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A COMPARATIVE STUDY OF THE PREVENTIVE EFFECTS OF MANDATORY SENTENCING LAWS FOR GUN CRIMES*

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I. INTRODUCTION

No policy designed to prevent firearm violence is more popular than mandatory sentence enhancements for gun crimes. By providing stiff and certain penalties when a gun is involved in an offense, sentence enhancement laws[1] promise to reduce the use of firearms by criminals. Because the laws apply only when a crime is committed, they impose no direct costs on legitimate gun owners. Opinion polls find that a large majority of the public favors mandatory sentence enhancements, and more than half the states have adopted them.[2] If these laws deliver their expected crime preventive effects, they are an especially attractive approach to regulating the use of firearms.

We previously conducted case studies to estimate the preventive effects of mandatory sentencing on firearm offenses in Detroit, Jacksonville, Tampa and Miami.[3] Based on the findings of these

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[1] Examples include: HAW. REV. STAT. § 706-660.1 (1985 & Supp. 1991); MINN. STAT. ANN. § 609.11 (West 1987 & Supp. 1992); N.H. REV. STAT. ANN. § 651:2, II-b (1986 & Supp. 1991); S.D. CODIFIED LAWS ANN. § 22-14-12 (1988); WASH. REV. CODE ANN. § 9.94A.125 (West 1988).

[2] *See* JAMES D. WRIGHT ET AL., UNDER THE GUN: WEAPONS, CRIME, AND VIOLENCE IN AMERICA 235 (1983); Franklin E. Zimring, *Firearms, Violence and Public Policy*, 265 SCI. AM. 48, 52 (1991).

[3] The Detroit research is reported in Colin Loftin et al., *Mandatory Sentencing and Firearms Violence: Evaluating an Alternative to Gun Control*, 17 LAW & SOC'Y REV. 287 (1983), and in Colin Loftin & David McDowall *"One With A Gun Gets You Two": Mandatory Sentencing and Firearms Violence in Detroit*, 455 ANNALS AM. ACAD. POL. & SOC. SCI. 150 (1981)

earlier studies, we concluded there was little evidence that sentence enhancement laws are successful in reducing violent crime. More recently, we completed similar studies in Philadelphia and Pittsburgh (Allegheny County), Pennsylvania. In this paper, we pool together the individual results to obtain a combined estimate of the impact of the laws. The pooled results lead to very different conclusions from the city-specific case studies. The analyzed data suggest that the mandatory sentencing laws substantially reduced the number of homicides; however, any effects on assault and robbery are not conclusive because they cannot be separated from imprecision and random error in the data.

Part II of this article describes the earlier case studies. Part III explains the methodology for combining the individual results and presents the pooled estimates. Part IV discusses our interpretation of the findings and Part V provides suggestions for future research.

II. CASE STUDIES

A. MANDATORY SENTENCING LAWS IN THREE STATES

Our analysis is based on six city-specific case studies, which monitored the effects of mandatory sentencing on violent crime in Detroit, Jacksonville, Tampa, Miami, Philadelphia and Pittsburgh. The key features of the laws were the same in each area.[4] First, each law required judges to impose a specified sentence on defendants convicted of an offense involving a gun. Second, mitigating devices such as probation, suspended sentences and parole were prohibited. In theory, all sentences specified by the laws had to be served in full.

[hereinafter Loftin & McDowall, *One With a Gun*]. The Florida studies are reported in Colin Loftin & David McDowall, *The Deterrent Effects of the Florida Felony Firearm Law*, 75 J. CRIM. L. & CRIMINOLOGY 250 (1984) [hereinafter Loftin & David McDowall, *The Deterrent Effects*]. The effect of the Detroit law on the processing of court cases is also evaluated in Milton Heumann & Colin Loftin, *Mandatory Sentencing and the Abolition of Plea Bargaining: The Michigan Felony Firearm Statute*, 13 LAW & SOC'Y REV. 393 (1979).

4 Specifically, the Florida law, FLA. STAT. ANN. § 775.087(2) (West 1976 & Supp. 1992), required a three-year sentence for persons convicted of committing any of 12 specified felonies while in possession of a firearm. The law went into effect on October 1, 1975. Michigan's law, MICH. COMP. LAWS ANN. § 750.227b (West 1991 & Supp. 1992), went into effect on January 1, 1977. It mandated a two-year sentence for the possession of a firearm while committing any felony. Pennsylvania's law, 42 PA. CONS. STAT. ANN. §§ 9712-9714 (1982), adopted in June 1982, required a five-year minimum sentence for any of seven violent crimes if (1) the offense was committed with visible possession of a firearm; (2) the defendant had been convicted of the same offense within the past seven years; or (3) the crime was committed in or near public transportation facilities. In Florida and Michigan, the mandatory sentences were to be served consecutively to the sentence for the triggering felony. In all three states, suspended, deferred and withheld sentences were explicitly prohibited, and parole was not possible until the firearm sentence was served.

Finally, all three states used advertising campaigns involving radio and television commercials, posters, bumper stickers and billboards to communicate the message that offenders would receive additional punishment if they used a gun to commit a crime. The laws are therefore similar enough in purpose and content that they can be regarded as replications of approximately the same experiment.

B. DESIGN OF THE CASE STUDIES

Each earlier city-specific case study used an interrupted time series research design[5] that compared the level of violent crime before and after the statutes were adopted. This comparison provides an estimate of the aggregate preventive effect of the announcement of the laws.[6] If the laws were effective in reducing firearm crimes, the number of gun offenses should decrease in the post-intervention period.

To further strengthen the basis for causal inference, our design incorporates several other features. First, to increase the precision of the estimates in each city, we examined long, monthly pre-intervention series (54 to 150 months) for three violent crimes: homicides, assaults and robberies.[7]

Second, because the statutes apply specifically to gun crimes, we analyzed companion series of gun offenses and non-gun offenses.[8] This additional analysis narrows the range of extraneous

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5 *See* THOMAS D. COOK & DONALD T. CAMPBELL, QUASI-EXPERIMENTATION: DESIGN AND ANALYSIS ISSUES FOR FIELD SETTINGS 207-32 (1979).

6 The results thus represent the net influence of deterrence, incapacitation and other preventive mechanisms. Because we cannot model individual behavior, it is not possible to isolate the specific mechanisms that might be responsible for an observed change in crime. This places some limits on the conclusions that can be drawn, but it is offset by the fact that the interrupted time series is among the strongest quasi-experimental designs. JACK P. GIBBS, CRIME, PUNISHMENT, AND DETERRENCE 29-93 (1975) provides an extensive discussion of the mechanisms through which a legal change might influence criminal behavior.

7 The length of each pre-intervention series was dictated by data availability. In Detroit, the pre-intervention series included 96 monthly observations for homicides and 120 monthly observations for assaults and robberies. There were 93 monthly pre-intervention observations for all crimes in each of the three Florida cities. For homicides in Pittsburgh and Philadelphia, the pre-intervention series included 150 monthly observations. For assaults and robberies in the state of Pennsylvania, there were 54 monthly pre-intervention observations.

8 In Florida, data for robbery and assault were drawn from the Uniform Crime Report (UCR) Return A tapes for January 1968 through December 1980 (156 monthly observations). Florida homicide data were taken from the UCR's Supplementary Homicide Report tapes for January 1968 through December 1978 (132 observations). In Detroit, robberies and assaults from January 1967 through December 1979 (156 observations) were taken from the Detroit Police Department's Computerized Monthly Reports. Detroit homicides were collected from Vital Statistics data tapes provided by

variables that could be confounded with the intervention. Another causal variable would be confounded with the law only if it influenced gun and non-gun crimes differently, and if it changed markedly at the intervention point. The contrast between the gun and non-gun series is also helpful in identifying displacement or substitution effects. Outcomes of this type would occur if offenders switched from guns to other weapons after the laws were implemented.[9]

Third, systematic within-series variation (nonstationarity and autocorrelation) was removed from each series using an autoregressive integrated moving average (ARIMA) noise model.[10] If the noise model is correctly specified, it will account for causes of violent crime (poverty, age structure, etc.) that operate consistently throughout a series. Unless these other variables change in an unusual way at the time of the intervention—a threat that Cook and Campbell call "history"—the noise model cannot explain an observed impact.[11]

After an appropriate noise model was separately developed for each series, an intervention model was added to represent the effects of the gun law.[12] We considered three types of intervention models: an abrupt permanent change model, a gradual permanent

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the Michigan Department of Public Health for January 1969 through December 1978 (120 observations). The Pennsylvania Commission on Crime and Delinquency provided state-level robbery and assault data for January 1978 through December 1984 (84 observations), and the Pennsylvania Department of Health supplied homicide data for Allegheny County (Pittsburgh) and the city of Philadelphia for January 1970 through December 1984 (180 observations).

All the homicide series are defined similarly: the number of gun homicides versus the number of homicides by other means. Because robberies were defined only as "armed" and "unarmed" in the Uniform Crime Reporting program prior to 1975, all the robbery series, except those for Pennsylvania, are the number of armed and the number of unarmed robberies. In Pennsylvania, it was possible to distinguish gun robberies from robberies with other weapons. The Detroit and Pennsylvania assault series are defined as gun assaults versus assaults by other means. In Florida, gun assaults are compared to knife assaults.

9 Substitution effects have been reported in similar contexts. *See, e.g.*, Lee R. McPheters et al., *Economic Response to a Crime Deterrence Program: Mandatory Sentencing for Robbery with a Firearm*, 22 ECON. INQUIRY 550 (1984); Glenn L. Pierce & William J. Bowers, *The Bartley-Fox Gun Law's Short-Term Impact on Crime in Boston*, 455 ANNALS AM. ACAD. POL. & SOC. SCI. 120 (1981); and Charles L. Rich et al., *Guns and Suicide: Possible Effects of Some Specific Legislation*, 147 AM. J. PSYCHIATRY 342 (1990).

10 G. E. P. BOX & G. JENKINS, TIME-SERIES ANALYSIS: FORECASTING AND CONTROL (1976).

11 COOK & CAMPBELL, *supra* note 5, at 211.

12 G. E. P. Box & G. C. Tiao, *A Change in Level of a Non-Stationary Time-Series*, 52 BIOMETRIKA 181 (1965); G. E. P. Box & G. C. Tiao, *Intervention Analysis with Applications to Economic and Environmental Problems*, 70 J. AM. STAT. ASS'N 70 (1975).

change model and an abrupt temporary change model.[13] For each series, the abrupt permanent change model provided the best fit to the data.[14]

C. RESULTS OF THE CASE STUDIES

The results of the city-specific case studies are summarized in Table 1 (homicides), Table 2 (assaults) and Table 3 (robberies).[15] The intervention coefficient for each offense, ω_0, represents the change in the number of monthly crime reports following the announcement of the statutes. The analyses for Detroit and the three Florida cities are presented in detail elsewhere,[16] and our major interest is in combining the estimates. Therefore, the individual case studies are only briefly discussed here.

In Detroit, there was a statistically significant decrease in gun homicides, but no significant change in any other offense. We concluded from this study that the results best fit a model in which the mandatory sentencing law did not have a preventive effect on crime.[17] Similarly, in Florida, there were significant decreases in Tampa gun homicides and Jacksonville gun assaults. Unarmed robberies increased significantly in Tampa and Miami, but armed robberies did not change. In addition, there was a significant increase in Tampa gun assaults. Again, we concluded that the results did not support a preventive effect model.[18]

Alone, the Pennsylvania estimates do not strongly challenge the conclusion that the statutes have no preventive effect. There were statistically significant decreases in gun homicides in both Pittsburgh and Philadelphia. The decrease in Philadelphia gun homicides was mirrored by a reduction in non-gun homicides, however,

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[13] *See* DAVID MCDOWALL ET AL., INTERRUPTED TIME SERIES ANALYSIS (1980) for details.

[14] *See id.* at 83-85 for the criteria used to select the best-fitting model.

[15] An appendix that describes the intervention analyses in more detail is available from the authors. Since the studies were originally conducted over several years using a variety of computer programs and machines, we have re-estimated the models to verify the results in a common computing environment. All of the series were re-estimated using BMDP88's P2T algorithm on an IBM 3081 running VM/CMS Release 5. Variations in the computing environments are responsible for most differences from previously published estimates, but an error in the earlier analysis is responsible for a change in Jacksonville gun assaults.

[16] Loftin et al., *supra* note 3; Loftin & McDowall, *One With a Gun, supra* note 3; Loftin & McDowall, *The Deterrent Effects, supra* note 3.

[17] Loftin et al., *supra* note 3, at 309-10; Loftin & McDowall, *One With a Gun, supra* note 3, at 162.

[18] Loftin & McDowall, *The Deterrent Effects, supra* note 3, at 256-57.

and there was no change in gun assaults or robberies in the state of Pennsylvania.

Although the results of the case studies are complex, no individual study provides clear support for the proposition that mandatory sentencing reduces firearm violence. If the studies are considered together, however, the no-effect conclusion is less certain. This is especially so for homicide. Gun homicides decreased in all six of the cities, significantly in four (Detroit, Tampa, Pittsburgh and Philadelphia) and insignificantly in two (Jacksonville and Miami). The argument for a preventive effect is stronger when the three crimes are compared across cities than when the findings for each city are examined separately.

The immediate goal in each city-specific case study was to obtain an unbiased estimate of the policy's impact in a given area. Yet the ultimate objective was not simply to describe what happened at a particular site, but rather to predict what would occur if other cities enacted mandatory sentencing statutes for gun crimes.

From this point of view, each city-specific case study represents a sample observation drawn from a population of studies that could be conducted under similar circumstances. If the effects of mandatory sentencing vary with features unique to a site's setting or law, a single case may provide an untrustworthy basis for inference. A more desirable approach would be to combine the results from several replications. An estimate based on combining several sites would be less sensitive to the characteristics of any particular area, and it would more precisely measure the expected impact in the population.

III. Comparative Analysis

To estimate the combined impact of the laws, we pooled the results from the six cities. This analysis treats the impact-estimate for each city as an observation from a distribution of possible responses to mandatory sentencing laws. The major motivation for pooling is to obtain an overall estimate of the effect of the statutes on each type of crime. Pooling, however, has other advantages as well. First, in conjunction with the case study designs, pooling makes it extremely unlikely that the estimates are confounded with other variables. Second, the pooled data make it possible to measure variation in the response across cities. Finally, pooling increases statistical power and efficiency, allowing the influence of the laws to be determined more precisely.

A. PROCEDURES USED FOR THE COMPARATIVE ANALYSIS

We obtained a combined estimate of the effect of the statutes on each type of violent crime using statistical methods developed for synthesizing the results from multiple studies.[19] Since the level of crime varies greatly among the cities, we first standardized the individual estimates by dividing each intervention coefficient by the standard deviation of its error term:

$$d_j = \frac{\omega_{0j}}{\sqrt{RMSE_j}}$$

Here, ω_{0j} is the estimate of the change in a crime for city j; d_j is the standardized estimate of the change; and $RMSE_j$ is the residual mean square error from the intervention model.

Standardization is necessary because the cities vary greatly in the number of violent crimes per month. For example, a decrease of ten gun homicides has a different meaning in Detroit than it would in Jacksonville. Many more homicides occur each month in Detroit than in Jacksonville, and an unweighted comparison of crime counts in the two cities would be misleading. The standardized effects measure the change in crime attributable to the intervention, expressed in standard deviation units.

To pool the individual standardized effects for each offense, we used a variance components model. This model is most easily understood by comparing it with a simpler approach, called a fixed effects model. The fixed effects model involves computing the mean of the standardized coefficients for each crime. The fixed effects model can be written as:

$$d_j = \gamma + e_j \qquad e_j \sim N(0,V)$$

In the fixed effects model, γ measures the change in crime attributable to the laws, and e_j is a random error term. The e_j vary from city to city because only a portion of the time series process generating crime is observed. The e_j are assumed to be distributed Normally with a mean of zero and a variance of V.

The fixed effects model is limited by assuming a common impact, γ, that holds across all cities. In other words, after removing random errors in sampling over time, the effect of the laws on a particular type of crime is identical in each area. This is probably

[19] This type of synthesis is often referred to as a "meta-analysis." The specific methods we use are described in LARRY V. HEDGES & INGRAM OLKIN, STATISTICAL METHODS FOR META-ANALYSIS 189-203 (1985), and Stephen W. Raudenbush & Anthony S. Bryk, *Empirical Bayes Meta-Analysis*, 10 J. EDUC. STAT. 75 (1985).

unrealistic. More likely, the effects will vary because of differences in the details of the laws, implementation, publicity and other factors specific to a given setting. In this case, there will be a distribution of effects instead of one common impact.

The variance components model that we estimate incorporates the site-specific effects. The variance components model can be written as:

$$d_j = \delta_j + e_j \qquad e_j \sim N(0,V)$$
$$\delta_j = \gamma + u_j \qquad u_j \sim N(0,\tau)$$

or:

$$d_j = \gamma + u_j + e_j$$

In the variance components model, γ may be interpreted as the average effect of the laws. No city may actually experience this average effect because the impact will vary from one setting to another depending on local conditions. The value for γ is a meaningful quantity, however, because it provides an estimate of the change in crime across the population of cities. In other words, holding unique characteristics and random error constant, γ is the expected impact of the announcement of the laws.

Besides the average impact, the variance components model provides an estimate of the dispersion of the effects across settings (τ). The larger the value of τ, the larger the expected variation in the effects. If τ is equal to zero, the variance components model reduces to a fixed effects model.

To estimate the variance components model, it is necessary to make an assumption about the probability distribution from which the site-unique effects are drawn. Following conventional practice, we assume that the operation of numerous random variables will generate a Normal distribution of effects. Given this assumption, the variance components model can be estimated in a variety of ways. We used an empirical Bayes algorithm developed by Raudenbush and Bryk.[20]

The analysis will allow us to select among three general theoretical models of community response to the announcement of the sentencing laws. If the reported number of gun crimes declines after the laws are implemented, and there is no similar decline in crimes without guns, then the data fit a preventive effect model. An

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[20] Raudenbush & Bryk, *supra* note 19. The algorithms are available in ANTHONY S. BRYK ET AL., AN INTRODUCTION TO HLM: COMPUTER PROGRAM AND USERS' GUIDE (1989) (manual and software distributed as *HLM Distribution Package Version 2.20*, April 1991, for DOS 3 and later, by Scientific Software, Inc., 1525 E. 53rd St., Suite 906, Chicago, Ill. 60615). We also assume that the replications are independent. Because cities from the same state are included in the analysis, this is probably only approximately correct.

increase in non-gun crimes and a decrease (or an increase of smaller magnitude) in gun offenses would be compatible with both a preventive effect model and a weapon substitution model. While weapon substitution may influence the pattern of injuries resulting from crimes, it will not reduce the total number of offenses that are committed. Other outcomes favor a model in which there is no preventive effect. The no effect model, like the preventive effect model, subsumes several different micro-level processes. Most notably, it does not distinguish between the case where the policy produces no change in sanctions and the case where a change in sanctions does not influence criminal behavior.

B. RESULTS OF THE COMPARATIVE ANALYSIS

The pooled analysis for homicides (Table 1) provides exceptionally strong support for the preventive effect model. The intervention estimates (ω_0) for gun homicides are negative in all six cities and statistically significant in Detroit, Tampa, Pittsburgh and Philadelphia. The estimate of the average standardized effect (γ) is .69. This implies that the expected reduction in gun homicides is about two-thirds of a standard deviation.[21]

To illustrate the magnitude of this effect, we can reverse the standardization procedure and express the reduction in terms of the number of homicides rather than in standardized units. For example, consider Detroit, a city with a pre-intervention mean of forty gun homicides per month and a standard deviation of eight. Here, a decrease of .69 standard deviation units represents an average of 5.5 lives saved each month, a fourteen percent reduction.

In contrast, there was little change in non-gun homicides. The signs of the intervention effects were positive in four cities (Detroit, Jacksonville, Tampa and Miami) and negative in two (Philadelphia and Pittsburgh). While the decrease in Philadelphia non-gun homicides was statistically significant, it was smaller than the reduction in homicides committed with a gun. The average standardized effect across all the cities is only $-.03$. It is hard to imagine data that would fit the preventive effect model better than these series.

Table 2 describes a similar analysis for assaults.[22] In this case,

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21 A rule of thumb, suggested in JACOB COHEN, STATISTICAL POWER ANALYSIS FOR THE BEHAVIORAL SCIENCES 24-27 (rev. ed. 1977), is that standardized effects of 0.2 may be regarded as small, 0.5 as medium and 0.8 as large. By these guidelines, the impact on gun homicides is substantial.

22 The assault series consist of aggravated assaults as defined by the Uniform Crime Reporting program: "Aggravated assault is an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of as-

the fit to the crime preventive model is poor. Gun assaults decreased significantly in Jacksonville, but they increased significantly in Tampa. Although there were also decreases in gun assaults in Detroit, Miami and the state of Pennsylvania, they were not large enough to be statistically significant. The average standardized change in gun offenses is small ($\gamma = -.36$) and not significantly different from zero. Other weapon assaults did not change appreciably in any of the areas, and the average standardized impact of $-.06$ is also statistically insignificant. The results, therefore, provide little solid evidence of a reduction in gun assaults that can be attributed to the statutes.

As with assaults, the robbery[23] data, presented in Table 3, do not fit the preventive effect model well. Armed robberies did not decrease significantly in any area following the introduction of the laws; in fact, the intervention coefficients are negative for only two of the five series. The estimate of γ is .08 and not statistically significant. For unarmed robbery, however, there were two cities, Tampa and Miami, that experienced large and significant increases following the laws' adoption. The average effect across all cities for unarmed robbery is a significant increase of two-thirds of a standard deviation. At best, one might argue that the sentencing laws prevented armed robberies from increasing in the same way as unarmed robberies.

The estimates of τ measure the amount of variation in the standardized effects across the cities. Chi-square tests[24] led to a rejection of the null hypothesis of zero variation in the effects for each crime. We conclude, therefore, that the impact of the laws differs from one setting to another. Because of this variation, the experience of any single city may not be an accurate guide to the average effect across the population of cities as a whole.

IV. Discussion

The results are a logical puzzle because different conclusions are reached depending on the weight given to the homicide data as

sault is usually accompanied by the use of a weapon or by means likely to produce death or great bodily harm. Attempts are included. . . ." *See* FEDERAL BUREAU OF INVESTIGATION, UNIFORM CRIME REPORTS FOR THE UNITED STATES 22 (1989). For Pennsylvania, we analyzed assaults for the entire state because weapon-specific monthly data were not available for Philadelphia and Pittsburgh.

[23] The robbery series are defined according to the conventions of the Uniform Crime Reporting program: "Robbery is the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear." *Id.* at 17.

[24] HEDGES & OLKIN, *supra* note 19, at 197-98.

opposed to the data for robbery and assault. In interpreting the earlier case studies in Detroit and the three Florida cities, we placed equal emphasis on each type of crime. The homicide estimates fit the prevention model in Detroit and Tampa, but the estimates for the other offenses were not consistent with a preventive effect. Because of this apparent irreconcilability, we attributed the homicide findings to chance and concluded that the data best fit the no effect model. That preliminary conclusion is now at odds with the results of the pooled homicide analysis.

It is not possible to select a single model if each type of crime is equally weighted. That is, across the three offenses, the findings are incompatible with *both* the preventive effect model and the no effect model. The consistency of the effects on gun homicide virtually rules out the possibility that factors confounded with mandatory sentencing could account for the reductions in this crime. Such an explanation would require that confounded factors reduce gun homicides, but not other types of homicides, in different years and in six different cities. Therefore, the accumulating evidence forces us to reject the no effect model as a general explanation of the results. There is clear and convincing evidence of preventive effects for homicide.

At the same time, the preventive effect model does not adequately fit the robbery and assault data. This result is perplexing because a reduction in homicides caused by the laws should be accompanied by a more general decrease in gun violence. In a sense, homicide is not a separate offense; it is a measure of the severity of injury associated with other assaultive crimes. Accordingly, one would not expect a mandatory sentencing law for gun offenses to have an effect on homicides without influencing either assaults or robberies.

Faced with this pattern of outcomes, it is necessary to consider a wider range of explanations. The simplest alternative is to assume that homicides are more completely and accurately reported than robberies and assaults. As a result, the effect of the laws is detected for homicide, but lost in the noise of the less sensitive robbery and assault series.

There is independent reason to believe that the homicide data are more precise than the data for robberies and assaults. First, homicides are uniformly serious, and they command attention in reporting and recording.[25] Variation in the seriousness of the other

[25] *See* Michael J. Hindelang, *The Uniform Crime Reports Revisited*, 2 J. CRIM. JUST. 1 (1974).

offenses produces discretion in reporting, and there is less consistency over time or between jurisdictions in recording practices.[26] Second, our experience in modeling the robbery and assault data suggests inconsistent and erratic patterns of recording. The homicide series were easy to model and the noise components were simple and fit well. The assault and robbery series, on the other hand, required complex models whose fit was relatively poor. This outcome would be expected in the presence of irregular shifts in the recording process. Finally, the Uniform Crime Reports did not permit us to distinguish between robbery offenses with and without guns. Accordingly, we compared armed with unarmed robberies for Detroit, Tampa, Jacksonville and Miami, and this necessarily introduced imprecision in the estimates. These considerations lead us to the working hypothesis that mandatory sentencing laws have a preventive effect on homicide, and probably on other gun crimes as well. However, the available data contain measurement errors that mask the preventive effects on assault and, perhaps, robbery.

Beyond the substantive findings, the analysis also illustrates the desirability of using replications to identify variation in the effects of a legal innovation in different areas. There is evidence that features of the local setting affected the magnitude of the preventive effects. The impact of the laws on homicide was negative in all the cities that we studied, but it varied greatly from case to case. If there were a measure of data quality (or any other factor that might explain the variation), it could be included in the variance components model.[27] Such measures are not available, however, and any explanation of the heterogeneity remains speculative.

Although the comparative analysis cannot account for the variation across cities, it shows the importance of considering these differences in studying the influence of the laws. Each case study provided an unbiased estimate of the impact of the law in a particular jurisdiction. Yet if areas differ in characteristics related to the law, individual estimates are of relatively limited value. These estimates will be drawn from a probability distribution of possible ef-

[26] *See* WRIGHT ET AL., *supra* note 2, at 154-56 for a summary of sources of error in the UCR data. For some of the sources of error, *see* Richard Block & Carolyn R. Block, *Decisions and Data: The Transformation of Robbery Incidents into Official Robbery Statistics*, 71 J. CRIM. L. & CRIMINOLOGY 622 (1980); Richard McCleary et al., *Uniform Crime Reports as Organizational Outcomes: Three Time Series Experiments*, 29 SOC. PROBS. 361 (1982); Victoria W. Schneider & Brian Wiersema, *Limits and Use of the Uniform Crime Reports*, *in* MEASURING CRIME: LARGE-SCALE, LONG-RANGE EFFORTS 21 (D. L. MacKenzie et al. eds., 1990); and David Seidman & Michael Couzens, *Getting the Crime Rate Down: Political Pressure and Crime Reporting*, 8 LAW & SOC'Y REV. 457 (1974).

[27] *See* Raudenbush & Bryk, *supra* note 19, at 88-93.

fects, and a single case will be inadequate to characterize the population response.

Because of heterogeneous effects, crime might even increase in some settings despite a strongly negative average impact in the population. For example, we found a mean decrease in gun homicides of .69 standardized units following the introduction of the laws. The variance of the estimates was .22, however, implying substantial differences from one city to another. Because of the dispersion, any city with an impact-estimate more than 1.47 standardized units above the mean would register an increase in gun homicides following the law.[28] Given that the effects are drawn from a Normal distribution, increases of this type would be expected about seven percent of the time. If one examined a single city and was unfortunate enough to select such a case, it would appear that the laws were responsible for higher levels of homicide.

V. Conclusions

There is reason for both confidence and caution in our findings. The confidence follows from the strength of the research design and the quality of the homicide data. The consistency of the homicide estimates across the six locations requires that we modify our earlier conclusions. The only plausible interpretation of the results is that the reductions in gun homicides are due to the announcement of the laws. Since there were no compensating increases in the number of homicides committed with weapons other than guns, these effects can be interpreted as truly preventive of homicides.

For reasons that we cannot directly evaluate, the robbery and assault series do not reflect the preventive effects. It seems likely, however, that this result is due to a lack of precision in the data. Assault and robbery may respond to the policy in different ways, but we cannot distinguish between the no effect model and measurement errors in these crimes.

There are several reasons for caution in interpreting the results. First, despite the powerful research design, the estimate of the average impact is probably not very precise. This is because only six cities were examined, and substantial heterogeneity existed in the size of the intervention coefficients. There is little doubt that the average effect is negative, at least for homicides. However, addi-

[28] That is, $\frac{D-(-.69)}{\sqrt{.22}} = 1.47.$

Any impact estimate more than 1.47 standard deviations above the mean of the distribution will therefore be positive in sign.

tional research is necessary to identify characteristics of the organizational environment and conditions of implementation that explain the variation in the impacts.

Second, we did not examine a probability sample of cities that have instituted mandatory sentencing laws. The cities were selected fortuitously as our interest in the topic progressed. We began with Detroit because it was convenient. We then examined Florida because news reports suggested that its law had reduced gun homicides. Pennsylvania was added because its law was enacted and widely publicized while we were working on the issue. The sample is thus composed of areas in which the policy change was heavily advertised, and inferences should be limited accordingly. Future research should select a probability sample of cities and study the effects of factors such as the form of the publicity campaign on the size of the preventive effects.

Third, the post-intervention periods were all relatively short, ranging from twenty-four months for Detroit to sixty-three months for assaults and robberies in the Florida cities. Our analysis thus addresses only short-term changes, and it does not allow inferences about the impact over a long period. The effects of the laws may decay, and it would be desirable to extend the study periods to examine this possibility.

Finally, we do not know what features of the policy are responsible for the preventive effects. Given the evidence that preventive effects exist, future research also should investigate the specific behavioral mechanisms responsible for the effects, factors that influence their magnitude and their temporal trajectory.

Table 1
SUMMARY OF ANALYSIS FOR HOMICIDE IN SIX CITIES

City	Parameter	Gun Homicide	Other Homicide
	Case Studies: Intervention Estimates		
Detroit	ω_0	−10.5700*	.0016
	d	−1.3893	.0049
Jacksonville	ω_0	−.8577	.1822
	d	−.3058	.0968
Tampa	ω_0	−1.1950*	.1167
	d	−.6165	.0875
Miami	ω_0	−.3441	.8031
	d	−.1253	.3258
Pittsburgh	ω_0	−1.0700*	−.3500
	d	−.4613	−.1772
Philadelphia	ω_0	−6.8300*	−2.2500*
	d	−1.2973	−.5507
	Meta-Analysis: Variance Components Model		
All Cities	γ	−.6904	−.0316
	σ_γ	.2108	.1236
	γ/σ_γ	−3.28*	−.26
	τ	.2225	.0516
	χ^2 (5 df)	28.79*	11.47*

ω_0 = Impact-estimate from intervention model
d = Standardized impact-estimate
γ = Grand mean standardized effect
σ_γ = Standard error of grand mean standardized effect
τ = Estimate of variance of parameters
χ^2 = Test of H_0: $\tau = 0$
* $p < .05$

Table 2
SUMMARY OF ANALYSIS FOR ASSAULT

Jurisdiction	Parameter	Gun Assault	Other Assault
	Case Studies: Intervention Estimates		
Detroit	ω_0	−.9967	.0327[a]
	d	−.0506	.3132
Jacksonville	ω_0	−20.9500*	−2.4650[b]
	d	−1.8830	.2937
Tampa	ω_0	10.2400*	−4.2750[b]
	d	1.1862	−.4732
Miami	ω_0	−9.5400	−3.6720[b]
	d	−.6856	−.3415
Pennsylvania	ω_0	−12.2500	36.0700[c]
	d	−.3862	.6064
	Meta-Analysis: Variance Components Model		
All Jurisdictions	γ	−.3641	−.0567
	σ_γ	.4959	.2039
	γ/σ_γ	−.734	−.28
	τ	1.1913	.1740
	χ^2 (4 df)	130.87*	23.35*

ω_0 = Impact-estimate from intervention model
d = Standardized impact-estimate
γ = Grand mean standardized effect
σ_γ = Standard error of grand mean standardized effect
τ = Estimate of variance of parameters
χ^2 = Test of H_0: $\tau = 0$

* $p < .05$
[a] Non-gun assault
[b] Knife assault
[c] Non-gun weapon assault

Table 3
SUMMARY OF ANALYSIS FOR ROBBERY

Jurisdiction	Parameter	Armed Robbery	Unarmed Robbery
	Case Studies: Intervention Estimates		
Detroit	ω_0	.0778	.0207
	d	.7044	.1844
Jacksonville	ω_0	2.5300	3.5580
	d	.1308	.4136
Tampa	ω_0	−3.7230	9.6590*
	d	−.3415	1.2361
Miami	ω_0	1.9440	30.1700*
	d	.0825	1.4541
Pennsylvania	ω_0	−19.0300[a]	3.4590[b]
	d	−.2264	.0987
	Meta-Analysis: Variance Components Model		
All Jurisdictions	γ	.0763	.6809
	σ_γ	.1826	.2787
	γ/σ_γ	.42	2.44*
	τ	.1351	.3539
	χ^2 (4 df)	21.75*	45.68*

ω_0 = Impact-estimate from intervention model
d = Standardized impact-estimate
γ = Grand mean standardized effect
σ_γ = Standard error of grand mean standardized effect
τ = Estimate of variance of parameters
χ^2 = Test of H_0: $\tau = 0$

* $p < .05$
[a] Gun robbery
[b] Other weapon robbery