Firearms Laws and the Reduction of Violence

A Systematic Review

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Overview

The Task Force on Community Preventive Services (the Task Force) is conducting systematic reviews of scientific evidence about diverse interventions for the prevention of violence, and resulting injury and death, including, among others, early childhood home visitation,[1,2] therapeutic foster care,[3] the transfer of juveniles to the adult justice system, school programs for the teaching of prosocial behavior, and community policing. This report presents findings about the effectiveness of firearms laws in preventing violence. Studies of the following firearms laws were included in the review: bans on specified firearms or ammunition; restrictions on firearms acquisition; waiting periods for firearms acquisition; firearms registration; licensing of firearms owners; "shall issue" carry laws that allow people who pass background checks to carry concealed weapons; child access prevention laws; zero tolerance laws for firearms in schools; and combinations of firearms laws.

The Task Force found the evidence available from identified studies was insufficient to determine the effectiveness of any of the firearms laws reviewed singly or in combination. A finding that evidence is insufficient to determine effectiveness means that we do not yet know what effect, if any, the law has on an outcome—not that the law has no effect on the outcome. This report describes how the reviews were conducted, gives detailed information about the Task Force's findings, and provides information about research gaps and priority areas for future research.

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Introduction

Although rates of firearms-related[a] injuries in the United States have declined since 1993, they remained the second leading cause of injury mortality in 2001, the most recent year for which complete data are available.[4] Of 29,573 firearms-related deaths in 2001—an average of 81 per day—16,869 (57.0%) were suicide; 11,671 (39.5%) were homicide or legal intervention (e.g., homicide by police); 802 (2.7%) were unintentional; and 231 (0.8%) were of undetermined circumstances. In 1998, for each firearm-related death, 2.1 nonfatal firearm-related injuries were treated in emergency departments.[5] It is estimated that 24.3% of all violent crimes—murder, aggravated assault, rape, and robbery—committed in 1999 (a total of 1,430,693) were committed with a firearm.[6] Rates of firearm-related homicide, suicide, and unintentional death in the United States exceed those of 25 other high-income nations (i.e., 1996 GNP ≥US$9636 per capita) for which data are available (Figure 1).[7] The cost of firearm-related violence in the United States is estimated to be approximately $100 billion per year.[8]

Approximately 4.5 million new (i.e., not previously owned) firearms are sold each year in the United States, including 2 million handguns. In addition, estimates of annual secondhand firearms transactions range from 2 to 4.5 million.[9,10] Further, it is estimated that approximately 0.5 million firearms are stolen annually.[10] Thus, the estimated total number of firearms transactions ranges from 7 to 9.5 million per year, of which between 47% and 64% are new firearms.

New firearms can be sold legally only by federal firearms licensees (FFLs); FFL transactions comprise the primary market.[10] FFLs are required to comply with the Permanent "Brady Law" (P.L. 103-159, Title XVIII, Section 922(t)) and initiate background checks to investigate whether would-be purchasers violate federal or state purchasing requirements (e.g., people convicted of a felony must be excluded). In the "secondary market" of firearms not sold by FFLs, private citizens

[a]A firearm is a weapon (e.g., handgun, rifle, or shotgun) in which a shot is propelled by gunpowder.

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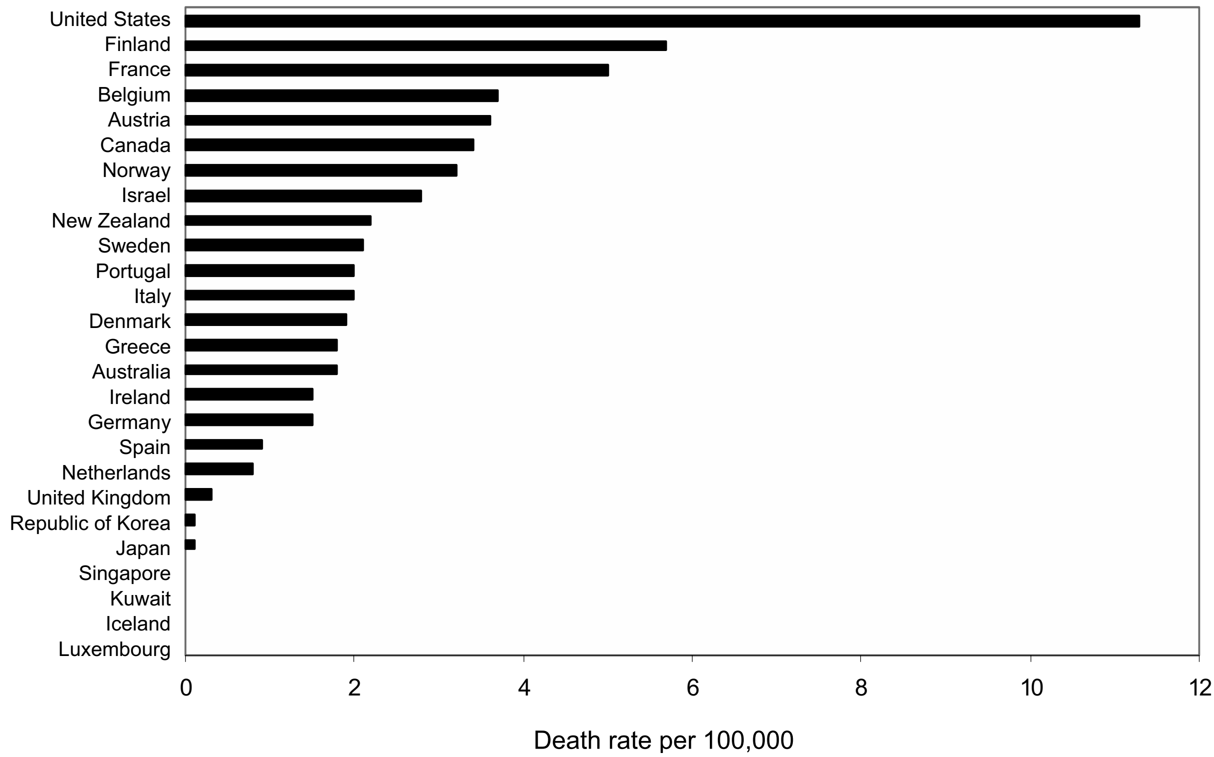

Figure 1. Firearm-related mortality for high-income World Health Organization Member States (most recent year available between 1990 and 2000). (Note: A firearm is defined as a weapon [e.g., handgun, rifle, or shotgun] in which a shot is propelled by gunpowder.)

may sell their firearms without a license; firearms shows constitute an important segment of the secondary market.[10] Private citizens are not supposed to knowingly sell firearms to people in excluded categories, but, although several states require background checks for private sales,[9] private sales are not federally regulated.[11]

The 1994 National Survey of the Private Ownership of Firearms (NSPOF) indicated that adults in the United States owned approximately 192 million working firearms—an average of one per adult.[12] NSPOF also indicated that firearms ownership was unevenly distributed in the population: 24.6% of U.S. adults owned a firearm—41.8% of men and 9.0% of women. Another survey[6] found that 41% of (adult) respondents reported having a firearm in their home in 1994, as did 32% in 2000. A third survey[13] reported that 35% of homes with children aged <18 years had at least one firearm. Of the 192 million firearms owned in the United States in 1994, 65 million were handguns, 70 million rifles, 49 million shotguns, and the remainder, other firearms.[12] Approximately 40% of handguns and long firearms were semiautomatic. Among handgun owners, 34.0% kept their firearms loaded and unlocked. An estimated 10 million handguns—one sixth of the handguns owned—are regularly carried by their owners, about half in the owners' cars and the other half on the owners' persons.[10]

The NSPOF also found that, among adult firearm owners, 9.7 million owned more than an average of ten firearms each, whereas 34.4 million owned a mean of approximately 2.5 firearms each. Among owners who only owned handguns, 74.4% reported owning for self-defense, 0.5% for hunting, 10.8% for target or sport shooting, and the remaining 13.5% for other purposes. Among owners of long firearms only (i.e., rifles and shotguns), 14.9% reported owning for self-defense, 69.9% for hunting, 6.1% for target or sport shooting, and the remaining 9.1% for other purposes.

This review examines firearms laws as one of many potential approaches to the reduction of firearm-related violence.[14,15] The manufacture, distribution, sale, acquisition, storage, transportation, carrying, and use of firearms in the United States are regulated by a complex array of federal, state, and local laws and regulations. The focus of this review is on assessing the effects of selected federal and state laws on violence-related public health outcomes, including death and injury resulting from violent crimes, suicide, and unintentional incidents; we also note effects on other outcomes, such as property crime, the apprehension of criminals, and school expulsion.

Reviews of firearms laws and studies of their effects have been conducted by many others.[16–20] The present review of selected laws differs from those reviews in that

Table 1. Selected *Healthy People 2010*[21] objectives related to firearms legislation, and proposed health-related outcomes

Injury prevention
Reduce firearm-related deaths from 11:3 to 4.1 per 100,000 population[a] (Objective 15-3).
Reduce the proportion of persons living in homes with firearms that are loaded and unlocked from 19% to 16%[a] (Objective 15-4).
Reduce nonfatal firearm-related injuries from 24.0 (in 1997) to 8.6 per 100,000 population (Objective 15-5).
Unintentional injury prevention
Reduce deaths caused by unintentional injuries from 35.0 to 17.5 per 100,000 population[a] (Objective 15-13).
(Developmental) Reduce nonfatal unintentional injuries (Objective 15-14).
Violence and abuse prevention
Reduce homicides from 6.5 to 3.0 per 100,000 population[a] (Objective 15-32).
Reduce the rate of physical assault by current or former intimate partners from 4.4 (in 1998) to 3.3 per 1000 persons aged ≥12 years (Objective 15-34).
Reduce the annual rate of rape or attempted rape from 0.8 (in 1998) to 0.7 per 1000 persons aged ≥12 years (Objective 15-35).
Reduce sexual assault other than rape from 0.6 (in 1998) to 0.4 per 1000 persons aged ≥12 years (Objective 15-36).
Reduce physical assaults from 31.1 (in 1998) to 13.6 per 1000 persons aged ≥12 years (Objective 15-37).
Reduce weapon carrying by adolescents on school property from 6.9% (in 1999) to 4.9% (students in grades 9 through 12, carrying during the past 30 days) (Objective 15-39).
Mental health and mental disorders
Reduce the suicide rate from 11.3 to 5.0 per 100,000 population[a] (Objective 18-1).
Reduce the 12-month average rate of suicide attempts from 2.6% to 1% among adolescents in grades 9 though 12 (Objective 18-2).

[a]Baseline: 1998 data, age adjusted to the year 2000 standard population.

it is based on systematic epidemiologic evaluations and syntheses of all available literature meeting specified criteria.

The *Guide to Community Preventive Services*

The systematic reviews in this report represent the work of the independent, nonfederal Task Force on Community Preventive Services (the Task Force). The Task Force is developing the *Guide to Community Preventive Services* (the *Community Guide*) with the support of the U.S. Department of Health and Human Services (DHHS) in collaboration with public and private partners. The Centers for Disease Control and Prevention (CDC) provides staff support to the Task Force for development of the *Community Guide*. A special supplement to the *American Journal of Preventive Medicine*, "Introducing the Guide to Community Preventive Services: Methods, First Recommendations and Expert Commentary," published in January 2000 (volume 18, supplement 1), presents the background and the methods used in developing the *Community Guide*. The *Community Guide* conducts reviews on a wide array of public health topics. The present review is part of a broader *Community Guide* review of violence prevention. The broader review focuses on youth as victims and perpetrators of violence, but this review addresses firearms laws affecting both adults and youth, since there are few laws directed specifically toward youth.

Healthy People 2010 Goals and Objectives

This review provides information on the state of knowledge about firearms laws interventions related to the violence prevention objectives in *Healthy People 2010*,[21] the disease prevention and health promotion agenda for the United States. These objectives identify some of the significant preventable threats to health and help focus the efforts of public health systems, policymakers, and law enforcement officials in their efforts to address those threats. Many of the proposed *Healthy People* objectives in Chapter 15, "Injury and Violence Prevention," include outcomes that might be affected by firearms laws (Table 1).

Conceptual Approach and Analytic Framework

The general methods for conducting systematic reviews for the *Community Guide* have been described in detail elsewhere.[22–25] This section describes the conceptual approach, the selection of laws for review, review methods, and the determination of which outcomes to consider in assessing the effects of firearms laws on violence.

The logic model used by the review team to evaluate the effectiveness of firearms laws in reducing violence (Figure 2) depicts the flow of influences of firearms laws on firearms from their manufacture, through their distribution, acquisition, storage, carrying, and use, to violent acts (including self-defense) and physical or psychosocial injury to direct and indirect victims. Enforcement plays a role at several stages in this process. The enforcement of firearms laws may prevent violence by averting illegal firearms use and may also deter potential violence. Inadequate enforcement may diminish the effect of a law and make it difficult to assess the potential effect of a law.

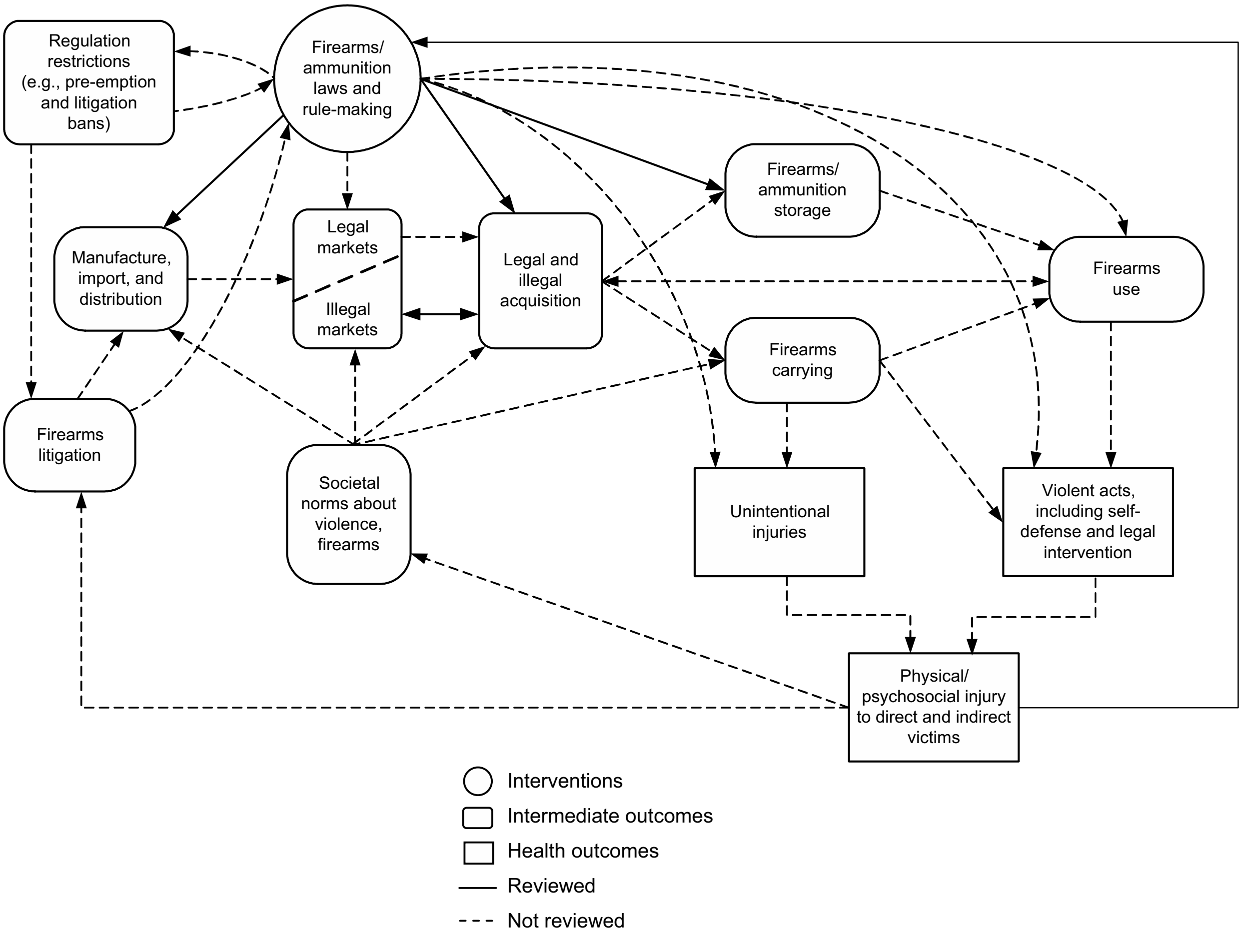

Figure 2. Effects of firearms laws on violence.

We note (Figure 2) two ways in which the legal process may itself be limited by regulation restrictions: (1) through bans on firearms litigation, and (2) through preemption laws that prohibit lower-level legislative bodies (e.g., counties) from enacting stronger firearms laws than those enacted at a higher level (e.g., states). The model also indicates how violent outcomes may, in turn, affect the legislative process by means of several feedback loops (e.g., the effects of mass shootings on efforts to pass laws). The laws reviewed here were chosen to cover different facets of this model. Many other legal measures also merit study (e.g., laws requiring firearm safety training, allowing purchase of only one firearm per month, increasing taxes, and requiring background checks in private sales).[26]

The present review focuses on firearms laws as one means of preventing violence. Our approach is consistent with the preventive orientation of public health and with the general approach of the *Community Guide*. Prevention is regarded as a complement to, not a replacement for, law enforcement. Subsequent reviews will examine several aspects of the justice system in reducing violence.

The scientific evidence of effectiveness was reviewed for seven firearms laws and for combinations of firearms laws (including combinations of the other laws reviewed):

Bans on specified firearms or ammunition
Restrictions on firearms acquisition
Waiting periods between application to purchase and acquisition of firearm
Licensing of firearms users and registration of firearms
Shall issue concealed-weapons carry laws (which obligate issuing agencies to grant permits for carrying concealed weapons to applicants unless excluded by specific criteria)
Child access prevention laws requiring safe storage of firearms by owners
Zero tolerance of firearms in school
Combinations or systems of firearms laws

Methods

In the *Community Guide*, evidence is summarized about (1) the effectiveness of interventions; (2) the applicability of findings (i.e., the extent to which available effectiveness data might apply to diverse populations and settings); (3) other positive or negative effects of the intervention, including positive or negative health and nonhealth outcomes; (4) economic impact; and (5) barriers to implementation of interventions. In the present review, in which sufficient evidence to determine the effects of firearms laws on violence was not found, we rarely included comments on applicability or barriers to implementation, and no economic evaluations were conducted.

As with other *Community Guide* reviews, the process that was used to review evidence systematically and then translate that evidence into the conclusions presented in this article involved:

- Forming a systematic review development team
- Developing a conceptual approach to organizing, grouping, and selecting interventions
- Selecting interventions to evaluate
- Searching for and retrieving evidence
- Assessing the quality of and abstracting information from each study
- Assessing the quality of and summarizing/synthesizing the body of evidence of effectiveness
- Translating the evidence about effectiveness into conclusions

Systematic Review Development Team

Three groups of individuals served on the systematic review development team:

- A coordination team drafted the conceptual framework for the reviews, coordinated the data collection and review process, and drafted evidence tables, summaries of the evidence, and the reports. This team consisted of a Task Force member, experts in the methods of systematic reviews and economics from the *Community Guide* and Prevention Effectiveness Branches, Division of Prevention Research and Analytic Methods, Epidemiology Program Office, CDC; and experts on violence prevention from the National Center for Injury Prevention and Control, CDC, the National Institutes of Health, and the National Institute of Justice, U.S. Department of Justice.
- A consultation team set initial priorities for the reviews and reviewed and commented on materials developed by the coordination team. The consultants are experts on violence-related topics in state and local public health settings, academic organizations, federal agencies, and voluntary organizations. These experts have backgrounds in sociology, medicine, public health, economics, health promotion, intervention design and implementation, health education, health policy, and epidemiology.
- An abstraction team collected and recorded data from studies for possible inclusion in the systematic reviews. (See Evaluating and Summarizing the Studies section.)

Search for Evidence

Electronic searches for literature were conducted in MEDLINE, EMBASE, ERIC, NTIS (National Technical Information Service), PSYCHLIT, PAIS (Public Affairs Information Service), Sociological Abstracts, NCJRS (National Criminal Justice Reference Service), CJPI (Criminal Justice Periodicals Index), Gale Group Legal Research Index, and ECONLIT. We also reviewed the references listed in all retrieved articles, and consulted with experts on the systematic review development team and elsewhere to find additional published reports of studies. We included journal articles, governmental reports, books, and book chapters. We also reviewed several papers that were in press at the time, identified in web searches and by consultants.

Articles were considered for inclusion in the systematic review if they had the following characteristics:

- Evaluated the specified law
- Assessed at least one of the violent outcomes specified
- Were conducted in an established market economy[b]
- Reported on a primary study rather than, for example, a guideline or review
- Compared a group of people who had been exposed to the intervention with a group of people who had not been exposed or who had been less exposed (the comparisons could be concurrent or in the same group over a period of time)
- Published between 1979 and March 2001.

We define a "study" as a research project conducted by a researcher or research group on a particular (study) population during a given time period, assessing specified research questions using specified methods. Some studies report analyses of a population at more than one time; multiple findings may thus be included within the study. A study may result in several "reports" on different aspects of the study (e.g., study theory or methods, study population, specific findings). We consider all reports together to constitute the study and use aspects of the reports that correspond to the topics of our review and our review criteria. In some cases, the distinction between studies and reports may be arguable—there is not always a clear line. When a research team completes a study, another team responds to it with a different analysis of the original population (a second study) and the original team then conducts yet a different version of their original study (e.g., using a new control population); we count the original team's new study as a different, third study, and note the connection to the original study and study team.

Outcomes Reviewed

The outcome measures evaluated to determine the effect of the laws reviewed were specific violent crimes (i.e., murder, aggravated assault, robbery, and rape), suicide, and unintentional firearm injury. Aggravated assault is considered a health-related outcome insofar as it is "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury."[6] Similarly, robbery is considered a health-related outcome insofar as it is "the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear."[6]

[b]Established market economies as defined by the World Bank are Andorra, Australia, Austria, Belgium, Bermuda, Canada, Channel Islands, Denmark, Faeroe Islands, Finland, France, Germany, Gibraltar, Greece, Greenland, Holy See, Iceland, Ireland, Isle of Man, Italy, Japan, Liechtenstein, Luxembourg, Monaco, the Netherlands, New Zealand, Norway, Portugal, San Marino, Spain, St. Pierre and Miquelon, Sweden, Switzerland, the United Kingdom, and the United States.

Although some studies reported firearm-specific outcomes (e.g., firearm-related suicide), we preferred to use outcomes that did not specify a relationship to firearms, because of a concern that the reduction in the firearm-specific outcome might be accompanied by an increase in non–firearm-specific outcomes (e.g., suicide by hanging), thus possibly reducing or even outweighing the firearm-related benefit. Because violence is not reduced if those who intend to commit suicide with firearms find other means when firearms are no longer available, we measure the overall change in outcomes (e.g., the rate of suicide).

Some studies[27,28] assessed the numbers of firearms retrieved in the course of investigating crimes, including violent crimes, as outcome measures. For example, studies of firearms bans may have considered counts of firearms found at crime scenes before and after bans as an indication of the effects of the bans. The use of such evidence to assess the effects of interventions on violent outcomes rests on many assumptions, for example, that rates of firearms retrieval are similar over time for different kinds of firearms, in different settings. We use such studies only as secondary evidence unless the researchers provide evidence that a high proportion of crime firearms are recovered or other evidence that the recovery process does not bias the assessment of the violent outcomes of interest.

Abstraction and Evaluation of Individual Studies

Two reviewers read each study that met the inclusion criteria, using standardized *Community Guide* criteria to assess the study evidence.[25] Disagreements between the reviewers were reconciled by consensus of the coordination team members. In addition, to ensure consistent assessment of study design suitability and limitations in execution quality within the body of evidence for each intervention, evaluated studies were discussed by the coordination team.

Assessing Suitability of Study Design

Design suitability was assessed for every included study.[22] Our study design classifications, chosen to ensure consistency in the review process, sometimes differ from the classification or nomenclature used by study authors. Studies of "greatest design suitability" were those with a concurrent comparison group, in which data were collected prospectively; studies of "moderate design suitability" were retrospective studies or those with multiple pre- or post-intervention measurements, but no concurrent comparison group; studies of "least suitable design" were cross-sectional studies or those with no concurrent comparison group and only single pre- and post-intervention measurements. Noncomparative studies (i.e., those without before-and-after intervention comparison or distinct concurrent comparison populations) were not considered in our reviews.

Assessing Study Quality and Summarizing the Body of Evidence of Effectiveness

Quality of study execution was systematically assessed using the published *Community Guide* methods.[22,25] Studies can have as many as nine limitations, including failure to describe the study population and intervention, measure exposures or outcomes effectively, demonstrate effective follow-up, use appropriate analytic methods, and control for confounding or other bias. Studies with zero or one limitation are reported to have "good execution"; studies with two to four limitations are reported to have "fair execution"; and studies with five or more limitations are reported to have "limited execution" and are not included in the body of evidence.

Unless otherwise noted, we represented results of each study as point estimates for the relative change in the rate of violent crime, suicide, or unintentional injury or death attributable to the interventions. We calculated percent changes and baselines using the following formulas for relative change:

For studies with before-and-after measurements and concurrent comparison groups:

$$(\text{Ipost}/\text{Ipre})/(\text{Cpost}/\text{Cpre}) - 1$$

where

Ipost = last reported outcome rate in the intervention group after the intervention
Ipre = reported outcome rate in the intervention group immediately before the intervention
Cpost = last reported outcome rate in the comparison group after the intervention
Cpre = reported outcome rate in the comparison group immediately before the intervention

For studies with post-intervention measurements only and concurrent comparison groups:

$$(\text{Ipost} - \text{Cpost})/\text{Cpost}$$

For studies with before-and-after measurements but no concurrent comparison:

$$(\text{Ipost} - \text{Ipre})/\text{Ipre}$$

We report the effect as "desired" when, compared with the absence of such a law, the law is associated with a decrease in a violent outcome examined, and as "undesired" when the law is associated with an increase in the violent outcome. When effect measures reported by the authors could not be converted into percentage changes (e.g., when results were presented as absolute change in rates, without information on baseline rates), the reported findings are described in the text. In the reporting of study findings, we used the standard two-tailed p-value cut-off at the 0.05 level as a measure of statistical significance.

We often had to select among several possible effect measures for inclusion in our summary measures of effectiveness. When available, we used measures adjusted for potential confounders in multivariate analysis in preference to crude effect measures. Although no studies were excluded from evaluation strictly on the basis of an insufficient follow-up period, follow-up periods of <1 year were considered an execution flaw, and studies with longer follow-up were preferred.

The studies we examined did not always share our research goals; they examined or provided data to assess outcomes of interest to us, but may have focused on outcomes that differed from those we sought to examine. For example, one study[29] examined the effect of misdemeanor restrictions on firearms purchase on subsequent first arrests for firearms or violent crime. Because we were specifically interested in violent, but not nonviolent, firearm-related crime, and in all subsequent arrests rather than only the first, we used only study findings on the outcomes of interest to our review, rather than those focused on by the authors.

As noted above, we often transformed the researcher's findings mathematically to make measures comparable across studies. For example, in one study[30] of the effects of shall issue concealed-weapons carry laws, the author focused on the difference in changes of rates for juveniles and adults, on the premise that the law should reduce homicide among adults at a greater rate than among juveniles (because the law does not directly apply to juveniles), and assuming that this comparison was an effective way to control confounding. We could not use the results of this analysis to compare with other studies that assessed changes in rates, so we used baseline information provided in this study, and calculated changes in adult and juvenile homicide rates associated with implementation of the law. Our modifications of study approaches are noted in the summary evidence tables available at the *Community Guide* website (*www.thecommunityguide.org/violence*).

In several firearms law reviews, two or more studies—most often conducted by different research teams—examined the same intervention (e.g., a specific law, in the same population, over the same time period) and reported on the same outcome(s), but differed in study design and execution quality. We characterize such studies as nonindependent because they represent a single experience of the assessed intervention. To avoid double counting of a single experience, we chose the study with the best combination of design suitability and quality of execution to represent the overlapping group of studies. We refer to separate analyses from one study, including distinct publications, as "reports." Some studies were only partially overlapping (e.g., providing overlapping national estimates but one or more unique state estimates). In those cases, we excluded the overlapping estimates but used the nonoverlapping ones. Some studies provided findings on several firearms laws and may thus be analyzed in two or more of our reviews.

We summarized the strength of the body of evidence based on numbers of available studies, strength of their design and execution, and size and consistency of reported effects using the *Community Guide* approach described in detail elsewhere.[22] When the number of studies and their design and execution quality were sufficient by *Community Guide* standards to draw a conclusion on effectiveness, results are summarized graphically and statistically. To summarize the findings about the effectiveness of an intervention across the studies in a body of evidence, we display results of individual studies in tables and figures and report median and interquartile range of effect measures. We note whether or not zero is included within the upper and the lower interquartile ranges. When the range includes zero, we infer that the results are inconsistent in direction; when the interquartile range does not include zero, we infer that the results are consistent in direction.

It is critical to note that when we conclude that evidence for the effectiveness of a given firearms law on an outcome is insufficient, we mean simply that we do not yet know what effect, if any, the law has on that outcome. We do not mean that the law has no effect on the outcome.

Other Effects

We routinely sought information on other (i.e., not violence-related) effects of these population-based interventions, such as property crime and school expulsions. We sought evidence of potential harms or benefits if they were mentioned in the effectiveness literature or considered important by the coordination team. With the exception of property crime, additional outcomes were not specifically assessed in the papers that we reviewed.

Economic Evaluations, Applicability of Interventions, and Barriers to Implementation

In *Community Guide* reviews, economic evaluations are summarized for each intervention found to have at least sufficient evidence of effectiveness.[22] Because we did not find sufficient evidence of effectiveness of any of the laws reviewed, no economic evaluations were performed.[24] The applicability of the intervention to populations and settings not specifically studied and the barriers to implementation of the intervention may be assessed whether or not the intervention is found to be effective.

Summarizing Research Gaps

Many systematic reviews in the *Community Guide* identify existing information on which to base public health practice. Whether or not a sufficient evidence base supports practice recommendations, an important benefit of these reviews is identification of areas where information is lacking or of poor quality. For the topics reviewed here, evidence was insufficient to develop recommendations. We summarized remaining questions about effectiveness, and identified key issues that had emerged from the review, based on the informed judgment of the systematic review development team.

Sources of Information for Firearms Law Effectiveness Studies

Studies of firearms law effectiveness have employed several sources of information, and the limitations of these sources should be understood. Information on laws—the "exposures" in these studies—are derived from federal government reports (e.g., Bureau of Alcohol, Tobacco and Firearms, 2000[11]) and published analyses (e.g., Cramer and Kopel, 1995[31]). There have been substantial discrepancies among sources in the specification of which jurisdictions have enacted which laws; this has led to differences in the classification of "exposure" to laws in evaluation studies and systematic reviews.

Evaluations of the effectiveness of firearms laws most often rely on two sources of information on violent outcomes: the Uniform Crime Report (UCR) from the Federal Bureau of Investigation (FBI), and Vital Statistics of the United States from the National Center for Health Statistics of the CDC. These record systems were initially developed for administrative uses and simple statistical monitoring, but have been widely used for research.

Most studies of the effects of firearms laws use the UCR to assess outcomes; the UCR documents reports of and arrests for violent crimes (i.e., murder, robbery, aggravated assault, and rape) and property crimes (i.e., burglary, larceny, automobile theft, and arson) sent to the FBI by the 18,413 law enforcement reporting agencies in the United States.[32] There are several limitations of UCR data. First, crime reporting to the police is not complete.[6] A population-based survey, the National Crime Victimization Survey, indicates that, in 2001, U.S. adults reported to law enforcement agencies only 61.4% of their 1.8 million experiences of violent crime victimization (excluding murder).

In addition to incomplete reporting to police by victims, law enforcement agencies substantially under-report crime to the FBI. For example, during the 36-month period from 1992 to 1994, only 64% of these agencies reported crimes for each month, and 5% provided no data at all.[32] Moreover, quality of reporting varies substantially by time and by state: from the mid-1980s to the late 1990s, 12 states reported problems with their data (e.g., using definitions for specific crimes that differed from UCR definitions) for >1 year, and these data could not be used in the UCR.

When data are missing in the UCR, they are imputed, generally on the assumption that information not reported for given reporting areas at given times is similar to that reported in other places or time periods. Maltz and Targonski[33] recently argued that UCR crime data at the county level are currently too unreliable for use in research; however, because crime generally occurs at higher rates in cities, city-level crime data are regarded as sufficiently reliable for research use. The problems of police reporting described by Maltz[32] compound the under-reporting of crime by victims. Since nationally representative surveys of victims indicate that victims report only 43.9% of violent victimizations and since the UCR represents 87% of the U.S. population, UCR crime data are likely to represent approximately 38.2% (i.e., 0.87×0.439) of violent victimizations in the United States.[34] Under-reporting by itself might not result in bias, but if under-reporting differs systematically across times or places—a plausible scenario—it could result in biases in either direction. The UCR data source supplies a special population data set that is reduced in numbers in proportion to the under-reporting in each reporting area: use of standard, unreduced population estimates from the Bureau of the Census will underestimate rates in these circumstances.

In addition to under-reporting, UCR data present another challenge for research: They are aggregated, so that numbers of events are reported, but not information on the circumstances of each event. Aggregate reporting limits the analysis of social "mechanisms" by which firearms laws might work. Several studies of the effects of firearms laws on homicide have used the FBI's *Supplemental Homicide Reports*[35] in which individual record information is available, allowing fuller analysis of the circumstances of homicides. The implementation of the FBI's National Incident-Based Reporting System[36] and the development of the National Violent Death Reporting System[37] may substantially address this limitation of the UCR data system.

The other principal source of data for firearms law evaluation outcomes, Vital Statistics of the United States—a report of U.S. deaths prepared by CDC's National Center for Health Statistics—includes information on homicides, suicides, and unintentional deaths, including firearm-related deaths. Although virtually all U.S. deaths, including deaths in all counties, are counted in this system, some misclassification occurs by cause of death (particularly for causes such as suicide and unintentional injury)[38] as well as by demographic characteristics.[39,40] Unintentional firearm-related deaths appear to be substantially undercounted (i.e., misclassified as due to another cause).[41] Furthermore, there is a lack of circumstantial detail in vital statistics data, particularly about the perpetrators of homicide and the agents of unintentional injuries.

Finally, sources of information on potential confounders in firearms law effectiveness studies have presented a challenge. Major confounders include phenomena such as poverty, unemployment, gangs, drug cycles, intensity of law enforcement, and other existing laws. There have been disagreements about how best to conceptualize and measure these, and data for some have been difficult, if not impossible, to find. Information on arrests for crime has been used as an independent variable in firearms law studies to control for degree of enforcement activity. Yet FBI arrest data may be even more problematic than UCR crime data in terms of under-reporting and differential reporting by crime and other characteristics.[32] Arrest rates (i.e., number of arrests per number of crimes) have been used to control for potential confounding by degree of law enforcement; however, the use of arrest rates creates statistical problems, because crime is then both the dependent and an independent variable in these analyses. Taken together, all of these features of available data sources severely limit the ability to understand the effectiveness of firearms law in preventing violence.

Results: Part I—Intervention Effectiveness

Bans on Specified Firearms or Ammunition

Bans on specified firearms and ammunition prohibit the acquisition and possession of certain categories of firearms (e.g., machine guns or assault weapons) or ammunition (e.g., large-capacity magazines or hollow-point bullets). They can also include prohibitions on the importation or manufacture of the specified firearms. Bans may be adopted at the federal, state, or local level, and may be combined with additional firearms regulations, such as requirements for safe storage, age restrictions on acquisition, or restrictive licensing requirements for firearms dealers. Bans are intended to decrease the availability of certain types of firearms to potential offenders, and thus reduce the capacity of such offenders to perpetrate crime.[27]

Bans are usually imposed on the types of firearms or ammunition that are either thought to be particularly dangerous and not well suited for hunting or self-defense (e.g., semiautomatic and fully automatic assault weapons) or disproportionately involved in crime (such as cheap, low-quality, small-caliber handguns usually referred to as "Saturday night specials"). Sometimes, especially in high-crime urban settings, bans may include a broad spectrum of firearms (e.g., the ban enacted in Washington DC in 1976,[42] on purchase, sale, transfer, and possession of all handguns by civilians unless the handguns were previously owned and registered).

Table 2. Bans of gun acquisition or possession: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	9[17,27,28,42–47]
Studies excluded, limited design or execution quality	0
Qualifying studies	9[17,27,28,42–47]
Independent studies included in body of evidence[a]	3[17,42,45–47b]
Studies assessing nonrecommendation outcomes	2[27,28]
Nonindependent studies, not in body of evidence[a]	2[43,44]
Designs of included studies	
Time series with concurrent comparison group	4[42,45–47]
Time series, no concurrent comparison group	1[28]
Retrospective with concurrent comparison group	1[27]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	3[17,42,45–47b]
Aggravated assault	1[17]
Robbery	1[17]
Rape	1[17]
Suicide	3[17,42,47]
Unintentional firearm-related injury death	1[17]
Gun counts or proportions	2[27,28]

[a]Studies are described as "independent" if they do not assess the same intervention in the same population for a similar follow-up period. Among nonindependent studies, the one with the longest follow-up or the best design or execution is chosen to represent this intervention experience.
[b]Three studies[42,46,47] are nonindependent, with no clear superiority of one study over the others in design or execution. All assessed the Washington DC handgun ban; each used a different control population.

Bans commonly exempt firearms in the banned category owned prior to implementation of the ban (i.e., they "grandfather" those weapons), although such bans may require the registration of grandfathered firearms. Grandfathering is a critical element in bans insofar as it could allow large stocks of the banned items to remain available after the ban goes into effect.

Review of evidence: effectiveness. Our search identified nine studies on the effects of bans on violent outcomes or on the use of the banned firearms.[17,27,28,42–47] Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 2. More detailed information on the studies used in this review are provided at the website (*www.thecommunityguide.org/violence*); Appendix A, which shows evidence used in the review of the effects of bans, is an example of the detailed tables for all firearms law evidence reviews available on the website.

Among the seven studies that evaluated violent outcomes, one[17] was of least suitable design; all seven studies had fair execution quality. Five studies[42–44,46,47] evaluated the 1976 Washington DC handgun ban. Two of these were not considered because they assessed follow-up periods that were relatively short (2 years) compared with the remaining studies of the DC ban.

Because the three remaining studies[42,46,47] (two[42,47] conducted by the same team of researchers) assessed the effects of the DC handgun ban on homicide during a similar time period, they were counted as nonindependent and as one study experience. They reached inconsistent conclusions about the effects of the law on homicide, principally because of methodologic differences and differences in comparison populations. Two found a decrease in homicide in Washington DC compared with surrounding regions,[42] and with Memphis and Philadelphia,[47] cities of comparable size. The third[46] found increases in homicide rates in Washington DC compared with Baltimore, a city with comparable crime rates. Because of the limitations of all the studies and inconsistent results and conclusions, and because there was no best study, we concluded that the evidence was insufficient to determine the effectiveness of the Washington DC handgun ban on reducing homicide.

Two studies of the Washington DC handgun ban[42,47] found a decrease in suicide, compared with control regions without a similar ban. These results, however, were inconsistent with the other study of the effect of bans on suicide,[17] which found increases as well as decreases in suicides associated with several types of bans.

One study examined the effects on homicide rates of the 1994 Federal Violent Crime Control Act that banned assault weapons and large-capacity ammunition magazines. Comparing states with bans similar to but enacted before the federal ban with states with no such ban, the study found a relative decline in homicide rates in states without a prior ban, suggesting a benefit associated with the new ban.[45] A study of least suitable (i.e., cross-sectional) design[17] assessed the effects of handgun possession, handgun sales, and bans of sales of Saturday night specials on homicide, aggravated assault, robbery, rape, fatal unintentional firearm-related injury, and suicide in the 170 U.S. cities with populations $>$100,000 in 1980, and found no consistent results.

Two studies evaluated Maryland laws—a 1988 law banning manufacture and sale of Saturday night specials,[27] and a 1994 law banning sales of assault pistols.[28] These studies evaluated outcomes not directly related to health, such as proportions of banned firearms among all recovered crime firearms, or counts of recovered banned firearms used in crime. They indicated reductions in banned firearms, either in comparison with firearms used prior to the ban[28] or with other cities without such a ban.[27] Because the decrease in the number of banned firearms exceeded the increase in the number of additional nonbanned firearms, there was a net reduction in firearm retrievals overall.[27]

Overall, the number of independent studies was small (three) and available evidence on violent outcomes was inconsistent. One study of greatest design suitability found a decrease in homicide,[45] while other nonindependent studies[42,46,47]—also of greatest design suitability—showed inconsistent findings. A study with a least suitable design[17] also found mixed effects for multiple outcomes. Additional evidence suggested that banned firearms are about half as likely to be used in crimes after the ban, compared with before the ban period or with areas where the same firearms are not banned.[27,28]

Other effects. In the period immediately preceding initiation of a ban, the production and sales of firearms about to be banned can increase dramatically.[45] Banning cheap firearms has been asserted[48] to decrease the capacity for self-protection among people in economically disadvantaged populations, who are also more likely to reside in high-crime neighborhoods. There is, however, no evidence for or against this hypothesis.

Conclusion. According to *Community Guide* criteria,[22] available evidence is insufficient to determine the effectiveness or ineffectiveness on violent outcomes of banning the acquisition and possession of firearms. The number of available studies was small, some available studies were limited in their design and execution, and results were inconsistent. Further research is needed to evaluate the effects of bans of specified weapons or ammunition on violence and related health and social outcomes.

Acquisition Restrictions

State governments and the federal government have made concerted efforts to deny the purchase of firearms to people with specified characteristics thought to indicate high risk for illegal or other harmful use of firearms. Restriction characteristics include criminal histories (e.g., felony conviction or indictment, domestic violence restraining order, fugitive of justice, or conviction on drug charges); personal histories (e.g., people adjudicated as "mental defective," illegal immigrants, those with a dishonorable military discharge); and other characteristics (e.g., juveniles). (The term "mental defective" is a determination by a lawful authority that a person, as a result of marked subnormal intelligence or mental illness, is a danger to self or others, or lacks the mental capacity to manage his or her own affairs. The term also includes a court finding of insanity in a criminal case, incompetence to stand trial, or not guilty by reason of lack of mental responsibility.[49])

The federal Interim Brady Handgun Violence Prevention Act (P.L. 103-159), hereafter Interim Brady Law, was implemented in March 1994 to strengthen the Gun Control Act of 1968 (P.L. 90-618) and to require the active investigation of the backgrounds of people applying to purchase handguns. Applications can be rejected if the applicant's background is found to include a felony indictment or conviction, domestic violence restraining order, unlawful use of or addiction to drugs, or dishonorable discharge, or if the applicant is a fugitive from justice or an illegal alien or has been adjudicated a "mental defective." The Interim Brady Law required a 5-day waiting period to allow the background investigation. (Evidence about the Interim Brady Law is included in the review of the effects of waiting periods.) The interim law was to be replaced by a permanent law following implementation of the National Instant Background Check System in 1998. The Lautenberg amendment (P.L. 104-208) of 1996 added a restriction that prohibits the sale of firearms to those convicted of a domestic violence misdemeanor. In 1997, in *Printz v. United States* (521 U.S. 98, 117 S.Ct. 2365 (1997)), the U.S. Supreme Court ruled that states could not be required to conduct background checks for the Interim Brady Law; for states that chose not to conduct background checks, the FBI had to conduct the checks.

The Permanent Brady Act (November 1998, P.L. 103-159), subsequently referred to as the Brady Law, required instant background checks for all firearms purchases, not only handguns. It eliminated the 5-day waiting period, but required firearms dealers to wait a maximum of 3 days to allow the location of required records, after which, if no prohibitory information had been identified, the purchase could proceed. Some states have restrictions in addition to the federal ones, and some states had such laws preceding the Interim Brady Law.[50,51]

Studies by the federal government[52,53] indicate difficulties in the instant background check system, primarily because of a lack of records on many restriction categories (e.g., on individuals adjudicated "mental defective," with a history of drug addiction, or with illegal immigrant status) or because criminal records are difficult and sometimes impossible to retrieve. The Bureau of Justice Statistics reports[54] that in 1999, of an estimated 59 million criminal history records available to states, 89.4% were automated. However, only a median of 69% of state records systems had the records of conviction status required to assess firearms restrictions. The investigation of individual applicant criminal histories may thus require the search of paper files—a time-consuming, costly, and not always successful activity, especially within the 3 days allowed.[55] Notable improvements in the background check system have been made,[56] but the system is still incomplete and lacks the records needed to be fully effective.

The Brady Law has prevented some prohibited people from purchasing firearms at the point of applica-

Table 3. Legal restrictions on gun acquisition: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	4[17,29,50,59]
Studies excluded, limited design or execution quality	0
Qualifying studies	4[17,29,50,59]
Independent studies included in body of evidence[a]	4[17,29,50,59]
Nonindependent studies, not in body of evidence[a]	0
Designs of included studies	
Prospective with concurrent comparison group	2[29,50,59]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	2[17,50]
Aggravated assault	1[17]
Robbery	1[17]
Rape	1[17]
Violent crime	2[29,59]
Suicide	2[17,50]
Unintentional firearm-related injury death	1[17]

[a]Studies are described as "independent" if they differ by intervention, population, or follow-up period.

tion for purchase. A review conducted in 1999[57] indicated that of 12.7 million handgun purchase applications (approximately 2.8 million per year) made during the period of the Interim Brady Law, 312,000 (2.4%) had been rejected—63.3% of those because of a felony conviction, 13.3% because of a domestic violence misdemeanor conviction or restraining order, 6.6% because of state-specific prohibitions, 6.1% because the applicant was a fugitive from justice, and 8.3% for other reasons. During the first year of the Permanent Brady Law, there were 8.8 million background checks, 2% resulting in denial; 17% of denials were appealed, of which 22% were reversed.[58] During the same period, 2230 fugitives of the law were identified, and 3353 prohibited people were found to have been erroneously permitted to acquire firearms.

Review of evidence: effectiveness. Our search identified four studies on the effects of acquisition restrictions on violent outcomes.[17,29,50,59] One additional study[60] examined only the waiting period component of the Brady Law (see review of waiting periods, below). Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 3. Details of the four independent qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

Two studies[50,59] examined the effects of restrictions based on prior felony conviction; one[59] assessed overall violent crime as an outcome, and the second[50] assessed homicide and suicide. One of these studies[50] examined the effect of the Interim Brady Law as a whole. Because felony convictions constitute the exclusion factor for the largest proportion of those excluded by the law, we assessed this study as evaluating the felony conviction restriction, and note that evaluation studies often assess several intervention components at once. One study[29] examined the effect of restrictions based on misdemeanor convictions on violent crime overall. Another study[17] examined the effect of "mental defective" status, drug abuse, alcohol, and (unspecified) age restrictions against minors on specific violent crimes, suicide, and unintentional injury. The studies of felony conviction restrictions[50,59] were of greatest design suitability and fair execution; the study of misdemeanor restrictions[29] was of greatest design suitability and good execution; and the study of "mental defective" status, drug abuse, alcohol, and age restrictions[17] was of least suitable design and fair execution.

One study[59] evaluated the effect of felony conviction restrictions in California, and concluded that subsequent arrest for violent crime among restricted felons was 19.4% lower (95% confidence interval [CI]=9.9%, 28.1%) than would have been expected had these felons been allowed to purchase firearms. The second study of felony conviction restrictions[50] indicated statistically nonsignificant declines for firearm-related homicide and suicide and total homicide and suicide in the U.S. population aged ≥21 years, and a statistically significant decline in firearm-related suicide deaths among people aged ≥55 years. However, by comparing outcomes in states that had a waiting period prior to the Brady Law with states that did not previously have a waiting period, this study showed that this reduction was attributable not to the felony restriction per se, but to the waiting period component of the Interim Brady Law.

A single study[29] indicated that a misdemeanor conviction restriction reduces the rate of first arrest for violent crime by 19.4% and arrests over a 3-year period for firearm or violent crime by 10.7%; however, neither result is statistically significant, and the single study is thus not sufficient to draw a conclusion about effectiveness, because it is not clear that either finding differs from no change.

One study[17] examined four personal history restrictions (i.e., "mental defective," drug abuse, alcohol, and minor age) and their associations with homicide, aggravated assault, robbery, rape, suicide, and unintentional injury. This cross-sectional study had 10 effects in the desired direction and 14 in the undesired direction, 2 of them statistically significant. Overall, evidence of consistent effect by restriction or outcome is limited, because of small numbers of studies of each outcome and inconsistent directions of effect.

One study[50] allowed assessment of the substitution effect (i.e., because the restriction or a waiting period makes firearms unavailable, people substitute other means to harm others or commit suicide). The researchers found evidence of a substitution effect for suicide, but not for homicide; however, the suicide substitution effect is relatively minor: an increase of 3.0% in non-gun suicide, compared with the firearm-specific suicide decline of 8.6%.

Other effects. Restrictions may facilitate the identification and capture of wanted persons.[56] Background checks may also act as a deterrent to application by people prohibited from purchasing weapons. However, we found no evidence of this or of whether denied applicants subsequently acquired firearms by other means (e.g., from the secondary market). One potential harm is false positives, that is, people falsely reported as having a restriction, who may subsequently be stigmatized and mistakenly denied a firearm.

Conclusion. According to the *Community Guide* criteria,[22] the available evidence is insufficient to determine the effect of firearms acquisition restrictions on public health and criminal violence, because of a small number of available studies, limitations in their design and execution, and variability in the direction and statistical significance of findings. The only restriction for which study design suitability and execution met our criteria was the misdemeanor conviction restriction; in this instance, the effect was in the expected direction, but was not statistically significant, and we were thus unable to draw a conclusion. Further research is needed to evaluate the effects of acquisition restriction laws on violence, other health-related outcomes, and related health and social effects.

Waiting Periods for Firearms Acquisition

Waiting periods for firearms acquisition require a specified delay between application for and acquisition of a firearm. This requirement is usually imposed to allow time to check the applicant's background or to provide a "cooling-off" period for people at risk of committing an impulsive crime or suicide. In addition to background checks, waiting periods can be combined with other provisions, such as a requirement for safety training.

The Interim Brady Handgun Violence Prevention Act, a federal law that went into effect in 1994, mandated a background check and a 5-day waiting period for handgun purchasers. In 1998, the 5-day waiting period required by the Interim Brady Law expired, and was replaced by a mandatory, computerized National Instant Criminal Background Check System (required not only for handguns, but for all firearms purchases), allowing dealers to sell the firearm if the FBI reported no adverse evidence to the dealer within 3 days of application. However, many states have their own provisions mandating longer waiting periods for handgun or long firearm purchases or both. Reports on the number of states with waiting periods for handgun purchases vary from 10 (National Rifle Association website: *www.nra.org*) to 15[61] to 19[62], with waiting periods ranging from 2 days (in Alabama, Nebraska, South Dakota, and Wisconsin) to 6 months (in New York).[61]

Table 4. Waiting periods for firearm acquisition: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	7[17,50,63–67]
Studies excluded, limited design or execution quality	0
Qualifying studies	7[17,50,63–67]
Designs of included studies	
Time series with concurrent comparison group	2[50,64]
Before and after, no concurrent comparison group	1[63]
Cross-sectional	4[17,65–67]
Outcomes reported in included studies	
Homicide	6[17,50,64–67]
Aggravated assault	5[17,64–67]
Robbery	5[17,64–67]
Rape	2[17,64]
Suicide	6[17,63–67]
Unintentional firearm-related injury death	3[17,64,66]

Review of evidence: effectiveness. Our search identified seven studies on the effects of waiting periods on violent outcomes.[17,50,63–67] Descriptive information about execution, design suitability, and outcomes evaluated in these studies is provided in Table 4. Details of the seven independent qualifying studies are available at the website (*www.thecommunityguide.org/violence*). One study[63] was conducted in Queensland, Australia; the remaining studies were conducted in the United States.

Among the seven qualifying studies, five[17,63,65–67] were of lowest design suitability, and two[50,64] of greatest design suitability; all seven studies had fair execution. One study[64] presented the effectiveness results as a mathematical function of the length of waiting period; for purposes of this review, we calculated an effect estimate for a 5-day waiting period (as required by the Interim Brady Law).

Of six studies that evaluated the effects of waiting periods on homicide, four[17,65–67] had least suitable designs. Results were mixed: three point estimates showed a reduction in homicide, two showed an increase (one study with results for 2 decades, the 1960s and 1970s), and none of these findings were statistically significant. Two studies[66,67] found that results were not statistically significant without providing either size or direction of the effect.

Six studies evaluated effects of waiting periods on suicide. One study[63] evaluated the effect of waiting periods for long firearm purchase, one[50] for handgun purchase (under the Interim Brady Law 5-day waiting period), and four[17,64,66,67] for both long firearm and handgun purchases. Two[17,63] studies presented data that allowed the calculation of relative percentage change in suicide rates; one[17] found a small (0.5%) increase and one[63] a small (2.9%) decrease in total suicides. Two[50,64] studies reported only absolute changes in suicide rates without data on baseline rates, which did not allow calculation of relative percent change. One study reported decreases in firearm suicide rates among children (aged 0 to 14 years) and adolescents[64] (aged 15 to 19 years), and the second study reported a decrease in both firearm-related and total suicide rates among adults (aged ≥21 years).[50] However the second study's decrease was statistically significant only in a subsample of people aged ≥55 years, and only for firearm-related suicide.[50] Two studies[66,67] reported that results were not significant, without providing either size or direction of the effect.

Evidence of the law's effects on aggravated assault, robbery, rape, and unintentional firearm-related injury death were inconsistent in direction, with six of the effect estimates indicating an increase, five indicating a decrease, and none being statistically significant.

Comparison of the effect on suicide of a 28-day waiting period for long firearms (in Queensland, Australia)[63] with a 5-day waiting period for handguns (associated with the Interim Brady Law)[50] indicated a greater effect associated with the longer waiting period for firearm-related suicide, but not for total suicide.

Several studies,[17,50,63] for which both firearm and non-firearm effect estimates were available, suggested the presence of a partial substitution effect for suicide, in which decreases in firearm-related suicide are offset, but at substantially lower levels, by increases in non-gun suicide. No such substitution effects were found for homicide, aggravated assault, or robbery.

Other effects. It has also been asserted[60] that waiting periods may give criminals (who may be more likely to acquire firearms by illegal means and avoid the waiting period) an advantage in obtaining firearms over law-abiding citizens (who may lack means of self-defense during the waiting period). However, there is no evidence for or against this hypothesis. One study[64] reported inconsistent effects of waiting periods on property crime; it found an increase in burglary and a decrease in larceny and auto theft.

Conclusion. According to the *Community Guide* criteria,[22] the evidence is insufficient to determine the effectiveness of waiting periods for the prevention of suicide, homicide, aggravated assault, robbery, rape, and unintentional firearm-related injury death, because of the small number of available studies, limitations in the design and execution of available studies, and effects that are inconsistent in direction or fail to reach statistical significance. Further research is needed to evaluate the effects of waiting period laws on violence, other health-related outcomes, and associated health and social effects.

Firearms Registration and Licensing of Firearm Owners

Registration requires that a record of the owners of specified firearms be created and retained.[68] Licensing requires an individual to obtain a license or other form of authorization or certification that allows the purchase or possession of a firearm.[68] Licensing and registration requirements are often combined with other firearms regulations, such as safety training or safe storage requirements.

The registration practices of states and the federal government vary widely.[69] Recorded information may be retained by a specified recorder, such as by federal firearms licensees; such records may be accessible under specified circumstances, such as criminal investigations. In some states, recorded information is kept in centralized registries. The Firearm Ownership Protection Act of 1986 specifically precludes the federal government from establishing and maintaining a national registry of firearms and their owners. Likewise, there are no current federal firearms licensing requirements or provisions for individual purchasers. However, several states have laws that require the licensing of firearm owners or registration of firearms, and recorded information is kept in centralized registries. For example, licensing of handgun owners is required in 17 states and the District of Columbia.[6] Statewide handgun registration laws currently exist in four states. Licensing and registration may serve as instruments for the control of illegal firearms ownership, transfer, and use,[56,70] and might also deter illegal acquisition and use.

Review of evidence: effectiveness. Our search identified five studies[17,65–67,71] on the effects of licensing on violent outcomes, two[17,71] of which also report on the effects of registration. One study[17] was based on data collected in 1979 to 1981, one[65] on data collected in the 1960s and 1970s, one[66] on data collected in 1978, and one[67] on data collected in 1969–1970; one[71] assessed firearms retrieved from crimes during a 1-year period (1997–1998). All five studies were of least suitable (cross-sectional) design and had fair execution. Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 5, and at the *Community Guide* website (*www.thecommunityguide.org/violence*). Details of the four independent qualifying studies are also available at the website.

Evidence of the effects of licensing and registration on diverse study outcomes was inconsistent, with eight of the effect estimates showing increases in violence, and eight showing decreases. (One study had data on three outcomes each for 1960 and 1970.) Two studies[66,67] reported that results were statistically nonsignif-

Table 5. Firearm registration and owner licensing: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	5[17,65–67,71]
Studies excluded, limited design or execution quality	0
Qualifying studies	5[17,65–67,71]
Studies used as secondary evidence[a]	1[71]
Designs of included studies	
Cross-sectional	5[17,65–67,71]
Outcomes reported in included studies	
Homicide	4[17,65–67]
Aggravated assault	4[17,65–67]
Robbery	4[17,65–67]
Rape	1[66]
Suicide	4[17,65–67]
Unintentional firearm-related injury death	2[17,66]
Gun counts or proportions	1[71]

[a]Secondary evidence does not directly measure a violent outcome, but may be suggestive of an effect.

icant without providing either size or direction of the effect.

One study[71] assessed recovered firearms that had been used in crimes in states with and without licensing and registration laws. We counted this study as secondary evidence because it provided neither a direct measure of violent outcomes or evidence that the use of recovered firearms is a good proxy measure of crime. This study reported that crime firearms purchase in-state was 48.5% lower in cities that had both licensing and registration requirements, compared with cities that had neither.

Other effects. Potential benefits that have been associated with the licensing of firearm owners and the registration of firearms include increased ability to enforce firearms laws, tracing sources of illegally possessed or used firearms, and data for research on the etiology of harmful and illegal firearms uses.[70,72] Potential harms that have been associated with licensing and registration are the perceived threat to the privacy and rights of owners.[73]

Conclusion. According to the *Community Guide* criteria,[22] the evidence on licensing and registration is insufficient to determine their effectiveness in reducing violence. Only a few studies were available, there were limitations in the studies' design and execution, and results were inconsistent. Further research is needed to evaluate the effects of licensing and registration laws on violence, other health-related outcomes, and associated health and social effects.

Shall Issue Concealed Weapons Carry Laws

Shall issue concealed-weapons carry laws (shall issue laws) require authorities to issue permits to carry concealed weapons to all applicants who are not found to have specified characteristics that disqualify them. In contrast, some states have adopted "may issue" laws, in which the issuing authority has the discretion to issue or deny a firearms permit based on criteria such as the perceived need or moral character of the applicant, and other states prohibit all carrying of concealed weapons (as of 2001, six states had such a prohibition).[6] Disqualification criteria in shall issue laws vary by state, but generally include, among others, prior felony conviction, conviction on a drug charge in the past 3 years, commitment to a mental hospital in the past 5 years, fugitive from justice, or age below a specified minimum. States also differ substantially in requirements such as firearms safety training, permit fees, and specifying places where firearms may not be carried.[11]

Before 1977, only eight states had shall issue laws, compared with 31 states as of 2000.[62] Researchers disagree on which states adopted shall issue laws and when.[74–76] For example, several studies consider Virginia to have had a shall issue law in 1988.[74,75,77–80] However, although the Virginia law at that time included the phrase "shall issue," the law also required demonstration of the applicant's need and "good character"—both characteristics of the more discretionary "may issue" laws. Differential classification of the laws may affect analyses of their effects.

Two principal hypotheses, which are not mutually exclusive, have been proposed to predict the consequences of shall issue laws. Some analysts have reasoned that, because the law allows for self-defense, potential criminals may be deterred by fear that a possible victim could be armed.[60] If so, publicity about the law and the perception on the part of potential criminals that individuals could be carrying concealed firearms is likely to be more important in reducing violence than the actual numbers of firearms carried. Others have reasoned that the presence of more firearms increases rates of unintended and intended injury in interpersonal confrontations, and, in addition, leads potential criminals to carry and use more lethal firearms more often.[81] If this is so, the actual number of additional firearms carried is important. In the only available survey on the attitudes of (imprisoned) felons, Wright and Rossi[82,83] report that felons claim to be deterred from committing a crime if they think that potential victims might be armed, but also carry firearms themselves to deter violence by victims. This finding suggests that shall issue laws may have contrary effects on firearms behavior—both deterring and escalating firearms carrying in the criminal population—with unknown net effect.

Review of evidence: effectiveness. Our search identified 12 studies[17,30,60,75,77–80,84–87] on the effects of shall issue laws on violent outcomes. Descriptive information about the quality, study design, and outcome measures from these studies is provided in Table 6. Details of all

Table 6. "Shall issue" carry laws: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	12[17,30,60,75,77–80,84–87]
Studies excluded, limited design and execution quality	0
Studies excluded, limited data quality[a]	8[60,75,77–80,84,85]
Qualifying papers	4[17,30,86,87]
Studies included in body of evidence	4[17,30,86,87]
Designs of included studies	
Time series with concurrent comparison group	2[30,87]
Time series, no concurrent comparison group	1[86]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	3[17,30,86]
Homicide of the police (i.e., police as homicide victims)	1[87]
Aggravated assault	2[17,30]
Robbery	2[17,30]
Rape	2[17,30]

[a]Because county-level crime data have been shown to be highly unreliable,[33] and because they have not been consistently used correctly, we excluded studies based on these data regardless of other design or execution qualities.

qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

In 1997, Lott and Mustard[74] published an analysis of the effects of shall issue laws based on a large data set and spanning a 17-year period. They tested multiple hypotheses about the effects of shall issue laws on diverse outcomes, including violent crimes, property crimes, unintentional injury deaths, and suicide.[60,64,88] Because crime rates vary considerably among counties, Lott and Mustard[74] focused their analysis on U.S. county-level rather than state-level data. Five additional studies[75,78–80,85] used Lott and Mustard's data[74] or independently derived county-level data[77] as the basis for their own analyses. However, county-level crime data are highly problematic.

At the county level, missing data and under-reporting are prevalent. Concerns have been raised about the procedures for extrapolating to estimate the extensive missing county-level data.[33] Lott and Mustard[74] and those who used these authors' data did not adjust for missing information by using population denominator data that corresponded to crime numerator data. Thus, Lott and Mustard's denominator numbers were often too high, leading to underestimated crime rates in regions with poor reporting.[33] For example, less populous regions may have lower rates of crime as well as less complete reporting; comparisons by region would then be biased. Finally, these county-level studies may have misclassified as many as three out of ten reviewed states as shall issue jurisdictions.[30] The relationships among available studies of shall issue laws by data source and unit of analysis (Figure 3)) indicate that most studies of these laws suffer from basic data problems associated with county-level information. Because of these critical concerns about the accuracy of county-level crime data for research purposes,[33] we did not use data from any of the county-level studies in our assessment of the effects of shall issue laws on violence.

The four qualifying studies of shall issue laws include one study[30] that examined national level effects on homicide using Vital Statistics reports (from the CDC's National Center for Health Statistics), one study[17] that used both Vital Statistics and UCR data to examine the effects of shall issue and other firearms laws on multiple violent outcomes, one study[86] that used Vital Statistics to assess the effects of shall issue laws in five selected counties, and one study[87] that used state-level UCR data to assess the effects of shall issue laws on homicides of police (homicides in which police are the victims). Thus, three qualifying studies assessed homicide as an outcome; of these one assessed homicide of police officers, and another multiple violent outcomes. Two of these studies are of greatest design suitability, one each of moderate and least suitable design, and all had fair execution. In contrast to county-level data from the UCR, county-level mortality Vital Statistics data are essentially complete.[89]

Two studies[17,30] suggested a reduction in homicide associated with shall issue laws at the national level, and the third[86] suggested mixed effects in five counties, with an overall increase in homicide associated with the laws. The study of police homicide[87] shows a small, statistically nonsignificant decline in the homicide of police associated with shall issue laws. Homicides of police occur at a rate of <100 per year, accounting for 0.6% of all U.S. homicides.

Conclusion. According to *Community Guide* criteria,[22] the small number of qualifying studies that evaluate the effects of shall issue laws on homicide, aggravated assault, robbery, rape, and homicide of police is not sufficient to determine the effectiveness of these laws in reducing the rate of these crimes. We have not included data from studies based on county-level evidence in our assessment, because county-level data have important systematic flaws that preclude reliable conclusions. Further research is needed to assess the effects of shall issue laws on violence.

Child Access Prevention Laws

Child access prevention (CAP) laws are designed to limit children's access to and use of firearms; states vary in the ages of children covered by the laws, from <14 to <18 years. The laws require firearm owners to store their firearms locked, unloaded, or both. In some states, firearm owners are liable when firearms are improperly stored or when a child uses the owner's

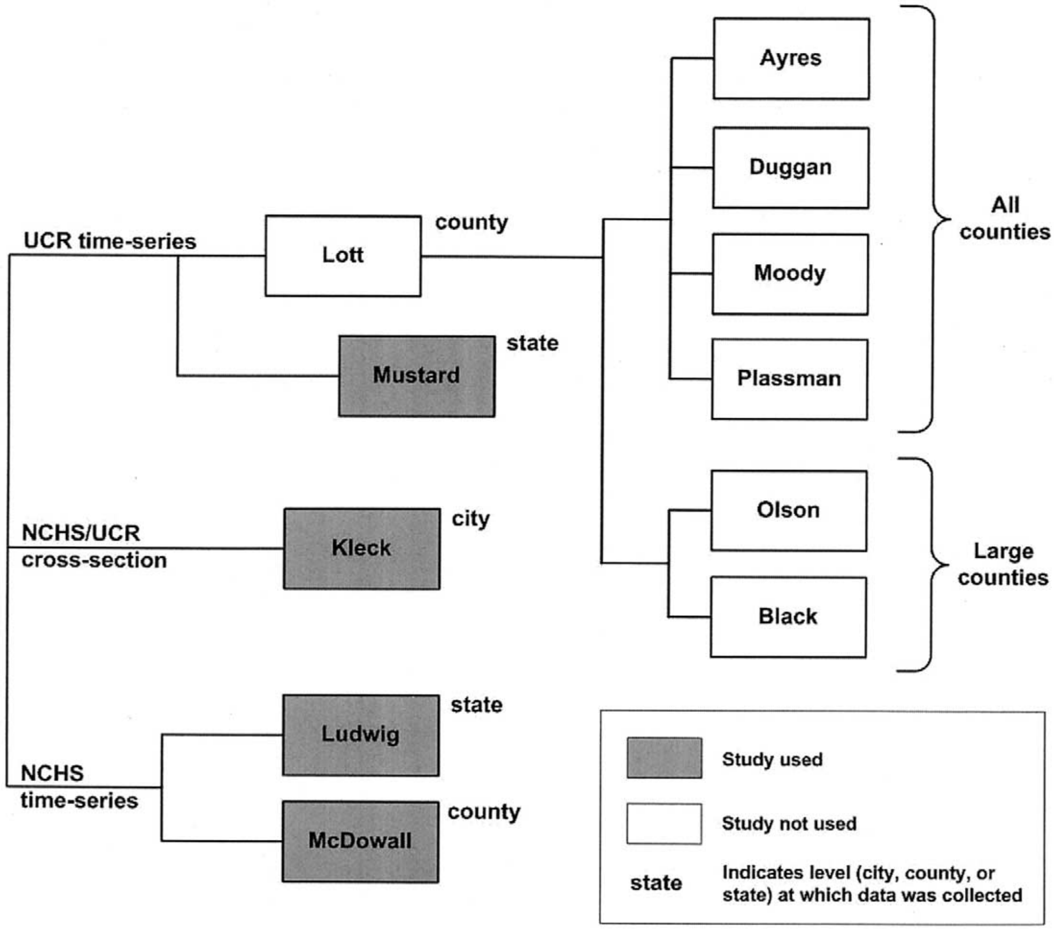

Figure 3. Sources of data and designs in studies of "shall issue" laws. NCHS, National Center for Health Statistics; UCR, Uniform Crime Report.

improperly stored firearm to threaten or harm him- or her-self or another person.

Laws aimed at preventing child access are a relatively recent development: Florida passed the first CAP law in 1989, and after the Columbine shootings in April 1999, two more states adopted CAP laws.[90] By 2000, a total of 16 states had adopted CAP laws.[62] In three states (FL, CT, CA), violating a CAP law is a felony; in the other states with CAP laws, it is a misdemeanor.

Review of evidence: effectiveness. Our search identified three studies[64,91,92] of the effect of CAP laws; all examined unintentional firearm-related injury deaths as outcomes, and one[92] examined firearm-related and non–firearm-related suicides and homicides. All three studies were of greatest design suitability and fair execution. On the untested assumption that locked and unloaded firearms may hinder rapid access to firearms for self-defense, one study[64] examined multiple outcomes, including violent crimes (i.e., homicide, aggravated assault, robbery, and rape) committed with and without firearms. All studies assessed outcomes among juveniles; one study[64] also examined effects for older age groups. Descriptive information about the quality, study design, and outcome measures from these studies is provided in Table 7. Details of the three qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

All the studies present a common challenge for purposes of analysis: The law is intended to reduce injuries

Table 7. Child access prevention laws: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	3[64,91,92]
Studies excluded, limited design and execution quality	0
Qualifying studies	3[64,91,92]
Designs of included studies	
Time series with concurrent comparison group	3[64,91,92]
Outcomes reported in included studies	
Homicide	2[64,92]
Aggravated assault	1[64]
Robbery	1[64]
Rape	1[64]
Suicide	2[64,92]
Unintentional firearm-related injury death	3[64,91,92]

caused by juveniles. The studies, however, assess juvenile **victims**, whose injuries (other than suicide) could be caused either by adults or by juveniles. As a result, the assessment of the effects of CAP laws on outcomes other than suicide may be biased. None of the studies assessed levels of publicity, awareness, or enforcement of CAP laws as mediators of their potential effects.

Two studies[64,91] examined effects of the same laws on unintentional firearm-related injury in the same populations in similar time periods. Of these, we chose the study[91] with the greatest suitability of design and execution scores to assess effects of the laws on unintentional firearm-related injury. However, we used the study with a lower execution score[64] to assess additional outcomes (i.e., homicide, assault, robbery, and rape).

An earlier study[92] indicated a reduction in unintentional firearm-related injury death among juveniles aged <15 years that was statistically significant in states providing a felony prosecution for CAP law violation, and a nonsignificant increase in unintentional firearm-related injury death among juveniles in states providing a misdemeanor prosecution. However, a later study,[91] including data from three additional states that had passed CAP laws and 3 more years of follow-up, confirms the earlier finding on states with misdemeanor prosecution, but shows that, among states with a felony prosecution, the effect of the law on unintentional firearm-related injury death among juveniles aged <15 years is statistically significant only in Florida (a state with a felony sanction) but not in the other two felony states.

One study[92] indicated a reduction associated with CAP laws in firearm-related suicide among juveniles aged <15 years. Data from studies of homicide, assault, robbery, and rape[64,92] indicate mixed results, with two findings indicating reductions (in firearm-related homicide among juveniles aged <15 years, and in assault among all ages), and three indicating increases (in total homicide, robbery, and rape in all ages) associated with CAP laws. Only the findings on robbery and rape are statistically significant. However, too few studies examine each outcome to determine the effect of the law on specific types of violence.

Other effects. One study[64] suggests that CAP laws may be associated with an increase of 2% in property crimes; the increase was statistically significant for burglary but not for property crime overall.

Conclusion. According to *Community Guide* criteria,[22] the small number of studies of CAP laws, all of limited quality of execution and inconsistent findings, is insufficient to determine the effectiveness of the laws in reducing violence or unintentional firearm-related injury and other violent outcomes. Further research with longer follow-up periods is needed to assess effects of CAP laws on violence, unintentional injury, and other outcomes of interest.

Zero Tolerance of Firearms in Schools

The Gun-Free Schools Act,[93] which affected 94% of schools in 1996–1997, stipulates that each state receiving federal funds under the Act must have a law requiring local education agencies to expel a student from school for ≥1 year if the student is found in possession of a firearm at school, although this expulsion requirement can be modified on a case-by-case basis. Expulsion may lead to alternative school placement or to "street" placement (full expulsion, with no formal education, for a specified length of time), after which students are generally allowed to return to their regular schools.

In the 1998–1999 school year, 3523 students were expelled for having a firearm in school. Of the total expelled, 44% were referred to alternative schools. (A national survey[94] indicated that, as of 1993, 66% of school districts reported implementing some type of alternative program to address school violence.) In 1996–1997, 4% of public schools reported having random, handheld metal detector checks on students, and in 1% of schools, students were required to pass through metal detectors every day.[95]

A national survey[96] indicates that approximately 3% of the 12th graders in 1997 (an estimated 80,190 students nationwide) reported carrying firearms on school property in the previous 4 weeks. According to these separate estimates, even if only seniors carry firearms, <4.4% of firearms (i.e., 3523/80,190) are being detected in association with the Gun-Free Schools Act. If students in lower grades also carry firearms (statistics are not available to determine this), the proportion of firearms being detected would be even lower.

The Gun-Free Schools Act does not require reporting on possible effects of its requirements on school safety conditions other than numbers of firearm-carrying students detected and expelled; however, reports from other sources indicate changes in some aspects of violence in the school environment. The carrying of weapons appears to have declined steadily during the 1990s, as did involvement in physical fights on school property.[97,98] However, the proportion of high school students who reported being threatened or injured with a weapon on school property in the past 12 months remained steady over this period, at 7% to 9%. The rate of serious violent crimes at or on the way to or from school peaked in 1994, and has declined from then until at least 2000.[98]

Review of evidence: effectiveness. No studies were located that attempted to evaluate the effects on school violence of zero tolerance of firearms in schools; nor did any study measure the specific effect of the Gun-Free Schools Act on firearm carrying in schools.

There was one study[99] of the effectiveness of metal detector programs in reducing the carrying of firearms in schools. Although firearms detection is not explicitly required in the Gun-Free Schools Act, the effectiveness of the law may depend on the ability to detect firearms. The study was a cross-sectional survey of New York school administrators and students to assess the association of metal detector programs with student behavior and attitudes. The metal detection program studied consisted of approximately weekly scanning of "randomly selected students" with a handheld device; the likelihood of detection was unclear. The study was of least suitable design and fair execution. Details about the quality, design, and outcome measure from this study are available at the website (*www.thecommunityguide.org/violence*).

The study compared rates or counts of firearms detection at schools with and without metal detection programs. Compared with schools without metal detection programs, schools with such programs had rates of carrying firearms to, from, or in school that were half as great (1.9% to 2.1% vs 4.0% to 4.6%), but did not differ in weapons carrying overall. Moreover, the study reported that schools did not differ in rates of threats or fights outside or inside of school. We could not determine the effectiveness of these programs because only a single study of least suitable design was available, and because the intermediate outcome of firearms carrying is not necessarily a good proxy for violence or injury.

Other effects. The effects of firearms detection programs in schools on students, school staff, or community are unknown; it is possible that such programs either reduce fear of harm or increase awareness, concern, and fear about the possibility of firearm-related violence. These effects may vary to the extent that a program is more or less effective in reducing firearms in schools.

A major, albeit unintended, harm of the Gun-Free Schools Act of 1994, particularly if firearms detection becomes more effective, is the "street" expulsion of thousands of students with low school achievement and high risk of violence. One review for the U.S. Department of Education[100] indicates that alternative schools for violent students may be effective as well as cost-effective in reducing violent behavior and enhancing emotional development for youth suspended or expelled from their usual schools; however, the review also notes that attendance at alternative schools may stigmatize students and increase discrimination against them. Even though the specific effect of firearm-related expulsion is not known, expulsion can result in a life course with fewer opportunities for (legal) employment, fewer resources, and a greater likelihood of criminal behavior and imprisonment compared with retention in special school programs.[101] The resulting lower productivity and increased criminal activity are likely to have high societal costs.[101]

Conclusion. It was not possible to assess the effectiveness of zero tolerance of firearms in schools because no studies of zero tolerance were identified and only a single study of least suitable design was identified that measured the effect of a school metal detector program on firearm-carrying behavior but not on violence per se. The effectiveness of such widespread policies in reducing violence and related health and social outcomes needs additional evaluation.

Combinations of Firearms Laws

Government jurisdictions (e.g., states or nations) differ in the degree to which they regulate firearms possession and use as well as in rates at which specific forms of violence occur (as is the case with the United States and Canada).[102] In our review, we considered whether these characteristics—degree of firearms regulation, and firearm-related and other forms of violent behavior—are causally associated. Causality is difficult to assess because levels of firearm-related violence and the degree of firearms regulation may each affect the other: high levels of firearm-related violence may lead to the increased regulation of firearms, and regulation may also lead to the reduction of violence. Moreover, these possibilities are not mutually exclusive. The interpretation of association is thus difficult and depends on temporal sequence, which cannot be determined in simple cross-sectional studies. An additional challenge to establishing a causal link may be the lack of comparable information from nations about laws, violent outcomes, and possible confounders of the association between them.

Review of evidence: effectiveness. We reviewed three forms of evidence: studies of the effects of comprehensive national laws within nations; cross-national studies of firearms law systems; and studies in which law types within jurisdictions (i.e., regulation of specific, defined aspects of firearms acquisition and use) are categorized and counted, and the counts correlated with rates of specific forms of violence within the same jurisdictions. We refer to these last as "index studies" because they develop indices of regulation based on the kinds and numbers of firearms laws found in different jurisdictions. We considered the three kinds of evidence together in drawing conclusions. Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Tables 8, 9, and 10. Details of the studies that met inclusion criteria are available at the website (*www.thecommunityguide.org/violence*).

We considered available studies of two comprehensive national laws, the Gun Control Act of 1968 (P.L. 90-618) in the United States and the Criminal Law Amendment Act of 1977 in Canada. Our search identified two studies[65,113] of the U.S. law that assessed violent outcomes and ten studies[103–112] of the Canadian

Table 8. Combinations of laws: Gun Control Act of 1986 (United States) and Firearms Control Legislation of 1977 (Canada)

	Studies (*n*)
Studies meeting inclusion criteria	12[65,103–113]
Studies excluded, limited design and execution quality	0
Qualifying papers	12[103]
Studies included in body of evidence	2[65,109]
Nonindependent studies, not in body of evidence[a]	10[103–108,110–113]
Designs of included studies	
Before-after, no concurrent comparison group	1[65]
Time series, no concurrent comparison group	1[109]
Outcomes reported in included studies	
Homicide	2[65,109]
Suicide	1[109]
Unintentional firearm-related injury death	1[109]

[a]Nonindependent studies are not included in the body of evidence because they assess the same intervention in the same population for the same (or a shorter) follow-up period, are not as well designed or executed as an included study, or both.

Table 10. Combinations of laws: firearm law index studies

	Studies (*n*)
Studies meeting inclusion criteria	8[17,65–67,117–120]
Studies excluded, limited design and execution quality	0
Qualifying papers	8[17,65–67,117–120]
Studies included in body of evidence	6[17,65,66,117,118,120]
Papers excluded, nonindependent[a]	2[67,119]
Study designs	
Cross-sectional	6[17,65–67,117,118,120]
Outcomes reported	
Homicide	4[17,65,66,118]
Suicide	5[17,66,117,118,120]
Unintentional firearm-related injury death	3[17,66,118]
Aggravated assault	4[17,65,66,118]
Robbery	4[17,65,66,118]
Rape	1[17]

[a]Nonindependent studies are not included in the body of evidence because they assess the same intervention in the same population for the same (or a shorter) follow-up period, are not as well designed or executed as an included study, or both.

law that assessed violent outcomes (Table 8). Because the studies of each law were not independent, we chose the study with the greatest design and execution scores to represent the effects of the U.S. law[65] and one Canadian study[109] to represent the effects of the Canadian law (on rates of homicide, suicide, and unintentional firearm-related deaths). The U.S. study was of least suitable design and fair execution; the Canadian study was of moderate design suitability and fair execution. The study of the Gun Control Act of 1968 yielded two nonsignificant results in opposing directions (i.e., an increase in homicide, adjusted for new firearms, and a decrease in homicide, adjusted for the total firearms stock). The study of the comprehensive Canadian firearms law indicated decreased rates of homicide, but increased rates of firearm-related suicide.

In the cross-national studies of comprehensive laws, the effects of more and less comprehensive firearms regulations on violence were assessed by comparing regions within two nations, the United States and Canada. Our search identified three such studies (Table 9).[114–116] All three studies were of least suitable design and fair execution. Because the two Canadian–U.S. comparisons of homicide assessed largely distinct populations in different time periods—1976 to 1980[114] and 1980 to 1986[115]—these two studies were regarded as independent.

Table 9. Combinations of laws: international comparative studies (United States and Canada)

	Studies (*n*)
Studies meeting inclusion criteria	3[114–116]
Studies excluded, limited design and execution quality	0
Qualifying papers	3[114–116]
Study designs	
Cross-sectional	3[114–116]
Outcomes reported	
Homicide	2[114,115]
Aggravated assault	2[114,115]
Robbery	1[115]
Suicide	1[116]

One study[115] comparing Seattle with Vancouver found an inverse association between the degree of firearms regulation in these cities and their rates of firearm-related aggravated assault (relative risk 7.7, 95% CI=6.7, 8.7) and homicide (relative risk 5.1, 95% CI=3.5, 7.3), but not of other forms of interpersonal violence. A second study in the same setting[116] found a similar inverse association of the degree of firearms regulation and firearm-related suicide, counterbalanced by an opposing difference in other forms of suicide; that is, the degree of regulation was associated with lower rates of firearm-related suicide and higher rates of other forms of suicide. The third study[114] compared U.S. and Canadian border states and provinces, respectively, and indicated no association between national levels of firearms regulation and rates of homicide; no summary statistic was reported.

The index studies compared degrees of firearms regulation and violent outcomes among U.S. states and cities. We found eight index studies,[17,65–67,117–120] of which all but two[67,119] qualified for analysis (Table 10). Several qualifying studies include separate analyses of data from different years; thus, separate findings from a single study (e.g., from 1960 and 1979 in the study by Maggadino and Medoff[65]) are included in our analysis, insofar as the study is independent from other studies.

One qualifying study[66] noted only the lack of statistically significant differences between levels of violence associated with the degree of regulation, without indicating the quantity or even the direction of difference. All six qualifying studies were of least suitable design and fair execution.

Index studies yielded heterogeneous results. Of six findings on homicide from three studies,[17,65,118] one indicated a statistically significant increase and five indicated decreases, two of which are statistically significant. One study with rape as an outcome indicated a statistically nonsignificant decrease. Three studies with aggravated assault, robbery, and unintentional firearm-related injury death as outcomes had inconsistent findings, some indicating an increase in the outcome associated with greater regulation, and others a decrease. Only for suicide did all index studies show a reduction associated with a greater amount of regulation; two of five results were statistically significant. Overall, index studies were found to have inconsistent results on violent outcomes.

Other effects. High levels of regulation may be seen as an infringement on individual rights.

Conclusion. Based on findings from national law assessments, cross-national comparisons, and index studies, evidence is insufficient to determine whether the degree or intensity of firearms regulation is associated with decreased (or increased) violence. Current evidence is inconsistent and, in general, methodologically inadequate, based on Task Force standards, to draw conclusions about causal effects. Moreover, even if findings were clear, the design of index studies conducted to date would not allow us to specify which firearms laws did or did not contribute to the reduction of violence. Additional research is needed to determine the relationship(s) between specific types and degree of firearms regulation and the rates of specified types of violence in given jurisdictions.

Results: Part II—Research Issues for Firearms Laws

Review of eight firearms laws and law types found insufficient evidence to determine whether the laws reviewed reduce (or increase) violence. Additional high-quality research is required to determine whether a relationship exists between firearms laws and violent outcomes. Areas for further potential study are discussed below.

General Research Issues

1. Violent outcome data sources

It was noted at the outset of this article and in the assessments of specific laws that multiple problems exist with the available data on outcomes used in studies of firearms laws. Much remains to be done to improve the recording of events and accessibility of the relevant data. Improvements would allow better evaluation of the effects of firearms laws as well as improvements in understanding of other aspects of violence and injury. These include:

Reporting systems for individual criminal and violent events and details of their circumstances

More detailed data on the location and perpetrators of the crime

More detailed data on agents in unintentional firearm-related injuries, linked to information on both the victim and the storage conditions of firearms involved

More detailed information on firearms used in crimes (e.g., type of firearm used, whether the firearm was carried legally, was registered, how it was acquired, and whether the owner was licensed)

More statistics relevant to changes in behaviors that can be attributed to laws (e.g., the numbers of concealed carry permits issued, or changes in safe storage practices).

2. Measurement of exposure: What laws are in place, and where?

Classification: There have been disputes about which states have which types of laws. Misclassification of state laws and their dates of implementation hinders firearms law research. Some differences among states in the effects of laws may be attributable to differences among states in provisions of the law, such as their requirements, penalties, or the presence of other laws. A recent analysis of firearms laws[62] may help to resolve some of these issues for researchers by providing a recent, systematic, and detailed analysis of major federal, state, and local firearms laws.

Implementation and enforcement. As with any intervention, the degree of implementation may affect the intervention's effectiveness. Data on implementation have typically not been included in the evaluation of firearms laws. How do the intensity and visibility of law enforcement differ among jurisdictions, and how do they affect the law's effectiveness?

Publicity and awareness of laws. Knowledge about laws may be one means by which they become effective. If deterrence is a factor in the effectiveness of a law, then public (and criminal) awareness is of particular importance. Awareness can mitigate a law's potential effects, as when firearms are purchased at increased rates prior to the implementation of a ban.

Duration of exposure and follow-up. Follow-up periods of <2 years may be inadequate to assess the long-term societal effects of a law. It will be useful to determine whether specific laws have immediate or gradual impact, and how effects change over time.

3. Measurement of violent outcomes

Specific measures. Studies should measure outcomes directly associated with the law being evaluated (e.g., violence outside the home for laws about firearm carrying outside the home, and child violence perpetration for laws about child access to and use of firearms in the home). Failure to do so may result from a lack of information on direct measures of the outcome of interest.

Intermediate outcomes. Even when outcomes of interest are directly assessed, it may be useful to have information on intermediate outcomes in order to understand the way in which the outcome of interest is achieved (e.g., decreasing violence by changing firearm storage or carrying behavior).

Population-specific effects. The measurement of the effects of laws (e.g., acquisition restrictions) on violence perpetrated by criminals is important. It is also important to measure or estimate overall population effects of the same laws—for example, whether felony conviction restrictions for firearms purchase affect not only rates of violence among people with felony convictions, but also rates of violence in the general population.

Substitution of weapons. If the goal of a firearms law is the reduction of harm, it is essential to determine whether, given that one weapon may become less available because of the law, that weapon is not readily replaced by another that causes the same (or more or less) harm.

Substitution of place. Similarly, given that many firearms laws are local, it is important to determine whether enacting a law in one location displaces harm from that setting to another (e.g., affecting crime in neighboring jurisdictions that do not have such a law).

4. Measurement of potential confounders and effect modifiers

Measuring and adjusting for confounders. In the analysis of firearms laws, important confounders (e.g., gang activity, drug-related issues, crime cycles, law enforcement practices) are often difficult to measure. Better measures should be developed and used.

Effect modification. It is critical to assess the conditions under which laws may work, may work best, and may not work (e.g., alone or in combination with other laws, or in some settings but not in others). Many laws have multiple provisions, and it is important to determine which combinations of laws or provisions are the most effective.

5. Methods

Appropriate design and analytic techniques. Where possible, the data should be collected as prospective time-series measurements; analyses of trends are preferable to analyses of before-and-after changes. Analytic techniques should include appropriate adjustment for autocorrelation of data in time series and in adjacent geographic locations.

Assumptions and validation. Analytic techniques commonly rest on assumptions about the study design or the characteristics of the study data. Assumptions should be validated, and, to the extent that they are violated, the consequences of violation considered and addressed.

Research Issues Specific to Reviewed Firearms-Related Topics

Several data and research gaps were uncovered in this evaluation that could be potential topics for study.

Bans

Examine effect of grandfathering and registration of grandfathered banned firearms on ban effectiveness.
Examine effects on purchases of firearms to be banned prior to implementation of the ban.
Examine substitution effects.

Restrictions

Examine effects of restriction requirements in the secondary market (gun shows, private sales).
Assess the proportion of firearm-related crimes committed by people in each of the prohibited categories.
Examine the effect of specific restrictions on violence by populations to whom the restrictions apply (e.g., felons, drug abusers, or those adjudicated "mental defective").

Waiting periods

Examine the effect of length of waiting period on violent outcomes.
Examine substitution effects (especially for suicide).
Compare effects of Interim and Permanent Brady laws on firearm-related violence.

Licensing and registration

Assess substitution effects.
Look for specifics in state laws (e.g., fingerprinting or other requirements) as effect modifiers.
Examine effects of licensing and registration in a recent time period, with before-and-after study design and comparison populations.

Shall issue carry laws

Focus specifically on crimes outside the home as outcomes.
Examine permit status for firearms used in crimes.
Examine the effects of differences in state laws on the number of permits issued.
Examine the deterrent effects of publicity about the law.

Child access prevention laws

Assess effects of laws on juvenile firearms users rather than victims.

Examine the effect of laws on storage practices, stratified by the presence of children in the home.

Assess the storage of firearms involved in unintentional injuries, suicide, and crime.

Assess effects of enforcement, punishment, and conviction on storage violation.

Compare effects of the CAP law in Florida (a state with felony sanction for CAP law violation) with effects in other states where violation is a misdemeanor.

Zero firearms tolerance in schools

Assess effects of zero firearms tolerance policies on school violence, firearm-related violence, and the school environment.

Assess school policies and practices for firearms detection, and their relative effectiveness.

Assess cost and benefit of "street" expulsion.

Multiple laws and systems of laws

Assess the effects of combinations of specific laws on specific forms of violence. Studies should allow the determination of which laws are critical to effective combinations and which are not.

Other Effects

The reviews also identified potential research questions related to outcomes in addition to violence. These include:

Property crime. Assess the effects of firearms laws on property crime.

Self-defense. Assess the effects of firearms laws on people's capacity to defend themselves legally. Determine whether all demographic population segments are similarly affected.

Legal rights. Assess the effects of firearms laws on legal rights. For example, expulsion under the Gun-Free Schools Act to keep schools safe may conflict with the rights of students to an education.

Justice. Assess the effects of firearms laws (such as licensing, registration, background checks of applicants) on the apprehension of "wanted persons," such as fugitives from justice.

Cost. Assess the costs and benefits associated with implementing and enforcing firearms laws.

Discussion: Reviewing Firearms Law Effects in the United States

International comparisons indicate that firearm-related violence is considerably higher in the United States than in other developed, industrialized nations.[7] As with other public health problems, efforts have been made to reduce firearm-related violence by means of legal interventions. However, at least based on identified studies of the range of firearms laws reviewed here, the evidence is insufficient to determine whether U.S. firearms laws affect violence. When we conclude that evidence for the effectiveness of a given firearms law on an outcome is insufficient, we do not imply that the law has no effect; rather, we mean that we do not yet know what effect, if any, the law has on that outcome. Other researchers have also noted "the absence of a critical mass of high-quality published studies evaluating the effectiveness of specific gun laws, relative to the magnitude of the problem in the United States."[62]

There are numerous challenges to evaluating the effects of firearms laws on violence in the United States. Information about firearms is collected to regulate, monitor, and investigate firearms transactions, but the collection and use of this information is also limited to protect the privacy of firearms owners. For example, firearms application information used in Brady Law background checks must be destroyed within a given time period. And the Firearms Owners Protection Act of 1986 (P.L. 99-308, 99 Cong., 2d Sess., 100 Stat. 449–461) forbids the federal government from establishing a federal registry of firearms owners. In addition, some of the data sources for violent outcomes (e.g., UCR) that have been most available and most widely used have also been of questionable value because of substantial under-reporting and questionable validity.

However, there are also emerging opportunities to determine whether existing laws are an effective means of reducing violence. The FBI's National Incident-Based Reporting System is designed to replace the UCR and will focus on the detailed circumstances of criminal events. The National Violent Death Reporting System will link multiple sources of information on violent deaths—including death certificates, and medical examiner, police, and crime lab reports—to provide comprehensive information on the circumstances of child abuse deaths, suicides, domestic violence homicides, and other forms of violent death. These reporting systems will greatly enhance the ability to evaluate the effects of firearms laws and other interventions to reduce these forms of violence.

Laws can and have played a prominent role in public health in the United States,[121] and may be one reasonable approach to the problem of firearm-related violence.[14] Further research is needed to understand how laws might affect firearm-related injury and death in the United States.

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Points of view are those of respective affiliated authors and do not necessarily reflect those of the Centers for Disease Control and Prevention; National Institute of Justice, U.S. Department of Justice; or National Institutes of Health.

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Appendix A

Table A1. Studies measuring effect of gun acquisition or possession bans on violence

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results: Reported effect measure	Results: Reported baseline	Results: Reported effect (p value)	Results: Value used in review (p value)
Britt[a] (1996)[1] Greatest: time=series with comparison ARIMA, examines effect of law and timing of effect Fair (3) Description: minimal population description Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: Baltimore MD (no comparable law), and before-and-after comparison	1968–1987/1989 Washington, DC and Baltimore, MD DC and Baltimore as units of analysis Sample size: two cities Sample characteristics: comparable sociodemographics and crime rates Follow-up percent: NA; regionwide study Follow-up length: 21 yr	Monthly firearm-related and nonfirearm-related homicide counts	None reported	Change in monthly firearm-related homicide counts (1968–1987, no effect, confirmed by additional years of data, 1987–1989) FBI data: Washington 1.5 (NS) Baltimore −2.6 ($p<0.05$) NCHS data (change in natural logarithm rate): Washington −0.002 (NS) Baltimore −3.8 ($p<0.01$)	Relative percent change in homicide rates: not calculable (no baseline provided)
Kleck (1993)[2] Least; cross-sectional Regression Fair (2) Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: ban on handgun possession, ban on handgun sales, ban on Saturday night specials (multiple dates, not specified) Control: cities with no such laws	1980 (1979–1981) USA, cities with population >100,000 Cities with >100,000 residents in 1980 as unit of analysis n=170 Multiple sample characteristics summarized Follow-up percent and length: NA	Natural logarithm of difference in total and firearm-related-specific crime, suicide, and unintentional injury rate between cities that had specified bans and those that did not	None reported	Effects of ban on handgun possession: Homicide total: 0.087 (NS) Assault total: 0.022 (NS) Robbery total: 0.104 (NS) Rape total: −0.092 (NS) Suicide total: −0.062 (NS) Firearm-related unintentional death: 0.009 (NS) Effects of ban on handgun sales:	Relative percent change Ban on handgun possession: Homicide total: 9.1 (NS) Assault total: 2.2 (NS) Robbery total: 11.0 (NS) Rape total: −8.8 (NS) Suicide total: −6.0 (NS) Firearm-related unintentional death: 0.9 (NS)

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Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (*p* value)	Value used in review (*p* value)
					Homicide total: 0.001 (NS) Assault total: −0.106 (NS) Robbery total: −0.105 (NS) Rape total: −0.112 (NS) Suicide total: −0.066 (NS) Firearm-related unintentional death: −0.099 (NS) Effects of Saturday night specials ban: Homicide total: 0.083 (NS) Assault total: 0.069 (NS) Robbery total: 0.060 (NS) Rape total: 0.084 (NS) Suicide total: 0.094 (NS) Firearm-related unintentional death: 0.063 (NS)	Ban on handgun sales: Homicide total: 0.1 (NS) Assault total: −10.1 (NS) Robbery total: −9.9 (NS) Rape total: −10.6 (NS) Suicide total: −6.4 (NS) Firearm-related unintentional death: −9.4 (NS) Saturday night specials ban: Homicide total: 8.7 (NS) Assault total: 7.1 (NS) Robbery total: 6.2 (NS) Rape total: 8.8 (NS) Suicide total: 9.9 (NS) Firearm-related unintentional death: 6.5 (NS)

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Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (*p* value)	Value used in review (*p* value)
Loftin (1991)[3] Greatest: time-series with comparison Before-and-after *t*-test and ARIMA Fair (4) Description: no population description Outcome: ecological measurement[b] Confounding: no control for some important confounders Other biases: change in rates before law adoption, population changes not accounted for	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: neighboring counties with no such law, and before-and-after comparison	1968–1987 Washington, DC and adjacent comparison counties of MD and VA (combined; DC-MD-VA SMSA) DC and adjacent comparison counties (combined) as unit of analysis Sample size: three regions Sample characteristics not described Follow-up percent: NA; regionwide study Follow-up length: 19 yr	Monthly nomicide and suicide counts: pre-law average levels and change after the law	Firearm-related homicides (deaths/month) DC: 13.0 MD/VA: 5.8 Non-firearm-related homicides DC: 7.3 MD/VA: 3.0 Firearm-related suicides DC: 2.6 MD/VA: 9.2 Non-firearm-related suicides DC: 4.4 MD/VA: 9.9	Change in firearm-related homicides (deaths/month): DC: −3.3 (p<0.001) MD/VA: −0.4 (NS) Change in non-firearm-related homicides: DC: −0.3 (NS) MD/VA: 0.7 (p<0.05) Change in firearm-related suicides: DC: −0.6 (p<0.05) MD/VA: 1.1 (p<0.05) Change in non-firearm-related suicides: DC: −0.4 (NS) MD/VA: −0.2 (NS)	Relative percent change (total estimates calculated from firearm-related and non-firearm-related estimates) Firearm-related homicide: −19.9 (p<0.001) Total homicide: −20.4 (NS) Firearm-related suicide: −12.6 (p<0.005) Total suicide: −18.1 (NS)
McDowall (1996)[4] Greatest: time-series with comparison Before-and-after change *t*-test Fair (4) Description: minimal population description Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: Boston and Memphis—similar size cities with no such law, and before-and-after change comparison	1968–1987/1990 Washington, DC and Baltimore, Boston, and Memphis DC and Baltimore, Boston, and Memphis as units of analysis Sample size: four regions Sample characteristics not described Follow-up percent: NA; regionwide study Follow-up length: 19 to 22 yr	Monthly homicide and suicide counts: change in average levels before and after law	None reported	Change in firearm-related homicides (deaths/month): DC: 2.08 (1968–1990) Memphis: 0.74 (1968–1987) Boston: −0.80 (1968–1987) Baltimore: −3.01 (1968–1987) Change in non–firearm-related homicides: DC: 0.61 (1968–1990) Memphis: 0.37 (1968–1987)	Relative percent change not calculable. Baseline rates not provided for comparison cities; data collection periods in this report differ for intervention and comparison cities, but available in earlier study[3]

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Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results: Reported effect measure	Results: Reported baseline	Results: Reported effect (p value)	Results: Value used in review (p value)
Other biases: change in rates before law adoption, population changes not accounted for					Boston: −0.31 (1968–1987) Baltimore: −1.41 (1968–1987) Change in firearm-related suicides: DC: −0.47 (1968–1990) Memphis: 0.65 (1968–1987) Boston: 0.10 (1968–1987) Baltimore: 0.17 (1968–1987) Change in non–firearm-related suicides: DC: −0.33 (1968–1990) Memphis: 0.30 (1968–1987) Boston: −0.26 (1968–1987) Baltimore: −0.62 (1968–1987)	
Roth (1999)[5] Greatest: time series with comparison Regression Fair (4) Description: minimal population description Outcome: ecological measurement[b] Follow-up: short follow-up period Confounding: no control for some important confounders	Intervention: Federal Violent Crime Control and Law Enforcement Act banning manufacture, transfer, and possession of certain semiautomatic firearms and large-capacity ammunition magazines, plus restrictions on firearms dealer licensing and age of gun acquisition (1994)	1980–1995 USA, 42 states State as unit of analysis n=42 Sample characteristics: U.S. states, populations not described Follow-up percent: NA, statewide study Follow-up length: 1 yr	Percentage difference between predicted and observed firearm homicide rates	None reported	States (n=15) that had no similar assault weapons ban before and had prior ban on juvenile handgun possession; New York State excluded, because of enactment of other firearms laws in same period: −6.7 (NS)	Relative percent change in firearm homicide rates, comparing states with and without similar weapons bans prior to federal ban; intervention and comparison states had prior bans on juvenile handgun possession; NY and CA excluded

(continued on next page)

Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
	Control: states that had similar laws before 1994					from comparison because of enactment of other firearms laws in same period: −6.7 (NS)
Vernick (1999)[6] Moderate: retrospective design with comparison Pre–post proportions of requests for traces of crime firearms; proportions of banned guns traced to purchase year pre- and post-ban in ban and non-ban cities Fair (4) Description: minimal population description Sampling: convenience sample of 16 cities in YCGII, excluding Washington, DC Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: MD law banning manufacture and sale of Saturday night specials (passed, 1988, effective 1990) Control: 15 YCGII cities without such a law	1985–1996/1997 Location: Baltimore and 15 comparison cities City as unit of analysis, n=16 Population characteristics not provided Follow-up percent: NA Follow-up length: 12 yr retrospective	Relative percent of banned crime gun trace requests (process by which law enforcement identifies source of weapon) among all gun trace requests in other cities compared with Baltimore, after the law, controlling for confounders	Baltimore, before the law: 13.6% Other cities before the law: 17.6%	Ratio of percent of banned crime gun trace requests among all gun trace requests in other cities compared with Baltimore, after the law, controlling for some confounders: 2.3 (p value <0.05)	Relative percent change in proportion of crime guns used between July 1996 and April 1997 that were traced to purchase dates before and after the ban, in Baltimore and comparison cities: −107.6 (p value NA)

(continued on next page)

Table A1. *(continued)*

Author & year **Design suitability: design** **Type of analysis** **Quality of execution** **(# of limitations)** **Specific limitations**	**Intervention; additional intervention components when used (date)** **Comparison**	**Study period** **Location** **Unit of analysis** **Sample size** **Sample characteristics** **Follow-up percent and length**	**Results**			
			Reported effect measure	**Reported baseline**	**Reported effect (p value)**	**Value used in review (p value)**
Weil (1997)[7] Moderate: time-series with no comparison Regression Fair (4) Description: population Outcome: ecological measurement[b] Follow-up: short follow-up period Confounding: no control for some important confounders	Intervention: MD law banning sales of assault pistols and high-capacity ammunition magazines (1994) Comparison: no separate control population, before-and-after comparison only	1989–1995 Location: Baltimore, MD Baltimore (data from first 6 months of each year) as unit of analysis Population characteristics not provided Follow-up percent: NA; regionwide study Follow-up length: 6 months	Difference between expected and actual number of assault guns recovered in first 6 months of 1995	None reported	Expected number of assault guns recovered: 52.5 Actual number of assault guns recovered: 24 55% reduction (p=0.018)	Relative percent change: −55.0 (p=0.018)

[a]Publications excluded because they report on the same intervention in the same population were: Jones ED. The District of Columbia's "Firearms Control Regulation Act of 1975": the toughest handgun control law in the United States—or is it? *Ann Am Acad Political Social Sci* 1981;455:138–49; and Nicholson R, Garner A. The Analysis of the Firearms Control Act of 1975: Handgun Control in the District of Columbia. Washington DC: U.S. Conference of Mayors, 1980.

[b]In ecological measurement, exposures and outcomes are measured in the same population, but it cannot be determined whether those in the population who are exposed are also those with the outcome (or whether those in the population who are not exposed are also those without the outcome), and thus, whether exposure and outcome are associated.

ARIMA, autoregressive integrated moving average; DC, Washington DC; FBI, Federal Bureau of Investigation; MD, Maryland; NCHS, National Center for Health Statistics; NA, not applicable or not available; NS, not statistically significant; SMSA, standard metropolitan statistical area; VA, Virginia; yr, year(s); YCGII, Youth Crime Gun Interdiction Initiative.

References for the Appendix

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