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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A PART OF THE RAND GUN POLICY IN AMERICA INITIATIVE
Effective gun policies in the United States must balance Second Amendment rights and public interest in gun ownership with concerns about public health and safety. However, current efforts to craft legislation related to guns are hampered by a paucity of reliable information about the effects of such policies. To help address this problem, the RAND Corporation launched the Gun Policy in America initiative. Throughout RAND’s 70-year history, in multiple projects, in many policy arenas, and on topics that are sensitive and controversial, researchers have conducted analyses, built tools, and developed resources to help policymakers and the public make effective decisions. The primary goal of the Gun Policy in America project is to create resources where policymakers and the general public can access unbiased information that facilitates the development of fair and effective firearm policies.

This initiative has yielded several research products, such as a state firearm law database; a survey of policy experts that identified where access to reliable data would be most useful in resolving policy debates; and the first edition of this report, which synthesized the available scientific data on the effects of 13 types of firearm policies on a variety of outcomes related to gun ownership. This second edition builds on that 2018 report, adding five new classes of gun policies and expanding the period over which we conducted our literature search to cover 1995 through the end of 2018. The result is a substantially expanded and updated synthesis of the available evidence concerning the effects of 18 types of gun policies.

The Gun Policy in America initiative did not attempt to evaluate the merits of different values or principles that sometimes drive policy disagreements. Rather, our focus is strictly on the empirical effects of policies on the eight outcomes specified in this report. All of our resources are publicly available on the project website at www.rand.org/gunpolicy.

The work should be of interest to policymakers and other stakeholders considering decisions related to firearm policy. Furthermore, this report may be of interest to the research community and to the general public.
Justice Policy Program

RAND Social and Economic Well-Being is a division of the RAND Corporation that seeks to actively improve the health and social and economic well-being of populations and communities throughout the world. This research was conducted in the Justice Policy Program within RAND Social and Economic Well-Being. The program focuses on such topics as access to justice, policing, corrections, drug policy, and court system reform, as well as other policy concerns pertaining to public safety and criminal and civil justice. For more information, email justicepolicy@rand.org.

Funding

Funding for the Gun Policy in America initiative was originally provided through unrestricted gifts from RAND supporters and income from operations. Since June 2018, this initiative has been supported by a grant from Arnold Ventures.

To support RAND’s efforts and enable initiatives like the Gun Policy in America project, contact our Office of Development at (310) 393-0411, ext. 6901 or giving@rand.org.
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In 2016, the RAND Corporation launched the Gun Policy in America initiative, a unique attempt to systematically and transparently assess available scientific evidence on the real effects of gun laws and policies. Our goal is to create resources where policymakers and the general public can access unbiased information that informs and enables the development of fair and effective policies. Through this initiative, we released the first edition of this report (RAND Corporation, 2018), which synthesized the available scientific data on the effects of 13 classes of firearm policies on firearm deaths, violent crime, the gun industry, participation in hunting and sport shooting, and other outcomes. The report, one of several research products stemming from RAND’s Gun Policy in America initiative (see the project website at www.rand.org/gunpolicy), built and expanded on earlier comprehensive reviews of scientific evidence on gun policy conducted more than a decade previously by the National Research Council (2004) and the Community Preventive Services Task Force (see Hahn et al., 2005).

This second edition builds on our 2018 report, adding five new classes of gun policies and extending the period over which we conducted our literature search from 1995 to the end of 2018. There has been a surge of new scientific publications on gun policy since the first edition of this volume, and we incorporate those studies here, sometimes drawing new or revised conclusions about the quality of evidence available to support claims about the effects of various policies.

Methodology

We used Royal Society of Medicine guidelines for conducting systematic reviews of a scientific literature (Khan et al., 2003). We focused on the empirical literature assessing the effects of 18 classes of firearm policies on any of eight outcomes, which include both public health outcomes and outcomes of concern to many gun owners. We reviewed scientific studies that have been published since 1995, a date chosen to correspond to when such econometric tools as cluster adjustment for serial correlation

1 Although not all guns are firearms, in this report, we follow conventional use in U.S. policy discussions and treat the terms gun and firearm as interchangeable.
in panel data—which are now understood to be critically important for estimating the significance of the effects of gun policies—became available in standard statistical packages (Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004; Schell, Griffin, and Morral, 2018).

The 18 classes of gun policies considered in this research are 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 kindergarten through grade 12 (K–12) schools.

The eight outcomes considered in this research are as follows:

1. suicide
2. violent crime
3. unintentional injuries and deaths
4. mass shootings
5. officer-involved shootings
6. defensive gun use
7. hunting and recreation
8. gun industry.

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2 The terms in these lists describe broad categories of policies and outcomes that are defined and described in detail in the full report.
Policy Analyses, by Outcome

Building on our earlier review (RAND Corporation, 2018) and using standardized, explicit, and pre-registered criteria for determining the strength of evidence that individual studies provide for the effects of gun policies, we produced research syntheses that describe the quality and findings of the best available scientific evidence. Each synthesis defines the class of policies being considered; presents and rates the available evidence; and describes what conclusions, if any, can be drawn about the policy’s effects on outcomes.

In many cases, we were unable to identify any research that met our criteria for considering a study as providing minimally persuasive evidence for a policy’s effects. Studies were excluded from this review if they offered only correlational evidence for a possible causal effect of the law, such as showing that states with a specific law had lower firearm suicides at a single point in time than states without the law. Correlations like these can occur for many reasons other than the effects of a single law, so this kind of evidence provides little information about the effects attributable to specific laws. We did not exclude studies on the basis of their findings, only on the basis of their methods for isolating causal effects. For studies that met our inclusion criteria, we summarize key findings and methodological weaknesses, when present, and provide our consensus judgment on the overall strength of the available scientific evidence. 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—

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3 The protocol for this updated review was pre-registered at the International Prospective Register of Systematic Reviews, or PROSPERO, database (no. CRD42019120105), prior to beginning review of the literature. 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.
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.

These ratings are meant to describe the relative strengths of evidence available across gun policy research domains, not any rating of our absolute confidence in the reported effects. For instance, when we find supportive evidence for the conclusion that child-access prevention laws reduce self-inflicted injuries and deaths, we do not mean to suggest that it is comparable to the evidence available in more-developed fields of social science. That is, in comparison to the evidence that smoking causes cancer, for example, the evidence base in gun policy research is very limited. Nevertheless, we believe that it may be valuable to the public and to policymakers to understand which laws currently have more or less persuasive evidence concerning the effects the laws are likely to produce.

Table S.1 summarizes our evidence ratings for all policy and outcome pairings. Several outcomes show multiple ratings, and these correspond to different characterizations of the specific policy-outcome association. For instance, we identified limited evidence that waiting periods reduce total suicides and moderate evidence that they reduce firearm suicides.

Rather than concerning how strong a policy’s effects are, our findings concern the strength of the available scientific evidence examining those effects. Thus, even when the available evidence is limited, the actual effect of the policy may be strong. Presumably, every policy has some effect on a range of outcomes, however small or unintended. Until researchers design studies that can detect these effects—studies that may require data that are not currently collected in a reliable way—available evidence is likely to remain inconclusive or limited. But this fact should not be confused with the conclusion that the policies themselves have limited effects. They may or may not have the effects they were designed to produce; available scientific research cannot yet answer that question. Moreover, even a policy with a small effect may nevertheless be beneficial to society or worth its costs. For instance, a policy that reduces firearm deaths by just a few percentage points could save more than 1,000 lives per year. This kind of “small” effect might be very difficult to detect with existing study methods but could represent an important contribution to public health and safety.
Table S.1
Strength of Evidence Across Gun Policies and Outcomes

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**NOTE:** I = inconclusive; L = limited; M = moderate; S = supportive. When we identified no studies meeting eligibility criteria, cells are blank. ↑ = the policy increases the outcome; ↓ = the policy decreases the outcome.

*We concluded that there is limited evidence that licensing and permitting requirements decrease total suicides among adults and inconclusive evidence for the effect of such laws on total suicides among young people.*

*We concluded that there is limited evidence that licensing and permitting requirements decrease firearm suicides among adults and inconclusive evidence for the effect of such laws on firearm suicides among young people.*
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Conclusions and Recommendations

Of more than 200 combinations of policies and outcomes, we found that surprisingly few were the subject of methodologically rigorous investigation. Looking across the columns in Table S.1, it is apparent that research into five outcomes is either unavailable or almost entirely inconclusive. It is noteworthy that three of these five outcomes—defensive gun use, hunting and recreation, and the gun industry—are issues of particular concern to gun owners or gun industry stakeholders, such as firearm manufacturers, firearm dealers, hunting outfitters, and firing ranges. The lack of research on a wide range of outcomes makes it difficult or impossible to conduct a comprehensive cost-benefit analysis of the gun policies. For instance, some of the strongest evidence we found suggests that child-access prevention laws could reduce firearm injuries or deaths among children. But restricting access to guns could also prevent gun owners from accessing their weapons in an emergency. The lack of research on defensive gun use means that we do not have a way of directly estimating how the benefits of these laws (in terms of the number of child lives saved) compares with the possible costs (in terms of forgone opportunities for self-defense).

Here, we summarize the key conclusions and recommendations that can be drawn from the policy-outcome combinations with the strongest available evidence (conclusions 1 through 11). Thereafter, we draw conclusions and recommendations concerning how to improve evidence on the effects of gun policies (conclusions 12 through 16).

Conclusions and Recommendations Based on the Existing Evidence Base

Our first set of conclusions and recommendations describes the policy-outcome combinations with the strongest available evidence as identified through our review of the existing literature, as well as recommendations for policy based on this evidence.

**Conclusion 1.** There is supportive evidence that child-access prevention laws, or safe storage laws, reduce self-inflicted fatal or nonfatal firearm injuries, including unintentional and intentional self-injuries, among youth. There is moderate evidence that these laws reduce firearm suicides among youth and limited evidence that the laws reduce total (i.e., firearm and nonfirearm) suicides among youth. In addition, there is limited evidence that these laws may reduce unintentional firearm injuries and deaths among adults. There is some evidence that felony child-access prevention laws have the greatest effects on unintentional firearm deaths.

- **Recommendation 1.** States without child-access prevention laws should consider adopting them as a strategy to reduce firearm suicides and unintentional firearm injuries and deaths.
- **Recommendation 2.** When adopting or refining child-access prevention laws, states should consider making child access to firearms a felony.
**Conclusion 2.** There is supportive evidence that stand-your-ground laws are associated with increases in firearm homicides and moderate evidence that they increase the total number of homicides.

- **Recommendation 3.** States with stand-your-ground laws should consider repealing them as a strategy for reducing homicides.

**Conclusion 3.** There is moderate evidence that state laws prohibiting gun ownership by individuals subject to domestic violence restraining orders decrease total and firearm-related intimate partner homicides, but there is inconclusive evidence for how such prohibitions for those convicted of stalking and misdemeanor domestic violence affect these outcomes.

- **Recommendation 4.** States without laws prohibiting gun ownership while individuals are subject to domestic violence restraining orders should consider passing such laws as a strategy for reducing total and firearm-related intimate partner homicides. States should consider the possibility that these laws may be most effective when they can be applied to a wide range of domestic violence cases and when the law ensures that information about the cases is included in databases used to conduct background checks.

**Conclusion 4.** There is moderate evidence that background checks reduce firearm homicides. Most available studies have examined the effects of dealer background checks or the combined effects of dealer and private-seller background checks when both are required by a state. Therefore, the evidence base for universal background checks compared with the dealer background checks required under federal law is quite limited. However, if performing background checks on a subset of firearm transfers causes reductions in homicides, then extending the practice to all firearm transfers, including private sales, could further reduce firearm homicides. We emphasize, though, that there currently is not strong scientific research that evaluates this inference.

**Conclusion 5.** There is moderate evidence that waiting periods reduce firearm suicides and total homicides and limited evidence that they reduce total suicides and firearm homicides.

- **Recommendation 5.** States without waiting-period laws should consider adopting them as a strategy for reducing suicides and homicides.

**Conclusion 6.** There is limited evidence that licensing and permitting requirements for purchasing a firearm reduce total suicides and firearm suicides among adults. The evidence for the effect of these laws on violent crime remains inconclusive.
Conclusion 7. There is limited evidence that laws prohibiting the purchase or possession of guns by individuals with histories of adjudicated mental illness or incapacity reduce violent crime.

- Recommendation 6. States that currently do not require a background check investigating all types of adjudicated mental health histories that lead to federal prohibitions on firearm purchase or possession should consider implementing robust mental health checks, which may reduce rates of gun violence. The most robust procedures involve sharing data on all prohibited possessors with the National Instant Criminal Background Check System.

Conclusion 8. There is limited evidence that shall-issue, or right-to-carry, laws increase violent crime rates.

Conclusion 9. There is limited evidence that before implementation of a ban on the sale of assault weapons and high-capacity magazines, there is an increase in the sales and prices of the products that the ban will prohibit.

Conclusion 10. There is limited evidence that a minimum age of 21 for purchasing firearms may reduce firearm suicides among youth.

Conclusion 11. No studies meeting our inclusion criteria have examined the effects of gun-free zones, laws allowing armed staff in K–12 schools, or required reporting of lost or stolen firearms. Only inconclusive evidence exists for the effects of minimum age of possession laws; firearm-surrender laws; extreme risk protection orders, or “red-flag” laws; firearm safety training requirements; firearm sales reporting, recording, and registration requirements; bans on low-quality handguns; and permitless-carry laws.

Conclusions and Recommendations for Improving Gun Policy Research

Considering the findings from our review of the existing literature on the effects of firearm policy changes, we offer the following conclusions and recommendations for improving the evidence base on the effects of gun laws.

Conclusion 12. The modest growth in knowledge about the effects of gun policy since 1995 reflects, in part, the past reluctance of the U.S. government to sponsor work in this area at levels comparable to its investment in other areas of public safety and health, such as transportation safety or opioid overdoses. The federal government’s support for research on gun violence prevention has been negligible for more than 20 years; however, in late 2019, Congress appropriated $25 million for this purpose (Pub. L. 116-94), which represents an important new federal initiative that, if sustained in future budgets, could make enormous contributions to knowledge about how to prevent gun-related injuries and deaths.
• **Recommendation 7.** To improve understanding of the real effects of gun policies, Congress should consider appropriating funds annually for a sustained and significant program of research on gun policy and gun violence reduction. This could include investments in firearm research portfolios not only at the CDC and the National Institutes of Health but also at the National Institute of Justice and the National Science Foundation at levels comparable to the government’s current investment in other threats to public safety and health.

• **Recommendation 8.** Until it is clear that federal investments in gun policy research will be sustained in future years and will support a large-scale program of research, private foundations should take further steps to ensure the development of improved data collection and research on gun policies.

**Conclusion 13.** Rigorous research examining the effects of many state gun policies on officer-involved shootings, defensive gun use, hunting and recreation, and the gun industry is virtually nonexistent.

• **Recommendation 9.** To improve understanding of outcomes of critical concern to many in gun policy debates, the U.S. government and private research sponsors should support research examining the effects of gun laws on a wider set of outcomes, including crime, defensive gun use, hunting and sport shooting, officer-involved shootings, and the gun industry.

**Conclusion 14.** The lack of data on gun ownership and availability and on guns in legal and illegal markets severely limits the quality of existing research.

• **Recommendation 10.** To make important advances in understanding the effects of gun laws, the Centers for Disease Control and Prevention or another federal agency should resume collecting voluntarily provided survey data on gun ownership and use.

• **Recommendation 11.** To foster a more robust research program on gun policy, Congress should consider whether to eliminate or loosen the restrictions it has imposed on the use of gun trace data for research purposes.

**Conclusion 15.** Monitoring systems for crime, victimization, and nonfatal firearm injuries are incomplete and not yet fulfilling their promise of supporting high-quality gun policy research in the areas we investigated.

• **Recommendation 12.** Because the National Violent Death Reporting System is so important in collecting data to use for evaluating gun policies, funding to support its implementation in every state should be continued indefinitely.

• **Recommendation 13.** The Bureau of Justice Statistics should continue to pursue its efforts to generate state-level victimization estimates. The current goal of gen-
erating such estimates for 22 states is a reasonable compromise between cost and
the public’s need for more-detailed information. However, the bureau should con-
tinue to expand its development of model-based victimization rates for all states
and for a wider set of victimization experiences (including, for instance, crimes
involving firearm use by an assailant or victim).

• **Recommendation 14.** The Agency for Healthcare Research and Quality at the
  U.S. Department of Health and Human Services should publish aggregated
  state-level estimates of firearm-related injuries from its Healthcare Cost and Uti-
  lization Project data sets in an easy-to-use format, akin to what the agency does
  for opioid-related hospitalizations or emergency department visits.\(^4\)

**Conclusion 16.** The methodological quality of research on firearms can be sig-
ificantly improved.

• **Recommendation 15.** As part of the Gun Policy in America initiative, we have
  published a database containing a subset of state gun laws from 1979 to 2019
  (Cherney et al., 2019). We ask that others with expertise on state gun laws help
  us improve the database by notifying us of its errors, proposing more-useful cat-
  egorizations of laws, or submitting information on laws not yet incorporated into
  the database. With such help, we hope to make the database a resource beneficial
to all analysts.

• **Recommendation 16.** Researchers, reviewers, academics, and science reporters
  should expect new analyses of the effects of gun policies to improve on earlier
  studies by persuasively addressing the methodological limitations of earlier stud-
  ies, such as problems with statistical power, model overfitting, covariate selection,
  poorly calibrated standard errors, multiple testing, undisclosed state variation in
  law implementation, and unjustified assumptions about the time course of each
  policy’s effects.

In conclusion, with a few exceptions, there is a surprisingly limited base of rigorous
scientific evidence concerning the effects of many commonly discussed gun policies.
This does not mean that these policies are ineffective; they might well be quite effect-
ive. Instead, it reflects the absence of scientific study of these policies or methodologi-
ical shortcomings in the existing literature that limit rigorous understanding of policy
effects. In addition, our review did not cover the full range of policy levers available
but rather focused on a set of policies that have been implemented in the U.S. context
and, therefore, have proven to be politically and legally feasible, at least in some U.S.
states. This decision meant that none of the policies we examined would dramatically
increase or decrease the stock of guns or gun ownership rates in ways that would pro-

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duce more readily detectable effects on public safety, health, and industry outcomes. The United States has a large stock of privately owned guns in circulation—estimated to be somewhere between 265 million and 393 million firearms (Karp, 2018; Azrael et al., 2017; Cook and Goss, 2014). Laws designed to change who may buy new weapons, what weapons they may buy, or how gun sales occur will predictably have only a small effect on, for example, homicides or participation in sport shooting, which are affected much more by the existing stock of firearms. Although small effects are especially difficult to identify with the statistical methods common in this field, they may be important. Even a 1-percent reduction in homicides nationally would correspond to approximately 1,500 fewer deaths over a decade.

By highlighting where scientific evidence is accumulating, we hope to build consensus around a shared set of facts that have been established through a transparent, nonpartisan, and impartial review process. In so doing, we also mean to highlight areas where more and better information could make important contributions to establishing fair and effective gun policies.
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Acknowledgments

We wish to thank many staff and researchers inside and outside RAND who helped us collect, interpret, and present the research discussed in this report. In particular, we wish to thank our quality assurance reviewers, who provided expert and valuable guidance on how to improve earlier versions of this report or sections of it. The reviewers for the first edition of this report were James Anderson, Deborah Azrael, John Donohue, Susan Gates, Andy Hoehn, and Priscillia Hunt, as well as Jack Riley, whom we wish to especially thank for his encouragement and support of the idea that RAND could make important contributions to the gun policy discourse in the United States. The report also benefited from candid written reviews provided by two reviewers who wished to remain anonymous, one affiliated with a gun rights advocacy organization and one affiliated with a gun violence prevention advocacy organization. For the second edition of this report, Anderson and Azrael once again served as quality assurance reviewers.

Within RAND, we wish to recognize the additional contributors to this report: Christine Chen, Sara-Laure Faraji, Garrett Baker, Goke Akinniranye, Sachi Yagyu, Jody Larkin, Daniel Schwam, and Samantha Cohen (literature search, screening, document retrieval, and data abstraction); as well as Greg Fauerbach (programming).

Also within RAND, we wish to acknowledge the exceptional support we received from our research librarians, Roberta Shanman and Sachi Yagyu; our publication editor, Allison Kerns; and members of RAND’s Office of External Affairs, including Lee Floyd, Chandra Garber, Stephan Kistler, Heather McCracken, Lauren Skrabala, Mary Vaiana, and Chara Williams.
Abbreviations

ATF Bureau of Alcohol, Tobacco, Firearms and Explosives
BJS Bureau of Justice Statistics
CAP child-access prevention
CDC Centers for Disease Control and Prevention
CI confidence interval
DVRO domestic violence restraining order
ERPO extreme risk protection order
FBI Federal Bureau of Investigation
FFL federal firearms licensee
IRR incidence rate ratio
K–12 kindergarten through grade 12
N/A not applicable
NIBRS National Incident-Based Reporting System
NICS National Instant Criminal Background Check System
NIS Nationwide Inpatient Sample
NRC National Research Council
NVDRS National Violent Death Reporting System
PART A

Introduction and Methods
CHAPTER ONE

Introduction

Americans are deeply divided on gun policy (Parker et al., 2017). Many Americans cherish the traditions of hunting, sport shooting, and collecting guns and value the security and protection that guns can provide. Many regions rely on hunting as an important driver of the tourism economy (Nelson, 2001; BBC Research & Consulting, 2008; Hodur, Leistritz, and Wolfe, 2008), and the wider gun industry employs hundreds of thousands of Americans, including instructors; hunting guides; shooting range operators; hunting equipment suppliers; and manufacturers, distributors, and retailers of firearms and ammunition. At the same time, many Americans have suffered grievous injuries and lost friends and family members in incidents involving firearms.1 More than 39,000 Americans die annually from deliberate and unintentional gun injuries, and two-thirds of these deaths are suicides (Kochanek et al., 2019). Another 50,000 to 150,000 Americans per year receive care in a hospital for a nonfatal gun injury (Centers for Disease Control and Prevention [CDC], 2019b; Avraham, Frangos, and DiMaggio, 2018).2

Multiple high-profile mass public shootings in 2018 and 2019 have focused national attention on the need to reduce the scale and scope of gun violence in the United States (Niforatos, Zheutlin, and Pescatore, 2019). Few Americans are satisfied with the levels of mortality and injury associated with firearms, but there is passionate disagreement about how policies could be shaped to create a better future. There is a quite limited base of science on which to build sound and effective gun policies. Instead, when the public or members of Congress consider proposals affecting gun policy, they encounter conflicting opinions and inconsistent evidence about the likely effects of new laws. Views on what is factual concerning gun policies, or what the facts imply for decisionmaking, frequently divide along political and partisan lines (Kahan, 2017).

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1 Although not all guns are firearms, in this report, we follow conventional use in U.S. policy discussions and treat the terms gun and firearm as interchangeable.

2 In this report, we present a list of references for each chapter. For the complete list of all references used in the first edition of this report, this updated edition, and related Gun Policy in America products, see the project website’s references page (https://www.rand.org/research/gun-policy/analysis/references.html).
Entrenched disagreements on gun policy are not surprising, given the number and variety of contested and contradictory studies, selective misuse of facts by some on all sides of the debate, and today’s hyper-partisan political environment. Moving past such roadblocks will be impossible unless decisionmakers can draw on a common set of facts based on transparent, nonpartisan, and impartial research and analysis. Even when individuals disagree about the objectives of gun policies, empirical evidence can help determine the most likely benefits and harms associated with such policies.

**Gun Policy in America**

To help fill the gap in impartial research and analysis, the RAND Corporation launched the Gun Policy in America initiative in 2016, premised on the idea that the real effects of policies can be objectively determined and that establishing these facts will help lead to sound policies. Our goal is to create a resource where policymakers and the general public can access unbiased information that informs and enables the development of fair and effective firearm policies.

Through this initiative, we released the first edition of this report (RAND Corporation, 2018), which synthesized the available scientific data on the effects of 13 classes of firearm policies on firearm deaths, violent crime, the gun industry, participation in hunting and sport shooting, and other outcomes. It built and expanded on earlier comprehensive reviews of scientific evidence on gun policy conducted more than a decade ago by the National Research Council (2004) and the Community Preventive Services Task Force (see Hahn et al., 2005). The report is one of several research products stemming from RAND’s Gun Policy in America initiative (see the project website at www.rand.org/gunpolicy).

This second edition builds on that 2018 report, adding five new classes of gun policies: prohibitions associated with domestic violence; extreme risk protection orders, or “red-flag” laws; firearm safety training requirements; bans on low-quality handguns, otherwise known as “Saturday night specials” or junk guns; and laws allowing armed staff in kindergarten through grade 12 (K–12) schools. In addition, we expanded our search for relevant literature to include publications since 2016, when we searched the literature for the first edition of this report, and we moved back the start date of our search from 2004 to 1995, for reasons discussed in Chapter Two. The result is a substantially expanded and revised synthesis of the available evidence concerning the effects of 18 classes of laws. In this edition of the report, we have not included updates to the supplementary essays (Part C of the first edition), which covered evidence on such topics as whether the prevalence of gun ownership has an effect on crime or suicide, problems in the measurement of defensive gun use, and what we can learn from Australia’s experience banning some semiautomatic weapons. Those essays remain available on the project website. The first edition also included an appendix on
the source data used for the forest plot figures, and our update to that information is available online as this report’s Appendix B.

In each edition of this report, we have made no attempt to evaluate the merits of different values and principles that sometimes drive policy disagreements. We also have not evaluated the legality of any candidate laws or how they may infringe on Second Amendment rights. Instead, our focus is strictly on the empirical effects of policies on the eight outcomes specified in this report. However, all of the policies we investigate have been implemented in multiple states, and many have withstood Supreme Court review; therefore, we have selected policies that have previously been found not to violate the Constitution.

Laws are not the only interventions that have been used to shape how guns are used in the United States, and research is available on the effectiveness of other approaches, such as public information campaigns, safety and training programs, policing interventions, and school and community programs. In this report, however, our focus is on what scientific studies tell us about the probable effects of certain laws.

Research Focus

The primary focus of this report is our systematic review of 18 broad classes of gun policies that have been implemented in some U.S. states and the effects of those policies on eight outcomes. We selected the 18 classes from a larger set of more than 100 gun policies that have been advocated for; proposed; or passed into law by the federal government, states, or municipalities. Specifically, we restricted our attention to policies or laws that have already been implemented in some states so that researchers could examine the effects of each. In addition, we sought policies designed to have a direct effect on our selected outcomes. These policies, the presumed mechanisms whereby they produce intended (and possibly unintended) effects on our selected outcomes, and the various ways that U.S. states have implemented them are discussed in detail in Chapters Three through Twenty of this report.

The 18 classes of gun policies considered in this research are 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.

When deciding on the outcomes to examine in our research, we first included those related to public health and safety—suicide, violent crime, unintentional injuries and deaths, mass shootings, and officer-involved shootings. These are the outcomes most commonly examined in the research literature we were familiar with. However, we recognized that such outcomes omit many of the benefits of gun ownership that are attractive to gun owners and that may also be affected by laws designed to reduce the gun-related harms to public health and safety. Therefore, we also systematically searched the research literature for studies examining how gun laws affect defensive gun use, hunting and recreation, and the gun industry. Together, these eight outcomes cover many of the areas of concern frequently discussed in debates on gun policy. Here, we provide a short description of each outcome.

Suicide
Official statistics on suicide in the United States are compiled by the CDC. Data from 2017 indicate that 47,173 suicides occurred that year, for a rate of 14.48 per 100,000 people. Of these, 23,854 (50.6 percent) were firearm suicides (CDC, 2019a). Researchers have often examined the effects of laws on total suicides (i.e., suicide deaths by any means, including those involving a firearm), firearm suicides, nonfirearm suicides, and suicide attempts. From a societal perspective, the most important of these outcomes is total suicide; that is, the goal is to reduce the total number of suicide deaths, regardless of the method used in the suicide attempt. In many cases, however, we would expect the effects of gun laws to be more easily observed in rates of firearm suicide, not total suicide. The consensus among public health experts is that there is strong evidence
that reducing firearm suicides in contexts where more-lethal means of attempting suicide are unavailable will result in reductions in the total suicide rate (see, for example, Office of the Surgeon General and National Action Alliance for Suicide Prevention, 2012; World Health Organization, 2014; for review, see Azrael and Miller, 2016). Nevertheless, it is also clear that some people prevented from attempting suicide with a firearm will substitute another lethal means and successfully end their lives. The rate at which this substitution occurs is not known. Thus, for laws that increase or decrease firearm suicides, the effects on total suicides are likely smaller and harder to detect but are fundamentally of greater interest for public policy. For this reason, we examine the effects of policies on both total suicides and firearm suicides.

Age-adjusted suicide rates in the United States have increased 33 percent since 1999 (Hedegaard, Curtin, and Warner, 2018). There is some degree of misclassification of suicide deaths, with some suicides likely classified as unintentional deaths (Kapusta et al., 2011) or overdose deaths (Bohnert et al., 2013), although firearm suicides may be misclassified less often (Rockett et al., 2018). The CDC provides limited nationwide data on suicides for all states. More-expansive data are contained in the National Violent Death Reporting System (NVDRS), also maintained by the CDC, but because that system currently releases information on just a subset of U.S. states, we cannot use this data set to characterize suicides nationally.

Data on nonfatal suicide attempts generally derive from two sources: hospital discharge data (i.e., emergency department and inpatient records) and self-reports. In hospital data, suicides are generally categorized as “self-harm” with unspecified intent; although there is a field to code cause of injury, this field is completed inconsistently across states (Coben et al., 2001). In 2014, there were 469,096 self-harm, nonfatal hospital admissions to emergency departments in the United States, 3,320 (less than 1 percent) of which were caused by a firearm (CDC, 2017c). This may be because between 83 and 91 percent of those who attempt suicide with a firearm die, which is a higher rate than with some other methods of suicide, such as drowning (66–84 percent) or hanging (61–83 percent) (Azrael and Miller, 2016).

Emergency room data contain only self-harm incidents that resulted in an emergency room visit; as a complementary data source, national data based on self-reports reveal that, in 2015, 1.4 million adults aged 18 or older (0.6 percent) attempted suicide in the past year (Piscopo et al., 2016).

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3 The age-adjusted rate standardizes the crude (observed) suicide rate to allow comparisons across time that are not confounded by changes in the age distribution of the population. Nevertheless, the crude rate and the absolute number of suicides have also increased.

4 The CDC is currently working to expand the NVDRS to all 50 states.

5 This is a crude estimate because nonfatal firearm injury estimates produced by the CDC are highly uncertain. See Campbell and Nass, 2019.
Violent Crime

The Federal Bureau of Investigation (FBI) defines violent crime as including forcible rape, robbery, aggravated assault, and murder or nonnegligent manslaughter. The last category excludes deaths caused by suicide, negligence, or accident, as well as justifiable homicides (such as the killing of a felon by a peace officer in the line of duty) (FBI, 2016e).

Gun policies could affect violent crime rates. Policies that make the use of firearms during assaults more or less common could affect both firearm and overall murder rates because assaults involving weapons that are less lethal than firearms will result in fewer deaths (Cook, 1983). Policies that expand the number of gun owners or people carrying guns could deter violent crime if would-be attackers fear confrontations with armed victims (Kleck, 2009), or the policies might make the consequences of violent crime less severe for victims if they are able to successfully use firearms to repel attackers. And policies that make it easier for criminals or suspected criminals to arm themselves could result in more officer-involved shootings if police officers expect most suspects to pose a threat of deadly force (Kivisto, Ray, and Phalen, 2017).

One source of data on violent crime is the FBI’s Uniform Crime Reporting Program, which relies on voluntary reporting of crimes by city, university/college, county, state, tribal, and federal law enforcement agencies. Data from the program indicate that there were approximately 1.25 million violent crimes in the United States in 2017, including 810,825 aggravated assaults, 319,356 robberies, 135,755 rapes, and 17,284 instances of murder or nonnegligent manslaughter (FBI, 2018d). The overall violent crime rate was 400.0 per 100,000 people, with the highest rate for aggravated assault (252.4 per 100,000), followed by robbery (101.2 per 100,000), rape (42.4 per 100,000), and murder or nonnegligent manslaughter (5.3 per 100,000) (FBI, 2018e). Nationwide, firearms were used in 72.6 percent of all instances of murder or nonnegligent manslaughter, 40.6 percent of robberies, and 26.3 percent of aggravated assaults in 2017 (FBI, 2018a).

As with the suicide outcome, we separately consider total homicides and firearm homicides because reductions in firearm homicides do not necessarily have a one-to-one correspondence with overall homicide rates, given that there may be some substitution of means for committing a homicide. That is, some prevented firearm homicides might still end in a homicide with a knife or other weapon.

Death certificate data and emergency department admission data provide additional insights into the prevalence and consequences of violent crime. Using mortality data, the CDC estimated that there were 19,510 homicides in the United States in 2017, for a rate of 5.99 per 100,000 people; of these, 14,542 (75 percent) were caused by a firearm (CDC, 2019a). Emergency department data show that in 2014 there were
more than 1.5 million admissions to hospital emergency departments for assault; of these, 60,470 (3.8 percent) were firearm-related (CDC, 2017c).  

**Unintentional Injuries and Deaths**

Like official statistics on suicide, those on unintentional injuries and deaths in the United States are compiled by the CDC. Data from 2017 indicate that 169,936 fatal unintentional injuries occurred that year, for a rate of 49.30 per 100,000 people (CDC, 2019a). Of these, 486 (less than 1 percent) were caused by a firearm. Although some of these fatal unintentional injuries were likely misclassified and were actually suicides or homicides (Hemenway and Solnick, 2015a), other unintentional firearm deaths may be substantially undercounted in the CDC’s vital data (Barber et al., 2002; Barber and Hemenway, 2011). Research suggests that inconsistent classification of child firearm deaths by local coroners may result in significant underreporting of unintentional firearm deaths among children aged 17 or younger because these deaths are often classified as homicides even if they are unintentional (e.g., a child accidentally shoots another child while playing with a firearm) (Schaechter et al., 2003; Luo and McIntire, 2013). We also include research examining nonfatal unintentional injuries. There were close to 29 million unintentional injury discharges from emergency rooms in 2014, of which 15,928 (less than 1 percent) were caused by a firearm (CDC, 2017c). These reports omit injuries that did not result in an emergency room visit.

**Mass Shootings**

Although only a small percentage of annual firearm deaths result from a mass shooting, these events attract enormous public, media, and social media attention in the country, and they frequently prompt discussions about legislative initiatives for how better to prevent gun violence. The U.S. government has never defined *mass shooting*, and there is no single universally accepted definition of the term. The FBI’s definition of a *mass murderer* requires at least four casualties, excluding the offender or offenders, in a single incident. Public law (the Investigative Assistance for Violent Crimes Act of 2012; Pub. L. 112-265) defines a *mass killing* as a single incident in which three or more people were killed. Alternative definitions include three or more injured victims; four or more people injured or killed, including the shooter; or six or more people shot, fatally or nonfatally (Kleck, 2016). Depending on which data source is referenced, and its definitions, there were 12, 358, or 426 mass shootings in the United States in 2018 (see a discussion of differential estimates by source in RAND Corporation, 2018, Chapter Twenty-Two).

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6 This is a crude estimate because nonfatal firearm injury estimates produced by the CDC are highly uncertain. See Campbell and Nass, 2019.

7 This is a crude estimate because nonfatal firearm injury estimates produced by the CDC are highly uncertain. See Campbell and Nass, 2019.
Often, the attention of researchers, the public, and policymakers is focused on mass shooting incidents that occur in public locations and that are indiscriminate in nature (i.e., that are not connected to other criminal activity and that do not primarily target members of the perpetrator’s family); we hereafter refer to such incidents as mass public shootings. Although mass public shootings are often highly visible and salient events that grip the attention of news media (Schildkraut, Elsass, and Meredith, 2018), they represent a small percentage of the total number of incidents in which four or more people are killed. Between 1976 and 2013, about 12 percent of mass killings were mass public shootings; the most common forms of mass killing were familicide (45 percent) and felony-related mass killings (25 percent) (Duwe, 2017). Between 1999 and 2013, a study of mass shootings with four or more fatalities found a relatively similar distribution, with about 20 percent of mass shootings classified as mass public shootings, 40 percent as familicides, and 39 percent as felony-related mass killings (Krouse and Richardson, 2015). Because these different types of mass shootings are characterized by distinct etiologies and may be affected differently by different policy mechanisms, we attempt to distinguish throughout the report between whether studies consider mass public shootings or mass shootings more broadly.

Officer-Involved Shootings

Police shootings of civilians have triggered fierce debates locally and nationally about when use of lethal force is appropriate and whether it is being used disproportionately against minorities. Although the FBI has tried to collect information on police shootings from about 17,000 local law enforcement agencies, recent efforts by news organizations (such as the Washington Post and the Guardian) have demonstrated that the FBI’s data collection misses many such cases. Whereas the FBI’s count typically comes to around 400 justifiable homicides by police in the line of duty per year (FBI, 2019b), the Washington Post documented news stories on 992 individuals shot and killed by law enforcement in 2018 (Tate, Jenkins, and Rich, 2019)—a number that could omit any individuals shot and killed by police about whom no news story was written. Similarly, states whose violent death data are available through the NVDRS have, on average, roughly twice as many officer-involved shootings counted in the system as appear in the FBI’s supplementary homicide data or the CDC’s vital statistics records (Barber et al., 2016). Moreover, a comparison of NVDRS and five open-source projects tracking police shootings suggested that NVDRS captures 97 percent of known shootings in the states with available NVDRS data (Conner et al., 2019). Several years ago, the FBI announced plans to begin a new data collection effort designed to track all incidents in which law enforcement seriously injure or kill citizens (Kindy, 2015), but results from this effort have not yet been publicly released.

Because reliable data on nonfatal police shootings are often available only for individual police departments, prior studies using such data typically present information at the city level. For example, using police reports and other administrative data,
Klinger et al. (2016) looked at 230 use-of-force shootings by police officers involving 373 suspects in St. Louis between 2003 and 2012. Similarly, medical records of shooting victims often contain information on whether the shooter was a member of the law enforcement community.

**Defensive Gun Use**

Defensive gun use has typically been measured in the empirical literature using self-reports on surveys of gun owners, although some studies have used firearm deaths coded as justifiable homicides to investigate subsets of defensive gun use. Although there are some variations, *defensive gun use* has often been defined as incidents that involve (1) protection against humans (i.e., not animals); (2) gun use by civilians (not official use by military, police, or security personnel); (3) contact between persons (not, for instance, carrying a firearm to investigate a suspicious sound when no intruder is encountered); and (4) use of a gun, at least as a visual or verbal threat (not incidents in which a gun may have simply been available for use). Definitions this broad would include defensive use of a gun by criminals during the commission of a crime, as well as use of a gun for personal defense by those who are prohibited by law from being in possession of a weapon (itself a crime). More-restrictive definitions specify that the defensive gun use be performed by the victim of certain crimes or by someone trying to protect the victim. These definitions may miss instances in which crimes were deterred or averted when a firearm was brandished.

Differences in the definitions of defensive gun use, and in the manner of collecting information about it, lead to wide differences in estimates of the annual incidence of defensive gun use. Low estimates (based on the experiences of crime victims) are a little more than 100,000 such incidents per year, and high estimates are 4.7 million per year (Cook and Ludwig, 1996, 1997, 1998; Hemenway and Azrael, 2000; Kleck and Gertz, 1995; McDowall, Loftin, and Wiersema, 1998). State firearm policy evaluations generally must measure defensive gun use based on justifiable homicide incidents from law enforcement data, and such incidents are believed to be underreported and are a subset of defensive gun use (Kleck, 1988; Cramer, 2016). Furthermore, there are particular challenges in using data on justifiable homicides to understand how specific firearm policies (e.g., stand-your-ground laws) influence defensive gun use because the policies themselves change what is classified as justifiable. The literature on defensive gun use and the challenges of defining and measuring such use were reviewed in the first edition of this report (see RAND Corporation, 2018, Chapter Twenty-Three).

**Hunting and Recreation**

Federal statistics on hunters largely come from the National Survey of Fishing, Hunting, and Wildlife-Associated Recreation, which is conducted every five years as a coordinated effort by the U.S. Fish and Wildlife Service and the U.S. Census Bureau. According to data from 2016, approximately 10 million people used firearms for hunt-
ing, more than 50 percent of all hunters participated in target shooting, and 22 percent of hunters visited shooting ranges (U.S. Fish and Wildlife Service, 2018). Target shooting is also a popular U.S. sport. Results from the 2016 survey indicate that 32 million people aged 6 years or older went target shooting with firearms in 2015 (U.S. Fish and Wildlife Service, 2018). However, data from the General Social Survey suggest that hunting has decreased significantly since 1977, when 31.6 percent of adults lived in households where they, their spouse, or both hunted. In 2014, the percentage of households with a hunter was down to 15.4 percent (Smith and Son, 2015).

**Gun Industry**

Estimates produced by the National Shooting Sports Foundation suggest that there are 149,000 jobs in the United States involving the manufacture, distribution, or retailing of ammunition, firearms, and hunting supplies and potentially another 162,845 jobs in supplier and ancillary industries connected with the firearm market (National Shooting Sports Foundation, 2019). According to the U.S. Census Bureau, in 2014, more than 90,000 people were employed in U.S. firms coded as being involved in just the manufacture of firearms, ammunition, or ordnance (North American Industry Classification System [NAICS] codes 332992, 332993, and 332994; U.S. Census Bureau, 2016). The gun and ammunition manufacturing industry alone is estimated to generate $13 billion in revenue annually (Longo, 2017). In 2016, hunters spent $3 billion on firearms and $1.4 billion on ammunition (U.S. Fish and Wildlife Service, 2018). More than 11 million firearms were manufactured in the United States in 2016, nearly triple the number manufactured one decade prior. An additional 4.5 million firearms were imported in 2016, while just more than 380,000 firearms were exported from the United States (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2018).

As of the end of fiscal year 2017, 136,081 federal firearms licensees had active licenses to sell firearms in the United States. Just more than 47 percent of these licenses were held by dealers or pawnbrokers, 41 percent were held by collectors,8 about 10 percent were held by manufacturers of ammunition or firearms, and less than 1 percent were held by importers (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2018).

**Organization of This Report**

The report is organized into five parts. Part A introduces the project scope and objectives in Chapter One and the methods used to conduct systematic reviews and syntheses of the literature in Chapter Two. In Parts B–D, we present a research synthesis on each of the 18 state gun policies selected for review (Chapters Three through Twenty).

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8 The collector (Type 03) license issued under 18 U.S.C. 44 pertains exclusively to firearms classified as curios and relics and is intended to facilitate a personal collection.
Each of these chapters defines the class of policy under review; presents and rates the available evidence; and describes what conclusions, if any, can be drawn about how each policy affects each outcome. In particular, Part B presents the research syntheses for the five included policies that regulate who may legally own, purchase, or possess firearms; Part C presents the research syntheses for the eight included policies that regulate firearm sales and transfers; and Part D presents the research syntheses for the five included policies that regulate the legal use, storage, or carrying of firearms. In Part E, we draw general conclusions from the main policy analyses and offer recommendations for how to improve the evidence base for the effects of state laws. Finally, Appendix A reviews the differences between the first and second editions of this report (e.g., changes to policy definitions and overall assessments). And Appendix B, available online, provides the source data used to display study effect sizes and rate study methodologies.
Chapter One References


CDC—See Centers for Disease Control and Prevention.


FBI—See Federal Bureau of Investigation.


United States Code, Title 18, Chapter 44, Firearms.


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). Changes from the first edition’s search strategy to reflect this updated and expanded report are described in detail in Appendix A.

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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

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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.
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.

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

<table>
<thead>
<tr>
<th>Database</th>
<th>Details</th>
</tr>
</thead>
<tbody>
<tr>
<td>PubMed</td>
<td>National Library of Medicine’s database of medical literature. Not used for gun industry or hunting searches.</td>
</tr>
<tr>
<td>PsycINFO</td>
<td>Journal articles, books, reports, and dissertations on psychology and related fields. Not used for gun industry or hunting searches.</td>
</tr>
<tr>
<td>Index to Legal Periodicals</td>
<td>Includes indexing of scholarly articles, symposia, jurisdictional surveys, court decisions, books, and book reviews.</td>
</tr>
<tr>
<td>Social Science Abstracts</td>
<td>Journal articles and book reviews on anthropology, crime, economics, law, political science, psychology, public administration, and sociology.</td>
</tr>
<tr>
<td>Web of Science</td>
<td>Includes the Book Citation Index, Science Citation, Social Science Citation, Arts &amp; Humanities Citation Indexes, and Conference Proceedings Citation Indexes for Science, Social Science, and Humanities, which include all cited references from indexed articles.</td>
</tr>
<tr>
<td>Criminal Justice Abstracts</td>
<td>Abstracts related to criminal justice and criminology; includes current books, book chapters, journal articles, government reports, and dissertations published worldwide.</td>
</tr>
<tr>
<td>National Criminal Justice Reference Service</td>
<td>Contains summaries of the more than 185,000 criminal justice publications housed in the National Criminal Justice Reference Service Library collection.</td>
</tr>
<tr>
<td>Social Science Abstracts</td>
<td>Citations and abstracts of sociological literature, including journal articles, books, book chapters, dissertations, and conference papers.</td>
</tr>
<tr>
<td>EconLit</td>
<td>Journal articles, books, and working papers on economics.</td>
</tr>
<tr>
<td>Business Source Complete</td>
<td>Business and economics journal articles, country profiles, and industry reports.</td>
</tr>
<tr>
<td>WorldCat</td>
<td>Catalog of books, web resources, and other material worldwide.</td>
</tr>
<tr>
<td>Scopus</td>
<td>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.</td>
</tr>
<tr>
<td>LawReviews (LexisNexis)</td>
<td>A database of legal reviews.</td>
</tr>
</tbody>
</table>
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 (violent* 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

<table>
<thead>
<tr>
<th>Study type and focus</th>
<th>Title and Abstract Review</th>
<th>Full-Text Review</th>
</tr>
</thead>
<tbody>
<tr>
<td>Empirical study that documents a relationship between a firearm policy and one of our eight outcomes</td>
<td>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</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Exclusion criteria</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study design</td>
</tr>
<tr>
<td>Commentary or narrative</td>
</tr>
<tr>
<td>Review or meta-analysis</td>
</tr>
<tr>
<td>Case study</td>
</tr>
<tr>
<td>Document type</td>
</tr>
<tr>
<td>Dissertation</td>
</tr>
<tr>
<td>Conference abstract</td>
</tr>
<tr>
<td>Legal statute or congressional hearing</td>
</tr>
<tr>
<td>Methods</td>
</tr>
<tr>
<td>Qualitative study</td>
</tr>
<tr>
<td>Descriptive study of outcome with no association to firearm policy</td>
</tr>
<tr>
<td>Did not use time-series data with pre-post policy data</td>
</tr>
<tr>
<td>Did not include a control or comparison group</td>
</tr>
<tr>
<td>Key variables were assumed rather than measured</td>
</tr>
<tr>
<td>Outcomes</td>
</tr>
<tr>
<td>Did not include one of our eight outcomes</td>
</tr>
<tr>
<td>Interventions</td>
</tr>
<tr>
<td>Not related to firearm policy</td>
</tr>
<tr>
<td>Did not specifically examine one of our 18 policies</td>
</tr>
<tr>
<td>Geography</td>
</tr>
<tr>
<td>Non-U.S. context</td>
</tr>
</tbody>
</table>
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. 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).

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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>
<thead>
<tr>
<th>Identification</th>
<th>Records identified through database searches (n = 21,686)</th>
<th>Additional records identified through other sources (n = 14)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Records removed for irrelevance (n = 8,784)</td>
<td></td>
</tr>
<tr>
<td>Screening</td>
<td>Records that underwent title and abstract review (n = 12,916)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Records excluded (n = 12,559)</td>
<td></td>
</tr>
<tr>
<td>Eligibility</td>
<td>Full-text articles assessed for eligibility (n = 357)</td>
<td>Full-text articles excluded (n = 234)</td>
</tr>
<tr>
<td>Articles included in review (n = 123)</td>
<td>Suicide: n = 23</td>
<td>Violent crime: n = 94</td>
</tr>
<tr>
<td></td>
<td>Unintentional injuries and deaths: n = 9</td>
<td>Mass shootings: n = 8</td>
</tr>
<tr>
<td></td>
<td>Officer-involved shootings: n = 0</td>
<td>Defensive gun use: n = 2</td>
</tr>
<tr>
<td></td>
<td>Hunting and recreation: n = 0</td>
<td>Gun industry: n = 13</td>
</tr>
<tr>
<td>Included</td>
<td>Articles included in review (n = 123)</td>
<td></td>
</tr>
</tbody>
</table>
Table 2.3
Studies Meeting Inclusion Criteria

<table>
<thead>
<tr>
<th>No.</th>
<th>Study</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Anderson and Sabia (2018)</td>
</tr>
<tr>
<td>2</td>
<td>Andrés and Hempstead (2011)</td>
</tr>
<tr>
<td>3</td>
<td>Aneja, Donohue, and Zhang (2011)</td>
</tr>
<tr>
<td>4</td>
<td>Aneja, Donohue, and Zhang (2014)</td>
</tr>
<tr>
<td>5</td>
<td>Anestis and Anestis (2015)</td>
</tr>
<tr>
<td>6</td>
<td>Ayres and Donohue (1999)</td>
</tr>
<tr>
<td>7</td>
<td>Ayres and Donohue (2002)</td>
</tr>
<tr>
<td>8</td>
<td>Ayres and Donohue (2003a)</td>
</tr>
<tr>
<td>9</td>
<td>Ayres and Donohue (2003b)</td>
</tr>
<tr>
<td>10</td>
<td>Ayres and Donohue (2009a)</td>
</tr>
<tr>
<td>11</td>
<td>Ayres and Donohue (2009b)</td>
</tr>
<tr>
<td>12</td>
<td>Barati (2016)</td>
</tr>
<tr>
<td>14</td>
<td>Benson and Mast (2001)</td>
</tr>
<tr>
<td>15</td>
<td>Black and Nagin (1998)</td>
</tr>
<tr>
<td>16</td>
<td>Blau, Gorry, and Wade (2016)</td>
</tr>
<tr>
<td>17</td>
<td>Brauer, Montolio, and Trujillo-Baute (2017)</td>
</tr>
<tr>
<td>18</td>
<td>Bronars and Lott (1998)</td>
</tr>
<tr>
<td>19</td>
<td>Cheng and Hoekstra (2013)</td>
</tr>
<tr>
<td>20</td>
<td>Cook and Ludwig (2003)</td>
</tr>
<tr>
<td>21</td>
<td>Crifasi et al. (2015)</td>
</tr>
<tr>
<td>22</td>
<td>Crifasi, Pollack, and Webster (2016)</td>
</tr>
<tr>
<td>23</td>
<td>Crifasi et al. (2018b)</td>
</tr>
<tr>
<td>24</td>
<td>Cummings et al. (1997a)</td>
</tr>
<tr>
<td>25</td>
<td>DeSimone and Markowitz (2005)</td>
</tr>
<tr>
<td>26</td>
<td>DeSimone, Markowitz, and Xu (2013)</td>
</tr>
<tr>
<td>27</td>
<td>Dezhbakhsh and Rubin (1998)</td>
</tr>
<tr>
<td>28</td>
<td>Diez et al. (2017)</td>
</tr>
<tr>
<td>29</td>
<td>Donohue (2003)</td>
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<tr>
<td>30</td>
<td>Donohue (2004)</td>
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<tr>
<td>31</td>
<td>Donohue (2017)</td>
</tr>
<tr>
<td>32</td>
<td>Donohue and Levitt (2001)</td>
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<tr>
<td>33</td>
<td>Donohue, Aneja, and Weber (2018)</td>
</tr>
<tr>
<td>34</td>
<td>Donohue, Aneja, and Weber (2019)</td>
</tr>
<tr>
<td>35</td>
<td>Duggan (2001)</td>
</tr>
<tr>
<td>36</td>
<td>Durlauf, Navarro, and Rivers (2016)</td>
</tr>
<tr>
<td>37</td>
<td>Duwe, Kovandzic, and Moody (2002)</td>
</tr>
<tr>
<td>38</td>
<td>Edwards et al. (2018)</td>
</tr>
<tr>
<td>39</td>
<td>French and Heagerty (2008)</td>
</tr>
<tr>
<td>40</td>
<td>Ginwalla et al. (2014)</td>
</tr>
<tr>
<td>41</td>
<td>Gius (2014)</td>
</tr>
<tr>
<td>42</td>
<td>Gius (2015a)</td>
</tr>
<tr>
<td>43</td>
<td>Gius (2015b)</td>
</tr>
<tr>
<td>44</td>
<td>Gius (2015c)</td>
</tr>
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<td>Gius (2017)</td>
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<td>46</td>
<td>Gius (2018)</td>
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<td>47</td>
<td>Glaeser and Glendon (1998)</td>
</tr>
<tr>
<td>48</td>
<td>Grambsch (2008)</td>
</tr>
<tr>
<td>49</td>
<td>Guettabi and Munasib (2018)</td>
</tr>
<tr>
<td>50</td>
<td>Hamill et al. (2019)</td>
</tr>
<tr>
<td>51</td>
<td>Helland and Tabarrok (2004)</td>
</tr>
<tr>
<td>52</td>
<td>Hempstead and Andrés (2009)</td>
</tr>
<tr>
<td>53</td>
<td>Hepburn et al. (2004)</td>
</tr>
<tr>
<td>54</td>
<td>Hepburn et al. (2006)</td>
</tr>
<tr>
<td>55</td>
<td>Humphreys, Gasparrini, and Wiebe (2017)</td>
</tr>
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<td>56</td>
<td>Kagawa et al. (2018)</td>
</tr>
<tr>
<td>57</td>
<td>Kalesan et al. (2017)</td>
</tr>
<tr>
<td>58</td>
<td>Kendall and Tamura (2010)</td>
</tr>
<tr>
<td>59</td>
<td>Kivisto and Phalen (2018)</td>
</tr>
<tr>
<td>60</td>
<td>Koper (2002)</td>
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<tr>
<td>62</td>
<td>Koper and Roth (2001)</td>
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### Table 2.3—Continued

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<th>Study</th>
<th>No.</th>
<th>Study</th>
</tr>
</thead>
<tbody>
<tr>
<td>63</td>
<td>Koper and Roth (2002)</td>
<td>64</td>
<td>Kovandzic, Marvell, and Vieraitis (2005)</td>
</tr>
<tr>
<td>67</td>
<td>La Valle (2013)</td>
<td>68</td>
<td>La Valle and Glover (2012)</td>
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<td>69</td>
<td>Lott (1998a)</td>
<td>70</td>
<td>Lott (1998b)</td>
</tr>
<tr>
<td>75</td>
<td>Lott and Mustard (1997)</td>
<td>76</td>
<td>Lott and Whitley (2001)</td>
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<td>91</td>
<td>Moody and Marvell (2009)</td>
<td>92</td>
<td>Moody and Marvell (2018a)</td>
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<td>93</td>
<td>Moody and Marvell (2018b)</td>
<td>94</td>
<td>Moody et al. (2014)</td>
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<td>Munasib, Kostandini, and Jordan (2018)</td>
<td>96</td>
<td>Mustard (2001)</td>
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<td>101</td>
<td>Roberts (2009)</td>
<td>102</td>
<td>Rosengart et al. (2005)</td>
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<td>Swanson et al. (2013)</td>
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<td>117</td>
<td>Webster, Crifasi, and Vernick (2014)</td>
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<td>Webster and Starnes (2000)</td>
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<tr>
<td>123</td>
<td>Zimmerman (2014)</td>
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</table>
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>
<thead>
<tr>
<th>Superseded</th>
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<tr>
<td>DeSimone and Markowitz (2005)</td>
<td>DeSimone, Markowitz, and Xu (2013)</td>
</tr>
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<td>Hempstead and Andrés (2009)</td>
<td>Andrés and Hempstead (2011)</td>
</tr>
<tr>
<td>Policy</td>
<td>Suicide</td>
</tr>
<tr>
<td>---------------------------------------------</td>
<td>--------------------------</td>
</tr>
<tr>
<td>Minimum age requirements</td>
<td>2, 43, 86, 102, 120</td>
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<tr>
<td>Prohibitions associated with mental illness</td>
<td>2, 107, 113</td>
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<tr>
<td>Prohibitions associated with domestic violence</td>
<td>107</td>
</tr>
<tr>
<td>Surrender of firearms by prohibited possessors</td>
<td>28, 100, 115, 121, 122</td>
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<tr>
<td>Extreme risk protection orders</td>
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<tr>
<td>Background checks</td>
<td>56, 80, 82, 107</td>
</tr>
<tr>
<td>Licensing and permitting requirements</td>
<td>2, 21, 80, 120</td>
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<tr>
<td>Waiting periods</td>
<td>5, 38, 80, 82</td>
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<tr>
<td>Firearm safety training requirements</td>
<td>73</td>
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<tr>
<td>Lost or stolen firearm reporting requirements</td>
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<tr>
<td>Firearm sales reporting, recording, and registration requirements</td>
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<tr>
<td>Bans on the sale of assault weapons and high-capacity magazines</td>
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<tr>
<td>Bans on low-quality handguns</td>
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### Table 2.5—Continued

<table>
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<tr>
<th>Policy</th>
<th>Suicide</th>
<th>Violent Crime</th>
<th>Unintentional Injury and Deaths</th>
<th>Mass Shootings</th>
<th>Officer-Involved Shootings</th>
<th>Defensive Gun Use</th>
<th>Hunting and Recreation</th>
<th>Gun Industry</th>
<th>Total</th>
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<tbody>
<tr>
<td>Stand-your-ground laws</td>
<td>49, 55</td>
<td>19, 23, 49, 55, 73, 87, 95, 117</td>
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<td>19, 87</td>
<td>116</td>
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<tr>
<td>Child-access prevention laws</td>
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<td>1, 24, 26, 76</td>
<td>24, 26, 43, 54, 76, 118, 120</td>
<td>1, 72</td>
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<td>Concealed-carry laws</td>
<td>26, 80, 102</td>
<td>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</td>
<td>26, 75</td>
<td>16, 37, 46, 72, 79</td>
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<td>Gun-free zones</td>
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<td></td>
<td></td>
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<tr>
<td>Laws allowing armed staff in K–12 schools</td>
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<td></td>
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<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>20</td>
<td>68</td>
<td>8</td>
<td>8</td>
<td>0</td>
<td>2</td>
<td>0</td>
<td>9</td>
<td>93</td>
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</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.

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.
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, $\beta$, 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


Part B of this report presents research syntheses on the included policies that regulate who may legally own, purchase, or possess firearms. These policies are designed to keep firearms out of the hands of individuals at elevated risk for perpetrating violence. Prohibiting conditions for the possession and purchase of firearms include certain criminal histories (e.g., conviction of a felony, conviction of a misdemeanor crime of domestic violence, or status as a fugitive from justice), criteria related to mental illness or substance use disorder (e.g., involuntary commitment to a mental institution, unlawful use of controlled substances), dishonorable discharge from the military, criteria related to citizenship status (e.g., undocumented immigrants or those who have renounced their U.S. citizenship), and age restrictions (e.g., age 18 is the minimum legal age for handgun possession at the federal level). Other policies establish mechanisms for the removal of firearms from individuals who are prohibited possessors or expand firearm prohibitions to apply to individuals who may be experiencing acute risk for gun violence but may not meet the predefined condition of a prohibited possessor.

Overall, this family of policies directly affects the size of the population that can legally obtain or maintain possession of a firearm. Of the 18 policy classes included in this review, five operate by restricting firearm possession and purchase by certain populations. Each is reviewed in the subsequent chapters in Part B, as follows:

- minimum age requirements (Chapter Three)
- prohibitions associated with mental illness (Chapter Four)
- prohibitions associated with domestic violence (Chapter Five)
- surrender of firearms by prohibited possessors (Chapter Six)
- extreme risk protection orders (Chapter Seven).
CHAPTER THREE

Minimum Age Requirements

Under federal law, licensed dealers cannot sell or deliver handguns to individuals under age 21 or long guns to those under age 18. Unlicensed individuals cannot sell, transfer, or deliver handguns to individuals under age 18. With some exceptions, federal law prohibits individuals under age 18 from possessing handguns, but it does not place age restrictions on the possession of long guns (18 U.S.C. 922).

Laws requiring a minimum age for purchase aim to make it more difficult for underage individuals to acquire a handgun through formal channels, while laws requiring a minimum age of possession are intended to make it more difficult or risky for an underage individual to carry firearms. Thus, although the mechanisms by which these laws influence youth access differ, both are designed to limit the availability of firearms to young people—and therefore reduce the gun violence and unintentional shootings they commit.

Firearm homicides and violent crimes disproportionately involve individuals under age 21, both as perpetrators and as victims. Indeed, in 2012, arrest rates for violent crimes peaked at age 18 (Office of Juvenile Justice and Delinquency Prevention, 2016). Of the 8,545 firearm homicides committed in 2016 for which the age of the offender was known, 46.8 percent were perpetrated by individuals aged 12–24 (Puzzanchera, Chamberlin, and Kang, 2018), although this group represents only 17.7 percent of the general U.S. population (U.S. Census Bureau, 2017). By influencing the possession of guns among youth, minimum age laws could, in theory, reduce rates of firearm crime perpetrated by juveniles. However, youth are similarly at high risk of victimization. Of all 2017 deaths among those aged 16–21, 16.8 percent were homicides, which is greater than the homicide rates for the next-highest risk ages (12.7 percent for those aged 22–27; 8.4 percent for those aged 28–33) (calculated using data from the Centers for Disease Control and Prevention [CDC], 2019a). In theory, therefore, stricter age limits on purchasing or possessing a firearm could reduce the incidence of defensive gun use by youth and potentially increase perpetration of violence against younger populations if offenders believe that the likelihood of encountering armed resistance is lower (Marvell, 2001).

Conceptually, by restricting youth access, minimum age restrictions could also reduce rates of firearm suicide or unintentional shootings by the affected age group.
Research suggests that the association between firearm availability and suicide is strongest among adolescents and young adults (Birckmayer and Hemenway, 2001; Miller and Hemenway, 1999). In 2017, there were 3,556 suicide deaths among individuals aged 16–21, 46.8 percent of which involved a firearm (calculated using data from CDC, 2019a). Evidence indicates that 50 percent to 60 percent of all firearm suicides by youth under age 21 involve a handgun, suggesting that minimum age laws that cover all firearms (i.e., long guns and handguns) may have larger effects on suicide rates compared with laws focused on handguns alone (Johnson et al., 2010; Wright, Wintemute, and Claire, 2008; Shah et al., 2000; Grossman, Reay, and Baker, 1999). Indeed, a 2019 study using high-quality data from the Violent Death Reporting System indicated that rifles and shotguns were used in more than half of suicides among adolescent men in rural areas (Hanlon et al., 2019).

The effects of laws requiring a minimum purchase age will depend largely on how youth acquire firearms. Much of the existing evidence on sources of guns to youth comes from surveys of juvenile offenders or high-risk adolescents and suggests that purchases from retailers are relatively rare among adolescents involved with criminal activity. Surveys have found that, among juveniles who have been incarcerated or arrested, youth offenders acquire their firearms through similar sources as adult offenders, with more than 80 percent citing a friend, a family member, or the black market as the means by which they acquired their weapon (Webster et al., 2002; LaFree and Birbeck, 1998). Furthermore, youth intending to use firearms for self-harm may also have easy access through sources other than retailers. An early study of firearms used by students in school-associated firearm deaths (both suicide and homicide) between 1992 and 1999 found that only 9.6 percent of the firearms used in homicide events and none of the firearms used in suicide events were purchased legally (CDC, 2001). These findings indicate that minimum age laws may be effective at limiting youth access to firearms through legitimate retail sources.

However, the effectiveness of minimum age laws is likely to be tempered by the ease with which many youth can obtain firearms from sources other than legal retailers. For example, an analysis of data from the National Fatality Review Case Reporting System found that 88 percent of firearms used in suicides by children aged 10–18 were owned by someone other than the child (Schnitzer et al., 2019). A study of inmates at adult jail and juvenile detention facilities in the city of St. Louis between 2003 and 2007 found that 63 percent of juvenile offenders felt that they would have little or no trouble obtaining a firearm (Watkins, Huebner, and Decker, 2008). Although this level of perceived firearm availability exceeds that found in surveys of the general youth population, a nationally representative survey of adolescents found that one in three respondents lived in a home with a firearm, and 41 percent of those adolescents reported that they had easy access to a firearm and the ability to shoot that firearm (Simonetti et al., 2015). Another study found that 50 percent of male respondents felt that they would have little or no trouble obtaining a gun (Sheley and Wright, 1998).
More-recent and nationally representative data suggest that children’s access to firearms through nonlegal means remains a significant issue. A 2015 survey suggests that up to 7 percent (4.6 million) of U.S. children (defined as those aged 17 or younger) reside in homes where at least one firearm is stored loaded and unlocked, a much higher estimate than reported in 2002, when a nationally representative survey assessed that an estimated 1.6 million children lived in homes with loaded and unlocked firearms (Azrael et al., 2018).

The effects of laws requiring a minimum age of possession will depend on the expected costs youth perceive to be associated with violating such laws, which will likely be influenced by state legal penalties and the level of effort devoted to enforcing the prohibition (Marvell, 2001). Semi-structured interviews with incarcerated adolescent males in 1998 found fear of arrest and incarceration as the most commonly reported reasons for choosing not to acquire or carry a gun (Freed et al., 2001). Still, in 2017, 4.8 percent of high school students (7.7 percent of high school males) reported carrying a gun on at least one day for purposes other than hunting or recreation (Kann et al., 2018). Given the relative importance of the home and family members as a source of guns to juveniles, the most-significant effects of minimum age of possession policies may occur if they create a disincentive for older individuals to keep guns at home or to allow guns in the home to be easily accessed (Marvell, 2001).

Much of the conversation about minimum age restrictions revolves around handguns rather than long guns. This is because handguns are more frequently used than long guns in firearm suicides and violent crime, so, in theory, raising the minimum age for such weapons could decrease violence without impacting lawful activities, such as hunting (Tritch, 2014). More-restrictive minimum age laws could plausibly impact the gun industry by reducing the size of the consumer population and decreasing the ownership and use of guns by youth for hunting or recreational purposes. Overall, hunting participation in the United States has declined dramatically over the past decades, and although data on youth recreational firearm use are limited (Vittes and Sorenson, 2005), estimates from 2015 suggest that 1.8 million youth aged 6–15 engaged in hunting (U.S. Fish and Wildlife Service, 2018). Furthermore, a large majority of adult hunters initiate hunting activities before age 20, and those who have not learned to hunt by age 20 have a very low likelihood of participating in hunting activities as an adult (Duda and Young, 1993). Should minimum age laws reduce initiation of firearm use for hunting or recreational purposes, there could be longer-term effects on these outcomes.

Data on suicides and self-inflicted nonfatal injury stratified by age are readily available; thus, analyses can directly test whether effects of minimum age laws on these outcomes are driven by the relevant age group affected by the policy. For outcomes of violent crime and non-self-inflicted injury, causal analyses could be improved with data that reported the age of the shooter. However, as most data sources report
only the age of the victim,\(^1\) none of the studies we identified that met our inclusion criteria for this policy used this type of data. Methodological approaches could also leverage state variation in the types of guns restricted under the minimum age laws for outcome data that have information on the type of firearm involved. For any analysis, estimates of causal effects would be strengthened with data showing how minimum age laws affected gun purchase or carrying behavior by youth of the affected age group. Although some national surveys (e.g., the Youth Behavioral Risk Surveillance System, National Survey of Drug Use and Health, National Longitudinal Study of Adolescent to Adult Health) ask youth about gun ownership or carrying behaviors, the samples are often limited to high school students, focused on handguns, not representative at the state level, or available for a limited set of years.

### State Implementation of Minimum Age Requirements

As of January 1, 2020, 17 states and the District of Columbia have minimum age requirements that exceed the federal minimum for the purchase of a handgun from unlicensed persons,\(^2\) and eight states and the District of Columbia have minimum age requirements that exceed the federal minimum for handgun possession.\(^3\) Several states also have minimums that are higher than the federal law for long gun purchase and possession.

Eight states and the District of Columbia restrict all handgun sales to individuals aged 21 or older and long gun sales to individuals aged 18 or older. In effect, this

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\(^1\) Exceptions include the Federal Bureau of Investigation’s Supplementary Homicide Reports, which contain age of victim and age of offender for murders when such information is known, and the National Violent Death Reporting System, which contains information for a subset of states on the age of the shooter for non-self-inflicted fatal injuries when such information is known. In 2018, 31 percent of homicides involved a perpetrator of unknown age (Federal Bureau of Investigation, 2019a).


Minimum Age Requirements

raises the minimum age restrictions above those set by federal law in two ways: The age to purchase handguns through private sales is raised from 18 to 21, and the minimum age for private sales of long guns is set to 18. Four states restrict sales for all firearms to those aged 21 or older. Other states set minimums below the federal limits. For instance, Maine imposes a minimum age of 18 for handgun sales and 16 for long gun sales. In practice, these restrictions affect only long gun sales from nondealers, because minimum age requirements for all other sales would be governed by the more-restrictive federal laws.

As mentioned, federal law places no minimum on the age of possession of long guns (18 U.S.C. 922), but several states have imposed such minimums. For instance, 13 states restrict possession of long guns to those aged 18 or older, and Illinois and the District of Columbia restrict long gun possession to those aged 21 or older. The minimum age for possession of a long gun in Alaska, Minnesota, and New York is 16, and it is 14 in Montana.

Effects on Suicide

Research Synthesis Findings

We identified five quasi-experimental longitudinal studies providing evidence on the impact of minimum age requirements on suicide. Marvell (2001) assessed how state and federal laws banning handgun possession by juveniles affected firearm and non-

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9 Alaska Stat. § 11.61.220; Minn. Stat. § 97B.021 (but individuals aged 14 or 15 and with firearm safety certificates may possess long guns); N.Y. Penal Code § 265.05.

10 Mont. Code Ann. § 45-8-344.
firearm suicide rates among youth aged 15–19 (an age group that overlaps the minimum age threshold of 18) based on data spanning 1979 to 1998. Using fixed-effects models that controlled for national trends, average state effects, a lag of the dependent variable, and state-specific linear trends, the author jointly estimated the effects of (1) state laws prohibiting juvenile handgun possession that were in place prior to the 1994 federal minimum age restriction, (2) state laws prohibiting juvenile handgun possession enacted the same year as the 1994 federal minimum age restriction, and (3) the federal ban on juvenile handgun possession. Almost all estimated effects were uncertain, with only one marginally suggestive effect ($p = 0.18$) for an increase in nonfirearm suicide following state bans that occurred prior to the year of the federal law. However, the mechanism through which age restrictions would affect nonfirearm suicides without correspondingly influencing firearm suicides is unclear, particularly given the lower case fatality rate for most nonfirearm suicide methods. Because of the large number of estimated parameters relative to observations (about one to seven), the model may have been overfit, resulting in estimates and confidence intervals (CIs) that are unreliable indicators of the true causal effects of the laws.

Using data from 1976 to 2001, Webster et al. (2004) examined the effect of state-level changes in minimum purchase and possession age laws on suicide rates among those aged 14–17 and 18–20. The authors used negative binomial regression models that employed generalized estimating equations and that included state fixed effects and other covariates. They found uncertain effects of the laws on suicide rates among those aged 14–17. However, states that increased the minimum purchase age to 21 saw a statistically significant decrease in firearm suicides among those aged 18–20, but the authors found uncertain effects of the laws on total or nonfirearm suicides. They found that the three states that increased the age of handgun possession to 21 experienced a statistically significant increase in total suicides among those aged 18–20, accounted for, in part, by a suggestive increase in firearm suicides in this group. The authors suggested that this result was weakly estimated, having been based on just three states, two of which implemented their laws in the final years of the study period, meaning there was little time over which to observe changes in state suicide rates attributable to the law. These limitations raise valid questions about whether the observed effects are attributable to raising the age of possession of handguns to 21 or to other factors affecting these states’ suicide rates. Finally, the authors examined the effect of federal minimum age of possession and purchase of handguns among states that previously had lower minimum age laws compared with those for which the federal law did not raise the minimum ages. These analyses identified a suggestive increase in total suicides among those aged 14–17 from raising the federal minimum possession age but only uncertain effects for other outcomes associated with raising the minimum age to purchase handguns among this age group.

Using a similar empirical model but focusing on suicides by males and the 1995 to 2004 period after the federal minimum age law, Andrés and Hempstead (2011)
assessed how state bans on the purchase of firearms by minors affected suicide rates in four age groups (15–24, 25–44, 45–64, 65 or older). The authors found that bans on the purchase of firearms by minors were associated with significantly lower rates of suicide among males aged 15–44, a reduction of 13 to 14 percent, and the relationship of the laws to suicide rates among the older adult men was uncertain. However, the study models had a ratio of estimated parameters to observations of about one to seven, and the authors did not appear to adjust standard errors to account for serial correlation, threatening the validity of their estimated effects and associated CIs.

Gius (2015b) examined how both state-specific laws for minimum age for firearm possession and federal laws for minimum age for handgun possession implemented in 1994 affected suicides by those aged 19 or younger. This analysis controlled for several state-level sociodemographic characteristics and the enactment of child-access prevention laws between 1981 and 2010. Its results suggest that state-level minimum age restrictions had uncertain effects on suicide. The weighted least-squares statistical model is not likely to produce reliable estimates for the nonlinear outcome of suicide rates, meaning the model’s estimates and their standard errors may be unreliable (Freedman, 2006). The study’s estimate for the federal minimum age law for handgun possession passed in 1994 did not meet our inclusion criteria, because, as specified in this model, there was no comparison group that did not get the identical intervention in 1994.

Rosengart et al. (2005) used a similar approach to model the effects of state laws between 1979 and 1998, when “seven states adopted and two states repealed a law restricting the minimum age for the private purchase of a handgun to 21 years, [and] five states adopted laws restricting the minimum age for the private possession of a handgun to 21 years” (p. 78, emphasis added). In these models controlling for state fixed effects, time trends, state-level variation in poverty and demographic factors, and three other firearm laws (but not the federal 1994 law imposing a minimum age requirement for handgun possession), they found mostly uncertain effects of these laws on the firearm suicide rate. However, they did find suggestive effects consistent with minimum possession age laws increasing the total suicide rate among those under age 20, as well as minimum purchase age laws increasing total suicides among those aged 20 or older. These models had limited information to use in identifying causal effects of these laws because relatively few states changed one or both laws over the study period; in addition, every state but one that raised its minimum age for possession did so the same year it implemented a minimum purchase law, making the effects of these laws confounded. Moreover, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning the model may have been

---

11 The other laws modeled simultaneously were “one-gun-a-month” laws; “shall-issue” laws, otherwise known as right-to-carry laws, which guarantee the right to a concealed-carry permit for all citizens who are not prohibited from possessing a firearm (see Chapter Eighteen); and “junk-gun” laws, which ban the sale of certain cheaply constructed handguns (see Chapter Fifteen).
overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws.

Figure 3.1 displays the incidence rate ratios (IRRs) and CIs associated with the minimum age requirements examined in these studies. We do not present estimates of the federal minimum possession age from Gius (2015b) because they do not meet our criteria for inclusion. Estimates of the federal minimum purchase age and minimum possession age laws from Webster et al. (2004) are included because, although details of the model are not specified, the study appears to satisfy our inclusion criteria based on the authors’ following statement: “The federal law establishing a minimum legal age for handgun purchase and possession was assumed to affect only states that, prior to the federal law, either had no minimum-age law of this type or had a law that established a minimum legal age younger than 18 years” (p. 595). Similarly, estimates of the federal minimum possession age laws from Marvell (2001) are included because the author estimated the effect of the federal law using identifying variation from states that had not already restricted handgun possession by juveniles by the time the federal policy change went into effect.

How to Read Forest Plots

The forest-plot figures in this report show the standardized effect sizes (or incidence rate ratios [IRRs]) and their 95-percent confidence intervals (CIs) for each outcome, by policy or law, as revealed in the studies examined. (See Chapter Two for details on how we calculated these effect sizes.) An effect size of 1.00 indicates that, after a state passes the law, we would expect the outcome (e.g., suicide or firearm suicide) to be unaffected. That is, the rate of suicide after the law was passed would be 1.00 times the rate before the law was passed. An effect size of less than 1.00 indicates that the law appears to reduce the outcome. For example, if the effect size for the effect of background checks on suicides were 0.92, we would expect the suicide rate to fall to 0.92 times the rate prior to passage of the background check law. Conversely, an effect size of more than 1.00 indicates that the law appears to increase the outcome by a factor equivalent to the effect size value. When the CIs do not include the value of 1.00, the estimated effect is statistically significant at \( p < 0.05 \).

Where relevant, we note in the text when individual analyses relied on methods that we thought might produce inaccurate estimates or CIs. IRRs corresponding to analyses for which we expressed such concerns are indicated by blue squares in the forest plots. Green circles indicate IRR estimates about which we raise no specific methodological concern. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. Information on the source data and methodological ratings is available in Appendix B.
Figure 3.1
Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>State minimum purchase age</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Total, aged 14–17</td>
<td>1.04 [0.90, 1.21]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Total, aged 18–20</td>
<td>0.97 [0.91, 1.05]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 15–24</td>
<td>0.87 [0.76, 1.00]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 25–44</td>
<td>0.86 [0.83, 0.90]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 45–64</td>
<td>0.99 [0.84, 1.16]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 65+</td>
<td>1.03 [0.89, 1.19]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Firearm, aged 14–17</td>
<td>1.04 [0.87, 1.16]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Firearm, aged 18–20</td>
<td>0.91 [0.83, 1.00]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Nonfirearm, aged 14–17</td>
<td>1.05 [0.85, 1.31]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Nonfirearm, aged 18–20</td>
<td>1.05 [0.94, 1.17]</td>
</tr>
</tbody>
</table>

| State minimum purchase age of 21         |                  |                |
| Rosengart et al. (2005)                  | Total            | 1.02 [0.98, 1.07] |
| Rosengart et al. (2005)                  | Total, aged 0–19 | 1.10 [0.94, 1.29] |
| Rosengart et al. (2005)                  | Total, aged 20+  | 1.04 [0.99, 1.10] |
| Rosengart et al. (2005)                  | Firearm          | 1.00 [0.94, 1.06] |
| Rosengart et al. (2005)                  | Firearm, aged 0–19 | 0.94 [0.80, 1.06] |
| Rosengart et al. (2005)                  | Firearm, aged 20+ | 1.02 [0.96, 1.08] |

| State minimum possession age              |                  |                |
| Webster et al. (2004)                     | Total, aged 14–17 | 0.97 [0.90, 1.05] |
| Webster et al. (2004)                     | Total, aged 18–20 | 1.13 [1.01, 1.27] |
| Webster et al. (2004)                     | Firearm, aged 14–17 | 1.02 [0.92, 1.12] |
| Webster et al. (2004)                     | Firearm, aged 18–20 | 1.14 [0.98, 1.34] |
| Gius (2015b)                              | Firearm, aged 0–19 | 0.98 [0.93, 1.02] |
| Webster et al. (2004)                     | Nonfirearm, aged 14–17 | 0.93 [0.82, 1.05] |
| Webster et al. (2004)                     | Nonfirearm, aged 19–20 | 1.07 [0.90, 1.27] |

| State minimum possession age of 18, prior to federal law |                  |                |
| Marvell (2001)                                         | Firearm, aged 15–19 | 0.99 [0.88, 1.11] |
| Marvell (2001)                                         | Nonfirearm, aged 15–19 | 1.14 [0.94, 1.37] |

| State minimum possession age of 18, year of federal law |                  |                |
| Marvell (2001)                                         | Firearm, aged 15–19 | 1.01 [0.86, 1.17] |
| Marvell (2001)                                         | Nonfirearm, aged 15–19 | 1.02 [0.81, 1.29] |

Figure continued on the next page
Conclusions

We identified three qualifying studies that examined how suicide rates were affected by laws requiring a minimum purchase age and four that examined how they were affected by laws requiring a minimum possession age.

Minimum age requirements for purchasing a firearm. Webster et al. (2004) found uncertain effects for minimum purchase age laws (with restrictions from ages 16 to 21) on suicides among those aged 14–17 and those 18–20. They also found uncertain effects on firearm suicides among the younger age group but a significant effect consistent with these laws reducing firearm suicides among the older group. When reestimating these effects only for states that set age 21 as the minimum for purchasing a firearm, the authors again found uncertain effects on total suicide rates for the older age group and a significant effect indicating that such laws reduce firearm suicides among those aged 18–20. Using overlapping, but shorter, time-series data, Rosengart et al. (2005) found the effects of laws requiring a minimum age of 21 to purchase a firearm to have uncertain effects on suicides and firearm suicides for all age groups, except for a sug-
gestive effect consistent with these laws increasing total suicides among adults aged 20 or older. In contrast, Andrés and Hempstead (2011) found that laws banning firearm purchases by minors were associated with significantly lower rates of suicide among males aged 15–44.

Considering these findings and an assessment of the relative strengths of these studies, we find inconclusive evidence for how minimum age requirements for purchasing a firearm affect total suicides. Studies of the effect of laws setting 21 as the minimum age of firearm purchase provide limited evidence that such laws may reduce firearm suicides among people aged 20 or younger.

Minimum age requirements for possessing a firearm. Marvell (2001) found uncertain effects of laws banning handgun possession by juveniles under age 18 on firearm and nonfirearm suicide rates among those aged 15–19, with the exception that state bans implemented prior to the year of the federal ban had a suggestive effect consistent with increasing nonfirearm suicides among this age group. Webster et al. (2004) found uncertain effects of minimum possession age laws (with restrictions from ages 14 to 21) on suicides and firearm suicides among those aged 14–17. However, they found that these laws significantly increase suicide rates among those aged 18–20 and found a suggestive effect consistent with increases in firearm suicide rates among this group. For laws requiring a minimum handgun possession age of 21, Rosengart et al. (2005) found uncertain effects on suicides overall and among those aged 20 or older, as well as a suggestive effect consistent with these laws increasing suicides among those under age 20. All effects of these laws on firearm suicides, however, were uncertain. Gius (2015b) found only uncertain effects of state minimum age of possession laws on firearm suicides among those aged 19 or younger.

Considering these findings and an assessment of study strengths, we find inconclusive evidence for how minimum age requirements for possessing a firearm affect total suicides and firearm suicides.
Effects on Violent Crime

Research Synthesis Findings

We identified three studies that met our criteria and provided evidence for the effect of minimum age requirements on violent crime. The earliest study (Marvell, 2001) assessed how state and federal laws banning handgun possession by juveniles (under age 18) affected homicide and other violent crimes based on data spanning 1968 to 1999. Using fixed-effects models that controlled for national trends, average state effects, a lag of the dependent variable, and state-specific linear trends, the author jointly examined the effects of (1) state laws prohibiting juvenile handgun possession that were in place prior to the 1994 federal minimum age restriction, (2) state laws prohibiting juvenile handgun possession enacted the same year as the 1994 federal minimum age restriction, and (3) the federal ban on juvenile handgun possession.

For the early state minimum age laws, the study found small and uncertain effects on firearm-related homicide but significant negative effects on nonfirearm homicides, although the magnitude, precision, and direction of the latter effect varied over different data sets and age groups analyzed. The mechanism through which age restrictions would affect nonfirearm homicides without correspondingly influencing firearm-related homicides is also unclear. The estimated effects for the 1994 laws (implemented either in state statute or through the federal law for states without preexisting minimum age laws) were contradictory: 1994 state laws were estimated to have suggestive effects consistent with increasing firearm and total homicides, while states impacted only by the 1994 federal prohibition experienced significant reductions in firearm and total homicide rates and suggestive reductions in nonfirearm homicide and assault rates. However, the 1994 federal law banning handgun possession by juveniles also established waiting periods and background checks, and other federal policy passed in 1994 created major crime reduction programs (the Violent Crime Control and Law Enforcement Act). If states that had not passed minimum age bans as of 1994 were also differentially impacted by the other changes to federal firearm policy (which may not be adjusted for by including year fixed effects), estimated effects of the federal change may be capturing the effects of this broader suite of policies.

Rosengart et al. (2005) analyzed state-level data over a shorter time frame, from 1979 to 1998, and examined the effects on violent crime of two types of state minimum age laws: (1) restricting handgun purchases to those aged 21 or older and (2) restricting private handgun possession to those aged 21 or older.

The authors controlled for whether a state had a shall-issue (otherwise known as right-to-carry) provision (see discussion in Chapter Eighteen), laws prohibiting the sale of low-quality guns (see discussion in Chapter Fifteen), and laws limiting the frequency of gun purchases to one gun per 30 days. The authors found uncertain effects of both types of minimum age laws on total homicide and firearm homicide rates. These models had limited information to use in identifying causal effects of these laws because rela-
tively few states changed one or both laws over the study period; in addition, every state but one that raised its minimum age for possession did so the same year it implemented a minimum purchase age law, making the effects of these laws confounded. Moreover, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning the model may have been overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws.

Rudolph et al. (2015) found a significant effect for a decrease in firearm homicides (and an uncertain effect for nonfirearm homicides) associated with the implementation of a law in Connecticut that established a requirement to have a permit to purchase a firearm and increased the minimum age of handgun purchase from age 18 to age 21. The firearm homicide rate after passage of both provisions was found to be 63 percent of what would have been expected without them. However, because the law included both policies simultaneously, the effect attributable specifically to the minimum age law cannot be identified. In addition, because only one state in the analysis experienced the law change, the effects of the law are not well identified. The observed reduction in firearm homicides could be due to the law or to other events occurring in Connecticut around the same time the law passed.

Figure 3.2 displays the IRRs and CIs associated with the minimum age requirements examined in these studies.

**Figure 3.2**
Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State minimum purchase age of 21</strong></td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Total</td>
<td>1.00 [0.94, 1.05]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Total, aged 0–19</td>
<td>0.92 [0.81, 1.05]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Total, aged 20+</td>
<td>1.01 [0.95, 1.06]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Firearm</td>
<td>0.98 [0.91, 1.06]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Firearm, aged 0–19</td>
<td>0.92 [0.80, 1.06]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Firearm, aged 20+</td>
<td>0.99 [0.93, 1.06]</td>
</tr>
</tbody>
</table>

| **State minimum possession age of 21** | |  |
| Rosengart et al. (2005) | Total | 1.02 [0.89, 1.18] |
| Rosengart et al. (2005) | Total, aged 0–19 | 0.98 [0.79, 1.20] |
| Rosengart et al. (2005) | Total, aged 20+ | 1.03 [0.88, 1.20] |
| Rosengart et al. (2005) | Firearm | 1.06 [0.88, 1.27] |
| Rosengart et al. (2005) | Firearm, aged 0–19 | 0.91 [0.72, 1.15] |
| Rosengart et al. (2005) | Firearm, aged 20+ | 1.08 [0.89, 1.31] |

*Figure continued on the next page*
NOTE: The Marvell (2001) study’s model included one- and two-year lags of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
Conclusions
We identified three qualifying studies that examined the effect of minimum age requirements for purchasing or possessing a firearm on total or firearm homicide rates.

Minimum age requirements for purchasing a firearm. Rosengart et al. (2005) found uncertain effects of laws making 21 the minimum age to purchase handguns on homicide rates and firearm homicide rates among all age groups. Rudolph et al. (2015) reported a significant effect consistent with minimum age requirements reducing firearm homicide rates, but they could not attribute this effect solely to a minimum purchase age policy because a permit-to-purchase provision was passed concurrently in the one state evaluated. On the basis of these results, and in consideration of the relative strengths of these studies, we find inconclusive evidence for how minimum age requirements for purchasing a firearm affect total and firearm homicides.

Minimum age requirements for possessing a firearm. Estimates by Marvell (2001) for the effect of laws making 18 the minimum age for possession of handguns on homicides and other violent crimes showed varying effects of the laws depending on whether the prohibitions were implemented through state or federal legislation and whether state prohibitions were established prior to or the same year as the federal policy. Estimates by Rosengart et al. (2005) for the effect of laws making 21 the minimum age for possession of handguns on total and firearm homicides were uncertain for all age groups examined. Therefore, we find inconclusive evidence for how minimum age requirements for possessing a firearm affect total homicides, firearm homicides, and other violent crime.

Effects on Unintentional Injuries and Deaths
Research Synthesis Findings
We identified one longitudinal study with a comparison group that examined the effects of minimum age requirements on unintentional injuries and deaths. Using data from 1981 to 2010, Gius (2015b) examined the effect of the 1994 federal law estab-
lishing a minimum age for handgun possession, as well as other state-specific minimum age requirements for handguns. This model controlled for time and state fixed effects, state-level sociodemographic characteristics, and state-level child-access prevention laws. The author found that state-level minimum age requirements had uncertain effects on unintentional deaths. The weighted least-squares statistical model used in this study may not have been appropriate for the rate outcome, with many values close to zero in state-year observations. The model’s lower bound at zero may result in violations of its assumptions and can yield biased and incorrect parameter estimates and CIs.

Figure 3.3 displays the IRR and CI associated with the minimum age requirements examined in Gius (2015b). The analysis of the federal minimum age of possession law in this study did not meet our inclusion criteria, because, as specified in this model, it appeared that there was no comparison group that did not get the identical intervention in 1994. Therefore, this effect is not included in Figure 3.3.

**Conclusions**

We identified one qualifying study examining the effect of laws requiring either minimum age to purchase or minimum age to possess a firearm. Gius (2015b) found a suggestive effect consistent with minimum possession age laws decreasing unintentional firearm deaths among those aged 19 or younger. Therefore, we find that there is inconclusive evidence that minimum age requirements for possessing a firearm may reduce unintentional firearm deaths.
Effects on Mass Shootings

Research Synthesis Findings

Our search yielded one study that met our inclusion criteria and examined the effects of minimum age requirements on mass shootings. Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2016) estimated the effects of minimum age requirements on a binary indicator for whether a mass shooting occurred in a given state-year. The authors defined mass shootings as incidents in which four or more individuals were killed (excluding the shooter), the occurrence was not connected to criminal activity, and at least three fatally injured victims were not related to the shooter (e.g., family, romantic partner). The authors included two measures of minimum age requirements: (1) an indicator variable for whether laws prohibit vendors from selling handguns to those under age 18 or prohibit those under age 18 from purchasing handguns and (2) an analogous indicator variable for laws that set the minimum age at 21. The authors’ analysis covered 1989–2014 and included controls for state fixed effects, national trends, and a host of other state-level gun policies, as well as time-varying state-level demographic, socioeconomic, and political characteristics. They found uncertain effects of laws setting 18 as the minimum age of purchase on the probability of a mass shooting event occurring, but they found a suggestive effect consistent with laws setting 21 as the minimum age of purchase reducing the likelihood of a mass shooting occurrence in models that included controls for political factors. However, it should be noted that assessing the effects of gun policies on mass shootings was not the primary focus of Luca, Malhotra, and Poliquin (2016), and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a rare dichotomous outcome. Therefore, model assumptions were likely violated, making model estimates and CIs unreliable.

Figure 3.4 displays the IRRs and CIs associated with the minimum age requirements examined in Luca, Malhotra, and Poliquin (2016).
Conclusions

We identified one qualifying study examining how minimum age requirements for purchasing a firearm affect the incidence of mass shootings. Luca, Malhotra, and Poliquin (2016) found that laws setting 18 as the minimum age to purchase a firearm had uncertain effects on mass shooting incidence, but they found a suggestive effect consistent with such laws reducing the incidence of mass shootings when the minimum purchase age is 21 in their model that controlled for political factors. On the basis of this study, we find inconclusive evidence for how minimum age requirements for purchasing a firearm affect mass shootings.

Outcomes Without Studies Examining the Effects of Minimum Age Requirements

We did not identify any research that met our inclusion criteria and examined the effects of minimum age requirements on the following outcomes:

- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Three References


CDC—See Centers for Disease Control and Prevention.


United States Code, Title 18, Section 922, Unlawful Acts.


Federal law prohibits the possession or purchase of firearms by certain individuals who have been adjudicated as mentally ill (18 U.S.C. 922),\(^1\) and, over the past two decades, approximately 2,000 potential gun purchases per year have failed at the background check stage as a result of this prohibition (Federal Bureau of Investigation, 2019c).\(^2\) However, the total number of people covered by this exclusion is not known. In 2017, an estimated 46.6 million adults in the United States had some form of *mental illness*—defined as any “diagnosable mental, behavioral, or emotional disorder, other than a developmental or substance use disorder”—in the past year (Substance Abuse and Mental Health Services Administration, 2018). Of these adults, 11.2 million suffer from a “serious mental illness” that results in substantial impairment in carrying out major life activities. Existing laws that prohibit those with adjudicated mental health conditions from accessing firearms affect a subset of individuals who likely fall into the “serious mental illness” category. Expanding such prohibitions has the potential to affect a much larger subset of individuals who fall into the “any mental illness” category, although broadening the scope of mental health restrictions poses technological, coordination, and legal (i.e., privacy) challenges (Liu et al., 2013).

If individuals with mental illness present a higher violence risk to themselves or others compared with those without mental illness, then restricting their access to firearms should reduce suicides and homicides. The magnitude of these effects will depend primarily on the reliability of the screening process instituted to identify disqualifying mental health conditions, the size of the marginal population affected by the expanded prohibitions, and the likelihood of individuals in that population committing harm to others or to themselves.

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1. The Gun Control Act of 1968 prohibited the sale of firearms to any person who has been “adjudicated as a mental defective or has been committed to any mental institution” (Pub. L. 90-618). This law also prohibits selling to any individual who “is an unlawful user of or addicted to any controlled substance,” a condition that may also be considered a mental health–related prohibition. For the purposes of this chapter, however, we consider mental health–related prohibitions that are not related to substance abuse.

2. The cited data indicate that, between November 30, 1998, and April 30, 2019, 40,698 (2.5 percent) of 1,630,324 total denials for firearm permits were attributed to mental health adjudications (Federal Bureau of Investigation, 2019c).
Epidemiological evidence suggests that a diagnosis of mental illness alone has little relation to risk of interpersonal violence (Swanson et al., 2015); in particular, studies estimate that between 2 percent and 4 percent of all violent behavior may be attributable to mental illness (Corrigan and Watson, 2005; Swanson, 1994). One study found that among a sample of convicted murderers in Indiana, perpetrators with serious mental illness were significantly less likely to have used a firearm compared with other perpetrators (Matejkowski et al., 2014). A study of 82,000 individuals with mental illness in Florida showed that the arrest rate for violent crimes involving a firearm was the same among the study population as the estimated general population rate—approximately 215 arrests per 100,000 people (Swanson et al., 2016). Elevated rates of violence tend to be reported for involuntarily committed patients (Choe, Teplin, and Abram, 2008), but this population is already barred from acquiring firearms through existing federal mental health–related prohibitions. Overall, between 2001 and 2010, less than 5 percent of the 120,000 firearm-related homicides in the United States were committed by individuals diagnosed with a mental illness (Metzl and MacLeish, 2015), suggesting that expanded prohibitions based on mental health status might be expected to affect less than 5 percent of firearm crimes.

Although media coverage often and increasingly links mass shootings with serious mental illness (McGinty et al., 2016, 2014), an analysis of 173 mass shooting events between 2009 and 2017 (Everytown for Gun Safety Support Fund, 2018) reported that in only one incident (0.5 percent) did the perpetrator have a history of mental illness that prohibited purchase of a firearm from a federally licensed dealer; however, a review of cases through 2015 (Fox and Fridel, 2016) found that formal concerns about the mental health of the perpetrator had been previously expressed for 15 cases (11.3 percent), and informal concerns about the shooter’s mental health had been previously expressed for 13 additional cases (9.8 percent). Although mass public shooters are more likely to have a psychotic disorder compared with perpetrators of multiple-victim shootings related to familicide or profit-motivation, the prevalence of severe mental illness among this subgroup is still quite low (Fox and Levin, 2015). Counting less-severe forms of mental illness, Follman, Aronsen, and Pan (2019) found that 59 of the 115 mass public shootings between 1982 and 2019 that were identified by Mother Jones magazine involved a shooter with a history of possible mental health problems.

At the same time, research indicates that individuals with mental disorders are more likely to be victims than perpetrators of violence (Desmarais et al., 2014). One study of persons with severe mental illness (in treatment at mental health agencies in Chicago) found that their annual exposure to violent crime victimization was more than four times higher than rates in the general population (Teplin et al., 2005). Another meta-analysis produced similar results, finding the prevalence of violent victimization among individuals with mental illness to be 24 percent (with estimates of the reviewed studies ranging from 7 percent to 63 percent) (Hughes et al., 2012). Extrapolating this estimate to the national population of individuals with serious mental illness in 2015...
would suggest that approximately 2.3 million individuals with serious mental illness are victims of violent crime each year; however, this is likely an overestimate because most studies sampled individuals who were receiving inpatient or outpatient treatment for diagnosed psychiatric illnesses or focused on severe mental illnesses (such as schizophrenia) (Hughes et al., 2012). For instance, although the National Crime Victimization Survey does not collect information on mental health directly, its estimates suggest that there are about 780,000 cases annually of violent crime against individuals with cognitive disabilities (defined as serious difficulty in concentrating, remembering, or making decisions because of a physical, mental, or emotional condition) (Harrell, 2017). Therefore, expanding the class of prohibited possessors to include more people with severe mental illness may lead to additional victimization because those people have reduced opportunities for defensive gun use. At the same time, such an expansion may decrease violent crime, mass shootings, and suicides carried out by this population.

Indeed, evidence supports that expanding prohibitions associated with mental illness may have larger effects for reducing rates of firearm suicides rather than interpersonal violence. Research has demonstrated a strong link between mental illness and suicide; it is estimated that between 47 percent and 74 percent of suicides are attributable to mental disorders (Li et al., 2011; Cavanagh et al., 2003). A study of 82,000 individuals with mental illness in Florida found that suicide was nearly four times as prevalent among this subpopulation compared with the general population, but firearms were half as likely to be used as a means of suicide; in more than 70 percent of these firearm suicide cases, the individual’s mental health records did not prohibit him or her from obtaining a firearm legally (Swanson et al., 2016). Among individuals in this study who had a mental illness, were eligible to purchase or possess a firearm, and died by suicide, more than half had records of previous short-term involuntary psychiatric holds (under the Florida Mental Health Act of 1971), which suggests that some of these suicides might have been prevented if this population had been prohibited from obtaining a firearm.

To assess the effects of expanded mental health–related prohibitions, the ideal data would distinguish outcomes between those who are affected by the expanded prohibitions and those who are not. This type of analysis would necessitate a detailed database containing rich information on the mental health and adjudication histories of perpetrators of crime or victims of suicide. Because an individual’s medical records are private, it may be particularly difficult to identify firearm-involved crime incidents in which the perpetrator was a prohibited possessor because of mental illness. Nevertheless, three studies examined the effect of implementing the Brady Handgun Violence Prevention Act (the Brady Act) background checks on people with certain mental illness adjudications. Implementation of this law had the effect of expanding the class of mentally ill people who could not purchase a firearm, so we review those studies in this chapter, as well as in Chapter Eight (on background checks).
State Implementation of Prohibitions Associated with Mental Illness

As of January 1, 2020, 35 states and the District of Columbia have laws restricting access to firearms by individuals with specific histories of mental illness. Although the laws vary in the language used, several states have basically adopted the same standards as the federal Brady Act (18 U.S.C. 922), which went into effect in 1994 and prohibited the sale of firearms to anyone “who has been adjudicated as a mental defective or has been committed to any mental institution” (understood to refer to involuntary commitment).

In other cases, states have narrower prohibitions than those established in the Brady Act. For example, ten states prohibit firearm possession by only those committed to psychiatric institutions, not those adjudicated as mentally incompetent, while Missouri applies the prohibition only to individuals adjudicated as mentally incompetent. In Michigan, North Carolina, and Oklahoma, the prohibition applies only to handguns. Tennessee prohibits only the transfer of firearms to these prohibited individuals, but the law is silent on whether such individuals may possess a firearm.

In contrast, some states have expanded the prohibitions to apply to a broader population than specified in the Brady Act. California, Connecticut, Illinois, Maryland, and the District of Columbia extended firearm restrictions to individuals who have been voluntarily admitted into psychiatric hospitals. Hawaii has extended the prohibition to those diagnosed with “significant” mental disorders, and California and

---

3 For example, Alabama prohibits “anyone of unsound mind” from owning, possessing, or controlling a firearm. Arkansas prohibits those adjudicated as “mentally ill,” and Illinois’ law applies to anyone adjudicated to have a “mental disability.” See Ala. Code § 13A-11-72; Ark. Stat. § 5-73-103; 430 Ill. Comp. Stat. 65/1.1.


9 Calif. Welf. and Inst. Code § 8100 (while voluntarily in treatment for being a threat to themselves or others); Conn. Gen. Stat. Ann. § 53a-217 (admitted within previous six months); 430 Ill. Comp. Stat. 65/1.1, 65/8 (admitted within past five years), 405 Ill. Comp. Stat. 5/6-103.1; Md. Ann. Code § 5-133 (admitted for more than 30 consecutive days); D.C. Code Ann. § 7-2502.03 (admitted within past five years).
Illinois have widened the class of prohibited possessors in other ways.10 Arizona, Michigan, Oregon, and Virginia have also extended the mental health–related prohibitions to individuals ordered to attend outpatient treatment.11 And some states, such as Arizona and Michigan, have applications that are both narrower and broader than the standards in the Brady Act.

Finally, ten states include standards related to criminal trials that would result in commitment to a mental institution, such as a ruling that someone is incompetent to stand trial, guilty but mentally ill, or not guilty by reason of insanity.12 California, Massachusetts, Rhode Island, and Wisconsin include standards related to the appointment of a guardian or conservator,13 which require prior adjudication of mental defect.

Effects on Suicide

Research Synthesis Findings

We identified three studies examining the effects of mental health–related prohibitions on suicide. Examining state-level data from 1995 to 2004, Andrés and Hempstead (2011) assessed how men’s suicide rates were related to different categories of firearm restrictions, including prohibitions for persons with a history of mental health problems. Using regression models that controlled for state and year fixed effects, state-level socioeconomic and demographic variables, and hunting licenses per capita, they presented analyses stratified by age (15–24, 25–44, 45–64, and 65 or older). They found that prohibitions related to mental health history were associated with significantly lower rates of suicide among men aged 25–44, while estimated effects were positive and suggestive (p = 0.08) for men aged 65 or older and uncertain for youth and men aged 45–64. However, the models had a ratio of estimated parameters to observations of about one to seven, and the authors did not report adjusting standard errors to account for serial correlation, threatening the validity of their estimated effects and associated confidence intervals (CIs).

10 In Hawaii, possession is prohibited by those “diagnosed as having a significant behavioral, emotional, or mental disorder” (Hawaii Rev. Stat. Ann. § 134-7). California has a long list of disqualifiers, including threats of physical violence, various lengths of detention, and court-ordered evaluation and counseling (Calif. Welf. and Inst. Code §§ 8100, 8103, 5200-5213). Illinois restricts possession from those adjudicated by the police as mentally incompetent (430 Ill. Comp. Stat. 65/8).


Using state-level data over a similar period (1996 to 2005), Sen and Panjamapirom (2012) assessed how different types of background checks conducted by states affect suicides. They noted that there is substantial variation in state laws regarding which mental health records must be considered in background checks. The authors characterized variation in whether states can examine relevant mental illness records as part of the background check process. Their regression models included state-level time-varying covariates, state-level suicide rates in 1990, and fixed effects for year and census subregion. Sen and Panjamapirom (2012) found that, compared with background checks that investigate only criminal history, checks of mental health records were associated with significantly lower firearm suicide and total suicide rates. Their estimates suggest that after implementing a state check on mental health records, the firearm suicide rate was 96 percent of the expected rate had this policy not been in effect, and the total suicide rate was 97 percent of the expected rate. However, the authors did not make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects.

Swanson et al. (2016) evaluated how changes in state reporting of gun-disqualifying mental health records to the Federal Bureau of Investigation’s National Instant Criminal Background Check System (NICS) database affected suicide rates among individuals in Florida with a disqualifying mental health condition relative to individuals diagnosed with a serious mental illness but not prohibited from purchasing a firearm. They found no significant difference between suicide rates before and after implementing expanded NICS reporting for the two groups.

Figure 4.1 displays the incidence rate ratios (IRRs) and CIs associated with the mental health–related prohibition policies examined in Sen and Panjamapirom (2012)

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Prohibitions for mental health problems</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 15–24</td>
<td>0.99 [0.95, 1.04]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 25–44</td>
<td>0.97 [0.94, 0.99]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 45–64</td>
<td>0.99 [0.95, 1.04]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Total, men aged 65+</td>
<td>1.04 [0.99, 1.10]</td>
</tr>
<tr>
<td>Background check on mental illness</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>0.96 [0.92, 0.99]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.97 [0.95, 0.99]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.
Conclusions
One study found mixed evidence for the effect of state-level prohibitions related to mental illness on suicide rates among males, with findings of significant reductions for one age group, suggestive increases for another, and uncertain effects for two age groups. Another study found evidence that when states check mental health records as part of the firearm background check process, their rates of firearm suicide and total suicide are reduced by a few percentage points. This study did not examine the effect of expanding mental illness–related prohibitions beyond those in federal law. Instead, it examined how improved compliance with existing federal law concerning mental illness checks affects suicide rates. Because improved compliance has the effect of prohibiting gun purchases by some with mental health conditions who would not previously have been prevented from purchasing a weapon, this study provides limited evidence that prohibitions associated with mental illness can reduce total suicides and firearm suicides. A third study reported finding no effect of expanded NICS reporting for mental illness–related prohibitions on suicide but did not provide detailed results.

Considering these results and the relative methodological quality of the included studies, we find that there is inconclusive evidence for the effect of state or federal laws prohibiting those with a mental illness from buying a gun on total and firearm suicides.

Effects on Violent Crime

Research Synthesis Findings
We identified three studies that examined the effects of implementing background checks for individuals prohibited from purchasing or possessing firearms because of a mental illness. Using state-level data from 1996 to 2005, Sen and Panjamapirom (2012) assessed how different types of background checks conducted by states affect total homicides and firearm homicides. They noted that there is substantial variation in state laws regarding which mental health records must be considered in background checks. The authors characterized variation in whether states can examine relevant mental illness records as part of the background check process. Their regression models included time-varying state-level covariates, state-specific homicide rates in 1990, and fixed effects for year and census subregion.
Sen and Panjamapirom (2012) found that, compared with background checks that examine only criminal history, background checks that include mental illness records are associated with fewer total homicides and firearm homicides. However, only the reductions for total homicides reached conventional levels of statistical significance, and estimates for firearm homicides were suggestive. The authors found that, after implementation of state background checks that included mental illness records, firearm homicide rates declined to 93 percent of the level that would otherwise be expected (see Figure 4.2). However, the authors did not make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects.

Swanson et al. (2013, 2016) merged administrative records from public health and criminal justice agencies to evaluate how changes in state reporting of gun-disqualifying mental health records to the NICS database affected violent crime arrest rates for individuals with a disqualifying mental health condition relative to individuals diagnosed with a serious mental health illness but not prohibited from purchasing a firearm. Swanson et al. (2013) obtained data from 2002 to 2009 for individuals in Connecticut who had been hospitalized for schizophrenia, bipolar disorder, or major depressive disorder. The authors estimated changes in violent crime arrests for individuals with at least one of the mental health adjudications reported to the NICS before and after Connecticut began reporting mental health records in 2007. The authors found a 31-percent decline in the probability of violent crime arrest in their sample of individuals who had a mental health adjudication but no disqualifying criminal conviction. For comparison, the authors also estimated the likelihood of violent crime arrest for individuals with at least one voluntary psychiatric hospitalization but no mental health adjudication (i.e., individuals with serious mental health problems who were not prohibited from purchasing firearms). Relative to the legally disqualified population, the nondisqualified group had lower rates of arrest both before and after the NICS reporting change, but the magnitude of the decrease following NICS reporting was smaller than the reduction seen in the “treated” group with a disqualifying condition. However, no statistical tests were provided to demonstrate that the difference was statistically significant, so we do not include the results of this study in our assessment of available evidence.

Using data from 2002 to 2011, Swanson et al. (2016) employed analogous methods to analyze the effects of NICS reporting changes in 2007 for two Florida counties. The authors similarly found a larger reduction in violent crime arrest rates for individuals with a disqualifying mental health condition relative to individuals with a serious mental health illness that did not legally prohibit firearm acquisition. This difference, a relative decline of 38 percent (see Figure 4.2), was statistically significant. However, estimates became insignificant when the outcome variable was restricted specifically to violent crimes involving firearms, which could indicate the absence of a causal connection or could be due to measurement error in classifying crimes as involving firearms (Swanson et al., 2016).
Figure 4.2 displays the IRRs and CIs associated with the mental health–related prohibition policies examined in these studies. Swanson et al. (2013) did not provide enough information for us to calculate IRRs and CIs for the effect size of interest, so we do not include that study’s results in the figure. The Swanson et al. (2016) estimate is the change from before to after the NICS reporting requirements for legally disqualified individuals relative to the change for nonlegally disqualified individuals.

Conclusions

We identified two qualifying studies that estimated how laws prohibiting gun purchases by those with a mental illness affect violent crime or homicides. Sen and Panjamapirom (2012) found that procedures to enforce state and federal mental health–related prohibitions significantly reduced total homicides. They also found a suggestive effect consistent with these procedures reducing firearm homicides. Swanson et al. (2016) found that enforcement of such federal prohibitions significantly decreased arrests for violent crime offenses in Florida among the targeted population relative to individuals without a disqualifying mental health adjudication.

Considering these results, we conclude that there is limited evidence that prohibitions associated with mental illness may decrease violent crime.

Evidence for this relationship is limited.

Prohibitions associated with mental illness have uncertain effects on total homicides and firearm homicides.

Evidence for this relationship is inconclusive.
some state or federal mental health–related prohibitions on gun ownership reduce violent crime. Evidence for the effect of these prohibitions on total homicides and firearm homicides specifically is inconclusive.

Outcomes Without Studies Examining the Effects of Prohibitions Associated with Mental Illness

We did not identify any research examining the effects of mental health–related prohibitions on the following outcomes:

- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Four References


United States Code, Title 18, Section 922, Unlawful Acts.
The presence of a firearm in a domestic violence situation can intensify the level of violence and risk of serious injury for the victim of abuse (Campbell et al., 2003). Intimate partner violence (a subset of domestic violence restricted to current or previous romantic partners) that involves a firearm is 12 times more likely to result in death than similar incidents that do not involve a firearm (Saltzman et al., 1992). Domestic abusers with guns also pose a threat to individuals outside their family unit (Smucker, Kerber, and Cook, 2018). Intimate partner homicides are often accompanied by one or more additional victims, as well as the perpetrator’s suicide (Smith, Fowler, and Niolon, 2014; Logan et al., 2008).

Although individuals convicted of domestic violence felonies have been prohibited from possessing or purchasing firearms and ammunition since the Gun Control Act of 1968 (Pub. L. 90-618), two key pieces of federal legislation have sought to impose additional restrictions to address the risks that domestic abusers with guns pose to their families and society more broadly. In 1994, Congress enacted the Violent Crime Control and Law Enforcement Act (Pub. L. 103-322), making it illegal to possess or receive a firearm while subject to a nontemporary restraining order protecting an intimate partner or the child of an intimate partner (Vigdor and Mercy, 2006). Subsequently, in 1996, the Lautenberg Amendment (Pub. L. 104-208) to the Gun Control Act of 1968 extended the prohibition on possession of a firearm by domestic violence offenders to anyone who has been convicted of a misdemeanor crime of domestic violence.\footnote{Federal law defines a \textit{misdemeanor crime of domestic violence} as a misdemeanor offense under federal, state, or tribal law that has, as an element, the use or attempted use of physical force, or the threatened use of a deadly weapon, committed by a current or former spouse, parent, or guardian of the victim, by a person with whom the victim shares a child in common, by a person who is cohabiting with or has cohabited with the victim as a spouse, parent, or guardian, or by a person similarly situated to a spouse, parent, or guardian of the victim. (18 U.S.C. 921)} Because of how federal law defines an intimate partner, these policies can be applied to current or former spouses, a person who shares a child in common with the victim, or a person who is currently cohabiting or previously cohabited with the victim. Thus, dating partners who have never cohabited are not covered by these laws.
Both before and after the 1994 and 1996 federal law changes, many states enacted additional legislation designed to reduce firearm access for accused and convicted domestic abusers. Most commonly, these state laws mirror federal regulation—barring firearm ownership and possession among those served with domestic violence restraining orders (DVROs) and those convicted of a misdemeanor crime of domestic violence. Although federal law applies at the state level, mirroring laws can streamline the process of implementing federal regulations at the state level by clearly delineating authority for firearm removal and a process for surrendering firearms (Cherney et al., 2019; Gold, 2003).

Furthermore, because state law may define a domestic partner more broadly than federal law does, mirroring state policies for those convicted of a domestic violence misdemeanor or subject to a DVRO may apply to a broader class of individuals. For instance, several states expanded on the federal definition of intimate partner by also including same-sex couples and dating partners who have not cohabited. It is unclear the extent to which a broader designation of intimate partner would expand the pool of prohibited possessors, but crime statistics show that a high proportion of women killed in domestic violence incidents are killed by their unmarried partner. In 2008, the proportion of intimate partner homicides committed by a spouse (46.7 percent) was nearly equal to the proportion committed by a boyfriend or girlfriend (48.6 percent) (Cooper and Smith, 2011). Although it is possible that some of these unmarried partners were cohabitants (and thus would be covered by federal law), it is likely that some of these relationships would not be covered by federal law.

States have also expanded domestic violence–related prohibitions to include ex parte DVROs, which are orders put into effect before the defendant attends a hearing to defend himself or herself, reducing the likelihood that the defendant will access firearms during the most lethal moment of an abusive relationship: when the abused party tries to leave the abuser (Langhinrichsen-Rohling, 2005; Langhinrichsen-Rohling et al., 2002). Ex parte orders typically last from one to two weeks to ensure that the defendant receives a hearing without significant delays. Some states also allow victims of harassment or stalking to apply for orders of protection that bar firearms, and some impose firearm prohibitions to those convicted of stalking misdemeanors.

Critics of ex parte provisions for DVROs object to how they could deny gun owners their rights without due process (i.e., before a defendant has the opportunity to state his or her side of the case) (National Rifle Association, Institute for Legislative Action, 2019a). Furthermore, ex parte orders typically require only a preponderance of evidence that the defendant poses a risk—a low standard of proof—so they could be abused to harm a domestic partner, such as one whose job requires him or her to carry a weapon (Sullum, 2019).

Finally, although federal law provides no guidance on how firearms should be removed once a person is barred from owning or possessing one, several state laws have established enforcement mechanisms. Some states specify to whom abusers must surrender their weapons while the order is in effect, such as the police or other designated
third parties. Some states also require or allow law enforcement officials to remove the firearms if the person fails to surrender them. For more information about firearm surrender and seizure laws, see Chapter Six.

Because the presence of a firearm in a domestic violence situation increases the likelihood that domestic violence will result in homicide (Campbell et al., 2003), prohibiting domestic abusers from accessing firearms is a strategy for reducing domestic violence–related injuries and homicides. Component parts of each law, including ex parte provisions and effective surrender guidelines, may increase the effectiveness of the law by removing firearms from abusers at the time victims are most at risk and by ensuring that abusers are not able to keep guns illegally.

Domestic violence–related firearm laws may also affect suicide rates. Research suggests that intimate partner homicides in which firearms are the primary weapon are more likely to result in the suicide of the perpetrator than are intimate partner homicides that do not involve firearms. Approximately 90 percent of intimate partner homicides that end with the perpetrator’s suicide are committed with firearms (Logan et al., 2008). In addition, access to a firearm may decrease the time abusers have to consider their behavior in a highly emotional situation (Smucker, Kerber, and Cook, 2018). Thus, removing a firearm from a domestic violence incident may lower the likelihood of not only the victim’s death but also the perpetrator’s suicide.

There is some evidence that mass shooters often have a history of domestic violence, so domestic violence–related firearm laws could affect mass shooting incidents. From 2009 to 2016, 54 percent of mass homicides (defined as four or more individuals killed in one incident) started with or included domestic or family violence (e.g., a man kills his wife and goes on to kill others in a mass attack), and all were committed with firearms (Everytown for Gun Safety Support Fund, 2017b). An earlier analysis of mass shootings (defined as four or more individuals, not including the offender, murdered with firearms in one incident) from 1999 to 2013 found that 40 percent involved familialicide; additionally, about 20 percent of all mass public shootings involved a domestic dispute as a contributing factor (Krouse and Richardson, 2015). It is possible, therefore, that prohibitions associated with domestic violence could disarm a potential mass shooter and prevent a mass shooting.

The presence of firearms in a domestic violence situation can also threaten law enforcement officers. A 2013 study of their deaths found that domestic dispute calls were among the deadliest for these officers. Between 1996 and 2010, 116 law enforcement officers were killed in the United States while responding to a domestic disturbance call, accounting for nearly 15 percent of all officer homicides during this time (Kercher et al., 2013). Domestic disturbances were the third most common encounter resulting in such homicides, and nearly all (94 percent) of these were firearm homicides. Limiting domestic abusers’ access to guns may reduce the lethality of such calls for law enforcement officers and reduce officer-involved shootings more generally. If an officer does not feel threatened by a suspect because he or she is not armed with a
gun, the officer may be less likely to use lethal force in the situation. Indeed, research reveals that most law enforcement homicides of civilians are of civilians who are armed with firearms (Hemenway et al., 2019a).

Alternatively, the process or threat of removing a firearm may increase tensions in a domestic violence situation, which may increase the risk of violence to the victim, officers responding to the situation, and the abuser. Moreover, some victims may fear retaliation and thus not come forward with a domestic violence charge if they know that it could result in a partner’s firearm being removed (Vittes et al., 2013).

It is unclear how domestic violence–related firearm laws would significantly affect defensive gun use or the gun industry. If implemented correctly, these laws should remove firearms only from perpetrators, not victims seeking firearms for self-defense; yet the perpetrators would then no longer have the ability to defend themselves with a firearm in a threatening situation either. Similarly, these laws are unlikely to affect sufficient numbers of gun buyers to have a material effect on gun sales or the gun industry.

To understand the potential effect size of these policies, it would be helpful to have information on the size of the affected population and the extent to which the laws succeed in restricting access to firearms by perpetrators of violence. Research could benefit from detailed information about state implementation of these policies because, although states may have similar laws, each state and even counties within a state may have very different processes for ensuring that domestic abusers do not access firearms (Zeoli et al., 2019).

Furthermore, existing research struggles with a lack of comprehensive national data on intimate partner homicides and other violent crimes. Such data sets as the Supplementary Homicide Reports database, which was the data set used in all but one of the studies we identified on the association of domestic violence–related prohibitions with violent crime, often contain incomplete accounts of homicides in the state, and details about the incident, such as whether the person killed or was killed by his or her partner, may also be lacking. Representative data on nonfatal gun use in intimate partner violence—such as on the use of guns to threaten, intimidate, coerce, or nonfatally injure—are substantially more limited (Sorenson and Schut, 2018). Of course, stronger data would strengthen research across all domains outlined in this report.

State Implementation of Prohibitions Associated with Domestic Violence

As of January 1, 2020, 42 states and the District of Columbia have laws prohibiting the possession of firearms by individuals subject to DVROs. Of those, 18 states prohibit

2 Alabama, Alaska, Arizona, California, Colorado, Connecticut, Delaware, Florida, Hawaii, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oregon, Pennsyl-
possession when an order is issued ex parte. Thirty-eight states have expanded the applicability of the DVRO firearm prohibition beyond the definition in federal law to include individuals in a dating relationship regardless of whether they cohabited or have a child in common. While some states prohibit possession of firearms to individuals subject to DVROs as a matter of course, others leave discretion to the judge on whether the person subject to the order should be allowed to possess a firearm; this judicial discretion can apply to standard DVROs, as well as ex parte orders.

Some state laws assert that a judge may include a firearm ban if one or more conditions are met (for example, if the person poses an immediate risk to others or if a firearm was previously used in an assault on a family member). Additionally, in states where a DVRO includes a firearm ban, some states provide judges with the discre-

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tion to decide whether the subject of the order must surrender his or her existing firearms to the state or provide evidence that the weapon was given to another person for safekeeping; eight states allow judges to decide whether a firearm can be removed by state law enforcement from the subject of the order if he or she does not surrender the weapon as instructed.\footnote{California, Hawaii, Illinois, Massachusetts, New Hampshire, and New Jersey allow removal in regular and ex parte cases. See Calif. Penal Code §§ 1524, 18250; Hawaii Rev. Stat. Ann. § 134-7.3(b); 725 Ill. Comp. Stat. 5/112A-14; Mass. Gen. Laws Ch. 209A, §§ 1, 3B; N.H. Rev. Stat. Ann. § 173-B:4, 5i; N.J. Stat. Ann. §§ 2C:25-28(i), 29(b). Delaware and New Mexico allow removal for regular cases. See 10 Del. Code § 1045(a)(11); N.M. Stat. § 40-13-5(f).}


The information is then included in the National Instant Criminal Background Check System, and, if a convicted individual attempts to purchase a firearm from a federally licensed firearm dealer, he or she should be unable to complete the purchase. At least one state’s laws require notifying the person who filed the restraining order or was the victim of the misdemeanor crime of domestic violence that the abuser attempted to purchase a firearm.\footnote{Wash. Rev. Code (ARCW) § 36.28A (added by 2017 c 261 § 5).}
Effects on Suicide

Research Synthesis Findings
We identified one study that met our inclusion criteria and examined how prohibitions on gun possession associated with a domestic violence conviction affected suicide. Using data from 1995 to 2004, Andrés and Hempstead (2011) assessed the association of men’s suicide rates with several categories of firearm restrictions, including prohibitions for persons with domestic violence convictions. Using regression models that controlled for state and year fixed effects, state-level socioeconomic and demographic variables, and hunting licenses per capita, the authors found that prohibitions related to domestic violence conviction were associated with significantly lower rates of suicide among men aged 45–64; the estimated relationship was suggestive and negative for men aged 65 or older and uncertain for men younger than age 45. However, the models had a ratio of estimated parameters to observations of about one to seven, and the authors did not appear to adjust standard errors to account for serial correlation, threatening the validity of their estimated effects and associated confidence intervals (CIs).

Rather than estimating the effects of laws related to DVROs and domestic violence misdemeanors, Sen and Panjamapirom (2012) examined the effect of whether a state, in its background check process, checks on restraining orders and misdemeanors. Controlling for year and census subregion fixed effects, baseline state suicide rates, and numerous state-level time-varying covariates, the authors found that checks on misdemeanors were associated with significantly reduced rates of firearm suicide but had an uncertain relationship with total suicide rates; effects of checks on restraining orders were also uncertain (see Figure 5.1). However, the authors did not appear to make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects.

Figure 5.1 displays the incidence rate ratios (IRRs) and CIs for estimated effects associated with prohibitions related to domestic violence from these studies.
One study found evidence of a negative relationship between prohibitions associated with domestic violence convictions and suicide rates among older men, with the effects being significant for suicide among men aged 45–64 and suggestive for men aged 65 or older (Andrés and Hempstead, 2011). Another study found that implementing a background check system that uses information on restraining orders had uncertain effects on total and firearm suicide rates, while using information on misdemeanor offenses in the background check system produced significant negative effects on firearm suicide but uncertain effects on total suicide (Sen and Panjamapirom, 2012). Considering the strength of evidence from these two studies, we find inconclusive evidence for how laws establishing firearm prohibitions for domestic violence offenders affect total and firearm suicides.
Effects on Violent Crime

Research Synthesis Findings

We identified six qualifying studies that examined the relationship between firearm prohibitions tied to domestic violence and violent crime outcomes. All but one of these studies (Sen and Panjamapirom, 2012) used substantially similar data sets, but over different time frames, and all but two (Sen and Panjamapirom, 2012; Vigdor and Mercy, 2006) focused only on intimate partner or domestic homicides as the outcome of interest. The state laws analyzed in our included literature for this class of policies primarily cover firearm prohibitions associated with DVROs and domestic violence misdemeanors, although two studies also analyze firearm prohibitions associated with stalking offenses, which are often associated with conflict involving intimate partners or former partners (indeed, Smith et al., 2017, found that 61.5 percent of women who had been stalked were stalked by a current or former intimate partner). Most of the studies reviewed here also considered the role of firearm surrender and seizure laws in their analyses, and those findings are discussed in Chapter Six.

The earliest of these studies (Vigdor and Mercy, 2006) analyzed the effects of laws that prohibit people under a DVRO from purchasing or possessing a gun and laws that prohibit people who have been convicted of a misdemeanor domestic violence offense from possessing a gun. The authors’ state-level analysis of intimate partner homicide rates from 1982 to 2002 used negative binomial regression and controlled for state and year fixed effects, unilateral divorce laws, other gun policies, per capita ethanol consumption, arrest rates, and state-level demographic characteristics. They found that DVRO laws resulted in significantly lower rates of total and firearm-specific intimate partner homicide, with larger effects for female victims and for DVRO policies that prohibited both purchase and possession (versus possession only). The results for the effects of prohibitions for domestic violence misdemeanants showed suggestive evidence consistent with these misdemeanor prohibitions decreasing firearm-related intimate partner homicide by about 8 percent but increasing rape and assault rates (by 8 and 12 percent, respectively).

To evaluate whether these observed effects were due to DVRO laws or to other unmeasured factors, the authors examined whether similar effects were observed for outcomes not expected to be affected by the DVRO laws. These analyses showed that the DVRO effects were found only for intimate partner homicide; estimated effects of DVRO-related prohibitions were uncertain for the outcomes of stranger homicide, rape, robbery, and assault (see Figure 5.2).

Raissian (2016) examined the 1996 Lautenberg Amendment, federal legislation that extended prohibited possessor status to those convicted of misdemeanor domestic violence offenses and that stipulated a mechanism for requiring that they surrender all firearms. Using a linear model controlling for state and year fixed effects, state-specific
linear trends, and a host of state-level covariates, the author identified the effect of the federal law by exploiting states’ preexisting variation in assault statutes, which, because of imprecise language in the Lautenberg Amendment, affected whether the new federal prohibitions applied to domestic violence misdemeanants. In 2009, the U.S. Supreme Court corrected the ambiguity in *United States v. Hayes*. Raissian (2016) found that firearm-related intimate partner homicides and other family homicides declined when domestic violence misdemeanants were prohibited from possessing firearms and were required to surrender any in their possession. However, because this study evaluated a policy change that simultaneously required firearm surrender and expanded the prohibited class, it could not disaggregate the separate effects of these provisions.

Díez et al. (2017) assessed how intimate partner homicide rates are influenced by prohibitions of firearm possession by persons convicted of a misdemeanor or subject to a restraining order related to intimate partner violence, as well as by a prohibition of firearm possession by persons convicted of stalking. Their analyses, which covered 1991 to 2015, used generalized estimating equations to estimate negative binomial models that controlled for national trends, the lagged intimate partner homicide rate, other violent crime outcomes, state-level sociodemographics, and average region effects. Exploratory models considering each of the firearm laws separately found uncertain effects for prohibitions related to stalking convictions and suggestive negative associations between prohibitions for domestic violence misdemeanants and firearm-related intimate partner homicide (regardless of relinquishment provisions). The negative relationship of DVRO policies with intimate partner homicide rates was suggestive in the absence of firearm relinquishment provisions but was large and statistically significant in the presence of relinquishment requirements, indicating a 10-percent reduction in total intimate partner homicide rates in the authors’ final models.

Zeoli et al. (2018) examined a similar set of policies as Díez et al. (2017), using data from 1980 to 2013 to examine the association of intimate partner homicide with four state laws that prohibit firearm access for domestic violence offenders: (1) prohibitions attached to DVROs; (2) prohibitions against persons convicted of misdemeanor crimes of domestic violence; (3) prohibitions against persons convicted of misdemeanor stalking; and (4) laws designating some stalking crimes as felonies, which could serve to prohibit firearm purchase and possession for domestic violence offenders convicted

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12 The Lautenberg Amendment applied to those charged with assault under a “domestic violence assault” statute. However, at the time of passage, only one-third of states had a specific domestic violence assault statute. The remaining two-thirds had a “general assault” statute, meaning that any assault—regardless of the relationship between the individuals—was treated the same way. Because the Lautenberg Amendment did not apply to those convicted under a general assault statute, two-thirds of states were unaffected by the change in federal law. This ambiguity was clarified in the 2009 decision by the U.S. Supreme Court in *United States v. Hayes*, which stipulated that the Lautenberg Amendment subsequently applied to all assault charges in which the two individuals involved were intimate partners (as defined by the law). See Nelson, 1999.
under the felony charge. The models used in this study are similar (although not equivalent) to those used in an earlier study by the same lead author that used city-level data from 1979 to 2003 to examine how firearm prohibitions for domestic violence offenders affected intimate partner homicide rates (Zeoli and Webster, 2010).

Zeoli et al. (2018) found that state DVRO restrictions were associated with significantly lower rates of total and firearm-related intimate partner homicide. In contrast, the authors found that prohibitions against persons convicted of misdemeanor crimes of domestic violence were associated with a suggestive increase in firearm-related intimate partner homicide, while effects for total intimate partner homicide were uncertain. These findings are aligned with but more precisely estimated than those of the earlier study (Zeoli and Webster, 2010). Furthermore, when examining the specific provisions of DVRO policies, Zeoli et al. (2018) found that the benefits of the laws accrued largely in states that included dating partners within the scope of DVRO restrictions and that included ex parte DVROs. Alternatively, the findings for prohibitions related to stalking misdemeanors showed that the laws were associated with significant increases in overall intimate partner homicide rates and suggestive increases in firearm-related intimate partner homicide rates; stalking felony laws had uncertain effects.

Finally, rather than estimating the effects of laws related to DVROs and domestic violence misdemeanors, Sen and Panjamapirom (2012) examined the effect of whether a state, in its background check process, conducts checks on restraining orders and misdemeanors. Controlling for year and census subregion fixed effects, baseline state suicide rates, and numerous state-level time-varying covariates, the authors found that checks on restraining orders were associated with significantly reduced rates of total and firearm homicide rates. State background checks on misdemeanor offenses showed uncertain effects (see Figure 5.2). However, the authors did not appear to make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects.

Figure 5.2 displays the IRRs and CIs for estimated effects associated with prohibitions related to domestic violence from these studies.

Zeoli et al. (2018) also estimate the effects of the federal DVRO-related prohibition and federal prohibitions for individuals convicted of misdemeanor crimes of domestic violence, but these analyses do not meet our inclusion criteria because they failed to include a comparison group. The authors’ estimated effects for laws requiring firearm surrender at the scene of a domestic violence incident are described in Chapter Six.
Figure 5.2  
Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>State DVRO prohibition</td>
<td>Intimate partner homicide rate</td>
<td></td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Total</td>
<td>0.90 [0.83, 0.97]</td>
</tr>
<tr>
<td>Zeoli &amp; Webster (2010)</td>
<td>Total</td>
<td>0.81 [0.68, 0.95]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Total</td>
<td>0.92 [0.86, 0.99]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Total (no surrender requirement)</td>
<td>0.93 [0.87, 1.00]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Total (surrender requirement)</td>
<td>0.89 [0.83, 0.96]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Firearm</td>
<td>0.67 [0.78, 0.97]</td>
</tr>
<tr>
<td>Zeoli &amp; Webster (2010)</td>
<td>Firearm</td>
<td>0.75 [0.62, 0.92]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Firearm</td>
<td>0.91 [0.84, 0.99]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Firearm (no surrender requirement)</td>
<td>0.94 [0.85, 1.03]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Firearm (surrender requirement)</td>
<td>0.85 [0.77, 0.94]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Stranger homicide</td>
<td>1.01 [0.81, 1.26]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Rape</td>
<td>1.00 [0.93, 1.08]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Robbery</td>
<td>1.01 [0.91, 1.12]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Assault</td>
<td>1.00 [0.88, 1.14]</td>
</tr>
<tr>
<td>Background check for restraining orders</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>0.91 [0.85, 0.98]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.87 [0.79, 0.95]</td>
</tr>
</tbody>
</table>

State DV misdemeanor prohibition

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Total</td>
<td>1.08 [0.92, 1.27]</td>
</tr>
<tr>
<td>Zeoli &amp; Webster (2010)</td>
<td>Total</td>
<td>1.08 [0.91, 1.27]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Total</td>
<td>1.00 [0.92, 1.09]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Total (no surrender requirement)</td>
<td>0.97 [0.87, 1.09]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Total (surrender requirement)</td>
<td>0.96 [0.90, 1.03]</td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Firearm</td>
<td>1.13 [0.94, 1.35]</td>
</tr>
<tr>
<td>Zeoli &amp; Webster (2010)</td>
<td>Firearm</td>
<td>1.18 [0.99, 1.41]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Firearm</td>
<td>0.92 [0.84, 1.02]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Firearm (no surrender requirement)</td>
<td>0.93 [0.86, 1.01]</td>
</tr>
<tr>
<td>Diez et al. (2017)</td>
<td>Firearm (surrender requirement)</td>
<td>0.93 [0.84, 1.02]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Stranger homicide</td>
<td>0.98 [0.81, 1.16]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Rape</td>
<td>1.08 [0.99, 1.18]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Robbery</td>
<td>0.97 [0.86, 1.08]</td>
</tr>
<tr>
<td>Vigdor &amp; Mercy (2006)</td>
<td>Assault</td>
<td>1.12 [0.99, 1.27]</td>
</tr>
</tbody>
</table>

Figure continued on the next page
Conclusions

Six studies met our inclusion criteria and evaluated how firearm prohibitions associated with domestic violence affect violent crime outcomes.

**DVRO policies and intimate partner homicide.** Five studies estimated the relationship of state DVRO-related prohibitions with intimate partner homicide or overall homicide rates. Three of these studies found that state firearm prohibitions related to DVROs resulted in significantly lower rates of total and firearm-related intimate partner homicide (Zeoli et al., 2018; Zeoli and Webster, 2010; Vigdor and Mercy, 2006). One study, which stratified the DVRO policy by whether firearm relinquishment was required, found that these negative relationships were significant when relinquishment was required but were negative and suggestive without required relinquishment (Díez et al., 2017). These four studies drew on sub-

### Figure 5.2—Continued

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>DV misdemeanor prohibition and surrender requirement</td>
<td>Intimate partner homicide rate</td>
<td></td>
</tr>
<tr>
<td>Raisian (2016)</td>
<td>Firearm</td>
<td>0.90 [0.81, 0.99]</td>
</tr>
<tr>
<td>Raisian (2016)</td>
<td>Nonfirearm</td>
<td>1.00 [0.91, 1.08]</td>
</tr>
<tr>
<td>Background check for misdemeanors</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>1.02 [0.95, 1.10]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.99 [0.90, 1.08]</td>
</tr>
<tr>
<td>Prohibitions for stalking conviction</td>
<td>Intimate partner homicide rate</td>
<td></td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Total</td>
<td>1.16 [1.04, 1.30]</td>
</tr>
<tr>
<td>Díez et al. (2017)</td>
<td>Total</td>
<td>0.97 [0.92, 1.02]</td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Firearm</td>
<td>1.11 [0.96, 1.29]</td>
</tr>
<tr>
<td>Díez et al. (2017)</td>
<td>Firearm</td>
<td>0.96 [0.89, 1.03]</td>
</tr>
<tr>
<td>State has stalking felony crime</td>
<td>Total</td>
<td>1.01 [0.92, 1.11]</td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Firearm</td>
<td>0.98 [0.86, 1.11]</td>
</tr>
</tbody>
</table>

NOTE: The study model in Díez et al. (2017) includes a one-year lag of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details. DV = domestic violence.
stantially similar data sets with largely overlapping time periods. One additional study
used a different data set and found that implementing a background check system that
uses information on restraining orders led to significant reductions in total and fire-
arm-related homicide rates (Sen and Panjamapirom, 2012). Considering these results,
we find moderate evidence that state laws establishing firearm prohibitions for individuals
subject to DVROs reduce total and firearm-related intimate partner homicides.

Domestic violence misdemeanor policies and intimate partner homicide. These same
five studies examined how intimate partner homicide rates were influenced by state
laws prohibiting firearm possession by individuals convicted of a domestic violence
misdemeanor. All found uncertain effects of the policy, and one found a suggestive
negative relationship if the prohibition was coupled with required firearm relinquis-
ishment (Díez et al., 2017).

One additional study that esti-
mated the effect of a policy that jointly
expanded firearm prohibitions to
include domestic violence misdemean-
ants and established a mechanism for
firearm surrender estimated a signifi-
cant negative effect of the policy on
firearm-related intimate partner homicide rates (Raissian, 2016). Considering
these results, we find inconclusive evi-
dence for how state firearm prohibitions
associated with domestic violence misde-
meanors influence total or firearm-related
intimate partner homicides.

Prohibitions associated with stalking
do not have positive or negative effects on total and
firearm-related intimate partner homicides.

Prohibitions associated with stalking
offenses and intimate partner homicide. Two studies (Zeoli et al., 2018; Vigdor
and Mercy, 2006) also examined how prohibitions related to stalking offenses
affected intimate partner homicide. One study found uncertain effects of stalking prohibitions on total and
firearm-specific intimate partner homicide (Vigdor and Mercy, 2006). The
other study found that stalking felony laws had uncertain effects but that fire-
arm prohibitions associated with misde-
meanor stalking offenses significantly
increased total intimate partner homi-
Prohibitions Associated with Domestic Violence  99

cide rates and had a suggestive effect consistent with increasing firearm-related intimate partner homicides (Zeoli et al., 2018). Considering these results, we find that there is inconclusive evidence for how state firearm prohibitions associated with stalking convictions influence firearm-related intimate partner homicides, and there is limited evidence that prohibitions associated with stalking misdemeanors increase total intimate partner homicides.

Other violent crime outcomes. Two studies examined outcomes other than intimate partner homicide (Sen and Panjamapirom, 2012; Vigdor and Mercy, 2006). Vigdor and Mercy (2006) found that DVRO-related prohibitions had an uncertain relationship with stranger homicides and nonfatal violent crime, but Sen and Panjamapirom (2012) found that policies related to more-robust enforcement or implementation of restraining order prohibitions led to significantly fewer total and firearm-related homicides. In contrast, policies related to prohibitions for domestic violence misdemeanants had uncertain effects or suggestive effects consistent with increasing rape and assault. Considering these results, we find inconclusive evidence for how laws establishing firearm prohibitions for domestic violence offenders affect violent crimes other than intimate partner homicide.

Outcomes Without Studies Examining the Effects of Prohibitions Associated with Domestic Violence

We did not identify any studies that met our inclusion criteria and examined the relationship between prohibitions associated with domestic violence and the following outcomes:

- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Five References


Public Law 104-208, Section 658, Gun Ban for Individuals Convicted of a Misdemeanor Crime of Domestic Violence, 1996.


United States Code, Title 18, Section 921, Definitions.


Federal law bans the sale of firearms to prohibited possessors, such as minors, illegal immigrants, convicted felons, fugitives from justice, users of controlled substances, those with adjudicated mental illnesses or involuntarily committed to mental institutions, those who have been dishonorably discharged from the military, those who have renounced their U.S. citizenship, those subject to restraining orders, and those convicted of domestic violence offenses (18 U.S.C. 922). However, there is no procedure under federal law for the removal of firearms from these same classes of prohibited possessors or for checking to see whether they have firearms at the time they become prohibited possessors.

While background checks and permit-to-purchase laws aim to prevent the purchase of firearms by prohibited individuals, laws requiring certain prohibited possessors to surrender firearms are designed to ensure that firearm owners relinquish their weapons once they are identified as belonging to a class of prohibited possessors. Through this mechanism, these laws should reduce rates of suicide or gun violence in this population, which is assumed to be at elevated risk. For instance, as discussed in Chapter Five, there is evidence that domestic violence offenders present an especially elevated risk of violence to their partners. For this reason, many state firearm-surrender laws focus on domestic violence offenders at the time they are convicted of such crimes.

To assess the impact of these policies, the ideal analyses would estimate effects on outcomes specifically for those populations required to surrender their firearms under the regulations. For instance, to study the impact on gun violence of laws requiring the removal or surrender of firearms by persons convicted of a domestic violence misdemeanor, one would like to estimate how violent crime rates changed after the law for this subgroup of the population relative to others not directly affected by the law. Furthermore, because these laws will be effective only to the extent that they are enforced, causal inference could be strengthened with information on the number of firearms that were surrendered or the proportion of prohibited possessors that have been disarmed.
State Implementation of Firearm-Surrender Laws

As of January 1, 2020, nine states have laws requiring the surrender of firearms by prohibited possessors. These states define a range of procedures for prohibited possessors to dispose of their firearms and time frames for doing so, and, in some cases, the laws stipulate roles for judicial officers or law enforcement to ensure that firearms are surrendered or confiscated. In addition, 29 states require the surrender of firearms pursuant to domestic violence restraining orders, to last for the duration of the order.


- Calif. Penal Code § 29810. The court shall provide the notifying defendant of prohibition against firearm possession and provide a form for facilitating the transfer of firearms. The form, which notes that the prohibition is effective immediately, allows the individual to designate another to have power of attorney for the purpose of disposing of or transferring the firearms (California Department of Justice, 2015). The power of attorney lasts for 30 days. The individual may also transfer possession to a licensed dealer for storage. See also Calif. Fam. Code § 6389. The individual shall surrender firearms for the period of the protective order.
- Conn. Gen. Stat. Ann. § 29036k. The individual has two business days to surrender firearms to the state or transfer them to an eligible individual. If surrendered, the individual has one year to transfer firearms to an eligible individual.
- Hawaii Rev. Stat. Ann. § 134-7.3. The individual has 30 days to dispose of or surrender firearms to law enforcement. If not surrendered, law enforcement may seize the firearms. See also Hawaii Rev. Stat. Ann. § 134-7(f). The individual must surrender firearms following any restraining order issued by a court.
- 430 Ill. Comp. Stat. 65/9.5. The individual must surrender his or her firearm owner’s identification card; submit a firearm disposition record; and, within 48 hours, place firearms in the location of or with the person reported on the disposition record.
- Mass. Gen. Laws Ch. 140 § 129B, 129D. Upon revocation of firearm identification card, the individual must surrender all firearms “without delay.” The individual then has one year to transfer firearms to a licensed dealer or permitted possessor.
- Nev. Rev. Stat. Ann. § 202.361. If a person is prohibited from owning, possessing, or having custody or control of a firearm pursuant to § 202.360, the court shall order the person to surrender the firearm to a designated law enforcement agency, a person designated by court order, or a licensed firearm dealer.
- N.Y. Crim. Proc. Law §§ 330.20, 380.96; N.Y. Penal Law § 400.05. A judge shall order revocation of an individual’s firearm license and demand the surrender of firearms. The individual has one year to transfer or sell firearms to a licensed dealer or to himself or herself (pursuant to obtaining a valid license).
- 18 Pa. Cons. Stat. Ann. § 6105. The individual has 60 days to sell or transfer firearms to an eligible individual outside his or her household.
- Wash. Rev. Code Ann. § 9.410.800. Upon a showing by clear and convincing evidence that a party has used, displayed, or threatened to use a firearm or other dangerous weapon in a felony or is ineligible to possess a firearm under § 9.41.040, the court shall require that the party immediately surrender all firearms and other dangerous weapons, as well as any concealed pistol license.

Surrender of Firearms by Prohibited Possessors

Of those, 17 states require individuals convicted of domestic violence misdemeanors or specific domestic violence offenses to surrender their firearms. 3

Effects on Violent Crime

Research Synthesis Findings

We identified five studies that provide some evidence on the effects of firearm-surrender laws, and all of the studies focus on surrender laws related to domestic violence. Vigdor and Mercy (2006) examined how intimate partner homicide is affected by laws that allow law enforcement officers to confiscate firearms at the scene of alleged domestic violence incidents. (For details on the study's analyses of the effects of laws that prohibit people under a domestic violence restraining order from purchasing or possessing a gun and that prohibit people who have been convicted of a misdemeanor domestic violence offense from possessing a gun, see Chapter Five.) The authors' state-level analysis of intimate partner homicide rates from 1982 to 2002 found no overall effect of confiscation policies. The authors analyzed the effects of the laws on other crimes that were not thought to be affected by confiscation laws (as a negative control) and found expected uncertain effects on rates of stranger homicide, rape, robbery, and motor vehicle theft but significant effects suggesting that confiscation laws may increase assault rates. The authors note that the effects of confiscation laws will depend, to a large extent, on how rigorously they are implemented and enforced and suggest that future research should examine associations between crime reduction and implementation differences.

Zeoli and Webster (2010) examined the effects of policies designed to restrict access to weapons by those with domestic violence–related restraining orders or those convicted of misdemeanors. Among the policies they examined were state laws allowing police to confiscate firearms from a domestic violence incident (they simultaneously examined state laws that allow police to make warrantless arrests for domestic violence restraining order violations and that mandate arrest for domestic violence restraining

They analyzed data from 46 cities between 1979 and 2003 and found no evidence that laws that allow police to confiscate firearms from a domestic violence incident affected rates of intimate partner homicide. A 2018 state-level analysis over a longer period (1980 to 2011) similarly found uncertain effects on intimate partner homicide of laws authorizing law enforcement to seize firearms from the scene of a domestic violence incident (Zeoli et al., 2018). Yet it is unclear how often seizure laws are enforced. One study found that Philadelphia police officers documented gun seizures in less than half of the domestic violence incidents they investigated in which a gun was used to threaten a domestic partner (Sorenson, 2017).

Díez et al. (2017) assessed how intimate partner homicide rates are influenced by prohibitions of firearm possession by persons convicted of a misdemeanor or subject to a restraining order related to intimate partner violence, with specific consideration of whether the laws included requirements for firearm relinquishment. Using data from 1991 to 2015 and negative binomial regression models with controls for the lagged intimate partner homicide rate, national trends, and region fixed effects, the authors found that the effects of the seizure laws were uncertain for both total and firearm-specific intimate partner homicide rates.

A final study meeting our inclusion criteria examined the 1996 Lautenberg Amendment, which was designed to extend prohibited-possessor status to those convicted of misdemeanor domestic violence offenses. The law also stipulated a mechanism for checking the firearm ownership of newly prohibited possessors and requiring that they surrender all firearms. Raissian (2016) identified the effect of the federal law by exploiting states’ variation in assault statutes, which, because of imprecise language in the Lautenberg Amendment, affected whether the new federal prohibitions applied to domestic violence misdemeanants. In 2009, the U.S. Supreme Court corrected the ambiguity in United States v. Hayes. Raissian (2016) found that intimate partner homicides and other family homicides declined when domestic violence misdemeanants were prohibited from possessing firearms and required to surrender any in their possession. However, because this study evaluated a policy change that simultaneously required firearm surrender and expanded the prohibited class, it did not isolate the specific effect of surrender, so we do not include this study as evidence for the effect of surrender per se.

Figure 6.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the firearm-surrender laws examined in these studies.

Conclusions
We identified four qualifying studies that examined the effect on any violent crimes of laws requiring prohibited possessors to surrender firearms. Vigdor and Mercy (2006) found such laws to have uncertain effects on total intimate partner homicides committed with firearms. Similarly, they found only uncertain effects of these laws on intimate partner homicides committed by any means, as well as uncertain effects for firearm intimate partner homicides of women. Zeoli and Webster (2010), Díez
et al. (2017), and Zeoli et al. (2018) also found the effects of surrender laws on intimate partner homicides and firearm intimate partner homicides to be uncertain. Additional analyses by Vigdor and Mercy (2006) that focused on other types of violent crime found significant effects of confiscation laws indicating that they increase assaults, but the authors found uncertain effects of these laws on stranger homicides, rapes, and robberies.

Considering the results of these studies, we find inconclusive evidence for how laws requiring prohibited possessors to surrender firearms affect violent crime generally and intimate partner homicides in particular.
Outcomes Without Studies Examining the Effects of Firearm-Surrender Laws

We did not identify any research that met our inclusion criteria and examined the relationship between the surrender of firearms by prohibited possessors and the following outcomes:

- suicide
- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Six References


United States Code, Title 18, Section 922, Unlawful Acts.


Extreme risk protection order (ERPO) laws, sometimes known as gun violence restraining order or “red-flag” laws, are risk-based, temporary, and preemptive protective orders that authorize the removal of firearms from individuals determined to be at risk for committing gun violence against others or themselves.¹ With ERPO laws in place, law enforcement, family members, or medical professionals can petition a court to temporarily restrict a person’s access to firearms if they believe that person is at risk of hurting him or herself or others. ERPO laws differ from prohibited purchaser regulations that prevent specific groups of individuals—such as those with a criminal record, those with a history of domestic abuse, or those who have been dishonorably discharged from the military—from owning, purchasing, or possessing firearms. They also differ from laws that require the removal of firearms from prohibited possessors (see Chapter Six) because ERPOs can be served to anyone if the court determines that the person is at high risk for firearm violence, regardless of whether he or she has committed a crime, has been diagnosed with a significant mental disorder, or has otherwise been disqualified from possessing firearms.

There are no federal ERPO laws, but state lawmakers have adopted a wide range of legislation that varies in terms of who can petition for a protection order, how long the order lasts, and the evidence required to demonstrate the need for an order. Typically, states with ERPO laws allow law enforcement officials or state’s attorneys to request an ERPO for an individual, although some states also permit family members and medical professionals to petition for an ERPO. If implemented, the duration of the order ranges from six to 12 months, but the person named in the ERPO is typically given the opportunity to request a hearing to terminate the order during the effective period.

¹ In our definition of ERPOs, we include laws permitting temporary risk-based firearm removal. Such laws seek the removal of firearms from individuals whom a court deems unable to keep the weapons safely, but removal petitions can be filed only against individuals who already possess firearms. ERPOs, in contrast, can be served against anyone, regardless of current gun ownership, and can block the subject of the order from purchasing firearms. However, available evidence suggests that both types of laws are largely used in the same way, which is why we include both in our ERPO definition (Kapoor et al., 2018).
Some state laws also offer ex parte ERPOs, a type of protection order that allows eligible individuals to petition for an ERPO in emergency cases without waiting to provide notice of a hearing to the respondent. Like ex parte orders related to domestic violence (see Chapter Five), these laws are designed for situations in which the waiting period for a full court appearance could undermine the effectiveness of the order. As with ex parte domestic violence procedures, those who oppose these laws cite concerns that, whether by error or by malfeasance, they will be misused or incorrectly applied to gun owners who are not dangerous if those individuals are not present during a hearing to assess the danger they pose (Kopel, 2019).

State laws also vary in the types of evidence sufficient to demonstrate that the respondent is at an elevated risk for committing violence. Evidence might include threats of violence by the respondent toward him or herself or another person in the past six months, a violation of a domestic violence restraining order, recent acquisition of a significant number of firearms, or the use of threats of physical force against another person or animals. In addition, a petitioner must typically include what he or she knows about the firearms and ammunition that the respondent possesses. If a respondent does not return firearms in his or her possession once the order is served, the court may issue a warrant to authorize law enforcement officials to retrieve the weapons.

ERPO laws are designed to respond to acute periods of elevated risk of violence by authorizing a temporary ban on new firearm purchases and the temporary removal of firearms currently in an individual’s possession. By providing a legal framework to remove firearms and reduce access to new firearms among high-risk individuals, ERPOs may decrease the overall rates of gun-involved crimes, homicides, mass shootings, and suicides. Because ERPOs specifically target individuals who may intentionally use a firearm to injure themselves or others, it is less clear how ERPOs would affect unintentional firearm injuries and deaths; although removal of firearms could have spillover effects in reducing accidental injury, these second-order effects are likely to be small.

ERPOs could potentially apply to a broader population than do federal prohibitions on gun possession by those with certain mental health histories or other prohibited possessor laws. For example, although most people who die by suicide suffer from a mental disorder (here, substance use disorders are included), only a small percentage of these individuals have a record of involuntary civil commitment or other disqualifying mental health or criminal conviction record (Swanson et al., 2016). However, ERPOs rely on observed behavior to identify those at elevated risk of committing firearm violence. If individuals who pose a high risk of violence can be correctly identified as such by simply observing their behavior rather than relying on specific criminal or mental health histories, then ERPO laws could decrease suicides and homicides among this population over and above the suicides and homicides prevented by existing prohibited purchaser laws (Vernick, Alcorn, and Horwitz, 2017).
In other words, the potential impact of these policies on such outcomes as suicide, homicide, and mass shootings will hinge critically on how the policies are used in practice. Although there is no systematic information collected about the number of ERPOs that are served nationwide, some state data are illustrative. Of states with publicly available data, most serve between 50 and 100 ERPOs per year. In California in 2016, 86 orders were served; in Washington in 2018, 48 were served. Between 2006 and 2013, 58 ERPOs were served per year in Indiana; between 1999 and 2013, 51 orders were served per year in Connecticut (Kapoor et al., 2018; Miletich, 2018; Koseff, 2017). In contrast, in the first six months since Maryland’s ERPO law went into effect in October 2018, the state granted 258 ERPOs (Yablon, 2019). Consequently, in general, it seems unlikely that ERPOs will affect the actions of more than 100 people per state per year, although there is clear variability across states (and even within states) in the intensity with which the orders are used. However, if ERPOs are well targeted and have a high likelihood of preventing a suicide or a homicide, then they could substantially lower state homicide or suicide rates.

Additionally, although high-profile homicide incidents have precipitated the passage of ERPOs in several states (Kapoor et al., 2018; Swanson et al., 2017), available data suggest that petitions for ERPOs most commonly cite concerns about self-harm or suicide as the reason for removal. Data from Connecticut suggest that about 61 percent of requests for gun removal involve a concern about self-harm (Swanson et al., 2017), which is similar to the percentage of firearm deaths that are suicides (60 percent) (Kochanek et al., 2019). Similarly, data from Indiana suggest that 68 percent of firearm removals were prompted by a person threatening or attempting suicide (Parker, 2015). A detailed review of case records from 159 ERPOs issued in California found that 21 (13.2 percent) involved an individual who had access to or was planning to access firearms and expressed or exhibited behavior suggesting intent to perpetrate a mass shooting (Wintemute et al., 2019). Thus, the impact of ERPOs might be concentrated in suicide rates rather than rates of mass shootings or homicides.

Nuances of the policy could influence the likelihood that the ERPOs affect rates of homicide and suicide with firearms. The impact of ERPOs could be mediated by the types of individuals authorized to petition for such an order. Typically, ERPO legislation extends the right to petition for an ERPO to law enforcement officers. However, some laws extend that right to family members and medical professionals. In general, by expanding the ability to request an ERPO to family members and others with knowledge of the high-risk individual’s behavior, we could expect that a higher percentage of at-risk individuals would have their firearms removed for their safety or to prevent violent crimes. In addition, laws that allow for ex parte orders may be more effective in time-sensitive cases because the subject of the order is likely to be separated from a firearm more quickly.
One concern regarding ERPOs is how they will affect the gun industry and gun owners who do not pose a threat to society or themselves. Expanding the number of people who can petition for an ERPO may increase the likelihood that the procedure could be misused—for instance, to harass a former spouse or disarm a potential victim (Kopel, 2019). And ERPO laws might negatively affect defensive gun use, hunting and recreation, and gun sales if firearms are denied to law-abiding citizens. ERPO legislation attempts to avoid these negative effects by forbidding the use of ERPOs as harassment (Roskam and Chaplin, 2017) and allowing respondents to petition the court to return their firearms while the order is in effect.

The possible effects of ERPOs on officer-involved shootings are unclear. The laws could decrease officer-involved shootings by allowing law enforcement officers to remove firearms from a person that they believe may threaten them or others and thus reducing the potential need for lethal force in an officer-involved confrontation. Alternatively, should the duty to enforce these orders result in increased police contact with armed individuals who are experiencing a crisis and are unwilling to relinquish their weapons, ERPOs may increase the risk of harm to officers and those whom they seek to disarm.

To better evaluate the impact of ERPOs, descriptive information on the characteristics of individuals subject to ERPOs, reasons for ERPO petitions, and removal processes could shed light on the outcomes most likely to be affected by such policies and the time frame over which effects may be expected. More-direct causal insights might also be obtained by estimating the effects of ERPOs on outcomes specifically for those populations required to temporarily turn in their firearms. In Connecticut, a comprehensive evaluation by Swanson et al. (2017) provided such information. Analyzing mortality information for 762 risk-warrant subjects during the initial 14 years of Connecticut’s ERPO law from 1999 to 2013, the authors found that 21 persons died by suicide (29 percent involving firearms) subsequent to the gun removal event. However, applying information on suicide case fatality rates by method and assuming a counterfactual pre-intervention probability of using a gun in a suicide attempt that was informed by the relationship between state-level gun ownership and the proportion of firearm suicides to total suicides, the authors estimated that an additional 72 suicide deaths would have occurred in the absence of the gun removal. Researchers found similar results in a study of persons subjected to gun seizure in Marion County through Indiana’s ERPO law (Swanson et al., 2019). These studies offer some evidence that ERPOs may be effective at reducing firearm-related suicides, but their results hinge on an extrapolation of the number of suicide attempts rather than observed suicide attempt data or a formal comparison group. Still, further effort to provide detailed process and outcome evaluations in other states with ERPOs would greatly advance research about the orders’ likely effects.
State Implementation of Extreme Risk Protection Orders

As of January 1, 2020, 16 states and the District of Columbia have laws that allow for the removal of firearms from individuals found to pose an imminent risk of harming themselves or others. Of those, 11 states allow individuals other than law enforcement officers to file a petition for the ERPO. Some states have expanded the class of potential petitioners only slightly; for instance, California allows immediate family members to file ERPO petitions. Maryland, on the other hand, allows petitions to be filed by a variety of medical and mental health professionals, spouses and cohabitants, other family members, coparents, current dating partners, and current or former legal guardians. In addition, 14 states and the District of Columbia call for the removal of firearms from individuals who are the subject of ex parte ERPOs—that is, orders that have been executed before the respondent has the opportunity to go to court to defend him or herself. With regard to the ex parte ERPOs, some states allow these only when the petitioner is a law enforcement officer, while others allow ex parte ERPOs to be

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4 Calif. Penal Code § 18150.

5 The law states that the following individuals qualify as mental health professionals:

   Physician, psychologist, clinical social worker, licensed clinical professional counselor, clinical nurse specialist in psychiatric and mental health nursing, psychiatric nurse practitioner, licensed clinical marriage or family therapist, or health officer or designee of a health officer who has examined the individual. (Md. Code Ann., Pub. Safety § 5-601(E)(2))


filed by an expanded group of petitioners. Most final ERPOs last one year. Ex parte ERPOs last for shorter periods, and there is greater variability in the length of ex parte ERPOs, ranging from one or two days in Maryland to up to 21 days in Oregon.

Effects on Suicide

Research Synthesis Findings
We identified one study that met our inclusion criteria and assessed the effects of ERPOs on suicide. Using a synthetic control approach, Kivisto and Phalen (2018) estimated the effects on suicides rates of three policy changes: the enactment of Connecticut’s ERPO law in 1999, increased enforcement of Connecticut’s ERPO law in 2007, and the enactment of Indiana’s ERPO law in 2006. The study distinguished between two intervention periods for Connecticut, the first of which coincided with the law’s effective date in 1999 and the second of which is stated to correspond with enhanced enforcement of the law following the Virginia Tech mass shooting in 2007.

Based on the 1999 enactment date of Connecticut’s ERPO law, the analyses showed small and uncertain effects on firearm suicide rates and on nonfirearm suicide rates (see Figure 7.1). Defining the postintervention period in Connecticut as occurring after the increased enforcement in 2007 resulted in estimates showing larger but still imprecise declines in firearm suicide, with uncertain effects on nonfirearm suicide. In the decade following the enactment of Indiana’s ERPO law, the firearm suicide rate in Indiana was approximately 7.5 percent lower than that of its synthetic control, a suggestive decline ($p = 0.05$); the estimated effect on nonfirearm suicide rates was small.

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11 In Maryland, the law states, Except as provided in subsection (e) of this section, or unless the judge continues the hearing for good cause, a temporary extreme risk protective order hearing shall be held on the first or second day on which a District Court judge is sitting after issuance of the interim extreme risk protective order. (Md. Code, Pub. Safety, § 5-603)

In Oregon, the respondent has 30 days to request a hearing, and the hearing must occur within 21 days. See Ore. Rev. Stat. Ann. § 166.527(9).

12 Another study by Swanson et al. (2017) extrapolated suicide rates based on several assumptions but did not meet our inclusion criteria for having observations before and after implementation of a policy on treated and control cases.

13 Our approach to estimating confidence intervals (CIs) for studies using synthetic control methods is discussed in Chapter Two.
and uncertain. Because only two states had ERPO laws that could be evaluated in this study, findings may not generalize to other contexts, there is limited ability to isolate the cause of any observed change to the passage of the law versus to other co-occurring factors, and the study may have had limited power to detect meaningful effects.

Figure 7.1 displays the incidence rate ratios (IRRs) and CIs associated with ERPOs as estimated by Kivisto and Phalen (2018).

### Figure 7.1
Incidence Rate Ratios Associated with the Effect of Extreme Risk Protection Orders on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Risk-based seizure law, Indiana enactment (2006)</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Firearm</td>
<td>0.92 [0.85, 1.00]</td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Nonfirearm</td>
<td>1.01 [0.96, 1.06]</td>
</tr>
<tr>
<td>Risk-based seizure law, Connecticut enactment (1999)</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Firearm</td>
<td>0.98 [0.88, 1.09]</td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Nonfirearm</td>
<td>1.06 [0.89, 1.23]</td>
</tr>
<tr>
<td>Risk-based seizure law, Connecticut enforcement (2007)</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Firearm</td>
<td>0.86 [0.65, 1.08]</td>
</tr>
<tr>
<td>Kivisto &amp; Phalen (2018)</td>
<td>Nonfirearm</td>
<td>1.06 [0.87, 1.26]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

### Conclusions
One study that examined the effects of ERPOs on suicide rates generated mixed findings. Specifically, Kivisto and Phalen (2018) found a suggestive reduction in firearm suicide rates in Indiana following the enactment of the ERPO law in 2006, but the authors found uncertain evidence for the effect of the passage and increased enforcement of Connecticut’s law in 1999 and 2007, respectively. Although the findings for Indiana’s law are suggestive, considering the strength of this evidence and potential issues of generalizability, we find inconclusive evidence for the effect of extreme risk protection orders on total and firearm suicides.
Outcomes Without Studies Examining the Effects of Extreme Risk Protection Orders

We did not identify any studies that met our inclusion criteria and examined the relationship between ERPOs and the following outcomes:

- violent crime
- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Seven References


Part C of this report presents research syntheses on the included policies that regulate firearm sales and transfers. These policies, which constitute the majority of existing firearm regulations, determine the processes by which firearm commerce is conducted, establish mechanisms for tracking the flow of firearms between parties, and scope the types of firearms and ammunition that are allowed to be purchased. At the federal level, most regulations on firearm sales and transfers apply only to federal firearm licensees. Regulations on private-party sales are instead generally the purview of states, many of which have adopted policies that extend federal dealer regulations to sales by unlicensed, private sellers; increase prerequisites or limits on firearm purchases; or mandate more-stringent oversight and recordkeeping with the aim of limiting the diversion of firearms from legal owners to prohibited possessors.

Overall, this family of policies directly affects the flow of firearms by influencing the cost of obtaining a firearm and by establishing mechanisms to prevent prohibited individuals (see Part B) from obtaining firearms. Of the 18 policy classes included in this review, eight operate by establishing restrictions or requirements on the sales and transfers of firearms in legal markets. Each policy is reviewed in the subsequent chapters in Part C, as follows:

- background checks (Chapter Eight)
- licensing and permitting requirements (Chapter Nine)
- waiting periods (Chapter Ten)
- firearm safety training requirements (Chapter Eleven)
- lost or stolen firearm reporting requirements (Chapter Twelve)
- firearm sales reporting, recording, and registration requirements (Chapter Thirteen)
- bans on the sale of assault weapons and high-capacity magazines (Chapter Fourteen)
- bans on low-quality handguns (Chapter Fifteen).
CHAPTER EIGHT

Background Checks

Background checks for gun purchases are designed to prevent access to guns by convicted felons and other prohibited possessors—such as minors, fugitives from justice, those who live in the United States illegally, users of controlled substances, those with certain histories of mental illness, those who have been dishonorably discharged from the military, those who have renounced their U.S. citizenship, those subject to a restraining order, and those convicted of domestic violence offenses (18 U.S.C. 922).

The Brady Handgun Violence Prevention Act (the Brady Act), which went into effect in 1994, imposed federal requirements for background checks on sales by licensed dealers (18 U.S.C. 922) but not for private sales or transfers of firearms (such as gifts). Several states have expanded this federal requirement to mandate that background checks be conducted for all firearm sales and transfers, including those between private parties. Such laws are referred to as universal background check laws.

Background check laws seek to prevent firearm purchases by individuals thought to be at high risk of presenting a danger to themselves or others. By restricting the means by which dangerous individuals could otherwise access guns, these laws are designed to reduce gun crime and violence. While compliance is likely to be imperfect, a universal background check may still reduce gun-related homicides or suicides by deterring prohibited possessors from attempting to acquire firearms or by making it harder or more expensive for them to succeed in doing so. Universal background checks may also reduce illegal gun trafficking. For instance, when analyzing crime guns, Webster, Vernick, and Bulzacchelli (2009) found that fewer of the out-of-state guns originated in states with universal background checks than in states with no background checks for private sales of firearms.

The magnitude of the effects of such laws will be influenced, in part, by the level of enforcement and the availability of firearms through alternative markets, such as illegal markets or legal markets in states without background checks for private transactions. Moreover, most firearms are purchased by individuals who already own a fire-

1 The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined crime gun as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”
Azrael et al. (2017) found that, on average, gun owners had close to five firearms each, and a large majority (62 percent) purchased their most recent weapon from a licensed gun dealer. For those who already own guns, a background check requirement may have little or no effect on crime or suicide risk.

There are no routinely collected data on how individuals obtain guns, but a 2015 national survey of gun owners who obtained a firearm within the previous two years found that 22 percent had purchased, or received as a gift or an inheritance, their most recent firearm without undergoing a background check (Miller, Hepburn, and Azrael, 2017). For firearms purchased through private sources, 50 percent were acquired without a background check (Miller, Hepburn, and Azrael, 2017). Obtaining firearms from private sources is likely substantially more common among prohibited possessors. Indeed, a 2004 survey of state prison inmates found that, among those who used a gun, only 10 percent purchased the weapon from a licensed dealer, whereas 70 percent acquired it from a friend, family member, or “street” source, such as an illicit broker (Cook, Parker, and Pollack, 2015). Using the same survey data but restricting the sample to 13 states considered by the authors to have less-restrictive firearm regulations, another study found that, among inmates who acquired their gun from a friend, family member, or “street” source, just more than 40 percent had a disqualifying condition (e.g., prior felony conviction, dishonorable discharge, under age 18) that should have prohibited them from obtaining the firearm had they undergone a background check (Vittes, Vernick, and Webster, 2012).

Universal background check policies may do little to limit existing illegal sources of firearms to criminal offenders (Kopel, 2016), and background check policies on their own can, at best, prevent such individuals only from acquiring new firearms, not from maintaining possession of those they owned before becoming a prohibited possessor. However, if the implementation and enforcement of such policies is successful in stemming the flow of new firearms to criminal markets, universal background check laws could reduce gun crime by increasing the price of firearms in the secondary markets on which criminals mostly rely (Cook, Molliconi, and Cole, 1995).

The effects of background check policies will hinge on the scope of disqualifications for high-risk individuals and whether these disqualifications correctly target individuals who present greater danger to themselves or others. As of December 2018, the Federal Bureau of Investigation (FBI)’s National Instant Criminal Background Check System (NICS) database included more than 19,300,000 active records on prohibited possessors (Criminal Justice Information Services Division, 2019). However, this figure substantially undercounts prohibited possessors because states’ reporting is incomplete, and the FBI does not maintain records on those prohibited only because they are underage. The excess risk of firearm violence attributable to prohibited individuals is unknown, although research has shown that the majority of violent offenders have previous involvement with the criminal justice system (Wright and Wintemute, 2010; Cook, Ludwig, and Braga, 2005; Kleck and Bordua, 1983). Similarly, although...
the risk posed by individuals prohibited from owning a firearm because of adjudicated mental health problems is not known, it is clear that individuals with severe mental disorders are at elevated risk of suicide (Chesney, Goodwin, and Fazel, 2014; see also RAND Corporation, 2018, Chapter Nineteen).

Background check policies could prevent individuals at risk of perpetrating mass shootings from obtaining firearms. One study found that of the 116 mass shooting incidents between 2009 and 2016 for which information was available, 44 incidents (34 percent) involved a prohibited possessor (Everytown for Gun Safety Support Fund, 2017b). A 2018 study of mass public shootings found that a majority of the shooters purchased at least one of their weapons legally and with a federal background check (Buchanan et al., 2018).

In assessing background check policies, the ideal analyses would estimate effects on outcomes specifically for those populations or individuals whose access to firearms became restricted under the regulations. For instance, to study the impact on suicide of background check laws that disqualify individuals with some histories of mental illness, one would like to estimate how, after the law was implemented, suicide rates changed specifically among individuals newly prohibited by the law. Similarly, data on the price of firearms in secondary and illegal markets would be valuable for understanding whether background check laws or their expansion to new populations of prohibited possessors cause access to firearms in secondary markets to become restricted.

However, there are numerous challenges to undertaking this type of analysis, because most data sources available to researchers lack detailed information on the characteristics of criminal offenders or suicide victims beyond age, gender, and race/ethnicity. (The National Violent Death Reporting System is an important exception, when that information is known.) In some cases (e.g., restraining orders), an individual may be only temporarily prohibited from possessing a firearm, and, in the case of crime outcomes, details on the criminal offender can be known only if the perpetrator is known. (See the discussion of data limitations in Chapter Twenty-One.) Given these challenges, it is unsurprising that most of the articles meeting our inclusion criteria for this policy did not use these types of data. Nevertheless, two studies (Swanson et al., 2013; Swanson et al., 2016) were able to merge administrative records from public health and criminal justice agencies to focus on violent crime outcomes for individuals with disqualifying mental health histories.

**State Implementation of Background Checks**

As of January 1, 2020, 14 states and the District of Columbia have comprehensive background check laws that require checks at the point of transfer for all firearms.²

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Even within these states, there are some differences in the laws. For example, California, Colorado, Delaware, Nevada, New Mexico, New York, Washington, and the District of Columbia require that all transfers to individuals (with some minor exceptions) are processed through licensed dealers, who conduct the background checks.3 Somewhat similarly, Oregon requires all transfers and background checks to be processed through dealers, except that sellers at gun shows may request background checks directly with the Department of State Police.4 Three more states—Maryland, New Jersey, and Pennsylvania—have the same universal background check requirements, but they are applicable only to handguns.5

Other states require background checks before law enforcement can issue a permit to purchase. Five states and the District of Columbia have promulgated such laws for all firearms,6 while six states have such laws for handguns only.7 Under these laws, firearms (or handguns in the latter four states) may not be purchased without permits, but the permitting systems and rules differ. For example, in Hawaii, a permit for a handgun must be used within ten days of receipt, and a new permit must be issued for each handgun transfer.8 In Illinois, however, a permit lasts ten years.9 Furthermore, some states allow exceptions for those who hold permits to carry or concealed-carry permits, which may have longer durations than the permits to purchase.10

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9 430 Ill. Comp. Stat. 65/7.

10 For example, individuals purchasing a firearm in Massachusetts must obtain a firearm identification card, which lasts three years; however, there is an exception for holders of permits to carry, which last up to six years (Mass. Gen. Laws Ch. 140 §§ 122, 129C). In Illinois, there is an exception for holders of concealed-carry permits, but those last only five years (compared with the ten years for the permit to purchase), so the exception does not typically extend the permit period for gun purchases (430 Ill. Comp. Stat. 65/2).
Effects on Suicide

Research Synthesis Findings

We identified five quasi-experimental studies that met our inclusion criteria and examined the impact of background check policies on suicide outcomes.11 The earliest of these (Ludwig and Cook, 2000) studied the impact of the 1994 Brady Act and found uncertain effects of the policy on total suicides, firearm suicides, and the proportion of adult suicides caused by a firearm. When restricted to suicides among those aged 55 or older, however, there was a statistically significant decrease in firearm suicides of around 6 percent and in the proportion of suicides involving a firearm of 2.2 percent. However, there was an offsetting increase in suicides by other means and thus only suggestive evidence of a statistically significant decrease in total suicides in this age group.

A limitation of the Ludwig and Cook (2000) study is that it had an unfavorable ratio of estimated parameters to observations (less than one to six), meaning it could have misleading parameter estimates and confidence intervals (CIs) due to model overfitting.12 Sen and Panjamapirom (2012) assessed how different types of background checks conducted by states affected suicides between 1996 and 2005. They noted that the supply of state and local records to the NICS is voluntary and that substantial variation exists in state laws regarding the categories of records included in background checks. The authors characterized variation across states in background check requirements using an index of the comprehensiveness of such checks, as well as individual indicators for whether states check on restraining orders, mental illness, fugitive status, misdemeanors, and other miscellaneous records. Using state-level data from 1996 to 2005, the authors examined the effects of these types of checks and the effects of a state having a pre-Brady Act background check requirement on both firearm and total suicides. Their regression models included time-varying state-level covariates, adjustment for state-level rates of suicide in 1990, and fixed effects for year and census subregion.

Sen and Panjamapirom (2012) found an effect of the total number of background check categories on firearm suicides (adjusted incidence rate ratio [IRR] = 0.98; 95-percent CI = 0.96, 1.00). Background checks for mental illness were related to

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11 We dropped one study (Duggan, Hjalmarsson, and Jacob, 2011) included in our original review that examined the relationship between gun shows and suicide outcomes in a state with background check requirements for gun shows (California) and a state without such requirements (Texas). Although the analyses might provide some insight into the effects of background check laws on private sellers, we determined that the study did not meet our inclusion criteria of analyzing data both before and after policy enactment; thus, we no longer consider it in this chapter’s synthesis. For analogous reasons, we do not consider a similar analysis of gun show effects by Matthay et al. (2017).

12 Ludwig and Cook (2000) also tested the effects of background checks specifically (separate from waiting periods, which were also imposed by the Brady Act) by comparing five of 32 states that were required to implement background checks but that did not experience a change in their waiting periods (either because they already had a waiting period of five days or more when the Brady Act required this nationally or they implemented an instantaneous background check). These analyses had a ratio of estimated parameters to observations of less than one to five, which did not meet our inclusion criteria.
lower firearm suicide and total suicide rates. Sen and Panjamapirom’s estimates suggest the post-policy firearm suicide rate to be 96 percent of the expected rate had this policy not been in effect and the total suicide rate to be 97 percent of the expected rate. Background checks for fugitive status were also associated with lower firearm suicide and total suicide rates; the estimated effect for checks of fugitive status suggests that these checks lower firearm suicide rates to 95 percent of what they would otherwise be, and they lower total suicide rates to 91 percent of the expected rate. In this case, however, so few states changed this policy during the study time frame that these effects cannot persuasively be attributed to the background check policy as opposed to other factors affecting suicides in the states around the same time their laws changed. Checks for misdemeanor offenses were also associated with a firearm suicide rate just 95 percent of the expected rate without such checks, although the effect on total suicide was uncertain. The authors of this study did not appear to make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects.

Similarly, Swanson et al. (2016) evaluated how changes in state reporting of gun-disqualifying mental health records to the NICS affected suicide rates among individuals in Florida with a disqualifying mental health condition relative to individuals diagnosed with serious mental health illness but not prohibited from purchasing a firearm. The authors found no significant difference between suicide rates before and after implementing expanded NICS reporting for the two groups.

Since the first edition of this report (RAND Corporation, 2018), two more studies used data spanning more than 20 years to analyze how background check laws influence suicide rates. Luca, Malhotra, and Poliquin (2017) used data from 1977 to 2014 to evaluate the effects of background check and waiting-period laws on suicide rates among adults aged 21 or older. Their analysis was based on log-linear models adjusting for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors. In models that adjusted for other state gun policies (e.g., concealed-carry laws, permitting requirements), the authors found that background check laws were associated with significant increases in nonfirearm suicide rates but had uncertain effects on total and firearm-related suicide rates. However, these estimates differed across model specifications, varying in terms of magnitude, precision, and direction depending on the period studied, whether state-specific trends were adjusted for, and whether regressions were population-weighted. Therefore, the estimates likely are not reliable.

Finally, Kagawa et al. (2018) used synthetic control methods to estimate how suicide rates were affected by the 1998 repeals of comprehensive background check laws in both Indiana and Tennessee. For each state separately, the authors estimated the percentage change in age-adjusted firearm suicide rates after the repeal of the law relative to the counterfactual rate expected based on a combination of states weighted to match firearm suicide rates (and other state-level covariates) in Indiana or Tennes-
see prior to their policy changes (i.e., relative to a similar synthetic control state). The authors found uncertain effects of both policy changes on firearm and nonfirearm suicide rates. Although analyzing two states with a policy change mitigates some of the concerns with looking at such a change in a single state, these methods remain limited in that they fail to account for potential confounds that may have influenced suicide rates over the post-implementation period. Furthermore, because the study restricted potential control units to states that had comprehensive background check or permit-to-purchase policies over the entire study period, only between seven and 11 states were available to inform the constructed counterfactual. This leads to worse pre-intervention fit and increases the likelihood that any estimated effect represents noise rather than the true effect of background checks.

Figure 8.1 displays the IRRs and CIs associated with the background check policies examined in these studies. Because Swanson et al. (2016) did not provide effect estimates or test statistics for their findings, we do not include effect sizes for that study in the figure.

**Figure 8.1**
Incidence Rate Ratios Associated with the Effect of Background Checks on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>State dealer background check law</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>1.02 [0.98, 1.06]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>1.03 [0.97, 1.09]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>1.09 [1.02, 1.16]</td>
</tr>
<tr>
<td>Brady Act</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Total, aged 21+</td>
<td>0.98 [0.93, 1.03]</td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Firearm, aged 21+</td>
<td>0.98 [0.94, 1.02]</td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Nonfirearm, aged 21+</td>
<td>1.01 [0.95, 1.08]</td>
</tr>
<tr>
<td>Index of background check comprehensiveness</td>
<td>Firearm</td>
<td>0.98 [0.96, 1.00]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Check on restraining order</td>
<td>Total</td>
<td>1.02 [0.98, 1.06]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>1.03 [0.98, 1.09]</td>
</tr>
<tr>
<td>Check on mental illness</td>
<td>Total</td>
<td>0.97 [0.95, 0.99]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.96 [0.92, 0.99]</td>
</tr>
</tbody>
</table>

*Figure continued on the next page*
Total suicides. We identified three qualifying studies that evaluated the effects of background checks on the total number of suicides using largely independent data sets (one examined state suicide rates from 1990 to 1997, one examined state rates from 1996 to 2005, and one used the same data set as the first study but covered a much longer time frame, 1977 to 2014). The earliest of these studies (Ludwig and Cook, 2000) concluded that dealer background checks have an uncertain effect on total suicide rates among those aged 21 or older. All of these effects were partially confounded with possible effects of waiting periods that were simultaneously introduced in many states when the Brady Act was implemented. The second study, Sen and Panjamapirom (2012), examined components of background checks, finding significant effects indicating that checks on mental illness and checks on fugitive status reduce total suicide rates. Three other components of background checks (checks on restraining orders, checks on misdemeanor records, and other miscellaneous checks) had only uncertain effects on total suicide rates. The third study (Luca, Malhotra, and Poliquin, 2017), which covered a longer period and addressed some of the methodological limitations of the
earlier studies, found that dealer background checks had uncertain effects on overall suicide rates.

Some aspects of background check implementation seem to show limited evidence of reducing total suicide rates; however, considering the relative strengths of these studies, we conclude that available research provides inconclusive evidence for how background checks affect total suicides.

Firearm suicides. We identified four qualifying studies that evaluated the effects of background checks on firearm suicide rates, including the three studies that examined total suicides. Ludwig and Cook (2000) found an uncertain effect of dealer background checks on firearm suicide rates among those aged 21 or older. Using the same data set but analyzing a longer time frame, Luca, Malhotra, and Poliquin (2017) also found an uncertain relationship between background check laws and firearm suicides among those aged 21 or older. In contrast, Sen and Panjamapiron (2012) found a statistically significant association between their background check comprehensiveness index and reduced firearm suicides. Across five other reported component analyses, checks on mental illness, fugitive status, and misdemeanors were associated with significant reductions in firearm suicides, whereas checks on restraining orders and other miscellaneous checks had only uncertain effects. Finally, in separate analyses of the effects of the repeal of comprehensive background check laws in Indiana and Tennessee on firearm suicide, Kagawa et al. (2018) found uncertain effects of the repeals on firearm suicide rates.

All but one study found inconclusive evidence for the effects of background checks on firearm suicide rates. One study found that some features of background checks may reduce firearm suicides. Given methodological concerns about this study, we find that the available research provides inconclusive evidence for how background checks affect firearm suicides.
Effects on Violent Crime

Research Synthesis Findings

Of studies that examined the relationship between background checks and violent crime, we identified 14 that met our inclusion criteria. Ludwig and Cook (2000) studied the impact of the 1994 Brady Act and found no difference in homicide rates across states that had laws comparable to those the Brady Act would impose (which initially included both background checks and a waiting period) and states that experienced larger changes in the law when the Brady Act was implemented. The Ludwig and Cook study had an unfavorable ratio of estimated parameters to observations (less than one to six), meaning its parameter estimates and CIs may not be accurate because of model overfitting.

Another study used similar identifying variation from the Brady Act but used a triple difference-in-difference-in-differences design, exploiting differential effects of the Brady Act across states and over time, as well as expected differential effects by age group, because juveniles under age 21 were already prohibited from purchasing a handgun (Monroe, 2008). Specifically, this model estimated the differential change in adult homicide rates before and after the Brady Act for states that were affected by the act versus those that were not, relative to the contemporaneous differential change in juvenile homicide rates. Results showed uncertain effects of the act on total and handgun-specific homicide rates but significant effects consistent with the act reducing firearm homicides that involved guns other than handguns.

Gius (2015a) examined the effect of the federal Brady Act, state-mandated dealer background checks (either a check that was in place before the Brady Act or checks for categories of state-prohibited possessors other than those mandated by the Brady Act), and state-mandated private-seller background checks on gun-related homicides (the paper did not evaluate the effect of these variables on total homicides). The analysis

13 We dropped one study (Duggan, Hjalmarsson, and Jacob, 2011) included in our original review that examined the relationship between gun shows and suicide outcomes in a state with background check requirements for gun shows (California) and a state without such requirements (Texas). Although the analyses could provide some insight into the effects of background check laws on private sellers, we determined that the study did not meet our inclusion criteria of analyzing data both before and after policy enactment; thus, we no longer consider it in this chapter’s synthesis. For analogous reasons, we do not consider a similar analysis of gun show effects by Matthay et al. (2017). We also dropped Wright, Wintemute, and Rivara (1999), which was included in our original review, because it compared outcomes for individuals with a felony conviction versus individuals with a felony arrest and did not provide evidence for background checks per se.

14 Ludwig and Cook (2000) also tested the effects of background checks specifically (separate from waiting periods, which were also imposed by the Brady Act) by comparing five of 32 states that were required to implement background checks but that did not experience a change in their waiting-period policies (either because they already had a waiting period of five days or more when the Brady Act required this nationally or they implemented an instantaneous background check). These analyses had a ratio of estimated parameters to observations of less than one to five, which did not meet our inclusion criteria. Cook and Ludwig (2003) presents results that are for a shorter period but are qualitatively similar.
of the federal Brady Act does not meet our criteria for inclusion because, although the regression model evaluated whether changes occurred after implementation of the Brady Act, there was no comparison (control) group. State dealer background checks were found to significantly reduce firearm homicides by 20 percent (see Figure 8.2), but the study’s design cannot distinguish whether this effect is attributable to a state’s implementation of background checks prior to the Brady Act, prohibition of more classes of people from owning guns after the Brady Act was passed, or some combination of the two. Private-seller background checks appeared to increase firearm homicides to levels 131 percent of what would be expected without the policy. However, in sensitivity analyses that restricted the study time frame to the Brady Act period (1994 onward) when all states were required to implement dealer background checks, the estimated effect of state-level private-seller background checks became small and uncertain or significantly negative. Because the authors did not provide information on the timing of state background check laws, it is unclear whether the sensitivity of the results is due to collinearity between state dealer and private-seller background check laws, heterogeneous effects of private-seller background check laws in the later versus earlier period, or some other factor.

La Valle (2013) used variation in when states adopted background checks like those required by the Brady Act to investigate the effects of such laws on firearm homicides and total homicides. Using data from 56 large U.S. cities over 1980–2010, the author found in his preferred models (population-weighted models with a one-year lag of the dependent variable included in the model, controlling for other state gun policies and including other time-varying state covariates that were interpolated over the period) that pre–Brady Act state background check requirements had an uncertain effect on either firearm homicides or total homicides.

Sen and Panjamapirom (2012) examined the effects of the types of background checks conducted by states on homicides. They noted that the supply of state and local records to the NICS is voluntary and that substantial variation exists in state laws regarding the categories of records included in background checks, such as restraining orders, mental illness, fugitive status, and misdemeanors. The authors characterized variation across states in background check requirements using an index of the comprehensiveness of such checks, as well as individual indicators for whether states check on restraining orders, mental illness, fugitive status, misdemeanors, and other miscellaneous records. Using state-level data from 1996 to 2005, the authors examined the effect of these types of checks on both firearm and total homicides. They found that, compared with background checks that examine only criminal history, background checks that include restraining orders, mental illness, and fugitive status are associated with significantly fewer total homicides and firearm homicides. Background checks that include restraining orders were associated with a 13-percent drop in firearm homicide rates and a 9-percent drop in overall homicide rates; background checks for mental illness were associated with a 7-percent drop in both firearm and overall homicide
rates; and background checks for fugitive status were associated with a 21-percent and 23-percent reduction in firearm and total homicide rates, respectively (see Figure 8.2). However, so few states changed criminal history background check or fugitive check policies during the study time frame that these effects cannot confidently be attributed to the background check policies as opposed to other factors affecting homicides in the states around the same time their laws changed. Additionally, the authors did not appear to make any adjustments to account for serial correlation and thus likely overstated the statistical significance of the estimated effects for all components of background checks analyzed. Although the authors also included a control for whether a state had a pre-Brady Act background check requirement, the variation in this policy variable was only across states and not over time because the period of analysis was post-Brady only. Thus, the analysis of the effect of pre-Brady background check policy does not meet our criteria for inclusion.

Lott (2010) examined how state-required background checks for private sales affect violent crime. Detailed results that include coefficients and test statistics were available for only one specification and for the outcome of homicide (Lott, 2010, Table A6.3). This model indicated an uncertain effect of background checks on homicide rates. This model had an unfavorable ratio of estimated parameters to observations (less than one to ten), meaning the estimated effects and significance values may be inaccurate because of model overfitting. Examining a shorter time frame (1979 to 1998) and using negative binomial models with a smaller set of covariates, Hepburn et al. (2004) estimated effects of similar magnitude, also finding that laws requiring background checks before the purchase of a handgun had an uncertain relationship with homicide rates (see Figure 8.2).

Swanson et al. (2013) and Swanson et al. (2016) merged administrative records from public health and criminal justice agencies to evaluate how changes in state reporting of gun-disqualifying mental health records to the NICS affected violent crime arrest rates for individuals with a disqualifying mental health condition relative to individuals diagnosed with a serious mental illness but not prohibited from purchasing a firearm. Swanson et al. (2013) obtained data from 2002 to 2009 for individuals in Connecticut who had been hospitalized for schizophrenia, bipolar disorder, or major depressive disorder. The authors estimated changes in violent crime arrest rates for individuals with at least one of the mental health adjudications reported to the NICS before and after Connecticut began reporting mental health records in 2007. The authors found a significant 31-percent decline in the probability of violent crime arrest in their sample of individuals who had a mental health adjudication but no disqualifying criminal record (see Figure 8.2). The authors also estimated the likelihood of violent crime arrest for individuals with at least one voluntary psychiatric hospitalization but no mental health adjudication. Relative to the legally disqualified population, the nondisqualified group had a lower likelihood of violent crime arrest both before and after the NICS reporting change, but the magnitude of the decrease following NICS
reporting was smaller than the reduction experienced by the “treated” group with a disqualifying mental health condition. However, neither test statistics nor CIs for this difference were reported.

Swanson et al. (2016) employed analogous methods to analyze the effects of NICS reporting changes in 2007 for two Florida counties using data from 2002 to 2011. The authors similarly found a larger reduction in violent crime arrest rates for individuals with a disqualifying mental health history compared with individuals with a serious mental illness that did not prohibit them by law from acquiring a firearm. This difference, a decline of 38 percent (see Figure 8.2), was statistically significant. However, estimates became insignificant when the outcome variable was restricted specifically to violent crimes involving firearms, which could indicate the absence of a causal connection or could be due to measurement error in classifying crimes as involving firearms (Swanson et al., 2016).

Since the first edition of this report, four more studies provide mixed evidence for the effect of background checks on violent crime, although the studies adopt different policy definitions or classifications in evaluating the relationship. Using state-level data from 1977 to 2014, Luca, Malhotra, and Poliquin (2017) evaluated the effects of waiting periods and state dealer background checks on homicide rates among adults aged 21 or older. Their analysis was based on log-linear models adjusting for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors. Although estimates were somewhat sensitive to model specification, they generally showed that background checks had an uncertain relationship with total and firearm homicide rates. Analyzing a similar time frame (1980 to 2013) but focusing on intimate partner homicide, Zeoli et al. (2018) found uncertain effects on total intimate partner homicide rates of comprehensive background check laws and laws that supplement the FBI’s background checks with searches of state databases on prohibited possessors. However, the authors estimated a suggestive effect consistent with comprehensive background check laws increasing firearm-specific intimate partner homicide.

Using county-level data from 1984 to 2015 and restricting their sample to large urban counties (i.e., large central metro or large fringe metro areas with populations exceeding 200,000), Crifasi et al. (2018b) found a significant effect consistent with comprehensive background check policies leading to 16-percent increases in firearm homicides, an effect partially offset by a suggestive decline in nonfirearm homicides. However, these effects were based on a definition of comprehensive background check policies without permit-to-purchase requirements. Separately estimating effects of permit-to-purchase laws, which include background check requirements, the authors’ models instead showed that the permitting requirements significantly reduced firearm homicides by 14 percent (see Chapter Nine). Because the authors made no adjustment for clustering of standard errors at the state level and did not test for overdispersion, the significance values estimated from their model may be invalid.
Kagawa et al. (2018) evaluated how the repeal of comprehensive background check policies affects homicide rates based on repeals in two states, Indiana and Tennessee. The authors used synthetic control methods to construct a counterfactual state based on a weighted combination of states with comprehensive background check laws in effect that provided an adequate match to trends in Tennessee or Indiana prior to each repeal of the background check law. The authors found uncertain effects for both policies on firearm homicide rates, while the estimate from Indiana showed that the repeal was associated with a significant increase in nonfirearm homicide rates. Although analyzing two states with a policy change mitigates some of the concerns with looking at such a change in a single state, these methods remain limited in that they fail to account for potential confounds that may have influenced suicide rates over the post-implementation period. Furthermore, because the study restricts potential control units to states that had comprehensive background check or permit-to-purchase policies over the entire study period, only between seven and 11 states were available to inform the constructed counterfactual; this leads to worse pre-intervention fit and increases the likelihood that any estimated effect represents noise rather than signal.

Finally, Vigdor and Mercy (2006) examined the effects of restraining order and violent misdemeanor background checks on intimate partner homicides and firearm intimate partner homicides by comparing states with more-comprehensive or less-comprehensive approaches to performing those checks. The authors found small differences in rates of such homicides between states with high and low capacities for performing such checks, but they did not provide a test of the significance of these differences.

Figure 8.2 displays the IRRs and CIs associated with the background check policies examined in these studies. Swanson et al. (2013) and Vigdor and Mercy (2006) did not provide sufficient data for us to calculate IRRs and CIs for the effect size of interest, so these are not displayed in the figure. Furthermore, we exclude the estimate of the Brady Act from Gius (2015a) because the estimate does not meet our criteria for inclusion. The Swanson et al. (2016) estimate in the figure is the change from before and after the NICS reporting requirements for legally disqualified individuals relative to the change for nonlegally disqualified individuals.
Figure 8.2  
Incidence Rate Ratios Associated with the Effect of Background Checks on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State dealer background check</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hepburn et al. (2004)</td>
<td>Total</td>
<td>1.02 [0.93, 1.12]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>1.03 [0.87, 1.20]</td>
</tr>
<tr>
<td>Gius (2015a)</td>
<td>Firearm</td>
<td>0.80 [0.73, 0.87]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>1.02 [0.83, 1.26]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>1.04 [0.93, 1.16]</td>
</tr>
<tr>
<td><strong>State private seller background check</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gius (2015a)</td>
<td>Firearm</td>
<td>1.31 [1.23, 1.39]</td>
</tr>
<tr>
<td>Lott (2010)</td>
<td>Total</td>
<td>1.02 [0.98, 1.07]</td>
</tr>
<tr>
<td><strong>Brady Act</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Total, aged 21+</td>
<td>0.97 [0.87, 1.08]</td>
</tr>
<tr>
<td>Monroe (2008)</td>
<td>Total, aged 21+</td>
<td>0.90 [0.53, 1.28]</td>
</tr>
<tr>
<td>La Valle (2013)</td>
<td>Total, age-adjusted</td>
<td>1.00 [0.89, 1.13]</td>
</tr>
<tr>
<td>La Valle (2013)</td>
<td>Firearm, age-adjusted</td>
<td>1.02 [0.89, 1.17]</td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Firearm, aged 21+</td>
<td>0.99 [0.86, 1.13]</td>
</tr>
<tr>
<td>Monroe (2008)</td>
<td>Handgun, aged 21+</td>
<td>0.81 [0.35, 1.26]</td>
</tr>
<tr>
<td>Monroe (2008)</td>
<td>Nonhandgun firearm, aged 21+</td>
<td>0.51 [0.08, 0.95]</td>
</tr>
<tr>
<td>Ludwig &amp; Cook (2000)</td>
<td>Nonfirearm, aged 21+</td>
<td>0.94 [0.87, 1.02]</td>
</tr>
<tr>
<td><strong>Index of background check comprehensiveness</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.93 [0.91, 0.96]</td>
</tr>
<tr>
<td><strong>Check on restraining order</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>0.91 [0.85, 0.98]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.87 [0.79, 0.95]</td>
</tr>
<tr>
<td><strong>Check on mental illness</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>0.93 [0.86, 0.99]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.93 [0.87, 1.01]</td>
</tr>
<tr>
<td><strong>Check on fugitive status</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>0.77 [0.71, 0.84]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.79 [0.72, 0.88]</td>
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<tr>
<td><strong>Check on misdemeanor</strong></td>
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<td></td>
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<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>1.02 [0.95, 1.10]</td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Firearm</td>
<td>0.99 [0.90, 1.08]</td>
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<tr>
<td><strong>Check on other miscellaneous records</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sen &amp; Panjamapirom (2012)</td>
<td>Total</td>
<td>1.12 [1.03, 1.22]</td>
</tr>
</tbody>
</table>
| Sen & Panjamapirom (2012)                            | Firearm                          | 1.05 [0.98, 1.13]       

Figure continued on the next page
Figure 8.2—Continued

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Point-of-contact background check</strong></td>
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<tr>
<td>Zeoli et al. (2018)</td>
<td>IPH</td>
<td>0.98 [0.91, 1.07]</td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Firearm-related IPH</td>
<td>1.00 [0.90, 1.11]</td>
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<tr>
<td><strong>Comprehensive background check</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>IPH</td>
<td>1.07 [0.94, 1.21]</td>
</tr>
<tr>
<td>Zeoli et al. (2018)</td>
<td>Firearm-related IPH</td>
<td>1.13 [0.94, 1.35]</td>
</tr>
<tr>
<td><strong>Comprehensive background check, no permit to purchase</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crifasi et al. (2018b)</td>
<td>Firearm, large urban counties</td>
<td>1.16 [1.13, 1.18]</td>
</tr>
<tr>
<td>Crifasi et al. (2018b)</td>
<td>Nonfirearm, large urban counties</td>
<td>0.97 [0.94, 1.01]</td>
</tr>
<tr>
<td><strong>Repeal of comprehensive background check law, Ind.</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Kagawa et al. (2018)</td>
<td>Firearm, age-adjusted</td>
<td>1.22 [0.73, 1.71]</td>
</tr>
<tr>
<td>Kagawa et al. (2018)</td>
<td>Nonfirearm, age-adjusted</td>
<td>1.23 [0.90, 1.60]</td>
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<tr>
<td><strong>Repeal of comprehensive background check law, Tenn.</strong></td>
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<td></td>
</tr>
<tr>
<td>Kagawa et al. (2018)</td>
<td>Firearm, age-adjusted</td>
<td>1.08 [0.82, 1.34]</td>
</tr>
<tr>
<td>Kagawa et al. (2018)</td>
<td>Nonfirearm, age-adjusted</td>
<td>1.06 [0.85, 1.27]</td>
</tr>
<tr>
<td><strong>NICS reporting, Fla.</strong></td>
<td>Violent crime arrest rate</td>
<td>0.62 [0.50, 0.76]</td>
</tr>
</tbody>
</table>

NOTE: The study model in La Valle (2013) includes a one-year lag of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details. IPH = intimate partner homicide.

Conclusions

**Homicides and violent crime.** We identified nine qualifying studies providing evidence on the effects of background checks, or some component of background checks, on violent crime. Six of these studies provided an overall effect of either dealer background checks or private-seller background checks on total homicide rates, although three of these estimated effects were partially confounded with the effect of waiting periods that were simultaneously introduced in many states when the Brady Act was passed. All six studies found those effects to be uncertain: the analyses of effects on those aged 21 or older in Ludwig and Cook (2000), Monroe (2008), and Luca, Malhotra, and Poliquin (2017); the Brady Act effect in La Valle (2013); the state dealer background check effect in Hepburn et al. (2004); and the private-seller background check effect in Lott (2010). One additional study (Zeoli et al., 2018) found uncertain effects of comprehensive background checks and point-of-contact background checks on intimate partner homicide rates.
Three background check component analyses identified significant effects indicating that mental illness checks, restraining order checks, or fugitive status checks reduced violent crime specific to homicides (Sen and Panjamapirom, 2012). A fourth component analysis found that mental illness checks significantly reduced violent crime arrests (Swanson et al., 2016). A component analysis of misdemeanor checks found that they had uncertain effects on homicides, while “other miscellaneous checks” had a suggestive effect consistent with increases in homicides (Sen and Panjamapirom, 2012).

The cumulative evidence is puzzling, as overall effects of background checks appear to be uncertain, but some components of background checks appear to significantly reduce homicides or violent crime. Because the studies examining component effects of background checks generally suffer from more-noted weaknesses, we conclude that available studies provide inconclusive evidence for the effect of background checks on violent crime and total homicide rates.

Firearm homicides. We identified nine qualifying studies that provided estimates for the effects of background checks, or some component of background checks, on firearm homicide rates. Six studies examined the overall effect of dealer background checks on firearm homicide rates: Two used large independent data sets and found significant effects indicating that dealer background checks reduce firearm homicides (Gius, 2015a; Sen and Panjamapirom, 2012), one found significantly reduced rates of nonhandgun firearm homicides (Monroe, 2008), and three found uncertain effects for the relationship (Luca, Malhotra, and Poliquin, 2017; La Valle, 2013; Ludwig and Cook, 2000). Four studies examined the effect of private-seller background check policies. One analysis found significant effects consistent with private-seller
checks increasing firearm homicides (Gius, 2015a), although these estimates became uncertain or significant in the opposite direction in different specifications. Another study found suggestive effects consistent with private-seller checks increasing firearm intimate partner homicides (Zeoli et al., 2018). Crifasi et al. (2018b) found that comprehensive background checks without permit-to-purchase requirements were associated with significant increases in firearm homicides in urban areas, whereas a study of the effects of repealing comprehensive background check policies (Kagawa et al., 2018) found uncertain effects of the policy change. Component analyses from a single study found significant effects indicating that restraining order and fugitive checks reduce firearm homicides (Sen and Panjamapirom, 2012). The analyses also found that mental illness adjudication checks have suggestive effects consistent with a reduction in firearm homicides, uncertain effects for misdemeanor checks, and a significant effect indicating that “miscellaneous checks” increase firearm homicides.

Considering these findings and an assessment of the relative strengths of the studies, we conclude that available studies provide moderate evidence that dealer background checks may reduce firearm homicides and inconclusive evidence for the effect of private-seller background checks on firearm homicides.

Effects on Mass Shootings

Research Synthesis Findings
We identified three studies examining the effects of background check policies on mass shootings in the United States. Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2016) estimated the effects of background check laws on a binary indicator for whether a mass shooting occurred in a given state-year. The authors included two measures of background check laws: an indicator for whether laws required a background check for all handgun transactions (including private sales) and an indicator for whether laws required a background check for all firearm transactions (including private sales). The authors’ regression analysis covered 1989–2014 and included controls for time-invariant state characteristics; national trends; a host of other state-level gun policies; and time-varying state-level demographic, socioeconomic, and political characteristics. Their findings showed an uncertain relationship between background check laws and the probability of at least one mass shooting event occurring (see Figure 8.3). However, assessing the effects of gun policies on mass shootings was not the primary focus of the study, and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a rare dichotomous outcome, and such a procedure makes CIs unreliable.
Examining a similar time frame, Gius (2018) evaluated how background check laws relate to total deaths and injuries from school shootings using data compiled from Klein (2012), Kalesan et al. (2017), and the Everytown for Gun Safety Support Fund. The author separately considered the effects of state laws requiring background checks for private sales and the federal law requiring background checks on gun purchases from licensed dealers, although the latter analysis did not meet our inclusion criteria because all states were exposed over the same period (i.e., there was no comparison group). The author estimated a population-weighted Poisson model that controlled for two other gun laws (concealed-carry laws and the assault weapon ban), fixed effects for state and year, state-level socioeconomic and demographic factors, and the ratio of firearm suicides to total suicides as a proxy for gun ownership. Given how rare these outcomes are—there was an average of 14 school shootings and 15 injuries from school shootings per year across the United States—it is not surprising that the author found only an uncertain relationship between state background check laws and the number of casualties from school shootings (see Figure 8.3).

Kalesan et al. (2017) also examined how background check laws are associated with school shootings but looked at data over the much shorter period of 2013 to 2015. Using negative binomial models, the authors considered two types of background check laws: those for firearm purchase and those for ammunition purchase. Although they found large and significant negative associations with the number of school shooting incidents and both types of background check laws (see Figure 8.3), their analyses did not adjust for any covariates besides a linear time trend. The fact that their methods were unable to account for any differences across states that may be correlated with both the presence of background check laws and the incidence of school shootings limits the ability to attribute causality to these laws. Furthermore, because very few states implemented the policies of interest over the short time frame of the study (e.g., Connecticut was the only state that moved from no background checks to background checks for ammunition over the study period), and because the models did not control for other potential differences across states, the authors’ estimates most likely reflect differences between states that have the laws and states that do not rather than differences within a state before and after the law was passed. Thus, this study is not likely to provide valid insights into the causal effects of background check laws on school shootings.

Figure 8.3 displays the IRRs and CIs associated with the background check policies examined in Luca, Malhotra, and Poliquin (2016) and Gius (2018). Given the limited ability of the analyses of Kalesan et al. (2017) to demonstrate a causal relationship between background check policies and outcomes of interest, we do not include estimates from that study or incorporate its findings in our conclusions.
Conclusions
We identified two qualifying studies that estimated the effects of background checks on mass shootings or school shootings. One study estimated how background checks for all handgun sales and for all firearm sales affect mass shootings and found uncertain effects of these universal background check laws on whether at least one mass shooting occurred in a state (Luca, Malhotra, and Poliquin, 2016). Another study found uncertain effects of background checks for private sales on school shooting casualties (Gius, 2018). Considering the methodological limitations in both studies, we consider there to be inconclusive evidence for the effect of background checks on mass shootings.

Effects on the Gun Industry
Research Synthesis Findings
We identified one study that examined differences in federal firearms licensee (FFL) dropout rates between 1994 and 1998, comparing dropout rates in states without a background check system in place prior to the 1994 Brady Act with states that did
have background check requirements in place prior to the federal law (Koper, 2002). Examining the universe of FFLs active as of summer 1994, slightly after the Brady Act was implemented, the author found an uncertain relationship between the implementation of background check requirements and the odds of FFL exit from the market by 1998 (see Figure 8.4). It should be noted that the focus of this study was on better understanding the role of recently closed dealers on supplying the criminal market for firearms, partly by estimating the association between (1) dealer characteristics and potential proxy indicators of gun trafficking (e.g., the number of Bureau of Alcohol, Tobacco, Firearms and Explosives [ATF] gun traces linked back to a given FFL) and (2) dealer dropout rates during a period of reforms to the federal firearms licensing system. Given that this time frame was characterized by several new requirements for FFLs (e.g., requirements for FFLs to report inventory thefts and losses to ATF within two days, enhanced ATF initiatives to ensure that licensees were conducting legitimate operations), which likely varied in their impact depending on the preexisting state legal environment, these findings cannot be attributed to background check requirements alone.

Figure 8.4 displays the IRRs and CIs associated with the background check policies examined in Koper (2002).

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**Figure 8.4**

*Incidence Rate Ratios Associated with the Effect of Background Checks on the Gun Industry*

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Brady Act</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Koper (2002)</td>
<td>FFL exit</td>
<td>1.02 [0.98, 1.06]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

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15 We excluded one study (Brauer, Montolio, and Trujillo-Baute, 2017) that examined the associations between (1) the number of firearm manufacturers and the number of NICS background checks for firearm purchases and (2) an index for comprehensive background checks and permit-to-purchase laws. However, the ratio of parameters to observations was less than one to five for these outcomes, so these estimates do not meet our inclusion criteria.
Conclusions
We identified a single qualifying study that provided estimates for the effect of background checks on dropout rates of FFLs. Exploiting the differential effects of the Brady Act on states depending on their preexisting background check laws, the author found an uncertain effect of the federal background check requirements on the likelihood that an FFL dropped out of the market. Therefore, the available study provides inconclusive evidence for the effect of background checks on the gun industry.

Outcomes Without Studies Examining the Effects of Background Checks
We did not identify any studies that met our inclusion criteria and examined the effects of background check policies on the following outcomes:

- unintentional injuries and deaths
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Eight References


United States Code, Title 18, Section 922, Unlawful Acts.


Federal law does not require individuals to obtain a license or permit to purchase a firearm. Several states, however, have permit-to-purchase laws that function similarly to universal background check laws. Both seek to ensure that individuals who acquire firearms through private transfers meet the same requirements as those who purchase firearms from federally licensed dealers. State policies that require permits or licenses to be renewed create a mechanism whereby law enforcement routinely confirms that a firearm owner remains eligible to possess or purchase a firearm, and the policies could facilitate firearm removal from owners who become ineligible. Requiring permits to purchase ammunition makes it more difficult for prohibited possessors to use their illicit firearms. Where no such checks occur, prohibited possessors may represent a considerable share of the market for ammunition. For instance, in a two-month period in the City of Los Angeles, prohibited possessors purchased at least 10,500 rounds of ammunition, accounting for about 2.6 percent of all such sales (Tita et al., 2006). The effects of these policies on violent crime and suicide will depend on whether they better identify disqualified firearm purchasers or possessors compared with the status quo, as well as whether these disqualifications correctly target individuals who are at greater risk of inflicting harm to themselves or others.

As with comprehensive background check laws, by restricting access to firearms for individuals presumed to present greater risk of misusing those firearms, licensing and permitting requirements are intended to reduce gun violence. Different designations for the types of conditions that disqualify an individual may generate differential impacts on such outcomes as homicide or mass shootings compared with suicides. Although compliance is likely to be imperfect, licensing and permitting laws may still reduce gun-related homicides or suicides by deterring prohibited possessors who do not already own firearms from attempting to acquire them. The magnitude of these effects will be influenced, in part, by the level of enforcement, the availability of firearms or ammunition through unregulated markets, and the likelihood that an individual who would be disqualified through the permitting process will seek to obtain a firearm through alternative markets.

Unlike background check laws, licensing and permitting regulations often require individuals seeking to purchase or possess a firearm to submit their applications in
person at a law enforcement agency and to submit to fingerprinting. There is some older evidence that even licensed dealers sometimes fail to require valid identification cards (U.S. General Accounting Office, 2001); thus, these additional procedural requirements may be more effective in limiting prohibited possessors from accessing firearms by preventing fraud or identification inaccuracies. However, licensing systems requiring substantial coordination among local, state, and federal databases and institutions may pose technical and regulatory challenges, and it is unknown how much the additional administrative requirements of licensing and permitting laws will reduce firearm access by prohibited individuals. Moreover, most firearms are purchased by individuals who already own a firearm. Azrael et al. (2017) found that, on average, gun owners had close to five firearms each, and a large majority (62 percent) purchased their most recent weapon from a licensed gun dealer. For those who already own guns, licensing and permitting regulations may have little or no effect on crime or suicide risk.

State laws that additionally require an individual to pass a safety course or exam to qualify for a license or permit could reduce unintended injuries and deaths, although these effects will depend on whether passing a safety course or exam affects the storage or handling behavior of firearm owners. A recent survey of firearm storage practices among adults in the United States found that 32 percent of gun owners who completed firearm training stored one or more firearms loaded and unlocked, compared with 26 percent of individuals who were not trained (Berrigan et al., 2019). These results suggest that existing trainings may not reduce unsafe firearm storage. On the other hand, one 1995 survey found that gun owners who received formal firearm training (where 80 percent of training courses covered proper gun storage) were significantly more likely to store their firearms loaded and unlocked compared with gun owners who had not received formal training; however, the most common source of training for this sample was through the military, which may not produce the same effects as the training available to civilians (Hemenway, Solnick, and Azrael, 1995). For further discussion of safety training laws, see Chapter Eleven.

These laws could also plausibly affect defensive or recreational gun use by increasing the costs of obtaining or continuing to possess a firearm. Although the monetary costs of acquiring a license or permit typically range between $10 and $100,1 the total time and energy costs, in addition to concerns about privacy, may dissuade some legal firearm purchasers, in which case the laws could affect sales of new firearms.

To evaluate whether the effects of licensing or permitting requirements on violent crime or suicides operate through more-effective identification of prohibited possessors (as applied to purchase, possession, or both), the ideal analyses would estimate effects on outcomes specifically for those populations that would be prevented from legally acquiring or owning a firearm under the licensing law. For outcome data in which the

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1 New York City’s license for handgun purchase and possession (which lasts three years) is the most expensive, at $340, not including an additional fingerprint fee (Csere, 2013).
type of weapon used can be identified, analyses also could exploit state-level variation in the types of guns that require licenses or permits and could estimate effects stratified by the type of weapon used in a violent crime, mass shooting, or suicide.

To assess whether licensing or permitting laws reduce violent crime through disrupting illegal firearm trafficking, causal inference could be strengthened by examining crime gun trace data and changes in homicide rates. Specifically, if permit-to-purchase laws restrict trafficking operations from in-state retailers, one should observe a larger share of crime guns originating from out-of-state sources after law passage, as well as a reduction in guns with a short time-to-crime (Webster and Wintemute, 2015; Braga et al., 2012). However, a series of provisions attached to Bureau of Alcohol, Tobacco, Firearms and Explosives appropriations (commonly known as the Tiahrt Amendments) has denied most researchers access to firearm trace data since 2003; therefore, while law enforcement agencies may analyze such data, the information generally has not been available for research purposes (Krouse, 2009).

State Implementation of Licensing and Permitting Requirements

As of January 1, 2020, 13 states and the District of Columbia have licensing or permitting requirements. These most commonly take the form of permits to purchase, but this policy class can also include licenses to own, registration, and firearm safety certificates.

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2 The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined crime gun as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”

3 Per Webster and Wintemute (2015), the metric known as time-to-crime is the “unusually short interval—ranging from less than 1 year to less than 3 years—between a gun’s retail sale and its subsequent recovery by police from criminal suspects or crime scenes . . . . A short [time-to-crime] is considered an indicator of diversion, especially when the criminal possessor is someone different from the purchaser of record.”


5 Connecticut, Hawaii, Iowa, Maryland, Massachusetts, Michigan, Nebraska, New Jersey, New York, North Carolina, Rhode Island, and the District of Columbia.

6 Illinois, Massachusetts, and Michigan.

7 District of Columbia.

8 California.
Six states and the District of Columbia have implemented licensing or permitting regimes for all firearms,9 and seven states have done so for handguns.10 Michigan’s law has a broad exemption for individuals who purchase handguns from licensed dealers following a background check.11

Some states require that applicants pass a safety course or exam in order to receive a license or permit.12 Another distinction between states’ laws is the duration of the credentials. A handful of states issue licenses or permits that are valid for a few days or months only,13 while those issued in other states may last years.14 In New Jersey, firearm identification cards are required for rifles and shotguns and remain valid indefinitely, unless the issuing or other law enforcement agency identifies specific behavior and character disqualifiers—such as being convicted of a crime, being subject to a restraining order, or having a drug dependency; for handguns, purchasers must obtain a permit to purchase, which lasts 90 days.15 Rhode Island’s law does not specify how long the permit to purchase is valid.16

Another feature that differs among the state regimes is whether the credential covers multiple purchases. The laws in Hawaii, New Jersey, and North Carolina require separate permits for each purchase, though with some differences.17 For example, Hawaii requires a permit for each handgun purchase but allows multiple long-gun purchases under a single permit.

Some of the aforementioned jurisdictions have also extended their licensing and permitting systems to the purchase or ownership of ammunition.18

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10 Iowa, Maryland, Michigan, Nebraska, New York, North Carolina, and Rhode Island.


13 Hawaii Rev. Stat. § 134-2 (ten days for handguns); Mass. Gen. Laws Ch. 140 § 131A (ten days for permit to purchase); Mich. Comp. Laws § 28.422 (30 days for handguns); N.J. Stat. Ann. § 2C:58-3 (90 days for handguns, may be renewed for another 90 days with good cause).

14 Calif. Penal Code § 31655 (five years); Conn. Gen. Stat. §§ 29-36h, 29-37r (five years); Hawaii Rev. Stat. § 134-2 (one year for long guns); 430 Ill. Comp. Stat. 65/7 (ten years); Md. Code, Pub. Safety, § 5-117.1 (ten years for handguns); Mass. Gen. Laws Ch. 140 § 129B (six years for license to own); Neb. Rev. Stat. Ann. § 69-2407 (three years for handguns); N.Y. Penal Law § 400.00 (five years for handguns); N.C. Gen. Stat. § 14-403 (five years for handguns); D.C. Code Ann. § 7-2502.07a (three years).


Effects on Suicide

Research Synthesis Findings
We identified four U.S.-based longitudinal studies examining the effect of firearm licensing or permitting requirements on suicide. Examining the effects of firearm policies on suicides among teens (aged 14–17) and young adults (aged 18–20) between 1976 and 2001, Webster et al. (2004) included an indicator variable for the presence of state permit-to-purchase laws. They used negative binomial models that employed generalized estimating equations and included state-level fixed effects, controls for other firearm policies, and time-varying covariates (including the proportion of suicides by firearm as a proxy of gun prevalence). Using these methods, the authors found that permit-to-purchase laws significantly increased the total suicide rate by 17.7 percent among those aged 18–20, driven by an estimated 22-percent increase in firearm suicides, with an uncertain change in nonfirearm suicides. The authors also found permit-to-purchase laws to be associated with a statistically significant 27-percent increase in nonfirearm suicides among those aged 14–17 but to have uncertain associations with firearm or total suicides among this age group. As the authors suggested, this perplexing set of results may be partially attributable to the fact that the effect estimate was based on changes to only three state laws during the study time frame. Therefore, the effect of permit-to-purchase laws is not well identified, and apparent effects may be attributable to other concurrent changes affecting suicide rates.

Using similar methods but a more recent time frame (1995 to 2004), Andrés and Hempstead (2011) estimated the effect of permitting requirements on men’s suicide rates, considering differential effects across age groups. Although the authors found that permitting requirements were associated with significantly reduced rates of suicide among men aged 45 or older, the estimated effects of the policy were positive and significant for men aged 15–24. However, the study models had a ratio of estimated parameters to observations of about one to seven, the authors did not appear to adjust standard errors to account for serial correlation, and there was virtually no variation in state permit laws over the study period. Together, these limitations raise serious questions about the validity of the identified effects for establishing how permitting requirements affect suicide.

Using a synthetic control approach, Crifasi et al. (2015) estimated the percentage change in total suicide and firearm suicide in Connecticut before and after the state established a permit-to-purchase law in 1995, as well as before and after the repeal of Missouri’s permit-to-purchase law in 2007. This approach enabled the researchers to estimate the likely outcomes had Connecticut and Missouri not enacted these laws, drawing on data from states that looked most similar in the pre-law period but that did not have or enact such policies (for Connecticut) or that had such policies and did not repeal them (for Missouri) during the study period.
Crifasi et al. (2015) found evidence that there was a reduction in firearm suicide rates in Connecticut and its synthetic comparison group after the law, but the reduction was greater in Connecticut. Specifically, the authors found a suggestive effect indicating that Connecticut’s firearm suicide rate was 15.4 percent lower than that of its synthetic control during the ten-year post-law period, decreasing from roughly four firearm suicides per 100,000 people the year the law was enacted to around three per 100,000 in the post-law period. The nonfirearm suicide rate remained constant in Connecticut but increased in its synthetic comparison group after the law. However, these findings were tempered by alternative regression model specifications in which Connecticut experienced a statistically significant increase in nonfirearm suicides after passage of the law and an uncertain effect on overall suicides.

Missouri’s firearm suicide rate was consistently higher than that of its synthetic control, and rates in both the state and its synthetic control increased after the repeal of the law, although Missouri’s rate grew more rapidly over the subsequent five years. Findings from the study’s primary analyses showed uncertain differences in firearm and nonfirearm suicide rates between Missouri and its synthetic control in the five-year post-repeal period.

Because both the Connecticut and Missouri analyses examined only a single state’s experience with either adoption or repeal of the law, the study offers limited evidence that noted differences are due to the change in the law rather than to other contemporaneous influences over each state’s suicide rate around the time the law was changed. For instance, in Connecticut, the permit-to-purchase law was implemented along with other rule changes, such as raising the minimum age to purchase handguns and requiring completion of eight hours of gun-safety training. Similarly, Missouri’s repeal occurred at the same time it implemented a stand-your-ground law. The study design cannot rule out that these other factors, rather than the permit-to-purchase requirement, were the cause of observed changes. Therefore, the estimates reported in Crifasi et al. (2015) may not be reliable indicators of the direction or magnitude of the true effects of permit-to-purchase laws on suicide.

Finally, a 2017 study of waiting periods and background checks included supplementary analyses that presented estimates for several other firearm policies, including handgun permit requirements (Luca, Malhotra, and Poliquin, 2017). The study models, which specified a linear relation between logged suicide rates and the independent variables, covered a longer period than previous studies (from 1970 or 1977 through 2014). After adjusting for state-level socioeconomic factors, age distribution, percentage black, percentage urban, and alcohol consumption, the authors estimated that handgun permit requirements were associated with significantly reduced rates of total and firearm suicides for adults aged 21 or older, with suggestive negative effects for nonfirearm suicides.

Figure 9.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the licensing and permitting policies examined in these studies.
Figure 9.1  
Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Permit requirement</strong></td>
<td><strong>Total suicide rate</strong></td>
<td></td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Men, aged 15–24</td>
<td>1.20 [1.13, 1.29]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Men, aged 25–44</td>
<td>0.97 [0.94, 1.01]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Men, aged 45–64</td>
<td>0.86 [0.83, 0.90]</td>
</tr>
<tr>
<td>Andrés &amp; Hempstead (2011)</td>
<td>Men, aged 65+</td>
<td>0.85 [0.82, 0.89]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 14–17</td>
<td>1.06 [0.92, 1.23]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 18–20</td>
<td>1.18 [1.04, 1.34]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Aged 21+</td>
<td>0.91 [0.85, 0.98]</td>
</tr>
<tr>
<td><strong>Firearm suicide rate</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 14–17</td>
<td>0.92 [0.76, 1.10]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 18–20</td>
<td>1.22 [1.04, 1.43]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Aged 21+</td>
<td>0.90 [0.84, 0.97]</td>
</tr>
<tr>
<td>Crifasi et al. (2015)</td>
<td>All ages</td>
<td>0.85 [0.64, 1.06]</td>
</tr>
<tr>
<td><strong>Nonfirearm suicide rate</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 14–17</td>
<td>1.27 [1.00, 1.61]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Aged 18–20</td>
<td>1.14 [0.93, 1.39]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Aged 21+</td>
<td>0.94 [0.87, 1.02]</td>
</tr>
<tr>
<td>Crifasi et al. (2015)</td>
<td>All ages</td>
<td>0.88 [0.63, 1.13]</td>
</tr>
<tr>
<td><strong>Repeal of permit law</strong></td>
<td><strong>Suicide rate</strong></td>
<td></td>
</tr>
<tr>
<td>Crifasi et al. (2015)</td>
<td>Firearm</td>
<td>1.16 [0.79, 1.54]</td>
</tr>
<tr>
<td>Crifasi et al. (2015)</td>
<td>Nonfirearm</td>
<td>1.04 [0.89, 1.20]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
Conclusions

We identified four qualifying studies examining the effects of permit-to-purchase laws on total and firearm suicides. Among children aged 14–17, Webster et al. (2004) identified an uncertain effect of these laws on total suicide and firearm suicide rates, as well as a significant effect consistent with an increase in nonfirearm suicides. They also identified a significant increase in suicides and firearm suicides among those aged 18–20. Andrés and Hempstead (2011) found mixed results, with permitting requirements resulting in significantly lower rates of suicide among men aged 45 or older but significantly higher rates of suicide among men aged 15–24 (effects were suggestive and negative for men aged 25–44). Crifasi et al. (2015) identified the effect of implementing a permit-to-purchase law in Connecticut and a separate effect of repealing such a law in Missouri. Both sets of effects suggested that these changes in law had uncertain effects on total suicides. However, implementation of the law led to suggestive reductions for firearm suicides in Connecticut, whereas repeal of the law in Missouri had only uncertain effects on firearm suicides. Finally, results from Luca, Malhotra, and Poliquin (2017) indicated that handgun permit requirements were associated with significantly reduced rates of total and firearm suicides for adults aged 21 or older.

Considering these studies, we find limited evidence that licensing and permitting requirements decrease total suicides and firearm suicides among adults but inconclusive evidence for the effect of licensing and permitting requirements on total suicides and firearm suicides among minors.
Effects on Violent Crime

Research Synthesis Findings
Our synthesis identified seven studies that examined the effects of permit-to-purchase laws on violent crime. Webster, Crifasi, and Vernick (2014) used state-level data from 1999 to 2010 to analyze the effect of Missouri’s repeal of a permit-to-purchase law that included a background check requirement even for private sellers and a requirement that background checks be requested at the local sheriff’s office. They found a significant increase in total homicides and firearm homicides from the repeal of the law and an uncertain effect on nonfirearm homicides. Specifically, after the repeal, the total homicide rate was 115 percent of the rate expected had the law not been repealed, and the firearm homicide rate was 125 percent of the expected rate (see Figure 9.2). However, because the focus of this study was a single state, the effects associated with the law may be confounded with other changes in the state that affected homicide rates around the same time the law was repealed. The statistical model used to arrive at these results used a large number of estimated parameters relative to observations (a ratio of about one to eight), meaning the model may have been overfit, and thus its estimates of the laws’ effects and their apparent statistical significance could provide little generalizable information about the true causal effects of the permit-to-purchase law.

Using a synthetic control approach, Rudolph et al. (2015) found a significant decrease in firearm homicides (and no statistically significant effect on nonfirearm homicides) from the implementation of a permit-to-purchase law in Connecticut that strengthened background check requirements for handguns sold by private sellers and licensed dealers by requiring purchasers to obtain an eligibility certificate in person from the local police department, increasing the minimum age of purchase from 18 to 21, and requiring individuals to complete eight hours of gun-safety training. After these policy changes, the firearm homicide rate was 63 percent of what was expected without such changes. Because only a single state experienced the law in this study, it is not possible to conclude that the changes were a result of the permit-to-purchase portion of the law as opposed to other factors influencing homicides in the state around the same time.

Crifasi, Pollack, and Webster (2016) also analyzed the effects of Missouri’s repeal and Connecticut’s enactment of permit-to-purchase laws, but the outcome of interest in this study was assault on law enforcement officers between 1984 and 2013. For fatal assaults on law enforcement, the authors’ two-way fixed-effects negative binomial regression models showed uncertain effects of permit-to-purchase repeal in Missouri and enactment in Connecticut for both handgun-related and nonhandgun-related

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19 Although La Valle (2013) includes license-to-own and permit-to-purchase regulations as covariates in his models for total and firearm homicide rates, no states enacted or repealed such laws during the period of study (based on the paper’s Table 1). Thus, the estimates for these policies in La Valle (2013) do not meet our inclusion criteria.
assaults, although the estimated effect of Connecticut’s policy on fatal handgun-related
assaults of law enforcement officers was suggestive and negative ($p = 0.16$). The authors
identified a suggestive increase in nonfatal assaults on law enforcement officers after
the repeal of Missouri’s policy, attributable to a more than 200-percent increase in
handgun-related assaults (IRR = 2.14; 95-percent CI = 0.89, 5.14). However, the results
of this study, based on law changes in two states analyzed separately, are subject to the
same methodological concerns that we noted for Rudolph et al. (2015)—that is, poten-
tial other confounders that influenced assaults on law enforcement officers around the
time of the law change in these two states. Additionally, with few treated policy units,
the clustering adjustment to the standard errors in this study likely resulted in under-
estimated standard errors and unreliable CIs (Cameron, Gelbach, and Miller, 2008;
Schell, Griffin, and Morral, 2018).

Three state-level studies using state-fixed-effects models and more than two
decades of data produced estimates for the effects of permitting laws. Specifically,
Gius (2017) examined the effects on firearm murder rates; Luca, Malhotra, and
Polyquin (2017) examined the effects on total and firearm homicide rates among adults
aged 21 or older; and Zeoli et al. (2018) examined the effects on total and firearm-
specific intimate partner homicide rates. Study models accounted for different but
overlapping sets of state-level covariates, and they estimated effects based on different
model assumptions (i.e., linear, linear in logged outcome, or negative binomial regres-
sion specifications). Both studies that examined total homicide rates—either among
adults aged 21 or older or among intimate partners—found uncertain effects of the
laws. The same two studies also found uncertain effects for firearm-specific homicides
among the populations considered. Gius (2017) presented two specifications for the
law’s effects on firearm-related murder rates, neither of which was significant. Because
Gius (2017) indicated no preference for one or the other specification, we display effects
in Figure 9.2 for the first presented specification, the linear model.

Finally, Crifasi et al. (2018b) used county-level data to estimate the effects of
permit-to-purchase laws on firearm and nonfirearm homicide rates in large central and
fringe metro counties with populations greater than 200,000 across the study period
of 1984 to 2015. The authors explicitly controlled for the existence of comprehensive
background check policies without a permitting requirement, as well as stand-your-
ground laws, shall-issue laws (see Chapter Eighteen), prohibitions associated with a
violent misdemeanor offense, and a small set of county-level sociodemographic factors
and state-level criminal justice measures. Using Poisson regression models that control
for flexible national trends and county random effects, Crifasi et al. (2018b) found that
permit-to-purchase regimes were significantly associated with 14 percent lower rates of
firearm homicide; estimates for nonfirearm homicide rates were uncertain. However,
the authors were able to examine only three states that passed permit-to-purchase laws
during the period of study; made no adjustment for clustering of standard errors at the
state level; and did not report a test for overdispersion, which threatens the validity of the significance values estimated from the model.

Figure 9.2 displays the IRRs and CIs associated with the licensing and permitting policies examined in these studies.

**Note:** IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details. LEO = law enforcement officer.
Conclusions
We identified seven qualifying studies examining the effects of permit-to-purchase laws on total and firearm homicides. Webster, Crifasi, and Vernick (2014) found that Missouri’s repeal of its law resulted in increased total and firearm suicide rates. Rudolph et al. (2015) reported a significant effect consistent with these laws reducing firearm homicide rates, but because a law establishing a minimum age for purchase was passed concurrently in the one state evaluated, they could not attribute this effect solely to permit-to-purchase laws. Crifasi, Pollack, and Webster (2016) found uncertain effects of changes to permitting laws in two states on fatal attacks on police officers, as well as one effect suggestive of the possibility that handgun assaults on officers may have decreased after the passage of Connecticut’s law. Three other studies found uncertain effects of permitting laws on firearm murder rates (Gius, 2017), total and firearm homicide rates (Luca, Malhotra, and Poliquin, 2017), and total and firearm intimate partner homicide rates (Zeoli et al., 2018). Finally, Crifasi et al. (2018b) reported a large (14-percent) reduction in firearm homicides in urban counties after the passage of permit-to-purchase laws. Considering this evidence and an evaluation of the studies’ strengths, we find inconclusive evidence for the effect of licensing and permitting requirements on total homicides and firearm homicides.

Effects on Mass Shootings
Research Synthesis Findings
Our search yielded two studies that met our inclusion criteria and examined the effects of licensing and permitting requirements on mass shootings in the United States.20 Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2016) estimated the effects of state laws requiring permits to purchase a handgun on a binary indicator for whether a mass shooting occurred in a given state-year.

20 These studies defined mass shootings differently. One defined a mass shooting as an incident in which four or more individuals were killed (excluding the shooter), the occurrence was not connected to criminal activity, and at least three fatally injured victims were not related to the shooter (Luca, Malhotra, and Poliquin, 2016). The other considered mass shootings (carried out by a single person, happened during a single incident, and occurred in a public place with at least four fatalities), spree shootings (carried out by a single person, happened across multiple locations with no break in time between the shootings, and occurred in a public place with at least two fatalities), and active shooter incidents (involved one person killing or attempting to kill people, occurred in a populated area, and involved firearms) (Blau, Gorry, and Wade, 2016).
The authors’ regression analysis covered 1989–2014 and included controls for time-invariant state characteristics; national trends; a host of other state-level gun policies; and time-varying state-level demographic, socioeconomic, and political characteristics. They found uncertain effects of handgun permitting requirements on the probability of a mass shooting event occurring. However, assessing the effects of gun policies on mass shootings was not the primary focus of Luca, Malhotra, and Poliquin (2016), and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a rare dichotomous outcome. Therefore, model assumptions were likely violated, making CIs unreliable.

The findings from Blau, Gorry, and Wade (2016) face similar limitations because the authors also used a linear probability model to estimate how requirements regarding permits to purchase and licenses to possess a firearm relate to the probability of a public shooting incident. Using data from 1982 to 2013, they estimated models that adjusted for state fixed effects and a linear time trend, as well as for the presence of several other state gun laws and a limited set of state covariates (i.e., population size and aggregated personal income). The authors found uncertain effects of permit-to-purchase regimes on the likelihood of a public shooting event. Although their estimates of the effect of permit requirements for firearm ownership were suggestive of a negative relationship with the incidence of a public shooting, their use of a linear model to predict a dichotomous (and rare) outcome likely violated model assumptions and rendered the results unreliable. The authors’ estimated linear probability model can yield predicted probabilities of active shooting incidence that extend far outside the definitional 0 to 1 range of a probability, depending on the particular combination of policies present in a given state. Moreover, the estimated model implies negative IRRs, which represent implausible effect sizes, for some of the gun policies that we are studying. This indicates a serious model misspecification (Cox and Snell, 1989; Aldrich and Nelson, 1984) and prevents us from interpreting the estimated coefficients as causal effect estimates. On these grounds, we discount the results for purposes of clarifying the effects of the law.

Figure 9.3 displays the IRR and CI associated with the licensing and permitting policies examined in Luca, Malhotra, and Poliquin (2016). We exclude estimates of effects from Blau, Gorry, and Wade (2016) because of the noted concerns with the study’s results.

21 For example, the model coefficient for stand-your-ground laws implies an IRR of –2, with a 95-percent CI entirely in the negative IRR range.
Conclusions
We identified two qualifying studies that estimated the effects of licensing and permitting laws on mass shootings. Both studies found uncertain effects of permit-to-purchase laws on whether at least one mass shooting occurred in a state (Blau, Gorry, and Wade, 2016; Luca, Malhotra, and Poliquin, 2016). In one study that distinguished between permits for purchase and licenses to possess a firearm, the estimate was suggestive of a negative relationship between license requirements for firearm possession and the likelihood that an active shooter incident occurred in a state (Blau, Gorry, and Wade, 2016). Given the methodological limitations of both studies, we find inconclusive evidence for the effect of licensing and permitting requirements on mass shootings.

Effects on the Gun Industry

Research Synthesis Findings
We identified one study that met our inclusion criteria and examined how a policy index that captured the stringency of state laws regarding comprehensive background checks and permit-to-purchase requirements related to variation in the number of firearm manufacturing plants in a state and the number of background checks conducted there through the National Instant Criminal Background Check System (Brauer, Montolio, and Trujillo-Baute, 2017). However, the models had a ratio of estimated parameters to observations of approximately one to four, and the study provided no additional evidence to demonstrate appropriateness of model fit. Therefore, in accordance with our
review methodology, we discount the evidence provided by this analysis because of the possibility that the model was overfit, and thus the estimated effects and their CIs may be unreliable indicators of the true causal effects of the laws.

Conclusions
Because of the methodological concerns with the one qualifying study we identified that examined how permit-to-purchase requirements related to gun industry outcomes, we find inconclusive evidence of the effects of permit-to-purchase requirements on the gun industry.

Outcomes Without Studies Examining the Effects of Licensing and Permitting Requirements

We did not identify any research that met our inclusion criteria and examined the effects of licensing and permitting requirements on the following outcomes:

- unintentional injuries and deaths
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Nine References


The Brady Handgun Violence Prevention Act (the Brady Act), which went into effect in 1994, imposed a five-day waiting period for handguns purchased from licensed dealers in states without robust procedures for conducting background checks. However, this requirement lasted only until 1998, when the National Instant Criminal Background Check System (NICS) became available. Since then, all firearm purchases have required NICS background checks, which normally take no more than a few minutes to complete. However, in approximately 10 percent of background checks, the NICS check requires supplementary reviews (Criminal Justice Information Services Division, 2019), and federal law allows the Federal Bureau of Investigation (FBI) up to three days to complete these (18 U.S.C. 922). After three days, the dealer may, but is not required to, transfer possession of a firearm to its purchaser even without completion of the background check. By giving the FBI three days to complete the checks before allowing someone to take possession of a new firearm, the federal law can introduce delays comparable to a waiting period, although most buyers experience no such delay.

Waiting-period laws are intended to reduce suicide, violent crime, and mass shootings in several ways. First, waiting periods are primarily designed to disrupt impulsive acts of violence and self-harm, giving angry or distraught buyers time to “cool off” or gain perspective. While it is plausible that this cooling-off period could reduce impulsive interpersonal gun violence, some evidence exists for the potential effects of this mechanism in reducing suicides. Many suicidal acts are impulsive, with a short time between ideation (thinking about suicide) and attempt (Miller, Azrael, and Barber, 2012; Simon et al., 2002). Suicidal crises are often short-lived and characterized by ambivalence (Daigle, 2005; Glatt, 1987). Delaying access to firearms for individuals in these circumstances can reduce suicide attempts (RAND Corporation, 2018, Chapter Sixteen). Even if many distraught suicide attempters would seek alternative means of killing themselves, waiting periods may still reduce total rates of suicide because of the high case-fatality ratio of firearms compared with other methods (Anestis, 2016; Miller, Hemenway, and Azrael, 2004; Vyrostek, Annest, and Ryan, 2004; Spicer and Miller, 2000). Additionally, waiting periods may reduce suicide rates by affording at-risk individuals time to seek help or receive intervention.
Still, for some individuals, waiting periods may serve only to delay suicides rather than prevent them. Evidence from a cohort of handgun purchasers in California found that, while almost no firearm suicides were committed by this population during the state’s 15-day waiting period, the most elevated relative risk of firearm suicide (compared with the general population) occurred in the first week after receipt of the weapon and remained highly elevated over the first month of purchase (Wintemute et al., 1999). Moreover, most firearms are purchased by individuals who already own a firearm. Azrael et al. (2017) found that, on average, gun owners had close to five firearms each, and a large majority (62 percent) purchased their most recent weapon from a licensed gun dealer. For those who already own guns, a waiting period may have little or no effect on suicide risk.

Second, waiting periods may provide law enforcement with opportunities to investigate possible straw purchases (in which a lawful buyer makes the purchase on the behalf of a prohibited buyer) under the theory that it is less difficult to intercept a weapon prior to delivery. To assess whether waiting periods disrupt illegal firearm trafficking or transfers through this mechanism, causal inference could be strengthened by examining crime gun trace data in addition to changes in homicide or violent crime rates.1 Specifically, if these laws restrict straw purchasing from in-state retailers, one should observe a larger share of crime guns originating from out-of-state sources after law passage, as well as a reduction in guns with a short time-to-crime (Webster and Wintemute, 2015; Braga et al., 2012).2 However, a series of provisions attached to Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) appropriations (commonly known as the Tiahrt Amendments) has denied most researchers access to firearm trace data since 2003, making it currently infeasible to conduct this type of analysis (Krouse, 2009).

Third, waiting periods provide law enforcement agencies with additional time to complete background checks that sometimes cannot be completed within the three-day window provided by the federal law. In 2018, for instance, 3,960 firearms were confirmed to be transferred from federally licensed firearm dealers to prohibited persons as a result of delays in NICS background checks that exceeded three business days (an additional 280 cases might have involved the transfer of a firearm to a prohibited possessor, but definitive information on what happened to the firearm could not be collected by NICS or ATF) (Crime Information Services Division, 2019). Often, these delays result from missing data in the NICS databases that must

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1 The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined crime gun as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”

2 Per Webster and Wintemute (2015), the metric known as time-to-crime is the “unusually short interval—ranging from less than 1 year to less than 3 years—between a gun’s retail sale and its subsequent recovery by police from criminal suspects or crime scenes . . . . A short [time-to-crime] is considered an indicator of diversion, especially when the criminal possessor is someone different from the purchaser of record.”
be manually tracked down by investigators (e.g., if final disposition of a case is not noted). A U.S. Department of Justice review of 2013 and 2014 data found that an additional 1 percent of all background checks, or about 230,000, could not be completed within 88 days and were thus purged from the NICS review system by law without a determination about whether the buyer was a prohibited possessor (Office of the Inspector General, U.S. Department of Justice, 2016). When a buyer is determined to have been a prohibited possessor and has taken possession of a firearm, the NICS alerts ATF, which is successful in recovering the weapon in the vast majority of cases (e.g., 116 of the 125 examined by the Office of the Inspector General, U.S. Department of Justice [2016]).

Waiting periods provide additional time that can facilitate a more thorough check before buyers take possession of a new weapon, thereby increasing the effectiveness of background check laws in limiting firearm access by prohibited possessors who are considered to present elevated risk of violence. As discussed in Chapter Eight, the majority of prohibited possessors who perpetrate gun violence acquire their firearms from social acquaintances or the black market; thus, a large portion of violent gun crime is unlikely to be affected through this mechanism. In addition, it is unclear whether extending the time to complete background checks would reduce mass shootings. An analysis of the sources of firearms used in a sample of 19 mass shootings between 2009 and 2018 found one instance (5.3 percent) in which the shooter acquired a firearm used in the assault because the background check could not be completed in three business days (Buchanan et al., 2018). One additional instance involved an administrative error that resulted in a failure to trigger an automatic rejection and delayed completion of the background check within the requisite three-day period (Buchanan et al., 2018). However, the small sample of mass shooting cases explored in this analysis makes generalizations about the association of waiting periods and mass shooting incidents unwarranted.

Waiting-period laws may have the unintended consequence of delaying needed self-protection, although little or no empirical evidence exists to assess how often this may occur. The waiting periods may inconvenience some hunters or sport shooters who would otherwise benefit from more quickly obtaining a new firearm and, by extension, could reduce gun sales. Moreover, the laws may discourage some gun sales because they can require buyers to make two trips to the dealer, and if the store is far away, this could pose a serious inconvenience and delay the satisfaction of taking possession of the weapon.

Ideally, the effects of waiting periods would be studied among those populations most directly affected by the presumed mechanisms of their effect. In particular, it would be valuable to examine the effects of waiting periods on suicide and violence among those who do not already own a gun. However, this information is not available in the large data sets typically used to analyze the effects of gun policy, although there are some data on the time frame between purchase of a firearm and suicide risk.
(see, for example, Grassel et al., 2003; Wintemute et al., 1999; and Cummings et al., 1997b; for a review of the relationship between suicide and firearm availability, see RAND Corporation, 2018, Chapter Sixteen). Similarly, understanding the effect of waiting periods on the gun industry would be straightforward if sales data were available at state or local levels.

Analyses could also exploit the types of firearms for which waiting periods are required, as well as the duration of the waiting period. The importance of accounting for such policy heterogeneity will depend on the extent to which different types of firearms are substitutes and the marginal effect of requiring an additional day or days of delay before transfer can occur. State waiting-period laws applying to only a subset of firearms (e.g., handguns) should primarily affect outcomes involving those firearms, although one might expect to observe substitution toward other firearms excluded from waiting-period requirements. With respect to the waiting-period length, should the urge to commit suicide subside within one day, waiting periods of 48 hours or two weeks should generate similar effects, but if suicidal impulses persist for one week, different waiting-period lengths may generate heterogeneous effects (Lewiecki and Miller, 2013).

**State Implementation of Waiting Periods**

As of January 1, 2020, five states and the District of Columbia impose a waiting period to purchase any firearm. Four other states impose waiting periods for certain classes of firearms. The length of waiting periods varies by state. For example, California and the District of Columbia require a ten-day waiting period before buyers take possession of a new firearm. In Hawaii, buyers must wait 14 days to receive a permit to purchase a firearm. Other states impose shorter waiting periods.

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5 Calif. Penal Code §§ 26815, 27540, 27545 (the waiting period applies to dealers, but, in California, all sales must be processed through a dealer); D.C. Code Ann. §§ 22-4508.

6 Hawaii Rev. Stat. Ann. § 134-2. A separate permit is required for each handgun purchase, and the permit expires after ten days; long-gun permits are valid for one year.

7 For example, Florida’s waiting period is three days, and Illinois’s is three days for handguns and one day for long guns. See Fla. Stat. § 790.0655(1)(a); 720 Ill. Comp. Stat. 5/24-3(A)(g).
Effects on Suicide

Research Synthesis Findings

We identified four studies that met our inclusion criteria and assessed how waiting-period laws influenced suicide. Cook and Ludwig (2003) provides results similar to the authors’ earlier paper (Ludwig and Cook, 2000). Because the earlier paper included a larger data set spanning a wider time frame, we focus on its analyses, although the results reported in the two papers are comparable. Both papers examined changes in suicide rates before and after the implementation of the Brady Act in 1994, which initially imposed waiting periods and background checks for purchases from licensed firearm dealers. When the Brady Act was implemented, 18 states and the District of Columbia already had background checks, 27 states were required to implement background checks and waiting periods, and five states were required to implement only background checks (because they already had waiting periods or had an instant background check procedure that satisfied the Brady requirements). Ludwig and Cook (2000) sought to identify the effects of waiting periods by comparing reductions in suicide rates found in the states that did and did not implement waiting periods. They found that, when compared with the 18 unaffected states (plus the District of Columbia), the states implementing and those not implementing waiting periods saw uncertain reductions in suicide and firearm suicide rates. A subgroup analysis found a significant 9-percent reduction in firearm suicide rates among older victims in states that introduced waiting periods, whereas the reductions in states that did not have to introduce waiting periods were smaller and uncertain. The paper did not provide estimates that would demonstrate that the difference between these rate reductions was statistically significant. In addition, the analyses of states that were not required to implement waiting periods had a ratio of estimated parameters to observations of less than one to three, and the study provided no additional evidence to demonstrate model fit. Therefore, in accordance with our review methodology, we discount the evidence provided by this analysis because of the possibility the model was overfit, and thus the estimated effects and their confidence intervals (CIs) may be unreliable indicators of the true causal effects of the laws.

Two recent studies (Edwards et al., 2018; Luca, Malhotra, and Poliquin, 2017) built on the work of Ludwig and Cook (2000), exploiting subsequent state-level variation in waiting-period legislation that allowed the authors to estimate the effects of waiting periods specifically. Both studies estimated the relationship between waiting periods and state-level rates of suicide using similar empirical specifications—linear models that had a logged outcome; that were weighted by state population; and that controlled for state background check policies, state demographic and socioeconomic factors, and state and year fixed effects. Considering policies that establish purchase delays either explicitly or as part of permitting requirements, Edwards et al. (2018) found that the presence of any purchase delay was associated with significant negative
effects on firearm suicides and with suggestive negative effects on total suicides, and longer waiting periods (one week or more) showed some evidence of larger effects. Although the authors’ preferred model with quadratic state-specific trends had potential issues with overfitting (with a ratio of parameters to observations of about one to seven), estimates were similar in their more-parsimonious specifications. Using data over a longer time frame, from 1977 to 2014, Luca, Malhotra, and Poliquin (2017) similarly found significant negative effects on total and firearm suicide rates of laws imposing a mandatory delay on the purchase of a handgun or a permitting system for purchases of firearms that incorporated waiting periods. Although the precision of the total suicide estimate was reduced to uncertain in a sensitivity analysis that included controls for state-specific linear trends, the estimated effect remained significant and negative for firearm suicides in nearly all robustness checks, including in models that separated the effects of waiting-period and permitting laws.

Finally, one study compared national trends in total suicide rates with the trends of two jurisdictions that changed their waiting-period laws. Anestis and Anestis (2015) found that suicide rates in South Dakota increased by 8.9 percent in the four years following the repeal of the state’s waiting-period law, while national suicide rates increased by 8.2 percent over the same period. Suicide rates in the District of Columbia decreased by 1.5 percent in the two years following a law that extended the waiting period to begin at time of purchase rather than at the time of application, while national suicide rates increased by 2.7 percent over the same period. Given that the study did not test for significance or adjust for any other factors that may have led to differential suicide trends, this descriptive evidence provides limited causal insights, and we thus discount the findings of this study.

Figure 10.1 displays the incidence rate ratios (IRR) and CIs associated with the waiting-period policies examined in Luca, Malhotra, and Poliquin (2017) and Edwards et al. (2018). Because Anestis and Anestis (2015) did not provide inferential statistics or control for other factors that may have influenced suicide rates in the two jurisdictions with policy changes, we do not include effect sizes for this study in the figure.

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8 Of note, in the preferred models of Edwards et al. (2018), estimated effect sizes were similar for the effects of waiting periods on firearm and nonfirearm suicide rates (see Figure 10.1). This finding might indicate an issue with model specification, whereby some confounder correlated with waiting-period enactment produced declines in suicide more broadly. However, it is also consistent with potential spillover effects of waiting periods on suicides by nonfirearm means; that is, a decline in firearm suicide induced by the waiting-period law might have reduced suicide attempts by other lethal means.
Waiting Periods

Conclusions

We identified four qualifying studies that estimated the effects of waiting periods on suicides, and two of these provide credible evidence (Edwards et al., 2018; Luca, Malhotra, and Poliquin, 2017). Both studies found that waiting-period requirements were associated with significant negative effects on firearm suicide rates; effects on total suicide rates were negative and suggestive (Edwards et al., 2018) or significant (Luca, Malhotra, and Poliquin, 2017), depending on the study. Given that both studies (1) used highly similar model specifications and the same data set and (2) analyzed overlapping time periods (1990 to 2013, 1977 to 2014), we find limited evidence that waiting periods may reduce total suicides and moderate evidence that waiting periods may reduce firearm suicides.

Figure 10.1
Incidence Rate Ratios Associated with the Effect of Waiting Periods on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Handgun purchase delay</td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Total</td>
<td>0.98 [0.97, 1.00]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>0.98 [0.96, 1.00]</td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Firearm</td>
<td>0.98 [0.96, 1.00]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>0.93 [0.90, 0.96]</td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Nonfirearm</td>
<td>0.98 [0.95, 1.01]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>0.99 [0.93, 1.06]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.

Waiting periods may decrease total suicides. Evidence for this relationship is moderate.

Waiting periods may decrease firearm suicides. Evidence for this relationship is limited.
Effects on Violent Crime

Research Synthesis Findings
We identified six studies that met our inclusion criteria and evaluated the effects of waiting periods on violent crime. Ludwig and Cook (2000) sought to identify the effects of waiting periods by comparing reductions in homicide rates in states that had to implement waiting periods in 1994 (because of the Brady Act) with reductions in states that did not. The authors found that, compared with the 18 unaffected states (plus the District of Columbia), states implementing waiting periods saw nonsignificant drops in homicide and nonfirearm homicide rates. In contrast, the five states that were not required to implement waiting periods saw nonsignificant increases in homicide and firearm homicide rates. However, the paper did not report whether these effects differed by a statistically significant amount. In addition, the analyses of states that were not required to implement waiting periods had a ratio of estimated parameters to observations of less than one to five, and the paper provided no additional evidence to demonstrate model fit. Therefore, in accordance with our review methodology, we discount the evidence provided by this analysis because of the possibility the model was overfit, and thus the estimated effects and their CIs may be unreliable indicators of the true causal effects of the laws.

We identified five studies that specifically examined the effect of waiting periods on violent crime. Mustard (2001) evaluated how waiting-period and shall-issue laws (see Chapter Eighteen) affect felonious deaths of law enforcement officers. The author estimated a variety of regression specifications (e.g., Poisson, logit, Tobit, weighted by state population, and unweighted) with different outcome variables (e.g., any felonious police officer death, police officer deaths per capita, police officer deaths per full-time equivalent officer). In the author’s preferred specifications, waiting-period laws were specified by a spline variable that allowed for differential before and after trends in states that enact waiting-period policies. In these preferred specifications, models generally showed that states that enacted waiting periods typically had lower rates of law enforcement officer deaths both before and after law implementation but that the break in trend concurrent with the law was not significant. Given that the study covered a relatively short time frame (1984 to 1996) and regression models included year fixed effects, state fixed effects, and numerous state-level covariates, the analyses had a ratio of estimated parameters to observations of about one to seven, indicating that the model may have been overfit. Additionally, no adjustments were made to account for correlation within states over time, and thus the estimated effects and their CIs may be unreliable indicators of the true causal effects of the laws.

Roberts (2009) separately analyzed the effects of waiting-period length (none, 24 hours, between two and seven days, and more than seven days) and shall-issue laws on intimate partner homicides (using county-level data from 1985 to 2004). The author found that a waiting period of between two and seven days significantly low-
Waiting Periods

tered total and firearm-specific intimate partner homicide rates compared with no waiting period, but longer (more than seven days) or shorter (24-hour) waiting periods (compared with no waiting period) had only suggestive effects on reducing total intimate partner homicides. The author also reported that a waiting period longer than seven days significantly increased firearm-related intimate partner homicides (compared with no waiting period). However, these analyses did not cluster standard errors at the state level, so serial correlation that was unaccounted for in the panel data could have resulted in biased standard errors and CIs.

Three other studies assessed the associations of waiting-period laws with homicide rates. Hepburn et al. (2004) used data from 1979 to 1998 and estimated negative binomial models, adjusted for state and year fixed effects, concealed-carry and background check laws, firearm sentencing enhancements, the ratio of firearm to total suicides as a proxy for state-level firearm prevalence, and other state-level factors. The authors found that waiting periods were associated with a suggestive negative effect on homicides. Using data over a longer time frame (1990 to 2013), linear models with a logged outcome, and controls for state-specific linear trends, Edwards et al. (2018) instead found uncertain effects of waiting periods on total homicide rates but suggestive effects consistent with the waiting periods decreasing firearm homicides. Although the preferred model in Edwards et al. (2018) had an unfavorable ratio of parameters to observations (less than one to ten), another study adopting a very similar specification but covering a longer time frame (1977 to 2014) found that waiting periods resulted in large and significant reductions in total and firearm homicide rates among adults aged 21 or older (Luca, Malhotra, and Poliquin, 2017) (see Figure 10.2).

Figure 10.2 displays the IRRs and CIs associated with the waiting-period policies examined in these studies, except for Ludwig and Cook (2000) (because of the reasons stated earlier) and Mustard (2001) (because the author’s preferred specification was a spline model, so we cannot uniquely calculate an effect size comparable to those in Figure 10.2).
Figure 10.2
Incidence Rate Ratios Associated with the Effect of Waiting Periods on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Handgun purchase delay</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Total</td>
<td>0.98 [0.91, 1.05]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>0.88 [0.79, 0.97]</td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Firearm</td>
<td>0.95 [0.89, 1.01]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>0.83 [0.72, 0.95]</td>
</tr>
<tr>
<td>Edwards et al. (2018)</td>
<td>Nonfirearm</td>
<td>1.01 [0.94, 1.09]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>0.97 [0.90, 1.04]</td>
</tr>
<tr>
<td><strong>Waiting period law</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hepburn et al. (2004)</td>
<td>Total</td>
<td>0.94 [0.86, 1.27]</td>
</tr>
<tr>
<td><strong>24-hour waiting period</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Total, intimate partner</td>
<td>0.56 [0.30, 1.05]</td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Firearm, intimate partner</td>
<td>0.58 [0.32, 1.05]</td>
</tr>
<tr>
<td><strong>2–7 day waiting period</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Total, intimate partner</td>
<td>0.47 [0.38, 0.59]</td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Firearm, intimate partner</td>
<td>0.72 [0.58, 0.88]</td>
</tr>
<tr>
<td><strong>&gt;1 week waiting period</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Total, intimate partner</td>
<td>0.74 [0.51, 1.08]</td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Firearm, intimate partner</td>
<td>1.56 [1.09, 2.22]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.

Conclusions

*Total homicides.* We identified six qualifying studies that examined the effects of waiting periods on homicide rates, but we discount the evidence provided by one (Ludwig and Cook, 2000). One study found uncertain effects of the laws (Edwards et al., 2018), one found that waiting periods had suggestive negative effects on total homicides (Hepburn et al., 2004), and one found that waiting periods significantly reduced total homicides among adults aged 21 years or older (Luca, Malhotra, and Poliquin,
Examining specific types of homicide, Mustard (2001) found that waiting periods had uncertain or suggestive negative effects on homicides of police officers. And Roberts (2009) found that waiting periods were associated with declines in intimate partner homicide, although these effects were significant only for waiting periods of between two and seven days; estimates were suggestive for both shorter and longer waiting periods. Considering these studies and our assessment of their strengths, we find moderate evidence that waiting periods may reduce total homicides.

Firearm homicides. We identified five qualifying studies that examined the effects of waiting periods on firearm homicide rates, but we discount the evidence provided by one (Ludwig and Cook, 2000). One study found that waiting periods had suggestive negative effects on firearm homicide rates (Edwards et al., 2018), and one found that waiting periods significantly reduced firearm homicides among adults aged 21 or older (Luca, Malhotra, and Poliquin, 2017). Examining specific types of homicide, Mustard (2001) found that waiting periods had uncertain effects on firearm homicides of police officers. One study that focused on firearm-related intimate partner homicide found mixed results depending on the length of the waiting period (Roberts, 2009): A waiting period of two to seven days significantly reduced firearm-related intimate partner homicide, 24-hour waiting periods produced suggestive declines in firearm-involved intimate partner homicides, and waiting periods of more than seven days significantly increased firearm-involved intimate partner homicides. Considering these studies and our assessment of their strengths, we find limited evidence that waiting periods may reduce firearm homicides.
Effects on Mass Shootings

Research Synthesis Findings

Our search yielded two studies that met our inclusion criteria and examined the effects of waiting periods on mass shootings in the United States.

Lott (2003) used a Poisson regression model to estimate the effect of waiting periods on fatalities, injuries, and the incidence of *multiple-victim public shootings*, which the author defined as events unrelated to other criminal activity in which two or more people were killed or wounded in a public location. The analysis covered 1977 to 1997, and regression models included controls for state and year fixed effects, other state firearm policies, and a broad range of state-level socioeconomic and demographic characteristics. The author characterized waiting-period laws using three variables: a dummy variable for whether state laws required a waiting period before delivery of a firearm, the length of the waiting period in days, and the length of the waiting period in days squared. For all three policy variables, findings showed effects that were small and not statistically significant for total casualties from multiple-victim public shootings and for total multiple-victim public shooting incidents. However, these models had an unfavorable ratio of estimated parameters to observations (approximately one to eight), suggesting that the model may have been overfit, and thus the estimated effects of these laws may be poor indicators of their true effects. In addition, the model did not adjust for clustered standard errors. Together, these shortcomings suggest that the model results may not accurately describe the true effects of waiting periods.

Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2016) estimated the effects of waiting periods on a binary indicator for whether a mass shooting occurred in a given state-year. They defined a *mass shooting* incident as one in which four or more individuals were killed (excluding the shooter), the occurrence was not connected to criminal activity, and at least three fatally injured victims were not related to the shooter. The authors included two measures of waiting periods: the number of days that purchasers must wait before accepting delivery of a handgun and the number of days before accepting delivery of a long gun. The authors’ regression analysis covered 1989–2014 and included controls for time-invariant state characteristics, national trends, and a host of other state-level gun policies, as well as time-varying state-level demographic, socioeconomic, and political characteristics. Their findings showed uncertain effects that were small in magnitude of both waiting-period measures on the probability of a mass shooting event. However, assessing the effects of gun policies on mass shootings was not the primary focus of Luca, Malhotra, and Poliquin (2016), and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used
a linear model to predict a rare dichotomous outcome. Therefore, model assumptions were likely violated, making CIs unreliable.

Figure 10.3 displays the IRRs and CIs associated with the waiting-period policies examined in Luca, Malhotra, and Poliquin (2016). Because the model in Lott (2003) estimated three effects for waiting-period policies that cannot be readily combined to indicate an overall or average effect, we do not include those estimates in Figure 10.3.

Figure 10.3
Incidence Rate Ratios Associated with the Effect of Waiting Periods on Mass Shootings

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Handgun waiting period (in days)</td>
<td>Mass shooting</td>
<td>1.03 [0.97, 1.1]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2016)</td>
<td>Any incident</td>
<td>0.93 [0.62, 1.24]</td>
</tr>
<tr>
<td>Long gun waiting period (in days)</td>
<td>Any incident</td>
<td></td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2016)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

Conclusions
We identified two qualifying studies examining the effect of waiting periods on mass shooting outcomes. Luca, Malhotra, and Poliquin (2016) found the length of waiting periods required for handguns and for long guns to have uncertain effects on the likelihood that at least one mass shooting occurred in a state. Lott (2003) found a suggestive effect consistent with the passage of any waiting-period law increasing the incidence of mass shootings. However, estimates in the same model also showed a suggestive effect of waiting-period length on decreasing the incidence of mass shootings, which complicates interpretation of the overall effect of the law. Further, Lott (2003) found uncertain effects of both waiting-period measures on the number of casualties from mass shooting events. Considering these studies, we find inconclusive evidence for the effect of waiting periods on mass shootings.
Effects on the Gun Industry

**Research Synthesis Findings**
Two studies met our inclusion criteria and assessed how waiting periods influence the gun industry or gun ownership. Brauer, Montolio, and Trujillo-Baute (2017) estimated a linear regression model with state and year fixed effects to understand the effects of various firearm policies, including laws imposing a waiting period for firearm purchases, on state-level counts of firearm manufacturing plants based on data from 1997 to 2010. Consistent with the authors’ hypotheses, waiting periods showed uncertain effects on firearm manufacturing locations. In supplemental analyses, the authors estimated the relationship between waiting periods and the number of NICS background checks as a proxy measure for firearm demand, using a method published previously by the authors that adjusts the NICS checks to more accurately reflect new firearms purchased; estimated effects of waiting periods on this outcome were also uncertain. However, the ratio of parameters to observations in these models was less than one to ten, which may indicate model overfit, and a linear model does not appear to correspond to the distributional features of the authors’ outcome variable, which limits the inference that can be drawn from this study.

Glaeser and Glendon (1998) used household survey data from 1973 to 1994 to better understand the determinants of gun ownership. Using linear models that adjusted for a range of person-level characteristics and state fixed effects, the authors estimated the effect of waiting periods on the probability that a respondent reported owning a gun or a handgun, assuming differential effects of the policy based on the arrest history of the respondent. They found uncertain effects of waiting periods on firearm ownership overall but suggestive effects consistent with waiting periods reducing gun ownership among individuals with prior arrests for non-traffic-related offenses relative to the overall population.

Figure 10.4 displays the IRRs and CIs associated with the waiting-period policies examined in Brauer, Montolio, and Trujillo-Baute (2017). Because Glaeser and Glendon (1998) separately estimated the effects of waiting periods for those with and without a prior arrest history, we do not include effect sizes for this study in the figure.
Conclusions

One study evaluated the effect of waiting periods on firearm manufacturing and found uncertain effects (Brauer, Montolio, and Trujillo-Baute, 2017). That study also found uncertain effects of waiting periods on a proxy measure for firearm demand (NICS background check rates), consistent with findings of another study that indicated that waiting periods had an uncertain relationship with overall household firearm ownership (Glaeser and Glendon, 1998). Considering these studies, we find inconclusive evidence for the effect of waiting periods on the gun industry.

Outcomes Without Studies Examining the Effects of Waiting Periods

We did not identify any research that met our inclusion criteria and examined the effects of waiting periods on the following outcomes:

- unintentional injuries and deaths
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Ten References


United States Code, Title 18, Section 922, Unlawful Acts.


According to results from a 2015 survey, approximately 61 percent of firearm owners in the United States have received formal training on firearm safety and use (Rowhani-Rahbar et al., 2018). Others receive informal training from their friends or family. Although there are no federal laws requiring private citizens to receive safety training, states sometimes require gun purchasers or those requesting concealed-carry permits to show proof of formal safety training on how to safely store, use, and maintain weapons. Advocates of such policies suggest that the regulations ensure a minimum competency for using guns safely, just as drivers’ tests are used to determine whether a person can safely drive a car before being permitted to operate one. However, detractors of the laws suggest that such regulations create unwarranted costs and barriers to firearm ownership and that such ownership should not be made conditional on training (Cole, 2014).

Firearm safety training courses may cover firearm operation and safe handling, the physics of firearms, how to clean and repair firearms, firearm laws and regulations, and best practices for keeping firearms away from children or other vulnerable individuals. Some courses include a live-fire demonstration to prove that the applicant can use a firearm safely. However, the components of safety training vary greatly. One study audited 20 basic handgun safety classes in three states that had requirements for safety training and four that did not (Hemenway et al., 2019b). Most trainers covered key safety issues, such as safely loading and unloading a gun, keeping one’s finger off the trigger until being ready to shoot, and being cognizant of the target and what is behind it. In 50 to 75 percent of the classes, trainers covered operating a safety lock and clearing jams and cartridge malfunctions, and they recommended storing guns unloaded and locked when the weapons were not in use (Hemenway et al., 2019b). However, much lower percentages of instructors discussed other safety issues, such as the role of firearms in suicide (10 percent) and domestic violence (10 percent) or the role of stolen firearms in gun crimes (20 percent).

The impact of safety training on key outcomes depends on the content of the programs, the effectiveness of the programs in conveying pertinent information, and the number of gun owners who then modify their behavior based on the information presented in the training. For example, if safety training increased safe firearm storage
practices, we might expect firearm suicides and accidental firearm injuries and deaths to decrease, although such storage practices might interfere with defensive gun use (see Chapter Seventeen, on child-access prevention laws). And the motivations of the individuals who receive firearm training could affect the overall impact of the training programs. For example, some states require individuals to attend safety training before they may obtain a permit to carry a firearm in public places, presumably for self-defense. Such requirements might mean that trainings are attended mostly by gun owners who could be less amenable to storing a firearm safely, because safe storage could theoretically impede quick access to a weapon for use in self-defense.

However, limited research investigates the relationship between the receipt of safety training and weapon safety behaviors. Results from one 1995 survey showed that gun owners who received formal firearm training (in which 80 percent of training courses covered proper gun storage) were significantly more likely to store their firearms loaded and unlocked compared with gun owners who had not received formal training; however, the most common source of training for this sample was through the military, which may not produce the same effects as the training required by states for civilian gun owners (Hemenway, Solnick, and Azrael, 1995). Nevertheless, these findings were supported in a 2015 survey of gun owners that showed similar rates of formal firearm training participation (60 percent of respondents) and similar rates of safe storage (32 percent storing all guns unloaded and locked and 46 percent storing at least one gun unloaded and unlocked or loaded and locked); in addition, the survey showed that receipt of safety training was negatively associated with safe storage (Berrigan et al., 2019). In a 2001–2003 survey of gun safety practices among 2,939 older adults (aged 55 or older) who reported a gun in their home, 20 percent reported storing the gun unlocked and loaded, and 55 percent reported attending a firearm safety training (Lum, Flaten, and Betz, 2016). The authors found no correlation between safe storage and having an adult in the house who attended gun safety training. Together, these results suggest that firearm safety training may not necessarily increase the prevalence of safe firearm storage practices.

This evidence of the relationship between self-reported training participation and firearm storage behaviors contrasts with results from studies of gun owners’ beliefs about how firearm safety training influences their behaviors and practices (Crifasi et al., 2018a). A 2016 survey of a national sample of gun owners found that 35 percent of respondents believed that their storage practices were influenced by a gun safety training course; the only factor endorsed more highly was concern about home defense (chosen by 43 percent of respondents). The respondents who reported that gun safety training influenced their storage behaviors were significantly more likely to report safe storage behaviors, although this does not provide good evidence that the trainings cause more safe storage. Overall, it is likely that the effect of a gun safety training course on firearm practices will vary by the components of the training course, the method of training delivery, the reasons an individual owns a gun, and other contex-
tual factors in the home. For instance, this same study found that most gun owners perceived law enforcement, hunting or outdoor organizations, the National Rifle Association, and the military as being more-credible messengers of gun safety training than were gun show managers, physicians, and celebrities, who were rated as credible messengers by fewer than half of respondents. Thus, credible messengers who promote safe storage practices might be more likely to change the behavior of gun owners than are noncredible messengers who promote safe storage.

More research is needed to understand the relationship between safety training and changes in firearm owners’ safety behavior, including safe handling, law compliance, and safe storage. Further research is also needed to determine whether those who take firearm safety training courses are better able to use their weapons for self-defense or whether courses do not provide enough training to sufficiently prepare owners for a defensive situation. Without such research, it is difficult to determine the impacts of firearm safety practices on other outcomes of interest, such as firearm deaths, injuries, and violent crime. Furthermore, the impacts of training on hunting and recreation and on the gun industry also remain unknown.

State Implementation of Firearm Safety Training Requirements

As of January 1, 2020, six states and the District of Columbia have laws requiring individuals to undergo some sort of safety training prior to being able to purchase, or in the case of Connecticut, carry, a firearm. California and Massachusetts have laws requiring such training for the purchase of both handguns and long guns.1 The District of Columbia’s law, which applies to handguns and long guns, goes further: It requires safety training prior to registration, and registration is required for possession of a firearm; thus, the training requirement applies to not only people purchasing new firearms but also people moving into the District who already own firearms.2 The laws in Connecticut, Hawaii, Maryland, and Rhode Island apply only to handguns.3 Washington’s law applies to semiautomatic rifles.4 Elements of safety training laws may include specifics about instructor qualifications or training,5 information about which

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1 Calif. Penal Code § 31615(a); Mass. Gen. Laws Ann. 140 § 131P.
2 D.C. Stat. §§ 7-2502.03(a), 7-2502.06(a).
5 For example, under Massachusetts law, “Firearms safety instructors shall be any person certified by a nationally recognized organization that fosters safety in firearms, or any other person in the discretion of said colonel, to be competent to give instruction in a basic firearms safety course” (Mass. Gen. Laws Ann. 140 § 131P(b)).
organizations may offer approved courses,\textsuperscript{6} curriculum requirements,\textsuperscript{7} and a requirement that trainees pass a written test.\textsuperscript{8}

In addition, 26 states and the District of Columbia require that applicants for concealed-carry permits demonstrate that they have received some sort of firearm training, either in a formal course or through some other setting, such as through military service. This includes jurisdictions with “may-issue”\textsuperscript{9} or “shall-issue” concealed-carry laws.\textsuperscript{10} See Chapter Eighteen for further information on variation in state implementation of concealed-carry laws.

\begin{itemize}
\item \textsuperscript{6} For example, Connecticut law provides that courses may be approved by the Commissioner of Emergency Services and Public Protection in the safety and use of pistols and revolvers including, but not limited to, a safety or training course in the use of pistols and revolvers available to the public offered by a law enforcement agency, a private or public educational institution or a firearms training school, utilizing instructors certified by the National Rifle Association or the Department of Energy and Environmental Protection and a safety or training course in the use of pistols or revolvers conducted by an instructor certified by the state or the National Rifle Association. (Conn. Gen. Stat. Ann. § 29-28(b); see also Hawaii Rev. Stat. § 134-2(g)).
\item \textsuperscript{7} For example, the Hawaii law states that, prior to being issued a permit to purchase a pistol or revolver, a person must complete A firearms training or safety course or class conducted by a state certified or National Rifle Association certified firearms instructor or a certified military firearms instructor that provides, at a minimum, a total of at least two hours of firing training at a firing range and a total of at least four hours of classroom instruction, which may include a video, that focuses on: (A) The safe use, handling, and storage of firearms and firearm safety in the home; and (B) Education on the firearm laws of the State. (Hawaii Rev. Stat. § 134-2(g); see also Md. Code, Pub. Safety, § 5-117.1(d)).
\item \textsuperscript{8} See, for example, Calif. Penal Code § 31640.
Effects on Violent Crime

Research Synthesis Findings
We identified one study that met our inclusion criteria and examined how the number of training hours required to obtain a concealed-carry permit affected murder rates. In estimates focused on understanding the impact of concealed-carry laws on violent crime, Lott (2010) controlled for differing concealed-carry permit requirements across states, including the number of hours of training required to get the permit. His regression models operationalized training-hour requirements in three parts: a continuous variable for the number of training hours required, a continuous variable for the squared number of training hours required, and a dummy variable for whether the state training requirements exceeded eight hours of instruction. He found a suggestive effect consistent with the number of training hours (specified as linear) reducing murder rates (incidence rate ratio [IRR] = 0.98; 95-percent confidence interval [CI] = 0.95, 1.00), but he found uncertain effects on the other training variables. However, this model did not appear to make any adjustments to account for autocorrelation within states, and the model had an unfavorable ratio of estimated parameters to observations (less than one to ten), meaning the model may have been overfit and its associated effect estimates and significance levels may be inaccurate. Because the models used by Lott (2010) specified training requirements using three separate variables that cannot be combined to generate an overall or average effect, we do not present these estimates in a figure.

Two other studies (Crifasi, Pollack, and Webster, 2016; Rudolph et al., 2015) estimated how violent crime was affected by Connecticut’s implementation of a permit-to-purchase law, which included the imposition of an eight-hour firearm safety training requirement. However, because the state law included a much broader set of regulations regarding permitting, we consider these studies as providing evidence for permit-to-purchase implementation (see Chapter Nine) rather than for the safety training component of the law.

Conclusions
One study examined how the number of training hours required to obtain a concealed-carry permit affected murder rates and found suggestive evidence that requiring a longer duration of training may reduce murder rates as one part of a multicomponent effect of these laws. Considering the methodological concerns with this single study, we find inconclusive evidence for the effect of firearm safety training requirements on total homicides.
Effects on the Gun Industry

Research Synthesis Findings
We identified one study that examined the effect of 2010 legislation in Arizona (State Bill 1108) that removed the permitting requirement to carry a concealed weapon, thus removing the previously mandated eight-hour training requirement (Ginwalla et al., 2014). The authors found that Arizona’s policy change was associated with a significant increase in the number of background checks run through the National Instant Criminal Background Check System in Arizona relative to the United States as a whole. However, this result could have been due to a variety of confounders that were not controlled for in the analyses, such as relatively higher rates of population growth in Arizona, demographic shifts, differential impacts of the Great Recession, or gun laws passed in other states. Furthermore, because the removal of firearm safety training requirements occurred as part of a broader policy that removed permitting requirements for concealed carry, this study could not isolate the effects of firearm safety training requirements. Thus, we do not present these estimates in a figure.

Conclusions
The one study that provided evidence for the effect of safety training requirements on the number of background checks conducted (a proxy measure of firearm purchases) identified effects using a law in a single state that changed such requirements as part of broader legislation that removed requirements for carrying a concealed weapon (Ginwalla et al., 2014). Given the confounding in this single study of a single state, we find inconclusive evidence for the effect of firearm safety training requirements on firearm purchases.
Outcomes Without Studies Examining the Effects of Firearm Safety Training Requirements

We did not identify any studies that met our inclusion criteria and examined the relationship between firearm safety training requirements and the following outcomes:

- suicide
- unintentional injuries and deaths
- mass shootings\(^{11}\)
- officer-involved shootings
- defensive gun use
- hunting and recreation.

\(^{11}\) Lott (2003) estimated Poisson regressions for the effect of the number of training hours required to obtain a concealed-carry permit on multiple-victim public shootings, but the results presented (Lott, 2003, Table 6.10) do not provide standard errors, test statistics, or exact $p$-values, so we do not interpret this evidence for this review.
Chapter Eleven References


Federal law requires licensed firearm dealers to report lost or stolen guns to local authorities or the U.S. Attorney General within 48 hours (18 U.S.C. 923). There is no federal law requiring individuals to report lost or stolen firearms.

In 2018, federally licensed firearm dealers reported 14,738 firearms (including pistols, rifles, shotguns, revolvers, and machine guns) as lost or stolen from their places of business (Bureau of Alcohol, Tobacco, Firearms and Explosives [ATF], 2019). Quantifying the number of firearms lost or stolen from private citizens is more challenging, but based on data from ATF, 173,675 firearms were reported lost or stolen from non–federal firearms licensee entities and private citizens in 2012 (ATF, 2013). Using an alternative data source, another study estimated that about 233,000 guns were stolen annually during household property crimes between 2005 and 2010, and about four out of five firearms stolen were not recovered (Langton, 2012). Data from police departments in 14 U.S. cities suggest that the number of guns reported lost or stolen in 2014 varied from 17 in San Francisco to 364 in Las Vegas (Everytown for Gun Safety Support Fund, Mayors Against Illegal Guns, and the National Urban League, 2016). Estimates from a 2015 national survey indicated that 2.4 percent of U.S. gun owners had at least one gun stolen in the past five years and that the average number of guns stolen per person was 1.5 (Hemenway, Azrael, and Miller, 2017). The authors of the study used these data to estimate that 380,000 guns were stolen per year.

Laws requiring gun owners to report lost or stolen firearms are intended to help prevent gun trafficking and straw purchases (in which a lawful buyer makes the purchase on behalf of a prohibited buyer) and to help ensure that prohibited possessors do not obtain access to firearms. Data collected from ATF trafficking investigations covering 1999 to 2002 showed that 6.6 percent (7,758 of 117,138) of diverted firearms were stolen from a residence or vehicle (Braga et al., 2012).

There are several plausible mechanisms through which these policies might reduce criminal use or trafficking of firearms. First, reporting requirements might encourage private gun owners to take steps that decrease the ease with which their firearms might be lost or stolen (e.g., by storing their firearms in a locked container). Second, reporting requirements could deter some straw purchasers who are reluctant to report
as stolen the guns they have diverted to prohibited possessors but who also fear that failure to report transferred guns as stolen could leave them accountable for explaining how their guns later turned up at crime scenes. Third, timelier reporting of gun losses or thefts may aid law enforcement gun-tracing efforts and increase criminal prosecutions of illegal users or traffickers of stolen firearms, potentially reducing the stock of firearms available to prohibited possessors or to individuals seeking to obtain firearms for criminal purposes. Thus, to estimate how requirements for reporting lost or stolen firearms affect such outcomes as violent crime, we might first examine to what extent such policies affect gun owners’ reporting and storage behavior.

To assess whether required reporting of lost or stolen guns reduces violent crime by disrupting illegal firearm trafficking, causal inference could be strengthened by examining crime gun trace data,¹ as well as changes in homicide or violent crime rates. Specifically, if these laws restrict trafficking operations from in-state sources, one should observe a larger share of crime guns originating from out-of-state sources after law passage (Webster and Wintemute, 2015; Braga et al., 2012). However, a series of provisions attached to ATF appropriations (commonly known as the Tiahrt Amendments) has denied most researchers access to firearm trace data since 2003, making it currently infeasible to conduct this type of analysis at a national level (Krouse, 2009).

Requiring gun owners to report lost or stolen firearms is unlikely to have measurable effects on such outcomes as suicide, unintentional injuries and deaths, defensive gun use, or hunting and recreation. If the requirements successfully discouraged straw purchases, it could have a small effect on firearm sales.

State Implementation of Lost or Stolen Firearm Reporting Requirements

As of January 1, 2020, 12 states and the District of Columbia require firearm owners to report to law enforcement when their weapons are lost or stolen. Most jurisdictions with these laws require individuals to report the loss or theft of all firearms, although state laws vary in the length of time allowed to elapse between the discovery and the report.² However, Maryland requires the reporting of loss or theft of handguns and

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¹ The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined crime gun as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”

assault weapons only,\textsuperscript{3} and Michigan requires the reporting of thefts, but not loss, of all firearms.\textsuperscript{4}

Outcomes Without Studies Examining the Effects of Lost or Stolen Firearm Reporting Requirements

We did not identify any research that met our inclusion criteria and examined the relationship between required reporting of lost or stolen firearms and the following outcomes:

- suicide
- violent crime
- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.

\textsuperscript{3} Md. Ann. Code § 5-146 (within 72 hours).
\textsuperscript{4} Mich. Comp. Laws § 28.430 (within five days).
Chapter Twelve References

ATF—See Bureau of Alcohol, Tobacco, Firearms and Explosives.


United States Code, Title 18, Section 923, Licensing.

CHAPTER THIRTEEN

Firearm Sales Reporting, Recording, and Registration Requirements

Under federal law, licensed dealers must maintain records of firearm sales indefinitely (18 U.S.C. 923). Although licensed dealers must respond to specific Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) inquiries about sales of individual guns, federal law does not mandate that dealers report sales to any federal authority. Similarly, there is no federal law requiring recording or reporting of firearm sales by private sellers (Giffords Law Center to Prevent Gun Violence, undated-e).

There is also no federal _firearm registration system_, which we define as a record-keeping system controlled by a government agency that both stores the names of current owners of each firearm of a specific class and requires that these records are updated after firearms are transferred to a new owner.\(^1\) Indeed, federal authorities are explicitly prohibited by law from establishing a firearm registry or from using data collected through the National Instant Criminal Background Check System (NICS) for developing a registry of gun ownership (18 U.S.C. 926(a)(3) and 28 C.F.R. 25.9 (b)(3)), although a federal registry is permitted and does exist for a small class of firearms specified in the 1934 National Firearms Act, such as machine guns and short barreled rifles and shotguns (26 U.S.C. 5841).

Federal law does not prohibit states from imposing reporting or registration laws, and several have implemented such laws. As with policies requiring the reporting of lost or stolen firearms, policies requiring the recording and reporting of gun sales or the establishment of firearm registries are designed to facilitate law enforcement traces of weapons used in crimes. Without such laws, tracing crime guns typically identifies where a gun was first legally sold, and to whom.\(^2\) However, secondary markets appear to be the leading source of guns used in crimes (Harlow, 2001). By requiring a record

\(^1\) There is no universally accepted definition of a firearm registration system. Some definitions differ from ours by emphasizing that such a system requires owners to periodically renew their registrations. Alternatively, some definitions emphasize that, rather than requiring _sellers_ to record when guns are transferred to a new owner (as in California), a firearm registration system requires _gun owners_ to proactively obtain registrations.

\(^2\) The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined _crime gun_ as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”
of each subsequent transfer or sale of a firearm after its initial sale by a licensed dealer, ATF and other law enforcement agencies would gain valuable investigative information. Presumably, gun registries or the required recordkeeping and reporting of private gun sales could also deter illegal sales or transfers of weapons to prohibited possessors and could help agencies enforce lost or stolen firearm reporting requirements (see Chapter Twelve).

Law enforcement access to sales data or firearm registration information could also facilitate the identification of firearm owners who have become prohibited possessors. For instance, California, which has a firearm registration system, passed Proposition 63 in 2016. The law, among other things, requires courts to search California’s centralized records of firearm sales and transfers whenever an individual is convicted of an offense or otherwise becomes a prohibited possessor. When such individuals are found to have purchased firearms, they are then required to relinquish or dispose of those firearms.3 Under Hawaii law, all firearms must be registered at the police department of the county of registration. Prior to registration, officials are required to check available records to determine whether the person is authorized to own a firearm.4

Required recordkeeping, reporting, or registration may impose costs to sellers of maintaining compliance, and concerns about privacy may deter some individuals seeking to acquire a firearm for self-protection or recreational gun use, with consequences for gun sales. In addition, some who oppose gun registries believe that they eventually will be used to facilitate the confiscation of banned (or, in some cases, all) firearms by the government (National Rifle Association, Institute for Legislative Action, 2019b). In this view, a gun registry is the first step toward the evisceration of gun owners’ Second Amendment rights and the liberty that the Constitution guarantees.

Because the principal intended benefit of recording, reporting, and registration laws concerns crime investigation, the data most relevant to understanding the effects of such laws would include firearm crime clearance rates, or the rates at which law enforcement is successful in identifying suspects in firearm-related crimes, including violent and property crimes, and firearm trafficking crimes. In California and other states that use these records to identify prohibited possessors with weapons, data on firearm-involved crime and violence perpetrated by prohibited possessors would be valuable, but such data are not generally available.

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3 Calif. Penal Code § 29810.
State Implementation of Firearm Sales Reporting, Recording, and Registration Requirements

Although federal law requires that federal firearms licensees maintain records of all sales, states can pass mirroring laws to ensure that state authorities can independently enforce the statute. As of January 1, 2020, 18 states and the District of Columbia require firearm sellers to keep records of at least some firearm sales. Eleven states and the District of Columbia require licensed dealers to maintain records of all firearm sales by dealers.\(^5\) Connecticut, Illinois, and Rhode Island also require private sellers to maintain records of sales.\(^6\) In California, Colorado, Delaware, New York, and the District of Columbia, all private sales are completed through dealers, who maintain records of all sales.\(^7\) Furthermore, seven states require licensed dealers to maintain records of handgun sales,\(^8\) and three states require private sellers to do so.\(^9\)

Some states have abolished their recordkeeping requirements. In 2015, for example, Alabama repealed the section of law requiring dealers to maintain detailed handgun sales records. In fact, the state enacted a law stating that, within 180 days of the new law’s passage, dealers and law enforcement must destroy any records they created to comply with the repealed law,\(^10\) although gun sellers’ federal recordkeeping requirements would remain.

In addition to laws requiring sellers to maintain their own records, ten states and the District of Columbia require that those sales records be transmitted to a law enforce-
ment agency. Four of these states require records of all sales to be transmitted, including those by licensed dealers and private sellers.\textsuperscript{11} Similarly, the District of Columbia’s registration requirement gives law enforcement access to all sales records.\textsuperscript{12} Five states require dealers and private sellers to report only handgun sales to law enforcement.\textsuperscript{13} Washington requires dealers and private sellers to report handgun and semiautomatic rifle sales to law enforcement.\textsuperscript{14}

Five jurisdictions have firearm registration schemes. California, Hawaii, and the District of Columbia require registration of all firearms, although, in California, some guns purchased before the relevant laws went into effect are not required to be registered until they are legally sold or transferred.\textsuperscript{15} New York and Maryland require registration of handguns only.\textsuperscript{16} Several states have specialized registration schemes, such as for .50-caliber rifles or for specific banned weapons that were purchased before the prohibitions went into effect (see Chapter Fourteen, on bans on the sale of assault weapons and high-capacity magazines).\textsuperscript{17}

**Effects on Mass Shootings**

**Research Synthesis Findings**

We identified one study that met our inclusion criteria and examined the effect on mass shootings of laws requiring gun owners to register their firearms with a designated law enforcement agency. Blau, Gorry, and Wade (2016) used a linear probability model to estimate how registration requirements relate to the probability of a public shooting incident based on state data covering 1982 to 2013. Controlling for state fixed effects and a linear time trend, as well as for the presence of several other state gun laws and two time-varying state covariates (population size and aggregated personal income), the authors found uncertain effects of gun registration regimes on the


\textsuperscript{12} D.C. Code Ann. §§ 7-2502.08, 22-4510.


\textsuperscript{14} Wash. Rev. Code Ann. §§ 9.41.110(9), 9.41.129.


\textsuperscript{16} N.Y. Penal Law §§ 265.00(22), 265.00(23), 400.00(10), (16-a), 400.02; Md. Code Ann., Pub. Safety, § 5-143.

likelihood of a public shooting event. However, their use of a linear model to predict a dichotomous (and rare) outcome likely violated model assumptions and rendered the results unreliable. Indeed, the estimated effect size is implausibly large in magnitude, with confidence intervals that span nearly the full possible range of outcomes. The authors’ estimated linear probability model can yield predicted probabilities of active shooting incidence that extend far outside the definitional 0 to 1 range of a probability, depending on the particular combination of policies present in a given state. Moreover, the estimated model implies negative incidence rate ratios (IRRs), which represent implausible effect sizes, for some of the gun policies that we are studying. This indicates a serious model misspecification (Cox and Snell, 1989; Aldrich and Nelson, 1984) and prevents us from interpreting the estimated coefficients as causal effect estimates. Given concerns with the model, we discount the results of this study for purposes of clarifying the effects of the law and do not produce a figure with its estimated effects.

Conclusions
One study found uncertain effects of firearm registration requirements on whether at least one mass shooting occurred in a state (Blau, Gorry, and Wade, 2016), and we discounted that study’s evidence for methodological reasons. Considering this one study, we find inconclusive evidence for the effect of firearm sales reporting, recording, and registration requirements on mass shootings.

Effects on the Gun Industry

Research Synthesis Findings
We identified one study that examined how state firearm registration laws influenced per capita rates of NICS background checks for firearm purchases from 1999 to 2010 (Steidley and Kosla, 2018). The authors estimated linear models with a logged outcome, adjusting for a linear national trend; random effects for states; region fixed effects; and state-level covariates reflecting political factors, demographics, and prox-

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18 For example, the model coefficient for stand-your-ground laws implies an IRR of –2, with a 95-percent confidence interval that lies entirely in the negative IRR range.

19 The authors defined registration laws as those in which “a firearms registry for any or all types of firearms is enforced” (Steidley and Kosla, 2018, pp. 92–93).
ies for firearm prevalence. Models showed a large and significant negative relationship between registration laws and NICS background checks for firearm purchases. However, insufficient information was provided in the paper to understand the extent to which this estimate reflects between-state versus within-state variation. Given the reported mean value for the registration law variable, the short time frame studied, and the absence of state fixed effect controls, it is unclear whether the study meets our inclusion criteria of having data both before and after policy implementation. Thus, we do not present these estimates in a figure.

Conclusions

We identified one study that reported a negative association between registration laws and a proxy for gun purchases (Steidley and Kosla, 2018). Because the paper did not provide sufficient information about variation in registration laws to determine whether the study qualified for inclusion in this review, we find inconclusive evidence for the effect of firearm sales reporting, recording, and registration requirements on firearm purchases.

Outcomes Without Studies Examining the Effects of Firearm Sales Reporting, Recording, and Registration Requirements

We did not identify any research that examined the relationship between firearm sales reporting, recording, and registration requirements and the following outcomes:

- suicide
- violent crime
- unintentional injuries and deaths
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Thirteen References


Code of Federal Regulations, Title 28, Section 25.9, Retention and Destruction of Records in the System.


Giffords Law Center to Prevent Gun Violence, “Maintaining Records of Gun Sales,” webpage, undated-e. As of October 18, 2017: 


United States Code, Title 18, Section 923, Licensing.

United States Code, Title 18, Section 926, Rules and Regulations.

United States Code, Title 26, Section 5841, Registration of Firearms.
The term *assault weapon* is controversial. In state and federal gun laws, it generally refers to specific semiautomatic firearm models that are designed to fire a high volume of ammunition in a controlled way or that have specified design features, such as folding stocks or pistol grips (for differences in definitions across states, see the next section).° Those in the gun industry refer to many of these firearms as *modern sporting rifles*, contending that *assault rifle* applies only to fully automatic weapons used by militaries (National Shooting Sports Foundation, undated). Furthermore, they argue that the characteristics used to differentiate banned firearms from nonbanned semiautomatic weapons are cosmetic and do not make them more deadly than similar weapons without those features. In 1994, Congress passed the Violent Crime Control and Law Enforcement Act, which banned “the manufacture of military-style assault weapons, assault weapons with specific combat features, ‘copy-cat’ models, and certain high-capacity ammunition magazines of more than ten rounds” (U.S. Department of Justice, 1994; see also Pub. L. 103-322). The law included a sunset provision, calling for its repeal after ten years. It was not renewed in 2004, and thus there is not currently a federal assault weapon ban (Plumer, 2012).

Although laws restricting assault weapons and those restricting high-capacity magazines are distinct constructs, these policies have almost always been implemented together in practice, so disentangling their potential effects is challenging. Both types of laws are primarily intended to reduce firearm-related casualties and fatalities from violent crime—and, more specifically, from mass shooting incidents. The bans could impact firearm-related violence by decreasing the number of shooting incidents, decreasing the number of casualties in a given shooting, and decreasing the

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° Semiautomatic pistols, rifles, and shotguns, as defined at the federal level, are firearms that use energy expended from the firing cartridge to extract the fired cartridge case and automatically chamber the next round of ammunition but require a pull of the trigger for each shot; semiautomatic *assault weapons* include a specific list of firearm models (e.g., TEC-DC9, Beretta AR70 (SC-70), Colt AR-15), as well as semiautomatic firearms that have at least two of a designated list of features (27 C.F.R. 478.11). In contrast, fully automatic weapons (i.e., machine guns) can produce continuous fire by a single trigger function without manual reloading, and their sale and possession has been federally regulated since the National Firearms Act of 1934 (currently codified as amended as 26 U.S.C. 5801 et seq.).
incident casualty rate. That is, other things being equal, a shooter with an assault weapon or other semiautomatic weapon equipped with a high-capacity magazine can fire more ammunition and hence inflict more casualties in a given length of time than would a shooter using weapons with a lower rate of fire and capacity. In a mass shooting incident, the lower rate of fire should allow for more people to evacuate and for law enforcement or others to intervene more easily. To most precisely characterize the causal effect of these laws on violent crime or mass shootings, the ideal data would distinguish crime and violence outcomes by whether a designated assault weapon or high-capacity magazine was used. Although limited data on the weapons used in homicides are available through the Federal Bureau of Investigation (FBI)’s Supplementary Homicide Reports and details of the weapons and ammunition used in mass shooting incidents are increasingly being compiled on a case-by-case basis (e.g., by Mother Jones magazine), none of the articles meeting our inclusion criteria for this policy analyzed crime or violence outcomes by weapon type.

The majority of firearm crimes are not conducted with rifles but with handguns, most of which are not considered assault weapons (although most assault weapon bans also list certain “assault pistols” among the banned firearms). In 2017, 403 of the 10,982 firearm-related murders reported in FBI data involved any type of rifle; the type of firearm used in 3,096 of these murders was not specified (FBI, 2018b). Assuming that no substitution in favor of other types of firearms would occur, the elimination of all rifle homicides would have decreased the number of firearm-related murders by 3.7 percent.

Assault weapons and high-capacity magazines are used disproportionately in mass public shootings and killings of law enforcement officers compared with murders overall. However, these incidents are relatively rare. Data combining 184 mass shooting, spree shooting, and active shooter events from 1982 to 2015 suggest that about 30 percent of incidents involved assault weapons and 37 percent of incidents involved high-capacity magazines (Blau, Gorry, and Wade, 2016). Another analysis that focused on mass shooting events involving four or more fatalities between 2009 and 2016 reported that 15 of these incidents (11 percent) involved an assault weapon or high-capacity magazine, resulting in 155 percent more injuries and 47 percent more fatalities compared with other incidents (Everytown for Gun Safety Support Fund, 2017b). In a 2018 update to the same analysis, the authors found that, of all mass shooting incidents between 2009 and 2017 in which the capacity of the firearm was known, 35 (58 percent) involved a high-capacity magazine (Everytown for Gun Safety Support Fund, 2018). These incidents resulted in 14 times the number of people injured and two times the number of fatalities compared with incidents that did not involve a high-capacity magazine. Of course, it could be that shooters who intend to kill many people prefer high-capacity magazines but that, even without such magazines, they would kill more people than those who do not use high-capacity magazines in their mass shootings. Other research, focused on a small subset of shootings in which multiple victims
were targeted, suggests that the rate of fire at mass shootings is not so high that reloading would affect the number of rounds fired (Kleck, 2016). If this finding generalized to all multiple-victim shootings, it would call into question the usefulness of laws banning high-capacity magazines, because the primary objective of such laws is to reduce the number of rounds a shooter can fire before having to reload.

Of the 50 felonious fatal shootings of law enforcement officers in 2018, 18.1 percent involved any type of rifle (FBI, 2019d). Although relatively outdated, estimates from 1994 suggest that between 31 percent and 41 percent of firearms used in murders of police officers involved assault weapons or other guns equipped with high-capacity magazines (Adler et al., 1995).

There is little theoretical or logical basis to suggest that bans of assault weapons and high-capacity magazines would impact rates of suicide or unintentional injury. And although these policies could plausibly impact defensive gun use, the magnitudes of any such effects are likely small. The FBI reported that, in 2017, just six of the 353 firearm-related justifiable homicides by private citizens involved any type of rifle (FBI, 2018c).

Laws banning assault weapons, high-capacity magazines, or both would have direct market effects for the gun industry, including impacts on production, price, and potential spillovers from primary to secondary markets (Koper, 2004). The market effects of restricting the manufacturing and sales of a class of weapons or ammunition will depend on the relative demand for these items, the availability of nonbanned weapons that serve as close substitutes, and the costs of modifying existing weapon types to meet the requirements of the ban, to name a few. A nationwide ban could also impact the industry more broadly by generating market effects for ancillary gun companies that produce or sell certain replacement parts, accessories, or specialized magazines and precision barrels used primarily for sport shooting.

Overall, the effects of these policies will depend largely on the design and implementation of the law. Except for heavily regulated weapons manufactured prior to May 1986, assault weapons capable of automatic fire are not available for sale in the United States. Thus, the specifics of which weapons or weapon features are prohibited by a particular ban are key to understanding the marginal effect of each policy on outcomes of interest. Targeting weapons or ammunition features with close substitutes likely limits any potential policy effects on violent crime. Similarly, bans targeting features unrelated to the deadliness of the weapon or its likelihood of being used in the perpetration of violence may lessen policy effects unless those features are correlated with characteristics of the weapon that determine its lethality. Furthermore, most existing state bans (and the federal ban of 1994) influence the flow of only new weapons or magazines and do little to affect the existing stock; the National Shooting Sports Foundation, a trade association for the gun industry, estimates that more than 8.5 million modern sporting rifles were either manufactured in or imported to the United States between the 1990s and 2013 (National Shooting Sports Founda-
Bans that are accompanied by a compensatory buyback scheme—as was implemented with Australia’s 1996 National Firearms Agreement (for more information, see RAND Corporation, 2018, Chapter Twenty-Four)—could arguably have far larger effects.

State Implementation of Bans on the Sale of Assault Weapons and High-Capacity Magazines

As of January 1, 2020, seven states and the District of Columbia ban assault weapons. Two of the eight jurisdictions list the specific weapon models that are banned and prohibit all weapons with specific features; one state bans only the weapons listed, and two states ban only specific features. The laws that list specific banned models are similar state to state, although the lists are not generally identical.

California is an example of a state that has a list of banned weapons, including rifles, shotguns, and firearms with specific design features. Specifically, it bans “all AK series including, but not limited to, the models identified,” and explains that the term series “includes all other models that are only variations, with minor differences, of those models listed in subdivision (a), regardless of the manufacturer.” Furthermore, the state provides a list of features, any one of which renders a firearm to be an assault weapon as defined under California’s state law and to be therefore banned. For example, the law states that a “semiautomatic, centerfire rifle that has the capacity to accept a detachable magazine” is an assault weapon if it also contains any of the following features: “(A) a pistol grip that protrudes conspicuously beneath the action of the weapon; (B) a thumbhole stock; (C) a folding or telescoping stock; (D) a grenade launcher or flare launcher; (E) a flash suppressor; (F) a forward pistol grip.”

Connecticut’s list is similar to California’s, but the language is different. For example, in its subsection banning the AK series of weapons, Connecticut’s law includes “[a]ny of the following specified semiautomatic centerfire rifles, or copies or duplicates thereof with the capability of any such rifles, that were in production prior to or on April 4, 2013.” In addition, like California, Connecticut has a long list of features, any of which renders a firearm banned. The District of Columbia’s list is shorter and does

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3 Calif. Penal Code § 30510.
4 Calif. Penal Code § 30515.f.
5 Calif. Penal Code § 30515.
not state that the ban includes similar makes and models to the ones listed. However, the law also bans firearms with specific design features.\(^7\) Maryland and Massachusetts are the other two states that ban by both list and features. Maryland bans weapons that possess any two features from its list.\(^8\) The Massachusetts law, which refers to the now-expired federal law (Pub. L. 103-322), also requires two features to be included.\(^9\)

New Jersey is the only state whose law includes a list of banned weapons but not generic features.\(^10\) Conversely, New York and Hawaii ban only certain firearm features, not specified models of firearms.\(^11\)

In addition to definitional differences, the laws are distinct in other ways—namely, their treatment of grandfathered weapons. For example, the District of Columbia does not allow grandfathering of assault weapons (Giffords Law Center to Prevent Gun Violence, undated-a); however, all seven states with assault weapon bans do, but under different regimes. Six of the states require registration of grandfathered assault weapons; in New Jersey, registration allows grandfathered assault weapons to be used only for target shooting.\(^12\)

The same jurisdictions that have banned assault weapons have also banned high-capacity magazines, as have Colorado and Vermont.\(^13\) Hawaii, which bans only assault pistols, similarly bans only high-capacity magazines for pistols.\(^14\) The rest ban high-capacity magazines for all firearms,\(^15\) although there are differences in definition here too. California, Connecticut, Hawaii, Maryland, Massachusetts, New Jersey, New York, and the District of Columbia ban magazines with a capacity of more than ten rounds.\(^16\) Colorado bans magazines with a capacity of more than 15 rounds.\(^17\)

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\(^7\) D.C. Code Ann. § 7-2501.01.
\(^8\) Md. Code Ann. § 4-301.
\(^17\) Colo. Rev. Stat. § 18-12-301.
Effects on Violent Crime

Research Synthesis Findings
We identified two studies that evaluated federal and state assault weapon bans and met our inclusion criteria. Lott (2010) examined the effect of assault weapon bans on violent crime. Detailed results that include coefficients and test statistics were available only for the outcome of homicide (Lott, 2010, Table A6.3). This model indicated an uncertain effect of assault weapon bans on homicide rates, but it had an unfavorable ratio of estimated parameters to observations (less than one to ten), meaning the model may have been overfit, and thus its effect estimates and significance levels may be inaccurate.

Gius (2014) analyzed state-level data from 1980 through 2009 to assess how firearm homicides were influenced by the 1994–2004 federal assault weapon ban and by state assault weapon bans. The analysis of the federal assault weapon ban does not meet our criteria for inclusion: The author included an indicator for years prior to and after the ban as a control, but there was no comparison (control) group. The author found a suggestive effect consistent with state assault weapon bans decreasing firearm-related homicides (see Figure 14.1). However, the model did not account for serial correlation in panel data, which can result in large biases in standard errors (Aneja, Donohue, and Zhang, 2014).

We also identified a 2018 study that assessed how bans on high-capacity magazines affect firearm homicide rates (Moody and Marvell, 2018b). Using a log-linear model with two-way fixed effects, the authors used a hybrid policy specification to estimate the joint effect of law passage on the levels and trends in firearm homicide rates, controlling for state-level socioeconomic and demographic factors, as well as two lags in the outcome variable. Because the estimated effects were identified based on policy changes occurring in only eight states, the authors recognized that clustered standard errors were likely underestimated (Conley and Taber, 2011). Following Helland and Tabarrok (2004), the authors implemented a “placebo law” method to obtain corrected standard errors. Based on the corrected values obtained through the placebo process, their findings showed an uncertain association between high-capacity magazine bans and firearm homicide rates (\( p = 0.53 \)) and a suggestive negative effect on trends in firearm homicide rates (\( p = 0.19 \)).

Figure 14.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the assault weapon ban policies examined in these studies. We exclude the estimate of the federal assault weapon ban from Gius (2014) because the estimate does not meet our criteria for inclusion. Because Moody and Marvell (2018b) only presented a hybrid model specification, for which we cannot calculate a single effect with standard errors, their estimate is also excluded from the figure.
Figure 14.1
Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Assault weapon ban</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Lott (2010)</td>
<td>Total</td>
<td>1.00 [0.93, 1.08]</td>
</tr>
<tr>
<td>State assault weapon ban</td>
<td>Firearm</td>
<td>0.92 [0.81, 1.02]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

Conclusions

We identified two qualifying studies that estimated the effects of assault weapon bans on different violent crime outcomes. One found uncertain effects of such bans on total homicide rates (Lott, 2010); the other found a suggestive effect consistent with assault weapon bans decreasing firearm homicides (Gius, 2014). Considering the relative strengths of these studies, available evidence is inconclusive for the effect of assault weapon bans on total homicides and firearm homicides. We also identified one qualifying study that estimated the effects of high-capacity magazine bans on firearm homicides and found uncertain effects (Moody and Marvell, 2018b), leading us to find that there is inconclusive evidence for the effect of high-capacity magazine bans on firearm homicides.
Effects on Mass Shootings

Research Synthesis Findings

We identified four studies that met our inclusion criteria and estimated the effects of state or federal assault weapon bans on multiple-victim shooting incidents or casualties. The studies evaluated somewhat different outcomes. Gius (2015c) focused on public mass shootings, which the author defined as incidents resulting in four or more firearm-related fatalities (excluding the offender), and the shooting occurred in a relatively public place, victims were selected indiscriminately, and the shooting was not related to criminal activity. Luca, Malhotra, and Poliquin (2016) set the same casualty threshold and also excluded any incident that occurred in connection with criminal activity, but they did not restrict incidents to public settings and excluded all events with fewer than three fatally injured victims who were not related to the shooter (e.g., family, romantic partner). Blau, Gorry, and Wade (2016) considered a broader set of incidents, including mass public shootings with a minimum of four fatalities occurring during a single incident and perpetrated by a single offender; spree shootings occurring across multiple locations in a public place with a minimum of two fatalities; and active shooter incidents, which involve an individual using a firearm and actively killing or attempting to kill others in a confined space or unconfined and populated area. Finally, Gius (2018) analyzed school shooting deaths and injuries using data compiled from Klein (2012), Kalesan et al. (2017), and the Everytown for Gun Safety Support Fund.

Using a Poisson model and data from 1982 through 2011, Gius (2015c) tested whether state assault weapon bans influence public mass shooting fatalities or public mass shooting injuries, controlling for the federal assault weapon ban and state-level variation in demographic, socioeconomic, and criminal justice characteristics.18 Findings showed that state assault weapon bans had a statistically significant but smaller effect of reducing mass shooting death rates to 55 percent of what would have been expected without the bans, but results indicated uncertain effects on mass shooting injuries (see Figure 14.2). This report provided little detail describing variation in the timing of the state bans in relation to the federal ban, and it is unclear whether the estimated effects were confounded by correlation between the state and federal bans. The model did not account for serial correlation in panel data, which can result in large biases in standard errors (Aneja, Donohue, and Zhang, 2014).

Using a linear probability model and data from a later period (1989–2014), Luca, Malhotra, and Poliquin (2016) estimated the effects of state assault weapon bans on a binary indicator for whether a mass shooting occurred in a given state-year. In contrast to Gius (2015c), Luca, Malhotra, and Poliquin (2016) did not control for the

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18 The author found a large and statistically significant association between implementation of the federal assault weapon ban and reductions in mass shooting deaths and injuries. However, because the model included an indicator for years prior to and after the federal ban as a control but there was no comparison group, the analysis of the federal ban does not meet our criteria for inclusion.
federal assault weapon ban from 1994 through 2004, but they controlled for a host of other state-level gun policies; state fixed effects; year fixed effects; and state-level demographic, socioeconomic, and political characteristics. Their findings showed uncertain effects of state assault weapon bans on the probability of a mass shooting incident occurring. However, the effects of gun policies on mass shootings were not the primary focus of Luca, Malhotra, and Poliquin (2016), and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a rare dichotomous outcome. Therefore, model assumptions were likely violated, making CIs unreliable.

Blau, Gorry, and Wade (2016) also used a linear probability model to estimate how a variety of gun laws, including state and federal assault weapon bans, relate to the probability of a public shooting incident occurring based on data covering 1982 to 2013. Controlling for state fixed effects and a linear time trend, as well as for the presence of several other state gun laws and a limited set of state covariates (i.e., population size and aggregated personal income), the authors found that state assault weapon bans were significantly and negatively associated with the likelihood of a public shooting event. However, the use of a linear model to predict a dichotomous (and rare) outcome likely violated model assumptions and rendered the results unreliable. The authors’ estimated linear probability model can yield predicted probabilities of active shooting incidence that extend far outside the definitional 0 to 1 range of a probability, depending on the particular combination of policies present in a given state. Moreover, the estimated model implies negative IRRs, which represent implausible effect sizes, for some of the gun policies that we are studying. This indicates a serious model misspecification (Cox and Snell, 1989; Aldrich and Nelson, 1984) and prevents us from interpreting the estimated coefficients as causal effect estimates.

Finally, Gius (2018) employed a model similar to that of Gius (2015c) to evaluate whether federal or state assault weapon bans influence school shooting fatalities or injuries, controlling for background check laws, state-level variation in demographic and socioeconomic characteristics, and state-level variation in the ratio of firearm suicides to total suicides as a proxy for variation in gun ownership prevalence. Using data from 1990 to 2014, the study showed that the presence of a state or federal assault weapon ban (these were collapsed into one dichotomous indicator) was significantly associated with a 54-percent reduction in the number of school shooting victims (see 19 The model included an indicator for the period of the federal ban, but because the federal ban applied to all states, there was no comparison group. Thus, the analysis of the federal ban does not meet our criteria for inclusion.

20 For example, the model coefficient for stand-your-ground laws implies an IRR of –2, with a 95-percent CI that lies entirely in the negative IRR range.
Figure 14.2). However, it is unclear the extent to which this estimate was identified from the change in the federal law versus from changes in state policy. Additionally, the author did not appear to make adjustments to standard errors to account for serial correlation in panel data, which may lead to overstated precision of the estimates.

Figure 14.2 displays the IRRs and CIs associated with the assault weapon ban policies examined in these studies. We exclude estimates of the federal assault weapon ban from Gius (2015c) and from Blau, Gorry, and Wade (2016) because they do not meet our criteria for inclusion. We also exclude estimates of effects of state assault weapon bans from Blau, Gorry, and Wade (2016), given the concerns with the study results noted earlier.

### Figure 14.2
**Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Mass Shootings**

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
<th>Figure 14.2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Assault weapon ban</td>
<td>Number of deaths and injuries from school shootings</td>
<td>0.46 [0.33, 0.63]</td>
<td>![IRR for assault weapon ban]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2016)</td>
<td>Any mass shooting incident</td>
<td>1.52 [0.60, 2.43]</td>
<td>![IRR for state assault weapon ban]</td>
</tr>
<tr>
<td>State assault weapon ban</td>
<td>Mass shooting</td>
<td></td>
<td>![IRR for state assault weapon ban]</td>
</tr>
<tr>
<td>Gius (2015c)</td>
<td>Deaths</td>
<td>0.55 [0.33, 0.92]</td>
<td>![IRR for deaths]</td>
</tr>
<tr>
<td>Gius (2015c)</td>
<td>Injuries</td>
<td>1.35 [0.81, 2.23]</td>
<td>![IRR for injuries]</td>
</tr>
</tbody>
</table>

**NOTE:** IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

### Conclusions

We identified four qualifying studies that estimated the effects of state assault weapon bans on different aspects of mass shootings. Gius (2015c) found that these bans significantly reduce mass shooting deaths but have uncertain effects on injuries resulting from mass shootings. Using similar models, however, Gius (2018) found that assault weapon bans resulted in significantly fewer casualties (deaths and nonfatal injuries) from school shootings. Using a data set similar to that used in Gius (2015c), Luca, Malhotra, and Poliquin (2016) found uncertain effects of state assault weapon bans on the annual incidence of mass shootings. And Blau,
Gorry, and Wade (2016) found that the bans significantly reduced the annual incidence of mass shootings. Considering our assessment of these findings and the relative strengths of these studies, we find inconclusive evidence for the effect of assault weapon bans on mass shootings.

Effects on the Gun Industry

Research Synthesis Findings

We identified one study examining the effects of the federal assault weapon ban on prices in secondary markets of assault weapons that were purchased before 1994 and thus not prohibited from being sold under the terms of the federal ban. In an update to earlier studies (Koper and Roth, 2001, 2002; Roth and Koper, 1997, 1999), Koper (2004) compared secondary-market prices for firearms banned under the law with prices for similar firearms unaffected by the ban between 1991 and 1999, a period that includes when the federal ban took effect (September 13, 1994). In an analysis of assault pistols covered under the ban, the author reported no significant changes in price before or after the ban. Although the comparison firearms, “Saturday night special” handguns (i.e., inexpensive, small-caliber guns), showed steady declines in price over the same period, the effect of the federal law on these different price trends was not well identified. An analysis of secondary-market prices for banned assault rifles compared with other semiautomatic rifles not covered under the ban found sharp increases in the price of the banned rifles in 1994 and 1995, but prices returned to pre-ban levels for the remainder of the study period. In contrast, the price of comparison rifles remained constant over the same time frame.

Koper (2004) also examined manufacturer production of banned and comparison weapons between 1985 and 2001. He found that production of banned assault pistols rose substantially in 1993 and 1994 before the ban took place, but it then fell to below pre-ban levels even though several manufacturers were producing modified versions of the banned assault pistols that were not covered by the law. Surprisingly, however, a similar surge and subsequent decline was found for the manufacture of “Saturday night special” handguns, which were not subject to the ban, although these shifts were not as large.

Production of assault rifles also surged immediately prior to the ban but declined to pre-ban levels by 1996. In contrast with the demand for assault pistols, a strong demand for semiautomatic rifles modified so as not to be covered by the ban is reflected in a surge of production by the end of the 1990s, and production remained above pre-1993 levels in 2000 and 2001.

Koper (2004) did not provide enough information for us to calculate IRRs and CIs for the effect sizes of interest, so we do not present them in a figure.
Conclusions

One study provided some evidence that secondary-market prices of assault rifles, but not assault pistols, surged immediately before and in the year after the ban took effect. The ban appeared to affect manufacturer behavior, with production of assault pistols and assault rifles rising in the two or three years prior to the law taking effect. The production of semiautomatic pistols modified so as not to be covered by the ban did not recover to pre-ban levels over the study period, at least for the four manufacturers analyzed. Production of semiautomatic rifles that were modified so as not to be covered by the ban did recover to greater than pre-ban levels. Because this is a single study on just one version of an assault weapon ban, we conclude that there is limited evidence that assault weapon bans lead to short-term price increases and that evidence for such laws’ effects on the production of different classes of banned weapons is inconclusive.

Outcomes Without Studies Examining the Effects of Bans on the Sale of Assault Weapons and High-Capacity Magazines

We did not identify any research that met our inclusion criteria and examined the effects of assault weapon or high-capacity magazine bans on the following outcomes:

- suicide
- unintentional injuries and deaths
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Fourteen References


Code of Federal Regulations, Title 27, Section 478.11, Meaning of Terms.


FBI—See Federal Bureau of Investigation.


United States Code, Title 26, Section 5801, Imposition of Tax.

The Gun Control Act of 1968 (Pub. L. 90-618) was the first federal regulation to impose restrictions on firearms based on actual or perceived quality. The stated goal of the policy was to restrict access to guns for high-risk individuals who relied on low-cost and concealable firearms to commit crimes but not for law-abiding gun owners who would use firearms for “sporting purposes” (Milne et al., 2003). Instead of identifying a specific test for quality, the law barred any person from knowingly importing firearms or ammunition into the United States if such weapons were not “generally recognized as suitable for or readily adaptable to sporting purposes” (Zimring, 1975). In doing so, federal lawmakers created a partly subjective definition intended to target small, less accurate firearms. Domestic manufacturers are exempt from the federal restrictions.

Since 1968, several state laws have aimed to extend or clarify design and safety requirements on all guns in circulation in the United States, not just imported guns. Specifically, lawmakers have targeted low-quality handguns, sometimes referred to as “Saturday night specials” or “junk guns” in policy initiatives. Although there is no official definition for low-quality handguns, they are often characterized as small, inexpensive, and made from low-quality materials or designs (Vernick, Webster, and Hepburn, 1999). State laws banning low-quality handguns typically apply to the manufacture, sale, and transfer of such guns, but some states outlaw even the possession of these weapons. The theory behind these laws is that the poor performance and quality of such firearms may put gun owners at risk of accidental injury or death (Wintemute, 1994), and their low cost and compact concealable designs make them attractive to criminals with limited resources (Cook, 1981). Although a range of firearms could be made with poor-quality materials and consequently threaten consumers’ safety, state policies regulating low-quality guns generally focus on handguns because they are so commonly preferred by criminals. Using a nationally representative survey of crime victims, the Bureau of Justice Statistics found that, in 2011, handguns were used in 73 percent of murders involving a firearm and in 88 percent of nonfatal violence involving a firearm (Planty and Truman, 2013).

States have adopted two general strategies for restricting the availability of low-quality handguns: laws that restrict handguns based on assessment of the quality
of materials used in their production (i.e., through melting point tests that evaluate whether the heat from firing the weapon threatens to damage the gun) and laws that restrict handguns based on assessment of the quality of the design of the handgun (i.e., through drop tests meant to evaluate whether the gun is liable to discharge accidentally, such as when dropped on a hard surface, and firing tests that establish the reliability and durability of the weapon). To sell products in a state with low-quality handgun regulations, importers or manufacturers must request that their firearms be evaluated by the state; to complete the test, the importers or manufacturers submit several of their handgun models to the relevant state officials, who conduct the tests and evaluate whether the firearms meet state requirements.

Some states also require specific handgun safety features that protect against unintended discharge. These laws may require that all handguns have a chamber loader indicator, which indicates when there is a cartridge in the firing chamber, or a magazine disconnect mechanism, which prevents a semiautomatic pistol with a detachable magazine from firing when there is no magazine in the pistol. Finally, some states create a roster of approved handguns that satisfy the state’s safety and design standards. In some states, these rosters simply include firearms that have passed melting, firing, or drop tests, while other states take a range of additional factors into consideration, such as a gun’s ballistic accuracy, caliber, detectability with security equipment, and concealability (typically measured by size), as well as its utility for legitimate sporting activities, self-protection, or law enforcement purposes.

If bans on low-quality firearms function as intended by removing unreliable firearms from the legal market, the laws should reduce unintentional injuries and deaths, although there is limited existing data that provide detail on the types of firearms involved in unintentional gun injuries sufficient to shed light on the potential magnitude of such policy effects.

By prohibiting inexpensive firearms, these laws may also lead to higher prices for firearms overall, plausibly lowering criminal access to such products if cost constraints are binding for these individuals. Indeed, several studies have documented the high proportion of low-quality and inexpensive handguns used in crimes (Vernick, Webster, and Hepburn, 1999; Zawitz, 1995). Using data from the Bureau of Alcohol, Tobacco, and Firearms and its Operation Concentrated Urban Enforcement program, Cook (1981) demonstrated that more than two-thirds of violent crime was perpetrated with a small handgun having a barrel length of 3 inches or less. Furthermore, although criminal offenders do not express a preference for inexpensive weapons when surveyed (Wright and Rossi, 1986), Cook (1981) reported that 40 percent of handguns used in violent crime were sold at retail for less than $50, suggesting that cheap and concealable guns were popular among criminals. Other studies of crime guns traced back to sales in Maryland similarly found that handguns with a barrel length of 3 inches or less constituted 40 percent of crime guns but just 27 percent of legal sales, and handguns
costing $150 or less accounted for about one-fourth of crime guns but only 7 percent of sales (Koper, 2003, 2014).

Acquisition of such inexpensive, low-quality weapons has also been shown to be associated with prior and prospective criminal behavior (Wright, Wintemute, and Claire, 2005; Wintemute et al., 1998). Wintemute et al. (1998) looked at 5,360 authorized purchasers of handguns in California, including eligible purchasers with a previous criminal history and a random sample of purchasers with no such history. Handgun purchasers with a previous criminal history were more likely than those without such a history to purchase a small, inexpensive handgun, while purchasers of inexpensive handguns were more likely than purchasers of other handguns to be charged with new crimes after handgun purchase.

These studies suggest that small and inexpensive handguns are disproportionately preferred by those engaged in criminal activity, but the effectiveness of bans on low-quality handguns for reducing homicide rates will hinge on the extent to which these regulations on the formal market decrease criminal access to firearms and the extent to which similar handguns are available through illicit channels or from states that do not impose these restrictions. For instance, although the low-quality handguns targeted by state regulations tend to be less expensive in terms of fair market value, a study in Boston suggested that these types of weapons face the most exorbitant markups in the illicit market (Hureau and Braga, 2018), indicating that there is a fixed cost of criminal-market transactions for well-known low-quality handguns. Indeed, the Boston study suggested that firearms of similar price but higher quality (in terms of fair market value) may be available in criminal markets. Additionally, although some evidence from gun trace data suggests that state bans on low-quality handguns are associated with significantly reduced exportation of crime guns (Webster et al., 2013), the availability of these weapons from other states may compensate for reduced exports from states with these bans.

If bans on low-quality handguns are able to reduce criminal access to firearms, they may also decrease the number of officer-involved shootings by limiting the number of armed individuals whom law enforcement encounters and reducing the need to use firearms for self-defense. However, by removing some of the least expensive guns from the legal market, such regulations may make it more difficult for economically disadvantaged individuals to obtain a weapon for self-defense (Funk, 1995). Conceptually, the overall effect that bans on low-quality handguns have on defensive gun use is ambiguous: The laws may reduce firearm access to some individuals who seek to purchase a firearm legally for self-defense while also increasing the probability of using

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1 The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined crime gun as “any firearm that is illegally possessed, used in a crime, or suspected to have been used in a crime. An abandoned firearm may also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed.”

It is less clear how design standards and low-quality handgun regulations would affect such outcomes as suicide, mass shootings, or hunting and recreation. Although handguns are commonly used in suicides (Hanlon et al., 2019) and individuals who die by suicide are more likely than the general population to have purchased a handgun in the previous three years (Grassel et al., 2003), it is doubtful that design features and quality tests alone would decrease the likelihood of suicide. These standards are meant to improve the accuracy and reliability of a weapon, which would not decrease suicide attempts or suicide rates. However, the higher prices that these regulations might promote could discourage some populations with less disposable income (such as adolescents or lower-income adults) from purchasing a weapon and subsequently using it with suicidal intent.

Similarly, the potential impact on mass shootings is unclear. Small handguns might be less detectable when brought into public places or schools, but a price increase resulting from quality regulations might deter some potential mass shooting perpetrators. However, handguns are frequently used in mass shootings. Between 2000 and 2016, 31 percent of mass shooting victims were shot by handguns (Sarani et al., 2019). Further research is needed to assess whether low-quality handgun regulations would reduce access to firearms among possible mass shooters if the potential shooters could not afford the more expensive handguns.

Because bans on low-quality handguns specifically do not target firearms used for hunting or sport shooting purposes, these laws are unlikely to affect hunters and recreational gun users. But such laws could affect the gun industry. Quality tests require manufacturers to invest in higher-quality materials and add specific features to their products, which likely increases production costs. Although manufacturers could pass this cost increase on to consumers through retail prices, they could lose a segment of the market that is willing to purchase a gun only at a lower price point. The magnitude of this effect would depend on how sensitive consumers are to price, as well as the extent to which—in the absence of such a ban—the retail price of low-quality handguns was already significantly marked up relative to the actual price of the raw materials and labor; without foreign competition, sellers of these guns may have been able to create substantial profit margins regardless of safety requirements. Finally, higher-quality standards could also protect the gun industry from future liability for faulty firearms that cause injury or death.²

² The gun industry is largely protected from liability from criminal use of its products, but gun manufacturers are liable for malfunctioning products, such as those that might be produced without quality regulations (Kurtzleben, 2015). In 2018, after several deaths were linked to misfiring Remington firearms, some 7.5 million owners of the relevant gun models filed a class-action suit alleging that the guns can fire without being triggered (Cohn, 2018). The case ultimately was settled.
The specifics of which weapons or weapon features are prohibited by a particular ban may be key to understanding the effects of these policies. Policies that target weapons with close substitutes or that are not commonly involved in unintentional or intentional gun injury will likely limit the magnitude of the policy effects. A stronger causal claim might be made by examining handgun-specific outcomes, but outcome data would need to have a significant amount of detail on handgun characteristics for analysts to be able to isolate effects on outcomes involving weapons explicitly banned by state policy. Understanding the potential time course of the effects of bans on low-quality handguns is also challenging. Because the bans typically apply only to the flow of new weapons, they may have limited effect on the existing stock, and it may take time for the full effects of a policy to occur. However, given that the lower-quality handguns affected by these bans may be less durable than other firearms, the existing stock of usable low-quality handguns might deplete more rapidly than in the case of, for instance, bans on assault weapons (see Chapter Fourteen). Conversely, in states where the law also applies to possession of lower-quality handguns, the full impact of the law could be more immediate as current owners remove their weapons from circulation to avoid criminal charges.

State Implementation of Bans on Low-Quality Handguns

As of January 1, 2020, seven states and the District of Columbia have laws implementing design safety standards for handgun manufacturers. Many of these jurisdictions use more than one test or mechanism to ensure that guns of insufficient quality are effectively banned. California, Massachusetts, and New York require handguns to pass drop and firing tests, while Hawaii, Illinois, Massachusetts, Minnesota, and New York have laws imposing a melting point test. California, Maryland, Massachusetts, and the District of Columbia publish lists of approved handguns.

California, Massachusetts, and New York also require handguns to have specific safety features. For example, California defines an “unsafe handgun” as a revolver that lacks a safety mechanism that causes the hammer to retract to a point where the firing pin cannot make contact with the primer of the cartridge; a pistol that does not have a positive manually operated safety device; a center fire semiautomatic pistol that does

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3 California, Hawaii, Illinois, Maryland, Massachusetts, Minnesota, New York, and the District of Columbia.
not have either a chamber load indicator or a magazine disconnect mechanism; or a center fire semiautomatic pistol that does not have both a chamber load indicator and, if it has a detachable magazine, a magazine disconnect mechanism.\(^8\)

In general, bans on low-quality handguns apply to the manufacture, sale, and transfer of a handgun in the state. To be sold by a licensed firearm distributor in the state, a manufacturer must prove that the firearm meets the state’s requirements. However, in some cases,\(^9\) state laws also apply to possession, asserting that any citizen who purchases a prohibited handgun after the law passes (or another designated time period) could be subject to criminal charges. And state laws often have unique exceptions. For example, in California, private sales may be exempt from safety regulations, while in Massachusetts, owners of handguns purchased on or before October 21, 1998, are exempt from safety requirements.

**Effects on Suicide**

**Research Synthesis Findings**

We identified one study that met our inclusion criteria and examined the association between bans on low-quality handguns and suicide. Analyzing data from 1979 to 1998, Rosengart et al. (2005) estimated the effect of laws prohibiting the sale of cheaply constructed handguns (referred to by the authors as “junk gun” bans) on total and firearm suicide mortality. Controlling for several other state gun laws (minimum age laws, limits on multiple gun purchases, and shall-issue concealed-carry laws), state-level demographic and socioeconomic characteristics, state and year fixed effects, and state-specific linear trends, the authors found that junk gun bans were associated with significantly lower rates of total suicide. Results showed negative but only suggestive effects for firearm suicide (see Figure 15.1). However, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning the model may have been overfit, resulting in estimates and confidence intervals (CIs) that are unreliable indicators of the true causal effects of the laws. Furthermore, during the period studied, only one state (Maryland) passed a ban on junk guns, limiting effect size generalizability and indicating that the cluster adjustment to the standard errors is still likely to underestimate the variance of the estimated policy effect (Schell, Griffin, and Morral, 2018; Cameron, Gelbach, and Miller, 2008).

Figure 15.1 displays the incidence rate ratios (IRRs) and CIs associated with the low-quality handgun bans examined in Rosengart et al. (2005).

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8 Calif. Penal Code § 31910.

Conclusions

One study estimating the effects of policies banning the sale of low-quality handguns found that the laws were significantly and negatively associated with total suicide rates, while the estimated effect of the laws on firearm-related suicides was negative but only suggestive. Given that the evidence from this study was derived entirely from the law change in Maryland, and given our other concerns about appropriate inferential statistics, we find inconclusive evidence for how bans on low-quality handguns affect total suicides and firearm suicides.

Effects on Violent Crime

Research Synthesis Findings

We identified four studies that met our inclusion criteria and examined the association between bans on low-quality handguns and homicide rates. Two studies provided evidence from a single state ban (Maryland), and two provided effect estimates drawing from multiple state laws, although study time frames were still restricted to a small set of policy changes. The earliest study (Webster, Vernick, and Hepburn, 2002) estimated an autoregressive integrated moving average model, while the other three studies used two-way fixed-effects regression models, controlling for average differences across states and national trends in homicide rates.

Webster, Vernick, and Hepburn (2002) estimated a first-differences model to examine how the passage (in 1988) and implementation (in 1990) of Maryland’s ban on low-quality handguns influenced firearm homicide rates. The authors controlled for firearm homicide rates in Pennsylvania and Virginia to account for confounding...
influences on handgun sales over the relevant period. Their preferred model included a second-order autoregressive process that assumed an immediate but gradual effect of the law (i.e., the effect of the law began in 1990 and increased linearly during the first four years after the law was enacted). The estimated effect from this model showed a significant 8.6-percent reduction in the firearm homicide rates following the ban relative to the counterfactual. However, this study of a single state may have limited generalizability. Moreover, because the model had fewer than five observations per estimated parameter, there is a substantial likelihood that the model was overfit. Thus, in keeping with our review methodology, we discount the results of this study.

Rosengart et al. (2005) used data from 1979 to 1998 to assess whether laws prohibiting the sale of cheaply constructed handguns (referred to by the authors as “junk gun” bans) influenced total and firearm homicide rates. Controlling for several other state gun laws (minimum age laws, limits on multiple gun purchases, and shall-issue concealed-carry laws), state-level demographic and socioeconomic characteristics, state and year fixed effects, and state-specific linear trends, the authors found that junk gun bans were associated with uncertain effects on both total and firearm homicide rates (see Figure 15.2). However, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning the model may have been overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws. Furthermore, during the period studied, only one state (Maryland) passed a ban on junk guns, limiting the generalizability of the estimated effect and indicating that the cluster adjustment to the standard errors is still likely to underestimate the variance of the estimated policy effect (Schell, Griffin, and Morral, 2018; Cameron, Gelbach, and Miller, 2008).

In estimating the effect of concealed-carry laws on murder rates, Lott (2010) controlled for several other state gun laws, including bans on low-quality handguns. Using data spanning 1977 to 2005 and including a wide variety of state-level time-varying covariates, the study’s log-linear specification indicated that the bans were associated with significant increases in murder rates. However, this model did not appear to make any adjustments to account for autocorrelation within states, and the model had an unfavorable ratio of estimated parameters to observations (less than one to ten), meaning the model may have been overfit and its associated effect estimates and significance levels may be inaccurate.

Covering a later period, from 1999 to 2012, Webster, Crifasi, and Vernick (2014) also produced estimates of the effects of low-quality handgun bans on homicide rates, although the focus of their study was on the effects of Missouri’s permit-to-purchase repeal (see Chapter Nine, on licensing and permitting requirements). Using linear models with state and year fixed effects and controlling for socioeconomic and criminal justice factors, the authors found a significant positive relationship between bans on low-quality handguns and overall age-adjusted homicide rates. Analyses by homicide type showed significant positive relationships between the bans and both firearm and
nonfirearm homicide rates. The authors did not provide information on the number of states that changed the bans during the study time frame, but it is likely that only one state (California) changed its low-quality handgun law over this period. Thus, the effect of these laws may not have been well identified, and the standard error of the effect estimate may have been underestimated. Furthermore, the statistical model used to arrive at these results used a large number of estimated parameters relative to observations (a ratio of about one to eight), meaning the model may have been overfit, and thus its estimates of the laws’ effects and their apparent statistical significance could provide little generalizable information.

Figure 15.2 displays the IRRs and CIs associated with the handgun bans examined in these studies. Estimates from Webster, Vernick, and Hepburn (2002) are not displayed for the reasons mentioned earlier.

**Figure 15.2**

**Incidence Rate Ratios Associated with the Effect of Low-Quality Handgun Bans on Violent Crime**

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ban on low-quality handguns</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Lott (2010)</td>
<td>Total</td>
<td>1.35 [1.15, 1.59]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Total</td>
<td>0.94 [0.78, 1.14]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Total, age-adjusted</td>
<td>1.06 [1.02, 1.11]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Firearm</td>
<td>0.94 [0.73, 1.19]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Firearm, age-adjusted</td>
<td>1.07 [1.02, 1.12]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Nonfirearm, age-adjusted</td>
<td>1.09 [1.03, 1.16]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

**Conclusions**

*Total homicides.* Three studies provided evidence for how bans on low-quality handguns affect total homicide rates. One additional study meeting our inclusion criteria was discounted on methodological grounds (Webster, Vernick, and Hepburn, 2002). One study found an uncertain effect (Rosengart et al., 2005), and two studies found that the bans significantly increased homicide
rates (Webster, Crifasi, and Vernick, 2014; Lott, 2010). These studies used two inde-
dependent data sets and covered varying time frames. Because both studies had serious
methodological weaknesses, we find inconclusive evidence for how bans on low-quality
handguns affect total homicides.

Firearm homicides. Three studies produced usable evidence for how bans on low-
quality handguns affect firearm homicide rates. One study of Maryland’s ban found
that the state law had an uncertain effect on firearm homicide (Rosengart
et al., 2005). However, another study
of Maryland’s law, which specified a
time-varying instead of constant effect
of the law, found that Maryland’s ban
on low-quality handguns resulted in sig-
nificantly lower rates of firearm homi-
cide (Webster, Vernick, and Hepburn,
2002). Finally, a study of a more recent
ban found that the law resulted in
significantly higher rates of firearm homicide, as well as higher rates of nonfirearm
homicide (Webster, Crifasi, and Vernick, 2014). Together, we find that the existing
research provides inconclusive evidence for how bans on low-quality handguns affect fire-
arm homicides.

Effects on the Gun Industry

Research Synthesis Findings
We identified one study that met our inclusion criteria and assessed how bans on
low-quality handguns affect gun industry outcomes. Using a first-differences model,
Webster, Vernick, and Hepburn (2002) examined how the passage (in 1988) and
implementation (in 1990) of Maryland’s ban on low-quality handguns influenced per
capita handgun sales in the state. The authors used models that controlled for hand-
gun sales in Pennsylvania to account for confounding influences on handgun sales
over the relevant period. They found a suggestive effect consistent with an increase in
per capita handgun sales that occurred in the two years between the law’s introduction
and its effective date, followed by a sharp drop in sales after the law’s effective date,
although the difference in the post-implementation period was not statistically signifi-
cant. However, given that this was a study of a single state over a relatively short period
(1983 to 1998), these findings may have limited generalizability. Furthermore, with
only 15 observations, the ratio of estimated parameters to observations was less than
one to five, which is below the threshold for contributing to our summary of available
evidence (as explained in Chapter Two).
Conclusions
One study (Webster, Vernick, and Hepburn, 2002) estimated how a ban on low-quality handguns in a single state influenced per capita handgun sales, but we discount this study’s evidence for methodological reasons. Therefore, we find inconclusive evidence for how bans on low-quality handguns affect firearm purchases.

Outcomes Without Studies Examining the Effects of Bans on Low-Quality Handguns

We did not identify any studies that met our inclusion criteria and examined the relationship between bans on low-quality handguns and the following outcomes:

- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Fifteen References


Part D of this report presents research syntheses on the included policies that regulate how legal firearm owners may use, store, or carry their firearms. While these policies might indirectly influence the decision to obtain a firearm (e.g., through effects on the perceived benefits of firearm ownership), they are intended to regulate the behaviors of existing legal gun owners. These policies encompass regulations that dictate the situations in which the use of firearms is deemed legal, how firearms must be stored when not in use, and where firearms can be legally carried and by whom.

Overall, this family of policies directly affects where and in what situations individuals who own guns can use and carry their firearms. Of the 18 policy classes included in this review, five operate by affecting the behaviors of gun owners. Each is reviewed in the subsequent chapters in Part D, as follows:

- stand-your-ground laws (Chapter Sixteen)
- child-access prevention laws (Chapter Seventeen)
- concealed-carry laws (Chapter Eighteen)
- gun-free zones (Chapter Nineteen)
- laws allowing armed staff in kindergarten through grade 12 (K–12) schools (Chapter Twenty).
Self-defense has long been available as a criminal defense for fatal and nonfatal confrontations. Traditionally, this defense imposes a duty to retreat before using force, if safe retreat is available. Stand-your-ground laws—referred to by some as shoot-first laws—remove this duty to retreat in some cases of self-defense. By removing that rule, stand-your-ground laws are intended to reduce barriers for self-defense with the aim of further deterring criminal victimization. Given the availability of self-defense laws for situations in which safe retreat is not possible, stand-your-ground laws primarily apply when an individual could safely retreat from an attack or when the availability of safe retreat is ambiguous.

By reducing the threshold for the justified use of lethal force for self-protection, stand-your-ground laws should increase defensive gun use and, if a deterrent effect exists, may reduce rates of crime and violence. Specifically, stand-your-ground laws reduce the expected legal costs of defensive gun use by reducing the probability of incurring criminal or civil liability for inflicting fatal or nonfatal injury. The laws, in turn, increase the expected costs of violent criminal behavior, as victims are more likely to respond using deadly force. This mechanism could serve to lower crime rates or could induce criminals to shift to other types of crime in which they are less likely to encounter armed resistance. In that case, crime rates could remain stable while the composition of crime types (e.g., robbery versus larceny) shifts.

Alternatively, by lowering the legal risks of using deadly force, these laws could encourage the escalation of aggressive encounters, resulting in an overall increase in firearm homicides or injuries. Furthermore, the greater likelihood of facing a citizen willing to use a firearm defensively under these policies could induce criminals to carry firearms more often and thus increase the share of violent or property crimes involving firearms.

To disentangle these mechanisms, the ideal analyses would distinguish between the effects of stand-your-ground laws on criminal violence and the effects on violence committed in self-defense. Data on homicides, violent crime, and property crime are readily available. Methodological weaknesses in collecting data on defensive gun use are well documented, but several data sources do exist (Kleck, 2018; Hemenway and Solnick, 2015b; National Research Council, 2004; see also RAND Corporation, 2018, Chapter Twenty-Three). Ideally, analyses of the effects of stand-your-ground laws on defensive gun use would use data that capture whether the laws affected self-defense
rates in the home (where castle-doctrine law already relieves victims of a duty to retreat) or in other areas as allowed under expanded stand-your-ground laws. This level of detail on the circumstances surrounding defensive gun use is not available from most existing data sources. The National Violent Death Reporting System does include detailed circumstance information for firearm deaths in participating states, but none of the studies meeting our inclusion criteria used this data source. Two studies we identified that met our inclusion criteria for this policy separately examined total homicides (as well as other crime types) and justifiable homicides using statistics collected through the Federal Bureau of Investigation (FBI)’s Uniform Crime Reporting Program—although, as the authors point out, the program’s definition of justifiable homicide does not capture certain incidents that would explicitly count as defensive gun use under expanded stand-your-ground laws (McClellan and Tekin, 2017; Cheng and Hoekstra, 2013).

There is likely to be little effect of stand-your-ground laws on hunting or recreational gun use. However, should these policies encourage more individuals to obtain or carry firearms, we might expect increased gun sales, unintentional injuries and deaths, and suicides following passage of the law. To assess this possibility, one would ideally like to know whether there are greater increases in gun ownership and carrying following passage of stand-your-ground laws compared with other states, but data on gun ownership have not been collected systematically over time. Only two of the studies we identified that met our criteria evaluated the effects of stand-your-ground laws on these outcomes: Both Humphreys, Gasparrini, and Wiebe (2017) and Guettabi and Munasib (2018) estimated the impact of such laws on suicide rates as placebo tests (i.e., on the theory that stand-your-ground laws should have no effect on suicides).

State Implementation of Stand-Your-Ground Laws

As of January 1, 2020, 34 states have stand-your-ground laws or have expanded castle doctrine to apply beyond the home. Although Utah passed the first stand-your-ground law in 1994, widespread legislative movement in this area did not begin until 2005, when Florida adopted a stand-your-ground law that became the basis for a model law adopted by the American Legislative Exchange Council. In the ensuing decade, an additional 24 states passed similar laws. Eight states have expanded castle doctrine to

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1 Standard castle doctrine states that a person in his or her own home does not have a duty to retreat prior to using force, including deadly force, in self-defense.


motor vehicles or the workplace, and we include these in our discussion here. Other sources (e.g., Everytown for Gun Safety Support Fund, 2013) adopt a more restrictive definition and therefore count fewer states with stand-your-ground laws.

Utah’s law states, “A person does not have a duty to retreat from the force or threatened force described in Subsection (1) in a place where that person has lawfully entered or remained, except as provided in Subsection (2)(a)(iii).” Subsection 1 says, in part, that force that is likely to cause death or serious injury is justified to “prevent death or serious bodily injury . . . as a result of another person’s imminent use of unlawful force, or to prevent the commission of a forcible felony.” The exception in (2)(a)(iii) applies to a situation in which the individual in question was the aggressor or was “engaged in combat by agreement,” unless the person has withdrawn from the combat or expressed an intention to do so.

Florida’s stand-your-ground law is similar to Utah’s. It says that a “person who is attacked in his or her dwelling, residence, or vehicle has no duty to retreat and has the right to stand his or her ground and use or threaten to use force, including deadly force, if he or she uses or threatens to use force in accordance with Sections 776.012(1) or (2) or sections 776.013(1) or (2).” Sections 776.012(2) and 776.013(2) both provide that deadly force is justified when “necessary to prevent imminent death or great bodily harm to [oneself] or another or to prevent the imminent commission of a forcible felony.”

States that followed Florida generally modeled their laws on those of Florida and Utah, sometimes with distinct features. A few other laws strayed further from the Florida and Utah statutes; for instance, Mississippi’s law uses the term felony rather than the narrower forcible felony. Other states do not include the language that there


5 Utah Code Ann. § 76-2-402.


9 Miss. Ann. Code § 97-3-15. Other examples include Iowa, Ohio, and West Virginia. See Ia. Code Ann. § 704.1, which states that deadly force may be used even if there is an alternative, if the alternative requires one to retreat from one’s dwelling or workplace. Ohio Rev. Code Ann. § 2901.09 applies to every section in the code that sets forth a criminal offense. W. Va. Ann. Code § 55-7-22 strays from Florida’s and Utah’s laws in the section dealing with civil actions, discussing lawsuits brought by intruders or attackers for injuries sustained.
is no duty to retreat to prevent the commission of a forcible felony, but they do allow individuals to use deadly force to prevent specific, named felonies. In most states, this is quite broad, either listing many types of felonies or describing a class of felonies. In some states, the list of felonies is quite limited. Finally, four states limit their laws to defense of self and others in the face of death or serious physical injury, thereby implicitly excluding any other felonies.

West Virginia, which discusses stand-your-ground laws only in the context of civil actions, does not require an individual to retreat if facing risk of death, serious bodily harm, or commission of a felony in his or her own home. However, the law requires the risk of death or serious bodily harm for the stand-your-ground provisions to apply when outside the home. In North Dakota, the stand-your-ground law applies in an individual’s home, workplace, or occupied motor home or travel trailer, unless the individual “is assailed by another individual who the individual knows also dwells or works there or who is lawfully in the motor home or travel trailer.” Ohio’s statute applies only in the person’s home, vehicle, or vehicle owned by an immediate family

10 For example, Ala. Code § 13-A-3-23 (kidnapping; assault; burglary; robbery; forcible rape; forcible sodomy; “using or about to use physical force against an owner, employee, or other person authorized to be on business property when the business is closed to the public while committing or attempting to commit a crime involving death, serious physical injury, robbery, kidnapping, rape, sodomy, or a crime of a sexual nature involving a child under the age of 12”; or a crime against someone who is “in the process of unlawfully and forcefully entering, or has unlawfully and forcefully entered, a dwelling, residence, business property, or occupied vehicle, or federally licensed nuclear power facility, or is in the process of sabotaging or attempting to sabotage a federally licensed nuclear power facility, or is attempting to remove, or has forcefully removed, a person against his or her will from any dwelling, residence, business property, or occupied vehicle when the person has a legal right to be there, and provided that the person using the deadly physical force knows or has reason to believe that an unlawful and forcible entry or unlawful and forcible act is occurring”); Alaska Stat. Ann. § 11.81.335 (in addition to death and serious physical injury, lists kidnapping, sexual assault, sexual abuse of a minor, and robbery); Ky. Rev. Stat. Ann. §§ 503.050, 503.055 (503.050 states that an individual may stand his or her ground when at risk of kidnapping or sexual intercourse compelled by force or threat of force, in addition to death, great bodily harm, or felony by force, while 503.055 states that individuals may stand their ground when they or other individuals face only death, great bodily harm, or felony by force); Mo. Stat. Ann. § 563.031 (adds defense of unborn child); and Nev. Rev. Stat. Ann. § 200.120 (“necessary self-defense, or in defense of an occupied habitation, an occupied motor vehicle or a person, against one who manifestly intends or endeavors to commit a crime of violence, or against any person or persons who manifestly intend and endeavor, in a violent, riotous, tumultuous or surreptitious manner, to enter the occupied habitation or occupied motor vehicle, of another for the purpose of assaulting or offering personal violence to any person dwelling or being therein”). See also La. Stat. Ann. § 14:20; N.H. Rev. Stat. Ann. §§ 627:4, 627:7; N.D. Ann. Code § 12.1-05-07; Ohio Rev. Code Ann. § 2901.09; S.C. Ann. Code §§ 16-11-440, 16-1-60; S.D. Laws §§ 22-18-4, 22-18-34, 22-18-35; Tex. Penal Code § 9.32.

11 Mich. Comp. Laws § 780.972 (sexual assault); N.C. Gen. Stat. Ann. §§ 14-51.3, 51.2 (forcibly entering home, motor vehicle, or workplace or attempting to remove someone from their home, motor vehicle, or workplace); 18 Pa. Cons. Stat. § 505 (kidnapping or sexual intercourse by force or threat).


member.\textsuperscript{15} In Wisconsin, the law applies in an individual’s home, motor vehicle, or place of business.\textsuperscript{16} In Iowa and Connecticut, it applies in the home or workplace.\textsuperscript{17}

Some states exclude specific situations from applying under the stand-your-ground doctrine. In Louisiana, it “shall not apply when the person committing the homicide is engaged, at the time of the homicide, in the acquisition of, the distribution of, or possession of, with intent to distribute a controlled dangerous substance in violation of the provisions of the Uniform Controlled Dangerous Substances Law.”\textsuperscript{18} Other policies are broader, excluding any situation where the individual is “actively engaged in conduct in furtherance of criminal activity.”\textsuperscript{19}

**Effects on Suicide**

**Research Synthesis Findings**

We identified two studies that met our criteria (Humphreys, Gasparrini, and Wiebe, 2017; Guettabi and Munasib, 2018), although their analyses of the impact of stand-your-ground laws on suicide rates were used as placebo tests (i.e., on the theory that stand-your-ground laws should have no effect on suicides) to support the authors’ primary findings of an effect of the laws on homicide rates.\textsuperscript{20}

Humphreys, Gasparrini, and Wiebe (2017) examined changes between 1999 and 2014 in Florida’s monthly total and firearm suicide rates before and after the introduction of Florida’s 2005 stand-your-ground law compared with changes over time in these rates in four of the 27 states without stand-your-ground laws at the beginning of the period (New Jersey, New York, Ohio, and Virginia). The paper reported that these were the only states with consistent monthly homicide data. It did not indicate whether suicide data were available on a wider set of control states. The authors found uncertain evidence of an effect of the stand-your-ground law on either total or firearm suicides in Florida; they did find a suggestive reduction in control states’ firearm suicide rates after Florida’s stand-your-ground law was passed, but they found no evidence that this effect was different from the uncertain change in Florida. Their model included no covariates to adjust for other sources of differences between Florida and control states in suicide rates over time, potentially obscuring the effects of the stand-your-ground law in Florida.

\textsuperscript{15} Ohio Rev. Code Ann. § 2901.09.
\textsuperscript{20} We identified one additional study that examined the effects of castle-doctrine legislation on the proportion of total suicides that used a firearm, as a proxy for firearm ownership (Wallace, 2014). However, without simultaneously examining firearm or total suicide rates, this outcome is difficult to interpret as providing a causal effect of stand-your-ground laws, so the study did not meet our inclusion criteria.
Using data spanning 1991 to 2011, Guettabi and Munasib (2018) employed the synthetic control method approach to evaluate how firearm suicide rates deviated from their counterfactual trends after states passed stand-your-ground laws. Because their analysis of suicide rates was intended as a falsification test, the authors examined the effect of stand-your-ground laws on firearm suicide rates only among the three states for which they found a significant effect of the law on firearm homicide rates (Alabama, Florida, and Michigan). Across the state-by-state analyses, the authors found an uncertain relationship between stand-your-ground law implementation and firearm suicide rates. It is challenging to draw conclusions about the effects of stand-your-ground policies on suicide rates from this study, given that suicide results were not presented for 11 states that passed stand-your-ground legislation during the study period.

Figure 16.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the stand-your-ground policies examined in Humphreys, Gasparrini, and Wiebe (2017). We do not include estimates from Guettabi and Munasib (2018) because the paper did not provide the detail necessary to estimate overall effects of stand-your-ground laws on suicide.

![Figure 16.1](image)

**Figure 16.1**
Incidence Rate Ratios Associated with the Effect of Stand-Your-Ground Laws on Suicide

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Humphreys, Gasparrini, &amp; Wiebe (2017)</td>
<td>Total</td>
<td>0.99 [0.59, 1.67]</td>
</tr>
<tr>
<td>Humphreys, Gasparrini, &amp; Wiebe (2017)</td>
<td>Firearm</td>
<td>1.03 [0.93, 1.14]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

**Conclusions**

We identified one qualifying study that estimated the effects of stand-your-ground laws on total suicides and two that estimated the effects on firearm suicides. The estimates for these effects in Humphreys, Gasparrini, and Wiebe (2017) and in Guettabi and Munasib (2018) suggest that such laws have an uncertain effect on both total suicides and firearm suicides. Therefore, available studies provide inconclusive evidence for the effect of stand-your-ground laws on total suicides and firearm suicides.
Effects on Violent Crime

Research Synthesis Findings

We identified seven studies that met our criteria and examined the effects of stand-your-ground laws on violent crime.\textsuperscript{21} Cheng and Hoekstra (2013) exploited state and time variation in the passage of stand-your-ground laws using data from 2000 to 2010 to estimate the laws’ effects on homicide rates. The authors defined stand-your-ground laws using a binary variable equal to one for policies that “remove the duty to retreat in some place outside the home” (Cheng and Hoekstra, 2013, p. 825). Controlling for state and year fixed effects, the authors explored several model specifications, including additional controls for region-by-year fixed effects, time-varying covariates that account for changes in policing and incarceration rates, and state-specific linear trends. Using negative binomial regression models, they found stand-your-ground laws to be associated with significant increases in homicide rates of 6 to 11 percent, a result that is relatively robust across model specifications. However, given the relatively short time frame studied and the large set of controls, the ratio of estimated parameters to observations was less than one to six in specifications that included time-varying covariates, indicating that the model may have been overfit, and thus its estimates and their CIs may be unreliable indicators of the true effects of the laws.

Covering a similar period (1999–2010) with state-level data, Webster, Crifasi, and Vernick (2014) analyzed the effects of stand-your-ground laws on age-adjusted homicide rates. Using generalized least-squares regression models, their estimates showed an uncertain association between stand-your-ground laws and homicide rates, firearm homicide rates, and nonfirearm homicide rates. The statistical model used to arrive at these results used a large number of estimated parameters relative to observations (a ratio of about one to eight), meaning the model may have been overfit, and thus its estimates of the laws’ effects may not generalize to other implementations of a stand-your-ground law.

McClellan and Tekin (2017) also examined the association between stand-your-ground laws and firearm homicide rates, using the same data source as Webster, Crifasi, and Vernick (2014) over a similar period (2000 to 2010) but specifying a log-linear model and examining monthly outcomes. In the authors’ primary specification that controlled for state fixed effects; month-by-year fixed effects; state-specific linear trends; and state-level time-varying socioeconomic, demographic, political, and criminal justice factors, the models showed that stand-your-ground laws removing the duty to retreat in all public venues were associated with a suggestive 8-percent increase in

\textsuperscript{21} An additional study (Lott, 2010) examined how violent crime is affected by castle-doctrine laws, which remove the requirement that people \textit{in their own homes} retreat prior to defending themselves from deadly force. The author found a significant negative relationship between the laws and murder rates, but because castle doctrine is conceptually distinct from stand-your-ground laws—which, in essence, expand castle doctrine to apply outside the home—we do not count this study as part of the evidence base for the effects of stand-your-ground laws.
firearm homicides (see Figure 16.2). Subgroup analyses seemed to indicate that these effects were driven largely by firearm homicides among white men. These findings remained robust across a variety of specifications (e.g., using annual data, specifying a linear relationship between homicide rates and the laws, Poisson models), showing a positive relationship between the law and firearm homicide rates but with varying precision of the estimates across models.

Using data from a subset of states, McClellan and Tekin (2017) also assessed how stand-your-ground laws influenced hospitalizations and emergency department visits related to firearm injury inflicted purposefully by a civilian (i.e., the authors excluded injuries with medical classification codes that indicated unintentional injury or injury inflicted by police officers). Results showed an uncertain relationship between stand-your-ground laws and firearm-related injuries (see Figure 16.2). However, the data source for the injury outcomes provided information for a limited set of years and states; given the number of controls included, the ratio of parameters to observations in the injury regressions was approximately one to six, meaning the model may have been overfit and was likely underpowered to detect any true effect of the policy.

Humphreys, Gasparrini, and Wiebe (2017) used segmented quasi-Poisson regression analysis to examine changes between 1999 and 2014 in Florida’s monthly homicide rate before and after the introduction of Florida’s 2005 stand-your-ground law. They compared these changes in four of the 27 states without stand-your-ground laws at the beginning of the period (New Jersey, New York, Ohio, and Virginia). The paper reported that these were the only states with reliable monthly homicide data. The authors found that the stand-your-ground law increased both total homicides and firearm homicides. Their estimates show that Florida experienced a statistically significant 24-percent increase in total homicides and 32-percent increase in firearm homicides following enactment of the stand-your-ground law in 2005 (see Figure 16.2). The comparison states experienced a statistically insignificant 6-percent increase in total homicides and 8-percent increase in firearm homicides after 2005. The authors’ model included no covariates to adjust for other sources of differences between Florida and control states in homicide rates over time, meaning that factors other than the stand-your-ground law cannot be ruled out as the cause of the observed differences between Florida and the control states.

Munasib, Kostandini, and Jordan (2018) estimated the effects of stand-your-ground laws on per capita rates of total firearm-related deaths, excluding deaths by suicide. Based on mortality data from 1999 to 2013, and using a state and year fixed-effects model with controls for state-level socioeconomic factors, racial distribution, and the proportion of suicides related to firearms (a proxy measure for firearm ownership), negative binomial regression models showed an uncertain relationship between stand-

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22 We classify this outcome under violent crime because more than 90 percent of firearm deaths that are not suicides are attributed to homicide (Centers for Disease Control and Prevention, undated-a).
your-ground policies and the study’s outcome of interest (see Figure 16.2). In models stratified by county urban-rural designation (central city, suburban, smaller urban area, rural), the authors observed some heterogeneity in effect sizes. When restricting the models to counties designated as central cities, the estimated effects were positive and statistically significant; effects were positive and suggestive for suburban and rural areas but were negative and uncertain for small urban areas. However, because not every urbanization designation exists in every state (e.g., South Dakota has no county that is designated as a central city), these stratified models were identifying effects from a different set of state laws and using different comparison groups, making it challenging to understand what was driving heterogeneous effects by urban-rural status.

Crifasi et al. (2018b) also sought to understand the role of firearm policies in shaping firearm violence while considering differential effects in urban versus rural areas. The authors restricted their analyses to urban counties (large central metro and fringe metro) and estimated Poisson regression models to understand how several state gun policies related to firearm and nonfirearm homicide rates. They controlled for secular trends, random effects for counties, time-varying county-level socioeconomic and demographic factors, and state-level incarceration rates and law enforcement expenditures. Using county-level data from 1984 to 2015, the authors found that stand-your-ground laws were associated with significantly higher rates of firearm homicide (IRR = 1.07; 95-percent CI = 1.05, 1.10), whereas the laws had an uncertain relationship with nonfirearm homicide rates. These estimates support that stand-your-ground laws increase firearm homicide rates, although the failure to adjust standard errors to account for serial correlation may have resulted in artificially inflated significance.

Finally, Guettabi and Munasib (2018) used a synthetic control method approach to estimate how stand-your-ground laws affect firearm-related homicides and unintentional deaths combined. For each of the 14 states that passed a stand-your-ground law between 2005 and 2007, the authors used the synthetic control method approach to generate a weighted combination of control states (i.e., a “synthetic control”) that approximated the pre-law firearm homicide and unintentional gun death trend of the stand-your-ground state. Because the authors did not aggregate across the single-state estimates, it is difficult to calculate an average treatment effect; however, the majority of states with stand-your-ground laws showed small and uncertain deviations from the counterfactual in the post-law period. Three of the 14 states (Alabama, Florida, and Michigan) had evidence of a significant positive association between stand-your-ground law enactment and gun-related homicide or unintentional deaths; the estimates for Michigan and Alabama became smaller in magnitude and more imprecise in sensitivity analyses, including analysis of murder and nonnegligent manslaughter. However, it is difficult to interpret these results in light of the 11 stand-your-ground

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23 We classify this outcome under violent crime because homicides constitute more than 90 percent of combined homicide and unintentional firearm-related deaths (Centers for Disease Control and Prevention, undated-a).
policies that exhibited uncertain effects and the absence of information about which features of the laws might differ in ways that meaningfully determine their impacts.

Figure 16.2 displays the IRRs and CIs associated with the stand-your-ground policies examined in the studies we identified. Because Guettabi and Munasib (2018) presented 14 state-specific effect sizes but no average effect of the law, we do not display their estimates here. And although McClellan and Tekin (2017) noted the marginal effects of stand-your-ground laws on total homicides and stated that those estimates were statistically significant, they did not present sufficient information for us to determine CIs around the estimates; thus, we exclude their findings on total homicides from Figure 16.2.

Figure 16.2
Incidence Rate Ratios Associated with the Effect of Stand-Your-Ground Laws on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Stand-your-ground law</td>
<td>Homicide rate</td>
<td>1.09 [1.03, 1.16]</td>
</tr>
<tr>
<td>Cheng &amp; Hoekstra (2013)</td>
<td>Total</td>
<td>1.02 [0.96, 1.07]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Total, age-adjusted</td>
<td>1.08 [1.00, 1.16]</td>
</tr>
<tr>
<td>McClellan &amp; Tekin (2017)</td>
<td>Firearm</td>
<td>1.00 [0.96, 1.04]</td>
</tr>
<tr>
<td>Munasib, Kostandini, &amp; Jordan (2018)</td>
<td>Firearm</td>
<td>1.07 [1.05, 1.10]</td>
</tr>
<tr>
<td>Crifasi et al. (2018b)</td>
<td>Firearm, urban areas</td>
<td>1.04 [0.96, 1.12]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Firearm, age-adjusted</td>
<td>1.00 [0.91, 1.10]</td>
</tr>
<tr>
<td>Webster, Crifasi, &amp; Vernick (2014)</td>
<td>Nonfirearm, age-adjusted</td>
<td>1.01 [0.97, 1.04]</td>
</tr>
<tr>
<td>Crifasi et al. (2018b)</td>
<td>Nonfirearm, urban areas</td>
<td>1.03 [1.00, 1.07]</td>
</tr>
<tr>
<td>Cheng &amp; Hoekstra (2013)</td>
<td>Robbery</td>
<td>1.04 [0.97, 1.10]</td>
</tr>
<tr>
<td>McClellan &amp; Tekin (2017)</td>
<td>Firearm assault</td>
<td>1.19 [0.55, 2.58]</td>
</tr>
</tbody>
</table>

NOTE: The Munasib, Kostandini, and Jordan (2018) outcome labeled “firearm” was calculated as total gun deaths minus firearm suicide deaths, so it primarily captured firearm homicides. IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
Conclusions

*Homicides and other violent crime.* We identified five qualifying studies that estimated the effects of stand-your-ground laws on total homicides or other violent crimes. Cheng and Hoekstra (2013) found that these laws significantly increase homicide rates, but they have uncertain effects on robbery, aggravated assault, and burglary rates. McClellan and Tekin (2017) also found significant increases in total homicides associated with the implementation of stand-your-ground laws. In contrast, Webster, Crifasi, and Vernick (2014) found that these laws have an uncertain effect on the total homicide rate. Finally, Humphreys, Gasparrini, and Wiebe (2017) and Guettabi and Munasib (2018) found significant effects consistent with the law increasing total homicides in Florida after its passage. These studies draw on two distinct data sources: FBI crime-rate data from the Uniform Crime Reporting Program and the Centers for Disease Control and Prevention’s Fatal Injury Reports.

Considering these findings, we conclude that there is moderate evidence that stand-your-ground laws may increase total homicide rates but inconclusive evidence for the effect of stand-your-ground laws on other types of violent crime.

*Firearm homicides.* We identified six qualifying studies that estimated the effects of stand-your-ground laws on firearm homicide rates. Two studies found that these laws have uncertain effects on firearm homicides at the state level (Munasib, Kostandini, and Jordan, 2018; Webster, Crifasi, and Vernick, 2014). Humphreys, Gasparrini, and Wiebe (2017) and Guettabi and Munasib (2018) found a significant effect suggesting that, after the law’s introduction, firearm homicides increased in Florida. Two other studies found that stand-your-ground laws were associated with significant increases in firearm homicide rates (Crifasi et al., 2018b; McClellan and Tekin, 2017). Considering these findings, we conclude that there is supportive evidence that stand-your-ground laws may increase firearm homicides.
Effects on Mass Shootings

Research Synthesis Findings
We identified one qualifying study that examined whether stand-your-ground laws are associated with the incidence of mass shootings. Using data from 1982 to 2013, Blau, Gorry, and Wade (2016) used a linear probability model to estimate how a variety of gun laws, including stand-your-ground laws, relate to the probability of a public shooting incident occurring in a given state-year. In their full model, which controls for state fixed effects, aggregate state-level income, state population, and a linear national time trend, the authors found that stand-your-ground laws were associated with a statistically significant decline in the likelihood of a public shooting incident. However, their use of a linear model to predict a dichotomous (and rare) outcome likely violated model assumptions and rendered the results unreliable. The authors’ estimated linear probability model can yield predicted probabilities of active shooting incidence that extend far outside the definitional 0 to 1 range of a probability, depending on the particular combination of policies present in a given state. Moreover, the estimated model implies negative IRRs, which represent implausible effect sizes, for some of the gun policies that we are studying. This indicates a serious model misspecification (Cox and Snell, 1989; Aldrich and Nelson, 1984) and prevents us from interpreting the estimated coefficients as causal effect estimates.

Conclusions
One study estimated the effects of stand-your-ground laws on the incidence of mass shootings. Blau, Gorry, and Wade (2016) found a statistically significant negative relationship between stand-your-ground laws and the study’s outcome for mass shootings. Because of the methodological concerns with this single study, we find inconclusive evidence for the effect of stand-your-ground laws on mass shootings.

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24 For example, the model coefficient for stand-your-ground laws implies an IRR of –2, with a 95-percent CI that lies entirely in the negative IRR range.
Effects on Defensive Gun Use

Research Synthesis Findings
We identified two studies that met our inclusion criteria and examined the effects of stand-your-ground laws on defensive gun use.

Cheng and Hoekstra (2013) exploited state-time variation in the passage of stand-your-ground laws using data from 2000 to 2010 to estimate such laws’ effects on justifiable homicides committed by private citizens. The authors defined stand-your-ground laws using a binary variable equal to one for polices that “remove the duty to retreat in some place outside the home” (Cheng and Hoekstra, 2013, p. 825), and data on justifiable homicides were collected from the FBI’s supplementary homicide data. Under the FBI’s classification in this data set, for a homicide to be considered justifiable, the incident must have occurred in conjunction with other offenses (e.g., the fatal shooting of an armed robber by a storeowner during the commission of the robbery), and those other offenses must have been reported. As noted by the authors, justifiable homicides are likely severely underreported in this data source. Controlling for state and year fixed effects, the authors explored several model specifications, including additional controls for region-by-year fixed effects, time-varying covariates that account for changes in policing and incarceration rates, and contemporaneous crime rates. Using negative binomial regression models, they found stand-your-ground laws to be associated with increases in justifiable homicide, ranging from an uncertain 28-percent rise to a significant 57-percent rise, depending on the model specification. However, given the relatively short time frame studied and the large set of controls, the ratio of estimated parameters to observations was less than one to six in specifications that included time-varying covariates, indicating that the model may have been overfit, and thus it may yield estimates that are unreliable indicators of the true causal effect of stand-your-ground laws.

McClellan and Tekin (2017) conducted an analysis very similar to that of Cheng and Hoekstra (2013), using the same data source for information on justifiable homicides by private citizens and largely the same time frame (2000 to 2009). The authors made slightly different choices regarding model specification (e.g., state-specific linear trends with no region-by-year effects), and they defined stand-your-ground laws as those that extend the no-duty-to-retreat provision to all places where an individual had a legal right to be. The authors’ findings were broadly similar to those of Cheng and Hoekstra (2013): They found that stand-your-ground laws were associated with increases in justifiable homicide rates, ranging from an uncertain 22-percent increase to a suggestive 46-percent increase, depending on model specification. Subgroup analyses indicated that these effects were most pronounced for justifiable homicides of white men. However, given the short time frame of the study and the large set of controls, the ratio of estimated parameters to observations was less than one to five. Thus, per the evaluation rubric described in Chapter Two, we discount the results of this paper.
Figure 16.3 displays the IRRs and CIs associated with the stand-your-ground policies examined in Cheng and Hoekstra (2013).

**Figure 16.3**

*Incidence Rate Ratios Associated with the Effect of Stand-Your-Ground Laws on Defensive Gun Use*

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Stand-your-ground law</td>
<td>Justifiable homicide</td>
<td>1.33 [0.84, 2.10]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

**Conclusions**

We identified two studies that estimated the effects of stand-your-ground laws on justifiable homicides, which is an imperfect measure of the rate of defensive gun use. In their specification that accounts for how justifiable homicides are counted and controls for time-varying state characteristics, Cheng and Hoekstra (2013) found that the effect of the law on this outcome is uncertain, while the results of McClellan and Tekin (2017) are discounted for methodological reasons. Given the methodological quality of these studies, we consider these findings inconclusive evidence for the effect of stand-your-ground laws on defensive gun use.

**Effects on the Gun Industry**

**Research Synthesis Findings**

One study (Wallace, 2014) met our inclusion criteria and assessed the effects of stand-your-ground laws on two proxy measures for firearm demand: the number of National Instant Criminal Background Check System (NICS) background checks conducted and the proportion of suicide deaths that involved a firearm, a measure frequently treated as a proxy for gun ownership rates at the state level. The author’s models produced contradictory evidence, showing a significant positive effect of stand-your-ground laws on the number of NICS background checks but a significant negative
effect of the laws on the ratio of firearm suicide to total suicide deaths. Because the models did not control for national trends (or state effects in the case of NICS background checks), the estimates may simply reflect that NICS background checks were increasing and the proportion of suicide deaths involving a firearm were decreasing over the study time frame of 2000 to 2010, when states were implementing stand-your-ground laws. Thus, the methods used in this study have substantial limitations for drawing causal inferences about the effects of stand-your-ground policies.

Conclusions
One study estimated the effects of stand-your-ground laws on the number of NICS background checks conducted and the proportion of suicide deaths that involved a firearm, measures intended to provide insights into how the policies affected gun acquisition and ownership (Wallace, 2014). The author found that stand-your-ground laws were associated with significant increases in the number of NICS background checks and significant declines in the proportion of suicides involving a firearm, but the study’s models had limited ability to provide causal insights. Given the methodological quality of this single study, we find inconclusive evidence for the effect of stand-your-ground laws on firearm ownership and purchases.

Outcomes Without Studies Examining the Effects of Stand-Your-Ground Laws
We did not identify any research that met our inclusion criteria and examined the effects of stand-your-ground laws on the following outcomes:

- unintentional injuries and deaths
- officer-involved shootings
- hunting and recreation.
Chapter Sixteen References


Child-access prevention (CAP) laws allow prosecutors to bring charges against adults who intentionally or carelessly allow children to have unsupervised access to firearms. CAP laws aim to reduce unintentional firearm injuries and deaths, suicides, and violent crime among youth chiefly by reducing children’s access to stored guns, although weaker laws targeting only reckless provision of firearms to children are sometimes considered alongside CAP laws.

In 2017, 1,814 children under age 18 were killed by firearms, and of these deaths, 729 (40.2 percent) were classified as suicide and 95 (5.2 percent) were classified as unintentional (calculated using data from Centers for Disease Control and Prevention [CDC], 2019a). Nonfatal gun injuries are considerably more common among this age group. According to the Nationwide Emergency Department Survey database, the largest and most comprehensive publicly available emergency department database, emergency departments treated 7,745 firearm-related injuries among children aged 1–17 in 2016.¹ Research suggests that, despite the risks, many children have access to firearms. In 2015, an estimated 7 percent of U.S. children (4.6 million) lived in homes in which at least one firearm was stored loaded and unlocked, twice as high as estimates reported in 2002 (Azrael et al., 2018).

Firearms are used in approximately 40 percent of suicides among children (calculated using data from CDC, 2019a). Using the National Fatality Review Case Reporting System, Schnitzer et al. (2019) reported that, in more than three-fourths of these suicides for which storage practices could be identified, the gun used by the child had been stored loaded and unlocked. Storage rates were no higher, and some analyses suggested that they were actually lower, when the child had elevated risk of suicide based on his or her prior talk, threats, or attempted suicide.

Youth and young adults are also disproportionately involved in crimes. In 2014, juvenile offenders were known to have been involved in approximately 650 murders

¹ This estimate was calculated from HCUPnet: Healthcare Cost and Utilization Project, undated. The other widely used source of nonfatal injury data—which is produced by the CDC and has been shown to have issues of precision and accuracy because of the small number of sampled hospitals (Campbell, Nass, and Nguyen, 2018; Cook et al., 2017)—estimated 7,854 nonfatal firearm injuries for children under age 18 in 2017 (CDC, 2019b).
nationwide, two-thirds of which involved a firearm (Office of Juvenile Justice and Delinquency Prevention, 2016). Young adults between ages 18 and 21 have among the highest rates of violent offending of any age group (Loeber and Stallings, 2011). Surveys have found that, among juveniles who have been incarcerated or arrested, the youth offenders acquired their firearms through similar sources as adult offenders, with more than 80 percent citing a friend, family member, or the black market as the source of the weapon (Webster et al., 2002; LaFree and Birbeck, 1998).

Conceptually, the effects of CAP laws may extend beyond those age groups that are directly targeted by the policies. In households where owners abide by CAP laws, because either underage children reside in the household or there are underage visitors, gun locks or gun safes could also serve to restrict access to guns by older members of the household. This limited availability could, in turn, influence suicides, unintentional injuries and deaths, and violent crime among the adult population.

Studies of adolescent and adult suicides have generally found that, relative to comparison groups of individuals who died other ways or living community members, those who died by firearm suicide lived in homes where guns were less securely stored (Conwell et al., 2002; Shenassa et al., 2004; Grossman et al., 2005). These studies suggest to one set of researchers a “dose-response” relationship between firearm accessibility and risk for suicide (Azrael and Miller, 2016). Indeed, Monuteaux, Azrael, and Miller (2019) used the findings from Grossman et al. (2005) to estimate that hundreds of children’s lives could be saved each year if an additional 20 percent of gun owners with children stored their guns securely. In contrast, Dahlberg, Ikeda, and Kresnow (2004) found no association between storage practices and firearm suicide among adults (versus suicide by other means).

Studies have generally found no difference in storage practices between adults who have thought about or attempted suicide versus those who have not (Smith, Currier, and Drescher, 2015; Ilgen et al., 2008; Betz et al., 2016; Oslin et al., 2004). Similarly, storage practices do not differ between those who have suicide risk factors and those who do not (Simonetti, Azrael, and Miller, 2019). Similarly, Simonetti et al. (2017) found no difference in storage practices among gun owners who had an adolescent with a mental illness in the home compared with the practices in homes with adolescents who did not have a mental illness. These findings, along with the finding that those who die by firearm suicide typically live in homes with less-secure storage of firearms, could suggest that the difference between those who use a firearm to die by suicide and those who do not die by suicide is related to firearm storage differences (Azrael and Miller, 2016).

In the absence of strong causal models, however, alternative explanations remain plausible. If, for instance, those most determined to kill themselves leave a weapon unsecured so that it will be available for use when they are ready to die, it could be that suicide risk determines storage practices rather than that storage practices determine suicide risk. Further research is needed to better understand the relationship among mental health, suicide risk, firearm ownership, and firearm behaviors.
Since 2003, only one individual-level study provided information on the association between firearm storage practices and unintentional injuries. Grossman et al. (2005) found that cases of unintentional firearm-related injury or death were less likely to occur in households where guns were stored unloaded or locked or where guns and ammunition were stored separately.

CAP laws could decrease gun crime rates by making theft of firearms more difficult. The laws could increase rates of crime victimization and decrease opportunities for legal defensive gun use by delaying gun owners’ access to their firearms. Similarly, if firearms in the home deter such crimes as burglaries, safe storage requirements could reduce the firearms’ deterrent value.

Data on suicides and self-inflicted nonfatal injury stratified by age are readily available, so analyses can directly test whether effects of CAP laws on these outcomes are driven by the relevant age group affected by the policy. For outcomes of violent crime and non-self-inflicted injury, causal analyses could be improved with data that report the age of the shooter. However, as most data sources report only the age of the victim or are missing information on the shooter, few of the studies we identified that met our inclusion criteria for this policy used this type of data (Anderson and Sabia [2018] examined school shooting outcomes by age of the perpetrator). In estimating potential spillover effects for other age groups, one would ideally know whether different outcomes are observed after implementation of CAP laws among those households most directly affected by the laws (such as households with children under ages 18 or 21) and households less directly affected by the safe storage policies.

For any analysis, estimates of causal effects would be strengthened with data showing how CAP laws actually affected gun storage behaviors, but national longitudinal data on firearm storage patterns are limited. The researchers who have been able to leverage national data on firearm storage practices and child gun access have found that CAP laws are associated with higher rates of safe storage (Prickett, Martin-Storey, and Crosnoe, 2014) and lower rates of gun carrying among high school students (Anderson and Sabia, 2018). Others have questioned whether knowledge of CAP laws is sufficiently well understood by gun owners to account for the apparent effects of these laws. If a person is unaware of a law, it is difficult to associate her or his behavior with that law. It may be, however, that knowledge of the law is not the key driver of improvements in gun storage after a CAP law is passed. If, for instance, passage of the law changes discourse around safe storage among gun owners and their influencers, the law could affect more gun owners than just those who know about the law. Too little work has been done on the mechanisms by which CAP laws or other laws influence their measured outcomes to conclude that low public awareness of the laws proves that they cannot be effective.

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2 Exceptions include the Federal Bureau of Investigation’s Supplementary Homicide Reports, which contain age of victim and age of offender for murders when such information is known, and the National Violent Death Reporting System, which contains information for a subset of states on the age of the shooter for non-self-inflicted fatal injuries when such information is known.
Researchers investigating the effect of CAP laws also face barriers related to data availability and quality. Counts of firearm suicides, nonfatal firearm injuries, and unintentional deaths by firearm have been shown to suffer from a range of coding errors, especially for children (Rockett, Kapusta, and Coben, 2014; Barber and Hemenway, 2011; Barber et al., 2002). These errors do not necessarily undermine the validity of causal evaluations of CAP laws, but higher levels of measurement error may make it more difficult to identify the statistical effects of the laws on childhood deaths and injuries.

State Implementation of Child-Access Prevention Laws

As of January 1, 2020, 29 states and the District of Columbia have CAP laws. Fifteen states and the District of Columbia have implemented laws concerning negligent storage, across which there is some variation. The strictest laws impose criminal penalties for negligent storage regardless of whether a child accesses any guns. Massachusetts, for instance, imposes criminal liability if a gun is stored where a minor “may have access.” Four other jurisdictions hold owners liable when they know or reasonably should know that access is “likely.” Four additional states impose criminal liability for negligent storage only where a child gains access to a gun, regardless of whether he or she uses it. Some of these jurisdictions impose liability even when the gun is not loaded.

Seven states impose liability for negligent storage if children publicly carry or use improperly stored firearms, although three of these states hold adults liable only if children’s access results in death or serious injury.

Many of the states imposing criminal liability for negligent storage allow for exceptions or defenses. The most common is when the gun has been stored in a locked

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3 Mass. Gen. Laws Ch. 140, § 131L.
8 In some states, certain actions are not excluded from the definition of the law, while in other states, the actions are affirmative defenses.
container. Other exceptions or defenses include that the firearm had been rendered inoperable, the person was carrying the firearm or it was close enough to be easily retrieved, or there was a reasonable expectation that children would not be present where the gun was stored. In addition, some states consider it an exception or defense when children enter a storage area illegally or use the firearm for self-defense. Some states have added other defenses too, such as those that apply to children who have a legal right to use firearms for hunting.

Fourteen states impose criminal liability for intentional, knowing, or reckless provision of firearms to children. These laws are weaker than negligent storage laws. Recklessness requires that the actor was aware of the risks involved in their actions, while negligence only requires that they should have been aware (American Law Institute, 1985). Six states impose penalties under the weaker standard for all firearms, three for loaded firearms, and five for handguns only. Some of the laws require the

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15 For example, Maryland’s law does not apply if the child has a hunting or firearm certificate (Md. Code Ann., Crim. Law § 4-104). In Texas, it is a defense if the child “was supervised by a person older than 18 years of age and was for hunting, sporting, or other lawful purposes” or is “engaged in an agricultural enterprise” (Tex. Penal Code Ann. § 46.13). In New Hampshire, the law does not apply if the child has completed a firearm safety or hunter safety course (N.H. Rev. Stat. Ann. § 650-C:1).


weapon to be used by the minor in some way. Exceptions and defenses for reckless provision of firearms to children are similar to those for negligent storage, such as that the firearm was in a locked container or had been rendered inoperable. Other exceptions include that the individual carried the firearm or it was close enough to be easily retrieved, the defendant had no reasonable expectation that a child would have access to the premises, or the child accessed the firearm through unlawful entry. Use of the firearm in hunting, hunter safety, and other sporting events or in self-defense are also exceptions.

In addition to the main distinctions among the CAP laws already discussed, another difference is how they define minors. In the majority of states, it is an offense to provide a firearm to an individual under age 18. In Texas, the age is 17. In seven states, the age is 16, and in another four states, a minor is under age 14.

In eight states and the District of Columbia, the act of negligent storage of a firearm is a misdemeanor. In Massachusetts, negligent storage is a felony. In nine states, some additional factor—such as the firearm being used to commit an act of violence,

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19 Ga. Code Ann. § 16-11-101.1 (must be used in the commission of a felony offense); Tenn. Code Ann. § 39-17-1320 (for liability to attach to parents or guardians, the gun must be used in the commission of a felony).
it being a second offense, or the child having committed a prior felony—is required for the act to be a misdemeanor.\textsuperscript{32} In eight states, such factors make the negligent storage a felony. In four such jurisdictions, these factors bump the crime from a misdemeanor to a felony,\textsuperscript{35} and in the other four, there is no misdemeanor offense, only these felonies.\textsuperscript{34} Texas makes clear that regardless of what additional factors are included, the crime is always a misdemeanor offense. Among states with laws that prohibit recklessly or knowingly providing firearms to minors, Mississippi and Tennessee make it a misdemeanor, Missouri and Kentucky make it a felony, and Tennessee makes it a felony for parents to recklessly or knowingly provide firearms to their children.

**Effects on Suicide**

**Research Synthesis Findings**

We identified six quasi-experimental studies providing insight into the impact of CAP laws on suicide. The two earliest of these studies, which applied different statistical models to nearly identical data sets, found somewhat conflicting evidence among those under age 15 (Cummings et al., 1997a; Lott and Whitley, 2001). The model specified by Cummings et al. (1997a) found suggestive effects consistent with a reduction in firearm suicides in a model with limited controls. On the other hand, Lott and Whitley (2001) found uncertain effects in models that employed both state and year fixed effects, states’ “shall-issue” or “right-to-carry” laws (see Chapter Eighteen), “one-gun-a-month” purchase rules, states that border one-gun-a-month states, waiting periods, mandatory prison penalties for using guns in the commission of a crime, and more than 36 state-level demographic controls. Combined with the state fixed effects, year fixed effects, and law effects, this model had an unfavorable ratio of estimated parameters to observations (approximately one to eight), suggesting that the model may have been overfit, and thus the estimated effects of these laws and their statistical significance may be poor indicators of their true effects. In addition, the model did not adjust for clustered standard errors. Together, these shortcomings suggest that the model results may not accurately describe the true effects of CAP laws.

Webster et al. (2004) examined the effect of CAP laws on suicides among teens (aged 14–17) and young adults (aged 18–20) between 1976 and 2001. In negative binomial models that employed generalized estimating equations and that included state-level fixed effects and other covariates (e.g., the proportion of suicides by firearm as a proxy for gun prevalence), the authors found that CAP laws significantly lowered the total suicide rate among those aged 14–17 by 8.3 percent, driven by an estimated

\textsuperscript{32} Florida, Illinois, Iowa, New Hampshire, North Carolina, Oklahoma, Rhode Island, Utah, and Wisconsin.

\textsuperscript{33} California, Nevada, Utah, and the District of Columbia.

\textsuperscript{34} Colorado, Connecticut, Georgia, and Indiana.
10.8-percent reduction in firearm suicides; in addition, they found uncertain effects on nonfirearm suicides. In this age group, the post-policy firearm suicide rate was 89 percent of the rate expected without the policy, and the total suicide rate was 92 percent of what was expected (see Figure 17.1). There was also an indication that the effect was strongest the first year after the CAP law went into effect. These findings were sensitive to the authors’ choices about model specifications: An alternatively specified and worse-fitting model yielded uncertain effects. The authors also found that CAP laws were associated with a reduction in total, firearm, and nonfirearm suicides among those aged 18–20. Relative to what would have been expected without the law, suicide rates in this age group were reduced to 89 percent, 91 percent, and 87 percent for total, firearm, and nonfirearm suicides, respectively. The authors questioned the validity of the causal effect detected for those aged 18–20, suggesting that the significant nonfirearm suicide effects “cast doubt on any causal connection between the laws and lower suicides rates among this group of older youth.” They did not, however, suggest that this skepticism should extend to the effects found for the lower age group, although the difference between the reductions in nonfirearm suicide detected for the two age groups was not significantly different. Therefore, for this review, we interpret both models as providing some evidence that CAP laws reduce total and firearm suicide.

Gius (2015b) examined data from 1981 to 2010 and found that CAP laws were associated with a reduction in firearm suicides among those aged 19 or younger, but he did not examine total or nonfirearm suicides. The author controlled for a variety of state-level sociodemographic characteristics, along with two other laws related to youth firearm access (state minimum age requirements for handgun possession and the federal minimum age requirement for handgun possession enacted in 1994). The effect he reports suggests that the post-policy firearm suicide rate was 89 percent of the rate expected if there were no such laws in place, which matches the estimate by Webster et al. (2004) for those aged 14–17 and is close to their estimate of 92 percent for those aged 18–20.

An additional study (DeSimone, Markowitz, and Xu, 2013) found evidence of an effect of CAP laws on nonfatal self-inflicted gun injuries recorded in the Nationwide Inpatient Sample (NIS). Self-inflicted gun injuries are not all suicide attempts; some are unintentional injuries. Because case fatality rates for suicide attempts with a firearm are between 83 and 91 percent (Conner, Azrael, and Miller, 2019; Azrael and Miller, 2016; Spicer and Miller, 2000), some proportion of nonfatal self-inflicted firearm injuries are likely the result of suicide attempts. Therefore, DeSimone, Markowitz, and Xu (2013) should be understood to evaluate the effects of CAP laws on nonfatal firearm injuries resulting from a combination of suicide attempts and self-inflicted uninten-

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35 Specifically, the primary model (which had better model fit based on Akaike information criterion statistics, which conceptually provide a measure of the expected relative distance between the estimated model and the true data generating mechanism; see Burnham and Anderson, 2004) included adjusting for national suicide rate trends using two linear trend parameters; the alternative model included year fixed effects.
tional injuries. The authors looked at hospital discharges in 11 states between 1988 and 2003 and employed fixed effects for state and year in their statistical models (along with other state- and hospital-level covariates). They found that CAP laws based on negligent storage alone or on both negligent storage and reckless provision were associated with a reduction of 66–69 percent in self-inflicted firearm injuries among those under age 18, although estimates showed an uncertain effect on self-inflicted injuries for those 18 or older. These estimated effects were largely unchanged when considering whether the CAP laws were specified as those more-stringent policies that impose criminal penalties for negligent storage or were more broadly defined to include both negligent storage and reckless provision. This similarity in estimated effects is likely because only two states (Colorado and Wisconsin) in the NIS passed reckless provision laws during the study time frame; thus, identification in both specifications was driven largely by changes in state laws regarding negligent storage.

In addition, cases of firearm self-injury among young people were extremely sparse in the data used by DeSimone, Markowitz, and Xu (2013), with just more than 200 such injuries reported in more than 9,000 hospital observations. And models fit to sparse data are prone to statistical and interpretational problems. Specifically, models estimated on sparse data can yield effect sizes that are biased to be greater in magnitude (more extreme) than the true effect size, with underestimated standard errors that inflate the statistical significances of those effects (Greenland, Mansournia, and Altman, 2016). Given that the study fits a relatively complex model to a data set that contains only 200 of the injuries of interest, there could be concerns that the estimated effect sizes are inflated.

Finally, Anderson and Sabia (2018) examined the effects of CAP laws on school shootings that involved a suicide death between 1991 and 2013. The authors separately considered incidents in which the perpetrator of the school shooting was a minor and incidents in which the perpetrator was aged 18 or older. Using linear probability models weighted by state population and adjusting for a large set of state-level time-varying covariates, state fixed effects, national trends, and state-specific linear trends, the authors found that the estimated associations between CAP laws and the probability of a state experiencing a school shooting involving a suicide were uncertain for both age groups. The estimates were highly imprecise, with 95-percent confidence intervals (CIs) that spanned the full range of outcomes (see Figure 17.1), reflecting low power to detect the study’s relatively rare outcome. The models also included a large number of covariates, which led to a ratio of parameters to observations of about one to eight, meaning the model may have been overfit.

Figure 17.1 displays the incidence rate ratios (IRRs) and CIs associated with the CAP laws examined in these studies. Estimates from the Tobit models used by Lott and Whitley (2001) could not be converted into the IRR effect sizes used in Figure 17.1, so those estimates are omitted from this figure.
Figure 17.1
Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Suicide and Self-Injury

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State CAP law</strong></td>
<td><strong>Suicide rate</strong></td>
<td></td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Total, aged 14–17</td>
<td>0.92 [0.86, 0.98]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Total, aged 18–20</td>
<td>0.89 [0.85, 0.93]</td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Firearm, aged 0–14</td>
<td>0.81 [0.66, 1.01]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Firearm, aged 14–17</td>
<td>0.89 [0.83, 0.96]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Firearm, aged 18–20</td>
<td>0.87 [0.82, 0.92]</td>
</tr>
<tr>
<td>Gius (2015b)</td>
<td>Firearm, aged 0–19</td>
<td>0.89 [0.84, 0.94]</td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Nonfirearm, aged 0–14</td>
<td>0.95 [0.75, 1.20]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Nonfirearm, aged 14–17</td>
<td>1.00 [0.91, 1.10]</td>
</tr>
<tr>
<td>Webster et al. (2004)</td>
<td>Nonfirearm, aged 18–20</td>
<td>0.91 [0.85, 0.98]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anderson &amp; Sabia (2018)</td>
<td>Any suicide, aged 0–17</td>
<td>1.41 [0.01, 2.81]</td>
</tr>
<tr>
<td>Anderson &amp; Sabia (2018)</td>
<td>Any suicide, aged 18+</td>
<td>1.58 [0.25, 2.91]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State CAP law, negligent storage (11 states)</strong></td>
<td><strong>Self-inflicted injury rate</strong></td>
<td></td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm, aged 0–17</td>
<td>0.31 [0.16, 0.61]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm, aged 18+</td>
<td>1.00 [0.64, 1.56]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm, aged 0–17</td>
<td>0.35 [0.19, 0.62]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm, aged 18+</td>
<td>1.17 [0.75, 1.83]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.

Conclusions

Total suicides. We identified one qualifying study that estimated the effect of CAP laws on total suicides in two population groups, those aged 14–17 and those aged 18–20 (Webster et al., 2004). For both groups, significant effects were found consistent with CAP laws reducing total suicides.

We conclude that available research offers limited evidence that child-access prevention laws reduce total suicides among youth aged 14–20.
Firearm suicides and firearm self-injury. We identified six qualifying studies that estimated the effect of CAP laws on firearm suicide or firearm self-injury. Cummings et al. (1997a) identified a suggestive effect consistent with CAP laws reducing firearm suicides among children aged 14 or younger. Using a similar data series, Lott and Whitley (2001) identified uncertain effects of CAP laws on those younger than age 15 and among those aged 15–17. Using a longer but overlapping data series, Webster et al. (2004) found significant effects suggesting that CAP laws reduce firearm suicide among those aged 14–17 and those aged 18–20. Gius (2015b) used a later, though partially overlapping, data series and similarly found a significant effect indicating that CAP laws reduce firearm suicides among those aged 19 or younger. Using data on hospitalizations for self-inflicted firearm injuries, DeSimone, Markowitz, and Xu (2013) found significant effects suggesting that CAP laws reduce such injuries among those aged 17 or younger, but they found uncertain effects among adults aged 18 or older. Finally, focusing on the specific subset of suicides that occurred in the context of a school shooting, Anderson and Sabia (2018) found uncertain effects of CAP laws.

Considering these studies, our assessment of their relative strengths, and the fact that effects are found across multiple data sets, we conclude that there is supportive evidence that child-access prevention laws reduce all firearm self-injuries (including suicide attempts and self-injuries that were not the result of suicide attempts) among young people. In addition, we find moderate evidence that child-access prevention laws reduce firearm suicides among this population.
Effects on Violent Crime

Research Synthesis Findings

We identified four quasi-experimental studies that provided evidence of the effects of CAP laws on violent crime (Cummings et al., 1997a; Lott and Whitley, 2001; DeSimone, Markowitz, and Xu, 2013; Anderson and Sabia, 2018). Using a limited set of controls and data spanning 1979 to 1994, Cummings et al. (1997a) found a suggestive relationship between CAP laws and firearm homicides for children aged 15 or younger and uncertain effects for nonfirearm homicides. In contrast, examining an overlapping period from 1977 to 1996, Lott and Whitley (2001) found that CAP laws were significantly related to higher rates of rape (9-percent increase) and robbery (10-percent increase). In additional analyses, estimates showed a suggestive relationship between CAP laws and lower rates of assault, as well as uncertain effects of CAP laws on murder rates. However, the authors’ model had an unfavorable ratio of estimated parameters to observations (approximately one to eight), meaning the model may have been overfit, and thus parameter estimates and their CIs may have been invalid. Furthermore, Lott and Whitley (2001) made no adjustment for clustering of standard errors at the state level, which threatens the validity of the significance values estimated from their model.

An additional study (DeSimone, Markowitz, and Xu, 2013) found evidence of an effect of CAP laws on nonfatal firearm assault injuries, as measured through hospital discharge data. The authors examined information in 11 states between 1988 and 2003 and employed fixed effects for state and year in their statistical models (along with other state- and hospital-level covariates). They found that CAP laws based on negligent storage alone or on either negligent storage or reckless provision were associated with a suggestive reduction in firearm assault injuries among those aged 18 or younger; in particular, states with CAP laws saw about 20 percent fewer nonfatal firearm assault injuries among minors. The authors also found that CAP laws were associated with significant reductions in firearm assault injuries among adults, with similarly sized reductions estimated at 17 to 23 percent, although the effect was significant only for CAP laws with negligent storage, not when CAP laws included either negligent storage or reckless provision. It is unclear why more-stringent CAP laws would produce larger effects, but it was difficult to differentiate effects across these law types because only two states (Colorado and Wisconsin) in the authors’ sample passed reckless provision laws during the study time frame. Thus, identification in both specifications was driven largely by changes in state laws regarding negligent storage.

As DeSimone, Markowitz, and Xu (2013) noted in sensitivity analyses, there is some evidence that gun assault injury rates may have been declining in states with CAP laws even prior to law enactment. Thus, rather than reflecting the causal impact of CAP laws, the estimated effect sizes may reflect differential trends in gun assault injury rates across states that did or did not pass CAP laws.
Finally, Anderson and Sabia (2018) examined the effects of CAP laws on school shootings that involved a homicide death between 1991 and 2013. The authors separately considered incidents in which the perpetrator of the school shooting was a minor and incidents in which the perpetrator was aged 18 or older. Using linear probability models weighted by state population and adjusting for a large set of state-level time-varying covariates, state fixed effects, national trends, and state-specific linear trends, the authors found that estimated associations between CAP laws and the probability of a state experiencing a school shooting involving a homicide were uncertain for both age groups. The estimates were highly imprecise, with 95-percent CIs that spanned the full range of outcomes, including implausible effect sizes (see Figure 17.2). These results indicate that the study had low power to detect its relatively rare outcome and that the assumptions of the linear probability model were likely violated. The models also included a large number of covariates, which led to a ratio of parameters to observations of about one to eight, meaning the model may have been overfit.

Figure 17.2 displays the IRRs and CIs associated with the CAP laws examined in these studies.

**Figure 17.2**
Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Violent Crime

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State CAP law</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Firearm homicide, aged 0–14</td>
<td>0.89 [0.76, 1.05]</td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Nonfirearm homicide, aged 0–14</td>
<td>0.96 [0.86, 1.06]</td>
</tr>
<tr>
<td>Lott &amp; Whitley (2001)</td>
<td>Murder</td>
<td>1.04 [0.97, 1.11]</td>
</tr>
<tr>
<td>Lott &amp; Whitley (2001)</td>
<td>Rape</td>
<td>1.10 [1.04, 1.16]</td>
</tr>
<tr>
<td>Lott &amp; Whitley (2001)</td>
<td>Robbery</td>
<td>1.11 [1.03, 1.20]</td>
</tr>
<tr>
<td>Lott &amp; Whitley (2001)</td>
<td>Assault</td>
<td>0.96 [0.91, 1.01]</td>
</tr>
<tr>
<td><strong>School shooting</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Anderson &amp; Sabia (2018)</td>
<td>Any homicide, shooter aged 0–17</td>
<td>0.64, [0, 1.86]</td>
</tr>
<tr>
<td>Anderson &amp; Sabia (2018)</td>
<td>Any homicide, shooter aged 18+</td>
<td>0.94 [0.17, 1.71]</td>
</tr>
<tr>
<td><strong>State CAP law, negligent storage (11 states)</strong></td>
<td>Assault injury rate</td>
<td></td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm assault, aged 0–17</td>
<td>0.81 [0.65, 1.02]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm assault, aged 18+</td>
<td>0.78 [0.63, 0.96]</td>
</tr>
<tr>
<td><strong>State CAP law, negligent storage or reckless provision (11 states)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm assault, aged 0–17</td>
<td>0.82 [0.67, 1.02]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Firearm assault, aged 18+</td>
<td>0.84 [0.66, 1.07]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
**Conclusion**

We identified four studies that met our evidence standards and evaluated the effect of CAP laws on any violent crime outcomes. Cummings et al. (1997a) reported a suggestive effect consistent with CAP laws reducing firearm homicide rates among children aged 14 or younger. Lott and Whitley (2001) found that these laws had an uncertain effect on murder rates but significantly increased rates of robbery and rape. They also reported a suggestive effect consistent with the laws decreasing assault rates. DeSimone, Markowitz, and Xu (2013) also found that CAP laws reduced nonfatal firearm assault injuries among both youth and adults; results showed suggestive or significant effects depending on the age group and class of law being evaluated. Examining the effect of CAP laws on the probability that a school shooting involving a homicide occurred, Anderson and Sabia (2018) found uncertain effects of the laws.

Considering the relative strengths of these studies, we find limited evidence that child-access prevention laws reduce firearm assault injuries and inconclusive evidence for the effect of child-access prevention laws on violent crimes generally and on specific violent crimes, including firearm homicides, robberies, and rapes.

**Effects on Unintentional Injuries and Deaths**

**Research Synthesis Findings**

We identified six studies that met our inclusion criteria and evaluated the effect of CAP laws on unintentional injuries or deaths. All six used state and time fixed-effects models to examine the relationship between state CAP laws and firearm-related unintentional injury or death.

With a limited set of controls, Cummings et al. (1997a) found that CAP laws were associated with a lower risk of unintentional firearm death in children aged 15 or younger (IRR = 0.77; 95-percent CI = 0.63, 0.94) and suggestive evidence that the laws reduced such deaths in those aged 20-24 as well. In their reanalysis adding three more years of data and more states with CAP laws, Webster and Starnes (2000) also...
found that CAP laws were associated with a significant decrease in unintentional firearm deaths among those aged 14 or younger. In addition, they showed that this effect was not consistent across all states that have CAP laws. Significant reductions in such deaths were observed in states with felony CAP laws, and in states without felony laws, the effects were uncertain. Indeed, the authors noted that much of the observed effect of CAP laws was attributable to a single state, Florida, without which the overall effect of CAP laws still suggested that they reduce deaths, but the effect was uncertain. On the other hand, Lott and Whitley (2001) found only uncertain effects among youth aged 19 or younger, with some suggestive effects of an increase in unintentional injuries among children aged 5–9. Nevertheless, this model used an unfavorable ratio of estimated parameters to observations (approximately one to eight), meaning the model may have been overfit, and thus parameter estimates and CIs may be invalid; furthermore, no adjustment was made for clustered standard errors, so the standard errors and significance values reported in the paper were unreliable.

Hepburn et al. (2006) examined the relationship between CAP laws and unintentional firearm deaths from 1979 to 2000 among children aged 14 or younger compared with adults aged 55–74. In their state and time fixed-effects models, CAP laws were significantly associated with fewer unintentional deaths in children 14 or younger (but effects were uncertain among adults aged 55–74). For those 14 or younger, the estimate in Hepburn et al. (2006) suggests that the post-law firearm death rate was 78 percent of what would have been expected without the law. Like the analysis by Webster and Starnes (2000), the reduction was greatest in a model with the subset of states with felony CAP laws, in which rates after the laws were implemented were just 64 percent of the expected rate. In states with misdemeanor CAP laws, the effects were smaller and uncertain; in models excluding California or Florida, the effects were smaller and suggestive for those aged 14 or younger (see Figure 17.3). The authors controlled for firearm availability (using the proportion of suicides that were carried out with a firearm as a proxy for availability) and for changes in the coding of causes of death between the ninth and tenth revisions of the International Statistical Classification of Diseases and Related Health Problems in 1999. Demographic, social, and economic covariates were not included in this model, meaning that state variation in factors that may correspond with adoption of CAP laws cannot be ruled out as explaining the apparent CAP law effects.

DeSimone, Markowitz, and Xu (2013) performed a fixed-effects analysis on unintentional non-self-inflicted gun injuries using hospital discharge data from the NIS spanning 1988 through 2003. They found that CAP laws based on negligent storage alone or on either negligent storage or reckless provision had uncertain effects on unintentional firearm injuries in children aged 18 or younger in the 11 states that were part of the NIS, but they did find a statistically significant effect of these laws on unintentional firearm deaths among those 18 or older. Specifically, CAP laws that included negligent storage rules only were associated with a decline to 71 percent of
the rates expected without implementing such laws; the policies that included either negligent storage or reckless provision rules were associated with a decline to 69 percent of the expected rate. This similarity in estimated effects is likely because only two states (Colorado and Wisconsin) in the NIS passed reckless provision laws during the study time frame; thus, identification in both specifications was driven largely by changes in state laws regarding negligent storage. The findings were generally confirmed in a second analysis adding more control states (states without a change in CAP laws over the period); however, in those analyses, safe storage or negligent provision laws were associated with a significant reduction in unintentional injuries for those aged 18 or younger.

Gius (2015b) also examined the relationship between unintentional firearm deaths among youth and state CAP laws, but in a wider age range (19 or younger) and between 1981 and 2010, which is a longer period than all prior analyses. Unlike the earlier studies of similar data sets, this study found uncertain evidence of a reduction in youth unintentional deaths associated with CAP laws. Gius (2015b) controlled for a variety of state-level sociodemographic characteristics, along with two other laws related to youth firearm access (state minimum age requirements for handgun possession and the federal minimum age requirement for handgun possession enacted in 1994). The weighted least-squares statistical model used in this study may not be appropriate for the rare outcome, with low values or zero in many state-year observations. The model’s lower bound at zero may result in violations of its assumptions and can yield biased and incorrect parameter estimates and CIs (Freedman, 2006).

Figure 17.3 displays the IRRs and CIs associated with the CAP laws examined in these studies. Estimates from the Tobit models used by Lott and Whitley (2001) could not be converted into the IRR effect sizes that we used, so they are omitted from the figure.
### Figure 17.3

**Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Unintentional Injuries and Deaths**

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State CAP law</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Firearm death, aged 0–14</td>
<td>0.77 [0.63, 0.94]</td>
</tr>
<tr>
<td>Webster &amp; Sterne (2000)</td>
<td>Firearm death, aged 0–14</td>
<td>0.83 [0.71, 0.97]</td>
</tr>
<tr>
<td>Hepburn et al. (2006)</td>
<td>Firearm death, aged 0–14</td>
<td>0.78 [0.61, 0.99]</td>
</tr>
<tr>
<td>Gius (2015b)</td>
<td>Firearm death, aged 0–19</td>
<td>0.96 [0.86, 1.06]</td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Firearm death, aged 15–19</td>
<td>0.91 [0.77, 1.08]</td>
</tr>
<tr>
<td>Cummings et al. (1997a)</td>
<td>Firearm death, aged 20–24</td>
<td>0.84 [0.68, 1.03]</td>
</tr>
<tr>
<td>Hepburn et al. (2006)</td>
<td>Firearm death, aged 55–74</td>
<td>0.88 [0.63, 1.22]</td>
</tr>
</tbody>
</table>

| **State CAP law, negligent storage (11 states)** | Firearm injury, aged 0–17 | 0.76 [0.53, 1.09] |
| **State CAP law, negligent storage or reckless provision (11 states)** | Firearm injury, aged 18+ | 0.71 [0.54, 0.94] |

| **Felony CAP law** | Firearm death, aged 0–14 | 0.69 [0.56, 0.85] |
| **Misdemeanor CAP law** | Firearm death, aged 0–14 | 0.93 [0.76, 1.13] |

**NOTE:** IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
Conclusions

We identified six qualifying studies of the effect of CAP laws on unintentional firearm injuries or deaths. Cummings et al. (1997a) found a significant effect consistent with these laws reducing unintentional firearm deaths among children aged 14 or younger, uncertain effects on unintentional injuries for those aged 15–19, and a suggestive effect consistent with CAP laws reducing unintentional firearm injuries among those aged 20–24. Across four age groups, Lott and Whitley (2001) used an overlapping data set but found three uncertain effects and one suggestive effect consistent with CAP laws increasing unintentional firearm deaths among children aged 5–9.

Using a data set that was extended by one year beyond Lott and Whitley’s, Webster and Starnes (2000) found a significant effect suggesting that CAP laws reduce such deaths in children aged 14 or younger. In subgroup analyses, they found that this effect remained strong when examining just those states with felony CAP laws, but the effect was uncertain in states with misdemeanor CAP laws. Hepburn et al. (2006) used a similar data set extended by three years and produced a pattern of findings identical to those of Webster and Starnes (2000). Gius (2015b) added a decade of data to those studied by Hepburn et al. (2006) and found uncertain effects of CAP laws on unintentional firearm injuries among those aged 19 or younger.

Using a separate data series, DeSimone, Markowitz, and Xu (2013) found that CAP laws significantly reduced unintentional firearm injuries among those aged 17 or younger and among those 18 or older.

Considering the relative strengths of these studies and the two distinct data sets used in them, we conclude that there is supportive evidence that child-access prevention laws reduce unintentional firearm injuries and deaths among children. Although much fewer in number, the studies that have examined effects on young adults or adults provide limited evidence that these laws may reduce unintentional firearm injuries and deaths among adults as well.
### Effects on Mass Shootings

#### Research Synthesis Findings

Our search yielded two studies that met our inclusion criteria and examined the effects of CAP laws on mass shootings in the United States. Using a Poisson specification, Lott (2003) estimated how state laws requiring that guns be safely stored affect fatalities, injuries, and the incidence of *multiple-victim public shootings*, which the author defined as events unrelated to other criminal activity in which two or more people were killed or wounded in a public location. The analysis covered 1977 to 1997, and regression models included controls for state and year fixed effects, other state firearm policies, and a broad range of state-level socioeconomic and demographic characteristics. The findings showed uncertain effects of safe storage laws on total casualties from multiple-victim public shootings and on total multiple-victim public shooting incidents. However, these models had an unfavorable ratio of estimated parameters to observations (approximately one to eight), suggesting that the model may have been overfit, and thus the estimated effects of these laws may be poor indicators of their true effects. In addition, the model did not adjust for clustered standard errors. Together, these shortcomings suggest that the model results may not accurately describe the true effects of safe storage laws.

Anderson and Sabia (2018) used data on school shootings between 1991 and 2013 to examine the effects of CAP laws on the probability of a school shooting occurring. This study considered any school shooting resulting in a death (homicide, suicide, or accidental), not just those that involved multiple fatalities. Of the 76 school shootings the authors observed, 12 (15.8 percent) involved four or more deaths. Using linear probability models weighted by state population and controlling for a large set of state-level time-varying covariates, state fixed effects, national trends, and state-specific linear trends, the authors found uncertain associations between CAP laws and the likelihood of a state having a school shooting involving a shooter either under age 18 ($p = 0.95$) or aged 18 or older ($p = 0.64$). The estimates were highly imprecise, with 95-percent CIs that spanned nearly the full range of outcomes, including implausible effect sizes (see Figure 17.4). These results indicate that the study had low power to detect its relatively rare outcome and that the assumptions of the linear probability model were likely violated. The models also included a large number of covariates, which led to a ratio of parameters to observations of about one to eight, meaning the model may have been overfit.

Figure 17.4 displays the IRRs and CIs associated with the CAP laws examined in these studies.
Conclusions

We identified one qualifying study of the effect of CAP laws on mass shootings and one qualifying study of the effect of CAP laws on school shootings. Lott (2003) found uncertain effects for these laws on mass shooting casualties and mass shooting incidents, and Anderson and Sabia (2018) found uncertain effects for the laws on the probability of a school shooting occurring. Therefore, we find inconclusive evidence for the effect of child-access prevention laws on mass shootings.

Effects on the Gun Industry

Research Synthesis Findings

We identified one study that met our inclusion criteria and examined the relationship between CAP laws and the number of firearm manufacturing plants in a given state (Brauer, Montolio, and Trujillo-Baute, 2017). The authors specified the policy using an index that reflected the presence of a CAP law in a state; in the index, states were
scored four for having the law and zero for having no law.\textsuperscript{36} Using linear models with state and year fixed effects over the 1986 to 1999 period, the authors found an uncertain relationship between CAP laws and the number of firearm manufacturing plants in a state; however, the estimated effect was sensitive across specifications, depending on which years were included in the regressions. The ratio of parameters to observations in these models was also just less than one to ten, which may indicate some issues with model overfit. Furthermore, a linear model does not appear to correspond to the distributional features of the authors’ outcome variable, which limits the inference that can be drawn from this single study.

Figure 17.5 displays the IRR and CI associated with the CAP laws examined in Brauer, Montolio, and Trujillo-Baute (2017).

\textbf{Figure 17.5}
\textit{Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on the Gun Industry}

\begin{center}
\begin{tabular}{llr}
\textbf{Study, by Policy} & \textbf{Outcome Measure} & \textbf{IRR [95\% CI]} \\
State CAP law index (0–4) & Firearm manufacturing & 1.00 [0.96, 1.04] \\
Brauer, Montolio, & Number of plants & \\
& & \\
& & \\
\end{tabular}
\end{center}

\textit{NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.}

\textbf{Conclusions}

We identified one qualifying study examining the effect of CAP laws on firearm manufacturing plant locations, and the authors found uncertain effects. Considering this single study, we find \textit{inconclusive evidence for the effect of child-access prevention laws on the gun industry.}

\textsuperscript{36} The authors also explored use of a different CAP index that reflected the presence of a state CAP law and safety provisions, including child safety locks, over a later period (2006 to 2010). This index ranged from zero to 11. However, this alternative model had a highly unfavorable ratio of parameters to observations (less than one to five) and thus was likely overfit. We therefore excluded those results from our synthesis.
Outcomes Without Studies Examining the Effects of Child-Access Prevention Laws

We did not identify any research that met our inclusion criteria and examined the relationship between CAP laws and the following outcomes:

- officer-involved shootings
- defensive gun use
- hunting and recreation.
Chapter Seventeen References

American Law Institute, Model Penal Code, Section 2.02 смтр. at 238, 1985.


CDC—See Centers for Disease Control and Prevention.


Apart from specifying classes of people who are prohibited from possessing any type of firearm, federal law imposes no restrictions on who may carry a concealed weapon in public, although it specifically grants concealed-carry rights to active and retired law enforcement officers (18 U.S.C. 926). State laws typically specify who may carry concealed weapons and the procedures those people must follow when they wish to exercise this right.

Prior to the Civil War, most states lacked legislation on the legality of carrying concealed weapons. States that did have such laws prohibited the practice. Following World War II, most states adopted laws giving law enforcement agencies discretion over who could carry concealed weapons (Cramer and Kopel, 2005). In the 1980s, 1990s, and early 2000s, a majority of states relaxed restrictions on concealed handguns. Several states allow individuals to carry concealed weapons without a permit (referred to as permitless carry), but most require gun owners to obtain a permit to carry a concealed handgun. Some states have shifted from laws that restrict concealed-carry permits to those who can demonstrate a legitimate need to carry a weapon or that give law enforcement some discretion in issuing concealed-carry permits (referred to as may-issue laws) to laws that guarantee the right to a concealed-carry permit for all citizens who are not prohibited from possessing a handgun (referred to as shall-issue or right-to-carry laws). The key difference among these law categories is that permitless-carry laws do not require individuals to obtain a permit or license before they can carry a concealed weapon, whereas may-issue and shall-issue laws set forth conditions by which such permits may be granted.

There are several ways that concealed-carry laws could affect gun violence and considerable disagreement about which laws are most likely to do so. Permitless-carry and shall-issue laws that make it easier for citizens to carry concealed weapons could increase the number of people carrying guns. The increased prevalence of concealed weapons could lead to increased crime and violence if disagreements, perceived threats, and conflicts are more likely to result in casualties when a handgun is readily available. Alternatively, concealed-carry laws could lead to reductions in the prevalence or severity of violent crime and mass shootings either because the prospect of encountering an armed victim serves as a deterrent or because victims will more frequently be able to use a gun to defend themselves (Fortunato, 2015).
Whether those who carry concealed weapons pose an elevated or reduced risk of crime or violence is the subject of debate (Violence Policy Center, 2017; Lott, Whitley, and Riley, 2016). A comparison of criminal conviction rates among holders and non-holders of concealed handgun licenses in Texas found that license holders were less likely to be convicted of crimes, but the license holders’ convictions were significantly more likely to involve deadly conduct and intentional killings (Phillips et al., 2013). The likelihood of encountering an armed victim may further lead to increased gun violence by inducing more criminals to carry and use firearms. Alternatively, these laws may result in criminals deciding to pursue other types of crime, such as larceny, where the probability of encountering armed resistance is lower (Kovandzic and Marvell, 2003).

Relaxed restrictions on concealed carrying could produce a range of effects associated with increasing the percentage of the population that is armed. Indeed, Rowhani-Rahbar et al. (2017) found large differences in the percentage of adults who routinely carried a loaded weapon between states with different concealed-carry laws. In states with more-restrictive may-issue laws, only 9.1 percent of handgun owners carried their firearm in a 30-day period. In contrast, in states with shall-issue and permitless-carry laws, more than 20 percent of handgun owners did so.

Data on the number of persons with concealed-carry permits are not readily available for many states. But recent estimates suggest that the number of concealed-carry permit holders in the United States was around 14 million in 2015 (Rowhani-Rahbar et al., 2017) and was more than 17 million in 2018 (Lott, 2018), with substantial variation across states depending on the permit fees in place, the duration that the law has been in effect, and whether the law allows local authorities discretion in issuing permits (i.e., may issue versus shall issue).

We identified only one study that analyzed how changes in the number of concealed-carry permits related to changes in various types of violent crime (Kovandzic and Marvell, 2003). The authors analyzed data from 58 Florida counties spanning 1980–2000, providing coverage of the period before and after the passage of Florida’s shall-issue law in 1987. Although this study did not analyze the effect of the shall-issue policy change, it did examine how changes in the number of concealed-carry permits over time and across counties corresponded with changes in various types of violent crime. The authors found uncertain effects of changes in per capita concealed-carry permit rates on violent crime.

There is likely to be little effect of concealed-carry laws on hunting or recreational gun use. However, shall-issue policies may encourage more individuals to obtain firearms, thereby increasing handgun sales (Steidley, 2016). To assess these or any other effects of concealed-carry laws, one would ideally like to know whether there are greater increases in gun ownership and carrying in states following passage of shall-issue or permitless-carry laws compared with states that have more-restrictive laws, but such data have not been collected systematically over time. The direct effects of increased concealed carrying by private citizens on suicides, unintentional injuries and deaths, and defensive gun use should be strongest for incidents involving handguns and that
Concealed-Carry Laws

occur outside the home (where the laws apply). Similarly, for violent crime, one would expect concealed-carry laws to have greater effects (either negative or positive, depending on the role of deterrence) on assaults or homicides occurring in public venues compared with those occurring within the home. Should the effects of concealed-carry laws be driven primarily by expanding the prevalence of gun ownership, then their effects could extend to both private and public areas for such outcomes as suicides, firearm homicides, and unintentional injuries and deaths.

State Implementation of Concealed-Carry Laws

As of January 1, 2020, 15 states have laws allowing people to carry concealed weapons without first receiving a permit; that includes Vermont, which has never required a permit for concealed carry.1 Mississippi allows concealed carry without a permit if the handgun is kept “in a sheath, belt holster or shoulder holster or in a purse, handbag, satchel, other similar bag or briefcase or fully enclosed case.”2 Twenty-six states and the District of Columbia have shall-issue laws, under which law enforcement agencies have no, or very limited, discretion to deny concealed-carry permits to citizens who are otherwise permitted to possess handguns.3 Eight states have may-issue laws, in which law enforcement agencies have significant discretion to deny permits.4


Many states have reciprocity clauses in their concealed-carry permit laws, meaning that they recognize the concealed-carry permits issued by some but not necessarily all other states (United States Concealed Carry Association, 2013). Often, states honor permits only from other states with laws similar to their own. There are some states, however, that recognize concealed-carry permits from states with less-restrictive laws. For instance, Delaware has a may-issue law but recognizes the concealed-carry permits from several states with shall-issue laws (USA Carry, 2017).

**Effects on Suicide**

**Research Synthesis Findings**

We identified three studies that met our inclusion criteria and examined the effect of concealed-carry laws on suicide; no studies examined the effects of permitless-carry laws on suicide. Using data from 1979 to 1998, Rosengart et al. (2005) modeled the effect of shall-issue laws on suicide mortality across states. In these models—which controlled for state fixed effects, national time trends, state-specific time trends, state-level variation in poverty and demographic factors, and four other firearm laws—the authors found uncertain effects of the adoption of more-permissive shall-issue laws on firearm suicide rates, but they found suggestive effects consistent with a reduction in total suicide rates (see Figure 18.1). Nevertheless, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning the model may have been overfit, resulting in estimates and confidence intervals (CIs) that are unreliable indicators of the true causal effects of the laws.

DeSimone, Markowitz, and Xu (2013) also performed a fixed-effects analysis and examined the effects of shall-issue laws on self-inflicted nonfatal gun injuries using hospital discharge data from the National Inpatient Sample spanning 1988 to 2003. The authors did not find that shall-issue laws were significantly associated with self-inflicted firearm injuries for children under age 18 in the 11 states that were part of the sample, but they did find a statistically significant effect of these laws on self-inflicted firearm injuries among those aged 18 or older. Specifically, their estimate suggests that, after a state implemented a shall-issue law, self-inflicted injuries were more than double what would have been expected without the law (see Figure 18.1), which would be extraordinary if true. However, the estimated effects of shall-issue laws in this study were based primarily on implementation in one state that changed its law during the study time frame (Arizona); thus, the study offers little evidence that the observed effects are due to the change in the law rather than to other factors affecting the state’s injury rates that occurred around the same time the law was changed.

Luca, Malhotra, and Poliquin (2017) used data from 1977 to 2014 to evaluate the effects of various firearm laws on suicide rates among adults aged 21 or older. Although the authors’ focus was on background check and waiting-period laws, they included
model specifications that additionally controlled for concealed-carry and permitting laws. Their analysis was based on log-linear models adjusting for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors; they found uncertain effects of shall-issue and may-issue laws on total and firearm-related suicide rates. However, relative to laws that prohibited concealed carry, both may-issue and shall-issue laws were associated with significant increases in nonfirearm suicide rates.

Figure 18.1 displays the incidence rate ratios (IRRs) and CIs associated with the concealed-carry laws examined in these studies.

**Figure 18.1**
Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Suicide and Self-Injury

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Shall–issue (vs. may–issue or no–issue)</strong></td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Total</td>
<td>0.98 [0.96, 1.01]</td>
</tr>
<tr>
<td>Rosengart et al. (2005)</td>
<td>Firearm</td>
<td>1.00 [0.97, 1.02]</td>
</tr>
<tr>
<td><strong>Self–inflicted injury rate</strong></td>
<td>Firearm, aged 0–17</td>
<td>1.94 [0.45, 8.38]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, and Xu (2013)</td>
<td>Firearm, aged 18+</td>
<td>2.10 [1.53, 2.89]</td>
</tr>
<tr>
<td><strong>Shall–issue (vs. no–issue)</strong></td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>1.01 [0.96, 1.07]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>1.01 [0.95, 1.07]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>1.09 [1.03, 1.15]</td>
</tr>
<tr>
<td><strong>May–issue (vs. no–issue)</strong></td>
<td>Suicide rate</td>
<td></td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Total, aged 21+</td>
<td>1.01 [0.96, 1.07]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Firearm, aged 21+</td>
<td>0.99 [0.92, 1.07]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2017)</td>
<td>Nonfirearm, aged 21+</td>
<td>1.10 [1.04, 1.16]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.

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5 Because the analyses of concealed-carry laws were included only as part of supplemental analyses in this study, the comparison group in these models was not entirely clear. But, from the information provided in the paper, we assumed that a no-issue law (i.e., no one is permitted to carry concealed firearms) was the omitted policy.
Conclusions
We identified three qualifying studies examining the effects of shall-issue concealed-carry laws on suicide rates or firearm self-injury rates. Rosengart et al. (2005) found suggestive evidence that shall-issue laws reduce suicides and uncertain effects of these laws on firearm suicides. DeSimone, Markowitz, and Xu (2013) found the effect of shall-issue laws on firearm self-injuries among those aged 17 or younger to be uncertain. Among all adults aged 18 or older, they found a significant effect indicating that shall-issue laws may increase firearm self-injury. Luca, Malhotra, and Poliquin (2017) found uncertain effects of shall-issue and may-issue laws on both total and firearm suicide rates among adults aged 21 or older.

Considering these studies and an assessment of their relative strengths, we find inconclusive evidence for the effect of shall-issue concealed-carry laws on total suicides, firearm suicides, and firearm self-injuries.

Effects on Violent Crime
Research Synthesis Findings
An explosion of research into the effects of shall-issue laws on violent crime was triggered in 1997 by the publication of analyses using county-level data from 1977 to 1992. Using these data, Lott and Mustard (1997) concluded that states implementing shall-issue laws saw significant decreases in rates of violent crime, murder, rape, and assault. Their “more guns, less crime” conclusion was immediately controversial and led to a proliferation of studies exploring the robustness of the study’s findings to alternate model specifications and to improvements or expansions to the data series. Table 18.1 lists studies from this early period of responses to Lott and Mustard (1997), as well as their counter-responses.

Two important reviews of the scientific literature on gun policy effects—one by the National Research Council (NRC), a part of the National Academy of Sciences (NRC, 2004), and one by the Community Preventive Services Task Force, established by the U.S. Department of Health and Human Services (Hahn et al., 2005)—evaluated this early literature and reached nearly identical conclusions. In their review of existing studies examining shall-issue laws, Hahn et al. (2005) found insufficient evidence for determining the effect of such laws on violent crime. NRC (2004) reviewed much of
### Table 18.1

<table>
<thead>
<tr>
<th>Study</th>
<th>Significant Effect Reported (Main Specification)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lott and Mustard (1997)(^a)</td>
<td>Decrease in violent crime, murders, rapes, and assaults</td>
</tr>
<tr>
<td>Bartley and Cohen (1998)</td>
<td>Decrease in violent crime robust to alternate model specifications</td>
</tr>
<tr>
<td>Black and Nagin (1998)</td>
<td>Increase in assaults</td>
</tr>
<tr>
<td>Bronars and Lott (1998)</td>
<td>Decrease in murders and rapes, displacement of crime to other jurisdictions</td>
</tr>
<tr>
<td>Lott (1998a)(^a)</td>
<td>Decrease in violent crime in most states implementing the law</td>
</tr>
<tr>
<td>Lott (1998b)(^a)</td>
<td>Decrease in violent crime; increase in property crime</td>
</tr>
<tr>
<td>Ludwig (1998)</td>
<td>None detected</td>
</tr>
<tr>
<td>Ayres and Donohue (1999)(^a)</td>
<td>Increase in property crime</td>
</tr>
<tr>
<td>Lott and Landes (1999)(^a)</td>
<td>Decrease in murders and injuries from multiple-victim public shootings</td>
</tr>
<tr>
<td>Lott (2000)(^a)</td>
<td>Decrease in all crime categories</td>
</tr>
<tr>
<td>Benson and Mast (2001)</td>
<td>Decrease in violent crime, murders, rapes, and robberies</td>
</tr>
<tr>
<td>Duggan (2001)</td>
<td>Decrease in assaults</td>
</tr>
<tr>
<td>Moody (2001)(^a)</td>
<td>Decrease in violent crime</td>
</tr>
<tr>
<td>Olson and Maltz (2001)</td>
<td>Decrease in firearm murders</td>
</tr>
<tr>
<td>Plassmann and Tideman (2001)</td>
<td>Decrease in murders and rapes; increase in robberies</td>
</tr>
<tr>
<td>Lott and Whitley (2003)(^a)</td>
<td>Decrease in violent crime, murders, rapes, and robberies</td>
</tr>
<tr>
<td>Plassmann and Whitley (2003)(^b)</td>
<td>Decrease in rapes and robberies</td>
</tr>
<tr>
<td>Rubin and Dezhbakhsh (2003)</td>
<td>Decrease in murders; increase in robberies</td>
</tr>
<tr>
<td>Ayres and Donohue (2003a)(^a)</td>
<td>Increase in more crime categories than saw a decrease</td>
</tr>
<tr>
<td>Ayres and Donohue (2003b)(^a)</td>
<td>Increase or no effect in all crime categories</td>
</tr>
<tr>
<td>Donohue (2003)(^a)</td>
<td>Mixed; effects were sensitive to model specifications and data</td>
</tr>
<tr>
<td>Helland and Tabarrok (2004)</td>
<td>Increase in property crime, auto thefts, and larcenies</td>
</tr>
</tbody>
</table>

\(^a\) These studies are treated in this report as being superseded by later studies by the same authors.
\(^b\) This same paper was earlier circulated as Lott, Plassmann, and Whitley (2002).
the same literature and reanalyzed data that were common to many of these analyses: a panel data set originally spanning 1977–1992, then expanded through 2000. After reviewing many of the studies listed in Table 18.1, the NRC (2004) panel, with one member dissenting, concluded:

Some studies find that right-to-carry laws reduce violent crime, others find that the effects are negligible, and still others find that such laws increase violent crime. The committee concludes that it is not possible to reach any scientifically supported conclusion because of (a) the sensitivity of the empirical results to seemingly minor changes in model specification, (b) a lack of robustness of the results to the inclusion of more recent years of data (during which there were many more law changes than in the earlier period), and (c) the statistical imprecision of the results. The evidence to date does not adequately indicate either the sign or the magnitude of a causal link between the passage of right-to-carry laws and crime rates. Furthermore, this uncertainty is not likely to be resolved with the existing data and methods. If further headway is to be made, in the committee’s judgment, new analytical approaches and data are needed.

In addition to the sensitivity of results to minor changes in model specification noted by the NRC report, these early studies suffered from multiple serious problems with data and methodology that lead us to discount their value for informing this synthesis of evidence on the effects of shall-issue laws. These problems include the following:

- Lott and Mustard’s data set used county population values that did not correspond to the crime statistics available for counties, especially those with weak reporting of crime statistics (Maltz and Targonski, 2002). Lott and Whitley (2003) discounted these and other concerns about the quality of county crime rate data, describing them as typical of the types of measurement error commonly encountered in statistical analyses. Furthermore, they suggested that the findings in Lott (2000) persisted even when analyzing the subset of counties with minimal error in crime statistics. After reviewing this exchange, the NRC panel disagreed with Lott and Whitley that the original effects reported by Lott (2000) survived this test: “The committee concludes that it is at least possible that errors in the [Uniform Crime Reporting] data may account for some of Lott’s results” (NRC, 2004, p. 137).
- Many of these studies followed the example of Lott and Mustard (1997) by including arrest rates as a model covariate. This led to these analyses excluding large numbers of counties that had no crimes of a given type and therefore an undefined arrest rate, an approach that differentially excluded locations where the introduction of shall-issue laws could have led only to an increase in crime rates (Ayres and Donohue, 2003a).
• There were errors in the classification of shall-issue states in the Lott and Mustard data set that were only later corrected (Ayres and Donohue, 2003a). There were multiple errors detected in the data sets used by Lott (1998b, 2000) and by Plassmann and Whitley (2003), and Plassmann subsequently acknowledged these errors to the NRC (NRC, 2004, p. 136). Correction of these errors eliminated many of the significant effects reported by Plassmann and Whitley (2003) (Ayres and Donohue, 2003a).

• Nearly all of the studies listed in Table 18.1 failed to control for serial correlation in the panel data set; the exceptions were Duggan (2001), Olson and Maltz (2001), Plassmann and Whitley (2003), Ayres and Donohue (2003a, 2003b), and Helland and Tabarrok (2004). This led to gross exaggerations of the statistical significance of study results and greatly elevated the risk of finding statistically significant effects that were in the opposite direction of any true effect (Schell, Griffin, and Morral, 2018; Moody and Marvell, 2018b; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

• Most of the studies used the large number of covariates first included in the Lott and Mustard (1997) analyses, which had a ratio of estimated parameters to observations of between one to eight and one to 14 across analyses. When the proportion of estimated parameters is this high, there is considerable risk that the statistical models are overfit, and the law effects that they estimate thus may not be generalizable. Among few exceptions, the models of Ludwig (1998) and Moody (2001) did not suffer from this problem.

Finally, we regard a majority of these early studies as having been superseded by later work by the same authors that improved upon their earlier contributions to this literature. As a result, we focus on their later efforts to evaluate the effect of shall-issue laws.

We first describe studies published since 2004 that aimed to estimate the effects of concealed-carry laws on violent crime using county-level data. We then turn to studies that focused on state-level data, then studies that employed city-level data. We conclude by discussing results from a set of studies in which the objective was not to identify the effects of shall-issue laws but that nonetheless present estimates that may be considered part of the evidence base for how concealed-carry policies influence violent crime outcomes (e.g., some studies of the effects of abortion rates on violent crime include shall-issue laws as a covariate in their models).

**County-Level Studies**

Many important shortcomings of county-level crime data identified through the early studies of shall-issue laws (Table 18.1) resulted from the fact that large numbers of county police agencies do not report crime statistics to the Federal Bureau of Investigation (FBI). Moreover, the way that county crime statistics address these missing data changed abruptly in the early 1990s, making data from the earlier part of the series...
not comparable with later data, according to the National Archive of Criminal Justice Data (undated). Nevertheless, several analyses have continued to use county-level crime data to evaluate law effects, or they have used homicide data from the Centers for Disease Control and Prevention (CDC)’s National Vital Statistics System, which has less of a problem with missing data (Loftin, McDowall, and Fetzer, 2008).

Roberts (2009) used the FBI’s Supplementary Homicide Reports to analyze the effect of shall-issue laws on intimate partner homicide rates using monthly county-level data spanning 1985–2004. The author found that (the more-restrictive) may-issue laws significantly increased intimate partner total homicides by 71 percent compared with shall-issue laws, but may-issue (compared with shall-issue) laws had an uncertain effect on intimate partner firearm homicides. The author also found uncertain effects of concealed-carry bans compared with shall-issue laws on either overall or firearm-related intimate partner homicides. However, neither analysis clustered standard errors at the state level, so serial correlation that was unaccounted for in the panel data likely resulted in underestimated standard errors and correspondingly misleading tests of statistical significance.

Aneja, Donohue, and Zhang (2014) analyzed the county-level data set used in NRC (2004), extended through 2006, and state-level data through 2010. The authors corrected the NRC analyses for several errors that they identified, including data-coding errors related to the timing of shall-issue legislation, an endogenous control variable (arrest rate), and a failure to cluster standard errors at the state level. The authors argued that the decision in NRC (2004) not to cluster the standard errors of the county-level analyses at the state level was incorrect and showed that CIs were badly misestimated when clustering was not accounted for. In their preferred county-level specification including state trend effects, they found no statistically significant effects of shall-issue laws on either the level or trend of any of seven crime rates, and they found only one suggestive effect across the 14 effects they tested.

Moody et al. (2014), responding to an earlier version of the Aneja, Donohue, and Zhang (2014) paper, reestimated their models after adding many more demographic control variables, robbery and assault rates, and a lagged outcome as a predictor meant to capture unmeasured state differences associated with crime rates. Moody et al. (2014) offered statistical tests suggesting that the model with added covariates predicted the data significantly better, which the authors interpreted as evidence that estimates in Aneja, Donohue, and Zhang (2014) suffered from omitted-variable biases. The revised hybrid model results in Moody et al. (2014) suggested that shall-issue laws significantly reduced the trends in rape and murder rates. They found no significant association between shall-issue laws and either assault or robbery. The fact that their model predicted a given outcome better than the Aneja, Donohue, and Zhang (2014) model is not sufficient to demonstrate the claim that the latter’s model suffered from omitted-variable bias or that the model preferred by Moody et al. (2014) offered a less biased estimate. An overfit model can predict the data exceptionally well while producing biased and unreliable coefficient estimates.
Using county-level panel data spanning 1979–2000, Durlauf, Navarro, and Rivers (2016) examined the sensitivity of analyses that estimate the relationship between shall-issue laws and violent crime. They reported that use of population weights may lead to inefficient estimates and upward biases in estimates of the effect of shall-issue laws on crime. In addition, they found that hybrid or spline models are preferred to dummy models and that models that allow for heterogeneity in the effect of laws (including effects that vary with region, rates of gun ownership, and the level of urbanization in an area) outperform models that do not allow for variation in effects. For the spline model specifications that the authors assessed to perform best for the outcome of violent crime, they estimated that shall-issue laws increase violent crime in the first year after law passage and that violent crime continues to increase in subsequent years. The authors concluded that, overall, there was substantial variation in the estimated effects for each model across the model space analyzed and, thus, there was little evidence that shall-issue laws generate either an increase or a decrease in crime on average.

Crifasi et al. (2018b) evaluated the effects of shall-issue laws and four other gun laws on homicides in large, urban counties between 1984 and 2015. Using a Poisson model that included year fixed effects, random effects for counties, and county-level demographic and economic covariates, the authors found that shall-issue laws were associated with a significant increase in firearm homicide rates. Specifically, after implementing these laws, counties would be expected to see 1.04 times more firearm homicides (95-percent CI = 1.02, 1.06). The authors also included a comparison outcome, nonfirearm homicides, on the theory that, if the effect of shall-issue laws is correctly estimated, it should be found only for firearm homicides, not nonfirearm homicides. However, their estimate for nonfirearm homicides was virtually identical to the estimate for firearm homicides (IRR = 1.03; 95-percent CI = 1.00, 1.06), which raises questions about the model or the authors’ theory that nonfirearm homicides should be unaffected by the law. The paper did not describe any corrections for serial correlation in the data used, without which incorrect claims of statistical significance would be expected to proliferate (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

State-Level Studies
Hepburn et al. (2004) evaluated the effects of shall-issue laws on homicide rates using data from 1979 to 1998 in a study that came out too late to be reviewed in either the NRC (2004) or the Hahn et al. (2005) reviews of firearm research. Using a negative binomial model with two-way fixed effects and controlling for demographic and economic variables, including a proxy for gun ownership, the authors found uncertain effects for shall-issue laws on state homicide rates. Estimated effects remained uncertain in subgroup analyses of adults aged 25 or older and of white men aged 35 or older (see Figure 18.2).
Rosengart et al. (2005) examined the effect of several state gun laws, including shall-issue laws, on firearm homicides and total homicides using state-level data. One limitation was that the data covered only 1979–1998, and other studies have shown the sensitivity of results to shorter periods, partly because shorter periods include observation of fewer states that have adopted shall-issue laws. The policy variable was specified as a dummy variable (indicating that a shall-issue law was or was not in place). The authors found suggestive effects that shall-issue laws increased firearm and total homicide rates. French and Heagerty (2008) tested the sensitivity of these results and similarly concluded that shall-issue laws had a suggestive effect consistent with the laws increasing firearm-related homicide rates, although estimates varied across specifications. However, the Rosengart et al. (2005) paper, and presumably the French and Heagerty (2008) paper, also had an unfavorable ratio of model covariates to observations (less than one to eight), suggesting that the model may have been overfit, and thus its estimates and their CIs may be unreliable.

Martin and Legault (2005) demonstrated that Lott (2000) used incorrect state crime rate estimates that differed substantially from official FBI state estimates. They replicated Lott (2000)’s model despite misgivings about its specification to demonstrate that the effects Lott reported were sensitive to this measurement error. In their replication exercise using state-level crime data from the FBI’s Uniform Crime Reports spanning 1977–1992, Martin and Legault (2005)’s estimates showed that shall-issue laws significantly reduced total violent crime and, specifically, aggravated assault. They found only suggestive effects that the laws reduced rates of robbery and murder, as well as uncertain effects on rape (see Figure 18.3). However, as with Lott (2000), the authors did not statistically adjust for serial correlation in the panel data, and the model’s ratio of estimated parameters to observations was less than one to ten, meaning the model may have been overfit, and thus its parameter estimates and their CIs may be unreliable.

Grambsch (2008) conducted a state-level analysis of (total) murder rates (relative to the U.S. murder rate) from 1976 to 2001 using the 25 states that passed shall-issue laws between 1981 and 1996. She found a selection effect among states adopting shall-issue laws—namely, that states that passed shall-issue laws in this period experienced an increasing trend in murder rates prior to adoption relative to other states. Her estimates showed that, after controlling for regression to the mean, there was either an uncertain effect or a significant positive effect of shall-issue laws on relative murder rates (i.e., shall-issue laws increased murder rates) depending on the model used. However, the model finding significant effects (the state fixed-effects model) had fewer than ten observations per estimated parameter, meaning the model may have been overfit, which can lead to unreliable estimates and standard errors. Furthermore, neither model included adjustments for serial correlation in the panel data.

Using a panel of state data, Lott (2010) provided an update of his earlier analyses examining the effect of shall-issue laws on violent crime. His preferred specification
included a set of dummy variables that indicated different time intervals before and after shall-issue legislation was in effect for states that passed such legislation. Many of Lott’s modeling results were presented as figures and did not indicate statistical significance. Detailed results were provided only for an analysis of homicide rates. These included information on the statistical significance of each coefficient in the model but not for a test comparing post-implementation time intervals with pre-implementation time intervals. Lott interpreted the pattern of effects as demonstrating that homicides declined significantly after implementation of shall-issue laws, but he did not provide test statistics or sufficient description to clarify what specific effect was observed. The author also included coefficients and their statistical significance from dummy and spline models similar to those from his earlier work, but he did not include standard errors or test statistics. All of the preferred models appear to have had a ratio of estimated parameters to observations that was less than one to ten, meaning the model may have been overfit, and thus the reported estimates and their CIs may be unreliable. Similarly, it does not appear that Lott used any adjustments for serial correlation in his panel data, so some of the effects reported as statistically significant might not be after correcting these analyses (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

DeSimone, Markowitz, and Xu (2013) evaluated the effects of child-access prevention laws (see Chapter Seventeen) on nonfatal injuries using data from 1988 to 2003, but they included sensitivity analyses that controlled for shall-issue laws. Using fixed-effects Poisson regression models, they found that shall-issue laws were significantly associated with firearm assault injuries for children under age 18, as well as for adults. Specifically, their estimate suggests that, after a state implemented a shall-issue law, assault injury rates were more than double what would have been expected without the law (see Figure 18.3), which would be extraordinary if true. However, the estimated effects of shall-issue laws in this study were based primarily on implementation in one state that changed its law during the study time frame (Arizona); thus, the study offers little evidence that the observed effects are due to the change in the law rather than to other factors affecting the state’s assault rate that occurred around the same time the law was changed.

Webster, Crifasi, and Vernick (2014) analyzed state-level data from 1999 to 2010, using generalized least-squares regression models to estimate the effect of shall-issue laws on age-adjusted homicide rates. They found suggestive effects indicating an association between the implementation of shall-issue laws and a 10-percent increase in rates of nonfirearm homicide, a 6-percent increase in rates of total homicide, and an 11-percent increase in rates of murder and nonnegligent manslaughter.6 However, their

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6 Most homicides reported in the CDC’s vital statistics data are counted among deaths reported to the FBI as murders and nonnegligent manslaughter. The authors used both data sources for this study because the vital statistics data differentiated firearm homicides from total homicides, whereas the FBI data spanned a longer period.
estimates showed an uncertain association between shall-issue laws and firearm homicide rates. The statistical model used to arrive at these results used a large number of estimated parameters relative to observations (a ratio of about one to eight), meaning the model may have been overfit, and thus its estimates and their apparent statistical significance could provide little generalizable information about the true causal effects of shall-issue laws.

Gius (2014) examined the effect of shall-issue laws on gun-related murder rates using state-level data from 1980 to 2009. He found that states with may-issue or more-restrictive policies had higher gun-related murder rates than shall-issue states. Relative to states with shall-issue laws, states with more-restrictive firearm-carry policies had rates of firearm homicide that were 11 percent higher (see Figure 18.3). However, this model did not statistically adjust for the known serial correlation in these panel data, which has been shown to result in misleadingly small standard errors (Schell, Griffin, and Morrall, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004). For this reason, the apparently significant effect observed in this study could be invalid.

Using their preferred specification with state-level data from 1979 to 2010 and a dummy, spline, or hybrid specification of shall-issue laws without state trends, Aneja, Donohue, and Zhang (2014) found suggestive evidence that shall-issue laws increase assaults by 8 percent (see Figure 18.2). In the dummy specification, shall-issue laws significantly increased rape by 12 percent, although estimates of this effect from the spline model were uncertain. The authors also found suggestive evidence that shall-issue laws increased rates of robbery, although estimates again became uncertain in other specifications. Effects of shall-issue laws on murder rates were uncertain. The authors tested the sensitivity of their results to less-parsimonious (including the Lott and Mustard [1997] specification) and more-parsimonious demographic specifications; the inclusion of state-specific time trends; the inclusion or exclusion of years that were likely to be influenced by the crack cocaine epidemic, which affected crime rates; and the specification of the policy variable (dummy, spline, hybrid). The authors noted that their results, which showed that the significance and sign of estimated effects varied substantially depending on the specification employed, underscored the sensitivity of gun-crime modeling estimates to modeling decisions.

Moody et al. (2014) and Moody and Marvell (2018a) critiqued several modeling decisions of the Aneja, Donohue, and Zhang (2014) paper, as well as an earlier version of that study (Aneja, Donohue, and Zhang, 2011). Foremost, the studies critiqued the decision to treat models without state-specific trends as the preferred ones. Thus, Moody et al. (2014) reestimated the hybrid models in Aneja, Donohue, and Zhang (2014), incorporating state-specific trends and additional covariates into an analysis of
state data. In doing so, the authors found, as they had with their county-level analyses, that their specification improved model fit over that of Aneja, Donohue, and Zhang (2014). They also found that the individual states’ trends were jointly significant, which they took as evidence supporting the need for their inclusion in the models of shall-issue law effects. Using hybrid models that included state-specific linear trends, Moody et al. (2014) found that shall-issue laws significantly increased assault rate trends and increased robbery rate levels, but the laws also significantly reduced murder rate trends. In an updated analysis that favored using a series of leading and lagging indicators of shall-issue laws over the hybrid model specification, Moody and Marvell (2018a) found largely uncertain effects of shall-issue laws on violent crime outcomes. As noted earlier, neither study demonstrated that its model estimates were less biased than those in Aneja, Donohue, and Zhang (2014) or that the Aneja, Donohue, and Zhang (2014) model suffered from omitted-variable biases. Furthermore, the state-level analyses of Moody et al. (2014) used a statistical model with a large number of estimated parameters relative to observations (close to one to five), meaning the model may have been overfit, and thus the estimates and inferential statistics may provide little generalizable information about the true causal effects of shall-issue laws.

In a series of analyses by John Donohue and colleagues, Donohue, Aneja, and Weber (2019) provided estimates of the effects of shall-issue laws; the study used updated data covering 1977–2014, during which 33 states implemented these laws. The authors’ two-way fixed-effects model—controlling for demographic, economic, and law enforcement factors—indicated uncertain effects on the logged murder and firearm murder rates but significant increases in violent crime and property crime generally.

Donohue, Aneja, and Weber (2019) also described an assessment of the effects of shall-issue laws that relies on constructing synthetic controls for each state that implemented a shall-issue law. Synthetic controls are weighted combinations of states that never implemented the law or that implemented it more than ten years after the treated state, such that, in the period before a state’s passage of the law, the temporal pattern of crime in the synthetic control closely matches that in the state. Repeating this procedure for each of 33 states with shall-issue laws, the authors concluded that violent crime increased over a ten-year period in 23 of 31 states with at least ten years of post-implementation data. In aggregate, the authors estimated that, five years after law passage, states with shall-issue laws had violent crime rates that were 7 percent higher than expected, which rose to 14 percent after ten years. The authors calculated significance levels for these estimates using a permutation test designed to estimate the distribution of treatment effects under the assumption that laws have no real effect. They concluded that, after the seventh year post-implementation, states with shall-issue laws had significantly elevated rates of violent crime. Synthetic control methods are relatively new, and especially when controls are made up of just a few states, as they
Barati (2016) explored whether the effect of shall-issue laws depends on whether the legal regime before implementing these laws was one of no issue (i.e., no one is permitted to carry concealed firearms) or may issue. The author used a weighted least squares regression of logged crime rates onto a model with state and year fixed effects; linear state-specific trends; and almost two dozen other social, economic, and legal covariates. When looking at violent crime outcomes, Barati (2016) found only a suggestive effect that the transition from no-issue to shall-issue laws caused a reduction in robberies. However, this model had an unfavorable ratio of estimated parameters to observations (about one to six), meaning the model may have been overfit, and its estimates and CIs may thus be unreliable.

Luca, Malhotra, and Poliquin (2017) used data from 1977 to 2014 to evaluate the effects of various firearm laws on homicide rates among adults aged 21 or older. Although the authors’ focus was on background check and waiting-period laws, they included model specifications that additionally controlled for concealed-carry and permitting laws. Their analysis was based on log-linear models adjusting for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors; they found that shall-issue and may-issue laws had uncertain effects on total and firearm homicide rates relative to no-issue regimes. Employing similar models but using data from the FBI’s Uniform Crime Reports over a shorter time frame (1986 to 2015), Hamill et al. (2019) similarly found uncertain effects of adopting a shall-issue or permitless-carry regime on overall rates of violent crime, homicide, rape, and aggravated assault; findings for robbery rates showed suggestive but small decreases associated with moving from a more restrictive to a more permissive concealed-carry regime (see Figure 18.2).

In contrast, using age-adjusted homicide rates and analyzing a shorter time period (1991 to 2015), Siegel et al. (2017b) found that, relative to may-issue laws, shall-issue laws resulted in significantly elevated rates of total homicide and firearm homicide. A shortcoming of the authors’ analysis was that it dropped several years of data for six states after 1998, because the CDC began suppressing homicide counts below ten in that year. Nevertheless, the authors report similar results from sensitivity analyses using a different data source, the Supplementary Homicide Reports database, that does not have the same suppression issues. The authors report using “robust standard errors that account for the clustering of observations, serial autocorrelation, and heteroskedasticity” (p. 1927), but they appear to have used a standard error adjustment that accounted for only heteroskedasticity and not the serial correlation that characterized their state-level panel data. Indeed, in a commentary on this study, Donohue (2017)’s replication of Siegel et al. (2017b)’s analyses produced estimated effects with properly clustered standard errors that were nearly twice as large as those shown in Siegel et al. (2017b)’s main analyses. However, even with the increased uncertainty around the effect sizes,
the estimated effects of shall-issue laws on total and firearm homicide rates remained positive and statistically significant.

Shi and Lee (2018) estimated a panel data model with interactive fixed effects and spatial dependence in order to evaluate how shall-issue or permitless-carry laws affected crime rates from 1977 to 2012. In contrast with most prior studies of the effects of concealed-carry laws, the authors did not estimate regression models that directly controlled for state-level covariates that likely influence firearm legislation and crime rates (e.g., socioeconomic factors, changes in law enforcement resources). Instead, they accounted for (potentially) nonlinear state-specific time trends as a function of unobserved national time trend factors interacted with state-specific factor loadings that determine the degree to which each state was differentially affected by the time trend factors. Their first-differences models also included a lagged outcome variable and covariates to account for potential spatial spillover effects. Their results were mixed. Some outcomes (e.g., robbery) indicated a significant increase immediately after shall-issue law enactment followed by a declining trend, while other outcomes (e.g., murder) showed significant declines but not until more than five years after law passage. Effects on rape rates and assault rates were uncertain or suggestive, depending on when (i.e., how long after implementation) the effect was assessed. However, for both outcomes showing significant effects, the study’s models had an unfavorable ratio of estimated parameters to observations (about one to three for murder rates and one to nine for robbery rates), which suggests that these models may have been overfit and thus produced unreliable estimates and CIs.

Finally, two studies estimated how shall-issue laws affected fatal or nonfatal assaults on police officers (Mustard, 2001; Crifasi, Pollack, and Webster, 2016). Mustard (2001) preferred a spline model, estimating the change in trends before versus after implementation of shall-issue laws for the outcome of felonious police deaths per capita or per full-time equivalent police officer from 1984 to 1996. Across multiple specifications (e.g., Poisson, Tobit), the author tended to find that shall-issue laws had uncertain effects, except when the outcome was measured as police deaths per full-time equivalent officer; in that case, shall-issue laws led to a negative shift in trend that was statistically significant. However, this model had an unfavorable ratio of estimated parameters to observations (about one to seven) and did not account for serial correlation within states, which suggests that the estimated effects and associated CIs may be unreliable. Crifasi, Pollack, and Webster (2016) extended the period of study through 2013 and instead evaluated how shall-issue or permitless-carry laws affected fatal or nonfatal assaults on law enforcement officers, measured as a rate per full-time equivalent officer. The authors found uncertain effects of the laws on fatal assaults but a suggestive effect ($p = 0.13$) consistent with less-restrictive concealed-carry laws resulting in lower rates of nonfatal assault on law enforcement officers.
City-Level Studies

Kovandzic, Marvell, and Vieraitis (2005) examined the effect of shall-issue laws on violent crime (homicide, robbery, assault, and rape) using panel data from 1980 to 2000 for 189 large U.S. cities. The authors clustered the standard errors at the state level, addressed coding errors in previous research, allowed for a time trend in the effect of shall-issue laws, allowed for city-specific time trends, and conducted analyses that allowed for heterogeneity in the effect of shall-issue laws across states. In their analysis that estimated the average effect of shall-issue laws for all included cities using a dummy model specification, Kovandzic, Marvell, and Vieraitis (2005) found uncertain effects for all of the violent crime outcomes analyzed. These findings were largely consistent when they instead modeled the effects of shall-issue laws as a trend variable, except that their preferred spline models showed effects consistent with shall-issue laws increasing assault rates (a significant effect) and increasing rape rates (a suggestive effect). Their estimates for the effect on assault suggest that shall-issue laws were associated with a 10-percent increase in aggravated assault rates after five years. In examining state-specific effects with their spline models, the authors further found that there were more states where shall-issue laws led to statistically significant increases in crime compared with decreases. However, this study had an unfavorable ratio of model covariates to observations (less than one to ten), meaning the model may have been overfit, and thus its estimates and CIs may be unreliable indicators of the true effects of the laws.

La Valle (2013) analyzed data from 56 cities spanning 1980–2010. The author noted that the analyses “include statistical corrections for variation in sample unit independence,” but he did not explicitly mention clustering the standard errors at the state level. La Valle (2013) used a dummy variable specification for the concealed-carry law. In his preferred specification (using interpolated control variables for inter-censal years, population weighted analysis, and a one-year lagged outcome as a covariate), he found that shall-issue laws significantly reduced gun homicides by 15 percent and total homicides by 13 percent (see Figure 18.2). Results were sensitive to specification, however, and other authors (e.g., Kovandzic, Marvell, and Vieraitis, 2005; Durlauf, Navarro, and Rivers, 2016) have expressed concern that weighting gives undue influence to localities with large populations and worsens, rather than improves, standard error estimation. In unweighted analyses using inter-censal years, La Valle (2013) found that shall-issue laws reduced gun homicides but not total homicides. In La Valle and Glover (2012), which used similar data (panel data on 57 cities from 1980 to 2006) and a similar approach, the authors included separate indicators for may-issue and shall-issue states. In the authors’ preferred analysis (with interpolated data for controls for inter-censal years and weighting), shall-issue laws were associated with a significant 23-percent increase in the homicide rate, and may-issue laws were associated with a significant 19-percent decrease in the homicide rate (compared with cities that the authors concluded did not have either a may-issue or shall-issue law). Similarly, shall-issue laws were associated with a significant 32-percent increase in the firearm homicide rate,
while may-issue laws were associated with a significant 33-percent reduction in the firearm homicide rate. (No estimates for unweighted data with inter-censal years were provided.) The diametric findings from these two studies further highlight the sensitivity of results to model specification, as well as to how shall-issue laws are classified.

**Other Studies**

Three studies that focused on the relationship between unmarried fertility or abortions and violent crime included shall-issue laws as a covariate in their models (Donohue and Levitt, 2001; Lott and Whitley, 2007; Kendall and Tamura, 2010). Using data from 1985 to 1997 and estimating weighted least squares with a logged outcome and state and year fixed effects, Donohue and Levitt (2001) found uncertain effects of shall-issue laws on violent crime and murder rates. Analyzing data over a partially overlapping period, from 1976 to 1998, and using a Poisson model that controlled for state and year fixed effects, state-specific linear trends, and time-varying state covariates, Lott and Whitley (2007) found suggestive or significant effects (depending on specification) indicating that murder rates fell approximately 1 percent faster after the adoption of shall-issue laws relative to the rates in states without such policies. Employing a different model specification over a longer period (1957–2002), Kendall and Tamura (2010) estimated that shall-issue laws had a suggestive but small association with reduced rates of murder and uncertain relationships with rates of rape, robbery, and assault.

Zimmerman (2014) extended prior research evaluating the role of private security measures in reducing crime (e.g., see Benson and Mast, 2001). Although the author’s focus was on understanding the crime rate implications of changes in employment within four private security occupation groups (security guards, detectives and investigators, security system installers, and locksmiths), he included shall-issue laws as a covariate in the models to account for the potential deterrent effects of allowing private citizens to carry handguns. Estimating linear models with a logged outcome and controlling for state and year fixed effects, state-specific linear trends, a lag of the dependent variable, and time-varying state characteristics, Zimmerman (2014) found that shall-issue laws led to significantly higher rates of murder and assault; estimated effects on robbery rates were suggestive but also consistent with an increase following the passage of shall-issue laws. However, the analyses had a ratio of estimated parameters to observations of less than one to five, and the paper provided no additional evidence to demonstrate model fit. Therefore, in accordance with our review methodology, we discount this evidence because of the possibility that the model was overfit, and thus the estimated effects and their CIs may be unreliable indicators of the true causal effects of the laws.

Manski and Pepper (2018) investigated the sensitivity of shall-issue effect estimates to a range of assumptions by comparing property and violent crime rates in two states under progressively less-restrictive assumptions about how the laws’ effects
may vary over time or between states. This study compared outcomes in just two states, meaning causal effects were not well identified. Moreover, it treated Virginia’s shall-issue law as having been implemented in 1989, when we believe the correct date is 1995. For these reasons, we do not review this paper’s results. Applying Bayesian model comparison techniques, Strnad (2007) reanalyzed models of the effects of shall-issue laws from Donohue (2004). In contrast to the approach of Donohue (2004) and many others, Strnad (2007) did not assess the evidence for or against shall-issue laws in terms of how frequently estimates of the effect were statistically significant or were found to have positive (as opposed to negative) estimated effects under different model specifications. Instead, he used model comparison techniques to establish which models fit the data best and to evaluate whether evidence favored models with or without shall-issue effects. He concluded that Donohue’s models provided much stronger support for a conclusion that shall-issue laws had little or no effect on most outcomes than Donohue (2004) concluded after examining patterns in the direction and significance levels of these effects. The exceptions were murder, which shall-issue laws appeared to cause to decline gradually, and robbery, which appeared to increase or decrease, depending on the state.

Figures 18.2 and 18.3 display the IRRs and CIs associated with the concealed-carry laws examined in the studies published after the NRC (2004) review. Figure 18.2 displays the studies for which we found no serious methodological issues, and Figure 18.3 displays the studies for which we did find methodological issues. In these figures, we highlight effect estimates based only on dummy-coded models, for reasons discussed in Chapter Two and in the first edition of this report (RAND Corporation, 2018, Appendix A). We exclude the estimates from Zimmerman (2014) for having a ratio of estimated parameters to observations of less than one to five and thus serious potential issues with model overfit. Furthermore, Lott (2010), Shi and Lee (2018), and Moody and Marvell (2018a) did not provide enough information for us to calculate IRRs and CIs for their effect sizes of interest, so we do not include these in the figures. In addition, the estimates in Durlauf, Navarro, and Rivers (2016) were available only for the spline specification; Kovandzic, Marvell, and Vieraitis (2005) preferred their own spline model; Moody and Marvell (2009) and Moody et al. (2014) offered only a hybrid model; and Manski and Pepper (2018) and Strnad (2007) did not seek to produce a preferred estimate of the effect of shall-issue laws. Because we could not readily calculate unique effect sizes and CIs for these studies, we do not include them in the figures.
Figure 18.2
Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with No Serious Methodological Problems

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure (Study Period)</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shall–issue (vs. may–issue or no–issue)</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>Donohue, Aneja, &amp; Weber (2019)</td>
<td>Total (1979–2014)</td>
<td>1.02 [0.92, 1.12]</td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Total (1986–2015)</td>
<td>1.00 [0.92, 1.08]</td>
</tr>
<tr>
<td>Aneja, Donohue, &amp; Zhang (2014)</td>
<td>Total (1979–2010)</td>
<td>1.03 [0.91, 1.17]</td>
</tr>
<tr>
<td>Kendall &amp; Tamura (2010)</td>
<td>Total (1957–2002)</td>
<td>1.00 [0.99, 1.00]</td>
</tr>
<tr>
<td>Hepburn et al. (2004)</td>
<td>Total (1979–1998)</td>
<td>1.01 [0.94, 1.10]</td>
</tr>
<tr>
<td>Donohue, Aneja, &amp; Weber (2019)</td>
<td>Firearm (1979–2014)</td>
<td>1.03 [0.90, 1.16]</td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Firearm (1986–2015)</td>
<td>1.07 [0.97, 1.17]</td>
</tr>
<tr>
<td>Donohue, Aneja, &amp; Weber (2019)</td>
<td>Nonfirearm (1979–2014)</td>
<td>1.02 [0.95, 1.08]</td>
</tr>
<tr>
<td>Violent crime rate</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Total violent crime (1986–2015)</td>
<td>0.99 [0.97, 1.01]</td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Rape (1986–2015)</td>
<td>1.00 [0.97, 1.03]</td>
</tr>
<tr>
<td>Kendall &amp; Tamura (2010)</td>
<td>Rape (1957–2002)</td>
<td>1.00 [0.99, 1.00]</td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Robbery (1986–2015)</td>
<td>0.97 [0.94, 1.01]</td>
</tr>
<tr>
<td>Aneja, Donohue, &amp; Zhang (2014)</td>
<td>Robbery (1979–2010)</td>
<td>1.15 [0.98, 1.34]</td>
</tr>
<tr>
<td>Kendall &amp; Tamura (2010)</td>
<td>Robbery (1957–2002)</td>
<td>1.00 [1.00, 1.00]</td>
</tr>
<tr>
<td>Hamill et al. (2019)</td>
<td>Assault (1986–2015)</td>
<td>0.99 [0.97, 1.01]</td>
</tr>
<tr>
<td>Aneja, Donohue, &amp; Zhang (2014)</td>
<td>Assault (1979–2010)</td>
<td>1.08 [0.99, 1.18]</td>
</tr>
<tr>
<td>Kendall &amp; Tamura (2010)</td>
<td>Assault (1957–2002)</td>
<td>1.00 [1.00, 1.00]</td>
</tr>
<tr>
<td>Assault rate on law enforcement officers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Fatal (1984–2013)</td>
<td>1.02 [0.81, 1.29]</td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Fatal, handgun (1984–2013)</td>
<td>0.92 [0.70, 1.21]</td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Fatal, nonhandgun (1984–2013)</td>
<td>1.27 [0.85, 1.88]</td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Nonfatal (1998–2013)</td>
<td>0.72 [0.47, 1.10]</td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Nonfatal, handgun (1998–2013)</td>
<td>0.74 [0.41, 1.33]</td>
</tr>
<tr>
<td>Crifasi, Pollack, &amp; Webster (2016)</td>
<td>Nonfatal, nonhandgun (1998–2013)</td>
<td>0.74 [0.42, 1.30]</td>
</tr>
<tr>
<td>Shall– or may–issue (vs. no–issue)</td>
<td>Homicide rate</td>
<td></td>
</tr>
<tr>
<td>La Valle (2013)</td>
<td>Total, age–adjusted (1980–2010)</td>
<td>0.87 [0.77, 0.98]</td>
</tr>
<tr>
<td>La Valle (2013)</td>
<td>Firearm, age–adjusted (1980–2010)</td>
<td>0.85 [0.73, 0.98]</td>
</tr>
</tbody>
</table>
NOTE: This figure includes only the studies reporting dummy-coded law effects published since the NRC (2004) review of gun policy effects. For a summary of effects reported for all studies discussed in this chapter, see Appendix B. IRR values marked with green circles indicate that we identified no significant methodological concerns. See Appendix B for details.

**Figure 18.3**
Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with Serious Methodological Problems
Figure 18.3—Continued

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure (Study Period)</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Violent crime rate</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Martin &amp; Legault (2005)</td>
<td>Total violent crime (1977–1992)</td>
<td>0.94 [0.91, 0.98]</td>
</tr>
<tr>
<td>Kovandzic, Marvell, &amp; Vieraitis (2005)</td>
<td>Rape (1980–2000)</td>
<td>1.00 [0.95, 1.04]</td>
</tr>
<tr>
<td>Martin &amp; Legault (2005)</td>
<td>Rape (1977–1992)</td>
<td>0.98 [0.94, 1.03]</td>
</tr>
<tr>
<td>Kovandzic, Marvell, &amp; Vieraitis (2005)</td>
<td>Robbery (1980–2000)</td>
<td>1.01 [0.95, 1.07]</td>
</tr>
<tr>
<td>Martin &amp; Legault (2005)</td>
<td>Robbery (1977–1992)</td>
<td>0.96 [0.91, 1.02]</td>
</tr>
<tr>
<td>Kovandzic, Marvell, &amp; Vieraitis (2005)</td>
<td>Assault (1980–2000)</td>
<td>0.98 [0.94, 1.02]</td>
</tr>
<tr>
<td>Martin &amp; Legault (2005)</td>
<td>Assault (1977–1992)</td>
<td>0.93 [0.89, 0.98]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, &amp; Xu (2013)</td>
<td>Assault injury, aged 0–17 (1988–2003)</td>
<td>2.49 [1.02, 6.08]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Shall–issue (vs. may–issue)</th>
<th>Violent crime rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Barati (2016)</td>
<td>Homicide (1991–2008)</td>
</tr>
<tr>
<td>Barati (2016)</td>
<td>Robbery (1991–2008)</td>
</tr>
<tr>
<td>Barati (2016)</td>
<td>Assault (1991–2008)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Shall–issue (vs. no–issue)</th>
<th>Homicide rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Barati (2016)</td>
<td>Homicide (1991–2008)</td>
</tr>
<tr>
<td>Barati (2016)</td>
<td>Robbery (1991–2008)</td>
</tr>
<tr>
<td>Barati (2016)</td>
<td>Assault (1991–2008)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>May–issue or no–issue (vs. shall–issue)</th>
<th>Homicide rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gius (2014)</td>
<td>Firearm (1980–2009)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>May–issue (vs. shall–issue)</th>
<th>Intimate partner homicide rate</th>
</tr>
</thead>
</table>

<table>
<thead>
<tr>
<th>No–issue (vs. shall–issue)</th>
<th>Intimate partner homicide rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Roberts (2009)</td>
<td>Total (1985–2004)</td>
</tr>
<tr>
<td>Roberts (2009)</td>
<td>Firearm (1985–2004)</td>
</tr>
</tbody>
</table>

NOTE: This figure includes only the studies reporting dummy-coded law effects published since the NRC (2004) review of gun policy effects. For a summary of effects reported for all studies discussed in this chapter, see Appendix B. The estimates from Kovandzic, Marvell, and Vieraitis (2005) are from the authors’ dummy model specification rather than their preferred spline model. IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.
Conclusions

Because so much more study has been done of the relationship between concealed-carry laws and violent crime than of any other gun policy and outcome, there is a much richer evidence base to draw on, including studies raising serious methodological concerns and several that did not raise as many concerns among our methodology review team. Therefore, to focus this review on the best available evidence, we draw our conclusions in this section based just on those 18 studies that did not raise serious methodological concerns. We incorporate all studies that met this criterion in our discussion, but we prioritize findings from studies with a study time frame that extended beyond 2000. We do so because studies omitting more-recent data (1) identify policy effects excluding a large number of states that have enacted shall-issue laws in the past 20 years and (2) have limited post-implementation data to allow these policies to establish their full effects.

Total homicides. Of the 18 studies without serious methodological concerns, 16 examined the effects of shall-issue laws on total homicides, and one examined the effects of the laws on fatal assaults of law enforcement officers. Of the eight studies that evaluated shall-issue laws and included data after 2000, five found only uncertain effects of these laws (Donohue, Aneja, and Weber, 2019; Hamill et al., 2019; Luca, Malhotra, and Poliquin, 2017; Crifasi, Pollack, and Webster, 2016; Aneja, Donohue, and Zhang, 2014). Kendall and Tamura (2010) found small suggestive effects consistent with shall-issue laws reducing homicides. Moody et al. (2014) found that shall-issue laws cause a downward trend in homicides, although a subsequent study that included four more years of data found uncertain effects of the law in seven of eight evaluated years, with a single significant negative effect in the seventh year (Moody and Marvell, 2018a). La Valle and Glover (2012) found that shall-issue laws increased homicides significantly relative to having no law for the legal carriage of a concealed firearm (no-issue laws); and La Valle (2013) found that shall-issue or may-issue laws reduce total homicides relative to no-issue laws. This result cannot be used to distinguish the effect of shall-issue laws per se, but it suggests that shall-issue laws, may-issue laws, or both contribute to reducing total homicides. Of the six studies focused on a period prior to 2000, two found that shall-issue laws caused a downward trend in homicides or murders (Strnad, 2007; Plassmann and Whitley, 2003), one found a suggestive negative effect (Olson and Maltz, 2001), and three found uncertain effects (Hepburn et al., 2004; Helland and Tabarrok, 2004; Ludwig, 1998). Because studies with comparable methodological quality reached inconsistent results, we find that the best available studies provide inconclusive evidence for the effect of shall-issue laws on total homicides.

Firearm homicides. Eight of the 18 studies examined the effects of shall-issue laws on firearm homicides. Among these eight, six evaluated data past 2000, and there was one suggestive (Hamill et al., 2019) and one significant (La Valle and Glover, 2012) effect indicating that these laws increase firearm homicides. La Valle (2013) found that shall-issue or may-issue laws cause decreases in firearm homicide rates relative to
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no-issue laws. This result cannot be used to distinguish the effect of shall-issue laws per se, but it suggests that shall-issue laws, may-issue laws, or both contribute to reducing firearm homicides. The two studies evaluating the longest period (1977 to 2014) found uncertain effects of shall-issue laws on firearm homicides (Donohue, Aneja, and Weber, 2019; Luca, Malhotra, and Poliquin, 2017). One study examined the effects of the laws on fatal handgun assaults of law enforcement officers and found uncertain effects (Crifasi, Pollack, and Webster, 2016). Of the two studies focused on a period prior to 2000, one found that shall-issue laws increase firearm homicides (French and Heagerty, 2008), and the other found that the laws decrease firearm homicides (Olson and Maltz, 2001). With seemingly conflicting evidence, we conclude that the best available studies provide inconclusive evidence for the effect of shall-issue laws on firearm homicides.

Robberies. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase robbery rates, while Hamill et al. (2019) instead found a suggestive effect indicating that shall-issue laws decrease robbery rates. Five studies, the three most recent of which included data after 2000, found largely uncertain effects of shall-issue laws on robberies (Moody and Marvell, 2018a; Moody et al., 2014; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide inconclusive evidence for the effect of shall-issue laws on robberies.

Assaults. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase assault rates, and Moody et al. (2014) found that shall-issue laws were associated with a significant upward trend in assault rates. In contrast, Moody and Marvell (2018a) found suggestive effects consistent with shall-issue laws leading to reduced assault rates, and Crifasi, Pollack, and Webster (2016) found that shall-issue laws had a suggestive negative effect on nonfatal assaults of law enforcement officers. Four studies, including two with data extending past 2000 (Hamill et al., 2019; Kendall and Tamura, 2010), found only uncertain effects of shall-issue laws on assault (Hamill et al., 2019; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide inconclusive evidence for the effect of shall-issue laws on assaults.

Rapes. Aneja, Donohue, and Zhang (2014) found that shall-issue laws significantly increase rates of rape. Moody et al. (2014) found that shall-issue laws produce a significant downward trend on rates of rape. Moody and Marvell (2018a) also found some evidence of significant declines in rape rates, although these effects did not emerge until four years after implementation of the law. Four studies, two of which
included data past 2000, found uncertain evidence of an association between shall-issue laws and rape (Hamill et al., 2019; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide inconclusive evidence for the effect of shall-issue laws on rapes.

**Violent crime.** Two studies (Donohue, Aneja, and Weber, 2019; Durlauf, Navarro, and Rivers, 2016) aggregated all violent crimes into a single category and found that shall-issue laws significantly increase violent crime rates. Three studies, one of which included data past 2000, found uncertain effects of shall-issue laws on overall violent crime (Hamill et al., 2019; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Because evidence for the effect of shall-issue laws on each component of violent crime is inconclusive, it could be argued that these two studies of the effect of these laws on all violent crimes should not suffice to suggest that there is more than inconclusive evidence for such an effect. However, because analyses on all violent crimes may have greater statistical power to detect any such effects, and because our scoring criteria indicate it, we conclude that there is limited evidence that shall-issue laws may increase violent crime.

**Effects on Unintentional Injuries and Deaths**

**Research Synthesis Findings**

We identified two quasi-experimental studies examining the effect of shall-issue concealed-carry laws on unintentional injuries and deaths. Lott and Mustard (1997) examined county-level data on unintentional handgun deaths from national Mortality Detail Records data spanning 1982 to 1991 in counties with and without shall-issue concealed-carry laws. In an ordinary-least-squares regression controlling for arrest rates, population density, and socioeconomic characteristics, shall-issue laws had uncertain effects on unintentional handgun deaths and suggestive effects consistent with increasing unintentional nonhandgun deaths (see Figure 18.4). However, the authors noted that, with only 156 unintentional handgun deaths in counties with more than 100,000 people in 1988, most of the observations in the data set were zeros. They reanalyzed the data using Tobit regression to account for this low number of unintentional deaths but still found uncertain effects, cautioning that, because of computing limitations of the time, they were unable to include covariates other than state dummies in these regressions.
Although Lott and Mustard’s 1997 study has been reanalyzed, including by the NRC (2004) review panel, the focus of most subsequent work has been on the violence and other crime outcomes they examined, not on unintentional deaths (see the previous section on the effects on violent crime).

We identified only one more-recent study meeting our inclusion criteria that examined the effect of shall-issue laws on unintentional injuries (no studies identified the relationship between permitless-carry laws and this outcome). DeSimone, Markowitz, and Xu (2013) performed a fixed-effects analysis to examine the effect of shall-issue laws on unintentional firearm injuries using hospital discharge data from the National Inpatient Sample spanning 1988 to 2003. In the 11 states that were part of the sample, the authors found a suggestive effect consistent with shall-issue laws increasing unintentional firearm injuries for children under age 18 and a statistically significant effect of these laws increasing self-inflicted firearm injuries among those aged 18 or older. Specifically, the estimate suggests that, after a state implemented a shall-issue law, unintentional firearm injuries among those aged 18 or older were more than twice as frequent as would be expected without the law, which would be extraordinary if true. However, the estimated effects of shall-issue laws in this study were based primarily on one state that changed its law during the study time frame (Arizona); thus, the study offers little evidence that the observed effects are due to the change in the law rather than to other factors affecting the state’s unintentional injury rate that occurred around the same time the law was changed.

Figure 18.4 displays the IRRs and CIs associated with the concealed-carry laws examined in these studies.

**Figure 18.4**
Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Unintentional Injuries and Deaths

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shall–issue (vs. may–issue or no–issue)</td>
<td>Unintentional injury rate</td>
<td></td>
</tr>
<tr>
<td>Lott &amp; Mustard (1997)</td>
<td>Handgun deaths</td>
<td>1.00 [0.91, 1.11]</td>
</tr>
<tr>
<td>Lott &amp; Mustard (1997)</td>
<td>Nonhandgun deaths</td>
<td>1.10 [0.99, 1.23]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, and Xu (2013)</td>
<td>Firearm injury, aged 0–17</td>
<td>1.70 [0.83, 3.47]</td>
</tr>
<tr>
<td>DeSimone, Markowitz, and Xu (2013)</td>
<td>Firearm injury, aged 18+</td>
<td>2.28 [1.57, 3.31]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.
Conclusions
We identified two qualifying studies that examined the effect of shall-issue laws on unintentional firearm deaths. Lott and Mustard (1997) found that shall-issue laws had an uncertain relationship with unintentional handgun deaths and a suggestive relationship with increased unintentional nonhandgun deaths. DeSimone, Markowitz, and Xu (2013) found a significant effect indicating that these laws increase unintentional injury rates among adults aged 18 or older and a suggestive effect in the same direction among youth aged 17 or younger.

Considering these studies and an assessment of their relative strengths, we find that there is inconclusive evidence for the effect of shall-issue concealed-carry laws on unintentional firearm injuries and deaths.

Effects on Mass Shootings

Research Synthesis Findings
We identified five studies that examined the effects of shall-issue laws on mass shootings or school shootings. All of these studies adopted state fixed-effects models that exploited state variation in the timing of law enactment to identify the causal effect of these policies on mass shooting incidents.\(^8\)

Using a Poisson specification, Lott (2003) estimated the effect of shall-issue laws on fatalities, injuries, and the incidence of multiple-victim public shootings. The analysis covered 1977 to 1997, and regression models included controls for state and year fixed effects, other state firearm policies, and a broad range of state-level socioeconomic and demographic characteristics. Results showed that shall-issue laws were significantly associated with reductions in total casualties from multiple-victim public shootings and in the total number of multiple-victim public shooting incidents. However, these models

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\(^8\) Gius (2018) specifically examined outcomes related to school shootings, and the other four studies adopted slightly different definitions for mass shooting. Lott (2003) examined multiple-victim public shootings, which the author defined as events unrelated to other criminal activity in which two or more people were killed or wounded in a public location. Duwe, Kovandzic, and Moody (2002) focused on mass public shootings, which the authors defined as incidents resulting in four or more firearm-related fatalities (excluding the offender), where both the victims and offender(s) were not engaged in criminal activities. Blau, Gorry, and Wade (2016) considered mass public shootings, spree shootings with two or more fatalities, and active shooting incidents that may not involve any casualties. Luca, Malhotra, and Poliquin (2016) set the same casualty threshold as Duwe, Kovandzic, and Moody (2002) but excluded any incident that occurred in connection with criminal activity or in which fewer than three of the fatally injured victims were not related to the shooter (e.g., family, romantic partner).
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had an unfavorable ratio of estimated parameters to observations (approximately one to eight), suggesting that the model may have been overfit, and thus the estimated effects of these laws may be poor indicators of their true effects. In addition, the model did not adjust for clustered standard errors. Together, these shortcomings suggest that the model results may not accurately describe the true effects of shall-issue laws.

Duwe, Kovandzic, and Moody (2002) used a fixed-effects negative binomial model—controlling for national time trends, state-level variation in socioeconomic and demographic factors, and state-level criminal justice characteristics (e.g., prison population)—to estimate the effect of these laws on the number of mass public shooting incidents, fatalities from mass public shootings, and injuries from mass public shootings between 1976 and 1999. In their model, shall-issue laws were represented using two separate measures. A step dummy variable that takes a value of 1 the year after the law went into effect (0 otherwise) captured the immediate impact of the law, while a time trend variable captured dynamic effects of the policy. The authors estimated several alternative models, including Poisson fixed-effects models and two dynamic fixed-effects negative binomial models, as specification checks. The findings showed uncertain effects (i.e., no statistically significant evidence) for a relationship between the laws and public mass shooting outcomes (see Figure 18.5). The preferred specification had an unfavorable ratio of estimated parameters to observations (less than one to ten), meaning the model may have been overfit, and thus the estimated effects of these laws may be poor indicators of their true effects.

Examining a partially overlapping but later period (1989–2014), Luca, Malhotra, and Poliquin (2016) used a linear probability model to estimate the impact of shall-issue concealed-carry laws on a binary indicator for whether a mass shooting occurred in a given state-year. Controlling for time-invariant state characteristics, national trends, and a host of other state-level gun policies, as well as time-varying state-level demographic, socioeconomic, and political characteristics, the authors found a small and uncertain effect of shall-issue laws and a large but statistically insignificant positive effect of permitless-carry laws on the probability of a mass shooting event occurring. However, assessing the effects of gun policies on mass shootings was not the primary focus of this paper, and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a rare dichotomous outcome. Therefore, model assumptions were likely violated, making CIs unreliable.

Similar empirical methods, and hence similar methodological concerns, were present in Blau, Gorry, and Wade (2016), who examined data from 1982 to 2013. The authors found that allowing concealed carry resulted in highly imprecise and uncertain effects on the likelihood of a mass shooting incident occurring, with CIs that spanned the range of possible effects. However, the estimated linear probability model in Blau,
Gorry, and Wade (2016) can yield predicted probabilities of active shooting incidence that extend far outside the definitional 0 to 1 range of a probability, depending on the particular combination of policies present in a given state. Moreover, the estimated model implies negative IRRs, which represent implausible effect sizes, for some of the gun policies that we are studying. This indicates a serious model misspecification (Cox and Snell, 1989; Aldrich and Nelson, 1984) and prevents us from interpreting the estimated coefficients as causal effect estimates.

Finally, Gius (2018) employed a population-weighted Poisson model with state and year fixed effects to evaluate how concealed-carry laws, assault weapon bans, and background checks influenced school shooting fatalities or injuries. Using data from 1990 to 2014 and state-level models that adjusted for variation in demographic factors, socioeconomic characteristics, and the ratio of state firearm suicides to total suicides as a proxy for variation in gun ownership prevalence, the authors found that concealed-carry laws had an uncertain relationship with the number of school shooting victims (see Figure 18.5). Given the relatively rare nature of this outcome, these analyses may have been underpowered to detect an effect. These analyses also did not include adjustments to standard errors to account for serial correlation in panel data, which may lead to overstated precision of even this relatively imprecise estimate.

Figure 18.5 displays the IRRs and CIs associated with the concealed-carry laws examined in these studies. Estimates from Duwe, Kovandzic, and Moody (2002) are

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Permitless carry</td>
<td>Mass shooting</td>
<td>2.27 [0, 5.24]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2016)</td>
<td>Any incident</td>
<td>0.91 [0.27, 1.55]</td>
</tr>
<tr>
<td>Shall–issue (vs. may–issue or no–issue)</td>
<td>Mass shooting</td>
<td>0.33 [0.19, 0.58]</td>
</tr>
<tr>
<td>Luca, Malhotra, &amp; Poliquin (2016)</td>
<td>Any incident</td>
<td>0.22 [0.16, 0.29]</td>
</tr>
<tr>
<td>Lott (2003)</td>
<td>Number of incidents</td>
<td>0.09 [0.01, 0.55]</td>
</tr>
<tr>
<td>Lott (2003)</td>
<td>Number of deaths and injuries</td>
<td>0.83 [0.59, 1.16]</td>
</tr>
<tr>
<td>May–issue or no–issue (vs. shall–issue)</td>
<td>School shooting</td>
<td>0.83 [0.59, 1.16]</td>
</tr>
<tr>
<td>Gius (2018)</td>
<td>Number of deaths and injuries</td>
<td>0.83 [0.59, 1.16]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. See Appendix B for details.

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9 For example, the model coefficient for stand-your-ground laws implies an IRR of –2, with a 95-percent CI that lies entirely in the negative IRR range.
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not included in this figure because their approach yielded effect sizes that vary with time. We also exclude estimates of effects from Blau, Gorry, and Wade (2016) because of the aforementioned concerns with their study results.

Conclusions

Permitless-carry laws. We identified one qualifying study that examined the effects of permitless-carry laws on the incidence of mass shootings. Luca, Malhotra, and Poliquin (2016) found that such laws had uncertain effects on the likelihood that at least one mass shooting event occurred in a given state. On the basis of this study, we find inconclusive evidence for the effect of permitless-carry laws on mass shootings.

Shall-issue concealed-carry laws. We identified five qualifying studies that examined the effect of shall-issue laws on mass shooting outcomes. Lott (2003) found that shall-issue laws were associated with significant reductions in multiple-victim shooting incidence and the number of deaths or injuries resulting from multiple-victim shootings. However, the other four studies (Duwe, Kovandzic, and Moody, 2002; Luca, Malhotra, and Poliquin, 2016; Blau, Gorry, and Wade, 2016; Gius, 2018) found uncertain effects of shall-issue laws on mass shooting outcomes (e.g., incidence, injuries, and fatalities). Considering these studies and an assessment of their relative strengths, we find inconclusive evidence for the effect of shall-issue laws on mass shootings.

Effects on the Gun Industry

Research Synthesis Findings

We identified three studies that met our inclusion criteria and examined the effects of concealed-carry laws on the gun industry. Duggan (2001) examined the effect of shall-issue laws on changes in gun ownership, using state-level subscription rates to Guns & Ammo magazine as a proxy for gun ownership. This study identified uncertain effects of these laws on gun ownership.
Steidley and Kosla (2018) examined the effects of shall-issue laws on per capita rates of National Instant Criminal Background Check System (NICS) background checks for firearm purchases as a proxy for firearm demand from 1999 to 2010. Logging this outcome variable, the authors estimated linear models using generalized least squares and adjusting for a linear national trend, state random effects, dummy variables for the South and West U.S. Census regions, and a limited set of time-varying state-level demographic and criminal justice covariates. They also controlled for the ratio of firearm suicides to total suicides as a proxy for firearm prevalence, which may be problematic because some of the effect of shall-issue laws on NICS background checks may occur through an individual’s decision to purchase a firearm. The study found an uncertain relationship between shall-issue or permitless-carry regimes and rates of background checks for firearm purchases.

We identified one study that examined the effects of 2010 legislation in Arizona (State Bill 1108) that removed the permitting requirement to carry a concealed weapon (Ginwalla et al., 2014). The authors found that Arizona’s policy change was associated with a significant increase in the number of NICS background checks conducted in Arizona relative to the United States as a whole (IRR = 1.06; 95-percent CI = 1.05, 1.06). However, this result could have been due to a variety of confounders that were not controlled for in the analyses, such as relatively higher rates of population growth in Arizona, demographic shifts, differential impacts of the Great Recession, or gun laws passed in other states. Given the potential confounders, this study provides relatively little information on how allowing permitless carry might influence demand for firearms.

Figure 18.6 displays the IRRs and CIs associated with the concealed-carry laws examined in Duggan (2001) and Steidley and Kosla (2018). Because the estimate from Ginwalla et al. (2014) had serious limitations in attributing causality to Arizona’s permitless-carry law, we exclude it from the figure.

### Figure 18.6

**Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on the Gun Industry**

<table>
<thead>
<tr>
<th>Study, by Policy</th>
<th>Outcome Measure</th>
<th>IRR [95% CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shall–issue (vs. may–issue or no–issue)</td>
<td>Gun ownership</td>
<td></td>
</tr>
<tr>
<td>Steidley &amp; Kosla (2018)</td>
<td>Background check rate</td>
<td>0.99 [0.89, 1.10]</td>
</tr>
<tr>
<td>Duggan (2001)</td>
<td>Guns &amp; Ammo magazine subscriptions</td>
<td>1.00 [0.98, 1.02]</td>
</tr>
</tbody>
</table>

NOTE: IRR values marked with blue squares indicate that methodological concerns are discussed in the text. Green circles indicate that we identified no significant methodological concerns. See Appendix B for details.
Conclusions

Two studies we identified (Steidley and Kosla, 2018; Duggan, 2001) found an uncertain effect of shall-issue concealed-carry laws on gun ownership proxy outcomes. One study found that allowing permitless carry in Arizona was correlated with significant increases in NICS background checks relative to the national trend. Considering the methodological strengths of these three studies, we find inconclusive evidence for the effect of concealed-carry laws on firearm ownership and purchases.

Outcomes Without Studies Examining the Effects of Concealed-Carry Laws

We did not identify any research that met our inclusion criteria and examined the effects of concealed-carry laws on the following outcomes:

- officer-involved shootings
- defensive gun use
- hunting and recreation.

Several of the studies reviewed here drew inferences about how concealed-carry laws influenced the deterrence and defensive benefits of guns, but none we identified directly examined the laws’ effects on defensive gun use.
Chapter Eighteen References


NRC—See National Research Council.


United States Code, Title 18, Section 926, Rules and Regulations.


Federal and state laws bar most individuals from carrying firearms or other weapons in certain locations. For instance, federal laws prohibit the possession of firearms in federal facilities, other than federal court facilities, except for hunting or other lawful purposes (18 U.S.C. 930). Similarly, firearms are prohibited on property belonging to the U.S. Department of Veterans Affairs (38 C.F.R. 1.218) or the U.S. Postal Service (39 C.F.R. 232.1).

Two federal laws restrict guns in or around schools offering elementary or secondary education. The Gun-Free School Zones Act of 1990 prohibits most firearms within 1,000 feet of a school, but it does not apply to possession by individuals with state licenses (18 U.S.C. 922). In addition, the Gun-Free Schools Act of 1994 applies to schools receiving federal funds and requires the schools to expel for at least one year any student found in possession of a firearm on school property (20 U.S.C. 7961).

Gun-free zones are intended to reduce violent crime, suicides, unintentional firearm injuries and deaths, and mass shootings in specific locations. In theory, the gun-free zone reduces or eliminates the presence of guns in these areas, thereby eliminating the risk of unintentional firearm injuries due to recklessness, escalatory conflicts, or criminal activity. Gun-free zones establish the legal foundation for imposing screening measures, such as bag checks at stadiums or magnetometer screening at some schools or public buildings, that can be used to ensure that fewer or no guns are present in the location.

Alternatively, if the presence or potential presence of armed civilians deters violence, gun-free zones could serve as more-attractive targets to violent criminals or mass shooters because perpetrators will be less likely to encounter armed resistance in these areas. There is debate over the extent to which perpetrators target gun-free zones. One analysis of 133 mass shooting events between 2009 and 2016 found that 10 percent of incidents occurred in designated gun-free zones (Everytown for Gun Safety Support

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1 The law states, “It shall be unlawful for any individual knowingly to possess a firearm that has moved in or that otherwise affects interstate or foreign commerce at a place that the individual knows, or has reasonable cause to believe, is a school zone.” A Supreme Court decision (United States v. Lopez, 514 U.S. 549) ruled the act to be an unconstitutional attempt to legislate under the Commerce Clause of the U.S. Constitution, so the law was amended in 1995 to restrict application to firearms that have moved via interstate commerce.
Fund, 2017b). However, another analysis focused on mass public shootings between 1998 and 2018 and reported that 97.8 percent of incidents took place in gun-free zones (Crime Prevention Research Center, 2018a). While the discrepancy in these estimates is partially due to differences in how mass shootings are defined—the latter study restricts analysis to mass public shootings—there also appears to be some disagreement about how gun-free zones are classified.

To evaluate the effects of gun-free zones, the ideal data would be at fine-enough geographic detail to examine changes in outcomes specifically in areas in which gun-free zones were implemented or removed. However, a nationwide database on gun-free zones does not exist, and different decisions about how to classify these areas can lead to widely differing conclusions. Determining whether a given shooting incident occurred in a gun-free zone requires collecting information on local firearm policies; determining whether the place an incident occurred had a policy of allowing or disallowing firearms; and determining whether it had a means of enforcing that policy, such as bag checks or magnetometer screening.

State Implementation of Gun-Free Zones

As of January 1, 2020, there are a multitude of state laws concerning gun-free zones and who may designate them as gun free. Designated gun-free zones can include government buildings, such as courthouses, airports, and police stations; government land; school properties; or specific private properties open to the public. For example, 39 states have enacted laws banning firearms or concealed firearms in state court buildings, subject to various caveats; and seven states have prohibitions related to the

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3 For example, Colorado’s restriction concerns carrying concealed weapons into public buildings guarded by security personnel (Colo. Rev. Stat. § 18-12-214). Wyoming’s law applies only to courtrooms (Wyo. Stat. § 6-5-209).
general access areas of airports.\(^4\) Some states restrict guns on government land, such as parks and preserves, and some restrict guns on private property open to the public, such as bars and restaurants serving alcohol.\(^5\) (See Giffords Law Center to Prevent Gun Violence, undated-i.)

Many states prohibit guns in schools for kindergarten through grade 12 (K–12). In addition, many states have designated colleges and other postsecondary schools as gun-free zones (Giffords Law Center to Prevent Gun Violence, undated-d). However, in some states, policymakers have decided to allow or encourage the arming of K–12 teachers and staff (see Chapter Twenty, on laws allowing armed staff in K–12 schools).

Another trend running counter to the designation of gun-free zones is the movement of some states to prohibit local authorities from designating certain areas as gun-free zones. Some states have passed laws requiring college and university campuses to allow concealed carry,\(^6\) although some of these states still prohibit, or allow schools to prohibit, guns in particular locations on campus.\(^7\) Idaho removed the authority of the governing bodies of colleges or universities to regulate or prohibit gun possession on campus.\(^8\) Tennessee allows nonstudents to carry concealed weapons on campus.\(^9\) In Colorado, the courts found that only the General Assembly can regulate firearm possession on any college campus, and according to statute, concealed weapons


\(^5\) The regulations on bars and restaurants are a hodgepodge. Some states prohibit both open carry and concealed carry in bars and restaurants, with different restrictions. In Washington, for example, the restriction applies to places that are off-limits to people under age 21 (Wash. Rev. Code Ann. § 9.41.300(1)). In Kentucky, the restriction applies only to loaded firearms and does not apply in restaurants that seat 50 or more people and earn at least half their income from food sales (Ky. Rev. Stat. Ann. § 244.125(1)). Other states, such as Mississippi and Missouri, prohibit concealed carry but allow open carry. These laws also have caveats, which, like Kentucky, relate to trying to keep the firearm regulations restricted to alcohol-centric restaurants or areas of restaurants (Miss. Code Ann. § 45-9-101(13); Mo. Rev. Stat. § 571.107.1(7)). Finally, Illinois, Louisiana, and some other states prohibit open carry but allow concealed carry. Like Kentucky and others, Illinois’s law does not apply in restaurants that seat at least 50 and earn at least 50 percent of their income from food sales; the law also has a further exception, allowing concealed-carry permit holders to “partially expose” their firearms (720 Ill. Comp. Stat. 5/24-1(a)(8); 430 Ill. Comp. Stat. 66/65(a)(9), 66/10(c)(1)). Louisiana allows concealed carry, not open carry, but only in restaurants with a “Class A” permit (La. Rev. Stat. § 14:95.5).


\(^7\) For example, Idaho and Texas.

\(^8\) Idaho Code Ann. § 18-3309.

are allowed on campus; schools may regulate but not ban guns.\textsuperscript{10} Similarly, Oregon’s Court of Appeals ruled that public colleges and universities may not ban weapons on campus grounds,\textsuperscript{11} and Virginia’s Attorney General similarly held that licensed individuals may not be prohibited from carrying firearms in open areas of public universities.\textsuperscript{12} In contrast, Oklahoma granted schools and universities authority to make their own policies concerning guns on campus.\textsuperscript{13}

**Outcomes Without Studies Examining the Effects of Gun-Free Zones**

We did not identify any research that met our inclusion criteria and examined the effects of gun-free zones on the following outcomes:

- suicide
- violent crime
- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.

\textsuperscript{10} Regents of the Univ. of Colorado v. Students of Concealed Carry on Campus, LLC, 271 P.3d 496 (Colo. 2012); Colo. Rev. Stat. Ann. § 18-12-201 et seq.


Chapter Nineteen References

Code of Federal Regulations, Title 38, Section 1.218, Security and Law Enforcement at VA Facilities.

Code of Federal Regulations, Title 39, Section 232.1, Conduct on Postal Property.


United States Code, Title 18, Section 922, Unlawful Acts.

United States Code, Title 18, Section 930, Possession of Firearms and Dangerous Weapons in Federal Facilities.

United States Code, Title 20, Section 7961, Gun-Free Schools Act.
Mass school shootings, like the one in Parkland, Florida, that took the lives of 14 students and three staff in February 2018, are rare in comparison with other types of gun violence in the United States, but they have encouraged an active policy debate and legislation concerning the role of firearms in ensuring school safety. Recently, for instance, the Marjory Stoneman Douglas High School Public Safety Commission (2019) recommended that every middle school and high school in Florida should have an armed school resource officer and that a program allowing for trained teachers to carry concealed weapons should be greatly expanded. A similar recommendation was made in a Federal Commission on School Safety (2018) report. In contrast, some law enforcement officials, the National Education Association, the American Federation of Teachers, and gun safety advocacy organizations (e.g., Everytown for Gun Safety Support Fund) strongly oppose arming teachers (Everytown for Gun Safety Support Fund, National Education Association, and American Federation of Teachers, 2019).

As discussed in Chapter Nineteen (on gun-free zones), two federal laws restrict who may carry guns in or around schools offering kindergarten through grade 12 (K–12) education: the Gun-Free School Zones Act of 1990 (18 U.S.C. 922) and the Gun-Free Schools Act of 1994 (20 U.S.C. 7961). These laws do not prohibit all people from carrying guns in schools, however. Law enforcement officers and individuals with valid state-issued concealed-carry permits are exempted from the laws’ prohibitions (18 U.S.C. 922(q)(2)(B)(ii)). Furthermore, gun owners can legally keep their firearms in a locked container or a locked firearm rack in a car on school grounds, and schools can allow individuals to carry firearms on campus for use in an approved program or in accordance with a contract entered into between a school and the individual (18 U.S.C. 922(q)(2)(B)(iv), (v); 922(q)(3)(B)(ii), (iii)). State and local laws and school district policies often further restrict whether law enforcement officers, properly licensed teachers, or others may carry firearms at primary and secondary schools.

Those who argue in favor of arming either teachers or law enforcement officers—often called school resource officers—contend that, without guns, teachers or other staff have only limited countermeasures available to them when confronted with a shooter. They can run or hide, but fighting a shooter without a gun can require sacrificing one’s own life to protect others. In addition, with more armed adults, effective response
might be brought to bear more quickly. At Parkland’s Marjory Stoneman Douglas High School, for instance, a school resource officer reached the school building under attack within 99 seconds of the first shot being fired, but 21 people had already been shot by then, nine fatally. The commission investigating this shooting concluded, “This makes clear that seconds matter and that [school resource officers] cannot be relied upon as the only protection for schools. Even if there is a rapid response by an [officer], it is insufficient in and of itself in safeguarding students and teachers” (Marjory Stoneman Douglas High School Public Safety Commission, 2019, p. 978). Finally, just the knowledge that teachers could be armed may deter some would-be shooters.

Arguments against arming teachers and school resource officers highlight the elevated risk of accidents and negligent use of firearms as more adults in schools are armed. The Associated Press reported, for instance, that there were more than 30 incidents between 2014 and 2018 that involved a firearm brought to a school by a law enforcement officer or that involved a teacher improperly discharging or losing control of a weapon (Penzenstadler, Foley, and Fenn, 2017). This compares with around 20 active-shooter attacks at schools over a comparable period (Cai and Patel, 2019). When even trained police officers have been found to successfully hit their intended targets in just 18 percent of incidents involving an exchange of gunfire (Rostker et al., 2008), critics question whether teachers can be expected to effectively return fire without inadvertently injuring the children they mean to protect (Vince, Wolfe, and Field, 2015). Finally, if teachers are holding guns or engaged in gunfire, it may make the job of law enforcement officers more difficult and dangerous when they arrive at the scene. Officers could mistake the teacher for an active shooter or could themselves be inadvertently shot by the teacher.

Data capturing firearm use on school campuses suggest that rates of violence on K–12 campuses have decreased substantially in the past few decades. Since the introduction of federal regulations related to guns on school property, rates of students carrying weapons in general and to school have decreased. In 1993, 22 percent of students in grades 9–12 carried a weapon, such as a gun or knife, in general; in 2017, about 16 percent of students carried a weapon in general (Musu et al., 2019). The percentage of students carrying weapons to school has also decreased. In 1993, 12 percent of students reported carrying a weapon on school property during the previous 30 days; in 2017, only 4 percent of students reported bringing a weapon to school (Musu et al., 2019).

Although discussions about firearm deaths of children are often dominated by discussions about mass school shootings, these events are relatively rare. Research finds that most students killed with firearms are shot in their own homes, typically because of a domestic dispute, accidental or negligent discharge of a gun, or suicide (Fowler et al., 2017). Deaths that take place on school property make up a small portion of all violent deaths among youth aged 5–18. Between 2015 and 2016, 1.2 percent of youth homicides and 0.2 percent of youth suicides took place on school property (Musu et al.,
Therefore, even policies that effectively reduce gun violence on K–12 campuses are likely to produce only a small absolute change in the overall rates of injury and may, therefore, be difficult to reliably detect.

It is unclear how policies increasing the number of authorized guns in schools would affect the gun industry. It is possible that allowing school resource officers, teachers, and other adults to carry weapons on K–12 campuses could create a new demand for specialized training or practice. However, surveys suggest that most teachers are opposed to bringing guns into schools, so uptake on teacher firearm certification may be low (Brenan, 2018).

Whether arming teachers and school resource officers leads to net harms or benefits is an empirical question that could be addressed with strong scientific research designs. Estimating these impacts with observational designs is complicated by the fact that state statutes often dictate that decisions around carrying a firearm in K–12 schools are made at the level of the school district or individual school. However, there is no comprehensive accounting of the extent to which school districts allow teachers or school personnel to carry guns, and this can vary widely across states (Lott, 2019; Richmond, 2019). Unlike many state laws for which experimental control over exposure to the effects of the law would never be feasible, states or possibly even school districts could possibly conduct randomized controlled trials to evaluate the effects of some school policies on gun violence, suicides, mass shootings, and accidental injuries. Nevertheless, an important obstacle to the success of such an experiment would be the low base rate of gun violence experienced by schools in the United States. Thus, it may be difficult to detect the effects of these policies unless many schools were included in the experiment over what might be several years of data collection.

State Implementation of Laws Allowing Armed Staff in K–12 Schools

As of January 1, 2020, 28 states allow schools to arm teachers or staff in at least some cases or as part of a specific program. In some of these states, such as Arkansas and Colorado, there are no statutes allowing armed school personnel but also no laws

explicitly prohibiting it, and state policymakers have decided to allow or encourage the arming of teachers, in sometimes innovative ways. In Arkansas, for example, the carrying of firearms on school grounds is prohibited, but there is an exception for law enforcement officers and “registered commissioned security guards.” Some school districts, claiming a lack of resources to hire conventional guards, have “obtained licenses from the Arkansas Board of Private Investigators and Private Security Agencies . . . designating school employees as security guards and allowing them to carry firearms on campus” (Keller, 2014, p. 688).

In contrast, several states, such as Missouri and Montana, have laws that do explicitly authorize teachers or other staff to be armed. The states differ with respect to who can be armed and under what circumstances. Alabama’s sentry program, for example, allows administrators in schools without a school resource officer to maintain and use authorized weapons (Ivey, 2018). Texas has a school marshal program, in which the board of trustees of the school district or governing body of a charter school may appoint one marshal for every 200 students or for each building of the campus, subject to various other rules. In distressed rural counties in Tennessee, schools may implement policies allowing the selection of certain employees to carry concealed weapons. Several states, including Colorado, Montana, and Ohio, allow armed teachers if the school district or charter school allows it. Other states, such as Indiana, allow individuals (including teachers) who have been specifically authorized by the school board to carry firearms on school property. Five states allow any individual with a concealed-carry permit to carry a gun into a K–12 school, and Wyoming allows school employees with such permits to carry a gun on school grounds. Relatively, some states allow any licensed concealed-carry permit holders who have been authorized by the school district or other relevant authority (as in Idaho) or enhanced permit holders performing their official duties (as in Mississippi) to carry weapons onto school property.

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4 Tenn. Code Ann. § 49-6-816.
5 Hernandez, 2018; Mont. Code Ann. § 45-8-361; Ohio Rev. Code § 2923.122(B).
8 Wyo. Stat. 21-3-132.
Outcomes Without Studies Examining the Effects of Laws Allowing Armed Staff in K–12 Schools

We did not identify any studies that met our inclusion criteria and examined the relationship between laws allowing armed staff in K–12 schools and the following outcomes:

- suicide
- violent crime
- unintentional injuries and deaths
- mass shootings
- officer-involved shootings
- defensive gun use
- hunting and recreation
- gun industry.
Chapter Twenty References


Crime Prevention Research Center, “States That Allow Teachers and School Staff to Carry Guns,” October 9, 2018b.


Lott, John R., Jr., “Schools That Allow Teachers to Carry Guns Are Extremely Safe: Data on the Rate of Shootings and Accidents in Schools That Allow Teachers to Carry,” Alexandria, Va.: Crime Prevention Research Center, April 25, 2019.


United States Code, Title 18, Section 922, Unlawful Acts.

United States Code, Title 20, Section 7961, Gun-Free Schools Act.

PART E

Summary of Findings and Recommendations
Although large majorities of Americans agree on the merits of some gun policies, gun policy is divisive in the United States. In this report, we have attempted to provide a rigorous and balanced assessment of what current scientific knowledge can tell the public and policymakers about the true effects of many gun policies that are frequently discussed in state legislatures. Comprehensive reviews that preceded our study, conducted more than a dozen years ago, found the research base too thin to draw any conclusions about the effects of gun laws. Specifically, a committee of the National Research Council (NRC) found that the evidence was so weak and contradictory that no causal associations between the laws it examined and crime or violence could be determined (NRC, 2004). Separately, the Community Preventive Services 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” (Hahn et al., 2005).

We have thoroughly updated and expanded on the findings in NRC (2004) and Hahn et al. (2005) with studies published between 1995 and fall 2018. We systematically reviewed all empirical research that examined the effects of 18 types of state gun policies on eight outcomes, including outcomes related to public health and safety and outcomes of interest to sport shooters, hunters, and those who work in the gun industry. We restricted our analysis to only those studies using methods designed to identify plausibly causal effects of the policies. After reviewing many thousands of candidate studies, we identified 123 meeting our inclusion criteria (described in Chapter Two), of which 93 were not superseded by other later studies by the same authors.

This is the second edition of our review of this literature, and it includes studies conducted over a longer span of time. Therefore, some of the conclusions we drew about the evidence in the first edition of this report (RAND Corporation, 2018) have been updated or modified in this report. For a comparison of changes in our evidence ratings between the first and second editions of this review, see Appendix A.

There is a need for a factual basis on which to make policy. This does not mean basing decisions just on facts about which policies will reduce homicides or suicides the most; it means basing decisions on an accurate understanding of the trade-offs that policies entail. To make fair and effective gun policies, we need to know more about
their implementation challenges, whom they affect most or least, what their unintended consequences might be, how they can be revised for better effect, what they cost society in general and individual stakeholder groups in particular, and other issues central to the acceptability of any policy. These scientific questions about what is true and knowable do not supersede questions of individual rights or Second Amendment rights. Both should be central considerations in policymaking.

Facts have never dictated policy, but they can inform it. The relevance of research to inform gun policy has been tarnished by deeply held assumptions about “true” policy effects, measurement error associated with key variables (such as gun ownership; see Cook, 2013), skepticism about research methods, and mistrust of researchers’ motives when they draw unwelcome conclusions or focus on just one aspect of what is a complex phenomenon affecting multiple stakeholders with diverse interests. We have attempted to address these concerns through the rigor and transparency of our methods and through our organizational commitment to nonpartisan, objective policy analysis. We hope, therefore, that all stakeholders in gun policy debates give our analysis of the available science a fair hearing and our recommendations careful consideration.

In this chapter, we summarize our judgments about the strength of evidence available for the effects of gun policies on outcomes of interest. We then outline our conclusions and recommendations, which are organized into two sections: What can we conclude about the effects of gun policies, and why don’t we know more?

**Summarizing the Strength of Evidence**

We categorized all policy and outcome pairings as having supportive, moderate, limited, inconclusive, or no evidence. We can never conclude that evidence shows that a policy has no effect. Even when multiple studies fail to find a significant effect, it is not correct that this implies that the policy has no effect. Instead, the effects may simply be too small to reliably detect, or the data available to assess the policy’s effects may not be sufficiently specific to the intended effects of the law. More generally, it seems reasonable to suspect that every policy has some effect on each outcome, however small or unintended. Therefore, the failure to detect a law’s effects reveals more about the weakness of the analytic methods than about the possibility that a policy truly has no effect.

We categorized evidence as *inconclusive* 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. We categorized evidence as *limited* 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. Effects for which there is *moderate* evidence are those for
which 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. Our finding of supportive evidence of an effect is limited to cases for which at least three studies not compromised by serious methodological weaknesses found suggestive or significant effects in the same direction, and the effect was found in at least two data sets that were reasonably independent of each other (e.g., firearm suicides and hospital admissions for self-inflicted firearm injuries).

Our ratings, therefore, reflect the relative strength of evidence, not, for instance, whether the evidence is strong enough that we can be highly confident that observed effects would be generalizable to future implementations of a particular law. Rather, evidence for these effects is strong relative to evidence for other gun policy effects and not necessarily strong relative to the quality and quantity of evidence available in other fields of study. For instance, the evidence that cigarette smoking causes cancer is vastly stronger than the evidence concerning any gun policy’s effect on any outcome.

Table 21.1 summarizes our evidence ratings for all 18 classes of policies across the eight outcomes. Several outcomes show multiple judgments, and these correspond to different characterizations of the specific policy-outcome association. For instance, we identified moderate evidence that dealer background checks reduce firearm homicides but inconclusive evidence for how private-seller background checks affect firearm homicides. Looking across the columns, it is apparent that research into five outcomes is either unavailable or almost entirely inconclusive. It is noteworthy that three of these five outcomes—defensive gun use, hunting and recreation, and the gun industry—are issues of particular concern to gun owners or gun industry stakeholders, such as firearm manufacturers, firearm dealers, hunting outfitters, and firing ranges. The lack of research on a wide range of outcomes makes it difficult or impossible to conduct a comprehensive cost-benefit analysis of the gun policies. For instance, some of the strongest evidence we found suggests that child-access prevention laws could reduce firearm injuries or deaths among children. But restricting access to guns could also prevent gun owners from accessing their weapons in an emergency. The lack of research on defensive gun use means that we do not have a way of directly estimating how the benefits of these laws (in terms of number of child lives saved) compares with the possible costs (in terms of forgone opportunities for self-defense).
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Child-Access Prevention Laws

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Laws Allowing Armed Staff in
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Permitless Carry

Prohibitions Associated with
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Firearm selfinjuries

Shall Issue

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Bans on the Sale of Assault
Weapons and High-Capacity
Magazines

Purchasing

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Outcome

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Firearm homicides

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Firearm suicides

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Total homicides

ConcealedCarry Laws
Minimum Age
Requirements

332

Table 21.1
Strength of Evidence Across Gun Policies and Outcomes


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<th>Hunting and recreation</th>
<th>Defensive gun use</th>
<th>Officer-involved shootings</th>
<th>Mass shootings</th>
<th>Unintentional firearm deaths and deaths among children</th>
<th>Unintentional firearm injuries and deaths among adults</th>
<th>Outcome</th>
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NOTE: I = inconclusive; L = limited; M = moderate; S = supportive. When we identified no studies meeting eligibility criteria, cells are blank. 

- The policy decreases the outcome. 
- The policy increases the outcome. 
- The policy decreases the outcome. 
- The policy increases the outcome.

We concluded that there is limited evidence that licensing and permitting requirements decrease total suicides among adults and inconclusive evidence for the effect of such laws on firearm suicides among young people.

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### Table 21.1—Continued

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**Table Notes Continued**

*c* We concluded that there is moderate evidence that dealer background checks decrease firearm homicides, and there is inconclusive evidence for the effect of private-seller background checks on firearm homicides.

d We found inconclusive evidence for the effect of assault weapon bans on firearm homicides, as well as inconclusive evidence for the effect of high-capacity magazine bans on firearm homicides.

e We concluded that there is moderate evidence that laws establishing firearm prohibitions for individuals subject to domestic violence restraining orders decrease total and firearm-related intimate partner homicides, there is limited evidence that prohibitions associated with stalking misdemeanors increase total intimate partner homicides, and there is inconclusive evidence for how firearm prohibitions for those convicted of stalking or of misdemeanor domestic violence affect total and firearm-related intimate partner homicides.
What Can We Conclude About the Effects of Gun Policies?

Our first set of conclusions and recommendations describes the policy-outcome combinations with the strongest available evidence as identified through our review of the existing literature, as well as recommendations for policy based on this evidence.

**Conclusion 1.** Available evidence supports the conclusion that child-access prevention (CAP) laws, or safe storage laws, reduce self-inflicted fatal or nonfatal firearm injuries, including unintentional and intentional self-injuries, among youth. There is moderate evidence that these laws reduce firearm suicides among youth and limited evidence that the laws reduce total (i.e., firearm and nonfirearm) suicides among youth. In addition, there is limited evidence that these laws may reduce unintentional firearm injuries and deaths among adults. There is some evidence that felony CAP laws have the greatest effects on unintentional firearm deaths.

In the available literature examining CAP laws, self-inflicted injuries represent an ambiguous outcome because not all self-inflicted firearm injuries are the result of a suicide attempt. Some are unintentional injuries. But because case fatality rates for suicide attempts with a firearm are between 83 and 91 percent (Azrael and Miller, 2016; Conner, Azrael, and Miller, 2019; Spicer and Miller, 2000), some self-inflicted firearm injuries are likely the result of a suicide attempt. Furthermore, there is a clear pattern of CAP laws appearing to reduce a range of related firearm injuries to youth, from unintentional injuries to suicides. That they also reduce the more ambiguous “self-inflicted injuries” fits squarely within that pattern and contributes to our confidence that the evidence currently supports a conclusion that CAP laws reduce these injuries and fatalities.

Across all of the 18 classes of policies that we studied, only CAP laws and stand-your-ground laws had any evidence that we classified as supportive for a particular conclusion. CAP laws differ from many of the other policies we considered in this report. Most of the others affect the acquisition of new firearms (e.g., background checks or waiting periods), or they are designed to affect a relatively small proportion of gun owners (e.g., prohibitions that target the mentally ill or domestic violence offenders). Thus, the other laws generally concern either the small proportion of guns that are newly acquired every year or a small proportion of the gun-owning population. CAP laws, in contrast, are designed to influence how all guns in a state are stored when children could be expected to encounter them. This likely represents a large proportion of all guns because one-third of all households in the country have children under age 18 (Vespa, Lewis, and Kreider, 2013), and many more have children as occasional visitors. With such large numbers of guns potentially affected, even imperfect compliance with CAP laws may have a greater chance than other types of laws of producing observable effects in population-level outcome statistics.

- **Recommendation 1.** States without CAP laws should consider adopting them as a strategy to reduce firearm suicides and unintentional firearm injuries and deaths.
• **Recommendation 2.** When adopting or refining CAP laws, states should consider making child access to firearms a felony.

Gun industry and gun-owner organizations have promoted voluntary and educational programs to promote the safe storage of firearms. Our conclusions and recommendations should not be interpreted to suggest that only CAP laws can reduce firearm deaths. Scientific evaluations of education campaigns have found that they can produce behavior change in domains other than gun storage, but rigorous evidence that they have successfully promoted safe storage of firearms is limited. On the other hand, there is evidence that clinicians who counsel patients (mostly families with children) can effectively promote safe storage practices, particularly if storage devices (e.g., gun locks) are provided along with the counseling (RAND Corporation, 2018, Chapter Twenty).

**Conclusion 2.** There is supportive evidence that stand-your-ground laws are associated with increases in firearm homicides and moderate evidence that they increase the total number of homicides.

In the first edition of this report, we found only limited or moderate evidence for the effect of stand-your-ground laws on total and firearm homicides, but four new studies meeting our inclusion criteria have since been published, and all of these suggest that stand-your-ground laws elevate homicide rates. Because these laws are designed to empower victims of crime to defend themselves more effectively, it might be suggested that the elevation in homicide rates is an intended effect of the law, if the increases were driven by a surge in justifiable homicides. But that does not appear to be the case. The significant incidence rate ratios detected for total and firearm homicide rates range from 1.07 to 1.22, which are effects too large to be explained by justifiable homicides. Consider, for instance, that there were a combined 2,201 firearm homicides in 2017 in Florida and Texas, two states with stand-your-ground laws (Centers for Disease Control and Prevention [CDC], undated-a). If the effect size estimates for stand-your-ground laws are correct, then between 144 and 396 of these deaths could be attributable to the law. But across the United States, there are only about 230 justifiable homicides recorded in the Federal Bureau of Investigation (FBI)’s Supplementary Homicide Reports annually (Violence Policy Center, 2018), so many of the additional homicides attributable to the law must be criminal homicides.

More than two-thirds of all states currently have stand-your-ground laws. The fact that there is now supportive evidence that these laws increase firearm homicides is concerning. These laws were adopted on the belief that they would reduce violent victimization either by deterring violent crime or by reducing barriers to victims defending themselves. On the existing evidence, these laws are actually making people less safe and instead more likely to be the victim of a firearm homicide.

• **Recommendation 3.** States with stand-your-ground laws should consider repealing them as a strategy for reducing firearm homicides.
**Conclusion 3.** There is moderate evidence that state laws establishing firearm prohibitions for individuals subject to domestic violence restraining orders decrease total and firearm-related intimate partner homicides, but there is inconclusive evidence for how such prohibitions for those convicted of stalking and misdemeanor domestic violence affect these outcomes.

State domestic violence protection orders are often broader than the federal laws, so they can be applied in more cases. For instance, federal law does not cover most intimate partners who are not a spouse, a coparent, or a cohabitant, but many state laws cover boyfriends, for instance, who neither live with the victim nor meet the other federal criteria. In addition, these laws sometimes include requirements for those under domestic violence restraining orders to surrender their firearms. States also vary in how reliably they submit information about domestic violence restraining orders to state and federal databases used to conduct background checks.

Domestic violence represents one of the largest categories of gun violence in the United States. Almost half of all women who were murdered in the United States between 2003 and 2014 were killed by a man who was their current or former intimate partner (Petrosky et al., 2017). Of almost 4,000 women murdered in the United States in 2017, more than half were killed with firearms (CDC, undated-a). In this context, the fact that there is now moderate evidence that domestic violence restraining orders and their associated prohibitions against gun ownership reduce total and firearm-related intimate partner homicides is noteworthy.

- **Recommendation 4.** States without laws prohibiting gun ownership while individuals are subject to domestic violence restraining orders should consider passing such laws as a strategy for reducing total and firearm-related intimate partner homicides. States should consider the possibility that these laws may be most effective when they can be applied to a wide range of domestic violence cases and when the law ensures that information about the cases is included in databases used to conduct background checks.

**Conclusion 4.** There is moderate evidence that background checks reduce firearm homicides. Most available studies have examined the effects of dealer background checks or the combined effects of dealer and private-seller background checks when both are required by a state. Therefore, the evidence base for universal background checks compared with the dealer background checks required under federal law is quite limited. However, if performing background checks on a subset of firearm transfers causes reductions in homicides, then extending the practice to all firearm transfers, including private sales, could further reduce firearm homicides. We emphasize, though, that there currently is not strong scientific research that evaluates this inference.
Conclusion 5. There is moderate evidence that waiting periods reduce firearm suicides and total homicides and limited evidence that they reduce total suicides and firearm homicides.

- Recommendation 5. States without waiting-period laws should consider adopting them as a strategy for reducing suicides and homicides.

Conclusion 6. There is limited evidence that licensing and permitting requirements for purchasing a firearm reduce total suicides and firearm suicides among adults. The evidence for the effect of these laws on violent crime remains inconclusive.

Of the 18 policies we evaluated, the two policies with the strongest evidence for reducing total and firearm suicides among the overall population (i.e., not only youth) both operated by regulating the processes around purchasing firearms. The strongest evidence was found for waiting periods, which have a strong conceptual link to firearm suicide risk, given evidence that (1) the risk of firearm suicide is substantially elevated in the days and weeks after a firearm is acquired (Wintemute et al., 1999); (2) many suicide attempts appear to be impulsive, with a very short interval of time between ideation and attempt (Lewiecki and Miller, 2013; Simon et al. 2002); and (3) the majority of individuals who survive a suicide attempt have subsequently low completion rates (Cox et al., 2013; Miller et al. 2013a; Owens, Horrocks, and House, 2002; Sakinofsky, 2000). We also found limited evidence that permit-to-purchase requirements, which often include a waiting period as part of the permitting process, reduce total and firearm suicides among adults.

Conclusion 7. There is limited evidence that laws prohibiting the purchase or possession of guns by individuals with histories of adjudicated mental illness or incapacity reduce violent crime.

Federal law prohibits some people who have been adjudicated as mentally ill from purchasing or possessing firearms, but this prohibition is not uniformly enforced across the nation. States maintain mental health records, but many have been reluctant to share those records for use in the FBI’s National Instant Criminal Background Check System (NICS), the federal database used for background checks. Although most states have laws allowing for the voluntary sharing of some mental health records with NICS, there is considerable variation in which classes of individuals prohibited under federal law are shared with NICS. Thus, by the end of 2018, there were large differences in the number of active mental health records in NICS across states; for example, Alaska, Montana, New Hampshire, Rhode Island, and Wyoming had contributed fewer than 1,000 records, whereas most other states had tens or hundreds of thousands of active mental health records in the database (Criminal Justice Information Services Division, 2018).

Our finding that there is limited evidence that some mental health–related background checks can reduce gun violence should be of interest to states that currently
share only partial or limited mental health data with NICS and that do not have a comprehensive in-state database that is reliably used for background checks for firearm sales. It is likely that many individuals with mental health histories making them prohibited possessors under federal law can nevertheless purchase firearms in these states. Moreover, states that do check state databases but do not share information on all individuals with disqualifying mental health histories with NICS create opportunities for prohibited possessors to purchase firearms out of state. Establishing procedures to prevent these people from purchasing firearms appears to yield small but appreciable reductions in suicides, homicides, and other violent crimes after implementing mental health checks.

- **Recommendation 6.** States that currently do not require a background check investigating all types of adjudicated mental health histories that lead to federal prohibitions on firearm purchase or possession should consider implementing robust mental health checks, which may reduce rates of gun violence. The most robust procedures involve sharing data on all prohibited possessors with NICS.

**Conclusion 8.** There is limited evidence that shall-issue, or right-to-carry, laws increase violent crime rates.

There has been more research on shall-issue laws than on all the other policies we examined combined, yet the evidence remains inconclusive for how these laws affect total and firearm suicides, total and firearm homicides, and all other outcomes that have been examined other than overall rates of violent crime. This lack of conclusive evidence could be because the types of violence most likely to be deterred or exacerbated by shall-issue laws are those that occur outside the home. Because this subset is not readily identifiable in most gun violence data sets, nearly all studies we reviewed looked at the effects of these laws on total numbers of, for instance, firearm suicides or homicides. Aggregating the most relevant events with data on events that are unlikely to be directly affected by shall-issue laws may make detection of their true effects difficult.

Because the presumed mechanism of effect for both shall-issue and permitless-carry laws is the same (reducing the barriers to people lawfully carrying concealed firearms), then, if it is true that shall-issue laws increase violent crime rates, it may also be the case that permitless-carry laws have the same effect. That said, we found few studies directly examining the effects of permitless-carry laws and none that provided better than inconclusive evidence for their effects.

**Conclusion 9.** There is limited evidence that before implementation of a ban on the sale of assault weapons and high-capacity magazines, there is an increase in the sales and prices of the products that the ban will prohibit.

This finding is based on persuasive evidence from a single case, the implementation of the Violent Crime Control and Law Enforcement Act of 1994, which banned
the sale of certain semiautomatic weapons designated in the law as assault weapons and prohibited most ammunition-feeding devices with a capacity to accept more than ten rounds of ammunition. Therefore, this finding may not generalize well to other instances of assault weapon bans or to other instances of high-capacity magazine bans. For example, the 1994 law grandfathered banned weapons sold before the law’s implementation date. This likely created a market for speculators who drove up sales and prices in the months preceding the ban (Koper, 2004).

**Conclusion 10.** There is limited evidence that a minimum age of 21 for purchasing firearms may reduce firearm suicides among youth.

**Conclusion 11.** No studies meeting our inclusion criteria have examined the effects of lost or stolen firearm reporting requirements, gun-free zones, or laws allowing armed staff in K–12 schools. Only inconclusive evidence exists for the effects of minimum age of possession laws; firearm-surrender laws; extreme risk protection orders, or “red-flag” laws; firearm safety training requirements; firearm sales reporting, recording, and registration requirements; bans on low-quality handguns; and permitless-carry laws.

### Why Don’t We Know More?

Considering the findings from our review of the existing literature on the effects of firearm policy changes, we offer the following conclusions and recommendations for improving the evidence base on the effects of gun laws.

**Conclusion 12.** The modest growth in knowledge about the effects of gun policy since 1995 reflects, in part, the past reluctance of the U.S. government to sponsor work in this area at levels comparable to its investment in other areas of public safety and health, such as transportation safety or opioid overdoses.

Of the 123 studies meeting our inclusion criteria that have been published since 1995, just 21 (17 percent) reported receiving any federal funding, and many of these were early in the period; fewer than 10 percent of studies published after 2003 were federally funded. The same number (21) received support from private foundations: Of foundations that supported more than one study, the Joyce Foundation supported 11, the Robert Wood Johnson Foundation supported four, and the Sloan and Soros Foundations each supported two. The large majority of studies we reviewed (84, or 68 percent) reported no extramural funding.

While most of the 123 studies focused on public safety or health outcomes (e.g., suicide and homicide), the number of high-quality quasi-experimental studies on which to base estimates of the effects of policies was surprisingly small compared with the literatures that evaluate the effects of many other policies, such as those designed to improve traffic safety, a problem that claims about as many lives each year as are lost in firearm suicides and homicides.
Federal funding for research on gun-related mortality has been far below the levels for other sources of mortality in the United States. Stark and Shah (2017), for instance, found that federal gun violence research funding is just 1.6 percent the amount predicted based on federal funding for other leading causes of death. With this federal inattention comes a corresponding deficit in research: Stark and Shah (2017) also found that the volume of research publications on gun mortality was just 4.5 percent of what would be expected based on publication volume for other leading causes of mortality.

The federal government previously supported a more robust program of research examining firearm violence and policy. In the 1990s, the CDC was sponsoring millions of dollars of research on firearm violence, until researchers found that having a gun in the home was associated with an elevated risk of firearm homicide for members of the household. This finding was viewed by some as a one-sided attempt to manipulate the gun policy debate.

In an effort led by the National Rifle Association (Cagle and Martinez, 2004), a sufficient proportion of Congress was persuaded to adopt the Dickey Amendment in 1996 and cut $2.6 million of funding from the CDC, an amount equal to what its injury prevention center had been spending on gun violence research. The Dickey Amendment also introduced new language forbidding the CDC from advocating or promoting gun control. This language did not explicitly prohibit all research on gun violence or gun policy, but concern that any gun research could be viewed as advocacy has led the CDC to avoid supporting gun policy research lest it invite a budget adjustment like that in 1996 (Kellermann and Rivara, 2013).

Congress has included Dickey Amendment language in each CDC appropriations bill since 1996. Moreover, in 2012, similar language was added to an appropriations bill for the National Institutes of Health in the Consolidated Appropriations Act of 2012 (Pub. L. 112-74).

Without significant federal investment, research on firearm policy and violence prevention has languished. According to a report by the advocacy organization Mayors Against Illegal Guns, by 2012, CDC funding of gun violence research had declined 96 percent since the mid-1990s, and the share of academic publishing on gun violence fell 64 percent from 1998 to 2012 (Mayors Against Illegal Guns, 2013; Alcorn, 2016). Although comparable numbers of people die in car crashes and by firearm suicides and homicides, federal investment in traffic safety research funding is more than 270 times greater than in firearm violence research (Mayors Against Illegal Guns, 2013).

As suggested in a 2015 joint statement by Jay Dickey, the sponsor of the Dickey Amendment, and Mark Rosenberg, who ran the CDC’s injury center when the amendment first passed, a gun violence research agenda should be developed with the dual goals of protecting citizens’ and gun owners’ rights and making U.S. homes and communities safer:

Our nation does not have to choose between reducing gun-violence injuries and safeguarding gun ownership. Indeed, scientific research helped reduce the motor
vehicle death rate in the United States and save hundreds of thousands of lives—all without getting rid of cars. For example, research led to the development of simple four-foot barricades dividing oncoming traffic that are preventing injuries and saving many lives. We can do the same with respect to firearm-related deaths, reducing their numbers while preserving the rights of gun owners. (Dickey and Rosenberg, 2015).

The science on which to base gun policy has advanced slowly since 2004, when the NRC panel concluded, “If policy makers are to have a solid empirical and research base for decisions about firearms and violence, the federal government needs to support a systematic program of data collection and research that specifically addresses that issue” (p. 3). The federal government’s support for research on gun violence prevention has been negligible for more than 20 years; however, in late 2019, Congress appropriated $25 million for this purpose (Pub. L. 116-94), which represents an important new federal initiative that, if sustained in future budgets, could make enormous contributions to knowledge about how to prevent gun-related injuries and deaths.

At the same time, states and foundations—such as the Joyce Foundation, the New Venture Fund, and Arnold Ventures, which recently created the National Collaborative on Gun Violence Research (a private philanthropy managed by the RAND Corporation)—are making vital contributions to sustaining research capabilities in the United States and to investigating specific gun policy or gun violence prevention topics, but these sources of funding are not likely to compensate for the absence of an ongoing federal research program that is proportional to the public safety and health problems that firearm injury poses for the United States.

- Recommendation 7. To improve understanding of the real effects of gun policies, Congress should consider appropriating funds annually for a sustained and significant program of research on gun policy and gun violence reduction. This could include investments in firearm research portfolios not only at the CDC and the National Institutes of Health but also at the National Institute of Justice and the National Science Foundation at levels comparable to the government’s current investment in other threats to public safety and health.
- Recommendation 8. Until it is clear that federal investments in gun policy research will be sustained in future years and will support a large-scale program of research, private foundations should take further steps to ensure the development of improved data collection and research on gun policies.

Conclusion 13. Rigorous research examining the effects of many state gun policies on officer-involved shootings, defensive gun use, hunting and recreation, and the gun industry is virtually nonexistent. This lack of rigorous research is problematic because many stakeholders in gun policy debates are especially concerned about the effects that laws could have on these
outcomes. The desire to protect oneself, for instance, is self-reported as one of the primary reasons for gun ownership among 63 percent of all U.S. gun owners and among 76 percent of all U.S. handgun owners (Azrael et al., 2017), yet rigorous studies of the effects of laws on this outcome have rarely been conducted. The lack of research in this area stems, to some extent, from difficulties defining and measuring legal defensive gun use. In some—perhaps most—such cases, guns may contribute to an individual’s self-defense by deterring crimes that would otherwise occur. For this reason and others, it has proven difficult to estimate the frequency with which guns are used defensively (RAND Corporation, 2018, Chapter Twenty-Three).

Nevertheless, opportunities for understanding how policies affect defensive gun use exist and should be pursued. For instance, it may be possible to examine whether policies change the rate at which gun owners are the victims of crime or are injured during a crime. Similarly, FBI records of justifiable homicides, although imperfect as a proxy for defensive gun use, may nevertheless be useful for examining one aspect of a policy’s effects on defensive gun use, as demonstrated by Cheng and Hoekstra (2013). Given the strength of evidence of CAP laws on self-inflicted and unintentional injuries, studying the impact of these policies on defensive gun use can help inform the trade-offs between this outcome and the potential public safety benefits.

The dearth of research examining how policies affect the gun industry is a particularly significant shortcoming in the available scientific literature. Data from the U.S. Bureau of Labor Statistics (2017) suggest that more than 47,000 people in the United States are employed just in the manufacture of small arms and ammunition. The National Shooting Sports Foundation, a gun industry trade association, estimates that an additional 260,000 people may be employed in the distribution and sale of firearms and hunting supplies or in ancillary services, such as operating gun ranges or providing supplies or services to manufacturers and retailers (National Shooting Sports Foundation, 2019). The National Survey of Fishing, Hunting, and Wildlife-Associated Recreation found that, in 2015, approximately 10 million people used firearms for hunting, with total expenditures on firearms of $2.9 billion and expenditures on ammunition of $1.4 billion (U.S. Fish and Wildlife Service, 2018). In addition, more than 32 million people in the United States participated in target shooting in 2015, often at shooting ranges (U.S. Fish and Wildlife Service, 2018). As important as the concerns of this industry may be to the fate of proposed gun policies, there is, at present, little scientific evidence available to the public on this topic.

- **Recommendation 9.** To improve understanding of outcomes of critical concern to many in gun policy debates, the U.S. government and private research sponsors should support research examining the effects of gun laws on a wider set of outcomes, including crime, defensive gun use, hunting and sport shooting, officer-involved shootings, and the gun industry.
Conclusion 14. The lack of data on gun ownership and availability and on guns in legal and illegal markets severely limits the quality of existing research.

There are no regularly collected data that describe gun ownership or use at the state level since the CDC suspended its collection of this information on the Behavioral Risk Factor Surveillance System after the 2004 survey. Most gun laws are designed to specify who can own guns or to change the ways that gun owners store and use their weapons. Therefore, gun ownership and use are the behaviors through which laws may affect such outcomes as firearm suicide, firearm homicide, hunting and recreation, and firearm sales. In the absence of reliable state-level information about gun ownership and use, researchers cannot assess the most-direct intended effects of policies—that is, the effects on gun ownership and use—which may otherwise be easier to detect than the downstream effects of such policies on comparatively rare outcomes, such as suicide and homicide. Is it the case that gun laws cannot have their intended effect because the stock of guns is so great in the United States that anyone who wants a gun can easily obtain one, whether or not they are prohibited? This is a question that cannot easily be answered with the available data on gun ownership and use.

- Recommendation 10. To make important advances in understanding the effects of gun laws, the CDC or another federal agency should resume collecting voluntarily provided survey data on gun ownership and use.

Additionally, the federal government no longer shares with researchers data on illegal gun markets, which investigators could use to examine how policies change the availability of firearms. This is a problem that has also worsened since NRC (2004) identified it as a critical shortcoming for research on gun policy. Specifically, the Tiahrt Amendments (a series of provisions attached to Bureau of Alcohol, Tobacco, Firearms and Explosives appropriations bills since 2003) block researchers and others from studying gun trace data and gun purchaser data. When trace data were available to researchers prior to 2003, the information provided important insights into how criminals obtain their weapons (Kennedy, Piehl, and Braga, 1996; Bureau of Alcohol, Tobacco, and Firearms, 1997); whether states with more-restrictive gun laws create shortages of guns for those who may be prohibited from purchasing them (Weil and Knox, 1996; Cook and Braga, 2001); how guns move between states with less- and more-restrictive gun laws (Cook and Braga, 2001; Webster, Vernick, and Hepburn, 2001); the characteristics of gun sales likely to be associated with diversion to prohibited possessors (Pierce et al., 2003); and other valuable, actionable, policy-relevant information (for further discussion, see Braga et al., 2012).

The Bureau of Alcohol, Tobacco, Firearms and Explosives regularly publishes aggregate statistics on its gun tracing program, but case-level details about the crimes that the guns were involved in, how they were acquired, and other key details are omitted from these reports. Although some researchers have been able to work with detailed
trace data from individual police departments or, rarely, whole states, trace data have not been available on a national level for most researchers.

Trace data and purchaser data have significant limitations that can make inferences about gun markets and crime difficult or uncertain. That is a caveat that applies to most data used in evaluating gun policies, but it should not be a reason for prohibiting access to trace data for research purposes. Trace data sometimes involve information that is pertinent to ongoing investigations, and the release of such data could interfere with those investigations. But this concern also could be overcome by limiting access to the most sensitive or identifying data fields or by releasing trace data on a delayed schedule, perhaps with a two-year lag between when the trace is conducted and when the data are released.

- **Recommendation 11.** To foster a more robust research program on gun policy, Congress should consider whether to eliminate or loosen the restrictions it has imposed on the use of gun trace data for research purposes.

**Conclusion 15.** Monitoring systems for crime, victimization, and nonfatal firearm injuries are incomplete and not yet fulfilling their promise of supporting high-quality gun policy research in the areas we investigated.

The National Violent Death Reporting System (NVDRS) and the National Incident-Based Reporting System (NIBRS) are promising sources of data for future research, although neither was used in any of the studies meeting the inclusion criteria for this report.

The NVDRS was designed to provide unprecedented detail on the circumstances of violent deaths in participating states, such as information on the victim’s life stresses, the relationship between the victim and the offender, and other crimes that were committed at the time of the suicide or homicide. The reason NVDRS data have not been used in the types of studies we reviewed for this report could be that there have not been enough states participating in the NVDRS collection process for long enough to permit the use of strong causal models. State participation in the NVDRS is voluntary, but it has been growing. Indeed, as of 2018, all 50 states were contributing data on their violent deaths. As these data accumulate over time, they will provide a rich source of information that will be useful for evaluating gun policies.

The NIBRS was designed to collect more-detailed information on incidents of crime in the United States than has been available through the FBI’s Uniform Crime Reporting Program. Whereas the FBI system collects summary or aggregate statistics on serious violent and property crimes reported to law enforcement agencies, NIBRS was designed to collect incident-level information about crimes reported to police. It officially launched in the mid-1980s, but by the early 2000s, only 16 percent of the U.S. population was served by a law enforcement agency that reported crime information to NIBRS (NRC, 2004, p. 33). Because the NIBRS program is voluntary and can
be costly for law enforcement agencies to adopt, participation rates have not improved as rapidly as many researchers hoped. By 2012, the proportion of U.S. residents served by a participating law enforcement agency had risen to just 30 percent (Bureau of Justice Statistics [BJS], 2017a).

The FBI plans to eliminate the Uniform Crime Reporting Program in January 2021 and is encouraging all police departments to switch to the NIBRS system by then (FBI, 2018f). Because losing Uniform Crime Reporting Program data would pose significant problems for crime tracking if police departments do not take up the NIBRS quickly enough, BJS created the National Crime Statistics Exchange in an attempt to recruit a representative sample of 400 law enforcement agencies and facilitate their participation in NIBRS. With this sampling approach and data from the more than 6,000 agencies already participating, BJS expects to be able to begin generating reliable national crime trend information based on NIBRS data (BJS, undated).

**Recommendation 12.** Because the NVDRS is so important in collecting data to use for evaluating gun policies, funding to support its implementation in every state should be continued indefinitely.

Another potentially valuable source of information on crimes is the National Crime Victimization Survey, which collects detailed information on crime from a panel of U.S. residents selected to be representative of the nation. This survey provides critically important information about crimes that may never be reported to the police, as well as credible information on how victims and potential crime victims have been able to use guns defensively. But the survey cannot readily be used to understand the effects of state gun laws on crime because it does not generate state-level estimates. Therefore, for the studies meeting our eligibility criteria, the authors primarily used data from the Uniform Crime Reporting Program (or its Supplemental Homicide Reports) when examining crimes, meaning they worked with data that had few details about individual crimes and that included only the subset of crimes reported to law enforcement.

Recognizing the need for state-level victimization data, BJS has explored options for generating such estimates through the National Crime Victimization Survey (BJS, 2017b). BJS is planning to expand the survey panel in order to generate reliable estimates for 22 states that account for 79 percent of the U.S. population. In addition, the bureau has published model-based state estimates for some types of crime over three-year periods from 1999 to 2013 (Fay and Diallo, 2015), although estimates for crimes involving firearms have not yet been published.

**Recommendation 13.** BJS should continue to pursue its efforts to generate state-level victimization estimates. The current goal of generating such estimates for 22 states is a reasonable compromise between cost and the public’s need for more detailed information. However, the bureau should continue to expand its devel-
opment of model-based victimization rates for all states and for a wider set of victimization experiences (including, for instance, crimes involving firearm use by an assailant or victim).

When analyzing the effects of state policy changes on firearm injury outcomes, researchers are often limited to examining fatal injuries. This reliance on mortality outcomes—which can pose statistical challenges (e.g., low statistical power; see Schell, Griffin, and Morral, 2018), given that deaths are considerably less common than nonfatal injuries—stems from several barriers to obtaining reliable and comparable state-level estimates of nonfatal firearm injuries over time. Publicly available data on nonfatal firearm injuries from a sample of emergency departments in the National Electronic Injury Surveillance System have never offered an adequate sample size to support state-level estimation (Mercy, Ikeda, and Powell, 1998). And the validity of data from this system even for national-level estimates has been called into question (Cook et al., 2017; Campbell, Nass, and Nguyen, 2018).

The Healthcare Cost and Utilization Project collects data on firearm injuries that resulted in hospitalization or admittance to an emergency department, and these data sets draw from a much larger sample of hospitals. The national data sets (the Nationwide Inpatient Sample and the National Emergency Department Sample) are weighted to be nationally and regionally representative but are not designed to be representative for each state; since 2011, the data have not contained state-level identifiers. Although the Healthcare Cost and Utilization Project offers a suite of state-level inpatient and emergency department databases, which represent a near-census of state-level firearm injuries that resulted in hospitalization or emergency department admittance, each state-year data set must be purchased separately. This expense represents a serious access barrier for most researchers interested in comparing states over time.

Federal investment to help develop a data infrastructure for nonfatal firearm injury surveillance is not unprecedented. In 1994, the CDC provided funding for seven states to develop, implement, and evaluate systems; New York City and California also invested in firearm-related injury surveillance (Saltzman and Ikeda, 1998). The results from this initial study highlighted the range of data sources that may need to be integrated—for example, trauma registries, hospital discharge records, emergency departments, emergency medical series, and law enforcement records—to be able to provide an accurate picture of overall nonfatal firearm injuries, and the effort would require substantial resources even within a given state or jurisdiction (Mercy, Ikeda, and Powell, 1998; Koo and Birkhead, 1998). However, the Healthcare Cost and Utilization Project already collects state-level data on nonfatal firearm injuries, which could be made more accessible to researchers in the same way that other state-level data from this effort are currently distributed.
• **Recommendation 14.** The Agency for Healthcare Research and Quality at the U.S. Department of Health and Human Services should publish aggregated state-level estimates of firearm-related injuries from its Healthcare Cost and Utilization Project data sets in an easy-to-use format, akin to what the agency does for opioid-related hospitalizations or emergency department visits.¹

**Conclusion 16.** The methodological quality of research on firearms can be significantly improved.

Over the past several decades, studies have offered a great deal of information about how to use what data are available to generate reliable and credible estimates of the effects of gun policies on various outcomes, and the computing power that researchers need to implement the increasingly demanding modeling requirements has more than kept pace with the diffusion of knowledge about appropriate statistical methods. Nevertheless, the scientific literature we reviewed shows that many of the best recent studies suffer from important methodological limitations that should be addressed in future research. These shortcomings concern the following:

• Interpretations of effects generated in models that lack the statistical power to have any reasonable chance of detecting the likely effects of policies. This problem can result in a high likelihood that statistically significant effects are in the opposite direction of the true effects or that the statistically significant effects grossly exaggerate the magnitude of the true effects.

• Estimates of too many parameters for the number of available observations. This problem can result in statistically significant effects that tell virtually nothing about the true generalizable effects of the policies.

• Poorly calibrated tests for whether the effects of policies are statistically significant. This problem can result in many discoveries of effects that reject the null hypothesis that the policy had no effect when, in fact, under proper inferential procedures, the discoveries would be consistent with the law having no effect (or a small effect in the opposite direction).

• Poorly justified selections of statistical models or covariates. This problem can result in estimates of a policy’s effects that are in the wrong direction or that badly misconstrue the magnitude or statistical significance of their effects.

• The presentation of the results of exploratory statistical modeling as though they reflect findings from a confirmatory analysis. When dozens of hypothesis tests are conducted, about 5 percent would be expected to achieve statistical significance at the $p < 0.05$ level even if the law had no effect. Failure to acknowledge that findings are the result of exploratory analysis can lead to overconfident interpretations of effect estimates that may not reflect the true effects of a policy.

¹ See, for instance, Healthcare Cost and Utilization Project Fast Stats, 2019.
Undisclosed categorization of which states had which laws and when they were implemented. Gun policy analysts need reliable and shared databases of state laws. Correct coding of state laws is challenging, and when researchers have disclosed their state law codings, those codings have often been found to contain errors that could affect results.

Poorly justified models of the time course of a policy’s effects. Statistical models of the effects of a policy impose assumptions about the period over which the effects of the policy will build. Often, the implicit assumption is that the full effect of the policy will be observed instantaneously in the first year after the date it is scheduled for implementation. At best, this can lead to underestimates of the effects of policies.

The use of spline and hybrid models that do not estimate coherent causal effects.

Inadequate attention to threats of reciprocal causation or simultaneity biases in effect estimation.

These are technical points of interest chiefly to researchers, so we relegated our detailed discussion of each point to Appendix A of the first edition of this report (RAND Corporation, 2018) and to Schell, Griffin, and Morral (2018). However, our final recommendations are for other researchers interested in the analysis of the effects of gun policies.

Recommendation 15. As part of the Gun Policy in America initiative, we have published a database containing a subset of state gun laws from 1979 to 2016 (Cherney et al., 2019). We ask that others with expertise on state gun laws help us improve the database by notifying us of its errors, proposing more-useful categorizations of laws, or submitting information on laws not yet incorporated into the database. With such help, we hope to make the database a resource beneficial to all analysts.

Recommendation 16. Researchers, reviewers, academics, and science reporters should expect new analyses of the effects of gun policies to improve on earlier studies by persuasively addressing the methodological limitations of earlier studies, such as problems with statistical power, model overfitting, covariate selection, poorly calibrated standard errors, multiple testing, undisclosed state variation in law implementation, and unjustified assumptions about the time course of each policy’s effects.

In conclusion, with a few exceptions, there is a surprisingly limited base of rigorous scientific evidence concerning the effects of many commonly discussed gun policies. This does not mean that these policies are ineffective; they might well be quite effective. Instead, it reflects shortcomings in the contributions that scientific study can currently offer to policy debates in these areas. It also reflects, in part, the policies
we chose to investigate, all of which have been implemented in some U.S. states and, therefore, have proven to be politically and legally feasible, at least in some states. This decision meant that none of the policies we examined would dramatically increase or decrease the stock of guns or gun ownership rates in ways that would produce more readily detectable effects on public safety, health, and industry outcomes. The United States has a large stock of privately owned guns in circulation—estimated to be somewhere between 265 million and 393 million firearms (Karp, 2018; Azrael et al., 2017; Cook and Goss, 2014). Laws designed to change who may buy new weapons, what weapons they may buy, or how gun sales occur will predictably have only a small effect on, for example, homicides or participation in sport shooting, which are affected much more by the existing stock of firearms. Although small effects are especially difficult to identify with the statistical methods common in this field, they may be important. Even a 1-percent reduction in homicides nationally would correspond to approximately 1,500 fewer deaths over a decade.

By highlighting where scientific evidence is accumulating, we hope to build consensus around a shared set of facts that have been established through a transparent, nonpartisan, and impartial review process. In so doing, we also mean to highlight areas where more and better information could make important contributions to establishing fair and effective gun policies.
Chapter Twenty-One References


BJS—See Bureau of Justice Statistics.


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https://www.bjs.gov/content/ncsx.cfm

———, “Data Collection: National Incident-Based Reporting System (NIBRS),” webpage, 2017a. As of May 12, 2017:
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———, “NCVS Redesign: Subnational,” webpage, 2017b. As of May 12, 2017:
https://www.bjs.gov/index.cfm?ty=tp&tid=911


CDC—See Centers for Disease Control and Prevention.

Centers for Disease Control and Prevention, “Underlying Cause of Death, 1999–2017,” WONDER data system, undated-a. As of July 6, 2019:


Cherney, Samantha, Andrew R. Morral, Terry L. Schell, and Sierra Smucker, RAND State Firearm Law Database, Santa Monica, Calif.: RAND Corporation, TL-283-1-RC, 2019. As of October 14, 2019:
https://www.rand.org/pubs/tools/TL283-1.html


FBI—See Federal Bureau of Investigation.


NRC—See National Research Council.


In addition to expanding our original review to incorporate five additional gun policies, we made some changes to definitions, search parameters, and report organization. Furthermore, we included additional studies in our analysis, which sometimes resulted in changes to our conclusions about the effect of a given policy on a given outcome. In this appendix, we document these changes.

**Changes to Policy Definitions**

We expanded the previous policy of firearm sales reporting and recording requirements to also include laws that call for a *gun registration system*, which we define as a recordkeeping system controlled by a government agency that (1) stores the names of current owners of each firearm of a specific class and (2) requires that these records are updated after a firearm is transferred to a new owner.

**Changes to Outcome Definitions**

We made two explicit changes to our outcome definitions. First, we expanded the mass shootings outcome to include school shootings. Although there is no official definition of mass shootings, the nature of many school shootings seemed appropriate to group into this outcome class. However, one study we identified (Anderson and Sabia, 2018) specifically considered school shootings that involved suicides, so we also considered those findings under the outcome of suicide. Additionally, because Anderson and Sabia (2018) separately distinguished all school shootings that involved a suicide from those that involved a homicide, we classified this study as providing evidence for violent crime.

Second, we expanded the gun industry outcome to explicitly consider studies that assessed the effects of one of our included policies on gun ownership or proxy measures of gun ownership. In the original review, we focused this outcome largely on supply-side aspects of the gun industry, but we decided to expand this focus to include demand-side factors.
Changes to Search Parameters

Given our updated policy list, we added “Saturday night special” and “Saturday night specials” to our universal search terms. We also added “concealed carry” to our universal search terms to ensure that we captured studies that examined concealed-carry policies but might not have used such terms as “law” or “policy.”

For our outcome-specific search strings, we added “school shootings” to our search string for mass shooting outcomes, reflecting our expanded definition of this outcome class. For the hunting and recreation outcome, we excluded “ammunition” and “bullets” as search terms because including them in our original review yielded highly irrelevant studies.

Finally, for officer-involved shootings, we amended an error that we discovered in the original review’s search. Specifically, the original report stated that the search terms for officer-involved shootings were “law enforcement” OR police* OR policing. However, it appears that all of our database searches restricted the search to studies that also included “use of force” OR “deadly force.” Thus, in this updated review, we used “law enforcement” OR police* OR policing OR “use of force” OR “deadly force” as search parameters for all databases. This greatly increased the number of articles identified under this search string.

Changes to Report Organization

In the first edition of this report (RAND Corporation, 2018), we presented our research syntheses of the 13 original gun policies as separate chapters in relatively random order. For this updated edition, we reorganized the chapters to group together policies that are related to each other based on the mechanism by which they are designed to operate. We adopted the following organizational structure.

Part B of the report comprises the following policies that regulate who may legally own, purchase, or possess firearms:

- minimum age requirements (Chapter Three)
- prohibitions associated with mental illness (Chapter Four)
- prohibitions associated with domestic violence (Chapter Five)
- surrender of firearms by prohibited possessors (Chapter Six)
- extreme risk protection orders (Chapter Seven).

Part C comprises the following policies that regulate firearm sales and transfers:

- background checks (Chapter Eight)
- licensing and permitting requirements (Chapter Nine)
- waiting periods (Chapter Ten)
• firearm safety training requirements (Chapter Eleven)
• lost or stolen firearm reporting requirements (Chapter Twelve)
• firearm sales reporting, recording, and registration requirements (Chapter Thirteen)
• bans on the sale of assault weapons and high-capacity magazines (Chapter Fourteen)
• bans on low-quality handguns (Chapter Fifteen).

Part D comprises the following policies that regulate the legal use, storage, or carrying of firearms:

• stand-your-ground laws (Chapter Sixteen)
• child-access prevention laws (Chapter Seventeen)
• concealed-carry laws (Chapter Eighteen)
• gun-free zones (Chapter Nineteen)
• laws allowing armed staff in kindergarten through grade 12 (K–12) schools (Chapter Twenty).

We acknowledge that there are various ways to group gun policies conceptually and that our organizational structure may depart from those previously published (see, for example, Siegel et al., 2017a; Vernick and Hepburn, 2003). However, we hope that the reorganization of policy reviews into these three parts provides an improved conceptualization of how different policies within the same group may produce similar or different effects on the relevant outcomes of interest.

In addition to reorganizing the policy review chapters, we removed the supplemental essays that constituted Part C of the original report (RAND Corporation, 2018, Chapters Sixteen through Twenty-Four). The methodology we used to produce those essays was different from the methodology we used to produce the policy reviews, and we did not update the essays as part of this expanded review; thus, they are not included in this report. These chapters remain available in the report’s first edition and via the Gun Policy in America website.

Changes to Our Determinations About Studies’ Inclusion and Quality

In addition to reviewing the new studies that we found through our expanded search criteria, we reassessed the 63 studies from the original report to determine whether their inclusion in our review and our assessment of their methodological quality were appropriate. This reassessment led to several changes.

In this updated edition, we excluded two of the studies assessed in our original review: Duggan, Hjalmarssson, and Jacob (2011) and Wright, Wintemute, and Rivara (1999). We did so because, although they provided indirect evidence for the potential
efficacy of background checks, we concluded that they met neither our original nor our revised inclusion criteria.

Upon re-reviewing the quality of each study, we also revised our assessment of five others:

- The findings of Sen and Panjamapirom (2012) contributed to the synthesis of evidence for the effect of background checks and of prohibitions associated with mental illness on suicide and violent crime. In the original review, we considered these findings to be of high methodological quality. However, upon re-review, we determined that the authors’ models did not make adequate adjustments to account for serial correlation within states. Specifically, although they included the baseline rate of the outcome variable (i.e., suicide rate or homicide rate) as a covariate in their specifications, this approach is more akin to including state fixed effects rather than including the lagged dependent variable in the model. Thus, in our updated review, all estimates from Sen and Panjamapirom (2012) are considered to be of weaker methodological quality.

- The findings of Grambsch (2008) from random effects models contributed to the synthesis of evidence for the effect of concealed-carry laws on violent crime. In the original review, we considered these findings to be of high methodological quality. However, upon re-review, we determined that the authors’ models did not make adequate adjustments to account for serial correlation within states. Thus, in our updated review, the estimates from Grambsch (2008) are considered to be of weaker methodological quality.

- The findings of DeSimone, Markowitz, and Xu (2013) on child-access prevention laws contributed to the synthesis of evidence for the effect of these laws on suicide and on unintentional injuries and deaths. In the original review, we down-weighted this study for not having longitudinal data and for describing effect sizes that were inconsistent with our calculation of incidence rate ratios based on the estimated coefficients. However, upon re-review, we determined the reason for the discrepancy in the authors’ estimated effect size and our calculated incidence rate ratios: DeSimone, Markowitz, and Xu (2013) estimated effect sizes based on marginal effects relative to the overall average outcome rate rather than the outcome rate under the null. Furthermore, even though the study did not have strictly longitudinal hospital-level data, this was not an adequate reason for us to fully lessen our confidence in these findings. Thus, in our updated review, this study’s estimates of the effect of child-access prevention laws are considered to be of stronger methodological quality.\(^1\)

\(^1\) We continue to treat DeSimone, Markowitz, and Xu (2013) as methodologically flawed for evaluating the effects of shall-issue laws because too few states passed such laws during the period the study evaluates.
• The findings of Gius (2015a) contributed to the synthesis of evidence for the effect of background checks on violent crime. In the original review, we down-weighted this study because of the author’s use of a linear model to estimate effects on murder rates, which we assessed to be a potentially problematic model specification. However, given the findings of a 2018 simulation study (Schell, Griffin, and Morral, 2018), we no longer treat this as a serious methodological shortcoming. Thus, in our updated review, the estimates from Gius (2015a) are considered to be of stronger methodological quality.

• The findings of Duggan (2001) contributed to the synthesis of evidence for the effect of concealed-carry laws on gun industry outcomes. In the original review, we down-weighted these findings because the ratio of parameters to observations that we calculated fell below our specified criterion of a ratio of one to ten. However, we discovered that we miscalculated this ratio. Thus, in our updated review, the estimates of the effect of concealed-carry laws in Duggan (2001) are considered to be of stronger methodological quality.

For all but one of these studies, these revisions did not materially affect our overall synthesis of the evidence. The lower rating for the methodological criteria of Sen and Panjamapirom (2012), combined with newly available studies, altered our overall conclusions for four policy-outcome pairs. We describe these changes in the next section.

Areas with New Studies, Revised Conclusions, or Both

In this section, we highlight changes to the number of studies included in our review and to the overall conclusions made since the first edition of this report.

For two of the policies examined in both reviews—background checks and prohibitions associated with mental illness—our downgrading of the methodological quality of Sen and Panjamapirom (2012), combined with the publication of new studies that found differing effects, led us to decrease our confidence in the evidence for the suicide and violent crime outcomes. Specifically, we downgraded our conclusions as follows:

• background checks and suicide:
  – original: limited evidence for decreasing total suicides and moderate evidence for decreasing firearm suicides
  – revised: inconclusive evidence for the effect on total and firearm suicides

• background checks and violent crime:
  – original: limited evidence for decreasing violent crime and total homicides
  – revised: inconclusive evidence for the effect on violent crime and total homicides
prohibitions associated with mental illness and suicide:
– original: limited evidence for decreasing total and firearm suicides
– revised: inconclusive evidence for the effect on total and firearm suicides
prohibitions associated with mental illness and violent crime:
– original: moderate evidence for decreasing violent crime and limited evidence for decreasing total homicides
– revised: limited evidence for decreasing violent crime and inconclusive evidence for the effect on total homicides.

In addition, we originally concluded that there was limited evidence that concealed-carry laws increased unintentional injuries and deaths among adults. However, because both of the studies that contributed to these findings (Lott and Mustard, 1997; DeSimone, Markowitz, and Xu, 2013) were determined to have serious methodological weaknesses, we downgraded this finding to inconclusive.

For four of the gun policies examined in both reviews, the new literature that our search identified was sufficient to upgrade our original conclusions to limited, moderate, or supportive evidence, as follows:

licensing and permitting requirements and suicides:
– original: inconclusive evidence for the effect on firearm suicides
– revised: limited evidence for decreasing firearm suicides among adults.
waiting periods and suicides:
– original: no studies
– revised: limited evidence that waiting periods decrease total suicides and moderate evidence that they decrease firearm suicides
waiting periods and violent crime:
– original: inconclusive evidence for the effect on violent crime and intimate partner homicides
– revised: moderate evidence that waiting periods decrease total homicides and limited evidence that they decrease firearm homicides
stand-your-ground laws and violent crime:
– original: limited evidence that stand-your-ground laws increase firearm homicides
– revised: supportive evidence that stand-your-ground laws increase firearm homicides
child-access prevention laws and violent crime:
– original: inconclusive evidence for the effect on assaults
– revised: limited evidence that child-access prevention laws decrease assaults.
For six of the gun policies examined, our original search identified no studies, but the updated search identified new research that yielded inconclusive evidence (all related to the gun industry). The policies that we revised from no studies to inconclusive evidence for gun industry outcomes are as follows:

- waiting periods: inconclusive evidence for the effect on gun ownership, firearm manufacturers or retailers, and firearm purchases
- firearm sales reporting, recording, and registration requirements: inconclusive evidence for the effect on firearm purchases
- child-access prevention laws: inconclusive evidence for the effect on firearm manufacturers or retailers
- concealed-carry laws (permitless carry): inconclusive evidence for the effect on firearm purchases
- minimum age requirements for possession of a firearm: inconclusive evidence for the effect on violent crimes other than homicide
- licensing and permitting requirements: inconclusive evidence for the effects on gun industry outcomes.

Of the five new gun policies included in this updated and expanded review, we identified studies that met our inclusion criteria for four of them. For laws allowing armed staff in K–12 schools, we identified no studies that met our inclusion criteria. For extreme risk protection orders, firearm safety training requirements, and bans on low-quality handguns, the studies we identified that met our inclusion criteria resulted in inconclusive evidence. In contrast, for the new policy class of prohibitions associated with domestic violence, we identified evidence of sufficient strength to conclude that there is moderate evidence that laws establishing firearm prohibitions for individuals subject to domestic violence restraining orders may reduce total and firearm-related intimate partner homicides.

Tables A.1 through A.6 highlight changes to the number of studies included in our review and to the overall conclusions made since the first edition of this report. Each table presents these changes for one of our examined outcomes. In both our original review and this update, we did not identify any studies that met our inclusion criteria and evaluated the effects of policies on officer-involved shootings or hunting and recreation, so there are no tables for these outcomes.
### Table A.1
Changes to the Number of Studies and Conclusions, Suicide

<table>
<thead>
<tr>
<th>Policy</th>
<th>Original Review</th>
<th>Updated Review</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No. of Studies</td>
<td>Conclusion</td>
<td>No. of Studies</td>
<td>Conclusion</td>
</tr>
<tr>
<td>Minimum age requirements</td>
<td>3</td>
<td>Purchasing: Inconclusive evidence for total suicides; limited evidence decreasing firearm suicides among people aged 20 or younger Possessing: Inconclusive evidence for total and firearm suicides</td>
<td>5</td>
<td>Purchasing: Inconclusive evidence for total suicides; limited evidence decreasing firearm suicides among people aged 20 or younger Possessing: Inconclusive evidence for total and firearm suicides</td>
</tr>
<tr>
<td>Prohibitions associated with mental illness</td>
<td>2</td>
<td>Limited evidence for decreasing total and firearm suicides</td>
<td>3</td>
<td>Inconclusive evidence for total and firearm suicides</td>
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<tr>
<td>Surrender of firearms by prohibited possessors</td>
<td>0</td>
<td>No studies</td>
<td>0</td>
<td>No studies</td>
</tr>
<tr>
<td>Extreme risk protection orders</td>
<td>N/A</td>
<td>Limited evidence for decreasing total suicides; moderate evidence for decreasing firearm suicides</td>
<td>1</td>
<td>Inconclusive evidence for total and firearm suicides</td>
</tr>
<tr>
<td>Background checks</td>
<td>3</td>
<td>Limited evidence for decreasing total suicides; moderate evidence for decreasing firearm suicides</td>
<td>4</td>
<td>Inconclusive evidence for total and firearm suicides</td>
</tr>
<tr>
<td>Licensing and permitting requirements</td>
<td>2</td>
<td>Inconclusive evidence for total and firearm suicides</td>
<td>4</td>
<td>Limited evidence for decreasing total and firearm suicides among adults</td>
</tr>
<tr>
<td>Waiting periods</td>
<td>1</td>
<td>No studies (discounted evidence)</td>
<td>4</td>
<td>Limited evidence for decreasing total suicides; moderate evidence for decreasing firearm suicides</td>
</tr>
<tr>
<td>Firearm safety training requirements</td>
<td>N/A</td>
<td>No studies</td>
<td>0</td>
<td>No studies</td>
</tr>
<tr>
<td>Lost or stolen firearm reporting requirements</td>
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</tr>
<tr>
<td>Firearm sales reporting, recording, and registration requirements</td>
<td>0</td>
<td>No studies</td>
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### Table A.1—Continued

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<tr>
<td></td>
<td>No. of Studies</td>
<td>Conclusion</td>
</tr>
<tr>
<td>Bans on the sale of assault weapons and high-capacity magazines</td>
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</tr>
<tr>
<td>Bans on low-quality handguns</td>
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<tr>
<td>Stand-your-ground laws</td>
<td>1</td>
<td>Inconclusive evidence for total and firearm suicides</td>
</tr>
<tr>
<td>Child-access prevention laws</td>
<td>5</td>
<td>Limited evidence for decreasing total suicides;</td>
</tr>
<tr>
<td></td>
<td></td>
<td>moderate evidence for decreasing firearm suicides;</td>
</tr>
<tr>
<td></td>
<td></td>
<td>supportive evidence for decreasing firearm self-injuries</td>
</tr>
<tr>
<td></td>
<td></td>
<td>among children</td>
</tr>
<tr>
<td>Concealed-carry laws</td>
<td>2</td>
<td>Shall issue: Inconclusive evidence for total and firearm</td>
</tr>
<tr>
<td></td>
<td></td>
<td>suicides</td>
</tr>
<tr>
<td>Gun-free zones</td>
<td>0</td>
<td>No studies</td>
</tr>
<tr>
<td>Laws allowing armed staff in K–12 schools</td>
<td>N/A</td>
<td>N/A</td>
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**NOTE:** N/A = not applicable.
Table A.2  
Changes to the Number of Studies and Conclusions, Violent Crime

<table>
<thead>
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<th>Original Review</th>
<th>Updated Review</th>
</tr>
</thead>
<tbody>
<tr>
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<td>No. of Studies</td>
<td>Conclusion</td>
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<td>and firearm homicides</td>
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### Table A.3
Changes to the Number of Studies and Conclusions, Unintentional Injuries and Deaths

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Changes to the Number of Studies and Conclusions, Mass Shootings

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### Table A.6
Changes to the Number of Studies and Conclusions, Gun Industry

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Appendix A References


To construct the figures in this report showing estimated effect sizes (i.e., the forest plots), we used results reported as the preferred models in each study. In some cases, these sources reported incidence rate ratios (IRRs) as the estimated effect of a law and provided confidence intervals (CIs). In such cases, we used these numbers as reported. In other cases, we calculated IRRs from effects estimated in the studies as regression coefficients, and we calculated CIs from standard errors, test statistics, or reported $p$-values. Discussion of these calculations is provided in Chapter Two. The data used to construct these figures, as well as the full data extraction file with information from each study, is provided in an online appendix to this report, available at www.rand.org/t/RR2088-1.
In this report, part of the RAND Corporation’s Gun Policy in America initiative, researchers seek objective information about what the scientific literature reveals about the likely effects of various gun laws. In this second edition of an earlier work, the authors add five gun policies to the 13 examined in the original analysis and expand the study time frame to incorporate a larger body of research. With those adjustments, the authors synthesize the available scientific data on the effects of 18 policies on firearm deaths, violent crime, the gun industry, defensive gun use, and other outcomes. By highlighting where scientific evidence is accumulating, the authors hope to build consensus around a shared set of facts that have been established through a transparent, nonpartisan, and impartial review process. In so doing, they also illuminate areas where more and better information could make important contributions to establishing fair and effective gun policies.