XAVIER BECERRA
Attorney General of California
MARK R. BECKINGTON
Supervising Deputy Attorney General
R. MATTHEW WISE, SBN 238485
Deputy Attorney General
1300 I Street, Suite 125
P.O. Box 944255
Sacramento, CA 94244-2550
Telephone: (916) 210-6046
Fax: (916) 324-8835
E-mail: Matthew.Wise@doj.ca.gov
Attorneys for Defendant Attorney General Xavier Becerra

IN THE UNITED STATES DISTRICT COURT

FOR THE EASTERN DISTRICT OF CALIFORNIA

MARK BAIRD and RICHARD GALLARDO, Plaintiffs, **v.** **XAVIER BECERRA, in his official capacity as Attorney General of the State of California, and DOES 1-10,** Defendant.	Case No. 2:19-cv-00617-KJM-AC **DECLARATION OF R. MATTHEW WISE IN SUPPORT OF DEFENDANT'S OPPOSITION TO PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION** Date: September 6, 2019 Time: 10:00 a.m. Courtroom: 3 Judge: Kimberly J. Mueller Trial Date: None set Action Filed: April 9, 2019

I, R. Matthew Wise, declare as follows:

1. I am a Deputy Attorney General in the California Attorney General's Office. I represent Defendant Xavier Becerra, in his official capacity as Attorney General of California, in the above-captioned matter. I have personal knowledge of each fact stated in this declaration, and if called as a witness I could and would testify competently thereto.

2. Attached hereto as Exhibit 1 is a true and correct copy of "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis," by Stanford Law Professor John J. Donohue III, et al., an article published in the April 2019 issue of the Journal of Empirical Legal Studies.

3. Attached hereto as Exhibit 2 is a true and correct copy of "RTC Laws Increase Violent Crime: Moody and Marvell Have Missed the Target," by Donohue, et al., an article published in the March 2019 issue of Econ Journal Watch.

4. Attached hereto as Exhibit 3 is a true and correct copy of "Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States," by Boston University Professor of Public Health Michael Siegel, et al., an article published in the December 2017 issue of the American Journal of Public Health.

I declare under penalty of perjury that the foregoing is true and correct. Executed on August 2, 2019, at Sacramento, California.

/s/ R. Matthew Wise
R. MATTHEW WISE

SA2019101934
13961031.docx

EXHIBIT 1

JOURNAL OF
EMPIRICAL LEGAL STUDIES

Journal of Empirical Legal Studies
Volume 16, Issue 2, 198–247, April 2019

Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis

*John J. Donohue, Abhay Aneja, and Kyle D. Weber**

This article uses more complete state panel data (through 2014) and new statistical techniques to estimate the impact on violent crime when states adopt right-to-carry (RTC) concealed handgun laws. Our preferred panel data regression specification, unlike the statistical model of Lott and Mustard that had previously been offered as evidence of crime-reducing RTC laws, both satisfies the parallel trends assumption and generates statistically significant estimates showing RTC laws *increase* overall violent crime. Our synthetic control approach also finds that RTC laws are associated with 13–15 percent *higher* aggregate violent crime rates 10 years after adoption. Using a consensus estimate of the elasticity of crime with respect to incarceration of 0.15, the average RTC state would need to roughly double its prison population to offset the increase in violent crime caused by RTC adoption.

I. Introduction

For two decades, there has been a spirited academic debate over whether "shall-issue" concealed carry laws (also known as right-to-carry or RTC laws) have an important impact on crime. The "More Guns, Less Crime" hypothesis originally articulated by John Lott and David Mustard (1997) claimed that RTC laws decreased violent

---

*Address correspondence to John J. Donohue, Stanford Law School, 559 Nathan Abbott Way, Stanford, CA 94305; email: donohue@law.stanford.edu. Abhay Aneja, Haas School of Business, 2220 Piedmont Avenue, Berkeley, CA 94720; email: aaneja@law.stanford.edu; Kyle D. Weber, Columbia University, 420 W. 118th Street, New York, NY 10027; email: kdw2126@columbia.edu.

We thank Phil Cook, Dan Ho, Stefano DellaVigna, Rob Tibshirani, Trevor Hastie, Stefan Wager, Jeff Strnad, and participants at the 2011 Conference of Empirical Legal Studies (CELS), 2012 American Law and Economics Association (ALEA) Annual Meeting, 2013 Canadian Law and Economics Association (CLEA) Annual Meeting, 2015 NBER Summer Institute (Crime), and the Stanford Law School faculty workshop for their comments and helpful suggestions. Financial support was provided by Stanford Law School. We are indebted to Alberto Abadie, Alexis Diamond, and Jens Hainmueller for their work developing the synthetic control algorithm and programming the Stata module used in this paper and for their helpful comments. The authors would also like to thank Alex Albright, Andrew Baker, Jacob Dorn, Bhargav Gopal, Crystal Huang, Mira Korb, Haksoo Lee, Isaac Rabbani, Akshay Rao, Vikram Rao, Henrik Sachs and Sidharth Sah who provided excellent research assistance, as well as Addis O'Connor and Alex Chekholko at the Research Computing division of Stanford's Information Technology Services for their technical support.

crime (possibly shifting criminals in the direction of committing more property crime to avoid armed citizens). This research may well have encouraged state legislatures to adopt RTC laws, arguably making the pair's 1997 paper in the *Journal of Legal Studies* one of the most consequential criminological articles published in the last 25 years.

The original Lott and Mustard paper as well as subsequent work by John Lott in his 1998 book *More Guns, Less Crime* used a panel data analysis to support the theory that RTC laws reduce violent crime. A large number of papers examined the Lott thesis, with decidedly mixed results. An array of studies, primarily those using the limited data initially employed by Lott and Mustard for the period 1977–1992 and those failing to adjust their standard errors by clustering, supported the Lott and Mustard thesis, while a host of other papers were skeptical of the Lott findings.[1]

It was hoped that the 2005 National Research Council report *Firearms and Violence: A Critical Review* (hereafter the NRC Report) would resolve the controversy over the impact of RTC laws, but this was not to be. While one member of the committee—James Q. Wilson—did partially endorse the Lott thesis by saying there was evidence that murders fell when RTC laws were adopted, the other 15 members of the panel pointedly criticized Wilson's claim, saying that "the scientific evidence does not support his position." The majority emphasized that the estimated effects of RTC laws were highly sensitive to the particular choice of explanatory variables and thus concluded that the panel data evidence through 2000 was too fragile to support any conclusion about the true effects of these laws.

This article answers the call of the NRC Report for more and better data and new statistical techniques to be brought to bear on the issue of the impact of RTC laws on crime. First, we revisit the state panel data evidence to see if extending the data for an additional 14 years, thereby providing additional crime data for prior RTC states as well as on 11 newly adopting RTC states, offers any clearer picture of the causal impact of allowing citizens to carry concealed weapons. We distill from an array of different panel data regressions for various crime categories for two time periods using two major sets of explanatory variables—including our preferred specification (DAW) and that of Lott and Mustard (LM)—a subset of regressions that satisfy the critical parallel trends assumption. All the statistically significant results from these regressions show RTC laws are associated with *higher* rates of overall violent crime, property crime, or murder.

Second, to address some of the weaknesses of panel data models, we undertake an extensive synthetic control analysis in order to present the most complete and robust

---

[1]In support of Lott and Mustard (1997), see Lott's 1998 book *More Guns, Less Crime* (and the 2000 and 2010 editions). Ayres and Donohue (2003) and the 2005 National Research Council report *Firearms and Violence: A Critical Review* dismissed the Lott/Mustard hypothesis as lacking credible statistical support, as did Aneja et al. (2011) (and Aneja et al. (2014) further expanding the latter). Moody and Marvell (2008) and Moody et al. (2014) continued to argue in favor of a crime-reducing effect of RTC laws, although Zimmerman (2014) and McElroy and Wang (2017) find that RTC laws *increase* violent crime and Siegel et al. (2017) find RTC laws increase murders, as discussed in Section III.B.

results to guide policy in this area.[2] This synthetic control methodology—first introduced in Abadie and Gardeazabal (2003) and expanded in Abadie et al. (2010, 2014)—uses a matching methodology to create a credible "synthetic control" based on a weighted average of other states that best matches the prepassage pattern of crime for each "treated" state, which can then be used to estimate the likely path of crime if RTC-adopting states had not adopted an RTC law. By comparing the actual crime pattern for RTC-adopting states with the estimated synthetic controls in the postpassage period, we derive year-by-year estimates for the impact of RTC laws in the 10 years following adoption.[3]

To preview our major findings, the synthetic control estimate of the average impact of RTC laws across the 33 states that adopt between 1981 and 2007[4] indicates that violent crime is substantially higher after 10 years than would have been the case had the RTC law not been adopted. Essentially, for violent crime, the synthetic control approach provides a similar portrayal of RTC laws as that provided by the DAW panel data model and undermines the results of the LM panel data model. According to the aggregate synthetic control models—regardless of whether one uses the DAW or LM covariates—RTC laws led to increases in violent crime of 13–15 percent after 10 years, with positive but not statistically significant effects on property crime and murder. The median effect of RTC adoption after 10 years is 12.3 percent if one considers all 31 states with 10 years worth of data and 11.1 percent if one limits the analysis to the 26 states with the most compelling prepassage fit between the adopting states and their synthetic controls. Comparing our DAW specification findings with the results generated using placebo treatments, we are able to reject the null hypothesis that RTC laws have no impact on aggregate violent crime.

The structure of the article proceeds as follows. Section II begins with a discussion of the ways in which increased carrying of guns could either dampen crime (by thwarting or deterring criminals) or increase crime by directly facilitating violence or aggression by permit holders (or others), greatly expanding the loss and theft of guns, and burdening the functioning of the police in ways that diminish their effectiveness in controlling crime. We then show that a simple comparison of the drop in violent crime from

---

[2]Abadie et al. (2014) identify a number of possible problems with panel regression techniques, including the danger of extrapolation when the observable characteristics of the treated area are outside the range of the corresponding characteristics for the other observations in the sample.

[3]The accuracy of this matching can be qualitatively assessed by examining the root mean square prediction error (RMSPE) of the synthetic control in the pretreatment period (or a variation on this RMSPE implemented in this article), and the statistical significance of the estimated treatment effect can be approximated by running a series of placebo estimates and examining the size of the estimated treatment effect in comparison to the distribution of placebo treatment effects.

[4]Note that we do not supply a synthetic control estimate for Indiana, even though it passed its RTC law in 1980, owing to the fact that we do not have enough pretreatment years to accurately match the state with an appropriate synthetic control. Including Indiana as a treatment state, though, would not meaningfully change our results. Similarly, we do not generate synthetic control estimates for Iowa and Wisconsin (whose RTC laws went into effect in 2011) or for Illinois (2014 RTC law), because of the limited postpassage data.

1977–2014 in the states that have resisted the adoption of RTC laws is almost an order of magnitude greater than in RTC-adopting states (a 42.3 percent drop vs. a 4.3 percent drop), although a spartan panel data model with only state and year effects reduces the differential to 20.2 percent. Section III discusses the panel data results, showing that the DAW model indicates that RTC laws have increased violent and property crime, with weaker evidence that RTC laws increased homicide (but not non-gun homicide) over our entire data period, while both the DAW and the LM model provide statistically significant evidence that RTC laws have increased murder in the postcrack period.

The remainder of the article shows that, using either the DAW or LM explanatory variables, the synthetic control approach uniformly supports the conclusion that RTC laws lead to substantial increases in violent crime. Section IV describes the details of our implementation of the synthetic control approach and shows that the mean and median estimates of the impact of RTC laws show greater than double-digit increases by the 10th year after adoption. Section V provides aggregate synthetic control estimates of the impact of RTC laws, and Section VI concludes.

II. The Impact of RTC Laws: Theoretical Considerations and Simple Comparisons

A. Gun Carrying and Crime

1. Mechanisms of Crime Reduction

Allowing citizens to carry concealed handguns can influence violent crime in a number of ways, some benign and some invidious. Violent crime can fall if criminals are deterred by the prospect of meeting armed resistance, and potential victims or armed bystanders may thwart or terminate attacks by either brandishing weapons or actually firing on the potential assailants. For example, in 2012, a Pennsylvania concealed carry permit holder became angry when he was asked to leave a bar because he was carrying a weapon and, in the ensuing argument, he shot two men, killing one, before another permit holder shot him (Kalinowski 2012). Two years later, a psychiatric patient in Pennsylvania killed his caseworker, and grazed his psychiatrist before the doctor shot back with his own gun, ending the assault by wounding the assailant (Associated Press 2014).

The impact of the Pennsylvania RTC law is somewhat ambiguous in both these cases. In the bar shooting, it was a permit holder who started the killing and another who ended it, so the RTC law may actually have increased crime. The case of the doctor's use of force is more clearly benign, although the RTC law may have made no difference: a doctor who routinely deals with violent and deranged patients would typically be able to secure a permit to carry a gun even under a may-issue regime. Only a statistical analysis can reveal whether in aggregate extending gun carrying beyond those with a demonstrated need and good character, as shall-issue laws do, imposes or reduces overall costs.

Some defensive gun uses can be socially costly and contentious even if they do avoid a robbery or an assault. For example, in 1984, when four teens accosted Bernie Goetz on a New York City subway, he prevented an anticipated robbery by shooting all four, permanently paralyzing one.[5] In 2010, a Pennsylvania concealed carry holder argued that he used a gun to thwart a beating. After a night out drinking, Gerald Ung, a 28-year-old Temple University law student, shot a 23-year-old former star lacrosse player from Villanova, Eddie DiDonato, when DiDonato rushed Ung angrily and aggressively after an altercation that began when DiDonato was bumped while doing chin ups on scaffolding on the street in Philadelphia. When prosecuted, Ung testified that he always carried his loaded gun when he went out drinking. A video of the incident shows that Ung was belligerent and had to be restrained by his friends before the dispute became more physical, which raises the question of whether his gun carrying contributed to his belligerence, and hence was a factor that precipitated the confrontation. Ung, who shot DiDonato six times, leaving DiDonato partially paralyzed with a bullet lodged in his spine, was acquitted of attempted murder, aggravated assault, and possessing an instrument of crime (Slobodzian 2011). While Ung avoided criminal liability and a possible beating, he was still prosecuted and then hit with a major civil action, and the incident did impose significant social costs, as shootings frequently do.[6]

In any event, the use of a gun by a concealed carry permit holder to thwart a crime is a statistically rare phenomenon. Even with the enormous stock of guns in the United States, the vast majority of the time that someone is threatened with violent crime no gun will be wielded defensively. A five-year study of such violent victimizations in the United States found that victims reported failing to defend or to threaten the criminal with a gun 99.2 percent of the time—this in a country with 300 million guns in civilian hands (Planty & Truman 2013). Adding 16 million permit holders who often dwell in low-crime areas may not yield many opportunities for effective defensive use for the roughly 1 percent of Americans who experience a violent crime in a given year, especially since criminals can attack in ways that preempt defensive measures.[7]

2. Mechanisms of Increasing Crime

Since the statistical evidence presented in this article suggests that the benign effects of RTC laws are outweighed by the harmful effects, we consider five ways in which RTC laws could increase crime: (a) elevated crime by RTC permit holders or by others, which can be induced by the greater belligerence of permit holders that can attend gun carrying or even through counterproductive attempts by permit holders to intervene protectively; (b) increased crime by those who acquire the guns of permit holders via loss or theft; (c) a change in culture induced by the hyper-vigilance about one's rights and the need

---

[5]The injury to Darrell Cabey was so damaging that he remains confined to a wheelchair and functions with the intellect of an eight-year-old, for which he received a judgment of $43 million against Goetz, albeit without satisfaction (Biography.com 2016).

[6]According to the civil lawsuit brought by DiDonato, his injuries included "severe neurological impairment, inability to control his bowels, depression and severe neurologic injuries" (Lat 2012).

[7]Even big city police officers rarely need to fire a weapon despite their far greater exposure to criminals. According to a 2016 Pew Research Center survey of 7,917 sworn officers working in departments with 100 or more officers, "only about a quarter (27%) of all officers say they have ever fired their service weapon while on the job" (Morin & Mercer 2017).

to avenge wrongs that the gun culture can nurture; (d) elevated harm as criminals respond to the possibility of armed resistance by increasing their gun carrying and escalating their level of violence; and (e) all of the above factors will either take up police time or increase the risks the police face, thereby impairing the crime-fighting ability of police in ways that can increase crime.

a. Crime committed or induced by permit holders: RTC laws can lead to an increase in violent crime by increasing the likelihood a generally law-abiding citizen will commit a crime or increasing the criminal behavior of others. Moreover, RTC laws may facilitate the criminal conduct of those who generally have a criminal intent. We consider these two avenues below.

i. The pathway from the law-abiding citizen

Evidence from a nationally representative sample of 4,947 individuals indicates that Americans tend to overestimate their gun-related abilities. For example, 82.6 percent believed they were less likely than the average person to use a gun in anger. When asked about their "ability to responsibly own a handgun," 50 percent of the respondents deemed themselves to be in the top 10 percent and 23 percent placed their ability within the top 1 percent of the U.S. population. Such overconfidence has been found to increase risk taking and could well lead to an array of socially harmful consequences ranging from criminal misconduct and gun accidents to lost or stolen guns (Stark & Sachau 2016).

In a number of well-publicized cases, concealed carry permit holders have increased the homicide toll by killing someone with whom they became angry over an insignificant issue, ranging from merging on a highway and talking on a phone in a theater to playing loud music at a gas station (Lozano 2017; Levenson 2017; Scherer 2016). In one particularly tragic example in January 2019 at a bar in State College, Pennsylvania, a lawful permit holder, Jordan Witmer, got into a fight with his girlfriend. When a father and son sitting at the bar tried to intervene, Witmer killed both of them, shot his girlfriend in the chest, and fled. When his car crashed, Witmer broke into a nearby house, killed the 82-year-old homeowner, who was with his wife on their 60th wedding anniversary, and then killed himself (Sauro 2019). Another such example occurred in July 2018 when Michael Drejka started to hassle a woman sitting in a car in a disabled parking spot while her husband and five-year-old son ran into a store. When the husband emerged, he pushed Drejka to the ground, who then killed him with a shot to the chest. The killing is caught on video and Drejka is being prosecuted for manslaughter in Clearwater, Florida (Simon 2018).

When Philadelphia permit holder Louis Mockewich shot and killed a popular youth football coach (another permit holder carrying his gun) over a dispute concerning snow shoveling in January 2000, Mockewich's car had an NRA bumper sticker reading "Armed with Pride" (Gibbons & Moran 2000). An angry young man, with somewhat of a paranoid streak, who has not yet been convicted of a crime or adjudicated as a "mental defective," may be encouraged to carry a gun if he resides in an RTC state.[8] That such

---

[8]The Gun Control Act of 1968 prohibits gun possession by felons and adjudicated "mental defectives" (18 U.S.C. 922(d)(4), 2016).

individuals will be more likely to be aggressive once armed and hence more likely to stimulate violence by others should not be surprising.

Recent evidence suggests that as gun carrying is increasing with the proliferation of RTC laws, road rage incidents involving guns are rising (Biette-Timmons 2017; Plumlee 2012). Incidents in which "someone in a car brandished a gun in a threatening manner or fired a gun at another driver or passenger have more than doubled in the last three years, from 247 in 2014 to 620 in 2016 …. The highest-profile recent road rage incidents involved two NFL players, Joe McKnight and Will Smith, killed … in separate road rage shootings in New Orleans" (Shen 2017).[9] In the nightmare case for RTC, two Michigan permit-holding drivers pulled over to battle over a tailgating dispute in September 2013 and each shot and killed the other (Stuart 2013). Without Michigan's RTC law, this would likely have not been a double homicide. Indeed, two studies—one for Arizona and one for the nation as a whole—found that "the evidence indicates that those with guns in the vehicle are more likely to engage in 'road rage'" (Hemenway et al. 2006; Miller et al. 2002).[10] These studies may suggest either that gun carrying emboldens more aggressive behavior or reflects a selection effect for more aggressive individuals.[11] If this is correct, then it may not be a coincidence that there are so many cases in which a concealed carry holder acts belligerently and is shot by another permit holder.[12]

---

[9]Joe McNight and Ronald Gasser were arguing through their open car windows as they drove for miles. When they were both stopped at a red light, McNight walked over to Gasser's car, and the "two argued through the passenger-side window until Gasser pulled a gun from between his seat and the center console and shot McKnight three times." Gasser was convicted of manslaughter and sentenced to a prison term of 30 years (Calder 2018).

[10]A perfect illustration was provided by 25-year-old Minnesota concealed carry permit holder Alexander Weiss, who got into an argument after a fender bender caused by a 17-year-old driver. Since the police had been called, it is hard to imagine that this event could end tragically—unless someone had a gun. Unfortunately, Weiss, who had a bumper sticker on his car saying "Gun Control Means Hitting Your Target," killed the 17-year-old with one shot to the chest and has been charged with second-degree murder (KIMT 2018).

[11]While concealed carry permit holders should be free of any felony conviction, and thus show a lower overall rate of violence than a group that contains felons, a study in Texas found that when permit holders do commit a crime, it tends to be a severe one: "the concentration of convictions for weapons offenses, threatening someone with a firearm, and intentionally killing a person stem from the ready availability of a handgun for CHL holders" (Phillips et al. 2013). See, for example, a Texas permit holder who told police he shot a man in the head at an IHOP restaurant in Galveston because "he was annoyed by the noise the victim and others were making just a table away" (ABC News 2018).

[12]We have just cited three of them: the 2012 Pennsylvania bar shooting, the 2000 Philadelphia snow-shoveling dispute, and the 2013 Michigan road-rage incident. Here are two more. Former NFL player Will Smith, a concealed carry permit holder with a loaded gun in his car, was engaged in a road rage incident with another permit holder, who killed him with seven shots in the back and one into his side and shot his wife, hitting both knees. The shooter was convicted of manslaughter and sentenced to 25 years in prison (Lane 2018). In yet another recent case, two permit holders glowered at each other in a Chicago gas station, and when one drew his weapon, the second man pulled out his own gun and killed the 43-year-old instigator, who died in front of his son, daughter, and pregnant daughter-in-law (Hernandez 2017). A video of the encounter can be found at https://www.youtube.com/watch?v=I2j9vvDHlBU. According to the police report obtained by the *Chicago Tribune*, a bullet from the gun exchange broke the picture window of a nearby garden apartment and another shattered the window of a car with four occupants that was driving past the gas station. No charges were brought against the surviving permit holder, who shot first but in response to the threat initiated by the other permit holder.

In general, the critique that the relatively low number of permit revocations proves that permit holders do not commit enough crime to substantially elevate violent criminality is misguided for a variety of reasons. First, only a small fraction of 1 percent of Americans commits a gun crime each year, so we do not expect even a random group of Americans to commit much crime, let alone a group purged of convicted felons. Nonetheless, permit revocations clearly understate the criminal misconduct of permit holders, since not all violent criminals are caught and we have just seen five cases where six permit holders were killed, so no permit revocation or criminal prosecution would have occurred regardless of any criminality by the deceased.[13] Second, and perhaps more importantly, RTC laws increase crime by individuals other than permit holders in a variety of ways. The messages of the gun culture, perhaps reinforced by the adoption of RTC laws, can promote fear and anger, which are emotions that can invite more hostile confrontations leading to violence. For example, if permit holder George Zimmerman hassled Trayvon Martin only because Zimmerman was armed, then the presence of Zimmerman's gun could be deemed to have encouraged a hostile confrontation, regardless of who ultimately becomes violent.[14]

Even well-intentioned interventions by permit holders intending to stop a crime have elevated the crime count when they ended with the permit holder either being killed by the criminal[15] or shooting an innocent party by

---

[13]In addition, NRA-advocated state laws that ban the release of information about whether those arrested for even the most atrocious crimes are RTC permit holders make it extremely difficult to monitor their criminal conduct.

[14]Psychologists have found that the very act of carrying a gun tends to distort perceptions of reality in a way that exaggerates perceived threats. "We have shown here that … the act of wielding a firearm raises the likelihood that nonthreatening objects will be perceived as threats. This bias can clearly be horrific for victims of accidental shootings" (Witt & Brockmole 2012). As one permit holder explained: "a gun causes its bearer to see the world differently. A well-lit city sidewalk full of innocent pedestrians becomes a scene—a human grouping one of whose constituents you might need to shoot. Something good in yourself is, by this means, sacrificed. And more. In a sudden, unwieldy hauling-out of your piece, or just by having your piece in your pocket, you can fumble around and shoot yourself, as often happens and isn't at all funny. Or you might shoot some little girl on a porch across the street or two streets away, or five streets away. Lots and lots of untoward things can happen when you're legally carrying a concealed firearm. One or two of them might turn out to be beneficial—to you. But a majority are beneficial to neither man nor beast. Boats are said, by less nautical types, always to be seeking a place to sink. Guns—no matter who has them—are always seeking an opportunity to go off. Anybody who says different is a fool or a liar or both" (Ford 2016).

[15]In 2016 in Arlington, Texas, a man in a domestic dispute shot at a woman and then tried to drive off (under Texas law it was lawful for him to be carrying his gun in his car, even though he did not have a concealed carry permit.) When he was confronted by a permit holder, the shooter slapped the permit holder's gun out of his hand and then killed him with a shot to the head. Shortly thereafter, the shooter turned himself into the police (Mettler 2016). Similarly, when armed criminals entered a Las Vegas Walmart in 2014 and told everyone to get out because "[t]his is a revolution," one permit holder told his friend he would stay to confront the threat. He was gunned down shortly before the police arrived, adding to the death toll rather than reducing it (NBC News 2014). Finally, in January 2010, Stephen Sharp arrived at work at a St. Louis power plant just as co-worker Timothy Hendron began firing at fellow workers with an AK-47. Retrieving a pistol from his truck, Sharp opened fire at Hendron, and fecklessly discharged all six rounds from across the parking lot. Unharmed, Hendron returned fire, grievously wounding Sharp and continuing his rampage unabated. When the police arrived, there was "no clear distinction between attacker and victims." In the end, Hendron killed three and wounded five before killing himself (Byers 2010).

mistake.[16] Indeed, an FBI study of 160 active shooter incidents found that in almost half (21 of 45) the situations in which police engaged the shooter to end the threat, law enforcement suffered casualties, totaling nine killed and 28 wounded (Blair & Schweit 2014). One would assume the danger to an untrained permit holder trying to confront an active shooter would be greater than that of a trained professional, which may in part explain why effective intervention in such cases by permit holders to thwart crime is so rare. Although the same FBI report found that in 21 of a total of 160 active shooter incidents between 2000 and 2013, "the situation ended after unarmed citizens safely and successfully restrained the shooter," there was only one case—in a bar in Winnemucca, Nevada in 2008—in which a private armed citizen other than an armed security guard stopped a shooter, and that individual was an active-duty Marine (Holzel 2008).

ii. The pathway from those harboring criminal intent

Over the 10-year period from May 2007 through January 2017, the Violence Policy Center (2017) lists 31 instances in which concealed carry permit holders killed three or more individuals in a single incident. Many of these episodes are disturbingly similar in that there was substantial evidence of violent tendencies and/or serious mental illness, but no effort was made to even revoke the carry permit, let alone take effective action to prevent access to guns. For example, on January 6, 2017, concealed handgun permit holder Esteban Santiago, 26, killed five and wounded six others at the Fort Lauderdale-Hollywood Airport, before sitting on the floor and waiting to be arrested as soon as he ran out of ammunition. In the year prior to the shooting, police in Anchorage, Alaska, charged Santiago with domestic violence, and visited the home five times for various other complaints (KTUU 2017). In November 2016, Santiago entered the Anchorage FBI office and spoke of "mind control" by the CIA and having "terroristic thoughts" (Hopkins 2017). Although the police took his handgun at the time, it was returned to him on December 7, 2016 after Santiago spent four days in a mental health facility because, according to federal officials, "there was no mechanism in federal law for officers to permanently seize the weapon"[17] (Boots 2017). Less than a month later, Santiago flew with his gun to Florida and opened fire in the baggage claim area.[18]

In January 2018, the FBI charged Taylor Wilson, a 26-year-old Missouri concealed carry permit holder, with terrorism on an Amtrak train when, while carrying a loaded

[16]In 2012, "a customer with a concealed handgun license … accidentally shot and killed a store clerk" during an attempted robbery in Houston (MacDonald 2012). Similarly, in 2015, also in Houston, a bystander who drew his weapon upon seeing a carjacking incident ended up shooting the victim in the head by accident (KHOU 2015). An episode in June 2017 underscored that interventions even by well-trained individuals can complicate and exacerbate unfolding crime situations. An off-duty Saint Louis police officer with 11 years of service was inside his home when he heard the police exchanging gunfire with some car thieves. Taking his police-issued weapon, he went outside to help, but as he approached he was told by two officers to get on the ground and then shot in the arm by a third officer who "feared for his safety" (Hauser 2017).

[17]Moreover, in 2012, Puerto Rican police confiscated Santiago's handguns and held them for two years before returning them to him in May 2014, after which he moved to Alaska (Clary et al. 2017).

[18]For a similar story of repeated gun violence and signs of mental illness by a concealed carry permit holder, see the case of Aaron Alexis, who murdered 12 at the Washington Navy Yard in September 2013 (Carter et al. 2013).

weapon, he tried to interfere with the brakes and controls of the moving train. According to the FBI, Wilson had (1) previously joined an "alt-right" neo-Nazi group and traveled to the Unite the Right rally in Charlottesville, Virginia in August 2017; (2) indicated his interest in "killing black people" and was the perpetrator of a road-rage incident in which he pointed a gun at a black woman for no apparent reason while driving on an interstate highway in April 2016; and (3) possessed devices and weapons "to engage in criminal offenses against the United States." Research is needed to analyze whether having a permit to legally carry weapons facilitates such criminal designs (Pilger 2018).

In June 2017, Milwaukee Police Chief Ed Flynn pointed out that criminal gangs have taken advantage of RTC laws by having gang members with clean criminal records obtain concealed carry permits and then hold the guns after they are used by the active criminals (Officer.com 2017). Flynn was referring to so-called human holsters who have RTC permits and hold guns for those barred from possession. For example, Wisconsin permit holder Darrail Smith was stopped three times while carrying guns away from crime scenes before police finally charged him with criminal conspiracy. In the second of these, Smith was "carrying three loaded guns, including one that had been reported stolen," but that was an insufficient basis to charge him with a crime or revoke his RTC permit (DePrang 2015). Having a "designated permit holder" along to take possession of the guns when confronted by police may be an attractive benefit for criminal elements acting in concert (Fernandez et al. 2015; Luthern 2015).

b. Increased gun thefts: The most frequent occurrence each year involving crime and a good guy with a gun is not self-defense but rather the theft of the good guy's gun, which occurs hundreds of thousands of times each year.[19] Data from a nationally representative web-based survey conducted in April 2015 of 3,949 subjects revealed that those who carried guns outside the home had their guns stolen at a rate over 1 percent per year (Hemenway et al. 2017). Given the current level of roughly 16 million permit holders, a plausible estimate is that RTC laws result in permit holders furnishing more than 100,000 guns per year to criminals.[20] As Phil Cook has noted, the relationship between gun theft and crime is a complicated one for which few definitive data are currently available (Cook

[19]According to Larry Keane, senior vice president of the National Shooting Sports Foundation (a trade group that represents firearms manufacturers): "There are more guns stolen every year than there are violent crimes committed with firearms." More than 237,000 guns were reported stolen in the United States in 2016, according to the FBI's National Crime Information Center. The actual number of thefts is obviously much higher since many gun thefts are never reported to police, and "many gun owners who report thefts do not know the serial numbers on their firearms, data required to input weapons into the NCIC." The best survey estimated 380,000 guns were stolen annually in recent years, but given the upward trend in reports to police, that figure likely understates the current level of gun thefts (Freskos 2017b). According to National Crime Information Center data, the number of guns reported stolen nationally jumped 60 percent between 2007 and 2016 (Freskos 2018a).

[20]While the Hemenway et al. study is not large enough and detailed enough to provide precise estimates, it establishes that those who have carried guns in the last month are more likely to have them stolen. A recent Pew Research Survey found that 26 percent of American gun owners say they carry a gun outside of their home "all or most of the time" (Igielnik & Brown 2017, surveying 3,930 U.S. adults, including 1,269 gun owners). If 1 percent of 16 million permit holders have guns stolen each year, that would suggest 160,000 guns were stolen. Only guns stolen outside the home would be attributable to RTC laws, so a plausible estimate of guns stolen per year owing to gun carrying outside the home might be 100,000.

2018). But if there was any merit to the outrage over the loss of about 1,400 guns during the Fast and Furious program that began in 2009 and the contribution that these guns made to crime (primarily in Mexico), it highlights the severity of the vastly greater burdens of guns lost by and stolen from U.S. gun carriers.[21] A 2013 report from the Bureau of Alcohol, Tobacco, Firearms, and Explosives concluded that "lost and stolen guns pose a substantial threat to public safety and to law enforcement. Those that steal firearms commit violent crimes with stolen guns, transfer stolen firearms to others who commit crimes, and create an unregulated secondary market for firearms, including a market for those who are prohibited by law from possessing a gun" (Office of the Director—Strategic Management 2013; Parsons & Vargas 2017).

For example, after Sean Penn obtained a permit to carry a gun, his car was stolen with two guns in the trunk. The car was soon recovered, but the guns were gone (Donohue 2003). In July 2015 in San Francisco, the theft of a gun from a car in San Francisco led to a killing of a tourist on a city pier that almost certainly would not have occurred if the lawful gun owner had not left it in the car (Ho 2015). Just a few months later, a gun stolen from an unlocked car was used in two separate killings in San Francisco and Marin in October 2015 (Ho & Williams 2015). According to the National Crime Victimization Survey, in 2013 there were over 660,000 auto thefts from households. More guns being carried in vehicles by permit holders means more criminals will be walking around with the guns stolen from permit holders.[22]

As Michael Rallings, the top law enforcement official in Memphis, Tennessee, noted in commenting on the problem of guns being stolen from cars: "Laws have unintended consequences. We cannot ignore that as a legislature passes laws that make guns more accessible to criminals, that has a direct effect on our violent crime rate" (Freskos 2017a). An Atlanta police sergeant elaborated on this phenomenon: "Most of our criminals, they go out each and every night hunting for guns, and the easiest way to get them is out of people's cars. We're finding that a majority of stolen guns that are getting in the hands of criminals and being used to commit crimes were stolen out of vehicles" (Freskos 2017c). In 2015, 70 percent of guns reported stolen in Atlanta came from cars and trucks (Freskos 2016). Another Atlanta police officer stated that weapons stolen from cars "are used in crimes to shoot people, to rob people" because criminals find these guns to be easy to steal and hard to trace. "For them, it doesn't cost them anything to break into a car and steal a gun" (Freskos 2016).[23]

---

[21] "Of the 2,020 guns involved in the Bureau of Alcohol, Tobacco, Firearms, and Explosives probe dubbed 'Operation Fast and Furious,' 363 have been recovered in the United States and 227 have been recovered in Mexico. That leaves 1,430 guns unaccounted for" (Schwarzschild & Griffin 2011). Wayne LaPierre of the NRA was quoted as saying: "These guns are now, as a result of what [ATF] did, in the hands of evil people, and evil people are committing murders and crimes with these guns against innocent citizens" (Horwitz 2011).

[22] In early December 2017, the sheriff in Jacksonville, Florida announced that his office knew of 521 guns that had been stolen so far in 2017—from unlocked cars alone! (Campbell 2017).

[23] Examples abound: Tario Graham was shot and killed during a domestic dispute in February 2012 with a revolver stolen weeks earlier out of pickup truck six miles away in East Memphis (Perrusquia 2017). In Florida, a handgun stolen from an unlocked Honda Accord in mid-2014 helped kill a police officer a few days before Christmas that year (Sampson 2014). A gun stolen from a parked car during a Mardi Gras parade in 2017 was used a few days later to kill 15-year-old Nia Savage in Mobile, Alabama, on Valentine's Day (Freskos 2017a).

Of course, the permit holders whose guns are stolen are not the killers, but they can be the but-for cause of the killings. Lost, forgotten, and misplaced guns are another dangerous byproduct of RTC laws.[24]

c. Enhancing a culture of violence: The South has long had a higher rate of violent crime than the rest of the country. For example, in 2012, while the South had about one-quarter of the U.S. population, it had almost 41 percent of the violent crime reported to police (Fuchs 2013). Social psychologists have argued that part of the reason the South has a higher violent crime rate is that it has perpetuated a "subculture of violence" predicated on an aggrandized sense of one's rights and honor that responds negatively to perceived insults. A famous experiment published in the *Journal of Personality and Social Psychology* found that southern males were more likely than northern males to respond aggressively to being bumped and insulted. This was confirmed by measurement of their stress hormones and their frequency of engaging in aggressive or dominant behavior after being insulted (Cohen et al. 1996). To the extent that RTC laws reflect and encourage this cultural response, they can promote violent crime not only by permit holders, but by all those with or without guns who are influenced by this crime-inducing worldview.

Even upstanding citizens, such as Donald Brown, a 56-year-old retired Hartford firefighter with a distinguished record of service, can fall prey to the notion that resort to a lawful concealed weapon is a good response to a heated argument. Brown was sentenced to seven years in prison in January 2018 by a Connecticut judge who cited his "poor judgment on April 24, 2015, when he drew his licensed 9mm handgun and fired a round into the abdomen of Lascelles Reid, 33." The shooting was prompted by a dispute "over renovations Reid was performing at a house Brown owns" (Owens 2018). Once again, we see that the RTC permit was the pathway to serious violent crime by a previously law-abiding citizen.

d. Increasing violence by criminals: The argument for RTC laws is often predicated on the supposition that they will encourage good guys to have guns, leading only to benign effects on the behavior of bad guys. This is highly unlikely to be true.[25] Indeed, the

---

[24]The growing TSA seizures in carry-on luggage are explained by the increase in the number of gun carriers who simply forget they have a gun in their luggage or briefcase (Williams & Waltrip 2004). A chemistry teacher at Marjory Stoneman Douglas High School in Parkland, Florida, who had said he would be willing to carry a weapon to protect students at the school, was criminally charged for leaving a loaded pistol in a public restroom. The teacher's 9mm Glock was discharged by an intoxicated homeless man who found it in the restroom (Stanglin 2018).

[25]Consider in this regard, David Friedman's theoretical analysis of how right-to-carry laws will reduce violent crime: "Suppose one little old lady in ten carries a gun. Suppose that one in ten of those, if attacked by a mugger, will succeed in killing the mugger instead of being killed by him—or shooting herself in the foot. On average, the mugger is much more likely to win the encounter than the little old lady. But—also on average—every hundred muggings produce one dead mugger. At those odds, mugging is a very unattractive profession—not many little old ladies carry enough money in their purses to justify one chance in a hundred of being killed getting it. The number of muggers—and muggings—declines drastically, not because all of the muggers have been killed but because they have, rationally, sought safer professions" (Friedman 1990). There is certainly no empirical support for the conjecture that muggings will "decline drastically" in the wake of RTC adoption. What Friedman's analysis overlooks is that muggers can decide not to mug (which is what Friedman posits) or they can decide to initiate their muggings by cracking the old ladies over the head or by being

evidence that gun prevalence in a state is associated with higher rates of lethal force by police (even controlling for homicide rates) suggests that police may be more fearful and shoot quicker when they are more likely to interact with an armed individual (Nagin forthcoming).[26] Presumably, criminals would respond in a similar fashion, leading them to arm themselves more frequently, attack more harshly, and shoot more quickly when citizens are more likely to be armed. In one study, two-thirds of prisoners incarcerated for gun offenses "reported that the chance of running into an armed victim was very or somewhat important in their own choice to use a gun" (Cook et al. 2009). Such responses by criminals will elevate the toll of the crimes that do occur.

Indeed, a panel data estimate over the years 1980 to 2016 reveals that the percentage of robberies committed with a firearm rises by 18 percent in the wake of RTC adoption (t = 2.60).[27] Our synthetic controls assessment similarly shows that the percentage of robberies committed with a firearm increases by 35 percent over 10 years (t = 4.48).[28] Moreover, there is no evidence that RTC laws are reducing the overall level of robberies: the panel data analysis associates RTC laws with a 9 percent higher level of overall robberies (t = 1.85) and the synthetic controls analysis suggests a 7 percent growth over 10 years (t = 1.19).

e. Impairing police effectiveness: According to an April 2016 report of the Council of Economic Advisers: "Expanding resources for police has consistently been shown to reduce crime; estimates from economic research suggests that a 10% increase in police size decreases crime by 3 to 10%" (CEA 2016:4). In summarizing the evidence on fighting crime in the *Journal of Economic Literature*, Aaron Chalfin and Justin McCrary note that adding police manpower is almost twice as effective in reducing violent crime as it is in reducing property crime (Chalfin & McCrary 2017). Therefore, anything that RTC laws do to occupy police time, from processing permit applications to checking for permit validity to dealing with gunshot victims, inadvertent gun discharges, and the staggering number of stolen guns is likely to have an opportunity cost expressed in higher violent crime.

The presence of more guns on the street can complicate the job of police as they confront (or shy away from) armed citizens. Daniel Nagin finds a pronounced positive association between statewide prevalence of gun ownership and police use of lethal force (Nagin forthcoming). A Minnesota police officer who stopped Philando Castile for a broken taillight shot him seven times only seconds after Castile indicated he had a permit to carry a weapon because the officer feared the permit holder might be reaching for the

---

prepared to shoot them if they start reaching for a gun (or even wear body armor). Depending on the response of the criminals to increased gun carrying by potential victims, the increased risk to the criminals may be small compared to the increased risk to the victims. Only an empirical evaluation can answer this question.

[26]See footnotes 29–31 and accompanying text for examples of this pattern of police use of lethal force.

[27]The panel data model uses the DAW explanatory variables set forth in Table 2.

[28]The weighted average proportion of robberies committed by firearm in the year prior to RTC adoption (for states that adopted RTC between 1981 and 2014) is 36 percent while the similar proportion in 2014 for the same RTC states is 43 percent (and for non-RTC states is 29 percent).

gun. Another RTC permit holder, stranded in his disabled car early one morning on a Florida highway exit ramp, grabbed the gun he had legally purchased three days earlier when a police officer in plainclothes pulled up in a van with tinted windows and no lights. "It was not immediately clear what happened after [the officer] got out of his van, but the permit holder at some point started running … and [the officer] fired six times," killing the permit holder, whose body fell "about 80 to 100 feet from his vehicle," with his undischarged handgun on the ground somewhere in between (Robles & Hauser 2015). After a similar encounter between an officer and a permit holder, the officer asked the gun owner: "Do you realize you almost died tonight?" (Kaste 2019).[29]

A policemen trying to give a traffic ticket has more to fear if the driver is armed. When a gun is found in a car in such a situation, a greater amount of time is needed to ascertain the driver's status as a permit holder. A lawful permit holder who happens to have forgotten his permit may end up taking up more police time through arrest and/or other processing.

Moreover, police may be less enthusiastic about investigating certain suspicious activities or engaging in effective crime-fighting actions given the greater risks that widespread gun carrying poses to them, whether from permit holders or the criminals who steal their guns.[30] In a speech at the University of Chicago Law School in October 2015, then-FBI Director James Comey argued that criticism of overly aggressive policing led officers to back away from more involved policing, causing violent crime to rise (Donohue 2017a). If the more serious concern of being shot by an angry gun toter impairs effective policing, the prospect of increased crime following RTC adoption could be far more substantial than the issue that Comey highlighted.[31]

---

[29]A permit to carry instructor has posted a YouTube video about "How to inform an officer you are carrying a handgun and live" that is designed to "keep yourself from getting shot unintentionally" by the police. The video, which has over 4.2 million views, has generated comments from non-Americans that it "makes the US look like a war zone" and leads to such unnatural and time-consuming behavior that "an English officer … would look at you like a complete freak" (Soderling 2016).

[30]"Every law enforcement officer working today knows that any routine traffic stop, delivery of a warrant or court order, or response to a domestic disturbance anywhere in the country involving people of any race or age can put them face to face with a weapon. Guns are everywhere, not just in the inner city" (Wilson 2016). In offering an explanation for why the United States massively leads the developed world in police shootings, criminologist David Kennedy stated: "Police officers in the United States in reality need to be conscious of and are trained to be conscious of the fact that literally every single person they come in contact with may be carrying a concealed firearm." For example, police in England and Wales shot and killed 55 people over the 25-year period from 1990–2014, while in just the first 24 days of 2015, the United States (with six times the population) had a higher number of fatal shootings by police (Lopez 2018).

[31]A vivid illustration of how even the erroneous perception that someone accosted by the police is armed can lead to deadly consequences is revealed in the chilling video of five Arizona police officers confronting an unarmed man they incorrectly believed had a gun. During the prolonged encounter, the officers shouted commands at an intoxicated 26-year-old father of two, who begged with his hands in the air not to be shot. The man was killed by five bullets when, following orders to crawl on the floor toward police, he paused to pull up his slipping pants. A warning against the open carry of guns issued by the San Mateo County, California, Sheriff's Office makes the general point that law enforcement officers become hyper-vigilant when encountering an armed individual: "Should the gun carrying person fail to comply with a law enforcement instruction or move in a way that could be construed as threatening, the police are forced to respond in kind for their own protection. It's well and good in hindsight to say the gun carrier was simply 'exercising their rights' but the result could be deadly" (Lunny 2010).

The presence of multiple gun carriers can also complicate police responses to mass shootings and other crimes. When police arrived at an Alabama mall in November 2018, they saw a 21-year-old concealed carry permit holder with gun drawn, and mistakenly killed him, thinking he was the shooter. In fact, the dead man had been assisting and protecting shoppers, and the real shooter escaped (McLaughlin & Holcombe 2018). Another benign intervention that ended in tragedy for the good guy with a gun occurred in July 2018 when police officers arrived as a "good Samaritan" with a concealed carry permit was trying to break up a fight in Portland, Oregon. The police saw the gun held by the permit holder—a Navy veteran, postal worker, and father of three—and in the confusion shot and killed him (Gueverra 2018).

Good guys with guns also can interfere with police anti-crime efforts. For example, police reported that when a number of Walmart customers (fecklessly) pulled out their weapons during a shooting on November 1, 2017, their "presence 'absolutely' slowed the process of determining who, and how many, suspects were involved in the shootings, said Thornton [Colorado] police spokesman Victor Avila" (Simpson 2017).

Similarly, in 2014, a concealed carry permit holder in Illinois fired two shots at a fleeing armed robber at a phone store, thereby interfering with a pursuing police officer. According to the police: "Since the officer did not know where the shots were fired from, he was forced to terminate his foot pursuit and take cover for his own safety" (Glanton & Sadovi 2014).

Indeed, preventive efforts to get guns off the street in high-crime neighborhoods are less feasible when carrying guns is presumptively legal. The passage of RTC laws normalizes the practice of carrying guns in a way that may enable criminals to carry guns more readily without prompting a challenge, while making it harder for the police to know who is and who is not allowed to possess guns in public.

Furthermore, negligent discharges of guns, although common, rarely lead to charges of violent crime but they can take up valuable police time for investigation and in determining whether criminal prosecution or permit withdrawal is warranted. For example, on November 16, 2017, Tennessee churchgoers were reflecting on the recent Texas church massacre in Sutherland Springs when a permit holder mentioned he always carries his gun, bragging that he would be ready to stop any mass shooter. While proudly showing his Ruger handgun, the permit holder inadvertently shot himself in the palm, causing panic in the church as the bullet "ripped through [his wife's] lower left abdomen, out the right side of her abdomen, into her right forearm and out the backside of her forearm. The bullet then struck the wall and ricocheted, landing under the wife's wheelchair." The gun discharge prompted a 911 call, which in the confusion made the police think an active shooting incident was underway. The result was that the local hospital and a number of schools were placed on lockdown for 45 minutes until the police finally ascertained that the shooting was accidental (Eltagouri 2017).[32]

---

[32]Negligent discharges by permit holders have occurred in public and private settings from parks, stadiums, movie theaters, restaurants, and government buildings to private households (WFTV 2015; Heath 2015). Thirty-nine-year-old Mike Lee Dickey, who was babysitting an eight-year-old boy, was in the bathroom removing his handgun from his waistband when it discharged. The bullet passed through two doors, before striking the child in his arm while he slept in a nearby bedroom (Associated Press 2015). In April 2018, a 21-year-old pregnant mother of two in

Everything that takes up added police time or complicates the job of law enforcement will serve as a tax on police, rendering them less effective on the margin, and thereby contributing to crime. Indeed, this may in part explain why RTC states tend to increase the size of their police forces (relative to nonadopting states) after RTC laws are passed, as shown in Table 1.[33]

B. A Simple Difference-in-Differences Analysis

We begin by showing how violent crime evolved over our 1977–2014 data period for RTC and non-RTC states.[34] Figure 1 depicts percentage changes in the violent crime rate over our entire data period for three groups of states: those that never adopted RTC laws, those that adopted RTC laws sometime between 1977 and before 2014, and those that adopted RTC laws prior to 1977. It is noteworthy that the 42.3 percent drop in violent crime in the nine states that never adopted RTC laws is almost an order of magnitude greater than the 4.3 percent reduction experienced by states that adopted RTC laws during our period of analysis.[35]

The NRC Report presented a "no-controls" estimate, which is just the coefficient estimate on the variable indicating the date of adoption of a RTC law in a crime rate panel data model with state and year fixed effects. According to the NRC Report: "Estimating the model using data to 2000 shows that states adopting right-to-carry laws saw 12.9 percent increases in violent crime—and 21.2 percent increases in property crime—relative to national crime patterns." Estimating this same model using 14 additional years of data (through 2014) and 11 additional adopting states (listed at the bottom of Appendix Table C1) reveals that the average postpassage increase in violent crime was

---

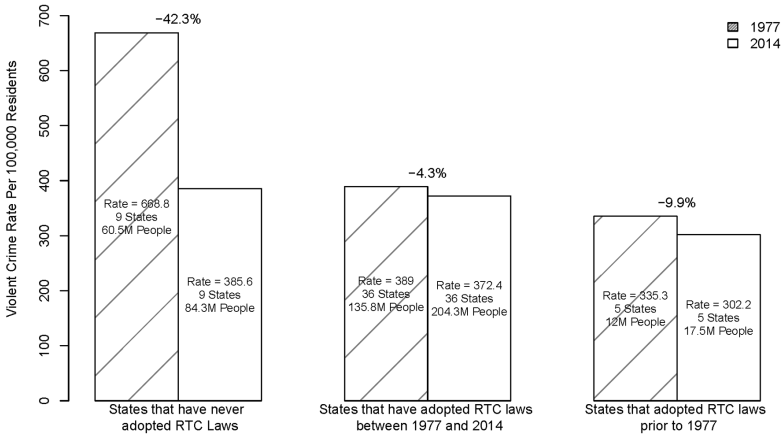
Indiana was shot by her three-year-old daughter when the toddler's father left the legal but loaded 9mm handgun between the console and the front passenger seat after he exited the vehicle to go inside a store. The child climbed over from the backseat and accidentally fired the gun, hitting her mother though the upper right part of her torso. (Palmer 2018) See also Savitsky (2019) (country western singer Justin Carter dies when the gun in his pocket discharges and hits him in the face); Schwarz (2014) (Idaho professor shoots himself in foot during class two months after state legalizes guns on campuses); Murdock (2018) (man shoots himself in the groin with gun in his waistband in the meat section of Walmart in Buckeye, Arizona); Barbash (2018) (California teacher demonstrating gun safety accidentally discharges weapon in a high school classroom in March 2018, injuring one student); Fortin (2018) (in February 2018, a Georgia teacher fired his gun while barricaded in his classroom); US News (2018) (in April 2018, an Ohio woman with a valid concealed carry permit accidentally killed her two-year-old daughter at an Ohio hotel while trying to turn on the gun's safety); and Fox News (2016) ("the owner of an Ohio gun shop was shot and killed when a student in a concealed carry permit class accidentally discharged a weapon," striking the owner in the neck in a different room after the bullet passed through a wall).

[33]See Adda et al. (2014), describing how local depenalization of cannabis enabled the police to reallocate resources, thereby reducing violent crime.

[34]The FBI violent crime category includes murder, rape, robbery, and aggravated assault.

[35]Over the same 1977–2014 period, the states that avoided adopting RTC laws had substantially smaller increases in their rates of incarceration and police employment. The nine never-adopting states increased their incarceration rate by 205 percent, while the incarceration rates in the adopting states rose by 262 and 259 percent, for those adopting RTC laws before and after 1977, respectively. Similarly, the rate of police employment rose by 16 percent in the never-adopting states and by 38 and 55 percent for those adopting before and after 1977, respectively.

Figure 1: The decline in violent crime rates has been far greater in states with no RTC laws, 1977–2014.



DATA SOURCES: UCR for crime rates; Census for state populations.
NOTE: Illinois excluded since its concealed carry law did not go into effect until 2014. From 1977–2013, the violent crime rate in Illinois fell by 36 percent, from 631 to 403 crimes per 100,000 people.

20.2 percent, while the comparable increase in property crime was 19.2 percent (both having p values less than 5 percent).[36]

Of course, it does not prove that RTC laws increase crime simply because RTC states experience a worse postpassage crime pattern. For example, it might be the case that some states decided to fight crime by allowing citizens to carry concealed handguns while others decided to hire more police and incarcerate a greater number of convicted criminals. If police and prisons were more effective in stopping crime, the “no-controls” model might show that the crime experience in RTC states was worse than in other states even if this were not a true causal result of the adoption of RTC laws. As it turns out, though, RTC states not only experienced higher rates of violent crime but they also had larger increases in incarceration and police than other states. Table 1 provides panel data evidence on how incarceration and two measures of police employment changed after RTC adoption (relative to nonadopting states). All three measures rose in RTC states, and the 7–8 percent greater increases in police in RTC states are statistically significant. In other words, Table 1 confirms that RTC states did *not* have relatively declining rates of

---

[36]The dummy variable model reports the coefficient associated with a RTC variable that is given a value of 0 when a RTC law is not in effect in that year, a value of 1 when a RTC law is in effect that entire year, and a value equal to the portion of the year a RTC law is in effect otherwise. The date of adoption for each RTC state is shown in Appendix Table A1. Note the fact that violent crime was noticeably higher in 1977 in the nine states that did not adopt RTC laws indicates that it will be particularly important that the parallel trends requirement of a valid panel data analysis is established, which is an issue to which we carefully attend in Section III.A.3. All our appendices are posted online at https://works.bepress.com/john_donohue/.

Table 1: Panel Data Estimates Showing Greater Increases in Incarceration and Police Following RTC Adoption: State- and Year-Fixed Effects, and No Other Regressors, 1977–2014

	Incarceration	*Police Employment per 100k*	*Police Officers per 100k*
	(1)	*(2)*	*(3)*
Dummy variable model	6.78 (6.22)	8.39*** (3.15)	7.08** (2.76)

NOTE: OLS estimations include state- and year-fixed effects and are weighted by population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The police employment and sworn police officer data are from the Uniform Crime Reports (UCR). The source of the incarceration rate is the Bureau of Justice Statistics (2014). *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. All figures reported in percentage terms.

incarceration or total police employees after adopting their RTC laws that might explain their comparatively poor postpassage crime performance.

III. A Panel Data Analysis of RTC Laws

A. *Estimating Two Models on the Full Data Period 1977–2014*

We have just seen that RTC law adoption is followed by *higher* rates of violent and property crime (relative to national trends) and that the elevated crime levels after RTC law adoption occur despite the fact that RTC states actually invested relatively more heavily in prisons and police than non-RTC states. While the theoretical predictions about the effect of RTC laws on crime are indeterminate, these two empirical facts based on the actual patterns of crime and crime-fighting measures in RTC and non-RTC states suggest that the most plausible working hypothesis is that RTC laws *increase* crime. The next step in a panel data analysis of RTC laws would be to test this hypothesis by introducing an appropriate set of explanatory variables that plausibly influence crime.

The choice of these variables is important because any variable that both influences crime and is simultaneously correlated with RTC laws must be included if we are to generate unbiased estimates of the impact of RTC laws. At the same time, including irrelevant and/or highly collinear variables can also undermine efforts at valid estimation of the impact of RTC laws. At the very least, it seems advisable to control for the levels of police and incarceration because these have been the two most important criminal justice policy instruments in the battle against crime.

1. The DAW Panel Data Model

In addition to the state and year fixed effects of the no-controls model and the identifier for the presence of an RTC law, our preferred "DAW model" includes an array of other factors that might be expected to influence crime, such as the levels of police and incarceration, various income, poverty, and unemployment measures, and six demographic controls designed to capture the presence of males in three racial categories (black, white, other) in two high-crime age groupings (15–19 and 20–39). Table 2 lists the full

Table 2: Table of Explanatory Variables for Four Panel Data Studies

Explanatory Variables	*DAW*	*LM*
Right-to-carry law	x	x
Lagged per capita incarceration rate	x	
Lagged police staffing per 100,000 residents	x	
Poverty rate	x	
Unemployment rate	x	
Per capita ethanol consumption from beer	x	
Percentage of state population living in metropolitan statistical areas (MSA)	x	
Real per capita personal income	x	x
Real per capita income maintenance		x
Real per capita retirement payments		x
Real per capita unemployment insurance payments		x
Population density		x
Lagged violent or property arrest rate		x
State population		x
6 Age-sex-race demographic variables —all 6 combinations of black, white, and other males in 2 age groups (15–19, 20–39) indicating the percentage of the population in each group	x	
36 Age-sex-race demographic variables —all possible combinations of black, white, and other males in 6 age groups (10–19, 20–29, 30–39, 40–49, 50–64, and over 65) and repeating this all for females, indicating the percentage of the population in each group		x

NOTE: The DAW model is advanced in this article and the LM model was previously published by Lott and Mustard.

set of explanatory variables for both the DAW model and the comparable panel data model used by Lott and Mustard (LM).[37]

Mathematically, the simple dummy model takes the following form:

$$\ln(\mathit{crime\ rate}_{it}) = \beta X_{it} + \gamma RTC_{it} + \alpha_t + \delta_i + \varepsilon_{it} \qquad (1)$$

where γ is the coefficient on the RTC dummy, reflecting the average estimated impact of adopting a RTC law on crime. The matrix X_{it} contains either the DAW or LM covariates

[37]While we attempt to include as many state-year observations in these regressions as possible, District of Columbia incarceration data are missing after the year 2001. In addition, a handful of observations are also dropped from the LM regressions owing to states that did not report any usable arrest data in various years. Our regressions are performed with Huber-White robust standard errors that are clustered at the state level, and we lag the arrest rates used in the LM regression models. The rationales underlying both choices are described in more detail in Aneja et al. (2014). All the regressions presented in this article are weighted by state population.

Table 3: Panel Data Estimates Suggesting that RTC Laws Increase Violent and Property Crime: State- and Year-Fixed Effects, DAW Regressors, 1979–2014

	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	2.27 (5.05)	2.90 (6.74)	1.53 (3.32)	9.02*** (2.90)	6.49** (2.74)

NOTE: All models include year- and state-fixed effects, and OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The violent and property crime data are from the Uniform Crime Reports (UCR) while the murder data are from the National Vital Statistics System (NVSS). Six demographic variables (based on different age-sex-race categories) are included as controls in the regression above. Other controls include the lagged incarceration rate, the lagged police employee rate, real per capita personal income, the unemployment rate, poverty rate, beer, and percentage of the population living in MSAs. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. All figures reported in percentage terms.

and demographic controls for state i in year t. The vectors α and δ are year and state fixed effects, respectively, while ε_{it} is the error term.

The DAW panel data estimates of the impact of RTC laws on crime are shown in Table 3.[38] The results are consistent with, although smaller in magnitude than, those observed in the no-controls model: RTC laws on average increased violent crime by 9.0 percent and property crime by 6.5 percent in the years following adoption.[39] The effect of RTC laws on murder is seen in Table 3 to be very imprecisely estimated and not statistically significant.[40]

We should also note one caveat to our results. Panel data analysis assumes that the treatment in any one state does not influence crime in nontreatment states. However, as we noted above,[41] RTC laws tend to lead to substantial increases in gun thefts and those guns tend to migrate to states with more restrictive gun laws, where they elevate violent crime. This flow of guns from RTC to non-RTC states has been documented by gun trace data (Knight 2013), and Olson et al. (2019) find that "firearm trafficking from states with less restrictive firearm legislation to neighboring states with more restrictive firearm legislation

[38]The complete set of estimates for all explanatory variables (except the demographic variables) for the DAW and LM dummy models are shown in Appendix Table B1.

[39]Defensive uses of guns are more likely for violent crimes because the victim will clearly be present. For property crimes, the victim is typically absent, thus providing less opportunity to defend with a gun. It is unclear whether the many ways in which RTC laws could lead to more crime, which we discuss in Section II.A.2, would be more likely to facilitate violent or property crime, but our intuition is that violent crime would be more strongly influenced, which is in fact what Table 3 suggests.

[40]We thank Phil Cook for informing us that UCR murder data are both less complete and less discerning than murder data collected by the National Vital Statistics. Note that we subtract all cases of justifiable homicides from the murder counts in our own Vital Statistics data.

[41]See text at footnotes 20–22.

increases firearm homicide rates in those restrictive states."[42] As a result, our panel data estimates of the impact of RTC laws are downward biased by the amount that RTC laws induce crime spillovers into non-RTC states.[43] One police investigation revealed that of the 224 guns a single gun trafficker in the DC area was known to have sold in just five months of 2015, 94 were later found at crime scenes from Virginia to New York (Hermann & Weiner 2019).

2. The LM Panel Data Model

Table 2's recitation of the explanatory variables contained in the Lott and Mustard (LM) panel data model reveals there are no controls for the levels of police and incarceration in each state, even though a substantial literature has found that these factors have a large impact on crime. Indeed, as we saw in Table 1, both factors grew substantially and statistically significantly after RTC law adoption. A Bayesian analysis of the impact of RTC laws found that "the incarceration rate is a powerful predictor of future crime rates," and specifically faulted this omission from the Lott and Mustard model (Strnad 2007:201, n.8). We have discussed an array of infirmities with the LM model in Aneja et al. (2014), including their reliance on flawed pseudo-arrest rates, and highly collinear demographic variables.

As noted in Aneja et al. (2014):

> The Lott and Mustard arrest rates … are a ratio of arrests to crimes, which means that when one person kills many, for example, the arrest rate falls, but when many people kill one person, the arrest rate rises, since only one can be arrested in the first instance and many can in the second. The bottom line is that this "arrest rate" is not a probability and is frequently greater than one because of the multiple arrests per crime. For an extended discussion on the abundant problems with this pseudo arrest rate, see Donohue and Wolfers (2009).

The LM arrest rates are also econometrically problematic since the denominator of the arrest rate is the numerator of the dependent variable crime rate, improperly leaving the dependent variable on both sides of the regression equation. We lag the arrest rates by one year to reduce this problem of ratio bias.

Lott and Mustard's use of 36 demographic variables is also a potential concern. With so many enormously collinear variables, the high likelihood of introducing noise into the estimation process is revealed by the wild fluctuations in the coefficient estimates on these variables. For example, consider the LM explanatory variables "neither black nor white male aged 30–39" and the identical corresponding female category. The LM dummy variable model for violent crime suggests that the male group will significantly

---

[42]"Seventy-five percent of traceable guns recovered by authorities in New Jersey [a non-RTC state] are purchased in states with weaker gun laws, according to … firearms trace data … compiled by the federal Bureau of Alcohol, Tobacco, Firearms and Explosives … between 2012 and 2016" (Pugliese 2018). See also Freskos (2018b).

[43]Some of the guns stolen from RTC permit holders may also end up in foreign countries, which will stimulate crime there but not bias our panel data estimates. For example, a recent analysis of guns seized by Brazilian police found that 15 percent came from the United States. Since many of these were assault rifles, they were probably not guns carried by American RTC permit holders (Paraguassu & Brito 2018).

Table 4: Panel Data Estimates of the Impact of RTC Laws: State-and Year-Fixed Effects, Using Actual and Modified LM Regressors, 1977–2014

Panel A: LM Regressors Including 36 Demographic Variables					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	–5.17 (3.33)	–3.91 (4.82)	–5.70** (2.45)	–1.38 (3.16)	–0.34 (1.71)
Panel B: LM Regressors with 6 DAW Demographic Variables					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	3.75 (5.92)	4.34 (7.85)	2.64 (4.02)	10.03** (4.81)	7.59** (3.72)
Panel C: LM Regressors with 6 DAW Demographic Variables and Adding Controls for Incarceration and Police					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	4.99 (5.50)	5.96 (7.20)	3.76 (4.29)	10.05** (4.54)	8.10** (3.63)

NOTE: All models include year- and state-fixed effects, and OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Panel A, 36 demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, only six demographic variables are included. In Panel C, only six demographic variables are included and controls are added for incarceration and police. For all three panels, other controls include the previous year's violent or property crime arrest rate (depending on the crime category of the dependent variable), state population, population density, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real retirement payments per person over 65. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. All figures reported in percentage terms.

increase crime (the coefficient is 219), but their female counterparts have an even greater dampening effect on crime (with a coefficient of –258). Both conflicting estimates (not shown in Appendix Table B1) are statistically significant at the 0.01 level, and they are almost certainly picking up noise rather than revealing true relationships. Bizarre results are common in the LM estimates among these 36 demographic variables.[44]

[44]Aneja et al. (2014) test for the severity of the multicollinearity problem using the 36 LM demographic variables, and the problem is indeed serious. The variance inflation factor (VIF) is shown to be in the range of 6 to 7 for the RTC variable in the LM dummy model when the 36 demographic controls are used. Using the six DAW variables reduces the multicollinearity for the RTC dummy to a tolerable level (with VIFs always below the desirable threshold of 5).

Table 4, Panel A shows the results of the LM panel data model estimated over the period 1977–2014. As seen above, the DAW model generated estimates that RTC laws raised violent and property crime (in the dummy model of Table 3), while the estimated impact on murders was too imprecise to be informative. The LM model generates no statistically significant estimates, except for an apparent decline in non-firearm-related murders. We can almost perfectly restore the DAW Table 3 findings, however, by simply limiting the inclusion of 36 highly collinear demographic variables to the more typical array used in the DAW regressions, as seen in Panel B of Table 4. This modified LM dummy variable model suggests that RTC laws increase violent and property crime, mimicking the DAW dummy variable model estimates, and this same finding persists if we add in controls for police and incarceration, as seen in Panel C of Table 4.

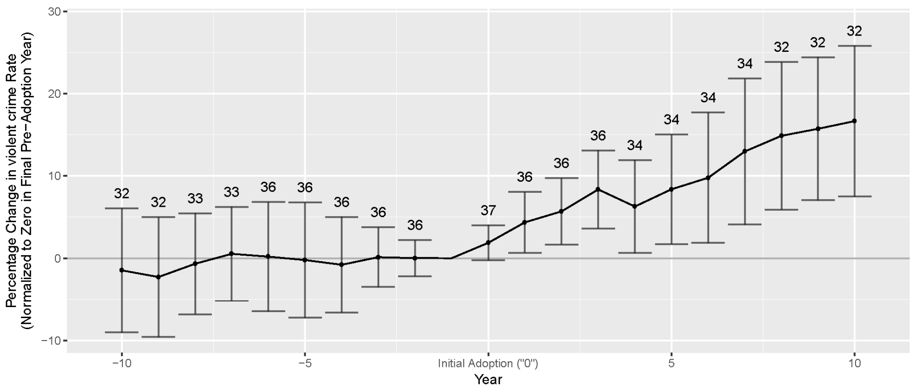
3. Testing the DAW and LM Models for the Parallel Trends Assumption

Many researchers are content to present panel data results such as those shown in Tables 3 and 4 without establishing their econometric validity. This can be a serious mistake. We have already registered concerns about the choice of controls included in the LM model, but, as we will see, the LM model regressions in Panel A of Table 4—including the spurious finding that RTC laws reduce non-firearm homicides—uniformly violate the critical assumption of parallel trends. In sharp contrast, the DAW model illustrates nearly perfect parallel trends in the decade prior to RTC adoption for violent crime and sufficiently satisfies this assumption in three of the other four regressions in Table 3 (murder, non-firearm murder, and property crime).

To implement this test and to provide more nuanced estimates of the impact of RTC laws on crime than in the simple dummy models of Tables 3 and 4, we ran regressions showing the values on yearly dummy variables for 10 years prior to RTC adoption to 10 years after RTC adoption. If the key parallel trends assumption of panel data analysis is valid, we should see values of the pre-adoption dummies that show no trend and are close to zero. Figure 2 shows that the DAW violent crime model performs extremely well: the pre-adoption dummies are virtually all zero (and hence totally flat) for the eight years prior to adoption, and violent crime starts rising in the year of adoption, showing statistically significant increases after the law has been in effect for at least a full year. The upward trend in violent crime continues for the entire decade after adoption. Figure 2 also highlights that the single dummy models of Tables 3 and 4 (which implicitly assume an immediate and constant post-adoption impact on crime) are mis-specified. Importantly, we can now see the exact timing and pattern of the estimated impact on crime, which can, and in this case does, provide further support for a causal interpretation of the estimated increase in violent crime.

In contrast to the ideal performance of the DAW violent crime model, all of the Table 4 regressions using the LM model perform extremely poorly. For example, consider the LM model for firearm murder depicted in Figure 3, which shows that there is

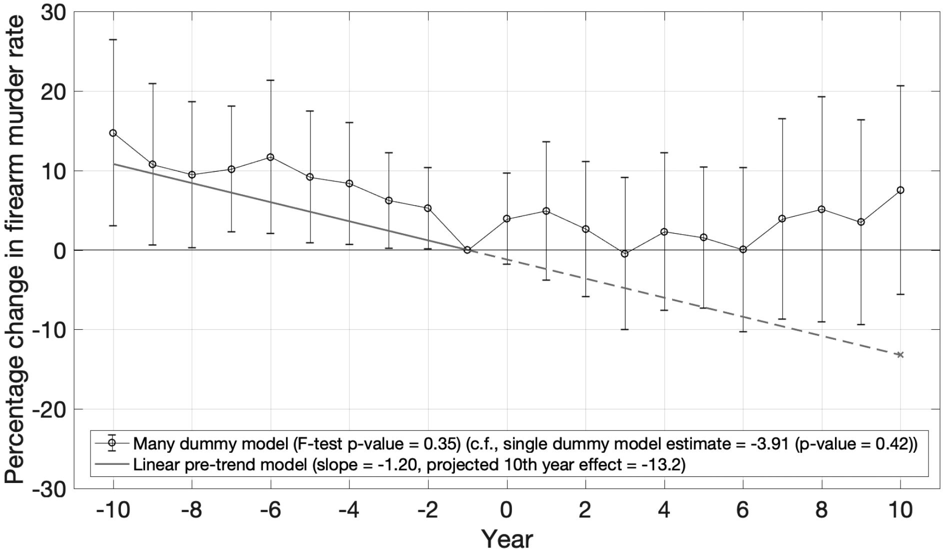
Figure 2: The impact of RTC laws on violent crime, DAW model, 1979–2014.



NOTE: We regress crime on dummies for pre– and post–passage years and DAW covariates. Reference year is year before adoption and adoption year is first year with RTC in place at any time, meaning that in states that adopt after January 1, this will capture only a partial effect of RTC laws. We display the 95 percent confidence interval for each estimate using cluster-robust standard errors and show the number of states that contribute to each estimate.

an enormously steep downward trend in the values of the pre-adoption dummies. Indeed, we see that the downward trend reverses just at the time of adoption of the RTC law and after six years we observe statistically significant increases in firearm

Figure 3: The impact of RTC laws on firearm murder, LM model, 1977–2014



murder above the prior trend. Thus, while Table 4 ostensibly showed a statistically insignificant 3.9 percent drop in violent crime, the more discerning analysis of Figure 3 shows that that estimate is econometrically invalid, given such an influential violation of the parallel trends requirement. In fact, the LM model estimated for Figure 3 provides evidence that the adoption of RTC laws reversed a previous benign trend starting exactly at the time of RTC adoption and led to higher levels of firearm homicide.

Appendix D depicts the same year-by -year estimates for the other crimes using both the DAW and LM models. It is worth noting that, for our entire data period, the four DAW and LM murder and firearm murder figures show an apparent malign break in trend at the time of RTC adoption, while the trend for non-firearm murder remains unchanged in the DAW and LM models. The unchanged downward trend in the LM non-firearm model illustrates the violation of the parallel trends assumption, invalidating the anomalous finding for that crime in Panel A of Table 4.[45]

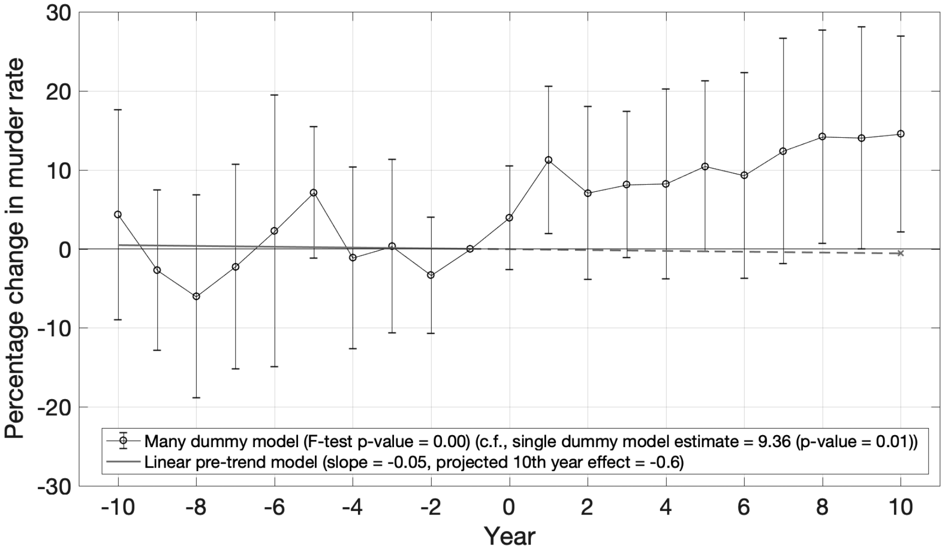
For the DAW and LM property crime panel data estimates, we see almost the same pattern. While the pre-adoption performance of the DAW property crime model (see Appendix Figure D2) is not quite as perfect as it was for violent crime, it still shows a roughly flat pattern for the eight years prior to adoption, followed by a persistent pattern of increasing property crime in the 10 years after RTC adoption. The increase in property crime turns statistically significant at the time of adoption. In Appendix Figure D3, however, we again see the same deficient pattern observed for the LM model in Appendix Figure D1: property crime falls in the 10 years prior to adoption, and the pattern reverses itself, leading to increasing property crime in the decade following RTC adoption.

We also conducted a panel data assessment looking at the 11 states that adopted RTC laws in the period from 2000–2014 when the confounding effect of the crack epidemic had subsided. The results provide further support that RTC laws increase crime, including estimates that overall murder and firearm murder rise substantially with RTC adoption. See further discussion and relevant figures and estimates in Appendix C. Figure 4 shows the year-by-year estimated effect of RTC laws on overall murder for the DAW model for this postcrack time period. The figure shows a flat pretrend (albeit with some variance around it) and then a sizeable jump in murder starting just at the year of RTC adoption. The LM model shows substantially the same statistically significant increase in murder.

---

[45]Appendix Figure D1 also illustrates why the LM dummy model estimate on violent crime in Panel A of Table 4 was not positive and statistically significant (as it was for the DAW model in Table 3 and the modified LM models in Panels B and C of Table 4): Appendix Figure D1 reveals that, for the LM model, violent crime was trending down throughout the pre-adoption period, dropping from 5 percentage points to zero over that decade, at which point it reverses and violent crime increases to roughly a 6 percent increase by 10 years after RTC adoption. The v-shape pattern over that two-decade period leads the LM dummy model to obscure the increase in violent crime that is clearly seen in Appendix Figure D1.

Figure 4: The impact of RTC laws on murder, DAW model, 2000–2014



B. Summary of Panel Data Analysis

The uncertainty about the impact of RTC laws on crime expressed in the NRC Report was based on an analysis of data only through 2000. The preceding evaluation of an array of different specifications over the full data period from the late 1970s through 2014 as well as in the postcrack period has given consistent evidence that something bad happened to murder and violent and property crime right at the time of RTC adoption. The most statistically significant crime increases for the full period were seen for DAW violent and property crime. For the postcrack period, the largest and most highly statistically significant increases were seen for murder and firearm murder.

Other work has also provided evidence that RTC laws increase murder and/or overall violent crime—see Zimmerman (2014), examining postcrack-era data and the recent work by Donohue (2017b) and Siegel et al. (2017) concluding that RTC laws increase firearm and handgun homicide. Work by McElroy and Wang (2017) reinforces this conclusion, with results from a dynamic model that accounts for forward-looking behavior finding that violent crime would be one-third lower if RTC laws had not been passed. We discuss other recent published studies finding that RTC laws increase violent crime in Appendix C.

Despite the substantial panel data evidence in the post-NRC literature that supports the finding of the pernicious influence of RTC laws on crime, the NRC suggestion that new techniques should be employed to estimate the impact of these laws is fitting. The important paper by Strnad (2007) used a Bayesian approach to argue that none of the published models used in the RTC evaluation literature rated highly in his model selection protocol when applied to data from 1977–1999.

Durlauf et al. attempt to sort out the different specification choices in evaluating RTC laws by using their own Bayesian model averaging approach using county data from 1979–2000. Applying this technique, the authors find that in their preferred spline (trend) model, RTC laws elevate violent crime in the three years after RTC adoption: "As a result of the law being introduced, violent crime increases in the first year and continues to increase afterwards" (2016:50). By the third year, their preferred model suggests a 6.5 percent increase in violent crime. Since their paper only provides estimates for three postpassage years, we cannot draw conclusions beyond this but note that their finding that violent crime increases by over 2 percent per year owing to RTC laws is a substantial crime increase. Moreover, the authors note: "For our estimates, the effect on crime of introducing guns continues to grow over time" (2016:50).[46]

Owing to the substantial challenges of estimating effects from observational data, it will be useful to see if yet another statistical approach that has different attributes from the panel data methodology can enhance our understanding of the impact of RTC laws. The rest of this article will use this synthetic control approach, which has been deemed "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey & Imbens 2017).

IV. Estimating the Impact of RTC Laws Using Synthetic Controls

The synthetic control methodology, which is becoming increasingly prominent in economics and other social sciences, is a promising new statistical approach for addressing the impact of RTC laws.[47] While most synthetic control papers focus on a single

[46]While our analysis focused on crime at the state level, there is obviously heterogeneity in crime rates within states, which is amalgamated into our population-weighted state average figures. A paper by Kovandzic et al. (KMV) buttresses the view that our state-focused estimates are not giving a misleading impression of the impact of RTC laws on violent crime. KMV limited their analysis to urban areas within each state, estimating the impact of RTC laws on crime using a panel data analysis from 1980–2000 on 189 cities with a population of 100,000 or more (Kovandzic et al. 2005). Although they did not estimate an overall violent crime effect, they did report that RTC laws were associated with a highly statistically significant increase in the rate of aggravated assault, the largest single component of violent crime. Their figures suggest that RTC laws led to a 20.1 percent increase in aggravated assault in the 10 years following adoption.

[47]The synthetic control methodology has been deployed in a wide variety of fields, including health economics (Nonnemaker et al. 2011), immigration economics (Bohn et al. 2014), political economy (Keele 2009), urban economics (Ando 2015), the economics of natural resources (Mideksa 2013), and the dynamics of economic growth (Cavallo et al. 2013).

treatment in a single geographic region, we look at 33 RTC adoptions occurring over three decades throughout the country. For each adopting ("treated") state we will find a weighted average of other states ("a synthetic control") designed to serve as a good counterfactual for the impact of RTC laws because it had a pattern of crime similar to that of the adopting state prior to RTC adoption. By comparing what actually happened to crime after RTC adoption to the crime performance of the synthetic control over the same period, we generate estimates of the causal impact of RTC laws on crime.[48]

A. *The Basics of the Synthetic Control Methodology*

The synthetic control method attempts to generate representative counterfactual units by comparing a treatment unit (i.e., a state adopting an RTC law) to a set of control units across a set of explanatory variables over a preintervention period. The algorithm searches for similarities between the treatment state of interest and the control states during this period and then generates a synthetic counterfactual unit for the treatment state that is a weighted combination of the component control states.[49] Two conditions are placed on these weights: they must be nonnegative and they must sum to 1. In general, the matching process underlying the synthetic control technique uses pretreatment values of both the outcome variable of interest (in our case, some measure of crime) and other predictors believed to influence this outcome variable.[50] For the reasons set forth in Appendix K, we use every lag of the dependent variable as predictors in the DAW and LM specifications. Once the synthetic counterfactual is generated and the weights associated with each control unit are assigned, the *synth* program then calculates values for the outcome variable associated with this counterfactual and the root mean squared prediction error (RMSPE) based on differences between the treatment and synthetic control units in the pretreatment period. The effect of the treatment can then be estimated by comparing the actual values of the dependent variable for the treatment unit to the corresponding values of the synthetic control.

B. *Generating Synthetic Controls for 33 States Adopting RTC Laws During Our Data Period*

To illustrate the procedure outlined above, consider the case of Texas, whose RTC law went into effect on January 1, 1996. The potential control group for each treatment state

---

[48]For a more detailed technical description of this method, we direct the reader to Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2014).

[49]Our analysis is done in Stata using the *synth* software package developed by Alberto Abadie, Alexis Diamond, and Jens Hainmueller.

[50]Roughly speaking, the algorithm that we use finds **W** (the weights of the components of the synthetic control) that minimizes $\sqrt{(\mathbf{X_1} - \mathbf{X_0 W})' \mathbf{V} (\mathbf{X_1} - \mathbf{X_0 W})}$, where **V** is a diagonal matrix incorporating information about the relative weights placed on different predictors, **W** is a vector of nonnegative weights that sum to 1, $\mathbf{X_1}$ is a vector containing pretreatment information about the predictors associated with the treatment unit, and $\mathbf{X_0}$ is a matrix containing pretreatment information about the predictors for all the control units.

consists of all nine states with no RTC legislation as of the year 2014, as well as states that pass RTC laws at least 10 years after the passage of the treatment state (e.g., in this case, the five states passing RTC laws after 2006, such as Nebraska and Kansas, whose RTC laws went into effect at the beginning of 2007). Since we estimate results for up to 10 years postpassage,[51] this restriction helps us avoid including states with their own permissive concealed carry laws in the synthetically constructed unit (which would mar the control comparison).

After entering the necessary specification information into the *synth* program (e.g., treatment unit, list of control states, explanatory variables, etc.), the algorithm proceeds to construct the synthetic unit from the list of control states specific to Texas and generates values of the dependent variable for the counterfactual for both the pretreatment and posttreatment periods. The rationale behind this methodology is that a close fit in the prepassage time series of crime between the treatment state and the synthetic control generates greater confidence in the accuracy of the constructed counterfactual. Computing the posttreatment difference between the dependent variables of the treatment state and the synthetic control unit provides the synthetic control estimate of the treatment effect attributable to RTC adoption in that state.

1. Synthetic Control Estimates of Violent Crime in Two States

Figure 5 shows the synthetic control graph for violent crime in Texas over the period from 1977 through 2006 (10 years after the adoption of Texas's RTC law). The solid black line shows the actual pattern of violent crime for Texas, and the vertical line indicates when the RTC law went into effect. Implementing the synthetic control protocol identifies three states that generate a good fit for the pattern of crime experienced by Texas in the pre-1996 period. These states are California, which gets a weight of 57.7 percent owing to its similar attributes compared to Texas, Nebraska with a weight of 9.7 percent, and Wisconsin with a weight of 32.6 percent.

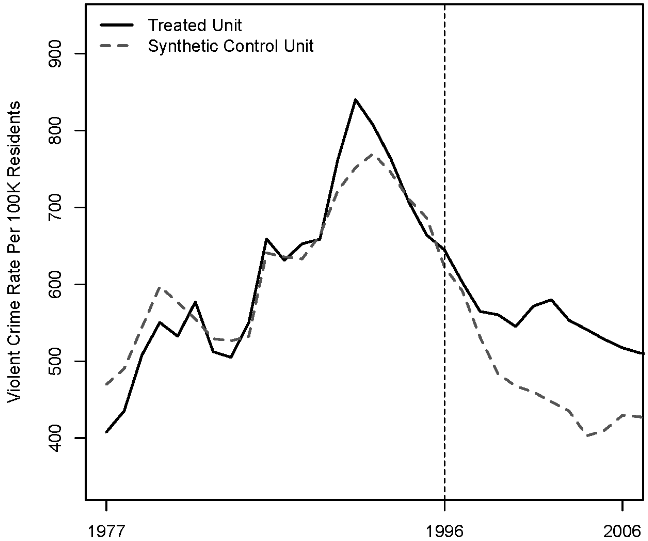
One of the advantages of the synthetic control methodology is that one can assess how well the synthetic control (call it "synthetic Texas," which is identified in Figure 5 by the dashed line) matches the pre-RTC-passage pattern of violent crime to see whether the methodology is likely to generate a good fit in the 10 years of postpassage data. Here the fit looks rather good in mimicking the rises and falls in Texas violent crime from 1977–1995. This pattern increases our confidence that synthetic Texas will provide a good prediction of what would have happened in Texas had it not adopted an RTC law.

Looking at Figure 5, we see that while both Texas and synthetic Texas (the weighted average violent crime performance of the three mentioned states) show declining crime rates in the postpassage decade after 1996, the crime drop is

---

[51]Our choice of 10 years is informed by the tradeoffs associated with using a different timeframe. Tables 5 and 6 indicate that the increase in violent crime due to RTC laws is statistically significant at the .01 level for all years after seven years post-adoption.

Figure 5: Texas: Violent crime rate.



Effect of 1996 RTC Law 10 Years After Adoption: 16.9%
NOTE: Passage Year Difference From SC: 3.6% Composition of SC: CA (0.577); NE (0.097); WI (0.326) CVRMSPE: 0.06 (8 of 33 states, where 1 denotes the state with the best pre-passage fit.).
States Never Passing RTC Laws Included in Synthetic Control: CA;
RTC Adopting States Included in Synthetic Control: NE (2007); WI (2012).

substantially greater in synthetic Texas, which had no RTC law over that period, than in actual Texas, which did. As Figure 5 notes, 10 years after adopting its RTC law, violent crime in Texas was 16.9 percent *higher* than we would have expected had it not adopted an RTC law.[52]

Figure 5 also illustrates perhaps the most important lesson of causal inference: one cannot simply look before and after an event to determine the consequence of the event. Rather, one needs to estimate the difference between what did unfold and the counterfactual of what would have unfolded without the event. The value of the synthetic control methodology is that it provides a highly transparent estimate of that counterfactual, using a tool designed to ensure the validity of the parallel trends assumption that we have already seen is so critical to achieving meaningful causal estimates. Thus, when Lott

---

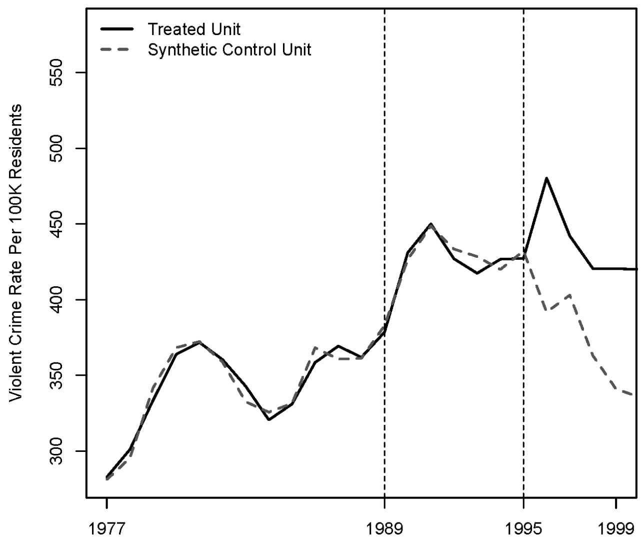
[52]Texas's violent crime rate 10 years post-adoption exceeds that of "synthetic Texas" by 20.41 percent $= \frac{517.3-429.6}{429.6} \times 100\%$. While some researchers would take that value as the estimated effect of RTC, we chose to subtract off the discrepancy in 1996 between the actual violent crime rate and the synthetic control value in that year. This discrepancy is 3.55 percent $= \frac{644.4-622.3}{622.3} \times 100\%$ (shown in the line just below the graph of Figure 5). See footnote 58 for further discussion of this calculation. Figure 5 shows a (rounded) estimated violent crime increase in Texas of 16.9 percent. We arrive at this estimate by subtracting the 1996 discrepancy of 3.55 percent from the 20.41 percent 10th-year discrepancy, which generates a TEP of 16.86 percent.

(2010) quotes a Texas District Attorney suggesting that he had reversed his earlier opposition to the state's RTC law in light of the perceived favorable experience with the law, we see why it can be quite easy to draw the inaccurate causal inference that Texas's crime decline was facilitated by its RTC law. The public may perceive the falling crime rate post-1996 (the solid black line), but our analysis suggests that Texas would have experienced a more sizable violent crime decline if it had not passed an RTC law (the dotted line). More specifically, Texas experienced a 19.7 percent decrease in its aggregate violent crime rate in the 10 years following its RTC law (between 1996 and 2006), while the state's synthetic control experienced a larger 31.0 percent decline. This counterfactual would not be apparent to residents of the state or to law enforcement officials, but our results suggest that Texas's RTC law imposed a large social cost on the state.

The greater transparency of the synthetic control approach is one advantage of this methodology over the panel data models that we considered above. Figure 5 makes clear what Texas is being compared to, and we can reflect on whether this match is plausible and whether anything other than RTC laws changed in these three states during the post-passage decade that might compromise the validity of the synthetic control estimate of the impact of RTC laws.

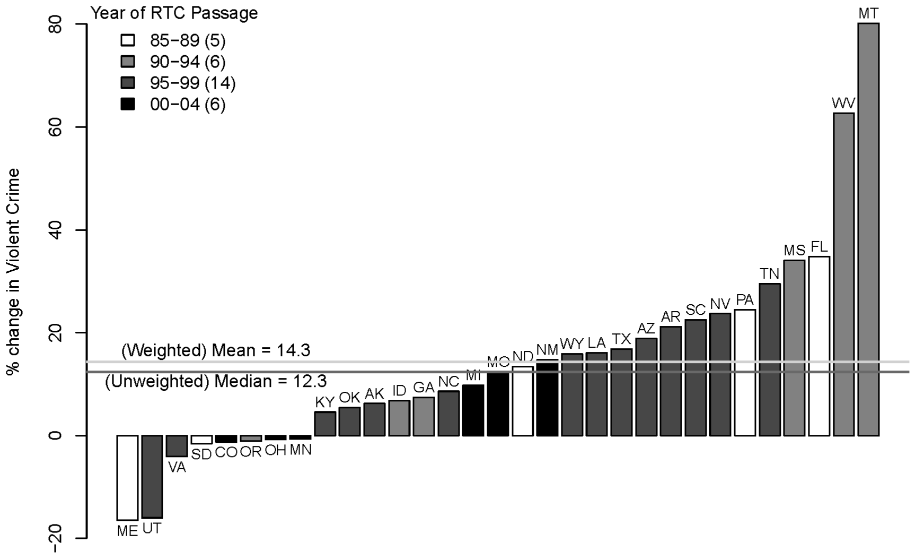
Figure 6 shows our synthetic control estimate for Pennsylvania, which adopted an RTC law in 1989 that did not extend to Philadelphia until a subsequent law went into

Figure 6: Pennsylvania: Violent crime rate.



Effect of 1989 RTC Law 10 Years After Adoption: 24.4%
NOTE: Passage Year Difference From SC: -1.1%. Composition of SC: DE (0.078); HI (0.073); MD (0.038); NE (0.016); NJ (0.103); OH (0.27); WI (0.424) CVRMSPE: 0.017 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.).
States Never Passing RTC Laws Included in Synthetic Control: DE; HI; MD; NJ;
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004); WI (2012).

Figure 7: The effect of RTC laws on violent crime after 10 years, synthetic control estimates for 31 states (1977–2014).



effect on October 11, 1995. In this case, synthetic Pennsylvania is comprised of eight states and the prepassage fit is nearly perfect. Following adoption of the RTC laws, synthetic Pennsylvania shows substantially better crime performance than actual Pennsylvania after the RTC law is extended to Philadelphia in late 1995, as illustrated by the second vertical line at 1996. The synthetic control method estimates that RTC laws in Pennsylvania increased its violent crime rate by 24.4 percent after 10 years.[53]

2. State-Specific Estimates Across All RTC States

Because we are projecting the violent crime experience of the synthetic control over a 10-year period, there will undoubtedly be a deviation from the "true" counterfactual and our estimated counterfactual. If we were only estimating the impact of a legal change for a single state, we would have an estimate marred by this purely stochastic aspect of changing crime. Since we are estimating an average effect across a large number of states, the

[53]In Appendix I, we include all 33 graphs showing the path of violent crime for the treatment states and the synthetic controls, along with information about the composition of these synthetic controls, the dates of RTC adoption (if any) for states included in these synthetic controls, and the estimated treatment effect (expressed in terms of the percent change in a particular crime rate) 10 years after adoption (or seven years after adoption for two states that adopted RTC laws in 2007, since our data end in 2014). The figures also document the discrepancy in violent crime in the year of adoption between the actual and synthetic control values.

stochastic variation will be diminished as the overestimates and underestimates will tend to wash out in our mean treatment estimates. Figure 7 shows the synthetic control estimates on violent crime for all 31 states for which we have 10 years of postpassage data. For 23 of the 31 states adopting RTC laws, the increase in violent crime is noteworthy.[54] Although three states were estimated to have crime reductions greater than the –1.6 percent estimate of South Dakota, if one averages across all 31 states, the (population-weighted) mean treatment effect after 10 years is a 14.3 percent *increase* in violent crime. If one instead uses an (unweighted) median measure of central tendency, RTC laws are seen to *increase* crime by 12.3 percent.

3. Less Effective Prepassage Matches

Section IV.B.1 provided two examples of synthetic controls that matched the crime of the treatment states well in the prepassage period, but this does not always happen. For example, we would have considerably less confidence in the quality of the synthetic control estimates for Maine, whose poor estimate is depicted in Appendix Figure I11. Maine also happens to be the state showing the greatest reduction in violent crime following RTC adoption, as indicated in Figure 7.

For Maine, one sees that the synthetic control and the state violent crime performance diverged long before RTC adoption in 1986, and that, by the date of adoption, Maine's violent crime rate was already 37.9 percent below the synthetic control estimate. The violent crime rate of actual Maine was trending down, while the synthetic control estimate had been much higher and trending up in the immediate pre-adoption period. The difficulty in generating good prepassage matches for states like Maine stems from their unusually low violent crime in the prepassage period.

Appendix Figure D11 reproduces Figure 7 while leaving out the five states for which the quality of prepassage fit is clearly lower than in the remaining 26 states.[55] This knocks out North Dakota, South Dakota, Maine, Montana, and West Virginia, thereby eliminating three of the five outlier estimates at both ends of the scale, and leaving the mean and median effects of RTC laws relatively unchanged from Figure 7. As Appendix Figure D11 shows, the (weighted) mean increase in crime across the listed 26 RTC-adopting states is 13.7 percent while the (unweighted) median increase is now 11.1 percent. Increases in violent crime of this magnitude are troubling. Consensus estimates of the elasticity of crime with respect to incarceration hover around 0.15 today, which suggests that to offset the increase in crime caused by RTC adoption, the average RTC state would need to approximately double its prison population.

---

[54]The smallest of these, Kentucky, had an increase of 4.6 percent.

[55]In particular, for these five states, the prepassage CVRMSPE—that is, the RMSPE transformed into a coefficient of variation by dividing by the average prepassage crime rate—was 19 percent or greater. See note 61 for further discussion of this statistic.

V. Aggregation Analysis Using Synthetic Controls

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[56] We estimate the percentage difference in violent crime between each treatment (RTC-adopting) state and the corresponding synthetic control in both the year of the treatment and in the 10 years following it. This estimate of the treatment effect percentage (TEP) obviously uses data from fewer posttreatment years for the two treatment states[57] in which RTC laws took effect less than 10 years before the end of our sample.

We could use each of these 10 percentage differences as our estimated effects of RTC laws on violent crime for the 10 postpassage years, but, as noted above, we make one adjustment to these figures by subtracting from each the percentage difference in violent crime in the adoption year between the treatment and synthetic control states. In other words, if 10 years after adopting an RTC law, the violent crime rate for the state was 440 and the violent crime rate for the synthetic control was 400, one estimate of the effect of the RTC law could be 10 percent $\left(= \frac{440 - 400}{400}\right)$. Rather than use this estimate, however, we have subtracted from this figure the percentage difference between the synthetic and treatment states in the year of RTC adoption. If, say, the violent crime rate in the treatment state that year was 2 percent higher than the synthetic control value, we would subtract 2 from 10 to obtain an estimated 10th-year effect of RTC laws of 8 percent.[58] We

[56]The closest paper to the present study is Arindrajit Dube and Ben Zipperer (2013), who introduce their own methodology for aggregating multiple events into a single estimated treatment effect and calculating its significance. Their study centers on the effect of increases in the minimum wage on employment outcomes, and, as we do, the authors estimate the percentage difference between the treatment and the synthetic control in the posttreatment period. While some papers analyze multiple treatments by aggregating the areas affected by these treatments into a single unit, this approach is not well-equipped to deal with a case such as RTC law adoption where treatments affect the majority of panel units and more than two decades separate the dates of the first and last treatment under consideration, as highlighted in Figure 7.

[57]These two states are Kansas and Nebraska, which adopted RTC laws in 2007. See note 4 discussing the states for which we cannot estimate the impact of RTC laws using synthetic controls.

[58]It is unclear ex ante whether one should implement this subtraction. The intuitive rationale for our choice of outcome variable was that pretreatment differences between the treatment state and its synthetic control at the time of RTC adoption likely reflected imperfections in the process of generating a synthetic control and should not contribute to our estimated treatment effect if possible. In other words, if the treatment state had a crime rate that was 5 percent greater than that of the synthetic control in both the pretreatment and posttreatment period, it would arguably be misleading to ignore the pretreatment difference and declare that the treatment increased crime rates by 5 percent. On the other hand, subtracting off the initial discrepancy might be adding noise to the subsequent estimates.

We resolve this issue with the following test of our synthetic control protocol: we pretend that each RTC-adopting state actually adopted its RTC law five years before it did. We then generate synthetic control estimates of this phantom law over the next five years of actual pretreatment data. If our synthetic control approach is working perfectly, it should simply replicate the violent crime pattern for the five pretreatment years. Consequently, the estimated "effect" of the phantom law should be close to zero. Indeed, when we follow our subtraction protocol, the synthetic controls match the pretreatment years more closely than when we do not provide this normalization. Specifically, with subtraction the estimated "effect" in the final pretreatment year is a wholly insignificant 3.2 percent; without subtraction, it jumps to a statistically significant 5.3 percent. Consequently, then look across all the state-specific estimates of the impact of RTC laws on violent crime for each of the 10 individual postpassage years and test whether they are significantly different from zero.[59]

A. RTC Laws Increase Violent Crime

We begin our analysis of the aggregated synthetic control results using predictors derived from the DAW specification. Table 5 shows our results on the full sample examining violent crime.[60] Our estimates of the normalized average treatment effect percentage (TEP) suggest that states that passed RTC laws experienced more deleterious changes in violent criminal activity than their synthetic controls in the 10 years after adoption. On average, treatment states had aggregate violent crime rates that were almost 7 percent higher than their synthetic controls five years after passage and around 14 percent higher 10 years after passage. Table 5 suggests that the longer the RTC law is in effect (up to the 10th year that we analyze), the greater the cost in terms of increased violent crime.

As we saw in Figures 6 (Pennsylvania) and I11(Maine), the validity of using the posttreatment difference between crime rates in the treatment state (the particular state adopting an RTC law that we are analyzing) and its corresponding synthetic control as a measure of the effect of the RTC law depends on the strength of the match between these two time series in the pretreatment period. To generate an estimate of pretreatment fit that takes into account differences in pretreatment crime levels, we estimate the coefficient of variation for the root mean squared prediction error (RMSPE), which

---

normalization is the preferred approach for violent crime. It should also be noted that our actual synthetic control estimates will be expected to perform better than this phantom RTC estimate since we will be able to derive our synthetic controls from five additional years of data, thereby improving our pretreatment fit.

As it turns out, the choice we made to subtract off the initial-year crime discrepancy is a conservative one, in that the estimated crime increases from RTC laws would be *greater* without subtraction. We provide synthetic control estimates for the DAW model without subtraction of the adoption-year percentage difference for violent crime, murder, and property crime in Appendix F. Comparison of these Appendix F estimates with those in the text (Table 5) reveals that our preferred method of subtracting yields more conservative results (i.e., a smaller increase in violent crime due to RTC). In Table 5, we estimate the 10th-year TEP for violent crime as roughly 13.5 to 14.3 percent, while the comparable estimates without subtraction are roughly 17–18 percent, as seen in Appendix Tables F1, F2, and F3. Indeed, without subtraction, every estimated impact would show RTC laws lead to a statistically significant increase in every crime category we consider except non-firearm homicide, as seen in Appendix F.

[59]This test is performed by regressing these differences in a model using only a constant term and examining whether that constant is statistically significant. These regressions are weighted by the population of the treatment state in the posttreatment year under consideration. Robust standard errors corrected for heteroskedasticity are used in this analysis.

[60]We discuss the synthetic control estimates for murder and property crime in Section V.F.

Table 5: The Impact of RTC Laws on the Violent Crime Rate, DAW Covariates, Full Sample, 1977–2014

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Average normalized treatment	–0.117	2.629*	3.631*	4.682**	6.876***	7.358**	10.068***	12.474***	14.021***	14.344***
effect percentage (TEP)	(1.076)	(1.310)	(1.848)	(2.068)	(2.499)	(3.135)	(2.823)	(3.831)	(3.605)	(2.921)
N	33	33	33	33	33	33	33	31	31	31
Pseudo *p* value	0.936	0.274	0.220	0.192	0.094	0.106	0.060	0.038	0.032	0.032

NOTE: Standard errors in parentheses. Column numbers indicate postpassage year under consideration; N = number of states in sample. The synthetic controls method is run using the nested option, and each year's estimate and statistical significance is computed as explained in note 59. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

is the ratio of the synthetic control's pretreatment RMSPE to the pretreatment average level of the outcome variable for the treatment state.[61]

To evaluate the sensitivity of the aggregate synthetic control estimate of the crime impact of RTC laws in Table 5, we consider two subsamples of treatment states: states whose coefficients of variation are less than two times the average coefficient of variation for all 33 treatments and states whose coefficients of variation are less than this average. We then rerun our synthetic control protocol using each of these two subsamples to examine whether restricting our estimation of the average treatment effect to states for which a relatively "better" synthetic control could be identified would meaningfully change our findings.

All three samples yield roughly identical conclusions: RTC laws are consistently shown to increase violent crime, with the 10th-year increase ranging from a low of 13.5 (when we remove the six states with above-average values of the CV RMSPE) to a high of 14.3 percent (Table 5).

B. The Placebo Analysis

Our ability to make valid inferences from our synthetic control estimates depends on the accuracy of our standard error estimation. To test the robustness of the standard errors that we present under the first row of Table 5, we incorporate an analysis using placebo treatment effects similar to Ando (2015).[62] For this analysis, we generate 500 sets of randomly generated RTC dates that are designed to resemble the distribution of actual RTC

[61]While the RMSPE is often used to assess this fit, we believe that the use of this measure is not ideal for comparing fit across states, owing to the wide variation that exists in the average pretreatment crime rates among the 33 treatment states that we consider. For example, the pretreatment RMPSE associated with our synthetic control analysis using the DAW predictor variables and aggregate violent crime as the outcome variable is nearly identical for Texas (37.1) and Maine (36.4), but the pretreatment levels of Texas's aggregate violent crime rate are far greater than Maine's. To be more specific, Texas's average violent crime rate prior to the implementation of its RTC law (from 1977 through 1995) was 617 violent crimes per 100,000 residents, while the corresponding figure for Maine was 186 violent crimes per 100,000 residents, less than one-third of Texas's rate. The more discerning CV of the RMSPE is 0.06 for Texas (with a year of adoption discrepancy of only 3.6 percent), while for Maine, the CV is a dramatically higher at 0.196 (with an initial year discrepancy of –37.9 percent). Accordingly, since the percentage imprecision in our synthetic pretreatment match for Maine is so much greater than for Texas, we have greater confidence in our estimates that in the 10th year, Texas's RTC law had increased violent crime by 16.9 percent than we do in an estimate that Maine's law had decreased violent crime by 16.5 percent.

[62]Ando (2015) examines the impact of constructing nuclear plants on local real per capita taxable income in Japan by generating a synthetic control for every coastal municipality that installed a nuclear plant. Although the average treatment effect measured in our article differs from the one used by Ando, we follow Ando in repeatedly estimating average placebo effects by randomly selecting different areas to serve as placebo treatments. (The sheer number of treatments that we are considering in this analysis prevents us from limiting our placebo treatment analysis to states that never adopt RTC laws, but this simply means that our placebo estimates will likely be biased *against* finding a qualitatively significant effect of RTC laws on crime, since some of our placebo treatments will be capturing the effect of the passage of RTC laws on crime rates.) Our estimated average treatment effect can then be compared to the distribution of average placebo treatment effects. Heersink and Peterson (2016) and Cavallo et al. (2013) also perform a similar randomization procedure to estimate the significance of their estimated average treatment effects, although the randomization procedure in the latter paper differs from ours by restricting the timing of placebo treatments to the exact dates when actual treatments took place.

passage dates that we use in our analysis.[63] For each of the 500 sets of randomly generated RTC dates, we then use the synthetic control methodology and the DAW predictors to estimate synthetic controls for each of the 33 states whose randomly generated adoption year is between 1981 and 2010. We use these data to estimate the percentage difference between each placebo treatment and its corresponding synthetic control during both the year of the treatment and each of the 10 posttreatment years (for which we have data) that follow it. Using the methodology described in notes 52 and 58, we then test whether the estimated treatment effect for each of the 10 posttreatment years is statistically significant.

To further assess the statistical significance of our results, we compare each of the 10 coefficient estimates in Table 5 with the distribution of the 500 average placebo treatment effects that use the same crime rate, posttreatment year, and sample as the given estimate. To assist in this comparison process, we report a pseudo p value that is equal to the proportion of our placebo treatment effects whose absolute value is greater than the absolute value of the given estimated treatment effect. This pseudo p value provides another intuitive measure of whether our estimated average treatment effects are qualitatively large compared to the distribution of placebo effects. Our confidence that the treatment effect that we are measuring for RTC laws is real increases if our estimated treatment effect is greater than the vast majority of our estimated average placebo treatment effects. Examining our pseudo p values in Table 5, we see that our violent crime results are always statistically significant in comparison to the distribution of placebo coefficients at the 0.05 level eight years or more past RTC adoption.

C. Synthetic Control Estimates Using LM's Explanatory Variables

In our Section III panel data analysis, we saw that RTC laws were associated with significantly higher rates of violent crime in the DAW model (Table 3), but not in the LM model (Table 4, Panel A). Under the synthetic controls approach, however, we find that the results are the same whether one uses the DAW or LM explanatory variables. This is necessarily true when one uses yearly lags in implementing the synthetic controls – see Kaul et al. (2016) – but it is also true when we use three lags of the dependent variable in our synthetic control protocol, as shown in Table 6. The detrimental effects of RTC laws on violent crime rates are statistically significant at the 0.05 level starting three years after the passage of an RTC law, and appear to increase over time. The treatment effects associated with violent crime in Table 6 range from 9.6 percent in the seventh posttreatment year to 12.8 percent in the 10th posttreatment year. Remarkably, the DAW and LM synthetic control estimates of the impact of RTC laws on violent crime are nearly identical

[63]More specifically, we randomly choose eight states to never pass RTC laws, six states to pass RTC laws before 1981, 33 states to pass RTC laws between 1981 and 2010, and three states to pass their RTC laws between 2011 and 2014. (Washington, DC is not included in the placebo analysis since it is excluded from our main analysis.) These figures were chosen to mirror the number of states in each of these categories in our actual data set.

Table 6: The Impact of RTC Laws on the Violent Crime Rate, LM covariates, Full Sample, 1977–2014

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Average Normalized TEP	0.309	1.981	4.063*	5.211*	7.159**	6.981**	9.644***	11.160***	12.115***	12.794***
	(1.318)	(1.646)	(2.192)	(2.572)	(2.887)	(3.319)	(3.016)	(3.680)	(3.857)	(3.200)
N	33	33	33	33	33	33	33	31	31	31

NOTE: Standard errors in parentheses. Column numbers indicate post-passage year under consideration; N = number of states in sample. The synthetic controls method is run using the non-nested option, and each year's estimate and statistical significance is computed as explained in footnote 59. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

(compare Tables 6 and Appendix Table K1), and this is true even when we limit the sample of states in the manner described above.[64]

D. The Contributions of Donor States to the Synthetic Control Estimates: Evaluating Robustness

One of the key elements of the synthetic control approach is its selection among plausible control states. For each state adopting an RTC law in year X, the approach selects among states that do not have RTC laws through at least ten years after X, including never-adopting states. Appendix Figure D10 lists all the states that are eligible under this criterion to serve as synthetic controls for one or more of the 33 adopting states, and shows how often they are selected. The horizontal length of each bar tells us how much that state contributes to our synthetic control violent crime estimates.[65] As the figure indicates, Hawaii appears most frequently—contributing to a synthetic control 18 of the 33 times it is eligible and averaging a 15.2 percent contribution—but California, a substantial contributor to multiple large states, edges it out for the largest average contribution (18.1 percent).

Hawaii's relatively large contribution as a donor state in the synthetic control estimates has some advantages but also raises concern that this small state might be unrepresentative of the states for which it is used as a control. For example, note that the largest share of Virginia's synthetic control comes from Hawaii (27.9 percent), with Rhode Island, Kansas, and Nebraska making up the lion's share of the remaining synthetic control. We had already mentioned one problem with the panel data analysis caused by the tendency of lax gun control states to serve as a source for guns that contribute to crime in the non-RTC states, and Virginia has always been a major source of that interstate flow. Since Virginia's guns are not likely to end up in Hawaii, the bias that the treatment infects the control is reduced for that particular match. Nonetheless, one may be concerned that Hawaii might be unduly skewing the estimates of the impact of RTC laws on violent crime.

To address this, as well as the analogous concern for other potentially idiosyncratic control states, we generated 18 additional TEP estimates, with each one generated by dropping a single one of the 18 states that appears as an element of our synthetic control analysis (as identified in Appendix Figure D10). The results of this exercise are presented in Appendix Figure D12, which shows that our estimated increase in violent crime resulting from the adoption of an RTC law is extremely robust: All 18 estimates remain statistically significant at the 0.01 percent level, and

---

[64]The 10th-year effect in the synthetic control analysis using the LM variables is 12.4 percent when we eliminate the three states with more than twice the average CV of the RMSPE. Knocking out the seven states with above-average values of this CV generates a similar 12.5 percent effect.

[65]In particular, it reflects the portion of each synthetic state it becomes part of, weighted by the treated state's population. For example, Texas's population is 13.6 percent of the total treated states' population. As a result, a state that made up 50 percent of synthetic Texas (but is not a donor for any other treatment state) would have a bar of size 6.8 percent.

the smallest TEP, which comes from dropping Illinois as a control state, is 12.0 percent. Note in particular that dropping Hawaii from the list of potential donor states slightly *increases* the estimate of the increase in violent crime caused by RTC laws. In fact, when we dropped Hawaii completely as a potential control and repeated the previous protocol of dropping one state at a time, the estimated increase in violent crime from RTC never fell below 12 percent (which was the value when New York was dropped as well as Hawaii). Indeed, the synthetic control finding that RTC laws increase violent crime is so robust that even if we drop California, New York, and Hawaii from the pool of potential donor states, RTC laws still increase violent crime by 8.9 percent after 10 years ($p = 0.018$).

E. Does Gun Prevalence Influence the Impact of RTC Laws?

The wide variation in the state-specific synthetic control estimates that was seen in Figures 7 and D11 suggests that there is considerable noise in some of the outlier estimates of a few individual states. For example, it is highly improbable that RTC laws led to a 16.5 percent decrease in violent crime in Maine and an 80.2 percent increase in violent crime in Montana, the two most extreme estimates seen in Figure 7. Since averaging across a substantial number of states will tend to eliminate the noise in the estimates, one should repose much greater confidence in the aggregated estimates than in any individual state estimate. Indeed, the fact that we can average across 33 separate RTC-adopting states is what generates such convincing and robust estimates of the impact of RTC laws on violent crime.

Another way to distill the signal from the noise in the state-specific estimates is to consider whether there is a plausible factor that could explain underlying differences in how RTC adoption influences violent crime. For example, RTC laws might influence crime differently depending on the level of gun prevalence in the state.

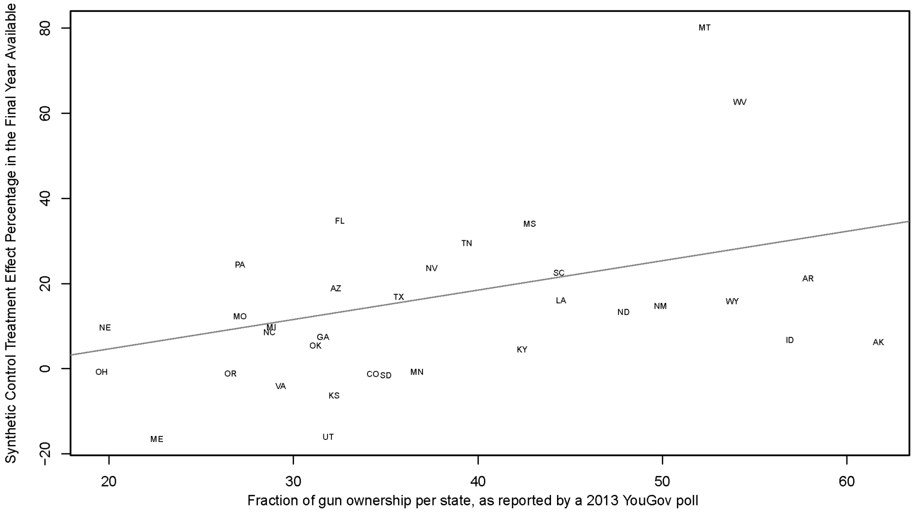
Figure 8 shows the scatter diagram for 33 RTC-adopting states, and relates the estimated impact on violent crime to a measure of gun prevalence in each RTC-adopting state. The last line of the note below the figure provides the regression equation, which shows that gun prevalence is positively related to the estimated increase in crime ($t = 2.39$).[66]

F. The Murder and Property Crime Assessments with Synthetic Controls

The synthetic control estimates of the impact of RTC laws on violent crime uniformly generate statistically significant estimates, and our phantom RTC law synthetic control estimates for the five pretreatment years (described in note 58) give us confidence that the synthetic control approach is working well for our violent crime estimates, as illustrated in Appendix Table L1. Since the estimated increases in violent crime are

[66]The gun prevalence data were collected by the data analytics firm YouGov in a 2013 online survey (Kalesan et al. 2016); 4,486 people were initially surveyed, although only 4,000 results are used in the final data set. YouGov used a proximity matching method to select the survey results for inclusion, matching respondents by race, age, gender, and education to the demographic breakdown of the 2010 American Community Survey.

Figure 8: The impact of gun ownership on the increase in violent crime due to RTC laws (synthetic control estimates, 1977–2014).



NOTE: Treatment effect displayed is for the 10th year after RTC adoption (but 7th post–passage year for Kansas and Nebraska). Treatment Effect = −9.15 + 0.69 * Gun Prevalence. t = 2.39; R^2 = 0.16. Regression weighted by population in the final TEP year.

statistically significant and consistently observed in both our panel data and synthetic control analyses, these represent our most robust finding.

Just as we saw in the panel data analysis, the synthetic controls provide evidence of increases in the murder and firearm murder categories, but it is weaker and less precise than our violent crime estimates. For example, both Appendix Tables E1 and E2 show estimated crime increases of 8.7 percent (murder) and 15.3 percent (firearm murder), but only the 8.7 figure is statistically significant at the 0.10 level. Interestingly, our phantom law test works well for murder and even suggests statistically significant increases in that crime beginning right at the time of RTC adoption (Appendix Table L3). The firearm murder estimates perform less well in this test, generating an estimated fall in crime of 6.8 percent in the year prior to RTC adoption (Appendix Table L5).

The results from implementing this phantom law approach for property crime are perhaps our less encouraging estimates. While our estimated "effect" in the year prior to adoption would ideally be close to zero in this test, for property crime it is 6.9 percent, with the latter significant at the 0.10 level. (The full results of this test for all the crime categories are shown in Appendix L.) If we accept our normalized estimate for the impact of RTC laws on property crime it would give little reason to reject a null hypothesis of no effect (Appendix Table E8). Because our synthetic control estimates for violent crime are validated by our phantom adoption test and generate uniform and highly robust results whether dropping selected donor states or states with poor fit, or using either the DAW or LM models, we have greater confidence in and therefore highlight our violent crime estimates. Accordingly, we consign our further discussion of the synthetic control estimates of murder and property crime to Appendix E.

VI. Conclusion

The extensive array of panel data and synthetic control estimates of the impact of RTC laws that we present uniformly undermine the "More Guns, Less Crime" hypothesis. There is not even the slightest hint in the data from any econometrically sound regression that RTC laws reduce violent crime. Indeed, the weight of the evidence from the panel data estimates as well as the synthetic control analysis best supports the view that the adoption of RTC laws substantially raises overall violent crime in the 10 years after adoption.

In our initial panel data analysis, our preferred DAW specification predicted that RTC laws have led to statistically significant and substantial increases in violent crime. We also presented both panel data and synthetic control estimates that RTC laws substantially increase the percentage of robberies committed with a firearm, while having no restraining effect on the overall number of robberies. Moreover, to the extent the massive theft of guns from carrying guns outside the home generates crime spillovers to non-RTC states, our estimated increases in violent crime are downward biased.

We then supplemented our panel data results using our synthetic control methodology, and the finding from our panel data analysis was strongly buttressed. Whether we used the DAW or LM specifications, states that passed RTC laws experienced 13–15 percent *higher* aggregate violent crime rates than their synthetic controls after 10 years (results that were significant at either the 0.05 or 0.01 level after five years).

The synthetic control effects that we measure represent meaningful increases in violent crime rates following the adoption of RTC laws, and this conclusion remained unchanged after restricting the set of states considered based on model fit and after considering a large number of robustness checks. The consistency across different specifications and methodologies of the finding that RTC elevates violent crime enables far stronger conclusions than were possible over a decade ago when the NRC Report was limited to analyzing data only through 2000 with the single tool of panel data evaluation.

The best available evidence using different statistical approaches—panel data regression and synthetic control—with varying strengths and shortcomings and with different model specifications all suggest that the net effect of state adoption of RTC laws is a substantial increase in violent crime.

References

Abadie, Alberto, Alexis Diamond, & Jens Hainmueller (2010) "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program," 105 (490) *J. of the American Statistical Association* 493.

——— (2014) Comparative Politics and the Synthetic Control Method," 59(2) *American J. of Political Science* 495.

Abadie, Alberto, & Javier Gardeazabal (2003) "The Economic Costs of Conflict: A Case Study of the Basque Country," 93(1) *American Economic Rev.* 113.

ABC News (2018) "Man Annoyed by IHOP Customer Before Allegedly Shooting Him in Head," *ABC News*. https://abc13.com/man-annoyed-by-ihop-customer-before-allegedly-shooting-him/3160627/

Adda, Jérôme, Brendon McConnell, & Imran Rasul (2014) "Crime and the Depenalization of Cannabis Possession: Evidence from a Policing Experiment," 122(5) *J. of Political Economy* 1130.

Ando, Michihito (2015) "Dreams of Urbanization: Quantitative Case Studies on the Local Impacts of Nuclear Power Facilities Using the Synthetic Control Method," 85 *J. of Urban Economics* 68.

Aneja, Abhay, John J. Donohue, & Alexandria Zhang (2011) "The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy," 13(2) *American Law & Economics Rev.* 565.

——— (2014) "The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy," *National Bureau of Economic Research Working Paper* 18294.

Associated Press (2014) "Official: Suspect in Deadly Hospital Shooting Had Lengthy History of Gun Arrests, Violence," July 26 *Fox News*. http://www.foxnews.com/us/2014/07/26/official-suspect-in-deadly-hospital-shooting-had-lengthy-history-gun-arrests.html

——— (2015) "8-Year-Old Arizona Boy Accidentally Shot by Baby Sitter," September 8 *Daily Record*. http://www.canoncitydailyrecord.com/ci_28778997/8-year-old-arizona-boy-accidentally-shot-by

Athey, Susan, & Guido W. Imbens (2017) "The State of Applied Econometrics: Causality and Policy Evaluation," 31(2) *J. of Economic Perspectives* 3.

Ayres, Ian, & John J. Donohue (2003) "The Latest Misfires in Support of the 'More Guns, Less Crime' Hypothesis," 55 *Stanford Law Rev.* 1371.

Barbash, Fred (2018) "Calif. Teacher Resigns After Unintentionally Firing Weapon in Gun Safety Class," April 12 *Washington Post*. https://www.washingtonpost.com/news/morning-mix/wp/2018/04/12/calif-teacher-resigns-after-unintentionally-firing-weapon-in-gun-safety-class/?noredirect=on&utm_term=.68faa7eb0133

Biette-Timmons, Nora (2017) "More People Are Pulling Guns During Road-Rage Incidents," August 10 *Trace*. https://www.thetrace.org/2017/08/guns-road-rage-cleveland-2017

Biography.com (2016) "Bernhard Goetz," November 15 *Online*. https://www.biography.com/people/bernhard-goetz-578520

Blair, J. Pete, & Katherine W. Schweit (2014) *A Study of Active Shooter Incidents in the United States Between 2000 and 2013*. Washington, DC: Texas State Univ. & Federal Bureau of Investigation, U.S. Department of Justice.

Bohn, Sarah, Magnus Lofstrom, & Steven Raphael (2014) "Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population?" 96(2) *Rev. of Economics & Statistics* 258.

Boots, Michelle Theriault (2017) "In Alaska, a High Bar for Taking Guns from the Mentally Ill," January 9 *Anchorage Daily News*. https://www.adn.com/alaska-news/2017/01/09/in-alaska-a-high-bar-for-the-mentally-ill-to-part-with-their-guns/

Bureau of Alcohol, Tobacco, Firearms, & Explosives (2012) *2012 Summary: Firearms Reported Lost and Stolen*. https://www.atf.gov/resource-center/docs/2012-firearms-reported-lost-and-stolenpdf-1/download

Bureau of Justice Statistics (2014) *The Nation's Two Measures of Homicide*. https://www.bjs.gov/content/pub/pdf/ntmh.pdf

Byers, Christine (2010) "Police Report Details AAB Shooting Chaos," November 18 *St. Louis Post-Dispatch*. https://www.stltoday.com/news/local/crime-and-courts/police-report-details-abb-shooting-chaos/article_bb52f36b-3757-5c95-a775-384c52dfd887.html

Calder, Chad (2018) "Legal Analysts Weigh in on Ronald Gasser's Defense in Joe McKnight Killing as Trial Set to Begin," January 15 *New Orleans Advocate*. https://www.theadvocate.com/new_orleans/news/courts/article_f51f97ca-fa45-11e7-a3eb-2fa820d7858f.html

Campbell, Elizabeth (2017) "521 Guns Stolen in 2017 from Unlocked Cars, Jacksonville Police Say," December 11 *News 4 Jax*. https://www.news4jax.com/news/local/jacksonville/521-guns-stolen-in-2017-from-unlocked-cars-jacksonville-police-say

Carter, Chelsea J., Ed Lavandera, & Evan Perez (2013) "Who Is Navy Yard Gunman Aaron Alexis?" *CNN*. http://www.cnn.com/2013/09/16/us/navy-yard-suspects/index.html

Cavallo, Eduardo, Sebastian Galiani, Ilan Noy, & Juan Pantano (2013) "Catastrophic Natural Disasters and Economic Growth," 95(5) *Rev. of Economics & Statistics* 1549.

CEA (2016) *Economic Perspectives on Incarceration and the Criminal Justice System*. Washington, DC: Council of Economic Advisors, Executive Office of the President of the United States.

Chalfin, Aaron, & Justin McCrary (2017) "Criminal Deterrence: A Review of the Literature," 55(1) *J. of Economic Literature* 5.

Clary, Mike, Megan O'Matz, & Lisa Arthur (2017) "Puerto Rico Police Seized Guns from Airport Shooter Esteban Santiago," January 13 *Sun Sentinel*. http://www.sun-sentinel.com/news/fort-lauderdale-hollywood-airport-shooting/fl-santiago-guns-puerto-rico-20170113-story.html

Cohen, Dov, Richard E. Nisbett, Brian F. Bowdle, & Norbert Schwarz (1996) "Insult, Aggression, and the Southern *Culture of Honor*: An 'Experimental Ethnography'," 70(5) *J. of Personality & Social Psychology* 945.

Cook, Philip J. (2018) "Gun Theft and Crime," 95(1) *J. of Urban Health* 305.

Cook, Philip J., Jens Ludwig, & Adam M. Samaha (2009) "Gun Control After *Heller*: Threats and Sideshows from a Social Welfare Perspective," 56(5) *UCLA Law Rev*. 1041.

DePrang, Emily (2015) "The Mystery of Milwaukee's 'Human Holster'," July 16 *Trace*. https://www.thetrace.org/2015/07/concealed-carry-wisconsin-human-holster/

Donohue, John J. (2003) "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis," 2 (3) *Criminology & Public Policy* 397.

——— (2017a) "Comey, Trump, and the Puzzling Pattern of Crime in 2015 and Beyond," 117(5) *Columbia Law Rev*. 1297.

——— (2017b) "Laws Facilitating Gun Carrying and Homicide," 107(12) *American J. of Public Health* 1864.

Donohue, John J., & Justin Wolfers (2009) "Estimating the Impact of the Death Penalty on Murder," 11(2) *American Law & Economics Rev*. 249.

Dube, Arindrajit, & Ben Zipperer (2013) "Pooling Multiple Case Studies Using Synthetic Controls: An Application to Minimum Wage Policies," *IZA Discussion Paper* 8944. https://ssrn.com/abstract=2589786

Durlauf, Steven N., Salvado Navarro, & David A. Rivers (2016) "Model Uncertainty and the Effect of Shall-Issue Right-to-Carry Laws on Crime," 81 *European Economic Rev*. 32.

Eltagouri, Marwa (2017) "Man Accidentally Shoots Himself and His Wife at a Church, Shortly After a Discussion on Shootings," November 17 *Washington Post*. https://www.washingtonpost.com/news/acts-of-faith/wp/2017/11/17/a-man-accidentally-shot-himself

Fernandez, Manny, Liam Stack, & Alan Blinder (2015) "9 Are Killed in Biker Gang Shootout in Waco," May 17 *New York Times*. http://www.nytimes.com/2015/05/18/us/motorcycle-gang-shootout-in-waco-texas.html

Ford, Richard (2016) "Richard Ford on America's Gun Problem," March 18 *Financial Times*. https://www.ft.com/content/d0cea3d0-eaab-11e5-bb79-2303682345c8

Fortin, Jacey (2018) "Georgia Teacher Fired Gun While Barricaded in Classroom, Police Say," February 28 *New York Times*. https://www.nytimes.com/2018/02/28/us/georgia-teacher-gun-shooting.html

Fox News (2016) "Ohio Gun Shop Owner Killed During Concealed Carry Class," June 19 *Fox News*. https://www.foxnews.com/us/ohio-gun-shop-owner-killed-during-concealed-carry-class

Freskos, Brian (2016) "Guns Are Stolen in America Up to Once Every Minute. Owners Who Leave Their Weapons in Cars Make it Easy for Thieves," September 21 *Trace*. https://www.thetrace.org/2016/09/stolen-guns-cars-trucks-us-atlanta/

——— (2017a) "As Thefts of Guns from Cars Surge, Police Urge Residents to Leave Their Weapons at Home," March 6 *Trace*. https://www.thetrace.org/2017/03/as-thefts-of-guns-from-cars-surge-police-urge-residents-to-leave-their- weapons-at-home/

——— (2017b) "Missing Pieces," November 20 *Trace*. https://www.thetrace.org/features/stolen-guns-violent-crime-america/

——— (2017c) "These Gun Owners Are at the Highest Risk of Having Their Firearms Stolen," April 11 *Trace*. https://www.thetrace.org/2017/04/gun-owners-high-risk-firearm-theft/

——— (2018a) "Citing *The Trace*'s Reporting, Top Gun Violence Scholar Calls for More Research on Threat of Stolen Firearms," April 26 *Trace*. https://www.thetrace.org/rounds/stolen-guns-research-agenda-phil-cook/

——— (2018b) "Maryland Will Invest in Gun Trafficking Crackdown," April 30 *Trace*. https://www.thetrace.org/2018/04/maryland-gun-trafficking-task-force-wiretapping-baltimore/

Friedman, David D. (1990) *Price Theory: An Intermediate Text*. South-Western Publishing Co. http://www.daviddfriedman.com/Academic/Price_Theory/PThy_Chapter_20/PThy_Chapter_20.html

Fuchs, Erin (2013) "Why the South Is More Violent Than the Rest of America," September 18 *Business Insider*. http://www.businessinsider.com/south-has-more-violent-crime-fbi-statistics-show-2013-9

Gibbons, Thomas, & Robert Moran (2000) "Man Shot, Killed in Snow Dispute," January 27 *Philadelphia Inquirer*. http://articles.philly.com/2000-01-27/news/25598207_1_snow-dispute-man-shot-christian-values

Glanton, Dahleen, & Carlos Sadovi (2014) "Concealed Carry Shooting Reignites Debate," July 31 *Chicago Tribune*. http://www.chicagotribune.com/news/ct-crestwood-concealed-carry-0730-20140730-story.html

Gueverra, Ericka Cruz (2018) "Man Killed by Armed PSU Officers Had Valid Concealed Carry Permit," June 30 *OPB*. https://www.opb.org/news/article/portland-state-shooting-victim-jason-erik-washington/

Hauser, Christine (2017) "White Police Officer in St. Louis Shoots Off-Duty Black Colleague," June 26 *New York Times*. https://www.nytimes.com/2017/06/26/us/saint-louis-black-officer.html?_r=0

Heath, Michelle (2015) "Gun Goes Off Inside Christus Facility, Injures Woman," October 19 *Beaumont Enterprise*. http://www.beaumontenterprise.com/news/article/Gun-goes-off-inside-Christus-facility-injures-6578001.php

Heersink, Boris, & Brenton Peterson (2016) "Measuring the Vice-Presidential Home State Advantage with Synthetic Controls," 44(4) *American Politics Research* 734.

Hemenway, David, Deborah Azrael, & Matthew Miller (2017) "Whose Guns Are Stolen? The Epidemiology of Gun Theft Victims," 4(1) *Injury Epidemiology* 11.

Hemenway, David, Mary Vriniotis, & Matthew Miller (2006) "Is an Armed Society a Polite Society? Guns and Road Rage," 38(4) *Accident Analysis and Prevention* 687.

Hermann, Peter, & Rachel Weiner (2019) "He Put 224 Guns on the Streets. His Family Would Pay a Price," January 24 *Washington Post*. https://www.washingtonpost.com/local/public-safety/he-put-224-guns-on-the-streets-his-family-would-pay-a-price/2019/01/23/68cd2520-1a57-11e9-8813-cb9dec761e73_story.html?utm_term=.8bfebbb0072e

Hernandez, Alex V. (2017) "Police: No Charges in Fatal Shootout at Elmwood Park Gas Station," April 10 *Chicago Tribune*. http://www.chicagotribune.com/suburbs/elmwood-park/news/ct-elm-elmwood-park-shooting-tl-0413-20170409-story.html

Ho, Vivian (2015) "Gun Linked to Pier Killing Stolen from Federal Ranger," July 8 *San Francisco Chronicle*. http://www.sfchronicle.com/crime/article/Gun-linked-to-S-F-pier-killing-was-BLM-6373265.php

Ho, Vivian, & Kale Williams (2015) "Gun in 2 Killings Stolen from Unlocked Car in Fisherman's Wharf, Cops Say," October 9 *San Francisco Chronicle*. http://www.sfgate.com/crime/article/Gun-in-2-killings-stolen-from-unlocked-car-in-6562039.php

Holzel, Dee (2008) "Shootout in Winnemucca: Three Dead, Two Injured in Early-Morning Gunfight," May 24 *Elko Daily Free Press*. https://elkodaily.com/news/local/shootout-in-winnemucca-three-dead-two-injured-in-early-morning/article_83fe3832-cc3b-528b-88bd-a85ce65f5967.html

Hopkins, Kyle (2017) "Accused Florida Airport Shooter to Appear in Alaska Case by Phone," March 28 *2 KTUU Anchorage*. http://www.ktuu.com/content/news/Diagnosed-with-serious-mental-illness-accused-airport-shooter-to-appear-in-Alaska-case-by-phone-417394013.html

Horwitz, Josh (2011) "Speaking of 'Fast and Furious': NRA Leaders Well-Versed in Fomenting Foreign Conflicts," September 13 *Huffington Post*. https://www.huffingtonpost.com/josh-horwitz/speaking-of-fast-and-furi_b_959633.html

Igielnik, Ruth, & Anna Brown (2017) *Key Takeaways on Americans' Views of Guns and Gun Ownership*. Pew Research Center. http://www.pewresearch.org/fact-tank/2017/06/22/key-takeaways-on-americans-views-of-guns-and-gun-ownership

Kalesan, Bindu, Marcos D. Villarreal, Katherine M. Keyes, & Sandro Galea (2016) "Gun Ownership and Social Gun Culture," 22(3) *Injury Prevention* 216.

Kalinowski, Bob (2012) "Police: Plymouth Homicide Suspect Shot by Patron," September 10 *Citizens' Voice*. http://citizensvoice.com/news/police-plymouth-homicide-suspect-shot-by-patron-1.1370815

Kaste, Martin (2019) "Gun Carry Laws Can Complicate Police Interactions," July 19 *NPR*. https://www.npr.org/2016/07/19/486453816/open-carry-concealed-carry-gun-permits-add-to-police-nervousness

Kaul, Ashok, Stefan Klobner, Gregor Pfeifer & Manuel Schieler (2016) "Synthetic Control Methods: Never Use All Pre-Intervention Outcomes as Economic Predictors."

Keele, Luke (2009) "An Observational Study of Ballot Initiatives and State Outcomes," *Working Paper*. https://www.researchgate.net/publication/228715196_An_observational_study_of_ballot_initiatives_and_state_outcomes

KHOU (2015) "One Man Injured After Carjacking, Shooting at Gas Station," September 27 *KHOU 11*. http://www.khou.com/news/one-man-injured-after-carjacking-shooting-at-gas-station/142447940

KIMT (2018) "Update: Court Documents Chronicle Tense Moments Prior to Rochester Shooting" January 17 *KIMT 3 News*. http://www.kimt.com/content/news/Rochester-shooting-Weiss-charged-with-2nd-degree-murder-469747873.html

Knight, Brian (2013) "State Gun Policy and Cross-State Externalities: Evidence from Crime Gun Tracing," 5(4) *American Economic J.: Economic Policy* 200.

Kovandzic, Tomislav, Thomas Marvell, & Lynne Vieraitis (2005) "The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates: Evidence from Panel Data for Large Urban Cities," 9 *Homicide Studies* 292.

KTUU (2017) "Esteban Santiago, Accused Fort Lauderdale Shooter, Agreed to Anger Management Courses in Alaska," January 9 *2 KTUU Anchorage*. http://www.ktuu.com/content/news/Esteban-Santiago-accused-Fort-Lauderdale-shooter-had-agreed-to-under-anger-management-in-Alaska-410177225.html

Lane, Emily (2018) "Cardell Hayes Again Claims Self-Defense in Will Smith Shooting Death: Appeal," February 15 *NOLA.com*. https://www.nola.com/crime/2018/02/cardell_hayes_self_defense_wil.html

Lat, David (2012) "*DiDonato v. Ung*: The Temple Law Shooter Gets Hit—With a Civil Suit," January 12 *Above the Law*. https://abovethelaw.com/2012/01/didonato-v-ung-the-sequelor-the-temple-law-shooter-gets-hit-with-a-lawsuit

Levenson, Eric (2017) "Judge Denies 'Stand Your Ground' Defense in Movie Theater Shooting," March 11 *CNN*. http://www.cnn.com/2017/03/10/us/stand-your-ground-movie-trial/index.html

Lopez, German (2018) "Police Shootings Are Also Part of America's Gun Problem," April 9 *Vox*. https://www.vox.com/2018/4/9/17205256/gun-violence-us-police-shootings

Lott, John R. (2010) *More Guns, Less Crime: Understanding Crime and Gun Control Laws*. Chicago, IL: Univ. of Chicago Press.

Lott, John R., & David B. Mustard (1997) "Crime, Deterrence, and Right-to-Carry Concealed Handguns," 26(1) *J. of Legal Studies* 1.

Lozano, Alicia Victoria (2017) "28-Year-Old David Desper Charged in Road Rage Killing of 18-Year-Old Bianca Roberson," July 2 *NBC Philadelphia*. https://www.nbcphiladelphia.com/news/local/Police-Update-on-Road-Rage-Killing-of-18-Yr-Old-432100983.html

Lunny, SanRay (2010) *Unloaded Open Carry*. San Mateo County Sheriff's Office. http://www.calgunlaws.com/wp-content/uploads/2012/09/San-Mateo-County-Sheriffs-Office_Unloaded-Open-Carry.pdf

Luthern, Ashley (2015) "Concealed Carry Draws Opposite Views—And a Murky Middle," June 11 *Milwaukee Wisconsin J. Sentinel*. http://www.jsonline.com/news/crime/concealed-carry-draws-opposite-views–and-a-murky-middle-b99510854z1-307079321.html

MacDonald, Sally (2012) "CHL Holder Fired Shot that Killed Store Clerk," May 31 *Free Republic*. http://www.freerepublic.com/focus/f-news/2889792/posts

McElroy, Majorie B., & Will Peichun Wang (2017) "Seemingly Inextricable Dynamic Differences: The Case of Concealed Gun Permit, Violent Crime and State Panel Data." https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2992058

McLaughlin, Eliott, & Madeline Holcombe (2018) "Mother of Man Killed by Police at Alabama Mall Ponders Open Casket as Family Seeks Justice," November 26 *CNN*. https://www.cnn.com/2018/11/25/us/alabama-shooting-family-seeks-answers/index.html

Mettler, Katie (2016) "'He Thought He Could Help': Concealed Carry Gun-Wielder Intervenes in Domestic Dispute and Is Shot Dead," May 3 *Washington Post*. https://www.washingtonpost.com/news/morning-mix/wp/2016/05/03/he-thought-he-could-help

Mideksa, Torben K. (2013) "The Economic Impact of Natural Resources," 65(2) *J. of Environmental Economics & Management* 277.

Miller, Matthew, Deborah Azrael, David Hemenway, & Frederic I. Solop (2002) "'Road Rage' in Arizona: Armed and Dangerous," 34(6) *Accident Analysis & Prevention* 807.

Moody, Carlisle E., & Thomas B. Marvell (2008) "The Debate on Shall-Issue Laws," 5(3) *Econ J. Watch* 269.

Moody, Carlisle E., Thomas B. Marvell, Paul R. Zimmerman, & Fasil Alemante (2014) "The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication," 4 *Rev. of Economics & Finance* 33.

Morin, Rich, & Andrew Mercer (2017) *A Closer Look at Police Officers Who Have Fired Their Weapon on Duty*. Pew Research Center. https://www.pewresearch.org/fact-tank/2017/02/08/a-closer-look-at-police-officers-who-have-fired-their-weapon-on-duty/

Murdock, Jason (2018) "Arizona Man Accidentally Shoots Himself in Groin in Walmart," November 29 *Newsweek*. https://www.newsweek.com/arizona-man-accidentally-shoots-himself-groin-walmart-1236287

Nagin, Daniel S. (Forthcoming) "Firearm Availability and Police Use of Force," *Annals of American Academy of Political & Social Science*.

National Research Council (2005) *Firearms and Violence: A Critical Review*. Washington, DC: National Academies Press.

NBC News (2014) "Cost of Bravery: Vegas Bystander Died Trying to Stop Rampage," June 10 *NBC News*. https://www.nbcnews.com/storyline/vegas-cop-killers/cost-bravery-vegas-bystander-died-trying-stop-rampage-n127361

Nonnemaker, James, Mark Engelen, & Daniel Shive (2011) "Are Methamphetamine Precursor Control Laws Effective Tools to Fight the Methamphetamine Epidemic?" 20(5) *Health Economics* 519.

Office of the Director—Strategic Management (2013) *2012 Summary: Firearms Reported Lost and Stolen*. U.S. Department of Justice, Bureau of Alcohol, Tobacco, Firearms & Explosives. https://www.atf.gov/resource-center/docs/2012-firearms-reported-lost-and-stolenpdf-1/download

Officer.com (2017) "Chief: Concealed-Carry Law Is 'Irresponsible'," June 29 *Officer.com*. https://www.officer.com/command-hq/news/12348064/milwaukee-police-chief-calls-concealedcarry-law-irresponsible

Olson, Erik J., Mark Hoofnagle, Elinore J. Kaufman, William C. Schwab, Patrick Reilly, & Mark J. Seamon (2019) "American Firearm Homicides: The Impact of Your Neighbors," February 7 *J. of Trauma & Acute Care Surgery*. https://journals.lww.com/jtrauma/Abstract/publishahead/American_Firearm_Homicides__The_Impact_of_Your.98406.aspx#pdf-link

Owens, David (2018) "Retired Hartford Firefighter Donald Brown Sentenced to 7 Years in Shooting," January 9 *Hartford Courant*. http://www.courant.com/news/connecticut/hc-hartford-donald-brown-sentenced-0110-story.html

Palmer, Ewan (2018) "Pregnant Woman Shot by Daughter, 3, After Finding Gun in Car," April 18 *Newsweek*. http://www.newsweek.com/pregnant-woman-shot-daughter-3-after-finding-gun-car-outisde-platos-closet-891073

Paraguassu, Lisandra, & Ricardo Brito (2018) "U.S. Biggest Source of Illegal Foreign Guns in Brazil: Report," January 10 *Reuters*. https://www.reuters.com/article/us-usa-brazil-arms/u-s-biggest-source-of-illegal-foreign-guns-in-brazil-report-idUSKBN1EZ2M5

Parsons, Chelsea, & Eugenio Weigend Vargas (2017) *Stolen Guns in America: A State-by-State Analysis*. Center for American Progress. https://cdn.americanprogress.org/content/uploads/2017/07/25052308/StolenGuns-report.pdf

Perrusquia, Marc (2017) "Stolen Guns: 'Getting Them Is the Easy Part'," *Commercial Appeal*. http://projects.commercialappeal.com/woundedcity/stolen-guns-this-fence-makes-a-bad-neighbor.php

Phillips, Charles D., Obioma Nwaiwu, Darcy K. McMaughan Moudouni, Rachel Edwards, & Szu hsuan Lin (2013) "When Concealed Handgun Licensees Break Bad: Criminal Convictions of Concealed Handgun Licensees in Texas, 2001–2009," 103(1) *American J. of Public Health* 86.

Pilger, Lori (2018) "FBI Accuses White Supremacist of Terror Attack on Amtrak Train in Rural Nebraska," January 4 *Lincoln J. Star*. http://journalstar.com/news/state-and-regional/nebraska/fbi-accuses-white-supremacist-of-terror-attack-on-amtrak-train/article_82f0860e-3c75-5a66-ab0c-a2e3a3c16aab.html

Planty, Michael, & Jennifer Truman (2013) "Firearm Violence, 1993–2011." *U.S. Department of Justice Bureau of Justice Statistics BJS Special Report* 241730.

Plumlee, Rick (2012) "Eight with Concealed-Carry Permits Charged with Felonies in Sedgwick County," November 17 *Wichita Eagle*. http://www.kansas.com/latest-news/article1103131.html

Pugliese, Nicholas (2018) "It's Tough to Buy a Gun in New Jersey. So Where Do All the Guns Used in Crimes Come From?" April 16 *NorthJersey.com*. https://www.northjersey.com/story/news/new-jersey/2018/04/16/nj-new-jersey-where-do-guns-used-crimes-come/503115002/

Robles, Frank, & Christine Hauser (2015) "Lawyers Provide Details in Police Shooting of Corey Jones in Florida," October 22 *New York Times*. https://www.nytimes.com/2015/10/23/us/florida-corey-jones-police-shooting.html

Sampson, Zachary T. (2014) "Stolen Guns, Like One Used to Kill Tarpon Springs Officer, Routine at Crime Scenes," December 24 *Tampa Bay Times*. http://www.tampabay.com/news/publicsafety/crime/gun-police-say-was-used-to-kill-tarpon-springs-officer-stolen-from/2211436

Sauro, Sean (2019) "Plans Made to Honor Men Killed in State College Shooting Spree," January 26 *Penn Live*. https://www.pennlive.com/news/2019/01/plans-made-to-honor-men-killed-in-state-college-shooting-spree.html

Savitsky, Sasha (2019) "Country Singer Justin Carter Dead at 35 After Accidental Shooting," March 22 *Fox News*. https://www.foxnews.com/entertainment/country-singer-justin-carter-dead-at-35-after-accidental-shooting

Scherer, Jasper (2016) "Fla. 'Loud Music' Murder: Firing into Car Full of Teens Playing Rap Music Not 'Self-Defense,' Court Rules," November 18 *Washington Post*. https://www.washingtonpost.com/news/morning-mix/wp/2016/11/18/fla-loud-music-murder-firing

Schwarz, Hunter (2014) "Idaho Professor Shoots Himself in Foot Two Months After State Legalizes Guns on Campuses," September 5 *Washington Post*. https://wapo.st/1nAtjTj?tid=ss_mail&utm_term=.a706e9990995

Schwarzschild, Todd, & Drew Griffin (2011) "ATF Loses Track of 1,400 Guns in Criticized Probe," July 12 *CNN*. http://www.cnn.com/2011/POLITICS/07/12/atf.guns/index.html

Shen, Aviva (2017) "When the Driver Who Just Cut You Off Also Has a Gun," April 10 *Trace*. https://www.thetrace.org/2017/04/road-rage-shootings-guns/

Siegel, Michael, Molly Pahn, Ziming Xuan, Craig S. Ross, Sandro Galea, Bindu Kalesan, Eric Fleegler, & Kristin A. Goss (2017) "Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States," 107(12) *American J. of Public Health* 1923.

Simon, Darran (2018) "Manslaughter Defendant in 'Stand Your Ground' Case Said He Felt Scared in Altercation," September 3 *CNN*. https://www.cnn.com/2018/09/03/us/michael-drejka-stand-your-ground-jailhouse-interview/index.html

Simpson, Kevin (2017) "Shoppers Pulled Guns in Response to Thornton Walmart Shooting, But Police Say that Slowed Investigation," November 2 *Denver Post*. http://www.denverpost.com/2017/11/02/shoppers-pulled-weapons-walmart-shooting/

Slobodzian, Joseph A. (2011) "Ung Acquitted in Wounding of DiDonato in Old City," February 16 *Inquirer*. http://www.philly.com/philly/news/local/20110216_Ung_acquitted_in_wounding_of_DiDonato_in_Old_City.html

Soderling, Luke (2016) *How to Inform an Officer You Are Carrying a Handgun and Live* [video file]. https://www.youtube.com/watch?v=fOO99qcASEM

Stanglin, Doug (2018) "Parkland Teacher Charged with Leaving Loaded Gun in Public Restroom," April 13 *USA Today*. https://www.usatoday.com/story/news/2018/04/13/parkland-teacher-charged-leaving-loaded-gun-public-restroom/514855002/

Stark, Emily, & Daniel Sachau (2016) "Lake Wobegon's Guns: Overestimating Our Gun-Related Competences," 4(1) *J. of Social & Political Psychology* 8.

Strnad, Jeff (2007) "Should Legal Empiricists Go Bayesian?" 9(1) *American Law & Economics Rev.* 195.

Stuart, Hunter (2013) "2 Concealed Carry Holders Kill Each Other In Road Rage Incident," September 19 *Huffington Post*. http://www.huffingtonpost.com/2013/09/19/michigan-concealed-carry-road-rage-two-dead_n_3956491.html

US News (2018) "Cops: Mom Was Turning on Safety When Gun Fired, Killing Girl," April 23 *US News*. https://www.usnews.com/news/best-states/ohio/articles/2018-04-23/cops-mom-was-turning-on-safety-when-gun-fired-killing-girl

Violence Policy Center (2017) *Mass Shootings Committed by Concealed Carry Killers: May 2007 to the Present*. http://concealedcarrykillers.org/wp-content/uploads/2017/06/ccwmassshootings.pdf

WFTV (2015) "3 Injured When Man's Gun Goes Off in Sanford Cracker Barrel," November 2 *WFTV 9*. http://www.wftv.com/news/local/man-not-charged-after-gun-goes-sanford-cracker-bar/26880670

Williams, Clois, & Steven Waltrip (2004) *Aircrew Security: A Practical Guide*. New York, NY: Ashgate Publishing.

Wilson, Robert (2016) "Common Sense," February 29 *American Scholar*. https://theamericanscholar.org/common-sense/#

Witt, Jessica K., & James R. Brockmole (2012) "Action Alters Object Identification: Wielding a Gun Increases the Bias to See Guns," 38(5) *J. of Experimental Psychology: Human Perception & Performance* 1159.

Zimmerman, Paul R. (2014) "The Deterrence of Crime Through Private Security Efforts: Theory and Evidence," 37 *International Rev. of Law & Economics* 66.

EXHIBIT 2

EJW
ECON JOURNAL WATCH
Scholarly Comments on
Academic Economics

Econ Journal Watch 16(1)
March 2019: 97–113

RTC Laws Increase Violent Crime: Moody and Marvell Have Missed the Target

John J. Donohue[1], Abhay Aneja[2], and Kyle D. Weber[3]

Link to Abstract

Donohue, Aneja, and Weber (2018), released as National Bureau of Economic Research working paper 23510, uses two distinct methodologies to provide the latest and most comprehensive evaluation of the impact on crime of state laws that confer on citizens a right to carry concealed weapons—so-called right-to-carry or RTC laws. Its most robust finding is that RTC laws *increased* violent crime: our preferred panel data estimate indicates a 9 percent increase, while our synthetic control analysis indicates that violent crime rose by about 14 percent in the first decade after RTC adoption.

In a comment on the Donohue, Aneja, and Weber (hereafter DAW) paper, Carlisle Moody and Thomas Marvell (hereafter MM) concede that the uniform approach of using population weights in panel data estimates of crime shows a strongly statistically significant increase of RTC laws on crime in the DAW model (MM 2019, 88). They make an unconvincing argument that the uniform practice should now be rejected and then proceed to show that simplistic panel data models not weighted by population (and using badly miscoded data) would diminish the strength of the finding that RTC laws increase violent crime (ibid., 85–88). We show that both of the proffered MM models violate the basic 'parallel trends' requirement of a valid panel data analysis, so their resulting estimates must be rejected. But even with these serious flaws, a more nuanced implementation and

1. Stanford Law School, Stanford, CA 94305.
2. Graduate student, Stanford Law School, Stanford, CA 94305.
3. Graduate student, Columbia University, New York, NY 10027.

evaluation of the MM models with attention to the requirements of panel data can illustrate and buttress the basic finding of the DAW panel data analysis that RTC laws *increase* violent crime.

MM (2019, 89–94) then present their own synthetic control analysis, which purports to establish that 14 states show statistically significant increases in violent crime while 12 states show statistically significant decreases. We have many criticisms of their implementation of the synthetic control analysis, from using inappropriate states as potential controls to failing to account for major pre-treatment differences. These problems cause MM to generate many severely inaccurate predictions, particularly for small states. Nonetheless, a simple aggregation of MM's overall synthetic controls results—whether weighted by state population or the inverse of the pre-treatment error fit—reveals a strong pattern of increasing violent crime in the decade following RTC adoption.

We discuss these points in turn and then summarize in the final section.

DAW's population-weighted model is superior to MM's models, and it provides clear evidence that RTC laws increase crime

Weighting by population is conceptually superior

The uniform practice in the literature on estimating the impact of RTC laws on crime from the early work of John Lott through the DAW paper has been to present population-weighted panel data estimates. Every regression run by the authors of the National Research Council (2005) report examining RTC laws was weighted by population. In fact, this is the standard practice in virtually all panel data studies looking at state or county crime data,[4] including in prior work by MM on RTC laws.[5] In their current paper, however, they argue that the standard practice should now be rejected, and they would repose confidence in regressions that are not designed to reflect the relative population of each state.

MM acknowledge the reason that all researchers have used population-weighted regressions:

4. For just two very recent examples, see Chalfin and McCrary 2018; Anderson, Sabia, and Tekin 2018.
5. *See* Moody and Marvell 2018; 2008; Moody, Marvell, Zimmerman, and Alemante 2014; Kovandzic, Marvell, and Vieraitis 2005; Moody 2001.

> [I]f the research goal is to estimate the overall national impact of a policy change, … then weighting can be justified by arguing that the impact of laws in large states should be emphasized simply because they affect more people. (MM 2019, 85)

Put simply, we are trying to estimate the impact that RTC laws have had on Americans, and this can only be identified by a population-weighted regression. Following the unweighted approach that MM have suddenly decided to champion would imply that the impact of RTC laws on 600,000 individuals in Wyoming is considered to be equally important as the impact on 28 million Texans. To illustrate the importance of weighting by population, consider the MM synthetic control estimates of the impact on violent crime of the RTC laws in these two states. Using their non-normalized synthetic control approach, MM would predict that the Texas RTC law *increased* violent crime by 19.5 percent after ten years but that the Wyoming law had generated a 36 percent *decrease* in violent crime over the decade following adoption (although they never show these estimates in their paper). While we discuss below why we think MM's Wyoming estimate is so flawed, the decision to equally weight Texas and Wyoming, as MM would have us do, generates a prediction that the combined RTC laws *reduced* crime by 8.25 percent. A population-weighted average would show the total effect on the residents of these states to be an 18.3 percent *increase* in violent crime.[6] In this example, the 18.3 percent increase would reflect the effect of RTC laws on the average American who experienced this legal adoption, and a population-weighted analysis alone would generate this estimate. MM's approach would badly mischaracterize the impact of RTC laws, heralding a significant decline in violent crime when in fact the two RTC laws led to a combined large increase in violent crime.

Having conceded the key reason for population weighting in the panel data regressions, MM (2019, 85–86) then mention a second possible advantage of population weighting: it may serve to address the problem of heteroskedasticity. This is not the primary rationale, but it is often—although not always—a secondary benefit of weighting by population. Since MM conclude that the White test indicates the presence of heteroskedasticity in the DAW population-weighted regressions, MM present estimates using a non-weighted regression approach (their OLS results) and a non-population-weighted approach that seeks to directly

6. MM's wildly inaccurate Wyoming estimate stems from their failure to normalize their synthetic control estimate, which leads them to attribute pre-treatment differences between the fit of the synthetic control and the treatment state to the effect of the treatment. Our DAW synthetic control estimates for the impact of RTC laws on violent crime showed a 16.9 percent increase for Texas and a 15.9 percent increase for Wyoming after ten years. The comparable *normalized* MM synthetic control estimates for these two states are a 13.4 percent increase for Texas and a 9.1 percent increase for Wyoming.

control for heteroskedasticity (Feasible Generalized Least Squares, or FGLS). Neither of these approaches can succeed in our primary mission, which is to estimate the experience of the average American exposed to RTC laws. But in addition to the conceptual flaw in failing to weight by population, both of the MM suggested alternatives have further problems, including the second problem that they both fail the very test for homoskedasticity that MM advocate using.[7]

The importance of investigating the parallel trends assumption

While that second problem underscores that the MM regressions are still marred by heteroskedasticity (or some specification error), a third problem with the simplistic MM models results from MM's failure to attend to the parallel trends assumption, which is critical to generating valid panel data estimates.

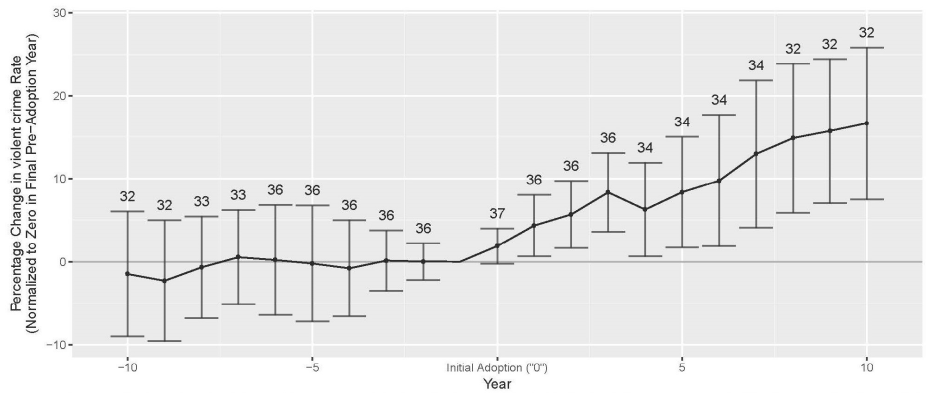
This third problem with MM's two new panel data regressions can be highlighted by comparing them to the results of the DAW population-weighted violent crime regression. The DAW paper provides the year-by-year effect on violent crime following RTC adoption from that regression (2018, 25), which we reproduce here as Figure 1 below. This figure illustrates the critical feature of a valid panel data model that the estimated values on the states that end up adopting RTC laws is virtually zero in the years prior to adoption. Not only are the deviations from zero small, but crucially there is virtually no slope to these pre-adoption values in the years prior to RTC passage. This is important because a panel data estimate will only reveal the causal effect of the RTC law if we can assume that the trends in crime between our two sets of states (adopters and non-adopters) would evolve similarly in the absence of the law.

Three lessons emerge from the Figure 1 DAW violent crime regression. First, we see an almost perfect pre-treatment pattern confirming the critical parallel trends assumption for a panel data regression. Controlling for an array of factors (the DAW explanatory variables), violent crime is flat prior to RTC adoption. Second, Figure 1 also reveals that there is a change in the previously stable relationship of crime in the RTC and non-RTC states, and that this change begins exactly in the year of adoption of the RTC laws. If RTC laws had no impact on violent crime, one would expect that flat pattern seen in the years before adoption would continue thereafter. If some factor other than RTC laws (and the array of explanatory variables controlled for in the DAW model) led to worse violent crime performance in RTC states, you would see an elevation in the violent crime estimates, but there

7. This is true for both the MM unweighted OLS regression and for their FGLS regression, both of which badly fail the White test for homoskedasticity with p-values < 0.00000001.

is no reason to think it would occur in exactly the year that the RTC law goes into effect. Figure 1 makes clear that a sharp secular increase in violent crime commences at the time of RTC adoption, again buttressing a causal interpretation of these results. Third, this increase in violent crime is statistically significant beginning in the first year after RTC adoption and every year thereafter.

Figure 1. The impact of RTC laws on violent crime, DAW model, 1979–2014 (population-weighted)



Note: We regress crime on dummies for pre− and post−passage years and DAW covariates. Reference year is year before adoption and adoption year is first year with RTC in place at any time, meaning that in states that adopt after January 1st, this will capture only a partial effect of RTC laws. We display the 95 percent confidence interval for each estimate using cluster−robust standard errors and show the number of states that contribute to each estimate.

Evaluating MM's simple panel models

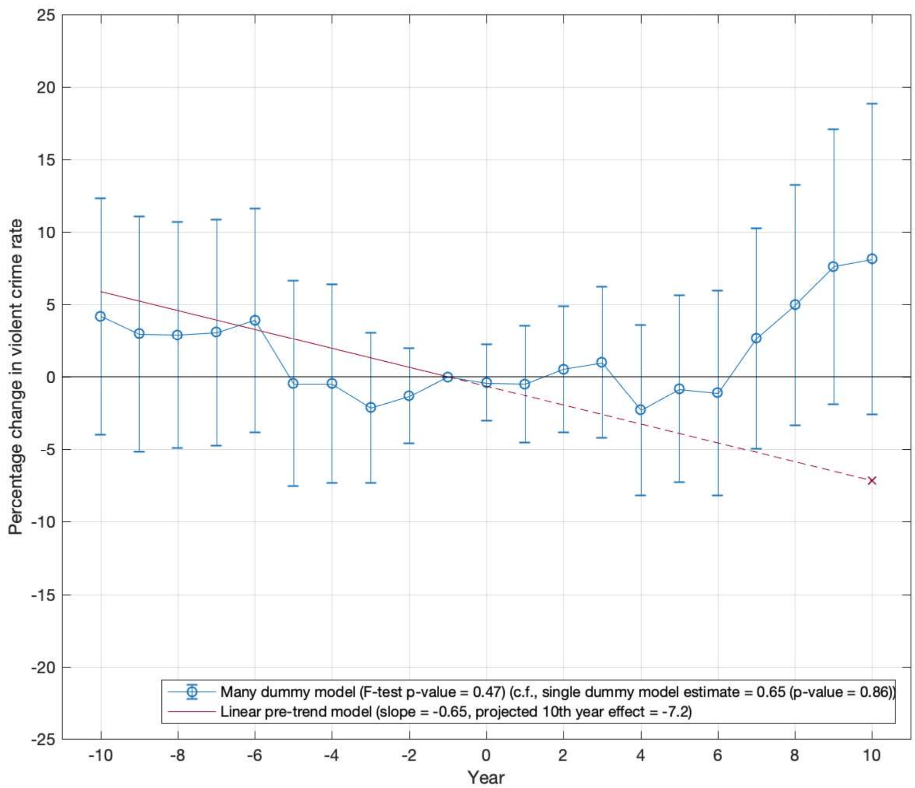
We can now compare the two alternative models—OLS and FGLS—that MM offer in place of the DAW violent crime estimates reflected in Figure 1. We must first discard all of the MM estimates because of serious coding errors they made in their panel data analysis. Specifically, the MM panel data analysis miscodes both North Dakota and South Dakota as having never adopted an RTC law during the 1977–2014 data period they analyze, even though North Dakota and South Dakota both adopted RTC laws in 1985. The error is perplexing because, in their subsequent synthetic control analysis, MM generate estimates for states adopting RTC laws, including both North and South Dakota, based on that actual year of adoption.[8] MM also code the date of adoption for Virginia differently in their two analyses. They give Virginia a starting date of 1996 in their synthetic control

8. MM also have a less precise coding of their RTC law than we use in our DAW paper: they simply use a zero-one dummy that becomes one the first full year the RTC is in effect, while we use an RTC dummy that takes the value of the fraction of the year an RTC law was in effect during the year it was adopted. MM also exclude DC from their panel analysis, while we only exclude DC from our synthetic control analysis.

analysis, which is consistent with their protocol of turning on their RTC indicator in the year after adoption. In their panel data analysis, however, MM use a Virginia date of 1995, which is doubly wrong in being both a violation of their own protocol and inconsistent with their treatment of Virginia in their synthetic control analysis.

Figure 2 shows violent crime estimates using the preferable DAW data but following MM's "OLS" approach, which does not weight by population. Three lessons emerge from this analysis. First, Figure 2 reveals a substantial violation of the critical parallel trends assumption: the red line illustrates the sharply sloping downward linear trend in crime for RTC states *prior to RTC adoption*.

Figure 2. The impact of RTC laws on violent crime, DAW model, 1979–2014 (not weighted by population)



Second, the dashed continuation of this line shows the predicted path of violent crime in RTC states had their pre-RTC-adoption trend continued, and by assumption of panel data analysis, the dashed line of Figure 2 suggests that crime would have fallen (relative to non-adopting states) by 7.2 percent after ten years without RTC adoption. Instead we see that the observed post-adoption crime path is always above this predicted downward trend, suggesting RTC laws *increased* crime relative to trend.

Third, by the sixth year after adoption and beyond, the estimated increase in violent crime is always statistically significantly above this trend (at the .05 level). But instead of providing this more nuanced analysis, MM simply look at one number for the OLS violent crime estimate: they run a single dummy model for this unweighted regression, which generates the small positive estimate of 0.65 (as shown in the legend to Figure 2). But by failing to realize that such a simple model is marred by the violation of the parallel trends assumption, they merely present an inaccurate and misleading estimate of the impact of RTC laws on violent crime. In other words, MM's violent crime unweighted OLS estimate (MM 2019, 88, Table 1, row 1, column 5) is inaccurate and misleading.[9]

MM also include an FGLS model designed to address the problem of heteroskedasticity (although we have already noted this model's extreme failure of the White test). Figure 3 shows the DAW violent crime year-by-year estimates using this FGLS approach. What are the lessons from this MM-suggested model? First, unlike in the DAW model in Figure 1 where all the pre-treatment values are close to zero and flat in the years prior to RTC adoption, Figure 3 reveals both greater variability in those values and another departure from the ideal parallel trends as captured again in the downward-sloping red line in the period *prior to RTC adoption*. Indeed, this FGLS model fails the most basic test of parallel trends since its pre-trend dummy values are not jointly zero.[10]

Second, the dashed continuation of this line shows the predicted path of violent crime in RTC states had their pre-RTC-adoption trend continued, and it suggests that crime would have fallen (relative to non-adopting states) by 1.2 percent after ten years without RTC adoption. As in Figure 2, we see that the observed post-adoption crime path is always above this predicted downward trend, again suggesting RTC laws *increased* violent crime relative to trend, and that this

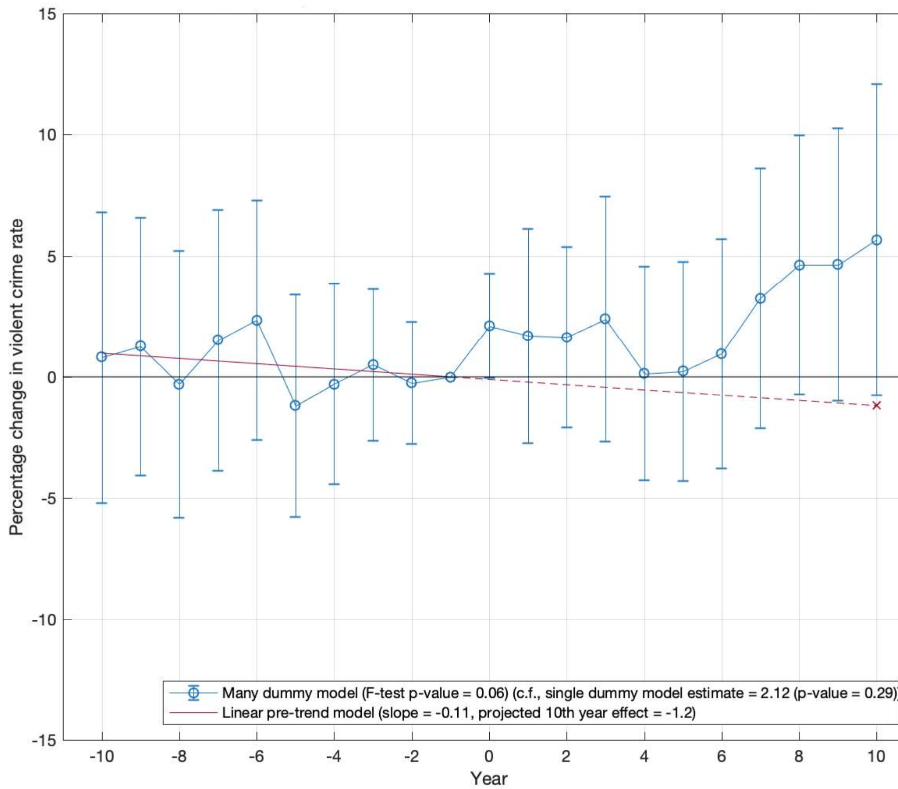
---

9. MM also present an additional row of "spline" estimates in their Table 1, which is a practice that also dates back to the initial Lott and Mustard (1997) paper. Since the RAND Corporation (2018) study on gun violence research has now argued that "spline" results should not be relied upon, we ignore that component of the MM paper (and have also dropped this model in our own forthcoming work). The RAND analysis of gun research identifies "the use of spline and hybrid effect codings that do not reveal coherent causal effect estimates" as a limitation of earlier studies (2018, xxvii).

10. The most basic statistical test of the assumption of parallel trends uses an F-test of the null hypothesis that the pre-period dummies are jointly equal to zero. Applying this test in Figure 3 generates a p-value of .057, which is too low to support the parallel trends assumption. For this very permissive initial test, one would typically like this p-value to be greater than .50 and certainly no lower than .20, so the Figure 3 FGLS model fails this test badly in a way that obscures any increase in violent crime resulting from RTC adoption. For comparison, the p-value on the same F-test for our far superior Figure 1 DAW violent crime population-weighted regression is .87.

reversal in the path of violent crime occurred in the year of RTC adoption.

Figure 3. The impact of RTC laws on violent crime, DAW model, 1979–2014 (FGLS)



Third, Figure 3 shows that after the seventh year following RTC adoption, the estimated increase in violent crime is always statistically significantly (at the .05 level) above the dashed projected downward trend. Again, if one were to run the single dummy model for this FGLS regression and ignore the violation of parallel trends as MM do, one would not be presenting valid results. Accordingly, the small positive estimate of 2.12 (as shown in the legend to Figure 3) that emanates from this flawed model again yields an inaccurate and misleading picture of the true path of increased violent crime after RTC adoption. In other words, the MM violent crime regressions (2019, 88, Table 1, row 1, columns 5–6)—other than the population-weighted regression which shows a statistically significant increase in violent crime—are inaccurate and misleading. But note that both the Figure 2 and Figure 3 models that are merely more informative versions of the overly simplistic OLS and FGLS that MM present (using their badly miscoded data) still lead us to a very clear conclusion: regardless of the flaws or limitations of the two models that MM present, their more accurate and revealing versions in Figures 2 and 3 can still detect that RTC states experience statistically significant increases in violent crime relative to pre-existing trends within a decade of adoption.

In other words, MM would reject the DAW panel data estimates that RTC laws increase violent crime by roughly 9 percent by instead offering regressions with key miscodings of RTC states that are conceptually inferior because they don't address the primary question of interest (which is the impact of RTC laws on Americans), empirically unsophisticated by virtue of their failure to address the parallel trends assumption, and offer no benefit in addressing the problem of heteroskedasticity.

MM's discussion of heteroskedasticity is largely a distraction from a more important issue: that the difference in results between the population-weighted and unweighted regressions is likely signaling a specification issue. This finding provides an additional reason to turn to the synthetic control analysis, which can give insight into this concern and also provide potentially superior estimates, at least for those states for which good pre-treatment matches can be found. But before turning to the synthetic control estimates, it is important to highlight once again that the DAW violent crime panel data model dominates the MM models both conceptually and econometrically for the reasons set out above.

Evaluating MM's synthetic control analysis, which despite its flaws is shown to reveal that RTC laws increase violent crime

The DAW synthetic control analysis aggregated across all RTC-adopting states generates a year-by-year prediction of the impact of RTC laws on violent crime over the ten years following adoption (2018, 36), shown here in Table 1.

TABLE 1. The impact of RTC laws on violent crime rate, DAW covariates, 1977–2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	−0.117	2.629*	3.631*	4.682**	6.876***	7.358**	10.068***	12.474***	14.021***	14.344***
	(1.076)	(1.310)	(1.848)	(2.068)	(2.499)	(3.135)	(2.823)	(3.831)	(3.605)	(2.921)
N	33	33	33	33	33	33	33	31	31	31
Pseudo p-value	0.936	0.274	0.220	0.192	0.094	0.106	0.060	0.038	0.032	0.032

Notes: Standard errors in parentheses. Column numbers indicate post-passage year under consideration; N = number of states in sample. Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment. See DAW (2018, 37–38) regarding how the pseudo p-value is estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The synthetic control analysis of Table 1 shows that after RTC laws have been in effect for a year, violent crime starts steadily rising (relative to the synthetic control state). After ten years, the DAW synthetic controls analysis estimates that violent crime is about 14.3 percent higher than it would be in the absence of the RTC law. Note that even though Figure 1 (panel data) and Table 1 (synthetic control analysis) are derived from entirely different methodologies, they both estimate that RTC laws increasingly elevate violent crime in the ten years after adoption, which mutually reinforces this conclusion.

Moreover, DAW (2018) showed that the synthetic control result was extremely robust. Indeed, one would generate very similar estimates whether one used the control variables of DAW (those used to derive the estimates shown in Table 1) or those of other papers examining the impact of RTC laws, such as those by Lott and David Mustard (1997) and the Brennan Center (Roeder et al. 2015), or an earlier Moody and Marvell paper (2008). Similarly, one could drop any single control state from the analysis or even completely drop New York and California from the set of potential controls and the results remained strong: RTC laws consistently led to statistically significant *increases* in violent crime after a decade.

DAW (2018) also showed that the result that RTC laws increase violent crime was not sensitive to whether one normalized the synthetic control estimates to be zero at the time of adoption or simply allowed the estimates to emerge from the matching protocol without adjustment. Similarly, the result was robust to efforts to trim off treatment states for which the synthetic control did not well match the target state in the period prior to RTC adoption. DAW also showed the violent crime results remained strong whether one used any of four different approaches designed to improve the fit of the synthetic control by including pre-treatment values of violent crime in the matching protocol or whether one included none of these values.

Since our finding was so strong and robust, we were surprised that Moody and Marvell (2019) offered their own synthetic control analysis that appeared to question the DAW results. Unfortunately, MM's analysis has gone astray, and the short answer is that they have not undermined the synthetic control finding that RTC laws *increase* violent crime in the first decade following adoption.

MM's flaws in implementing their synthetic control analysis

The first step in a successful synthetic control analysis is to denote a set of possible states—called donor states—from which the synthetic control can be constructed. MM got off on the wrong foot by making a mess of that process. In total, we found 57 erroneous donor pool decisions by MM. Sometimes a state that should not be in the donor pool was included; other times, states that should have been included were left out. For example, in their synthetic control analysis, MM erroneously treat Alabama as not becoming an RTC state until 2014 while the dominant coding that we employ treats Alabama as an RTC state as of 1975 (which MM also did in their panel data analysis).[11] Accordingly, as an RTC state, Alabama cannot serve as a control, yet MM treat it as a potential donor state for 26 out of the 33 RTC states they analyze (and a component of the synthetic control in 14 of those 26 RTC adopters). Seventeen states have some other difference between the donor pool used by Moody and Marvell (2019) and the appropriate states used by DAW (2018). Out of 33 states in the analysis, MM used only five donor pools identical to the correct pools used by DAW.[12]

While the various problems in the MM synthetic control analysis are not worth extended discussion, we just want to highlight how their abbreviated presentation omits any discussion of some of the major pitfalls in their approach. One obvious problem can be seen by examining their own synthetic control estimate of the impact of RTC laws on violent crime in Idaho. MM indicate that Idaho had a violent crime rate of 290 per 100,000 during the first full year of having a RTC law in 1991. Unfortunately, their poorly fit synthetic control had an estimated value of 500 per 100,000 that year. For the next two years, that rather wide disparity between the actual and MM synthetic control estimates of violent crime remained roughly stable, suggesting there had been little impact on crime in those two years, yet under MM's assumptions these were years of more than 40 percent crime drops engineered by the adoption of RTC laws! In other words, MM attributed the massive discrepancy between violent crime in synthetic Idaho and actual Idaho *before* Idaho's RTC law was adopted—resulting from their poor fit—as a crime-reducing benefit of the RTC law.

Over the ten-year period following RTC adoption, the violent crime drop

---

11. While there is some ambiguity in the appropriate date that Alabama should be coded as having an RTC law, we believe that MM were correct in their treatment of Alabama in their panel data analysis but wrong in using a 2014 RTC date for the state in their synthetic control analysis. The Rand Corporation's Gun Policy in America initiative "developed a longitudinal data set of state firearm laws" that codes the start of Alabama's RTC law as occurring in 1975, as we do (see https://www.rand.org/pubs/tools/TL283.html for the downloadable database). This is also consistent with the codings used by the National Rifle Association (NRA), John Lott, and the NRC *Firearms and Violence* report. Indeed, if one looks at Lott's estimated percentage of citizens with concealed carry permits, Alabama ranked first among all the states for which he had data. Lott lists the Alabama percentage as greater than 8 percent for 2007—seven years before the date that MM use for Alabama in their synthetic control analysis (Lott 2010, 238). Moreover, the 2014 date that MM use would imply that Alabama was one of the last states in the union to adopt a RTC law, which would not be consistent with the gun politics of the region nor the estimated percentage of permit holders in the state seven years prior to 2014. The NRA clearly would have successfully pushed for an RTC law in Alabama decades ago if Alabama was thought not to have one.

12. DAW (2018, 60, Table A1) provides the complete list of dates for RTC adoption.

in MM's synthetic Idaho was estimated to be over 35 percent (from 501 to 324), which was substantially better than the far smaller 16 percent drop in actual Idaho (from 290 to 243). Yet MM treat this as evidence of statistically significant and substantial crime drops caused by Idaho's RTC law. Note that the DAW synthetic controls analysis was superior because it produced a much better fit (the DAW initial year synthetic Idaho estimate was 344, versus the MM estimate of 501!), but also because DAW did not treat that pre-existing difference as evidence that the RTC law immediately caused a major drop in crime.[13] By doing so, MM were able to mask the fact that their own analysis frequently showed that the synthetic control performed much better (with either larger crime drops or smaller crime increases) than the comparable RTC-adopting state over the ten years following adoption.[14]

Aggregating MM's synthetic control estimates reveals that RTC laws increase violent crime

This unpromising beginning ends in an array of synthetic control estimates that on the whole are considerably less promising than those contained in the DAW synthetic control analysis. Essentially, MM got some very bad fits on small states and then used those poor fits to argue that there is no support for the DAW position because 14 states adopting RTC laws experienced statistically significant increases in crime and 12 experienced decreases.[15] (Note that our more accurate synthetic control analysis would show a 15-to-8 advantage for RTC laws causing statistically significant *increases* in crime, which grows to 16-to-4 if one limits the

13. One can see this same problem illustrated in MM's synthetic control graph of the murder rate in Texas (MM 2019, 92, Figure 1). MM's poorly fitting synthetic Texas has a substantially higher murder rate than actual Texas at the time of adoption of the Texas RTC law. Their graph highlights that this occurred because Texas enjoyed a substantial drop in murder relative to the synthetic control—*prior to the adoption of the RTC law!* The MM calculus treats that ill-fitting differential as a benefit of the law, even though if one examined how crime changed in both Texas and synthetic Texas in the aftermath of RTC adoption, no such murder-reduction benefit would be observed.

14. Since we were trying to show whether the panel data finding that RTC laws increased crime was supported by a synthetic control analysis, it was important to use the same 1979–2014 time period for both approaches, which we did. Extending the data set further backwards creates data problems for variables such as poverty and unemployment, which were either not available or not consistently gathered prior to 1979. Disregarding these concerns, MM started their panel data analysis in 1977, and, without explanation, used a different time period (extending back to 1970) for their synthetic control analysis.

15. MM show that, for their statistically significant results, the majority of states experienced an increase in violent crime using the preferred "nested approach" but then go on to present inferior "default" results perhaps because the inferior estimates weakened their finding of a 14-to-12 state dominance for RTC laws increasing violent crime. This is not good practice, and the "default" estimates, which are only appropriate when "nested" results cannot be computed, should be ignored in the MM paper. See the documentation for the Stata synth program, which states that the nested option offers "better performance" than the default option (Abadie, Diamond, and Hainmueller 2014).

analysis to six to ten years after adoption, reflecting the consistent pattern that the harm of RTC laws rises over the decade following adoption.) Even though the errors in implementation invalidate the MM synthetic control analysis, if MM had simply computed how much violent crime was estimated to have changed in aggregate for the 33 RTC-adopting states for each of the ten years using their own estimates, they would have generated the estimated impacts of RTC laws on violent crime shown in Table 2.

TABLE 2. The impact of RTC laws on the violent crime rate, MM synthetic control methodology and data, 1970–2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average non-normalized TEP	7.21*	7.61*	6.64	8.06	9.81*	10.97**	11.01**	12.55**	14.86**	16.26***
	(3.82)	(4.05)	(4.21)	(4.72)	(4.78)	(4.76)	(4.79)	(5.41)	(5.05)	(4.80)
N	33	33	33	33	33	33	33	33	33	31
P-value	0.07	0.07	0.13	0.10	0.05	0.03	0.03	0.03	0.01	0.00

Notes: Standard errors in parentheses. Column numbers indicate post-passage year under consideration; N = number of states in sample. Dependent variable is the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the aggregate, population-weighted impact of RTC laws on violent crime using MM's own data and synthetic control methodology (which does not normalize the estimates to equate the actual and synthetic control crime rates at the time of RTC adoption). In other words, Table 2 just takes MM's actual individual state estimates—which they fail to show—and aggregates them. The finding is clear: RTC laws consistently generated a statistically significant increase in violent crime, rising from a 7.2 percent increase in the first year to 16.3 percent in the tenth year. Note that this is even a larger violent crime increase than that predicted in the DAW synthetic control table reproduced in Table 1 above. Remarkably, MM have completely disguised the key finding of their own synthetic control analysis, which is that, in aggregate, RTC laws are estimated to have substantially increased violent crime.[16]

16. The MM (2019) synthetic control analysis goes astray so badly because their non-normalized violent crime estimates tend to be large and positive for big states (for example, four of the five highest population states have positive estimates and three of those four are bigger than 15 percent by the fifth year after RTC adoption) and large and negative for small states (four of the five lowest population states have negative estimates by the fifth year, ranging from −29 percent for Wyoming to −78 percent for North Dakota). Not surprisingly, the unrealistically large negative results tend to be found in the states with the worst pre-treatment fits between synthetic control and treatment states. The DAW (2018) paper documents the ratio of the root mean-squared prediction error (RMSPE) to the mean violent crime rate as a measure of goodness of pre-treatment fit and indicated particular concern when this value rose above 19 percent. To highlight how the MM synthetic control model was doing a particularly bad job for generating plausible controls for small states, note that the error ratio averaged a whopping 48.3 percent for MM's estimates for

We are quite confident that the DAW (2018) paper has the best available synthetic control estimates of the impact of RTC laws on crime because our synthetic control analysis is done with greater care, with more accurate coding of RTC law adoption dates, and with a far more probing array of robustness checks than the MM analysis.

Conclusion: The best evidence shows that RTC laws increase violent crime

We have shown that the DAW population-weighted panel data estimates shown in Figure 1 satisfy the parallel-trends assumption of a valid panel data analysis, while neither of the alternative models advanced by MM do. This is on top of the serious miscoding problems of the MM panel data analysis. Nonetheless, a proper interpretation of the two MM models (shown in Figures 2 and 3) can reveal that RTC laws alter the path of violent crime starting at the date of adoption and generate statistically significant deviations from prior trends within a decade of passage.

Of course, the fact that our Figure 1 is the best panel data model does not mean it is perfect, and we take the MM critique as providing another reason to be interested in the results of the synthetic control approach to gain insight into the difficult problem of specification that exists in every panel data analysis. While we find the MM synthetic control approach to be too flawed and primitive to rival the more accurate, thorough, and sound analysis in the DAW paper, it is encouraging to see that their analysis conducted over a longer time frame (1970–2016, while ours extended from 1977–2014) and using a non-normalized set of estimates (in contrast to our normalized estimates) still found that a majority of states experienced statistically significant increases in violent crime from RTC adoption. It is likewise encouraging that the aggregated impact across all states mimicked our own analysis in finding strongly increasing violent crime over the decade following RTC adoption (compare our estimates, shown in Table 1, with those aggregated from the MM results, shown in Table 2).

In summary, there is consistent evidence that RTC laws elevate violent crime in the decade after adoption whether one looks at DAW's panel data estimates (Figure 1) or synthetic controls estimates (Table 1) or the properly interpreted

the nine smallest states and only 8.5 percent for the nine largest states. Accordingly, the clear pattern that RTC laws increase violent crime in the ten-year period following adoption emerges whether one weights the actual MM state estimates by population (as we show in Table 2), weights by the inverse of this error ratio, or simply drops the worst fits from the analysis.

panel data results using MM's suggested non-population weighted or FGLS approaches (Figures 2 and 3) or the MM synthetic controls estimates (Table 2). Policymakers and citizens should recognize that the best available empirical data to date supports the view that RTC laws have resulted in statistically significant increases in violent crime in the ten-year period after adoption.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2014 [2011]. SYNTH: Stata Module to Implement Synthetic Control Methods for Comparative Case Studies. Statistical Software Components S457334. Boston College Department of Economics (Boston, Mass.). **Link**

Anderson, D. Mark, Joseph J. Sabia, and Erdal Tekin. 2018. Child Access Prevention Laws and Juvenile Firearm-Related Homicides. *NBER Working Paper* 25209. National Bureau of Economic Research (Cambridge, Mass.). **Link**

Chalfin, Aaron, and Justin McCrary. 2018. Are U.S. Cities Underpoliced? Theory and Evidence. *Review of Economics and Statistics* 100(1): 167–186.

Donohue, John J., Abhay Aneja, and Kyle D. Weber (DAW). 2018. Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis. *NBER Working Paper* 23510 [revised]. November. **Link**

Kovandzic, Tomislav V., Thomas B. Marvell, and Lynne M. Vieraitis. 2005. The Impact of "Shall-Issue" Concealed Handgun Laws on Violent Crime Rates: Evidence From Panel Data on Large Urban Cities. *Homicide Studies* 9(4): 292–323.

Lott, John R. Jr. 2010. *More Guns, Less Crime: Understanding Crime and Gun Control Laws*, 3rd ed. Chicago: University of Chicago Press.

Lott, John R. Jr., and David B. Mustard. 1997. Crime, Deterrence, and Right-to-Carry Concealed Handguns. *Journal of Legal Studies* 26(1): 1–68.

Moody, Carlisle E. 2001. Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness. *Journal of Law and Economics* 44(S2): 799–813.

Moody, Carlisle E., and Thomas B. Marvell. 2008. The Debate on Shall-Issue Laws. *Econ Journal Watch* 5(3): 269–293. **Link**

Moody, Carlisle E., and Thomas B. Marvell. 2018. The Impact of Right-to-Carry Laws: A Critique of the 2014 Version of Aneja, Donohue, and Zhang. *Econ Journal Watch* 15(1): 51–66. **Link**

Moody, Carlisle E., and Thomas B. Marvell (MM). 2019. Do Right to Carry Laws Increase Violent Crime? A Comment on Donohue, Aneja, and Weber. *Econ Journal Watch* 16(1): 84–96. **Link**

Moody, Carlisle E., Thomas B. Marvell, Paul R. Zimmerman, and Fasil Alemante. 2014. The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication. *Review of Economics and Finance* (Better Advances Press, Toronto) 4(1): 33–43. **Link**

National Research Council. 2005. *Firearms and Violence: A Critical Review*, eds. Charles

F. Wellford, John V. Pepper, and Carol V. Petrie. Washington, D.C.: National Academies Press.

RAND Corporation. 2018. *The Science of Gun Policy: A Critical Synthesis of Research Evidence on the Effects of Gun Policies in the United States*. Santa Monica, Calif.: RAND Corporation. **Link**

Roeder, Oliver, Lauren-Brooke Eisen, and Julia Bowling. 2015. What Caused the Crime Decline? February 12. Brennan Center for Justice, New York University School of Law (New York). **Link**

About the Authors

John J. Donohue III is an economist as well as a lawyer and is well known for using empirical analysis to determine the impact of law and public policy in a wide range of areas, including civil rights and antidiscrimination law, employment discrimination, crime and criminal justice. Before rejoining the Stanford Law School faculty in 2010, Professor Donohue was the Leighton Homer Surbeck Professor of Law at Yale Law School. He is a member of the American Academy of Arts and Sciences, and the former editor of the *American Law and Economics Review* and president of the American Law and Economics Association and the Society of Empirical Legal Studies. He is also a Research Associate of the National Bureau of Economic Research. His email address is donohue@law.stanford.edu.

Abhay Aneja is a J.D./Ph.D. candidate studying at Stanford Law School and the University of California, Berkeley. His email address is aneja@berkeley.edu.

Kyle D. Weber is currently a doctoral student in Economics at Columbia University, having previously worked as a research fellow at Stanford Law School. His primary research interests are industrial organization and media economics. His email address is kdw2126@columbia.edu.

Go to archive of Comments section
Go to March 2019 issue

Discuss this article at Journaltalk:
https://journaltalk.net/articles/5983/

EXHIBIT 3

Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States

Michael Siegel, MD, MPH, Ziming Xuan, ScD, SM, MA, Craig S. Ross, PhD, MBA, Sandro Galea, MD, DrPH, MPH, Bindu Kalesan, PhD, MPH, MSc, Eric Fleegler, MD, MPH, and Kristin A. Goss, PhD, MPP

Objectives. To examine the relation of "shall-issue" laws, in which permits must be issued if requisite criteria are met; "may-issue" laws, which give law enforcement officials wide discretion over whether to issue concealed firearm carry permits or not; and homicide rates.

Methods. We compared homicide rates in shall-issue and may-issue states and total, firearm, nonfirearm, handgun, and long-gun homicide rates in all 50 states during the 25-year period of 1991 to 2015. We included year and state fixed effects and numerous state-level factors in the analysis.

Results. Shall-issue laws were significantly associated with 6.5% higher total homicide rates, 8.6% higher firearm homicide rates, and 10.6% higher handgun homicide rates, but were not significantly associated with long-gun or nonfirearm homicide.

Conclusions. Shall-issue laws are associated with significantly higher rates of total, firearm-related, and handgun-related homicide. (*Am J Public Health.* 2017;107:1923–1929. doi:10.2105/AJPH.2017.304057)

See also Donohue, p. 1864, and also Galea and Vaughan, p. 1867.

Firearm violence is a major public health problem. In 2015, there were approximately 36 000 firearm-related deaths in the United States; 13 463 were homicides, 22 018 were suicides, and 489 were unintentional injuries.[1] During the same year, 72.9% of homicides were firearm homicides[1] and, of these, approximately 90% were committed with a handgun. A central question in the debate about public policies to reduce firearm violence is whether easier access to concealed handguns increases or decreases the rate of firearm-related homicides.[2] Some have argued that the feared or actual presence of armed citizens may deter violent crime.[3] Others have suggested that a higher prevalence of people carrying guns will increase the likelihood that an altercation results in a fatality.[4] Thus, having a clear understanding of the impact of concealed-carry laws on firearm-related homicide would help guide policymakers who are aiming to reduce firearm violence.

As of the end of 2015, all states allowed certain persons to carry concealed handguns, but there were 3 major variations in permitting policy[5] (Table 1). In 9 states, law enforcement officials had wide discretion over whether to issue concealed-carry permits; these are referred to as "may-issue" states. In 32 states, there was little or no discretion; these are referred to as "shall-issue" states because permits must be issued if requisite criteria are met. In an additional 9 states, there was no permit necessary to carry a concealed handgun; these are referred to as "permitless-carry" states. The wide variation in these policies between states and over time presents the opportunity to compare homicide rates between states with varying concealed-carry permitting policies to examine the impact of concealed-carry laws on homicide.

The critical difference between may-issue and shall-issue laws is that in may-issue states, law enforcement officials may use their judgment in making decisions about whether to approve or deny a permit application, whereas in shall-issue states, no judgment is involved—the application must be approved unless the applicant is categorically prohibited from concealed handgun possession. In may-issue states, the element of discretion allotted to law enforcement is typically a judgment regarding the "suitability" or "need" of a person to carry a concealed weapon (Table 2). Law enforcement officials have a wide degree of latitude in making these judgments. In shall-issue states, the categorical prohibitions consist of a list of specific criminal convictions.

Unfortunately, the existing literature on the impact of concealed carry laws is inconsistent. At least 10 national studies have examined the relationship between shall-issue concealed-carry laws and firearm-related or total homicide rates at the state level (Table A, available as a supplement to the online version of this article at http://www.ajph.org).[3,6–14] In 2 studies, shall-issue laws were found to decrease homicide rates.[3,6] In 2 studies, these laws were found to increase homicide rates.[7,8] Six studies reported no clear impact of shall-issue laws on homicide rates.[9–14] The inconsistency of these results has understandably created some confusion about what approach is most effective to address the firearm violence problem.

Most of the published literature on this topic includes data that are more than a decade old: the most recent year of data analyzed was

ABOUT THE AUTHORS

Michael Siegel, Ziming Xuan, Craig S. Ross, and Sandro Galea are with the Boston University School of Public Health, Boston, MA. Bindu Kalesan is with the Boston University School of Medicine. Eric Fleegler is with Children's Hospital Boston. Kristin A. Goss is with the Sanford School of Public Policy, Duke University, Durham, NC.

Correspondence should be sent to Michael Siegel, MD, MPH, Department of Community Health Sciences, Boston University School of Public Health, 801 Massachusetts Ave, Boston, MA 02118 (e-mail: mbsiegel@bu.edu). Reprints can be ordered at http://www.ajph.org by clicking the "Reprints" link.

This article was accepted August 1, 2017.
doi: 10.2105/AJPH.2017.304057

TABLE 1—Concealed-Carry Permitting Laws and Age-Adjusted Firearm Homicide Rates by US State, 2015, and Status of Laws During the Period of 1991 to 2015

State	Age-Adjusted Firearm Homicide Rate,[a] 2015 (per 100 000)	Status of Concealed-Carry Permitting Law, 2015	Effective Date of Current (as of 2015) Concealed-Carry Law
Hawaii[b]	0.75	May issue	Before 1991
New Hampshire	0.96	Shall issue	Before 1991
Rhode Island	0.99	May issue	Before 1991
Maine	1.14	Shall issue	Before 1991
Massachusetts	1.26	May issue	Before 1991
Utah	1.39	Shall issue	1995
Idaho	1.29	Shall issue	Before 1991
Iowa	1.62	Shall issue	Before 1991
North Dakota	1.69	Shall issue	Before 1991
Vermont	1.76	Permitless carry	Before 1991
Minnesota	1.77	Shall issue	2003
South Dakota	1.97	Shall issue	Before 1991
New York	2.07	May issue	Before 1991
Wyoming	2.16	Permitless carry	2011[c]
Montana	2.17	Shall issue	Before 1991
Washington	2.32	Shall issue	Before 1991
Oregon	2.35	Shall issue	Before 1991
Connecticut	2.43	May issue	Before 1991
Colorado	2.46	Shall issue	2003
Nebraska	2.67	Shall issue	2007
West Virginia	2.89	Shall issue	Before 1991
Wisconsin	3.18	Shall issue	2011
New Jersey	3.22	May issue	Before 1991
Virginia	3.29	Shall issue	1995
Kansas	3.35	Shall issue	2007
California	3.52	May issue	Before 1991
Arizona	3.56	Permitless carry	2010[c]
Kentucky	3.96	Shall issue	1996
Texas	4.04	Shall issue	1995
Pennsylvania	4.34	Shall issue	Before 1991
Ohio	4.38	Shall issue	2004
Nevada	4.49	Shall issue	1995
North Carolina	4.54	Shall issue	1995
Indiana	4.61	Shall issue	Before 1991
Florida	4.66	Shall issue	Before 1991
Michigan	4.74	Shall issue	2001
New Mexico	4.79	Shall issue	2001
Alaska	5.22	Permitless carry	2003[c]

Continued

2010, and only 3 of the 10 studies examined data past the year 1998 (Table A, available as a supplement to the online version of this article at http://www.ajph.org). Since 1998, 11 additional states have enacted shall-issue laws.[5] This provides more variation over time and a longer follow-up period to examine this research question. Moreover, Ayres and Donohue[15] and Hepburn et al.[11] have suggested that the relationship between concealed-carry laws and homicide rates may have been different during the period before and after the early 1990s. In addition, studies that included homicide rates from before 1994 were examining a trend that was increasing, whereas studies examining homicide rates after 1994 were capturing declining trends. For these reasons, a reexamination of this research question with more recent data is needed.

One limitation of the existing literature is that no previously published research has examined the specific impact of concealed-carry laws on handgun versus long-gun homicide rates. This is important because if such laws increase homicide by making it easier for people at high risk for violence to carry handguns, this effect should only be observed in relation to handgun-related homicides, not homicides committed with long guns. On the other hand, if permissive concealed-carry laws deter crime by generating fear among potential perpetrators of encountering an armed individual, then all crime including handgun, long-gun, and nonfirearm homicide should decrease.

Another limitation of previous studies is that nearly all of them used linear models. However, homicide rates represent count data, and the distribution of homicide rates across states is highly skewed[16] (Figure A, available as a supplement to the online version of this article at http://www.ajph.org). Plassmann and Tideman argued that a count model (such as a Poisson or negative binomial model) is the most reliable for analyzing crimes, such as homicides, with low occurrence rates.[16] Beyond the Plassmann and Tideman study, only 1 other study[11] used a count model.

We examined the relationship between shall-issue concealed-carry laws and total, firearm-related, and non–firearm-related homicide rates, as well as handgun versus long-gun homicide rates across all 50 states

TABLE 1—*Continued*

State	Age-Adjusted Firearm Homicide Rate,[a] 2015 (per 100 000)	Status of Concealed-Carry Permitting Law, 2015	Effective Date of Current (as of 2015) Concealed-Carry Law
Arkansas	5.34	Shall issue	1995
Illinois	5.45	Shall issue	2013
Tennessee	5.51	Shall issue	1994
Georgia	5.73	Shall issue	Before 1991
Oklahoma	5.87	Shall issue	1995
Delaware	6.12	May issue	Before 1991
South Carolina	7.55	Shall issue	1996
Maryland	7.69	May issue	Before 1991
Missouri	7.92	Shall issue	2003
Alabama	8.43	Shall issue	2013
Mississippi	9.11	Shall issue	1991
Louisiana	9.96	Shall issue	1996

Note. "May-issue" states are those in which law enforcement officials had wide discretion over whether to issue concealed-carry permits. "Shall-issue" states are those in which there was little or no discretion; permits must be issued if requisite criteria are met. "Permitless-carry" states are those in which there was no permit necessary to carry a concealed handgun.

[a]From Centers for Disease Control and Prevention (CDC).[1]

[b]Data for Hawaii are unavailable for the years 2010 to 2015 because the CDC's Web-Based Injury Statistics Query and Reporting Systems does not report homicide counts fewer than 10. The data here are from 2009.

[c]Changed from "may issue" to "shall issue" in 1994.

during the 25-year time period of 1991 to 2015 with both count and linear regression models. We examined the specificity of the relationship between concealed-carry laws and homicide rates by separately modeling firearm versus nonfirearm homicide rates and then within firearm-related homicides by modeling handgun versus long-gun homicide rates. We analyzed the relationship between shall-issue concealed-carry laws and homicide rates by using both a count and a linear regression model, thus examining the robustness of results to the type of model used.

METHODS

We used a quasi-experimental panel design, taking advantage of changes in state concealed-carry permitting laws over time, to explore the relationship between these laws and total, firearm-related, and non–firearm-related homicide rates in the 50 states over a 25-year period, 1991 to 2015. We modeled homicide rates in 2 ways: (1) using a negative binomial regression with homicide rates as the outcome variable and (2) using linear regression with log-transformed homicide rates as the outcome variable. In both cases, we included year and state fixed effects and controlled for a range of time-varying, state-level factors.

Variables and Data Sources

Outcome variables. The main outcome variable was the age-adjusted firearm homicide rate in each year analyzed. For example, Missouri's shall-issue law went into effect in 2003; thus, we analyzed homicide rates associated with Missouri's shall-issue law for the years 2004 to 2015. We obtained homicide rates from the Centers for Disease Control and Prevention's (CDC's) Web-Based Injury Statistics Query and Reporting Systems (WISQARS) database.[1] This is the ideal source for homicide data because there is complete annual reporting from all 50 states and because the data are extracted from the Vital Statistics death registry maintained by the National Center for Health Statistics, which is based on standardized death certificates. The completeness of reporting is approximately 99%.[17] The CDC age-adjusted the rates to the 2000 standard population.

The second outcome variable was the handgun or long-gun homicide rate, obtained from the Federal Bureau of Investigation's Uniform Crime Reports, Supplemental Homicide Reports (SHR).[18] Although WISQARS does provide mortality data from *International Classification of Diseases, Ninth Revision* and *Tenth Revision,* codes that can list handgun and long gun as the cause of death, unfortunately, most death certificates involving a firearm homicide do not specify the type of weapon used. Therefore, most firearm homicide deaths in WISQARS are classified as "other and unspecified" firearm, and it is not possible to use these data to disaggregate handgun and long-gun homicides.[19] By contrast, the SHR is missing data on the type of weapon used in firearm homicides in just 13.4% of cases. Thus, the SHR is the best, if not only, source for state-specific, firearm type–specific homicide data.

The SHR disaggregates firearm homicides into handgun, rifle, shotgun, and other (and unknown). We used the handgun deaths to generate handgun homicide rates and the sum of rifle, shotgun, and other gun deaths to generate long-gun homicide rates for each state and year. Although SHR data may include listing of multiple weapons in an incident, only 1 weapon may be associated with a homicide death.[20] Because of missing data on weapon type, we excluded 13.4% of firearm homicide cases in estimating handgun homicide rates. Nevertheless, there was little discrepancy between the firearm homicide totals from WISQARS and the SHR, which were correlated at $r = 0.98$.

Because not all local law enforcement agencies complete the supplemental reports, the SHR data set excludes approximately 10% of all homicides.[21] This problem was addressed by applying weights that adjusted each state- and year-specific estimate up to the overall number of homicides reported in the Uniform Crime Report for that state and year. Fox kindly provided us with updated SHR files that added previously

TABLE 2—Elements of Discretion in Law Enforcement Decisions to Approve or Deny Concealed Handgun Carry Permits: "May-Issue" US States, 2015

State	Elements of Discretion	Citation
California	Applicant must be of "good moral character" and must have "good cause" for issuance of the license.	California Penal Code § 26150, § 26155
Connecticut	Applicant must intend only to make "legal use" of the handgun and must be a "suitable person to receive such permit."	Connecticut General Statutes § 29-28
Delaware	Applicant must be "of good moral character," must desire the handgun for "personal protection" or "protection of the person's property," and must submit signed, written statements of 5 "respectable citizens" of the county who testify that the applicant is a person "of sobriety and good moral character" and "bears a good reputation for peace and good order in the community" and that a handgun is "necessary for the protection of the applicant or the applicant's property." The Superior Court has discretion to approve or deny the application.	Delaware Code § 1441
Hawaii	Must be "an exceptional case," the applicant must show "reason to fear injury to the applicant's person or property," the applicant must be "a suitable person" to be licensed, and the chief of police must determine that the person "is qualified to use the firearm in a safe manner."	Hawaii Revised Statutes § 134-9
Maryland	Applicant must have a "good and substantial reason to wear, carry, or transport a handgun, such as a finding that the permit is necessary as a reasonable precaution against apprehended danger," and the applicant must not have "exhibited a propensity for violence or instability that may reasonably render the person's possession of a handgun a danger to the person or to another."	Maryland Public Safety Code § 5-306
Massachusetts	Applicant must be a "suitable" person and must not be judged to potentially create a risk to public safety.	Massachusetts General Laws 140 § 131
New Jersey	Applicant must demonstrate a "justifiable need to carry a handgun" and must submit endorsements by 3 individuals who have known the applicant for at least 3 years that the applicant is "a person of good moral character and behavior."	New Jersey Statutes § 2C:58–4
New York	Applicant must be "of good moral character," must be "of good character, competency, and integrity," and there must be no "good cause" for denial of the license.	New York Penal Law § 400.00
Rhode Island	Applicant must have "good reason to fear an injury to his or her person or property" or have "any other proper reason" for carrying a handgun and must be a "suitable person to be so licensed."	General Laws of Rhode Island § 11-47-11

Note. "May-issue" states are those in which law enforcement officials had wide discretion over whether to issue concealed-carry permits.

missing data for Florida and included data through 2015.[21]

Main predictor variable. Using *Thomson Reuters Westlaw* to access historical state statutes and session laws, we developed a database indicating the presence or absence of 100 provisions of firearm laws in each state over the 25-year period.[5] We coded laws by the year they went into effect, regardless of the month of the effective date. However, in the analytic models, we lagged the state laws by 1 year, which ensured that all laws were in effect during the year in which their impact was being assessed. Following Lott and Mustard,[22] we assessed the impact of laws starting in the first full year they were in effect.

We examined the potential impact of shall-issue laws, comparing them to may-issue laws. In other words, using the may-issue states as the reference group, we estimated the impact of shall-issue laws on homicide rates. Because only 4 states had permitless-carry laws in place during the study period, there were not enough observations to allow any meaningful analyses of these laws. Therefore, we deleted state–year observations in which a permitless-carry law was in effect.

Control variables. We controlled for 12 state-level factors that (1) were found in the previous literature[3,6–14] to be significantly related to homicide rates and (2) were significantly related to the presence of shall-issue laws in our data set (i.e., the regression coefficient for the variable was significant at a level of $P = .05$ in a logistic regression with shall-issue law as the dependent variable): household firearm ownership (using the standard proxy, which is the percentage of all suicides committed with a firearm), proportion of Blacks, proportion of young adults (aged 18 to 29 years), proportion of men among young adults, proportion of the population living in urban areas, total population, population density, per capita alcohol consumption, the nonhomicide violent crime rate (aggravated assault, robbery, and forcible rape), the poverty rate, unemployment rate, median household income, per capita disposable income, incarceration rate, and per capita number of law enforcement officers. Variable definitions and data sources are provided in Table B, available as a supplement to the online version of this article at http://www.ajph.org. We also controlled for the following state firearm laws that could serve as alternative explanations for changes in homicide during the study period: (1) universal background checks required for all handgun purchases, (2) waiting periods required for all handgun purchases, and (3) permits required to purchase or possess firearms.

Analysis

Count models. Because homicide rates are not normally distributed but skewed and overdispersed, we modeled this outcome by using a negative binomial distribution. To control for clustering in our data by year (25 levels) and by state (50 levels), we entered year and state as fixed effects in the regression models. We used robust standard errors that account for the clustering of observations, serial autocorrelation, and heteroskedasticity.[23]

Our final model was as follows:

$$(1)\quad Pr(H_{st}=h_{st}) = \left[\Gamma\left(y_{st}+\alpha^{-1}\right)/\Gamma\left((y_{st}+1)\Gamma\alpha^{-1}\right)\right]\left[1/(1+\alpha\,\mu_{st})\right]^{1\alpha}\left[\mu_{st}/\left(\alpha^{-1}+\mu_{it}\right)\right]^{yst},$$

where $Pr(H_{st}=h_{st})$ is the probability that state s in year t has a homicide rate equal to h_{st}, $E(H_{st})=\mu_{st}$, and $Var(H_{st})=\mu_{st}+\mu^2{}_{st}$.

The mean homicide rate was then modeled as follows:

$$(2)\quad ln(\mu_{st}) = \alpha + \beta_1 CC_{st} + \beta_2 C_{st} + S + T + e,$$

where CC_{st} is a dummy variable for the presence of a shall-issue law, C is a vector of control variables, S represents state fixed effects, and T represents year fixed effects.

The negative binomial regression coefficients are reported as incidence rate ratios (IRRs). The IRR indicates the percentage difference in homicide rate for states with a shall-issue concealed-carry law compared with states with a may-issue law.

Linear models. To check the robustness of our findings, we repeated the analyses with a linear regression model, with the log-transformed homicide rate as the outcome variable, again by using robust standard errors.[23] As with the negative binomial models, we included year and state fixed effects, and we included the same state-level control variables.

We conducted analyses with Stata version 14.1 (StataCorp LP, College Station, TX). We evaluated the significance of regression coefficients by using a Wald test at $\alpha = 0.05$.

We checked the robustness of our results by conducting several sensitivity analyses, including

1. Restricting the analysis to the 23 states in which shall-issue laws were adopted during the study period,
2. Using raw count data instead of homicide rates,
3. Restricting the analysis to states with population greater than 1 000 000,
4. Restricting the analysis to the period 1991 to 2002,
5. Restricting the analysis to the period 2003 to 2015, and
6. Using SHR instead of WISQARS homicide data (thus avoiding the problem of missing data for some smaller states after 1998).

RESULTS

During the study period, 23 states adopted shall-issue laws (Table 1). By 2015, 37 states had such laws. In the same year, the average firearm homicide rate in the states with shall-issue laws was 4.11 per 100 000, compared with 3.41 per 100 000 in the may-issue states. The number of states that had permitless-carry laws in effect at all during the study period was small (n = 4), as was the number of observations (n = 46), limiting our ability to analyze the impact of these laws. Because CDC does not report homicide counts of fewer than 10 in years after 1998, we were missing outcome data for several years for 6 states (Hawaii, New Hampshire, North Dakota, South Dakota, Vermont, and Wyoming); a sensitivity analysis with SHR data revealed that these omissions do not affect our findings.

In negative binomial regression models, shall-issue concealed-carry permitting laws were significantly associated with 6.5% higher total homicide rates compared with may-issue states (IRR = 1.065; 95% confidence interval [CI] = 1.032, 1.099; Table 3). The association was specific to firearm homicide rates, which were 8.6% higher in shall-issue states (IRR = 1.086; 95% CI = 1.047, 1.126). There was no significant association between shall-issue laws and nonfirearm homicide rates (IRR = 1.014; 95% CI = 0.963, 1.068). Further disaggregation within firearm homicides showed that the association between shall-issue laws and firearm homicide rates was specific to handgun homicide. Shall-issue states had handgun homicide rates that were 10.6% higher (IRR = 1.106; 95% CI = 1.039, 1.177), but there was no significant association with long-gun homicide rates (IRR = 0.999; 95% CI = 0.915, 1.090).

The results of the linear regression analyses were similar. Here, shall-issue laws were significantly associated with 6.6% higher total homicide rates compared with may-issue states (95% CI = 3.0%, 10.4%; data not shown). The association was specific to firearm homicide rates, which were 11.7% higher in "shall issue" states (95% CI = 6.4%, 17.2%); there was no significant association between these laws and nonfirearm homicide rates. Further disaggregation within firearm homicides showed that the association between shall-issue laws and firearm homicide rates was specific to handgun homicide. Shall-issue states had handgun homicide rates that were 19.8% higher (95% CI = 10.3%, 30.1%), but rates of long-gun homicide were not significantly different in states with shall-issue compared with may-issue laws.

The significant association between shall-issue laws and higher total, firearm, and handgun-related homicide rates remained when we restricted the analysis to the 23 states in which these laws were adopted during the study period (Table 3). This pattern of results was robust to a series of additional sensitivity checks, including using raw count data, restricting the analysis to states with a population of more than 1 000 000, restricting the analysis to the period 1991 to 2002, restricting the analysis to the period 2003 to 2015, and using SHR instead of WISQARS homicide data.

DISCUSSION

To the best of our knowledge, this is the first study to examine the relationship between concealed-carry permitting laws and handgun-specific homicide rates. We found that, when we used both count and linear models and after we controlled for a range of time-varying state factors and for unobserved time-invariant state factors by using a fixed-effects model, shall-issue concealed-carry permitting laws were significantly associated with 6.5% higher total homicide rates, 8.6% higher firearm-related homicide rates, and 10.6% higher handgun-specific homicide rates compared with may-issue states.

A major reason for inconsistent results in the existing literature on the effects of concealed-carry laws may be that the relationship between concealed-carry laws and homicide rates was different during the period before and after the early 1990s.[11,15] It is possible that despite the enactment of early shall-issue laws in the 1970s and 1980s, the demand for handgun permits in those states was modest. There has been a striking increase in the demand for pistols, especially those designed for concealed carry, during the past decade.[24] Recently, Steidley found that the adoption of shall-issue laws during the period 1999 to 2013 was associated with a persistent, long-term increase in handgun sales in all 7 states studied.[25] Our analysis provides further support for the hypothesis that the relationship between shall-issue laws and higher homicide rates increased over time, as the regression coefficients for these laws was higher for the second half of the study period (2003–2015) compared with the first half (1991–2002).

Our finding that the association between shall-issue laws and homicide rates is specific to handgun homicides adds plausibility to the observed relationship. If the relationship between shall-issue laws and homicide rates were spurious, one might expect to see the relationship hold for long-gun as well as handgun homicide rates. Moreover, this finding is inconsistent with the hypothesis that permissive concealed-carry laws deter crime by increasing the presence of armed individuals. Were that the case, one would expect to see lower handgun, nonhandgun, and nonfirearm homicide rates in shall-issue compared with may-issue states. The lack of an association between shall-issue laws and long-gun homicide rates is also inconsistent with the hypothesis that the presence of more concealed weapons escalates the level of violence in encounters that may involve a long gun.

Strengths and Limitations

This study has several novel strengths, including the use of both count and linear models, the use of recent data (through 2015), and the disaggregation of homicide rates. Nevertheless, caution should be exercised in assessing causality from an ecological study such as this one. In particular, these results should be interpreted with caution because of the possibility that they reflect a reverse association. That is, it is possible that the adoption of shall-issue concealed carry laws is associated with higher baseline homicide rates so that we are picking up not a causal effect of these laws on homicide but a systematic difference in baseline homicide rates between states that do or do not have these laws. However, our findings hold even when the analysis is restricted to states that started with may-issue laws at the beginning of the study period and adopted shall-issue laws during the study period.

An additional limitation of this study is that we could not consider the enforcement of concealed-carry laws.[26] Enforcement of these laws may vary not only among states, but also among counties in the same state.[11] In addition, we did not have information on the number of concealed-carry permits issued in each state or the number of homicides committed by concealed-carry permittees.

It is also important to note that we examined only fatal firearm injuries. Further research should investigate potential effects of concealed-carry laws on nonfatal firearm injuries.

Finally, we were unable to analyze the impact of permitless-carry laws because of the small number of observations. Only 4 states

TABLE 3—Sensitivity Analyses of Relationship Between “Shall-Issue” Concealed-Carry Permitting Laws and Homicide Rates: United States, 1991–2015

	Homicide Rate, IRR (95% CI)		
Type of Analysis	Total	Firearm	Handgun
Main analysis	1.065 (1.032, 1.099)	1.086 (1.047, 1.126)	1.106 (1.039, 1.177)
Analysis restricted to states that adopted shall-issue concealed-carry laws during study period	1.063 (1.028, 1.099)	1.068 (1.030, 1.108)	1.074 (1.002, 1.150)
Analysis using raw count of homicides with population as the exposure variable	1.051 (1.020, 1.083)	1.079 (1.039, 1.120)	1.139 (1.067, 1.217)
Analysis restricted to states with population > 1 million	1.055 (1.023, 1.087)	1.067 (1.030, 1.105)	1.095 (1.029, 1.166)
Analysis restricted to years before 2003 (1991–2002)	1.058 (1.014, 1.104)	1.067 (1.019, 1.116)	1.107 (1.037, 1.180)
Analysis restricted to years after 2002 (2003–2015)	1.064 (1.009, 1.122)	1.100 (1.028, 1.176)	1.274 (1.092, 1.488)
Analysis using Supplemental Homicide Report data instead of Vital Statistics data	1.044 (1.006, 1.083)	1.094 (1.047, 1.143)	1.106 (1.039, 1.177)

Note. “Shall-issue” states are those in which there was little or no discretion; permits must be issued if requisite criteria are met. CI = confidence interval; IRR = incidence rate ratio. All models include year and state fixed effects and control for the following time-varying, state-level factors: household gun-ownership levels, proportion of young men, proportion of young adults, proportion of Blacks, proportion living in an urban area, total population, population density, median household income, poverty rate, unemployment rate, per capita disposable income, per capita alcohol consumption, violent crime rate, incarceration rate, per capita law enforcement officers, universal background check laws for all handguns, waiting periods for all handguns, and permits required for all firearms.

had permitless-carry laws in place during the study period. However, in the past 2 years, an additional 5 states have enacted such laws. Elucidating the impact of permitless-carry laws will require follow-up for the 9 states that now have such laws in effect.

Conclusions

Despite these limitations, this study suggests that there is a robust association between shall-issue laws and higher rates of firearm homicides. The trend toward increasingly permissive concealed-carry laws is inconsistent with public opinion, which tends to oppose the carrying of guns in public.[27] Our findings suggest that these laws may also be inconsistent with the promotion of public safety. AJPH

CONTRIBUTORS

M. Siegel conceptualized the study, led the data analysis and writing, and was the principal author of this article. Z. Xuan and C. S. Ross assisted with the study design and analytical plan. All authors contributed toward the interpretation of data analyses, critical review of the article, and revision of the article.

ACKNOWLEDGMENTS

Support for this research was provided by the Robert Wood Johnson Foundation Evidence for Action Program (grant 73337).

We gratefully acknowledge the assistance of James Alan Fox, PhD, the Lipman Family Professor of Criminology, Law, and Public Policy at the School of Criminology and Criminal Justice at Northeastern University, who kindly provided the Multiply-Imputed Supplemental Homicide Reports File, 1976–2015, including the data sets and a codebook.

Note. The views expressed here do not necessarily reflect those of the Robert Wood Johnson Foundation.

HUMAN PARTICIPANT PROTECTION

This study made use of secondary data only and did not require institutional review board approval.

REFERENCES

1. Centers for Disease Control and Prevention. Web-Based Injury Statistics Query and Reporting Systems: fatal injury reports. Available at: http://www.cdc.gov/injury/wisqars/fatal_injury_reports.html. Accessed March 15, 2017.

2. Donohue JJ. Guns, crime, and the impact of state right-to-carry laws. *Fordham Law Rev*. 2005;73:623–652.

3. Lott JR. *More Guns, Less Crime: Understanding Crime and Gun Control Laws*. 3rd ed. Chicago, IL: The University of Chicago Press; 2010.

4. Miller M, Azrael D, Hemenway D. Firearms and violent death in the United States. In: Webster DW, Vernick JS, eds. *Reducing Gun Violence in America: Informing Policy With Evidence and Analysis*. Baltimore, MD: The Johns Hopkins University Press; 2013.

5. Siegel M, Pahn M, Xuan Z, et al. Firearm-related laws in all 50 states, 1991–2016. *Am J Public Health*. 2017; epub ahead of print May 18, 2017.

6. Lott JR Jr, Whitley JE. Safe-storage gun laws: accidental deaths, suicides, and crime. *J Law Econ*. 2001;44:659–689.

7. Zimmerman PR. The deterrence of crime through private security efforts: theory and evidence. *Int Rev Law Econ*. 2014;37:66–75.

8. Ludwig J. Concealed-gun-carrying laws and violent crime: evidence from state panel data. *Int Rev Law Econ*. 1998;18:239–254.

9. Aneja A, Donohue JJ, Zhang A. The impact of right-to-carry laws and the NRC report: lessons for the empirical evaluation of law and policy. *Am Law Econ Rev*. 2011;13(2):565–632.

10. Rosengart M, Cummings P, Nathens A, Heagerty P, Maier R, Rivara F. An evaluation of state firearm regulations and homicide and suicide death rates. *Inj Prev*. 2005;11(2):77–83.

11. Hepburn L, Miller M, Azrael D, Hemenway D. The effect of nondiscretionary concealed weapon carrying laws on homicide. *J Trauma*. 2004;56(3):676–681.

12. Sommers PM. Deterrence and gun control: an empirical analysis. *Atl Econ J*. 1980;8:89–94.

13. DeZee MR. Gun control legislation: impact and ideology. *Law Policy Q*. 1983;5(3):367–379.

14. Murray DR. Handguns, gun control laws and firearm violence. *Soc Probl*. 1975;23:81–93.

15. Ayres I, Donohue JJ III. Shooting down the "more guns, less crime" hypothesis. *Stanford Law Rev*. 2003;55:1193–1300.

16. Plassmann F, Tideman TN. Does the right to carry concealed handguns deter countable crimes? Only a count analysis can say. *J Law Econ*. 2001;44:771–798.

17. Regoeczi W, Banks D. *The Nation's Two Measures of Homicide*. Washington, DC: US Department of Justice, Office of Justice Programs, Bureau of Justice Studies; 2014.

18. National Archive of Criminal Justice Data. *Uniform Crime Reporting Program Data Series. Supplemental Homicide Reports, 1981–2015*. Ann Arbor, MI: Inter-university Consortium for Political and Social Research; 2016.

19. Rokaw WM, Mercy JA, Smith JC. Comparing death certificate data with FBI crime reporting statistics on US homicides. *Public Health Rep*. 1990;105(5):447–455.

20. Supplementary homicide report (OMB form no. 1110–0002), offense 1a. Murder and nonnegligent manslaughter. Washington, DC: Federal Bureau of Investigation; 2017. Available at: https://ucr.fbi.gov/nibrs/addendum-for-submitting-cargo-theft-data/shr. Accessed June 17, 2017.

21. Fox J. Multiply-imputed supplementary homicide reports file, 1976–2015. Boston, MA: Northeastern University; 2017.

22. Lott JR, Mustard DB. Crime, deterrence, and right-to-carry concealed handguns. *J Legal Stud*. 1997;26:1–68.

23. White H. A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica*. 1980;48(4):817–838.

24. Smith VM, Siegel M, Xuan Z, et al. Broadening the perspective on gun violence: an examination of the firearms industry, 1990–2015. *Am J Prev Med*. 2017; Epub ahead of print.

25. Steidley T. *Movements, Malefactions, and Munitions: Determinants and Effects of Concealed Carry Laws in the United States* [dissertation]. Columbus, OH: The Ohio State University; 2016.

26. Lott JR. Not all right-to-carry laws are the same, yet much of the literature keeps ignoring the differences. Crime Prevention Research Center. 2014. Available at: https://ssrn.com/abstract=2524729. Accessed April 15, 2017.

27. Wolfson JA, Teret SP, Azrael D, Miller M. US public opinion on carrying firearms in public places. *Am J Public Health*. 2017;107(6):929–937.

CERTIFICATE OF SERVICE

Case Name: **Baird, Mark v. Xavier Becerra** No. **2:19-cv-00617-KJM-AC**

I hereby certify that on August 2, 2019, I electronically filed the following documents with the Clerk of the Court by using the CM/ECF system:

DECLARATION OF R. MATTHEW WISE IN SUPPORT OF DEFENDANT'S OPPOSITION TO PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

I certify that **all** participants in the case are registered CM/ECF users and that service will be accomplished by the CM/ECF system.

I declare under penalty of perjury under the laws of the State of California the foregoing is true and correct and that this declaration was executed on August 2, 2019, at Sacramento, California.

Tracie L. Campbell	***/s/ Tracie Campbell***
Declarant	Signature

SA2019101934
13978109.docx