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Does Gun Control Reduce Violent Crime?

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Abstract

Do gun control laws reduce violence? To answer this question, a city-level cross-sectional analysis was performed on data pertaining to every U.S. city with a population of at least 25,000 in 1990 ($n =$ 1,078), assessing the impact of 19 major types of gun control laws, and controlling for gun ownership levels and numerous other possible confounders. Models were estimated using instrumental variables (IVs) regression to address endogeneity of gun levels due to reverse causality. Results indicate that gun control laws generally show no evidence of effects on crime rates, possibly because gun levels do not have a net positive effect on violence rates. Although a minority of laws seem to show effects, they are as likely to imply violence-increasing effects as violence-decreasing effects. There were, however, a few noteworthy exceptions: requiring a license to possess a gun and bans on purchases of guns by alcoholics appear to reduce rates of both homicide and robbery. Weaker evidence suggests that bans on gun purchases by criminals and on possession by mentally ill persons may reduce assault rates, and that bans on gun purchase by criminals may also reduce robbery rates.

Keywords

gun control, violence, gun ownership

The United States has higher rates of violent crime, both fatal and nonfatal, than all but a handful of the industrialized nations of the world (Killias, van Kesteren, & Rindlisbacher, 2001). Many of these crimes are committed by offenders armed with guns. In 2014, 67.9% of homicides, 40.3% of robberies, and 22.5% of aggravated assaults known to police were committed by criminals with guns (U.S. Federal Bureau of Investigation [FBI], 2015). The United States also has a higher rate of private gun ownership than any other industrialized nation (Killias et al., 2001). This combination of facts has led many to conclude that America's high rate of gun ownership must be at least partially responsible for the nation's high rates of violence, or at least its high rate of homicide. This in turn has led many to conclude that stricter gun laws can reduce violent crime, especially the homicide rate (e.g., Cook & Ludwig, 2000).

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Theory

Why should gun levels influence rates of crime or violence? And if gun levels do have effects, how might gun control laws decrease crime rates? If a gun is available to a prospective aggressor, it can 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 (Cook, 1976; Kleck, 1997, pp. 215–240; Newton & Zimring, 1969). The sight of a gun also might trigger attacks by angered persons, due to the learned association between guns and violence. On the other hand, research on real-world crime incidents indicates that aggressor possession of guns is generally associated with a *lower* likelihood of attack and injury to the victim (Kleck & McElrath, 1991). Once an injury is inflicted, however, it is more likely to result in death 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 among attackers using guns, rather than just the deadliness of the weapon itself (Cook, 1982, pp. 247–248; Wright, Rossi, & Daly, 1983, pp. 189–212).

Gun control laws, in turn, are intended to reduce crime and violence rates by restricting the availability of firearms among persons believed to be at higher risk of committing acts of violence. Although some laws hypothetically might do this by reducing gun levels in the general population, neither the federal government nor any state has ever banned the ownership of guns or even any large subset of guns, such as handguns. Further, prior research indicates that existing laws have no measurable effect on overall gun ownership levels in the population as a whole (Kleck & Patterson, 1993). Instead, gun laws are intended to block acquisition, possession, and criminal use of guns by members of high-risk subsets of the population, such as convicted criminals, mentally ill persons, alcoholics, or drug addicts.

Further, some gun laws are designed to reduce violence in ways that do not require reducing gun ownership in any subset of the population. For example, some controls aim to reduce unlicensed carrying of concealed guns through public spaces, reducing gun possession in situations likely to erupt in violence. Other controls try to deter criminal use of firearms by imposing enhanced (add-on) penalties for gun use in crimes, above and beyond the baseline penalties provided for the underlying crimes.

On the other hand, critics argue that gun control laws could increase crime, by disarming prospective victims, reducing their ability to effectively defend themselves, and possibly reducing any deterrent effect that victim gun possession might have on offenders (Kovandzic & Marvell, 2003; Lott & Mustard, 1997; Moody & Marvell, 2005). This could happen even with laws narrowly aimed at disarming subsets of the population at high risk of offending, since such groups are also at high risk of victimization (Kleck, 1997, Chapter 5; Tark & Kleck, 2004). For example, few mentally ill people commit violent acts, but they are at higher risk of victimization (Friedman, 2006), so banning sales of guns to this group could reduce defensive and deterrent effects of their gun ownership more than it reduced its violence-elevating effects. If this happened, the net effect on violence rates could be positive.

The purpose of the present study is to provide a methodologically sound evaluation of the impact of gun control laws on violent crime rates.

Prior Research on the Impact of Gun Control Laws on Crime

There are many macro-level studies of the impact of gun control laws on violent crime rates. Most report no significant negative association between violent crime rates and the gun control law under study (see reviews by National Research Council, 2004; Hahn et al., 2005; Kleck, 2013), though a few (e.g., Koper & Roth, 2001; Loftin, McDowall, Wiersema, & Cottey, 1991) find evidence of crime-reducing effects of some gun control laws. Regardless of their findings, all these studies can be criticized on methodological grounds. The central problems have been (1) the failure of analysts to properly account for other factors that affect violence (omitted variable bias), (2) studying heterogeneous states rather than more homogenous cities or counties, (3) a failure to control for gun ownership levels, (4) a failure to take account of local gun ordinances, (5) the use of unreliable secondary sources of information on gun laws, (6) the use of uninformative "gun control strictness" indexes that lump together heterogeneous mixtures of gun laws, and (7) the opposite problem of studying a single arbitrarily chosen instance of a given type of gun control, which precludes generalization of findings and risks confusing the effects of a gun law with the effects of other crime-control measures likely to accompany it.

One research design commonly used to assess the impact of gun control laws on violence rates has been the interrupted time-series design (ITSD). In the typical ITSD study, monthly violence rates for a single jurisdiction are analyzed to see if there is a significant downward shift in violent crime rates around the time a new gun law went into effect. The ITSD design requires that the researcher convincingly rule out changes in other crime-related factors (other than the legal change under study) as alternative explanations for observed shifts in crime trends (Campbell & Stanley, 1963). This problem is especially acute in time-series studies of gun laws due to the fact that state legislatures are almost continuously making large numbers of changes in the criminal law, often for the express purpose of reducing crime. For example, over the period 1973–1992, the Florida legislature passed an annual average of 381 general bills (this total excludes resolutions), including an average of 2.45 gun control bills per year (Etten, 2002). Almost every enactment of a new gun law is accompanied by dozens or hundreds of other changes in criminal law passed during the same legislative session, making it virtually impossible to separate the effects of one new law from those of others, enacted at the same time, and also intended to reduce crime. Since the ITSD approach is univariate, it cannot explicitly rule out any specific changes that might account for observed shifts in violent crime rates.

Panel/Multiple Time-Series Designs

After 1997, ITSDs of individual laws in single jurisdictions were largely replaced by panel designs in which violent crime rates in large numbers of areas with and without the law under study were tracked over time. For example, at least two dozen panel studies have assessed the impact of "right to carry" (RTC) laws, using a county-level panel data set first compiled by Lott and Mustard (1997). They are a highly overlapping set of analyses of the same fatally flawed body of data (see Kovandzic, Marvell, & Vieraitis, 2005, pp. 294–302, for a summary; the review by Moody & Marvell, 2008).[1] The results of both the original Lott–Mustard study and two dozen reanalyzes of their data set are all essentially uninterpretable because they are, regardless of methodological variations, based on analyses of a meaningless set of county-level "crime rates." Better empirical evidence on the impact of these laws was provided by researchers who gathered their own crime data rather than merely reanalyzing the flawed Lott–Mustard county data set. The best evidence was produced by Kovandzic, Marvell, and Vieraitis (2005), whose panel data set pertained to 189 large cities (covering the period 1980–2000) using city-level crime data that did not have the problems associated with the attempt to aggregate crime counts of multiple local jurisdictions to create county violent crime rates. They found that RTC laws have no measurable effect on violent crime rates. Kovandzic and Marvell (2003) likewise found no impact of RTC laws on violence levels when using more complete county-level crime data for Florida counties and a more refined measure of the laws' "treatment" effects—the number of valid concealed carry permits in each county in each year.

Panel studies (also referred to as multiple time-series studies) were a substantial improvement over ITSD studies because they exploit evidence concerning numerous instances of new gun laws in multiple jurisdictions, but they also suffer, albeit it to a lesser degree, from potential bias due to omitted variables. The omitted variable bias problem occurs here because analysts fail to or are unable to control for more than a few genuinely crime-related, time-varying factors (including other gun laws) that affect violence rates.

It is important to note that the issue here is the lack of needed controls for time-varying factors likely to influence violent crime rates (such as the passage of other gun laws), because the typical panel study uses a "fixed effects model" that is based *solely* on the cross-temporal relationship between the gun law under study and violence rates. Panel studies typically include dummy variables that represent each year and each state, in an attempt to indirectly control for the effects of variables that differ across years and states, but that are not explicitly measured—these are called "fixed effects" variables. There is a common misperception among some scholars that the inclusion of these fixed effects variables minimizes the need to explicitly control for potential confounding time-varying factors. Although fixed effects help in creating ceteris paribus conditions by capturing all unobserved, time-invariant factors that affect violent crime rates and are correlated with the policy variable under study, they can still produce inaccurate estimates of the effect of the policy variable if other omitted variables are correlated with *changes* in the policy variable (Wooldridge, 2000).

How consequential can omitted variable bias be in panel studies with respect to conclusions about the effect of gun laws on violence rates? A recent state panel study of 13 gun laws enacted between 1977 and 2000 by Moody and Marvell (2006) found that researchers analyzing any of the laws in isolation would have got their conclusions wrong for 5 of the 13 laws (see their table 7).

Pioneers in the use of panel data for crime policy studies, Moody and Marvell conclude that "policy analysts must be careful to properly identify the coefficients in any policy analysis equation and to avoid omitted variable bias due to omitting other relevant laws which can lead to spurious results" (p. 14). Lott and Mustard (1997, p. 38) controlled for just two other types of gun laws in their assessment of "shall issue" carry laws, whereas Ludwig and Cook (2000) controlled for none in their evaluation of the federal Brady law. In contrast, Kleck and Patterson (1993, pp. 259–260) controlled for up to 19 types of gun laws in their cross-sectional (CX) analysis of gun laws.

Given the importance of controlling for potential confounding time-varying factors, why have researchers chosen to include so few in their violence models? The answer is quite simple—the data simply do not exist or would be difficult to collect. For example, the U.S. Census Bureau and other federal/state government agencies only compute intercensal estimates for a handful of potential crime-related variables (e.g., poverty), and even those estimates are only available for larger areas such as states.

The problem with using larger aggregates such as states as a unit of analysis in a study of gun laws is that states are more heterogeneous than cities and counties, which aggravates the problem of aggregation bias. For example, a state as a whole might be high on both gun ownership and violence rates, even though the parts of the state that have high gun ownership have low violence rates. Unwary analysts who used state-level data might only find that states with more guns had more violence, failing to realize the specific places with higher gun ownership were not the places with higher violence rates.

Further, the use of state-level data can lead to mismeasurement of the strictness of gun controls to which residents of a given city are subject, as it precludes taking into account local gun control laws, the most restrictive in the nation. Thus, cities or counties are better units of analysis for a study of gun laws, both because they are more homogenous and because they allow analysts to control for both local and state gun controls. Unfortunately, if one wanted to do any kind of longitudinal research, such as a panel study, and needed to gather data describing cities or counties in the years between censuses (e.g., 1994, 1995, etc.), one would find that intercensal data on crime-related variables for cities or counties are virtually nonexistent.

CX Designs

CX designs compare legal jurisdictions, such as states, with each other to see if those with a gun law have lower levels of violence, other things being equal, than those lacking the law. CX designs are often judged by scholars to be weaker than panel designs because it is inherently harder for researchers to establish ceteris paribus conditions with a CX design. If, for example, there are factors unobserved by the researcher that affect violent crime rates and are likely to be correlated with the gun law under study, then one is likely to get a biased estimate of the causal effect of the gun law on crime. The solution is for the analyst to control as many potentially confounding factors as possible before attributing crime reduction effects to gun regulation. Of course, it is not possible to literally hold all else equal because there is no way to know exactly which variables might generate spurious associations or suppress or distort genuine causal effects. Thus, the most sensible procedure is to control for as many relevant factors as available data allow. Unlike ITSD or panel studies, however, CX studies can take advantage of the very large volume of data that is available in census years for cities, counties, or states on possible determinants of violent crime rates.

Likewise, in CX studies, it is easier for researchers to control for other types of gun laws by using, for example, the Bureau of Alcohol, Tobacco, and Firearms (BATF) report on state and local firearm ordinances. Controlling for the presence or absence of other preexisting gun laws is especially important for CX studies because it is likely that places whose residents favor one type of gun control are likely to favor others as well. Thus, failing to control for existing gun laws could lead to ordinary least squares (OLS) estimates for gun laws under study being biased in the negative direction (i.e., implying more of a crime-reducing effect) due to the omission of other gun laws and lead one to conclude that certain gun laws are effective when it is actually other gun laws that reduce violence.

Panel designs are generally preferred to CX designs for purposes of establishing ceteris paribus conditions, but unfortunately panel approaches are simply not feasible in some situations, and this happens to be one of them. This is because (1) it is essential to control for gun ownership levels to avoid biasing gun control law coefficients in a negative direction, but (2) there are no known valid indicators of cross-temporal variation in gun levels, making it impossible to control for gun levels in any kind of cross-temporal analysis (Kleck, 2004). The spurious association problem derives from the simple political fact that larger numbers of gun-owning voters in high gun ownership areas make it politically more risky for legislators to vote in favor of additional gun control measures. Thus, although higher gun ownership might contribute to higher violence rates, it also reduces the likelihood that a given area will have any given gun control law. If gun ownership increases violence but reduces the strictness of gun control, it will generate a spurious negative association between gun laws and violence rates, giving an erroneous impression that weaker gun laws caused higher violence rates, when in fact it was the higher gun levels that caused the higher violence rates. Consequently, any analysis of gun law impact that fails to control for gun ownership levels will yield misleading results.

It is, however, currently impossible to control for changes over time in gun levels because there are *no* valid measures of such changes. Even proxy measures that are excellent indicators of cross-area variation in gun levels, such as the percentage of suicides that are committed with guns (PSG), show no validity for measuring changes over time (Kleck, 2004, pp. 19–25). Although a few scholars have claimed that their validity checks indicated validity of PSG for use in panel studies (Cook & Ludwig, 2003; Duggan, 2001, p. 1093; Moody & Marvell, 2005), in fact the associations they observed between PSG and direct survey measures of gun ownership (used as criterion measures) were almost entirely attributable to cross-area covariation (Kovandzic, Schaffer, & Kleck, 2013). As we demonstrate later, there is virtually *no* correlation over time between PSG and direct survey measures of gun prevalence.

We believe CX data can also be used to approximate ceteris paribus conditions, given the immense amount of macro-level data available to researchers for census years. In fact, one might argue that in the context of policy studies of violent crime that CX data might actually be preferred to panel data sets as few macro-level determinants of violent crime are measured at regular intervals between census years, making it difficult for panel researchers to rule out omitted variables bias. In the next section, we discuss the inability to control for perhaps the most important time-varying factor related to the passage of gun laws, gun ownership levels.

The Need to Control for Gun Ownership Levels

Low or declining gun ownership may be part of what makes it politically feasible to pass new restrictions on guns, but declines in gun levels could also independently reduce violence rates, even if gun laws had no effect of their own on either gun levels or crime. Likewise, in CX analyses, the strictness of gun controls is likely to be negatively correlated with gun ownership levels for the aforementioned political reasons. In our sample of over a thousand U.S. cities, a principle components factor of gun laws was correlated −.52 with our measure of gun ownership—that is, where gun ownership was higher in the general population, gun control was weaker. One of the main arguments for gun control is that gun levels, at least within some high-risk subsets of the population, affect at least some kinds of violent crime. If this were not true, it would be harder to argue that laws restricting guns could affect violence rates. On the other hand, if gun levels do affect crime, and also affect whether gun controls are implemented, then gun ownership levels are an important confounding factor, which must be controlled to avoid spurious negative associations between gun law variables and violence rates. It should be stressed that virtually all gun control laws in the United States are not designed to have their effects by reducing the level of gun ownership in the general population. Confirming this, past research indicates that gun laws in fact do not affect gun ownership levels in the general population (Kleck & Patterson, 1993). Rather, gun control laws are intended to reduce violence either by reducing gun levels within small high-risk subsets of the population or by other means that do not entail reducing gun levels within any part of the population, such as discouraging the unlicensed carrying of guns in public places or their use in crimes. Thus, gun laws do not have their effects on violence by reducing gun levels in the general public; indeed, it is unlikely that it would be politically feasible in America to pass any gun control measures that were likely to significantly reduce gun ownership among the noncriminal majority.

Consequently, general gun levels do not mediate the relationship between gun laws and violence rates. Therefore, we do not include gun ownership levels in our models for the purpose of testing for their indirect effects of gun laws on violence via their effects on general gun ownership levels. The measure of gun levels that we use, the PSG, is a measure of gun prevalence in the general population and has been validated against estimates drawn from surveys of the general population. Instead, we control for general gun levels because the gun ownership level of the general population is a confounding factor that may affect violence rates but is also likely to influence the degree of gun control strictness in a given jurisdiction, thereby generating a spurious negative association between gun laws and violence rates.

An evaluation of the validity of 21 previously used proxies for gun levels shows that although some are valid for purposes of CX comparisons, none show even minimal evidence of validity as a cross-temporal proxies for gun ownership. The CX correlation between the PSG and General Social Survey measures of household gun prevalence at the state-level was .92, whereas the correlation was .95 with similar CX survey estimates for nations. On the other hand, when evaluated for the United

States across years, the correlations for this proxy were exactly .00 with the survey-measured prevalence of handgun ownership, and actually *negative* with survey-based measures of ownership of all guns (Kleck, 2004).

Some authors have nevertheless insisted that PSG is valid for intertemporal purposes, based on the fact that PSG is correlated with General Social Surveys (GSS) gun prevalence estimates in their panel analyses (Cook & Ludwig, 2000; Moody & Marvell, 2005). What these authors all failed to note was that this correlation is driven entirely by the CX correlation between PSG and the GSS gun measures. The cross-temporal correlations are negligible (Kovandzic et al., 2013). Thus, these tests actually indicated that PSG has *no* validity for measuring changes in gun levels.

The exact same problems afflict the effort of Duggan (2001) to establish that the rate of subscriptions to *Guns & Ammo* magazine (GAR) is a valid proxy for changes in gun levels. As shown in Kovandzic, Schaffer, and Kleck (2013), the association he documented between state-level survey estimates of household gun prevalence and GAR was entirely attributable to CX covariation. Across years, there is no significant correlation between GAR and the survey-based criterion measure ($R^2 = .002$).

Because there are no known valid cross-temporal proxies for gun ownership available, it is, at present, impossible to control for gun levels using *any* kind of longitudinal design, including the otherwise preferable panel design. In the absence of valid time-series proxies for gun levels, researchers who want to isolate the effect of gun laws from the effects of gun ownership levels presently have no choice but to rely on CX data. For this and other reasons already discussed above, we use a CX approach in this article.

Method

Our study assesses the impact of gun control laws on violent crime rates using CX data from all U.S. cities with a 1990 population of 25,000 or more ($n = 1{,}078$). These cities accounted for roughly three quarters of the violent crime in the United States in 1990 (U.S. FBI, 1991, pp. 150–151). We use data for 1990 rather than 2000 or 2010 because the city-level suicide data needed to measure the proxy for gun levels were no longer publicly available and there are no feasible and valid alternative measures of gun levels (Kleck, 2004). We are not aware of any evidence that the effects of gun laws of the type that were present in 1990 would have different effects in other time periods, and it has been empirically demonstrated that the effect of gun prevalence on violence has not changed over time (Kovandzic et al., 2013, pp. 528–539). We cannot, of course, say anything about types of gun control that did not exist circa 1990, but note that newer measures have generally been weak controls, due to the unfavorable political climate for passing stronger controls in recent years, and that research has generally found no impact on crime rates for these measures, such as child access protection laws and one-gun-a-month laws (Kleck, 2013, pp. 1405–1406, 1409–1410).

We use a double-log model in which both dependent and independent variables (except for gun law dummy variables) are expressed in their natural logs.[2] Because the dependent variables are logged, the coefficients for the gun law dummy variables can be interpreted as elasticities. The coefficient, when multiplied by 100, is the percentage difference in rates of violent crime in cities with a particular gun law versus those without the law, holding all other factors fixed. Thus, a coefficient of $-.16$ for a particular type of gun law in a robbery analysis means that cities with the gun law in effect have 16% lower robbery rates that cities without the law. Heteroscedasticity was detected using the Pagan–Hall (Pagan & Hall, 1983) statistic and was handled by using the Huber–White robust estimator of standard errors, which is valid in the presence of heteroscedasticity of unknown form (Wooldridge, 2006).

Violent Crime Rate Variables

The dependent variables are the rates per 100,000 population of total homicide, gun homicide, nongun homicide, total robbery, and total aggravated assaults.[3,4] Data on the total number of homicides, robberies, and aggravated assaults for each city were taken from the FBI Uniform Crime Reports (UCR) for 1989 to 1991 (U.S. Federal Bureau of Identification 1990–1992). Gun and nongun homicide data were taken from the FBI Supplementary Homicide Reports (SHR) computer data set (Fox, 2001; see Note 4). To estimate the number of gun homicides for each city in each year, we multiplied the total number of homicides in the published UCR reports for that year by the ratio of the number of SHR-recorded gun homicides to the total number of SHR-recorded homicides for the corresponding year. Nongun homicides were estimated using the same procedure.

Following convention for CX studies, violence rates were averaged over 3 years, 1989–1991, to reduce the influence of random year-to-year aberrations. For cities missing crime count data for a given year, we estimated missing values by computing the average crime rate for that year in cities in the same census region and same population group (e.g., 25,000–99,999), among cities with valid crime data.

Gun Law Variables

Cities were coded for the presence of 19 major forms of gun control restrictions that were in existence as of 1989 at either the state or city level. Descriptions of the laws, variable names, means, and standard deviations are provided in Table 1. Gun law coding for most laws was based on *State Laws and Published Ordinances—Firearms—1989*, an authoritative and comprehensive verbatim collection of state statutes and local ordinances compiled by the U.S. BATF (1989).

The coding for most gun laws was 1 if the law was present at either the state or city level, regardless of whether the law applied to all types of guns or, as was often the case, only to handguns, and 0 if it was absent. Most of the gun laws fall into one of the six categories: (1) bans on gun possession by members of "high-risk" groups such as criminals and minors, (2) restrictions on sale/transfer/purchase of guns to or by members of these groups, (3) restrictions on the carrying of guns in public places, (4) laws requiring the licensing of gun owners or registration of guns in order for a gun to be legally owned or possessed, (5) restrictions or bans on special gun types such as handguns, and (6) laws requiring a state or local license to be in the business of selling guns, in addition to the license required by the federal government.[5] Gun laws that were too minor or technical to be likely to have any detectable effect on violence rates were not coded, nor were laws that were either universal (or nearly so) across states (e.g., federal laws or bans on machinegun possession) or that were unique to a single jurisdiction. In either of the latter two situations, there was too little variation across cities to reliably detect effects of the laws. The complete gun law coding protocol may be obtained from the senior author.

These state and local laws do not merely duplicate or overlap similar controls at the federal law. The scope of state controls is often considerably broader than seemingly similar federal controls. For example, a state ban on acquisition or possession of guns by convicted criminal may apply to certain misdemeanants as well as felons, whereas the federal ban generally applies only to felons. Likewise, some state restrictions on juvenile acquisition or possession of long guns apply to 18- to 20-year-olds as well as those under 18, while federal law prohibits acquisition only by those under 18. Further, state and local capacity to effectively administer their controls is often considerably greater than that of the federal government. Even after the Brady Act was passed in 1994 (after our study period), background checks in connection with the federal Brady law could not make any significant use of records concerning mental illness—in 2005, over 4.9 million people applied to buy a gun from a federally licensed dealer, 66,705 were rejected via an FBI records check, but only one half of 1% of

Table 1. Variables Used in the Analysis and Descriptive Statistics.[a]

Variables	Mean	*SD*	Source[b]
Crime rates (1989–1991 average, rates per 100,000 residents)			
CRMUR, total homicides	8.02	10.30	A
CRGUNMR, gun homicides	4.80	7.64	B
CRNGUNMR, nongun homicides	3.19	3.31	B
CRROB, total robberies	242.85	282.17	A
CRASLT, total aggravated assaults	454.61	405.73	A
Gun ownership proxy			
PSG, % of suicides with guns, 1987–1993, county	55.67	12.96	C
Excluded instrumental variables (used to instrument for gun ownership)			
PCTREP92, % vote cast for Republican presidential candidate, 1992, County	36.87	7.87	D
VIETNAM, Vietnam veterans per 100,000 population, county	3,343.2	889.91	D
Gun law variables			
Bans on Possession of guns			
CRIMPOS, prohibit possession, criminals	0.80	0.40	E
MINORPOS, prohibit possession, minors	0.50	0.50	E
DRUGPOS, prohibit possession, drug addicts	0.66	0.47	E
ALCPOS, prohibit possession, alcoholics	0.51	0.50	E
MENTPOS, prohibit possession, mentally ill	0.63	0.48	E
Restrictions on transfer of guns			
CRIMBUY, ban on gun purchase by criminals	0.78	0.42	E
MINORBUY, ban on gun purchase by minors	0.95	0.22	E
DRUGBUY, ban on gun purchase by drug addicts	0.74	0.44	E
ALCBUY, ban on gun purchase by alcoholics	0.61	0.48	E
MENTBUY, ban on gun purchase by mentally ill	0.71	0.46	E
BYPERMIT, permit required to purchase gun	0.19	0.39	E
BYAPLIC, application required to purchase gun	0.42	0.49	E
WAITPERH, waiting period to receive handgun	0.50	0.50	E
REGISTER, transfer/sale of guns must be registered with a governmental agency	0.43	0.50	E
CARYHIDN, concealed carrying of loaded handgun prohibited or permit hard to get	0.82	0.39	F
Restrictions on ownership/home possession			
LICENSE, license required to possess gun in home	0.13	0.34	E
Restrictions on special gun types			
HGBYBAN, handgun sales ban	0.00	0.06	E
SNSBAN, ban on sale of cheap handguns	0.12	0.32	E
Regulation of dealing in firearms			E
DEALER, state or city license required for gun dealers	0.53	0.50	E
Control variables			
PCTBLACK, % resident population Black	11.66	15.60	D
LIVLONE, % persons living alone	25.92	6.77	D
PCTHISP, % resident pop. Hispanic origin	10.59	15.55	D
DENSITY, persons per square mile	3,783.2	3,440.8	D
PCTDIV, % resident population 15 and older divorced	9.23	2.27	D
PCT18T24, % resident population age 18–24	12.32	6.60	D
PCT25T34, % resident population age 25–34	18.12	2.91	D
PCTPOOR, % resident population < poverty line, 1989	13.21	8.14	D
OWNEROCC, % housing units owner occupied	58.23	12.79	D
PCTVACAT, % housing units vacant	7.16	3.99	D
PCTHIGH, % persons 25 and up with high school degree	77.54	10.65	D

Note. B = Fox (2001); C = U.S. National Center for Health Statistics (1997); D = U.S. Bureau of the Census (1994); E = U.S. Bureau of Alcohol, Tobacco, and Firearms (1989); F = Thomas Marvell, personal communication (2001).
[a]Unless otherwise noted, each variable refers to a city, as of 1990. In variable descriptions, “county” indicates variable refers to county in which city is located. [b]A = U.S. Federal Bureau of Investigation (1990–1992).

these FBI rejections were for reasons of mental illness. In contrast, some states like Illinois have registries of all persons admitted to psychiatric hospitals in the state, while local enforcement agencies have access to local mental health sources. Consequently, much higher shares of the rejections by state and local agencies were for reasons of mental illness (Bordua, Lizotte, & Kleck, 1979; U.S. Bureau of Justice Statistics, 2006, pp. 2, 5). Likewise, the requirements for state and local dealer licenses are stricter than those imposed by the federal government for issuance of its license. For example, although many states and localities required a criminal background check to become a licensed gun dealer, the federal government, as of 1989, did not do so (U.S. BATF, 1989).

Gun Ownership Levels

Gun levels were measured using the PSG, which research has shown to be the best proxy to use in CX research (Kleck, 2004). This measure has a near-perfect correlation with direct survey measures of household gun prevalence, that is, the percentage of households with one or more guns, so we interpret our measure of gun levels as a measure of household gun prevalence. Vital statistics data for 1987–1993 do not identify locations of deaths for cities with populations smaller than 100,000, so it was not possible to compute city-level measures of PSG for most of our cities. Therefore, we used PSG for the *county* in which the city was located as a proxy for city-level gun availability. Some of the smaller counties had few suicides per year, so misclassification of a few suicides as homicides or accidents in small counties could produce substantial measurement error in a single year's count. Therefore, PSG was computed using data covering the 7-year period from 1987 to 1993, bracketing the census year of 1990. Data were derived from special Part III Mortality Detail File computer tapes (not the general public use tapes) made available to the senior author by the National Center for Health Statistics (U.S. National Center for Health Statistics, 1997). Unfortunately, data access restrictions adopted later by the Centers for Disease Control and Prevention made it virtually impossible to acquire similar data for the years bracketing 2000 or 2010.

Control Variables

In addition to the gun levels measure and gun law dummies, we included 11 city-level control variables that prior macro-level crime research have shown to be reliable predictors of crime. Decisions as to which control variables were included in the violent crime models were based on a review of previous macro-level studies linking violent crime to structural characteristics of macro-level units like cities and states. Most of these control variables account for the causal effects emphasized by motivational, opportunity, and compositional theories of criminal behavior (see Kovandzic, Vieraitis, & Yeisley, 1998; Sampson, 1986; Vieraitis, 2000; the studies reviewed therein). Thus for each gun control law, we control for 18 other gun laws and 11 of the most important structural covariates of violent crime as determined by theory and prior research. Although it is impossible to know for sure if any important time-invariant confounding factors have been omitted, we suggest by holding constant 29 potential relevant factors that we can satisfactorily test the gun law efficacy hypothesis. Table 1 lists and provides a brief description of each control variable along with their means, standard deviations, and data sources.

Fortunately, correlations between the gun law variables and the control variables were generally weak in almost all cases. Of the 209 bivariate correlations between the gun law variables and control variables (19 Gun Laws $\times$ 11 Control Variables), none exceeded .5, and only one reached .4. Thus, there was no serious collinearity between gun law variables and control variables. This was confirmed by examination of condition indices and variance-decomposition proportions (Belsley, Kuh, & Welsch, 1980).

Analytic Procedures

As discussed above, it is important to control for gun levels to isolate the effect of gun laws, but this introduces a complication in estimation of the models. There is a strong theoretical basis, and a large body of empirical support, for the belief that higher violence rates drive up gun ownership levels, as more people acquire guns for self-protection (Bice & Hemley, 2002; Bordua, 1986; Clotfelter, 1981; Kleck, 1979, 1984; Kleck & Kovandzic, 2009; Kleck & Patterson, 1993; Kovandzic, Schaffer, & Kleck, 2012, 2013; McDowall, 1986; Rosenfeld, Baumer, & Messner, 2007; Southwick, 1997). Cities with higher violence rates, therefore, may tend to have higher gun ownership levels, even if gun availability reduced or had no effect on violence rates. When explanatory variables such as gun ownership are endogenous (affected by other variables in the analysis), the OLS estimator is biased and inconsistent (Wooldridge, 2000).

The most common estimation procedure used to address potential endogeneity bias, and the procedure used here, is IVs regression. The key challenge in using IV methods is finding a source of identifying variation: here, variables that are correlated with gun ownership (instrument relevance), that are exogenous with respect to violent crime (i.e., not affected by violent crime—"instrument validity"), and that a priori reasoning and evidence suggest should be excluded from the violent crime equations, that is, do not directly affect violent crime. In the terminology of IV estimation, the instruments used for gun levels are "excluded instruments," and the control variables are "included instruments." If appropriate IVs can be found for gun levels, the method of IVs will produce consistent estimates of the effect of gun levels on violent crime (Wooldridge, 2000).

The excluded IVs used in this article to instrument gun levels are the percentage of the 1992 Presidential vote for the Republican candidate (PCTREP92) and the 1990 county rate of Vietnam-era veterans per 100,000 population (VIETNAM). Both excluded IVs are theoretically important determinants of gun ownership that are plausibly otherwise unrelated to levels of violence. VIETNAM serves as a measure of military training or service, while PCTREP92 serves as a measure of political conservatism. Prior research suggests both variables are significant predictors of gun ownership (Cook & Ludwig, 1997, p. 35; Kleck, 1997, pp. 70–72; Lizotte & Bordua, 1980). In contrast to past research, we carried out extensive specification testing to demonstrate that our excluded IVs VIETNAM and PCTREP92 are relevant and valid.

Tests of the Relevance and Validity of the Instruments

We test for instrument relevance using a heteroscedasticity-robust F-test of the joint significance of the excluded instruments VIETNAM and PCTREP92 in an OLS estimation of the first-stage equation of gun levels (PSG), and we also examine the significance of VIETNAM and PCTREP92 separately using conventional t-statistics. Research by Bound, Jaeger, and Baker (1995) and Staiger and Stock (1997) indicates that an F-test is useful for examining the explanatory power of the excluded IVs, and that F-statistics below 10 indicate weak instruments (Staiger & Stock, 1997, p. 557). The results of the F-test for VIETNAM and PCTREP92 are reported at the bottom of column 2 in Table 2. The second column in Table 2 also reports the estimated coefficients of the excluded IVs and the remaining exogenous regressors in the first-stage equation of gun levels. The first-stage F-statistic reported in column 2 is 38.9, well above the Staiger–Stock rule-of-thumb value of 10. Both of the excluded instruments are correlated with gun levels in the expected directions and at the 0.1% significance level: cities in counties with a greater proportion of Vietnam veterans and persons voting for the Republican candidate in the 1992 Presidential election have higher gun ownership levels. We conclude that our excluded instruments in this estimation are relevant and not "weak."

The second requirement for excluded IVs is that they be uncorrelated with the error process in the violent crime equations. Because the violence equations are overidentified, we are able to assess the

Table 2. The Estimated Impact of Gun Availability and Gun Laws on City-Level Violence Rates: Instrumental Variables Estimates, Gun Ownership Treated as Endogenous.

Predictor Variables	First-Stage Regression	IV Estimation: Dependent Variables: Natural Log of the Violent Crime Rate per 100,000 Persons				
	Gun Ownership	Total Homicide	Gun Homicide	Nongun Homicide	Total Robbery	Total Assault
PSG	—	−0.57 (1.17)	−0.39 (0.72)	−0.14 (0.35)	−0.92 (1.69)	0.55 (1.10)
CRIMINAL	0.02 (1.25)	−0.05 (0.64)	−0.06 (0.76)	−0.04 (0.66)	0.09 (1.29)	0.10 (1.27)
CRIMBUY	−0.08 (3.22)**	−0.03 (0.31)	−0.15 (1.25)	0.10 (1.08)	−0.32 (2.83)**	−0.30 (2.54)*
MINORPOS	−0.08 (4.20)**	−0.06 (0.73)	−0.11 (1.25)	0.04 (0.60)	−0.04 (0.46)	0.01 (0.06)
MINORBUY	0.02 (0.66)	0.13 (1.25)	0.09 (0.78)	0.15 (1.58)	0.30 (2.61)**	−0.09 (0.77)
DRUGPOS	−0.19 (5.89)**	−0.14 (1.00)	−0.11 (0.70)	0.01 (0.08)	−0.05 (0.29)	−0.11 (0.79)
DRUGBUY	0.27 (9.26)**	0.59 (3.45)**	0.54 (3.00)**	0.29 (2.02)*	0.53 (2.92)**	0.13 (0.73)
ALCPOS	0.17 (5.41)**	0.26 (2.00)*	0.26 (1.86)	0.03 (0.27)	0.16 (1.10)	0.46 (3.46)**
ALCBUY	−0.27 (10.11)**	−0.61 (3.63)**	−0.57 (3.11)**	−0.30 (2.13)*	−0.56 (3.13)**	−0.19 (1.13)
MENTALPOS	−0.02 (1.26)	−0.05 (0.57)	−0.06 (0.67)	−0.02 (0.34)	−0.00 (0.03)	−0.19 (2.22)*
MENTBUY	0.02 (0.84)	−0.20 (2.04)*	−0.11 (1.05)	−0.16 (1.90)	0.12 (1.11)	0.20 (2.00)*
BYPERMIT	−0.14 (5.86)**	−0.15 (1.29)	−0.12 (0.95)	−0.05 (0.53)	−0.19 (1.50)	0.32 (2.48)*
BYAPLIC	0.10 (3.51)**	−0.13 (1.17)	−0.02 (0.20)	−0.24 (2.42)*	−0.13 (1.04)	0.15 (1.21)
REGISTER	0.05 (1.96)	0.15 (1.74)	0.12 (1.38)	0.08 (1.11)	0.26 (3.10)**	0.06 (0.70)
WAITPERH	−0.05 (1.59)	0.05 (0.46)	0.07 (0.63)	0.02 (0.21)	−0.16 (1.23)	0.13 (1.09)
CARYHIDN	0.08 (4.64)**	0.25 (2.91)**	0.23 (2.50)*	0.12 (1.66)	−0.02 (0.28)	0.03 (0.38)
LICENSE	−0.18 (7.45)**	−0.31 (2.25)*	−0.34 (2.32)*	−0.11 (0.96)	−0.52 (3.37)**	0.11 (0.78)
HGBYBAN	0.13 (1.59)	0.52 (1.40)	0.69 (1.43)	0.19 (0.92)	0.54 (3.69)**	0.30 (1.64)
SNSBAN	−0.07 (2.21)*	0.03 (0.31)	0.02 (0.15)	0.12 (1.32)	0.01 (0.08)	0.01 (0.05)
DEALER	−0.04 (1.78)	0.24 (3.15)**	0.16 (2.19)*	0.24 (3.64) **	0.24 (3.22)**	0.04 (0.55)
PCTBLACK	−0.00 (0.92)	0.25 (14.89)**	0.27 (14.31)**	0.16 (10.51)**	0.38 (21.27)**	0.20 (9.55)**
PCTHISP	−0.03 (6.46)**	0.00 (0.13)	0.02 (0.59)	0.02 (0.66)	0.11 (3.72)**	0.13 (4.13)**
PCT18T24	0.02 (0.95)	−0.22 (2.19)*	−0.12 (1.16)	−0.32 (3.41)**	−0.16 (1.42)	−0.26 (2.52)*
PCT25T34	−0.11 (2.98)**	−0.13 (0.73)	−0.21 (1.18)	−0.02 (0.13)	−0.34 (1.91)	0.14 (0.82)
PCTPOOR	0.13 (8.44)**	0.35 (3.79)**	0.27 (2.71)**	0.30 (3.73)**	0.20 (1.97)*	0.38 (4.10)**
OWNER	0.10 (2.47)*	0.00 (0.01)	0.05 (0.26)	−0.23 (1.53)	0.05 (0.26)	0.03 (0.21)
PCTVACAT	0.02 (1.73)	0.22 (3.43)**	0.11 (1.63)	0.15 (2.67)**	0.02 (0.27)	0.21 (3.01)**
LIVLONE	−0.16 (5.70)**	−0.31 (2.01)*	−0.39 (2.35)*	−0.06 (0.41)	0.04 (0.26)	−0.04 (0.31)
PCTHIGH	0.03 (0.64)	−1.39 (6.83)**	−1.31 (6.09)**	−0.66 (3.21)**	−1.45 (6.42)**	−0.84 (3.75)**
DENSITY	−0.05 (5.89)**	0.04 (0.77)	0.05 (0.98)	−0.00 (0.03)	0.28 (4.40)**	0.07 (1.31)

(continued)

Table 2. (continued)

Predictor Variables	First-Stage Regression	IV Estimation: Dependent Variables: Natural Log of the Violent Crime Rate per 100,000 Persons				
	Gun Ownership	Total Homicide	Gun Homicide	Nongun Homicide	Total Robbery	Total Assault
PCTDIV	0.13 (4.45)**	0.49 (3.05)**	0.47 (2.92)**	0.25 (1.85)	0.73 (4.31)**	0.15 (1.04)
First-stage regression						
F-statistic	39.76**					
VIETNAM	0.18 (7.62)**					
PCTREP92	0.07 (3.02)**					
Overidentification test						
J-statistic	—	$\chi^2(1) = 0.02$	$\chi^2(1) = 0.16$	$\chi^2(1) = 1.47$	$\chi^2(1) = 0.32$	$\chi^2(1) = 0.16$
p value		.88	.69	.22	.57	.69
R^2	.709	.605	.840	.852	.705	.550
N	1,078	1,078	1,078	1,078	1,078	1,078

Note. Standard errors are computed using Huber–White robust estimate of variance. Robust *t*-statistics in parentheses. Excluded instruments are VIETNAM and PCTREP92.
*Significant at 5%. **Significant at 1%.

validity of the excluded instruments with an overidentification test.[6] Although several such tests exist, we use the *J*-statistic of Hansen (1982) because it is robust in the presence of heteroscedasticity.[7] Hansen's *J*-statistic tests the null hypothesis that the excluded instruments and/or control variables (i.e., included instruments) are exogenous (Baum, Schaffer, & Stillman, 2003). For our IVs to be valid, we should fail to reject the null hypothesis. This test is especially valuable because it fails to have power only if *all* excluded IVs are invalid, that is, are not exogenous. As long as even one of our excluded instruments is valid, the *J*-test is effective in detecting the invalidity of the instruments.[8]

The results of the *J*-test for each violent crime model are reported at the bottom of Table 2. The *J*-statistics are all both small and statistically insignificant. We therefore cannot reject the null that VIETNAM and PCTREP92 and the control variables are exogenous. Thus, the evidence suggests that both of our excluded IVs are exogenous and are correctly excluded from the violent crime equations. To summarize, VIETNAM and PCTREP92 easily pass the relevance (*F*-test) and validity requirements (*J*-test) for instruments.

It might be argued that gun control laws should also be treated as endogenous because the passage of laws intended to reduce violent crime is affected by violent crime. That is, coefficients on the gun law variables might be biased upwards due to another form of endogeneity bias—simultaneity bias attributable to higher violent crime rates leading to passage of gun laws. We consider this to be unlikely, for several reasons. First, most major gun laws in effect in 1989 were originally enacted decades earlier (compare Newton & Zimring, 1969, appendix G with U.S. Bureau of Justice Statistics, 1996), so their enactment could not have been influenced by violent crime rates in 1989–1991, or indeed in any recent years. Second, prior research directly testing for an impact of violent crime rates on gun control strictness has found no effect (Bruce & Wilcox, 1998).

The probable source of the belief that crime affects passage of gun laws is the fact that highly publicized individual acts of violence, such as mass shootings, sometimes trigger the enactment of new gun control laws. Strictly speaking, the effect of news coverage of crime is irrelevant to whether *crime rates* affect the enactment of gun laws unless one assumes a significant correlation between crime rates and *news coverage* of crime. Past research, however, indicates that there is virtually no correlation between crime and the amount of news coverage of crime (Dorfman & Schiraldi, 2001; Garofalo, 1981; Marsh, 1989). In sum, crime news affects passage of gun laws, but crime rates do not.

Finally, there is little reason to believe that higher violent crime rates increase public support for gun control, since survey research shows that public support for gun control is not affected by higher crime rates in the area where a person resides, prior victimization, or fear of crime but instead derives from more stable cultural determinants not directly related to crime (Kleck, 1996; Kleck & Kovandzic, 2009). If higher crime rates do not increase the likelihood that people support more gun laws, there is little reason to expect that higher crime rates, in the past or the present, would increase the level of gun control strictness (Kleck, 1997; Wright et al., 1983). In sum, there is no sound basis for regarding gun control laws as endogenous or influenced by violent crime rates.

Results

Effects of Gun Ownership Levels on Crime

Estimates of the impact of gun ownership levels (as proxied by PSG) on crime can be found in Table 2, in the first row of each column referring to a crime. For example, the coefficient on PSG in the total homicide equation using IV methods is $-.57$ and it is not statistically significant at the 5% level. The basic finding in Table 2 is that when gun levels are treated as endogenous and instrumented with VIETNAM and PCTREP92, gun levels show no net significant positive (violence-increasing) effect on homicide, robbery, or aggravated assault rates. As discussed in Kovandzic et al. 2013, this IVs strategy would probably overstate a violence-elevating effects of gun levels on violent crime, if there were any. Yet, it still produces estimates suggesting net negative (albeit not significant) or null effects. Thus, our findings of a null or negative impact of guns on homicide are strengthened because the potential bias in estimation is likely to be positive, which would work against our interpretation. The implication is that our coefficient estimate is an upper bound on the estimated effect of gun levels on violence rates.

One problem with using city-level data is that cities in the same state may share unobservable characteristics that could lead to intrastate correlation of errors. In such a situation, standard errors are underestimated, leading to inflated *t*-ratios on PSG and the gun law variables. Therefore, we reestimated the regressions in Table 3 using robust standard errors corrected for clustering by state (which allow for arbitrary within-state correlation), and the *t*-ratios for PSG and the gun law variables became much smaller. Nevertheless, when we reestimated regressions analogous to those in Table 2 using the two-step efficient generalized method of moments (GMM) estimator instead of the IV estimator, we obtained results similar to those reported in Table 2. The benefit of the GMM estimator relative to the traditional IV estimator is that it produces parameter estimates that are both consistent and efficient in the presence of heteroscedasticity of unknown form, whereas the IV estimator is consistent but inefficient (Wooldridge, 2000). The coefficients obtained for PSG using the GMM estimator were as follows (*t*-ratios in parentheses): −.56 (1.16) for total homicide, .42 (0.79) for gun homicide, −.14 (0.33) for nongun homicide, −.91 (−1.68) for robbery, and .54 (1.09) for assault. Thus, the results once again indicate no significant positive (violence-elevating) effect of gun ownership on crime rates.

To summarize, we have strong evidence that higher gun levels do not cause more crime. One implication of these findings is that general gun ownership could not mediate the effect of gun control laws on crime. Consequently, if gun laws were passed that were intended to reduce violent crime by reducing general gun ownership levels, they would be likely to fail because even if they did succeed in reducing general gun levels, this would not lead to a reduction in violent crime. Gun ownership levels among criminals, however, may have violence-increasing effects that are canceled out by violence-decreasing effects of gun ownership among noncriminals. Thus, our results do not allow us to rule out the possibility of violence-increasing effects of criminal gun possession.

Effects of Gun Control Laws on Gun Ownership Levels

We then estimate a model testing the effects on gun laws on gun ownership levels, as measured by PSG. Results of the first-stage estimation for this PSG model are presented in column 2 of Table 2. In all, 7 of the 19 gun laws showed an apparent negative effect on gun ownership levels, while 4 others showed an apparent positive effect. This mix of signs on the coefficients suggest the operation of random chance in generating the estimates, in the absence of any compelling reasons to expect some gun laws to increase gun ownership and others to reduce it. Significant negative estimates should in any case be viewed with caution, as they may reflect negative effects of gun levels on the passage of gun control laws, due to the fact that larger numbers of gun-owning voters discourage legislators from supporting new gun controls.

Effects of Gun Control Laws on Violence Rates

The estimates displayed in Table 2 also indicate that most gun control measures appear to have no significant negative direct effect on total (gun plus nongun) violence rates—total homicide, total robbery, and total aggravated assault. Indeed, if the statistical results are taken at face value, some laws appear to increase violence rates. Of the 57 possible effects examined (19 laws, paired with

Table 3. The Estimated Impact of Gun Levels and Gun Laws on City-Level Violence Rates: Ordinary Least Squares (OLS) Estimates, Gun Ownership Treated as Exogenous.

Predictor Variables	OLS Estimation: Dependent Variables: Natural Log of the Violent Crime Rate per 100,000 Persons				
	Total Homicide	Gun Homicide	Nongun Homicide	Total Robbery	Total Assault
PSG	0.23 (1.77)	0.39 (2.98)**	−0.00 (0.00)	−0.06 (0.41)	−0.17 (1.15)
CRIMINAL	−0.06 (0.83)	−0.07 (0.95)	−0.04 (0.70)	0.08 (1.12)	0.12 (1.39)
CRIMBUY	0.03 (0.29)	−0.09 (0.82)	0.11 (1.30)	−0.26 (2.52)*	−0.35 (3.13)**
MINORPOS	−0.00 (0.06)	−0.05 (0.70)	0.05 (0.86)	0.02 (0.31)	−0.05 (0.60)
MINORBUY	0.12 (1.14)	0.08 (0.67)	0.15 (1.56)	0.28 (2.43)*	−0.07 (0.67)
DRUGPOS	0.00 (0.04)	0.04 (0.33)	0.04 (0.35)	0.11 (0.94)	−0.24 (2.22)*
DRUGBUY	0.38 (3.50)**	0.34 (3.02)**	0.25 (2.55)*	0.31 (2.64)**	0.31 (2.71)**
ALCPOS	0.14 (1.36)	0.14 (1.28)	0.01 (0.08)	0.02 (0.22)	0.57 (5.43)**
ALCBUY	−0.39 (4.00)**	−0.35 (3.36)**	−0.26 (2.94)**	−0.32 (3.07)**	−0.38 (3.75)**
MENTALPOS	−0.02 (0.20)	−0.03 (0.33)	−0.02 (0.26)	0.03 (0.40)	−0.22 (2.60)**
MENTBUY	−0.23 (2.40)*	−0.14 (1.35)	−0.16 (1.99)*	0.09 (0.83)	0.23 (2.35)*
BYPERMIT	−0.03 (0.29)	0.00 (0.03)	−0.03 (0.38)	−0.05 (0.58)	0.21 (2.14)*
BYAPLIC	−0.20 (1.96)*	−0.10 (0.95)	−0.25 (2.66)**	−0.21 (1.86)	0.21 (2.02)*
REGISTER	0.12 (1.50)	0.10 (1.17)	0.07 (1.07)	0.23 (3.00)**	0.08 (1.00)
WAITPERH	0.10 (0.89)	0.12 (1.07)	0.03 (0.30)	−0.11 (0.89)	0.09 (0.81)
CARYHIDN	0.18 (2.54)*	0.17 (2.12)*	0.11 (1.68)	−0.09 (1.24)	0.09 (1.14)
LICENSE	−0.15 (1.54)	−0.18 (1.83)	−0.08 (0.99)	−0.34 (3.34)**	−0.04 (0.36)
HGBYBAN	0.46 (1.36)	0.63 (1.42)	0.17 (0.91)	0.48 (3.10)**	0.36 (2.32)*
SNSBAN	0.08(0.69)	0.06 (0.55)	0.13 (1.41)	0.05 (0.50)	−0.03 (0.27)
DEALER	0.26 (3.45)**	0.19 (2.53)*	0.24 (3.68)**	0.26 (3.58)**	0.03 (0.33)
PCTBLACK	0.25 (15.66)**	0.27 (14.93)**	0.16 (10.60)**	0.39 (22.22)**	0.20 (9.44)**
PCTHISP	0.04 (1.67)	0.05 (2.07)*	0.02 (1.16)	0.15 (6.45)**	0.10 (4.18)**
PCT18T24	−0.25 (2.51)*	−0.14 (1.43)	−0.32 (3.49)**	−0.19 (1.71)	−0.24 (2.30)*
PCT25T34	−0.05 (0.30)	−0.13 (0.79)	−0.01 (0.04)	−0.25 (1.49)	0.07 (0.43)
PCTPOOR	0.25 (3.71)**	0.17 (2.44)*	0.28 (4.52)**	0.09 (1.23)	0.48 (7.01)**
OWNER	−0.08 (0.47)	−0.03 (0.17)	−0.25 (1.65)	−0.04 (0.21)	0.10 (0.63)
PCTVACAT	0.19 (3.12)**	0.08 (1.28)	0.15 (2.67)**	−0.02 (0.26)	0.24 (3.41)**
LIVLONE	−0.15 (1.25)	−0.23 (1.83)	−0.03 (0.25)	0.21 (1.73)	−0.19 (1.65)
PCTHIGH	−1.44 (7.31)**	−1.36 (6.61)**	−0.67 (3.27)**	−1.51 (6.87)**	−0.79 (3.68)**
DENSITY	0.09 (2.23)*	0.10 (2.47)*	0.01 (0.24)	0.34 (7.08)**	0.02 (0.56)
PCTDIV	0.36 (2.56)*	0.34 (2.48)*	0.23 (1.91)	0.59 (3.98)**	0.27 (2.08)*
Endogeneity test					
C-statistic	2.86	2.58	0.09	2.82	2.29
p value	.09	.11	.76	.09	.13
R^2	.619	.555	.497	.716	.562
N	1,078	1,078	1,078	1,078	1,078

Note. Standard errors are computed using Huber–White robust estimate of variance. Robust *t*-statistics in parentheses.
*Significant at 5%. **Significant at 1%.

each of 3 violent crime types), results for 2 effects were strongly supportive of gun control effectiveness, whereas 5 others were at least weakly supportive. There were 20 gun law coefficients significant at the .05 level (counting total homicide results, but not counting those for gun homicide and nongun homicide). Given that we had a sample size exceeding a thousand, it is not surprising that many associations were statistically significant, as even weak associations can be statistically significant when analyzing so large a macro-level sample. The 8 negative coefficients, however, were outnumbered by 12 positive ones. There is no clear pattern of these ‘‘effects’’ by either type of gun control or type of violent crime affected. Thus, while it is possible that some gun laws really do increase violent crime while others reduce it, some of these significant coefficients may reflect nothing more than random chance operating with a large number (57) of hypothesis tests, each of them based on a large sample.

There was nevertheless solid support for two beneficial effects of gun control laws on violent crime. First, we found that state laws forbidding the purchase of guns by, or sale of guns to, alcoholics or persons under the influence (ALCBUY) reduced homicide. This is a relatively strong finding because the law not only showed significant negative effects on total (gun plus nongun) homicide but also showed a significant negative effect on gun homicide and a weaker effect on nongun homicide.

Second, the results provide relatively strong evidence that laws requiring a license to possess a gun in the home (LICENSE) reduce homicide. This impact may reflect the consequences of more extensive state-level background checks conducted in connection with licensing. Like the results for laws restricting gun sales to alcoholics, these results showed a strongly supportive pattern of results by gun involvement—a significant negative effect on gun homicide, combined with no significant effect on nongun homicide.

Only weaker evidence is available for gun law effects on robbery and assault, since flaws in available data made it impossible to reliably compare gun law effects on gun violence with their effects on nongun violence (e.g., gun robbery vs. nongun robbery). These weaker findings suggest that robbery may be reduced via state bans on purchases of guns by convicted criminals (CRIMBUY), bans on gun purchases by alcoholics (ALCBUY), and requiring a license to possess a gun (LICENSE). Confidence is increased in the results concerning the latter two laws because our evidence indicated these laws also may reduce gun ownership (some portion of which is ownership by criminals) and appear to reduce homicide, suggesting some capacity to deny guns to criminals.

Two types of gun control laws appear to reduce aggravated assault, though again the findings should be viewed as tentative, for the same reasons stated with connection with findings bearing on robbery. First, state or local bans on the purchase of guns by criminals (CRIMBUY) may reduce aggravated assaults. Second, the results suggest that bans on possession of guns by mentally ill persons may reduce aggravated assault. This latter interpretation, however, is questionable in light of the finding of a significant *positive* association between bans on *purchase* of guns by mentally ill persons and aggravated assault. There is no obvious explanation why banning gun purchases by mentally ill persons would increase assaults, while banning gun *possession* by such people would decrease them.

Using a More Limited Set of Gun Laws

Because we could not know in advance which of the 19 gun law measures affected violent crime, we initially specified all 19 gun control variables to be included in each violent crime equation. As discussed above, close examination of collinearity diagnostics did not reveal a harmful degree of collinearity among the gun law variables. Nevertheless, there is *some* collinearity among the gun law variables that could inflate standard errors somewhat and thereby bias hypothesis tests in favor of the null (no effect) hypothesis. Therefore, each violent crime equation was reestimated so as to test the effects of just nine stronger gun laws thought to be especially likely to show effects on crime—bans on the possession of guns by criminals and minors (CRIMPOS, MINORPOS), bans on sale/transfer of guns to criminals and minors (CRIMBUY, MINORBUY), laws requiring a permit and/or license to purchase a gun (BYPERMIT, BYAPLIC), laws requiring a license to possess a gun in a home (LICENSE), laws controlling the concealed carrying of loaded handguns in public places (CARYHIDN), and bans on the sale of "Saturday Night Specials" (SNSBAN). We obtained results virtually identical to those obtained using the full set of 19 gun law variables except that the coefficient for CRIMBUY was no longer significant and negative in the assault equation. Gun levels still showed no positive effect on violence rates (estimates are available from the senior author).

Using Lagged Violent Crime as a Proxy Variable for Omitted Historical Variables

As noted above, it is impossible to control for literally all potential confounding factors, even though CX data are widely available for a rich variety of variables for census years. Despite our best attempts to control for the most likely confounding factors, we cannot rule out the possibility that the gun law variables are correlated with one or more omitted variables that affect violent crime rates. One way to address potential bias due to omitted historical variables in the context of CX data is to control for the value of the dependent variable from a previous time period. As Wooldridge (2000) notes

> using a lagged dependent variable in a CX equation increases the data requirements, but it also provides a simple way to account for historical factors that cause current differences in the dependent variable that are difficult to account for in other ways." (p. 289)

If, for example, cities with historically high violent crime rates were also more likely to have stricter gun laws, we would fail to get an unbiased estimator of the causal effect of gun laws on violent crime rates. Therefore, we reestimated the violence equations in Table 2 but also included the natural log of the violent crime rate for 1980 as an additional independent variable in an attempt to control for city unobservables that affect violent crime and may be correlated with the gun law variables. By including the violent crime rate for 1980 in the violence equations, we are examining whether cities with similar previous violent crime rates in 1980 and 1990 values for the socio-demographic control variables had lower violent crime rates in 1990 due to the presence of any of the 19 gun laws studied here. To conserve space, the results of these analyses are not shown but are available upon request from the senior author. Not surprisingly, the homicide, robbery, and assault rates for 1990 were strongly related to the past violent crime rate. With respect to the gun law variables, the results were almost identical to those reported in Table 2. The only exception pertained to the robbery equation—the coefficient for the gun law banning the sale of handguns (HGBYBAN) was positive but no longer statistically significant. Thus, the evidence does not support the suspicion that estimates for the gun law variables were biased upwards due to the omission of historical factors responsible for differences in violent crime rates across cities in 1990.

OLS Estimates With Gun Ownership Treated as Exogenous

Although prior research indicates that OLS estimates of the effect of gun ownership levels on homicide rates are likely to be biased, the possibility that these biases are small or negligible cannot be ruled out. If this were indeed the case, then gun ownership could be treated as an exogenous regressor, and estimation by OLS would be preferred to IV because it is the more efficient (lower variance) estimator. The standard approach to this question is to conduct a test of the endogeneity of gun ownership. Such a test relies implicitly on a comparison of an estimation in which gun ownership is treated as exogenous and one in which it is treated as endogenous. For the test to have any meaning, it is therefore essential that the OLS estimation be contrasted with a well-specified IV estimation, that is, one that uses instruments for gun ownership that are both relevant and valid. Testing for the endogeneity of gun ownership by comparing OLS to a misspecified IV estimation cannot provide evidence that OLS is acceptable. Having shown that our instruments satisfy the requirements of both validity and reliability, we turn to the issue of whether gun ownership (again proxied by PSG) is endogenous.

We tested for the endogeneity of gun ownership using the *C*-test, which detects the impact of adding a restriction, in this case assuming that violence rates have zero immediate effect on gun ownership (thus treating gun levels as exogenous; Baum et al., 2003). The impact should be small if both equations (with and without the restriction) are valid. On the other hand, if there is a large change in the estimates of parameters, it suggests that the equation with the extra restriction (assuming gun ownership to be exogenous) is wrong.

The results of the *C*-test for the endogeneity of PSG are presented in the bottom half of Table 3. The *C*-statistic suggests gun ownership may be endogenous in the total homicide and robbery equations, although it is only significant at the .10 level. Additionally, the results indicate that although gun ownership may also be endogenous to gun homicide and assault, the *C*-statistic in these equations is not significant at conventional significance levels. In light of the mixed *C*-test results, we reestimated the violence rate equations presented in Table 3 but treated gun ownership as exogenous to rates of violence and estimated models with OLS. These results are shown in Table 3.

The OLS estimates indicate that gun ownership has a positive association with total homicide that is barely significant at the 5% (one-tailed) level ($t = 1.77$) but is still not significantly related to robbery or assault rates. Thus, we have consistent findings regarding robbery and assault—gun ownership has no significant positive effect. But we have mildly contradictory findings regarding the impact of gun ownership on the homicide rate. The IV results, which are appropriate if homicide rates affect gun acquisition, indicate that gun ownership has no significant net effect on homicide rates, while the OLS results, which are appropriate only if homicide rates have no effect on gun acquisition, indicate a marginally significant positive effect of gun ownership on homicide rates.

We doubt that gun ownership can be treated as exogenous, given the prior evidence of individual-level survey studies indicating that violent crime rates affect gun ownership. These studies are critical for breaking the deadlock concerning causal order because they are not subject to the same uncertainties concerning exogeneity and model identification that may afflict the numerous aggregate-level studies that have found an effect of violent crime rates on gun ownership. The survey studies relate the gun ownership of individual persons or households to the violent crime rates of the areas in which the individuals reside (Kleck & Kovandzic, 2009). Because it is highly unlikely that the gun ownership of any one person or household could materially affect the violent crime rates of an entire city or county, it is reasonable to conclude that the violent crime/gun ownership relationship in such studies is unidirectional, and that estimates of the effect of crime rates on gun ownership are not distorted by two-way causation.

These studies find that violent crime rates of surrounding areas increase, directly or indirectly, the likelihood that a person or household owns a gun (Kleck & Kovandzic, 2009; Lizotte, Bordua, & White, 1981, p. 501; Smith & Uchida, 1988). Therefore, we think it is advisable, in macro-level studies, to treat gun ownership as endogenous, based on strong and consistent evidence that violent crime rates affect levels of gun ownership, especially handgun ownership. Thus, on the basis of prior information, the IV estimates of models assuming gun levels to be endogenous should be regarded as more reliable than OLS estimates that require the strong assumption that violent crime rates have no effect on gun levels.

When gun levels were treated as exogenous (Table 3), nine effects of gun laws on violence rates substantially changed (i.e., coefficients changed from significant to nonsignificant, or the reverse). Five changes were in a direction favorable to the hypothesis that gun control either reduces violent crime or has no effect: the previously significant positive (counterproductive) effects of bans on gun possession among alcoholics (ALCPOS) became nonsignificant, while four previously nonsignificant negative effects became significant (BYAPLIC → HOMICIDE, BYAPLIC → ROBBERY, DRUGPOS → ASSAULT, ALCBUY → ASSAULT). The remaining four changes were unfavorable to the gun control efficacy hypothesis: the previously significant negative effect of gun owner licensing became nonsignificant, while three previously nonsignificant positive (counterproductive) effects became significant.

For the rest of the potential effects, resolution of the exogeneity issue turned out not to materially affect the results of primary interest, the impact of gun laws on violence rates—the OLS results were qualitatively identical to those obtained using IV methods. On net, results were no more supportive of gun control than those obtained when gun ownership was treated as endogenous. Considering the OLS estimates as a whole, the 10 significant negative associations were counterbalanced by 14 significant positive coefficients. This pattern suggests, as did the IV results, that if one interprets these associations as causal effects, gun control laws are more likely to increase violent crime than to decrease it. We also estimated models with gun ownership omitted altogether, and results for gun law variables were essentially identical to those produced when gun ownership was included, but as an exogenous variable.

Given the inevitable uncertainties of even the most careful nonexperimental research, it cannot be stated with certainty that gun ownership levels have no effect on violent crime rates. If they do not, gun ownership is not a confounder that needs to be controlled to isolate the effects of gun laws, but one of the most important underlying premises of gun control is undercut. On the other hand, if the level of gun ownership does affect violence rates, then it is a confounder that must be controlled if estimates of gun control effects are to be given much credence. Our results are therefore stronger than those of past research because we could rule out the possibility of a spurious negative association between gun laws and violent crime rates attributable to the negative association of gun control strictness with gun ownership levels.

Discussion and Conclusion

For the most part, the evidence fails to support the hypothesis that gun control laws reduce violent crime. The absence of any apparent impact may be partly because most laws do not disarm significant numbers of violence-prone persons in the first place. It is also possible that gun laws have both violence-reducing and violence-increasing effects, the latter due to disarming of prospective victims. Opposite-sign effects may counterbalance each other, yielding no net effect.

There were nevertheless some findings that point to possible gun law effects on violent crime rates, both desirable and undesirable (summarized in Table 4). Of 57 possible effects of a type of gun law on a type of violent crime, 20 were significantly different from zero—8 negative, 12 positive. Some of these findings may be the product of chance, operating in combination with the very large number of tests for effects that were performed, though this would probably produce no more than around three coefficients significant at the 5% level ($.05 \times 57 = 3$). Taken at face value as causal effects, these findings indicate that gun control laws are at least as likely to increase violent crime as to decrease it, though on net gun control laws as a whole do not affect violent crime rates.

Along with the two strong findings (the effect of bans on purchase by alcoholics and the impact of requiring a gun license on homicide rates) and five moderate-to-weak findings supportive of gun control efficacy, Table 4 also shows 12 possible *positive* effects of gun laws on violent crime rates. Unlike our tests of violence-reducing effects of gun laws on homicide, no sharp tests of violence-increasing effects are possible because reduction of gun levels among prospective victims could increase either crime committed with guns or crime committed without guns. Thus, findings pointing to violence-elevating effects are necessarily weaker than findings pointing to violence-reducing effects on homicide. If interpreted as causal effects, the positive associations would indicate violence-increasing effects of gun control measures, perhaps due to potential victims (many within high-risk prohibited groups) being disarmed, making crime less risky for offenders. Among the more intriguing apparent counterproductive effects was that of laws restricting the concealed carrying of

Table 4. Summary of Effects of Gun Control Laws on Crime Rates.

Type of Gun Control Law	Types of Violent Crime Affected
Violence-reducing effects	
Strongly supported	
Ban on gun purchase by alcoholics	Homicide
License required to possess gun in home	Homicide
Moderately supported	
Ban on gun purchase by alcoholics	Robbery
License required to possess gun in home	Robbery
Weakly supported	
Ban on gun purchase by criminals	Robbery, assault
Prohibit possession, mentally ill	Assault
Violence-increasing effects	
(All weakly supported)	
Ban on gun purchase by minors	Robbery
Ban on gun purchase by drug addicts	Homicide, robbery
Prohibit possession, alcoholics	Homicide, assault
Ban on gun purchase by mentally ill	Assault
Permit required to purchase gun	Assault
Transfer/sale of guns must be registered with a governmental agency	Robbery
Concealed carrying of loaded handgun prohibited or permit hard to get	Homicide
Handgun sales ban	Robbery
State or city license required for gun dealers	Homicide, robbery

guns on homicide. To the extent that these laws reduce gun carrying by prospective victims more than by offenders, the laws could increase violent crime by reducing any deterrent effects generated by victim gun carrying. The strongest prior research on this question, however, indicates that replacing restrictive carry laws with more lenient "shall issue" licensing has no net impact on violent crime rates (Kovandzic et al., 2005). Further, laws reducing the carrying of guns outside the home should have their strongest influence on crimes typically committed in nonresidential locations, such as robberies, but our results indicated no effect on the robbery rate.

It is possible that still other laws, implemented since 1989 or not yet implemented, might have effects not produced by the older laws. Perhaps gun control strictness has not yet reached some unknown threshold level, below which no measurable crime reductions can be achieved. The laws enacted since 1989, however, are generally weaker than those passed earlier. The single possible exception is the federal Brady Act, but preliminary evaluation indicates that this law, at least in the first few years after its passage, was ineffective (Ludwig & Cook, 2000). In any case, this sort of speculation is nonfalsifiable, as one could continue to entertain it no matter how strict controls became in the future, and no matter how negative the results of research continued to be. It is also possible that some laws have effects, but they are too small to be statistically detectable using our models and data, even with a sample size exceeding a thousand.

The main policy implication of this research is that the past performance of existing gun laws does not justify much optimism that new gun laws will reduce violent crime. Support for even the least promising strategy can be sustained by ultimately nonfalsifiable speculations about what might be achieved by the next, heretofore untried, variant of the strategy, but this is not a very practical way to set priorities for the allocation of limited resources for reducing social problems. This does not imply that we should not explore new variants of gun control, but it does imply that such efforts have less a priori potential for measurable impact on crime rates than alternatives such as well-evaluated programs to reduce poverty or rehabilitate criminals (Walker, 2011).

On a more positive note, the minority of gun control measures that show evidence of effectiveness share an important element in common—background checks on persons attempting to acquire firearms. Both licenses authorizing gun possession and permits for purchasing guns are implemented using background checks to screen out persons in prohibited categories, such as criminals, alcoholics, and mentally ill persons. Likewise, bans on gun purchases by criminals or alcoholics are little more than hollow recommendations if not backed up by a system for identifying whether persons fall into these prohibited categories. Currently, persons attempting to acquire guns from licensed gun dealers are required to pass a background check under federal law, but those trying to get guns from private (nondealer) sources are not required to do so, under either federal law or the laws of most states. Consequently, some reduction in violent crime could be produced by a federal law requiring background checks on all persons seeking to obtain a firearm, regardless of the source.

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Notes

1. Although Lott and Mustard (1997) represented their key data as county rates of crimes known to the police, their data in fact reflected only crimes within subsets of local police jurisdictions within each county that reported crime to state UCR agencies—subsets of jurisdictions that frequently changed from year to year. Their "crime rates" for a given county changed in this data set merely because different sets of areas contributed crime statistics in 1 year compared to the previous year, rather than because rates of crime (or even crimes known to police) changed (Maltz & Targonski, 2002; see also Martin and Legault, 2005, regarding similar problems in state-level crime data analyzed by Lott and Mustard). It is exactly as if a different set of counties were included in each wave of the panel. Lott and Mustard did nothing to correct this critical problem, nor did critics who reanalyzed their unmodified data set.
2. A few low population cities reported zero homicides for the 1989–1991 study period. Because the logarithm of zero is undefined, 1 was added to the average annual number of total, gun, and nongun homicides before we computed the rates and then took the natural logs of those rates. The procedure was applied to all cities, not just those with zero homicides, to maintain relative homicide levels.
3. We did not study the effects of gun ownership and gun laws on rape rates. Because less than 3% of rapes involve offenders armed with guns (U.S. Bureau of Justice Statistics, 2006), it is unlikely that gun ownership or gun laws could exert a detectable effect on the rape rate.
4. It would have been desirable to separately assess rates of all types of violent crime with and without guns, to provide sharper tests of the hypotheses that gun levels and gun laws affect violence rates. Unfortunately, close examination of UCR data on gun versus nongun varieties of robbery and aggravated assault revealed that the data often covered less than 12 months of the year, were often coded incorrectly (e.g., *all* robberies were coded as gun robberies), or there were implausibly large or small numbers of crimes reported as involving guns. Consequently, we could reliably distinguish gun and nongun crimes only for homicide, by using SHR data.
5. For the gun carrying law variable (CARYHIDN), 1 indicated that gun carrying was either completely unlawful or required a license that was rarely issued, and 0 indicated that either the city was located in a

nondiscretionary "shall-issue" state where authorities were required to issue carry permits to applicants meeting certain objective criteria, or no license was required at all to carry guns.
6. That is, the number of excluded instruments exceeds the number of endogenous regressors.
7. The *J*-statistic for the instrumental variable estimator is numerically equivalent to Sargan's (1958) NR2 overidentification statistic.
8. Where the test will lack power is if all the instruments fail the requirement of exogeneity and, in addition, they all imply the same bias in the estimate of gun levels.

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