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IMPACTS OF ENHANCED PENALTIES FOR CRIMES WITH DEADLY WEAPONS

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Thomas B. Marvell
Carlisle E. Moody, Jr.

Justec Research
306 South Henry St.
Williamsburg, VA 23185

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ABSTRACT

A frequent legislative response to the gun problem is to mandate greater prison terms for crimes committed with deadly weapons. The issue here is whether these laws have any ultimate impacts on crime rates and prison populations. Research concerning initial impacts, such as use of guns in crimes, is hindered by conceptual and measurement problems. We use a pooled time series-cross section design with data for each state during the 1970s and 1980s. The results showed great differences between states, but with a slight tendence for the weapons laws to lead to longer prison stays and to fewer murders and property crimes.

INTRODUCTION

In all but three states laws, usually enacted between 1975 and 1980, mandate extra prison terms for felonies committed with deadly weapons. Deadly weapons are generally defined as firearms, although knives and other weapons are included in many states.

The question addressed here is whether the deadly weapon laws have furthered the goals intended by the state governments. Determining the precise goals is difficult because legislative records are incomplete (Beha 1977:107; Sheppard 1986:633-637), and because various actors have different goals (see Casper and Brereton 1984). In general, the deadly weapon laws are attempts to control the use of guns in a manner that will not be contested by the "gun lobby" (Loflin 1984:250; Beha 1977:107-108). Commentary suggests that lawmakers believed that the courts have been too lenient when sentencing for violent crimes, that greater punishment for use of weapons is just retribution, and that longer and more certain sentences for crimes with deadly weapons will reduce such crimes by deterring potential offenders and by incapacitating past offenders (e.g., Loflin 1984:251-52).

The crime-reduction goal is the focus of the present study. The theoretical considerations behind how the weapon laws might advance this goal are complex, and it is not feasible to model the complete processes. Consequently, this research addresses only the ultimate impacts of the weapon laws.

Deterrence. Theoretical considerations underlying research on the effectiveness of deterrence measures seldom advance beyond the basic hypothesis that offenders are less likely to use deadly weapons when committing crimes because they fear the stiffer sentence. But the actual impact of imprisonment as deterrence is uncertain, and research has reached differing conclusions (e.g., the summary in Blumstein, et al. 1978). In addition, the actual processes through which deterrence might operate are uncertain. By and large, research suggests that the certainty of a

punishment has greater deterrence impact than the severity of punishment.

The impact of sanctions is partly a function of potential offenders' beliefs concerning the existence of the sanctions and the likelihood of their application. Lack of knowledge about such initial stages of the deterrence process have long been considered a weak point in deterrence theory, and one cannot make assumptions concerning this point.

A deterrent effect is possible only if potential offenders have some information about the weapon laws and their impacts. Hence, the laws are often accompanied by publicity efforts (Loflin and McDowall 1984:251; Bowers; Loflin); but little is known about whether the efforts are successful. In addition information about a new law is likely to come through word of mouth concerning sanctions imposed under the new law, probably from the actual sentences given to acquaintances of potential offenders. Again, little is known about this process.

The impact of the weapons laws, in the first instance, depend partly on whether, the laws are enforced. This issue is discussed more fully below with reference to incapacitation. However, it is not correct to assume, as most researchers have (e.g., Loftin), that the deterrent effects of laws depend on whether greater sanctions are actually applied. This assumption is at best incomplete because the impact is determined by offenders' beliefs concerning how the law operates, based on very imperfect knowledge. This may explain the conclusion by Loftin and his colleagues that here is evidence of a deterrent impact for murder, but not assault and robbery, even though they found little evidence that the laws resulted in longer sentences were given for murder.

Deterrent effects of the weapons laws, if any, can take any number of forms, depending in part on the information possessed by offenders. At the narrowest, deterrence could be limited to the specific focus of a law - that is, fewer crimes committed with the particular weapons specified in the law. But the impact, which depends on the offenders' interpretation of whatever information they receive, are likely to be broader. Offenders may believe that the law applies to a broader classification of weapons or activities then specified. They may interpret the legislative activity and resulting longer sentences as a general policy towards stiffer sanctions and greater law enforcement efforts. This may apply equally to all types of crimes or, very likely, with greater force to major crimes, especially violent crimes.

Likewise, little is know concerning which crimes might be most deterred by weapon laws. At the most obvious level, deterrence is most likely for to crimes committed with weapons, typically murder, rape, assault, and robbery. The

2

deterrence impact, however, may apply to crimes that seldom require the overt use of weapons, such as theft and burglary, because the offenders may carry weapons for protection. The extent that a particular state's weapon laws deters such crimes may depend on whether the law applies to possession of (rather than only the use of) weapons and whether the law applies to all deadly weapons or just firearms. Also, again, the impact depends on the potential offenders' beliefs about the legal provisions and actual operation of the laws.

The deterrent effect of the weapons laws probably applies more to crimes involving premeditation. One would except the greatest impact for property crimes (including robbery). For assaults, it is unlikely that offenders consider the impact of weapons laws in the heat of argument, although they might consider the laws when determining whether to carry a weapon in the first place. Murders and rape fall between assaults and property crimes.

Finally, the deterrent effect is probably greater for lesser crimes, because the marginal impact of the enhanced penalties is greater than for major crimes. Offenders, for example, would view the difference between probation and a year's imprisonment as greater than that between one and two years' imprisonment.

Displacement. To the extent that there is a deterrence effect in the sense that offenders fear harsher sentences for weapon use, they may refrain from committing certain crimes and instead commit others with less chance of falling under the weapon laws. The substitute crimes may be either the weapon crimes with less chance of apprehension (e.g., armed robbery of older persons) or crimes without weapons. Because the substitute crimes are generally less lucrative, offenders must commit more to achieve gains commensurate with those from the substituted crimes. Thus, perhaps surprisingly, the weapon laws can lead to more crimes, including more crimes involving weapon use. This may counteract any deterrent effect the laws might have, leading to the possible hypothesis that the weapon laws increase the amount of crime, with or without weapons. The most likely impact, however, is probably more lesser crimes conducted without weapons.

Prison Population and Incapacitation.

A second purpose of the law is to reduce crime through incapacitation. Stiffer prison sentences for those using weapons in past crimes presumably means fewer crimes simply because more offenders are in prison. This effect, however, is not at all clear. It assumes that there actually is a noticeable incarceration impact due to imprisonment, for which the evidence is not totally clear. Second, it assumes that the enhanced penalties are actually imposed. The evidence is that the criminal justice system partly negates

the effect of the law. Beha (1978:120-145) found that
Massachusetts police sometimes circumvented the state's gun
law by failing to charge gun possession, and the judges, who
disliked the law, were more prone to acquit in gun cases,
but only if there was a valid reason for acquittal (overall,
Beha concluded that the criminal justice system did not
substantially negate the effects of the law). Loftin and
McDowall (1981) concluded that judges circumvented the
Detroit gun law in murder and robbery cases by reducing the
initial prison term to compensate for the mandatory two-year
addition for gun use (for assault, where the usual prison
term is less than two years, the new law did increase terms,
but not by the two years required by the law).

Measurement Problems.

The weapons laws may affect crime statistics, although
not actual crimes, through an impact on crime reporting.
Citizens may be more likely to report crimes, especially
crimes with weapons (Pierce and Bowers 1981:130), because
the publicity given the laws heightens sensitivity or
because the citizens believe that the offender is more
likely to be adequately punished because of the new law.

The weapon laws, and their publicity, may prompt the
police to be more likely to classify a crime as involving a
weapon (Loftin 1981). On other hand, police may try to
mitigate the impact of law in marginal cases by not
reporting weapon use. Also, offenders may respond to the
weapons laws by better hiding or disposing of weapons before
apprehension.

Impact on Prisons.

The second major impact of the gun laws is the impact
on prison populations and admissions, with the resulting
increased cost to the taxpayer and loss of freedom by
prisoners. On the other hand, the weapons laws can cause
prison admissions and population to drop if the laws have a
substantial deterrent effect.

Summary

The gist of the forgoing discussion is that current
criminology theory and research findings do not permit one
to hypotheses whether the deadly weapon laws affect prison
variables or crime rates, or even to hypothesize what
direction an impact might be in. Furthermore, one cannot
specify the mechanism through which any impact might
operate. The only way to deal with this situation is to
focus on the ultimate impacts, without preconceived
assumptions about the expected results, and to adopt the
best procedures feasible to control for missing variables.

RESEARCH DESIGN

The research uses the pooled time series-cross section (TSCS) design, combining data from the fifty states over a number of years. The TSCS design has long been considered one of the best designs for social science evaluation, second only to the random experiment (Campbell and Stanley 1967:55-57; Blumstein, Cohen, and Nagin 1978:11,17; Berk et al. 1979). Lempert (1966:130-131) called it "the research design par excellence" for evaluating the impact legal changes, especially suitable in this country where the states can be used as separate units for analysis. In general, the TSCS design has become particularly attractive in recent years because many criminology data series, such as prison and crime statistics, either began in the early 1970s or are consistent since that time, and there are now enough years to use in TSCS analysis but not in a single time series analysis.

The TSCS design has many benefits over a time series or cross section alone: 1) it provides more degrees of freedom, 2) it facilitates the evaluation of a large number of separate legal changes, 3) it permits the use of control groups in the sense that the other 49 states act as control groups for each individual state (through the use of state dummy variables), 4) it provides controls for missing variables that cause year-to-year changes in the states (through the use of year dummy variables).

Statistical Procedure. Ordinary least squares is not appropriate for these data because it assumes that observations are independent. The standard procedure for addressing this problem, and the procedure used here, is the fixed effect model (Mundlak 1978; Pindyck and Rubinfeld 1991:224-226; Hsiao 1986:41-58,72). This is an analysis of covariance, with dummy variables for each state and each year (except the first). The coefficient associated with a state dummy estimates the influence of specific factors ("fixed effects") unique to the state, and a year dummy coefficient similarly estimates factors unique to individual years. Omission of the state and year effects, unless they are not significant as a group, cause the estimates of the other variables to be biased (Rubinfeld 1991:226). The fixed effects, of course, reduce the degrees of freedom by nearly the number of states and years included (and an additional degree of freedom for each state is lost in the correction for autocorrelation), but the analyses still have some 600 to 900 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. Like regression analysis generally, it assumes that the coefficients are the same for each observation, but unlike most regressions it relaxes the assumption that the intercept is the same for each observation, allowing it to vary between states and years.

5

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

$$Y_{it} = a + bX_{it} + cV_{it} + g_2W_{2t} + g_3W_{3t} + \ldots + g_NW_{Nt}$$
$$+ d_2Z_{i2} + d_3Z_{i3} + \ldots + d_TZ_{iT} + e_{it}$$

where X_{it} and V_{it} represent the weapon law variables and control variables, respectively. For the year and state dummies,
$W_{it} = 1$ for the i'th state $i = 2, \ldots, N$; otherwise $W_{it} = 0$; and $Z_{it} = 1$ for the t'th year, $t = 2, \ldots, T$; otherwise $Z_{it} = 0$.

A second TSCS model, less used in recent years, is the random effects model (e.g., Hsiao 1986:32-33,72; Loftin).[1] It is useful when the sample size is small because it does not suffer the loss of degrees of freedom due to state and year dummies, not a problem here. In this application it is far inferior to the fixed effect model (see generally Mundlak 1978; Helms 1985). The random effects model assumes that state or year effects are not correlated with any exogenous variables, highly unlikely here because the state and year effect are highly significant (see Tables 3-6). It requires the same number of time units for each state, which would require that we leave some states out of some analyses even though data are missing for only a few years (see Table 2). Correction for autocorrelation is difficult, if not impossible, in the random effects model, and the Durbin-Watson statistic indicates that corrections are necessary (see Tables 3-6).

Other Statistical Issues. The TSCS analysis here encounters autocorrelation problems (see the initial Durbin-Watson statistics given in Tables 3 to 6). We use the standard correction, applying separate first-order serial correlation coefficients for each state (Pindyck and Rubinfeld 1991:228-229).

The continuous variables are expressed as natural logs of per capita data. We use logs because we wish to mitigate the impact of outliers because the coefficients on the dummy variables representing the laws are conveniently the fraction change in the dependent variable (logged)

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1. 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 clearly incorrect assumption that there are no state effects.

attributed to the weapon law. We use per capita variables so that the dummy variable coefficients are not dominated by large states (if unstandardized continuous variables were used, the variance of such variables would be much greater in large states).

Heteroscedasticity is a problem because, as a practical matter, per capita variables have greater variation in small states. Experiments with different weights lead us to conclude that weighing the regressions by the square root of population best addressed this source of heteroscedasticity.

In the analysis presented here encountered no collinearity problems, although such problems did prevent us from entering more than one lag for individual state weapon law variables and from entering control variables for other laws that occurred near the same time (these problems are discussed below).

Level of Analysis. The basic rule is that the level of analysis should conform with the policy and theoretical issues underlying the research (e.g., Lieberson 1986:107-115). This research uses state-level data primarily because we are evaluating state laws; so the state-wide impact, rather than the impact on specific individuals or communities, is of greatest interest to policy makers.

VARIABLES
Felonies committed with deadly weapons.

At present, 47 states have laws that increase penalties for crimes committed with deadly weapons; nearly all of which were enacted in the past two decades. The laws and their effective dates were obtained through statutory research (the author is a lawyer), and as much as possible was checked against the relevant secondary sources, especially law review literature.

The laws differ considerably in their details. Approximately half apply to any deadly weapons, and half to firearms only (Table 1). 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 1, 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 1 does not indicate the harsher penalties often given for defendants convicted of repeat crimes involving weapons).

7

We also do not include more general weapon laws, such as those prohibiting possession of unregistered firearms or concealed weapons, that do not specifically give additional penalties for felonies committed with the weapons.

The weapons laws mandate mandatory minimum sentences, 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, however, 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 accompanied by other changes, such as enhanced 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 comprehensive sentence reforms in Alaska, Arizona, Connecticut, Indiana, Maine, and Pennsylvania.

We have categorized the laws according to three dimensions: 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 1 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 1, but excluding the states where judges have discretion not to apply the mandatory minimum (indicated by the superscript "6").

Prison variables.

The study uses three basic prison variables: Prison admissions, prison population, and prison departures. The latter is used to create the "prison term index," the number of departures divided by the prison population. All variables are for prisoners sentenced to more than one year, although 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 more than 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.

Prison population. Prison population is the number of
prisoners in custody sentenced to more than a year in state
institutions, 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.

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

9

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.

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.

Prison population, as used here, includes inmates in local jails due to prison 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. Prison population is scored as missing data before 1976 for Alabama, Louisiana, Mississippi, and Virginia, as well Vermont for 1971.

Prison admissions. In order to obtain a consistent measure of prison admissions from 1971, we use the number of court commitments, parole or conditional release violators returned, and escapees returned.[2] Court commitments comprise approximately 70 percent of this variable. (The number of court commitments alone is available only from 1974, and using that variable produces results consistent with the broad measure used here.

In some states, part or all of the data for admissions 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 do jails are not counted. These states and years are scored as missing data.

Prison departures. Prison departures are the total prison departures ("total releases" in later BJS reports), available since 1974. This variable has two widespread problems. For some years and some states it includes departures for authorized temporary absences (such as court appearances). Thus for 1974 data for Connecticut, Indiana,

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2. The data for total admissions also include transfers from other jurisdictions, return from appeal bonds, and other admissions. A major problem with this measure is that it also includes returns from authorized temporary absences for some years for some states.

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 when 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 2 lists the states and years where the data are not available for prison departures.

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.

Crime rates.

Crime is measured by UCR index crimes for 1971-1989 (Federal Bureau of Investigation 1991, and similar reports for earlier years, using the report for the subsequent years). Reported crime is the only consistent state-level measure of crime; arrest rates, for example, very greatly from year to year because the many jurisdictions report arrest data sporadically. Larceny includes thefts of less then $50. The first year is 1971 because total larceny data are not available for earlier years; the larceny data for 1971 are unpublished statistics obtained from the FBI.

We use the total crime index and the seven components. Reported crime statistics are often criticized, especially for under-reporting, but we are not aware of any reason why such errors would bias the results here (see Gove, Hughes, and Geerken 1985). Also, year dummies mitigate the effects of nation-wide trends towards more or less under-reporting. Illinois was deleted from the analysis because of severe reporting problems in the 1970s and early 1980s (Federal Bureau of Investigation 1985:4).

RESULTS

The TSCS regressions results are given in Tables 3 and 4 for the impacts on prison variables and Tables 5 and 6 for

the impacts on crime rates. The results are far from clear-cut. The regressions were conducted, first, with all laws aggregated into a single variable and, second, with each law a separate variable. The aggregate variable provides a crude approximation of the nation-wide impact of the laws, but its use assumes that each weapon law variable has the same coefficient, which is far from the case. The use of separate variables for individual laws, on the other hand, runs into the problem that one would expect some coefficients to be significant as a matter of chance. For example, even of none of the laws have an impact, two would be expected to be significant at the .05 level. Also, as will be stressed later, the results for individual laws are likely to be affected by other, contemporaneous changes in the criminal justice system.

The aggregated weapon law variable shows a small positive impact on prison population, significant only to the .10 level. Because the dependent variable is logged, the coefficient represents the portion change resulting from the law. The coefficient of .03 is very small, suggesting that the laws have increased prison population nationwide by at most a few percentage points. The impacts on admissions and court commitments are negligible. The weapon laws do have a significant impact on the release index, but only barely so. That is, the laws tend to increase prison population relative to the number of releases, suggesting that term length has increased. The coefficient of .06 translates to an increase of some 6 percent, with a 95 percent confidence interval of zero to 12 percent.

When the weapon laws are represented by separate variables, the results vary widely, even erratically, with an overall impression that the laws have little or no impact. Of the 36 laws entered into the prison population regression (those effective after 1975), 20 have positive coefficients and 16 have negative coefficients. Twelve are significant to the .05 level, six negative coefficients and six positive (Table 7).

Of the 35 laws entered into the analysis of prison admissions, 14 have positive coefficients (four significant) and 21 have negative coefficients (five significant). The results for court commitments are consistent with those for the broader admissions variable, although only the 25 laws after 1976 are included. The release index, on the other hand, shows a preponderance of laws with positive coefficients, fifteen to ten; and there are four positive significant coefficients, as opposed to one negative.

When one looks at individual states the picture similarly lacks clear focus. Weapons laws in Alabama and Connecticut seem to have produced more prisoners through the imposition of longer terms; Alaska through more commitments; and Indiana, and possibly Pennsylvania, through both mechanisms. In Iowa, Louisiana, and New York increased

12

prison admissions did not lead to significant increases in prison population, probably due to (nonsignificant) declines in the release ratio. Where prison populations decline significantly, the major mechanism appears to be fewer persons sent to prison (see Kentucky, Maine, Minnesota, Nebraska, and Oregon).

Impact on Crime Rates. The TSCS regression results for impacts on crime rates suggest that the weapon laws have had a slight deterrent impact. When weapons laws are aggregated the coefficients in six of the seven regressions are negative, but none are more than marginally significant - robbery barely to the .05 level, and murder, burglary, and larceny to the .10 level. For these four crimes, the coefficients vary from -.02 to -.05, suggesting very modest impacts (Table 5).

When the different laws are entered as separate variables, the results again are not uniform among the states, but there are some suggestive patterns (Table 6). There is a clear predominance of negative coefficients for murder, robbery, burglary, and auto theft; and among significant coefficients, the negative greatly outnumber the positive for these four crimes plus larceny (Table 7). In sum, there is a likely small deterrence impact among murder and the property crimes, including robbery.

Some seventeen of the thirty-nine weapons laws seem to be associated with substantial reductions in crime rates. In eleven states - Arizona, Florida, Idaho, Indiana, Kentucky, Michigan, Missouri, Ohio, Tennessee, Virginia, and Wyoming - there is a clear association: coefficients in at least five of the seven regressions in Table 6 are negative, at least two are significant, and no positive coefficient is significant. In six more states - Alabama, Connecticut, Maine, New York, Pennsylvania, and New Jersey - there is lesser evidence of negative association - coefficients are negative in at least four regressions, significant in at least one, and with no positive significant coefficients. On the other hand, there are only three states - Louisiana, West Virginia, and Wisconsin - where application of such criteria would suggest that the laws are followed by higher crime rates.

This picture is essentially the same as the impression one gets from a review of the prior research - that is, uncertain relationships and differences between states. Also, the results for individual states obtained here are consistent with the results obtained in the prior research, even though they looked only at the short term impacts, though monthly data, and focused on the city level rather than the state level. The Boston studies concluded that murder and robbery rates declined after the Massachusetts weapons law (   ), similar to the results for Massachusetts in Table 6 (although for murder significant only to the .10 level. Table 6 also shows the increase in

13

assault rates found in the Boston studies, attributed to
greater tendency to report.

The results here differ substantially from those found
by Loftin and his colleagues (e.g. Loftin, McDowall, and
Wiersma 1989). They consistently found negative
relationships between gun homicides (the vast majority of
homicides), significant in Detroit, in one of three cities
studied in Florida, and in both cities in Pennsylvania. In
our study there were negative coefficients for Michigan,
Florida, and Pennsylvania, but all far from significant.
They concluded that there is little or no impact on assault
or robbery (with or without guns). That corresponds with
our findings for assault in the three states (Table 6), but
we found a significant negative impact on robbery in
Michigan and, for the current year variable, in Florida.
The difference between our results and those of Loftin and
his colleagues may be explained by the difference between
short and long run impacts or by the difference between city
trends and state-wide trends.

Other Variables. By far the most important control
variables are the state and year dummies. These, again, are
individual dummy variables for each state and each year
(except the first state and year), and they represent
factors unique to each state and to each year. The F Values
for the combined state and year dummies, given in Tables 3
to 6, are extremely high. Therefore, there are very
important factors associated with individual states and
individual years that are not represented by other variable
in the analysis, and without these fixed effect variables
the regression would be severely misspecified. The
regressions, of course, do not identify these factors, and
there are numerous possibilities, such as state differences
and yearly changes in penal policy.

The remaining control variables are usually far less
important, and the results may be biased, especially because
of nonstationarity. Levels of major crime (UCR violent
crime plus burglary) are strongly associated with more
prisons admissions and commitments in the following year,
but not with the prison population, perhaps because prison
terms decline. The percent of population in the 18-24 and
25-34 age groups are negatively associated with prison
admissions per capita (but apparently not with court
commitments), but positively associated with the release
index. This suggests that prison officials react to the
increasing prison-age population by reducing admissions for
lesser offenses, such that those admitted tend to have
longer sentences on the average.

The impact of age groups on crimes is similar to that
evidence by arrest statistics for different age groups. The
negative coefficient in the murder regression for the 15-17
age group almost surely reflects positive associations for
older age groups, and the lack of any significant age group

14

for auto theft is probably due to the fact that most thefts are by professional thieves who tend to be older (although, in separate analyses for murder an rape, no single older age group was more than marginally significant).

The state of the economy, as measured by real personal income in the prior year, is significant and positive for all crimes except burglary and larceny. This suggests the predominance of opportunity factors over need factors; that is, greater economic activity provides more interactions and increases the chances of violent crime, and it provides more tempting targets for robbers and auto thieves. But these results are difficult to interpret; they may be the result of common trends, since both crime rates and personal income are generally increasing.

CONCLUSION

The results concerning the impacts of the weapon laws are generally in the direction that proponents of the laws presumably intend. That is, the release ratio (which is similar to prison term) tended to increase and crime rates tended to decline. But the results are far from firm. When the laws are aggregated into one variable, the significance levels are low, and the magnitudes of the impacts are modest. When the individual laws are represented by separate variables, the results differ greatly from state to state. In all, the impacts of the laws are small in view of the average year-to-year changes in the prison and crime variables and small in relation to the impact of the control variables - especially the state and year dummies - entered in the regression.

The next, obvious question is what might account for the differing effects of the various state laws. This is a very difficult problem because the population size is small, limited to the fifty states, and the number of variables likely to influence the impacts of the laws is large, such that there are far too few degrees of freedom for a regression analysis.

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Table 1

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

State	Code section	Law number	Effective date	Instrument[1]	Mandatory minimum	Enhancement
01 Alabama	13a-5-6	1981-840	5-27-81	W	10 yr.[2]	
02 Alaska	12.55.125	1978-166	1-1-80	WP	7 yr.[2,3]	
03 Arizona	13-604	1977-142	10-1-78	W	1/2 sent.[2]	oth.[5]
04 Arkansas	5-4-505	1975-280	1-1-76	F		15 yr.
05 California	P. 12022	---	old	W		1 yr.[2]
06 Colorado	16-11-309	1976-547	5-10-76	W	120 days	oth.[5]
07 Connecticut	53A-216	1981-342	10-1-81	F	5 yr.	
08 Delaware	11-1447	1973-203	7-1-73	WP	3 yr.[3]	3-30 yr.
10 Florida	775.087	1975-7	10-1-75	W	3 yr.	oth.[5]
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.[6]	
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[4]	
16 Iowa	902.7	1976-1245	1-1-78	FP,[7]	5 yr.	
17 Kansas	21-4618	1976-168	7-1-76	F	NS[4]	
18 Kentucky	533.060	1976-180	6-19-76	F	NS[4]	
19 Louisiana	893.3	1981-139	9-11-81	F	5 yr.[3]	
20 Maine	17A-1252	1975-499	5-1-76	W	1 yr.[2]	oth.[5]
21 Maryland	27-36b	1972-13	6-1-72	F	5 yr.	5-20 yr.
22 Mass.	265-18B	1974-830	4-1-75	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.[2]	
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.[5]
29 Nevada	193.165	1973-759	7-1-73	W	NS[4,7]	oth.[5]
30 New Hamp.	651:2	1977-397	9-3-77	F	1 yr.	oth.[5]
31 New Jersey	2C:43-6	1981-31	2-12-81	FP	18 mo.[2]	

Table 1 (cont.)

	State	Code section	Law number	Effective date	Instru- ment[1]	Mandatory minimum	Enhance- ment
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.[5]
34	North Car.	none					
35	North Dak.	12.1-32-02.1	1977-12	7-1-77	W	2 yr.[2]	
36	Ohio	2929.71	1982-199	1-5-83	FP	3 yr.	3 yr.
37	Oklahoma	21-1287	1969-220	7-29-69	WP		2-10 yr.[3]
38	Oregon	161.610	1979-779	10-2-79	F	5 yr.[6]	
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.[3]
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[4]	
45	Utah	16-3-203	1976-9	4-1-76	F		1 yr.[2]
46	Vermont	13-4005	--	old	WP		oth.[5]
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.[3]
49	West Va.	62-12-2	1979-87	6-8-79	F	NS[4]	
50	Wisconsin	939.63	1979-114	3-1-80	W	3 yr.[6]	
51	Wyoming	6-8-101	1982-75	6-1-82	F		oth.[5]

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 can give 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.

Table 2

Years for Which Prison Data Are Not Available

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

Table 3

Impact of Weapon Laws in the Aggregate on Prisons

	Dependent Variable[1]							
	Prison Population		Admissions		Court Commitments		Release Index	
Weapons Laws (lagged)	.03	1.87	.00	.02	.01	.62	.06	2.02*
Major crime (lagged)	-.01	.18	.25	3.69***	.36	4.52***	-.14	1.45
Percent pop. 18-24	.12	.48	-1.69	4.92***	-.57	1.44	1.50	2.76**
Percent pop. 25-34	-.07	.31	-.90	2.78**	.58	1.27	.79	1.45
Per. inc. (lagged)	.02	.14	.27	1.60	.38	1.74	.06	.24
F Values								
Year dummies	26.02***		8.88***		4.61***		3.85***	
State dummies	27.61***		49.09***		49.05***		11.06***	
Dependent var. mean	4.82		4.25		4.05		.67	
Degrees of freedom	807		656		574		551	
Adjusted R-square	.99		.99		.99		.71	
D-W before correction	.56		.60		.83		.96	
D-W after correction	1.67		1.97		1.92		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 release index), and all are in natural logs. The time periods covered, excluding one year lost in the autocorrelation correction, are 1972-89 for population, 1973-88 for admissions, 1975-88 for commitments and the release index. The two columns below each dependent variable are the coefficients and absolute values of the T Ratios.

Table 4

Impact of Individual Weapons Laws on Prison Variables

	Dependent Variable[1]			
	Prison Population	Admissions	Court Commitments	Release Index

Weapon Laws[3] (lagged)

	Prison Population		Admissions		Court Commitments		Release Index	
01 Alabama (1981)	.12	2.35*	-.03	.27	-.15	1.11	.28	3.23**
02 Alaska2 (1980)	.29	2.62**	.14	1.26	.38	3.53***	.27	1.48
03 Arizona[2] (1978)	.12	1.45	.15	1.40	.21	2.53*	.22	1.65
04 Arkansas (1976)	-.01	.19	-.09	.82	-.05	.53	--	--
06 Colorado (1976)	-.04	.41	-.30	3.31**	--	--	--	--
07 Conn.[2] (1981)	.13	2.01*	.05	.47	.11	1.13	.24	2.30*
11 Georgia (1976)	-.36	5.51***	-.09	.99	--	--	--	--
12 Hawaii (1976)	.25	1.66	.12	1.29	--	--	--	--
13 Idaho (1977)	-.06	.61	-.29	1.77	-.27	1.09	.25	2.30*
15 Indiana[2] (1977)	.21	3.65***	.17	2.65**	.26	2.90**	.28	2.51*
16 Iowa (1978)	.10	1.27	.25	3.46***	.21	2.87**	-.13	.83
17 Kansas (1976)	.16	1.74	-.07	.42	--	--	--	--
18 Kentucky (1976)	-.20	2.78**	-.11	2.13*	--	--	--	--
19 Louisiana (1981)	.08	1.10	.11	2.01*	.10	1.57	-.11	1.13
20 Maine[2] (1976)	-.21	2.87**	-.68	9.06***	--	--	--	--
23 Michigan (1977)	.05	.87	-.10	1.26	-.15	1.54	--	--
24 Minnesota[2] (1979)	-.37	8.50***	-.19	2.92**	-.10	1.27	-.13	1.86
26 Missouri (1979)	.04	.65	.11	1.34	.04	.57	-.06	.68
27 Montana (1978)	.05	.63	-.26	1.69	-.28	1.72	.30	1.89
28 Nebraska (1978)	-.25	3.75***	-.29	2.53*	-.21	1.62	.19	1.48
30 New Hamp. (1977)	.14	.99	-.07	.47	-.10	.94	.65	3.67***
31 New Jersey (1981)	.18	2.67**	--	--	--	--	--	--
32 New Mexico (1977)	-.18	1.59	-.08	.62	-.12	.66	-.33	2.38*
33 New York (1980)	.08	1.38	.18	2.86**	.17	1.89	-.06	.62
35 North Dak. (1977)	-.15	1.22	.04	.30	.25	1.88	.14	.78
36 Ohio (1983)	.01	.32	-.06	.99	-.01	.26	.13	1.40
38 Oregon (1979)	-.13	2.06*	-.09	.80	-.17	1.80	-.09	.59
39 Penn.[2] (1982)	.11	2.30*	.10	1.11	.05	.57	.18	1.47
41 South Car. (1986)	-.03	.27	-.17	1.23	-.16	1.02	.26	1.85

Table 4 (page 2)

	Prison Population		Admissions		Court Commitments		Release Index	
42 South Dak. (1985)	.08	.82	-.01	.07	-.06	.28	-.01	.04
43 Tennessee (1976)	.01	.12	.03	.30	--	--	--	--
44 Texas (1977)	.09	1.27	.09	1.41	-.13	1.24	.12	.78
45 Utah (1976)	-.08	.91	.12	1.03	--	--	--	--
49 West Va. (1979)	-.10	.92	-.02	.19	.16	1.40	-.05	.37
50 Wisconsin (1980)	-.07	1.12	-.03	.32	.03	.22	-.06	.50
51 Wyoming (1982)	-.09	1.06	-.19	1.26	-.16	.98	.17	1.07

Other Variables[1]

	Prison Population		Admissions		Court Commitments		Release Index	
Major crime (lagged)	-.06	1.27	.24	3.64***	.34	4.21***	-.10	.98
Percent pop. 18-24	-.15	.57	-1.79	4.86***	-1.21	3.11**	1.07	1.89
Percent pop. 25-34	.11	.46	-.76	2.15*	-.07	.16	.70	1.29
Per. inc. (lagged)	-.11	.89	.29	1.78	.52	2.43*	.23	.85

F Values

	Prison Population	Admissions	Court Commitments	Release Index
Weapon laws	5.83***	5.19***	2.61***	3.02***
Year dummies	23.24***	7.39***	4.45***	3.98***
State dummies	37.52***	37.75***	44.26***	10.10***
Dependent var. mean	4.82	4.25	4.05	.67
Degrees of freedom	772	621	548	527
Adjusted R-square	.99	.99	.99	.77
D-W before correction	.69	.72	.94	.96
D-W after correction	1.60	1.90	1.90	1.89

* = significant to .05 level; ** to .01 level; *** to .001 level.
1) See Note 1, Table 3.
2) The weapon laws went into effect within a year of the data that a new general sentencing law went into effect.
3) The states not listed are 1) those without specific deadly weapon laws (Illinois, Mississippi, and North Carolina), and 2) those with laws effective before 1976 for prison population and admissions (California, Delaware, Florida, Maryland, Massachusetts, Nevada, Oklahoma, Rhode Island, Vermont, Virginia and Washington), and those effective before 1977 for commitments and the release index, and 3) states where the data for the independent variable are useable for less than two years before or after the law.

Table 5

Impact of Weapons Laws in the Aggregate on Crime Rates

	Dependent Variable[1]			
	Murder	Rape	Assault	Robbery
Weapon Laws (lagged)	-.04 1.94	.00 .05	-.01 .50	-.05 2.20*
Percent pop. 15-17	-.68 3.28**	.12 .68	.29 1.07	1.24 5.78***
Percent pop. 18-24	.48 1.79	.78 3.50***	.57 2.40*	-.13 .48
Per. inc. (lagged)	.55 3.58***	.38 2.95**	.36 2.54*	.89 5.74***
F Values				
Year dummies	6.03***	9.76***	7.14***	15.04***
State dummies	150.95***	67.90***	77.74***	151.43***
Dep. var. mean	1.94	3.28	5.37	4.86
Deg. of freedom	878	878	878	878
Adjusted R-square	.94	.98	.99	.99
D-W before correction	1.47	.77	.59	.72
D-W after correction	2.05	2.09	1.73	1.80

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

1) The variables, except the dummies, 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. The time periods for the regressions, excluding one year lost in the autocorrelation correction, are 1971-89, except that the period for larceny is 1972-89.

Table 5 (cont.)

	Dependent Variable[1]		
	Burglary	Larceny	Auto Theft
Weapons Laws (lagged)	-.02 1.85	-.02 1.96	-.02 1.14
Percent pop. 15-17	.63 4.71***	.31 2.95**	.35 1.78
Percent pop. 18-24	.02 .10	.16 1.21	.11 .55
Real per. inc. (lagged)	.02 .17	-.07 1.02	.61 4.58***
F Values			
Year dummies	43.76***	97.72***	11.95***
State dummies	47.72***	46.49***	27.90***
Dependent var. mean	7.09	7.87	5.92
Degrees of freedom	878	829	878
Adjusted R-square	.99	.99	.99
D-W before correction	.49	.52	.49
D-W after correction	1.73	1.67	1.68

Table 6

Impact of Individual Weapons Laws on Violent Crime Rates

	Dependent Variable[1]							
	Murder		Rape		Assault		Robbery	

Weapon Laws[3] (lagged)

01 Alabama (1981)	-.19	1.42x	-.12	1.91x	.02	.32	.00	.02
02 Alaska[2] (1980)	.09	.62	.26	2.00*	.07	.49	.05	.55
03 Arizona[2] (1978)	.05	.89	-.14	1.94	-.04	.51	-.01	.10
04 Arkansas (1976)	-.17	1.68	-.08	1.24	-.09	.92	.02	.23
06 Colorado (1976)	-.10	1.34	.00	.00	.02	.23	-.03	.18
07 Conn.[2] (1981)	.08	.64	.07	1.02	.04	.62	-.01	.10
10 Florida (1975)	-.11	.90	-.03	.33	.09	1.75	-.01	.08x
11 Georgia (1976)	-.21	2.25*	.16	2.96**	-.07	.75	.06	.51
12 Hawaii (1976)	-.15	1.35	.10	1.41	-.06	.51	.15	.87
13 Idaho (1977)	-.18	2.15*	-.10	1.67	.06	.32	-.03	.13
15 Indiana[2] (1977)	-.01	.07	-.08	1.02	.00	.03	-.24	2.79**
16 Iowa (1978)	.02	.24	-.10	1.82x	.34	4.55***	.03	.51
17 Kansas (1976)	-.02	.27	.10	1.68	.12	1.24	.03	.28
18 Kentucky (1976)	-.20	2.26*	-.11	1.83	-.18	1.38	-.20	3.17**
19 Louisiana (1981)	-.03	.41	-.01	.16	.06	.68	.24	2.99**
20 Maine[2] (1976)	-.09	1.11	.10	1.34	-.22	1.22	-.26	1.57
22 Mass. (1975)	-.13	1.81	.16	4.23***	.31	4.49***	-.18	2.03*
23 Michigan (1977)	.03	.30	-.05	.64	.00	.02	-.24	3.11**
24 Minnesota[2] (1979)	-.09	.81	.11	1.24	.04	.52	.03	.40
26 Missouri (1979)	-.04	.53	-.22	3.41***	.09	1.65	-.16	2.48*
27 Montana (1978)	-.13	1.45	-.03	.56	-.13	.98	-.31	2.91**
28 Nebraska (1978)	-.05	.62	-.09	1.61x	-.02	.16	-.18	2.40*
30 New Hamp. (1977)	-.14	1.43x	.41	5.28***	.11	1.66x	-.03	.24
31 New Jersey (1981)	-.16	1.78	-.05	.81	-.01	.19	-.01	.16
32 New Mexico (1977)	.02	.17	-.11	1.16	.08	.75	-.07	.61
33 New York (1980)	.06	.95	-.21	3.45***	-.01	.06	-.01	.25
35 North Dak. (1977)	.27	3.66***	.14	2.05*	-.30	3.15**	-.19	1.72
36 Ohio (1983)	-.15	2.23*	.10	1.84	-.06	.88	-.18	3.42***
38 Oregon (1979)	.19	2.60**	.00	.04	-.18	1.71x	.36	4.34***
39 Penn.2 (1982)	-.05	.59	-.02	.37	-.06	1.16	-.04	.54
41 South Car. (1986)	.06	.33	.09	1.01	-.01	.12	.12	1.18

Table 6 (page 2)

	Murder		Rape		Assault		Robbery	
42 South Dak. (1985)	.28	2.62**	.30	2.73**	-.31	1.80x	-.31	2.76**
43 Tennessee (1976)	-.22	3.07**	.10	1.44	-.20	1.58	-.02	.19
44 Texas (1977)	.15	1.20	.15	3.51***	.12	1.49	.19	2.98**
45 Utah (1976)	.16	2.00*	-.12	1.33	.09	.66	-.01	.04
47 Virginia (1975)	-.20	2.55*	-.24	3.47***	-.42	5.27***	-.23	2.79**
49 West Va. (1979)	-.10	1.28	.19	2.64**	.01	.05	-.04	.52
50 Wisconsin (1980)	.13	2.37*	.11	1.93	.31	4.94***	.15	1.67
51 Wyoming (1982)	-.37	3.66***	-.06	.64	-.07	.60	-.50	4.58***

Other Variables

	Murder		Rape		Assault		Robbery	
Percent pop. 15-17	-.10	.42	.03	.17	.23	.97	1.15	4.92***
Percent pop. 18-24	.36	1.26	.70	3.04**	.41	1.43	-.17	.55
Per. inc. (lagged)	.67	4.21***	.34	2.80**	.23	1.51	.85	5.51***

F Values

	Murder	Rape	Assault	Robbery
Weapon laws	2.87***	4.72***	3.59***	3.86***
Year dummies	4.00***	10.08***	8.27***	14.02***
State dummies	115.28***	55.22***	70.82***	109.35***
Dependent var. mean	1.94	3.27	5.37	4.86
Degrees of freedom	840	840	840	840
Adjusted R-square	.95	.99	.99	.99
D-W before correction	1.66	.92	.73	.92
D-W after correction	2.04	1.99	1.67	1.79

* = significant to .05 level; ** to .01 level; *** to .001 level.
x = significant when the law is not lagged.

1) See Note 1 in Table 5.

2) See Note 2 in Table 4.

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

Table 6 (page 3)

Impact of Individual Weapons Laws on Crime Rates

	Dependent Variable[1]					
	Burglary		Larceny		Auto Theft	

Weapon Laws[3] (lagged)

	Burglary		Larceny		Auto Theft	
01 Alabama (1981)	-.06	.86	.00	.04	-.06	.63
02 Alaska[2] (1980)	-.05	.60	-.04	.71	.01	.05
03 Arizona[2] (1978)	-.09	1.76x	-.05	.99	-.10	1.99*
04 Arkansas (1976)	.06	.95	.01	.16	.22	2.87**
06 Colorado (1976)	-.05	.79	.08	1.06	-.15	1.68x
07 Conn.[2] (1981)	-.03	.65	-.06	2.13*	-.20	2.49*
10 Florida (1975)	-.16	2.42*	-.03	.66x	-.13	1.41x
11 Georgia (1976)	-.02	.35	.18	4.21***	.04	.36
12 Hawaii (1976)	-.07	.82	.04	.84	-.06	.45
13 Idaho (1977)	.01	.08	-.11	1.75x	-.13	.86
15 Indiana[2] (1977)	-.13	3.30**	-.14	3.65***	-.08	1.26
16 Iowa (1978)	.21	5.15***	-.04	1.71	.00	.05
17 Kansas (1976)	.05	.92	-.05	1.42x	.00	.04
18 Kentucky (1976)	.03	.64	-.03	.56	-.08	.73
19 Louisiana (1981)	.17	2.92**	.17	4.33***	.07	.72
20 Maine[2] (1976)	-.22	2.35*	.01	.12	.08	.54
22 Mass. (1975)	-.03	.49	-.01	.13	-.20	2.11*
23 Michigan (1977)	-.08	1.65x	-.08	2.20*	-.07	1.02x
24 Minnesota[2] (1979)	.02	.36	.01	.18	-.18	1.69x
26 Missouri (1979)	-.02	.?.	.01	.13	-.01	.16
27 Montana (1978)	.06	1.01	.05	1.21	-.13	1.22
28 Nebraska (1978)	.15	4.51***	.05	2.12*	-.29	3.27**
30 New Hamp. (1977)	-.01	.07	.02	.29	-.11	.81
31 New Jersey (1981)	-.25	6.93***	-.06	1.68	.08	1.23
32 New Mexico (1977)	-.02	.17x	.08	.96	.08	.73
33 New York (1980)	-.18	4.97***	.02	.84	.10	1.77
35 North Dak. (1977)	.11	2.32*	.09	1.34	-.03	.22
36 Ohio (1983)	-.08	2.51*	-.07	2.52*	-.06	1.13
38 Oregon (1979)	.11	1.74	.00	.04	-.01	.07
39 Penn.[2] (1982)	-.12	4.01***	-.03	.84	.00	.07
41 South Car. (1986)	.04	.64	.03	.49	.01	.10

Table 6 (page 4)

	Burglary		Larceny		Auto Theft	
42 South Dak. (1985)	-.01	.16	-.01	.11	-.33	2.63**
43 Tennessee (1976)	-.02	.43	-.07	1.82x	-.06	.51
44 Texas (1977)	.06	1.14	-.10	2.16*	.24	3.03**
45 Utah (1976)	.09	.84	.06	.85	.06	.41
47 Virginia (1975)	-.08	1.44	-.08	2.50*	-.15	1.80
49 West Va. (1979)	.08	.99	.01	.09	-.02	.20
50 Wisconsin (1980)	.07	2.23*	-.01	.14	.00	.06
51 Wyoming (1982)	-.07	.94	-.11	1.92	-.54	3.88***

Other Variables

	Burglary		Larceny		Auto Theft	
Percent pop. 15-17	.92	6.65***	.48	4.38***	.30	1.42
Percent pop. 18-24	-.13	-.76	.02	.15	-.16	.66
Real per. inc. (lagged)	.06	.65	.03	.43	.58	4.21***

F Values

	Burglary	Larceny	Auto Theft
Weapon laws	5.14***	2.99***	2.24***
Year dummies	40.17***	97.39***	11.30***
State dummies	52.94***	29.65***	22.77***
Dependent var. mean	7.09	7.87	5.92
Degrees of freedom	840	791	840
Adjusted R-square	.99	.99	.99
D-W before correction	.65	.63	.64
D-W after correction	1.68	1.60	1.65

Table 7

Summary of Results of Tables 4 and 6

	Negative Coefficients[1]		Positive Coefficients[1]	
	Total	Significant	Total	Significant
Prison				
Population	16	6	20	6
Admissions	21	5	14	4
Release Index	10	1	15	5
Crime Rates				
Murder	25	9	14	5
Rape	20	6	17	8
Assault	19	4	18	4
Robbery	26	12	12	3
Burglary	23	10	16	5
Larceny	20	10	17	3
Auto theft	24	10	11	2

1 - Number of states, excluding coefficients that are zero when rounded to two decimal places. Significant coefficients include those significant when the variable is not lagged.