Effects of Concealed-Carry Laws on Violent Crime

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Summary: There is supportive evidence that shall-issue concealed-carry laws may increase total and firearm homicides. Evidence for the effect of permitless-carry laws on total homicides is inconclusive. Evidence that shall-issue concealed-carry laws may increase violent crime is limited.

An explosion of research into the effects of shall-issue laws on violent crime was triggered in 1997 by the publication of analyses using county-level data from 1977 to 1992. Using these data, Lott and Mustard (1997) concluded that states implementing shall-issue laws saw significant decreases in rates of violent crime, murder, rape, and assault. Their "more guns, less crime" conclusion was immediately controversial and led to a proliferation of studies exploring the robustness of the study's findings to alternate model specifications and to improvements or expansions to the data series. The table below lists studies from this early period of responses to Lott and Mustard (1997), as well as their counter-responses.

Two important reviews of the scientific literature on gun policy effects—one by the National Research Council (NRC), a part of the National Academy of Sciences (NRC, 2004), and

Key Findings

Shall-issue concealed-carry laws may **increase** total and firearm homicides.

Evidence for this relationship is **supportive**.

At least three studies not compromised by serious methodological weaknesses found suggestive or significant effects in the same direction using at least two independent data sets. Read more about how we determined the strength of gun policy analysis research.

Permitless-carry laws have **uncertain** effects on total homicides.

Evidence for this relationship is **inconclusive**.

Studies with comparable methodological rigor identified inconsistent evidence for the policy's effect

on an outcome, or a single study found only uncertain or suggestive effects.

Shall-issue concealed-carry laws may **increase** violent crime.

Evidence for this relationship is **limited**.

At least one study meeting our inclusion criteria and not otherwise compromised by noted methodological weaknesses reported a significant effect of the policy on the outcome, and no studies with equivalent or stronger methods provided contradictory evidence.

one by the Community Preventive Services Task Force, established by the U.S. Department of Health and Human Services (Hahn et al., 2005)—evaluated this early literature and reached nearly identical conclusions. In their review of existing studies examining shall-issue laws, Hahn et al. (2005) found insufficient evidence for determining the effect of such laws on violent crime. NRC (2004) reviewed much of the same literature and reanalyzed data that were common to many of these analyses: a panel data set originally spanning 1977–1992, then expanded through 2000. After reviewing many of the studies listed in the table below, the NRC (2004) panel, with one member dissenting, concluded:

> Some studies find that right-to-carry laws reduce violent crime, others find that the effects are negligible, and still others find that such laws increase violent crime. The committee concludes that it is not possible to reach any scientifically supported conclusion because of (a) the sensitivity of the empirical results to seemingly minor changes in model specification, (b) a lack of robustness of the results to the inclusion of more recent years of data (during which there were many more law changes than in the earlier period), and (c) the statistical imprecision of the results. The evidence to date does not adequately indicate either the sign or the magnitude of a causal link between the passage of right-to-carry laws and crime rates. Furthermore, this uncertainty is not likely to be resolved with the existing data and methods. If further headway is to be made, in the committee's judgment, new analytical approaches and data are needed (p. 7).

Studies Exploring the Effects of Shall-Issue Concealed-Carry Laws on Violent Crime, 1997–2004

Study	Significant Effect Reported (Main Specification)
Lott and Mustard (1997)[a]	Decrease in violent crime, murders, rapes, and assaults
Bartley and Cohen (1998)	Decrease in violent crime robust to alternate model specifications
Black and Nagin (1998)	Increase in assaults

Study	Significant Effect Reported (Main Specification)
Bronars and Lott (1998)	Decrease in murders and rapes, displacement of crime to other jurisdictions
Lott (1998a)[a]	Decrease in violent crime in most states implementing the law
Lott (1998b)[a]	Decrease in violent crime; increase in property crime
Ludwig (1998)	None detected
Ayres and Donohue (1999)[a]	Increase in property crime
Lott and Landes (1999)[a]	Decrease in murders and injuries from multiple-victim public shootings
Lott (2000)[a]	Decrease in all crime categories
Benson and Mast (2001)	Decrease in violent crime, murders, rapes, and robberies
Duggan (2001)	Decrease in assaults
Moody (2001)[a]	Decrease in violent crime
Olson and Maltz (2001)	Decrease in firearm murders
Plassmann and Tideman (2001)	Decrease in murders and rapes; increase in robberies
Lott and Whitley (2003)[a]	Decrease in violent crime, murders, rapes, and robberies
Plassmann and Whitley (2003)[b]	Decrease in rapes and robberies
Rubin and Dezhbakhsh (2003)	Decrease in murders; increase in robberies
Ayres and Donohue (2003a)[a]	Increase in more crime categories than saw a decrease
Ayres and Donohue (2003b)[a]	Increase or no effect in all crime categories
Donohue (2003)[a]	Mixed; effects were sensitive to model specifications and data
Helland and Tabarrok (2004)	Increase in property crime, auto thefts, and larcenies

[a] These studies are treated in this report as being superseded by later studies by the same authors.

[b] This same paper was earlier circulated as Lott, Plassmann, and Whitley (2002).

In addition to the sensitivity of results to minor changes in model specification noted by the NRC report, these early studies suffered from multiple serious problems with data and methodology that lead us to discount their value for informing this synthesis of evidence on the effects of shall-issue laws. These problems include the following:

- Lott and Mustard's 1997 data set used county population values that did not correspond to the crime statistics available for counties, especially those with weak reporting of crime statistics (Maltz and Targonski, 2002). Lott and Whitley (2003) discounted these and other concerns about the quality of county crime rate data, describing these concerns as

typical of the types of measurement error commonly encountered in statistical analyses. Furthermore, they suggested that the findings in Lott (2000) persisted even when analyzing the subset of counties with minimal error in crime statistics. After reviewing this exchange, the NRC panel disagreed with Lott and Whitley that the original effects reported by Lott (2000) survived this test: "The committee concludes that it is at least possible that errors in the [Uniform Crime Reporting] data may account for some of Lott's results" (NRC, 2004, p. 137).

- Many of these studies followed the example of Lott and Mustard (1997) by including arrest rates as a model covariate. This led to these analyses excluding large numbers of counties that had no crimes of a given type and therefore an undefined arrest rate, an approach that differentially excluded locations where the introduction of shall-issue laws could have led only to an increase in crime rates (Ayres and Donohue, 2003a).
- There were errors in the classification of shall-issue states in the Lott and Mustard data set that were only later corrected (Ayres and Donohue, 2003a). There were multiple errors detected in the data sets used by Lott (1998b, 2000) and by Plassmann and Whitley (2003), and Plassmann subsequently acknowledged these errors to the NRC (NRC, 2004, p. 136). Correction of these errors eliminated many of the significant effects reported by Plassmann and Whitley (2003) (Ayres and Donohue, 2003a).
- Nearly all of the studies listed in the table above failed to control for serial correlation in the panel data set; the exceptions were Duggan (2001), Olson and Maltz (2001), Plassmann and Whitley (2003), Ayres and Donohue (2003a, 2003b), and Helland and Tabarrok (2004). This led to gross exaggerations of the statistical significance of study results and greatly elevated the risk of finding statistically significant effects that were in the opposite direction of any true effect (Schell, Griffin, and Morral, 2018; Moody and Marvell, 2018b; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).
- Most of the studies used the large number of covariates first included in the Lott and Mustard (1997) analyses, which had a ratio of estimated parameters to observations of between one to eight and one to 14 across analyses. When the proportion of estimated parameters is this high, there is considerable risk that the statistical models are overfit, and the law effects that they estimate thus may not be generalizable. Among few exceptions, the models of Ludwig (1998) and Moody (2001) did not suffer from this problem.

Finally, we regard a majority of these early studies as having been superseded by later work by the same authors that improved on their earlier contributions to this literature. As a result, we focus on their later efforts to evaluate the effect of shall-issue laws.

We first describe studies published since 2004 that aimed to estimate the effects of concealed-carry laws on violent crime using county-level data. We then turn to studies that focused on state-level data, then studies that employed city-level data. We conclude by discussing results

from a set of studies in which the objective was not to identify the effects of shall-issue laws but that nonetheless present estimates that may be considered part of the evidence base for how concealed-carry policies influence violent crime outcomes (e.g., some studies of the effects of abortion rates on violent crime include shall-issue laws as a covariate in their models).

County-Level Studies

Many important shortcomings of county-level crime data identified through the early studies of shall-issue laws (see the table above) resulted from the fact that large numbers of county police agencies do not report crime statistics to the Federal Bureau of Investigation (FBI). Moreover, the way that county crime statistics address these missing data changed abruptly in the early 1990s, making data from the earlier part of the series not comparable with later data, according to the National Archive of Criminal Justice Data (undated). Nevertheless, several analyses have continued to use county-level crime data to evaluate law effects, or they have used homicide data from the Centers for Disease Control and Prevention (CDC)'s National Vital Statistics System, which has less of a problem with missing data (Loftin, McDowall, and Fetzer, 2008).[1]

Roberts (2009) used the FBI's Supplementary Homicide Reports to analyze the effect of shall-issue laws on intimate partner homicide rates using monthly county-level data spanning 1985–2004. The author found that (the more restrictive) may-issue laws significantly increased intimate partner total homicides by 71 percent compared with shall-issue laws, but may-issue (compared with shall-issue) laws had an uncertain effect on intimate partner firearm homicides. The author also found uncertain effects of concealed-carry bans when compared with shall-issue laws on either overall or firearm-related intimate partner homicides. However, neither analysis clustered standard errors at the state level, so serial correlation that was unaccounted for in the panel data likely resulted in underestimated standard errors and correspondingly misleading tests of statistical significance.

Aneja, Donohue, and Zhang (2014) analyzed the county-level data set used in NRC (2004) that was extended through 2006 and state-level data through 2010. The authors corrected the NRC analyses for several errors that they identified, including data-coding errors related to the timing of shall-issue legislation, an endogenous control variable (arrest rate), and a failure to cluster standard errors at the state level. The authors argued that the decision in NRC (2004) not to cluster the standard errors of the county-level analyses at the state level was incorrect and showed that CIs were badly misestimated when clustering was not accounted for. In their preferred county-level specification including state trend effects, they found no statistically significant effects of shall-issue laws on either the level or trend of any of seven crime rates, and they found only one suggestive effect across the 14 effects they tested.

Moody et al. (2014), responding to an earlier version of the Aneja, Donohue, and Zhang (2014) paper, reestimated their models after adding many more demographic control variables, robbery and assault rates, and a lagged outcome as a predictor meant to capture unmeasured state differences associated with crime rates. Moody et al. (2014) offered statistical tests suggesting that the model with added covariates predicted the data significantly better, which the authors interpreted as evidence that estimates in Aneja, Donohue, and Zhang (2014) suffered from omitted-variable biases. The revised hybrid model results in Moody et al. (2014) suggested that shall-issue laws significantly reduced the trends in rape and murder rates. They found no significant association between shall-issue laws and either assault or robbery rates. The fact that their model predicted a given outcome better than the Aneja, Donohue, and Zhang (2014) model is not sufficient to demonstrate the claim that the latter's model suffered from omitted-variable bias or that the model preferred by Moody et al. (2014) offered a less biased estimate. An overfit model can predict the data exceptionally well while producing biased and unreliable coefficient estimates.

Using county-level panel data spanning 1979–2000, Durlauf, Navarro, and Rivers (2016) examined the sensitivity of analyses that estimate the relationship between shall-issue laws and violent crime. They reported that use of population weights may lead to inefficient estimates and upward biases in estimates of the effect of shall-issue laws on crime. In addition, they found that hybrid or spline models are preferred to dummy models and that models that allow for heterogeneity in the effect of laws (including effects that vary with region, rates of gun ownership, and the level of urbanization in an area) outperform models that do not allow for variation in effects. For the spline model specifications that the authors assessed to perform best for the outcome of violent crime, they estimated that shall-issue laws increase violent crime in the first year after law passage and that violent crime continues to increase in subsequent years. The authors concluded that, overall, there was substantial variation in the estimated effects for each model across the model space analyzed and, thus, there was little evidence that shall-issue laws generate either an increase or a decrease in crime on average.

Crifasi et al. (2018b) evaluated the effects of shall-issue laws and four other gun laws on homicides in large, urban counties between 1984 and 2015. Using a Poisson model that included year fixed effects, random effects for counties, and county-level demographic and economic covariates, the authors found that shall-issue laws were associated with a significant increase in firearm homicide rates. Specifically, after implementing these laws, counties would be expected to see 1.07 times more firearm homicides (95-percent CI = 1.05, 1.09). The authors also included a comparison outcome—nonfirearm homicides—based on the theory that, if the effect of shall-issue laws is correctly estimated, it should be found only for firearm homicides, not nonfirearm homicides. However, their estimate for nonfirearm homicides was very similar to the estimate for firearm homicides (IRR = 1.04; 95-percent CI = 1.01, 1.07), which raises questions about the model or the authors' theory that nonfirearm homicides should be

unaffected by the law. The paper did not describe any corrections for serial correlation in the data used, without which incorrect claims of statistical significance would be expected to proliferate (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

State-Level Studies

Hepburn et al. (2004) evaluated the effects of shall-issue laws on homicide rates using data from 1979 to 1998 in a study that came out too late to be reviewed in either the NRC (2004) or the Hahn et al. (2005) reviews of firearm research. Using a negative binomial model with two-way fixed effects and controlling for demographic and economic variables, including a proxy for gun ownership, the authors found uncertain effects for shall-issue laws on state homicide rates. Estimated effects remained uncertain in subgroup analyses of adults aged 25 or older and of white men aged 35 or older (see the first figure below).

Rosengart et al. (2005) examined the effect of several state gun laws, including shall-issue laws, on firearm homicides and total homicides using state-level data. One limitation was that the data covered only 1979–1998, and other studies have shown the sensitivity of results to shorter periods, partly because shorter periods include observation of fewer states that have adopted shall-issue laws. The policy variable was specified as a dummy variable (indicating that a shall-issue law was or was not in place). The authors found suggestive effects that shall-issue laws increased firearm and total homicide rates. French and Heagerty (2008) tested the sensitivity of these results and similarly concluded that shall-issue laws had a suggestive effect consistent with the laws increasing firearm-related homicide rates, although estimates varied across specifications. However, the Rosengart et al. (2005) paper and presumably the French and Heagerty (2008) paper also had unfavorable ratios of model covariates to observations (less than one to eight), suggesting that the model may have been overfit, and thus its estimates and their CIs may be unreliable.

Martin and Legault (2005) demonstrated that Lott (2000) used incorrect state crime rate estimates that differed substantially from official FBI state estimates. They replicated Lott (2000)'s model despite misgivings about its specification to demonstrate that the effects Lott reported were sensitive to this measurement error. In their replication exercise using state-level crime data from the FBI's Uniform Crime Reports spanning 1977–1992, Martin and Legault (2005)'s estimates showed that shall-issue laws significantly reduced total violent crime and, specifically, aggravated assault. The authors found only suggestive effects that the laws reduced rates of robbery and murder, as well as uncertain effects on rates of rape (see the second figure below). However, as with Lott (2000), the authors did not statistically adjust for serial correlation in the panel data, and the model's ratio of estimated parameters to observations was less than one to ten, meaning the model may have been overfit, and thus its parameter estimates and their CIs may be unreliable.

Grambsch (2008) conducted a state-level analysis of (total) murder rates (relative to the U.S. murder rate) from 1976 to 2001 using the 25 states that passed shall-issue laws between 1981 and 1996. She found a selection effect among states adopting shall-issue laws—namely, that states that passed shall-issue laws in this period experienced an increasing trend in murder rates prior to adoption relative to other states. Her estimates showed that, after controlling for regression to the mean, there was either an uncertain effect or a significant positive effect of shall-issue laws on relative murder rates (i.e., shall-issue laws increased murder rates) depending on the model used. However, the model finding significant effects (the state fixed-effects model) had fewer than ten observations per estimated parameter, meaning the model may have been overfit, which can lead to unreliable estimates and standard errors. Furthermore, neither model included adjustments for serial correlation in the panel data.

Using a panel of state data, Lott (2010) provided an update of his earlier analyses examining the effect of shall-issue laws on violent crime. His preferred specification included a set of dummy variables that indicated different time intervals before and after shall-issue legislation was in effect for states that passed such legislation. Many of Lott's modeling results were presented as figures and did not indicate statistical significance. Detailed results were provided only for an analysis of homicide rates. These included information on the statistical significance of each coefficient in the model but not for a test comparing post-implementation time intervals with pre-implementation time intervals. Lott interpreted the pattern of effects as demonstrating that homicides declined significantly after implementation of shall-issue laws, but he did not provide test statistics or sufficient description to clarify what specific effect was observed. The author also included coefficients and their statistical significance from dummy and spline models similar to those from his earlier work, but he did not include standard errors or test statistics. All of the preferred models appear to have had a ratio of estimated parameters to observations that was less than one to ten, meaning the model may have been overfit, and thus the reported estimates and their CIs may be unreliable. Similarly, it does not appear that Lott used any adjustments for serial correlation in his panel data, so some of the effects reported as statistically significant might not be after correcting these analyses (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004).

DeSimone, Markowitz, and Xu (2013) evaluated the effects of child-access prevention laws on nonfatal injuries using data from 1988 to 2003, but they included sensitivity analyses that controlled for shall-issue laws. Using fixed-effects Poisson regression models, they found that shall-issue laws were significantly associated with firearm assault injuries for children under age 18, as well as for adults. Specifically, their estimate suggests that, after a state implemented a shall-issue law, assault injury rates were more than double what would have been expected without the law (see the second figure below), which would be extraordinary if true. However, the estimated effects of shall-issue laws in this study were based primarily on implementation in one state that changed its law during the study time frame (Arizona); thus, the study offers

little evidence that the observed effects are due to the change in the law rather than to other factors affecting the state's assault rate that occurred around the same time the law was changed.

Webster, Crifasi, and Vernick (2014) analyzed state-level data from 1999 to 2010, using generalized least-squares regression models to estimate the effect of shall-issue laws on age-adjusted homicide rates. They found suggestive effects indicating an association between the implementation of shall-issue laws and a 10-percent increase in rates of nonfirearm homicide, a 6-percent increase in rates of total homicide, and an 11-percent increase in rates of murder and nonnegligent manslaughter.[2] However, their estimates showed an uncertain association between shall-issue laws and firearm homicide rates. The statistical model used to arrive at these results used a large number of estimated parameters relative to observations (a ratio of about one to eight), meaning the model may have been overfit, and thus its estimates and their apparent statistical significance could provide little generalizable information about the true causal effects of shall-issue laws.

Gius (2014) examined the effect of shall-issue laws on gun-related murder rates using state-level data from 1980 to 2009. He found that states with may-issue or more-restrictive policies had higher gun-related murder rates than shall-issue states. Relative to states with shall-issue laws, states with more-restrictive firearm-carry policies had rates of firearm homicide that were 11 percent higher (see the second figure below). However, this model did not statistically adjust for the known serial correlation in these panel data, which has been shown to result in misleadingly small standard errors (Schell, Griffin, and Morral, 2018; Aneja, Donohue, and Zhang, 2014; Helland and Tabarrok, 2004). For this reason, the apparently significant effect observed in this study could be invalid.

Using their preferred specification with state-level data from 1979 to 2010 and a dummy, spline, or hybrid specification of shall-issue laws without state trends, Aneja, Donohue, and Zhang (2014) found suggestive evidence that shall-issue laws increase assaults by 8 percent (see the first figure below). In the dummy specification, shall-issue laws significantly increased rates of rape by 12 percent, although estimates of this effect from the spline model were uncertain. The authors also found suggestive evidence that shall-issue laws increased rates of robbery, although estimates again became uncertain in other specifications. Effects of shall-issue laws on murder rates were uncertain. The authors tested the sensitivity of their results to less parsimonious (including the Lott and Mustard [1997] specification) and more-parsimonious demographic specifications; the inclusion of state-specific time trends; the inclusion or exclusion of years that were likely to be influenced by the crack cocaine epidemic, which affected crime rates; and the specification of the policy variable (dummy, spline, hybrid). The authors noted that their results, which showed that the significance and sign of estimated effects varied substantially depending on the specification employed, underscored the sensitivity of gun-crime modeling estimates to modeling decisions.

Moody et al. (2014) and Moody and Marvell (2018a) critiqued several modeling decisions of the Aneja, Donohue, and Zhang (2014) paper, as well as an earlier version of that study (Aneja, Donohue, and Zhang, 2011). Foremost, the studies critiqued the decision to treat models without state-specific trends as the preferred ones.[3] Thus, Moody et al. (2014) reestimated the hybrid models in Aneja, Donohue, and Zhang (2014), incorporating state-specific trends and additional covariates into an analysis of state data. In doing so, the authors found, as they had with their county-level analyses, that their specification improved model fit over that of Aneja, Donohue, and Zhang (2014). They also found that the individual states' trends were jointly significant, which they took as evidence supporting the need for their inclusion in the models of shall-issue law effects. Using hybrid models that included state-specific linear trends, Moody et al. (2014) found that shall-issue laws significantly increased assault rate trends and increased robbery rate levels, but the laws also significantly reduced murder rate trends. In an updated analysis that favored using a series of leading and lagging indicators of shall-issue laws over the hybrid model specification, Moody and Marvell (2018a) found largely uncertain effects of shall-issue laws on violent crime outcomes. As noted earlier, neither study demonstrated that its model estimates were less biased than those in Aneja, Donohue, and Zhang (2014) or that the Aneja, Donohue, and Zhang (2014) model suffered from omitted-variable biases. Furthermore, the state-level analyses of Moody et al. (2014) used a statistical model with a large number of estimated parameters relative to observations (close to one to five), meaning the model may have been overfit, and thus the estimates and inferential statistics may provide little generalizable information about the true causal effects of shall-issue laws.

In a series of analyses by John Donohue and colleagues, Donohue, Aneja, and Weber (2019) provided estimates of the effects of shall-issue laws; the study used updated data covering 1977–2014, during which 33 states implemented such laws. The authors' two-way fixed-effects model—controlling for demographic, economic, and law enforcement factors—indicated uncertain effects on the logged murder and firearm murder rates but significant increases in rates of violent crime and property crime generally.

Donohue, Aneja, and Weber (2019) also described an assessment of the effects of shall-issue laws that relies on constructing synthetic controls for each state that implemented a shall-issue law. *Synthetic controls* are weighted combinations of states that never implemented the law or that implemented it more than ten years after the treated state such that, in the period before a state's passage of the law, the temporal pattern of crime in the synthetic control closely matched that in the state. Repeating this procedure for each of 33 states with shall-issue laws, the authors concluded that violent crime increased over a ten-year period in 23 of 31 states with at least ten years of post-implementation data. In aggregate, the authors estimated that, five years after law passage, states with shall-issue laws had violent crime rates that were 7 percent higher than expected, which rose to 14 percent after ten years. The authors calculated significance levels for these estimates using a permutation test designed to

estimate the distribution of treatment effects under the assumption that laws have no real effect. They concluded that, after the seventh year post-implementation, states with shall-issue laws had significantly elevated rates of violent crime. Synthetic control methods are relatively new, and especially when controls are made up of just a few states, as they were in this case, their usefulness for identifying causal effects may be compromised (Schell, Smart, and Morral, 2022).

Still, the attempt to pool estimates across multiple states' synthetic control estimates offers an improvement on prior work that used synthetic control methods to produce state-specific estimates of the effects of transitioning from *no-issue* (i.e., no one is permitted to carry concealed firearms) to shall-issue (Gius, 2019b). This earlier study found mixed results across the eight states of interest, with evidence of significant increases in both total and gun-related murders in only one state (New Mexico), but the inferential methods used by this study (*t*-test for post-period) are unlikely to accurately characterize estimate uncertainty. Although a sensitivity analysis using two-way fixed-effects regression found that, on average, transitioning from prohibiting concealed carry to a shall-issue regime was associated with significantly higher rates of gun-related murder and suggestive increases in overall murder rates, it is difficult to interpret these results in light of the heterogeneous control group. For both sets of analyses, the control group was composed of states that did not switch from no-issue to shall-issue laws over the period; however, some of these states may have switched from may-issue to shall-issue or shall-issue to permitless-carry laws. Another study, which attempted to construct a cleaner control group by examining a shorter time frame and estimating effects for both the transition from no issue to shall issue and from may issue to shall issue, found a suggestive effect that the transition from no-issue to shall-issue laws caused a reduction in robbery rates but uncertain effects for rates of all other violent crimes (Barati, 2016). However, this model had an unfavorable ratio of estimated parameters to observations (about one to six), meaning the model may have been overfit, and its estimates and CIs may thus be unreliable.

Luca, Malhotra, and Poliquin (2017) used data from 1977 to 2014 to evaluate the effects of various firearm laws on homicide rates among adults aged 21 or older. Although the authors' focus was on background check and waiting-period laws, they included model specifications that additionally controlled for concealed-carry and permitting laws. Their analysis used log-linear models adjusting for national trends, state fixed effects, and a limited set of state-level time-varying sociodemographic factors; they found that shall-issue and may-issue laws had uncertain effects on total and firearm homicide rates relative to no-issue regimes. Employing similar models but using data from the FBI's Uniform Crime Reports over a shorter time frame (1986 to 2015), Hamill et al. (2019) similarly found uncertain effects of adopting a shall-issue or permitless-carry regime on overall rates of violent crime, homicide, rape, and aggravated assault; findings for robbery rates showed suggestive but small decreases associated with

moving from a more restrictive to a more permissive concealed-carry regime (see the first figure below).

In contrast, using age-adjusted homicide rates and analyzing a shorter time period (1991 to 2015), Siegel et al. (2017b) found that, relative to may-issue laws, shall-issue laws resulted in significantly elevated rates of total homicide and firearm homicide. A shortcoming of the authors' analysis was that it dropped several years of data for six states after 1998, because the CDC began suppressing homicide counts below ten per county in that year. Nevertheless, the authors report similar results from sensitivity analyses using a different data source, the Supplementary Homicide Reports database, that does not have the same suppression issues. The authors report using "robust standard errors that account for the clustering of observations, serial autocorrelation, and heteroskedasticity" (p. 1927), but they appear to have used a standard error adjustment that accounted for only heteroskedasticity and not the serial correlation that characterized their state-level panel data. Indeed, in a commentary on this study, Donohue (2017)'s replication of Siegel et al. (2017b)'s analyses produced estimated effects with properly clustered standard errors that were nearly twice as large as those shown in Siegel et al. (2017b)'s main analyses. However, even with the increased uncertainty around the effect sizes, the estimated effects of shall-issue laws on total and firearm homicide rates remained positive and statistically significant. Similar results were found by Fridel (2021), who instead estimated effects using generalized estimating equations to account for clustering within states over time and controlled for a wider set of time-varying state-level covariates and region fixed effects (rather than state fixed effects).

A subsequent study by Siegel et al. (2019) that distinguished between shall-issue and permitless-carry laws and that used data from 1991 to 2016 similarly showed significant increases in homicide rates with shall-issue laws and uncertain effects on these rates of permitless-carry laws. However, results for permitless-carry laws relied on policy changes in a few states with very short post-policy periods, which reduces statistical power and may threaten the validity of the studies' standard errors. Knopov et al. (2019) examined the same period but included a different set of covariates and considered differential effects by race. They found that shall-issue laws were associated with a significant increase in homicide rates for the total population, with no evidence of a differential effect among the race groups (black and white); permitless-carry laws were associated with suggestive reductions in homicide rates.

Shi and Lee (2018) estimated a panel data model with interactive fixed effects and spatial dependence in order to evaluate how shall-issue or permitless-carry laws affected crime rates from 1977 to 2012. In contrast with most prior studies of the effects of concealed-carry laws, the authors did not estimate regression models that directly controlled for state-level covariates that likely influence firearm legislation and crime rates (e.g., socioeconomic factors, changes in law enforcement resources). Instead, they accounted for (potentially) nonlinear state-specific time trends as a function of unobserved national time trend factors interacted with state-

specific factor loadings that determine the degree to which each state was differentially affected by the time trend factors. Their first-differences models also included a lagged outcome variable and covariates to account for potential spatial spillover effects. Their results were mixed. Some outcomes (e.g., robbery) indicated a significant increase immediately after shall-issue law enactment followed by a declining trend, while other outcomes (e.g., murder) showed significant declines but not until more than five years after law passage. Effects on rape rates and assault rates were uncertain or suggestive, depending on when (i.e., how long after implementation) the effect was assessed. However, for both outcomes showing significant effects, the study's models had an unfavorable ratio of estimated parameters to observations (about one to three for murder rates and one to nine for robbery rates), which suggests that these models may have been overfit and thus produced unreliable estimates and CIs.

Schell et al. (2020) estimated the effects of three gun laws (shall-issue, stand-your-ground, and child-access prevention) within a Bayesian modeling framework. Using an autoregressive negative binomial regression model shown to have optimal statistical properties for estimating policy effects on firearm deaths (Schell, Griffin, and Morral, 2018), they parameterized policy effects to have a nonlinear phase-in over a six-year period, at which time the policies' effects were evaluated. They estimated uncertain effects of shall-issue laws on total suicide rates and firearm suicide rates. These estimates were associated with a 65-percent and 77-percent probability that shall-issue laws increase total and firearm homicide, respectively.

Two studies focused on how concealed-carry laws affect workplace homicide rates (Doucette, Crifasi, and Frattaroli, 2019; Sabbath, Hawkins, and Baum, 2020). Doucette, Crifasi, and Frattaroli (2019) estimated effects of shall-issue or permitless-carry laws using a negative binomial regression model that controlled for several other firearm laws, 14 social and economic state covariates, state and year fixed effects, region fixed effects, and random intercepts for between-state effects.[4] Using data from 1992 to 2017, they estimated large and significant increases in firearm workplace-related homicides associated with more-permissive concealed-carry laws (IRR = 1.34; 95-percent CI = 1.16, 1.45). Covering a much shorter period (2011 to 2016), Sabbath, Hawkins, and Baum (2020) evaluated how the number of concealed-carry restrictions in a state (ranging from zero to seven) affected workplace homicide rates. Estimating gamma regressions that controlled for year fixed effects, other firearm laws, state-level socioeconomics, and the nonhomicide violent crime rate, the authors found that an additional restrictive concealed-carry provision reduced workplace homicide rates by 9 percent (95-percent CI = 0.87, 0.96).

Two studies estimated how shall-issue laws affected fatal or nonfatal assaults on police officers (Mustard, 2001; Crifasi, Pollack, and Webster, 2016). Mustard (2001) preferred a spline model, estimating the change in trends before versus after implementation of shall-issue laws for the outcome of felonious police deaths per capita or per full-time equivalent police officer

from 1984 to 1996. Across multiple specifications (e.g., Poisson, Tobit), the author tended to find that shall-issue laws had uncertain effects, except when the outcome was measured as police deaths per full-time equivalent officer; in that case, shall-issue laws led to a negative shift in trend that was statistically significant. However, this model had an unfavorable ratio of estimated parameters to observations (about one to seven) and did not account for serial correlation within states, which suggests that the estimated effects and associated CIs may be unreliable. Crifasi, Pollack, and Webster (2016) extended the period of study through 2013 and instead evaluated how shall-issue or permitless-carry laws affected fatal or nonfatal assaults on law enforcement officers, measured as a rate per full-time equivalent officer. The authors found uncertain effects of the laws on fatal assaults but a suggestive effect ($p = 0.13$) consistent with less-restrictive concealed-carry laws resulting in lower rates of nonfatal assault on law enforcement officers.

Finally, one study (Gius, 2019a) evaluated the effects of state campus-carry laws, which explicitly allow the carrying of concealed firearms on college campuses by students, faculty, and other staff. They also considered the effects of more-general state concealed-carry laws, distinguishing between shall-issue laws and permitless-carry laws. The study focused on how these laws influence rates of violent crime occurring on college campuses using offense data submitted by postsecondary institutions to the U.S. Department of Education. The author aggregated these data to the state-year level, then used a log-linear specification with state and year fixed effects, as well as state-level time-varying controls including more-general concealed-carry laws, college enrollment rates, demographics, and overall state-level violent crime rates. Findings showed uncertain associations of all types of carry laws with violent crime rates occurring on college campuses. However, the statistical model had an unfavorable ratio of covariates to observations (less than one to eight), meaning that it may have been overfit, resulting in estimates and CIs that are unreliable indicators of the true causal effects of the laws.

City-Level Studies

Kovandzic, Marvell, and Vieraitis (2005) examined the effect of shall-issue laws on rates of violent crime (homicide, robbery, assault, and rape) using panel data from 1980 to 2000 for 189 large U.S. cities. The authors clustered the standard errors at the state level, addressed coding errors in previous research, allowed for a time trend in the effect of shall-issue laws, allowed for city-specific time trends, and conducted analyses that allowed for heterogeneity in the effect of shall-issue laws across states. In their analysis that estimated the average effect of shall-issue laws for all included cities using a dummy model specification, Kovandzic, Marvell, and Vieraitis (2005) found uncertain effects for all of the violent crime outcomes analyzed. These findings were largely consistent when they instead modeled the effects of shall-issue laws as a trend variable, except that their preferred spline models showed effects consistent with shall-issue laws increasing assault rates (a significant effect) and increasing rape rates (a

suggestive effect). Their estimates for the effect on assault suggest that shall-issue laws were associated with a 10-percent increase in aggravated assault rates after five years. In examining state-specific effects with their spline models, the authors further found that there were more states where shall-issue laws led to statistically significant increases in crime compared with decreases. However, this study had an unfavorable ratio of model covariates to observations (less than one to ten), meaning the model may have been overfit, and thus its estimates and CIs may be unreliable indicators of the true effects of the laws.

La Valle (2013) analyzed data from 56 cities spanning 1980–2010. The author noted that the analyses "include statistical corrections for variation in sample unit independence," but he did not explicitly mention clustering the standard errors at the state level. La Valle (2013) used a dummy variable specification for the concealed-carry law. In his preferred specification (using interpolated control variables for inter-censal years, population weighted analysis, and a one-year lagged outcome as a covariate), he found that shall-issue laws significantly reduced gun homicides by 15 percent and total homicides by 13 percent (see the first figure below). In La Valle and Glover (2012), which used similar data (panel data on 57 cities from 1980 to 2006) and a similar approach, the authors included separate indicators for may-issue and shall-issue states. In the authors' preferred analysis (with interpolated data for controls for inter-censal years and weighting), shall-issue laws were associated with a significant 23-percent increase in the homicide rate, and may-issue laws were associated with a significant 19-percent decrease in the homicide rate (compared with cities that the authors concluded did not have either a may-issue or shall-issue law). Similarly, shall-issue laws were associated with a significant 32-percent increase in the firearm homicide rate, while may-issue laws were associated with a significant 33-percent reduction in the firearm homicide rate. (No estimates for unweighted data with inter-censal years were provided.) The diametric findings from these two studies further highlight the sensitivity of results to model specification. In both studies, however, the authors coded states as having or not having right-to-carry laws in ways that are strikingly different than found under more-conventional definitions (Donohue et al., 2022). For instance, in La Valle (2013), 23 states have implementation dates (or are missing them) for right-to-carry laws that differ from those found in RAND's State Firearm Law database by more than one year, including some states, like Hawaii, which are usually treated as having a restrictive may-issue law.[5] For these reasons, the findings from these two studies provide doubtful evidence on the effects of may-issue and right-to-carry laws as these are conventionally understood.

Siegel et al. (2020b) estimated the effects of stringent may-issue laws, which grant law enforcement authorities a high level of discretion for granting concealed-carry permits, on homicide rates between 1991 and 2016 within 197 medium-to-large U.S. cities (populations of at least 100,000 residents) and, separately, within approximately 50 nonurban areas.[6] The authors estimated policy effects using negative binomial models with controls for year and city fixed effects, state and jurisdiction-level covariates, and five additional firearm policies. For medium-to-large cities, they estimated a significant 17-percent reduction in rates of

firearm homicide after repeal of "heightened showing may issue" laws and no discernable effect on rates of nonfirearm homicide, resulting in an estimated 13-percent reduction in total homicide rates. In nonurban areas, estimated effects were small and uncertain (see the first figure below).

Finally, Smith and Petrocelli (2019) examined the impact of a 2010 law in Arizona that repealed licensing, background check, and training requirements for carrying a concealed handgun. They evaluated effects on violent crime outcomes in Tucson, Arizona, relative to a control city, El Paso, Texas, which they argued is geographically close and similar in size. Using quarterly data from 2007 to 2013, they found uncertain effects of concealed-carry deregulation on murders, robberies, or aggravated assaults. However, given that this study examines a state policy change in a single city and does not control for any potential differences between Tucson and El Paso that might be confounded with the effects of interest (e.g., the population in El Paso is 80 percent Hispanic and in Tucson it is 42 percent Hispanic), this study is severely limited in providing causal evidence.

Other Studies

Three studies that focused on the relationship between unmarried fertility or abortions and violent crime included shall-issue laws as a covariate in their models (Donohue and Levitt, 2001; Lott and Whitley, 2007; Kendall and Tamura, 2010). Using data from 1985 to 1997 and estimating weighted least squares with a logged outcome and state and year fixed effects, Donohue and Levitt (2001) found uncertain effects of shall-issue laws on violent crime and murder rates. Analyzing data over a partially overlapping period, from 1976 to 1998, and using a Poisson model that controlled for state and year fixed effects, state-specific linear trends, and time-varying state covariates, Lott and Whitley (2007) found suggestive or significant effects (depending on specification) indicating that murder rates fell approximately 1 percent faster after the adoption of shall-issue laws relative to the rates in states without such policies. Employing a different model specification over a longer period (1957–2002), Kendall and Tamura (2010) estimated that shall-issue laws had a suggestive but small association with reduced rates of murder and uncertain relationships with rates of rape, robbery, and assault.

Zimmerman (2014) extended prior research evaluating the role of private security measures in reducing crime (e.g., see Benson and Mast, 2001). Although the author's focus was on understanding the crime rate implications of changes in employment within four private security occupation groups (security guards, detectives and investigators, security system installers, and locksmiths), he included the existence of shall-issue laws as a covariate in the models to account for the potential deterrent effects of allowing private citizens to carry handguns. Estimating linear models with a logged outcome and controlling for state and year fixed effects, state-specific linear trends, a lag of the dependent variable, and time-varying state characteristics, Zimmerman (2014) found that shall-issue laws led to significantly higher

rates of murder and assault; estimated effects on robbery rates were suggestive but also consistent with an increase following the passage of shall-issue laws. However, the analyses had a ratio of estimated parameters to observations of less than one to five, and the paper provided no additional evidence to demonstrate model fit. Therefore, in accordance with our review methodology, we discount this evidence because of the possibility that the model was overfit, and thus the estimated effects and their CIs may be unreliable indicators of the true causal effects of the laws.

Manski and Pepper (2018) investigated the sensitivity of shall-issue effect estimates to a range of assumptions by comparing property and violent crime rates in two states under progressively less-restrictive assumptions about how the laws' effects may vary over time or between states. This study compared outcomes in just two states, meaning causal effects were not well identified. Moreover, it treated Virginia's shall-issue law as having been implemented in 1989, when we believe the correct date is 1995. For these reasons, we do not review this paper's results. Applying Bayesian model comparison techniques, Strnad (2007) reanalyzed models of the effects of shall-issue laws from Donohue (2004). In contrast to the approach of Donohue (2004) and many others, Strnad (2007) did not assess the evidence for or against shall-issue laws in terms of how frequently estimates of the effect were statistically significant or were found to have positive (as opposed to negative) estimated effects under different model specifications. Instead, he used model comparison techniques to establish which models fit the data best and to evaluate whether evidence favored models with or without shall-issue effects. He concluded that Donohue's models provided much stronger support for a conclusion that shall-issue laws had little or no effect on most outcomes than Donohue (2004) concluded after examining patterns in the direction and significance levels of these effects. The exceptions were murder rates, which shall-issue laws appeared to cause to decline gradually, and robbery rates, which appeared to increase or decrease depending on the state.

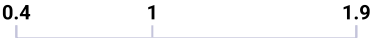
The figures below display the IRRs and CIs associated with the concealed-carry laws examined in the studies published after the NRC (2004) review. The first figure displays the studies for which we found no serious methodological issues, and the second figure displays the studies for which we did find methodological issues. In these figures, we highlight effect estimates using only dummy-coded models for reasons discussed in Chapter Two and in the first edition of this report (RAND Corporation, 2018, Appendix A). We exclude the estimates from Zimmerman (2014) for having a ratio of estimated parameters to observations of less than one to five and thus serious potential issues with model overfit. We exclude estimates from Smith and Petrocelli (2019) because of the aforementioned concerns with their study results, and we exclude the state-specific synthetic control estimates from Gius (2019b) because of insufficient information on effect sizes and inferential statistics. Furthermore, Lott (2010), Shi and Lee (2018), and Moody and Marvell (2018a) did not provide enough information for us to calculate IRRs and CIs for their effect sizes of interest, so we do not include these in the figures. In

addition, the estimates in Durlauf, Navarro, and Rivers (2016) were available only for the spline specification; Moody et al. (2014) offered only a hybrid model; and Manski and Pepper (2018) and Strnad (2007) did not seek to produce a preferred estimate of the effect of shall-issue laws. Because we could not readily calculate unique effect sizes and CIs for these studies, we do not include them in the figures.

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with No Serious Methodological Problems

HOW TO READ THIS CHART

STUDY, BY POLICY	OUTCOME MEASURE	EFFECT SIZE (IRR) [95% CI]
Shall-issue vs. may- or no-issue	**Homicide rate**	
Donohue, Aneja, & Weber (2019)	Total (1979–2014)	1.02 [0.92, 1.12]
Hamill et al. (2019)	Total (1986–2015)	1.00 [0.92, 1.08]
Aneja, Donohue, & Zhang (2014)	Total (1979–2010)	1.03 [0.91, 1.17]
Kendall & Tamura (2010)	Total (1957–2002)	1.00 [0.99, 1.00]
Hepburn et al. (2004)	Total (1979–1998)	1.01 [0.94, 1.10]
Donohue, Aneja, & Weber (2019)	Firearm (1979–2014)	1.03 [0.90, 1.16]
Hamill et al. (2019)	Firearm (1986–2015)	1.07 [0.97, 1.17]
French & Heagerty (2008)	Firearm (1979–1998)	1.06 [1.00, 1.12]
Donohue, Aneja, & Weber (2019)	Nonfirearm (1979–2014)	1.02 [0.95, 1.08]
	Violent crime rate	
Donohue, Aneja, & Weber (2019)	Total violent crime (1979–2014)	1.09 [1.03, 1.15]
Hamill et al. (2019)	Total violent crime (1986–2015)	0.99 [0.97, 1.01]
Hamill et al. (2019)	Rape (1986–2015)	1.00 [0.97, 1.03]
Aneja, Donohue, & Zhang (2014)	Rape (1979–2010)	1.12 [1.00, 1.26]
Kendall & Tamura (2010)	Rape (1957–2002)	1.00 [0.99, 1.00]
Hamill et al. (2019)	Robbery (1986–2015)	0.97 [0.94, 1.01]
Aneja, Donohue, & Zhang (2014)	Robbery (1979–2010)	1.15 [0.98, 1.34]
Kendall & Tamura (2010)	Robbery (1957–2002)	1.00 [1.00, 1.00]
Hamill et al. (2019)	Assault (1986–2015)	0.99 [0.97, 1.01]
Aneja, Donohue, & Zhang (2014)	Assault (1979–2010)	1.08 [0.99, 1.18]
Kendall & Tamura (2010)	Assault (1957–2002)	1.00 [1.00, 1.00]
	Assault rate on law enforcement officers	



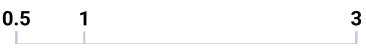
STUDY, BY POLICY	OUTCOME MEASURE	EFFECT SIZE (IRR) [95% CI]
Crifasi, Pollack, & Webster (2016)	Fatal (1984–2013)	1.02 [0.81, 1.29]
Crifasi, Pollack, & Webster (2016)	Fatal, handgun (1984–2013)	0.92 [0.70, 1.21]
Crifasi, Pollack, & Webster (2016)	Fatal, nonhandgun (1984–2013)	1.27 [0.85, 1.88]
Crifasi, Pollack, & Webster (2016)	Nonfatal (1998–2013)	0.72 [0.47, 1.10]
Crifasi, Pollack, & Webster (2016)	Nonfatal, handgun (1998–2013)	0.74 [0.41, 1.33]
Crifasi, Pollack, & Webster (2016)	Nonfatal, nonhandgun (1998–2013)	0.74 [0.42, 1.30]
Shall- or may-issue vs. no-issue	**Homicide rate**	
La Valle (2013)	Total, age-adjusted (1980–2010)	0.87 [0.77, 0.98]
La Valle (2013)	Firearm, age-adjusted (1980–2010)	0.85 [0.73, 0.98]
Shall-issue vs. no-issue	**Homicide rate**	
La Valle & Glover (2012)	Total (1980–2006)	1.23 [1.05, 1.44]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+ (1977–2014)	1.06 [0.88, 1.27]
La Valle & Glover (2012)	Firearm (1980–2006)	1.32 [1.14, 1.52]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+ (1977–2014)	1.08 [0.86, 1.36]
Luca, Malhotra, & Poliquin (2017)	Nonfirearm, aged 21+ (1977–2014)	1.05 [0.93, 1.18]
May-issue vs. no-issue	**Homicide rate**	
La Valle & Glover (2012)	Total (1980–2006)	0.81 [0.71, 0.92]
Luca, Malhotra, & Poliquin (2017)	Total, aged 21+ (1977–2014)	1.06 [0.88, 1.29]
La Valle & Glover (2012)	Firearm (1980–2006)	0.77 [0.66, 0.90]
Luca, Malhotra, & Poliquin (2017)	Firearm, aged 21+ (1977–2014)	1.05 [0.83, 1.32]
Luca, Malhotra, & Poliquin (2017)	Nonfirearm, aged 21+ (1977–2014)	1.13 [0.98, 1.30]

NOTE: This figure includes only the studies reporting dummy-coded law effects published since the NRC (2004) review of gun policy effects. IRR values marked with empty circles indicate that we identified concerns with the study's methodology, and these concerns are described in the text above. Filled circles indicate that we identified no significant methodological concerns.

Incidence Rate Ratios Associated with the Effect of Concealed-Carry Laws on Violent Crime: Studies with Serious Methodological Problems

STUDY, BY POLICY	OUTCOME MEASURE	EFFECT SIZE (IRR) [95% CI]

STUDY, BY POLICY	OUTCOME MEASURE	EFFECT SIZE (IRR) [95% CI]
Shall-issue vs. may- or no-issue	**Homicide rate**	
Siegel et al. (2017b)	Total, age-adjusted (1991–2015)	1.06 [1.03, 1.10]
Webster, Crifasi, & Vernick (2014)	Total, age-adjusted (1999–2010)	1.06 [0.99, 1.13]
Grambsch (2008)	Total (1976–2001)	1.01 [0.98, 1.03]
Rosengart et al. (2005)	Total (1979–1998)	1.07 [0.98, 1.17]
Martin & Legault (2005)	Total (1977–1992)	0.95 [0.90, 1.01]
Kovandzic, Marvell, & Vieraitis (2005)	Total (1980–2000)	1.00 [0.94, 1.07]
Siegel et al. (2017b)	Firearm, age-adjusted (1991–2015)	1.09 [1.05, 1.13]
Webster, Crifasi, & Vernick (2014)	Firearm, age-adjusted (1999–2010)	1.06 [0.96, 1.16]
Crifasi et al. (2018b)	Firearm, urban only (1984–2015)	1.04 [1.02, 1.06]
Rosengart et al. (2005)	Firearm (1979–1998)	1.11 [0.99, 1.24]
Siegel et al. (2017b)	Nonfirearm, age-adjusted (1991–2015)	1.01 [0.96, 1.07]
Webster, Crifasi, & Vernick (2014)	Nonfirearm, age-adjusted (1999–2010)	1.10 [0.99, 1.21]
Crifasi et al. (2018b)	Nonfirearm, urban only (1984–2015)	1.03 [1.00, 1.06]
	Violent crime rate	
Martin & Legault (2005)	Total violent crime (1977–1992)	0.94 [0.91, 0.98]
Kovandzic, Marvell, & Vieraitis (2005)	Rape (1980–2000)	1.00 [0.95, 1.04]
Martin & Legault (2005)	Rape (1977–1992)	0.98 [0.94, 1.03]
Kovandzic, Marvell, & Vieraitis (2005)	Robbery (1980–2000)	1.01 [0.95, 1.07]
Martin & Legault (2005)	Robbery (1977–1992)	0.96 [0.91, 1.02]
Kovandzic, Marvell, & Vieraitis (2005)	Assault (1980–2000)	0.98 [0.94, 1.02]
Martin & Legault (2005)	Assault (1977–1992)	0.93 [0.89, 0.98]
DeSimone, Markowitz, & Xu (2013)	Firearm assault injury, aged 0–17 (1988–2003)	2.49 [1.02, 6.08]
DeSimone, Markowitz, & Xu (2013)	Firearm assault injury, aged 18+ (1988–2003)	2.72 [1.74, 4.26]
Shall-issue vs. may-issue	**Violent crime rate**	
Barati (2016)	Homicide (1991–2008)	1.02 [0.97, 1.08]
Barati (2016)	Robbery (1991–2008)	1.05 [0.96, 1.15]
Barati (2016)	Assault (1991–2008)	1.05 [0.95, 1.15]
Shall-issue vs. no-issue	**Violent crime rate**	
Barati (2016)	Homicide (1991–2008)	0.94 [0.84, 1.05]
Barati (2016)	Robbery (1991–2008)	0.93 [0.86, 1.01]
Barati (2016)	Assault (1991–2008)	1.04 [0.96, 1.14]
May-issue or no-issue vs. shall-issue	**Homicide rate**	
Gius (2014)	Firearm (1980–2009)	1.11 [1.05, 1.16]
May-issue vs. shall-issue	**Intimate partner homicide rate**	
Roberts (2009)	Total (1985–2004)	1.71 [1.34, 2.19]
Roberts (2009)	Firearm (1985–2004)	1.12 [0.90, 1.40]
No-issue vs. shall-issue	**Intimate partner homicide rate**	



STUDY, BY POLICY	OUTCOME MEASURE	EFFECT SIZE (IRR) [95% CI]
Roberts (2009)	Total (1985–2004)	0.96 [0.62, 1.50]
Roberts (2009)	Firearm (1985–2004)	0.86 [0.56, 1.33]

NOTE: This figure includes only the studies reporting dummy-coded law effects published since the NRC (2004) review of gun policy effects. The estimates from Kovandzic, Marvell, and Vieraitis (2005) are from the authors' dummy model specification rather than their preferred spline model (denoted by asterisks). IRR values marked with empty circles indicate that we identified concerns with the study's methodology, and these concerns are described in the text above. An arrow on either end of a CI indicates that the interval is wider than can be displayed on the scale.

Conclusions

Because so much more study has been done of the relationship between concealed-carry laws and violent crime than of any other gun policy and outcome, there is a much richer evidence base to draw on, including studies that raise serious methodological concerns and several that did not raise as many concerns among our methodology review team. Therefore, to focus this review on the best available evidence, we draw our conclusions in this section using just those 22 studies that did not raise serious methodological concerns. We incorporate all studies that met this criterion in our discussion, but we prioritize findings from studies with a study time frame that extended beyond 2000. We do so because studies omitting more-recent data (1) identify policy effects excluding a large number of states that have enacted shall-issue laws in the past 20 years and (2) have limited post-implementation data to allow these policies to establish their full effects.

Total homicides. Of the 22 studies without serious methodological concerns, 18 examined the effects of shall-issue laws on total homicides, and one examined the effects of the laws on fatal assaults of law enforcement officers. Of the ten studies that evaluated shall-issue laws and included data after 2000, six found only uncertain effects of these laws (Donohue, Aneja, and Weber, 2019; Hamill et al., 2019; Luca, Malhotra, and Poliquin, 2017; Crifasi, Pollack, and Webster, 2016; Aneja, Donohue, and Zhang, 2014; Schell et al., 2020). Kendall and Tamura (2010) found small suggestive effects consistent with shall-issue laws reducing homicides. Moody et al. (2014) found that shall-issue laws cause a downward trend in homicides, although a subsequent study that included four more years of data found uncertain effects of the law in seven of eight evaluated years, with a single significant negative effect in the seventh year (Moody and Marvell, 2018a). Two recent studies found that shall-issue laws increased homicide rates (Knopov et al., 2019; Siegel et al., 2019), and one found that stringent may-issue laws requiring a heightened showing of suitability led to significantly lower rates of homicide in medium-to-large cities (Siegel et al., 2020b). Of the six studies focused on a period prior to 2000, two found that shall-issue laws caused a downward trend in homicide or murder rates (Strnad,

2007; Plassmann and Whitley, 2003), one found a suggestive negative effect (Olson and Maltz, 2001), and three found uncertain effects (Hepburn et al., 2004; Helland and Tabarrok, 2004; Ludwig, 1998). Weighing the relative strengths of these studies, and considering their analyses of different data sets and periods, we find that the existing literature provides *supportive evidence that shall-issue laws increase total homicides*.

Two studies separately estimated the effects of permitless carry laws-on homicides or murders, and both found uncertain effects (Knopov et al., 2019; Siegel et al., 2019). We therefore conclude that there is *inconclusive evidence for the effect of permitless-carry laws on total homicides*.

Firearm homicides. Ten of the 22 studies examined the effects of shall-issue laws on firearm homicides, one examined effects of the laws on fatal assaults of law enforcement officers, and one examined the effects of the laws on workplace firearm homicides. Among these 12 studies, ten evaluated data past 2000, and there was one with suggestive (Hamill et al., 2019) and four with significant (Doucette, Crifasi, and Frattaroli, 2019; Knopov et al., 2019; Fridel, 2021; Siegel et al., 2019) evidence of effects indicating that more-permissive concealed-carry laws increase firearm homicides. Siegel et al. (2020b) found that more-stringent concealed-carry laws significantly reduce firearm homicides in larger cities but have uncertain effects in nonurban areas. Three studies found uncertain effects of shall-issue laws on firearm homicides (Donohue, Aneja, and Weber, 2019; Luca, Malhotra, and Poliquin, 2017; Schell et al., 2020). One study examined the effects of the laws on fatal handgun assaults of law enforcement officers and found uncertain effects (Crifasi, Pollack, and Webster, 2016). Of the two studies focused on a period prior to 2000, one found that shall-issue laws increase firearm homicides (French and Heagerty, 2008), and the other found that the laws decrease firearm homicides (Olson and Maltz, 2001). Weighing the relative strengths of these studies, and considering their analyses of different data sets and periods, we conclude that the existing literature provides *supportive evidence that shall-issue laws increase firearm homicides*.

Robberies. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase robbery rates, while Hamill et al. (2019) instead found a suggestive effect indicating that shall-issue laws decrease robbery rates. Five studies, the three most recent of which included data after 2000, found largely uncertain effects of shall-issue laws on robberies (Moody and Marvell, 2018a; Moody et al., 2014; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide *inconclusive evidence for the effect of shall-issue laws on robbery rates*.

Assaults. Aneja, Donohue, and Zhang (2014) found a suggestive effect that shall-issue laws may increase assault rates, and Moody et al. (2014) found that shall-issue laws were associated with a significant upward trend in assault rates. In contrast, Moody and Marvell (2018a) found suggestive effects consistent with shall-issue laws leading to reduced assault rates, and Crifasi, Pollack, and Webster (2016) found that shall-issue laws had a suggestive negative effect on

nonfatal assaults of law enforcement officers. Four studies, including two with data extending past 2000 (Hamill et al., 2019; Kendall and Tamura, 2010), found only uncertain effects of shall-issue laws on assault (Hamill et al., 2019; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide *inconclusive evidence for the effect of shall-issue laws on assaults*.

Rapes. Aneja, Donohue, and Zhang (2014) found that shall-issue laws significantly increase rates of rape. Moody et al. (2014) found that shall-issue laws produce a significant downward trend in rates of rape. Moody and Marvell (2018a) also found some evidence of significant declines in rape rates, although these effects did not emerge until four years after implementation of the law. Four studies, two of which included data past 2000, found uncertain evidence of an association between shall-issue laws and rape (Hamill et al., 2019; Kendall and Tamura, 2010; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Therefore, we conclude that the best available studies provide *inconclusive evidence for the effect of shall-issue laws on rapes*.

Violent crime. Two studies (Donohue, Aneja, and Weber, 2019; Durlauf, Navarro, and Rivers, 2016) aggregated all violent crimes into a single category and found that shall-issue laws significantly increase violent crime rates. Three studies, one of which included data past 2000, found uncertain effects of shall-issue laws on overall violent crime (Hamill et al., 2019; Helland and Tabarrok, 2004; Plassmann and Whitley, 2003). Because evidence for the effect of shall-issue laws on each component of violent crime is inconclusive, it could be argued that these two studies of the effect of these laws on all violent crimes should not suffice to suggest that there is more than inconclusive evidence for such an effect. However, because analyses on all violent crimes may have greater statistical power to detect any such effects, and because our scoring criteria indicate it, we conclude that there is *limited evidence that shall-issue laws may increase violent crime*.

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CONCEALED-CARRY LAWS VIOLENT CRIME

Notes

1. We identified one study that analyzed how changes in the number of concealed-carry permits related to changes in various types of violent crime (Kovandzic and Marvell, 2003). The authors analyzed data from 58 Florida counties spanning 1980–2000 and found uncertain effects of changes in per capita concealed-carry permit rates on violent crime. Although this study provided coverage of the period before and after the passage of Florida's shall-issue law in 1987, it did not analyze the effect of the shall-issue policy change and thus did not meet our inclusion criteria. ↩

2. Most homicides reported in the CDC's vital statistics data are counted among deaths reported to the FBI as murders and nonnegligent manslaughter. The authors used both data sources for this study because the vital statistics data differentiated firearm homicides from total homicides, whereas the FBI data spanned a longer period. ↩

3. Moody and Marvell (2018a) cite several other concerns regarding the truncation of the sample to omit confounding from the crack epidemic, incorrect standard errors, preference of state-level versus county-level crime data, absence of adjustments for multiple hypothesis testing, and interpretation of estimated effects when lead and lag dummy variables for the law were included. Donohue (2018) responds to these criticisms. ↩

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

5. See, for instance, the Supreme Court's decision in *New York State Rifle and Pistol Association v. Bruen*, 2022, which classifies Hawaii as among the few remaining states with a may-issue licensing regime. ↩

6. *Nonurban areas* are defined as cities not classified as medium-to-large cities or areas with crime reported at the county level (with the exception of Jefferson Parish, Louisiana, and Arlington County, Virginia), which the authors then aggregated up to the state level. Nine states that did not include any large or medium cities were treated as nonurban areas, although two of these states (Vermont and North Dakota) were dropped "because the homicide counts were too low to generate stable rate estimates" (Siegel et al., 2020b, p. 259). ↩

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