American Journal of Criminal Justice
https://doi.org/10.1007/s12103-020-09604-7

The Impact of Firearm Levels on Homicide Rates: the Effects of Controlling for Cultural Differences in Cross-National Research

Tomislav Kovandzic[1] · Gary Kleck[2]

Received: 8 August 2020 / Accepted: 11 December 2020/Published online: 04 January 2021

Abstract

We analyze the largest set of nations ($n = 55$) with a valid measure of gun ownership levels ever used to test the effect of national gun levels on homicide rates. We control for measures of national culture to better isolate the effects of firearm availability. We find that, while national gun levels have a significant positive *bivariate* correlation with homicide rates, once one controls for violence-related cultural differences between nations, the association disappears. With this larger, more diverse set of nations, the U.S. is not influential – gun levels are unrelated to homicide rates regardless of whether it is included in the analysis.

Keywords Firearms · Homicide · Cross-national

Introduction

Gun control advocates often support the need for stricter gun laws by making a pairwise comparison between the United States, a nation with both one of the highest levels of lethal violence among developed nations and the highest rate of civilian gun ownership, and a nation having both low gun ownership and low homicide rates, such as Japan or Great Britain. Advocates infer from this comparison that America's high rate of gun ownership must be at least partially responsible for the nation's high rates of

✉ Tomislav Kovandzic
tkovan@utdallas.edu

Gary Kleck
gkleck@fsu.edu

[1] Program in Criminology, University of Texas at Dallas, P.O. Box 830688, GR31, Richardson, TX 75083-0688, USA

[2] College of Criminology and Criminal Justice, Florida State University, Tallahassee, FL 32306-1127, USA

homicide (Duggan, 2001; Killias, 1993a; Sloan et al., 1988). This belief in a causal effect of gun levels on violence rates, and not merely on criminals' choice of weaponry, has likewise inclined many to conclude that making the acquisition of guns more difficult would lead to significant reductions in homicide (e.g., Clarke & Mayhew, 1988, p. 106; Zimring & Hawkins, 1997). For their part, pro-gun writers likewise "cherry pick" pairs of nations to buttress their case, selectively choosing nations with high gun ownership and low homicide rates such as Switzerland, or low gun ownership but high homicide rates such as Mexico, Brazil, or Russia, inferring that gun ownership has no significant impact on homicide, or actually lowers it (e.g., Lott, 1998; Sherrill, 1973, p. 176 [citing pro-gun witnesses at Congressional hearings]).

Pairwise comparisons of this sort provide propaganda material that can be used to influence public opinion, but a bivariate analysis based on a sample size of two has negligible evidentiary value for establishing a causal link, or the lack thereof, between any two covariates of ecological units. A few studies, however, have examined the association of gun ownership levels with homicide rates using somewhat larger, but still limited samples of nations. Eight cross-national studies of homicide rates have measured gun levels either with telephone surveys of household gun ownership handful of nations (Killias, 1993a, 1993b; Killias, van Kesteren, & Rindlisbacher, 2001) or a validated proxy of gun ownership, the percentage of suicides committed with guns (PSG) (Hemenway & Miller, 2000; Hoskin, 2001; Lester, 1996; Moody, 2010; Kleck 1997). Four studies reported significant positive associations between gun prevalence levels and homicide rates, and their authors inferred from these findings that more guns leads to more homicides, at least in highly industrialized nations ((Hemenway & Miller, 2000, p. 987; Hoskin, 2001; Killias, 1993a; Lester, 1996). On the other hand, four other analyses of cross-national data found no significant association between gun prevalence and total homicide rates (Altheimer & Boswell, 2012, p.693; Killias et al., 2001, pp. 436, 440; Kleck, 1997, p.254; Moody, 2010). Thus, no consensus in findings has been generated by prior cross-national research.

The present study addresses several questions concerning the cross-national association between gun availability and homicide rates: (1) is the association spuriously due to differences in culture across nations?, (2) is the association different if one analyzes a less restricted samples of nations, and (3) is the association sensitive to the inclusion of the U.S. in the sample?

The paper proceeds as follows. First, we provide a theoretical discussion of how differences in gun prevalence levels might affect homicide rates. Then we discuss the methodological problems encountered in prior cross-national research and strategies used in the present study to address them. Next we describe our data and methods. Then we report our estimates of gun prevalence on homicide rates. Finally we draw conclusions.

Theory

Why should gun levels influence rates of homicide? If a gun is available to a prospective aggressor, it might encourage attacks, especially by weaker attackers on stronger or more numerous victims, and can facilitate attacks from a distance, or attacks by persons too squeamish to attack with messier weapons like knives or too timid to

attack at close quarters. Similarly, guns may enable some people to attempt robberies they could not complete unarmed (Newton & Zimring, 1969; Cook, 1976; Kleck, 1997, pp. 215–240), some of which could result in the robber killing the victim.

Some experimental psychologists have suggested that the sight or possession of a gun might trigger attacks by already angered persons, due to the learned association between guns and violence. Experimental research on this issue is evenly divided in its findings, though the more realistic studies generally do not support the hypothesis (Kleck, 1997, pp. 222–224). Contradicting the hypothesis, research on real-world crime incidents consistently indicates that aggressors who possess guns are less likely to attack or injure the victim (Kleck & McElrath, 1991; Kleck, 1997 and studies reviewed therein).

Once an injury is inflicted, however, it is more likely to result in death – i.e. a homicide - if a gun was used, due to the weapon's greater lethality (Block, 1977; Kleck & McElrath, 1991; Newton & Zimring, 1969). Part of the higher fatality rates of gun attacks, however, is probably due to greater deadliness of intent on the part of attackers choosing guns, rather than just the deadliness of the weapon itself (Cook, 1982, pp. 247–248; Wright, Rossi, & Daly, 1983, pp. 189–212). Finally, higher national rates of gun ownership in the population as a whole are likely to imply higher gun possession rates among prospective crime *victims* as well as offenders, with more victims owning, carrying, and using guns for self-protection. Prior research indicates that victims who use guns for self-protection are less likely to be injured than otherwise similar victims who either do not resist or resist without guns (see Tark & Kleck, 2004 and evidence reviewed therein). If fewer victims are injured, fewer are likely to be injured fatally. Thus, higher gun ownership among crime victims could have a negative effect on national homicide rates, even if higher gun possession among aggressors had a net positive effect.

Given this complex set of potential effects of opposite sign, it is indeterminate what the predicted sign of any effect of gun prevalence on national homicide rates should be. Since both positive and negative effects of gun availability are possible, there could be no net effect at all.

Methodological Problems in Prior Cross-National Research

Use of Small, Biased Samples

Cross-national macro-level studies often have small sample sizes, but the problem is especially severe in this body of research. The smaller the sample, the more vulnerable the results are to small sample biases and the disproportionate influence of one or two unusual cases. In particular, when the sample of nations is small, the influence of the United States on the guns/homicide association can be quite pronounced. Killias, in a series of largely overlapping analyses, examined this association in samples of just 18 nations (Killias, 1993a), 16 nations (Killias, 1993b), and even 11 nations (Killias, 1990). The samples were composed entirely of highly developed nations, almost all in Europe or North America. The same was true in a study by Lester (1996), who reported a significant positive bivariate association, based on only 12 nations. It became evident that the small size and unrepresentative character of these samples affected the results

when Kleck (1997, p. 254) analyzed a larger, more diverse set of 36 nations and found a nonsignificant 0.27 bivariate correlation. Likewise, when Killias (2001, p. 436) expanded his sample to include 21 nations, he found no significant association ($r = .0123$, $p = .958$) between gun rates and total homicide rates. Since there were no variables controlled in any of these analyses, the differences in findings cannot be due to differences in variables controlled. The huge differences in bivariate guns/homicide correlations were solely due to differences in which small set of nations were studied.

The associations found within these small samples was especially sensitive to whether the U.S. was included. Kleck (1997) found that the correlation was 0.27 when the U.S. was in his full sample of 36 nations, but when the U.S. was excluded the association dropped to 0.20 – a 35% decrease due to the exclusion of a single case. The influence of the U.S. is even more pronounced when the total sample is smaller. In the report of his 18-nation study, Killias (1993a, p. 294) claimed that "the overall [guns/homicide] correlation is not contingent upon a few countries with extreme scores on the dependent and independent variable," reporting a still-significant guns/homicide correlation when *two* nations were excluded. This was misleading, however, because Killias never showed results when just one nation was excluded, instead excluding a pair of countries. The two nations that Killias excluded had opposite-sign effects on the correlation, one (the U.S) increasing it, and the other (Northern Ireland) decreasing it. Excluding two cases with opposite effects had little *net* effect on the association**,** but reanalysis of Killias' data revealed that if one excludes *only* the U.S. from the sample, the correlation between household guns ownership levels and total homicide rates dropped from a significant $r = .658$ in Killias' full sample of 18 nations, down to a nonsignificant $r = .120$ ($p = .324$) when only the U.S. was excluded from the sample - an 82% reduction (Kleck, 1997, p. 253). Likewise, Hemenway and Miller (2000, pp. 986–987) found that the association, while significant with the U.S. included, became nonsignificant when the U.S. was excluded.

Thus, the positive homicide-guns association characterized as "international" by Killias actually did not reflect a pattern prevailing across a large set of nations, but rather reflected the unique status of a single nation that had both high gun ownership and high homicide.

The samples of countries used in these studies were not random samples, but rather samples whose composition was necessarily determined by data availability. This problem cannot be completely avoided, but could have been significantly mitigated by using measures of gun prevalence that were available for a larger and more diverse set of nations. The overlapping studies by Killias (1990, 1993a, 1993b; Killias et al., 2001) all had extremely small samples because he used data from the International Crime Survey (ICS) to measure gun levels, and the ICS covered only 21 or fewer nations when the studies were conducted. Using PSG instead allowed Kleck (1997) and Hoskin (2001) to include 36 nations. Further, given the concealing of gun ownership in surveys, it is by no means clear that direct survey measures are superior to PSG.

Hemenway and Miller (2000) analyzed two overlapping sets of nations, one composed of 36 nations and the other a subset of 26 nations. They focused most of their readers' attention on the results generated for the smaller sample of 26 nations even though the other sample was 28% larger. The authors argued that the smaller sample was more homogenous with respect to income levels, and thus there are smaller differences among these 26 nations with respect to income-related variables that might

affect homicide rates. The authors were thus basically hinting that there was less of a problem with omitted variable bias that could create a spurious guns/homicide association. This argument is appealing, but incorrect, for two reasons. First, there will still be important cross-cultural differences across nations even within the more homogenous 26-nation sample that could account for between-nation variability in homicide rates. Although these differences on confounding variables might be smaller in the more homogenous 26-nation sample, there is no way of telling whether this gain outweighs the disadvantages of relying on a much smaller sample, and of losing the information contained in the 10 omitted nations. Second, the 26-nation sample will not only be more homogenous regarding potentially confounding independent variables, it will also be more homogenous regarding gun levels and homicide rates. That is, the smaller sample will have less variation on both the principle independent variable of interest (gun availability) and on the dependent variable, which makes it harder to reliably estimate the relationship between the two. Since there is no methodological benefit from analyzing the smaller subsample, the authors' decision to emphasize the results from the smaller sample of nations was inappropriate. By reporting an alternative set of findings based on the smaller 26-nation sample, however, the authors roughly doubled the guns/homicide association (compare their Table 2 with Table 3).

In both samples, the authors' analyses were likely to be highly unstable, a point that they confirmed when they showed what happened when they dropped just one nation, the U.S., from their samples. They downplayed the huge drop in the guns/homicide association that occurred when this one case was removed by noting only that "the results remain significant," without acknowledging that, in the analyses using the validated measure of gun availability (PSG), 64% of the guns/homicide association disappeared when the U.S. was dropped (the correlation dropped from .69 to .25 – see their Table 2, second column).

Invalid or Contaminated Gun Measures

To compute a meaningful association between gun prevalence and homicide rates requires a valid measure of gun prevalence. Killias (1990, 1993a, 1993b; Killias et al., 2001) used direct survey measures of household gun prevalence derived from the International Crime Survey, while other researchers used a proxy measure that nonetheless appears to be highly valid. Hemenway and Miller (2000), Hoskin (2001), Kleck (1997), and Moody (2010) used the percent of suicides committed with guns (PSG), a proxy that has been well-validated. Its correlation across nations with the percent of households reporting gun ownership in the ICS is a near-perfect 0.95 (Kleck, 2004, p. 18).

Other proxy measures of gun prevalence, however, are less valid. While Hemenway and Miller (2000) used PSG as their gun measures in some analyses, in other analyses they used the Cook index – the average of PSG and the percent of homicides committed with guns. Results based on the Cook index were contaminated by a common components problem – the number of gun homicides was both the numerator in the homicide half of the Cook index ((gun homicides/total homicides) × 100%) and also part of the numerator of the homicide rate ((gun homicides + nongun homicides)/population). Because the same quantity appeared in the numerators of both the

independent and dependent variables, part of the association between the two variables was an artifact of this common component (see National Research Council, 2005, pp. 168–170 for a refutation of the claim by Azrael, Cook, & Miller, 2004 that the common components problem does not distort the guns/violence association).

The effect of using contaminated measures was substantial in the Hemenway and Miller (2000) study. The guns/homicide correlation was 42% higher in the full sample of 36 nations when the Cook index was used, compared to the correlation using the uncontaminated PSG measure (p. 987). This difference cannot be due to any superior measurement validity of the Cook index, since its cross-national validity is substantially inferior to that of PSG (Kleck, 2004, p. 18). Similarly, Lester (1996) used the percent of homicides committed with guns as a measure of gun prevalence in a cross-national analysis of homicide rates, leading to the same common components problem. It is worth noting that since data for the PSG measure are always available whenever data for the Cook's measure are available, the common components problem is completely avoidable.

Altheimer and Boswell (2012) used a unique measure of gun levels, which appears to have resulted from the authors' misinterpretation of the suicide-related measure used in other studies. Rather than measuring the *percent* of suicides committed with guns (PSG), these authors measured the per capita gun suicide rate, i.e. gun suicides per 100,000 population (p. 690). This measure, unlike PSG, necessarily measures the suicide proneness of the population, rather than gun prevalence. There is no evidence that this measure is strongly correlated with direct survey measures of gun prevalence, and thus it has not been validated.

The problems of small, biased samples and invalid measures of gun prevalence can both be reduced simply by using PSG to measure gun prevalence. The requisite data are available for a far larger and more diverse set of nations than survey measures, are highly comparable across nations, and the measure has been strongly validated against survey measures (Kleck, 2004).

The Failure to Control for Cultural and Other Violence-Related Differences between Nations

Finally, it is fair to say the cross-national research in this area has not done a good job of controlling for likely confounders of the guns/homicide relationship, i.e. factors that affect homicide rates but are also correlated with gun levels. Certainly this is true of the studies that only measured the bivariate guns/homicide association (Killias, 1990, 1993a, 1993b; Killias et al., 2001; Kleck, 1997; Lester, 1996; Moody, 2010), since they did not control for *any* variables. In particular, past research has failed to account for cultural differences among nations. Adherence to traditional values may discourage rule-breaking in general, and rules forbidding violent behavior in particular. This cultural trait clearly varies substantially across nations. Cross-nationally comparable data are not available for many confounding cultural variables, but that is no excuse for failing to control for the few for which the necessary data are available. Although a secondary focus of this paper, another possible confounder is a nation's level of economic development, based on the assumption that a higher level of prosperity and economic security helps reduce crime and violence. Altheimer and Boswell (2012) found a significant negative

association between gross domestic product (GDP) and total homicide rates for two of three regions studied (pp. 694–695).

Hoskin (2001) made some effort to control confounders, explicitly controlling for three variables (p. 586). One of these, the percent male age 15–24, was not significantly related to homicide rates, and thus could not be a confounder. As to the remaining two variables that were significantly related to homicide (the "ethnic heterogeneity index" and the "welfare state index"), neither was shown to be correlated with gun ownership, so there was no affirmative evidence that these two variables were confounders either. In any case, none of these studies controlled for any cultural differences and only one controlled for affluence levels.

Finally, Altheimer and Boswell (2012) used a fixed effects panel design to analyze annual national-level data on 43 nations for the period 2000–2005. The authors controlled for six variables (not including the lagged homicide rate or the year dummy) in their total homicide equation, but only only one of the significant variables (urbanization) is known to be correlated with gun levels. Thus, these authors probably controlled just one genuine confounding variable.

The authors found significant positive associations between gun availability and homicide among Latin American nations, but found no significant association in the total sample of all 43 nations, and significant *negative* associations among Western nations and among Eastern European nations. The authors admitted they were unable to address simultaneity, due to the lack of suitable instrumental variables (p. 692). And as previously noted, the authors used an invalid measure of gun availability, the number of gun suicides per 100,000 population (p. 690). Since the authors did not actually measure gun availability, the associations they reported cannot be regarded as associations between gun availability and homicide rates.

The Direction of the Relationship between Gun Rates and Homicide Rates

It has long been suggested that, although gun levels may affect homicide rates, it is also possible that homicide rates affect gun levels because higher rates of murder and other crime may motivate more people to get guns for protection (Kleck, 1979). Any positive correlation between gun rates and homicide rates could be entirely due to the effect of homicide rates on gun rates rather than the reverse. If authors do nothing to deal with this problem, their estimates of the effect of gun rates could be afflicted by simultaneity bias (Kovandzic, Schaffer, & Kleck, 2011). To his credit, Hoskin (2001) tried to address this possible simultaneity bias by using instrumental variable (IV) methods.

For his estimates to be meaningful, it was necessary that at least one of the variables that predicted gun levels could legitimately be excluded from the homicide rate equation – that is, Hoskin had to assume that the variable had no direct effect on homicide rates. His exclusion restrictions used to identify the model, however, were arbitrary and implausible, and directly contradicted by his own theoretical assertions. To achieve identification in his homicide equation, Hoskin excluded (1) population density, (2) the percent of the population that was male and aged 15 to 34 (p. 584), and (3) an East Asia dummy. Yet just a few pages earlier he had asserted, quite plausibly, that the first two of these variables should affect homicide rates, and his discussion of the third was limited to the remark that both homicide and firearms ownership rates are low in East Asia (pp. 580–1). Hoskin did not report any tests of instrument validity, i.e.

whether it can legitimately be excluded from the homicide equation. In the absence of such information, there is no basis for believing that any of these instruments were adequate or that the models were identified, and thus no basis for believing that resulting estimates were interpretable. There was probably little that Hoskin could have done to alleviate this problem, as we know of no variables for which cross-national data are available that would serve as valid instrumental variables.

This paper is intended to contribute to the cross-national literature on the effect of gun levels on homicide in the following ways: (1) control for cultural differences and economic development across nations, (2) analyze a larger and more diverse set of nations than has previously been studied, (3) use a validated, non-contaminated measure of gun levels, and (4) test whether the estimates are sensitive to the inclusion of the U.S. in the sample.

Data and Methods of the Present Study

We examine the cross-national relationship c. 2006–2010 between gun levels and homicide rates for all 55 nations for which complete data on all variables were available. The sample size is therefore considerably larger than those used in prior research (36 or fewer), which reduces standard errors of coefficients, yielding more precise estimates of the guns/homicide relationship. These 55 nations are also much more diverse than the sets of nations used in prior research. They spanned 12 different regions of the world, including four that have never been included in prior cross-national studies: (1) Eastern Europe (Czech Republic, Poland, Bulgaria, Romania, Slovakia and Hungary), (2) Central America (Mexico), (3) South America (Colombia, Venezuela, Argentina, Brazil, Chile, Peru, and Uruguay), and (4) the Caribbean (Trinidad and Tobago). We therefore had data on a more diverse set of nations that provided a more credible foundation for generalizing findings beyond the heavily Western European-dominated samples used in prior research. In addition, by including nations from other regions of the world we are able to increase the variability in the gun levels variable - Western European nations were largely concentrated in the low end of the gun prevalence variable. This is important because, like the larger sample size, it reduces the variance in the regression estimator for gun prevalence and thus contributes to a more precise estimate of the effect of gun prevalence on homicide.

As a proxy for gun ownership levels, we use the percent of suicides committed with guns (PSG), which has been strongly validated as a measure of gun ownership for cross-sectional research across nations, as well as cities, counties, and states (Azrael et al., 2004; Kleck, 2004). Using survey-based estimates of household gun ownership for 18 developed nations from the 1989 and 1992 International Crime Surveys (ICS) as a criterion-measure of gun availability, Kleck (2004, p. 18) found a near perfect correlation of $r = .95$ of PSG with the ICS direct survey measure of household gun ownership. Data on the percentage of suicides committed with firearms were obtained from the sources listed at the bottom of Table 1. Sixty-two percent of the nations ($n = 34$) provided data for 2009 or 2010; data for the remaining 21 nations were calculated using the most recent year for which data on PSG were readily available: 1994–1999 for six nations, 2000–2008 for 15 nations.

Table 1 Bivariate and multivariate associations between gun levels and homicide rates

Column:	(1) Full ($n = 55$) sample of countries	(2) Same as column 1 but excluding U.S.	(3) All 55 nations, with controls for confounding factors	(4) Same as column 3 but excluding U.S.
Independent variables				
Gun level (PSG)	0.024* (0.010)	0.023* (0.010)	0.012 (0.010)	0.012 (0.010)
Log of GDP, 2005			−0.898** (0.261)	−.898** (0.262)
Traditional values index			−0.335** (0.121)	−0.336** (0.121)
Survival values index			0.053 (0.135)	0.047 (0.137)
Constant	0.385** (0.168)	0.398** (0.173)	9.428** (2.604)	9.420** (2.616)
Observations	55	54	55	54
R-squared	0.069	0.066	0.566	0.564

Robust standard errors in parentheses

* significant at 5% level; ** significant at 1% level

Data limitations make it impossible to gather data on homicide, PSG, and our cultural values variables for the same year. The Appendix Table 2 displays the nations included in our sample, the homicide rates and gun levels of the countries, and the years to which the data for each nation pertain. Data on the total homicide rate were available for the same year to which the gun data pertained for 50 of the 55 nations in the study sample. For the remaining five nations, we used homicide data for the year closest (but always later than) to the year for which data on the gun ownership proxy was available. We used homicide data (year of PSG data is in parentheses) for 2004 in Brazil (2000), 2005 in Chile (2002), 2001 in Hong Kong (1999), 2004 in Peru (2000) and 2000 in Trinidad and Tobago (1995). With the exception of Argentina, homicide data were obtained from the United Nations Office on Drugs and Crime (2011); data for Argentina were obtained from Krug, Powell, and Dahlberg (1998). The homicide rate was logged in all analyses.

Therefore some measurement error in gun prevalence could be introduced by use of these different years of data, to the extent that gun prevalence changed substantially in the 1998–2010 period. Survey data for the U.S., however, indicate that household gun prevalence was essentially constant in this period. The General Social Survey indicated it was 35% in 1998 and 34% in 2010 (Roper iPoll database, accessed on May 14, 2014). To the extent that this degree of stability is similar in other nations, the problem of using different years of data should be minimal.

Control Variables We wanted to control for cultural differences between nations that we

hypothesized could affect homicide rates. The World Values Survey (WVS) provided the data on cultural differences between nations. The WVS is composed of representative national surveys of the populations of over 100 societies on all six

inhabited continents, containing 90% of the world's population. We hypothesized that two master clusters of cultural traits affect violent behavior: (1) traditional values (contrasted with secular-rational values) and (2) survival values (contrasted with self-expression values). Populations committed to traditional values emphasize the importance of religion, parent-child ties, deference to authority and traditional family values . These values strengthen the power of social control and the ability of institutions such as governments and the family to discourage violations of moral norms. Thus, countries that score high on traditional values should have less homicide and other forms of deviant behavior. Survival values place emphasis on economic and physical security and encourage low levels of trust and tolerance. If mistrust and intolerance encourage aggression, one would expect that countries that score high on survival values would have more homicide. National scores on the Traditional Values Index and the Survival Values Index were obtained from the WVS website (World Values Survey, 2017). Details of the construction of these indexes, including survey items used and factor loadings, can be found in Inglehart (2006). We also controlled for national levels of economic development using data on Gross Domestic Product (GDP).

Findings

Table 1, column 1 displays estimates of the bivariate guns/homicide association based on our full sample of all 55 nations for which valid data on gun ownership, homicide rate, and national culture could be obtained. We found a bivariate guns/homicide association ($r = .26$) that is considerably weaker than in previous studies based on smaller, more biased samples of nations but is nevertheless significant at the .05 level.

In previous research based on small samples of nations, the association between gun levels and the total homicide rate was heavily influenced by the U.S., to the point where the association largely disappeared if the U.S. was excluded from such samples. It was not surprising that a single extreme case like the U.S. could be strongly influential in very small samples, but this is not as likely when 55 nations are studied. Column 2 of Table 1 shows the bivariate estimates for our larger sample but with the U.S. omitted. Unlike the results from analyses of small samples of nations, the results are no longer sensitive to inclusion of the U.S. - the bivariate guns/homicide association is essentially unchanged by the exclusion of the U.S. The reduced influence of the U.S. may also be due to the fact that U. S. homicide rates dropped sharply from 1993 to 2000, making the U.S. less of an outlier among nations in 2000 than it had been in earlier years.

We then addressed the question of whether controlling for cultural and economic differences affected the guns-homicide association, within our larger sample of 55 countries. The relevant multivariate results are shown in the right half of Table 1, in columns 3 and 4. Survival values were not significantly related to homicide rates, but, as expected, a country's stronger commitment to traditional values appears to significantly reduce homicide. Likewise, a higher level of economic development appears to reduce homicide. Once Traditional Values, Survival Values, and GDP were controlled, the already modest guns-homicide association was cut in half, and was no longer significantly different from zero. This supports the view that the significant positive guns-homicide associations found in previous studies of international data were spurious, and did not reflect a causal effect of gun levels on homicide rates.

Finally, the multivariate analysis confirmed the finding of the bivariate analysis that in our larger, more diverse sample of 55 nations, the inclusion of the U.S. did not significantly influence the estimate of the guns/homicide association. The multivariate association remained unchanged when the U.S. was omitted (Table 1, column 4 compared with column 3).

Discussion

One limitation of this research is that we could not solve the problem of the possible two-way causation between homicide rates and gun prevalence. There is ample evidence that homicide rates have a positive effect on gun prevalence (Bice & Hemley, 2002; Bordua & Lizotte, 1979; Clotfelter, 1981; Duggan, 2001; Kleck, 1979; Kleck, 1984; Kleck & Kovandzic, 2009; Kleck & Patterson, 1993; Magaddino & Medoff, 1984; McDowall & Loftin, 1983; Southwick, 1997). Therefore, the likely consequence of failing to separate this effect from the effect of gun prevalence on homicide rates is an upwardly biased estimate of the effect of gun levels on homicide – i.e., making estimates of the effect more positive than the true effect. Normally one would use an instrumental variables estimator to produce estimates of gun prevalence on homicide rates, but it was not feasible to do so in this case because there are no international data on plausible instruments, i.e. variables that affect gun prevalence but not homicide rates, or that affect homicide rates but not gun prevalence. Because we have not taken account of the positive effect of homicide rates on gun ownership rates, our estimates of the effect of gun levels on homicide rates are likely to be biased upward because they partly reflect the positive effect of homicide rates on gun levels.

In this study, however, this flaw is not substantively consequential since even our positively biased estimates are not significantly different from zero. Eliminating this positive bias could only bring the estimated effect of gun levels even closer to zero. It is even possible that gun availability has a mild negative net causal effect on homicide rates that is obscured by the positive effect of homicide rates on gun levels. This is what was found in analyses of U.S. data that were able to address the possible two-way causation operating between gun levels and homicide rates. Kovandzic, Schaffer, and Kleck (2013) applied instrumental variables methods to county-level data to model this relationship, carefully testing the relevance and validity of their instrumental variables. They found a small but significant negative effect of gun prevalence (measured the same as in the present study) on homicide rates. This confirmed the results of a different analysis of the same data using general method of moments estimation (Kovandzic et al., 2011).

Conclusions

The significant positive association between gun availability and total homicide rates found in some prior international studies appears to have been a product of sample bias and the failure to control for differences in culture and economic development between nations. Once one studies a larger more diverse sample of the world's nations and controls for cultural differences and economic development, the guns-homicide

association disappears. The absence of any significant cross-national association between gun levels and total homicide rates confirms the results of the most methodologically sound prior research conducted at subnational levels of analysis such as regions, states, counties, or cities (Kleck, 2015). These findings cast doubt on the hypothesis that more guns cause more homicide, and thus on the hypothesis that reducing overall gun levels via gun control will reduce homicide. On the other hand, gun control measures narrowly targeting high-risk subsets of the population such as convicted criminals or mentally ill persons might reduce violence.

Appendix

Table 2 Gun levels (percent suicides with guns) and homicide rates for 55 nations

Nation	% Suicides with gun year	% suicides with guns[d]	Homicide rate year	Homicide rate[c]	Year from WVS used to create culture indexes
Albania	2004	27.4	2004	3.80817	2000
Argentina	1994	45.45[a]	1994[a]	4.51	1995
Australia	2008	7.76	2008	1.21317	2006
Austria	2010	17.84	2010	0.63143	2000
Azerbaijan	2007	1.82	2007	2.14261	1995
Belgium	2006	10.7	2006	2.10979	2000
Brazil	2000	2.93[b]	2004	22.4584	2006
Bulgaria	2010	6.752	2010	1.96148	2006
Canada	2006	16.69	2006	1.8573	2006
Chile	2002	10.46[b]	2005	3.53337	2006
Colombia	2009	20.7[e]	2009	34.6453	2006
Croatia	2010	13.38	2010	1.40803	2000
Cyprus	2009	25.81	2009	1.74236	2006
Czech Republic	2009	8.33	2009	1.73376	2000
Denmark	2006	9.74	2006	0.532829	2000
Estonia	2010	9.42	2010	5.21944	2000
Finland	2010	18.82	2010	2.19963	2006
France	2009	13.78	2009	1.09217	2006
Georgia	2009	2.13	2009	4.8015	1995
Germany	2010	7.7	2010	0.838371	2006
Greece	2009	27.88	2009	1.26252	2000
Hong Kong	1999	0.23[b]	2001	0.966485	2006
Hungary	2009	2.93	2009	1.38969	2000
Iceland	2009	11.11	2009	0.316914	2000
Ireland	2010	5.14	2010	1.20808	2000
Israel	2009	13.18	2009	2.14848	2000
Italy	2009	12.21	2009	0.979275	2006
Japan	1998	0.19[a]	1998	.571312	2000

Table 2 (continued)

Nation	% Suicides with gun year	% suicides with guns[d]	Homicide rate year	Homicide rate[c]	Year from WVS used to create culture indexes
Latvia	2010	4.82	2010	3.10827	2000
Lithuania	2010	3.24	2010	6.58922	2000
Luxembourg	2009	8.47	2009	1.8079	2000
Malta	2010	22.58	2010	.96035	?
Mexico	2001	19[b]	2001	10.1501	2000
Netherlands	2010	2.5	2009	1.08097	2006
New Zealand	2006	9.41	2006	1.17088	2006
Norway	2010	15.36	2010	0.593884	2006
Peru	2000	12.72[b]	2004	5.60164	2000
Poland	2010	0.73	2010	1.13908	2006
Portugal	2010	10.54	2010	1.16153	2000
Republic of Korea	2002	.2139[b]	2002	1.62	2000
Romania	2009	0.348	2009	1.95476	2006
Serbia	2010	16.96	2010	1.24794	2006
Singapore	1998	1.35[b]	1998	0.983712	?
Slovakia	2010	8.08	2010	1.1968	2000
Slovenia	2010	11.59	2010	0.739033	2006
Spain	2010	6.11	2010	0.846409	2006
Sweden	2010	9.93	2010	0.970182	2006
Switzerland	2008	18.2	2008	0.71304	2006
Republic of Macedonia	2010	10.66	2010	1.94122	2000
Trinidad and Tobago	1995	0.96[d]	2000	9.28751	2006
United Kingdom	2009	2.379	2009	1.17108	2006
United States of America	2009	50.76	2009	5.00477	2006
Uruguay	2000	40.96[b]	2000	6.4476	2006
Venezuela	2000	25.73[b]	2000	32.9469	2000

[a] Suicide and homicide data for Argentina were obtained from Krug, E G, K E Powell and L L Dahlberg. 1998. 'Firearm-Related Deaths in the United States and 35 Other High- and Upper-Middle-Income Countries.' *International Journal of Epidemiology*; Table 1 (27), p. 216. Atlanta: National Centre for Injury Prevention & Control, Centers for Disease Control & Prevention / CDC. 16 April

[b] Suicide data were obtained from Jackson, Thomas. 2005. 'Global Gun Deaths.' *NISAT Firearm Mortality Database 2005*. Oslo: Norwegian Initiative on Small Arms Transfers. 1 January. Data on the total suicide rate for South Korea in 2002 was obtained from the Organization for Economic Cooperation and Development

[c] Homicide data were obtained from the United Nations Office on Drugs and Crime, 2011 Global Study on Homicide, except where noted below. Available at http://www.unodc.org/unodc/en/data-and-analysis/homicide.html

[d] Suicide data for most nations were obtained from the World Health Organization Regional Office for Europe's European Detailed Mortality Database, except where noted below. Available at http://data.euro.who.int/dmdb/

[e] Valenzuela, Diana M. 2010.'Suicide Epidemiology, Colombia, 2009 (Epidemiología del Suicidio, Colombia, 2009).' Forensis 2009: Data for Life. Bogota: National Institute of Legal Medicine and Forensic Science, 1 May

References

Altheimer, I., & Boswell, M. (2012). Reassessing the association between gun availability and homicide at the cross-national level. *American Journal of Criminal Justice, 37*, 682–704.

Azrael, D., Cook, P. J., & Miller, M. (2004). State and local prevalence firearms ownership: Measurement, structure, and trends. *Journal of Quantitative Criminology, 20*, 43–62.

Bice, D. C., & Hemley, D. D. (2002). The market for new handguns. *Journal of Law and Economics, 45*, 251–265.

Block, R. (1977). *Violent Crime*. Lexington, Mass: Lexington Books.

Bordua, D. J., & Lizotte, A. J. (1979). Patterns of legal firearms ownership: A cultural and situational analysis of Illinois counties. *Law and Policy Quarterly, 1*, 147–175.

Clarke, R. V., & Mayhew, P. (1988). The British gas suicide story and its criminological implications. *Crime and Justice, 10*, 79–116.

Clotfelter, C. T. (1981). Crime, disorders and the demand for handguns. *Law & Policy Quarterly, 3*, 425–446.

Cook, P. J. (1976). A strategic choice analysis of robbery. In W. Skogan (Ed.), *Sample surveys of the victims of crime* (pp. 173–187). Cambridge: Ballinger.

Cook, P. J. (1982). The role of firearms in violent crime. In M. E. Wolfgang & N. A. Weiner (Eds.), *Criminal violence* (pp. 236–291). Beverly Hills: Sage.

Duggan, M. (2001). More guns, more crime. *Journal of Political Economy, 109*, 1086–1114.

Hemenway, D., & Miller, M. (2000). Firearm availability and homicide rates across 26 high-income countries. *Journal of Trauma, 49*, 985–988.

Hoskin, A. W. (2001). Armed Americans. *Justice Quarterly, 18*, 569–592.

Inglehart, R. (2006). Mapping global values. *Comparative Sociology, 5*, 115–136.

Killias, M. (1990). Gun ownership and violent crime. *Security Journal, 1*, 162–174.

Killias, M. (1993a). Gun ownership, suicide, and homicide: An international perspective. In A del Frate, U. Zvekic, and J. J. M. van Dijk (eds), Understanding crime: Experiences of crime and crime control (pp. 289–303). Rome: UNICRI.

Killias, M. (1993b). International correlations between gun ownership and rates of homicide and suicide. *Canadian Medical Association Journal, 148*, 1721–1725.

Killias, M., van Kesteren, J., & Rindlisbacher, M. (2001). Guns, violent crime, and suicide in 21 countries. *Canadian Journal of Criminology, 43*, 429–448.

Kleck. (1984). The relationship between gun ownership levels and rates of violence in the United States. In D. B. Kates (Ed.), *Firearms and violence: issues of public policy* (pp. 99–135). Cambridge, Mass, Ballinger.

Kleck, G. (1979). Capital punishment, gun ownership, and homicide. *American Journal of Sociology, 84*, 882–910.

Kleck, G. (1997). *Targeting guns: Firearms and their control*. New York: Aldine.

Kleck, G. (2004). Measures of gun ownership levels for macro-level crime and violence research. *Journal of Research in Crime and Delinquency, 41*, 3–36.

Kleck, G. (2015). The impact of gun ownership rates on crime rates: A methodological review of the evidence. *Journal of Criminal Justice, 43*(1), 40–48.

Kleck, G., & Kovandzic, T. (2009). City-level characteristics and individual handgun ownership: Effects of collective security and homicide. *Journal of Contemporary Criminal Justice, 25*, 45–66.

Kleck, G., & McElrath, K. (1991). The impact of weaponry on human violence. *Social Forces, 69*, 669–692.

Kleck, G., & Patterson, E. B. (1993). The impact of gun control and gun ownership levels on violence rates. *Journal of Quantitative Criminology, 9*, 249–288.

Kovandzic, T., Schaffer, M. E., & Kleck, G. (2011). Gun prevalence, homicide rates and causality: A GMM approach to endogeneity bias. In D. Gadd, S. Karstedt, & S. F. Messner (Eds.), *The sage handbook of criminological research methods*. Thousand Oaks, CA: Sage.

Kovandzic, T., Schaffer, M. E., & Kleck, G. (2013). Estimating the causal effect of gun prevalence on homicide rates: A local average treatment effect approach. *Journal of Quantitative Criminology, 29*, 477–541.

Krug, E. G., Powell, K. E., & Dahlberg, L. L. (1998). Firearm-related deaths in the United States and 35 other high- and upper-middle-income countries. *International Journal of Epidemiology, 27*, 214–221.

Lester, D. (1996). Gun ownership and rates of homicide and suicide. *European Journal of Psychiatry, 10*, 83–85.

Lott Jr, J. R. (1998). *More Guns, Less Crime*. Chicago: University of Chicago Press.

Magaddino, J. P., & Medoff, M. H. (1984). An empirical analysis of federal and state firearm control laws. In D. B. Kates (Ed.), *Firearms and violence: issues of public policy* (pp. 225–258). Cambridge, Mass: Ballinger.

McDowall, D., & Loftin, C. (1983). Collective security and the demand for legal handguns. *American Journal of Sociology, 88*, 1146–1161.

Moody, C. E. (2010). Firearms and homicide. In B. Bruce Benson & P. Zimmerman (Eds.), *Handbook on the economics of crime* (pp. 432–451). Northhampton, MA: Edward Elgar.

National Research Council. (2005). *Firearms and violence: A critical review*. Washington, D.C.: National Academies Press.

Newton, G. D., & Zimring, F. (1969). *Firearms and violence in American life. A staff report to the National Commission on the causes and prevention of violence*. Washington, D.C.: U.S. Government Printing Office.

Sherrill, R. (1973). *The Saturday night special*. New York: Charterhouse.

Sloan, J. H., Kellermann, A. L., Reay, D. T., Ferris, J. A., Koepsell, T., Rivara, F. P., Rice, C., Gray, L., & LoGerfo, J. (1988). Handgun regulations, crime, assaults and homicide. *New England Journal of Medicine, 319*, 1256–1262.

Southwick, L. (1997). Do guns cause crime? Does crime cause guns? A Granger test. *American Economic Journal, 25*, 256–273.

Tark, J., & Kleck, G. (2004). Resisting crime: The effects of victim action on the outcomes of crimes. *Criminology, 42*(4), 861–909.

United Nations Office on Drugs and Crime. (2011). Global Study on Homicide. Available at http://www.unodc.org/unodc/en/data-and-analysis/homicide.html. Accessed 1 July 2018.

World Values Survey. (2017). *National-level Value Scores by country*. Available online at http://www.worldvaluessurvey.org/wvs/articles/folder_published/article_base_111. Accessed 1 July 2018.

Wright, J. D., Rossi, P. H., & Daly, K. (1983). *Under the gun: Weapons, crime and violence in America*. New York, NY: Aldine.

Zimring, F. E., & Hawkins, G. (1997). *Crime is not the problem*. New York, NY: Oxford University Press.

Tomislav V. Kovandzic is an associate professor of criminology at the University of Texas at Dallas in the School of Economic, Political & Policy Sciences. His primary areas of research interest are gun control, crime policy, and deterrence/incapacitation. His recent work has been published inHomicide Studies, Journal of Quantitative Criminology, Journal of Criminal Justice, and Journal of Contemporary Criminal Justice.

Gary Kleck is the Emeritus David J. Bordua Professor of Criminology and Criminal Justice at Florida State University. His research has focused on the topics of the impact of firearms and gun control on violence, deterrence, crime control, and violence. He is the author of Point Blank: Guns and Violence in America, which won the 1993 Michael J. Hindelang Award of the American Society of Criminology. He also wrote Targeting Guns (1997) and, with Don B. Kates, Jr., The Great American Gun Debate (1997) and Armed (2001). His articles have been published in the American Sociological Review, American Journal of Sociology, Social Forces, Criminology, Journal of Criminal Law and Criminology, Law & Society Review, Journal of Research in Crime and Delinquency, Crime and Delinquency, and many other journals. He has testified before Congress and state legislatures on gun control issues and his work has been cited by the U.S. Supreme Court. He has served as a consultant to the National Research Council, National Academy of Sciences Panel on the Understanding and Prevention of Violence, as a member of the U.S. Sentencing Commission's Drugs-Violence Task Force, and as a member of the National Research Council Committee on Priorities for a Public Health Research Agenda to Reduce the Threat of Firearm-related Violence. His most recent book is Punishment and Crime, with Brion Sever.