SECOND EDITION

The Science of Gun Policy

A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States

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CHAPTER TWO

Methods

In this second edition of our gun policy research syntheses, we rely largely on the systematic review procedures developed for the first edition, which used the Royal Society of Medicine (Khan et al., 2003) approach to conducting systematic reviews of a scientific literature. This approach consists of five steps: framing questions for review, identifying relevant literature, assessing the quality of the literature, summarizing the evidence, and interpreting the findings. To augment this protocol and strengthen the robustness of our methodological approach, we consulted guidelines from the Campbell Collaboration to ensure that our review criteria were based on relevant factors prescribed for reviews of social and policy interventions (e.g., determination of independent findings, statistical procedures) (Campbell Collaboration, 2001).

Our primary objective was to identify and assess the quality of evidence provided in research that estimated the causal effect of one of the selected gun policies on any of our eight key outcomes. Because of its flexibility and applicability to social and policy interventions, the Royal Society of Medicine approach is particularly suited to the multidisciplinary nature of this review. We adopted this approach because we knew that we would need to draw on primarily observational studies across a range of disciplines, including economics, psychology, public health, sociology, and criminology. Other common approaches for systematic reviews (e.g., Higgins and Green, 2011; Institute of Medicine, 2011) are designed primarily for reviews specific to health care.

The protocol for this updated review was pre-registered in the International Prospective Register of Systematic Reviews, or PROSPERO, database (no. CRD42019120105).[1] Changes from the first edition's search strategy to reflect this updated and expanded report are described in detail in Appendix A.

[1] Pre-registration allows readers of this report to assess whether the search terms, outcomes, evaluation criteria, and synthesis procedures used in this report are the same as we said we were going to use in advance of conducting the research. Pre-registration improves transparency in the research process by demonstrating that the methods used to evaluate the literature have not been revised in the interest of producing a biased set of results.

Selecting Policies

For the 13 policies reviewed in the first edition of this report, we assembled a list of close to 100 distinct gun policies advocated by diverse organizations, including the White House and other U.S. government organizations, advocacy organizations focused on gun policy (such as the National Rifle Association and the Brady Campaign to Prevent Gun Violence), academic organizations focused on gun policy or gun policy research, and professional organizations that had made public recommendations related to gun policy (e.g., the International Association of Chiefs of Police and the American Bar Association). Our objective was to evaluate state firearm laws because there is considerable variation that could be examined to understand the causal effects of such laws. Moreover, because the laws are applied statewide, observed effects may generalize to new jurisdictions better than the effects of local gun policies or programs that may be more tailored to the unique circumstances giving rise to them. We therefore eliminated policies that chiefly concerned local programs or interventions that are not mandated by state laws (e.g., gun buy-back programs or policing strategies that have been recommended on the basis of favorable research findings). For the same reason, we eliminated policies that either have never been passed into state laws or that have not yet had their intended effects (e.g., laws requiring new handguns to incorporate smart-gun technologies). We excluded policies that we concluded were likely to have only an indirect effect on any of the eight outcomes we were examining (e.g., policies concerning mental health coverage in group health insurance plans; the public availability of Bureau of Alcohol, Tobacco, Firearms and Explosives data on gun traces). We offer no opinion on the efficacy of policies or laws that we did not examine.

We also clustered some policy proposals that we regarded as sufficiently similar in concept to be included in the same general class of policies (e.g., policies of repealing the Safe Schools Act and the conceptually similar policy to prohibit gun-free zones).

This process resulted in 13 classes of firearm policies that we subsequently reviewed with multiple representatives of two advocacy organizations (one strongly aligned with enhanced gun regulation and one strongly aligned with reduced gun regulation). The purpose of these consultations was to establish whether we had identified policies that are important, coherent, and relevant to current gun policy debates. This consultation resulted in substituting two of our original 13 classes of laws.

For this second edition of the report, we retained the original 13 policies and added five more policies. Our approach to selecting the new policies was guided by feasibility (i.e., we did not have the resources to study every existing or proposed firearm policy), as well as by external perceptions and agreement on each policy's importance. We first reviewed our original full list of considered policies and updated it with additional policies, programs, or strategies that had been noted by advocacy groups, had been implemented by states, or had garnered media or legislative attention since we assembled the original list. From this updated list, each member of our research team independently ranked a maximum of five new policies that he or she thought should

be incorporated into the expanded research synthesis. Rankings were informed by the approximate number of jurisdictions that had enacted the policy (i.e., implementation and spread), how recently the policy had been enacted (i.e., policies that had been implemented over a period of more than five years were favored because some empirical research on them could have been published), and the extent to which the policy had been widely discussed within the past three years (i.e., importance and attention). Combining rankings across the research team, we selected the following five policies to add for this second version of the report: prohibitions associated with domestic violence, extreme risk protection orders, firearm safety training requirements, bans on low-quality handguns, and laws allowing armed staff in kindergarten through grade 12 (K–12) schools. As noted in Chapter One, the final set of policies, defined and explained in Chapters Three through Twenty, is as follows:

Policies regulating who may legally own, purchase, or possess firearms

1. minimum age requirements
2. prohibitions associated with mental illness
3. prohibitions associated with domestic violence
4. surrender of firearms by prohibited possessors
5. extreme risk protection orders

Policies regulating firearm sales and transfers

6. background checks
7. licensing and permitting requirements
8. waiting periods
9. firearm safety training requirements
10. lost or stolen firearm reporting requirements
11. firearm sales reporting, recording, and registration requirements
12. bans on the sale of assault weapons and high-capacity magazines
13. bans on low-quality handguns

Policies regulating the legal use, storage, or carrying of firearms

14. stand-your-ground laws
15. child-access prevention laws
16. concealed-carry laws
17. gun-free zones
18. laws allowing armed staff in K–12 schools.

These classes of gun policies do not comprehensively account for all—or necessarily the most effective—laws or programs that have been implemented in the United States with the aim of reducing gun violence. For example, our set of policies does not include mandatory minimum sentencing guidelines for crimes with firearms. Furthermore, by restricting our evaluation to state policies, we exclude local interventions (e.g.,

problem-oriented policing, focused deterrence strategies) that have been found to reduce overall crime in prior meta-analyses (Braga, Papachristos, and Hureau, 2014; Braga and Weisburd, 2012; Braga, Weisburd, and Turchan, 2018). Accordingly, we offer no conclusions on the efficacy of such approaches. However, we recognize the potential importance of these other interventions and believe that a similar systematic review of their effects on outcomes relevant to the firearm policy debate merits future research.[2]

Selecting and Reviewing Studies

Our selection and review of the identified literature involved the following steps:

1. Article retrieval: Across all outcomes, we identified a common set of search terms to capture articles relevant to firearm prevalence or firearm policies. We then identified additional search terms unique to each outcome.
2. Title and abstract review: Two team members independently screened article titles and abstracts using DistillerSR, a web-based systematic review software. The screeners used predetermined inclusion and exclusion criteria; discrepancies resulted in input from a third reviewer and were resolved by consensus.
3. Full-text review: All studies retained after the title and abstract review received full-text review using DistillerSR. The purpose of this review was to identify studies that examined the effects of one or more of our policies on any of our outcomes and that employed methods designed to clarify the causal effects of the policy. Once we identified the subset of quasi-experimental studies for each outcome and policy,[3] two raters independently extracted data on each study's methods and findings; discrepancies were resolved by consensus. Members of our multidisciplinary methodology team then reviewed the selected studies and met to discuss the strengths and limitations of each.
4. Synthesis of evidence: The team discussed each set of studies available for a policy-outcome pair to make a determination about the level of evidence supporting the effect of the policy on each outcome.

Article Retrieval

In November 2018, we queried all databases listed in Table 2.1 for English-language studies published between 1995 and October 31, 2018. We selected 1995 as the start date for our electronic searches for a combination of reasons. First, prior to the mid-1990s, methodological concerns inherent in the study designs commonly employed in this literature were not widely recognized, and analytic solutions for those concerns

[2] For a review of the evidence on criminal justice interventions to reduce criminal access to firearms, see Braga, 2017. For a review of the evidence on how firearm policies affect diversions of guns for criminal use, see Crifasi et al., 2019.

[3] We identified no experimental studies.

Table 2.1
Databases Searched for Studies Examining the Effects of Firearm Policies

Database	Details
PubMed	National Library of Medicine's database of medical literature. *Not used for gun industry or hunting searches.*
PsycINFO	Journal articles, books, reports, and dissertations on psychology and related fields. *Not used for gun industry or hunting searches.*
Index to Legal Periodicals	Includes indexing of scholarly articles, symposia, jurisdictional surveys, court decisions, books, and book reviews.
Social Science Abstracts	Journal articles and book reviews on anthropology, crime, economics, law, political science, psychology, public administration, and sociology.
Web of Science	Includes the Book Citation Index, Science Citation, Social Science Citation, Arts & Humanities Citation Indexes, and Conference Proceedings Citation Indexes for Science, Social Science, and Humanities, which include all cited references from indexed articles.
Criminal Justice Abstracts	Abstracts related to criminal justice and criminology; includes current books, book chapters, journal articles, government reports, and dissertations published worldwide.
National Criminal Justice Reference Service	Contains summaries of the more than 185,000 criminal justice publications housed in the National Criminal Justice Reference Service Library collection.
Social Science Abstracts	Citations and abstracts of sociological literature, including journal articles, books, book chapters, dissertations, and conference papers.
EconLit	Journal articles, books, and working papers on economics.
Business Source Complete	Business and economics journal articles, country profiles, and industry reports.
WorldCat	Catalog of books, web resources, and other material worldwide.
Scopus	An abstract and citation database with links to full-text content, covering peer-reviewed research and web sources in scientific, technical, medical, and social science fields, as well as arts and humanities.
LawReviews (LexisNexis)	A database of legal reviews.

were not available in statistical software programs. The first commercial implementation of an appropriate cluster correction for panel data was in Stata in 1993 (Rogers, 1993) and would not show up in published articles until 1995 or later. Second, because many of these observational studies measure outcomes using time-series secondary data that are publicly available, these earlier studies are often updated by the same group of authors, estimating the same effect size of interest but with additional years of data. Indeed, from our original review of 13 policies (RAND Corporation, 2018), the earliest study meeting our inclusion criteria that was not superseded by subsequent work was published in 1999 (Wright, Wintemute, and Rivara, 1999). Finally, the estimated effects of firearm policies passed prior to the 1990s might have less relevance in today's context.

We conducted separate searches for each of the eight outcomes. The search strings that were applied universally across all outcomes included the following:

- gun OR guns OR firearm* OR handgun* OR shotgun* OR rifle* OR longgun* OR machinegun* OR "machine gun*" OR pistol* OR "automatic weapon" OR "assault weapon" OR "semi-automatic weapon*" OR "automatic weapons" OR "assault weapons" OR "semiautomatic weapon*" OR "Saturday night special" OR "Saturday night specials"
  AND
- ownership OR own OR owns OR availab* OR access* OR possess* OR purchas* OR restrict* OR regulat* OR distribut* OR "weapon carrying" OR "weapon-carrying" OR legislation OR legislating OR legislative OR law OR laws OR legal* OR policy OR policies OR "ban" OR "bans" OR "banned" OR "concealed carry."

In addition, we searched for the following outcome-specific search terms (using an "AND" operator before each string):

- suicide: suicide* OR "self-harm*" OR "self-injur*" OR "self injur*"
- violent crime: homicide* OR murder* OR manslaughter OR "domestic violence" OR "spousal abuse" OR "elder abuse" OR "child abuse" OR "family violence" OR "child maltreatment" OR "spousal maltreatment" OR "elder maltreatment" OR "intimate relationship violence" OR "intimate partner violence" OR "dating violence" OR (violen* AND [crime* OR criminal*]) OR rape OR rapes OR rapist* OR "personal crime" OR "personal crimes" OR robbery OR assault* OR stalk* OR terroris*
- unintentional injuries and deaths: accident* OR unintentional
- mass shootings: "mass shooting" OR "mass shootings" OR "mass murder" OR "school shootings"
- officer-involved shootings: "law enforcement" OR police* OR policing OR "use of force" OR "deadly force"
- defensive gun use: self-defense OR "self defense" OR "personal defense" OR defens* OR self-protect* OR "self protect*" OR DGU OR SDGU
- hunting and recreation: hunt OR hunting OR "sport shooting" OR "shooting sports" OR recreation*
- gun industry: industr* OR manufactur* OR produc* OR distribut* OR supply OR trade OR price* OR export* OR revenue* OR sales OR employ* OR profit* OR cost OR costs OR costing OR "gun show" OR tax OR taxes OR taxing OR taxation OR payroll OR "federal firearms license."

Because we were intentionally broad in our search terms, we anticipated that the yield from each of these searches would be large and include irrelevant articles. Thus, for feasibility, a RAND librarian and the study's principal investigator reviewed the

yielded lists for each of these searches, and any obviously irrelevant titles were removed prior to title and abstract screening. As an example, an article resulting from the hunting and recreational gun use search was titled "Ground Squirrel Shooting and Potential Lead Exposure in Breeding Avian Scavengers" and described the use of radiographic imaging to detect lead fragments in squirrel carcasses. Other examples include articles from biology and chemistry that were captured in the search based on such terms as "shotgun proteomics" or "electron gun."

After the article retrieval step, the next steps in the study review process used standardized review criteria to identify all studies with evidence for policy effects meeting minimum evidence standards. Table 2.2 describes our inclusion and exclusion criteria for the title and abstract review and the full-text review steps of this process, and further details are provided in the subsequent sections. Using the experience of our original review, we recognized that it was often challenging to determine during the title and abstract screening step which firearm policies were evaluated in a given study or the specific methodological approach employed (i.e., whether the study was a pooled cross-

Table 2.2
Inclusion and Exclusion Criteria, by Screening Step

	Title and Abstract Review	Full-Text Review
Inclusion criteria		
Study type and focus	• Empirical study that documents a relationship between a firearm policy and one of our eight outcomes	• Empirical study using time-series data with a comparison group to demonstrate a relationship between one of our eight outcomes and at least one of our policies of interest
Exclusion criteria		
Study design	• Commentary or narrative • Review or meta-analysis • Case study	
Document type	• Dissertation • Conference abstract • Legal statute or congressional hearing	
Methods	• Qualitative study • Descriptive study of outcome with no association to firearm policy	• Did not use time-series data with pre-post policy data • Did not include a control or comparison group • Key variables were assumed rather than measured
Outcomes	• Did not include one of our eight outcomes	
Interventions	• Not related to firearm policy	• Did not specifically examine one of our 18 policies
Geography	• Non-U.S. context	

sectional analysis or whether the study leveraged the time-series nature of the analyzed data). Thus, we established hierarchical selection criteria that were less restrictive at the stage of title and abstract review. We developed all criteria based on our research questions, and we pilot-tested these criteria on a sample of ten articles for each step.

Title and Abstract Review

At this stage, we screened article titles and abstracts to determine whether they met our primary inclusion criteria—specifically, *any empirical study (i.e., not theoretical or conceptual) that demonstrated a relationship between a firearm-related public policy and a relevant outcome*. Two trained reviewers independently screened the titles and abstracts of the identified articles using the screening criteria outlined in Table 2.2. Discrepancies resulted in input from a third reviewer and were resolved by consensus.

As shown in the table, we excluded studies at this stage if they did not concern one of the eight outcomes we selected, relate to a firearm policy, or include quantitative analyses. In addition, we excluded studies if they were commentaries or conceptual discussions, systematic reviews or meta-analyses, case studies, dissertations, conference abstracts, legal statutes or congressional hearings, descriptive studies, or studies in which key variables were assumed rather than measured (e.g., a region was assumed to have higher rates of gun ownership). In addition, because of the United States' unique legal, policy, and gun ownership context, we excluded studies that focused on a non-U.S. context. (For discussion of the effects of the 1996 National Firearms Agreement in Australia, see RAND Corporation, 2018, Chapter Twenty-Four).

Full-Text Review

Next, we used full-text review to ensure that the studies included thus far did not meet any of the first set of exclusion criteria and to additionally exclude studies that had no credible claim to having identified a causal effect of policies or that did not concern one of the 18 policies of interest we selected.

Our research syntheses (Chapters Three through Twenty) focus exclusively on studies that used research methods designed to identify causal effects among observed associations between policies and outcomes. Specifically, we required, at a minimum, that studies include time-series data and use such data to establish that policies preceded their apparent effects (a requirement for a causal effect) and that studies include a control group or comparison group (to demonstrate that the purported causal effect was not found among those who were not exposed to the policy). Experimental designs provide the gold standard for establishing causal effects, but we identified none in our literature reviews. On a case-by-case basis, we examined studies that made a credible claim to causal inference on the basis of data that did not include a time series.

We refer to the studies that met our inclusion criteria as *quasi-experimental*. We distinguish these from simple *cross-sectional* studies that may show an association between states with a given policy and some outcome but that have no strategy for

ensuring that it is the policy that caused the observed differences across states. For instance, there could be some other factor associated with both state policy differences and outcome differences, or there could be reverse causality (that is, differences in the outcome across states could have caused states to adopt different policies). In excluding cross-sectional studies from this review, we have adopted a more stringent standard of evidence for causal effects than has often been used in systematic reviews of gun policy.

Although excluding cross-sectional research eliminates a large number of studies on gun policy, longitudinal data are much better for estimating the causal effect of a policy. Specifically, empirical demonstration of causation generally requires three types of evidence (Mill, 1843):

- The cause and effect regularly co-occur (i.e., association).
- The cause occurs before the effect (i.e., precedence).
- Alternative explanations for the association have been ruled out (i.e., elimination of confounds).

Cross-sectional research is largely limited to demonstrating association. Longitudinal studies that include people or regions that are exposed to a policy and those that are not exposed have the potential to provide all three types of evidence. Such a design can demonstrate that the policy preceded the change in the outcome of interest, and it can rule out a wider range of potential confounds, including historical time trends and the time-invariant characteristics of the jurisdictions in which the policies were implemented (Wooldridge, 2002).

We also excluded studies that offered no insight into the causal effects of individual policies. For instance, we excluded studies that evaluated the effects of an aggregate state score describing the totality of each state's gun policies or studies of the aggregate effects of legislation that included multiple gun policies.[4] In rare cases, we excluded from consideration studies that provided insufficient information about their methodologies to evaluate whether they used a credible approach to isolating a causal effect of policies. In cases in which authors updated prior published analyses, we generally chose the updated study. However, in one case (Cook and Ludwig, 2003), we present the results from the earlier analysis (Ludwig and Cook, 2000), which was inclusive of more years of data, provided more detail, and included multiple model specifications (although findings were qualitatively the same). The identified studies included individual-level studies (i.e., studies comparing outcomes among people over time) and ecological studies (i.e., studies comparing outcomes in regions over time).

[4] In a few cases, we included studies that measured a conceptually distinct law using a scale. For instance, Brauer, Montolio, and Trujillo-Baute (2017) measure the strength of state child-access prevention laws using a scale. This scale does not, however, combine the effects of multiple different law classes as we have defined them, so we consider the study in our synthesis of evidence for the effect of child-access prevention laws.

Data Extraction

Information from each included study was extracted into a database with predesignated fields. Extracted information included metadata (e.g., title, authors, date of publication, source), study features (e.g., time period, data sources, and population), statistical methods (e.g., model type, unit of analysis, covariate inclusion), and estimated effects (e.g., coefficient point estimates, standard errors, confidence intervals). One reviewer extracted data of interest from each study and entered them into the standardized form. A second reviewer independently extracted information on estimated effects and checked all other fields for accuracy and completeness; discrepancies were resolved by consensus.

Quality Assessment

In judging the quality of studies, we considered common methodological shortcomings found in the existing gun policy scientific literature, especially the following:

- *Models that may have too many estimated parameters for the number of available observations*. We consistently note whenever estimates were based on models with a ratio of less than ten observations per estimated parameter. When the ratio of estimated parameters to observations dropped below one to five and no supplemental evidence of model fit was provided (such as the use of cross-validation or evidence from an analysis of the relative fit of different model specifications), we discount the study's results and do not calculate effect sizes for its estimates.
- *Models making no adjustment to standard errors for the serial correlation regularly found in panel data frequently used in gun policy studies.* We consistently note when studies did not report having made any such adjustment. When a study noted a correction for only heteroscedasticity, we consider that to be evidence of some correction, although this does not generally fully correct bias in the standard errors due to clustering (Aneja, Donohue, and Zhang, 2014).
- *Models for which the dependent variable appears to violate model assumptions, such as linear models of rare outcomes (many of which are close to zero).* We consistently note when the data appeared to violate modeling assumptions.
- *Effects with large changes in direction and magnitude across primary model specifications*. We consistently note when a study presented evidence that model results were highly sensitive to different model specifications.
- *Models that identify the effect of policies with too few cases.* We consistently note when the effects of policies were identified on the experiences of a single state or a small number of states. These analyses generally provide less persuasive evidence that observed differences between treated and control cases result from the effects of the policy as opposed to other contemporaneous influences on the outcome.

Several of these methodological problems—including insufficient adjustment of standard errors, model misspecification, and the use of data with too few instances of the policy—were shown in simulation studies to result in biased estimates or inflated

Type 1 error rates (Schell, Griffin, and Morral, 2018). And overfitting and sensitivity to model specification are widely recognized as yielding unreliable estimates. Moreover, there is some evidence that most models used in the existing literature may have low power, poorly calibrated standard errors and Type 1 error rates, and other problems. Nevertheless, the criteria that we selected for evaluating individual studies represent threats to validity that we could evaluate objectively.

In the first edition of this report (RAND Corporation, 2018, Appendix A) and another project-related report (Schell, Griffin, and Morral, 2018), we describe other common shortcomings in the existing literature that we do not explicitly discuss in our research syntheses. For instance, in the main chapters of this report and the previous edition, we do not note when papers provided no goodness-of-fit tests or other statistical evidence to justify their covariate selections. We also do not focus on interpretational difficulties and confusion frequently present in studies using spline or hybrid models to estimate the effects of policies. These problems are so common in this literature that consistently commenting on them as shortcomings would become repetitive and cumbersome.

Notably, in this report, we do not evaluate studies based on whether their effect estimates are plausible, either in magnitude or direction. The goal of this systematic review of the literature is to identify empirical associations between various gun policies and the outcomes that matter to policymakers and the public. Therefore, even when an article's authors found the findings to be implausible or unexpected, we do not discount or discard those findings. Instead, we try to avoid filtering the empirical results through any particular theoretical lens. If there were consensus across the field on the direct and indirect effects of a gun policy, it would be reasonable to critique studies that produce effect estimates that are outside theoretically plausible ranges. Nothing like that level of agreement exists for any of the gun policies we studied. Indeed, in many cases, knowledgeable analysts disagree on even the direction of the effects expected from the policies (Morral, Schell, and Tankard, 2018). Thus, we discount effects when the underlying study had methodological weaknesses or when the effect estimate was not mathematically possible (e.g., a claim that a law could prevent more homicides than actually occurred). Similarly, we note when a study produced an effect estimate that was empirically outside the range that was found by other studies.

Synthesis of Evidence

Members of the research team summarized all available evidence from prioritized studies for each of the 18 policies on each of the eight outcomes. When at least one study met inclusion criteria, a multidisciplinary group of methodologists on the research team discussed each study to identify its strengths and weaknesses. The consensus judgments from these group discussions are summarized in the research syntheses. Then, the group discussed the set of available studies as a whole to make a determination about the level of evidence supporting the effect of the policy on each outcome.

When considering the evidence provided by each analysis in a study, we counted effects with *p*-values greater than 0.20 as providing *uncertain* evidence for the effect of a policy. We use this designation to avoid any suggestion that the failure to find a statistically significant effect means that the policy has no effect. We assume that every policy will have some effect, however small or unintended, so any failure to detect it is a shortcoming of the science, not the policy. When the identified effect has a *p*-value less than 0.05, we refer to it as a *significant effect*. Finally, when the *p*-value is between 0.05 and 0.20, we refer to the effect as *suggestive*. These classifications serve primarily as a semantic simplification for the narrative discussion, and although these distinctions helped guide our qualitative syntheses of the evidence, our conclusions are based on fuller consideration of study effect sizes and precision.

We include the suggestive category for several reasons. First, following current guidance (Ryan, Synnot, and Hill, 2016), we are interested in incorporating evidence from studies that may not meet conventional levels of statistical significance ($p < 0.05$). This is particularly because our observation of the existing literature is that it is underpowered (see Schell, Griffin, and Morral, 2018), meaning that even true effects are not likely to be found to be statistically significant. Conducting analyses with low statistical power results in an uncomfortably high probability that effects found to be statistically significant at $p < 0.05$ are in the wrong direction and all effects have exaggerated effect sizes (Gelman and Carlin, 2014). If we had restricted our assessment of evidence to just statistically significant effects, we might base our judgments on an unreliable and biased set of estimates while ignoring the cumulative evidence available in studies reporting nonsignificant results. Traditionally, this problem is overcome in systematic reviews by conducting a meta-analysis, in which the results of multiple underpowered studies can be combined to produce a better-powered estimate of the effect of interest. We cannot generally pursue that strategy in the gun policy literature, however, because most studies of any particular policy use identical or highly overlapping data sets, so the estimates are not independent of each other. For instance, one study will examine the effects of a law on suicides between 1994 and 2000, and another will examine the law's effects on suicides in the same data set between 1994 and 2006. The overlap in outcome data means that, although the estimates are not necessarily identical, they are not independent and thus cannot be combined as a single estimate.

While the selection of $p < 0.20$ as the criterion for rating evidence as suggestive is arbitrary, this threshold corresponds to effects that are meaningfully more likely to be in the observed direction than in the opposite direction. For instance, if we assume that the policy has about as much chance of having a nonzero effect as having no effect, and the power of the test is 0.8, then $p < 0.20$ suggests that there is only a 20-percent probability of incorrectly rejecting the null hypothesis of no effect. For tests that are more weakly powered, as is common in models we review, a *p*-value of less than 0.20 will result in false rejection less than half the time so long as the power of the test is above 0.2 (see, for example, Colquhoun, 2014).

In the final step, we rated the overall strength of the evidence in support of each possible effect of the policy. We approached these evidence ratings with the knowledge that research in this area is modest. Compared with the study of the effects of smoking on cancer, for instance, the study of gun policy effects is in its infancy, so it cannot hope to have anything like the strength of evidence that has accrued in many other areas of social science. Nevertheless, we believed that it would be useful to distinguish the gun policy effects that have relatively stronger or weaker evidence, given the limited evidence base currently available. We did this by establishing the following relativistic scale describing the strength of available evidence:

1. *No studies.* This designation was made when no studies meeting our inclusion criteria evaluated the policy's effect on the outcome.
2. *Inconclusive evidence.* This designation was made when studies with comparable methodological rigor identified inconsistent evidence for the policy's effect on an outcome or when a single study found only uncertain or suggestive effects.
3. *Limited evidence.* This designation was made when at least one study meeting our inclusion criteria and not otherwise compromised by serious methodological problems reported a significant effect of the policy on the outcome, and no studies with equivalent or stronger methods provided contradictory evidence.
4. *Moderate evidence.* This designation was made when two or more studies—at least one of which was not compromised by serious methodological weaknesses—found significant effects in the same direction, and contradictory evidence was not found in other studies with equivalent or stronger methods.
5. *Supportive evidence.* This designation was made when at least three studies not compromised by serious methodological weaknesses found suggestive or significant effects in the same direction using at least two independent data sets. Our requirement that the effect be found in distinct data sets reflects the fact that many gun policy studies use identical or overlapping data sets (e.g., state homicide rates over several years). Chance associations in these data sets are likely to be identified by all who analyze them. Therefore, our supportive evidence category requires that the effect be confirmed in a separate data set.

These rating criteria provided a framework for our assessments of where the weight of evidence currently lies for each of the policies, but they did not eliminate subjectivity from the review process. In particular, the studies we reviewed spanned a wide range of methodological rigor. When we judged a study to be particularly weak, we discounted its evidence in comparison with stronger studies, which sometimes led us to apply lower evidence rating labels than had the study been stronger. In Appendix A, we discuss which policies and outcomes have received revised strength-of-evidence assessments relative to the first edition of this report.

Effects of the Inclusion and Exclusion Criteria on the Literature Reviewed

Figure 2.1 presents the results of the literature search across all eight outcomes. The bottom of the figure shows the number of studies meeting all inclusion criteria. No studies satisfying our inclusion criteria were found for two of the eight outcomes, and some studies examined more than one outcome.

Table 2.3 lists the 123 studies meeting all inclusion criteria.

In several cases, some studies published updates to earlier works that expanded the time frame of the analysis, corrected errors, or applied more-advanced statistical methods to a nearly identical data set. In these cases, we do not treat both the earlier and later works as each contributing an equally valid estimate of the effects of a policy. Instead, we treat the latest version of the analysis as superseding the earlier versions, and we focus our reviews on the superseding analysis. In one case, we substituted an

Figure 2.1
Flow Diagram of Search Results

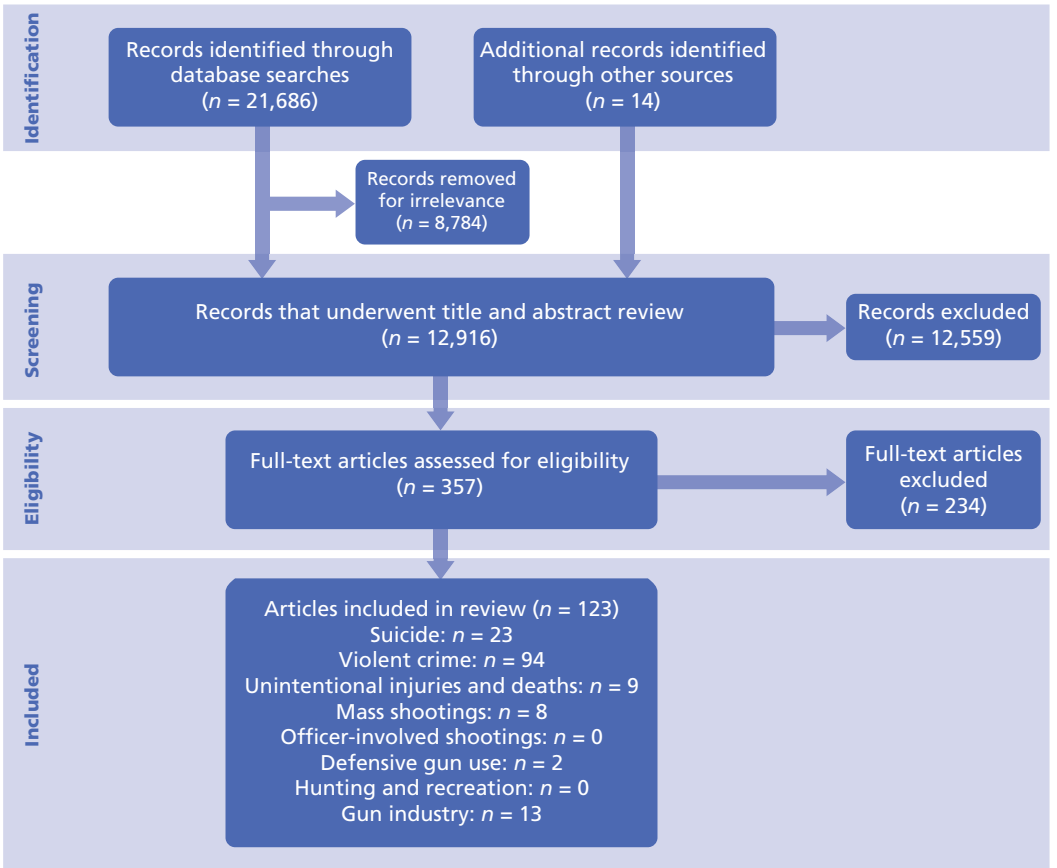

Table 2.3
Studies Meeting Inclusion Criteria

No.	Study
1	Anderson and Sabia (2018)
2	Andrés and Hempstead (2011)
3	Aneja, Donohue, and Zhang (2011)
4	Aneja, Donohue, and Zhang (2014)
5	Anestis and Anestis (2015)
6	Ayres and Donohue (1999)
7	Ayres and Donohue (2002)
8	Ayres and Donohue (2003a)
9	Ayres and Donohue (2003b)
10	Ayres and Donohue (2009a)
11	Ayres and Donohue (2009b)
12	Barati (2016)
13	Bartley and Cohen (1998)
14	Benson and Mast (2001)
15	Black and Nagin (1998)
16	Blau, Gorry, and Wade (2016)
17	Brauer, Montolio, and Trujillo-Baute (2017)
18	Bronars and Lott (1998)
19	Cheng and Hoekstra (2013)
20	Cook and Ludwig (2003)
21	Crifasi et al. (2015)
22	Crifasi, Pollack, and Webster (2016)
23	Crifasi et al. (2018b)
24	Cummings et al. (1997a)
25	DeSimone and Markowitz (2005)
26	DeSimone, Markowitz, and Xu (2013)
27	Dezhbakhsh and Rubin (1998)
28	Díez et al. (2017)
29	Donohue (2003)
30	Donohue (2004)
31	Donohue (2017)
32	Donohue and Levitt (2001)
33	Donohue, Aneja, and Weber (2018)
34	Donohue, Aneja, and Weber (2019)
35	Duggan (2001)
36	Durlauf, Navarro, and Rivers (2016)
37	Duwe, Kovandzic, and Moody (2002)
38	Edwards et al. (2018)
39	French and Heagerty (2008)
40	Ginwalla et al. (2014)
41	Gius (2014)
42	Gius (2015a)
43	Gius (2015b)
44	Gius (2015c)
45	Gius (2017)
46	Gius (2018)
47	Glaeser and Glendon (1998)
48	Grambsch (2008)
49	Guettabi and Munasib (2018)
50	Hamill et al. (2019)
51	Helland and Tabarrok (2004)
52	Hempstead and Andrés (2009)
53	Hepburn et al. (2004)
54	Hepburn et al. (2006)
55	Humphreys, Gasparrini, and Wiebe (2017)
56	Kagawa et al. (2018)
57	Kalesan et al. (2017)
58	Kendall and Tamura (2010)
59	Kivisto and Phalen (2018)
60	Koper (2002)
61	Koper (2004)
62	Koper and Roth (2001)

Table 2.3—Continued

No.	Study
63	Koper and Roth (2002)
64	Kovandzic, Marvell, and Vieraitis (2005)
65	La Valle (2007)
66	La Valle (2010)
67	La Valle (2013)
68	La Valle and Glover (2012)
69	Lott (1998a)
70	Lott (1998b)
71	Lott (2000)
72	Lott (2003)
73	Lott (2010)
74	Lott and Landes (1999)
75	Lott and Mustard (1997)
76	Lott and Whitley (2001)
77	Lott and Whitley (2003)
78	Lott and Whitley (2007)
79	Luca, Malhotra, and Poliquin (2016)
80	Luca, Malhotra, and Poliquin (2017)
81	Ludwig (1998)
82	Ludwig and Cook (2000)
83	Manski and Pepper (2015)
84	Manski and Pepper (2018)
85	Martin and Legault (2005)
86	Marvell (2001)
87	McClellan and Tekin (2017)
88	Monroe (2008)
89	Moody (2001)
90	Moody and Marvell (2008)
91	Moody and Marvell (2009)
92	Moody and Marvell (2018a)
93	Moody and Marvell (2018b)
94	Moody et al. (2014)
95	Munasib, Kostandini, and Jordan (2018)
96	Mustard (2001)
97	Olson and Maltz (2001)
98	Plassmann and Tideman (2001)
99	Plassmann and Whitley (2003)
100	Raissian (2016)
101	Roberts (2009)
102	Rosengart et al. (2005)
103	Roth and Koper (1997)
104	Roth and Koper (1999)
105	Rubin and Dezhbakhsh (2003)
106	Rudolph et al. (2015)
107	Sen and Panjamapirom (2012)
108	Shi and Lee (2018)
109	Siegel et al. (2017b)
110	Steidley and Kosla (2018)
111	Strnad (2007)
112	Swanson et al. (2013)
113	Swanson et al. (2016)
114	Vigdor and Mercy (2003)
115	Vigdor and Mercy (2006)
116	Wallace (2014)
117	Webster, Crifasi, and Vernick (2014)
118	Webster and Starnes (2000)
119	Webster, Vernick, and Hepburn (2002)
120	Webster et al. (2004)
121	Zeoli et al. (2018)
122	Zeoli and Webster (2010)
123	Zimmerman (2014)

earlier study (Ludwig and Cook, 2000) for a later study (Cook and Ludwig, 2003). We did this because the earlier study included a longer data series, used a model with greater statistical power, and provided more-detailed results; in addition, the estimated effects of policies in the two papers were identical for the estimates of interest to us in this review. Table 2.4 lists the superseded studies and their superseding versions.

Table 2.5 describes the policies and outcomes evaluated by each study that was not superseded, and studies are indicated with their corresponding number in Table 2.3. These studies are discussed in detail in subsequent chapters.

Table 2.4
Superseded Studies

Superseded	Superseding
Aneja, Donohue, and Zhang (2011); Ayres and Donohue (1999, 2002, 2003a, 2003b, 2009a, 2009b); Donohue (2003, 2004)	Aneja, Donohue, and Zhang (2014)
Cook and Ludwig (2003)	Ludwig and Cook (2000)
DeSimone and Markowitz (2005)	DeSimone, Markowitz, and Xu (2013)
Dezhbakhsh and Rubin (1998)	Rubin and Dezhbakhsh (2003)
Donohue (2017); Donohue, Aneja, and Weber (2018)	Donohue, Aneja, and Weber (2019)
Hempstead and Andrés (2009)	Andrés and Hempstead (2011)
Koper and Roth (2001, 2002); Roth and Koper (1997, 1999)	Koper (2004)
La Valle (2007, 2010)	La Valle (2013), La Valle and Glover (2012)
Lott and Landes (1999)	Lott (2003)
Lott (1998a, 1998b, 2000); Lott and Whitley (2003)	Lott (2010)
Manski and Pepper (2015)	Manski and Pepper (2018)
Moody (2001); Moody and Marvell (2008, 2009)	Moody et al. (2014)
Vigdor and Mercy (2003)	Vigdor and Mercy (2006)

Table 2.5
Included Studies, by Policy and Outcome

Policy	Suicide	Violent Crime	Unintentional Injuries and Deaths	Mass Shootings	Officer-Involved Shootings	Defensive Gun Use	Hunting and Recreation	Gun Industry	Total
Minimum age requirements	2, 43, 86, 102, 120	86, 102, 106	43	79					**7**
Prohibitions associated with mental illness	2, 107, 113	107, 112, 113							**4**
Prohibitions associated with domestic violence	107	28, 100, 107, 115, 121, 122							**6**
Surrender of firearms by prohibited possessors		28, 100, 115, 121, 122							**5**
Extreme risk protection orders	59								**1**
Background checks	56, 80, 82, 107	23, 42, 53, 56, 67, 73, 80, 82, 88, 107, 112, 113, 115, 122		46, 57, 79				60	**18**
Licensing and permitting requirements	2, 21, 80, 120	22, 23, 45, 80, 106, 117, 122		16, 79					**12**
Waiting periods	5, 38, 80, 82	38, 53, 80, 82, 96, 101		72, 79				17, 47	**11**
Firearm safety training requirements		73						40	**2**
Lost or stolen firearm reporting requirements									**0**
Firearm sales reporting, recording, and registration requirements				16				110	**2**
Bans on the sale of assault weapons and high-capacity magazines		41, 73, 92		16, 44, 46				61	**7**
Bans on low-quality handguns	102	73, 102, 117, 119						119	**4**

Table 2.5—Continued

Policy	Suicide	Violent Crime	Unintentional Injuries and Deaths	Mass Shootings	Officer-Involved Shootings	Defensive Gun Use	Hunting and Recreation	Gun Industry	Total
Stand-your-ground laws	49, 55	19, 23, 49, 55, 73, 87, 95, 117		16		19, 87		116	**10**
Child-access prevention laws	1, 24, 26, 43, 76, 120	1, 24, 26, 76	24, 26, 43, 54, 76, 118, 120	1, 72				17	**10**
Concealed-carry laws	26, 80, 102	4, 12, 13, 14, 15, 18, 22, 23, 26, 32, 34, 36, 39, 41, 48, 50, 51, 53, 58, 64, 67, 68, 73, 78, 80, 81, 84, 85, 93, 94, 96, 97, 98, 99, 101, 102, 105, 108, 109, 111, 117, 123	26, 75	16, 37, 46, 72, 79				35, 40, 110	**51**
Gun-free zones									**0**
Laws allowing armed staff in K–12 schools									**0**
Total	**20**	**68**	**8**	**8**	**0**	**2**	**0**	**9**	**93**[a]

NOTE: Numbers refer to individual studies; see Table 2.3 to view which study corresponds to which number. Totals along the bottom row do not exactly match those in Figure 2.1 because superseded studies are not counted in this table.

[a] Of the 123 studies that met all inclusion criteria, 31 were superseded. However, one study (DeSimone, Markowitz, and Xu, 2013) was only partially superseded, so it is counted among both the 93 included studies and the 31 superseded studies.

Effect Size Estimates

To compare the magnitude of effects across studies, we calculated and presented incidence rate ratios (IRRs) for most of the estimates of policy effects that we considered in reaching our consensus ratings. In rare cases noted in the text, we were unable to calculate IRRs from the information provided in the paper. Studies reporting the results from a negative binomial or Poisson regression model are directly reported in our forest plot figures as IRRs with their associated confidence intervals (CIs). Given the low probability of most of our outcomes, odds ratios were interpreted and reported as IRRs with their associated CIs.

Many studies used fixed-effects ordinary linear regression models. In these cases, an average base rate (usually taken from the study's paper itself) of the outcome of interest was determined. We then used the base rate to transform the regression estimate, β, to an IRR using the following formula:

$$IRR = \frac{(\text{average base rate} + \beta)}{\text{average base rate}}.$$

However, if the linear model used a logged dependent variable, we used the exponentiated estimate as its IRR. CIs for the IRRs derived from the linear regression models were transformed in a similar fashion.

When a study did not report a measure of variation, we performed back calculation from a test statistic to estimate the CIs. For studies using synthetic control methods (Crifasi et al., 2015; Guettabi and Munasib, 2018; Kagawa et al., 2018; Kivisto and Phalen, 2018; Rudolph et al., 2015), we inferred approximate standard errors from the *p*-value associated with a permutation test presented to demonstrate the likely statistical significance of the reported finding. Specifically, we used the method to calculate *p*-values from permutation tests recommended by Phipson and Smyth (2010). In studies that included a lagged outcome term as a predictor, the effect estimates that authors calculated using dummy coding were lower than the effect estimates in studies without a lagged term, so we highlight these cases in the forest plots with a figure note. For several other studies, we note that we could not extrapolate an IRR or its CIs from the data provided in the paper.

Models estimating linear or other trend effects for policies do not have a constant effect size over time. Even if we selected an arbitrary period over which to calculate an effect size, these papers do not provide sufficient information to estimate CIs for such effects. Therefore, we do not calculate or display IRR values that take into account trend effects (*spline models*) or effects calculated as the combination of a trend and a step effect (*hybrid models*). Similarly, IRR values are not calculated or presented for models that flexibly model lagged effects of the policy by including year-by-year specific dummy variables for post-implementation (often referred to as

event study models). Although we report the authors' interpretation of these effects, we do not calculate standardized effect sizes for them. When the same papers present dummy coded effects along with spline or hybrid effects, we calculate effect sizes from those analyses.

IRRs are calculated and graphed so that estimates of the effects of policies can be compared on a common metric. We do not use them to construct meta-analytic estimates of policy effects for two reasons. First, most studies we reviewed examining the effect of a policy on a particular outcome used nearly identical data sets, meaning the studies do not offer independent estimates of the effect. Second, there are usually only two or three studies available on which to estimate the effect of the policy, and these studies often differ considerably in their methodological rigor. These limitations in the existing literature led us to pursue a more qualitative evaluation of the conclusions that available studies can support. As more research or relevant databases become available, meta-analyses may become feasible for conducting qualitative syntheses in this area.

Chapter Two References

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