The Magnitude and Sources of Disagreement Among Gun Policy Experts

Andrew R. Morral, Terry L. Schell, Margaret Tankard

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Preface

Effective gun policies must balance the constitutional right to bear arms and public interest in gun ownership with concerns about public health and safety. However, current efforts to craft legislation related to guns are hampered by a paucity of reliable information about the effects of such policies. To help address this problem, the RAND Corporation launched the Gun Policy in America initiative. The primary goal of the project is to create resources where policymakers and the general public can access unbiased information that facilitates development of fair and effective firearm policies.

This report is one of several research products stemming from the initiative. The report describes a survey of policy experts to identify where access to reliable data would be most useful in resolving policy debates. Based on the results, we also developed a tool that allows users to explore how different combinations of policies are likely to affect a range of outcomes related to gun ownership. Another major study component is a synthesis of the available scientific evidence on how gun policies affect a range of outcomes. In a parallel effort, RAND analyzed statistical approaches currently used to estimate outcomes related to gun policies and identified optimal approaches; we will use the results of these analyses to produce new and, we believe, more-reliable estimates of the effects of state laws.

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

This 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.

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Summary

The effects of gun policies, though frequently debated, have rarely been the subject of rigorous scientific evaluation in comparison with other policies affecting public safety, health, and recreation (RAND Corporation, 2018b; Stark and Shah, 2017). Without strong scientific evidence of the effects of laws, policymakers and the public rely heavily on what advocates or social scientists believe the effects are most likely to be, taking into account their studies of similar laws, their general expertise, and their reputations in the field. This makes gun policy experts' opinions about the true effects of policies an important influence on gun policy debates and decisions.

In this report, we describe the results of a survey in which gun policy experts estimated the likely effects of 15 policies on 12 societally important outcomes. We use these and other responses to

1. establish both the diversity of beliefs among gun policy experts about the true effects of gun laws and where experts are in more or less agreement on those effects
2. evaluate whether differences in the policies favored by experts result from disagreements about the policies' true effects or disagreements in experts' policy objectives or values
3. develop an online policy comparison tool that allows users to explore the effects of different combinations of gun policies that would be expected by experts with different perspectives.

This study builds, in several ways, on earlier efforts to identify experts' views on the effectiveness of gun policies. First, our sampling procedure actively sought competing and alternative views on gun policy so that we could characterize major differences in the views of groups with similar perspectives. We did not attempt to establish a consensus or average estimate of experts' views on the likely effects of policies.

Second, we asked respondents to rate the effects of laws not just on homicides or violent crime but on 12 outcomes representing many of the concerns most often raised in policy debates on gun laws, such as how laws affect privacy and constitutional rights to gun ownership.

Third, instead of using a one-sided qualitative measure of effectiveness, these judgments were made on a two-sided, quantitative, percent-change scale. That is, experts could indicate their belief that laws might worsen some outcomes, not just improve them, as these types of questions have frequently been posed.

Finally, we asked experts to indicate how favorably they regarded each policy. We use this information to characterize different schools of thought about the likely effects of policies, as well as to examine whether differing favorability toward policies reflects different policy objectives or values or, instead, different views on factual matters concerning the effects that the policies are likely to have on important outcomes (e.g., suicides, homicides, gun sales, mass shootings, and the rights of gun owners). In addition, we use experts' estimates of the likely effects of gun laws to construct an online policy comparison tool (RAND Corporation, 2018a) that allows users to explore the likely national and state-level effects of different combinations of gun laws on 12 outcomes. This report documents the development of this comparison tool and the data it uses.

Key Findings

Classes of experts. We identified clusters of experts with similar patterns of *favorability ratings* (that is, how respondents rated their "overall opinion" of a policy) across the 15 policies we examined. This resulted in two classes of experts who were sharply differentiated not just on their favorability ratings (which were used to identify clusters) but also on their ratings of which advocacy or membership organizations had gun policy positions closest to their own.

The first class of experts preferred such policies as a stand your ground law, permitless carry, and the elimination of gun-free zones. Members of this group (n = 16) reported that their own views on gun policy were strongly aligned with those of organizations like the National Rifle Association, the National Sport Shooting Foundation, the Second Amendment Foundation, and Gun Owners of America. For ease of reference, we labeled this group the *permissive* class, comprising experts who favor more-permissive regulatory approaches to gun ownership and use.

The second class of experts (n = 79) preferred such policies as universal background checks, requiring a license to purchase a firearm or ammunition, and surrender of firearms by prohibited possessors.[1] Members of this class of experts had gun policy positions that they indicated were closely aligned with organizations like the Brady Campaign to Prevent Gun Violence, Mayors Against Illegal Guns/Everytown for Gun Safety, the Violence Policy Center, and Coalition to Stop Gun Violence. We labeled this group the *restrictive* class, comprising experts who favor more-restrictive regulatory approaches to gun ownership and use. Because this was not a representative survey, the

[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.

difference in the sizes of these groups provides no information about the relative numbers of experts in the community whose views align with one or the other perspective.

Areas of relative agreement between expert classes. Despite often diametric favorability ratings and organizational affinities, the two groups' estimates of the likely effects of 15 policies on a wide range of outcomes related to gun ownership often agreed on the likely direction of effects. Indeed, across 134 judgments about the effects of the policies, only 12 times did the median judgment for each group disagree on the direction of the effect, where, for instance, one group thought the effect of a law would be to increase the rate of homicides, and the other thought the law would decrease homicides. More than half of these instances concerned two policies: permitless carry and elimination of gun-free zones. Those in the permissive class expected these policies to reduce firearm homicides; mass shootings; other violent crime; and, in the case of permitless carry, property crime. Those in the restrictive class expected the opposite.

Whereas the two classes of experts strongly disagreed on the merits of most policies, four policies generated substantially less disagreement between classes of experts in their overall favorability ratings: expanded mental health prohibitions (against gun ownership), required reporting of lost or stolen firearms, a media campaign to prevent child access, and surrender of firearms by prohibited possessors. For each of these policies, the median favorability rating of the group favoring more-permissive policies was neutral, and the median favorability rating of the group favoring more-restrictive policies was positive. There was relatively strong agreement on the direction and magnitude of expected effects of these policies on the outcomes examined, with three of the four showing stronger agreement between groups on the quantitative outcome measures than on any other policies, and all four being among the five policies for which there was the strongest agreement about their effects on the right to bear arms, individuals' privacy, and the satisfaction of gun ownership (the qualitatively measured outcomes).

Extrapolation of experts' views for combinations of policies. Using a series of assumptions for combining experts' effect estimates across multiple policies, we illustrate how the online policy comparison tool we developed can be used to explore the effects of combinations of laws, based on the expectations of each expert class. For instance, we illustrate what each class of experts might expect the combined effects to be of the four policies on which they have the greatest agreement, described in the previous paragraph. In this case, both groups would expect large reductions in firearm suicides, firearm homicides, mass shootings, and other outcomes, with relatively minor infringements on gun owners' privacy and constitutional rights or on the various satisfactions of gun ownership (e.g., collecting firearms, feeling more secure). We then show how adding a policy favored by experts in the permissive group but disfavored by those in the restrictive group could substantially moderate the few negative effects perceived by the permissive group with the earlier combination of four policies while nevertheless delivering large reductions in firearm suicides, firearm homicides, and mass shootings, as well as other improvements, according to the expectations of both groups.

Substitution effects. A striking result of the survey concerns the wide disparity between estimates made by the two expert groups about means substitution. That is, they disagree about the extent to which any reductions of firearm suicides and homicides attributable to a policy are undermined because individuals simply use other means to achieve those ends. The median respondent with more-permissive regulatory preferences indicated that if a policy successfully reduced a state's firearm suicides, 90 percent of the prevented suicides would still end as a suicide by some other means. In contrast, the median respondent with more-restrictive regulatory preferences responded that 20 percent would still end as a suicide. They produced the same stark differences in expected substitution of means for firearm homicides: 90 percent for those favoring permissive policies, and 20 percent for those favoring restrictive policies. Those who favor permissive policies view reductions of firearm suicides and homicides as largely futile efforts because these outcomes will continue, largely uninhibited, through other means. This wide disparity in views on means substitution effects may be an important impediment to reaching any consensus on firearm legislation.

Sources of disagreement between expert classes. Views on the merits of the policies we studied were strikingly polarized, with almost no overlap in favorability ratings between these two classes of experts. We examined whether these nearly diametrical perspectives result from differences in beliefs about the true effects of the policies or from differences of opinion about which outcomes matter most or should be the proper targets of gun policy.

We reasoned that if disagreements about the true effects of laws explain differences in policy favorability ratings, then, in a model predicting favorability ratings, estimates of the empirical effects of policies might explain favorability ratings in comparable ways for both groups of experts. On the other hand, if groups value some outcomes differently (say, reductions in homicides or protection of the right to bear arms), this would be revealed by the two expert classes placing different emphases or weights on outcomes in their favorability ratings.

Our results strongly suggest that the differing favorability ratings evident between the two expert classes are almost exclusively explained by differences in assessments of what the true effects of the policies will be, not by differences in which policy outcomes predict the experts' favorability judgments. Indeed, there was an overwhelming consensus between the groups that their preferred policies were those they saw as reducing firearm suicides and homicides. Secondary priorities in evaluating policies appear to be protecting privacy rights, facilitating participation in hunting and sport shooting, reducing mass shootings, and protecting gun rights.

Including interactions by class of expert provided almost no additional explanatory power in the model. When we reran the model without these interaction terms, model performance was barely degraded (explained variance declining from 0.71 to 0.70). This suggests that even though minor differences may exist in the policy goals for these two classes of experts, those differences had negligible associations with the experts' favorability ratings.

We cannot conclude that differences in beliefs about the true effects of policies *cause* differences in experts' favorability toward those policies. Our analyses are equally consistent with the possibility that one or both groups of experts bend their assessments of the likely effects to match their overall opinions of policies. Interestingly, however, our findings suggest that gun policy disagreements may not be primarily driven by differences in what each group is hoping to achieve through gun policies. Instead, experts who favor more-permissive policies and those who favor more-restrictive ones appear to have a broadly similar set of values or objectives that lead them to agree on what policymakers should attempt to achieve through improved gun policy. That is, whether or not the experts truly believe that the laws they favor will have those effects, the effects each group claims for the policies it favors suggest that both groups agree on what the objectives of gun policy should be and how much to weight each of the outcomes we examined. This may be a surprising finding to those on either side of gun policy debates who suspect that their opponents suffer from badly misplaced priorities, if not deep moral failings.

A practical implication of our findings is that scientific evidence may be useful in resolving disagreements among policy experts about the merits of different gun policies. We do not believe that longstanding and politically contentious disagreements about the true effects of gun policies will be easily overcome when better scientific evidence on those effects becomes available. Indeed, there is compelling evidence that the public and experts may be motivated to discount evidence that disagrees with their own views or the views of their social groups if accepting the implications of new evidence could damage their professional alliances, their status in the group, or their economic well-being (Kahan, 2016; Kahan et al., 2017; Koehler, 1993). Nevertheless, given that members of permissive and restrictive gun policy groups appear to share a set of objectives, and given that there is currently only weak scientific evidence on which to base those judgments, collecting more and stronger evidence about the true effects of policies is, we believe, a necessary and possibly promising step toward building greater consensus.

Recommendations

Our findings support the following tentative recommendations:

1. Those on each side of the gun policy debate should be aware that, despite some appearances, there is evidence that their opponents may share many of the same policy objectives. This may seem equally improbable to both sides, and there are certainly instances, people, or objectives for which this is not true. However, recognizing that the principal sources of disagreement may lie in the means of achieving the shared goals rather than in what the goals should be could be useful in gun policy negotiations.

2. If 70 percent of the variance in experts' favorability ratings is explained by their beliefs about what the empirical effects of those policies might be, then the vast majority of policy disagreements are associated with factual questions about policies' true effects that are, in principle, knowable. For the past two decades, however, Congress has been reluctant to support the collection of new evidence on the factual questions at the heart of policy disputes. New and significant investment in the scientific study of gun policies, which would require the support of Congress, offers a promising and available path for building consensus on gun policy. Because beliefs about gun policies are deeply entangled with personal and political identities, the credibility of new scientific information is certain to be challenged by those whose presumptions and group ideologies are contradicted by it. Nevertheless, because there may be general agreement on the objectives of gun policies, efforts to improve the science base on how to achieve those objectives through improved policy may help win converts to an expanded consensus view.
3. One factual question that appears to be of key importance concerns the magnitude of firearm substitution effects; that is, when firearm suicides or homicides are prevented, how many will still result in deaths by other means? Although both classes of experts typically believe that such substitution occurs, estimates of the magnitude of these effects vary dramatically. We believe that better information about this question could have implications for how people on all sides of gun policy debates evaluate the merits of individual policies. Therefore, we recommend that funders and researchers prioritize investigating whether substitution occurs and the conditions under which it does or does not occur.
4. We recommend that gun policy analysts, those engaged in negotiations over policies, and the public explore how combinations of laws might affect each U.S. state and the trade-offs created when, in the view of one or both sets of experts, a group of policies improves one set of outcomes but undermines others. This can be done through the gun policy comparison tool (RAND Corporation, 2018a).

Overall, we believe that without new, more-rigorous, and more-conclusive scientific research estimating the effects of gun policies on the outcomes considered in this report, policymakers and the public will depend on their own beliefs about those effects and the beliefs of the experts they trust. While there are considerable differences of opinion about these effects among experts, there is very little solid empirical research that can currently resolve these differences of opinion (RAND Corporation, 2018b). Thus, for the time being, expert opinion may be the best guidance available for crafting fair and effective firearm policies.

Acknowledgments

We wish to thank the gun policy experts who took the time to complete the survey described in this report. The survey instrument we used benefited from critical reviews by staff at Everytown for Gun Safety and the National Shooting Sports Foundation, as well as by Philip Cook, who also provided a helpful review of this report. We also wish to thank Matt Cefalu and Robert Lempert for their valuable critiques of earlier drafts of the report.

CHAPTER ONE

Introduction

Gun policies are debated and laws are passed or not passed based on arguments about their likely effects. These debates commonly cite the potential effects on suicide and homicide rates, the constitutional right to bear arms, individuals' rights to protect themselves and their families and property, unintentional firearm injuries, violent crime, mass shootings, access to hunting and recreational sport shooting, and the gun industry. Often, however, there is no consensus on what the effects of any law might be, and arguments claiming diametrically opposed effects are common. For instance, gun-free zones are claimed both to elevate the risk of gun violence (Lott, 2015) and to reduce this risk (Giffords Law Center to Prevent Gun Violence, undated).

It may be true that a law could increase one type of gun violence and decrease another, or that the effects of a law implemented in one state might be different from the effects of a similar law in another state. But the average effect of a policy on a given outcome like firearm homicides cannot be both positive and negative.[1] There is a factual set of effects for each policy but often little agreement on what those effects are. Moreover, unlike with many other areas of health, public safety, and economic activity, rigorous scientific evidence on the effects of different gun policies is comparatively rare. Stark and Shah (2017) found, for instance, that the volume of scientific publications on mortality resulting from firearm injuries was just 4.5 percent of what would be expected based on the volume of research published on causes of mortality resulting in similar numbers of deaths, such as traffic accidents or sepsis.

More recently, the RAND Gun Policy in America project team completed a systematic review of studies examining the causal effects of 13 classes of gun policies on a range of outcomes related to gun ownership, including suicide, violent crime, unintentional injuries and deaths, mass shootings, officer-involved shootings, defensive gun use, hunting and recreation, and the gun industry (RAND Corporation, 2018b). In total, we sought scientific evidence on 104 effects across the 13 types of policies. We found that there was no rigorous scientific evidence for more than half of the effects, and most of the others had been investigated in just a single study or were otherwise

[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.

classified by us as having only inconclusive evidence on what the direction or magnitude of the effect might be.

Without strong scientific evidence to rely on, policymakers and the public rely heavily on what advocates or social scientists believe the laws' effects are most likely to be, taking into account their studies of similar laws, their general expertise, and their reputations in the field. This makes gun policy experts' opinions about the true effects of policies an important influence on gun policy debates and decisions.

In this report, we describe the results of a survey of gun policy experts that was designed to quantify differences in their beliefs about the likely effects of 15 policies on 12 outcomes. That is, we sought to clarify where expert opinions tended to agree or disagree across policies and outcomes and to clarify how large the differences on factual matters might be. After aggregating experts according to the gun policies they favor, we used their judgments to construct an online policy comparison tool (RAND Corporation, 2018a), described in this report, that allows users to explore how outcomes would change for the nation, and state by state, if different combinations of state policies were enacted or repealed nationwide.

Second, we used survey responses to clarify whether differences between experts' ratings of the merits of the 15 policies reflect their different views of the likely effects of the policies or differences in the policy objectives the experts are interested in achieving. That is, two experts could agree on the likely effects of a policy but disagree on whether it is a good policy because they disagree on which of the policy's effects are most important. For instance, is it more important to prevent some firearm suicides or to protect the constitutional right to bear arms? Alternatively, if experts have different expectations about the likely effects of a gun policy, they may disagree on the merits of the policy even though they share the same objectives.

Gun policy debates are highly polarized in the United States. To the extent that these divisions rest on different assumptions about the factual effects of laws, there may be a place for more and better scientific study to clarify what is true and help build consensus on which policies will best achieve shared objectives. On the other hand, to the extent that divisions are driven more by differences in values or preferences, the path forward to build consensus may be less straightforward. Of course, even if different views on gun policy result from different beliefs about the likely effects of various policies rather than what the policy objectives should be, there is no guarantee that improved scientific information will do much to facilitate a consensus on gun policy. As we discuss in the concluding chapter, individuals may be motivated to reject strong scientific evidence because acknowledging its validity could force a break with the views of people and groups they identify with (Kahan, 2017). Nevertheless, if groups differ in their expectations about the true effects of laws and there is not good scientific evidence available on which to test those expectations, a strong case can be made to improve the evidence base on gun policy as a first step toward building a larger consensus. That is, to the extent that expert and public opinion is malleable, it is plausible

that improved scientific information could sway the opinions of the persuadable center toward a consensus view of what constitutes good gun policy.

Our approach to establishing the experts' views on the likely effects of gun policies differs in important ways from earlier efforts. Several studies have examined the association between support of gun policies among members of the general public and their estimates of the effectiveness of those policies (Hartnagel, 2002; Kleck, Gertz, and Bratton, 2009; Mauser and Margolis, 1992; Sorenson, 2015; Smith, 2000). These studies frequently demonstrate that support for a gun policy is associated with the belief that it will be effective, but belief in the effectiveness of the law cannot fully explain why members of the public support it. Kleck, Gertz, and Bratton (2009), for instance, concluded that

> Support for gun control derives partly from a belief that gun control is an effective method for reducing violence, but this explanation has only limited power to account for positions on the issue. Many people favor control measures even though they think they will *not* reduce crime, while others oppose controls despite their beliefs that they *will* reduce crime. (p. 503)

The authors go on to argue that much of the unexplained variance in support of gun policies may not be attributable to perceptions of the effectiveness of gun policies, but instead to cultural differences between groups, such as their beliefs and attitudes about gun owners and whether the police can manage crime problems.

Typically, in these surveys, respondents are asked to rate what effects a gun policy would have on a single outcome, such as violent crime. Kleck, Gertz, and Bratton (2009) asked about the effects of handgun bans on two outcomes, homicides and robberies. It is possible, however, that respondents' judgments about whether to support gun policies consider other outcomes as well. For instance, they might consider the effects of the policies on suicides or the harms the laws may do to Second Amendment rights. If so, then the fact that respondents' effectiveness ratings on one or two crime outcomes do not fully explain whether they support a law may not imply that their judgments are not based on the perceived effects of the laws. Instead, respondents may be taking into account many more effects than have previously been considered.

In the present study, we asked about a wide range of possible effects of each policy, including effects on crime and violence, as well as effects on individuals' ability to defend themselves, their participation in hunting and sport shooting, their rights and freedoms, and other outcomes. If differences in overall opinions of the policies can be largely explained by individuals' effect estimates, this suggests that different overall opinions may not reflect different policy objectives or values, but instead might simply reflect different beliefs about the true effects of laws. Of course, these different beliefs may still be tied to cultural or ideological differences between individuals that predispose them to certain beliefs about the effects of different gun policies.

As noted by Sorenson (2015), research in this area has often asked respondents whether laws will reduce crime but has done so with one-sided response options; that is, respondents can indicate that the law will or will not reduce crime, but they cannot indicate when they believe the law is likely to *increase* crime. Sorenson surveyed college students on their support for seven gun policies and asked the students to rate the effects of those policies on six outcomes using a qualitative scale that ranged from –100 ("a LOT worse") to +100 ("a LOT better"), with 0 at the center of the scale labeled "no change." These ratings were applied to a wider range of outcomes than we have found in earlier research: homicides, suicides, accidental firearm deaths, mass shootings, violent crime, and a generic gun violence outcome that did not refer to a specific crime. Sorenson found that one policy, armed officers in schools, was rated by those who supported it and those who did not as having negative mean effectiveness ratings across the measured outcomes. In addition, she found that respondents made little distinction in their effectiveness ratings across five of the six outcomes. The exception was suicide, which was regularly rated as less likely to be influenced by the policies.

Sorenson (2015) used college students at an East Coast university whose knowledge of the likely effectiveness of gun policies on each of the studied outcomes might be comparable to that of the general public and, like the general public's knowledge, is likely shaped by the views of the experts and advocates they or their peer groups regard as credible interpreters of the science of gun policy (Kahan, 2017). Moreover, the experts' expectations may be more nuanced than the public's and could better differentiate the likely effects of laws on such outcomes as homicides, mass shootings, and violent crime.

Several surveys of gun policy researchers and other experts have been conducted in recent years (Bui and Sanger-Katz, 2017; Lott and Mauser, 2016; and an ongoing panel survey of firearm researchers conducted by the Harvard Injury Control Research Center, 2014). Most recently, for instance, the *New York Times* polled 32 academic researchers described as having published extensively in peer-reviewed journals and asked how effective each of 29 gun policies would be at reducing firearm homicides or mass shootings (Bui and Sanger-Katz, 2017). These questions were asked using a one-sided qualitative scale, so experts could not indicate when they believed policies might worsen homicides or mass shootings. In parallel, they conducted the same survey with a representative sample of the U.S. electorate.

The authors used the results to establish what gun policy experts believe are the most-effective policies and how that corresponds to public support. For instance, the study concluded that this group of experts, on average, believed that universal background checks and prohibitions against the purchase of firearms by those convicted of violent misdemeanors are the two policies of the 29 that would be most effective at reducing firearm homicides. As with other such surveys, a key limitation of this one was that the sample frame was not a representative sample of experts. It is not clear how a representative sample could even be constructed. Given the polarization of views on gun policy, results are likely to be highly sensitive to the ratio of what Bui and Sanger-

Katz described as "supporters of gun control" and "opponents of gun control." In their survey, that ratio was 27 to five, and they acknowledged that the pattern of effectiveness ratings among the five opponents was markedly different from the pattern among the supporters. This raises the question of whether it is meaningful to combine the groups to establish an average effectiveness rating. Just as it would be absurd to suggest that the average resident of the East and West coasts lives somewhere in the middle of the country, combining often diametrically opposed views into an average "consensus" judgment is misleading, and more so when the sample of experts is not representative.

Others have tried to avoid the problem of identifying a representative sample of experts by instead looking for subgroups of experts whose views are especially coherent or authoritative on the effectiveness of gun policy. Lott and Mauser (2016), for instance, compared views of gun policy experts who were trained as economists with views of criminologist gun policy experts. They found that the 35 economists they surveyed were more likely than the 39 criminologists to expect gun-free zones to increase the risk of crime and to expect increases in the concealed carry of handguns to decrease murder rates.

The authors argued that comparison between these disciplines is revealing for two reasons: First, economists have a unified theory of behavior (the "law of demand") and criminologists do not and, second, economists are far more likely to be Republicans than Democrats. Nevertheless, it is not clear what to make of such disciplinary differences, given that each of the two groups appears to comprise people with a mix of different perspectives on gun policy (though especially, in Lott and Mauser's sample, the criminologist group).

The Lott and Mauser (2016) sample was not designed to be representative, and the authors' finding that "researchers, as a group, believe that guns are used more in self-defense than in crime; gun-free zones attract criminals; . . . and permitted concealed handguns lower the murder rate" (p. 28) is inconsistent with findings from other surveys of experts. For instance, the Harvard Injury Control Research Center's (2014) survey of 122 researchers found that more than 70 percent disagreed with the statement, "guns are used in self-defense far more often than they are used in crime," and Bui and Sanger-Katz (2017) concluded that their experts believed that placing greater *restrictions* on concealed-carry permits would reduce gun deaths. It is unlikely that expert consensus has swung so wildly between views in the three years over which these studies were conducted. Instead, the most likely explanation is that the samples in each study had quite different ratios of experts who favor more- and less-restrictive policies on gun access and use.

The present study builds on earlier efforts to identify experts' views on the effectiveness of gun policies in the following ways:

1. We make no effort to establish a consensus or average estimate of experts' views on policy effectiveness. Instead, our sampling procedure actively sought com-

peting and alternative views on gun policy so that we could instead characterize major differences in the views of groups with similar perspectives on gun policy. In pursuing this strategy, we included not just academic researchers in the sample but also policy analysts associated with advocacy organizations and membership organizations that have taken public positions on gun policies.

2. We asked respondents to rate the effects of policies not just on homicides or violent crime but on 12 outcomes representing many of the concerns most often raised in policy debates, including how the policies might abridge Second Amendment rights, how they might affect firearm suicides, how they could interfere with individuals' rights to protect themselves and their property, and other outcomes.
3. Instead of using a one-sided qualitative measure of effectiveness, we asked respondents to estimate how much change they would expect in each outcome after a typical state without the law implemented it. These judgments were made on a two-sided, quantitative, percent-change scale.
4. While other surveys have left it to respondents to decide how to rate the effectiveness of policies that they believe could not be enforced, that could be undermined by policies in neighboring states, or that could take many years to achieve their full effects, we provided respondents with explicit instructions on these questions to ensure a more consistent response frame.

In Chapters Two and Three, we describe the development of the sample frame and survey instrument, respectively. Chapter Four provides descriptive statistics of the results. Chapter Five presents a set of statistical models examining whether differences between groups of experts with divergent views are explained by differences in their views of what the laws will do or by incompatible preferences or values concerning which outcomes are most important. In Chapter Six, we describe our use of the survey results to develop a web-based gun policy comparison tool. We offer a discussion and concluding comments in Chapter Seven.

CHAPTER TWO

Sample of Policy Experts

There is no sufficiently comprehensive information on the distribution of gun policy expertise to construct a representative sample of policy experts. Therefore, we make no attempt to construct a sample from which we could estimate what the typical or average viewpoint is among experts. Instead, our sampling strategy was designed to allow us to characterize some of the diversity of perspectives on gun policies among recognized experts, evaluate the range of disagreements among clusters of experts with similar viewpoints, and evaluate the nature of those disagreements (for instance, whether they reflect differences in how experts value personal rights versus public health or stem from differences about the likely effects of different gun laws).

Sample Identification

With the objective of identifying experts with diverse views, our sampling strategy focused on three populations: (1) academic researchers with a strong publication history on gun policy topics, (2) advocacy and professional organizations that have taken public stances on gun policy, and (3) staff serving on one of four congressional committees that routinely consider gun policy legislation. We used a different approach to sample identification for each of these populations, as described next. All invitees were offered a $50 Amazon gift card for their participation.

Academic Researchers

To identify all academic researchers with expertise in U.S. gun policy and its effects, we conducted a systematic search of the Social Science Citation Index (available through the Thomson Reuters Web of Science database), which indexes articles in most behavioral science, economics, and law review journals. Specifically, we searched for all U.S.-based researchers whose publications in the past 20 years (January 1996 through June 2016) satisfied either of the following conditions:

- researchers with five or more gun policy publications
- researchers who were the first author of a publication that was cited 30 or more times by June 2016.

To identify gun policy publications, we used the following topic search terms: *violent or violence or homicide or suicide or crime or self-defense or defensive gun use or murder or accident or injury or hunting* AND *gun or guns or firearm or handgun or shotgun or rifle or long gun or pistol* AND *policy or law or legislation or program.*

We conducted this search on all document types tagged as being articles, editorial material, reviews, or book chapters. Because our focus is on experts on U.S. gun policy, we eliminated from consideration 12 researchers who had five or more publications but whose institutional affiliation was outside the United States. In addition, we eliminated two researchers whose expertise did not concern firearms (one was an expert on nail gun injury prevention). From the list of authors of publications with 30 or more citations, we excluded their publications that were purely descriptive of some phenomenon (e.g., firearm mortality reports from the Centers from Disease Control and Prevention and descriptive studies on the prevalence of suicides among dentists). While sometimes meeting our citation threshold, these studies do not directly consider the effects of policies on these outcomes. We excluded publications that focused exclusively on foreign gun policies, because the U.S. context for implementing firearm policies is markedly different given constitutional protections and the great abundance of firearms in the United States, among other factors. Finally, two research academics were known to have died at the time we constructed the sample frame and were thus excluded from it.

This procedure resulted in our sample frame of 118 academic researchers, listed in the box on the next page.

Sample Frame of Academic Researchers

Margaret Adamek
Mensah Adinkrah
Michael D. Anestis
Joseph L. Annest
Paul S. Appelbaum
Ian Ayres
Debra Azrael
Susan P. Baker
Catherine Barber
Shari Barkin
Joseph Blocher
Alfred Blumstein
Anthony Braga
Charles Branas
David Brent
Maria T. Bulzacchelli
Brad J. Bushman
Carlos A. Camargo
Jacquelyn C. Campbell
Barbara E. Claire
Yeates Conwell
Philip Cook
Nicholas Corsaro
Tamera Coyne-Beasley
Peter Cummings
Rebecca M. Cunningham
Linda L. Dahlberg
Joseph Dake
John Donohue
Mark Duggan
Robert Durant
Jeffrey Fagan
Richard Felson
Eric W. Fleegler
Shannon Frattaroli
Sandro Galea
Marc Gertz
Madelyn S. Gould
Douglas Gray
David Grossman
Robert Hahn
Stephen Hargarten
Kathleen Heide
David Hemenway
Nathalie Huguet
John C. Hunsaker
Sean Joe
Renee M. Johnson
Dan M. Kahan
Bindu Kalesan
Mark Kaplan
Ichiro Kawachi
Art L. Kellermann
Jagdish Khubchandani
Gary Kleck
Chris Koper
Augustine Kposowa
David Lester
John R. Lott
Jens Ludwig
Martin Mahon
Thomas B. Marvell
Gary Mauser
Andrew McClurg
Bentson H. McFarland
Edmund F. McGarrell
Emma E. Mcginty
J. Reid Meloy
James A. Mercy
Steven F. Messner
Darrell A. H. Miller
Matt Miller
Raymond Miltenberger
Beth Molnar
Kenneth J. Mukamal
Wade C. Myers
Michael L. Nance
Constance Nathanson
Tina Orwall
Andrew V. Papachristos
Anthony. Philippakis
Glenn L. Pierce
Kenneth E. Powell
James H. Price
Therese S. Richmond
Frederick P. Rivara
Richard Rosenfeld
Matthew Rosengart
Carol W. Runyan
Lisa B. E. Shields
C. William Schwab
Edmond D. Shenassa
Thomas Simon
Gary Smith
Susan B. Sorenson
Richard Spano
Kenneth Tardiff
Stephen Teret
Amy Thompson
Michael Tonry
Melissa Tracy
Marcia Valenstein
Robert Valois
John Vernick
Elizabeth Vigdor
Katherine A. Vittes
David Vlahov
Eugene Volokh
Maureen A. Walton
Daniel Webster
William Wells
Douglas J. Wiebe
J. Harvie Wilkinson
Garen Wintemute
Mona Wright
April M. Zeoli
Marc A. Zimmerman
Franklin Zimring

Advocacy and Professional Organizations

The second population from which we sought to identify experts was advocacy and professional organizations. Here, too, we sought diverse perspectives by selecting large membership organizations that had issued public statements advocating specific gun policies. We did not try to identify every such organization. Instead, we sought a balance of organizations representing the interests of those who favor more-restrictive policies toward gun access or use (Group 1 in the box below), those favoring more-permissive policies (Group 2), and those for whom gun policy advocacy is not a primary objective but whose membership has professional interests in specific gun policies (health services in Group 3, and law enforcement in Group 4).

After reviewing the largest and most well-known of these organizations, we selected those in the list to approach for nominations of gun policy experts who either worked with the organization or whom individuals at the organization regarded as most knowledgeable about the effects of gun policies. Through phone calls and emails, we successfully solicited nominations from most of the organizations listed. In addi-

Advocacy and Professional Organizations Approached for Nominations of Experts

Group 1 (favor more-restrictive policies)	Group 2 (favor more-permissive policies)
Brady Campaign to Prevent Gun Violence (7)	Gun Owners of America (3)
Coalition to Stop Gun Violence (5)	National African American Gun Association (1)
Everytown for Gun Safety (5)	National Association for Gun Rights (6)
Law Center to Prevent Gun Violence/Americans for Responsible Solutions (11)	National Rifle Association (13)
	National Shooting Sports Foundation (7)
Sandy Hook Promise (7)	Second Amendment Foundation (13)
Violence Policy Center (6)	

Group 3 (health services groups with interest in gun policy)	Group 4 (law enforcement groups with interest in gun policy)
American Academy of Emergency Medicine (1)	Association of Prosecuting Attorneys (2)
American Academy of Pediatrics (3)	Fraternal Order of Police (6)
American College of Emergency Physicians (9)	International Association of Chiefs of Police (4)
American Foundation for Suicide Prevention (5)	Major Cities Chiefs Association (7)
National Alliance on Mental Illness (1)	National Association of Police Organizations (2)
	National District Attorneys Association (2)
	National Sheriffs' Association (5[a])

NOTE: The number in parentheses denotes the number of experts who were nominated by or affiliated with that organization.

[a] Organizations could nominate experts or request passwords for the survey so that they could invite experts to participate anonymously. We provided the National Sheriffs' Association with five passwords under this arrangement.

tion to soliciting nominations of experts, we invited survey participation from policy analysts working at the organizations (or, in the case of the Second Amendment Foundation, serving on the editorial board of its *Journal of Firearms and Public Policy*).

The final sample frame of experts from advocacy and professional organizations included 127 experts whom we invited to participate in the survey. An additional 15 experts nominated by these organizations had already been identified as academic researchers (and are among those listed in that sample frame).

Congressional Committee Staff

The minority and majority chiefs of staff for the U.S. House of Representatives and U.S. Senate committees that most frequently consider gun policy proposals were invited to participate in the survey (and to nominate other committee members to participate). These individuals were associated with the following:

- Senate Judiciary Committee
- House Judiciary Committee
- Senate Health, Education, Labor, and Pensions Committee
- House Energy and Commerce Committee.

None of these eight chiefs of staff nominated any other committee members to participate in the survey, so the entire sample frame for this population is eight people.

Sample Frame 1: Number of Individuals Directly Invited to Respond

Given our sampling procedures, we can characterize two different sample frames: individuals who were directly invited to respond (Sample Frame 1) and the total number of potential respondents (Sample Frame 2). Invitations to complete the survey were sent to 253 individuals (Sample Frame 1) composed of 118 academics (15 of whom were also nominated by organizations), 127 nominees or members of selected organizations, and eight chiefs of staff of congressional committees. Among the 253 direct invitations issued, 233 were sent by email and 20 were sent by regular mail (when email addresses could not be identified).

Sample Frame 2: Total Number of Potential Respondents

When organizations expressed reluctance to nominate staff or outside experts out of concern for the privacy of these individuals, RAND offered to provide five passwords to the survey that could be distributed by the organization anonymously. This accommodation was requested by one organization (the National Sheriffs' Association) and was additionally provided to each of the eight chiefs of staff of congressional commit-

tees to encourage them to invite other committee members to participate. We treat these anonymous passwords as a second sample frame with a maximum possible size of 36, although we do not know whether (and, if so, how many) any of these passwords were delivered to potential respondents.

CHAPTER THREE

Survey Design and Analysis Plan

Survey respondents were directed to a password-protected online survey, where they answered questions about their beliefs and opinions on the effects of specific gun policies on a range of outcomes related to gun ownership. In addition, respondents were asked to indicate their level of agreement with the policy prescriptions of several advocacy or professional organizations (see Appendix A for the full survey).

Selecting Gun Policies and Outcomes for Analysis

This survey was conducted in parallel with another RAND Gun Policy in America project to conduct a systematic review of scientific evidence on the effects of 13 gun policies. More-complete details on the selection of policies and outcomes for that review, and subsequently for this survey, are found in the report on that review (see RAND Corporation, 2018b). In brief, the 13 policies we selected were drawn from more than 100 gun policy proposals we identified that had been advocated by diverse organizations, such as the White House, the National Rifle Association, the Brady Campaign to Prevent Gun Violence, and the International Association of Chiefs of Police, among many others. We narrowed the list to 13 by considering just those that had been implemented as state law at some point, so that there would be an evidence base from which to draw scientific or expert conclusions. We also excluded policies likely to have only an indirect effect on our primary outcomes, such as laws requiring improved recordkeeping or expanding access to mental health care. Finally, we combined several policies that were conceptually similar.

The 15 policies included in the survey are the 13 selected for our systematic review of the literature, with three exceptions. First, one of the classes of policies we selected in the systematic review concerned concealed-carry laws generally. In the survey, we specified one form of concealed-carry law for experts to rate: permitless carry. Second, we added one policy—firearm and ammunition taxes—that was not originally included in our systematic review but that was recommended to us when we sought comments on our draft survey instrument from representatives of organizations advocating more-permissive or more-restrictive approaches to gun access and use. Finally, we added one

policy—a media campaign to prevent child access—that was not a state law but that we believed to be a favored policy of some advocacy organizations. Table 3.1 shows the policies and descriptions that were provided to survey respondents.

The 12 outcomes we selected for inclusion in the survey are also largely drawn from those we investigated as part of our systematic review of the literature. These were originally selected to represent the wide range of concerns that are frequently raised in

Table 3.1
Policies and Descriptions Provided to Survey Respondents

Policy		Description
1	Universal background checks	People who are prohibited by law from having firearms sometimes obtain them through private sales that do not require background checks. Universal background checks require background checks prior to all transfers of firearms, including private sales over the Internet, at gun shows, and between friends (temporary loans and gifts between family members are exempted). Background checks for private sales are conducted by a government agency or by a licensed gun dealer.
2	A ban on sale of "assault weapons" and high capacity magazines	This law bans certain semi-automatic firearms with detachable magazines and other features, such as pistol grips, folding stocks, or the ability to mount a bayonet. The law also bans magazines that hold more than 15 rounds of ammunition. Owners of these weapons at the time the law is passed may keep them if each weapon is registered with a state authority.
3	A stand your ground law	This law permits a person to use deadly force without the duty to retreat when confronting a threat that could reasonably result in death or serious injury. Without this law, people outside their homes must try to withdraw from a serious threat, if possible, before using deadly force.
4	Expanded mental health prohibitions	When a judge has committed someone to an inpatient mental institution or has found them to be unable to manage their own affairs, federal law prohibits that person from having firearms. This law expands the mental health histories leading to prohibition to include people ordered to receive outpatient mental health treatment and those involuntarily confined because a mental health professional determined they present a danger to themselves or others.
5	Required reporting of lost or stolen firearms	Firearm owners must report lost or stolen firearms to law enforcement authorities within three days of discovering the loss. Penalties for failure to report include prohibition on firearm ownership for five years and civil liability if the firearm is used in a crime.
6	Requiring a license to purchase a firearm or ammunition	This law requires a firearms license to purchase or possess a firearm or ammunition. These licenses require successful completion of a safety training course or safety test and a background check, and cost $100. They must be renewed every ten years.
7	Required reporting and recording of firearms sales	This law requires reporting all firearms sales to a government agency, including information on the firearms and who bought them. This applies to sales by both firearms dealers and private sellers. Law enforcement is permitted to retain the data indefinitely for two purposes: to trace firearms found at crime scenes and to retrieve firearms from individuals who become prohibited possessors.
8	A child access prevention law	This law imposes criminal penalties on firearm owners when a child accesses a usable weapon that was stored in a location where the owner should have known a child could access it.

Table 3.1—Continued

Policy		Description
9	A media campaign to prevent child access	This policy educates the public about the benefits of safe storage through a media campaign. The campaign provides educational materials through news media and the internet and to gun stores for display and distribution.
10	Surrender of firearms by prohibited possessors	When a judge's rulings place an individual in a class that is prohibited by law from possessing or purchasing a firearm, the judge must also determine whether that individual has firearms, and must order their surrender. Prohibited possessors include people convicted of a felony, those convicted of misdemeanor domestic violence, and those subject to a domestic violence protective order.
11	Firearm and ammunition taxes	This policy imposes a special $25 tax on the sale of firearms and a 25% tax on the sale of ammunition.
12	Minimum age requirements	Currently, federal law generally prohibits those younger than 18 from having a handgun, and licensed dealers are prohibited from selling them to anyone younger than 21. Those younger than 18 may have a long gun, but licensed dealers may not sell them to anyone younger than 18. The minimum age requirements policy raises the minimum age for purchase or possession of handguns and long guns to 21.
13	Permitless carry	This policy allows anyone who is at least 21 years old and not prohibited by law from having a firearm to carry a concealed weapon in public without a permit. For the questions below, assume that before adopting *permitless carry*, the state required concealed carry permits that were issued to those with good moral character and sufficient reason for a concealed firearm.
14	Requiring a ten-day waiting period to purchase a firearm	This law imposes a waiting period of ten days between the purchase of a firearm and when the buyer can take possession of it. For this question, assume that the state already has a universal background check requirement.
15	The elimination of gun-free zones	Federal and some state laws prohibit carrying a firearm near schools and certain other public places. This policy allows firearms in these previously prohibited locations. For this question, assume federal and state laws change in a state that previously prohibited private citizens from carrying firearms into schools, universities, government buildings, and parks.

gun policy debates concerning, for instance, a policy's possible effects on firearm suicides, firearm homicides, unintentional firearm deaths, mass shootings, participation in hunting and sport shooting, legal acts of defensive gun use, and other outcomes. For the survey, we added three outcomes that had not been included in the research synthesis because we did not believe they had been subject to the kind of quantitative scientific research we were reviewing. These were the right to bear arms; individuals' privacy; and satisfaction of gun ownership, which we defined for survey respondents as including "satisfaction from collecting firearms, feeling safe, or recreational use." Finally, we replaced the outcome of officer-involved shootings used in the research review with property crime in the survey (which we defined for respondents as "burglary, theft, and auto theft") in order to include one outcome with only indirect relevance to changes in firearm laws. Table 3.2 lists the survey's 12 outcomes and the

Table 3.2
Outcomes Included in the Survey and the Response Scale for Each

	Outcome	Response Scale
1	Firearm suicides	Proportion scale (< 100% = decrease, 100% = no change, > 100% = increase)
2	Firearm homicides	
3	Accidental firearms deaths	
4	Mass shootings (incidents in which four or more people are killed, not including the shooter)	
5	Other violent crime (e.g., nonfirearm homicides, robbery, rape, aggravated assault)	
6	Property crime (burglary, theft, and auto theft)	
7	Participation in hunting and sport shooting	
8	Legal acts of defensive gun use (using a firearm to protect oneself or others from imminent death or serious injury)	
9	Sales of new firearms	
10	Right to bear arms	Likert scale (1 = major threat, 7 = major protection)
11	Individuals' privacy	
12	Satisfaction of gun ownership (includes satisfaction from collecting firearms, feeling safe, or recreational use)	Likert scale (1 = major decrease, 7 = major increase)
	Opinion	Likert scale (1 = very bad policy, 5 = very good policy)

response scale used for each; the survey also asked for the experts' overall opinion about each of the 15 policies. The response scale and opinion question are described in more detail in the next section.

Estimating the Effects of Gun Policies

The survey was designed to collect expert judgments on the effects of 15 policies (Table 3.1) on 12 outcomes related to gun ownership (Table 3.2), as well as to collect expert opinions about the merits of each policy. We believed, however, that a survey asking about all 195 combinations of policies and outcomes or opinions would be too lengthy and would discourage participation. Therefore, each respondent was asked to estimate the effects of a randomly selected subset of ten policies. However, respondents were asked the overall opinion question for all 15 policies. The order in which policies were presented to respondents was randomized.

To focus respondents' estimates on any generalizable effects attributable to the policies they were rating, we asked that they indicate the effect they would expect for

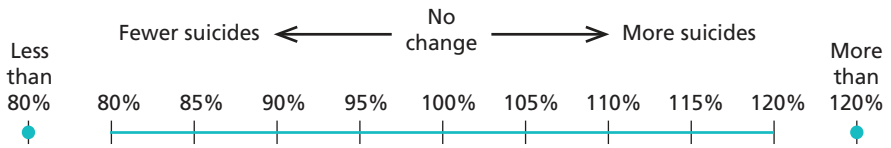
a "typical U.S. state" that currently has no associated laws that go beyond federal firearm laws. Further, we asked that they factor into their judgments how well policies like those they were rating are likely to be implemented and enforced, and then indicate the maximum effect they would expect after sufficient time passed to observe that maximum. Finally, the instructions indicated that if the experts' effect estimates for a typical state depended on whether other states also implemented the same law or policy, then they should assume that all states implemented the law together. These instructions were presented prior to rating the first law and were always accessible to respondents at an "instructions" link presented on every page where ratings were requested.

Nine of the 12 effect estimates used a quantitative outcome scale indicating the proportion change in the outcome that would be expected if a typical state that did not have such a policy were to implement it (the first group in Table 3.2). For instance, if *universal background checks* was the first policy a respondent was asked about, the survey provided a definition of universal background checks, and the first quantitative response scale question looked like the sample in Figure 3.1.

Respondents selected a point on the scale to indicate how the number of suicides after effecting universal background checks would compare with the number before implementing the law. Thus, the response scale was centered at 100 percent (no change), with less than 100 percent indicating a decreased rate compared with before implementation and more than 100 percent indicating an increased rate compared with before implementation. The central portion of the scale ranged from 80 percent to 120 percent, reflecting our belief that respondents would be unlikely to rate the types of policies presented in this survey as changing any of the outcomes by more than 20 percent in either direction. If we are mistaken on this point, the scale we used could have anchored respondents to more-conservative effect estimates than they might otherwise have selected. That said, respondents who believed the effects would

Figure 3.1
Sample Quantitative Response Scale

If a state implemented universal background checks, how much would firearm suicides change? This question is only about *firearm* suicide. Later we will ask about all suicides. Mark the suicide rate after implementing the law as a percentage of the rate before implementation by clicking on the black line or one of the endpoints.



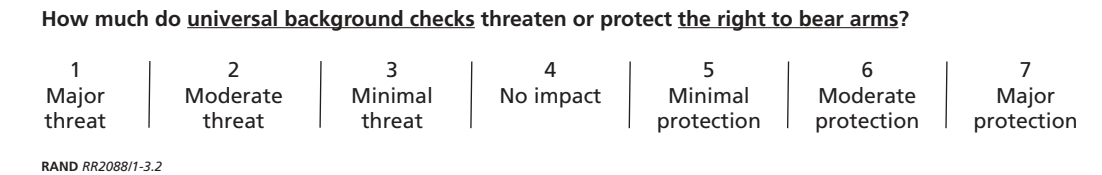
Example: If you select **93%**, you are saying that a state that had **1,000** suicides before implementing this policy would be likely to have **930** suicides after implementing this policy.

be greater than available on the main scale could select more-extreme values by clicking their mouse on "Less than 80%" or "More than 120%." This produced a text box where respondents were asked to indicate the percentage change they expected. Values between 0 percent and 200 percent were accepted in these entries.

We rejected an alternative set of response options for assessing the effects of policies on these outcomes that would have asked experts to indicate the percentage change they would expect in the outcome and then indicate whether the change was an increase or decrease. The latter approach was rejected as being prone to misunderstanding because the term "percentage change" could be interpreted as referring either to the units of the outcome itself (e.g., when the respondents mean that the rate of violent crime would go from 1 percent per year to 2 percent per year, they may indicate a "1-percent increase") or to the post-policy rate as a function of the pre-policy rate (e.g., when the respondents mean that the rate of violent crime would go from 1 percent per year to 2 percent per year, they may indicate a "100-percent increase"). In contrast, our chosen response options—which parallel the risk ratios commonly used to express these effects in the scientific literature—unambiguously refer to the post-policy rate of the outcome divided by the pre-policy rate.

Three outcomes did not lend themselves conceptually to a quantitative scale of effects. Instead, for questions about the right to bear arms, individuals' privacy, and satisfaction of gun ownership, respondents were provided a qualitative seven-point response scale with text descriptions for each response option. As an example, for the effects of universal background checks on the right to bear arms, the question and response options are shown in Figure 3.2.

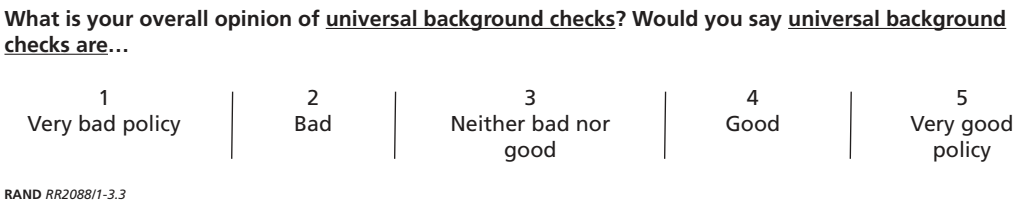
Figure 3.2
Sample Qualitative Response Scale



Overall Opinion of Policies or Laws

For ten randomly selected policies from among the 15 policies under study, respondents provided all quantitative and qualitative ratings for the 12 outcomes. When providing these 12 ratings on each of the ten policies, a final, thirteenth question asked for respondents' "overall opinion" of the policy on a five-point scale, as shown in Figure 3.3.

Figure 3.3
Sample Opinion Response Scale



Respondents were also asked this overall opinion question for the five policies not selected for detailed effect estimation. Specifically, after completing effect estimates for the ten randomly selected policies, the survey presented the five remaining policies on a single page, along with their short descriptions, and respondents were asked to provide their "overall opinion" on each using the scale in Figure 3.3. In this report, we often refer to the respondents' answers to this question as *favorability ratings*.

Firearm Death Substitution Effect Estimates

After completing effect estimates for ten policies and favorability ratings for all 15 policies, respondents were asked what effects policies that reduce firearm suicides have on total suicides. Specifically, the question asked about the proportion of firearm suicides prevented by a policy that would still result in death by suicide by some means other than firearm injury. Respondents were then asked the same question for firearm and total homicides. Response options from 0 percent to 100 percent were available in 10-percentage-point intervals.

Characterizing Respondent Perspectives on Gun Policy

Participants responded to additional background questions about their beliefs and perceptions regarding gun violence and gun policy. Ten items asked respondents to rate how similar their own views on firearm policies are to each of ten organizations (scale: 1 = very different, 6 = very similar, 7 = not sure). The ten organizations were selected because they represent diverse perspectives on gun policy and their policy positions would be familiar to many gun policy experts. With the order of presentation randomized for each participant, these organizations were

- Brady Campaign to Prevent Gun Violence
- Coalition to Stop Gun Violence
- Gun Owners of America

- International Association of Chiefs of Police
- Law Center to Prevent Gun Violence
- Mayors Against Illegal Guns/Everytown for Gun Safety
- National Rifle Association
- National Shooting Sports Foundation
- Second Amendment Foundation
- Violence Policy Center.

Respondents were then asked to indicate their "relationship to gun policy," choosing all applicable options among the following: professional researcher/scientist, policy analyst, policy advocate, interested layperson, government official, congressional staff member. Finally, respondents indicated how much they believe that each of ten different factors—such as easy access to guns, mental health problems, and inadequate enforcement of existing gun laws—contributes to the problem of firearm homicides in the United States (scale: 1 = a great deal, 4 = not at all).

Analytic Plan

Imputation of Missing Values

As described previously, respondents were asked about their overall opinions of each of the 15 policies but, to reduce response burden, were asked to estimate just ten of the policies' effects on each of the 12 outcome dimensions. Thus, each respondent was missing responses on 12 outcome ratings for five randomly selected policies. These planned missing data were imputed using the MICE (Multiple Imputation by Chained Equations) package in the R statistical language. In addition, we imputed a small amount of unplanned missingness on the outcome dimensions when the following conditions were met: the respondent answered at least half of all of the outcome ratings that were presented, the respondent provided an overall rating for the specific policy whose effects were being evaluated (five-point anchored Likert scale), and the respondent's ratings had not been excluded as a result of extreme values (as discussed in the next chapter). For both planned and unplanned missingness, we conducted imputation using a relatively simple, but highly predictive, linear regression model in which each effect of a given policy (e.g., the effect of permitless carry on firearm homicide) was predicted by (1) that respondent's overall opinion of that policy (e.g., does the person think permitless carry is a good policy?) and (2) the average estimate of that policy's effect on that outcome for other experts in the same class (e.g., how do other experts who prefer more-restrictive gun policies think permitless carry will affect firearm homicides?). Imputations are based on random draws from the posterior predicted distribution from this model. Subsequent analyses were conducted using these imputed data.

We included as a usable response every survey we received that provided at least one estimate of the likely effects of at least five of the ten policies on which respondents were asked to provide such ratings.

Class Identification

We sought to identify clusters of experts with similar opinions about the merits of each of the 15 policies considered in the survey. To do so, we used latent class analysis conducted in Mplus. In this model, the means, variances, and covariances across the respondents' favorability ratings on the 15 polices were explained as a function of a latent dichotomous variable. As discussed later, we considered two- and three-cluster solutions.

In Chapter Five and Appendix B, we provide details on the methods we used to examine how experts' judgments about the effects of policies are associated with their favorability judgments.

Statistics for Describing Effect Ratings by Members of Expert Classes

When describing the distribution of expert opinions and attitudes, we present medians and interquartile ranges (IQRs) rather than means and standard deviations. We do so for three reasons. First, the median and interquartile range are easier to understand for a lay audience; the median is the response of the "typical" expert, and the interquartile range represents the range of values that captures half of all expert opinion. Second, the responses on the survey are often non-normally distributed; on several scales, the central tendency is near a limit (e.g., a 5 on a scale of 1 to 5). In such cases, the distribution is highly asymmetric around the mean, and the standard deviation is a misleading measure of dispersion. Finally, we are aware that some of our respondents have strong professional interest in, and personal feelings about, the gun control policies being investigated in the survey. This may be reflected in the fact that some scales saw extreme responses that could skew the survey results unrealistically. Using a simple average of responses is highly sensitive to these extreme values, allowing outliers to have disproportionate leverage over our description of expert opinion. In contrast, the median is relatively insensitive to such outlier responses. Thus, using the median provides reassurance to those who may be concerned that our results may include artifacts caused by manipulation of the survey by activists.

Inferential tests of equality between medians for different groups of experts were performed using permutation tests. This is a statistical resampling method that allows one to compare the observed medians with the distribution that would occur if participants were randomly sampled from the same population.

CHAPTER FOUR

Survey Findings

This chapter describes the respondent sample, how the sample split into classes of experts based on overall opinions (or favorability ratings) of the 15 policies, and how these classes evaluated the likely effects of each policy.

Respondent Sample

We treated as usable the surveys in which the respondent answered at least one of the 12 questions about estimated effects for at least five of the ten policies.

The 95 respondents in the final sample were primarily drawn from Sample Frame 1 (97 percent), which had an overall response rate of 37.9 percent. Because we know their identities, we can provide more-detailed information about completion rates for these 92 respondents (see Table 4.1). For ten of the 253 Sample Frame 1 invitations, the email or FedEx delivery was returned and not received. Among the 243 individuals successfully contacted, 92 (38 percent) completed at least half of the policy outcome effect estimates they were presented with, so these respondents were included in the sample. Of the 36 potential survey invitations distributed as part of Sample Frame 2, only three anonymous passwords were used.

Among the final sample of 95 respondents, 50 were from the academic researcher sample (including seven who were also nominated by organizations), 44 were from the organizational sample, and one was a congressional staff member.

Response patterns suggested that a small number of respondents were confused by the nine quantitative policy effect scales that asked what they would expect the frequency of an outcome would be after implementing the policy, expressed as a percentage of the frequency before it was implemented. As designed, if respondents believed that a policy would have no effect, they should indicate 100 percent (that is, the frequency of the outcome after the policy is introduced would be 100 percent what it was before the policy). But a simple misunderstanding of the scale resulted in four respondents using 0 percent to indicate no change in frequency of the outcome (rather than 100 percent) or 5 percent to indicate a small increase (rather than 105 percent), as the scale was intended.

Table 4.1
Survey Response Rate and Composition of Final Sample

	Response Rate (Sample Frame 1)					Final Sample (including Sample Frame 2)
Respondent Type	Invited (N)	Contacted Successfully (N)	Started Survey (N)	Included in Sample (N)	Response Rate (included/ contacted)	Proportion of Sample
Academic researcher only	103	96	45	43	44.8% (43/96)	45.3% (43/95)
Academic researcher and organizational affiliate	15	15	7	7	46.7% (7/15)	7.4% (7/95)
Organizational affiliate only	127	124	45	41	33.1% (41/124)	46.3% (44/95)
Congressional staff	8	8	1	1	12.5% (1/8)	1.1% (1/95)
Total	253	243	98	92	37.9% (92/243)	100% (95/95)

Several patterns of evidence led us to conclude that these nine respondents misunderstood the scale in this way. First, while nearly all respondents provided values between 50 percent and 150 percent (a range that encompasses improbably large effects for any of the policies under consideration), these nine respondents provided most of their responses in the 0 percent to 20 percent range, including responses on questions where most other respondents believed the policy could have no effect on the outcome in question. Second, among the response values placed near 0 percent, respondents tended to make fine distinctions between values (e.g., 0 percent, 1 percent, 2 percent, and 5 percent). Again, if these respondents had intended to indicate extreme reductions in the outcome (as a value of 0 percent or 5 percent would imply on the intended scale), it seems unlikely that they would emphasize small gradations, such as a 98-percent reduction as opposed to 95 percent or 100 percent. Finally, respondents who selected low response values on some items tended to do so on other items as well, suggesting a consistent misinterpretation of the scale.

To diminish the distorting effects of estimates based on a misinterpretation of the scale and still retain respondent estimates on other scales, we implemented the following rule: If a respondent had two or more policies for which the minimum response value was less than or equal to 10 percent (across the nine items using the quantitative policy effect scale), we treated as missing all of the respondent's values on this scale (across the nine items on all policies). This rule led to the elimination of the nine respondents' quantitative scale responses. One of these nine belonged to the permissive class of experts (see next section), and the remainder were from the restrictive class. No

respondent gave a response greater than 200 percent (a doubling of the outcome after policy implementation), which is comparable in magnitude to an estimate of 50 percent (a halving of the outcome), so we did not create a parallel rule for disregarding estimates that were so high that they suggested a misunderstanding of the scale.

Class Identification

Our latent class analysis of the means, variances, and covariances of experts' favorability judgments identified a two-class solution with classes of size 16 and 79. The model placed respondents in one of these two classes with almost no ambiguity; all posterior probabilities of class membership in the respondent's most likely class were greater than 0.99. This high degree of separation is consistent with the fact that the distributions of experts' favorability ratings of the 15 policies were bimodal, with modes at (or near) the two extremes of the response scale. The 16 respondents in Class 1 favored policies that we interpret as being more permissive in terms of access to and use of firearms (Table 4.2). For instance, this group's median rating of ten-day waiting periods is 1 ("very bad policy"), and their median rating of a stand your ground law is 5 ("very good policy"). In contrast, the 79 respondents in Class 2 favored policies that we interpret as more restrictive in terms of access to and use of firearms. For instance, median ratings by this group on ten-day waiting periods and a stand your ground law were at the extreme opposite end of the scale as the Class 1 experts' ratings. Therefore, for simplicity, we refer to experts in Class 1 as those favoring *permissive* gun policies and experts in Class 2 as those favoring *restrictive* gun policies.

Because class membership was determined by experts' favorability ratings of the 15 policies, it is by design that the groups differ in their ratings of these policies. Nevertheless, the patterns of favorability ratings are informative. In addition to the clear separation in median ratings of policies, interquartile ranges for the two groups did not overlap on any law except expanded mental health prohibitions (against gun ownership), for which the upper end of the range for experts favoring more-permissive regulations was also the lower range value for those favoring more-restrictive regulations (at the scale value of 4, "good policy").

For four policies, the two groups were not on opposite sides of the opinion scale's midpoint value of 3 (representing the opinion that the policy is "neither good nor bad"), as depicted in Table 4.2. These policies were expanded mental health prohibitions, required reporting of lost or stolen firearms, a media campaign to prevent child access, and surrender of firearms by prohibited possessors. For each of these policies, the median opinion of the group with a more permissive regulatory approach was neutral, and the median opinion of the group with a less permissive regulatory approach was positive. These four policies are the only ones for which the median members of

Table 4.2
Favorability Rating for Each Policy, by Expert Class

		Permissive Class			Restrictive Class				
	What is your overall opinion of . . .	25th%	50th% (median)	75th%	25th%	50th% (median)	75th%	Difference[a]	Opposite Sides[b]
1	Universal background checks	1	2	3	5	5	5	3	Yes
2	Ban on sale of "assault weapons" and high capacity magazines	1	1	1	4	5	5	4	Yes
3	A stand your ground law	3	5	5	1	1	2	–4	Yes
4	Expanded mental health prohibitions	2	3	4	4	5	5	2	No
5	Required reporting of lost or stolen firearms	2.5	3	3	4	5	5	2	No
6	Requiring a license to purchase a firearm or ammunition	1	1	1.5	5	5	5	4	Yes
7	Required reporting and recording of firearms sales	1	1	2	4	5	5	4	Yes
8	A child access prevention law	2	2.5	3	4	5	5	2.5	Yes
9	A media campaign to prevent child access	3	3	4	4	5	5	2	No
10	Surrender of firearms by prohibited possessors	2	3	4	5	5	5	2	No
11	Firearm and ammunition taxes	1	1	2	4	4	5	3	Yes
12	Minimum age requirements	1	2	3	4	5	5	3	Yes
13	Permitless carry	3	4	5	1	1	1	–3	Yes
14	Requiring a ten-day waiting period	1	1	2	4	5	5	4	Yes
15	The elimination of gun-free zones	4	4	5	1	1	2	–3	Yes

NOTE: We present the 25th percentile (first quartile), 50th percentile (median), and 75th percentile (third quartile) for each group's favorability rating of each policy. The scale ranged from 1 = very bad policy to 5 = very good policy. These 15 ratings are the basis for determining whether each respondent belonged in the permissive or restrictive class.

[a] This column displays the difference in group medians (the median response value for the restrictive class minus the median response value for the permissive class).

[b] This column indicates whether the medians of the two groups are on opposite sides of the response scale's central point (3).

both classes of experts believed that the policy was not bad; thus, these may form the basis of consensus policy proposals to reduce the harmful effects of firearms.

Although class membership was based solely on respondents' favorability ratings of the 15 policies, these classes also sharply distinguished experts on questions about how similar their own views were to those of familiar policy advocacy and membership organizations. For instance, as shown in Table 4.3, the median rating of similarity with the policy positions of the National Rifle Association was 6 ("very similar") for those who favor more-permissive policies (Class 1) and 1 ("very different") for those who favor more-restrictive policies (Class 2). The converse is true for similarities with the Brady Campaign, Mayors Against Illegal Guns/Everytown for Gun Safety, and other organizations, for which permissive-class respondents indicated that their own views were very different from these organizations' (median of 1), and restrictive-class respondents closely identified with these organizations' views (median of 6). These differences between classes are extremely large. In several cases, they are as large as is mathematically possible on the response scale used for these questions. Respondents from the permissive class generally agree with the policy positions of gun rights advocacy groups and disagree with gun control advocacy groups, while those from the

Table 4.3
Respondent Perceptions of How Similar Their Views Are to Those of Stakeholder Organizations, by Expert Class

Please indicate how similar your own views on firearm policies are to those of each organization.	Permissive Class			Restrictive Class		
	25th%	50th% (median)	75th%	25th%	50th% (median)	75th%
National Rifle Association	4	6	6	1	1	1
Second Amendment Foundation	5	5	6	1	1	1
Gun Owners of America	4	5	6	1	1	1
National Shooting Sports Foundation	4	5	6	1	1	2
International Association of Chiefs of Police	1	3	4	4	5	6
Brady Campaign to Prevent Gun Violence	1	1	1	5	6	6
Mayors Against Illegal Guns/Everytown for Gun Safety	1	1	2	5	6	6
Violence Policy Center	1	1	2	5	6	6
Coalition to Stop Gun Violence	1	1	2	5	6	6
Law Center to Prevent Gun Violence	1	1	2	5	6	6

NOTE: We present the 25th percentile (first quartile), 50th percentile (median), and 75th percentile (third quartile) for each response. The scale for similarity ranged from 1 = very different to 6 = very similar; 7 = not sure.

restrictive class agree with the policy positions of gun control advocacy groups and disagree with gun rights advocacy groups.

One set of questions on the survey asked respondents to indicate their relationship to gun policy. As shown in Table 4.4, experts in the restrictive class were twice as likely as experts in the permissive class to indicate that they are professional researchers or scientists; experts in the permissive class were more likely than their counterparts to describe themselves as policy analysts and interested laypeople. Approximately one-third of both groups described themselves as policy advocates.

We also investigated an alternative, three-class solution. Under that solution, 13 respondents formed a third group whose responses were less extreme than the members of the other two groups. Eleven of these 13 experts were classified as favoring more-restrictive policies in the two-class solution. It is not clear whether this group should be interpreted substantively as having unique views on firearm policy or whether it captures individual differences in the respondents' willingness to use the extreme ends of the scale. Because of the difficulty in interpreting the three-factor solution, the fact that the two-class solution was strongly associated with respondent views of advocacy organizations, and the small sample sizes, we proceeded using the two-class solution.

In general, all of our analyses that are designed to characterize the distribution of expert opinion are stratified by these two classes of experts. That is, we do not attempt to use this study to identify a single central tendency of expert opinion that characterizes the population of gun policy experts. Rather, we separately describe the distribution of opinion for the two classes of experts: those who prefer more-permissive gun policies versus those who prefer more-restrictive gun policies.

Table 4.4
Respondent Relationship to Gun Policy, by Expert Class

Which of the following best describes your relationship to gun policy?	Permissive Class (%)	Restrictive Class (%)
Professional researcher/scientist	31	61
Policy analyst	31	16
Policy advocate	38	35
Interested layperson	31	11
Government official	0	3
Congressional staff member	0	3

NOTE: Columns do not sum to 100 percent because respondents could select more than one category.

Estimated Effects of Policies on Outcomes

For each expert class, Figure 4.1 presents the distribution of policy effect estimates for the nine quantitatively measured outcomes and the three qualitatively measured outcomes. We present (1) medians as an indicator of what the typical member of each group estimated and (2) interquartile ranges as an indicator of the range of values that capture the central half of the expert judgments for a given class. The quantitative judgments are on a scale similar to an incidence rate ratio (commonly used in the empirical literature evaluating the effectiveness of gun policy). On this scale, 100 percent indicates that the policy will have no effect on the outcome. That is, after a policy is implemented, the outcome would be expected to occur at 100 percent the rate it occurred before implementation. Similarly, 110 percent indicates that the policy will increase the rate of the outcome by 10 percent, and 90 percent indicates the policy will decrease the rate of the outcome by 10 percent. The three outcomes assessed with qualitative response options are scaled from one to seven, with lower scores indicating that the policy would have a harmful effect and higher scores indicating a beneficial effect. Detailed results are presented in full in Appendix C.

Figure 4.1 shows the 180 judgments per expert class and their interquartile ranges. For 65 of the 180 effect estimates (36 percent), the medians for the two groups agreed on the direction of the effect on the outcome or agreed that the policy would have no effect. For another 103 effect estimates (57 percent), median estimates for one group suggested that the policy would have no effect on the outcome, while the other group believed it would have a positive or negative effect.

It was fairly rare for groups to disagree on the direction of the effect (i.e., one group said the outcome would increase or worsen and the other group said it would decrease or improve as a result of implementing a law). This occurred with just 12 effect estimates (7 percent); see Table 4.5, where these are notated by an arrow pointing both left and right. More than half of these instances of disagreement concern two policies: permitless carry and elimination of gun-free zones. Those favoring more-permissive gun policies believed that these policies would reduce firearm homicides; mass shootings; other violent crime; and, in the case of permitless carry, property crime. In contrast, those favoring more-restrictive gun policies estimated that the policies would increase each of these outcomes. In the most extreme example of such disagreement on the quantitative scales, experts favoring more-restrictive policies expected that eliminating gun-free zones would increase mass shootings by a factor of 1.05 (a 5-percent increase over rates before the law is introduced), whereas experts favoring more-permissive laws believed that eliminating gun-free zones would reduce mass shootings to just 0.875 the rate preceding the policy. Thus, the largest disagreement we observe between the medians for the two classes of experts on the quantitative effect scales amounts to one group believing that eliminating gun-free zones would result in mass shootings being 18 percent higher than the other group expects.

Figure 4.1
Median Response for Each Policy, by Expert Class and Outcome

NOTE: The circles indicate the group medians (50th percentile), and the lines indicate the interquartile ranges (25th to 75th percentile).

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Although the two classes of experts rarely had median effect estimates with opposing directions, the magnitude of the expected effects was often substantially different between groups. These differences were statistically significant in 73 of the 180 judgments. Detailed results, presented in Appendix C, demonstrate that medians differing by approximately 5 or more percentage points on the quantitative scale, or 1.5 to 2 points on the qualitative scale, were usually significantly different at $p < 0.05$.

Interestingly, the policies for which the two classes of experts had the most similar favorability ratings also tended to have higher-than-average levels of agreement on the expected effects of the policies. To assess the level of agreement across the classes for a given policy, we calculated a between-class difference score averaged over the 12 effect ratings for each policy. Specifically, we computed the absolute difference between the medians of the two classes on each policy outcome and then averaged these differences over the nine quantitative outcomes; we then did the same for the three qualitative outcomes (see Table 4.5). When looking at the quantitative outcomes, there was the most agreement between classes of experts for expanded mental health prohibitions, required reporting of lost or stolen firearms, a media campaign to prevent child access, and a child access prevention law. On the qualitative outcomes, there was the most agreement for a media campaign to prevent child access, surrender of firearms by prohibited possessors, expanded mental health prohibitions, a stand your ground law, a child access prevention law, and required reporting of lost or stolen firearms. Thus, the policies for which there was the most agreement in respondents' favorability ratings of a policy also showed the most agreement in respondents' expected effects on important outcomes. In contrast, when looking across both the quantitative and qualitative outcomes, the two classes of experts differed sharply in their expected effects for four policies: universal background checks, requiring a license to purchase a firearm or ammunition, permitless carry, and a ban on "assault weapons" and high capacity magazines.

In Table 4.6, we present each group's perception of the policy (or policies) that would lead to the largest benefits on each outcome and the policy that would lead to the largest harms. In nearly all cases, the two groups disagreed about which policies would lead to the greatest effects. Permitless carry emerged as a particularly polarizing policy, with the permissive class viewing it as highly beneficial for the right to bear arms and satisfaction of gun ownership and the restrictive class viewing it as leading to increases in violence and crime. However, both groups perceived a child access prevention law and a media campaign to prevent child access as leading to the greatest reduction in accidental firearms deaths.

Table 4.5
Differences in Median Estimated Policy Effects Between Classes of Experts, by Outcome

Policy	1. Firearm Suicides	2. Firearm Homicides	3. Accidental Firearms Death	4. Mass Shootings	5. Other Violent Crime	6. Property Crime	7. Hunting and Sport Shooting	8. Legal Acts of Defensive Gun Use	9. Sales of New Firearms	10. Right to Bear Arms	11. Individuals' Privacy	12. Satisfaction of Gun Ownership	Average Quantitative Difference[a]	Average Qualitative Difference[b]
1 Universal background checks	–10.0	–14.1	–5.0	–9.0	–4.0	0.0	8.8	9.6	3.2	2.5	2.6	2.9	6.6	2.8
2 A ban on sale of "assault weapons" and high capacity magazines	0.0	–6.4⇔	–1.1	–15.0	–6.0	0.0	9.5	4.1	8.7	3.0	2.0	1.0	5.4	2.3
3 A stand your ground law	0.0	15.0⇔	1.0	0.0	10.0	13.3	0.0	5.0	3.4	–1.9	–0.1	–0.7	5.0	1.6
4 Expanded mental health prohibitions	–2.3	0.0	–2.1⇔	–0.2	–1.7	0.2	2.7	2.7	0.0	1.8	1.0	1.0	1.4	1.3
5 Required reporting of lost or stolen firearms	–1.0	–5.0	–1.0	–1.3	–5.0	–3.0	0.0	0.0	0.0	1.0	1.9	1.0	1.7	1.6
6 Requiring a license to purchase a firearm or ammunition	–10.0	–11.7	–8.5	–5.0	–1.6	–3.8⇔	10.7	5.3	11.7	3.0	1.7	2.1	7.1	2.6
7 Required reporting and recording of firearms sales	–5.0	–16.0⇔	–5.0	–5.0	–3.6	–1.0	5.0	0.6	0.2	2.2	1.1	1.7	4.4	2.3
8 A child access-prevention law	–5.0	–1.5	–5.0	0.0	0.0	0.0	1.3	5.0	0.0	1.7	1.3	1.0	2.0	1.6
9 A media campaign to prevent child access	–6.7	–2.0	–5.0	–1.0	0.0	0.0	0.0	3.0	0.0	0.0	0.8	0.6	1.8	1.1
10 Surrender of firearms by prohibited possessors	–6.9	–14.0	–4.8	–4.5	–5.0	–0.7	1.0	0.0	0.0	1.0	1.4	0.0	3.8	1.1

Table 4.5—Continued

Policy	1. Firearm Suicides	2. Firearm Homicides	3. Accidental Firearms Death	4. Mass Shootings	5. Other Violent Crime	6. Property Crime	7. Hunting and Sport Shooting	8. Legal Acts of Defensive Gun Use	9. Sales of New Firearms	10. Right to Bear Arms	11. Individuals' Privacy	12. Satisfaction of Gun Ownership	Average Quantitative Difference[a]	Average Qualitative Difference[b]
11 Firearm and ammunition taxes	–1.7	–2.3	–2.0	0.0	0.0	0.0	12.2	3.4	9.9	2.2	2	1.5	3.4	2.2
12 Minimum age requirements	–4.8	–5.0	–5.0	–4.0	–0.9	0.0	8.1	2.5	1.0	2.4	1.5	1.7	3.4	2.1
13 Permitless carry	2.0	13.2⇔	7.4	11.5⇔	15.0⇔	5.1⇔	0.0	–4.6	0.0	–3.0	–2.0	–1.0	6.2	2.0
14 Requiring a ten-day waiting period to purchase a firearm	–14.3	–7.8	0.0	–5.0	–1.1	0.0	1.6	2.7	2.9	2.0	2.5	2.6	3.7	3.0
15 The elimination of gun-free zones	0.0	15.0⇔	0.0	17.7⇔	2.8⇔	4.5	0.0	–8.0	3.8	–2.0	–1.0	–1.6	5.4	1.9

NOTES: The table presents differences in group medians (the median response value for the restrictive class minus the median response value for the permissive class). As noted previously, we imputed responses that were planned to be missing. See Appendix C for detailed results.

[a] The mean of the absolute value of the between-class differences in medians across the nine quantitative outcome measures (1–9).

[b] The mean of the absolute value of the between-class differences in medians across the three qualitative measures (10–12).

⇔ The medians of the two groups are on opposite sides of the response scale's central point (100 for outcomes 1–9, 4 for outcomes 10–12, and 3 for the opinion item).

Table 4.6
Policies Rated as Most Beneficial and Most Harmful for Each Outcome, by Expert Class

Outcome		Most Beneficial Policy		Most Harmful Policy	
		Permissive Class	Restrictive Class	Permissive Class	Restrictive Class
1	Firearm suicides	Expanded mental health prohibitions	Ten-day waiting period	None	None
2	Firearm homicides	Stand your ground law, Elimination of gun-free zones (tie)	Surrender of firearms by prohibited possessors	Required reporting and recording of firearms sales	Permitless carry
3	Accidental firearms deaths	Child access prevention law, Media campaign to prevent child access (tie)	Child access prevention law, Media campaign to prevent child access (tie)	None	Permitless carry
4	Mass shootings	Elimination of gun-free zones	Ban on sale of "assault weapons" and high capacity magazines	None	Permitless carry
5	Other violent crime	Stand your ground law, Permitless carry (tie)	Required reporting of lost or stolen firearms, Surrender of firearms by prohibited possessors (tie)	Ban on sale of "assault weapons" and high capacity magazines	Permitless carry
6	Property crime	Stand your ground law	Required reporting of lost or stolen firearms	Requiring a license to purchase a firearm or ammunition	None
7	Hunting and sport shooting	None	None	Firearm and ammunition taxes	None
8	Legal acts of defensive gun use	Elimination of gun-free zones	Stand your ground law	Universal background checks	None
9	Sales of new firearms	Permitless carry	Stand your ground law, Permitless carry (tie)	Requiring a license to purchase a firearm or ammunition	Firearm and ammunition taxes

Table 4.6—Continued

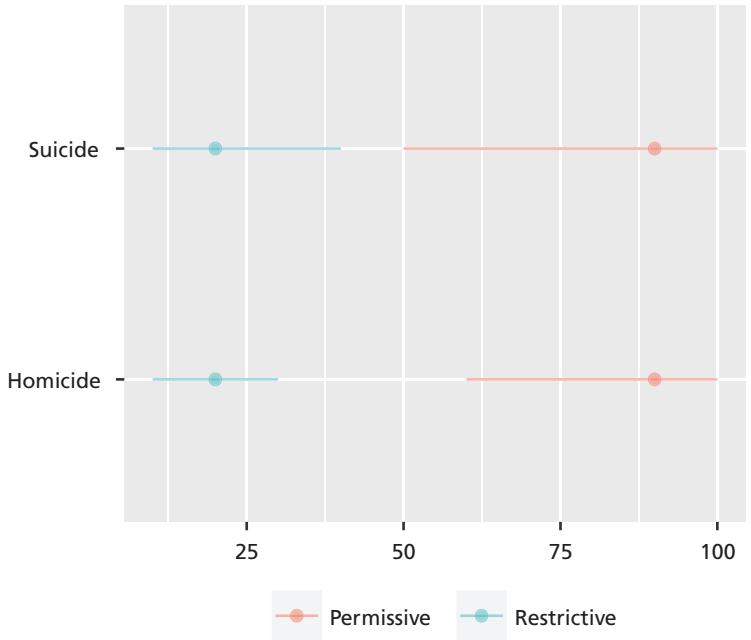
Outcome	Most Beneficial Policy		Most Harmful Policy	
	Permissive Class	Restrictive Class	Permissive Class	Restrictive Class
10 The right to bear arms	Permitless carry	None	Ban on sale of "assault weapons" and high capacity magazines, Requiring a license to purchase a firearm or ammunition (tie)	None
11 Individuals' privacy	Permitless carry	None	Universal background checks	Expanded mental health prohibitions
12 The satisfaction of gun ownership	Permitless carry	Permitless carry	Universal background checks	Ban on sale of "assault weapons" and high capacity magazines

NOTE: "None" indicates that no studied policy was seen by the median group member as creating a beneficial (or harmful) effect of more than 2 percent for quantitative items or one-half a scale point for qualitative items. For this analysis, we treated increases in hunting and sport shooting, legal acts of defensive gun use, and sales of new firearms to be beneficial.

Beliefs About Lethal Means Substitution

In a separate analysis, we examined whether beliefs about the substitution of lethal means were important to explaining how favorable experts were toward policies. There were large differences by expert class in estimates of whether other lethal means would be used in place of guns for suicide and homicide (Figure 4.2; see Appendix C for detailed results). The median respondent with more-permissive regulatory preferences indicated that if a policy successfully reduced a state's firearm suicides, 90.0 percent of the prevented suicides would still end as a suicide by some other means; the median respondent with more-restrictive regulatory preferences responded that 20.0 percent would still end as a suicide. These same levels of substitution were found for homicide: Median responses by the permissive class suggested that 90 percent of

Figure 4.2
Suicide and Homicide Substitution Effects, by Expert Class



NOTE: The circles indicate the group medians (50th percentile), and the lines indicate the interquartile ranges (25th to 75th percentile). The figure depicts perceived substitution of lethal means: Respondents indicated what percentage (0–100 percent) of potential firearm suicides and homicides prevented by a policy would still end in death (that is, in what percentage of cases a different means would be substituted).

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prevented firearm homicides would end as a homicide by another means, and median responses by the restrictive class estimated that just 20 percent would. The middle 50 percent of responses from each group (i.e., between the 25th and 75th percentile) did not overlap.

CHAPTER FIVE

Correlates of Experts' Divergent Views

The previous chapter demonstrates that the two classes of experts have widely divergent views about the merits and effects of many gun policies. These disagreements could result from differences on what the goals of gun policies should be. For example, experts in the permissive class might feel that it is most important to protect gun owners' civil rights, while experts in the restrictive class might feel that it is most important to prevent firearm deaths. Alternatively, the two classes of experts could be trying to achieve the same policy goals but disagree on factual questions about which policies are most likely to achieve those aims. For example, both types of experts might consider preventing homicides the most important goal of firearm regulation but disagree on which policies will produce that effect. This distinction is important. In the first case, where groups do not share the same values, there may be no factual or scientific analysis that can be conducted to help resolve disagreements on the merits of policies. On the other hand, if there is a shared set of goals for these policies but disagreement on their empirical effects, then scientific research that clarifies the effects of these policies may reduce the disagreement among policy experts.

In this chapter, we examine whether experts' favorability ratings (i.e., overall opinions) about gun policies and their judgments about the likely effects of these policies can be used to clarify whether their differences stem from differences in preferences, differences on factual matters, or some combination of the two. To examine these alternative explanations, we model policy ratings as a function of experts' assessments of their likely effects on the 12 outcomes. If experts share a common set of preferred outcomes—but perhaps disagree on which policies are most likely to achieve those outcomes—we would expect a single model to predict policy ratings well for both groups of experts. That is, the outcomes that are most influential in determining how members of the permissive class evaluate a policy will be the same as those determining how members of the restrictive class evaluate a policy, and these effects will be similar in magnitude and direction. Alternatively, if groups value policy outcomes differently, we would expect a single model not to perform well unless group differences on some outcomes (the interaction terms, described later) are accounted for in the model.

Overview of Methods

With the statistical modeling, we had three goals. First, we wanted to assess the extent to which favorability ratings could be predicted by the respondents' beliefs about the effects of each policy, without including information about respondents' class membership or other indicators of their ideologies and value systems. Second, we wanted to assess which of the several effects each policy produces are most associated with experts' overall opinions of the policies—and thus may be drivers of their favorability ratings. For example, how important is a 1-percent reduction in firearm homicides versus a 1-percent reduction in hunting and sport shooting for predicting experts' favorability ratings of the policy? Third, we wanted to assess whether the two classes of experts appear to value the policy effects differently. For example, is the relationship between favorability rating of a policy and the policy's effect on firearm homicide the same for experts favoring permissive gun policies as for those favoring restrictive policies? Do experts from the two groups appear to value these societal outcomes differently?

We used Bayesian regression models to predict overall policy ratings across all respondents as a function of the 12 policy effect estimates corresponding to each outcome. In some models, we also allowed those 12 effect estimates to interact with the class of expert (permissive versus restrictive) to directly investigate evidence that members of the two classes weigh policy outcomes differently in their favorability ratings of policies. The models do not contain any individual-level predictors (e.g., an indicator of class of expert or fixed effects for experts) but are designed to explore how well experts' favorability ratings can be explained solely through the experts' expectations about how each policy would affect important societal outcomes.

We used ordinal logistic regression to predict the experts' overall policy favorability ratings. To avoid overfitting, we do not present a model that includes all possible main effects and interactions, but we excluded terms from the model when doing so improved the overall model fit. The final, best-fitting model is presented in this chapter, while details of the model development process, selection of Bayesian prior probability distributions, statistical assumptions, and sensitivity tests are presented in Appendix B.

The interaction effects are parameterized such that the main effect is the average coefficient across the two classes of experts. The interaction is computed to be orthogonal to that main effect and represents the difference in the coefficient between the permissive and restrictive classes, as discussed in Appendix B.

The effects for the quantitative policy outcome scales were rescaled to reflect an a priori hypothesis about nonlinearity of the policies' effects. Specifically, we hypothesized that these predictors will have larger effects when their values are near 100 percent (no change) than when the estimates are large or small. For example, a policy that is seen as preventing 1,000 homicides and a policy that prevents 1,100 homicides will be judged quite similarly, despite the 100 additional homicides. In contrast, a policy that prevents 50 homicides will be judged more favorably than one that causes 50 homicides, even though this is also a difference of 100 homicides. To incorporate

this nonlinearity, we estimated a model in which the effects are linear between values of 90 percent and 110 percent, and variability in the predictors outside of this range has no effect on the predictions. Thus, the model coefficients can still be interpreted as linear effects of a 1-percentage-point increase of the predictor (on the log-odds of a more favorable policy rating) for all differences in the predictor between 90 percent and 110 percent. See Appendix B for additional information about this model nonlinearity, including comparisons with other plausible assumptions.

Results

Assessment of Model Fit

The best-fitting model of favorability ratings included effects on ten of the 12 main effects on outcomes (effects on other violent crime and the satisfaction of gun ownership did not contribute to model fit after accounting for other factors) and three terms representing the interaction of expert class by effects on firearm suicides, legal acts of defensive gun use, and individuals' privacy. This final model explained experts' policy favorability ratings extremely well, with a squared polychoric correlation of 0.76 between actual and model predicted favorability ratings, where model predicted ratings are the median values of the highest probability ratings across the model's posterior distribution. Assuming average reliability for a single Likert scale response, this is near the theoretical limit of how well any Likert item can be predicted. Importantly, the model fit the data well for members of the permissive and restrictive classes: $R^2 = 0.81$ for the permissive class and 0.70 for the restrictive class. Across 143 individual policy ratings made by experts in the permissive class, the median posterior estimate exactly matched the experts' rating 55 percent of the time and fell within one scale point of the correct value in 93 percent of all cases. Across 703 policy ratings by experts in the restrictive class, the median posterior estimate matched the true value in 62 percent of all ratings and fell within one scale point of the correct value 94 percent of the time.

Table 5.1 further illustrates this fit by comparing model-based estimates of policy ratings with the actual policy favorability ratings provided by members of each group. These policies were rated on a five-point Likert scale (values 1 to 5). Across policies, the average difference between mean predicted ratings and mean actual ratings for the permissive class was less than one-third of a scale point (0.31), or about one-fifth of a standard deviation on this scale, which has a standard deviation of 1.47. No predicted mean value differed from the true value by more than 0.7 of a scale point. For experts in the restrictive class, the model was even more precise, with an average divergence across policies of just 0.13 of a scale point (or less than one-tenth of a standard deviation), and no policy had a mean predicted score more than 0.6 of a scale point different from the true mean. In short, the model almost perfectly reproduces the widely divergent views across these two classes of experts in their favorability ratings of policies.

Table 5.1
Comparison of Model-Based and Actual Mean Policy Ratings, by Policy and Expert Class

		Permissive Class		Restrictive Class	
Policy		**Actual**	**Predicted**	**Actual**	**Predicted**
1	Universal background checks	2.0	1.3	4.9	4.8
2	A ban on sale of "assault weapons" and high capacity magazines	1.7	1.7	4.6	4.6
3	A stand your ground law	3.9	4.1	1.6	2.3
4	Expanded mental health prohibitions	2.4	3.0	4.4	4.5
5	Required reporting of lost or stolen firearms	2.8	2.3	4.5	4.4
6	Requiring a license to purchase a firearm or ammunition	1.4	1.6	4.7	4.7
7	Required reporting and recording of firearms sales	1.7	1.6	4.6	4.6
8	A child access prevention law	2.2	2.3	4.4	4.3
9	A media campaign to prevent child access	3.0	3.5	4.4	4.4
10	Surrender of firearms by prohibited possessors	2.7	2.6	4.8	4.7
11	Firearm and ammunition taxes	1.5	1.5	3.9	4.0
12	Minimum age requirements	2.2	1.9	4.4	4.4
13	Permitless carry	4.1	4.7	1.5	1.5
14	Requiring a ten-day waiting period to purchase a firearm	1.7	1.8	4.6	4.6
15	The elimination of gun-free zones	4.2	4.7	1.9	2.4

The interaction terms in the final model suggest that three policy effects (on firearm suicide, legal acts of defensive gun use, and individuals' privacy) have different associations with overall policy ratings for the two classes of experts. These interactions may reflect differences in which effects on outcomes the groups most value when rendering judgments of the overall merits of policies. To understand how important these interaction terms are to our overall model fit, we ran a comparison model consisting of only the ten main effects in the final model (i.e., excluding the three interaction effects). The correlations between predicted and true favorability judgments for the models with and without interactions barely differed, suggesting that interactions play a minor role in explaining those judgments. Specifically, although the polychoric R^2 for the association of predicted favorability ratings to true ratings was 0.71 for both expert classes in the full model with interactions, R^2 was 0.82 for the permissive class and 0.70 for the restrictive class in the model without interactions. This suggests little, if any,

erosion of model performance when interaction terms are removed. Similarly, although the full model had median rating predictions that exactly matched respondent policy ratings for 53 percent of experts' judgements in the permissive group and 62 percent of judgments in the restrictive group, exact matches for predictions from the model without interactions declined to just 50 percent and 60 percent for the permissive and restrictive groups, respectively.

In short, therefore, while there is evidence that some effects on outcomes are valued differently by experts who prefer permissive versus restrictive gun policies, the large divergence in favorability ratings between these two classes is very well explained even when we ignore those interactions. Furthermore, the large divergence in which of a policy's many possible effects each expert class values the most is well explained by the fact that the classes have different expectations about how the policies will affect a range of societal outcomes. That is, the different favorability ratings exhibited by each class of experts reflect that the classes have differing beliefs about the effects these policies will have on, for instance, homicides, not that one class prefers policies that achieve one set of objectives while the other class seeks to achieve a different set.

Model Results

Table 5.2 summarizes mean effect estimates from the model's posterior distribution, along with 95-percent credibility intervals for these means, all expressed as odds ratios (ORs). Effect odds ratios for the quantitative predictors are interpreted as the odds ratio of providing a more positive policy favorability rating that is associated with a 1-percentage-point shift in the predictor. Because the predictors use two different scales (some are assessed on a Likert scale and others a percentage change scale), Table 5.2 also provides standardized odds ratio coefficients, interpreted as the odds ratio for giving a more positive policy favorability rating that is associated with a one-standard-deviation change in the predictor.

As evident from the standardized coefficients, reduction in firearm homicides had the greatest association with more-favorable policy ratings of all measured effects on outcomes (i.e., it had an odds ratio that was furthest from 1.0). The standardized odds ratio for this effect indicates that a policy seen as producing a one-standard-deviation increase in firearm homicide deaths predicts a change in the odds of receiving a more favorable rating by a factor of 0.41, which represents a substantial decline in the favorability ratings. Conversely, a policy leading to a one-standard-deviation decrease in firearm homicides would suggest that policy has odds greater than two to one (1/0.41 = 2.44) of receiving a higher rating. We found little evidence that the role of expected effects on firearm homicides in predicting overall policy ratings differed by class of expert, and this interaction effect was excluded from the final model because of evidence that it was more parsimonious to assume the effect to be zero than to estimate it (see Appendix B for details).

Table 5.2
Final Model Coefficients

Outcome		OR	OR 95% Credibility Interval		Standardized OR
			2.5%	97.5%	
1	Firearm suicides	0.87	0.83	0.91	0.63
2	Firearm homicides	0.81	0.77	0.84	0.41
3	Accidental firearms deaths	0.97	0.93	1.00	0.89
4	Mass shootings	0.91	0.88	0.96	0.72
6	Property crime	0.95	0.90	1.00	0.88
7	Hunting and sport shooting	1.15	1.09	1.22	1.36
8	Legal acts of defensive gun use	0.93	0.89	0.98	0.82
9	Sales of new firearms	0.96	0.93	1.00	0.87
10	Right to bear arms	1.14	1.00	1.37	1.11
11	Individuals' privacy	1.41	1.21	1.69	1.27
Class * firearm suicides		1.26	1.11	1.42	1.27
Class * individuals' privacy		0.87	0.83	0.91	0.63

Fit Statistics	Estimate	Standard Error
p_waic	16.3	0.9
waic	1,616.9	51.2

NOTES: *Standardized OR* is the odds ratio for a one-standard-deviation change in the predictor; *p_waic* is the estimated effective number of parameters; *waic* is the Watanabe-Akaike widely applicable information criterion (WAIC). Odds ratios for the right to bear arms and individuals' privacy that are greater than 1.00 indicate that policies are more likely to receive a higher favorability rating when they are viewed as strengthening these rights rather than threatening them.

The effect of firearm suicides on policy favorability ratings is the second-most influential, with an effect size a little over half as great as that for homicides. Across all experts, policies associated with a one-standard-deviation increase in firearm suicides have odds of receiving a higher favorability rating by a factor of 0.63. Equivalently, policies that reduce firearm suicides by one standard deviation are predicted to have higher favorability ratings, with the odds of a more favorable rating increased by a factor of 1.59. For this variable, there was evidence of an interaction by the type of expert, with the policy favorability ratings for the permissive class being more strongly predicted by the policy's effects on suicide than was the case for the restrictive class. The credibility interval on this interaction gives us strong evidence that this interaction effect is differ-

ent from no effect (OR = 1). To interpret the size of this effect, it is useful to combine the firearm suicide main effect and interaction by class to produce separate main effects for each of the two classes. This combined standardized odds ratio is 0.33 for the permissive group and 0.71 for the restrictive group. Thus, there is a stronger association between a policy's expected effect on suicide and overall favorability for the restrictive group than for the permissive group, which is consistent with the experts in the restrictive class considering suicide prevention a more important policy goal than experts in the permissive class, when other policy effects are held constant.

After firearm homicide and firearm suicide effects, the most-influential effects on policy favorability judgments are a policy's effects on mass shootings and on hunting and sport shooting, both of which have standardized effect sizes about half as great as that for firearm homicides. Other effects held equal, policies that reduce mass shootings or increase participation in hunting and sport shooting are seen more favorably by both classes of experts.

Individuals' privacy has a small main effect but also an interaction term that remained in the model. Policies that threaten individuals' privacy are associated with less-favorable ratings for both classes. The combined main effect of individuals' privacy on favorability judgments has a standardized odds ratio of 1.34 for the permissive group and 1.12 for the restrictive group. That is, strengthening individuals' privacy rights is associated with slightly greater favorability ratings for the permissive than the restrictive class of experts.

The remaining effects are all quite small relative to the firearm homicide effect. Legal acts of defensive gun use, property crime, sales of new firearms, and accidental firearms deaths each have comparatively small overall standardized odds ratios. This is not to suggest that these factors are unassociated with favorability ratings. Rather, it suggests that they do not predict favorability ratings while controlling for other factors in the model, some of which may be highly correlated with these variables. For instance, the correlation for ratings of the effects of policies on individuals' privacy and on the right to bear arms is 0.65. As a consequence of these associations, the model is often unable to reliably determine which of these effects is larger, and their credibility intervals substantially overlap. The overall model results clearly suggest that the effect of the policies on firearm homicides is more important to respondents' favorability ratings than the effects of the other outcomes. In contrast, the model does not clearly distinguish which of the smaller effects are most important. Differences in magnitude of the effects among these smaller effects (including the two factors that were dropped from the model on the basis of parsimony) are not reliably estimated.

CHAPTER SIX

Developing a Gun Policy Comparison Tool Using Survey Data

One of the goals of the survey was to facilitate the development of a web-based policy comparison tool that would allow users to get expert predictions about the possible impacts of various gun policies or combinations of policies. In this chapter, we describe the methods, assumptions, and limitations of this approach.

A key simplification we make to construct estimates of the combined effects of policies or laws is that the expected effect of any policy is independent of the effects of any other policy. That is, if an expert expects a ban on the sale of "assault weapons" and high capacity magazines to result in firearm homicides declining to 0.9 times the level before implementing the ban, this same effect would be expected whether or not any other policies were simultaneously introduced. Thus, the combined effect of multiple policies on firearm homicides (or other outcomes) is simply the multiplicative product of the effects on firearm homicide that the expert predicts separately for each policy. Obviously, a shortcoming of this assumption is that policies may have interaction effects that are ignored in this procedure. For example, a child access prevention law and a media campaign to prevent child access may have overlapping effects on accidental firearm deaths among children, in which case we might not expect the net effect of these two policies to be as large as the sum of their effects if implemented separately. In contrast, other policies might amplify one another's effects on a given outcome if implemented in tandem. We do not know whether such interaction effects occur in real life or whether the gun policy experts we surveyed would expect such interaction effects.

The second important assumption is that the effect of eliminating a policy is the reciprocal of the expected effect from introducing it. Suppose, for instance, the expected effect of introducing a law is 0.9. Thus, after introducing the law, a state with a base rate of 1,000 would expect only 900 after implementation of the law. By assuming that eliminating a law has an effect equal to the reciprocal of the effect of implementing the same law, we are saying that if a state with a base rate of 900 eliminates this same law, the outcome would go from 900 back to 1,000: 900 / 0.9 = 1,000. This assumption, too, likely introduces errors into our estimates of the combined effects of policies or laws. For instance, suppose implementation of a permitless carry policy encouraged more people to purchase handguns and that, as more people owned hand-

guns, the frequency of firearm suicides increased. It is not obvious in this scenario that eliminating the permitless carry policy would result in a decline in handgun ownership equal to the increases associated with implementing the policy, so it may not be the case that firearm suicides decline as rapidly or as far as they increased when introducing the policy.

In Table 6.1, we provide two examples of policy combinations. First, we combine two of the policies—expanded mental health prohibitions (policy 4) and required reporting of lost or stolen firearms (policy 5)—for which the two groups differ least in their ratings of the policies' effects on firearm homicide (outcome 2). Then, we combine two of the policies—a ban on sale of "assault weapons" and high capacity magazines (policy 2) and a stand your ground law (policy 3)—for which the two groups differ most in their ratings (one of which is favored by each group) of the policies' effects on individuals' privacy (outcome 11).

In the online policy comparison tool (see RAND Corporation, 2018a), the median values of such policy combinations across experts are used to construct estimates of the joint effects of laws selected by users to enact or repeal, according to the judgments of our expert classes. Separate such estimates and their interquartile ranges are constructed for each expert class. Because the comparison tool requires a value from each respondent on every policy effect, six respondents (two from the permissive class and four from the restrictive class) that had missing values are omitted from the tool's calculations.

Such a strategy of evaluating sets of policies, rather than evaluating individual policies in isolation, may be productive for developing solutions that are acceptable to individuals with distinct perspectives in the gun debate. Further efforts in this vein should address potential interaction effects among policies that are combined.

The effects of these combined estimates will differ by state, however, because states that already have a law the user chooses to enact nationally are assumed to experience no changes in outcomes as a result of that change. Similarly, states with no law of a kind turned off or repealed by the user are unaffected by that change. By summing the net effects of any combination of laws selected by users across states, taking into account the state-level base rates of each outcome and whether and how the laws selected by users affect each outcome (according to the two expert classes), we calculate the net effects of any combination of laws on national outcomes. Additional details on the data sources used, assumptions made to generate state-level base rates on each outcome, and information on current state laws are provided in Appendix D.

We asked experts about average expected state effects and apply these average effects to all states in the gun policy comparison tool, regardless of what might be important differences among states in the likely effectiveness of the laws. A core assumption of the model, therefore, is that experts' effect estimates can be applied equally to all states in the country. This, too, is a strong assumption that likely affects the validity of results produced by the comparison tool. For instance, experts might make quite different ratings of a law's effects in more-rural states versus more-urbanized states.

Table 6.1
Example Policy Combination Scores

	Quantitative Combination			Qualitative Combination		
	Policy 4: Outcome 2 (%)	Policy 5: Outcome 2 (%)	Combination of Policies 4 and 5: Outcome 2 (%)	Policy 2: Outcome 11	Policy 3: Outcome 11	Combination of Policies 2 and 3: Outcome 11
Calculations of combination scores at the respondent level						
Respondent 18	86.70	100.00	86.70	3.50	1.57	2.53
Respondent 19	99.00	95.00	94.05	3.00	4.00	3.50
Respondent 20	95.00	95.00	90.25	3.74	4.00	3.87
. . . [all other respondents]						
Descriptive statistics for these scores, by class of expert						
Permissive-class median	95.00	100.00	95.40	2.02	4.60	3.40
Restrictive-class median	95.00	95.00	91.20	4.00	4.00	4.00

NOTE: Combination scores were calculated at the respondent level. We then obtained descriptive statistics for each combination score within each expert class. Combination scores were calculated at the respondent level, using multiplication for quantitative outcomes and averaging for qualitative outcomes. In the examples provided, some respondent-level values are not round numbers, because of the imputation procedures described in Chapter Three.

Using the Gun Policy Comparison Tool

The online tool (RAND Corporation, 2018a) allows users to observe the effects of turning a policy on (implementing a law nationally), turning a policy off (repealing a law nationally), or leaving the policy unchanged in every state, as would be expected by each of our expert groups. Users are presented with the 15 policies discussed in this report and can turn on, turn off, or leave unchanged as many of those policies as they wish, which offers more than 14 million policy combinations to explore. After selecting a combination, users select a "see results" button and are taken to a pair of state maps showing state-level estimates of the effects of the policy combination on one of the 12 outcomes examined in the report. One map displays the results expected based on effect estimates by the permissive class, and the other map shows expectations based on estimates by the restrictive class. Users can choose any of the 12 outcomes to be displayed on the maps and can see detailed state-level information on the effects of the policy combinations by hovering the cursor over individual states.

In addition to state-by-state estimates, the results page shows a national estimate that aggregates the state-level effects of the policies. Therefore, a 1-percent reduction in firearm homicides in a state that accounts for a large percentage of homicides nationally will exert a larger effect on changes in the national rate of firearm homicides than would a 1-percent reduction in firearm homicides in a state with only a small share of total homicides nationally. For qualitative outcomes (e.g., the right to bear arms), the national effect is the average of state effects, and state populations are used to weight this mean.

With this tool, users can explore which policies or combinations of policies are likely to achieve the effects of greatest interest to them, according to the two classes of experts. By searching for the combinations of policies for which both groups of experts see net benefits on key outcomes, it is possible to examine which combinations might offer the greatest promise of achieving benefits and achieving agreement between groups. We illustrate such an analysis in Chapter Seven.

Policy Opinion Scale

As part of the policy comparison tool, we wanted to let users know whether their own favorability ratings are more similar to the permissive or the restrictive class of experts from our survey. The algorithm used to classify experts was based on their favorability ratings for all 15 policies; however, excellent classification could be achieved with only a fraction of these policies. For this reason, we wanted to identify a brief scale that used favorability ratings of a few policies to classify the web users. We used responses to four policy favorability questions to construct a short scale to assess the extent to which others' opinions about the policies correspond most closely with those of the

expert class favoring more-permissive policies or the class favoring more-restrictive policies. The four policies were selected for the scale based on the size of the difference in median group responses, likely public familiarity with the policies, and conceptual differences among the selected policies to maximize user interest.

Scale scores are computed as the mean of each respondent's favorability ratings of requiring a license to purchase a firearm or ammunition (policy 6), required reporting and recording of firearms sales (policy 7), requiring a ten-day waiting period to purchase a firearm (policy 14), and the elimination of gun-free zones (policy 15), with policy 15 opinions reverse-scored. The scale on which users rate these policies is the same five-point scale we used with the experts, ranging from 1 ("very bad policy") to 5 ("very good policy"). Table 6.2 provides the descriptive statistics for the expert groups on this scale, which the online tool uses to describe the similarity between each web tool user and the two expert classes. As can be seen in the table, favorability ratings for this set of four policies differed dramatically across the two classes of experts (with means approximately six standard deviations apart), allowing us to identify which expert class the web user's opinion most closely resembles.

Table 6.2
Descriptive Statistics for Expert Classes on the Favorability Rating Scale for Four Policies

Expert Class	N	Mean	Standard Deviation	25th%	50th% (median)	75th%
Permissive class	16	1.55	0.55	1.19	1.38	1.81
Restrictive class	78	4.58	0.45	4.25	4.75	5.00

NOTE: We present the 25th percentile (first quartile), 50th percentile (median), and 75th percentile (third quartile) for each group.

CHAPTER SEVEN

Discussion and Conclusions

Gun policy debates in the United States are polarized and polarizing. We expected, therefore, that our survey of individuals whose work or research focuses on gun policy would identify groups of experts with distinct viewpoints on the merits of different gun policies and laws. After statistically clustering experts' favorability ratings of 15 gun policies, we found two distinct classes of experts who differed not just in their overall opinions about the merits of the policies but also strongly in their ratings of which advocacy or membership organizations had gun policy positions closest to their own.

The first class of experts preferred such policies as a stand your ground law, permitless carry, and the elimination of gun-free zones. Members of this group reported that their own views on gun policy were strongly aligned with those of organizations like the National Rifle Association, the National Sport Shooting Foundation, the Second Amendment Foundation, and Gun Owners of America. For ease of reference, we labeled this group the *permissive* class, comprising experts who favor more-permissive regulatory approaches to gun ownership and use.

The second class of experts preferred such policies as universal background checks, requiring a license to purchase firearms or ammunition, and surrender of firearms by prohibited possessors. Members of this group had gun policy positions that they indicated were closely aligned with organizations like the Brady Campaign to Prevent Gun Violence, Mayors Against Illegal Guns/Everytown for Gun Safety, the Violence Policy Center, and Coalition to Stop Gun Violence. We labeled this group the *restrictive* class, comprising experts who favor more-restrictive regulatory approaches to gun ownership and use.

In total, we received 16 usable responses from experts who favor more-permissive gun policies and 79 usable responses from those favoring more-restrictive policies. Because this was not a representative survey, the difference in the sizes of these groups provides no information about the relative numbers of experts in the community whose views align with one or the other perspective. The smaller number of respondents favoring permissive policies could indicate that the sample frame we used to invite expert participation underrepresented this group; that members of this group

were less inclined to participate in our survey when invited; or that there are, in fact, fewer of them than experts who favor more-restrictive policies. We cannot draw any empirical conclusions about which of these possibilities is correct.

Expert Groups' Assessments of Policies and Their Effects Showed Some Areas of Agreement

Despite the sharp differentiation between expert classes on their favorability ratings and their alignment with the policy positions of gun policy advocacy and membership organizations, the two groups' estimates of the likely effects of 15 policies on a wide range of outcomes—though clearly differentiated—often agreed on the likely direction of effects. Indeed, across 134 judgments about the effects of the policies, only 12 times did the median judgment for each group disagree on the direction of the effect. For instance, one group thought that the effect of a policy would be to increase the rate of firearm homicides, and the other group thought that the policy would decrease firearm homicides. More than half of these 12 instances concerned two policies: permitless carry and the elimination of gun-free zones. Those in the permissive class expected these policies to reduce firearm homicides; mass shootings; other violent crime; and, in the case of permitless carry, property crime. Those in the restrictive class expected the opposite.

Four policies appeared to generate comparatively strong agreement between classes of experts in their overall favorability ratings: expanded mental health prohibitions, required reporting of lost or stolen firearms, a media campaign to prevent child access, and surrender of firearms by prohibited possessors. For each of these policies, the median favorability rating of the group favoring more-permissive policies was neutral, and the median favorability rating of the group favoring more-restrictive policies was positive. There was relatively strong agreement on the direction and magnitude of expected effects of these policies on the outcomes examined, with three of the four showing stronger agreement between groups on the quantitative outcome measures than on any other policies, and all four being among the five policies for which there was the strongest agreement about their effects on the right to bear arms, individuals' privacy, and the satisfaction of gun ownership (the qualitatively measured outcomes).

Under the assumptions for combining experts' effect estimates across multiple policies described in Chapter Six, if these four policies were enacted in every state that does not already have them, this would produce an extraordinary benefit to society from the perspective of the restrictive class of experts, with dramatic reductions in gun violence, crime, and death, with only modest effects on legal acts of defensive gun use, participation in hunting and sport shooting, and gun sales (Table 7.1).

Table 7.1
Calculated Change in Outcomes Nationally After Implementing Four Gun Policies According to Effect Estimates, by Expert Class

Outcome	Permissive			Restrictive		
	25th%	50th% (median)	75th%	25th%	50th% (median)	75th%
Quantitative						
1 Firearm suicides	–14%	–10%	–5%	–34%	–23%	–15%
2 Firearm homicides	–13%	–9%	5%	–31%	–20%	–12%
3 Accidental firearms deaths	–14%	–3%	7%	–31%	–19%	–7%
4 Mass shootings	–12%	–8%	4%	–24%	–15%	–7%
5 Other violent crime	–5%	1%	4%	–20%	–12%	–4%
6 Property crime	–2%	1%	3%	–14%	–7%	–1%
7 Hunting and sport shooting	–9%	–5%	–1%	–4%	–1%	1%
8 Legal acts of defensive gun use	–14%	–8%	–2%	–6%	–2%	0%
9 Sales of new firearms	–7%	0%	8%	–7%	–1%	3%
Qualitative						
10 Right to bear arms	–1	–1	0	0	0	0
11 Individuals' privacy	–1	–1	–1	0	0	0
12 Satisfaction of gun ownership	–1	–1	0	0	0	0

NOTE: Table values show the net change in outcomes at the national level after implementing expanded mental health prohibitions, required reporting of lost or stolen firearms, a media campaign to prevent child access, and surrender of firearms by prohibited possessors in states that do not already have such laws. The 25th and 75th percentiles describe the interquartile range of combined effect estimates for the group.

The expectations of those in the permissive class are more divided and less uniformly favorable across outcomes, perhaps explaining these respondents' overall neutral favorability ratings of the four policies. This group tends to support the view that firearm deaths would be decreased by implementing the policies nationally, although only in the case of firearm suicide is there broad agreement on this point. There is also broad agreement that legal acts of defensive gun use and participation in hunting and sport shooting would decline, perhaps substantially, both of which would be viewed as unfavorable outcomes by many.

For experts in the permissive class, the combination of implementing the four policies would threaten or degrade the right to bear arms, individuals' privacy rights,

and the satisfaction of gun ownership by one point on the seven-point Likert scale, a change labeled "minimal threat" on the survey response scale. Experts in the restrictive class saw the combined effect of these policies as having little or no effect on the qualitative outcomes.

If the expectations revealed by our two classes of survey respondents are broadly representative of those held by advocates of more-permissive and more-restrictive policies on access to and use of guns, this would suggest that policies could be implemented across states that would, in the view of most stakeholders, lead to large reductions in many of the societal harms associated with gun use, although they would entail some harms to the right to bear arms, privacy rights, firearm sales, and participation in hunting and sport shooting. Because the median ratings of the overall merits of these policies ranged from neutral to highly favorable between groups, the modest harms might be counterbalanced by the perceived benefits, certainly for the group favoring more-restrictive policies, but possibly also for the group favoring a more permissive approach.

Nevertheless, a median neutral rating among experts in the permissive class is not a ringing endorsement, and there may be little motivation within this group to accept the uncertainties, intragroup conflict, and possible unintended consequences of endorsing such a plan. Our findings point to possible concessions that those in the restrictive class might consider to improve the attractiveness of a bargain among stakeholders while still achieving many of the desired societal benefits. For instance, according to our policy comparison tool (RAND Corporation, 2018a), if the elimination of gun-free zones were implemented along with the four other policies, both expert groups would still expect large reductions in firearm suicides, firearm homicides, and mass shootings, as well as smaller reductions in property crime, other violent crime, and accidental firearms deaths; at the same time, the negative effects of just the four policies on legal acts of defensive gun use and participation in hunting and sport shooting would be substantially moderated. Similarly, the negative net effects on the qualitative outcomes would be moderated in the view of the permissive class and would result in only small effects according to the restrictive class.

Good inferences about how opposing parties in gun policy negotiations might view such combinations of policies cannot be drawn from this study. The judgments of the expert classes may not be representative of the views of those likely to be influential in policy debates, and our approach to combining effect estimates requires strong assumptions that we cannot fully evaluate. We believe, however, that there is merit to considering the net effects of policies and laws on the interests and concerns of all or a wide range of stakeholders and to exploring tools like those we developed from the survey results, recognizing that the tools' chief value may be in generating hypotheses or ideas for compromise packages of laws that might gain the support of both sides of gun policy debates.

Experts' Estimates of the Probable Effects of Policies Are Broadly Consistent with Scant Available Science

Reviews of gun policy research routinely find that there are few rigorous studies of the effects of policies like those considered in this report, and when rigorous methods have been used to establish the causal effects, results have often been inconclusive because they appear to be highly sensitive to minor adjustments to the statistical modeling approach, have little statistical power (or chance of finding true effects of the policies), or suffer from other methodological limitations (Hahn et al., 2005; Lee et al., 2017; National Research Council, 2004; RAND Corporation, 2018b). The limited scientific evidence base is compounded by the fact that scientific publications on gun violence and other outcomes are far less common than publications on traffic fatalities, sepsis, or other causes of comparable numbers of deaths, which is itself a consequence of the federal government investing only about 1.6 percent as much in research on gun mortality as it does in research on those other causes of death (Stark and Shah, 2017).

Our own recent comprehensive review of the most-rigorous available studies on the effects of gun policies concurred that available evidence for the effects of policies on a wide range of outcomes is weak or that, in many cases, no rigorous studies are yet available (RAND Corporation, 2018b). Indeed, across the 104 policy and outcome pairs on which we searched for rigorous research, the strongest available evidence was for the effects of child access prevention laws and their effects on suicide and unintentional injuries, and for the effects of enforcing mental health prohibitions on gun purchases on homicide and other violent crime.

For child access prevention laws, we identified five studies sufficiently rigorous to meet our inclusion criteria, and four found these laws to be associated with reduced suicide or self-inflicted injuries among individuals 19 or younger, including statistically significant reductions in three of the studies (Cummings et al., 1997; DeSimone, Markowitz, and Xu, 2013; Gius, 2015; Webster et al., 2004). The fifth study found no evidence for an effect of these laws (Lott and Whitley, 2001). Across these studies, we extracted six estimates for the effect of strong child access prevention laws (i.e., excluding from this category laws that do not require safe storage of firearms) on firearm suicides among youths. Although these effects concerned a range of different age categories for individuals younger than 20, the median across the effects (an incidence rate ratio, or effect size, of 0.87) offers a rough idea of the central tendency of these estimates.

According to the Centers for Disease Control and Prevention (CDC)'s Fatal Injury Reports, there were 22,018 firearm suicides in 2015, of which 1,296 were among people aged 20 or younger (CDC, 2017). If child access prevention laws have a 0.87 effect on this group, that would correspond to preventing about 168 such suicides annually. This would correspond to a small effect (0.99) on all firearm suicides (all ages), the outcome on which our survey respondents estimated each policy's effects.

This effect is comparable to the median effect estimate of 1.0 (IQR: 0.98–1.0) made by experts favoring more-permissive gun policies. The median estimate of 0.95 (IQR: 0.90–0.98) among those favoring more-restrictive policies corresponds to an expectation of 1,101 fewer firearm suicides. Although it is not plausible that child access prevention laws could eliminate 1,101 of 1,296 youth firearm suicides, it is reasonable to assume that such laws could also reduce firearm suicides among individuals over age 20—for instance, among household members without access to locked weapons. This makes the 0.95 effect estimate more plausible. However, our review found no rigorous studies of the effects of these laws on individuals older than 20, so we cannot provide scientific evidence for what those effects might be.

Our review also found relatively strong evidence that child access prevention laws can reduce unintentional firearm injuries and deaths. For this outcome, there were 11 effect size estimates for various age ranges of individuals aged 19 or younger (Cummings et al., 1997; Gius, 2015; Hepburn et al., 2006; Lott and Whitley, 2001; Webster and Starnes, 2000). The median of these effects was 0.91. The CDC's Fatal Injury Reports indicate that there were 489 unintentional firearm deaths in 2015, of which 100 involved individuals aged 19 or younger (CDC, 2017). If child access prevention laws reduce these deaths by a factor of 0.91, that would suggest nine fewer such deaths. Our review identified just two studies with effect sizes for individuals older than 19: One study estimated an effect of 0.84 (95-percent confidence interval: 0.68–1.03) for those aged 20–24 (Cummings et al., 1997), and the other study estimated an effect of 0.88 (95-percent confidence interval: 0.63–1.22) for those aged 55–74 (Hepburn et al., 2006). Neither of these estimates was particularly precise, however. Applying the average of these effects to the 2015 unintentional firearm fatalities of those aged 20 or older suggests that child access prevention laws would prevent an additional 54 deaths in this age group, for a total of 63 prevented deaths out of 489, or an effect of 0.87 for the population.

Weaknesses in the scientific literature, particularly for unintentional firearm injuries of adults, leads us to suspect that this estimate is imprecise. Therefore, we regard both the estimate of 0.95 (IQR: 0.90–1.0) by experts favoring a more permissive approach to policy and the estimate of 0.90 (IQR: 0.80–0.97) by those favoring a more restrictive approach as broadly consistent with the scientific literature.

Our review identified less-robust evidence for the effects of stand your ground laws. Across three studies, all found these laws to be associated with increases in homicide rates, and two of the estimates were statistically significant. The median effect size across these studies was 1.10 (Cheng and Hoekstra, 2013; Humphreys, Gasparrini, and Wiebe, 2017; Webster, Crifasi, and Vernick, 2014). Stand your ground laws were one of the few types of policies on which experts estimated effects in opposite directions. Specifically, while experts in the restrictive class had a median estimated effect of 1.05, which is reasonably close to the median effect from the literature, experts in the permissive class expected a reduction in homicides by a factor of 0.90.

Our research synthesis report identified no other policies (of the 15 we queried experts on) for which the weight of available evidence suggested a particular effect. We conclude, therefore, that where rigorous scientific research has been conducted and found that policies have an effect, the expert judgments of those effects are broadly consistent with the evidence. The one exception we noted is that the survey experts disagreed about the likely direction of the effects of a stand your ground law, while the limited empirical literature is most consistent with an increase in homicide.

Substitution Effects Are an Important Point of Expert Disagreement

A striking result of the survey concerns the wide disparity between estimates made by the two expert groups about means substitution. That is, they disagree about the extent to which any reductions of firearm suicides and homicides attributable to a policy are undermined because individuals simply use other means to carry out suicide or homicide. The median respondent in the permissive class indicated that if a policy successfully reduced a state's firearm suicides, 90 percent (IQR: 50–100 percent) of the prevented suicides would still end as a suicide by some other means. In contrast, the median respondent in the restrictive class responded that 20 percent (IQR: 10–40 percent) would still end as a suicide. They produced the same stark differences in expected substitution of means for firearm homicides: 90 percent (IQR: 60–100 percent) for the permissive class and 20 percent (IQR: 10–30 percent) for the restrictive class. Those who favor more-permissive policies view reductions of firearm suicides and homicides as largely futile efforts because these outcomes will continue, largely uninhibited, through other means.

This wide disparity in views on means substitution effects may be an important impediment to reaching any consensus on firearm legislation. As we show in our modeling of experts' favorability ratings, the extent to which a policy is seen as affecting suicide and homicide rates is the best predictor of overall favorability ratings; for both classes of experts, preventing suicide and homicide are the key features of good firearm policies. Thus, this large disagreement on means substitution for homicide and suicide may help explain the lack of consensus on firearm policies. For example, if child access prevention laws could reduce firearm suicides by 1,000 cases per year but proponents of more-permissive policies interpret this as a net reduction of 100 suicides and proponents of more-restrictive policies interpret this as a net reduction of 800 suicides, then it is easy to imagine that they could disagree on the relative merits of the policy, even if they fully agree on everything else about it.

Unlike many other topics on which these groups might disagree, substitution effects concern a factual that could be empirically investigated. Indeed, there has been considerable research suggesting that some substitution of means does occur (e.g., Office of the Surgeon General and National Action Alliance for Suicide Prevention,

2012; World Health Organization, 2014; for reviews, see Azrael and Miller, 2016; Barber and Miller, 2014). Neither of our expert groups appears to disagree with this finding, or at least the median judgment of each group acknowledges that some lethal means substitution will occur. Instead, they disagree on how much means substitution should be expected, a question that is not well understood. It is connected with a series of open questions requiring further research, such as "When a suicidal person's access to a lethal method is blocked, what determines whether he or she substitutes a more lethal versus less lethal method, or abandons an attempt entirely?" (Barber and Miller, 2014, p. S270). Although few studies have provided rigorous estimates of the magnitude of means substitution (e.g., Reisch et al., 2013), available estimates are imprecise and, in some cases, have uncertain generalizability to the United States.

The uncertain magnitude of lethal means substitution represents a potentially important gap in the science—and one on which expert groups strongly disagree. As a result, we believe that improved information on the magnitude of expected substitution effects and the conditions and populations for which they might be expected could help build consensus views of gun policy effects.

Group Differences Concern Factual Questions, Not Different Policy Objectives

Views on the merits of the policies we studied are strikingly polarized, with almost no overlap in favorability ratings between the permissive and restrictive classes of experts. We examined whether these nearly diametrical perspectives result from differences in beliefs about the true effects of the policies or from differences of opinion about which outcomes matter most or should be the proper targets of gun policy.

Prior work has routinely concluded that differences in the expected effects of policies account for only a small or modest portion of the disagreements between opposing parties in gun debates (e.g., Hartnagel, 2002; Kleck, Gertz, and Bratton, 2009; Mauser and Margolis, 1992; Smith, 2000; Sorenson, 2015), leading some to suggest that at the root of policy disagreements are not questions of fact but instead deep cultural differences that might be harder to resolve (Kleck, Gertz, and Bratton, 2009). But these studies have typically considered a narrow set of possible gun policy effects, such as effects on homicide, gun violence, or gun crime. As is clear in debates over gun policies, many other effects of policies are critically important, such as how they might affect suicides, the right to bear arms, the right to defend oneself, unintentional firearm injuries, and the economic well-being of the gun industry.

To evaluate the role of factual information versus policy objectives in gun policy differences, we therefore included a much wider set of possible outcomes of policies. We reasoned that if disagreements about the true effects of policies explain differences in policy favorability ratings, then, in a model predicting favorability ratings, estimates

of the empirical effects of policies might explain favorability judgments in comparable ways for both groups of experts. On the other hand, if groups differ on which outcomes they value or prioritize (say, reductions in homicides or protection of the right to bear arms), this would be revealed by interaction terms in the model indicating between-group differences in how their expected effects of a policy predict their overall favorability rating for that policy. For example, one might hypothesize that experts in the restrictive class favor policies that they believe will reduce homicides, while those in the permissive class favor those they see as protecting the Second Amendment.

However, our results strongly suggest that experts' differing favorability ratings in the permissive and restrictive groups are explained largely, and indeed almost exclusively, by differences in assessments of what the true effects of the policies will be, not by differences in which policy outcomes predict their favorability judgments. Indeed, there was an overwhelming consensus between the groups that their preferred policies were those they saw as reducing firearm suicides and homicides. Secondary priorities in evaluating policies appear to be protecting privacy rights, facilitating participation in hunting and sport shooting, reducing mass shootings, and protecting gun rights.

Including interactions by class of expert provided almost no additional explanatory power in the model. When we reran the model without these interaction terms, model performance was barely degraded (explained variance fell from 0.71 to 0.70). This suggests that even though minor differences may exist in the policy goals for these two classes of experts, those differences were not associated with appreciable differences in experts' policy favorability ratings.

Our finding that group differences in favorability ratings are almost exclusively explained by differences in the experts' perceptions of the effects of the policies, not on which effects they value the most, might be questioned by those who wonder how influential our choice of prior distributions was for the interaction terms or by others who question whether the group favoring more-permissive policies was too small (n = 16) for such effects to be detected. Although reasonable, both concerns are substantially mitigated by the exceptionally good predictive performance of the final model, which explained more than 70 percent of variance in each group's favorability ratings—and indeed explained more of the variance in the smaller permissive class than in the restrictive class. Thus, if there are group differences that went undetected in our model, they are necessarily dwarfed in importance compared with the explanatory power of the differences in policy effect estimates.

Moreover, across the policies we considered, differences in experts' judgments about the policies' true effects usually concerned the magnitude of likely effects, not their direction. However, even when experts disagreed on the direction of effects, our results suggest that they are each trying to achieve the same ends. Thus, although both groups especially favor policies that they believe will reduce firearm homicides, one group believes that permitless carry will achieve that goal (i.e., that lawful citizens carrying guns will deter criminals), while the other group believes that policy will do the

opposite (that more people carrying guns will lead to a greater use of guns in interpersonal conflicts). Importantly, what appears to divide the groups is a question of fact that may be knowable: Does permitless carry increase or decrease firearm homicides?

The modeling shows that, regardless of whether respondents favor making gun laws more restrictive or more permissive, their favorability ratings were very well predicted by their factual beliefs about the effects of the policy on suicides, homicides, and other outcomes. This finding does not necessarily imply that the respondent's favorability ratings were caused by those beliefs. Our data are correlational and do not support strong causal inferences. For example, one's factual beliefs about the effects of a policy could have been affected by one's overall opinion of the policy, or they could be molded by advocacy groups that promote both policy positions and particular beliefs about the effects of a policy. Regardless of the causal origins of these beliefs about the effects of policies, disagreements in those beliefs may be a substantial impediment to improving gun policies. Whatever the origin of the belief, once someone believes that a given policy would increase the risk of suicide, homicide, and other harmful societal outcomes, it would be irrational for the individual to support that policy. One does not need to interpret our model as showing a causal effect of beliefs on favorability ratings in order to view those beliefs as critical factors in preventing compromise.

The fact that overall opinions of gun policies are associated with differences in beliefs about the results of factual questions rather than differences in values or policy objectives suggests an important role for new and improved collection of scientifically valid information about the true effects of gun policies. Nevertheless, we do not believe that such new information will be readily or easily accepted by those whose established views are contradicted by it. Individuals' views on gun policies—supporting a permissive or restrictive approach—are often important expressions of their identification with various social groups on which they depend psychologically and economically (Kahan, 2017). There is compelling evidence that the public and experts themselves selectively discount scientific information if accepting it could threaten ties or status within these important social groups, such as by damaging professional alliances, peer acceptance, or economic well-being (Kahan, 2016; Kahan et al., 2017; Koehler, 1993).

Therefore, we cannot conclude that differences in beliefs about a policy's true effects *cause* differences in experts' favorability toward those policies. Our analyses are equally consistent with the possibility that one or both groups of experts bend their assessments of the likely effects to match their overall opinions of policies. Interestingly, however, our findings suggest that gun policy disagreements may not be primarily driven by differences in what each group is hoping to achieve through gun policies. Instead, experts who favor more-permissive policies and those who favor more-restrictive ones appear to have a broadly similar set of values or objectives that lead them to agree on what policymakers should attempt to achieve through improved gun policy. That is, whether or not the experts truly believe that the laws they favor

will have those effects, the effects each group claims for the policies it favors suggest that both groups agree on what the objectives of gun policy should be and how much to weight each of the outcomes we examined. This may be a surprising finding to those on either side of gun policy debates who suspect that their opponents suffer from badly misplaced priorities, if not deep moral failings.

Because all of the policies we examined in this study have been implemented in some states, they all have records of effects that could be more carefully evaluated. For the past two decades, however, research on the effects of gun policy has not been a priority for the federal government. Indeed, as described previously, Stark and Shah (2017) found that federal investment in firearm research is just 1.6 percent what would be expected given the government's investments in other causes of mortality that have similar impact on the lives of U.S. residents.

We regard the analyses presented in this report as exploratory. They are based on a relatively small sample of experts and just 15 gun policies, all of which offer only marginal changes to existing U.S. regulations that have been found to be consistent with the Second Amendment. Our focus on 12 outcomes substantially improves on earlier such work but may still neglect important effects of gun policies. Because this is exploratory analysis, we regard our inferences from it as interesting hypotheses that should be further tested and examined in later work designed to confirm or refute them.

Recommendations

Our findings support the following tentative recommendations:

1. Those on each side of the gun policy debate should be aware that, despite some appearances, there is evidence that their opponents may share many of the same policy objectives. This may seem equally improbable to both sides, and there are certainly instances, people, or objectives for which this is not true. Nevertheless, recognizing that the principal sources of disagreement may lie in the means of achieving the shared goals rather than in what the goals should be could be useful in gun policy negotiations.
2. If 70 percent of the variance in experts' favorability ratings is explained by their beliefs about what the empirical effects of those policies might be, then the vast majority of policy disagreements are associated with factual questions about policies' true effects that are, in principle, knowable. For the past two decades, however, Congress has been reluctant to support the collection of new evidence on the factual questions at the heart of policy disputes. New and significant investment in the scientific study of gun policies, which would require the support of Congress, offers a promising available path for building consensus on gun policy. Because beliefs about gun policies are deeply entangled with per-

sonal and political identities, the credibility of new scientific information is certain to be challenged by those whose presumptions and group ideologies are contradicted by it. Nevertheless, because there may be general agreement on the objectives of gun policies, efforts to improve the science base on how to achieve those objectives through improved gun policy may help win converts to an expanded consensus view.

3. One factual question that appears to be of key importance concerns the magnitude of firearm substitution effects; that is, when firearm suicides or homicides are prevented, how many will still result in deaths by other means? Although both classes of experts typically believe that such substitution occurs, estimates of the magnitude of these effects vary dramatically. We believe that better information about this question could have implications for how people on all sides of gun policy debates evaluate the merits of individual policies. Therefore, we recommend that funders and researchers prioritize investigating questions about whether substitution occurs and the conditions under which it does or does not occur.
4. We recommend that gun policy analysts, those engaged in negotiations over policies, and the public explore how combinations of laws might affect each U.S. state and the trade-offs created when, in the view of one or both sets of experts, a group of policies improves one set of outcomes but undermines others. This can be done through the gun policy comparison tool (RAND Corporation, 2018a).

Overall, we believe that without new, more-rigorous, and more-conclusive scientific research estimating the effects of gun policies on the outcomes considered in this report, policymakers and the public will depend on their own beliefs about those effects and the beliefs of the experts they trust. While there are considerable differences of opinion about these effects among experts, there is very little solid empirical research that can currently resolve these differences of opinion (RAND Corporation, 2018b). Thus, for the time being, expert opinion may be the best guidance available for crafting fair and effective firearm policies.

APPENDIX A

RAND Survey of Firearms Experts

This appendix reproduces the text presented to survey respondents. Programmer instructions are listed in brackets and in blue font. Survey respondents saw all text shown in black. Item numbers are in red and were not displayed to respondents.

RAND Survey of Firearms Experts
(Version 8-5-16)

This survey is part of a research project estimating the likely effects of a wide range of gun policies, including both the benefits and harms of each. The results of this survey will provide valuable information on policies most likely to protect individual liberties, safety, and health.

We are reaching out to experts from across the spectrum of views on gun policy. You were nominated to participate in this survey by a national organization involved in firearms policy, or because of your prior work on this topic.

This survey asks for your expert judgments on the effects of different policies, and your policy preferences. We ask how these policies affect many outcomes, including health, crime, safety, individual rights, hunting and recreation.

Your participation is confidential: we will never disclose your responses in a way that identifies you. Participation is voluntary and should pose no risk to you. You may skip questions or discontinue participation at any time, although we hope you choose to complete the survey.

The survey takes about 45 minutes to complete. If you have any questions about this study, please contact the RAND principal investigator Andrew Morral at morral@rand.org or 703-413-1100 extension 5572.

[The following instructions are shown before the next set of questions (1–8) but are also accessible to the respondent during the evaluation of policies through an "instructions" link on each page.]

Our first questions ask you to estimate the effect of a policy on outcomes for a typical U.S. state.

- Unless stated otherwise, assume that before adopting the policy, the state's firearm laws do not go beyond federal firearm regulations.
- Use your expert judgment on how well the policies are likely to be implemented and enforced.
- If the policy would take a while to have an effect, indicate the maximum effect you would expect after sufficient time has passed.
- If your estimates depend on whether other states adopt the policy, assume that all states adopt the policy.

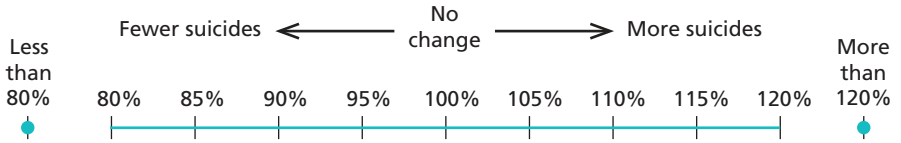
You may click the "Suspend" button at the bottom of each page to exit this survey at any time. When you return later to complete the survey, you will be taken to the last question you answered.

Click the "Next" button to begin.

[The presentation of policies and 12 follow-up questions are illustrated here using Policy 1. In an actual survey, each respondent is asked all 12 follow-up questions on a random subset (subset_size = 10) of the complete set of policies (total_policy_count = 16). Policies in the selected subset are presented in randomized order—except that, when selected, Policies 14–16 are presented after presentation of all other policies in the selected subset. Once the respondent completes all policies in the selected subset, the remaining policies (remaining_policy_count = total_policy_count – subset_size) will be presented in randomized order but with only a single follow-up question after each: Q13PX. These final remaining_policy_count policies should be preceded by the following instruction: There are a few more policies we would like to ask you about, but we will not ask you about their effects. Instead, we just want to know your overall opinion of each.

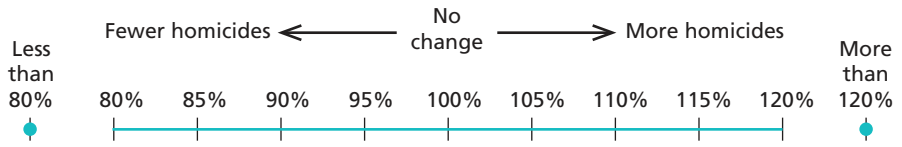
The policy should be bold in a text box and remain at the top of the page for questions 1–8 and 9–12.]

1. Q1PX If a state implemented universal background checks, how much would firearm suicides change? [The first time this question is asked, add the following sentence: This question is only about *firearm* suicide. Later we will ask about all suicides. Mark the suicide rate after implementing the law as a percentage of the rate before implementation by clicking on the black line or one of the endpoints.]

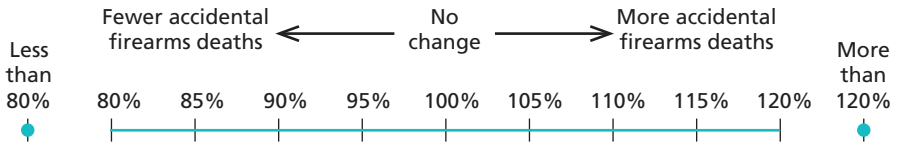


[For the first presentation of Q1PX only, show the following text: Example: if you select **93%**, you are saying that a state that had **1,000** suicides before implementing this policy would be likely to have **930** suicides after implementing this policy.]

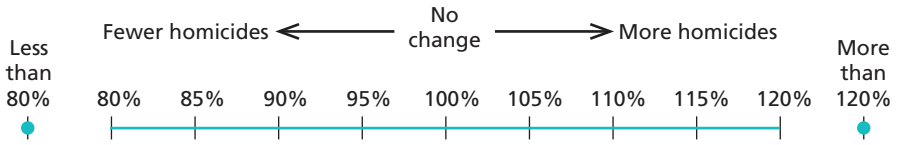
2. Q2PX If a state implemented universal background checks, how would firearm homicides change? [The first time this question is asked, add the following sentence: This question is only about *firearm* homicides. Later we will ask about all homicides.]



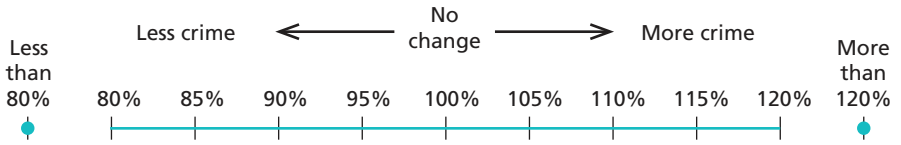
3. Q3PX If a state implemented universal background checks, how much would accidental firearms deaths change?



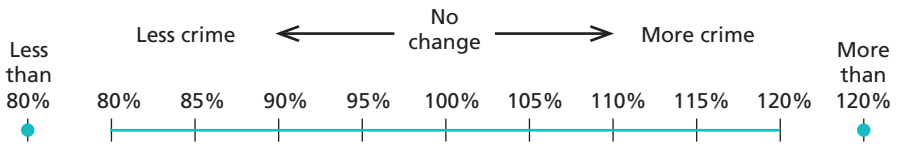
4. Q4PX If a state implemented universal background checks, how would mass shootings change? Mass shootings refer to incidents where four or more people are killed, not including the shooter.



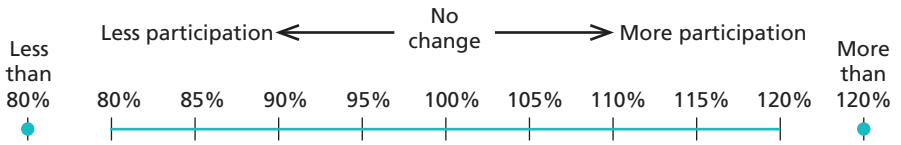
5. Q5PX If a state implemented universal background checks, how would the rate of other violent crime change (like non-firearm homicides, robbery, rape, and aggravated assault)?



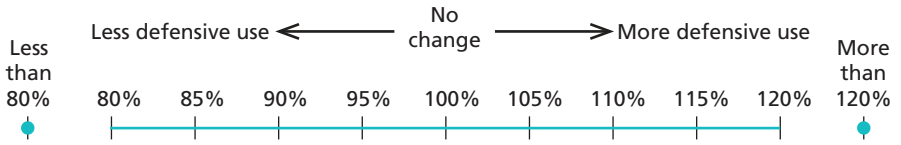
6. Q6PX If a state implemented universal background checks, how would the rate of burglary, theft, and auto theft change?



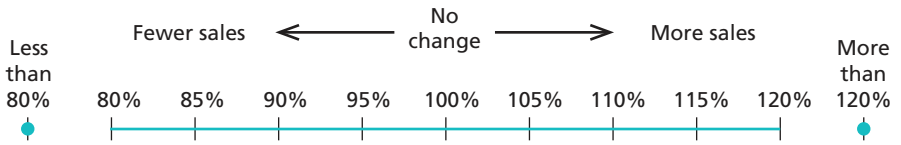
7. Q7PX If a state implemented universal background checks, how would participation in hunting and sport shooting change?



8. Q8PX If a state implemented universal background checks, how would legal acts of defensive gun use change? This refers to using a firearm to protect oneself or others from imminent death or serious injury.



9. Q9PX If a state implemented universal background checks, how would sales of new firearms change?



10. Q10PX How much do universal background checks threaten or protect the right to bear arms?

1 Major threat	2 Moderate threat	3 Minimal threat	4 No impact	5 Minimal protection	6 Moderate protection	7 Major protection

11. Q11PX How much do universal background checks threaten or protect individuals' privacy?

1 Major threat	2 Moderate threat	3 Minimal threat	4 No impact	5 Minimal protection	6 Moderate protection	7 Major protection

12. Q12PX How much do universal background checks change the satisfaction of gun ownership? This includes satisfaction from collecting firearms, feeling safe, or recreational use.

1 Major decrease	2 Moderate decrease	3 Minimal decrease	4 No impact	5 Minimal increase	6 Moderate increase	7 Major increase

13. Q13PX What is your overall opinion of universal background checks? Would you say universal background checks are…

1	2	3	4	5
Very bad policy	Bad	Neither bad nor good	Good	Very good policy

[Questions Q1PX–Q13PX are asked for each of the selected policies, substituting the underlined phrase associated with each policy for "universal background checks."] [When policies 13, 14, and/or 15 are selected to present to the survey respondents, they must appear at the end of the list of selected polices.]

Policy 1. *Universal background checks.*
People who are prohibited by law from having firearms sometimes obtain them through private sales that do not require background checks. *Universal background checks* require background checks prior to all transfers of firearms, including private sales over the internet, at gun shows, and between friends (temporary loans and gifts between family members are exempted). Background checks for private sales are conducted by a government agency or by a licensed gun dealer.

Policy 2. *A ban on sale of "assault weapons" and high capacity magazines.*
This law bans certain semi-automatic firearms with detachable magazines and other features such as pistol grips, folding stocks, or the ability to mount a bayonet. The law also bans magazines that hold more than 15 rounds of ammunition. Owners of these weapons at the time the law is passed may keep them if each weapon is registered with a state authority.

Policy 3. *A stand your ground law.*
This law permits a person to use deadly force without the duty to retreat when confronting a threat that could reasonably result in death or serious injury. Without this law, people outside their homes must try to withdraw from a serious threat, if possible, before using deadly force.

Policy 4. *Expanded mental health prohibitions.*
When a judge has committed someone to an inpatient mental institution or has found them to be unable to manage their own affairs, federal law prohibits that person from having firearms. This law expands the mental health histories leading to prohibition to include people ordered to receive outpatient mental health treatment, and those involuntarily confined because a mental health professional determined they present a danger to themselves or others.

Policy 5. *Required reporting of lost or stolen firearms.*
Firearm owners must report lost or stolen firearms to law enforcement authorities within three days of discovering the loss. Penalties for failure to report include prohibition on firearm ownership for five years and civil liability if the firearm is used in a crime.

Policy 6. *Requiring a license to purchase a firearm or ammunition*.
This law requires a firearms license to purchase or possess a firearm or ammunition. These licenses require successful completion of a safety training course or safety test and a background check, and cost $100. They must be renewed every ten years.

Policy 7. *Required reporting and recording of firearms sales.*
This law requires reporting all firearms sales to a government agency, including information on the firearms and who bought them. This applies to sales by both firearms dealers and private sellers. Law enforcement is permitted to retain the data indefinitely for two purposes: to trace firearms found at crime scenes and to retrieve firearms from individuals who become prohibited possessors.

Policy 8. *A child access-prevention law.*
This law imposes criminal penalties on firearm owners when a child accesses a usable weapon that was stored in a location where the owner should have known a child could access it.

Policy 9. *A media campaign to prevent child access*.
This policy educates the public about the benefits of safe storage through a media campaign. The campaign provides educational materials through news media and the internet and to gun stores for display and distribution.

Policy 10. *Surrender of firearms by prohibited possessors*.
When a judge's rulings place an individual in a class that is prohibited by law from possessing or purchasing a firearm, the judge must also determine whether that individual has firearms, and must order their surrender. This includes people convicted of a felony, those convicted of misdemeanor domestic violence, and those subject to a domestic violence protective order.

Policy 11. *Firearm and ammunition taxes*.
This policy imposes a special $25 tax on the sale of firearms and a 25% tax on the sale of ammunition.

Policy 12. *Minimum age requirements.*
Currently, federal law generally prohibits those younger than 18 from having a handgun, and licensed dealers are prohibited from selling them to anyone younger than 21. Those younger than 18 may have a long gun, but licensed dealers may not sell them to anyone younger than 18. The minimum age requirements policy raises the minimum age for purchase or possession of handguns and long guns to 21.

Policy 13. *Permitless carry.*
This policy allows anyone who is at least 21 years old and not prohibited by law from having a firearm to carry a concealed weapon in public without a permit. For the questions below, assume that before adopting *permitless carry*, the state required concealed carry permits that were issued to those with good moral character and sufficient reason for a concealed firearm.

Policy 14. *Requiring a ten-day waiting period to purchase a firearm.*
This law imposes a waiting period of ten days between the purchase of a firearm and when the buyer can take possession of it. For this question, assume that the state already has a universal background check requirement.

Policy 15. *The elimination of gun-free zones.*
Federal and some state laws prohibit carrying a firearm near schools and certain other public places. This policy allows firearms in these previously prohibited locations. For this question, assume federal and state laws change in a state that previously prohibited private citizens from carrying firearms into schools, universities, government buildings and parks.

14. QSE1 If a policy successfully reduced a state's firearm suicides, how many of those prevented from killing themselves with a firearm would still kill themselves using another method?

0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%

15. QSE2 If a policy successfully reduced a state's firearm homicides, how many of the prevented firearm homicides would still end as a homicide because a different lethal attack would be substituted?

0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%

16. Listed below are organizations selected because they have taken public positions on firearms policies and represent diverse perspectives on those policies. Please indicate how similar your own views on firearm policies are to those of each organization. If you are unfamiliar with an organization's positions, mark "Not Sure."

    [Present list in randomized order for each participant. Use the following response scale:]

1 Very different	2 Mostly different	3 Slightly different	4 Slightly similar	5 Mostly similar	6 Very similar	7 Not sure

    a. QOS1 The National Rifle Association
    b. QOS2 The Second Amendment Foundation
    c. QOS3 Gun Owners of America
    d. QOS4 The National Shooting Sports Foundation
    e. QOS5 International Association of Chiefs of Police
    f. QOS6 The Brady Campaign to Prevent Gun Violence
    g. QOS7 Mayors Against Illegal Guns/Everytown for Gun Safety[1]
    h. QOS8 Violence Policy Center
    i. QOS9 Coalition to Stop Gun Violence
    j. QOS10 The Law Center to Prevent Gun Violence

17. Which of the following best describes your relationship to gun policy (choose all that apply):

    [Present list in randomized order for each participant.]

    a. QP1_1 Professional researcher/scientist
    b. QP1_2 Policy analyst
    c. QP1_3 Policy advocate
    d. QP1_4 Interested layperson
    e. QP1_5 Government official
    f. QP1_6 Congressional staff member

[1] When the survey was fielded, Mayors Against Illegal Guns/Everytown for Gun Safety was inadvertently listed as Mayors Against Gun Violence/Everytown for Gun Safety. High rates of claimed similarity of views among experts with restrictive policy preferences and low rates among those with permissive approaches suggest that this error did not confuse most respondents.

18. Thinking about firearm homicides in the United States, how much do you think each of the following factors contributes to this problem?

    [Present list in randomized order for each participant. Use the following response scale for each. All items can be placed in a grid.]

1 A great deal	2 Some	3 Not much	4 Not at all

    a. QCV1 Easy access to guns
    b. QCV2 Violence in movies, music and video games
    c. QCV3 Mental health problems
    d. QCV4 Poor firearms training or practices
    e. QCV5 Drug trafficking and criminal gang activity
    f. QCV6 Inadequate enforcement of existing gun laws
    g. QCV7 Racial or income inequalities
    h. QCV8 Inadequate parenting or poor role models
    i. QCV9 Laws making it difficult to use firearms for protection
    j. QCV10 Inadequate penalties for gun crimes

19. Q19 If you have comments or clarifications you would like us to consider, please provide them here.

APPENDIX B

Details About the Statistical Modeling of Expert Favorability Ratings

In this appendix, we provide details and sensitivity analysis testing of the statistical model used to explain experts' favorability toward the policies examined. As described in Chapter Five, this model predicts experts' favorability judgments by using their estimates of the effects of each policy on each of the qualitative and quantitative outcomes.

Regression Model and Selection of Prior Probability Distributions

We implemented the model in Stan, a flexible probabilistic programming language for Bayesian inference (Stan Development Team, 2016). The Bayesian framework allows us to introduce explicit prior probability distributions (or *priors*) into the regression model. These priors can create a more parsimonious model and reduce the risk of overfitting given the moderately small number of observations (N = 846), the inclusions of between 12 and 24 predictors (depending on the model), and a moderately high degree of correlation among predictors. The priors also reflect strong expectations about the direction in which expert favorability ratings are related to experts' beliefs about the policy's effects; all else equal, experts would not prefer policies that would increase suicides, homicides, accidents, mass shootings, violent crime, and property crime, nor would they prefer policies that would harm gun rights, individuals' privacy, or the satisfaction of gun ownership.

The following model represents the range of parameters estimated and the priors that were assigned to those parameters:

$$Y_i \sim OrderedLogistic(\boldsymbol{\beta} X_i + \boldsymbol{\theta} Z_i, \mathbf{c})$$

$$\boldsymbol{\beta}_j \sim \begin{cases} \textbf{Normal}(0, \omega_j \gamma_j) \text{ if outcome } j \text{ has nondirectional priors} \\ \textbf{Half-Normal}(0, \omega_j \gamma_j) \text{ if outcome } j \text{ has directional priors} \end{cases}$$

$$\boldsymbol{\theta}_j \sim \begin{cases} \textbf{Normal}(0, \omega_j \gamma_j) \text{ if outcome } j \text{ has nondirectional priors} \\ \textbf{Normal}(0, \frac{abs(\boldsymbol{\beta}_j)}{0.75}) \text{ if outcome } j \text{ has directional priors} \end{cases}$$

$$\gamma \sim Half\text{-}Normal(0, 0.5),$$

where $\boldsymbol{Y}_i$ is an individual's overall favorability rating of the ith gun policy; c is a vector of four cut-points between rating scale scores of 1 and 5; $\boldsymbol{\beta}$ is a vector of coefficients to be estimated, where each β_j is the coefficient to be estimated for the jth of 12 policy effects on outcomes (e.g., the coefficient for the average effect of changes to "firearm homicides" on favorability judgments estimated across policies); $\boldsymbol{X}_i$ is a matrix of each respondent's estimates of the effects of policies, i, on each of the 12 outcomes; and $\boldsymbol{\theta}$ is a vector of coefficients for each of the 12 interaction terms, $\boldsymbol{Z}_i$, between class membership (experts who prefer permissive or restrictive gun policies) and the policy effect estimate made for policy i. These interaction variables, $\boldsymbol{Z}_i$, were centered to be orthogonal to the main effects for each of the 12 policy effects, so that the elements of $\boldsymbol{\beta}$ can be interpreted as the average impact of a given policy effect on experts' overall opinion of the policy, while the elements of $\boldsymbol{\theta}$ represent the extent to which the two classes of experts differ in how policy effects are associated with their overall opinions about each policy.[1] The hyperparameter γ is used to regularize the variance of $\boldsymbol{\beta}$ and $\boldsymbol{\theta}$ terms, after scaling it by ω_j, a scaler, which takes one value for quantitative outcomes and a second value for qualitative outcomes. This scaling factor is used to adjust the hyperprior to account for the fact that outcomes are measured on two different scales with different variances.

As discussed earlier, we have strong priors about the direction of the effects that some of these policy effect dimensions can have on overall policy ratings. For instance, we assume that, all else held equal, a policy that increases suicides, homicides, mass shootings, or crime will be less preferred than one that decreases these outcomes. Similarly, other effects held constant, we assume that a policy that protects or enhances the right to bear arms, individuals' privacy, or satisfaction of gun ownership will be preferred over policies that weaken these outcomes. To improve the model's efficiency, we impose these assumptions in the form of range constraints on the individual elements of $\boldsymbol{\beta}$. Specifically, the coefficients for suicide, homicide, accidents, mass shootings, violent crime, and property crime were constrained to be negative or zero. For effects on these outcomes, we specify a *half-normal prior*—that is, a truncated normal distribution that excludes positive values and has a modal value of zero. Similarly, we constrain coefficients for the right to bear arms, privacy, and satisfaction of gun ownership to be zero or positive. Finally, we impose no range constraints on coefficients for hunting and sport shooting, legal acts of defensive gun use, or gun sales, because we were not confident that all sides would agree that, other outcomes held constant, increases in these outcomes are necessarily a good or a bad result of a policy. Priors for these coef-

[1] The centered interaction variables were created by multiplying each predictor variable by an indicator of group membership: −0.83 for the permissive class and +0.17 for the restrictive class. Therefore, the coefficient on the interaction term for a given predictor variable represents the difference across the two groups in the main effect of that predictor. Because these interaction effects have a mean of zero in the sample, the main effects for a given predictor have the same interpretation regardless of whether the corresponding interaction was included in the model.

ficients were specified as normal distributions with a mean of zero. We later check the influence of the directional constraints by rerunning our final model without them. All of our substantive findings from this final model are unchanged if we remove the directionality constraints.

We use partial pooling of information about the distribution of coefficients across elements of $\boldsymbol{\beta}$, which provides some of the benefits of Bayesian regularization (e.g., improved estimates in the presence of collinear predictors or limited data) without having to specify tight priors based entirely on researcher assumptions. Specifically, the variance of the priors for the $\boldsymbol{\beta}$ and $\boldsymbol{\theta}$ terms were scaled as a function of a hyperparameter γ. This allows us to incorporate a penalty for complexity into the model that serves to regularize the estimates. Essentially, the model prefers to have prior distributions on the $\boldsymbol{\beta}$ terms that are relatively tight around zero, treating extreme coefficients as unlikely. However, if the data suggest that some elements of $\boldsymbol{\beta}$ are large, then the model will relax the priors on all elements $\boldsymbol{\beta}$, treating more-extreme $\boldsymbol{\beta}$*'s* as more likely.

In our case, we have not standardized the predictor variables, so the effect of the hyperparameter is not necessarily the same across predictors with different variance. To account for this, we rescale hyperparameter γ so that it applies equivalently to the quantitative policy effect dimensions (that have higher variability) and the qualitative policy effect dimensions (that are constrained to fall between 1 and 7). Specifically, we create a scaling factor ω_j that is 0.304 for the outcomes, j, assessed on a quantitative, percentage scale and is 1.274 for the outcomes assessed on a 1-to-7 Likert scale. These values derive from the average standard deviations for the two types of response scales in this sample and ensure that the constraint imposed by the hyperparameter is similar for both types of response scales. Initially, we set the hyperparameter γ to be half-normal with a mean of zero and standard deviation of 0.5. This corresponds to assuming that a typical effect size across the quantitative predictors would be 0.19, corresponding to an increase in the odds of responding with a higher response category by a factor of 1.2 for every percentage point increase in that policy effect. We tested the sensitivity of the results to this assumption and found that all of our findings are substantively equivalent if we used higher or lower values.

Finally, we constrain the coefficients for the interaction terms, $\boldsymbol{\theta}$, to reduce the likelihood that any interaction effects could result in one or another group having a main and interaction effect that violates the range constraint for any given outcome. Specifically, we set the standard deviation for interaction coefficients $\boldsymbol{\theta}_j$ for effects j subject to range constraints to be the absolute value of the estimated main effect, $\boldsymbol{\beta}_j$, for the same policy effect, divided by 0.75. This does not guarantee that the resulting combined effect will obey the range constraints, but it encourages this outcome and was sufficient so that the means of the posteriors for both groups were always on the hypothesized side of zero. For $\boldsymbol{\theta}_j$ corresponding to effects j for which there is no range constraint, we assumed that these effects were drawn from the same distribution as the corresponding main effect in $\boldsymbol{\beta}$.

The model is estimated on experts' policy favorability ratings. However, the model does not include fixed or random effects for the 95 individual experts. The goal of the model is to see how well we can predict policy favorability ratings from the experts' beliefs about the effects of the policy on key outcomes. Including effects for individual experts would allow other unmeasured factors, such as an expert's value system, to influence the prediction. It would also dramatically complicate the model, making model overfitting much more likely. It is important to note, however, that the model posterior distributions that we get from our model are not necessarily the same ones one would get from a model that had included random effects for the 95 experts.

Missing Data

The total sample size of 857 reflects all expert favorability ratings for which at least six of the 12 policy outcome ratings were not missing. Although the policy effect ratings that were randomly selected to be missing (to reduce the respondent burden of the survey) were imputed for use in some of our analyses, they were not included when estimating these models. Because all of the predictors were missing for those favorability judgments, there is no information lost by excluding those judgements from the model, and the resulting models are not as heavily dependent on imputation assumptions. We do use the imputed values for the minor amount of unplanned missing ratings, so long as at least six of the predictors were measured for that respondent on that policy.

Investigation of Model Fit and Linearity Assumptions

We hypothesized that the relationship between a given quantitative estimate of a policy's effect on an outcome and the expert's rating on that effect would be nonlinear. Specifically, we hypothesized that policy effect differences near 1 would have a larger effect on policy favorability ratings than the same differences elsewhere on the scale. For example, we assumed that a policy rated as having a 101-percent effect on homicide would be judged much more negatively than a policy with an effect of 99 percent; that is, experts would strongly prefer a policy that prevents approximately 100 deaths a year over a policy that causes 100 deaths. However, we assumed that a similar magnitude difference elsewhere on the scale would have a smaller impact on the favorability ratings. For example, two policies rated as having a 75-percent and 73-percent effect on homicide are both likely to be equally favored; that is, a policy that saves 2,500 lives a year will be seen as quite similar to a policy that saves 2,700 lives a year.

To capture this hypothesized nonlinearity, we investigated four simple transformations of our quantitative predictor variables, and we selected the transformation that

resulted in the best overall model fit. Each transformation assumed that the influence of the policy effect rating on overall policy favorability was linear over a constrained range near 100 but that the influence asymptotes outside of that range. We investigated transformations in which the effect was linear between values above and below 100—the value corresponding to no change. The 90/110 transformation provided the best overall model fit as assessed by the Watanabe-Akaike widely applicable information criterion (WAIC). This remained true in the final model (see Table B.1), for which WAIC was minimized using the 90/110 transformation. We used this transformation for the analyses reported here. Thus, the regression coefficients we report are scaled as the influence of a 1-percentage-point change in these predictors between 90 and 110. Values of the predictors above 110 or below 90 are assumed to have no additional effect on overall expert favorability judgments beyond that produced by effects with values of 110 or 90, respectively.

Because of the relatively large number of correlated predictors that we wish to investigate (12 main effects and 12 interactions), we were concerned about model overfitting and unstable parameter estimates. To minimize these problems, we began with relatively simple models and added model complexity when there was evidence that the overall model fit improved, as assessed by the WAIC. Our initial model included only the 12 main effects and no interactions. This starting point reflects our hypothesis that the individual policy effects included in the survey were relevant to overall policy favorability and that we did not have specific hypotheses about interactions by expert class. Inclusion of specific interactions was exploratory. Within the main-effect model, we noted that two predictors had effects that were extremely small (the effect of the policy on other violent crime and on the satisfaction of gun ownership) while controlling for the other effects. When these two predictors were dropped from the model, the overall model WAIC showed an improvement, so these terms were dropped in all subsequent models. This main-effect model is presented in Table B.2.

Table B.1
Final Model Fit Statistics Imposing Different Range Constraints over the Linear Portion of the Effect Estimates

Range of Linear Effect	WAIC	Standard Error
99/101	1,627.9	46.3
90/110	1,616.9	51.2
80/120	1,680.2	53.0
50/150	1,730.0	60.7
No adjustment	1,746.7	66.7

NOTE: The *range of linear effect* refers to the lower and upper bounds of the policy effect estimates assumed to have a linear effect on favorability judgments in the final model.

Table B.2
Model with No Interaction Terms

Outcome		OR	OR Credibility Interval		Standardized OR
			2.50%	97.50%	
1	Firearm suicides	0.877	0.837	0.918	0.643
2	Firearm homicides	0.808	0.771	0.845	0.412
3	Accidental firearms deaths	0.971	0.933	0.999	0.901
4	Mass shootings	0.911	0.87	0.954	0.714
6	Property crime	0.958	0.909	0.998	0.888
7	Hunting and sport shooting	1.173	1.11	1.241	1.408
8	Legal acts of defensive gun use	0.94	0.897	0.986	0.836
9	Sales of new firearms	0.968	0.935	1.001	0.88
10	Right to bear arms	1.225	1.026	1.498	1.183
11	Individuals' privacy	1.66	1.365	2.019	1.427

Fit Statistics	Estimate	Standard Error
elpd_waic	–821.3	25.7
p_waic	15.0	0.9
waic	1,642.7	51.4

NOTES: *Standardized OR* is the odds ratio for a one-standard-deviation change in the predictor; *elpd_waic* is the expected log pointwise predictive density and standard error; *p_waic* is the estimated effective number of parameters; *waic* is the Watanabe-Akaike WAIC.

We added interactions involving expert class (permissive or restrictive) to the base model one at a time and noted the model WAIC. We then ran a model that included the ten main effects and all of the interactions that improved the WAIC when added individually to the base model. In a final step, we removed interaction terms from this model when doing so improved the WAIC. This yielded a final model that included only two interactions. These interactions incorporate possible differences in the role of suicide and individuals' privacy on overall policy favorability for the two expert classes. This model serves as the final model presented and interpreted in the body of the report.

While we selected the final model specifically because it had the best overall model fit, the model fit improved from a WAIC of 1,642.7 with no interactions to 1,616.9 with the two interactions. This is a relatively modest improvement considering

that we explored ten different interaction effects for inclusion. That is, the 25.8-point improvement in the WAIC is a small proportion of the standard error for those values (standard error = 51). Thus, while the model with interactions is marginally more parsimonious, one should not infer that the inclusion of interactions in the final model offers a significant improvement in model prediction. Indeed, although the polychoric R^2 for the association of predicted favorability ratings to true ratings was 0.81 for the permissive class and 0.70 for the restrictive class in the full model with interactions, the model without interactions in Table B.2 had an R^2 of 0.82 for the permissive class and 0.70 for the restrictive class. This suggests little, if any, erosion of model performance when interaction terms are removed. Similarly, although the full model had median rating predictions that exactly matched respondent policy ratings for 55 percent of experts in the permissive class and 62 percent of the restrictive class, exact matches for predictions from the model without interactions declined to just 50 percent and 60 percent for the permissive and restrictive groups, respectively.

Investigating the Influence of Priors and Range Constraints on Model Results

As discussed earlier, we imposed a priori restrictions on the range of several model coefficients to prevent estimates that we assumed could not be valid. For instance, we constrained the coefficient describing the effect of a policy on murder to be negative because we did not believe that average gun policy experts in either expert group would favor policies that increase murder rates, other effects held constant. Similarly, we used a Bayesian modeling framework so that we could impose priors on many of the estimates. To investigate how influential these assumptions were, we refit the final model using R's polr() ordinal logistic regression package. This model incorporated no range constraints or priors, and it was not estimated as a hierarchical model, meaning there was no pooling of information about the distribution of coefficient estimates.

Table B.3 shows the results of the unconstrained model alongside corresponding estimates from the Bayesian model that used the constraints. In this table, we report estimates and their standard errors (ordinary constraints) or standard deviations (Bayesian constraints) rather than the odds ratios derived from the estimates that are presented in the body of this report. As seen in Table B.3, all estimates are similar across models. No estimate changes sign, and most differ by just a few hundredths. As expected because of its regularizing priors, the Bayesian model's estimates tend to be slightly closer to zero than those from the non-Bayesian model. Thus, although the assumptions we imposed a priori on the model were, we believe, reasonable and may have assisted us in model development by reducing the effective number of parameters to estimate, they do not prove to have meaningful effects on the final model results.

Table B.3
Comparison of Model Estimates With (Bayesian) and Without (Ordinary) Range Constraints and Distributional Priors

		Ordinary		Bayesian	
Outcome		Estimate	Standard Error	Estimate	Standard Deviation
1	Firearm suicides	−0.139	0.024	−0.140	0.165
2	Firearm homicides	−0.212	0.023	−0.215	0.136
3	Accidental firearms deaths	−0.034	0.022	−0.032	0.120
4	Mass shootings	−0.092	0.023	−0.089	0.135
6	Property crime	−0.056	0.028	−0.048	0.023
7	Hunting and sport shooting	0.159	0.029	0.144	0.024
8	Legal acts of defensive gun use	−0.081	0.024	−0.069	0.019
9	Sales of new firearms	−0.045	0.017	−0.036	0.023
10	Right to bear arms	0.150	0.100	0.131	0.026
11	Individuals' privacy	0.325	0.111	0.346	0.030
Class * firearm suicides		0.256	0.065	0.228	0.025
Class * individuals' privacy		−0.535	0.173	−0.493	0.018

APPENDIX C

Descriptive Statistics by Expert Class, Outcome, and Policy

This appendix provides detailed descriptive information about the survey responses made by experts in the restrictive and permissive classes. In Table C.1, policies and outcomes are indicated by number, as follows:

Policies

1. Universal background checks
2. A ban on sale of "assault weapons" and high capacity magazines
3. A stand your ground law
4. Expanded mental health prohibitions
5. Required reporting of lost or stolen firearms
6. Requiring a license to purchase a firearm or ammunition
7. Required reporting and recording of firearms sales
8. A child access prevention law
9. A media campaign to prevent child access
10. Surrender of firearms by prohibited possessors
11. Firearm and ammunition taxes
12. Minimum age requirements
13. Permitless carry
14. Requiring a ten-day waiting period to purchase a firearm
15. The elimination of gun-free zones.

Outcomes

1. Firearm suicides
2. Firearm homicides
3. Accidental firearms deaths
4. Mass shootings
5. Other violent crime
6. Property crime
7. Hunting and sport shooting participation

8. Legal acts of defensive gun use
9. Sales of new firearms
10. Right to bear arms
11. Individuals' privacy
12. Satisfaction of gun ownership.

Table C.1
Descriptive Statistics by Expert Class, Outcome, and Policy

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
1	1	14	98.19	100.00	100.00	99.25	70	85.00	90.00	95.00	89.21	–10.00	0.009	No
1	2	14	100.00	100.00	100.00	99.59	71	98.13	100.00	100.00	98.16	0.00	1.000	No
1	3	15	100.00	100.00	100.00	100.28	70	99.76	100.00	100.00	100.26	0.00	1.000	No
1	4	14	88.98	92.25	98.00	91.68	69	80.00	90.00	95.00	87.57	–2.25	0.060	No
1	5	15	100.00	100.00	100.00	102.09	71	92.03	99.00	100.00	94.95	–1.00	1.000	No
1	6	15	99.00	100.00	100.49	101.47	69	81.07	90.00	95.00	87.20	–10.00	0.021	No
1	7	15	98.40	100.00	105.70	101.05	71	90.00	95.00	100.00	93.02	–5.00	0.205	No
1	8	15	98.26	100.00	100.00	99.13	70	90.00	95.00	98.00	91.94	–5.00	0.058	No
1	9	15	95.00	100.00	100.00	99.40	71	87.57	93.30	98.00	91.97	–6.70	0.163	No
1	10	14	95.00	100.00	100.00	101.64	70	80.62	93.11	96.58	89.68	–6.89	0.112	No
1	11	14	100.00	100.00	100.00	100.51	69	95.00	98.25	100.00	97.28	–1.75	0.354	No
1	12	14	98.58	99.85	100.00	98.38	70	89.14	95.00	98.00	92.10	–4.85	0.026	No
1	13	14	100.00	100.00	100.00	102.84	68	100.00	102.00	108.07	105.61	2.00	0.325	No
1	14	14	96.00	100.00	100.00	97.64	68	80.62	85.70	95.00	88.50	–14.30	0.020	No
1	15	14	97.72	100.00	100.00	98.28	68	100.00	100.00	105.27	102.87	0.00	1.000	No
2	1	14	95.00	100.00	105.00	104.70	70	80.00	85.94	91.48	86.08	–14.06	0.000	No
2	2	14	100.00	101.39	106.59	106.44	71	83.69	95.00	97.00	91.07	–6.39	0.069	Yes
2	3	15	86.71	90.00	98.87	92.66	70	100.00	105.00	110.83	106.10	15.00	0.020	Yes

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
2	4	14	87.77	95.00	97.87	93.03	69	90.00	95.00	98.00	92.77	0.00	0.630	No
2	5	15	96.00	100.00	100.00	100.82	71	86.79	95.00	98.00	91.02	–5.00	0.066	No
2	6	15	98.00	100.00	100.00	101.78	69	80.00	88.28	95.00	87.02	–11.72	0.028	No
2	7	15	100.00	106.04	116.43	109.22	71	80.00	90.00	95.00	88.40	–16.04	0.031	Yes
2	8	15	99.00	100.00	100.00	101.45	70	94.27	98.50	100.00	96.13	–1.50	0.456	No
2	9	15	97.01	100.00	100.00	101.93	71	93.23	98.00	100.00	95.88	–2.00	0.579	No
2	10	14	95.00	99.00	100.00	101.99	70	79.96	85.00	93.00	83.14	–14.00	0.002	No
2	11	14	100.00	100.00	100.00	100.06	69	95.00	97.72	100.00	97.02	–2.28	0.311	No
2	12	14	99.00	100.00	100.00	99.02	70	86.31	95.00	99.00	92.92	–5.00	0.105	No
2	13	14	90.00	96.85	98.27	94.93	68	105.00	110.00	116.24	111.85	13.15	0.004	Yes
2	14	14	98.94	100.00	101.00	100.94	68	88.09	92.23	95.46	92.22	–7.77	0.122	No
2	15	14	85.00	90.00	100.00	89.24	68	100.63	105.00	110.00	106.66	15.00	0.000	Yes
3	1	14	100.00	100.00	105.00	101.99	70	90.00	95.00	99.00	93.99	–5.00	0.068	No
3	2	14	99.00	100.00	101.29	99.48	71	95.00	98.95	100.00	96.74	–1.05	0.538	No
3	3	15	95.75	100.00	100.00	99.04	70	100.00	101.00	107.48	102.97	1.00	1.000	No
3	4	14	97.81	100.58	105.16	102.24	69	95.00	98.43	100.00	96.67	–2.15	0.509	Yes
3	5	15	100.00	100.00	100.00	103.49	71	90.30	99.00	100.00	94.91	–1.00	1.000	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
3	6	15	96.62	100.00	100.00	99.45	69	85.34	91.53	98.00	91.38	–8.47	0.189	No
3	7	15	97.99	100.00	111.83	103.24	71	87.29	95.00	100.00	92.70	–5.00	0.092	No
3	8	15	90.31	95.00	100.00	95.40	70	80.00	90.00	97.00	87.94	–5.00	0.041	No
3	9	15	90.00	95.00	99.00	94.42	71	80.00	90.00	97.00	85.64	–5.00	0.336	No
3	10	14	100.00	100.00	102.88	104.25	70	90.00	95.21	100.00	94.18	–4.79	0.020	No
3	11	14	100.00	100.00	100.00	99.69	69	95.20	98.00	100.00	97.32	–2.00	0.320	No
3	12	14	100.00	100.00	111.75	103.98	70	85.00	95.00	99.00	91.52	–5.00	0.069	No
3	13	14	99.00	100.00	100.00	100.33	68	103.06	107.37	112.69	108.81	7.37	0.168	No
3	14	14	100.00	100.00	100.00	100.39	68	95.00	100.00	100.00	98.07	0.00	1.000	No
3	15	14	100.00	100.00	100.00	100.36	68	100.00	100.00	109.01	102.54	0.00	1.000	No
4	1	14	86.96	99.00	100.00	97.32	70	83.19	90.00	95.00	90.12	–9.00	0.008	No
4	2	14	100.00	100.00	124.40	111.67	71	77.74	85.00	90.00	82.21	–15.00	0.007	No
4	3	15	94.47	100.00	100.00	96.65	70	100.00	100.00	103.46	102.07	0.00	1.000	No
4	4	14	84.61	90.23	100.00	91.45	69	84.22	90.00	95.00	88.22	–0.23	0.075	No
4	5	15	100.00	100.00	100.17	102.83	71	92.74	98.65	100.00	93.98	–1.35	0.391	No
4	6	15	100.00	100.00	100.00	100.55	69	80.00	95.00	98.00	88.94	–5.00	0.121	No
4	7	15	97.87	100.00	106.84	103.07	71	90.00	95.00	98.46	92.39	–5.00	0.078	No
4	8	15	94.61	100.00	100.00	97.21	70	96.59	100.00	100.00	97.59	0.00	1.000	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
4	9	15	98.93	100.00	100.00	100.57	71	93.58	99.00	100.00	96.53	–1.00	1.000	No
4	10	14	96.00	99.50	100.00	102.27	70	89.24	95.00	98.00	91.92	–4.50	0.065	No
4	11	14	100.00	100.00	100.00	100.10	69	98.00	100.00	100.00	98.79	0.00	1.000	No
4	12	14	91.44	100.00	100.00	96.60	70	90.00	96.00	100.00	94.18	–4.00	0.197	No
4	13	14	88.69	94.00	98.04	88.42	68	103.00	105.50	110.00	108.87	11.50	0.000	Yes
4	14	14	100.00	100.00	105.96	103.25	68	85.00	95.00	99.00	92.67	–5.00	0.043	No
4	15	14	78.19	87.29	90.00	79.01	68	100.00	105.00	110.64	106.77	17.71	0.002	Yes
5	1	14	97.73	100.00	100.00	100.46	70	85.00	96.00	100.00	93.60	–4.00	0.161	No
5	2	14	100.00	106.00	110.83	108.19	71	93.16	100.00	100.00	96.45	–6.00	1.000	No
5	3	15	80.00	90.00	100.00	90.32	70	100.00	100.00	102.07	100.53	10.00	0.004	No
5	4	14	97.91	100.00	103.46	100.81	69	90.00	98.34	100.00	95.41	–1.66	1.000	No
5	5	15	100.00	100.00	100.00	101.15	71	88.24	95.00	100.00	91.75	–5.00	0.051	No
5	6	15	100.00	100.00	109.98	104.63	69	90.00	98.37	100.00	94.71	–1.63	1.000	No
5	7	15	98.00	100.00	110.40	105.44	71	90.00	96.38	100.00	94.56	–3.62	0.374	No
5	8	15	100.00	100.00	106.02	105.94	70	98.00	100.00	100.00	99.53	0.00	1.000	No
5	9	15	98.12	100.00	101.00	102.08	71	96.53	100.00	100.00	98.07	0.00	1.000	No
5	10	14	98.00	100.00	100.00	99.99	70	89.50	95.00	100.00	93.18	–5.00	0.141	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
5	11	14	100.00	100.00	100.00	100.48	69	99.00	100.00	100.00	99.23	0.00	1.000	No
5	12	14	99.00	100.00	100.00	99.23	70	95.00	99.11	100.00	97.69	–0.89	0.327	No
5	13	14	84.46	90.00	95.00	88.72	68	100.00	105.00	110.00	105.63	15.00	0.000	Yes
5	14	14	99.00	100.00	100.00	99.73	68	92.82	98.89	100.00	96.36	–1.11	1.000	No
5	15	14	95.00	97.66	100.00	95.65	68	100.00	100.50	105.00	102.87	2.84	0.044	Yes
6	1	14	100.00	100.00	104.49	100.89	70	91.86	100.00	100.00	95.19	0.00	1.000	No
6	2	14	100.00	100.00	100.34	100.38	71	95.33	100.00	100.00	97.56	0.00	1.000	No
6	3	15	83.75	86.68	96.00	89.31	70	99.20	100.00	101.15	100.58	13.32	0.006	No
6	4	14	97.44	99.84	101.86	98.72	69	92.28	100.00	100.00	94.76	0.16	1.000	No
6	5	15	95.00	100.00	100.00	98.22	71	90.13	97.00	100.00	94.66	–3.00	0.360	No
6	6	15	100.00	103.19	105.00	103.56	69	95.24	99.38	100.00	97.61	–3.81	1.000	Yes
6	7	15	100.00	100.00	105.00	100.57	71	90.00	99.00	100.00	95.75	–1.00	1.000	No
6	8	15	100.00	100.00	103.96	100.62	70	97.42	100.00	100.00	98.33	0.00	1.000	No
6	9	15	100.00	100.00	102.28	101.19	71	99.00	100.00	100.00	99.08	0.00	1.000	No
6	10	14	100.00	100.00	101.55	100.75	70	95.00	99.33	100.00	96.25	–0.67	1.000	No
6	11	14	100.00	100.00	100.00	100.57	69	98.00	100.00	100.00	99.08	0.00	1.000	No
6	12	14	100.00	100.00	100.85	101.47	70	95.60	100.00	100.00	97.41	0.00	1.000	No
6	13	14	90.00	95.50	100.00	94.52	68	100.00	100.61	107.27	104.20	5.11	0.072	Yes

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
6	14	14	99.73	100.00	100.00	99.78	68	94.23	99.96	100.00	96.85	–0.04	1.000	No
6	15	14	90.70	95.50	100.00	94.43	68	100.00	100.00	103.35	101.28	4.50	0.020	No
7	1	14	89.01	91.16	95.00	91.24	70	98.35	100.00	100.00	99.36	8.84	0.000	No
7	2	14	84.16	90.54	100.00	90.80	71	97.56	100.00	100.00	98.86	9.46	0.006	No
7	3	15	100.00	100.00	100.33	100.26	70	100.00	100.00	100.00	99.91	0.00	1.000	No
7	4	14	94.35	97.33	100.00	95.63	69	99.26	100.00	100.00	99.30	2.67	1.000	No
7	5	15	100.00	100.00	100.00	98.89	71	100.00	100.00	100.00	99.67	0.00	1.000	No
7	6	15	85.00	89.28	93.99	90.05	69	98.45	100.00	100.00	99.52	10.72	0.005	No
7	7	15	90.63	95.00	98.24	94.63	71	98.19	100.00	100.00	99.32	5.00	0.021	No
7	8	15	94.34	98.68	100.00	96.20	70	99.70	100.00	100.00	99.33	1.32	1.000	No
7	9	15	98.00	100.00	100.00	99.10	71	99.35	100.00	100.00	99.45	0.00	1.000	No
7	10	14	93.44	99.00	100.00	95.95	70	98.00	100.00	100.00	98.98	1.00	0.050	No
7	11	14	80.00	86.66	90.00	78.90	69	95.00	98.87	100.00	98.11	12.22	0.000	No
7	12	14	84.00	90.00	95.00	88.98	70	91.82	98.06	100.00	95.67	8.06	0.021	No
7	13	14	100.00	100.00	103.12	101.64	68	100.00	100.00	100.00	100.83	0.00	1.000	No
7	14	14	95.00	98.43	100.00	96.39	68	100.00	100.00	100.00	100.17	1.57	0.003	No
7	15	14	100.00	100.00	100.00	100.74	68	100.00	100.00	100.07	100.67	0.00	1.000	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
8	1	14	86.17	90.45	100.00	91.66	70	96.43	100.00	100.00	98.53	9.55	0.001	No
8	2	14	94.33	95.87	98.00	93.87	71	98.79	100.00	100.00	98.66	4.13	0.016	No
8	3	15	96.71	105.00	110.00	104.25	70	100.79	110.00	115.26	109.48	5.00	0.567	No
8	4	14	94.71	97.34	99.00	95.35	69	97.46	100.00	100.00	98.34	2.66	0.104	No
8	5	15	98.01	100.00	100.00	97.53	71	100.00	100.00	100.00	99.36	0.00	1.000	No
8	6	15	89.41	93.93	100.00	94.26	69	95.21	99.27	100.00	97.80	5.34	1.000	No
8	7	15	94.98	99.38	100.00	96.37	71	96.64	100.00	100.00	98.33	0.62	1.000	No
8	8	15	90.00	95.00	99.00	93.78	70	99.00	100.00	100.00	99.41	5.00	0.004	No
8	9	15	95.00	97.00	99.50	96.59	71	99.00	100.00	100.00	99.55	3.00	0.005	No
8	10	14	95.00	100.00	100.00	96.92	70	96.43	100.00	100.00	97.56	0.00	1.000	No
8	11	14	86.87	96.58	100.00	93.45	69	98.00	100.00	100.00	98.83	3.42	0.089	No
8	12	14	93.02	97.50	100.00	95.94	70	97.43	100.00	100.00	98.68	2.50	0.053	No
8	13	14	102.00	106.14	110.00	111.81	68	100.00	101.53	105.39	103.97	–4.61	0.036	No
8	14	14	90.00	97.30	99.00	94.77	68	98.50	100.00	100.00	99.07	2.70	0.034	No
8	15	14	100.00	107.99	113.56	110.05	68	98.66	100.00	103.50	100.65	–7.99	0.016	No
9	1	14	88.72	95.00	103.00	96.07	70	94.93	98.21	101.91	98.17	3.21	0.757	No
9	2	14	80.00	86.93	90.00	83.87	71	90.00	95.64	105.00	97.32	8.71	0.009	No
9	3	15	100.00	101.60	105.00	102.33	70	100.00	105.00	110.00	105.45	3.40	0.625	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
9	4	14	95.00	99.00	105.75	99.65	69	95.00	99.00	100.00	97.76	0.00	0.213	No
9	5	15	99.00	100.00	100.00	98.31	71	100.00	100.00	102.05	100.80	0.00	1.000	No
9	6	15	77.64	84.29	90.00	85.08	69	90.00	96.00	100.65	95.64	11.71	0.188	No
9	7	15	93.96	95.78	100.00	95.39	71	94.18	96.00	100.00	96.39	0.22	0.639	No
9	8	15	98.00	100.00	100.00	98.98	70	100.00	100.00	101.13	100.55	0.00	1.000	No
9	9	15	100.00	100.00	103.61	100.40	71	98.66	100.00	100.52	99.63	0.00	1.000	No
9	10	14	98.00	100.00	105.00	102.54	70	98.33	100.00	100.10	99.66	0.00	1.000	No
9	11	14	74.84	85.09	93.00	82.52	69	90.00	95.00	100.00	94.51	9.91	0.077	No
9	12	14	93.16	96.00	98.00	95.82	70	93.36	97.00	100.00	96.13	1.00	0.847	No
9	13	14	103.00	105.00	114.29	106.93	68	100.96	105.00	110.00	108.08	0.00	1.000	No
9	14	14	90.00	95.13	97.33	94.66	68	95.00	98.00	100.00	97.26	2.87	0.212	No
9	15	14	100.00	100.38	105.00	102.35	68	100.00	104.21	108.68	104.60	3.83	1.000	No
10	1	15	1.00	1.46	2.00	1.71	77	3.84	4.00	4.00	3.96	2.54	0.000	No
10	2	15	1.00	1.00	2.11	1.61	78	3.00	4.00	4.00	3.62	3.00	0.000	No
10	3	16	4.99	5.85	6.58	5.67	77	4.00	4.00	4.95	4.38	–1.85	0.019	No
10	4	15	2.00	2.05	3.00	2.53	76	3.00	3.83	4.00	3.53	1.78	0.001	No
10	5	16	2.50	3.00	4.00	3.08	78	4.00	4.00	4.00	4.10	1.00	0.007	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
10	6	16	1.00	1.00	2.00	1.40	76	3.25	4.00	4.13	3.91	3.00	0.001	No
10	7	15	1.00	1.77	2.56	1.90	78	3.15	4.00	4.00	3.88	2.23	0.000	No
10	8	16	2.00	2.32	3.00	2.46	77	4.00	4.00	4.00	3.97	1.68	0.000	No
10	9	16	3.38	4.00	4.09	3.78	78	4.00	4.00	4.37	4.12	0.00	1.000	No
10	10	15	1.85	3.00	4.00	2.91	77	3.51	4.00	4.71	4.10	1.00	0.085	No
10	11	15	1.00	1.71	2.26	1.72	76	3.00	3.87	4.00	3.61	2.16	0.000	No
10	12	15	1.00	1.50	3.00	1.94	77	3.00	3.87	4.00	3.53	2.36	0.000	No
10	13	15	6.00	7.00	7.00	6.70	75	4.00	4.00	5.00	4.62	–3.00	0.008	No
10	14	15	1.00	2.00	2.79	1.91	75	3.15	4.00	4.00	3.74	2.00	0.000	No
10	15	15	4.56	6.00	7.00	5.80	75	4.00	4.00	5.03	4.47	–2.00	0.034	No
11	1	15	1.00	1.39	2.00	1.54	77	3.00	4.00	4.00	3.63	2.61	0.000	No
11	2	15	1.73	2.02	2.51	2.14	78	3.88	4.00	4.00	3.88	1.98	0.000	No
11	3	16	4.00	4.13	6.00	4.45	77	3.27	4.00	4.50	3.87	–0.13	0.016	No
11	4	15	1.00	2.00	2.86	1.98	76	2.86	3.00	3.52	3.01	1.00	0.000	No
11	5	16	2.00	2.14	3.00	2.32	78	3.00	4.00	4.00	3.63	1.86	0.000	No
11	6	16	1.00	1.94	2.71	1.87	76	3.00	3.60	4.00	3.54	1.66	0.000	No
11	7	15	1.00	2.00	2.11	1.78	78	3.00	3.05	4.00	3.32	1.05	0.113	No
11	8	16	2.00	2.75	3.20	2.78	77	3.65	4.00	4.00	3.77	1.25	0.000	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
11	9	16	2.92	3.16	4.00	3.32	78	4.00	4.00	4.03	4.02	0.84	1.000	No
11	10	15	2.00	2.65	4.00	2.66	77	3.00	4.00	4.00	3.64	1.35	0.000	No
11	11	15	2.00	2.00	2.91	2.30	76	4.00	4.00	4.00	3.92	2.00	0.000	No
11	12	15	2.00	2.52	3.00	2.41	77	3.66	4.00	4.00	3.84	1.49	0.041	No
11	13	15	5.00	6.00	7.00	5.92	75	4.00	4.00	4.00	4.12	–2.00	0.004	No
11	14	15	1.00	1.48	3.00	1.80	75	3.74	4.00	4.00	3.86	2.52	0.000	No
11	15	15	4.00	5.00	6.00	4.99	75	3.61	4.00	4.00	3.90	–1.00	0.004	No
12	1	15	1.00	1.10	2.53	1.72	77	3.66	4.00	4.69	4.02	2.91	0.000	No
12	2	15	1.14	2.00	2.20	1.95	78	2.46	3.00	3.61	3.06	1.00	0.111	No
12	3	16	4.33	5.00	6.46	5.32	77	4.00	4.32	5.00	4.48	–0.68	1.000	No
12	4	15	2.72	3.00	4.00	3.28	76	3.00	4.00	4.00	3.67	1.00	0.089	No
12	5	16	1.86	3.00	3.01	2.69	78	3.06	4.00	4.00	3.81	1.00	0.138	No
12	6	16	1.00	1.89	3.00	1.95	76	3.24	4.00	4.10	3.82	2.11	0.001	No
12	7	15	1.10	2.30	3.00	2.26	78	3.00	4.00	4.00	3.64	1.70	0.002	No
12	8	16	2.74	3.00	3.78	3.10	77	3.00	4.00	4.00	3.79	1.00	0.156	No
12	9	16	3.00	3.40	4.00	3.39	78	4.00	4.00	4.63	4.14	0.60	0.013	No
12	10	15	2.00	4.00	4.00	3.17	77	3.00	4.00	4.00	3.68	0.00	1.000	No

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
12	11	15	1.00	2.00	3.00	2.13	76	3.00	3.52	4.00	3.39	1.52	0.001	No
12	12	15	2.00	2.30	3.00	2.49	77	3.00	4.00	4.00	3.74	1.70	0.006	No
12	13	15	5.00	6.00	7.00	6.11	75	4.00	5.00	5.68	4.79	–1.00	0.185	No
12	14	15	1.00	1.36	2.78	1.78	75	3.00	4.00	4.00	3.78	2.64	0.000	No
12	15	15	5.00	5.61	6.00	5.51	74	4.00	4.00	5.18	4.53	–1.61	0.015	No
Opinion	1	15	1.00	2.00	3.00	2.20	77	5.00	5.00	5.00	4.88	3.00	0.000	Yes
Opinion	2	15	1.00	1.00	1.00	1.40	78	4.00	5.00	5.00	4.55	4.00	0.000	Yes
Opinion	3	15	3.00	5.00	5.00	3.93	77	1.00	1.00	2.00	1.56	–4.00	0.000	Yes
Opinion	4	15	2.00	3.00	4.00	2.87	76	4.00	5.00	5.00	4.37	2.00	0.000	No
Opinion	5	16	2.50	3.00	3.00	2.75	78	4.00	5.00	5.00	4.53	2.00	0.000	No
Opinion	6	16	1.00	1.00	1.50	1.31	75	5.00	5.00	5.00	4.77	4.00	0.000	Yes
Opinion	7	15	1.00	1.00	2.00	1.60	78	4.00	5.00	5.00	4.67	4.00	0.000	Yes
Opinion	8	16	2.00	2.50	3.00	2.44	77	4.00	5.00	5.00	4.44	2.50	0.000	Yes
Opinion	9	15	3.00	3.00	4.00	3.13	78	4.00	5.00	5.00	4.42	2.00	0.000	No
Opinion	10	15	2.00	3.00	4.00	2.93	77	5.00	5.00	5.00	4.86	2.00	0.000	No
Opinion	11	15	1.00	1.00	2.00	1.40	76	4.00	4.00	5.00	4.05	3.00	0.000	Yes
Opinion	12	14	1.00	2.00	3.00	2.21	77	4.00	5.00	5.00	4.42	3.00	0.000	Yes
Opinion	13	15	3.00	4.00	5.00	3.93	75	1.00	1.00	1.00	1.41	–3.00	0.000	Yes

Table C.1—Continued

		Permissive Class					Restrictive Class					Permissive vs. Restrictive		
Outcome	Policy	N	25th%	50th% (median)	75th%	Mean	N	25th%	50th% (median)	75th%	Mean	Difference[a]	p[b]	Opposite Sides[c]
Opinion	14	15	1.00	1.00	2.00	1.60	75	4.00	5.00	5.00	4.64	4.00	0.000	Yes
Opinion	15	15	4.00	4.00	5.00	4.33	74	1.00	1.00	2.00	1.73	–3.00	0.002	Yes
Suicide substitution		15	50.00	90.00	100.00	75.33	74	10.00	20.00	40.00	29.19	–70.00	0.000	–
Homicide substitution		15	60.00	90.00	100.00	80.67	74	10.00	20.00	30.00	23.38	–70.00	0.000	–

NOTE: We present the 25th percentile (first quartile), 50th percentile (median), and 75th percentile (third quartile) for each group on each policy-outcome combination. We imputed responses that were planned to be missing, as described in Chapter Three.

[a] This column displays the median response value in the restrictive class minus the median response value in the permissive class.

[b] The *p* value, computed using permutation tests of significance, corresponds to the difference in medians and was computed prior to imputation of missing responses.

[c] This column indicates whether the medians of the two groups are on opposite sides of the response scale's central point (100 for outcomes 1–9, 4 for outcomes 10–12, 3 for the opinion item).

APPENDIX D

Data Sources, Calculations, and Additional Assumptions for the Gun Policy Comparison Tool

RAND's online gun policy comparison tool (RAND Corporation, 2018a) estimates the effect of enacting or repealing gun laws at the state level, using estimates of these effects provided by the two classes of experts we surveyed in summer 2016. The tool generates estimates for 12 outcomes in each of 50 states and Washington, D.C.

Data used in this tool derive from multiple sources and reflect the following assumptions and calculations:

- *State laws*. Information on state laws, current as of January 1, 2017, was drawn from the RAND State Firearm Law Database (Cherney, Morral, and Schell, 2018), and the gun policy research synthesis report (RAND Corporation, 2018b). We included Colorado as a state with a ban on the sale of assault weapons and high-capacity magazines, although its law prohibits the sale of only high-capacity magazines. For minimum age requirement policies, we included all states with a minimum age of 21 for possession of a handgun. We did not include states with castle doctrines, or castle doctrines expanded to apply to the workplace or the car, as full stand-your-ground laws. We treated the policy for a media campaign to prevent child access as though it were not present in any state. The resulting matrix of state laws as of January 1, 2017, is shown in Table D.1.
- *Population data*. We used 2015 state population estimates provided by the CDC's WONDER data system (CDC, undated). The policy comparison tool assumes that state population remains constant before and after implementation of laws and that only the laws turned on or off have any effect on the outcomes. These values are displayed in Table D.2.
- *Firearm suicides*. State firearm suicide counts were calculated from 2015 firearm suicide data provided through the CDC's Wonder data system (CDC, undated). As a privacy protection, the CDC replaces state firearm suicide counts less than ten with a missing value. We replaced all such missing values with the value 5. These values are displayed in Table D.2.
- *Firearm homicides*. State firearm homicide counts were calculated from 2015 firearm homicide data provided through the CDC's Wonder data system (CDC, undated). As a privacy protection, the CDC replaces state firearm homicide

counts less than ten with a missing value. We replaced all such missing values with the value 5. These values are displayed in Table D.2.

- *Unintentional firearm deaths*. State unintentional firearm death counts were calculated from 2015 firearm unintentional death data provided through the CDC's Wonder data system (CDC, undated). As a privacy protection, the CDC replaces state firearm unintentional death counts less than ten with a missing value. We replaced all such missing values with the value 5. These values are displayed in Table D.2.
- *Mass shootings*. Many definitions have been proposed for what constitutes a mass shooting (for a discussion of these definitions, see RAND Corporation, 2018b). We used a definition that results in a fairly large number of incidents being counted as mass shootings, including some domestic violence and gang violence. Specifically, we used the number of individuals injured or killed with a firearm in all single events in which four or more people (including possibly the shooter) were shot in the same general location and time, as determined by the Gun Violence Archive (undated).

  Crude state casualty rates were calculated as the average yearly mass shooting casualty total for the state from 2013 to 2016 divided by the state's population. Because mass shooting casualties are relatively rare, the crude casualty rates for small states are poorly estimated compared with rates for large states, and several small states have crude rates of 0, which is a poor index of risk of such casualties in the state.

  To improve the base rate estimates for small states, we generated adjusted state rates using Laplace smoothing—which has the effect of pulling all state base rates toward the U.S. average mass shooting casualty rate—but doing so in proportion to the size of the state. Specifically, the adjusted count of mass shooting casualties for a state, *i*, is calculated as

$$Pop_i\left(\frac{CC_i + USR * K}{Pop_i + K}\right),$$

where Pop_i is the state's population, CC_i is its crude casualty count, USR is the average mass shooting casualty rate across the United States, and K is a constant we selected to be equal to the smallest state population (586,107 in West Virginia). Therefore, for West Virginia, the smoothed mass casualty count is the mean of West Virginia's crude count (which is 0) and count that would be expected if West Virginia had a mass casualty rate identical to the nation's (about 0.483 per 100,000 population). For states larger than West Virginia, the adjusted estimates are weighted more toward the states' crude count, and more so the larger the state. This adjustment has only a small effect on the total number of casualties across

states. The crude annual number is 1,550, and the adjusted number is 1,566. Final state values are displayed in Table D.2.

- *Violent crime*. Data on violent crime (other than homicide) counts in 2015 were drawn from the Uniform Crime Reporting system's *Crime in the United States* tables (Federal Bureau of Investigation, 2016). Specifically, state totals were calculated as the difference between state totals for violent crime and state totals for murder and nonnegligent manslaughter. These values are displayed in Table D.2.
- *Property crime*. Data on property crime counts in 2015 were drawn from Table 5 of the *Crime in the United States* tables (Federal Bureau of Investigation, 2016). These values are displayed in Table D.2.
- *Hunting licenses*. We used state hunting license numbers for 2015 from the U.S. Fish and Wildlife Service's historical hunting license data for 2004 through 2015 (U.S. Fish and Wildlife Service, 2015). Hunting licenses serve in this model as a proxy for the outcome we surveyed experts about, which was "participation in hunting and sport shooting." These values are displayed in Table D.2.
- *Firearm sales*. We are not aware of current, publicly available, state-level data on gun sales. However, the Bureau of Alcohol, Tobacco, Firearms and Explosives publishes annual national-level data on the number of firearms manufactured, imported, and exported (see Bureau of Alcohol, Tobacco, Firearms and Explosives, 2017).

  We used this national data to generate model-based estimates of the numbers of firearms sold in each state, under the following assumptions: (1) the total number of new firearms sold in 2015 is approximately equal to the number manufactured or imported, minus the number exported; (2) firearm sales in states are proportional to the number of adults in the states living in households where there is a firearm (we call this *ownership rate*); and (3) firearm ownership rates across states in 2015 are well correlated with—though not necessarily equivalent to—firearm ownership rates in 2004, when these were last measured by the federal government using the Behavioral Risk Factor Surveillance System (as reported in Miller et al., 2013).

  Specifically, we estimated each state's share of national firearm sales as their fraction of all gun owners nationally:

$$\frac{Pop_i * OR_i}{\sum_i \left(Pop_i * OR_i\right)} * sales,$$

where Pop_i is the state population, OR_i is the state firearm ownership rate, and *sales* is the 2015 domestic firearm sales estimate, calculated as the sum of manufactured and imported firearms, minus the number exported.

The resulting sales estimates are displayed in Table D.2.

Table D.1
Presence (1) or Absence (0) of Gun Policy Types, by State, as of January 1, 2017

	Policy													
State	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**10**	**11**	**12**	**13**	**14**	**15**
Ala.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Alaska	0	0	1	0	0	0	0	0	0	0	0	1	0	0
Ariz.	0	0	1	1	0	0	0	0	0	0	0	1	0	0
Ark.	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Calif.	1	1	0	1	1	1	1	1	1	0	0	0	1	0
Colo.	1	1	0	0	0	0	0	0	1	0	0	0	0	1
Conn.	1	1	0	1	1	1	1	1	1	0	1	0	0	0
Del.	1	0	0	0	1	0	0	0	0	0	0	0	0	0
D.C.	1	1	0	1	1	1	1	1	0	0	1	0	1	0
Fla.	0	0	1	0	0	0	0	1	0	0	0	0	0	0
Ga.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Hawaii	1	1	0	1	0	1	1	1	1	0	1	0	1	0
Ida.	0	0	0	0	0	0	0	0	0	0	0	1	0	1
Ill.	1	0	0	1	1	1	0	1	1	0	1	0	0	0
Ind.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Ia.	1	0	1	0	0	1	0	1	1	0	1	0	0	0
Kan.	0	0	1	0	0	0	0	0	0	0	0	1	0	1
Ky.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
La.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Me.	0	0	0	0	0	0	0	0	0	0	0	1	0	0
Md.	1	1	0	1	1	1	1	1	1	0	1	0	0	0
Mass.	1	1	0	0	1	1	1	1	1	0	1	0	0	0
Mich.	1	0	1	0	1	1	1	0	0	0	0	0	0	0
Minn.	0	0	0	0	0	1	0	1	1	0	0	0	0	0
Miss.	0	0	1	0	0	0	0	0	0	0	0	1	0	1
Mo.	0	0	1	0	0	0	0	0	0	0	0	1	0	0
Mont.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Neb.	1	0	0	0	0	1	0	0	0	0	0	0	0	0

Table D.1—Continued

	Policy													
State	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**10**	**11**	**12**	**13**	**14**	**15**
Nev.	1	0	1	0	0	0	0	0	0	0	0	0	0	0
N.H.	0	0	1	0	0	0	0	1	1	0	0	0	0	0
N.J.	1	1	0	0	1	1	1	1	0	0	1	0	0	0
N.M.	0	0	0	0	0	0	0	0	0	0	0	0	0	0
N.Y.	1	1	0	1	1	1	1	0	1	0	1	0	0	0
N.C.	1	0	1	0	0	1	0	1	1	0	0	0	0	0
N.D.	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Ohio	0	0	0	0	1	0	0	0	0	0	0	0	0	0
Okla.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Oreg.	1	0	0	1	0	0	0	0	0	0	0	0	0	1
Pa.	1	0	1	0	0	0	1	0	1	1	0	0	0	0
R.I.	1	0	0	0	1	1	0	1	0	0	0	0	0	0
S.C.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
S.D.	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Tenn.	0	0	1	0	0	0	0	0	1	1	0	0	0	1
Tex.	0	0	1	0	0	0	0	1	0	0	0	0	0	1
Utah	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Vt.	0	0	0	0	0	0	0	0	0	0	0	1	0	0
Va.	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Wash.	1	0	0	0	0	0	0	0	0	0	0	0	0	0
W. Va.	0	0	1	0	0	0	0	0	0	0	0	1	0	0
Wisc.	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Wyo.	0	0	0	0	0	0	0	0	0	0	0	1	0	0

NOTE: For the policies that correspond to each number, see Table 3.1. Policy 9 (a media campaign to prevent child access) is excluded from this table because we treated that policy as if it were not present in any state.

Table D.2
State Population and Outcome Values Used in the Gun Policy Comparison Tool

State	Population	Firearm Suicides	Firearm Homicides	Accidental Firearms Deaths	Mass Shootings	Other Violent Crime	Property Crime	Hunting Licenses	Firearm Sales
Ala.	4,858,979	528	391	20	26.62	22,604	144,746	507,926	310,272
Alaska	738,432	123	42	5	2.42	5,333	20,806	107,131	54,407
Ariz.	6,828,065	707	236	5	21.03	27,703	207,107	326,779	268,313
Ark.	2,978,204	342	155	17	11.14	15,345	96,836	200,092	215,774
Calif.	39,144,818	1,559	1,396	29	197.38	165,022	1,024,914	283,539	961,386
Colo.	5,456,574	541	137	5	14.75	17,339	144,136	281,201	234,521
Conn.	3,590,886	104	80	5	17.26	7,728	65,066	42,535	79,372
D.C.	672,228	11	105	5	15.56	8,369	31,435	—	14,859
Del.	945,934	55	53	5	6.07	4,657	25,455	16,786	30,201
Fla.	20,271,272	1,630	880	30	130.56	92,585	570,270	175,349	622,321
Ga.	10,214,860	823	584	21	73.84	38,028	308,723	395,219	501,748
Hawaii	1,431,603	44	5	5	2.01	4,182	54,346	10,537	17,580
Ida.	1,654,930	215	22	5	2.09	3,536	28,858	258,547	113,805
Ill.	12,859,995	495	692	15	128.24	48,610	255,729	320,765	331,630
Ind.	6,619,680	517	294	14	36.36	25,280	171,847	278,322	308,897
Ia.	3,123,899	193	47	5	4.28	8,864	63,957	219,798	176,461
Kan.	2,911,641	224	95	5	14.84	11,225	79,199	239,335	153,745
Ky.	4,425,092	499	174	5	15.75	9,467	96,362	340,902	260,830

Table D.2—Continued

State	Population	Firearm Suicides	Firearm Homicides	Accidental Firearms Deaths	Mass Shootings	Other Violent Crime	Property Crime	Hunting Licenses	Firearm Sales
La.	4,670,724	468	456	17	64.49	24,727	156,629	370,528	258,101
Me.	1,329,328	128	14	5	2.83	1,706	24,327	165,781	65,296
Md.	6,006,401	244	445	5	35.38	26,946	139,048	124,187	162,267
Mass.	6,794,422	118	86	5	16.19	26,434	114,871	56,797	91,778
Mich.	9,922,576	692	436	14	60.74	40,660	187,101	763,618	499,576
Minn.	5,489,594	304	92	5	17.47	13,186	121,984	592,125	276,387
Miss.	2,992,333	292	264	18	15.96	7,995	84,790	218,161	202,100
Mo.	6,083,672	605	459	18	44.54	29,759	173,642	496,583	328,709
Mont.	1,032,949	174	20	5	2.60	3,575	27,100	229,317	79,912
Neb.	1,896,190	114	50	5	6.17	5,150	42,495	175,591	104,782
Nev.	2,890,845	293	128	5	10.25	19,940	77,137	65,606	120,697
N.H.	1,330,608	107	12	5	1.97	2,638	23,229	59,068	50,653
N.J.	8,958,013	189	273	5	38.09	22,516	145,701	74,067	121,004
N.M.	2,085,109	275	91	5	10.41	13,564	77,094	97,103	102,419
N.Y.	19,795,791	421	408	5	78.02	74,556	317,529	535,915	461,870
N.C.	10,042,802	787	438	39	40.94	34,335	276,183	545,032	480,965
N.D.	756,927	76	11	5	1.60	1,791	16,020	148,793	52,052
Ohio	11,613,423	861	484	15	47.91	33,398	300,525	404,997	484,878

Table D.2—Continued

State	Population	Firearm Suicides	Firearm Homicides	Accidental Firearms Deaths	Mass Shootings	Other Violent Crime	Property Crime	Hunting Licenses	Firearm Sales
Okla.	3,911,338	468	221	5	15.73	16,272	112,878	419,445	220,941
Oreg.	4,028,977	373	94	5	11.42	10,369	118,719	264,102	197,901
Pa.	12,802,503	932	522	16	56.26	39,681	232,085	969,633	597,410
R.I.	1,056,298	40	10	5	4.55	2,533	20,043	8,624	15,565
S.C.	4,896,146	469	360	12	31.78	24,301	161,245	206,397	258,533
S.D.	858,469	73	15	5	3.32	3,257	16,680	244,182	63,251
Tenn.	6,600,299	638	363	30	52.66	39,994	193,796	727,229	380,939
Tex.	27,469,114	1,994	1,110	43	107.29	111,911	777,739	1,060,455	1,248,073
Utah	2,995,919	313	41	5	5.71	7,017	89,278	207,331	165,553
Vt.	626,042	59	11	5	1.98	729	8,806	72,930	33,826
Va.	8,382,993	630	276	14	37.70	16,016	156,470	276,660	380,886
Wash.	7,170,351	537	162	5	20.41	20,183	248,369	180,829	299,373
W. Va.	1,844,128	215	50	5	4.80	6,161	37,251	220,811	131,345
Wisc.	5,771,337	424	175	5	10.74	17,407	113,924	717,381	304,746
Wyo.	586,107	95	13	5	1.42	1,286	11,151	133,568	47,502

SOURCES: See the description of data sources in the text of this appendix.

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The effects of firearm policies have rarely been the subject of rigorous scientific evaluation in comparison with most other policies with similarly consequential effects on public safety, health, and the economy. Without strong scientific evidence of the effects of laws, policymakers and the public rely heavily on the expert judgments of advocates or social scientists. This makes gun policy experts' estimates of the true effects of policies an important influence on gun policy debates and decisions.

In this report, RAND researchers describe the results of a survey in which gun policy experts estimated the likely effects of 15 gun-related policies on 12 societal outcomes. The researchers use these and other responses to establish the diversity of beliefs among gun policy experts about the true effects of gun laws, establish where experts are in more or less agreement on those effects, and evaluate whether differences in the policies favored by experts result from disagreements about the policies' true effects or disagreements in experts' policy objectives or values. The analysis suggests that experts on both sides of the gun policy debate share some objectives but disagree on which policies will achieve those objectives. Therefore, collecting more and stronger evidence about the true effects of policies is, the researchers believe, a necessary step toward building greater consensus.

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