-.02	.26	-.33	2.66**
43 Tennessee (1976)	-.05	1.00	-.08	1.86	-.03	.50
44 Texas (1977)	.05	.81	-.12	2.51*	.20	2.46*
45 Utah (1976)	.07	.70	.05	.71	.05	.33
47 Virginia (1975)	-.09	1.93	-.08	2.59**	-.12	1.30
49 West Va. (1979)	.11	1.51	-.01	.15	.00	.04
50 Wisconsin (1980)	.09	2.82**	.00	.06	-.02	.28
51 Wyoming (1982)	-.06	.86	-.12	2.17*	-.54	3.82***



Table V-6 (page 3)

	Burglary		Larceny		Auto Theft	
<u>Other Variables</u>						
Percent pop. 15-17	.89	6.26***	.47	4.34***	.16	.78
Percent pop. 18-24	-.16	.91	-.05	.33	-.33	1.32
Real per. inc. (lagged)	.08	.84	.01	.14	.59	4.18***
<u>F Values</u>						
Sentence reform	6.66***		4.54***		4.71***	
Weapon laws	3.98***		3.88***		1.95**	
Year dummies	40.12***		101.17***		11.06***	
State dummies	63.93***		51.14***		22.97***	
Dependent var. mean	7.09		7.87		5.92	
Degrees of freedom	826		777		826	
Adjusted R-square	.99		.99		.99	
Durbin-Watson	1.64		1.61		1.65	

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

1) The dependent variables and "other" variables are per capita variables and are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

2) The sentencing reform law went into effect on the same date, or nearly the same date, as a weapon law.

3) The states not listed are 1) those with weapon laws with effective dates corresponding to sentence reform laws (see note 2), 2) those without specific weapon laws (Illinois, Mississippi, and North Carolina), and 3) those with laws effective before 1975 (California, Delaware, Maryland, Massachusetts, Nevada, Oklahoma, Rhode Island, Vermont, and Washington).

## Chapter 6. SPEEDY TRIAL LAW IMPACTS

Speedy trial laws, to the extent they enhance timely administration of justice, may help deter some criminals. Also, to the extent that they hasten sentencing, they may cause prison populations to grow. But intuitively these relationships with prison population and crime rates seem far more tenuous than the putative relationship between sentencing laws and prison population or crime rates.

As seen in Tables VI-1 through VI-3, there is some slight evidence that the speedy trial laws increase prison populations. Using the aggregate speedy trial law variable - with all laws effective after 1973 subsumed under one variable, there is a statistically significant relationship with prison population (Table VI-1), but it is quite small, some 7 percent of the dependent variable mean. The impact on prison admissions is far from significant (Table VI-1). This is not inconsistent with the finding that there was an impact on prison population because it is likely to be a one-shot occurrence, leading to more admissions in a short spurt as courts come into compliance with the laws, whereas the impact on prison population should linger over several years.

Table VI-2 divides the speedy trial laws into those with short and long time limits. The latter, with limits over six months, are Arkansas, Connecticut, Massachusetts, Mississippi,

and Ohio. If anything, these laws have a greater impact than those with time limits of six months or less. The individual laws generally have no significant impacts on prisons (Table VI-3), and the significant coefficients are roughly equally divided between those with positive and negative signs. This pattern is similar to that found when studying sentencing reforms (see Tables IV-3, V-5, and V-6).

To study the impact on crime rates, we use the major crime variable - UCR violent crime plus burglary - as the dependent variable. There is virtually no evidence of an impact.

Table VI-1

Impact of Speedy Trials Laws in the Aggregate

	<u>Dependent Variables<sup>1</sup></u>					
	<u>Prison Population</u>		<u>Prison Admissions</u>		<u>Major Crime<sup>2</sup></u>	
<u>Speedy Trial Laws</u>	.07	2.89**	.04	1.08	.00	.28
<u>Other Variables</u>						
Major crime <sup>2</sup> (lagged)	-.02	.53	.24	3.62***	--	--
Percent pop. 15-17	--	--	--	--	.58	4.69***
Percent pop. 18-24	.08	.32	-1.71	5.06***	.12	.81
Percent pop. 25-34	-.02	.08	-.94	2.88**	--	--
Real per. inc. (lagged)	-.01	.04	.28	1.63	.15	1.70
 F Values						
Year dummies	26.63***		8.89***		36.92***	
State dummies	27.31***		41.19***		82.87***	
Dependent var. mean	4.82		4.25		7.39	
Degrees of freedom	807		656		878	
Adjusted R-square	.99		.99		.99	
Durbin-Watson stat.	1.67		1.98		1.70	

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

1) The dependent variables and "other" variables are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

2) Major crime is the sum of reported murder, rape, assault, robbery, and burglary.

Table VI-2

Impact of Speedy Trials Law Types

	Dependent Variables <sup>1</sup>					
	Prison Population		Prison Admissions		Major Crime <sup>2</sup>	
<u>Speedy Trial Laws (time limits)</u>						
Six months or less	.06	1.79	.05	1.18	-.02	.92
Over six months	.08	2.56*	.02	.46	.02	.73
<u>Other Variables</u>						
Major Crime <sup>2</sup> (lagged)	-.03	.60	.24	3.59***	--	--
Percent pop. 15-17	--	--	--	--	.58	4.71***
Percent pop. 18-24	.04	.17	-1.69	4.90***	.11	.74
Percent pop. 25-34	.06	.27	-.93	2.87**	--	--
Real per. inc. (lagged)	.01	.09	.30	1.76	.14	1.62
<u>F Values</u>						
Year dummies	25.09***		8.80***		36.92***	
State dummies	44.42***		40.89***		83.09***	
Dependent var. mean	4.82		4.25		7.39	
Degrees of freedom	806		655		877	
Adjusted R-square	.99		.99		.99	
Durbin-Watson stat.	1.66		1.98		1.70	

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

1) The dependent variables and "other" variables are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

2) Major crime is the sum of reported murder, rape, assault, robbery, and burglary.

Table VI-3

Impact of Individual Speedy Trial Laws

	Dependent Variables <sup>1</sup>					
	Prison Population		Prison Admissions		Major Crime <sup>2</sup>	
<u>Speedy Trial Laws</u>						
03 Arizona (1973)	--	--	--	--	-.05	.58
04 Arkansas (1980)	.04	.57	-.21	2.47*	.11	1.69
07 Conn. (1983)	.10	1.45	.03	.31	.01	.17
12 Hawaii (1977)	.24	2.42*	.14	1.59	-.05	.74
13 Idaho (1980)	-.03	.30	-.18	1.39	-.09	1.03
19 Louisiana (1981)	.07	.98	.12	2.01*	.18	3.47***
21 Maryland (1979)	-.13	1.44	-.42	3.97***	.01	.13
22 Mass. (1979)	.14	2.78**	.11	1.48	-.12	3.21**
25 Mississippi (1976)	--	--	--	--	.20	3.88***
26 Missouri (1978)	.02	.33	.06	.61	-.02	.38
34 North Car. (1978)	-.17	2.14*	-.20	1.69	-.03	.44
36 Ohio (1974)	.10	1.91	-.03	.29	.03	.77
39 Penn. (1973)	--	--	--	--	-.08	1.88
42 South Dak. (1985)	.06	.50	-.08	.41	-.08	.87
44 Texas (1978)	.10	1.20	.08	1.21	.09	1.76
<u>Other Variables</u>						
Major Crime <sup>2</sup> (lagged)	-.05	1.09	.25	3.60***	--	--
Percent pop. 15-17	--	--	--	--	.64	5.40***
Percent pop. 18-24	.06	.22	-1.58	4.47***	.02	.17
Percent pop. 25-34	-.07	.28	-1.11	3.23***	--	--
Real per. inc. (lagged)	-.05	.40	.24	1.41	.11	1.29

Table VI-3 (cont.)

F Values

Speedy trial laws	2.30**	3.20***	3.58***
Year dummies	24.29***	9.14***	37.86***
State dummies	39.44***	37.70***	86.57***
Dependent var. mean	4.82	4.25	7.39
Degrees of freedom	796	645	864
Adjusted R-square	.99	.99	.99
Durbin-Watson stat.	1.64	1.94	1.64

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

1) The dependent variables and "other" variables are in natural logs. The two columns below each dependent variable are the coefficients and absolute value of the T Ratio.

2) Major crime is the sum of reported murder, rape, assault, robbery, and burglary.

## Chapter 7. CONCLUSIONS

The net result of this considerable analysis is a general absence of findings that the two types of criminal court reform, sentencing laws and speedy trial rules, have important long range impacts on the major features of the criminal justice system. Determinate sentencing, presumptive sentencing, sentencing guidelines, mandatory minimum sentences for the use of weapons, all evidence at most modest impacts on crime rates and prison populations. On the other hand, these laws have not added appreciably to the prison burden, and it is possible to interpret the slight impacts observed - e.g., increases in prison population at the same time that some law times went into effect - as simply a legislative reaffirmance of a trend already under way. The analysis of speedy trial laws also uncovered little impact. All these laws, however, have numerous immediate and intermediate goals that were not addressed in this research, such as reducing sentence disparity and trial court delay. Our results are encouraging in one regard: whenever benefits were obtained in these goals, they were not gained at the expense of problems further down the line, in the form of more prisoners or more crime.

The research here only studied the impacts of the laws on broad classifications of crime, and there may be impacts on subcategories. In particular, we did not explore whether the



weapon laws reduced the use of weapons during felonies. But we can conclude that any change in weapon use had little or no impact on the volume of crimes typically committed with weapons, especially murder and manslaughter, aggravated assault, and robbery.

Furthermore, the research design only addressed the long range effects of reforms. There may have been impacts for a year or two after the law went into effect, dissipating thereafter. Such fleeting impacts may not show up in our analysis.

The regression analyses with individual laws entered did produce several significant results, and it appears that the laws in some states did affect crime rates and prison populations. The next logical step, one might argue, is to determine why the laws have such impacts in some places but not others. But I believe that this is not a feasible research topic; it is what Lieberman (1985) calls "undoable" research. It would require cross-section analysis, which is generally incapable of determining short-term causation (Lieberman 1985:179-183). There is a sizeable grey area in which individual laws may or may not have impacts. More important, the fifty states provide a meager sample size; the number of observations is nearly matched by the important independent variables. These include the particular sentences that statutes specify for various crimes, the amount of discretion accorded judges, variations in procedure such as use of presentence

reports, other criminal justice changes made in the state at the time of sentencing reform, the varying capacity of the prison systems and their willingness to use local jails, the standard operating procedures in the courts, and so on. Finally, political, social, and cultural aspects of the states need to be considered as background factors.

Faced with this limitation of social science research, we can only conclude that in the aggregate the laws have little or no impact on crime rates and prisons, although there is an outside chance that, for whatever reason, the laws in a particular jurisdiction may have an impact.

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