FOURTH EDITION

The Science of Gun Policy

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

Rosanna Smart, Andrew R. Morral, James P. Murphy, Rupa Jose, Amanda Charbonneau, Sierra Smucker

RAND

A PART OF THE RAND Gun Policy in AMERICA INITIATIVE

For more information on this publication, visit **www.rand.org/t/RRA243-9**.

About RAND

RAND is a research organization that develops solutions to public policy challenges to help make communities throughout the world safer and more secure, healthier and more prosperous. RAND is nonprofit, nonpartisan, and committed to the public interest. To learn more about RAND, visit www.rand.org.

Research Integrity

Our mission to help improve policy and decisionmaking through research and analysis is enabled through our core values of quality and objectivity and our unwavering commitment to the highest level of integrity and ethical behavior. To help ensure our research and analysis are rigorous, objective, and nonpartisan, we subject our research publications to a robust and exacting quality-assurance process; avoid both the appearance and reality of financial and other conflicts of interest through staff training, project screening, and a policy of mandatory disclosure; and pursue transparency in our research engagements through our commitment to the open publication of our research findings and recommendations, disclosure of the source of funding of published research, and policies to ensure intellectual independence. For more information, visit www.rand.org/about/research-integrity.

RAND's publications do not necessarily reflect the opinions of its research clients and sponsors.

Published by the RAND Corporation, Santa Monica, Calif.
© 2024 RAND Corporation
RAND® is a registered trademark.

Library of Congress Cataloging-in-Publication Data is available for this publication.
ISBN: 978-1-9774-1363-5

Limited Print and Electronic Distribution Rights

This publication and trademark(s) contained herein are protected by law. This representation of RAND intellectual property is provided for noncommercial use only. Unauthorized posting of this publication online is prohibited; linking directly to its webpage on rand.org is encouraged. Permission is required from RAND to reproduce, or reuse in another form, any of its research products for commercial purposes. For information on reprint and reuse permissions, please visit www.rand.org/pubs/permissions.

About This Report

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, 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, RAND 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; surveys of policy experts that identified where access to reliable data would be most useful in resolving policy debates; various tools and methodological reports for researchers; visualizations of firearm deaths across states and demographic groups; empirical estimates of the effects of various gun laws on firearm deaths; and three previous editions of this report, which synthesized the available scientific data on the effects of 18 types of firearm policies on a variety of outcomes related to gun ownership. This fourth edition builds on the second edition by updating the literature review to include studies published through February 2023. The result is a substantially updated synthesis of the available evidence concerning the effects of 18 types of gun policies.

The Gun Policy in America initiative does 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 RAND 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.

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 earlier editions of this report were James Anderson, Samuel Peterson, 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 first edition of this 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 this fourth edition of this report, we thank James Anderson and Samuel Peterson for serving as quality assurance reviewers.

Within RAND, we wish to recognize the additional contributors to earlier editions of this report: Rajeev Ramchand, Jhacova Williams, Carole Roan Gresenz, Samantha Cherney, Lea Xenakis, Justin Lee, Matt Cefalu, Samuel Peterson, Christine Chen, Sara-Laure Faraji, Garrett Baker, Goke Akinniranye, Jody Larkin, Daniel Schwam, Samantha Cohen, Justin Lee, Roberta Shanman, Sachi Yagyu, Kyleanne Hunter, and Greg Fauerbach. We also wish to recognize contributors to this edition: Emily Hoch, Alyson Youngblood, Pierrce Holmes, Sangeeta Ahluwalia, Kelsey O'Hollaren, Terry Schell, and Denis Agniel.

Also within RAND, we wish to acknowledge the exceptional support we received from our research librarian, Jody Larkin; our publication editor, Rachel Ostrow; and members of RAND's Communications and External Affairs office, including Babitha Balan, Heather McCracken, Emily Ward, and Alyson Youngblood.

Summary

In 2016, RAND 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 has been 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.[1] In 2020, a second edition was released in which we expanded the classes of policies considered to 18, and in which we updated evidence for all laws with newly published research through 2018 (Smart et al., 2020). A third edition updated our findings and conclusions with research published through October 2020 (Smart et al., 2023). This fourth edition updates our findings and conclusions with research published through February 2023.

Methodology

We followed similar systematic review procedures used for the third edition of this report (Smart et al., 2023). 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 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). Our results for this fourth edition now cover the literature published between January 1995 and February 2023.

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

[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.

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 (CAP) 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. police shootings
6. defensive gun use
7. hunting and recreation
8. gun industry.[2]

Policy Analyses, by Outcome

Building on our earlier review (Smart et al., 2023) 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.[3] Each synthesis defines the class of policies

[2] The terms in these lists describe broad categories of policies and outcomes that are defined and described in detail in the body of the report.

[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 those we said we were going to use

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 weak 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 such a 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. Specifically, our methodological inclusion criteria 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 or comparison group (to demonstrate that the purported causal effect was not found among those who were not exposed to the policy).

For studies that met our inclusion criteria, we summarized 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—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.

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.

These ratings are meant to describe the relative strengths of evidence available across gun policy research domains and should not be interpreted as ratings of our absolute confidence in the reported effects. For instance, when we find supportive evidence for the conclusion that CAP 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 with the evidence that smoking causes cancer, for example, the evidence base in gun policy research is very limited. Additionally, our ratings reflect the relative strength of evidence within a particular outcome domain. For example, a designation of *limited* evidence of the effects of a given policy on suicide may represent a stronger body of evidence than a designation of limited evidence of the effects of that same policy on mass shootings because of the particular challenges that arise in evaluating policy effects for sparse outcomes (such as mass shootings; Smart and Schell, 2021). Nevertheless, we believe that such research may be valuable to the public and policymakers to understand which existing laws 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 that examines 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 are able to 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. Furthermore, unless multiple jurisdictions have implemented a policy and sufficient time has elapsed for outcome data to become available, rigorous and generalizable evidence on the policy's effects is likely to be limited. But concluding that there is limited evidence on the effects of some policies 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.

Conclusions and Recommendations

Of more than 200 combinations of policies and outcomes that we considered, we found that there is growing evidence to support the effects of certain policies on firearm deaths and crime, but many areas have received little 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 CAP 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) compare with the possible costs (in terms of forgone opportunities for self-defense).

In the section that follows, 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

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 and recommendations for policy based on this evidence.

Conclusion 1. We find supportive evidence, our highest evidence rating, that CAP laws, or safe-storage laws, reduce self-inflicted fatal or nonfatal firearm injuries, unintentional firearm injuries and deaths, and firearm homicides among youth. There is also moderate evidence that CAP laws reduce firearm suicides among young people, and limited evidence that such laws reduce unintentional firearm injuries among adults. The evidence is stronger for negligent-storage laws than for reckless endangerment laws; reckless endangerment laws are sometimes considered a weaker form of CAP law.

- *Recommendation 1.* States without negligent-storage CAP laws should consider adopting them or other safe-storage laws as a strategy to reduce total and firearm suicides, unintentional firearm injuries and deaths, and firearm homicides among youth.
- *Recommendation 2.* States implementing CAP or other safe-storage laws should support data collection and investigation of the mechanisms by which CAP laws affect injury outcomes, for example, by investigating effects on gun ownership, storage, and thefts.

Conclusion 2. There is supportive evidence that stand-your-ground laws are associated with increases in firearm homicides and in total homicides.

TABLE S.1

Strength of Evidence Across Gun Policies and Outcomes

Outcome	Minimum Age Requirements		Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks		Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws		Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
	Purchasing	Possessing					Dealer	Dealer and Private										Shall Issue	Permitless Carry		
Suicide																					
Total suicides	↓L[a]	I	I	I		↓L	I	I	I	↓L				I	I	I	↓M, I[b]	I	I		
Firearm suicides	↓S[c]	↓L[c]	I	I	I	↓L	I	I	I	↓M			I	I	I	I	↓S, I[b]	I	I		
Violent crime			↓L		I		I									↑L	I	↑S		I	
Total homicides	I	I	I	↓L	I	I	I	↓M	I	↓M	I			I	I	↑S	I	↑S	I		
Firearm homicides	I	I	I	↓L	I	I	↓M	↓L	I	↓L			I	I	I	↑S	↓S[d]	↑S	I		
Intimate partner homicides				↓M, ↑L, I[e]	I, ↓L[f]					I											
Robberies		I		I	I											I	I	↑L			
Assaults		I		I	I											I	I[d]	↑M			
Rapes		I		I	I												I	I			

Table S.1—Continued

Outcome	Minimum Age Requirements: Purchasing	Minimum Age Requirements: Possessing	Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks: Dealer	Background Checks: Dealer and Private	Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Reporting, Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws: Shall Issue	Concealed-Carry Laws: Permitless Carry	Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
Unintentional injuries and deaths		I																I			
Unintentional firearm injuries and deaths		I				I											↓L, ↓S[g]	I			
Mass shootings	I			I	I	I	I	I	↓L	I	I		I	I, ↓L[h]		I	I	I	I		
Police shootings								I						I					↑L		
Defensive gun use																I					
Hunting and recreation																					
Gun industry																					
Firearm ownership										I						I		I			
Firearm manufacturers or retailers							I		I	I				I			I				

Table S.1—Continued

Outcome	Minimum Age Requirements		Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks		Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Reporting, Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws		Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
	Purchasing	Possessing					Dealer	Dealer and Private										Shall Issue	Permitless Carry		
Firearm purchases									I	I	I		I		I	I		I[i]	I		
Prices or sales of banned firearms in the short term														↑L							

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

[a] We concluded that there is limited evidence that higher minimum age requirements for purchasing a handgun may reduce suicides among young adults.

[b] We found moderate evidence for reductions in total suicides among youths, and we found supportive evidence for reductions in firearm suicides and firearm self-injuries among youths. Evidence for effects on outcomes among the full population was inconclusive.

[c] We concluded that there is moderate evidence that minimum age requirements for purchasing a firearm decrease firearm suicides among young people and limited evidence that minimum age requirements for possessing a firearm decrease firearm suicides among young people.

[d] We found supportive evidence that child-access prevention laws reduce firearm homicides or firearm assault injuries among young people. Evidence for effects on outcomes among the overall population was inconclusive.

[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 misdemeanor domestic violence affect total and firearm-related intimate partner homicides.

[f] We found inconclusive evidence for the effects of firearm-surrender laws alone but moderate evidence that firearm-surrender laws, when paired with expansions of prohibited-possessor classes, reduce firearm-involved intimate partner homicides. We found limited evidence that firearm-surrender laws, when paired with expansions of prohibited-possessor classes, reduce all intimate partner homicides.

[g] We concluded that there is supportive evidence that child-access prevention laws decrease unintentional firearm injuries and deaths among children and limited evidence that they decrease such injuries and deaths among adults.

[h] We found inconclusive evidence for the effects of assault weapon bans on mass shootings and their fatalities and limited evidence that bans on high-capacity magazines reduce mass shootings and fatalities.

[i] We found inconclusive evidence for the effects of may-issue, shall-issue, and permitless-carry laws on firearm purchases.

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

Conclusion 3. There is supportive evidence that shall-issue laws increase total homicides, firearm homicides, and overall violent crime. There is also moderate evidence that shall-issue laws lead to an increase in rates of assault and limited evidence that they increase rates of robbery. Finally, there is limited evidence that permitless-carry laws—which are arguably a form of shall-issue law and are often implemented while maintaining a shall-issue licensing system to ensure license reciprocity with other states—lead to increases in fatal police shootings.

- *Recommendation 4.* States with shall-issue (including permitless-carry) laws should consider whether other regulations, either through requirements implemented as part of the permitting process or through other aspects of law (e.g., laws that may prevent firearm theft), might ensure that the effects of concealed-carry laws are aligned with public safety.

Conclusion 4. There is supportive evidence that increasing the minimum age required to purchase a firearm above the threshold set by federal law can reduce firearm suicides among young people. There is also limited evidence that higher minimum age requirements for purchasing or possessing a firearm may reduce total suicides among young adults and firearm suicides among young people.

- *Recommendation 5:* States should consider raising the minimum age to purchase firearms and ammunition above the level of federal requirements as a strategy to reduce firearm suicides among youths.

Conclusion 5. 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, and there is limited evidence that laws prescribing a mechanism for ensuring the surrender of firearms by prohibited possessors, when combined with laws expanding the class of prohibited possessors, reduce intimate partner homicides.

- *Recommendation 6.* 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, when the law ensures that information about the cases is included in databases used to conduct background checks, and when the prohibitions are accompanied by firearm surrender requirements.

Conclusion 6. There is moderate evidence that dealer background checks reduce firearm homicides, moderate evidence that background checks on both dealer and private sales

reduce total homicides, and limited evidence that background checks on private sales reduce firearm homicides.

- *Recommendation 7.* States that do not require background checks for the private sale or transfer of firearms should consider mandating such checks as a strategy for reducing total and firearm homicide rates.

Conclusion 7. 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 8.* States without waiting-period laws should consider adopting them as a strategy for reducing suicides and homicides.
- *Recommendation 9.* States should consider regulations that delay access to firearms by those who may be in crisis. Examples of laws that may delay such access and for which there is at least moderate evidence that they reduce total or firearm suicides include minimum age of purchase and possession laws, waiting-period laws, and child-access prevention laws.

Conclusion 8. There is limited evidence that extreme-risk protection order (ERPO) laws may reduce total and firearm suicides.

- *Recommendation 10.* States without ERPO laws should consider adopting such laws as a potential strategy for reducing total or firearm suicides.

Conclusion 9. 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 11.* States that 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 10. The sparseness of mass shooting data creates challenges for drawing strong inferences about policy effects because it is difficult to detect signal from noise, and low statistical power means that real policy effects are unlikely to be detected as statistically significant and significant effects are likely to be exaggerated in magnitude and may even be wrong-signed. With these caveats, we find limited evidence that permit-to-purchase requirements and bans on high-capacity magazines may reduce mass shootings and their associated fatalities.

Conclusion 11. No studies meeting our inclusion criteria have examined the effects of lost or stolen firearm reporting requirements or laws allowing armed staff in K–12 schools. Only inconclusive evidence exists for the effects of gun-free zones; firearm safety training requirements; firearm sales reporting, recording, and registration requirements; and bans on low-quality handguns.

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. Particularly over the past decade, researchers have advanced our understanding of gun violence and policy by adopting more-rigorous methodological approaches in several areas, but sustained support of high-quality research is needed to fill gaps in the evidence base.

- *Recommendation 12.* To improve the development of firearm violence prevention strategies, the federal government should commit to an ongoing program of research funding. This could include investments in firearm research portfolios not only at the Centers for Disease Control and Prevention and the National Institutes of Health but also at the National Institute of Justice and the National Science Foundation at levels comparable with the government's existing investments in research on other threats to public safety and health.
- *Recommendation 13.* Given uncertainty about whether federal investments in gun policy research will be sustained in future years so as to 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 violence prevention.

Conclusion 13. Rigorous research examining the effects of many state gun policies on defensive gun use, hunting and recreation, and gun industry outcomes is limited or nonexistent.

- *Recommendation 14.* 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 on how to best measure and evaluate a wider set of outcomes, including defensive gun use, hunting and sport shooting, and gun industry outcomes (e.g., sales, prices, purchasing).

Conclusion 14. The lack of data on (1) gun ownership and availability and (2) guns in legal and illegal markets severely limits researchers' ability to determine the mechanisms by which policies have their effects, understand policy implementation and enforcement, and answer basic scientific questions about the relationships between policies, firearm availability or access, and public health and safety.

- *Recommendation 15.* 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, acquisition, and use with sample sizes sufficient to support state- or substate-level analyses.
- *Recommendation 16.* 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 detailed gun trace data for research purposes.
- *Recommendation 17.* Federal agencies and private foundations should consider substantial funding for a dedicated research program to improve data infrastructure to track illegal firearm markets and jurisdictional policies and practices focused on those markets.

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 18.* Policing agencies not yet contributing crime data through the National Incident-Based Reporting System (NIBRS) should prioritize full participation in the program. States can support the transition by providing technical and financial support to local agencies, subsidizing updated record management systems, and establishing reporting standards or mandates.
- *Recommendation 19.* The Federal Bureau of Investigation's Criminal Justice Information Services Division should modify the NIBRS data collection system to allow agencies to report data on gunshot wounds sustained by crime victims, offenders, and law enforcement officers, as well as incidents during which a firearm was discharged.
- *Recommendation 20.* The Bureau of Justice Statistics' (BJS's) plan to generate crime victimization estimates for the 22 most populous states is a reasonable compromise between cost and the public's need for more-detailed information. BJS should work to facilitate access to and use of these estimates and the National Crime Victimization Survey microdata and consider whether a small grant program could support researcher use of these data. In addition, BJS should continue 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 21.* The Agency for Healthcare Research and Quality at the U.S. Department of Health and Human Services should publish aggregated annual or quarterly 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] Ideally, data could be analyzed by whether an in-hospital death occurred.
- *Recommendation 22.* Agencies responsible for other ongoing surveillance efforts that measure nonfatal firearm-related injuries should develop mechanisms for sharing these data with researchers in a manner that could support evaluation of variation across jurisdictions and over time.

Conclusion 16. While a growing number of studies are using more-rigorous methods to evaluate the causal effects of gun policies, the overall methodological quality of research on firearms can be significantly improved.

- *Recommendation 23.* As part of the Gun Policy in America initiative, we have published a database containing a subset of state gun laws from 1979 to 2021 (Cherney et al., 2022). 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 24.* Researchers, reviewers, academics, and science reporters should ensure that new analyses of the effects of gun policies improve on these studies by persuasively addressing the methodological limitations of earlier studies, such as inadequate attention to causal identification assumptions, problems with statistical power, model overfitting, poorly calibrated standard errors, multiple testing, undisclosed variation in law implementation, and unjustified assumptions about the time course of each policy's effects.

In conclusion, the evidence base has grown substantially since the publication of the first edition of this report, and there are now several policy areas for which we have more robust evidence of the effects—beneficial or harmful—on at least some outcomes. In light of this evidence, decisionmakers may want to reconsider their existing policy regimes and reflect on whether changes to policy are warranted. Political or contextual considerations, as well as ongoing Second Amendment litigation, may make some policy options more feasible or attractive than others. Additionally, because we have limited or nonexistent evidence for how these policies will affect many outcomes that are important to stakeholders, decisions will have to be made without a robust empirical understanding of the potential trade-offs. As evidence mounts in certain areas, however, decisionmakers should take that evidence into consideration.

Still, 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

[4] See, for instance, Healthcare Cost and Utilization Project Fast Stats, 2019.

ineffective; the policies themselves might well be quite effective. Instead, it reflects (1) the absence of scientific study of these policies, (2) insufficient variation in the policies across jurisdictions or over time so as to draw rigorous generalizable insights, (3) inadequate data infrastructure to evaluate effects on important outcomes, or (4) methodological 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 places. 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 might 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 more than 2,000 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.

Summary References

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, *The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy*, Stanford Law School, Olin Working Paper No. 461, December 1, 2014.

Azrael, Deborah, Lisa Hepburn, David Hemenway, and Matthew Miller, "The Stock and Flow of U.S. Firearms: Results from the 2015 National Firearms Survey," *Russell Sage Foundation Journal of the Social Sciences*, Vol. 3, No. 5, 2017.

Cherney, Samantha, Andrew R. Morral, Terry L. Schell, Sierra Smucker, and Emily Hoch, *Development of the RAND State Firearm Law Database and Supporting Materials*, RAND Corporation, TL-A243-2-v2, 2022. As of October 1, 2022: https://www.rand.org/pubs/tools/TLA243-2-v2.html

Cook, Philip J., and Kristin A. Goss, *The Gun Debate: What Everyone Needs to Know*, Oxford University Press, 2014.

Healthcare Cost and Utilization Project Fast Stats, "Opioid-Related Hospital Use," web tool, April 2019. As of November 14, 2019: https://www.hcup-us.ahrq.gov/faststats/OpioidUseServlet

Helland, E., and A. Tabarrok, "Using Placebo Laws to Test 'More Guns, Less Crime,'" *Advances in Economic Analysis and Policy*, Vol. 4, No. 1, 2004.

Karp, Aaron, *Estimating Global Civilian-Held Firearms Numbers*, Small Arms Survey, June 2018.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of August 22, 2019: https://www.rand.org/pubs/research_reports/RR2088.html

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019: https://www.rand.org/pubs/research_reports/RR2685.html

Smart, Rosanna, Andrew R. Morral, Rajeev Ramchand, Amanda Charbonneau, Jhacova Williams, Sierra Smucker, Samantha Cherney, and Lea Xenakis, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States: Third Edition*, RAND Corporation, RR-A243-4, 2023. As of March 25, 2024: https://www.rand.org/pubs/research_reports/RRA243-4.html

Smart, Rosanna, Andrew R. Morral, Sierra Smucker, Samantha Cherney, Terry L. Schell, Samuel Peterson, Sangeeta C. Ahluwalia, Matthew Cefalu, Lea Xenakis, Rajeev Ramchand, and Carole Roan Gresenz, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, 2nd ed., RAND Corporation, RR-2088-1-RC/AV, 2020. As of March 30, 2020: https://www.rand.org/pubs/research_reports/RR2088-1.html

Smart, Rosanna, and Terry L. Schell, "Mass Shootings in the United States," in Rajeev Ramchand and Jessica Saunders, eds., *Contemporary Issues in Gun Policy: Essays from the RAND Gun Policy in America Project*, RAND Corporation, RR-A243-2, 2021. As of August 31, 2022: https://www.rand.org/pubs/research_reports/RRA243-2.html

Contents

About This Report iii
Summary v
Figures and Tables xxix

PART A
Introduction and Methods 1

CHAPTER 1
Introduction 3
Gun Policy in America 4
Research Focus 4
Suicide 7
Violent Crime 8
Unintentional Injuries and Deaths 9
Mass Shootings 10
Police Shootings 11
Defensive Gun Use 12
Hunting and Recreation 13
Gun Industry 13
Organization of This Report 14
Chapter 1 References 15

CHAPTER 2
Methods 23
Selecting Policies 23
Selecting and Reviewing Research 25
Article Retrieval 25
Title and Abstract Review 28
Full-Text Review 29
Synthesis of Evidence 33
Effects of the Inclusion and Exclusion Criteria on the Literature Reviewed 35
Effect Size Estimates 44
Chapter 2 References 45

PART B
Evidence for the Effects of Policies Regulating Who May Legally Own, Purchase, or Possess Firearms 59

CHAPTER 3
Minimum Age Requirements 61
State Implementation of Minimum Age Laws 64
Effects on Suicide 66
Conclusions 70
Effects on Violent Crime 74
Conclusions 77
Effects on Unintentional Injuries and Deaths 78
Effects on Mass Shootings 79
Outcomes Without Studies Examining the Effects of Minimum Age Requirements 81
Chapter 3 References 81

CHAPTER 4
Prohibitions Associated with Mental Illness 85
State Implementation of Prohibitions Associated with Mental Illness 88
Effects on Suicide 90
Effects on Violent Crime 92
Outcomes Without Studies Examining the Effects of Prohibitions Associated with Mental Illness 95
Chapter 4 References 96

CHAPTER 5
Prohibitions Associated with Domestic Violence 101
State Implementation of Prohibitions Associated with Domestic Violence 105
Effects on Suicide 107
Effects on Violent Crime 109
Effects on Mass Shootings 120
Conclusions 123
Outcomes Without Studies Examining the Effects of Prohibitions Associated with Domestic Violence 123
Chapter 5 References 123

CHAPTER 6
Surrender of Firearms by Prohibited Possessors 129
State Implementation of Firearm-Surrender Laws 131
Effects on Suicide 132
Effects on Violent Crime 133
Effects on Mass Shootings 139
Outcomes Without Studies Examining the Effects of Firearm-Surrender Laws 141
Chapter 6 References 142

CHAPTER 7
Extreme-Risk Protection Orders 145
State Implementation of Extreme-Risk Protection Orders 149
Effects on Suicide 150
Effects on Violent Crime 153
Effects on Unintentional Injuries and Deaths 155
Effects on Mass Shootings 156
Outcomes Without Studies Examining the Effects of Extreme-Risk Protection Orders 157
Chapter 7 References 157

PART C
Evidence for the Effects of Policies Regulating Firearm Sales and Transfers 161

CHAPTER 8
Background Checks 163
State Implementation of Background Checks 166
Effects on Suicide 168
Effects on Violent Crime 174
Effects on Mass Shootings 188
Conclusions 190
Effects on Police Shootings 191
Effects on the Gun Industry 192
Conclusions 193
Outcomes Without Studies Examining the Effects of Background Checks 193
Chapter 8 References 194

CHAPTER 9
Licensing and Permitting Requirements 199
State Implementation of Licensing and Permitting Requirements 202
Effects on Suicide 204
Conclusions 206
Effects on Violent Crime 208
Conclusions 213
Effects on Mass Shootings 214
Conclusions 216
Effects on the Gun Industry 216
Conclusions 218
Outcomes Without Studies Examining the Effects of Licensing and Permitting Requirements 218
Chapter 9 References 218

CHAPTER 10
Waiting Periods 223
State Implementation of Waiting Periods 226
Effects on Suicide 227
Conclusions 228
Effects on Violent Crime 229
Conclusions 231
Effects on Mass Shootings 234
Conclusions 235
Effects on the Gun Industry 236
Conclusions 237
Outcomes Without Studies Examining the Effects of Waiting Periods 237
Chapter 10 References 238

CHAPTER 11
Firearm Safety Training Requirements 241
State Implementation of Firearm Safety Training Requirements 243
Effects on Violent Crime 245
Conclusions 245
Effects on Mass Shootings 246
Effects on the Gun Industry 247
Outcomes Without Studies Examining the Effects of Firearm Safety Training Requirements 248
Chapter 11 References 248

CHAPTER 12
Lost or Stolen Firearm Reporting Requirements 251
State Implementation of Lost or Stolen Firearm Reporting Requirements 253
Outcomes Without Studies Examining the Effects of Lost or Stolen Firearm Reporting Requirements 253
Chapter 12 References 254

CHAPTER 13
Firearm Sales Reporting, Recording, and Registration Requirements 255
State Implementation of Firearm Sales Reporting, Recording, and Registration Requirements 257
Effects on Suicides 259
Effects on Violent Crime 260
Effects on Mass Shootings 261
Effects on the Gun Industry 262
Outcomes Without Studies Examining the Effects of Firearm Sales Reporting, Recording, and Registration Requirements 263
Chapter 13 References 263

CHAPTER 14
Bans on the Sale of Assault Weapons and High-Capacity Magazines 265
State Implementation of Bans on the Sale of Assault Weapons and High-Capacity Magazines 268
Effects on Suicide 270
Effects on Violent Crime 271
Effects on Mass Shootings 274
Effects on Police Shootings 279
Effects on the Gun Industry 280
Outcomes Without Studies Examining the Effects of Bans on the Sale of Assault Weapons and High-Capacity Magazines 282
Chapter 14 References 282

CHAPTER 15
Bans on Low-Quality Handguns 287
State Implementation of Bans on Low-Quality Handguns 291
Effects on Suicide 292
Effects on Violent Crime 293
Effects on the Gun Industry 296
Outcomes Without Studies Examining the Effects of Bans on Low-Quality Handguns 297
Chapter 15 References 297

PART D
Evidence for the Effects of Policies Regulating the Legal Use, Storage, or Carrying of Firearms 301

CHAPTER 16
Stand-Your-Ground Laws 303
State Implementation of Stand-Your-Ground Laws 305
Effects on Suicide 308
Effects on Violent Crime 310
Effects on Mass Shootings 318
Effects on Defensive Gun Use 319
Effects on the Gun Industry 320
Outcomes Without Studies Examining the Effects of Stand-Your-Ground Laws 321
Chapter 16 References 322

CHAPTER 17
Child-Access Prevention Laws 325
State Implementation of Child-Access Prevention Laws 329
Effects on Suicide 332

Effects on Violent Crime 338
Effects on Unintentional Injuries and Deaths 342
Effects on Mass Shootings 347
Effects on the Gun Industry 348
Outcomes Without Studies Examining the Effects of Child-Access Prevention Laws 350
Chapter 17 References 350

CHAPTER 18
Concealed-Carry Laws 355
State Implementation of Concealed-Carry Laws 357
Effects on Suicide 359
Effects on Violent Crime 363
Effects on Unintentional Injuries and Deaths 386
Effects on Mass Shootings 388
Effects on Police Shootings 391
Effects on the Gun Industry 392
Outcomes Without Studies Examining the Effects of Concealed-Carry Laws 394
Chapter 18 References 394

CHAPTER 19
Gun-Free Zones 403
State Implementation of Gun-Free Zones 404
Effects on Violent Crime 406
Outcomes Without Studies Examining the Effects of Gun-Free Zones 407
Chapter 19 References 408

CHAPTER 20
Laws Allowing Armed Staff in K–12 Schools 409
State Implementation of Laws Allowing Armed Staff in K–12 Schools 412
Outcomes Without Studies Examining the Effects of Laws Allowing Armed Staff in K–12 Schools 413
Chapter 20 References 413

PART E
Summary of Findings and Recommendations 417

CHAPTER 21
Summary and Conclusions 419
Summarizing the Strength of Evidence 420
What Can We Conclude About the Effects of Gun Policies? 421
Why Don't We Know More? 433
Chapter 21 References 445

Abbreviations 451

APPENDIXES

A. Changes from the Third Edition of This Report 453

B. Source Data Used to Produce the Forest Plot Figures 479

Figures and Tables

Figures

2.1. Flow Diagram of Search Results 36
3.1. Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Suicide 71
3.2. Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Violent Crime 76
3.3. Incidence Rate Ratio Associated with the Effect of Minimum Age Requirements on Unintentional Injuries and Deaths 79
3.4. Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Mass Shootings 80
4.1. Incidence Rate Ratios Associated with the Effect of Mental Health–Related Prohibitions on Suicide 92
4.2. Incidence Rate Ratios Associated with the Effect of Mental Health–Related Prohibitions on Violent Crime 94
5.1. Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Suicide 109
5.2. Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Violent Crime 116
5.3. Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Mass Shootings 122
6.1. Incidence Rate Ratios Associated with the Effect of Firearm-Surrender Laws on Violent Crime 137
6.2. Incidence Rate Ratios Associated with the Effect of Firearm-Surrender Laws on Mass Shootings 141
7.1. Incidence Rate Ratios Associated with the Effect of Extreme-Risk Protection Orders on Suicide 152
7.2. Incidence Rate Ratios Associated with the Effect of Extreme-Risk Protection Orders on Violent Crime 154
7.3. Incidence Rate Ratio Associated with the Effect of Extreme-Risk Protection Orders on Unintentional Firearm Deaths 155
7.4. Incidence Rate Ratio Associated with the Effect of Extreme-Risk Protection Orders on Mass Shootings 157
8.1. Incidence Rate Ratios Associated with the Effect of Background Checks on Suicide 172
8.2. Incidence Rate Ratios Associated with the Effect of Background Checks on Violent Crime 183
8.3. Incidence Rate Ratios Associated with the Effect of Background Checks on Mass Shootings 190
8.4. Incidence Rate Ratio Associated with the Effect of Background Checks on Police Shootings 191

8.5. Incidence Rate Ratios Associated with the Effect of Background Checks on the Gun Industry 193
9.1. Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Suicide 207
9.2. Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Violent Crime 212
9.3. Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Mass Shootings 216
9.4. Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on the Gun Industry 217
10.1. Incidence Rate Ratios Associated with the Effect of Waiting Periods on Suicide 229
10.2. Incidence Rate Ratios Associated with the Effect of Waiting Periods on Violent Crime 232
10.3. Incidence Rate Ratios Associated with the Effect of Waiting Periods on Mass Shootings 235
10.4. Incidence Rate Ratios Associated with the Effect of Waiting Periods on the Gun Industry 237
11.1. Incidence Rate Ratios Associated with the Effect of Firearm Safety Training Requirements on Mass Shootings 247
14.1. Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Suicide 271
14.2. Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Violent Crime 274
14.3. Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Mass Shootings 277
14.4. Incidence Rate Ratio Associated with the Effect of Assault Weapon Bans on Police Shootings 279
15.1. Incidence Rate Ratios Associated with the Effect of Low-Quality Handgun Bans on Suicide 293
15.2. Incidence Rate Ratios Associated with the Effect of Low-Quality Handgun Bans on Violent Crime 295
16.1. Incidence Rate Ratios Associated with the Effect of Stand-Your-Ground Laws on Suicide 309
16.2. Incidence Rate Ratios Associated with the Effect of Stand-Your-Ground Laws on Violent Crime 316
16.3. Incidence Rate Ratio Associated with the Effect of Stand-Your-Ground Laws on Defensive Gun Use 320
17.1. Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Suicide and Self-Injury 336
17.2. Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Violent Crime 341
17.3. Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Unintentional Injuries and Deaths 345

17.4. Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Mass Shootings 348
17.5. Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on the Gun Industry 349
18.1. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Suicide and Self-Injury 362
18.2. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with No Serious Methodological Problems 380
18.3. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with Serious Methodological Problems 382
18.4. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Unintentional Injuries and Deaths 387
18.5. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Mass Shootings 389
18.6. Incidence Rate Ratio Associated with the Effect of Concealed-Carry Laws on Police Shootings 391
18.7. Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on the Gun Industry 393
19.1. Incidence Rate Ratio Associated with the Effect of Gun-Free Zones on Violent Crime 407

Tables

S.1. Strength of Evidence Across Gun Policies and Outcomes x
2.1. Databases Searched for Studies Examining the Effects of Firearm Policies 27
2.2. Inclusion and Exclusion Criteria, by Screening Step 28
2.3. Studies Meeting Inclusion Criteria 37
2.4. Superseded Studies 40
2.5. Included Studies, by Policy and Outcome 41
18.1. Studies Exploring the Effects of Shall-Issue Concealed-Carry Laws on Violent Crime, 1997–2004 365
21.1. Strength of Evidence Across Gun Policies and Outcomes 422
A.1. Comparison of Domains with Cochrane ROBINS-I 455
A.2. Guidance on Domain-Specific Judgments 457
A.3. Results from Studies with Analyses Rated as Being of High Methodological Quality 460
A.4. Changes to the Number of Studies and Conclusions, Suicide 464
A.5. Changes to the Number of Studies and Conclusions, Violent Crime 466
A.6. Changes to the Number of Studies and Conclusions, Unintentional Injuries and Deaths 469
A.7. Changes to the Number of Studies and Conclusions, Mass Shootings 470
A.8. Changes to the Number of Studies and Conclusions, Police Shootings 472
A.9. Changes to the Number of Studies and Conclusions, Defensive Gun Use 473
A.10. Changes to the Number of Studies and Conclusions, Gun Industry 474

PART A

Introduction and Methods

CHAPTER 1

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 39,500 Americans die annually from deliberate and unintentional gun injuries, and more than half of these deaths are suicides (Kochanek et al., 2023). Another 50,000 to 150,000 Americans per year receive care in a hospital for a nonfatal gun injury (Miller et al., 2024; Schnippel et al., 2021; Kaufman et al., 2021; Avraham, Frangos, and DiMaggio, 2018).[2]

Multiple recent high-profile mass public shootings 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; Biden, 2022; Peterson and Densley, 2022). Few Americans are satisfied with the levels of mortality and injury associated with firearms, and while there is broad support for some policies and programs (e.g., Montanaro, 2019; Bonn, 2019), there is also 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).

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

[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 three editions 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).

the debate, and 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, RAND 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 with which 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).

A second edition expanded the 2018 report to include 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 (Smart et al., 2020). In addition, we broadened 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 2. A third edition incorporated new literature that had been published between 2018 and 2020.

This fourth edition updates findings for the 18 policies reviewed in the previous editions, incorporating literature published from 2020 through February 2023. As in the earlier editions of this report, we include a revised and updated online Appendix B that tabulates all study effect sizes reported in this edition, including all source data used for the forest plot figures.

Research Focus

The focus of this report and its predecessor editions is on the kinds of firearm regulations that have been implemented or considered in state legislatures across the country in recent years. We discuss variations in how each class of laws has been implemented in different states, address arguments for and against the laws, and carefully examine the scientific litera-

ture that has sought to identify whether each law appears to have had its intended effects (or whether such laws have had unintended effects). This review may be helpful in several ways.

Where evidence for a policy's effects exists, those effects should be a central consideration in policymaking decisions. Where evidence is weak or uncertain, researchers and their sponsors should consider investing in producing more-definitive studies or in gathering the data needed for such studies.

Importantly, however, where we conclude that evidence for a policy is weak, that does not mean that the policy is ineffective. Relatedly, it should not be the case that the only policies considered for passage into law are those shown scientifically to have their intended effects. Both suggestions are wrong. The absence of evidence about a law can result from the law not having been studied or studied well. In some cases, the data most appropriate for investigating the effects of a law are unavailable. Typically, therefore, we cannot conclude that a lack of evidence is evidence that a policy has no effect or even a small effect. We can conclude only that we do not yet know what the policy's effects are.

Similarly, requiring scientific evidence before passing a law would be an impossible standard to meet. Laws typically have to be implemented before they can be rigorously evaluated. Second, it is an unreasonable standard that shows undue favor to the status quo. Even if there is no statistically significant evidence that a new policy improves on the existing one, there is also no statistically significant evidence that the current policy is better than the new one. Therefore, rather than requiring "proof beyond a reasonable doubt" before replacing a policy, a more reasonable standard would require that a "preponderance of the evidence" support such a change (Cook and Ludwig, 2006a). Rarely would rigorous scientific evidence be available to support such an assessment (although when it is, it should be given considerable weight). Instead, the evidence favoring policies is likely to come from careful reasoning about the likely effects of policies, possibly from anecdotal or correlational evidence, or from corroborating—but not definitive—evidence, such as observations that policies have some of their intended effects on behaviors (e.g., safe storage of firearms) or other mechanisms (e.g., theft of firearms) believed to be critical to achieving such desired objectives as reduced gun violence.

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 whether 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 previously have been found not to violate the Constitution.

Specifically, in this report, we provide a 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. We restricted our attention to laws that have already been implemented in some states so that researchers could examine the effects of each, and we prioritized policies

designed to have a direct effect on our selected outcomes. 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.

The 18 classes of gun policies considered in this research, and discussed in detail in Chapters 3 through 20 of this report, 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 police shootings. These are the outcomes most commonly examined in the research literature with which we were familiar. 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 Centers for Disease Control and Prevention (CDC). Data from 2021 indicate that 48,183 suicides occurred that year for an age-adjusted rate of 14.0 per 100,000 people.[3] Of these, 26,328 (54.6 percent) were firearm suicides (CDC, undated-f). 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 who are prevented from attempting suicide with a firearm will substitute another lethal means and 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 may be 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 increased nearly 30 percent between 2001 and 2021 (CDC, undated).[4] There is some degree of misclassification of suicide deaths, with some suicides likely misclassified as unintentional deaths (Kapusta et al., 2011; Snowdon, 2023), although firearm suicides may be misclassified less often than suicides completed via other means (Rockett et al., 2018). The CDC's National Vital Statistics System and associated resources, such as the CDC's Wide-Ranging Online Data for Epidemiologic Research (WONDER) system, provide limited nationwide data on suicides for all states. The National Violent Death Reporting System (NVDRS), also maintained by the CDC, contains more-expansive data, although the system did not represent all 50 states, the District of Columbia, and Puerto Rico until 2018 (Nazarov et al., 2019).

[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.

[4] The crude rate and the absolute number of suicides both increased.

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). From 2016 to 2020, there were approximately 490,000 self-harm, nonfatal hospital admissions to emergency departments in the United States annually (CDC, 2022c), and estimates suggest that less than 1 percent of these nonfatal injuries were caused by a firearm (Kaufman et al., 2021). This may be because approximately 90 percent of those who attempt suicide with a firearm die, which is a higher rate than with some other methods of suicide, such as hanging (85 percent) or drowning (80 percent) (Cai et al., 2022).

There are several limitations to relying on emergency department data or hospitalization data for understanding the prevalence of nonfatal suicide attempts. For one, the diagnostic codes available in medical claims data cannot distinguish between suicide attempts and self-inflicted intentional injury without intent to die (Hedegaard et al., 2018). Second, emergency room data contain only self-harm incidents that resulted in an emergency room visit; as a complementary data source, national data that use self-reports indicate that, annually from 2015 to 2019, an average of 1.4 million adults aged 18 or older (0.6 percent) had attempted suicide in the past year (Ivey-Stephenson et al., 2022). In 2019, an estimated 8.9 percent of high-school students had attempted suicide in the past year; an estimated 2.5 percent of high-school students had made a suicide attempt in the past year that required treatment by a doctor or nurse (CDC, 2020a).

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 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; Braga et al., 2021). 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 police shootings if police officers expect most suspects to pose a threat of deadly force (Kivisto, Ray, and Phalen, 2017).

As of this writing, the primary source of data on violent crime is the FBI's National Incident-Based Reporting System (NIBRS), which relies on voluntary reporting of crimes by

federal, state, local, and tribal law enforcement agencies.[5] NIBRS estimates indicate that there were approximately 1.3 million violent crimes in the United States in 2022, including roughly 949,200 aggravated assaults, 215,800 robberies, 141,500 rapes, and 21,600 instances of murder or nonnegligent manslaughter.[6] The overall violent crime rate was 398.5 per 100,000 people; the highest rate was for aggravated assault (284.8 per 100,000), followed by robbery (64.7 per 100,000), rape (42.5 per 100,000), and murder or nonnegligent manslaughter (6.5 per 100,000). Nationwide, in 2019, firearms were used in an estimated 79 percent of all instances of murder or nonnegligent manslaughter, 41 percent of robberies, and 40 percent of aggravated assaults (FBI, 2024b).

Death certificate data offer another source of information on homicides and firearm homicides. These mortality data indicate that there were 26,031 total homicides in the United States in 2021 for an age-adjusted rate of 8.2 per 100,000 people; of these, 20,958 (80.5 percent) were caused by a firearm (CDC, undated-b). As with suicides, 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 another weapon.

Other estimates of nonfatal criminal firearm violence come from emergency department or hospital admission records, law enforcement administrative data, and other data compilation efforts. Emergency department data show that from 2017 to 2021, there were, on average, about 1.5 million visits to hospital emergency departments for assault each year, and 7.8 percent of those visits involved a firearm injury (CDC, undated-f).

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 2021 indicate that 224,935 fatal unintentional injuries occurred that year for a rate of 64.7 per 100,000 people (CDC, undated-f). Of these, 549 (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 (Hemen-

[5] The Uniform Crime Reporting Summary Reporting System (SRS) was a key source of crime data for decades, and 2020 was supposed to be its final year of data collection. The NIBRS is a more detailed, incident-level data system. The transition between systems caused notable problems because of incomplete reporting and low participation rates (see Barber, Cook, and Parker, 2022; and Parker, 2022, for discussions of these data sources for analyses of firearm crime and injury). SRS reports were not accepted in 2021, but they were reinstated in 2022—in an effort to mitigate some of these issues—for agencies that had not yet transitioned to the NIBRS .

[6] In 2022, NIBRS data covered an estimated 93.5 percent of the U.S. population. The roughly 30-percent increase in participation relative to 2021 can be attributed to (1) nearly 1,500 agencies transitioning to the NIBRS and (2) the FBI's decision to include SRS reports from more than 2,400 agencies that had not yet transitioned to the NIBRS; SRS reports were not accepted in 2021 (FBI, undated).

way and Solnick, 2015a; Solnick and Hemenway, 2019), other unintentional firearm deaths may be substantially undercounted in the CDC's vital data (Barber et al., 2002; Barber and Hemenway, 2011).[7] 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 21.5 million visits to emergency departments for unintentional injuries in 2021, of which 30,303 (less than 1 percent) were caused by a firearm (CDC, undated-f).[8] These reports omit injuries that did not result in an emergency room visit.

Mass Shootings

Although only a small percentage of 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. Federal statute (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 are 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 that source's definitions, there were between six and 503 mass shootings in the United States in 2019 (see Smart and Schell, 2021, for a discussion of differential estimates by source). Examining more-recent data for 2023, we see that estimates range between eight mass shootings (The Violence Project, undated) and 656 mass shootings (Gun Violence Archive, undated-a).

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

[7] Barber and Hemenway (2011) note that the CDC's NVDRS is highly sensitive and specific, whereas the vital statistics data are more prone to classification errors.

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

the total number of incidents in which four or more people are killed. Between 1976 and 2018, about 18 percent of mass killings were mass public shootings (Duwe, 2020); more-common forms of mass killing are familicide (40 to 45 percent) and felony-related mass killings (25 to 40 percent) (Duwe, 2017; 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.

Police Shootings

Police shootings of civilians have sparked fierce debates locally and nationally about when the use of lethal force is appropriate and whether lethal force is being used disproportionately against minorities. Although the FBI has tried to collect information on police shootings from more than 18,000 law enforcement agencies, recent efforts by news organizations (such as the *Washington Post* and *The Guardian*) have demonstrated that the FBI's data collection efforts miss many such cases. Whereas the FBI's count typically comes to around 300 to 400 justifiable homicides by police in the line of duty per year (FBI, 2024a), the *Washington Post* documented news stories on 1,165 individuals shot and killed by law enforcement in 2023 (Jenkins et al., 2024)—a number that could omit any individuals shot and killed by police about whom no news story was written. The Gun Violence Archive, which collects data on fatal and nonfatal police shootings using a combination of news reports and administrative records, reported 1,445 police shootings with 810 fatalities in 2023 (Gun Violence Archive, undated-a). Similarly, states whose violent death data are available through the NVDRS have, on average, roughly twice as many police 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 the NVDRS and five open-source projects tracking police shootings suggested that the NVDRS captures 97 percent of known shootings in the states with available NVDRS data (Conner et al., 2019). In 2015, 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), with official data collection beginning in 2019 (FBI, 2022a). Law enforcement agency participation in this effort is voluntary; as of 2023, 57 percent of federal, state, local, and tribal law enforcement agencies (representing 68 percent of sworn officers) provided use-of-force data (FBI, 2024c).

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, Missouri, 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. However, recent work has begun to examine national state-level data on police shootings

(Doucette et al., 2022; Nagin, 2020; Hemenway et al., 2019a; Kivisto, Ray, and Phalen, 2017; Mesic et al., 2018).

Defensive Gun Use

Defensive gun use typically has 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 and 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, 2021; Kleck and Gertz, 1995; McDowall, Loftin, and Wiersema, 1998). State firearm policy evaluations generally must measure defensive gun use with data on justifiable homicide incidents from law enforcement, 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. An alternative resource, the Gun Violence Archive, compiles data on defensive gun use from news reports, which capture an incomplete and selected sample of incidents. For example, in 2023, the Gun Violence Archive recorded 1,217 defensive gun use incidents in the United States (Gun Violence Archive, undated-a). A review of a subset of 2019 cases found that nearly 90 percent involved the victim discharging their firearm, suggesting that news sources fail to capture incidents where guns are used defensively but no individuals are shot (Hemenway, Shawah, and Lites, 2022). 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 hunting, 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). More-recent estimates for 2021, from an online survey of 18,000 individuals conducted by Sports Marketing Surveys USA, indicate that 10.8 million people aged six and older used rifles for hunting in 2022, a slight decline from the five years prior (Outdoor Foundation, undated).[9] Target shooting is also a popular U.S. sport. Results from a 2022 survey indicate that 46.2 million people aged six years or older went target shooting with firearms in 2021 (U.S. Fish and Wildlife Service, 2023). 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 2022, the percentage of households with a hunter was down to 11 percent (GSS Data Explorer, undated). Nevertheless, in a Pew Research Center survey, approximately 30 percent of gun owners reported that hunting or target shooting was one of the "major reason[s]" for owning a firearm (Pew Research Center, 2023).

Gun Industry

Estimates produced by the National Shooting Sports Foundation suggest that, in 2022, there were 172,697 jobs in the United States involving the manufacture, distribution, or retailing of ammunition, firearms, and hunting supplies and potentially another 220,999 jobs in supplier and ancillary industries connected with the firearm market (National Shooting Sports Foundation, 2023). According to the U.S. Census Bureau, in 2021, approximately 36,000 people were employed in U.S. firms coded as being involved in just the manufacture of small arms, small arms ammunition, ordnance, and ordnance accessories (North American Industry Classification System [NAICS] codes 332992 and 332994; U.S. Census Bureau, 2023), generating more than $15.2 billion in revenue (U.S. Census Bureau, 2022a).[10] Industry reports that encompass a broader set of market participants estimate that companies involved in the manufacture, distribution, and sales of firearms, ammunition, and hunting equipment employed more than 172,000 people in 2022 (National Shooting Sports Foundation, 2023).

[9] According to this survey, participation in hunting with a rifle was most common (10.8 million), followed by hunting with a shotgun (7.9 million), bow (4.7 million), and then handgun (3.0 million); the results reported do not indicate the amount of overlap in participants across these activities.

[10] Approximately 14,000 additional individuals were employed in firms classified as being involved in the manufacturing of ammunition other than for small arms (e.g., > 30mm ammunition, grenades, bombs).

According to data from the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF), as of fiscal year 2022, 136,563 federal firearms licensees had active licenses in the United States. About 44 percent of these licenses were held by dealers or pawnbrokers, 39 percent were held by collectors,[11] about 16 percent were held by manufacturers of ammunition or firearms, and about 1 percent were held by importers (ATF, 2023). More than 13.8 million firearms were manufactured in the United States in 2021 (ATF, 2023), nearly twice the number manufactured in 2019 (ATF, 2021). In 2020, more than 6.8 million firearms were imported into the United States, nearly ten times the number of firearms that were exported (ATF, 2022a).

Unlike with firearm production data, systematically collected data on firearm sales or transfers are not available. However, some published estimates help characterize various aspects of domestic firearm sales. A report by the U.S. Department of Justice's Office of the Inspector General (2007) estimated that between 2,000 and 5,200 gun shows occurred in the United States each year; more recently, researchers compiled information from 2011 to 2019 from magazines and websites to estimate about 2,300 gun shows annually (Pérez Esparza et al., 2022). Using National Instant Criminal Background Check System (NICS) data and applying a methodology intended to generate a lower bound of the number of firearms associated with a NICS transaction—in which a firearm is transferred to a new possessor (who may or may not already own other firearms)—a 2022 ATF study reported that the estimated minimum sales volume of firearms in 2020 was 19,541,756, a 62-percent increase from 2017 (ATF, 2022a). However, these estimates do not account for multiple firearm sales in a single transaction, transfers in which NICS alternate permits exempt a purchaser from a background check, or private sales that do not involve a federal firearms licensee.

Organization of This Report

The report is organized into five parts. Part A introduces the project scope and objectives in Chapter 1 and the methods used to conduct systematic reviews and syntheses of the literature in Chapter 2. In Parts B through D, we present a research synthesis on each of the 18 state gun policies selected for review (Chapters 3 through 20). 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.

[11] 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.

Finally, Appendix A provides additional detail on methodology and reviews the differences between this edition and the third edition of this report (e.g., changes in overall assessments). And Appendix B, available online, provides the source data used to display study effect sizes and rate study methodologies.

Chapter 1 References

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

Avraham, Jacob B., Spiros G. Frangos, and Charles J. DiMaggio, "The Epidemiology of Firearm Injuries Managed in US Emergency Departments," *Injury Epidemiology*, Vol. 5, No. 38, 2018.

Azrael, Deborah, and Matthew Miller, "Reducing Suicide Without Affecting Underlying Mental Health: Theoretical Underpinnings and a Review of the Evidence Base Lining the Availability of Lethal Means and Suicide," in Rory C. O'Connor and Jane Pirkis, eds., *The International Handbook of Suicide Prevention*, 2nd ed., John Wiley and Sons, 2016.

Barber, Catherine, Deborah Azrael, Amy Cohen, Matthew Miller, Deonza Thymes, David Enze Wang, and David Hemenway, "Homicides by Police: Comparing Counts from the National Violent Death Reporting System, Vital Statistics, and Supplementary Homicide Reports," *American Journal of Public Health*, Vol. 106, No. 5, May 2016.

Barber, Catherine, Philip J. Cook, and Susan T. Parker, "The Emerging Infrastructure of US Firearms Injury Data," *Preventive Medicine*, July 2022.

Barber, Catherine, and David Hemenway, "Too Many or Too Few Unintentional Firearm Deaths in Official U.S. Mortality Data?" *Accident Analysis and Prevention*, Vol. 43, No. 3, 2011.

Barber, C., D. Hemenway, J. Hochstadt, and D. Azrael, "Underestimates of Unintentional Firearm Fatalities: Comparing Supplementary Homicide Report Data with the National Vital Statistics System," *Injury Prevention*, Vol. 8, No. 3, 2002.

BBC Research & Consulting, *The Economic Impacts of Hunting, Fishing and Wildlife Watching in Colorado*, September 26, 2008.

Biden, Joseph R., "Remarks by President Biden on Gun Violence in America," White House, July 2, 2022.

Bonn, Tess, "Majority of Voters Support Assault Weapons Ban, Buybacks: Poll," *The Hill*, September 26, 2019.

Braga, Anthony A., Elizabeth Griffiths, Keller Sheppard, and Stephen Douglas, "Firearm Instrumentality: Do Guns Make Violent Situations More Lethal?" *Annual Review of Criminology*, Vol. 4, 2021.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *Firearms Commerce in the United States: Annual Statistical Update 2021*, U.S. Department of Justice, 2021.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *National Firearms Commerce and Trafficking Assessment (NFCTA): Firearms in Commerce*, U.S. Department of Justice, May 5, 2022a.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *Annual Firearms Manufacturers and Export Report (AFMER) Year 2021*, U.S. Department of Justice, January 20, 2023.

Cai, Ziyi, Alvin Junus, Qingsong Chang, and Paul S. F. Yip, "The Lethality of Suicide Methods: A Systematic Review and Meta-Analysis," *Journal of Affective Disorders*, Vol. 300, March 2022.

Campbell, Sean, and Daniel Nass, "The CDC's Gun Injury Data Is Becoming Even Less Reliable," *FiveThirtyEight*, March 11, 2019.

CDC—*See* Centers for Disease Control and Prevention.

Centers for Disease Control and Prevention, "Underlying Cause of Death, 1999–2020," WONDER data system, undated-b. As of August 17, 2022:
https://wonder.cdc.gov/ucd-icd10.html

Centers for Disease Control and Prevention, "Injury Counts and Rates," WISQARS database, undated-f. As of February 9, 2024:
https://wisqars.cdc.gov/reports/

Centers for Disease Control and Prevention, "Trends in the Prevalence of Suicide-Related Behaviors National YRBS: 1991–2019," webpage, August 20, 2020a. As of February 16, 2024:
https://www.cdc.gov/healthyyouth/data/yrbs/factsheets/2019_suicide_trend_yrbs.htm

Centers for Disease Control and Prevention, "Assault or Homicide," webpage, January 5, 2022a. As of June 16, 2022:
https://www.cdc.gov/nchs/fastats/homicide.htm

Centers for Disease Control and Prevention, "Nonfatal Injury Reports, 2000–2020," WISQARS database, 2022c. As of June 16, 2022:
https://wisqars.cdc.gov/nonfatal-reports

Coben, J. H., C. A. Steiner, M. Barrett, C. T. Merrill, and D. Adamson, "Completeness of Cause of Injury Coding in Healthcare Administrative Databases in the United States," *Injury Prevention*, Vol. 12, No. 3, 2001.

Conner, Andrew, Deborah Azrael, Vivian H. Lyons, Catherine Barber, and Matthew Miller, "Validating the National Violent Death Reporting System as a Source of Data on Fatal Shootings of Civilians by Law Enforcement Officers," *American Journal of Public Health*, Vol. 109, No. 4, 2019.

Cook, Philip J., "The Influence of Gun Availability on Violent Crime Patterns," *Crime and Justice*, Vol. 4, 1983.

Cook, Philip J., and Jens Ludwig, *Guns in America: Results of a Comprehensive National Survey on Firearms Ownership and Use*, Police Foundation, 1996.

Cook, Philip J., and Jens Ludwig, *Guns in America: National Survey on Private Ownership and Use of Firearms: Research in Brief*, National Institute of Justice, 1997.

Cook, Philip J., and Jens Ludwig, "Defensive Gun Uses: New Evidence from a National Survey," *Journal of Quantitative Criminology*, Vol. 14, No. 2, 1998.

Cook, Philip J., and Jens Ludwig, "Aiming for Evidence-Based Gun Policy," *Journal of Policy Analysis and Management*, Vol. 25, No. 3, 2006a.

Cramer, Clayton E., "Why the FBI's Justifiable Homicide Statistics Are a Misleading Measure of Defensive Gun Use," *University of Florida Journal of Law and Public Policy*, Vol. 27, 2016.

GSS Data Explorer, "General Social Survey, 1972–2024," data set, undated. As of April 24, 2024:
https://gssdataexplorer.norc.org/

Doucette, Mitchell L., Julie A. Ward, Alex D. McCourt, Daniel Webster, and Cassandra K. Crifasi, "Officer-Involved Shootings and Concealed Carry Weapons Permitting Laws: Analysis of Gun Violence Archive Data, 2014–2020," *Journal of Urban Health*, Vol. 99, No. 3, 2022.

Duwe, Grant, "The Patterns and Prevalence of Mass Public Shootings in the United States, 1915–2013," in Laura C. Wilson, ed., *The Wiley Handbook of the Psychology of Mass Shootings*, John Wiley and Sons, 2017.

Duwe, Grant, "Patterns and Prevalence of Lethal Mass Violence," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

FBI—*See* Federal Bureau of Investigation.

Federal Bureau of Investigation, "UCR Summary of Crime in the Nation, 2022," Uniform Crime Reporting Program, undated.

Federal Bureau of Investigation, "Crime in the United States 2015: Violent Crime," U.S. Department of Justice, 2016e. As of May 8, 2017:
https://ucr.fbi.gov/crime-in-the-u.s/2015/crime-in-the-u.s.-2015/offenses-known-to-law-enforcement/violent-crime/violentcrimemain_final.pdf

Federal Bureau of Investigation, "Expanded Homicide Data Table 14: Justifiable Homicide, by Weapon, Law Enforcement, 2014–2018," webpage, Crime in the United States 2018, 2019b. As of November 18, 2019:
https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/tables/expanded-homicide-data-table-14.xls

Federal Bureau of Investigation, "Crime Data Explorer: Use-of-Force," webpage, 2022a. As of August 18, 2022:
https://crime-data-explorer.app.cloud.gov/pages/le/uof

Federal Bureau of Investigation, "Table 1: Crime in the United States, by Volume and Rate per 100,000 Inhabitants, 2000–2019," webpage, Crime in the United States 2019, 2022b. As of June 16, 2022:
https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/table-1

Federal Bureau of Investigation, "Expanded Homicide Data, Table 14: Justifiable Homicide, by Weapon, Law Enforcement, 2018–2022," webpage, April 24, 2024a.

Federal Bureau of Investigation, "Crime Data Explorer: NIBRS Estimates," webpage, April 24, 2024b.

Federal Bureau of Investigation, "Crime Data Explorer: National Use-of-Force Data Collection," webpage, April 24, 2024c.

Gun Violence Archive, homepage, undated-a. As of February 9, 2024:
https://www.gunviolencearchive.org/

Gun Violence Archive, "Past Summary Ledgers," webpage, updated August 18, 2022. As of August 18, 2022:
http://www.gunviolencearchive.org/past-tolls/

Hahn, Robert A., Oleg Bilukha, Alex Crosby, Mindy T. Fullilove, Akiva Liberman, Eve Moscicki, Susan Snyder, Farris Tuma, and Peter A. Briss, "Firearms Laws and the Reduction of Violence: A Systematic Review," *American Journal of Preventive Medicine*, Vol. 28, No. 2, 2005.

Hedegaard, Holly, Michael Schoenbaum, Cynthia Claassen, Alex Crosby, Kristin Holland, and Scott Proescholdbell, "Issues in Developing a Surveillance Case Definition for Nonfatal Suicide Attempt and Intentional Self-Harm Using International Classification of Diseases, Tenth Revision, Clinical Modification (ICD-10-CM) Coded Data," *National Health Statistics Reports*, Vol. 108, February 2018.

Hemenway, David, and Deborah Azrael, "The Relative Frequency of Offensive and Defensive Gun Uses: Results from a National Survey," *Violence and Victims*, Vol. 15, No. 3, 2000.

Hemenway, David, Deborah Azrael, Andrew Conner, and Matthew Miller, "Variation in Rates of Fatal Police Shootings Across U.S. States: The Role of Firearm Availability," *Journal of Urban Health,* Vol. 96, No. 1, 2019a.

Hemenway, David, Chloe Shawah, and Elizabeth Lites, "Defensive Gun Use: What Can We Learn from News Reports?" *Injury Epidemiology*, Vol. 9, No. 19, 2022.

Hemenway, David, and Sara J. Solnick, "Children and Unintentional Firearm Death," *Injury Epidemiology*, Vol. 2, No. 26, 2015a.

Hodur, Nancy M., F. Larry Leistritz, and Kara L. Wolfe, "Developing the Nature-Based Tourism Sector in Southwestern North Dakota," *Great Plains Research: A Journal of Natural and Social Sciences*, Vol. 18, Spring 2008.

Ivey-Stephenson, Asha Z., Alex E. Crosby, Jennifer M. Hoenig, Shiromani Gyawali, Eunice Park-Lee, and Sarra L. Hedden, "Suicidal Thoughts and Behaviors Among Adults Aged ≥18 Years—United States, 2015–2019," *MMWR Surveillance Summary*, Vol. 71, No. 1, 2022.

Jenkins, Jennifer, Monika Mathur, John Muyskens, Razzan Nakhlawi, Steven Rich, and Andrew Ba Tran, "Fatal Force," *Washington Post*, updated February 9, 2024.

Kahan, Dan M., "On the Sources of Ordinary Science Knowledge and Extraordinary Science Ignorance," in Kathleen Hall Jamieson, Dan M. Kahan, and Dietram A. Scheufele, eds., *Oxford Handbook of the Science of Science Communication*, Oxford University Press, 2017.

Kapusta, N. D., U. S. Tran, I. R. Rockett, D. DeLeo, C. P. Naylor, T. Niederkrotenthaler, M. Voracek, E. Etzersdorfer, and G. Sonneck, "Declining Autopsy Rates and Suicide Misclassification: A Cross-National Analysis of 35 Countries," *Archives of General Psychiatry*, Vol. 68, No. 10, 2011.

Kaufman, Elinore J., Douglas J. Wiebe, Ruiying Aria Xiong, Christopher N. Morrison, Mark J. Seamon, and M. Kit Delgado, "Epidemiologic Trends in Fatal and Nonfatal Firearm Injuries in the US, 2009–2017," *JAMA Internal Medicine*, Vol. 181, No. 2, 2021.

Kindy, Kimberly, "FBI to Sharply Expand System for Tracking Fatal Police Shootings," *Washington Post*, December 8, 2015.

Kivisto, Aaron J., Bradley Ray, and Peter L. Phalen, "Firearm Legislation and Fatal Police Shootings in the United States," *American Journal of Public Health*, Vol. 107, No. 7, 2017.

Kleck, Gary, "Crime Control Through the Private Use of Armed Force," *Social Problems*, Vol. 35, No. 1, 1988.

Kleck, Gary, *Point Blank: Guns and Violence in America*, AldineTransaction, 2009.

Kleck, Gary, "Large-Capacity Magazines and the Casualty Counts in Mass Shootings: The Plausibility of Linkages," *Justice Research and Policy*, Vol. 17, No. 1, 2016.

Kleck, Gary, "What Do CDC's Surveys Say About the Prevalence of Defensive Gun Use?" *American Journal of Criminal Justice*, Vol. 46, No. 3, June 2021.

Kleck, Gary, and Marc G. Gertz, "Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun," *Journal of Criminal Law and Criminology*, Vol. 86, No. 1, 1995.

Klinger, D., R. Rosenfeld, D. Isom, and M. Deckard, "Race, Crime, and the Micro-Ecology of Deadly Force," *Criminology and Public Policy*, Vol. 15, No. 1, February 2016.

Kochanek, Kenneth D., Sherry L. Murphy, Jiaquan Xu, and Elizabeth Arias, "Deaths: Final Data for 2020," *National Vital Statistics Reports*, Vol. 72, No. 10, September 22, 2023.

Krouse, William J., and Daniel J. Richardson, *Mass Murder with Firearms: Incidents and Victims, 1999–2013*, Congressional Research Service, R44126, 2015.

Longo, Daniel, *Lock and Load: After Recent Sales Surge, Demand Normalizes as Fears of Gun Control Subside*, IBISWorld Industry Report 33299a: Guns and Ammunition Manufacturing in the U.S., June 2017.

Luo, Michael, and Mike McIntire, "Children and Guns: The Hidden Toll," *New York Times*, September 28, 2013.

McDowall, D., C. Loftin, and B. Wiersema, "Estimates of the Frequency of Firearm Self Defense from the National Crime Victimization Survey," State University of New York, School of Criminal Justice, Violence Research Group Discussion Paper 20, 1998 (unpublished).

Mesic, Aldina, Lydia Franklin, Alev Cansever, Fiona Potter, Anika Sharma, Anita Knopov, and Michael Siegel, "The Relationship Between Structural Racism and Black-White Disparities in Fatal Police Shootings at the State Level," *Journal of the National Medical Association*, Vol. 110, No. 2, 2018.

Miller, Gabrielle F., Sarah Beth L. Barnett, Curtis S. Florence, Kathleen McDavid Harrison, Linda L. Dahlberg, and James A. Mercy, "Costs of Fatal and Nonfatal Firearm Injuries in the US, 2019 and 2020," *American Journal of Preventive Medicine*, Vol. 66, No. 2, February 2024.

Montanaro, Domenico, "Poll: Most Americans Want to See Congress Pass Gun Restrictions," NPR, September 10, 2019.

Nagin, Daniel S., "Firearm Availability and Fatal Police Shootings," *Annals of the American Academy of Political and Social Science*, Vol. 687, No. 1, 2020.

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

National Shooting Sports Foundation, *Firearms and Ammunition Industry Economic Impact Report 2022*, 2023.

Nazarov, Oybek, Joseph Guan, Stanford Chihuri, and Guohua Li, "Research Utility of the National Violent Death Reporting System: A Scoping Review," *Injury Epidemiology*, Vol. 6, No. 18, 2019.

Nelson, Charles M., *Economic Implications of Land Use Patterns for Natural Resource Recreation and Tourism*, Michigan Land Resource Project, Public Sector Consultants, 2001.

Niforatos, Joshua D., Alexander R. Zheutlin, and Richard M. Pescatore, "Public Interest in Gun Control in the USA," *Injury Prevention*, Vol. 25, Supp. 1, 2019.

Office of the Surgeon General and National Action Alliance for Suicide Prevention, *National Strategy for Suicide Prevention: Goals and Objectives for Action: A Report of the U.S. Surgeon General and of the National Action Alliance for Suicide Prevention*, U.S. Department of Health and Human Services, 2012.

Outdoor Foundation, *2022 Outdoor Participation Trends Report*, undated.

Parker, Kim, Juliana Menasce Horowitz, Ruth Igielnik, Baxter Oliphant, and Anna Brown, *America's Complex Relationship with Guns*, Pew Research Center, June 22, 2017.

Parker, Susan T., "Measuring Gun Violence in Police Data Sources: Transitioning to NIBRS," *Injury Epidemiology*, Vol. 9, No. 15, 2022.

Pérez Esparza, David, Eugenio Weigend Vargas, Tony Payan, and Carlos Pérez Ricart, "Examining a Dataset on Gun Shows in the US, 2011–2019," *Journal of Illicit Economies and Development*, Vol. 4, No. 1, 2022.

Peterson, Jillian, and James Densley, *Public Mass Shootings: Database Amasses Details of a Half Century of U.S. Mass Shootings with Firearms, Generating Psychosocial Histories*, National Institute of Justice, February 3, 2022.

Pew Research Center, "For Most U.S. Gun Owners, Protection Is the Main Reason They Own a Gun," August 16, 2023.

Public Law 112-265, Investigative Assistance for Violent Crimes Act of 2012, January 14, 2013.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of August 22, 2019: https://www.rand.org/pubs/research_reports/RR2088.html

Rockett, Ian R. H., Eric D. Caine, Steven Stack, Hilary S. Connery, Kurt B. Nolte, Christa L. Lilly, Ted R. Miller, Lewis S. Nelson, Sandra L. Putnam, Paul S. Nestadt, and Haomiao Jia, "Method Overtness, Forensic Autopsy, and the Evidentiary Suicide Note: A Multilevel National Violent Death Reporting System Analysis," *PLoS One*, Vol. 13, No. 5, May 2018.

Schaechter, Judy, Isis Duran, Jacqueline De Marchena, Glendene Lemard, and Maria Elena Villar, "Are 'Accidental' Gun Deaths as Rare as They Seem? A Comparison of Medical Examiner Manner of Death Coding with an Intent-Based Classification Approach," *Pediatrics*, Vol. 111, No. 4, 2003.

Schildkraut, Jaclyn, H. Jaymi Elsass, and Kimberly Meredith, "Mass Shootings and the Media: Why All Events Are Not Created Equal," *Journal of Crime and Justice*, Vol. 41, No. 3, 2018.

Schnippel, Kathryn, Sarah Burd-Sharps, Ted R. Miller, Bruce A. Lawrence, and David I. Swedler, "Nonfatal Firearm Injuries by Intent in the United States: 2016–2018 Hospital Discharge Records from the Healthcare Cost and Utilization Project," *Western Journal of Emergency Medicine*, Vol. 22, No. 3, 2021.

Smart, Rosanna, Andrew R. Morral, Sierra Smucker, Samantha Cherney, Terry L. Schell, Samuel Peterson, Sangeeta C. Ahluwalia, Matthew Cefalu, Lea Xenakis, Rajeev Ramchand, and Carole Roan Gresenz, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, 2nd ed., RAND Corporation, RR-2088-1-RC/AV, 2020. As of March 30, 2020:
https://www.rand.org/pubs/research_reports/RR2088-1.html

Smart, Rosanna, and Terry L. Schell, "Mass Shootings in the United States," in Rajeev Ramchand and Jessica Saunders, eds., *Contemporary Issues in Gun Policy*, RAND Corporation, RR-A243-2, 2021. As of August 31, 2022:
https://www.rand.org/pubs/research_reports/RRA243-2.html

Snowdon, John, "Hidden Suicides: Their Misclassification as Accidental, 'Undetermined' or Unknown Cause Deaths," *Journal of Suicidology*, Vol. 18, No. 2, 2023.

Solnick, Sara J., and David Hemenway, "Unintentional Firearm Deaths in the United States 2005–2015," *Injury Epidemiology*, Vol. 6, October 2019.

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

U.S. Census Bureau, *Annual Survey of Manufacturers: Summary Statistics for Industry Groups and Industries in the U.S.: 2018–2021*, Table AM1831BASIC01, December 2022a.

U.S. Census Bureau, *Number of Firms, Number of Establishments, Employment, and Annual Payroll by Enterprise Employment Size for the United States, All Industries: 2021*, December 2023.

U.S. Department of Justice, *The Bureau of Alcohol, Tobacco, Firearms, and Explosives' Investigative Operations at Gun Shows*, Office of the Inspector General, Evaluation and Inspections Report I-2007-007, June 2007.

U.S. Fish and Wildlife Service, *2016 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*, FHW/16-NAT, April 2018.

U.S. Fish and Wildlife Service, *2022 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*, September 2023.

The Violence Project, "Mass Shooter Database," web tool, undated. As of February 9, 2024: https://www.theviolenceproject.org/mass-shooter-database/

World Health Organization, *Preventing Suicide: A Global Imperative*, 2014.

CHAPTER 2

Methods

In this fourth edition of our gun policy research syntheses, we followed the same general systematic review procedures used for the third edition (see Smart et al., 2023); some modifications are described in Appendix A. The protocol for this updated review was pre-registered in the International Prospective Register of Systematic Reviews, or PROSPERO, database (no. CRD42019120105).[1] We update the third edition by incorporating approximately two additional years of published literature.

Selecting Policies

The fourth edition of this report updates our review of evidence for the effects of the 18 policies included in the second and third editions. Our approach to selecting these policies, which is described in greater detail in Smart et al. (2020), was guided by feasibility (i.e., we did not have the resources to study every existing or proposed firearm policy), variability (i.e., there had to be sufficient variation in the enactment of the law across states or over time to potentially understand the causal effects of the law), and external perceptions of and agreement on each policy's importance. We focus on state firearm laws because states are a predominant source of variability in firearm-related legislation, and we believe that laws applied statewide may generalize to new jurisdictions better than local gun policies or programs that may be more tailored to the unique circumstances giving rise to them.

The 18 policies reviewed in this edition and the previous two editions, defined and explained in Chapters 3 through 20, are as follows:

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

1. minimum age requirements
2. prohibitions associated with mental illness

[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.

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.

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, community violence interventions, built environment interventions) that have been evaluated in prior reviews and meta-analyses (Braga, Papachristos, and Hureau, 2014; Braga and Weisburd, 2012; Braga, Weisburd, and Turchan, 2018; Butts et al., 2015; Gobaud et al., 2022a; Kondo et al., 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]

[2] For a review of the evidence on criminal justice interventions to reduce criminal access to firearms, see Braga, 2017. For a review of other community-based approaches aimed at reducing violence, see John Jay College Research Advisory Group on Preventing and Reducing Community Violence, 2020. For a review of the evidence on how firearm policies affect diversions of guns for criminal use, see Crifasi et al., 2019.

Selecting and Reviewing Research

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 the predetermined inclusion and exclusion criteria from the previous edition's protocol (see Chapter Two of Smart et al., 2020).
3. Full-text review: All studies retained after the title and abstract review received full-text review by two team members 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 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. 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

The third edition of this review (Smart et al., 2023), the search for which was conducted in October 2020, covered studies published between January 1, 1995, and October 20, 2020. In March 2023, we updated this search by querying all databases listed in Table 2.1 for English-language studies published between August 1, 2020, and February 28, 2023.

We queried the databases listed in Table 2.1 and conducted searches for our 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 pistol* OR "automatic weapon*" OR "assault weapon*" OR "semi-automatic weapon*" OR "semiautomatic weapon*" OR "Saturday night special*" 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

[3] We identified no experimental studies.

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 the outcome strings):

- suicide: suicid* OR "self-harm*" OR "self-injur*" OR "self injur*"
- violent crime: homicide* OR murder* OR manslaughter OR "domestic violence" OR "spousal abuse" OR "spouse abuse" OR "elder abuse" OR "child abuse" OR "family violence" OR "child maltreatment" OR "spousal maltreatment" OR "elder maltreatment" OR "elder mistreatment" OR "intimate relationship violence" OR "intimate partner violence" OR "dating violence" OR (violen* AND [crime* OR criminal*]) OR rape OR rapes OR rapist* OR "personal crime" OR "personal crimes" OR robbery OR robberies OR assault* OR stalk* OR terroris*
- unintentional injuries and deaths: accident* OR unintentional* OR "un intentional*"
- mass shootings: "mass shooting*" OR "mass murder" OR "school shooting*"
- police shootings: "law enforcement" OR police* OR policing[4]
- 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 supplies OR trade* OR trading 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" OR FFL.

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. For example, we prescreened out 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 that met minimum evidence standards. Table 2.2 describes our inclusion and exclusion criteria for the title and abstract review and 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

[4] The WorldCat search included additional search terms: "use of force" and "deadly force."

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

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

[a] These six databases were searched together via the EBSCO platform.

(i.e., whether the study was a pooled cross-sectional analysis or 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 using our research questions, and we pilot-tested these criteria on a sample of 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 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 were resolved by consensus among reviewers.

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

TABLE 2.2
Inclusion and Exclusion Criteria, by Screening Step

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

excluded studies that focused on a non-U.S. context. (For discussion of the effects of the 1996 National Firearms Agreement in Australia, see Ramchand and Saunders, 2021).

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 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 3 through 20) 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 or that did not include a control group of individuals or jurisdictions that were unexposed to the policy.

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 that described the totality of each state's gun policies or studies of the aggregate effects of legislation that included multiple gun policies.[5] 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).

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, 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 used prespecified criteria that have been shown to be important methodological considerations in quasi-experimental policy evaluations, particularly in firearm policy research. We revised the quality assessment criteria used in previous editions of this review for three reasons: (1) to formalize our ratings for methodological issues that we had previously handled under the heading of "other concerns"; (2) to better distinguish between minor, serious, and critical methodological concerns; and (3) to better align our criteria with those outlined in the ROBINS-I ("Risk of Bias in Non-Randomised Studies

[5] In some cases, we included studies that measured a conceptually distinct law using a scale. For instance, Brauer, Montolio, and Trujillo-Baute (2017) measured 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.

of Interventions") tool (see details in Appendix A). Effect estimates, defined at the study-by-policy-by-outcome level, were rated as having no, minor, serious, or critical methodological concerns on five specific domains; an overall methodological quality judgment was rendered using the most problematic domain-specific rating. For all effect estimates included in our review, we consistently note serious or critical methodological concerns related to each of these five domains (see Appendix A for details):

- *Threats to causal identification.* We note serious concerns when the study design—although meeting our inclusion criteria of having data before and after the policy change and a comparison group—is unlikely to produce unbiased estimates of the causal effects of the policies of interest. When studies do not account for any state-specific differences that may affect both outcomes and policy adoption or do not account for national trends in the outcome, we consider analysis to have critical methodological concerns unless other evidence is provided to support the model's identifying assumptions.
- *Few treated units, control units, and pre- or post-treatment data.* We note serious concerns when the effects of policies were identified on the experiences of a single policy transition or a small number of policy transitions. These analyses generally provide less-persuasive evidence that observed differences between treated and control units result from the effects of the policy rather than from other contemporaneous influences on the outcome. When an analysis has both a small number of treated units and a small number of control units, we consider it to have critical methodological concerns.
- *Policy classification errors.* We note whether studies have subsequently been shown to have made critical errors in policy coding; such errors are likely to render the results unreliable.
- *Systematic outcome data missingness.* Unless the study makes efforts to account for differential missingness, we note issues of missingness that are a function of the value of the outcome of interest (e.g., if mortality data are suppressed when there are ten or fewer deaths) because the policy's effect on the outcome levels would then relate to this missingness. The extent to which the issue prompts minor, serious, or critical methodological concerns depends on the extent of the missingness.
- *Violation of statistical assumptions and inaccurate inferential statistics.* We note when the assumptions of the statistical approach used are likely to be violated in ways that render estimates of effect sizes or uncertainty unreliable. This issue rises to the level of a critical methodological concern when inferential statistics are not provided or when such statistics are produced using nonstandard methods that are known to over-reject the null.
- *Risk of model overfit.* We note serious concerns whenever estimates were based on models with a ratio of less than ten observations per estimated parameter and a ratio of less than ten events (i.e., counts of the outcome variable) per estimated parameter, and the study provided no additional evidence to mitigate concerns with overfit models (such as the use of cross-validation or evidence from an analysis of the relative fits of different model specifications). When the ratio of estimated parameters to observations

or events dropped below one to five and no supplemental evidence of model fit was provided, we consider the analysis to have critical methodological concerns.

Effect estimates produced by analyses that were rated as having a critical methodological concern on any domain are disregarded in our conclusions and are not presented in our summary figures. Additionally, to the extent that stronger studies for that policy and outcome are available, we do not discuss the results of studies with critical methodological concerns. We briefly note that we exclude these studies and describe the reason that they were rated as having a critical methodological concern, although Appendix B includes the results from these studies.

We caution that these domains do not represent a full accounting of potential methodological concerns with the studies of firearm policies that are included in this review. For instance, we do not discuss when papers provided no statistical or conceptual evidence to justify their covariate selections. We do not discuss potential issues that may occur when studies controlled for variables that are plausibly endogenously related to the law of interest (e.g., many studies control for gun ownership or a gun ownership proxy measure, but the effect of the law on crime or death may be mediated through how the law affects ownership). We also do not focus on the interpretational difficulties or confusion that are 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. See Schell, Smart, and Morral (2022) for discussion of some of these additional methodological challenges and concerns.

Additionally, we do not comment on general methodological issues that can arise in the context of laws producing heterogeneous effects. For example, recent literature has highlighted important conditions under which two-way fixed-effects regressions, as they are commonly specified in gun policy evaluation studies, can produce biased effect estimates when multiple units (e.g., states) implement a policy of interest at different points in time (Athey and Imbens, 2022; Borusyak, Jaravel, and Spiess, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Gibbons, Serrato, and Urbancic, 2019; Goodman-Bacon, 2021; Imai and Kim, 2021). In this context, the two-way fixed-effects estimator can be biased—and in some cases, severely biased—because the estimate produced by the standard two-way fixed-effects regression is a weighted average of treatment effects for several comparison groups, including the comparison of newly treated units with units that have already been treated (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021); this latter comparison can bias effect estimates if, for example, early- versus late-adopting states experience systematically different policy effects. Because the potential bias produced by these issues in the estimation of gun policies has not yet been well evaluated, we do not delve into these issues

in this review.[6] However, we highlight whether studies make efforts to investigate or account for this potential issue.

Finally, 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 their 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 (Smart, Morral, and Schell, 2021). 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 semantic simplifications 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.

[6] Two analyses assess these issues for stand-your-ground laws (Cunningham, 2021) and right-to-carry laws (Donohue et al., 2022). Both find little evidence of bias resulting from treatment effect heterogeneity.

We include the suggestive category for several reasons. First, following 2022 guidance (Cumpston et al., 2022), we are interested in incorporating evidence from studies that may not meet conventional levels of statistical significance ($p < 0.05$). This is particularly important 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 one another. 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 reviewed, 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 existing evidence base. 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 third edition of this report.

Effects of the Inclusion and Exclusion Criteria on the Literature Reviewed

Figure 2.1 follows the guidance of Stovold et al. (2014) and presents the study flow diagram for the updated 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 182 studies meeting all inclusion criteria.

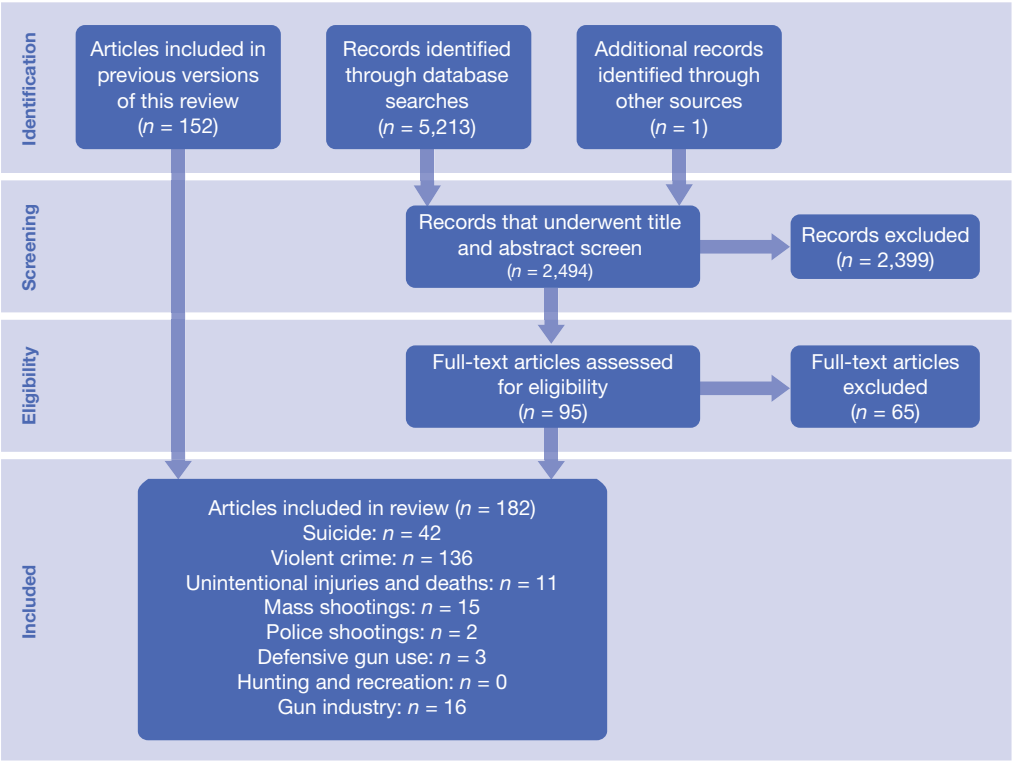
In several cases, some studies published updates to earlier works that expanded the time frame of the analysis, corrected errors, or applied more-advanced statistical methods to a nearly identical data set. In these cases, we do not treat both the earlier and later works as each

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 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.

FIGURE 2.1

Flow Diagram of Search Results



NOTE: The one additional record identified (Luca, Malhotra, and Poliquin, 2020) is the final peer-reviewed publication of a working paper (Luca, Malhotra, and Poliquin, 2016) included in the third edition of this review. The record was inadvertently screened out as duplicative (rather than superseding a previous study) in the third edition.

TABLE 2.3
Studies Meeting Inclusion Criteria

No.	Study
1	Adams (2022)
2	Anderson and Sabia (2018)
3	Anderson, Sabia, and Tekin (2018)
4	Anderson, Sabia, and Tekin (2021)
5	Andrés and Hempstead (2011)
6	Aneja, Donohue, and Zhang (2011)
7	Aneja, Donohue, and Zhang (2014)
8	Anestis and Anestis (2015)
9	Ayres and Donohue (1999)
10	Ayres and Donohue (2002)
11	Ayres and Donohue (2003a)
12	Ayres and Donohue (2003b)
13	Ayres and Donohue (2009a)
14	Ayres and Donohue (2009b)
15	Azad et al. (2020)
16	Balakrishna and Wilbur (2022)
17	Barati (2016)
18	Bartley and Cohen (1998)
19	Benson and Mast (2001)
20	Bhatt et al. (2020)
21	Black and Nagin (1998)
22	Blau, Gorry, and Wade (2016)
23	Brauer, Montolio, and Trujillo-Baute (2017)
24	Bronars and Lott (1998)
25	Castillo-Carniglia et al. (2019)
26	Cheng and Hoekstra (2013)
27	Chien and Gakh (2020)
28	Cook and Ludwig (2003)
29	Crifasi et al. (2015)
30	Crifasi et al. (2018b)
31	Crifasi, Pollack, and Webster (2016)
32	Cummings et al. (1997a)
33	D'Alessio et al. (2022)
34	Dalafave (2021)
35	Degli Esposti et al. (2022)
36	Depew and Swensen (2022)
37	DeSimone and Markowitz (2005)
38	DeSimone, Markowitz, and Xu (2013)
39	Dezhbakhsh and Rubin (1998)
40	Díez et al. (2017)
41	Donohue (2003)
42	Donohue (2004)
43	Donohue (2017)
44	Donohue, Aneja, and Weber (2018)
45	Donohue, Aneja, and Weber (2019)
46	Donohue et al. (2023)
47	Donohue and Levitt (2001)
48	Doucette, Crifasi, and Frattaroli (2019)
49	Doucette et al. (2021)
50	Doucette et al. (2022)
51	Doucette et al. (2023a)
52	Duggan (2001)
53	Durlauf, Navarro, and Rivers (2016)
54	Duwe, Kovandzic, and Moody (2002)
55	Edwards et al. (2018)
56	French and Heagerty (2008)
57	Fridel (2021)
58	Fridel (2021a)
59	Ginwalla et al. (2014)
60	Gius (2014)
61	Gius (2015a)
62	Gius (2015b)
63	Gius (2015c)
64	Gius (2016)
65	Gius (2017)
66	Gius (2018)
67	Gius (2019a)
68	Gius (2019b)

Table 2.3—Continued

No.	Study
69	Gius (2020a)
70	Gius (2020b)
71	Glaeser and Glendon (1998)
72	Gobaud et al. (2022b)
73	Grambsch (2008)
74	Guettabi and Munasib (2018)
75	Hamill et al. (2019)
76	Hamlin (2021)
77	Hasegawa, Webster, and Small (2019)
78	Helland and Tabarrok (2004)
79	Hempstead and Andrés (2009)
80	Hepburn et al. (2004)
81	Hepburn et al. (2006)
82	Humphreys, Gasparrini, and Wiebe (2017)
83	Kagawa et al. (2018)
84	Kagawa et al. (2023)
85	Kalesan et al. (2017)
86	Kappelman and Fording (2021)
87	Kaufman et al. (2020)
88	Kendall and Tamura (2010)
89	Kester et al. (2023)
90	Kivisto et al. (2021)
91	Kivisto and Phalen (2018)
92	Klarevas, Conner, and Hemenway (2019)
93	Knopov et al. (2019)
94	Koper (2002)
95	Koper (2004)
96	Koper and Roth (2001)
97	Koper and Roth (2002)
98	Kovandzic, Marvell, and Vieraitis (2005)
99	La Valle (2007)
100	La Valle (2010)
101	La Valle (2013)
102	La Valle and Glover (2012)
103	Levy et al. (2020)
104	Liu, Siegel, and Sen (2022)
105	Lott (1998a)
106	Lott (1998b)
107	Lott (2000)
108	Lott (2003)
109	Lott (2010)
110	Lott and Landes (1999)
111	Lott and Mustard (1997)
112	Lott and Whitley (2001)
113	Lott and Whitley (2003)
114	Lott and Whitley (2007)
115	Luca, Malhotra, and Poliquin (2016)
116	Luca, Malhotra, and Poliquin (2017)
117	Luca, Malhotra, and Poliquin (2020)
118	Ludwig (1998)
119	Ludwig and Cook (2000)
120	Manski and Pepper (2015)
121	Manski and Pepper (2018)
122	Martin and Legault (2005)
123	Marvell (2001)
124	McClellan and Tekin (2017)
125	McCourt et al. (2020)
126	Miller and Pepper (2020)
127	Moe et al. (2020)
128	Monroe (2008)
129	Moody (2001)
130	Moody and Marvell (2008)
131	Moody and Marvell (2009)
132	Moody and Marvell (2018a)
133	Moody and Marvell (2018b)
134	Moody et al. (2014)
135	Munasib, Kostandini, and Jordan (2018)
136	Mustard (2001)

Table 2.3—Continued

No.	Study
137	Neufeld et al. (2022)
138	Oliphant (2022)
139	Olson and Maltz (2001)
140	Pear et al. (2022a)
141	Plassmann and Tideman (2001)
142	Plassmann and Whitley (2003)
143	Raifman et al. (2020)
144	Raissian (2016)
145	Roberts (2009)
146	Rochford, Berg, and Peek-Asa (2022)
147	Rogna and Nguyen (2022)
148	Rosengart et al. (2005)
149	Roth and Koper (1997)
150	Roth and Koper (1999)
151	Rubin and Dezhbakhsh (2003)
152	Rudolph et al. (2015)
153	Saadi et al. (2020)
154	Sabbath, Hawkins, and Baum (2020)
155	Scantling et al. (2022)
156	Schell et al. (2020)
157	Sen and Panjamapirom (2012)
158	Shi and Lee (2018)
159	Siegel et al. (2017b)
160	Siegel et al. (2019)
161	Siegel et al. (2020a)
162	Siegel et al. (2020b)
163	Smith and Petrocelli (2019)
164	Steidley (2019)
165	Steidley and Kosla (2018)
166	Strnad (2007)
167	Swanson et al. (2013)
168	Swanson et al. (2016)
169	Van Der Wal (2022)
170	Vigdor and Mercy (2003)
171	Vigdor and Mercy (2006)
172	Wallace (2014)
173	Wallace et al. (2021)
174	Wallin, Holliday, and Zeoli (2022)
175	Webster, Crifasi, and Vernick (2014)
176	Webster et al. (2004)
177	Webster et al. (2020)
178	Webster and Starnes (2000)
179	Webster, Vernick, and Hepburn (2002)
180	Zeoli et al. (2018)
181	Zeoli and Webster (2010)
182	Zimmerman (2014)

TABLE 2.4

Superseded Studies

Superseded	Superseding
Anderson, Sabia, and Tekin (2018)	Anderson, Sabia, and Tekin (2021)
Aneja, Donohue, and Zhang (2011); Ayres and Donohue (1999, 2002, 2003a, 2003b, 2009a, 2009b); Donohue (2003, 2004)	Aneja, Donohue, and Zhang (2014)
Cook and Ludwig (2003)[a]	Ludwig and Cook (2000)[a]
DeSimone and Markowitz (2005)	DeSimone, Markowitz, and Xu (2013)
Dezhbakhsh and Rubin (1998)	Rubin and Dezhbakhsh (2003)
Donohue (2017); Donohue, Aneja, and Weber (2018)	Donohue, Aneja, and Weber (2019)
Hempstead and Andrés (2009)	Andrés and Hempstead (2011)
Koper and Roth (2001, 2002); Roth and Koper (1997, 1999)	Koper (2004)
La Valle (2007, 2010)	La Valle (2013), La Valle and Glover (2012)
Lott and Landes (1999)	Lott (2003)
Lott (1998a, 1998b, 2000); Lott and Whitley (2003)	Lott (2010)
Luca, Malhotra, and Poliquin (2016)	Luca, Malhotra, and Poliquin (2020)
Manski and Pepper (2015)	Manski and Pepper (2018)
Moody (2001); Moody and Marvell (2008, 2009)	Moody et al. (2014)
Vigdor and Mercy (2003)	Vigdor and Mercy (2006)

[a] While Cook and Ludwig (2003) was published more recently than Ludwig and Cook (2000), the earlier paper included a larger data set spanning a wider time frame, and thus we focus on its analyses. However, the results reported in the two papers are comparable.

TABLE 2.5

Included Studies, by Policy and Outcome

Policy	Suicide	Violent Crime	Unintentional Injuries and Deaths	Mass Shootings	Police Shootings	Defensive Gun Use	Hunting and Recreation	Gun Industry	Total
Minimum age requirements	5, 20, 62, 86, 123, 143, 148, 160	123, 127, 137, 148, 152, 160	60	76, 117					14
Prohibitions associated with mental illness	5, 89, 157, 168	137, 157, 167, 168							6
Prohibitions associated with domestic violence	5, 89, 157	27, 40, 48, 58, 93, 137, 144, 146, 154, 155, 157, 171, 173, 174, 180, 181		58, 177					19
Surrender of firearms by prohibited possessors	89, 104	27, 40, 93, 104, 137, 144, 171, 173, 174, 180, 181		161, 177					14
Extreme-risk protection orders	34, 89, 91, 140, 153	34, 70, 140, 155	34	161					8
Background checks	25, 83, 84, 89, 104, 116, 119, 125, 157, 160, 168	25, 30, 58, 61, 72, 80, 83, 84, 87, 93, 101, 104, 109, 116, 119, 125, 128, 137, 154, 157, 160, 162, 167, 168, 171, 174, 180		58, 66, 85, 117, 161, 177	147			23, 94	36
Licensing and permitting requirements	5, 20, 29, 36, 104, 116, 125, 176	30, 31, 36, 48, 49, 58, 65, 77, 93, 104, 116, 125, 137, 152, 162, 174, 175, 180		22, 58, 117, 161, 177				23, 164	28

Table 2.5—Continued

Policy	Suicide	Violent Crime	Unintentional Injuries and Deaths	Mass Shootings	Police Shootings	Defensive Gun Use	Hunting and Recreation	Gun Industry	Total
Waiting periods	8, 55, 89, 116, 119, 138	55, 58, 80, 112, 116, 119, 128, 136, 145		58, 108, 117				23, 71	16
Firearm safety training requirements		109		76				59	3
Lost or stolen firearm reporting requirements									0
Firearm sales reporting, recording, and registration requirements	104	104		22				165	3
Bans on the sale of assault weapons and high-capacity magazines	160	58, 60, 109, 133, 137, 154, 160		22, 58, 63, 66, 92, 117, 161, 177	147			16, 95	17
Bans on low-quality handguns	148, 160	109, 148, 160, 175, 179						179	5
Stand-your-ground laws	35, 74, 82, 156, 160	1, 26, 30, 33, 35, 48, 49, 64, 74, 82, 93, 103, 124, 126, 135, 137, 154, 155, 156, 160, 162, 175		22		26, 103, 124		172	24
Child-access prevention laws	2, 15, 32, 38, 62, 69, 86, 90, 112, 156, 176	2, 4, 15, 32, 38, 112, 154, 156	15, 32, 38, 62, 81, 112, 178	2, 76, 108				4, 23	18

Table 2.5—Continued

Policy	Suicide	Violent Crime	Unintentional Injuries and Deaths	Mass Shootings	Police Shootings	Defensive Gun Use	Hunting and Recreation	Gun Industry	Total
Concealed-carry laws	36, 38, 104, 116, 148, 156, 160	1, 7, 17, 18, 19, 21, 24, 30, 31, 36, 38, 45, 46, 47, 48, 49, 51, 53, 56, 57, 58, 60, 67, 68, 73, 75, 78, 80, 88, 93, 98, 101, 102, 104, 109, 114, 116, 118, 121, 122, 132, 134, 136, 137, 139, 141, 142, 145, 148, 151, 154, 155, 156, 158, 159, 160, 162, 163, 166, 169, 175, 182	38, 111	22, 54, 58, 66, 108, 117, 161, 177	50			52, 59, 164, 165	75
Gun-free zones		67							1
Laws allowing armed staff in K–12 schools									0
Total	39	109	9	14	2	3	0	12	149[a]

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

[a] Of the 182 studies that met all inclusion criteria, 33 were superseded.

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 papers. Studies reporting the results from a negative binomial or Poisson regression model are directly reported in our forest plot figures as IRRs with their associated confidence intervals (CIs). Given the low probability of most of our outcomes, odds ratios were interpreted and reported as IRRs with their associated CIs.

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

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

However, if the linear model used a logged dependent variable, we used the exponentiated estimate as its IRR. CIs for the IRRs that were 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,[7] 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.

Some models operationalize policy variables in a way that does not produce a single summary effect size estimate. For example, 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 generally 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

[7] In the gun policy evaluation context, the synthetic control method typically is used to construct weighted combinations of jurisdictions (e.g., states, counties) that never implemented the law or that implemented it many years after the treated jurisdiction such that, in the period before the passage of the law, the temporal pattern of crime or death in the synthetic control closely matched that in the implementing jurisdiction. For further discussion of these methods in the context of gun policy evaluation, see Schell, Smart, and Morral (2022) and Degli Esposti et al. (2022a).

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 that examined 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 2 References

Adams, K. Alexander, "No Retreat: The Impact of Stand Your Ground Laws on Violent Crime," *Criminal Justice Review*, September 2022, online ahead of print.

Anderson, D. Mark, and Joseph J. Sabia, "Child-Access-Prevention Laws, Youths' Gun Carrying, and School Shootings," *Journal of Law and Economics*, Vol. 61, No. 3, August 2018.

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin, *Child Access Prevention Laws and Juvenile Firearm-Related Homicides*, National Bureau of Economic Research, Working Paper No. 25209, December 2018.

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin, "Child Access Prevention Laws and Juvenile Firearm-Related Homicides," *Journal of Urban Economics*, Vol. 126, November 2021.

Andrés, Antonio Rodríguez, and Katherine Hempstead, "Gun Control and Suicide: The Impact of State Firearm Regulations in the United States, 1995–2004," *Health Policy*, Vol. 101, No. 1, 2011.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, "The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy," *American Law and Economics Review*, Vol. 13, No. 2, 2011.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, *The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy*, Stanford Law School, Olin Working Paper No. 461, December 1, 2014.

Anestis, Michael D., and Joye C. Anestis, "Suicide Rates and State Laws Regulating Access and Exposure to Handguns," *American Journal of Public Health*, Vol. 105, No. 10, 2015.

Athey, Susan, and Guido W. Imbens, "Design-Based Analysis in Difference-in-Differences Settings with Staggered Adoption," *Journal of Econometrics*, Vol. 226, No. 1, January 2022.

Ayres, Ian, and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review*, Vol. 1, No. 1–2, January 1999.

Ayres, Ian, and John J. Donohue III, *Shooting Down the More Guns, Less Crime Hypothesis*, National Bureau of Economic Research, Working Paper No. 9336, November 2002.

Ayres, Ian, and John J. Donohue III, "Shooting Down the More Guns, Less Crime Hypothesis," *Stanford Law Review*, Vol. 55, No. 4, 2003a.

Ayres, Ian, and John J. Donohue III, "The Latest Misfires in Support of the More Guns, Less Crime Hypothesis," *Stanford Law Review*, Vol. 55, No. 4, 2003b.

Ayres, Ian, and John J. Donohue III, "Yet Another Refutation of the More Guns, Less Crime Hypothesis—with Some Help from Moody and Marvell," *Econ Journal Watch*, Vol. 6, No. 1, January 2009a.

Ayres, Ian, and John J. Donohue III, "More Guns, Less Crime Fails Again: The Latest Evidence from 1977–2006," *Econ Journal Watch*, Vol. 6, No. 2, May 2009b.

Azad, Hooman Alexander, Michael C. Monuteaux, Chris A. Rees, Michael Siegel, Rebekah Mannix, Lois K. Lee, Karen M. Sheehan, and Eric W. Fleegler, "Child Access Prevention Firearm Laws and Firearm Fatalities Among Children Aged 0 to 14 Years, 1991–2016," *JAMA Pediatrics*, Vol. 174, No. 5, May 2020.

Balakrishna, Meenakshi, and Kenneth C. Wilbur, "Do Firearm Markets Comply with Firearm Restrictions? How the Massachusetts Assault Weapons Ban Enforcement Notice Changed Registered Firearm Sales," *Journal of Empirical Legal Studies*, Vol. 19, No. 1, March 2022.

Barati, Mehdi, "New Evidence on the Impact of Concealed Carry Weapon Laws on Crime," *International Review of Law and Economics*, Vol. 47, 2016.

Bartley, William Alan, and Mark A. Cohen, "The Effect of Concealed Weapons Laws: An Extreme Bound Analysis," *Economic Inquiry*, Vol. 36, No. 2, 1998.

Benson, Bruce L., and Brent D. Mast, "Privately Produced General Deterrence," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Bhatt, Apurva, Xi Wang, An-Lin Cheng, Kalee L. Morris, Luke Beyer, Abbie Chestnut, Kristy Steigerwalt, and Jeffrey Metzner, "Association of Changes in Missouri Firearm Laws with Adolescent and Young Adult Suicides by Firearms," *JAMA Network Open*, Vol. 3, No. 11, November 2020.

Black, Dan A., and Daniel S. Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies*, Vol. 27, No. 1, 1998.

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, *Revisiting Event Study Designs: Robust and Efficient Estimation*, arXiv, April 2022.

Braga, Anthony A., "Guns and Crime," in F. Parisi, ed., *The Oxford Handbook of Law and Economics, Vol. 3: Public Law and Legal Institutions*, Oxford University Press, 2017.

Braga, Anthony A., Andrew V. Papachristos, and David M. Hureau, "The Effects of Hot Spots Policing on Crime: An Updated Systematic Review and Meta-Analysis," *Justice Quarterly*, Vol. 31, No. 4, 2014.

Braga, Anthony A., and David L. Weisburd, "The Effects of Focused Deterrence Strategies on Crime: A Systematic Review and Meta-Analysis of the Empirical Evidence," *Journal of Research in Crime and Delinquency*, Vol. 49, No. 3, 2012.

Braga, Anthony A., David Weisburd, and Brandon Turchan, "Focused Deterrence Strategies and Crime Control: An Updated Systematic Review and Meta-Analysis of the Empirical Evidence," *Criminology and Public Policy*, Vol. 17, No. 1, 2018.

Brauer, Jurgen, Daniel Montolio, and Elisa Trujillo-Baute, "How Do US State Firearms Laws Affect Firearms Manufacturing Location? An Empirical Investigation, 1986–2010," *Journal of Economic Geography*, Vol. 17, No. 4, 2017.

Bronars, Stephen G., and John R. Lott, Jr., "Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns," *American Economic Review*, Vol. 88, No. 2, 1998.

Butts, J. A., C. G. Roman, L. Bostwick, and J. R. Porter, "Cure Violence: A Public Health Model to Reduce Gun Violence," *Annual Review of Public Health*, Vol. 36, 2015.

Callaway, Brantly, and Pedro H. C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, Vol. 225, No. 2, December 2021.

Castillo-Carniglia, Alvaro, Rose M. C. Kagawa, Magdalena Cerdá, Cassandra K. Crifasi, Jon S. Vernick, Daniel W. Webster, and Garen J. Wintemute, "California's Comprehensive Background Check and Misdemeanor Violence Prohibition Policies and Firearm Mortality," *Annals of Epidemiology*, Vol. 30, February 2019.

Cheng, Cheng, and Mark Hoekstra, "Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine," *Journal of Human Resources*, Vol. 48, No. 3, 2013.

Chien, L-C., and M. Gakh, "The Lagged Effect of State Gun Laws on the Reduction of State-Level Firearm Homicide Mortality in the United States from 1999 to 2017," *Public Health*, Vol. 189, December 2020.

Colquhoun, D., "An Investigation of the False Discovery Rate and the Misinterpretation of p-Values," *Royal Society Open Science*, Vol. 1, No. 3, 2014.

Cook, Philip J., and Jens Ludwig, "The Effect of the Brady Act on Gun Violence," in B. Harcourt, ed., *Guns, Crime, and Punishment in America*, New York University Press, 2003.

Crifasi, Cassandra K., Alexander D. McCourt, Marisa D. Booty, and Daniel W. Webster, "Policies to Prevent Illegal Acquisition of Firearms: Impacts on Diversions of Guns for Criminal Use, Violence, and Suicide," *Current Epidemiology Reports*, Vol. 6, No. 2, 2019.

Crifasi, Cassandra K., Molly Merrill-Francis, Alex McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster, "Association Between Firearm Laws and Homicide in Urban Counties," *Journal of Urban Health*, Vol. 95, No. 3, 2018b.

Crifasi, C. K., J. S. Meyers, J. S. Vernick, and D. W. Webster, "Effects of Changes in Permit-to-Purchase Handgun Laws in Connecticut and Missouri on Suicide Rates," *Preventive Medicine*, Vol. 79, 2015.

Crifasi, Cassandra K., Keshia M. Pollack, and Daniel W. Webster, "Effects of State-Level Policy Changes on Homicide and Nonfatal Shootings of Law Enforcement Officers," *Injury Prevention*, Vol. 22, No. 4, 2016.

Cummings, P., D. C. Grossman, F. P. Rivara, and T. D. Koepsell, "State Gun Safe Storage Laws and Child Mortality Due to Firearms," *JAMA*, Vol. 278, No. 13, 1997a.

Cumpston, Miranda, Toby Lasserson, Jacqueline Chandler, and Matthew J. Page, "Chapter III: Reporting the Review," in Julian P. T. Higgins, James Thomas, Jacqueline Chandler, Miranda Cumpston, Tianjing Li, Matthew Page, and Vivian Welch, eds., *Cochrane Handbook for Systematic Reviews of Interventions*, Version 6.3, Cochrane Collaboration, February 2022.

Cunningham, Scott, "Callaway and Sant'Anna DD Estimator: A Story of Differential Timing and Heterogeneity," Scott's Substack, March 8, 2021. As of August 21, 2022: https://causalinf.substack.com/p/callaway-and-santanna-dd-estimator

Dalafave, Rachel, "An Empirical Assessment of Homicide and Suicide Outcomes with Red Flag Laws," *Loyola University Chicago Law Journal*, Vol. 52, 2020.

D'Alessio, Stewart J., Lisa Stolzenberg, Rob T. Guerette, and Kristen M. Zgoba, "The Effect of Self-Defense Laws on Firearm Use Among Criminal Offenders," *Crime and Delinquency*, Vol. 69, No. 13–14, December 2022.

de Chaisemartin, Clément, and Xavier D'Haultfoeuille, "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, Vol. 110, No. 9, September 2020.

Degli Esposti, Michelle, Douglas J. Wiebe, Antonio Gasparrini, and David K. Humphreys, "Analysis of 'Stand Your Ground' Self-Defense Laws and Statewide Rates of Homicides and Firearm Homicides," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Degli Esposti, Michelle, Douglas Wiebe, Elinore Kaufman, and Carl Bonander, "Synthetic Control Methodology for Examining Firearm Policy," *Current Epidemiology Reports*, Vol. 9, No. 3, 2022a.

Depew, Briggs, and Isaac Swensen, "The Effect of Concealed-Carry and Handgun Restrictions on Gun-Related Deaths: Evidence from the Sullivan Act of 1911," *Economic Journal*, Vol. 132, No. 646, August 2022.

DeSimone, Jeff, and Sara Markowitz, *The Effect of Child Access Prevention Laws on Non-Fatal Gun Injuries*, National Bureau of Economic Research, Working Paper No. 11613, September 2005.

DeSimone, J., S. Markowitz, and J. Xu, "Child Access Prevention Laws and Nonfatal Gun Injuries," *Southern Economic Journal*, Vol. 80, No. 1, 2013.

Dezhbakhsh, Hashem, and Paul H. Rubin, "Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime," *American Economic Review*, Vol. 88, No. 2, 1998.

Díez, Carolina, Rachel P. Kurland, Emily F. Rothman, Megan Bair-Merritt, Eric Fleegler, Ziming Xuan, Sandro Galea, Craig S. Ross, Bindu Kalesan, Kristin A. Goss, and Michael Siegel, "State Intimate Partner Violence–Related Firearm Laws and Intimate Partner Homicide Rates in the United States, 1991 to 2015," *Annals of Internal Medicine*, Vol. 167, No. 8, 2017.

Donohue, John J., "The Impact of Concealed Carry Laws," in Jens Ludwig and Philip J. Cook, eds., *Evaluating Gun Policy: Effects on Crime and Violence*, Brookings Institution Press, 2003.

Donohue, John J., "Guns, Crime, and the Impact of State Right-to-Carry Laws," *Fordham Law Review*, Vol. 73, No. 2, 2004.

Donohue, John J., "Laws Facilitating Gun Carrying and Homicide," *American Journal of Public Health*, Vol. 107, No. 12, 2017.

Donohue, John J., Abhay Aneja, and Kyle D. Weber, *Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis*, National Bureau of Economic Research, Working Paper No. 23510, November 2018.

Donohue, John J., Abhay Aneja, and Kyle D. Weber, "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data, the LASSO, and a State-Level Synthetic Control Analysis," *Journal of Empirical Legal Studies*, Vol. 16, No. 2, June 2019.

Donohue, John J., Samuel V. Cai, Matthew V. Bondy, and Philip J. Cook, *More Guns, More Unintended Consequences: The Effects of Right-to-Carry on Criminal Behavior and Policing in US Cities*, National Bureau of Economic Research, Working Paper No. 30190, June 2022.

Donohue, John J., Samuel V. Cai, Matthew V. Bondy, and Philip J. Cook, *Why Does Right-to-Carry Cause Violent Crime to Increase?* National Bureau of Economic Research, Working Paper No. 30190, revised June 2023.

Donohue, John J., and Steven D. Levitt, "The Impact of Legalized Abortion on Crime," *Quarterly Journal of Economics*, Vol. 116, No. 2, 2001.

Doucette, Mitchell L., Cassandra K. Crifasi, and Shannon Frattaroli, "Right-to-Carry Laws and Firearm Workplace Homicides: A Longitudinal Analysis (1992–2017)," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Doucette, Mitchell L., Christa Green, Jennifer Necci Dineen, David Shapiro, and Kerri M. Raissian, "Impact of ShotSpotter Technology on Firearm Homicides and Arrests Among Large Metropolitan Counties: A Longitudinal Analysis, 1999–2016," *Journal of Urban Health*, Vol. 98, No. 5, October 2021.

Doucette, Mitchell L., Alexander D. McCourt, Cassandra K. Crifasi, and Daniel W. Webster, "Impact of Changes to Concealed-Carry Weapons Laws on Fatal and Nonfatal Violent Crime, 1980–2019," *American Journal of Epidemiology*, Vol. 192, No. 3, February 24, 2023a.

Doucette, Mitchell L., Julie A. Ward, Alex D. McCourt, Daniel Webster, and Cassandra K. Crifasi, "Officer-Involved Shootings and Concealed Carry Weapons Permitting Laws: Analysis of Gun Violence Archive Data, 2014–2020," *Journal of Urban Health*, Vol. 99, No. 3, 2022.

Duggan, Mark, "More Guns, More Crime," *Journal of Political Economy*, Vol. 109, No. 5, 2001.

Durlauf, Steven, Salvador Navarro, and David Rivers, "Model Uncertainty and the Effect of Shall-Issue Right-to-Carry Laws on Crime," *European Economic Review*, Vol. 81, 2016.

Duwe, Grant, Tomislav Kovandzic, and Carlisle E. Moody, "The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings," *Homicide Studies*, Vol. 6, No. 4, 2002.

Edwards, Griffin, Erik Nesson, Joshua J. Robinson, and Fredrick Vars, "Looking Down the Barrel of a Loaded Gun: The Effect of Mandatory Handgun Purchase Delays on Homicide and Suicide," *Economic Journal*, Vol. 128, No. 616, 2018.

French, B., and P. J. Heagerty, "Analysis of Longitudinal Data to Evaluate a Policy Change," *Statistics in Medicine*, Vol. 27, No. 24, 2008.

Fridel, Emma E., "Comparing the Impact of Household Gun Ownership and Concealed Carry Legislation on the Frequency of Mass Shootings and Firearms Homicide," *Justice Quarterly*, Vol. 38, No. 5, 2021.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Gelman, Andrew, and John Carlin, "Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors," *Perspectives on Psychological Science*, Vol. 9, No. 6, 2014.

Gibbons, Charles E., Juan Carlos Suárez Serrato, and Michael B. Urbancic, "Broken or Fixed Effects?" *Journal of Econometric Methods*, Vol. 8, No. 1, 2019.

Ginwalla, Rashna, Peter Rhee, Randall Friese, Donald J. Green, Lynn Gries, Bellal Joseph, Narong Kulvatunyou, Dafney Lubin, Terence O'Keeffe, Gary Vercruysse, Julie Wynne, and Andrew Tang, "Repeal of the Concealed Weapons Law and Its Impact on Gun-Related Injuries and Deaths," *Journal of Trauma and Acute Care Surgery*, Vol. 76, No. 3, 2014.

Gius, Mark, "An Examination of the Effects of Concealed Weapons Laws and Assault Weapons Bans on State-Level Murder Rates," *Applied Economics Letters*, Vol. 21, No. 4, 2014.

Gius, Mark, "The Effects of State and Federal Background Checks on State-Level Gun-Related Murder Rates," *Applied Economics*, Vol. 47, No. 38, 2015a.

Gius, Mark, "The Impact of Minimum Age and Child Access Prevention Laws on Firearm-Related Youth Suicides and Unintentional Deaths," *Social Science Journal*, Vol. 52, No. 2, 2015b.

Gius, Mark, "The Impact of State and Federal Assault Weapons Bans on Public Mass Shootings," *Applied Economics Letters*, Vol. 22, No. 4, 2015c.

Gius, Mark, "The Relationship Between Stand-Your-Ground Laws and Crime: A State-Level Analysis," *Social Science Journal*, Vol. 53, No. 3, 2016.

Gius, Mark, "Effects of Permit-to-Purchase Laws on State-Level Firearm Murder Rates," *Atlantic Economic Journal*, Vol. 45, No. 1, 2017.

Gius, Mark, "The Effects of State and Federal Gun Control Laws on School Shootings," *Applied Economics Letters*, Vol. 25, No. 5, 2018.

Gius, Mark, "Campus Crime and Concealed Carry Laws: Is Arming Students the Answer?" *Social Science Journal*, Vol. 56, No. 1, March 2019a.

Gius, Mark, "Using the Synthetic Control Method to Determine the Effects of Concealed Carry Laws on State-Level Murder Rates," *International Review of Law and Economics*, Vol. 57, March 2019b.

Gius, Mark, "Examining the Impact of Child Access Prevention Laws on Youth Firearm Suicides Using the Synthetic Control Method," *International Review of Law and Economics*, Vol. 63, September 2020a.

Gius, Mark, "Using the Synthetic Control Method to Determine the Effects of Firearm Seizure Laws on State-Level Murder Rates," *Applied Economics Letters*, Vol. 27, No. 21, 2020b.

Glaeser, Edward L., and Spencer Glendon, "Who Owns Guns? Criminals, Victims, and the Culture of Violence," *American Economic Review*, Vol. 88, No. 2, May 1998.

Gobaud, Ariana N., Ahuva L. Jacobowitz, Christina A. Mehranbod, Nadav L. Sprague, Charles C. Branas, and Christopher N. Morrison, "Place-Based Interventions and the Epidemiology of Violence Prevention," *Current Epidemiology Reports*, Vol. 9, No. 4, December 2022a.

Gobaud, Ariana N., Christina A. Mehranbod, Paul M. Reeping, Brady R. Bushover, and Christopher N. Morrison, "Firearm Assaults Against US Law Enforcement Officers in the Line-of-Duty: Associations with Firearm Ownership and State Firearm Laws," *Preventive Medicine Reports*, Vol. 30, December 2022b.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, Vol. 225, No. 2, December 2021.

Grambsch, P., "Regression to the Mean, Murder Rates, and Shall-Issue Laws," *American Statistician*, Vol. 62, No. 4, 2008.

Guettabi, Mouhcine, and Abdul Munasib, "Stand Your Ground Laws, Homicides and Gun Deaths," *Regional Studies*, Vol. 52, No. 9, 2018.

Hamill, Mark E., Matthew C. Hernandez, Kent R. Bailey, Martin D. Zielinski, Miguel A. Matos, and Henry J. Schiller, "State Level Firearm Concealed-Carry Legislation and Rates of Homicide and Other Violent Crime," *Journal of the American College of Surgeons*, Vol. 228, No. 1, 2019.

Hamlin, Daniel, "Are Gun Ownership Rates and Regulations Associated with Firearm Incidents in American Schools? A Forty-Year Analysis (1980–2019)," *Journal of Criminal Justice*, Vol. 76, September–October 2021.

Hasegawa, Raiden B., Daniel W. Webster, and Dylan S. Small, "Evaluating Missouri's Handgun Purchaser Law: A Bracketing Method for Addressing Concerns About History Interacting with Group," *Epidemiology*, Vol. 30, No. 3, May 2019.

Helland, E., and A. Tabarrok, "Using Placebo Laws to Test 'More Guns, Less Crime,'" *Advances in Economic Analysis and Policy*, Vol. 4, No. 1, 2004.

Hempstead, Katherine, and Antonio Rodríguez Andrés, *Gun Control and Suicide: The Impact of State Firearm Regulations, 1995–2004*, Institute for Advanced Development Studies, Development Research Working Paper No. 17/2009, December 2009.

Hepburn, L., D. Azrael, M. Miller, and D. Hemenway, "The Effect of Child Access Prevention Laws on Unintentional Child Firearm Fatalities, 1979–2000," *Journal of Trauma: Injury Infection and Critical Care*, Vol. 61, No. 2, 2006.

Hepburn, Lisa, Matthew Miller, Deborah Azrael, and David Hemenway, "The Effect of Nondiscretionary Concealed Weapon Carrying Laws on Homicide," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 56, No. 3, 2004.

Humphreys, David K., Antonio Gasparrini, and Douglas J. Wiebe, "Evaluating the Impact of Florida's 'Stand Your Ground' Self-Defense Law on Homicide and Suicide by Firearm: An Interrupted Time Series Study," *JAMA Internal Medicine*, Vol. 177, No. 1, 2017.

Imai, Kosuke, and In Song Kim, "On the Use of Two-Way Fixed Effects Regression Models for Causal Inference with Panel Data," *Political Analysis*, Vol. 29, No. 3, July 2021.

John Jay College Research Advisory Group on Preventing and Reducing Community Violence, *Reducing Violence Without Police: A Review of Research Evidence*, Research and Evaluation Center, John Jay College of Criminal Justice, City University of New York, November 2020.

Kagawa, Rose M. C., Alvaro Castillo-Carniglia, Jon S. Vernick, Daniel Webster, Cassandra Crifasi, Kara E. Rudolph, Magdalena Cerdá, Aaron Shev, and Garen J. Wintemute, "Repeal of Comprehensive Background Check Policies and Firearm Homicide and Suicide," *Epidemiology*, Vol. 29, No. 4, 2018.

Kagawa, Rose, Amanda Charbonneau, Christopher McCort, Alexander McCourt, Jon Vernick, Daniel Webster, and Garen Wintemute, "Effects of Comprehensive Background-Check Policies on Firearm Fatalities in 4 States," *American Journal of Epidemiology*, Vol. 192, No. 4, April 2023.

Kalesan, Bindu, Kinan Lagast, Marcos Villarreal, Elizabeth Pino, Jeffrey Fagan, and Sandro Galea, "School Shootings During 2013–2015 in the USA," *Injury Prevention*, Vol. 23, No. 5, 2017.

Kappelman, Jack, and Richard C. Fording, "The Effect of State Gun Laws on Youth Suicide by Firearm: 1981–2017," *Suicide and Life-Threatening Behavior*, Vol. 51, No. 2, April 2021.

Kaufman, Elinore J., Christopher N. Morrison, Erik J. Olson, David K. Humphreys, Douglas J. Wiebe, Niels D. Martin, Carrie A. Sims, Mark H. Hoofnagle, C. William Schwab, Patrick M. Reilly, and Mark J. Seamon, "Universal Background Checks for Handgun Purchases Can Reduce Homicide Rates of African Americans," *Journal of Trauma and Acute Care Surgery*, Vol. 88, No. 6, June 2020.

Kendall, Todd D., and Robert Tamura, "Unmarried Fertility, Crime, and Social Stigma," *Journal of Law and Economics*, Vol. 53, No. 1, 2010.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Kivisto, Aaron J., Katherine L. Kivisto, Erica Gurnell, Peter Phalen, and Bradley Ray, "Adolescent Suicide, Household Firearm Ownership, and the Effects of Child Access Prevention Laws," *Journal of the American Academy of Child and Adolescent Psychiatry*, Vol. 60, No. 9, September 2021.

Kivisto, Aaron J., and Peter L. Phalen, "Effects of Risk-Based Firearm Seizure Laws in Connecticut and Indiana on Suicide Rates, 1981–2015," *Psychiatric Services*, Vol. 69, No. 8, 2018.

Klarevas, Louis, Andrew Conner, and David Hemenway, "The Effect of Large-Capacity Magazine Bans on High-Fatality Mass Shootings, 1990–2017," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Kondo, Michelle C., Elena Andreyeva, Eugenia C. South, John M. MacDonald, and Charles C. Branas, "Neighborhood Interventions to Reduce Violence," *Annual Review of Public Health*, Vol. 39, April 2018.

Koper, Christopher S., "Federal Legislation and Gun Markets: How Much Have Recent Reforms of the Federal Firearms Licensing System Reduced Criminal Gun Suppliers?" *Criminology and Public Policy*, Vol. 1, No. 2, 2002.

Koper, Christopher S., *Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence 1994–2003*, National Institute of Justice, 2004.

Koper, Christopher S., and Jeffrey A. Roth, "The Impact of the 1994 Federal Assault Weapon Ban on Gun Violence Outcomes: An Assessment of Multiple Outcome Measures and Some Lessons for Policy Evaluation," *Journal of Quantitative Criminology*, Vol. 17, No. 1, 2001.

Koper, Christopher S., and Jeffrey A. Roth, "Impact of the 1994 Federal Assault Weapons Ban on Gun Markets: An Assessment of Short-Term Primary and Secondary Market Effects," *Journal of Quantitative Criminology*, Vol. 18, No. 3, 2002.

Kovandzic, T. V., T. B. Marvell, and L. M. Vieraitis, "The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates—Evidence from Panel Data for Large Urban Cities," *Homicide Studies*, Vol. 9, No. 4, 2005.

La Valle, James M., "Rebuilding at Gunpoint: A City-Level Re-Estimation of the Brady Law and RTC Laws in the Wake of Hurricane Katrina," *Criminal Justice Policy Review*, Vol. 18, No. 4, 2007.

La Valle, James M., "Re-Estimating Gun-Policy Effects According to a National Science Academy Report: Were Previous Reports of Failure Premature?" *Journal of Crime and Justice*, Vol. 33, No. 1, 2010.

La Valle, James M., "'Gun Control' vs. 'Self-Protection': A Case Against the Ideological Divide," *Justice Policy Journal*, Vol. 10, No. 1, 2013.

La Valle, James M., and Thomas C. Glover, "Revisiting Licensed Handgun Carrying: Personal Protection or Interpersonal Liability?" *American Journal of Criminal Justice*, Vol. 37, No. 4, 2012.

Levy, Marc, Wilmer Alvarez, Lauren Vagelakos, Michelle Yore, and Bertha Ben Khallouq, "Stand Your Ground: Policy and Trends in Firearm-Related Justifiable Homicide and Homicide in the US," *Journal of the American College of Surgeons*, Vol. 230, No. 1, January 2020.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Lott, John R., Jr., "The Concealed-Handgun Debate," *Journal of Legal Studies*, Vol. 27, No. 1, January 1998a.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 1st ed., University of Chicago Press, 1998b.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 2nd ed., University of Chicago Press, 2000.

Lott, John R., Jr., *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong*, Regnery Publishing, Inc., 2003.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Lott, John R., Jr., and William M. Landes, *Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement*, University of Chicago Law School, John M. Olin Law and Economics Working Paper No. 73, 1999.

Lott, John R., Jr., and D. B. Mustard, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," *Journal of Legal Studies*, Vol. 26, No. 1, 1997.

Lott, John R., Jr., and John E. Whitley, "Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime," *Journal of Law and Economics*, Vol. 44, No. 2, 2001.

Lott, John R., Jr., and John E. Whitley, "Measurement Error in County-Level UCR Data," *Journal of Quantitative Criminology*, Vol. 19, No. 2, June 2003.

Lott, John R., Jr., and John E. Whitley, "Abortion and Crime: Unwanted Children and Out-of-Wedlock Births," *Economic Inquiry*, Vol. 45, No. 2, 2007.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, *The Impact of Mass Shootings on Gun Policy*, working paper, Harvard Business School, 2016.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, 2020.

Ludwig, Jens, "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data," *International Review of Law and Economics*, Vol. 18, No. 3, 1998.

Ludwig, J., and P. J. Cook, "Homicide and Suicide Rates Associated with Implementation of the Brady Handgun Violence Prevention Act," *JAMA*, Vol. 284, No. 5, 2000.

Manski, Charles F., and John V. Pepper, *How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions*, National Bureau for Economic Research, NBER Working Paper 21701, November 2015.

Manski, Charles F., and John V. Pepper, "How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions," *Review of Economics and Statistics*, Vol. 100, No. 2, 2018.

Martin, Robert A., and Richard L. Legault, "Systematic Measurement Error with State-Level Crime Data: Evidence from the 'More Guns, Less Crime' Debate," *Journal of Research in Crime and Delinquency*, Vol. 42, No. 2, May 2005.

Marvell, Thomas B., "The Impact of Banning Juvenile Gun Possession," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

McClellan, Chandler, and Erdal Tekin, "Stand Your Ground Laws, Homicides, and Injuries," *Journal of Human Resources*, Vol. 52, No. 3, 2017.

McCourt, Alexander D., Cassandra K. Crifasi, Elizabeth A. Stuart, Jon S. Vernick, Rose M. C. Kagawa, Garen J. Wintemute, and Daniel W. Webster, "Purchaser Licensing, Point-of-Sale Background Check Laws, and Firearm Homicide and Suicide in 4 US States, 1985–2017," *American Journal of Public Health*, Vol. 110, No. 10, October 2020.

Mill, John Stuart, *A System of Logic*, Parker, 1843.

Miller, Megan, and John Pepper, "Assessing the Effect of Firearms Regulations Using Partial Identification Methods: A Case Study of the Impact of Stand Your Ground Laws on Violent Crime," *Law and Contemporary Problems*, Vol. 83, 2020.

Moe, Caitlin A., Miriam J. Haviland, Andrew G. Bowen, Ali Rowhani-Rahbar, and Frederick P. Rivara, "Association of Minimum Age Laws for Handgun Purchase and Possession with Homicides Perpetrated by Young Adults Aged 18 to 20 Years," *JAMA Pediatrics*, Vol. 174, No. 11, 2020.

Monroe, Jeffrey D., *Homicide and Gun Control: The Brady Handgun Violence Prevention Act and Homicide Rates*, LFB Scholarly Publishing, 2008.

Moody, Carlisle E., "Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Moody, Carlisle E., and Thomas B. Marvell, "The Debate on Shall-Issue Laws," *Econ Journal Watch*, Vol. 5, No. 3, 2008.

Moody, Carlisle E., and Thomas B. Marvell, "The Debate on Shall-Issue Laws, Continued," *Econ Journal Watch*, Vol. 6, No. 2, 2009.

Moody, Carlisle E., and Thomas B. Marvell, "The Impact of Right-to-Carry Laws: A Critique of the 2014 Version of Aneja, Donohue, and Zhang," *Econ Journal Watch*, Vol. 15, No. 1, January 2018a.

Moody, Carlisle E., and Thomas B. Marvell, "Clustering and Standard Error Bias in Fixed Effects Panel Data Regressions," *Journal of Quantitative Criminology*, May 2018b.

Moody, Carlisle E., Thomas B. Marvell, Paul R. Zimmerman, and Fasil Alemante, "The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication," *Review of Economics and Finance*, Vol. 4, 2014.

Munasib, Abdul, Genti Kostandini, and Jeffrey L. Jordan, "Impact of the Stand Your Ground Law on Gun Deaths: Evidence of a Rural Urban Dichotomy," *European Journal of Law and Economics*, Vol. 45, No. 3, 2018.

Mustard, David B., "The Impact of Gun Laws on Police Deaths," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Oliphant, Stephen N., "Effects of Wisconsin's Handgun Waiting Period Repeal on Suicide Rates," *Injury Prevention*, Vol. 28, No. 6, 2022.

Olson, David E., and Michael D. Maltz, "Right-to-Carry Concealed Weapon Laws and Homicide in Large U.S. Counties: The Effect on Weapon Types, Victim Characteristics, and Victim-Offender Relationships," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Pear, Veronica A., Garen J. Wintemute, Nicholas P. Jewell, and Jennifer Ahern, "Firearm Violence Following the Implementation of California's Gun Violence Restraining Order Law," *JAMA Network Open*, Vol. 5, No. 4, 2022a.

Phipson, Belinda, and Gordon K. Smyth, "Permutation P-Values Should Never Be Zero: Calculating Exact P-Values When Permutations Are Randomly Drawn," *Statistical Applications in Genetics and Molecular Biology*, Vol. 9, No. 1, 2010.

Plassmann, Florenz, and T. Nicolaus Tideman, "Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Plassmann, F., and J. E. Whitley, "Comments: Confirming 'More Guns, Less Crime,'" *Stanford Law Review*, Vol. 55, No. 4, 2003.

Raifman, Julia, Elysia Larson, Colleen L. Barry, Michael Siegel, Michael Ulrich, Anita Knopov, and Sandro Galea, "State Handgun Purchase Age Minimums in the US and Adolescent Suicide Rates: Regression Discontinuity and Difference-in-Differences Analyses," *BMJ*, Vol. 370, 2020.

Raissian, Kerri M., "Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?" *Journal of Policy Analysis and Management*, Vol. 35, No. 1, Winter 2016.

Ramchand, Rajeev, and Jessica Saunders, "The Effects of the 1996 National Firearms Agreement in Australia on Suicide, Homicide, and Mass Shootings," in Rajeev Ramchand and Jessica Saunders, eds., *Contemporary Issues in Gun Policy: Essays from the RAND Gun Policy in America Project*, RAND Corporation, RR-A243-2, 2021. As of August 31, 2022: https://www.rand.org/pubs/research_reports/RRA243-2.html

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of August 22, 2019: https://www.rand.org/pubs/research_reports/RR2088.html

Roberts, Darryl W., "Intimate Partner Homicide: Relationships to Alcohol and Firearms," *Journal of Contemporary Criminal Justice*, Vol. 25, No. 1, 2009.

Rochford, Hannah I., Mark Berg, and Corinne Peek-Asa, "The 'Boyfriend Loophole' and Intimate Partner Homicides: A Longitudinal Analysis Using the National Violent Death Reporting System," *Journal of Prevention*, Vol. 43, No. 6, December 2022.

Rogna, Marco, and Bich Diep Nguyen, "Firearms Law and Fatal Police Shootings: A Panel Data Analysis," *Applied Economics*, Vol. 54, No. 27, 2022.

Rosengart, M., P. Cummings, A. Nathens, P. Heagerty, R. Maier, and F. Rivara, "An Evaluation of State Firearm Regulations and Homicide and Suicide Death Rates," *Injury Prevention*, Vol. 11, No. 2, 2005.

Roth, Jeffrey A., and Christopher S. Koper, *Impact Evaluation of the Public Safety and Recreational Firearms Use Protection Act of 1994: Final Report*, Urban Institute, 1997.

Roth, Jeffrey A., and Christopher S. Koper, *Impacts of the 1994 Assault Weapons Ban: 1994–96*, National Institute of Justice, March 1999.

Rubin, Paul H., and Hashem Dezhbakhsh, "The Effect of Concealed Handgun Laws on Crime: Beyond the Dummy Variables," *International Review of Law and Economics*, Vol. 23, No. 2, 2003.

Rudolph, K. E., E. A. Stuart, J. S. Vernick, and D. W. Webster, "Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides," *American Journal of Public Health*, Vol. 105, No. 8, 2015.

Saadi, Altaf, Kristen R. Choi, Sae Takada, and Fred J. Zimmerman, "The Impact of Gun Violence Restraining Order Laws in the U.S. and Firearm Suicide Among Older Adults: A Longitudinal State-Level Analysis, 2012–2016," *BMC Public Health*, Vol. 20, No. 334, 2020.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Scantling, Dane R., Daniel N. Holena, Elinore J. Kaufman, Allyson M. Hynes, Justin Hatchimonji, James P. Byrne, Douglas Wiebe, and Mark J. Seamon, "Modifiable Factors Related to Firearm Homicides: A Broader View of Our Lane," *Annals of Surgery*, 2022, online ahead of print.

Schell, Terry L., Matthew Cefalu, Beth Ann Griffin, Rosanna Smart, and Andrew R. Morral, "Changes in Firearm Mortality Following the Implementation of State Laws Regulating Firearm Access and Use," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 26, 2020.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Schell, Terry L., Rosanna Smart, and Andrew R. Morral, *Suggestions for Estimating the Effects of State Gun Policies: Commentary on Four Methodological Problems in the Current Literature*, RAND Corporation, RR-A243-1, 2022. As of August 9, 2022:
https://www.rand.org/pubs/research_reports/RRA243-1.html

Sen, B., and A. Panjamapirom, "State Background Checks for Gun Purchase and Firearm Deaths: An Exploratory Study," *Preventive Medicine*, Vol. 55, No. 4, 2012.

Shi, Wei, and Lung-Fei Lee, "The Effects of Gun Control on Crimes: A Spatial Interactive Fixed Effects Approach," *Empirical Economics*, Vol. 55, No. 1, 2018.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Siegel, Michael, Benjamin Solomon, Anita Knopov, Emily F. Rothman, Shea W. Cronin, Ziming Xuan, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates in Suburban and Rural Areas Compared to Large Cities in the United States, 1991–2016," *Journal of Rural Health*, Vol. 36, No. 2, 2020b.

Siegel, Michael, Ziming Xuan, Craig S. Ross, Sandro Galea, Bindu Kalesan, Eric Fleegler, and Kristin A. Goss, "Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States," *American Journal of Public Health*, Vol. 107, No. 12, 2017b.

Smart, Rosanna, Andrew R. Morral, Rajeev Ramchand, Amanda Charbonneau, Jhacova Williams, Sierra Smucker, Samantha Cherney, and Lea Xenakis, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States, Third Edition*, RAND Corporation, RR-A243-4, 2023. As of March 28, 2024: https://www.rand.org/pubs/research_reports/RRA243-4.html

Smart, Rosanna, Andrew R. Morral, and Terry L. Schell, *The Magnitude and Sources of Disagreement Among Gun Policy Experts*, 2nd ed., RAND Corporation, RR-A243-3, 2021. As of August 9, 2022: https://www.rand.org/pubs/research_reports/RRA243-3.html

Smart, Rosanna, Andrew R. Morral, Sierra Smucker, Samantha Cherney, Terry L. Schell, Samuel Peterson, Sangeeta C. Ahluwalia, Matthew Cefalu, Lea Xenakis, Rajeev Ramchand, and Carole Roan Gresenz, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, 2nd ed., RAND Corporation, RR-2088-1-RC/AV, 2020. As of September 7, 2021: https://www.rand.org/pubs/research_reports/RR2088-1.html

Smith, Michael R., and Matthew Petrocelli, "The Effect of Concealed Handgun Carry Deregulation in Arizona on Crime in Tucson," *Criminal Justice Policy Review*, Vol. 30, No. 8, 2019.

Steidley, Trent, "The Effect of Concealed Carry Weapons Laws on Firearm Sales," *Social Science Research*, Vol. 78, February 2019.

Steidley, Trent, and Martin T. Kosla, "Toward a Status Anxiety Theory of Macro-Level Firearm Demand," *Social Currents*, Vol. 5, No. 1, 2018.

Stovold, Elizabeth, Deirdre Beecher, Ruth Foxlee, and Anna Noel-Storr, "Study Flow Diagrams in Cochrane Systematic Review Updates: An Adapted PRISMA Flow Diagram," *Systematic Reviews*, Vol. 3, No. 54, May 2014.

Strnad, Jeff, "Should Legal Empiricists Go Bayesian?" *American Law and Economics Review*, Vol. 9, No. 1, Spring 2007.

Swanson, Jeffrey W., Michele M. Easter, Allison G. Robertson, Marvin S. Swartz, Kelly Alanis-Hirsch, Daniel Moseley, Charles Dion, and John Petrila, "Gun Violence, Mental Illness, and Laws That Prohibit Gun Possession: Evidence from Two Florida Counties," *Health Affairs*, Vol. 35, No. 6, 2016.

Swanson, J. W., A. G. Robertson, L. K. Frisman, M. A. Norko, H. Lin, M. S. Swartz, and P. J. Cook, "Preventing Gun Violence Involving People with Serious Mental Illness," in D. W. Webster and J. S. Vernick, eds., *Reducing Gun Violence in America: Informing Policy with Evidence and Analysis*, Johns Hopkins University Press, 2013.

Van Der Wal, Willem M., "Marginal Structural Models to Estimate Causal Effects of Right-to-Carry Laws on Crime," *Statistics and Public Policy*, Vol. 9, No. 1, 2022.

Vigdor, E. R., and J. A. Mercy, "Disarming Batterers: The Impact of Domestic Violence Firearms Laws," in Jens Ludwig and Philip J. Cook, eds., *Evaluating Gun Policy: Effects on Crime and Violence*, Brookings Institution Press, 2003.

Vigdor, E. R., and J. A. Mercy, "Do Laws Restricting Access to Firearms by Domestic Violence Offenders Prevent Intimate Partner Homicide?" *Evaluation Review*, Vol. 30, No. 3, 2006.

Wallace, Lacey N., "Castle Doctrine Legislation: Unintended Effects for Gun Ownership?" *Justice Policy Journal*, Vol. 11, No. 2, Fall 2014.

Wallace, Maeve E., Dovile Vilda, Katherine P. Theall, and Charles Stoecker, "Firearm Relinquishment Laws Associated with Substantial Reduction in Homicide of Pregnant and Postpartum Women," *Health Affairs*, Vol. 40, No. 10, September 22, 2021.

Wallin, Mikaela A., Charvonne N. Holliday, and April M. Zeoli, "The Association of Federal and State-Level Firearm Restriction Policies with Intimate Partner Homicide: A Re-Analysis by Race of the Victim," *Journal of Interpersonal Violence*, Vol. 37, No. 17–18, September 2022.

Webster, D., C. K. Crifasi, and J. S. Vernick, "Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides," *Journal of Urban Health*, Vol. 91, No. 2, 2014.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Webster, Daniel W., and Marc Starnes, "Reexamining the Association Between Child Access Prevention Gun Laws and Unintentional Shooting Deaths of Children," *Pediatrics*, Vol. 106, No. 6, 2000.

Webster, Daniel W., Jon S. Vernick, and Lisa M. Hepburn, "Effects of Maryland's Law Banning 'Saturday Night Special' Handguns on Homicides," *American Journal of Epidemiology*, Vol. 155, No. 5, 2002.

Webster, D. W., J. S. Vernick, A. M. Zeoli, and J. A. Manganello, "Association Between Youth-Focused Firearm Laws and Youth Suicides," *JAMA*, Vol. 292, No. 5, 2004.

Wooldridge, J. M., *Econometric Analysis of Cross Section and Panel Data*, MIT Press, 2002.

Zeoli, April M., Alexander McCourt, Shani Buggs, Shannon Frattaroli, David Lilley, and Daniel W. Webster, "Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Associations with Intimate Partner Homicide," *American Journal of Epidemiology*, Vol. 187, No. 11, 2018.

Zeoli, A. M., and D. W. Webster, "Effects of Domestic Violence Policies, Alcohol Taxes and Police Staffing Levels on Intimate Partner Homicide in Large U.S. Cities," *Injury Prevention*, Vol. 16, No. 2, 2010.

Zimmerman, Paul R., "The Deterrence of Crime Through Private Security Efforts: Theory and Evidence," *International Review of Law and Economics*, Vol. 37, 2014.

PART B

Evidence for the Effects of Policies Regulating Who May Legally Own, Purchase, or Possess Firearms

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 3)
- prohibitions associated with mental illness (Chapter 4)
- prohibitions associated with domestic violence (Chapter 5)
- surrender of firearms by prohibited possessors (Chapter 6)
- extreme-risk protection orders (Chapter 7).

CHAPTER 3

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 firearms through formal channels. Because background check systems have not routinely accessed juvenile records, establishing a minimum age for firearm purchase above the age of criminal majority can also allow for more time for patterns of adult criminal behavior or other indicators of risk of harm to be observed and recorded in the background check system.[1] This mechanism may become less important over time because 2022 federal legislation expanded background check procedures for firearm purchasers between 18 and 21 years of age to screen for disqualifying juvenile justice or mental health records and to, as necessary, allow authorities additional time to make these determinations (18 U.S.C. 922(t)).

Laws requiring a minimum age of possession are instead 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 restrictions on purchase ages and restrictions on possession ages 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, although individuals between the ages of 15 and 20 comprise an estimated 7.8 percent of the U.S. population (U.S. Census Bureau, 2022), they accounted for approximately 14 percent of violent crime arrests in 2022 (Federal Bureau of Investigation [FBI], 2023a).[2] Of the 12,319 firearm homicides committed in 2020 for which the age of the offender was known, 44 percent were perpetrated by individuals aged 12–24 (Puzzanchera, Chamberlin, and Kang, 2021), although this group represents only 17 percent of the general U.S. population (U.S. Census Bureau, 2022). By influencing the possession of

[1] While most states set an age of criminal majority at 17 or 18, there has been substantial variability across states and over time (Arora, 2023).

[2] In 2022, 93.5 percent of the U.S. population was covered by law enforcement agencies that submitted data to the FBI's National Incident-Based Reporting System (FBI, 2023b).

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 2021 deaths among those aged 16–20, 20.3 percent were homicides, 94.7 percent of which were firearm homicides. This is greater than the relative proportions for other high-risk age groups (14.5 percent for those aged 21–25, 91.9 percent of which were firearm homicides; 10.7 percent for those aged 26–30, 89.2 percent of which were firearm homicides) (calculated using data from the Centers for Disease Control and Prevention [CDC], undated-f). Furthermore, in 2021, among violent crimes for which offender age was known and that were reported by law enforcement agencies to the FBI, 60 percent of violent victimizations of juveniles involved an adult offender (Bureau of Justice Statistics, 2022).[3] 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 a strong association between firearm availability and suicide among adolescents and young adults (Birckmayer and Hemenway, 2001; Miller and Hemenway, 1999). In 2021, there were 2,735 suicide deaths among individuals aged 16–20, 51.3 percent of which involved a firearm (calculated using data from CDC, undated-f). Between 50 percent and 65 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 (Barber et al., 2022; 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). Another study of suicides in five states from 2015 to 2017 (Barber et al., 2022) found that individuals under age 21 who used their own guns to complete suicide predominantly used long guns (56.9 percent of 76 deaths); those using someone else's gun predominantly used handguns (70.5 percent of 173 deaths).

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

[3] In 2021, 65 percent of the U.S. population was covered by law enforcement agencies that reported crimes to the FBI's National Incident-Based Reporting System (FBI, 2023e).

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). In the five-state study mentioned previously, Barber and colleagues (2022) found that decedents used their own firearms in 84.1 percent of suicides among those ages 21 and older, 42.1 percent among those ages 18 to 20, and 18.7 percent among children under age 18; children most commonly acquired the firearms through parents or other family. These findings suggest that minimum age laws may be effective at limiting youth access to firearms through legitimate retail sources.

Still, 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. As of April 2021, an estimated 30 million children lived in households with firearms; in more than 15 percent of households, at least one gun was stored loaded and unlocked (Miller and Azrael, 2022).

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 and 2019, 6.8 percent of high school males and 1.9 percent of high school females reported carrying a gun on at least one day in the past year for purposes other than hunting or recreation (Simon et al., 2022). 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 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, because most data sources report only the age of the victim,[4] only two studies we identified that met our inclusion criteria for this policy used data on shooter age (Marvell, 2001; Moe et al., 2020). 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 or youth under age 18, focused on handguns, not representative at the state level, or available for a limited set of years.

State Implementation of Minimum Age Laws

As of January 1, 2024, 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,[5]

[4] Exceptions include the FBI's Supplementary Homicide Reports and National Incident-Based Reporting System, which contains age of victim and age of offender when such information is known, and the National Violent Death Reporting System, which contains information on the age of the shooter for non-self-inflicted fatal injuries when such information is known. In 2022, 27.5 percent of homicides involved a perpetrator of unknown age (FBI, 2023c).

[5] California, Connecticut, Delaware, Florida, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Nebraska, New Jersey, New York, Ohio, Rhode Island, Vermont, Washington, Wyoming, and the District of Columbia. See Calif. Penal Code § 27505; Conn. Gen. Stat. § 29-34(b); Del. Code Ann. Tit. 24, §§ 901, 903; Fla. Stat.

and eight states and the District of Columbia have minimum age requirements that exceed the federal minimum for handgun possession.[6] 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 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.[7] Five states restrict sales for all firearms to those aged 21 or older.[8] 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.[9] 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, 12 states restrict possession of long guns to those aged 18 or older;[10] Illinois and the District of Columbia restrict all long gun possession to those aged 21 or older, while Washington limits posses-

§ 790.065(13); Hawaii Rev. Stat. Ann. § 134-2(d); 430 Ill. Comp. Stat. 65/4(a); Ia. Code § 724.22(1),(2); Md. Ann. Code Art. 27 § 5-134(b); Mass. Gen. Laws Ch. 140 § 130; Neb. Rev. Stat. § 69-2404; N.J. Stat. Ann. § 2C:58-6.1(a); N.Y. Penal Law § 400; Ohio Rev. Code Ann. §§ 2923.21, 2923.211; R.I. Gen. Laws §§ 11-47-30(a), 11-47-35(a)(1), 11-47-37; 13 Vt. Stat. Ann. § 4020(a); Wash. Rev. Code Ann. § 9.41.241; Wyo. S.1977 § 6-8-404(d); D.C. Code Ann. § 22-4507.

6 Illinois, Iowa, Maryland, Massachusetts, New Jersey, New Mexico, New York, Washington, and the District of Columbia. See 430 Ill. Comp. Stat. 65/2, 65/4(a); Ia. Code § 724.22; Md. Code Ann., Pub. Safety § 5-133(d); Mass. Gen. Laws Ann. Ch. 140, § 131; N.J. Stat. Ann. § 2C:58-6.1; N.M. Stat. Ann. § 30-7-2.2; N.Y. Penal Law § 400; Wash. Rev. Code Ann. § 9.41.240; D.C. Code § 7-2502.03.

7 Connecticut, Delaware, Iowa, Maryland, Massachusetts, New Jersey, Ohio, Rhode Island, and the District of Columbia. See Conn. Gen. Stat. Ann. §§ 29-34, 29-37a; Del. Code Tit. 24 § 901, 903, Tit. 11 § 1445; Ia. Code Ann. § 724.22; Md. Code Ann., Pub. Safety §§ 5-101, 5-134; Mass. Gen. Laws Ch. 140 §§ 121, 130; N.J. Stat. Ann. § 2C:39-10; Ohio Rev. Code Ann. § 2923.21; R.I. Gen. Laws §§ 11-47-30, 11-47-35; D.C. Code Ann. §§ 7-2507.07, 22-4507.

8 California, Florida, Hawaii, Illinois, and Vermont. See Calif. Penal Code § 27505; Fla. Stat. Ann. § 790.22; Hawaii Rev. Stat. Ann. § 134-2; Ill. Comp. Stat. 65/3, 65/4; 13 Vt. Stat. Ann. § 4020(a). Although Hawaii's law is silent about sales, the state requires permits for all purchases, and permits are issued only to those aged 21 or older. Illinois requires a firearm owner's identification card for transfer, and the card is issued only to those aged 21 or older. However, 720 Ill. Comp. Stat. 5/24-3.1 prohibits sales of handguns to those under age 18.

9 Me. Rev. Stat. Ann. Tit. 17-A §§ 554-A, 554-B.

10 Idaho, Indiana, Iowa, Michigan, Nevada, New Jersey, Oklahoma, Oregon, Pennsylvania, Rhode Island, Utah, and Wisconsin. See Idaho Code Ann. § 18-3302E; Ind. Ann. Code § 35-47-10-5; Ia. Code Ann. § 724.22; Mich. Comp. Laws Ann. § 750.234f; Nev. Rev. Stat. Ann. § 202.300; N.J. Stat Ann. § 2C:58-6.1; Okla. Stat. Ann. §§ 1272, 1273; Ore. Rev. Stat. Ann. § 166.250; Pa. Cons. Stat. § 6110.1; R.I. Gen. Laws Ann. § 11-47-33; Utah Code Ann. § 76-10-509; Wisc. Stat. § 948.60.

sion of just semiautomatic rifles.[11] The minimum age for possession of a long gun in Alaska, Minnesota, and New York is 16,[12] and it is 14 in Montana.[13]

Effects on Suicide

Research Synthesis Findings

We identified nine quasi-experimental longitudinal studies providing evidence on the impact of minimum age requirements on suicide, one of which (Bhatt et al., 2020) contributes new findings to this updated review.

Minimum Age Requirements for Purchasing a Firearm

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. Estimating negative binomial regression models that employed generalized estimating equations and that included state fixed effects, year fixed effects, and other covariates, they found uncertain effects of state or federal laws that increased the minimum age of purchase to 18 on suicide rates among those aged 14–17. Examining the population aged 18–20, however, Webster et al. (2004) found that state laws increasing the minimum purchase age to 21 were associated with a statistically significant decrease in firearm suicides, although effects on total suicides were uncertain. Another study using a similar statistical approach but covering a shorter time frame (1978 to 1998) found mostly uncertain effects of increasing the minimum purchase age to 21 on firearm or total suicide rates (Rosengart et al., 2005). Estimates from both studies are subject to minor methodological concerns related to limited discussion of or justification for their models' identifying assumptions.

Evaluating a more recent time frame, Kappelman and Fording (2021) examined the effects of minimum purchase age laws and, separately, minimum possession age laws on suicides among those aged 24 and younger between 1981 and 2017. They included indicators for "strict" or "weak" child-access prevention laws (described in Chapter 17) in negative binomial regression models. The regression models accounted for state and year fixed effects, the lagged number of suicide deaths, and robust standard errors to account for serial correlation. The authors found that minimum age of purchase laws that set higher age limits than federal law were associated with significant reductions in firearm suicides among those aged 24 or younger, while there were uncertain effects on nonfirearm suicides. Post-hoc analyses stratified by gender suggested that these effects were driven by an effect among boys and young

[11] Ill. Comp. Stat. Ann. 65/4 (regulates the firearm owner's identification card); D.C. Code Ann. § 7-2502.03; Washington Rev. Code Ann. § 9.41.241.

[12] 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.

[13] Mont. Code Ann. § 45-8-344.

men aged 24 or younger. An earlier analysis by Andrés and Hempstead (2011), focusing on suicides by men and the 1995–2004 period after the federal minimum age law was enacted, similarly found that bans on the purchase of firearms by minors were associated with significantly lower rates of suicide among men aged 15–44; the relationship of the laws to suicide rates among the older adult men was uncertain. However, the authors did not appear to adjust standard errors to account for serial correlation, threatening the validity of their estimated confidence intervals (CIs).

Finally, Raifman et al. (2020) examined the effects of minimum age laws for handgun purchase on suicide rates among those aged 13–20 using two approaches. In the first approach, the authors examined the extent to which suicide rates in adolescents aged 18–20 diverged from those of adolescents aged 13–17 in states where the minimum age required to purchase a handgun is 18 or older. Although this approach showed an increase in suicide rates among young adults aged 18–20 (compared with those of individuals aged 21 or older), the model is limited in its design. The authors had relatively crude measures of age in years (rather than being able to examine a narrow window around individuals' 21st birthdays), and they provided no evidence that potentially confounding variables were comparable before and after the selected age cutoff. Instead, the authors showed that the percentage of adolescents in each racial group was similar around the cutoff. To better identify causal effects, we would need information regarding other potentially confounding variables (e.g., alcohol consumption) and those variables' prevalence around the cutoff.

The authors' second approach used a difference-in-differences strategy to examine temporal trends in states that changed their minimum age requirements relative to states with no policy change. The authors used a linear regression model that included state and year fixed effects (standard errors were clustered at the state level), and the results showed that state policies that limited the age to purchase a handgun to 21 or older were associated with significantly lower suicide rates for young adults aged 18–20. By having fewer than ten observations per estimated parameter, the model may have been overfit, prompting minor concerns that the model's estimates and standard errors may be unreliable (Freedman, 2006). Furthermore, sensitivity analyses suggested that the authors' findings were largely driven by policy changes that lowered the minimum purchase age in two states (Missouri and South Carolina), raising some questions about the generalizability of their findings.

Minimum Age Requirements for Possessing a Firearm

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. Using negative binomial regression models that employed generalized estimating equations and that included state fixed effects, year fixed effects, and other covariates, they found mostly uncertain effects of state or federal laws that increase the minimum possession age to 18 on suicide rates among those aged 14–17; there is the exception that states affected by the federal law change saw a suggestive increase in nonfirearm suicides. In contrast, state laws increasing the age of handgun possession to 21 were associated with a statistically sig-

nificant increase in total suicides among those aged 18–20; this increase accounted for, in part, a suggestive increase in firearm suicides in this group. However, effects of laws setting a minimum age of 21 for possession were identified from policy changes in just three states, two of which implemented such laws in the final years of the study period. This small sample offers little time over which to observe changes in state suicide rates that may be attributable to the law, raising serious methodological concerns. Another study using a similar statistical approach but covering a shorter time frame (1979 to 1998) found uncertain effects of increasing the minimum possession age to 21 on firearm or total suicide rates (Rosengart et al., 2005). This study differed from Webster et al. (2004) in that the authors seemingly treated minimum age of possession laws implemented via permitting schemes (see Chapter 9) as equivalent to minimum age laws. While this decision creates more policy transitions that contribute to identifying effects, there may be a trade-off in pooling effect estimates across different state laws with plausibly heterogeneous effects on suicide rates.

Examining a longer time frame of 1981 to 2010, 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. Results from linear regression models, weighted by state population and with standard errors clustered at the state level, showed that state-level minimum age restrictions had uncertain effects on suicide. Extending the time frame of analysis (through 2017), Kappelman and Fording (2021) estimated the effects of minimum possession age laws on suicides among a broader age range (those aged 24 and younger). Included in their negative binomial regression models were controls for "strict" or "weak" child-access prevention laws (described in Chapter 17), minimum age of purchase laws, state and year fixed effects, the lagged number of suicide deaths, and other time-varying state-level covariates. Robust standard errors were used to account for serial correlation. The authors found that minimum age of possession laws that set higher age limits than federal law were associated with significant reductions in firearm suicides among those aged 24 or younger, while there were uncertain effects on nonfirearm suicides. Post-hoc analyses stratified by gender suggested that these effects were larger among boys and young men aged 24 or younger.

Siegel et al. (2019) estimated the effects of minimum age laws for handgun possession on state suicide rates from 1991 through 2016, specifying log-linear models that controlled for year and state fixed effects, state social and economic covariates, and nine other firearm laws. This model revealed a suggestive effect consistent with minimum age of possession laws reducing suicides by 3 percent and a significant effect for these laws reducing firearm suicides by 6 percent. However, because the authors also found that the laws were associated with significant reductions in nonfirearm suicides, they questioned whether their estimates accurately reflected the causal impacts of minimum age laws. Furthermore, the regression models included controls for a proxy of gun ownership composed of a linear combination of the ratio of firearm suicides to suicides and hunting licenses; including the same measure (firearm suicides or suicides) in both the outcome term and predictor terms can create spu-

rious correlations that bias the study's estimates of policy effects and their standard errors (National Research Council, 2004).

Marvell (2001) also assessed how state and federal laws banning handgun possession by juveniles affected firearm and nonfirearm suicide rates among youth aged 15–19 (an age group that overlaps the minimum age threshold of 18) using data that span 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 that were 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 suicides 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. Furthermore, because of minor concerns related to model overfit and serious concerns related to the treatment of zeroes in the log-linear specification, estimates and CIs may be unreliable indicators of the true causal effects of the laws.

Finally, Bhatt et al. (2020) used a synthetic control method approach to assess the relationship between firearm suicide rates among youth and changes in Missouri's concealed-carry permit age (which changed the minimum age from 23 to 21 in 2011, and from 21 to 19 in 2014).[14] The authors found uncertain effects of the 2011 policy change and suggestive increases in firearm suicide rates among youth following the 2014 policy change. Because these estimates were based on policy changes in a single state and methods for inference were not clearly described, there are serious methodological concerns with drawing causal insights from this study.

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 age-specific estimates from Rosengart et al. (2005) because of critical concerns with model overfit (i.e., fewer than five observations per estimated parameter). 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).

[14] The policy change evaluated in Bhatt et al. (2020) involves minimum age requirements for concealed-carry permits. We include the study here, although it may have some relevance for concealed-carry laws (Chapter 18).

BOX 3.1

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 CIs for each outcome, by policy or law, as revealed in the studies examined. (See Chapter 2 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 serious concerns are indicated by unfilled circles in the forest plots, while analyses for which we raised critical concerns are not shown if stronger analyses are available. Filled circles indicate IRR estimates about which we raised minor or no specific methodological concerns. 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.

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.

Conclusions

We identified five qualifying studies that examined how suicide rates were affected by laws requiring a minimum purchase age, six that examined how suicide rates were affected by laws requiring a minimum possession age, and one that examined how suicide rates were affected by changes in the minimum age for which concealed-carry permits were authorized.

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 reduc-

FIGURE 3.1

Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Suicide

Study, by Policy	Outcome Measure	IRR [95% CI]
State minimum purchase age	**Suicide rate**	
Webster et al. (2004)	Total, aged 14–17	1.04 [0.90, 1.21]
Andrés & Hempstead (2011)	Total, men aged 15–24	0.87 [0.76, 1.00]
Andrés & Hempstead (2011)	Total, men aged 25–44	0.86 [0.83, 0.90]
Andrés & Hempstead (2011)	Total, men aged 45–64	0.99 [0.84, 1.16]
Andrés & Hempstead (2011)	Total, men aged 65+	1.03 [0.89, 1.19]
Webster et al. (2004)	Firearm, aged 14–17	1.04 [0.87, 1.16]
State minimum purchase age of 21		
Rosengart et al. (2005)	Total	1.02 [0.98, 1.07]
Webster et al. (2004)	Total, aged 18–20	0.97 [0.91, 1.05]
Raifman et al. (2020)	Total, aged 18–20	0.82 [0.66, 0.98]
Rosengart et al. (2005)	Firearm	1.00 [0.94, 1.06]
Webster et al. (2004)	Firearm, aged 18–20	0.91 [0.83, 1.00]
Raifman et al. (2020)	Firearm, aged 18–20	0.64 [0.42, 0.87]
Kappelman & Fording (2021)	Firearm, aged 0–24	0.94 [0.90, 0.98]
State minimum possession age		
Webster et al. (2004)	Total, aged 14–17	0.97 [0.90, 1.05]
Webster et al. (2004)	Firearm, aged 14–17	1.02 [0.92, 1.12]
Gius (2015b)	Firearm, aged 0–19	0.98 [0.93, 1.02]
State minimum possession age of 18, prior to federal law		
Marvell (2001)	Firearm, aged 15–19	0.99 [0.88, 1.11]
State minimum possession age of 18, same year as federal law		
Marvell (2001)	Firearm, aged 15–19	1.01 [0.86, 1.17]
State minimum possession age of 21		
Siegel et al. (2019)	Total	0.97 [0.93, 1.01]
Rosengart et al. (2005)	Total	1.03 [0.96, 1.11]
Webster et al. (2004)	Total, aged 18–20	1.13 [1.01, 1.27]
Siegel et al. (2019)	Firearm	0.94 [0.89, 1.00]
Rosengart et al. (2005)	Firearm	0.99 [0.88, 1.13]
Kappelman & Fording (2021)	Firearm, aged 0–24	0.88 [0.79, 0.97]
Webster et al. (2004)	Firearm, aged 18–20	1.14 [0.98, 1.34]

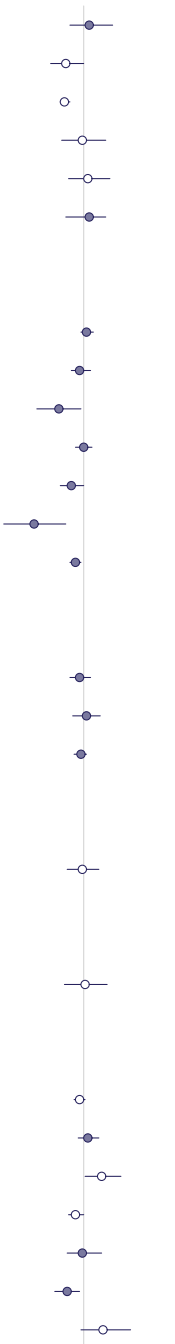
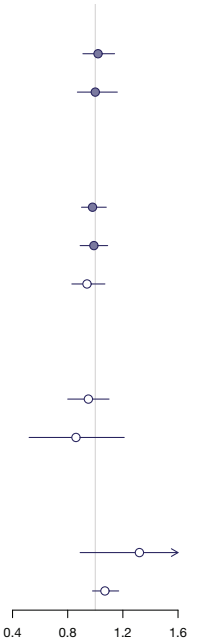


Figure continued on the next page

Figure 3.1—Continued

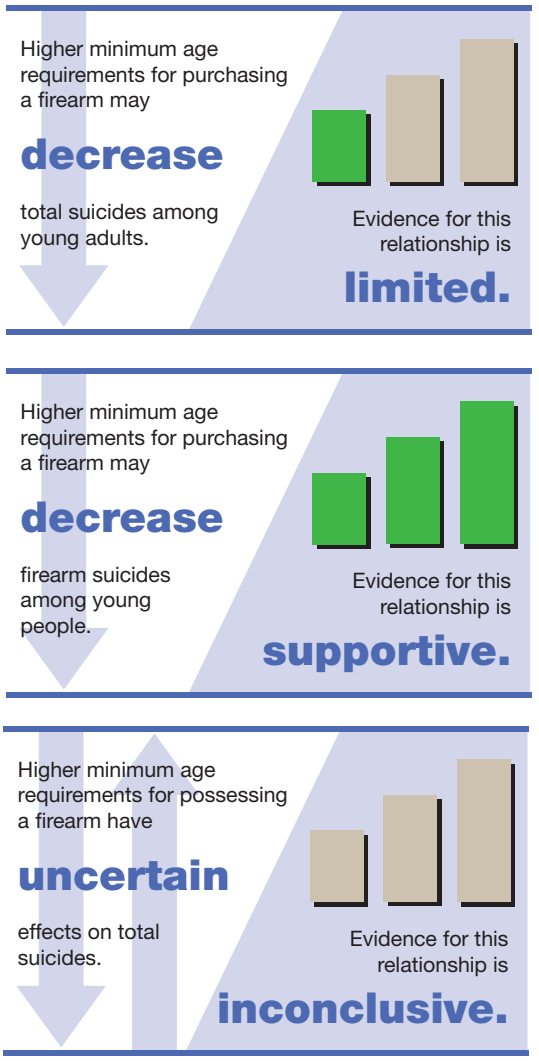
Study, by Policy	Outcome Measure	IRR [95% CI]
Federal minimum purchase age		
Webster et al. (2004)	Total, aged 14–17	1.02 [0.91, 1.14]
Webster et al. (2004)	Firearm, aged 14–17	1.00 [0.87, 1.16]
Federal minimum possession age		
Webster et al. (2004)	Total, aged 14–17	0.98 [0.90, 1.08]
Webster et al. (2004)	Firearm, aged 14–17	0.99 [0.89, 1.09]
Marvell (2001)	Firearm, aged 15–19	0.94 [0.83, 1.07]
Concealed carry permit age reduced from 23 to 21 (Mo., 2011)		
Bhatt et al. (2020)	Firearm, aged 14–18	0.95 [0.80, 1.10]
Bhatt et al. (2020)	Firearm, aged 19–24	0.86 [0.52, 1.21]
Concealed carry permit age reduced from 21 to 19 (Mo., 2014)		
Bhatt et al. (2020)	Firearm, aged 14–18	1.32 [0.89, 1.75]
Bhatt et al. (2020)	Firearm, aged 19–24	1.07 [0.98, 1.17]



NOTE: The Marvell (2001) study's model included one- and two-year lags of the dependent variable, and the Kappelman and Fording (2021) study included a one-year lag of the dependent variable, so the effect sizes from these studies are not directly comparable with others in this figure. IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

ing 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 that laws requiring a minimum age of 21 to purchase a firearm have uncertain effects on suicides and firearm suicides. 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. Kappelman and Fording (2021) also found that states with higher minimum ages of purchase than federal law requires were associated with reductions in firearm suicides among those 24 and younger. Finally, Raifman et al. (2020) reported that laws requiring a minimum age of 21 for all handgun purchases were associated with significant reductions in total and firearm-specific suicides among young adults aged 18–20.

Considering these findings and an assessment of the relative strengths of these studies, we find *limited evidence that higher minimum age requirements for purchasing a firearm can reduce total suicides among young adults.* Studies of the effect of laws setting 21 as the

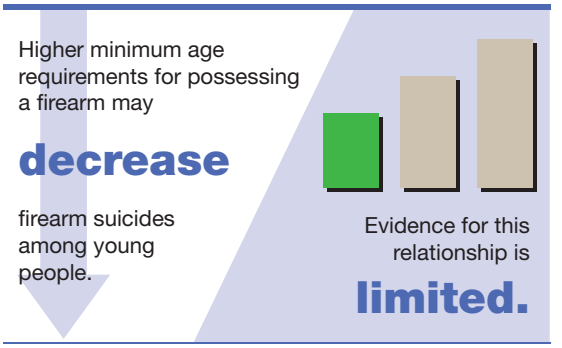


minimum age of firearm purchase provide *supportive evidence that such laws may reduce firearm suicides among young people*.

Minimum age requirements for possessing a firearm. Marvell (2001) found uncertain effects of laws banning handgun possession by juveniles 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. Siegel et al. (2019) also found suggestive negative effects of these laws on total suicide rates and significant negative effects on firearm suicide rates. 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. Kappelman and Fording (2021) found a significant effect of laws with higher minimum age of handgun possession than required by federal law on reducing firearm suicides among those aged 24 and younger. Finally, Bhatt et al. (2020) found that reducing the legal age to obtain a concealed-carry permit results in uncertain effects or suggestive increases in firearm suicides among youth and young adults.

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*

limited evidence that higher minimum age requirements for possession may reduce firearm suicides among young people.



Effects on Violent Crime

Research Synthesis Findings

We identified six studies that met our criteria and provided evidence for the effect of minimum age requirements on violent crime, one of which (Neufeld et al., 2022) contributes new findings to this updated review.[15] One study (Rudolph et al., 2015), which is not discussed further, was rated as having critical methodological concerns because it had a single treated unit and few control units.

The earliest study that we examined (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, baseline differences between states, 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. The study evaluated outcomes by both age of the victim and age of the perpetrator.

For the early state minimum age laws, the study found small and uncertain effects on firearm-related homicides but significant negative effects on nonfirearm homicides, although the magnitude, precision, and direction of the latter effect varied over 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

[15] The study focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Because there is evidence that most firearm injury hospitalizations are the result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

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. Using two-way fixed-effects Poisson regression models that also controlled for state-specific linear trends, several other state firearm laws, and a small set of time-varying economic and demographic measures, 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 relatively few states changed one or both laws over the study period; in addition, according to the authors' coding, every state but one that raised its minimum age for possession did so the same year it implemented a minimum purchase age law, confounding the effects of the laws. Moreover, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), prompting minor methodological concerns that the model may have been overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws.

Subsequent studies that used similar two-way fixed-effects specifications but evaluated longer time frames and were less subject to concerns of model overfit have also found uncertain effects of minimum age restrictions. Siegel et al. (2019) estimated the effects of minimum age laws for handgun possession on state homicide rates from 1991 to 2016, specifying log-linear models that controlled for year and state fixed effects, state social and economic covariates, and nine other firearm laws. Their results showed uncertain associations of minimum age of possession laws with homicides and firearm homicides. Evaluating an overlapping period (1995 to 2017) with a relatively similar model specification but instead focusing on homicides perpetrated by those aged 18 to 20, Moe et al. (2020) estimated uncertain effects of raising the minimum age to purchase or possess a handgun from 18 to 20. Although Neufeld et al. (2022) found a suggestive effect—consistent with laws restricting firearm possession to those aged 21 or older—reducing firearm injury hospitalization rates between 2000 and 2016, their estimates were based on policy changes in only two states, raising serious concerns about generalizability to other contexts, statistical power, and the ability to isolate the cause of any observed change to the passage of the law rather than other co-occurring factors. Furthermore, their analysis did not account for the uncertainty produced by the imputed nature of the outcome series (Carpenter and Kenward, 2012; Rubin, 1987), nor did it provide an assessment of the model's identifying assumption, raising minor concerns.

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

Study, by Policy	Outcome Measure	IRR [95% CI]
State minimum purchase age of 21	**Homicide rate**	
Rosengart et al. (2005)	Total	1.00 [0.94, 1.05]
Rosengart et al. (2005)	Firearm	0.98 [0.91, 1.06]
Moe et al. (2020)	Firearm, offender aged 18–20	1.14 [0.89, 1.45]
State minimum possession age of 21		
Siegel et al. (2019)	Total	0.93 [0.82, 1.07]
Rosengart et al. (2005)	Total	1.02 [0.89, 1.18]
Siegel et al. (2019)	Firearm	0.93 [0.78, 1.12]
Rosengart et al. (2005)	Firearm	1.06 [0.88, 1.27]
State minimum possession age of 21	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm injury	0.88 [0.75, 1.03]
State minimum possession age of 18, prior to federal law	**Violent crime rate**	
Marvell (2001)	Total homicide	1.00 [0.97, 1.04]
Marvell (2001)	Firearm homicide	1.00 [0.95, 1.05]
Marvell (2001)	Firearm homicide, aged 15–19	1.00 [1.00, 1.00]
Marvell (2001)	Rape	0.99 [0.96, 1.02]
Marvell (2001)	Robbery	1.00 [0.96, 1.03]
Marvell (2001)	Assault	1.00 [1.00, 1.00]
State minimum possession age of 18, same year as federal law		
Marvell (2001)	Total homicide	1.05 [0.99, 1.11]
Marvell (2001)	Firearm homicide	1.06 [0.99, 1.14]
Marvell (2001)	Firearm homicide, aged 15–19	1.19 [0.98, 1.43]
Marvell (2001)	Rape	0.97 [0.94, 1.01]
Marvell (2001)	Robbery	1.02 [0.97, 1.07]
Marvell (2001)	Assault	0.98 [0.94, 1.02]

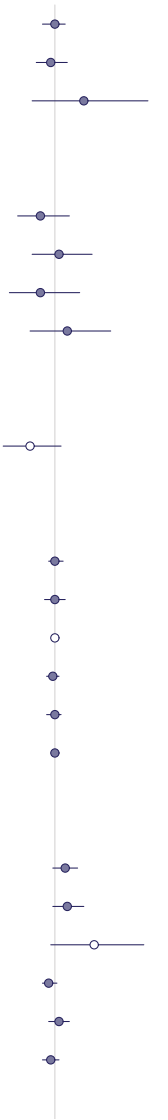
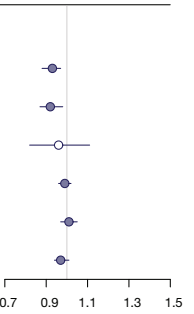


Figure continued on the next page

Figure 3.2—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Federal minimum possession age of 18		
Marvell (2001)	Total homicide	0.93 [0.88, 0.97]
Marvell (2001)	Firearm homicide	0.92 [0.87, 0.98]
Marvell (2001)	Firearm homicide, aged 15–19	0.96 [0.82, 1.11]
Marvell (2001)	Rape	0.99 [0.96, 1.02]
Marvell (2001)	Robbery	1.01 [0.97, 1.05]
Marvell (2001)	Assault	0.97 [0.94, 1.01]

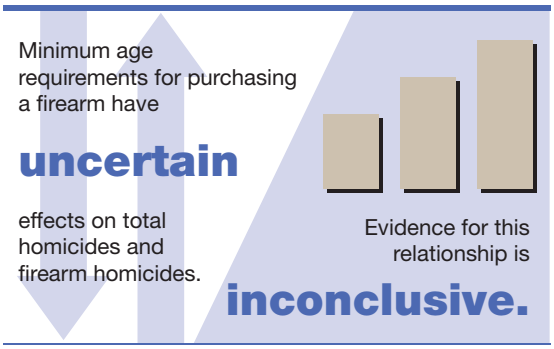


NOTE: The Marvell (2001) study's model included one- and two-year lags of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Age ranges refer to the age of the decedent unless otherwise specified. See text and Appendix B for details.

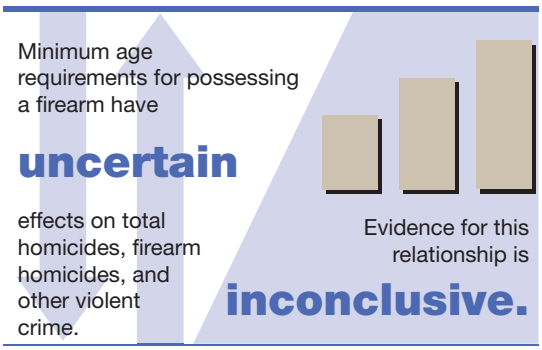
Conclusions

Excluding studies with critical methodological concerns, we identified four qualifying studies that examined the effect of minimum age requirements for purchasing or possessing a firearm on total or firearm homicide rates, as well as one qualifying study that examined the effect of minimum age of possession requirements on hospitalizations for firearm injury.

Minimum age requirements for purchasing a firearm. Rosengart et al. (2005) found uncertain effects of laws that make 21 the minimum age to purchase handguns on homicide rates and firearm homicide rates among all age groups. Moe et al. (2020) also found uncertain effects of increasing the minimum age for purchase from 18 to 20 on firearm and nonfirearm homicides perpetrated by young adults. On the basis of these results, 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 pos-



session 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. Three other studies (Moe et al., 2020; Rosengart et al., 2005; Siegel et al., 2019) found uncertain effects of laws making 21 the minimum age for possession of handguns on total or firearm homicide rates. One study (Neufeld et al., 2022) found a suggestive effect indicating that laws making 21 the minimum age for possession of handguns reduce firearm injury hospitalization rates. Therefore, we find *inconclusive evidence for how minimum age requirements for possessing a firearm affect total homicides, firearm homicides, firearm assaults, 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 establishing 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. Furthermore, the analysis drops state-years with ten or fewer deaths, which constitute almost 50 percent of the sample; this creates critical concerns for potential bias because of systematically missing outcome data.

Figure 3.3 displays the IRR and CI associated with the minimum age requirements examined in Gius (2015b). While the estimated effect of state minimum age requirements from this study is subject to critical methodological concerns, we present the findings in light of the absence of stronger evidence for this policy and outcome. However, 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.

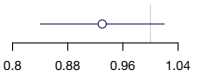
Conclusions

We identified one qualifying study examining the effect of laws requiring either a minimum age to purchase or a minimum age to possess a firearm. Gius (2015b) found a suggestive effect consistent with minimum possession age laws decreasing unintentional firearm deaths

FIGURE 3.3

Incidence Rate Ratio Associated with the Effect of Minimum Age Requirements on Unintentional Injuries and Deaths

Study, by Policy	Outcome Measure	IRR [95% CI]
State minimum possession age	**Unintentional injury rate**	
Gius (2015b)	Firearm deaths, aged 0–19	0.93 [0.84, 1.02]



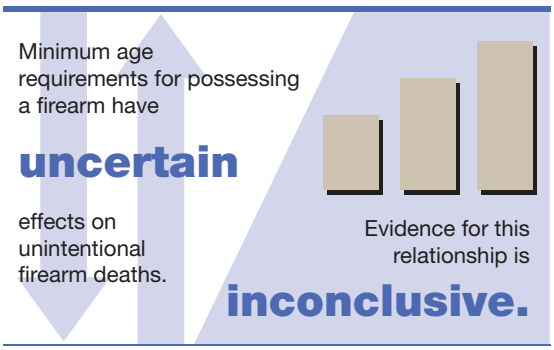
NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. See text and Appendix B for details.

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, as well as one new study that examined the effects of minimum age requirements on school shootings (Hamlin, 2021). Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2020) 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 both types of law 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 (2020), 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). Additionally, potential for model overfit (fewer than two events per parameter) and likely violations of model assumptions because of data sparsity render these model estimates and CIs highly unreliable.

Hamlin (2021) used school shooting data from 1980 to 2019 to evaluate the relationship between firearm ownership rates and the number of school shooting incidents and casualties in a state. As part of these analyses, the author controlled for several state firearm laws relevant to youth, including laws that increase the minimum age for firearm purchases to 21. Using negative binomial regression that controlled for a linear time trend, region fixed effects, estimated state gun ownership rates based on survey and administrative data, other gun laws, and poverty rates, the authors estimated an uncertain relationship between minimum age restrictions and school shooting incidence but found suggestive negative effects for school shooting injuries and fatalities. However, because the focus of this study was evaluating the relationship of firearm ownership with school shootings rather than evaluating minimum age laws, these findings are unlikely to provide substantive causal insights for the likely effects of minimum age requirements.

Figure 3.4 displays the IRRs and CIs associated with the minimum age requirements examined in Luca, Malhotra, and Poliquin (2020) and Hamlin (2021).

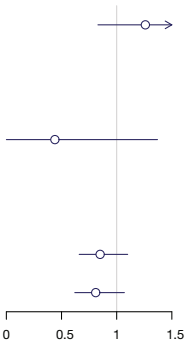
Conclusions

We identified two qualifying studies that examined how minimum age requirements for purchasing a firearm affect mass shootings or school shootings. Luca, Malhotra, and Poliquin

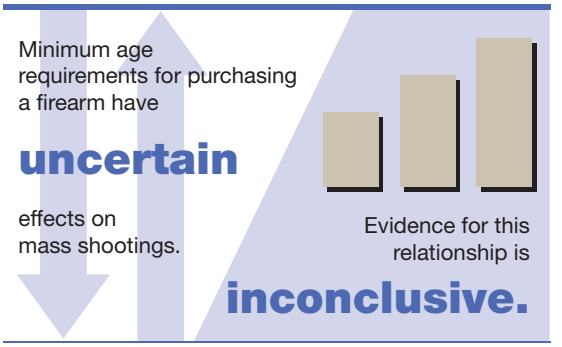
FIGURE 3.4

Incidence Rate Ratios Associated with the Effect of Minimum Age Requirements on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
State minimum purchase age of 18	**Mass shooting**	
Luca, Malhotra, & Poliquin (2020)	Any incident	1.26 [0.83, 1.68]
State minimum purchase age of 21		
Luca, Malhotra, & Poliquin (2020)	Any incident	0.44 [0.00, 1.37]
State minimum purchase age of 21	**School shooting**	
Hamlin (2021)	Number of incidents	0.85 [0.66, 1.10]
Hamlin (2021)	Number of casualties	0.81 [0.62, 1.07]



NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. See text and Appendix B for details.



(2020) found that laws setting 18 or 21 as the minimum age to purchase a firearm had uncertain effects on mass shooting incidence. Hamlin (2021) found that setting 21 as the minimum age to purchase a firearm had a significant negative effect on school shooting incidence and a suggestive negative effect on school shooting casualties. On the basis of these studies and their methodological quality, 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:

- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 3 References

Andrés, Antonio Rodríguez, and Katherine Hempstead, "Gun Control and Suicide: The Impact of State Firearm Regulations in the United States, 1995–2004," *Health Policy*, Vol. 101, No. 1, 2011.

Arora, Ashna, "Juvenile Crime and Anticipated Punishment," *American Economic Journal: Economic Policy*, Vol. 15, No. 4, November 2023.

Barber, Catherine, Deborah Azrael, Matthew Miller, and David Hemenway, "Who Owned the Gun in Firearm Suicides of Men, Women, and Youth in Five US States?" *Preventive Medicine*, Vol. 164, November 2022.

Bhatt, Apurva, Xi Wang, An-Lin Cheng, Kalee L. Morris, Luke Beyer, Abbie Chestnut, Kristy Steigerwalt, and Jeffrey Metzner, "Association of Changes in Missouri Firearm Laws with Adolescent and Young Adult Suicides by Firearms," *JAMA Network Open*, Vol. 3, No. 11, November 2020.

Birckmayer, J., and D. Hemenway, "Suicide and Firearm Prevalence: Are Youth Disproportionately Affected?" *Suicide and Life-Threatening Behavior*, Vol. 31, No. 3, 2001.

Bureau of Justice Statistics, "LEARCAT: Law Enforcement Agency Reported Crime Analysis Tool," Federal Bureau of Investigation, National Incident-Based Reporting System, last updated December 12, 2022. As of October 28, 2023:
https://learcat.bjs.ojp.gov/AnalyticsOnDemand

Carpenter, James R., and Michael G. Kenward, *Multiple Imputation and Its Application*, John Wiley and Sons, 2012.

CDC—*See* Centers for Disease Control and Prevention.

Centers for Disease Control and Prevention, "Injury Counts and Rates," WISQARS database, undated-f. As of February 9, 2024:
https://wisqars.cdc.gov/reports/

Centers for Disease Control and Prevention, "Source of Firearms Used by Students in School-Associated Violent Deaths—United States, 1992–1999," *Morbidity and Mortality Weekly Report*, Vol. 50, No. 31, 2001.

Duda, M. D., and K. C. Young, *Factors Related to Hunting and Fishing Participation in the United States*, Responsive Management, report for the U.S. Fish and Wildlife Service, Grant #14-48-0009-92-1252, 1993.

FBI—*See* Federal Bureau of Investigation.

Federal Bureau of Investigation, "UCR Summary of Crime in Nation, 2022," webpage, Uniform Crime Reporting Program, undated. As of October 28, 2023:
https://cde.ucr.cjis.gov/LATEST/webapp/#/pages/home

Federal Bureau of Investigation, "Expanded Homicide Data Table 8: Murder Victims, by Weapon, 2018–2022," webpage, Crime in the United States 2022, 2023a. As of October 22, 2023:
https://cde.ucr.cjis.gov/LATEST/webapp/#/pages/downloads

Federal Bureau of Investigation, "Expanded Homicide Data Table 15: Justifiable Homicide, by Weapon, Private Citizen, 2018–2022," webpage, Crime in the United States 2022, 2023b. As of October 22, 2022:
https://cde.ucr.cjis.gov/LATEST/webapp/#/pages/downloads

Federal Bureau of Investigation, "Expanded Homicide Data Table 3: Murder Offenders, by Age, Sex, Race, and Ethnicity, 2022," webpage, Crime Data Explorer, Crime in the United States Annual Reports, Expanded Homicide Tables, 2023c.

Federal Bureau of Investigation, "FBI Releases 2022 Crime in the Nation Statistics," press release, October 16, 2023e.

Freed, L. H., D. W. Webster, J. J. Longwell, J. Carrese, and M. H. Wilson, "Factors Preventing Gun Acquisition and Carrying Among Incarcerated Adolescent Males," *JAMA Pediatrics*, Vol. 155, 2001.

Freedman, David A., "On the So-Called 'Huber Sandwich Estimator' and 'Robust Standard Errors,'" *American Statistician*, Vol. 60, No. 4, 2006.

Gius, Mark, "The Impact of Minimum Age and Child Access Prevention Laws on Firearm-Related Youth Suicides and Unintentional Deaths," *Social Science Journal*, Vol. 52, No. 2, 2015b.

Grossman, D. C., D. T. Reay, and S. A. Baker, "Self-Inflicted and Unintentional Firearm Injuries Among Children and Adolescents," *JAMA Pediatrics*, Vol. 153, No. 8, 1999.

Hamlin, Daniel, "Are Gun Ownership Rates and Regulations Associated with Firearm Incidents in American Schools? A Forty-Year Analysis (1980–2019)," *Journal of Criminal Justice*, Vol. 76, September–October 2021.

Hanlon, Thomas J., Catherine Barber, Deborah Azrael, and Matthew Miller, "Type of Firearm Used in Suicides: Findings from 13 States in the National Violent Death Reporting System, 2005–2015," *Journal of Adolescent Health*, Vol. 65, No. 3, 2019.

Johnson, R. M., C. Barber, D. Azrael, D. E. Clark, and D. Hemenway, "Who Are the Owners of Firearms Used in Adolescent Suicides?" *Suicide and Life-Threatening Behavior*, Vol. 40, No. 6, 2010.

Kappelman, Jack, and Richard C. Fording, "The Effect of State Gun Laws on Youth Suicide by Firearm: 1981–2017," *Suicide and Life-Threatening Behavior*, Vol. 51, No. 2, April 2021.

LaFree, G., and C. Birbeck, *Controlling New Mexico Juveniles' Possession of Firearms*, New Mexico Criminal Justice Statistical Analysis Center, Working Paper 27, 1998.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

Marvell, Thomas B., "The Impact of Banning Juvenile Gun Possession," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Miller, Matthew, and Deborah Azrael, "Firearm Storage in US Households with Children: Findings from the 2021 National Firearm Survey," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Miller, M., and D. Hemenway, "The Relationship Between Firearms and Suicide: A Review of the Literature," *Aggression and Violent Behavior*, Vol. 4, No. 1, 1999.

Moe, Caitlin A., Miriam J. Haviland, Andrew G. Bowen, Ali Rowhani-Rahbar, and Frederick P. Rivara, "Association of Minimum Age Laws for Handgun Purchase and Possession with Homicides Perpetrated by Young Adults Aged 18 to 20 Years," *JAMA Pediatrics*, Vol. 174, No. 11, 2020.

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Office of Juvenile Justice and Delinquency Prevention, *OJJDP Statistical Briefing Book*, U.S. Department of Justice, October 29, 2021.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Public Law 117-159, Bipartisan Safer Communities Act, June 25, 2022.

Puzzanchera, C., G. Chamberlin, and W. Kang, "Easy Access to the FBI's Supplementary Homicide Reports: 1980–2020," Office of Juvenile Justice and Delinquency Prevention, 2021.

Raifman, Julia, Elysia Larson, Colleen L. Barry, Michael Siegel, Michael Ulrich, Anita Knopov, and Sandro Galea, "State Handgun Purchase Age Minimums in the US and Adolescent Suicide Rates: Regression Discontinuity and Difference-in-Differences Analyses," *BMJ*, Vol. 370, 2020.

Rosengart, M., P. Cummings, A. Nathens, P. Heagerty, R. Maier, and F. Rivara, "An Evaluation of State Firearm Regulations and Homicide and Suicide Death Rates," *Injury Prevention*, Vol. 11, No. 2, 2005.

Rubin, Donald B., *Multiple Imputation for Nonresponse in Surveys*, John Wiley and Sons, 1987.

Rudolph, K. E., E. A. Stuart, J. S. Vernick, and D. W. Webster, "Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides," *American Journal of Public Health*, Vol. 105, No. 8, 2015.

Schnitzer, Patricia G., Heather K. Dykstra, Theodore E. Trigylidas, and Richard Lichenstein, "Firearm Suicide Among Youth in the United States, 2004–2015," *Journal of Behavioral Medicine*, Vol. 42, No. 4, 2019.

Shah, S., R. E. Hoffman, L. Wake, and W. M. Marine, "Adolescent Suicide and Household Access to Firearms in Colorado: Results of a Case-Control Study," *Journal of Adolescent Health*, Vol. 26, No. 3, 2000.

Sheley, J. F., and J. D. Wright, *High School Youths, Weapons, and Violence: A National Survey*, National Institute of Justice, 1998.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, 2019.

Simon, Thomas R., Heather B. Clayton, Linda L. Dahlberg, Corinne David-Ferdon, Greta Kilmer, and Colleen Barbero, "Gun Carrying Among Youths, by Demographic Characteristics, Associated Violence Experiences, and Risk Behaviors—United States, 2017–2019," *Morbidity and Mortality Weekly Report*, Vol. 71, No. 30, 2022.

Simonetti, Joseph A., Jessica L. Mackelprang, Ali Rowhani-Rahbar, Douglas Zatzick, and Frederick P. Rivara, "Psychiatric Comorbidity, Suicidality, and in-Home Firearm Access Among a Nationally Representative Sample of Adolescents," *JAMA Psychiatry*, Vol. 72, No. 2, 2015.

Tritch, Teresa, "Keep Handguns Away from Teenagers," *New York Times*, May 30, 2014.

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

U.S. Census Bureau, "U.S. and World Population Clock," 2022. As of August 17, 2022: https://www.census.gov/popclock/

U.S. Fish and Wildlife Service, *2016 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*, FHW/16-NAT, April 2018.

Vittes, K. A., and S. B. Sorenson, "Recreational Gun Use by California Adolescents," *Health Education and Behavior*, Vol. 32, No. 6, 2005.

Watkins, Adam M., Beth M. Huebner, and Scott H. Decker, "Patterns of Gun Acquisition, Carrying, and Use Among Juvenile and Adult Arrestees: Evidence from a High-Crime City," *Justice Quarterly*, Vol. 25, No. 4, 2008.

Webster, D. W., L. H. Freed, S. Frattaroli, and M. H. Wilson, "How Delinquent Youths Acquire Guns: Initial Versus Most Recent Gun Acquisitions," *Journal of Urban Health*, Vol. 79, No. 1, 2002.

Webster, D. W., J. S. Vernick, A. M. Zeoli, and J. A. Manganello, "Association Between Youth-Focused Firearm Laws and Youth Suicides," *JAMA*, Vol. 292, No. 5, 2004.

Wright, M. A., G. J. Wintemute, and B. E. Claire, "Gun Suicide by Young People in California: Descriptive Epidemiology and Gun Ownership," *Journal of Adolescent Health*, Vol. 43, No. 6, 2008.

CHAPTER 4

Prohibitions Associated with Mental Illness

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 26 years, approximately 2,900 potential gun purchases per year have been denied at the background check stage as a result of this prohibition (Federal Bureau of Investigation, 2023f).[2] However, the total number of people covered by this exclusion is not known. In 2021, an estimated 57.8 million adults in the United States had some form of *mental illness*—defined as any "mental, behavioral, or emotional disorder in the past year" other than a developmental or substance use disorder (Substance Abuse and Mental Health Services Administration, 2022). Of these adults, 14.1 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 set 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.

The effects of expanding firearm prohibitions also likely depend on rates of firearm ownership and access among individuals who are newly disqualified from possessing or purchasing firearms, particularly in the absence of mechanisms to remove existing firearms from

[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, 1968). 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 July 31, 2023, 75,776 (3.4 percent) of 2,239,934 total denials for firearm permits were attributed to mental health adjudications (Federal Bureau of Investigation, 2023e).

prohibited possessors (see Chapter 6). While reliable data on who owns firearms in the United States are limited, some studies suggest that there are comparable rates of ownership among individuals with and without more-common mental health conditions, diagnoses, or symptoms (Hullenaar et al., 2023; Perlis et al., 2022; Richards et al., 2021; Swanson et al., 2015). However, a limited body of research among population subgroups (e.g., military veterans, adolescents) has found that certain psychiatric disorders (e.g., conduct disorder, intermittent explosive disorder) may be associated with higher rates of gun carrying (Bossarte et al., 2021; Kagawa et al., 2019). Overall, this research suggests that a substantial number of people with psychiatric disorders or symptoms own or have access to firearms.

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). While research shows that the risk of an individual perpetrating an act of violence varies by specific psychiatric disorder and the presence of comorbidities, few studies have examined firearm violence specifically (Whiting, Lichtenstein, and Fazel, 2021). 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.

The potential relationship between mass shootings and mental illness is also unclear. Although media coverage often 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). A study of 106 perpetrators of mass public shootings in the United States between 1990 and 2014 found that

less than 5 percent of offenders had a history of involuntary commitment or adjudication of dangerousness that would have prohibited them from purchasing a firearm following the federal mental health background check (Silver, Simons, and Craun, 2018). A study of 575 perpetrators of mass shootings in the United States between 1970 and 2019 found that 8 percent of offenders had a history of psychotic symptoms (e.g., delusions, thought disorganization, hallucinations) and 23 percent of offenders had histories of symptoms of any psychiatric or severe neurologic disorder (Brucato et al., 2021). Counting less severe forms of mental illness, Follman, Aronsen, and Pan (2022) found that 62 of the 125 mass public shootings between 1982 and 2021 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 past-year 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. Potential costs of firearm restrictions for people with mental illness may be compounded by more-limited knowledge about or concern with gun rights among this population; for example, a study in South Carolina found that psychiatrists—who can, in some states, approve requests to have gun rights restored for those disqualified for mental health reasons—had limited knowledge about the restoration process for patients' gun rights (Nagle et al., 2021).[3] At the same time, such an expansion may decrease violent crime, mass shootings, and suicides carried out by this population.

[3] In April 2007, Congress passed the NICS Improvement Amendments Act of 2007, which included provisions that federal and state programs should allow individuals to seek restoration of their gun rights if they were restricted for reasons of mental illness.

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 them 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). This suggests that some of these suicides might have been prevented if individuals in this population had been prohibited from obtaining a firearm.

However, despite higher risk, the size of the population affected by existing prohibitions may be too small to meaningfully affect population-level suicide rates. Indeed, a simulation study of the hypothetical effects of prohibiting firearm ownership for individuals with past psychiatric hospitalization in New York City showed null effects on rates of firearm suicide; the authors estimated that 144,000 individuals would need to be disqualified from firearm possession to prevent one suicide (Keyes et al., 2019).

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 8 (on background checks).

State Implementation of Prohibitions Associated with Mental Illness

As of January 1, 2024, 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,[4] 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).[5]

In other cases, states have narrower prohibitions than those established in the Brady Act. For example, 11 states prohibit possession of all firearms by only those committed to psychiatric institutions, not those adjudicated as mentally incompetent,[6] while Missouri applies the prohibition only to individuals adjudicated as mentally incompetent.[7] In Michigan, North Carolina, and Oklahoma, the prohibition applies only to handguns.[8] Tennessee prohibits the transfer of firearms to individuals who have been committed to psychiatric institutions or adjudicated as mentally incompetent, but the law is silent on whether such individuals may possess a firearm.[9]

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.[10] Hawaii has extended the prohibition to those diagnosed with "significant" mental disorders, and California and Illinois have widened the class of

[4] 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.

[5] Arkansas, Florida, Nevada, New York, Ohio, Pennsylvania, Rhode Island, South Carolina, Utah, West Virginia, and Wyoming. See Ark. Code Ann. § 5-73-103; Fla. Stat. Ann. § 790.065; Nev. Rev. Stat. Ann. § 202.360; N.Y. Penal Law §§ 265.00, 400, N.Y. Mental Hygiene Law § 9.46; Ohio Rev. Code Ann. § 2923.13, 426.130; 18 Pa. Cons. Stat. § 6105; R.I. Gen. Laws § 11-47-6; S.C. Ann. Code § 23-31-1040; Utah Code Ann. § 76-10-503; W. Va. Ann. Code § 61-7-7; Wyo. Stat. Ann. § 6-8-404.

[6] Arizona, Delaware, Kansas, Louisiana, Maine, Massachusetts, Minnesota, New Jersey, North Dakota, Washington, and Wisconsin. See Ariz. Rev. Stat. Ann. §§ 13-3101, 13-3102, 36-540; Del. Code Ann. Tit. 11 § 1448; Kan. Stat. Ann. §§ 21-6301, 59-2946; La. Stat. Ann. R.S. 14:95.1; Me. Rev. Stat. Ann. § 393; Mass. Gen. Laws Ann. § 129B; Minn. Stat. Ann. § 624.713; N.J. Stat. Ann. § 2C:39-7; N.D. Cent. Code § 62.1-02-01; Wash. Rev. Code § 941.040; Wisc. Stat. §§ 941.29, 51.20, 54.10.

[7] Mo. Stat. Ann. § 571.070.

[8] Mich. Comp. Laws Ann. § 28.422; N.C. Gen. Stat. Ann. § 140-402; 21 Okla. Stat. Ann. § 1290.10. Furthermore, in Michigan, the prohibition applies only to those committed to psychiatric institutions, and, in Oklahoma, only to those adjudicated as mentally ill.

[9] Tenn. Code Ann. § 39-17-1316.

[10] Calif. Welf. and Inst. Code § 8100 (while a person is voluntarily in treatment for being a threat to themselves or others); Conn. Gen. Stat. Ann. § 53a-217 (admitted within the previous six months); 430 Ill. Comp. Stat. 65/1.1, 65/8 (admitted within the 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 the past five years).

prohibited possessors in other ways.[11] Arizona, Michigan, Oregon, and Virginia have also extended the mental health–related prohibitions to individuals ordered to attend outpatient treatment.[12] And some states, such as Arizona and Michigan, have applications that are both narrower and broader than the standards in the Brady Act.

Finally, 11 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.[13] California, Massachusetts, Rhode Island, and Wisconsin include standards related to the appointment of a guardian or conservator,[14] which require prior adjudication of mental defect.

Effects on Suicide

Research Synthesis Findings

We identified four studies examining the effects of mental health–related prohibitions on suicide, one of which (Kester et al., 2023) contributes new findings to this updated review but is subject to critical methodological concerns because of an unfavorable ratio of covariates to observations (less than one to five) and thus is not discussed further. 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

[11] In Hawaii, possession is prohibited for 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).

[12] Ariz. Rev. Stat. §§ 36-540(A)(1), 13-3101(A)(7); Mich. Comp. Laws Serv. § 28.422; Ore. Rev. Stat. §§ 166.250(1)(c)(D), 426.133; Va. Code Ann. § 18.2-308.1:3(A).

[13] Alabama, Arizona, California, Connecticut, Delaware, Florida, Louisiana, Maine, Maryland, Minnesota, and Nevada. See Ala. Code § 13A-11-72; Ariz. Rev. Stat. § 13-3101; Calif. Welf. and Inst. Code §§ 8100, 8103; Conn. Gen. Stat. Ann. § 53a-217; 11 Del. Code § 1448; Fla. Stat. Ann. § 790.064; La. Stat. Ann. R.S. 14:95.1; 15 Me. Rev. Stat. Ann. § 393; Md. Code, Pub. Safety, § 5-133; Minn. Stat. Ann. § 624.713; Nev. Rev. Stat. 202.360.

[14] Calif. Welf. and Inst. Code § 8103; Mass. Gen. Laws Ann. 140 § 129B; R.I. Gen. Laws 1956, § 11-47-6; Wisc. Stat. Ann. 54.10.

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).

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 states implemented a formal 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) and Andrés and Hempstead (2011). Swanson et al. (2016) did not provide effect estimates or test statistics, so we do not include effect sizes for this study in the figure.

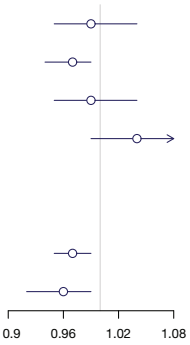
Conclusions

Excluding one study that was subject to critical methodological concerns, three qualifying studies examined the relationships between suicide and prohibitions associated with mental illness or related indicators of risk of harm. 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

FIGURE 4.1

Incidence Rate Ratios Associated with the Effect of Mental Health–Related Prohibitions on Suicide

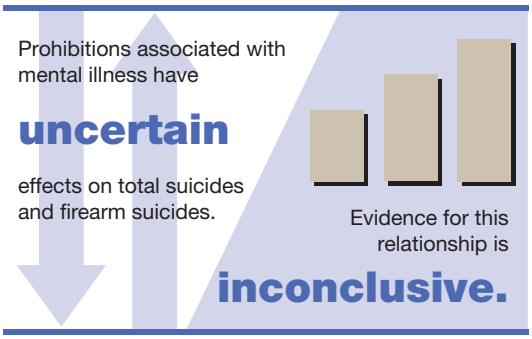
Study, by Policy	Outcome Measure	IRR [95% CI]
Prohibitions for mental health problems	**Suicide rate**	
Andrés & Hempstead (2011)	Total, men aged 15–24	0.99 [0.95, 1.04]
Andrés & Hempstead (2011)	Total, men aged 25–44	0.97 [0.94, 0.99]
Andrés & Hempstead (2011)	Total, men aged 45–64	0.99 [0.95, 1.04]
Andrés & Hempstead (2011)	Total, men aged 65+	1.04 [0.99, 1.10]
Background check on mental illness		
Sen & Panjamapirom (2012)	Total	0.97 [0.95, 0.99]
Sen & Panjamapirom (2012)	Firearm	0.96 [0.92, 0.99]



NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

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. All three studies had serious methodological concerns.

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 four studies that examined the effects of implementing background checks for individuals prohibited from purchasing or possessing firearms because of a mental illness; one of these (Neufeld et al., 2022) contributes new findings to this updated

review.[15] 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.

[15] Neufeld et al. (2022) focused on firearm injury hospitalization rates, which include a mix of injuries attributed to assault, intentional self-injury, and accident. Because of the evidence suggesting that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

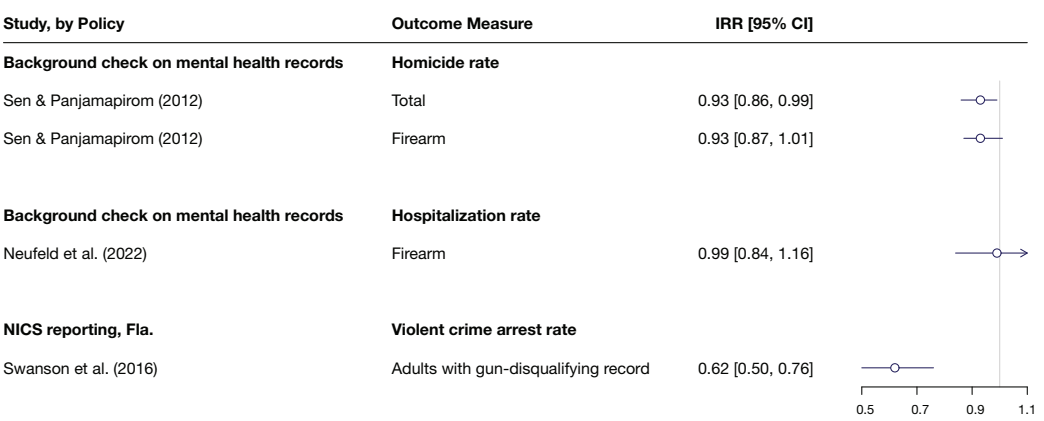
Using data from 2002 to 2011, Swanson et al. (2016) employed analogous methods to analyze the effects of NICS reporting changes in 2007 for two Florida counties. The authors similarly found a larger reduction in violent crime arrest rates for individuals with a disqualifying mental health condition relative 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).

Finally, Neufeld et al. (2022) used data from 2000 to 2016 to evaluate how requiring the assessment of state mental health records as part of the background check process influences firearm injury hospitalization rates. Using two-way fixed effects regression models that adjusted for a small set of state-level time-varying covariates (demographic and socioeconomic characteristics, a proxy measure for household firearm ownership, the violent crime rate, and per capita alcohol consumption) but no other state firearm laws, they found uncertain effects of these requirements on firearm injury hospitalizations. However, their estimates were based on policy changes in only two states with three or more years of pre-period data, raising serious concerns about statistical power, generalizability to other contexts, and ability to isolate the cause of any observed change to the passage of the law versus to other co-occurring factors. Furthermore, their analysis did not account for the uncertainty produced by the imputed nature of the outcome series (Carpenter and Kenward, 2012; Rubin, 1987), nor did it provide an assessment of the model's identifying assumption, raising minor concerns.

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 infor-

FIGURE 4.2

Incidence Rate Ratios Associated with the Effect of Mental Health–Related Prohibitions on Violent Crime

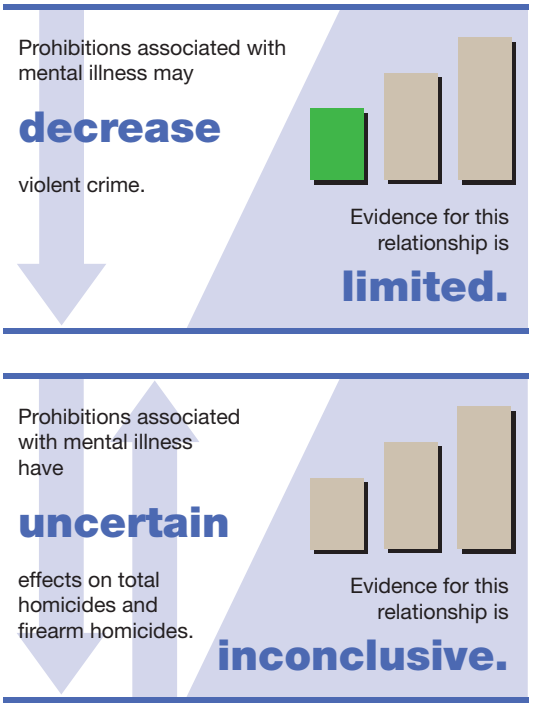


NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

mation 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 non–legally disqualified individuals.

Conclusions

Excluding those studies subject to critical methodological concerns, we identified three qualifying studies that estimated how laws prohibiting gun purchases by those with a mental illness affect violent crime, homicides, or firearm injuries that result in hospitalization. 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. Neufeld et al. (2022) instead found that requiring an assessment of state mental health records as part of the background check process had uncertain effects on firearm injury hospitalization rates.



Considering these results, we conclude that there is *limited evidence that 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
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 4 References

Andrés, Antonio Rodríguez, and Katherine Hempstead, "Gun Control and Suicide: The Impact of State Firearm Regulations in the United States, 1995–2004," *Health Policy*, Vol. 101, No. 1, 2011.

Bossarte, Robert M., Hannah N. Ziobrowski, David M. Benedek, Catherine L. Dempsey, Andrew J. King, Matthew K. Nock, Nancy A. Sampson, Murray B. Stein, Robert J. Ursano, and Ronald C. Kessler, "Mental Disorders, Gun Ownership, and Gun Carrying Among Soldiers After Leaving the Army, 2016–2019," *American Journal of Public Health*, Vol. 111, No. 10, October 2021.

Brucato, Gary, Paul S. Appelbaum, Hannah Hesson, Eileen A. Shea, Gabriella Dishy, Kathryn Lee, Tyler Pia, Faizan Syed, Alexandra Villalobos, and Melanie M. Wall, "Psychotic Symptoms in Mass Shootings v. Mass Murders Not Involving Firearms: Findings from the Columbia Mass Murder Database," *Psychological Medicine*, Vol. 52, No. 15, 2021.

Carpenter, James R., and Michael G. Kenward, *Multiple Imputation and Its Application*, John Wiley and Sons, 2012.

Cavanagh, J. T., A. J. Carson, M. Sharpe, and S. M. Lawrie, "Psychological Autopsy Studies of Suicide: A Systematic Review," *Psychological Medicine*, Vol. 33, 2003.

Choe, J. Y., L. A. Teplin, and K. M. Abram, "Perpetration of Violence, Violent Victimization, and Severe Mental Illness: Balancing Public Health Outcomes," *Psychiatric Services*, Vol. 59, No. 2, 2008.

Corrigan, P. W., and A. C. Watson, "Findings from the National Comorbidity Survey on the Frequency of Violent Behavior in Individuals with Psychiatric Disorders," *Psychiatry Research*, Vol. 136, 2005.

Desmarais, S. L., R. A. Van Dorn, K. L. Johnson, K. J. Grimm, K. S. Douglas, and M. S. Swartz, "Community Violence Perpetration and Victimization Among Adults with Mental Illness," *American Journal of Public Health*, Vol. 104, 2014.

Everytown for Gun Safety Support Fund, "Mass Shootings in the United States: 2009–2017," December 6, 2018. As of May 15, 2019:
http://everytownresearch.org/reports/mass-shootings-analysis/

Federal Bureau of Investigation, "FBI Releases 2022 Crime in the Nation Statistics," press release, October 16, 2023e.

Federal Bureau of Investigation, "Federal Denials: Reasons Why the NICS Section Denies," webpage, August 31, 2023f. As of August 29, 2023:
https://www.fbi.gov/file-repository/federal_denials.pdf/view

Follman, Mark, Gavin Aronsen, and Deanna Pan, "US Mass Shootings, 1982–2022: Data from Mother Jones' Investigation," *Mother Jones*, July 17, 2022.

Fox, James Alan, and Emma E. Fridel, "The Tenuous Connections Involving Mass Shootings, Mental Illness, and Gun Laws," *Violence and Gender*, Vol. 3, No. 1, 2016.

Fox, James A., and Jack Levin, *Extreme Killing: Understanding Serial and Mass Murder*, 3rd ed., Sage Publications, 2015.

Harrell, Erika, *Crime Against Persons with Disabilities, 2009–2015—Statistical Tables*, U.S. Department of Justice, Bureau of Justice Statistics, NCJ 250632, 2017.

Hughes, Karen, Mark A. Bellis, Lisa Jones, Sara Wood, Geoff Bates, Lindsay Eckley, Ellie McCoy, Christopher Mikton, Tom Shakespeare, and Alana Officer, "Prevalence and Risk of Violence Against Adults with Disabilities: A Systematic Review and Meta-Analysis of Observational Studies," *Lancet*, Vol. 379, 2012.

Hullenaar, Keith L., Ali Rowhani-Rahbar, Erin R. Morgan, Chelsea D. Hicks, and Frederick P. Rivara, "Mental Health Comorbidities, Household Firearm Ownership, and Firearm Access Among Children," *Pediatrics*, Vol. 151, No. 6, June 1, 2023.

Kagawa, Rose M. C., Dahsan S. Gary, Garen J. Wintemute, Kara E. Rudolph, Veronica A. Pear, Katherine Keyes, and Magdalena Cerdá, "Psychiatric Disorders and Gun Carrying Among Adolescents in the United States," *Journal of Pediatrics*, Vol. 209, June 2019.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Keyes, Katherine M., Ava Hamilton, Jeffrey Swanson, Melissa Tracy, and Magdalena Cerdá, "Simulating the Suicide Prevention Effects of Firearms Restrictions Based on Psychiatric Hospitalization and Treatment Records: Social Benefits and Unintended Adverse Consequences," *American Journal of Public Health*, Vol. 109, No. 3, 2019.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Li, Z., A. Page, G. Martin, and R. Taylor, "Attributable Risk of Psychiatric and Socio-Economic Factors for Suicide from Individual-Level, Population-Based Studies: A Systematic Review," *Social Science and Medicine*, Vol. 72, No. 4, 2011.

Liu, E. C., E. Bagalman, V. S. Chu, and C. S. Redhead, *Submission of Mental Health Records to the NICS and the HIPAA Privacy Rule*, Congressional Research Service, R43040, 2013.

Matejkowski, J., J. Fairfax-Columbo, S. W. Cullen, S. C. Marcus, and P. L. Solomon, "Exploring the Potential of Stricter Gun Restrictions for People with Serious Mental Illness to Reduce Homicide in the United States," *Journal of Forensic Psychiatry and Psychology*, Vol. 25, No. 3, 2014.

McGinty, Emma E., Alene Kennedy-Hendricks, Seema Choksy, and Colleen L. Barry, "Trends in News Media Coverage of Mental Illness in the United States: 1995–2014," *Health Affairs*, Vol. 35, No. 6, 2016.

McGinty, E. E., D. W. Webster, M. Jarlenski, and C. L. Barry, "News Media Framing of Serious Mental Illness and Gun Violence in the United States," *American Journal of Public Health*, Vol. 104, No. 3, 2014.

Metzl, J. M., and K. T. MacLeish, "Mental Illness, Mass Shootings, and the Politics of American Firearms," *American Journal of Public Health*, Vol. 105, No. 2, 2015.

Nagle, Megan E., Kaustubh G. Joshi, Richard L. Frierson, Martin W. Durkin, and Alexandra Karydi, "Knowledge and Attitudes of Psychiatrists About the Gun Rights of Persons with Mental Illness," *Journal of the American Academy of Psychiatry and the Law*, Vol. 49, No. 1, March 2021.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Perlis, Roy H., Matthew D. Simonson, Jon Green, Jennifer Lin, Alauna Safarpour, Kristin Lunz Trujillo, Alexi Quintana, Hanyi Chwe, John Della Volpe, Katherine Ognyanova, Mauricio Santillana, James Druckman, David Lazer, and Matthew A. Baum, "Prevalence of Firearm Ownership Among Individuals with Major Depressive Symptoms," *JAMA Network Open*, Vol. 5, No. 3, March 2022.

Public Law 90-618, Gun Control Act of 1968, October 22, 1968.

Richards, Julie E., Elena Kuo, Christine Stewart, Jennifer F. Bobb, Kayne D. Mettert, Ali Rowhani-Rahbar, Marian E. Betz, Rebecca Parrish, Ursula Whiteside, Jennifer M. Boggs, and Gregory E. Simon, "Self-Reported Access to Firearms Among Patients Receiving Care for Mental Health and Substance Use," *JAMA Health Forum*, Vol. 2, No. 8, 2021.

Rubin, Donald B., *Multiple Imputation for Nonresponse in Surveys*, John Wiley and Sons, 1987.

Sen, B., and A. Panjamapirom, "State Background Checks for Gun Purchase and Firearm Deaths: An Exploratory Study," *Preventive Medicine*, Vol. 55, No. 4, 2012.

Silver, James, Andre Simons, and Sarah Craun, *A Study of the Pre-Attack Behaviors of Active Shooters in the United States Between 2000 and 2013*, U.S. Department of Justice, Federal Bureau of Investigation, June 2018.

Steadman, Henry J., Edward P. Mulvey, John Monahan, Pamela Clark Robbins, Paul S. Appelbaum, Thomas Grisso, Loren H. Roth, and Eric Silver, "Violence by People Discharged from Acute Psychiatric Inpatient Facilities and by Others in the Same Neighborhoods," *Archives of General Psychiatry*, Vol. 55, No. 5, 1998.

Substance Abuse and Mental Health Services Administration, *Results from the 2020 National Survey on Drug Use and Health: Detailed Tables*, 2022.

Swanson, Jeffrey W., "Mental Disorder, Substance Abuse, and Community Violence: An Epidemiological Approach," in J. Monahan and H. Steadman, eds., *Violence and Mental Disorder*, University of Chicago Press, 1994.

Swanson, Jeffrey W., Michele M. Easter, Allison G. Robertson, Marvin S. Swartz, Kelly Alanis-Hirsch, Daniel Moseley, Charles Dion, and John Petrila, "Gun Violence, Mental Illness, and Laws That Prohibit Gun Possession: Evidence from Two Florida Counties," *Health Affairs*, Vol. 35, No. 6, 2016.

Swanson, J. W., E. E. McGinty, S. Fazel, and V. M. Mays, "Mental Illness and Reduction of Gun Violence and Suicide: Bringing Epidemiologic Research to Policy," *Annals of Epidemiology*, Vol. 25, No. 5, 2015.

Swanson, J. W., A. G. Robertson, L. K. Frisman, M. A. Norko, H. Lin, M. S. Swartz, and P. J. Cook, "Preventing Gun Violence Involving People with Serious Mental Illness," in D. W. Webster and J. S. Vernick, eds., *Reducing Gun Violence in America: Informing Policy with Evidence and Analysis*, Johns Hopkins University Press, 2013.

Teplin, L. A., G. M. McClelland, K. M. Abram, and D. A. Weiner, "Crime Victimization in Adults with Severe Mental Illness: Comparison with the National Crime Victimization Survey," *Archives of General Psychiatry*, Vol. 62, No. 8, 2005.

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

Whiting, Daniel, Paul Lichtenstein, and Seena Fazel, "Violence and Mental Disorders: A Structured Review of Associations by Individual Diagnoses, Risk Factors, and Risk Assessment," *Lancet Psychiatry*, Vol. 8, No. 2, February 2021.

CHAPTER 5

Prohibitions Associated with Domestic Violence

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), and more than half of all intimate partner homicides involve the use of a firearm (Fridel and Fox, 2019). Domestic abusers with guns also pose a threat to individuals outside their family units (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).

Federal law has prohibited individuals who have been convicted of domestic violence felonies from possessing or purchasing firearms since the Gun Control Act of 1968 (Pub L. 90-618, 1968). In 1994, these prohibitions were extended to individuals subject to some domestic violence restraining orders (DVROs) that protect an intimate partner or the child of an intimate partner, and in 1996 federal prohibitions were extended to individuals who have been convicted of a misdemeanor crime of domestic violence. 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.[1] Because of how federal law defines

[1] Federal law defines a *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, by a person similarly situated to a spouse, parent, or guardian of the victim, or by a person who has a current or recent former dating relationship with the victim (18 U.S.C. 921(a)(33)).

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. Historically, dating partners who never cohabited were not covered by these laws, a gap (often referred to as the *boyfriend loophole*) that some argued had significant negative consequences for individual and public safety (Stolberg, 2017). The 2022 Bipartisan Safer Communities Act (Pub. L. 117-159) amended the federal law to prohibit current or former dating partners from purchasing or possessing a firearm if they had been convicted of a misdemeanor crime of domestic violence; however, the firearm prohibition for dating partners expires after five years.

Many states have enacted additional legislation designed to reduce firearm access for accused and convicted domestic abusers. While many state laws predate the 1994 and 1996 federal laws, the peak in state domestic violence firearm law adoption occurred in the years immediately surrounding the changes to federal policy (Schiller and Sidorsky, 2022). Most commonly, these state laws mirror federal regulation—barring firearm ownership and possession among those served with 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, mirrored 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 the federal definition of *intimate partner* by 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 law enforcement and vital statistics data show that the proportion of intimate partner homicides committed by a spouse is nearly equal to the proportion committed by a boyfriend or girlfriend (Cooper and Smith, 2011; Sabri, Campbell, and Messing, 2021).[2]

States have also expanded domestic violence–related prohibitions to include ex parte DVROs, which are temporary orders put into effect prior to a hearing at which the subject of the order may defend themselves. Ex parte DVROs are designed to reduce 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 on those convicted of stalking misdemeanors.

[2] 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 were not covered by federal law.

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 their side of the case) (National Rifle Association, Institute for Legislative Action, 2019a). Furthermore, because ex parte orders typically require lower standards of evidence that the defendant poses a risk than final protective orders, such orders might be used to harm a domestic partner whose job, for example, requires them to carry a weapon (Sullum, 2019). This concern is mitigated by federal and many state laws that have official use exemptions, which allow individuals subject to protective orders to possess firearms while at work if required for their employment by a government agency.

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. Although surrender laws exist in these states, there is evidence that they may not be implemented well. In North Carolina, for instance, judges are required to inquire about firearm ownership and availability in ex parte and full DVRO hearings, and those judges must require the defendant to surrender their firearms to a sheriff when they have made serious threats of violence. After reviewing 406 such hearings between 2016 and 2017, Kafka et al. (2022) estimated that firearm availability was discussed in less than one-quarter of all cases and that firearm surrender was ordered in less than half of cases in which the defendant was alleged to have threatened to kill the plaintiff. Furthermore, when firearms are ordered surrendered, there is often no clear procedure for enforcing such orders (Frattaroli, Zeoli, and Webster, 2021). For more information about firearm surrender and seizure laws, see Chapter 6.

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. If the laws predominantly affect intimate partner homicides, these effects may have to be larger to generate detectable impacts on overall homicides. Of the 8,330 murders in 2022 reported to the Federal Bureau of Investigation (FBI) for which there was a known relationship between victim and offender, 6.5 percent were committed by a spouse or ex-spouse and 8.8 percent were committed by a boyfriend or girlfriend (FBI, 2022d).[3] Thus, a law that reduced intimate partner homicides by 5 percent nationally might result in 64 fewer deaths, which might be difficult to attribute to the law using existing causal inference methods.

[3] In the FBI data, the spousal category includes common-law marriages. There are a variety of limitations to these data (Thompson, Saltzman, and Bibel, 1999; Kaplan, 2023; Lantz, 2022).

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. An analysis of mass shooting data from the Gun Violence Archive from 2014 to 2019 found that 68 percent of the 110 *mass shootings* (defined as incidents with four or more deaths by gunfire, excluding the perpetrator) involved a perpetrator with a history of domestic violence or a family member as one of the victims (Geller, Booty, and Crifasi, 2021). 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 familicide; additionally, about 20 percent of all mass public shootings involved a domestic dispute as a contributing factor (Krouse and Richardson, 2015). Another study of mass murders from 2006 to 2016 (74 percent of which were shootings) found that 30 percent of familicides, 7 percent of mass public killings, and 3 percent of felony-related killings involved an offender with a known history of domestic violence (Fridel, 2017). Although a substantial proportion of these individuals likely had no prior interaction with the criminal justice system related to domestic violence (Zeoli and Paruk, 2020), it remains possible 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 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 police shootings more generally. If an officer does not feel threatened by a suspect because the suspect 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. There is also evidence that some victims are at higher risk of being wrongly identified as the abuser (Deverell, 2022; Reeves, 2023), and laws that remove firearm access from abusers could consequently limit victims' access to firearms for self-defense. However, much of this research has taken place outside the United States, and generalizability to the U.S. context is unclear. Given that these laws are unlikely to affect a large proportion of gun buyers, it is also unclear how these laws would 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 four 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 their 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). However, growing evidence indicates that firearm access is associated with a higher likelihood of nonfatal firearm-related abuse, as well as higher rates of physical, psychological, financial, and social harm related to this abuse (Kafka et al., 2021; Adhia et al., 2021). 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, 2024, 42 states and the District of Columbia have laws prohibiting the possession of firearms by all individuals subject to DVROs.[4] Of those, 13 states prohibit possession

[4] 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,

when an order is issued ex parte.[5] Twenty-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.[6] 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,[7] as well as ex parte orders.[8]

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 some states where a DVRO includes a firearm ban, judges have the discretion to decide whether the subject of the order

Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and the District of Columbia.

[5] California, Colorado, Connecticut, Hawaii, Illinois, Massachusetts, New York, North Carolina, Texas, Utah, Virginia, West Virginia, and Wisconsin. See Calif. Fam. Code §§ 6218, 6389; Colo. Rev. Stat. §§ 13-14-105.5(11), 13-14-104.5(4); Conn. Gen. Stat. §§ 217(a)(4), 46b-15(b); Hawaii Rev. Stat. Ann. §§ 586-3, 134-7(f); Ill. Stat. Ch. 725 §§ 5/112A-14(14.5), 5/112A-17.5(b); Mass. Gen. Laws Ch. 209A, §§ 3B, 4; N.Y. Fam. Ct. Act § 828, 842-a(1), (2); N.C. Gen. Stat. § 50B-3.1; Tex. Penal Code § 25.07(a); Utah Code Ann. § 78B-7-106(2)(f); Va. Code § 18.2-308.1:4 (a-c); W. Va. Code §§ 53-8-7(d)(1)(F), 48-207-204(4), 48-27-403; Wisc. Stat. § 941.29(1m)(f).

[6] Alaska, Arizona, California, Connecticut, Delaware, Hawaii, Illinois, Iowa, Louisiana, Massachusetts, Minnesota, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oregon, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Utah, West Virginia, and Wisconsin. See Alaska Stat. § 18.66.990(5); Ariz. Rev. Stat. § 13-3601; Calif. Fam. Code § 6210; Conn. Gen. Stat. § 46b-38a(2); 10 Del. Code § 1041(2)(b); Hawaii Rev. Stat. Ann. § 586-1; 725 Ill. Comp. Stat. 5/112A-3(3); Ia. Code § 236.2(5); La. Rev. Stat. § 46:2136.3; Mass. Gen. Laws Ch. 209A, § 1(e); Minn. Stat. § 518B.01, subd. 2; Mont. Code § 45-5-26(2); Neb. R.S. § 42-903(3); Nev. Rev. Stat. Ann. § 33.018; N.J. Stat. Ann. § 2C:25-19; N.M. Stat. § 30-7-16; N.Y. Crim. Proc. Law § 530.11; N.C. Gen. Stat. § 50B-1(b); N.D. Cent. Code § 14-07.1-01.4; Ore. Rev. Stat. §135.230; 23 Pa. Cons. Stat. Ann. § 6102(a); R.I. Gen. Laws § 15-15-1(10); S.D. Codified Laws § 25-10-3.1(2); Tenn. Code Ann. § 36-3-601(5)(C); Tex. Fam. Code Ann. § 71.0021; Utah Code Ann. § 78B-7-106(2)(f); W. Va. Code § 48-27-204; Wisc. Stat. § 813.12(ag).

[7] Alaska, Arizona, Hawaii, Kansas, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Montana, Nebraska, Nevada, North Carolina, Ohio, Oregon, Rhode Island, South Carolina, Utah, and West Virginia. See Alaska Stat. 18.66.100(c); Ariz. Rev. Stat. § 13-3602(G)(4); Hawaii Rev. Stat. Ann. § 134-7(f); Kan. Stat. Ann. § 21-6301(a); La. Rev. Stat. § 46:2136.3; Me. Stat. 15 § 393(1)(D); Mass. Gen. Laws Ch. 209A, § 3B; Mich. Comp. Laws §600.2950a(3); Minn. Stat. § 609.2242 Subd. 3(f); Mont. Code §§ 40-15-201(2), 40-15-204(3); Neb. Rev. Stat. § 42-924(1)(g); Nev. Rev. Stat. Ann. §§ 33.031(1)(b), 33.018; N.C. Gen. Stat. § 14-269.8(a), 50B-3.1I(a); Ohio Rev. Code Ann. § 3113.31 (F)(2); Ore. Rev. Stat. § 166.255(1); R.I. Gen. Laws § 15-15-3(4); S.C. Code § 16-25-30; Utah Code Ann. § 78B-7-106(2)(f); W. Va. Code, § 61-7-7(a)(7), (8).

[8] Colorado, Hawaii, Maine, Massachusetts, Michigan, Montana, New Jersey, North Carolina, North Dakota, Pennsylvania, and Utah. See Colo. Rev. Stat. §§ 13-14-105.5(11), 13-14-104.5(4); Hawaii Rev. Stat. Ann. §§ 586-3, 134-7(f); Me. Stat. 19 § 4006; Mass. Gen. Laws Ch. 209A, § 3B, 4; Mich. Comp. Laws § 600.2950a; Mont. Code §§ 40-15-201(2), 40-15-102(1); N.J. Stat. Ann. §§ 2C:25-28fj, 2C:25-29b; N.C. Gen. Stat. § 50B-3.1; N.D. Cent. Code § 14-07.1-03; 23 Pa. Cons. Stat. Ann. § 6107(b)(3); Utah Code Ann. § 78B-7-102(2).

must surrender their 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 the individual does not surrender the weapon as instructed.[9]

Many states go further than just prohibiting possession of firearms by individuals subject to DVROs. Twenty-three states require (or allow judges to require) individuals to surrender their firearms.[10] Seven states additionally allow or require law enforcement officers to remove firearms from prohibited possessors.[11]

Finally, four states go further than federal law and require that court officials notify federal and state agencies of a misdemeanor crime of domestic violence conviction.[12] 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, they 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.[13]

Effects on Suicide

Research Synthesis Findings

We identified three studies that met our inclusion criteria and examined effects on suicide of policies prohibiting gun possession based on criteria related to domestic violence; one of

[9] 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, 5l; N.J. Stat. Ann. §§ 2C:25-28(j), 29(b). Delaware and New Mexico allow removal for regular cases. See 10 Del. Code § 1045(a) (11); N.M. Stat. § 40-13-5(4).

[10] Alaska, Arizona, California, Colorado, Delaware, Florida, Hawaii, Indiana, Iowa, Louisiana, Maryland, Massachusetts, Minnesota, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Rhode Island, Tennessee, Washington, and Wisconsin. See Alaska Stat. § 18.66.100(c); Ariz. Rev. Stat. § 13-3602(G)(4); Calif. Penal Code § 29825(b); Colo. Rev. Stat. § 13-14-105.5(1); 11 Del. Code § 1448(a)(8); Fla. Stat. § 741.31(4)(a); Hawaii Rev. Stat. Ann. § 134-7(f); Ind. Code Ann. § 34-26-5-9(c)(4); Ia. Code § 724.26(4); La. Stat. Ann. Code of Crim. Proc. Art. 1002; Md. Code, Family Law § 4-506(f); Mass. Gen. Laws Ch. 209A, § 3B; Minn. Stat. § 609.2242, Subd. 3(f); Nev. Rev. Stat. Ann. § 33.031(1)(b); N.H. Rev. Stat. Ann. § 173-B:5; N.J. Stat. Ann. §§ 2C:25-29(b), 2C:58-3c(6); N.M. Stat. § 40-13-5(4); N.Y. Fam. Ct. Act § 828, 842-a; N.C. Gen. Stat. § 50B-3.1I(a); R.I. Gen. Laws § 8-8.1-3; Tenn. Code Ann. § 39-17-1307(f)(1)(B); Wash. Rev. Code Ann. § 9.41.040(2)(a); Wisc. Stat. § 813.12(ag).

[11] California, Delaware, Hawaii, Illinois, Massachusetts, New Hampshire, and New Mexico. See Calif. Penal Code § 1524; 10 Del. Code § 1045(a)(11); Hawaii Rev. Stat. Ann. § 134-7.3(b); 725 Ill. Comp. Stat. 60/214(b)(s14.5); Mass. Gen. Laws Ch. 209A, § 3B; N.H. Rev. Stat. Ann. § 173-B:5; N.M. Stat. § 40-13-5(4).

[12] Illinois, Massachusetts, Minnesota, and New York. See 725 Ill. Comp. Stat. 5/112A-11.1, 5/112A-11.2; Mass. Gen. Laws Ch. 265, § 13N; Minn. Stat. § 624.713, subd. 5; N.Y. Crim. Proc. Law §§ 370.15, 380.97..

[13] Wash. Rev. Code (ARCW) § 36.28A (added by 2017 c 261 § 5).

these (Kester et al., 2023) was newly identified for this review, but it is not discussed further because of critical concerns with interpreting estimates from the study's univariate analyses as causal effects and an unfavorable ratio of covariates to observations (less than one to five).

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.

Conclusions

Excluding one study that was subject to critical methodological concerns, two qualifying studies examined the relationship between firearm prohibitions related to an individual's history of domestic violence and suicide. One study found evidence of a negative relationship between prohibitions associated with domestic violence convictions and suicide rates among older men; the effects were 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 informa-

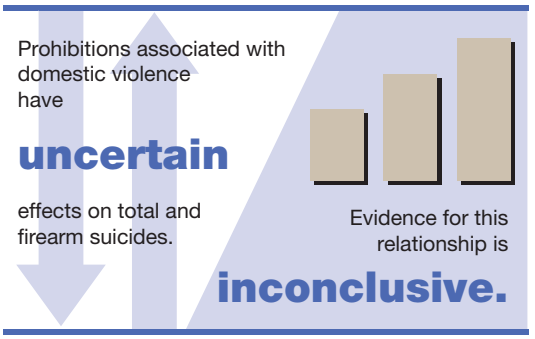
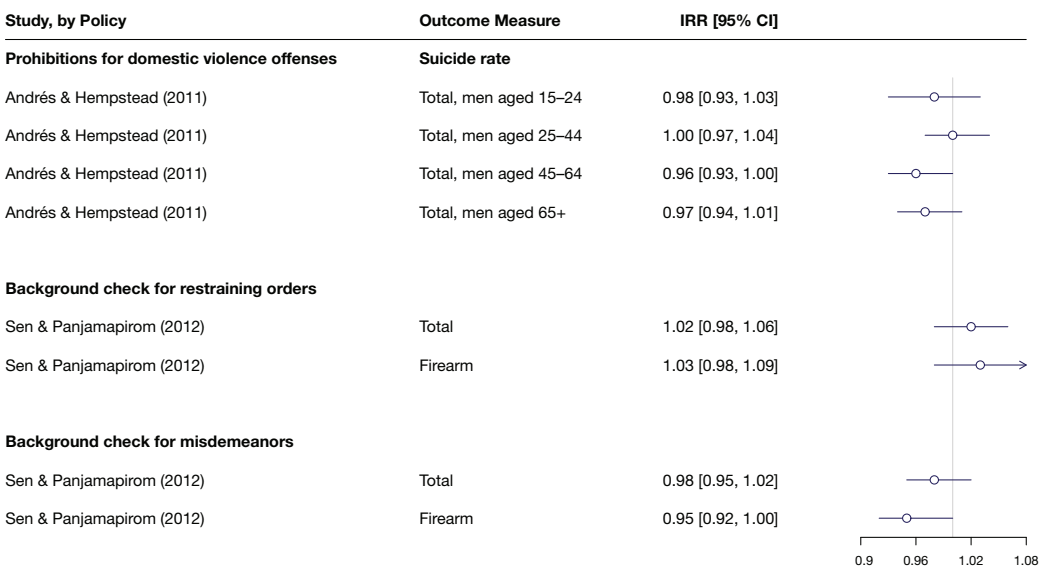


FIGURE 5.1

Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Suicide



NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

tion 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 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 16 qualifying studies that examined the relationship between firearm prohibitions tied to domestic violence and violent crime outcomes, seven of which (Chien and Gakh, 2020; Fridel, 2021a; Wallace et al., 2021; Neufeld et al., 2022; Rochford, Berg, and Peek-Asa, 2022; Scantling et al., 2022; Wallin, Holliday, and Zeoli, 2022) contribute new findings to this

updated review.[14] Two of these studies (Rochford, Berg, and Peek-Asa, 2022; Scantling et al., 2022) were rated as having critical methodological concerns and are not discussed further.[15]

Several of the 14 remaining studies used substantially similar data sets (Vigdor and Mercy, 2006; Raissian, 2016; Díez et al., 2017; Zeoli et al., 2018) but over different time frames, and several focused only on intimate partner or pregnancy-associated homicides as the outcome of interest (Raissian, 2016; Díez et al., 2017; Zeoli et al., 2018; Wallace et al., 2021; Wallin, Holliday, and Zeoli, 2022). Two additional studies (Doucette, Crifasi, and Frattaroli, 2019; Sabbath, Hawkins, and Baum, 2020) focused on workplace homicides. The state laws that were analyzed in our included literature for this class of policies primarily cover firearm prohibitions associated with DVROs and domestic violence misdemeanors, although three studies also specifically analyze firearm prohibitions associated with stalking offenses, which are often linked to 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). Two studies (Chien and Gakh, 2020; Sabbath, Hawkins, and Baum, 2020) used an index measure of 21 domestic violence–related firearm law provisions. 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 6.

One study (Raissian, 2016) focused on the effects of the 1996 Lautenberg Amendment, which was federal legislation that extended prohibited-possessor status to those convicted of misdemeanor domestic violence offenses and stipulated a mechanism for requiring the surrender of all convictees' firearms. Using a linear model controlling for state and year fixed effects, state-specific linear trends, and many state-level covariates (including demographics, socioeconomic measures, social insurance policies, crime rates, unilateral divorce laws, and other firearm and criminal policies), the author identified the effect of the federal law by taking into account states' preexisting variation in assault statutes, which, because of imprecise language in the Lautenberg Amendment, led to variation across states in whether the new federal prohibitions applied to domestic violence misdemeanor cases.[16] In 2009, with

[14] The study by Neufeld and colleagues (2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Given evidence suggesting that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

[15] Scantling et al. (2022) conducted two sets of analyses, a univariate analysis that had critical issues related to causal identification and a multivariate analysis that had critical issues related to potential model overfit (i.e., fewer than five observations per estimated parameter). The short time frame (2013–2017) and limited set of controls included in the selection-on-observables approach of Rochford, Berg, and Peek-Asa (2022) raise critical methodological concerns with causal identification.

[16] 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, the U.S. Supreme Court corrected the ambiguity in federal law that produced this variation. Raissian (2016) found that the rate of firearm-related intimate partner homicides and the rate of other family homicides declined when domestic violence misdemeanants were prohibited from possessing firearms and were required to surrender those 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.

Five studies evaluated intimate partner violence outcomes, but instead analyzed the effects of state laws prohibiting firearm purchase or possession based on domestic violence history. All of these studies are subject to minor methodological concerns related to not assessing or justifying their models' identifying assumptions, having inadequate descriptions of policy variation, or having potential model overfit. 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, region time trends, unilateral divorce laws, other gun policies, per capita ethanol consumption, arrest rates, and state-level demographic characteristics. The authors 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, which were intended as a falsification exercise, showed that the significant 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).

Four subsequent studies expanded the time series evaluated and considered additional state laws with potential relevance for influencing intimate partner violence specifically. 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 esti-

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.

mating 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 under the felony charge.[17] 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. In a study that used nearly equivalent methods but stratified regression models by decedent race (White, Black), Wallin, Holliday, and Zeoli (2022) suggested that these overall effects (decreased intimate partner homicide with state DVRO restrictions, increased intimate partner homicide with prohibitions for stalking misdemeanors) were

[17] Zeoli et al. (2018) also estimated 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 6.

driven by effects on White decedents; however, while estimated effects for Black decedents were mostly uncertain and small in magnitude, the authors did not report a statistical test for differences by race.

Instead of examining intimate partner homicides, Wallace et al. (2021) assessed how state DVROs, domestic violence misdemeanor prohibitions, and associated firearm relinquishment requirements affect pregnancy-associated homicides (i.e., homicides with a decedent who is pregnant or within one year from the end of pregnancy at the time of their death). Using data from 2011 to 2019, the authors estimated a linear regression model, weighting by the number of live births in a given state-year, controlling for state fixed effects and year fixed effects, and considering a set of state-level time-varying covariates reflecting broader homicide rates, economic variables, and demographic variables. Their results indicated that, when combined with firearm relinquishment requirements, DVROs and domestic violence misdemeanor prohibitions significantly reduced pregnancy-associated homicides; domestic violence misdemeanor prohibitions also showed suggestive effects consistent with a reduction in firearm-related pregnancy-associated homicides. However, several regression models had large numbers of parameters relative to observations (ratios ranging from three to six). While models that dropped all time-varying covariates had less potential for serious problems because of model overfit, the exclusion of controls for these potential confounds without adequate justification or assessment of the validity of the parallel trends assumption required for unbiased estimates produces serious methodological concerns with the accuracy of these estimates and their inferential statistics.

Four studies evaluated the influence of domestic violence–related prohibitions on broader forms of violence (Neufeld et al., 2022; Fridel, 2021a; Chien and Gakh, 2020; Knopov et al., 2019). Chien and Gakh (2020) estimated the association between the number of state laws related to domestic violence prohibitions (including surrender requirements) and state-level firearm homicide rates. To account for possible delayed law effects, they interacted the number of state laws with a natural cubic spline to allow a flexible phase-in of the effect of provisions lasting up to eight years. They used a Poisson quasi-maximum likelihood estimator to fit their model, which includes a penalized spline for time to account for national trends, random intercepts for states, state-level controls for demographics and socioeconomic factors, and controls for the violent crime rate and law enforcement officer per capita rate. The pattern of results is puzzling because they indicate that a very high number of laws significantly increases firearm homicide rates up until about the six-year lag, when that relationship flips to become highly negative. Generating cumulative estimates over the eight years post–policy implementation shows negative effects associated with the number of domestic violence prohibitions that reach statistical significance when the number of provisions is six or higher. However, the study does not account for serial correlation, which renders its findings subject to serious methodological concerns. Fridel (2021a) conducted a more straightforward two-way fixed-effects analysis of the relationship of DVRO prohibitions with total homicide rates and found uncertain effects, although the purpose of this study was to evaluate shall-issue laws, and other firearm laws were intended as ancillary covariates, thus sub-

jecting these results to serious methodological concerns with a causal interpretation (Hünermund and Louw, 2023; Westreich and Greenland, 2013).

Neufeld et al. (2022) used data from 2000 to 2016 to evaluate how requiring surrender of firearms for individuals convicted of domestic violence misdemeanors or subject to DVROs influences firearm injury hospitalization rates. Using two-way fixed effects regression models that adjusted for a small set of state-level time-varying covariates (demographic and socioeconomic characteristics, a proxy measure for household firearm ownership, violent crime rate, and per capita alcohol consumption) but no other state firearm laws, they found uncertain effects of these requirements on firearm injury hospitalizations. However, their analysis did not account for the uncertainty produced by the imputed nature of the outcome series (Carpenter and Kenward, 2012; Rubin, 1987), nor did the authors provide an assessment of the model's identifying assumption, raising minor concerns related to inferential validity. In an alternative specification, they found that laws allowing or requiring removal of firearms by law enforcement for these same offenders significantly reduced firearm injury hospitalization rates; however, these effects were based on law transitions in only two states, raising serious concerns with statistical power, generalizability to other contexts, and the ability to isolate the cause of any observed change to the passage of the law versus to other co-occurring factors.

Knopov et al. (2019) evaluated the effects on Black and White citizens separately of state laws that either prohibit firearm ownership by those with a history of violent misdemeanors or require firearm surrender by those subject to a DVRO. Using a log-linear model with two-way fixed effects; a dozen social, economic, and crime covariates, including lagged firearm homicide rates; and several interaction terms, the authors modeled the effects of seven classes of laws on total and firearm homicides in 39 states between 1991 and 2016. Although prohibitions associated with violent misdemeanors and DVRO firearm surrender laws were significantly associated with reductions in total and firearm homicide rates among Black citizens, effects for White citizens were uncertain. Moreover, because this law class blended two types of laws, it is unclear what portion of the observed effects is attributable to just the DVRO prohibition.

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.

Finally, two studies estimated the effects of domestic violence–related prohibitions on workplace homicides. In a study focused on right-to-carry laws, Doucette, Crifasi, and Frattaroli (2019) also examined ex parte DVRO laws and laws that expand the definition of *domes-*

tic violence to include violence by dating partners. The authors' negative binomial regression model controlled for four other firearm laws, 14 social and economic state covariates, state and year fixed effects, region fixed effects, and random intercepts for between-state effects.[18] These analyses found uncertain effects of dating partner laws and only suggestive evidence that ex parte DVRO laws may reduce workplace homicides.

In contrast, Sabbath, Hawkins, and Baum (2020) found that as the number of state domestic violence laws restricting access to or ownership of firearms increases, there is a significant reduction in workplace homicides. Specifically, the authors estimated that a shift from the 25th percentile in the number of state domestic violence laws to the 75th percentile was associated with a 5.3-percent reduction in workplace homicides between 2011 and 2017. The gamma regression model used to generate this estimate included year fixed effects and six demographic and economic covariates. The model did not include state fixed or random effects, and it is not clear that the included covariates would suffice to control for other state differences that could explain both the adoption of domestic violence prohibitions and changes in workplace homicide rates; thus, there are serious concerns that the estimated law effects may reflect confounding differences across states rather than the causal impacts of the law.

Figure 5.2 displays the IRRs and CIs for estimated effects associated with prohibitions related to domestic violence from these studies. Knopov et al. (2019) did not provide CIs or other details for their estimate of the law effects on firearm homicides, so only their estimates for total homicides are included in the figure. Wallace et al. (2021) also did not provide CIs or other inferential statistics that could be used to construct CIs, and thus their estimates are not shown. Finally, we do not present results from Chien and Gakh (2020) because their manner of parameterizing law effects does not produce a single effect size measure.

Conclusions

Excluding studies with critical methodological concerns, 14 studies met our inclusion criteria and evaluated how firearm prohibitions associated with domestic violence affect violent crime outcomes.

DVRO policies and intimate partner homicide. Six studies estimated the relationship of state DVRO-related prohibitions with intimate partner or pregnancy-associated homicide rates. Four of these studies found that state firearm prohibitions related to DVROs resulted in significantly lower rates of total and firearm-related intimate partner homicide overall (Zeoli et al., 2018; Zeoli and Webster, 2010; Vigdor and Mercy, 2006) or among White victims (Wallin, Holliday, and Zeoli, 2022). Three studies, which stratified the DVRO policy by

[18] As described in the study, the model may have been overparameterized. It is not clear how state fixed effects, region fixed effects, and random effects could all be identified in a single model. However, in the Stata software package used for these analyses, such overparameterization would not necessarily result in uninterpretable policy effect estimates because superfluous state or region fixed effects would be dropped automatically.

FIGURE 5.2

Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
State DVRO prohibition	**Intimate partner homicide rate**	
Zeoli et al. (2018)	Total	0.90 [0.83, 0.97]
Zeoli & Webster (2010)	Total	0.81 [0.68, 0.95]
Vigdor & Mercy (2006)	Total	0.92 [0.86, 0.99]
Wallin, Holliday, & Zeoli (2022)	Total, White	0.90 [0.83, 0.98]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.01 [0.88, 1.15]
Zeoli et al. (2018)	Firearm	0.87 [0.78, 0.97]
Zeoli & Webster (2010)	Firearm	0.75 [0.62, 0.92]
Vigdor & Mercy (2006)	Firearm	0.91 [0.84, 0.99]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.89 [0.80, 0.99]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	0.98 [0.82, 1.17]
State DVRO prohibition	**Violent crime rate**	
Fridel (2021a)	Total homicide	1.02 [0.98, 1.05]
Knopov et al. (2019)	Total homicide, white	0.97 [0.92, 1.02]
Knopov et al. (2019)	Total homicide, black	0.87 [0.81, 0.94]
Vigdor & Mercy (2006)	Stranger homicide	1.01 [0.81, 1.26]
Vigdor & Mercy (2006)	Rape	1.00 [0.93, 1.08]
Vigdor & Mercy (2006)	Robbery	1.01 [0.91, 1.12]
Vigdor & Mercy (2006)	Assault	1.00 [0.88, 1.14]
Number of DV-related laws	**Workplace homicide rate**	
Sabbath, Hawkins, & Baum (2020)	Total	0.97 [0.96, 0.99]
Dating partner DVROs		
Doucette, Crifasi, & Frattaroli (2019)	Firearm	0.99 [0.87, 1.13]
Ex parte DVROs		
Doucette, Crifasi, & Frattaroli (2019)	Firearm	0.91 [0.78, 1.04]
State background checks for restraining orders	**Homicide rate**	
Sen & Panjamapirom (2012)	Total	0.91 [0.85, 0.98]
Sen & Panjamapirom (2012)	Firearm	0.87 [0.79, 0.95]

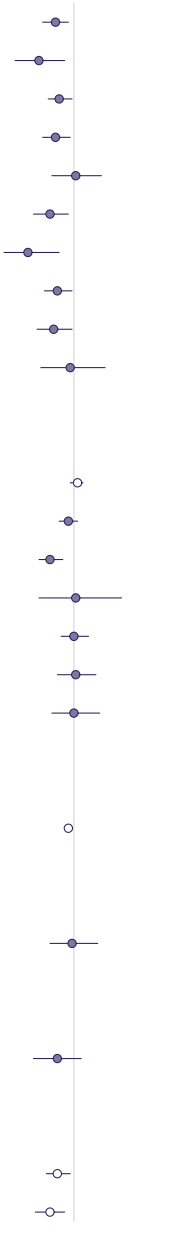


Figure continued on the next page

Figure 5.2—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
State DV misdemeanor prohibition	**Intimate partner homicide rate**	
Zeoli et al. (2018)	Total	1.08 [0.92, 1.27]
Zeoli & Webster (2010)	Total	1.08 [0.91, 1.27]
Vigdor & Mercy (2006)	Total	1.00 [0.92, 1.09]
Wallin, Holliday, & Zeoli (2022)	Total, White	0.99 [0.85, 1.16]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.00 [0.88, 1.14]
Zeoli et al. (2018)	Firearm	1.13 [0.94, 1.35]
Zeoli & Webster (2010)	Firearm	1.18 [0.99, 1.41]
Vigdor & Mercy (2006)	Firearm	0.92 [0.84, 1.02]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	1.04 [0.87, 1.24]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	1.06 [0.91, 1.23]
State DV misdemeanor prohibition	**Violent crime rate**	
Vigdor & Mercy (2006)	Stranger homicide	0.98 [0.81, 1.18]
Vigdor & Mercy (2006)	Rape	1.08 [0.99, 1.18]
Vigdor & Mercy (2006)	Robbery	0.97 [0.86, 1.08]
Vigdor & Mercy (2006)	Assault	1.12 [0.99, 1.27]
DV misdemeanor prohibition and surrender requirement	**Intimate partner homicide rate**	
Díez et al. (2017)	Total	0.96 [0.90, 1.03]
Wallin, Holliday, & Zeoli (2022)	Total, White	0.93 [0.86, 1.01]
Wallin, Holliday, & Zeoli (2022)	Total, Black	0.95 [0.86, 1.05]
Raissian (2016)	Firearm	0.90 [0.81, 0.99]
Díez et al. (2017)	Firearm	0.93 [0.84, 1.02]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.93 [0.84, 1.03]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	0.87 [0.75, 1.01]
Surrender requirements at DV scene		
Wallin, Holliday, & Zeoli (2022)	Total, White	0.98 [0.86, 1.13]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.07 [0.95, 1.21]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.98 [0.82, 1.16]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	1.03 [0.86, 1.22]
Surrender requirements at DV scene	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm	0.94 [0.83, 1.07]
State background checks for misdemeanor convictions	**Homicide rate**	
Sen & Panjamapirom (2012)	Total	1.02 [0.95, 1.10]
Sen & Panjamapirom (2012)	Firearm	0.99 [0.90, 1.08]

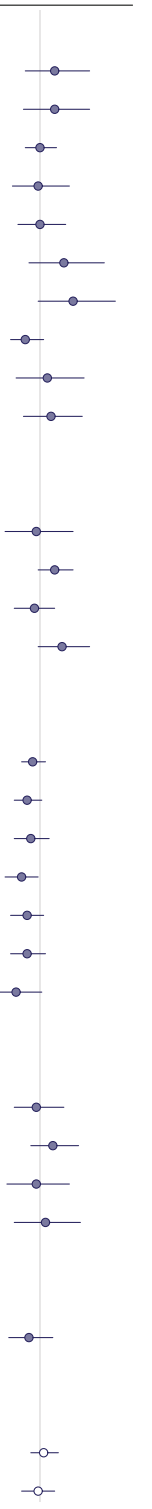
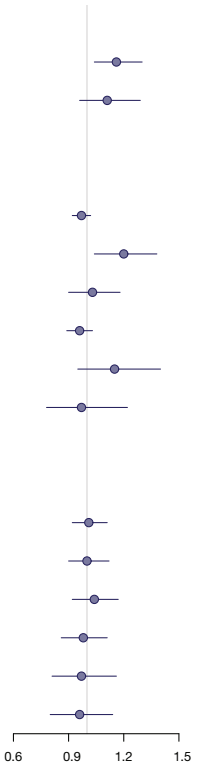


Figure continued on the next page

Figure 5.2—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Prohibition for stalking misdemeanor	**Intimate partner homicide rate**	
Zeoli et al. (2018)	Total	1.16 [1.04, 1.30]
Zeoli et al. (2018)	Firearm	1.11 [0.96, 1.29]
Prohibition for stalking crimes		
Díez et al. (2017)	Total	0.97 [0.92, 1.02]
Wallin, Holliday, & Zeoli (2022)	Total, White	1.20 [1.04, 1.38]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.03 [0.90, 1.18]
Díez et al. (2017)	Firearm	0.96 [0.89, 1.03]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	1.15 [0.95, 1.40]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	0.97 [0.78, 1.22]
Stalking as felony crime		
Zeoli et al. (2018)	Total	1.01 [0.92, 1.11]
Wallin, Holliday, & Zeoli (2022)	Total, White	1.00 [0.90, 1.12]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.04 [0.92, 1.17]
Zeoli et al. (2018)	Firearm	0.98 [0.86, 1.11]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.97 [0.81, 1.16]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	0.96 [0.80, 1.14]

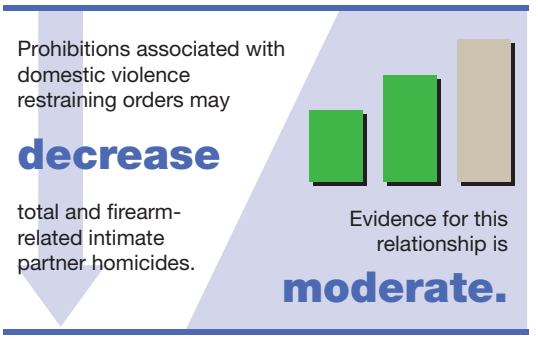


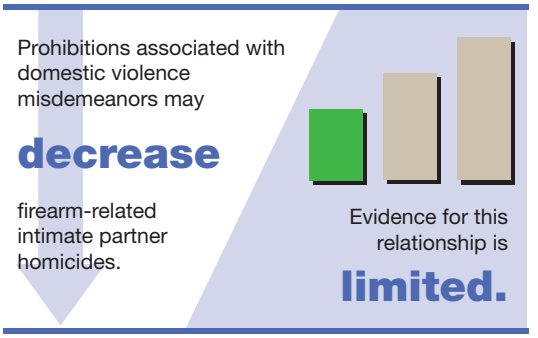
NOTE: The study model in Díez et al. (2017) includes a one-year lag of the dependent variable, and the models of Knopov et al. (2019) include a one-year lag of the firearm homicide rate, so the effect sizes from these studies are not directly comparable with others in this figure. IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details. DV = domestic violence. IPH = intimate partner homicide.

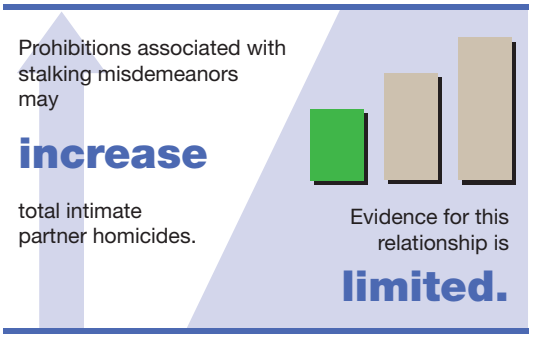
whether firearm relinquishment was required, found that negative relationships with intimate partner homicide or pregnancy-associated homicide were significant when relinquishment was required but were negative and suggestive or uncertain without required relinquishment (Wallin, Holliday, and Zeoli, 2022; Wallace et al., 2021; Díez et al., 2017). All but one of these studies drew on substantially similar data sets with largely overlapping time periods. Considering these results and the relative methodological quality of the studies, 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 six studies examined how intimate partner homicide rates or pregnancy-associated 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, although two found a suggestive or significant negative relationship if the prohibition was coupled with

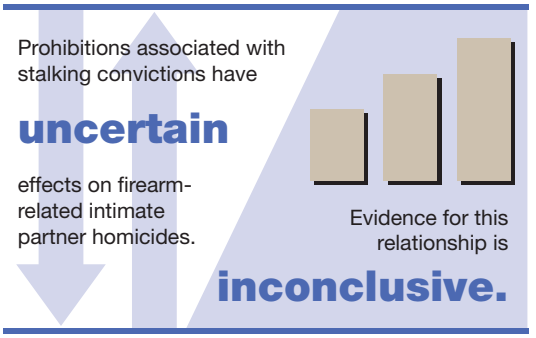
required firearm relinquishment (Wallace et al., 2021; Díez et al., 2017). One additional study that estimated the effect of a policy that jointly expanded firearm prohibitions to include domestic violence misdemeanants and established a mechanism for firearm surrender estimated a significant negative effect of the policy on firearm-related intimate partner homicide rates (Raissian, 2016). Another study that evaluated the same policy by stratifying analyses by victim race found suggestive reductions in intimate partner homicides for White victims and suggestive reductions in firearm-related intimate partner homicides for Black victims (Wallin, Holliday, and Zeoli, 2022). Considering these results, we find *limited evidence that firearm prohibitions associated with domestic violence misdemeanors can reduce firearm-related intimate partner homicides*. We note that the weight of this evidence draws on findings from the federal policy change that combined the prohibition with surrender requirements.







Prohibitions associated with stalking offenses and intimate partner homicide. Three studies (Wallin, Holliday, and Zeoli, 2022; 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). One study found that stalking felony laws had uncertain effects but that firearm prohibitions associated with misdemeanor stalking offenses significantly increased total intimate partner homicide rates and had a suggestive effect consistent with increasing firearm-related intimate partner homicides (Zeoli et al., 2018). A subgroup analysis of these laws, stratifying by victim race, found that results for misdemeanor stalking offenses were significant for intimate partner homicides involving White victims and suggestive for firearm-related intimate partner homicides involving White victims; effects were uncertain for Black victims (Wallin, Holliday, and Zeoli, 2022).



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. Eight qualifying studies without critical methodological concerns examined outcomes other than intimate partner homicide (Neufeld et al., 2022; Fridel, 2021a; Chien and Gakh, 2020; Sen and Panjamapirom, 2012; Vigdor and Mercy, 2006; Doucette, Crifasi, and Frattaroli, 2019; Knopov et al., 2019; Sabbath, Hawkins, and Baum, 2020). Neufeld et al. (2022) found an uncertain relationship between firearm injury hospitalizations and laws requiring the surrender of firearms among domestic violence offenders with misdemeanors or restraining orders. Two studies found that DVRO-related prohibitions had an uncertain relationship with total homicides, stranger homicides, or nonfatal violent crime (Fridel, 2021a; Vigdor and Mercy, 2006), 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 (Vigdor and Mercy, 2006). Chien and Gakh (2020) showed a strange pattern of results but generally found cumulative longer-term effects consistent with more domestic violence–related firearm restrictions being associated with lower rates of firearm homicide. Knopov et al. (2019) found uncertain effects of a combination of violent misdemeanor prohibitions and DVRO firearm surrender laws for White populations, but significant reductions in total and firearm-related homicides for Black populations. Doucette, Crifasi, and Frattaroli (2019) found uncertain effects of dating partner and ex parte DVRO laws on firearm homicides in the workplace, whereas Sabbath, Hawkins, and Baum (2020) found significant reductions in workplace homicides as the number of provisions aimed at restricting firearm access based on domestic violence increased in a state. Considering these results, *we find limited evidence that domestic violence firearm prohibitions may reduce total and firearm-related homicides among some subgroups.*

Effects on Mass Shootings

Research Synthesis Findings

We identified two studies that met our inclusion criteria and examined the relationship between prohibitions associated with domestic violence and mass shootings, one of which

contributes new findings to this updated review (Fridel, 2021a). Webster et al. (2020) identified homicides between 1984 and 2017 that involved four or more firearm fatalities (excluding offender fatalities and drug- or gang-related shootings) using the FBI Supplementary Homicide Reports and two additional publicly available data sources. The authors estimated effects on mass shooting incidence and mass shooting fatalities, distinguishing between mass shootings that involved a domestic relationship and those that did not.[19] Among the 14 types of policies included in the study were four related to domestic violence: firearm prohibitions for subjects of DVROs that include ex parte orders, firearm prohibitions for subjects of DVROs that include dating partners in the definition of domestic violence, firearm prohibitions for subjects of DVROs that do not include ex parte orders or dating partners, and laws requiring surrender of all firearms by subjects of DVROs. The authors fit negative binomial regression models that included all 14 laws, state fixed effects, linear and quadratic time trends, and numerous time-varying covariates related to gun ownership, economic conditions, alcohol and drug consumption, and demographics. They found uncertain effects of all four domestic violence–related prohibitions on mass shootings. These results are subject to minor methodological concerns related to model overfit and limited discussion or assessment of the validity of their models' identifying assumptions.

Fridel (2021a) instead analyzed the relationship of DVRO prohibitions with mass murder rates; *mass murders* are defined as incidents with four or more fatalities (excluding the perpetrator) within a 24-hour period. Using data from 1991 to 2016, the author used negative binomial models that employed generalized estimating equations and included region-level fixed effects, year fixed effects, controls for other firearm policies, and a large set of state-level time-varying covariates (e.g., a proxy for firearm ownership, socioeconomic factors, crime rates). Results showed uncertain effects of the law, although the purpose of this study was to evaluate shall-issue laws; because these other firearm laws were intended as ancillary covariates, these results are subject to serious methodological concerns, thus limiting a causal interpretation.

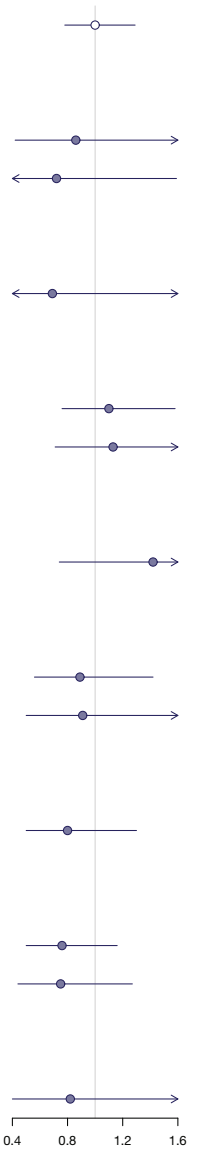
Figure 5.3 displays the IRRs and CIs for estimated effects associated with prohibitions related to domestic violence from these two studies.

[19] The authors identified mass shootings based on the Supplementary Homicide Report classification of the offender's relationship with the first victim and including any family relationship, boyfriend or girlfriend, or ex-spouse. This classification produces potential errors because, for example, an incident in which an offender killed three members of their family and one stranger might be classified as a familicide or a mass public shooting depending on who the first victim listed in the Supplementary Homicide Report is.

FIGURE 5.3

Incidence Rate Ratios Associated with the Effect of Domestic Violence–Related Prohibitions on Mass Shootings

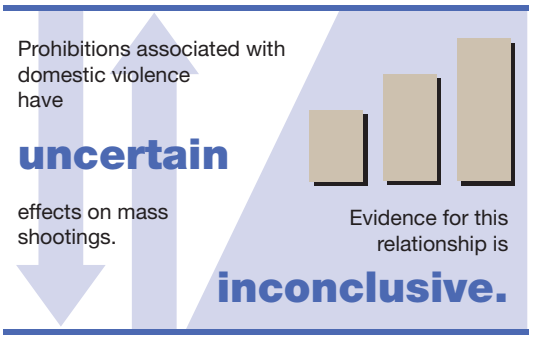
Study, by Policy	Outcome Measure	IRR [95% CI]
DVRO prohibition	**Mass murder**	
Fridel (2021a)	Number of incidents	1.00 [0.78, 1.29]
DVRO prohibition with final order, no dating partners	**Mass shooting**	
Webster et al. (2020)	Number of incidents	0.86 [0.42, 1.77]
Webster et al. (2020)	Number of fatalities	0.72 [0.33, 1.59]
DVRO prohibition with final order, no dating partners	**Domestic-linked mass shooting**	
Webster et al. (2020)	Number of fatalities	0.69 [0.22, 2.13]
DVRO prohibition includes ex parte	**Mass shooting**	
Webster et al. (2020)	Number of incidents	1.10 [0.76, 1.58]
Webster et al. (2020)	Number of fatalities	1.13 [0.71, 1.77]
DVRO prohibition includes ex parte	**Domestic-linked mass shooting**	
Webster et al. (2020)	Number of fatalities	1.42 [0.74, 2.74]
DVRO prohibition includes dating partners	**Mass shooting**	
Webster et al. (2020)	Number of incidents	0.89 [0.56, 1.42]
Webster et al. (2020)	Number of fatalities	0.91 [0.50, 1.65]
DVRO prohibition includes dating partners	**Domestic-linked mass shooting**	
Webster et al. (2020)	Number of fatalities	0.80 [0.50, 1.30]
DVRO prohibition with surrender	**Mass shooting**	
Webster et al. (2020)	Number of incidents	0.76 [0.50, 1.16]
Webster et al. (2020)	Number of fatalities	0.75 [0.44, 1.27]
DVRO prohibition with surrender	**Domestic-linked mass shooting**	
Webster et al. (2020)	Number of fatalities	0.82 [0.40, 1.67]



NOTE: IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.

Conclusions

Two studies met our inclusion criteria and evaluated how firearm prohibitions associated with domestic violence affect mass shootings or mass murders (Fridel, 2021a; Webster et al., 2020). Both studies found uncertain evidence of each of the laws on these outcomes. Considering these results, we find *inconclusive evidence for how laws establishing firearm prohibitions for domestic violence offenders affect mass shootings.*



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
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 5 References

Adhia, Avanti, Vivian H. Lyons, Caitlin A. Moe, Ali Rowhani-Rahbar, and Frederick P. Rivara, "Nonfatal Use of Firearms in Intimate Partner Violence: Results of a National Survey," *Preventive Medicine*, Vol. 147, June 2021.

Andrés, Antonio Rodríguez, and Katherine Hempstead, "Gun Control and Suicide: The Impact of State Firearm Regulations in the United States, 1995–2004," *Health Policy*, Vol. 101, No. 1, 2011.

Campbell, J. C., D. Webster, J. Kozio-McLain, C. Block, D. Campbell, M. A. Curry, F. Gary, N. Glass, J. McFarlane, C. Sachs, P. Sharps, Y. Ulrich, S. A. Wilt, J. Manganello, X. Xu, J. Schollenberger, V. Frye, and K. Laughon, "Risk Factors for Femicide in Abusive Relationships: Results from a Multisite Case Control Study," *American Journal of Public Health*, Vol. 93, No. 7, 2003.

Carpenter, James R., and Michael G. Kenward, *Multiple Imputation and Its Application*, John Wiley and Sons, 2012.

Cherney, Samantha, Andrew R. Morral, Terry L. Schell, and Sierra Smucker, *RAND State Firearm Law Database*, RAND Corporation, TL-283-1-RC, 2019. As of October 14, 2019: https://www.rand.org/pubs/tools/TL283-1.html

Chien, L-C., and M. Gakh, "The Lagged Effect of State Gun Laws on the Reduction of State-Level Firearm Homicide Mortality in the United States from 1999 to 2017," *Public Health*, Vol. 189, December 2020.

Cooper, Alexia, and Erica L. Smith, *Homicide Trends in the United States 1980–2008*, U.S. Department of Justice, November 2011.

Deverell, Erin, *Misidentification of the Predominant Aggressor in Tasmania: Practitioner Perspectives from Engender Equality*, Engender Equality, December 2022.

Díez, Carolina, Rachel P. Kurland, Emily F. Rothman, Megan Bair-Merritt, Eric Fleegler, Ziming Xuan, Sandro Galea, Craig S. Ross, Bindu Kalesan, Kristin A. Goss, and Michael Siegel, "State Intimate Partner Violence–Related Firearm Laws and Intimate Partner Homicide Rates in the United States, 1991 to 2015," *Annals of Internal Medicine*, Vol. 167, No. 8, 2017.

Doucette, Mitchell L., Cassandra K. Crifasi, and Shannon Frattaroli, "Right-to-Carry Laws and Firearm Workplace Homicides: A Longitudinal Analysis (1992–2017)," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

FBI—*See* Federal Bureau of Investigation.

Federal Bureau of Investigation, "Expanded Homicide Data Table 10: Murder Circumstances, by Relationship, 2019," webpage, Crime in the United States 2019, 2020a. As of August 17, 2022: https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/expanded-homicide-data-table-10.xls

Federal Bureau of Investigation, "Crime Data Explorer," homepage, 2022d. As of November 15, 2023:
https://cde.ucr.cjis.gov/LATEST/webapp/#/pages/explorer/crime/crime-trend

Frattaroli, Shannon, April M. Zeoli, and Daniel W. Webster, "Armed, Prohibited and Violent at Home: Implementation and Enforcement of Restrictions on Gun Possession by Domestic Violence Offenders in Four U.S. Localities," *Journal of Family Violence*, Vol. 36, No. 5, 2021.

Fridel, Emma E., "A Multivariate Comparison of Family, Felony, and Public Mass Murders in the United States," *Journal of Interpersonal Violence*, Vol. 36, Nos. 3–4, 2017.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Fridel, Emma E., and James Alan Fox, "Gender Differences in Patterns and Trends in U.S. Homicide, 1976–2017," *Violence and Gender*, Vol. 6, No. 1, 2019.

Geller, Lisa B., Marisa Booty, and Cassandra K. Crifasi, "The Role of Domestic Violence in Fatal Mass Shootings in the United States, 2014–2019," *Injury Epidemiology*, Vol. 8, No. 1, May 31, 2021.

Gold, Sharon L., "Why Are Victims of Domestic Violence Still Dying at the Hands of Their Abusers? Filling the Gap in State Domestic Violence Gun Laws," *Kentucky Law Journal*, Vol. 91, No. 4, 2003.

Hemenway, David, Deborah Azrael, Andrew Conner, and Matthew Miller, "Variation in Rates of Fatal Police Shootings Across U.S. States: The Role of Firearm Availability," *Journal of Urban Health*, Vol. 96, No. 1, 2019a.

Hünermund, Paul, and Beyers Louw, "On the Nuisance of Control Variables in Regression Analysis," *Organizational Research Methods*, December 25, 2023, online ahead of print.

Kafka, Julie M., Kathryn E. Moracco, Deanna S. Williams, and Claire G. Hoffman, "What Is the Role of Firearms in Nonfatal Intimate Partner Violence? Findings from Civil Protective Order Case Data," *Social Science and Medicine*, Vol. 283, August 2021.

Kafka, Julie M., Kathryn E. Moracco, Deanna S. Williams, and Claire G. Hoffman, "Disarming Abusers: Domestic Violence Protective Order (DVPO) Firearm Restriction Processes and Dispositions," *Criminology and Public Policy*, Vol. 21, No. 2, May 2022.

Kaplan, Jacob, *National Incident-Based Reporting System (NIBRS) Data: A Practitioner's Guide*, March 28, 2023.

Kercher, Cassandra, David I. Swedler, Keshia M. Pollack, and Daniel W. Webster, "Homicides of Law Enforcement Officers Responding to Domestic Disturbance Calls," *Injury Prevention*, Vol. 19, No. 5, 2013.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Krouse, William J., and Daniel J. Richardson, *Mass Murder with Firearms: Incidents and Victims, 1999–2013*, Congressional Research Service, R44126, 2015.

Langhinrichsen-Rohling, Jennifer, "Top 10 Greatest 'Hits': Important Findings and Future Directions for Intimate Partner Violence Research," *Journal of Interpersonal Violence*, Vol. 20, No. 1, 2005.

Langhinrichsen-Rohling, Jennifer, Russell E. Palarea, Jennifer Cohen, and Martin L. Rohling, "Breaking Up Is Hard to Do: Unwanted Pursuit Behaviors Following the Dissolution of a Romantic Relationship," in K. E. Davis, I. H. Frieze, and R. D. Maiuro, eds., *Stalking: Perspectives on Victims and Perpetrators*, Springer, 2002.

Lantz, Brendan, "Why More Agencies and Researchers Should Embrace the Upcoming NIBRS Transition: Contributions and Promise of the NIBRS Data," *American Journal of Criminal Justice*, Vol. 47, No. 3, June 2022.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Logan, J., Holly A. Hill, Michele Lynberg Black, Alex E. Crosby, Debra L. Karch, Jamar D. Barnes, and Keri M. Lubell, "Characteristics of Perpetrators in Homicide-Followed-by-Suicide Incidents: National Violent Death Reporting System—17 U.S. States, 2003–2005," *American Journal of Epidemiology*, Vol. 168, No. 9, 2008.

National Rifle Association, Institute for Legislative Action, "Bloomberg Course Continues Bloomberging Rights Away," May 24, 2019a.

Nelson, Jodi L., "The Lautenberg Amendment: An Essential Tool for Combatting Domestic Violence," *North Dakota Law Review*, Vol. 75, No. 2, 1999.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Public Law 90-618, Gun Control Act of 1968, October 22, 1968.

Public Law 103-322, Violent Crime Control and Law Enforcement Act of 1994, 1994.

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

Public Law 117-159, Bipartisan Safer Communities Act, June 25, 2022.

Raissian, Kerri M., "Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?" *Journal of Policy Analysis and Management*, Vol. 35, No. 1, Winter 2016.

Reeves, Ellen, "A Culture of Consent: Legal Practitioners' Experiences of Representing Women Who Have Been Misidentified as Predominant Aggressors on Family Violence Intervention Orders in Victoria, Australia," *Feminist Legal Studies*, Vol. 31, No. 3, November 2023.

Rochford, Hannah I., Mark Berg, and Corinne Peek-Asa, "The 'Boyfriend Loophole' and Intimate Partner Homicides: A Longitudinal Analysis Using the National Violent Death Reporting System," *Journal of Prevention*, Vol. 43, No. 6, December 2022.

Rubin, Donald B., *Multiple Imputation for Nonresponse in Surveys*, John Wiley and Sons, 1987.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Sabri, Bushra, Jacquelyn C. Campbell, and Jill T. Messing, "Intimate Partner Homicides in the United States, 2003–2013: A Comparison of Immigrants and Nonimmigrant Victims," *Journal of Interpersonal Violence*, Vol. 36, Nos. 9–10, May 2021.

Saltzman, Linda E., James A. Mercy, Patrick W. O'Carroll, Mark L. Rosenberg, and Philip H. Rhodes, "Weapon Involvement and Injury Outcomes in Family and Intimate Assaults," *JAMA*, Vol. 267, No. 22, 1992.

Scantling, Dane R., Daniel N. Holena, Elinore J. Kaufman, Allyson M. Hynes, Justin Hatchimonji, James P. Byrne, Douglas Wiebe, and Mark J. Seamon, "Modifiable Factors Related to Firearm Homicides: A Broader View of Our Lane," *Annals of Surgery*, 2022, online ahead of print.

Schiller, Wendy J., and Kaitlin N. Sidorsky, "Federalism, Policy Diffusion, and Gender Equality: Explaining Variation in State Domestic Violence Firearm Laws 1990–2017," *State Politics & Policy Quarterly*, Vol. 22, No. 3, 2022.

Sen, B., and A. Panjamapirom, "State Background Checks for Gun Purchase and Firearm Deaths: An Exploratory Study," *Preventive Medicine*, Vol. 55, No. 4, 2012.

Smith, Sharon G., Jieru Chen, Kathleen C. Basile, Leah K. Gilbert, Melissa T. Merrick, Nimesh Patel, Margie Walling, and Anurag Jain, *The National Intimate Partner and Sexual Violence Survey: 2010–2012 State Report*, Centers for Disease Control and Prevention, 2017.

Smith, Sharon G., Katherine A. Fowler, and Phyllis H. Niolon, "Intimate Partner Homicide and Corollary Victims in 16 States: National Violent Death Reporting System, 2003–2009," *American Journal of Public Health*, Vol. 104, No. 3, 2014.

Smucker, Sierra, Rose E. Kerber, and Philip J. Cook, "Suicide and Additional Homicides Associated with Intimate Partner Homicide: North Carolina 2004–2013," *Journal of Urban Health*, Vol. 95, No. 3, 2018.

Sorenson, Susan B., and Rebecca A. Schut, "Nonfatal Gun Use in Intimate Partner Violence: A Systematic Review of the Literature," *Trauma, Violence, and Abuse*, Vol. 19, No. 4, 2018.

Stolberg, Sheryl Gay, "Domestic Abusers Are Barred from Gun Ownership, but Often Escape the Law," *New York Times*, November 6, 2017.

Sullum, Jacob, "Colorado's New 'Red Flag' Law Illustrates the Pitfalls of Disarming People Based on Their Future Behaviors," Reason Foundation, April 29, 2019.

Thompson, Martie P., Linda E. Saltzman, and Daniel Bibel, "Applying NIBRS Data to the Study of Intimate Partner Violence: Massachusetts as a Case Study," *Journal of Quantitative Criminology*, Vol. 15, No. 2, June 1999.

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

Vigdor, E. R., and J. A. Mercy, "Do Laws Restricting Access to Firearms by Domestic Violence Offenders Prevent Intimate Partner Homicide?" *Evaluation Review*, Vol. 30, No. 3, 2006.

Vittes, Katherine A., Daniel W. Webster, Shannon Frattaroli, Barbara E. Claire, and Garen J. Wintemute, "Removing Guns from Batterers: Findings from a Pilot Survey of Domestic Violence Restraining Order Recipients in California," *Violence Against Women*, Vol. 19, No. 5, 2013.

Wallace, Maeve E., Dovile Vilda, Katherine P. Theall, and Charles Stoecker, "Firearm Relinquishment Laws Associated with Substantial Reduction in Homicide of Pregnant and Postpartum Women," *Health Affairs*, Vol. 40, No. 10, September 22, 2021.

Wallin, Mikaela A., Charvonne N. Holliday, and April M. Zeoli, "The Association of Federal and State-Level Firearm Restriction Policies with Intimate Partner Homicide: A Re-Analysis by Race of the Victim," *Journal of Interpersonal Violence*, Vol. 37, Nos. 17–18, September 2022.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Westreich, Daniel, and Sander Greenland, "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients," *American Journal of Epidemiology*, Vol. 177, No. 4, February 15, 2013.

Zeoli, April M., *Domestic Violence and Firearms: Research on Statutory Interventions*, Battered Women's Justice Project, 2018.

Zeoli, April M., Shannon Frattaroli, Kelly Roskam, and Anastasia K. Herrera, "Removing Firearms from Those Prohibited from Possession by Domestic Violence Restraining Orders: A Survey and Analysis of State Laws," *Trauma, Violence, and Abuse*, Vol. 20, No. 1, 2019.

Zeoli, April M., Alexander McCourt, Shani Buggs, Shannon Frattaroli, David Lilley, and Daniel W. Webster, "Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Associations with Intimate Partner Homicide," *American Journal of Epidemiology*, Vol. 187, No. 11, 2018.

Zeoli, April M., and Jennifer K. Paruk, "Potential to Prevent Mass Shootings Through Domestic Violence Firearm Restrictions," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Zeoli, A. M., and D. W. Webster, "Effects of Domestic Violence Policies, Alcohol Taxes and Police Staffing Levels on Intimate Partner Homicide in Large U.S. Cities," *Injury Prevention*, Vol. 16, No. 2, 2010.

CHAPTER 6

Surrender of Firearms by Prohibited Possessors

Federal law bans the sale of firearms to prohibited possessors, such as minors, illegal immigrants, convicted felons, and subjects of domestic violence restraining orders (DVROs) (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. In 2015, there were at least 19,000 prohibited firearm owners who had legally purchased their firearm in California, a figure used to extrapolate a conservative estimate of 100,000 firearm owners nationally who are prohibited possessors because of a felony conviction (Pear et al., 2021).

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 become 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 5, there is evidence that domestic violence offenders present an especially elevated risk of violence to their partners. For this reason, state firearm-surrender laws often focus on domestic violence offenders at the time they are convicted of such crimes.

Although firearm-surrender laws exist in several states, statutory provisions, implementation, and enforcement of such laws vary considerably. For example, state laws that require surrender or removal of firearms from persons under a DVRO vary along multiple dimensions, including whether relinquishment is mandatory, the time limit for relinquishment, whether proof of relinquishment is required, whether law enforcement remove guns when serving the order, the levying of noncompliance penalties, and whether the court may order search and seizure (Zeoli et al., 2019). Moreover, several studies highlight that enforcement of such laws, as written, is often lacking. North Carolina, for instance, requires judges to inquire about firearm ownership and availability in ex parte and full DVRO hearings, and judges must compel the defendant to surrender their firearms to a sheriff when they have made serious threats of violence. However, using a review of 406 such hearings between 2016 and 2017, Kafka et al. (2022) estimated that firearm availability was discussed in less than one-quarter of all cases and that firearm surrender was ordered in less than half of cases in which the

defendant was alleged to have threatened to kill the plaintiff. Even when firearms are ordered to be surrendered, there is often no clear procedure for enforcing such orders (Frattaroli, Zeoli, and Webster, 2021).

As another example, Rhode Island passed legislation in 2017 prohibiting all persons subject to a final DVRO from possessing firearms and requiring judges to issue a surrender order to all defendants who are subject to a final order of protection. An advocacy organization's analysis (Everytown for Gun Safety Support Fund, 2019a) of legal proceedings and court records found that surrender orders were issued to 5 percent of such defendants prior to enactment, which increased to 34 percent after the law was enacted. About one-third of defendants complied in the year following enactment, which increased to 63 percent during the second year that the law was in effect. Defendants are required by law to demonstrate relinquishment, but the courts do not have a system in place for monitoring those records or taking action against noncompliers.

In contrast, California cross-references firearm-surrender documentation against a database of all firearm purchasers and a list of prohibited persons, which is updated daily. The California Department of Justice uses the Armed and Prohibited Persons System (APPS) to identify prohibited persons who continue to possess firearms, locate the individuals, and deploy specialized agents to seize these individuals' firearms. In 2022, 9,917 individuals were removed from the prohibited persons list, 9,277 were added to the list, and 1,437 firearms were seized; as of January 1, 2023, the APPS system contained 23,869 armed and prohibited persons, more than half (51 percent) of whom were prohibited because of past felony convictions (California Department of Justice, 2022). The system has been criticized because of its backlogs (Zavala, 2023), but there is some evidence that the 2006 implementation of the APPS program was associated with a reduction in firearm homicides (estimated as between a 4- and 43-percent reduction; Ben-Michael et al., 2023). While suggestive of the potential benefits of proactive firearm removal strategies, this single-state analysis precludes disentangling estimated effects that are attributable to the firearm surrender component of the program from other aspects of the program (e.g., requirements to maintain records related to firearm sales and ownership [see Chapter 13]) or other contemporaneous changes specific to California in 2006.

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 other subgroups that were 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

According to the Giffords Law Center to Prevent Gun Violence (undated-k), as of January 1, 2024, eight states have laws requiring the surrender of firearms by prohibited possessors that extend beyond surrender requirements related to domestic violence incidents.[1] 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 DVROs to last for the duration of the order.[2] Of

[1] States requiring surrender are California, Connecticut, Hawaii, Illinois, Massachusetts, Nevada, New York, and Pennsylvania. See

- 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. § 29-36k. The individual has two business days to surrender firearms to the state or transfer them to an eligible individual. If the firearms are surrendered, the individual has one year to transfer them 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 the firearms are not surrendered, law enforcement may seize them. 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 the firearms in the location of or with the person reported on the disposition record.
- Mass. Gen. Laws Ch. 140 § 129B, 129D. Upon revocation of the 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 §§ 370.25. A judge shall order the immediate surrender of firearms, notify local police authorities and the division of state police, and direct the authority receiving the surrendered firearms to immediately notify the court of such surrender.
- 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.

[2] Alaska, Arizona, California, Colorado, Connecticut, Delaware, Florida, Hawaii, Illinois, Indiana, Iowa, Louisiana, Maryland, Massachusetts, Minnesota, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oregon, Pennsylvania, Rhode Island, South Dakota, Tennessee, Washington, and Wisconsin. See Alaska Stat. §§ 18.66.990(5), 18.66.100(c); Ariz. Rev. Stat. § 13-3602(G)(4); Calif. Fam. Code § 6389; Colo. Rev. Stat. § 13-14-105.5; Conn. Gen. Stat. § 29-36k(a); 11 Del. Code § 1448(a)(8); Fla. Stat. § 741.31(4)(a); Hawaii Rev. Stat. Ann. § 134-7(f); 725 Ill. Comp. Stat. 5/112A-3(3), 60/214(b)(14.5); Ind. Code Ann. § 34-26-5-9(c)(4); Ia. Code §§ 236.5(1)(b), 236.2(2), 724.26(4); La. Stat. Ann. Code of

those, 21 states require individuals convicted of domestic violence misdemeanors or specific domestic violence offenses to surrender their firearms.[3]

Effects on Suicide

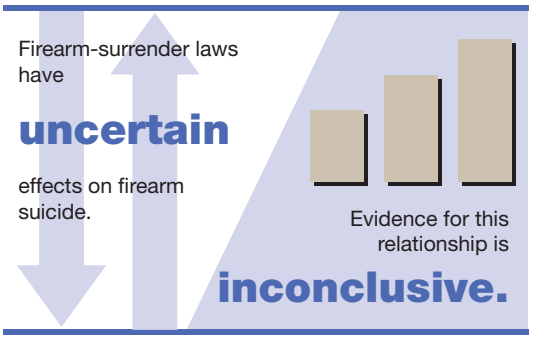
Research Synthesis Findings

We identified two new studies that evaluated the relationship of firearm-surrender laws with suicide (Kester et al., 2023; Liu, Siegel, and Sen, 2022). One of these studies (Kester et al., 2023) is subject to critical methodological concerns with interpreting estimates from this study's univariate analyses as causal effects, so this study is not discussed further.

Liu, Siegel, and Sen (2022) evaluated the relationship of firearm suicide rates with state laws requiring people to relinquish their firearms after they become prohibited from possessing them. Using data from the 48 contiguous states from 2000 to 2019, the authors used a linear spatial autoregressive model that predicted firearm suicide rates in a given state-year as a function of that state's firearm laws, other states' firearm laws, and other states' firearm suicide rates, weighting relationships with other state factors inversely to their geographic distance and controlling for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are unconventional because all states affect and are affected by all other states but with varying strength of such relationships. The authors calculated overall law effects by comparing the hypothetical scenario in which all states required firearm relinquishment with the hypothetical scenario in which no states did; the authors also decomposed total policy effects into the direct within-state effect of adopting the policy and the indirect interstate spillover effect. Their estimates showed a positive overall association between relinquishment laws and firearm suicide rates, the result of a very large positive interstate association more than offsetting the small nega-

Crim. Proc. Art. 1002; Md. Code, Fam. Law § 4-506; Mass. Gen. Laws Ch. 209A §3B; Minn. Stat. § 518B.01, subd. 6(g); Nev. Rev. Stat. Ann. §§ 33.031(1)(a), 33.033(2); N.H. Rev. Stat. Ann. § 173-B:5; N.J. Stat. Ann. §§ 2C:25-28(j), 2C:25-29(b); N.M. Stat. § 40-13-5(4); N.Y. Fam. Ct. Act § 842-a; N.C. Gen. Stat. Ann. § 50B-3.1; N.D. Cent. Code § 14-07.1-01.4; 2019 Ore. House Bill 2013; 23 Pa. Cons. Stat. Ann. § 6108; R.I. Gen. Laws §§ 8-8.1-3(a) (4), 15-15-3(a)(4); S.D. Codified Laws §§ 25-10-3.1(2), 25-10-24; Tenn. Code Ann. § 36-3-625(a); Wash. Rev. Code Ann. § 9.410.800; Wisc. Stat. §§ 813.1285(3), (4).

[3] California, Colorado, Connecticut, Hawaii, Illinois, Iowa, Louisiana, Maryland, Massachusetts, Minnesota, Nevada, New Hampshire, New Jersey, New York, Oregon, Pennsylvania, Rhode Island, Tennessee, Virginia, Washington, and Wisconsin. See Calif. Penal Code § 29810(a); Colo. Rev. Stat. §§ 18-1-1001(3)(c), 18-12-108(6)(c)(I), 18-6-801(8); Conn. Gen. Stat. § 29-36k(a); Hawaii Rev. Stat. Ann. § 134-7.3(b); 730 Ill. Comp. Stat. 5/5-6-3; Ia. Code Ann. § 724.26; La. Code Crim. P. Title XXXV, Art. 1001(A)(1), (2); 2018 Md. Chap. 251; Mass. Gen. Laws Ch. 140, §§ 129B, 129D; Minn. Stat. Ann. § 2242; Nev. Rev. Stat. Ann. § 202.361; N.H. Rev. Stat. Ann. §§ 173-B:4; N.J. Stat. Ann. §§ 2C:25-27; N.Y. Crim. Proc. Law § 370.25; 2019 Ore. HB 2013; 18 Pa. Cons. Stat. Ann. § 6105(a)(1)(i); R.I. Gen. Laws §§ 11-47-5.4, 12-29-5(d), (h); Tenn. Code Ann. § 39-13-111; Va. Code Ann.§ 18.2-308.1:4; Wash. Rev. Code Ann. §§ 10.99.040; Wisc. Stat. §§ 813.12.



tive within-state association. However, given that associations were identified from only three treated states with sufficient pre- and post-policy data, these results are subject to serious methodological concerns. Furthermore, this estimation strategy does not lend itself to producing effect estimates that can be interpreted as the average effect for a state transitioning between law regimes. Thus, this study is incomparable with other studies reviewed and we do not provide a figure plotting effect sizes from this study.

Conclusions

Excluding one study that is subject to critical methodological concerns, one qualifying study examined the relationship between firearm relinquishment requirements and suicide. Given the methodological limitations of this study, we find *inconclusive evidence for how laws requiring prohibited possessors to surrender firearms affect firearm suicide.*

Effects on Violent Crime

Research Synthesis Findings

We identified 11 studies that provide some evidence for the effects of firearm-surrender laws on violence, five of which (Chien and Gakh, 2020; Liu, Siegel, and Sen, 2022; Neufeld et al., 2022; Wallace et al., 2021; Wallin, Holliday, and Zeoli, 2022) contribute new findings to this updated review.[4] All but one of these studies focus on surrender laws related to domestic violence.

Six studies evaluated the effects of surrender laws related to domestic violence on intimate partner homicides. Four of these studies measured outcomes at the state-year level using the same data set, defined laws similarly, studied overlapping time periods, and used similar analytic methods: count models that control for state or region fixed effects, national trends (operationalized as year fixed effects or linear and quadratic trends), and time-varying state-level covariates related to nonintimate partner homicide rates, sociodemographic measures, and measures related to law enforcement. All four studies found that laws authorizing law enforcement officers to confiscate firearms at the scenes of alleged domestic violence inci-

[4] One study (Neufeld et al., 2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Because there is evidence that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study in the violent crime section.

dents had uncertain effects on total or firearm-related intimate partner homicide rates (Díez et al., 2017; Vigdor and Mercy, 2006; Zeoli et al., 2018; Wallin, Holliday, and Zeoli, 2022).[5] Another study analyzing city-level data from 46 cities also showed uncertain relationships between laws that allow police to confiscate firearms from a domestic violence incident and rates of intimate partner homicide (Zeoli and Webster, 2010). All of these studies were subject to minor methodological concerns related to a lack of evidence to support their models' identifying assumptions.

Rather than evaluating state laws, Raissian (2016) 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. That mechanism, requiring the defendant to attest to having disposed of all their firearms, is a relatively weak version of a surrender law. 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 could not disaggregate the separate effects of these provisions.

Instead of examining intimate partner homicides, Wallace et al. (2021) assessed how firearm relinquishment requirements affect *pregnancy-associated homicides*, defined as homicides with a decedent who is pregnant or within one year from the end of pregnancy at the time of her death. Using data from 2011 to 2019, they estimated a linear regression model, weighting by the number of live births in a given state-year, controlling for state fixed effects and year fixed effects, and considering a set of state-level time-varying covariates that reflect broader homicide rates, economic variables, and demographic variables. Their results indicated that when combined with firearm relinquishment requirements, DVROs and domestic violence misdemeanor prohibitions significantly reduced pregnancy-associated homicides. Unlike the aforementioned studies, Wallace et al. (2021) explicitly tested for whether domestic violence–based firearm prohibitions, when combined with firearm relinquishment requirements, had differential effects relative to prohibitions alone. The authors found that relative to prohibitions without relinquishment requirements, relinquishment requirements were associated with significant or suggestive reductions (of approximately 30 percent) in

[5] The study by Vigdor and Mercy (2006) also analyzed the effects of the laws allowing law enforcement officers to confiscate firearms at the scenes of alleged domestic violence incidents on other crimes that were not thought to be affected by confiscation laws (as a negative control). The authors found expected uncertain effects on rates of stranger homicide, rape, and robbery, but they found significant effects suggesting that confiscation laws may increase assault rates.

pregnancy-associated homicides for DVRO prohibitions and domestic violence misdemeanor prohibitions, respectively. However, the authors' preferred adjusted regression models had critical methodological concerns with respect to model overfit (fewer than one observation per five estimated parameters). While models that dropped all time-varying covariates had less potential for serious problems from model overfit, the exclusion of controls for these potential confounds without adequate justification or assessment of the validity of the parallel trends assumption required for unbiased estimates produces serious methodological concerns with the accuracy of these estimates and their inferential statistics.

Three additional studies focused on surrender laws related to domestic violence but evaluated effects on overall rates of homicide or firearm violence. Chien and Gakh (2020) estimated the association between the number of state laws related to domestic violence prohibitions (including surrender requirements) and state-level firearm homicide rates. To accommodate the potential for lagged law effects, they interacted the number of state laws with a natural cubic spline to allow a flexible phase-in of the effect of provisions lasting up to eight years. The authors used a Poisson quasi-maximum likelihood estimator to fit their model, which includes a penalized spline for time to account for national trends, random intercepts for states, state-level controls for demographics and socioeconomic factors, and controls for the violent crime rate and law enforcement officer per capita rate. The pattern of results is puzzling because they indicate that a high number of laws significantly increases firearm homicide rates up until about the six-year lag, when that relationship flips to become highly negative. Generating cumulative estimates over the eight years post–policy implementation shows that there are negative effects associated with the number of domestic violence prohibitions that reach statistical significance when the number of provisions is six or higher. However, failure to account for serial correlation renders these findings subject to serious methodological concerns.

Neufeld et al. (2022) used data from 2000 to 2016 to evaluate how requiring the surrender of firearms for individuals convicted of domestic violence misdemeanors or subject to DVROs influences firearm injury hospitalization rates. Using two-way fixed effects regression models that adjusted for several state-level time-varying covariates (demographic and socioeconomic characteristics, a proxy measure for household firearm ownership, violent crime rate, and per capita alcohol consumption) but no other state firearm laws, they found uncertain effects of these requirements on firearm injury hospitalizations. However, their analysis did not account for the uncertainty produced by the imputed nature of the outcome series (Carpenter and Kenward, 2012; Rubin, 1987), nor did it provide an assessment of the model's identifying assumption, raising minor concerns related to inferential validity. In an alternative specification, they found that laws allowing or requiring removal of firearms by law enforcement for these same offenders significantly reduced firearm injury hospitalization rates; however, these effects were based on data from law transitions in only two states, raising serious concerns about statistical power, generalizability to other contexts, and the ability to isolate the cause of any observed change to the passage of the law versus to other co-occurring factors. Knopov et al. (2019) evaluated the effects on Black and White citizens of state laws that either prohibit firearm ownership by those with a history of violent misde-

meanors or require firearm surrender by those subject to a DVRO. Using a log-linear model with two-way fixed effects; several social, economic, and crime covariates, including lagged firearm homicide rates; and several interaction terms, they estimated the effects of seven classes of laws on total and firearm homicides in 39 states between 1991 and 2016. Prohibitions associated with violent misdemeanors and DVRO firearm-surrender laws were significantly associated with reductions in total and firearm homicide rates among Black citizens, but no significant effects were found for White citizens.

Finally, one study (Liu, Siegel, and Sen, 2022) evaluated how broader relinquishment laws that require that individuals forfeit any firearms if they become prohibited from possessing them affect firearm homicides. Analyzing data from the 48 contiguous states from 2000 to 2019, the authors used a linear spatial autoregressive model that predicted firearm homicide rates in a given state-year as a function of that state's firearm laws, other states' firearm laws, and other states' firearm homicide rates, weighting relationships with other state factors inversely to their geographic distance and controlling for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are not intuitive because all states affect and are affected by all other states but with varying strength of that relationship. The authors calculated overall law effects by comparing the hypothetical scenario in which all states required firearm relinquishment with the hypothetical scenario in which no states did; they also decomposed total policy effects into the direct within-state effect of adopting the policy and the indirect interstate spillover effect. Their estimates showed uncertain overall, direct, and indirect associations between relinquishment laws and firearm homicides. However, given that associations were identified from only three treated states with sufficient pre- and post-policy data, these results are subject to serious methodological concerns.

Figure 6.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the firearm-surrender laws examined in these studies. Because the estimation strategy used by Liu, Siegel, and Sen (2022) does not lend itself to producing effect estimates that can be interpreted as the average effect for a state transitioning between law regimes, we do not plot effect sizes from this study. Similarly, we do not present results from Chien and Gakh (2020) because their manner of parameterizing law effects does not produce interpretable summary measures. Finally, Wallace et al. (2021) did not provide CIs or other exact inferential statistics that could be used to construct CIs, and thus their estimates are not presented.

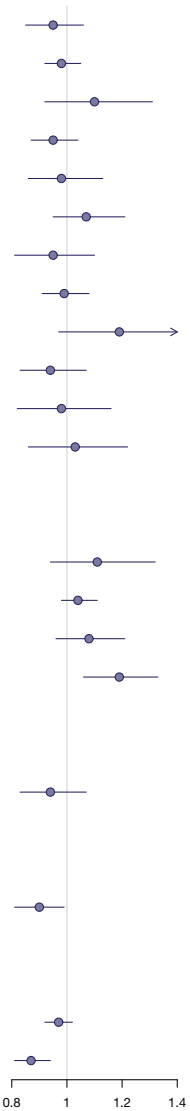
Conclusions

Effects of surrender laws alone. We identified five qualifying studies that examined the effects on rates of any violent crimes of laws requiring prohibited possessors to surrender firearms whereby surrender requirements were operationalized or tested as a distinct policy measure. 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

FIGURE 6.1

Incidence Rate Ratios Associated with the Effect of Firearm-Surrender Laws on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
State confiscation law	**IPH rate**	
Zeoli et al. (2018)	Total	0.95 [0.85, 1.06]
Díez et al. (2017)	Total	0.98 [0.92, 1.05]
Zeoli & Webster (2010)	Total	1.10 [0.92, 1.31]
Vigdor & Mercy (2006)	Total	0.95 [0.87, 1.04]
Wallin, Holliday, & Zeoli (2022)	Total, White	0.98 [0.86, 1.13]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.07 [0.95, 1.21]
Zeoli et al. (2018)	Firearm	0.95 [0.81, 1.10]
Díez et al. (2017)	Firearm	0.99 [0.91, 1.08]
Zeoli & Webster (2010)	Firearm	1.19 [0.97, 1.46]
Vigdor & Mercy (2006)	Firearm	0.94 [0.83, 1.07]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.98 [0.82, 1.16]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	1.03 [0.86, 1.22]
State confiscation law	**Violent crime rate**	
Vigdor & Mercy (2006)	Stranger homicide	1.11 [0.94, 1.32]
Vigdor & Mercy (2006)	Rape	1.04 [0.98, 1.11]
Vigdor & Mercy (2006)	Robbery	1.08 [0.96, 1.21]
Vigdor & Mercy (2006)	Assault	1.19 [1.06, 1.33]
State confiscation law	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm injury	0.94 [0.83, 1.07]
Confiscation law & DV misdemeanor prohibitions	**IPH rate**	
Raissian (2016)	Firearm	0.90 [0.81, 0.99]
Confiscation law & DV misdemeanor prohibitions	**Homicide rate**	
Knopov et al. (2019)	White	0.97 [0.92, 1.02]
Knopov et al. (2019)	Black	0.87 [0.81, 0.94]

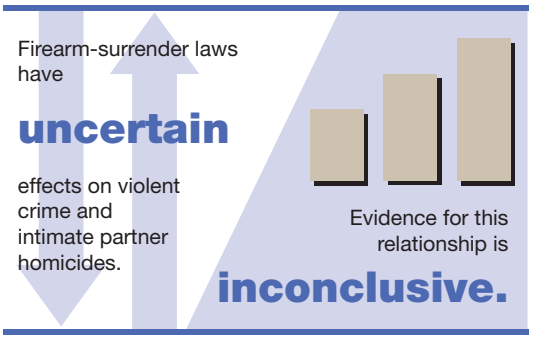


NOTE: The study model in Díez et al. (2017) includes a one-year lag of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no significant methodological concerns. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B and text for details. DV = domestic violence.

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. Liu, Siegel, and Sen (2022) found uncertain associations of relinquishment laws with firearm homicide rates.

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.*

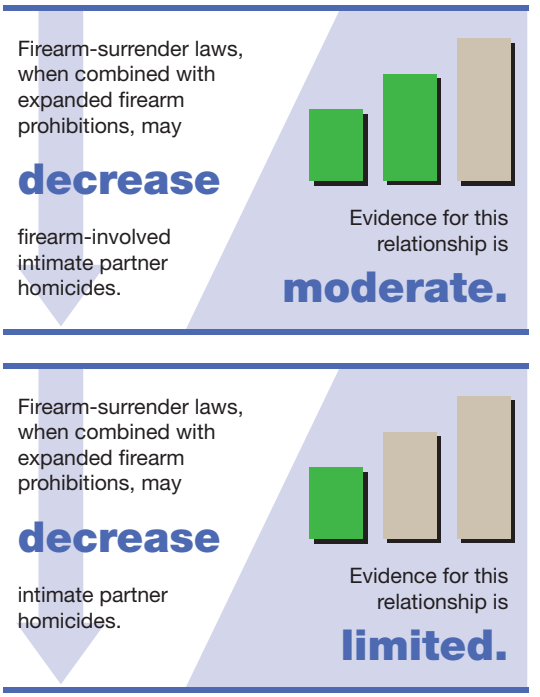
Effects of surrender laws related to or in combination with expanded prohibitions against firearm ownership on certain violence offenses. Seven studies evaluated a policy change that combined requiring a mechanism for ensuring firearm surrender and expanding the prohibited class to include additional violent misdemeanants. We treat these studies as providing evidence for that joint effect rather than as providing evidence for the effect of surrender per se. Specifically, Díez et al. (2017) found that prohibitions against firearm possession by subjects of DVROs in conjunction with firearm surrender significantly reduced both intimate partner homicides and firearm-involved intimate partner homicides. In an analysis stratified by decedent race, Wallin, Holliday, and Zeoli (2022) found that this relationship was significant for White decedents but uncertain for Black decedents. Raissian (2016) found that prohibitions on gun ownership by domestic violence misdemeanants combined with firearm surrender significantly reduced the firearm-related intimate partner homicide rate and gun deaths among family members more generally. Finally, Knopov et al. (2019) found uncertain effects of a class of laws that included firearm surrender laws and prohibitions on violent misdemeanants on homicides and firearm homicides of White state residents but significant effects associated with reductions in homicides of Black residents. Similarly, in an analysis of firearm hospitalization rates, Neufeld et al. (2022) found uncertain effects of laws requiring surrender of firearms for domestic violence offenders with misdemeanors or restraining orders. Chien and Gakh (2020) evaluated a policy index that was based on the number of domestic violence firearm regulations, including surrender requirements related to domestic violence, and found some evidence of a negative relationship with firearm homicide, but findings varied depending on the duration of the lag examined and the number of provisions in a state.



One study (Wallace et al., 2021) explicitly tested whether domestic violence–based firearm prohibitions combined with firearm relinquishment requirements had differential

effects relative to prohibitions alone. The authors found that, compared with prohibitions without relinquishment requirements, prohibitions with relinquishment requirements were associated with significant or suggestive reductions in pregnancy-associated homicides for DVRO prohibitions and domestic violence misdemeanor prohibitions, respectively.

Together, these studies suggest that *there is moderate evidence that firearm-surrender laws, when combined with prohibitions against gun possession by certain violent misdemeanants or subjects of DVROs, reduce firearm-involved intimate partner homicides* and that *there is limited evidence that they reduce all intimate partner homicides.*



Effects on Mass Shootings

Research Synthesis Findings

We identified two studies that met our inclusion criteria and examined the relationship between firearm-surrender laws and mass shootings, one of which (Siegel et al., 2020a) contributes new findings to this updated review. Webster et al. (2020) identified homicides between 1984 and 2017 that involved four or more firearm fatalities (excluding offender fatalities and drug- or gang-related shootings) using the Federal Bureau of Investigation's Supplementary Homicide Reports and two additional publicly available data sources. The authors estimated effects on mass shooting incidence and mass shooting fatalities, distinguishing between mass shootings that involved a domestic relationship and those that did not.[6] Among the 14 types of policies included in the study were four related to domestic violence, including laws requiring surrender of all firearms by subjects of DVROs. In negative binomial regression models that included all 14 laws, state fixed effects, linear and quadratic time trends, and numerous time-varying covariates (e.g., gun ownership proxy, socioeconomics, alcohol and

[6] The authors identify mass shootings based on the Supplementary Homicide Report classification of the offender's relationship with the first victim and including any family relationship, boyfriend or girlfriend, or ex-spouse. This classification produces potential errors because, for example, an incident in which an offender killed three members of their family and one stranger might be classified as a familicide or a mass public shooting depending on who the first victim listed in the Supplementary Homicide Report is.

drug consumption, demographics), the authors found uncertain effects of surrender requirements on mass shootings. As with most empirical studies of mass shootings, incidents and fatalities are quite sparsely represented in the data that Webster et al. (2020) used for these models. Incidents occurred at a rate of less than 0.4 per state-year, and fewer than two deaths occurred per state-year. Without adjustments for known biases in maximum likelihood estimates using sparse data, the resulting effect estimates and confidence intervals may be biased and unreliable (Kenne Pagui, Salvan, and Sartori, 2022).

Siegel et al. (2020a) examined a longer time frame—from 1976 to 2018—and instead focused on *mass public shootings*, which they defined as incidents with four or more victims fatally shot in a public location within a 24-hour period and unconnected to other criminal activity. They evaluated multiple firearm laws, including state relinquishment laws that require gun owners to surrender their firearms once the owners become prohibited from possessing them. They used a logistic regression model estimated via generalized estimating equations to assess the relationships of the laws with the odds that any mass public shooting would occur in a given state-year, controlling for linear and quadratic time trends, state population and population density, sociodemographics and economic measures, a proxy for firearm ownership, and broader death or crime rates. They estimated an uncertain relationship between relinquishment laws and mass public shooting occurrence. While Siegel et al. (2020a) examine a longer time frame than Webster et al. (2020), they use a narrower definition of mass shootings; thus, this study is also subject to concerns about sparse data and model overfit. While Siegel et al. (2020a) account for this by using a relatively parsimonious model specification, this leads to concerns that their covariates may not adequately control for all relevant confounds and, thus, estimated effects may not reflect the causal effects of policy.

Figure 6.2 displays the IRRs and CIs associated with the firearm-surrender laws examined in this study.

Conclusions

Two studies (Siegel et al., 2020a; Webster et al., 2020) met our inclusion criteria and evaluated how firearm-surrender laws or firearm-surrender laws associated with DVROs affect mass shootings. Both studies found uncertain effects for these laws on mass shooting incidents and fatalities. Considering these results, we find *inconclusive evidence for how laws requiring firearm surrender by prohibited possessors affect mass shootings.*

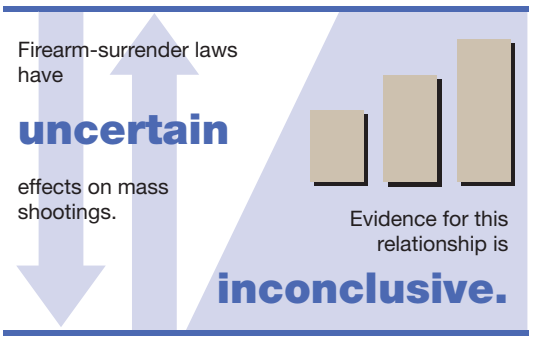
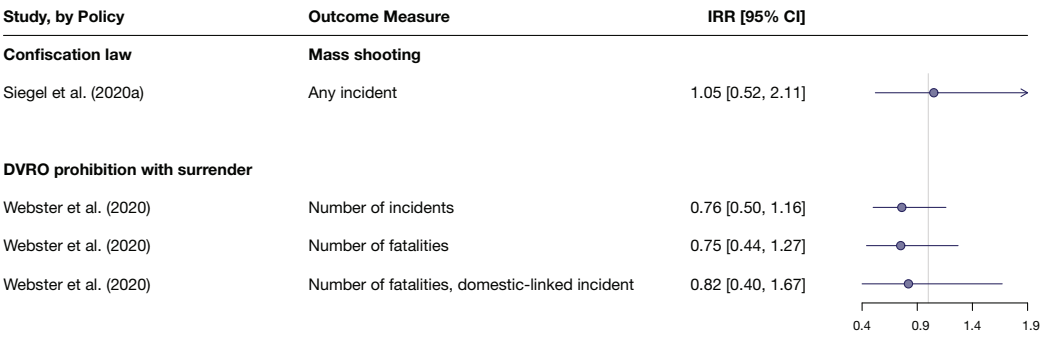


FIGURE 6.2

Incidence Rate Ratios Associated with the Effect of Firearm-Surrender Laws on Mass Shootings



NOTE: IRR values marked with filled circles indicate that we identified no serious methodological concerns. See Appendix B for details.

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
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 6 References

Ben-Michael, Eli, David Arbour, Avi Feller, Alexander Franks, and Steven Raphael, "Estimating the Effects of a California Gun Control Program with Multitask Gaussian Processes," *Annals of Applied Statistics*, Vol. 17, No. 2, June 2023.

California Department of Justice, "General Notice of Firearm Prohibition and Power of Attorney for Firearms Relinquishment, Sale, or Transfer for Storage," BOF 110, October 2015.

California Department of Justice, *Armed and Prohibited Persons System Report 2022: Annual Report to the Legislature*, SB 94 Legislative Report, 2022.

Carpenter, James R., and Michael G. Kenward, *Multiple Imputation and Its Application*, John Wiley and Sons, 2012.

Chien, L-C., and M. Gakh, "The Lagged Effect of State Gun Laws on the Reduction of State-Level Firearm Homicide Mortality in the United States from 1999 to 2017," *Public Health*, Vol. 189, December 2020.

Díez, Carolina, Rachel P. Kurland, Emily F. Rothman, Megan Bair-Merritt, Eric Fleegler, Ziming Xuan, Sandro Galea, Craig S. Ross, Bindu Kalesan, Kristin A. Goss, and Michael Siegel, "State Intimate Partner Violence–Related Firearm Laws and Intimate Partner Homicide Rates in the United States, 1991 to 2015," *Annals of Internal Medicine*, Vol. 167, No. 8, 2017.

Everytown for Gun Safety Support Fund, *Domestic Violence and Firearm Surrender in Rhode Island*, November 2019a.

Frattaroli, Shannon, April M. Zeoli, and Daniel W. Webster, "Armed, Prohibited and Violent at Home: Implementation and Enforcement of Restrictions on Gun Possession by Domestic Violence Offenders in Four U.S. Localities," *Journal of Family Violence*, Vol. 36, No. 5, 2021.

Giffords Law Center to Prevent Gun Violence, "Who Can Have a Gun: Firearm Relinquishment," webpage, undated-k. As of November 16, 2023: https://giffords.org/lawcenter/gun-laws/policy-areas/who-can-have-a-gun/firearm-relinquishment/

Kafka, Julie M., Kathryn E. Moracco, Deanna S. Williams, and Claire G. Hoffman, "Disarming Abusers: Domestic Violence Protective Order (DVPO) Firearm Restriction Processes and Dispositions," *Criminology and Public Policy*, Vol. 21, No. 2, May 2022.

Kenne Pagui, Euloge Clovis, Alessandra Salvan, and Nicola Sartori, "Improved Estimation in Negative Binomial Regression," *Statistics in Medicine*, Vol. 41, No. 13, 2022.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Pear, Veronica A., Christopher D. McCort, Yueju Li, Laurel Beckett, Daniel Tancredi, David M. Studdert, Philip H. Kass, Glenn L. Pierce, Anthony A. Braga, Mona A. Wright, Hannah S. Laqueur, Nicole Kravitz-Wirtz, and Garen J. Wintemute, "Armed and Prohibited: Characteristics of Unlawful Owners of Legally Purchased Firearms," *Injury Prevention*, Vol. 27, No. 2, 2021.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Raissian, Kerri M., "Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?" *Journal of Policy Analysis and Management*, Vol. 35, No. 1, Winter 2016.

Rubin, Donald B., *Multiple Imputation for Nonresponse in Surveys*, John Wiley and Sons, 1987.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Sorenson, Susan B., "Guns in Intimate Partner Violence: Comparing Incidents by Type of Weapon," *Journal of Women's Health*, Vol. 26, No. 3, 2017.

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

Vigdor, E. R., and J. A. Mercy, "Do Laws Restricting Access to Firearms by Domestic Violence Offenders Prevent Intimate Partner Homicide?" *Evaluation Review*, Vol. 30, No. 3, 2006.

Wallace, Maeve E., Dovile Vilda, Katherine P. Theall, and Charles Stoecker, "Firearm Relinquishment Laws Associated with Substantial Reduction in Homicide of Pregnant and Postpartum Women," *Health Affairs*, Vol. 40, No. 10, September 22, 2021.

Wallin, Mikaela A., Charvonne N. Holliday, and April M. Zeoli, "The Association of Federal and State-Level Firearm Restriction Policies with Intimate Partner Homicide: A Re-Analysis by Race of the Victim," *Journal of Interpersonal Violence*, Vol. 37, Nos. 17–18, September 2022.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Zavala, Ashley, "California's Armed and Prohibited Persons List Sees Slight Decrease in Backlog," KCRA, April 3, 2023.

Zeoli, April M., Shannon Frattaroli, Kelly Roskam, and Anastasia K. Herrera, "Removing Firearms from Those Prohibited from Possession by Domestic Violence Restraining Orders: A Survey and Analysis of State Laws," *Trauma, Violence, and Abuse*, Vol. 20, No. 1, 2019.

Zeoli, April M., Alexander McCourt, Shani Buggs, Shannon Frattaroli, David Lilley, and Daniel W. Webster, "Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Associations with Intimate Partner Homicide," *American Journal of Epidemiology*, Vol. 187, No. 11, 2018.

Zeoli, A. M., and D. W. Webster, "Effects of Domestic Violence Policies, Alcohol Taxes and Police Staffing Levels on Intimate Partner Homicide in Large U.S. Cities," *Injury Prevention*, Vol. 16, No. 2, 2010.

CHAPTER 7

Extreme-Risk Protection Orders

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 the 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 6) 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.

Although there are no federal ERPO laws, the 2022 Bipartisan Safer Communities Act set aside $750 million to support state and local implementation of ERPOs; the U.S. Department of Justice had previously published model ERPO legislation (U.S. Department of Justice, 2021). Currently, 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 often ranges from six to 12 months (Johns Hopkins Center for Gun Violence Solutions, 2023), 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 5), 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). For individuals who do attend the assessment hearing, concerns have been raised about whether they receive adequate legal representation (Kopel, 2019; Richardson, 2021). A study of California ERPO cases found that approximately one-half of cases resulted in a one-year order of protection after a hearing (in large part because longer orders were not sought in one-third of cases), and only 18.3 percent of respondents with a hearing had legal representation (Pear et al., 2022a).

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 themselves 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 they know about the firearms and ammunition that the respondent possesses. If a respondent does not return firearms in their 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 another 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, although the number of petitions varies greatly within states over time. California had 71 ERPO respondents in 2016 (the year legislation was passed), which increased to 717 by 2019 (Pallin et al., 2020). In King County, Washington, 146 petitions were served in 2018, up from 40 in 2017 (Rowhani-Rahbar et al., 2020); 93 petitions were filed in the first 15 months of Oregon's 2018 ERPO law (Zeoli et al., 2021); and 110 petitions were granted in the first year of Colorado's 2020 ERPO law (Barnard et al., 2021). 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). 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); more-recent data suggest that about 400 ERPOs are granted per year in Maryland (Engel, 2022). In 2022, four years after Florida enacted an ERPO law, more than 2,800 people were under risk protection orders in the state (Contorno, Santiago, and Royal, 2022), and almost 9,000 such orders had been imposed since the passage of the law (MacFarlane, 2022). Within the first two years (2019 to 2021) of New Jersey's ERPO implementation, petitioners filed 760 ERPO petitions, resulting in the granting of 664 temporary ERPOs (325 of which became permanent or final ERPOs; Bruck, 2021). Consequently, the effects of ERPO laws on state outcomes are likely to be quite variable, depending on how widely used these emergency orders are. 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.

Although high-profile homicide incidents have precipitated proposals for and the passage of ERPOs in several states (Kapoor et al., 2018; Pallin et al., 2021; Swanson et al., 2017), available data suggest that petitions for ERPOs often 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). However, ERPO case reviews in other states (e.g., Washington, Colorado, and California) find higher prevalence of other-directed harm versus self-directed harm (Barnard et al., 2021; Pear et al., 2022; Rooney et al., 2021; Rowhani-Rahbar et al., 2020). 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). A more recent study of ERPO petitions from six states found that 10 percent of such cases (662 of 6,787) involved threats of killing three or more people. Petitions concerning threats to multiple victims ranged from 3 percent of all petitions

(46 of 1,406) in Connecticut to 13.2 percent (139 of 1,057) in California. Temporary orders were granted in more than 90 percent of these cases, and a large majority had these orders extended after a full hearing (Zeoli et al., 2022).

Nuances of the policy could influence the likelihood that the ERPOs affect rates of homicide and suicide with firearms. 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. Likewise, 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. While some laws extend that right to family members and medical professionals, studies suggest that law enforcement in these states are still responsible for the majority of petitions (Rooney et al., 2021; Rowhani-Rahbar et al., 2020; Zeoli et al., 2022), and there are noted barriers to civilian petitions, including time costs, administrative and legal burdens, and lack of knowledge (Hollo, VanderStoep, and Frattaroli, 2022; Prater et al., 2022). These barriers may mitigate the effects of expanding the ability to request an ERPO to family members and medical professionals, although if these other petitioners are more knowledgeable of the individual's high-risk behavior, we might also expect them to generate more-effective ERPO petitions.

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 police shootings are unclear. The laws could decrease police 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 a confrontation with police. 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, when the authors applied information on suicide case fatality rates by method and assumed 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, they 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, 2024, 19 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.[2] Of those, 12 states allow individuals other than law enforcement officers to file a petition for the ERPO.[3] Some states have expanded the class of potential petitioners only slightly; for instance, California allows immediate family members to file ERPO petitions.[4] Maryland, on the other hand, allows petitions to be filed by a variety of medical and mental health professionals,[5] spouses and cohabitants, other family members, coparents, current

[2] California, Colorado, Connecticut, Delaware, Florida, Hawaii, Illinois, Indiana, Maryland, Massachusetts, Nevada, New Jersey, New Mexico, New York, Oregon, Rhode Island, Vermont, Virginia, Washington, and the District of Columbia. See Calif. Penal Code § 18150; Colo. Rev. Stat. § 13-14.5-103; Conn. Gen. Stat. § 29-38c; Del. Code Ann. Tit. 10, §§ 7701, 7704; Fla. Stat. § 790.401(3)(a); Hawaii Rev. Stat. Ann. § 134-C; 430 Ill. Comp. Stat. 67/35, 67/40; Ind. Code Ann. § 35-47-14-2; Md. Code Ann., Pub. Safety § 5-601(E)(2); Mass. Gen. Laws Ch. 140 § 131R; Nev. Rev. Stat. Ann. § 33.580; N.J. Stat. Ann. § § 2C:58-21, 23; 2019 N.M. H.B. 83 (NS) 6(a)(b), 10; N.Y. CPLR §§ 6340, 6341; Ore. Rev. Stat. Ann. § 166.527; R.I. Gen. Laws § 8-8.3-4; Vt. Stat. Ann. Tit. 13, § 4053; Va. Code § 18.2-308.1:6; Wash. Rev. Code §§ 7.94.030(1), 7.94.020(2); D.C. Stat. § 7-2510.04.

[3] California, Colorado, Delaware, Hawaii, Illinois, Maryland, Massachusetts, Nevada, New Jersey, New York, Oregon, and Washington. See Calif. Penal Code § 18150; Colo. Rev. Stat. § 13-14.5-104; Del. Code Ann. Tit. 10, § 7701, 7704 (family and household members can petition for non-emergency orders); Hawaii Rev. Stat. Ann. § 134-C; 430 Ill. Comp. Stat. 67/35, 67/40; Md. Code Ann., Pub. Safety § 5-601(E)(2); Mass. Gen. Laws Ch. 140, §§ 121, 131R; Nev. Rev. Stat. Ann. § 33.580; N.J. Stat. Ann. § 2C:58-21; N.Y. CPLR § § 6340, 6341; Ore. Rev. Stat. Ann. §§ 166.527; Wash. Rev. Code § 7.94.030(1).

[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)).

dating partners, and current or former legal guardians.[6] In addition, 16 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.[7] With regard to the ex parte ERPOs, some states allow these only when the petitioner is a law enforcement officer,[8] while others allow ex parte ERPOs to be filed by an expanded group of petitioners.[9] Most final ERPOs last one year.[10] 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.[11]

Effects on Suicide

Research Synthesis Findings

We identified five studies that met our inclusion criteria and assessed the effects of ERPOs on suicide, three of which contribute new findings to this updated review (Dalafave, 2021; Kester et al., 2023; Pear et al., 2022a). Two studies were rated as having critical methodological concerns and are not discussed further.[12]

---

[6] Md. Code Ann., Pub. Safety § 5-601(E)(2).

[7] California, Colorado, Delaware, Florida, Hawaii, Illinois, Maryland, Massachusetts, Nevada, New Jersey, New Mexico, New York, Oregon, Rhode Island, Vermont, Washington, and the District of Columbia. See Calif. Pen. Code § 18150(b); Colo. Rev. Stat. § 13-14.5-103; Del. Code Ann. Tit. 10, § 7703; Fla. Stat. § 790.401(4)(a); Hawaii Rev. Stat. Ann. § 134-C; 430 Ill. Comp. Stat. 67/35; Md. Code Ann., Pub. Safety § 5-603; Mass. Gen. Laws Ch. 140, §§ 121, 131R, 131S, 131T; Nev. Rev. Stat. Ann. § 33.580; N.J. Stat. Ann. §§ 2C:58-21, 23; 2019 N.M. H.B. 83 (NS) 6(a)(b), 10; N.Y. CPLR §§ 6340-6342; Ore. Rev. Stat. Ann. §§ 166.525, 166.527; R.I. Gen. Laws § 8-8.3-4; Vt. Stat. Ann. Tit. 13, § 4054(a)(1); Wash. Rev. Code § 7.94.050; D.C. Stat. § 7-2510.04.

[8] Delaware, Florida, Rhode Island, and Vermont. See Del. Code Ann. Tit. 10, § 7703; Fla. Stat. § 790.401(4)(a); R.I. Gen. Laws § 8-8.3-4; Vt. Stat. Ann. Tit. 13, § 4054(a)(1).

[9] Hawaii, Illinois, Maryland, Massachusetts, New Jersey, New York, Oregon, and Washington. See Hawaii Rev. Stat. Ann. § 134-C; 430 Ill. Comp. Stat. 67/5, 67/35; Md. Code Ann., Pub. Safety §§ 5-601(E)(2), 5-602, 5-603; Mass. Gen. Laws Ch. 140, §§ 121, 131R, 131S, 131T; N.J. Stat. Ann. § 2C:58-21; N.Y. CPLR §§ 6340-6342; Ore. Rev. Stat. Ann. §§ 166.525, 166.527; Wash. Rev. Code § 7.94.050.

[10] Exceptions are Illinois (six months), Vermont (six months), and New Jersey (indefinite). See 430 Ill. Comp. Stat. 67/35; 13 Vt. Stat. Ann. § 4053; N.J. Stat. Ann. § 2C:58-25.

[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] Kester et al. (2023) did not control for secular trends, prompting critical methodological concerns related to causal identification. The analysis by Saadi et al. (2020) had critical concerns related to potential model

Using data from 1990 to 2018, Dalafave (2021) estimated log-linear two-way fixed-effects regression models that controlled for various demographic and economic covariates, as well as state unintentional poisoning death rates and state unintentional firearm death rates, to compare changes in state-level suicide rates after state ERPO adoption with changes in states without such laws. The author found that ERPO laws produced significant reductions in total suicides and firearm suicides; effect sizes corresponded to reductions of approximately 4 percent and 6 percent, respectively (see Figure 7.1). While the author's approach dropped state-year observations with suppressed mortality data (i.e., state-year observations with fewer than ten firearm suicides were dropped), which could bias the estimates, this appears to have affected only eight of the 1,479 observations (about 0.5 percent) and thus is considered a minor methodological concern.

Two other studies used synthetic control approaches to examine effects of ERPO laws in particular states. Kivisto and Phalen (2018) estimated the effects on suicide 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.[13] 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 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.

Finally, Pear et al. (2022a) evaluated the effects of San Diego County's implementation of California's ERPO law (known in the state as its *Gun Violence Restraining Order law*) on annual rates of firearm self-harm compared with other California counties using a combination of hospitalization and mortality data. While California's ERPO legislation was enacted statewide, San Diego City Attorney Mara Elliott acted as a particularly vocal proponent of

overfit (i.e., fewer than five observations per estimated parameter). One other study did not meet our prespecified methodological exclusion criteria. A 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 2.

the law, and San Diego County has accounted for a disproportionate share of ERPOs in the state (Pallin et al., 2020). Pear et al. (2022a) effectively dichotomized this differential "dosage" of ERPOs to use the synthetic control methods to estimate the effects of California's ERPO law in San Diego County as compared with other California counties that issued very few ERPOs from 2016 to 2019. The authors' approach produced uncertain estimates of the effects of ERPO implementation in San Diego on intentional firearm self-harm injuries. However, their analysis of a single county's implementation 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.

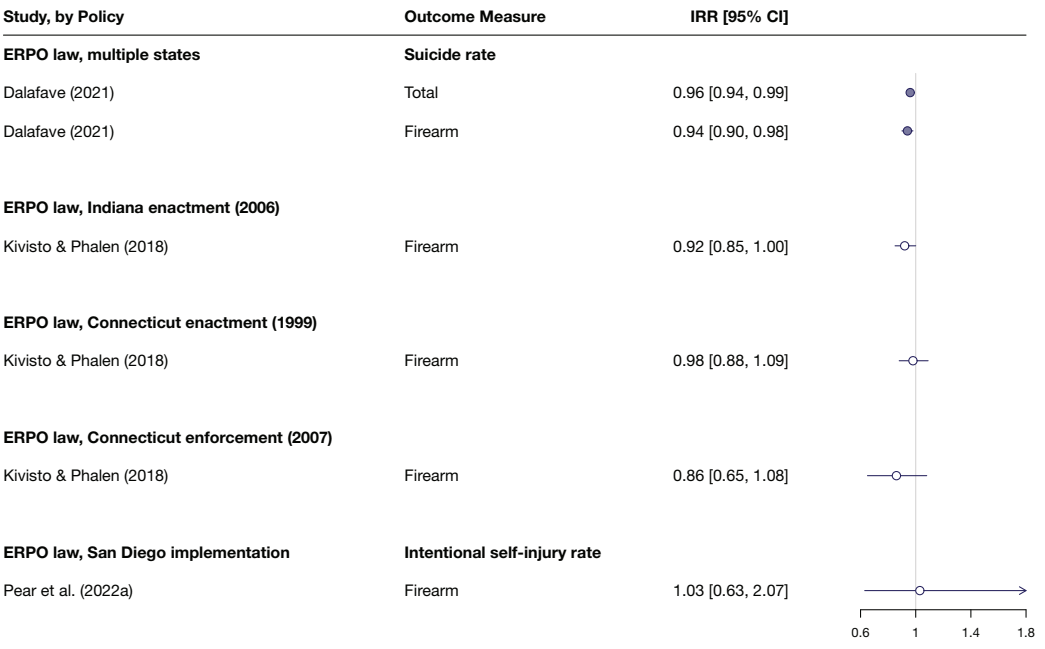
Figure 7.1 displays the incidence rate ratios (IRRs) and CIs associated with ERPOs as estimated by Dalafave (2021), Kivisto and Phalen (2018), and Pear et al. (2022a).

Conclusions

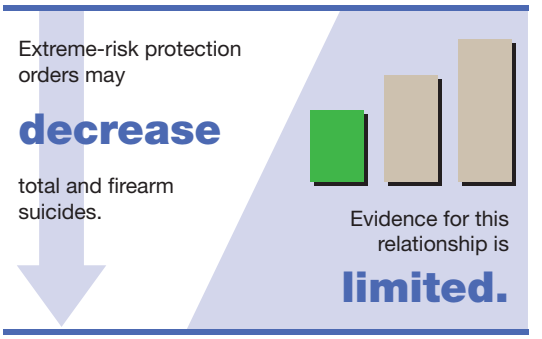
Excluding studies with critical methodological concerns, three studies examined the effects of ERPOs on suicide rates. All found uncertain effects or effects consistent with decreases in suicide rates. Kivisto and Phalen (2018) found a suggestive reduction in firearm suicide

FIGURE 7.1

Incidence Rate Ratios Associated with the Effect of Extreme-Risk Protection Orders on Suicide



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.



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. Pear et al. (2022a) found uncertain effects of San Diego County's ERPO implementation relative to other California counties where very few orders were issued. Finally, averaging across multiple states, Dalafave (2021) found that ERPO laws were associated with significant reductions in total and firearm suicide rates. In all, we find *limited evidence that extreme-risk protection orders reduce total and firearm suicides.*

Effects on Violent Crime

Research Synthesis Findings

We identified four studies meeting our inclusion criteria that assessed the effects of ERPOs on homicide, three of which contribute new findings to this updated review (Dalafave, 2021; Pear et al., 2022a; Scantling et al., 2022). Two studies were rated as having critical methodological concerns and are not discussed further.[14]

Using data from 1990 to 2018, Dalafave (2021) estimated log-linear two-way fixed-effects regression models that control for various demographic and economic covariates, as well as state unintentional poisoning death rates and state unintentional firearm death rates, to compare changes in state-level homicide rates after state ERPO adoption with changes in states without such laws. The author found that ERPO laws had uncertain effects on total homicides and firearm homicides. However, the authors' approach dropped state-year observations with suppressed mortality data (i.e., state-year observations with fewer than ten homicides were dropped), which could bias effect estimates, and this decision affected nearly 10 percent of observations, so this is considered a serious methodological concern.

[14] Gius (2020b) assessed the significance of effects by interpreting a post-treatment t-test for the difference between the mean firearm-related murder rate in a given state post-ERPO adoption and the corresponding mean firearm-related murder rate in the synthetic control. This treats each year of post-policy data from a given state as independent, does not account for the fact that the weights assigned to each unit in the synthetic control are themselves estimated, and does not account for problems with pre-policy fit, leading to critical methodological concerns (however, see Chernozhukov, Wuthrich, and Zhu [2022] for a proposed self-normalized t-test method for synthetic controls). Scantling et al. (2022) had critical concerns related to potential model overfit (i.e., fewer than five observations per estimated parameter).

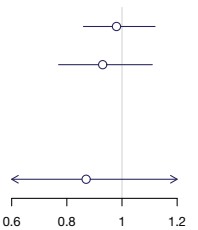
Using data from 2005 to 2016, Pear et al. (2022a) evaluated the effects of San Diego's implementation of California's ERPO law on annual county-level rates of firearm assault as measured through a combination of hospitalization and mortality data. While California's ERPO legislation was enacted statewide, San Diego City Attorney Mara Elliott acted as a particularly vocal proponent of the law, and San Diego County has accounted for a disproportionate share of the use of ERPOs in the state (Pallin et al., 2020). Pear et al. (2022a) effectively dichotomized this differential "dosage" of ERPOs to use the synthetic control method approach to estimate the effects of California's ERPO law in San Diego County as compared with other California counties that issued very few ERPOs after law passage. The authors' approach produced uncertain estimates of the effects of ERPO implementation in San Diego on intentional firearm assault injuries. However, their analysis of a single county's implementation 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 7.2 displays the IRRs and CIs associated with ERPOs as estimated by Dalafave (2021) and Pear et al. (2022a).

FIGURE 7.2

Incidence Rate Ratios Associated with the Effect of Extreme-Risk Protection Orders on Violent Crime

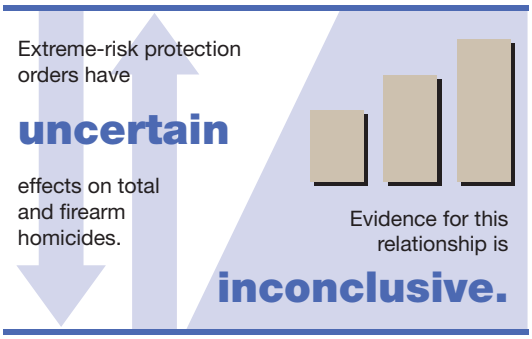
Study, by Policy	Outcome Measure	IRR [95% CI]
ERPO law, multiple states	**Homicide rate**	
Dalafave (2021)	Total	0.98 [0.86, 1.12]
Dalafave (2021)	Firearm	0.93 [0.77, 1.11]
ERPO law, San Diego implementation	**Assault rate**	
Pear et al. (2022a)	Firearm	0.87 [0.10, 2.25]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

Conclusions

Excluding studies with critical methodological concerns, two qualifying studies examined the effects of ERPOs on homicide or firearm assault rates. Both found uncertain effects; thus, we find *inconclusive evidence for the effect of extreme-risk protection orders on total and firearm homicides.*



Effects on Unintentional Injuries and Deaths

Research Synthesis Findings

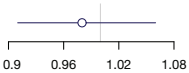
We identified one study that met our inclusion criteria and assessed the effects of ERPOs on unintentional firearm deaths. Using data from 1990 to 2018, Dalafave (2021) estimated log-linear two-way fixed-effects regression models that controlled for various demographic and economic covariates, as well as state unintentional poisoning death rates, to compare changes in state-level unintentional firearm injury death rates after state ERPO adoption with changes in states without such laws. The author found that ERPO laws had uncertain effects on unintentional firearm injury death rates. However, the purpose of this analysis was to assess whether unintentional firearm injury death rates could be included as a control variable in the study's primary models (of homicide and suicide outcomes). Combined with the fact that the author dropped a large number of observations with suppressed values (i.e., state-years with fewer than ten deaths), which can bias effect estimates, this result is subject to serious methodological concerns.

Figure 7.3 displays the IRR and CIs associated with ERPOs as estimated by Dalafave (2021).

FIGURE 7.3

Incidence Rate Ratio Associated with the Effect of Extreme-Risk Protection Orders on Unintentional Firearm Deaths

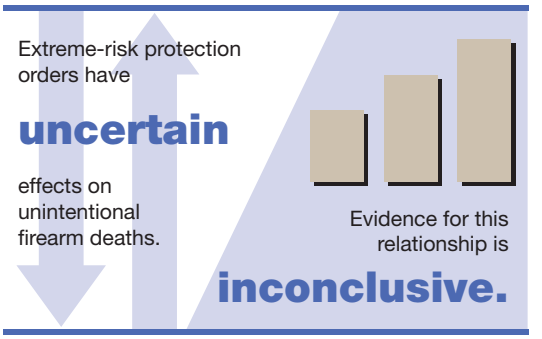
Study, by Policy	Outcome Measure	IRR [95% CI]
ERPO law	**Unintentional injury rate**	
Dalafave (2021)	Firearm	0.98 [0.91, 1.06]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

Conclusions

One study found uncertain effects of ERPOs on unintentional firearm deaths. We thus find *inconclusive evidence for the effect of extreme-risk protection orders on unintentional firearm deaths.*



Effects on Mass Shootings

Research Synthesis Findings

We identified one study in this updated review that met our inclusion criteria and assessed the effects of ERPOs on mass shooting outcomes. Using a logistic regression, Siegel et al. (2020a) examined the effects of state ERPO laws on the occurrence of any *mass public shooting incident*, defined as an incident unconnected to other criminal activity in which four or more victims were fatally shot in a public location. Multiple sources of data were aggregated to obtain mass public shooting incidence information for the years 1976 to 2018. Regression models controlled for linear and quadratic national time trends, as well as 13 time-varying state-level characteristics (e.g., population, demographic composition, a proxy for state firearm ownership rates, age-adjusted firearm homicide rate, and several other firearm laws). Estimates from logistic regressions showed that ERPO laws had uncertain associations with mass public shooting incidents; similar findings were shown in zero-inflated negative binomial models. A serious methodological concern arises from the authors' reliance on only two treated states, which makes it hard to attribute observed changes to the passage of these laws rather than other contemporaneous changes, and there are minor concerns related to having fewer than ten observations per parameter and the absence of discussion around identifying assumptions (e.g., the extent to which estimated effects reflect across-state versus within-state variation).

Figure 7.4 displays the IRR and CIs associated with ERPOs as estimated by Siegel et al. (2020a).

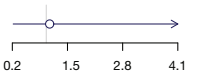
Conclusions

One study found uncertain effects of ERPOs on the likelihood of a mass shooting occurring. We thus find *inconclusive evidence for the effects of extreme-risk protection orders on mass shootings.*

FIGURE 7.4

Incidence Rate Ratio Associated with the Effect of Extreme-Risk Protection Orders on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
ERPO law	**Mass public shooting**	
Siegel et al. (2020a)	Any incident	1.08 [0.22, 5.19]

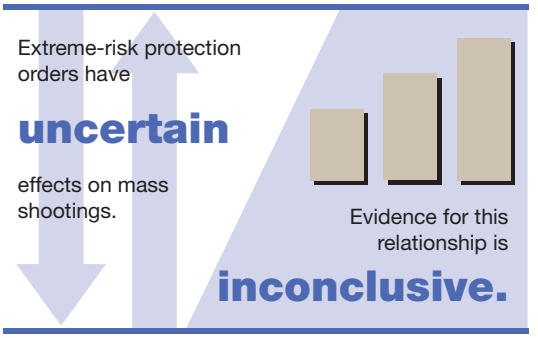


NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

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:

- police shootings
- defensive gun use
- hunting and recreation
- gun industry.



Chapter 7 References

Barnard, Leslie M., Megan McCarthy, Christopher E. Knoepke, Sabrina Kaplan, James Engeln, and Marian E. Betz, "Colorado's First Year of Extreme Risk Protection Orders," *Injury Epidemiology*, Vol. 8, No. 59, 2021.

Bruck, Andrew J., "Acting AG Bruck: More Than 300 'Extreme Risk Protective Orders' Issued in New Jersey Since Landmark Gun Safety Law Went into Effect Two Years Ago," Office of the Attorney General, State of New Jersey, September 1, 2021.

Chernozhukov, Victor, Kaspar Wuthrich, and Yinchu Zhu, "A T-Test for Synthetic Controls," arXiv, arXiv:1812.10820, version 7, July 6, 2022. As of January 8, 2024: https://arxiv.org/abs/1812.10820

Contorno, Steve, Leyla Santiago, and Denise Royal, "Florida's Red Flag Law, Championed by Republicans, Is Taking Guns from Thousands of People," CNN Politics, June 1, 2022.

Dalafave, Rachel, "An Empirical Assessment of Homicide and Suicide Outcomes with Red Flag Laws," *Loyola University Chicago Law Journal*, Vol. 52, No. 3, 2021.

Engel, Amanda, "Just Under 50% of Emergency Risk Protection Order Requests Are Granted: Digging into Maryland Gun Protection Laws," ABC Baltimore, May 26, 2022.

Gius, Mark, "Using the Synthetic Control Method to Determine the Effects of Firearm Seizure Laws on State-Level Murder Rates," *Applied Economics Letters*, Vol. 27, No. 21, 2020b.

Hollo, Ashley, Amy VanderStoep, and Shannon Frattaroli, "Physicians' Perspectives on Extreme Risk Protection Orders (ERPOs) in the Clinical Setting: Challenges and Opportunities for Gun Violence Prevention," *PLoS One*, Vol. 17, No. 9, September 13, 2022.

Johns Hopkins Center for Gun Violence Solutions, "Extreme Risk Protection Orders: State Laws at a Glance," Johns Hopkins Bloomberg School of Public Health, August 1, 2023.

Kapoor, Reena, Elissa Benedek, Richard J. Bonnie, Tanuja Gandhi, Liza Gold, Seth Judd, and Debra A. Pinals, "APA Resource Document on Risk-Based Gun Removal Laws," *Developments in Mental Health Law*, Vol. 37, No. 2, 2018.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Kivisto, Aaron J., and Peter L. Phalen, "Effects of Risk-Based Firearm Seizure Laws in Connecticut and Indiana on Suicide Rates, 1981–2015," *Psychiatric Services*, Vol. 69, No. 8, 2018.

Kochanek, Kenneth D., Sherry L. Murphy, Jiaquan Xu, and Elizabeth Arias, "Deaths: Final Data for 2017," *National Vital Statistics Reports*, Vol. 68, No. 9, June 24, 2019.

Kopel, David B., "Red Flag Laws: Examining Guidelines for State Action," testimony before the U.S. Senate Judiciary Committee, March 26, 2019.

MacFarlane, Scott, "Red Flag Laws Found in Florida Work When Used Correctly, Data Show," Wink, June 7, 2022.

Pallin, Rocco, Amanda J. Aubel, Christopher E. Knoepke, Veronica A. Pear, Garen J. Wintemute, and Nicole Kravitz-Wirtz, "News Media Coverage of Extreme Risk Protection Order Policies Surrounding the Parkland Shooting: A Mixed-Methods Analysis," *BMC Public Health*, Vol. 21, No. 1, 2021.

Pallin, Rocco, Julia P. Schleimer, Veronica A. Pear, and Garen J. Wintemute, "Assessment of Extreme Risk Protection Order Use in California from 2016 to 2019," *JAMA Network Open*, Vol. 3, No. 6, 2020.

Parker, George F., "Circumstances and Outcomes of a Firearm Seizure Law: Marion County, Indiana, 2006–2013," *Behavioral Sciences and the Law*, Vol. 33, Nos. 2–3, 2015.

Pear, Veronica A., Rocco Pallin, Julia P. Schleimer, Elizabeth Tomsich, Nicole Kravitz-Wirtz, Aaron B. Shev, Christopher E. Knoepke, and Garen J. Wintemute, "Gun Violence Restraining Orders in California, 2016–2018: Case Details and Respondent Mortality," *Injury Prevention*, Vol. 28, 2022.

Pear, Veronica A., Garen J. Wintemute, Nicholas P. Jewell, and Jennifer Ahern, "Firearm Violence Following the Implementation of California's Gun Violence Restraining Order Law," *JAMA Network Open*, Vol. 5, No. 4, 2022a.

Prater, Laura, Lauren Rooney, Andrew G. Bowen, Kelsey Conrick, Ayah Mustafa, Megan Moore, Frederick P. Rivara, and Ali Rowhani-Rahbar, "Civilian Petitioners and Extreme Risk Protection Orders in the State of Washington," *Psychiatric Services*, Vol. 73, No. 11, November 1, 2022.

Richardson, John R., "Red Flag Laws and Procedural Due Process: Analyzing Proposed Utah Legislation," *Utah Law Review*, Vol. 2021, No. 3, 2021.

Rooney, Lauren, Kelsey M. Conrick, M. Alex Bellenger, Megan Moore, Miriam J. Haviland, Emma Gause, Frederick P. Rivara, and Ali Rowhani-Rahba, "Understanding the Process, Context, and Characteristics of Extreme Risk Protection Orders: A Statewide Study," *Journal of Health Care for the Poor and Underserved*, Vol. 32, No. 4, 2021.

Roskam, Kelly, and Vicka Chaplin, "The Gun Violence Restraining Order: An Opportunity for Common Ground in the Gun Violence Debate," *Developments in Mental Health Law*, Vol. 36, No. 2, 2017.

Rowhani-Rahbar, Ali, M. Alex Bellenger, Lauren Gibb, Heather Chesnut, Madison Lowry-Schiller, Emma Gause, Miriam J. Haviland, and Frederick P. Rivara, "Extreme Risk Protection Orders in Washington: A Statewide Descriptive Study," *Annals of Internal Medicine*, Vol. 173, No. 5, September 2020.

Saadi, Altaf, Kristen R. Choi, Sae Takada, and Fred J. Zimmerman, "The Impact of Gun Violence Restraining Order Laws in the U.S. and Firearm Suicide Among Older Adults: A Longitudinal State-Level Analysis, 2012–2016," *BMC Public Health*, Vol. 20, No. 334, 2020.

Scantling, Dane R., Daniel N. Holena, Elinore J. Kaufman, Allyson M. Hynes, Justin Hatchimonji, James P. Byrne, Douglas Wiebe, and Mark J. Seamon, "Modifiable Factors Related to Firearm Homicides: A Broader View of Our Lane," *Annals of Surgery*, 2022, online ahead of print.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Swanson, Jeffrey W., Michele M. Easter, Kelly Alanis-Hirsch, Charles M. Belden, Michael A. Norko, Allison G. Robertson, Linda K. Frisman, Hsiu-Ju Lin, Marvin S. Swartz, and George F. Parker, "Criminal Justice and Suicide Outcomes with Indiana's Risk-Based Gun Seizure Law," *Journal of the American Academy of Psychiatry and the Law*, Vol. 47, No. 2, 2019.

Swanson, Jeffrey W., Michele M. Easter, Allison G. Robertson, Marvin S. Swartz, Kelly Alanis-Hirsch, Daniel Moseley, Charles Dion, and John Petrila, "Gun Violence, Mental Illness, and Laws That Prohibit Gun Possession: Evidence from Two Florida Counties," *Health Affairs*, Vol. 35, No. 6, 2016.

Swanson, Jeffrey W., Michael A. Norko, Hsiu-Ju Lin, Kelly Alanis-Hirsch, Linda K. Frisman, Madelon V. Baranoski, Michele M. Easter, Allison G. Robertson, Marvin S. Swartz, and Richard J. Bonnie, "Implementation and Effectiveness of Connecticut's Risk-Based Gun Removal Law: Does It Prevent Suicides?" *Law and Contemporary Problems*, Vol. 80, 2017.

U.S. Department of Justice, "Commentary for Extreme Risk Protection Order Model Legislation," June 7, 2021.

Vernick, Jon S., Ted Alcorn, and Joshua Horwitz, "Background Checks for All Gun Buyers and Gun Violence Restraining Orders: State Efforts to Keep Guns from High-Risk Persons," *Journal of Law, Medicine, and Ethics*, Vol. 45, Supp. 1, 2017.

Wintemute, Garen J., Veronica A. Pear, Julia P. Schleimer, Rocco Pallin, Sydney Sohl, Nicole Kravitz-Wirtz, and Elizabeth A. Tomsich, "Extreme Risk Protection Orders Intended to Prevent Mass Shootings: A Case Series," *Annals of Internal Medicine*, August 2019.

Yablon, Alex, "Use of Red Flag Laws Varies Widely Among Local Police," The Trace, April 23, 2019.

Zeoli, April M., Shannon Frattaroli, Leslie Barnard, Andrew Bowen, Annette Christy, Michele Easter, Reena Kapoor, Christopher Knoepke, Wenjuan Ma, Amy Molocznik, Michael Norko, Elise Omaki, Jennifer K. Paruk, Veronica A. Pear, Ali Rowhani-Rahbar, Julia P. Schleimer, Jeffrey W. Swanson, and Garen J. Wintemute, "Extreme Risk Protection Orders in Response to Threats of Multiple Victim/Mass Shooting in Six U.S. States: A Descriptive Study," *Preventive Medicine*, Vol. 165, Pt. A, December 2022.

Zeoli, April M., Jennifer Paruk, Charles C. Branas, Patrick M. Carter, Rebecca Cunningham, Justin Heinze, and Daniel W. Webster, "Use of Extreme Risk Protection Orders to Reduce Gun Violence in Oregon," *Criminology and Public Policy*, Vol. 20, No. 2, May 2021.

PART C

Evidence for the Effects of Policies Regulating Firearm Sales and Transfers

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 firearms 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 8)
- licensing and permitting requirements (Chapter 9)
- waiting periods (Chapter 10)
- firearm safety training requirements (Chapter 11)
- lost or stolen firearm reporting requirements (Chapter 12)
- firearm sales reporting, recording, and registration requirements (Chapter 13)
- bans on the sale of assault weapons and high-capacity magazines (Chapter 14)
- bans on low-quality handguns (Chapter 15).

CHAPTER 8

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, individuals who use 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). However, private sales are a large part of the firearm market. A 2019 national survey of gun owners who had obtained a firearm within the previous five years found that 21 percent had purchased or received as a gift or an inheritance their most recent firearm without undergoing a background check (Hepburn, Azrael, and Miller, 2022). Several states have therefore expanded on the 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*. Polling data show relatively high support for such laws: 81 percent of adults (72 percent of gun owners, 87 percent of non–gun owners) express a favorable view of making all private gun sales subject to background checks (Van Green, 2021).

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. Universal background check laws make background checks a requirement for all or nearly all transfers of firearms, thereby extending the federal background check requirements that apply only to transactions between a gun dealer (a federal firearms licensee) and a buyer to transactions between private parties.

Better information on the role of private transactions in supplying criminal markets is key to understanding the potential for expanded background check requirements to reduce illegal gun trafficking. Although little is known about the role of private transactions, a 2016 survey of state prison inmates found that among those who possessed a gun during the offense for which they were imprisoned, only 8 percent had purchased or traded the weapon from a licensed dealer, whereas 70 percent had acquired it from a friend, family member, or "street" source, such as an illicit broker (Alper and Glaze, 2019). Of those who obtained their firearms

from a licensed dealer, the majority (more than 80 percent) reported that a background check was conducted (Alper and Glaze, 2019). Using information from the 2004 version of the same survey and restricting the sample to 13 states considered by the authors to have less restrictive firearm regulations, another study found that more than 40 percent of inmates who acquired a gun from a friend, family member, or "street" source 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).

Although many crime guns appear to be acquired through informal or private channels,[1] the magnitude of the effects of universal background check laws will be determined, in large part, by the levels of compliance and enforcement and the availability of firearms through alternative markets, such as illegal markets or markets in states without background checks, for private transactions.

There are good reasons to suspect that compliance is a problem for universal background check programs. A study of California gun owners estimated that 17 percent of those who purchased their most recent firearm in California following implementation of the state's comprehensive background check law did not complete a background check (Kravitz-Wirtz et al., 2021). Similarly, a 2019 national survey of gun owners found that individuals who obtained a firearm within the past five years through private sources were more likely to have undergone a background check if they were in a state with comprehensive background check laws, but 40 percent of these individuals still acquired a firearm without a background check (versus 74 percent in states without comprehensive background check laws) (Hepburn, Azrael, and Miller, 2022). A 2016 survey study of 1,444 firearm owners found that state laws regarding comprehensive background checks or permit-to-purchase requirements had little relationship with decisions to engage in private sales or check purchasers for eligibility (Merrill-Francis et al., 2021). When states implement universal background checks, the number of background checks conducted in such states should increase substantially, but multiple studies have found at most modest increases (Castillo-Carniglia et al., 2018; Castillo-Carniglia, Webster, and Wintemute, 2019). These results may reflect intentional defiance of the law, inadequate information about the law, inconsistent enforcement, substitution from more-regulated to less regulated types of private sellers, or cross-state border shopping (Castillo-Carniglia et al., 2018; Hepburn, Azrael, and Miller, 2022; Gobaud et al., 2022c; Vernick, Alcorn, and Horwitz, 2017).

Even with imperfect compliance, a universal background check law may still reduce gun-related homicides or suicides by deterring firearm owners from selling to prohibited possessors, deterring prohibited possessors from attempting to acquire firearms, or making it harder or more expensive for prohibited possessors to succeed in doing so. Universal background

[1] The Bureau of Alcohol, Tobacco, and Firearms (ATF) (2002, p. A-3) defined a *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."

checks may also reduce illegal gun trafficking. For instance, Webster, Vernick, and Bulzacchelli (2009) found that fewer crime guns originally purchased out of state came from states with universal background checks than from states with no background checks for private firearm sales.

Law effects are also likely to be moderated by preexisting levels of gun ownership because 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 background check requirement may have little or no effect on crime or suicide risk.

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 prohibited possessors. 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 multiple factors, including whether the disqualifying conditions checked for are accurate indicators of risk and the completeness of records accessible by the background check system. The excess risk of firearm violence attributable to prohibited individuals is unknown. Although research has shown that most violent offenders have previous involvement with the criminal justice system (Wright and Wintemute, 2010; Cook, Ludwig, and Braga, 2005; Kleck and Bordua, 1983) and that individuals with severe mental disorders are at elevated risk of suicide (Chesney, Goodwin, and Fazel, 2014; see also RAND Corporation, 2018, Chapter Nineteen), it is unclear the extent to which related disqualifying indicators assessed in the background check process (e.g., prior felony convictions, adjudicated mental health problems) adequately proxy for these risks.

Furthermore, the primary database used to conduct background checks, the Federal Bureau of Investigation's (FBI's) National Instant Criminal Background Check System (NICS), fails to accurately capture all disqualifying conditions. The NICS is composed of information from three distinct data sources (the National Crime Information Center, the Interstate Identification Index, and the NICS Indices), which capture convictions for relevant prohibiting crimes along with information about mental health adjudications and domestic violence incidents. In 2021, 411,575,054 background checks were conducted through the NICS and, of those, 153,565 were denied (Criminal Justice Information Services Division, 2022). Although NICS records are expansive (including nearly 30 million active records on prohibited possessors, as of 2023), states and court staff struggle to keep NICS records up to date (Criminal Justice Information Services Division, 2023b; Goggins and Gallegos, 2016). Pressure on FBI staff also might lead to checks taking longer than three days, at which point, in some states, a person is automatically approved (Smucker et al., 2022). Gaps in the underlying database

used by the NICS and by state background check systems undermine the effectiveness of the policy. One study found that of the 240 mass shooting incidents between 2009 and 2020, one in three involved a prohibited possessor (Everytown for Gun Safety Support Fund, 2021). In contrast, recent studies of mass *public* shootings found that most of the shooters purchased at least one of their weapons legally and with a federal background check (Buchanan et al., 2018; Peterson and Densley, 2022).

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 of background check laws that disqualify individuals with histories of mental illness related to suicide, one would ideally estimate how, after such laws were implemented, suicide rates changed among individuals newly prohibited by such laws. Similarly, data on the prices 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 restrict access to firearms in secondary markets.

However, there are numerous challenges to undertaking these types 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 21.) 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, 2024, 17 states and the District of Columbia have comprehensive background check laws that require checks for all firearm sales.[2] Even within these states, there are some

[2] California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Massachusetts, Nevada, New Jersey, New Mexico, New York, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and the District of Columbia. See Calif. Penal Code § 28220; Colo. Rev. Stat. § 18-12-112; Conn. Gen. Stat. Ann. § 29-37a(d); 11 Del. Code § 1448A; Hawaii Rev. Stat. Ann. § 134-2; Ill. Comp. Stat. 65/3(a-10); Mass. Gen. Law 140 § 131 (a-c); N.J. Stat. Ann. § 2C:58-3b, 3e; Nev. Rev. Stat. Ann. § 202.254; N.J. Stat. Ann. § 2C:58-3(a); N.M. Stat. Ann. 1978, § 30-7-7.1; N.Y. Gen. Bus. Law §§ 897, 898; Ore. Rev. Stat. § 166.435; 18 Pa. Cons. Stat. Ann. § 6111; R.I. Gen. Laws § 11-47-35; 13 Vt. Stat. Ann. § 4019; Va. Code § 18.2-308.2:5; Wash. Rev. Code Ann. § 9.41.113; D.C. Code Ann. §§ 7-2502.01, 7-2502.03.

differences in the laws. For example, California, Colorado, Delaware, Nevada, New Mexico, New York, Pennsylvania, Washington, and the District of Columbia require that all transfers to individuals (with some minor exceptions) be 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] Four more states—Maryland, Michigan, Minnesota, and Nebraska—have the same universal background check requirements, but they are applicable only to handguns or to handguns and some semiautomatic rifles.[5]

Some of these 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 five states have such laws for handguns only.[7] Under these laws, firearms (or handguns in the latter five 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]

---

[3] Calif. Penal Code §§ 27545, 27875; Colo. Rev. Stat. § 18-12-112; 24 Del. Code § 904A; Nev. Rev. Stat. Ann. § 202.254; N.M. Stat. Ann. 1978, § 30-7-7.1; N.Y. Gen. Bus. Law § 898; Wash. Rev. Code Ann. § 9.41.113; D.C. Code Ann. § 7-2505.02.

[4] Ore. Rev. Stat. §§ 166.435, 166.436.

[5] Md. Code Ann., Pub. Safety § 5-124; Mich. Comp. Laws Serv. § 28.422; Minn. Stat. § 624.7134, subds. 2; Neb. Rev. Stat. § 69-2404. Minnesota's requirement for a background check on private sales can be achieved either by getting a permit to purchase or by finding a federal firearms licensee who will service the transaction by conducting a background check.

[6] Connecticut, Hawaii, Illinois, Massachusetts, New Jersey, and the District of Columbia. See Conn. Gen. Stat. §§ 29-33, 28, 36f, 36g; Hawaii Rev. Stat. Ann. § 134-2; 430 Ill. Comp. Stat. 65/2, 65/4; Mass. Gen. Laws Ch. 140 §§ 129B, 129C; Minn. Stat. § 624.7134, subds. 2; N.J. Stat. Ann. § 2C: 58-3; D.C. Code Ann. §§ 7-2502.01, 7-2502.03.

[7] Maryland, Michigan, Nebraska, New York, and Rhode Island. See Ia. Code § 724.15; Md. Code Ann., Pub. Safety § 5-117.1; Mich. Comp. Laws § 28.422; Neb. Rev. Stat. Ann. §§ 69-2404, 69-2405; N.Y. Penal Law §§ 400.00(2),(10); R.I. Gen. Laws § 11-47-35(a)(2).

[8] Hawaii Rev. Stat. Ann. § 134-2. But note that long-gun permits last for one year and can be used for multiple purchases.

[9] 30 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 11 quasi-experimental studies that met our inclusion criteria and examined the impact of background check policies on suicide outcomes, three of which (Kagawa et al., 2023; Kester et al., 2023; Liu, Siegel, and Sen, 2022) contribute new findings to this updated review. Two of the 11 studies (Castillo-Carniglia et al., 2019; Kester et al., 2023) had critical methodological concerns and are not discussed further.[11]

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 the sample was 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 decrease in total suicides in this age group. A minor limitation of the Ludwig and Cook (2000) study is that it had an unfavorable ratio of estimated parameters to observations (about one to six), meaning it could have misleading parameter estimates and confidence intervals (CIs) because of model overfitting.[12]

Adopting similar methods but extending the study time frame from 1977 to 2014, Luca, Malhotra, and Poliquin (2017) evaluated 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 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.

Siegel et al. (2019) evaluated a more recent time frame (1991 to 2016) but focused on the effects of universal background check laws for the purchase of any firearm, either at point of

[11] Kester et al. (2023) has critical methodological concerns related to its failure to control for national trends. Castillo-Carniglia et al. (2019) has critical methodological issues because the suicide rate analysis that the authors undertook was based on a single state (California) where background check legislation was adopted the same year as misdemeanor violence prohibitions. It is not possible to identify which policy would account for changes in suicide rates.

[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, resulting in critical methodological concerns with model overfit.

purchase or through a licensing or permitting requirement. They specified log-linear models with year and state fixed effects, state social and economic covariates, and controls for nine other firearm laws. One of these controls was a proxy of gun ownership composed of a linear combination of the ratio of firearm suicides to suicides and hunting licenses; including the same measure (firearm suicides or suicides) in both the outcome term and predictor terms can create spurious correlations that bias the study's estimates of policy effects and their standard errors (National Research Council, 2004). Results from these models revealed only uncertain effects of background check laws on overall and firearm-specific suicide rates.

Four additional studies (Castillo-Carniglia et al., 2019; Kagawa et al., 2018; Kagawa et al., 2023; and McCourt et al., 2020) used synthetic control methods to estimate the effects of comprehensive background check laws in particular states. Kagawa et al. (2018) estimated 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 Tennessee prior to the 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.

McCourt et al. (2020) examined the effects of point-of-sale background check laws in Pennsylvania (1995) and Maryland (1996) relative to *synthetic control states*, defined as those without the law of interest during the study comparison periods (1985 to 2017 for Pennsylvania and 1985 to 2013 for Maryland). Results for Maryland showed a suggestive 15-percent decline in the firearm suicide rate compared with the synthetic control, but a significant 22-percent decline in the nonfirearm suicide rate, suggesting that observed differences might be due to something other than gun laws. Furthermore, the synthetic control for Maryland gave positive weights to only two states, prompting critical methodological concerns related to having few treated and control units. Estimated effects on firearm suicide rates in Pennsylvania were uncertain. Because these are two analyses of a single treated state, any differences between treated and control states cannot be reliably attributed to background check laws as opposed to other changes in the states that affect suicide rates.

Kagawa et al. (2023) used a synthetic control method to examine the effect of more-recent comprehensive background check legislation on firearm suicides in four states (Colorado, Delaware, Oregon, and Washington) that implemented policies between 2013 and 2015. In three states, the authors found uncertain effects. No inferences could be drawn for Dela-

ware because the synthetic control models fit poorly for the pre-intervention period. Consequently, there are serious concerns with drawing conclusions from a small sample size of only three treated states. Moreover, the pre-intervention fit for one of these three states (Colorado) was also described as "fairly poor" (Kagawa et al., 2023, p. 5) for pre-intervention firearm suicide trends.

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 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 that 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 precision of their estimates.

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.

Liu, Siegel, and Sen (2022) used a novel approach to assess the effects of universal background checks and handgun-specific background checks at gun shows on firearm suicides,

allowing for spillover effects of the policy into nearby states. Evaluating data from 2000 to 2019, the authors used a linear spatial autoregressive model to decompose policy effects into a direct effect on rates in the state adopting the policy and an indirect spillover effect on other states; spillover effects were assumed to be inversely proportional to each non-adopting state's distance from the adopting state. Summing the direct and indirect effects, the authors estimated an uncertain association between background check requirements for handguns at gun shows and firearm suicides. They found a suggestive negative association between universal background check requirements and a reduction in total firearm deaths, which includes firearm suicides. However, this result came from preliminary models in which each law was included individually (i.e., other firearm laws were not controlled for); the authors did not analyze universal background check requirements' effects in their final models or in models for firearm suicides specifically because of collinearity between gun show requirements and universal background checks. The study's analysis of gun show handgun background checks presented more-minor methodological concerns related to the absence of discussion of model assumptions. The collinearity issues for universal background checks lead to more-serious concerns about the study's ability to infer effects of these policies on firearm suicides.

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. We also omit effect sizes from Liu, Siegel, and Sen (2022) because the spatial autoregressive model's estimands are fundamentally different from those of the other studies that we reviewed, and the reported effect sizes are not directly comparable with other studies.

Conclusions

Total suicides. We identified five qualifying studies that evaluated the effects of background checks on the total number of suicides using somewhat independent data sets (one examined state suicide rates from 1990 to 1997, one examined state rates from 1996 to 2005, one examined individual-level outcomes in a single state from 2002 to 2011, and two covered longer time frames, 1977 to 2014 and 1991 to 2016). 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 (Swanson et al., 2016) did not find significant effects on suicide rates in Florida after adopting *mandatory* reporting of gun-disqualifying mental health records. The fourth study (Luca, Malhotra, and Poliquin, 2017), which covered a longer period and addressed some of

FIGURE 8.1

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

Study, by Policy	Outcome Measure	IRR [95% CI]
State dealer background check	**Suicide rate**	
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.02 [0.98, 1.06]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	1.03 [0.97, 1.09]
Brady Act		
Ludwig & Cook (2000)	Total, aged 21+	0.98 [0.93, 1.03]
Ludwig & Cook (2000)	Firearm, aged 21+	0.98 [0.94, 1.02]
Comprehensive background check		
Siegel et al. (2019)	Total	0.99 [0.95, 1.03]
Siegel et al. (2019)	Firearm	0.97 [0.91, 1.03]
Index of background check comprehensiveness		
Sen & Panjamapirom (2012)	Firearm	0.98 [0.96, 1.00]
Check on restraining order		
Sen & Panjamapirom (2012)	Total	1.02 [0.98, 1.06]
Sen & Panjamapirom (2012)	Firearm	1.03 [0.98, 1.09]
Check on mental illness		
Sen & Panjamapirom (2012)	Total	0.97 [0.95, 0.99]
Sen & Panjamapirom (2012)	Firearm	0.96 [0.92, 0.99]
Check on fugitive status		
Sen & Panjamapirom (2012)	Total	0.91 [0.87, 0.95]
Sen & Panjamapirom (2012)	Firearm	0.95 [0.90, 0.99]
Check on misdemeanor		
Sen & Panjamapirom (2012)	Total	0.98 [0.95, 1.02]
Sen & Panjamapirom (2012)	Firearm	0.95 [0.92, 1.00]
Check on other miscellaneous records		
Sen & Panjamapirom (2012)	Total	1.00 [0.97, 1.03]
Sen & Panjamapirom (2012)	Firearm	1.01 [0.97, 1.05]

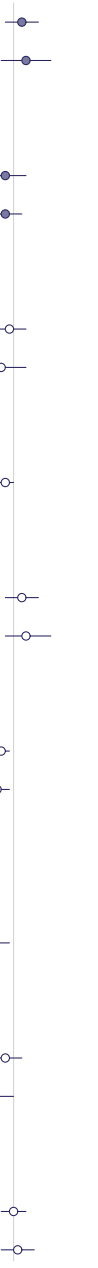
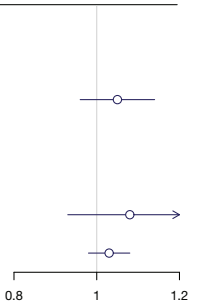


Figure continued on the next page

Figure 8.1—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Single-state studies		
Comprehensive background check	**Suicide rate**	
McCourt et al. (2020), Penn.	Firearm	1.05 [0.96, 1.14]
Repeal comprehensive background check law		
Kagawa et al. (2018), Ind.	Firearm, age-adjusted	1.08 [0.93, 1.23]
Kagawa et al. (2018), Tenn.	Firearm, age-adjusted	1.03 [0.98, 1.08]

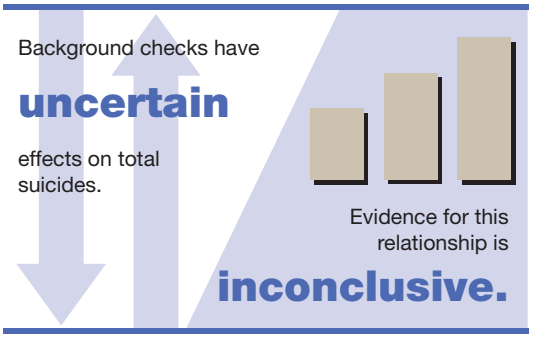


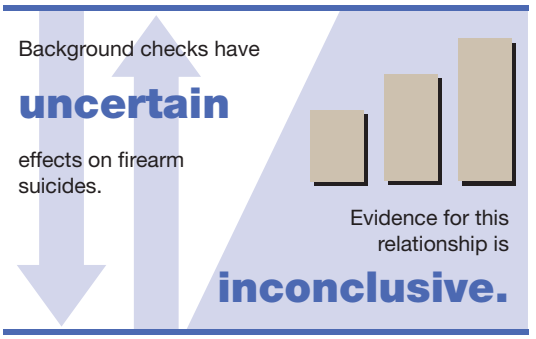
NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

the methodological limitations of the earlier studies, found that dealer background checks had uncertain effects on overall suicide rates. Finally, Siegel et al. (2019) found uncertain effects of universal background check laws on state suicide rates between 1991 and 2016.

Some aspects of background check implementation seem to show limited evidence of reducing total suicide rates, while several studies find uncertain effects of the laws taken as a whole; 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 11 qualifying studies that evaluated the effects of background checks on firearm suicide rates, including the five studies that examined total suicides. Nine of the 11 studies did not have critical methodological concerns. 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 Panjamapirom (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. Kagawa et al. (2023) found uncertain effects of comprehensive background checks on firearm suicides in three states. Analyzing Florida alone, Swanson et al. (2016) did not find significant effects on firearm suicides of required reporting of gun-





disqualifying mental health records to the NICS in Florida. Finally, across several single-state analyses of comprehensive background check laws, two studies (Kagawa et al., 2018; McCourt et al., 2020) found uncertain effects on firearm suicide rates in three states (Indiana, Tennessee, and Pennsylvania) and a suggestive decline in Maryland. Liu, Siegel, and Sen (2022) found uncertain associations between handgun-specific background checks at gun shows and 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

We identified 21 studies that met our inclusion criteria and examined the relationship between background checks and violent crime, six of which (Fridel, 2021a; Gobaud et al., 2022b; Kagawa et al., 2023; Liu, Siegel, and Sen, 2022; Neufeld et al., 2022; Wallin, Holliday, and Zeoli, 2022) contribute new findings to this updated review.[13] However, several additional studies focused specifically on permit-to-purchase or licensing schemes for the acquisition of firearms. Because such schemes are some of the mechanisms used by states to ensure that all transfers of firearms receive background checks, the results of these studies have direct bearing on the available evidence for universal background checks. Therefore, although we offer a detailed review of evidence on permitting and licensing regulations in the next chapter, we consider the evidence from such studies in our conclusions about the evidence supporting universal background checks in our analysis in this section.

Dealer Background Check Laws

Ludwig and Cook (2000) studied the impact of the 1994 Brady Act across states that had laws comparable with those that the Brady Act would impose (which initially included both back-

[13] One study (Neufeld et al., 2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Because there is evidence that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

ground checks and a waiting period) and states that experienced larger changes in the law when the Brady Act was implemented.[14] They found uncertain effects of the policy on total homicides and firearm homicides and a suggestive decrease in nonfirearm homicides. When the sample was restricted to homicides among those aged 55 or older, however, there was a suggestive increase in firearm homicides of around 7 percent; it is unclear why older adults would be differentially affected by this policy change. A minor limitation of the Ludwig and Cook (2000) study is that it had an unfavorable ratio of estimated parameters to observations (about one to six); 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 and 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.

La Valle (2013) used variation in when states adopted background checks, such as 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 between 1980 and 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.

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 study did not evaluate the effect of these variables on total homicides). The analysis 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

[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 because they implemented an instantaneous background check). These analyses had a ratio of estimated parameters to observations of less than one to five, resulting in critical methodological concerns with model overfit. Cook and Ludwig (2003) present results that are for a shorter period but are qualitatively similar.

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.

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 that adjust 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.

Background Checks for Private Sales

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; it also 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. Using data from 1991 to 2016, Fridel (2021a) instead found a statistically significant, 10-percent decrease in total homicide rates associated with universal background check laws. In all three studies, inclusion of background check laws in the models was seemingly intended to account for potential confounds when trying to estimate the causal effects of shall-issue laws, creating serious concerns with interpreting estimated associations for the background check law variables as causal (Hünermund and Louw, 2023; Westreich and Greenland, 2013).

In contrast, using data from 1980 to 2011, Gius (2015a) found that 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. A study of firearm injury hospitalization rates from 2000 to 2016 (Neufeld et al., 2022) also found uncertain effects of universal background check laws using two-way fixed-effects regression models that adjusted for a small set of state-level time-varying covariates (demographic and socioeconomic characteristics, a proxy measure

for household firearm ownership, the violent crime rate, and per capita alcohol consumption) but no other firearm laws.

Analyzing state-level data from 1980 to 2013, 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. In a similar analysis that stratified by race of the decedent, Wallin, Holliday, and Zeoli (2022) found suggestive effects consistent with comprehensive background check laws increasing firearm-specific intimate partner homicides and total intimate partner homicides among White victims and Black victims, respectively.

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 10-percent increases in firearm homicides, an effect partially offset by a significant 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 11 percent while having uncertain effects on nonfirearm homicides (see Chapter 9). Because the authors made no adjustment for clustering of standard errors at the state level and did not test for overdispersion, the standard errors estimated from their model may be invalid.

Siegel et al. (2019) estimated the effects of universal background check laws for the purchase of any firearm (either at point of purchase or through a licensing or permitting requirement) on state homicide rates from 1991 to 2016, specifying log-linear models that controlled for year and state fixed effects, state social and economic covariates, and nine other firearm laws. These models revealed statistically significant 15-percent reductions in both firearm homicides and total homicides associated with the introduction of universal background check laws. Similarly, in a separate set of analyses, Siegel et al. (2020b) estimated the effects of state point-of-sale universal background checks enacted between 1991 and 2016 on homicides within 197 medium-to-large U.S. cities (populations of at least 100,000) and, separately, in nonurban areas. They estimated the effect of the policy on homicide rates using negative binomial models controlling for year and city fixed effects, state- and area-level covariates, and nine additional firearm policies. In these cities, there was a 13-percent reduction in firearm homicides after passage of universal background check policies and no discernable effect on nonfirearm homicides, resulting in an estimated 12-percent reduction in total homicides. This effect was not observed in nonurban areas.

Liu, Siegel, and Sen (2022) used a novel approach to assess the effects of universal background checks and handgun-specific background checks at gun shows on firearm homicides, allowing for spillover effects of the policies to other states. Evaluating data from the 48 con-

tiguous states from 2000 to 2019, the authors used a linear spatial autoregressive model that predicted firearm homicide rates in a given state-year as a function of that state's firearm laws, other states' firearm laws, and other states' firearm homicide rates, weighting relationships with other state factors inversely to the geographic distance between states and controlling for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are not intuitive because all states affect and are affected by all other states, but there is varying strength of that relationship. The authors calculated overall law effects by comparing the hypothetical scenario in which all states imposed the background check requirement with the hypothetical scenario in which no states did; they also decomposed total policy effects into the direct within-state effect of adopting the policy and the indirect interstate spillover effect. While the authors found a suggestive negative overall effect of requiring background checks for handguns transferred at gun shows, this was entirely driven by the indirect interstate spillover effect (the within-state association was positive and uncertain), and it is unclear what policy implications this has. The authors also found that interstate effects resulted in a suggestive negative association between universal background check requirements and total firearm deaths, which includes firearm suicides, although the authors caution against interpreting this finding because of collinearity between gun show requirements and universal background checks.

Four additional studies (Castillo-Carniglia et al., 2019; Kagawa et al., 2018; Kagawa et al., 2023; and McCourt et al., 2020) used synthetic control methods to estimate the effects of comprehensive background check laws in particular states. Looking at repeals in Indiana and Tennessee, Kagawa et al. (2018) evaluated how the repeal of comprehensive background check policies affects homicide rates. 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 homicide 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 (Schell, Smart, and Morral, 2022).

Kagawa et al. (2023) extended these analyses to four states (Colorado, Delaware, Oregon, and Washington) that implemented comprehensive background check policies between 2013 and 2015. In three states, the authors found uncertain effects. No inferences could be drawn for Delaware because the synthetic control models fit poorly for the pre-intervention period. Because using these methods means conducting state-specific analyses without pooling results, the methods are subject to serious concerns with potential confounds that may have influenced homicide rates during or after the post-implementation period.

Castillo-Carniglia et al. (2019) studied California's simultaneous 1992 implementation of background checks for nearly all firearm sales and a ten-year prohibition on the purchase and possession of firearms among individuals convicted of violent misdemeanor crimes. The authors applied a synthetic control approach with data from states that did not have either policy during the study period (1981–2000). Ten years after implementation of the policy, this analysis found uncertain associations of the California law with firearm and nonfirearm homicide rates (Castillo-Carniglia et al., 2019). Examining the effects of point-of-sale background check laws in Pennsylvania (1995) and Maryland (1996) relative to synthetic control states, McCourt et al. (2020) estimated suggestive increases in firearm homicide rates of 18 percent for Maryland and 22 percent for Pennsylvania. The authors also estimated a suggestive 33-percent increase in the nonfirearm homicide rate in Maryland, suggesting that observed differences may be due to something other than gun laws. Post-hoc analyses excluding Baltimore and Philadelphia showed that results were highly sensitive to the inclusion of these large cities, which is unsurprising given the high concentration of gun violence in these cities. Because each analysis examined a single treated state, any differences in homicide rates between treated and control states cannot be reliably attributed to background check laws, as opposed to other changes in the states that affect homicide rates. Furthermore, more than 95 percent of the weight in the synthetic control for Pennsylvania came from just three states, which raises more critical concerns that contemporaneous factors in these other states drove differences in homicide rates.

Two studies (Kaufman et al., 2020; Knopov et al., 2019) evaluated the effects of universal background checks on Black and White citizens separately. Using a log-linear model with two-way fixed effects; several social, economic, and crime covariates, including lagged firearm homicide rates; and several interaction terms, Knopov et al. (2019) modeled the effects of seven classes of laws on total and firearm homicides in 39 states between 1991 and 2016. In these models, universal background checks for all firearms were associated with 11.2-percent lower homicide rates for both Black and White populations, with no evidence of a differential effect among the race groups. This effect included a significant 14.2-percent reduction in firearm homicides and a nonsignificant 5-percent reduction in nonfirearm homicides. Because the models included a lagged outcome as a predictor, model estimates for the effects of universal background checks are likely attenuated: In the second year after the law's implementation and thereafter, a portion of the law effect will be captured by the lagged outcome term.[15] Using a similar specification but with one additional year of data, Kaufman et al. (2020) found a significant 19-percent reduction in firearm homicides among Black Americans after passage of universal background check laws for handguns (including gun shows and private sales), but uncertain effects among White Americans. However, the authors did not formally test for differences by race, and the CIs around these estimates overlap (see Figure 8.2). Furthermore, Kaufman et al. (2020) dropped all state-years for which there were

[15] Here, we assume that the lagged outcome was lagged by one year. If the lag was longer than one year, the law effect ascribed to the lagged outcome would take longer to affect the model estimate for the law effect.

fewer than ten homicides, a decision that eliminated 30 percent of the state-years otherwise available for the analysis of Black Americans. Omitting low-homicide state-years may produce biased results, and this exclusion drove the ratio of observations to estimated parameters below ten to one, raising the possibility that the model for Black Americans was overfit and the parameter estimates were unreliable.

Finally, one study evaluated the extent to which universal background check laws are associated with the likelihood that a law enforcement officer is assaulted in the state (Gobaud et al., 2022b). Using data from 2006 to 2016, the authors estimated logistic regression models that controlled only for state random effects and a small set of covariates (e.g., population, firearm ownership, demographics). While they found significantly higher odds of assault of a law enforcement officer in agencies that serve states with universal background check laws, the study has critical methodological concerns because the models do not control for national trends and include only limited controls for confounds that are likely to vary across states with and without universal background check laws; furthermore, failing to adjust for serial correlation likely resulted in underestimated standard errors.

Components of Background Check Records and Requirements

Sen and Panjamapirom (2012) examined the effects of different types of state background checks conducted 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 varia-

tion 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 the pre-Brady background check policy does not meet our criteria for inclusion.

Sabbath, Hawkins, and Baum (2020) characterized background check laws using an index constructed from the State Firearm Laws Database (Siegel et al., 2017a) that represents the summation of 11 possible background check provisions. Considering six states that strengthened background check laws from 2011 to 2017 using this index, the authors examined the association of stronger background check laws with workplace homicide rates. Using a log-link model with gamma-distributed errors, year fixed effects, and six time-varying socioeconomic variables at the state-year level, the authors found that changes in an index of the strength of state background check laws that would move a state from the 25th to the 75th percentile for the number of background check laws in place were associated with a roughly 5-percent reduction in workplace homicides. Because these models did not include state fixed effects, the estimated effects of background checks will be biased if the six time-varying state covariates included in the model fail to capture all potential state-level confounds—state differences that could affect both changes in background check laws and changes in workplace homicide rates—which seems likely.[16]

In another study of components of background check policies, 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.

Finally, 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 hospital-

[16] The six included covariates were unemployment rate, percentage of residents below the federal poverty line, percentage of Black residents, percentage of residents with a college education, percentage of male residents, and nonhomicide violent crime rate.

ization 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).

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. 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. Finally, we do not present results from Gobaud et al. (2022b) because of critical concerns about threats to causal identification.

Conclusions

Dealer Background Checks

Homicides and violent crime. Ten studies meeting our inclusion criteria examined the effects of dealer background checks, or components of those checks, on homicide and violent crime rates. All five studies estimating the overall effect of dealer background checks found uncertain results: 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); and the state dealer background check effect in Hepburn et al. (2004). One additional study (Zeoli et al., 2018) found uncertain effects of states consulting their own databases on firearm ownership eligibility in addition to running a federal NICS check.

Several analyses of components of dealer background checks have found evidence for their effectiveness in reducing homicide rates. Sen and Panjamapirom (2012) found significant effects indicating that mental illness checks, restraining order checks, or fugitive status checks reduced violent crime specific to homicides. The same study found suggestive effects on homicides for "other miscellaneous checks" and uncertain effects for checks on misdemeanor offenses. Using data from Connecticut, Swanson et al. (2013) found that reporting of mental health records to the NICS significantly reduced violent crime arrests among individuals with and without a disqualifying condition. Using data from Florida, Swanson et al.

FIGURE 8.2

Incidence Rate Ratios Associated with the Effect of Background Checks on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
State dealer background check	**Homicide rate**	
Hepburn et al. (2004)	Total, all ages	1.02 [0.93, 1.12]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.03 [0.87, 1.20]
Gius (2015a)	Firearm	0.80 [0.73, 0.87]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	1.02 [0.83, 1.26]
State private-seller background check		
Gius (2015a)	Firearm	1.31 [1.23, 1.39]
Lott (2010)	Total	1.02 [0.98, 1.07]
Brady Act		
Ludwig & Cook (2000)	Total, aged 21+	0.97 [0.87, 1.08]
Monroe (2008)	Total, aged 21+	0.90 [0.53, 1.28]
La Valle (2013)	Total, age-adjusted	1.00 [0.89, 1.13]
La Valle (2013)	Firearm, age-adjusted	1.02 [0.89, 1.17]
Ludwig & Cook (2000)	Firearm, aged 21+	0.99 [0.86, 1.13]
Monroe (2008)	Handgun, aged 21+	0.81 [0.35, 1.26]
Monroe (2008)	Non-handgun firearm, aged 21+	0.51 [0.08, 0.95]
Index of background check comprehensiveness		
Sen & Panjamapirom (2012)	Firearm	0.93 [0.91, 0.96]
Check on mental illness	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm	0.99 [0.84, 1.16]
Check on mental illness	**Homicide rate**	
Sen & Panjamapirom (2012)	Total	0.93 [0.86, 0.99]
Sen & Panjamapirom (2012)	Firearm	0.93 [0.87, 1.01]
Check on restraining order		
Sen & Panjamapirom (2012)	Total	0.91 [0.85, 0.98]
Sen & Panjamapirom (2012)	Firearm	0.87 [0.79, 0.95]
Check on fugitive status		
Sen & Panjamapirom (2012)	Total	0.77 [0.71, 0.84]
Sen & Panjamapirom (2012)	Firearm	0.79 [0.72, 0.88]

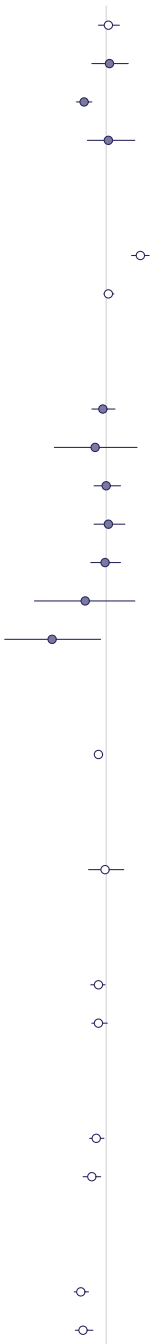


Figure continued on the next page

Figure 8.2—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Check on misdemeanor		
Sen & Panjamapirom (2012)	Total	1.02 [0.95, 1.10]
Sen & Panjamapirom (2012)	Firearm	0.99 [0.90, 1.08]
Check on other miscellaneous records		
Sen & Panjamapirom (2012)	Total	1.05 [0.98, 1.13]
Sen & Panjamapirom (2012)	Firearm	1.12 [1.03, 1.22]
Point-of-contact background check	**IPH rate**	
Zeoli et al. (2018)	Total	0.98 [0.91, 1.07]
Wallin, Holliday, & Zeoli (2022)	Total, White	0.97 [0.89, 1.06]
Wallin, Holliday, & Zeoli (2022)	Total, Black	0.98 [0.89, 1.08]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	0.99 [0.88, 1.11]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	0.93 [0.82, 1.05]
Point-of-contact background check	**Homicide rate**	
Zeoli et al. (2018)	Firearm	1.00 [0.90, 1.11]
Comprehensive background check		
Fridel (2021a)	Total	0.91 [0.85, 0.97]
Knopov et al. (2019)	Total, age-adjusted	0.89 [0.81, 0.97]
Siegel et al. (2020b)	Total, mid to large cities	0.88 [0.80, 0.98]
Siegel et al. (2020b)	Total, nonurban areas	0.98 [0.89, 1.07]
Siegel et al. (2019)	Total	0.85 [0.76, 0.95]
Siegel et al. (2019)	Firearm	0.85 [0.74, 0.97]
Siegel et al. (2020b)	Firearm, mid to large cities	0.87 [0.77, 0.97]
Siegel et al. (2020b)	Firearm, nonurban areas	1.00 [0.87, 1.14]
Kaufman et al. (2020)	Firearm, non-Hispanic Black	0.81 [0.70, 0.94]
Kaufman et al. (2020)	Firearm, non-Hispanic White	0.93 [0.73, 1.20]
Sabbath, Hawkins, & Baum (2020)	Workplace homicide	0.97 [0.94, 0.99]
Zeoli et al. (2018)	IPH	1.07 [0.94, 1.21]
Wallin, Holliday, & Zeoli (2022)	IPH, White	1.05 [0.90, 1.24]
Wallin, Holliday, & Zeoli (2022)	IPH, Black	1.09 [0.97, 1.23]
Zeoli et al. (2018)	Firearm-related IPH	1.13 [0.94, 1.35]
Wallin, Holliday, & Zeoli (2022)	Firearm IPH, White	1.18 [0.95, 1.46]
Wallin, Holliday, & Zeoli (2022)	Firearm IPH, Black	1.08 [0.90, 1.29]

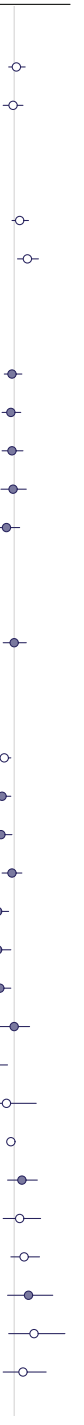
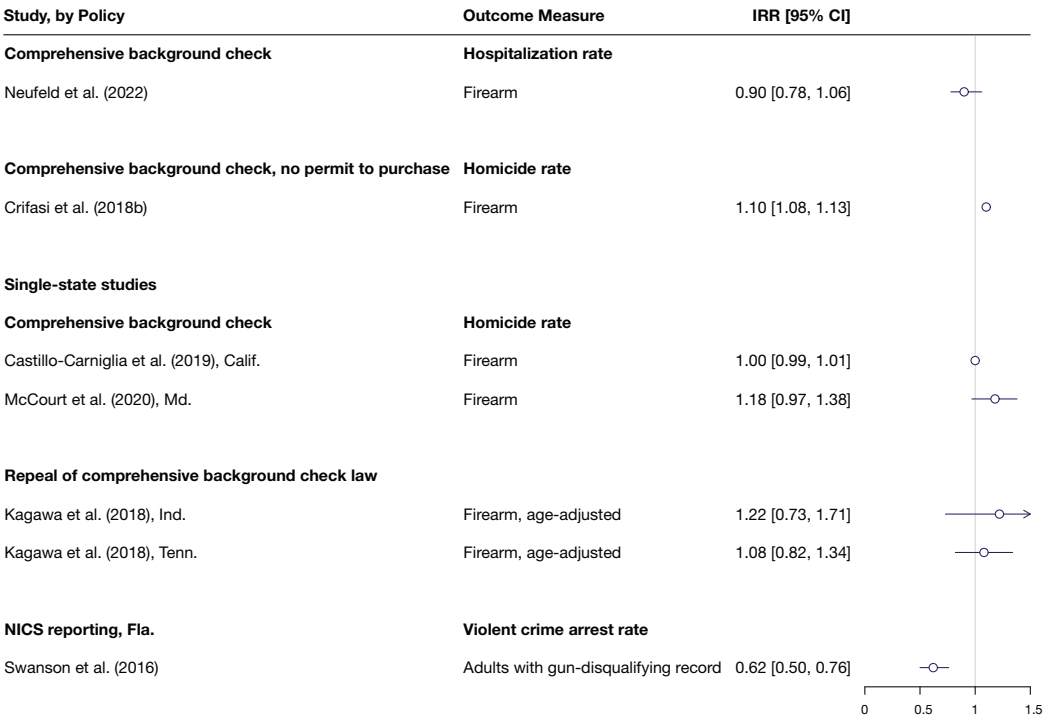


Figure continued on the next page

Figure 8.2—Continued

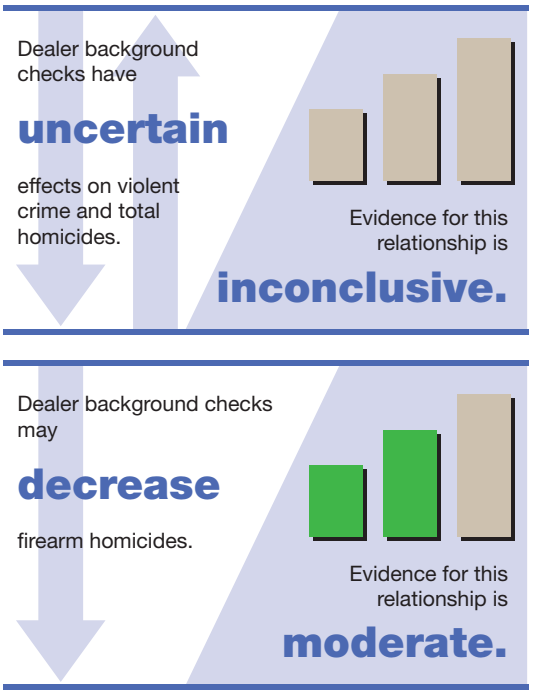


NOTE: The study model in La Valle (2013) includes a one-year lag of the dependent variable, so the effect sizes from this study are not directly comparable with others in this figure. IRR values marked with empty circles indicate that we identified concerns with the study's methodology, and these concerns are described in the text above. Filled circles indicate that we identified no significant methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. IPH = intimate partner homicide.

(2016) found that mental illness checks significantly reduced violent crime arrests. Finally, Vigdor and Mercy (2006) found significant evidence that states with a strong system for checking whether applicants to purchase firearms had restraining orders against them had fewer intimate partner homicides. They found uncertain effects of having a similarly strong system for checking domestic violence misdemeanor databases.

Whereas the overall effects of dealer background checks remain uncertain, some components of background checks appear to significantly reduce homicides or violent crime. This is a confusing finding that requires additional investigation before a clear picture of the effects of dealer background checks can be resolved. Until then, we conclude that there is *inconclusive evidence suggesting that dealer background checks reduce homicide and other violent crimes*.

Firearm homicides. We identified eight qualifying studies that provided estimates for the effects of dealer background checks or some component of those checks on firearm homicide or murder 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 indi-



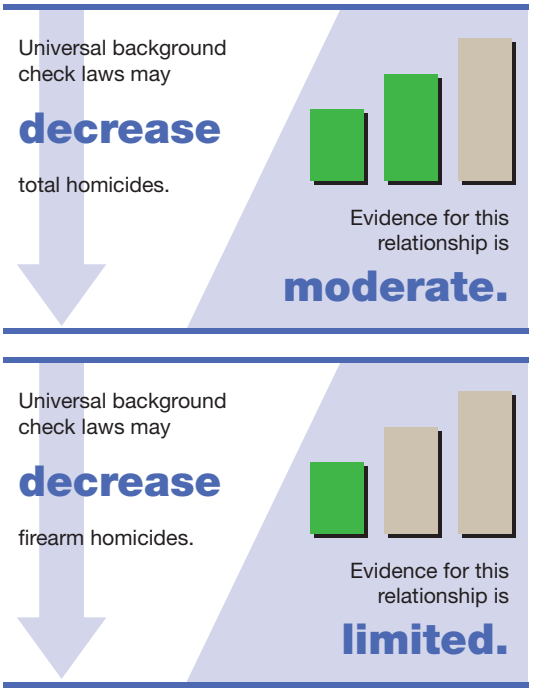
cating that dealer background checks reduce firearm homicides (Gius, 2015a; Sen and Panjamapirom, 2012), one found significantly reduced rates of nonhandgun firearm homicides (Monroe, 2008), and three found uncertain effects for the relationship (Luca, Malhotra, and Poliquin, 2017; La Valle, 2013; Ludwig and Cook, 2000). One additional study (Zeoli et al., 2018) found uncertain effects for firearm-related intimate partner homicides of states consulting their own databases on firearm ownership eligibility in addition to running a federal NICS check.

Analyses of components of background checks from one 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 "other miscellaneous checks" increase firearm homicides. Similarly, Vigdor and Mercy (2006) found that states with strong systems for checking restraining orders saw significantly fewer intimate partner 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.*

Universal Background Checks

Homicides. Excluding one study with critical methodological concerns, eight studies examined the effects of universal background checks, implemented as either permit-to-purchase or private-seller background check systems, on homicides or murders. Lott (2010) and Zeoli et al. (2018) found uncertain effects of such laws. Restricting to intimate partner homicides, Wallin, Holliday, and Zeoli (2022) found that universal background check laws had suggestive effects consistent with an increase in intimate partner homicides for Black decedents but uncertain effects for White decedents. Siegel et al. (2020b) found uncertain effects for universal background checks in rural areas, but a significant 12-percent reduction in homicides in U.S. cities after these laws were implemented. Similarly, Knopov et al. (2019) and Siegel et al. (2019) found that these laws were associated with reductions in homicide rates of a simi-



lar magnitude (11 percent and 15 percent, respectively). Fridel (2021a) found that the laws were associated with a 20-percent reduction in homicides. Finally, Sabbath, Hawkins, and Baum (2020) found that more-comprehensive background check laws were associated with a significant 3-percent reduction in workplace homicides. Evidence reviewed in Chapter 9 on permitting and licensing regulations, which offer one specific approach to ensuring universal background checks, is consistent with our conclusions that *there is moderate evidence that universal background check laws reduce total homicides*.

Firearm homicides. Fourteen studies meeting our inclusion criteria examined the effects of universal background check laws on firearm homicides or murders committed with a firearm. Castillo-Carniglia et al. (2019), Kagawa et al. (2018), and Kagawa et al. (2023) found uncertain effects for these laws, as did Siegel et al. (2020b) for the effects of these laws in rural counties. Siegel et al. (2020b) found significant reductions in firearm homicides in medium-to-large cities after those cities passed universal background check laws, and Kaufman et al. (2020) found significant reductions in firearm homicides for Black—but not White—populations. Knopov et al. (2019) and Siegel et al. (2019) found that these laws were significantly associated with reductions in state-level firearm homicide rates. Estimated reductions ranged from approximately 5 to 20 percent. Liu, Siegel, and Sen (2022) found a negative association between firearm homicide rates and laws requiring background checks for handgun sales at gun shows, although the fact that their effect sizes were driven by interstate effects does not allow for drawing strong policy implications.

Several other studies found associations in the opposite direction. Specifically, Gius (2015a) found a significant 30-percent increase in firearm-related murder rates associated with universal background checks, although these estimates became uncertain or significant in the opposite direction in different specifications of the statistical model. Focusing on urban counties, Crifasi et al. (2018b) found a significant increase in firearm homicides as well, but only for universal background check laws that were not implemented as part of a permit-to-purchase regulatory system; the authors estimated that universal background checks implemented as part of permit-to-purchase laws were associated with significantly reduced firearm homicides. McCourt et al. (2020) found a suggestive increase in firearm homicide rates in Maryland and Pennsylvania when those states implemented their laws, and

Zeoli et al. (2018) found marginally suggestive evidence that universal background checks may increase firearm-related intimate partner homicides. In analyses stratified by decedent race, Wallin, Holliday, and Zeoli (2022) found that universal background check laws had suggestive effects consistent with an increase in firearm-related intimate partner homicides for White decedents but uncertain effects on Black decedents. Here, too, evidence reviewed in the next chapter on permitting and licensing regulations, which offer one approach to ensuring universal background checks, is consistent with our conclusions that there is *limited evidence that universal background checks reduce firearm homicides*.

Effects on Mass Shootings

Research Synthesis Findings

We identified six studies examining the effects of background check policies on mass shootings in the United States, two of which (Fridel, 2021a; Siegel et al., 2020a) contribute new findings to this updated review. Two studies were rated as having critical methodological concerns and are not discussed further.[17]

Examining 1990 to 2014, Gius (2018) evaluated how background check laws relate to total deaths and injuries from school shootings using mortality 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).

More recently, Webster et al. (2020) assessed the effects of background check laws passed between 1984 and 2017 on *public mass shootings*, which are defined as incidents that involved four or more firearm fatalities (excluding offender fatalities and drug- or gang-related shootings) and identified using the FBI Supplementary Homicide Report and two additional publicly available data sources. In negative binomial models that included 14 laws, state fixed

[17] Kalesan et al. (2017) covered a short time frame (2013 to 2015) and controlled only for linear national time trends, raising critical methodological concerns about drawing insights into the causal effects of background check laws on school shootings. Luca, Malhotra, and Poliquin (2020) was subject to critical methodological concerns related to model overfit due to fewer than two events per estimated parameter.

effects, linear and quadratic time trend terms, and 14 social and economic state covariates, the authors found that comprehensive background checks requiring purchaser licensing and in-person application or permitting were associated with significant 50- to 60-percent reductions in mass shooting incidence and fatalities; in contrast, laws requiring point-of-sale background checks had only uncertain effects on mass shootings. However, as with most empirical studies of mass shootings, incidents and fatalities are quite sparsely represented in the data used for these models. Incidents occurred at a rate of less than 0.4 per state-year, and fewer than two deaths occurred per state-year. Without adjustments for known biases in maximum likelihood estimates using sparse data, the resulting effect estimates and CIs may be biased and unreliable (Kenne Pagui et al., 2022).

Examining a slightly longer time frame (1976 to 2018), Siegel et al. (2020a) analyzed the relationship of universal background check laws with the occurrence of any *mass public shooting incident*, defined as an incident unconnected to other criminal activity in which four or more victims were fatally shot in a public location. Multiple sources of data were aggregated to obtain mass public shooting incident information over the study period. Regression models controlled for linear and quadratic national time trends, as well as 13 time-varying state-level characteristics (e.g., population, demographic composition, ratio of firearm suicides to suicides, age-adjusted firearm homicide rate, several other firearm laws). Estimates from logistic regressions showed that universal background check laws had uncertain associations with the likelihood of any mass public shooting incident occurring, and similar findings were shown in zero-inflated negative binomial models. In addition to concerns with sparse outcome data, these results are subject to minor concerns related to having fewer than ten observations per parameter and the absence of discussion around identifying assumptions (e.g., the extent to which estimated effects reflect across-state versus within-state variation).

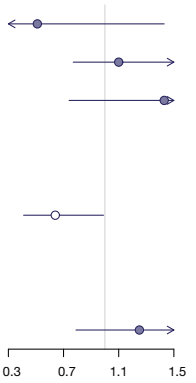
Finally, Fridel (2021a) evaluated the shorter time frame of 1991 to 2016 and assessed the relationship of universal background check laws with rates of *mass murder*, defined as an incidents with four or more fatalities (excluding the perpetrator) within a 24-hour period. The author used negative binomial models that employed generalized estimating equations and included region-level fixed effects, year fixed effects, controls for other firearm policies, and a large set of state-level time-varying covariates (e.g., a proxy for firearm ownership, socioeconomic factors, crime rates). Results showed that universal background check laws were associated with significantly lower mass murder incidence rates, although the purpose of this study was to evaluate shall-issue laws; because these other firearm laws were intended as ancillary covariates, these results are subject to serious methodological concerns with ascribing a causal interpretation to the study's estimate.

Figure 8.3 displays the IRRs and CIs associated with the background check policies examined in studies that did not have critical methodological concerns.

FIGURE 8.3

Incidence Rate Ratios Associated with the Effect of Background Checks on Mass Shootings

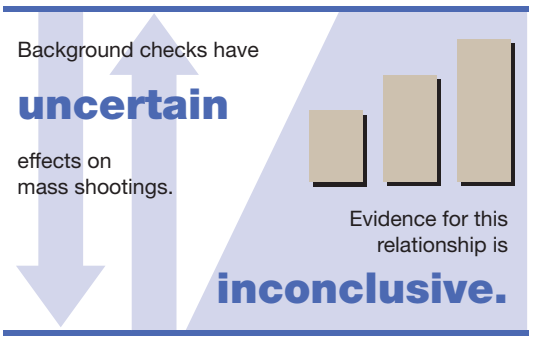
Study, by Policy	Outcome Measure	IRR [95% CI]
Comprehensive background check, point of sale	**Mass shooting**	
Siegel et al. (2020a)	Any public incident	0.51 [0.18, 1.43]
Webster et al. (2020)	Number of incidents	1.10 [0.77, 1.58]
Webster et al. (2020)	Number of fatalities	1.43 [0.74, 2.77]
Comprehensive background check, point of sale	**Mass murder**	
Fridel (2021a)	Number of incidents	0.64 [0.41, 0.99]
Background checks, private party sales	**School shooting**	
Gius (2018)	Number of deaths and injuries	1.25 [0.79, 1.99]



NOTE:IRR values marked with unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.

Conclusions

Excluding two studies with critical methodological concerns, we identified four qualifying studies that estimated the effects of background checks on mass shootings or school shootings. One study found uncertain effects of background checks for private sales on school shooting casualties (Gius, 2018), and one study found uncertain effects of universal background check laws on the likelihood of a mass public shooting occurring (Siegel et al., 2020a). One study found that universal background checks were associated with significantly lower rates of mass murder (Fridel, 2021a). Finally, one study (Webster et al., 2020) found that comprehensive background checks that were not associated with permit-to-purchase regulatory systems were associated with uncertain effects on mass shootings and related fatalities; instead, comprehensive background checks implemented through a purchaser licensing system requiring in-person application or fingerprinting were associated with significantly reduced mass shootings (see discussion in Chapter 9). Considering the methodological limitations in these studies, we consider there to be *inconclusive evidence for the effect of background checks on mass shootings.*



Effects on Police Shootings

Research Synthesis Findings

Our updated search identified one study that met our inclusion criteria and evaluated the relationship between background check laws and police shootings (Rogna and Nguyen, 2022). Rogna and Nguyen (2022) drew on data from the crowdsourced Fatal Encounters database between 2012 and 2018 to evaluate the effects of more-restrictive background check policies as measured by the Giffords Law Center's background checks and access to firearms scale, a composite index that incorporates state policies related to background checks and mental health access. Using linear two-way fixed-effects models that controlled for urbanicity, socioeconomic factors, the violent crime rate, and a proxy for firearm ownership, the authors estimated an uncertain association of stricter background check policies with fatalities from police shootings.[18] The study has serious limitations because of a lack of clarity regarding the treatment variable, which prevents us from evaluating whether there is sufficient policy variation to draw reliable inferences about policy effects during a relatively short time span.

Figure 8.4 displays the IRR and CI associated with the background check policies examined in Rogna and Nguyen (2022).

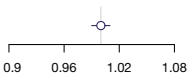
Conclusions

We identified one qualifying study that found an uncertain relationship between more-restrictive background check policies and fatal police shootings (Rogna and Nguyen, 2022). On the basis of this single study, we find *inconclusive evidence for the effects of background check laws on police shootings.*

FIGURE 8.4

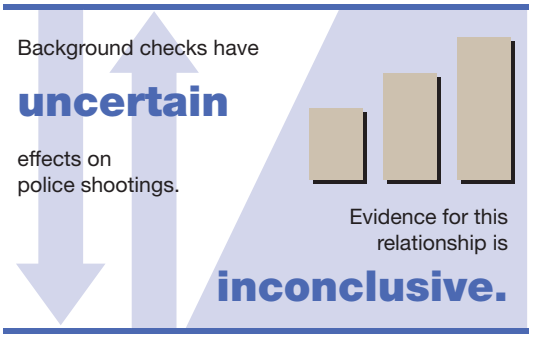
Incidence Rate Ratio Associated with the Effect of Background Checks on Police Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
Background check index		
Rogna & Nguyen (2022)	Fatal police shootings	1.00 [0.99, 1.01]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. See Appendix B for details.

[18] Rogna and Nguyen's (2022) preferred model specification used a Poisson link function to estimate the rate of fatal police shootings directly rather than the standard practice of modeling the count and including a population offset in the structural model. Consequently, we prioritize results from the authors' supplementary linear two-way fixed-effects models, but both produce similar results. However, because the authors' linear specification uses a transform of the dependent variable that complicates interpretation, Figure 8.4 presents IRR results from the Poisson specification.



Effects on the Gun Industry

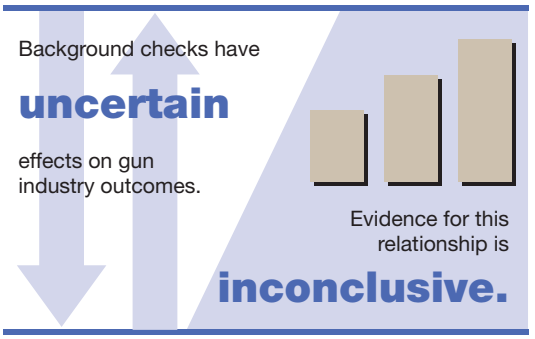
Research Synthesis Findings

We identified two studies that met our inclusion criteria and examined how background check laws might affect firearm industry interests. One of these (Brauer, Montolio, and Trujillo-Baute, 2017) used a policy index of data from 1986 to 1999 that captured the stringency of state laws regarding background checks, firearm registration requirements, and permit-to-purchase requirements to examine how these laws affected variation in the number of firearm manufacturing plants in a state. Using both linear and Poisson regression models with state and year fixed effects, the authors found suggestive effects consistent with more-stringent background check requirements reducing the number of firearm manufacturing plants in a state. However, the authors note serious challenges with the coding of their policy index measure, and they do not provide sufficient information to clarify the type of policy variation that is used to identify effects.

Koper (2002) 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 in 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 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 the 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 8.5 displays the IRRs and CIs associated with the background check policies examined in Koper (2002) and Brauer, Montolio, and Trujillo-Baute (2017).

Conclusions



We identified two studies that provided estimates for the effect of background checks on firearm manufacturer or FFL operations. Exploiting the differential effects of the Brady Act on states depending on their preexisting background check laws, one study (Koper, 2002) found an uncertain effect of the federal background check requirements on the likelihood that an FFL dropped out of the market. The other study (Brauer, Montolio, and Trujillo-Baute, 2017) found suggestive evidence that stronger background check requirements decreased the number of firearm manufacturing plants operating in a state. Considering the methodological limitations of these studies, we conclude that the available literature provides *inconclusive evidence for the effect of background check laws on the gun industry.*

Outcomes Without Studies Examining the Effects of Background Checks

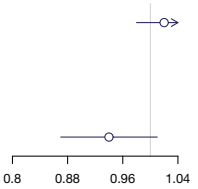
We did not identify any studies that met our inclusion criteria and examined the effects of background check policies on the following outcomes:

- unintentional injuries and deaths
- defensive gun use
- hunting and recreation.

FIGURE 8.5

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

Study, by Policy	Outcome Measure	IRR [95% CI]
Brady Act		
Koper (2002)	FFL exit	1.02 [0.98, 1.06]
Background check and permit index		
Brauer, Montolio, and Trujillo-Baute (2017)	Number of firearm manufacturers	0.94 [0.87, 1.01]



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

Chapter 8 References

Alper, Mariel, and Lauren Glaze, *Source and Use of Firearms Involved in Crimes: Survey of Prison Inmates 2016*, U.S. Department of Justice, 2019.

Azrael, Deborah, Lisa Hepburn, David Hemenway, and Matthew Miller, "The Stock and Flow of U.S. Firearms: Results from the 2015 National Firearms Survey," *Russell Sage Foundation Journal of the Social Sciences*, Vol. 3, No. 5, 2017.

Brauer, Jurgen, Daniel Montolio, and Elisa Trujillo-Baute, "How Do US State Firearms Laws Affect Firearms Manufacturing Location? An Empirical Investigation, 1986–2010," *Journal of Economic Geography*, Vol. 17, No. 4, 2017.

Buchanan, Larry, Josh Keller, Richard A. Oppel, Jr., and Daniel Victor, "How They Got Their Guns," *New York Times*, February 14, 2018.

Bureau of Alcohol, Tobacco and Firearms, *Following the Gun: Enforcing Federal Laws Against Firearms Traffickers,* Department of the Treasury, June 2000.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Castillo-Carniglia, Alvaro, Rose M. C. Kagawa, Magdalena Cerdá, Cassandra K. Crifasi, Jon S. Vernick, Daniel W. Webster, and Garen J. Wintemute, "California's Comprehensive Background Check and Misdemeanor Violence Prohibition Policies and Firearm Mortality," *Annals of Epidemiology*, Vol. 30, February 2019.

Castillo-Carniglia, Alvaro, Rose M. C. Kagawa, Daniel W. Webster, Jon S. Vernick, Magdalena Cerdá, and Garen J. Wintemute, "Comprehensive Background Check Policy and Firearm Background Checks in Three US States," *Injury Prevention*, Vol. 24, No. 6, 2018.

Castillo-Carniglia, Alvaro, Daniel W. Webster, and Garen J. Wintemute, "Effect on Background Checks of Newly-Enacted Comprehensive Background Check Policies in Oregon and Washington: A Synthetic Control Approach," *Injury Epidemiology*, Vol. 6, No. 6, 2019.

Chesney, Edward, Guy M. Goodwin, and Seena Fazel, "Risks of All-Cause and Suicide Mortality in Mental Disorders: A Meta-Review," *World Psychiatry*, Vol. 13, No. 2, 2014.

Cook, Philip J., and Jens Ludwig, "The Effect of the Brady Act on Gun Violence," in B. Harcourt, ed., *Guns, Crime, and Punishment in America*, New York University Press, 2003.

Cook, Philip J., J. Ludwig, and A. A. Braga, "Criminal Records of Homicide Offenders," *JAMA*, Vol. 294, No. 5, 2005.

Cook, Philip J., Stephanie Molliconi, and Thomas B. Cole, "Regulating Gun Markets," *Journal of Criminal Law and Criminology*, Vol. 86, No. 1, 1995.

Crifasi, Cassandra K., Molly Merrill-Francis, Alex McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster, "Association Between Firearm Laws and Homicide in Urban Counties," *Journal of Urban Health*, Vol. 95, No. 3, 2018b.

Criminal Justice Information Services Division, *National Instant Criminal Background Check System (NICS) Section: 2020–2021 Operations Report*, U.S. Department of Justice, Federal Bureau of Investigation, 2022.

Criminal Justice Information Services Division, "Active Records in the NICS Indices as of January 3, 2023," U.S. Department of Justice, Federal Bureau of Investigation, 2023b.

Everytown for Gun Safety Support Fund, "Mass Shootings in the United States: 2009–2020," webpage, June 4, 2021. As of August 18, 2022: http://everytownresearch.org/reports/mass-shootings-analysis/

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Gius, Mark, "The Effects of State and Federal Background Checks on State-Level Gun-Related Murder Rates," *Applied Economics*, Vol. 47, No. 38, 2015a.

Gius, Mark, "The Effects of State and Federal Gun Control Laws on School Shootings," *Applied Economics Letters*, Vol. 25, No. 5, 2018.

Gobaud, Ariana N., Christina A. Mehranbod, Paul M. Reeping, Brady R. Bushover, and Christopher N. Morrison, "Firearm Assaults Against US Law Enforcement Officers in the Line-of-Duty: Associations with Firearm Ownership and State Firearm Laws," *Preventive Medicine Reports*, Vol. 30, December 2022b.

Gobaud, Ariana N., Christopher N. Morrison, Christina A. Mehranbod, and Mark H. Hoofnagle, "Gun Shows and Universal Background Check Laws Across State Lines," *Preventive Medicine*, Vol. 165, Pt. A, December 2022c.

Goggins, Becki, and Anne Gallegos, *State Progress in Record Reporting for Firearm-Related Background Checks: Mental Health Submissions*, U.S. Department of Justice, Bureau of Justice Statistics, February 2016.

Hepburn, Lisa, Deborah Azrael, and Matthew Miller, "Firearm Background Checks in States With and Without Background Check Laws," *American Journal of Preventive Medicine*, Vol. 62, No. 2, 2022.

Hepburn, Lisa, Matthew Miller, Deborah Azrael, and David Hemenway, "The Effect of Nondiscretionary Concealed Weapon Carrying Laws on Homicide," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 56, No. 3, 2004.

Hünermund, Paul, and Beyers Louw, "On the Nuisance of Control Variables in Regression Analysis," *Organizational Research Methods*, December 25, 2023, online ahead of print.

Kagawa, Rose M. C., Alvaro Castillo-Carniglia, Jon S. Vernick, Daniel Webster, Cassandra Crifasi, Kara E. Rudolph, Magdalena Cerdá, Aaron Shev, and Garen J. Wintemute, "Repeal of Comprehensive Background Check Policies and Firearm Homicide and Suicide," *Epidemiology*, Vol. 29, No. 4, 2018.

Kagawa, Rose, Amanda Charbonneau, Christopher McCort, Alexander McCourt, Jon Vernick, Daniel Webster, and Garen Wintemute, "Effects of Comprehensive Background-Check Policies on Firearm Fatalities in 4 States," *American Journal of Epidemiology*, Vol. 192, No. 4, April 2023.

Kalesan, Bindu, Kinan Lagast, Marcos Villarreal, Elizabeth Pino, Jeffrey Fagan, and Sandro Galea, "School Shootings During 2013–2015 in the USA," *Injury Prevention*, Vol. 23, No. 5, 2017.

Kaufman, Elinore J., Christopher N. Morrison, Erik J. Olson, David K. Humphreys, Douglas J. Wiebe, Niels D. Martin, Carrie A. Sims, Mark H. Hoofnagle, C. William Schwab, Patrick M. Reilly, and Mark J. Seamon, "Universal Background Checks for Handgun Purchases Can Reduce Homicide Rates of African Americans," *Journal of Trauma and Acute Care Surgery*, Vol. 88, No. 6, June 2020.

Kenne Pagui, Euloge Clovis, Alessandra Salvan, and Nicola Sartori, "Improved Estimation in Negative Binomial Regression," *Statistics in Medicine*, Vol. 41, No. 13, 2022.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Kleck, G., and D. J. Bordua, "The Factual Foundation for Certain Key Assumptions of Gun Control," *Law and Policy*, Vol. 5, No. 3, 1983.

Klein, Jessie, *The Bully Society: School Shootings and the Crisis of Bullying in America's Schools*, New York University Press, 2012.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Kopel, D. B., "Background Checks for Firearms Sales and Loans: Law, History, and Policy," *Harvard Journal on Legislation*, Vol. 53, 2016.

Koper, Christopher S., "Federal Legislation and Gun Markets: How Much Have Recent Reforms of the Federal Firearms Licensing System Reduced Criminal Gun Suppliers?" *Criminology and Public Policy*, Vol. 1, No. 2, 2002.

Kravitz-Wirtz, Nicole, Rocco Pallin, Rose M. C. Kagawa, Matthew Miller, Deborah Azrael, and Garen J. Wintemute, "Firearm Purchases Without Background Checks in California," *Preventive Medicine*, Vol. 145, April 2021.

La Valle, James M., "'Gun Control' vs. 'Self-Protection': A Case Against the Ideological Divide," *Justice Policy Journal*, Vol. 10, No. 1, 2013.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, *The Impact of Mass Shootings on Gun Policy*, working paper, Harvard Business School, 2016.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

Ludwig, J., and P. J. Cook, "Homicide and Suicide Rates Associated with Implementation of the Brady Handgun Violence Prevention Act," *JAMA*, Vol. 284, No. 5, 2000.

McCourt, Alexander D., Cassandra K. Crifasi, Elizabeth A. Stuart, Jon S. Vernick, Rose M. C. Kagawa, Garen J. Wintemute, and Daniel W. Webster, "Purchaser Licensing, Point-of-Sale Background Check Laws, and Firearm Homicide and Suicide in 4 US States, 1985–2017," *American Journal of Public Health*, Vol. 110, No. 10, October 2020.

Merrill-Francis, Molly, Emma E. McGinty, Colleen L. Barry, Daniel W. Webster, and Cassandra K. Crifasi, "Association Between Gun Owner Attitudes and Their Behavior in Private Firearm Sales," *Preventive Medicine*, Vol. 147, June 2021.

Monroe, Jeffrey D., *Homicide and Gun Control: The Brady Handgun Violence Prevention Act and Homicide Rates*, LFB Scholarly Publishing, 2008.

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Pérez Esparza, David, Eugenio Weigend Vargas, Tony Payan, and Carlos Pérez Ricart, "Examining a Dataset on Gun Shows in the US, 2011–2019," *Journal of Illicit Economies and Development*, Vol. 4, No. 1, 2022.

Peterson, Jillian, and James Densley, *Public Mass Shootings: Database Amasses Details of a Half Century of U.S. Mass Shootings with Firearms, Generating Psychosocial Histories*, National Institute of Justice, February 3, 2022.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of August 22, 2019: https://www.rand.org/pubs/research_reports/RR2088.html

Rogna, Marco, and Bich Diep Nguyen, "Firearms Law and Fatal Police Shootings: A Panel Data Analysis," *Applied Economics*, Vol. 54, No. 27, 2022.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Scalia, John, *Federal Firearm Offenders, 1992–98: With Preliminary Data for 1999*, Bureau of Justice Statistics, June 2000.

Schell, Terry L., Rosanna Smart, and Andrew R. Morral, *Suggestions for Estimating the Effects of State Gun Policies: Commentary on Four Methodological Problems in the Current Literature*, RAND Corporation, RR-A243-1, 2022. As of August 9, 2022: https://www.rand.org/pubs/research_reports/RRA243-1.html

Sen, B., and A. Panjamapirom, "State Background Checks for Gun Purchase and Firearm Deaths: An Exploratory Study," *Preventive Medicine*, Vol. 55, No. 4, 2012.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Siegel, Michael, Molly Pahn, Ziming Xuan, Craig S. Ross, Sandro Galea, Bindu Kalesan, Eric Fleegler, and Kristin A. Goss, "Firearm-Related Laws in All 50 US States, 1991–2016," *American Journal of Public Health*, Vol. 107, No. 7, 2017a.

Siegel, Michael, Benjamin Solomon, Anita Knopov, Emily F. Rothman, Shea W. Cronin, Ziming Xuan, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates in Suburban and Rural Areas Compared to Large Cities in the United States, 1991–2016," *Journal of Rural Health*, Vol. 36, No. 2, March 2020b.

Smucker, Sierra, Max Griswold, Amanda Charbonneau, Rose Kerber, Terry L. Schell, and Andrew R. Morral, *Using National Instant Criminal Background Check Data for Gun Policy Analysis: A Discussion of Available Data and Their Limitations*, RAND Corporation, TL-A243-4, 2022. As of October 1, 2022: https://www.rand.org/pubs/tools/TLA243-4.html

Swanson, Jeffrey W., Michele M. Easter, Allison G. Robertson, Marvin S. Swartz, Kelly Alanis-Hirsch, Daniel Moseley, Charles Dion, and John Petrila, "Gun Violence, Mental Illness, and Laws That Prohibit Gun Possession: Evidence from Two Florida Counties," *Health Affairs*, Vol. 35, No. 6, 2016.

Swanson, J. W., A. G. Robertson, L. K. Frisman, M. A. Norko, H. Lin, M. S. Swartz, and P. J. Cook, "Preventing Gun Violence Involving People with Serious Mental Illness," in D. W. Webster and J. S. Vernick, eds., *Reducing Gun Violence in America: Informing Policy with Evidence and Analysis,* Johns Hopkins University Press, 2013.

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

Van Green, Ted, "Wide Differences on Most Gun Policies Between Gun Owners and Non-Owners, but Also Some Agreement," Pew Research Center, August 4, 2021.

Vernick, Jon S., Ted Alcorn, and Joshua Horwitz, "Background Checks for All Gun Buyers and Gun Violence Restraining Orders: State Efforts to Keep Guns from High-Risk Persons," *Journal of Law, Medicine and Ethics*, Vol. 45, Supp. 1, 2017.

Vigdor, E. R., and J. A. Mercy, "Do Laws Restricting Access to Firearms by Domestic Violence Offenders Prevent Intimate Partner Homicide?" *Evaluation Review*, Vol. 30, No. 3, 2006.

Vittes, K. A., J. S. Vernick, and D. W. Webster, "Legal Status and Source of Offenders' Firearms in States with the Least Stringent Criteria for Gun Ownership," *Injury Prevention*, Vol. 19, No. 1, June 23, 2012.

Wallin, Mikaela A., Charvonne N. Holliday, and April M. Zeoli, "The Association of Federal and State-Level Firearm Restriction Policies with Intimate Partner Homicide: A Re-Analysis by Race of the Victim," *Journal of Interpersonal Violence*, Vol. 37, Nos. 17–18, September 2022.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Webster, Daniel W., Jon S. Vernick, and Maria T. Bulzacchelli, "Effects of State-Level Firearm Seller Accountability Policies on Firearm Trafficking," *Journal of Urban Health: Bulletin of the New York Academy of Medicine*, Vol. 86, No. 4, 2009.

Westreich, Daniel, and Sander Greenland, "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients," *American Journal of Epidemiology*, Vol. 177, No. 4, February 15, 2013.

Wright, M. A., and G. J. Wintemute, "Felonious or Violent Criminal Activity That Prohibits Gun Ownership Among Prior Purchasers of Handguns: Incidence and Risk Factors," *Journal of Trauma and Acute Care Surgery*, Vol. 69, No. 4, 2010.

Zeoli, April M., Alexander McCourt, Shani Buggs, Shannon Frattaroli, David Lilley, and Daniel W. Webster, "Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Associations with Intimate Partner Homicide," *American Journal of Epidemiology*, Vol. 187, No. 11, 2018.

CHAPTER 9

Licensing and Permitting Requirements

Federal law does not require individuals to obtain a license or permit to own, possess, or purchase a firearm. Several states, however, have licensing or permitting requirements, which may come in the form of permits to purchase firearms, licenses to own firearms, or firearm registration requirements. Such licenses and permits are provided by the state to confirm that the license holder has passed a background check and is entitled to purchase or possess a firearm. They do not identify which individual firearms a person owns (see our discussion of "registration" programs in Chapter 13), nor do we talk here about licenses to carry a concealed weapon; these are discussed separately in Chapter 18.

Permit-to-purchase laws are the most common form of state licensing or permitting requirements. These laws generally 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 may routinely confirm that an individual remains eligible to possess or purchase a firearm. Permit-to-purchase systems may also be used to reduce the frequency with which firearm purchasers receive a background check. In several states, for instance, permits to purchase firearms require a background check but then entitle the permit holder to purchase firearms without an updated background check for a period of up to five years. As Smucker et al. (2022) found, background checks for gun sales declined substantially in Alabama when permit-to-purchase regulations were in place.

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 when compared with the status quo, as well as whether these disqualifications correctly target individuals who are at greater risk of inflicting harm on 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. While compliance with licensing and

permitting laws is likely to be imperfect, such laws may still reduce firearm 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 of the law, 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.

In addition to requiring background checks on private transfers, licensing and permitting regulations often impose other requirements on firearm purchasers or owners that may also influence firearm-related outcomes. For instance, 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 2019 survey of firearm storage practices among adults in the United States found that 32 percent of gun owners who completed formal firearm training stored one or more firearms loaded and unlocked, compared with 26 percent of individuals who were not trained doing so (Berrigan et al., 2019). These results suggest that existing trainings may not improve firearm storage practices. Similarly, a 1995 survey found 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 training offered through service in the U.S. 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 11.

Licensing and permitting regulations sometimes 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. In-person application processes that involve direct contact with law enforcement may also deter some individuals who are seeking a firearm for criminal purposes (e.g., for criminal use, for transfer to a prohibited person or to illegal markets) from obtaining weapons. Indeed, some evidence suggests that permit-to-purchase laws, when added to background check laws, can reduce the diversion of firearms for criminal use (Crifasi et al., 2017).

Finally, the process of obtaining a license can impose a waiting time between the purchase decision and acquisition of the firearm. Unlike the length of mandatory waiting periods (see Chapter 10), the lengths of these waiting times are generally a function of bureaucratic or administrative delays and thus can vary across states and transactions; information from some states suggests that permits may entail delays of up to two days (e.g., Nebraska), up to between 20 and 40 days (e.g., Hawaii, Illinois, Massachusetts, New Jersey, North Carolina), or up to six months (e.g., New York) (Koenig and Schindler, 2023). Similar to mandatory wait-

ing periods, these delays may still provide temporal distance between a purchase decision with intent to use a firearm for self-harm or interpersonal violence and the perpetration of violence itself.

While potentially promising, licensing systems requiring substantial coordination among local, state, and federal databases and institutions may pose technical and regulatory challenges for states, and these challenges may reduce the effectiveness of the laws. 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) of gun owners 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. Indeed, requiring a license to purchase a firearm and requiring a license to own a firearm may have very different effects because of the large existing stock of firearms in the United States.

Both types of licensing 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. Perhaps reflective of concerns with added costs, support for handgun purchaser licensing laws is higher among non–gun owners than among gun owners. Combining survey results from 2013, 2015, 2017, and 2019, Crifasi et al. (2020) found that 61 percent and 81 percent of gun owners and non–gun owners, respectively, supported handgun purchaser licensing. That said, gun owners living in states with licensing laws were significantly more likely to support the laws than gun owners living in states without such laws (77 percent versus 59 percent) (Crifasi et al., 2020).

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. Using outcome data in which the type of weapon used can be identified, analyses also exploit state-level variation in the types of guns that require licenses or permits and 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.[2] Specifically, if permit-to-purchase laws restrict traf-

[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).

[2] The Bureau of Alcohol, Tobacco, and Firearms (ATF) (2002, p. A-3) defined a *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."

ficking 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).[3] However, a series of provisions attached to ATF appropriations (commonly known as the Tiahrt Amendments) has denied most researchers access to detailed firearm trace data since 2003; therefore, while some law enforcement agencies may analyze such data, the information generally has not been available for research purposes (Krouse, 2009). Some researchers have obtained access to crime gun trace data for specific jurisdictions (e.g., Baltimore, Maryland) or have analyzed aggregate statistics on crime guns published by the ATF, and this research suggests that permit-to-purchase laws may be effective in preventing the diversion of firearms from within-state retailers to criminal markets (Collins et al., 2018; Crifasi et al., 2017; Kahane, 2020; Li et al., 2023).

State Implementation of Licensing and Permitting Requirements

As of January 1, 2024, 10 states and the District of Columbia have licensing or permitting requirements.[4] These most commonly take the form of permits to purchase,[5] but this policy class can also include licenses to own[6] and registration.[7]

Five states and the District of Columbia have implemented licensing or permitting regimes for all firearms,[8] and five states have done so for handguns.[9] Michigan's law has a

[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."

[4] Connecticut, Hawaii, Illinois, Maryland, Massachusetts, Michigan, Nebraska, New Jersey, New York, Rhode Island, and the District of Columbia. See Calif. Penal Code § 31610; Conn. Gen. Stat. §§ 29-33, 29-37a; Hawaii Rev. Stat. § 134-2; 430 Ill. Comp. Stat. 65/2; Ia. Code § 724.15; Md. Code, Pub. Safety, § 5-117.1; Mass. Gen. Laws Ch. 140 §§ 129B, 131A, 131E; Mich. Comp. Laws § 28.422; Neb. Rev. Stat. Ann. § 69-2404; N.J. Stat. Ann. § 2C:58-3; N.C. Gen. Stat. § 14-402; R.I. Gen. Laws § 11-47-35; D.C. Code Ann. § 7-2502.01. California has a firearm safety certificate that is considered by some researchers a form of licensing and permitting requirement.

[5] Connecticut, Hawaii, Maryland, Massachusetts, Michigan, Nebraska, New Jersey, New York, Rhode Island, and the District of Columbia.

[6] Illinois, Massachusetts, and Michigan.

[7] District of Columbia.

[8] Connecticut, Hawaii, Illinois, Massachusetts, New Jersey, and the District of Columbia.

[9] Maryland, Michigan, Nebraska, New York, and Rhode Island. New York's law applies to handguns and semi-automatic rifles.

broad exemption for individuals who purchase handguns from licensed dealers following a background check.[10]

Some states require that applicants pass a safety course or exam in order to receive a license or permit.[11] Another distinction between states' laws is the duration of the validity of the credentials. A handful of states issue licenses or permits that are valid for a few days or months only,[12] while those issued in other states may last years.[13] 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.[14] Rhode Island's law does not specify how long the permit to purchase is valid.[15]

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.[16] 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.[17]

---

[10] Mich. Comp. Laws § 28.422.

[11] Calif. Penal Code §§ 31610, 31640; Conn. Gen. Stat. §§ 29-36f, 29-37p; Hawaii Rev. Stat. § 134-2; Md. Code, Pub. Safety, § 5-117.1; Mass. Gen. Laws Ch. 140 § 131P; R.I. Gen. Laws § 11-47-35; Wash. Stat. § 9.41.090; D.C. Code Ann. § 7-2502.03. The laws in Hawaii, Maryland, and Rhode Island apply to handguns only.

[12] 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).

[13] 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).

[14] N.J. Stat. Ann. § 2C:58-3.

[15] R.I. Gen. Laws § 11-47-35.

[16] Hawaii Rev. Stat. § 134-2; N.J. Stat. Ann. § 2C:58-3; N.C. Gen. Stat. § 14-403 (handguns only).

[17] Connecticut, Illinois, Massachusetts, New Jersey, and the District of Columbia. See Conn. Gen. Stat. §§ 29-38n; 430 Ill. Comp. Stat. 65/2; Mass. Gen. Laws Ch. 140 § 129C; N.J. Stat. Ann. § 2C:58-3.3; D.C. Code Ann. § 7-2502.02.

Effects on Suicide

Research Synthesis Findings

We identified eight U.S.-based longitudinal studies examining the effect of firearm licensing or permitting requirements on suicide, three of which (Bhatt et al., 2020; Depew and Swensen, 2022; Liu, Siegel, and Sen, 2022) contribute new findings to this updated review. One of these studies (Bhatt et al., 2020) has critical methodological concerns related to having few treated and control units, and this study is not discussed further.[18]

Depew and Swensen (2022) evaluated the effects of New York's 1911 Sullivan Act, which included the requirement that individuals obtain a license for carrying a concealed weapon and obtain a permit to own or purchase a concealable handgun. Although their primary analysis cannot disentangle the effects of the various components of the Sullivan Act, the authors conducted supplemental analyses of data from 1900 to 1929 that included additional states that made further changes to permit requirements during this time. Using their two-way fixed-effects estimation strategy, the authors found that requiring an individual to obtain a license to own a handgun had uncertain effects on firearm suicide and total suicide rates. While this study is subject to only minor methodological concerns related to model overfit and a lack of evidence that the model's identifying assumptions hold, the historical nature of its analysis may make these results less generalizable to today's policy context.

Also using two-way fixed-effects regression methods but evaluating 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 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. These limitations raise serious questions about the validity of the identified effects for establishing how permitting requirements affect suicide.

Two studies (Crifasi et al., 2015; McCourt et al., 2020) used a synthetic control approach to evaluate changes to permit-to-purchase laws. Both evaluated the establishment of a permit-to-purchase law in Connecticut in 1995 and the repeal of Missouri's permit-to-purchase law in 2007. Crifasi et al. (2015) restricted their post-law period to ten years following the law change in Connecticut (covering 1981 to 2006) and analyzed outcomes up to five years after Missouri's law repeal (covering 1981 to 2012). McCourt et al. (2020) evaluated different—albeit overlapping—periods: 1985 to 2017 for Connecticut, and 1997 to 2016 for Missouri.

Crifasi et al. (2015) found evidence of a reduction in firearm suicide rates in Connecticut and its synthetic comparison group after the law, but the reduction was greater in Connecticut.

[18] Bhatt et al. (2020)'s synthetic control analysis of Missouri's repeal of permit-to-purchase requirements had one treated state and fewer than four states, which accounted for more than 95 percent of the weight in the synthetic control.

Specifically, the authors found a suggestive effect indicating that Connecticut's firearm suicide rate was 15 percent lower than that of its synthetic control during the ten-year 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. Including longer post-law periods for both states, the synthetic control method analyses of McCourt et al. (2020) showed a suggestive 33-percent decrease in firearm suicides relative to the synthetic control for Connecticut's permit-to-purchase law enactment and a suggestive 24-percent increase in firearm suicides following Missouri's repeal of permit-to-purchase requirements. For both states, estimated effects on nonfirearm suicides showed uncertain associations.

Because both the Connecticut and Missouri analyses examined only a single state's experience with either adoption or repeal of such laws, both studies offer relatively 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 the completion of eight hours of gun-safety training. Similarly, Missouri's repeal occurred at the same time that the state implemented a stand-your-ground law. The studies' designs 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) and McCourt et al. (2020) may not be reliable indicators of the direction or magnitude of the true effects of permit-to-purchase laws on suicide.

Two studies included permit-to-purchase laws as a control variable in analyses focused on the causal effects of other types of firearm laws. Using data from 1970 through 2014, Luca, Malhotra, and Poliquin (2017) focused on waiting periods but included supplementary analyses presenting estimated coefficients for several other firearm policies, including handgun permit requirements. After adjusting for state and year fixed effects, state-level socioeconomic and demographic factors, 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. Examining the effects of youth-focused 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. Adjusting for state fixed effects, other firearm policies, and time-varying covariates, the authors found that permit-to-purchase laws significantly increased the total suicide rate by 17.7 percent among

those aged 18–20; this increase was driven by an estimated 22-percent increase in firearm suicides, and there was 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 they found such laws to have uncertain associations with firearm or total suicides among this age group. Because both of these studies' methods were designed to assess the causal effects of other laws, and permit requirements were a control variable, these findings have serious causal interpretation concerns (Hünermund and Louw, 2023; Westreich and Greenland, 2013). Furthermore, in the analysis of Webster et al. (2004), the effect estimate was based on changes to only three state laws during the study time frame, prompting serious concerns that apparent effects may be attributable to other concurrent changes that affected suicide rates.

Finally, Liu, Siegel, and Sen (2022) pursued a unique approach to assess the effects of permit-to-purchase requirements on firearm suicides in both the states that adopted such policies and nearby states. Using data from 2000 to 2019, the authors estimated a linear spatial autoregressive model to decompose policy effects into a direct effect on rates in the state adopting the policy and an indirect spillover effect on other states assumed to be inversely proportional to their distance from the adopting state. Considering the total effect (incorporating within-state and spillover effects), there was an uncertain association between permit requirements and firearm suicides, and there was substantial uncertainty around the estimates for both direct and indirect effects. However, fewer than three states changed their permit-to-purchase policies during the study period, which leads to serious concerns that estimates may reflect other contemporaneous events in these states rather than the effects of permitting laws alone.

Figure 9.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the licensing and permitting policies examined in these studies. We omit effect sizes from Liu, Siegel, and Sen (2022) because the spatial autoregressive model's estimands are fundamentally different from other studies reviewed, and thus their reported effect sizes are not directly comparable with other studies.

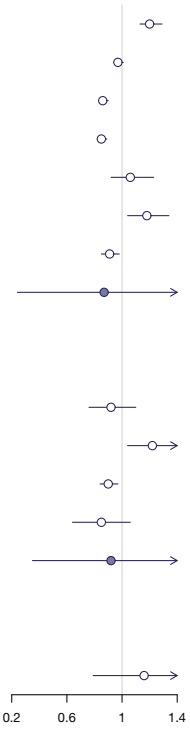
Conclusions

Excluding one study with critical methodological concerns, we identified seven 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 permit-to-purchase 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) identi-

FIGURE 9.1

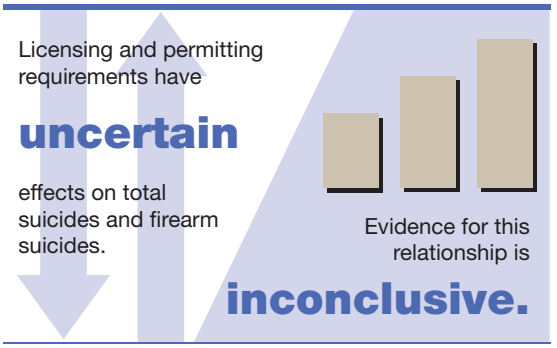
Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Suicide

Study, by Policy	Outcome Measure	IRR [95% CI]
Permit requirement	**Total suicide rate**	
Andrés & Hempstead (2011)	Men, aged 15–24	1.20 [1.13, 1.29]
Andrés & Hempstead (2011)	Men, aged 25–44	0.97 [0.94, 1.01]
Andrés & Hempstead (2011)	Men, aged 45–64	0.86 [0.83, 0.90]
Andrés & Hempstead (2011)	Men, aged 65+	0.85 [0.82, 0.89]
Webster et al. (2004)	Aged 14–17	1.06 [0.92, 1.23]
Webster et al. (2004)	Aged 18–20	1.18 [1.04, 1.34]
Luca, Malhotra, & Poliquin (2017)	Aged 21+	0.91 [0.85, 0.98]
Depew & Swensen (2022)	All ages	0.87 [0.24, 1.50]
Permit requirement	**Firearm suicide rate**	
Webster et al. (2004)	Aged 14–17	0.92 [0.76, 1.10]
Webster et al. (2004)	Aged 18–20	1.22 [1.04, 1.43]
Luca, Malhotra, & Poliquin (2017)	Aged 21+	0.90 [0.84, 0.97]
Crifasi et al. (2015)	All ages	0.85 [0.64, 1.06]
Depew & Swensen (2022)	All ages	0.92 [0.35, 1.48]
Repeal of permit law	**Suicide rate**	
Crifasi et al. (2015)	Firearm	1.16 [0.79, 1.54]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

fied 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. Applying similar methodology, McCourt et al. (2020) reported a suggestive decrease in firearm suicides in Connecticut after the enactment of the law and a suggestive increase in firearm suicides in Missouri after the repeal of the law. 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. In their analysis of laws from the early 20th century, Depew and Swensen (2022) found an uncertain relationship between requiring a license to own a handgun and both total and firearm suicide rates. Finally, Liu, Siegel, and Sen (2022) found an uncertain relationship between permit-to-purchase laws and firearm suicides.



Considering these studies and their methodological strengths and weaknesses, we find *inconclusive evidence for the effect of licensing and permitting requirements on total suicides and firearm suicides.*

Effects on Violent Crime

Research Synthesis Findings

Our synthesis identified 18 studies that examined the effects of permit-to-purchase laws on violent crime, six of which (Depew and Swensen, 2022; Doucette et al., 2021; Fridel, 2021a; Liu, Siegel, and Sen, 2022; Neufeld et al., 2022; Wallin, Holliday, and Zeoli, 2022) contribute new findings to this updated review.[19] One study (Rudolph et al., 2015) is rated as having critical methodological concerns because of having a single treated unit and few control units, so this study is not discussed further.

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 for all 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 following 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 that the law was repealed. Indeed, Hasegawa, Webster, and Small (2019) noted that Missouri's repeal occurred at the start of the Great Recession in 2007, which might have differentially affected trends in homicide rates based on preexisting homicide rate levels, potentially confounding estimated effects of repealing the firearm law. To partially

[19] One study (Neufeld et al., 2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Because there is evidence that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section. 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.

address this concern, the authors used a bracketing method that compared outcomes in Missouri after repeal of the law with (1) bordering states with higher pre-repeal firearm homicide rates and (2) bordering states with lower pre-repeal firearm homicide rates. Using this approach, the authors estimated that the repeal of Missouri's permit-to-purchase law significantly increased firearm homicides by between 17 and 27 percent. However, this selection of bracketing control groups does not obviate concerns that there were other changes in firearm homicide determinants that were specific to Missouri in 2007, or that time-varying factors after repeal in Missouri drove firearm homicide rate trends. Additionally, it is unclear from the limited pre-period data presented whether defining the upper and lower control groups using solely geographic contiguity and preexisting firearm homicide rate levels provided adequate bounding on Missouri's counterfactual firearm homicide rate trend.

One study used a synthetic control method to assess the effects of permit-to-purchase laws on homicides. Using data from 1985 to 2017, McCourt et al. (2020) estimated that Connecticut experienced a suggestive 28-percent decrease in firearm homicides relative to the synthetic control following law enactment; the effect on nonfirearm homicides was uncertain. Using a separate synthetic control analysis, they also examined the effects of Missouri's repeal of that state's permit-to-purchase requirement, estimating that the repeal led to a suggestive 47-percent increase in the firearm homicide rate and a suggestive 18-percent increase in the nonfirearm homicide rate. Because these analyses each had a single treated state, it is not possible to confidently attribute any changes in homicide rates to a single factor, like the change in one law; other changes in the affected states might also have contributed to observed homicide rate patterns. Furthermore, only two states (Iowa and North Carolina) contributed positive weight to the synthetic control for evaluating firearm homicides in Missouri, creating further concerns that changes in other factors (e.g., policies, economic outcomes) in either one of these states may have driven divergent firearm homicide trends.

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 assaults 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 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, but there was substantial uncertainty such that CIs spanned a vast range of plausible effect sizes (IRR = 2.14; 95-percent CI = [0.89, 5.14]). However, this study's results, which were based on law changes in two states analyzed separately, are subject to the same methodological concerns resulting from insufficient adjustments for potential confounders that could have 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 underestimated standard errors and unreliable CIs (Cameron, Gelbach, and Miller, 2008; Schell, Griffin, and Morral, 2018).

Seven state-level studies using state-fixed-effects models and more than 15 years of data produced estimates for the effects of permitting laws that showed an uncertain relationship with assaults or homicides. Specifically, Neufeld et al. (2022) examined effects on firearm injury hospitalizations; Gius (2017) examined the effects on firearm murder rates; Depew and Swensen (2022) examined effects on total homicide rates; Luca, Malhotra, and Poliquin (2017) examined the effects on total and firearm homicide rates among adults aged 21 or older; Zeoli et al. (2018) and Wallin, Holliday, and Zeoli (2022) examined the effects on total and firearm-specific intimate partner homicide rates; and Doucette, Crifasi, and Frattaroli (2019) examined the effects on the comparatively infrequent outcome of workplace firearm homicides. 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 regression specifications).

Knopov et al. (2019) also used state-level data to examine changes in homicides associated with permit-to-purchase requirements in states between 1991 and 2016. Using a log-linear model with two-way fixed effects; several social, economic, and crime covariates, including lagged firearm homicide rates; and several interaction terms, the authors modeled the effects of seven classes of laws on total and firearm homicides in 39 states. In these models, permit-to-purchase requirements were significantly associated with an 11-percent decrease in homicide rates and an 18-percent reduction in firearm homicides. These effects were based on just three states changing their laws, so the effects of permit-to-purchase laws per se may not be well identified in their models and standard errors are likely underestimated. Fridel (2021a) examined the same time frame but estimated a negative binomial regression model that controlled for state and year fixed effects, lagged total homicide rate, and a similar set of time-varying state-level covariates. While results showed that universal permitting requirements were associated with a suggestive reduction in homicide rates, permit laws were an ancillary covariate for an analysis focused on shall-issue laws; thus, these findings are subject to serious concerns with accurate causal interpretation.

Three studies (Crifasi et al., 2018b; Doucette et al., 2021; Siegel et al., 2020b) considered the effects of permit-to-purchase laws in urban areas specifically. 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. 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 11-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. These limitations threaten the validity of the CIs and reported significance of the effect estimates from the model.

Siegel et al. (2020b) used data from 1991 to 2016 to estimate the effects of state-level permit-to-purchase requirements on homicide rates within 197 medium-to-large U.S. cities (populations of at least 100,000 residents) and, separately, nonurban areas. Effects were estimated using log-linear models with year and state fixed effects, state social and economic covariates, and nine other firearm laws. Within larger cities, permit-to-purchase policies were associated with a significant 21-percent reduction in firearm homicides and no discernable effect on nonfirearm homicides, resulting in a significant 17-percent reduction in total homicides. In nonurban areas, there was a significant 20-percent reduction in firearm homicides and no discernable effect on nonfirearm homicides, resulting in a suggestive 11-percent reduction in total homicides. In subsequent tests, the effects in urban and nonurban areas were not significantly different from each other. In a study using data from 1999 to 2016 from 68 large metropolitan counties, Doucette et al. (2021) also found that permit-to-purchase policies were associated with a significant reduction in firearm homicides. However, the purpose of this study was to estimate the effects of ShotSpotter (a proprietary gunfire detection technology); permit-to-purchase laws were included as a control variable, so these findings are subject to serious concerns with accurate causal interpretation.

Finally, Liu, Siegel, and Sen (2022) adopted a unique approach to assess the effects of permit-to-purchase requirements on firearm homicides in both the states adopting such policies and nearby states. Evaluating data from 2000 to 2019, the authors used a linear spatial autoregressive model to decompose policy effects into a direct effect on rates in the state adopting the policy and an indirect spillover effect on other states that was assumed to be inversely proportional to their distance from the adopting state. The total effect showed a large and significant negative association between permit requirements and firearm homicides, which was a function of significant within-state reductions in firearm homicides and significant negative spillover effects to other states' firearm homicide rates.

With two exceptions (Gius, 2017; Depew and Swensen, 2022), all studies that met our inclusion criteria were subject to serious methodological concerns related to identifying policy effects from fewer than four policy transitions. Indeed, the fact that very few states have adopted or repealed licensing and permitting requirements over the past five decades represents an inherent limitation in trying to draw strong insights regarding the likely effects of this policy. Depew and Swensen (2022) were able to examine four policy transitions by focusing on the early 20th century, but this carries its own set of concerns in drawing generalizable insights to today's context. Similarly, the analysis of Gius (2017) identified effects from more policy transitions because a broader set of permitting-type policies were grouped together (e.g., Gius [2017] includes California's safety training certificate requirement, while other researchers do not). This may produce average effect estimates that mask important differences in the effectiveness of different permitting approaches, and, in the context of staggered adoption, grouping heterogeneous policies may even produce biased estimates of average policy effects.

Figure 9.2 displays the IRRs and CIs associated with the licensing and permitting policies examined in these studies. Because CIs could not be constructed for Knopov et al. (2019), no

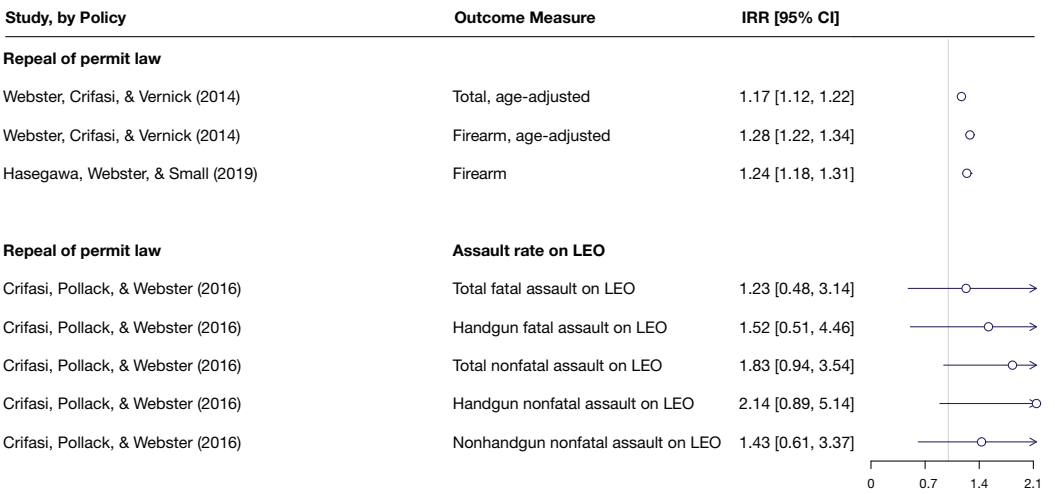
FIGURE 9.2

Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
Permit requirement	**Homicide rate**	
Knopov et al. (2019)	Total, age-adjusted	0.89 [0.80, 0.98]
Depew & Swensen (2022)	Total	1.16 [0.73, 1.60]
Fridel (2021a)	Total	0.88 [0.75, 1.02]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.05 [0.88, 1.26]
Siegel et al. (2020b)	Total, mid to large cities	0.83 [0.78, 0.88]
Siegel et al. (2020b)	Total, nonurban areas	0.89 [0.78, 1.00]
Gius (2017)	Firearm, all ages	0.95 [0.62, 1.12]
McCourt et al. (2020)	Firearm, all ages	0.72 [0.44, 1.00]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	1.09 [0.85, 1.39]
Doucette et al. (2021)	Firearm, large metro counties	0.84 [0.72, 0.99]
Siegel et al. (2020b)	Firearm, mid to large cities	0.79 [0.72, 0.86]
Siegel et al. (2020b)	Firearm, nonurban areas	0.80 [0.69, 0.93]
Permit requirement	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm injury	0.98 [0.86, 1.11]
Permit requirement	**Intimate partner homicide rate**	
Zeoli et al. (2018)	Total	1.04 [0.85, 1.28]
Wallin, Holliday, & Zeoli (2022)	Total, White	1.05 [0.88, 1.25]
Wallin, Holliday, & Zeoli (2022)	Total, Black	1.03 [0.77, 1.36]
Zeoli et al. (2018)	Firearm	1.06 [0.83, 1.37]
Wallin, Holliday, & Zeoli (2022)	Firearm, White	1.07 [0.92, 1.25]
Wallin, Holliday, & Zeoli (2022)	Firearm, Black	1.16 [0.66, 2.01]
Permit requirement	**Assault rate on LEO**	
Crifasi, Pollack, & Webster (2016)	Total fatal assault on LEO	0.45 [0.10, 2.08]
Crifasi, Pollack, & Webster (2016)	Handgun fatal assault on LEO	0.20 [0.02, 1.90]
Permit requirement	**Workplace homicide rate**	
Doucette, Crifasi, & Frattaroli (2019)	Total	0.98 [0.81, 1.18]
Permit to purchase or comprehensive background check	**Homicide rate**	
Crifasi et al. (2018b)	Firearm, urban areas	0.89 [0.85, 0.93]

Figure continued on the next page

Figure 9.2—Continued

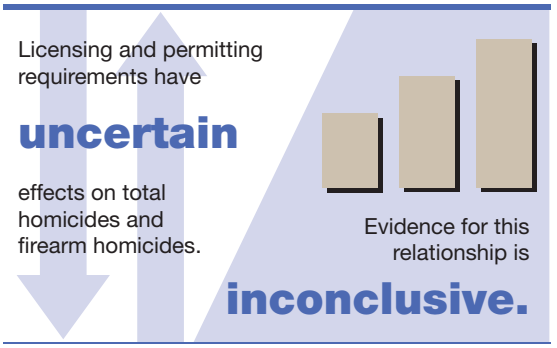


NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details. LEO = law enforcement officer.

estimate is shown for firearm homicides from it. We also omit effect sizes from Liu, Siegel, and Sen (2022) because the spatial autoregressive model's estimands are fundamentally different from other studies reviewed in this report, making their reported effect sizes not comparable.

Conclusions

Excluding one study with critical methodological concerns, we identified nine qualifying studies examining the effects of permit-to-purchase laws on total homicides and 14 studies examining these effects on firearm homicides or assaults. Most focused on the unique experiences of one or two states. Webster, Crifasi, and Vernick (2014) found that Missouri's repeal of its law resulted in significantly increased total and firearm homicide rates. Hasegawa, Webster, and Small (2019) also reported evidence that the repeal of Missouri's permit-to-purchase law was associated with a significant increase in firearm homicides. McCourt et al. (2020) found a suggestive increase in firearm homicides from the repeal of Missouri's permit-to-purchase law and a suggestive decrease in firearm homicides from Connecticut's adoption of such a law. 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. Eight other studies found uncertain effects of permitting laws on firearm murder rates (Gius, 2017), firearm injury hospitalizations (Neufeld et al., 2022), total or firearm homicide rates (Depew and Swensen, 2022; Fridel, 2021a; Luca, Malhotra, and Poliquin, 2017), total and firearm intimate partner homicide rates (Wallin, Holliday, and Zeoli, 2022;



Zeoli et al., 2018), and workplace firearm homicide rates (Doucette, Crifasi, and Frattaroli, 2019). Finally, Knopov et al. (2019) found a significant reduction in both firearm and total homicides associated with implementation of the law in the small number of states that did so over the study period; Crifasi et al. (2018b) and Doucette et al. (2021) reported a large reduction in firearm homicides in urban counties or large metropolitan counties, respectively, after the passage of permit-to-purchase laws; Fridel (2021a) found that universal permitting requirements had suggestive effects consistent with reducing total homicides; and Siegel et al. (2020b) found that permit-to-purchase laws reduced total homicides in urban areas and reduced firearm homicides in both urban and nonurban areas.

Together, these studies provide compelling evidence that permit-to-purchase laws are associated with declines in firearm and total homicides. However, because none of the studies that found significant effects had estimates based on more than three states having changed their laws, our a priori scoring rule for rating the quality of available evidence leads us to conclude that *evidence remains inconclusive for the effect of permit-to-purchase laws on firearm and total homicides*. The reason for this scoring rule is that estimates that are based on very few treated units are well known to be unreliable indicators of how effects might generalize beyond the small sample of states that have changed this law. Therefore, although we believe that there is evidence that permit-to-purchase laws were associated with reductions in total and firearm homicides in three states, we are not yet confident that other states can expect similar results based on the available evidence.

Effects on Mass Shootings

Research Synthesis Findings

Our search yielded five studies that met our inclusion criteria and examined the effects of licensing and permitting requirements on mass shootings, two of which (Fridel, 2021a; Siegel et al., 2020a) contribute new findings to this updated review. Two studies (Blau, Gorry, and Wade, 2016; Luca, Malhotra, and Poliquin, 2020) are subject to critical methodological concerns related to model overfit (fewer than three events per estimated parameter) and thus are not discussed further.

Using data from 1984 to 2017, Webster et al. (2020) estimated negative binomial regression models to evaluate the effects of 14 different firearm laws, including in-person licensing or fingerprint requirements, on mass shootings involving four or more firearm fatalities (excluding offender fatalities and drug- or gang-related shootings). Controlling for state

fixed effects, linear and quadratic time trends, and 14 social and economic state covariates, the authors found that in-person licensing or fingerprint requirements were associated with 56 percent fewer mass shooting incidents and 57 percent fewer deaths. These effects remained statistically significant (and were of larger magnitude) when restricted to non–domestic-related incidents, while effects were more uncertain for domestic-related incidents. However, as with most empirical studies of mass shootings, incidents and fatalities were quite sparsely represented in the data used for these models (i.e., incidents occurred at a rate of less than 0.4 per state-year, and fewer than two deaths occurred per state-year). Without adjustments for known biases in maximum likelihood estimates using sparse data, the resulting effect estimates and CIs may be biased and unreliable, particularly given the relatively small number of policy changes used to identify effects (Kenne Pagui, Salvan, and Sartori, 2022).

Siegel et al. (2020a) evaluated a slightly longer time frame (1976 to 2018) and focused on the relationship of permit-to-purchase laws with *mass public shooting incidents*, defined as incidents unconnected to other criminal activity in which four or more victims were fatally shot in a public location. Regression models controlled for linear and quadratic national time trends and 13 time-varying state-level characteristics, including population, demographic composition, ratio of firearm suicides to suicides, age-adjusted firearm homicide rate, and several other firearm laws. Estimates from logistic regressions showed that permit-to-purchase laws had significant negative associations with the likelihood of any mass public shooting incident occurring; similar findings are shown in zero-inflated negative binomial models. In addition to concerns with sparse outcome data, these results are subject to minor concerns related to having fewer than ten observations per parameter and the absence of discussion around identifying assumptions (e.g., the extent to which estimated effects reflect across-state versus within-state variation).

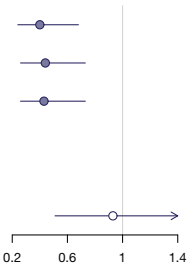
Finally, Fridel (2021a) evaluated the shorter time frame of 1991 to 2016 and assessed the relationship between universal permitting requirements and rates of *mass murder*, defined as an incident with four or more fatalities (excluding the perpetrator) within a 24-hour period. The author used negative binomial models that employed generalized estimating equations and included region-level fixed effects, year fixed effects, controls for other firearm policies, and a large set of state-level time-varying covariates (including a proxy for firearm ownership, socioeconomic factors, and crime rates). Results showed that permitting requirements had uncertain effects. However, the purpose of this study was to evaluate shall-issue laws; because other firearm laws were intended as ancillary covariates, these results are subject to serious methodological concerns with drawing causal conclusions.

Figure 9.3 displays the IRRs and CIs associated with the licensing and permitting policies examined in these studies.

FIGURE 9.3

Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
Permit-to-purchase requirement	**Mass shooting**	
Siegel et al. (2020a)	Any public incident	0.40 [0.24, 0.68]
Webster et al. (2020)	Number of incidents	0.44 [0.26, 0.73]
Webster et al. (2020)	Number of fatalities	0.43 [0.26, 0.73]
Permit-to-purchase requirement	**Mass murder**	
Fridel (2021a)	Number of incidents	0.93 [0.51, 1.68]



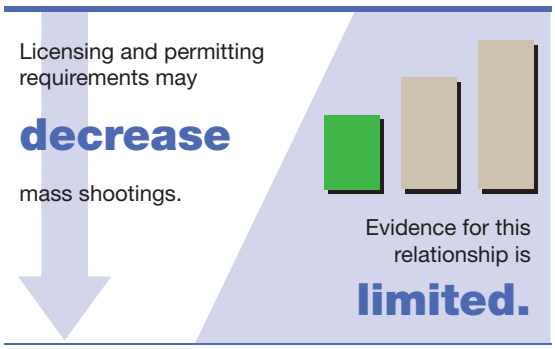
NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

Conclusions

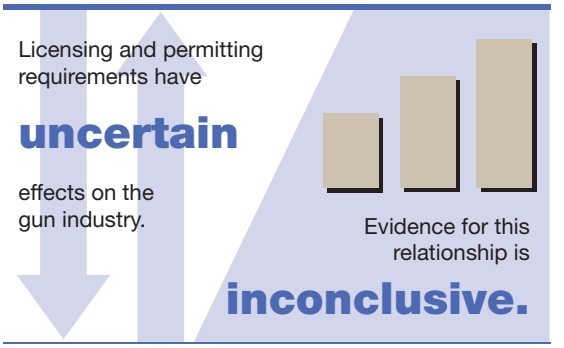
Excluding two studies with critical methodological concerns, we identified three qualifying studies that estimated the effects of licensing and permitting laws on mass shootings or mass murders. Two studies (Siegel et al., 2020a; Webster et al., 2020) found statistically significant negative relationships between permit-to-purchase laws and mass shooting outcomes (i.e., the likelihood of any mass public shooting occurring in a state, the number of mass shooting incidents or fatalities). One study (Fridel, 2021a) found an uncertain relationship between permit requirements and mass murder incidence rates. Considering this evidence and an evaluation of the studies' strengths, we find *limited evidence that licensing and permitting requirements may reduce mass shootings.*

Effects on the Gun Industry

Research Synthesis Findings



We identified two studies that met our inclusion criteria and examined how permit-to-purchase laws might affect firearm industry interests. One of these (Brauer, Montolio, and Trujillo-Baute, 2017) used a policy index, measured from 1986 to 1999, that captured the stringency of state laws regarding background checks, firearm registration requirements, and permit-to-purchase



requirements to examine how these laws affected variation in the number of firearm manufacturing plants in a state. Based on results from both linear and Poisson regression models with state and year fixed effects, the authors found suggestive effects that were consistent with more-stringent background check requirements reducing the number of firearm manufacturing plants in a state. However, the authors admitted serious challenges with the coding of their policy index measure, and they did not provide sufficient information to clarify the type of policy variation that is used to identify effects.

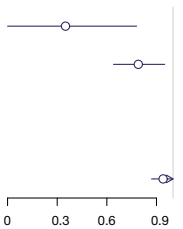
Steidley (2019) examined the effect of permit-to-purchase laws on the number of National Instant Criminal Background Check (NICS) background checks performed in each state from 1999 to 2016. This study used a linear model of NICS background checks per capita, controlling for state fixed effects, several economic and political covariates, and three other gun laws. Although the study reported significant effects of permit-to-purchase laws, indicating that such laws reduce the number of background checks for handguns by 65 percent and for long guns by 21 percent, only one state (Missouri) changed its permit-to-purchase law over the study period, meaning that the effect of these laws was not well identified. Moreover, because the model did not control for time trends, it is unclear whether the changes associated with Missouri's repeal of its permit law are properly attributed to the law or to broader temporal trends in background checks.

Figure 9.4 displays the IRRs and CIs associated with the permit-to-purchase laws examined in Steidley (2019) and Brauer, Montolio, and Trujillo-Baute (2017).

FIGURE 9.4

Incidence Rate Ratios Associated with the Effect of Licensing and Permitting Requirements on the Gun Industry

Study, by Policy	Outcome Measure	IRR [95% CI]
Permit-to-purchase requirement for handguns	**NICS background checks**	
Steidley (2019)	Handguns	0.35 [0.00, 0.78]
Steidley (2019)	Long guns	0.79 [0.64, 0.95]
Background check and permitting index		
Brauer, Montolio, and Trujillo-Baute (2017)	Number of firearm manufacturers	0.94 [0.87, 1.01]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. See Appendix B for details.

Conclusions

Because of the methodological concerns with the studies we identified examining permit-to-purchase requirements' effects on 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
- police shootings
- defensive gun use
- hunting and recreation.

Chapter 9 References

Aldrich, John H., and Forrest D. Nelson, *Linear Probability, Logit, and Probit Models*, Sage Publications, 1984.

Andrés, Antonio Rodríguez, and Katherine Hempstead, "Gun Control and Suicide: The Impact of State Firearm Regulations in the United States, 1995–2004," *Health Policy*, Vol. 101, No. 1, 2011.

Azrael, Deborah, Lisa Hepburn, David Hemenway, and Matthew Miller, "The Stock and Flow of U.S. Firearms: Results from the 2015 National Firearms Survey," *Russell Sage Foundation Journal of the Social Sciences*, Vol. 3, No. 5, 2017.

Berrigan, John, Deborah Azrael, David Hemenway, and Matthew Miller, "Firearms Training and Storage Practices Among US Gun Owners: A Nationally Representative Study," *Injury Prevention*, Vol. 25, Supp. 1, 2019.

Bhatt, Apurva, Xi Wang, An-Lin Cheng, Kalee L. Morris, Luke Beyer, Abbie Chestnut, Kristy Steigerwalt, and Jeffrey Metzner, "Association of Changes in Missouri Firearm Laws with Adolescent and Young Adult Suicides by Firearms," *JAMA Network Open*, Vol. 3, No. 11, November 2020.

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Braga, Anthony A., Garen J. Wintemute, Glenn L. Pierce, Philip J. Cook, and Greg Ridgeway, "Interpreting the Empirical Evidence on Illegal Gun Market Dynamics," *Journal of Urban Health*, Vol. 89, No. 5, 2012.

Brauer, Jurgen, Daniel Montolio, and Elisa Trujillo-Baute, "How Do US State Firearms Laws Affect Firearms Manufacturing Location? An Empirical Investigation, 1986–2010," *Journal of Economic Geography*, Vol. 17, No. 4, 2017.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, Vol. 90, No. 3, 2008.

Collins, T., R. Greenberg, M. Siegel, Z. Xuan, E. F. Rothman, S. W. Cronin, and D. Hemenway, "State Firearm Laws and Interstate Transfer of Guns in the USA, 2006–2016," *Journal of Urban Health*, Vol. 95, No. 3, June 2018.

Cox, D. R., and E. J. Snell, *Analysis of Binary Data*, 2nd ed., Chapman and Hall, 1989.

Crifasi, Cassandra K., Shani A. L. Buggs, Seema Choksy, and Daniel W. Webster, "The Initial Impact of Maryland's Firearm Safety Act of 2013 on the Supply of Crime Handguns in Baltimore," *Russell Sage Foundation Journal of the Social Sciences*, Vol. 3, No. 5, 2017.

Crifasi, Cassandra K., Molly Merrill-Francis, Alex McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster, "Association Between Firearm Laws and Homicide in Urban Counties," *Journal of Urban Health*, Vol. 95, No. 3, 2018b.

Crifasi, C. K., J. S. Meyers, J. S. Vernick, and D. W. Webster, "Effects of Changes in Permit-to-Purchase Handgun Laws in Connecticut and Missouri on Suicide Rates," *Preventive Medicine*, Vol. 79, 2015.

Crifasi, Cassandra K., Keshia M. Pollack, and Daniel W. Webster, "Effects of State-Level Policy Changes on Homicide and Nonfatal Shootings of Law Enforcement Officers," *Injury Prevention*, Vol. 22, No. 4, 2016.

Crifasi, Cassandra K., Elizabeth M. Stone, Beth McGinty, Jon S. Vernick, Colleen L. Barry, and Daniel W. Webster, "Differences in Public Support for Handgun Purchaser Licensing," *Injury Prevention*, Vol. 26, No. 1, 2020.

Csere, M., *State Comparison of Gun Permit Fees*, Connecticut General Assembly, Office of Legislative Research, OLR Research Report 2013-R-0048, 2013.

Depew, Briggs, and Isaac Swensen, "The Effect of Concealed-Carry and Handgun Restrictions on Gun-Related Deaths: Evidence from the Sullivan Act of 1911," *Economic Journal*, Vol. 132, No. 646, August 2022.

Doucette, Mitchell L., Cassandra K. Crifasi, and Shannon Frattaroli, "Right-to-Carry Laws and Firearm Workplace Homicides: A Longitudinal Analysis (1992–2017)," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Doucette, Mitchell L., Christa Green, Jennifer Necci Dineen, David Shapiro, and Kerri M. Raissian, "Impact of ShotSpotter Technology on Firearm Homicides and Arrests Among Large Metropolitan Counties: A Longitudinal Analysis, 1999–2016," *Journal of Urban Health*, Vol. 98, No. 5, October 2021.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Gius, Mark, "Effects of Permit-to-Purchase Laws on State-Level Firearm Murder Rates," *Atlantic Economic Journal*, Vol. 45, No. 1, 2017.

Hasegawa, Raiden B., Daniel W. Webster, and Dylan S. Small, "Evaluating Missouri's Handgun Purchaser Law: A Bracketing Method for Addressing Concerns About History Interacting with Group," *Epidemiology*, Vol. 30, No. 3, May 2019.

Hemenway, David, Sara J. Solnick, and Deborah R. Azrael, "Firearm Training and Storage," *JAMA*, Vol. 273, No. 1, 1995.

Hünermund, Paul, and Beyers Louw, "On the Nuisance of Control Variables in Regression Analysis," *Organizational Research Methods*, December 25, 2023, online ahead of print.

Kahane, Leo H., "State Gun Laws and the Movement of Crime Guns Between States," *International Review of Law and Economics*, Vol. 61, 2020.

Kenne Pagui, Euloge Clovis, Alessandra Salvan, and Nicola Sartori, "Improved Estimation in Negative Binomial Regression," *Statistics in Medicine*, Vol. 41, No. 13, 2022.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Koenig, Christoph, and David Schindler, "Impulse Purchases, Gun Ownership, and Homicides: Evidence from a Firearm Demand Shock," *Review of Economics and Statistics*, Vol. 105, No. 5, September 2023.

Krouse, W. J., *Gun Control: Statutory Disclosure Limitations on ATF Firearms Trace Data and Multiple Handgun Sales Reports*, Congressional Research Service, 7-5700, 2009.

La Valle, James M., "'Gun Control' vs. 'Self-Protection': A Case Against the Ideological Divide," *Justice Policy Journal*, Vol. 10, No. 1, 2013.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Li, Michelle, Dylan Small, Ting Ye, Yuzhou Lin, and Daniel Webster, "Examining a Hypothesized Causal Chain for the Effects of the 2007 Repeal of the Permit-to-Purchase Licensing Law in Missouri: Homicide Guns Recovered in State and Within a Year of Purchase," *Journal of Urban Health*, Vol. 100, No. 3, June 2023.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

McCourt, Alexander D., Cassandra K. Crifasi, Elizabeth A. Stuart, Jon S. Vernick, Rose M. C. Kagawa, Garen J. Wintemute, and Daniel W. Webster, "Purchaser Licensing, Point-of-Sale Background Check Laws, and Firearm Homicide and Suicide in 4 US States, 1985–2017," *American Journal of Public Health*, Vol. 110, No. 10, October 2020.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Rudolph, K. E., E. A. Stuart, J. S. Vernick, and D. W. Webster, "Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides," *American Journal of Public Health*, Vol. 105, No. 8, 2015.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Siegel, Michael, Benjamin Solomon, Anita Knopov, Emily F. Rothman, Shea W. Cronin, Ziming Xuan, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates in Suburban and Rural Areas Compared to Large Cities in the United States, 1991–2016," *Journal of Rural Health*, Vol. 36, No. 2, March 2020b.

Smucker, Sierra, Max Griswold, Amanda Charbonneau, Rose Kerber, Terry L. Schell, and Andrew R. Morral, *Using National Instant Criminal Background Check Data for Gun Policy Analysis: A Discussion of Available Data and Their Limitations*, RAND Corporation, TL-A243-4, 2022. As of October 1, 2022:
https://www.rand.org/pubs/tools/TLA243-4.html

Steidley, Trent, "The Effect of Concealed Carry Weapons Laws on Firearm Sales," *Social Science Research*, Vol. 78, February 2019.

Tita, G. E., A. A. Braga, G. Ridgeway, and G. L. Pierce, "The Criminal Purchase of Firearm Ammunition," *Injury Prevention*, Vol. 12, No. 5, 2006.

U.S. General Accounting Office, *Firearms Purchased from Federal Firearm Licensees Using Bogus Identification*, GAO-01-427NI, 2001.

Wallin, Mikaela A., Charvonne N. Holliday, and April M. Zeoli, "The Association of Federal and State-Level Firearm Restriction Policies with Intimate Partner Homicide: A Re-Analysis by Race of the Victim," *Journal of Interpersonal Violence*, Vol. 37, Nos. 17–18, September 2022.

Webster, D., C. K. Crifasi, and J. S. Vernick, "Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides," *Journal of Urban Health*, Vol. 91, No. 2, 2014.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Webster, D. W., J. S. Vernick, A. M. Zeoli, and J. A. Manganello, "Association Between Youth-Focused Firearm Laws and Youth Suicides," *JAMA*, Vol. 292, No. 5, 2004.

Webster, D. W., and G. J. Wintemute, "Effects of Policies Designed to Keep Firearms from High-Risk Individuals," *Annual Review of Public Health*, Vol. 36, 2015.

Westreich, Daniel, and Sander Greenland, "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients," *American Journal of Epidemiology*, Vol. 177, No. 4, February 15, 2013.

Zeoli, April M., Alexander McCourt, Shani Buggs, Shannon Frattaroli, David Lilley, and Daniel W. Webster, "Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Associations with Intimate Partner Homicide," *American Journal of Epidemiology*, Vol. 187, No. 11, 2018.

CHAPTER 10

Waiting Periods

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 9 percent of background checks, the NICS check requires supplementary reviews (Criminal Justice Information Services Division, 2023), and federal law allows the Federal Bureau of Investigation (FBI) up to three days to complete these reviews (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 a background check 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. In keeping with this theory, Koenig and Schindler (2023) found that states with firearm purchase delays saw fewer purchases after the "demand shock" of the Sandy Hook Elementary School shooting in 2012 and that these states also saw fewer homicides after the demand shock. In addition, 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 suicide, waiting periods may still reduce total rates of suicide because of the high case-fatality ratio of firearms when 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 attempting suicides rather than preventing it. Evidence from a cohort of handgun purchasers in California found that, although 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 during the first month of purchase (Wintemute et al., 1999). A more recent analysis of handgun owners in California found that the risk of firearm suicide was highest immediately following the end of the state's ten-day waiting period (i.e., 11 to 30 days after the purchase application), but more than half of all firearm suicides among handgun owners occurred more than one year after the purchase application (Studdert et al., 2020). 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) because, presumably, it is easier 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 ATF appropriations (commonly known as the Tiahrt Amendments) has denied most researchers access to detailed firearm trace data since 2003, making it infeasible to conduct this type of analysis (Krouse, 2009). Some researchers have analyzed aggregate statistics on crime guns published by the ATF, and this research suggests that waiting-period requirements may be effective in preventing the diversion of firearms from within-state retailers to criminal markets (Kahane, 2020; Collins et al., 2018).

Third, waiting-period laws 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 2022, for instance, 4,112 firearms were confirmed to be transferred from federally licensed firearm dealers to prohibited persons because of delays in NICS background checks that exceeded three business days (Criminal Justice Information Services

[1] The Bureau of Alcohol, Tobacco, and Firearms (ATF) (2002, p. A-3) defined a *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."

Division, 2023). Often, these delays result from missing data in the NICS databases that must be manually tracked down by investigators (e.g., if final disposition of a case is not noted). A review of 2022 data found that an additional 0.9 percent of all background checks, or about 290,000, could not be completed within 90 days and were thus purged from the NICS review system by law without determinations about whether the buyers were prohibited possessors (Criminal Justice Information Services Division, 2023). When a buyer is determined to have been a prohibited possessor and has taken possession of a firearm, the NICS alerts the 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 new weapons, thereby increasing the effectiveness of background check laws in limiting firearm access by prohibited possessors who are considered to present an elevated risk of violence. As discussed in Chapter 8, 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.

It is unclear whether extending the time to complete background checks would reduce mass shootings. An analysis of the sources of 377 firearms used in 169 mass shootings between 1966 and 2019 for which information on the source of the firearm used was available found one instance (0.6 percent) in which the shooter acquired a firearm used in the assault because the background check could not be completed in three business days (Peterson and Densley, 2021, p. 163). 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, information on how the firearms were obtained was unavailable for nearly one-third of the incidents.

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; thus, by extension, waiting periods could reduce gun sales. Moreover, these 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. 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, Studdert et al., 2020; 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 and 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, 2024, six states and the District of Columbia impose a waiting period to purchase any firearm.[3] Four other states impose waiting periods for certain classes of firearms.[4] States with permit-to-purchase laws impose a de facto waiting period, depending on how long it takes the state to provide a permit. 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 new firearms.[5] In Hawaii, buyers must wait 14 days to receive a permit to purchase firearms.[6] Other states impose shorter waiting periods.[7]

In addition to the aforementioned states, several others with licensing procedures, such as requiring permits to purchase or possess firearms, impose de facto waiting periods because the process for acquiring a license can take days or weeks to complete (see Chapter 9).

---

[3] California, Colorado, Florida, Hawaii, Illinois, Rhode Island, and the District of Columbia. See Calif. Penal Code § 27540; Colo. Rev. Stat. § 18-12-115; Fla. Ch. 790, Sec. 0655; Hawaii Rev. Stat. Ann. § 134-2(e) (1991); 720 Ill. Comp. Stat. 5/24-3(A)(g); R.I. Gen. Laws § 11-47-35, 35.2; D.C. Code § 22-4508.

[4] Maryland (handguns), Minnesota (handguns and semiautomatic rifles), New Jersey (handguns), and Washington (handguns and semiautomatic rifles). See Fla. Stat. § 790.0655(1)(a); Md. Code Ann., Pub. Safety, § 5-124(a); Minn. Stat. Ann. §624.7132, subd. 4; N.J. Stat. Ann. § 2C:58-2a(5)(a); Wash. Rev. Code Ann. § 9.41.092.

[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 and Colorado's waiting periods are three days, and Illinois's waiting period is three days for handguns and one day for long guns. See Fla. Stat. § 790.0655(1)(a); Colo. Rev. Stat. § 18-12-115; 720 Ill. Comp. Stat. 5/24-3(A)(g).

Effects on Suicide

Research Synthesis Findings

We identified six studies that met our inclusion criteria and assessed how waiting-period laws influenced suicide, two of which (Kester et al., 2023; Oliphant, 2022) contribute new findings to this updated review. Both new studies and one previously reviewed study (Anestis and Anestis, 2015) were subject to critical methodological concerns, so those studies will not be discussed further in this review.[8]

Ludwig and Cook (2000) 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). When examining the Brady Act overall (i.e., examining joint effects of the background check and waiting-period requirements), Ludwig and Cook (2000) found uncertain effects of the policy on total suicides, firearm suicides, and the proportion of adult suicides caused by a firearm. Effects mostly remained uncertain in analyses stratified by whether the state did or did not have a change in waiting period requirements as a result of the Brady Act, with the exception of suggestive declines in firearm suicide rates among adults in states that introduced waiting periods. A subgroup analysis also 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. However, the paper did not provide estimates that would demonstrate that the difference between these rate reductions was statistically significant.

Two subsequent 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

[8] In their analysis of waiting period laws' effects on suicides, Anestis and Anestis (2015) did not test the statistical significance of their parameter estimates or adjust for any characteristics that may confound the association between waiting-period laws and suicide rates. This descriptive evidence provides limited causal insights, and we thus discount the findings of this study. Kester et al. (2023) had critical methodological concerns related to drawing causal conclusions from models that did not adjust for potential state-level or year-level confounders. Subsequent multivariable models were used to evaluate other policies' effects on firearm suicide rates, but they excluded waiting-period laws because of their nonsignificant association with the outcome. Finally, Oliphant's (2022) synthetic control approach predicting total suicide rates and firearm suicide rates had critical risks of bias because only three control states contributed weight to the synthetic control. Moreover, the models used the ratio of firearm suicides to total suicides as a measure of gun ownership rates in the same year as the outcome. In effect, the outcomes were used as predictors of themselves, which is a critical issue.

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.[9] 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 the authors' 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.

Figure 10.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the waiting-period policies examined in these studies.

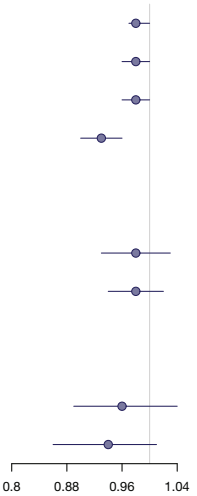
Conclusions

Excluding three studies with critical methodological concerns, we identified three studies that provide some evidence of the effects of waiting-period requirements on suicide (Edwards et al., 2018; Luca, Malhotra, and Poliquin, 2017; Ludwig and Cook, 2000). Two 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. One study (Ludwig and Cook, 2000) found that states that implemented waiting-period requirements (alongside potential changes in background check requirements) as a result of the Brady Act had suggestive declines in firearm suicide rates among adults. Given that these studies (1) used highly similar model specifications and the same data set and (2) analyzed overlapping time

[9] 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.

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

Study, by Policy	Outcome Measure	IRR [95% CI]
Handgun purchase delay	**Suicide rate**	
Edwards et al. (2018)	Total	0.98 [0.97, 1.00]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	0.98 [0.96, 1.00]
Edwards et al. (2018)	Firearm	0.98 [0.96, 1.00]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	0.93 [0.90, 0.96]
Brady Act		
Ludwig & Cook (2000)	Total, aged 21+	0.98 [0.93, 1.03]
Ludwig & Cook (2000)	Firearm, aged 21+	0.98 [0.94, 1.02]
Brady Act (with waiting period change)		
Ludwig & Cook (2000)	Total, aged 21+	0.96 [0.89, 1.04]
Ludwig & Cook (2000)	Firearm, aged 21+	0.94 [0.86, 1.01]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

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.*

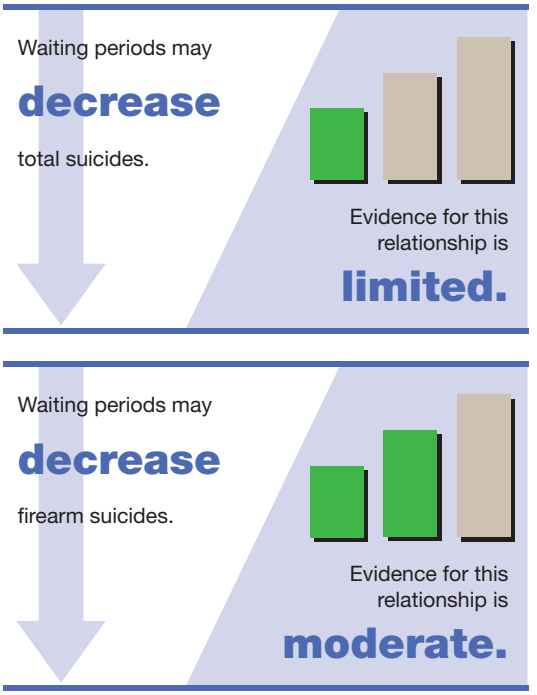
Effects on Violent Crime

Research Synthesis Findings

We identified nine studies that met our inclusion criteria and provided evidence for the effects of waiting periods on violent crime, one of which (Fridel, 2021a) contributes new findings to this updated review. We also now discuss results from two previously identified studies within this chapter, and we discount the findings from one previously identified study because of critical methodological concerns.[10]

Ludwig and Cook (2000) examined changes in homicide rates before and after the implementation of the Brady Act in 1994, which initially imposed waiting periods and background

[10] We now discuss the Brady Act results from Monroe (2008), previously only discussed in Chapter 8, in this chapter because the policy variation used in that study involved changes in both background check and waiting period requirements. We also now include the results from Lott and Whitley (2001) on waiting-period laws, although these results represent estimates from ancillary covariates and thus are subject to serious interpretational issues. Finally, we now discount and do not discuss further the results from Mustard (2001) because of critical concerns with model overfit (i.e., fewer than five events per estimated parameter).



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). When examining the Brady Act overall (i.e., examining joint effects of the background check and waiting period requirements), Ludwig and Cook (2000) found uncertain effects of the policy on total homicides, firearm homicides, and the proportion of adult homicides caused by a firearm. Effects remained uncertain in analyses that were stratified by whether the state did or did not have a change in waiting period requirements because of the Brady Act. However, the paper did not report whether differences in the effects for states with versus without waiting period changes were statistically significant.

Another study (Monroe, 2008) used similar identifying variation based on states that implemented both background check and waiting requirements following the Brady Act. The author adopted a triple difference-in-difference-in-differences design, exploiting differential effects of the Brady Act across states and over time and expected differential effects by age group, because juveniles under age 21 were already prohibited from purchasing a handgun. Results showed uncertain effects of the Brady Act on total and handgun-specific homicide rates, but the authors found significant effects consistent with the act reducing firearm homicides that involved guns other than handguns.

We identified six studies that specifically examined the effect of waiting periods on violent crime. 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 lowered 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.

Five other studies assessed the associations of waiting-period laws with homicide or violent crime rates. Hepburn et al. (2004) used data from 1979 to 1998 and estimated negative binomial models that were 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 a similar specification with more-recent data from 1991 to 2016, Fridel (2021a) instead found that waiting period requirements were associated with significant increases in total homicide rates. Instead estimating a two-way fixed-effects linear regression with a logged outcome, Lott and Whitley (2001) found largely uncertain evidence for the relationship of waiting-period requirements with violent crime rates, although their use of multiple parameters to characterize waiting-period policies (without subsequent integration) complicates drawing policy conclusions. However, given that all three of these studies are focused on identifying the causal effects of other firearm laws (shall-issue laws or child-access prevention laws) and include waiting-period laws as control variables, these findings have serious causal interpretation concerns (Hünermund and Louw, 2023; Westreich and Greenland, 2013).

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 Lott and Whitley (2001) because insufficient information was provided to integrate estimates across the multiple parameters used to characterize waiting-period requirements.

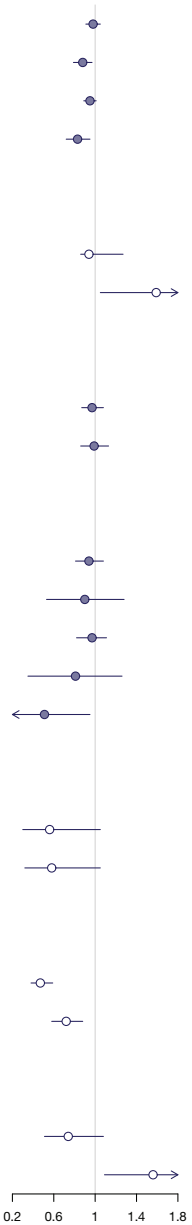
Conclusions

Total homicides and violent crime. Excluding one study with critical methodological concerns, we identified eight qualifying studies that examined the effects of waiting periods on homicide rates. Based on analyses of the Brady Act that cannot disentangle the effects of background checks from those of waiting-period requirements, two studies found uncertain effects (Ludwig and Cook, 2000; Monroe, 2008). Examining changes to waiting-period

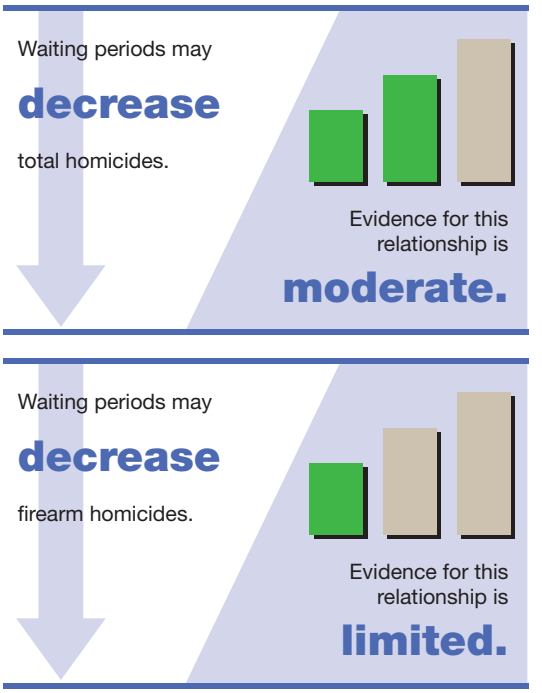
FIGURE 10.2

Incidence Rate Ratios Associated with the Effect of Waiting Periods on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
Handgun purchase delay	**Homicide rate**	
Edwards et al. (2018)	Total	0.98 [0.91, 1.05]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	0.88 [0.79, 0.97]
Edwards et al. (2018)	Firearm	0.95 [0.89, 1.01]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	0.83 [0.72, 0.95]
Waiting-period law		
Hepburn et al. (2004)	Total	0.94 [0.86, 1.27]
Fridel (2021a)	Total	1.59 [1.05, 2.39]
Brady Act		
Ludwig & Cook (2000)	Total, aged 21+	0.97 [0.87, 1.08]
Ludwig & Cook (2000)	Firearm, aged 21+	0.99 [0.86, 1.13]
Brady Act (with waiting period change)		
Ludwig & Cook (2000)	Total, aged 21+	0.94 [0.81, 1.08]
Monroe (2008)	Total, aged 21+	0.90 [0.53, 1.28]
Ludwig & Cook (2000)	Firearm, aged 21+	0.97 [0.82, 1.11]
Monroe (2008)	Handgun, aged 21+	0.81 [0.35, 1.26]
Monroe (2008)	Non-handgun firearm, aged 21+	0.51 [0.08, 0.95]
24-hour waiting period		
Roberts (2009)	Total, intimate partner	0.56 [0.30, 1.05]
Roberts (2009)	Firearm, intimate partner	0.58 [0.32, 1.05]
2–7 day waiting period		
Roberts (2009)	Total, intimate partner	0.47 [0.38, 0.59]
Roberts (2009)	Firearm, intimate partner	0.72 [0.58, 0.88]
>1 week waiting period		
Roberts (2009)	Total, intimate partner	0.74 [0.51, 1.08]
Roberts (2009)	Firearm, intimate partner	1.56 [1.09, 2.22]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.



requirements specifically, two studies found uncertain effects of the laws (Edwards et al., 2018; Lott and Whitley, 2001), one found that waiting periods had suggestive negative effects on total homicides (Hepburn et al., 2004), one found that waiting periods significantly increased total homicides (Fridel, 2021a), and one found that waiting periods significantly reduced total homicides among adults aged 21 years or older (Luca, Malhotra, and Poliquin, 2017). 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. Finally, one study found uncertain effects of the laws on violent crime (Lott and Whitley, 2001). Considering these studies and our assessment of their strengths, we find *moderate evidence that waiting periods may reduce total homicides.*

Firearm homicides. Excluding one study with critical methodological concerns, we identified five qualifying studies that examined the effects of waiting periods on firearm homicide rates. Based on analyses of the Brady Act that cannot disentangle the effects of changes in background check laws from those of waiting-period requirements, one study found uncertain effects on firearm homicide rates (Ludwig and Cook, 2000), while one found uncertain effects on handgun homicides but significant reductions in non-handgun firearm homicides (Monroe, 2008). Examining changes to waiting period requirements specifically, 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). Finally, 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.*

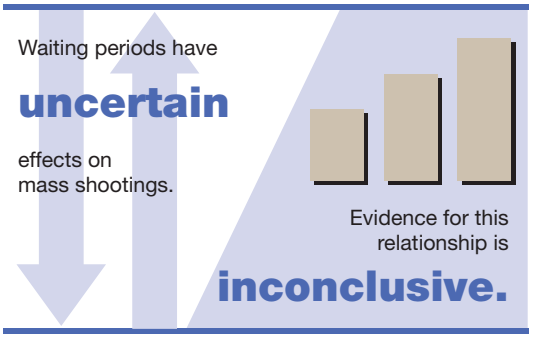
Effects on Mass Shootings

Research Synthesis Findings

Our search yielded three studies that met our inclusion criteria and examined the effects of waiting periods on mass shootings in the United States, one of which (Fridel, 2021a) contributes new findings to this updated review.

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. However, this study was focused on assessing the causal effects of concealed-carry laws, and waiting-period requirements were included as control variables; thus, these findings have serious causal interpretation concerns (Hünermund and Louw, 2023; Westreich and Greenland, 2013). Additionally, the analysis of multiple-victim public shooting incidents was subject to critical concerns with overfit models because the data had fewer than five incidents per estimated parameter.

Using a two-way fixed-effects linear probability model, Luca, Malhotra, and Poliquin (2020) 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 other 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 state and year fixed effects, a host of other state-level gun policies, and 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 (2020), 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). Additionally, potential for model overfit (fewer than two events per parameter) and likely violations of model assumptions because of data sparsity render these model estimates and CIs possibly unreliable and subject to critical methodological concerns.



Finally, Fridel (2021a) evaluated a shorter time frame of 1991 to 2016 and assessed the relationship between waiting period requirements and *mass murder* rates, defined as incidents with four or more fatalities (excluding the perpetrator) within a 24-hour period. The author used negative binomial models that employed generalized estimating equations and included region-level fixed effects, year fixed effects, controls for other firearm policies, and a large set of state-level time-varying covariates (including a proxy for firearm ownership, socioeconomic factors, and crime rates). Results showed that mandatory waiting-period requirements were associated with significant increases in mass murder incidence rates. However, the purpose of this study was to evaluate shall-issue laws; because other firearm laws were intended as ancillary covariates, these results are subject to serious methodological concerns with drawing causal conclusions.

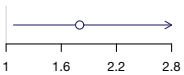
Figure 10.3 displays the IRR and CIs associated with the waiting-period policies examined in Fridel (2021a). Because the model in Lott (2003) estimated three effects for waiting-period policies that cannot be readily combined to indicate an overall or average effect, we do not include those estimates in Figure 10.3. We do not include estimates from Luca, Malhotra, and Poliquin (2020) because of critical concerns with overfit models.

Conclusions

Excluding one study with critical methodological concerns, we identified two qualifying studies examining the effect of waiting periods on multiple-victim shooting or mass murder outcomes. Fridel (2021a) found that mandatory waiting-period requirements were associated with significant increases in mass murder incidence. Lott (2003) found a suggestive effect consistent with the passage of any waiting-period law increasing the incidence of multiple-

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

Study, by Policy	Outcome Measure	IRR [95% CI]
Waiting-period law	**Mass murder**	
Fridel (2021a)	Number of incidents	1.80 [1.08, 3.01]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details.

victim public shooting casualties. 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. Furthermore, Lott (2003) found uncertain effects of both waiting-period measures on the number of casualties from multiple-victim public shooting events. Considering these studies and their methodological weaknesses, 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.

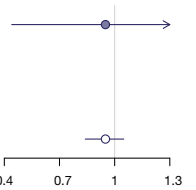
Glaeser and Glendon (1998) used household survey data from 1973 to 1994 to better understand the determinants of gun ownership. Using linear models that adjusted for a range of person-level characteristics and state fixed effects, the authors estimated the effect of waiting periods on the probability 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. However, their models did not appear to control for national trends, which creates critical concerns that their estimates reflect broader differential trends in gun ownership rather than the effects of waiting periods per se.

Figure 10.4 displays the IRRs and CIs associated with the waiting-period policies examined in Brauer, Montolio, and Trujillo-Baute (2017). Because of the aforementioned concerns with interpreting the results of Glaeser and Glendon (1998) as causal policy effects, we do not include effect sizes for this study in the figure.

FIGURE 10.4

Incidence Rate Ratios Associated with the Effect of Waiting Periods on the Gun Industry

Study, by Policy	Outcome Measure	IRR [95% CI]
Waiting-period law	**Firearm manufacturing**	
Brauer, Montolio, and Trujillo-Baute (2017)	Number of plants	0.95 [0.44, 1.47]
Waiting-period law	**NICS background checks**	
Brauer, Montolio, and Trujillo-Baute (2017)	Background checks for firearm purchases	0.95 [0.84, 1.05]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.

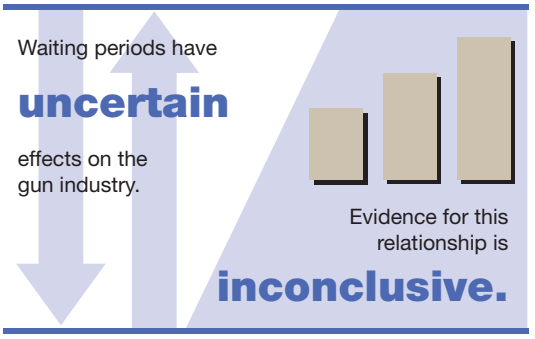
Conclusions

One study evaluated the effects 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 effects 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
- police shootings
- defensive gun use
- hunting and recreation.



Chapter 10 References

Anestis, M. D., "Prior Suicide Attempts Are Less Common in Suicide Decedents Who Died by Firearms Relative to Those Who Died by Other Means," *Journal of Affective Disorders*, Vol. 189, No. 1, 2016.

Anestis, Michael D., and Joye C. Anestis, "Suicide Rates and State Laws Regulating Access and Exposure to Handguns," *American Journal of Public Health*, Vol. 105, No. 10, 2015.

Azrael, Deborah, Lisa Hepburn, David Hemenway, and Matthew Miller, "The Stock and Flow of U.S. Firearms: Results from the 2015 National Firearms Survey," *Russell Sage Foundation Journal of the Social Sciences*, Vol. 3, No. 5, 2017.

Braga, Anthony A., Garen J. Wintemute, Glenn L. Pierce, Philip J. Cook, and Greg Ridgeway, "Interpreting the Empirical Evidence on Illegal Gun Market Dynamics," *Journal of Urban Health*, Vol. 89, No. 5, 2012.

Brauer, Jurgen, Daniel Montolio, and Elisa Trujillo-Baute, "How Do US State Firearms Laws Affect Firearms Manufacturing Location? An Empirical Investigation, 1986–2010," *Journal of Economic Geography*, Vol. 17, No. 4, 2017.

Buchanan, Larry, Josh Keller, Richard A. Oppel, Jr., and Daniel Victor, "How They Got Their Guns," *New York Times*, February 14, 2018.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Collins, T., R. Greenberg, M. Siegel, Z. Xuan, E. F. Rothman, S. W. Cronin, and D. Hemenway, "State Firearm Laws and Interstate Transfer of Guns in the USA, 2006–2016," *Journal of Urban Health*, Vol. 95, No. 3, June 2018.

Cook, Philip J., and Jens Ludwig, "The Effect of the Brady Act on Gun Violence," in B. Harcourt, ed., *Guns, Crime, and Punishment in America*, New York University Press, 2003.

Criminal Justice Information Services Division, *National Instant Criminal Background Check System (NICS) System 2022 Operational Report*, U.S. Department of Justice, Federal Bureau of Investigation, 2023.

Cummings, P., T. D. Koepsell, D. C. Grossman, J. Savarino, and R. S. Thompson, "The Association Between the Purchase of a Handgun and Homicide or Suicide," *American Journal of Public Health*, Vol. 87, No. 6, 1997b.

Daigle, M. S., "Suicide Prevention Through Means Restriction: Assessing the Risk of Substitution—A Critical Review," *Accident Analysis and Prevention*, Vol. 37, 2005.

Edwards, Griffin, Erik Nesson, Joshua J. Robinson, and Fredrick Vars, "Looking Down the Barrel of a Loaded Gun: The Effect of Mandatory Handgun Purchase Delays on Homicide and Suicide," *Economic Journal*, Vol. 128, No. 616, 2018.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Glaeser, Edward L., and Spencer Glendon, "Who Owns Guns? Criminals, Victims, and the Culture of Violence," *American Economic Review*, Vol. 88, No. 2, May 1998.

Glatt, K. M., "Helpline: Suicide Prevention at a Suicide Site," *Suicide and Life-Threatening Behavior*, Vol. 17, 1987.

Grassel, K. M., G. J. Wintemute, M. A. Wright, and M. P. Romero, "Association Between Handgun Purchase and Mortality from Firearm Injury," *Injury Prevention*, Vol. 9, No. 1, 2003.

Hepburn, Lisa, Matthew Miller, Deborah Azrael, and David Hemenway, "The Effect of Nondiscretionary Concealed Weapon Carrying Laws on Homicide," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 56, No. 3, 2004.

Hünermund, Paul, and Beyers Louw, "On the Nuisance of Control Variables in Regression Analysis," *Organizational Research Methods*, December 25, 2023, online ahead of print.

Kahane, Leo H., "State Gun Laws and the Movement of Crime Guns Between States," *International Review of Law and Economics*, Vol. 61, 2020.

Kester, Louis, Daniel N. Holena, Allyson M. Hynes, Elinore J. Kaufman, Tejal Brahmbhatt, Sabrina Sanchez, James P. Byrne, Tracey Dechert, Mark Seamon, and Dane R. Scantling, "Preventing the Most Common Firearm Deaths: Modifiable Factors Related to Firearm Suicide," *Surgery*, Vol. 173, No. 2, February 2023.

Koenig, Christoph, and David Schindler, "Impulse Purchases, Gun Ownership, and Homicides: Evidence from a Firearm Demand Shock," *Review of Economics and Statistics*, Vol. 105, No. 5, September 2023.

Krouse, W. J., *Gun Control: Statutory Disclosure Limitations on ATF Firearms Trace Data and Multiple Handgun Sales Reports*, Congressional Research Service, 7-5700, 2009.

Lewiecki, E. M., and S. A. Miller, "Suicide, Guns, and Public Policy," *American Journal of Public Health*, Vol. 103, No. 1, 2013.

Lott, John R., Jr., *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong*, Regnery Publishing, Inc., 2003.

Lott, John R., Jr., and John E. Whitley, "Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime," *Journal of Law and Economics*, Vol. 44, No. 2, 2001.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

Ludwig, J., and P. J. Cook, "Homicide and Suicide Rates Associated with Implementation of the Brady Handgun Violence Prevention Act," *JAMA*, Vol. 284, No. 5, 2000.

Miller, M., D. Azrael, and C. Barber, "Suicide Mortality in the United States: The Importance of Attending to Method in Understanding Population-Level Disparities in the Burden of Suicide," *Annual Review of Public Health*, Vol. 33, 2012.

Miller, M., D. Hemenway, and D. Azrael, "Firearms and Suicide in the Northeast," *Journal of Trauma and Acute Care Surgery*, Vol. 57, No. 3, 2004.

Monroe, Jeffrey D., *Homicide and Gun Control: The Brady Handgun Violence Prevention Act and Homicide Rates*, LFB Scholarly Publishing, 2008.

Mustard, David B., "The Impact of Gun Laws on Police Deaths," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Office of the Inspector General, U.S. Department of Justice, *Audit of the Handling of Firearms Purchase Denials Through the National Instant Criminal Background Check System*, September 2016.

Oliphant, Stephen N., "Effects of Wisconsin's Handgun Waiting Period Repeal on Suicide Rates," *Injury Prevention*, Vol. 28, No. 6, 2022.

Peterson, Jillian, and James Densley, *The Violence Project: How to Stop a Mass Shooting Epidemic*, Abrams, 2021.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of March 2, 2018: https://www.rand.org/pubs/research_reports/RR2088.html

Roberts, Darryl W., "Intimate Partner Homicide: Relationships to Alcohol and Firearms," *Journal of Contemporary Criminal Justice*, Vol. 25, No. 1, 2009.

Simon, T. R., A. C. Swann, K. E. Powell, L. B. Potter, M. Kresnow, and P. W. O'Carroll, "Characteristics of Impulsive Suicide Attempts and Attempters," *Suicide and Life-Threatening Behavior*, Vol. 32, Supp. 1, 2002.

Spicer, R. S., and T. R. Miller, "Suicide Acts in 8 States: Incidence and Case Fatality Rates by Demographics and Method," *American Journal of Public Health*, Vol. 90, No. 12, 2000.

Studdert, David M., Yifan Zhang, Sonja A. Swanson, Lea Prince, Jonathan A. Rodden, Erin E. Holsinger, Matthew J. Spittal, Garen J. Wintemute, and Matthew Miller, "Handgun Ownership and Suicide in California," *New England Journal of Medicine*, Vol. 382, No. 23, 2020.

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

Vyrostek, S. B., J. L. Annest, and G. W. Ryan, "Surveillance for Fatal and Nonfatal Injuries—United States, 2001," *MMWR Surveillance Summary*, Vol. 53, 2004.

Webster, D. W., and G. J. Wintemute, "Effects of Policies Designed to Keep Firearms from High-Risk Individuals," *Annual Review of Public Health*, Vol. 36, 2015.

Westreich, Daniel, and Sander Greenland, "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients," *American Journal of Epidemiology*, Vol. 177, No. 4, February 15, 2013.

Wintemute, G. J., C. A. Parham, J. J. Beaumont, M. Wright, and C. Drake, "Mortality Among Recent Purchasers of Handguns," *New England Journal of Medicine*, Vol. 341, No. 21, 1999.

CHAPTER 11

Firearm Safety Training Requirements

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 individuals requesting concealed-carry permits to show proof of formal safety training, which typically concerns safe storage, use, and maintenance of weapons. Public support for requiring concealed-carry applicants to pass a test demonstrating safe and lawful use of firearms is relatively high; an estimated 74 percent of U.S. adults supported such requirements in a 2021 survey (Crifasi et al., 2022). 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. However, detractors of the laws suggest that such regulations create unwarranted costs and barriers to firearm ownership and that the right to own a firearm should not be 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. Indeed, applicants for concealed-carry permits in Maryland must demonstrate 70-percent accuracy when firing at least 50 rounds from a distance of no more than 25 yards (Maryland State Police, undated). Research suggests that live firearm-training requirements attached to the concealed-carry permitting process may be an important moderator of how transitioning to a permitless-carry policy environment affects assault rates (Doucette et al., 2023). In a May 2022 survey, one-third of gun owners indicated that they believed that live-fire training should be a prerequisite for obtaining a concealed-carry permit (Grene, Dharani, and Siegel, 2023).

The components of safety training vary greatly across states. 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 17, 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.

Limited research investigates the relationship between the receipt of safety training and weapon safety behaviors, and most of this research finds that training is not associated with improved 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 service in the U.S. 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 signifi-

cantly 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 contextual factors. One factor, for instance, may be the messenger or trainer. The same study that found high rates of belief that storage practices were related to safety training 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. A study of Louisiana firearm owners and instructors conducted in 2021 found that both groups believed that messenger relatability and the alignment between lethal means safety messaging and firearm owner values were important to increasing acceptability of firearm training (Houtsma et al., 2023). These findings underscore that a nuanced understanding of the delivery and components of firearm training will likely moderate any behavior change among firearm owners.

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, 2024, eight states and the District of Columbia have laws requiring individuals to undergo some sort of safety training prior to purchasing, or in the case of Connecticut and New Mexico, carrying, a firearm.[1] California, Massachusetts, and Washington have laws requiring such training for the purchase of both handguns and long guns.[2] 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.[3] The laws in Connecticut, Hawaii,

[1] California, Connecticut, Hawaii, Maryland, Massachusetts, New Mexico, Rhode Island, Washington, and the District of Columbia.

[2] Calif. Penal Code § 31615(a); Mass. Gen. Laws Ann. 140 § 131P; Wash. Stat. § 9.41.090.

[3] D.C. Stat. §§ 7-2502.03(a), 7-2502.06(a).

Maryland, and Rhode Island apply only to handguns.[4] Elements of safety training laws may include specifics about instructor qualifications or training,[5] information about which organizations may offer approved courses,[6] curriculum requirements,[7] and a requirement that trainees pass a written test.[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 not just those jurisdictions with "may-issue" concealed-carry laws prior to June 2022, when the Supreme Court declared shall-issue laws unconstitutional,[9] but also states with histories of more-permissive "shall-issue" concealed-carry laws.[10] See Chapter 18 for further information on variation in state implementation of concealed-carry laws.

---

[4] Conn. Gen. Stat. Ann. § 29-28(b); Hawaii Rev. Stat. § 134-2(g); Md. Code, Pub. Safety, § 5-117.1(d); R.I. Gen. Laws 1956 § 11-47-35.

[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)).

[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)).

[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)).

[8] See, for example, Calif. Penal Code § 31640.

[9] California, Connecticut, Delaware, Hawaii, Massachusetts, New Jersey, Rhode Island, and the District of Columbia. See Calif. Penal Code § 26165; Conn. Gen. Stat. § 29-28(b) (Connecticut does not distinguish between open- and concealed-carry permits); Del. Code Ann. Tit. 11, § 1441(a); Hawaii Rev. Stat. Ann. § 134-2(g) (Hawaii requires safety training for a permit to purchase, not specifically for concealed carry); Mass. Gen. Laws Ann. 140 § 131P (the same section of the law as for firearm safety training requirement for purchase); N.J. Stat. Ann. 2C:39-6j; R.I. Gen. Laws 1956, § 11-47-15 (Rhode Island's law is about ability rather than safety knowledge); D.C. Code Ann. § 7-2509.03.

[10] Arkansas, Colorado, Florida, Illinois, Iowa, Kentucky, Michigan, Minnesota, Montana, Nevada, New Mexico, North Carolina, Ohio, Oklahoma, Oregon, South Carolina, Tennessee, Texas, and Utah. See Ark. Code Ann. § 5-73-309(13); Colo. Rev. Stat. Ann. § 18-12-203(1)(h); Fla. Stat. Ann. § 790.06; 430 Ill. Comp. Stat. 66/75(b); Ia. Code § 724.9; Ky. Rev. Stat. § 237.110(4)(i); Mich. Comp. Laws Ann. 28.425b;

Effects on Violent Crime

Research Synthesis Findings

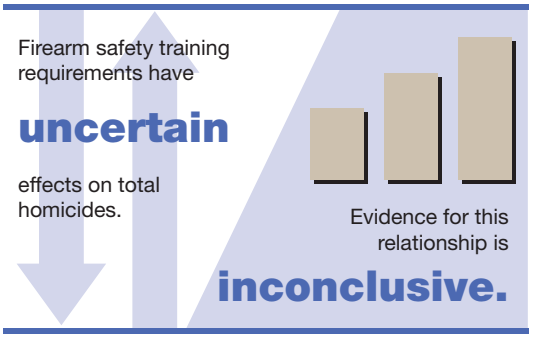
We identified one study that met our inclusion criteria and examined how the number of training hours required to obtain a concealed-carry permit affected murder rates. In estimates focused on understanding the impact of concealed-carry laws on murder, Lott (2010) controlled for differing concealed-carry 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, which may bias the standard errors. 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 9) rather than for the safety training component of the law. Another study (Smith and Petrocelli, 2019) examined changes in violent crime in Tucson, Arizona, after the elimination of permitting requirements for the concealed carry of firearms, which obviated the safety training requirements that were previously mandated. Because the law involved changing to a permitless-carry regime, we consider that study as providing evidence for concealed-carry laws (see Chapter 18) 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.

Minn. Stat. § 624.714, Subd. 2; Mont. Code Ann. 45-8-321(3); Nev. Rev. Stat. 202.3657(3)(c); N.M. Stat. Ann. 1978, § 29-19-4A(10); N.C. Gen. Stat. Ann. § 14-415.12(a)(4); Ohio Rev. Code § 2923.125; 21 Okla. Stat. Ann. § 1290.9(4); Ore. Rev. Stat. § 166.291(1)(f); S.C. Code Ann. § 23-31-215(A)(5); Tenn. Code Ann. § 39-17-1307; Tex. Code Ann. § 39-17-1351(e); Utah Code Ann. 1953 § 53-5-704(8).



Considering the methodological concerns with this single study, we find *inconclusive evidence for the effects of firearm safety training requirements on total homicides.*

Effects on Mass Shootings

Research Synthesis Findings

We identified two studies that met our inclusion criteria and assessed the relationship between firearm safety training requirements and school shootings, one of which contributes new findings to this updated review (Hamlin, 2021). One earlier study (Lott, 2003) did not provide inferential statistics, and thus that study is not further discussed here.[11]

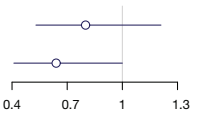
Hamlin (2021) used school shooting data from 1980 to 2019 to evaluate the relationship between firearm ownership rates and the number of school shooting incidents and casualties in a state. As part of these analyses, the author controlled for several state firearm laws relevant for youth, including laws that required safety training to own a handgun. Using negative binomial regressions that controlled for a linear time trend, region fixed effects, estimated state gun ownership rates based on survey and administrative data, other gun laws, and poverty rates, the author estimated an uncertain relationship between safety training requirements and school shooting incidence but found significant negative effects for school shooting injuries and fatalities. Because the focus of this study was evaluating the relationship of firearm ownership with school shootings rather than evaluating minimum age laws, the author viewed these analyses as exploratory. However, the statistically significant result prompted the author to conduct several interrupted time-series analyses to separately examine pre-post policy changes in school shooting outcomes among the six states that implemented firearm safety training requirements during the study time frame. Findings varied across states, and the post-hoc nature of the analysis, combined with almost no discussion of model specification or identifying assumptions, presents serious methodological concerns for interpreting the results as causal evidence for safety training requirements' effects on school shootings.

[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.

Figure 11.1 displays the IRRs and CIs associated with the firearm safety training laws examined in Hamlin's (2021) exploratory analysis because the parameterization of the state-specific time series analyses does not lend itself to producing overall policy effect estimates.

FIGURE 11.1
Incidence Rate Ratios Associated with the Effect of Firearm Safety Training Requirements on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
Safety training to own handgun	**School shooting**	
Hamlin (2021)	Number of incidents	0.80 [0.53, 1.21]
Hamlin (2021)	Number of casualties	0.64 [0.41, 1.00]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. See Appendix B for details.

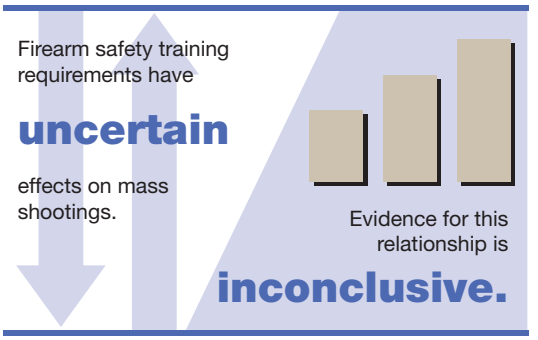
Conclusions

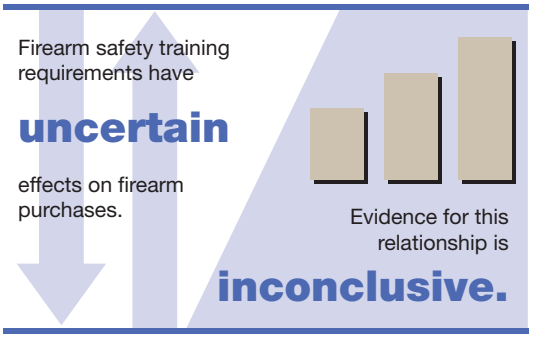
Excluding one study with critical methodological concerns, we identified one qualifying study that provided evidence for the effect of safety training requirements on school shootings. We thus find *inconclusive evidence for how firearm safety training requirements affect mass shootings*.

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. Given critical issues for drawing causal insights from this study, we do not present these estimates in a figure.

Conclusions

The one study that examined 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
- police shootings
- defensive gun use
- hunting and recreation.

Chapter 11 References

Berrigan, John, Deborah Azrael, David Hemenway, and Matthew Miller, "Firearms Training and Storage Practices Among US Gun Owners: A Nationally Representative Study," *Injury Prevention*, Vol. 25, Supp. 1, 2019.

Cole, David, "Mandatory Firearms Training? What If We Had Mandatory Free Speech Training," *Ammoland: Shooting Sports News*, June 13, 2014.

Crifasi, Cassandra K., Mitchell L. Doucette, Emma E. McGinty, Daniel W. Webster, and Colleen L. Barry, "Storage Practices of US Gun Owners in 2016," *American Journal of Public Health*, Vol. 108, No. 4, 2018a.

Crifasi, Cassandra K., Keshia M. Pollack, and Daniel W. Webster, "Effects of State-Level Policy Changes on Homicide and Nonfatal Shootings of Law Enforcement Officers," *Injury Prevention*, Vol. 22, No. 4, 2016.

Crifasi, Cassandra K., Julie A. Ward, Emma E. McGinty, Colleen L. Barry, and Daniel W. Webster, "Public Opinion on Laws Regulating Public Gun Carrying," *Preventive Medicine*, Vol. 159, June 2022.

Doucette, Mitchell L., Cassandra K. Crifasi, Alex D. McCourt, Julie A. Ward, Rebecca L. Fix, and Daniel W. Webster, "Deregulation of Public Civilian Gun Carrying and Violent Crimes: A Longitudinal Analysis 1981–2019," *Criminology and Public Policy*, 2023.

Ginwalla, Rashna, Peter Rhee, Randall Friese, Donald J. Green, Lynn Gries, Bellal Joseph, Narong Kulvatunyou, Dafney Lubin, Terence O'Keeffe, Gary Vercruysse, Julie Wynne, and Andrew Tang, "Repeal of the Concealed Weapons Law and Its Impact on Gun-Related Injuries and Deaths," *Journal of Trauma and Acute Care Surgery*, Vol. 76, No. 3, 2014.

Grene, Kathleen, Amani Dharani, and Michael Siegel, "Gun Owners' Assessment of Gun Safety Policy: Their Underlying Principles and Detailed Opinions," *Injury Epidemiology*, Vol. 10, 2023.

Hamlin, Daniel, "Are Gun Ownership Rates and Regulations Associated with Firearm Incidents in American Schools? A Forty-Year Analysis (1980–2019)," *Journal of Criminal Justice*, Vol. 76, September–October 2021.

Hemenway, David, Steven Rausher, Pina Violano, Toby A. Raybould, and Catherine W. Barber, "Firearms Training: What Is Actually Taught?" *Injury Prevention*, Vol. 25, No. 2, 2019b.

Hemenway, David, Sara J. Solnick, and Deborah R. Azrael, "Firearm Training and Storage," *JAMA*, Vol. 273, No. 1, 1995.

Houtsma, Claire, Jeffrey Powers, Amanda M. Raines, Matthew Bailey, Catherine Barber, and Gala True, "Engaging Stakeholders to Develop a Suicide Prevention Learning Module for Louisiana Firearm Training Courses," *Injury Epidemiology*, Vol. 10, 2023.

Lott, John R., Jr., *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong*, Regnery Publishing, Inc., 2003.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Lum, Hillary D., Hanna K. Flaten, and Marian E. Betz, "Gun Access and Safety Practices Among Older Adults," *Current Gerontology and Geriatrics Research*, Vol. 2016, 2016.

Maryland State Police, "Wear and Carry Permit Training," webpage, undated. As of September 12, 2022:
https://mdsp.maryland.gov/Organization/Pages/CriminalInvestigationBureau/LicensingDivision/Training/WearandCarryPermit.aspx

Rowhani-Rahbar, Ali, Vivian H. Lyons, Joseph A. Simonetti, Deborah Azrael, and Matthew Miller, "Formal Firearm Training Among Adults in the USA: Results of a National Survey," *Injury Prevention*, Vol. 24, No. 2, 2018.

Rudolph, K. E., E. A. Stuart, J. S. Vernick, and D. W. Webster, "Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides," *American Journal of Public Health*, Vol. 105, No. 8, 2015.

Smith, Michael R., and Matthew Petrocelli, "The Effect of Concealed Handgun Carry Deregulation in Arizona on Crime in Tucson," *Criminal Justice Policy Review*, Vol. 30, No. 8, 2019.

CHAPTER 12

Lost or Stolen Firearm Reporting Requirements

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 2022, federally licensed firearm dealers reported nearly 15,000 firearms (including pistols, rifles, shotguns, revolvers, derringers, and machine guns) as lost or stolen from their places of business (Bureau of Alcohol, Tobacco, Firearms and Explosives [ATF], 2023a). Quantifying the number of firearms lost or stolen from private citizens is more challenging, but the ATF (ATF, 2023b) reported that 201,731 firearms were stolen from private citizens in 2021, according to data voluntarily reported to the Federal Bureau of Investigation's (FBI's) National Crime Information Center (NCIC) by state, local, and tribal law enforcement agencies. Using the NCIC counts and estimates of underreporting from the National Crime Victimization Survey, the ATF estimated that approximately 266,000 firearms per year were stolen from private citizens between 2017 and 2021.

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). Annual estimates of guns stolen for the 2010–2014 period were similar, at just more than 251,000, with about 75 percent of theft incidents reported to police (Cook, 2018). 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. An analysis of FBI crime data for 149 cities (O'Toole, Szkola, and Burd-Sharps, 2022) found that the number of guns reported stolen remained relatively stable from 2015 to 2019, then sharply increased in 2020, in large part driven by increased thefts from cars.

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). A study linking gun theft data reported to the Chicago Police Depart-

ment with crime guns recovered and traced by the Chicago Police Department estimated that 3 percent of crime guns had previously been reported stolen (Cook, 2018).[1] The extent to which theft plays a role in supplying criminal gun markets is unknown and likely varies substantially across states and local jurisdictions (e.g., see information specific to California in Laqueur et al. [2023] and Robinson et al. [2024]).

There are several plausible mechanisms through which policies requiring the reporting of lost or stolen firearms 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 fear that failure to report their firearms as stolen could leave them accountable if their guns later turned up at a crime scene.[2] Third, timelier reporting of gun losses or thefts may aid law enforcement's 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. Finally, reporting requirements could deter theft to the extent that potential offenders expect theft reporting requirements to increase the likelihood of detection or sanction. 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, 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 detailed firearm trace data since 2003, making it difficult to conduct this type of analysis well at a national level (Krouse, 2009). That said, some analyses of aggregate crime gun trace data, the release of which has been explicitly authorized since fiscal year 2008 (Krouse, 2009), suggest that states with stricter laws mandating the reporting of lost or stolen firearms are less likely to be sources of crime guns to other states and more likely to be the destination for interstate crime gun flows (Knight, 2013).

---

[1] The ATF (2002, p. A-3) defined a *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] Some research supports that firearm purchasers are responsive to the threat of penalties. In an experimental study in Los Angeles, California, that mailed letters to a random subset of recent firearm purchasers notifying them of their criminal liability should the gun they purchased be involved in a crime, letter recipients were significantly more likely than nonrecipients to then report their guns as stolen (Ridgeway et al., 2011). While it is unclear whether this was due to increased reporting of guns that were actually stolen or false reporting by straw purchasers, the results support that informing the public about mandatory reporting of lost and stolen firearms increases reporting.

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, they could have a small effect on firearm sales.

State Implementation of Lost or Stolen Firearm Reporting Requirements

As of January 1, 2024, 15 states and the District of Columbia require firearm owners to report to law enforcement when their weapons are lost or stolen.[3] Most jurisdictions with these laws require individuals to report the loss or theft of all firearms, although laws vary in the length of time allowed to elapse between the discovery and the report.[4] However, Maryland requires the reporting of loss or theft of handguns and assault weapons only,[5] and Michigan requires the reporting of thefts, but not loss, of all firearms.[6]

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
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

[3] California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maryland, Massachusetts, Michigan, New Jersey, New York, Ohio, Oregon, Rhode Island, Virginia, and the District of Columbia.

[4] Calif. Penal Code § 25250 (report within five days); Colo. Rev. Stat. § 18-12-113 (within five days); Conn. Gen. Stat. § 53-202g (within 72 hours); Del. Code Tit. 11 § 1461 (within seven days); Hawaii Rev. Stat. § 134-29(a) (report within 24 hours); 720 Ill. Comp. Stat. 5/24-4.1 (within 72 hours); Mass. Gen. Laws Ch. 140 § 129C (report "forthwith"); N.J. Stat. Ann. § 2C:58-19 (within 36 hours); N.Y. Penal Law § 400.10 (within 24 hours); Ohio Rev. Code § 923.20 (report "forthwith"); Oreg. Rev. Stat. § 166.397; R.I. Gen. Laws § 11-47-48.1 (within 24 hours); Va. H.B. 9 § 18.2-287.5 (within 48 hours); D.C. Code Ann. § 7-2502.08 (report "immediately").

[5] Md. Ann. Code § 5-146 (within 72 hours).

[6] Mich. Comp. Laws § 28.430 (within five days).

Chapter 12 References

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

Braga, Anthony A., Garen J. Wintemute, Glenn L. Pierce, Philip J. Cook, and Greg Ridgeway, "Interpreting the Empirical Evidence on Illegal Gun Market Dynamics," *Journal of Urban Health*, Vol. 89, No. 5, 2012.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *2012 Summary: Firearms Reported Lost and Stolen*, U.S. Department of Justice, 2013.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *Federal Firearms Licensee (FFL) Theft/Loss Report: January 1, 2022–December 31, 2022*, U.S. Department of Justice, 2023a.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *National Firearms Commerce and Trafficking Assessment (NFCTA): Crime Guns—Volume Two Part V: Firearm Thefts*, U.S. Department of Justice, 2023b.

Cook, Philip J., "Gun Theft and Crime," *Journal of Urban Health*, Vol. 95, No. 3, June 2018.

Everytown for Gun Safety Support Fund, Mayors Against Illegal Guns, and the National Urban League, *Strategies for Reducing Gun Violence in American Cities*, June 2016.

Hemenway, David, Deborah Azrael, and Matthew Miller, "Whose Guns Are Stolen? The Epidemiology of Gun Theft Victims," *Injury Epidemiology*, Vol. 4, No. 1, 2017.

Knight, Brian, "State Gun Policy and Cross-State Externalities: Evidence from Crime Gun Tracing," *American Economic Journal: Economic Policy*, Vol. 5, No. 4, 2013.

Krouse, W. J., *Gun Control: Statutory Disclosure Limitations on ATF Firearms Trace Data and Multiple Handgun Sales Reports*, Congressional Research Service, 7-5700, 2009.

Langton, L., *Firearms Stolen During Household Burglaries and Other Property Crimes, 2005–2010*, U.S. Department of Justice, Bureau of Justice Statistics, NCJ 239436, 2012.

Laqueur, Hannah S., Christopher McCort, Colette Smirniotis, Sonia Robinson, and Garen J. Wintemute, "Trends and Sources of Crime Guns in California: 2010–2021," *Journal of Urban Health*, Vol. 100, No. 5, October 2023.

O'Toole, Megan J., Jay Szkola, and Sarah Burd-Sharps, *Gun Thefts from Cars: The Largest Source of Stolen Guns*, Everytown Research and Policy, May 2022.

Ridgeway, Greg, Anthony A. Braga, George Tita, and Glenn L. Pierce, "Intervening in Gun Markets: An Experiment to Assess the Impact of Targeted Gun-Law Messaging," *Journal of Experimental Criminology*, Vol. 7, No. 1, March 2011.

Robinson, Sonia L., Christopher D. McCort, Colette Smirniotis, Garen J. Wintemute, and Hannah S. Laqueur, "Purchaser, Firearm, and Retailer Characteristics Associated with Crime Gun Recovery: A Longitudinal Analysis of Firearms Sold in California from 1996 to 2021," *Injury Epidemiology*, Vol. 11, February 26, 2024.

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

Webster, D. W., and G. J. Wintemute, "Effects of Policies Designed to Keep Firearms from High-Risk Individuals," *Annual Review of Public Health*, Vol. 36, 2015.

CHAPTER 13

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.[1] 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 recordkeeping 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.[2] 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 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

[1] Licensed dealers are required to report to the ATF the sale of *multiple* pistols or revolvers to the same purchaser at the same time or within five business days, but the ATF and any other law enforcement agency receiving this report must destroy it within 20 days (18 U.S.C. 923(g)(3) and 27 C.F.R. 478.126(a)).

[2] 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.

and to whom.[3] However, secondary markets appear to be the leading source of guns used in crimes (Harlow, 2001). By requiring a record of each subsequent transfer or sale of a firearm after its initial sale by a licensed dealer, the 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 12).

Requirements to maintain sales records or register firearms, when accompanied by mechanisms to provide de-identified data to researchers, can also help support broader research on firearm purchase behaviors and firearm violence. For example, some researchers have been granted access to information from the California Department of Justice Dealer Record of Sale database, which contains records of legal handgun transfers that have occurred since 1985 in California; these data have been used to support valuable research on the relationship between handgun ownership and suicide (Studdert et al., 2020), the relationship between cohabiting with a legal handgun owner and the risk of homicide or suicide death (Miller et al., 2022; Studdert et al., 2022), factors that mediate the relationship between handgun purchase and suicide (Schleimer et al., 2021a), suicide risk prediction (Laqueur et al., 2022), drivers of handgun acquisitions (Studdert et al., 2017), and sources of crime guns (Laqueur et al., 2023). Many of these studies could not have provided evidence that was as useful and compelling without access to a state's firearm ownership information system.

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.[4] 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.[5]

Required recordkeeping, reporting, or registration may impose costs to sellers of maintaining compliance, and concerns about privacy may deter some individuals who seek 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

[3] The ATF (2002, p. A-3) defined a *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."

[4] Calif. Penal Code § 29810.

[5] Hawaii Rev. Stat. Ann. §§ 134-3(a), (b), 134-4.

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 and arresting 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.

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, 2024, 20 states and the District of Columbia require firearm sellers to keep records of at least some firearm sales. Thirteen states and the District of Columbia require licensed dealers to maintain records of all firearm sales by dealers.[6] Connecticut, Illinois, and Rhode Island also require private sellers to maintain records of sales.[7] In California, Colorado, Delaware, Nevada, New York, Virginia, and the District of Columbia, all private sales are completed through dealers, who maintain records of all sales.[8] Furthermore, seven

[6] California, Connecticut, Illinois, Maine, Maryland, Massachusetts, Michigan, Nevada, New Jersey, Oregon, Pennsylvania, Rhode Island, Virginia, and the District of Columbia. See Calif. Penal Code § 28100; Conn. Gen. Stat. Ann. § 29-31; 40 Ill. Comp. 65/3, 720 Ill. Comp. 5/24-4; Me. Rev. Stat. Ann. Tit. 15 § 455; Md. Code Ann., Pub. Safety § 5-120(b), Md. Code Regs. 29.03.01.09(D); Mass. Gen. Laws Ch. 140 § 123; Mich. Comp. Laws §§ 28.422, 28.422a, 750.232; Nev. Rev. Stat. Ann. § 202. Sec 5; N.J. Stat. Ann. §2C: 58-2, N.J. Admin. Code § 13:54-3.14; Ore. Rev. Stat. §§ 166.412, 166.434; 18 Pa. Cons. Stat. §§ 6111, 6102; R.I. Gen. Laws §§ 11-47-35, 11-47-35.2; Va. Code Ann. § 18.2-308.2:5; D.C. Code Ann. § 7-2504.

[7] Conn. Gen. Stat. Ann. § 29-31; 430 Ill. Comp. 65/3, 720 Ill. Comp. 5/24-4; R.I. Gen. Laws §§ 11-47-35, 11-47-35.2.

[8] Calif. Penal Code §§ 27545, 28210; Colo. Rev. Stat. § 18-12-112; Del. Code Ann. Tit. 11 § 1448B, Tit. 24 § 904; Nev. Rev. Stat. Ann. § 202. Sec 5; N.Y. Gen. Bus. Law § 898l, N.Y. Penal Law §§ 265.00(3), 400.00(12); Va. Code Ann. § 18.2-308.2:5; D.C. Code Ann. §§ 7-2505.02, 7-2504.04.

states require licensed dealers to maintain records of handgun sales,[9] and three states require private sellers to do so.[10]

Some states have abolished their recordkeeping requirements. In 2015, for example, Alabama repealed the section of law that required 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 that they created to comply with the repealed law,[11] 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 records be transmitted to a law enforcement agency. Four of these states require records of all sales to be transmitted, including those by licensed dealers and private sellers.[12] Similarly, the District of Columbia's registration requirement gives law enforcement access to all sales records.[13] Five states require dealers and private sellers to report only handgun sales to law enforcement.[14] Washington requires dealers and private sellers to report handgun and semiautomatic rifle sales to law enforcement.[15]

Six jurisdictions have firearm registration schemes. California, Connecticut, Hawaii, Massachusetts, 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.[16] New York and Maryland require registration of handguns only.[17] Several states have specialized registration schemes, such as for .50-caliber

---

[9] Colorado, Delaware, Florida, New York, North Carolina, Vermont, and Washington. See Colo. Rev. Stat. §§ 12-26-102, 12-26-103, 18-12-112; Del. Code Ann. Tit. 11, § 1448B, Tit. 24, §§ 904, 904A; Fla. Stat. §§ 790.065, 790.0655; N.Y. Gen. Bus. Law § 898, N.Y. Penal Law §§ 265.00(3), 400.00(12); N.C. Gen. Stat. § 14-406; Vt. Stat. Ann. Tit. 13, § 4006; Wash. Rev. Code Ann. §§ 9.41.110(9), 9.41.129.

[10] Maryland, Michigan, and New Jersey. See Md. Code Ann., Pub. Safety §§ 5-101, 5-120, 5-145, Md. Code Regs. 29.03.01.09(D); Mich. Comp. Laws §§ 28.422(5), 28.422a(2), 750.232; N.J. Stat. Ann. §§ 2C:58-2b, 2C:58-3h. In addition, Pennsylvania and Washington require private sales to be conducted through licensed dealers, so dealers maintain records of private sales. See 18 Pa. Cons. Stat. §§ 6111(b)(1), (1.1), (1.4), (c), 6113(a)(5), (d), 37 Pa. Code § 33.111; Wash. Rev. Code Ann. § 9.41.110(9).

[11] 2015 Ala. H.B. 47, amending Ala. Code § 13A-11-79.

[12] California, Connecticut, Hawaii, and Massachusetts. See Calif. Penal Code §§ 1106, 27545 (private sales must be conducted through licensed dealers, who in turn must report); Conn. Gen. Stat. §§ 29-33, 29-37; Hawaii Rev. Stat. § 134-2; Mass. Gen. Laws Ch. 140 §§ 123, 128A.

[13] D.C. Code Ann. §§ 7-2502.08, 22-4510.

[14] Maryland, Michigan, New Jersey, New York, and Pennsylvania. See Md. Ann. Code §§ 5-120, 5-124 (firearm applications for private sales must be reported); Mich. Comp. Laws §§ 28.422, 28.422a; N.J. Stat. Ann. § 2C:58-2; N.Y. Penal Law § 400.00; 18 Pa. Cons. Stat. § 6111.

[15] Wash. Rev. Code Ann. §§ 9.41.110(9), 9.41.129.

[16] Calif. Penal Code §§ 28100, 28160, 11106(a)(1), 11106(b)(1); Conn. Gen. Stat. §§ 29-31, 29-33E; Hawaii Rev. Stat. Ann. §§ 134-3(a), 134-3(b), 134-4; Mass. Gen. Law 140 § 123; D.C. Code Ann. §§ 7-2502.01-7-2502.10, D.C. Mun. Regs. Tit. 24, §§ 2311-2320.

[17] N.Y. Penal Law §§ 265.00(22), 265.00(23), 400.00(10), (16-a), 400.02; Md. Code Ann., Pub. Safety, § 5-143.

rifles or for specific banned weapons that were purchased before the prohibitions went into effect (see Chapter 14, on bans on the sale of assault weapons and high-capacity magazines).[18]

Effects on Suicides

Research Synthesis Findings

We identified one study as part of this updated review that evaluated the relationship between all firearm deaths and laws requiring private sellers and licensed dealers to keep and retain records of handgun sales (Liu, Siegel, and Sen, 2022). While this study did not evaluate this policy's effects on suicides specifically, its findings may provide some insights for firearm suicides and thus are noted here.

Evaluating data from the 48 contiguous states from 2000 to 2019, Liu, Siegel, and Sen (2022) used a linear spatial autoregressive model that predicted firearm death rates in a given state-year as a function of that state's firearm laws, other states' firearm laws, and other states' firearm death rates, weighting relationships with other state factors inversely to their geographic distance. Models controlled for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are unconventional because all states affect and are affected by all other states, with the magnitude of these effects varying based on states' geographic distance from one another. The authors calculated overall law effects by comparing the hypothetical scenario in which all states imposed the recordkeeping requirement with the hypothetical scenario in which no states did so. They also decomposed total policy effects into the direct within-state effect of adopting the policy and the indirect interstate spillover effect. While the authors estimated a significant overall association between recordkeeping requirements for handguns and firearm deaths, this was mostly driven by the indirect interstate spillover effect (although within-state association was negative and suggestive), and the unusual counterfactual framework makes it unclear what to infer in terms of policy implications. Furthermore, the authors cautioned against drawing strong conclusions from these results because of collinearity between recordkeeping requirements and the other firearm laws considered.

Given these methodological and interpretational issues, combined with the limited insights for firearm suicide in particular, we do not present these results graphically.

[18] Calif. Penal Code §§ 30510-30530, 30600-30675, 30900-30965; Conn. Gen. Stat. §§ 53-202d(a), 53-202p(a)(1), 53-202q; Hawaii Rev. Stat. Ann. §§ 134-3(a), (b), 134-4; Md. Code Ann., Crim. Law § 4-303; N.J. Stat. Ann. §§ 2C:39-5f, 2C:58-12; N.Y. Penal Law §§ 265.00(22)(e)-(f), 265.00(23), 400.00(10), (16-a), 400.02.

Conclusions

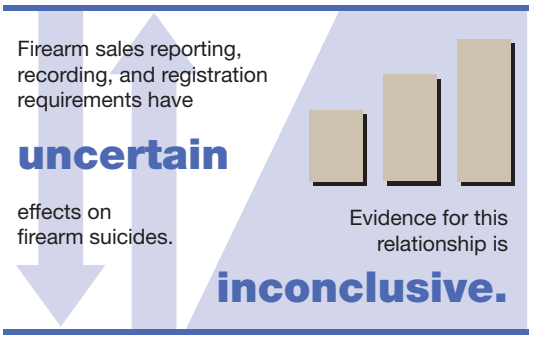
On the basis of one study that evaluated the relationship between all firearm deaths and laws requiring private sellers and licensed dealers to keep and retain records of handgun sales, we find *inconclusive evidence for how firearm sales reporting, recording, and registration requirements affect firearm suicides.*

Effects on Violent Crime

Research Synthesis Findings

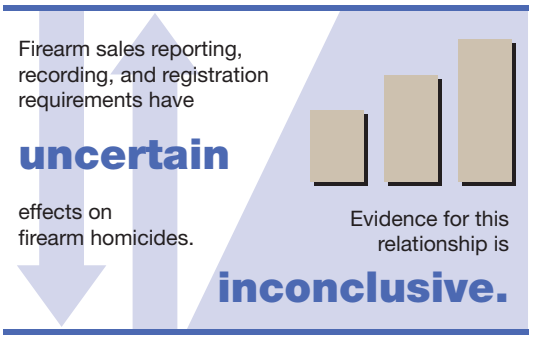
We identified one study as part of this updated review that evaluated the relationship between all firearm deaths and laws requiring private sellers and licensed dealers to keep and retain records of handgun sales (Liu, Siegel, and Sen, 2022). While this study did not evaluate this policy's effects on violent crime specifically, its findings may provide some insights for firearm homicides and thus are noted here.

Evaluating data from the 48 contiguous states from 2000 to 2019, Liu, Siegel, and Sen (2022) used a linear spatial autoregressive model that predicted firearm death rates in a given state-year as a function of that state's firearm laws, other states' firearm laws, and other states' firearm death rates, weighting relationships with other state factors inversely to their geographic distance. Models controlled for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are unconventional because all states affect and are affected by all other states, with the magnitude of these effects varying based on states' geographic distance from one another. The authors calculated overall law effects by comparing the hypothetical scenario in which all states imposed the recordkeeping requirement with the hypothetical scenario in which no states did so; they also decomposed total policy effects into the direct within-state effect of adopting the policy and the indirect interstate spillover effect. While the authors estimated a significant overall association between recordkeeping requirements for handguns and firearm deaths, this was mostly driven by the indirect interstate spillover effect (although within-state association was negative and suggestive), and the unusual counterfactual framework makes it unclear what to infer in terms of policy implications. Furthermore, the authors cautioned against interpreting these results because of collinearity between recordkeeping requirements and the other firearm laws considered.



Given these methodological and interpretational issues, combined with the limited insights for firearm homi-

cide in particular, we do not present this result graphically.



Conclusions

On the basis of one study that evaluated the relationship between all firearm deaths and laws requiring private sellers and licensed dealers to keep and retain records of handgun sales, we find *inconclusive evidence for how firearm sales reporting, recording, and registration requirements affect firearm homicides.*

Effects on Mass Shootings

Research Synthesis Findings

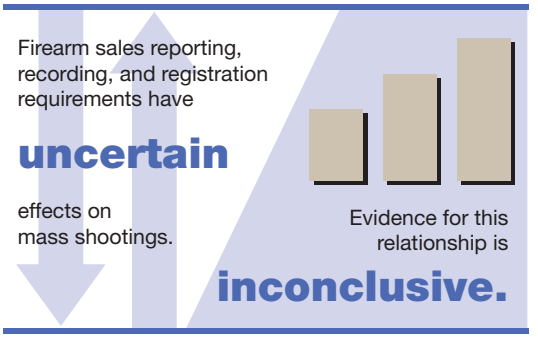
We identified one study that met our inclusion criteria and examined the effect on mass shootings of laws requiring gun owners to register their firearms with a designated law enforcement agency. Blau, Gorry, and Wade (2016) used a linear probability model to estimate how registration requirements relate 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 likelihood of a public shooting event. However, these results are based on a model estimated with fewer than three events per parameter, and thus the results are subject to critical concerns with model overfit and concerns about inaccurate point estimates and confidence intervals. Indeed, the estimated effect size is implausibly large in magnitude, with confidence intervals that span nearly the full possible range of outcomes. Because of these issues, we discount the results of this study for the purpose 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 effects of firearm sales reporting, recording, and registration requirements on mass shootings*.

Effects on the Gun Industry

Research Synthesis Findings

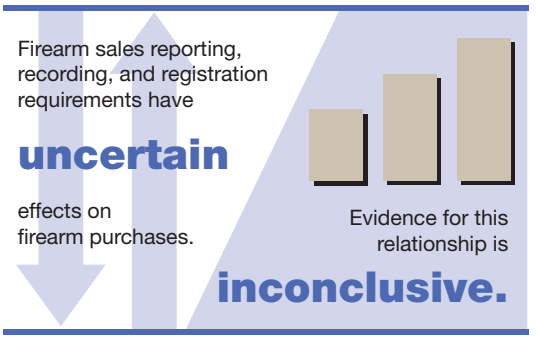


We identified one study that examined how state firearm registration laws influenced per capita rates of NICS background checks for firearm purchases from 1999 to 2010 (Steidley and Kosla, 2018).[19] The authors estimated linear models with a logged outcome, adjusting for a linear national trend, state random effects, region fixed effects, and covariates reflecting state politics, demographics, and proxies 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 effects of firearm sales reporting, recording, and registration requirements on firearm purchases.*



[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).

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:

- unintentional injuries and deaths
- police shootings
- defensive gun use
- hunting and recreation.

Chapter 13 References

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Code of Federal Regulations, Title 27, Section 478.126a, Reporting Multiple Sales or Other Disposition of Pistols and Revolvers.

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:
http://lawcenter.giffords.org/gun-laws/policy-areas/gun-sales/maintaining-records-of-gun-sales/

Harlow, Caroline Wolf, *Survey of Inmates in State and Federal Correctional Facilities: Firearm Use by Offenders*, U.S. Department of Justice, Bureau of Justice Statistics, NCJ 189369, November 2001.

Laqueur, Hannah S., Christopher McCort, Colette Smirniotis, Sonia Robinson, and Garen J. Wintemute, "Trends and Sources of Crime Guns in California: 2010–2021," *Journal of Urban Health*, Vol. 100, No. 5, October 2023.

Laqueur, Hannah S., Colette Smirniotis, Christopher McCort, and Garen J. Wintemute, "Machine Learning Analysis of Handgun Transactions to Predict Firearm Suicide Risk," *JAMA Network Open*, Vol. 5, No. 7, July 2022.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Miller, Matthew, Yifan Zhang, Lea Prince, Sonja A. Swanson, Garen J. Wintemute, Erin E. Holsinger, and David M. Studdert, "Suicide Deaths Among Women in California Living with Handgun Owners vs Those Living with Other Adults in Handgun-Free Homes, 2004–2016," *JAMA Psychiatry*, Vol. 79, No. 6, 2022.

National Rifle Association, Institute for Legislative Action, "New Zealand Experience Further Proves Registration Facilitates Confiscations," July 8, 2019b.

Schleimer, Julia P., Mona A. Wright, Aaron B. Shev, Christopher D. McCort, Rameesha Asif-Sattar, Sydney Sohl, Susan L. Stewart, Garen J. Wintemute, and Rose M. C. Kagawa, "Alcohol and Drug Offenses and Suicide Risk Among Men Who Purchased a Handgun in California: A Cohort Study," *Preventive Medicine*, Vol. 153, 2021a.

Steidley, Trent, and Martin T. Kosla, "Toward a Status Anxiety Theory of Macro-Level Firearm Demand," *Social Currents*, Vol. 5, No. 1, 2018.

Studdert, David M., Yifan Zhang, Erin E. Holsinger, Lea Prince, Alexander F. Holsinger, Jonathan A. Rodden, Garen J. Wintemute, and Matthew Miller, "Homicide Deaths Among Adult Cohabitants of Handgun Owners in California, 2004 to 2016: A Cohort Study," *Annals of Internal Medicine*, Vol. 175, No. 6, June 2022.

Studdert, David M., Yifan Zhang, Jonathan A. Rodden, Rob J. Hyndman, and Garen J. Wintemute, "Handgun Acquisitions in California After Two Mass Shootings," *Annals of Internal Medicine*, Vol. 166, No. 10, 2017.

Studdert, David M., Yifan Zhang, Sonja A. Swanson, Lea Prince, Jonathan A. Rodden, Erin E. Holsinger, Matthew J. Spittal, Garen J. Wintemute, and Matthew Miller, "Handgun Ownership and Suicide in California," *New England Journal of Medicine*, Vol. 382, No. 23, 2020.

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.

CHAPTER 14

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

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).[1] 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, 1994). 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 incident casualty rate. That is, other things being equal,

[1] 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.).

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's (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), there are disagreements regarding how to define *assault weapons* and identify their use in criminal incidents (DiMaggio et al., 2019; Klarevas, 2019). Only one of the articles meeting our inclusion criteria for this policy analyzed crime or violence outcomes by weapon type or high-capacity magazine involvement (Klarevas, Conner, and Hemenway, 2019).

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 2022, 541 (3.7 percent) of the 14,789 firearm-related murders reported in FBI data involved any type of rifle; the type of firearm used in 5,704 of these murders was not specified (FBI, 2023a). Assuming that no substitution in favor of other types of firearms would occur, and murders with missing firearm type information have a similar composition of handgun and long gun involvement, the elimination of all rifle homicides would have decreased the number of firearm-related murders by 3.7 percent. In data on crime guns traced and recovered in the United States in 2022—which are subject to far less missingness but do not necessarily reflect firearms used in violent crime—about 11 percent were rifles (Bureau of Alcohol, Tobacco, Firearms and Explosives [ATF], 2023c). An analysis of crime guns in select U.S. cities suggests that high-capacity magazine firearms, a broader class of weapon that includes what are commonly considered assault weapons, are involved in a higher percentage (about 20 to 40 percent) of gun crimes (Koper et al., 2018).

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 on mass shootings or active-shooter events suggest that 10 to 30 percent of incidents involved assault weapons and 40 to 60 percent of incidents involved high-capacity magazines (Smart and Schell, 2021; Koper et al., 2018; The Violence Project, undated-a). Another analysis that focused on mass shooting events involving four or more fatalities found that, of all mass shooting incidents between 2009 and 2022 in which the weapon type was known, 20.8 percent involved an assault weapon, a high-capacity magazine, or both (Everytown for Gun Safety Support Fund, 2023). These incidents resulted in nearly five times the number of people shot compared with incidents that did not involve a high-capacity magazine or assault weapon. Given these differences in injury rates, combined with the different prevalence of mass shootings that do versus do not involve high-capacity maga-

zines, one analysis (Koper, 2020) projected that eliminating the ability of mass shooters to use high-capacity magazines could result in a nearly 25-percent reduction in total mass shooting injuries. This calculation assumes that victim differentials are driven entirely by access to a high-capacity magazine, which may not be plausible. For example, 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 during 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 49 felonious fatal shootings of law enforcement officers in 2022, six (12 percent) were known to involve any type of rifle, but the type of firearm was reported for only 19 cases (39 percent) (FBI, 2023d). An analysis of firearms used in 219 murders of police found that 16 percent and 41 percent of firearms used in murders of police officers involved semiautomatic rifles or high-capacity-magazine-compatible firearms, respectively (Koper et al., 2018).

There is little theoretical or logical basis to suggest that bans of assault weapons and high-capacity magazines would affect rates of suicide or unintentional injury. And although these policies could plausibly affect defensive gun use, the magnitudes of any such effects are likely small. The FBI reported that, in 2022, just 11 of the 411 firearm-related justifiable homicides by private citizens involved any type of rifle, although the type of firearm was not known for 115 of these homicides (FBI, 2023b).

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). Indeed, one study that estimated a model of the national civilian firearms market found that domestic firearm sales were significantly lower during the period when the federal assault weapon ban was in place, while other federal laws (e.g., Gun-Free School Zones Act, Brady Act) showed no significant association with sales (McDougal, Montolio, and Brauer, 2023). The overall 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 affect 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. If the ban were announced in advance, anticipatory purchases could mitigate any effects the law might otherwise have on weapon availability; one study of an enforcement notice issued in Massachusetts to announce a new interpretation of aspects of the state's assault weapon ban found clear evidence of anticipatory behavior, with assault rifle sales increasing by more than 500 percent in the five days after issuance of the notice (Balakrishna and Wilbur, 2022).

Overall, the effects of these policies will depend largely on the design and implementation of such policies. Except for heavily regulated weapons manufactured prior to May 1986, assault weapons capable of automatic fire are not available for legal 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 19.8 million modern sporting rifles were in circulation in the United States in 2020 (National Shooting Sports Foundation, 2020a). Bans that are accompanied by a compensatory buyback scheme—as was implemented with Australia's 1996 National Firearms Agreement (for more information, see Ramchand and Saunders, 2021)—could arguably have far larger effects.

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

As of January 1, 2024, ten states and the District of Columbia ban assault weapons.[2] Six of these eleven 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 from jurisdiction to jurisdiction, 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."[3] 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.[4] For example, the law states that a "semiautomatic, centerfire

[2] California, Connecticut, Delaware, Hawaii, Illinois, Maryland, Massachusetts, New Jersey, New York, Washington, and the District of Columbia. See Calif. Penal Code § 30505; Conn. Gen. Stat. § 53-202b; Del. Code Tit. 11 § 1466; Hawaii Rev. Stat. Ann. § 134-4; Ill. Compiled Statutes 720 ILCS 5/24-1 (a)(11-16); Md. Code Ann. § 4-303; Mass. Gen. Laws Ch. 140 § 131M; N.J. Stat. Ann. § 2C:39-1 and 39-5; N.Y. Penal Law § 265.02; Wash. Rev. Code 9.41.010; 9.41.0001; D.C. Code Ann. § 7-2502.02.

[3] Calif. Penal Code § 30510.

[4] Calif. Penal Code § 30515.f.

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."[5]

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.[6] The District of Columbia's list is shorter and does 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, 1994), 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—notably, 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, Rhode Island, and Vermont.[13] Hawaii, which bans only assault pistols, similarly bans only high-capacity magazines for pistols.[14] The rest ban high-capacity

[5] Calif. Penal Code § 30515.

[6] Conn. Gen. Stat. § 53-202a.

[7] D.C. Code Ann. § 7-2501.01.

[8] Md. Code Ann. § 4-301.

[9] Mass. Gen. Laws Ch. 140 § 121.

[10] N.J. Stat. Ann. § 2C: 39-1.

[11] N.Y. Penal Law § 265.00; Hawaii Rev. Stat. Ann. § 134-1.

[12] Calif. Penal Code § 30605; Conn. Gen. Stat. § 53-202c; Hawaii Rev. Stat. Ann. § 134-4; Md. Code Ann. § 4-303; Mass. Gen. Laws Ch. 140 §§ 121, 123, 131, 131M; N.J. Stat. Ann. § 2C: 58-12; N.Y. Penal Law § 265.20.

[13] Colo. Rev. Stat. Ann. §§ 18-12-301, 303; R.I. Gen. Laws §§ 11-47.1-3 (a); 13 Vt. Stat. Ann. § 4021.

[14] Hawaii Rev. Stat. Ann. § 134-8.

magazines for all firearms,[15] although there are differences in definition here too. California, Connecticut, Hawaii, Maryland, Massachusetts, New Jersey, New York, Rhode Island, 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] Vermont bans long gun magazines that can hold more than ten rounds and handgun magazines that can hold more than 15 rounds.[18]

Effects on Suicide

Research Synthesis Findings

One study (Siegel et al., 2019) met our inclusion criteria and estimated the effects of state-level assault weapon bans and high-capacity magazine bans on state suicide rates from 1991 to 2016. The authors defined *high-capacity magazine bans* as laws that banned the sale of both assault pistol ammunition and magazines with a capacity of more than ten rounds of ammunition. Using log-linear regression models with controls for year and state fixed effects, state social and economic covariates, and nine other firearm laws, the authors estimated uncertain associations of both types of state-level bans with firearm and total suicides. Estimates for the effects of state assault weapon bans were based on policy changes in only three states with at least three years of pre-policy and post-policy data, likely resulting in inaccurate estimates of uncertainty and weakening generalizability. Additionally, the models controlled for a proxy measure of household gun ownership that is a linear combination of the ratio of firearm suicides to suicides and hunting licenses. Thus, the models included total suicides and firearm suicides as both dependent and independent variables, which is likely to invalidate the results.

Figure 14.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the assault weapon ban and high-capacity magazine ban policies examined in this study.

Conclusions

We identified one qualifying study that estimated the effects of assault weapon bans and high-capacity magazine bans on suicide, which found uncertain effects of such bans on total

[15] Calif. Penal Code § 32310; Colo. Rev. Stat. § 18-12-302; Conn. Gen. Stat. § 53-202w; Md. Code Ann. § 4-305; Mass. Gen. Laws Ch. 140 § 131M; N.J. Stat. Ann. § 2C: 39-3; N.Y. Penal Law § 265.02; D.C. Code Ann. § 7-2506.01.

[16] Calif. Penal Code §§ 16350, 16740; Conn. Gen. Stat. § 53-202w; Hawaii Rev. Stat. Ann. § 134-8; Md. Code Ann. § 4-305; Mass. Gen. Laws Ch. 140 § 121; N.J. Stat. Ann. § 2C: 39-1; N.Y. Penal Law § 265.00; D.C. Code Ann. § 7-2506.01; R.I. Gen. Laws §§ 11-47.1-3.

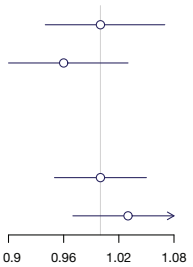
[17] Colo. Rev. Stat. § 18-12-301.

[18] Vt. Stat. Ann. Tit. 13, § 4021(e)(1).

FIGURE 14.1

Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Suicide

Study, by Policy	Outcome Measure	IRR [95% CI]
Assault weapon ban	**Suicide rate**	
Siegel et al. (2019)	Total	1.00 [0.94, 1.07]
Siegel et al. (2019)	Firearm	0.96 [0.90, 1.03]
Ban on HCM and assault pistol ammunition		
Siegel et al. (2019)	Total	1.00 [0.95, 1.05]
Siegel et al. (2019)	Firearm	1.03 [0.97, 1.09]

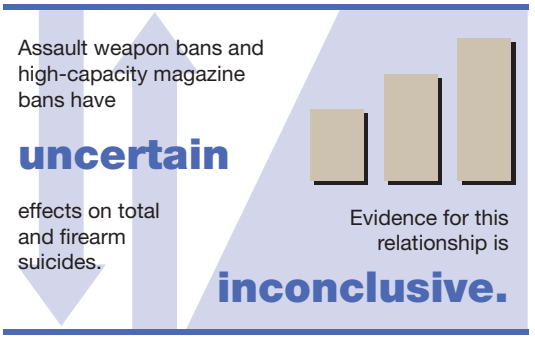


NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details. HCM = high-capacity magazine.

and firearm suicide rates (Siegel et al., 2019). The available evidence is thus *inconclusive for the effects of assault weapon bans and high-capacity magazine bans on total and firearm suicides*.

Effects on Violent Crime

Research Synthesis Findings



We identified seven studies that evaluated federal and state assault weapon bans and met our inclusion criteria, two of which (Fridel, 2021a; Neufeld et al., 2022) contribute new findings to this updated review.[19]

Lott (2010) examined the effect of assault weapon bans on violent crime using state-level data from 1977 to 2005. Detailed results that include coefficients and test statistics were available only for the outcome of homicide (Lott, 2010, Table A6.3). Using a two-way fixed-effects model that controlled for more than 70 time-varying state-level covariates, the author estimated an uncertain effect of assault weapon bans on homicide rates. Using a similar but more parsimonious approach, Gius (2014) analyzed state-level data from

[19] The study by Neufeld et al. (2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accidents. Because there is evidence suggesting that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

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. For state-level policies, the author found a suggestive effect consistent with state assault weapon bans decreasing firearm-related homicides (see Figure 14.2). However, the models of both Lott (2010) and Gius (2014) did not account for serial correlation in panel data, which can result in serious biases in standard errors.

A 2018 study 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 and 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 ($p = 0.19$).

Using more-recent data, Siegel et al. (2019) estimated the effects of state-level assault weapon bans and bans of high-capacity ammunition magazines on state homicide rates from 1991 to 2016. The authors defined *high-capacity magazine bans* as laws that banned the sale of both assault pistol ammunition and magazines with a capacity of more than ten rounds of ammunition. Using log-linear regression models with controls for year and state fixed effects, state social and economic covariates, and nine other firearm laws, the authors found suggestive effects for assault weapon bans reducing homicides and firearm homicides by approximately 10 percent in their primary models. Bans on high-capacity magazines had uncertain effects on these outcomes. However, the study period included only three states that implemented assault weapon bans and had at least three years of data before and after the law was implemented, which creates serious concerns about inaccurate estimates of uncertainty and weakens external generalizability. Analyzing the same time frame using a negative binomial regression model with controls for year and state fixed effects, the lagged homicide rate, and a larger set of socioeconomic and policy covariates, Fridel (2021a) found significant effects for assault weapon bans reducing homicides by about 14 percent and uncertain effects of banning high-capacity magazines. However, the purpose of this study was to evaluate shall-issue laws, and other firearm laws were included only as ancillary covariates. Consequently, this study was not designed to infer effects of assault weapon bans, and there are serious concerns with using these estimates to judge the effectiveness of such bans. Sabbath, Hawkins, and Baum (2020) examined the effects on workplace homicides of state laws that strengthened bans on the purchase, transfer, and possession of assault weapons or high-capacity magazines between 2011 and 2017. The primary focus of the study was an index of 132 laws, but

the authors also separately fit indexes for 13 subcategories of laws, estimating effects using log-link models with gamma-distributed errors that controlled for year fixed effects and six socioeconomic variables. The index composed of assault weapon and high-capacity magazine ban features showed an uncertain relationship with workplace homicides. There were, at most, only two states with changes to their laws and at least three years of data before and after implementation of the changes, so the effects of this index are not well identified in this model. In addition, it seems unlikely that the six socioeconomic variables used as covariates in the model fully account for confounding factors related to both the adoption of these laws and workplace homicide rates.[20]

Finally, Neufeld et al. (2022) used data from 2000 to 2016 to evaluate how laws limiting magazines to ten rounds or fewer influence firearm injury hospitalization rates. Using two-way fixed-effects regression models that adjusted for a small set of state-level time-varying covariates (demographic and socioeconomic characteristics, a proxy measure for household firearm ownership, the violent crime rate, and per capita alcohol consumption) and other state firearm laws, the authors found uncertain effects of these requirements on firearm injury hospitalizations. However, these effects were based on law transitions in fewer than four states with three years of pre-policy and post-policy data, which raises serious concerns about statistical power, generalizability to other contexts, and the ability to isolate the cause of any observed change to the passage of the law versus other co-occurring factors.

Figure 14.2 displays the IRRs and 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.

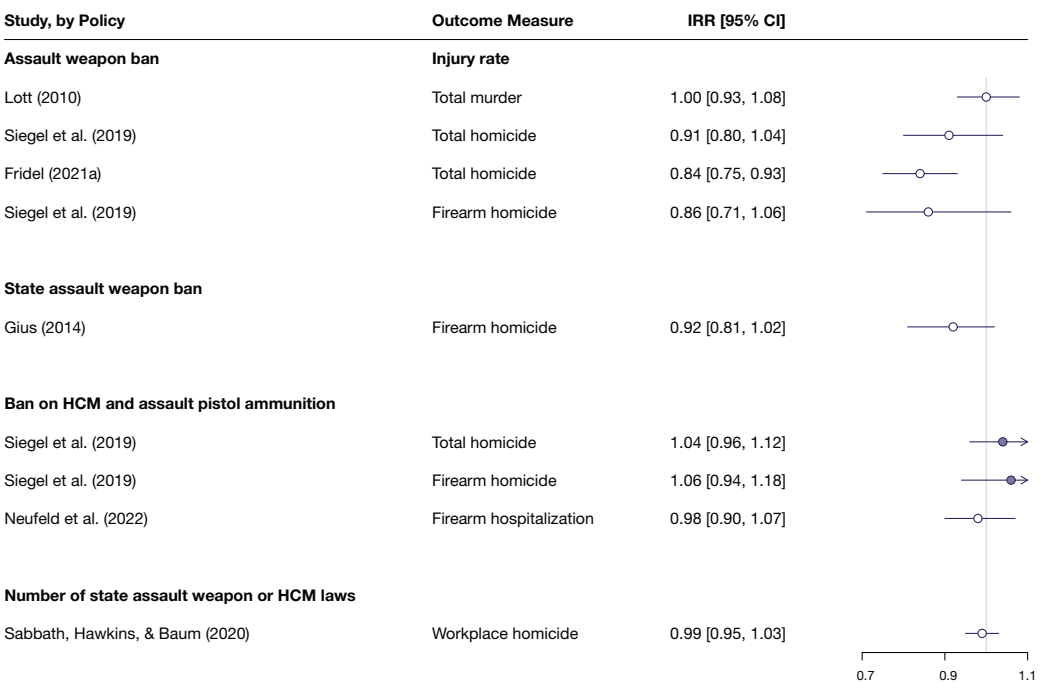
Conclusions

We identified seven qualifying studies that estimated the effects of assault weapon bans or high-capacity magazine bans on different violent crime outcomes. One found uncertain effects of assault weapon bans on total homicide rates (Lott, 2010), two found suggestive effects consistent with assault weapon bans decreasing firearm homicides (Gius, 2014; Siegel et al., 2019) or total homicides (Siegel et al., 2019), and one found significant effects consistent with assault weapon bans decreasing total homicides (Fridel, 2021a). Four studies found uncertain effects of high-capacity magazine bans on total homicides (Fridel, 2021a; Siegel et al., 2019), firearm homicides (Moody and Marvell, 2018b; Siegel et al., 2019), or firearm injury hospitalizations (Neufeld et al., 2022). Finally, one study found uncertain effects of assault weapon or high-capacity magazine bans on workplace homicides (Sabbath, Hawkins,

[20] The six covariates included in this analysis were unemployment rate, percentage of residents below the federal poverty line, percentage of Black residents, percentage of residents with a college education, percentage of male residents, and nonhomicide violent crime rate.

FIGURE 14.2

Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Violent Crime



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. See Appendix B for details. HCM = high-capacity magazine.

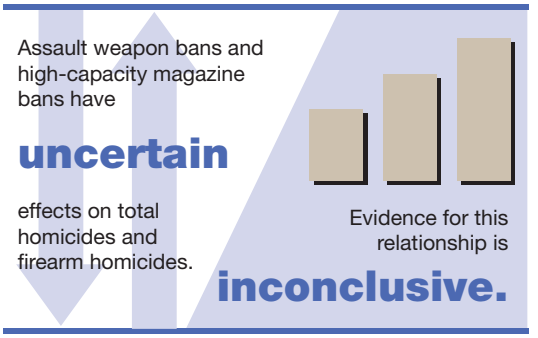
and Baum, 2020). Considering the relative strengths of these studies, available evidence is *inconclusive for the effect of assault weapon bans on total homicides and firearm homicides.* Similarly, we find *inconclusive evidence for the effect of high-capacity magazine bans on total and firearm homicides.*

Effects on Mass Shootings

Research Synthesis Findings

We identified eight studies that met our inclusion criteria and estimated the effects of state or federal assault weapon bans on multiple-victim shooting incidents or casualties. Two of these studies (Fridel, 2021a; Siegel et al., 2020a) contribute new findings to this updated review.[21] Three studies (Blau, Gorry, and Wade, 2016; Klarevas, Conner, and Hemenway,

[21] We exclude DiMaggio et al. (2019), who studied the federal assault weapon ban; we determined that the study does not meet our inclusion criteria because of the absence of a comparison group. Although the



2019; Luca, Malhotra, and Poliquin, 2020) were rated as having critical methodological concerns; they are not discussed further and they do not contribute to our conclusions.[22]

The studies that we identified used a variety of definitions of *mass shooting*, which we highlight in the following sections. Gius (2015c) focused on *public mass shootings*, defined as incidents unrelated to other criminal activity that occurred in a public place and resulted in four or more firearm-related fatalities, excluding the offender, whose victims were chosen indiscriminately. Using a Poisson model and data from 1982 through 2011, Gius (2015c) tested whether state assault weapon bans influence these shooting fatalities or injuries, controlling for the federal assault weapon ban and state-level variations in demographic, socioeconomic, and criminal justice characteristics.[23] 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 14.3). 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 data from 1984 to 2017, Webster et al. (2020) again set the same casualty threshold (four or more killed, not including the shooter) and excluded drug- or gang-related shootings. They did not exclude domestic-related incidents but conducted sensitivity analyses

authors focus on an outcome defined as the proportion of all firearm homicide deaths that occurred during mass shootings, this is not equivalent to having a comparison group; mass shooting deaths are included in both the numerator and the denominator. Furthermore, the use of firearm homicides as a comparison group has obvious methodological problems, given that the assault weapon ban (and other 1994 legislation passed coincident with the assault weapon ban) might be expected to affect firearm homicide rates.

[22] Klarevas, Conner, and Hemenway's (2019) analysis used an estimated 63 parameters to model only 69 mass shooting incidents over a 27-year period. The model estimates are likely biased because of the sparsity of the outcome (Kenne Pagui, Salvan, and Sartori, 2022). Similarly, the analysis of Luca, Malhotra, and Poliquin (2020) and Blau, Gorry, and Wade (2016) both had fewer than five mass shooting incidents per estimated model parameter, which poses a critical risk to model fit. Both Blau, Gorry, and Wade (2016) and Luca, Malhotra, and Poliquin (2020) used linear models to predict a rare dichotomous outcome, which likely renders their estimates' confidence intervals unreliable because of a violation of model assumptions.

[23] The author found a large and statistically significant association between the 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.

stratified by this characteristic. In negative binomial models including 14 laws, state fixed effects, linear and quadratic time trend terms, and 14 social and economic state covariates, the authors found uncertain evidence that assault weapon bans were associated with mass shooting incidents or deaths. However, they found that states with high-capacity magazine bans had a significant 48-percent reduction in mass shooting incidents, and a suggestive 33-percent reduction in mass shooting fatalities. Because the number of incidents and fatalities is quite low over this period, maximum likelihood estimates like those used in this study may be biased (Kenne Pagui, Salvan, and Sartori, 2022).

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 model similar to that in Gius (2015c), this study evaluated 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. The study showed that the presence of a state or federal assault weapon ban between 1990 and 2014 was significantly associated with a 54-percent reduction in the number of school shooting victims (see Figure 14.3). This study had only minor statistical concerns related to sample size and potential model overfit, but it is unclear the extent to which this estimate identified the effect of the change in the federal law versus the effects of state-level policy changes.

Siegel et al. (2020a) examined effects of state assault weapon laws and high-capacity magazine bans on the occurrence of mass public shooting events at the state-year level between 1976 and 2018. The authors aggregated multiple data sources to construct a dataset of *mass public shootings*, defined as incidents in which four or more individuals are fatally shot in a public location within a 24-hour period in the absence of other criminal activities. In the first stage of their analysis, the authors modeled whether there was at least one mass shooting in each state-year using logistic regression within a generalized estimating equations framework to account for state-level clustering. This analysis showed a suggestive negative association ($p = 0.18$) between high-capacity magazine bans and the occurrence of a mass shooting and an uncertain association for assault weapon bans.[24] This analysis had only minor methodological concerns related to the limited number of years before and after policy shifts and potential model overfit because there were fewer than ten mass shooting events per model parameter in the logistic regression.

Fridel (2021a) examined *mass murders*, defined as incidents in which an offender intentionally kills four or more people (excluding the offender and unborn children) within a

[24] Siegel et al. (2020a) included additional analyses using zero-inflated negative binomial models to estimate the association of the policies with the number of fatalities and nonfatal injuries per mass shooting event. This analysis presented more serious concerns than the model of any mass shooting occurring because a limited number of population covariates were included to adjust for confounding. Our conclusions were based on only the logistic regression of any mass shooting occurrence.

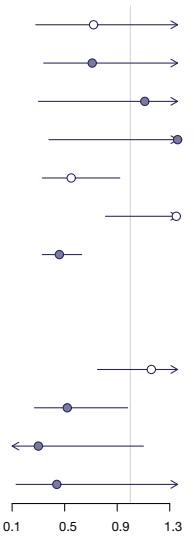
24-hour period, regardless of location. The study focused on the effects of household gun ownership and concealed-carry laws on mass murders between 1991 and 2016, but it included a set of results that showed estimated coefficients for a variety of laws that were included in the regression models as controls for potential confounds, including those for high-capacity magazine and assault weapon bans. These results showed high uncertainty in the association between the bans and mass murders. Because the study was not designed to estimate the association of the bans with the outcomes, there are serious methodological issues in relying on this finding to draw conclusions on the policy's causal effect on mass shootings. Figure 14.3 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).

Conclusions

We identified five qualifying studies without critical methodological concerns 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

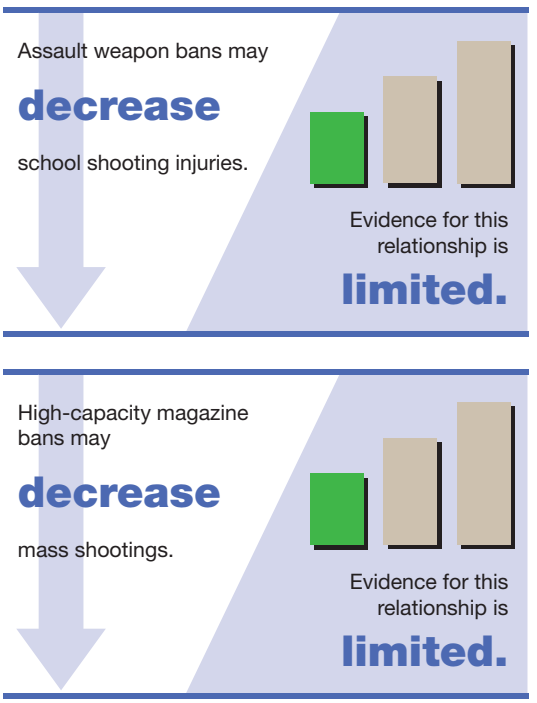
FIGURE 14.3
Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
Assault weapon ban		
Fridel (2021a)	Mass murder incidents	0.72 [0.28, 1.90]
Webster et al. (2020)	Mass shooting incidents	0.71 [0.34, 1.48]
Webster et al. (2020)	Mass shooting deaths	1.11 [0.30, 4.16]
Siegel et al. (2020a)	Any mass public shooting	1.36 [0.38, 4.86]
Gius (2015c)	Mass public shooting deaths	0.55 [0.33, 0.92]
Gius (2015c)	Mass public shooting injuries	1.35 [0.81, 2.23]
Gius (2018)	Deaths & injuries from school shootings	0.46 [0.33, 0.63]
High-capacity magazine ban		
Fridel (2021a)	Mass murder incidents	1.16 [0.75, 1.80]
Webster et al. (2020)	Mass shooting incidents	0.52 [0.27, 0.98]
Webster et al. (2020)	Mass shooting deaths	0.30 [0.08, 1.10]
Siegel et al. (2020a)	Any mass public shooting	0.44 [0.13, 1.44]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. See text and Appendix B for details.

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. Fridel (2021a), Siegel et al. (2020a), and Webster et al. (2020) found uncertain associations between state assault weapon bans and mass shooting incidents and fatalities. In light of these findings and the relative strengths of these studies, we find *inconclusive evidence for the effects of assault weapon bans on mass shootings*. Based on Gius (2018), we find *limited evidence that assault weapon bans reduce school shooting injuries.* Webster et al. (2020) found significant or suggestive associations between state bans of high-capacity magazines and lower rates of mass shooting incidents. Siegel et al. (2020a) found suggestive evidence that these bans reduce the likelihood that a state will have a mass shooting in a given year. Fridel (2021a) found an uncertain association of high-capacity magazine bans with mass murders, but these policies were an ancillary covariate in a model not designed to assess their effects. Considering our assessment of these findings and the studies' relative strengths, we find *limited evidence that high-capacity magazine bans reduce mass shootings*.



Effects on Police Shootings

Research Synthesis Findings

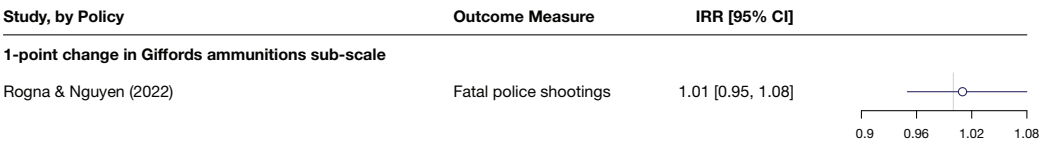
Our updated search identified one new study that met our inclusion criteria and examined fatal police shootings (Rogna and Nguyen, 2022). Rogna and Nguyen (2022) drew on 2012–2018 data from the crowdsourced Fatal Encounters database to evaluate the effects of more-restrictive assault weapons policies, as measured by the Giffords Law Center's hardware and ammunitions scale. This composite index largely characterizes the restrictiveness of state policies that regulate access to automatic and semiautomatic weapons and high-capacity magazines, although it also considers policies related to the regulation of ammunition, silencers, and ghost guns more broadly. Using two-way fixed-effects models (either Poisson or linear with a power-transformed outcome), the authors found uncertain relationships between stricter regulation of assault weapons and police shooting fatalities.[25] The study has serious limitations because it lacks clarity regarding the treatment variable, which prevents us from evaluating whether there is sufficient policy variation to draw reliable inferences about policy effects during a relatively short time span. Figure 14.4 displays the IRR and CIs associated with a one-point increase in the Giffords hardware and ammunitions scale as examined in Rogna and Nguyen (2022).

Conclusions

We identified one qualifying study that found an uncertain relationship between fatal police shootings and a policy index that included assault weapon restrictions (Rogna and Nguyen, 2022). Based on this single study, we find *inconclusive evidence for the effect of assault weapon and high-capacity magazine bans on police shootings.*

FIGURE 14.4

Incidence Rate Ratio Associated with the Effect of Assault Weapon Bans on Police Shootings



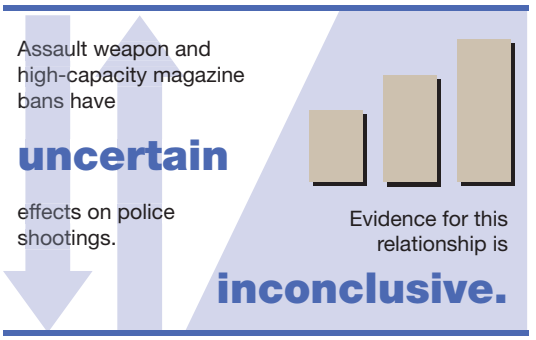
NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. See Appendix B for details.

[25] Rogna and Nguyen's (2022) preferred model specification used a Poisson link function to estimate the rate of fatal police shootings directly rather than the standard practice of modeling the count and including a population offset. Consequently, we prioritize results from the authors' supplementary linear two-way fixed-effects models, but both produce similar results. However, because the authors' linear specification uses a transform of the dependent variable that complicates interpretation, Figure 14.4 presents IRR results from the Poisson specification.

Effects on the Gun Industry

Research Synthesis Findings

We identified two studies examining the effects of assault weapon bans on gun industry outcomes. In an update to earlier studies (Koper and Roth, 2001, 2002; Roth and Koper, 1997, 1999), Koper (2004) assessed the effects of the federal assault weapon ban on prices of assault weapons in secondary markets purchased before 1994 and thus not prohibited from being sold under the terms of the federal ban. The study 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 prices 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 prices 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 that had been 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.

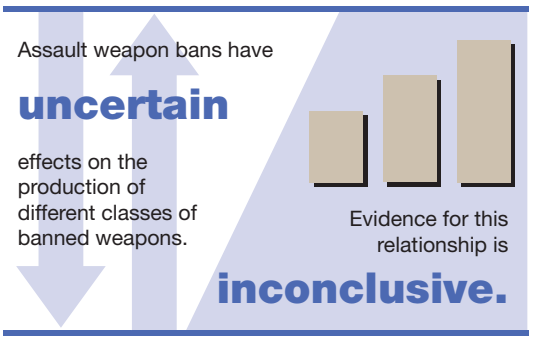
A second study (Balakrishna and Wilbur, 2022) examined the effects of Massachusetts' state-level assault weapon ban on gun sales. Rather than directly assessing the effects of this ban, Balakrishna and Wilbur (2022) evaluated the changes following a 2016 enforcement notice issued by the Massachusetts Office of the Attorney General. This notice clarified the types of firearms banned under the state's legal definition of assault weapons. The study used

daily expected firearm sales if the clarification had not been issued based on lagged sales, linear and quadratic trends, and several time fixed effects representing such factors as differences between weekdays and weekends. The authors did not quantify uncertainty in their estimates of the difference between the observed and counterfactual trends or conduct formal statistical inference, which represents a critical methodological concern. There are additional serious concerns related to the use of a single state and the model's identifying assumptions.

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. IRRs from Balakrishna and Wilbur (2022) are also not presented because of critical concerns with the study's methodological approach.

Conclusions

One study without critical methodological concerns 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: Production of assault pistols and assault rifles rose 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:

- unintentional injuries and deaths
- defensive gun use
- hunting and recreation.

Chapter 14 References

Aldrich, John H., and Forrest D. Nelson, *Linear Probability, Logit, and Probit Models*, Sage Publications, 1984.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, *The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy*, Stanford Law School, Olin Working Paper No. 461, December 1, 2014.

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

Balakrishna, Meenakshi, and Kenneth C. Wilbur, "Do Firearm Markets Comply with Firearm Restrictions? How the Massachusetts Assault Weapons Ban Enforcement Notice Changed Registered Firearm Sales," *Journal of Empirical Legal Studies*, Vol. 19, No. 1, March 2022.

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Bureau of Alcohol, Tobacco, Firearms and Explosives, *Firearms Trace Data—2022*, webpage, October 11, 2023c. As of December 1, 2023:
https://www.atf.gov/resource-center/firearms-trace-data-2022

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

Conley, Timothy G., and Christopher R. Taber, "Inference with 'Difference in Differences' with a Small Number of Policy Changes," *Review of Economics and Statistics*, Vol. 93, No. 1, 2011.

Cox, D. R., and E. J. Snell, *Analysis of Binary Data*, 2nd ed., Chapman and Hall, 1989.

DiMaggio, Charles, Jacob Avraham, Cherisse Berry, Marko Bukur, Justin Feldman, Michael Klein, Noor Shah, Manish Tandon, and Spiros Frangos, "Changes in US Mass Shooting Deaths Associated with the 1994–2004 Federal Assault Weapons Ban: Analysis of Open-Source Data," *Journal of Trauma and Acute Care Surgery*, Vol. 86, No. 1, January 2019.

Everytown for Gun Safety Support Fund, "Mass Shootings in the United States," webpage, March 2023. As of October 22, 2023:
https://everytownresearch.org/mass-shootings-in-america/

FBI—*See* Federal Bureau of Investigation.

Federal Bureau of Investigation, "Expanded Homicide Data Table 8: Murder Victims, by Weapon, 2018–2022," webpage, Crime in the United States 2022, 2023a.

Federal Bureau of Investigation, "Expanded Homicide Data Table 15: Justifiable Homicide, by Weapon, Private Citizen, 2018–2022," webpage, Crime in the United States 2022, 2023b.

Federal Bureau of Investigation, "Table 28: Law Enforcement Officers Feloniously Killed, Type of Weapon, 2013–2022," webpage, Law Enforcement Officers Killed and Assaulted, 2022, 2023d.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Giffords Law Center to Prevent Gun Violence, "Assault Weapons," webpage, undated-a. As of October 18, 2017:
http://lawcenter.giffords.org/gun-laws/policy-areas/hardware-ammunition/assault-weapons/

Gius, Mark, "An Examination of the Effects of Concealed Weapons Laws and Assault Weapons Bans on State-Level Murder Rates," *Applied Economics Letters*, Vol. 21, No. 4, 2014.

Gius, Mark, "The Impact of State and Federal Assault Weapons Bans on Public Mass Shootings," *Applied Economics Letters*, Vol. 22, No. 4, 2015c.

Gius, Mark, "The Effects of State and Federal Gun Control Laws on School Shootings," *Applied Economics Letters*, Vol. 25, No. 5, 2018.

Helland, E., and A. Tabarrok, "Using Placebo Laws to Test 'More Guns, Less Crime,'" *Advances in Economic Analysis and Policy*, Vol. 4, No. 1, 2004.

Kalesan, Bindu, Kinan Lagast, Marcos Villarreal, Elizabeth Pino, Jeffrey Fagan, and Sandro Galea, "School Shootings During 2013–2015 in the USA," *Injury Prevention*, Vol. 23, No. 5, 2017.

Kenne Pagui, Euloge Clovis, Alessandra Salvan, and Nicola Sartori, "Improved Estimation in Negative Binomial Regression," *Statistics in Medicine*, Vol. 41, No. 13, 2022.

Klarevas, Louis, "Letter to the Editor Re: DiMaggio, C. et al. 'Changes in US Mass Shooting Deaths Associated with the 1994–2004 Federal Assault Weapons Ban: Analysis of Open-Source Data,'" *Journal of Trauma and Acute Care Surgery*, Vol. 86, No. 5, 2019.

Klarevas, Louis, Andrew Conner, and David Hemenway, "The Effect of Large-Capacity Magazine Bans on High-Fatality Mass Shootings, 1990–2017," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Kleck, Gary, "Large-Capacity Magazines and the Casualty Counts in Mass Shootings: The Plausibility of Linkages," *Justice Research and Policy*, Vol. 17, No. 1, 2016.

Klein, Jessie, *The Bully Society: School Shootings and the Crisis of Bullying in America's Schools*, New York University Press, 2012.

Koper, Christopher S., *Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence 1994–2003*, National Institute of Justice, 2004.

Koper, Christopher S., "Assessing the Potential to Reduce Deaths and Injuries from Mass Shootings Through Restrictions on Assault Weapons and Other High-Capacity Semiautomatic Firearms," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Koper, Christopher S., William D. Johnson, Jordan L. Nichols, Ambrozine Ayers, and Natalie Mullins, "Criminal Use of Assault Weapons and High-Capacity Semiautomatic Firearms: An Updated Examination of Local and National Sources," *Journal of Urban Health*, Vol. 95, No. 3, 2018.

Koper, Christopher S., and Jeffrey A. Roth, "The Impact of the 1994 Federal Assault Weapon Ban on Gun Violence Outcomes: An Assessment of Multiple Outcome Measures and Some Lessons for Policy Evaluation," *Journal of Quantitative Criminology*, Vol. 17, No. 1, 2001.

Koper, Christopher S., and Jeffrey A. Roth, "Impact of the 1994 Federal Assault Weapons Ban on Gun Markets: An Assessment of Short-Term Primary and Secondary Market Effects," *Journal of Quantitative Criminology*, Vol. 18, No. 3, 2002.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

McDougal, Topher L., Daniel Montolio, and Jurgen Brauer, "Modeling the U.S. Firearms Market," *International Social Science Journal*, Vol. 73, No. 248, June 2023.

Moody, Carlisle E., and Thomas B. Marvell, "Clustering and Standard Error Bias in Fixed Effects Panel Data Regressions," *Journal of Quantitative Criminology*, May 2018b.

National Shooting Sports Foundation, "Modern Sporting Rifle: Introduction," webpage, undated. As of October 14, 2019:
https://www.nssf.org/msr/

National Shooting Sports Foundation, "NSSF Releases Most Recent Firearm Production Figures," webpage, November 16, 2020a. As of August 31, 2022:
https://www.nssf.org/articles/nssf-releases-most-recent-firearm-production-figures/

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Plumer, Brad, "Everything You Need to Know About the Assault Weapons Ban, in One Post," *Washington Post*, December 17, 2012.

Public Law 103-322, Violent Crime Control and Law Enforcement Act of 1994, 1994.

Ramchand, Rajeev, and Jessica Saunders, "The Effects of the 1996 National Firearms Agreement in Australia on Suicide, Homicide, and Mass Shootings," in Rajeev Ramchand and Jessica Saunders, eds., *Contemporary Issues in Gun Policy: Essays from the RAND Gun Policy in America Project*, RAND Corporation, RR-A243-2, 2021. As of August 31, 2022:
https://www.rand.org/pubs/research_reports/RRA243-2.html

Rogna, Marco, and Bich Diep Nguyen, "Firearms Law and Fatal Police Shootings: A Panel Data Analysis," *Applied Economics*, Vol. 54, No. 27, 2022.

Roth, Jeffrey A., and Christopher S. Koper, *Impact Evaluation of the Public Safety and Recreational Firearms Use Protection Act of 1994: Final Report*, Urban Institute, 1997.

Roth, Jeffrey A., and Christopher S. Koper, *Impacts of the 1994 Assault Weapons Ban: 1994–96*, National Institute of Justice, March 1999.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Smart, Rosanna, and Terry L. Schell, "Mass Shootings in the United States," in Rajeev Ramchand and Jessica Saunders, eds., *Contemporary Issues in Gun Policy: Essays from the RAND Gun Policy in America Project*, RAND Corporation, RR-A243-2, 2021. As of August 31, 2022:
https://www.rand.org/pubs/research_reports/RRA243-2.html

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

U.S. Department of Justice, "Violent Crime Control and Law Enforcement Act of 1994 Fact Sheet," October 24, 1994. As of May 30, 2017:
https://www.ncjrs.gov/txtfiles/billfs.txt

The Violence Project, homepage, undated-a. As of April 23, 2024:
https://www.theviolenceproject.org

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

CHAPTER 15

Bans on Low-Quality Handguns

The Gun Control Act of 1968 (Pub. L. 90-618, 1968) 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 acts 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 rounds could 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 regulations governing the sale and use of low-quality handguns, 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 the firearm's 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 are 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 also may 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. Other research has found that bans on such guns reduce their use in crimes, suggesting that policy changes affect criminal behavior and not just the behaviors of law-abiding citizens (Vernick, Webster, and Hepburn, 1999).

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 (about $165 in 2022), 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).[1]

[1] The Bureau of Alcohol, Tobacco, and Firearms (2002, p. A-3) defined a *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

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 small, inexpensive handguns, 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 mark-ups 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, Hureau and Braga (2018) 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 reduce criminal access to firearms, these bans may also decrease the number of police 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 a firearm successfully for self-defense—and without self-injury—among those who do purchase one for self-protection.

It is also unclear 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

also be categorized as a crime gun if it is suspected it was used in a crime or illegally possessed."

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, so reducing their availability could increase the probability of detecting a potential mass shooter before such an individual perpetrates violence. A price increase resulting from quality regulations might also deter some potential mass shooting perpetrators who have limited budgets. 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]

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

[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. Legislation signed in California in 2022 requires new state standards for firearm safety, marketing, and sale, and allows individuals, local governments, and the Attorney General of California to sue firearm suppliers for damages resultant from failure to meet these standards; the law is expected to be challenged in court (Healey, 2022).

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 14). 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, 2024, seven states and the District of Columbia have laws implementing design safety standards for handgun manufacturers.[3] 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,[4] while Hawaii, Illinois, Massachusetts, Minnesota, and New York have laws imposing a melting point test.[5] California, Maryland, Massachusetts, and the District of Columbia publish lists of approved handguns for personal ownership.[6]

California, Massachusetts, and New York also require handguns to have specific safety features.[7] 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 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. For a firearm to be sold by a licensed firearm distributor in the state, a manufacturer must prove that the firearm meets the state's requirements. However,

[3] California, Hawaii, Illinois, Maryland, Massachusetts, Minnesota, New York, and the District of Columbia.

[4] Calif. Ann. Penal Code §§ 31900, 31905; Mass. Gen. Laws Ann. 140 § 123; 9 N.Y. CRR 482.6.

[5] Hawaii Rev. Stat. Ann. § 134-15; 720 Ill. Comp. Stat. 5/24-3(A)(h); Mass. Gen. Laws Ann. 140 § 123; Minn. Stat. Ann. § 624.712; 9 N.Y. CRR 482.5.

[6] Calif. Ann. Penal Code § 32015; Md. Code, Pub. Safety, § 5-404; Mass. Gen. Laws Ch. 140 § 1313/4; 24 D.C. MR § 2323.

[7] Calif. Ann. Penal Code §§ 31910, 16900; Mass. Gen. Laws Ch. 140, § 123; 9 N.Y. CRR 482.5.

[8] Calif. Ann. Penal Code § 31910.

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 two studies 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 effects 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, during the period studied, only one state (Maryland) passed a ban on junk guns, which limits effect size generalizability, means that the cluster adjustment to the standard errors is likely to underestimate the variance of the estimated policy effect (Schell, Griffin, and Morral, 2018; Cameron, Gelbach, and Miller, 2008), and creates serious concerns with attributing estimated effects to the ban rather than to other contemporaneous events in Maryland.

Analyzing data from 1991 to 2016, Siegel et al. (2019) estimated the effect on suicide of the enactment (in California and Massachusetts) or repeal (South Carolina) of bans on the sale of low-quality handguns, specifying a log-linear model of suicide rates and controlling for state and year fixed effects, nine other firearm laws, and 12 social and economic time-varying state covariates. Results indicated that bans on low-quality handguns were associated with a significant 6-percent decrease in overall suicide rates and a 13-percent reduction in the firearm suicide rate. This study also had serious methodological concerns. Because Siegel et al.'s (2019) estimates were based on law changes in just three states, standard errors for the estimates are likely underestimated, and results may not generalize well to other implementations of these laws. Additionally, the authors noted that their results failed a falsification test; specifically, they also found a significant decrease for nonfirearm suicides, although the effect size for nonfirearm suicides was substantively smaller and there may be mechanisms by which changes in firearm suicide rates because of the law subsequently affect nonfirearm suicide rates (Smart et al., 2020a).

[9] Hawaii and the District of Columbia. See Hawaii Rev. Stat. Ann. § 134-15(a); D.C. Code Ann. § 7-2505.04(b).

Figure 15.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the low-quality handgun bans examined in Rosengart et al. (2005) and Siegel et al. (2019).

Conclusions

One study (Rosengart et al., 2005) 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. Another study (Siegel et al., 2019) also found that the laws were negatively associated with total and firearm suicide rates. Both studies presented serious methodological concerns. Based on our assessment of these findings and the relative strengths of these studies, we find *inconclusive evidence for the effect of low-quality handgun bans on total and firearm suicides.*

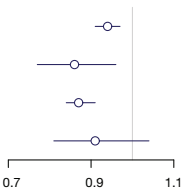
Effects on Violent Crime

Research Synthesis Findings

We identified five 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. One of these studies (Webster, Vernick, and Hepburn, 2002) had critical methodological concerns and is not discussed further.[10] Rosengart et al. (2005) used data from 1979 to 1998 to

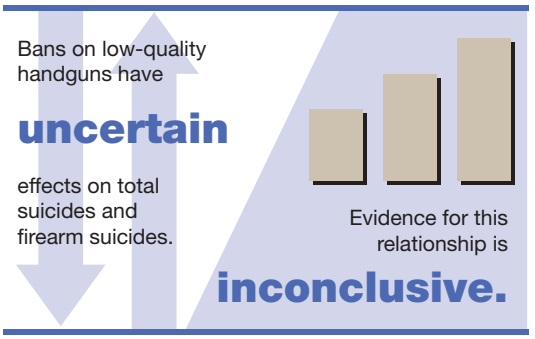
FIGURE 15.1
Incidence Rate Ratios Associated with the Effect of Low-Quality Handgun Bans on Suicide

Study, by Policy	Outcome Measure	IRR [95% CI]
Ban on low-quality handguns	**Suicide rate**	
Siegel et al. (2019)	Total	0.94 [0.91, 0.97]
Rosengart et al. (2005)	Total	0.86 [0.77, 0.96]
Siegel et al. (2019)	Firearm	0.87 [0.84, 0.91]
Rosengart et al. (2005)	Firearm	0.91 [0.81, 1.04]



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

[10] Webster, Vernick, and Hepburn (2002) examine changes in firearm-involved homicide rates in Maryland following a ban on the sale of "Saturday night special" guns. They used a single interrupted time



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, during the period studied, only one state (Maryland) passed a ban on junk guns, which limits the generalizability of the estimated effect, means that the cluster adjustment to the standard errors is likely to underestimate the variance of the estimated policy effect (Cameron, Gelbach, and Miller, 2008; Schell, Griffin, and Morral, 2018), and creates serious concerns with attributing estimated effects to the ban rather than to other contemporaneous events in Maryland.

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 9, 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

series design, which lacks a control group of states with which Maryland's trends could be compared. Moreover, Webster, Vernick, and Hepburn's (2002) data included fewer than five observations per parameter in their statistical model, which poses a critical concern with model overfit.

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.

Finally, analyzing data from 1991 to 2016, Siegel et al. (2019) estimated the effect on homicide of the enactment (in California and Massachusetts) or repeal (in South Carolina) of laws banning the sale of low-quality handguns using a log-linear model that controlled for state and year fixed effects, state social and economic covariates, and nine other firearm laws. The estimated associations between these laws and homicides (total and firearm) were uncertain. Because they were estimated from just three changes in state laws, the estimates from this model may not generalize well to other implementations of similar laws.

Figure 15.2 displays the IRRs and CIs associated with the handgun bans examined in these studies.

Conclusions

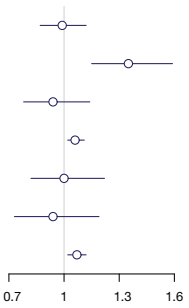
Total homicides. Four studies without critical methodological concerns provided evidence for how bans on low-quality handguns affect total homicide rates. Two studies found an uncertain effect (Rosengart et al., 2005; Siegel et al., 2019), and two studies found that the bans significantly increased homicide rates (Webster, Crifasi, and Vernick, 2014; Lott, 2010). These studies used two independent data sets and covered varying time frames. Because all studies had serious methodological concerns, we find *inconclusive evidence for how bans on low-quality handguns affect total homicides*.

Firearm homicides. Four 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

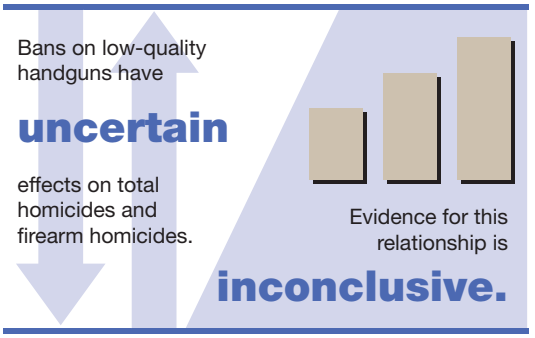
FIGURE 15.2

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

Study, by Policy	Outcome Measure	IRR [95% CI]
Ban on low-quality handguns	**Homicide rate**	
Siegel et al. (2019)	Total	0.99 [0.87, 1.12]
Lott (2010)	Total	1.35 [1.15, 1.59]
Rosengart et al. (2005)	Total	0.94 [0.78, 1.14]
Webster, Crifasi, & Vernick (2014)	Total, age-adjusted	1.06 [1.02, 1.11]
Siegel et al. (2019)	Firearm	1.00 [0.82, 1.22]
Rosengart et al. (2005)	Firearm	0.94 [0.73, 1.19]
Webster, Crifasi, & Vernick (2014)	Firearm, age-adjusted	1.07 [1.02, 1.12]



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

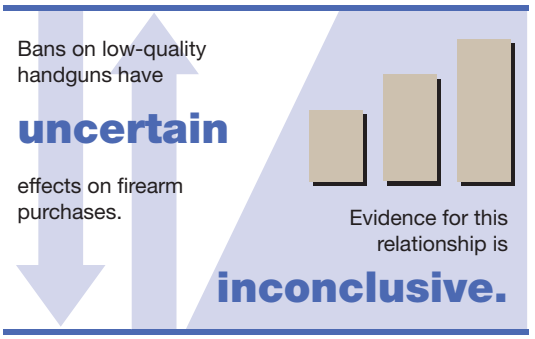


law had an uncertain effect on firearm homicide (Rosengart et al., 2005). Webster, Crifasi, and Vernick (2014) reported the opposite finding for a more recent ban, which appeared to significantly increase rates of firearm homicide and those of nonfirearm homicide. Finally, Siegel et al. (2019) found only uncertain effects of bans on low-quality handguns on firearm homicides. Therefore, we find that the existing research provides *inconclusive evidence for how bans on low-quality handguns affect firearm 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 handgun 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 significant. However, because 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, using our criteria, prompts critical methodological concern related to model overfit (as explained in Chapter 2).



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 because of critical methodological concerns.

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
- police shootings
- defensive gun use
- hunting and recreation.

Chapter 15 References

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Reports (2000): Memphis, Tennessee*, U.S. Department of the Treasury, July 2002.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, Vol. 90, No. 3, 2008.

Cohn, Scott, "Remington Rifle Settlement, Including Free Trigger Replacement, Is Official," CNBC, October 24, 2018.

Cook, Philip J., "The Saturday Night Special: An Assessment of Alternative Definitions from a Policy Perspective," *Journal of Criminal Law and Criminology*, Vol. 72, No. 4, 1981.

Funk, T. Markus, "Gun Control and Economic Discrimination: The Melting-Point Case-in-Point," *Journal of Criminal Law and Criminology*, Vol. 85, No. 3, 1995.

Grassel, K. M., G. J. Wintemute, M. A. Wright, and M. P. Romero, "Association Between Handgun Purchase and Mortality from Firearm Injury," *Injury Prevention*, Vol. 9, No. 1, 2003.

Hanlon, Thomas J., Catherine Barber, Deborah Azrael, and Matthew Miller, "Type of Firearm Used in Suicides: Findings from 13 States in the National Violent Death Reporting System, 2005–2015," *Journal of Adolescent Health*, Vol. 65, No. 3, 2019.

Healey, Jon, "California Opens the Door to Suing Gun Makers. Here's What the New Law Does," *Los Angeles Times*, July 13, 2022.

Hureau, David M., and Anthony A. Braga, "The Trade in Tools: The Market for Illicit Guns in High-Risk Networks," *Criminology*, Vol. 56, No. 3, 2018.

Koper, Christopher S., *Crime Gun Risk Factors: Buyer, Seller, Firearm, and Transaction Characteristics Associated with Gun Trafficking and Criminal Gun Use*, National Institute of Justice, 2003.

Koper, Christopher S., "Crime Gun Risk Factors: Buyer, Seller, Firearm, and Transaction Characteristics Associated with Gun Trafficking and Criminal Gun Use," *Journal of Quantitative Criminology*, Vol. 30, No. 2, 2014.

Kurtzleben, Danielle, "Fact Check: Are Gun-Makers 'Totally Free of Liability for Their Behavior'?" NPR, October 6, 2015.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Milne, John S., Stephen W. Hargarten, Arthur L. Kellermann, and Garen J. Wintemute, "Effect of Current Federal Regulations on Handgun Safety Features," *Annals of Emergency Medicine*, Vol. 41, No. 1, 2003.

Planty, Michael, and Jennifer L. Truman, *Firearm Violence, 1993–2011*, U.S. Department of Justice, Bureau of Justice Statistics, NCJ 2411730, May 2013.

Public Law 90-618, Gun Control Act of 1968, October 22, 1968.

Rosengart, M., P. Cummings, A. Nathens, P. Heagerty, R. Maier, and F. Rivara, "An Evaluation of State Firearm Regulations and Homicide and Suicide Death Rates," *Injury Prevention*, Vol. 11, No. 2, 2005.

Sarani, Babak, Cheralyn Hendrix, Mary Matecki, Jordan Estroff, Richard L. Amdur, Bryce R. H. Robinson, Geoff Shapiro, Stephen Gondek, Roger Mitchell, and E. Reed Smith, "Wounding Patterns Based on Firearm Type in Civilian Public Mass Shootings in the United States," *Journal of the American College of Surgeons*, Vol. 228, No. 3, 2019.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Smart, Rosanna, Terry L. Schell, Matthew Cefalu, and Andrew R. Morral, "Impact on Nonfirearm Deaths of Firearm Laws Affecting Firearm Deaths: A Systematic Review and Meta-Analysis," *American Journal of Public Health*, Vol. 110, No. 10, October 2020a.

Vernick, Jon S., Daniel W. Webster, and Lisa M. Hepburn, "Effects of Maryland's Law Banning Saturday Night Special Handguns on Crime Guns," *Injury Prevention*, Vol. 5, No. 4, 1999.

Webster, D., C. K. Crifasi, and J. S. Vernick, "Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides," *Journal of Urban Health*, Vol. 91, No. 2, 2014.

Webster, Daniel W., Jon S. Vernick, and Lisa M. Hepburn, "Effects of Maryland's Law Banning 'Saturday Night Special' Handguns on Homicides," *American Journal of Epidemiology*, Vol. 155, No. 5, 2002.

Webster, Daniel W., Jon S. Vernick, Emma E. McGinty, and Ted Alcorn, "Preventing the Diversion of Guns to Criminals Through Effective Firearm Sales Laws," in Daniel W. Webster and Jon S. Vernick, eds., *Reducing Gun Violence in America: Informing Policy with Evidence and Analysis*, Johns Hopkins University Press, 2013.

Wintemute, Garen J., *Ring of Fire: The Handgun Makers of Southern California*, Violence Prevention Research Program, 1994.

Wintemute, Garen J., Carrie A. Parham, Mona A. Wright, James J. Beaumont, and Christiana M. Drake, "Weapons of Choice: Previous Criminal History, Later Criminal Activity, and Firearm Preference Among Legally Authorized Young Adult Purchasers of Handguns," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 44, No. 1, 1998.

Wright, James D., and Peter H. Rossi, *Armed and Considered Dangerous: A Survey of Felons and Their Firearms*, Aldine de Gruyter, 1986.

Wright, M. A., G. J. Wintemute, and B. E. Claire, "People and Guns Involved in Denied and Completed Handgun Sales," *Injury Prevention*, Vol. 11, No. 4, 2005.

Zawitz, Marianne W., *Guns Used in Crime*, U.S. Department of Justice, Bureau of Justice Statistics, NCJ-148201, 1995.

Zimring, Franklin E., "Firearms and Federal Law: The Gun Control Act of 1968," *Journal of Legal Studies*, Vol. 4, No. 1, 1975.

PART D

Evidence for the Effects of Policies Regulating the Legal Use, Storage, or Carrying of Firearms

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 16)
- child-access prevention laws (Chapter 17)
- concealed-carry laws (Chapter 18)
- gun-free zones (Chapter 19)
- laws allowing armed staff in kindergarten through grade 12 schools (Chapter 20).

CHAPTER 16

Stand-Your-Ground Laws

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 because 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.

Most states with stand-your-ground laws have implemented these statutes in the past 20 years, although many of these states already had something like stand-your-ground laws in place through case law. Indeed, the Giffords Law Center to Prevent Gun Violence, an advocacy organization, reports that eight states without stand-your-ground statutes nevertheless have existing case law that effectively establishes a stand-your-ground policy; this includes, for example, California, which is often viewed as a state with among the most-restrictive laws (Giffords Law Center to Prevent Gun Violence, undated-f). Because almost all research we have reviewed considers only state statutes, not case law, existing studies may inadvertently include states with practically similar stand-your-ground policies in both their treated groups (states with the statute) and their comparison groups (states with stand-your-ground through

case law), potentially reducing the magnitude of the estimated effects of these laws. It is possible, however, that stand-your-ground statutes have different effects on public behavior than case law, in which case the focus on statutes might be justified. For instance, passage of statutes has, at times, been widely discussed in the media, possibly making more people aware that they need not retreat from a violent confrontation (Siegel, 2022). In addition, although most case law can be used only as a criminal defense, many state statutes allow defendants to avoid civil liability as well. Similarly, some state statutes require police to have probable cause that force was used illegally before they can arrest a shooter, which differs from case law (Giffords Law Center to Prevent Gun Violence, undated-f).

To disentangle the possible effects of these laws or different versions of them (statutory versus case law, for instance), 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 (including murders specifically), and property crime are readily available. Methodological weaknesses in collecting data on defensive gun use are well documented, but several data sources do exist (Hemenway, Shawah, and Lites, 2022; 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 already relieves victims of a duty to retreat)[1] 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's (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. Four of the studies we identified that met our criteria evaluated the effects of stand-your-ground laws on suicide outcomes: Two of these (Humphreys, Gasparrini, and

[1] Standard *castle doctrine* is that a person in their own home does not have a duty to retreat prior to using force, including deadly force, in self-defense.

Wiebe, 2017; Guettabi and Munasib, 2018) estimated the impact of such laws on suicide rates explicitly as placebo tests (i.e., on the theory that stand-your-ground laws should have no effect on suicides).

Although not included among the eight outcomes covered in this review, legal processes in cases involving self-defense are often discussed as important considerations related to the adoption and implementation of stand-your-ground laws. While stand-your-ground laws ostensibly broaden protections from criminal (and, in some cases, civil) liability to all law-abiding citizens acting to defend themselves, some have argued that the application of these laws serves to perpetuate or even exacerbate racial disparities in the criminal justice system because of differences in how Black and White defendants are treated in cases claiming a homicide was justified (Roman, 2012). Others have investigated interactive effects of race and gender on the implementation and enforcement of stand-your-ground laws (Murphy, 2018) or have called for such research (Light, Thomas, and Yakubovich, 2023). For a systematic review of studies analyzing self-defense claims under Florida's stand-your-ground law, see Yakubovich et al. (2021).

State Implementation of Stand-Your-Ground Laws

As of January 1, 2024, 35 states had stand-your-ground statutes or had statutes expanding 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.[2] In the ensuing decades, an additional 28 states passed similar laws.[3] Five states have expanded castle doctrine to motor vehicles or the workplace,[4] and we include these in our discussion here. Other sources (e.g., Everytown for

[2] Fla. Stat. Ann. § 776.013(3); Utah Code Ann. § 76-2-402(3).

[3] Alabama, Alaska, Arkansas, Arizona, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Missouri, Montana, Nevada, New Hampshire, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, West Virginia, and Wyoming. See Ala. Code § 13A-3-23(b); Alaska Stat. Ann. § 11.81.335; Ark. Code § 5-2-607; Ariz. Rev. Stat. Ann. § 13-411; Ga. Code Ann. § 16-3-23.1; Ida. Code § 19-202A(3); Ind. Code Ann. § 35-41-3-2(c)(2); Ia. Code Ann. § 704.1; Kan. Stat. Ann. § 21-5222; Ky. Rev. Stat. Ann. §§ 503.050(4), 503.055(3); La. Stat. Ann. § 14:20(C); Mich. Comp. Laws § 780.972(2); Miss. Ann. Code § 97-3-15(4); Mo. Stat. Ann. § 563.031; Mont. Code Ann. § 45-3-110; Nev. Rev. Stat. Ann. § 200.120(2); N.H. Rev. Stat. Ann. § 627:4(III); N.D. Cent. Code § 12.1-05-07(2)(b)(2); N.C. Gen. Stat. Ann. § 14-51.3; Ohio Rev. Code § 2901.05; Okla. Stat. Ann. Tit. 21 § 1289.25(D); 18 Pa. Cons. Stat. Ann. § 505(b)(2.3); S.C. Ann. Code § 16-11-440(C); S.D. Laws § 22-18-4; Tenn. Ann. Code § 39-11-611(b); Tex. Penal Code § 9.32(C); W. Va. Code § 55-7-22; Wyo. Stat. § 6-2-602(e).

[4] Connecticut, Delaware, Hawaii, Nebraska, and Wisconsin. See Conn. Gen. Stat. Ann. § 53a-20; 11 Del. Code §§ 4-464; 465l; Hawaii Rev. Stat. Ann. § 703-304(5)(b)(i); Neb. Rev. St. § 28-1409(4)(b); Wisc. Stat. § 939.48.

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.[5]

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)."[6] 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."[7]

States that followed Florida generally modeled their laws on those of Florida and Utah,[8] 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*.[9] Other states do not include the language that there 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

5 Utah Code Ann. § 76-2-402.

6 Fla. Stat. Ann. § 776.013.

7 Fla. Stat. Ann. §§ 776.012, 776.013.

8 In particular, Georgia, Indiana, Montana, and Oklahoma. See Ga. Code Ann. §§ 16-3-23.1, 16-3-21; Ind. Code Ann. § 35-41-3-2; Mont. Code Ann. §§ 45-3-102, 103, 104, 110; Okla. Stat. Ann. Tit. 21 § 1289.25.

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.

felonies or describing a class of felonies.[10] In some states, the list of felonies is quite limited.[11] Finally, four states limit their laws to defense of self and others if facing risk of death or serious physical injury, thereby implicitly excluding any other felonies.[12]

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.[13] 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."[14] Ohio's statute applies only in a person's home or vehicle or in a vehicle owned

[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 an 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 a 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).

[12] Iowa, Kansas, Tennessee, and Wisconsin. See Ia. Code Ann. § 704.1; Kan. Stat. Ann. §§ 21-5222, 5223, 5230; Tenn. Ann. Code § 39-11-611; Wisc. Stat. Ann. § 939.48.

[13] W. Va. Ann. Code § 55-7-22.

[14] N.D. Ann. Code § 12.1-05-07.

by an immediate family member.[15] In Wisconsin, the law applies in an individual's home, motor vehicle, or place of business.[16] In Connecticut, it applies in the home or workplace.[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."[18] Other policies are broader, excluding any situation where the individual is "actively engaged in conduct in furtherance of criminal activity."[19]

Effects on Suicide

Research Synthesis Findings

We identified five studies that met our criteria for evaluating the impact of stand-your-ground laws on suicide, one of which (Degli Esposti et al., 2022) contributes new findings to this updated review.[20]

Schell et al. (2020) estimated the effects of stand-your-ground laws (in addition to shall-issue and child-access prevention laws) within a Bayesian modeling framework, using an autoregressive negative binomial regression model shown to have good statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018). In addition to adjusting for temporal trends, the authors adjusted for other state-level time-varying characteristics using 17 principal component scores from a principal components analysis of 34 covariates. The authors parameterized policy effects to have a nonlinear phase-in over a six-year period, at which time the policies' effects were estimated to have an uncertain association with both total suicide rates and firearm suicide rates.

Siegel et al. (2019) instead estimated the effects of stand-your-ground laws on suicide rates by specifying a log-linear model with controls for state and year fixed effects, 11 social and economic state-varying covariates, and nine other types of firearm policies. Results showed a suggestive 2-percent decline in the rate of total suicides but uncertain effects on firearm suicides. However, the authors' regression models included controls for a proxy of gun ownership that was a linear combination of the ratio of firearm suicides to suicides and hunting

[15] Ohio Rev. Code Ann. § 2901.09.

[16] Wisc. Stat. Ann. § 939.48.

[17] Conn. Gen. Stat. Ann. § 53a-20.

[18] La. Stat. Ann. § 14:20.

[19] Nev. Rev. Stat. Ann. § 200.120; Tex. Penal Code § 9.32; Wisc. Stat. Ann. § 939.48.

[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.

licenses; including the same measure (firearm suicides or suicides) in both the outcome term and predictor terms can create spurious correlations that bias the study's estimates of policy effects and their standard errors (National Research Council, 2004).

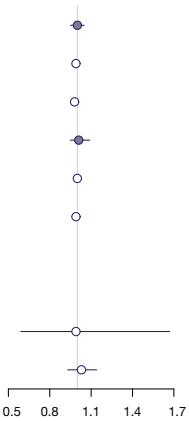
Three additional studies analyzed the relationship of stand-your-ground laws with suicide rates, but they treated the analysis of suicide as a falsification exercise to support the validity of their findings for homicide outcomes (Degli Esposti et al., 2022; Guettabi and Munasib, 2018; Humphreys, Gasparrini, and Wiebe, 2017). Evaluating overlapping time periods but using different methodologies to estimate policy effects, all three of these studies found uncertain relationships between stand-your-ground laws and firearm suicide rates. All three studies were subject to serious methodological concerns related to analyzing few treated states or failing to account for or assess potential serial correlation in suicide rates within states over time.

Figure 16.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the stand-your-ground policies examined in these studies. 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. For Schell et al. (2020), 95-percent CIs are extrapolated from the 80-percent credibility intervals provided in the published report.

FIGURE 16.1

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

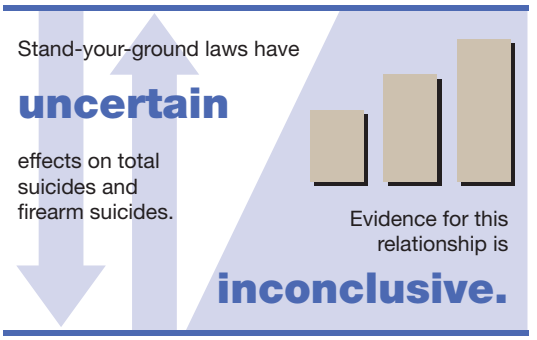
Study, by Policy	Outcome Measure	IRR [95% CI]
Stand-your-ground law	**Suicide rate**	
Schell et al. (2020)	Total	1.00 [0.95, 1.05]
Degli Esposti et al. (2022)	Total	0.99 [0.98, 1.01]
Siegel et al. (2019)	Total	0.98 [0.96, 1.00]
Schell et al. (2020)	Firearm	1.01 [0.95, 1.09]
Degli Esposti et al. (2022)	Firearm	1.00 [0.98, 1.02]
Siegel et al. (2019)	Firearm	0.99 [0.96, 1.02]
Stand-your-ground law, Fla.		
Humphreys, Gasparrini, & Wiebe (2017)	Total	0.99 [0.59, 1.67]
Humphreys, Gasparrini, & Wiebe (2017)	Firearm	1.03 [0.93, 1.14]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. See Appendix B for details.

Conclusions

We identified four qualifying studies that estimated the effects of stand-your-ground laws on total suicides and five that estimated the effects on firearm suicides (Degli Esposti et al., 2022; Guettabi and Munasib, 2018; Humphreys, Gasparrini, and Wiebe, 2017; Schell et al., 2020; Siegel et al., 2019). For both outcomes, all studies found only uncertain effects for stand-your-ground laws, except for Siegel et al. (2019) and Degli Esposti et al. (2022), which found a small suggestive decline in total suicide rates associated with these laws. 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 22 studies that met our criteria and examined the effects of stand-your-ground laws on violent crime, six of which contribute new findings to this updated review (Adams, 2022; D'Alessio et al., 2022; Degli Esposti et al., 2022; Doucette et al., 2021; Neufeld et al., 2022; Scantling et al., 2022).[21] Three studies were rated as having critical methodological concerns and are not discussed further.[22]

[21] The study by Neufeld et al. (2022) focused on firearm injury hospitalization rates, which include a mix of injuries from assault, intentional self-injury, and accident. Because there is evidence suggesting that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section. An additional study (Lott, 2010) examined how violent crime is affected by castle-doctrine laws, which remove the requirement that people 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 laws are 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.

[22] Levy et al. (2020) and Scantling et al. (2022) did not adjust for secular time trends, and thus we have critical concerns that their effect estimates reflect broader national trends rather than causal effects of the law. D'Alessio et al. (2022) evaluated the relationship of stand-your-ground laws with the percentage of reported crime incidents involving a firearm; in addition to concerns with controlling for many plausibly endogenous variables (e.g., the percentage of firearm crime incidents occurring in a residence), without discussing whether estimated effects reflect a change in the numerator or denominator of this metric, their findings cannot speak to whether stand-your-ground laws have beneficial or harmful effects vis-á-vis violent crime.

Degli Esposti et al. (2022) estimated the relationship between homicide rates and stand-your-ground laws implemented by legislative statute, excluding from the analysis seven states that upheld stand-your-ground principles through case law but not statute. They fit quasi-Poisson regressions within a generalized additive model framework, allowing for flexible, smoothed, state-specific trends and controlling for state fixed effects and seasonal effects. Based on data from 1999 to 2017, their results indicated that stand-your-ground laws significantly increased rates of homicide and firearm-specific homicide by about 8 percent (see Figure 16.2). Findings were similar across a range of sensitivity tests, including the use of alternative modeling frameworks. Subgroup analyses suggested that there were heterogeneous effects across states, and there was some evidence of larger increases in homicide rates following stand-your-ground law implementation in southern states.

These results correspond to the findings of several earlier studies using shorter time frames for analyses. 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 1 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. Effects of these laws on burglary, robbery, and aggravated assault rates were uncertain.

Covering the same period (2000–2010) but using a different source of mortality data and analyzing data at the monthly level, McClellan and Tekin (2017) found that stand-your-ground laws that removed the duty to retreat in all public venues were associated with a suggestive 8-percent increase in firearm homicides (see Figure 16.2). These findings remained robust across a variety of specifications (e.g., using annual data, specifying a linear relationship between homicide rates and the laws), showing a positive relationship between the law and firearm homicide rates but with varying precision of the estimates across models.

Gius (2016) examined a longer time series (1980–2011) to estimate the effects of stand-your-ground laws on murders, robberies, rapes, and aggravated assaults. Using a linear model with a logged outcome that controlled for state, time, and 11 time-varying state effects, the author found that these laws were associated with significant increases in murder rates, which more than doubled, although the effect on firearm-related murders was uncertain. Gius (2016) also found significant increases associated with stand-your-ground laws of approximately 5 percent for robbery, rape, and aggravated assault rates. Miller and Pepper (2020) evaluated a similar period but used a partial identification approach to construct bounded estimates of the effects of stand-your-ground laws on various types of violent crime. They restricted their stand-your-ground states of interest to those that implemented the law in 2006, then generated bounds on the estimated policy effect under joint assumptions that bound the coun-

terfactual violent crime rates that stand-your-ground law states are expected to have experienced in the absence of enacting the law. Results indicated that stand-your-ground laws increased murder rates (IRR = 1.03, point-identified) and the overall violent crime rate (IRR between 1.01 and 1.13), while effects on robbery and assault rates were uncertain. Restricting the analyses to state laws passed in 2006 prompts some concerns that estimates may be confounded by other events in 2006 that led to both law adoption in these particular states and changes in violent crime rates.

Several state-level studies identified only uncertain associations of stand-your-ground laws with homicide rates. 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.[23] 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-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.

Using a short panel of data from 2000 to 2010, Webster, Crifasi, and Vernick (2014) found an uncertain association between stand-your-ground laws and age-adjusted homicide rates, firearm homicide rates, and nonfirearm homicide rates. While these results have serious limitations because stand-your-ground laws were included as an ancillary covariate to better estimate the effects of permit-to-purchase policies, uncertain relationships have been found in other studies as well. Siegel et al. (2019) estimated the effects of stand-your-ground laws on state homicide rates from 1991 to 2016, specifying log-linear models that controlled for year and state fixed effects, state social and economic covariates, and nine other firearm laws. Their estimates revealed only uncertain effects on total and firearm homicides. Knopov et al. (2019) used a similar specification, evaluated the same period, and similarly estimated uncertain associations of the laws with total and firearm homicide rates. Using a longer time series, from 1980 to 2016, Schell et al. (2020) used a Bayesian framework to estimate an autoregressive negative binomial regression model shown to have good statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), and they parameter-

[23] 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).

ized policy effects to have a nonlinear phase-in over a six-year period, at which time policy effects were evaluated. Their results showed uncertain effects of stand-your-ground laws on both total and firearm-specific homicides. However, their estimates indicated an 82-percent probability that stand-your-ground laws increased firearm homicides, and an 80-percent probability that the laws increased total homicides. Also aiming to follow the methodological guidance of Schell, Griffin, and Morral (2018), Adams (2022) assessed the relationship of stand-your-ground policies with violent crime from 1980 to 2018 using a negative binomial regression with year fixed effects and a lagged dependent variable and used change-coding for the policy variable. The author found uncertain results for all outcomes, but the study has serious methodological issues, such as a lack of model clarity, a failure to adjust for population differences across states, and seeming issues with how the lagged dependent variable is operationalized.

Two studies provide state-specific estimates of the effects of stand-your-ground laws on homicide outcomes. 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 with those experienced 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 estimated 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, while the comparison states experienced a statistically insignificant 6-percent increase in total homicides and 8-percent increase in firearm homicides after 2005. However, 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.

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.[24] For each of the 14 states that passed a stand-your-ground law between 2005 and 2007, the authors used a 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

[24] 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).

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 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.

Several other studies have examined the effects of stand-your-ground laws on subsets of firearm homicides defined either geographically (Crifasi et al., 2018b; Doucette et al., 2021; Munasib, Kostandini, and Jordan, 2018; Siegel et al., 2020b) or by focusing on just workplace homicides (Doucette, Crifasi, and Frattaroli, 2019; Sabbath, Hawkins, and Baum, 2020). Crifasi et al. (2018b) examined the effects of these laws in urban counties (large central metro and fringe metro). They estimated Poisson regression models to understand how several state gun policies were 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 = 1.08; 95-percent CI = 1.05, 1.10), whereas the laws had an uncertain relationship with nonfirearm homicide rates. These estimates support the assertion 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. Using an autoregressive negative binomial regression model that better accounts for serial correlation and distinguishing between 197 medium-to-large U.S. cities (populations of at least 100,000 residents) and nonurban areas, Siegel et al. (2020b) found uncertain effects of stand-your-ground laws on total and firearm murders and nonnegligent homicides in both urban and nonurban areas. In a study using data from 1999 to 2016 from 68 large metropolitan counties, Doucette et al. (2021) also found uncertain effects of stand-your-ground laws on firearm homicide rates. However, the purpose of this study was to estimate the effects of ShotSpotter (a proprietary gunfire detection technology); permit-to-purchase laws were included as a control variable, so these findings are subject to serious concerns with accurate causal interpretation.

Two studies that focused on workplace homicides found uncertain associations with stand-your-ground laws. Sabbath, Hawkins, and Baum (2020) examined the effects of stand-your-ground laws on workplace homicides between 2011 and 2017 using a log-link model with gamma-distributed errors that controlled for year fixed effects and six time-varying socioeconomic variables. They found uncertain effects on workplace homicides, but this estimate is based on a single state implementing a stand-your-ground law during the study time frame, meaning that the causal effect of the policy likely was not well identified. Doucette, Crifasi, and Frattaroli (2019) also examined the effect of stand-your-ground laws on workplace firearm homicides over a longer period from 1992 to 2017. Using a negative binomial regression model that controlled for four other firearm laws, 14 social and economic covariates, state and year fixed effects, region fixed effects, and random intercepts for between-state

effects,[25] they found that stand-your-ground laws had uncertain effects on workplace firearm homicides.

Finally, two studies found uncertain associations between stand-your-ground laws and firearm injury hospitalizations. Using data from a subset of states, McClellan and Tekin (2017) assessed how stand-your-ground laws influenced hospitalizations and emergency department visits related to firearm injuries 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 and is not designed to be state-representative (Healthcare Cost and Utilization Project, 2016). Furthermore, given the number of controls included, the ratio of parameters to observations in the injury regressions was approximately one to six, meaning that the model may have been overfit and was likely underpowered to detect any true effect of the policy. That said, an uncertain relationship between stand-your-ground laws and firearm injury hospitalizations was also found in the study by Neufeld et al. (2022). Using an imputed data set of state-year firearm injury hospitalizations from 2000 to 2016, Neufeld et al. (2022) estimated two-way fixed-effects regression models to assess the effect of a wide variety of firearm laws and found uncertain effects of stand-your-ground laws on firearm injury hospitalization rates.

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. Finally, because the uncertainty quantification from the bounded estimates produced by Miller and Pepper (2020) is not comparable to the CIs produced by the other studies, we do not include the results from Miller and Pepper (2020) in Figure 16.2.

Conclusions

Homicides. Excluding one study with critical methodological concerns, we identified 14 qualifying studies that estimated the effects of stand-your-ground laws on total homicides or murders. Cheng and Hoekstra (2013), Degli Esposti et al. (2022), Gius (2016), and Miller and Pepper (2020) all found significant increases in total homicides associated with the implementation of stand-your-ground laws. Humphreys, Gasparrini, and Wiebe (2017) and

[25] As described in the paper, the model may have been overparameterized. It is not clear how state fixed effects, region fixed effects, and random effects could all be identified in a single model. However, in the Stata software package used for these analyses, such overparameterization would not result in uninterpretable policy effect estimates because superfluous state or region effects would be dropped automatically.

FIGURE 16.2

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

Study, by Policy	Outcome Measure	IRR [95% CI]
Stand-your-ground law	**Homicide rate**	
Degli Esposti et al. (2022)	Total	1.08 [1.04, 1.12]
Siegel et al. (2019)	Total	1.01 [0.95, 1.07]
Schell et al. (2020)	Total	1.04 [0.95, 1.13]
Cheng & Hoekstra (2013)	Total	1.09 [1.03, 1.16]
Webster, Crifasi, & Vernick (2014)	Total (age-adjusted)	1.02 [0.96, 1.07]
Knopov et al. (2019)	Total (age-adjusted)	1.03 [0.97, 1.09]
Gius (2016)	Total (murder)	1.04 [1.00, 1.08]
Adams (2022)	Total (murder)	1.01 [0.95, 1.07]
Siegel et al. (2020b)	Total (murder), mid to large cities	1.00 [0.93, 1.07]
Siegel et al. (2020b)	Total (murder), nonurban areas	1.01 [0.95, 1.07]
Degli Esposti et al. (2022)	Firearm	1.08 [1.03, 1.13]
Siegel et al. (2019)	Firearm	1.05 [0.96, 1.15]
Schell et al. (2020)	Firearm	1.05 [0.94, 1.17]
McClellan & Tekin (2017)	Firearm	1.08 [1.01, 1.15]
Munasib, Kostandini, & Jordan (2018)	Firearm	1.00 [0.96, 1.04]
Webster, Crifasi, & Vernick (2014)	Firearm (age-adjusted)	1.04 [0.96, 1.12]
Gius (2016)	Firearm (murder)	1.03 [0.97, 1.10]
Crifasi et al. (2018b)	Firearm, urban areas only	1.08 [1.05, 1.10]
Doucette et al. (2021)	Firearm, large metro counties	1.06 [0.95, 1.19]
Siegel et al. (2020b)	Firearm (murder), mid to large cities	1.02 [0.92, 1.12]
Siegel et al. (2020b)	Firearm (murder), nonurban areas	1.02 [0.94, 1.10]
Adams (2022)	Robbery	1.00 [0.96, 1.04]
Adams (2022)	Aggravated assault	1.00 [0.96, 1.04]
Adams (2022)	Rape	1.01 [0.98, 1.05]
Stand-your-ground law	**Workplace homicide rate**	
Sabbath, Hawkins, & Baum (2020)	Total	1.02 [0.88, 1.18]
Doucette, Crifasi, & Frattaroli (2019)	Firearm	0.91 [0.76, 1.07]
Stand-your-ground law	**Violent crime rate**	
Cheng & Hoekstra (2013)	Robbery	1.03 [0.98, 1.07]
Gius (2016)	Robbery	1.05 [1.01, 1.10]
Cheng & Hoekstra (2013)	Aggravated assault	1.04 [0.97, 1.10]
Gius (2016)	Aggravated assault	1.05 [1.01, 1.10]
Gius (2016)	Rape	1.05 [1.02, 1.09]

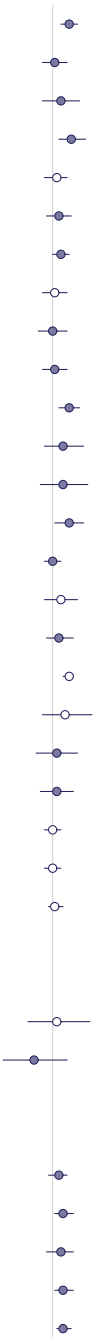
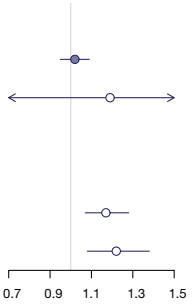


Figure continued on the next page

Figure 16.2—Continued

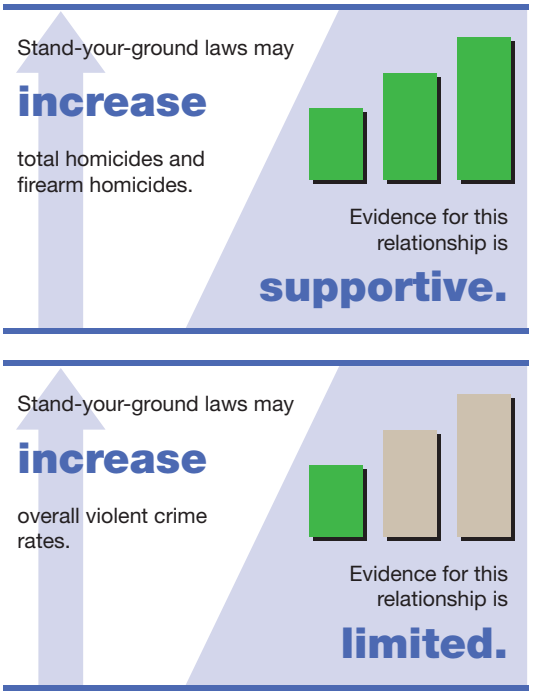
Study, by Policy	Outcome Measure	IRR [95% CI]
Stand-your-ground law	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm	1.02 [0.95, 1.09]
McClellan & Tekin (2017)	Firearm assault	1.19 [0.55, 2.58]
Stand-your-ground law, Fla.	**Homicide rate**	
Humphreys, Gasparrini, & Wiebe (2017)	Total	1.17 [1.07, 1.28]
Humphreys, Gasparrini, & Wiebe (2017)	Firearm	1.22 [1.08, 1.38]



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 unfilled circles indicate that serious methodological concerns are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Results with critical methodological concerns are not presented. See Appendix B and text for details.

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. Additionally, Degli Esposti et al. (2022) document significant increases in homicide rates for independent population subgroups. Six other studies found uncertain effects on homicides (Adams, 2022; Siegel et al., 2020b; Siegel et al., 2019; Knopov et al., 2019; Schell et al., 2020; Webster, Crifasi, and Vernick, 2014), as did one that focused on workplace homicides (Sabbath, Hawkins, and Baum, 2020). However, given the relative methodological quality of the studies, and the findings of significant effects using different data sources and population groups, we conclude that there is *supportive evidence that stand-your-ground laws may increase total homicide rates.*

Firearm homicides. Excluding one study with critical methodological concerns, we identified 14 qualifying studies that estimated the effects of stand-your-ground laws on firearm homicide rates. Eight studies found that these laws have uncertain effects on firearm homicides or firearm murders (Doucette et al., 2021; Gius, 2016; Munasib, Kostandini, and Jordan, 2018; Webster, Crifasi, and Vernick, 2014; Siegel et al., 2019; Siegel et al., 2020b; Knopov et al., 2019; Schell et al., 2020), and one found uncertain effects on workplace firearm homicides (Doucette, Crifasi, and Frattaroli, 2019). 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. Three other studies found that stand-your-ground laws were associated with significant increases in firearm homicide rates (Crifasi et al., 2018b; Degli Esposti et al., 2022; McClellan and Tekin, 2017). Significant increases were shown across two data sources (Supplemental Homicide Reports and National Vital Statistics System; McClellan and Tekin, 2017) and across independent population groups (male and female victims; Degli Esposti et al., 2022). Considering these findings, we conclude that there is *supportive evidence that stand-your-ground laws may increase firearm homicides.*



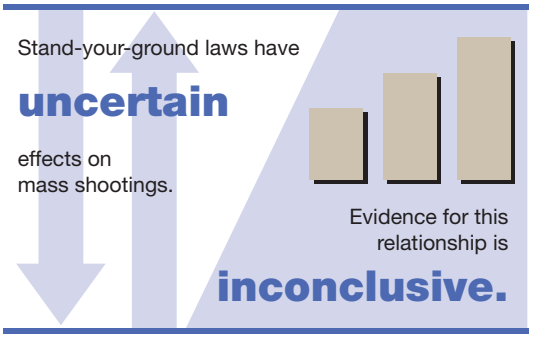
Violent crimes other than homicide. Cheng and Hoekstra (2013) and Adams (2022) found that stand-your-ground laws had uncertain effects on robbery, aggravated assault, and rape rates. Gius (2016), in contrast, reported significant associations between adoption of these laws and increases in robbery and aggravated assault. The bounded estimates from Miller and Pepper (2020) indicate that stand-your-ground laws increase violent crime. Neufeld et al. (2022) found uncertain associations of stand-your-ground laws with firearm injury hospitalizations. Finally, McClellan and Tekin (2017) estimated uncertain associations of stand-your-ground laws with purposefully inflicted firearm-related injury hospitalizations and emergency department visits, although estimates for some subgroups are significant and positive. Overall, we conclude that there is *limited evidence that stand-your-ground laws may increase overall violent crime rates.*

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 effect estimates were based on sparse data, with fewer than three mass shooting incidents per estimated parameter. Predicting a dichotomous (and rare) outcome with such a complex regression specification likely violated model assumptions and rendered the results unreliable. Indeed, the estimated model implies negative IRRs, which represent implausible effect sizes, for some of the gun policies that we are

studying.[26] This raises critical concerns about model specification (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 effects of stand-your-ground laws on mass shootings*.

Effects on Defensive Gun Use

Research Synthesis Findings

We identified three studies that met our inclusion criteria and examined the effects of stand-your-ground laws on defensive gun use. Two of these are subject to critical methodological concerns and are not discussed further.[27]

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 1 for policies 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

[26] 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.

[27] McClellan and Tekin (2017) had critical methodological concerns related to model overfit (i.e., a ratio of below one to five for estimated parameters to observations). Levy et al. (2020) had critical concerns related to drawing causal conclusions from models that did not adjust for potential state-level or year-level confounders.

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 possibility that changes to these law enforcement data reflect changes in the recording of homicide incidents as "justifiable" versus actual changes in incidence, these estimates should be interpreted cautiously because they may reflect changes in reporting rather than the causal effects of stand-your-ground laws on the incidence of justifiable homicides.

Figure 16.3 displays the IRR and CIs associated with the stand-your-ground policies examined in Cheng and Hoekstra (2013).

Conclusions

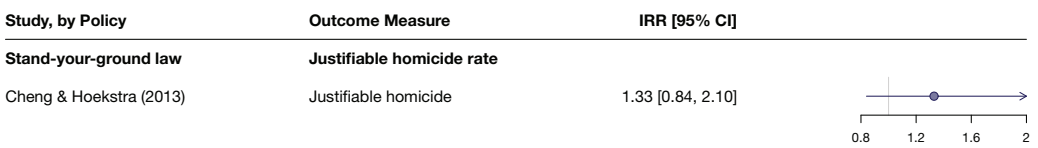
Excluding two studies with critical methodological weaknesses, we identified one study that estimated the effects of stand-your-ground laws on justifiable homicides, which is an imperfect measure of the rate of defensive gun use. In their specification that accounts for how justifiable homicides are counted and controls for time-varying state characteristics, Cheng and Hoekstra (2013) found that the effect of the law on this outcome is uncertain. 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

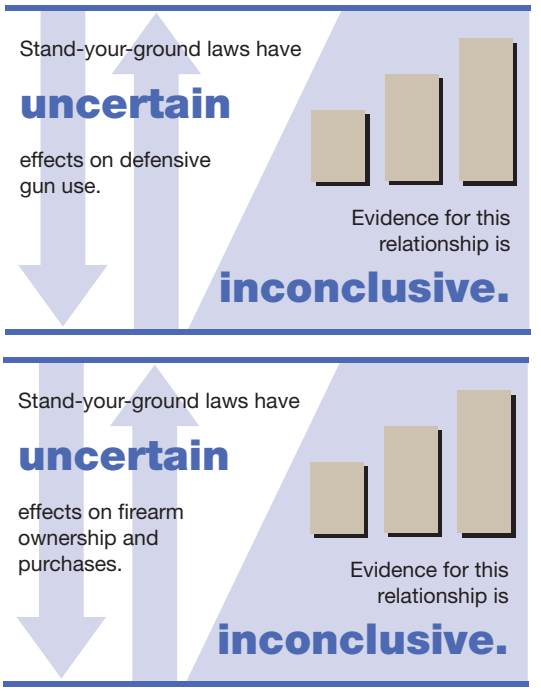
One study (Wallace, 2014) met our inclusion criteria and assessed the effects of stand-your-ground laws on two proxy measures for firearm demand: the number of National Instant Criminal Background Check System (NICS) background checks conducted and the proportion of suicide deaths that involved a firearm, 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 back-

FIGURE 16.3
Incidence Rate Ratio Associated with the Effect of Stand-Your-Ground Laws on Defensive Gun Use



NOTE: IRR values marked with green squares indicate that we identified no serious methodological concerns. Estimates from studies with critical methodological concerns are not presented. See Appendix B and text for details.

ground 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 raise critical methodological concerns 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
- police shootings
- hunting and recreation.

Chapter 16 References

Adams, K. Alexander, "No Retreat: The Impact of Stand Your Ground Laws on Violent Crime," *Criminal Justice Review*, September 2022, online ahead of print.

Aldrich, John H., and Forrest D. Nelson, *Linear Probability, Logit, and Probit Models*, Sage Publications, 1984.

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Centers for Disease Control and Prevention, "Underlying Cause of Death, 1999–2017," WONDER data system, undated-a. As of July 6, 2019:
http://wonder.cdc.gov/ucd-icd10.html

Cheng, Cheng, and Mark Hoekstra, "Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine," *Journal of Human Resources*, Vol. 48, No. 3, 2013.

Cox, D. R., and E. J. Snell, *Analysis of Binary Data*, 2nd ed., Chapman and Hall, 1989.

Crifasi, Cassandra K., Molly Merrill-Francis, Alex McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster, "Association Between Firearm Laws and Homicide in Urban Counties," *Journal of Urban Health*, Vol. 95, No. 3, 2018b.

D'Alessio, Stewart J., Lisa Stolzenberg, Rob T. Guerette, and Kristen M. Zgoba, "The Effect of Self-Defense Laws on Firearm Use Among Criminal Offenders," *Crime and Delinquency*, Vol. 69, Nos. 13–14, December 2022.

Degli Esposti, Michelle, Douglas J. Wiebe, Antonio Gasparrini, and David K. Humphreys, "Analysis of 'Stand Your Ground' Self-Defense Laws and Statewide Rates of Homicides and Firearm Homicides," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Doucette, Mitchell L., Cassandra K. Crifasi, and Shannon Frattaroli, "Right-to-Carry Laws and Firearm Workplace Homicides: A Longitudinal Analysis (1992–2017)," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Doucette, Mitchell L., Christa Green, Jennifer Necci Dineen, David Shapiro, and Kerri M. Raissian, "Impact of ShotSpotter Technology on Firearm Homicides and Arrests Among Large Metropolitan Counties: A Longitudinal Analysis, 1999–2016," *Journal of Urban Health*, Vol. 98, No. 5, October 2021.

Everytown for Gun Safety Support Fund, *Shoot First: "Stand Your Ground" Laws and Their Effect on Violent Crime and the Criminal Justice System*, 2013.

Giffords Law Center to Prevent Gun Violence, "'Stand Your Ground' Laws," webpage, undated-f. As of October 18, 2017:
http://lawcenter.giffords.org/gun-laws/policy-areas/guns-in-public/stand-your-ground-laws/

Gius, Mark, "The Relationship Between Stand-Your-Ground Laws and Crime: A State-Level Analysis," *Social Science Journal*, Vol. 53, No. 3, 2016.

Guettabi, Mouhcine, and Abdul Munasib, "Stand Your Ground Laws, Homicides and Gun Deaths," *Regional Studies*, Vol. 52, No. 9, 2018.

Healthcare Cost and Utilization Project, "Why the NIS Should Not Be Used to Make State-Level Estimates," webpage, January 5, 2016. As of August 2, 2023:
https://hcup-us.ahrq.gov/db/nation/nis/nis_statelevelestimates.jsp

Hemenway, David, Chloe Shawah, and Elizabeth Lites, "Defensive Gun Use: What Can We Learn from News Reports?" *Injury Epidemiology*, Vol. 9, No. 1, 2022.

Hemenway, David, and Sara J. Solnick, "The Epidemiology of Self-Defense Gun Use: Evidence from the National Crime Victimization Surveys 2007–2011," *Preventive Medicine*, Vol. 79, 2015b.

Humphreys, David K., Antonio Gasparrini, and Douglas J. Wiebe, "Evaluating the Impact of Florida's 'Stand Your Ground' Self-Defense Law on Homicide and Suicide by Firearm: An Interrupted Time Series Study," *JAMA Internal Medicine*, Vol. 177, No. 1, 2017.

Kleck, Gary, "Response Errors in Surveys of Defensive Gun Use: A National Internet Survey Experiment," *Crime and Delinquency*, Vol. 64, No. 9, 2018.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Levy, Marc, Wilmer Alvarez, Lauren Vagelakos, Michelle Yore, and Bertha Ben Khallouq, "Stand Your Ground: Policy and Trends in Firearm-Related Justifiable Homicide and Homicide in the US," *Journal of the American College of Surgeons*, Vol. 230, No. 1, January 2020.

Light, Caroline, Janae Thomas, and Alexa Yakubovich, "Gender and Stand Your Ground Laws: A Critical Appraisal of Existing Research," *Journal of Law, Medicine, and Ethics*, Vol. 51, No. 1, Spring 2023.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

McClellan, Chandler, and Erdal Tekin, "Stand Your Ground Laws, Homicides, and Injuries," *Journal of Human Resources*, Vol. 52, No. 3, 2017.

Miller, Megan, and John Pepper, "Assessing the Effect of Firearms Regulations Using Partial Identification Methods: A Case Study of the Impact of Stand Your Ground Laws on Violent Crime," *Law and Contemporary Problems*, Vol. 83, 2020.

Munasib, Abdul, Genti Kostandini, and Jeffrey L. Jordan, "Impact of the Stand Your Ground Law on Gun Deaths: Evidence of a Rural Urban Dichotomy," *European Journal of Law and Economics*, Vol. 45, No. 3, 2018.

Murphy, Justin, "Are 'Stand Your Ground' Laws Racist and Sexist? A Statistical Analysis of Cases in Florida, 2005-2013*," *Social Science Quarterly*, Vol. 99, No. 1, March 2018.

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of March 2, 2018: https://www.rand.org/pubs/research_reports/RR2088.html

Roman, John, "Do Stand Your Ground Laws Worsen Racial Disparities?" *Urban Wire*, Urban Institute blog, August 8, 2012. As of September 15, 2022: https://www.urban.org/urban-wire/do-stand-your-ground-laws-worsen-racial-disparities

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Scantling, Dane R., Daniel N. Holena, Elinore J. Kaufman, Allyson M. Hynes, Justin Hatchimonji, James P. Byrne, Douglas Wiebe, and Mark J. Seamon, "Modifiable Factors Related to Firearm Homicides: A Broader View of Our Lane," *Annals of Surgery*, 2022, online ahead of print.

Schell, Terry L., Matthew Cefalu, Beth Ann Griffin, Rosanna Smart, and Andrew R. Morral, "Changes in Firearm Mortality Following the Implementation of State Laws Regulating Firearm Access and Use," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 26, 2020.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019: https://www.rand.org/pubs/research_reports/RR2685.html

Siegel, Michael, "New Evidence for State-Specific Heterogeneity in the Association of Stand Your Ground Laws with Firearm Violence," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Siegel, Michael, Benjamin Solomon, Anita Knopov, Emily F. Rothman, Shea W. Cronin, Ziming Xuan, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates in Suburban and Rural Areas Compared to Large Cities in the United States, 1991–2016," *Journal of Rural Health*, Vol. 36, No. 2, March 2020b.

Wallace, Lacey N., "Castle Doctrine Legislation: Unintended Effects for Gun Ownership?" *Justice Policy Journal*, Vol. 11, No. 2, Fall 2014.

Webster, D., C. K. Crifasi, and J. S. Vernick, "Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides," *Journal of Urban Health*, Vol. 91, No. 2, 2014.

Yakubovich, Alexa R., Michelle Degli Esposti, Brittany C. L. Lange, G. J. Melendez-Torres, Alpa Parmar, Douglas J. Wiebe, and David K. Humphreys, "Effects of Laws Expanding Civilian Rights to Use Deadly Force in Self-Defense on Violence and Crime: A Systematic Review," *American Journal of Public Health*, Vol. 111, No. 4, 2021.

CHAPTER 17

Child-Access Prevention Laws

A recent national survey estimated that 30 million children in the United States live in households with firearms (Miller and Azrael, 2022). Although multiple studies have found that living in a household with a child under the age of 18 is associated with higher rates of secure firearm storage (Berrigan et al., 2019; Crifasi et al., 2018a; Salhi, Azrael, and Miller, 2021), a 2021 national survey indicated that in 36 percent of households with both firearms and children, guns are stored unlocked; in an estimated 15 percent of these homes, guns are stored loaded and unlocked (Miller and Azrael, 2022; see also Berrigan et al., 2019). Children's exposure and access to firearms varies substantially by state and urbanicity (e.g., see Pallin, Wintemute, and Kravitz-Wirtz, 2021; Jennissen et al., 2021).

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 2020, firearm injuries surpassed motor vehicle crashes as the leading cause of death among children aged 1 to 17 years (Lee, Douglas, and Hemenway, 2022). Of the 2,590 children under age 18 who were killed by firearms in 2021, 827 (31.9 percent) of those deaths were classified as suicides and 140 (5.4 percent) were classified as unintentional (calculated using data from the Centers for Disease Control and Prevention [CDC], undated-g). 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 10,722 firearm-related injuries among children aged 1 to 17 years in 2020.[1]

The extent to which CAP laws can prevent suicide among youth will depend in large part on how youth access firearms for the purpose of self-harm and the extent to which alternative and similarly lethal means are available. In 2021, firearms were used in approximately 47 percent of suicides among children under age 18 (calculated using data from CDC, undated-g). Using the National Fatality Review Case Reporting System, Schnitzer et al. (2019) reported

[1] This estimate was calculated as the sum of treat-and-release emergency department visits (7,464) and emergency department visits resulting in hospitalization (10,722) from HCUPnet: Healthcare Cost and Utilization Project, undated.

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 an elevated risk of suicide based on their prior talk, threats, or attempted suicide.

CAP laws could also influence homicide outcomes because youth and young adults are disproportionately involved in crimes. In 2020, juvenile offenders were known to have been involved in approximately 1,122 murders nationwide, representing approximately 8 percent of known murder offenders; about 85 percent of homicides committed by known juvenile offenders in 2020 involved a firearm (Office of Juvenile Justice and Delinquency Prevention, 2021). 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; more than 80 percent of youth offenders cited a friend, a 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 or burglars. This limited availability could, in turn, influence suicides, unintentional injuries and deaths, and violent crime among the adult population. This may be particularly relevant in multigenerational homes because older adults (aged 65 years or older) have a high risk of suicidal ideation and cognitive impairment (Betz et al., 2023).

Studies of adolescent and adult suicides have generally found that, relative to comparison groups of individuals who died in 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 and 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). Simonetti et al. (2017) also found no difference in storage practices among gun owners who had adolescents with mental illness in their homes compared with the practices in homes with adolescents who did not have mental illness. These findings, along with the finding that those who die by firearm suicide typically live in homes with less secure stor-

age 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 the 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, because most data sources report only the age of the victim or are missing information on the shooter,[2] 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, and Anderson, Sabia, and Tekin [2021] examined firearm homicide deaths with known perpetrators aged 12–17). 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

[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.

of gun carrying among high school students (Anderson and Sabia, 2018). However, others have found that significant estimates of higher safe-storage rates among firearm owners in CAP states are rendered small and imprecise in models that adjust for individual-level and state-level differences (Miller et al., 2022); this possibly indicates that associations found between CAP laws and safe storage could be attributable to other correlated factors, such as individual-level characteristics (e.g., the number and types of firearms owned by survey respondents) or state-level characteristics (e.g., overall prevalence of household gun ownership). Cross-sectional analyses like this one cannot establish, however, whether storage practices improved after passage of a CAP law. Ideally, matching of gun owners in states with and without the law would occur prior to the implementation of CAP laws to observe how they affected storage behavior.

Some have further questioned whether the implications of CAP laws are sufficiently 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. For instance, if the 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.

Another factor complicating the evaluation of the impact of CAP laws is that parents may believe that their firearms are safely stored when they are not. New data from 2019 indicate that parents who own firearms often underestimate the risk of their child gaining access to the household firearm. Using a sample of male parent-child dyads, Salhi, Azrael, and Miller (2021) found that in homes where parents believed that their child could not access a firearm, 22 percent of their children stated that they could access a firearm within five minutes and 15 percent stated that they could access a firearm within one hour. If changes in storage behavior induced by CAP laws do not actually change youths' ability to access firearms, this would lessen effects of the laws mediated through youth access.

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, as well as the relatively low counts of firearm-related deaths among young children, 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, 2024, 35 states and the District of Columbia have CAP laws.[3] Twenty-seven 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 does or could access the guns. Massachusetts, for instance, imposes criminal liability if a gun is kept or stored "unless such weapon is secured in a locked container or equipped with a tamper-resistant mechanical lock or other safety device, properly engaged so as to render such weapon inoperable by any person other than the owner or other lawfully authorized user."[4] In 2021, Oregon passed a similarly broad safe-storage law.[5] Six other jurisdictions hold owners liable when they know or reasonably should know that access is "likely."[6] States vary in the extent to which the imposition of criminal liability requires that a child actually access the firearm, whether the child has to brandish the firearm in public, or whether the child's access must result in death or serious injury. Similarly, states vary on whether criminal penalties are classed as misdemeanors or felonies.

Many of the states imposing criminal liability for negligent storage allow for exceptions or defenses.[7] The most common is when the gun has been stored in a locked container.[8] Other exceptions or defenses include that the firearm had been rendered inoperable,[9] the person was carrying the firearm or it was close enough to be easily retrieved,[10] or there was a reasonable expectation that children would not be present where the gun was stored.[11] In addition,

[3] California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Indiana, Iowa, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Oklahoma, Oregon, Pennsylvania, Rhode Island, Tennessee, Texas, Utah, Vermont, Virginia, Washington, Wisconsin, and the District of Columbia.

[4] Mass. Gen. Laws Ch. 140, § 131L.

[5] Oreg. Rev. Stat. § 166.395 (2021).

[6] California, Minnesota, Oregon, New York, Washington, and the District of Columbia. See Calif. Penal Code § 25100; Minn. Stat. Ann. § 609.666; Oreg. Rev Stat § 166.395 (2021); N.Y. Penal Law §§ 265.45, 265.50; Wash. RCW 9.41.360; D.C. Code Ann. § 7-2507.02.

[7] In some states, certain actions are not excluded from the definition of the law, while in other states, the actions are affirmative defenses.

[8] Calif. Penal Code § 25105; Conn. Gen. Stat. Ann. § 29-37i; Fla. Stat. Ann. § 790.174; Hawaii Rev. Stat. § 134-10.5; Ia. Code Ann. § 724.22; N.H. Rev. Stat. Ann. § 650-C:1; N.J. Stat. Ann. 2C: 58-15; R.I. Gen. Laws Ann. § 11-47-60.1; Tex. Penal Code Ann. § 46.13; D.C. Code Ann. § 7-2507.02.

[9] Calif. Penal Code § 25105; Fla. Stat. Ann. § 790.174; Ia. Code Ann. § 724.22; N.J. Stat. Ann. 2C: 58-15; N.C. Gen. Stat. Ann. § 14-315.1; R.I. Gen. Laws Ann. § 11-47-60.1; Tex. Penal Code Ann. § 46.13.

[10] Calif. Penal Code § 25105; Conn. Gen. Stat. Ann. § 29-37i; Hawaii Rev. Stat. § 134-10.5; N.H. Rev. Stat. Ann. § 650-C:1; R.I. Gen. Laws Ann. § 11-47-60.1; D.C. Code Ann. § 7-2507.02.

[11] Calif. Penal Code § 25105; N.H. Rev. Stat. Ann. § 650-C:1; R.I. Gen. Laws Ann. § 11-47-60.1.

some states consider it an exception or defense when children enter a storage area illegally[12] or use the firearm for self-defense.[13] Some states have added other defenses too, such as those that apply to children who have a legal right to use firearms for hunting.[14]

Nine states impose criminal liability only for intentional, knowing, or reckless provision of firearms to children. These laws are often considered 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). Four states impose penalties under the weaker standard for all firearms[15] and five for handguns only.[16] Some of the laws require the weapon to be used by the minor in some way.[17] 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[18] or had been rendered inoperable.[19] Other exceptions include that the individual carried the firearm or it was close enough to be easily retrieved,[20] the defendant had no reasonable expectation that a child would have access to the premises where the firearm was located,[21] or the child accessed the

[12] Calif. Penal Code § 25105; Fla. Stat. Ann. § 790.174; Ia. Code Ann. § 724.22; Hawaii Rev. Stat. § 707-714.5; Md. Code Ann., Crim. Law § 4-104; Minn. Stat. Ann. § 609.666; N.H. Rev. Stat. Ann. § 650-C:1; N.J. Stat. Ann. 2C: 58-15; N.C. Gen. Stat. Ann. § 14-315.1; R.I. Gen. Laws Ann. § 11-47-60.1; Tex. Penal Code Ann. § 46.13; D.C. Code Ann. § 7-2507.02.

[13] Calif. Penal Code § 25105; R.I. Gen. Laws Ann. § 11-47-60.1; Tex. Penal Code Ann. § 46.13.

[14] 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).

[15] Missouri, Oklahoma, Pennsylvania, and Utah. See Mo. Stat. Ann. § 571.060; Okla. Stat. Ann. § 1273; 18 Pa. Cons. Stat. Ann. § 6110.1(c); Utah Ann. Code § 76-10-509.6 (applies only to parents and guardians regarding minors who have been convicted or adjudicated of a violent felony).

[16] Georgia, Indiana, Kentucky, Mississippi, and Tennessee. See Ga. Code Ann. § 16-11-101.1; Ind. Code Ann. § 35-47-10-6; Ky. Rev. Stat. Ann. § 527.110; Miss. Ann. Code § 97-37-15 (parents, guardians, custodians); Tenn. Code Ann. § 39-17-1320.

[17] 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).

[18] Del. Code Ann. § 603; Nev. Rev. Stat. Ann. § 202.300; N.Y. Penal Law § 265.45; Wisc. Stat. Ann. § 948.55.

[19] Del. Code Ann. Tit. 11 § 603; N.Y. Penal Law § 265.45; Wisc. Stat. Ann. § 948.55.

[20] Wisc. Stat. Ann. § 948.55.

[21] Wisc. Stat. Ann. § 948.55.

firearm through unlawful entry.[22] Use of the firearm in hunting, hunter safety, and other sporting events[23] or use in self-defense[24] are also exceptions.

In addition to the main distinctions among the CAP laws already discussed, another difference is how such laws define *minors*. In the majority of jurisdictions, it is an offense to provide a firearm to an individual under age 18.[25] In Texas, the age is 17.[26] In eight states, the age is 16,[27] and in another four states, a minor is under age 14.[28]

In ten states and the District of Columbia, the act of negligent storage of a firearm is a misdemeanor.[29] In Massachusetts, negligent storage is a felony. In nine other states, negligent storage becomes a felony or misdemeanor only if some harm results from the negligence, such as a child actually gaining access to the firearm, a child discharging the firearm, or the firearm being used by a child to commit a felony or misdemeanor.[30] In eight states and the District of Columbia, such factors make the negligent storage a felony. In five such jurisdictions, these factors bump the crime from a misdemeanor to a felony,[31] and in the other four, there is no misdemeanor offense, only these felonies.[32] 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 Ten-

[22] Nev. Rev. Stat. Ann. § 202.300; Wisc. Stat. Ann. § 948.55.

[23] Ind. Ann. Code § 35-47-10-1; Miss. Ann. Code § 97-37-14; Nev. Rev. Stat. Ann. § 202.300 (if accompanied or under the charge of a parent or guardian); Okla. Stat. Ann. § 1273 (with permission of parent or guardian); Tenn. Code Ann. § 39-17-1319.

[24] Miss. Ann. Code § 97-37-14; Tenn. Code Ann. § 39-17-1319 (if given permission by a parent or guardian).

[25] California, Colorado, Delaware, Georgia, Indiana, Kentucky, Massachusetts, Minnesota, Mississippi, Missouri, Nevada, North Carolina, Oklahoma, Tennessee, Utah, and the District of Columbia. See Calif. Penal Code § 25000; Colo. Rev. Stat. § 18-12-108.7; Del. Code Ann. Tit. 11 § 603; Ga. Code Ann. § 16-11-101.1; Ind. Code Ann. § 35-47-10-1; Ky. Rev. Stat. Ann. § 527.110; Mass. Gen. Laws Ch. 140, § 131L; Minn. Stat. Ann. § 609.666; Miss. Ann. Code § 97-37-15; Mo. Stat. Ann. § 571.060; Nev. Rev. Stat. Ann. § 202.300; N.C. Gen. Stat. Ann. § 14-315.1; Okla. Stat. Ann. § 1273; Tenn. Code Ann. § 39-17-1320; Utah Code Ann. § 76-10-509.6; D.C. Code Ann. § 7-2507.02.

[26] Tex. Penal Code Ann. § 46.13.

[27] Connecticut, Florida, Hawaii, Maine, Maryland, New Hampshire, New Jersey, and Rhode Island. See Conn. Gen. Stat. Ann. § 29-37i; Fla. Stat. Ann. § 790.174; Hawaii Rev. Stat. § 707-714.5; Me. Stat. 17-A § 554(1)(B-4); Md. Code Ann., Crim. Law § 4-104; N.H. Rev. Stat. Ann. § 650-C:1; N.J. Stat. Ann. § 2C:58-15; R.I. Gen. Laws Ann. § 11-47-60.1.

[28] Illinois, Iowa, Virginia, and Wisconsin. See 720 Ill. Comp. Stat. 5/24-9; Ia. Code Ann. § 724.22; Va. Code Ann. § 18.2-56.2; Wisc. Stat. Ann. § 948.55.

[29] California, Colorado, Delaware, Hawaii, Maryland, Minnesota, New Mexico, Nevada, New Jersey, Virginia, and the District of Columbia.

[30] Florida, Illinois, Iowa, New Hampshire, North Carolina, Oklahoma, Rhode Island, Utah, and Wisconsin.

[31] California, Michigan, Nevada, Utah, and the District of Columbia.

[32] Colorado, Connecticut, Georgia, and Indiana.

nessee 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 11 quasi-experimental studies that examined the impact of CAP laws on suicide, none of which are new in this edition of the report. Two studies were rated as having critical methodological concerns and are not discussed further.[33]

Two early 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 and controlled for states' "shall-issue" or "right-to-carry" laws (see Chapter 18), "one-gun-a-month" purchase rules, waiting periods, mandatory prison penalties for using guns in the commission of a crime, bordering states with one-gun-a-month rules, and more than 36 state-level demographic covariates. 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 10.8-percent reduction in firearm suicides. They also 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

[33] Gius (2020a) was rated as having critical concerns because of violations of statistical assumptions: (1) Estimates are based on extremely noisy data, which are generally not well suited for analyses using a synthetic control method (Abadie, 2021); (2) for many CAP states, the synthetic control consists of only one or two states; and (3) methods of inference are not aligned to the synthetic control method approach. The analysis of school shooting suicides in Anderson and Sabia (2018) was rated as having critical concerns with model overfit.

were sensitive to the authors' choices about model specifications: An alternatively specified and worse-fitting model yielded uncertain effects.[34] The authors also found that CAP laws were associated with reductions in rates of 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 suicide rates among this group of older youth" (p. 600). 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 suicides.

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. 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 reported 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.

Using data from 1980 to 2016, Schell et al. (2020) estimated the effects of three gun laws (shall-issue, stand-your-ground, and CAP) on total and firearm-specific suicide deaths within a Bayesian modeling framework. Using a Bayesian autoregressive negative binomial regression model shown to have good statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), they parameterized policy effects to have a nonlinear phase-in over a six-year period, at which time the policies' effects were evaluated. They estimated that CAP laws had a suggestive effect consistent with reducing total suicides (incidence rate ratio [IRR] = 0.95), but uncertain effects on firearm suicides. Supplementary analyses showed estimates of similar magnitude when restricting to deaths among youths aged 0 to 19 years. Overall, the primary estimates corresponded to a 95-percent probability that CAP laws reduce total suicides and a 90-percent probability that the laws reduce firearm homicides.

Several studies have tried to isolate the effects of specific provisions of CAP laws. DeSimone, Markowitz, and Xu (2013) separately examined two types of CAP law provisions: those barring negligent storage of firearms and those prohibiting reckless provision of guns to children. They examined CAP law effects on nonfatal self-inflicted gun injuries recorded in

[34] 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.

the Nationwide Inpatient Sample (NIS). *Self-inflicted gun injuries* include suicide attempts and 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, but the majority are likely accidental injuries. 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 unintentional injuries. The authors examined hospital discharges in 11 states between 1988 and 2003 and estimated policy effects using Poisson regression with controls for state and year fixed effects, as well as 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, and estimates showed an uncertain effect on self-inflicted injuries for those aged 18 or older. These estimated effects were largely unchanged when specifying CAP laws as those more-stringent policies that impose criminal penalties for negligent storage or, more broadly, to include both negligent storage and reckless provision. This similarity in estimated effects is likely because only two states (Colorado and Wisconsin) in their NIS data 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.

Azad et al. (2020) sought to further distinguish the effects of different types of CAP laws on firearm suicide deaths among those ages 14 and under from 1991 to 2016. Specifically, the authors distinguished among three types of negligent-storage CAP laws: those imposing penalties if (1) a child could access the firearm, (2) a child did access a firearm, and (3) a child accessed and used a firearm. Two final classes of states were those with no negligent-storage CAP law but a reckless provision law and states with neither negligent-storage nor reckless provision laws. In negative binomial models with controls for random effects for states, five state time-varying covariates, three other gun laws, and an unspecified control for time, they found uncertain effects for reckless provision laws but a significant effect for the three classes of negligent-storage laws combined consistent with a 12-percent reduction in firearm suicides. Among the three types of negligent-storage laws, the authors found a significant effect for *child accessed* (IRR = 0.84), a suggestive effect for *child could access* (IRR = 0.74), and uncertain effects for *child used*. Because these analyses did not adjust standard error esti-

mates for serial correlation within states over time, the confidence intervals (CIs) and significance levels that were reported may overstate the precision of each effect. Moreover, both the *child accessed* and *child could access* laws had only one state implementing each law, with a full three years of pre- and post-implementation data available in the study time frame, meaning that these effects may not be reliably estimated.

Kivisto et al. (2021) examined an even finer decomposition of CAP law features, identifying an 11-category typology of CAP laws, but in this analysis too, most estimated effects were identified on fewer than four states with at least three years of pre- and post-implementation data. In their negative binomial models with state fixed effects, a time-by-state interaction to account for state-specific temporal trends, and seven time-varying state covariates, the authors found evidence that firearm suicides declined after the passage of two types of CAP laws: those that mandate that safety locks be provided for handguns sold through firearm dealers and those providing criminal liability for gun owners, regardless of whether the gun is loaded or unloaded. In addition, they found a significant association between the number of negligent-storage and safety lock provisions in a state's laws and reductions in child firearm deaths. Reductions in total suicides were significantly negatively associated with state regulations on firearm safety locks; requirements to store firearms securely whenever they are not in use; criminal liability for owners whose guns are used or carried by an unsupervised child; criminal liability that is not conditional on guns being loaded; and criminal liability if a child under the ages of 14, 16, or 18 accesses a firearm. For total suicides, reductions also were significantly associated with the number of state provisions concerning negligent storage and safety locks and the number of laws imposing criminal liability based on the age of children. Because these analyses did not adjust standard error estimates for serial correlation within states over time, and because many policy effect estimates are drawn from policy changes in a small number of states, the CIs and significance levels reported may overstate the precision of each effect.

Finally, Kappelman and Fording (2021) examined the effects of "weak" or "strict" CAP laws on firearm suicides among those aged 24 or younger based on the presence of four features of the law: (1) reckless provision, (2) negligent storage, (3) law defines a *minor* as younger than 18, and (4) law imposes civil liability on the owner if a minor uses their firearm. States with three or four features were counted as "strict" ($N = 6$), those with just one or two were "weak" ($N = 22$), and 22 states had no CAP laws. The authors used an autoregressive negative binomial model with controls for year and state fixed effects, eight time-varying state covariates, and minimum age firearm laws. Strict CAP laws were associated with suggestive reductions in firearm suicides (but not nonfirearm suicides) among those aged 24 or younger, while the effects of weak CAP laws were uncertain.

Figure 17.1 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 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

Study, by Policy	Outcome Measure	IRR [95% CI]
State CAP law	**Suicide rate**	
Webster et al. (2004)	Total, aged 14–17	0.92 [0.86, 0.98]
Webster et al. (2004)	Total, aged 18–20	0.89 [0.85, 0.93]
Cummings et al. (1997a)	Firearm, aged 0–14	0.81 [0.66, 1.01]
Webster et al. (2004)	Firearm, aged 14–17	0.89 [0.83, 0.96]
Webster et al. (2004)	Firearm, aged 18–20	0.87 [0.82, 0.92]
Gius (2015b)	Firearm, aged 0–19	0.89 [0.84, 0.94]
State CAP law	**Self-inflicted injury rate**	
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 0–17	0.35 [0.19, 0.62]
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 18+	1.17 [0.75, 1.83]
State CAP law, negligent storage	**Suicide rate**	
Schell et al. (2020)	Total	0.96 [0.91, 1.01]
Schell et al. (2020)	Firearm	0.95 [0.89, 1.03]
Azad et al. (2020)	Firearm, aged 0–14	0.88 [0.80, 0.98]
State CAP law, negligent storage	**Self-inflicted injury rate**	
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 0–17	0.31 [0.16, 0.61]
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 18+	1.00 [0.64, 1.56]
State CAP law, reckless provision	**Suicide rate**	
Azad et al. (2020)	Firearm, aged 0-14	1.06 [0.91, 1.24]
State CAP law, could gain access		
Kivisto et al. (2021)	Total, aged 14-18	1.00 [0.80, 1.24]
Kivisto et al. (2021)	Firearm, aged 14–18	1.03 [0.76, 1.41]
State CAP law, gains access		
Kivisto et al. (2021)	Total, aged 14–18	0.98 [0.82, 1.16]
Kivisto et al. (2021)	Firearm, aged 14–18	0.88 [0.69, 1.13]
State CAP law, uses or carries		
Kivisto et al. (2021)	Total, aged 14–18	0.86 [0.75, 0.98]
Kivisto et al. (2021)	Firearm, aged 14–18	0.92 [0.77, 1.10]

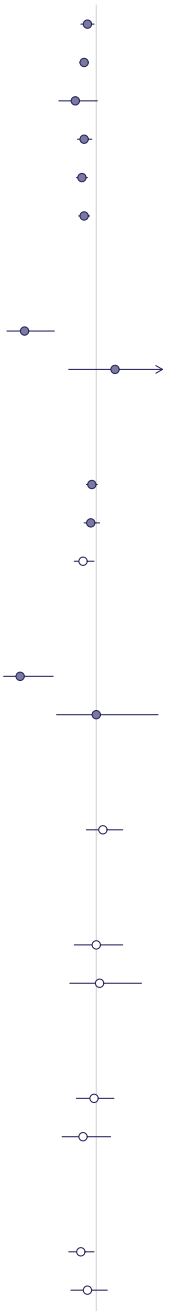
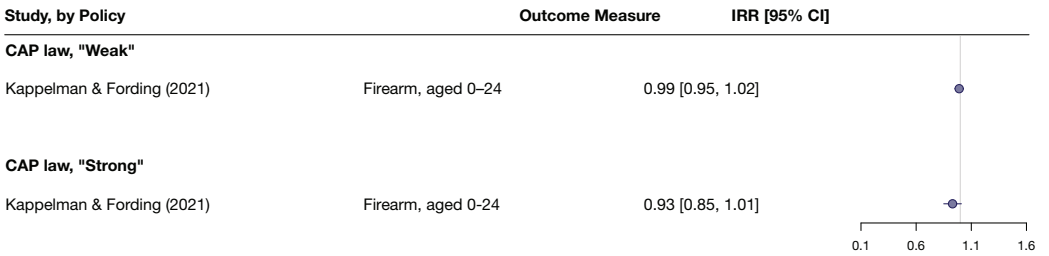


Figure continued on the next page

Figure 17.1—Continued



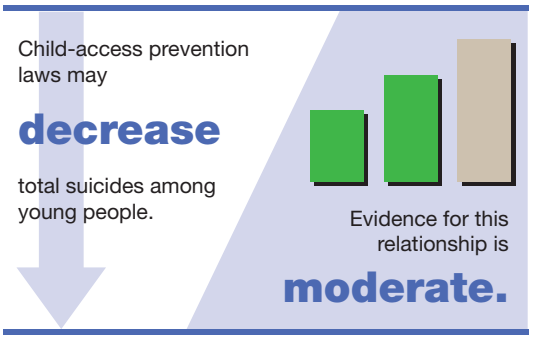
NOTE: The model used in Kappelman and Fording (2021) includes a one-year lag of the dependent variable, so the effect sizes from this study are not directly comparable to others in this figure. IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details.

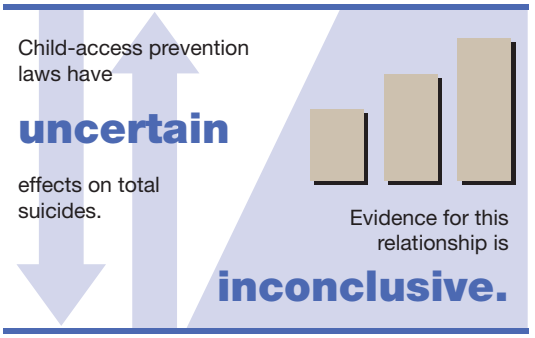
Conclusions

Total suicides. We identified three qualifying studies that estimated the effect of CAP laws on total suicides in the general population (Schell et al., 2020) or populations in specific age ranges (Kivisto et al., 2021; Webster et al., 2004). Schell et al. (2020) estimated a 95-percent probability that CAP laws reduce total suicides in the general population. Kivisto et al. (2021) found that, among those aged 14–18, increasing numbers of provisions related to negligent storage and safety locks were significantly associated with reductions in total suicides. Finally, Webster et al. (2004) found that CAP laws were significantly associated with reductions in suicides among those aged 14–17 and those aged 18–20.

We conclude that available research offers *moderate evidence that child-access prevention laws reduce total suicides among young people and inconclusive evidence that they reduce total suicides in the general population.*

Firearm suicides and firearm self-injury. Excluding two studies with critical methodological concerns, we identified nine qualifying studies that estimated the effect of CAP laws on firearm suicide or firearm self-injury. Among studies investigating the association of CAP laws with firearm suicide deaths among young people, 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)

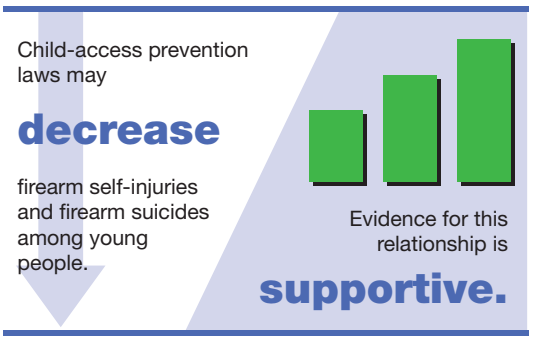


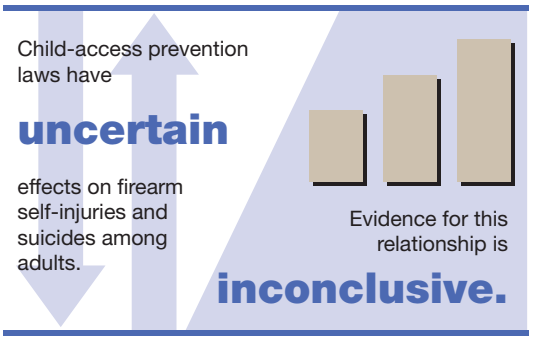


identified uncertain effects of CAP laws on those younger than age 15 and among those aged 15–17. Using a longer but overlapping data series, Webster et al. (2004) found significant effects suggesting that CAP laws reduce firearm suicide among those aged 14–17 and those aged 18–20. Gius (2015b) used 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. Kivisto et al. (2021) found that increasing numbers of state provisions concerning negligent storage and safety locks were significantly associated with reductions in firearm suicides among those aged 14–18, and Kappelman and Fording (2020) found that strict CAP laws reduced firearm suicides among those aged 24 and younger.

Among studies examining the effects of CAP laws on adults or the general population, Schell et al. (2020) estimated a 90-percent probability that CAP laws reduce firearm suicides in the total population. DeSimone, Markowitz, and Xu (2013) found uncertain effects of CAP laws on firearm-related self-injuries resulting in hospitalization among adults aged 18 or older.

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 firearm self-injuries among young people (including firearm suicides, suicide attempts, and self-injuries that were not the result of suicide attempts). Evidence for the effects of child-access prevention laws on firearm self-injuries and suicides among the total population is inconclusive.*





Effects on Violent Crime

Research Synthesis Findings

We identified eight quasi-experimental studies that provided evidence of the effects of CAP laws on violent crime, one of which (Anderson, Sabia, and Tekin, 2021) supersedes a working paper included in the previous edition of this report (Anderson, Sabia, and Tekin, 2018). One study's analysis was rated as having critical methodological concerns and is thus not discussed further.[35]

[35] Anderson and Sabia's (2018) analysis of homicides occurring in schools was rated as having critical concerns with model overfit (i.e., fewer than five incidents per estimated parameter).

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, and there were 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.

Evaluating a longer time frame (1985 to 2013) and restricting data on the basis of the age of the assailant, Anderson, Sabia, and Tekin (2021) examined the effects of CAP laws on murders. Using a Poisson model with controls for state and year fixed effects, region-by-year fixed effects, and state-specific trends, as well as several time-varying covariates measuring state-level demographic characteristics, socioeconomic conditions, police expenditures, and five other firearm laws, they estimated that CAP laws were associated with a significant 17-percent reduction in homicides involving juvenile assailants. When the authors distinguished negligent-storage from reckless endangerment CAP laws, negligent-storage laws significantly predicted reduced homicide numbers, while the effects of reckless endangerment laws on homicides were uncertain. They also found only uncertain evidence for the effects of CAP laws on homicides committed by adults and on nonfirearm homicides committed by youths. This study's results are supported by the presentation of evidence that the identifying assumptions of the model (parallel trends) were met.

Schell et al. (2020) estimated the effects of three gun laws (shall-issue, stand-your-ground, and CAP) on homicides among the full population within a Bayesian modeling framework. Using an autoregressive negative binomial regression model shown to have optimal statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), the authors parameterized policy effects to have a nonlinear phase-in over a six-year period, at which time the policies' effects were evaluated. They estimated that CAP laws were associated with a suggestive reduction in firearm homicides six years after their implementation (IRR = 0.93), corresponding to a 91-percent probability that CAP laws reduce firearm homicides. The effect of CAP laws on total homicides was uncertain, though it corresponded to an 82-percent probability that these laws reduced total homicides.

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. Because only two states (Colorado and Wisconsin) in the authors' sample passed reckless provision laws during the study time frame, identification in both specifications was driven largely by changes in state laws regarding negligent storage.

Azad et al. (2020) sought to distinguish the effects of different types of CAP laws on firearm homicide rates among victims ages 14 and under from 1991 to 2016. Specifically, the authors distinguished among three types of negligent-storage CAP laws: those imposing penalties if (1) a child could access the firearm, (2) a child did access a firearm, and (3) a child accessed and used a firearm. Two final classes of states were those with no negligent-storage CAP law but a reckless provision law and states with neither negligent-storage nor reckless provision laws. In negative binomial models with controls for random effects for states, five state time-varying covariates, three other gun laws, and an unspecified control for time, they found uncertain effects for reckless provision laws but a significant effect for the three classes of negligent-storage laws combined consistent with a 15-percent reduction in firearm homicides. Among the three types of negligent-storage laws, the authors found a significant effect for *child accessed* (IRR = 0.82) and suggestive effects for *child could access* (IRR = 0.82) and *child used* (IRR = 0.90). Because these analyses did not adjust standard error estimates for serial correlation within states over time, the CIs and significance levels reported may overstate the significance of each effect. Moreover, both the *child accessed* and *child could access* laws had only one state implementing each law, with a full three years of pre- and post-implementation data available in the study time frame, meaning that these effects may not be reliably estimated.

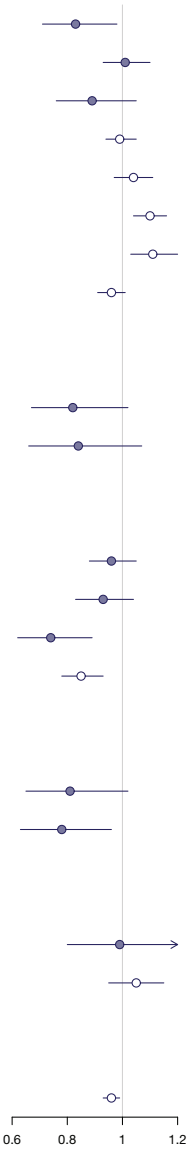
One final study evaluated the effects of CAP laws on workplace homicides. Sabbath, Hawkins, and Baum (2020) found that an increase in regulations regarding firearm storage related to restricting minors' access was associated with significantly reduced workplace homicide rates. Specifically, they estimated that a shift from the 25th percentile in the number of child safety provisions to the 75th percentile was associated with a 4-percent reduction in workplace homicides between 2011 and 2017. The gamma regression model used to generate this estimate, while including year fixed effects and six demographic and economic covariates, did not include state effects; it is unclear that the included covariates would suffice to control for other state differences that could explain both the adoption of safe-storage provisions and workplace homicide rates, and therefore the covariates could serve as confounds for the estimated law effects.

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

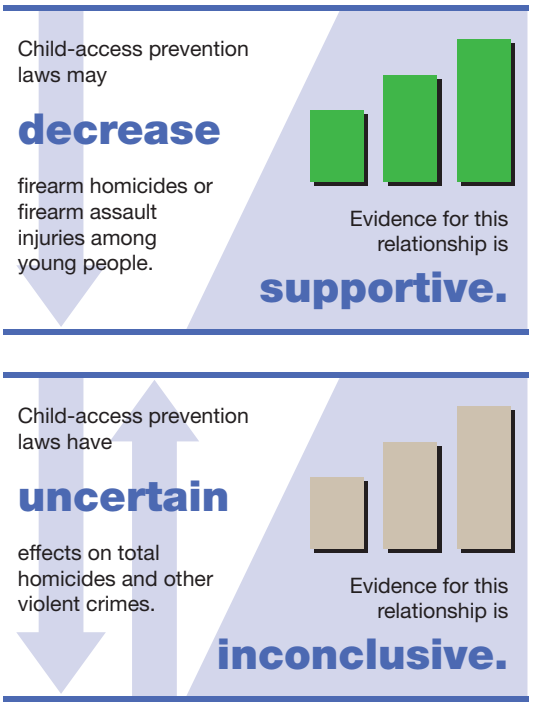
FIGURE 17.2

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

Study, by Policy	Outcome Measure	IRR [95% CI]
State CAP law	**Violent crime rate**	
Anderson, Sabia, & Tekin (2021)	Firearm murder, offender aged 12–17	0.83 [0.71, 0.98]
Anderson, Sabia, & Tekin (2021)	Firearm murder, offender aged 18+	1.01 [0.93, 1.10]
Cummings et al. (1997a)	Firearm homicide, aged 0–14	0.89 [0.76, 1.05]
Lott & Whitley (2001)	Total violent crime	0.99 [0.94, 1.05]
Lott & Whitley (2001)	Murder	1.04 [0.97, 1.11]
Lott & Whitley (2001)	Rape	1.10 [1.04, 1.16]
Lott & Whitley (2001)	Robbery	1.11 [1.03, 1.20]
Lott & Whitley (2001)	Assault	0.96 [0.91, 1.01]
State CAP law	**Assault injury rate**	
DeSimone, Markowitz, & Xu (2013)	Firearm assault, aged 0–17	0.82 [0.67, 1.02]
DeSimone, Markowitz, & Xu (2013)	Firearm assault, aged 18+	0.84 [0.66, 1.07]
State CAP law, negligent storage	**Homicide rate**	
Schell et al. (2020)	Total homicide	0.96 [0.88, 1.05]
Schell et al. (2020)	Firearm homicide	0.93 [0.83, 1.04]
Anderson, Sabia, & Tekin (2021)	Firearm murder, offender aged 12–17	0.74 [0.62, 0.89]
Azad et al. (2020)	Firearm homicide, aged 0–14	0.85 [0.78, 0.93]
State CAP law, negligent storage	**Assault injury rate**	
DeSimone, Markowitz, & Xu (2013)	Firearm assault, aged 0–17	0.81 [0.65, 1.02]
DeSimone, Markowitz, & Xu (2013)	Firearm assault, aged 18+	0.78 [0.63, 0.96]
State CAP law, reckless provision	**Homicide rate**	
Anderson, Sabia, & Tekin (2021)	Firearm murder, offender aged 12–17	0.99 [0.80, 1.23]
Azad et al. (2020)	Firearm homicide, aged 0–14	1.05 [0.95, 1.15]
Number of provisions for locking/storage around minors		
Sabbath, Hawkins, & Baum (2020)	Workplace homicide	0.96 [0.93, 0.99]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Age ranges refer to the age of the decedent unless otherwise specified. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details.



Conclusions

Excluding one study with critical methodological concerns, we identified four studies that examined the effects of CAP laws on firearm homicide or murder rates. Cummings et al. (1997a) reported a suggestive effect consistent with CAP laws reducing firearm homicide rates among children aged 14 or younger. Anderson, Sabia, and Tekin (2021) found that CAP laws were significantly associated with reductions in firearm murders committed by youths. Azad et al. (2020) found evidence that CAP laws that include negligent-storage provisions significantly reduce firearm homicide victimizations among those 14 and under. Finally, Schell et al. (2020) found suggestive evidence that these laws reduce firearm homicides in the total population. In addition, DeSimone, Markowitz, and Xu (2013) 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. Considering the relative strengths of these studies, we find *supportive evidence that child-access prevention laws reduce firearm homicides or firearm assault injuries among young people*.

Three studies examined the effects of CAP laws on overall homicide rates or other violent crime outcomes. 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. Sabbath, Hawkins, and Baum (2020) found that the number of provisions related to safe storage was significantly associated with reduced workplace homicide rates, and Schell et al. (2020) found uncertain effects of CAP laws on total homicide rates. Considering the relative strengths of these studies, we find *inconclusive evidence for the effects of child-access prevention laws on total homicide rates and other violent crimes.*

Effects on Unintentional Injuries and Deaths

Research Synthesis Findings

We identified seven studies that met our inclusion criteria and evaluated the effects of CAP laws on unintentional injuries or deaths. We identified no new relevant studies as part of this

update, but we rated one previously included study (Gius, 2015b) as having critical methodological concerns, so this study no longer contributes to our assessment of existing evidence.[36]

Five studies used state and time fixed-effects models to examine the relationship between state CAP laws and firearm-related unintentional injury or death. One study (Azad et al., 2020) instead controlled for state random effects and an unspecified temporal trend. With one exception—an estimate of the effect of reckless endangerment CAP laws in the state random effects specification—all estimated CAP law effects were consistent with the laws reducing unintentional firearm injuries or deaths among the population studied, although the precision of these estimates varied across studies and model specifications.

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 the effects were uncertain in states without felony laws. 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, no adjustment was made for clustered standard errors, so the standard errors and significance values reported in the paper were likely to be 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

[36] Gius (2015b) excludes state-years with ten or fewer deaths; such state-years constitute almost 50 percent of the sample, so this creates critical concerns with potential bias from systematically missing outcome data.

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 of 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 aged 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 implementation of such laws; 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 this analysis, safe-storage or negligent provision laws were associated with a significant reduction in unintentional injuries for those aged 18 or younger.

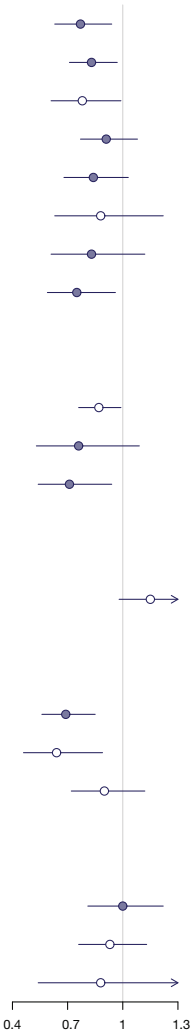
Azad et al. (2020) sought to distinguish the effects of different types of CAP laws on unintentional firearm death rates among victims ages 14 and under from 1991 to 2016. Specifically, the authors distinguished among three types of negligent storage CAP laws: those imposing penalties if (1) a child could access the firearm, (2) a child did access a firearm, and (3) a child accessed and used a firearm. Two final classes of states were those with no negligent-storage CAP law but a reckless provision law and states with neither negligent-storage nor reckless provision laws. In negative binomial models that included state random effects, five state time-varying covariates, three other gun laws, and an unspecified control for time, the authors found that negligent-storage CAP laws significantly reduced unintentional deaths among children by 13 percent, while reckless provision laws had a suggestive effect indicating a 15-percent increase in these deaths. Among the three types of negligent-storage laws, the authors found a significant effect for *child could access* (IRR = 0.41; 95-percent CI = [0.32, 0.51]), a suggestive effect for *child accessed* (IRR = 0.89; 95-percent CI = [0.79, 1.04]), and an uncertain effect for *child used* (IRR = 0.94; 95-percent CI = [0.80, 1.10]). Because these analyses did not adjust standard error estimates for serial correlation within states over time, the CIs and significance levels reported may overstate the significance of each effect. Moreover, both the *child accessed* and *child could access* laws had only one state implementing each law, with a full three years of pre- and post-implementation data available in the study time frame, meaning that these effects may not be reliably estimated.

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

Study, by Policy	Outcome Measure	IRR [95% CI]
State CAP law	**Unintentional injury rate**	
Cummings et al. (1997a)	Firearm death, aged 0–14	0.77 [0.63, 0.94]
Webster & Starnes (2000)	Firearm death, aged 0–14	0.83 [0.71, 0.97]
Hepburn et al. (2006)	Firearm death, aged 0–14	0.78 [0.61, 0.99]
Cummings et al. (1997a)	Firearm death, aged 15–19	0.91 [0.77, 1.08]
Cummings et al. (1997a)	Firearm death, aged 20–24	0.84 [0.68, 1.03]
Hepburn et al. (2006)	Firearm death, aged 55–74	0.88 [0.63, 1.22]
DeSimone, Markowitz, & Xu (2013)	Firearm injury, aged 0–17	0.83 [0.61, 1.12]
DeSimone, Markowitz, & Xu (2013)	Firearm injury, aged 18+	0.75 [0.59, 0.96]
State CAP law, negligent storage		
Azad et al. (2020)	Firearm death, aged 0–14	0.87 [0.76, 0.99]
DeSimone, Markowitz, & Xu (2013)	Firearm injury, aged 0–17	0.76 [0.53, 1.09]
DeSimone, Markowitz, & Xu (2013)	Firearm injury, aged 18+	0.71 [0.54, 0.94]
State CAP law, reckless provision		
Azad et al. (2020)	Firearm death, aged 0–14	1.15 [0.98, 1.34]
Felony CAP law		
Webster & Starnes (2000)	Firearm death, aged 0–14	0.69 [0.56, 0.85]
Hepburn et al. (2006)	Firearm death, aged 0–14	0.64 [0.46, 0.89]
Hepburn et al. (2006)	Firearm death, aged 55–74	0.90 [0.72, 1.12]
Misdemeanor CAP law		
Webster & Starnes (2000)	Firearm death, aged 0–14	1.00 [0.81, 1.22]
Hepburn et al. (2006)	Firearm death, aged 0–14	0.93 [0.76, 1.13]
Hepburn et al. (2006)	Firearm death, aged 55–74	0.88 [0.54, 1.44]

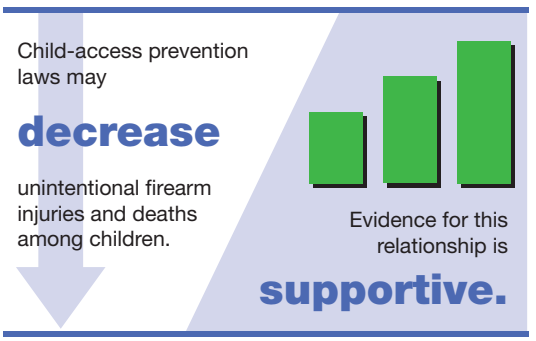


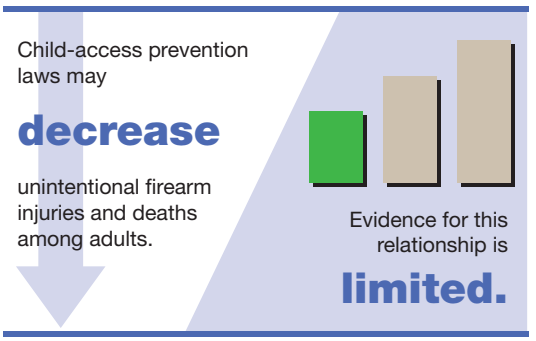
NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effects estimated from models with critical methodological concerns are not presented. See text and Appendix B for details.

Conclusions

Excluding one study with critical methodological concerns, we identified six qualifying studies of the effect of CAP laws on unintentional firearm injuries or deaths. Across four age groups, Lott and Whitley (2001) found three uncertain effects and one suggestive effect consistent with CAP laws increasing unintentional firearm deaths among children aged 5–9. Using the same data set but analyzing different years and different classifications of CAP laws, four other studies found effects indicating that CAP laws reduce unintentional firearm deaths among youths. 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. Similarly, 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). Using more-recent data from 1991 to 2016, Azad et al. (2020) found that negligent-storage CAP laws reduced unintentional firearm deaths among those aged 14 and under, whereas reckless provision CAP laws showed a suggestive effect indicating that the laws increased unintentional firearm deaths among this age group.

Finally, using a separate data series on firearm-related hospitalizations, DeSimone, Markowitz, and Xu (2013) found that CAP laws significantly reduced unintentional firearm injuries among those aged 17 or younger and among those aged 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 three studies that met our inclusion criteria and examined the effects of CAP laws on mass shootings in the United States, one of which contributes new findings to this updated review (Hamlin, 2021). Under our new quality assessment criteria, the analysis of one earlier study (Anderson and Sabia, 2018) was rated as subject to critical concerns with model overfit (i.e., fewer than two events per estimated parameter) and is thus not discussed further.

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. However, this study was focused on assessing the causal effects of concealed-carry laws, and safe-storage laws were included as a control variable; thus, these findings have serious causal interpretation concerns (Hünermund and Louw, 2023; Westreich and Greenland, 2013).

Hamlin (2021) used school shooting data from 1980 to 2019 to evaluate the relationship between firearm ownership rates and the number of school shooting incidents and casualties in a state. As part of these analyses, the author controlled for several state firearm laws relevant for youth, including laws that hold firearm owners criminally responsible for allowing a minor to access firearms. Using negative binomial regressions that controlled for a linear time trend, region fixed effects, estimated state gun ownership rates based on survey and administrative data, other gun laws, and poverty rates, the author estimated an uncertain relationship between CAP laws and school shooting incidence but found significant negative effects for school shooting injuries and fatalities. However, because the focus of this study was evaluating the relationship of firearm ownership with school shootings rather than evaluating CAP laws, these findings are unlikely to provide substantive causal insights for the likely effects of this policy.

Figure 17.4 displays the IRRs and CIs associated with the CAP laws examined in these studies. We do not show results from Lott (2003) for effects on multiple-victim public shooting incidents because of critical concerns that the models are overfit, given that there are fewer than five incidents per estimated parameter.

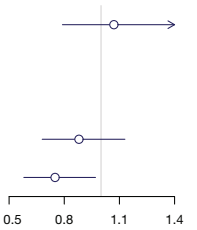
Conclusions

Excluding one study with critical methodological concerns, 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

FIGURE 17.4

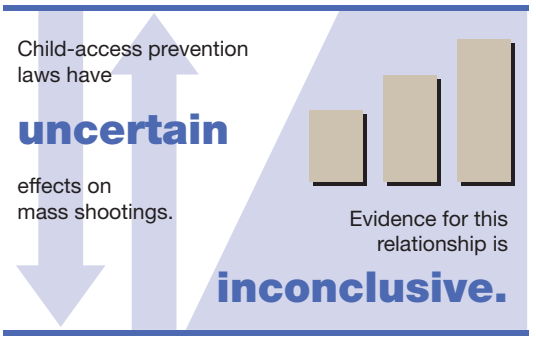
Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on Mass Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
State CAP law	**Multiple-victim shooting**	
Lott (2003)	Number of deaths and injuries	1.07 [0.79, 1.46]
State CAP law	**School shooting**	
Hamlin (2021)	Number of incidents	0.88 [0.68, 1.13]
Hamlin (2021)	Number of deaths and injuries	0.75 [0.58, 0.97]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Results from studies with critical methodological concerns are not presented. See text and Appendix B for details.

shooting casualties and mass shooting incidents, and Hamlin (2021) found that the laws were associated with a significantly lower number of school shooting fatalities and injuries. Given the methodological quality of these studies, 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 two studies that met our inclusion criteria and examined the relationship between CAP laws and the gun industry or ownership outcomes, one of which provides new information for this review (Anderson, Sabia, and Tekin, 2021). Brauer, Montolio, and Trujillo-Baute (2017) specified CAP policy using an index that scored the presence of a law as four, and zero otherwise.[37] 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

[37] 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.

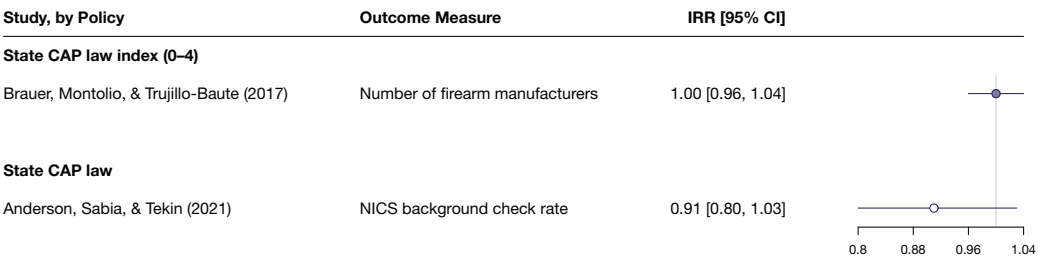
model does not appear to correspond to the distributional features of the authors' outcome variable, which limits the inference that can be drawn from this study.

In a study that showed a negative relationship between CAP laws and firearm homicides among youth, Anderson, Sabia, and Tekin (2021) conducted a supplementary analysis of NICS background check rates (intended to proxy for firearm ownership) to assess whether changes in firearm ownership are a mechanism explaining law effects on violence. Using a Poisson model with controls for state and year fixed effects, region-by-year fixed effects, and state-specific trends, as well as several time-varying covariates measuring state-level demographic characteristics, socioeconomic conditions, police expenditures, and five other firearm laws, the authors estimated suggestive effects consistent with CAP laws reducing total NICS background check rates and NICS background check rates for handguns. However, these analyses included a large number of parameters relative to the number of observations (a ratio of more than five to one), prompting critical concerns that these models were overfit.

Figure 17.5 displays the IRRs and CIs associated with the CAP laws examined in Brauer, Montolio, and Trujillo-Baute (2017) and Anderson, Sabia, and Tekin (2021).

FIGURE 17.5

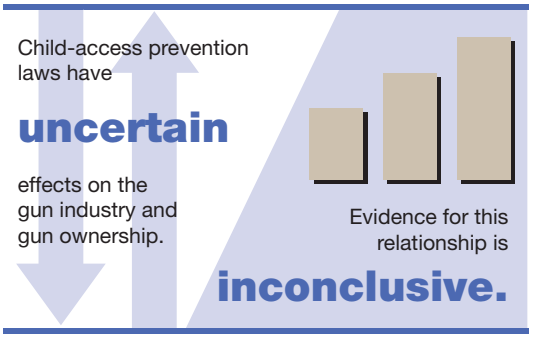
Incidence Rate Ratios Associated with the Effect of Child-Access Prevention Laws on the Gun Industry



NOTE: IRR values marked with unfilled circles indicate that there are serious or critical methodological concerns that are discussed in the text. See text and Appendix B for details.

Conclusions

We identified one qualifying study that found uncertain effects of CAP laws on firearm manufacturing plant locations, and one study that found suggestive effects showing that CAP laws reduced NICS background check rates (a proxy measure for gun ownership). Considering these studies, we find *inconclusive evidence for the effect of child-access prevention laws on the gun industry and gun ownership.*



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:

- police shootings
- defensive gun use
- hunting and recreation.

Chapter 17 References

Abadie, Alberto, "Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects," *Journal of Economic Literature*, Vol. 59, No. 2, June 2021.

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

Anderson, D. Mark, and Joseph J. Sabia, "Child-Access-Prevention Laws, Youths' Gun Carrying, and School Shootings," *Journal of Law and Economics*, Vol. 61, No. 3, August 2018.

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin, *Child Access Prevention Laws and Juvenile Firearm-Related Homicides*, National Bureau of Economic Research, Working Paper No. 25209, December 2018.

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin, "Child Access Prevention Laws and Juvenile Firearm-Related Homicides," *Journal of Urban Economics*, Vol. 126, November 2021.

Azad, Hooman Alexander, Michael C. Monuteaux, Chris A. Rees, Michael Siegel, Rebekah Mannix, Lois K. Lee, Karen M. Sheehan, and Eric W. Fleegler, "Child Access Prevention Firearm Laws and Firearm Fatalities Among Children Aged 0 to 14 Years, 1991–2016," *JAMA Pediatrics*, Vol. 174, No. 5, May 2020.

Azrael, Deborah, and Matthew Miller, "Reducing Suicide Without Affecting Underlying Mental Health: Theoretical Underpinnings and a Review of the Evidence Base Lining the Availability of Lethal Means and Suicide," in Rory C. O'Connor and Jane Pirkis, eds., *The International Handbook of Suicide Prevention*, 2nd ed., John Wiley and Sons, 2016.

Barber, Catherine, and David Hemenway, "Too Many or Too Few Unintentional Firearm Deaths in Official U.S. Mortality Data?" *Accident Analysis and Prevention*, Vol. 43, No. 3, 2011.

Barber, C., D. Hemenway, J. Hochstadt, and D. Azrael, "Underestimates of Unintentional Firearm Fatalities: Comparing Supplementary Homicide Report Data with the National Vital Statistics System," *Injury Prevention*, Vol. 8, No. 3, 2002.

Berrigan, John, Deborah Azrael, David Hemenway, and Matthew Miller, "Firearms Training and Storage Practices Among US Gun Owners: A Nationally Representative Study," *Injury Prevention,* Vol. 25, Supp. 1, 2019.

Betz, Marian E., Shannon Frattaroli, Christopher E. Knoepke, Rachel Johnson, Annette Christy, Julia P. Schleimer, Veronica A. Pear, Megan McCarthy, Reena Kapoor, Michael A. Norko, Ali Rowhani-Rahbar, Wenjuan Ma, Garen J. Wintemute, Jeffrey W. Swanson, and April M. Zeoli, "Extreme Risk Protection Orders in Older Adults in Six U.S. States: A Descriptive Study," *Clinical Gerontologist*, September 9, 2023.

Betz, M. E., M. Miller, C. Barber, B. Beaty, I. Miller, C. A. Camargo, Jr., and E. D. Boudreaux, "Lethal Means Access and Assessment Among Suicidal Emergency Department Patients," *Depression and Anxiety*, Vol. 33, No. 6, 2016.

Brauer, Jurgen, Daniel Montolio, and Elisa Trujillo-Baute, "How Do US State Firearms Laws Affect Firearms Manufacturing Location? An Empirical Investigation, 1986–2010," *Journal of Economic Geography*, Vol. 17, No. 4, 2017.

Burnham, Kenneth P., and David R. Anderson, "Multimodel Inference: Understanding AIC and BIC in Model Selection," *Sociological Methods and Research*, Vol. 33, No. 2, 2004.

CDC—*See* Centers for Disease Control and Prevention.

Centers for Disease Control and Prevention, "Multiple Cause of Death, 2018–2021, Single Race: Results," WONDER data system, undated-g. As of December 27, 2023: https://wonder.cdc.gov/mcd-icd10-expanded.html

Centers for Disease Control and Prevention, "Fatal Injury Reports, National, Regional, and State, 1981–2020," WISQARS database, 2022b. As of June 16, 2022: https://wisqars.cdc.gov/fatal-reports

Conner, Andrew, Deborah Azrael, and Matthew Miller, "Suicide Case-Fatality Rates in the United States, 2007 to 2014: A Nationwide Population-Based Study," *Annals of Internal Medicine*, Vol. 171, No. 12, 2019.

Conwell, Y., P. R. Duberstein, K. Connor, S. Eberly, C. Cox, and E. D. Caine, "Access to Firearms and Risk for Suicide in Middle-Aged and Older Adults," *American Journal of Geriatric Psychiatry*, Vol. 10, No. 4, 2002.

Crifasi, Cassandra K., Mitchell L. Doucette, Emma E. McGinty, Daniel W. Webster, and Colleen L. Barry, "Storage Practices of US Gun Owners in 2016," *American Journal of Public Health*, Vol. 108, No. 4, 2018a.

Cummings, P., D. C. Grossman, F. P. Rivara, and T. D. Koepsell, "State Gun Safe Storage Laws and Child Mortality Due to Firearms," *JAMA*, Vol. 278, No. 13, 1997a.

Dahlberg, L. L., R. M. Ikeda, and M. J. Kresnow, "Guns in the Home and Risk of a Violent Death in the Home: Findings from a National Study," *American Journal of Epidemiology*, Vol. 160, No. 10, 2004.

DeSimone, J., S. Markowitz, and J. Xu, "Child Access Prevention Laws and Nonfatal Gun Injuries," *Southern Economic Journal*, Vol. 80, No. 1, 2013.

Freedman, David A., "On the So-Called 'Huber Sandwich Estimator' and 'Robust Standard Errors,'" *American Statistician*, Vol. 60, No. 4, 2006.

Gius, Mark, "The Impact of Minimum Age and Child Access Prevention Laws on Firearm-Related Youth Suicides and Unintentional Deaths," *Social Science Journal*, Vol. 52, No. 2, 2015b.

Gius, Mark, "Examining the Impact of Child Access Prevention Laws on Youth Firearm Suicides Using the Synthetic Control Method," *International Review of Law and Economics*, Vol. 63, September 2020a.

Greenland, Sander, Mohammad Ali Mansournia, and Douglas G. Altman, "Sparse Data Bias: A Problem Hiding in Plain Sight," *BMJ*, Vol. 353, 2016.

Grossman, D. C., B. A. Mueller, C. Riedy, M. D. Dowd, A. Villaveces, J. Prodzinski, J. Nakagawara, J. Howard, N. Thiersch, and R. Harruff, "Gun Storage Practices and Risk of Youth Suicide and Unintentional Firearm Injuries," *JAMA*, Vol. 293, No. 6, 2005.

Hamlin, Daniel, "Are Gun Ownership Rates and Regulations Associated with Firearm Incidents in American Schools? A Forty-Year Analysis (1980–2019)," *Journal of Criminal Justice*, Vol. 76, September–October 2021.

HCUPnet: Healthcare Cost and Utilization Project, web tool, undated. As of December 27, 2023: https://datatools.ahrq.gov/hcupnet/#setup

Hepburn, L., D. Azrael, M. Miller, and D. Hemenway, "The Effect of Child Access Prevention Laws on Unintentional Child Firearm Fatalities, 1979–2000," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 61, No. 2, 2006.

Hünermund, Paul, and Beyers Louw, "On the Nuisance of Control Variables in Regression Analysis," *Organizational Research Methods*, December 25, 2023, online ahead of print.

Ilgen, M. A., K. Zivin, R. J. McCammon, and M. Valenstein, "Mental Illness, Previous Suicidality, and Access to Guns in the United States," *Psychiatric Services*, Vol. 59, No. 2, 2008.

Jennissen, Charles A., Kristel M. Wetjen, Cole C. Wymore, Nicholas R. Stange, Gerene M. Denning, Junlin Liao, and Kelly E. Wood, "Firearm Exposure and Storage Practices in the Homes of Rural Adolescents," *Western Journal of Emergency Medicine*, Vol. 22, No. 3, May 19, 2021.

Kappelman, Jack, and Richard C. Fording, "The Effect of State Gun Laws on Youth Suicide by Firearm: 1981–2017," *Suicide and Life-Threatening Behavior*, Vol. 51, No. 2, April 2021.

Kivisto, Aaron J., Katherine L. Kivisto, Erica Gurnell, Peter Phalen, and Bradley Ray, "Adolescent Suicide, Household Firearm Ownership, and the Effects of Child Access Prevention Laws," *Journal of the American Academy of Child and Adolescent Psychiatry*, Vol. 60, No. 9, September 2021.

LaFree, G., and C. Birbeck, *Controlling New Mexico Juveniles' Possession of Firearms*, New Mexico Criminal Justice Statistical Analysis Center, Working Paper 27, 1998.

Lee, Lois K., Katherine Douglas, and David Hemenway, "Crossing Lines—A Change in the Leading Cause of Death Among U.S. Children," *New England Journal of Medicine*, Vol. 386, No. 16, April 16, 2022.

Loeber, R., and R. Stallings, "Modeling the Impact of Interventions on Local Indicators of Offending, Victimization, and Incarceration," in R. Loeber and D. P. Farrington, eds., *Young Homicide Offenders and Victims: Risk Factors, Prediction, and Prevention from Childhood*, Springer, 2011.

Lott, John R., Jr., *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong*, Regnery Publishing, Inc., 2003.

Lott, John R., Jr., and John E. Whitley, "Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime," *Journal of Law and Economics*, Vol. 44, No. 2, 2001.

Miller, Matthew, and Deborah Azrael, "Firearm Storage in US Households with Children: Findings from the 2021 National Firearm Survey," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Miller, Matthew, Wilson Zhang, Ali Rowhani-Rahbar, and Deborah Azrael, "Child Access Prevention Laws and Firearm Storage: Results from a National Survey," *American Journal of Preventive Medicine*, Vol. 62, No. 3, 2022.

Monuteaux, M. C., D. Azrael, and M. Miller, "Association of Increased Safe Household Firearm Storage with Firearm Suicide and Unintentional Death Among US Youths," *JAMA Pediatrics*, Vol. 173, No. 7, 2019.

Office of Juvenile Justice and Delinquency Prevention, *OJJDP Statistical Briefing Book*, U.S. Department of Justice, October 29, 2021.

Oslin, D. W., C. Zubritsky, G. Brown, M. Mullahy, A. Puliafico, and T. Ten Have, "Managing Suicide Risk in Late Life: Access to Firearms as a Public Health Risk," *American Journal of Geriatric Psychiatry*, Vol. 12, No. 1, 2004.

Pallin, Rocco, Garen J. Wintemute, and Nicole Kravitz-Wirtz, "Firearm Practices, Perceptions of Safety, and Opinions on Injury Prevention Strategies Among California Adults With vs Without Children," *JAMA Network Open*, Vol. 4, No. 8, August 2021.

Prickett, Kate C., Alexa Martin-Storey, and Robert Crosnoe, "State Firearm Laws, Firearm Ownership, and Safety Practices Among Families of Preschool-Aged Children," *American Journal of Public Health*, Vol. 104, No. 6, 2014.

Rockett, Ian R. H., Nestor D. Kapusta, and Jeffrey H. Coben, "Beyond Suicide: Action Needed to Improve Self-Injury Mortality Accounting," *JAMA Psychiatry*, Vol. 71, No. 3, 2014.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Salhi, Carmel, Deborah Azrael, and Matthew Miller, "Parent and Adolescent Reports of Adolescent Access to Household Firearms in the United States," *JAMA Network Open*, Vol. 4, No. 3, March 9, 2021.

Schell, Terry L., Matthew Cefalu, Beth Ann Griffin, Rosanna Smart, and Andrew R. Morral, "Changes in Firearm Mortality Following the Implementation of State Laws Regulating Firearm Access and Use," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 26, 2020.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Schnitzer, Patricia G., Heather K. Dykstra, Theodore E. Trigylidas, and Richard Lichenstein, "Firearm Suicide Among Youth in the United States, 2004–2015," *Journal of Behavioral Medicine*, Vol. 42, No. 4, 2019.

Shenassa, E. D., M. L. Rogers, K. L. Spalding, and M. B. Roberts, "Safer Storage of Firearms at Home and Risk of Suicide: A Study of Protective Factors in a Nationally Representative Sample," *Journal of Epidemiology and Community Health*, Vol. 58, No. 10, 2004.

Simonetti, Joseph A., Deborah Azrael, and Matthew Miller, "Firearm Storage Practices and Risk Perceptions Among a Nationally Representative Sample of U.S. Veterans With and Without Self-Harm Risk Factors," *Suicide and Life-Threatening Behavior*, Vol. 49, No. 3, 2019.

Simonetti, Joseph A., Mary Kay Theis, Ali Rowhani-Rahbar, Evette J. Ludman, and David C. Grossman, "Firearm Storage Practices in Households of Adolescents With and Without Mental Illness," *Journal of Adolescent Health*, Vol. 61, No. 5, 2017.

Smith, P. N., J. Currier, and K. Drescher, "Firearm Ownership in Veterans Entering Residential PTSD Treatment: Associations with Suicide Ideation, Attempts, and Combat Exposure," *Psychiatry Research*, Vol. 229, No. 1–2, 2015.

Spicer, R. S., and T. R. Miller, "Suicide Acts in 8 States: Incidence and Case Fatality Rates by Demographics and Method," *American Journal of Public Health*, Vol. 90, No. 12, 2000.

Webster, D. W., L. H. Freed, S. Frattaroli, and M. H. Wilson, "How Delinquent Youths Acquire Guns: Initial Versus Most Recent Gun Acquisitions," *Journal of Urban Health*, Vol. 79, No. 1, 2002.

Webster, Daniel W., and Marc Starnes, "Reexamining the Association Between Child Access Prevention Gun Laws and Unintentional Shooting Deaths of Children," *Pediatrics*, Vol. 106, No. 6, 2000.

Webster, D. W., J. S. Vernick, A. M. Zeoli, and J. A. Manganello, "Association Between Youth-Focused Firearm Laws and Youth Suicides," *JAMA*, Vol. 292, No. 5, 2004.

Westreich, Daniel, and Sander Greenland, "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients," *American Journal of Epidemiology*, Vol. 177, No. 4, February 15, 2013.

CHAPTER 18

Concealed-Carry Laws

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). From the 1980s to the early 2000s, restrictions on the concealed carrying of firearms were progressively relaxed in most states. Many states transitioned from prohibitions on concealed carry to systems in which those who can demonstrate a need to carry a concealed weapon can apply for a permit to do so or to systems that give law enforcement discretion over who may obtain a permit (referred to as *may-issue laws*), and finally to systems in which there is no law enforcement discretion and permits may be obtained by nearly all legal gun owners (referred to as *shall-issue* or *right-to-carry laws*). Between the early 2000s and early 2024, regulation of concealed carry has been further relaxed; a majority of states no longer require any permits (referred to as *permitless carry*). Although several states still had may-issue laws in effect, in the 2022 *New York State Rifle and Pistol Association v. Bruen* decision, the Supreme Court ruled unconstitutional the use of subjective criteria in granting concealed-carry permits, overturning New York's may-issue law in prompting changes to laws in at least five other may-issue states.

The ease with which citizens can legally carry a concealed weapon or the permissiveness of the concealed-carry law likely affects the number of people who choose to carry weapons. Rowhani-Rahbar et al. (2022), for instance, found 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, 19.7 percent of handgun owners reported carrying their firearm in a 30-day period. In contrast, in states with shall-issue and permitless-carry laws, approximately 30 percent of handgun owners did so. Compared with research conducted in 2015, these findings suggest a national increase in the percentage of gun owners who recently carried a loaded firearm in public. The largest increases were observed in states with may-issue laws (which had an estimated prevalence of carrying of 9.1 percent in 2015; Rowhani-Rahbar et al., 2017). And although data on the number of persons with concealed-

carry permits are not readily available for many states, recent estimates suggest that the number of concealed-carry permit holders in the United States was around 11.1 million in 2014 and was more than 21.8 million in 2023 (Lott, Moody, and Wang, 2023). These estimates do not include the number of people who legally carry firearms without a permit, which is allowed in more than half of U.S. states as of 2024. Other sources of state variation in permitting include the permit fees charged, the amount of time that more-permissive laws have been in effect, and whether such laws allow local authorities to exercise discretion in issuing permits (i.e., may-issue versus shall-issue).

There are several ways that concealed-carry laws could affect gun violence, and assessing these effects is complicated because of the various ways in which laws, carrying behavior, defensive gun use, and violence might interact. By increasing the number of people carrying guns in public, permitless-carry and shall-issue laws that make it easier for citizens to carry 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. Such effects could be exacerbated if perceptions of crime and violence lead to an increase in the demand for concealed-carry permits or an increase in carrying behavior, as some studies suggest (Depew and Swensen, 2019; Stansfield, Semenza, and Silver, 2023), or if the expectation of encountering an armed victim induces more criminals to carry and use firearms.[1] Alternatively, concealed-carry laws could lead to reductions in the prevalence or severity of violent crime either because the prospect of encountering an armed victim deters criminal activity or shifts criminals into nonviolent crimes (e.g., larceny) for which the probability of encountering armed resistance is lower (Kovandzic and Marvell, 2003) or because victims will more frequently be able to use a gun to defend themselves (Fortunato, 2015). In a 2024 working paper that aimed to test the role of defensive gun use in reducing violence incidence and severity, Donohue et al. (2024) found that, at least among a selected set of criminal incidents, the use of a gun in self-defense reduces the likelihood of death or injury to the potential victim; however, in the aggregate, more-widespread incidents of defensive gun use are not protective and may even be harmful for public safety. Another analysis of 433 active-shooter incidents from 2000 through 2021 found that 64 of these (15 percent) ended because a bystander shot ($n = 22$) or otherwise subdued ($n = 42$) the attacker (Buchanan and Leatherby, 2022).

Concealed-carry laws could also influence violent crime through effects on several other mechanisms related to criminal markets for firearms and law enforcement actions. If more-

[1] Note that this theory—that increased carrying elevates violence—does not necessitate that those who newly carry under a shall-issue or permitless-carry regime are fully responsible for the increased violence. Indeed, some limited evidence suggests that those with concealed-carry permits have a lower likelihood of criminal involvement. A comparison of criminal conviction rates among holders and nonholders 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). A study comparing new concealed handgun permit holders in North Carolina with a matched sample of nonholders found a negative relationship between having a permit and subsequently being arrested, although estimates were imprecise (Billings, 2023).

permissive concealed-carry laws increase public carrying of firearms, these laws could increase criminal access to guns through theft, which may subsequently increase the use of guns in criminal activity. Indeed, one published paper (Billings, 2023) and one working paper (Donohue et al., 2023) suggest that stolen guns are an important mechanism that links concealed-carry laws with crime outcomes. Differences in concealed-carry policies may also prompt changes in law enforcement behavior, for example, through reallocating officer effort (e.g., processing permit applications). Changes in police expectations and activities could influence violent crime through effects on officers' effectiveness in deterring and solving crimes (Donohue et al., 2024). Furthermore, by changing officer perceptions about the likelihood of encountering an armed person during officer-civilian interactions, concealed-carry laws may directly affect police shootings (Doucette et al., 2022).

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 the 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 increases in concealed carrying by private citizens on suicides, unintentional injuries and deaths, and defensive gun use should be strongest for incidents involving handguns and that 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 these 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, 2024, 27 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.[2] Mississippi allows concealed carry without a permit if the handgun is kept "in

[2] Alabama, Alaska, Arizona, Arkansas, Florida, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Mississippi, Missouri, Montana, Nebraska, New Hampshire, North Dakota, Ohio, Oklahoma, South Dakota, Tennessee, Texas, Utah, Vermont, West Virginia, and Wyoming. See Ala. Code § 13A-11-85; Alaska Stat. § 11.61.220; Ariz. Rev. Stat. § 13-3112; Ark. Code Ann. §§ 5-73-101 (10); Ark. Code Ann. §§ 5-73-120(c)(4); Fla. Stat. § 790.01; Ga. Code § 16-11-126 (f); Ida. Code Ann. § 18-3302 (applies only outside cities); Ind. Code Ann. § 14-16-1-23 (b); Ia. Code § 724.5; Kan. Stat. Ann. §§ 21-6301, 21-6302; Ky. Rev. Stat. Ann. § 237; Me. Rev. Stat. Ann. § 2001-A; Miss. Ann. Code § 45-9-101; Mo. Rev. Stat. § 571.030; Mont. Code Ann. § 45-8-316; Neb. Rev. Stat. § 28-1202.01; N.H. Rev. Stat. Ann. § 159:6; N.D. Cent. Code §§ 62.1-04-01, 62.1-04-02; Ohio Rev. Code Ann. § 2923.111; Okla. Stat. Ann. Tit. 21, § 1272; S.D. Codified Laws § 23-7-7;

a sheath, belt holster or shoulder holster or in a purse, handbag, satchel, other similar bag or briefcase or fully enclosed case."[3] Twenty-three states and the District of Columbia require permits but 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.[4]

On June 22, 2022, the Supreme Court ruled that the use of subjective criteria in granting concealed-carry permits was unconstitutional with its decision in *New York State Rifle and Pistol Association v. Bruen*. At the time of the Supreme Court's *Bruen* decision, eight states had may-issue laws, in which law enforcement agencies had significant discretionary authority to deny permits.[5] In light of the *Bruen* ruling, many formerly may-issue states have had to redesign their permitting systems. Interestingly, however, the Supreme Court explicitly found that the may-issue regimes in Delaware, Rhode Island, and Connecticut were operating without unconstitutional discretionary criteria and so were, effectively, shall-issue states. Therefore, even under *Bruen*, some fairly restrictive concealed-carry permits with elements of discretion have survived. For instance, in Delaware, permits are conditional on, among other considerations, demonstration of "good moral character" as testified to by five "respectable citizens of the county in which the applicant resides."[6]

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

Tenn. Code Ann. § 39-17-1307; Tex. Penal Code § 46.02; Utah Code Ann. § 76-10-523; Vt. Stat. Ann. Tit. 13, §§ 4004, 4016 (concerning the only two places where one cannot carry a concealed weapon in Vermont); W. Va. Ann. Code § 61-7-3; Wyo. Stat. Ann. § 6-8-104.

3 Miss. Ann. Code § 45-9-101.

4 California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, South Carolina, Virginia, Washington, Wisconsin, and the District of Columbia. See Calif. Penal Code § 26150; Colo. Rev. Stat. Ann. § 18-12-203; Conn. Gen. Stat. § 29-28(b); Del. Code Tit. 11 §§ 1441, 1442; Hawaii Rev. Stat. Ann. § 134-9; 430 Ill. Comp. Stat. 66/4; La. Stat. Ann. § 1379.1.1; Md. Code Ann., Pub. Safety, §§ 5-301–5-314; Mass. Gen. Laws Ch. 140, § 131; Mich. Comp. Laws Ann. § 28.425a; Minn. Stat. Ann. § 624.714; Nev. Rev. Stat. Ann. § 202.3657; N.J. Stat. Ann. § 2C:58-4; N.M. Stat. Ann. § 29-19-4; N.Y. Penal Law § 400.00(2)(f); N.C. Gen. Stat. § 14-415.11; Oreg. Rev. Stat. Ann. § 166.291; Pa. Cons. Stat. 18 § 6109; R.I. Gen. Laws § 11-47-11; S.C. Code § 23-31-215; Va. Ann. Code § 18.2-308.04; Wash. Rev. Code § 9.41.070; Wisc. Stat. Ann. § 175.60; D.C.: via case law, *Wrenn v. District of Columbia*, 864 F.3d 650, D.C. Cir. 2017.

5 California, Delaware, Hawaii, Maryland, Massachusetts, New Jersey, New York, and Rhode Island. See Calif. Penal Code § 26150; Del. Code Tit. 11 §§ 1441, 1442; Hawaii Rev. Stat. Ann. § 134-9; Md. Code Ann., Pub. Safety, §§ 5-301–5-314; Mass. Gen. Laws Ch. 140, § 131; N.J. Stat. Ann. § 2C:58-4; N.Y. Penal Law § 400.00(2)(f); R.I. Gen. Laws § 11-47-11.

6 Del. C. 11 Sec. 1441(a)(2).

quite restrictive law but recognizes concealed-carry permits from several states with more-permissive shall-issue laws (USA Carry, 2017).

Effects on Suicide

Research Synthesis Findings

We identified seven studies that met our inclusion criteria and examined the effect of concealed-carry laws on suicide, two of which (Depew and Swensen, 2022; Liu, Siegel, and Sen, 2022) contribute new findings to this updated review.[7] One of these (Siegel et al., 2019) examined the specific 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 types of 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).

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, which spanned 1988 to 2003. Using data on the 11 states that made up their sample, they found a statistically significant effect of shall-issue laws on self-inflicted firearm injuries among those aged 18 or older.[8] 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 used log-linear models adjusting for national trends, state fixed effects, and a limited set of

[7] We treat the study by Bhatt et al. (2020) that evaluated policies reducing the minimum age at which individuals can obtain a concealed-carry permit in Missouri as contributing evidence to minimum age laws (see Chapter 3).

[8] The authors also evaluated effects on children under age 18, but these analyses had fewer than four events (injuries) per estimated parameter, which we consider as producing critical concerns because of model overfit.

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,[9] both may-issue and shall-issue laws were associated with significant increases in nonfirearm suicide rates. These results have serious limitations given that concealed-carry laws were included as ancillary covariates to better estimate the causal effects of background check and waiting period requirements.

Using a similar specification and data from 1991 to 2016, Siegel et al. (2019) examined the separate effects of shall-issue laws and permitless-carry laws on total and firearm-specific suicide rates. Controlling for nine other types of firearm policies, a proxy for the percentage of household gun ownership, and 11 socioeconomic variables, the authors found uncertain effects of shall-issue laws on suicide and firearm suicide. In contrast, results showed that permitless-carry laws were associated with a significant 5-percent increase in overall suicide rates and a suggestive 6-percent increase in firearm suicide rates. However, fewer than four states that adopted permitless carry in the study time frame had sufficient post-policy data, which threatens the reliability of these estimates. Additionally, the authors noted that their results for permitless carry failed a falsification test because the law was associated with (suggestive) increases in both firearm and nonfirearm suicide rates, although there may be mechanisms by which changes in firearm suicide rates because of the law subsequently affect nonfirearm suicide rates (Smart et al., 2020a). Furthermore, the authors' regression models controlled for a proxy of gun ownership that was a linear combination of the ratio of firearm suicides to suicides and hunting licenses; including the same measure (firearm suicides or suicides) in both the outcome term and predictor terms can create spurious correlations that bias the study's estimates of policy effects and their standard errors (National Research Council [NRC], 2004).

Schell et al. (2020) estimated the effects of three gun laws (shall-issue, stand-your-ground, and child-access prevention) within a Bayesian modeling framework. Using an autoregressive negative binomial regression model shown to have optimal statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), the authors parameterized policy effects to have a nonlinear phase-in over a six-year period, after which the policies' effects were evaluated. They estimated uncertain effects of shall-issue laws on total suicide rates and firearm suicide rates. These estimates were associated with an 83-percent and 90-percent probability that shall-issue laws increase total and firearm suicides, respectively.

Liu, Siegel, and Sen (2022) adopted a unique approach to assess the effects of shall-issue concealed-carry laws on firearm suicides in both the states that implemented such policies and nearby states. Analyzing data from 2000 to 2019, the authors used a linear spatial autoregressive model to decompose policy effects into a direct effect on rates in the state adopting the policy and an indirect spillover effect on other states. The regression model controlled

[9] 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.

for state fixed effects, year fixed effects, and several covariates related to demographics, economics, firearm ownership proxies, political variables, and the total number of state firearm laws. The counterfactual assumptions of this model are unconventional because all states affect and are affected by all other states, with the magnitudes of these effects varying based on states' geographic distance from one another. The results showed uncertain relationships between may-issue laws and firearm suicide rates overall. Estimates for within-state and spillover effects were also uncertain.

Finally, Depew and Swensen (2022) took a historical perspective and evaluated the effects of New York's 1911 Sullivan Act, which included the requirement that individuals obtain a license for carrying a concealed weapon and obtain a permit to own or purchase a concealable handgun. While their primary analysis could not disentangle the effects of the various components of the Sullivan Act, the authors conducted supplemental analyses of data from 1900 to 1929 that included additional states making further changes to concealed-carry requirements during this time. Using their two-way fixed-effects estimation strategy, the authors found that when comparing states, adopting may-issue requirements for carrying a concealed handgun had suggestive effects consistent with reducing overall suicide rates and had significant negative effects on firearm suicide rates. While this study is subject to only minor methodological concerns related to model overfit and a lack of evidence that the model's identifying assumptions hold, the historical nature of its analysis may make these results less generalizable to today's policy context.

Figure 18.1 displays the incidence rate ratios (IRRs) and confidence intervals (CIs) associated with the concealed-carry laws examined in these studies. We do not present the findings for child hospitalizations from DeSimone, Markowitz, and Xu (2013) because of critical concerns with model overfit, and we do not present results from Liu, Siegel, and Sen (2022) because the study's unconventional counterfactual makes its effect size estimates fundamentally unanalogous to other studies reviewed. Finally, we note that the CIs for the estimates from Schell et al. (2020) are derived from credible intervals presented in the paper and are thus not directly comparable with CIs.

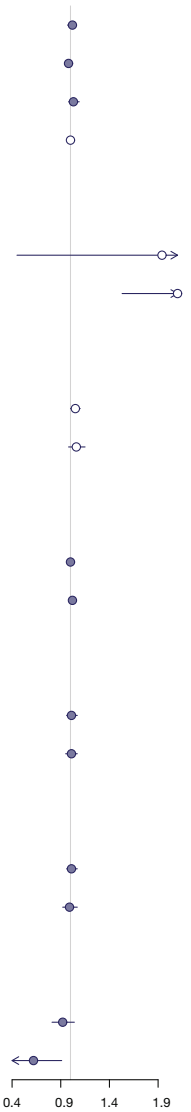
Conclusions

We identified seven 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. Depew and Swensen (2022) found suggestive evidence that may-issue laws reduce suicides and significant effects of may-issue laws for reducing firearm suicides. DeSimone, Markowitz, and Xu (2013) found that, among adults aged 18 or older, there was 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. Liu, Siegel, and Sen (2022) also found uncertain associations of concealed-carry requirements with firearm suicide rates. Siegel

FIGURE 18.1

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

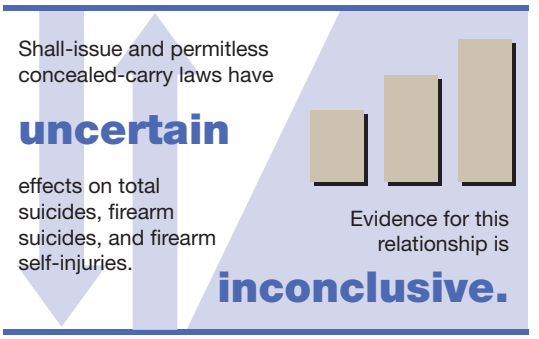
Study, by Policy	Outcome Measure	IRR [95% CI]
Shall-issue or permitless vs. may- or no-issue	**Suicide rate**	
Schell et al. (2020)	Total	1.02 [0.97, 1.05]
Rosengart et al. (2005)	Total	0.98 [0.96, 1.01]
Schell et al. (2020)	Firearm	1.03 [0.98, 1.09]
Rosengart et al. (2005)	Firearm	1.00 [0.97, 1.02]
Shall-issue or permitless vs. may- or no-issue	**Self-inflicted injury rate**	
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 0–17	1.94 [0.45, 8.38]
DeSimone, Markowitz, & Xu (2013)	Firearm, aged 18+	2.10 [1.53, 2.89]
Permitless-carry vs. may- or no-issue	**Suicide rate**	
Siegel et al. (2019)	Total	1.05 [1.00, 1.10]
Siegel et al. (2019)	Firearm	1.06 [0.98, 1.15]
Shall-issue vs. may- or no-issue		
Siegel et al. (2019)	Total	1.00 [0.98, 1.03]
Siegel et al. (2019)	Firearm	1.02 [0.99, 1.04]
Shall-issue vs. no-issue		
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.01 [0.96, 1.07]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	1.01 [0.95, 1.07]
May-issue vs. no-issue		
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.01 [0.96, 1.07]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	0.99 [0.92, 1.07]
May-issue vs. else		
Depew & Swensen (2022)	Total	0.92 [0.81, 1.04]
Depew & Swensen (2022)	Firearm	0.62 [0.32, 0.91]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.

et al. (2019) found that permitless carry increased total suicides, and Schell et al. (2020) found uncertain effects on both total suicides and firearm suicides.

Considering these studies and an assessment of their relative strengths, we find *inconclusive evidence for the effect of shall-issue and permitless 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 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 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 (p. 7).

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 1997 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 these concerns 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 on their earlier contributions to this literature. As a result, we focus on their later efforts to evaluate the effect of shall-issue laws.

TABLE 18.1
Studies Exploring the Effects of Shall-Issue Concealed-Carry Laws on Violent Crime, 1997–2004

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

[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).

Given the large volume of literature devoted to studying the relationship between concealed-carry laws and violent crime, we focus our review on the studies that we identified as meeting our inclusion criteria, that have been published since the release of the NRC (2004) report, and that were not rated as having critical methodological concerns.[10] We thus prioritize reviewing the findings of 43 studies, eight of which contribute new findings to the updated version of this report. For the interested reader, Appendix B contains details on the full set of 63 non-superseded studies, including those published prior to 2004, that met our inclusion criteria for this policy and outcome.

We first describe studies 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 many county law enforcement 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).[11]

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 span-

[10] We rated seven studies as meeting our quality assessment criteria for critical methodological concerns. Three studies (Mustard, 2001; Scantling et al., 2022; Zimmerman, 2014) had fewer than five events or observations per estimated parameter. Three studies (La Valle, 2013; La Valle and Glover, 2012; Manski and Pepper, 2018) had substantial errors in the coding of shall-issue laws, as documented in the prior edition of this report. One study (Smith and Petrocelli, 2019) failed to account for sufficient unit-specific or time-specific differences across the two cities analyzed and thus should not be interpreted as producing causal effect estimates.

[11] We identified 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 and found uncertain effects of changes in per capita concealed-carry permit rates on violent crime. Although this study provided coverage of the period before and after the passage of Florida's shall-issue law in 1987, it did not analyze the effect of the shall-issue policy change and thus did not meet our inclusion criteria.

ning 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 uncertain effects on intimate partner firearm homicides. The author also found uncertain effects of concealed-carry bans when 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) that was 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 rape and murder rates. They found no significant association between shall-issue laws and either assault or robbery rates. 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 models that use dummy variables, and they also found 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 esti-

mated 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 7 percent more firearm homicides (IRR = 1.07; 95-percent CI = [1.05, 1.09]). The authors also included a comparison outcome—nonfirearm homicides—based 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 very similar to the estimate for firearm homicides (IRR = 1.04; 95-percent CI = 1.01, 1.07), 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

An early set of studies that evaluated the relationship of shall-issue laws with homicide or murder rates produced conflicting findings. 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. Analyzing the same data and time period using a Poisson regression model that included state-specific linear trends, Rosengart et al. (2005) instead 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.

A contemporaneous study by Grambsch (2008) that analyzed data over a slightly longer time frame (1976 to 2001) documented a selection effect among states adopting shall-issue laws—specifically, that states that passed shall-issue laws in this period experienced an increasing trend in murder rates prior to adoption relative to other states. The author's estimates showed that, after controlling for regression to the mean, there were either uncertain effects or significant positive effects of shall-issue laws on relative murder rates (i.e., shall-issue laws increased murder rates) depending on the model used. These results contrast with

those found in two later studies. Lott (2010) estimated the effect of shall-issue laws using a regression model that 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. From this preferred specification, Lott (2010) interpreted the pattern of effect estimates as demonstrating that homicides declined significantly after the implementation of shall-issue laws.[12] Gius (2014) similarly found that states with may-issue or more-restrictive policies had higher gun-related murder rates than shall-issue states. However, the findings from all three of these studies (Gius, 2014; Grambsch, 2008; Lott, 2010) are based on models that did not include adjustments for serial correlation in the panel data, so the standard errors from these studies are likely underestimated (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

Conflicting findings from this earlier literature may, in large part, reflect limitations with analyzing shorter time periods of data and inaccurate inferences from a failure to account for the strong serial correlation in state-level homicide and violent crime data. However, subsequent studies that have had longer study time frames and thus estimated policy effects using a larger number of policy transitions have not always produced concordant results. Because these studies are largely relying on the same data sources, differences in results primarily reflect disagreement on which statistical methods are best suited for estimating the effects of concealed-carry laws on violent crime outcomes.

This disagreement resulted in a series of articles that assessed the implications for how changing model specifications can alter conclusions about the effects of concealed-carry laws. For example, Aneja, Donohue, and Zhang (2014) tested the sensitivity of effect estimates across a series of different specifications, varying the number of included demographic covariates; 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). 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, the authors found suggestive evidence that shall-issue laws increase assaults by 8 percent (see Figure 18.2). Effects of shall-issue laws on murder rates were uncertain. In the dummy specification, shall-issue laws significantly increased rates of rape by 12 percent, although some evidence indicated that the identifying assumptions of the model for rape rates were violated and other model specifications (e.g., the spline model)

[12] Lott (2010) provided an update of his earlier analyses that examined the effect of shall-issue laws on violent crime. 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 results 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 the 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.

produced uncertain effects. The authors also found suggestive evidence that shall-issue laws increased rates of robbery, although estimates again became uncertain in other specifications. 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.[13] Consequently, 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 Aneja, Donohue, and Zhang's (2014) model. They also found that the state-specific 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) constructed the overall policy effect estimate by summing over the state-specific estimates as though they were independent, a method unlikely to produce accurate inferential statistics.

As part of a series of analyses, Donohue, Aneja, and Weber (2019) provided estimates of the effects of shall-issue laws using updated data covering 1977–2014. During this period, 33 states implemented shall-issue laws. The authors' two-way fixed-effects model—which also controlled for demographic, economic, and law enforcement factors—indicated uncertain effects of the laws on murder and firearm murder rates but significant increases in rates of violent crime and property crime generally. Given evidence that the identifying assumptions of this two-way fixed-effects model (i.e., the parallel trends assumption) may have been violated, Donohue, Aneja, and Weber (2019) assessed the effects of shall-issue laws by constructing synthetic controls for each state that implemented a shall-issue law to, in theory, better predict how violent crime would have evolved in each shall-issue-implementing state had the

[13] Moody and Marvell (2018a) cite several other concerns regarding the truncation of the sample to omit confounding from the crack epidemic, incorrect standard errors, preference of state-level versus county-level crime data, absence of adjustments for multiple hypothesis testing, and interpretation of estimated effects when lead and lag dummy variables for the law were included. Donohue (2018) responds to these criticisms.

state not implemented the law. The authors estimated that, five years after passing shall-issue laws, states with such laws, on average, 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.

A subsequent study (Doucette et al., 2023a) used an augmented synthetic control method—which was designed to address some of the methodological limitations of the standard synthetic control approach—to examine the effects of shall-issue laws on violent crime, disaggregating by type of crime and firearm involvement. Using meta-analytic techniques to integrate across the state-specific policy estimates produced by the augmented synthetic control method, the authors found that shall-issue laws were associated with a significant 9-percent increase in both the nonfirearm homicide rate and the firearm aggravated assault rate (see Figure 18.2); effects on the firearm homicide rate and robbery rates were uncertain. Both Doucette et al. (2023a) and Donohue, Aneja, and Weber (2019) used methods for aggregating state-specific policy effect estimates that relied on independence assumptions, which may be violated if donor control states contribute to multiple state-specific estimates. This issue may threaten the accuracy of the studies' CIs for the pooled estimates (Ben-Michael, Feller, and Rothstein, 2021; Gleser and Olkin, 2009; Van den Noortgate et al., 2013).

Still, the attempt to pool estimates across multiple states' synthetic control estimates offers an improvement on prior work that used synthetic control methods to produce state-specific estimates of the effects of transitioning from no-issue (i.e., no one is permitted to carry concealed firearms) to shall-issue (Gius, 2019b). This earlier study found mixed results across the eight states of interest, with evidence of significant increases in both total and gun-related murders in only one state (New Mexico), but the inferential methods used by this study (t-test for post-period) are unlikely to accurately characterize estimate uncertainty. Although a sensitivity analysis using two-way fixed-effects regression found that, on average, transitioning from prohibiting concealed carry to a shall-issue regime was associated with significantly higher rates of gun-related murder and suggestive increases in overall murder rates, it is difficult to interpret these results in light of the heterogeneous control group. For both sets of analyses, the control group was composed of states that did not switch from no-issue to shall-issue laws over the period; however, some of these states may have switched from may-issue to shall-issue or shall-issue to permitless-carry laws. Another study—which attempted to construct a cleaner control group by examining a shorter time frame of 1991 to 2008 and estimating effects for both the transition from no issue to shall issue and the transition from may issue to shall issue—found a suggestive effect that the transition from no-issue to shall-issue laws caused a reduction in robbery rates but uncertain effects for rates of all other violent crimes (Barati, 2016). Employing similar models and data but covering a longer time frame (1986 to 2015), Hamill et al. (2019) 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 from 1991 to 2015 and estimating negative binomial regression models, 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 per county in that year. Nevertheless, the authors reported similar results from sensitivity analyses using a different data source, the FBI's Supplementary Homicide Reports, 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 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. Similar results were found by Fridel (2021) and Fridel (2021a), who instead estimated effects using generalized estimating equations to account for clustering within states over time and controlled for a wider set of time-varying state-level covariates and region fixed effects (rather than state fixed effects).

A subsequent study by Siegel et al. (2019) that distinguished between shall-issue and permitless-carry laws and that used data from 1991 to 2016 similarly showed significant increases in homicide rates with shall-issue laws and uncertain effects on these rates of permitless-carry laws. However, results for permitless-carry laws relied on policy changes in a few states with very short post-policy periods, which reduces statistical power and may threaten the validity of the studies' standard errors. Knopov et al. (2019) examined the same period but included a different set of covariates and considered differential effects by race. They found that shall-issue laws were associated with a significant increase in homicide rates for the total population, with no evidence of a differential effect among the race groups (Black and White); permitless-carry laws were associated with suggestive reductions in homicide rates.

Shi and Lee (2018) estimated a panel data model with interactive fixed effects and spatial dependence 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 out-

come 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 effects were assessed. However, for the outcome of murder, the study's models had an unfavorable ratio of estimated parameters to observations (about one to three), which prompts critical concerns that these models may have been overfit and thus produced unreliable estimates and CIs. Using a different approach for accommodating spatial dependencies, Liu, Siegel, and Sen (2022) instead estimated an uncertain overall association between concealed-carry restrictions and firearm homicide rates.

Schell et al. (2020) estimated the effects of three gun laws (shall-issue, stand-your-ground, and child-access prevention) within a Bayesian modeling framework using data from 1980 to 2016. Using an autoregressive negative binomial regression model shown to have well-performing statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), they parameterized policy effects to have a nonlinear phase-in over a six-year period, at which time the policies' effects were evaluated. They estimated uncertain effects of shall-issue laws on total suicide rates and firearm suicide rates. These estimates were associated with a 65-percent and 77-percent probability that shall-issue laws increase total and firearm homicide, respectively.

Van Der Wal (2022) applied marginal structural models to estimate the effects of shall-issue laws on violent crime using a long time series of data from 1959 to 2016. This method aimed to address time-dependent confounding by calculating a propensity score for the existence of a shall-issue law in each state-year, then estimating an outcome model that predicts the effect of shall-issue laws on violent crimes, weighting each state-year by the product of the propensity scores for all prior years for that state. Including state random effects to account for some time-invariant state-level characteristics and a natural spline for time to account for national trends, Van Der Wal (2022) found that shall-issue laws are significantly associated with higher violent crime rates. The author estimated a 7-percent increase in violent crime overall, reflecting a significant increase in murder and manslaughter (6 percent), robbery (11 percent), and aggravated assault (6 percent). Estimates were similar across a series of sensitivity analyses, although there was more variability in the estimated effects and associated standard errors for the murder and manslaughter outcome.

Two studies focused on how concealed-carry laws affect workplace homicide rates (Doucette, Crifasi, and Frattaroli, 2019; Sabbath, Hawkins, and Baum, 2020). Doucette, Crifasi, and Frattaroli (2019) estimated effects of shall-issue or permitless-carry laws using a negative binomial regression model that controlled for several other firearm laws, 14 social and economic state covariates, state and year fixed effects, region fixed effects, and random inter-

cepts for between-state effects.[14] Using data from 1992 to 2017, they estimated large and significant increases in firearm workplace-related homicides associated with more-permissive concealed-carry laws (IRR = 1.34; 95-percent CI = 1.16, 1.45). Covering a much shorter period (2011 to 2016), Sabbath, Hawkins, and Baum (2020) evaluated how the number of concealed-carry restrictions in a state (ranging from zero to seven) affected workplace homicide rates. Estimating gamma regressions that controlled for year fixed effects, other firearm laws, state-level socioeconomics, and the nonhomicide violent crime rate, the authors found that an additional restrictive concealed-carry provision reduced workplace homicide rates by 9 percent (95-percent CI = 0.87, 0.96).

One study estimated how shall-issue laws affected fatal or nonfatal assaults on police officers. Crifasi, Pollack, and Webster (2016) examined data from 1984 through 2013 to evaluate 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.

One study estimated how shall-issue laws affected firearm injury hospitalizations.[15] Neufeld et al. (2022) used data from 2000 to 2016 to evaluate the association of shall-issue concealed-carry laws with inpatient hospitalizations for firearm injury. The authors employed two-way fixed-effects regression models that adjusted for a set of state-level time-varying covariates, including demographic and socioeconomic characteristics, a proxy measure for household firearm ownership, the violent crime rate, per capita alcohol consumption, and other state firearm laws. They found suggestive effects indicating that shall-issue laws increase firearm injury hospitalizations. However, their analysis did not account for the uncertainty produced by the imputed nature of the outcome series (Carpenter and Kenward, 2012; Rubin, 1987), nor did it provide assessment of the model's identifying assumption, raising minor concerns related to inferential validity.

Depew and Swensen (2022) took a historical perspective and evaluated the effects of New York's 1911 Sullivan Act, which included the requirement that individuals obtain a license for carrying a concealed weapon and obtain a permit to own or purchase a concealable handgun. While their primary analysis could not disentangle the effects of the various components of the Sullivan Act, the authors conducted supplemental analyses of data from 1900 to 1929 that

[14] As described in the paper, the model may have been overparameterized. It is not clear how state fixed effects, region fixed effects, and random effects for between-state effects could all be identified in a single model. However, in the Stata software package used for these analyses, such overparameterization would not necessarily result in uninterpretable policy effect estimates because superfluous state or region effects would be dropped automatically.

[15] The study focused on firearm injury hospitalization rates, which include a mix of injuries due to assault, intentional self-injury, and accident. Given evidence that most firearm injury hospitalizations are a result of assaults (Peek-Asa, Butcher, and Cavanaugh, 2017; Leventhal, Gaither, and Sege, 2014), we include this study under the violent crime section.

included additional states making further changes to concealed-carry requirements during this time. Using their two-way fixed-effects estimation strategy, the authors found that when comparing states, adopting may-issue requirements for carrying a concealed handgun had an uncertain effect on state-level homicide rates. While this study is subject to only minor methodological concerns related to model overfit and a lack of evidence that the model's identifying assumptions hold, the historical nature of its analysis may make these results less generalizable to today's policy context.

Finally, one study (Gius, 2019a) evaluated the effects of state campus-carry laws, which explicitly allow the carrying of concealed firearms on college campuses by students, faculty, and other staff. The author also considered the effects of more-general state concealed-carry laws, distinguishing between shall-issue laws and permitless-carry laws. The study focused on how these laws influence rates of violent crime occurring on college campuses using offense data submitted by postsecondary institutions to the U.S. Department of Education. Aggregating these data to the state-year level, the author then used a log-linear specification with state and year fixed effects, as well as state-level time-varying controls, including more-general concealed-carry laws, college enrollment rates, demographics, and overall state-level violent crime rates. Findings showed uncertain associations of all types of carry laws with violent crime rates occurring on college campuses. However, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning that it may have been overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws.

City-Level Studies

Kovandzic, Marvell, and Vieraitis (2005) examined the effect of shall-issue laws on rates of 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, it is unclear how city-year observations with zeroes in the outcome were handled in the log-linear specification, prompting concerns that these estimates and CIs may be unreliable indicators of the true effects of the laws.

Siegel et al. (2020b) estimated the effects of stringent may-issue laws—which grant law enforcement authorities a high level of discretion for granting concealed-carry permits—on homicide rates between 1991 and 2016 within 197 medium-to-large U.S. cities (populations of at least 100,000 residents) and, separately, within approximately 50 nonurban areas.[16] The authors estimated policy effects using negative binomial models with controls for year and city fixed effects, state and jurisdiction-level covariates, and five additional firearm policies. For medium-to-large cities, they estimated a significant 17-percent reduction in rates of firearm homicide after repeal of "heightened showing may issue" laws and no discernable effect on rates of nonfirearm homicide, resulting in an estimated 13-percent reduction in total homicide rates. In nonurban areas, estimated effects were small and uncertain (see Figure 18.2).

Using four decades of data from approximately 65 cities with populations above 250,000 people, Donohue et al. (2023) evaluated the effects of shall-issue or permitless-carry laws on violent crime rates and assessed potential mechanisms by which these laws might produce their effects. In the primary analysis, the authors estimated log-linear regressions weighted by city population, controlling for state and year fixed effects, as well as demographic, socioeconomic, and law enforcement covariates. The authors also conducted event study analyses to support their models' identifying assumptions and to assess dynamic policy effects, conducted sensitivity analyses using newer difference-in-differences methods to accommodate heterogeneous treatment effects in the staggered adoption context, and assessed the robustness of results that drop city-years with zero crime cases to results that instead used a multiple imputation approach. Their results showed significant effects of shall-issue or permitless-carry laws increasing violent crime rates by about 20 percent in large cities (populations above 250,000), although additional analyses showed smaller (and likely statistically nonsignificant) effects on violent crime in smaller cities. Further evaluation of mechanisms suggested that gun thefts and reduced police clearance rates played an important role in shaping the effects of concealed-carry laws on violent crime.

Other Studies

Two studies that focused on the relationship between unmarried fertility or abortions and violent crime included shall-issue laws as a covariate in their models (Lott and Whitley, 2007; Kendall and Tamura, 2010). Analyzing data 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

[16] *Nonurban areas* are defined as cities not classified as medium-to-large cities or areas with crime reported at the county level (with the exception of Jefferson Parish, Louisiana, and Arlington County, Virginia), which the authors then aggregated up to the state level. Nine states that did not include any large or medium cities were treated as nonurban areas, although two of these states (Vermont and North Dakota) were dropped "because the homicide counts were too low to generate stable rate estimates" (Siegel et al., 2020b, p. 259).

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.

Six other studies focused on identifying the causal effects of other types of firearm policies but included shall-issue or permitless-carry laws as a covariate in their models (Adams, 2022; DeSimone, Markowitz, and Xu, 2013; Doucette et al., 2021; Gius, 2019a; Luca, Malhotra, and Poliquin, 2017; Webster, Crifasi, and Vernick, 2014). Luca, Malhotra, and Poliquin (2017) used data from 1977 to 2014 to evaluate the effects of background check and waiting-period laws on homicide rates among adults aged 21 or older. Their analysis used log-linear models that adjusted for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors. In sensitivity analyses that controlled for concealed-carry and permitting laws, they found that shall-issue and may-issue laws had uncertain effects on total and firearm homicide rates relative to no-issue regimes. In their state-level study of permit-to-purchase laws using data from 1999 to 2010, Webster, Crifasi, and Vernick (2014) instead 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.[17] However, their estimates showed an uncertain association between shall-issue laws and firearm homicide rates.

In a county-level analysis of the effects of ShotSpotter adoption, Doucette et al. (2021) controlled for whether a county was located in a state with a shall-issue or permitless concealed-carry regime. Their results indicated that concealed-carry laws that disallowed law enforcement discretion in permit issuance were associated with a significant 21-percent increase in firearm homicide rates. The stand-your-ground law study of Adams (2022) also found significant associations between concealed-carry laws and state-level rates of murder, aggravated assault, rape, and robberies. However, the referent policy group is unspecified and the author does not present marginal effects, which complicates drawing any sort of policy implications from these findings.

DeSimone, Markowitz, and Xu (2013) evaluated the effects of child-access prevention laws (see Chapter 17) 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 extraor-

[17] 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.

dinary 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.

One study (Gius, 2019a) evaluated the effects of state campus-carry laws, which explicitly allow the carrying of concealed firearms on college campuses by students, faculty, and other staff. They also considered the effects of more-general state concealed-carry laws, distinguishing between shall-issue laws and permitless-carry laws. The study focused on how these laws influence rates of violent crime occurring on college campuses using offense data submitted by postsecondary institutions to the U.S. Department of Education. The author aggregated these data to the state-year level, then used a log-linear specification with state and year fixed effects, as well as state-level time-varying controls, including more-general concealed-carry laws, college enrollment rates, demographics, and overall state-level violent crime rates. Findings showed uncertain associations of all types of carry laws with violent crime rates occurring on college campuses.

Two additional studies were primarily focused on methodological or data quality questions rather than on testing hypotheses about the effects of concealed-carry laws. 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, the study 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. The author 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 rates, which shall-issue laws appeared to cause to decline gradually, and robbery rates, which appeared to increase or decrease depending on the state.

Finally, 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 that 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 to 1992, Martin and Legault (2005)'s estimates showed that shall-issue laws significantly reduced total violent crime and, specifically, aggravated assault. The authors found only suggestive effects that the laws reduced rates of robbery and murder, as well as uncertain effects on rates of rape (see Figure 18.3). However, as with Lott (2000), the authors did not statistically adjust for serial correlation in the panel data, raising serious concerns that the estimates' CIs may be unreliable.

Figure 18.2 displays the IRRs and CIs associated with the concealed-carry laws examined in the studies published after the NRC (2004) review and not subject to critical methodological concerns. Figure 18.3 displays the studies for which we did find serious methodological issues. In these figures, we highlight effect estimates using only dummy-coded models for reasons discussed in Chapter 2 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. We exclude estimates from Smith and Petrocelli (2019) because of the aforementioned concerns with their study results, and we exclude the state-specific synthetic control estimates from Gius (2019b) because of insufficient information on effect sizes and inferential statistics. 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; Moody et al. (2014) offered only a hybrid model; Adams (2022) and Liu, Siegel, and Sen (2022) parameterized effects in ways that preclude comparison alongside the other studies; 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.

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 that raise 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 using just those 27 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 27 studies without serious methodological concerns, 17 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 12 studies that evaluated shall-issue laws and included data after 2000, six found only uncertain effects of these laws (Donohue, Aneja, and Weber, 2019; Hamill et al., 2019; Barati, 2016; Crifasi, Pollack, and Webster, 2016; Aneja, Donohue, and Zhang, 2014; Schell et al., 2020). Moody and Marvell (2018a) found uncertain effects of shall-issue laws on homicide rates in seven of eight evaluated years following policy adoption, with a single significant negative effect in the seventh year. Four recent stud-

FIGURE 18.2

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with No Serious Methodological Problems

Study, by Policy	Outcome Measure	IRR [95% CI]
Shall-issue or permitless vs. may- or no-issue	**Homicide rate**	
Van Der Wal (2022)	Total	1.06 [1.00, 1.11]
Schell et al. (2020)	Total	1.01 [0.95, 1.09]
Donohue, Aneja, & Weber (2019)	Total	1.02 [0.92, 1.12]
Fridel (2021a)	Total	1.05 [1.02, 1.08]
Hamill et al. (2019)	Total	1.00 [0.92, 1.08]
Aneja, Donohue, & Zhang (2014)	Total	1.03 [0.91, 1.17]
Hepburn et al. (2004)	Total	1.01 [0.94, 1.10]
Schell et al. (2020)	Firearm	1.03 [0.95, 1.12]
Donohue, Aneja, & Weber (2019)	Firearm	1.03 [0.90, 1.16]
Fridel (2021)	Firearm	1.11 [1.03, 1.20]
Hamill et al. (2019)	Firearm	1.07 [0.97, 1.17]
French & Heagerty (2008)	Firearm	1.06 [1.00, 1.12]
Doucette, Crifasi, & Frattaroli (2019)	Firearm, workplace homicides	1.34 [1.16, 1.45]
Shall-issue or permitless vs. may- or no-issue	**Violent crime rate**	
Donohue et al. (2023)	Total violent crime, large cities	1.20 [1.07, 1.36]
Van Der Wal (2022)	Total violent crime	1.07 [1.04, 1.11]
Donohue, Aneja, & Weber (2019)	Total violent crime	1.09 [1.03, 1.15]
Hamill et al. (2019)	Total violent crime	0.99 [0.97, 1.01]
Van Der Wal (2022)	Rape	1.01 [0.98, 1.04]
Hamill et al. (2019)	Rape	1.00 [0.97, 1.03]
Van Der Wal (2022)	Robbery	1.11 [1.05, 1.17]
Hamill et al. (2019)	Robbery	0.97 [0.94, 1.01]
Aneja, Donohue, & Zhang (2014)	Robbery	1.15 [0.98, 1.34]
Van Der Wal (2022)	Assault	1.06 [1.02, 1.11]
Hamill et al. (2019)	Assault	0.99 [0.97, 1.01]
Aneja, Donohue, & Zhang (2014)	Assault	1.08 [0.99, 1.18]
Shall-issue or permitless vs. may- or no-issue	**LEO assault rate**	
Crifasi, Pollack, & Webster (2016)	Fatal	1.02 [0.81, 1.29]
Crifasi, Pollack, & Webster (2016)	Fatal, handgun	0.92 [0.70, 1.21]
Crifasi, Pollack, & Webster (2016)	Nonfatal	0.72 [0.47, 1.10]
Crifasi, Pollack, & Webster (2016)	Nonfatal handgun	0.74 [0.41, 1.33]
Crifasi, Pollack, & Webster (2016)	Nonfatal, nonhandgun	0.74 [0.42, 1.30]

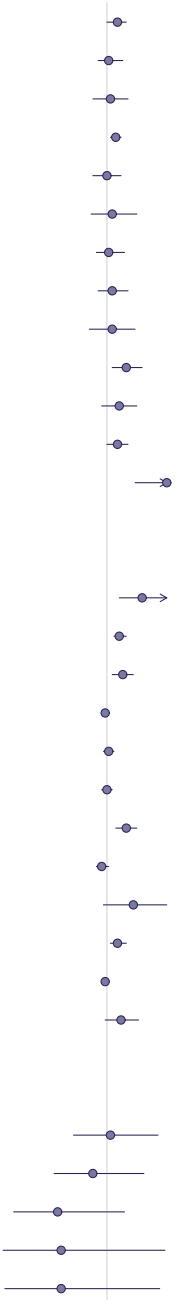
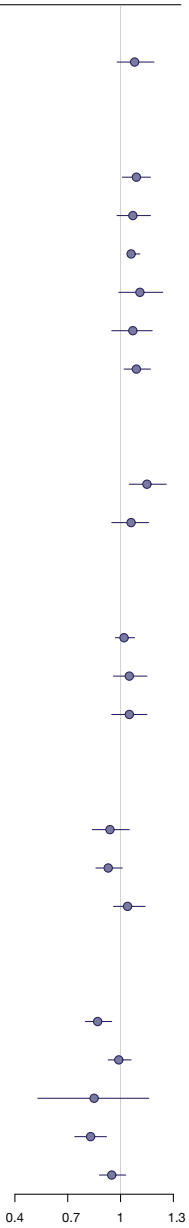


Figure continued on the next page

Figure 18.2—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Shall-issue or permitless vs. may- or no-issue	**Hospitalization rate**	
Neufeld et al. (2022)	Firearm injury	1.08 [0.98, 1.19]
Shall-issue vs. may- or no-issue	**Violent crime rate**	
Siegel et al. (2019)	Total homicide	1.09 [1.01, 1.17]
Rosengart et al. (2005)	Total homicide	1.07 [0.98, 1.17]
Knopov et al. (2019)	Age-adjusted homicide	1.06 [1.04, 1.11]
Rosengart et al. (2005)	Firearm homicide	1.11 [0.99, 1.24]
Doucette et al. (2023)	Firearm robbery	1.07 [0.95, 1.18]
Doucette et al. (2023)	Firearm assault	1.09 [1.02, 1.17]
Shall-issue vs. may- or no-issue	**Homicide rate**	
Siegel et al. (2019)	Firearm	1.15 [1.05, 1.26]
Doucette et al. (2023)	Firearm	1.06 [0.95, 1.16]
Shall-issue vs. may-issue	**Violent crime rate**	
Barati (2016)	Homicide	1.02 [0.97, 1.08]
Barati (2016)	Robbery	1.05 [0.96, 1.15]
Barati (2016)	Assault	1.05 [0.95, 1.15]
Shall-issue vs. no-issue		
Barati (2016)	Homicide	0.94 [0.84, 1.05]
Barati (2016)	Robbery	0.93 [0.86, 1.01]
Barati (2016)	Assault	1.04 [0.96, 1.14]
Stringent may-issue vs. else	**Homicide rate**	
Siegel et al. (2020b)	Total, mid to large cities	0.87 [0.80, 0.95]
Siegel et al. (2020b)	Total, nonurban areas	0.99 [0.93, 1.06]
Depew & Swensen (2022)	Total	0.85 [0.53, 1.16]
Siegel et al. (2020b)	Firearm, mid to large cities	0.83 [0.74, 0.92]
Siegel et al. (2020b)	Firearm, nonurban areas	0.95 [0.88, 1.03]



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 filled circles indicate that we identified no serious 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

Study, by Policy	Outcome Measure	IRR [95% CI]
Permitless-carry vs. may- or no-issue	**Homicide rate**	
Siegel et al. (2019)	Total	1.02 [0.90, 1.14]
Knopov et al. (2019)	Total, age-adjusted	0.94 [0.86, 1.02]
Siegel et al. (2019)	Firearm	1.03 [0.87, 1.22]
Shall-issue or permitless vs. may- or no-issue		
Siegel et al. (2017b)	Total, age-adjusted	1.06 [1.03, 1.10]
Webster, Crifasi, & Vernick (2014)	Total, age-adjusted	1.06 [0.99, 1.13]
Donohue & Levitt (2001)	Total	0.99 [0.93, 1.05]
Kendall & Tamura (2010)	Total	1.00 [0.99, 1.00]
Grambsch (2008)	Total	1.01 [0.98, 1.03]
Martin & Legault (2005)	Total	0.95 [0.90, 1.01]
Kovandzic, Marvell, & Vieraitis (2005)	Total	1.00 [0.94, 1.07]
Doucette et al. (2021)	Firearm	1.00 [0.94, 1.07]
Siegel et al. (2017b)	Firearm, age-adjusted	1.09 [1.05, 1.13]
Webster, Crifasi, & Vernick (2014)	Firearm, age-adjusted	1.06 [0.96, 1.16]
Crifasi et al. (2018b)	Firearm, urban only	1.07 [1.05, 1.09]
Rosengart et al. (2005)	Firearm	1.11 [0.99, 1.24]
Shall-issue or permitless vs. may- or no-issue	**Violent crime rate**	
Martin & Legault (2005)	Total violent crime	0.94 [0.91, 0.98]
Donohue & Levitt (2001)	Total violent crime	1.00 [0.97, 1.02]
Aneja, Donohue, & Zhang (2014)	Rape	1.12 [1.00, 1.26]
Kovandzic, Marvell, & Vieraitis (2005)	Rape	1.00 [0.95, 1.04]
Martin & Legault (2005)	Rape	0.98 [0.94, 1.03]
Kendall & Tamura (2010)	Rape	1.00 [0.99, 1.00]
Kovandzic, Marvell, & Vieraitis (2005)	Robbery	1.01 [0.95, 1.07]
Martin & Legault (2005)	Robbery	0.96 [0.91, 1.02]
Kendall & Tamura (2010)	Robbery	1.00 [1.00, 1.00]
Kovandzic, Marvell, & Vieraitis (2005)	Assault	0.98 [0.94, 1.02]
Martin & Legault (2005)	Assault	0.93 [0.89, 0.98]
Kendall & Tamura (2010)	Assault	1.00 [1.00, 1.00]
DeSimone, Markowitz, & Xu (2013)	Firearm assault injury, aged 0–17	2.49 [1.02, 6.08]
DeSimone, Markowitz, & Xu (2013)	Firearm assault injury, aged 18+	2.72 [1.74, 4.26]
Permitless-carry vs. may-issue		
Gius (2019a)	On college campuses	1.04 [0.83, 1.30]

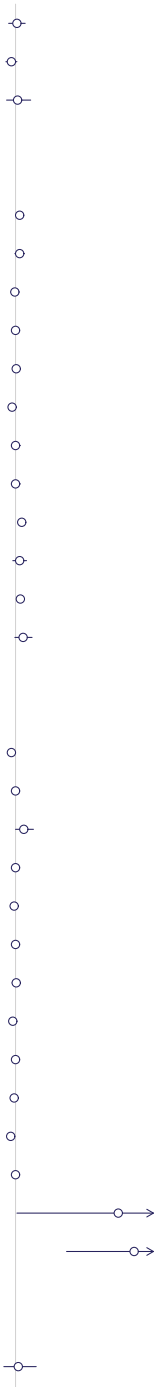
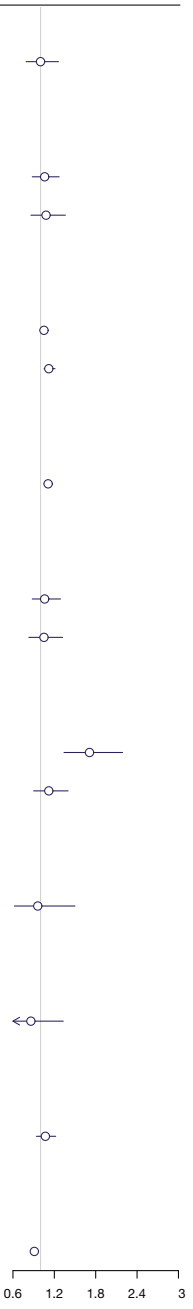


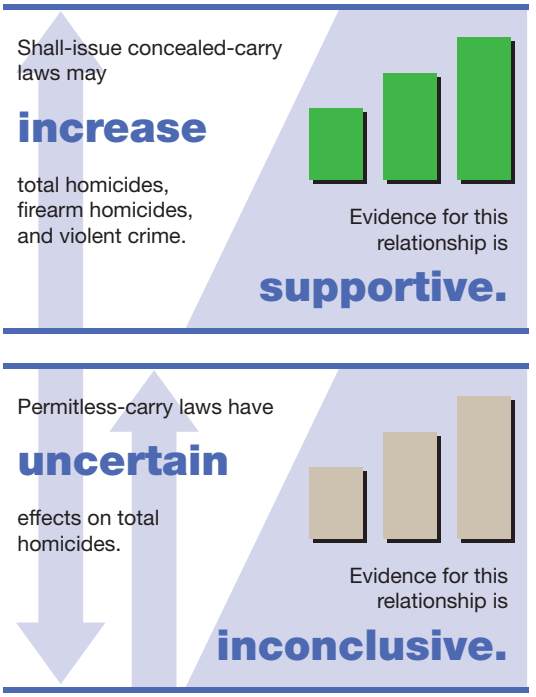
Figure continued on the next page

Figure 18.3—Continued

Study, by Policy	Outcome Measure	IRR [95% CI]
Shall-issue vs. may-issue		
Gius (2019a)	On college campuses	1.00 [0.79, 1.26]
Shall-issue vs. no-issue	**Homicide rate**	
Luca, Malhotra, & Poliquin (2017)	Homicide, aged 21+	1.06 [0.88, 1.27]
Luca, Malhotra, & Poliquin (2017)	Firearm homicide, aged 21+	1.08 [0.86, 1.36]
Change from prohibited to shall-issue	**Violent crime rate**	
Gius (2019b)	Homicide (murder)	1.05 [0.99, 1.11]
Gius (2019b)	Firearm homicide (murder)	1.12 [1.05, 1.21]
May-issue or no-issue vs. shall-issue	**Homicide rate**	
Gius (2014)	Firearm	1.11 [1.05, 1.16]
May-issue vs. no-issue		
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+	1.06 [0.88, 1.29]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+	1.05 [0.83, 1.32]
May-issue vs. shall-issue	**Intimate partner homicide rate**	
Roberts (2009)	Total IPH	1.71 [1.34, 2.19]
Roberts (2009)	Firearm IPH	1.12 [0.90, 1.40]
No-issue vs. shall-issue		
Roberts (2009)	Total IPH	0.96 [0.62, 1.50]
No issue vs. shall-issue		
Roberts (2009)	Firearm IPH	0.86 [0.56, 1.33]
Campus concealed carry allowed	**Violent crime rate**	
Gius (2019a)	On college campuses	1.07 [0.94, 1.22]
Number of concealed carry restrictions	**Homicide rate**	
Sabbath, Hawkins, & Baum (2020)	Workplace homicide	0.91 [0.87, 0.96]



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 unfilled circles indicate that serious methodological concerns are discussed in the text. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details.



ies found that shall-issue laws increased homicide rates (Van Der Wal, 2022; Fridel, 2021a; Knopov et al., 2019; Siegel et al., 2019), and one found that stringent may-issue laws requiring a heightened showing of suitability led to significantly lower rates of homicide in medium-to-large cities (Siegel et al., 2020b). Of the five studies focused on a period prior to 2000, one found that shall-issue laws caused a downward trend in homicide or murder rates (Strnad, 2007), one found a suggestive positive effect (Rosengart et al., 2005), and three found uncertain effects (Depew and Swensen, 2022; Hepburn et al., 2004; Ludwig, 1998). Weighing the relative strengths of these studies and considering these studies' analyses of different data sets and periods, we find that the existing literature provides *supportive evidence that shall-issue laws increase total homicides*.

Two studies separately estimated the effects of permitless-carry laws on homicides or murders, and both found uncertain effects (Knopov et al., 2019; Siegel et al., 2019). We therefore conclude that there is *inconclusive evidence for the effect of permitless-carry laws on total homicides*.

Firearm homicides. Eleven of the 27 studies examined the effects of shall-issue laws on firearm homicides, one examined effects of the laws on fatal assaults of law enforcement officers, and one examined the effects of the laws on workplace firearm homicides. Among these 13 studies, 11 evaluated data past 2000, and there was one with suggestive (Hamill et al., 2019) and four with significant (Doucette, Crifasi, and Frattaroli, 2019; Knopov et al., 2019; Fridel, 2021; Siegel et al., 2019) evidence of effects indicating that more-permissive concealed-carry laws increase firearm homicides. Siegel et al. (2020b) found that more-stringent concealed-carry laws significantly reduce firearm homicides in larger cities but have uncertain effects in nonurban areas. Five studies found uncertain effects of shall-issue laws on firearm homicides (Donohue, Aneja, and Weber, 2019; Doucette et al., 2023a; Liu, Siegel, and Sen, 2022; Luca, Malhotra, and Poliquin, 2017; Schell et al., 2020). 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). Both of the studies that focused on a period prior to 2000 found suggestive effects consistent with shall-issue laws increasing firearm homicides (French and Heagerty, 2008; Rosengart et al., 2005). Across all studies of firearm homicides, mean effect sizes generally corresponded to an average 3-percent to 20-percent increase in firearm homicide rates subsequent to shall-issue law implementation. Weighing the relative strengths of

these studies, and considering their analyses of different data sets and periods, we conclude that the existing literature provides *supportive evidence that shall-issue laws increase firearm homicides*.

Robberies. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase robbery rates, and Van Der Wal (2022) found that shall-issue laws significantly increase robbery rates. Shi and Lee (2018) also found significant short-term increases in robbery rates following shall-issue law adoption. In contrast, Hamill et al. (2019) instead found a suggestive effect indicating that shall-issue laws decrease robbery rates. Barati (2016) found a suggestive effect indicating that transitioning from no-issue to shall-issue laws reduces robbery rates, but transitioning from may-issue to shall-issue laws had an uncertain positive association with robbery rates. Three studies, the two most recent of which included data after 2000, found largely uncertain effects of shall-issue laws on robberies (Doucette et al., 2023a; Moody and Marvell, 2018a; Strnad, 2007). Weighing the relative strengths of these studies, we conclude that the best available studies provide *limited evidence that shall-issue laws increase robbery rates*.

Assaults. Doucette et al. (2023) found that shall-issue laws significantly increased firearm assault rates, and Van Der Wal (2022) found that shall-issue laws significantly increased total aggravated assault rates. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase assault rates. Shi and Lee (2018) also found a suggestive effect consistent with shall-issue laws increasing assault rates, although this did not manifest until seven years after law implementation. 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 (Barati, 2016; Hamill et al., 2019), found uncertain effects of shall-issue laws on assault (Barati, 2016; Hamill et al., 2019; Helland and Tabarrok, 2004; Strnad, 2007). Across all studies, mean effect size magnitudes generally corresponded to an average 0-percent to 10-percent increase in aggravated assault rates subsequent to shall-issue law implementation. Weighing the relative strengths of these studies, we conclude that the best available studies provide *moderate evidence that shall-issue laws increase assaults*.

Rapes. Three studies, two of which included data past 2000, found evidence of significant or suggestive declines in rape rates following shall-issue law adoption, although these effects often did not emerge until three or four years after implementation of the law (Moody and Marvell, 2018a; Shi and Lee, 2018; Strnad, 2007). Two other studies found uncertain evidence of an association between shall-issue laws and rape (Hamill et al., 2019; Van Der Wal, 2022). Therefore, we conclude that the best available studies provide *inconclusive evidence for the effect of shall-issue laws on rapes*.

Violent crime. Four studies (Donohue, Aneja, and Weber, 2019; Donohue et al., 2023; Durlauf, Navarro, and Rivers, 2016; Van Der Wal, 2022) aggregated all violent crimes into a single category and found that shall-issue laws significantly increase violent crime rates. Three studies, two of which included data past 2000, found largely uncertain effects of shall-issue laws

on overall violent crime (Hamill et al., 2019; Helland and Tabarrok, 2004; Shi and Lee, 2018). One study found an uncertain relationship between college-campus concealed-carry restrictions and violent crime on college campuses. Bolstered by evidence for the effect of shall-issue laws on specific components of violent crime, we conclude that there is *supportive evidence that shall-issue laws increase violent crime*.

Effects on Unintentional Injuries and Deaths

Research Synthesis Findings

We identified two quasi-experimental studies examining the effects 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 socio-economic characteristics, shall-issue laws had uncertain effects on unintentional handgun deaths and suggestive effects consistent with increasing unintentional non-handgun 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. Analyzing data with fewer than five incidents per estimated parameter presents critical methodological concerns with Lott and Mustard's (1997) estimates of the effect of concealed-carry laws on unintentional handgun deaths. While the outcome of unintentional non-handgun deaths was not as rare, failure to account for serial correlation and inadequate discussion of the method for ascertaining handgun involvement in the accidental death data lead to serious concerns with the accuracy of the policy effect estimates and their standard errors.

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).

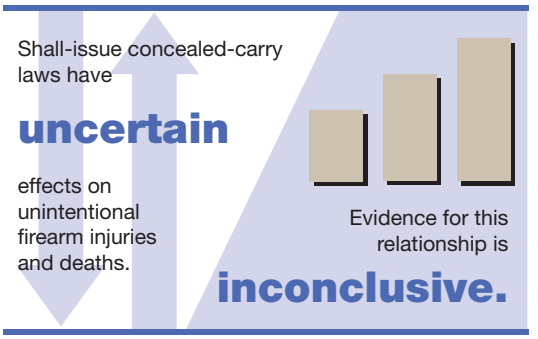
We identified only one more-recent study meeting our inclusion criteria that examined the effect of shall-issue laws on unintentional injuries (no studies identified the relationship between permitless-carry laws and this outcome). DeSimone, Markowitz, and Xu (2013) conducted a two-way fixed-effects 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 sug-

gests 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.

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.

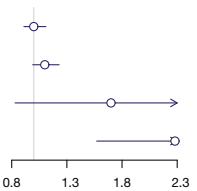


Considering these studies and an assessment of their relative strengths, we find that there is *inconclusive evidence for the effect of shall-issue concealed-carry laws on unintentional firearm injuries and deaths.*

FIGURE 18.4

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Unintentional Injuries and Deaths

Study, by Policy	Outcome Measure	IRR [95% CI]
Shall-issue vs. may- or no-issue	**Unintentional injury rate**	
Lott & Mustard (1997)	Handgun deaths	1.00 [0.91, 1.11]
Lott & Mustard (1997)	Nonhandgun deaths	1.10 [0.99, 1.23]
DeSimone, Markowitz, & Xu (2013)	Firearm injuries, aged 0–17	1.70 [0.83, 3.47]
DeSimone, Markowitz, & Xu (2013)	Firearm injuries, aged 18+	2.28 [1.57, 3.31]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. See Appendix B for details.

Effects on Mass Shootings

Research Synthesis Findings

We identified nine studies that examined the effects of shall-issue laws on mass shootings or school shootings, two of which (Fridel, 2021a; Siegel et al., 2020a) contribute new findings to this updated review. Two of the nine studies had critical methodological concerns and are not discussed further.[18]

Four studies used regression models for count data that controlled for state fixed effects, national trends (via either polynomial functions or year fixed effects), and various time-varying state-level covariates. While each study used different criteria for defining the outcome of interest and varied in the details of the exact model's specification, they mostly found uncertain associations between concealed-carry laws and school shootings (Gius, 2018), mass public shootings (Duwe, Kovandzic, and Moody, 2002), and mass shootings unconnected to other criminal activities (Webster et al., 2020). The one exception to this was Lott (2003), which used data from 1977 to 1997 to estimate the effect of shall-issue laws 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 author estimated Poisson regression models that 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 associated with significant reductions in total casualties from multiple-victim public shootings. However, the study estimated effects using Poisson regression models without modifications or standard error adjustments to account for the overdispersion in the outcome data. This can lead to severely underestimated standard errors (Cameron and Trivedi, 2005), which raises serious concerns about whether this study's estimates accurately describe the true effects of shall-issue laws.

Three additional studies evaluated the relationship of shall-issue concealed-carry laws with mass killings by estimating more-parsimonious model specifications that did not control for state fixed effects. Fridel (2021) examined the effect of shall-issue or permitless-carry laws (lagged by one year) on mass shootings involving four or more fatalities from 1991 to 2016. The author used negative binomial models with year fixed effects, region fixed effects, and a series of state-level controls that included measures of nonhomicide violent crime, nonfirearm homicide, socioeconomics, demographics, and gun ownership. Results showed uncertain effects of more-permissive concealed-carry laws on mass shooting incidents in comparison to states with may-issue laws. Effect estimates were similar across multiple sensitivity tests. In a response to commentary on the original study, Fridel (2021a) also found uncertain effects of shall-issue or permitless-carry laws on rates of *mass murder*, defined as an incident with

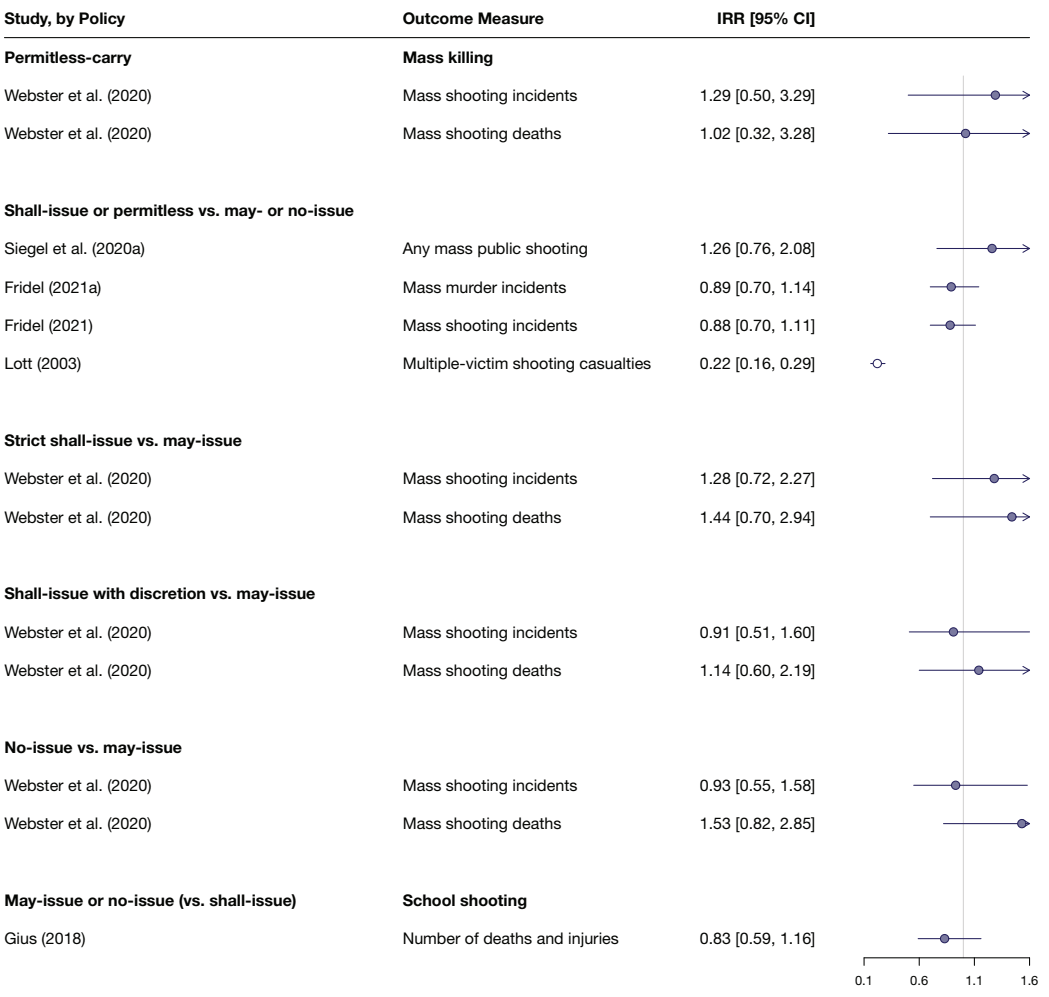
[18] The models estimated by Luca, Malhotra, and Poliquin (2020) and Blau, Gorry, and Wade (2016) had fewer than five mass shooting incidents per model parameter, which poses critical concerns related to overfit models producing inaccurate estimates and standard errors.

four or more persons murdered within 24 hours (not including the perpetrator or unborn children). Using logistic regression to estimate how shall-issue laws were associated with the probability of any mass public shooting incident occurring in a given state-year, Siegel et al. (2020a) similarly found uncertain effects of more-restrictive concealed-carry laws. A challenge across all of these studies is the relatively rare nature of these outcomes, which likely renders analyses underpowered to detect even fairly large effects.

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 not included in

FIGURE 18.5

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Mass Shootings



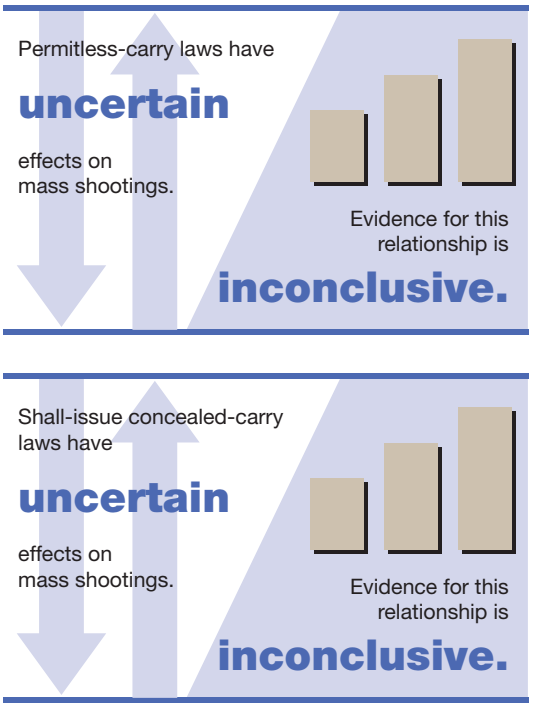
NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. Effect estimates with critical methodological concerns are not presented. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See text and Appendix B for details.

this figure because their approach yielded effect sizes that vary with time. We do not show results from Lott (2003) for effects on multiple-victim public shooting incidents because of critical concerns that the models are overfit given that there are fewer than five incidents per estimated parameter.

Conclusions

Permitless-carry laws. Excluding one study with critical methodological concerns, we identified one qualifying study (Webster et al., 2020) that examined the effects of permitless-carry laws on mass shooting incidence or fatalities and found uncertain effects of the laws. We thus find *inconclusive evidence for the effect of permitless-carry laws on mass shootings.*

Shall-issue concealed-carry laws. Excluding two studies with critical methodological concerns, we identified seven 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 the number of deaths or injuries resulting from multiple-victim shootings. Five additional studies (Duwe, Kovandzic, and Moody, 2002; Fridel, 2021; Gius, 2018; Siegel et al., 2020a; Webster et al., 2020) found uncertain effects of concealed-carry laws on mass shooting outcomes (e.g., incidence, injuries, and fatalities). One study (Fridel, 2021a) found uncertain effects of shall-issue or permitless-carry laws, considered together, on mass murder incidence rates. 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 Police Shootings

Research Synthesis Findings

Our updated search identified one study that met our inclusion criteria and assessed the effects of concealed-carry laws on police shootings (Doucette et al., 2022). Using fatal police shooting data from the crowdsourced Gun Violence Archive, Doucette et al. (2022) fit augmented synthetic control models to produce state-specific estimates for the change in police shootings per capita after a state adopted a permitless concealed-carry law, relative to the expected change in the absence of the law as estimated via a weighted combination of outcomes in states that retained shall-issue laws throughout the study period. Pooling the state-specific estimates with random-effects meta-analysis, the authors estimate that, on average, transitioning from a shall-issue to a permitless concealed-carry policy regime corresponded with a statistically significant 13-percent increase in police shootings. However, the meta-analytic techniques used to produce the pooled effect estimate treat the underlying state-specific effect estimates as independent; because each state-specific effect estimate is constructed using the same pool of potential donors as the synthetic control, the estimates are not necessarily independent, which may threaten the validity of the pooled estimate's standard errors (Van den Noortgate et al., 2013). Figure 18.6 displays the IRR and CIs associated with the concealed-carry laws examined in Doucette et al. (2022).

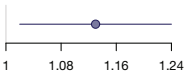
Conclusions

We identified one study that examined the effect of concealed-carry laws on police shootings. Doucette et al. (2022) found that permitless concealed-carry policies led to a significant increase in fatal police shootings. Because this study was subject to minor methodological concerns, we find *limited evidence that permitless-carry laws increase fatal police shootings of civilians.*

FIGURE 18.6

Incidence Rate Ratio Associated with the Effect of Concealed-Carry Laws on Police Shootings

Study, by Policy	Outcome Measure	IRR [95% CI]
Permitless-carry vs. shall-issue		
Doucette et al. (2022)	Police shooting rate	1.13 [1.02, 1.24]



NOTE: IRR values marked with filled circles indicate that we identified no serious methodological concerns. See Appendix B for details.

Effects on the Gun Industry

Research Synthesis Findings

We identified four studies that met our inclusion criteria and examined the effects of concealed-carry laws on the gun industry. One of the four (Ginwalla, 2014) was rated as having critical methodological concerns because of its failure to sufficiently account for potential confounds. Consequently, this study is not discussed further and does not contribute to our conclusions.

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.

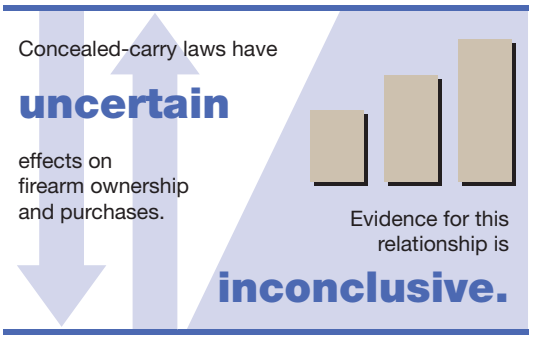
Steidley and Kosla (2018) examined the effects of shall-issue laws on per capita rates of National Instant Criminal Background Check System (NICS) background checks for firearm purchases as a proxy for firearm demand from 1999 to 2010. Logging this outcome variable, the authors estimated linear models 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.

Steidley (2019) extended this research, examining a longer time frame (1999 to 2016); distinguishing between may-issue laws, shall-issue laws, and permitless-carry laws; and separately evaluating effects on NICS background checks for handgun sales, long gun sales, and the proportion of sales that are handguns. This study used linear regression models that included state-level fixed effects and excluded the potentially problematic control for the ratio of firearm suicides to total suicides, but it did not control for temporal trends. Relative to states prohibiting concealed carry,[19] the author found uncertain effects on long gun sales and of may-issue laws but significantly higher rates of handgun background checks associated with shall-issue and permitless-carry laws. Given that model specifications do not control for overall national trends that may include growth in permissive concealed-carry laws and handgun purchases, it is unclear the extent to which these estimates reflect causal effects of the laws. Additionally, the estimates for permitless-carry laws are identified from relatively few state law changes, meaning that uncertainty around estimated effect sizes may be underestimated and effect estimates may reflect only short-run effects of the laws.

[19] Note that, according to the author's legal research, only seven states prohibited concealed carry at some point in the study time frame, and no states prohibited concealed carry after 2013. Accordingly, this comparison group may actually have little weight in the overall average estimate of the policy effect (Goodman-Bacon, 2021).

Figure 18.7 displays the IRRs and CIs associated with the concealed-carry laws examined in Duggan (2001), Steidley and Kosla (2018), and Steidley (2019).

Conclusions

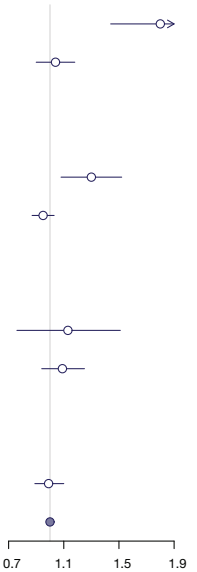


We identified three studies that were not subject to critical methodological concerns and that assessed the effects of concealed-carry laws on gun ownership or gun industry outcomes. Two studies (Steidley and Kosla, 2018; Duggan, 2001) found uncertain effects of shall-issue concealed-carry laws on gun ownership proxy outcomes. One study (Steidley, 2019) found that shall-issue and permitless-carry laws were associated with significant increases in a proxy measure for handgun sales, but that more-permissive concealed-carry laws had uncertain associations with long gun sales. Considering the methodological strengths of these four studies, we find *inconclusive evidence for the effects of concealed-carry laws on firearm ownership and purchases.*

FIGURE 18.7

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on the Gun Industry

Study, by Policy	Outcome Measure	IRR [95% CI]
Permitless-carry vs. no-issue		
Steidley (2019)	Background check rate, handgun	1.80 [1.44, 2.17]
Steidley (2019)	Background check rate, long gun	1.04 [0.90, 1.18]
Shall-issue vs. no-issue		
Steidley (2019)	Background check rate, handgun	1.30 [1.08, 1.52]
Steidley (2019)	Background check rate, long gun	0.95 [0.87, 1.03]
May-issue vs. no-issue		
Steidley (2019)	Background check rate, handgun	1.13 [0.76, 1.51]
Steidley (2019)	Background check rate, long gun	1.09 [0.94, 1.25]
Shall-issue vs. may- or no-issue		
Steidley & Kosla (2018)	Background check rate	0.99 [0.89, 1.10]
Duggan (2001)	Guns & Ammo magazine subscriptions	1.00 [0.98, 1.02]



NOTE: IRR values marked with unfilled circles indicate that there are serious methodological concerns that are discussed in the text. Filled circles indicate that we identified no serious methodological concerns. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale. See Appendix B for details.

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:

- 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 that we identified directly examined the laws' effects on defensive gun use.

Chapter 18 References

Adams, K. Alexander, "No Retreat: The Impact of Stand Your Ground Laws on Violent Crime," *Criminal Justice Review*, September 2022, online ahead of print.

Aldrich, John H., and Forrest D. Nelson, *Linear Probability, Logit, and Probit Models*, Sage Publications, 1984.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, "The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy," *American Law and Economics Review*, Vol. 13, No. 2, 2011.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, *The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy*, Stanford Law School, Olin Working Paper No. 461, December 1, 2014.

Ayres, Ian, and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review*, Vol. 1, No. 1–2, January 1999.

Ayres, Ian, and John J. Donohue III, "Shooting Down the More Guns, Less Crime Hypothesis," *Stanford Law Review*, Vol. 55, No. 4, 2003a.

Ayres, Ian, and John J. Donohue III, "The Latest Misfires in Support of the More Guns, Less Crime Hypothesis," *Stanford Law Review*, Vol. 55, No. 4, 2003b.

Barati, Mehdi, "New Evidence on the Impact of Concealed Carry Weapon Laws on Crime," *International Review of Law and Economics*, Vol. 47, 2016.

Bartley, William Alan, and Mark A. Cohen, "The Effect of Concealed Weapons Laws: An Extreme Bound Analysis," *Economic Inquiry*, Vol. 36, No. 2, 1998.

Ben-Michael, Eli, Avi Feller, and Jesse Rothstein, "The Augmented Synthetic Control Method," *Journal of the American Statistical Association*, Vol. 116, No. 536, 2021.

Benson, Bruce L., and Brent D. Mast, "Privately Produced General Deterrence," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Bhatt, Apurva, Xi Wang, An-Lin Cheng, Kalee L. Morris, Luke Beyer, Abbie Chestnut, Kristy Steigerwalt, and Jeffrey Metzner, "Association of Changes in Missouri Firearm Laws with Adolescent and Young Adult Suicides by Firearms," *JAMA Network Open*, Vol. 3, No. 11, November 2020.

Billings, Stephen B., "Smoking Gun? Linking Gun Ownership to Crime Victimization," *Journal of Public Economics*, Vol. 222, June 2023.

Black, Dan A., and Daniel S. Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies*, Vol. 27, No. 1, 1998.

Blau, Benjamin M., Devon H. Gorry, and Chip Wade, "Guns, Laws, and Public Shootings in the United States," *Applied Economics*, Vol. 48, No. 49, 2016.

Bronars, Stephen G., and John R. Lott, Jr., "Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns," *American Economic Review*, Vol. 88, No. 2, 1998.

Buchanan, Larry, and Lauren Leatherby, "Who Stops a 'Bad Guy with a Gun'?" *New York Times*, June 22, 2022.

Cameron, A. Colin, and Pravin K. Trivedi, *Microeconometrics: Methods and Applications*, Cambridge University Press, 2005.

Carpenter, James R., and Michael G. Kenward, *Multiple Imputation and Its Application*, John Wiley and Sons, 2012.

Cox, D. R., and E. J. Snell, *Analysis of Binary Data*, 2nd ed., Chapman and Hall, 1989.

Cramer, Clayton E., and David B. Kopel, "'Shall Issue': The New Wave of Concealed Handgun Permit Laws," *Tennessee Law Review*, Vol. 62, No. 3, 2005.

Crifasi, Cassandra K., Molly Merrill-Francis, Alex McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster, "Association Between Firearm Laws and Homicide in Urban Counties," *Journal of Urban Health*, Vol. 95, No. 3, 2018b.

Crifasi, Cassandra K., Keshia M. Pollack, and Daniel W. Webster, "Effects of State-Level Policy Changes on Homicide and Nonfatal Shootings of Law Enforcement Officers," *Injury Prevention*, Vol. 22, No. 4, 2016.

Depew, Briggs, and Isaac Swensen, "The Decision to Carry: The Effect of Crime on Concealed-Carry Applications," *Journal of Human Resources*, Vol. 54, No. 4, Fall 2019.

Depew, Briggs, and Isaac Swensen, "The Effect of Concealed-Carry and Handgun Restrictions on Gun-Related Deaths: Evidence from the Sullivan Act of 1911," *Economic Journal*, Vol. 132, No. 646, August 2022.

DeSimone, J., S. Markowitz, and J. Xu, "Child Access Prevention Laws and Nonfatal Gun Injuries," *Southern Economic Journal*, Vol. 80, No. 1, 2013.

Donohue, John J., "The Impact of Concealed Carry Laws," in Jens Ludwig and Philip J. Cook, eds., *Evaluating Gun Policy: Effects on Crime and Violence*, Brookings Institution Press, 2003.

Donohue, John J., "Guns, Crime, and the Impact of State Right-to-Carry Laws," *Fordham Law Review*, Vol. 73, No. 2, 2004.

Donohue, John J., "Laws Facilitating Gun Carrying and Homicide," *American Journal of Public Health*, Vol. 107, No. 12, 2017.

Donohue, John J., "More Gun Carrying, More Violent Crime," *Econ Journal Watch*, Vol. 15, No. 1, January 2018.

Donohue, John J., Abhay Aneja, and Kyle D. Weber, "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis," *Journal of Empirical Legal Studies*, Vol. 16, No. 2, 2019.

Donohue, John J., Samuel V. Cai, Matthew V. Bondy, and Philip J. Cook, *Why Does Right-to-Carry Cause Violent Crime to Increase?* National Bureau of Economic Research, Working Paper No. 30190, revised June 2023.

Donohue, John J., and Steven D. Levitt, "The Impact of Legalized Abortion on Crime," *Quarterly Journal of Economics*, Vol. 116, No. 2, 2001.

Donohue, John J., Alex Oktay, Amy L. Zhang, and Matthew Benavides, *Does Defensive Gun Use Deter Crime?* National Bureau of Economic Research, Working Paper No. 32108, February 2024.

Doucette, Mitchell L., Cassandra K. Crifasi, and Shannon Frattaroli, "Right-to-Carry Laws and Firearm Workplace Homicides: A Longitudinal Analysis (1992–2017)," *American Journal of Public Health*, Vol. 109, No. 12, 2019.

Doucette, Mitchell L., Christa Green, Jennifer Necci Dineen, David Shapiro, and Kerri M. Raissian, "Impact of ShotSpotter Technology on Firearm Homicides and Arrests Among Large Metropolitan Counties: A Longitudinal Analysis, 1999–2016," *Journal of Urban Health*, Vol. 98, No. 5, October 2021.

Doucette, Mitchell L., Alexander D. McCourt, Cassandra K. Crifasi, and Daniel W. Webster, "Impact of Changes to Concealed-Carry Weapons Laws on Fatal and Nonfatal Violent Crime, 1980–2019," *American Journal of Epidemiology*, Vol. 192, No. 3, February 24, 2023a.

Doucette, Mitchell L., Julie A. Ward, Alex D. McCourt, Daniel Webster, and Cassandra K. Crifasi, "Officer-Involved Shootings and Concealed Carry Weapons Permitting Laws: Analysis of Gun Violence Archive Data, 2014–2020," *Journal of Urban Health*, Vol. 99, No. 3, 2022.

Duggan, Mark, "More Guns, More Crime," *Journal of Political Economy*, Vol. 109, No. 5, 2001.

Durlauf, Steven, Salvador Navarro, and David Rivers, "Model Uncertainty and the Effect of Shall-Issue Right-to-Carry Laws on Crime," *European Economic Review*, Vol. 81, 2016.

Duwe, Grant, Tomislav Kovandzic, and Carlisle E. Moody, "The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings," *Homicide Studies*, Vol. 6, No. 4, 2002.

Fortunato, David, "Can Easing Concealed Carry Deter Crime?" *Social Science Quarterly*, Vol. 96, No. 4, December 2015.

French, B., and P. J. Heagerty, "Analysis of Longitudinal Data to Evaluate a Policy Change," *Statistics in Medicine*, Vol. 27, No. 24, 2008.

Fridel, Emma E., "Comparing the Impact of Household Gun Ownership and Concealed Carry Legislation on the Frequency of Mass Shootings and Firearms Homicide," *Justice Quarterly*, Vol. 38, No. 5, 2021.

Fridel, Emma E., "The Futility of Shooting Down Strawmen: A Response to Kleck (2020)," *Justice Quarterly*, Vol. 38, No. 5, 2021a.

Ginwalla, Rashna, Peter Rhee, Randall Friese, Donald J. Green, Lynn Gries, Bellal Joseph, Narong Kulvatunyou, Dafney Lubin, Terence O'Keeffe, Gary Vercruysse, Julie Wynne, and Andrew Tang, "Repeal of the Concealed Weapons Law and Its Impact on Gun-Related Injuries and Deaths," *Journal of Trauma and Acute Care Surgery*, Vol. 76, No. 3, 2014.

Gius, Mark, "An Examination of the Effects of Concealed Weapons Laws and Assault Weapons Bans on State-Level Murder Rates," *Applied Economics Letters*, Vol. 21, No. 4, 2014.

Gius, Mark, "The Effects of State and Federal Gun Control Laws on School Shootings," *Applied Economics Letters*, Vol. 25, No. 5, 2018.

Gius, Mark, "Campus Crime and Concealed Carry Laws: Is Arming Students the Answer?" *Social Science Journal*, Vol. 56, No. 1, March 2019a.

Gius, Mark, "Using the Synthetic Control Method to Determine the Effects of Concealed Carry Laws on State-Level Murder Rates," *International Review of Law and Economics*, Vol. 57, March 2019b.

Gleser, Leon J., and Ingram Olkin, "Stochastically Dependent Effect Sizes," in Harris Cooper, Larry V. Hedges, and Jeffrey C. Valentine, eds., *The Handbook of Research Synthesis and Meta-Analysis*, 2nd ed., Russell Sage Foundation, 2009.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, Vol. 225, No. 2, December 2021.

Grambsch, P., "Regression to the Mean, Murder Rates, and Shall-Issue Laws," *American Statistician*, Vol. 62, No. 4, 2008.

Hahn, Robert A., Oleg Bilukha, Alex Crosby, Mindy T. Fullilove, Akiva Liberman, Eve Moscicki, Susan Snyder, Farris Tuma, and Peter A. Briss, "Firearms Laws and the Reduction of Violence: A Systematic Review," *American Journal of Preventive Medicine*, Vol. 28, No. 2, 2005.

Hamill, Mark E., Matthew C. Hernandez, Kent R. Bailey, Martin D. Zielinski, Miguel A. Matos, and Henry J. Schiller, "State Level Firearm Concealed-Carry Legislation and Rates of Homicide and Other Violent Crime," *Journal of the American College of Surgeons*, Vol. 228, No. 1, 2019.

Helland, E., and A. Tabarrok, "Using Placebo Laws to Test 'More Guns, Less Crime,'" *Advances in Economic Analysis and Policy*, Vol. 4, No. 1, 2004.

Hepburn, Lisa, Matthew Miller, Deborah Azrael, and David Hemenway, "The Effect of Nondiscretionary Concealed Weapon Carrying Laws on Homicide," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 56, No. 3, 2004.

Kendall, Todd D., and Robert Tamura, "Unmarried Fertility, Crime, and Social Stigma," *Journal of Law and Economics*, Vol. 53, No. 1, 2010.

Knopov, Anita, Michael Siegel, Ziming Xuan, Emily F. Rothman, Shea W. Cronin, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates Among Black and White Populations in the United States, 1991–2016," *Health and Social Work*, Vol. 44, No. 4, 2019.

Kovandzic, Tomislav V., and Thomas B. Marvell, "Right-to-Carry Concealed Handguns and Violent Crime: Crime Control Through Gun Control?" *Criminology and Public Policy*, Vol. 2, No. 3, 2003.

Kovandzic, T. V., T. B. Marvell, and L. M. Vieraitis, "The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates—Evidence from Panel Data for Large Urban Cities," *Homicide Studies*, Vol. 9, No. 4, 2005.

La Valle, James M., "'Gun Control' vs. 'Self-Protection': A Case Against the Ideological Divide," *Justice Policy Journal*, Vol. 10, No. 1, 2013.

La Valle, James M., and Thomas C. Glover, "Revisiting Licensed Handgun Carrying: Personal Protection or Interpersonal Liability?" *American Journal of Criminal Justice*, Vol. 37, No. 4, 2012.

Leventhal, John M., Julie R. Gaither, and Robert Sege, "Hospitalizations Due to Firearm Injuries in Children and Adolescents," *Pediatrics*, Vol. 133, No. 2, February 2014.

Liu, Ye, Michael Siegel, and Bisakha Sen, "Association of State-Level Firearm-Related Deaths with Firearm Laws in Neighboring States," *JAMA Network Open*, Vol. 5, No. 11, November 2022.

Loftin, Colin, David McDowall, and Matthew D. Fetzer, "A Comparison of SHR and Vital Statistics Homicide Estimates for U.S. Cities," *Journal of Contemporary Criminal Justice*, Vol. 24, No. 1, 2008.

Lott, John R., Jr., "The Concealed-Handgun Debate," *Journal of Legal Studies*, Vol. 27, No. 1, January 1998a.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 1st ed., University of Chicago Press, 1998b.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 2nd ed., University of Chicago Press, 2000.

Lott, John R., Jr., *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong*, Regnery Publishing, Inc., 2003.

Lott, John R., Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*, 3rd ed., University of Chicago Press, 2010.

Lott, John R., Jr., and William M. Landes, *Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement*, University of Chicago Law School, John M. Olin Law and Economics Working Paper No. 73, 1999.

Lott, John R., Jr., Carlisle E. Moody, and Rujun Wang, *Concealed Carry Permit Holders Across the United States: 2023*, Crime Prevention Research Center, November 29, 2023.

Lott, John R., Jr., and D. B. Mustard, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," *Journal of Legal Studies*, Vol. 26, No. 1, 1997.

Lott, John R., Florenz Plassmann, and John E. Whitley, "Confirming More Guns, Less Crime," December 9, 2002.

Lott, John R., Jr., and John E. Whitley, "Measurement Error in County-Level UCR Data," *Journal of Quantitative Criminology*, Vol. 19, No. 2, June 2003.

Lott, John R., Jr., and John E. Whitley, "Abortion and Crime: Unwanted Children and Out-of-Wedlock Births," *Economic Inquiry*, Vol. 45, No. 2, 2007.

Lott, John R., Jr., John E. Whitley, and Rebekah C. Riley, *Concealed Carry Permit Holders Across the United States*, Crime Prevention Research Center, 2016.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, *The Impact of Mass Shootings on Gun Policy*, working paper, Harvard Business School, 2016.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "The Impact of Mass Shootings on Gun Policy," *Journal of Public Economics*, Vol. 181, January 2020.

Ludwig, Jens, "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data," *International Review of Law and Economics*, Vol. 18, No. 3, 1998.

Maltz, M. D., and J. Targonski, "A Note on the Use of County-Level UCR Data," *Journal of Quantitative Criminology*, Vol. 18, No. 2, 2002.

Manski, Charles F., and John V. Pepper, "How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions," *Review of Economics and Statistics*, Vol. 100, No. 2, 2018.

Martin, Robert A., and Richard L. Legault, "Systematic Measurement Error with State-Level Crime Data: Evidence from the 'More Guns, Less Crime' Debate," *Journal of Research in Crime and Delinquency*, Vol. 42, No. 2, May 2005.

Moody, Carlisle E., "Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Moody, Carlisle E., and Thomas B. Marvell, "The Debate on Shall Issue Laws, Continued," *Econ Journal Watch*, Vol. 6, No. 2, 2009.

Moody, Carlisle E., and Thomas B. Marvell, "The Impact of Right-to-Carry Laws: A Critique of the 2014 Version of Aneja, Donohue, and Zhang," *Econ Journal Watch*, Vol. 15, No. 1, January 2018a.

Moody, Carlisle E., and Thomas B. Marvell, "Clustering and Standard Error Bias in Fixed Effects Panel Data Regressions," *Journal of Quantitative Criminology*, May 2018b.

Moody, Carlisle E., Thomas B. Marvell, Paul R. Zimmerman, and Fasil Alemante, "The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication," *Review of Economics and Finance*, Vol. 4, 2014.

Mustard, David B., "The Impact of Gun Laws on Police Deaths," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

National Archive of Criminal Justice Data, "Resource Guide: Uniform Crime Reporting Program," webpage, undated. As of October 20, 2019: https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

Neufeld, Miriam Y., Michael Poulson, Sabrina E. Sanchez, and Michael B. Siegel, "State Firearm Laws and Nonfatal Firearm Injury-Related Inpatient Hospitalizations: A Nationwide Panel Study," *Journal of Trauma and Acute Care Surgery*, Vol. 92, No. 3, March 1, 2022.

New York State Rifle and Pistol Association v. Bruen, 597 U.S. ___, 2022.

NRC—*See* National Research Council.

Olson, David E., and Michael D. Maltz, "Right-to-Carry Concealed Weapon Laws and Homicide in Large U.S. Counties: The Effect on Weapon Types, Victim Characteristics, and Victim-Offender Relationships," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Peek-Asa, Corinne, Brandon Butcher, and Joseph E. Cavanaugh, "Cost of Hospitalization for Firearm Injuries by Firearm Type, Intent, and Payer in the United States," *Injury Epidemiology*, Vol. 4, No. 1, December 2017.

Phillips, C. D., O. Nwaiwu, D. K. McMaughan Moudouni, R. Edwards, and S. Lin, "When Concealed Handgun Licensees Break Bad: Criminal Convictions of Concealed Handgun Licensees in Texas, 2001–2009," *American Journal of Public Health*, Vol. 103, No. 1, 2013.

Plassmann, Florenz, and T. Nicolaus Tideman, "Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say," *Journal of Law and Economics*, Vol. 44, No. S2, 2001.

Plassmann, F., and J. E. Whitley, "Comments: Confirming 'More Guns, Less Crime,'" *Stanford Law Review*, Vol. 55, No. 4, 2003.

RAND Corporation, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*, RR-2088-RC, 2018. As of March 2, 2018: https://www.rand.org/pubs/research_reports/RR2088.html

Roberts, Darryl W., "Intimate Partner Homicide: Relationships to Alcohol and Firearms," *Journal of Contemporary Criminal Justice*, Vol. 25, No. 1, 2009.

Rosengart, M., P. Cummings, A. Nathens, P. Heagerty, R. Maier, and F. Rivara, "An Evaluation of State Firearm Regulations and Homicide and Suicide Death Rates," *Injury Prevention*, Vol. 11, No. 2, 2005.

Rowhani-Rahbar, Ali, Deborah Azrael, Vivian H. Lyons, Joseph A. Simonetti, and Matthew Miller, "Loaded Handgun Carrying Among US Adults, 2015," *American Journal of Public Health*, Vol. 107, No. 12, 2017.

Rowhani-Rahbar, Ali, Amy Gallagher, Deborah Azrael, and Matthew Miller, "Trend in Loaded Handgun Carrying Among Adult Handgun Owners in the United States, 2015–2019," *American Journal of Public Health*, Vol. 112, No. 12, December 2022.

Rubin, Donald B., *Multiple Imputation for Nonresponse in Surveys*, John Wiley and Sons, 1987.

Rubin, Paul H., and Hashem Dezhbakhsh, "The Effect of Concealed Handgun Laws on Crime: Beyond the Dummy Variables," *International Review of Law and Economics*, Vol. 23, No. 2, 2003.

Sabbath, Erika L., Summer Sherburne Hawkins, and Christopher F. Baum, "State-Level Changes in Firearm Laws and Workplace Homicide Rates: United States, 2011 to 2017," *American Journal of Public Health*, Vol. 110, No. 2, 2020.

Scantling, Dane R., Daniel N. Holena, Elinore J. Kaufman, Allyson M. Hynes, Justin Hatchimonji, James P. Byrne, Douglas Wiebe, and Mark J. Seamon, "Modifiable Factors Related to Firearm Homicides: A Broader View of Our Lane," *Annals of Surgery*, 2022, online ahead of print.

Schell, Terry L., Matthew Cefalu, Beth Ann Griffin, Rosanna Smart, and Andrew R. Morral, "Changes in Firearm Mortality Following the Implementation of State Laws Regulating Firearm Access and Use," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 26, 2020.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Schell, Terry L., Rosanna Smart, and Andrew R. Morral, *Suggestions for Estimating the Effects of State Gun Policies: Commentary on Four Methodological Problems in the Current Literature*, RAND Corporation, RR-A243-1, 2022. As of August 9, 2022:
https://www.rand.org/pubs/research_reports/RRA243-1.html

Shi, Wei, and Lung-Fei Lee, "The Effects of Gun Control on Crimes: A Spatial Interactive Fixed Effects Approach," *Empirical Economics*, Vol. 55, No. 1, 2018.

Siegel, Michael, Max Goder-Reiser, Grant Duwe, Michael Rocque, James Alan Fox, and Emma E. Fridel, "The Relation Between State Gun Laws and the Incidence and Severity of Mass Public Shootings in the United States, 1976–2018," *Law and Human Behavior*, Vol. 44, No. 5, 2020a.

Siegel, Michael, Molly Pahn, Ziming Xuan, Eric Fleegler, and David Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine*, Vol. 34, No. 10, 2019.

Siegel, Michael, Benjamin Solomon, Anita Knopov, Emily F. Rothman, Shea W. Cronin, Ziming Xuan, and David Hemenway, "The Impact of State Firearm Laws on Homicide Rates in Suburban and Rural Areas Compared to Large Cities in the United States, 1991–2016," *Journal of Rural Health*, Vol. 36, No. 2, March 2020b.

Siegel, Michael, Ziming Xuan, Craig S. Ross, Sandro Galea, Bindu Kalesan, Eric Fleegler, and Kristin A. Goss, "Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States," *American Journal of Public Health*, Vol. 107, No. 12, 2017b.

Smart, Rosanna, Terry L. Schell, Matthew Cefalu, and Andrew R. Morral, "Impact on Nonfirearm Deaths of Firearm Laws Affecting Firearm Deaths: A Systematic Review and Meta-Analysis," *American Journal of Public Health*, Vol. 110, No. 10, October 2020a.

Smith, Michael R., and Matthew Petrocelli, "The Effect of Concealed Handgun Carry Deregulation in Arizona on Crime in Tucson," *Criminal Justice Policy Review*, Vol. 30, No. 8, 2019.

Stansfield, Richard, Daniel Semenza, and Ian Silver, "The Relationship Between Concealed Carry Licenses and Firearm Homicide in the US: A Reciprocal County-Level Analysis," *Journal of Urban Health*, Vol. 100, No. 4, August 2023.

Steidley, Trent Taylor, *Movements, Malefactions, and Munitions: Determinants and Effects of Concealed Carry Laws in the United States*, dissertation, Ohio State University, 2016.

Steidley, Trent, "The Effect of Concealed Carry Weapons Laws on Firearm Sales," *Social Science Research*, Vol. 78, February 2019.

Steidley, Trent, and Martin T. Kosla, "Toward a Status Anxiety Theory of Macro-Level Firearm Demand," *Social Currents*, Vol. 5, No. 1, 2018.

Strnad, Jeff, "Should Legal Empiricists Go Bayesian?" *American Law and Economics Review*, Vol. 9, No. 1, Spring 2007.

Texas Department of Public Safety, *Conviction Rates for Handgun License Holders Reporting Period: 01/01/2021–12/31/2021*, March 18, 2022.

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

United States Concealed Carry Association, "Traveling? Know Concealed-Carry Permit Info by State," August 7, 2013.

USA Carry, "Concealed-Carry Permit Reciprocity Maps," webpage, April 20, 2017. As of June 29, 2017:
http://www.usacarry.com/concealed_carry_permit_reciprocity_maps.html

Van den Noortgate, Wim, José Antonio López-López, Fulgencio Marín-Martínez, and Julio Sánchez-Meca, "Three-Level Meta-Analysis of Dependent Effect Sizes," *Behavior Research Methods*, Vol. 45, No. 2, June 2013.

Van Der Wal, Willem M., "Marginal Structural Models to Estimate Causal Effects of Right-to-Carry Laws on Crime," *Statistics and Public Policy*, Vol. 9, No. 1, 2022.

Violence Policy Center, "Concealed Carry Killers," webpage, 2017. As of March 23, 2017:
http://concealedcarrykillers.org/

Webster, D., C. K. Crifasi, and J. S. Vernick, "Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides," *Journal of Urban Health*, Vol. 91, No. 2, 2014.

Webster, Daniel W., Alexander D. McCourt, Cassandra K. Crifasi, Marisa D. Booty, and Elizabeth A. Stuart, "Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States," *Criminology and Public Policy*, Vol. 19, No. 1, 2020.

Zimmerman, Paul R., "The Deterrence of Crime Through Private Security Efforts: Theory and Evidence," *International Review of Law and Economics*, Vol. 37, 2014.

CHAPTER 19

Gun-Free Zones

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).[1]

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). The act also mandates annual reporting of such incidents to the U.S. Department of Education (Jones, Sable, and Li, 2023). During the 2017–2018 academic year, 8.6 percent of elementary and secondary schools reported having at least one firearm incident; this fell to 6.6 percent during the 2018–2019 school year. During the school years most affected by the coronavirus disease 2019 (COVID-19) pandemic (2019–2020 and 2020–2021), the percentage of schools reporting at least one firearm incident fell to 5.3 and 3.5 percent, respectively.

In light of the recent Supreme Court decision in *New York State Rifle and Pistol Association v. Bruen* (597 U.S. ___), which ruled unconstitutional the use of subjective criteria in granting concealed-carry permits, several states have alternatively sought to restrict public carrying of firearms by expanding the locations designated as *sensitive places*, where guns are prohibited. New York, for instance, enacted legislation banning firearm carriage in privately owned locations unless the property owner grants explicit consent (Willinger, 2022). It is unclear whether these prohibitions will withstand subsequent litigation, but they represent a potentially substantial expansion of the types of gun-free zones designated by state law.

[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.

Gun-free zones are intended to reduce violent crime, suicides, unintentional firearm injuries and deaths, and mass shootings in specific locations. In theory, gun-free zones reduce or eliminate the presence of guns in these areas, thereby eliminating the risk of unintentional firearm injuries from 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 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 have 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 in which an incident occurred had a policy of allowing or disallowing firearms, which may necessitate fine-grained detail on the location of the incident; and determining whether the location had a means of enforcing that policy, such as bag checks or magnetometer screening.

State Implementation of Gun-Free Zones

As of January 1, 2024, there are a multitude of state laws concerning gun-free zones and who may designate them as gun free, and this is an area of law that is actively being contested. Following the Supreme Court's 2022 *New York State Rifle & Pistol Association, Inc. v. Bruen* decision, which declared New York state's concealed-carry permitting system to be unconstitutional, New York and other states passed legislation to expand the class of *sensitive places* where concealed carry would be prohibited. In New York, these new gun-free zones included private property, churches, public parks, bars, Times Square in New York City, and public transportation (New York State Concealed Carry Improvement Act).[2] In December 2023,

[2] N.Y. Penal L. § 265.01-e.

a panel of the Second Circuit Court of Appeals affirmed the constitutionality of some of New York's expanded classes of sensitive places in a ruling that may yet be appealed to the Supreme Court (Bromwich, 2023).

A large majority of states have enacted laws banning firearms or concealed firearms in some sensitive places, such as state court buildings,[3] subject to various caveats,[4] and seven states have prohibitions related to the general access areas of airports.[5] 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.[6] (See Giffords Law Center to Prevent Gun Violence, undated-i.)

---

[3] Alabama, Alaska, Arkansas, California, Colorado, Florida, Georgia, Idaho, Illinois, Kansas, Kentucky, Louisiana, Maine, Michigan, Minnesota, Mississippi, Missouri, Nebraska, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming. See Ala. Code 1975 § 13A-11-61.2; Alaska Stat. Ann. § 11.61.220; Ark. Code Ann. § 5-73-122; Calif. Penal Code § 171b; Colo. Rev. Stat. § 18-12-214; Fla. Stat. Ann. § 790.06; Ga. Stat. Ann. § 16-11-127; Ida. Code Ann. § 18-3302C; 430 Ill. CS 66/65; Kan. Stat. Ann. § 21-6309; Ky. Rev. Stat. Ann. § 237.110; La. Stat. Ann. § 1379.3; 17 Me. Rev. Stat. Ann. § 1058; Administrative Order of the Michigan Supreme Court 2001-1; Minn. Stat. Ann. § 609.66; Miss. Ann. Code § 45-9-101; Mo. Stat. Ann. § 571.030; Neb. Rev. Stat. § 69-2441; Nev. Rev. Stat. Ann. § 202.3673; N.H. Rev. Stat. § 159:19; N.M. Stat. Ann. § 29-19-11; N.C. Gen. Stat. Ann. § 14-269.4; N.D. Cent. Code 62.1-02-05; Ohio Rev. Code Ann. § 2923.123; Okla. Stat. Ann. § 1272; Ore. Rev. Stat. Ann. § 166.370; 18 Pa. Cons. Stat. § 91; R.I. Supreme Court Executive Order 2003-6; S.C. Ann. Code § 23-31-215; S.D. Code Laws § 22-14-23; Tenn. Code Ann. § 39-17-1306; Tex. Penal Code § 46.03; Utah Code Ann. § 78A-2-203; 18 Vt. Stat. Ann. § 4016; Va. Code Ann. § 18.2-283.1; Wash. Rev. Code Ann. § 9.41.300; W. Va. Code § 61-7-11a; Wisc. Stat. Ann. § 175.60; Wyo. Stat. § 6-5-209.

[4] 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).

[5] Illinois and Virginia prohibit concealed carry and open carry (430 Ill. Comp. Stat. 66/65(a)(19); Va. Code Ann. § 18.2-287.01). Nevada and Michigan prohibit concealed carry altogether, while Arkansas and Mississippi have exceptions for enhanced concealed-carry permits (Nev. Rev. Stat. 202.3673.1; Mich. Comp. Laws Ann. 259.80f; Ark. Code Ann. § 5-73-306; Miss. Code Ann. §§ 45-9-101(13), 97-37-7(2)). Georgia also prohibits carrying a firearm in an airport terminal but has an exception for license holders (Ga. Code Ann. § 16-12-127).

[6] 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 for each. 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).

Many states prohibit guns in schools that serve students in 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 20 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,[7] although some of these states still prohibit, or allow schools to prohibit, guns in particular locations on campus.[8] Idaho removed the authority of the governing bodies of colleges or universities to regulate or prohibit gun possession on campus.[9] Tennessee allows nonstudents to carry concealed weapons on campus.[10] In Colorado, the courts found that only the state's General Assembly can regulate firearm possession on any college campus, and according to statute, concealed weapons are allowed on campus; schools may regulate but not ban guns.[11] Similarly, Oregon's Court of Appeals ruled that public colleges and universities may not ban weapons on campus grounds,[12] and Virginia's Attorney General similarly held that licensed individuals may not be prohibited from carrying firearms in open areas of public universities.[13] In contrast, Oklahoma granted schools and universities authority to make their own policies concerning guns on campus.[14]

Effects on Violent Crime

Research Synthesis Findings

We identified one study that met our inclusion criteria and examined the effects of gun-free zones on violent crime. Gius (2019a) evaluated the effects of state campus-carry laws, which explicitly allow the carrying of concealed firearms on college campuses by students, faculty,

---

[7] Arkansas, Georgia, Idaho, Kansas, Louisiana, Mississippi, Texas, and Utah. See Ark. Code Ann. § 5-73-322; Ga. Code § 16-11-127.1(c)(6),(20); Ida. Code Ann. § 18-3309; Kan. Stat. Ann. § 75-7c20 (concealed weapons shall not be prohibited unless adequate security measures are in place to ensure that no weapons are permitted); La. Rev. Stat. Ann. § 40:1379.3(c); Miss. Ann. Code §§ 97-37-7, 45-9-101 (advanced permit required); Tex. Code § 411.2031; Utah Code Ann. § 53B-3-103.

[8] For example, Idaho and Texas.

[9] Ida. Code Ann. § 18-3309.

[10] Tenn. Code Ann. § 39-17-1309.

[11] *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.

[12] *Ore. Firearms Educ. Found. v. Bd. of Higher Educ.*, 264 P.3d 160 (Ore. Ct. App. 2011).

[13] Op. Atty. Gen. Va. 05-078 (January 4, 2006).

[14] Okla. Stat. Ann. Tit. 21, § 1277.

and other staff. The study focused on how these laws influence rates of violent crime occurring on college campuses by looking at offense data submitted by postsecondary institutions to the U.S. Department of Education. The author aggregated these data to the state-year level, then used a log-linear specification with state and year fixed effects, as well as state-level time-varying controls, including more-general concealed-carry laws, college enrollment rates, demographics, and overall state-level violent crime rates. Findings showed uncertain associations of campus-carry laws with violent crime rates occurring on college campuses. These findings are subject to minor methodological concerns because of an unfavorable ratio of covariates to observations (less than one to eight), meaning that 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. Figure 19.1 displays the incidence rate ratio (IRR) and confidence intervals (CIs) for estimated effects associated with gun-free zones from this study.

Conclusions

One study (Gius, 2019a) found an uncertain relationship between laws allowing concealed carry of firearms on college campuses and violent crime rates occurring on campus. Therefore, we conclude that there is *inconclusive evidence for the effect of gun-free zones on violent crime.*

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:

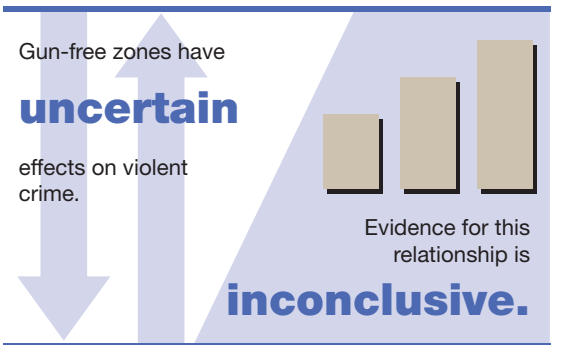
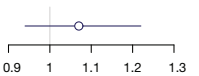


FIGURE 19.1
Incidence Rate Ratio Associated with the Effect of Gun-Free Zones on Violent Crime

Study, by Policy	Outcome Measure	IRR [95% CI]
Campus concealed carry allowed	**Violent crime rate**	
Gius (2019a)	On college campuses	1.07 [0.94, 1.22]



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

- suicide
- unintentional injuries and deaths
- mass shootings
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 19 References

Bromwich, Jonah E., "New York's Ban on Guns in Parks and Other 'Sensitive' Sites Is Affirmed," *New York Times*, December 8, 2023.

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.

Crime Prevention Research Center, "Updated: Mass Public Shootings Keep Occurring in Gun-Free Zones: 94% of Attacks Since 1950," June 15, 2018a.

Everytown for Gun Safety Support Fund, "Mass Shootings in the United States: 2009–2016," April 11, 2017b.

Giffords Law Center to Prevent Gun Violence, "Guns in Schools," webpage, undated-d. As of October 18, 2017:
https://lawcenter.giffords.org/gun-laws/policy-areas/guns-in-public/guns-in-schools

Giffords Law Center to Prevent Gun Violence, "Location Restrictions," webpage, undated-i. As of January 27, 2020:
https://lawcenter.giffords.org/gun-laws/policy-areas/guns-in-public/location-restrictions

Gius, Mark, "Campus Crime and Concealed Carry Laws: Is Arming Students the Answer?" *Social Science Journal*, Vol. 56, No. 1, March 2019a.

Jones, Wehmah, Jennifer Sable, and Jiayi Li, *State Report on the Implementation of the Gun-Free Schools Act: U.S. States and Other Jurisdictions, 2020–21 School Year*, National Center on Safe Supportive Learning Environments, U.S. Department of Education, March 2023.

New York State Rifle and Pistol Association v. Bruen, 597 U.S. ___, 2022.

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.

Willinger, Andrew, "New York's Response to Bruen: The Outer Limits of the 'Sensitive Places' Doctrine," Duke Center for Firearms Law, blog, July 13, 2022. As of August 23, 2022:
https://firearmslaw.duke.edu/2022/07/new-yorks-response-to-bruen-the-outer-limits-of-the-sensitive-places-doctrine/

CHAPTER 20

Laws Allowing Armed Staff in K–12 Schools

Mass school shootings, such as the one in Uvalde, Texas, that took the lives of 19 students and two teachers in May 2022, are rare in comparison with other types of gun violence in the United States, but they have resulted in an active policy debate and legislation concerning the role of firearms in ensuring school safety (Hurst, 2022). For instance, the Marjory Stoneman Douglas High School Public Safety Commission (2019) recommended placing an armed school resource officer in every middle school and high school in Florida and expanding a program allowing trained teachers to carry concealed weapons. Officials made similar recommendations 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).

Survey evidence from 2022 indicates that only one-fifth of a nationally representative sample of 973 primary and secondary school teachers in the United States believe that allowing teachers to carry firearms would make schools safer, and more than half believe it would make schools less safe (Jackson et al., 2023), although this support varies depending on teacher gender, race, and confidence in other emergency plans in the school (Comer, Connolly, and Fuller, 2023; Jackson et al., 2023). A large survey of high school students found that about half of students opposed policies that would allow teachers and school staff to receive special training to carry firearms in school buildings; more than one-third of students reported that their school would be less safe if their teachers were armed (Croft, Moore, and Guffy, 2019). The general population is similarly split on whether they support the arming of teachers and nonteaching staff (Jonson et al., 2021), although support may have increased in recent years (Morning Consult and Politico, 2022). There is more agreement among the general population that armed law enforcement officers who are based in schools (known as *school resource officers*) will improve safety: 70 percent of the general population support armed staff in this position (Jonson et al., 2021).

As discussed in Chapter 19 (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 per-

mits 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 favor arming either teachers or school resource officers contend that without guns, teachers or other staff have limited countermeasures available to them if they are confronted with a shooter. School staff can run or hide, but actively trying to stop a shooter *without* a gun is unacceptably risky. In addition, with more armed adults in school buildings, effective response may occur more quickly. During the 2018 shooting at Marjory Stoneman Douglas High School in Parkland, Florida, for instance, a school resource officer reached the school building that was under attack within 99 seconds of the first shot being fired, but by then, 21 people had already been shot, nine fatally (Marjory Stoneman Douglas High School Public Safety Commission, 2019). During the 2022 shooting at Robb Elementary School in Uvalde, Texas, during which law enforcement waited more than an hour to mount a response, reports indicate that the shooter fired more than 100 rounds within the first three minutes of the attack (Texas House of Representatives, 2022). If armed teachers or school staff can provide a timelier and effective response, then shootings in schools with armed personnel may result in fewer injuries or fatalities. Furthermore, 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. For instance, the Associated Press reported that there were more than 30 incidents of negligent discharge or loss of control of a weapon at schools between 2014 and 2018 involving a teacher or law enforcement officer (Foley and Fenn, 2018). 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 to school have decreased. In 1993, nearly 12 percent of students in grades 9–12 carried a weapon on school property; in 2021, just over 3 percent reported carrying a weapon on school property (Centers for Disease Control and Prevention, undated-h). A 2019 report shared evidence

that suggests that schools have experienced decreasing levels of serious violent crime over the past few decades (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., 2019). From 2013 to 2018, 40 students in grades K–12 were fatally shot by an assailant, which corresponds with a rate of less than one in 1 million enrolled students (Flannery et al., 2021). 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 that change may, therefore, be difficult to reliably detect.

Still, given the substantial and far-reaching harms of school shootings, even small reductions in risk can yield large benefits. Research suggests that even indirect exposure to a school shooting can be associated with short-term mental distress (Lowe and Galea, 2017; Rossin-Slater et al., 2020). Those who are directly exposed to a school shooting experience increased risk of developing psychological disorders (Lowe and Galea, 2017), negative effects on educational and labor-market outcomes (Cabral et al., 2024), and potentially elevated risk of premature mortality (Levine and McKnight, 2020a).

It is unclear how policies increasing the number of authorized guns in schools would affect the gun industry. Jackson et al. (2023), using results from a 2022 survey, estimated that 550,000 of the 3 million K–12 teachers in the United States would carry firearms at school if allowed. Therefore, 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.

Whether arming teachers and school resource officers leads to net harms or benefits is an empirical question that potentially could be addressed with strong scientific research designs or observational studies. 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 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 at 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.

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). Moreover, even

when school systems are authorized or required to implement programs arming teachers, such programs have sometimes been delayed or derailed by difficulty obtaining or retaining school liability insurance (Frankel, 2018).

State Implementation of Laws Allowing Armed Staff in K–12 Schools

As of January 1, 2024, at least 28 states allow schools to arm teachers or staff (not just trained guards or peace officers) in at least some cases or as part of a specific program.[1] 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 Mississippi, Missouri, and Montana, have laws that do explicitly authorize teachers or other staff to be armed.[2] 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.[3] In distressed rural counties in Tennessee, schools may implement policies allowing the selection of certain

[1] Alabama, Arkansas, Colorado, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, New Hampshire, New Jersey, North Dakota, Ohio, Oklahoma, Oregon, South Dakota, Tennessee, Texas, Utah, Washington, and Wyoming. See Ivey, 2018; Crime Prevention Research Center, 2018b; Hernandez, 2018; Fla. Stat. § 1006.12; Ga. Code Ann. § 20-8-5; Hawaii Rev. Stat. § 302A-1134(b); Ida. Code Ann. § 18-3302D; Ind. Code Ann. § 35-47-9-1(a); Ia. Code § 724.4B; Kan. Stat. Ann. § 75-7c10; Mass. Gen. Laws Ann. Ch. 269, § 10; Mich. Comp. Laws Ann. § 750.237a; Minn. Stat. § 609.66 subdivision 1d, item 8; Miss. Code Ann. § 97-37-17; Mo. Ann. Stat. § 571.107; Mont. Code Ann. § 45-8-361; N.H. Rev. Stat. Ann. § 193:13; N.J. Stat. Ann. 2C:39-5; N.D. Cent. Code § 62.1-02-14; Ohio Rev. Code § 2923.122(B); 21 Okla. Stat. Ann. § 1277; Ore. Rev. Stat. Ann. § 166.370; S.D. Codified Laws § 13-64-1; Tenn. Code Ann. § 49-6-816; Tex. Educ. Code Ann. § 37.0811; Utah Code Ann. § 76-10-505.5; Knicely, 2018; Wyo. Stat. Ann. § 21-3-132.

[2] Miss. Code Section 45-9-181 (4) (7)(e); Mo. Ann. Stat. § 571.107; Mont. Code Ann. § 45-8-361.

[3] Tex. Educ. Code Ann. § 37.0811.

employees to carry concealed weapons.[4] Several states, including Colorado, Montana, and Ohio, allow armed teachers if the school district or charter school allows it.[5] Other states, such as Indiana, allow individuals (including teachers) who have been specifically authorized by the school board to carry firearms on school property.[6] Five states allow any individual with a concealed-carry permit to carry a gun into a K–12 school,[7] and Wyoming allows school employees with such permits to carry a gun on school grounds.[8] Relatedly, 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.[9]

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
- police shootings
- defensive gun use
- hunting and recreation
- gun industry.

Chapter 20 References

Brenan, Megan, "Most U.S. Teachers Oppose Carrying Guns in Schools," *Gallup*, March 16, 2018.

[4] Tenn. Code Ann. § 49-6-816.

[5] Hernandez, 2018; Mont. Code Ann. § 45-8-361; Ohio Rev. Code § 2923.122(B).

[6] Ind. Code Ann. § 35-47-9-1(a).

[7] Alaska, New Hampshire, Oregon, Rhode Island, and Utah. See Alaska Stat. §§ 18.65.755, 11.61.210; N.H. Rev. Stat. Ann. §§ 193-D:1, 193:13; Ore. Rev. Stat. § 166.370; R.I. Gen. Laws § 11-47-60; Utah Code Ann. § 76-10-505.5.

[8] Wyo. Stat. 21-3-132.

[9] Ida. Code Ann. § 18-3302D(g); Miss. Code. Ann. § 97-37-17.

Cabral, Marika, Bokyung Kim, Maya Rossin-Slater, Molly Schnell, and Hannes Schwandt, "Trauma at School: The Impacts of Shootings on Students' Human Capital and Economic Outcomes," Working Paper No. 28311, National Bureau of Economic Research, May 2022. As of April 15, 2024:
https://www.nber.org/papers/w28311

Cai, Weiyi, and Jugal K. Patel, "A Half-Century of School Shootings Like Columbine, Sandy Hook and Parkland," *New York Times*, May 11, 2019.

Centers for Disease Control and Prevention, "1993 and 2021 Youth Risk Behavior Survey Data," data set, undated-h. As of April 9, 2024:
https://www.cdc.gov/yrbs

Comer, Benjamin P., Eric J. Connolly, and Matthew B. Fuller, "An Examination of Texas K-12 Teachers' Opinions on Teacher and Staff Gun Carrying," *Journal of School Violence*, November 2023.

Crime Prevention Research Center, "States That Allow Teachers and School Staff to Carry Guns," October 9, 2018b.

Croft, Michelle, Raeal Moore, and Gretchen Guffy, *Creating Safe Schools: Examining Student Perceptions of Their Physical Safety at School*, ACT Center for Equity in Learning, August 2019.

Everytown for Gun Safety Support Fund, National Education Association, and American Federation of Teachers, *Keeping Our Schools Safe: A Plan to Stop Mass Shootings and End Gun Violence in American Schools*, February 11, 2019.

Federal Commission on School Safety, *Final Report of the Federal Commission on School Safety*, U.S. Department of Education, U.S. Department of Justice, U.S. Department of Health and Human Services, and U.S. Department of Homeland Security, December 18, 2018.

Flannery, Daniel J., James Alan Fox, Lacey Wallace, Edward Mulvey, and William Modzeleski, "Guns, School Shooters, and School Safety: What We Know and Directions for Change," *School Psychology Review*, Vol. 50, Nos. 2–3, 2021.

Foley, Ryan J., and Larry Fenn, "Correction: Guns in School-Accidents Story," Associated Press, May 7, 2018.

Fowler, Katherine A., Linda L. Dahlberg, Tadesse Haileyesus, Carmen Gutierrez, and Sarah Bacon, "Childhood Firearm Injuries in the United States," *Pediatrics*, Vol. 140, No. 1, 2017.

Frankel, Todd C., "One Roadblock to Arming Teachers: Insurance Companies," *Washington Post*, May 26, 2018.

Hernandez, Elizabeth, "At Least 30 Colorado School Districts and Charter Schools Allow Teachers to Carry Guns, but No Statewide Training Standards Regulate Them," *Denver Post*, July 8, 2018.

Hurst, Kiley, "About a Third of K–12 Parents Are Very or Extremely Worried a Shooting Could Happen at Their Children's School," Pew Research Center, October 18, 2022

Ivey, Kay, Governor, "Memorandum: Implementation of the Alabama Sentry Program and the School Safety Training and Compliance Teams," State of Alabama, May 30, 2018.

Jackson, Brian A., Melissa Kay Diliberti, Pauline Moore, and Heather L. Schwartz, *Teachers' Views on School Safety: Consensus on Many Security Measures, But Stark Division About Arming Teachers*, RAND Corporation, RR-A2641-1, 2023. As of April 3, 2024:
https://www.rand.org/pubs/research_reports/RRA2641-1.html

Jonson, Cheryl Lero, Alexander L. Burton, Francis T. Cullen, Justin T. Pickett, and Velmer S. Burton, Jr., "An Apple in One Hand, a Gun in the Other: Public Support for Arming Our Nation's Schools," *Criminology and Public Policy*, Vol. 20, No. 2, May 2021.

Keller, Thomas Christoph, "ABC's and AR-15's: Arming Arkansas's Teachers," *Arkansas Law Review*, Vol. 67, No. 3, 2014.

Knicely, John, "One Washington School District Already Has Staff Armed with Guns," KIRO 7, February 23, 2018.

Levine, Phillip B., and Robin McKnight, "Exposure to a School Shooting and Subsequent Well-Being," Working Paper No. 28307, National Bureau of Economic Research, December 2020a.

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," Crime Prevention Research Center, April 25, 2019.

Lowe, Sarah R., and Sandro Galea, "The Mental Health Consequences of Mass Shootings," *Trauma, Violence, and Abuse*, Vol. 18, No. 1, 2017.

Marjory Stoneman Douglas High School Public Safety Commission, *Initial Report Submitted to the Governor, Speaker of the House of Representatives and Senate President*, January 2, 2019.

Morning Consult and Politico, *National Tracking Poll*, No. 2205143, May 25, 2022.

Musu, Lauren, Anlan Zhang, Ke Wang, Jizhi Zhang, and Barbara A. Oudekerk, *Indicators of School Crime and Safety: 2018*, National Center for Education Statistics, NCES 2019-047, NCJ 252571, April 2019.

Office of Juvenile Justice and Delinquency Prevention, "Offending by Juveniles," *OJJDP Statistical Briefing Book*, September 21, 2020. As of August 23, 2022: https://www.ojjdp.gov/ojstatbb/offenders/qa03505.asp?qaDate=2019&text=yes&print=yes&maplink=link2

Reeping, Paul M., Ariana N. Gobaud, Charles C. Branas, and Sonali Rajan, "K–12 School Shootings: Implications for Policy, Prevention, and Child Well-Being," *Pediatric Clinics of North America*, Vol. 68, No. 2, April 2021.

Richmond, Matthew, "With No National Standards, Policies for Arming Teachers Are Often Left to Local School Districts," Guns & America, March 22, 2019.

Rossin-Slater, Maya, Molly Schnell, Hannes Schwandt, Sam Trejo, and Lindsey Uniat, "Local Exposure to School Shootings and Youth Antidepressant Use," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 38, September 22, 2020.

Rostker, Bernard D., Lawrence M. Hanser, William M. Hix, Carl Jensen, Andrew R. Morral, Greg Ridgeway, and Terry L. Schell, *Evaluation of the New York City Police Department Firearm Training and Firearm-Discharge Review Process*, RAND Corporation, MG-717-NYPD, 2008. As of October 6, 2019: https://www.rand.org/pubs/monographs/MG717.html

Texas House of Representatives, *Investigative Committee on the Robb Elementary Shooting: Texas House of Representatives Interim Report 2022*, July 17, 2022.

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

United States Code, Title 20, Section 7961, Gun-Free Schools Act.

Vince, Joseph J., Timothy Wolfe, and Layton Field, *Firearms Training and Self-Defense: Does the Quality and Frequency of Training Determine the Realistic Use of Firearms by Citizens for Self-Defense?* National Gun Victims Action Council, 2015.

PART E

Summary of Findings and Recommendations

CHAPTER 21

Summary and Conclusions

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 existing scientific knowledge can tell the public and policymakers about the true effects of many gun policies that are frequently discussed in state legislatures.

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.

With this fourth edition of our review of empirical evidence for the effects of gun laws, we have attempted to address such concerns through the rigor and transparency of our methods and through our organizational commitment to nonpartisan, objective policy analysis. For this updated edition, we extended our prior review by systematically reviewing all new empirical research published between our last search (October 2020) and February 28, 2023. Specifically, we sought new research that examined the effects of 18 types of state gun policies on eight outcomes, including those related to public health and safety and those of interest to sport shooters, hunters, and gun industry participants. As in prior versions of this report, 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 30 new studies meeting our inclusion criteria (described in Chapter 2). With the 152 studies included in the last edition of this review, this report now summarizes the findings

of and draws conclusions from the 182 studies that met our inclusion criteria, of which 149 were not superseded by other later studies by the same authors. For a comparison of changes in our strength of evidence ratings between the third and fourth editions of this report, see Appendix A.

In this chapter, we summarize our judgments about the current 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 limitations of the analytic methods, the available data, or the potential policy variation 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).

Therefore, our ratings 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 evi-

dence that cigarette smoking causes cancer is vastly stronger than the evidence concerning any gun policy's effect on any outcome.

Additionally, our ratings reflect the relative strength of evidence within a particular outcome domain. For example, a designation of limited evidence of the effects of a given policy on suicide may represent a stronger body of evidence than a designation of limited evidence of the effects of that same policy on mass shootings. In particular, because our strength-of-evidence ratings rely on a combination of criteria that include statistical significance of estimated effects, we are implicitly treating *p*-values as though they reliably reflect the strength of the evidence against the null hypothesis of zero policy effect. Setting aside issues with using sharp cutoffs for *p*-values in our designation of suggestive or significant effects, this relationship breaks down if statistical power is low (Halsey et al., 2015). Thus, our strength-of-evidence ratings for sparser outcomes (e.g., mass shootings) in particular should be interpreted cautiously.

Table 21.1 summarizes our evidence ratings for all 18 classes and subclasses of policies across the eight outcomes and their subclasses. Several outcomes show multiple judgments, and these correspond to different characterizations of the specific policy-outcome association. For instance, we identified moderate evidence that laws that establish firearm prohibitions for individuals who are subject to domestic violence restraining orders decrease firearm-related intimate partner homicides and inconclusive evidence for how firearm prohibitions for those convicted of stalking or misdemeanor domestic violence affect this outcome. 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 (CAP) 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).

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. We find supportive evidence, our highest evidence rating, that CAP laws, or safe-storage laws, reduce self-inflicted fatal or nonfatal firearm injuries, unintentional

TABLE 21.1

Strength of Evidence Across Gun Policies and Outcomes

Outcome	Minimum Age Requirements		Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks		Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws		Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
	Purchasing	Possessing					Dealer	Dealer and Private										Shall Issue	Permitless Carry		
Suicide																					
Total suicides	↓L[a]	I	I	I		↓L	I	I	I	↓L				I	I	I	↓M, I[b]	I	I		
Firearm suicides	↓S[c]	↓L[c]	I	I	I	↓L	I	I	I	↓M			I	I	I	I	↓S, I[b]	I	I		
Violent crime			↓L		I		I									↑L	I	↑S		I	
Total homicides	I	I	I	↓L	I	I	I	↓M	I	↓M	I			I	I	↑S	I	↑S	I		
Firearm homicides	I	I	I	↓L	I	I	↓M	↓L	I	↓L			I	I	I	↑S	↓S[d]	↑S	I		
Intimate partner homicides				↓M, ↑L, I[e]	I, ↓L[f]					I											
Robberies		I		I	I											I	I	↑L			
Assaults		I		I	I											I	I[d]	↑M			
Rapes		I		I	I												I	I			

Table 21.1—Continued

Outcome	Minimum Age Requirements		Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks		Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Reporting, Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws		Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
	Purchasing	Possessing					Dealer	Dealer and Private										Shall Issue	Permitless Carry		
Unintentional injuries and deaths		I																I			
Unintentional firearm injuries and deaths		I				I											↓L, ↓S[g]	I			
Mass shootings	I			I	I	I	I	I	↓L	I	I		I	I, ↓L[h]		I	I	I	I		
Police shootings								I						I					↑L		
Defensive gun use																I					
Hunting and recreation																					
Gun industry																					
Firearm ownership										I						I		I			
Firearm manufacturers or retailers							I		I	I				I			I				

Table 21.1—Continued

Outcome	Minimum Age Requirements		Prohibitions Associated with Mental Illness	Prohibitions Associated with Domestic Violence	Surrender of Firearms by Prohibited Possessors	Extreme-Risk Protection Orders	Background Checks		Licensing and Permitting Requirements	Waiting Periods	Firearm Safety Training Requirements	Lost or Stolen Firearm Reporting Requirements	Firearm Sales Reporting, Recording, and Registration Requirements	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Bans on Low-Quality Handguns	Stand-Your-Ground Laws	Child-Access Prevention Laws	Concealed-Carry Laws		Gun-Free Zones	Laws Allowing Armed Staff in K–12 Schools
	Purchasing	Possessing					Dealer	Dealer and Private										Shall Issue	Permitless Carry		
Firearm purchases									I	I	I		I		I	I		I[i]	I		
Prices or sales of banned firearms in the short term														↑L							

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

[a] We concluded that there is limited evidence that higher minimum age requirements for purchasing a handgun may reduce suicides among young adults.

[b] We found moderate evidence for reductions in total suicides among youths, and we found supportive evidence for reductions in firearm suicides and firearm self-injuries among youths. Evidence for effects on outcomes among the full population was inconclusive.

[c] We concluded that there is moderate evidence that minimum age requirements for purchasing a firearm decrease firearm suicides among young people and limited evidence that minimum age requirements for possessing a firearm decrease firearm suicides among young people.

[d] We found supportive evidence that child-access prevention laws reduce firearm homicides or firearm assault injuries among young people. Evidence for effects on outcomes among the overall population was inconclusive.

[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 misdemeanor domestic violence affect total and firearm-related intimate partner homicides.

[f] We found inconclusive evidence for the effects of firearm-surrender laws alone but moderate evidence that firearm-surrender laws, when paired with expansions of prohibited-possessor classes, reduce firearm-involved intimate partner homicides. We found limited evidence that firearm-surrender laws, when paired with expansions of prohibited-possessor classes, reduce all intimate partner homicides.

[g] We concluded that there is supportive evidence that child-access prevention laws decrease unintentional firearm injuries and deaths among children and limited evidence that they decrease such injuries and deaths among adults.

[h] We found inconclusive evidence for the effects of assault weapon bans on mass shootings and their fatalities and limited evidence that bans on high-capacity magazines reduce mass shootings and fatalities.

[i] We found inconclusive evidence for the effects of may-issue, shall-issue, and permitless-carry laws on firearm purchases.

firearm injuries and deaths, and firearm homicides among youth. There is also moderate evidence that CAP laws reduce firearm suicides among young people, and limited evidence that such laws reduce unintentional firearm injuries among adults. The evidence is stronger for negligent-storage laws than for reckless endangerment laws; reckless endangerment laws are sometimes considered a weaker form of CAP law.

Across all 18 classes of policies that we studied, only four (CAP laws, shall-issue concealed-carry laws, stand-your-ground laws, and minimum age requirements for purchasing a weapon) had any evidence of effects that we classified as *supportive* for a particular conclusion. Three of these laws (CAP laws, shall-issue concealed-carry laws, and stand-your-ground laws) differ from many of the other policies we considered in this report that mainly affect the acquisition of new firearms (e.g., background checks or waiting periods) or that 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. In theory, CAP laws, as well as other laws that regulate the legal uses of firearms by all gun owners, may well affect larger numbers of firearm owners and thus could potentially have larger population-level effects.

However, the mechanisms by which CAP laws produce effects are unclear. CAP laws, as well as other types of safe-storage requirements, are designed to influence how all guns in a state are required to be stored when children might be expected to encounter them. This likely represents a large proportion of all guns, because about 40 percent of all households in the United States have children under age 18 (U.S. Census Bureau, 2023); an estimated 40 percent of adults living with children under age 18 have firearms in their households (Miller and Azrael, 2022), and many more likely have children as occasional visitors. Because such large numbers of guns are potentially affected, even imperfect knowledge of or compliance with CAP laws may have a greater chance than other types of laws of producing observable effects in population-level outcome statistics.

However, the extent to which CAP and other safe-storage laws affect the acquisition of firearm safety devices and firearm storage behavior is not well understood, and there are other potential mechanisms by which safe-storage laws may reduce firearm injuries and deaths among youths. Changes in adult behaviors that reduce youth access to firearms or changes in social norms around how firearms are handled could plausibly reduce firearm suicides and unintentional injuries. With respect to firearm homicides, effects may also or alternatively arise if safe-storage practices required by CAP laws reduce the incidence of firearms being stolen and entering criminal firearm markets. It could also be that the time it takes to access and load a properly secured firearm may suffice to deter routine use of firearms in situations that could escalate to homicide. Other mechanisms may also explain this relationship. If research continues to support a causal relationship between CAP laws and firearm homicides, further work assessing underlying mechanisms driving this relationship may help with designing and implementing more-effective policies.

- *Recommendation 1.* States without negligent-storage CAP laws should consider adopting them or other safe-storage laws as a strategy to reduce total and firearm suicides, unintentional firearm injuries and deaths, and firearm homicides among youth.

Gun industry and gun-owner organizations have promoted voluntary and educational programs to encourage the safe storage of firearms. Our conclusions and recommendations should not be interpreted to suggest that only CAP and safe-storage 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. Similarly, there is some 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, although the extent to which this translates into reductions in child injury or access to firearms is unknown (Ramchand, 2022). There is a need to conduct more-robust methodological research to inform whether other types of strategies for improving safe storage (e.g., school programs, safety lock requirements) are effective in changing storage and use behaviors, access to firearms, and injury and crime or victimization outcomes.

- *Recommendation 2*. States implementing CAP or other safe-storage laws should support data collection and investigation of the mechanisms by which CAP laws affect injury outcomes, for example, by investigating effects on gun ownership, storage, and thefts.

Conclusion 2. There is supportive evidence that stand-your-ground laws are associated with increases in firearm homicides and in total homicides.

Multiple studies that met our inclusion criteria and are of high methodological quality have been published and replicated these findings; all of these studies either find that stand-your-ground laws elevate homicide rates or estimate uncertain (and generally positive) relationships. 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. Among studies of high methodological quality or with more-minor methodological concerns, the suggestive or significant incidence rate ratios detected for total and firearm homicide rates range from 1.07 to 1.10, which are effects too large to be explained by justifiable homicides given that Federal Bureau of Investigation (FBI) estimates suggest that fewer than 3 percent of firearm homicides by private citizens are determined justifiable (FBI, 2020b, 2020c).

More than two-thirds of all states currently have stand-your-ground laws. The fact that there is supportive evidence that these laws increase 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 evi-

dence, these laws actually are making people less safe and instead more likely to be the victims of a homicide.

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

Conclusion 3. There is supportive evidence that shall-issue laws increase total homicides, firearm homicides, and overall violent crime. There is also moderate evidence that shall-issue laws lead to an increase in rates of assaults and limited evidence that they increase rates of robbery. Finally, there is limited evidence that permitless-carry laws lead to increases in fatal police shootings. Permitless-carry laws are, arguably, a form of shall-issue law, and nearly all research that compares may-issue laws with shall-issue laws categorizes permitless-carry states as shall-issue states. Thus, we interpret this pattern of findings as meaning that although evidence for permitless-carry laws per se is limited, the more general effects of shall-issue laws are likely to apply to states with permitless-carry policies.

Since the publication of the previous edition of this review, several new studies have bolstered prior findings that shall-issue laws are associated with increased rates of overall homicide and firearm homicide. Additionally, several new studies have adopted more-rigorous methods to better understand the relationship of shall-issue laws with nonfatal forms of violent crime, and these more-recent studies have near-unanimously found suggestive or significant effects consistent with shall-issue laws leading to higher rates of assaults or robberies. There is evidence to suggest that elevated rates of violent crime increase concealed-carry license applications and issuances (Depew and Swensen, 2019; Stansfield, Semenza, and Silver, 2023), which creates concerns about a reverse relationship in which it is violent crime that causes changes in concealed-carry policy. However, the weight of the evidence supports that this is not driving results and that moving from a may-issue regime to a shall-issue concealed-carry regime leads to increased violent crime and homicides. Our understanding of the effects of permitless-carry laws, which have been more recently adopted, is largely inconclusive, although we now have limited evidence pointing toward increased rates of fatal police shootings following the transition from a shall-issue regime to a permitless-carry regime.

These conclusions may be particularly concerning in light of the 2022 Supreme Court decision in *New York State Rifle and Pistol Association v. Bruen* that would seem to preclude the ability of states to adopt or sustain may-issue concealed-carry laws. If may-issue concealed-carry laws are no longer constitutional, states or local jurisdictions may need to use other strategies to try to avert potential increases in violent crime that are associated with the transition to a shall-issue regime. Scoping these new policy levers would benefit from a better understanding of the mechanisms by which concealed-carry laws affect violence. The association of shall-issue laws with violent crimes appears not to be attributable to crimes committed by properly permitted gun users, as evidence generally shows that, as a group, license holders are particularly law abiding and rarely are convicted for violent crimes (see,

e.g., Texas Department of Public Safety, 2022; Minnesota Department of Public Safety, 2022). However, even if license holders themselves are not responsible for criminal violence, widespread behavioral changes associated with permissive concealed-carry laws may have unintended consequences that could explain the association of these laws with increased rates of violence. For instance, it could be that as more people carry firearms, there are more opportunities for these firearms to be stolen and thereafter enter criminal firearm markets (e.g., see Donohue et al., 2023, and Billings, 2023). Conceivably, as more law-abiding citizens carry weapons, others who may be higher-risk and prohibited from possessing a firearm become more likely to carry guns illegally. Changes in law enforcement's role in the permitting process and changes in carrying behavior may also shape police behaviors in ways that influence crime (Donohue et al., 2023). These are candidate theories that may or may not explain the observed effects of these laws, and further research is needed to understand the true mechanisms driving these effects.

- *Recommendation 4.* States with shall-issue (including permitless-carry) laws should consider whether other regulations, either through requirements implemented as part of the permitting process or as through other aspects of law (e.g., laws that may prevent firearm theft), might ensure the effects of concealed-carry laws are aligned with public safety.

Conclusion 4. There is supportive evidence that increasing the minimum age required to purchase a firearm above the threshold set by federal law can reduce firearm suicides among young people. There is also limited evidence that higher minimum age requirements for purchasing or possessing a firearm may reduce total suicides among young adults and firearm suicides among young people.

Federal law prohibits the sale or transfer of handguns or handgun ammunition to persons under age 18, which is the minimum age for possession of handguns, and the law prohibits licensed dealers from selling or otherwise transferring handguns or handgun ammunition to those under age 21. For long guns and ammunition for long guns, federal law sets no age limit on private sales, and the law requires buyers to be aged 18 or older to purchase such guns or ammunition from dealers. Many states have laws with higher minimum age requirements than those in federal law.

Firearm suicides are especially likely in the days and weeks that follow the purchase or acquisition of a firearm (Studdert et al., 2020). Although federal laws restrict the age at which young people may purchase firearms from a federally licensed firearms dealer or possess handguns, many states have adopted more-stringent minimum age laws. These laws may impede access to firearms by young people at a time when they are impulsively considering harming themselves or others. By delaying access to firearms, these laws may provide some young people with time to manage an immediate crisis, with life-saving benefits. Because there is evidence that the relationship between impulsivity and suicidal behavior may be more pronounced at younger ages (McGirr et al., 2008), barriers to lethal means may have particu-

lar importance for influencing suicide outcomes among youth and young adults (Donohue, Cai, and Ravi, 2023).

As with CAP laws, for which we found supportive evidence of reductions in interpersonal and self-directed violence among youth, our designation of supportive evidence for the effects of minimum age requirements *covers such requirements' effects on outcomes among young people rather than across the full population.* While it is reasonable for a research design to focus on the narrow age band that is affected by a change in minimum age requirements (which typically increase or decrease the age requirement by only a few years), the implications of these more-targeted laws for producing benefits at the population level may be more limited. That said, suicide rates have risen dramatically among young people in the past decade (Centers for Disease Control and Prevention [CDC], undated-b); thus efforts to effectively mitigate these trends are needed.

- *Recommendation 5:* States should consider raising the minimum age to purchase firearms and ammunition above the level of federal requirements as a strategy to reduce firearm suicides among youths.

Conclusion 5. 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, and there is limited evidence that laws prescribing a mechanism for ensuring the surrender of firearms by prohibited possessors, when combined with laws expanding the class of prohibited possessors, reduce intimate partner homicides.

State domestic violence protection orders are often broader than 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. Half of all women who died by homicide in the United States in 2016 and for whom relationship with the perpetrator was known were killed by a current or former intimate partner (Ertl et al., 2019). Of the 4,947 women who died by homicide in the United States in 2021, 69 percent were killed with firearms (CDC, undated-e). In this context, 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 6.* 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, when the law ensures that information about the cases is included in databases used to conduct background checks, and when the prohibitions are accompanied by firearm surrender requirements.

Conclusion 6. There is moderate evidence that dealer background checks reduce firearm homicides, moderate evidence that background checks on both dealer and private sales reduce total homicides, and limited evidence that background checks on private sales reduce firearm homicides.

- *Recommendation 7.* States that do not require background checks for the private sale or transfer of firearms should consider mandating such checks as a strategy for reducing total and firearm homicide rates.

Conclusion 7. 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 8*. States without waiting-period laws should consider adopting them as a strategy for reducing suicides and homicides.

Of the 18 policies we evaluated, the three policies with the strongest evidence for reducing total and firearm suicides all involve some delayed or impeded access to firearms (minimum age of purchase or possession laws, waiting-period laws, and CAP laws). There are good reasons to suspect that these delays may be an important mechanism leading to reduced firearm suicide rates, given that (1) the risk of firearm suicide is substantially elevated in the days and weeks after a firearm is acquired (Wintemute et al., 1999; Studdert et al., 2020); (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). Finally, some studies have shown some 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, but it is challenging to draw strong conclusions from this because the evidence is based on law changes in only a few states.

Conclusion 8. There is limited evidence that extreme-risk protection order (ERPO) laws may reduce total and firearm suicides.

ERPO laws offer a mechanism for removing or impeding access to firearms by individuals who are experiencing an acute crisis and are found to be at risk of harming themselves or others, although the civil court process required for granting an ERPO means that the interrupted firearm access may not occur immediately. Similar to the aforementioned policies with stronger evidence for reducing suicides, ERPO laws may function through delaying firearm acquisition by individuals who are at high risk of self-harm. However, unlike the other

policies, ERPO laws have an additional function in that they facilitate the removal of firearms already owned by the subject of the order. Additionally, ERPO laws primarily function as a means of preventing firearm access among individuals who are legally allowed to purchase and possess firearms; thus, unlike other purchase prohibitions or requirements, these may be less effective for reducing suicides or firearm suicides among prohibited possessors (e.g., youth, people with felony records).[1] While evidence points to the potential promise of ERPO laws as a way to prevent suicides and firearm suicides, it is important to note that most of these laws have been enacted within the past decade, so the evidence base is still developing. There is also substantial variation in the ERPO process across states and jurisdictions, the extent to which petitions are being filed and granted, and the reasons for filing; thus, average policy effects as estimated in the literature may mask important variation across these dimensions. Finally, ERPOs are available to reduce risk among individuals, not broad classes of gun owners or all gun owners (as in the case of safe-storage laws), so the effects of this law may be difficult to observe at the population level.

The Bipartisan Safer Communities Act's dedicated $750 million in support to states to implement and improve crisis intervention services, including ERPO laws, may spur broader adoption and implementation of these laws. Harmonizing the types of implementation and outcome data collected and disseminated by states and local jurisdictions could support more-rigorous evaluation of the effects of these laws.

- *Recommendation 9*. States should consider regulations that delay access to firearms by those who may be in crisis. Examples of laws that may delay such access and for which there is at least moderate evidence that they reduce total or firearm suicides include minimum age of purchase and possession laws, waiting-period laws, and CAP laws.
- *Recommendation 10*. States without ERPO laws should consider adopting such laws as a potential strategy for reducing total or firearm suicides.

Conclusion 9. 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 states 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 the NICS, there is considerable variation in which classes of individuals prohibited under federal law are shared with the

[1] In theory, one could petition for an ERPO to remove a firearm from an individual who was disqualified from owning that firearm, and this should result in the removal of that firearm and a criminal charge for unlawful possession. We are not aware of studies evaluating the extent to which this occurs.

NICS. Thus, by the beginning of 2023, there were large differences in the number of active mental health records in the NICS across states; for example, Alaska, Montana, New Hampshire, and Wyoming had contributed fewer than 1,000 records (Montana and Wyoming have contributed fewer than 40), whereas most other states had tens or hundreds of thousands of active mental health records in the database (Criminal Justice Information Services Division, 2023a).

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 the 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 that make 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 the 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 11.* States that currently do not require a background check that investigates 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 NICS.

Conclusion 10. The sparseness of mass shooting data creates challenges for drawing strong inferences about policy effects because it is difficult to detect signal from noise, and low statistical power means that real policy effects are unlikely to be detected as statistically significant and significant effects are likely to be exaggerated in magnitude and may even be wrong-signed. With these caveats, we find limited evidence that permit-to-purchase requirements and bans on high-capacity magazines may reduce mass shootings and their associated fatalities.

Conclusion 11. No studies meeting our inclusion criteria have examined the effects of lost or stolen firearm reporting requirements or laws allowing armed staff in K–12 schools. Only inconclusive evidence exists for the effects of gun-free zones; firearm safety training requirements; firearm sales reporting, recording, and registration requirements; and bans on low-quality handguns.

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 with its investment in other areas of public safety and health, such as transportation safety or opioid overdoses. Researchers, particularly over the past decade, have advanced the understanding of gun violence and policy by adopting more-rigorous methodological approaches in several areas, but sustained support of high-quality research is needed to fill gaps in the evidence base.

Of the 182 studies meeting our inclusion criteria that have been published since 1995, just 25 (13 percent) reported receiving any federal or state funding, and many of these were funded in the 1990s. Private foundations provided support to 49 (27 percent) of studies. Among the foundations funding the largest number of these studies, the Robert Wood Johnson Foundation supported 18, and the Joyce Foundation supported 17. The majority of studies we reviewed (124, or 68 percent) reported no extramural funding. Of the 30 new studies incorporated in this fourth edition, 16 (53 percent) did not report any funding, seven (23 percent) reported foundation funding, and six (20 percent) reported federal funding.

While most of the 182 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. Before 1996, the federal government supported a modest program of research examining firearm violence and policy. In the early 1990s, a CDC-sponsored publication found that having a gun in the home was associated with an elevated risk of firearm homicide among members of the household, and 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 thus 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 for or promoting gun control. In 2012, similar language was added to an appropriations bill for the National Institutes of Health in the Consolidated Appropriations Act (Pub. L. 112-74), and this also seems to have had a chilling effect on federal funding for firearm research. Stark and Shah (2017), for instance, found that federal gun violence research funding from 2004 to 2014 was just 1.6 percent of the amount predicted on the basis of 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.

Fortunately, since 2019, the federal government has funded approximately $150 million in firearm violence prevention research, and the volume of research projects and scientific publications on this topic has correspondingly increased (Lin et al., 2024). In addition to the recent resumption of federal research funding, several foundations have made substantial commitments to the field over the past five or more years, such as the Joyce Foundation, the Robert Wood Johnson Foundation, and Arnold Ventures, which created the National Collaborative on Gun Violence Research (a private philanthropy managed by RAND) in 2018. Together with the new federal funds, these efforts have supported scores of new funded research projects across the country, raising hopes that new lifesaving discoveries will soon be available. This will require, however, a sustained program of research not unlike the investments the federal government has made in research on smoking cessation, HIV/AIDS, or traffic safety. Uncertainty regarding the future funding landscape, highlighted by the November 2023 House Republican vote to prohibit the CDC from funding research on the causes and prevention of gun violence (amendment to U.S. House of Representatives, 2023), threatens to block progress and slow the development of large-scale data infrastructure projects that require dedicated long-term funding.

- *Recommendation 12*. To improve the development of firearm violence prevention strategies, the federal government should commit to an ongoing program of research funding. 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 with the government's existing investments in research on other threats to public safety and health.
- *Recommendation 13*. Given uncertainty about whether federal investments in gun policy research will be sustained in future years so as to 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 violence prevention.

Conclusion 13. Rigorous research examining the effects of many state gun policies on defensive gun use, hunting and recreation, and gun industry outcomes is limited or 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 about three-quarters of U.S. gun owners (Schaeffer, 2023). The majority opinion in the *Bruen* decision by the Supreme Court specified that the availability of weapons for self-defense is a central component of the Second Amendment. However, 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 (Hemenway, Shawah, and Lites, 2022; 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 several studies reviewed in this report (e.g., Cheng and Hoekstra, 2013; Levy et al., 2020; McClellan and Tekin, 2017). Given the strength of evidence of CAP laws on self-inflicted and unintentional injuries, studying the impact of these policies on defensive gun use could help inform the trade-offs between this outcome and the potential public safety benefits. Because shall-issue concealed-carry laws and stand-your-ground laws aim to allow legal gun owners to better protect themselves or deter criminal acts—but evidence supports that such laws increase rates of violence—a more comprehensive understanding of how such laws shape defensive gun use behaviors would help inform trade-offs and could improve policy design and implementation. Triangulating FBI information on justifiable homicides with other data sources that capture aspects of defensive gun use (e.g., Gun Violence Archive, the National Crime Victimization Survey, the National Violent Death Reporting System [NVDRS], surveys of gun owners, surveys of inmates in prisons or jails) may yield a more comprehensive understanding of this outcome.

The relative dearth of research examining how policies affect the gun industry and recreational gun use is another significant shortcoming in the available scientific literature. Estimates produced by the National Shooting Sports Foundation suggest that, in 2022, there were 172,697 jobs in the United States involving the manufacture, distribution, or retailing of ammunition, firearms, and hunting supplies and potentially another 220,999 jobs in supplier and ancillary industries connected with the firearm market (National Shooting Sports Foundation, 2023). According to the U.S. Census Bureau, in 2021, approximately 36,000 people were employed in U.S. firms coded as being involved in just the manufacture of small arms, small arms ammunition, or ordnance (North American Industry Classification System codes 332992 and 332994; U.S. Census Bureau, 2023). The gun and ammunition manufacturing industry alone is estimated to generate $15.2 billion in revenue annually (U.S. Census Bureau, 2022a). Estimates suggest that more than 10 million people use firearms for hunting each year (Outdoor Foundation, undated; U.S. Fish and Wildlife Service, 2018), and more than 46 million people went target shooting with firearms in 2021 (U.S. Fish and Wildlife Service, 2023). 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 14.* 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 on how to best measure and evaluate a wider set of outcomes, including defensive gun use, hunting and sport shooting, and gun industry outcomes (e.g., sales, prices, purchasing).

Conclusion 14. The lack of data on (1) gun ownership and availability and (2) guns in legal and illegal markets severely limits researchers' ability to determine the mechanisms by which policies have their effects, understand policy implementation and enforcement, and answer basic scientific questions about the relationship between policies, firearm availability or access, and public health and safety.

There are no regularly collected national data that describe gun ownership or use at the state or substate level since the CDC suspended its collection of this information on the core module of 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 15.* 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, acquisition, and use with sample sizes sufficient to support state- or substate-level analyses.

Additionally, the federal government no longer shares data on illegal gun markets with researchers, which investigators could use to examine how policies change the availability of firearms. This is a problem that has worsened since National Research Council (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 accessing detailed information on gun trace data and gun purchaser data. When detailed 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

individuals 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 in which the guns were involved, how they were acquired, and other key details are omitted from these reports. While aggregate-level information can support some lines of inquiry about interstate crime gun flows, this information is severely limited in advancing knowledge about the flow of guns to criminal markets. For example, knowing the extent to which crime guns are traced back to a small number of federal firearms licensees (FFLs), whether those recovered guns were reported as lost or stolen by those particular FFLs, whether certain seller or jurisdictional characteristics are predictive of these outcomes, and whether these statistics vary across states and over time would help clarify the role of straw purchasing from FFLs in supplying criminal markets. 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. Combining more-detailed trace data with other ongoing or novel data collection efforts relevant to criminal firearm and ammunition markets could substantially improve understanding of how guns flow to and within illegal markets, the role of these markets and market features (e.g., price, availability, number of suppliers) in fueling violence, and effective strategies to disrupt criminal access to firearms and ammunition.

- *Recommendation 16*. To foster a more robust research program on gun policy, Congress should consider whether to eliminate or loosen the restrictions that it has imposed on the use of detailed gun trace data for research purposes.
- *Recommendation 17*. Federal agencies and private foundations should consider substantial funding for a dedicated research program to improve data infrastructure that provides information on illegal firearm markets and jurisdictional policies and practices focused on those markets.

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 NVDRS and the National Incident-Based Reporting System (NIBRS) are promising sources of data for future research, although only two studies meeting the inclusion criteria for this report used either data set.

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. For example, in the one study we identified that used the NVDRS (Rochford, Berg, and Peek-Asa, 2022), the authors restricted their sample to states with contributing data in all years of study, leading to a study time frame of only five years (2013 to 2017) and coverage of only 18 states. However, state participation in the NVDRS has been growing and, as of 2018, all 50 states and the District of Columbia were contributing at least partial data on violent deaths in their jurisdictions. 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's Summary Reporting System (SRS). Whereas the SRS collects summary or aggregate statistics on serious violent and property crimes reported to law enforcement agencies, the NIBRS was designed to collect *incident-level* information. The NIBRS has several advantages for the study of gun violence because it (1) captures a broader set of crimes than the SRS, (2) allows for the recording of firearm involvement for a broader set of crimes, (3) has a series of quality checks that may reduce incomplete or inaccurate reporting, and (4) does not impose a hierarchy rule and thus can capture crimes involving guns that occurred concomitantly with more-serious nonfirearm crimes (e.g., an incident involving theft of a firearm and a nonfirearm aggravated assault) (Parker, 2022).

As of this writing, it is not possible for state law enforcement agencies to report through NIBRS nonfatal gunshot injuries sustained by victims, offenders, or law enforcement, nor is it possible to record incidents of shots fired without any known injuries, thus limiting NIBRS's utility as a source of information on these common types of gun violence. In 2021, the FBI's Criminal Justice Information Services Division recommended to the director of the FBI that at least data on victim gunshot injuries and shots fired during an incident should be collected through the NIBRS (Roman and Cook, 2021). This recommendation was approved in August 2021 (FBI, 2022e), but as of the 2023 version of the NIBRS instruction manual and technical documentation, no such fields have been added (Criminal Justice Information Services Division, 2023a). Prioritizing the implementation of these approved changes could substantively improve the surveillance of nonfatal firearm incidents. However, because many police agen-

cies have found compliance with NIBRS recordkeeping burdensome, it remains to be seen whether sufficiently complete and accurate information on these outcomes can be collected through this process.

Indeed, because the NIBRS program is voluntary and its data submission requirements can be costly for law enforcement agencies to adopt, participation rates have been historically far below those of the SRS (Asher, 2023). Despite inadequate NIBRS participation rates, the FBI discontinued its 90-year SRS in January 2021 to transition to data collection solely through the NIBRS. Challenges with the transition prompted the FBI to allow non-transitioned agencies to submit information using the old SRS (FBI, 2023e). Thus, published crime data for 2022 used data submitted through the NIBRS by 13,293 law enforcement agencies (covering 76.5 percent of the U.S. population) and through the SRS for an additional 2,341 agencies (covering an additional 16.6 percent of the U.S. population; FBI, 2023e). Even though the NIBRS should be a superior data system once participation rates are higher, the SRS should be continued until the NIBRS can support state crime estimates. And even when participation in the NIBRS is sufficient, substantive high-quality technical and methodological work will be needed to (1) assess the quality and completeness of these data, (2) develop procedures that can account for incomplete reporting to produce reliable national and state (and ideally substate) crime estimates from the NIBRS, (3) develop methods that bridge crime information to support more-rigorous comparisons of crime estimates across the NIBRS transition period, and (4) create resources and analytic products that will facilitate researcher use of these data (e.g., see Kaplan, 2023).

- *Recommendation 18.* Policing agencies not yet contributing crime data through the NIBRS should prioritize full participation in the program. States can support the transition by providing technical and financial support to local agencies, subsidizing updated record management systems, and establishing reporting standards or mandates.
- *Recommendation 19.* The FBI's Criminal Justice Information Services Division should modify the NIBRS data collection system to allow agencies to report data on gunshot wounds sustained by crime victims, offenders, and law enforcement officers, as well as incidents during which a firearm was discharged.

Another potentially valuable source of information on crimes is the National Crime Victimization Survey (NCVS), which collects detailed information on crime from a panel of U.S. residents selected to be representative of the country. 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 Supplementary 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, the Bureau of Justice Statistics (BJS) has explored options for generating such estimates through the NCVS (BJS, 2017b). In 2016, BJS began expanding the survey panel to generate reliable estimates for 22 states that account for 79 percent of the U.S. population. The state-level NCVS estimates are available through an application process, and the NCVS microdata with subnational identifiers are available through a Census Research Data Center following approval of a restricted data access application. In addition, the Census Bureau has published model-based state estimates for some types of crime over three-year periods from 1999 to 2019 (Fay and Diallo, 2015; Kena and Morgan, 2023), although estimates for crimes involving firearms have not yet been published.

- *Recommendation 20*. BJS's plan to generate crime estimates for the 22 most populous states is a reasonable compromise between cost and the public's need for more-detailed information. BJS should work to facilitate access to and use of these estimates and the NCVS microdata and consider whether a small grant program could support researcher use of these data. In addition, the bureau should continue 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).

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).

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 such 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 department records, emergency medical services records, 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 (Barber, Cook, and Parker, 2022; Mercy, Ikeda, and Powell, 1998; Koo and Birkhead, 1998). In 2020, the CDC funded an initiative for ten state health departments to provide near–real time data on emergency department visits for non-

fatal firearm injuries, and this has been expanded in 12 jurisdictions (five continuing from the previous initiative) to include injuries related to other forms of violence, as well as emergency department visits related to mental health conditions (CDC, 2021). While ongoing efforts in a subset of these states are exploring the possibility of linking emergency department data to other sources of nonfatal injury data, to our knowledge, none of these data have been made available to researchers.

However, two existing sources of data already collect and provide access to data on nonfatal firearm injuries, although these data could be made more accessible to researchers. First, the National Trauma Data Bank aggregates data from individual trauma center registries across the United States, and since 2008, the scope of coverage of this data set has increased substantially. Although the data set is not nationally representative, it contains rich information on the nature and severity of injuries, as well as patient demographics and outcomes, but state and substate identifying information is not included in the files made available to researchers. Second, 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. Although the Healthcare Cost and Utilization Project offers a suite of state-level inpatient and emergency department databases, which represent a near-census of discharge-level firearm injuries from inpatient hospitals or emergency departments, each state-year data set must be purchased separately. This expense represents a serious access barrier for most researchers who are interested in analyzing state firearm injuries over time. However, the Healthcare Cost and Utilization Project has released a redesigned HCUPnet tool, which allows users to freely access some aggregate statistics on hospitalizations and emergency department visits, including information at the state-year level for states that allow their data to be accessed through this mechanism (and subject to some suppression constraints). Although this tool allows researchers to access some aggregate information on firearm injuries that result in a hospital inpatient or emergency department visit, obtaining time series data across multiple states and population subgroups can be highly cumbersome given the system's setup. Furthermore, the tool does not appear to allow the user to distinguish observations by whether a visit resulted in an in-hospital death (i.e., fatal and nonfatal incidents are combined).

- *Recommendation 21.* The Agency for Healthcare Research and Quality at the U.S. Department of Health and Human Services should publish aggregated annual or quarterly 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.[2] Ideally, data could be parsed by whether an in-hospital death occurred.
- *Recommendation 22.* Agencies responsible for other ongoing surveillance efforts that measure nonfatal firearm-related injuries should develop mechanisms for sharing these

[2] See, for instance, Healthcare Cost and Utilization Project Fast Stats, 2019.

data with researchers in a manner that could support evaluation of variation across jurisdictions and over time.

Conclusion 16. While a growing number of studies are using more-rigorous methods to evaluate the causal effects of gun policies, the overall 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. There have also been significant advancements in causal inference methods, and gun policy evaluation research is increasingly adopting these methods. Nevertheless, on the basis of the results of our title and abstract screen and our methodological quality assessment of studies that met our inclusion criteria, the overall landscape of gun policy evaluation research leaves much room for improvement. Even excluding the routinely published studies that conduct cross-sectional analyses (despite the availability of longitudinal data that could support more rigorous analyses), 25 (16 percent) of the 149 non-superseded studies that were included in our review had critical methodological limitations that rendered their results invalid for making strong causal claims. The field as a whole would be improved by demanding a higher bar that addresses these more common critical issues:

- presenting cross-sectional data as though they provide suggestive evidence for a causal effect when longitudinal data are available to test any such claims
- analyzing time-series data for multiple states but estimating associations of policy with outcomes without accounting for any potential temporal confounds (e.g., factors common across states that influence how outcomes trend over time) or jurisdictional confounds (e.g., differences across states with versus states without the policy that are associated with both policy decisions and outcomes of interest)
- comparing a single policy-implementing jurisdiction with a small set of comparison jurisdictions (or a single comparison jurisdiction) without an assessment or discussion of whether those comparison jurisdictions represent a plausible counterfactual for the implementing jurisdiction
- failing to characterize uncertainty in estimated policy effects or producing inferential statistics using nonstandard methods that are known to over-reject the null
- estimating too many parameters, given the number of available observations or the sparseness of the outcome data. This problem leads to less accurate variance estimates and can result in statistically significant effects that provide virtually no information about the true generalizable effects of the policies.

While studies with these critical methodological weaknesses continue to be published, several of the studies that we evaluated have demonstrated a much higher degree of methodological rigor. Of the 13 studies that we rated as having at least one analysis of high methodological quality (see Table A.3), ten were published in the past decade and four are newly included studies in this review update. There will continue to be methodological advance-

ments, and researchers should continue to probe the extent to which the statistical and causal identification assumptions underlying their methods hold. But future studies can draw on the methodological improvements in the field of firearm research and broader policy evaluation methodological literature to address the following persistent issues:

- Inadequate discussion of and tests for the plausibility of assumptions under which the estimated models will produce unbiased estimates of the causal effects of policies on outcomes of interest. Many researchers fail to justify their choice of empirical approach in their studies, as well as their choice of which covariates to include in their models, but these decisions are key to determining whether model estimates are likely to represent the causal effects of 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).
- 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 often have been found to contain errors that could affect results. Furthermore, this information is necessary to determine the sources of policy variation being used to identify policy effect estimates.
- 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 during 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.
- Inadequate attention to threats of reciprocal causation or simultaneity biases in effect estimation. Interpretations of effects generated in models 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.

These are technical points of interest chiefly to researchers, so we refer interested readers to other reports, principally Schell, Smart, and Morral (2022) and Schell, Griffin, and Morral (2018). However, our final recommendations are for other researchers interested in the analysis of the effects of gun policies.

- *Recommendation 23.* As part of the Gun Policy in America initiative, we have published a database containing a subset of state gun laws from 1979 to 2021 (Cherney et al., 2022). 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 24.* Researchers, reviewers, academics, and science reporters should ensure that new analyses of the effects of gun policies improve on earlier studies by persuasively addressing the methodological limitations of earlier studies, such as inadequate attention to causal identification assumptions, problems with statistical power, model overfitting, poorly calibrated standard errors, multiple testing, undisclosed variation in law implementation, and unjustified assumptions about the time course of each policy's effects.

In conclusion, the base of evidence has grown substantially since the publication of the first edition of this report, and there are now several policy areas for which we have more robust evidence for their effects—beneficial or harmful—on at least some outcomes. In light of this evidence, decisionmakers may want to reconsider their existing policy regimes and reflect on whether changes to policy are warranted. Political or contextual considerations, as well as ongoing Second Amendment litigation, may make some policy options more feasible or attractive than others. Additionally, because we have limited or nonexistent evidence for how these policies will affect many outcomes that are important to stakeholders, decisions will have to be made without a robust empirical understanding of the potential trade-offs. As evidence mounts in certain areas, however, decisionmakers should take that evidence into consideration.

While our understanding of gun policy effects is improving, many gaps remain in extant rigorous scientific evidence. This does not mean that unstudied or poorly studied policies are ineffective; they might well be quite effective. Instead, this reflects shortcomings in the contributions that scientific study can offer to policy debates in these areas. These shortcomings reflect several interrelated issues in gun policy research, including

- an inadequate data infrastructure
- years of research funding neglect by federal agencies
- outcomes that are so sparse as to make detection of laws' effects difficult or impossible (e.g., mass shootings)
- policies that have not been implemented in enough jurisdictions or for enough years to support strong causal effect estimates.

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 places. 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 might 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 2,000 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 21 References

Alcorn, Ted, "Trends in Research Publications About Gun Violence in the United States, 1960 to 2014," *JAMA Internal Medicine*, Vol. 177, No. 1, January 2017.

Asher, Jeff, "Who Reported NIBRS Data in 2022?" *Jeff-alytics* Substack blog, August 11, 2023. As of February 9, 2024:
https://jasher.substack.com/p/who-reported-nibrs-data-in-2022

Barber, Catherine, Philip J. Cook, and Susan T. Parker, "The Emerging Infrastructure of US Firearms Injury Data," *Preventive Medicine*, July 2022.

Billings, Stephen B., "Smoking Gun? Linking Gun Ownership to Crime Victimization," *Journal of Public Economics*, Vol. 222, June 2023.

BJS—*See* Bureau of Justice Statistics.

Braga, Anthony A., Garen J. Wintemute, Glenn L. Pierce, Philip J. Cook, and Greg Ridgeway, "Interpreting the Empirical Evidence on Illegal Gun Market Dynamics," *Journal of Urban Health*, Vol. 89, No. 5, 2012.

Bureau of Alcohol, Tobacco, and Firearms, *Crime Gun Trace Analysis Reports: The Illegal Youth Firearms Market in Seventeen Communities*, U.S. Department of the Treasury, 1997.

Bureau of Justice Statistics, "NCVS Redesign: Subnational," webpage, 2017b. As of May 12, 2017:
https://www.bjs.gov/index.cfm?ty=tp&tid=911

Cagle, M. Christine, and J. Michael Martinez, "Have Gun, Will Travel: The Dispute Between the CDC and the NRA on Firearm Violence as a Public Health Problem," *Politics & Policy*, Vol. 32, No. 2, 2004.

Campbell, Sean, Daniel Nass, and Mai Nguyen, "The CDC Is Publishing Unreliable Data on Gun Injuries. People Are Using It Anyway," *FiveThirtyEight*, October 4, 2018.

CDC—*See* Centers for Disease Control and Prevention.

Centers for Disease Control and Prevention, "Underlying Cause of Death, 1999–2020," WONDER data system, undated-b. As of February 8, 2024:
https://wonder.cdc.gov/ucd-icd10.html

Centers for Disease Control and Prevention, "Multiple Cause of Death, 2018–2021, Single Race," WONDER data system, undated-e. As of February 8, 2024:
https://wonder.cdc.gov/mcd.html

Centers for Disease Control and Prevention, "Funded Research [Violence Prevention]," webpage, last reviewed May 4, 2021. As of December 6, 2021:
https://www.cdc.gov/violenceprevention/firearms/funded-research.html

Cheng, Cheng, and Mark Hoekstra, "Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence From Expansions to Castle Doctrine," *Journal of Human Resources*, Vol. 48, No. 3, 2013.

Cherney, Samantha, Andrew R. Morral, Terry L. Schell, Sierra Smucker, and Emily Hoch, *Development of the RAND State Firearm Law Database and Supporting Materials*, RAND Corporation, TL-A243-2-v2, 2022. As of October 1, 2022:
https://www.rand.org/pubs/tools/TLA243-2-v2.html

Cook, Philip J., "The Great American Gun War: Notes from Four Decades in the Trenches," *Crime and Justice*, Vol. 42, No. 1, 2013.

Cook, Philip J., and Anthony A. Braga, "Comprehensive Firearms Tracing: Strategic and Investigative Uses of New Data on Firearms Markets," *Arizona Law Review*, Vol. 43, No. 2, 2001.

Cook, Philip J., and Kristin A. Goss, *The Gun Debate: What Everyone Needs to Know*, Oxford University Press, 2014.

Cook, Philip J., Ariadne E. Rivera-Aguirre, Magdalena Cerdá, and Garen Wintemute, "Constant Lethality of Gunshot Injuries from Firearm Assault: United States, 2003–2012," *American Journal of Public Health*, Vol. 107, No. 8, 2017.

Cox, Georgina R., Christabel Owens, Jo Robinson, Angela Nicholas, Anne Lockley, Michelle Williamson, Yee Tak Derek Cheung, and Jane Pirkis, "Interventions to Reduce Suicides at Suicide Hotspots: A Systematic Review," *BMC Public Health*, Vol. 13, 2013.

Criminal Justice Information Services Division, *2023.0 National Incident-Based Reporting System Technical Specification*, U.S. Department of Justice, Federal Bureau of Investigation, June 30, 2023a. As of March 15, 2024:
https://le.fbi.gov/file-repository/nibrs-technical-specification-063023.pdf/view

Criminal Justice Information Services Division, "Active Records in the NICS Indices as of January 3, 2023," U.S. Department of Justice, Federal Bureau of Investigation, January 3, 2023b.

Depew, Briggs, and Isaac Swensen, "The Decision to Carry: The Effect of Crime on Concealed-Carry Applications," *Journal of Human Resources*, Vol. 54, No. 4, Fall 2019.

Donohue, John J., Samuel V. Cai, Matthew V. Bondy, and Philip J. Cook, *Why Does Right-to-Carry Cause Violent Crime to Increase?* National Bureau of Economic Research, Working Paper No. 30190, revised June 2023.

Donohue, John J., Samuel V. Cai, and Arjun Ravi, "Age and Suicide Impulsivity: Evidence from Handgun Purchase Delay Laws," Working Paper No. 31917, National Bureau of Economic Research, November 2023.

Ertl, Allison, Kameron J. Sheats, Emiko Petrosky, Carter J. Betz, Keming Yuan, and Katherine A. Fowler, "Surveillance for Violent Deaths—National Violent Death Reporting System, 32 States, 2016," *Morbidity and Mortality Weekly Report Surveillance Summaries*, Vol. 68, No. 9, 2019.

Fay, Robert E., and Mamadou S. Diallo, *Developmental Estimates of Subnational Crime Rates Based on the National Crime Victimization Survey*, Westat, 2015.

FBI—*See* Federal Bureau of Investigation.

Federal Bureau of Investigation, "Expanded Homicide Data Table 8: Murder Victims, by Weapon, 2015–2019," webpage, Crime in the United States 2019, 2020b. As of August 31, 2022: https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/expanded-homicide-data-table-8.xls

Federal Bureau of Investigation, "Expanded Homicide Data Table 15: Justifiable Homicide, by Weapon, Private Citizen, 2015–2019," webpage, Crime in the United States 2019, 2020c. As of August 31, 2022:
https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/expanded-homicide-data-table-15.xls

Federal Bureau of Investigation, "UCR Program Quarterly: March 2022 (22-1)," March 2022e. As of March 15, 2024:
https://le.fbi.gov/file-repository/ucr-program-quarterly-march-2022.pdf/view

Federal Bureau of Investigation, "FBI Releases 2022 Crime in the Nation Statistics," press release, October 16, 2023e.

Halsey, Lewis G., Douglas Curran-Everett, Sarah L. Vowler, and Gordon B. Drummond, "The Fickle P Value Generates Irreproducible Results," *Nature Methods*, Vol. 12, No. 3, March 2015.

Hanson, Emily J., *NIBRS Participation Rates and Federal Crime Data Quality*, Congressional Research Service, May 24, 2022.

Healthcare Cost and Utilization Project Fast Stats, "Opioid-Related Hospital Use," web tool, April 2019. As of November 14, 2019:
https://www.hcup-us.ahrq.gov/faststats/OpioidUseServlet

Hemenway, David, Chloe Shawah, and Elizabeth Lites, "Defensive Gun Use: What Can We Learn from News Reports?" *Injury Epidemiology*, Vol. 9, No. 19, 2022.

Kaplan, Jacob, *National Incident-Based Reporting System (NIBRS) Data: A Practitioner's Guide*, March 28, 2023.

Karp, Aaron, *Estimating Global Civilian-Held Firearms Numbers*, Small Arms Survey, June 2018.

Kellermann, Arthur L., and Frederick P. Rivara, "Silencing the Science on Gun Policy," *JAMA*, Vol. 309, No. 6, 2013.

Kena, Grace, and Rachel E. Morgan, *Criminal Victimization in the 22 Largest U.S. States, 2017–2019*, U.S. Department of Justice, Office of Justice Programs, Bureau of Justice Statistics, NCJ 305402, March 2023.

Kennedy, D. M., A. M. Piehl, and A. A. Braga, "Youth Violence in Boston: Gun Markets, Serious Youth Offenders, and a Use-Reduction Strategy," *Law and Contemporary Problems*, Vol. 59, No. 1, 1996.

Koo, Denise, and Guthrie S. Birkhead, "Prospects and Challenges in Implementing Firearm-Related Injury Surveillance in the United States: Not a Flash in the Pan," *American Journal of Preventive Medicine*, Vol. 15, No. 3, Supp. 1, 1998.

Levy, Marc, Wilmer Alvarez, Lauren Vagelakos, Michelle Yore, and Bertha Ben Khallouq, "Stand Your Ground: Policy and Trends in Firearm-Related Justifiable Homicide and Homicide in the US," *Journal of the American College of Surgeons*, Vol. 230, No. 1, January 2020.

Lewiecki, E. M., and S. A. Miller, "Suicide, Guns, and Public Policy," *American Journal of Public Health*, Vol. 103, No. 1, 2013.

Lin, John C., Christopher Chang, Madison S. McCarthy, Abe Baker-Butler, Guangyu Tong, and Megan L. Ranney, "Trends in Firearm Injury Prevention Research Funding, Clinical Trials, and Publications in the US, 1985–2022," *JAMA Surgery*, February 7, 2024.

Longo, Daniel, *Lock and Load: After Recent Sales Surge, Demand Normalizes as Fears of Gun Control Subside*, IBISWorld Industry Report 33299a: Guns and Ammunition Manufacturing in the U.S., June 2017.

Mayors Against Illegal Guns, "Access Denied: How the Gun Lobby Is Depriving Police, Policy Makers, and the Public of the Data We Need to Prevent Gun Violence," Everytown for Gun Safety Support Fund, 2013.

McClellan, Chandler, and Erdal Tekin, "Stand Your Ground Laws, Homicides, and Injuries," *Journal of Human Resources*, Vol. 52, No. 3, 2017.

McGirr, A., J. Renaud, A. Bureau, M. Seguin, A. Lesage, and G. Turecki, "Impulsive-Aggressive Behaviours and Completed Suicide Across the Life Cycle: A Predisposition for Younger Age of Suicide," *Psychological Medicine*, Vol. 38, No. 3, 2008.

Mercy, James A., Robin Ikeda, and Kenneth E. Powell, "Firearm-Related Injury Surveillance: An Overview of Progress and the Challenges Ahead," *American Journal of Preventive Medicine*, Vol. 15, No. 3, Supp. 1, 1998.

Miller, Matthew, and Deborah Azrael, "Firearm Storage in US Households with Children: Findings from the 2021 National Firearm Survey," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Miller, Matthew, Katherine Hempstead, Tuan Nguyen, Catherine Barber, Sarah Rosenberg-Wohl, and Deborah Azrael, "Method Choice in Nonfatal Self-Harm as a Predictor of Subsequent Episodes of Self-Harm and Suicide: Implications for Clinical Practice," *American Journal of Public Health*, Vol. 103, No. 6, 2013a.

Minnesota Department of Public Safety, "BCA Releases 2021 Permit to Carry Annual Report," March 1, 2022.

National Research Council, *Firearms and Violence: A Critical Review*, National Academies Press, 2004.

National Shooting Sports Foundation, *Firearms and Ammunition Industry Economic Impact Report 2022*, 2023.

New York State Rifle and Pistol Association v. Bruen, 597 U.S. ___, 2022.

Outdoor Foundation, *2022 Outdoor Participation Trends Report*, undated.

Owens, David, Judith Horrocks, and Allan House, "Fatal and Non-Fatal Repetition of Self-Harm," *British Journal of Psychiatry*, Vol. 181, No. 3, 2002.

Parker, Susan T., "Measuring Gun Violence in Police Data Sources: Transitioning to NIBRS," *Injury Epidemiology*, Vol. 9, No. 15, 2022.

Pierce, Glenn L., Anthony A. Braga, Christopher Koper, Jack McDevitt, David Carlson, Jeffrey Roth, Alan Saiz, Raymond Hyatt, and Roberta E. Griffith, *The Characteristics and Dynamics of Crime Gun Markets: Implications for Supply-Side Focused Enforcement Strategies*, National Institute of Justice, 2003.

Public Law 112-74, Consolidated Appropriations Act, 2012, December 23, 2011.

Ramchand, Rajeev, *Personal Firearm Storage in the United States: Recent Estimates, Patterns, and Effectiveness of Interventions*, RAND Corporation, RR-A243-5, 2022. As of February 7, 2024:
https://www.rand.org/pubs/research_reports/RRA243-5.html

Rochford, Hannah I., Mark Berg, and Corinne Peek-Asa, "The 'Boyfriend Loophole' and Intimate Partner Homicides: A Longitudinal Analysis Using the National Violent Death Reporting System," *Journal of Prevention*, Vol. 43, No. 6, December 2022.

Roman, John K., and Philip Cook, *Final Report: Improving Data Infrastructure to Reduce Firearms Violence*, NORC at the University of Chicago, October 2021.

Rowhani-Rahbar, Ali, Deborah Azrael, Vivian H. Lyons, Joseph A. Simonetti, and Matthew Miller, "Loaded Handgun Carrying Among US Adults, 2015," *American Journal of Public Health*, Vol. 107, No. 12, 2017.

Sakinofsky, Isaac, "Repetition of Suicidal Behavior," in K. Hawton and K. P. van Heeringen, eds., *The International Handbook of Suicide and Attempted Suicide*, John Wiley and Sons, 2000.

Saltzman, Linda E., and Robin M. Ikeda, "Recommended Data Elements for Firearm-Related Injury Surveillance," *American Journal of Preventive Medicine*, Vol. 15, No. 3, Supp. 1, 1998.

Schaeffer, Katherine, "Key Facts About Americans and Guns," Pew Research Center, September 13, 2023.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of October 1, 2019:
https://www.rand.org/pubs/research_reports/RR2685.html

Schell, Terry L., Rosanna Smart, and Andrew R. Morral, *Suggestions for Estimating the Effects of State Gun Policies: Commentary on Four Methodological Problems in the Current Literature*, RAND Corporation, RR-A243-1, 2022. As of August 9, 2022:
https://www.rand.org/pubs/research_reports/RRA243-1.html

Simon, T. R., A. C. Swann, K. E. Powell, L. B. Potter, M. Kresnow, and P. W. O'Carroll, "Characteristics of Impulsive Suicide Attempts and Attempters," *Suicide and Life-Threatening Behavior*, Vol. 32, Supp. 1, 2002.

Stansfield, Richard, Daniel Semenza, and Ian Silver, "The Relationship Between Concealed Carry Licenses and Firearm Homicide in the US: A Reciprocal County-Level Analysis," *Journal of Urban Health*, Vol. 100, No. 4, August 2023.

Stark, David E., and Nigam H. Shah, "Funding and Publication of Research on Gun Violence and Other Leading Causes of Death," *JAMA*, Vol. 317, No. 1, January 3, 2017.

Studdert, David M., Yifan Zhang, Sonja A. Swanson, Lea Prince, Jonathan A. Rodden, Erin E. Holsinger, Matthew J. Spittal, Garen J. Wintemute, and Matthew Miller, "Handgun Ownership and Suicide in California," *New England Journal of Medicine*, Vol. 382, No. 23, 2020.

Texas Department of Public Safety, *Conviction Rates for Handgun License Holders Reporting Period: 01/01/2021–12/31/2021*, March 18, 2022.

U.S. Census Bureau, *Annual Survey of Manufacturers: Summary Statistics for Industry Groups and Industries in the U.S.: 2018–2021*, Table AM1831BASIC01, December 2022a.

U.S. Census Bureau, *Number of Firms, Number of Establishments, Employment, and Annual Payroll by Enterprise Employment Size for the United States, All Industries: 2021*, December 2023.

U.S. Census Bureau, "Three-Quarters of Children Younger Than 6 Years Old Live with Two Parents," press release, November 21, 2023a.

U.S. Fish and Wildlife Service, *2016 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*, FHW/16-NAT, April 2018.

U.S. Fish and Wildlife Service, *2022 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation*, September 2023.

U.S. House of Representatives, Making Appropriations for the Departments of Labor, Health and Human Services, and Education, and Related Agencies for the Fiscal Year Ending September 30, 2024, and for Other Purposes, Bill 5894, October 6, 2023.

Webster, Daniel W., Jon S. Vernick, and Lisa M. Hepburn, "Relationship Between Licensing, Registration, and Other Gun Sales Laws and the Source State of Crime Guns," *Injury Prevention*, Vol. 7, 2001.

Weil, Douglas S., and Rebecca C. Knox, "Effects of Limiting Handgun Purchase on Interstate Transfer of Firearms," *JAMA*, Vol. 275, No. 22, 1996.

Wintemute, G. J., C. A. Parham, J. J. Beaumont, M. Wright, and C. Drake, "Mortality Among Recent Purchasers of Handguns," *New England Journal of Medicine*, Vol. 341, No. 21, 1999.

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
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
ROBINS-I	Risk of Bias in Non-Randomised Studies - of Interventions
SRS	Summary Reporting System

APPENDIX A

Changes from the Third Edition of This Report

In addition to updating our 2023 review to incorporate more recently published literature, we made some changes to the quality designations assigned to earlier studies. Furthermore, the inclusion of additional studies in our analysis 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 Search Parameters

We made minor changes to our search parameters to fix syntax to improve consistency and ensure correct retrieval (e.g., instead of searching on "mass shooting" OR "mass shootings," we now search on "mass shooting*"). We also added some additional terms to the outcome search strings ("un intentional," "robberies," "spouse abuse," "elder mistreatment," "supplies," "trading," and "FFL"). All changes were done at the recommendation of our research librarian to better align our search syntax with existing standards.

Changes to Our Quality Assessment Criteria

As noted in Chapter 2, in this edition of the report, we revised our approach to assessing the methodological quality of study estimates to formalize some criteria that had previously been grouped into our "other concerns" quality assessment domain and to better align our quality assessment process with that of the ROBINS-I ("Risk of Bias in Non-Randomised Studies - of Interventions") tool (Sterne et al., 2016). This new plan for quality assessment was preregistered before we began this update. We thus now adopt a four-level system of quality classification (high methodological quality, minor concerns, serious concerns, critical concerns) rated across six domains, and we have an "other concerns" category as a seventh domain. Table A.1 maps our quality assessment domains and definitions to those in ROBINS-I and provides some narrative discussion of differences. Table A.2 documents the quality assessment criteria that we used to assess potential methodological concerns with study effect esti-

mates (defined at the study-policy-outcome level), and guidance provided to the raters for determining ratings is shown by assessment domain.

While we considered using ROBINS-I, we determined that this tool was unlikely to be a useful metric to gauge the quality or risk of bias of our included studies for several reasons. First, despite the ROBINS-I tool's stated aim of providing risk-of-bias assessment for non-randomized studies of interventions, the way it is constructed appears to be tailored particularly for individual-level cohort designs, whereby individuals receive different interventions and then are followed over time. The detailed guidance for ROBINS-I gives examples of the types of relevant study designs as "cohort studies, case-control studies, controlled before-and-after studies, interrupted-time-series studies and controlled trials in which intervention groups are allocated using a method that falls short of full randomization (sometimes called 'quasi-randomized' studies)" (Sterne et al., 2016, p. 3); this may be an issue of disciplinary terminology, but the types of study designs most commonly used to evaluate the effects of state gun policies (e.g., difference-in-differences, synthetic control method, comparative time series) are not referenced.

Perhaps because of this construction, the way ROBINS-I provides guidance on bias from confounding is such as to render all studies that would be included in this review as being at serious or critical risk of bias (e.g., see the Yakubovich et al. [2021] review of self-defense laws, which rates every included study as critical or "no information" on the confounding domain of ROBINS-I). The ROBINS-I detailed guidance about this domain also fails to recognize that studies in the gun policy field often use quasi-experimental methods, in which the identifying assumptions requisite for interpreting estimated policy effects as causal do not necessitate regression-based adjustments for all baseline or time-varying confounders (which seems to be the focus of the ROBINS-I guidance for judging this domain).

Additionally, ROBINS-I uses some criteria that are rarely relevant for policy evaluation studies like those that we include in this review (e.g., selection bias, as defined by ROBINS-I, would almost always receive a "low" rating given its lack of applicability). Abiding these criteria would be challenging and time-consuming with little benefit to actually gauging the methodological quality of the studies we review. There are other domains (e.g., reporting bias) that are difficult to judge because pre-analysis plans are not conventionally used in gun policy research, and thus it would be entirely up to the rater's judgment about whether certain analyses were conducted but not presented in ways likely to misrepresent findings. Particularly because different disciplines and different journals have different expectations about how extensive the number of results and robustness checks should be, it is challenging to fairly and accurately rate these domains in our context. Finally, there are numerous statistical issues (e.g., number of estimated parameters, distributional assumptions) that have been shown to matter for producing accurate effect estimates and measures of uncertainty, none of which are incorporated into the ROBINS-I tool (Schell, Griffin, and Morral, 2018; Schell, Smart, and Morral, 2022).

TABLE A.1

Comparison of Domains with Cochrane ROBINS-I

Our Domain and Definition or Reason for Not Including	ROBINS-I Domain and Definition
Threats to causal identification: The extent to which a study design is unlikely to produce unbiased estimates of the causal effects of the policies of interest (i.e., a pre-treatment, post-treatment, and comparison group approach is used, but the estimated policy effect should not be interpreted as causal because there is a likely violation of the model's identifying assumptions) **Note:** This bears some similarities to the ROBINS-I domain, but it has broader consideration of quasi-experimental methods whereby confounding is not solely addressed through regression-based adjustment. Thus, such methods as difference-in-differences, synthetic control methods, and instrumental variables carry different assumptions and require different judgments from those that are focused on in the ROBINS-I tool.	**Bias due to confounding:** The extent to which baseline or time-varying confounding is likely to produce bias in estimated effects
Few treated units, control units, and pre- or post-treatment data: In our context, this is mostly going to be picked up with the causal identification threats domain mentioned in the first row of this table. However, with a small number of treated units, there is a much higher risk that something else, such as another policy, occurred coincident with the policy of interest (or subsequent to) in ways that are not easy to address. There are also often additional concerns with accurate inference (Conley and Taber, 2011) and external generalizability.	**Bias due to deviations from intended interventions:** There are systematic differences in the care provided to the experimental intervention and comparator groups (beyond the assigned interventions). These differences reflect additional aspects of care, or intended aspects of care that were not delivered. Cointerventions are a potentially important source of bias.
Policy classification: With policy evaluation, there are concerns with misclassification, but identifying "misclassification" is not straightforward, because researchers may rightfully make different decisions about how to group policies depending on the theory guiding their analysis. Thus, we note this issue only for studies that have been subsequently shown to have had serious errors in policy coding that are likely to render their results invalid.	**Bias in classification of interventions:** Bias may be introduced if there is *differential misclassification*—when misclassification of intervention status is related to the subsequent outcome or to the risk of the outcome—of intervention status. It is important that, wherever possible, interventions are defined and categorized without knowledge of subsequent outcomes.
Systematic outcome data missingness: For almost all of our outcomes, it is difficult to think of a reason missingness might relate to the policy of study. Exceptions are missingness that is a function of the value of the outcome of interest, because the policy effect on the outcome values and levels would then relate to missingness.	**Bias due to missing data:** Differentials in missing data between intervention groups are key, along with the reasons for data being missing. If (1) the proportion of missing data and (2) the reasons for missing data are similar across intervention groups, then there would typically be only limited bias in the effect estimate, so that risk of bias would be considered low or moderate.

Table A.1—Continued

Our Domain and Definition or Reason for Not Including	ROBINS-I Domain and Definition
Violation of model assumptions and inaccurate inference: This domain reflects the potential for fundamental problems with statistical approaches commonly used in the field, which commonly rely on assumptions about independence of residuals, normality, etc. Violating these assumptions often results in inaccurate estimates of uncertainty and, in some cases, can produce biased effect estimates (Schell, Griffin, and Morral, 2018). We also include in this domain failure to report inferential statistics and the use of inferential methods that are widely known to over-reject the null.	Not applicable.
Potential for model overfit: This domain reflects potential issues that arise in overfit models, including that a rich set of covariates can help address concerns with omitted variable bias, but there is a trade-off in that estimating too many parameters relative to the sample size (or relevant to the number of observed events) runs the risk of model overfit, thus potentially reducing the accuracy of the effect estimates and inferential statistics (Peduzzi et al., 1995; Riley et al., 2019). While this will most commonly lead to inflated estimates of variance, in the extreme, this can produce severe bias and inflated Type I error.	Not applicable.
Not included: This does not seem applicable to our type of policy evaluation studies of interest. To the extent that selection bias is a concern in our context, it is covered under the aforementioned confounding domain.	**Bias of selection of participants into the study:** Selection bias occurs when some eligible participants, the initial follow-up time of some participants, or some outcome events are excluded in a way that leads to the association between intervention and outcome that differs from the association that would have been observed in the target trial.
Not included: As with missing data, for almost all included outcomes, it is difficult to think of a reason that outcome measurement would be related to the policy of interest. There is one notable exception to this: justifiable homicide based on law enforcement report as an outcome for stand-your-ground laws. We note these issues in the text, but we do not rate this particular domain across all studies.	**Bias in measurement of outcomes (misclassification or measurement error):** Bias may be introduced if outcomes are misclassified or measured with error. *Differential measurement error* is measurement error–related intervention status. It will bias the intervention-outcome relationship. This is often referred to as *detection bias*.
Not included: For low or moderate risk of bias, the ROBINS-I tool requires a pre-analysis plan, which has not been the disciplinary standard for these types of policy evaluation studies. Furthermore, there are huge disciplinary and journal differences in the extent to which certain results are expected to be presented, so it is difficult for us, as readers, to make judgment calls about which outcomes, analyses, or subgroups may have been evaluated but not shown for selective reporting reasons.	**Bias in selection of the reported result:** *Selective outcome reporting* occurs when the effect estimate for an outcome is selected from among analyses of multiple outcome measurements for the outcome domain. Selective analysis reporting occurs when results are selected from intervention effects estimated in multiple ways. Selection of a subgroup from a larger cohort may also provide evidence of selective reporting.

TABLE A.2
Guidance on Domain-Specific Judgments

Domain	Rating (done at the policy x outcome level)
Threats to causal identification	**No concern:** Discusses the model's identifying assumptions and provides evidence to support that the model's assumptions are met or provides evidence of similar results under various reasonable assumptions. **Minor concern:** Does not discuss or provide evidence in support of models' identifying assumptions and/or the validity of the control group, but the approach does not have serious or critical concerns as defined below. **Serious concern:** Accounts for unit-level and time-level confounds, but (a) for regression-based approaches, does not account for any unit-specific time-varying confounds that are likely to affect both the probability of policy adoption and the outcome of interest **and** does not provide evidence in support of parallel trends or conditional ignorability assumptions (as relevant); (b) for synthetic control methods, does not discuss or provide evidence of pre-period fit; or (c) for other, less commonly used approaches, this will be rated via group discussion. **or** Accounts for unit-level, time-level, and unit-specific time-varying confounds (or lagged outcomes) but provides some evidence that identifying assumptions may be violated and does not address the violations. **or** Accounts for unit-level, time-level, and unit-specific time-varying confounds (or lagged outcomes), but the policy is not the focus of the paper (e.g., the policy is a control variable) and evidence specifically in support of meeting identifying assumptions for the ancillary policy is not discussed or provided. **or** Reflects coefficient estimates on a policy variable that was intended to represent a covariate (or confound control) rather than the causal estimand of interest and is not subject to critical concerns, as discussed below. **Critical concern:** Accounts for only unit-level or only time-level confounds (but not both, and not unit-specific time-varying confounds) and does not provide any justification as related to identifying assumptions. **or** Accounts for unit-level and time-level confounds but provides strong evidence that identifying assumptions may be violated and does not adequately address the violations.
Few treated units, control units, and pre- or post-treatment data	**No concern:** Includes four or more groups with policy transitions (e.g., implement law, repeal law) with three or more years of pre-transition data and three or more years of post-transition data. **Minor concern:** Does not provide sufficient information on policy data source, coding method, or policy variation to assess the number of states contributing to identifying variation with pre- and post-treatment data but is likely to meet criteria based on the RAND legal database or State Firearm Law Database. **Serious concern:** Includes fewer than four treated groups with three years of pre-policy and post-policy data, **or** insufficient info and is unlikely to meet criteria based on the RAND legal database or State Firearm Law Database. **Critical concern:** Fewer than four treated groups with three or more years pre-policy and post-policy data **and** fewer than four control units (or, in SCM studies, more than 95 percent of weight drawn from three or fewer donors).

Table A.2—Continued

Domain	Rating (done at the policy x outcome level)
Policy classification	**No concern:** Assumed to hold unless literature has established critical errors in law coding. **Critical concern:** Literature has established critical errors in law coding that are not explained or justified by the article.
Systematic outcome data missingness	**No concern:** Data are reasonably complete, or proportions of and reasons for missing data are similar across treated and control groups; thus, missing data are unlikely to be correlated with policy, or missing data are addressed appropriately. **Minor concern:** Has a substantial amount of missingness or proportions of and reasons for missing data differ slightly across treated and control groups (and are not addressed analytically), but missing data are unlikely to be correlated with policy. **Serious concern:** Analyses omit outcome data that are related to outcome data values (e.g., dropping suppressed state-years from the Centers for Disease Control and Prevention's [CDC's] WONDER database; log-linear models that drop outcomes with value zero), but this affects a reasonably small proportion of observations. Do not treat this as a serious issue if states or units with suppression are dropped in their entirety (rather than dropping unit-years). **Critical concern:** Omission of outcome data that are related to outcome data values (e.g., dropping suppressed state-years from the CDC's WONDER database; log-linear models that drop outcomes with value zero), and this affects a high proportion of observations.
Violation of model assumptions and inaccurate inference	**No concern:** No serious concerns with distributional assumptions; methods correct for nonindependence (e.g., clustered standard errors) in data known to require adjustments. **Minor concern:** No serious concerns with distributional assumptions or serial correlation, but potential concerns from some other forms of nonindependence (e.g., meta-analyzing state-specific estimates produced by synthetic control methods) are not adequately addressed. **Serious concern:** Serial correlation is not addressed (for outcomes likely to be characterized by strong serial correlation), or there are linear models of logged outcomes where outcomes are rare enough that there are a nontrivial number of zero cells or linear models of binary outcomes with prevalence of less than 15 percent. **and** No additional specifications or pieces of evidence are provided to support validity of distributional assumptions or effect estimates. **Critical concern:** Inferential statistics for relevant policy effect estimates are not provided or are estimated in a way that is critically inappropriate given the approach (e.g., use of basic t-tests with synthetic control methods).
Potential for model overfit	**No concern:** number of observations (*n*):estimated parameters (k)>=10 and number of events (e):k>=10. **Minor concern:** (5< *n*:k < 10 & 10=<e:k) or (10<=*n*:k & 5<e:k<10). **Serious concern:** 5<*n*:k<10 and 5<e:k<10. **Critical concern:** *n*:k<5 or e:k <5.
Other	Judged on a case-by-case basis.

Broader issues with evaluator burden and low interrater reliability for ROBINS-I and other assessment tools for nonrandomized studies have been noted elsewhere (Jeyaraman et al., 2020; Minozzi et al., 2019).

Changes to Our Determinations About Studies' Inclusion and Quality

In addition to reviewing the new studies that we found through our expanded search time frame, we reassessed the 152 studies from the 2023 report to determine whether there were changes in our assessment of their methodological quality. Given the revisions to our quality assessment tool, this reassessment led to a large number of changes in how we discussed the methodological quality of various analyses across multiple studies. With two exceptions (described in the following section), these changes did not lead us to substantively revise our criteria and thus we do not describe them fully here, although Appendix B includes a table that, for each effect estimate extracted as part of this review, compares our methodological quality designation from the previous edition of this report (Smart et al., 2023) with our methodological quality designation used in this report.

Under our new quality assessment criteria, we included a designation of *high methodological quality* when effect estimates were based on analyses that, for all of our quality assessment domains (see Tables A.1 and A.2), did not meet our criteria for even minor concerns. We highlight those studies and the policies and outcomes evaluated within in Table A.3. While a designation of high methodological quality does not indicate that the studies have no issues, these represent the more methodologically rigorous studies that evaluated the effects of our 18 included firearm policies on our eight included outcomes.

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 third edition of this report.

Our downgrading of the methodological quality of the estimates in Luca, Malhotra, and Poliquin (2017) led us to decrease our confidence in the evidence for the effects of permit-to-purchase requirements on suicide outcomes. Specifically, we downgraded our conclusions as follows:

- permit-to-purchase laws and suicides:
  - previously: limited evidence for decreasing total and firearm suicides among adults
  - revised: inconclusive evidence for effects on total suicides or firearm suicides.

TABLE A.3

Results from Studies with Analyses Rated as Being of High Methodological Quality

Study	Policy	Outcome	IRR [95% CI]
Anderson, Sabia, and Tekin (2021)	Child-access prevention	Firearm murder, offender <18	0.83 [0.71–0.98]
Degli Esposti et al. (2022)	Stand-your-ground	Total homicide	1.08 [1.04–1.12]
Degli Esposti et al. (2022)	Stand-your-ground	Firearm homicide	1.08 [1.03–1.13]
McClellan and Tekin (2017)	Stand-your-ground	Firearm homicide	1.08 [1.01–1.15]
Donohue et al. (2023)	Shall-issue	Violent crime	1.20 [1.07–1.36]
Van der Wal (2022)	Shall-issue	Violent crime	1.08 [1.04–1.11]
Hepburn et al. (2004)	Shall-issue	Total homicide	1.01 [0.94–1.10]
Van der Wal (2022)	Shall-issue	Murder/manslaughter	1.06 [1.01–1.11]
French and Heagerty (2008)	Shall-issue	Firearm homicide	1.06 [1.00–1.12]
Aneja, Donohue, and Zhang (2014)	Shall-issue	Aggravated assault	1.08 [0.99–1.18]
Van der Wal (2022)	Shall-issue	Aggravated assault	1.07 [1.03–1.11]
Van der Wal (2022)	Shall-issue	Rape	1.01 [0.98–1.04]
Van der Wal (2022)	Shall-issue	Robbery	1.11 [1.06–1.17]
Luca, Malhotra, and Poliquin (2017)	Waiting period	Total homicide	0.88 [0.80–0.97]
Luca, Malhotra, and Poliquin (2017)	Waiting period	Firearm homicide	0.83 [0.72–0.95]
Luca, Malhotra, and Poliquin (2017)	Waiting period	Total suicide	0.98 [0.96–1.00]
Luca, Malhotra, and Poliquin (2017)	Waiting period	Firearm suicide	0.93 [0.90–0.96]
Raissian (2016)	Domestic violence misdemeanor prohibition and surrender	Firearm intimate partner homicide	0.90 [0.81–0.99]

NOTE: We also rated as high methodological quality the analysis of the federal assault weapon ban conducted by Koper (2004), which compared prices of banned assault weapons with a comparison set of weapons, and the analysis of the relationship between concealed-carry laws and violent crime, rape, and aggravated assault by Shi and Lee (2018); however, these analyses did not support the construction of incidence rate ratios (IRRs) and associated confidence intervals (CIs).

Our upgrading of the methodological quality of the estimates in Raifman et al. (2020) and Rosengart et al. (2005) led us to increase our confidence in the evidence for the effects of minimum age of purchase requirements on suicide outcomes. Specifically, we upgraded our conclusions as follows:

- minimum age laws and suicides:
  - previously: Purchasing limits set at age 21 or older have inconclusive evidence for effects on total suicides and moderate evidence for decreasing firearm suicides among people under age 25
  - revised: Purchasing limits set at age 21 or older have limited evidence for reducing total suicides among people ages 18 to 20 and supportive evidence for decreasing firearm suicides among people under age 25.

For five of the gun policies that we examined, the new literature that our search identified was sufficient to upgrade our previous conclusions to limited, moderate, or supportive evidence, as follows:

- prohibitions for individuals subject to domestic violence restraining orders and violent crime:
  - previously: inconclusive evidence for effects of laws prohibiting those with domestic violence misdemeanor convictions from possessing a firearm on total and firearm-related intimate partner homicides
  - revised: limited evidence that domestic violence misdemeanor prohibitions decrease firearm-related intimate partner homicides

- extreme-risk protection orders and suicides
  - previously: no studies
  - revised: limited evidence that extreme-risk protection orders may reduce total and firearm suicides

- permit-to-purchase laws and mass shootings:
  - previously: inconclusive evidence for the effects of permit-to-purchase laws on mass shooting incidents and fatalities
  - revised: limited evidence for permit-to-purchase laws decreasing mass shooting incidents and fatalities

- stand-your-ground laws and violent crime:
  - previously: moderate evidence that stand-your-ground laws increase firearm homicide rates
  - revised: supportive evidence that stand-your-ground laws increase firearm homicide rates

- concealed-carry laws and police shootings:
  - previously: no studies
  - revised: limited evidence that permitless-carry laws increase fatal police shootings

- concealed-carry laws and violent crime:
  - previously: limited evidence that concealed-carry laws increase overall violent crime; inconclusive evidence about the effects of concealed-carry laws on rates of rape, robbery, and aggravated assault
  - revised: supportive evidence that concealed-carry laws increase overall violent crime, moderate evidence that such laws increase assaults, limited evidence that such laws increase robberies, and inconclusive evidence about the effects of such laws on rates of rape.

For six of the gun policies examined, our previous searches identified no studies, but the updated search identified new research that yielded inconclusive evidence. The policy-outcome pairs that we revised from no studies to inconclusive evidence are as follows:

- surrender of firearms by prohibited possessors and suicides: inconclusive evidence for the effect on firearm suicides of laws requiring surrender of firearms by individuals prohibited from possessing firearms
- extreme-risk protection orders and unintentional injury or death: inconclusive evidence for the effect of extreme-risk protection orders on unintentional firearm injury deaths
- extreme-risk protection orders and mass shootings: inconclusive evidence for the effect of extreme-risk protection orders on mass shooting incidents or fatalities
- background check requirements and police shootings: inconclusive evidence for the effects of background check requirements on fatal police shootings
- firearm safety training requirements and mass shootings: inconclusive evidence for the effects of firearm safety training requirements on mass shootings
- sales reporting, recording, and registration requirements and suicides: inconclusive evidence for the effects of such requirements on firearm suicides
- sales reporting, recording, and registration requirements and violent crime: inconclusive evidence for the effects of such requirements on firearm homicides
- assault weapon bans and police shootings: inconclusive evidence for the effect of assault weapon bans on fatal police shootings.

Finally, for one of the gun policies examined, the new studies identified in our review update led us to revise our conclusions to accommodate greater specificity on the evidence for particular provisions of the policies. This change is as follows:

- assault weapon bans and mass shootings:
  - previously: inconclusive effects of assault weapon bans on mass shootings; limited evidence that bans on high-capacity magazines reduce mass shootings

- revised: limited evidence that assault weapon bans may reduce school shooting injuries; limited evidence that bans on high-capacity magazines may reduce mass shootings.

Tables A.4 through A.10 highlight changes to the number of studies included in our review and to the overall conclusions made since the prior edition of this report was published. Each table presents these changes for one of our examined outcomes. In both our prior review and this update, we did not identify any studies that met our inclusion criteria and evaluated the effects of policies on hunting and recreation, so there is no table for this outcome.

TABLE A.4

Changes to the Number of Studies and Conclusions, Suicide

	2022 Review		Updated 2024 Review	
Policy	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	8	**Purchasing:** Inconclusive evidence for total suicides; moderate evidence decreasing firearm suicides among people under age 25 **Possessing:** Inconclusive evidence for total suicides; limited evidence for decreasing firearm suicides among people under age 25	9	**Purchasing:** Limited evidence for decreasing total suicides among people aged 18 to 20; supportive evidence for decreasing firearm suicides among people under age 25 **Possessing:** Inconclusive evidence for total suicides; limited evidence for decreasing firearm suicides among people under age 25
Prohibitions associated with mental illness	3	Inconclusive evidence for total and firearm suicides	4	Inconclusive evidence for total and firearm suicides
Prohibitions associated with domestic violence	2	Inconclusive evidence for total and firearm suicides	3	Inconclusive evidence for total and firearm suicides
Surrender of firearms by prohibited possessors	0	No studies	2	Inconclusive evidence for firearm suicides
Extreme-risk protection orders	2	Inconclusive evidence for total and firearm suicides	5	Limited evidence for decreasing total and firearm suicides
Background checks	8	Inconclusive evidence for total and firearm suicides	11	Inconclusive evidence for total and firearm suicides
Licensing and permitting requirements	5	Limited evidence for decreasing total and firearm suicides among adults	8	Inconclusive evidence for total and firearm suicides
Waiting periods	4	Limited evidence for decreasing total suicides; moderate evidence for decreasing firearm suicides	6	Limited evidence for decreasing total suicides; moderate evidence for decreasing firearm suicides
Firearm safety training requirements	0	No studies	0	No studies
Lost or stolen firearm reporting requirements	0	No studies	0	No studies

Table A.4—Continued

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Firearm sales reporting, recording, and registration requirements	0	No studies	1[a]	Inconclusive evidence for firearm suicides
Bans on the sale of assault weapons and high-capacity magazines	1	Inconclusive evidence for total and firearm suicides	1	Inconclusive evidence for total and firearm suicides
Bans on low-quality handguns	2	Inconclusive evidence for total and firearm suicides	2	Inconclusive evidence for total and firearm suicides
Stand-your-ground laws	4	Inconclusive evidence for total and firearm suicides	5	Inconclusive evidence for total and firearm suicides
Child-access prevention laws	11	Moderate evidence for decreasing total suicides among young people; supportive evidence for decreasing firearm self-injuries and firearm suicides among young people; inconclusive evidence for decreasing total and firearm suicides overall	11	Moderate evidence for decreasing total suicides among young people; supportive evidence for decreasing firearm self-injuries and firearm suicides among young people; inconclusive evidence for decreasing total and firearm suicides overall
Concealed-carry laws	5	**Permitless carry:** Inconclusive evidence for total and firearm suicides **Shall issue:** Inconclusive evidence for total and firearm suicides	7	**Permitless carry:** Inconclusive evidence for total and firearm suicides **Shall issue:** Inconclusive evidence for total and firearm suicides
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

[a] One study assessed the relationship of this policy with all firearm deaths, a large proportion of which are firearm suicides, but the study did not examine this policy's relationship with firearm suicides specifically.

TABLE A.5

Changes to the Number of Studies and Conclusions, Violent Crime

	2022 Review		Updated 2024 Review	
Policy	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	5	**Purchasing:** Inconclusive evidence for total and firearm homicides **Possessing:** Inconclusive evidence for total homicides, firearm homicides, robberies, assaults, and rapes	6	**Purchasing:** Inconclusive evidence for total and firearm homicides **Possessing:** Inconclusive evidence for total homicides, firearm injury hospitalizations, firearm homicides, robberies, assaults, and rapes
Prohibitions associated with mental illness	3	Limited evidence for decreasing violent crime; inconclusive evidence for total and firearm homicides	4	Limited evidence for decreasing violent crime; inconclusive evidence for total and firearm homicides
Prohibitions associated with domestic violence	9	Inconclusive evidence for total homicides, firearm homicides, robberies, assaults, and rapes; moderate evidence that laws prohibiting those with domestic violence restraining orders from possessing a firearm decrease total and firearm-related intimate partner homicides; limited evidence that laws prohibiting those with domestic violence restraining orders from possessing a firearm decrease other (non–intimate partner) types of homicides; inconclusive evidence for how firearm prohibitions for those convicted of stalking or of misdemeanor domestic violence affect total and firearm-related intimate partner homicides; limited evidence that prohibitions associated with stalking misdemeanors increase total intimate partner homicides	16	Inconclusive evidence for total homicides, firearm homicides, and nonfatal violent crime; moderate evidence that laws prohibiting those with domestic violence restraining orders from possessing a firearm decrease total and firearm-related intimate partner homicides; limited evidence that laws prohibiting those with domestic violence restraining orders from possessing a firearm decrease other (non–intimate partner) types of homicides; limited evidence that domestic violence misdemeanor prohibitions decrease firearm-related intimate partner homicides; inconclusive evidence for how firearm prohibitions for those convicted of stalking affect total and firearm-related intimate partner homicides; limited evidence that prohibitions associated with stalking misdemeanors increase total intimate partner homicides

Table A.5—Continued

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Surrender of firearms by prohibited possessors	6	Inconclusive evidence for surrender laws alone on total homicides, intimate partner homicides, robberies, assaults, and rapes; moderate evidence that surrender laws, in combination with prohibitions related to violent offenses, reduce total intimate partner homicides; limited evidence that surrender laws, in combination with prohibitions related to violent offenses, reduce firearm-related intimate partner homicides	11	Inconclusive evidence for surrender laws alone on total homicides, intimate partner homicides, robberies, assaults, and rapes; moderate evidence that surrender laws, in combination with prohibitions related to violent offenses, reduce total intimate partner homicides; limited evidence that surrender laws, in combination with prohibitions related to violent offenses, reduce firearm-related intimate partner homicides
Extreme-risk protection orders	1	Inconclusive evidence for total and firearm homicides	4	Inconclusive evidence for total and firearm homicides
Background checks	21	Inconclusive evidence for dealer background checks and total homicides or total violent crime; moderate evidence that more restrictive background check laws, including private seller background check laws, decrease total homicides; moderate evidence that dealer background check laws decrease firearm homicides; limited evidence for the effect of private-seller background checks on firearm homicides	27	Inconclusive evidence for dealer background checks and total homicides or total violent crime; moderate evidence that more restrictive background check laws, including private seller background check laws, decrease total homicides; moderate evidence that dealer background check laws decrease firearm homicides; limited evidence for the effect of private-seller background checks on firearm homicides
Licensing and permitting requirements	12	Inconclusive evidence for total and firearm homicides	18	Inconclusive evidence for total and firearm homicides
Waiting periods	7	Moderate evidence for decreasing total homicides; limited evidence for decreasing firearm homicides; inconclusive evidence for intimate partner homicides	9	Moderate evidence for decreasing total homicides; limited evidence for decreasing firearm homicides; inconclusive evidence for intimate partner homicides
Firearm safety training requirements	1	Inconclusive evidence for total homicides	1	Inconclusive evidence for total homicides

Table A.5—Continued

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	0	No studies	1[a]	Inconclusive evidence for firearm homicides
Bans on the sale of assault weapons and high-capacity magazines	5	Inconclusive evidence for total and firearm homicides	7	Inconclusive evidence for total and firearm homicides
Bans on low-quality handguns	5	Inconclusive evidence for total and firearm homicides	5	Inconclusive evidence for total and firearm homicides
Stand-your-ground laws	16	Moderate evidence for increasing total homicides; supportive evidence for increasing firearm homicides; limited evidence for increasing other violent crimes	22	Supportive evidence for increasing total and firearm homicides; limited evidence for increasing other violent crimes
Child-access prevention laws	8	Supportive evidence for decreasing assaults (all ages) and firearm homicides among young people; inconclusive evidence for violent crime, firearm homicides, robberies, and rapes	8	Supportive evidence for decreasing assaults (all ages) and firearm homicides among young people; inconclusive evidence for violent crime, firearm homicides, robberies, and rapes
Concealed-carry laws	52	**Shall issue:** Supportive evidence for increasing total and firearm homicides; limited evidence for increasing overall violent crime **Permitless carry:** Inconclusive evidence for total homicides, firearm homicides, and violent crimes	62	**Shall issue:** Supportive evidence for increasing total homicides, firearm homicides, and total violent crime; moderate evidence for increasing assaults; limited evidence for increasing robberies **Permitless carry:** Inconclusive evidence for total homicides, firearm homicides, and violent crimes
Gun-free zones	1	Inconclusive evidence for effects on violent crimes on college campuses	1	Inconclusive evidence for effects on violent crimes on college campuses
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

[a] One study assessed the relationship of this policy with all firearm deaths, a large proportion of which are firearm suicides, but this study did not examine this policy's relationship with firearm suicides specifically.

TABLE A.6

Changes to the Number of Studies and Conclusions, Unintentional Injuries and Deaths

	2022 Review		Updated 2024 Review	
Policy	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	1	Possessing: Inconclusive	1	Possessing: Inconclusive
Prohibitions associated with mental illness	0	No studies	0	No studies
Prohibitions associated with domestic violence	0	No studies	0	No studies
Surrender of firearms by prohibited possessors	0	No studies	0	No studies
Extreme-risk protection orders	0	No studies	1	Inconclusive
Background checks	0	No studies	0	No studies
Licensing and permitting requirements	0	No studies	0	No studies
Waiting periods	0	No studies	0	No studies
Firearm safety training requirements	0	No studies	0	No studies
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	0	No studies	0	No studies
Bans on the sale of assault weapons and high-capacity magazines	0	No studies	0	No studies
Bans on low-quality handguns	0	No studies	0	No studies
Stand-your-ground laws	0	No studies	0	No studies
Child-access prevention laws	7	Limited evidence for decreasing unintentional firearm injuries and deaths among adults; supportive evidence for decreasing unintentional firearm injuries and deaths among children	7	Limited evidence for decreasing unintentional firearm injuries and deaths among adults; supportive evidence for decreasing unintentional firearm injuries and deaths among children
Concealed-carry laws	2	**Shall issue:** Inconclusive evidence for unintentional firearm injuries and deaths	2	**Shall issue:** Inconclusive evidence for unintentional firearm injuries and deaths
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

TABLE A.7

Changes to the Number of Studies and Conclusions, Mass Shootings

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	1	**Purchasing:** Inconclusive	2	**Purchasing:** Inconclusive
Prohibitions associated with mental illness	0	No studies	0	No studies
Prohibitions associated with domestic violence	1	Inconclusive	2	Inconclusive
Surrender of firearms by prohibited possessors	1	Inconclusive for the effect of laws requiring surrender of firearms by individuals subject to domestic violence restraining orders	2	Inconclusive
Extreme-risk protection orders	0	No studies	1	Inconclusive
Background checks	4	Inconclusive	6	Inconclusive
Licensing and permitting requirements	3	Inconclusive	5	Limited evidence for decreasing mass shooting incidents and fatalities
Waiting periods	2	Inconclusive	3	Inconclusive
Firearm safety training requirements	0	No studies	1	Inconclusive
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	1	Inconclusive	1	Inconclusive
Bans on the sale of assault weapons and high-capacity magazines	5	**High-capacity magazine ban:** Limited evidence for decreasing mass shooting incidents and fatalities **Assault weapon bans:** Inconclusive evidence	7	**High-capacity magazine ban:** Limited evidence for decreasing mass shooting incidents and fatalities **Assault weapon bans:** Limited evidence for decreasing school shooting injuries
Bans on low-quality handguns	0	No studies	0	No studies

Table A.7—Continued

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Stand-your-ground laws	1	Inconclusive	1	Inconclusive
Child-access prevention laws	2	Inconclusive	3	Inconclusive
Concealed-carry laws	7	**Shall issue:** Inconclusive **Permitless carry:** Inconclusive	9	**Shall issue:** Inconclusive **Permitless carry:** Inconclusive
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

TABLE A.8

Changes to the Number of Studies and Conclusions, Police Shootings

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	0	No studies	0	No studies
Prohibitions associated with mental illness	0	No studies	0	No studies
Prohibitions associated with domestic violence	0	No studies	0	No studies
Surrender of firearms by prohibited possessors	0	No studies	0	No studies
Extreme-risk protection orders	0	No studies	0	No studies
Background checks	0	No studies	1	Inconclusive
Licensing and permitting requirements	0	No studies	0	No studies
Waiting periods	0	No studies	0	No studies
Firearm safety training requirements	0	No studies	0	No studies
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	0	No studies	0	No studies
Bans on the sale of assault weapons and high-capacity magazines	0	No studies	1	Inconclusive
Bans on low-quality handguns	0	No studies	0	No studies
Stand-your-ground laws	0	No studies	0	No studies
Child-access prevention laws	0	No studies	0	No studies
Concealed-carry laws	0	No studies	1	**Permitless carry:** Limited evidence for increasing police shootings
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

TABLE A.9

Changes to the Number of Studies and Conclusions, Defensive Gun Use

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	0	No studies	0	No studies
Prohibitions associated with mental illness	0	No studies	0	No studies
Prohibitions associated with domestic violence	0	No studies	0	No studies
Surrender of firearms by prohibited possessors	0	No studies	0	No studies
Extreme-risk protection orders	0	No studies	0	No studies
Background checks	0	No studies	0	No studies
Licensing and permitting requirements	0	No studies	0	No studies
Waiting periods	0	No studies	0	No studies
Firearm safety training requirements	0	No studies	0	No studies
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	0	No studies	0	No studies
Bans on the sale of assault weapons and high-capacity magazines	0	No studies	0	No studies
Bans on low-quality handguns	0	No studies	0	No studies
Stand-your-ground laws	3	Inconclusive	3	Inconclusive
Child-access prevention laws	0	No studies	0	No studies
Concealed-carry laws	0	No studies	0	No studies
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

TABLE A.10

Changes to the Number of Studies and Conclusions, Gun Industry

	2022 Review		Updated 2024 Review	
Policy	No. of Studies	Conclusion	No. of Studies	Conclusion
Minimum age requirements	0	No studies	0	No studies
Prohibitions associated with mental illness	0	No studies	0	No studies
Prohibitions associated with domestic violence	0	No studies	0	No studies
Surrender of firearms by prohibited possessors	0	No studies	0	No studies
Extreme-risk protection orders	0	No studies	0	No studies
Background checks	2	Inconclusive for federal firearms licensees exiting the market	2	Inconclusive for federal firearms licensees exiting the market and prevalence of firearm manufacturers
Licensing and permitting requirements	2	Inconclusive for firearm purchases	2	Inconclusive for firearm purchases and firearm manufacturers
Waiting periods	2	Inconclusive for gun ownership, firearm manufacturing or retailers, and firearm purchases	2	Inconclusive for gun ownership, firearm manufacturing or retailers, and firearm purchases
Firearm safety training requirements	1	Inconclusive for firearm purchases	1	Inconclusive for firearm purchases
Lost or stolen firearm reporting requirements	0	No studies	0	No studies
Firearm sales reporting, recording, and registration requirements	1	Inconclusive for firearm purchases	1	Inconclusive for firearm purchases
Bans on the sale of assault weapons and high-capacity magazines	1	Limited evidence for increasing prices of banned firearms in the short term	2	Limited evidence for increasing prices of banned firearms in the short term; inconclusive evidence for effects on sales
Bans on low-quality handguns	1	Inconclusive for firearm purchases	1	Inconclusive for firearm purchases
Stand-your-ground laws	1	Inconclusive evidence for the effects on firearm ownership and purchases	1	Inconclusive evidence for the effects on firearm ownership and purchases

Table A.10—Continued

Policy	2022 Review		Updated 2024 Review	
	No. of Studies	Conclusion	No. of Studies	Conclusion
Child-access prevention laws	1	Inconclusive for firearm manufacturers or retailers	2	Inconclusive for firearm manufacturers or retailers and firearm purchases
Concealed-carry laws	4	May issue: Inconclusive for firearm purchases Shall issue: Inconclusive for gun ownership and firearm purchases Permitless carry: Inconclusive for firearm purchases	4	May issue: Inconclusive for firearm purchases Shall issue: Inconclusive for gun ownership and firearm purchases Permitless carry: Inconclusive for firearm purchases
Gun-free zones	0	No studies	0	No studies
Laws allowing armed staff in K–12 schools	0	No studies	0	No studies

Appendix A References

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin, "Child Access Prevention Laws and Juvenile Firearm-Related Homicides," *Journal of Urban Economics*, Vol. 126, November 2021.

Aneja, Abhay, John J. Donohue III, and Alexandria Zhang, *The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy*, Stanford Law School, Olin Working Paper No. 461, December 1, 2014.

Conley, Timothy G., and Christopher R. Taber, "Inference with 'Difference in Differences' with a Small Number of Policy Changes," *Review of Economics and Statistics*, Vol. 93, No. 1, 2011.

Degli Esposti, Michelle, Douglas J. Wiebe, Antonio Gasparrini, and David K. Humphreys, "Analysis of 'Stand Your Ground' Self-Defense Laws and Statewide Rates of Homicides and Firearm Homicides," *JAMA Network Open*, Vol. 5, No. 2, 2022.

Donohue, John J., Samuel V. Cai, Matthew V. Bondy, and Philip J. Cook, *Why Does Right-to-Carry Cause Violent Crime to Increase?* National Bureau of Economic Research, Working Paper No. 30190, June 2023.

French, B., and P. J. Heagerty, "Analysis of Longitudinal Data to Evaluate a Policy Change," *Statistics in Medicine*, Vol. 27, No. 24, 2008.

Hepburn, Lisa, Matthew Miller, Deborah Azrael, and David Hemenway, "The Effect of Nondiscretionary Concealed Weapon Carrying Laws on Homicide," *Journal of Trauma: Injury, Infection, and Critical Care*, Vol. 56, No. 3, 2004.

Jeyaraman, Maya M., Rasheda Rabbani, Leslie Copstein, Reid C. Robson, Nameer Al-Yousif, Michelle Pollock, Jun Xia, Chakrapani Balijepali, Kimberly Hofer, Samer Mansour, Mir S. Fazeli, Mohammed T. Ansari, Andrea C. Tricco, and Ahmed M. Abou-Setta, "Methodologically Rigorous Risk of Bias Tools for Nonrandomized Studies Had Low Reliability and High Evaluator Burden," *Journal of Clinical Epidemiology*, Vol. 128, December 2020.

Koper, Christopher S., *Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence 1994–2003*, National Institute of Justice, 2004.

Luca, Michael, Deepak Malhotra, and Christopher Poliquin, "Handgun Waiting Periods Reduce Gun Deaths," *Proceedings of the National Academy of Sciences*, Vol. 114, No. 46, 2017.

McClellan, Chandler, and Erdal Tekin, "Stand Your Ground Laws, Homicides, and Injuries," *Journal of Human Resources*, Vol. 52, No. 3, 2017.

Minozzi, Silvia, Michela Cinquini, Silvia Gianola, Greta Castellini, Chiara Gerardi, and Rita Banzi, "Risk of Bias in Nonrandomized Studies of Interventions Showed Low Inter-Rater Reliability and Challenges in Its Application," *Journal of Clinical Epidemiology*, Vol. 112, August 2019.

Mullahy, John, and Edward C. Norton, *Why Transform Y? A Critical Assessment of Dependent-Variable Transformations in Regression Models for Skewed and Sometimes-Zero Outcomes*, National Bureau of Economic Research, Working Paper No. 30735, December 2022.

Peduzzi, Peter, John Concato, Alvan R. Feinstein, and Theodore R. Holford, "Importance of Events per Independent Variable in Proportional Hazards Regression Analysis II. Accuracy and Precision of Regression Estimates," *Journal of Clinical Epidemiology*, Vol. 4, No. 12, December 1995.

Raifman, Julia, Elysia Larson, Colleen L. Barry, Michael Siegel, Michael Ulrich, Anita Knopov, and Sandro Galea, "State Handgun Purchase Age Minimums in the US and Adolescent Suicide Rates: Regression Discontinuity and Difference-in-Differences Analyses," *BMJ*, Vol. 370, 2020.

Raissian, Kerri M., "Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?" *Journal of Policy Analysis and Management*, Vol. 35, No. 1, Winter 2016.

Riley, Richard D., Kym I. E. Snell, Joie Ensor, Danielle L. Burke, Frank E. Harrell, Jr., Karel G. M. Moons, and Gary S. Collins, "Minimum Sample Size for Developing a Multivariable Prediction Model: Part I–Continuous Outcomes," *Statistics in Medicine*, Vol. 38, No. 7, March 30, 2019.

Rosengart, M., P. Cummings, A. Nathens, P. Heagerty, R. Maier, and F. Rivara, "An Evaluation of State Firearm Regulations and Homicide and Suicide Death Rates," *Injury Prevention*, Vol. 11, No. 2, 2005.

Schell, Terry L., Beth Ann Griffin, and Andrew R. Morral, *Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study*, RAND Corporation, RR-2685-RC, 2018. As of April 12, 2024: https://www.rand.org/pubs/research_reports/RR2685.html

Schell, Terry L., Rosanna Smart, and Andrew R. Morral, *Suggestions for Estimating the Effects of State Gun Policies: Commentary on Four Methodological Problems in the Current Literature*, RAND Corporation, RR-A243-1, 2022. As of April 12, 2024: https://www.rand.org/pubs/research_reports/RRA243-1.html

Shi, Wei, and Lung-Fei Lee, "The Effects of Gun Control on Crimes: A Spatial Interactive Fixed Effects Approach," *Empirical Economics*, Vol. 55, No. 1, 2018.

Smart, Rosanna, Andrew R. Morral, Rajeev Ramchand, Amanda Charbonneau, Jhacova Williams, Sierra Smucker, Samantha Cherney, and Lea Xenakis, *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States, Third Edition*, RAND Corporation, RR-A243-4, 2023. As of January 10, 2023: https://www.rand.org/pubs/research_reports/RRA243-4.html

Sterne, Jonathan A. C., Julian P. T. Higgins, Roy G. Elbers, and Barney C. Reeves, *Risk of Bias in Non-Randomized Studies of Interventions (ROBINS-I): Detailed Guidance*, updated October 20, 2016.

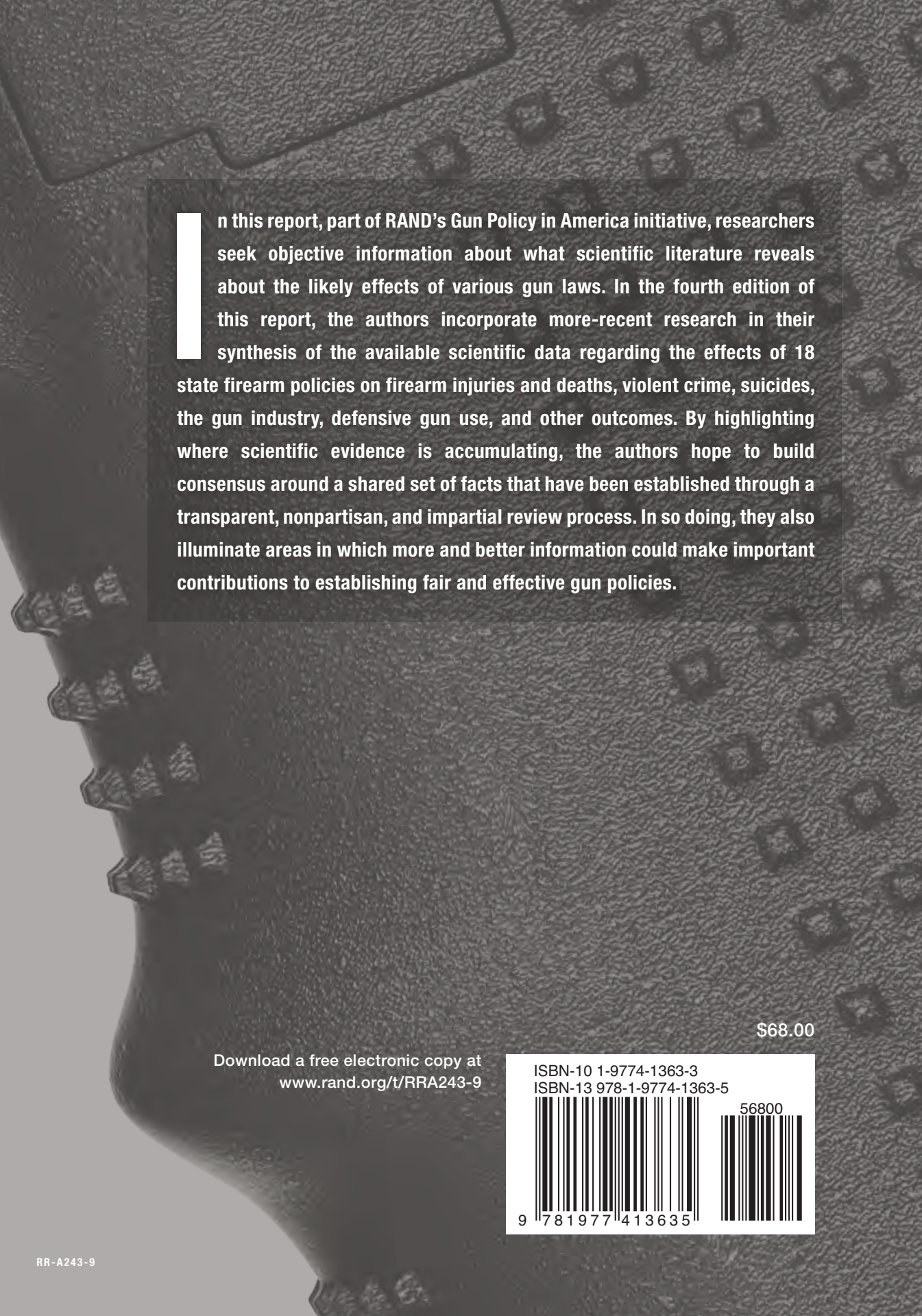
Van Der Wal, Willem M., "Marginal Structural Models to Estimate Causal Effects of Right-to-Carry Laws on Crime," *Statistics and Public Policy*, Vol. 9, No. 1, 2022.

Yakubovich, Alexa R., Michelle Degli Esposti, Brittany C. L. Lange, G. J. Melendez-Torres, Alpa Parmar, Douglas J. Wiebe, and David K. Humphreys, "Effects of Laws Expanding Civilian Rights to Use Deadly Force in Self-Defense on Violence and Crime: A Systematic Review," *American Journal of Public Health*, Vol. 111, No. 4, 2021.

APPENDIX B

Source Data Used to Produce the Forest Plot Figures

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 2. The data used to construct these figures, as well as the full data extraction file with information from each study, are provided in an online appendix to this report, available at www.rand.org/t/RRA243-9.



In this report, part of RAND's Gun Policy in America initiative, researchers seek objective information about what scientific literature reveals about the likely effects of various gun laws. In the fourth edition of this report, the authors incorporate more-recent research in their synthesis of the available scientific data regarding the effects of 18 state firearm policies on firearm injuries and deaths, violent crime, suicides, 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 in which more and better information could make important contributions to establishing fair and effective gun policies.

$68.00

Download a free electronic copy at
www.rand.org/t/RRA243-9

ISBN-10 1-9774-1363-3
ISBN-13 978-1-9774-1363-5

RR-A243-9