OCTOBER 2022

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Issue Brief

More Guns, Same Amount of Crime? Analyzing the Effect of Right-to-Carry Laws on Homicide and Violent Crime

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Introduction

The past 40 years have seen nothing short of a revolution in Americans' right to carry a concealed firearm in public. In 1980, the vast majority of states either did not grant concealed weapon permits or offered them only on a "may-issue" basis, meaning that authorities retained discretion to deny applications. Since then, many states have adopted "shall-issue" laws, under which anyone who meets certain objective requirements—such as passing a background check, paying a fee, and getting some training—is guaranteed a permit. In recent years, more than 20 states have decided not even to require permits, though restrictions based on age, criminal history, etc., still apply. And earlier this year, in *New York State Rifle & Pistol Association Inc. v. Bruen*, the Supreme Court ruled that the Constitution protects the right to public carry while striking down New York's requirement that permit applicants demonstrate a special need to carry, but allowing states to continue to require objective criteria.

Now that right-to-carry (RTC) is becoming universal, the purpose of this brief is to ask what the policy's consequences for crime rates have been thus far. In many ways, it is the perfect "natural experiment." One by one, most of the states throughout the country decided to make it much easier to carry guns in public; if either side of the gun debate is correct, these policy changes should have led to sizable shifts in crime rates. In theory, measuring such shifts should be easy because during times when some states were changing their laws, others were not—and the latter may serve as a handy control group for the former. With so many experiments running for so many years, the results should be clear by now, both in the raw data and with the aid of modern statistics.

That is not how things have played out. Twenty-five years after the first rigorous studies on RTC were published, social science has not resolved the issue. Different researchers, often using the same basic methods in slightly different ways, have long reached varying conclusions. Over

time, findings that RTC reduces crime have become less common, and findings that it increases crime have become more common—but recent work still contains plenty of null results, meaning that any effect was too small to measure. In a sense, we are actually *losing* ground because some of those basic methods—used for decades to address this topic and many similar ones—have technical flaws that only recently have come to light.

This is a story about the limitations of modern social science more than a story about the potential of guns to create or solve problems. But for states that are being forced into a right-to-carry era that they would have preferred to avoid, the fallout from the ruling is likely to be subtle; gun-rights supporters' hopes, as well as gun-control proponents' fears, about RTC have been largely misplaced. Further, the Supreme Court left these states a *lot* of leeway to minimize the number of civilians who receive permits and to restrict the behavior of civilians who do receive them, meaning that any negative consequences seen in voluntary RTC states can be minimized in states now forced to change their laws. These states do not have to charge into RTC with the gusto that other states, with different cultures and very different attitudes toward guns, have chosen to exhibit. Indeed, in the short time since the ruling, several states have already acted to keep their gun-permit regimes as strict as legally allowed.

The Theory Behind Right-to-Carry (and Some Terminology)

A priori, one can make a case that right-to-carry (RTC) should decrease crime, that it should increase crime, or that it should have little effect.

The "more guns, less crime" hypothesis is rooted in incentives. RTC specifically empowers the law-abiding to arm themselves to prevent victimization. (Even in the states with the loosest gun laws, felons and other high-risk individuals are still prohibited—though, of course, criminals do not always follow the law.) This increases the chance that criminals will encounter armed victims and thus discourages them from committing crimes, especially interpersonal crimes. If the guns of the law-abiding are concealed, all the better, because even people not carrying can benefit from criminals' concern that they might be.

The "more guns, more crime" theory, by contrast, holds that the presence of guns—even carried by those without serious criminal records—tends to make things worse. A petty squabble that might have ended in some curse words, or, at most, a fistfight, instead ends with gunshots. Concealed carriers sometimes leave their guns in unlocked cars or forget them in bathrooms, unintentionally arming criminals. Meanwhile, police have a harder time stopping illegal gun-carrying when many civilians carry guns legally; police become less proactive when the individuals they stop are more likely to be armed, and they simply waste a lot of manpower processing permit applications.

A third hypothesis is that the policy might not matter too much. Some of the aforementioned effects will cancel each other out. Although legal gun-carriers may be unlikely to commit crimes themselves, they may also be relatively unlikely to become crime victims, whether they carry or not. For example, a lot of violence occurs among young criminals, in incidents where neither the victim nor the aggressor might be able to carry legally, for reasons of age, criminal history, and/or willingness to jump through the hoops needed to get a permit. Even more fundamentally, one might argue that weaponry in general is far from the top of the list of factors that determine how violent a society is.[1]

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A few terms to know before we proceed: The modern debate is over concealed carry, meaning the carriage of handguns in ways not visible to the public, such as in a holster that sits under one's clothes—as opposed to open carry, as with a holster worn visibly on the belt, as a police officer would. Concealed-carry permits can be regulated on a may-issue basis, where officials have some discretion to deny permits, or on a shall-issue basis, which means that officials must grant permits to any applicant who meets the state's requirements (such as passing a background check, paying a fee, and/or receiving training). There is also permitless carry, sometimes called "constitutional carry" by its proponents, though I will avoid that term here; under this policy, no permit is required to carry a gun, though some categories of individuals, such as felons, remain prohibited from doing so. If a state has a shall-issue law or allows permitless carry, it is considered a right-to-carry state.

The Legal Landscape

As the 1980s began, only five states had a right to carry concealed weapons. Many others had long allowed open carry[2]—but that practice was falling out of fashion as Americans came to see the open display of weapons as needlessly threatening. Then the RTC trend took off: per the RAND Corporation's database of gun laws,[3] which I will rely on in cases where a state's status is disputed, thanks to vague laws or court rulings, eight states enacted RTC in the 1980s, 18 did so in the 1990s, and another eight did so in the 2000s, with three stragglers plus D.C. joining in the 2010s. When the Supreme Court decision came down, RTC had already become the norm in most states, including in many that are home to large urban areas. These changes typically took the form of shall-issue laws, meaning that permit applicants had to satisfy various requirements, such as training and fees (though they were guaranteed a permit if they did).

More recently, a second wave of permitless-carry statutes has swept the country. Permitless carry was rare as late as 2010. Vermont never restricted concealed carry on the state level to begin with, and a 1903 court decision prevented local governments from doing so as well. But throughout the 20th century, no other state had the same level of permissiveness. In the 2000s, one state, Alaska, adopted the policy. But during the 2010s, 14 more states went permitless, mostly in the second half of the decade. Adding laws from the current decade, including some that have not yet taken effect, the U.S. Concealed Carry Association lists 25 states that have enacted permitless carry.[4]

Even before the Supreme Court's *Bruen* decision, every state allowed at least some individuals to carry concealed weapons, but these policies varied immensely—not just across the may-issue, shall-issue, and permitless categories, but even within them. In some states, vague statutes and court decisions, as well as bureaucratic practices, generated confusion over which category the state even fell into, especially may- vs. shall-issue.[5]

Contrast, also, the experiences of two people who live in nominally "shall-issue" jurisdictions. After a recent move, I received a five-year Wisconsin permit on the basis of a several-hour class that cost $89 and did not involve firing a gun, along with a $40 fee paid to the state with an online application. Under Wisconsin's statute, I could have skipped that carry-focused training and instead submitted a hunters'-safety certificate.[6] (I received mine in 1997, at the age of 13.) Renewals currently cost $22. The process was roughly similar in Virginia, the last state I lived in.

Law professor Randy Barnett, on the other hand, lives in D.C., which was forced into a shall-issue regime by a 2017 court decision. Here is how he described his experience:

> I had to pay an application fee of $75. I had to submit my application in person at the Metropolitan Police Department headquarters and be photographed and fingerprinted at an additional cost of $35. I had to pass a federal background check. I had to enroll in and pay for an approved firearms training course, which included 16 hours of classroom study of D.C. gun laws as well as the law governing the use of deadly force, plus another two hours of range instruction. In 2018, the course cost $250 plus $20 for the range fee.… Every two years, I must renew the license. If I miss renewing within the 30-day window before my permit expires, I have to start all over. So, two years later, I had to pay another $75 fee and complete a recertification class consisting of four hours of training, and two hours of range training from an MPD-certified firearms training instructor, which cost $160.[7]

Tempering Expectations: Reviewing Some Raw Data

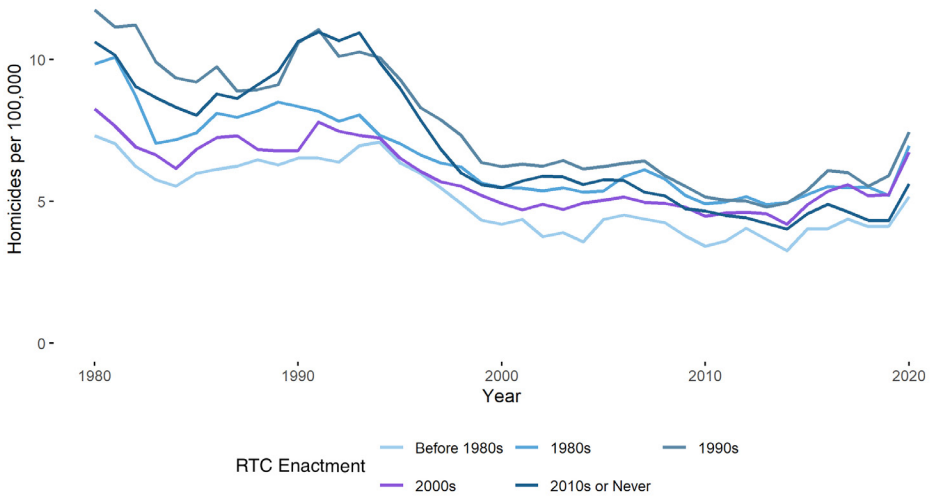
In this brief, we will review some studies that use complicated statistical methods. But first, it will be helpful to see some more straightforward presentations of the underlying data, which make it clear that any effect of RTC laws must be relatively subtle. These laws do not, for example, immediately double or halve crime rates in the states that enact them.

In **Figure 1** below, I have grouped states according to when they enacted RTC and plotted the FBI's homicide rates (which count murders and non-negligent manslaughters) over time for each group as a whole.8 By tracing each line—bearing in mind the decade when the law changed in those states and treating the other cohorts as control groups—one can mentally perform a poor man's version of the analyses that we will review in the coming sections.

Figure 1

State RTC Decade of Enactment and Homicide Rate

Homicide



There are some noticeable trend breaks in the figure above, as well as a general tendency for the groups' rates to spread out when crime is high nationwide and to converge when crime is low. Notably, crime fell throughout the country during the period when states were liberalizing their gun-carry laws. But the more granular trends do not have any readily noticeable relationship to the periods when these groups of states enacted concealed carry.

Note, especially, the two groups that did not change their RTC laws during (at least most of) this period—those that had already adopted RTC and those that resisted into the 2010s or later—because their lack of change makes them especially useful control groups.

The earliest adopters—a relatively rural group of states comprising Alabama, Connecticut, New Hampshire, Vermont, and Washington—always had low crime. (This group would have had even lower crime without Alabama, the sole Southern state, which typically has at least double the homicide rate of the others.) Over time, the group's trends tracked those in the states that changed their laws.

The late/never adopters are interesting as well. Iowa, Wisconsin, Illinois, and D.C. went RTC in the 2010s (Illinois and D.C. only because court decisions forced them to); California, Delaware, Hawaii, Maryland, Massachusetts, New Jersey, New York, and Rhode Island never did. Relative to trends elsewhere, this group saw a dramatic increase in homicide going into the 1990s—but also a bigger relative fall thereafter. Toward the end of the period, these locations became comparatively safe, rivaling the more rural early RTC adopters.

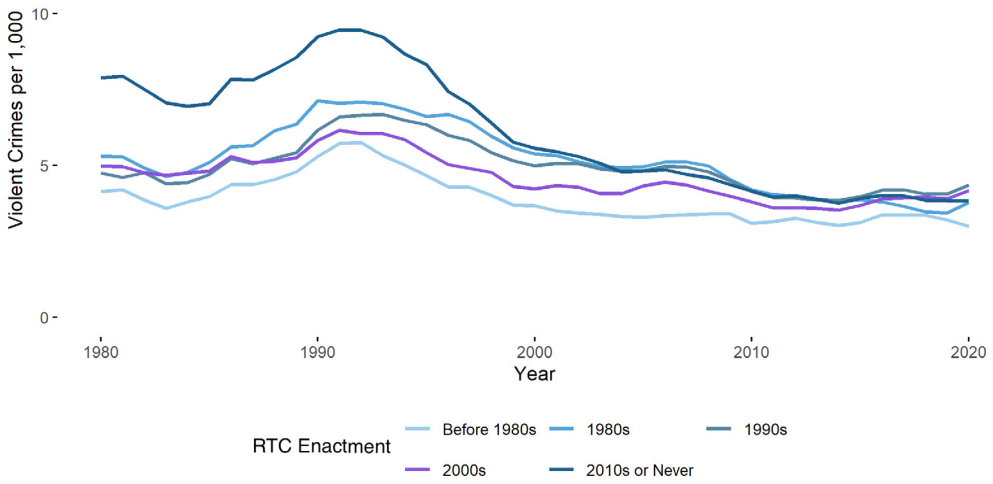
One could argue that these states became relatively safer thanks to RTC driving up crime elsewhere, but a look at the other groups does not really support that reading. The remaining groups had rather parallel trends throughout the period; there is little sign that each group sees crime go up as it implements the law. An alternative explanation for trends in the late/never adopters is that these relatively urban areas were disproportionately affected by the factors that drove up crime heading into the 1990s and then reduced it thereafter (such as the crack epidemic, for the former; and the NYPD-led revolution in policing, for the latter).

Figure 2 is a similar chart for overall violent-crime rates, which aggregate murder, nonnegligent manslaughter, aggravated assault, robbery, and rape; as in most studies of RTC, they are drawn from FBI data that, in turn, were collected from police departments. We see some of the same patterns: the late/never adopters see a dramatic crime decline, though they start with shockingly high crime rates, and the other groups have roughly parallel trends. However, it is possible to weave a narrative where the laws have a subtle but bad effect, focusing on the groups that enacted laws between the 1980s and 2000s. (The 1980s adopters broke away from the 1990s and 2000s adopters during the 1980s as their laws went into effect; but those gaps closed up as the 1990s and 2000s, respectively, passed and those cohorts enacted their laws.)

Figure 2

State RTC Decade of Enactment and Violent-Crime Rate

Violent Crime



The broad category of "violent crime" has played a growing role in the RTC literature recently, as we will explore in depth in sections below. Here, it is worth pointing out some conceptual difficulties of turning to overall violent crime when there is no clear effect on homicides.

A traditional pro-gun-control talking point is that the presence of guns turns run-of-the-mill violence and criminal activity into homicides by introducing a highly lethal tool into tense situations. This is why some studies have even controlled for violent-crime rates when analyzing whether gun ownership increases homicide specifically.[9] It would be odd for the opposite to be true—that violent crime, but not homicides, would rise thanks to laxer gun laws. Further, while most homicides in the U.S. are committed with guns, most violent crimes in general are not.[10]

Perhaps most daunting of all, while homicides are well measured because it is hard for authorities to ignore a dead body, other violent crimes (the overwhelming majority of the total) make it into the FBI data only if they are reported to police, properly documented (which became easier as agencies implemented and improved their computer systems for this task), and then reported to the federal government. Even the broadest national trends in the FBI's violent-crime data have not always matched similar estimates based on victimization surveys,[11] and a comparison of the two sources at the state level fared little better in a recent Bureau of Justice Statistics–funded study.[12] (Survey-based estimates for states with similar police-reported rates "can be very different," the authors noted.) If violent-crime estimates are dubious even in the broadest and biggest ways, it is difficult to put much stock into subtler differences such as the ones in the chart above.

Although statistics on crime by concealed carriers are disputed—permit revocations are rare and typically do not involve gun crimes,[13] but revocations do not always happen when they should[14]—there is general agreement that, because legal carry is limited to those without serious criminal records, carriers are a relatively law-abiding group.[15] Data on gun thefts from permit holders are also lacking, but some cities have undeniably had problems with guns being stolen from cars, which had often been left unlocked.[16]

Meanwhile, the two sides of the debate have gone to great lengths to compile anecdotes illustrating what they see as typical effects of concealed carry, with "concealed carry killers"[17] from one side and "armed citizens" stopping crime from the other.[18] These compilations can be worth reading for a sense of how RTC laws can play out in violent circumstances. If nothing else, they would seem to prove that RTC has some effect on crime. The question is whether we can measure that effect with any confidence.

A More Formal Approach

For decades, social scientists have done a more formal version of the trend-line exercises outlined in the previous section. It is a combination of two very commonsense ways of asking how a policy turned out.

One of those ways is the before-and-after comparison: After the policy went into effect, did crime go up or down in the state that implemented it? The big limitation of this approach is that sometimes crime goes up or down everywhere at once, not just in states that changed their laws. For example, crime fell basically everywhere in the U.S. between 1993 and 1999—so it means little if, say, Maine implemented some kind of crime policy in 1993 and then saw crime drop.

The other commonsense way to study whether a policy worked is the cross-sectional analysis: Do places with a policy have higher or lower crime rates than the places without it? This suffers from a similar limitation. Many policies are disproportionately implemented by certain types of states—such as red states, or urban states, or states in the West—and different types of states tend to have different crime rates, even aside from their policy decisions. If Maine has a lower homicide rate than Louisiana today, we should not be too quick to credit a policy that Maine enacted in 1993—especially if Maine also had a lower homicide rate than Louisiana for decades before the policy went into effect.

By combining the before-and-after comparison with the cross-sectional analysis, we can overcome these limitations. Known as a "difference-in-difference" design, the idea is to take account of national trends and underlying cross-sectional differences in a single framework. In this type of analysis, a law that Maine passed in 1993 does not earn praise for lowering crime unless crime declines faster in Maine than it does in the rest of the country after the law goes into effect. It cannot take credit for Maine's overall low crime rate or the nationwide 1990s crime decline.

A common way of implementing this concept is the "two-way fixed-effects" (TWFE) model. In a right-to-carry study, "state fixed effects" might control for the fact that some states, like Louisiana, almost always have higher crime rates than other states, such as Maine. And "year fixed effects" control for broad national trends, such as crime rising from 1960 through the early 1990s and declining thereafter.

With those state-to-state and year-to-year patterns statistically removed from the data, we can ask a more precise question: When a law is in effect, does the crime rate tend to be higher or lower than we would normally expect for the state that enacted it, after accounting for overall crime trends in the country at large? And if we are worried about other variables throwing off the analysis—e.g., maybe alcohol consumption happened to rise at the same time the law went into effect—we can include those variables in the model, too.

A Quarter-Century of Debate

The empirical debate over RTC began in earnest in the mid-to-late 1990s, with the publication of "Crime, Deterrence, and Right-to-Carry Concealed Handguns," a study from John R. Lott, Jr. and David B. Mustard that appeared in the *Journal of Legal Studies* in 1997 and had previously circulated as a working paper.[19] The paper used then-cutting-edge statistical techniques and involved massive amounts of work in assembling the data set (including calling officials in every state to gather numbers that were not publicly available). Its abstract makes clear why it made such a splash:

> Using cross-sectional time-series data for U.S. counties from 1977 to 1992, we find that allowing citizens to carry concealed weapons deters violent crimes, without increasing accidental deaths. If those states without right-to-carry concealed gun provisions had adopted them in 1992, county- and state-level data indicate that approximately 1,500 murders would have been avoided yearly. Similarly, we predict that rapes would have declined by over 4,000, robbery by over 11,000, and aggravated assaults by over 60,000. We also find criminals substituting into property crimes involving stealth, where the probability of contact between the criminal and the victim is minimal. Further, higher arrest and conviction rates consistently reduce crime. The estimated annual gain from all remaining states adopting these laws was at least $5.74 billion in 1992. The annual social benefit from an additional concealed handgun permit is as high as $5,000.

Adopting RTC appeared to reduce murders nearly 8% and to reduce violent crime in general about 5%. Lott followed up with the provocatively titled book *More Guns, Less Crime* in 1998.[20]

It wasn't long before competing analyses appeared. Ian Ayres and John J. Donohue III claimed to "shoot down"[21] the more guns, less crime hypothesis in the early 2000s, publishing results suggestive of crime increases, if anything, rather than reductions; Donohue celebrated "the final bullet"[22] in the hypothesis shortly thereafter. Michael D. Maltz and Joseph Targonski, meanwhile, raised serious concerns about the county-level crime data that the original Lott and Mustard paper used (though later analyses included state-level results, too).[23]

Study after study appeared, reaching differing conclusions and ultimately facilitating two major reviews of the literature. The first, from a National Research Council panel in 2005,[24] found that "in light 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 are many more law changes than in the earlier period), and (c) the imprecision of some results, it is impossible to draw strong conclusions from the existing literature on the causal impact of these laws." The prominent political scientist James Q. Wilson penned an unusual dissent from his co-panelists, writing that "the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate, though their effect on other crimes is ambiguous."

Much more recently, the RAND Corporation undertook a similar review.[25] "Shall-issue concealed-carry laws have uncertain effects on total homicides, firearm homicides, robberies, assaults, and rapes," it concluded.

According to the RAND review, there is only one outcome for which we have maybe learned something from 20-plus years of research: violent crime in the aggregate. There is "limited" evidence that RTC increases this outcome. The reasoning behind this classification is worth quoting in full:

> Two [high-quality] 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.

One study finding an increase in total violent crime—Donohue, Aneja, and Weber's oft-cited 2019 analysis[26]—has provoked direct criticism from other researchers. Two papers, one from William English[27] and the other from Carlisle E. Moody and Thomas B. Marvell,[28] have shown that the results disappear when the model is changed in defensible ways.

Over time, "more guns, less crime" results have become rare, and "more guns, more crime" results have become more common.[29] But that result remains hotly debated—and much of the most recent evidence indicates that, for all the bickering, RTC laws just might not matter that much.

Researcher Degrees of Freedom

How is it that everyday citizens have been wandering around American states with concealed handguns for decades, and we still are not sure what effect they have?

RTC papers tend to overlap strongly in terms of the data they rely on and the statistical techniques they employ. But that overlap is far from complete, as researchers have many subjective decisions to make as they decide how to set up any analysis of crime data. It is an example of what has been dubbed "researcher degrees of freedom." In this case, even restricting ourselves to the traditional TWFE studies, sources of variation include:

* Set of control variables. Researchers generally agree on broad categories of variables to include when studying fluctuations in crime rates—demographics, economics, law enforcement, etc.—but not on exactly how to do so. For example, to account for economic trends, should one control for unemployment rates, incomes, poverty rates, or some combination thereof?[30] Some control variables would obviously be desirable but would be difficult to implement, such as a measure of policing strategies and not just manpower.

* State- or county-level data? Or even cities? RTC laws are enacted at the state level, and state-level data tend to be higher quality than more local numbers, but smaller geographies can produce insights as well. For instance, do criminals near state borders shift their activity toward

non-RTC states out of fear of encountering armed civilians? Do counties with higher permitting rates see bigger changes in crime after an RTC law passes? What effect do these laws have on major cities specifically?

* Time period. As time passes, more years' worth of data become available. In general, the more data the better—but not always. For example, in states that adopted right-to-carry in the 1980s, crime trend data from 30 years later may not say much about the laws' effects. In general, studies focusing on earlier periods (such as the original Lott/Mustard paper) are more likely to find that right-to-carry reduces crime, while studies restricted to later periods (including some analyses in the Donohue, Aneja, and Weber paper, as well as a study of 10 laws, including right-to-carry, by Michael Siegel et al.)[31] often find crime increases.

* Technical aspects of how the analysis is run. Without going into gory detail, various types of regressions can be run on the same underlying crime data, and they do not always give the same results. For much of the history of this literature, it was standard to run "ordinary least squares" regressions on the logarithms of per-capita crime rates. Other techniques, however, can be used to analyze raw counts of crimes (with the role of population accounted for elsewhere in the formula). There is also debate over when logarithms are necessary and when they are not.

* Measuring trends. Year fixed effects control for overall nationwide trends. But some researchers also control for state-specific trends, in order to account for situations where a state's crime rates were rising or falling relative to the rest of the country, even before their RTC law went into effect. Some studies also phase in the effect of the law over time, rather than implementing it all at once when the statute goes into effect.

* Weighting states by population. When studying whether right-to-carry laws affect crime rates, should trends in Montana be given equal weight to trends in Texas, despite the more than 25-fold difference in population between the two? Giving bigger states greater weight has long been standard in this literature, and there are good reasons to do so. As RAND researchers have noted, if all states are given equal weight, "a death in Wyoming has nearly 100 times the effect on [the results] as a death in California, and the sample is unrepresentative of the nation in several ways (e.g., the analyses are conducted in a sample that is far more rural and white than the country actually is). This could lead to biases in estimates if either the probability of implementing the law or the effect of the law is associated with state characteristics."[32] However, in their 2019 paper, Moody and Marvell[33] argued that there are technical issues with weighting by population, and they noted that large states dominate the analysis with weights. William English has approvingly cited Moody and Marvell's arguments, adding that states are the "relevant unit of analysis" because, at a "conceptual level, we want to know how RTC laws affect crime rates in states that adopt them versus states that don't."[34]

Modern Methods

A lot has changed since 1997. Statistical methods always evolve, but in the past few years especially, serious problems with the now-classic TWFE model have come to light. This kind of model can behave in bizarre ways—particularly if a policy is implemented in different places at different times and has effects that vary across place and time—for somewhat complicated reasons. The issues stem from the fact that these models, in effect, compare each state with a control group made up of all other states, including states that are under the ongoing and potentially shifting influence of the treatment.[35] These concerns are acute in the RTC context: different RTC states have very different rules and permitting rates, and within any state that enacts RTC, the number of residents with permits tends to grow over time.

There is broad agreement that a different approach is needed, and there are several proposals on how to address the old models' problems. In general, the newer models are more careful in terms of how they put together control groups for states that change their laws, but they vary in terms of how they do this and how they weigh the various state-to-state and year-to-year comparisons that can be made.

To throw out the canonical TWFE model is to throw out much of the RTC literature. But there have been a handful of attempts to study the topic with other methods, and it's worth taking a look at several. Once again, different analyses have produced rather different results.

The aforementioned Donohue, Aneja, and Weber paper[36] serves as a bridge to the newer methods because, in addition to presenting some classic TWFE models, it offers results based on a technique called "synthetic controls." Rather than comparing trends in states that change their laws with trends in all other states, this method puts together a custom control group for every treated state—the state is compared with a mix of other states that are similar in terms of previous crime trends, demographics, and so on, though these combinations are not always intuitively a good match. (For instance, Texas is compared with an imaginary state that is nearly 60% California, plus about one-third Wisconsin and one-tenth Nebraska.[37] Nine-tenths of Texas, in other words, is represented either by a West Coast blue state, or by an Upper Midwestern purple state.) Donohue et al.'s application of this technique suggests that RTC increases overall violent crime. The Moody/Marvell and English studies mentioned above, however, employ variants of this method to produce different results.

The English study also offers what it calls a "significant methodological advance." Rather than just measuring the effect of RTC laws—which have traditionally been captured by a dummy variable, coded as 0 to indicate that a state lacked RTC in a given year and 1 to indicate that an RTC law was in effect—English measures the effect of permitting rates. (An earlier example of this approach, though looking only at county-level permitting in Florida, was a 2006 paper from Marvell and Tomislav Kovandzic.)[38] Given the wide variation in how often permits are granted in different RTC states, permit rates may do a better job of capturing the effect of carry laws than the mere existence of the law itself does. With this approach, English finds no effect of RTC on violent crime.

English's methods have limits as well. First, if RTC laws have a deterrent effect on criminals or a dampening effect on proactive law enforcement, those effects may partly come from the existence of the law and criminals' and cops' knowledge of it, rather than from the actual prevalence of permit holders. (Other mechanisms through which the laws could change crime rates, such as gun thefts, are clearly better indicated by permitting rates.) Second, and more crucially, comprehensive data on permit holders simply do not exist. English writes that there are "12 states [with] full data, 10 states with extensive data, and 20 states with sparse data, but with at least one data point for each state, comprising 408 state-years out of 1,067"; he thus had to statistically impute missing numbers for more than half the observations from treated states. States that do not even require permits any more will pose further issues to anyone seeking to extend English's analysis—which terminates in 2014, when few states had gone permitless—to newer data.

Yet another study, from researchers at the RAND Corporation,[39] used an unconventional model that had proved adept at detecting the effect of state-level crime policies in simulations.[40] Further breaking with convention, the study reported its results using "80% credibility intervals," as opposed to the traditional "95% confidence intervals," making it difficult to tell which results would be considered statistically insignificant under the normal rules. (Bigger intervals are more likely to include the possibility of zero effect, rendering a result statistically insignificant, and "confidence" and "credibility" intervals are calculated in different ways.)[41] Nonetheless, the RTC results were underwhelming, with even these 80% intervals generally leaving open the

possibility of zero effect as of the sixth year after implementation.[42] The credibility intervals for the "incident rate ratios" associated with RTC—where a result of 1 means no effect, a result of 1.05 means an increase of 5%, etc.—ran from 1.00 to 1.07 for overall gun deaths and gun suicides, from 0.98 to 1.09 for gun homicides, from 0.99 to 1.04 for total suicides, and from 0.97 to 1.06 for total homicides.

RAND's results for total homicide and suicide rates, though placed in an appendix, are important to account for some key dynamics that might play out when guns are not available. Someone unable to defend himself with a gun may be stabbed to death; someone unable to commit suicide with a gun may hang himself. Measures limited to gun deaths, by contrast, treat both these outcomes as a success for gun control.

This year, a new working paper from Donohue and another group of coauthors[43] buttressed Donohue's claims of increases in nonlethal violent crime, with analyses mindful of the recent revelations about TWFE models. The paper focuses only on crime reported by big-city agencies, and once again reaches the conclusion that violent crime rises under RTC while finding effects on homicide that are smaller and statistically insignificant. By drilling deeper into these agencies' data, the authors further find that the violent-crime increases are concentrated among crimes committed with firearms, that gun thefts rise dramatically, and that police become significantly less likely to clear violent crimes under RTC. (The result for violent crime with guns is large and statistically significant, the result for overall violent crime far smaller and borderline significant at best, and the result for non-gun crime even smaller and insignificant.) How this study will fare under review from skeptical researchers remains to be seen;[44] for now, it is one of the stronger arguments that RTC has bad effects, at least in some places.

Most recently, a study from Mitchell L. Doucette and coauthors[45] employs both synthetic controls and an effort to capture variation in shall-issue laws. Rather than relying on permitting rates, the authors measure restrictions put on permit issuance, such as denying permits to those with violent misdemeanor records in addition to felons. The results suggest that, overall, RTC increases aggravated gun assaults by about 10% and—oddly—non-gun homicides by 9%, while the results for gun homicides, gun and knife robberies, and knife assaults are statistically insignificant. Also notable is that "Shall-issue laws that prohibited violent misdemeanants from obtaining CCW permits were not associated with changes in gun-related violent crime," raising the possibility of a relatively easy "fix" for the problems reported in some studies.

A Few More Models for Good Measure

I sought to apply some newer methods to the state-level annual RTC data myself. I assembled a basic data set including criminal homicides and violent crimes from the FBI's Summary Reporting System data for 1981–2019,[46] along with RTC policies drawn from the RAND database, plus a simple collection of control variables similar to those found in other studies (though not a direct reproduction of any in particular): total population; 10 variables measuring the percentage of the state population[47] comprising every combination of five racial/ethnic categories (white, black, Hispanic, Asian, Native American) and two age categories (15–39 and 40 and over);[48] beer, wine, and spirits consumption per capita;[49] one-year lags of per-capita police-officer staffing[50] and state imprisonment;[51] and poverty and unemployment[52] rates. Results are reported in **Table 1** below.

In selecting these variables, I placed heavy emphasis on the ease of assembling the data set, with minimal merging of different sources and no use of imputation to guess missing values. This reduces effort; but it also simplifies the analysis and reduces the chance of basic errors.

I also excluded Washington, D.C., which is an extreme outlier in its murder rate. It is also missing incarceration data for half the period, suffered heavily from the crack epidemic, and has experienced unusual demographic trends as well (including a sizable increase in the non-Hispanic-white share of the population since the turn of the century).[53]

I see this as an illustrative exercise more than a formal study, showing how RTC fares under a basic analysis with updated methods and how results vary with different modeling choices. I ran TWFE models for reference (specifically "negative binomial" models that moot the issue of population weights),[54] as well as several other models that have been proposed to deal with the issues of TWFE models (including a method proposed by John Gardner,[55] as well as one from Liyang Sun and Sarah Abraham,[56] and one by Brantly Callaway and Pedro H. C. Sant'Anna).[57]

The key independent variable is whether a state has RTC in a given year, though, as William English has stressed, this does not account for variation in permitting rates. Each model is run with and without the control variables (with the exception of population, which is always included); the models without controls simply compare trends across states without considering other variables that may have changed at the same time, which can leave out important factors but also alleviate concerns that the control variables were cherry-picked. The Callaway-Sant'Anna models treat control variables differently from the others and do not run with the full set of controls, so they are presented only in their no-controls versions. The results can approximately be interpreted as proportional changes (i.e., –0.05 would mean that the law decreases crime by 5%).

Table 1

Estimates of the Impact of RTC Laws on Criminal Homicide

Outcome Variable: Criminal Homicide

Modeling Approach	Outcome Form	Result w/ Controls	Result w/o Controls
Negative Binomial w/TWFE	Count	–0.00	0.04
+ Sun-Abraham Interactions (ATT)	Count	0.03	0.11
Two-Stage DiD w/ Pop Weights	Log Rate	0.08	0.13
Two-Stage DiD, No Weights	Log Rate	0.1	0.03
Callaway-Sant'Anna Group ATT	Log Rate		0.05

Outcome Variable: Violent Crime

Modeling Approach	Outcome Form	Result w/ Controls	Result w/o Controls
Negative Binomial w/TWFE	Count	–0.03	0.05
+ Sun-Abraham Interactions (ATT)	Count	–0.02	0.14
Two-Stage DiD w/ Pop Weights	Log Rate	0.07	**0.27*** **
Two-Stage DiD, No Weights	Log Rate	0.09	**0.2***
Callaway-Sant'Anna Group ATT	Log Rate		**0.12***

* p < .05; ** p < .01; *** p < .001. ATT = average treatment effect on the treated. DiD = difference-in-difference. The control group for the Callaway-Sant'Anna models is set to "not yet treated."

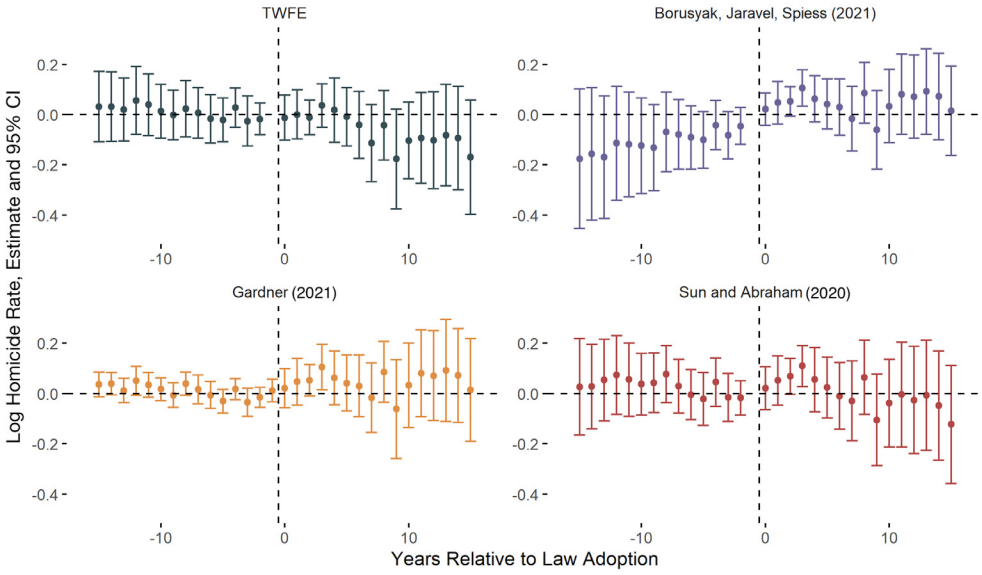
Certainly, there is little sign here that RTC laws meaningfully reduce crime. But the models with controls produce insignificant results with both positive and negative signs. Most of the control-free models produce insignificant results as well, though the results for violent crime in the two-stage and Callaway-Sant'Anna models are notable exceptions, and some of the positive insignificant results are large enough in magnitude to advise a hint of caution as well.

In addition, using the "did2s" package in R, I created "event-study" charts using various methods simultaneously, showing how the exact same data (in this case, log crime rates and the control variables listed above, with the pre-1981 adopters removed because some of these models don't handle "always-treated" groups) can generate different results (**Figure 3**). These charts also show trends before and after passage in adopting states, relative to trends elsewhere, which can reveal problematic "pre-trends" under way in adopting states even before passage. Here are some results with controls showing trends for adopting states within 15 years of passage; the Appendix has results for even more models without any controls whatsoever. (This is the simplest approach and allows more models to run. For violent crime, some of the no-controls models have obvious pre-trends—i.e., violent crime was rising before passage and simply continued rising thereafter—but two of them have steady pre-trends and crime increases after law adoption, mirroring some of Donohue's findings.)

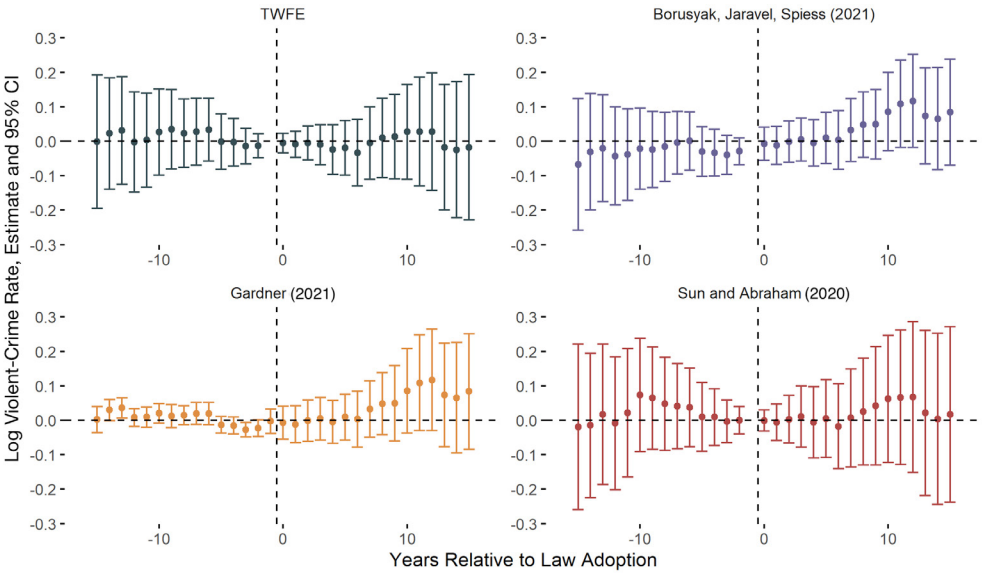
Figure 3

Event-Study Charts for the Impact of RTC Laws on Homicide and Violent-Crime Rates

Homicide Event Studies



Violent-Crime Event Studies



Source: Author's calculation based on the "did2s" package presented in Kyle Butts and John Gardner, "did2s: Two-Stage Difference-in-Differences," **arXiv, May, 20, 2022.**

Models include: John Gardner, "Two-Stage Differences In Differences," **technical paper, April 2021; Kirill Borusyak, Xavier Jaravel, and Jann Spiess,** "Revisiting Event Study Designs: Robust and Efficient Estimation," **arXiv, August 2021; Liyang Sun and Sarah Abraham,** "Estimating Dynamic Treatment Effects in Event Studies With Heterogeneous Treatment Effects," **SSRN working paper, Sept. 22, 2020.**

Given that I easily produced varying results with similar models and that numerous "researcher degrees of freedom" remain for others to exploit within all these techniques, we should have little hope that recent advances will resolve the RTC issue.

Conclusion: Next Steps for Reluctant May-Issue States

After decades of intense research, it remains difficult to say what impact right-to-carry has on crime. That in itself should be somewhat encouraging to the states that now must grant concealed-carry permits more freely.

The Supreme Court decision is rather limited: states may no longer insist on a special reason for a carry permit, but they still may erect large barriers. Even if RTC has had ill effects in states that have implemented it voluntarily—and often with untempered zeal, as the trend toward permitless carry reveals—that need not be the case in the states now being dragged into RTC kicking and screaming.

States can still require lengthy training. They can impose fairly high fees. They can designate many areas as sensitive and off-limits to gun-carriers. They can emphasize that private businesses are allowed to ban guns on their premises. They can ban leaving guns unattended in cars. In general, they can minimize the number of people who receive permits and discourage permit-holders

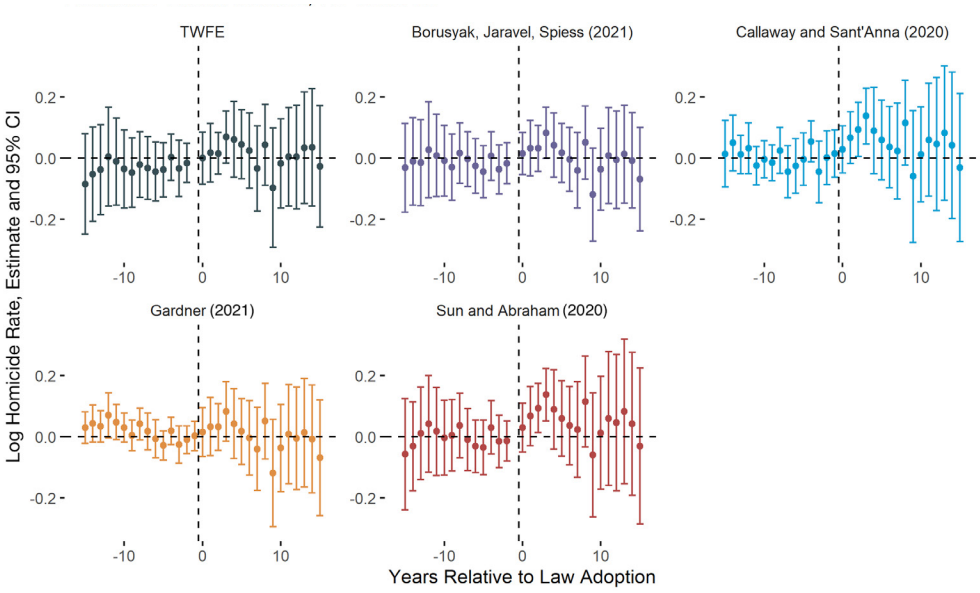
from carrying by making it hard to go about one's life armed. If you cannot take your gun on the subway, to your place of employment, or to your favorite restaurant, and you also cannot leave it in your car, you might as well just leave it at home.

Indeed, since the decision, the most dedicated may-issue states have rushed to do all this and more, thereby running a substantial risk that courts will strike down parts of their new regimes as unduly restrictive. In New York, for example, applicants must provide a list of their social-media accounts as part of the application process, which could create First Amendment difficulties; also, gun-carrying is presumptively banned at private businesses, permitted only if the business owner posts a sign explicitly saying that it is allowed.[58] California's attorney general aims to leverage the state's preexisting "good moral character" requirement to deny permits, favorably noting in a "Legal Alert" the policies of several local agencies in the state for assessing that criterion—including one barring those with any arrest "in the last 5 years, regardless of the disposition" and another promising "consideration of honesty, trustworthiness, diligence, reliability, respect for the law, integrity, candor, discretion, observance of fiduciary duty, respect for the rights of others, absence of hatred and racism, fiscal stability, profession-specific criteria such as pledging to honor the constitution and uphold the law, and the absence of criminal conviction."[59]

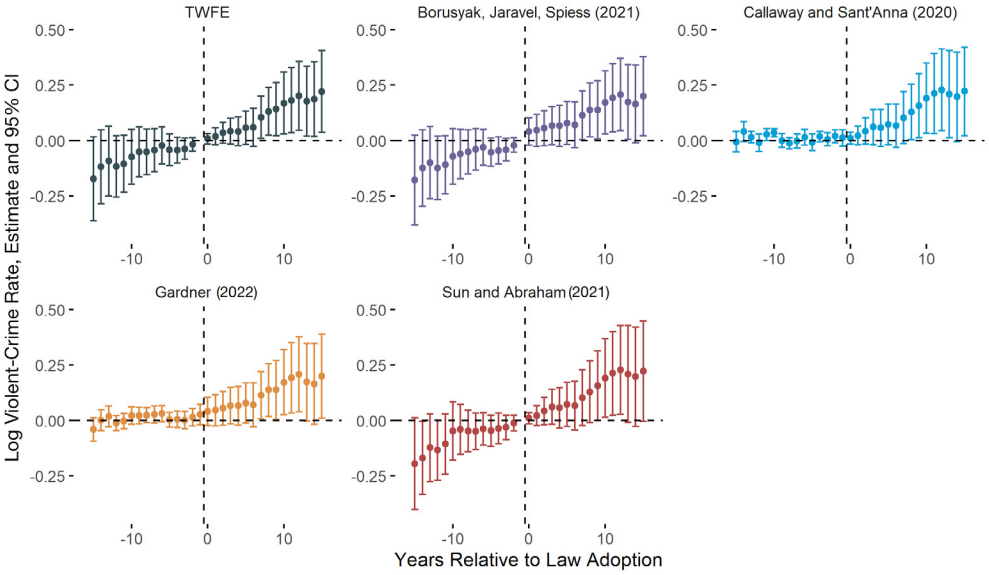
Future court decisions may curtail these more aggressive moves. Ultimately, these states will still be run by antigun politicians with a lot of freedom to pursue antigun policies.

Appendix: Event-Study Charts, No Controls

Homicide Event Studies, No Controls



Violent-Crime Event Studies, No Controls



Source: Author's calculation based on the "did2s" package presented in Kyle Butts and John Gardner, "did2s: Two-Stage Difference-in-Differences," **arXiv, May, 20, 2022.**

Models include: John Gardner, "Two-Stage Differences In Differences," **technical paper, April 2021; Brantly Callaway and Pedro H.C. Sant'Anna, "Difference-In-Differences with Multiple Time Periods,"** *Journal of Econometrics* **225, no. 2 (December 2021): 200–230; Kirill Borusyak, Xavier Jaravel, and Jann Spiess, "**Revisiting Event Study Designs: Robust and Efficient Estimation**," arXiv, August 2021; Liyang Sun and Sarah Abraham,** "Estimating Dynamic Treatment Effects in Event Studies With Heterogeneous Treatment Effects," **SSRN working paper, Sept. 22, 2020.**

Endnotes

1 See Steven Pinker, *The Better Angels of Our Nature* (New York: Penguin, 2012).

2 "What You Need to Know About Open Carry in America," *Trace,* July 18, 2016.

3 Details and download link available at Samantha Cherney et al., "Development of the RAND State Firearm Law Database and Supporting Materials," RAND Corporation., June 28, 2022.

4 U.S. Concealed Carry Association (USCCA), "Constitutional Carry / Unrestricted / Permitless Carry."

5 Under Rhode Island law, for instance, local authorities "shall" issue permits, but only if the applicant "has good reason to fear an injury to his or her person or property or has any other proper reason for carrying a pistol or revolver, and … he or she is a suitable person to be so licensed," and the attorney general "may" issue permits as well. USCCA classifies it as shall-issue, but RAND considers it may-issue. See USCCA, "Summary of Rhode Island Gun Laws"; RAND Corporation., "State Firearm Law Navigator."

6 Wisconsin Department of Justice, "Training Requirements."

7 Randy Barnett, "A Minor Impact on Gun Laws but a Potentially Momentous Shift in Constitutional Method," *SCOTUSblog,* June 27, 2022.

8 Each group's rate is calculated by dividing its total number of homicides for a year by its total population in that year; homicide and population data come from the Uniform Crime Reports, while gun laws are from the RAND Corporation's database.

9 See, e.g., Matthew Miller, David Hemenway, and Deborah Azrael, "State-Level Homicide Victimization Rates in the US in Relation to Survey Measures of Household Firearm Ownership, 2001–2003," *Social Science & Medicine* 64, no. 3 (February 2007): 656–64, which controls for aggravated-assault and robbery rates; together these constitute the vast majority of the overall violent-crime category. See also Federal Bureau of Investigation (FBI), "Crime in the United States 2019," table 1.

10 FBI, "Crime in the United States 2019," Violent Crime.

11 Janet L. Lauritsen, Maribeth L. Rezey, and Karen Heimer, "When Choice of Data Matters: Analyses of U.S. Crime Trends, 1973–2012," *Journal of Quantitative Criminology* 32, no. 3 (Sept. 1, 2016): 335–55.

12 Dan Liao, Stephanie Zimmer, and Marcus Berzofsky, "Small Area Estimation for the National Crime Victimization Survey: A Guide for Data Processing and Estimation Procedures," Office of Justice Programs, April 2021.

13 John R. Lott and Rujun Wang, "Concealed Carry Permit Holders Across the United States: 2021," SSRN Scholarly Paper, Oct. 6, 2021.

14 John Agar, "Ready, Aim, Misfire: Analysis Finds Mistakes, Misunderstanding in Gun Reports," *mlive,* June 27, 2011.

15 See, e.g., Amy Sherman, "Donald Trump Minimizes Number of Revoked Gun Permits in Florida," *Politifact*, June 2, 2016. Gary Kleck and Daniel Webster, from opposing sides of the gun debate, agree that legal carriers are mostly law-abiding.

16 Brian Freskos, "Guns Are Stolen in America Up to Once Every Minute. Owners Who Leave Their Weapons in Cars Make It Easy for Thieves," *Trace*, Sept. 21, 2016.

17 Violence Policy Center, "Concealed Carry Killers."

18 National Rifle Association–Institute for Legislative Action, "Armed Citizen."

19 John R. Lott, Jr. and David B. Mustard, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," *Journal of Legal Studies* 26, no. 1 (January 1997): 1–68.

20 John R. Lott, Jr., *More Guns, Less Crime: Understanding Crime and Gun-Control Laws* (Chicago: University of Chicago Press, 1998).

21 Ian Ayres and John J. Donohue III, "Shooting Down the 'More Guns, Less Crime' Hypothesis," *Stanford Law Review* 55, no. 4 (April 2003): 1193–1312.

22 John J. Donohue III, "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis," *Criminology & Public Policy* 2, no. 3 (July 2003): 397–410.

23 Michael D. Maltz and Joseph Targonski, "A Note on the Use of County-Level UCR Data," *Journal of Quantitative Criminology* 18, no. 3 (September 2002): 297–318.

24 National Research Council, *Firearms and Violence: A Critical Review*, ed. Charles F. Wellford, John V. Pepper, and Carol V. Petrie (Washington, DC: National Academies Press, 2005).

25 I rely here on the 2020 update of this analysis: RAND Corp., "Effects of Concealed-Carry Laws on Violent Crime," Apr. 22, 2020.

26 John J. Donohue III, Abhay Aneja, and Kyle D. Weber, "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis," *Journal of Empirical Legal Studies* 16, no. 2 (April 2019): 198–247 (available earlier as National Bureau of Economic Research, working paper no. 23510, June 2017).

27 William English, "The Right to Carry Has Not Increased Crime: Improving an Old Debate Through Better Data on Permit Growth over Time," Georgetown McDonough School of Business Research Paper no. 3887151, July 14, 2021.

28 Carlisle E. Moody and Thomas B. Marvell, "Do Right to Carry Laws Increase Violent Crime? A Comment on Donohue, Aneja, and Weber," *Econ Journal Watch* 16, no. 1 (March 2019): 84–96.

29 This paper notes the pattern and suggests that stricter rules in more recent RTC states might be to blame: Carlisle E. Moody and John R. Lott, Jr., "Do Right-to-Carry Concealed Weapons Laws Still Reduce Crime?" *Academia Letters,* February 2022.

30 For a handy contrast of the controls included in Lott/Mustard and those in Donohue, Aneja, and Weber, see the 2017 NBER working paper version of the latter at p. 19.

31 Michael Siegel et al., "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," *Journal of General Internal Medicine* 34, no. 10 (Oct. 1, 2019): 2021–28.

32 Terry L. Schell, Beth Ann Griffin, and Andrew R. Morral, "Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths: A Simulation Study," RAND Corporation., Dec. 10, 2018.

33 Moody and Marvell, "Do Right to Carry Laws Increase Violent Crime?"

34 English, "The Right to Carry Has Not Increased Crime."

35 For a more concrete example, if a state's law changes in 1995, and as a result crime gradually increases each year until 2000, that state will still be part of the control group for a state that changes its law in 1997—baking in the assumption that crime should rise in that second state, too, without that increase being counted as an effect of the law. Kyle Butts and John Gardner have noted two situations where problems like this do not occur, though these are rare in practice. One is where the treatment effect doesn't vary across place and time; the other is where all units are treated at the same time (e.g., if every state to pass RTC did so simultaneously), in which case the model will accurately estimate the average effect even if the effect varies ("{did2s}: Two-Stage Difference-in-Differences," *arXiv*, May 20, 2022).

As Scott Cunningham has written, the assorted solutions to this issue work by "avoiding using already-treated units as controls. Avoiding those comparisons introduces forms of sample selection, though, which must be accounted for. Authors have different strategies to accomplish this, and most of those strategies involve building to a larger ATT [average effect of treatment on the treated] through a weighting scheme based on estimating smaller building block ATTs, like the group-time ATT or estimated individual treatment effect itself." See "Two Stage DiD and Taming the DiD Revolution," *Causal Inference* Substack (blog). For reasonably digestible primers on these issues, see also Andrew Baker's slideshow "Difference-in-Differences: What It DiD?" May 25, 2020.

36 Donohue, Aneja, and Weber, "Right-to-Carry Laws and Violent Crime."

37 Ibid., p. 227.

38 Tomislav V. Kovandzic and Thomas B. Marvell, "Right-to-Carry Concealed Handguns and Violent Crime: Crime Control Through Gun Decontrol?" *Criminology & Public Policy* 2, no. 3 (March 2006): 363–96.

39 Terry L. Schell et al., "Changes in Firearm Mortality Following the Implementation of State Laws Regulating Firearm Access and Use," *Proceedings of the National Academy of Sciences* (*PNAS*) 117, no. 26 (June 15, 2020): 14906–10.

40 In a previous paper, three of the four same authors had started with real-life crime data, randomly assigned states to "enact laws" in various years, and then changed the crime data to reflect an effect of the simulated laws (or not, when simulating ineffective laws). Then they ran models on the resulting data, revealing which models were best able to find the effects that had been planted and the least likely to find an effect where there was none. The winner was a negative-binomial model with year fixed effects, an autoregressive term, and "change coding" to denote the period of time over which a law's effect would be expected to phase in—a combination that no previous studies had used. Schell, Griffin, and Morral, "Evaluating Methods to Estimate the Effect of State Laws on Firearm Deaths."

41 Luiz Hespanhol et al., "Understanding and Interpreting Confidence and Credible Intervals Around Effect Estimates," *Brazilian Journal of Physical Therapy* 23, no. 4 (2019): 290–301.

42 The authors also report the results for other policies, as well as combinations of policies, that are outside the scope of this brief.

43 John J. Donohue et al., "More Guns, More Unintended Consequences: The Effects of Right-to-Carry on Criminal Behavior and Policing in US Cities," NBER, working paper no. 30190, June 2022.

44 For early comments from Moody and Lott, see "Comments on the Latest Donohue Paper: 'More Guns, More Unintended Consequences,' " Crime Prevention Research Center, June 29, 2022.

45 Mitchell L. Doucette et al., "Impact of Changes to Concealed Carry Weapons Laws on Fatal and Nonfatal Violent Crime, 1980–2019," *American Journal of Epidemiology* (September 2022).

46 FBI, "Crime Data Explorer," "estimated_crimes_1979_2020" spreadsheet (from "Documents and Downloads" section of site).

47 Compiled from the State Intercensal Datasets, for 1981–89, and from the National Center for Health Statistics, "Bridged-Race Population Estimates," for 1990–2020.

48 Children of all races, who rarely commit violent crimes (and are relatively unlikely to be victims of such crimes as well) thus act as an "excluded category." This prevents all the demographic percentages from adding up to 100% for each observation (in which case, the regression would not run, owing to perfect collinearity). I find this categorical scheme a good balance between comprehensively covering the population—capturing both age and race, though not sex, as in much of the rest of the literature—and not adding too many highly correlated control variables.

49 Numbers compiled from Megan E. Slater and Hillel R. Alpert, "Surveillance Report #117: Apparent per Capita Alcohol Consumption: National State, and Regional Trends, 1977–2019," National Institute on Alcohol Abuse and Alcoholism, April 2021.

50 FBI, "Crime Data Explorer," "pe_1960_2020" spreadsheet (from "Documents and Downloads" section of site).

51 Bureau of Justice Statistics, "Corrections Statistical Analysis Tool," Advanced Query (total year-end population by state for all years).

52 Poverty data from U.S. Census Bureau, "Number of Poor and Poverty Rate by State: 1980 to 2021." Unemployment data from Iowa State University Community Indicators Program, "Annual Unemployment Rates by State"; 2019 data from Bureau of Labor Statistics, "Unemployment Rates for States."

53 Author's analysis of the census data previously discussed and also confirmed in the American Community Survey microdata; the share rose from about a quarter at the turn of the century to more than a third in recent years. See also Mychael Schnell, "DC Only Place Where Share of White Population Increased Last Year: Census," *The Hill,* Aug. 12, 2021.

54 It models counts rather than rates and automatically gives greater weight to observations with higher counts. I control for population rather than using population as an "offset" so that the model can fit the appropriate coefficient—states whose populations increase, say, 10% do not necessarily see their crime counts increase by exactly 10% in turn. Greater density might drive up crime, for example, or states might grow by being economically prosperous and attracting upwardly mobile, low-crime new residents.

55 John Gardner, "Two-Stage Differences in Differences," *arXiv*, July 12, 2022.

56 Liyang Sun and Sarah Abraham, "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects," *Journal of Econometrics* 225, no. 2 (December 2021): 175–99.

57 Brantly Callaway and Pedro H. C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics* 225, no. 2 (December 2021): 200–230.

58 Greg B. Smith, "New York Passed a Law Defining No-Guns Zones. Where Exactly? That's Complicated," *The City,* July 13, 2022.

59 California Department of Justice, Office of Attorney General, Legal Alert, no. OAG-2022-02, "U.S. Supreme Court's Decision in *New York State Rifle & Pistol Association v. Bruen, No. 20-843,*" June 24, 2022.