Macro-Level Research on the Effect of Firearms Prevalence on Suicide Rates: A Systematic Review and New Evidence*

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Objective. To systematically review the methods and findings of previous macro-level research on the effect of firearms prevalence on suicide rates, and carry out a better state-level analysis. *Methods.* A cross-sectional model of suicide rates is estimated with weighted least squares, using direct survey measures of gun prevalence. *Results and Conclusion.* Prior macro-level research is afflicted by the use of small samples of large heterogeneous units, invalid measures of gun prevalence, and few controls for confounders. The methodologically soundest prior research indicates that gun prevalence affects rates of gun suicides, but not total suicides. The new analysis likewise finds no significant effect of gun prevalence on total suicide rates.

Does more widespread gun ownership cause more suicide? No one disputes that availability of a gun is a logical necessity for committing a *gun* suicide, or that more widespread gun ownership is therefore likely to cause a higher share of suicides to be committed with guns. The issue that matters from the standpoint of the public's well-being, however, is whether higher gun prevalence causes more people to kill themselves. This issue is, of course, intimately tied up with the issue of gun control, whose advocates assert that stricter gun laws will reduce suicide because they will reduce gun availability to suicide-prone persons, causing fewer of them to die (e.g., Miller, Azrael, and Hemenway, 2002c).

Strictly speaking, reducing *firearms* suicides is not, in and of itself, a public benefit. If a gun law caused the number of firearms suicides to decline by 50, but also caused the number of suicides committed by other lethal methods to increase by 50, there would be no net savings in lives. And unless one were willing to argue that there is public benefit in getting people to kill themselves by hanging (or other nonshooting methods) instead of shooting, there would be no suicide-related benefit from this law. Thus, it is inconsequential by itself if more guns cause more *gun* suicides, but very important if more guns cause more *total* suicides.

The most commonly cited reason why higher gun prevalence would affect the total number of suicides, and not just the share committed with firearms, is the purportedly greater lethality of shooting attempts compared with that of methods likely to be substituted for shooting (e.g., Kubrin and Wadsworth, 2009; Kposowa, Hamilton, and Wang, 2016). National data, however, indicate that the incident fatality rates of shooting suicide attempts are not significantly higher than attempts by hanging, the second-most common method of suicide in the United States (Kleck, 2018). Gun prevalence is also unrelated to rates of major depression or suicidal thoughts (Hemenway and Miller, 2002), so it remains uncertain why more guns would cause more suicide.

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Methodological Issues

The macro-level research on the effect of gun rates on suicide rates has been characterized by at least three major problems: (1) use of invalid or contaminated measures of gun ownership prevalence, (2) the use of needlessly small samples of macro-level units, and (3) the failure to control any significant number of confounding variables.

Invalid or Contaminated Measures of Gun Ownership Prevalence

"Contaminated" measures refer to variables intended to measure gun prevalence but that contain large components that are also found in the dependent variable, the suicide rate. Suicides can, for our purposes, be broken down into two types: (1) firearm suicides (FS) and (2) nonfirearm suicides (NF). One measure of gun prevalence often used in this body of research is the percent of suicides committed with guns (PSG), which can be computed as FS/(FS+NF). This is an excellent measure to use in cross-sectional analyses of violence and crime rates other than suicide rates (Kleck, 2004), but should not be used in suicide analyses because it creates an overlap between the suspected cause, gun rates, and the hypothesized effect, suicide rates. The dependent variable in models of the firearms suicide rate is FS/population, while the dependent variable in models of the total suicide rate is (FS+NF)/population. Thus, when researchers use the PSG measure (FS/(FS+NF)) as their measure of gun prevalence, it overlaps with the dependent variable for purely artificial reasons, as a result of the analyst's choice of a proxy measure of gun prevalence.

When the researcher is analyzing whether gun levels affect the firearm suicide rate, FS is the sole component in the numerator of the independent variable (gun prevalence) *and* also the sole component of the numerator of the dependent variable (firearms suicide rate). Even if there was no causal effect of gun prevalence on the gun suicide rate, any errors in counting up gun suicides would tend to create a positive association simply because the amount of error would be exactly the same in the numerators of the two variables. The problem is only slightly less severe when the dependent variable is the total suicide rate. Because most U.S. suicides are committed with guns (52 percent in 1999–2014—Centers for Disease Control and Prevention (CDC), 2016), FS is the *main* component of the numerator of the dependent variable (the total suicide rate), as well as the *sole* component of the numerator of PSG, the measure of gun prevalence.

Miller, Azrael, and Hemenway (2002a, 2002b) claimed to have shown that this is not really a problem, performing a Monte Carlo simulation that supposedly showed the common components problem does not distort estimates of the effect of gun prevalence on suicide rates. The National Research Council's Committee to Improve Research Information and Data on Firearms reviewed the work of Miller and his colleagues and concluded that they had failed to provide enough information about how they conducted their simulation for anyone to replicate or evaluate it. The Committee then performed its own simulation, demonstrating that estimates of the effect of gun prevalence could indeed be distorted by use of the PSG measure in a suicide analysis (2005:169–70).

Thus, using PSG to measure gun prevalence in an analysis of suicide rates is inappropriate. Some scholars have been careful to avoid the common components problem. For example, Kleck and Patterson used a five-item index to measure gun prevalence in their analyses of crime rates, which included PSG as one of its components, but when the dependent variable was the suicide rate they took care to omit PSG (1993: 263). Other scholars, however, used PSG as their sole measure of gun prevalence (Miller, Azrael, and Hemenway, 2002c; Kubrin and Wadsworth, 2009) or used it as one of two components in a combined measure (Lester, 1988b; Miller, Azrael, and Hemenway, 2002b; Siegel and Rothman, 2016). In Table 1, contaminated measures are displayed in italics.

It has also been repeatedly demonstrated that PSG has no validity for measuring changes over time in gun prevalence (Kleck, 2004; Kovandzic, Schaffer, and Kleck, 2013). Therefore, it cannot be used in panel, time-series, or other longitudinal research designs, though some researchers have used it in research using a panel design anyway (Miller, Azrael, and Hemenway, 2002b, 2002c; Siegel and Rothman, 2016).

Some have used strictness of gun control laws as proxies for gun prevalence but this is an extremely indirect measure that has far too weak a correlation with direct survey measures of gun prevalence to serve as an effective proxy. For example, across states, Lester (1988a) found that an index of the strictness of gun control laws had only a −0.49 correlation with a survey-based measure of household gun prevalence, implying that only 24 percent of the variation in gun prevalence is shared with gun control strictness ($1-[-0.49^2] = 0.24$). Other measures used as proxies are also known to have only weak correlations with gun prevalence, such as the hunting license rate (Kleck, 2004:9; $r = 0.37$, $r^2 = 0.14$) or the rate of subscriptions to gun-related magazines ($r = 0.34$–0.49; Kleck, 2004:14).

A better approach is to use multiple indicators of gun prevalence combined together in a factor score. Even if each one indicator has only a modest correlation with gun prevalence, a multi-item factor score can have a strong correlation. Kleck (1991) and Kleck and Patterson (1993) combined (1) the percent of homicides committed with guns, (2) the percent of robberies committed with guns, (3) the percent of aggravated assaults committed with guns, and (4) the percent of stolen property dollar value attributable to stolen firearms into an index.

Survey-based measures of gun prevalence are generally desirable because they are much more direct measures of prevalence than the aforementioned proxies. Most existing survey measures have nevertheless been inadequate for use in panel studies because the numbers of respondents interviewed in any one year in each area are inadequate to establish statistically significant changes in gun prevalence. For example, in the region-level panel studies conducted by Birckmayer and Hemenway (2001) and Miller, Azrael, and Hemenway (2002a, 2002c) the average number of respondents per region each year in the General Social Surveys was only about 111, so only enormous (and highly unlikely) year-to-year changes of 13 percentage points or more would be statistically significant (Kleck, 2004).

Table 1 summarizes key methodological attributes of 32 analyses of the effect of gun levels on suicide rates, reported in 29 different studies. Of the 32 analyses, 20 used either survey measures of household gun prevalence or multi-item indexes devoid of contamination by inclusion of a suicide component. The remaining 12 analyses used proxy measures that were contaminated or unreliable measures of gun prevalence, or measured changes in gun levels using inadequate sample sizes.

Small Samples of Highly Aggregated, Internally Heterogeneous Cases

This body of research is remarkable for how many studies were based on sample sizes that, even compared with other macro-level studies, were extraordinarily small, even if one excludes subscientific studies based on comparisons of a single pair of cases (e.g., Sloan et al., 1990; Killias, 1990). Cross-sectional analyses of U.S. regions have been based on just nine cases (Markush and Bartolucci, 1984; Lester, 1988a; Kaplan and Geling, 1998), an analysis of Canadian provinces was based on nine provinces (Moyer and Carrington,

TABLE 1

Macro-Level Studies of the Association Between Gun Ownership Levels and Suicide Rates

Study	Sample	Number of Controls: Total	Number of Controls: Significant	Measure of Gun Levels[a]	Significant Association with: Gun Suicide?	Significant Association with: Total Suicide?	Significant Control Variables
Markush and Bartolucci (1984)	9 U.S. regions, 973–977	0	0	S	Yes	No[b]	
Lester (1987)	48 states, 1970	0	0	FGA, PHG	Yes	No	
Lester (1988a)	9 U.S. regions, 1970	3	3	S	-	No	Divorce rate, % black, handgun controls
Lester (1988b)	48 states, 1970	0	0	*PSG, magazine subs*	Yes	-	
Lester (1988c)	6 Australian states	0	0	S	No	No	
Clarke and Jones (1989)	13/26 years[c]	0	0	S	Yes[d]	No	
Lester (1989)	48 states, 1980	0	0	Magazine subs	Yes	Yes	
Lester (1990)	20 nations	0	0	PHG	Yes	No[e]	
Kleck (1991)	170 cities	24	5	4-item index	No	No	Divorce rate, % transient, alcoholism, density, hospital beds
Moyer and Carrington (1992)	10 Canada provinces	1	0	S	No	No[f]	
Killias (1993)	16 nations	0	0	S	Yes	No	
Kleck and Patterson (1993)	170 cities	21	7	4-item index	2-way: No	No[g]	Divorce rate, % transient, alcoholism, density, % live alone

Continued

TABLE 1

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Study	Sample	Number of Controls: Total	Number of Controls: Significant	Measure of Gun Levels[a]	Significant Association with: Gun Suicide?	Significant Association with: Total Suicide?	Significant Control Variables
					1-way: No/Yes	No/Yes[g]	% 65+, gun dealer licenses
Kaplan and Geling (1998)	9 regions	0	0	S	Yes	-	
Lester (2000)	Canada, 26 years	0	0	FGA	No	No	
Birckmayer and Hemenway (2001)	9 regions × 16 years	4	2	S	Yes	Yes	
Killias, van Kesteren, and Rindlisbacher (2001)	21 nations	0	0	S	Yes	No	
Miller, Azrael, and Hemenway (2002a)	50 states x 10 years	3	0	S	Yes	Yes	
(5–14-yr. olds only)	9 regions × 10 years	0	0	S	Yes	Yes	
Miller, Azrael, and Hemenway (2002b)	50 states × 10 years	2	0	*PSG, PHG*	Yes	Yes	
(female suicide only)	9 regions × 10 years	0	0	S	Yes	No	
Miller, Azrael, and Hemenway (2002c)	9 regions × 10 years	0	0	S	Yes	Yes	
			50 states × 10 years	6	0	*PSG*	
Smith and Stevens (2003)	2003 14 nations	0	0	S	Yes	No	
Miller, Azrael, and Hemenway (2004)	7 NE states	0	0	S	Yes	Yes	
Miller et al. (2006)	22 years, United States	00	S	Yes	Yes		

Continued

TABLE 1

Continued

Study	Sample	Number of Controls		Measure of Gun Levels[a]	Significant Association with:		Significant Control Variables
		Total	Significant		Gun Suicide?	Total Suicide?	
Miller et al. (2007)	50 states	6	0	S	Yes	Yes	
Kubrin and Wadsworth (2009)	179 U.S. cities	5	1–2	*PSG*	-	Yes	Northeast, disadvantage
Miller et al. (2013)	44 metro areas	0	0	S	Yes	Yes	
Kpsowa et al. (2016)	50 states, c. 2012	7	1–3	S	Yes	Yes	Divorce rate, gun control strictness, long-term unemployment
Siegel and Rothman (2016)	50 states × 33 years	14	6?[h]	*PSG/Hunt*	Males: Yes	Yes	Divorce, West, alcohol consumption, violent crime
					Females: Yes	No	Population density, gross domestic product

NOTE: Table covers only studies in which gun ownership levels were actually measured. It does not include studies that merely assume a guns-suicide association but did not actually estimate one (e.g., Farmer and Rohde, 1980; Boor, 1981; Boyd, 1983; Boyd and Mowscicki, 1986; Wintemute, 1987; Cantor and Lewin, 1990; Dudley et al., 1992), nor does it cover studies that merely compared a single pair of nations or cities (e.g., Sloan et al., 1990; Killias, 1990).

[a]S = survey measure of percent of households with guns; Magazine Subs = subscription rates for gun magazines; PSG = percent of suicides committed with guns; PHG = percent of homicides committed with guns; FGA = fatal gun accident rate; Hunt = hunting license rate. Gun measures contaminated by a suicide component (see text) are shown in italics.

[b]Significant positive correlation was only found if eccentric weighting scheme was used. Conventional unweighted results indicated no significant association.

[c]Time series data set included 26 years total, but only 13 had real data on gun ownership levels; the rest were interpolations.

[d]Handgun prevalence related to suicide rates, total gun prevalence unrelated.

[e]Lester did not report this result. It was computed from his published data.

[f]Both bivariate and multivariate associations of guns with total suicide were insignificant, for both sets of years authors used. Authors reported erroneous significance level for multivariate association for 1987–1989 data—it was 0.08, not 0.008.

[g]Association was barely significant ($p = 0.04$) when (1) a one-way relationship was assumed, and (2) the suicide rate was logged; not significant when a two-way association was assumed, or the suicide rate was not logged.

[h]The authors ambiguously stated in their text that six control variables "consistently predicted" suicide rates but did not state that the associations were significant, or that they applied to total suicide rates rather than just firearms suicide rates. Further, they did not document these associations by reporting any parameter estimates and standard errors.

1992), and cross-national studies were based on 14 to 21 nations (Lester, 1990; Killias, 1993; Killias, van Kesteren, and Rindlisbacher, 2001; Smith and Stevens, 2003). Miller, Azrael, and Hemenway (2004) even performed a cross-sectional analysis of a nonrandomly selected sample of just *seven* states, while Lester (1988c) analyzed just *six* Australian states.

The problem with such tiny samples is that key results can change radically if just one or two cases had not been included in the sample, or just one or two additional cases had been included. For example, Killias, van Kesteren, and Rindlisbacher found that the cross-national association between survey-based measures of household gun prevalence and the total suicide rate was a nonsignificant 0.10 in the full sample of 21 nations, but more than tripled to 0.36 when just a single nation was dropped from the sample (2001:436). Unfortunately, readers of these studies are virtually never informed as to just how unstable the results were since, unlike Killias, van Kesteren, and Rindlisbacher (2001), their authors did not report any sensitivity checks by reestimating their findings after deleting one or two cases.

Use of extremely small samples also makes it impractical for researchers to control for more than a very few potential confounding variables because it reduces the number of degrees of freedom so much and makes estimates of coefficients wildly unstable depending on which confounders are controlled. Authors of these studies might protest that they cannot help it if there are only nine Census regions in the United States or just 21 nations for which surveys have asked questions about gun ownership, but such an argument is disingenuous since the authors did not have to use such units.

High aggregation also increases aggregation bias. Findings obtained at high levels of aggregation do not necessarily apply to lower levels of aggregation such as cities, counties, or individual persons. Thus, one could find that states with higher gun levels also have higher suicide rates, even though analysis of counties would show that the counties with high gun ownership were not the places that had high suicide rates. This is more likely to happen with very aggregated units because the higher the level of aggregation, the more internally heterogeneous the cases tend to be. Thus, the best units of analysis to use in macro-level analyses, if relevant data are available, would be the smallest units.

Table 1 shows that only three studies (Kleck, 1991; Kleck and Patterson, 1993; Kubrin and Wadsworth, 2009) used cities or counties as units of analysis, while eight studies used the very large and heterogeneous U.S. regions (e.g., Birckmayer and Hemenway, 2001; Miller et al., 2002a, 2002c) and four studies analyzed nations (Lester, 1990; Killias, 1993; Killias, van Kesteren, and Rindlisbacher, 2001; Smith and Stevens, 2003).

Inadequate Controls for Confounding Variables

Probably the most consequential flaw in research in this area is the near-total failure to adequately control for confounding variables. A confounding variable has both of two properties: (1) it affects the dependent variable and (2) is correlated with the independent variable of interest. In this area of research, only control variables that both affect suicide rates and are correlated with gun prevalence are confounders. Controlling for variables that are not confounding variables does nothing to improve the estimate of one variable's causal effect on another. Researchers who fail to control for confounder variables will obtain biased estimates of the effect of the target variable (gun ownership levels) on the dependent variable (suicide rates). The more confounders omitted, the worse the bias is likely to be.

The summary in Table 1 shows that in 26 of 32 analyses, the researchers did not control for a single variable that was shown to be significantly related to suicide rates (e.g., Miller, Azrael, and Hemenway, 2002c, 2004; Miller et al., 2006). In many studies the researchers controlled for multiple variables, but did not report any estimates of the coefficients, standard errors, or significance levels of the control variables, and thus did not document that any of the control variables were significantly related to suicide rates (e.g., Miller, Azrael, and Hemenway, 2002c, 2004; Miller et al., 2006). Most of the remaining studies were only marginally better. For example, Birckmayer and Hemenway (2001) controlled for a total of four variables, but only two of the variables were significantly related to suicide rates, and one of these ("education") is not correlated with gun rates at the state level. Thus, they actually controlled for just a single confounder. Likewise, Kubrin and Wadsworth (2009) controlled for five variables, but only one or two of them (depending on which model one considers) were significantly related to suicide rates. Only two studies controlled for more than three variables that were shown to be significantly related to suicide rates. Kleck (1991) and Kleck and Patterson (1993) controlled for eight significant control variables in city-level analyses. It is fair to say that, with these two exceptions, researchers in this area have largely failed to control for confounding variables, and their estimates of the impact of gun levels on suicide rates are therefore almost certainly biased.

The Biasing Effect of Omitted Confounders on Estimates of Gun Effects on Suicide Rates

Table 2 demonstrates just how profound the distorting effect of failures to control confounders can be. Miller et al. (2007) reported controlling for six variables besides the gun ownership level, seeming to imply that they had controlled for six confounders. They did not, however, report parameter estimates for any of the control variables. I reconstructed their data set and estimated the same model of suicide rates that they did. I found that only *one* of their control variables was significantly related to suicide rates (use of illicit drugs other than marijuana), and that lone variable was not correlated with gun ownership rates (Table 2, $r = -0.002$). Thus, the authors had actually done nothing to control for confounders, while giving their readers the impression that they controlled for as many as six confounders (Table 2, Model 1). When I reestimated the model with no control variables at all included, that is, with only the gun ownership variable, its coefficient was roughly the same ($b = 0.015$) as what it was in the authors' multivariate model (0.019) (Table 2, Model 2). That is, the authors might just as well have included none of their control variables for all the difference it made in their estimate of the effect of gun levels.

I then estimated a revised model in which I included five genuine confounders, that is, variables significantly related to both suicide rates and gun ownership rates. Once a few actual confounders were controlled, any appearance that gun levels increase the total suicide rate disappeared—the coefficient for the gun ownership variable was not significantly different from zero, or even close to it (Table 2, Model 3). Thus, the authors' principal finding was highly sensitive to which variables they controlled, and their choice of control variables was as poor as it could possibly have been. The appearance of an effect of gun levels on suicide rates was an artificial product of the authors' failure to control for any confounding variables.

Likely Confounders that Need to Be Controlled

Using a data set describing states as they were in 2000, we looked for variables that were correlated with gun ownership levels and were also significantly related to the total suicide rate in a multivariate model that included the full set of 14 possible confounders, regardless

TABLE 2

An Example of the Effects of Failing to Control for Confounders: Results from Alternative Versions of Models of State Suicide Rates in Miller et al. (2007)

Model Number Variable	(1) Miller et al. (2007) Model	(2) Guns-Only Model	(3) Improved Model
% Households reporting guns, 2001	0.019 (4.96)	0.015 (6.83)	0.003 (0.98)
% Civilian labor force unemployed, 2000	−0.002 (0.78)		
% Living in urban areas, 2000	0.002 (0.68)		
% Under the poverty line, 2000	−0.011 (−0.82)		0.025 (4.02)
% Adults suffering from serious mental illness, 2002	−0.015 (−0.51)		
% Reporting alcohol dependence in 2001	0.001 (0.02)		
% Reporting illicit drug use besides marijuana, 2002	0.182 (3.18)		
State is in West region			0.101 (2.08)
% Born in same state as current residence, 2000			−0.011 (−7.27)
% Catholic, 1990			−0.004 (−2.29)
% African American, 2000			−0.008 (−3.68)
% Foreign born, 2000			−0.025 (−6.28)
Constant	1.107	1.891	2.701
R_A^2	0.558	0.482	0.876

NOTE: Dependent variable is natural log of total suicide rate for 1999–2002. Weighted least squares estimates, based on 50 states.

of their significance. Table 3 reports their multivariate associations with the total suicide rate and their bivariate correlations with gun ownership levels.

The last column indicates the likely biasing effect of failing to control for the variable. Although it is not possible to be certain what the effect would be in a fully specified multivariate model, these signs represent what the biasing effect would be in a model in which two variables affected suicide rates, gun ownership and the confounder. For example, the divorce rate (the confounder) positively affects suicide rates and is also positively correlated with gun levels. A researcher who omitted the divorce rate from the model would wrongly attribute to gun levels a suicide-elevating effect that was actually due to higher divorce rates, biasing the estimated effect of gun levels upward. A simple way to derive this prediction is to note the signs (positive or negative) of the associations of a given potential confounder with gun levels and with the suicide rate. If the signs are the same (either both positive or both negative), the biasing effect tends to be positive; if the signs are different, the biasing effect tends to be negative.

Table 3, in conjunction with Table 1, indicates that there are many likely confounders that have not been controlled in prior research, and that many of the variables that have

TABLE 3

Some Possible Confounders of the State-Level Guns-Suicide Relationship

Variable	Correlation with: Gun Ownership *R*	*p*	Coefficient in Full Model of Total Suicide Rate *b*	One-Tailed Signif. *p*	Likely Sign of Biasing Effect
% Foreign-born	−0.718	0.00	−0.234	0.00	+
Divorce rate	0.309	0.03	0.417	0.03	+
% Moved past 5 years	0.198	0.17	0.244	0.00	+
Veterans/100k pop	−0.004	0.98	0.040	0.10	−
West region	0.140	0.33	1.604	0.04	+
% Below poverty line	0.371	0.01	0.054	0.32	+
% African American	−0.049	0.37	−0.053	0.08	+
% Catholic	−0.655	0.00	−0.019	0.30	+
% Age 65+	−0.180	0.21	0.206	0.08	−
Alcohol abuse	187	0.19	0.331	0.13	+
Unemployment	0.146	0.31	0.743	0.04	+
% Mentally ill	0.599	0.00	−0.274	0.14	−
% Live alone	0.032	0.82	0.090	0.40	+
Serious drug use	−0.002	0.99	0.231	0.36	−

NOTES: Sample is 50 states, as of c. 2000, weighted by square root of resident population. Dependent variable = Annual average number of suicides, 1999–2002 per 100,000 resident population. "Full model" is one that includes all the variables listed above, regardless of significance. "Likely sign of biasing effect" is the effect on the gun prevalence coefficient of omitting the indicated variable from a model of state suicide rates. A plus sign means the coefficient is likely to be biased upward by the omission, a negative sign means it is likely to be biased downwards.

been controlled in prior research are probably not confounders. Further, the biasing effect of failing to control for the variables that probably are confounders is positive for 10 of 14 of them. That is, failing to control them leads to estimates of the effect of gun levels that are too high.

Overall Patterns of Findings

The summary of findings in Table 1 indicates that most analyses find a significant positive association between firearms prevalence and the rate of *firearms* suicide, consistent with the view that where guns are more widely available, more people will commit suicide *with guns*. On the other hand, the literature appears to be evenly split on the issue of whether firearm prevalence affects the *total* suicide rate—15 of 29 analyses did not find any significant association of firearms prevalence with the total suicide rate. This appearance is misleading. Research done by Matthew Miller, Deborah Azrael, and David Hemenway (MAH) almost invariably (10 of their 11 findings) yielded a significant positive association between gun levels and total suicide rates, while the rest of the research community has generally found *no* significant association. MAH contributed two-thirds of the findings of a significant positive association between firearm prevalence and the total suicide rate.

TABLE 4

Weighted Least Squares Estimates of a More Plausible State-Level Model of Total Suicide Rates

Variable	Unstandardized Regression Coefficient	Ratio, Coefficient *SE*	One-Tailed Significance	Tolerance
% Household with guns	0.026	0.752	0.23	0.194
% Foreign born	−0.193	−3.244	0.00	0.194
Divorces/1,000 population	0.351	1.761	0.04	0.602
% Moved, 1995–2000	0.247	4.515	0.00	0.408
Veterans/1,000 population, 1999	0.062	2.909	0.02	0.542
West region	1.822	2.719	0.01	0.404
% Below poverty line	0.207	2.363	0.01	0.469
% African American	−0.406	−1.715	0.05	0.557
Constant	−8.543	−2.667	0.01	
$R_A{}^2 = 0.82$				

NOTES: Sample: 50 states as of 2000, weighted by the square root of resident population. Dependent variable: Annual average number of suicides, 1999–2002, per 100,0000 resident population.

This stark contrast cannot be attributed to the superior character of the research done by MAH, since they studied very small samples (as small as $n = 7$) of extremely large and heterogeneous areas like regions or states, used contaminated measures of gun prevalence, and make little effort to control for confounders. In seven of their 11 analyses they did not control for a single variable shown to be significantly related to total suicide rates (Table 1), while most of those that they did control in the remaining four analyses were probably not confounders (see Table 3). If we exclude studies done by MAH, prior research is overwhelmingly contrary to the proposition that higher gun levels cause higher total suicide rates.

The methodologically strongest studies done to date are those of Kleck (1991:285–86) and Kleck and Patterson (1993). These researchers (a) studied a far larger sample of areas ($n = 170$ cities) than examined in other studies, (b) which areas studied (cities) were more homogenous than states or regions, (c) used a four-item index of validated proxies for gun ownership with no common components problems, and (d) controlled for eight significant control variables. Kleck and Patterson obtained four estimates of the effect of gun levels on total suicide rates, differing from each other with regard to (1) whether they used a model incorporating two-way causation, and (2) whether the suicide rate was logged. If people living in a household with a suicide-prone person become more reluctant to acquire or retain guns when they hear about suicides, the suicide rate could have a negative effect on the prevalence of firearms, raising the possibility of two-way causation between gun rates and suicide rates. Regarding the other variation in methods, it is not clear that the suicide rate should be logged. The most common reason for doing so is to make a positively skewed variable take on a more normal distribution, but in the city-level data set suicide rates were already normally distributed without using this transformation, as is also true of state-level data. In the state-level data set used in Tables 3 and 4, the total suicide rate has skewness = 0.628 and kurtosis = 0.528, both well within the −1 to +1 range considered to be consistent with a normal distribution.

Of the four estimates yielded by the Kleck/Patterson analyses, only one supported a significant positive effect of gun levels on total suicide rates, and that one was only marginally significant. When it was assumed that the occurrence of suicides could *not* affect whether people acquired or retained guns (no two-way causation), and the suicide rate *was* logged, the coefficient for the gun level variable was marginally significant at the 0.034 level. Under the other three combinations of conditions, the estimates indicated no significant effect of gun levels on total suicide rates (Kleck, 1991:286).

This review of prior published research should be viewed in the context of the universal finding, in all scientific disciplines, that null findings are less likely to be published than those finding support for the tested hypothesis (Cooper, 2017). Thus, the body of studies reviewed here probably underrepresents findings of no effect of gun levels on total suicide rates.

Results from Estimation of a More Plausible Model of State Suicide Rates

Finally, Table 4 shows the results of a new state-level analysis of total suicide rates. The sample was the 50 states as they were in 2000. Data pertaining to 2000 had to be used because the only published state-level survey measures of household gun prevalence that were available pertained to 2002 (Okoro et al., 2005), and 2000 was the Census year closest to 2002. The model was estimated using weighted least squares methods, with the states weighted by the square root of their 2000 resident population. This weight serves to give larger states more influence on estimates. The dependent variable was the rate of total suicides per 100,000 resident population, using the annual average number of suicides in the period 1999–2002. The variable was not logged because it was normally distributed without any transformations. Results for the effect of gun prevalence were the same if the suicide rate was logged.

The specification of the set of control variables was based on our systematic review of prior research (Table 1), preliminary analysis of likely confounders (Table 3), and the elimination of variables found to have no significant multivariate association with the total suicide rate. The model is more credible than those used in prior state-level research because it controls for a substantial number (eight) of variables known *a priori* to be likely confounders and because the control variables are all shown to be significantly related to the total suicide rate. Finally, the measure of firearm prevalence was based on surveys fielded in 2002 of substantial samples (average $n = 4{,}476$) of each of the 50 states (Okoro et al., 2005), and is free of any contaminating components that would produce an artifactual association between the gun measure and the suicide rate.

The model has an excellent fit to the data, with an adjusted R-squared of 0.82 without benefit of either the use of the lagged suicide rate as a predictor or the inclusion of any predictors that overlap with the dependent variable. The tolerance statistic reflects the degree of multicollinearity among predictor variables, and was never less than 0.194 for any of the predictors. Most commonly, a value over 0.10 is recommended as the minimum tolerance level preferred (e.g., Tabachnick and Fidell, 2001), so our tolerance statistics suggest that there is no serious problem with multicollinearity.

The estimates indicate that household gun prevalence has no significant association with the total suicide rate once eight relevant variables are controlled. It is not claimed that this is the only correct model of state suicide rates, but that it is a better one than any previously used. Analysis of these state-level data confirm the city-level findings of Kleck and Patterson (1993)—gun levels do not affect the rate at which people kill themselves.

Conclusion

The methodologically weakest prior research indicates that there is an effect of gun prevalence on total suicide rates, while the technically strongest research indicates there is no effect. The customary scholarly practice is to tentatively accept the findings of the best available research. All prior studies that documented controlling for more than two significant confounders and used an uncontaminated gun measure found that gun levels are significantly related to *firearms* suicide rates but found no association with *total* suicide rates. The new analysis reported here likewise found no significant effect of gun levels on total suicide rates. Therefore, we tentatively conclude that gun prevalence affects whether suicides are committed with firearms rather than other lethal methods, but has no effect on how many people commit suicide. This conclusion echoes the assessment of the National Commission on the Causes and Prevention of Violence of a half-century ago: "There is little reason to expect that reducing the availability of firearms would cause a significant reduction in suicides" (Newton and Zimring, 1969:37).

The policy implication is that if gun control succeeded in reducing gun prevalence in the general population, it would have no effect on the number of people who killed themselves. This does not, however, rule out the possibility that policies that reduced gun prevalence among select suicide-prone subsets of the population might reduce suicides. Unfortunately, there are no such group-specific gun prevalence data currently available to test this idea.

The null findings of the present study comport with the findings of a natural quasi-experiment in Australia, which implemented what is arguably the most massive national gun control intervention ever carried out in a democracy. In 1996 the country banned all semi-automatic and pump-action rifles and shotguns (handguns were already effectively banned) and implemented a massive effort to buy back the banned guns. Although the firearms suicide rate did decline after 1996, it had already been declining before 1996. Further, the NF rate declined just as much as FS, indicating that gun-specific factors were not responsible, and that instead factors that influence suicides regardless of gun involvement were involved (Chapman, Alpers, and Jones, 2016).

States are heterogeneous units of analysis, so our results are subject to aggregation bias. Unfortunately, there are no data available on any valid measure of gun prevalence for smaller units such as counties or cities. Future macro-level research should use the smallest, most homogeneous units of analysis for which the requisite data are available, use survey or multi-item proxy measures of gun prevalence devoid of contaminating suicide components, control for as many likely confounders as possible, and test effects of gun prevalence on total suicide rates, not just the firearm suicide rate.

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