Visualizing Firearm Mortality and Law Effects

An Interactive Web-Based Tool

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About This Tool

The RAND Corporation launched the Gun Policy in America initiative in January 2016 with the goal of creating objective, factual resources for policymakers and the public on the effects of gun laws. As part of this mission, we have investigated a variety of data sources that could help shed light on key questions about whether and how gun laws affect important public health and criminal justice outcomes. In this document and the accompanying tool, we explain the assumptions, data, and analysis that support the Firearm Law Effects and Mortality Explorer visualization tool on RAND's Gun Policy in America website. Development of this tool was supported by Arnold Ventures.

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Summary

The RAND Corporation launched the Gun Policy in America initiative in January 2016 with the goal of creating objective, factual resources for policymakers and the public on the effects of gun laws. As part of this mission, we have investigated a variety of data sources that could help shed light on key questions about whether and how gun laws affect important public health and criminal justice outcomes. In this document, we describe the data sources used to produce the visualizations in the Firearm Law Effects and Mortality Explorer tool on RAND's Gun Policy in America website. We also describe the assumptions underlying the data and the statistical models producing the law effect estimates depicted in the visualizations.

The Firearm Law Effects and Mortality Explorer is designed to provide users with information about the distribution of firearm deaths across states and demographic subgroups. In addition, it allows users to explore how those deaths might be affected by the implementation of a set of commonly enacted state firearm laws using estimates of those effects produced by the RAND research team.

Contents

Tables

Chapter 1. Mortality Data

The Firearm Law Effects and Mortality Explorer (hereafter, the visualization) on the RAND Corporation's Gun Policy in America website is designed to provide users with information about the distribution of firearm deaths across states and demographic subgroups. In addition, it allows users to explore how those deaths might be affected by the implementation of a set of common state firearm laws using estimates of those effects that we produced. In this document, we describe the data sources used to produce the visualization, the assumptions underlying the visualization, and the statistical models producing the law effect estimates that the visualization depicts.

The visualization depicts estimates of mortality rates for each state. Specifically, users can select among the following five types of mortality:

1. total firearm deaths
2. firearm suicides
3. firearm homicides
4. total suicides (including those not involving a firearm)
5. total homicides (including those not involving a firearm).

By default, the visualization presents mortality rates for an entire state's population, but users can choose to display mortality rates by the following ten subpopulations within states using decedent characteristics:

1. male
2. female
3. Black, not Hispanic
4. White, not Hispanic
5. Hispanic
6. ages 14–24
7. ages 25–44
8. ages 45–85
9. urban (see the appendix for definitions)
10. non-urban (see the appendix for definitions).

Thus, across 50 states and the District of Columbia, five outcomes, and 11 populations, the visualization depicts a total of 2,805 mortality rate estimates for all state-outcome-population combinations (cross-classifications between population subgroups—such as Black, not Hispanic female—are not presented in the tool).

Specifically, the visualization presents estimates of these state mortality rates relative to the national mortality rate for that outcome and population; we refer to this as the *relative mortality rate*. Thus, a state with a firearm mortality rate that is estimated to be 20 percent lower than the national average for the selected population will be shown as –20 percent. In addition, more-detailed information about each estimate, including *absolute mortality rates* that are expressed as deaths per 100,000 population for each outcome, can be found by selecting individual states.

Mortality rates are calculated using information drawn from the Centers for Disease Control and Prevention's (CDC's) Wide-Ranging Online Data for Epidemiologic Research (WONDER) system, a website that releases public data from the National Vital Statistics System (NVSS) (CDC, undated).

Ensuring Reliable Estimates That Preserve Privacy

Many of the outcomes we present are rare in a statistical sense, and several of the subpopulations may be too small within a single state and year to provide a reliable estimate of the relative mortality rate. In addition, CDC privacy protections prohibit the disclosure of mortality rates using data on fewer than ten decedents. Our estimates are designed to address these challenges by using data from multiple years to assess the state relative mortality rate when the estimate based on the most recent year of data would be unreliable or would compromise privacy. Specifically, when estimating the relative mortality rate, we generally use the shortest period necessary to satisfy the following two requirements:

1. The number of person-years used in the calculation for each state is large relative to the national average mortality rate. Specifically, the number of person-years must be large enough that one would expect 20 deaths if the state death rate were the same as the national average for a given subpopulation.
2. The combined number of deaths for that period is ten or more within the relevant state subpopulation.

The first rule ensures that relative mortality rate estimates are based on sufficiently large populations that they are stable. The second rule ensures that our estimates observe the CDC's data-use restrictions that prohibit the calculation of mortality rates using fewer than ten decedents.

Because of these rules, when we estimate the relative mortality rate for larger populations (e.g., the population of an entire state) and more-common outcomes (e.g., total firearm mortality), all estimates exclusively use the most recent year's data from CDC WONDER. When we look at smaller state subpopulations and rarer outcomes, however, the relative mortality rate may be computed over more years to ensure reliable estimates with appropriate privacy. In most cases, these estimates also use data from CDC WONDER. However, when we cannot meet our reliability and privacy requirements even when combining data over a decade, we do not provide estimates using the death counts available in CDC WONDER.

What we do in these instances depends on whether the limitation is because the population is too small to produce any reliable estimate or the mortality rate itself is so low that we cannot meet the CDC privacy requirements. We never present estimates when the subpopulation itself is so small that no reliable estimate can be produced even when we look at a decade of data. This includes cases in which a conceptual subpopulation does not exist (e.g., urban residents of Wyoming). For these estimates, the state is shaded in gray in the visualization, regardless of whether death counts are available in CDC WONDER. However, in other instances, a state may have enough person-years over a ten-year period to expect at least 20 deaths if those deaths occurred at the national rate for the population, but that state has fewer than ten deaths recorded in WONDER. For the small number of relative mortality rate estimates for which this occurs, we provide an upper-bound estimate by assuming that there were nine deaths in the past decade; this is consistent with CDC privacy requirements that one not base estimates on the actual number of decedents when there are fewer than ten. Although these are upper-bound estimates, the estimates always show that the rate in that state is substantially lower than the national average.

In short, the relative mortality rates are calculated using the most recent year of data when both rules are satisfied. When both rules are not satisfied, relative mortality rates are based on the fewest years necessary to satisfy both rules. (We search over the past two, three, five, or ten years.) If both rules are not satisfied with ten years of data, then either (1) no estimate of the state's relative mortality rate is produced (if the population is too small to satisfy rule 1) or (2) an upper-bound estimate of the relative mortality rate is generated (if rule 1 is satisfied but there are still fewer than ten deaths recorded in WONDER).

Table 1.1 describes the frequency with which state relative mortality rates were calculated using one or more years of data or were suppressed because rule 2 was not met even with ten years of data.

Table 1.1. Length of Time Used for Estimates of Relative Mortality Rates

Period Used for Estimate	Number	Percentage
One year	2,326	85
Two years	175	6
Three years	66	2
Five years	49	2
Ten years	77	3
Not estimable	42	2

NOTE: Estimates of state relative mortality risk across all outcomes and state subpopulations (*N* = 2,735) while applying our requirements for reliability and privacy; 2020 was the most recent year of data available.

In addition to the visualization, which shows how relative mortality rates vary across states, selecting any state will display details about that state in a box to the right of the maps in the visualization. Within these state data boxes, we provide details about the relative mortality rate estimate, including the specific years used in the calculation, the absolute mortality rates per

100,000 people, and confidence intervals for all estimates. When presenting the absolute mortality rates, we normalize all estimates to the most recent year regardless of the time interval over which we estimated the relative mortality rate. Specifically, we compute the rate per 100,000 by multiplying the relative mortality rate (which sometimes uses multiple years of data) by the national rate of that outcome per 100,000 people for that subpopulation in the most recent year. Thus, our method for estimating the absolute mortality rate in the most recent year assumes that the relative mortality rate was constant over the period used to derive the estimate for those estimates that required multiple years. This assumption appears to be appropriate for these data; there is evidence of high stability in the state relative mortality rate regardless of the length of the period used for estimation. For instance, Table 1.2 presents the correlation of state relative mortality rate estimates using the most recent year of data and those using longer ranges of data for the total firearm deaths outcome.

Table 1.2. Correlation of State Relative Mortality Rate Estimates Based on the Most Recent Year of Data and on Longer Periods of Data

	Two Years	Three Years	Five Years	Ten Years
Correlation	0.995	0.992	0.988	0.985

NOTE: Correlations are for estimates of the total firearm deaths outcome and total state population; 2020 was the most recent year of data available.

The *relative mortality rate* estimate for any given state can be accessed on the online visualization by clicking on the box corresponding to that state. In addition, the online maps of state mortality rates are shaded to indicate which states have higher or lower mortality rates using an eight-color ordinal rescaling of the relative mortality rate estimates. We considered two types of scaling to produce the color categories: (1) equal-interval scaling, in which the eight categories represent equal step-sizes on the underlying metric, preserving the differences in the underlying metric but potentially creating categories with substantially different numbers of observations; and (2) equal quantile scaling, which maximizes the number of ordinal distinctions across states that can be displayed in the visualization for any fixed number of categories but that are unevenly spaced on the underlying measure. Because the measure being scaled is a ratio with a long right-side tail, these two options result in substantially different definitions for the ordinal categories. Equal interval scaling resulted in figures that do not make many distinctions between states with higher or lower risk because most states fell into only a couple of the categories, while equal quantile scaling resulted in some distinctions between colors corresponding to very large differences in mortality risk and other distinctions corresponding to small increments of risk. The scaling we chose is a compromise between these approaches. The scaling is at an equal interval from the lowest values through 20 percent. For the three categories above that point, the intervals between categories get progressively larger so that they include a larger percentage of the observations, which is closer to an equal quantile scale. Across all the outcomes and populations presented in the visualization, the median values of relative mortality rate for the eight ordered categories are –71, –51, –29, –9, 9, 33, 67, and 134.

Chapter 2. Law Effect Estimates

The visualization allows users to turn laws on and off to see estimates of how state firearm mortality rates might differ under different law regimes. These state-level effects present the law regimes that exist in each state to illustrate that the effects of hypothetical law changes would not be the same across states.

The law effects incorporated in the visualization cover six classes of laws, two of which have multiple levels:[1]

1. laws requiring background checks for all handgun sales
2. laws setting a minimum age of purchase to 20 years
3. laws regulating waiting periods between handgun purchase from a dealer and full possession:
    a. laws requiring at least a 24-hour waiting period
    b. laws requiring at least a seven-day waiting period
4. laws that reduce restrictions on carrying a concealed weapon relative to jurisdictions that require concealed-carry permits and allow law enforcement discretion in issuing permits:
    a. shall-issue concealed-carry laws that do not allow law enforcement discretion to deny the right to carry a concealed weapon to all those who meet permit requirements
    b. laws that allow concealed carry without a permit for anyone who can legally possess a firearm
5. stand-your-ground laws that permit the use of lethal force for self-defense outside the defender's home or vehicle, even when a retreat from danger would have been possible (According to the RAND State Firearm Law Database, case law is not classified as a stand-your-ground law in the absence of a state statute granting those rights [Cherney et al., 2022].)
6. child-access prevention laws that specify either civil or criminal penalties for storing a handgun in a manner that allows access by a minor.

Data on when these laws were implemented or repealed in each state were drawn from the RAND State Firearm Law Database, Version 4.0 (Cherney et al., 2022).

[1] The underlying model used to generate estimates includes two types of federal laws that cannot be manipulated in the visualization: (1) minimum age of possession of 18 or older and (2) background checks required for dealer sales. The model assesses the effects of state laws, not the effect of laws implemented federally, which may have substantially different effects. However, these two laws are implemented by federal law and can no longer be changed by individual states. If users wish to manipulate those laws, a change to federal law would be required (the effects of which are unknown). For this reason, we leave these two laws turned on for all estimates produced in the visualization.

To estimate the effects of these laws on firearm death rates, we used NVSS mortality data drawn from CDC WONDER for 50 states from 1981 to 2019. The specific model used for effect estimation was pre-registered prior to including policy effects (see Morral et al., 2021); this model was selected based on Monte Carlo simulations of candidate models to identify the best-performing method for estimating causal effects of state-level policies on firearm deaths. Model performance was evaluated based on the accuracy of both the estimates themselves and the estimated uncertainty around those estimates (Cefalu et al., 2021; Schell, Griffin, and Morral, 2018).

The full model specification is detailed in the appendix. We used a negative binomial regression of each outcome on (1) an offset equal to the natural logarithm of the population in that state-year; (2) effects for each year in the data; (3) first- and second-order autoregressive effects equal to the natural logarithm of the rate of the outcome in the relevant prior years for a given state; (4) state characteristics included as covariates; (5) indicators for each individual law; and (6) terms to remove the bias that occurs in autoregressive models of causal effects because such models control for the prior year's outcome, which is endogenous to the treatment.

The effects of each law were parameterized to allow a flexible phase-in of the effect over the five years following implementation, incorporating both an instant effect at the time of implementation and a linear phase-in term that changes gradually over the five-year period. Our primary effect estimate is the combination of the instant and phased-in effects (i.e., the total effect of the policy at the five-year post-implementation time point). The full modeled function of law effects from implementation through six years is presented in figures in the tool.

We used Bayesian estimation of the model, an approach that has three advantages for our purposes. First, simulation studies revealed that the model we are using yields slightly underestimated standard errors for causal effects derived from combinations of model parameters when the model is estimated using standard maximum likelihood optimization algorithms (Cefalu et al., 2021). Bayesian estimation gave a more accurate picture of the uncertainty in the estimates. Second, Bayesian methods allowed us to estimate the probability that a given law is associated with an increase or a decrease in firearm deaths. Such probabilities likely are helpful to policymakers who, when faced with a choice to support a given firearm law, may gain more-valuable information from knowing the likelihood that a law will be helpful than from knowing whether any effect of the law is statistically significant (Cook and Ludwig, 2006). Finally, simulations revealed that estimates of the effects of state gun policies often lack sufficient statistical power to detect effects of the size likely to be found for common gun policies, even when these effects are of a magnitude that would be of substantial interest to policymakers (e.g., a law that would reduce firearm deaths by 1,000 nationally every year) (Schell, Griffin, and Morral, 2018). Conducting significance testing with such low statistical power results in a high probability of producing inconclusive or inaccurate results, even when there is useful information about the true effect within the available data. Using Bayesian inference generally avoids these inferential problems in the same data when estimated with reasonable priors (Gelman and Tuerlinckx, 2000).

Priors for all covariates were weakly informative and centered on zero. Priors for each law's effect were selected such that the total effect on firearm deaths of each law evaluated five years after implementation was normally distributed and centered on no effect (i.e., an equal likelihood that the law increased or decreased firearm deaths). When integrated over the coefficients for each law, the standard deviation of the prior implied a 0.95 probability that the total effect size for each law on firearm mortality falls between a ln(incidence rate ratio [IRR]) of –0.2 and 0.2 (corresponding to IRRs of 0.82 and 1.22). This implies that it is unlikely that any single policy would change firearm mortality rates by more than about 20 percent. The selection of this prior is based on findings from a survey of gun policy experts, which showed this range of expected gun policy effect sizes (Smart, Morral, and Schell, 2021). Regression coefficients for several covariates judged as unlikely to represent confounds were estimated with a regularizing prior (Bayesian Lasso). The appendix contains further discussion of priors and model results using minimally informative priors.

These models were run separately for each of the five outcomes. The posterior distributions generated from these models include effect estimates for each gun law, which can be combined to estimate the effect of aggregations of gun laws over a five-year period. To estimate how state mortality rates for a given outcome would be expected to change with any new combination of laws, the visualization uses a sample of 5,000 draws from the posterior distribution of the corresponding model to estimate the ratio of expected mortality given the selected laws, compared with the expected risk of the states' actual laws. This ratio expresses how mortality would change under the hypothetical combination of laws selected by the user, compared with mortality as expected by the model with the states' actual laws. For instance, an IRR of 1.05 would indicate that expected mortality under the user-selected laws would be 1.05 times that of the expected mortality under the states' actual law regime. Detailed descriptions of these calculations are provided in the appendix.

In the visualization, users can toggle each of the law classes or their subtypes on or off at the national level to see how estimated mortality rates for each state would change. When a law is turned on, the visualization produces an estimate of how death rates in each state would change if each state had implemented the law five or more years ago. These estimates are derived from the RAND model and presented in the visualization as expected percentage differences in mortality rates compared with each state's true law regime. A –5-percent change, for instance, indicates that if the user-selected law combination had been passed five years earlier, the expected mortality rate would be 5 percent lower than the state's observed mortality rate. For states that have had the law for five or more years, the user's selection of that law has no effect on expected death rates.

Complete results from this model have been submitted for publication elsewhere. Additional model details and sensitivity tests are included in the appendix.

Chapter 3. Key Assumptions and Limitations

Our approach to characterizing mortality rates in 2020—and describing how state firearm laws affect those rates—relies on several assumptions and has noteworthy limitations:

- In those cases in which our estimate of a relative mortality rate uses multiple years of data, we assume that these relative rates change little over the period used for estimation. As shown in Table 1.2, this assumption appears to be valid. This assumption is made for only the relative rate estimates, but it affects both the relative and absolute mortality rate estimates. Specifically, we do not assume that the absolute mortality rate is constant over that period; we assume only that it varies over the period in the same manner as the national rate for that specific outcome and subpopulation, consistent with a constant relative rate estimate.
- The statistical model we use for estimating causal effects of state firearm policies relies on policy variation across states for identification. Thus, although some of the policies we evaluate have been enacted at the federal level, our method cannot provide effect estimates for federal policies. The effects of national laws could be different from the effects of similar policies implemented by individual states; for example, if it is easier to evade firearm purchase restrictions when such restrictions are not enforced in neighboring states.
- We limit this analysis to those state laws with the largest number of policy transitions over the study period because causal effects are not reliably estimated when few states provide information on the effects of a law (Schell, Griffin, and Morral, 2018). Therefore, our analysis may not capture some of the most-innovative or -effective approaches to mitigating firearm violence. For instance, there is some evidence that permit-to-purchase laws reduced firearm suicides and homicides in Missouri and Connecticut (Crifasi et al., 2015; Webster, Crifasi, and Vernick, 2014). However, because very few states changed their permit-to-purchase laws over our study period, the effects of this law class were not separately estimated; instead, they were grouped with other laws requiring background checks on private sales of firearms in our universal background check policy. Other similarly rare laws also could not be included separately in our model.

Appendix. Technical Details of the Visualization Tool, Underlying Statistical Model, and Calculations

In this technical appendix, we provide additional details about the data used to produce the visualization and model estimates, the statistical model that generates the effect estimates, and the method used to calculate state mortality rates under user-selected combinations of laws.

Data

Mortality Data

Mortality data from 1979 to 2020 come from the NVSS, which contains information on coroners' cause-of-death determinations for a near-census of deaths in the United States. All data were extracted from CDC WONDER (CDC, undated), using WONDER specifications for age, gender, race, ethnicity, and urbanicity.

WONDER's urbanicity categorization is based on the 2013 National Center for Health Statistics' Urban-Rural Classification Scheme for Counties (CDC, 2017), which assigns one of five categories to each county in the United States: large central metro, large fringe metro, medium metro, small metro, micropolitan (non-metro), and non-core (non-metro). The urban category is made up of all large central metro and large fringe metro counties in each state. The nonurban category is made up of all other categories. Using these definitions, 11 states have no urban counties (Alaska, Hawaii, Idaho, Iowa, Maine, Montana, Nebraska, North Dakota, South Dakota, Vermont, and Wyoming), so no urban mortality estimates are generated for these states. Similarly, there are no rural counties in Rhode Island or the District of Columbia.

State Laws

The model used by the law visualization tool estimates effects for the ten separate laws or policies that fall into six law classes using law data from the RAND State Firearm Law Database (Cherney et al., 2022). Tables A.1 and A.2 contain data on the years over which each law type applied to each state. More detail on the specific months of implementation and the law citations are available in the RAND State Firearm Law Database.

Table A.1. Years of Implementation for Laws Governing Purchase and Possession of Firearms over the Studied Period, by State

State	Dealer Background Check	Universal Background Check	Waiting Period, 24 Hours	Waiting Period, Seven Days	Minimum Age Possession: 18	Minimum Age Purchase: 20
Alabama	1994–2019	N/A	1975–2000	N/A	1994–2019	N/A
Alaska	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Arizona	1994–2019	N/A	N/A	N/A	1993–2019	N/A
Arkansas	1994–2019[a]	N/A	1994–1997	N/A	1989–2019	N/A
California	1975–2019	1991–2019	1975–2019	1976–2019	1975–2019	1991–2019
Colorado	1994–2019	2013–2019	N/A	N/A	1993–2019	N/A
Connecticut	1994–2019	1995–2019	1975–1995	1975–1995	1994–2019	1994–2019
Delaware	1990–2019	2013–2019	N/A	N/A	1994–2019	1987–2019
Florida	1990–2019	N/A	1991–2019	N/A	1994–2019	2018–2019
Georgia	1994–2019	N/A	1994–1996	N/A	1994–2019	1975–1994
Hawaii	1975–2019	1975–2019	1988–2019	1988–2019	1994–2019	1994–2019
Idaho	1994–2019	N/A	N/A	N/A	1994–2019	N/A
Illinois	1975–2019	N/A	1975–2019	N/A	1975–2019	1975–2019
Indiana	1983–2019	N/A	1975–1998	1975–1998	1994–2019	N/A
Iowa	1990–2019	1990–2019	1978–2019	1978–2019	1979–2019	1979–2019
Kansas	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Kentucky	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Louisiana	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Maine	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Maryland	1975–2019	1996–2019	1975–2019	1975–2019	1994–2019	1975–2019
Massachusetts	1975–2019	N/A	N/A	N/A	1994–2019	1998–2019
Michigan	1975–2019	N/A	N/A	N/A	1991–2019	N/A
Minnesota	1977–2019	N/A	1977–2019	1977–2019	1975–2019	N/A
Mississippi	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Missouri	1975–2019	1975–2007	N/A	N/A	1994–2019	1981–2007
Montana	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Nebraska	1991–2019	1991–2019	1991–2019	1991–2019	1977–2019	1991–1994
Nevada	1994–2019	N/A	N/A	N/A	1994–2019	N/A
New Hampshire	1994–2019	N/A	1994–1994	N/A	1994–2019	N/A

State	Dealer Background Check	Universal Background Check	Waiting Period, 24 Hours	Waiting Period, Seven Days	Minimum Age Possession: 18	Minimum Age Purchase: 20
New Jersey	1975–2019	1975–2019	1979–2019	1979–2019	1975–2019	2001–2019
New Mexico	1994–2019	2019–2019	1994–1998	N/A	1994–2019	N/A
New York	1975–2019	1975–2019	N/A	N/A	1994–2019	2000–2019
North Carolina	1994–2019	1995–2019	1975–2019	1975–2019	1993–2019	N/A
North Dakota	1994–2019	N/A	1994–1998	N/A	1985–2019	N/A
Ohio	1994–2019[a]	N/A	N/A	N/A	1994–2019	1975–2019
Oklahoma	1994–2019	N/A	1994–1998	N/A	1993–2019	N/A
Oregon	1990–2019	2015–2019	1975–1996	1990–1996	1990–2019	N/A
Pennsylvania	1994–2019	1998–2019	1975–1998	N/A	1994–2019	N/A
Rhode Island	1975–2019	1975–2019	1975–2019	1990–2019	1994–2019	1975–2019
South Carolina	1994–2019	N/A	N/A	N/A	1975–2019	1975–1998
South Dakota	1994–2019	N/A	1975–2009	N/A	1994–2019	N/A
Tennessee	1975–2019	1975–1998	1975–1998	1989–1998	1994–2019	N/A
Texas	1994–2019	N/A	1994–1998	N/A	1994–2019	N/A
Utah	1994–2019	N/A	N/A	N/A	1993–2019	N/A
Vermont	1994–2019	2018–2019	1994–1998	N/A	1994–2019	2018–2019
Virginia	1989–2019	N/A	N/A	N/A	1993–2019	N/A
Washington	1975–2019	2014–2019	1975–2019	2014–2019	1994–2019	1975–1994; 2019–2019
West Virginia	1994–2019	N/A	1994–1998	N/A	1989–2019	N/A
Wisconsin	1991–2019	N/A	1976–2015	N/A	1975–2019	N/A
Wyoming	1994–2019	N/A	1994–1998	N/A	1994–2019	2010–2019

NOTE: The study period covers January 1975 to December 2019. Laws implemented prior to 1975 are listed as starting in 1974.

[a] Arkansas and Ohio had temporary discontinuations of requirements for dealers to conduct background checks related to *Printz v. United States*. We treat these states as not having federal dealer background check requirements for partial periods in 1997 and 1998. Detailed information about effective dates and law citations is provided in the RAND State Firearm Law Database (Cherney et al., 2022).

Table A.2. Years of Implementation for Laws Governing Firearm Storage and Use over the Studied Period, by State

State	Child-Access Prevention	Stand-Your-Ground	Shall-Issue	Permitless-Carry
Alabama	N/A	2006–2019	1975–2019	N/A
Alaska	N/A	2013–2019	1994–2019	N/A
Arizona	N/A	2006–2019	1994–2019	2010–2019
Arkansas	N/A	N/A	1995–2019	N/A
California	1992–2019	N/A	N/A	N/A
Colorado	N/A	N/A	2003–2019	N/A
Connecticut	1990–2019	N/A	1975–2019	N/A
Delaware	1994–2019	N/A	N/A	N/A
Florida	1989–2019	2005–2019	1987–2019	N/A
Georgia	N/A	2006–2019	1989–2019	N/A
Hawaii	1992–2019	N/A	N/A	N/A
Idaho	N/A	N/A	1990–2019	N/A
Illinois	2000–2019	N/A	2013–2019	N/A
Indiana	N/A	2006–2019	1980–2019	N/A
Iowa	1990–2019	2017–2019	2011–2019	N/A
Kansas	N/A	2006–2019	2007–2019	2015–2019
Kentucky	N/A	2006–2019	1996–2019	2019–2019
Louisiana	N/A	2006–2019	1996–2019	N/A
Maine	N/A	N/A	1985–2019	N/A
Maryland	1992–2019	N/A	N/A	N/A
Massachusetts	1998–2019	N/A	N/A	N/A
Michigan	N/A	2006–2019	2001–2019	N/A
Minnesota	1993–2019	N/A	2003–2019	N/A
Mississippi	N/A	2006–2019	1990–2019	2016–2019
Missouri	N/A	2016–2019	2004–2019	2017–2019
Montana	N/A	2009–2019	1991–2019	N/A
Nebraska	N/A	N/A	2007–2019	N/A
Nevada	1991–2019	2011–2019	1995–2019	N/A
New Hampshire	2001–2019	2011–2019	1975–2019	2017–2019
New Jersey	1992–2019	N/A	N/A	N/A

State	Child-Access Prevention	Stand-Your-Ground	Shall-Issue	Permitless-Carry
New Mexico	N/A	N/A	2004–2019	N/A
New York	N/A	N/A	N/A	N/A
North Carolina	1993–2019	2011–2019	1995–2019	N/A
North Dakota	N/A	N/A	1985–2019	N/A
Ohio	N/A	N/A	2004–2019	N/A
Oklahoma	N/A	2006–2019	1996–2019	2019–2019
Oregon	N/A	N/A	1990–2019	N/A
Pennsylvania	N/A	2011–2019	1989–2019	N/A
Rhode Island	1995–2019	N/A	N/A	N/A
South Carolina	N/A	2006–2019	1996–2019	N/A
South Dakota	N/A	2006–2019	1985–2019	2019–2019
Tennessee	N/A	2007–2019	1996–2019	N/A
Texas	1995–2019	2007–2019	1996–2019	N/A
Utah	N/A	1994–2019	1995–2019	N/A
Vermont	N/A	N/A	1975–2019	N/A
Virginia	1992–2019	N/A	1995–2019	N/A
Washington	2019–2019	N/A	1975–2019	N/A
West Virginia	N/A	2008–2019	1989–2019	2016–2019
Wisconsin	1992–2019	N/A	2011–2019	N/A
Wyoming	N/A	2018–2019	1994–2019	N/A

NOTE: The study period covers January 1975 to December 2019. Detailed information about effective dates and law citations is provided in the RAND State Firearm Law Database (Cherney et al., 2022).

Covariates

The model of law effects used to estimate how different combinations of laws would affect state mortality rates includes 28 covariates that are divided into two broad classes (see Table A.3). Five of the covariates were identified by the research team as potential confounds as a result of our understanding of the existing theory and literature; these covariates represent factors that might affect both (1) whether or when states altered their firearm regulations and (2) the rate of firearm deaths in the state. For example, states in which the government is controlled by Democrats might be more likely to implement restrictions on firearms but also might make a variety of other policy choices that could affect firearm death rates. Similarly, states with high rates of gun ownership might be less likely to implement restrictions on firearms and also have more firearm deaths for reasons unrelated to those regulations.

Table A.3. State Characteristics Included as Covariates

Variable	Notes
Possible confounds	
Household gun ownership rate	Collider
Political control of state	Collider
Uniform Crime Reporting Program robbery + aggravated assault crime rate	Collider
Incarcerated persons per capita	Collider
Proportion of income for top 10 percent	
Other state characteristics	
Percentage unemployed	
Change in unemployment from prior	
Average income (inflation adjusted)	
Poverty rate	
Percentage younger than 15	
Percentage aged 15–29	
Percentage aged 30–59	Referent
Percentage aged 60 and over	
Percentage Caucasian	Referent
Percentage African American	
Percentage Asian/Pacific Islander	
Percentage American Indian/Alaska Native	
Percentage Hispanic	
Percentage married	Referent
Percentage divorced, separated, or widowed	
Percentage never married	
Percentage with a bachelor's degree or higher	
Gender ratio	
Percentage of children in single-parent household	
Percentage foreign-born	
Percentage military veterans	
Percentage urban households	
Percentage >25 years old, Black, and urban	
Wave of U.S. expansion	

Variable	Notes
Population density	
Alcohol consumption per capita	

NOTE: Variables that were judged to be possible colliders may be affected by the firearm laws or by firearm deaths. To minimize biases caused by this, they were lagged by five years (e.g., the 1995 version of the covariate is used when predicting outcomes in 2000). Variables that are treated as referents are included in the model by virtue of being linearly dependent on the other variables included in the model.

The remaining 23 state characteristics were seen as unlikely to be substantial confounds but might be associated with firearm deaths and represent sources of exogenous variation, which might lead to less accurate causal effect estimates if not accounted for in the models. As we discuss next in the Statistical Model section, this class of covariates was included in the model with regularization. Their coefficients were shrunk toward zero such that they were included in the model only to the extent that their inclusion improved prediction of firearm deaths.

In addition to classifying whether each covariate is a likely confound, the research team classified whether they were potential *colliders*, or variables that may be affected by either the laws of interest or by firearm deaths. Including such variables as covariates in the model will bias causal effect estimates (Pearl, 2009). To mitigate such problems, the four variables identified as potential colliders in Table A.3 were lagged by five years; that is, when predicting deaths in 2000, we use the 1995 value of the variable. This lagging reduces possible collider bias because the data precede the date of policy implementation at the five-year time point at which we are assessing the causal effect of policies.

The state characteristics are primarily standard measures from publicly available government sources (e.g., the U.S. Census Bureau, Federal Bureau of Investigation, National Institute on Alcohol Abuse and Alcoholism). The four exceptions are (1) household gun ownership rate, which is taken from Schell, Peterson et al. (2020); (2) state political control, which is derived as the proportion of legislative veto points controlled by Republicans using data from the National Conference of State Legislatures; (3) proportion of income for the top 10 percent of the population, which is a measure of income inequality included in the World Inequality Database (World Inequality Database, undated); and (4) wave of U.S. expansion, which is a three-level measure of how recently a state was admitted to the union—with the original 13 states, before 1865, and after 1865.

Prior to analysis, these variables were transformed to address undesirable distributional properties. In a few cases, we imputed missing state-year values using linear interpolation between the prior-year and subsequent-year values for that state. For a few predictors with extreme outliers, we applied transformations to limit the influence of outlier values: We applied the minimal power transformation (e.g., square root) that ensured that all values were within four standard deviations (SDs) of the mean. We reduced collinearity between these state characteristics and the year effects included in the model by removing the national trend from each. Finally, time-varying covariates were standardized to mean = 0, SD = 1.

Statistical Model

The selection of the model was based on a series of simulation studies to identify the best model for analyzing state-level firearm death data (Cefalu et al., 2021; Schell, Griffin, and Morral, 2018). These studies demonstrated that a particular autoregressive model yielded accurate type I error (i.e., unbiased standard errors), the highest statistical power (i.e., lowest error of the estimate), and minimal bias. These models substantially outperformed more-common models, such as two-way fixed effects, with respect to accurate standard errors, low variance/accuracy estimates, and the sensitivity of results to included covariates. However, to get the benefits of any autoregressive model, one needs to address bias in the magnitude of the effect size estimate that occurs because the outcome at the prior period is endogenous to the treatment of interest. Although we have demonstrated three methods to do this (Cefalu et al., 2021; Schell, Cefalu et al., 2020; Schell, Griffin, and Morral, 2018), this study uses the method demonstrated in Cefalu et al. (2021). Of the methods we have investigated, this is the most accurate and flexible approach to correct for the biases in the effect sizes of autoregressive models of count outcomes.

For simplicity, we present a version of the model estimating the effect of a single law below, although our final model included indicators for all ten laws so that all effects control for the possible effects of correlated laws. For a specific mortality outcome and a single law, we have a model that takes the following form:

$$Y_t \sim \text{NegativeBinomial}\left(m_t, {}^{1}\!/_{f}\right)$$

$$\ln(m_t) = \ln(N_t) + d_1 \ln\left({}^{Y_{t-1}}\!/_{N_{t-1}}\right) + d_2 \ln\left({}^{Y_{t-2}}\!/_{N_{t-2}}\right) + b_1 PI_t + b_2 PS_t + b_3 PL_t + \boldsymbol{gX_t} + \boldsymbol{nU_t} + a - d_1 b_1 PI_{t-1} - d_1 b_2 PS_{t-1} - d_1 b_3 PL_{t-1} - d_2 b_1 PI_{t-2} - d_2 b_2 PS_{t-2} - d_2 b_3 PL_{t-2},$$

where Y_t is the number of firearm deaths in a given state in year t predicted in a log-link, negative binomial model from

- the population in that state-year, N_t, which is logged and used as an offset
- second-order autoregressive predictors, $\ln(Y_{t-1}/N_{t-1})$ and $\ln(Y_{t-2}/N_{t-2})$
- a dichotomous indicator of when a given policy is in effect, *PI*
- a time spline, *PS*, that increases linearly from 0 to 1 for five years following that policy's implementation and continues at 1 thereafter

- a time spline, PL, that increases linearly from 0 beginning five years after that policy's implementation, reaching 1 at ten years post-implementation and continuing at 1 thereafter (Next, we discuss this term's role in the model.)
- a vector of covariates that we judged to be possible confounds, $\boldsymbol{X}$, including indicators of year and the variables indicated in Table A.3
- a vector of covariates that we judged unlikely to be confounds, $\boldsymbol{U}$
- a constant, a.

The terms in the second row of equation 1 include no additional parameters, but are included to debias the causal effect estimates by removing the portion of the prediction at time t that is endogenous to the policy at times t–1 and t–2 as a result of including Y_{t-1} and Y_{t-2} as predictors in the model, even though those variables also depend on b_1, b_2, and b_3 (see Cefalu et al., 2021).

The model was estimated using Bayesian methods in Stan 2.22. The priors used were

- $d_1 \sim N$(mean = 0.5, SD = 1). The first order autoregressive coefficient is likely to fall between zero and one. This was selected to be a minimally informative prior.
- $d_2 \sim N$(mean = 0, SD = 1). The second order autoregressive coefficient is likely to fall between –1 and 1. This was selected to be a minimally informative prior.
- b_1, b_2, b_3 ~ N(mean = 0, SD = 0.071) with policy variables in the range of 0/1. The standard deviation of the total policy effect at five years post-implementation expected with this prior ($b_1 + b_2$) is 0.071 × sqrt(2) = 0.10 for total firearm deaths and firearm homicides. For firearm suicides, we use N(mean = 0, SD = 0.064), which corresponds to a total effect standard deviation of 0.09 across both parameters. ,
- $\boldsymbol{g} \sim N$(mean = 0, SD = 0.1) for standardized, continuous $\boldsymbol{X}$'s and N(mean = 0, SD = 0.2) for the 0/1 year indicators. These are weakly informative priors within this model because these outcomes are highly stable year to year and the model conditions on prior years' mortality rates.
- $\boldsymbol{n}$ ~ Laplace(mean = 0, SD = Q). We use a Bayesian LASSO to regularize covariates in $\boldsymbol{X}$. $\boldsymbol{n}$ is the parameter that controls the extent of regularization.
- Q ~ Half-cauchy(mean = 0, SD = 1). This is a relatively uninformative prior on the regularizing hyperprior.
- f ~ Half-normal(mean = 0, SD = 0.1). This is an uninformative prior on the inverse of the negative binomial overdispersion parameter.

The priors for each law's total effect at five years post-implementation (i.e., $b_1 + b_2$ for a given law) were selected such that the total effect on ln(firearm deaths) is normally distributed and centered on no effect (i.e., equal likelihood that the law increased or decreased firearm deaths). When integrated over the coefficients for each law, the standard deviation of the prior implies a 0.95 probability that the total effect size for each law on firearm mortality falls between a ln(IRR) of –0.2 and 0.2 (corresponding to IRRs of 0.82 and 1.22). This is an informative prior. The selection of this prior is based on an earlier survey of gun policy experts, which showed expected gun policy effect sizes in the range described by this prior (Smart, Morral and Schell, 2021). Specifically, from that survey, we determined that expert expectations of the effect sizes for a wide variety of gun laws (expressed on a log IRR scale) deviate from zero by an average of

0.10 when predicting total firearm deaths, an average of 0.10 for firearm homicide, and an average of 0.09 for firearm suicide.

To correctly parameterize the effect of each law class, we must make an assumption about how long laws take to achieve their full effects. For example, a law may make it easier to get a concealed-carry permit immediately on its implementation date, but it may take years for the percentage of the population with such permits to increase to a stable level. In our models, we have chosen the effect during the fifth year post-implementation as our primary time point for the estimation of the effects. The specific coding of the laws we used will allow a nonlinear phase-in of the effect over the first five years after implementation. Specifically, each of the ten law indicators has one function, *PI*, that allows for an immediate change after implementation of the policy, and a second function, *PS*, that phases in slowly over five years. The total effect of each law indicator is then assessed by combining these separate effects.

The primary specification also includes a third function of each law, *PL*, that is designed to ensure that our estimates of law effects are not influenced by changes in mortality rates that occur a decade or more after their implementation. Changes in mortality rates that occur between five and ten years after law implementation have progressively less influence on our estimate of policy effects. This model term is included to ensure that the changes in mortality rates used to identify causal effects occurred near the time of implementation. It also reduces concerns about the inclusion of several covariates that may be endogenous to gun deaths or gun policy. As discussed earlier, these covariates were lagged by five years prior to inclusion in the model but could be seen as collider variables beyond that five-year period. The inclusion of the *PL* spline helps ensure that our effect estimates are not biased by these colliders.

The outcome is modeled as discrete time, based on calendar year, although data on the implementation of various laws are known with greater precision than yearly. To take advantage of that precision, we first compute monthly values of the various treatment indicators. Then the annual values used in the model (*PI*, *PS*, *PL*) are computed as the average of the monthly spline value over each calendar year. Thus, the nominal five-year phase-in period extends across six calendar years for laws not implemented on January 1 of the implementation year.

Finally, the identification of policy effects for almost all policies comes from states implementing the policy in question. However, some states also repealed policies over the study period. For this effect estimation, we assume that the effects of implementation and repeal are symmetrical in shape and magnitude, but in opposite directions. Thus, effect estimates are identified by changes in mortality rates after implementation and after repeal.

Estimating Marginal Effects for User-Specified Law Combinations

When a user selects a set of laws to implement nationally, the visualization computes the marginal effect for each state, comparing that hypothetical policy regime with the actual policy regime in 2019. This is done assuming that the hypothetical regime was implemented five or more years before so that the estimate reflects the fully phased-in effect of the laws. The calculations are done in 5,000 draws from the model's posterior, allowing us to estimate both the median marginal effect of the hypothetical law regime and the 80-percent credible interval for that marginal effect. These effects are computed as IRRs but presented as percentage changes, (IRR–1) × 100. In addition, these effects are converted to the marginal change in the absolute mortality rate by multiplying the IRR by the absolute mortality rate in the most recent year of data, as discussed earlier. These estimates are provided for each state and outcome but are not given by subpopulation.

Sensitivity Tests

Sensitivity of Findings to the Chosen Priors

Our informative priors for the effects of laws were specified in our preregistration. The effect sizes implied by those priors are from a study in which gun policy experts were asked to estimate effect sizes for a wide variety of policies and outcomes (Smart, Morral, and Schell, 2021). We replicated the primary results using looser priors to address any concerns that the conclusions of the model depend on the choice of priors. Specifically, we estimated that the model with all priors one order of magnitude more dispersed was used in the primary specification (e.g., a $N(0,0.1)$ prior was replaced with a $N(0,1)$ for this sensitivity test).

The results of this sensitivity test show that, as expected, effect sizes are slightly larger with less informative priors. On average, the IRRs with looser priors are 0.003 further from 1 for the total firearm death rate outcome, and their 8-percent credible intervals are 0.009 wider. The use of informative priors makes the largest difference when estimating effects on firearm homicides, which is the sparsest outcome and has effect estimates with the widest credible intervals. For that outcome, the IRRs with looser priors are 0.013 further from 1, on average, and their credible intervals are 0.050 wider, on average. However, these small changes from loosening priors do not affect any of the study conclusions.

Finally, we evaluated whether there was any empirical evidence that the priors were well chosen. Specifically, we compared the leave-one-out cross-validation (LOO CV) prediction of the model with our specific, informative priors with one with uninformative priors. For all three outcomes, LOO CV error was lower with the specific, informative priors. This provides some empirical evidence that the priors used in the primary model specification were appropriate for the data.

Sensitivity to Inclusion of the Policy Lagged Spline

One nonstandard feature of the primary model was to condition estimates on a "distractor" indicator of treatment, labeled PL in the equation presented earlier. This is a lagged treatment indicator that begins five years after implementation and phases in linearly over an additional five years. Although this term is derived from the laws, it is not used in our computation of policy effects. We had two reasons for including this term. First, we wished to ensure that our estimates of effects at five years post-implementation were not influenced by variation that occurred substantially later than that—variation that may occur because of other policy or environmental factors that are associated with implementing these laws. Second, the model includes several covariates that are plausible colliders because of likely dependence on either the policies being evaluated or the outcomes. We mitigated that potential problem by lagging those indicators by five years, but this alone would be insufficient to prevent biased estimates beyond five years post-implementation if we allowed effects this late to contribute to our law effect estimates. The *PL* spline is designed to prevent or diminish such biases.

Despite that rationale, the particular covariates in question were weakly associated with these outcomes while controlling for the prior year's outcome, such that the size of any collider bias would be negligible. In addition, this is a type of dynamic time-series model that is inherently less sensitive to changes in the outcomes that occur many years after implementation. We also re-ran the models without those terms. For each of the three outcomes, the LOO CV error was slightly better in the simpler model that excluded the *PL* terms. The policy effects from the simplified model are very close to those from our preregistered main model. Across the three outcomes, the average difference in the posterior median IRR between the models with and without the PL terms is 0.006. The 80-percent credible intervals are also slightly narrower—by 0.003, on average—without those terms. However, none of the estimates changed in a way that would modify our conclusions.

Abbreviations

CDC	Centers for Disease Control and Prevention
IRR	incidence rate ratio
LOO CV	leave-one-out cross-validation
NVSS	National Vital Statistics System
SD	standard deviation
WONDER	Wide-Ranging Online Data for Epidemiologic Research

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