Tool

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Visualizing Firearm Mortality and Law Effects

An Interactive Web-Based Tool, Second Edition

For more information on this publication, visit **www.rand.org/t/TLA243-6-v2**.

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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 second version of the Firearm Law Effects and Mortality Explorer visualization tool on RAND's Gun Policy in America website, which was released in 2024. Development of this tool was supported by Arnold Ventures.

Justice Policy Program

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

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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 initiative, 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 second version of the Firearm Law Effects and Mortality Explorer tool, which was released in 2024, on RAND's Gun Policy in America website. We also describe the assumptions underlying the data and the statistical models that produce 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, this tool 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 that we produced.

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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. 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. women
2. men
3. Black, not Hispanic
4. White, not Hispanic
5. Hispanic
6. urban (see the appendix for definitions)
7. nonurban (see the appendix for definitions)
8. ages 14–24
9. ages 24–44
10. ages 45 and older.

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).

The law data used by the tool were updated on July 3, 2024.

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 such 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 (2021) (CDC, undated). 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 protections. 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 CDC 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, or four years first. If both required conditions are not met, we next look over the ten-year period from 2011 to 2020 (CDC does not provide a mortality dataset that spans the decade ending in 2021). If both rules are not satisfied with these 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	2,648	87
Two years	169	6
Three years	61	2
Four years	37	1
Ten years	25	1
Not estimable	101	3

NOTE: These are estimates of state relative mortality risk across all outcomes and state subpopulations (*N* = 3,041) while applying our requirements for reliability and privacy; 2021 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 data from 2020 and estimates using longer ranges of data for the total firearm deaths outcome.

Table 1.2. Correlation of 2020 State Relative Mortality Rate Estimates and Estimates Using 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.

The *relative mortality rate* estimate for any given state can be accessed on the 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 results 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.

Longitudinal Mortality Data

The 2024 update to the Firearm Law Effects and Mortality Explorer allows users to examine state mortality rates over time from 1979 to 2021 across the five mortality types: firearm deaths, firearm suicides, firearm homicides, total suicides, and total homicides. Data for these analyses were drawn from CDC WONDER. In the few cases in which state counts were suppressed because fewer than ten deaths were recorded, we calculated mortality rates over two-year intervals. Thus, for instance, the firearm homicide rate for a state in 1984 would be based on the rate from 1983 to 1984, and the rate for 1985 would be calculated as the rate from 1984 to 1985. In these cases, the state data window indicates that rates are calculated using sliding two-year windows. In all other cases, the state data windows indicate that rates are based on annual mortality rates.

Law data presented in these longitudinal mortality visualizations are drawn from the July 3, 2024, version of RAND’s State Firearm Law Database (Cherney et al., 2024).

Chapter 2. Law Effect Estimates

The visualization allows users to turn laws on and off to see estimates of how state firearm mortality rates could be expected to differ under different law regimes. These state-level effects assume that each state's existing law regime remains unchanged to illustrate that the effects of hypothetical law changes would not necessarily be identical across states. That is, when a user turns on a law—such as child-access prevention—nationally, it would cause a change in expected mortality only for states that did not already have such a law.

The law effects incorporated in the visualization cover 12 classes of laws, two of which have multiple levels:[1]

1. laws requiring background checks for all handgun sales, including private sales
2. laws setting a minimum age of purchase of handguns to 20 years
3. laws requiring at least a 24-hour waiting period between handgun purchase from a dealer and full possession by the purchaser
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., 2024].)
6. child-access prevention laws that specify either civil or criminal penalties for an adult storing a handgun in a manner that allows access by a minor
7. bans on the sale of assault weapons or high-capacity magazines; definitions of each type of banned equipment are made by the state
8. extreme risk protection orders or *red flag* laws that allow police to petition a court to have firearms removed from an individual who is suspected of posing a risk to themselves or others

[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.

9. domestic violence restraining orders, which prohibit firearm possession for anyone subject to such an order; users of the tool can select
    a. any kind of domestic violence restraining order
    b. domestic violence restraining orders with *ex parte* provisions that allow temporary emergency firearm removal orders before the subject of the order appears in court
10. state prohibitions on the purchase or possession of firearms for reasons of mental illness, incapacity, or court-mandated mental health care
11. prohibitions on the purchase or possession of firearms by individuals convicted of violent misdemeanors
12. comprehensive state preemption of local firearm regulations, which broadly prohibit any type of firearm regulations from being passed by substate jurisdictions, such as cities.

Data on when these laws were implemented or repealed in each state were drawn from the RAND State Firearm Law Database, July 3, 2024, version (Cherney et al., 2024).

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 2021. The specific model used for effect estimation was pre-registered prior to including policy effects (see Morral, Smart, and Schell, 2023); this model was selected using 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 on the basis of 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 an 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 uses 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. For example, a –5-percent change 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.

Additional model details and sensitivity tests are included in the appendix.

Chapter 3. Key Assumptions and Limitations

Our approach to characterizing mortality rates in 2021—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 generated 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 2021 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.

CDC WONDER does not offer a ten-year period over which to calculate subgroup mortality rates that ends in 2021. Instead, there is a series from 1999 to 2020, and a new series that runs from 2018 to 2021. The two series code race differently. The earlier series used four bridged race categories that assigned a single race to all decedents (American Indian or Alaska Native, Asian or Pacific Islander, Black or African American, or White), regardless of whether they are listed in death records as having more than one race. The newer series offers several different race codings, but not the bridged-race coding. For this data series, we use a six-race categorization (American Indian or Alaska Native, Asian, Black or African American, Native Hawaiian or other Pacific Islander, White, or more than one race).

Therefore, our 2024 version of the visualization counts as Black and White only those decedents listed as having a single race of either Black or White, not any that are classed as more than one race. When we must estimate mortality rates for these groups based on a full decade of data (2011–2020)—that is, when the four years from 2018 to 2021 are insufficient to produce a valid mortality rate—we are approximating the extent to which single-race individuals in a state have mortality rates that are higher or lower than the same race's mortality rate in the country using data concerning individuals listed as Black or White using the bridged-race categories. This procedure introduces some inaccuracy if, for instance, the state mortality rates of individuals classed as Black in the bridged-race coding differ from national rates for the bridged-race Black population by more or less than the state mortality rates of Black decedents using single-race coding differ from that population's national mortality rates. We believe that any resulting distortions in risk estimates for race are minor, and they concern only the subset of estimates requiring a decade of data to produce an estimate.

State Laws

The model used by the law visualization tool estimates effects for the ten separate laws or policies that fall into 12 law classes using law data from the RAND State Firearm Law Database, July 3, 2024, version (Cherney et al., 2024). Tables A.1, A.2, and A.3 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 Regulating Who May Legally Own, Purchase, or Possess Firearms over the Studied Period, by State

State	Minimum Age 20 Sales	Extreme Risk Protection Orders	Domestic Violence Restraining Orders	Domestic Violence Restraining Orders, Ex Parte	Mental Health Prohibitions	Violent Misdemeanor Prohibitions
Alabama			2015–2021		2015–2021	1979–2021
Alaska			1996–2021		2014–2021	
Arizona			1996–2021		1979–2021	
Arkansas					1979–2021	
California	1990–2021	2016–2021	1994–2021	1994–2021	1979–2021	1990–2021
Colorado		2019–2021	2013–2021	2013–2021		
Connecticut	1994–2021	1999–2021	2001–2021	2016–2021	1994–2021	1994–2021
Delaware	1987–2021	2018–2021	1999–2007		1992–2021	1979–2021
Florida	2018–2021	2018–2021	1998–2021		2018–2021	
Georgia	1979–1994					
Hawaii	1994–2021	2020–2021	1993–2021	1994–2021	1990–2021	1991–2021
Idaho						
Illinois	1979–2021	2019–2021	1996–2021	2010–2021	1979–2021	1996–2021
Indiana		2005–2021	2002–2021			
Iowa	1979–2021		2010–2021			
Kansas			2018–2021		2011–2021	
Kentucky						
Louisiana			2014–2021		2018–2021	
Maine			1997–2021	2003–2021	2002–2021	
Maryland	1979–2021	2018–2021	1996–2021		1988–2021	2003–2021
Massachusetts	1998–2021	2018–2021	1994–2021	1994–2021	1979–2021	
Michigan			1996–2021		1979–2021	

State	Minimum Age 20 Sales	Extreme Risk Protection Orders	Domestic Violence Restraining Orders	Domestic Violence Restraining Orders, Ex Parte	Mental Health Prohibitions	Violent Misdemeanor Prohibitions
Minnesota			2014–2021		1979–2021	2003–2021
Mississippi						
Missouri	1981–2007				1981–2021	
Montana			1995–2021			
Nebraska	1991–1994		2012–2021			
Nevada		2020–2021	2007–2021		2003–2021	
New Hampshire			2000–2021	2000–2021		
New Jersey	2001–2021	2019–2021	1991–2021	1991–2021	1979–2021	
New Mexico		2020–2021	2019–2021			
New York	2000–2021	2019–2021	1996–2021	1996–2021	1979–2021	
North Carolina			2003–2021	2003–2021	1982–2021	
North Dakota			1997–2021	1997–2021	1985–2021	1989–2021
Ohio	1979–2021		2008–2021		1979–2021	
Oklahoma					1995–2021	
Oregon		2018–2021	2016–2021		1994–2021	
Pennsylvania			2006–2021	2006–2021	1995–2021	
Rhode Island	1979–2021	2018–2021	2005–2021		1979–2021	
South Carolina	1979–1998		2015–2021		1979–2021	
South Dakota			1989–2021			
Tennessee			2009–2021		2010–2021	
Texas			2001–2021	2008–2021		
Utah			2008–2021	2018–2021	1994–2021	
Vermont	2018–2021	2018–2021	2001–2021			
Virginia		2020–2021	1994–2021	1994–2021	1990–2021	
Washington	1979–1994, 2019–2021	2016–2021	1994–2021	1994–2021	1992–2021	
West Virginia			2000–2021	2001–2021	1989–2021	
Wisconsin			1996–2021		1994–2021	
Wyoming	2010–2021				2010–2021	

NOTE: The study period covers January 1975 to December 2021. Detailed information about effective dates and law citations is provided in the RAND State Firearm Law Database, July 3, 2024, version (Cherney et al., 2024).

Table A.2. Years of Implementation for Laws Regulating Firearm Sales and Transfers over the Studied Period, by State

State	Universal Background Checks	Waiting Periods	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Comprehensive State Preemption
Alabama		1979–2000	1994–2004	2013–2021
Alaska		1994–1998	1994–2004	1986–2021
Arizona		1994–1994	1994–2004	1979–2021
Arkansas		1994–1997	1994–2004	1993–2021
California	1991–2021	1979–2021	1990–2021	
Colorado	2013–2021		1994–2004, 2013–2021	2003–2021
Connecticut	1994–2021	1979–2021	1993–2021	
Delaware	2013–2021		1994–2004	1986–2021
Florida		1991–2021	1994–2004	1987–2021

State	Universal Background Checks	Waiting Periods	Bans on the Sale of Assault Weapons and High-Capacity Magazines	Comprehensive State Preemption
Georgia		1994–1996	1994–2004	1999–2021
Hawaii	1979–2021	1979–2021	1992–2021	
Idaho		1994–1994	1994–2004	2008–2021
Illinois	1979–2021	1979–2021	1994–2004	1979–2021
Indiana	1983–1998	1979–1998	1994–2004	2011–2021
Iowa	1990–2021	1979–2021	1994–2004	1990–2021
Kansas		1994–1998	1994–2004	2005–2021
Kentucky		1994–1998	1994–2004	1984–2021
Louisiana		1994–1998	1994–2004	1985–2021
Maine		1994–1998	1994–2004	1989–2021
Maryland	1996–2021	1979–2021	1994–2021	1985–2021
Massachusetts	1979–2021	1979–2021	1994–2021	
Michigan	1979–2021	1979–2021	1994–2004	1991–2021
Minnesota		1979–2021	1994–2004	1985–2021
Mississippi		1994–1998	1994–2004	1986–2021
Missouri	1979–2007	1979–2007	1994–2004	1985–2021
Montana		1994–1998	1994–2004	1985–2021
Nebraska	1991–2021	1991–2021	1994–2004	
Nevada	1997–2021		1994–2004	1989–2021
New Hampshire		1994–1994	1994–2004	2004–2021
New Jersey	1979–2021	1979–2021	1990–2021	
New Mexico	2019–2021	1994–1998	1994–2004	1986–2019
New York	1979–2021	1979–2021	1994–2021	
North Carolina	1979–2021	1979–2021	1994–2004	1996–2021
North Dakota		1994–1998	1994–2004	1985–2021
Ohio			1994–2004	2007–2021
Oklahoma		1994–1998	1994–2004	1985–2021
Oregon	2015–2021	1979–1996	1994–2004	1996–2021
Pennsylvania	1998–2021	1979–1998	1994–2004	1979–2021
Rhode Island	1979–2021	1979–2021	1994–2004	1986–2021
South Carolina			1994–2004	1986–2021
South Dakota		1979–2009	1994–2004	1983–2021
Tennessee	1979–1998	1979–1998	1994–2004	1989–2021
Texas		1994–1998	1994–2004	1987–2021
Utah			1994–2004	1999–2021
Vermont	2018–2021	1994–1998	1994–2004, 2018–2021	1988–2021
Virginia	2020–2021		1994–2004	1987–2021
Washington	2014–2021	1979–2021	1994–2004	1983–2021
West Virginia		1994–1998	1994–2004	1982–2021
Wisconsin		1979–2015	1994–2004	1995–2021
Wyoming		1994–1998	1994–2004	1995–2021

NOTE: The study period covers January 1975 to December 2021. Detailed information about effective dates and law citations is provided in the RAND State Firearm Law Database, July 3, 2024, version (Cherney et al., 2024).

Table A.3. Years of Implementation for Laws Regulating the Legal Use, Storage, or Carrying of Firearms over the Studied Period, by State

State	Shall-Issue Concealed Carry Laws	Permitless-Carry Laws	Stand-Your-Ground Laws	Child-Access Prevention Laws
Alabama	1979–2021		2006–2021	
Alaska	1994–2021	2003–2021	2013–2021	
Arizona	1994–2021	2010–2021	2006–2021	
Arkansas	1995–2021	2021–2021	2021–2021	
California				1992–2021
Colorado	2003–2021			2021–2021
Connecticut	1979–2021			1990–2021
Delaware				1994–2021
Florida	1987–2021		2005–2021	1989–2021
Georgia	1989–2021		2006–2021	
Hawaii				1992–2021
Idaho	1990–2021	2016–2021	2018–2021	
Illinois	2013–2021			2000–2021
Indiana	1980–2021		2006–2021	
Iowa	2011–2021	2021–2021	2017–2021	1990–2021
Kansas	2007–2021	2015–2021	2006–2021	
Kentucky	1996–2021	2019–2021	2006–2021	
Louisiana	1996–2021		2006–2021	
Maine	1985–2021	2015–2021		2021–2021
Maryland				1992–2021
Massachusetts				1998–2021
Michigan	2001–2021		2006–2021	
Minnesota	2003–2021			1993–2021
Mississippi	1990–2021	2016–2021	2006–2021	
Missouri	2004–2021	2017–2021	2016–2021	
Montana	1991–2021	2021–2021	2009–2021	
Nebraska	2007–2021			
Nevada	1995–2021		2011–2021	1991–2021
New Hampshire	1979–2021	2017–2021	2011–2021	2001–2021
New Jersey				1992–2021
New Mexico	2004–2021			
New York				2019–2021
North Carolina	1995–2021		2011–2021	1993–2021
North Dakota	1985–2021	2017–2021	2021–2021	
Ohio	2004–2021		2019–2021	
Oklahoma	1996–2021	2019–2021	2006–2021	
Oregon	1990–2021			2021–2021
Pennsylvania	1989–2021		2011–2021	
Rhode Island				1995–2021
South Carolina	1996–2021		2006–2021	
South Dakota	1985–2021	2019–2021	2006–2021	
Tennessee	1996–2021	2021–2021	2007–2021	
Texas	1996–2021	2021–2021	2007–2021	1995–2021
Utah	1995–2021	2021–2021	1994–2021	
Vermont	1979–2021	1979–2021		
Virginia	1995–2021			1991–2021
Washington	1979–2021			2019–2021

State	Shall-Issue Concealed Carry Laws	Permitless-Carry Laws	Stand-Your-Ground Laws	Child-Access Prevention Laws
West Virginia	1989–2021	2016–2021	2008–2021	
Wisconsin	2011–2021			1992–2021
Wyoming	1994–2021	2011–2021	2018–2021	

NOTE: The study period covers January 1975 to December 2021. Detailed information about effective dates and law citations is provided in the RAND State Firearm Law Database, July 3, 2024, version (Cherney et al., 2024).

Covariates

The model of law effects used to estimate how different combinations of laws would affect state mortality rates includes 21 covariates that are divided into two broad classes (see Table A.4). 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 Democratic Party politicians 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.4. 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
State DW-NOMINATE dimension 1	Collider
State DW-NOMINATE dimension 2	Collider
Other state characteristics	
Proportion of income for top 10 percent	
Change in unemployment from prior	
Average income (inflation adjusted)	
Incarcerated persons per capita	Collider
Percentage aged 15–29	
Percentage African American	
Percentage Asian or Pacific Islander	
Percentage Hispanic	
Percentage divorced, separated, or widowed	
Percentage with a bachelor's degree or higher	
Percentage of children in a single-parent household	
Percentage foreign-born	
Percentage military veterans	
Percentage of urban households	
Population density	
Alcohol consumption per capita	

NOTE: *Colliders* are covariates that may be affected by the firearm laws or by firearm deaths. To minimize biases caused by colliders, the covariates were lagged by five years (e.g., the 1995 version of the covariate is used when predicting outcomes in 2000). DW-NOMINATE = dynamic, weighted nominal three-step estimation. DW-NOMINATE dimensions were derived from congressional roll call votes by Lewis et al., undated. Dimension 1 is liberal versus conservative; dimension 2 captures differences within parties on select issues.

The remaining 16 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 with the aim that 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 six variables identified as potential colliders in Table A.4 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) the household gun ownership rate, which is taken from Schell, Peterson et al. (2020); (2) two state political ideology dimensions corresponding to DW-NOMINATE scalings of votes by the state's legislators in the U.S. Congress (Lewis et al., undated); (3) a state political control variable, which is derived as the proportion of legislative veto points controlled by Republican Party politicians using data from the National Conference of State Legislatures; and (4) the proportion of state income received by the top 10 percent of earners in the population, which is a measure of income inequality included in the World Inequality Database (World Inequality Database, undated).

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 and 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 14 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:

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

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

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 vector of covariates that we judged to be possible confounds, $\boldsymbol{X}$, including indicators of year and the variables indicated in Table A.4
- a vector of covariates that we judged unlikely to be confounds, $\boldsymbol{U}$
- a constant, *a*.

The final four terms in the equation above 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 and b_2 (see Cefalu et al., 2021).

The model was estimated using Bayesian methods in Stan 2.22, which is a software platform designed for statistical modeling. The priors used were

- $a \sim N$(mean = 0, SD = sqrt(10)).
- $d_1 \sim N$(mean = 0.5, SD = 1). The first-order autoregressive coefficient is likely to fall between 0 and 1. 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, $b_2 \sim N$(mean = 0, SD = 0.071) with policy variables in the range of 0 and 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 uses 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 following 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 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*) 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.

To ensure that our estimates of the effects of each law class are identified from within-state changes in gun policy during the study period rather than from preexisting policy differences across states, we transformed the treatment indicators. Specifically, for each policy, we treat states that have the policy in effect for our entire study period (e.g., states that implemented a given policy in 1960) as control states (i.e., the model treats them as if they never had the policy). This is done by subtracting the 1979 value of each treatment indicator from the subsequent year's values. We report the results of sensitivity analyses comparing law effect estimates with and without this transformation in the "Sensitivity Tests" section.

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 $N(0,1)$ prior 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 across the five outcome models, and their 80-percent credible intervals are 10 percent 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 0.005 further from 1, on average, and their credible intervals are 16 percent 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 five 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 of Findings to Differencing Treatment Indicators

We examined the effect of subtracting the 1979 values for each law's implementation status from each subsequent year's treatment indicator by comparing results using this transformation with results without that transformation. The purpose of this transformation was to ensure that law effects are based more on changes in outcomes following a change in the laws than on preexisting differences between states that have and do not have the law. Therefore, we expected real differences between models using the transformation and those that did not.

The largest differences in effect estimates for individual laws were found in models of firearm homicides and total homicides, the models with the most sparse outcome data. Specifically, there was a 0.049 difference in the effect estimated for waiting periods; the model used transformed law indicators estimating an IRR = 0.99 (80-percent CI: 0.94–1.03), whereas the model without the transformation estimated an IRR = 1.04 (80-percent CI: 0.99–1.09). Estimates for universal background checks and comprehensive preemption laws also differed by just more than 0.04 in this model, again with the model using untransformed data estimating median IRRs greater than 1.00 and the model using transformed data estimating effects at or below IRR = 1. The same pattern of results was found for total homicides, although differences between the two models were smaller (less than 0.04) for each of these laws. In all other models, law effect estimates differed by a maximum of 0.024 and an average of 0.0005.

Abbreviations

CDC	Centers for Disease Control and Prevention
DW-NOMINATE	dynamic, weighted nominal three-step estimation
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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CDC—*See* Centers for Disease Control and Prevention.

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