Impact on Nonfirearm Deaths of Firearm Laws Affecting Firearm Deaths: A Systematic Review and Meta-Analysis

Rosanna Smart, PhD, Terry L. Schell, PhD, Matthew Cefalu, PhD, and Andrew R. Morral, PhD

Background. There is debate whether policies that reduce firearm suicides or homicides are offset by increases in non–firearm-related deaths.

Objectives. To assess the extent to which changes in firearm homicides and suicides following implementation of various gun laws affect nonfirearm homicides and suicides.

Search Methods. We performed a literature search on 13 databases for studies published between 1995 and October 31, 2018 (PROSPERO CRD42019120105).

Selection Criteria. We included studies if they (1) estimated an effect of 1 of 18 included classes of gun policy on firearm homicides or suicides, (2) included a control group or comparison group and evaluated time series data to establish that policies preceded their purported effects, and (3) provided estimated effects of the policy and inferential statistics for either total or nonfirearm homicides or suicides.

Data Collection and Analysis. We extracted data from each study, including study timeframe, population, and statistical methods, as well as point estimates and inferential statistics for the effects of firearm policies on firearm deaths as well as either nonfirearm or overall deaths. We assessed quality at the estimate (study–policy–outcome) level by using prespecified criteria to evaluate the validity of inference and causal identification. For each estimate, we derived the mortality multiplier (i.e., the ratio of the policy's effect on total homicides or suicides; expressed as a change in the number of deaths) as a proportion of its effect on firearm homicides or suicides. Finally, we performed a meta-analysis to estimate overall mortality multipliers for suicide and homicide that account for both within- and between-study heterogeneity.

Main Results. We identified 16 eligible studies (study timeframes spanning 1977–2015). All examined state-level policies in the United States, with most estimating effects of multiple policies, yielding 60 separate estimates of the mortality multiplier. From these, we estimated that a firearm law's effect on homicide, expressed as a change in the number of total homicide deaths, is 0.99 (95% confidence interval = 0.76, 1.22) times its effect on the number of firearm homicides. Thus, on average, changes in the number of firearm homicides caused by gun policies are neither offset nor compounded by second-order effects on nonfirearm homicides. There is insufficient evidence in the existing literature on suicide to indicate the extent to which the effects of gun policy changes on firearm suicides are offset or compounded by their effects on nonfirearm suicides.

Authors' Conclusions. State gun policies that reduce firearm homicides are likely to reduce overall homicides in the state by approximately the same number. It is currently unknown whether the same holds for state gun policies that significantly reduce firearm suicides. The small number of studies meeting our inclusion criteria, issues of methodological quality within those studies, and the possibility of reporting bias are potential limitations of this review.

Public Health Implications. Policies that reduce firearm homicides likely have large benefits for public health as there is little evidence to support a strong substitution effect between firearm and nonfirearm homicides at the population level. Further research is needed to determine whether policies that produce population-level reductions in firearm suicides will translate to overall declines in suicide rates. (*Am J Public Health*. 2020; 110: e1–e9. doi:10.2105/AJPH.2020.305808)

See also Azrael and Miller, p. 1456.

PLAIN-LANGUAGE SUMMARY

There is substantial public interest in identifying policies to reduce firearm deaths. However, the overall value of such laws will depend on the extent to which the changes in firearm deaths caused by a law are offset or compounded by corresponding changes in nonfirearm deaths. For example, a law that prevents 1000 firearm suicides would have little benefit if it leads to 1000 additional suicides by other means. Conflicting views about these second-order consequences of gun laws contribute to disagreements between those who support stricter gun regulation and those who oppose it. We analyzed data from 16 studies that provided 60 estimates of the effects of firearm laws on firearm deaths as well as either nonfirearm or overall deaths, and we found little evidence that the effects of firearm laws on firearm homicides are either offset or compounded by effects on nonfirearm homicides. Findings suggest that if a gun law prevents 100 firearm homicides, it is expected to prevent 99 total homicides after accounting for possible lethal means substitution, violence contagion, and other possible second-order effects. There is insufficient evidence in the existing literature on suicide to accurately assess the extent of the second-order effects of firearm laws on nonfirearm suicides.

In 2017, there were 47 173 suicides in the United States, half of which were firearm-related. Another 19 510 individuals were killed by homicide, nearly 75% of which were firearm-related.[1] While the scale of gun violence in the United States has led to public debate regarding policies that can reduce firearm death and injury, the overall benefits of policies that successfully reduce firearm-related suicides or homicides will depend on the extent to which any prevented firearm deaths are offset by increases in non–firearm-related deaths. As a hypothetical example, a gun law with a direct effect of reducing 1000 firearm deaths will have minimal public health benefit if 1000 additional individuals are killed by other means. Some have hypothesized that this type of deadly means substitution could substantially undermine any benefit of gun laws.[2]

Alternatively, the total public health benefit of a firearm law may be larger than just its effects on firearm outcomes. For example, reducing 1000 firearm homicides may break cycles of retaliatory violence, improve police effectiveness at crime prevention, or change community norms in ways that reduce nonfirearm homicides as well. Such spillover or behavioral contagion effects have been hypothesized for both suicide[3] and homicide,[4] although it is unclear how large such positive synergies might be.

Conflicting views about these second-order consequences of gun laws contribute to disagreements between those who support stricter gun regulation and those who oppose it. A survey of gun policy experts found that those favoring more-permissive gun laws expected that 90% of individuals prevented from dying by firearm suicide or homicide would still die by an alternative lethal means. By contrast, experts favoring more-restrictive gun laws expected that only 20% of prevented firearm deaths would result in death by alternative means.[5] Existing public health research demonstrating that some gun policies reduce firearm deaths may be entirely unconvincing to those who believe that the policy's second-order effects will systematically offset those reductions.

Although disagreements about the second-order effects of gun laws appear to play a large role in divergent policy views, it is a challenging issue to systematically investigate because this theory ties a policy's effect on nonfirearm deaths directly to its effect on firearm deaths. Therefore, the point of disagreement cannot be resolved by reviewing evidence for the effects of firearm laws on nonfirearm deaths alone; effects on nonfirearm deaths instead need to be evaluated as a function of the policy's effects on firearm deaths, and no empirical literature has presented such estimates. While several studies find support for small and partial lethal means substitution at the individual level for both suicide[6,7] and homicide,[8] individual-level means substitution is just 1 of several mechanisms that could undermine or enhance the effect of gun laws, so such studies may not address broader concerns about potential second-order mortality effects.

Although existing literature has not attempted to estimate the extent to which, on net, potential unintended effects of gun policies tend to undermine or enhance their direct effects on firearm outcomes, it does contain information that may allow us to estimate those effects. For example, many studies present causal effect estimates for firearm policies on both firearm homicide and nonfirearm homicide. Using this information, we estimated a mortality multiplier (i.e., a meta-analytic parameter relating a gun law's effect on all homicides or suicides to the size and direction of its effect on firearm homicides or firearm suicides). This provides valuable information on the extent to which hypothesized second-order effects of firearm laws (e.g., lethal means substitution, violence contagion) undermine or enhance the effects of gun laws on firearm deaths.

METHODS

We performed this study in 4 stages. First, we systematically reviewed the literature to identify research on the association between 18 classes of gun policy and 8 outcomes. Second, we extracted those studies that evaluated the relationship between firearm policy and both firearm deaths (homicide or suicide) as well as either total or nonfirearm homicide or suicide deaths. Using information within each study, we derived an estimate of a mortality multiplier, the ratio of the change in the number of total homicides or suicides attributable to the law as a proportion of the change in the number of firearm homicides or suicides attributable to the law. Finally, we created meta-analytic estimates across studies for homicide and suicide separately.

Search, Inclusion Criteria, and Data Extraction

Our review and meta-analysis followed Preferred Reporting Items for Systematic Reviews and Meta-Analyses guidelines. Identification of studies to inform these analyses followed from our systematic review of the effects of gun policy.[9] We registered the review protocol in PROSPERO (CRD42019120105) and preregistered the protocol for this study via Open Science Framework (https://osf.io/saem7). We set search strategy and inclusion criteria a priori according to this protocol.

Search. In November 2018, we searched 13 databases (PubMed, PsycInfo, Index to Legal Periodicals, Social Science Abstracts, Web of Science, Criminal Justice Abstracts, National Criminal Justice Reference Service, Sociological Abstracts, EconLit, Business Source Complete, WorldCat, Scopus, and LawReviews [LexisNexis]) for English-language working papers, books, or peer-reviewed journal articles that estimated a relationship between 1 of 18 classes of gun policies and 1 of 8 outcomes, including homicide and suicide. We used a broad set of search terms relevant for firearm policy (e.g., "gun," "firearm," "concealed carry") and for outcomes (e.g., "suicide," "murder"; details in Appendix A and Appendix B, available as supplements to the online version of this article at http://www.ajph.org). The search timeframe covered January 1, 1995, through October 31, 2018.

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Screening. Two trained reviewers independently screened titles and abstracts of identified articles, using a set of screening criteria developed by the research team. Discrepancies were resolved by consensus with input from a third reviewer. Final inclusion of studies was based on full-text evaluation. All screening was conducted in DistillerSR.

Inclusion criteria. Eligible studies were those that estimated an effect of 1 of our 18 classes of gun policy, evaluated time series data to establish that policies preceded their effects, and included a control or comparison group in model estimation. For the purposes of this study, we further required that the article provided estimated effects and standard errors for (1) firearm homicide death and either nonfirearm or total homicide death or (2) firearm suicide death and either nonfirearm or total suicide death. To allow greater comparability across study estimates, we excluded studies of homicide or suicide in specific subpopulations (e.g., intimate partner homicide), although we considered these in sensitivity analyses.

Extraction. Extracted information included metadata (e.g., title, authors), study features (e.g., timeframe, data sets), statistical methods (e.g., model type, analytic unit), and estimated effects (e.g., coefficient estimates, standard errors). One reviewer (R. S.) extracted data into a pretested standardized spreadsheet-based form. A second reviewer independently extracted information on estimated effects and checked other fields for accuracy; discrepancies were resolved by consensus.

Many studies provided multiple effect estimates. A single study may have estimated the effects of multiple different policies (e.g., waiting period laws and background check laws), assessed effects across different populations (e.g., children and adults), or estimated effects using different model specifications (e.g., linear and log-link models). When a study provided the required information for multiple different policies, we extracted each of these estimates; thus, a single study could contribute multiple estimates. When a study provided results for multiple populations, we extracted effects for the most representative population provided. If a study presented different estimates based on different model specifications, we extracted information only from the specification considered best suited to estimating the policy's causal effect. This was typically the authors' preferred specification; in a few cases, we selected a linear model although the authors preferred a log-link model because deriving the construct of interest from linear effect estimates requires fewer assumptions.

Quality assessment. We assessed risk of bias by using prespecified criteria shown to be important methodological considerations in quasi-experimental policy evaluations, and particularly in firearm policy research.[10,11] We conducted quality assessment for each estimate (study–outcome–policy) through discussion with the full review team regarding the following domains:

- evidence of model overfit,
- adjustment for serial correlation,
- validity of model assumptions,
- sensitivity of results to model specification,
- number of treated units and pre- and posttreatment data, and
- other threats to causal identification (e.g., failure to adjust for confounds).

We indicated whether each estimate had an issue on each domain as described in Appendix C (available as a supplement to the online version of this article at http://www.ajph.org). We used quality criteria to perform standard error adjustments for estimates that failed to adjust for serial correlation (see "Sensitivity Analysis") and to narratively describe the quality of the underlying studies contributing to the meta-analyses.

Estimating the Mortality Multiplier

We aimed to estimate the extent to which causal effects on firearm suicide or homicide translate into changes in overall suicide or homicide. Thus, we defined the mortality multiplier (m) as the total effect of the firearm policy on all homicides or all suicides, given a unit change in firearm homicide or firearm suicide, where the causal effect of the policy only affects nonfirearm mortality through its effects on firearm mortality. We expressed m separately for homicide and suicide as

$$(1)\quad m = \frac{\Delta^T}{\Delta^F} = \left(\frac{\Delta^{NF}}{\Delta^F} + 1\right)$$

where Δ^F is the direct effect of the firearm policy on firearm death rates, Δ^{NF} is the second-order effect of the policy on nonfirearm death rates, and Δ^T is the total effect of the policy on homicide or suicide. The interpretation of m is

1. $m > 1$: spillover or contagion (e.g., policies that increase firearm suicides generate spillovers that increase nonfirearm suicides, or policies that decrease firearm suicides have spillovers that reduce nonfirearm suicides);
2. $m = 1$: the effect of the firearm policy is exclusive to firearm outcomes with no effects on nonfirearm deaths,
3. $0 < m < 1$: partial substitution (e.g., policies that decrease firearm homicides are partially offset by increases in nonfirearm homicides),
4. $m = 0$: complete lethal means substitution (e.g., preventing firearms suicides is fully offset by increases in nonfirearm suicides), and
5. $m < 0$: more than perfect substitution (e.g., preventing firearm suicides results in more total suicides).

Our conceptualization of m was neutral regarding the direction of the policy's first-order effect on firearm deaths. Specifically, it assumed that the extent to which policy effects on firearm deaths are offset or compounded by effects on nonfirearm deaths is equivalent for policies that increase firearm deaths and policies that decrease firearm deaths. This allowed m to be estimated from policies that were found to increase or to decrease firearm deaths.

As no study provided a direct estimate of m, we had to convert estimated causal effects from each study into estimates that reflected this construct. For linear models, effect estimates were already expressed as differences in rates, the required units to compute the Δ quantities. However, for studies that expressed effects as incidence rate ratios (IRRs), we needed to convert the provided effect sizes and SEs. This required knowing the ratio of firearm to nonfirearm deaths (suicides or homicides) for each study (proof in Appendix D, available as a supplement to the online version of this article at http://www.ajph.org), information not always provided in the articles themselves. To apply this information consistently across studies, we used data on firearm and nonfirearm homicides and suicides from the National Center for Health Statistics[12] for the specific years of each study, using these base rates to convert IRRs and confidence intervals (CIs) into differences in rates.

The process for computing the distributional characteristics of m using information extracted from the studies was more complex than for many other statistics. Specifically, m was a ratio of 2 statistics, each of which had an approximate normal distribution. While the probability density function of the resulting ratio was fully defined, the mean and variance of that distribution may be undefined because the dispersion of the distribution of m can be infinite if the denominator (Δ^F) contained substantial density near zero; thus, a study that estimated a zero effect of a policy on firearm deaths provided little information about m. This corresponds to the observation that when a policy had zero causal effect on firearm deaths, any estimated causal effect on nonfirearm deaths cannot be interpreted as substitution or contagion.

Given the Δ^F and Δ^{NF} distributions from each study, we derived an estimate of m for each effect through a statistical simulation. We dropped estimates of m before meta-analysis when they had extremely large variance ($SE > 2$; i.e., a 95% CI width for m exceeding 8; Appendix D). These would have received effectively zero weight in the meta-analysis, had they been included. We meta-analyzed the individual estimates of m by using the *meta* package in R.[13] While we had neither a priori nor substantial empirical evidence of excessive dispersion across studies, we present both fixed- and random-effect estimators of m because the CIs of the fixed-effect estimators may be too narrow if m varied across different types of firearm regulations. We weighted fixed-effect meta-analytic estimates based on the inverse variance of the individual estimates, assuming all individual estimates were drawn from a single common distribution. Random-effect estimates allowed for variance in the true value of m across studies, assessed by using Cochran's Q, allowing greater uncertainty in the overall meta-analytic estimate as that variance increased. To provide an upper bound on the statistical uncertainty in the overall estimate, we used the Sidik and Jonkman[14] estimate of the random-effects dispersion parameter because it yielded larger CIs than other standard options.

Sensitivity Analyses

We used prespecified sensitivity analyses to address concerns about either the underlying studies or our methods of extracting estimates from them. One serious concern about these studies was the extent to which the SEs for the effects presented in the original studies were correct. Studies have shown that failure to use a cluster correction to address the violation of the independent error assumption in these data results in SEs underestimated by a factor of 2.5 averaged across a range of model types and policy distributions.[15–19] About 16 out of 60 estimates we extracted from the literature did not use the required cluster correction. To avoid giving these methodologically weaker studies the largest weights, we performed an SE correction by multiplying the unclustered SEs by 2.5. However, we also present a sensitivity analysis using the original SEs.

In addition, to assess the dependence of our results on assumptions required to convert IRRs into linear effect estimates, we conducted sensitivity tests in which we varied the assumed ratio of nonfirearm to firearm deaths used in this conversion. Specifically, we re-estimated the meta-analytic m after (1) increasing the ratio by 20% and (2) decreasing the ratio by 20%.

Finally, 2 exploratory analyses considered whether m may vary across different populations or forms of violence. First, because adolescents may show different patterns of substitution or contagion than the broader population,[20] we included a sensitivity test in which we excluded sources that exclusively used child or adolescent populations. Second, while our primary analysis excluded studies of homicide subtypes, we conducted a secondary meta-analysis of m for intimate partner homicide (Appendix F, available as a supplement to the online version of this article at http://www.ajph.org).

Meta-analytic estimates of m were minimally affected by the type of reporting bias that presents concerns in most meta-analyses (i.e., failure to publish small and nonsignificant effects on the primary outcome). This is because one cannot estimate the effect of reducing firearm homicides on total homicides if there is no effect on firearm deaths. Because the variance of m approaches infinity when the estimated effect on firearm deaths is zero, the inverse variance weighting of meta-analyses effectively drops such estimates from the meta-analytic estimate. Unlike most meta-analyses, the normal reporting biases of the field result in the omission of precisely those studies whose results would not inform our estimate (Appendix A provides further discussion).

RESULTS

We screened titles and abstracts of 21 700 studies. From those, 357 merited full-text review, from which 16 provided point estimates and inferential statistics that we could use to generate estimates of m (Figure 1). Appendix A presents details on search strategy, inclusion and exclusion decisions, and risk of reporting bias.

In total, the 16 included studies provided 60 usable estimates to inform m (Table 1). For homicide (15 studies; 37 estimates), the most commonly studied policies were concealed carry laws (27% of estimates) and background check requirements (24%), followed by age prohibitions (14%) and waiting periods (8%). For suicide (8 studies; 23 estimates), the most commonly evaluated policies were background check requirements (30%) and age prohibitions (30%), followed by waiting period (13%) and child access prevention laws (9%). All studies evaluated the US context and used a quasi-experimental differences-in-differences type design, controlling for year fixed-effects and geographic fixed or random effects. Based on our quality assessment, all but 8 estimates had at least 1 methodological concern; none had more than 2 flags for methodological issues (Appendix C, Table C1).

Only 3 studies[27,28,35] provided linear effect estimates. For the remaining studies, we transformed the estimated effect sizes as discussed previously. The estimated firearm and nonfirearm effects, their SEs, and the simulated distribution of m are presented for each study and law combination in Appendix D, Tables D1 and D2.

Our meta-analysis of the mortality multiplier for suicide suggests that the available literature does not yet support such an estimate ($m = 1.41$; 95% CI = 0.97, 1.84; Appendix E, Figure E1). A single source, a child access prevention law estimate,[33] received 90% of the weight in the meta-analytic

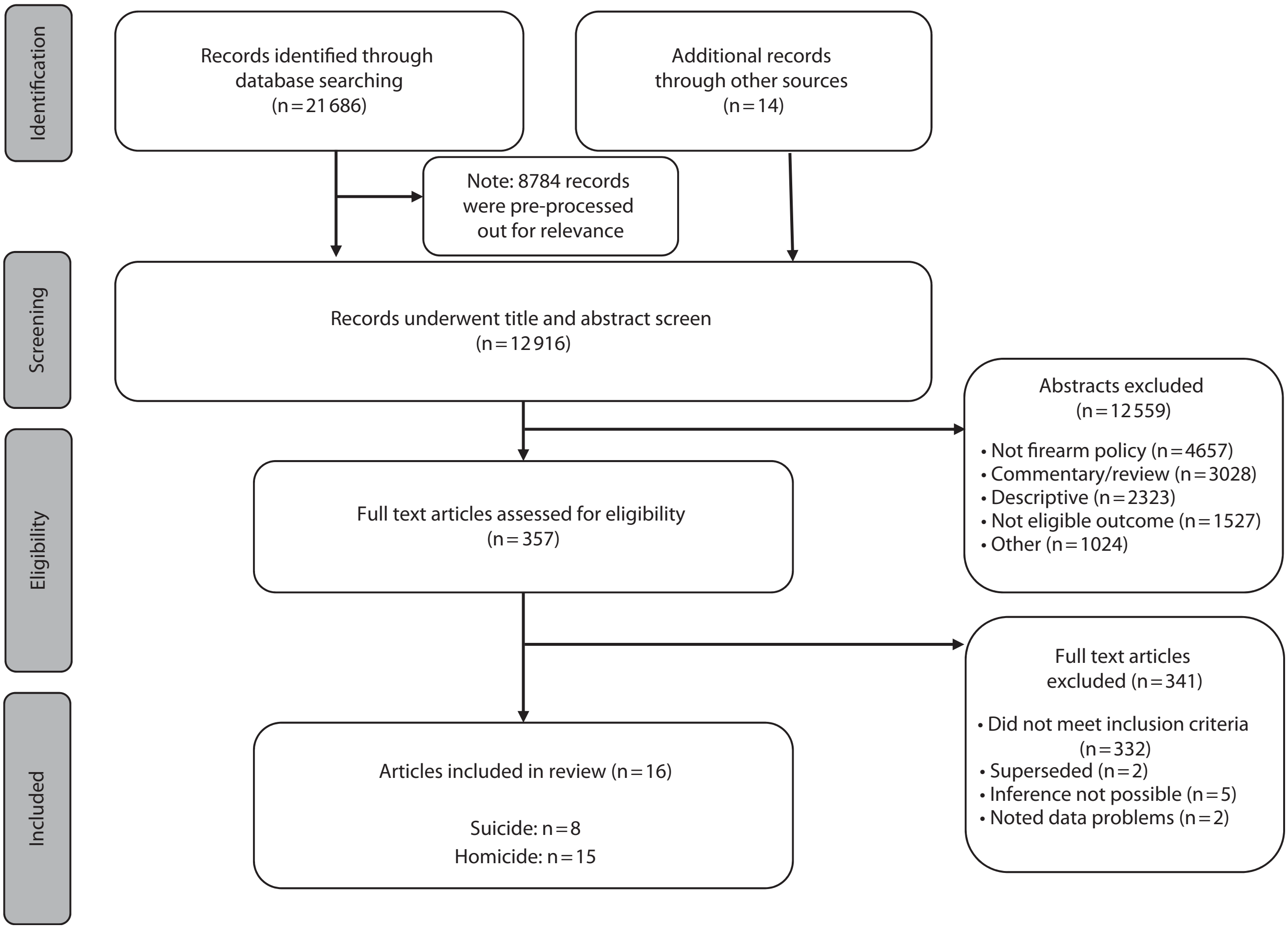


FIGURE 1—Study Flow Diagram of Literature Search and Selection of Studies: United States, January 1, 1995–October 31, 2018

combination. Only 2 other sources[22,27] provided marginally useful information (i.e., CI width < 8). Relying on this single study is particularly problematic because this effect was based only on the subpopulation aged 18 to 20 years, which was not the population hypothesized to show the full effect of the child access prevention laws; because a comparable effect was not found in the adolescent population where the effect was hypothesized, the authors themselves discounted this estimate as spurious or noncausal. Thus, our meta-analytic estimate was effectively based on a single source, estimated in a small subpopulation that was not where the authors hypothesized a causal effect.

By contrast, our meta-analysis of the mortality multiplier for homicide provides a more informative estimate, with 14 sources contributing useful information (Figure 2). The fixed-effect meta-analytic estimate was 0.99 (95% CI = 0.76, 1.22). The random-effect estimate was functionally identical, finding no evidence of significant dispersion in the true value of *m* across policies and studies (Q = 1.57; df = 13; *P* = .99). Notably, half of the contributing estimates exhibited at least 1 methodological quality concern, primarily related to potential model overfit or failure to adjust for serial correlation; even with our applied SE adjustment factor, the pooled estimate may understate uncertainty around *m*.

The meta-analytic estimate of *m* and its CI for homicide were highly consistent across sensitivity tests designed to assess robustness of the estimate to our methodological choices (Table 2). The alternative specification with the largest effect on the estimate was removing the SE adjustment factor. This change had a small effect on the estimate itself, but greatly reduced the CI for the overall fixed-effect estimate.

Our *m* estimate for homicide suggests that a gun law that prevents 100 firearm homicides is expected to prevent 99 total homicides after accounting for possible means substitution, violence contagion, and other second-order effects. There remains substantial uncertainty around this estimate, but the meta-analysis rules out dramatic second-order effects.

DISCUSSION

Although a broad literature has evaluated how various gun laws affect firearm-related homicide and suicide,[11,37,38] none directly estimate the extent to which effects on firearm-related mortality are offset or

TABLE 1—Characteristics of Included Studies That Contained Information to Construct the Mortality Multipliers: United States, January 1, 1995–October 31, 2018

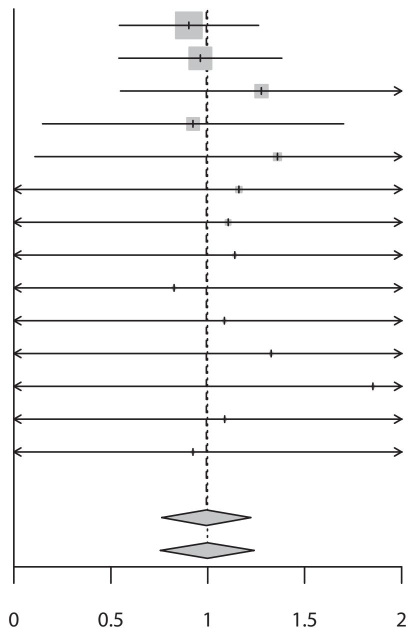
Study	Model	Unit of Analysis	Period	Population	Homicide Outcomes	Suicide Outcomes	Policies Evaluated
Donohue et al.[21]	Log-linear	State	1979–2014	All	T, F, NF	. . .	1. Shall-issue law
Edwards et al.[22]	Log-linear	State	1990–2013	All	T, F, NF	T, F, NF	1. Waiting period
Crifasi et al.[23]	Poisson	County	1984–2015	Urban[a]	F, NF	. . .	1. Permit to purchase 2. CBC and no permit to purchase 3. Shall-issue law 4. VM prohibition 5. Stand-your-ground law
Hamill et al.[24]	Log-linear	State	1986–2015	All	T, F	. . .	1. Shall-issue law
Donohue[25]	NB	State	2000–2014	All	T, F, NF	. . .	1. Shall-issue law
Siegel et al.[26]	NB	State	1991–2015	All	T, F, NF	. . .	1. Shall-issue law
Luca et al.[27]	Linear	State	1977–2014	Ages ≥ 21 y	T, F, NF	T, F, NF	1. Waiting period 2. Dealer BC
Webster et al.[28]	Linear	State	1999–2010	All	T, F, NF	. . .	1. Permit-to-purchase repeal 2. Stand-your-ground law 3. Juvenile offense restrictions 4. "Saturday night special" ban 5. Shall-issue law
La Valle[29]	Log-linear	City	1980–2010	All	T, F	. . .	1. Dealer BC[b] 2. Shall-issue or may-issue law
La Valle and Glover[30]	Log-linear	City	1980–2006	All	T, F	. . .	1. Shall-issue law 2. May-issue law
Sen and Panjamapirom[31]	NB	State	1996–2005	All	T, F, NF	T, F, NF	1. BC for restraining order 2. BC for mental illness 3. BC for fugitive status 4. BC for misdemeanor 5. BC for other conditions
Rosengart et al.[32]	Poisson	State	1979–1998	All	T, F, NF	T, F, NF	1. Shall-issue law 2. State minimum age for purchase 3. State minimum age for possession 4. Bulk purchase limit 5. "Saturday night special" ban 6. CAP law
Webster et al.[33]	NB	State	1976–2001	Ages 18–20 y	. . .	T, F, NF	1. CAP law 2. State minimum age for purchase 3. State minimum age for possession 4. Permit-to-purchase
Marvell[34]	Log-linear	State	1979–1998	All (homicide); ages 15–19 y (suicide)	F, NF	F, NF	1. State minimum age possession (pre-1994) 2. State minimum age possession (in 1994) 3. Federal minimum age for possession
Ludwig and Cook[35]	Linear	State	1990–1997	Ages ≥ 21 y	T, F, NF	T, F, NF	1. Dealer BC[b] 2. Waiting period[b]
Cummings et al.[36]	NB	State	1979–1994	Ages 0–14 y	F, NF	F, NF	1. CAP law

Note. BC = background check; CAP = child access prevention; CBC = comprehensive background check; F = firearm-related; NB = negative binomial; NF = non–firearm-related; RO = restraining order; T = total; VM = violent misdemeanor. All studies evaluated the US context and used quasi-experimental designs that evaluated pre–post policy data and included a control group without the policy or policies of interest. Log-linear models have a linear function but log-transform the outcome variable.

[a]Sample restricted to counties designated as large central or large fringe metro, with populations greater than 200 000 across the study period.

[b]These studies estimated policy effects using the passage of the federal Brady Act as the source of identifying variation. While the Brady Act requirements to implement background checks and a 5-day waiting period applied to all states, many states were exempted because they already had state legislation requiring a background check of individuals who purchased handguns from federal firearms licensees; these states with pre-existing background check requirements effectively serve as a control group for the policy effect estimation.

Source	Mortality Multiplier (95% CI)	Weight (fixed)	Weight (random)
Crifasi et al.[23] (CBC only)	0.90 (0.54, 1.26)	41.0%	39.1%
Crifasi et al.[23] (BC misdemeanor)	0.96 (0.54, 1.38)	29.9%	29.6%
La Valle and Glover[30]	1.28 (0.55, 2.00)	10.0%	10.6%
Webster et al.[28] (PTP repeal)	0.92 (0.15, 1.70)	8.8%	9.4%
Marvell[34] (possess age fed)	1.36 (0.11, 2.61)	3.4%	3.7%
Luca et al.[27] (waiting)	1.16 (−0.46, 2.78)	2.0%	2.2%
La Valle and Glover[30] (RTC)	1.11 (−0.72, 2.93)	1.6%	1.7%
Marvell[34] (possess age 1994)	1.14 (−1.82, 4.10)	0.6%	0.7%
Crifasi et al.[23] (PTP)	0.83 (−2.25, 3.90)	0.6%	0.6%
Crifasi et al.[23] (SYG)	1.09 (−2.07, 4.24)	0.5%	0.6%
La Valle[29] (RTC)	1.33 (−2.07, 4.73)	0.5%	0.5%
Webster et al.[28] (junk gun)	1.85 (−1.61, 5.31)	0.4%	0.5%
Siegel et al.[26] (RTC)	1.09 (−2.62, 4.79)	0.4%	0.4%
Rosengart et al.[32] (RTC)	0.92 (−2.97, 4.81)	0.3%	0.4%
Fixed effect model	**0.99 (0.76, 1.22)**	**100.0%**	**...**
Random effects model	**1.00 (0.76, 1.24)**	**...**	**100.0%**



Note. BC = background check for misdemeanor offenses; CBC only = comprehensive background checks and no permit-to-purchase policy; CI = confidence interval; junk gun = ban on low-quality handguns; possess age fed = federal minimum age for possession; possess age 1994 = state minimum age for possession, enacted in 1994; PTP = permit-to-purchase; RTC = right-to-carry law; SYG = stand-your-ground law; waiting = waiting-period law. The sizes of the boxes in the figure represent the weight given each estimate in the meta-analysis.

FIGURE 2—Forest Plot of the Mortality Multiplier for Homicide: United States, January 1, 1995–October 31, 2018

compounded by second-order effects on non–firearm-related mortality. While many studies recognize the potential for these second-order effects, they address this by estimating policy effects on total homicides or suicides and comparing the sign and significance of this effect to that for firearm-specific fatalities.[2] However, one cannot accurately assess the size or direction of these second-order effects by comparing statistical significance between the effect on firearm homicide and total homicide; a difference in significance should not be interpreted as a significant difference. This is an important gap in the current evidence base.

The primary aim of this meta-analysis was to use information from existing studies to examine whether policies that affect firearm deaths have second-order effects on non-firearm deaths that undermine or enhance their public health impact. Our results suggest that preventing 1 homicide death by firearms has a net effect of preventing 1 homicide in total; while this estimate is relatively imprecise, we can effectively rule out the possibility of substantial homicide substitution or contagion (i.e., > 30%) at the population level. This does not necessarily rule out substantial lethal means substitution at the individual level. It may be that individual-level substitution (e.g., individuals switch from firearm to nonfirearm assaults) does not fully offset reduced firearm mortality given the higher case–fatality rate for firearms.[39] It could also be that there is both individual-level lethal means substitution (undermining benefits of gun laws) and violence contagion (enhancing effects of gun laws), resulting in minimal net change for nonfirearm homicides.

We cannot provide an informative meta-analytic estimate about the mortality multiplier for suicide because the studies that included the necessary information generally found only weak law effects on firearm suicide. This is somewhat surprising because several published studies have identified significant effects of firearm regulations on

TABLE 2—Sensitivity Tests of the Mortality Multiplier Estimate for Homicide

Meta-analysis Specification	Effect Estimate (95% CI) Fixed	Effect Estimate (95% CI) Random	No. of Used Sources
Primary estimate	0.99 (0.76, 1.22)	1.00 (0.76, 1.24)	14
Sensitivity analyses			
Omit SE correction for clustering	0.97 (0.90, 1.04)	1.03 (0.89, 1.18)	18
Convert IRRs using a high ratio	0.99 (0.71, 1.26)	0.99 (0.71, 1.28)	12
Convert IRRs using a low ratio	0.99 (0.80, 1.18)	0.99 (0.80, 1.19)	16
Exclude studies on children	0.99 (0.76, 1.22)	1.00 (0.76, 1.24)	14

Note. CI = confidence interval; IRR = incidence rate ratio.

firearm suicide.[40–42] However, those studies had to be excluded from the analysis because they (1) did not present effect estimates for both firearm suicides and total or nonfirearm suicide or (2) used synthetic control methods and a single implementing state, in which case there is no accurate way to get standard errors for the effect estimate from the paper.

Limitations

As in any meta-analysis, our findings relied on the validity of studies that informed our estimates. Specifically, we required that study estimates represent the causal effect of a given policy on firearm deaths and that, conditional on covariates, the causal effect on nonfirearm mortality occurred only through these effects on firearm mortality. While we restricted our sample to studies that used causal inference designs, all studies were quasi-experimental, and some of these estimates may not reflect true causal effects. Furthermore, the estimates that contributed the most weight to the meta-analytic results had some methodological limitations that might result in our CIs around the meta-analytic *m* being too narrow.

Our definition of the mortality multiplier assumed that it has a constant value across different policies, time periods, and populations. We did not find significant variance in *m* across the policies and studies we included, but it is possible that these assumptions were incorrect and will require additional research as more estimates become available. While our review considered a broad range of gun laws, many were not included; incorporating evidence from other gun violence prevention interventions (e.g., urban blight remediation, community-based outreach), may have yielded better information to construct pooled estimates of the mortality multipliers, particularly if these other policies have much larger effects on firearm-related homicides or suicides.

Our conversion of study effect estimates to mortality multipliers relied on several assumptions. While we provide analyses to test the importance of these assumptions, our study-specific and pooled estimates were necessarily approximations. In addition, the meta-analysis combined studies that used overlapping data sets that were not fully independent; thus, it is unclear whether the independent errors assumption of the meta-analysis was met. A direct estimate of the mortality multiplier from a single study jointly estimating policy effects on firearm and nonfirearm deaths using the most complete data and optimal statistical methods might produce a more accurate estimate of the mortality multiplier than the meta-analytic estimates presented here.

Finally, the number of studies excluded from this review for lack of relevant results presents concerns about potential reporting bias. While a failure to publish studies showing nonsignificant effects on firearm deaths would not bias our estimate of *m*, if there was systematic bias against presenting results showing a specific pattern of effects across firearm homicide and total homicide, this could bias our results, although the direction of this potential bias is unknown.

Conclusions

Policymakers and researchers have sometimes argued that most or all of any firearm violence reduction benefits of gun policies will be counteracted by adverse, unintended consequences of those benefits, such as through lethal means substitution.[2,43] This review estimated the direction and magnitude of these unintended, second-order effects of gun laws. Our estimates rule out the possibility that more than 30% of any reductions in firearm homicides attributable to a gun policy would be counteracted by systematic increases in nonfirearm homicides. However, the current literature does not support a useful estimate of such effects for suicide. AJPH

CONTRIBUTORS

R. Smart, T. L. Schell, and A. R. Morral conceptualized the research. All authors contributed to developing the statistical methodology, conducting the analyses, interpreting the results, and writing the article.

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CONFLICTS OF INTEREST

Authors have no conflicts of interest to disclose.

HUMAN PARTICIPANT PROTECTION

RAND's institutional review board determined that this project does not involve human participants as defined by the regulations at 45 CFR 46.102(f).

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