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The Impact of Gun Ownership Rates on Crime Rates: A Methodological Review of the Evidence

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ABSTRACT

Purpose: This paper reviews 41 English-language studies that tested the hypothesis that higher gun prevalence levels cause higher crime rates, especially higher homicide rates.
Methods: Each study was assessed as to whether it solved or reduced each of three critical methodological problems: (1) whether a validated measure of gun prevalence was used, (2) whether the authors controlled for more than a handful of possible confounding variables, and (3) whether the researchers used suitable causal order procedures to deal with the possibility of crime rates affecting gun rates, instead of the reverse.
Results: It was found that most studies did not solve *any* of these problems, and that research that did a better job of addressing these problems was less likely to support the more-guns-cause-more crime hypothesis. Indeed, none of the studies that solved all three problems supported the hypothesis.
Conclusions: Technically weak research mostly supports the hypothesis, while strong research does not. It must be tentatively concluded that higher gun ownership rates do not cause higher crime rates, including homicide rates.

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Introduction

The central premise behind gun control as a policy to reduce crime or violence is that gun availability affects rates of crime or violence. In particular, many scholars assert that gun levels affect homicide rates, primarily because use of a gun in attacks increases the likelihood that they will result in the victim's death. Prior reviews of the literature have arrived at radically different conclusions about the effect of gun levels on crime rates: (1) more guns cause more crime (Hepburn & Hemenway, 2004), (2) more guns do not cause more violence, and may even reduce it (Kates & Mauser, 2007), or (3) we do not know one way or the other (National Research Council, 2004). None of these reviews identified the methodologically strongest studies and compared their findings with those of weaker studies.

Unfortunately, research on the effect of gun levels on homicide and other crime rates has generally been of poor quality, and prior reviews of the evidence have failed to systematically sort out the methodologically better studies from the less sound ones. It is especially discouraging that scholars keep making the exact same mistakes over and over again, so it is critically important to differentiate better studies from

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worse ones. All research is flawed, and all bodies of research are incomplete, but that does not mean we cannot distinguish the less flawed work from the more flawed, and draw tentative conclusions based on the best available research conducted so far.

Beginning students in research methods are taught that in order to establish that one variable, X, has a causal effect on another variable, Y, one must establish that (1) there is a statistical association between X and Y, (2) this association is not spurious, that is, it is not completely the product of confounder variables (antecedent variables that affect both X and Y), and (3) X is causally antecedent to Y, rather than (or in addition to) the reverse.

Correspondingly, the most fundamental flaws in this body of research are failures to establish these essential conditions. First, in order to establish that there is an association between gun levels and crime rates, one must have a valid measure of gun levels, but most studies use proxies that are either known to be invalid or whose validity has not been established. Second, most researchers make little effort to control for confounders, and many make no effort at all. Those that made some effort typically controlled for arbitrarily chosen sets of variables based on little more than the fact that at least one prior study had found a significant association between the supposed confounder and the outcome (dependent) variable. Third, virtually none of the studies properly modeled the possible two-way relationship between gun levels and violence rates, and may have confused the effect of crime rates on gun levels with the effect of gun levels on violence rates. These three are by no means the only problems with studies in this area – small sample sizes (50 or fewer in most studies), sample bias, and needlessly high levels of aggregation, of highly heterogeneous units of analysis such as nations, regions, or states, are also common flaws. These three are, however, fundamental, in that they directly bear on whether the essential conditions of causality are likely to have been met.

The goal of this paper is to summarize what research on this topic indicates, and to sort out the methodologically stronger studies, on the aforementioned criteria, in order to determine if their findings differ from those of the less sound studies. Studies were included in the review if they were published English-language macro-level studies that provided quantitative estimates of the effect of gun levels on crime rates. The review does not cover studies of the impact of gun control laws or studies that merely inferred gun levels from the strictness of gun control (e.g., Miron, 2001), nor does it cover studies of individual-level criminal victimization or offending. Likewise we excluded studies that assessed only the impact of gun levels on rates of *gun* crime, such as gun homicide, but not total crime, such as total (gun plus nongun) homicide, since the association of gun possession levels with gun violence is at least partly tautological – by definition, a crime cannot be counted as a gun crime unless the offender possessed a gun.

Multiple findings for a given study were included in the review if they either pertained to different crime types or were based on independent samples. Where there were multiple estimates regarding the effect of gun levels on a given crime rate, the one based on the strongest methods was included - e.g., the finding based on the model with the largest number of significant control variables, the strongest methods for addressing causal order, or the most valid measure of gun prevalence. Using these guidelines, there were 90 distinct findings, i.e. distinct tests of the hypothesis that higher gun levels cause higher crime rates.

Was a guns-crime association established? – Validity of the measures of gun prevalence

To determine whether the prevalence of guns is even associated with crime rates, it is of course necessary to have a valid measure of the prevalence of guns. Without this, it is impossible to even compute a valid statistical association between gun levels and crime rates. Table 1 summarizes the 41 published English-language studies in this area we located, and shows that, while researchers have used a wide variety of proxy measures of gun prevalence (described in detail in footnote b), direct evaluations of their validity have indicated that nearly all of these measures are invalid (Kleck, 2004). Criterion validity is typically assessed by computing the correlation of the proxy with direct survey measures of gun prevalence. Valid gun measures are indicated in Table 1 with bold type. Only the percent of suicides committed with guns (PSG) shows strong validity for purposes of measuring levels of gun ownership in different areas. Further, *none* of the proxies used in prior research, including PSG, have been shown to be valid for purposes of judging trends over time (Kleck, 2004, pp. 19–26).

This problem is therefore especially serious in studies using a longitudinal design, such as a panel design, since those using such designs appear to implicitly assume that any proxies that are valid for establishing differences in gun levels across areas must also be valid for establishing changes in gun levels over time. Direct tests of the validity of nearly 20 proxies used in this body of research clearly indicate that this assumption is false (Kleck, 2004; Kovandzic, Schaffer, & Kleck, 2012, 2013). Some scholars nevertheless insist that PSG is valid for measuring trends in gun ownership, creating a misleading appearance of validity through the use of one of several techniques. One method is to report the correlation of PSG with direct survey measures of gun ownership, while combining *both* cross-sectional covariation and cross-temporal covariation (e.g., Azrael, Cook, & Miller, 2004; Cook & Ludwig, 2003; Duggan, 2001; Miller, Azrael, & Hemenway, 2002). This is misleading because it takes advantage of the genuinely strong correlation of PSG and survey measures *across areas* (Kleck, 2004) while concealing the nonexistent correlation of PSG and survey measures *across time*. Kovandzic et al. (2013) directly demonstrated that all of the correlation between PSG and survey measures of gun prevalence, when cross-area and cross-temporal data are mixed together, is in fact due to cross-area correlation. Thus, the supposed validity tests of Duggan and the rest actually indicated nothing about the ability of PSG to track changes over time in gun prevalence.

Table 2 reports cross-temporal correlations of GSS survey estimates of gun prevalence and PSG. Using all the GSS data available up through 2006, the cross-temporal correlations between PSG and the direct GSS measures of gun ownership prevalence were computed, first using the levels of the variables, then correlating year-to-year changes in the variables. The figures in the first column indicate that, contrary to Cook and Ludwig (2006), PSG does not significantly correlate with the GSS measures for four of the nine regions, even when the correlations are computed using the levels of the variables, and the five correlations that are significantly different from zero are far too weak to indicate that PSG is a good proxy for changes in gun prevalence. For example, even for the region with the strongest correlation, the West South Central region, the correlation of 0.694 implies that only 48 percent ($0.694^2 = 0.48$) of the variation over time in PSG is shared with variation in the GSS measure. That is, most of the variation (52%) in PSG is independent of variation over time in gun prevalence as measured in the GSS. By no stretch of the imagination can a proxy measure be regarded as having good validity if *most* of the variation in the proxy is independent of the target construct being measured. Further, the second column of numbers indicates that when year-to-year changes are analyzed, there is essentially *no* association over time between changes in PSG and changes in direct survey measures of gun prevalence. In sum, PSG is apparently useless for tracking changes in gun prevalence, despite its considerable ability to assess differences in gun prevalence across areas. The same is true of all other gun proxies tested for validity. Consequently, the findings of nearly all studies that have attempted to relate changes over time in gun ownership to changes in PSG are uninterpretable, because the researchers were not actually measuring changes in gun levels (e.g., Cook & Ludwig, 2003, 2006; Miller et al., 2002; Moody & Marvell, 2005).

The failure to find any proxies to be longitudinally valid may be due to an absence of meaningful change over time in gun prevalence. Although gun prevalence differs enormously across different areas of the U.S., there may have been little or no actual change over time to