


Spring 1984

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### Recommended Citation

Colin Loftin, David McDowall, The Deterrent Effects of the FLorida Felony Firearm Law, 75 J. Crim. L. & Criminology 250 (1984)

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# THE DETERRENT EFFECTS OF THE FLORIDA FELONY FIREARM LAW\*

COLIN LOFTIN\*\*  
AND  
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## I. INTRODUCTION

In the area of firearms policy, no measure has received more legislative attention than mandatory minimum sentences for felonies committed with a gun. At the latest count, fifteen states had adopted such a law, and a dozen or so more were considering it.<sup>1</sup> On the national level, the Attorney General's Task Force on Violent Crime has recommended mandatory minimum sentencing for federal crimes committed with firearms.<sup>2</sup> There are many reasons for the popularity of the policy, but one of its more important attractions is the expectation that mandatory minimum sentences will reduce gun-related crime without imposing additional constraints on the behavior of "law-abiding" citizens who wish to own guns.

In previous studies we investigated the effects of mandatory sentences for gun felonies on violent crime in Detroit, Michigan, con-

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\* The research reported here was supported in part by grants 79-NI-AX-0094 and 81-IJ-CX-0060 from the National Institute of Justice. Points of view or opinions stated are those of the authors and do not necessarily reflect the official position or policies of the United States Department of Justice. We would like to thank Matthew R. DeZee for advice on criminal statistics in Florida and for assistance in obtaining data. Bill Bowers and Glenn Pierce made a substantial contribution to this project by providing the data and valuable criticism. Brian Wiersema and Stephen Schmaltz also assisted with the analysis.

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<sup>1</sup> See R. MORELLI, C. EDELMAN & R. WILLOUGHBY, PENNSYLVANIA COMM'N ON CRIME AND DELINQUENCY, A SURVEY OF MANDATORY SENTENCING IN THE U.S., Table 1 at 7 (1981).

<sup>2</sup> U.S. DEP'T OF JUSTICE, ATTORNEY GENERAL'S TASK FORCE ON VIOLENT CRIME: FINAL REPORT 29 (Recommendation 17) (1981). Recommendation 5 in Phase I and Recommendations 17 through 22 in Phase II address firearm policy. See *id.*

cluding that there were no discernible changes in the level of crime which could be attributed to Michigan's gun law.<sup>3</sup> In this Article we extend the analysis to Florida's version of the law by examining its effect on violent crime in three cities in that state.

## II. BACKGROUND

The Florida Felony Firearm Law<sup>4</sup> went into effect on October 1, 1975.<sup>5</sup> It mandates a "flat" three-year sentence for possessing a firearm while committing or attempting to commit any of eleven specified felonies. Suspended, deferred, and withheld sentences are explicitly prohibited, as is parole, until the minimum three years have been served.<sup>6</sup>

The Florida statute enjoyed great popular support and was the subject of an extensive media campaign before its passage. Supporters of the law felt that its "ironclad" guarantee of a three-year sentence would deter many offenders.<sup>7</sup> In addition, a prominent theme in the press re-

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<sup>3</sup> Loftin & McDowall, *'One With a Gun Gets You Two': Mandatory Sentencing and Firearms Violence in Detroit*, 455 ANNALS 150 (1981); Loftin, Heumann & McDowall, *Mandatory Sentencing and Firearms Violence in Detroit: Evaluating an Alternative to Gun Control*, 17 LAW & SOC'Y REV. 301 (1983).

<sup>4</sup> FLA. STAT. ANN. § 775.087(2) (West 1976).

<sup>5</sup> Some of the elements of the law were in effect before this time, but according to Ku, *Florida: The 1975 Firearm Law*, in AMERICAN PRISONS AND JAILS Vol. IV 39-52 (J. Mullen ed. 1980), the Florida Department of Correction designated October 1975 as the effective date. This date is cited in the news coverage as the effective date for the law and it corresponds to the period of the publicity campaign about the law. See Harrison, *14-Month-Old '3-To-Life' Law Held Successful*, Orlando Sentinel Star, Dec. 12, 1976, at A18, col. 1.

<sup>6</sup> FLA. STAT. ANN. § 775.087(2) (West 1976) provides:

Any person who is convicted of any murder, sexual battery, robbery, burglary, arson, aggravated assault, aggravated battery, kidnapping, escape, breaking and entering with intent to commit a felony, or aircraft piracy, or any attempt to commit the aforementioned crimes, and who had in his possession a "firearm," as defined in subsection 790.001(6), or "destructive device," as defined in subsection 790.001(4), shall be sentenced to a minimum term of imprisonment of 3 years. Notwithstanding the provisions of § 948.01, adjudication of guilt or imposition of sentence shall not be suspended, deferred, or withheld, nor shall the defendant be eligible for parole, prior to serving such minimum sentence.

<sup>7</sup> Harrison, *14-Month-Old '3-To-Life' Law Held Successful*, Orlando Sentinel Star, Dec. 12, 1976, at A18, col. 1. To make sure that the threat got across, they organized a massive publicity campaign supported by a grant from the Law Enforcement Assistance Administration and orchestrated by a professional public relations firm. The campaign included radio and television spots, 200 billboards, press releases, public speaking engagements, and posters placed on the doors of high-risk establishments. See Ku, *supra* note 5; Dillin, *Mandatory Jail for Gun Toters: How Effective?*, Christian Science Monitor, Jan. 2, 1976, at 1, col. 1.

Senator James Glisson, author of the statute, felt strongly that the publicity campaign was the key to the success of the law. He argued that the best way to make the law effective would be "to get the word to people inclined to do this kind of thing, that if they use a gun when they break the law, punishment will be swift and certain . . . and they mustn't have any doubts about it." Harrison, *supra* note 7, at A18, col. 1. Before the law went into effect and before the information campaign, a survey of Florida residents between the ages of 16 and 25 found that only 19% had heard of the law. See Barbour & Monroe, Executive Sum-

ports about the law was indignation over well-intended but naive policies of the past that stressed rehabilitation rather than deterrence.<sup>8</sup> Although no systematic evaluation of the deterrent effect of the law has been done, it is widely believed to have produced dramatic reductions in such crimes as armed robbery and assault.<sup>9</sup> In Michigan, where a similar statute was being implemented, news of the apparent success of the Florida law was widely circulated in the media.<sup>10</sup>

Given the apparent failure of the Michigan law to reduce violent gun crime in Detroit,<sup>11</sup> the success of the Florida law would be especially interesting because it might suggest conditions and policies that enhance or diminish the success of mandatory sentencing. Therefore, we conducted an evaluation, similar in most respects to the earlier study in Detroit, of the preventive effect of the Florida gun law.

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mary: Attitudes and Opinions of Persons 15 to 25 Regarding Hand Gun Control (Nov. 1975) (Barbour and Monroe Marketing Research, Orlando, Florida) (unpublished report). We have been unable to find a comparable study after the information campaign. Ku, *supra* note 5, at 41, mentions a figure of 49%, but does not describe the source or the sample. Burr's survey of convicted felons in five Florida correctional facilities found that 79% were aware of the law. See E. BURR, FLORIDA BUREAU OF CRIMINAL JUSTICE PLANNING AND ASSISTANCE, FINAL REPORT: HANDGUN REGULATION Appendix 4-11 (1977).

<sup>8</sup> Glisson said, "Everybody was thinking in terms of 'How is this going to help in terms of rehabilitation?' and we said 'it's not!'. . . . It's intended as plain old hardcore punishment . . . retribution, which equals deterrent." Harrison, *supra* note 7, at A18, col. 1.

A similar theme was sounded by Jim Barrett, assistant to the attorney general in Florida: "Punishment in and of itself should not be discarded as a form of rehabilitation. A pat on the head simply hasn't worked in the past. Maybe we should try a kick in the ass." Neubacher, *Prison-for-Gunmen Law Will Get State Test Soon*, Detroit Free Press, Nov. 30, 1976, at A3.

<sup>9</sup> Loftin, Heumann & McDowall, *supra* note 3, at 304. Ku's brief assessment of the impact of the law on the Florida prison population parallels some of the effects of the Michigan law: "Admissions to prison have not increased . . . while the three year minimum term for those sentenced under the statute may be substantially longer than would have been the case had the firearms law not been enacted." Ku, *supra* note 5, at 49. The Michigan study found, among other things, that in Detroit's Recorders Court the gun law had little influence on the probability of incarceration among those convicted, but the length of sentence was longer for those that were sentenced to prison. See Loftin, Heumann & McDowall, *supra* note 3.

<sup>10</sup> Articles and editorials in Detroit's two major daily newspapers reported that in the first six months of 1976 (after the Florida law went into effect) armed robberies with firearms dropped by 39%. *New Gun Law Offers Hope in War on Crime*, Detroit News, Dec. 28, 1976, at B6; Neubacher *supra* note 8, at A3. But see Rothman, *Gun law cuts Fla. crime but still is criticized*, Detroit News, Jan. 2, 1977, at A3 (referring to a "30% cutback in armed robberies"). Such dramatic reports have not been limited to the news media; they are also found in scholarly discussions of firearms policy. Hardy and Kates, for example, cited the same 39% reduction in Florida firearms robberies that was mentioned in the Detroit newspapers. See Hardy & Kates, *Handgun Availability and the Social Harm of Robbery: Recent Data and Some Projections*, in RESTRICTING HANDGUNS: THE LIBERAL SKEPTICS SPEAK OUT 125 (D. Kates ed. 1979).

<sup>11</sup> See generally Loftin & McDowall, *supra* note 3; Loftin, Heumann & McDowall, *supra* note 3.

## III. THE EFFECT OF THE STATUTE ON VIOLENT CRIME

The focus of the analysis is the development of an estimate of the impact of the mandatory sentence on violent crimes such as robbery, assault, and homicide. The estimates are derived from an interrupted time-series design which we applied to three types of crimes in three different Florida cities. The cities (Miami, Tampa, and Jacksonville) were selected because they are the largest in Florida, together accounting for a third or more of the total number of violent crimes in the state,<sup>12</sup> and because they are geographically separated and demographically distinct, providing three diverse opportunities to replicate the analysis.

In each city, we gathered monthly data on the number of robberies, assaults, and homicides committed by individuals with guns and with other means.<sup>13</sup> Several commentators have pointed out that although interrupted time-series research designs are among the strongest of quasi-experiments, there are a number of possible threats to the validity of inferences drawn from them.<sup>14</sup> The most relevant of these threats for our analysis is *history*, the possibility that events occurring at about the same time as the intervention were actually responsible for an observed change.

In order to reduce historical threats, we used a control series for each analysis. Armed robberies were compared to unarmed robberies; gun assaults were compared to knife assaults; and gun homicides were compared to nongun homicides.<sup>15</sup> If an experimental series declined in the post-intervention period while its control series remained stable, we

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<sup>12</sup> FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES, 1968 67, 171 (1969). Note, however, that by 1979, the percentage had dropped to approximately 22 according to the FBI's figures. See FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES, 1979 49, 95-96 (1980).

<sup>13</sup> Homicide data for the period January 1968 through December 1978 were compiled from Supplementary Homicide Report tapes provided by the Uniform Crime Reporting Division of the Federal Bureau of Investigation (FBI). Robbery and assault data for the period January 1967 through December 1980 were compiled from Return A tapes also provided by the FBI Uniform Crime Reporting Division. The assault series includes only aggravated assaults. Simple assaults are excluded from the analysis.

<sup>14</sup> D. CAMPBELL & J. STANLEY, EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS FOR RESEARCH (1966); T. COOK & D. CAMPBELL, QUASI-EXPERIMENTATION: DESIGN AND ANALYSIS ISSUES FOR FIELD SETTINGS (1979).

<sup>15</sup> Initially, we attempted to use total nongun assaults, rather than knife assaults, as the assault control series. The covariance structure between successive observations of the nongun assault series, however, shifted over time, perhaps reflecting gradual change in reporting practices. A shifting covariance structure is a violation of the assumptions of ARIMA modeling, see *infra* note 17 and accompanying text, and made the total assaults series unanalyzable. We therefore substituted the knife assault series, which was not subject to this problem. Although some of the other series exhibit a slightly shifting covariance structure, in no case except total nongun assaults was it serious.

concluded that the law had an effect. If both the experimental and control series declined, we concluded that the introduction of the law was confounded with history. This method does not completely rule out alternative explanations of a decline in the experimental series, but it does make them less plausible.

Replication in different places under different conditions is also an important means of ruling out historical artifacts. Interrupted time-series designs are given high credibility in the physical sciences, not only because of experimental isolation, but also because the experiments are repeated in many places under different circumstances.<sup>16</sup> Comparisons between Detroit and the three Florida cities are therefore an important part of our evaluation.

A visual examination of the data showed little evidence of the dramatic decline in gun crime that has been attributed to the Florida law. In general, however, simple visual inspection of the crime time-series is not very enlightening, and based upon such an inspection it is impossible to say whether or not the law had its intended effect.

In order to obtain more rigorous estimates of the impact of the law, we analyzed the data as a set of interrupted time-series experiments.<sup>17</sup> This procedure begins with the development of an Autoregressive Integrated Moving Average (ARIMA) *noise model* to control for the effects of nonstationarity and autocorrelation in the data. Autocorrelation and nonstationarity represent the systematic forces driving the time-series through the pre- and post-intervention periods, and they must be accounted for by the noise model if incorrect inferences about the effects of the intervention are to be avoided.

Once an appropriate noise model has been specified, an *intervention model* is added to represent the effects of the gun law. A change in the level of the series following the introduction of the law would show up in the parameters of the intervention model. For each series, we considered three types of intervention models. These included an abrupt permanent change model, a gradual permanent change model, and an abrupt temporary change model.<sup>18</sup> Although more complex intervention models are possible, these three seemed reasonable, and did not require elaborate assumptions about patterns of impact. Taking advantage of arithmetic relationships between the models, we were led

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<sup>16</sup> See D. CAMPBELL & J. STANLEY, *supra* note 14, at 42.

<sup>17</sup> We used techniques developed by Box and Tiao. See generally Box & Tiao, *Intervention Analysis with Applications to Economic and Environmental Problems*, 70 J. AM. STATISTICAL A. 70 (1975); see also G. BOX & G. JENKINS, *TIME-SERIES ANALYSIS: FORECASTING AND CONTROL* (1976).

<sup>18</sup> See D. MCDOWALL, R. MCCLEARY, E. MEIDINGER & R. HAY, *INTERRUPTED TIME SERIES ANALYSIS* (1980) for further discussion of these models.

**TABLE 1**  
SUMMARY OF INTERVENTION ANALYSIS

SERIES	$\hat{\omega}$	95% Confidence Interval	
		LOWER LIMIT	UPPER LIMIT
<b>JACKSONVILLE</b>			
GUN HOMICIDE	-0.8577	-1.9277	0.2123
NONGUN HOMICIDE	0.2009	-0.5287	0.9307
ARMED ROBBERY	6.8822	-14.1070	27.8710
UNARMED ROBBERY	2.5030	-2.0682	7.0742
GUN ASSAULT	-5.6291	-21.1670	9.9086
KNIFE ASSAULT	-2.8453	-14.2730	8.5828
<b>TAMPA</b>			
GUN HOMICIDE	-1.2084*	-1.9589	-0.4580
NONGUN HOMICIDE	0.0329	-0.5007	0.5665
ARMED ROBBERY	-4.1475	-19.9490	11.6540
UNARMED ROBBERY	8.5516*	1.9562	15.1470
GUN ASSAULT	9.7495*	3.4815	16.0180
KNIFE ASSAULT	7.8959	-3.8217	19.6130
<b>MIAMI</b>			
GUN HOMICIDE	-0.3441	-1.3922	0.7040
NONGUN HOMICIDE	0.8032	-0.0652	1.6716
ARMED ROBBERY	-17.7520	-44.0070	8.5033
UNARMED ROBBERY	27.7420*	10.0880	45.3960
GUN ASSAULT	-7.5774	-22.1950	7.0399
KNIFE ASSAULT	-4.0525	-8.6878	5.8275

\*Change significant with 95% confidence.

to the abrupt permanent change model as the most appropriate for each of the series.

A summary of the results is presented in Table 1. Since the best intervention model was the same for all series, we simply list the estimate of the parameter ( $\hat{\omega}$ ) that represents the shift in the level of the series and its ninety-five percent confidence interval. The main conclusion to be derived from the analysis is that there is little evidence that the introduction of the Florida gun law was followed by a systematic decline in violent gun crimes in the cities that we examined. Of the eighteen series analyzed, only one, homicides with a gun in Tampa, showed a statistically significant decline after the implementation of the law. In three other cases there was a statistically significant *increase* in

the series in the post-intervention period. One of these was a gun series (gun assaults in Tampa) and two were control series (unarmed robberies in Tampa and Miami).

Given this pattern of results, is there any coherent theory that could be used to interpret the data as evidence that the statute reduced the level of violent crime? The strongest finding is the decline in gun homicides in Tampa. This type of decline in the gun, but not in the nongun, series is exactly what we would expect to find if the gun law had a deterrent effect. Two other features of the data, however, make us cautious in attributing the decline to the gun law. First, gun assaults, unlike homicides, did not decline in any of the three cities. Assuming that there are no reporting artifacts, it is unlikely that the gun law would produce a decline in gun homicides without producing a similar decline in gun assaults. There are, of course, complex theories that would reconcile the results.<sup>19</sup> Perhaps there were various measurement artifacts. Without additional information, such interpretations, though interesting, are speculative.

Second, the decline in gun homicides occurred in only one of the three cities. It would be risky to discount the negative findings in Jacksonville and Miami and attribute the decline in Tampa gun homicides to the gun law. It is interesting, however, that the gun homicide series was the only Detroit series that declined significantly after the Michigan law was implemented. Also, a careful examination of Table 1 shows that in Jacksonville and Miami, the signs of the intervention effects were negative, though statistically not significant. This is admittedly limited evidence that would not, by itself, sustain an argument for an intervention effect, but it may turn out to be an important finding in an accumulating body of experience with policy experiments on mandatory minimum sentencing.

The increase in unarmed robberies in Miami and Tampa suggests two slightly different hypotheses: first, that offenders in Miami and Tampa switched from armed to unarmed robbery, and second, that the unarmed robbery series provides a measure of what would have happened to the armed robberies were it not for the intervention effect of the statute. Thus, the increase in unarmed robbery could be evidence of a preventive effect of the law on armed robbery.

The first hypothesis, that offenders switched from armed to unarmed robberies, can be dismissed quickly because it is inconsistent with the data. Aside from the fact that the increase in unarmed robbery did

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<sup>19</sup> One theory is that the law influenced a subgroup of especially deadly gun assaults that reduced the gun homicides, but not the gross level of gun assaults.



not occur in Jacksonville, such a weapon switching hypothesis can be ruled out because there was no reduction in the number of armed robberies in Miami or Tampa. The rise in unarmed robberies cannot be attributed to a weapons-switching effect of the statute unless there was a compensating decline in unarmed robberies, and the analysis shows that this was not the case. It is, of course, true that weapon switching, *e.g.*, from gun to knife, could have occurred without detection in our analysis. This, however, is a different issue that could not explain the rise in unarmed robbery.<sup>20</sup>

The second hypothesis is that the difference in the behavior of the armed robbery series and the unarmed robbery series can be interpreted as a measure of the intervention effect. The increase in unarmed robberies while armed robberies remained at the same level in two of the three cities might imply that the statute prevented an increase in armed robberies that otherwise would have taken place.

The validity of this hypothesis depends on the assumption that the unarmed robbery series is an adequate control for the armed robberies. More precisely, the assumption is that factors that influence the two series, other than the statute, are not systematically related to the intervention, and that, except for the statute, armed and unarmed robberies are subject to similar exogenous forces across the study period. This is a strong assumption, given what we know about the character of armed and unarmed robbery. At this point, the prudent conclusion seems to be that the data are not inconsistent with the model in which the gun law prevented armed robberies from rising in Tampa and Miami. It should be treated as a hypothesis with tentative support in two of the three Florida cities.

The interpretation of the assault data is much simpler. Since the only significant change was an increase in gun assaults in Tampa, there is no reasonable theory that would imply that the gun law reduced violent assaults. Our interpretation is, therefore, that the law had no preventive effects for assaults. The rise in gun assaults in Tampa is probably unrelated to the gun law, but one possible interpretation is that the publicity associated with the law led to an increase in the reporting of gun assaults.<sup>21</sup> Since the increase occurred in only one of the three cities, it is probably unrelated to the law.

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<sup>20</sup> Since weapon-specific robbery data were not available until January 1975, it is not possible to do an appropriate statistical test of the hypothesis of weapon switching (gun to knife or other weapon) within the armed robbery series. A visual inspection of the weapon-specific robbery data after January 1975, however, does not indicate any increase in the robberies with weapons other than guns in the post-intervention period.

<sup>21</sup> Pierce and Bowers report that gun assaults without battery rose in Boston after a law requiring a mandatory one-year sentence for illegally carrying a firearm was implemented. While there is no direct evidence indicating a change in citizen reporting, they suggest that

## V. CONCLUSION

The analysis leads us to two conclusions. First, the enthusiastic initial reports announcing the sweeping success of the Florida law in reducing violent gun crimes such as robbery and assault were overly optimistic. The results of our analysis are complex, but they are clearly inconsistent with a model in which violent gun crimes uniformly declined. Indeed, in only one of the nine gun crime series examined was there a significant decline.

Second, although it is possible to construct a theory that would account for the pattern of results and still attribute some deterrent effect to the Florida law, the very complexity of the theory challenges its credibility. It lacks parsimony and attains consistency with the facts only at the expense of ad hoc exceptions. The theory requires that one attribute the decline in gun homicides in Tampa to the law, but discount the lack of an effect in Jacksonville and Miami; that one interpret the failure of armed robberies to decline in Tampa and Miami as evidence of a preventive effect because unarmed robberies increased, but to ignore the same finding in Jacksonville because unarmed robberies did not rise; and finally, that one put aside the failure of any of the gun assault series to decline or knife assault series to rise. Additional hypotheses about measurement artifacts could also be added to account for such dissident findings as the rise in gun assaults in Tampa. The appropriate conclusion is not that these hypotheses are either correct or incorrect, but that they are interesting speculations subject to further testing and examination.

Our current working hypothesis, and by far the simplest interpretation of the data, is that the Florida gun law did not have a measurable deterrent effect on violent crime. Not only does this interpretation provide a good fit to the data from both Detroit and Florida, but it is also quite plausible on purely logical grounds. Legally and behaviorally, the scope of this type of law is narrow. It applies only to individuals who actually commit a felony using a gun. A citizen who possesses a gun for protection and who does not anticipate using it in a crime is not affected. The target of the law is the offender who chooses whether or not to use a gun when committing a particular offense, or whether or not to carry it opportunistically. In this narrow context, the marginal effect of the threat of a three-year increment in a sentence would be small for two reasons. First, the underlying felony already carries the threat of a sentence that is much greater than three years; therefore, the increment in potential sentence length is relatively small. Second, for most offenders

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this is a likely explanation. See generally Pierce & Bowers, *The Bartley-Fox Gun Law's Short-Term Impact on Crime in Boston*, 455 ANNALS 120 (1981).

the threat of a three-year sentence is more than offset by the value of the gun both in committing the offense and in providing protection. The mandatory sentence would have its greatest deterrent effect on offenders who do not place much value on the use of a gun, but who might, for situational reasons, carry one while committing an offense.<sup>22</sup>

The point is that even in the theoretical world, where a clear and credible threat of a three-year mandatory sentence is communicated to a group of utility-maximizing offenders, the marginal effect would be small. If we add to this the usual noise and friction associated with the real world, such as incomplete information, impulsive behavior, and doubt about the credibility of the threat, it is quite plausible that the gun law would have negligible deterrent effects.

Statutes providing a mandatory sentence for felonies committed with a firearm are appealing because they appear to offer a way of reducing gun violence at little cost to the public. Our analysis of the Florida experience, however, provides evidence that, as in Michigan, this type of gun control policy does not in fact produce the promised reductions in violent crime.

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<sup>22</sup> Interviews conducted with a sample of convicted felons in Florida prisons in the spring of 1977 support this interpretation. See generally BURR, *supra* note 7. Seventy-three percent of the 277 respondents indicated that they would not be deterred from carrying a gun by the mandatory sentence. The typical reason given for continuing to carry a gun was that they needed it for protection and would rather take the chance of being arrested for possession of an illegal weapon than to be found by their friends and associates without one. Another typical response was that when you are committing a felony that carries a penalty of 10 to 20 years anyway, an extra three years is not significant. *Id.* at 23-24.