MEASURES OF GUN OWNERSHIP LEVELS FOR MACRO-LEVEL CRIME AND VIOLENCE RESEARCH

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Valid measures of macro-level gun levels are essential to assessing the impact of gun levels on crime and violence rates, yet almost all prior research on this topic uses proxies that are either invalid or whose validity has been assumed rather than demonstrated. The present study uses city, state, cross-national, and time series data to assess the criterion validity of over two dozen gun indicators. The criterion measures used are primarily direct survey measures of household gun prevalence. The results indicate that (1) most measures used in past research have poor validity, making past findings uninterpretable, (2) the best measure for cross-sectional research is the percentage of suicides committed with guns, and (3) there are no known measures that are valid indicators of trends in gun levels, making credible longitudinal research on the subject impossible at present.

Keywords: *guns; violence; measurement*

Macro-level research on the possible links between guns and violence is essential because there is no other way to estimate the net effects of societal or community gun levels on crime or violence rates. Studies that attempt to link the gun ownership of individuals to their experiences as victims (e.g. Kellermann et al. 1993) do not effectively determine how an individual's risk of victimization is affected by gun ownership of other people, especially those not living in the gun owner's own household. Likewise, studies of crime incidents (e.g., Kleck and Delone 1993) can address the effects of actual offensive gun use by offenders and defensive gun use by victims on the outcome of the incident (e.g., injury to the victim), but cannot assess whether gun ownership among potential victims deters criminals from attempting crimes in the first place, or whether gun ownership encourages offenders to seek hostile contact with victims in the first place. If gun ownership by prospective victims has deterrent or other violence-reducing effects, this effect

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cannot be assessed by studying individuals because the effect would not be limited to gun owners, and might not even differ between owners and non-owners (Kleck and Kates 2001:153-54). Thus, the population-wide combined impact of both violence-increasing and violence-decreasing effects of gun ownership can only be assessed using macro-level research.

There are dozens of macro-level studies of the impact of gun ownership levels on rates of homicide, other crimes, and suicide rates, using a diverse array of gun measures. Usually the gun measure is used as an independent variable (predictor) in models of violence rates, while in other studies gun levels are the dependent variable, and in a few others, a two-way relationship between gun levels and violence rates is assumed. With a few limited exceptions, the gun measures have not been subjected to any validation. This article assesses more than two dozen previously used or potential gun measures and reports the results of validity checks. The objective is to determine the best macro-level indicators of gun levels.

How consequential can measurement error be with respect to conclusions about the effect of gun levels on crime or violence rates? Moody and Marvell (2001) analyzed national time series data using Duggan's (2001) measure of gun levels, the rate of subscriptions to *Guns & Ammo* magazine, and concluded that Duggan had overestimated the effect of guns on homicide by a factor of four, while underestimating the effect of homicide on gun levels by an equal amount. As a result, he probably got his conclusions "completely backwards." Moody and Marvell (2001) attributed Duggan's (2001) erroneously reversed interpretation of his evidence to his failure to recognize the problems in using imperfect proxies and the fact that the elasticity of the proxy with respect to actual gun levels (roughly analogous to our validity correlations) appears to be substantially less than one—they estimate it to be about .26. In this instance, the use of an imperfect proxy appears to have been disastrous. A proxy clearly is not sufficiently valid merely because it has a nonzero association with the criterion.

METHODS

The most direct feasible measure of gun availability would probably be one based on surveys in which people were directly asked whether they own guns. Surveys themselves are subject to errors and probably underestimate gun ownership, perhaps by 5 percent to 13 percent (Kleck 1997:65-9), but would still be acceptable ways of measuring cross-area variation in gun levels, when and where available, as long as underreporting of gun ownership was relatively similar across areas or time periods. Unfortunately, surveys asking gun ownership questions are usually carried out either in a single limited area, such as one city or state, or are fielded across a nation, but without enough sample cases in each state, city, county, or metropolitan area to provide meaningful estimates for most subnational areas.

Therefore, less direct proxies that use data that are available for large numbers of units must be used. We can nevertheless use survey measures to perform validity checks on the indirect indicators, by studying the more limited number of areas for which survey data are available. In this study, survey estimates of household gun and handgun prevalence are used as the criteria for judging the validity of the various proxies.

Because survey measures are themselves imperfect, this can weaken indicator-criterion correlations, a problem inevitably afflicting all criteria used to assess measurement validity. If survey measures were subject only to random error such as sampling error, the effect would generally be to bias associations toward zero, but systematic errors, such as patterns of response errors or patterned changes in response rates, could bias associations in any direction. For example, a sharp one-time drop of about seven percentage points in household gun prevalence occurred in national surveys within months of the 1994 enactments of the federal Assault Weapons ban and the Brady Act, following decades near-constant gun prevalence in national surveys. Given the implausibility of a virtually overnight drop of this magnitude after 35 years of stable levels, this "decrease" was very likely artificial—an artifact of increased gun owner unwillingness to report gun ownership to survey interviewers. Because it occurred in the middle of the 1991-2000 declines in national violence rates, it contributed to a positive guns-violence association over time that was probably at least partly synthetic.

The validity checks consisted of computing "validity coefficients" (Nunnally 1967:76-9) consisting of Pearson correlation coefficients between direct survey measures (the criterion measures) and the various indirect indicators of gun levels. The correlations in Tables 1 through 4 are all based on data weighted by the square root of the population of each place, divided by the average, within each sample, of the square root of the population (this last step avoids artificially inflating sample sizes and consequently distorting significance levels). The rationale for this weighting scheme is simply that it gives greater influence to aggregates representing the experience of larger numbers of individual persons. Variables were expressed in natural logs so that their distributions were more nearly normal and the effect of exceptionally high observations was reduced. To permit logging all observations, including zeros, .1 was added to each variable before taking its natural log. Results were substantially the same (i.e. the same in sign, the same as to whether statistically significant, and roughly the same in ordering of relative validity among indicators) regardless of whether variables were logged.[1]

Possible proxies were assessed with respect to their validity in measuring both cross-sectional and cross-temporal variation in gun levels. The validity checks were based on (1) a very rich data set pertaining to a limited set of 45 large U.S. cities, (2) a more limited data set pertaining to a much larger sample of 1,078 U.S. cities, (3) a state-level data set, (4) a cross-national sample of 36 nations, and (5) a U.S. national-level annual time series data set covering the period 1972 to 1999.

The following 25 macro-level indicators of gun levels were assessed. The first four listed are direct survey measures used as validity criteria, while the rest are indirect measures that either have been used in prior macro-level research or are available for use in future research. The sources noted are generally the ones used for 1980 data, but data for later years are available in later versions of the same sources.

Measures Used in Table 1

1. *Percentage households with gun*. The percentage of households interviewed in the General Social Surveys (GSS) that reported a gun in their household. In the 45-city data set, this measure, and the others from the GSS, were computed only for the 45 cities for which one could cumulate at least 30 sample cases across all 11 of the GSS surveys from 1973 to 1989 (bracketing 1980) that asked gun ownership questions (Davis and Smith 1994).
2. *Percentage households with handgun*. The same as the previous measure, but applying only to handguns.
3. *Percentage respondents own gun*. The percentage of GSS respondents that reported personally owning a gun of their own.
4. *Percentage respondents own handgun*. The percentage of GSS respondents that reported one or more handguns in their household and also reported personally owning a gun of their own.
5. *Percentage suicides with guns* (PSG). The percentage of suicides committed in 1979 to 1982 with a gun (Inter-University Consortium for Political and Social Research 1985).
6. *Percentage homicides with guns*. The percentage of homicides committed in 1979 to 1982 with a gun (Inter-University Consortium for Political and Social Research 1985).
7. *Percentage robberies with guns*. The percentage of robberies committed with a gun, 1979 to 1980 (from unpublished Uniform Crime Reports data for cities) (Inter-University Consortium for Political and Social Research 1983).
8. *Percentage aggravated assaults with guns*. The percentage of aggravated assaults committed with a gun, 1979 to 1980 (from unpublished Uniform Crime Reports data for cities) (Inter-University Consortium for Political and Social Research 1983).

9. *Fatal gun accident rate*. Fatal gun accidents per 100,000 resident population, 1979 to 1981 (Inter-University Consortium for Political and Social Research 1985).
10. *Guns share of stolen property*. The percentage of the dollar value of all property stolen that was accounted for by guns, 1979 to 1981 (from unpublished Uniform Crime Reports data on property stolen and recovered) (Inter-University Consortium for Political and Social Research 1984).
11. *Gun/outdoor magazine subscriptions factor*. A principle components factor score based on subscriptions to each of the four highest circulation gun or hunting magazines (*Guns & Ammo*, *Field and Stream*, *Outdoor Life*, and *Sports Afield*) in 1979 to 1982, per 100,000 resident population, for the county in which the city is located (Audit Bureau of Circulations 1979-1982). Three were added to all factor scores to insure that they were positive and could therefore be logged.
12. Guns & Ammo *subscriptions rate*. Paid subscriptions to *Guns & Ammo* magazine per 100,000 resident population, for the county in which the city is located (Audit Bureau of Circulations 1979-1982).
13. *Five-item gun indicator factor*. A five-item principle components factor, based on indicators 5 through 8, and 10 (Kleck and Patterson 1993).
14. *Cook's (1979) gun density*. The two-item gun measure used by Cook (1979), which is basically an average of indicators (5) and (6).
15. *National Rifle Association (NRA) membership*. NRA members per 100,000 resident population, 1980 (NRA 1985).
16. *Hunting license rate*. Hunting license holders per 100,000 resident population, 1980, in the state in which the city is located (U.S. Fish and Wildlife Service 1982).
17. *Weapons arrests per 100 sworn officers*. Weapons arrests per 100 sworn police officers, 1979 to 1981 (from unpublished Uniform Crime Reports arrest data for cities—U.S. Federal Bureau of Investigation, no date).
18. *Weapons arrests per 100,000 population*. Weapons arrests per 100,000 resident population, 1979 to 1981 (from unpublished Uniform Crime Reports arrest data for cities—U.S. Federal Bureau of Investigation, no date).

Additional Measures Used in Tables 2 through 5 but Not in Table 1

19. *Percentage population that hunts*. 1991, state, based on survey (U.S. Fish and Wildlife Service 1993).
20. *Carry permits rate*. Permits to carry a concealed firearm per 1,000 population, 1999, state (Bird 2000:275).
21. *Federal firearms licensees rate*. Federal Firearms License holders (gun dealers) per 100,000 population, 1999, state (U.S. Bureau of Alcohol, Tobacco and Firearms 2000).

22. *Gun retailers rate*. Federal Firearms License holders, dealers and pawnbrokers only, per 1,000 population, 1999 (U.S. Bureau of Alcohol, Tobacco and Firearms 2000).
23. *Cumulated gun stock rate*. Cumulated (as of the end of the calendar year) number of guns manufactured in United States, or imported, minus guns exported, per 100,000 population (Kleck [1997:96-7] and sources cited therein).
24. *Cumulated handgun stock rate*. Same as previous indicator, pertaining to handguns only.
25. *Weapons arrests as percentage of total arrests*. (From unpublished Uniform Crime Reports arrest data for cities—U.S. Federal Bureau of Investigation, no date).

Indicators 7, 8, and 10 were based on data from the Uniform Crime Reports Return A data set. Close examination of these data indicate both a great deal of missing data and many clearly implausible values, so they should be regarded with considerable skepticism.

RESULTS

Indicators of Cross-Sectional Variation in Gun Levels

CROSS-CITY CORRELATIONS—45 LARGE U.S. CITIES, 1980

The richest data set of potential gun level indicators pertains to a sample of 45 large U.S. cities where there were enough GSS cases to estimate household gun prevalence. Table 1 displays the correlations among 18 different potential indicators of gun ownership levels, based on all 45 cities for which at least 30 respondents were interviewed in the 11 General Social Surveys that were fielded with gun ownership questions from 1973 to 1989. These surveys have a response rate of 77 percent (Davis and Smith 1994). The reader should note that indicator 13 overlaps with indicators 5 through 8 and 10, while indicator 14 is composed of the average of indicators 5 and 6, so correlations involving these variables are partly artifactual, due to common components.

Leaving aside these artifact-biased correlations and those among the survey criterion measures themselves, the indicators with the highest correlations with the survey criterion measures are indicators 5, 14, and 13: the percentage of suicides committed with guns (PSG), Cook's (1991) 2-item measure, and Kleck and Patterson's (1993) 5-item factor. The latter two proxies both contain PSG as a component, so all of the top three indicators were variants of PSG.

TABLE 1: Validity of Gun Indicators across 45 Large Cities with Survey Measures, 1980 (all variables are logged)

	Pearson Correlations/One-Tailed Significance																
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*	*(17)*	*(18)*
(1) % Households with gun (GSS)	.91	.85	.85	.87	.35	.43	.37	.38	.66	.63	.70	.74	.82	.40	.37	.60	.13
	.00	.00	.00	.00	.00	.00	.01	.03	.02	.00	.00	.00	.00	.00	.01	.00	.20
(2) % Households with handgun (GSS)		.82	.89	.78	.40	.47	.38	.36	.53	.40	.55	.71	.77	.18	.17	.60	.19
		.00	.00	.00	.00	.00	.00	.01	.00	.00	.00	.00	.00	.11	.13	.00	.10
(3) % Respondents own gun (GSS)			.92	.64	.20	.26	.25	.33	.56	.54	.53	.52	.57	.35	.20	.38	–.09
			.00	.00	.07	.04	.05	.02	.00	.00	.00	.00	.00	.01	.10	.00	.28
(4) % Respondents own handgun (GSS)				.71	.33	.35	.32	.36	.54	.49	.55	.61	.67	.29	.13	.39	–.02
				.00	.00	.01	.02	.01	.00	.00	.00	.00	.00	.03	.21	.00	.46
(5) % Suicides with guns					.43	.53	.50	.38	.65	.63	.72	.84	.92	.36	.24	.59	.24
					.00	.00	.00	.00	.00	.00	.00	.00	.00	.01	.06	.00	.05
(6) % Homicides with guns						.62	.67	.44	.08	-.10	.08	.73	.68	-.27	.09	.30	.27
						.00	.00	.00	.29	.25	.29	.00	.00	.04	.28	.02	.04
(7) % Robberies with guns							.79	.40	.26	.22	.36	.80	.63	.05	.03	.53	.43
							.00	.00	.04	.07	.01	.00	.00	.36	.42	.00	.00
(8) % Aggravated assaults with guns								.37	.18	.20	.20	.80	.60	–.01	.14	.38	.37
								.01	.11	.09	.09	.00	.00	.48	.18	.01	.01
(9) Fatal gun accidents/100k population									.24	.04	.22	.48	.48	.17	.07	.50	.38
									.06	.41	.07	.00	.00	.13	.32	.00	.00
(10) % $ value stolen property due to guns										.49	.49	.58	.57	.52	.28	.36	–.08
										.00	.00	.00	.00	.00	.03	.01	.31
(11) Factor score for 4 gun/outdoor magazine subscription rates											.70	.40	.43	.72	.26	.34	.08
											.00	.00	.00	.00	.04	.01	.30

(continued)

TABLE 1: (continued)

	Pearson Correlations/One-Tailed Significance																
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*	*(17)*	*(18)*
(12) *Guns & Ammo* subscriptions per 100,000 population												.50	.56	.57	.12	.40	.15
												.00	.00	.00	.21	.00	.16
(13) 5-item gun indicator factor													.92	.16	.21	.60	.34
													.00	.14	.09	.00	.01
(14) Cook's gun density														.17	.27	.59	.26
														.13	.03	.00	.04
(15) NRA members per 100 population															.22	.16	–.01
															.07	.15	.48
(16) Hunting license holders per 100,000 population																.23	–.01
																.07	.46
(17) Weapons arrests per 100 sworn officers																	.75
																	.00
(18) Weapons arrests per 100,000 population																	

It is not unreasonable to suppose that multiple-indicator indexes would be preferable to single-indicator measures. For example, Cook (1991:43) appealed to "measurement theory" to justify his use of the percentage of homicides with guns, in addition to PSG, to measure gun availability. He argued that combining both measures would make for a "more reliable indicator." In fact, the additional indicators included in Cook's two-item measure (indicator 14) and in the five-item gun factor used by Kleck and Patterson (1993) (indicator 13), do not add anything to the validity of the measure over simply using PSG (indicator 5). For example, while PSG alone showed a correlation of .87 with survey-measured household gun prevalence, Cook's (1991) addition of the percentage of homicides committed with guns actually degrades the measure's validity slightly, reducing the validity correlation down to .82. Adding the homicide percentage-gun measure (and similar robbery and assault measures) is especially problematic because it may, as discussed later, reflect the violence proneness of the population, which would influence violence rates independently of gun levels. Thus, adding such components to a gun index would create a positive association with violence rates that was due to effects of the population's violence proneness rather than gun levels.

Because it is more strongly correlated with the criterion measures, and on the grounds of simplicity and ease of data gathering, the single-item measure, PSG, is preferable to the multiple indicator measures. PSG therefore is the best indicator to use in research across cities among those evaluated here.

Indicators were generally less strongly correlated with the survey measure of respondents' personal gun ownership than with the survey measure of household gun ownership, perhaps because the additional question needed to determine whether the respondent personally owns guns provides an additional opportunity for response error. Correlations involving survey measures of handgun ownership were likewise weaker than those involving survey measures of gun ownership in general. Again, the reason might be the additional opportunity for response error provided by the question establishing whether any of the household's guns were handguns.

Some of the less successful indicators may be useful measures of some concept, but not of gun levels. The weapons arrest rate measures (indicators 17 and 18) are probably more appropriately treated as measures of police effort to enforce gun laws (and other weapons laws) than of gun ownership levels. Likewise, the gun magazine subscription rates and hunting rate measure (indicators 11, 12, and 16) may serve better as indicators of interest in hunting and other gun-related outdoor recreation (as in Kleck and Patterson 1993), or as indicators of a firearms-related "sporting culture" (Bordua and Lizotte 1979), than of gun levels per se.

Note that the *Guns & Ammo* measure (indicator 12), recently used by Duggan (2001), is distinctly inferior to PSG as an indicator of cross-sectional variation in gun levels. While Duggan mentioned PSG (p. 1092), he nevertheless used the inferior *Guns & Ammo* measure, even though PSG can be computed for each year for counties and states, the units of analysis used by Duggan. He went to considerable lengths to persuade readers that his novel measure was adequate, yet never compared its validity with PSG. Instead, he compared his proxy only with even more inferior measures such the fatal gun accident rate or NRA membership (pp. 1089-1094). This illustrates the importance of comparing validity levels among the better alternative measures available, rather than simply using whatever proxy has a significant association with criterion measures.

PSG can be obtained for the United States as a whole, all Census regions, all states, all counties, and large cities (population 100,000 or larger), using the individual death certificate data in the Mortality Detail File computer tapes distributed by the National Center for Health Statistics (e.g., Inter-university Consortium for Political and Social Research [ICPSR] 1985).

"Part III" versions of these tapes, unlike the public use versions, permit identification of the county of death even for the smallest counties (U.S. National Center for Health Statistics 2001). It is even available for at least 36 nations. Thus the measure is very flexible and widely usable. It is also especially attractive because mortality data are themselves measured so accurately, and because it is easy for medical examiners to distinguish gun suicides from other suicides (Kleck 1988). The measure cannot be used where one is interested in the impact of gun levels on suicide rates, because of a common component problem—the number of gun suicides would be a component in the numerator of both PSG and the suicide rate.

PSG is impossible to compute for units with zero suicides, and unstable for those with just a few. The smaller the unit of analysis, the fewer suicides there will be for the average unit, making it necessary to cumulate multiple years of data for smaller-population counties or cities. Even cumulating multiple years, however, might not be adequate for the very smallest counties, which might have no more than one or two suicides even over a decade. Consequently, use of this indicator may have to be confined to places with minimally large populations (e.g., 25,000 or larger). But this is a modest limitation given that cities of this size accounted for 72 percent of the violent crime in the United States in 2000 (U.S. Federal Bureau of Investigation 2001:195).

CROSS-CITY CORRELATIONS—1,077 U.S. CITIES, 1990

As a check on the results from the limited set of large cities, especially those pertaining to the validity of measures based on magazine subscription rates, and to see if some results can be generalized to smaller cities and a more recent time period, correlations were computing using 1990 data pertaining to all U.S. cities with a population of 25,000 or larger for which there were also Uniform Crime Reports crime data. In this data set, GSS survey measures are not available, due to insufficient sample cases. Based on the results of the previous analysis, PSG is treated as a criterion measure for the sake of assessing the rest of the potential proxies. Because it is itself an imperfect, though very strong, measure of gun levels, its correlations with other indicators may be weaker than would be correlations between these indicators and direct survey measures.

Table 2 displays the correlations of gun magazine subscription measures and the hunting rate with PSG. The results are disappointing, with correlations that are not very strong, ranging from .34 to .49. This indicates that magazine subscription measures and hunting rates cannot be interpreted as valid measures of gun levels. In particular, Duggan's (2001) *Guns & Ammo* measure is not supported as a valid proxy for gun levels at the city level, because it has only a .48 correlation with PSG, implying that most of the variation in the *Guns & Ammo* rate is independent of variation in the criterion. Correlations within this data set were probably weakened somewhat because PSG pertained to cities while the rest of the variables, due to limits on data availability, pertained to counties or states.

CROSS-STATE CORRELATIONS—50 STATES AND D.C., 1999

Table 3 displays correlations between the survey measures of household gun prevalence and various potential gun level indicators, using state data for 1999. Two survey measures are available as criteria at the state level. GSS survey data were used as a criterion measure, using combined samples of six surveys, 1990 to 1998 by states, but were available for only 21 larger states and were based on samples not designed to be representative of state populations (Smith and Martos 1999:16-17). Alternatively, surveys conducted by the Centers for Disease Control and Prevention in 1991 to 1995, which did use samples representative of state populations, were available for 23 states (Powell et al. 1998:970). Most of these state-level proxies were used in the large city analysis, but a few new ones were used that were available only for states. State NRA membership was computed as the sum of the paid circulation of magazines received by NRA members (*American Rifleman*, *American Hunter*, and *American Guardian*). Nearly all members get one and only one magazine, and subscriptions are almost entirely confined to NRA members (Audit Bureau of Circulations 2000).

For state-level analyses, the best measure once again appears to be PSG, which correlates .92 with the GSS survey measure and .93 with the Center for

TABLE 2: Validity of Gun Indicators Across 1,078 Cities 25,000+ Population, 1990 (unit to which data pertain is indicated in parentheses)

	Pearson Correlations[a]					
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
(1) % Suicides committed with guns, 1987 to 1993 (city)	.36	.48	.44	.35	.34	.49
(2) % Population that hunts, 1991 (state)[b]		.05	.67	.68	.67	.54
(3) *Guns & Ammo* subscriptions per 100,000 population (county)			.42	.38	.34	.56
(4) *Field and Stream* subscriptions per 100,000 population (county)				.97	.92	.91
(5) *Outdoor Life* subscriptions per 100,000 population (county)					.92	.87
(6) *Sports Afield* subscriptions per 100,000 population (county)						.84
(7) Gun magazine factor (the 4 magazines above) (county)						

a. One-tailed significance was below .01 for all correlations, which were all based on 1,068 to 1,078 cases. Data pertain to 1990 unless otherwise noted.
b. Taken from the *1991 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*.

Disease Control survey measure. The results confirm that Cook's (1991) gun density measure, which incorporates the percentage of homicides with guns as well as PSG, does not improve on PSG alone; indeed, it substantially degrades the measure's correlation with the criterion measures (from .92 to .77, using the GSS criterion). The state-level measures newly introduced in Table 3 show weaker correlations with the criterion measures. Survey measures of the prevalence of hunting (indicator 10), the NRA membership rate (11), the rate of people licensed to carry concealed firearms (12), the rate of federal firearms licensees (13), and the rate of gun retailers (14) all evince poorer validity than PSG. In particular, the concealed carry permit measure used by Stolzenberg and D'Alessio (2000:1469) has mediocre correlations with the survey measures of gun prevalence ($r = .52$ and $r = .36$) and so should be regarded as an inadequate measure of gun levels or of "legal gun availability," as Stolzenberg and D'Alessio interpreted it.

CROSS-NATIONAL CORRELATIONS—36 NATIONS, C. 1990

International data based on a direct survey measure of gun ownership are available for a small sample of nations. Telephone surveys connected with the International Crime Surveys (ICS) were conducted in 1989 and 1992 in 17 countries, including the United States, asking about gun ownership and many crime-related topics (Killias 1993). Data on suicides that distinguish

TABLE 3: Validity of Gun Indicators Across States (including D.C.), c. 1999

	Pearson Correlations/Number of States/One-Tailed Significance												
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*
(1) % Households with gun, 1990 to 1998 (GSS)	.96	.92	.33	.68	.76	.65	.85	.77	.62	.51	.52	.78	.84
	13	31	29	31	31	31	31	29	31	31	29	31	31
	.00	.00	.04	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
(2) % Households with gun, 1991-1995 (CDC)		.93	.62	.47	.65	.61	.90	.86	.82	.73	.36	.85	.86
		23	22	23	23	23	23	22	23	23	22	23	23
		.00	.00	.01	.00	.01	.00	.00	.00	.00	.05	.00	.00
(3) % Suicides with guns, 1995-1998			.28	.58	.64	.67	.68	.91	.69	.49	.55	.59	.67
			48	51	51	51	51	48	51	51	47	51	51
			.03	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
(4) % Murders, manslaughters with guns, 1999				.57	.43	.22	–.10	.55	–.02	–.23	.21	–.16	–.13
				48	48	48	48	48	48	48	44	48	48
				.00	.00	.07	.25	.00	.43	.06	.08	.14	.18
(5) % Robberies with guns, 1999					.68	.52	.08	.65	.27	.04	.18	.05	.11
					51	51	48	51	51	51	47	51	51
					.00	.00	.14	.00	.34	.08	.21	.36	.22
(6) % Aggravated assaults with guns, 1999						.50	.33	.67	.42	.15	.18	.19	.26
						51	51	48	51	51	47	51	51
						.00	.01	.00	.00	.14	.11	.10	.03
(7) Fatal gun accidents/100k population, 1995 to 1998							.38	.69	.45	.20	.20	.36	.41
							51	48	51	51	47	51	51
							.00	.00	.00	.08	.08	.00	.00
(8) *Guns & Ammo* subscriptions per 100,000 population								.50	.77	.86	.45	.88	.90
								48	51	51	47	51	51
								.00	.00	.00	.00	.00	.00

(continued)

TABLE 3 (continued)

	Pearson Correlations/Number of States/One-Tailed Significance												
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*
(9) Cook's gun density, 1995-1998									.54	.26	.57	.41	.48
									48	48	44	48	48
									.00	.04	.00	.00	.00
(10) % Persons 16+ who hunt, 1996										.71	.22	.78	.85
										51	47	51	51
										.00	.07	.00	.00
(11) NRA members per 1,000 population, 2000											.36	.85	.84
											47	51	51
											.01	.00	.00
(12) Carry permits per 1,000 population, 1999												.35	.38
												47	47
												.01	.00
(13) Federal firearms dealer licensees per 100,000 population, 1997													.98
													51
													.00
(14) Gun retailers per 1,000 population, 2000													

those committed with guns from other suicides are available for 36 nations (Krug, Powell, and Dahlberg 1998), permitting computation of PSG for these countries.

Table 4 displays the results of validity checks using cross-national data. Examining the figures in the upper diagonal, PSG has a virtually perfect association with the survey gun ownership measure (*r* = .95) and a fairly high correlation with the survey handgun measure (*r* = .83) (confirming Killias 1993). Thus, once again, PSG appears to be the best indicator of gun levels, though its validity as a measure of handgun levels is weaker. The percentage of homicides committed with guns, on the other hand, is more weakly associated with the total gun and handgun prevalence survey measures. Using the Cook (1991) measure once again degrades validity compared to using PSG alone (*r* = .79 vs. *r* = .95). The fatal gun accident rate is strongly related to the criterion measures, though it is inferior to PSG. This measure might therefore be used in cross-national research on suicide rates, where PSG could not be used.

Previous cross-national research indicates gun-violence associations are highly sensitive to whether the United States is included (Kleck 1997:254). Because some analyses are likely to be done without the United States for comparative purposes, the correlations were computed for a sample excluding the United States and are shown in the part of Table 4 below the diagonal. As expected, the correlations are highly sensitive to whether the United States is included, and generally get weaker without the United States. Furthermore, the apparent validity of percentage of homicides with guns and the Cook's measure decreases to a pronounced degree. PSG, however, remains strongly correlated (*r* = .91) with the criteria.

Therefore PSG is the best measure to use in cross-national research. Indeed, with a .95 correlation with the criterion, it would be hard to improve on PSG as a measure of household gun prevalence. Because this measure is available for a larger number of nations (36 at present, and probably more in future) than the ICS measures, and is, unlike the survey data, likely to be consistently available for multiple years in future, it is a more flexible and widely usable measure than the survey-measured percentage of households with guns.

Where comparable associations were measured in city, state, and national data sets, the correlations were generally larger in the state and national data. This reflects the usual tendency of associations to be higher among larger aggregates but is also likely to be at least partly due to the fact that the samples used to estimate national household gun ownership rates numbered at least in the hundreds (Krug et al. 1998), while the samples used to estimate the large city gun levels could include as few as 30 respondents, and state samples were often not much bigger. All of the validity correlations reported here, but especially the city-level correlations in Table 1, should probably be regarded

TABLE 4: Validity of Gun Indicators Across Nations, 1990-1995[a]

	Pearson Correlations/ Number of Nations/ One-Tailed Significance					
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
(1) % Households with gun, 1989/1992 ICS[b]		.80	.95	.62	.79	.89
		19	18	18	18	18
		.00	.00	.00	.00	.00
(2) % Households with handgun, 1989/1992 ICS	.64		.83	.77	.84	.79
	15		18	18	18	18
	.00		.00	.00	.00	.00
(3) % Suicides committed with gun, 1990-1995	.91	.64		.87	.95	.75
	15	15		34	34	34
	.00	.00		.00	.00	.00
(4) % Homicides committed with gun, 1990-1995	.41	.62	.85		.98	.64
	15	15	31		34	34
	.07	.01	.00		.00	.00
(5) Cook's gun density, 1990-1995	.63	.68	.95	.97		.71
	15	15	31	31		34
	.01	.00	.00	.00		.00
(6) Fatal gun accidents per 100,000 population, 1990-1995	82	.54	.69	.59	.65	
	15	15	31	31	31	
	.00	.02	.00	.00	.00	

a. Figures above the diagonal refer to the full sample of nations for which requisite data were available; figures below the diagonal pertain to samples with the United States excluded. Sample sizes are weighted sample counts.
b. ICS = International Crime Survey, for 1989 and 1992. For nations covered in both years, two-year averages were computed.

as conservative because they would be larger if survey measures were not affected by random sampling error.

While it has been shown that a valid gun ownership measure is available for multiple nations, cross-national research nevertheless shows only very limited potential for assessing the impact of gun levels on violence levels, for several reasons. First, gun availability data are still available for only a handful of nations (36 in the largest relevant study—Kleck 1997:254), making results extremely vulnerable to slight changes in the composition of the samples analyzed. Second, outside of the United States, there is far less variation in gun prevalence across those nations for which data are available than there is across cities, states, or counties within the United States. This limits the capacity to detect the effects on violence of differing levels of gun ownership. In the International Crime Survey sample, excluding the United States, the percentage of households with guns was confined to the range from 2 percent to 32 percent (Killias 1993), while in the United States, the figure ranges from as low as 1 percent or 2 percent in urban areas in the northeast (Kleck and Patterson 1993) to as high as 80 percent or more in rural areas of the west or south. For example, 1990s GSS data indicated that only 15.5 percent of households in New Jersey reported a gun (Smith and Martos 1999), while a 1990 survey of Montana adults found that 78 percent of the households possessed at least one gun (Floyd and Wilson 1990).

Validity of Indicators of Trends in Gun Levels

Some analysts have performed tests of gun indicators' cross-sectional validity, and simply assumed that they must also be valid as indicators of changes in gun levels over time. For example, Miller, Azrael, and Hemenway (2001: 478) used a multiple time series design to estimate the effect of gun levels on the incidence of fatal gun accidents, but their test of the validity of their gun proxy, Cook's (1991) measure, was based entirely on correlations with survey measures across states and regions. Unfortunately, none of the proxies that are valid indicators of cross-sectional variation in gun levels, including PSG, are valid indicators of cross-temporal variation.

Table 5 displays correlations among gun indicators using national-level annual data for the period 1972 through 1999. Variables were expressed as the percentage change from the previous year. The criterion measures were once again direct survey measures of household gun prevalence. To have survey measures for every year, results of all known national surveys were used, and averaged where multiple surveys were available for a given year (Kleck 1997:98-99; Lexis-Nexis 1999). Gun prevalence for 1979, the only year without national survey data on gun ownership, was estimated as the average of the 1978 and 1980 figures. A wide array of indicators were tested, ranging from measures reflecting the cumulated size of the national private gun stock (indicators 3 and 4, used in Kleck 1979, 1984) to measures based on the share of violent acts involving guns (indicators 5 to 8), NRA membership (figures derived from wire service reports of NRA membership claims—Lexis-Nexis 1999), indicators of hunting, the rate of gun dealers (Federal Firearms License holders per 100,000 population—U.S. Bureau of Alcohol, Tobacco and Firearms 2000), and measures of law enforcement activity aimed at violations of weapons laws (indicators 13-16).

Detailed discussion of most of these potential proxies is unnecessary because the data indicate that none of them show evidence of validity as measures of trends in gun levels. Focusing on the top row of numbers, in columns 3 to 16, the correlations are all weak and often negative. None of the measures, including the gun stock measures (indicators 3 and 4) used by Kleck

TABLE 5: Validity of Gun Indicators Over Time—Annual Observations, United States, 1972 to 1999 (all variables were expressed as percentage change from previous year)

	Pearson Correlations/One-Tailed Significance														
	(2)	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*
(1) % Households with gun (GSS)	.65	.10	–.02	–.34	–.02	–.17	.06	–.04	.00	.14	–.18	–.00	–.19	–.15	–.03
	.00	.32	.46	.04	.45	.20	.39	.42	.50	.23	.19	.49	.18	.24	.44
(2) % Households with handgun (GSS)		.29	.23	.00	.23	.04	.25	.13	–.17	.34	.18	.22	–.04	.02	.07
		.09	.16	.50	.15	.44	.14	.28	.25	.05	.21	.21	.44	.47	.38
(3) Cumulated gun stock per 100,000 population			.93	.39	–.02	–.14	–.03	.03	.28	.52	.16	.66	.31	.10	.22
			.00	.02	.46	.26	.45	.44	.10	.00	.21	.00	.06	.32	.14
(4) Cumulated hand-gun stock per 100,000 population				.47	.01	–.00	.06	.12	.27	.47	.23	.69	.34	.19	.34
				.01	.48	.50	.38	.27	.11	.01	.13	.00	.05	.18	.04
(5) % Suicides with guns					.29	.16	.33	.26	–.17	.49	.68	.46	.57	.44	.43
					.07	.23	.04	.10	.21	.00	.00	.02	.00	.01	.01
(6) % Homicides with guns						.52	.61	.36	–.07	.27	.90	–.12	.09	.70	.61
						.00	.00	.03	.37	.08	.00	.32	.33	.00	.00
(7) % Robberies with guns							.55	.27	.03	–.02	.49	–.15	.10	.37	.28
							.00	.09	.45	.46	.01	.26	.32	.04	.09
(8) % Aggravated assaults with guns								.51	.00	.04	.62	.10	.36	.52	.42
								.00	.50	.43	.00	.34	.04	.00	.01
(9) Fatal gun accidents per 100,000 population									–.34	.06	.40	.09	.26	.62	.57
									.05	.38	.02	.36	.10	.00	.00
(10) % $ value stolen property due to guns										–.10	–.13	.25	.31	–.29	–.29
										.33	.27	.15	.07	.08	.08
(11) *Guns & Ammo* subscriptions per 100,000 population											.42	–.06	.17	.18	.23
											.01	.40	.20	.19	.13
(12) Cook's gun density												.10	.33	.75	.67
												.34	.05	.00	.00

(13) NRA members per 100 population	.32	.18	.31
	.10	.23	.10
(14) Hunting license holders per 100,000 population		.13	.13
		.26	.26
(15) Weapons arrests/total arrests			.87
			.00
(16) Weapons arrests per 100,000 population			

(1979, 1984), is significantly and positively correlated with either gun or handgun prevalence among U.S. households, as measured by surveys.

Two measures merit special attention because they have been used in recent research. First, despite its excellence as an indicator of cross-sectional variation in gun levels, PSG is not positively and significantly correlated with either of the survey criterion measures. Indeed, changes in PSG are actually *negatively* correlated with changes in household gun prevalence. Change in PSG is significantly correlated with change in many of the other potential measures of gun levels (measures 3, 4, 8, and 11-16), but even these correlations are far too weak—most under .50, the largest .57 (excluding the artifactual association with Cook's [1991] measure) to inspire confidence in PSG as an adequate proxy for trends in gun levels. PSG was used in recent multiple time series studies by Cook and Ludwig (2002) and Miller et al. (2002).

Second, change in the rate of subscriptions to *Guns & Ammo*, used by Duggan (2001), is likewise not positively and significantly correlated over time with change in household gun or handgun prevalence, and is even less strongly correlated with the other potential proxies than PSG was (none exceeded .52).

Unfortunately, there are at present no known proxies that can be shown to successfully track trends in gun prevalence. Yet analysts have nevertheless claimed certain proxies are valid, so it is worth examining how they justify these claims. Cook and Ludwig (2002) used PSG in their multiple time series study of gun levels and claimed that PSG is significantly associated over time with the GSS measure of the percentage of households reporting guns, measured for selected years over 1973 to 1998, for each of the nine Census regions. They tested the validity of PSG using a multiple time series design, estimating a fixed effect model with the GSS survey measure of household gun prevalence as the dependent variable, and PSG as an independent variable, controlling for dummy variables representing each Census region, thereby controlling for cross-region differences in gun levels.

Cook and Ludwig (2002) concluded that the PSG measure was a valid indicator of trends in gun levels merely because the regression coefficient for PSG was large relative to its standard error and thus statistically significant. The size of a regression coefficient, however, cannot tell the analyst whether the proposed proxy is a good one. Because there is no upper limit to the size of a multiple regression coefficient (or the ratio of the coefficient over its standard error), there is no way to meaningfully judge how large or close to a perfect correlation the association is. And of course even very weak associations can be statistically significant. Instead of establishing a strong association between their indicator and the criterion, all Cook and Ludwig demonstrated was that the association was not likely to be zero.

Very different findings are obtained if one simply computes the conventional bivariate correlation coefficients. The correlations for the association over time, within each Census region, between PSG and GSS measures of household gun prevalence are shown in Table 6. They indicate that PSG is not significantly correlated over time with direct survey measures of gun prevalence. This is true even within regions (i.e. controlling for cross-region differences). In fact, PSG is generally not even positively correlated with survey measures, never mind strongly correlated. Whether the variables were measured in their levels or as the percentage change from the previous year, they were only weakly associated with the GSS criterion measure of gun prevalence; 11 of the 19 correlations were *negative*, and only one of the eight positive correlations was significant at the .05 level (one-tailed). Thus, the full set of findings demonstrate that PSG is not significantly correlated over time with direct survey measures of gun prevalence.

In sum, despite its value as an indicator of cross-sectional differences in gun levels, PSG is uncorrelated over time, at either national or regional levels of analysis, with gun levels. It therefore cannot be used in longitudinal research such as work using time series, panel, or multiple time series designs. Oddly enough, this is essentially the same conclusion that Cook (1985) arrived at many years ago: "I recommend against the use of [percentage of suicides committed with guns and percentage of homicides committed with guns] in statistical work involving intertemporal data" (p. 8). It is unclear why he reversed himself and decided to use PSG for cross-temporal research (Cook and Ludwig 2002). The more recent data presented here show that his initial assessment was correct.

Likewise, Duggan's (2001:1093) support for his *Guns & Ammo* measure's cross-temporal validity was based on the same kind of largely uninterpretable regression coefficient estimated with a fixed-effects model. Data in Table 5 indicate that even at the national level, where correlations are generally larger than at the state and county levels that Duggan worked with, the rate of *Guns & Ammo* subscriptions has only a weak and nonsignificant .14 cross-temporal correlation with the GSS measure of household gun prevalence. It is noteworthy that neither Duggan nor Cook and Ludwig reported any simple cross-temporal correlations between their proxies and survey-measured gun prevalence, like those in Tables 5 and 6.

Part of the reason for the lack of support for the cross-temporal validity of these various indicators may simply be that gun prevalence has not actually varied much over the past 40 years. The same survey measures that show enormous variation across areas within the United States, show virtually no variation over time, for either the nation as a whole or its regions. The percentage of U.S. households reporting a gun was 49 in a 1959 Gallup poll, the

TABLE 6: The Validity of the Percentage of Suicides Committed with Guns as an Indicator of Trends in Gun Levels—Correlations with Survey Gun Prevalence Over Time within Each Region[a]

	Pearson Correlation Coefficients (one-tailed significance level)								
Census Region	*New England*	*Middle Atlantic*	*East North Central*	*West North Central*	*South Atlantic*	*East South Central*	*West South Central*	*Rocky Mountain*	*Pacific*
Variables in levels	.35	–.06	–.14	–.14	–.23	–.42	.38	–.44	–.30
	(.09)	(.41)	(.30)	(.30)	(.19)	(.05)	(.07)	(.04)	(.12)
Differences	–.01	.26	.05	.44	.12	.11	–.37	–.12	–.04
	(.49)	(.16)	(.43)	(.04)	(.32)	(.34)	(.08)	(.34)	(.44)

a. The criterion measure was the percentage of households reporting a gun in the General Social Surveys, for all years in which the gun questions were asked: 1973, 1974, 1976, 1980, 1982, 1984, 1985, 1987, 1988, 1989, 1990, 1991, 1993, 1994, 1996, and 1998.

first national survey to ask a gun ownership question. In 1993 it was an identical 49 percent in a Gallup poll, and a near-identical 48 percent in a CBS News poll fielded in August of 1999. Household prevalence of handguns, though it may have increased around 1972 to 1982, has also been roughly constant (25 percent ± 3 percent) since 1982 (Kleck 1997:98-100; Lexis-Nexis 1999).

Likewise, within regions, gun prevalence was virtually constant over the 1973 to 2000 period for which GSS data are available. This alone may be reason enough to question the utility of longitudinal designs for assessing the effects of gun prevalence—if it does not vary over time, there is no opportunity to estimate its effects. Figure 1 visually illustrates the minimal variation in survey-measured gun prevalence and the erratic relationship that two prominent indicators have with this variation in national data.

The apparent near-total lack of variation in gun prevalence means that correlations with all other variables are likely to be highly unstable and probably not very meaningful. Indeed, the observed correlations may primarily reflect correlated errors in measuring the variables, rather than true associations between gun levels and the proxies because known sources of measurement error could easily account for most of the observed variation in measured gun prevalence. Of course, another implication of near-constant gun prevalence after 1973 is that neither upward nor downward shifts in crime and violence rates in this period can be plausibly attributed to (nonexistent) changes in gun prevalence. Thus, Blumstein's (1995) claims that supposed "gun diffusion" among minority youth contributed to homicide increases around 1986 to 1991, and Lott's (2000:41-42, 113-14) contrary claim that supposed increases in gun ownership (based on noncomparable voter exit polls) *suppressed* crime rates between 1988 and 1996, are both implausible because they attributed crime changes to changes in gun levels that apparently did not occur. (Contrary to Blumstein [1995], survey data for the period between the mid-1980s and the early 1990s provide no more indication of an increase in gun availability among young urban Black males than for increases within the population as a whole—Kleck 1997:72-74, 103).

It is possible that multiple-indicator measures may track gun trends more successfully that single indicators, but analysis of the national annual time series data used in Table 5 provides no support for this hope. The best measure combining items listed in that table was a factor score created from the total gun stock, handgun stock, *Guns & Ammo*, and NRA membership indicators (numbers 3, 4, 11, and 13 in Table 5). Annual percentage changes in this factor correlated only .04 with annual percentage changes in the household gun rate and .20 for the handgun rate.

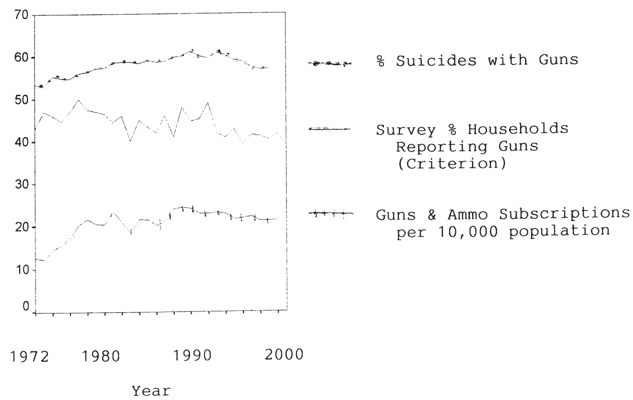

Figure 1: U.S. Gun Indicator Trends, 1972-2000

DISCUSSION AND IMPLICATIONS FOR PAST RESEARCH

The best currently available indicator to use in cross-sectional guns-violence research is the percentage of suicides committed with guns, which has a correlation with the GSS survey measure of household gun prevalence of .87 across large cities, .92 across states, and .95 across nations (Tables 1, 3, and 4). None of the currently available indicators, including PSG, appears to be valid for use in longitudinal research. Therefore, credible longitudinal research on the impact of gun levels on crime and violence rates is not currently feasible, and the results of such research conducted in the past are not credible.

With these findings in mind, we can now reassess prior research on the impact of gun levels on crime rates. Table 7 summarizes this research. The first thing that is apparent is that there has been an enormous variety of ways of measuring aggregate gun levels. With few exceptions (e.g., Cook 1979; Kleck and Patterson 1993), researchers using these measures failed to validate them using any criterion, such as establishing that they correlate well with more direct survey measures. The present validity check results indicate that validity was inadequate for nearly all of the measures used in past cross-sectional research and was poor for *all* those used in research with longitudinal designs.

Beyond poor associations with validity criteria, these measures also have other serious flaws. The measures of the percentage of various crimes committed with guns are vulnerable to the possibility of artifactual associations with crime rates. For example, the number of gun homicides is a component in the numerators of both the percentage of homicides committed with guns

TABLE 7: Gun Level Measures Used in Macro-Level Studies of the Impact of Gun Levels on Crime Rates[a]

Study, Year	*Sample*	*Measure of Gun Level*[b]	*Crime Rates*[c]	*Results*[d]
Brearley (1932)	42 states	PGH	THR	Yes
Krug (1967)	50 states	HLR	ICR	No
Newton and Zimring (1969)	4 years, Detroit	NPP	THR, TRR, AAR, GHR	Yes
Seitz (1972)	50 states	GHR, FGA, AAR	THR	Yes
Murray (1975)	50 states	SGR,SHR	GHR, AAR, TRR	No
Fisher (1976)	9 years, Detroit	NPP, GRR, PGH	THR	Yes
Phillips, Votey, and Howell (1976)	18 years, U.S.	PROD	THR	Yes
Brill (1977)	11 cities	PGC	ICR, THR, TRR	No, Yes, No
Kleck (1979)	27 years, U.S.	PROD	THR	Yes
Cook (1979)	50 cities	PGH, PSG	TRR, RMR	No, Yes
Kleck (1984)	32 years, U.S.	PROD	THR, TRR	No, Yes
Maggadino and Medoff (1984)	31 years, U.S.	PROD	THR	No
Lester (1985)	37 cities	PCS	VCR	No
Bordua (1986)	102 counties, 9 regions	GLR,SIR	HAR, THR, GHR	No
McDowall (1986)	48 cities, 2 years[e]	PGH, PSG	TRR	No
Lester (1988)	9 regions	SGR	THR	Yes
McDowall (1991)	36 years, Detroit	PSG, PGR	THR	Yes
Killias (1993)	16 nations	SGR	THR, GHR	Yes
Kleck and Patterson (1993)	170 cities	[f]	THR, GHR, TRR, GRR, AAR, GAR	No
Lester (1996)	12 nations	PGH, PSG	THR, GHR	Yes
Southwicke (1997)	48 years, U.S.	PROD	THR, TPR, TRR, AAR	No
Stolzenberg and D'Alessio (2000)	4 years, 46 counties	CCW, GUNSTOL	VCR	Yes
Hemenway and Miller (2000)	26 countries	PGH, PSG	THR	No
Lott (2000)	15 states, 2 years	SGR	THR, TPR, TRR, AAR, 3 others	No
Duggan (2001)	19 years, 50 states	GMR	THR, TPR, TRR, AAR	Yes

(continued)

TABLE 7: (continued)

Study, Year	*Sample*	*Measure of Gun Level*[b]	*Crime Rates*[c]	*Results*[d]
Hoskin (2001)	36 nations	PSG	THR	Yes
Killias, van Keseteren, and Rindlisbacher (2001)	21 nations	SGR	THR, TRR, TAR, GHR, GRR, GAR	No
Sorenson and Berk (2001)	22 years	HGS	THR	Yes
Cook and Ludwig (2002)	22 years, 50 states	PSG	BUR	Yes
Miller, Azrael, and Hemenway (2002)	10 years, 50 states	PSG, PHG, SGR	THR	Yes

a. Table covers only studies and findings where 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.
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 $ value of stolen property due to guns; HGS = handgun sales; 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)
c. Crime rates: AAR = Aggravated assault rate; BUR = burglary rate; GAR = Gun aggravated assault rate; GHR = Gun homicide rate; HAR = Homicide, assault and robbery index (factor score); ICR = Index crime rate; RMR = Robbery murder rate; THR = Total homicide rate; TPR = Total rape rate; TRR = Total robbery rate; VCR = Violent crime rate
d. Yes = Study found significant positive association between gun levels and violence; No = Study did not find such a link.
e. Panel design, two waves.
f. 5-item factor composed of PSG, PGH, PGR, PGA, and the percentage of dollar value of stolen property due to stolen guns.

(the gun levels measure, usually used as an independent variable) and either the gun homicide rate or the total (gun plus nongun) homicide rate (used as a dependent variable). This could create a positive association between the gun ownership measure and the crime rate, even if there were no causal relationship, especially given that gun homicides make up a majority of total homicides. Whereas Cook (1979) and Kleck and Patterson (1993) took steps to avoid this problem, other researchers did not (Brearly 1932; Brill 1977; Fisher 1976; Seitz 1972). For example, Hemenway and Miller (2000) used Cook's (1991) measure in a way that Cook was wise enough to avoid: as a predictor of homicide rates. While the authors found no significant association between PSG (a valid gun measure without artifactual association problems) and homicide rates across 26 nations, they found significant associations twice as large when using the Cook measure, and based their conclusions on the latter findings. As we have seen, the Cook measure's homicide component adds nothing to its validity as a gun proxy. The likely reason for the far larger correlations obtained when the Cook measure was used is that both national homicide rates ([gun homicides + nongun homicides]/population) and the percentage of homicides committed with guns ([gun homicides/total homicides] × 100 percent) contain a common component in their numerators: the number of gun homicides, which artificially inflates the association.

The "percent gun" measures also reflect not only the availability of guns but also the preference of the criminal population for using guns in crimes and thus their willingness to inflict fatal injury (Brill 1977:19-20). While availability certainly affects how often criminals use guns in crimes, the "lethality" of offenders (i.e. their willingness to inflict potentially lethal injury on others) affects this choice as well (Cook 1982). Consequently, the "percent gun" indicators confound gun availability with the average lethality or violence proneness of the criminal population, and can thereby produce gun/violence associations that are virtual tautologies.

In similar fashion, if PSG is used as a predictor of suicide rates (as was done in Miller et al. 2002), it not only will have the common components problem but will also reflect the average level of suicidal intent in the population, assuming that suicidal intent (the intention to actually kill one's self rather than merely make a suicidal gesture as a "cry for help") is on average higher among people who kill themselves with guns than those who, perhaps accidentally, kill themselves with usually nonlethal methods such as swallowing a few prescription pills (for evidence supporting this assumption see Kleck 1997:272).

Furthermore, PSG could vary due to shifts in prospective suicides' preferences for suicide methods that might be substituted for shooting, rather than because of variations in gun availability.

The problems that can arise with use of percentage-gun measures can be seen with a time series study of the impact of gun levels on homicide rates in Detroit. McDowall (1991) used a two-item index of gun availability composed of (1) the percentage of robberies committed with guns, and (2) PSG.

Neither measure is valid as an indicator of variation in gun levels over time. The data in Table 5 showed that the correlation over time between the percentage of households reporting gun ownership and either of these measures is actually negative (see column 7 of the first row). Although McDowall cited Cook (1979) for validation of his gun measures, Cook himself had explicitly rejected the use of PSG as an indicator of cross-temporal variation in gun availability (Cook 1985). Instead of measuring trends in gun availability, the robbery measure in McDowall's (1991) index more likely reflected changes in the average "lethality," or willingness to inflict potentially lethal violence, among Detroit robbers. This would independently influence trends in lethal violence, as well as being correlated with the share of robbers who carry guns.

The percentage of stolen property due to stolen guns (indicator 10 in Table 1) is likewise a measure of multiple concepts. Stolzenberg and D'Alessio (2000) interpreted this measure as only a reflection of "illegal gun availability," (p. 1468) (i.e. gun possession among criminals), but it necessarily must also reflect gun availability among the largely noncriminal population from whom the guns were stolen.

The most extreme examples of poor measurement in research on guns and violence are found in studies that conclude or hint that there is a causal link between the two, but without measuring gun levels at all. Instead, trends in gun levels are simply indirectly inferred from the relative size of changes in gun violence (e.g., homicides committed with guns) and changes in nongun violence (e.g., homicides committed without guns).

Since national homicide data first became available in 1933, there have been two periods of significant increases in national homicide rates, one in 1963 to 1974, when the rate jumped from 4.7 homicides per 100,000 population to 9.9, and a more limited one in 1987 to 1991, when the rate increased from 8.6 to 10.4 (Kleck 1997:262-63). In both periods, almost all of the increase occurred among gun homicides, leading some observers to infer that the increases were caused by increases in gun levels (Blumstein 1995; Farley 1980).

For example, Blumstein (1995) noted that homicide increases among young Blacks in the 1985 to 1991 period occurred almost entirely in the gun homicide category, and speculated that violence among drug sellers had motivated others, including those not involved in drug selling, to acquire guns, leading to increased minority youth homicide. Blumstein's reasoning was fallacious because there is no necessary logical relationship between trends in gun availability and the relative sizes of shifts in gun homicide and nongun homicide, or changes in the share of homicides committed with guns (Kleck 1997:256-58).

Indeed, there is no cross-temporal correlation at all between gun availability and the share of homicides committed with guns (see Table 5, column 6, first row). Furthermore, gun homicide rates in the United States are more volatile than nongun homicide rates, and both increases and decreases in homicide are proportionally larger among gun homicides than among nongun homicides, even during periods when changes in gun availability or gun control strictness could not have been responsible (Britt et al. 1996; Kleck et al. 1993).

There is no direct empirical indication, apart from gun violence increases themselves, that gun availability (including gun carrying) increased during 1985 to 1991 either in the general population or in high-violence subsets of the population, and thus no support for the Blumstein (1995) speculation about "gun diffusion" among urban minorities. Surveys indicate no trend, or perhaps a slight decline, in gun prevalence among high-violence subsets of the population during this period (Kleck 1997:103, 257).

A simpler and more plausible explanation than Blumstein's (1995) of the gun/nongun homicide trends would follow naturally from his observation that homicide increases were largely confined to killings linked to illicit drug transactions or street gangs. Because 90 percent of all murders of drug dealers and gang members were committed with guns (U.S. Federal Bureau of Investigation, 1995:20), increases in drug- and gang-related homicides would occur almost entirely in the gun homicide category even in the complete absence of any increases in gun availability (including gun carrying in public places), either in the general population as a whole or among high-risk persons. In sum, there is no substitute for actually measuring gun levels because one cannot infer differences in gun levels, across areas or over time, from the fact that violence differed or changed more in the gun category than in the nongun category.

CONCLUSIONS

Many scholars have claimed to have found a significant positive effect of gun ownership on crime or violence rates (see Table 7), but all of these studies share at least one of two critical problems: (1) use of an invalid measure of gun levels, and (2) a failure to convincingly resolve causal order issues in the relationship between gun levels and crime/violence rates.

Table 7 (see note b) shows that past macro-level guns-violence studies have used a large and diverse set of proxies for gun levels, all of which are variants of the measures assessed here. The present results indicate that almost all of the measures used in cross-sectional research, and *all* of those used in longitudinal studies, are apparently invalid measures of gun levels. Thus, with the exception of the few studies that used PSG (or indexes including PSG) or direct survey measures in cross-sectional research (e.g., Cook 1979; Killias et al. 2001; Kleck and Kovandzic 2001; Kleck and Patterson 1993), the supposed gun-crime associations estimated in nearly all past research must be regarded as uninterpretable on the simple grounds that gun levels were not adequately measured and many of the proxies used were measuring some violence-related concept other than gun levels. Even ignoring severe problems in identification and model specification, most past research, and all longitudinal research, has generated meaningless findings on gun effects because the proxies used cannot be legitimately interpreted as measures of gun availability.

Discounting this voluminous body of uninterpretable work, the best available research indicates that there is no net effect of general (criminal and noncriminal combined) gun ownership on violence rates (Kleck 1997:248-51; Kleck and Kovandzic 2002; Kleck and Patterson 1993:274). This null association, however, may mask two causal effects of gun ownership on violence rates of opposite sign. Gun ownership among criminals may increase crime while guns among noncriminals may decrease it. Measures of gun ownership that do not distinguish gun ownership in the two populations may miss these differing effects.

On the other hand, there is no empirical evidence that the two are independent. Given that most criminals acquire guns as a result of transfers from noncriminals, it is likely that the same places that have high noncriminal gun ownership also have high gun levels among criminals. If so, proxies for noncriminal gun levels might serve as satisfactory proxies for criminal gun levels. It would not, however, be possible to separate the effects of criminal and noncriminal gun possession. This issue needs to be explored, and devising methods for distinguishing levels of gun ownership among criminals from levels among noncriminals therefore should be a high priority in future research.

There are no obvious existing proxies that clearly measure gun availability among criminals. This does not, however, mean that such measures are unattainable. For example, if police executives were genuinely serious about tracking gun availability among the criminals in their jurisdictions, it would be an easy matter to revise arrest report forms to include check-off boxes indicating whether the arrestee possessed a weapon at the time of arrest, and what type of weapon it was. If significant numbers of agencies adopted this simple and inexpensive practice, variations in gun levels among criminals, across areas and over time, could be directly measured.

NOTE

1. It was never necessary to add .1 to values to permit logging in the time series analysis, and it was necessary for just one (percent gun, homicides) of the measures in the cross-national analysis, and two (fatal gun accidents, carry permits) of the 14 indicators in the state analysis. It was necessary for 5 of the 18 indicators in the 45-city analysis. In this analysis, the gun theft measure was 0 for 8 cities, 4 other variables were scored 0 for just 1 or 2 cities, and the remaining 13 measures did not have any zeroes.

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