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The Impact of Gun Control and Gun Ownership Levels on Violence Rates

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What effects do gun control restrictions and gun prevalence have on rates of violence and crime? Data were gathered for all 170 U.S. cities with a 1980 population of at least 100,000. The cities were coded for the presence of 19 major categories of firearms restriction, including both state- and city-level restrictions. Multiple indirect indicators of gun prevalence levels were measured and models of city violence rates were estimated using two-stage least-squares methods. The models covered all major categories of intentional violence and crime which frequently involve guns: homicide, suicide, fatal gun accidents, robbery, and aggravated assaults, as well as rape. Findings indicate that (1) gun prevalence levels generally have no net positive effect on total violence rates, (2) homicide, gun assault, and rape rates increase gun prevalence, (3) gun control restrictions have no net effect on gun prevalence levels, and (4) most gun control restrictions generally have no net effect on violence rates. There were, however, some possible exceptions to this last conclusion—of 108 assessments of effects of different gun laws on different types of violence, 7 indicated good support, and another 11 partial support, for the hypothesis of gun control efficacy.

KEY WORDS: gun control; violence.

1. INTRODUCTION

Crime is widely viewed by the public as one of the most important problems facing our society, and violent crime is regarded as the most serious and fearful kind of crime. While violence is often regarded as an intractable problem difficult to reduce through deliberate governmental effort, many have argued that it, nonetheless, may be reduced through the regulation of weapons, especially firearms.

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The rationale for gun control, of course, includes the assumption that the availability of guns has a significant net positive effect on violence rates. This assumption has not yet been consistently supported by a credible body of evidence, partly because evidence from better studies has largely been negative or mixed regarding the assumption and partly because so much of the evidence is too weak to be credible one way or the other (see overviews by Wright *et al.*, 1983, pp. 129–137; Kleck and McElrath, 1991). There are a number of possible effects which gun availability could have on violence rates. If a gun is available, it could encourage attacks, especially by weaker attackers on stronger victims, and could facilitate attacks from a distance or attacks by persons too squeamish to attack with messier weapons such as knives or too timid to attack at close quarters. Similarly, guns may enable some people to attempt robberies they could not complete unarmed (Newton and Zimring, 1969; Cook, 1976). The sight of a gun might "trigger" attacks by angered persons, due to the learned association between guns and violence (Berkowitz and LePage, 1967). On the other hand, research also indicates that the presence of guns usually inhibits the expression of aggression, reducing the likelihood of attack (Kleck and McElrath, 1991; Kleck and DeLone 1993). There is support for the claim that once an injury is inflicted, it is more likely to result in death if a gun was used, due to the weapon's greater lethality (Newton and Zimring, 1969; Block, 1977; Kleck and McElrath, 1991), although part of the higher fatality rates of gun attacks is probably due to greater seriousness of intent on the part of those using guns, rather than just the weapon itself (Wright *et al.*, 1983). Regarding suicide, some authors argue that guns provide a uniquely quick, easy, and sure means of self-destruction which reduces the chances of successful outside intervention (Newton and Zimring, 1969). On the other hand, many highly lethal and otherwise satisfactory means for committing suicide are even more widely available than guns, and can easily be substituted where guns are not available.

Prior studies of the aggregate relationship between gun availability and violence rates have used a variety of measures, none entirely satisfactory (Cook, 1982, pp. 264–272). These studies have failed to generate consistent evidence of a net positive effect of gun availability on violent crime rates (Kleck, 1984a, 1991, Chap. 5). The present study measures gun levels through the use of multiple indirect indicators, for two purposes: (1) to assess the impact of gun availability on violence rates and (2) to assess the effects of gun laws on violence rates, including both direct effects and indirect effects operating through the impact of gun control on gun availability. The study addresses every major form of gun control and every major form of violence involving firearms, including not only the violent crimes of homicide, robbery, assault and rape, but also suicides and fatal gun accidents.

2. METHODS OF PRIOR RESEARCH

Two general strategies have been used to assess the impact of gun control laws on violence rates: interrupted time series designs and cross-sectional designs. In the typical time series design, monthly violence rates for a single jurisdiction are analyzed with ARIMA or regression time series methods to see if there is a significant downward shift in crime around the time a new gun law goes into effect. Cross-sectional designs compare legal jurisdictions, usually states, with each other to see if those having a given type of gun law have lower levels of violence than those lacking the law.

Studies of gun control's impact on violence have been characterized by a variety of methodological flaws. The first is the failure to control adequately for other determinants of violence rates besides gun control laws, before attributing crime reduction effects to gun regulation. This is at least as much of a problem for time series studies as for cross-sectional ones. Careful modeling of preintervention trends in violence is required in time series studies, rather than simple before-and-after comparisons, because the time when an intervention is most likely to be implemented is at, or shortly after, the time when the target problem peaks, i.e., when it is most likely to stimulate attempts to combat it. Thus, one would expect to find decreases in the problem after an intervention even if the intervention were ineffective, due to this simple timing issue—the problem was peaking and thus was going to decline at the time of intervention anyway, even if nothing was done about it. Unfortunately, if this reasoning applies to the intervention being evaluated, it also applies to other "interventions" as well. Other efforts, public or private, collective or individual, to reduce the target problem would also be most likely to start (or peak) at about the same time. Time series modelers attempting to isolate the impact of gun laws necessarily assume that the evaluated intervention was the only new element in the causal structure generating trends in violence rates. This is, at best, a convenient simplification; at worst, an implausible one.

Cross-sectional designs can take advantage of considerable data in census years for cities, metropolitan areas, or states on extraneous determinants of crime rates, while time series data on most such variables, except at the national level, are nonexistent. Consequently, time series designs usually do not explicitly control for any other important determinants of crime which might show changes coincident with changes in gun laws. Thus, they do not allow the analyst to rule out explicitly any alternative explanations of violence decreases. Instead they, at best, make do with comparisons to "control" jurisdictions which, it is assumed, would show crime trends similar to those in the intervention jurisdiction, were it not for the impact of the gun law changes. This was the strategy followed by Pierce and Bowers (1981). Other time series studies use trends in

*non*gun violence rates within the impact jurisdiction as internal controls, relying on the implicit, and implausible, assumption that gun and nongun rates would follow similar trends were it not for changes in gun regulation (e.g., Loftin and McDowall, 1981, 1984). Evidence from the present study (Table III) indicates that gun violence and nongun violence rates are driven by different sets of exogenous variables (apart from gun laws and gun prevalence), suggesting that they are likely to show divergent trends even in the absence of new gun laws.

Cross-sectional studies of a large number of jurisdictions offer clear advantages over longitudinal designs if one wants to identify which specific features of gun regulation are likely to generally produce violence reductions. The former tests the average effect of many specific instances of a form of regulation, while the latter tests only the effects of a single new gun law in a single jurisdiction, allowing little generalizability. With the former design it is possible to separate the effects of different types of gun controls which are sometimes lumped together in a single new law, while this is impossible in the latter.

Further, it is impossible to state for certain, a priori, *when* the effect of a new law should become evident, rendering the gun law efficacy hypothesis difficult to falsify with a time series design. For example, some analysts have assumed that any impact should begin at the law's "effective date," while others assert that effects can begin earlier, due to an "announcement effect" (Pierce and Bowers, 1981). Loftin and his colleagues (1991) even concluded that local handgun bans reduced homicide in the District of Columbia, even though the declines in both gun homicides and total homicides began *2 years* before the law went into effect! One could just as easily argue that effects would only become evident after a lag of indeterminate length. In contrast, with a cross-sectional design the corresponding question is *where* the law would have its effects, and there is little doubt that the effects should be most pronounced in the jurisdiction which implemented the regulation.

The principal weakness of cross-sectional studies is one shared by time series studies—the difficulty of meeting the *ceteris paribus* condition by correctly specifying a model of how crime rates are generated. It should, however, be noted that the cross-sectional design does *not* require, as Wright *et al.* (1983, p. 285) assert, that "the investigator have a fairly complete understanding of how the particular crime rates are generated." This is an impossible standard to meet and fortunately, an unnecessary one. Instead, unbiased estimates of the impact of a gun control measure can be obtained if one includes in the model only those extraneous variables which affect crime rates *and* which also have nontrivial correlations with the gun control measures. It turns out that none of the known

causes of variation in violence rates are strongly correlated with gun laws, making this a less crucial empirical issue than it seemed.

With only two exceptions (Geisel *et al.*, 1969; Cook, 1979), prior cross-sectional studies have exclusively used states as their unit of analysis. This exacerbates the problem of aggregation bias. States are larger units than cities and, also, more heterogeneous with regard to levels of violence and variables affecting violence rates. Consequently, the best level of aggregation to use would be the lowest and most homogeneous one at which gun law is made—the city level.

Another problem with state-level analyses is that they cannot incorporate measures of local gun controls. Only one prior study has measured gun regulation at both the state level and the city level (Geisel *et al.*, 1969), yet the most restrictive gun laws in the nation are at the local level. Many, even most, of the residents of a given state might be subject to very strong gun laws, at the city level, yet be subject to little or no state regulation. Consequently, studies failing to measure local ordinances seriously mismeasure the degree of gun control to which much of the population is subject.

For some gun laws, one presumed reason for any effects on violence they may have is that they reduce levels of gun prevalence or availability, which in turn affects violence rates. Indeed, regardless of the way the laws were designed to work, almost any restriction on guns could in practice discourage gun ownership, by reinforcing public perceptions of guns as dangerous objects. Conversely, most gun laws could hypothetically reduce violence in ways other than by reducing gun ownership, e.g., by making carrying or criminal use more risky or reducing the immediate availability of guns in violence-prone situations. Only three of the studies published to date explicitly measured gun prevalence or availability (see column 5 in Table I). Thus it was usually impossible to tell whether observed effects were produced through reductions in gun ownership or through some other causal mechanism. Further, if high gun prevalence makes it harder to pass gun laws, and also contributes to higher violence rates, failing to control for gun prevalence could result in a spurious negative association between gun laws and violence rates.

None of the three gun law studies which measured gun levels treated the gun–violence relationship as a simultaneous reciprocal one. This is problematic because there is both individual-level and aggregate-level evidence that violence rates can motivate gun acquisition and increase aggregate gun ownership levels (Lizotte and Bordua, 1980; Lizotte *et al.*, 1981; Kleck, 1984a; McDowall, 1986; Smith and Uchida, 1988). If the relationship were a simultaneous reciprocal one, failing to model it properly would result in biased and inconsistent estimates of the gun coefficient, and

Table I. Previous Studies of the Impact of Gun Control on Violent Crime Rates[a]

Study	Weakness							Gun control effective?
	1	2	3	4	5	6	7	
Wisconsin (1960)	×	×	×	×	×			No
Krug (1967)	×	×	×	×	×	×		No
Geisel *et al.* (1969)			(×)	×	×	×		No
Olin Mathieson (1969?)	×	×	×	×	?	×		No
Seitz (1972)	×	×	×	(×)	×	×		Yes
Murray (1975)	×	×	×	(×)	×			No
Zimring (1975)	×	–	–		–	–	×	Mixed
Beha (1977)	×		×	(×)	–	–	×	Mixed[b]
Deutsch and Alt (1977)	×		–	×	–	–	×	Mixed[b]
Cook (1979)			×		?			No
Hay and McCleary (1979)	×		–	×	–	–	×	No[b]
Nicholson and Garner (1980)	×		–	×	–	–	×	Mixed
Sommers (1980)	×	×	×	×	×		×	Mixed
Jones (1981)	×	–	–	×	–	–	×	Mixed
Lester and Murrell (1981)	×		×	×	×	×		No
Pierce and Bowers (1981)	×		–	×	–	–	×	Mixed[b]
Lester and Murrell (1982)	×	×	×	×	×	×		Mixed
Magaddino and Medoff (1982)	×	×	×	×	×			No
DeZee (1983)		×	×	×	×			No
Loftin *et al.* (1983)	×			×			×	No
Loftin and McDowell (1984)	×			×			×	No
Magaddino and Medoff I (1984)		×	×	×	×			No
Magaddino and Medoff II (1984)		–	–		–	–	×	No
McPheters *et al.* (1984)	×	–	–	×	–	–	×	Yes
Lester and Murrell (1986)	×	×	×	×	×	×		No
Lester (1987)	×	×	×	×	×	×		No
Lester (1988)	(×)	×	×		×	×		Yes
Jung and Jason (1988)	×		–	×	–		×	No
Loftin *et al.* (1991)	×			×			×	Yes

[a]Summary: 4 yes, 8 mixed, 17 no. "Gun control effective?" means "Did gun laws appear to reduce significantly total (gun plus nongun) rates of violence or crime?" Weakness codes: ×, problem existed; blank, no problem; –, problem is irrelevant; (×), partial presence of problem or problem inadequately dealt with. Weaknesses: (1) included no, or very few, control variables; (2) state level of analysis used, rather than city; (3) no measure of local gun control laws; (4) no measure of gun ownership included; (5) only one source of information on gun control laws used; (6) lumped heterogeneous mixture of gun laws together, without separate measures of impact of different types of gun laws; (7) studied just one specific law; little generalizability.

[b]These four studies are not independent since they are all evaluations of the same law (the Massachusetts Bartley–Fox law) in the same time period, using the same general methods. They contributed three of the eight studies classified as "mixed." Their findings are classified this way because, taken as a whole, they indicate that the law had no effect on homicide, may have reduced robbery (two studies indicated this, one did not), and reduced gun assaults by a moderate amount, while increasing nongun assaults by a larger amount.

the positive effects of violence on gun levels would be confused with the possible positive effects of gun levels on violence rates.

Finally, close examination of the various surveys and compilations of gun laws reveals significant differences between sources, indicating in many instances that at least one source was in error. Consequently, studies using a single source of information are especially vulnerable to error in measurement of the key variables. This was true of all prior studies of multiple laws.

3. RESULTS OF PRIOR RESEARCH

The Table I summary of prior research on gun law effects indicates that most of the 29 studies found no impact of gun laws on total violence rates. [Throughout this paper, the term "total violence rate" refers to rates of gun violence plus nongun violence in a given violence category. For example, the term could refer to total homicide (gun homicide plus nongun homicide) or to total robbery (gun robbery plus nongun robbery), and so on. It does not refer to homicide plus robbery plus assault and so on.] Of the 12 studies yielding favorable or mixed results, 3 were time series evaluations of the same law, the Bartley–Fox carrying law. Of these three, the Pierce and Bowers (1981) study found a drop in violence which *preceded* the law's effective date, casting doubt on the authors' favorable assessment of the law. Further, a fourth study of this same law concluded that evidence regarding the law's impact was inconsistent and that the optimistic conclusions of previous researchers were premature (Hay and McCleary, 1979). The middle columns in Table I indicate that most of the rest of the studies offering at least mixed support for gun control efficacy are seriously flawed. Taking prior research as a whole, it would be fair to say at this point that a consistent, credible case for gun control efficacy in reducing violence has not yet been made.[3] [For reviews of research on the impact of gun

[3]Assessments of the studies' implications regarding gun control efficacy are based on their empirical findings, not necessarily on their authors' conclusions. As an example of conclusions diverging from data, Geisel *et al.* (1969) concluded that increased gun control severity would save lives, based on analyses using an index which lumped together all forms of gun control. Construction of this index involved a weighting scheme which, contrary to the author's claims, biased results in favor of finding a stronger correlation with violence rates (see p. 659). Even so, the results of analyses using the index did not generally support the author's conclusions. Of the seven violence rates studied, only two showed a significant negative association with the index: gun suicides (but not total suicides, indicating nothing more than a substitution effect) and accidental death by firearm (p. 663). Further, buried in the last page of their Appendix was a one-paragraph summary of the results of their more appropriate analysis (which even the authors described as "more refined"), using separate dummy variables for each type of gun control: "We could obtain no significant or even meaningful results" (p. 676).

prevalence on crime, suicide, and gun accident rates, see Kleck (1991, pp. 187–188, 214–214, 248–250, 265, 303–304).]

4. METHODS OF THE PRESENT STUDY

The present study is a city-level cross-sectional study. Data were gathered on all 170 U.S. cities which had a population of 100,000 or larger in 1980, i.e., all large cities. Cities were chosen as the unit of analysis because they are the smallest, most homogeneous unit or area to which gun laws apply, and analyses which use larger units necessarily must ignore laws passed by smaller constituent areas. A majority of the reported violent crimes in the United States occurred in these 170 cities [U.S. Federal Bureau of Investigation (FBI), 1981, p. 173]. Smaller cities could not be included because person-level vital statistics mortality data do not identify locations of deaths for cities with populations smaller than 100,000 [U.S. National Center for Health Statistics, (NCHS), 1983, p. 8]. These data were needed to obtain city counts of gun homicides and gun suicides, data which were essential both as components in dependent variables and as indirect indicators of gun prevalence.

The dependent variables are the rates per 100,000 resident population of homicide, suicide, aggravated assault, robbery, rape, and fatal gun accidents. For all but the last two of these, we had data allowing separate analyses of rates of violence with guns, without guns, and with gun and nongun events combined.

The violence rates were averaged over 3 years, 1979 to 1981, thus bracketing the Census year of 1980 for which data on most of the control variables were available. Some of the smaller cities had fewer than a half-dozen homicides or suicides per year; thus, misclassification of just one or two homicides or suicides as other kinds of deaths could substantially alter a single year's official count. Therefore, 3 years were covered, to minimize the potential measurement error produced by misclassification and to minimize the instability due to year-to-year fluctuations.

The dependent variables were expressed as natural logs. The transformation produced more normal distributions on the violence rate variables. (Without exception, skewness and kurtosis statistics moved closer to zero after the transformation.) It also helped to stabilize the variance of the residuals, reducing heteroscedasticity.

Models of violence rates were estimated using two-stage least-squares procedures because a simultaneous reciprocal relationship was specified between violence rates and gun prevalence levels, based on the assumption that higher violent crime rates could motivate gun acquisition, in addition to gun prevalence increasing violence rates. No effect of suicide and fatal

gun accident rates on gun acquisition was expected, so models of these violence rates were specified as recursive and estimated using ordinary least-squares methods. Figure 1 illustrates the general form of the models estimated. This is the general causal structure assumed for all models estimated, except that we assumed there was no effect of suicide and gun accidents on gun prevalence. There was a total of 14 models (one for each type of violence rate listed in Table II), and each model consisted of two equations, one for the violence rate and one for the gun prevalence level.

The initial choice of possible control variables to include in the models was based on a review of previous city-level and metro area-level studies. An effort was made to include all predictors which had frequently and consistently been found to significantly predict the violence rates examined here. Most of the violence predictors besides the gun law dummies and gun prevalence indicators were measures of the relative sizes of population groups which have especially high or low violence rates, or were measures of social integration, isolation, or transience, or measured the prevalence of statuses which can give rise to violence, such as divorce, alcoholism, and unemployment. Theoretical rationales for including these variables, and relevant empirical evidence, can be found in numerous sources (e.g., Byrne, 1986; Sampson, 1986; Land *et al.*, 1990, and studies reviewed therein). Exogenous variables which remained in the final models were those whose coefficients in the violence rate equations were significant at the 0.10 level in preliminary screening using OLS.

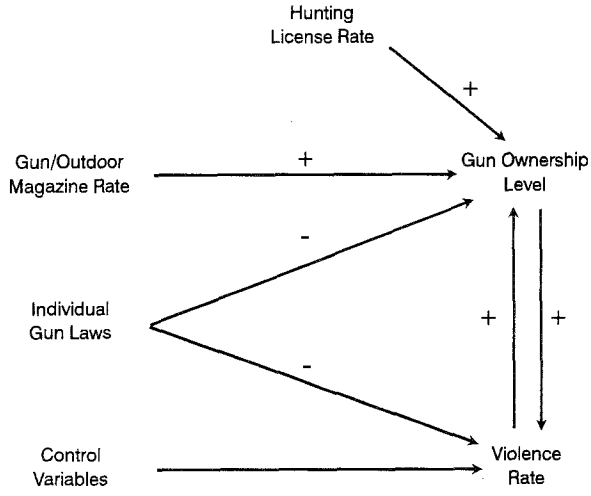

Fig. 1. General causal diagram of violence rate models.

Table II. Descriptive Statistics for Variables Used in Analysis ($N = 170$ Cities)[a]

	Mean	SD	Source[b]
Violence rates (1979–1981 average, rates per 100,000 resident population, in natural logs)			
LNMR, Homicides (total)	2.47	0.78	a
LNASLT, Aggravated assaults (total)	5.90	0.58	b
LNROB, Robberies (total)	5.79	0.75	b
LNRAPE, Forcible rapes (total)	4.04	0.54	b
LNSUICID, Suicides (total)	2.63	0.35	a
LNFGA, Fatal gun accidents (total)	0.50	1.35	a
LNGUNMR, Homicides with gun	1.98	0.88	a
LNNGMR, Homicides without gun	1.42	0.72	a
LNGNASLT, Assaults with gun	4.55	0.75	b, c
LNNGASLT, Assaults without gun	5.55	0.60	b, c
LNGNROB, Robberies with gun	4.87	0.76	b, c
LNNGROBR, Robberies without gun	5.22	0.84	b, c
LNGNSUIC, Suicides with gun	1.94	0.56	a
LNNGSUIC, Suicides without gun	1.81	0.47	a
Gun prevalence indicators			
PGH7982, % gun, homicide, 1979–1982	61.48	11.89	d
PCTGNAST, % gun, aggr. assault, 1979–1980	28.31	11.39	c
PCTGNROB, % gun, robbery, 1979–1980	42.00	13.11	c
PGS7982, % gun, suicide, 1979–1982	53.37	14.91	a
GUNSTOL, ($ value, stolen guns/$ value, all stolen property) × 100	1.20	0.75	e
Instrumental variables			
RGUNMAG, Subscription rate top 4 gun/hunting magazines, county	6,564.74	8,656.41	f
HUNTERS, Hunting License holder rate per 100K pop., state	6,985.58	4,252,36	g
NRA, NRA members per 100K pop.	870.90	634.59	t
LIBERAL, % 1972 presidential vote for McGovern, county	38.46	9.90	u

[a]Unless otherwise noted, each variable refers to a city, as of 1980. In variable descriptions, "county" indicates variable refers to county in which city was located, and "state" indicates variable refers to state in which city is located. Methods of estimating missing values may be obtained from senior author.

[b](a) Tabulations from Mortality Detail Files (U.S. NCHS, 1983); (b) U.S. FBI (1980–1982); (c) ICPSR (1983); (d) ICPSR (1984a); (e) ICPSR (1984b); (f) Audit Bureau of Circulations (1979–1982); (g) U.S. Fish and Wildlife Service (1980); (h) Blose and Cook (1980); (i) U.S. Bureau of Alcohol, Tobacco and Firearms (1980); (j) Ronhovde and Sugars (1982); (k) Jones and Ray (1980); (l) Wright *et al.* (1983); (m) U.S. Bureau of the Census (1983a); (n) U.S. Bureau of the Census (1983b); (o) Quinn *et al.* (1982); (p) U.S. Bureau of the Census (1981); (q) Gastil (1971); (r) U.S. Bureau of Justice Statistics (1982); (s) U.S. Federal Bureau of Investigation (undated); (t) unpublished membership counts supplied to senior author by National Rifle Association; (u) Scammon (1972).

Table II. Continued

	Mean	SD	Source[b]
Gun control variables			
LICENSE, License to possess gun in home	0.11	0.32	h, i, j
BYPERMIT, Permit to purchase or acquire	0.34	0.47	h, i, j
WAITPER, Waiting period to buy, receive, etc	0.44	0.50	h, j
CRIMINAL, Prohibit possession (poss.)—criminals	0.82	0.38	i, j, k
MENTAL, Prohibit poss., mentally ill, incomp.	0.25	0.43	i, j, k
ADDICT, Prohibit poss., drug addicts, users	0.41	0.49	i, j, k
ALCOHOLIC, Prohibit poss., alcoholics, etc.	0.19	0.40	i, j, k
MINORS, Prohibit purchase by minors	0.98	0.15	i, j
REGISTER, Registration of guns	0.47	0.50	h, i
DEALER, State or city license, gun dealers	0.61	0.49	h, i, j
CARYHIDN, Concealed handgun carrying forbidden or permit hard to get	0.88	0.33	j, k
CARYOPEN, Open handgun carrying forbidden or permit hard to get	0.56	0.50	j
MANDPEN, Mandatory penalty, illegal carrying	0.12	0.33	j
ADDONDIS, Additional penalty for committing crimes with gun, discretionary	0.58	0.50	j
ADDONMND, Additional penalty for committing crimes with gun, mandatory	0.61	0.49	j
RTBRARMS, State constitutional provision—individual right to bear arms	0.43	0.50	j
HGBAN, De facto ban on handgun possession	0.01	0.11	i
SNSBAN, Saturday Night Special sales ban	0.04	0.20	i
HGBYBAN, Ban on handgun sales	0.01	0.11	i
Control variables			
PCTBLACK, % respop, black	19.27	16.69	m
PCTHISP, % respop, Spanish origin	8.82	12.23	m
PCTM1524, % respop, male, age 15–24	10.05	2.30	n
PCTOLD, % respop, age 65+	11.20	3.53	m
RUNM1624, Unemployment rate males, age 16–24	13.18	6.12	n
RPOV, % respop < poverty line 1979	13.97	5.16	m
MFI, Median family income, $s, 1979	19,435.52	3,592.01	m
INEQUALT, % hshlds w. income > $10K or > $50K	35.51	6.91	m
OWNEROCC, % housing units owner-occupied	54.14	11.19	m
COLLEGE, College enrollment/100K respop	7,619.66	4,267.42	n
PCTMOVE, % respop age 5+ not in same house as 5 yr before	51.01	8.44	m
TRNSIENT, % respop, born out of state	42.74	15.79	m
PCTFOREN, % respop, foreign born	7.68	8.25	n
POPCHANG, % pop change 1970 to 1980	7.32	20.37	m
CNTDIVRT, Divorces per 100K respop, county	639.20	245.25	m
FEMHEAD, % families headed by females	21.21	10.93	m
CHRCHMEM, Church membership per 100 respop, county	20.38	12.02	o

Table II. Continued

	Mean	SD	Source[b]
ALCHLSM, Alcoholic liver disease deaths per 100K respop	7.77	4.45	a
ADDICTRT, Deaths due to nonmedical accidental poisoning by opiates per 100K respop	0.22	0.52	a
PCTSMSA, City respop as a % of SMSA respop	34.58	22.73	n
VISITORS, Lodging receipts in dollars/100K respop, SMSA	111.00	269.38	p
INVPOP, Inverse population, 1/(respop in 100,000s)	0.56	0.29	m
HSACTRAT, Household activity ratio—fraction of households not of husband–wife, wife not working type	0.71	0.05	n
HOSPITAL, Hospital beds per 100K respop	1,013.90	661.20	m
LIVLONE, % households with 1 person	10.18	2.91	m
STORES, Retail establishments/100K respop	851.72	167.09	m
MAXTEMP, Avg. daily max temperature, July	87.16	6.64	m
CROWDING, Percent of occupied housing units with 1.01+ persons/room	4.89	3.23	m
DENSITY, Persons per square mile	4,334,26	3,375.96	m
STHNBORN, Percent respop born in South	12.93	6.33	n
SOUTH, South region dummy	0.32	0.47	m
WEST, West region dummy	0.28	0.45	
STHNNESS, Gastil "Southernness Index"	20.24	7.43	q
POLEXP, Police expenditures per capita	70.65	24.92	m
COPS, Sworn police officers/100K respop	207.57	82.40	b
STPRISRT, State prisoners/100K respop	157.90	164.58	r
WEAPARST, Weapons arrests, avg. for 1979–1981, per 100 sworn police officers	58.26	30.83	s
ACCIDENT, Accidental deaths, excl. gun accidents/ 100K respop	46.43	15.45	a

It is important to stress at this point that the exact combination of control variables included in each model was not critical with respect to the gun control results. Gun law coefficient estimates were not sensitive to the choice of control variables to include because correlations between the gun law variables and the control variables were almost all weak. Of 290 bivariate correlations between gun law variables and control variables, none exceeded 0.4, and only 7 even reached 0.3. Multicollinearity involving the gun law variables was generally minor. In the final violence rate equations, variance inflation factors (VIF) for each of the 19 gun law variables were under 10, and all but two were under 4. [Kennedy (1985, p. 153) suggests that a VIF over 10 incidates harmful collinearity.] Thus, regardless of which theoretical perspectives might be used to inform the specification of control variables, the key coefficient estimates were not

substantially affected by specification decisions concerning which control variables to include in the models.

A few of the control variables are sufficiently uncommon to require comment. Like nearly all aggregate analyses of violence, the present study uses ratio variables, with city population being the denominator in many variables, both exogenous and endogeneous. Some critics have argued that the presence of common components in ratio variables can lead to biased or artifactual associations. Firebaugh and Gibbs (1985, p. 715) recommended that if one seeks unbiased coefficient estimates in a regression model containing both endogeneous and exogenous variables with a common component (commonly population size) in the denominator, one should also include one divided by the common component as another predictor. Thus we have included, in all models, one divided by resident population (in 100,000's) as a predictor.

Computing aggregate crime variables as per capita rates is conventionally done to control for the size of the population at risk of either committing crimes or being victimized in crime. Standard city resident population figures, however, are not completely adequate for this purpose because they do not count nonresident persons at risk, including daily commuters and visitors such as tourists and business travelers. We roughly controlled for the omission of commuters by including as a separate predictor the city population as a fraction of the surrounding metropolitan area, on the assumption that cities located in much larger metro areas are likely to have more commuters, in which case resident population would be a more serious underestimate of the population at risk [see Gibbs and Erickson (1976) for a fuller rationale]. We controlled for the contribution of short-term visitors by including as a separate predictor a "visitors index": the per capita total receipts for hotels, motels, and other lodging places, for the metropolitan area in which a city is located, in 1977. This is an especially important control for cities with large numbers of tourists relative to resident population, such as Las Vegas, Orlando (Disney World), and Miami.

4.1. Measurement of Gun Laws

Table II lists all of the variables which are included in later tables, as well as control variables which were evaluated but found to be unrelated to violence rates, along with the sources of the data. The following four sources were used for gun law coding, in descending order of importance: U.S. Bureau of Alcohol, Tabacco, and Firearms (BATF) (1980), Jones and Ray (1980), Blose and Cook (1980), and Ronhovde and Sugars (1982). Multiple sources were used wherever possible because each source provided

some information the others did not, and each served as a reliability check on the others. When sources conflicted, state statute books were consulted.

Both state laws and city ordinances were coded. Nineteen major categories of existing gun laws which could affect violence rates were included in the analysis. The philosophy guiding coding of the gun law variables was to code them so that each variable would measure the presence or absence of a given form of regulation, regardless of what other elements might have accompanied it in a given law, and regardless of what governmental level imposed the restriction. Thus a gun law variable was coded 1 if the form of regulation applied in 1980 to a given city, either due to a city ordinance or because the city was located in a state with such a law, whether the law applied to all types of guns or, as was usually the case, only to handguns; the city was coded 0 otherwise. A single law therefore might result in a city being coded 1 on two or three different gun control variables.

The gun law variables were constructed in such a way that any city subject to a gun license law was also subject to purchase permit requirements, since existing license laws all include as a component a requirement that a license be presented in order to buy guns from licensed dealers, in addition to requiring a license for home possession of guns. On the other hand, a city could be subject to a purchase permit requirement without requiring a license for home possession of firearms.

The gun registration variable was coded 1 if gun sales were recorded in such a way that a governmental agency received a record of a specific gun being sold to a specific person or if all persons currently possessing a gun were required to record their ownership of each gun with an agency.

The codings for most gun law variables were simply 1 for the regulation being present at either the state or the local level and 0 if they were absent. However, for the gun carrying law variables (CARYHIDN, CARYOPEN), 1 indicated that gun carrying (concealed or open, respectively) was either completely unlawful or required a license which was hard to get and rarely issued, while 0 indicated that the city was located in a so-called "shall issue" state—carry permits are fairly easy to get because they must be granted to applicants unless they have certain specified disqualifying attributes (Blackman, 1985).

4.2. Measurement of Gun Prevalence

We measured gun prevalence using a principal-components factor based on multiple indirect indicators. For cities, Cook (1979) used a simple index consisting of the average of two indicators: the percentage of suicides committed with guns and the percentage of nonfelony homicides com-

mitted with guns. He showed this measure to be highly correlated with survey measures of urban household gun prevalence, aggregated over eight regions, indicating validity for purposes of cross-sectional analyses. Earlier researchers had used similar indirect measures (Brearley, 1932, p. 71; Seitz, 1972; Curtis, 1974, p. 110; Brill, 1977, p. 20).

We improved on these measures by using as many as five indicators of city gun prevalence levels: (1) percentage of suicides committed with guns, 1979–1982; (2) percentage of nonfelony homicides committed with guns, 1979–1982; (3) percentage of aggravated assaults known to the police committed with guns, 1979–1980; (4) percentage of robberies known to the police committed with guns, 1979–1980; and (5) percentage of the dollar value of all stolen property reported to the police which was due to firearms thefts, 1979–1981. We also evaluated three other indicators: the fatal gun accident rate, the rate of National Rifle Association members, and the rate of contributors to the Second Amendment Foundation, another gun owners' group. However, in a factor analysis these did not load with the other indicators. A simple explanation would be that the latter group of indicators reflects mainly gun prevalence among noncriminals, while the first five measures reflect mainly gun prevalence among criminals.

In each model, when the dependent variable could have an artifactual association with one of the gun prevalence indicators, that indicator was deleted. Thus, for example, the percentage of homicides involving guns was omitted from the homicide model, the gun percentage of assaults was omitted from the assault model, etc.

All these indicators but the suicide item relate on their face to criminal gun possession. Therefore, we interpret the gun index as an indirect measure of gun prevalence among criminals. For conceptual and theoretical purposes, and at the individual level of empirical analysis, it is important to maintain the distinction between criminal and noncriminal gun possession. However, at the city level it is doubtful whether the two can be distinguished, as we suspect they are highly correlated. One simple reason would be the high rate of illegal gun transfers (Wright and Rossi, 1986)—cities with high noncriminal gun ownership will also have high criminal gun ownership because criminals steal guns from noncriminals. Therefore, as a practical matter, our indicators probably necessarily serve as indicators of noncriminal gun prevalence, as well as gun prevalence among criminals.

4.3. Validation of the Gun Prevalence Measure

Following Cook (1979), we assessed the validity of our gun indicators by measuring their associations with survey-based measures of gun

prevalence. We combined the results of three national surveys, the General Social Surveys for 1980, 1982 and 1984, to compute reported gun prevalence figures for the nine major U.S. census regions, among persons living in places of 100,000 or larger population. Comparable measures were computed for each of our gun indicator variables by weighting each city measure by the city's population and calculating a weighted average for our cities in each of the nine regions.

All but one of the indirect indicators was strongly correlated across regions with the regional survey measures of gun prevalence, and the indicators were highly correlated among themselves. The only indicator about which there was some doubt is one of the two used by Cook (1979) —the percentage of homicides committed with guns. It was correlated only 0.38 with the survey-based percentage of households reporting a gun, over the nine regions, which was not significantly different from zero. The other indicators showed the following significant correlations with the percentage of households reporting a gun: 0.69 for percentage of aggravated assaults committed with a gun, 0.83 for percentage of robberies committed with a gun, 0.86 for percentage of suicides committed with guns, and 0.90 for the percentage of the value of reported stolen property attributable to guns. This last measure, not previously used in gun research, appeared to be the best single indicator of gun prevalence. These same results were confirmed using survey-based measures of respondent (as opposed to household) gun prevalence and both household and respondent prevalence of handguns. An important finding of this validity test was that all of the indicators were more strongly associated with survey measures of handgun prevalence than with gun prevalence in general. Thus our indicators may reflect handgun prevalence more strongly than longgun prevalence. This is probably advantageous, since handguns are the predominant gun type involved in crime (U.S. Bureau of Justice Statistics, 1987).

4.4. Reciprocal Effects

Levels of violence might influence how much gun control a city has, as well as the reverse. If violence levels and the presence of gun laws had a simultaneous reciprocal relationship, a nonrecursive model would be called for, using an appropriate estimation procedure. However, gun laws were not passed frequently enough for violence levels in 1979–1981 to influence the passage of any significant number of gun laws during the same period [see Jones and Ray (1980, Appendix III) regarding the pace of gun law changes]. Rather, the level of gun control strictness in 1979–1981 was almost entirely a cumulative product of legislative activity before 1979. Further, there is no evidence that actual or measured violence

rates have any impact on legislative decisions regarding gun controls. Nevertheless, the relationship was treated as a simultaneous one in supplementary estimations, and recursive models were specified.

We always treated the relationships between gun prevalence and violent crime rate as simultaneous reciprocal ones, expecting that while gun levels may affect crime levels, crime may also simultaneously stimulate gun acquisitions (Kleck, 1984a). We used the rate of subscriptions to gun-related magazines and the state hunting license rate as measures of recreational interest in firearms. They served as instruments which should have a direct effect on gun prevalence but not on violence or crime rates, allowing identification of the model. [For a good introduction to identification problems, see Maddala (1988, pp. 293–304).]

This study improves on previous work in the following ways: (1) we modeled the two-way relationship between gun levels and violence levels, (2) we measured gun prevalence, and used multiple, validated indicators of gun prevalence levels, instead of just one or two, (3) we used extensive controls for possible sources of spuriousness, (4) we used cities as the unit of analysis, a smaller, more homogeneous unit than states, (5) we took account of both city and state gun laws, (6) we used four different sources for measuring gun laws, (7) we assessed 19 different types of gun laws instead of just 1 or 2, (8) we assessed whether the effectiveness of gun laws depends on the level of enforcement of weapons laws, and (9) we used a large sample of 170 cases, rather than the 50 or fewer common in prior studies.

5. INFERENTIAL LOGIC

The conditions under which one could tentatively conclude that gun laws reduce violence are as follows: If gun laws are effective, they should have (1) a significant negative association with the *gun* violence rate (e.g., the rate of homicides committed with guns), (2) a significant negative association with the *total* violence rate [e.g., the total homicide (gun homicide plus nongun homicide) rate], and, preferably, (3) a weaker association with the *nongun* violence rate (e.g., the rate of homicides not committed with guns) than with the gun violence rate.

If 1 is true, but not 2, it would generally indicate that gun laws merely shift people from guns to nongun weapons, with no net reduction in deaths or crimes. If 2 is true, but 1 is not, it suggests that gun laws are merely associated with some omitted variables which have an effect on total violence rates but that gun laws themselves have no effect, since they should have their effects by, at minimum, reducing rates of violence committed with guns. Interpretation is ambiguous if 1 and 2 are true, but 3 is

not (i.e., gun laws are as strongly negatively associated with nongun rates as with gun rates). This would suggest that either (a) the gun control variable is simply a correlate of some omitted variable which affects the violence rate, since there is no strong a priori reason why gun controls should reduce the rate of violent acts without guns, or (b) the gun control does reduce acts of violence with guns but is also a correlate of some factor which reduces violent acts without guns as well. Interpretation is also ambiguous if 1 is true, 2 is not true, *and* the gun control was not significantly associated with the nongun violence rate. As noted, the first two circumstances would ordinarily suggest substitution of nongun means for guns, with no net effect on total violence. However, the fact that the gun law did not show any evidence of increasing the nongun violence rate would seem to contradict this interpretation, making a clear interpretation impossible.

Note that this logic is irrelevant to the analyses of rape and fatal gun accidents since there were no data available to separately measure gun and nongun rates of rape, and the inferential logic is irrelevant to gun accidents. For these two, interpretations had to be based entirely on findings concerning the total rape and fatal gun accident rates.

6. FINDINGS

Table III reports two-stage least-squares (2SLS) parameter estimates of the effects of gun laws, gun prevalence, and control variables on rates of total (gun plus nongun) violence, gun violence, and nongun violence. To clarify interpretation of Table III, consider A, pertaining to homicide rates. It reports estimates for three homicide models, with each pair of columns referring to a two-equation model of a given type of homicide. For example, the columns 2 and 3 present estimates of a two-equation model, column 2 pertaining to the total (gun plus nongun) homicide equation and column 3 pertaining to the gun prevalence equation.

Now consider estimates pertaining to a particular predictor variable. The row of numbers for BYPERMIT is estimates of coefficients reflecting the effects of laws requiring gun purchase permits on: (column 2) the total homicide rate, (column 3) gun prevalence in the total homicide model, (column 4) the rate of homicides committed with guns, (column 5) gun prevalence in the gun homicide model, (column 6) the rate of homicides not committed with guns, and (column 7) gun prevalence in the nongun homicide model, respectively. These estimates indicate that this type of gun control appears to have a significant negative effect on the total homicide rate, no significant negative effect on the gun homicide rate, and a significant negative effect on the nongun homicide rate. The interpreta-

Table III. Two-Stage Least-Squares Estimates (Standardized Coefficients)

(A) Homicide models

	Total homicide	Gun prevalence	Gun homicide	Gun prevalence	Nongun homicide	Gun prevalence
PCTHISP	−0.035	−0.017	−0.041	−0.005	−0.030	−0.028
RPOV	0.762***	−0.257	0.704***	−0.298	0.746***	−0.175
COLLEGE	−0.299***	0.034	−0.302***	0.068	−0.254***	−0.017
CNTDIVRT	0.243***		0.164***		0.325***	
PCTSMSA	−0.135**	0.030	−0.132**	0.033	−0.121*	0.022
INVPOP	−0.223***		−0.213***		−0.232***	
DENSITY	−0.037	−0.266***	−0.008	−0.268***	−0.056	−0.275***
STHNNESS	0.253*	0.472***	0.289**	0.400***	0.129	0.582***
RGUNMAG		0.150**		0.131**		0.174**
HUNTERS		0.247***		0.237***		0.255***
LICENSE	−0.077	−0.028	−0.083	−0.011	−0.047	−0.052
BYPERMIT	−0.150**	−0.13	−0.095	−0.030	−0.248***	0.012
WAITPER	−0.060	0.049	−0.041	0.046	−0.088	0.055
CRIMINAL	−0.035	−0.150**	−0.026	−0.138**	−0.032	−0.167***
MENTAL	−0.018**	0.029	−0.177***	0.046	−0.020**	0.021
ADDICT	0.112	0.072	0.114	0.053	0.092	0.099
ALCOHOLIC	0.037	0.028	0.035	0.020	0.033	0.040
MINORS	0.015	0.049	0.020	0.041	−0.010	0.064
REGISTER	0.124*	0.079	0.120*	0.068	0.127*	0.091
DEALER	−0.065	−0.133	−0.079	−0.117	−0.039	−0.155*
CARYHIDN	0.077	0.075	0.033	0.078	0.143	0.070
CARYOPEN	−0.056	−0.078	−0.058	−0.064	−0.023	−0.105
MANDPEN	−0.050	0.003	−0.075	0.025	−0.027	−0.013
ADDONDIS	−0.088	−0.058	−0.115**	−0.027	−0.033	−0.095
ADDONMND	−0.023	−0.071	−0.030	−0.054	0.019	−0.094
RTBRARMS	−0.047	−0.003	−0.038	−0.005	−0.031	−0.012
HHGBAN	0.08 7	0.014	0.093	−0.002	0.073	0.298
SNSBAN	0.083	−0.086	0.089*	−0.094	0.088	−0.082
HGBYBAN	0.001	0.005	−0.013	0.011	0.028	−0.003
LNMR		0.487**				
LNGUNMR				0.561**		
LNMGMR						0.413*
Gun prevalence[a]	−0.283		−0.111		−0.525*	
Gun Law Index[b]	0.409**	−0.775	0.408**	−0.714	0.324*	−0.799

Table III. Continued

(B) Aggravated assault models

	Total assault	Gun prevalence	Gun assault	Gun prevalence	Nongun assault	Gun prevalence
RPOV	0.626***		0.487***		0.591***	
COLLEGE	−0.154***		−0.116*		−0.132*	
CNTDIVRT	0.247***	0.078	0.168***	0.079	0.264***	0.077
ALCHLSM	0.232***		0.253***		0.210***	
PCTSMSA	−0.124*		−0.111		−0.116	
INVPOP	0.087		−0.036		0.128*	
STHNNESS	0.103	0.640***	0.159	0.580***	0.109	0.659***
RGUNMAG		0.133**		0.143***		0.127**
GUNTERS		0.177***		0.158***		0.183***
LICENSE	−0.040	−0.083	−0.029	−0.075	−0.068	−0.083
BYPERMIT	0.114	−0.064	0.129	−0.069	0.072	−0.056
WAITPER	−0.014	0.013	−0.028	−0.021	−0.033	−0.003
CRIMINAL	−0.028	−0.105*	−0.167**	−0.079	0.051	−0.114*
MENTAL	0.109	−0.118*	0.112	−0.125*	0.093	−0.112
ADDICT	0.093	0.050	0.161	0.026	0.049	0.057
ALCOHOLIC	0.082*	0.130**	0.017	0.128**	0.019	0.132**
MINORS	−0.044	0.064	−0.036	0.061	−0.043	0.065
REGISTER	0.013	0.019	0.111	−0.002	0.134	0.027
DEALER	−0.167	−0.086	−0.225**	−0.056	−0.137	−0.096
CARYHIDN	0.045	0.025	0.040	0.020	0.017	0.028
CARYOPEN	0.118	−0.060	0.004	−0.049	0.166*	−0.061
MANDPEN	−0.026	−0.025	−0.050	−0.020	−0.024	−0.024
ADDONDIS	−0.078	−0.118**	−0.096	−0.103*	−0.026	−0.127**
ADDONMND	0.014	−0.109	0.068	−0.111	−0.011	−0.111
RTBRARMS	0.098	−0.008	0.046	−0.007	0.122	−0.009
HGBAN	0.022	−0.026	0.045	−0.037	0.017	−0.024
SNSBAN	0.069	−0.064	0.156**	−0.075	0.043	−0.065
HGBYBAN	−0.106	0.038	−0.103	0.044	−0.084	0.034
LNASLT		0.126				
LNGNASLT				0.190**		
LNNGASLT						0.107
Gun prevalence[c]	−0.021		0.277		−0.194	
Gun Law Index[b]	0.095	−0.607**	0.014	−0.711***	0.107	−0.531**

Table III. Continued

(C) Robbery models

	Total robbery	Gun prevalence	Gun robbery	Gun prevalence	Nongun robbery	Gun prevalence
PCTBLACK	0.525*	0.541**	0.375	0.446***	0.610*	0.550***
PCTM1524	−0.073		−0.101		−0.051	
INEQUALT	0.458***		0.385***		0.438***	
COLLEGE	−0.197***		−0.087		−0.236***	
ADDICTRT	0.082		0.096*		0.070	
PCTSMSA	−0.201**	0.085	−0.311***	0.103	−0.010	0.089
VISITORS	0.257***	0.042	0.278***	0.004	0.212***	0.046*
INVPOP	−0.277***		−0.252***		−0.276***	
WEST	0.176		0.219**		0.160	
RGUNMAG		0.082		0.105		0.064
HUNTERS		0.100		0.108		0.085
LICENSE	−0.013	−0.078	0.012	−0.085	−0.029	−0.072
BYPERMIT	−0.089	−0.143*	−0.081	−0.132	−0.077	−0.129
WAITPER	0.033	−0.175	0.066	−0.227**	−0.024	−0.150
CRIMINAL	−0.070	0 .034	−0.107*	0.031	−0.033	0.038
MENTAL	−0.142	−0.292***	−0.035	−0.321***	−0.234	−0.273***
ADDICT	0.164	0.249**	0.160	0.233**	0.180	0.234**
ALCOHOLIC	0.066	0.040	0.047	0.037	0.059	0.038
MINORS	−0.002	0.013	−0.008	0.016	0.004	0.012
REGISTER	−0.007	−0.126	0.020	−0.145*	−0.065	−0.122
DEALER	−0.143*	−0.125	−0.126*	−0.110	−0.155	−0.121
CARYHIDIN	0.063	0.094	0.088	0.077	0.049	0.089
CARYOPEN	−0.032	−0.112	0.008	−0.126	−0.082	−0.106
MANDPEN	−0.147**	−0.066	−0.124**	−0.062	−0.164**	−0.066
ADDONDIS	−0.167**	−0.114	−0.110*	−0.102	−0.181**	−0.113
ADDONMND	0.018	−0.003	0.054	−0.011	−0.017	0.000
RTBRARMS	0.032	0.137	0.014	0.119	0.062	0.138
HGBAN	0.104	−0.031	0.194***	−0.052	0.051	−0.031
SNSBAN	0.060	0.019	0.070	0.020	0.074	0.019
HGBYBAN	−0.105*	0.007	−0.095*	0.034	−0.095	−0.003
LNROB		−0.149				
LNGNROB				0.012		
LNNGROBR						−0.206*
Gun prevalence[d]	−0.538		0.197		−0.793*	
Gun Law Index[b]	0.140	−0.216	−0.062	−0.538***	−0.043	−0.197***

Table III. Continued

(D) Rape and fatal gun accident models

	Rape	Gun prevalence	Fatal gun accidents	Gun prevalence
PCTBLACK	0.750***		0.296***	0.384***
CNTDIVRT	0.249***			
INVPOP	−0.242***		−0.117	−0.064
WEST	0.340***	−0.310*		
DENSITY	−0.168		0.088	
MFI		0.340		
OWNEROCC		0.556**		
ALCHLSM			−0.143	
ACCIDENT			0.217**	
RGUNMAG		0.187		0.155***
HUNTERS		−0.065		0.178***
LICENSE	0.079	−0.191*	−0.101	−0.129
BYPERMIT	−0.109	0.079	0.025	−0.189**
WAITPER	−0.061	−0.050	0.053	−0.248**
CRIMINAL	0.053	−0.106	0.123	0.023
MENTAL	−0.045	−0.076	−0.157	−0.287***
ADDICT	0.215	−0.038	0.001	0.176*
ALCOHOLIC	0.103	−0.127	0.030	0.068
MINORS	0.087	−0.057	−0.062	−0.033
REGISTER	−0.097	−0.059	−0.018	−0.039*
DEALER	−0.063	0.114	0.098	−0.159*
CARYHIDN	0.078	0.111		
CARYOPEN	−0.015	−0.098		
MANDPEN	−0.096	0.113		
ADDONDIS	−0.066	−0.038		
ADDONMND	0.133	−0.237*		
RTBRARMS	0.182**	−0.144		
HGBAN	−0.092	0.138	0.009	−0.028
SNSBAN	0.084	−0.128	0.063	0.000
HGBYBAN	−0.112	0.055	−0.099	0.061
LNRAPE		1.088***		
Gun prevalence[e]	−0.249		0.121	
Gun Law Index[b]	−0.051	−0.593	0.111	−1.262***

Table III. Continued

(E) Suicide models (OLS estimates)

	Total suicide	Gun suicide	Nongun suicide	Gun prevalence
TRNSIENT	0.240***	0.098*	0.286***	
CNTDIVRT	0.159**	0.165***	0.004	0.134
ALCHLSM	0.332***	0.255***	0.275***	
INVPOP	0.020	−0.071	0.125*	
DENSITY	−0.237***	−0.386***	0 .017	−0.197*
HOSPITAL	0.069	0.101*	0.008	
LIVLONE	0.183**	0.065	0.257***	
PCTOLD	0.138*	0.064	0.113	
RGUNMAG				0.065
HUNTERS				0.063
LICENSE	−0.033	−0.062	0.008	−0.171*
BYPERMIT	−0.089	−0.146**	0.053	−0.005
WAITPER	0.005	−0.025	0.008	−0.211*
CRIMINAL	0.071	0.090	−0.056	−0.129
MENTAL	−0.071	−0.134*	0.014	−0.095
ADDICT	0.058	−0.008	0.154	0.240***
ALCOHOLIC	−0.038	−0.010	−0.087	0.041
MINORS	−0.038	−0.018	−0.048	0.036
REGISTER	−0.063	−0.089	0.016	−0.139
DEALER	−0.229***	−0.140**	−0.207**	0.001
CARYHIDN				
CARYOPEN				
MANDPEN				
ADDONDIS				
ADDONMND				
RTBRARMS				
HGBAN	−0.062	−0.095	−0.037	0.114
SNSBAN	0.094	−0.014	0.148**	−0.013
HGBYBAN	−0.066	0.051	−0.093	0.107
Gun prevalence[f]	0.132**	0.252***	−0.101	
Gun Law Index[b]	−0.242	−0.319*	0.005	−0.084

[a]Principal-components factor with indicators PCTGNAST, PCTGNROB, PGS7982, and GUNSTOL.

[b]Principal-components factor with indicators: all gun laws.

[c]Principal-components factor with indicators PGH7982, PCTGNROB, PGS7982, and GUNSTOL.

[d] Principal-components factor with indicators PGH7982, PCTGNAST, PGS7982, and GUNSTOL.

[e]Principal-components factor with indicators PCTGNAST, PCTGNROB, PGS7982, GUNSTOL, and PGH7982.

[f]Principal-components factor with indicators PGH7982, PCTGNROB, PCTGNAST, and GUNSTOL.

*$P < 0.10$.

**$P < 0.05$.

***$P < 0.01$.

tion of this pattern of results is that the law is ineffective in reducing homicide, since it did not have a significant negative association with the rate of gun homicide.

6.1. Effects of Gun Prevalence Levels on Violence Rates

Estimates of the impact of gun prevalence on violence rates can be found in Table III in the penultimate row of each column referring to a violence rate. For example, the 2SLS coefficient estimating the impact of gun prevalence on the total murder rate is a nonsignificant −0.283 (column 2 in A).

Gun prevalence had an apparent significant positive effect on total rates of suicide, but not on any of the other five types of violence. The apparent effect of gun prevalence on suicide rates, however, is not entirely stable, being evident only when the suicide models were estimated with OLS. Some would argue that high suicide rates could discourage gun acquisition among people living in households with a person they believed to be suicide-prone. If this were true, then gun prevalence should be treated as endogeneous in the suicide models, just as in the other models (though for different reasons). When gun prevalence was treated as endogeneous, and the model was estimated with 2SLS, the results indicated no significant impact of gun prevalence on suicide. We tentatively conclude that gun prevalence rates *may* increase total suicide rates but have no effect on total rates of homicide, robbery, aggravated assault, rape, or fatal gun accidents.

6.2. Effects of Violence Rates on Gun Prevalence Levels

Coefficients estimating these effects can be found in the Gun Prevalence columns in Table III, in the rows near the bottom of each panel labeled with the names of the various violence rates. For example, in column 3 in A, the LNMR coefficient is a significant 0.487, indicating that the total homicide rate appears to have a positive impact on gun prevalence.

Homicide (gun, nongun, and total), gun assault, and rape rates all had significant positive coefficients in the gun prevalence equations. This supports the hypothesis that some violence rates encourage the acquisition of firearms for self-defense, accounting at least partially for bivariate positive associations observed between gun prevalence levels and violence levels. That rape in particular should have this effect is consistent with survey evidence that women's gun ownership, while lower than men's, is disproportionately likely to be motivated by self-defense concerns and with county-level findings that female gun ownership rates are more responsive to violence rates than men's ownership rates are (Bordua and Lizotte,

1979, p. 172). More generally, the results support the simple idea that rates of more serious violent crimes are more likely to increase gun acquisition.

6.3. Effects of Gun Controls on Gun Prevalence Levels

The effects of 19 types of gun regulations on gun prevalence levels are summarized in Table IVA. The effect of each gun restriction on gun prevalence was estimated multiple times, once in each of six violence rate models. Because the exact set of gun prevalence indicators used varied from one model to the next, it therefore was possible for estimated effects of gun controls on gun prevalence levels to vary somewhat from one violence rate model to the next. None of the gun controls appeared to have any impact on gun prevalence. Each law's effect on gun prevalence was initially estimated six times, but only bans on gun possession by criminals and mentally ill persons showed significant effects in even half of the initial tests.

Table IV. Summary of Effects of Gun Prevalence and Gun Controls on Violence Rates

(A) Significant negative impact of gun controls on gun prevalence?[a]

	Violence rate model					
	Homicide	Aggrvtd. assault	Robbery	Rape	Gun accidents	Suicide
LICENSE	No	No/Yes	No/Yes	Yes	No/Yes	Yes
BYPERMIT	No	No/Yes	Yes	No	Yes	No
WAITPER	No	No	No	No	Yes	Yes
CRIMINAL	Yes	Yes	No	No	No	No/No/Yes
MENTAL	No	Yes	Yes	No	Yes	No
ADDICTS	No	No	No	No	No	No
ALCOHOLIC	No	No	No	No	No	No
MINORS	No	No	No	No	No	No
REGISTER	No	No	No/No/Yes	No	Yes	No
DEALER	No	No	No	No	Yes	No
CARYHIDN	No	No	No	No		
CARYOPEN	No	No	No	No		
MANDPEN	No	No	No	No		
ADDONDIS	No	Yes	No	No		
ADDONMND	No	No	No	Yes		
RTBRARMS	No	No	No	No		
HGBAN	No	No	No	No	No	No
SNSBAN	No	No	No	No	No	No
HGBYBAN	No	No	No	No	No	No

Table IV. Continued

(B) Effects of gun prevalence and gun laws on violence rates

	Violence rate model					
	Homicide	Aggrvtd. assault	Robbery	Rape	Gun accidents	Suicide
Significant positive effect of gun prevalence?	No	No	No	No	No	(Yes)[b]
Significant negative effect of gun laws?[a]						
LICENSE	No/Yes/Maybe	No	No	No	No	No/Maybe/No
BYPERMIT	No/Maybe/No	No	No	No	No	Maybe
WAITPER	No	No	No	No	No	No
CRIMINAL	No	Maybe	Maybe	No	No	No
MENTAL	Yes	No	No	No	No	Maybe
ADDICT	No	No	No	No	No	No
ALCOHOLIC	No	No	No	No	No	No
MINORS	No	No	No	No	No	No
REGISTER	No	No	No	No	No	No
DEALER	No	Maybe	Yes	No	No	Maybe
CARYHIDN	No	No	No	No		
CARYOPEN	No	No	No	No		
MANDPEN	No	No	Maybe	No		
ADDONDIS	Maybe/ /Yes	No	Maybe	No/ /Yes		
ADDONMND	No	No	No	No		
RTBRARMS	No	No	No	No		
HGBAN	No	No	No	No/Yes/Yes	No	No/No/Maybe
SNSBAN	No	No	No	No	No	No
HGBYBAN	No	No	Yes	No	No	No
Gun Law Index	No	No	No	No	No	No

[a]Where more than one interpretation appears in a cell, it means that interpretations became more supportive of the gun control efficacy hypothesis when different specifications were used. (1) The first (and usually the only) interpretation pertains to models containing all 19 gun laws and no provision for interactions; (2) the second one pertains to results when using a reduced set of four gun control variables; (3) the third one pertains to results when multiplicative terms testing for interactions between gun laws and enforcement levels were specified (see text). Unsupportive results which remained unsupportive (No) under the latter two alternative specifications are not shown, to simplify the table.

[b]An effect of gun prevalence on total suicide rates was evident only when the model was estimated with OLS. When gun prevalence was treated as endogenous and the model was estimated with 2SLS, results did not indicate an impact of gun prevalence.

We checked to see if gun control effects on gun prevalence would become evident if we used a reduced set of four of the stronger gun laws (listed in a later section). The results for just one type of gun control changed (indicated by Yes appearing after one slash in a given cell in Table IVA)—gun owner licensing appears to reduce gun prevalence in five of the six violence models. However, since this apparent effect is evident only when there are no controls for other gun laws, this result may reflect the cumulative, albeit apparently slight, effects of other, correlated, gun laws as well as the effects of licensing itself. Therefore, interpretation of this result must remain ambiguous.

We also checked for interactions between gun laws and police enforcement effort by adding to each gun prevalence equation a multiplicative term for each gun control variable, consisting of the gun control variable multiplied times the weapons arrest rate. Of 108 tests for interactions, only 2 suggested an effect of gun controls on gun prevalence which was contingent upon enforcement effort, where no impact of the controls had been evident in the additive analysis. These are denoted by Yes appearing after two slashes in any of the cells in Table IVA (see CRIMINAL in the Suicide model and REGISTER in the Robbery model). Given the large number of tests, we believe that these two deviant results could be the product of chance. Thus, our evidence generally fails to support the hypothesis that the impact of gun controls on gun levels depends on the level of police enforcement.

6.4. Effects of Gun Control Laws on Violence Rates

Table III contains detailed results on this issue, which are summarized in Table IV. The findings indicate that most gun restrictions appear to exert no significant negative effect on total violence rates, though some gun controls do seem to be effective. Of 102 possible effects tested, 7 were consistently supportive of, and 11 others were at least partially consistent with, a hypothesis of gun control effectiveness, albeit using fairly generous evaluative criteria. As described below, each gun law's effect on a given form of violence was estimated under three conditions: (1) with all gun law variables specified in the models but with no measure of enforcement effort included, (2) with all gun control variables specified in the models and with interactions of gun laws and enforcement effort included, and (3) with a reduced set of four especially strong gun control variables included in the models. In the subsequent discussion, each law is assessed based on the most supportive of the three sets of results, i.e., the results most supportive of a violence-reducing impact of the law. Thus, the gun control efficacy hypothesis was given 18 chances at confirmation for any one form of gun

control, with hypothesis tests in three sets of circumstances, in each of six violence rate models. (There were, however, no tests of the impact of carry laws, add-on penalties for committing a crime with a gun, or right-to-bear-arms provisions on suicide or gun accident rates, as these regulations were considered irrelevant to suicides or accidents. For example, nearly all gun suicides are committed in a private location and thus are unlikely to be affected by carry laws.)

6.4.1. Results with All 19 Gun Control Variables Included, No Enforcement Interactions

Because we could not know in advance which gun control measures affected violence rates, we initially specified all 19 gun control variables in each violence rate equation (with the exceptions described in the previous paragraph). As noted previously, collinearity among these variables was generally slight, so this was not a serious statistical problem. We first present interpretations based on these specifications, followed by discussion of any results which were modified when a reduced set of gun laws were used or when interactions with enforcement levels were specified.

Requiring permits to buy guns (BYPERMIT) may reduce rates of suicide. Bans on possession of guns by convicted criminals (CRIMINAL) may reduce rates of aggravated assault and robbery. Bans on possession of guns by mentally ill persons (MENTAL) appear to reduce homicide and may reduce suicide. Requiring a state or local license to be a gun dealer (DEALER) appears to reduce rates of robbery and may reduce aggravated assaults and suicides. Laws that provide mandatory penalties for unlawful gun carrying (MANDPEN) may reduce robbery. Laws providing discretionary additional penalties for committing crimes with a gun (ADDONDIS) may reduce murder and robbery. Finally, local bans on the purchase of handguns appear to reduce robbery rates.

6.4.2. Results Using a Reduced Set of Gun Law Variables

While the problem is mild, there is some collinearity among the gun law variables which could inflate standard errors somewhat and thereby bias hypothesis tests in favor of the null hypothesis. Therefore the violence rate models were reestimated with just four gun law variables thought to be especially likely to show effects, since they were fairly strong measures—licenses, purchase permits, handgun possession bans, and bans on sale of "Saturday Night Specials." When this was done, four of the previous results were altered so as to strengthen, to varying degrees, support for the hypothesis of gun control efficacy. (Two results changed mildly from No to Maybe, while two changed substantially from No to Yes.) With the reduced set of gun law variables, estimates indicated that owner licensing

appears to reduce homicides and may reduce total suicides. Purchase permits may reduce homicides (there was still, however, a stronger negative association of permits with nongun homicide than with gun homicide). These estimations also indicated that handgun bans appear (somewhat implausibly, given how rarely rapists use guns) to reduce rapes, but not any other forms of violence. The rest of the gun law assessments were unaffected. Gun prevalence still showed no positive effect on any of the violence rates except the gun suicide and total suicide rates, the same as with models including the full set of gun laws. (Results are summarized in Table IV; estimates are not reported here but are available from the senior author.)

6.4.3. Interactions with Enforcement Level

It could be argued that gun laws are not always given a fair chance to work because in many places they are not adequately enforced. We tested this idea by forming multiplicative interaction terms between each gun law variable and a measure of police enforcement effort, the number of weapons arrests per 100 sworn police officers (WEAPARTS), and adding these terms into our models of violence rates. The resulting estimates generally confirmed the previous results. The coefficients for the interaction terms were rarely negative and significant, indicating that the effects of gun laws apparently were not dependent on the level of police enforcement effort, at least not based on the measure of effort used and not within the range of enforcement effort currently exerted in large U.S. cities. Of 102 possible interaction effects tested, only 5 suggested possible gun law effectiveness contingent upon the level of law enforcement effort: (1) laws providing discretionary add-on penalties for committing crimes with a gun appear to reduce the total homicide rate when accompanied by sufficient enforcement effort, (2) the same appears to be true for rape, (3) owner licensing may have such a contingent effect on homicide (4) handgun bans appear to have a contingent effect on the rape rate, and (5) handgun bans may have such an effect on the suicide rate. Given the large number of tests for interaction effects, however, five "significant" results might be little more than a product of chance. (Interaction test results are summarized in Table IV; estimates are available from the senior author.)

6.5. Gun Control as a Single Endogenous Variable

As noted before, we consider it unlikely that there is a simultaneous reciprocal relationship between gun laws and violence rates. Nevertheless, we estimated models of violence rates which assumed that such a relationship was possible. To do this, a Gun Law Index (GLI) was created

from all 19 gun control variables, using principal components analysis. This variable was treated as endogenous, in a model which assumed that simultaneous relationships existed among the GLI, the violence rate, and gun prevalence. Two instrumental variables were assumed to affect directly the GLI but not violence rates or gun ownership: LIBERAL, the percentage of a city's voters who voted for George McGovern in the 1972 presidential election (a measure of political liberalism), and NRA, the city's rate of membership in the National Rifle Association.

Estimates of the GLI coefficient are reported near the bottom of each violence rate column in Table III. Note that these are estimates from separate models which did *not* include the individual gun control variables and, thus, are not a part of the models to which the rest of the coefficients in Table III correspond. These estimates indicate that the overall level of gun control in a city does not appear to exert a significant negative effect on any of the six violence rates. The only hint of a possible exception was with suicide. Although the GLI was not related to the total suicide rate, its coefficient was negative and marginally significant ($0.05 \leqslant P < 0.10$) in the gun suicide equation and nonsignificant in the nongun suicide equation. Thus, treating gun control as a single endogenous variable did not strengthen support for the gun control efficacy hypothesis.

7. DISCUSSION

These results generally support the view that (1) existing gun control laws do not reduce gun prevalence in U.S. cities, (2) gun prevalence does not have any measurable net positive effect on violence rates except for a possible effect on suicide rates, and (3) most gun control laws do not reduce violence rates, though a few may do so.

For many gun regulations, such as carry controls or add-on penalties, it is not surprising that they do not reduce gun ownership, since they were not intended to do so. Still other gun controls may operate to restrict ownership only among "high-risk" groups such as criminals or alcoholics. However, results indicated that most gun controls fail to reduce gun use in acts of violence, undercutting the idea that controls reduce gun prevalence even in criminally involved subsets of the population. One simple explanation for this failure would be the huge size of the U.S. gun stock. With over 200 million guns in private hands, it is hard to keep guns away from anyone who strongly desires one.

Few of the tests unambiguously supported the gun law efficacy hypothesis. However, it increases confidence in some of these few supportive findings to know that they correspond closely with similar results in past research. (1) The present study found partial support for the claim

that laws establishing additional penalties for committing felonies with a gun may reduce total robbery rates, and prior research by McPheters *et al.* (1984) indicated the same thing. (2) Bans on gun possession by mentally ill persons may reduce suicide, consistent with the findings of Sommers (1984). (3) Mixed evidence suggested that handgun bans *may* reduce suicide, though this weak result reflected such controls in only two cities (New York City and Washington, DC). This is consistent with results of Loftin *et al.* (1991). (4) Finally, a previous study indicated that a mandatory penalty carry law, the Bartley–Fox law, appeared to reduce robbery (Deutsch and Alt, 1977), and the present research also indicates that such laws may reduce robbery.

As actually administered, "mandatory penalty" carry laws do not impose penalties in a truly mandatory fashion but, rather, merely in a relatively less discretionary one (Beha, 1977). Rather than mandatory penalties being viewed as essential, a more plausible interpretation of these results is that the mandatory penalty provision serves as an indicator of strong support among court actors for relatively severe punishment of unlicensed gun carrying. Where such laws exist, prosecutors may devote more resources to prosecuting illegal weapons carriers, and may be more likely to seek stiff penalties, even though they could evade the mandatory provisions if they chose to do so.

One type of gun law which clearly appeared to have some beneficial effect was a somewhat surprising one. Laws requiring a state or local license to be a firearms dealer were negatively related to aggravated assault, robbery, and suicide rates, with the results being strong (i.e., a Yes conclusion) for robbery. Because dealers everywhere in the United States are required to have a federal gun dealer license, additional state or local licensing requirements might seem trivial. However, if these requirements are more stringent or require high licensing fees, they can reduce the number of retail gun outlets and possibly reduce casual acquisition of guns among persons not sufficiently motivated or persistent to seek out less convenient stores or nonretail sources (Blose and Cook, 1980, p. 20). Although results summarized in Table IVA do not support the idea that this law reduces aggregate gun prevalence levels, it may affect a subset of weakly motivated buyers.

7.1. Gun Prevalence Effects

Why do gun prevalence levels have no apparent net positive effect on violence rates, with the possible exception of suicide? The absence of any net effects of gun levels could be due to counterbalancing effects of opposite sign, with criminal ownership increasing the rates and noncriminal

ownership decreasing them, due to deterrent effects of ownership among prospective victims (Kleck, 1988). If this were so, it might still be useful to reduce gun levels among criminals if measures used to accomplish this did not also reduce gun levels among noncriminals by an equal or greater amount.

Ordinary least-squares results indicated that gun prevalence may influence the choice of method in suicides and also the overall frequency of suicide. Gun prevalence was positively associated with both total suicide rates and gun suicide rates and negatively (though nonsignificantly) related to the nongun suicide rate.

No impact of gun prevalence on fatal gun accident rates was detected. Given the random component in accident causation and the rarity of fatal gun accidents (one or two a year in most cities), the absence of a relationship is perhaps not that surprising. It may also be that many cities with a higher gun prevalence, especially smaller cities and those in the South and West, have gun owners more thoroughly socialized from childhood into safe handling of guns, as opposed to getting guns as adults, without training.

The present results confirm those of the two best previous studies of city gun ownership and robbery rates, which also found no evidence of a net impact of gun ownership levels on the total robbery rate (Cook, 1979; McDowall, 1986). The present findings indicate that gun ownership levels increase (albeit nonsignificantly) gun robbery and decrease nongun robbery, suggesting that where guns are not available, robbers substitute other weapons, with no net effect on total robbery rates. Gun ownership levels also may have no net effect on total robbery because they may have a mixture of both positive and negative effects. On the one hand, guns make it possible for larger numbers of people to rob, including those too timid to rob without a gun, and expand the number of targets a given robber can successfully tackle. On the other hand, guns also enable robbers to rob more lucrative targets, increasing the average "take" per robbery and allowing them to gain a given amount of income with fewer robberies (Cook, 1976; Wright *et al.*, 1983). Also, gun ownership by prospective victims, especially retail store owners, may deter some robbers (Wright and Rossi, 1986, pp. 141–159; Kleck, 1988). The finding are consistent with an interpretation that these effects of opposite sign cancel each other out, with no net effect on the total robbery rate.

In assaultive crimes such as homicide and aggravated assault, gun availability also seems to have a mixture of positive and negative effects. In an individual-level analysis of violent incidents, Kleck and McElrath (1991) found that an aggressor's possession or use of a gun appears to reduce the probability of a physical attack (as opposed to a mere threat) on the victim

and appears to reduce the probability that the attack will result in a physical injury, while increasing the probability that an injury will be fatal. Further, possession of guns by prospective victims may exert a modest deterrent effect on would-be aggressors (Wright and Rossi, 1986; Kleck, 1988). The present aggregate level findings are consistent with a claim that the negative, violence-reducing effects of gun ownership may roughly cancel out the violence-increasing effects, consistent with the findings of previous time series research indicating no net effect, positive or negative, of gun ownership levels on the homicide rate (Kleck, 1984a).

7.2. Gun Law Effects

Why do most of 19 different major varieties of gun control laws appear to have no impact, with a few exceptions, on the types of violence which frequently involve guns? Many explanations are suggested by both our own results and those of prior research. First, some gun laws are intended to have their effects by reducing gun ownership levels, so some gun laws may fail because they do not achieve their proximate goal of reducing gun ownership (Table IVA). However, our results also generally indicate that gun prevalence levels do not have a net positive effect on violence rates (top row, Table IVB). Consequently, gun laws may fail simply because, even if they did reduce gun prevalence, this would not produce a reduction in violence rates.

On the other hand, the rationale for some gun regulations does not rely on an assumption that gun ownership levels affect violence. For example, carrying laws are intended to make guns less immediately available in public places rather than to reduce overall gun ownership levels; the rationale for such laws assumes only that the immediate availability of guns in public places is relevant to some violence rates, especially robbery. Likewise, add-on penalties are intended to discourage criminals from choosing guns to use in their crimes. It is also possible that gun laws have only a short-term effect on violence rates when they are passed and that the effect then fades. Most of the laws we have evaluated were implemented well in the past, so we cannot assess this idea.

Most gun laws regulate only handguns, or regulate handguns more stringently than the more numerous longguns such as rifles and shotguns (Kleck, 1991, Chap. 8). This permits the substitution of relatively unregulated longguns for the more heavily regulated handguns. While longguns are larger than handguns, and thus not so easily concealed or conveniently carried on the person, such a limitation is rarely relevant for suicides and is also irrelevant for many violent crimes, because either (1) the crime is committed in or near a private place, in a way which

does not require carrying or concealment of the gun, or (2) the crime was committed after some advance planning, in a way which would require only short-term carrying or which could involve use of a longgun whose barrel and stock had previously been cut down to render it concealable. Longguns are generally more lethal than handguns. Thus, while restrictions on handgun availability could cause some violent persons to go without guns of any kind, they may also have the undesirable effect of encouraging others to substitute more lethal longguns. The implication for the homicide rate would be that these effects would cancel out or, worse, produce a net increase in homicides (Kleck, 1984b).

No matter how severe current measures are, it is always possible that stronger measures are needed. However, even fairly strong measures such as banning sales of "Saturday Night Specials" and de facto bans on handgun possession appear generally to exert no negative effect on violence rates. Nevertheless, the findings reported herein cannot inform us about the effectiveness of gun control measures not yet tried.

It has been argued that many gun laws fail because they are local and that guns from more lenient jurisdictions "leak" into the stricter jurisdictions. Thus, federal measures regulating acquisition of guns might work (Newton and Zimring, 1969). Research on existing federal regulations has failed to generate consistent evidence of their effectiveness (Zimring, 1975; Magaddino and Medoff, 1984), but these controls were very weak, loophole-ridden measures. Some of the few measures found in this study to be effective were controls which are not vulnerable to this "leakage" problem. "Leakage" is an issue relevant mainly to regulations aimed at the acquisition of guns, rather than their use. In contrast, laws forbidding possession of handguns, regulating the carrying of guns, or providing for add-on penalties for using guns in crimes are not affected by interjurisdictional leakage because the legal risks of possessing or carrying a gun or using it in a crime in a given jurisdiction are the same regardless of whether bordering areas have similar measures.

It cannot be argued that the effects of gun ownership and gun control could not be detected due to a lack of meaningful variation in these variables. It is clear from the standard deviations for the gun prevalence indicators and the means for the gun law dummies in Table II that levels of both gun prevalence and gun control strictness vary enormously across U.S. cities. Direct survey measures of gun prevalence in very large cities indicate that the fraction of households reporting a gun varies from extremely low levels, such as 6% in New York City and Washington, DC (lower than in many Western European nations), to high levels, such as 61% in Houston (unpublished tabulations from specially geocoded General Social Surveys for 1973–1989).

Three limitations of this study should be noted. First, we had no measures of how strictly permit and license laws are administered, e.g., how narrowly authorities interpret rules defining which applicants are qualified, as distinct from how much effort is put into apprehending and punishing violators. Second, analysts always need to be skeptical about restrictions used to achieve identifiability in structural equation models. The key identification restrictions needed to model the assumed reciprocal relationship between gun prevalence and violence rates were the exclusion of gun magazine subscription rates and hunting license rates from the violence equations. Interest in hunting and other gun-related sports was assumed to affect gun prevalence rates but to not directly affect violence rates. One might argue that such interests may reflect, or even generate, proviolent attitudes, but Eskridge (1986) and Bordua (1986) have found county hunting license rates to have small to moderate *negative* associations with violence rates. Finally, it is possible that we have failed to control for some confounding variable which suppresses a guns–violence or gun law–violence association, though we do not know what that variable might be.

8. CONCLUSIONS

While the results are generally negative for the violence control effectiveness of gun control, the significance of the few supportive results should not be overlooked. There do appear to be some gun controls which work, all of them relatively moderate, popular, and inexpensive. Thus, there is support for a gun control policy organized around gun owner licensing or purchase permits (or some other form of gun buyer screening), stricter local dealer licensing, bans on possession of guns by criminals and mentally ill people, stronger controls over illegal carrying, and possibly discretionary add-on penalties for committing felonies with a gun. On the other hand, popular favorites such as waiting periods and gun registration do not appear to affect violence rates.

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