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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

Table 1
Macro-Level Studies of the Impact of Gun Levels on Crime Rates[a]

Study	Sample	Measure of Gun Levels[b]	Crime Rates[c]	Number of Significant Control Variables[d]	Causal Order?[e]	Findings[f]
Brearley (1932)	2 states	PGH	THR	0	No	**+, p < .01**
Krug (1967)	50 states	HLR	ICR	0	No	-, p < .01
			THR	0	No	-, p > .10
			TAR	0	No	-, p < .01
			TRR	0	No	-, p < .01
Newton and Zimring (1969)	4 years, Detroit	NPP	THR	0	No	+, ?
			TRR	0	No	+, ?
			AAR	0	No	+, ?
Seitz (1972)	50 states	GHR, AAR, FGA	THR	1	No	+, ?
Fisher (1976)	9 years, Detroit	NPP, GRR, PGH	THR	1	No	**+, p < .01**
Phillips, Votey, and Howell (1976)	18 years, U.S.	PROD	THR	1	No	**+, p < .01**
Kleck (1979)	27 years, U.S.	PROD	THR	6	(No)	**+, p < .01**
Cook (1979)	50 cities	PGH, **PSG**	TRR	4	No	+, p > .10
			RMR	0	No	+, .05 < p < .10
Kleck (1984)	32 years, U.S.	PROD	THR	5	(No)	+, p > .10
			TRR	2	No	**+, p < .01**
Magaddino and Medoff (1984)	31 years, U.S.	PROD	THR	6	No	+, .05 < p < .10
Lester (1985)	37 cities	PCS	VCR	0	No	-, p > .10
Bordua (1986)	102 counties	GLR	THR	0	No	?, p > .05
	9 regions	**SIR**	VCR	0	No	-, p < .05
			THR	0	No	-, p < .05
McDowall (1986)	48 cities, 2 years[g]	PGH, **PSG**	TRR	0	(No)	+, p > .10
Lester (1988)	9 regions	**SGR**	THR	0	No	**+, p < .05**
Killias (1990)	11 nations	**SGR**	THR	0	No	**+, p < .05**
McDowall (1991)	36 years, Detroit	PSG, PGR	THR	3	(No)	**+, p < .05**
Killias (1993a)	17 nations	**SGR**	THR	0	No	**+, p < .02**
Killias (1993b)	16 nations	**SGR**	THR	0	No	**+, p < .05**
Kleck and Patterson (1993)	170 cities	5-item factor	THR	7	Yes	-, p > .10
		incl. **PSG**[g]	TRR	7	Yes	-, p > .10
			AAR	4	Yes	-, p > .10
			TPR	5	Yes	-, p > .10
Lester (1996)	12 nations	PGH, **PSG**	THR	0	No	**+, p < .01**
Kleck (1997)	36 nations	**PSG**	THR	0	No	+, .05 < p < .10
Southwick (1997)	48 years, U.S.	PROD	THR	0	(No)	?, p > .10
			TRR	0	(No)	?, p > .10
			AAR	0	(No)	-, p > .10
			TPR	0	(No)	?, p > .10
			BUR	0	(No)	-, p < .10
			LAR	0	(No)	-, p < .10
			ATR	0	(No)	-, p > .10
Hemenway and Miller (2000)	26 nations	PGH, **PSG**	THR	0	No	**+, p < .05**
Lester (2000)	27 years, Canada	PGH, PSG	THR	1	No	**+, p < .01**
Stolzenberg and D'Alessio (2000)	4 years, 46 counties	CCW, GUNSTOL	VCR	0-1	No	**+, p < .01**
Duggan (2001)	19 years, 49 states	GMR	THR	0	(No)	+, .05 < p < .10
			TRR	0	(No)	+, p > .10
			AAR	0	(No)	-, p > .10
			TPR	0	(No)	-, p > .10
			BUR	0	(No)	-, p > .10
			LAR	0	(No)	+, p > .10
			ATR	0	(No)	+. P > .10
	19 years, 460 counties	GMR	THR	0	(No)	**+, p < .05**
			TRR	0	(No)	+, .05 < p < .10
			AAR	0	(No)	+, p > .10
			TPR	0	(No)	+, p > .10
			BUR	0	(No)	+, p > .10
			LAR	0	(No)	+, p > .10
			ATR	0	(No)	+ .05 < p < .10
Hoskins (2001)	36 nations	**PSG**	THR	2	(No)	**+, p < .05**
Killias et al. (2001)	21 nations	**SGR**	THR	0	No	+, p > .10
			TRR	0	No	+, p > .10
			AAR	0	No	+, p > .10
Sorenson and Berk (2001)	22 years, California	HGS	THR	0	(No)	+, p = ?
Miller et al. (2002)	10 years, 50 states	PSG, PHG	THR	0	No	**+, p < .05**
	10 years, 9 regions	**SGR**	THR	0	No	+, p > .05
Cook and Ludwig (2003)	50 states	**PSG**	BUR	1	(No)	+, p > .05
	? counties	**PSG**	BUR	0	(No)	+, p > .05
Azrael et al. (2004)	49 states	**PSG**	THR	1	No	**+, p < .01**
Moody and Marvell (2005)	22 years, c. 29 states	PSG, **SHR**	THR	6	(No)	-, p > .10
			TRR	8	(No)	+, p > .10
			AAR	6	(No)	-, p > .10
			TPR	6	(No)	+, .05 < p < .10
			BUR	7	(No)	+, p > .10
Cook and Ludwig (2006)	20 years, 50 states	PSG	THR	0	(No)	**+, p < .05**
			TPR	0	(No)	-, p > .10

Table 1 (*continued*)

Study	Sample	Measure of Gun Levels[b]	Crime Rates[c]	Number of Significant Control Variables[d]	Causal Order?[e]	Findings[f]
			AAR	0	(No)	-, p > .10
			LAR	0	(No)	+, p > .10
			ATR	0	(No)	+, p > .10
	20 years, 200 counties	PSG	THR	3	(No)	+, .05 < p < .10
			TPR	0	(No)	-, p > .10
			AAR	0	(No)	-, p > .10
			LAR	0	(No)	+, p > .10
			ATR	0	(No)	+, p > .10
Hass, Jarvis, Jefferies, and Turley (2007)	55 counties, 3 years	CCW	VCR	4	No	**+, p < .05**
Miller et al. (2007)	50 states	**SGR**	THR	5	No	**+, p < .001**
Moody (2010)	27 years, 50 states	PSG	THR	3	(No)	+, p > .20
	U.S., 59 years	PROD	THR	0	(No)	+, p > .10
	30 nations	**SGR**, GRR	THR	0	No	-, p > .10
Kovandzic et al. (2012)	1,456 counties	**PSG**	THR	10	Yes	-, p < .01
Kovandzic et al. (2013)	1,456 counties	**PSG**	THR	10	Yes	-, p < .01
Bangalore & Messerli (2014)	27 nations	GRR, others	ICR	0	No	+, .05 < p < .10
Siegel et al. (2014)	30 years, 50 states	PSG, HLR	THR	7	No	**+, p < .05**

Notes: a. Table covers only studies in which the dependent variable was a crime rate, as opposed to the fraction of crimes committed with guns, and where gun ownership levels were actually measured, rather than assumed. Studies that examined only gun violence rates (e.g. only gun homicides but not total homicides) were excluded (e.g., Murray, 1975).

b. Measures of Gun Level: CCW = concealed carry permits rate; FGA = Fatal gun accident rate; GLR = Gun owners license rate; GMR = Gun magazine subscription rates; GRR = Gun registrations rate; GUNSTOL = % of the dollar value of stolen property due to guns; HGS = handgun sales (retail); HLR = Hunting license rate; NPP = Number of handgun purchase permits; PGA = % aggravated assaults committed with guns; PGC = % homicides, aggravated assaults and robberies (combined together) committed with guns; PCS = same as PGC, but with suicides lumped in as well; PGH = % homicides committed with guns; PGR = % robberies committed with guns; PSG = % suicides committed with guns; PROD = Guns produced minus exports plus imports, U.S.; SGR = Survey measure, % households with gun(s); SHR = Survey measure, % households with handgun(s); SIR = Survey measure, % individuals with gun(s).

Abbreviations in bold type denote measures of gun prevalence that have been shown to be valid for use with the research design used in the indicated study, while those not in bold are unvalidated. PSG is valid in cross-sectional studies of different areas, but not valid in studies of changes over time, which encompasses all studies analyzing multiple years (as indicated in the Sample column).

c. Crime Rates: AAR = Aggravated assault rate; ATR = auto theft rate; BUR = burglary rate; GAR = Gun aggravated assault rate; GHR = Gun homicide rate; GRR = Gun robbery rate; HAR = Homicide, assault and robbery index (factor score); ICR = Index crime rate; LAR = Larceny rate; NHR = nongun homicide rate; RMR = Robbery murder rate; THR = Total homicide rate; TPR = Total rape rate; TRR = Total robbery rate; VCR = Violent crime rate.

d. Number of significant control variables in the most complete crime rate equations, excluding "fixed effects" variables (dummy variables denoting area or time period) and lagged dependent variables. Multiple coefficients for dummy variables representing subcategories of a single concept, such as the age distribution of the population, were treated as if they were a single coefficient in counting up significant control variables. If authors did not report estimates affirmatively indicating the significance of control variables, they were treated as nonsignificant.

e. Did research use technically sound methods to establish the causal order between gun levels and crime rates? (No) means researchers took steps to address the issue, but used ineffective methods such as merely lagging the gun variable (e.g., Duggan, 2001), or used instrumental variables methods with instruments that were not relevant and valid (e.g., Cook & Ludwig, 2003; Hoskins, 2001).

f. Sign and significance of gun/crime association. Significant positive associations are denoted with bold type.

g. Five-item factor composed of PSG, PGH, PGR, PGA, and the percent of dollar value of stolen property due to stolen gun.

Table 2

Correlations of Percent of Suicides with Guns, and General Social Surveys Measure of Household Gun Prevalence, Across the Years 1973-2006[a]

Region	Levels	Year-to-year Changes[b]
New England	.452*	-.118
Middle Atlantic	.047	.249
East North Central	.381*	.061
West North Central	.117	.188
South Atlantic	.572**	.093
East South Central	.405*	.041
West South Central	.694**	-.214
Mountain	.117	-.049
Pacific	.252	-.021
U.S.	.359	-.073

Significance Levels:

* p < .05, 1-tailed.

** p < .01, 1-tailed.

Notes:

a. There were 21 General Social Surveys (GSS) conducted during this period, in 1973, 1974, 1976, 1977, 1980, 1982, 1984, 1985, 1987, 1988, 1989, 1990, 1991, 1993, 1994, 1996, 1998, 2000, 2002, 2004, and 2006. The GSS data on percent of households reporting guns (excluding those who refused or otherwise failed to answer the question) were computed using the facilities of the University of California at Berkeley Survey Documentation & Analysis website at http://sda.berkeley.edu/cgi-in/hsda?harcsda+gss06. Data on the percent of suicides committed with guns were computed using the facilities on the CDC Wonder website at http://wonder.cdc.gov/mortSQL.html. Suicide data for 2006 were not yet available, so 2005 data were substituted.

b. Year-to-year changes in the percent of suicides committed with guns (PSG) in a given region were correlated with year-to-year changes in the percent of households reporting guns in the GSS. For example, the year-to-year change in PSG for New England in 1974 was PSG in 1974 minus PSG in 1973, divided by PSG in 1973. Thus, this figure represents the proportional change in PSG from 1973 to 1974.

measure during the periods covered by most of the reviewed longitudinal studies (c. 1959–1993). National surveys indicate that the share of households reporting a gun rarely fell outside the range from 44% to 52% between 1959 and 1993 (Kleck, 1997), so there may have been no opportunity to estimate what effects on violent crime rates might be produced by changes in gun prevalence (Kleck, 2004). The absence of any meaningful change, however, is another reason why incorporating a longitudinal element into research designs, while helpful when applied to other problems, was not very useful in this area.

An alternative to using proxies for gun levels is to use direct survey measures of gun ownership. Survey measures of gun ownership are themselves subject to serious error, mostly in the form of underreporting of gun ownership, but do have the merit of being fairly direct modes of measurement. The main problem with the studies that have used this method so far (e.g., Miller et al., 2002), however, is that (a) the survey's sample sizes for the areas used in the study (typically Census regions or states) were far too small to provide meaningful estimates of changes in gun prevalence. The number of respondents in any one region in the GSS is often less than 100 (see sources cited in note a, Table 2), so only the largest (and most implausible) changes in regional gun prevalence measures could be statistically significant. For example, Miller et al. (2002) claimed that region-level changes in survey-measured gun prevalence caused changes in homicide rates, but they did not show that any of their survey-based year-to-year changes in regional gun prevalence were statistically significant. In fact, reanalysis of the GSS regional data indicates that very few of the changes were significant, and the handful that were significant were implausibly large and erratic. For example, the GSS results indicate that in New England the percent of households with guns supposedly jumped from 16.6 in 1982 to 42.9 percent in 1984 (a 158% increase in two years!), and then dropped back to 25.1 in 1985 (author's analysis of GSS data – see source in note a of Table 2). It is highly unlikely that New England, or any other region actually experienced changes in gun prevalence this radical in such short time periods or that were this erratic. More likely, these apparent changes largely reflect sampling error and changes in the willingness of gun owners to their report gun ownership. Miller and his colleagues were thus probably mostly modeling statistical noise.

In contrast, Killias, van Kesteren, and Rindlisbacher (2001) also used survey measures of gun prevalence, but in a study of entire nations for which they had ample sample sizes (generally over a thousand per nation) for estimating each country's gun prevalence. They found no significant effect of gun prevalence on rates of total homicide, total robbery, or total aggravated assault; gun prevalence only affected the share of these crimes committed with guns. Thus, it appears that it matters a great deal whether survey-based studies have sufficient sample sizes for stable estimates of gun prevalence.

Valid measures of gun prevalence are indicated in Table 1 by displaying them in bold type. Two measures were assessed as valid: the percent of suicides committed with guns, when used in a cross-sectional study, and direct survey measures. The general pattern evident in the table is that nearly all studies claiming to find crime-increasing effects of gun levels either (1) used invalid gun measures (e.g., Duggan, 2001; McDowall, 1991; Southwick, 1997; Stolzenberg & D'Alessio, 2000), or (2) used PSG inappropriately to measure *changes* in gun levels in studies using longitudinal designs (Cook & Ludwig, 2003, 2006; McDowall, 1991; Miller et al., 2002; Moody & Marvell, 2005; Siegel et al., 2014).

Controls for confounding variables

It is also essential that researchers seeking to estimate the effect of gun levels on crime rates statistically control for confounding variables – those factors that affect crime rates, but that are also associated with gun prevalence rates. If this is not done, the supposed effects of gun levels will be confused with the effects of the confounding variables. The more of these likely confounding variables that a researcher controls, the less likely this problem will be a serious one. Statisticians describe this as the "omitted variables" problem, because researchers failed to include confounding variables in their multivariate equations predicting crime rates. For example, if an area was characterized by a culture that encouraged violent behavior, but gun ownership was also common in that area, then that violent subculture would be a confounding variable because it affects violence rates but is also correlated with gun ownership. Because the southern parts of the U.S. are thought to be characterized by a regional culture of violence, and also have higher gun ownership rates (Kleck, 1997, p. 102), more careful analysts control for the regional location of states or cities as a way of indirectly controlling for a possible Southern subculture of violence whose effects on violence might be confused with effects of gun levels.

A variable must, at minimum, possess both of two properties in order to actually be a confounder: (1) it must show a statistically significant association with the outcome (dependent) variable, and (2) must be associated with the predictor of interest – gun prevalence in the present case. If a supposed confounder lacks either of these attributes, it is not in fact a confounder, and controlling for it does not help isolate the effect of the predictor of interest. Therefore, the number of statistically significant ($p < .05$) control variables included in the researcher's model of crime rates was recorded for each study. Note that some of the control variables that are related to crime rates may not be associated with gun levels, so even the number of significant control variables is a generous measure of the number of confounders controlled. Unfortunately, researchers rarely report correlations of their control variables with gun levels.

Returning to Table 1, it is clear that the vast majority of studies of the effect on gun levels on crime rates did a poor job of controlling for likely confounding variables, in that their own reported findings indicated that the authors controlled for few control variables that had a documented statistically significant association with crime rates. Of the 41 studies reviewed, fourteen did not control for *a single confounder* (e.g., Duggan, 2001; Fisher, 1976; Hemenway & Miller, 2000; Killias, 1993a, 1993b; Killias et al., 2001; Lester, 1996; McDowall, 1986; Newton & Zimring, 1969). Only six studies controlled for more than five statistically significant control variables (Kleck, 1979; Kleck & Patterson, 1993; Kovandzic et al., 2012, 2013; Magaddino & Medoff, 1984; Moody & Marvell, 2005). All six of these studies found no significant positive effect of gun levels on violence rates. The pattern, then, is highly consistent and simple to summarize. When researchers do a poor job of controlling for potential confounding variables, they often find apparent support for the hypothesis that more guns lead to more crime. When authors do even a minimally adequate job of controlling confounders, they find no support for the hypothesis.

Causal order – Did the researchers distinguish the effect of gun levels on crime rates from the effect of crime rates on gun levels?

Gun prevalence might affect crime rates, but it also possible that higher crime rates cause higher gun prevalence, as more people acquire guns, particularly handguns, for self-protection. A large and varied body of research strongly supports the hypothesis that crime rates – especially homicide rates - have a positive effect on rates of gun ownership, especially handgun ownership. The implication for macro-level studies of the impact of gun levels on crime rates is that researchers who fail to adopt appropriate methods for addressing causal order are likely to mistake (a) a positive effect of crime rates on gun levels for (2) a positive effect of gun levels on crime rates.

At least eleven published macro-level studies have found evidence indicating a positive effect of crime rates on gun levels. Kleck (1979), Bordua and Lizotte (1979), McDowall and Loftin (1983), Kleck (1984, p. 116), Magaddino and Medoff (1984, p. 246), Kleck and Patterson (1993), Southwick (1997), Bice and Hemley (2002), Rosenfeld, Baumer, and Messner (2007), Kovandzic et al. (2012), and Kovandzic et al. (2013) all found significant positive effects of gun ownership rates on crime rates, especially homicide rates. Further, most of these studies adopted arguably appropriate ways to address the causal order issue, and still consistently found that crime rates have significant positive effects on gun rates. Kleck (1979, 1984), Kleck and Patterson (1993), Rosenfeld et al. (2007), and Kovandzic et al. (2012, 2013) all used instrumental variables methods to model possible two-way causation, while Southwick (1997) used Granger causality methods.

There are also a variety of individual-level studies that indicate that the likely consequences of higher crime rates, such as individual victimization experiences or fear of crime have a positive effect on ownership of guns, especially handguns, the type of gun most likely to be owned for reasons of self-protection (Kleck, 1997). Most of these studies unfortunately also face a causal order problem. As Wright and Rossi (1983) noted many years ago, it may be that "the initially most fearful may arm themselves and then feel psychologically safer because of it" (p. 129). That is, it is possible that fear causes gun acquisition, which then causes fear reduction. Consequently, a cross-sectional study might find no fear-gun association even if fear did motivate gun acquisition.

Fortunately, there are ways to reduce the causal order problem, and studies that are less seriously afflicted by this problem have found that higher crime rates, especially higher homicide rates, and the by-products of higher crime rates such as fear of crime, make it more likely people will acquire guns, especially handguns. For example, survey research on the residents of large U.S. cities indicates that individuals who live in high-homicide cities are more likely to own handguns (Kleck & Kovandzic, 2009). There is unlikely to be a causal order issue in this research because, while the homicide rates prevailing in an individual survey respondent's city could affect that individual's decision to own a handgun, a single individual's handgun ownership is unlikely to have a measureable effect on the homicide rate of a large U.S. city.

Another study showed that, among people who do not currently own guns, those who are more fearful of crime are more likely to say that they plan to acquire a gun in the near future (Kleck, Kovandzic, Saber, & Hauser, 2011). While actually owning a handgun might reduce its owner's fear, it is unlikely that merely planning to acquire one would do so. Thus, it is unlikely that, in this study, the positive fear-handgun association is due to an effect of planning to get a gun on fear. Instead, the causal direction is likely to run from fear to gun acquisition.

A later study addressed the causal order issue by using a panel design, relating changes in gun owner status over time to changes in fear of crime (Hauser & Kleck, 2013). The study indicated that increases in fear do motivate gun acquisition, but decreases in fear do not motivate getting rid of guns. At the macro-level, the combined operation of these two effects should generate a positive association between gun levels and crime rates, assuming that higher crime rates lead to higher levels of fear of crime. In sum, the hypothesis that crime rates affect gun ownership is more than a mere logical possibility – there is considerable empirical evidence to indicate that such an effect is a reality.

It therefore is important that researchers in this area address the causal order issue, and use appropriate methods for doing so. Table 1 shows that few scholars even made an attempt to address the causal order problem. Researchers have typically adopted one of four unhelpful responses to this problem: (1) ignoring the issue altogether (e.g., Hemenway & Miller, 2000; Killias, 1993a, 1993b; Lester, 1988), (2) mentioning the issue but arguing that it is not really a problem (e.g., Miller, Azrael & Hemenway, 2002; Stolzenberg & D'Alessio, 2000), (3) acknowledging it as a possible problem in a *pro forma* way, as a mere logical possibility, but without conveying its seriousness or doing anything about it (e.g., Azrael et al., 2004; Miller, Hemenway, & Azrael, 2007; Siegel et al., 2014)), or (4) forthrightly acknowledging the problem but applying inadequate solutions (e.g., Cook & Ludwig, 2003, 2006; Duggan, 2001; Kleck, 1979; Sorenson & Berk, 2001).

The most common variant of an inadequate response is to simply relate the current year's crime rate to the previous year's gun levels, that is, to lag the independent variable (e.g., Cook & Ludwig, 2003; Duggan, 2001; McDowall, 1986; Moody & Marvell, 2005; Sorenson & Berk, 2001; Southwick, 1997). Its intuitive appeal is based on the indisputable point that this year's crime rates cannot affect last year's gun ownership levels. Thus, the tactic appears to avoid the causal order problem. Unfortunately, the approach is more an evasion of the problem than a solution. An analyst adopting this strategy includes the lagged gun ownership measure as a predictor of crime rates, but not the current gun ownership measure. Excluding the current gun level is legitimate only if gun ownership levels have no immediate (within one year) effect on crime rates. By specifying the statistical model this way, the analyst is simply *assuming* that gun ownership affects crime rates only after a one year lag, and has no immediate impact, rather than empirically demonstrating this to be so. If the assumption is wrong, gun levels are endogenous, and using ordinary least squares regression methods to estimate the model will result in biased and inconsistent estimates of the effect of gun levels (Maddala, 1992).

This tactic also does not solve the main statistical problem that results if gun levels are endogenous, i.e. affected by crime rates - the correlation of the gun variable with the error term for the homicide equation. Gun ownership levels are quite similar from one year to the next, so the lagged gun variable is highly correlated with the unlagged (current) gun variable. Therefore, if the unlagged version of the gun variable is correlated with the error term, the lagged version is also likely to be so correlated, and thus the statistical problem created by endogeneity of gun levels is not solved by simply lagging gun levels.

Another inadequate response is to use instrumental variables methods, but improperly, without using instrumental variables that are relevant and valid (e.g., Cook & Ludwig, 2003; Hoskins, 2001; Kleck, 1979). For example, Cook and Ludwig used "percent rural" as an instrumental variable in an analysis of the impact of gun levels on burglary rates, but it could only function as an instrument if it not only affected gun rates (i.e., was "relevant") but also had no effect on burglary rates (i.e., was "valid"). While the former may be true, the latter almost certainly is not.

This issue turns out to matter a great deal, since exactly opposite results have been obtained depending on how researchers responded to the problem. The few researchers who have applied appropriate methodological responses to the causal order problem, applying variants of instrumental variables approaches, and using relevant and valid instrumental variables, have all found no positive effect of gun levels on crime rates, including homicide rates (Kleck & Patterson, 1993; Kovandzic et al., 2012, 2013).

How not to study the effect of gun prevalence on crime rates – A recent example

A recent study serves as a good example of just how badly research can go wrong, despite decades of scholarly discussion and cautions about the key mistakes that a careful researcher must avoid in order to obtain meaningful estimates of the effect of gun levels on homicide rates (e.g., Kleck, 1979; National Research Council, 2004). Siegel et al. (2014) published, in a prestigious public health journal, a study of the impact of gun ownership on the homicide rates of the 50 states, studied over the period 1981–2010. Since the study was longitudinal, this meant the authors had to have a measure of gun prevalence that was valid for measuring changes over time. The authors apparently did not understand this, or simply assumed that a proxy that was valid for measuring differences in gun levels across areas must also be valid for measuring changes over time. As previously noted, this is clearly not true. They appeared to believe this despite having read a study (Kleck, 2004 – their source 47) that had demonstrated the absence of longitudinal validity for the very proxies they used. Operating under this erroneous assumption, the authors supported their choices of gun proxies by citing research that established only cross-sectional validity (p. 1914, their sources 4, 6, 9, 46, 48–50). The proxies they used in fact have no validity for measuring changes in gun prevalence – year-to-year changes in these variables have no significant association with year-to-year changes in gun prevalence as directly measured by surveys (Kleck, 2004, pp. 19–26). Consequently, given their lack of a valid measure of gun levels, these authors failed to even establish a valid statistical association between gun levels and homicide rates.

Further, there was no affirmative evidence in their article that the authors controlled for even a single confounding variable. Although they reported that their statistical models included no less than 18 control variables, they did not present any evidence that any of these variables were significantly associated with homicide rates, never mind whether they were also correlated with gun prevalence. It is an unfortunate custom in many public health and medical journals to not routinely report parameter estimates for control variables, presumably to save journal space. Regardless of the reason, the result is that there is no affirmative evidence that any of the control variables in the Siegel et al. study were confounders, since a confounder must at minimum be significantly related to the dependent variable. This same objection applies to most of the research reports on this topic published in medical or public health journals – the customs of these journals make it impossible for their readers to judge the adequacy of the control variables used by authors, and thus the ability of their methods to rule out spuriousness.

The authors' choice of control variables was also highly arbitrary. Their sole stated justification for including their particular set of control variables was that they had "been identified as related to homicide rates and might also be related to firearm ownership rates" (p. 1914). This vague condition could have been satisfied even if they knew of just one study that found an association between the control variable and homicide rates, leaving open the possibility that a much larger number of studies found these control variables to *not* be related to homicide rates. In fact, at least half of their control variables have not been found to consistently have a significant association with homicide rates in prior research - % age15-29, % male 15–29, education levels, unemployment rates, median household income, per capita alcohol consumption, and the suicide rate (e.g., see Marvell & Moody, 1991 regarding the two age variables; Duggan, 2001 regarding per capita income; Kleck, 1979, pp. 892, 899 regarding per capita alcohol consumption; Fagan & Freeman, 1999 regarding unemployment rates). In fact, only seven of the explicitly measured control variables (excluding year and region dummies) were significant at the .05 level (2-tailed) (Siegel, 2014). Still, this total is considerably better than that for most other studies. Similarly, Duggan (2001) never controlled for more than three explicitly measured control variables, and all three of these had no significant association with crime rates. While he indirectly controlled for some possible confounders by including "fixed effects" dummy variables for years, he carried out the fixed effects methodology only halfway, failing to include dummy variables for areas (see Moody, 2010 for an extended critique of this study).

The overall patterns of findings

The patterns of findings on the effect of gun levels on crime rates are summarized in Table 3. The left half of the tables pertains to all findings regarding all crime types, while the right half pertains only to homicide findings. A separate focus on homicides is justified by the fact that it is the most serious of the crimes, and the crime type most often studied in this body of research. The 41 studies generated 90 distinct findings, 40 of which pertained to homicide. There is no point to providing separate tabulations for any other crime type, since no other crime type yielded more than ten findings, and there was virtually no variation in the non-homicide findings – nearly all indicated that gun levels did not have a significant positive effect (Table 1).

The first thing to note about Table 3 is that it indicates that the overall quality of this body of research is poor, with many primitive studies and a handful of more sophisticated ones. Of the 90 total findings, only 28 (31%) were based on valid measures of gun prevalence, only six (7%) were based on appropriate methods to address causal order (instrumental variables methods, using instruments demonstrated to be relevant and valid), and only eleven (12%) controlled for more than five statistically significant control variables. Only four findings (8%) were produced by research that met all three conditions for establish a causal effect and only ten were produced by research that met two or more of the conditions.

Of the 90 tests of the hypothesis, 26 (29%) found significant positive associations between gun levels and crime rates. The primary focus of research in this area, however, is on homicide. Of 40 findings regarding homicide, 21 (52%) were positive and significant at the .05 level. Thus, most findings appear to support the hypothesis that higher gun rates cause higher homicide rates. Once one takes account of differences in fundamental methodological flaws in the research, a very different pattern emerges. The findings of lower quality studies are diametrically opposed to those of higher quality studies. When researchers used an invalid measure of gun prevalence, 62% of the homicide findings were positive and significant, but when a valid gun measure was used, only 36% of the homicide findings were positive and significant. Of the 37 homicide findings generated by studies failing to use appropriate methods for addressing causal order, 57% were positive and significant, but *none* of the homicide findings generated by studies using proper causal order methods supported it. When researchers controlled five or fewer significant control variables, 59% of the homicide findings were positive and significant, but when more than five significant control variables were controlled, only 17% of the findings were positive and significant. Finally, there were only three studies that used a valid gun measure, *and* controlled for more than five significant control variable, *and* addressed the causal order issue with appropriate methods. None of these methodologically stronger studies supported the hypothesis. Conversely, among studies that failed to properly deal with *any* of these three fundamental problems, 65% of the homicide findings supported the hypothesis. The overall pattern is very clear – the more methodologically adequate research is, the less likely it is to support the more guns-more crime hypothesis.

These patterns are not likely to coincidental, since each of the flaws can bias findings in favor of a misleading positive guns/violence association. Failing to properly model causal order leads researchers to misinterpret the well-documented positive effects of crime rates on gun rates as a positive effect of gun rates on crime rates (see Kovandzic et al., 2012, 2013 for direct demonstrations). Failing to control for confounders that have a positive effect on crime rates but are also positively associated with gun rates (such as a pro-violence culture) leads to an upward omitted variables bias in estimates of the effect of gun levels on crime rates. And using invalid measures of gun prevalence that actually measure pro-violence culture or some other factor with a positive

Table 3
Patterns in the Findings on the Effect of Gun Levels on Crime Rates

	All Crime Types			Homicide Only		
	Total Findings	Significant Positive Effect of Gun Levels on Crime Rates?		Total Findings	Significant Positive Effect of Gun Levels on Crime Rates?	
		Yes	No		Yes	No
All Studies	90	26 (29%)	64 (71%)	40	21 (52%)	19 (48%)
Used Validated Measure of Gun Prevalence[a]	28	5 (18%)	23 (82%)	14	5 (36%)	9 64%)
Did Not Use Validated Measure of Gun Prevalence	62	21 (34%)	41 (66%)	26	16 (62%)	10 (38%)
Used Appropriate Causal Order Methods[b]	6	0 (0%)	6 (100%)	3	0 (0%)	3 (100%)
Did Not Use Appropriate Causal Order Methods	84	26 (31%)	58 (69%)	37	21 (57%)	16 (43%)
Controlled for > 5 Significant Control Variables	11	1 (9%)	10 (91%)	6	1 (17%)	5 (83%)
Controlled for 5 or Fewer Significant Control Variables	79	25 (32%)	54 (68%)	34	20 (59%)	14 (41%)
Number of Methodological Conditions Met:						
0	58	19 (33%)	39 (67%)	23	15 (65%)	8 (35%)
1	22	7 (32%)	15 (68%)	14	6 (43%) 8	(57%)
2	6	0 (0%)	6 (100%)	0	0 (–)	0(–)
3	4	0 (0%)	4 (100%)	3	0 (0%)	3 (100%)

Notes:
a. Used (1) percent of suicides committed with guns, in a cross-sectional study, or (2) direct survey measures of gun prevalence. Denoted by bold type in Table 1.
b. Used instrumental variables methods with instruments demonstrated to be relevant and valid. Denoted by a "Yes" in the "Causal Order" column of Table 1.

effect on crime rates leads to researchers misinterpreting effects of these other factors as effects of gun prevalence.

Conclusions

To summarize, the only prior research that supports the hypothesis that higher gun ownership rates cause higher crime rates is research that makes at least one, and usually all of, the three fundamental methodological errors identified here. Conversely, research that avoids or minimizes these flaws consistently finds no support for the hypothesis.

It is clear that researchers have not failed to solve all three methodological problems because it is impossible to do so. Kovandzic et al. (2013) used a valid measure of gun prevalence (the percent of suicides committed with guns, used in a cross-sectional study), controlled for a large number of potential confounding variables, and used statistically defensible methods for disentangling causal order. They used instrumental variables methods to model the possible two-way relationship between guns and homicide, and carefully tested the relevance and validity of the instrument variables they used. They directly demonstrated how much difference it makes in the results whether one properly models the possible two-way relationship between gun levels and homicide rates. When this issue was ignored, and it was simply assumed that homicide rates could have no immediate effect on gun acquisition, the results seemed to indicate a significant homicide-increasing effect of gun prevalence. Once the model was modified to take account of the contemporaneous effects of homicide rates on gun ownership, however, these apparent effects of gun levels completely disappeared, and even reversed slightly. That is, the prevalence of gun ownership indicated a small but statistically significant negative effect on homicide rates. Failing to properly treat gun prevalence as an "endogenous" variable (in this case, a variable affected by homicide rates) creates a misleading impression that higher gun prevalence leads to higher homicide rates, when the best available evidence indicates that the reverse is true – higher violence rates cause higher gun ownership rates. In sum, findings indicating that higher gun levels cause higher crime rates, including homicide rates, appear to be the product of flaws in the methods used.

Further, the ability to solve these problems is not a recent development. As far back as 1993, Kleck and Patterson used a valid measure of gun prevalence (percent of suicides committed with guns, applied in a cross-sectional context), controlled for numerous significant control variables, and addressed the causal order problem by using instrumental variables. They demonstrated the relevance of their instrumental variables, showing that they significantly predicted gun prevalence (p. 267). Although they did not report a test of the validity of these instruments (whether they could be legitimately excluded from the crime rate equations), their validity was demonstrated in later research using similar instruments (Kovandzic et al., 2012, 2013). A still earlier study (Kleck, 1979) also used instrumental variable methods, but it is doubtful whether the instruments used (median family income, and lagged gun levels) were valid.

In this light, it is something of a mystery why so many poor quality studies on this topic continue to be published, despite their conspicuous failures to solve any of the most fundamental methodological problems. At minimum, the work by Kleck and Patterson (1993) and Kovandzic et al. (2012, 2013) demonstrated that the problems identified herein are amenable to credible solutions, and that research that makes a serious effort to solve these problems arrives at conclusions exactly opposite to those drawn in studies that do not make such efforts. While numerous poor quality studies point to crime-increasing effects of higher gun ownership rates, no methodologically adequate studies have done so.

Why does gun prevalence not have a significant positive effect on homicide? The most likely explanation is that (a) most guns are possessed by noncriminals whose only involvement in crime is as victims, and (b) defensive gun use by crime victims is both common (see the 19 surveys summarized in Kleck, 2001) and effective in preventing the offender from injuring the victim (Kleck & Delone, 1993; Southwick, 2000; Tark & Kleck, 2004). These violence reducing-effects of guns in the hands of victims may roughly cancel out the violence-increasing effects of guns in the hands of offenders, resulting in a near-zero net effect on homicide rates (Kovandzic et al., 2012, 2013).

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