MANDATORY SENTENCING AND FIREARMS VIOLENCE: EVALUATING AN ALTERNATIVE TO GUN CONTROL

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Michigan's Felony Firearm Statute (Gun Law) imposed a two-year mandatory add-on sentence for defendants convicted of possession of a firearm in the commission of a felony. The Law was widely advertised with proponents claiming that it would introduce greater equity in sentences, ensure certainty of punishment, and decrease violent crime in the state. We examine the processing of these Gun Law cases in Detroit Recorders Court, as well as the effects of the law on crime, and find that most of the goals of the Law's proponents are not met. Notwithstanding a rigid prosecutorial policy which prohibited plea bargaining in these gun cases, alternative mechanisms developed to mitigate the Law's effects and, in most instances, to preserve the "going rate" for various crime categories. Similarly, using an interrupted time-series model, we are unable to uncover effects of the law, or the associated publicity campaign, on violent crime.

I. INTRODUCTION

The past decade has witnessed one of the most sweeping efforts to reform criminal sentencing in our history. The demise of the rehabilitative ideal, as well as its rejection by critics on the left and on the right, has led a number of states and the Congress to seek ways to decrease and structure discretion. Some of these reforms are animated by a desire to reduce sentencing disparity; others, while sharing these goals in general, are more concerned with enhancing the certainty and severity of punishment.

In this paper, we evaluate one such reform, Michigan's Felony Firearm Statute. The Gun Law (as it is popularly known in Michigan) went into effect on January 1, 1977. It mandates a two-year "flat-time" sentence for those who possess

* The research reported here was supported in part by grants 78-NI-AX-0021 and 79-NI-AX-0094 from the National Institute of Justice and T32-MH 14598-05 from the National Institute of Mental Health. Points of view or opinions stated are those of the authors and do not necessarily reflect the official position or policies of the U.S. Department of Justice.

LAW & SOCIETY REVIEW, Volume 17, Number 2 (1983)

firearms while committing felonies. The sentence is added on to and served consecutively with the sentence for the triggering felony.[1]

For some of its supporters, the Gun Law was indeed but a first step toward the more general imposition of mandatory sentences (Cahalan, 1973; 1977a). For others it was a means to "do something" about crime in a way that was neither offensive to opponents of gun control nor unacceptable to a variety of liberal constituencies. The NRA's support, for example, was attracted by coupling the applicability of the Gun Law to the commission of an additional felony. And given the general climate of concern about crime in Michigan, it was difficult for traditionally liberal groups to oppose a statute specifically designed for violent offenders. Indeed, the UAW ultimately supported it, thus ensuring its passage by an overwhelming margin.[2]

In evaluating the Gun Law as a policy intervention, we have collected data on court sanctions and crime in Detroit. Detroit was selected because: (1) it accounts for a large percentage of the violent crime in Michigan;[3] (2) the Prosecutor's office and the court, at least when we began the study, had an automated data management system which facilitated sample selection;[4] and (3) the Wayne County Prosecutor (Detroit) was a leading supporter of the Gun Law. The Prosecutor was instrumental in mounting a publicity campaign that included billboards and bumper stickers proclaiming "One With a Gun Gets You Two." More importantly, he adopted, as a matter of office practice, a policy that required his subordinates to charge the Gun Law whenever the facts warranted it, with a concomitant

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[1] MICH. COMP. LAWS ANN. § 750.227b. For a second or third conviction under the statute, the mandatory sentence escalates. Since the time period for this paper is limited to the first two years of the statute's life, this provision is not considered, though it is not impossible for a defendant to plead to several gun-related felonies and receive the escalated penalty for the second charge. During the first two years this was highly unusual, but there was at least one well-publicized example. See *Detroit Free Press*, March 1, 1977: A12.

[2] The Gun Law passed in the Michigan House by a vote of 100 to 5 and in the Senate by a vote of 28 to 8.

[3] In 1976, 65 percent of murders, 37 percent of the rapes, 70 percent of the robberies, and 27 percent of the assaults reported to the police in Michigan were committed in the city of Detroit. Federal Bureau of Investigation (1976: Tables 3 and 6).

[4] The Prosecutor's Management Information System—PROMIS—was especially useful for case selection. Unfortunately, funding for PROMIS ceased in October 1978. We are confident that case selection for the remaining months of 1978 was consistent with the cases yielded by PROMIS. However, the laborious and expensive process of selecting cases with the court's data base precluded including subsequent years in our analysis.

prohibition on any subsequent plea bargaining.[5] This restriction means our data are not affected by a common response to mandatory sentences, namely, a shift in discretionary decision-making from the court to the prosecutor.[6] Thus, within Detroit, the opportunity for testing the efficacy of mandatory sentences on the court's sentencing practices and Detroit's violent crime rates was particularly attractive.

II. THE COURT

A. Method

To estimate the impact of the law on sanctions, we coded information on all cases disposed of by Detroit Recorders Court during 1976, 1977, and 1978 in which the original charge was any one of eleven violent felonies.[7] Though we refer to the data as a sample, they are, in fact, a universe of all cases that met our specifications of time and original charge.

In addition, we conducted a series of interviews with Recorders Court judges, prosecutors, and defense attorneys. The first set of interviews took place in 1977, during what might be called the "implementation phase" of the Gun Law. Subsequent interviews were designed to track the consequences of the Gun Law over time. Collectively, these data structure the interpretation we accord our quantitative data.

In this paper we analyze only six of the original eleven types of felonies. They are:

Murder (first or second degree)
Criminal Sexual Conduct (first degree)
Armed Robbery
Assault with Intent to Commit Murder
Assault with Intent to Commit Great Bodily Harm
Felonious Assault[8]

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[5] For a more detailed discussion of this policy and its implementation, see Heumann and Loftin (1979).

[6] See, for example, Alschuler (1978) and Zimring (1977). Norval Morris has aptly characterized this shifting of discretion as a "hydraulic theory" of discretion. "Discretion is rather like matter, it's not destructible. It's only movable or controllable" (Travis and O'Leary, 1979).

[7] Recorders Court has jurisdiction over all felonies committed in the city of Detroit from arrest to final disposition. For a recent description of case processing in Recorders Court, see Eisenstein and Jacob (1977); also Heumann and Loftin (1979).

[8] The four other charged crimes that we collected data on [manslaughter, criminal sexual conduct (second and third degree), and assault with intent to rob and steal (armed)] are not analyzed because: (1) there are too few cases to analyze them separately, and (2) the offenses are too different in character

In the analysis which follows, the cases are further divided into four groups depending on whether the offense was committed with a gun or not (Gun/No Gun) and whether it occurred before or after the law went into effect (Pre/Post). The contrasts among the four groups are used to estimate three parameters in a model: (1) the effect of using a gun in the offense; (2) the effect of committing the offense after January 1, 1977; and (3) an interaction effect representing post intervention effects which are unique to the gun cases. The interaction effect will serve as our estimate of the impact of the Gun Law.

As in any observational study from which causal inferences are drawn, we make crucial but conventional assumptions about variables which are not explicitly included in the model (disturbance variables).[9] The most important assumption, for present purposes, is that extraneous (disturbance) variables which influence the behavior of the court in the post intervention period have comparable effects on gun and nongun cases.[10] In general, if factors other than the Gun Law do not have selective effects on gun cases in the post intervention period, the interaction effect will be an unbiased estimate of the effect of the Gun Law.

For our dependent variable, we needed a quantitative measure of the amount of time a defendant could expect to spend in confinement. To avoid problems posed by indeterminate sentencing, suspended sentences, good-time discounts, life sentences, and the like, we created a measure which we call *expected minimum sentence* (EMS), according to the following conventions:

1. Cases in which the defendant was not incarcerated (e.g., acquitted, dismissed, suspended sentence, or

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to justify grouping them together. In cases where more than one of these offenses is charged, the case is assigned a "case defining charge" depending on the configuration of charges. In general the case defining charge is the most serious of the set of charges.

[9] See Johnston (1972: 121-75) and Kmenta (1971: 201-46) for a discussion of some of the implications.

[10] An example of such an extraneous factor is the "crash program," which the judges of Recorders Court began in early 1977 to reduce a backlog of cases on the docket. We assume that the effects of this program will be reflected in the Pre/Post parameter along with other factors. However, since there is no reason to believe that the "crash program" had different effects on gun cases, we assume that it does not influence the interaction effect. We cannot ensure the independence of extraneous factors because we cannot randomly assign cases to treatments. The logic of our procedure is to treat the assumption that the disturbances are independent of the explanatory variables as a working hypothesis, explicitly conditioning our conclusions on it, and testing its validity whenever possible.

probation) are coded zero. In these cases the defendant was charged with an offense, but the charge did not result in post-conviction confinement.[11]

2. All minimum sentences are discounted for "good-time" using the same procedures used by the Michigan Department of Corrections.[12] We simply calculated the maximum amount of good-time and subtracted it from the minimum sentence.
3. All life sentences, and minimum sentences which were over ten years after they had been discounted for good-time, were coded as ten-year sentences. This corresponds to the policy of the Michigan Department of Corrections as prescribed by statute. The Parole Board has jurisdiction over cases after ten calendar years of incarceration.

EMS roughly corresponds to the expected length of sentence, but more precisely it is the length of time to first possible release.[13]

B. Results

Figure 1 presents the distribution of the expected minimum sentence for the four groups of armed robberies. This offense is selected for illustration here because of its numerical and symbolic importance. Armed robbery is a major public concern and was a central target of the Gun Law. The success or failure of the law must, at least to some degree, be gauged in terms of its impact on this offense.

Two features of the distributions are noteworthy. First, in all conditions, a very large proportion of the cases fall in the lowest sentence category (less than one year). Most of these are cases that were dismissed or acquitted. Very few of those

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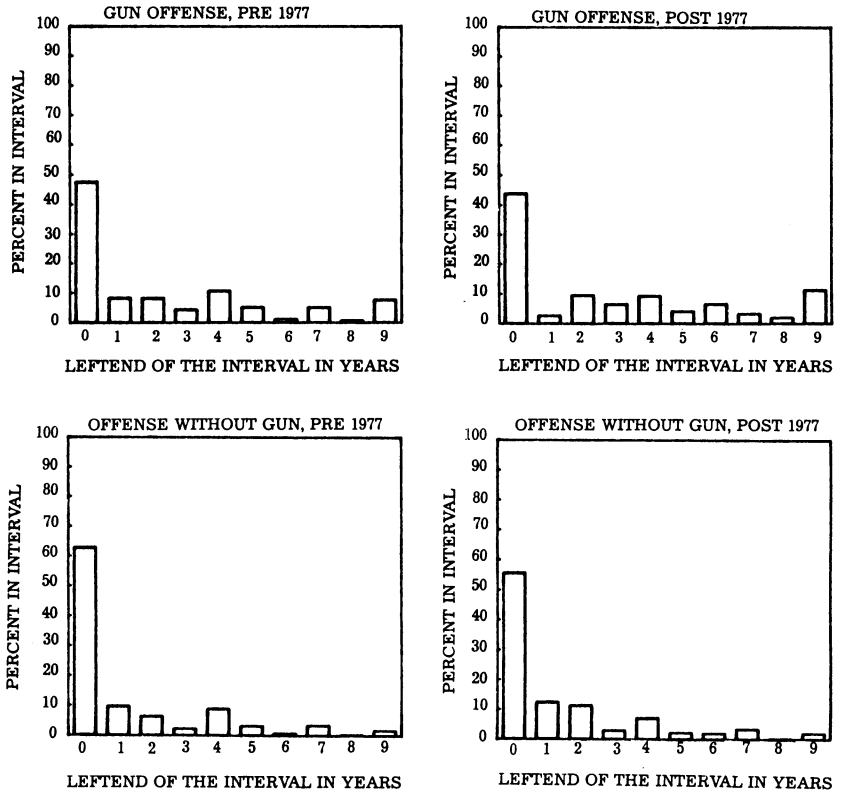
[11] Of course, we do not assume that all defendants are guilty. We simply assume that the probability of being guilty of the charge is unrelated to other variables in the analysis. For some of the analysis we include acquitted and dismissed cases along with those that are found guilty because the Gun Law may have influenced the probability of conviction as well as the length of sentences given to convicted cases. In other places we use only those cases which resulted in convictions.

[12] The provisions of the "good-time" discounts are defined in Section 33, Act No. 118 as amended, Section 800.33, Compiled Laws of 1948. In October 1978, a public referendum ended the good-time discounts for most violent offenses, but this change does not affect our study period to any significant degree.

[13] None of the results reported here are sensitive to the choice of a measure of length of sentence. Alternative measures, such as the actual minimum sentence (with life sentences excluded or assigned an arbitrary value), lead to the same conclusions.

Figure 1. Expected Minimum Sentence for Armed Robbery in Days By Weapon and Time Period



in this category received any sentence at all. Second, it does not appear that sentences changed dramatically in the post intervention period. In both the pre and the post periods, robberies with a gun received longer sentences than those without a gun, but there is no step-like change in the sentence distribution of gun cases in the post intervention period.

In order to estimate the magnitude of the effect of the Gun Law for each type of offense, we fit a set of models in which sentence is a function of whether or not a gun was used in the offense, the time period, and an interaction term:

$$Y_i = f(X_1, X_2, X_3) \quad (1)$$

Where,

Y_i = Expected Minimum Sentence

X_1 = 1 if gun case
0 otherwise

X_2 = 1 if committed in post intervention period
0 otherwise

X_3 = 1 if gun case and committed in post intervention period
0 otherwise.

https://doi.org/10.2307/3053349 Published online by Cambridge University Press

Because the distribution of EMS is truncated at zero and there is a concentration of cases at the lower limit, ordinary least squares estimates are not appropriate. Therefore, we present maximum likelihood estimates based on the Tobit model (Tobin, 1958; Goldberger, 1964: 253-55), which are more suitable for these distributions.[14]

The results of the analysis for the six original charge categories (see Table 1) confirm the impression that one gets from a visual inspection of figures like Figure 1. The Gun Law did not produce an across-the-board increase in the average sentence. In four of the six offenses no significant change in the distribution of EMS can be attributed to the Gun Law (i.e., there was no significant interaction effect). Only in the cases of felonious assault and assault with intent to commit murder is there a significant interaction.[15]

Table 1. Tobit Estimates of Length of Sentence by Original Charge

	Constant	No Gun/Gun	Pre/Post	Interaction
Murder (1st. and 2nd.) N=1142				
B	897	−26	406	−57
B/S(B)	(4.82)*	(−.13)	(1.55)	(−.18)
Criminal Sexual Conduct (1st.) N=924				
B	−197	709	−272	−495
B/S(B)	(−1.39)	(2.64)*	(−1.50)	(−1.32)
Armed Robbery N=2767				
B	−227	637	310	−7
B/S(B)	(−2.30)*	(5.63)*	(2.33)*	(−.05)
Assault with Intent to Commit Murder N=705				
B	−412	−137	−475	785
B/S(B)	(−1.87)	(−.55)	(−1.53)	(2.19)*
Assault with Intent to Commit Great Bodily Harm N=533				
B	−728	−146	−150	162
B/S(B)	(−6.64)*	(−.81)	(−.97)	(.62)
Felonious Assault N=1281				
B	−677	−214	−6.34	303
B/S(B)	(−11.17)*	(−2.37)*	(−.08)	(2.51)*

* = Significant at .05 level or beyond
Y_i = Expected Minimum Sentence in Days
B/S(B) is the ratio of the coefficient to its standard error

[14] In the Tobit model, EMS is not a linear function of the parameters. For the appropriate form, see Goldberger, 1964: 253-54.

[15] The basic conclusions are not modified by the choice of an estimator or the particular measure of sentence length. Ordinary least squares estimates lead one to the same general results. Similar results were also obtained using

Perhaps we fail to find a consistent effect of the law because we are looking in the wrong place. After all, a major justification for the law was that it would make punishment more certain for offenses committed with a gun. It is possible that the Gun Law increased the certainty of punishment but did it in such a way that it is not evident in the analysis of a gross measure like the average sentence. We investigated this possibility by decomposing the EMS into more refined elements: (1) the probability of confinement given conviction, (2) the probability of conviction, and (3) the length of sentence given confinement. EMS depends on each of these three factors. Since the Gun Law may have had different effects on each, we analyzed them separately.

Unquestionably, a major objective of the Gun Law was to increase the likelihood that persons convicted of violent crimes with a gun would serve some time in prison. It is clear from both the legislative analysis of the bill and the debate in the Michigan Legislature that one source of concern was the belief that many armed criminals did not receive prison sentences because sentencing judges had the discretion to give suspended sentences and probation.[16]

Table 2 presents the results of an analysis of the effect of the law on the probability of being sentenced to confinement if convicted. Since the dependent variable is binary:

$$y = \begin{cases} 1 \text{ if sentence involves incarceration} \\ 0 \text{ otherwise,} \end{cases} \qquad (2)$$

we use the probit model (Goldberger, 1964: 250-51). The independent variables remain the same as in (1) above.[17]

The results are surprising. The only statistically significant interaction term is in the model for felonious assault. There is also a relatively large (but not statistically significant) interaction for assaults with intent to commit murder, but in none of the other offenses was there a significant interaction effect. The finding that the probability of serving time did not generally change is especially important because the primary objective of the law was to influence this part of the sentencing process.

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the actual minimum sentence with life sentences excluded, as well as with other measures.

[16] Michigan House Bills, Analysis H.B. 5073, 2-4-76.

[17] As with the Tobit model, the function is not a linear function of the parameters. See Goldberger (1964: 250) for a description of the probit function.

Table 2. Probit Model Estimates for the Probability of Confinement Given Conviction by Original Charge

	Constant	No Gun/Gun	Pre/Post	Interaction
Murder (1st. and 2nd.) N=829				
B	1.07	−.01	.10	.34
B/S(B)	(7.51)*	(−.08)	(.48)	(1.36)
Criminal Sexual Conduct (1st.) N=855				
B	.54	.50	.14	−.30
B/S(B)	(5.68)*	(2.28)*	(1.06)	(−.98)
Armed Robbery N=1,748				
B	.88	.43	.38	−.13
B/S(B)	(9.22)*	(3.74)*	(2.74)*	(−.77)
Assault with Intent to Commit Murder N=411				
B	.34	−.02	.01	.51
B/S(B)	(1.98)*	(−.12)	(.03)	(1.76)
Assault with Intent to Commit Great Bodily Harm N=286				
B	−.20	−.06	−.07	.05
B/S(B)	(−1.58)	(−.26)	(.40)	(.15)
Felonious Assault N=665				
B	−.30	−.35	−.20	.69
B/S(B)	(−2.95)*	(−2.36)*	(−1.46)	(3.38)*

* = Significant at .05 level or beyond
Y_i = 1 If Sentenced to Confinement, 0 Otherwise
B/S(B) is the ratio of the coefficient to its standard error

Next we investigated the impact of the law on the likelihood of conviction. Studies of mandatory sentencing in other contexts have shown that strict sentencing requirements decrease the probability of conviction (The Association of the Bar of New York, 1978; Rossman *et al.*, 1979). If judges and juries are less likely to convict defendants facing the mandatory two-year add-on, this would reduce the average sentence and tend to offset any increase in the sentences of those convicted. Table 3 shows that the Gun Law did indeed reduce the probability of conviction for felonious assaults and perhaps armed robberies.[18]

Finally, we can ask: Did the length of sentence, given conviction, respond to the Gun Law? Table 4 summarizes the results of a least squares regression analysis of the length of the expected minimum sentence for those defendants who

[18] In this analysis conviction refers to conviction on any charge, not necessarily the original charge. A more detailed analysis, which is not reported in this paper, shows that the reduction in convictions is due primarily to an increase in cases which were dismissed rather than to an increase in acquittals at trial.

Table 3. Probit Model Estimates for the Probability of Conviction by Original Charge

	Constant	No Gun/Gun	Pre/Post	Interaction
Murder (1st. and 2nd.) N=1,143				
B	.65	−.14	.27	−.18
B/S(B)	(6.12)*	(1.18)	(1.62)	(.97)
Criminal Sexual Conduct (1st.) N=924				
B	.37	−.12	−.13	−.14
B/S(B)	(5.04)*	(.80)	(1.35)	(.67)
Armed Robbery N=2,768				
B	.20	.13	.22	−.18
B/S(B)	(3.15)*	(1.75)	(2.51)*	(1.74)
Assault with Intent to Commit Murder N=705				
B	.44	−.18	−.26	.10
B/S(B)	(3.13)*	(1.13)	(1.35)	(.46)
Assault with Intent to Commit Great Bodily Harm N=533				
B	.26	−.18	−.17	−.12
B/S(B)	(2.69)*	(1.13)	(.77)	(.53)
Felonious Assault N=1,282				
B	.07	−.08	.22	−.36
B/S(B)	(.99)	(.82)	(2.17)*	(2.55)*

* = Significant at .05 level or beyond
Y_i = 1 If Convicted, 0 Otherwise
B/S(B) is the ratio of the coefficient to its standard error

were convicted. On this dimension we find more general evidence that the change had an impact. There are significant positive interaction effects for four of the six offenses. Only murder and criminal sexual conduct, the two most serious of the six offenses, failed to respond to the Gun Law.

The analysis illustrates the importance of distinguishing among the major components of the average sentence. When we estimated the impact of the law on the expected sentence (Table 1), we found little impact except for felonious assaults and assaults with intent to commit murder. If our inference is about the average sentence that any given offender can expect to receive, the conclusion is that the law had little impact. A similar conclusion should be drawn with regard to the certainty of prison for those who are convicted. The law produced little increase except for felonious assaults. The analysis of the probability of conviction shows that for some cases the Gun Law reduced the probability of conviction and so increased the number of cases that were subject to no punishment at all. Finally, if we refer only to those who were convicted and

Table 4. Least Squares Estimates of Length of Sentence Given Confinement by Original Charge

	Constant	No Gun/Gun	Pre/Post	Interaction
Murder (1st. and 2nd.) N=720				
B	2224	229	96	−92
B/S(B)	(19.10)*	(1.71)*	(.58)	(−.471)
Criminal Sexual Conduct (1st.) N=416				
B	1687	773	−412	−7.54
B/S(B)	(16.80)*	(4.00)*	(−3.10)*	(−.027)
Armed Robbery N=1,576				
B	1183	482	−117	388
B/S(B)	(15.76)*	(5.62)*	(−1.18)	(3.34)*
Assault with Intent to Commit Murder N=282				
B	1149	82	−477	662
B/S(B)	(6.84)*	(.42)	(−1.96)*	(2.37)*
Assault with Intent to Commit Great Bodily Harm N=123				
B	629	−39	−261	563
B/S(B)	(6.77)*	(−.24)	(−1.96)*	(2.37)*
Felonious Assault N=228				
B	284	−30	61	266
B/S(B)	(5.77)*	(−.38)	(.88)	(2.57)*

* = Significant at .05 level or beyond
Y_i = Expected Minimum Sentence, Given Confinement
B/S(B) is the ratio of the coefficient to its standard error

sentenced to prison, there was a moderate but significant increase in the expected length of sentence for some, but not all, offenses. Putting the tables together suggests that the Gun Law had a consistent and clear impact only in the case of felonious assault. It reduced the probability of conviction, but for those who were convicted the probability of incarceration and the average time served went up. These findings may well be two sides of the same coin. The data suggest that because the Gun Law did enhance the sentences of those charged with felonious assault, the courts and/or the prosecutors were more reluctant to convict. Where sentence enhancement was less likely, amelioration by dismissal or acquittal was not necessary.

C. Discussion

The Gun Law was designed to add two years to the sentence of all defendants within its scope. Yet the data suggest that it did not do so. Why? The law seems straightforward enough—a defendant possessing a firearm while committing a felony must receive two years on the

separate Gun Law charge in addition to the sentence (if any) on the primary felony. And in Detroit the prosecutor promised—and for the most part kept his promise—to charge the Gun Law whenever appropriate and to forbid plea bargaining on this charge.[19] How, then, can we account for our findings, which suggest, with some exceptions in terms of a moderate increase in sentence length, that the situation stayed pretty much the same after the introduction of the mandatory two-year sentence?

We think the answer lies in the centrality of the notion of a "going rate" to court participants. By going rate, court actors mean the "typical" sentence accorded the perpetrator of a stereotypical felony. These, of course, are rough estimates and are subject to modification on the basis of the particulars of the case. But, through the lenses by which experienced court participants view cases, there is still a sense that cases are "worth" a certain price. As one prosecutor remarked:

> . . . it's like the currency, the value of money. Everybody uses the currency, we [in court] just happen to have a slightly different system, well quite a bit different system, than the rest. But every experienced attorney as well as the experienced prosecutor, I mean you could look at the same case and know who your judge was and probably get a fairly accurate prediction as to the current value . . . of this crime . . . (27, prosecutor).

The notion of a rough tariff, or going rate, for specific crime/criminal combinations structured the court's response to the Gun Law in two ways. In serious cases which, even prior to the Gun Law, would have resulted in substantial time in prison, sentences on the primary felony were adjusted downward to compensate for the two-year Gun Law increment. As one prosecutor explained:

> If we know for a fact that the going rate was seven and a half to fifteen before, now it's five and a half to fifteen plus two for the gun with almost all of the judges on this bench, and we should have known to begin with that no state statute is, I mean the judges here aren't going to say, "The state legislature has spoken, I don't have any discretion, I'm going to routinely give you what I would have given you before plus two for the gun." You know human nature doesn't work that way. With each judge, the judge looks at what he would have given before and then legally he can cut that

[19] For a detailed discussion see Heumann and Loftin (1979: 397-407).

down, because that's in the area of his powers, two years and add two years for the gun (36).

Interviews with judges confirmed that the process of sentence adjustment or "absorption" was widespread:

> . . . when I have sentenced on both the felony firearm and in gun cases, I have taken into consideration the result that I wish to achieve in terms of how many years he ought to get. All in all, looked at from that point of view, I have a suspicion that the felony firearm, other than to create a few knotty, troublesome legal problems, has not had much of an effect on actual sentences received by felons (26).
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> IF I TOOK ALL YOUR ARMED ROBBERIES FROM '76 AND ALL YOUR ARMED ROBBERIES FROM '77 AND '78 AND COMPARED SENTENCES . . .
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> . . . pretty much the same. So you didn't say this is an armed robber, a five-year armed robber and now he is a five-year armed robber plus the two-year gun . . . . No. I'd do two plus three, for sure (41).

We believe that it is because of these adjustments that the quantitative data do not show a step-like two-year increase in time, even for the sample of sentenced defendants. Yet we did find some increase in the time served, despite the general effort to preserve existing going rates. Our interviews suggest some reasons for this. Several respondents maintained that defendants who received short sentences prior to the Gun Law were victims of a "trickle-up" effect:[20]

> I know that the only time, when you get below minimums, bare minimums, then the felony firearm mandatory two can't become quite irrelevant. I appreciate that it is possible for judges to simply do the math and adjust the sentence so that everybody gets the same sort of shake. It doesn't work in instances where the judge wants to give a sentence of under two years though (26, judge).
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> . . . prior to the Gun Law, if I had a person who had only one count against him, robbery, most judges view the robbery armed statute as meaning that you cannot give a term of probation, a year and a day is what most judges say, and if you had a client who had no prior record or had a very minimal record, or if he had a job, some good things in their background, you could

[20] It is possible that over a longer period of time the Gun Law might, as a result of this "trickle-up" theory, lead to a qualitative change in the going rates for particular constellations of defendant and crime characteristics. This would result from the elevation of sentences for those at the bottom of the scale, coupled with some notion of proportionality with respect to the sentences for more serious offenders.

> usually get them a year and a day. Now, most judges who are inclined, who were formerly inclined to give a year and a day, must give a year and a day [assuming a plea or conviction on armed robbery] plus the two on the gun (42, defense attorney).

The Gun Law may also have served to increase sentences in the subset of the most serious felonies. As one defense attorney (31) suggested, a "bad guy" law may have been operating. "Bad guys," that is, defendants with prior records accused of serious crimes, are likely to have a packet of charges thrown at them. After its passage, the Gun Law became part of that packet. Courts faced with such serious offenders are less concerned with the going rate. Thus, sentences which were very high prior to the Gun Law may, after the Gun Law, be even higher. However, these very long sentences are collapsed by the good-time calculations of the prison system and so may not in the long run lead to a major increment in time actually served.[21] The enhanced sentences would still be reflected in our data as an increase in the sentence length for the serious felonies.[22]

Thus far we have examined cases in which the court managed to adjust sentences (with some changes at the margins) to preserve the old norms. However, for cases in which the norm was no time in prison, the going rate could not be preserved by merely adjusting the sentence on the primary felony. The two years that would have to be served on the Gun Law would still be a drastic departure from what was regarded as "right." We see evidence that such a departure occurred in the increase in sentence length for our least serious crime, felonious assault. At the same time, in a number of these cases the judiciary sought to preserve the going rate. They did this by refusing to convict. In felonious assaults this occurred often

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[21] As our coding assumptions make clear, good-time and parole practices collapse extreme sentences. Thus, by including the Gun Law as part of the sentence, the court can appear to be symbolically harsh, while at the same time, defendants may have little incentive to expend resources to contest the Gun Law increment to the total sentencing package.

[22] Preliminary analysis of the quantitative disposition data supports this two-tailed model of sentence length increase. For example, in the armed robbery convictions, it appears that for cases in the middle of the sentencing distribution—the garden variety armed robbery—the proportion of defendants remains fairly constant. On the other hand, sentences for armed robberies at the polar ends of the distribution seemed to have changed more substantially.

Interestingly, the interviews also suggest competing considerations that may have driven some of the sentences down in serious cases that included a Gun Law count. In addition to the "compensatory discount" which we have mentioned, it was suggested that some prosecutors were less concerned with the overall sentence so long as the sacred Gun Law charge was affixed and the defendant sentenced under it.

enough so that the decrease in the conviction rate following the Gun Law was statistically significant.

What sentence adjustments allow the court to accomplish in more serious cases can often be achieved by bench or waiver trials when cases are, by Detroit's standards, relatively minor ("family" disputes as well as other felonies in which the equities militate against incarceration). Defense attorneys either negotiated in advance the outcome of bench trials [these quickly became labelled "wired trials" (37)] or gambled on the fact that a particular judge would find a way to circumvent the two-year penalty. Among the options open to a judge were to find a defendant completely innocent, to find him/her guilty of the felony but not of the Gun Law charge, or to find him/her guilty of a misdemeanor, in which case the *felony* firearm count automatically disappeared. The prosecutors, although precluded from plea bargaining in Gun Law cases, appeared to acquiesce in this process, and occasionally even seemed to suggest that a defense attorney explore it as an option with a judge. Consider the following comments:

> In those cases [in which defendants previously did not receive time] a lot of time you can do nothing unless you have a judge who doesn't like the felony firearm statute as it's applied in non-serious cases. Then you have a waiver trial and the judge may say, "I don't think the prosecutor can prove beyond a reasonable doubt that the gun was operable. It wasn't a firearm under the new felony firearm statute; I'm dismissing that; find [him] guilty of FA [felonious assault] or whatever and give him probation or give six months or whatever.". . . Almost all the waiver trials that I've had, either I've had a tremendous defense on my side, or I simply have gone to the judge and sometimes with the prosecutor, depending on who the prosecutor was, told the judge what the facts were after I had gotten complete discovery . . . . Most judges in this building will tell you: "this is my inclination." And most of the time their inclination is what they are actually going to do. And you try a case and quite often it's like a long plea with a sentence bargain and a plea bargain negotiated in chambers.
>
> FOR EXAMPLE, WITH WAIVER TRIALS, DO THEY [THE PROSECUTORS] IMPLICITLY GO ALONG WITH THOSE AS WELL?
>
> Almost all the time if you have a waiver trial and you have a gun count, the prosecutor knows well what you are trying to do and most of them will tell the judge, "Well, look, the only reason that this person didn't take a plea is because of the felony firearm, and the quickest way to dispose of this case, you know, this guy

> isn't that bad, the quickest way to dispose of this case is to have a waiver trial, and you get rid of the felony firearm."
>
> BUT ON THE RECORD?
>
> But on the record they recommend a certain sentence, object to any kind of dismissal of the felony firearm, and they are completely protected because nobody, defense attorneys, prosecutors or judges, mention what's gone on in chambers (42, defense attorney).
>
> * * * * *
>
> You see what the lawyers are doing. They have been very clever. They have been waiving juries in those cases where the equities come into play . . . . And I'll be frank with you; there have been cases where I found that it was not the possession of a firearm, where I am looking at the end as justifying the means . . . . Especially a case where, let's say, a man and his wife, a domestic dispute. I'm not talking about an animal that has been brutalizing the woman for years, he should go to jail, but I'm talking about a man and his wife who are suddenly involved in a domestic crisis, money or divorce or something, they get into a fight and he knocks her around and he's accused of, and is probably guilty of, FA, and incidental to that felony he's got a gun. Here's a guy with four kids who's been a good provider for those kids for twenty years; suddenly he's got a problem. Is it right to put him in prison for two years? I don't think that it is. I think he should be made to have some counseling; he should be forced to provide for his family and to stay out of trouble and probably he would . . . (43, judge).

While the bench trial seemed generally to provide a satisfactory mechanism for the preservation of "no time" going rates, there was, as with sentence adjustment in the more serious cases, some "slippage" and thus some change at the margins in the sentencing patterns of the court. Some defendants may not have been able to "wire" the bench trial; some may have lost their gamble that the judge would "take care" of the Gun Law; some may have hoped that a jury would nullify by acquitting, only to find themselves facing a mandatory two-year sentence. Most importantly, the changes in conviction and sentences thus realized were not systematic, were not across-the-board, and were not, therefore, congruent with one of the goals of the Gun Law—namely, equal punishment across sets of similarly situated gun offenders. The situation remained much the same, and, to the extent change was evident, it was *ad hoc*, non-patterned, and raised serious questions of equality in the application of the law. We will return to this theme in our concluding section. First,

however, it is important to consider the arena in which many believed the Gun Law would have its greatest impact—the reduction of the crime rate in a "cost-effective" manner.

III. CRIME

Though many intangible benefits were promised by proponents of the Gun Law, the key to its popularity is that it promises to reduce gun-related crime without restricting the use of guns by law-abiding citizens. Perhaps the decisive issue, therefore, is whether the Gun Law reduced the level or seriousness of violent crime in the city.

There are several mechanisms by which the Gun Law might have reduced gun-related crime. Potential offenders might have been deterred from committing offenses, or they could have chosen to substitute another weapon. Alternatively, they could have been incapacitated by prison sentences which were enhanced because of the law. To the extent the Gun Law had a deterrent effect, this might have been due to the media campaign which accompanied the implementation of the law or to changes that the law produced in the behavior of judges, prosecutors, and defense counsel.

Our research strategy is based on an interrupted time-series design which compares the volume of weapon-specific crimes before and after the law went into effect. The most serious threat to the validity of inferences in research of this type is that events which occur at about the time of the intervention may produce changes which will be mistakenly attributed to the intervention. This threat to validity is conventionally called "history" (Cook and Campbell, 1979). In order to reduce the likelihood of such an error, we compare gun offenses with nongun offenses. This allows us to control for the threat of "history" so that events other than the law that are likely to affect the crime rate have similar effects in both series. We cannot control for the possibility that the Gun Law might have reduced both gun and nongun offenses, but there is little reason to expect this effect.

Some of the most obvious events which might have influenced crime rates in Detroit at about the time the Gun Law went into effect include: the laying off of nearly a thousand police officers; a massive backlog of cases awaiting disposition in Recorders Court; attacks by street gangs on spectators at the Bicentennial fireworks display and at a Cobo Hall rock concert; the use of the Michigan State Police to patrol the city's freeways because of attacks on stalled motorists; a

federal investigation of narcotics trafficking which implicated top police executives; and the replacement of the Police Chief. The violent crime rates in the city soared in 1976. In spite of the fact that other cities with a population of 250,000 or more experienced a decline of 5 percent in violent index crimes, Detroit experienced a 5 percent increase. Armed robbery rates increased by 7 percent in Detroit while they fell by an average of 13 percent in her sister cities; and the homicide rate reached 49.7 per 100,000, just below the peak of 52.4, which occurred in 1974.[23]

In any given year it is unlikely that such an incredible series of events would occur. It is not surprising, therefore, that the situation got better in 1977. How much better, however, was a pleasant surprise. As 1977 began and the Gun Law went into effect, a virtual criminological miracle occurred. Detroit went for six days without a murder, an event which had not happened in ten years. At first people attributed it to the weather or to chance. William Hart, the new Police Chief, said that it was a "million-to-one shot" (*Detroit Free Press*, March 25, 1977: A22). But then it happened again and this time the interval between murders was seven days. Compared to the annual figures for 1976, the decline in crime in the city was dramatic. Both property and personal crime declined by about 18 percent.

Not surprisingly, many observers suggested that the Gun Law was responsible, at least in part, for the decline in crime (Cahalan, 1977b: 2; Lucas and Ledgerwood, 1978). The timing of major events makes this a natural inference, but, as we shall see, a closer look at the patterns and a statistical analysis of the crime data do not support this view.

The basic data for our analysis are presented in Figures 2-5.[24] There are three features of the data that lead us to doubt

[23] Most of the crime data are from U.S. Bureau of the Census (1976: 170; 1977: 179). The 1974 murder rate was calculated from data in the Uniform Crime Report (Federal Bureau of Investigation, 1974: 109) and in City of Detroit, Department of Health (1974).

[24] Our crime data are taken from the records of the Michigan Department of Public Health and the Detroit Police Department. The monthly homicide series (1969 to 1978) are from computer tapes of the death certificate records of the Michigan Department of Health. The classification is based on the Eighth Revision International Classification of Diseases (E960-969 are homicides, E965 are gun homicides). The first monthly robbery series (armed and unarmed 1967 to 1979) are from the *Computerized Monthly Report* of the Detroit Police Department Records Section. The second monthly robbery series (gun and nongun 1975 to 1979) are compiled from computer tapes provided by the Information Systems Bureau of the Detroit Police Department. We excluded the unknown weapon category and grouped all weapons other than firearms into the nongun category. The monthly assault series (1967 to 1979) are also from the computer tapes provided by the Information Systems Bureau.

that the Gun Law had a preventive effect on violent crime. First, the decline in homicide and robbery which is apparent in Figures 2, 3, and 4 began several months before the law went into effect. The peak month for violent crime in 1976 was July, the month of the widely publicized youth gang incidents. The general trend down in offense levels began five months before the law went into effect and four months before the publicity campaign.[25]

Had the publicity campaign begun earlier, one might speculate that the effects of the law appeared prior to the date of implementation because potential offenders were confused or uncertain as to when the law went into effect. One might even speculate that there would have been an effect at about the time the law passed the legislature in February of 1976. However, there seems to be no logical reason to expect that the effects of the Gun Law would have begun in the summer of 1976.

Second, only for homicides was the decline significantly different for offenses committed with a gun. Robberies with a gun fell dramatically in 1977, but so did unarmed robberies. The best comparison for robberies is probably Figure 4, where the monthly weapon-specific offenses are plotted for the years 1975–1979. The patterns are the same in all of the figures. Both types of robbery follow the same general trend. Finally, as is evident from Figure 5, there is no apparent change in the number of assaults.

Visually inspecting time-series data, while interesting, can be misleading because systematic features of the data may be confounded with the effects of the intervention. To avoid these problems, we use statistical models based on the work of Box and Jenkins (1976; Box and Tiao, 1975). In general terms, the procedure starts with the development of a *noise model* to

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Nongun assaults include assaults with a weapon other than a gun, aggravated assaults without a weapon, and simple assaults. Other combinations (including the exclusion of simple assaults) produce the same results as those reported here. Three missing observations have been replaced with forecasted values. The monthly stranger and affinal homicide series (1969 to 1979) are based on the records of the Homicide Section of the Detroit Police Department. Stranger homicides are those where there was no known acquaintance between victim and offender. Affinal homicides are those where the victim and offender were close friends or relatives. The acquaintance category was excluded to provide contrast.

[25] The bill establishing the Gun Law was signed into law on February 11, 1976. See *Detroit News* (February 12, 1976: A3). But no article about the law appeared in the Detroit newspapers between February 12, 1976, and December 1976, when the publicity campaign was announced. See "Ad Drive To Spell Out Jail-For-Gunman Law" (*Detroit News*, December 1, 1976: A8).

Figure 2. Detroit Homicides (1969-1978)

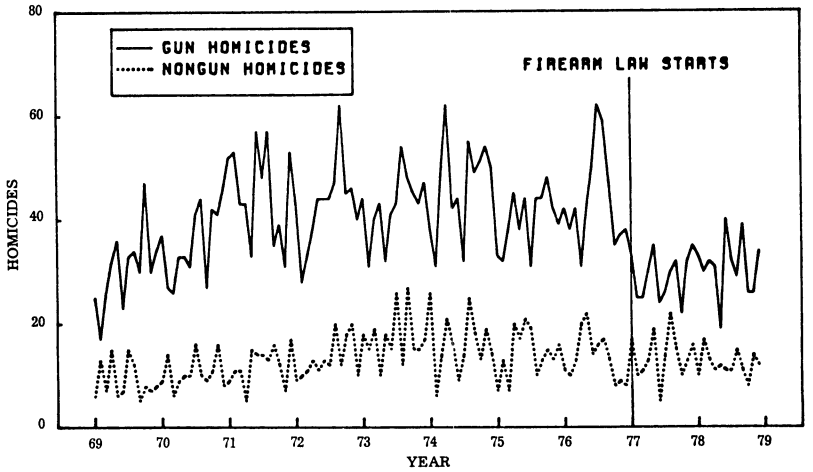
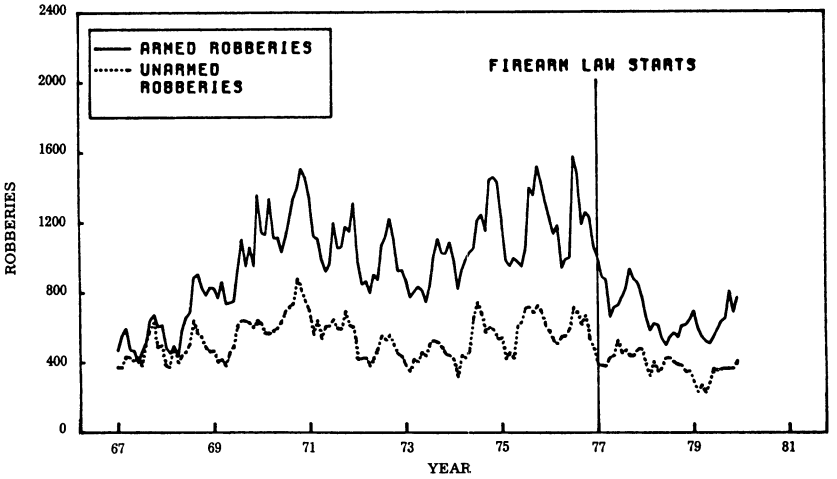


Figure 3. Detroit Robberies (1967-1979)



https://doi.org/10.2307/3053349 Published online by Cambridge University Press

Figure 4. Detroit Armed Robberies (1975-1979)

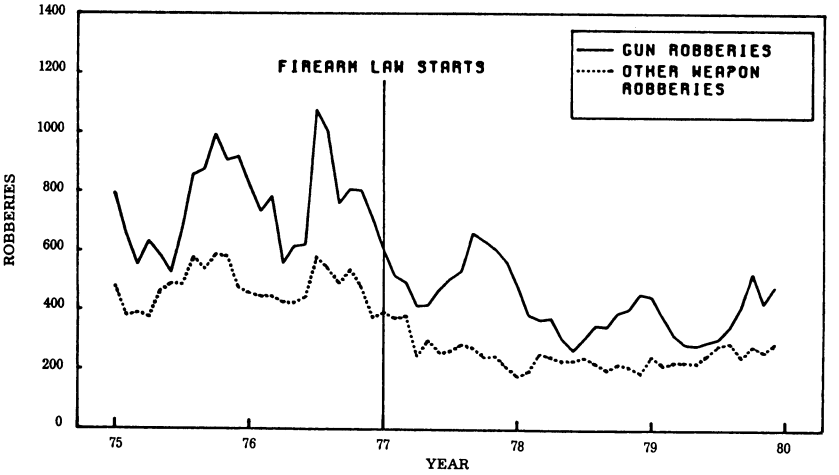
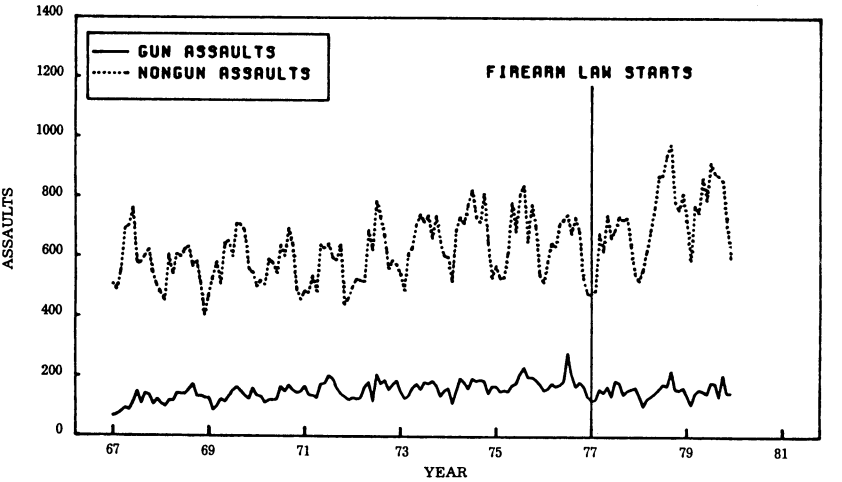


Figure 5. Detroit Assaults (1967-1979)



account for trend or drift, seasonality, and autocorrelation in the time-series. Having specified an appropriate noise model to control the effects of these series characteristics, an *intervention model* is added to represent the effect of the law. If there is a change in the series at the point of intervention, the intervention model will have statistically significant effects. For each series,[26] we considered three intervention models: an abrupt temporary change model, a gradual permanent change model, and an abrupt permanent change model. While more complex models are possible, these three seem reasonable and do not require elaborate assumptions about impact patterns (McCleary *et al.*, 1980). Taking advantage of arithmetic relationships between the models, we were led to the abrupt permanent change model as the most appropriate for each of the series.

Specific information on the time-series analysis is presented in the appendix. The conclusions derived from that analysis are generally consistent with what was suggested by Figures 2-5. Homicide with a gun is the only offense which experienced a statistically significant decline after the Gun Law went into effect. Gun homicides went down abruptly at a rate of almost eleven per month, while none of the other violent offenses experienced a statistically significant shift in level which could be attributed to the Gun Law.[27]

By comparing gun and nongun offenses, we should be able to detect any pattern of weapon substitution, but we can also formulate a specific test of the substitution hypothesis by calculating the proportion of offenses that were committed with a gun.[28] If there were weapon substitution (e.g., a knife for a gun), it would be reflected in a lower proportion of offenses committed with a gun. The results of this analysis (see appendix for details) are quite consistent with the findings already presented. The proportion of homicides committed

[26] No statistical analysis was possible for the monthly weapon-specific robbery series (Figure 4) because there were too few observations prior to the intervention.

[27] The conclusion that there was no significant intervention effect for robberies may seem to contradict the common-sense evidence in Figure 3, which shows very clearly that the level of robbery drifts down after mid-1976. This, of course, is the reason for using the statistical model. A common-sense examination of a few data points can be misleading. A careful examination of Figure 3 shows that the level of robbery drifted up in the early part of the study period and then down near the end. The technique makes use of this information to estimate an error term which represents random variability in the series. When this information is taken into account, the appropriate conclusion is not that there was no change in the level of the series after mid-1976, but rather that the change that did occur is consistent with what would be expected if the null hypothesis (no intervention effect) were true.

[28] This formulation was first suggested to us by Philip J. Cook. His critical comments on an earlier version of our research are gratefully acknowledged.

with a gun went down in the post intervention period, but there was no significant change in the proportion of assaults with a gun or the proportion of robberies that were armed.

The decline in gun homicides is particularly intriguing because of the relatively constant rate of gun assaults. If deterrence were responsible for the reduction in homicides, an effect should have been revealed in the assault series as well, for if deterrence works to reduce gun homicides, it should work by reducing the number of instances in which people pull guns on each other. In order to further probe the possibility that the Gun Law deterred gun homicides, we took advantage of some information about who killed whom.

If the Gun Law were responsible for the change in gun homicides, it is reasonable to expect that there would be differential effects depending on the type of homicide. The greatest effect should occur among premeditated and goal-oriented offenses such as felony-related homicides and those committed by strangers. Little if any effect should be associated with murders by friends and family members or homicides that grow out of lovers' quarrels and brawls, since these typically result from spontaneous eruptions of violence.[29]

To test this hypothesis, we divided Detroit homicides into three categories based on the victim-offender relationship: (1) strangers; (2) acquaintances; and (3) affines (close friends or relatives). We then constructed an eleven-year monthly time-series for stranger homicides and affinal homicides which could be analyzed separately. The results of the analysis (see appendix for details) show very little difference between the two series and therefore no evidence that the decline should be attributed to the Gun Law. Both declined dramatically with a similar pattern in the post intervention period.[30]

When all of the evidence is considered, it appears the Gun Law did not have a discernible effect on the level or the pattern of violent crime in Detroit. Early in our work, we thought the decline in homicides might reflect a change in gun assaults,

[29] The hypothesis that deterrence effects vary with the victim-offender relationship is often mentioned in the literature (Andenaes, 1974: 20, 48; Zimring and Hawkins, 1973: 106, 129; Ehrlich, 1974: 80), but it has been subject to only a few empirical tests. See, for example, Parker and Smith (1979).

[30] Another analysis, not shown here, tests a hypothesis derived from Philip Cook's (1981) discussion of a "vulnerability pattern" in robbery. Since guns are more valuable against protected and guarded targets such as commercial establishments, the effect of the Gun Law should be less on commercial robberies than on robberies of noncommercial targets. An analysis of commercial and noncommercial robberies in Detroit between January 1975 and December 1980 indicated that the gun and nongun series were very similar to each other and, while there were significantly more guns used in commercial than in noncommercial robberies, the trends over time were quite similar in both types of robberies.

which could be linked to the change in sentences for assaults in Recorders Court (Loftin and McDowall, 1980). However, now that we have examined all of the available data, especially the assault series, this interpretation no longer fits the facts. Since there was no significant change in gun assaults, which might explain the decline in gun homicides, we believe that the decline in gun homicides occurred because of extraneous factors which were not a direct consequence of the Gun Law.

IV. CONCLUSIONS

Why did the Gun Law not produce the expected reduction in gun crime in Detroit? Since this type of policy is popular and likely to be tried in other jurisdictions, it is important to consider why the policy failed in Detroit. We would like to know whether the causes are unique to Detroit or whether they could be expected to operate in other places as well. Without studying other cities, we cannot provide definitive answers, but we will discuss the available evidence.

Two general explanations have been advanced for the failure of the Gun Law to reduce violent crime. The first is that the judiciary undermined the law by finding ways to circumvent its stringent sentences (Cahalan, 1981: 7; *Detroit News*, March 23, 1981). The second is that offenders were not responsive to a mandatory two-year increment in sanctions for offenses which already carry maximum sentences much greater than two years. Although we cannot rule out the first explanation, it seems less likely than the second. At the time that the law went into effect, there were strong indications that the price of committing a felony with a gun would be increased. It was a credible message. The publicity campaign, which involved bumper stickers, billboards, and TV messages, was designed to put potential gun offenders on notice that they were subject to a two-year mandatory sentence. Furthermore, the Wayne County Prosecutor was very aggressive in charging the law and resisting attempts to circumvent it. Our interviews indicate that, while there was some "courthouse cynicism" about the impact of the law, many defendants were apprehensive about the effect of the law on sentences. In short, a strong message was transmitted to the community. It is likely, therefore, that offenders, sensitive to the marginal costs of two years' additional imprisonment, would have been deterred by the new threat.

In the months after the law was implemented, it is possible that the word got out that the "going rate" had not changed very much, but if this were the case, the pattern would have

been that of an abrupt temporary intervention: an initial preventive effect of the law followed by a drift back to the original level as knowledge of the behavior of the court diffused. No such pattern is evident in the data. Thus, to attribute the failure of the law to the behavior of the judiciary, one must assume that offenders were indifferent to the threat conveyed by the publicity campaign but would have responded had the actual sentencing pattern turned out to be as advertised. While it is possible that offenders react in this way, it seems unlikely.

The second possibility, that offenders are not responsive to a two-year mandatory add-on to existing sentences, seems more likely. The legal and behavioral scope of this type of policy is rather narrow. Legally, it applies only to felonies committed while in possession of a gun. Since the lesser included offense of carrying a concealed weapon is explicitly excepted, the law cannot be used to enhance sentences for persons who illegally carry guns.[31] Behaviorally, the scope of the law is also narrow. It can only influence those who do not place much value on having a gun while committing a felony. Since Detroit is a city with a well-earned reputation for gun violence, it is likely that most calculating offenders consider the possibility that they may face armed resistance and therefore place great value on a gun. It must be relatively easy to discount the possibility of a two-year mandatory sentence when the alternative may be facing a victim who is packing a gun and who has a significant incentive to use it. Finally, the law does nothing to increase the certainty of punishment. Indeed, it may serve to reduce it for some crimes. Most research on deterrence suggests that marginal increases in severity have relatively little impact on the offense rates for most crimes, and where severity effects are found, they tend to be dwarfed by effects attributable to the differential certainty of punishment.

Another possibility, not inconsistent with the others, is that not enough time has passed in order for the effect to be discernible. If the effect were quite small, which would be in line with what we know about the change in sanctions, more time would be necessary for our statistical models to show an impact. Though the length of our post intervention series is sufficient to detect even relatively small changes, nevertheless

[31] There is some indication based on a study of Massachusetts' Bartley-Fox law, which requires a one-year mandatory sentence for the illegal carrying of a gun, that a law more general in scope can reduce some types of violent crime (Pierce and Bowers, 1981).

there may have been changes which were so small that our models missed them.

Mandatory sentencing or sentence-enhancement for crimes committed with a gun are politically popular because they offer an apparent means of controlling gun violence without apparent cost to law-abiding gun owners. In principle, the costs are borne completely by criminals and by the criminal justice system in the form of longer sentences, more litigation, etc. The Detroit experience indicates, however, that the benefits may be more apparent than real. If the policy does not reduce gun violence, and at the same time diverts attention and resources from alternative policies, its costs are clearly greater than its benefits.

Perhaps the problem with the Michigan effort was that the sentence enhancement was not great enough or that the courts were not sufficiently constrained. It is always possible to devise new schemes to plug holes and prevent courts from moderating sentences. However, the Detroit experience suggests that this possibility may exist only in the realm of theory. This is a tough law. There were no obvious loopholes in its formulation, and the Prosecutor's policy of enforcing it was not just a "grandstand" act. The policy was enforced and cases were vigorously prosecuted.[32] Any scheme likely to produce a greater increment in average sanctions would have to be very different from the Michigan Law.

If the law had reduced gun offenses, there would be cause for celebration. We would have had a low-cost, seemingly fair policy which would distribute sanctions consistently, prevent violence, and save lives. Unfortunately, the evidence suggests a different reality. We do not have a criminological penicillin. Legislators contemplating similar policies might find laetrile the appropriate analogy.

APPENDIX
DESCRIPTION OF THE TIME-SERIES ANALYSIS

Gun Homicides

We identified and estimated an ARIMA$(1,0,0)(1,0,0)_{12}$ noise model for the gun homicide series. Parameter estimates and diagnostic statistics for this model are presented in the first panel of Table 5. The results of the intervention analysis, presented in the second panel of

[32] Of course, as we saw earlier, the efforts to preserve the going rate, coupled with the problems engendered in "equity cases," led the court to adopt a number of techniques which served to partially undermine these prosecutorial efforts.

Table 5, indicate that gun homicides declined by almost eleven per month following the intervention.

Table 5. Gun Homicides

Noise Model	Intervention Model
ARIMA(1,0,0)(1,0,0)$_{12}$	Y_t^* $= a_t + \omega_0(I_t)$
a_t $= (1 - \phi_1 B)(1 - \phi_{12}B^{12})Y_t - \theta_0$	I_t = 0 for observations 1-96
$\hat{\theta}_0$ = 38.23 95% c.i. = 34.30 to 42.16	= 1 for observations 97-120
$\hat{\phi}_1$ = .5002 95% c.i. = .3410 to .6594	
$\hat{\phi}_{12}$ = .2755 95% c.i. = .0903 to .4606	$\hat{\omega}_0$ = −10.857 95% c.i. = −17.107 to −4.607
Residual mean square = 64.443 w/df = 118	Residual mean square = 59.902 w/df = 117
Q = 20.452 w/df = 22 p > .50	Q = 17.39 w/df = 22 p > .50

Nongun Homicides

Perhaps because of its relatively low level throughout the period, a floor effect constrained the variance of the nongun homicide series. Although logarithmic transformations are known to help in situations such as this (McCleary and Musheno, 1981), a transformation did not fully remove the problem here. Therefore, in addition to transforming the series, we also treated six extreme observations as missing and forecast replacements for them. After replacing these observations, we identified and estimated an ARIMA(0,0,0) noise model (see Table 6, first panel). In contrast to the gun homicides, the intervention component for the nongun homicides was statistically insignificant (Table 6, second panel), and we conclude that there was no change in nongun homicides in the post intervention period.

Table 6. Nongun Homicides
(Series transformed by natural logarithm)

Noise Model	Intervention Model
ARIMA(0,0,0)	Y_t^* $= a_t + \omega_0(I_t)$
a_t $= Y_t - \theta_0$	I_t = 0 for observations 2-96
$\hat{\theta}_0$ = 2.5181 95% c.i. = 2.4580 to 2.5783	= 1 for observations 97-120
	$\hat{\omega}_0$ = −.0008 95% c.i. = −.1566 to −.1550
Residual mean square = .1076 w/df = 118	Residual mean square = .1086 w/df = 117
Q = 27.86 w/df = 24 p > .25	Q = 27.86 w/df = 24 p > .25

Note: Observation 1 was dropped, and observations 5, 9, 55, 57 and 61 were forecast in the noise model.

Armed Robberies

In order to produce a constant variance throughout the armed robbery series, we first logarithmically transformed it. After transformation, we identified and estimated an ARIMA(0,1,0)$(3,0,0)_6$ noise model (Table 7, panel one). Our estimate of the intervention parameter was statistically insignificant (Table 7, panel two), indicating no change in the series following the introduction of the Gun Law. Although it appears from Figure 3 that robberies went down in the post intervention period, our analysis indicates that all of this apparent effect can be attributed to the factors in the noise model. When they are removed, there is no change in the level of the series.

Table 7. Armed Robberies
(Series transformed by natural logarithm)

Noise Model	Intervention Model
ARIMA(0,1,0)(3,0,0)$_6$	$Y_t^* = a_t + \omega_0(I_t)$
$a_t = (1 - B)(1 - \phi_6 B^6 - \phi_{12} B^{12} - \phi_{18} B^{18}) Y_t$	I_t = 0 for observations 1-120
$\hat{\phi}_6$ = −.2119 95% c.i. = −.3781 to −.0457	= 1 for observations 121-156
$\hat{\phi}_{12}$ = .2041 95% c.i. = .0385 to .3696	
$\hat{\phi}_{18}$ = −.2414 95% c.i. = −.4024 to −.0804	$\hat{\omega}_0$ = .0676 95% c.i. = −.1540 to −.2893
Residual mean square = .0130 w/df = 152	Residual mean square = .01304 w/df = 151
Q = 28.48 w/df = 22 p > .10	Q = 28.48 w/df = 22 p > .10

Note: The noise model was initially estimated with a trend parameter but the trend was not statistically significant and was dropped.

Unarmed Robberies

The unarmed robbery series also required an initial logarithmic transformation. After transformation, we identified and estimated an ARIMA(1,1,0)(1,0,0)$_{12}$ noise model (first panel, Table 8). Again our estimate of the intervention component was not statistically significant (Table 8, second panel), and we conclude that there was no change in unarmed robberies following the intervention.

Table 8. Unarmed Robberies
(Series transformed by natural logarithm)

Noise Model	Intervention Model
ARIMA(1,1,0)(1,0,0)$_{12}$	$Y_t^* = a_t + \omega_0(I_t)$
$a_t = (1 - B)(1 - \phi_1 B)(1 - \phi_{12} B^{12}) Y_t$	I_t = 0 for observations 1-120
$\hat{\phi}_1$ = −.2983 95% c.i. = −.4548 to −.1418	= 1 for observations 121-156
$\hat{\phi}_{12}$ = .6289 95% c.i. = .4975 to .7604	$\hat{\omega}_0$ = −.0700 95% c.i. = −.2535 to .1134
Residual mean square = .0127 w/df = 153	Residual mean square = .0127 w/df = 152
Q = 28.75 w/df = 23 p > .10	Q = 29.97 w/df = 23 p > .10

Note: The noise model was initially estimated with a trend parameter but the trend was not statistically significant and was dropped.

Gun Assaults

We identified and estimated an ARIMA(0,1,1)(0,1,1)$_{12}$ noise model for the gun assault series (Table 9, first panel). The change parameter of the intervention model was statistically insignificant (second panel, Table 9), leading us to conclude that gun assaults were unaffected in the post intervention period.

Table 9. Gun Assaults

Noise Model	Intervention Model
ARIMA(0,1,1)(0,1,1)$_{12}$	$Y_t^* = a_t + \omega_0(I_t)$
$(1 - \theta_1 B)(1 - \theta_{12} B^{12}) a_t = (1 - B)(1 - B^{12}) Y_t$	I_t = 0 for observations 1-120
$\hat{\theta}_1$ = .7583 95% c.i. = .6564 to .8602	= 1 for observations 121-156
$\hat{\theta}_{12}$ = .8835 95% c.i. = .8380 to .9290	$\hat{\omega}_0$ = −.3335 95% c.i. = − 1.1788 to .5118
Residual mean square = 408.145 w/df = 141	Residual mean square = 410.065 w/df = 140
Q = 18.40 w/df = 23 p > .50	Q = 19.40 w/df = 23 p > .50

Note: The noise model was initially estimated with a trend parameter but the trend was not statistically significant and was dropped.

Nongun Assaults

We also identified and estimated an ARIMA(0,1,1)(0,1,1)$_{12}$ noise model for the nongun assault series (Table 10, first panel). As was the case for the gun assaults, the intervention parameter estimate for the nongun assaults was statistically insignificant (Table 10, second panel), indicating no change in the series following the introduction of the Gun Law.

Table 10. Nongun Assaults

Noise Model	Intervention Model
ARIMA(0,1,1)(0,1,1)$_{12}$	Y_t^* $= a_t + \omega_0(I_t)$
$(1 - \theta_1 B)(1 - \theta_{12} B^{12}) a_t = (1 - B)(1 - B^{12}) Y_t$	I_t = 0 for observations 1-120
$\hat{\theta}_1$ = .7229 95% c.i. = .6116 to .8342	= 1 for observations 121-156
$\hat{\theta}_{12}$ = .8590 95% c.i. = .8075 to .9105	$\hat{\omega}_0$ = .0244 95% c.i. = −.0600 to .1088
Residual mean square = 3951.01 w/df = 141	Residual mean square = 3971.51 w/df = 140
Q =31.10 w/df = 23 p > .10	Q = 31.10 w/df = 23 p > .10

Note: The noise model was initially estimated with a trend parameter but the trend was not statistically significant and was dropped.

Homicide Proportions (Gun Homicides/Total Homicides)

We had a choice in calculating the nongun homicide component of total homicides for the denominator of the homicide proportion series: either to use the original series or to replace the six extreme observations as we did in our analysis of the nongun homicides (see above). We computed and analyzed the proportions both ways, with nearly identical results, and the ARIMA(0,0,0) noise model we present (Table 11, panel one) is for the original series. The intervention analysis (second panel, Table 11) shows that the proportion of all homicides which were gun homicides was about 5 percent less following the intervention, and this decline was statistically significant.

Table 11. Homicide Proportions

Noise Model	Intervention Model
ARIMA(0,0,0)	Y_t^* $= a_t + \omega_0(I_t)$
$a_t = Y_t - \theta_0$	I_t = 0 for observations 1-96
$\hat{\theta}_0$ = .7458 95% c.i. = .7332 to .7583	= 1 for observations 97-120
	$\hat{\omega}_0$ = −.0503 95% c.i. = −.0857 to −.0262
Residual mean square = .0047	Residual mean square = .0043
Q =28.70 w/df = 24 p > .25	Q =20.33 w/df = 24 p > .50

Robbery Proportions (Armed Robberies/Total Robberies)

Based on our identification, we estimated an ARIMA(2,0,0)(1,0,0)$_{12}$ noise model for the robbery proportions (Table 12, first panel). The change parameter in the intervention model was not statistically significant (Table 12, second panel), and we conclude that there was no change in the series after the Gun Law was introduced.

Table 12. Robbery Proportions

Noise Model	Intervention Model
ARIMA(2,0,0)(1,0,0)$_{12}$	Y_t^* $= a_t + \omega_0(I_t)$
$a_t = (1 - \phi_1 B^2)(1 - \phi_{12} B^{12}) Y_t - \theta_0$	I_t = 0 for observations 1-120
$\hat{\theta}_0$ = .6438 95% c.i. = .6082 to .6793	= 1 for observations 121-156
$\hat{\phi}_1$ = .5953 95% c.i. = .4350 to .7556	$\hat{\omega}_0$ = .0195 95% c.i. = −.0323 to .0543
$\hat{\phi}_2$ = .2104 95% c.i. = .0517 to .3690	
$\hat{\phi}_{12}$ = .3588 95% c.i. = .1959 to .5216	
Residual mean square = .00078	Residual mean square = .00079
Q = 26.85 w/df = 21 p > .10	Q = 26.44 w/df = 21 p > .10

Assault Proportions (Gun Assaults/Total Assaults)

We identified and estimated an ARIMA(1,0,0)(1,0,0)$_{12}$ noise model for the assault proportion series. The parameter estimates and diagnostic statistics for this model are presented in the first panel of Table 13. The change parameter in the intervention model (Table 13, second panel) was statistically insignificant, leading us to conclude that the series was unaffected following the intervention.

Table 13. Assault Proportions

Noise Model	Intervention Model
ARIMA(1,0,0)(1,0,0)$_{12}$	Y_t^* $=a_t + \omega_0(I_t)$
a_t $= (1 - \phi_1 B)(1 - \phi_{12}B^{12})Y_t - \theta_0$	I_t = 0 for observations 1-120
$\hat{\theta}_0$ = .1919 95% c.i. = .1785 to .2052	= 1 for observations 121-156
$\hat{\phi}_1$ = .5877 95% c.i. = .4591 to .7164	
$\hat{\phi}_{12}$ = .3674 95% c.i. = .2128 to .5219	$\hat{\omega}_0$ = −.0205 95% c.i. = −.0425 to .0015
Residual mean square = .00050	Residual mean square = .00049
Q = 29.17 w/df = 22 p > .10	Q = 28.65 w/df = 22 p > .10

Stranger Homicides

We identified and estimated an ARIMA(1,0,0) noise model for the stranger homicide series. Parameter estimates and diagnostic statistics for this model are presented in the first panel of Table 14. The change parameter in the intervention model (Table 14, second panel) is statistically significant and indicates that stranger homicides declined by over four a month following the intervention.

Table 14. Stranger Homicides

Noise Model	Intervention Model
ARIMA(1,0,0)	Y_t^* $=a_t + \omega_0(I_t)$
a_t $= (1 - \phi_1 B)Y_t - \theta_0$	I_t = 0 for observations 1-96
$\hat{\theta}_0$ = 10.7940 95% c.i. = 5.6421 to 11.9470	= 1 for observations 97-132
$\hat{\phi}_1$ = .3558 95% c.i. = .1906 to .5209	$\hat{\omega}_0$ = −4.4881 95% c.i. = −6.4013 to −2.5748
Residual mean square = 18.2920	Residual mean square = 16.2840
Q = 20.92 w/df = 23 p > .50	Q = 9.7393 w/df = 23 p > .90

Affinal Homicides

The noise model we initially developed for the affinal homicide series no longer fit the data adequately after the intervention component was added, and we thus identified and estimated an ARIMA(0,0,0) noise model based only on the 96 pre intervention observations. Parameter estimates and diagnostic statistics for this model are presented in the first panel of Table 15. The intervention model, presented in the second panel of Table 15, shows a statistically significant decline of over four homicides a month following the introduction of the Gun Law.

Table 15. Affinal Homicides

Noise Model	Intervention Model
ARIMA(0,0,0)	Y_t^* $=a_t + \omega_0(I_t)$
a_t $= Y_t - \theta_0$	I_t = 0 for observations 1-96
$\hat{\theta}_0$ = 13.3650 95% c.i. = 12.4160 to 14.3130	= 1 for observations 97-132
	$\hat{\omega}_0$ = −4.2813 95% c.i. = −5.9420 to −2.6206
Residual mean square = 21.5180	Residual mean square = 18.0540
Q = 15.78 w/df = 24 p > .75	Q = 18.0940 w/df = 24 p > .75

Note: The noise model for this series was altered with the introduction of the intervention. The model presented here is thus based only on the 96 pre intervention months.

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