

# Has the Gun Deterrence Hypothesis Been Discredited?

A Reply to McDowall et al., *Criminology*,  
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by Gary Kleck

*In the November 1991 issue of Criminology, authors David McDowall, Alan Lizotte, and Brian Wiersema analyzed several of the more famous cases--such as Orlando and Kennesaw--in which increased attention to defensive gun ownership is often said to have resulted in sharply reduced crime. Applying statistical analysis, McDowall and his co-authors concluded that in no case had gun ownership led to a statistically perceptible drop in the crime rate. Here, Gary Kleck answers the McDowall article. Gary Kleck is a Professor of Criminology at Florida State University, in Tallahassee. His 1991 book **Point Blank** was awarded the American Society of Criminology's Hindelang Prize, for the most significant contribution to criminology in a three-year period.*

In a number of places, I have suggested that, in addition to any crime-increasing effects gun ownership among criminals may have, widespread gun ownership among noncriminals may exert various beneficial effects, including the reduction of some kinds of crime through deterrent effects (e.g. Kleck and Bordua 1983; Kleck 1986; 1988; 1991; Kleck and Sayles 1990; Kleck and DeLone 1993). David McDowall and his colleagues (1991) tried to test for deterrent effects using simple univariate times series analyses of crime rates, and have presented these tests as if they were tests of my ideas.

In a 1988 article published in *Social Problems*, I offered some anecdotes in which I noted decreases in rape following implementation of a highly publicized Orlando gun training program for women, a dampening of robbery increases in Kansas City after implementation of a gun training program for grocers, and decreases in residential burglaries after Kennesaw, Georgia required its citizens to keep guns in their homes (Kleck 1988, pp. 13-15). McDowall et al. responded to these anecdotes by performing low power statistical tests on very small samples, hypothesis tests using inappropriate dependent variables, and tests of hypotheses that do not follow from, and have no bearing on, my ideas about the deterrent effects of civilian gun ownership. They concluded that there "is no solid empirical support" for any deterrent effect of civilian gun ownership (McDowall et al. 1991, p. 556).

Increases in actual gun ownership are ordinarily fairly gradual, making it hard to detect any effects of increases in civilian gun ownership levels on crime. However, highly publicized programs to train citizens in gun use amount to "gun awareness" programs that could conceivably produce sharp changes in prospective criminals' awareness of gun ownership among potential victims. There are advantages to assessing the impact of these programs because they have distinct times of onset and spans of operation that make it easier to say when they might be most likely to affect crime.

The *Social Problems* article presented some very limited data on crime trends before and after the implementation of programs of this type, as well as other highly publicized events related to defensive gun use and ownership. The data were not offered as part of an attempt to formally test a deterrence hypothesis, but rather as illustrative anecdotes, albeit statistical ones. Unfortunately, these anecdotes, perhaps because they contained quantitative information, were misunderstood, and McDowall and his colleagues (1991) followed up on them by

attempting formal hypothesis tests using the same very limited data.

One can interpret their efforts in either of two ways. First they might have believed that their analyses were themselves useful formal tests of the deterrence hypothesis. If so, I believe they are wrong, because the samples are too small for even strong deterrent effects to be detected, and because there were no data allowing controls for other confounding factors that might have influenced crime trends.

Second, McDowall et al. might have merely been making the point that the changes I noted in my anecdotes could be attributable to random chance factors. If so, this is a trivial technical point that they need hardly have bothered making, given that it is largely a product of the arbitrary factor of how many crime observations happened to be available, rather than any lack of merit in the deterrence hypothesis. Since it was not I who presented the information in connection with a formal hypothesis test, the issue of statistical significance is irrelevant. Further, it is hard to see any justification for a twenty page journal article for making this minor point, which could have been made adequately in a sentence, such as "With only 14 annual observations in the Orlando rape data, or 26 annual observations of Kansas City robbery rates, almost any patterns Kleck observed might be attributable to random chance rather than deterrent effects."

Given the use of the anecdotes for illustrative purposes, the only valid criticisms one could make would either be that they are not very illustrative of, or germane to, the point being made (clearly not the criticism McDowall et al. made) or that point itself is known to be false. Since neither McDowall et al. nor I have presented or cited any strong evidence one way or the other on the deterrence hypothesis, it remains an open question whether the point is false, i.e. whether there are deterrent effects of civilian gun ownership. About all one can say is that the evidence, including (for reasons made clear later in the paper) that presented by McDowall et al. is consistent with that hypothesis. In short, we may legitimately continue to draw

precisely the same weak conclusion that I drew in the Social Problems article, that “gun ownership among prospective victims may ... have ... a crime-inhibiting effect” (Kleck 1998, p. 17, emphasis added).

I now take up each of the analyses performed by McDowall et al., to address whether their results are in fact consistent with a deterrence hypothesis.

## Rape and the Orlando Gun Training Program

McDowall et al. applied tests of statistical significance to 14 years of annual rape counts for Orlando, to test the idea that the highly publicized gun training program offered to women in Orlando had reduced rape. Both the direction and magnitude of their impact estimates confirmed my “statistical anecdote,” indicating about a 76% drop in rape,<sup>1</sup> i.e. a proportionally enormous reduction. (I had reported a simple 88% drop in rape—Kleck 1988, p. 13). The authors, however, chose to emphasize only the significance tests results—however huge the drop, it was not statistically significant.

What the authors did not report was that no matter how correct the deterrence hypothesis was, and no matter how strong the impact of the training program and associated publicity was, it would have been impossible for the deterrence hypothesis to pass their significance testing procedures. Even if the program had directly caused a complete elimination of rape in Orlando, it could not have achieved a statistically significant result, given a sample size of just 14 annual time points.<sup>2</sup> In effect, the authors were demanding the impossible of the hypothesis, given the limits of the data. In a very technically worded remark, buried in a footnote, the authors effectively conceded this point, noting that with so few observations, their test provided “low power against a maintained hypothesis” (McDowall et al. 1991, p. 546, fn. 9).

## **Robbery and the Kansas City Gun Training Program**

With respect to Kansas City robberies, they found a nonsignificant drop in robberies after a gun training program for Kansas City grocers, accompanied by significant increases in robbery in the surrounding region and in the United States. I had interpreted this pattern of findings as an indication the program might have prevented, in Kansas City, the robbery increases that occurred elsewhere, i.e. that it had a dampening effect on previously increasing robbery rates. I did not assert that Kansas City robberies decreased after the training program, but rather I explicitly stated that they “leveled off” (Kleck 1988, p. 13).

Oddly enough, when McDowall and Wiersema obtained the exact same combination of findings in a 1991 study of a gun control law (no change in the target crime series, accompanied by increases in the control series), they too interpreted it as indicating that the law “had a dampening effect on the increasing incidence of” robberies (O’Carroll, Loftin, Waller, McDowall, Bukoff, Scott, Mercy and Wiersema 1991, p. 578). In sharp contrast, when the “intervention” in question was a gun training program, they merely concluded that it had no effect that could “be distinguished from chance from chance variation” (McDowall et al. 1991, p. 549), not even mentioning the dampening effect interpretation.

## **Burglary and Kennesaw's Ordinance Requiring Guns in the Home**

In 1982, the city of Kennesaw, Georgia passed an ordinance requiring its residents to keep a gun in their home. With respect to the McDowall et al. analysis of Kennesaw burglary trends, I have shown elsewhere that the appearance of no impact was created by the authors largely by mismeasuring the dependent

variable. Instead of measuring the per capita rate of residential burglaries, they measured the raw counts of all burglaries. The failure to compute rates, in which population is taken account of, caused any burglary rate reductions to be obscured by the 70% increase in population Kennesaw experienced between 1980 and 1987 (Kleck 1991, pp. 136-138).

More significantly, the use of total burglaries rather than just residential burglaries was inappropriate in light of the fact that my hypothesis of a deterrent effect pertained specifically to *residential* burglaries, for the obvious reason that the Kennesaw ordinance applied only to the keeping of guns in residences, not in stores, offices, factories, etc. (Kleck 1988, p. 15) I also hypothesized in this article that the keeping of guns in homes may induce burglars to either shift to nonresidential targets or to burglarize residences only after they made sure that no one was home (pp. 15-16). If burglars were deterred from entering occupied homes; this would not necessarily reduce the total burglary rate, but could instead cause a redistribution of burglary targets that would be beneficial because it reduced victim-burglar confrontations and thus burglary-linked injuries. Consequently, a test of my hypothesis of the deterrent effect of the Kennesaw ordinance (and/or associated publicity) would necessarily have to focus on residential burglaries separately. The McDowall et al. analysis did not. Consequently, they did not test the hypothesis that I had stated.

These issues are not mere quibbles—the difference in change scores between the correct and incorrect measures is enormous. When the correct dependent variable, the residential burglary rate, is used, the 1981-1986 percent decrease is *twice* as large as when one uses the inappropriate measure McDowall and his colleagues used (Kleck 1991, p. 137).

## **The Morton Grove and Evanston Handgun Bans**

With respect to handgun bans in Morton Grove and Evanston, McDowall et al. constructed their own hypotheses, rather than (as in the Orlando, Kansas City and Kennesaw cases) addressing episodes I had discussed. If they believed that their hypotheses were derived from my ideas, or contradicted the deterrence thesis, they were mistaken.

McDowall et al. asserted, rather simplistically, that if gun ownership exerts a deterrent effect on burglaries, there should be an increase in burglaries if handguns are banned (“disarmament policies might raise [crime rates]”—McDowall et al. 1991, p. 552). This hypothesis was implicitly based on the assumption that burglars would believe that passing a handgun ban would reduce their risk of facing a gun-armed victim.

It is more likely that burglars believed that handgun-owning residents would adapt to handgun bans in either of two ways. First, many burglars would assume that prospective victims would react to the ban the same, as they would, i.e. simply ignore it. This was especially easy to do in light of the local authorities’ public promise that they had no intention of searching homes for illicit handguns (*Chicago Tribune* 9-14-82, p. 1-3). Second, some burglars might anticipate that prospective victims who did surrender their handguns would adapt by substituting long guns such as shotguns and rifles, just most felons say they would do if they could not get a handgun (Wright and Rossi 1986, p. 217).

I have argued that if one restricts only the ownership of handguns, the most likely adaptation by those denied handguns would be to substitute long guns such as rifles or shotguns (Kleck and Bordua 1983; Kleck 1991; Kleck 1997; but esp. Kleck 1986). Thus, if handgun ownership were banned, criminals would substitute long guns, and some would presumably assume that their victims had done the same. While it would be hard to substitute long guns for handguns for purposes of carrying guns concealed in public places, there is little reason to expect anything less than complete substitution of long guns in residences, among those who gave up handguns in the first place. Consequently, there is no sound reason to expect that burglars would perceive

lower rates of home gun ownership among their prospective victims as a result of a ban applying only to handguns, and hence no reason to expect a decline in the deterrent effect of gun ownership or an increase in burglaries. Quite the contrary, given that long guns are more lethal than handguns (Kleck 1986), if burglars' perceptions of risk were altered at all, they could even have increased.

In addition, one of the themes that is invariably a part of the public debate preceding handgun bans is that there are “too many guns out there,” that “we are a gun-ridden society,” and so on. Thus, the highly publicized debate typically preceding passage of a gun ban inadvertently serves to remind prospective criminals of how likely it is that their victims own a gun. In combination with the expectation that the law would not reduce total gun ownership, this should increase any deterrent effects of gun ownership, at least in the short run.

Thus, my perspective leads to the prediction that there would be short-term decreases in burglaries following handgun bans, if there were any effects at all. These decreases would occur not because burglars need handguns to commit burglaries (they do not), but rather because the preceding public debate inadvertently serves to remind them of the risks of victim gun use, and because some of them might anticipate the substitution of more lethal long guns among their prospective victims. Burglary decreases are precisely what McDowall et al. found following the Morton Grove and Evanston handgun bans, thereby supporting this perspective. Needless to say, this is not the conclusion McDowall and his colleagues drew.

## Conclusions

In sum, their *non sequitur* interpretations notwithstanding, all of the McDowall et al. findings supported the deterrence hypothesis. Nevertheless, it should be reiterated that I did not cite



these episodes for purposes of hypothesis testing, but rather only as anecdotes that illustrated the deterrent and displacement processes that I believed operated in connection with civilian ownership and use of guns.

More generally, univariate analyses of time series data on crime or violence counts are not adequate for purposes of assessing the impact of gun laws, gun training programs, or other gun-related events. As discussed in detail elsewhere (Kleck et al. 1993; Britt et al. 1996; Kleck 1997, Chapter 11), univariate interrupted time series studies are close to worthless, and sometimes counterproductive, for assessing the impact of laws, programs, and other interventions. Although results (including those of McDowall et al. 1991) have been consistent with the gun deterrence hypothesis, “natural experiments” nevertheless provide only the weakest sort of evidence available on the issue.

On the other hand, much stronger individual-level evidence consistently supports the hypothesis that actual defensive uses of guns by victims “disrupt” criminal attempts, i.e. reduce the chances that the victim will be injured or lose property (Kleck 1988, pp. 79; 1991, pp. 122-126, 149; 1997, Ch. 5; Kleck and DeLone 1993, pp. 68-69; Cook 1991, p. 57; Southwick 1996) and that these defensive gun uses occur quite frequently in the U.S.—perhaps 2.5 million times a year (Kleck and Gertz 1995, and the thirteen earlier surveys reviewed therein; Kleck 1997, Chapter 5).

## References

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**ENDNOTES**

1 McDowall et al. reported an annual average of 15 rapes (p. 547), and their impact parameter indicated a drop of 11.3846 rapes after the gun training program was implemented (p. 548);  $11.3846/15=0.759$ .

2 Since Orlando averaged only 15 rapes per year over this period, a 100% reduction would imply an "impact" parameter of about -15. With a standard error of 10.1188 for their estimate of the intervention's impact (McDowall et al. 1991, p. 548), even a 100% reduction would imply a t-ratio test statistic of only -1.48, less than the -1.771 needed for statistical significance with 13 degrees of freedom.