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RIGHT-TO-CARRY LAWS AND VIOLENT CRIME: COMPREHENSIVE ASSESSMENT USING PANEL DATA, THE LASSO, AND A STATE-LEVEL SYNTHETIC CONTROLS ANALYSIS

John J. Donohue
Abhay Aneja
Kyle D. Weber

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Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data, the LASSO, and a State-Level Synthetic Controls Analysis
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ABSTRACT

The 2005 report of the National Research Council (NRC) on Firearms and Violence recognized that violent crime was higher in the post-passage period (relative to national crime patterns) for states adopting right-to-carry (RTC) concealed handgun laws, but because of model dependence the panel was unable to identify the true causal effect of these laws from the then-existing panel data evidence. This study uses 14 additional years of state panel data (through 2014) capturing an additional eleven RTC adoptions and new statistical techniques to see if more convincing and robust conclusions can emerge.

Our preferred panel data regression specification (the “DAWmodel”) and the Brennan Center (BC) model, as well as other statistical models by Lott and Mustard (LM) and Moody and Marvell (MM) that had previously been offered as evidence of crime-reducing RTC laws, now only generate statistically significant estimates showing RTC laws increase overall violent crime and/or murder when run on the most complete data. A LASSO analysis finds that RTC laws are always associated with increased violent crime. To the extent the large increases in gun thefts induced by RTC laws generate crime increases in non-RTC states, the panel data estimates of the increase in violent crime will be understated.

We then use the synthetic control approach of Alberto Abadie and Javier Gardeazabal (2003) to generate state-specific estimates of the impact of RTC laws on crime. Our major finding is that under all four specifications (DAW, BC, LM, and MM), RTC laws are associated with higher aggregate violent crime rates, and the size of the deleterious effects that are associated with the passage of RTC laws climbs over time. Ten years after the adoption of RTC laws, violent crime is estimated to be 13-15 percent higher than it would have been without the RTC law. Unlike the panel data setting, these results are not sensitive to the covariates included as predictors. The magnitude of the estimated increase in violent crime from RTC laws is substantial in that, using a consensus estimate for the elasticity of crime with respect to incarceration of .15, the average RTC state would have to double its prison population to counteract the RTC-induced increase in violent crime.

John J. Donohue
Stanford Law School
Crown Quadrangle
559 Nathan Abbott Way
Stanford, CA 94305
and NBER
donohue@law.stanford.edu

Abhay Aneja
Stanford Law School
559 Nathan Abbott Way
Stanford, CA 94305
abhay_aneja@haas.berkeley.edu

Kyle D. Weber
Department of Economics
Columbia University
kdw2126@columbia.edu

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 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 twenty-five 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 their 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, 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 paper 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 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 eleven newly adopting RTC states, offers any clearer picture of the causal impact of allowing citizens to carry concealed weapons. Across seven different permutations from four major sets of explanatory variables—including our pre-

[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, Donohue, and Zhang (2011) (and Aneja, Donohue, and Zhang (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) and III(C).

ferred model (DAW) plus models used by the Brennan Center (BC), Lott and Mustard (LM), and Moody and Marvell (MM)—all of the statistically significant results show RTC laws are associated with *higher* rates of overall violent crime and/or murder. Second, we probe the full array of explanatory variables across all these models using a LASSO analysis, which uniformly finds that RTC law over the 1979-2014 period were associated with higher rates of violent crime.

Finally, to address some of the weaknesses of panel data models, we undertake an extensive synthetic controls analysis in order to present the type of convincing and robust results that can reliably guide policy in this area.[2] This synthetic controls methodology—first introduced in Abadie and Gardeazabal (2003) and expanded in Abadie, Diamond, and Hainmueller (2010) and Abadie, Diamond, and Hainmueller (2014)—uses a matching methodology to create a credible "synthetic control" based on a weighted average of other states that best matches the pre-passage 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 post-passage period, we derive year-by-year estimates for the impact of RTC laws in the ten years following adoption.[3]

To preview our major findings, the synthetic controls estimate of the average impact of RTC laws across the 33 states that adopt between 1981 and 2007[4] indicate that violent crime is substantially higher after ten years than would have been the case had the RTC law not been adopted. Essentially, for violent crime, the synthetic controls approach provides a similar portrayal of RTC laws as that provided by the DAW and BC panel data models and undermines the results of the LM and MM panel data models. According to the aggregate synthetic control models—whether one uses the DAW, BC, LM, or MM covariates—RTC laws led to increases in violent crime of 13-15 percent after ten years, with positive but not statistically significant effects on property crime and murder. The median effect of RTC adoption after ten years is 12.3 percent if one considers all 31 states with ten years of data and 11.1 if one limits the analysis to the 26 states with the most compelling pre-passage 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

[2]Abadie, Diamond, and Hainmueller (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 pre-treatment period (or a variation on this RMSPE implemented in this paper), 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 pre-treatment 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) and for Illinois (2014 RTC law), because of the limited post-passage data.

reject the null hypothesis that RTC laws have no impact on aggregate violent crime.

The structure of the paper proceeds as follows. Part II begins with a discussion of the ways in which increased carrying of guns could either dampen 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 dampens their effectiveness in controlling crime. We then show that a simple comparison of the change in violent crime from 1977-2014 in the states that have resisted the adoption of RTC laws is almost an order of magnitude greater drop in crime compared to the adopting states (a 42.3 percent drop versus a 4.3 percent drop), although a simple panel data model with only state and year effects reduces the differential to 20.2 percent. Part III discusses the panel data results for the four different models, showing that the DAW and BC models indicate that RTC laws have increased violent and property crime, while the LM and MM models provide evidence that RTC laws have increased murder. We argue that the DAW set of explanatory variables are the most plausible and show that modest and advisable corrections to the LM and MM specifications also generate estimates that RTC laws increase violent crime, which is largely confirmed by a LASSO analysis.

The remainder of the paper shows that the synthetic controls approach under all four sets of explanatory variables uniformly supports the conclusion that RTC laws lead to substantial increases in violent crime. Part IV describes the details of our implementation of the synthetic controls approach and shows that the mean and median estimates of the impact of RTC laws show greater than double digit increases by the tenth year after adoption. Part V provides aggregate synthetic controls estimates of the impact of RTC laws, and Part 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 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 got 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 Pennsylania 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 an overall statistical analysis can reveal whether 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 hit with a major civil action, and 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 U.S., 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 failed to defend or to threaten the criminal with a gun 99.2 percent of the time—this in a country with 300 million guns

[5]The injury to Darrell Cabey was so damaging that he remains confined to a wheelchair and functions with the intellect of an 8-year-old, for which he received a judgment of $43 million against Goetz, albeit without satisfaction (Biography.com 2016).

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

in civilian hands (Planty and Truman 2013). Adding 16 million permit holders who often dwell in very low-crime areas will likely 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 tend to attack in ways that prevent defensive measures.

2. Mechanisms of Increasing Crime

Since the statistical evidence suggests that the benign effects of RTC laws are outweighted 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 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.

1) The Pathway from the Law-abiding Citizen

There are clearly cases in which 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). 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 and Moran 2000). An angry young man, with somewhat of a paranoid streak, who hasn't yet been convicted of a crime or adjudicated to be a mental defective, may be encouraged to carry a gun if he resides in an RTC state. That such 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). In the nightmare case for RTC, two Michigan permit-holding drivers pulled over to battle over a tailgating dispute in September of 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, the authors of two studies – one for Arizona and one for the nation as a whole – stated that "the evidence indicates that those with guns in the vehicle are more likely to engage in 'road rage'" (Hemenway, Vriniotis and Miller 2006; Miller et al. 2002).[7] These studies may suggest either that gun carrying emboldens more aggressive behavior or reflects a selection effect for more aggressive individuals.[8] 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.[9]

In general, the critique that the relatively low number of permit revocations proves that permit holders don't commit enough crime to substantially elevate violent criminality is misguided for a variety of reasons. First, only a small fraction of one 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 theoretically 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 four cases where five permit holders were killed, so no permit revocation or criminal prosecution would have occurred regardless of any criminality by the deceased.[10] 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

[7] 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).

[8] A study of Texas concealed carry permit holders found that while their demographic composition is consistent with lower rates of overall crime, when they 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).

[9] 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. 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.

[10] In addition, NRA efforts to pass 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 for researchers to monitor their criminal conduct.

laws, can promote fear and anger, which are emotions that can invite more hostile confrontations leading to violence. Presumably, George Zimmerman would not have hassled Trayvon Martin if Zimmerman had not had a gun, so the gun encouraged a hostile confrontation, regardless of who ultimately becomes violent.

Even well-intentioned interventions by permit holders intending to stop a crime have elevated the crime count when they ended either with the permit holder being killed by the criminal[11] or shooting an innocent party by mistake.[12] Indeed, an FBI study of 160 active shooter incidents found that in almost half (21 of 45) of the situations in which police engaged the shooter to end the threat, law enforcement suffered casualties, totaling nine killed and 28 wounded (Blair and 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. While 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 citizen other than an armed security guard stopped a shooter, and that individual was an active-duty Marine (Id.).

2) The Pathway from those Harboring Criminal Intent

Over the ten 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

[11]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).

In 2014, when armed criminals entered a Las Vegas Walmart and told everyone to get out because "This 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).

[12]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 eleven 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) as he ran out of ammunition. In the year prior to the shooting, police in Anchorage, Alaska, charged Santiago with domestic violence in January 2016, and visited the home five times during the year for various other complaints (KTUU 2017). In November 2016, Santiago visited 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"[13] (Boots 2017). Less than a month later, Santiago flew with his gun to Florida and opened fire in the baggage claim area.[14]

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 weapon, he tried to interfere with the brakes and controls of the moving train. According to the FBI, Wilson had previously joined an "alt-right" neo-Nazi group and travelled to the Unite the Right rally in Charlottesville, Virginia in August 2017, had indicated his interest in "killing black people," was the perpetrator of a road-rage incident in April 2016 in which a man pointed a gun at a black woman for no apparent reason while driving on an interstate highway, and possessed devices and weapons "to engage in criminal offenses against the United States." It sounds as though Wilson was a person with various criminal designs, and one can imagine that having the permit to legally carry weapons facilitated those 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. One egregious example of this was Wisconsin permit holder Darrail Smith, who 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 seems to be an attractive benefit for criminal elements acting in concert (Fernandez, Stack, and Blinder 2015; Luthern 2015).

[13] 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, O'Matz and Arthur 2017).

[14] 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, Lavandera and Perez 2013).

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.[15] Data from a nationally representative web-based survey conducted in April 2015 of 3949 subjects revealed that those who carried guns outside the home had their guns stolen at a rate over one percent per year (Hemenway, Azrael and Miller 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.[16] The outrage over the loss of about 1400 guns in the Fast and Furious program that began in 2009 and the contribution that these guns made to crime puts in perspective the severity of the far greater burdens of guns lost by and stolen from U.S. gun carriers.[17] 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).[18]

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

[15] 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 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 2017*b*).

[16] While the Hemenway, Azrael and Miller 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 gunowners say they carry a gun outside of their home "all of most of the time" (Igielnik and Brown 2017, surveying 3930 U.S. adults, including 1269 gunowners). If one 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.

[17] "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 and 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).

[18] See also Parsons and Vargas (2017). 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).

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 and Williams 2015). According to the National Crime Victimization Survey, in 2013 there were over 660,000 auto thefts from households. The more guns being carried in vehicles by permit holders, the more criminals will be walking around with the guns taken from the car of some permit holder.

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 2017*a*) 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 2017*c*) 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) [19]

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 by-product of RTC laws, as the growing TSA seizures in carry-on luggage attest.[20]

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

[19]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 2017*a*).

[20]See Williams and Waltrip (2004).

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" (Owens 2018).

d. Increasing Violence by Criminals

The argument for RTC laws is often predicated on the view that they will encourage good guys to have guns but will have no impact on the behavior of bad guys. This is highly unlikely to be true. Just as the police tend to shoot quicker when they fear someone may be armed, criminals will tend 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, Ludwig and Samaha 2009). Such responses by criminals will elevate the toll of the crimes that do occur.

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, p. 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 and 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 or 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. A Minnesota police officer who stopped Philando Castile for a broken tail light 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 gun. After a similar experience between an officer and a permit holder, the officer told the gun owner, "Do you realize you almost died tonight?" (Kaste 2016)[21] A policemen trying to give a traffic ticket has far 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 given the greater risks that widespread gun carrying poses to them, whether from permit holders or the criminals who steal their guns. In a famous speech at the University of Chicago Law School in October of 2015, then-FBI Director James Comey blamed the rising violent crime that year on the criticism of overly aggressive policing, which led officers to back away from more involved policing (Donohue 2017*a*). If the greater threat of being shot by an angry gun toter impairs effective policing, a similar concern about increased crime could be raised.

The presence of multiple gun carriers can also complicate police responses to mass shootings and other crimes. For example, according to the police, 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, which according to the police, interfered with a pursuing police officer: "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 and 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 when a permit holder

[21] 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 towards 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 hypervigilant 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)).

mentioned he always carries his gun, bragging that he would be ready to stop any mass shooter. In subsequently showing his Ruger handgun, the man 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, which led to the local hospital and a number of schools being placed on lockdown for 45 minutes until the police finally ascertained that the shooting was accidental (Eltagouri 2017).[22]

Everything that takes up added police time or complicates the job of law enforcement will serve as a tax on police, and therefore one would expect law enforcement to be less effective on the margin, thereby contributing to crime. Indeed, this may in part explain why RTC states tend to increase the size of their police forces (relative to non-adopting states) after RTC laws are passed, as shown in Table 2, below.[23]

B. The No-Controls Model

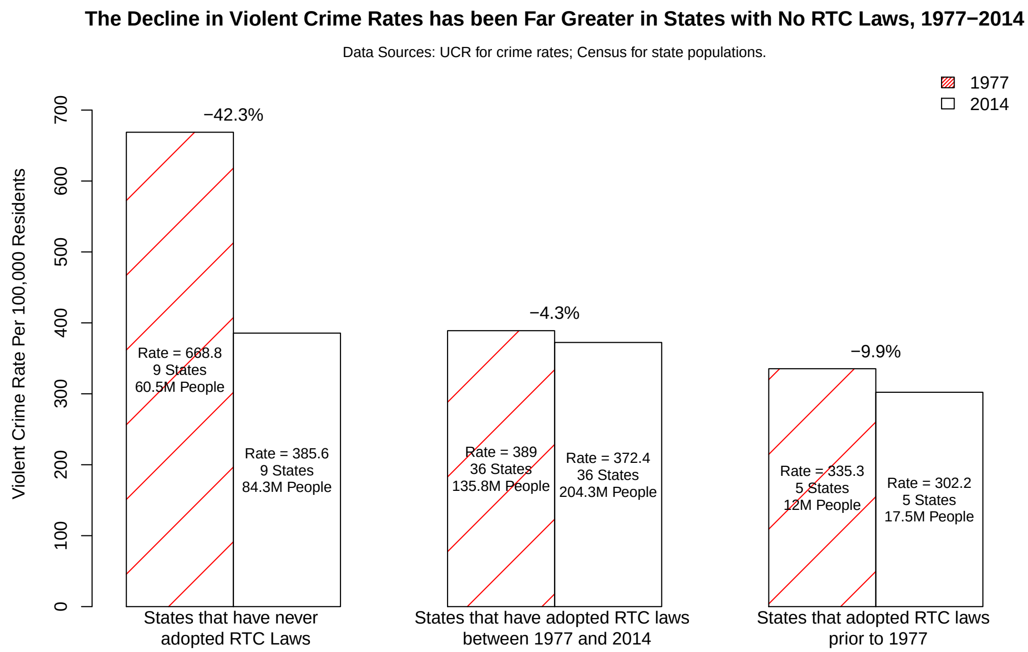
We follow the NRC Report by beginning with the basic facts about how crime has unfolded relative to national trends for states adopting RTC laws. Figure 1 depicts percentage changes in the violent crime rate over our entire data period for three groups of states: those that never adopted RTC laws, those 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.[24]

The NRC Report presented a "no-controls" estimate, which is just the coefficient estimate on the variable indicating the date of adoption of an 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

[22]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). 39-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).

[23]See Adda, McConnell and Rasul (2014), describing how local depenalization of cannabis allowed the police to re-allocate resources, thereby reducing violent crime.

[24]Over the same 1977-2014 period, the states that avoided adopting RTC laws had substantially lower 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.



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.

Figure 1

crime—and 21.2 percent increases in property crime—relative to national crime patterns."

We now estimate this same model using 14 additional years of data (through 2014) and eleven additional adopting states (listed at the bottom of Table 9). Row 1 of Table 1 shows the results of this "no-controls" panel data approach using a dummy model, which just estimates how much on average crime changed after RTC laws were passed (relative to national trends).[25] According to this model, the average post-passage increase in violent crime was 20.2 percent, while the comparable increase in property crime was 19.2 percent. Row 1 also reports the impact of RTC laws on the murder rate (Column 1) and the murder count using a negative binomial model (Column 2), which provide statistically insignificant estimates that RTC law adoption is linked to murder increases of 4-5 percent.

The NRC Report also presented a spline model to estimate how RTC adoption might alter the trend in crime for adopting states, which suggested violent crime and property crime declined relative to trend in the data through 2000, while the trend in murder was unchanged. Row 2 of

[25]The dummy variable model reports the coefficient associated with an RTC variable that is given a value of zero if an RTC law is not in effect in that year, a value of one if an RTC law is in effect that entire year, and a value equal to the portion of the year an RTC law is in effect otherwise. The date of adoption for each RTC state is shown in Appendix Table A1.

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

	Murder Rate (1)	Murder Count (2)	Violent Crime Rate (3)	Property Crime Rate (4)
Dummy Variable Model	3.85 (8.79)	1.05 (0.05)	20.21*** (6.84)	19.19*** (6.07)
Spline Model	−0.28 (0.61)	1.00 (0.00)	0.22 (0.79)	0.14 (0.50)

All models include state- and year-fixed effects, and the OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Column 2 we present Incidence Rate Ratios (IRR) estimated using negative binomial regression, where population is included as a control variable, as STATA does not have a weighting function for nbreg. The null hypothesis for these models is that the IRR equals 1. The crime data is from the Uniform Crime Reports (UCR). * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

Table 1 recomputes this "no-controls" spline model on data through 2014, which eliminates the earlier suggestion that RTC laws were associated with any drop (relative to trend) in violent or property crime, and reaffirms the null finding for murder.[26] In other words, more and better data have darkened the picture of RTC laws from that presented in the NRC model without controls.

While the Table 1 dummy model indicates that RTC states experience a worse post-passage crime pattern, this does not prove that RTC laws increase crime. 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 2 provides panel data evidence on how incarceration and two measures of police employment changed after RTC adoption (relative to non-adopting 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 2 confirms that RTC states did *not* have relatively declining rates of incarceration or total police employees after adopting their RTC laws that might explain their comparatively poor crime performance.

[26]The spline model reports results for a variable which is assigned a value of zero before the RTC law is in effect and a value equal to the portion of the year the RTC law was in effect in the year of adoption. After this year, the value of the this variable is incremented by one annually for states that adopted RTC laws between 1977 and 2014. The spline model also includes a second trend variable representing the number of years that have passed since 1977 for the states adopting RTC laws over the sample period.

Table 2: 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)

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 is from the Uniform Crime Reports (UCR). The source of the incarceration rate is the Bureau of Justice Statistics (BJS). * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

III. A Panel Data Analysis of RTC Laws

A. Estimating Four Models on the Full Data Period 1977-2014

We know from the analysis of the dummy model in the NRC Report and in Table 1 that RTC law adoption is followed by *higher* rates of crime (relative to national trends) and from Table 2 that the poorer crime performance after RTC law adoption occurs 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 are 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

Table 3: Table of Explanatory Variables For Four Panel Data Studies

Explanatory Variables	DAW	BC	LM	MM
Right to Carry Law	x	x	x	x
Lagged Per Capita Incarceration Rate	x			x
Lagged Log of Per Capita Incarceration Rate		x		
Lagged Police Staffing Per 100,000 Residents	x			
Lagged Log of Sworn Police Officers Per Resident Population		x		
Lagged Number of Executions		x		
Poverty Rate	x			x
Unemployment Rate	x	x		x
Per Capita Ethanol Consumption from Beer	x	x		
Percentage of the State Population living in Metropolitan Statistical Areas (MSAs)	x			
Real Per Capita Personal Income	x		x	x
Nominal Per Capita Income (Median Income in BC)		x		
Real Per Capita Income Maintenance			x	x
Real Per Capita Retirement Payments			x	x
Real Per Capita Unemployment Insurance Payments			x	x
Population Density			x	
Lagged Violent or Property Arrest Rate			x	x
State Population			x	x
Crack Index				x
Lagged Dependent Variable				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			
3 Age-Group Percentages (15-19, 20-24, 25-29), and Black Percentage of Population		x		
36 Age-Sex-Race Demographic Variables -all possible combinations of black and white 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	x

Note: The DAW model is advanced in this paper, while the other three models were previously published by the Brennan Center (BC), Lott and Mustard (LM), and Marvell and Moody (MM). See footnote 79 in Appendix B for an explanation of the differences in the retirement payments variable definition between the LM and MM specifications. The crack index variable in the MM specification is available only for 1980-2000.

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). The full set of explanatory variables is listed in Table 3, along with the regression models used in three other studies that have estimated the impact of RTC laws on crime.[27]

Mathematically, the dummy model takes the following form:

$$ln(crime\,rate_{it}) = \beta X_{it} + \gamma RTC_{it} + \alpha_t + \delta_i + \varepsilon_{it} \tag{1}$$

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 varying arrays of covariates 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 spline model allows RTC laws to change the trend in crime, as reflected in the following equation:

$$ln(crime\,rate_{it}) = \beta X_{it} + \gamma AFTER_{it} * CHG_i + \lambda TREND_t * CHG_i + \alpha_t + \delta_i + \varepsilon_{it} \tag{2}$$

The coefficient of interest, γ, now measures the change in trend for each post-passage year in RTC adopting states relative to those that do not adopt RTC. AFTER measures the number of years after RTC adoption. CHG (change) is a binary variable that is equal to one if the state adopts an RTC law during our analysis period. TREND is a time trend that measures the number of years since the beginning of the analysis period (1979 for the DAW panel data model).[28] Thus, the interaction of AFTER and TREND with CHG ensures that pre-1977 adopters such as Vermont do not contribute to our Spline effect. As in the dummy model, α, β and δ capture the effects of year fixed effects, covariates and state fixed effects, while ε_{it} is the error term.

The DAW panel data model in Table 4 (run on data from 1979-2014) is consistent with the same basic pattern observed in Table 1:[29] RTC laws on average increased violent crime by 9.0 percent and property crime by 6.5 percent in the years following adoption according to the dummy model, but again showed no statistically significant effect in the spline model.[30] As in the no-

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

[28]t starts with 1978 for the BC model, 1977 for LM and 1979 for MM.

[29]The complete set of estimates for all explanatory variables (except the demographic variables) for the DAW, BC, LM, and MM dummy and spline models is shown in appendix Table A2.

[30]Defensive uses of guns are more likely for violent crimes because the victim will clearly be present. For property

controls model, the estimated effect of RTC laws in Table 4 on the murder rate is very imprecisely estimated and not statistically significant.

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 non-treatment states. But as we noted above,[31] 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). 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.[32]

Table 4: Panel Data Estimates Suggesting that RTC Laws increase Violent and Property Crime: State and Year Fixed Effects, DAW Regressors, 1979-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	0.21 (5.33)	1.05 (0.05)	9.02*** (2.90)	6.49** (2.74)
Spline Model	−0.33 (0.53)	1.00 (0.00)	0.01 (0.64)	0.11 (0.39)

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 Column 2 we present Incidence Rate Ratios (IRR) estimated using negative binomial regression, where population is included as a control variable, as STATA does not have a weighting function for nbreg. The null hypothesis is that the IRR equals 1. The crime data is from the Uniform Crime Reports (UCR). 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. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

2. The BC Panel Data Model

Table 3 lists the variables used in the Brennan Center (BC) crime regression model, which differ in a few respects from the DAW model (although to a lesser degree than the LM and MM models).

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 4 suggests.

[31] See text at footnotes 16-18.

[32] 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, although since many of these were assault rifles, they were probably not guns carried by American RTC permit holders (Paraguassu and Brito 2018).

The BC model (Roeder et al. 2015) controls for both incarceration and police rates (as in DAW), but the BC model takes the log of both these rates. The BC model alone controls for the number of executions, and unlike DAW does not control for either the state poverty rate or the percentage of the state population living in a Metropolitan Statistical Area. Moreover, while DAW includes six demographic variables, BC uses three age groupings over the ages 15-29, and simply controls for the black percentage of the state population.

The results of running the BC model over the period from 1978-2014 are presented in Table 5, Panel A. With the exception that the BC dummy variable model estimate of the increase in violent crime is somewhat higher than that for DAW (10.90 percent increase versus 9.02 percent increase), the DAW and BC model estimates are almost identical in suggesting higher rates of violent and property crime (the dummy models) but no impact in the spline models. If we replace the four BC demographic variables with the six DAW demographic variables (Table 5, Panel B), the size of the estimated increase in violent crime (in the dummy models) is essentially the same as the DAW violent crime results in Table 4 (and only modestly lower for property crime).

Table 5: Panel Data Estimates Suggesting that RTC Laws increase Violent and Property Crime: State and Year Fixed Effects, BC Regressors, 1978-2014

Panel A: BC Regressors Including 4 Demographic Variables	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	3.38 (5.71)	1.05 (0.05)	10.90*** (3.66)	6.75** (3.28)
Spline Model	−0.52 (0.52)	1.00 (0.00)	0.16 (0.66)	0.09 (0.35)

Panel B: BC Regressors with 6 DAW Demographic Variables	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	1.84 (5.51)	1.06 (0.05)	8.87*** (3.28)	5.47* (2.87)
Spline Model	−0.36 (0.49)	1.00 (0.00)	0.21 (0.60)	0.13 (0.35)

All models include year and state fixed effects, and the OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Column 2 we present Incidence Rate Ratios (IRR) estimated using negative binomial regression, where population is included as a control variable, as STATA does not have a weighting function for nbreg. The null hypothesis is that the IRR equals 1. The crime data is from the Uniform Crime Reports (UCR). Four demographic variables (percent black, percent aged 15-19, percent aged 20-24, and percent aged 25-29) are included in the Panel A regressions. The 6 DAW demographic variables are used in the Panel B regressions. Other controls include log of the lagged incarceration rate, lagged police employment per resident population, the unemployment rate, nominal per capita income, lagged number of executions, gallons of beer consumed per capita. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

3. The LM Panel Data Model

Table 3's recitation of the explanatory variables contained in the Lott and Mustard (LM) panel data model reveals two obvious omissions: 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 above in Table 2, both of these 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, fn. 8). Without more, then, we have reason to believe that the LM model is mis-specified, but in addition to the obvious omitted variable bias, we have discussed an array of other infirmities with the LM model in Aneja, Donohue, and Zhang (2014), including their reliance on flawed pseudo-arrest rates, and highly collinear demographic variables.

As noted in Aneja, Donohue, and Zhang (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 vastly *increase* crime (the coefficient is 219!), but their female counterparts have an enormously dampening effect on crime (with a coefficient of -258!). Both of those highly implausible estimates (not shown in Table A2) 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.[33]

Table 6, 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 4), while the estimated impact on murders was too imprecise to be informative. The LM model flips these predictions by showing strong estimates of increased murder (in the spline model) and imprecise and not statistically significant estimates for

[33] Aneja, Donohue, and Zhang (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 six to seven for the RTC variable in both the LM dummy and spline models 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). Indeed, the degree of multicollinearity for the individual demographics of the black-male categories are astonishingly high with 36 demographic controls—with VIFs in the neighborhood of 14,000! This analysis makes us wary of estimates of the impact of RTC laws that employ the Lott-Mustard set of 36 demographic controls (as does the MM model).

Table 6: 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	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	−4.60 (3.43)	1.03 (0.03)	−1.38 (3.16)	−0.34 (1.71)
Spline Model	0.65** (0.33)	1.01** (0.00)	0.41 (0.47)	0.28 (0.28)

Panel B: LM Regressors with 6 DAW Demographic Variables and Adding Controls for Incarceration and Police

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	3.61 (5.69)	1.06 (0.05)	10.05** (4.54)	8.10** (3.63)
Spline Model	0.30 (0.43)	1.00 (0.00)	0.50 (0.57)	0.50 (0.34)

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 6 demographic variables are included and controls are added for incarceration and police. For both 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 < .1, ** p < .05, *** p < .01. All figures reported in percentage terms.

violent and property crime. We can almost perfectly restore the DAW Table 4 findings, however, by simply following the typical pattern of crime regressions by limiting the inclusion of 36 highly collinear demographic variables and including measures for police and incarceration. These results appear in Panel B of Table 6, and this modified LM dummy variable model suggests that RTC laws increase violent and property crime, mimicking the DAW and BC dummy variable model estimates.

In summary, the LM model that had originally been employed using data through 1992 to argue that RTC laws *reduce* crime, no longer shows any statistically significant evidence of crime reduction. Indeed, using more complete data, the LM spline model (Panel A of Table 6) suggests that RTC laws *increase* the murder rate by 6-7 percent (and the murder count by ten percent) after ten years, which are the only statistically significant results in Panel A. Those who are skeptical of these results because the LM specification is plagued by omitted variable bias, flawed pseudo-arrest rates, too many highly collinear demographic variables, and other problems, might prefer the estimates in Panel B, which simply limit the LM demographic variables from 36 to 6, and add the incarceration and police controls. These changes once again restore the Table 4 DAW and Table 5 BC dummy variable model result that RTC laws *increase* both violent and property crime.

4. The MM Panel Data Model

Table 3 reveals that the Moody and Marvell (MM) model improves on the LM model in that it includes the key incarceration variable, but MM also omit the critical control for police found in both the DAW and BC specifications. The MM model also contains the problematic pseudo-arrest rates and over-saturated and highly collinear demographic variables that LM employ.[34] MM introduce another problematic control variable by including a lagged dependent variable that will almost certainly purge some of the effect of the RTC law on crime. Panel A of Table 7 estimates the MM model for the period 1979-2014.[35]

Interestingly, the MM model also provides evidence that RTC laws *increase* violent crime, but now this effect appears in the spline model (rather than in the dummy model, as seen in the DAW and BC regressions). The only other statistically significant estimate is for the murder rate in the spline model, which suggests that the murder rate would be roughly four percent higher ten years after RTC adoption. This finding is roughly similar to the Table 6, Panel A finding of increased murder in the LM spline model.

Panel B of Table 7 mimics our previous critique of the LM model by including a measure of police and using more appropriate demographic controls. These modifications once again revive a dummy variable model estimate of increased violent crime, although as one would expect given the problematic lagged dependent variable, the estimate is far smaller than that of the DAW and BC models.

[34]While our Table 7 MM panel data specification follows Moody and Marvell (2008) in including lagged values of the dependent variable as a regressor, no analogous variable is explicitly included below in our synthetic control analysis featuring the Moody-Marvell predictor variables. Since all lagged values of the dependent variable are already included as predictors in our synthetic controls analysis, including the lagged dependent variable would be redundant.

[35]MM use the crack index of Fryer et al. (2013), but this comes at the price of limiting the available data years for the MM panel data analysis to the years 1980-2000. We estimated the MM model on the data period from 1980-2000 with and without the crack cocaine variable, which yielded virtually identical results. Therefore, in Table 7, we exclude the crack cocaine variable, which allows us to use 15 years of additional data to estimate the effect of RTC laws (from 1979, as well as 2001 through 2014).

Table 7: Panel Data Estimates of the Impact of RTC Laws: State and Year Fixed Effects, Using Actual and Modified MM Regressors without Crack Cocaine, 1979-2014.

Panel A: MM Regressors Without Crack Cocaine and Including 36 Demographic Variables

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	−1.85 (1.84)	1.02 (0.03)	0.69 (0.77)	0.48 (0.69)
Spline Model	0.38** (0.16)	1.00 (0.00)	0.17** (0.08)	0.10 (0.07)

Panel B: MM Regressors Without Crack Cocaine, With 6 DAW Demographic Variables, and Adding a Control for Police

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	1.21 (1.76)	1.03 (0.03)	1.50*** (0.53)	0.52 (0.53)
Spline Model	0.24 (0.17)	1.00 (0.00)	0.14 (0.09)	0.05 (0.05)

All models include year and state fixed effects, and the OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Column 2 we present Incidence Rate Ratios (IRR) estimated using negative binomial regression, where population is included as a control variable, as STATA does not have a weighting function for nbreg. The null hypothesis is that the IRR equals 1. 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 6 demographic variables are included and a control is added for police. For both panels, other controls include the lagged dependent variable, the previous year's violent or property crime arrest rate (depending on the crime category of the dependent variable), state population, the lagged incarceration rate, the poverty rate, the unemployment rate, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real per capita retirement payments. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

5. The Lessons from the Panel Data Studies Estimated Over the Full Data Range

All four models shown in Table 4 through Table 7 showed evidence that RTC laws increased murder and/or overall violent crime. The dummy models of DAW and BC showed almost identical increases in violent crime of 9-11 percent and property crime of 6-7 percent. The LM spline model (Table 6, Panel A)—the heart of the original More Guns, Less Crime hypothesis—estimates a statistically significant *increase* in murder will follow RTC adoption. A similar finding of increased murder emerges for the MM spline model (Table 7, Panel A), which also predicts an increase in violent crime. If we look at the modified versions of the LM and MM models in their respective Panel B's, the LM dummy model (Table 6) almost perfectly replicates the increased violent and property crime estimates of DAW and BC, while the MM model (Table 7) continues to show a small but statistically significant increase in the violent crime rate.

The strongest result to emerge from the seven panels across the four sets of panel data specifications in Tables 4-7 is that six of these seven panels show statistically significant evidence that RTC laws increase violent crime. The only exception (LM Panel A) shows statistically significant evidence of *increases* in murder. In other words, all seven panels support the conclusion that RTC laws *increase* overall violent crime and/or murder. Across the 56 estimated effects in the seven panels, not one showed any evidence of a *decrease* in crime at even the .10 level of significance.

6. Varying RTC Adoption Dates to Probe Model Validity

While the DAW and BC dummy models provide similar estimates of the increase in violent crime from RTC laws, the dummy models of LM and MM do not provide similar evidence. One test of which model is best capturing the impact of RTC laws as opposed to merely capturing some other crime phenomenon correlated with RTC laws is to vary the adoption date of RTC laws to see whether the true date of adoption or some earlier or later date will generate the smallest p-value and lowest standard error of the estimated coefficient on the RTC dummy.

To implement this test we allowed the start date for each law to vary uniformly from ten years before actual adoption to ten years after adoption. For example, the actual passage of RTC in Texas took place in 1996. Our test will run our dummy model on the assumption that Texas (and every other RTC adopter) adopted its law ten years prior to passage (so 1986 for Texas), thereby incorporating ten years of pre-passage effects into our estimate of the impact of RTC laws on violent crime. This is repeated 20 times through ten years after adoption (which would be 2006 for Texas).

Figure 2 depicts the 21 estimated coefficients on the RTC dummy for the DAW model (based on the assumed starting dates ten years prior to adoption up to ten years after adoption), with the shaded area showing the 95 percent confidence intervals for each. The numbers on the middle line present the p-values for each of the 21 estimates, and as Figure 2 illustrates and Table 8 confirms, the DAW model has the smallest p-value and standard error in the actual year of RTC adoption. In other words, the DAW dummy model reveals that something changes at exactly the time of RTC adoption that leads to increased violent crime.

We ran this same exercise for the three other specifications – BC, LM, and MM – as well as for the no-controls model. Table 8 shows that only the DAW model minimized both the p-value and standard error in the actual year of adoption. The model with no controls shows a similar pattern, although the standard error is minimized for the year prior to adoption. The BC model also comes close, with its lowest p-value one year after adoption and the lowest standard error in the year of adoption (and the p-value is barely higher in the adoption year—0.0029 v. 0.0026).

The results of this test on the two models that do not show a RTC effect on violent crime are quite different, as illustrated in Table 8. For the LM and MM models, the p-values and standard errors are minimized far from the true adoption dates, which suggests that the RTC dummy in these models is picking up some other influence on crime that started well before (or after) RTC adoption. Specifically, the lowest p-values and standard errors for the LM model come five years *before* actual adoption. For the MM model, Table 8 shows that a model that turns on the RTC dummy nine years *before* adoption would generate the lowest p-value of .02, predicting an ostensibly statistically significant decline in violent crime, starting almost a decade before the actual RTC law went into effect. The MM model also generated its lowest standard error for the RTC estimate when simulating RTC adoption to be ten years *after* actual adoption.

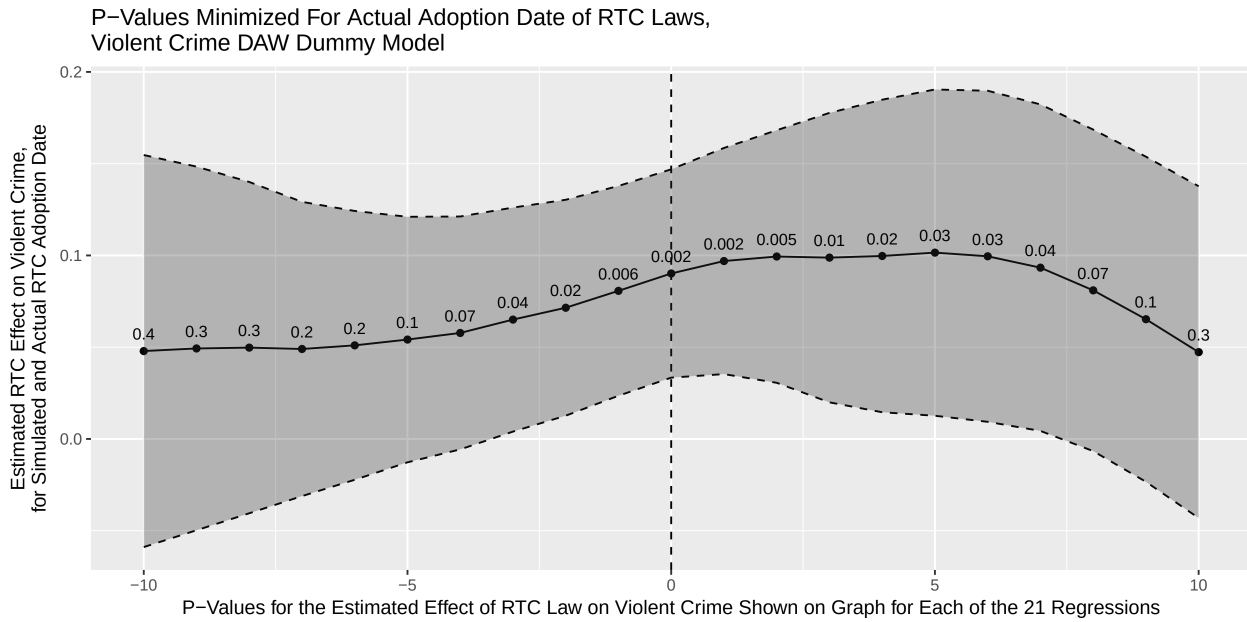


Figure 2

Table 8: Year which minimizes p-value or standard error when varying RTC Adoption Dates

Model	P-Value Minimum				Standard Error Minimum			
	RTC Year	Coefficient	P-Value	S.E.	RTC Year	Coefficient	P-Value	S.E.
DAW	0	0.090	0.002	0.029	0	0.090	0.002	0.029
BC	1	0.114	0.003	0.038	0	0.109	0.003	0.037
LM	-5	-0.040	0.131	0.026	-5	-0.040	0.131	0.026
MM	-9	-0.015	0.018	0.006	10	0.003	0.586	0.005
No-Controls	0	0.202	0.003	0.068	-1	0.198	0.004	0.068

Note: The table shows that the DAW dummy model estimates that RTC laws lead to a nine percent increase in violent crime (with a p-value of .002) when the dummy starts in the actual year of adoption (equals “year zero”). Year zero is also the year that minimizes the p-value and standard error for the DAW dummy coefficient.

7. Estimating the Impact of RTC Laws on Violent Crime with the LASSO

The DAW and BC model estimates on our full data range from the late 1970s through 2014 suggested that RTC laws increase violent crime by 9.0 and 10.9 percent, respectively, with both estimates significant at the .01 level (See Tables 4 and 5.A). Conversely, the comparable dummy variable model estimates for the LM and MM models were close to zero and not statistically significant: Table 6.A showed an estimate of -1.4 with a standard error of 3.2, and Table 7.A showed an estimate of 0.7 with a standard error of 0.8. Both of these LM and MM estimates became positive and statistically significant with plausible specification changes (Tables 6.B and 7.B), which suggests that the LASSO may provide useful information on the preferred selection of explanatory variables in these panel data regressions.

If we simply combine all of the explanatory variables that were used in our four basic models, we end up with a total of 62 variables, which includes the 13 variables from DAW, eight more from BC (two other BC variables were identical to DAW variables and thus already included), and all 41 variables from the LM model (41 out of 43 MM variables were thus already included, and we excluded the lagged dependent variable and the crack index from MM in our LASSO analysis so that we could include data through 2014). The LASSO will generate an estimated effect of RTC laws on violent crime that begins with all 62 explanatory variables and will incrementally penalize included variables until the RTC law is expelled from the regression. We never penalize, and thus always include, state and year fixed effects in the LASSO analysis.

It turns out that, as the penalty for included variables is increased, the last of the 62 variables to be expelled from the violent crime regression is the RTC variable, which establishes it as the single most important variable in predicting violent crime rates over the full data period. Figure 3 depicts the result of this exercise until the penalty λ at which only the RTC estimate and fixed effects remain ($\lambda = 0.0095$). One can see that the estimated impact of RTC laws is always positive, ranging from 1.7 percent with a penalty sufficiently small that all 62 explanatory variables (plus state and year fixed effects) are included in the panel data model, rising to a peak estimated effect of 8.4 percent (at $\lambda = 0.00385$) before falling to 4.8 percent at the first value of λ where only the RTC variable and the fixed effects remain. Note that the low levels of λ (where 62 explanatory variables in addition to the state and year fixed effects are included) will almost certainly overfit the data during the crack crime boom, which in part explains the smaller estimated impact on violent crime on the left side of the figure. Across the entire span of λ values shown in Figure 3, the mean estimated RTC effect is 6.6 percent.

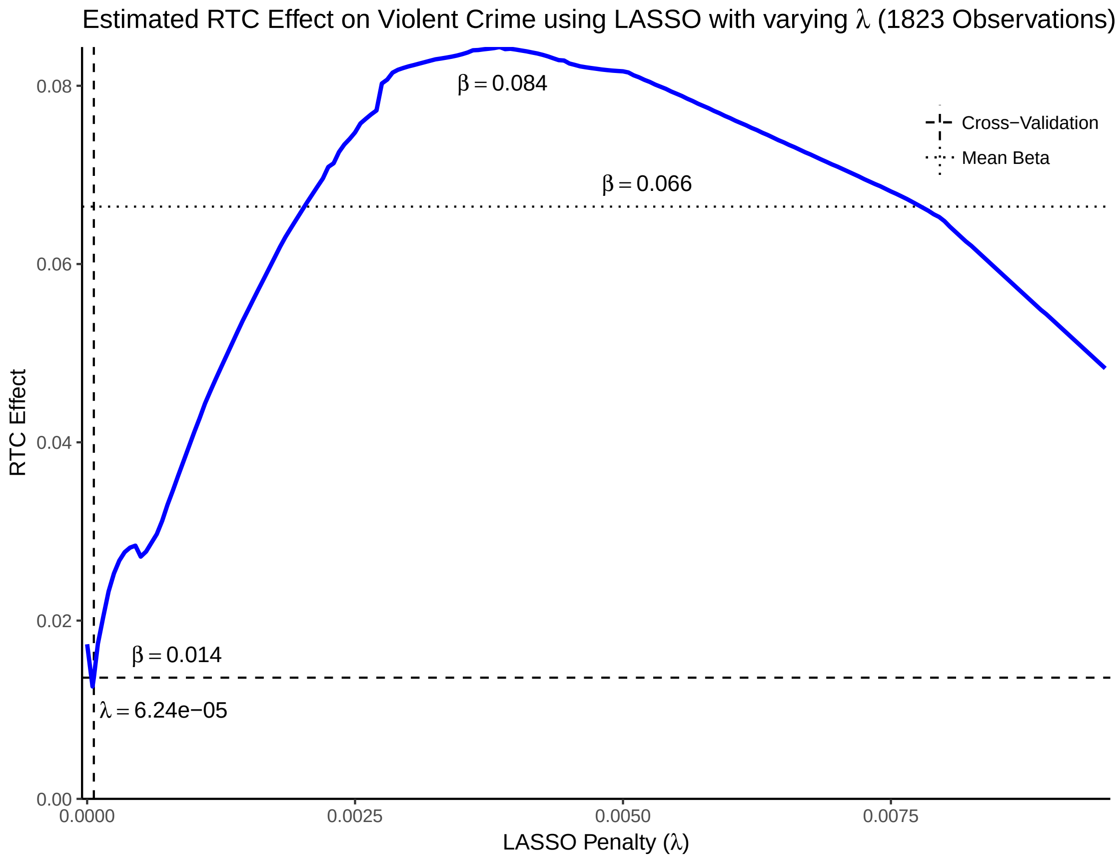


Figure 3

B. Panel Data Models Estimated for the Post-Crack Period

Our previous discussion has focused on panel data estimates of the impact of RTC laws on crime over the full period from the late 1970s through 2014. Zimmerman (2014) examines the impact of various crime prevention measures on crime using a state panel data set from 1999-2010. He finds that RTC laws *increased* murder by 15.5 percent for the eight states that adopted RTC laws over the period he analyzed. The advantage of using this data period to explore the impact of RTC laws is that it largely avoids the problem of omitted variable bias owing to the crack phenomenon, since the crack effect had largely subsided by 1999. The disadvantage is that one can only gain estimates based on the eight states that adopted RTC laws over that twelve-year spell.[36] Zimmerman describes his finding as follows: “The shall-issue coefficient takes a positive sign in all regressions save for the rape model and is statistically significant in the murder, robbery, assault, burglary, and larceny models. These latter findings may imply that the passage of shall-issue laws

[36]The relatively short time span of the Zimmerman analysis makes the assumption of state fixed effects more plausible but it also limits the amount of pre-adoption data for an early adopter such as Michigan (2001) and the amount of post-adoption data for the late adopters Nebraska and Kansas (both in 2007).

increases the propensity for crime, as some recent research (e.g., Aneja, Donohue, & Zhang, 2012) has suggested" (71).[37]

In Table 9, we show the results of all four basic models that we discussed above—DAW, BC, LM, and MM—when run over the period 2000-2014 for eleven adopting states.[38] The DAW model mimics the Zimmerman finding of a large jump in murder, rising at a rate of about one percent each year the RTC law is in effect. The BC dummy model provides evidence at the .10 level of statistical significance that RTC laws increase the murder rate (and in the spline model the murder count), and even stronger evidence that RTC laws lead to an eight percent increase in the overall violent crime rate. The results for this shortened period using the LM and MM models are never statistically significant at the .05 level.

A recent paper by Siegel et al. (2017) uses a negative binomial model for data from 1991 to 2015 to estimate the impact of RTC laws on five homicide measures based on Centers for Disease Control and Supplemental Homicide Report data, rather than the UCR crime data used throughout this paper. Controlling for year and state fixed effects and an array of time-varying, state-level factors, Siegel et al. conclude that RTC laws increase murders, particularly firearm and handgun murders, but seem to have virtually no effect on non-gun murders or long gun murders. Donohue (2017*b*) uses the same data used by Siegel et al., but limits the analysis to the 2000-2014 post-crack period. While Siegel et al. using their own model on the 1991-2015 CDC data found that overall homicides rose by 6.5 percent, firearm homicides rose by 8.6 percent, and handgun homicides rose by 10.6 percent, Donohue (2017*b*) running the DAW model on the 2000-2014 period generated comparable estimates of 6.0 percent, 9.5 percent, and 15.8 percent for overall, firearm, and handgun homicides, respectively (although the 6.0 estimate for overall homicides lost statistical significance at the .05 level).

[37] Aneja, Donohue and Zhang (2014) also ran the ADZ model over the same 1999-2010 period that Zimmerman employs, which generated an estimate that murder rates rose about 1.5 percentage points each year that an RTC law was in effect.

[38] We started this time period in 2000 because the sharp crime decreases of the 1990s ended by then and crime starting in 2000 was more stable for the remainder of our data period than it had previously been.

Table 9: Panel Data Estimates of the Impact of RTC Laws Using DAW, BC, LM, and MM specifications, 2000 - 2014.

Panel A: Panel Data Estimates, State and Year Fixed Effects, DAW Regressors, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	5.58 (3.58)	1.02 (0.04)	5.00 (3.55)	-1.50 (2.29)
Spline Model	1.08^{*} (0.58)	1.01^{**} (0.01)	0.54 (1.12)	0.41 (0.43)

Panel B: Panel Data Estimates, State and Year Fixed Effects, Brennan Center Regressors, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	7.10^{*} (4.09)	1.03 (0.04)	7.93^{**} (3.60)	-1.88 (2.48)
Spline Model	0.89 (0.66)	1.01^{*} (0.01)	0.58 (1.30)	0.36 (0.46)

Panel C: Panel Data Estimates With 36 Collinear Demographic Variables, State and Year Fixed Effects, LM Regressors, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	2.74 (3.64)	1.03 (0.04)	-0.87 (3.35)	-3.06 (1.94)
Spline Model	0.85 (0.78)	1.01 (0.01)	-0.06 (0.73)	-0.32 (0.51)

Panel D: Panel Data Estimates With 36 Collinear Demographic Variables, State and Year Fixed Effects, MM Regressors without Crack Cocaine, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	2.44 (3.33)	1.02 (0.04)	0.14 (1.49)	-1.44^{*} (0.86)
Spline Model	0.68 (0.81)	1.01 (0.01)	0.30 (0.33)	0.17 (0.18)

All models include year and state fixed effects, and the OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Column 2 we present IRRs estimated using negative binomial regression, where population is included as a control variable. Panels A, B, C, and D replicate the standard specifications on 2000 - 2014 data. To allow for estimation in this period for the MM model, the crack index variable is dropped. The following 11 states adopted RTC Laws during the period of consideration: CO (2003), IA (2011), IL (2014), KS (2007), MI (2001), MN (2003), MO (2004), NE (2007), NM (2004), OH (2004), and WI (2011).
* $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

C. 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 has eliminated any statistically significant evidence of benign effects on crime from the adoption of RTC laws. Considerable evidence that RTC laws increase murder and/or overall violent crime has now been amassed and a LASSO analysis buttresses the view that RTC laws increase violent crime. In addition, three of five models estimated on post-crack-era data (Zimmerman, DAW, and BC) provide further support for this conclusion, as does the recent work by Donohue (2017*b*) and Siegel et al. (2017) concluding that RTC laws increase firearm and handgun homicide. Pending 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.

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, and we followed this advice by implementing a LASSO analysis. 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, Navarro, and Rivers (2016) 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” (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 post-passage years, we cannot draw conclusions beyond this but note that their finding that violent crime increases by over two percent per year owing to RTC laws is a substantial crime increase. Moreover, the authors note that “For our estimates, the effect on crime of introducing guns continues to grow over time” (50).

One member of the NRC panel (Joel Horowitz) doubted whether a panel data model could ever convincingly establish the causal impact of RTC laws: “the problems posed by high-dimensional estimation, misspecified models, and lack of knowledge of the correct set of explanatory variables seem insurmountable with observational data” (NRC 2005: 308). 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 paper will use this synthetic controls approach, which has been deemed "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey and Imbens 2017).

IV. Estimating the Impact of RTC Laws Using Synthetic Controls

The synthetic controls 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.[39] A number of papers have used the synthetic control technique to evaluate various influences on crime. Rudolph et al. (2015) construct a synthetic control for the state of Connecticut yielding evidence that the state's firearm homicide rate (but not its non-firearm homicide rate) fell appreciably after the implementation of a permit-to-purchase handgun law. Munasib and Guettabi (2013) use this methodology to examine the effect of Florida's "Stand Your Ground" law, concluding that this law was associated with an increase in overall gun deaths. Similarly, Cunningham and Shah (2017) study the effect of Rhode Island's unexpected decriminalization of indoor prostitution on the state's rape rate (among other outcome variables); Lofstrom and Raphael (2013) estimate the effect of California's public safety realignment on crime rates; and Pinotti (2013) examines the consequences of an influx of organized crime into two Italian provinces in the late 1970s.

While these papers focus on a single treatment in a single geographic region, we look at 33 RTC adoptions 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 counter-factual for the impact of RTC laws because it had a similar pattern of crime to 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.[40]

[39] The synthetic control methodology has been deployed in a wide variety of fields, including health economics (Engelen, Nonnemaker, and Shive 2011), immigration economics (Bohn, Lofstrom, and Raphael 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).

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

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 pre-intervention 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.[41] Two conditions are placed on these weights: they must be non-negative and they must sum to one. In general, the matching process underlying the synthetic control technique uses pre-treatment values of both the outcome variable of interest (in our case, some measure of crime) and other predictors believed to influence this outcome variable.[42] For the reasons set forth in Appendix I, we use every lag of the dependent variable as predictors in the DAW, BC, LM, and MM specifications.[43] 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 pre-treatment 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.

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

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

We use the *nested* option in Stata to generate the relevant weights. This option uses standard optimization techniques to find the weights associated with each predictor that minimize the pre-treatment RMSPE of the resulting synthetic control. The Stata module that we use also can generate the relevant weights using a less computationally intensive regression-based technique, but Hainmueller, Abadie and Diamond (2015) indicate that the nested option is the better-performing technique, with the even slower "allopt" option being most useful as a "robustness check" of convergence. We have only produced nested placebos for the DAW and BC sets of covariates due to the extensive computing power required for LM and MM. This is not a major concern as DAW and BC nested and non-nested placebos produced similar results. Tables employing nested placebos include a note that they use "the *optimization technique* described in our main text" (e.g. Table 10) and non-nested placebos are noted as "using the *regression methodology* described in the main text" (e.g. Table 15).

[43] We considered using one lag (either the final pre-treatment year's crime rate or the average crime rate over the entire pre-treatment period), three lags (from the beginning, middle, and end of the pre-treatment period), no lags and yearly lags as predictors. We eventually chose to use yearly pre-treatment crime rates since that option minimized the average coefficient of variation of the RMSPE during the validation period, as shown in Appendix I. Table A27 shows, importantly, that the estimated treatment effect from the passage of an RTC law remains similar for violent crime regardless of which of the five lag choices is used, with the RTC laws estimated to increase violent crime ranging from 11.8 percent (one average lag) to 15.4 percent (three lags).

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 consists of all nine states with no RTC legislation as of the year 2014, as well as states that pass RTC laws at least ten 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 ten years post-passage,[44] 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 pre-treatment and post-treatment periods. The rationale behind this methodology is that a close fit in the pre-passage time series of crime between the treatment state and the synthetic control generates greater confidence in the accuracy of the constructed counterfactual. Computing the post-treatment difference between the dependent variables of the treatment state and the synthetic control unit provides the synthetic controls estimate of the treatment effect attributable to RTC adoption in that state.

1. Synthetic Controls Estimates of Violent Crime in Four States

Figure 4 shows the synthetic controls graph for violent crime in Texas over the period from 1977 through 2006 (ten 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.

[44] Our choice of ten years in this context is informed by the tradeoffs associated with using a different time frame. Table 16 indicates that for at least the initial decade, the number of permits grows substantially in RTC states, so we would expect the impact of RTC laws to grow over this period, as Table 17 corroborates. Tables 10-15 indicate that the increase in violent crime due to RTC laws is statistically significant at the .01 level for all years after 7 years post-adoption.

Using a longer post-passage period would enable us to estimate the impact of RTC laws for states in which there were more than ten years of post-passage data, but it could reduce the accuracy of our estimates of the effect of the treatment in earlier periods, owing to the exclusion of additional control states from consideration in the composition of our synthetic control. See also the discussion of permit data in Section V(E)2.

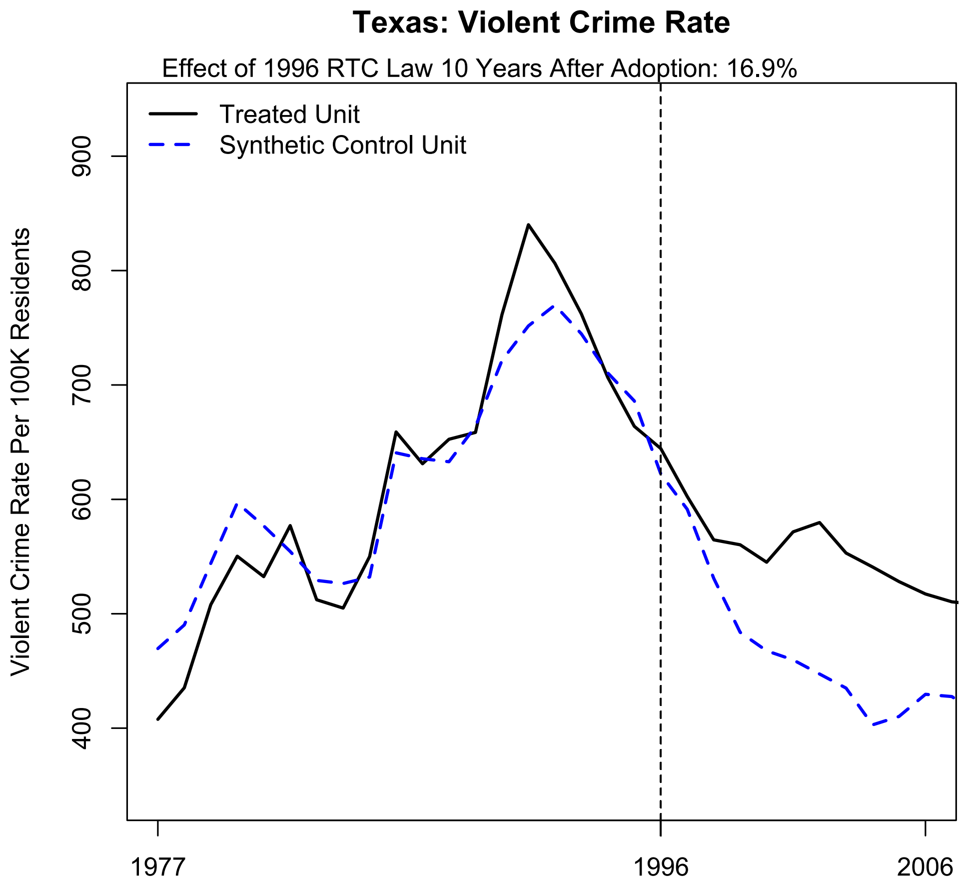
One of the advantages of the synthetic controls methodology is that one can assess how well the synthetic control (call it "synthetic Texas," which is identified in Figure 4 by the dashed line) matches the pre-RTC-passage pattern of violent crime to see whether the methodology is likely to generate a good fit in the ten years of post-passage 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 4, 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 post-passage decade after 1996, the crime drop is substantially greater in synthetic Texas, which had no RTC law over that period, than in actual Texas, which did. As Figure 4 notes, ten 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.[45]

Figure 4 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 controls methodology is that it provides a highly transparent estimate of that counterfactual. Thus, when Lott (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' 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 ten 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 controls approach is one advantage of this methodology over the panel data models that we considered above. Figure 4 makes clear what Texas is being compared to, and we can reflect on whether this match is plausible and whether anything other than

[45] Texas' violent crime rate ten 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 4). See footnote 56 for further discussion of this calculation. Figure 4 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 tenth year TEP, which equals 16.86 percent.



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)

Figure 4

RTC laws changed in these three states during the post-passage decade that might compromise the validity of the synthetic controls estimate of the impact of RTC laws.

Specifically, if one agreed with some of John Lott's written work that the death penalty is a powerful deterrent one might be concerned that Texas's far greater use of the death penalty during the post-passage period than in the states comprising synthetic Texas might bias downward the prediction that RTC laws increased crime by 16.9 percent in Texas.[46] Conversely, the greater increases in incarceration and police in synthetic Texas would lead to the opposite bias, albeit not plausibly enough to reduce the estimated violent crime increase below ten percent.[47]

[46]Texas executed 275 convicts during the post-passage decade while California executed 11, Nebraska 2, and Wisconsin executed no one (Death Penalty Information Center 2015).

[47]Because Texas had an enormous jump in its incarceration rate from 1992-1996, the growth in the Texas incarceration rate from 1996-2006 was only 7.6 percent, while for "synthetic Texas" the growth rate was 21.7 percent.

Figure 5 shows our synthetic controls estimate for Pennsylvania, which adopted an RTC law in 1989 that did not extend to Philadelphia until a subsequent law went into effect on October 11, 1995. In this case, synthetic Pennsylvania is comprised of eight states and the pre-passage 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.[48] The synthetic controls method estimates that RTC laws in Pennsylvania increased its violent crime rate by 24.4 percent after ten years.

Figures 6 and 7 show the comparable synthetic controls matches for Ohio and Mississippi. Again both states show overall good pre-passage fit between the violent crime rates of the treatment state and the synthetic control, although the Ohio estimate is potentially less reliable because of the discrepancy of 11.6 percent between the actual and synthetic elements in the 2004 year of RTC adoption.[49] In contrast, the discrepancy between the actual violent crime rate and the synthetic control estimate in the year of adoption for Mississippi is only 1.1 percent. The estimate that adopting a RTC law increased violent crime in Mississippi by 34.1 percent is therefore arguably more robust than the estimate that Ohio's RTC law led to a slight dip in violent crime of 0.8 percent.[50]

2. State-Specific Estimates Across all RTC States

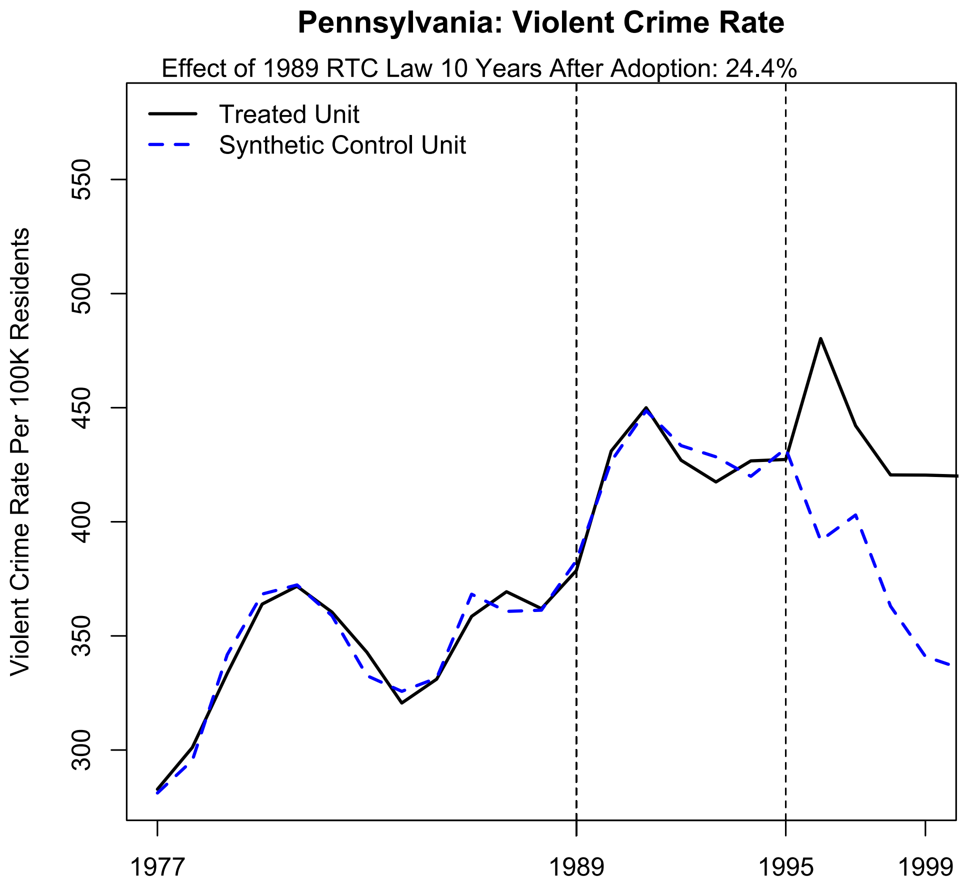
Because we are projecting the violent crime experience of the synthetic control over a ten-year period, there will undoubtedly be a deviation from the "true" counterfactual and our estimated counterfactual. If we were only estimating the impact of a legal change for a single state, we

Individually, the growths for the three synthetic control states were 6.8 percent (CA), 69.0 percent (WI), 26.6 percent (NE). The growth rate in the Texas police employment rate over the decade was -0.6 percent, while for "synthetic Texas" the growth rate was 7.8 percent. Individually, the growths for the three synthetic control states were 9.0 percent (CA), 5.5 percent (WI), 8.2 percent (NE). Using plausible crime elasticities for police and incarceration suggests that accounting for these two factors could conceivably shrink the estimated impact on violent crime by 30 percent. Even this would suggest that the Texas RTC law increased violent crime in the tenth year by roughly ten percent. As we noted in Table 2, the overall increases in incarceration and police rates were greater in RTC states than in non-RTC states, which would tend to make our overall estimates of the increased violence in all RTC states conservative.

[48] In our synthetic controls approach, we treat the year of passage to be the first year in which an RTC law was in effect for the majority of that year. Accordingly, we mark Philadelphia's law passage in 1996, as documented in Appendix B.I.

[49] As mentioned in footnote 45 and discussed in footnote 56, some researchers would not follow our approach and subtract off this year of adoption difference, which would lead to an estimate of a substantial increase in violent crime from Ohio's RTC law.

[50] In Appendix G, 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) ten years after adoption (or seven years after adoption for two states that adopt RTC laws in 2007, since our data ends in 2014). The figures also document the discrepancy in violent crime in the year of adoption between the actual and synthetic control values.



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 5

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 stochastic variation will be diminished as the over-estimates and under-estimates will tend to wash out in our mean treatment estimates. Figure 8 shows the synthetic control estimates on violent crime for all 31 states for which we have ten years of post-passage data. For 23 of the 31 states adopting RTC laws, the increase in violent crime is noteworthy.[51] While 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 ten 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.

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

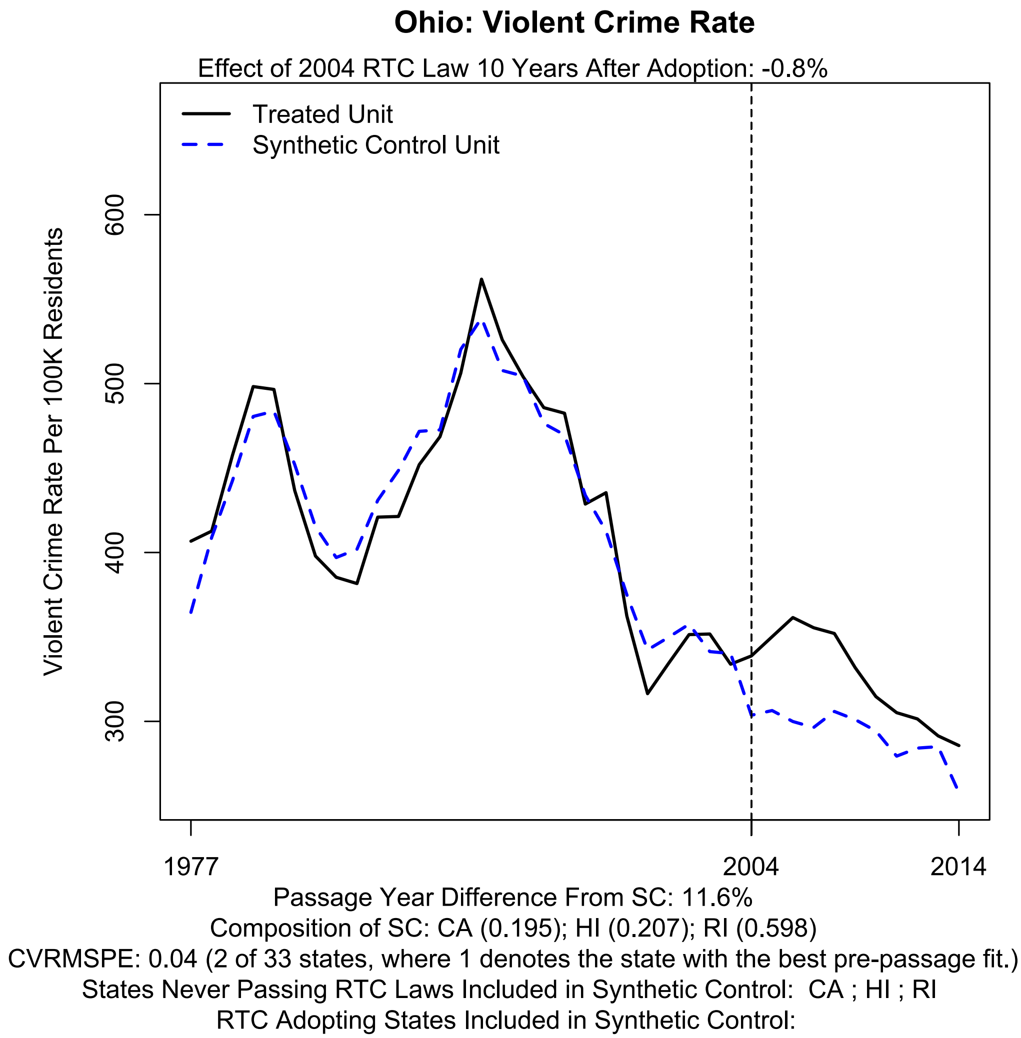


Figure 6

3. Less Effective Pre-Passage Matches

Section 1 above provided four examples of synthetic controls that matched the crime of the treatment states well in the pre-passage period, but this does not always happen. Two states for which we would have considerably less confidence in the quality of the synthetic controls estimate are South Dakota and Maine, whose poor estimates are depicted in Figures 9 and 10. These are two of the eight states showing improvements in crime following RTC adoption as indicated in Figure 8.

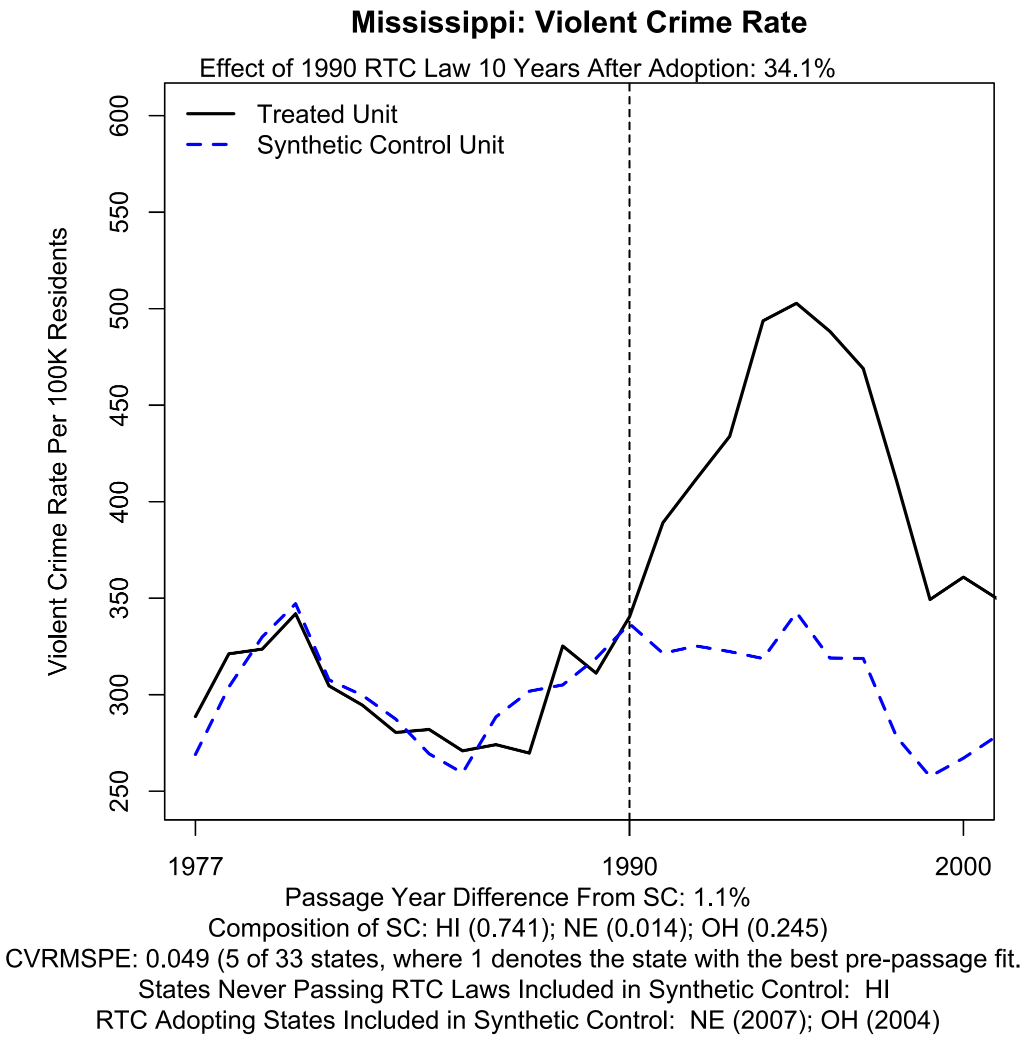

Mississippi: Violent Crime Rate
Effect of 1990 RTC Law 10 Years After Adoption: 34.1%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
250
300
350
400
450
500
550
600
1977
1990
2000
Passage Year Difference From SC: 1.1%
Composition of SC: HI (0.741); NE (0.014); OH (0.245)
CVRMSPE: 0.049 (5 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004)

Figure 7

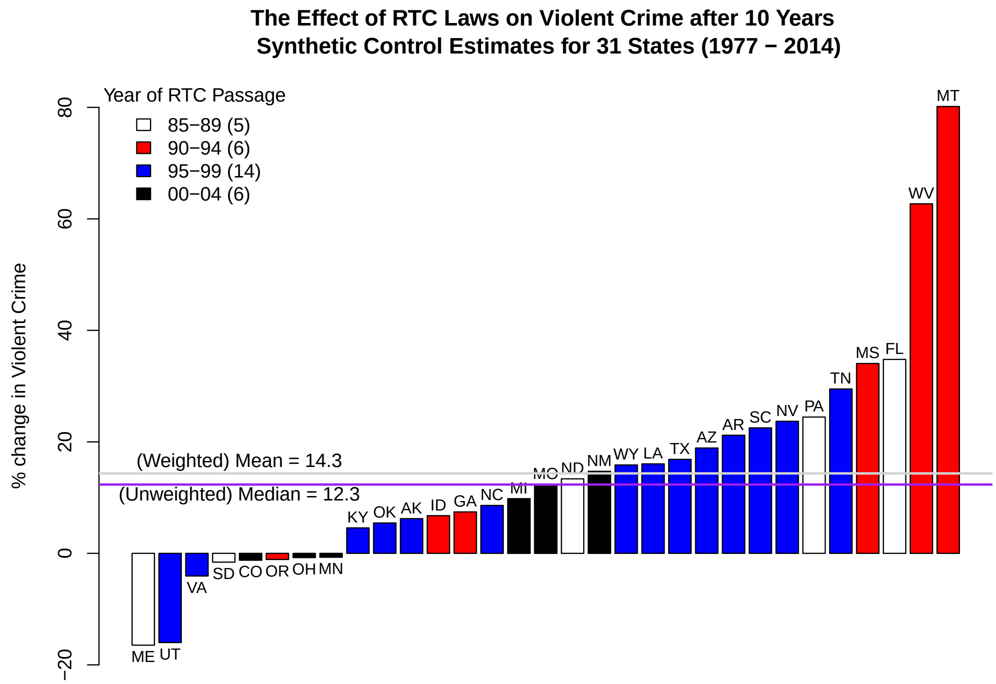
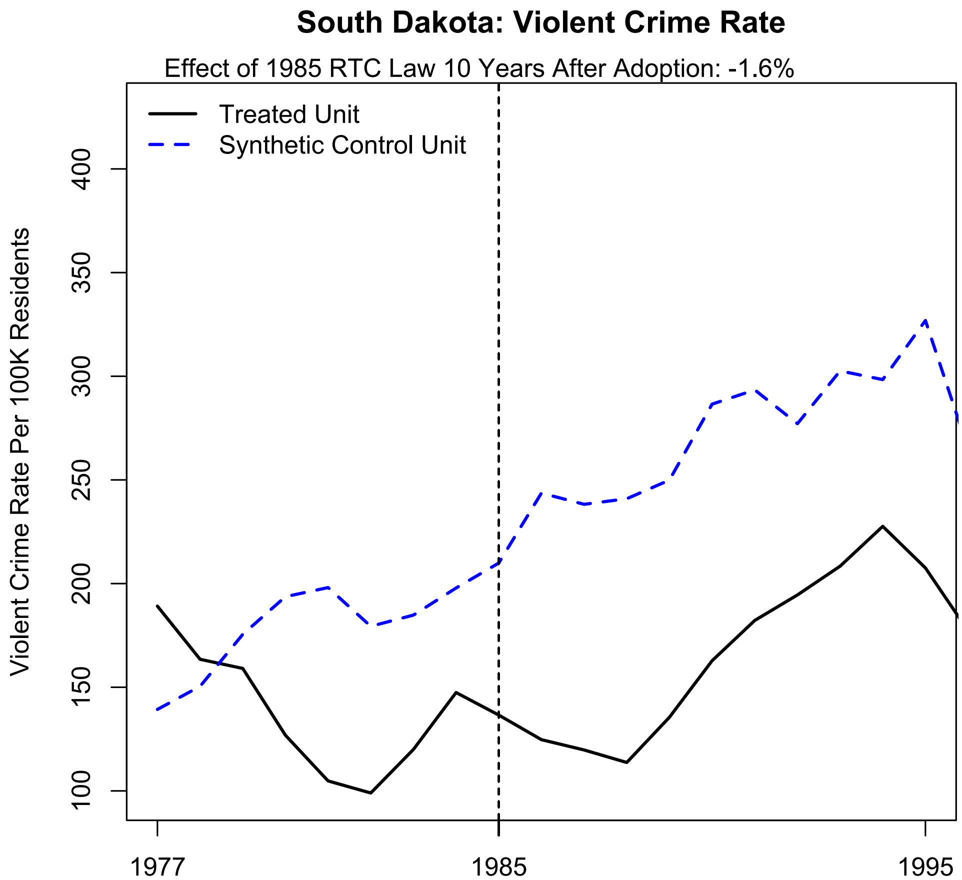


Figure 8

For South Dakota, one sees that the synthetic control and the state violent crime performance diverged long before RTC adoption in 1985, and that, by the date of adoption, South Dakota's violent crime rate was already 34.9 percent below the synthetic control estimate. The violent crime rate of actual South Dakota was trending down, while the synthetic control estimate had been much higher and trending up in the immediate pre-adoption period. A similar pattern can be seen emerging for Maine, which had a violent crime rate that was almost 38 percent below the synthetic controls estimate at the time of RTC adoption. Such discrepancies undermine confidence in the synthetic controls estimates for these two states. The difficulty in generating good pre-passage matches for South Dakota and Maine stems from their unusually low violent crime in the pre-passage period.

Figure 11 reproduces Figure 8 while leaving out the five states for which the quality of pre-passage fit is clearly lower than in the remaining 26 states.[52] This knocks out ND, SD, ME, MT, and WV, 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 8. As Figure

[52] In particular, for these five states, the pre-passage CVRMSPE—that is, the RMSPE transformed into a coefficient of variation by dividing by the average pre-passage crime rate—was 19 percent or greater. See Footnote 59 for further discussion of this statistic.



Passage Year Difference From SC: -34.9%
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)

Figure 9

11 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 .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.

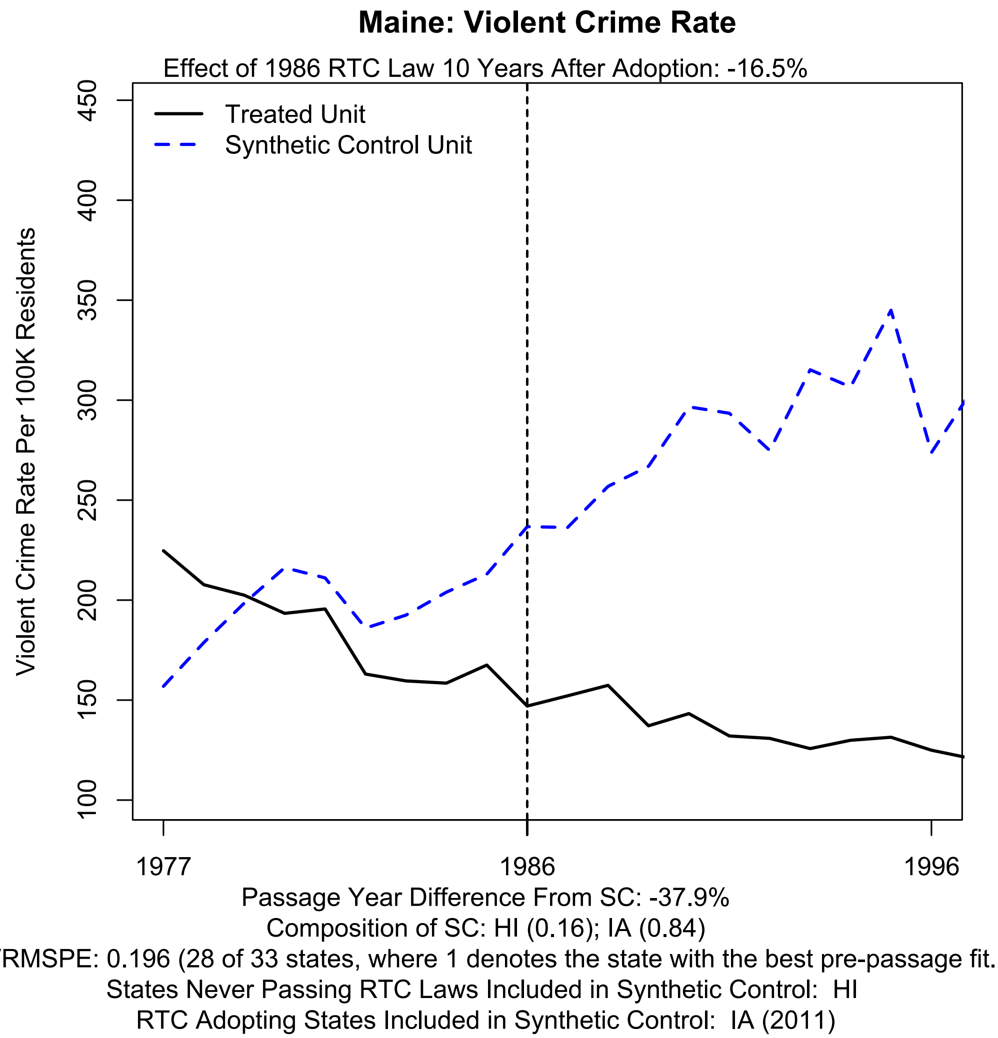


Figure 10

V. Aggregation Analysis Using Synthetic Controls

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[53] We estimate the percentage difference in violent crime between each treatment (RTC-

[53]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 post-treatment 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

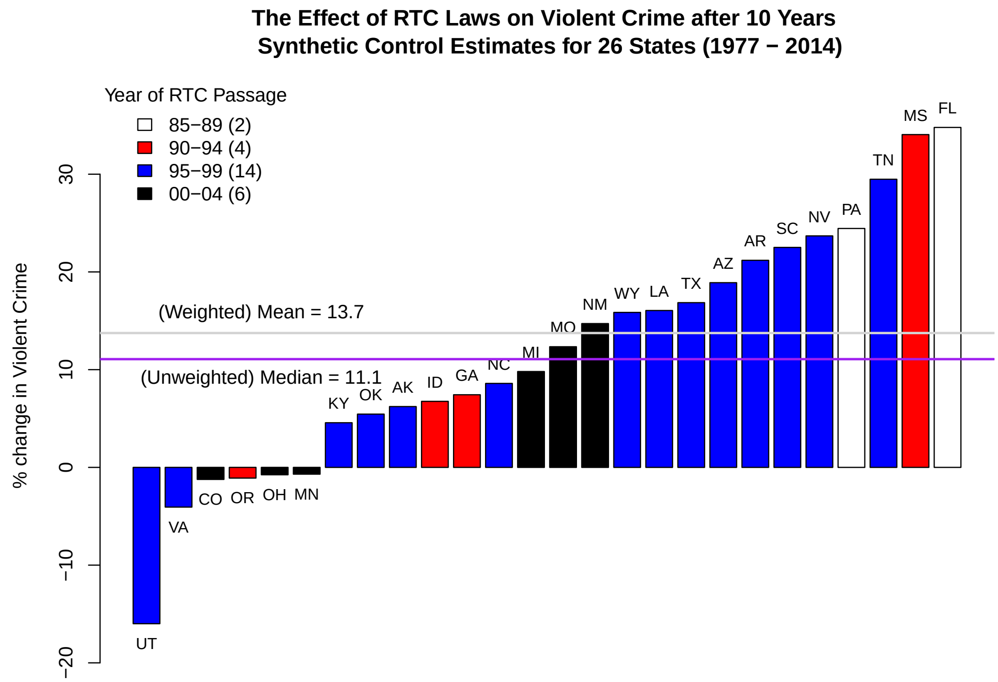


Figure 11

adopting) state and the corresponding synthetic control in both the year of the treatment and in the ten years following it. This estimate of the treatment effect percentage (TEP) obviously uses data from fewer post-treatment years for the two treatment states[54] that had RTC laws that took effect less than ten years before the end of our sample.[55]

We could use each of these ten percentage differences as our estimated effects of RTC laws on violent crime for the ten post-passage 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 ten 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 ten percent $\left(= \frac{440-400}{400}\right)$. Rather than use this estimate, however, we have subtracted from this figure the percentage difference

of panel units and more than two decades separate the dates of the first and last treatment under consideration, as highlighted in Figure 8.

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

[55]Dube and Zipperer (2013) average the estimated post-treatment percentage differences and convert this average into an elasticity estimated over the entire post-treatment period. Our work reports separate average treatment effects for ten yearly intervals following the time of the treatment.

between the synthetic and treatment states in the year of RTC adoption. If, say, the violent crime rate that year were two percent higher in the treatment state (above the synthetic control value), we would subtract two from ten to obtain an estimated tenth-year effect of RTC laws of eight percent.[56] We then aggregate all the state-specific estimates of the impact of RTC laws on violent crime for each of the ten years and test whether they are significantly different from zero.[57]

A. RTC Laws Increase Violent Crime

We now turn to the aggregated results of our synthetic control analysis using predictors derived from the DAW specification. Table 10 shows our results on the full sample examining violent crime.[58] 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 ten years after adoption. On average, treatment states had aggregate violent crime rates that were almost seven percent higher than their synthetic controls five years after passage and around 14 percent higher ten years after passage. Table 10 suggests that the

[56]It is unclear whether one should implement this subtraction. The intuitive rationale for our choice of outcome variable was that pre-treatment 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 five percent greater than that of the synthetic control in both the pre-treatment and post-treatment period, it would arguably be misleading to ignore the pre-treatment difference and declare that the treatment increased crime rates by five 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 controls protocol: we pretend that each RTC-adopting state actually adopted its RTC law five years before it did. We then generate synthetic controls estimates of this phantom law over the next five years of actual pre-treatment data. If our synthetic control approach is working perfectly, it should simply replicate the violent crime pattern for the five pre-treatment 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 pre-treatment years more closely than when we do not provide this normalization. Specifically, with subtraction the estimated "effect" in the final pre-treatment year is a wholly insignificant 3.2 percent; without subtraction, it jumps to a statistically significant 5.3 percent. Consequently, normalization is the preferred approach for violent crime. It should also be noted that our actual synthetic controls 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 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 D. Comparison of these Appendix D estimates with those in the text (Tables 10, 11, 12) reveals that our preferred method of subtracting yields more conservative results (i.e. a smaller increase in violent crime due to RTC). In Tables 10, 11 and 12, we estimate the tenth year TEP for violent crime is roughly 13.5 to 14 percent, while the comparable estimates without subtraction are roughly 17-18 percent, as seen in Tables A11, A12 and A13.

[57]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 post-treatment year under consideration. Robust standard errors corrected for heteroskedasticity are used in this analysis.

[58]We discuss the synthetic controls estimates for murder and property crime in section V(F) below.

longer the RTC law is in effect (up to the tenth year that we analyze), the greater the cost in terms of increased violent crime.

Table 10: 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 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
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.154	0.196	0.216	0.212	0.216	0.214	0.220	0.234	0.226	0.240
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.084	0.110	0.124	0.126	0.146	0.142	0.148	0.146	0.156	0.138
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.018	0.024	0.038	0.036	0.042	0.048	0.062	0.064	0.068	0.060

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As we saw in Figures 4-7 and 9-10, the validity of using the post-treatment 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 pre-treatment period. To generate an estimate of pre-treatment fit that takes into account differences in pre-treatment crime levels, we estimate the coefficient of variation for the root mean squared prediction error (RMSPE), which is the ratio of the synthetic control's pre-treatment RMSPE to the pre-treatment average level of the outcome variable for the treatment state.[59] To evaluate the sensitivity of the aggregate synthetic controls estimate of the crime impact of RTC laws in Table 10, we consider two subsamples of treatment states: states whose coefficients of variation are less than two times the average coefficient of variation for all thirty-three treatments and states whose coefficients of variation are less than this average. We then re-run our synthetic controls 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

[59]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 pre-treatment crime rates among the 33 treatment states that we consider. For example, the pre-treatment 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 pre-treatment 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 .06 for Texas (with a year of adoption discrepancy of only 3.6 percent), while for Maine, the CV is a dramatically higher .196 (with an initial year discrepancy of -37.9 percent). Accordingly, since the percentage imprecision in our synthetic pre-treatment match for Maine is so much greater than for Texas, we have greater confidence in our estimates that in the tenth 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.

findings.

Table 11 repeats the Table 10 analysis while dropping the four states with a CV of the RMSPE that is more than twice the average of the sample. Table 12 uses a more stringent measure of assessing how well the synthetic control fits the pre-passage data by dropping the six states with an above average CV for the RMSPE. It is striking how all three tables yield roughly identical conclusions: RTC laws are consistently shown to increase violent crime, with the tenth-year increase ranging from a low of 13.5 (Table 11) to a high of 14.3 percent (Table 10).

Table 11: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, $<$ 2x Average Coefficient of Variation of the RMSPE, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.168	2.487*	3.702*	4.695**	7.124***	7.178**	9.901***	12.087***	13.040***	13.513***
	(1.101)	(1.331)	(1.893)	(2.121)	(2.547)	(3.231)	(2.910)	(3.958)	(3.701)	(2.966)
N	29	29	29	29	29	29	29	27	27	27
Pseudo P-Value	0.910	0.308	0.208	0.180	0.080	0.098	0.062	0.042	0.042	0.044
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.166	0.192	0.198	0.204	0.198	0.206	0.214	0.226	0.222	0.226
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.094	0.118	0.124	0.124	0.126	0.134	0.138	0.144	0.142	0.146
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.014	0.028	0.038	0.040	0.034	0.042	0.060	0.052	0.054	0.050

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS NC NE NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: MT ND SD WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, $<$ 1x Average Coefficient of Variation of the RMSPE, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.201	2.584*	3.907*	4.972**	7.420***	7.431**	10.155***	12.338***	13.330***	13.745***
	(1.124)	(1.356)	(1.918)	(2.144)	(2.573)	(3.274)	(2.939)	(3.962)	(3.698)	(2.969)
N	27	27	27	27	27	27	27	26	26	26
Pseudo P-Value	0.900	0.306	0.196	0.152	0.066	0.090	0.056	0.032	0.026	0.030
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.158	0.182	0.188	0.182	0.166	0.182	0.202	0.210	0.196	0.204
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.086	0.108	0.112	0.116	0.104	0.118	0.130	0.136	0.120	0.138
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.028	0.030	0.046	0.046	0.046	0.054	0.038	0.042	0.048

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: ME MT ND NE SD WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 Tables 10-12, we incorporate an analysis using placebo treatment effects similar to Ando (2015).[60] For this analysis, we generate 500 sets of randomly generated RTC dates that are designed to resemble the distribution of actual RTC passage dates that we use in our analysis.[61] 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 this 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 ten post-treatment years (for which we have data) that follow it. Using the methodology described in footnotes 45 and 56, we then test whether the estimated treatment effect for each of the ten post-treatment years is statistically significant.

At the bottom of Tables 10-12, we list the proportion of each post-treatment year's placebo regressions that were "significant" at the .10 level, .05 level, and .01 level. Table 10 shows that the placebo results appear to be significant at the .01 level 1.8 percent of the time for our first year after passage to 6.0 percent in the tenth year. In other words, the standard errors we report at the top of Table 10 are potentially underestimated, as our placebo averages are "statistically significant" more often than would be expected by chance. We cannot say that with certainty, however, since (for the reason noted in footnote 60), these placebo estimates are capturing some of the effect of RTC laws.[62]

As another check on the statistical significance of our results, we compare each of the ten coefficient estimates in Table 10 with the distribution of the 500 average placebo treatment effects that use the same crime rate, post-treatment year, and sample as the given estimate. To assist in this comparison process, we report a pseudo p-value which is equal to the proportion of our placebo

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[60] 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. While the average treatment effect measured in our paper 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 (2014) 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.

[61] 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, D.C. 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.

[62] In general, the difference between the proportion of placebo results significant at a given level and the significance level itself varies across crime rates and treatment selection criteria. We do not observe any consistent tendency for the significance levels and proportion of placebo results significant at those levels to converge when restricting the sample to states with a relatively low RMSPE.

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 Tables 10-12, we see that our violent crime results are always statistically significant in comparison to the distribution of placebo coefficients at the .05 level eight years or more past RTC adoption.

C. Synthetic Control Estimates Using Other Sets of Explanatory Variables

1. Synthetic Control Estimates Using the BC Explanatory Variables

Table 13 provides synthetic control estimates of the impact of RTC laws on violent crime using the set of predictors in the BC model.[63] This model estimates that RTC laws increase violent crime consistently after adoption, rising to 13.3 percent after ten years (significant at the .01 level using our standard approach).[64] This tenth-year effect is also close to the corresponding DAW model's synthetic control estimate (Table 10), and broadly consistent with the DAW and BC panel data models' dummy variable coefficients (Tables 4-5).

Table 13: The Impact of RTC Laws on the Violent Crime Rate, BC covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.035	3.047*	4.683*	4.995**	6.538**	7.026**	9.618***	12.904***	13.135***	13.288***
	(1.203)	(1.517)	(2.363)	(2.272)	(2.548)	(3.148)	(3.049)	(3.999)	(3.780)	(3.163)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.970	0.196	0.122	0.162	0.114	0.116	0.064	0.030	0.040	0.048
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.160	0.202	0.200	0.196	0.212	0.210	0.222	0.222	0.232	0.238
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.104	0.124	0.130	0.118	0.136	0.152	0.140	0.152	0.162	0.150
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.022	0.032	0.042	0.040	0.048	0.058	0.068	0.056	0.050

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[63]For certain treatment states with 0 executions prior to RTC adoption, the synthetic control program is unable to generate a counterfactual unit. To resolve this problem, and to maintain consistency in the process of generating a counterfactual unit for the 33 treatment states, the executions variable is dropped from the BC model in the synthetic controls analysis.

[64]The tenth-year effect in the synthetic controls analysis using the BC variables is 13.4 percent when we eliminate the five 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 13.1 percent effect.

2. Synthetic Control Estimates Using the LM Explanatory Variables

In our Part II panel data analysis, we saw that RTC laws were associated with significantly higher rates of violent crime in the DAW model (Table 4), the BC model (Table 5, Panel A), and the MM model (Table 7, Panel A), but not in the LM model (Table 6, Panel A), although both the LM and MM models did show RTC laws increased murder. Table 14 estimates the impact of RTC laws on violent crime using the LM specification.[65] The detrimental effects of RTC laws on violent crime rates are statistically significant at the .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 14 range from 11.7 percent in the seventh post-treatment year to 14.3 percent in the ninth post-treatment year. Remarkably, the DAW, LM, and BC synthetic control estimates of the impact of RTC laws on violent crime are nearly identical (compare Tables 10, 13, and 14), and this is true even when we limit the sample of states in the manner described in Tables 11-12.[66]

Table 14: 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	0.280	2.934*	4.716**	5.509**	7.630***	8.027**	11.741***	13.292***	14.306***	14.199***
	(1.182)	(1.503)	(1.949)	(2.153)	(2.544)	(3.121)	(2.957)	(3.930)	(3.751)	(2.888)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.852	0.214	0.088	0.110	0.064	0.090	0.034	0.028	0.036	0.034
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.154	0.178	0.184	0.200	0.208	0.218	0.240	0.244	0.264	0.256
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.078	0.094	0.108	0.106	0.124	0.144	0.164	0.160	0.184	0.174
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.034	0.038	0.046	0.042	0.054	0.058	0.066	0.076	0.062

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[65]The modified panel data analyses of LM and MM, shown in Panel B of Tables 6 and 7, did find RTC laws increase violent crime. In conducting the LM panel data analysis, we used the lagged violent and property arrest rates rather than the contemporaneous crime-specific arrest rates described by Lott and Mustard (1997) owing to the fact that the LM approach would essentially (and improperly) place the same variable on both sides of the regression model. (We used the lagged violent crime arrest rate in the panel data murder equations as well.) This objection is less applicable under the synthetic control framework. For this reason, we use their contemporaneous crime-specific arrest rates in our synthetic control model using the Lott and Mustard (1997) control variables, and use the murder arrest rate in the LM murder estimates and violent crime arrest rate in the MM murder estimates.

[66]The tenth-year effect in the synthetic controls analysis using the LM variables is 13.9 percent when we eliminate the three states with more than twice the average CV of the RMSPE. Knocking out the six states with above-average values of this CV generates a similar 13.5 percent effect. We also estimated the impact of RTC laws on violent crime using the synthetic controls approach and the LM model modified to use six DAW demographic variables. This change increased the estimated tenth-year increase in violent crimes from 14.2 percent to 15.5 percent.

3. Synthetic Control Estimates Using the MM Explanatory Variables

Table 15 provides synthetic control estimates of the impact of RTC laws on violent crime using the MM predictors.[67] The table reveals that RTC states experienced overall violent crime rates that were roughly 15 percent greater than those of their synthetic controls ten years after passage, which was statistically significant at the .01 level.[68] The similarity of the DAW, BC, LM, and MM synthetic controls estimates of the impact of RTC laws on violent crime is striking. Moreover, these four sets of estimates are remarkably consistent with the DAW and BC panel data estimates of the impact of RTC laws, which bolsters the case that the DAW and BC panel data specifications provide more reliable estimates of the impact of RTC laws on violent crime than either the LM or MM models.

Table 15: The Impact of RTC Laws on the Violent Crime Rate, MM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.067	1.634	3.116*	4.708*	7.553**	8.196**	11.282***	13.424***	14.623***	15.290***
	(1.186)	(1.535)	(1.833)	(2.366)	(2.835)	(3.171)	(3.236)	(3.997)	(4.256)	(3.796)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.964	0.468	0.274	0.166	0.070	0.096	0.050	0.032	0.030	0.034
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.152	0.162	0.160	0.188	0.196	0.208	0.224	0.234	0.234	0.240
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.088	0.100	0.108	0.104	0.126	0.138	0.138	0.150	0.154	0.148
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.026	0.030	0.040	0.036	0.042	0.058	0.054	0.062	0.058	0.056

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

Results reported for the constant term resulting from this regression

States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY

States excluded for poor pre-treatment fit:

The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D. The Contributions of Donor States to the Synthetic Controls Estimates - Evaluating Robustness

One of the key elements of the synthetic controls 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. Figure 12 lists all the states that are eligible under this criterion to serve as synthetic controls for one or more

[67] For the same reasons described in footnote 65, we used lagged arrest rates in our panel data regression tables but use the contemporaneous arrest rate as a predictor in our synthetic controls code for the MM specification.

[68] As we have seen previously, leaving out states with larger CVRMPSEs barely changes the results: Eliminating the three states with twice the average CVRMSPE leads to an estimated tenth-year effect using MM variables of 15.0 percent, and eliminating the six states with above-average CVRMSPE values leads to an estimated effect of 14.7 percent. We also estimated the impact of RTC laws on violent crime using the synthetic controls approach and the MM model modified to use six DAW demographic variables. This change increased the estimated tenth-year increase in violent crimes from 15.3 percent to 15.4 percent.

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 the synthetic controls estimates in our violent crime estimates.[69] 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).

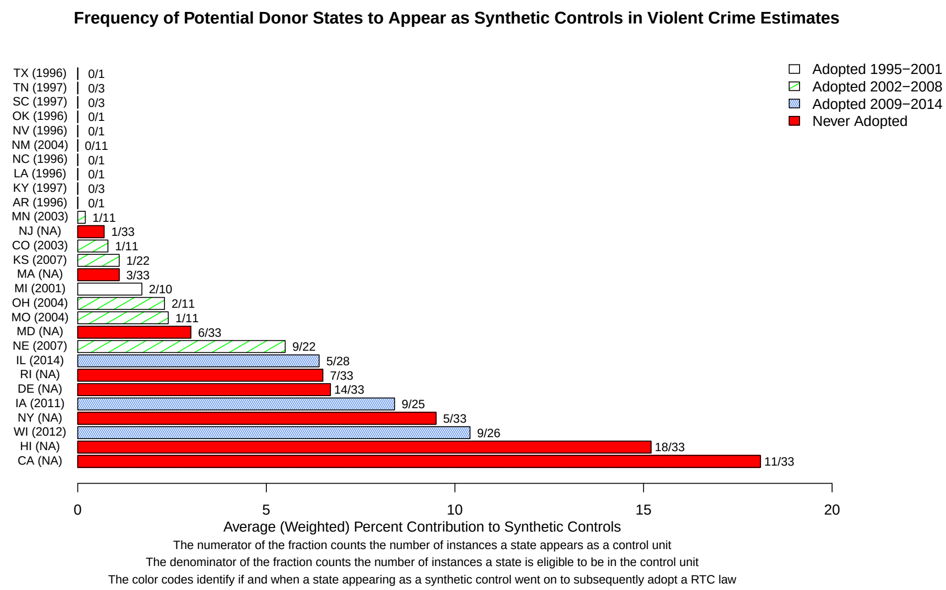
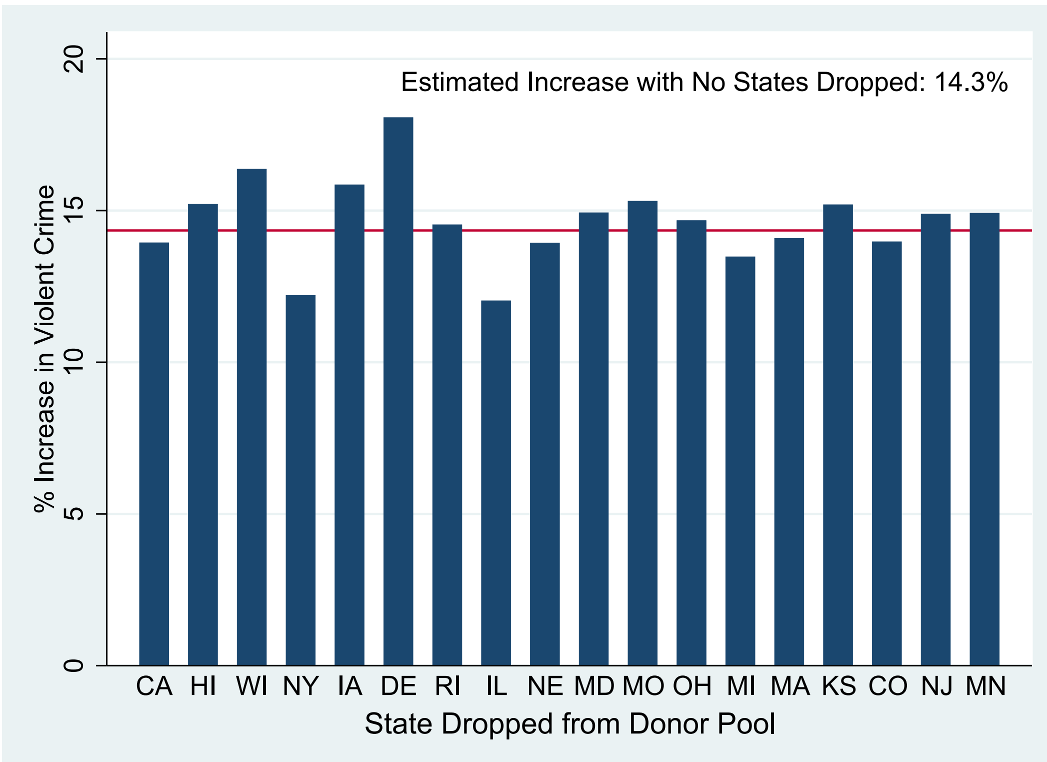


Figure 12

Hawaii's relatively large contribution as a donor state in the synthetic controls 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 not likely to be a concern 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 control states, we generated 18 additional TEP estimates, with each one generated by dropping a single one of the

[69]In particular, it reflects the portion of each synthetic state it becomes part of, weighted by the treated state's population. For example, Texas' population is 13.6 percent of the total treated states' population. As a result, a state that makes up 50 percent of synthetic Texas would have a bar of size 6.8 percent.

18 states that appears as an element of our synthetic controls analysis (as identified in Figure 12). The results of this exercise are presented in Figure 13, 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 .01 percent level, and 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.



This graph shows the overall synthetic-controls estimate of the impact of RTC laws on violent crime ten years after adoption when barring individual states from inclusion in the synthetic control. (The horizontal line shows the estimate when no states are barred.) The states are arranged in declining order of population-weighted average contribution to synthetic controls (see Figure 12), from a high of 18.1 percent for California to a low of 0.2 percent for Minnesota.

Figure 13: Estimated Increase in Violent Crime ten Years After RTC Adoption, Dropping One Donor State at a Time

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

1. Prevalence Proxied by Fraction of Suicides Using Guns

The wide variation in the state-specific synthetic control estimates that was seen in Figures 8 and 11 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 8. 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, it is the fact that we can average across 33 separate RTC-adopting states that 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 at the time of adoption, or after the law has been in place for ten years.

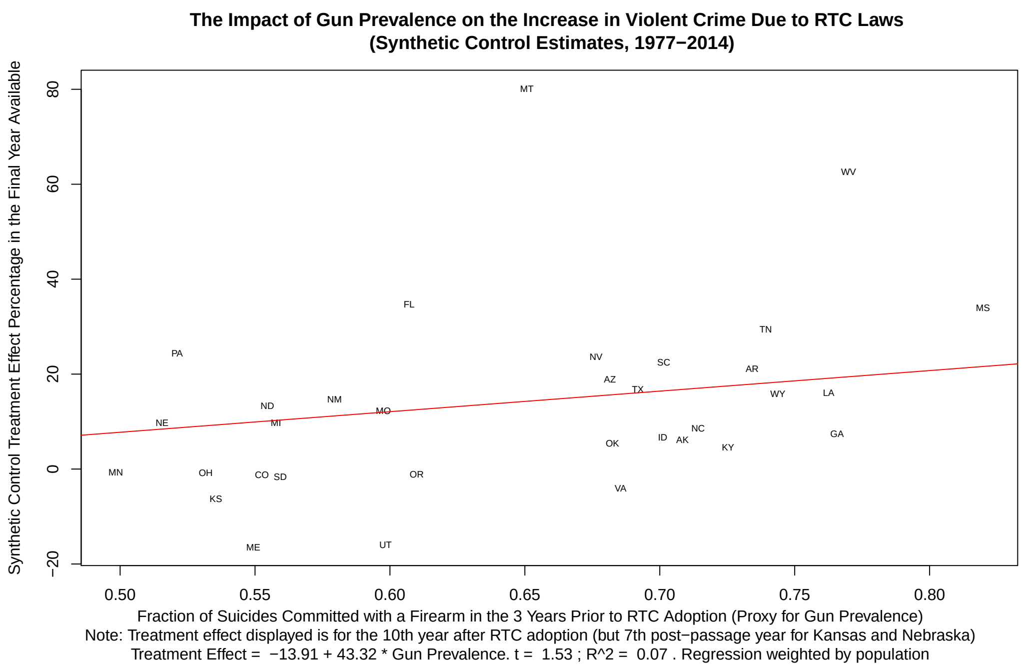


Figure 14

Figure 14 shows the scatter diagram for 33 RTC-adopting states, and relates the estimated impact on violent crime to a measure of gun prevalence in the three years prior to RTC adoption. (Gun prevalence is proxied by the commonly used measure showing the fraction of suicides in a state that are committed with guns.) The last line of the note below the Figure provides the regression equation, which shows that the gun prevalence proxy is positively related to the estimated increase in crime, but the coefficient is not statistically significant (t = 1.53) and the R^2 value is low.[70] The population-weighted mean gun proxy level across our 33 states is 0.64 (roughly the level of Montana), which would be associated with a 13.8 percent higher rate of violent crime ten years after RTC adoption.

Figure 15 shows the same graph and scatterplot, except gun prevalence is measured at the end of the 10-year period. Specifically, for the 28 states that adopted RTC from 1981-2004, gun prevalence is averaged across the 9th, 10th, and 11th post-passage years. For the other five states, we average across the last three years available (2012-2014). The estimated effect from gun prevalence and the R^2 value are larger, and the coefficient approaches statistical significance at the .05 level. The population-weighted mean gun proxy level by this metric, 0.57, is associated with a 13.6 percent higher rate of violent crime ten years after RTC adoption.

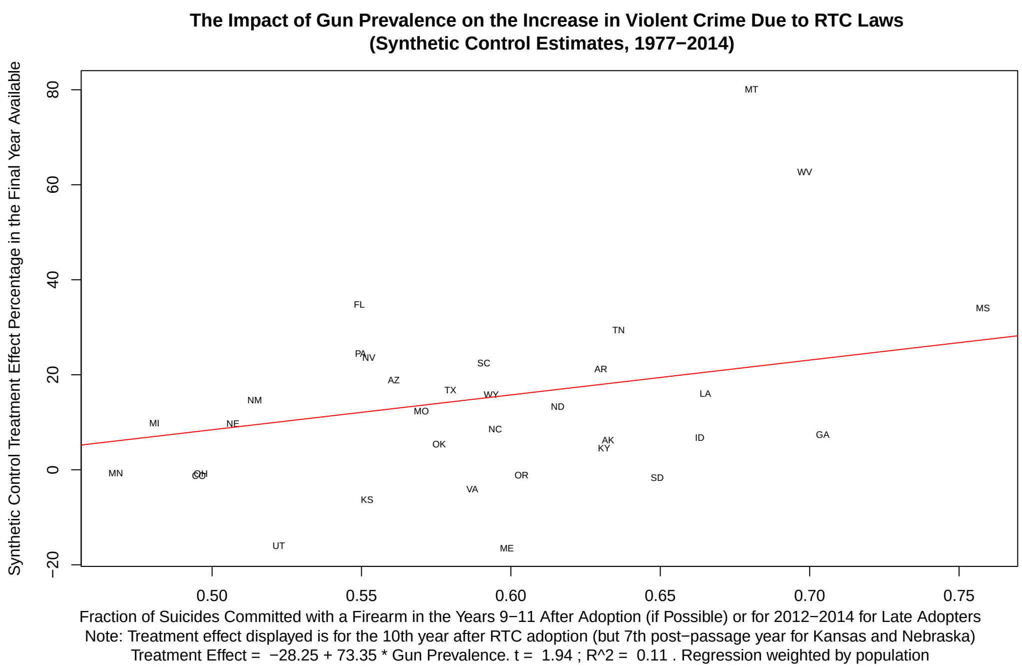


Figure 15

2. Permit Data Analysis

A detailed, nation-wide analysis of the increases in and effects of concealed carry permits is not possible due to the limited availability of state level permit data. Nonetheless, we were able to

[70]A bivariate regression that weights by the inverse of the CV of the RMSPE, rather than by state population yields results substantively identical to those in Figure 14. We also repeat this analysis while dropping the five states with the worst pre-passage fit (NE, WV, MT, SD, and ND), and this modification again does not substantively change the Figure 14 regression results.

obtain substantial amounts of useful data for four states: Alaska, Tennessee, Texas, and Utah.[71] The data suggests that the effect of RTC on violent crime should grow over a period of at least ten years after the passage of the law. Table 16 presents the increases in absolute numbers of permits and permits per person for the ten years following adoption of RTC laws (eight years for Alaska). All four states show substantial growth in permit holding following passage. Tennessee and Utah have an average 126-fold increase in permit holding per person from the year prior to passage to ten years after. Alaska and Texas both approximately double in the prevalence of permits from the year of passage to eight or ten years later, respectively.

Since we have the number of permits for each of these four states and an estimated impact on violent crime for each year after RTC adoption from our synthetic controls analysis, we can use our limited permit data to directly test for an association between violent crime and the increasing numbers of permits in these states. Although we only have 41 observations, we were able to obtain meaningful results from a panel data regression with state fixed effects of absolute numbers of permits on our normalized estimated differences in violent crime between each state and its synthetic control (TEP). Table 17 shows that every 1,000 additional permits is associated with a .213 percentage point increase in crime. This estimate is statistically significant at the .001 level (t-statistic of 18.8), and the R-squared for the regression is .77. In spite of the clear limitations of our data, Table 17 does suggest that the increasing numbers of active permits after RTC passage puts significant upward pressure on violent crime.

[71] For Texas, we obtained access to exact numbers of carry permits at year end starting with the time of RTC passage. The other three states gave us access to the annual numbers of permits issued and, for Tennessee, revoked, after passage. We estimated numbers of active permits by year based on the numbers of permits issued by year, the time until permit expiration, and, where available, the numbers of permits revoked by year. While not exact, our estimation method compares quite well with the incomplete data on active permits we received from Tennessee and Utah. Our calculations for Alaska are less precise since we did not have access to numbers of renewed permits, as we did for the other states, which should lead us to underestimate active permit numbers several years after RTC passage. Since Alaska instituted permitless carry in 2003, we only considered permit data for the period 1995-2002.

Table 16: The Increase in Concealed Carry Permits After RTC Adoption in Four States

State	Year	Years from RTC Passage	Number of Permits	Permits per Person
Alaska	1995	0	3901	0.006
Alaska	2002	8	10304	0.016
Tennessee	1996	-1	1137	0.000
Tennessee	2007	10	213782	0.035
Texas	1996	0	113640	0.006
Texas	2006	10	258162	0.011
Utah	1994	-1	621	0.000
Utah	2005	10	68420	0.028

The permit data is from the Alaska Department of Public Safety, Tennessee Department of Public Safety, Texas Department of Public Safety, and Utah Department of Public Safety.

Table 17: Panel Data Estimate Showing Positive Association Between Numbers of Permits and Violent Crime in Four States (N = 41)

	Normalized TEP
Number of Active Permits (in Thousands)	0.213^{***} (0.011)

OLS estimation includes state-fixed effects and is weighted by population. Robust standard error (clustered at the state level) is provided next to point estimate in parentheses. The permit data is from the Alaska Department of Public Safety, Tennessee Department of Public Safety, Texas Department of Public Safety, and Utah Department of Public Safety. * $p < .1$, ** $p < .05$, *** $p < .01$.

F. The Murder and Property Crime Assessments with Synthetic Controls

The synthetic controls estimates of the impact of RTC laws on violent crime uniformly generate statistically significant estimates, and our phantom RTC law synthetic controls estimates for the five pre-treatment years (described in footnote 56) give us confidence that the synthetic controls approach is working well for our violent crime estimates. Using this phantom law approach for murder and property crime yields less encouraging estimates. While our estimated “effect” in the year prior to adoption would ideally be close to zero, for murder it is 7.8 percent and for property crime it is 6.9 percent, with the latter significant at the .01 level. Without normalization, these estimates jump to 9.9 percent (significant at the .10 level) for murder and 16.7 percent (significant at the .01 level) for property crime. (The full results of this test for all the crime categories are shown in Appendix J). In other words, our synthetic controls estimates for violent crime are far more validated by our phantom adoption test than the murder and property crime estimates and, for that reason and the uniform results across all four models (DAW, BC, LM and MM), we have greater confidence in and highlight our violent crime estimates.

Our synthetic control estimates of the impact of RTC laws on murder and property crime appear in Tables A3-A10 of the appendix. In all cases the tenth-year effect for these crimes is positive, although only the estimated 6.4 increase in murder using the BC covariates is statistically significant at the .10 level (Table A5). For murder, the point estimates suggest an increase of 4-6 percent, and for property crime, the point estimates range from 1-3.5 percent increases.

The relatively smaller impact of RTC laws on property crime is not surprising. Much property crime occurs when no one is around to notice, so gun use is much less potentially relevant in property crime scenarios than in the case of violent crime, where victims are necessarily present. Most of the pernicious effects of RTC laws—with the exception of gun thefts—are likely to operate to increase violent crime more powerfully than property crime. The fact that the synthetic controls approach confirms the DAW panel data estimates showing that RTC laws increase violent crime while simultaneously showing far more modest effects on property crime (thereby conflicting with the DAW panel data estimate showing substantial increases in property crime) may be thought to enhance the plausibility of the synthetic controls estimates.

But then what are we to make of the relatively small estimated impact of RTC laws on murder? This might seem to be at odds with our theoretical expectations, and in conflict with the estimated increases in overall violent crime since one might expect violent crime and murder to move together. A number of points should be noted. First, it is possible that we simply cannot rely on the murder estimates because of the relatively poor performance of the synthetic controls for this crime, compared to the violent crime estimates (see Appendix J). This is not conclusive because it is possible that our actual murder estimates become sufficiently accurate with the five more years of data that we actually use that we can rely on the resulting estimates. In that event, a 4.3 to 6.4 percent increase in murder over a ten-year period is not a small effect. Part of the explanation for the lower level of statistical significance for murder is that we are able to get more precise estimates of the impact of RTC laws on violent crime then for the far less numerous, and hence much more volatile, crime of murder. Indeed, the standard errors for the synthetic controls estimate of increased murder in the tenth year is 26 percent higher than the comparable standard error for violent crime (compare Table 10 with Table A3).

But a second and possibly more important fact is also at work that likely causes the synthetic controls approach to understate the increase in crime caused by RTC laws, particularly for murder. We know from Table 2 that RTC states increased police employment by 8.4 percent more and increased incarceration by almost seven percent more in the wake of RTC adoption than did non-RTC states. This suggests that our synthetic controls estimates of the crime-increasing impact of RTC laws could be biased downward, and since police and incarceration are more effective in stopping murder than either overall violent or property crime, the extent of any bias would be greatest for the crime of murder. In other words, the greater ability of police and prison to stop murders than overall violent (or property) crime may explain why the synthetic controls estimates for murder are weaker than those for violent crime. An increase in police employment of 8.4 percent alone would be expected to suppress murders in RTC states (relative to non-RTC states) by about 5.6 percent.[72] Since the synthetic controls approach does not control for the higher police employment and incarceration in the post-adoption phase for RTC states, it may be appropriate to elevate the synthetic controls estimates on murder to reflect the murder-dampening effect of the two factors.

To adjust our synthetic control estimates of the impact of RTC laws on murder to reflect the post-adoption changes in the rates of police employment and incarceration, we can compare how these crime-reducing elements change in the wake of adoption for each RTC-adopting state and for its particular synthetic control. Consistent with the panel data finding of Table 2 that police and incarceration grew more post-RTC-adoption, we found that the population-weighted average percent change in the incarceration rate from the year of adoption to the 10th year after adoption (the 7th year after adoption for Kansas and Nebraska) is 28 percent for the treated unit and only 20 percent for the synthetic control unit. For the police employee rate, the analogous numbers are 9.1 percent for the treated unit and 7.6 percent for the synthetic control unit.[73]

We correct for this underestimation by restricting the synthetic control unit to have the same growth rate in incarceration and police as the treated unit.[74] Once we have computed an adjusted murder rate for the 31 synthetic control units in the 10th year after adoption, we then use the formula described in part IV to construct an adjusted aggregate treatment effect.[75] The impact of controlling for police and incarceration is substantial: the 10th year impact of RTC laws rises from 4.30 percent (t = 1.17) to 8.99 percent (t = 1.76).[76] In other words, the ostensible puzzle that RTC laws generated a large and statistically significant increase in overall violent crime but led

[72]The important recent paper by Professors Aaron Chalfin and Justin McCrary concludes that higher police employment has a dampening effect on crime, and, most strikingly, on murder. Specifically, Chalfin and McCrary (2013) find elasticities of -0.67 for murder but only -0.34 for violent crimes and -0.17 for property crimes.

[73]22 of the 33 states experienced growth in the incarceration rate (17/33 for police employee rates) that was greater than their respective synthetic controls growth rate (obtained using DAW covariates and the murder rate).

[74]By comparing the synthetic control unit's adjusted police/incarceration figures with its actual police/incarceration figures, and by applying standard estimates of the elasticity of murder with respect to police (-0.67) and incarceration (-0.15), we can create an adjusted version of the control unit's murder rate for each year after RTC adoption. For example, if the adjusted police and incarceration rates for the synthetic control unit were both ten percent greater than the actual rates in the 10th year after adoption for an RTC-adopting state, we would adjust the murder rate for the synthetic control unit downwards by 0.67*10 + 0.15*10 = 8.2 percent (thereby elevating the predicted impact of RTC laws on murder).

[75]Kansas and Nebraska, both 2007 adopters, have no comparable data for ten years after adoption and are thus not included in this calculation.

[76]If one only corrects for the larger jump in police experienced by the treatment states, the 10th year effect jumps from 4.30 percent (t = 1.17) to 7.08 percent (t = 1.49).

to a smaller and less statistically significant increase in murder may be explained by the fact that RTC-adopting states constrained the RTC-induced increase in murder by elevating their rates of police and incarceration.

Finally, we have chosen to present synthetic controls estimates that subtract off the initial year discrepancy between the actual and synthetic controls crime figures, which we think is validated by our Appendix J analysis. While these would be be our preferred estimates, Tables A14-A16 in Appendix D show that without subtraction the DAW tenth year synthetic control estimates of the increase in the murder rate from RTC adoption range from 11-14 percent, and are statistically significant at or above the .05 level.[77]

VI. Conclusion

The extensive array of panel data and synthetic controls 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 that RTC laws reduce violent crime. Indeed, the weight of the evidence from the panel data estimates as well as the synthetic controls analysis best supports the view that the adoption of RTC laws substantially raises overall violent crime in the ten years after adoption.

In our initial panel data analysis, our preferred DAW specification as well as the BC specification predicted that RTC laws have led to statistically significant and substantial increases in violent crime. When the LM and MM models were appropriately adjusted, they generated the same findings, but even without adjustment, these models showed RTC laws increased murder significantly in the spline models that Lott and Mustard once championed. We then conducted a LASSO analysis based on all 62 explanatory variables from the various models, and it yielded estimates that RTC laws increased crime from 1.4 to 8.4 percent. 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, again using the DAW, BC, LM, and MM specifications. Now the results were uniform: for all four specifications, states that passed RTC laws experienced 13-15 percent *higher* aggregate violent crime rates than their synthetic controls after ten years (results that were significant at either the .05 or .01 level after five years).

As judged by our validation test for the five pre-treatment years, our synthetic controls esti-

[77]The Table A3 DAW estimate for murder in the tenth year after the RTC adoption is 4.3 percent and not statistically significant (with subtraction) but rises to a statistically significant value of 11.2 percent without subtraction (Table A14). Similarly, when not subtracting the adoption year percentage difference, the tenth year TEP for property crime is over ten percentage points larger and becomes significant at the five percent level (Table A17).

mates are less reliable for murder and property crime than they are for violent crime, and therefore we place lesser emphasis on them. Nevertheless, the synthetic controls estimates for the impact of RTC laws on murder range from 4.3 to 6.4 percent higher after ten years (but are not statistically significant). If one adjusts the synthetic controls estimates for the increased rates of police and incarceration that follow RTC adoption, the RTC-induced increases in murder are almost nine percent with a p-value of 0.089. In addition, the murder effects rise to 11-14 percent for the DAW model and become statistically significant at the .05 level if we do not subtract off the initial year differential between the actual and synthetic control murder rates.

The synthetic controls 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. While our placebo analysis suggests that the standard errors associated with some of these estimates may have been biased downward, the size of our average estimated treatment effect in comparison to the distribution of placebo effects indicates that the deleterious effects associated with RTC laws that we estimate for aggregate violent crime are qualitatively large compared to those that we would expect to observe by chance.

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. Nonetheless, estimation using observational data always rests on numerous assumptions, so one must be alert to potential shortcomings. For example, if states that were expected to experience future increases in crime were more likely to adopt RTC laws, then we might exaggerate the detrimental effect of RTC laws on crime. Given the very limited ability of politicians, pundits, and even academic experts to correctly predict crime trends over this period, though, this problem of endogeneity is unlikely to mar our results. Indeed, a greater concern is that an unexpected spike in crime would lead to RTC adoption, and the ensuing regression of crime to the mean would be inappropriately attributed to the RTC law. While any analysis can be susceptible to problems of omitted variable bias, the synthetic controls approach was designed to better address that concern for unknown or unmeasured effects than a panel data analysis would.

The results presented in this paper also help to explain the longstanding discrepancy that has existed between the econometric results suggesting that RTC laws increase crime and the perception "on the ground" in some states that RTC laws were not associated with a contemporaneous increase in crime rates. The conflict between these findings is resolved when one realizes that since the crime spikes of the late 1980s and early 1990s, most states, including many adopting RTC laws, experienced large and important violent crime decreases. (Some notable exceptions were the following RTC states, which all saw jumps in violent crime after RTC adoption: Pennsylvania, Mississippi, Montana, North Dakota, South Dakota, and West Virginia.) However, our analysis suggests that had states like Alaska, Arizona, Florida, Louisiana, and Texas avoided adoption of RTC laws, they would have experienced greater drops in violent crime, ranging from 6.2 percent lower in Alaska to 34.8 percent lower in Florida. Indeed, as Figure 1 illustrated, while RTC states in aggregate have now fallen modestly below their violent crime rates of 1977, the states that did not adopt RTC laws enjoyed violent crime drops from the late 1970s of over 40 percent.

The fact that these different statistical approaches—panel data regression, the LASSO, and synthetic controls—with varying strengths and shortcomings and with different model specifications all suggest that RTC laws increase violent crime constitutes persuasive evidence that any beneficial effects from gun carrying are outweighed by the increases in violent crime that these laws stimulate.

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

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

Appendix

Appendix A: RTC Adoption Dates

Table A1 shows the date of adoption for all RTC states and identifies those without RTC laws. An RTC adoption year of 0 indicates that a state did not adopt an RTC law between 1977 and 2014. If the fraction of year in effect during the adoption year is less than 0.5, the RTC date used in the synthetic control analysis is the following year.

RTC dates before the year 1977 may not be exact, since differences between these dates would neither affect our regression results nor our synthetic control tables. We follow earlier convention in the academic literature on the RTC issue in assigning pre-1977 RTC adoption dates for Alabama and Connecticut. We also note that there has been a great deal of confusion over the proper date of Virginia's RTC law, which we place in 1995, while Lott and Mustard (1997) had used 1988. If one looks at the data on concealed carry permit applications presented in the relevant Virginia State of the Judiciary Reports, it is clear from the large jump in permit applications in 1995 that that is the preferable date.[78]

[78] See, "The Virginia 1999 State of the Judiciary Report" (1999).

Table A1: RTC Adoption Dates

State	Effective Date of RTC Law	Fraction of Year In Effect Year of Passage	RTC (Date in Synthetic Controls Analysis)
Alabama	1975		1975
Alaska	10/1/1994	0.252	1995
Arizona	7/17/1994	0.460	1995
Arkansas	7/27/1995	0.433	1996
California	N/A		0
Colorado	5/17/2003	0.627	2003
Connecticut	1970		1970
Delaware	N/A		0
District of Columbia	N/A		0
Florida	10/1/1987	0.252	1988
Georgia	8/25/1989	0.353	1990
Hawaii	N/A		0
Idaho	7/1/1990	0.504	1990
Illinois	1/5/2014		2014
Indiana	1/15/1980	0.962	1980
Iowa	1/1/2011	1.000	2011
Kansas	1/1/2007	1.000	2007
Kentucky	10/1/1996	0.251	1997
Louisiana	4/19/1996	0.702	1996
Maine	9/19/1985	0.285	1986
Maryland	N/A		0
Massachusetts	N/A		0
Michigan	7/1/2001	0.504	2001
Minnesota	5/28/2003	0.597	2003
Mississippi	7/1/1990	0.504	1990
Missouri	2/26/2004	0.847	2004
Montana	10/1/1991	0.252	1992
Nebraska	1/1/2007	1.000	2007
Nevada	10/1/1995	0.252	1996
New Hampshire	1959		1959
New Jersey	N/A		0
New Mexico	1/1/2004	1.000	2004
New York	N/A		0
North Carolina	12/1/1995	0.085	1996
North Dakota	8/1/1985	0.419	1986
Ohio	4/8/2004	0.732	2004
Oklahoma	1/1/1996	1.000	1996
Oregon	1/1/1990	1.000	1990
Pennsylvania	6/17/1989	0.542	1989
Philadelphia	10/11/1995	0.225	1996
Rhode Island	N/A		0
South Carolina	8/23/1996	0.358	1997
South Dakota	7/1/1985	0.504	1985
Tennessee	10/1/1996	0.251	1997
Texas	1/1/1996	1.000	1996
Utah	5/1/1995	0.671	1995
Vermont	1970		1970
Virginia	5/5/1995	0.660	1995
Washington	1961		1961
West Virginia	7/7/1989	0.488	1990
Wisconsin	11/1/2011	0.167	2012
Wyoming	10/1/1994	0.252	1995

Appendix B: Complete Regression Output

Table A2: Panel Data Violent Crime Coefficients using DAW, BC, LM, and MM models, State and Year Fixed Effects

Panel A: Dummy Variable Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law	9.02*** (2.90)	10.90*** (3.66)	−1.38 (3.16)	0.69 (0.77)
Lagged Incarceration Rate	0.04* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		24.64** (9.68)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		4.54 (13.78)		
Lagged Arrest Rate for Violent Crimes			−0.16** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				87.12*** (1.45)
Real Per Capita Personal Income (×100)	0.00 (0.00)		0.00* (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance (×100)			0.00 (0.01)	0.01** (0.01)
Real Per Capita Income Maintenance			0.04 (0.03)	0.02** (0.01)
Real Per Capita Retirement Payments and Other (Lott version) (×100)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version) (×100)				−0.00* (0.00)
Nominal Per Capita Income		−0.00 (0.00)		
Unemployment Rate	−0.02 (0.78)	−1.18* (0.69)		−0.37 (0.23)
Poverty Rate	−0.32 (0.49)			−0.12 (0.09)
Lagged Number of Executions		0.08 (0.16)		
Beer	60.82*** (17.55)	69.61*** (18.61)		
Population			0.00 (0.00)	−0.00 (0.00)
Percent of the population living in MSAs	1.10*** (0.32)			
Population Density			−0.01 (0.02)	
Observations	1823	1874	1896	1781

Panel B: Spline Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law (Change in Trend)	0.01 (0.64)	0.16 (0.66)	0.41 (0.47)	0.17** (0.08)
Pre-Passage Trend for Changer States	0.92* (0.49)	0.98* (0.53)	0.12 (0.39)	−0.07 (0.08)
Lagged Incarceration Rate	0.03* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		21.79*** (8.19)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		3.36 (13.72)		
Lagged Arrest Rate for Violent Crimes			−0.17** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				86.65*** (1.46)
Real Per Capita Personal Income (×100)	0.00 (0.00)		0.00** (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance (×100)			−0.00 (0.02)	0.01 (0.01)
Real Per Capita Income Maintenance			0.03 (0.03)	0.01 (0.01)
Real Per Capita Retirement Payments and Other (Lott version) (×100)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version) (×100)				−0.00** (0.00)
Nominal Per Capita Income		0.00 (0.00)		
Unemployment Rate	0.52 (0.87)	−0.45 (0.82)		−0.28 (0.23)
Poverty Rate	−0.42 (0.50)			−0.15 (0.10)
Lagged Number of Executions		0.13 (0.17)		
Beer	62.09*** (16.18)	65.25*** (15.63)		
Population			0.00 (0.00)	−0.00* (0.00)
Percent of the population living in MSAs	0.92*** (0.29)			
Population Density			0.00 (0.02)	
Observations	1823	1874	1896	1781

Estimations include year and state fixed effects and are weighted by state population. Coefficients on demographic variables and the constant omitted. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The crime data is from the Uniform Crime Reports (UCR). * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms. The DAW model is run on data from 1979-2014, the BC model from 1978-2014, the LM model from 1977-2014, and the MM model (without the crack cocaine index) from 1979-2014.

Appendix C: Synthetic Control Estimates of the Impact of RTC Laws on Murder and Property Crime for Four Different Models

Table A3: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.784	-2.195	-1.584	-2.635	-6.032	-6.412	-6.413	1.438	-0.498	4.302
	(1.931)	(4.189)	(4.573)	(4.722)	(5.034)	(4.504)	(5.272)	(4.927)	(4.355)	(3.683)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.788	0.566	0.710	0.638	0.298	0.318	0.358	0.840	0.942	0.578
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.130	0.156	0.190	0.156	0.176	0.192	0.192	0.188	0.190	0.216
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.072	0.082	0.114	0.102	0.118	0.126	0.122	0.122	0.124	0.142
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.024	0.024	0.028	0.044	0.044	0.040	0.046	0.056	0.046

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 murder rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.224	1.336	2.354	0.679	0.600	1.557	0.677	1.554	1.070	1.334
	(0.998)	(1.306)	(2.535)	(2.709)	(2.734)	(2.580)	(2.465)	(2.319)	(2.406)	(2.325)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.852	0.456	0.348	0.822	0.864	0.650	0.864	0.708	0.800	0.784
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.144	0.176	0.166	0.196	0.192	0.206	0.182	0.200	0.198	0.204
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.070	0.088	0.084	0.090	0.114	0.120	0.106	0.120	0.130	0.132
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.020	0.030	0.034	0.024	0.030	0.044	0.048	0.040	0.040

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 property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The Impact of RTC Laws on the Murder Rate, BC covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.921	-1.270	-0.337	-1.179	-3.673	-3.763	-4.178	4.225	2.295	6.435*
	(1.912)	(3.951)	(4.352)	(4.398)	(4.939)	(4.564)	(4.811)	(4.773)	(4.030)	(3.486)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.544	0.716	0.960	0.834	0.550	0.546	0.532	0.564	0.758	0.432
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.130	0.168	0.188	0.170	0.170	0.174	0.210	0.184	0.202	0.226
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.066	0.084	0.100	0.098	0.120	0.126	0.128	0.118	0.130	0.162
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.022	0.028	0.030	0.044	0.044	0.048	0.046	0.058	0.048

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 murder rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: The Impact of RTC Laws on the Property Crime Rate, BC covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.347	1.212	2.318	0.545	0.413	1.424	0.517	1.488	1.009	1.054
	(1.020)	(1.329)	(2.534)	(2.702)	(2.747)	(2.586)	(2.522)	(2.338)	(2.377)	(2.279)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.756	0.514	0.350	0.856	0.908	0.678	0.894	0.722	0.800	0.828
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.140	0.156	0.154	0.182	0.188	0.208	0.176	0.200	0.202	0.206
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.080	0.090	0.086	0.100	0.110	0.130	0.116	0.120	0.132	0.126
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.030	0.030	0.036	0.024	0.032	0.034	0.052	0.044	0.040

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 property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: The Impact of RTC Laws on the Murder Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.107	-4.355	-2.770	-3.382	-5.337	-3.972	-4.913	2.498	1.501	4.542
	(1.713)	(4.166)	(4.501)	(4.661)	(5.323)	(5.155)	(5.484)	(5.562)	(5.019)	(4.141)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.962	0.236	0.556	0.510	0.350	0.514	0.462	0.750	0.824	0.598
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.132	0.148	0.154	0.164	0.182	0.182	0.170	0.172	0.218	0.220
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.078	0.088	0.074	0.088	0.090	0.106	0.102	0.114	0.132	0.142
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.022	0.018	0.024	0.036	0.038	0.026	0.036	0.034	0.040	0.044

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 murder rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: The Impact of RTC Laws on the Property Crime Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.208	1.262	2.211	1.039	0.077	1.099	1.525	3.218	2.544	3.420
	(1.005)	(1.163)	(2.616)	(2.688)	(2.719)	(2.575)	(2.387)	(2.380)	(2.719)	(3.050)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.836	0.446	0.346	0.714	0.992	0.758	0.692	0.414	0.510	0.430
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.138	0.162	0.178	0.188	0.200	0.214	0.184	0.208	0.204	0.198
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.066	0.102	0.100	0.120	0.116	0.134	0.122	0.128	0.112	0.122
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.022	0.014	0.026	0.030	0.038	0.034	0.048	0.054	0.044	0.046

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 property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: The Impact of RTC Laws on the Murder Rate, MM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.785	-2.359	-1.162	-1.538	-3.798	-3.175	-2.909	3.156	2.642	5.876
	(1.774)	(3.987)	(4.179)	(4.266)	(4.571)	(4.428)	(4.431)	(4.464)	(4.155)	(4.071)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.578	0.556	0.772	0.722	0.496	0.586	0.670	0.654	0.712	0.480
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.128	0.162	0.164	0.166	0.160	0.170	0.172	0.186	0.206	0.218
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.090	0.086	0.098	0.092	0.100	0.102	0.118	0.122	0.144
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.030	0.026	0.022	0.020	0.030	0.032	0.034	0.028	0.050	0.058

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 murder rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: The Impact of RTC Laws on the Property Crime Rate, MM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.334	1.231	2.369	1.543	1.586	2.676	1.863	2.919	2.758	3.062
	(0.941)	(1.157)	(2.526)	(2.637)	(2.596)	(2.381)	(2.440)	(2.347)	(2.315)	(2.342)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.744	0.480	0.322	0.600	0.604	0.454	0.630	0.458	0.502	0.476
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.134	0.176	0.178	0.198	0.218	0.202	0.202	0.202	0.182	0.204
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.114	0.120	0.114	0.128	0.110	0.112	0.124	0.114	0.120
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.030	0.024	0.040	0.038	0.036	0.042	0.038	0.050	0.050	0.044

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 property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D: Synthetic Control Estimates of RTC Law Impact on Three Crimes Without Adoption Year Normalization (DAW)

Table A11: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.467	5.228**	6.241**	7.301**	9.503***	9.995**	12.715***	15.047***	16.613***	16.941***
	(1.689)	(2.066)	(2.576)	(2.927)	(3.241)	(3.798)	(3.507)	(4.605)	(4.278)	(3.724)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.618	0.314	0.248	0.192	0.100	0.104	0.050	0.030	0.020	0.018
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.270	0.278	0.258	0.266	0.240	0.280	0.278	0.284	0.288	0.274
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.178	0.180	0.182	0.182	0.160	0.184	0.204	0.188	0.186	0.188
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.068	0.072	0.080	0.086	0.066	0.080	0.066	0.074	0.070	0.062

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	3.722**	6.377***	7.588***	8.573***	10.996***	11.050***	13.773***	15.911***	16.873***	17.337***
	(1.552)	(1.980)	(2.455)	(2.851)	(3.111)	(3.771)	(3.451)	(4.621)	(4.355)	(3.777)
N	29	29	29	29	29	29	29	27	27	27
Pseudo P-Value	0.420	0.212	0.152	0.122	0.052	0.066	0.032	0.020	0.020	0.020
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.284	0.278	0.270	0.274	0.266	0.266	0.282	0.286	0.274	0.274
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.194	0.178	0.182	0.180	0.178	0.182	0.192	0.188	0.180	0.186
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.074	0.072	0.064	0.082	0.064	0.078	0.074	0.068	0.068	0.054

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS NC NE NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: MT ND SD WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	4.209**	6.994***	8.311***	9.367***	11.806***	11.812***	14.533***	16.492***	17.487***	17.893***
	(1.537)	(1.953)	(2.410)	(2.814)	(3.065)	(3.748)	(3.404)	(4.574)	(4.299)	(3.736)
N	27	27	27	27	27	27	27	26	26	26
Pseudo P-Value	0.292	0.116	0.078	0.070	0.030	0.050	0.028	0.016	0.020	0.018
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.216	0.234	0.252	0.252	0.250	0.258	0.262	0.260	0.262	0.262
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.160	0.142	0.146	0.152	0.158	0.164	0.174	0.180	0.166	0.170
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.066	0.046	0.054	0.060	0.056	0.062	0.062	0.048	0.058	0.050

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: ME MT ND NE SD WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	8.565*	5.573	6.173	5.105	1.691	1.298	1.294	8.489	6.554	11.184**
	(4.365)	(3.816)	(4.639)	(5.316)	(4.723)	(5.315)	(5.226)	(5.738)	(4.571)	(5.298)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.204	0.444	0.400	0.532	0.832	0.888	0.896	0.360	0.496	0.264
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.252	0.282	0.282	0.272	0.264	0.280	0.256	0.250	0.280	0.244
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.182	0.196	0.200	0.182	0.196	0.200	0.196	0.190	0.192	0.166
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.086	0.096	0.082	0.088	0.098	0.092	0.090	0.084	0.090	0.076

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 murder rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: The Impact of RTC Laws on the Murder Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	9.575**	6.497	7.204	6.173	2.607	2.928	2.906	10.800*	7.395	11.729**
	(4.533)	(3.903)	(4.825)	(5.529)	(4.885)	(5.358)	(5.292)	(5.764)	(4.750)	(5.516)
N	31	31	31	31	31	31	31	29	29	29
Pseudo P-Value	0.152	0.376	0.344	0.454	0.776	0.760	0.752	0.262	0.454	0.242
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.272	0.282	0.280	0.274	0.274	0.286	0.262	0.260	0.276	0.238
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.184	0.200	0.190	0.196	0.216	0.196	0.194	0.192	0.194	0.160
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.084	0.096	0.082	0.090	0.104	0.090	0.096	0.084	0.092	0.082

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 murder rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MO MS MT NC NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit: MN ND
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: The Impact of RTC Laws on the Murder Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	9.660***	2.942	4.538	2.263	-0.348	1.171	-0.088	10.838*	8.871*	14.020***
	(2.559)	(3.372)	(5.319)	(5.590)	(5.513)	(5.550)	(5.758)	(5.909)	(5.088)	(4.789)
N	21	21	21	21	21	21	21	21	21	21
Pseudo P-Value	0.120	0.720	0.586	0.804	0.972	0.920	0.992	0.290	0.390	0.184
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.260	0.276	0.312	0.286	0.290	0.290	0.282	0.254	0.268	0.238
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.184	0.190	0.200	0.196	0.206	0.210	0.212	0.174	0.190	0.162
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.068	0.092	0.082	0.088	0.094	0.094	0.090	0.088	0.088	0.088

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 murder rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AR AZ CO FL GA KY ME MI MO MS NC NM OH OK OR PA SC TN UT VA WV
States excluded for poor pre-treatment fit: AK ID KS LA MN MT ND NE NV SD TX WY
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	9.654**	11.289**	12.368**	10.740**	10.709*	11.725**	10.903**	11.698**	11.288**	11.606**
	(3.980)	(4.197)	(4.886)	(4.863)	(5.484)	(5.593)	(4.680)	(4.781)	(5.271)	(5.183)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.016	0.004	0.004	0.010	0.014	0.006	0.022	0.008	0.032	0.020
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.206	0.198	0.224	0.232	0.230	0.206	0.198	0.202	0.182	0.182
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.138	0.130	0.134	0.152	0.156	0.146	0.128	0.116	0.108	0.118
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.038	0.034	0.042	0.054	0.052	0.044	0.050	0.046	0.038	0.040

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 property crime rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	11.231***	12.864***	13.926***	12.195**	12.457**	13.402**	12.286**	13.218**	12.497**	12.904**
	(4.063)	(4.289)	(5.000)	(4.985)	(5.597)	(5.727)	(4.794)	(4.896)	(5.463)	(5.363)
N	30	30	30	30	30	30	30	28	28	28
Pseudo P-Value	0.000	0.000	0.000	0.000	0.000	0.000	0.004	0.004	0.014	0.016
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.186	0.156	0.194	0.192	0.206	0.212	0.188	0.182	0.170	0.168
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.114	0.094	0.112	0.116	0.144	0.124	0.124	0.100	0.106	0.098
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.026	0.026	0.032	0.036	0.040	0.040	0.038	0.044	0.036	0.036

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 property crime rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS LA ME MI MN MO MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A19: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	14.559***	15.329***	14.329***	11.856***	13.038**	14.723**	13.131***	14.659***	14.482**	15.266**
	(3.990)	(4.073)	(4.003)	(3.840)	(5.073)	(5.639)	(4.650)	(4.956)	(5.696)	(5.740)
N	23	23	23	23	23	23	23	22	22	22
Pseudo P-Value	0.000	0.000	0.000	0.002	0.002	0.000	0.002	0.004	0.004	0.010
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.184	0.162	0.164	0.168	0.172	0.182	0.178	0.192	0.158	0.154
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.096	0.088	0.084	0.088	0.096	0.106	0.122	0.104	0.100	0.104
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.028	0.014	0.016	0.024	0.024	0.030	0.024	0.024	0.028

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 property crime rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR CO FL GA ID KS LA ME MI MO MT NC NM NV OH OK OR SC TN UT VA WY
States excluded for poor pre-treatment fit: AZ KY MN MS ND NE PA SD TX WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the optimization technique described in our main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix E: Data Methodologies

I. Data Issues

The state-level data set used in this paper updated through 2014 earlier data sets used in Aneja, Donohue, and Zhang (2014) and Aneja, Donohue, and Zhang (2011). We further update this data set to incorporate changes to the various primary sources that have occurred since first released, and to include the additional predictor variables that are featured in the DAW and BC models. All variables are collected for the years 1977-2014 unless otherwise noted.[79]

Annual state-level crime rates are taken from the FBI's Uniform Crime Reporting program.[80] Four state-level income variables (personal income, income maintenance payments, retirement payments, and unemployment insurance payments) are taken from the BEA's Regional Economic Accounts. The personal income, income maintenance, and unemployment insurance payment variables are estimated in real per capita terms (defined using the CPI). The LM and MM specifications use alternative versions of the retirement variable that are described in footnote 79. State-level population and the proportional size of LM's 36 age-race-sex demographic groups are estimated using the Census Bureau's intercensal population estimates. (When the most recent form of these data were not accessible at the state level, state-level figures were generated by aggregating the Census Bureau's county-level population estimates by age, sex, and race.) Population density is estimated by dividing a state's population by the area of that state reported in the previous decennial census. State-level unemployment rate data is taken from the Bureau of Labor Statistics, while the poverty rate is taken from two Census series (the 1979 state-level poverty rate is derived from the Decennial Census and the 1980-2014 poverty rates are generated using the Current Population Survey). The percentage of population living in an MSA was constructed as a hybrid of two measures to account for shifts over time.[81] A measure of incarceration (incarcerated individuals per 100,000

[79] Many of the data sources that we used in our earlier analysis are revised continuously, and we use a newer version of these data series in this paper than we did in the earlier ADZ analysis. We sometimes made data changes during the data cleaning process. For instance, a detailed review of the raw data underlying arrest statistics uncovered a small number of agencies which reported their police staffing levels twice, and we attempted to delete these duplicates whenever possible. Moreover, we sometimes use variables that are defined slightly differently from the corresponding variable used in Lott and Mustard (1997) or Moody and Marvell (2008). For example, after examining the extension of Lott's county data set to the year 2000, we found that our estimates more closely approximated Lott's per capita retirement payment variable when we (a) used the total population as the denominator rather than population over 65 and (b) used as our numerator a measurement that includes retirement payments along with some other forms of government assistance. As a result, we use a modified retirement variable that incorporates these changes in the MM specification. Our retirement variable in the LM specification, in contrast, uses the population over 65 as a denominator and uses a tighter definition of retirement payments.

[80] For our main analysis, we formulate our crime rates by dividing FBI reported crime counts by FBI reported state-level populations. As a robustness check we used the rounded state-level crime rates reported by the FBI while using the DAW regressors and aggregate violent crime as an outcome variable. We find that this alternative crime rate definition does not qualitatively affect our findings.

[81] We use Census delineation and NBER population files to find the fraction of individuals residing in a county

state residents) is calculated from tables published by the Bureau of Justice Statistics counting the number of prisoners under the jurisdiction of different state penal systems. Our primary estimates for crime-specific state-level arrest rates are generated by adding together estimates of arrests by age, sex, and race submitted by different police agencies. We then divided this variable by the estimated number of incidents occurring in the same state (according to the UCR) in the relevant crime category.[82] We also use the index of crack cocaine usage constructed by Fryer et al. (2013) for our analysis, which is only available between the years 1980 and 2000, and therefore we drop this variable from the MM model when we estimate this model on data through 2014. Since we already include controls that incorporate information on the racial composition of individual states in our analysis, we use the unadjusted version of the crack index instead of the version that is adjusted to account for differences in state racial demographics. [83]

Abadie, Diamond, and Hainmueller (2010) emphasize that researchers may want to "[restrict] the comparison group to units that are similar to the exposed units [in terms of the predictors which are included in the model]" (496). Given that the District of Columbia had the highest per capita personal income, murder rate, unemployment rate, poverty rate, and population density at various points in our sample, Abadie's admonition would seem to support omitting the District as one of our potential control units.[84] Consequently, we decided to exclude the District of Columbia from the synthetic controls analysis owing to its status as a clear outlier whose characteristics are less likely to be meaningfully predictive for other geographic areas.We should note, however, that including DC in the synthetic controls analysis has little impact on our estimates showing that RTC laws increase violent crime.

We consider two separate police measures for the purposes of our analysis. Our reported re-

which at least partially overlap with an MSA in 1980 (some New England counties were assigned by town). Since MSA definitions shift over time, we use the UCR implied fraction of population living in an MSA beginning in 1981. Observations for states incorrectly reported as 0 percent MSA by UCR in those early years were replaced according to the 1980 definition with updated Census population estimates. These values jump due to MSA redefinition over time. When we checked the robustness of our panel results by replacing our percentage MSA definition with the predictions from state-specific second-order time trends to smooth out jumps (compare table A2), DAW right-to-carry dummy variable estimates for violent crime increased by 1.5 to 10.56 and spline estimates increased by 0.17 to 0.20.

[82]We chose this variable as the primary one that we would use in this analysis after confirming that this variable was more closely correlated with Lott's state-level arrest variables in the most recent data set published on his website (a data set which runs through the year 2005) than several alternatives that we constructed.

[83]No data for the crack cocaine index that we use was available for the District of Columbia, and our matching methodology does not allow the District of Columbia to be included in our analysis in specifications that include this variable as a predictor.

[84]Another advantage of excluding the District of Columbia from our sample is that the Bureau of Justice Statistics stops estimating the incarcerated population of the District of Columbia after the year 2001 owing to the transfer of the district's incarcerated population to the federal prison system and the DC Jail. While we have tried to reconstruct incarceration data for DC for these years using other data sources, the estimates resulting from this analysis were not, in our view, plausible substitutes for the BJS estimates we use for all other states. The raw data set that we use to gather information about state-level arrest rates is also missing a large number of observations from the District of Columbia's main police department, which further strengthens the case for excluding DC from our data set.

sults are based on the same police variable that we used in Aneja, Donohue, and Zhang (2014). To construct this variable, we take the most recent agency-level data provided by the FBI and use this information to estimate the number of full-time police employees present in each state per 100,000 residents. We fill in missing observations with staffing data from previous years in cases where the FBI chose to append this information to their agency entries, and we divide the resulting estimate of the total number of police employees by the population represented by these agencies. This variable, which was originally constructed for our regression analysis, has the advantage of not having any missing entries and is closely correlated (r = .96) with an alternative measure of police staffing generated by extrapolating missing police agency data based on the average staffing levels reported by agencies in the same year and type of area served (represented by a variable incorporating nineteen categories separating different types of suburban, rural, and urban developments.) As an alternative, we use data published by the Bureau of Justice Statistics on the number of full-time equivalent employees working for police agencies (figures that were also included in the data set featured in John R Lott and David B Mustard (1997)). (We do not rely on this variable in our main analysis owing to the large number of missing years present in this data set and owing to discrepancies in the raw data provided by the BJS, which sometimes needed to be corrected using published tables.) We find that our estimated average treatment effects for aggregate violent crime and the conclusions that we draw from these averages are qualitatively unaffected by substituting one police employment measure for another, which suggests that measurement error associated with our estimates of police activity is not driving our results.

II. The Dates of Adoption of RTC Laws

We use the same effective RTC dates used in Aneja, Donohue, and Zhang (2014) with one small modification. Owing to the fact that we are using annual panel data, the mechanics of the synthetic control methodology require us to specify a specific year for each state's RTC date. To take advantage of the information we have collected on the exact dates when RTC laws went into effect in each state, each state's effective year of passage is defined as the first year in which an RTC law was in effect for the majority of that year.[85] This causes some of the values of our RTC variable to shift by one year (for instance, Wisconsin's RTC date shifts from 2011 to 2012, since the state's RTC law took effect on November 1, 2011).[86]

While there have been numerous disagreements about the exact laws that should be used to

[85]A table showing each state's original adoption date and adjusted adoption date (for our Synthetic Controls Analysis) is shown in Table A1 of Appendix A.

[86]By default, we also take this adjustment into account when deciding which states adopt RTC laws within ten years of the treatment state's adoption of the given law. As a robustness check, we re-ran our aggregate violent crime codes under the DAW specification without considering the modified RTC dates in our selection of control units, finding that this change did not affect our qualitative findings meaningfully.

determine when states made the transition from a "may issue" to a "shall issue" state, we believe that the dates used in this paper accurately reflect the year when different states adopted their RTC law. We supplemented our analysis of the statutory history of RTC laws in different states with an extensive search of newspaper archives to ensure that our chosen dates represented concrete changes in concealed carry policy. We document the changes that were made to our earlier selection of right-to-carry dates and the rationales underlying these changes in Appendix G of Aneja, Donohue, and Zhang (2014). The coding of these dates may not reflect administrative or logistical delays that may have prevented the full implementation of an RTC law after authorities were legally denied any discretion in rejecting the issuing of RTC permits. Ideally, a researcher would be able to control for the actual level of RTC permits in existence each year for each state. Although this data would be preferable to a mere indicator variable for the presence of an RTC law, such comprehensive information unfortunately is not available.

Appendix F: Replicating Our Analysis

In implementing the synthetic control technique, one can find the selection of the synthetic control to be sensitive to seemingly inconsequential details when using maximum likelihood to select the weights associated with different predictors in our analysis. Specifically, when using the excellent "synth" package for Stata created by Abadie, Hainmueller, and Diamond along with the *nested* option (which implements the optimization technique described in footnote 42), the version of Stata (e.g., SE vs. MP), the specifications of the computer running the command, and the order in which predictors are listed can affect the composition of the synthetic control and by extension the size of the estimated treatment effect.

The root cause of the differences between Stata versions is explained by a 2008 StataCorp memo, which noted that:

> "When more than one processor is used in Stata/MP, the computations for the likelihood are split into pieces (one piece for each processor) and then are added at the end of the calculation on each iteration. Because of round-off error, addition is not associative in computer science as it is in mathematics. This may cause a slight difference in results. For example, a1+a2+a3+a4 can produce different results from (a1+a2)+(a3+a4) in numerical computation. When changing the number of processors used in Stata, the order in which the results from each processor are combined in calculations may not be the same depending on which processor completes its calculations first."[87]

[87] This memo can be found at the following link: http://www.webcitation.org/6YeLV03SN.

Moreover, this document goes on to note that the differences associated with using different versions of Stata can be minimized by setting a higher threshold for *nrtolerance()*. This optimization condition is actually relaxed by the synth routine in situations where setting this threshold at its default level causes the optimization routine to crash, and we would therefore expect the results of Stata SE and MP to diverge significantly whenever this occurs. In our analysis, we use the UNIX version of Stata/MP owing to the well-documented performance gains associated with this version of the software package.

Another discrepancy that we encountered is that memory limitations sometimes caused our synthetic control analyses to crash when using the *nested* option. When this occurred, we would generate our synthetic control using the regression-based technique for determining the relative weights assigned to different predictors. We encountered this situation several times when running our Stata code on standard desktop computers, but this problem occurred less often when using more powerful computers with greater amounts of memory. For this reason, to replicate our results with the greatest amount of precision, we would recommend that other researchers run our code on the same machines that we ran our own analysis: a 24-core UNIX machine with 96GB of RAM or a 16-core UNIX machine with 64GB of RAM running Stata/MP.

Appendix G: Synthetic Control Graphs Estimating Impact of RTC Laws On Violent Crime Using the DAW Model[88]

Figures A1-A33

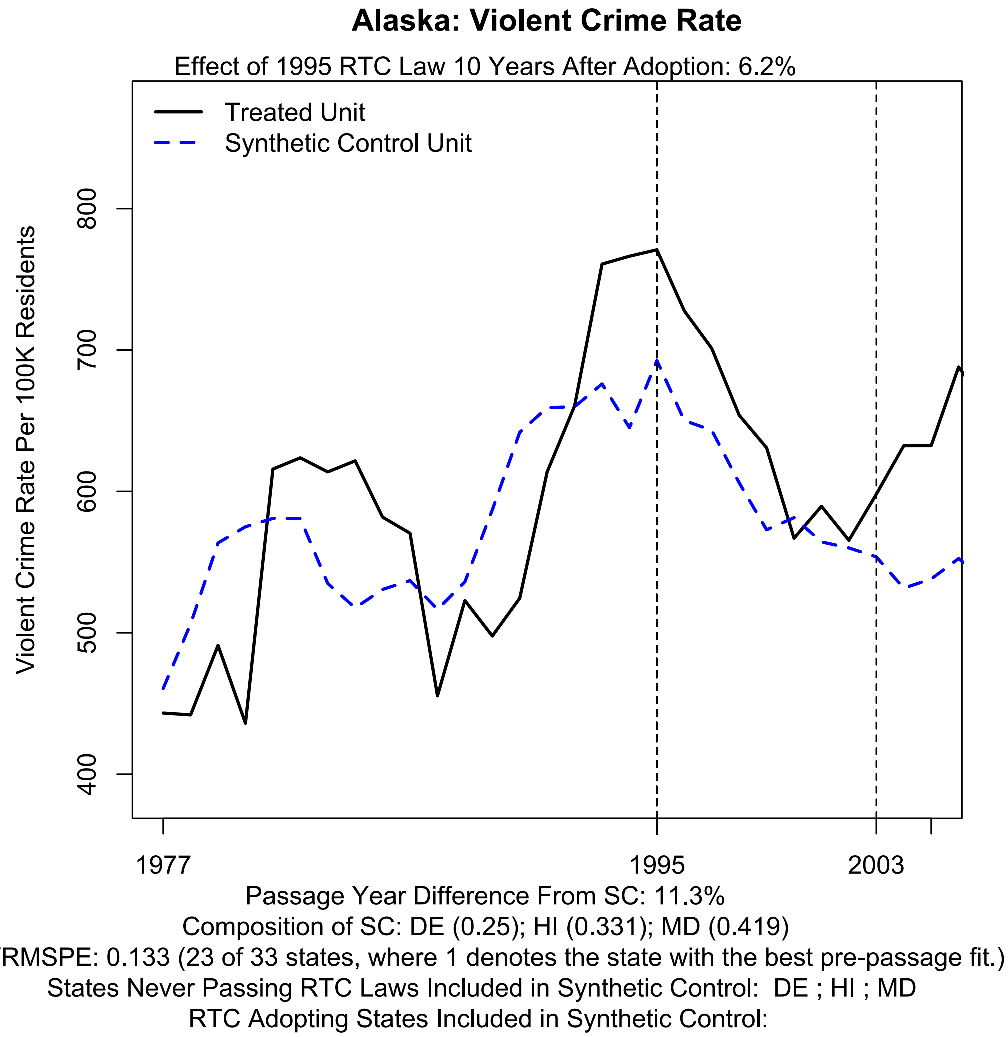
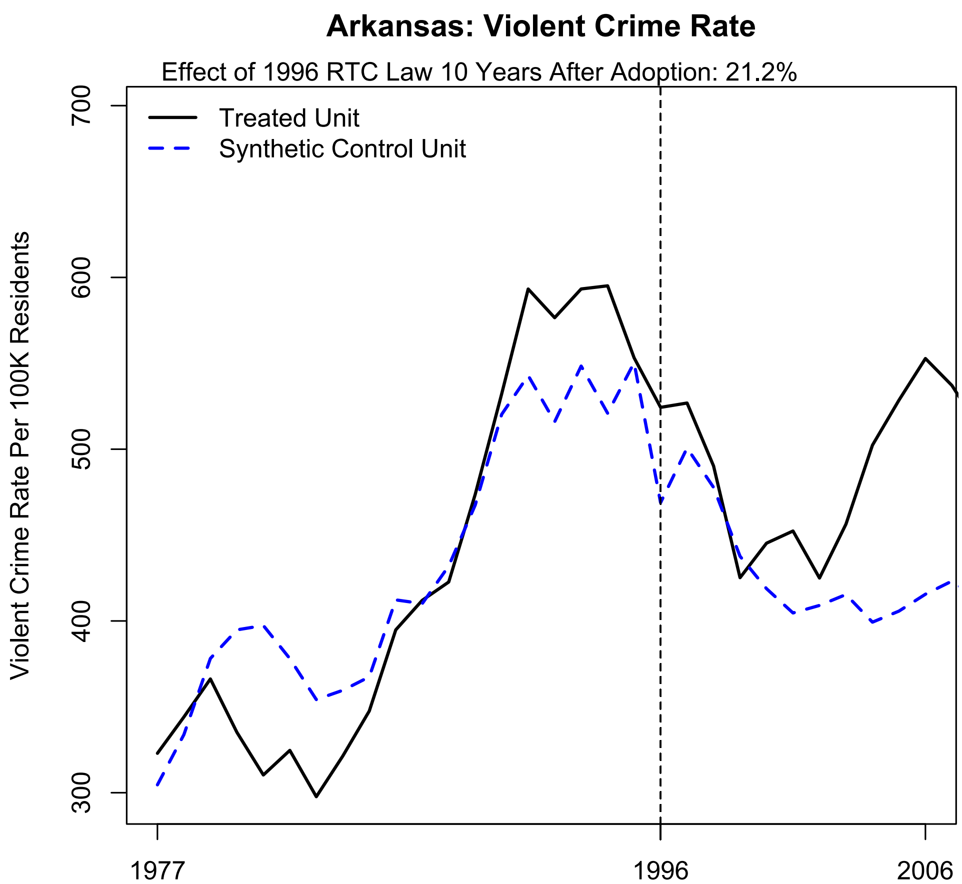


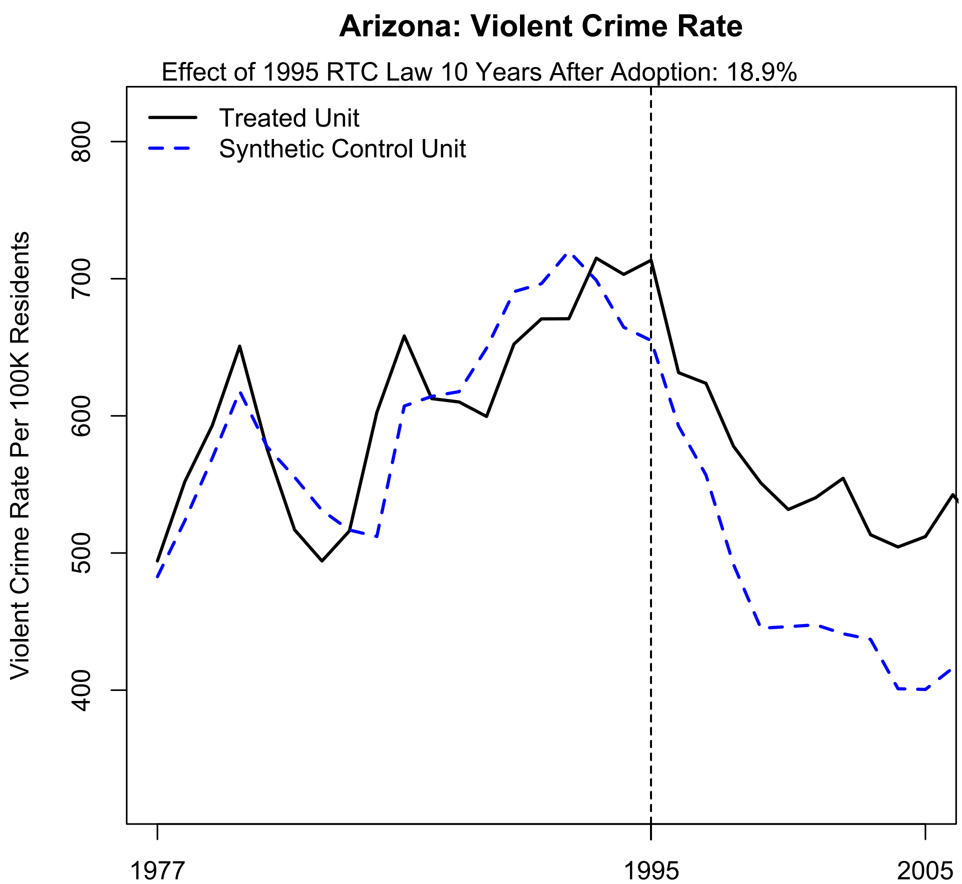
Figure A1

[88]Recall that each state's effective year of passage is defined as the first year in which an RTC law was in effect for the majority of that year.



Passage Year Difference From SC: 11.8%
Composition of SC: CA (0.205); DE (0.19); IA (0.604)
CVRMSPE: 0.099 (19 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control: IA (2011)

Figure A2



Passage Year Difference From SC: 8.9%
Composition of SC: CA (0.536); HI (0.464)
CVRMSPE: 0.062 (10 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI
RTC Adopting States Included in Synthetic Control:

Figure A3

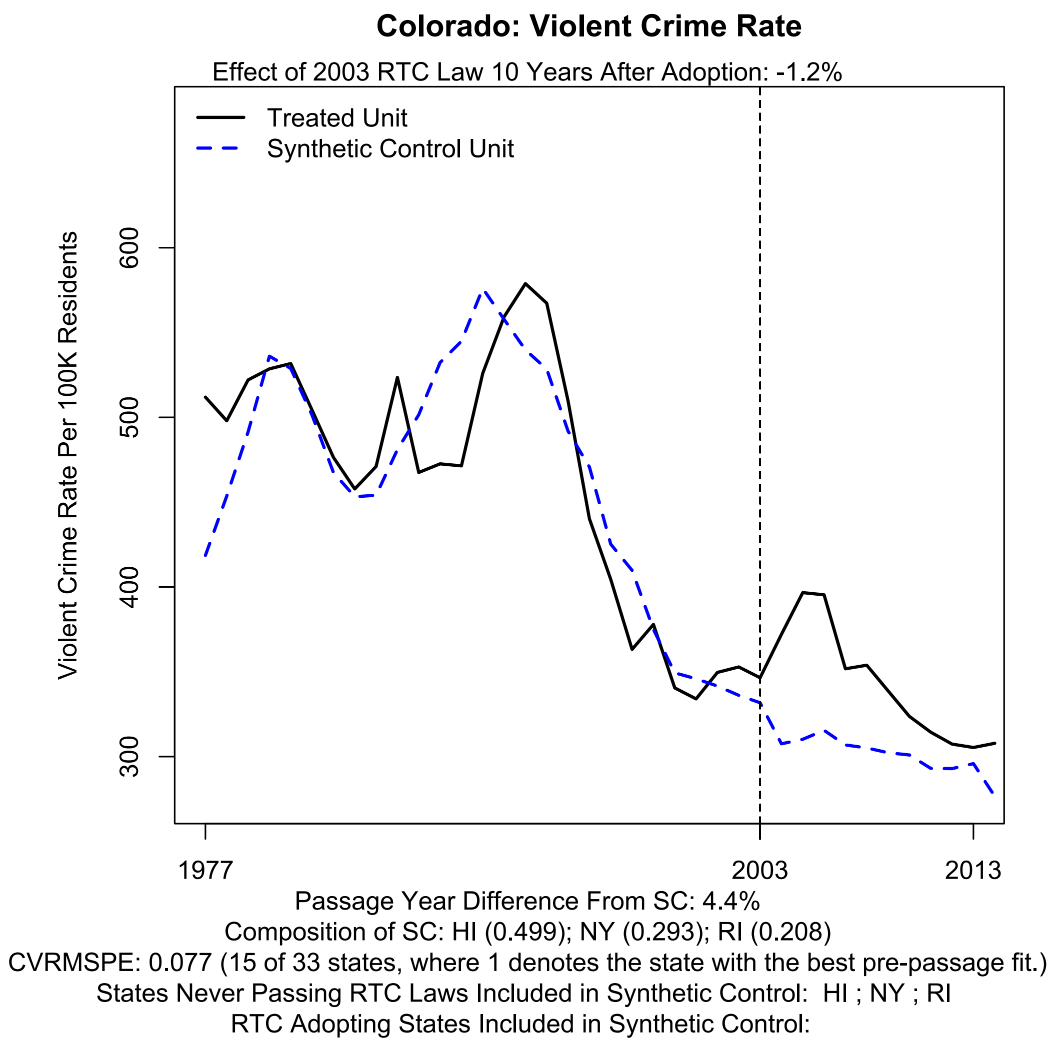
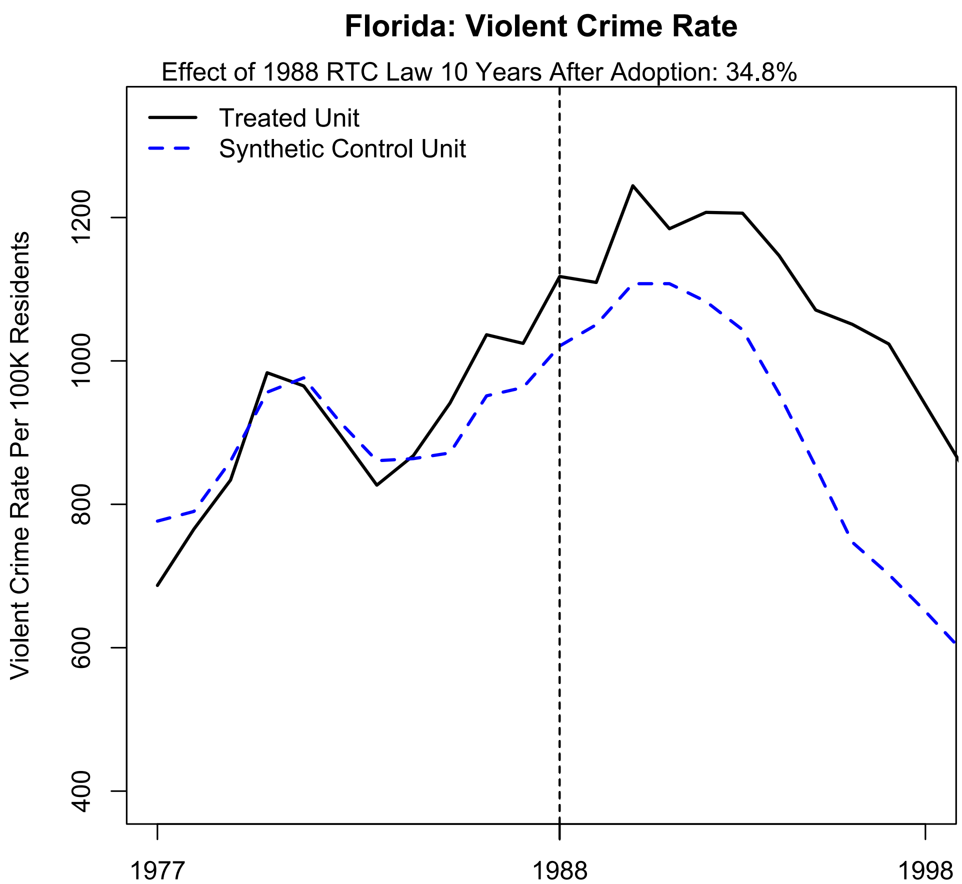
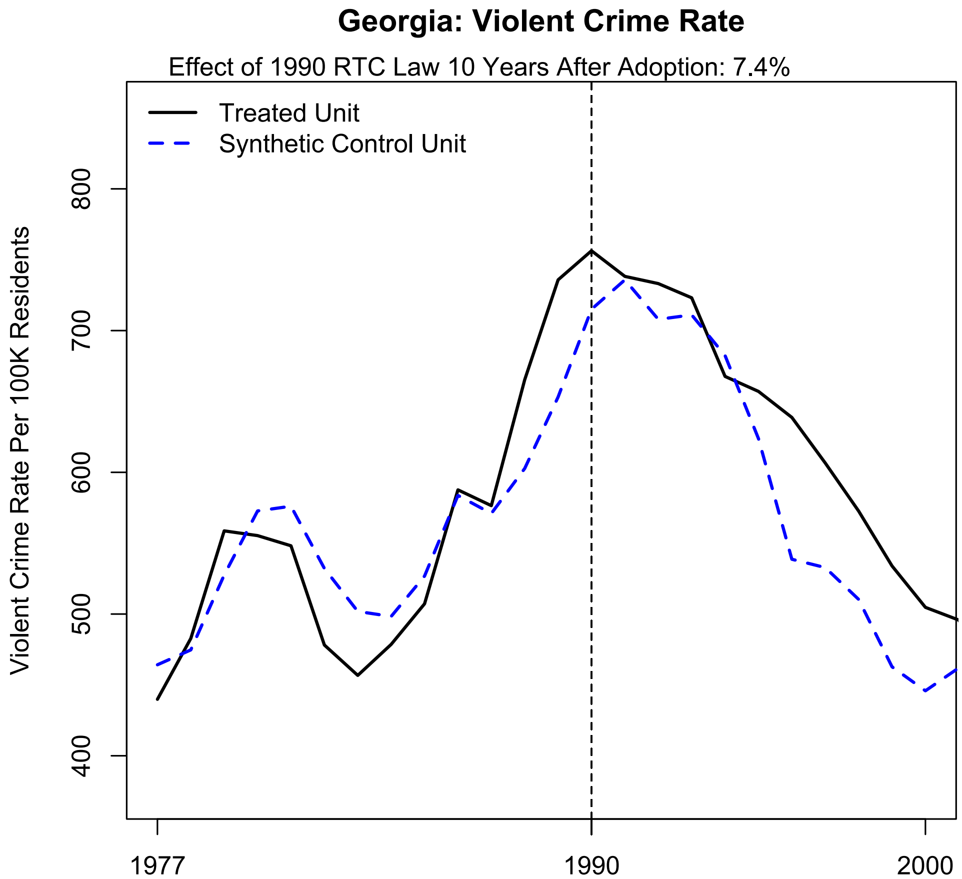

Colorado: Violent Crime Rate
Effect of 2003 RTC Law 10 Years After Adoption: -1.2%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
300
400
500
600
1977
2003
2013
Passage Year Difference From SC: 4.4%
Composition of SC: HI (0.499); NY (0.293); RI (0.208)
CVRMSPE: 0.077 (15 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; NY ; RI
RTC Adopting States Included in Synthetic Control:

Figure A4



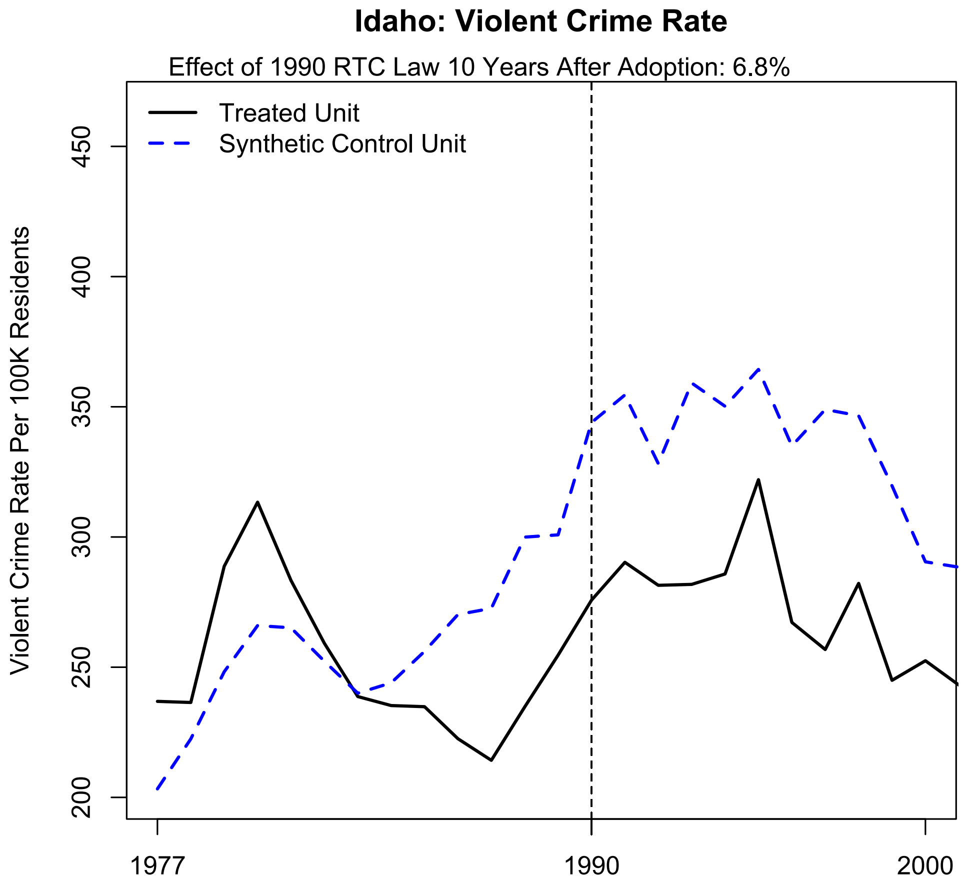
Passage Year Difference From SC: 9.5%
Composition of SC: CA (0.223); MI (0.11); NY (0.667)
CVRMSPE: 0.056 (7 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: MI (2001)

Figure A5



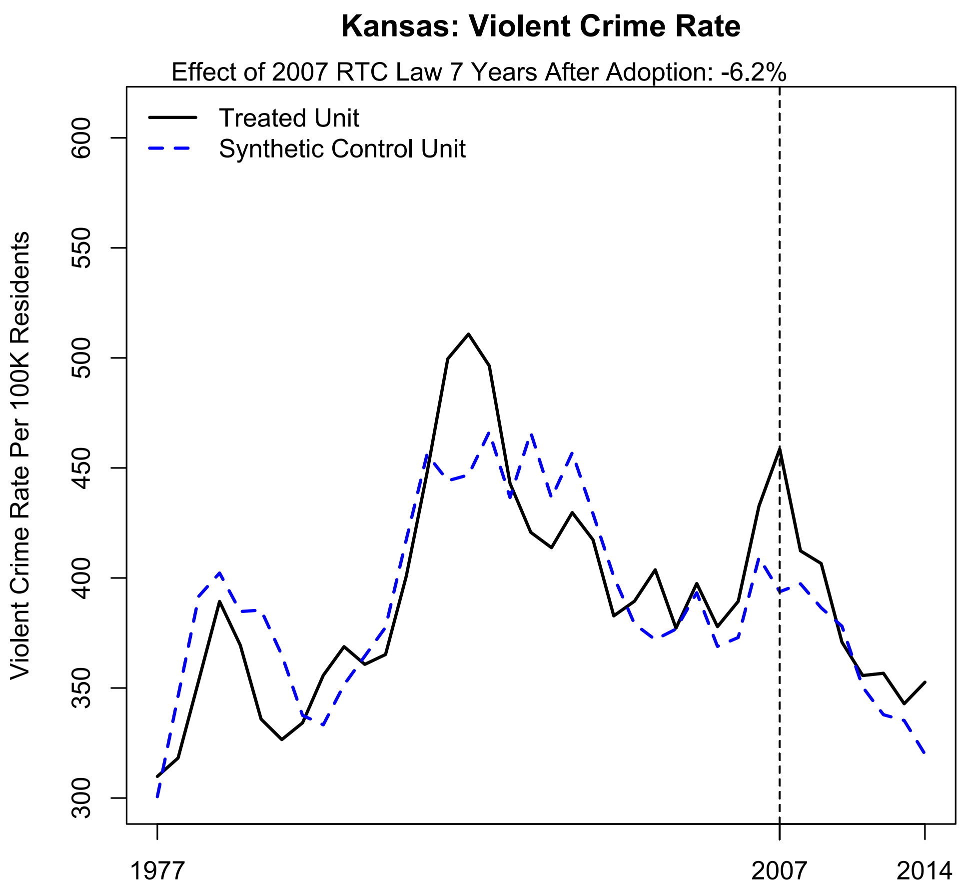
Passage Year Difference From SC: 5.8%
Composition of SC: IA (0.265); MO (0.499); NY (0.236)
CVRMSPE: 0.071 (13 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: NY
RTC Adopting States Included in Synthetic Control: IA (2011); MO (2004)

Figure A6



Passage Year Difference From SC: -19.8%
Composition of SC: HI (0.015); IA (0.45); NE (0.256); RI (0.278)
CVRMSPE: 0.149 (24 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic Control: IA (2011); NE (2007)

Figure A7

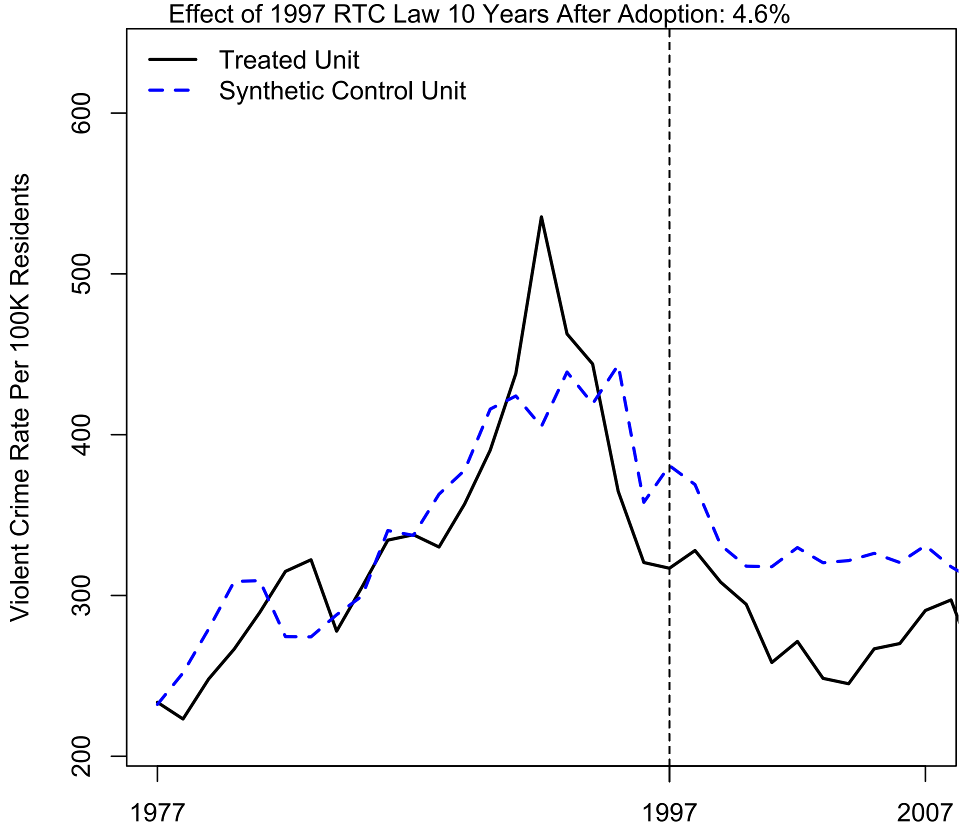


Passage Year Difference From SC: 16.5%
Composition of SC: DE (0.184); HI (0.583); MA (0.234)
CVRMSPE: 0.069 (12 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MA
RTC Adopting States Included in Synthetic Control:

Figure A8

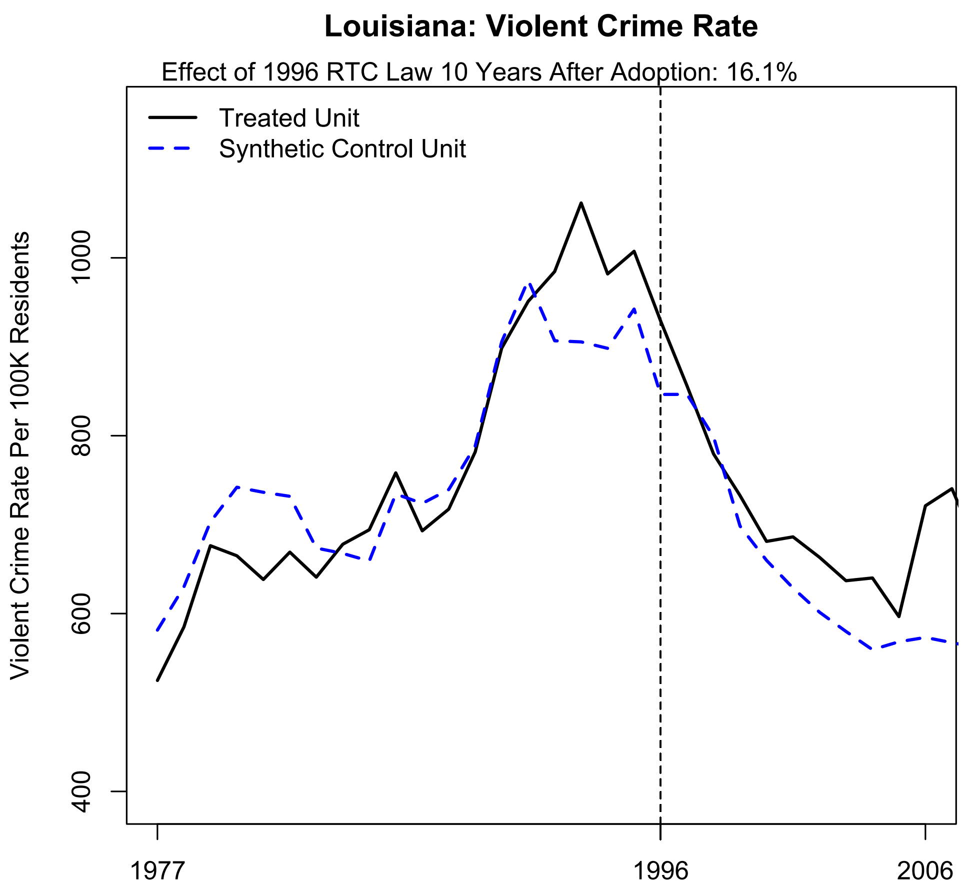
Kentucky: Violent Crime Rate

Effect of 1997 RTC Law 10 Years After Adoption: 4.6%



Passage Year Difference From SC: -16.8%
Composition of SC: CA (0.124); IA (0.849); NY (0.027)
CVRMSPE: 0.125 (22 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: IA (2011)

Figure A9



Passage Year Difference From SC: 9.8%
Composition of SC: DE (0.198); IL (0.802)
CVRMSPE: 0.08 (17 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014)

Figure A10

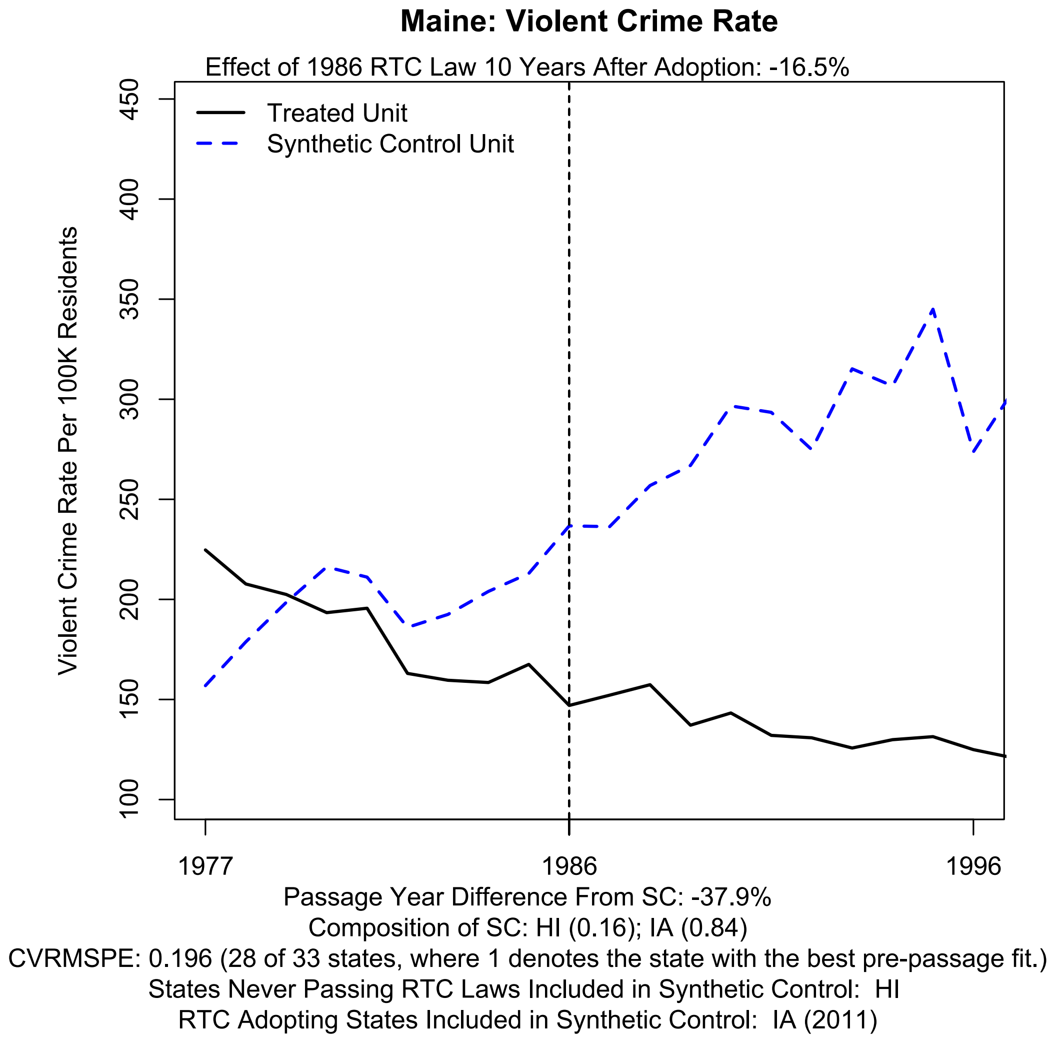
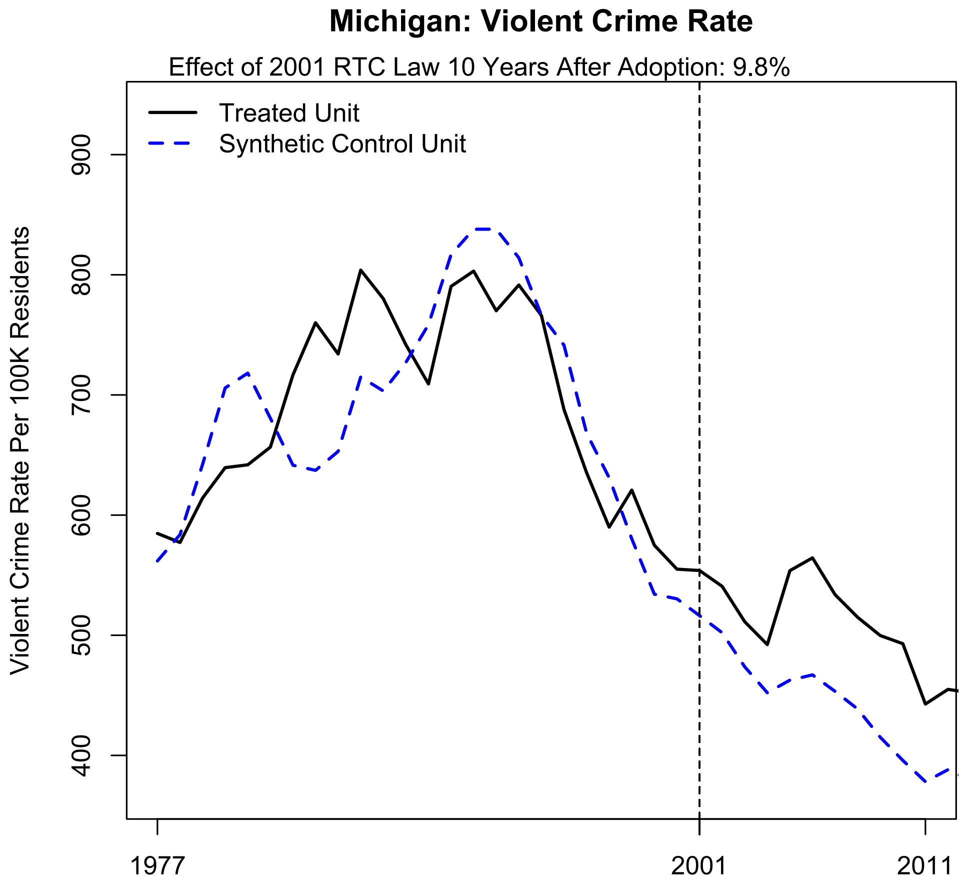
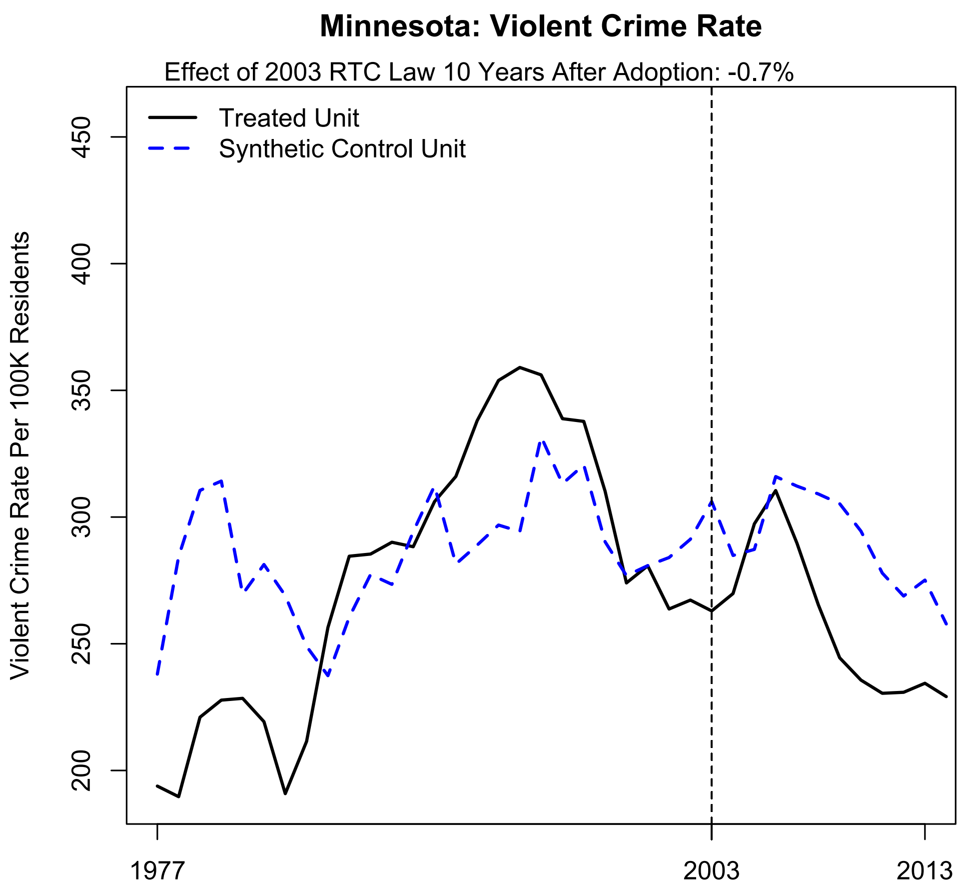

Maine: Violent Crime Rate
Effect of 1986 RTC Law 10 Years After Adoption: -16.5%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
450
400
350
300
250
200
150
100
1977
1986
1996
Passage Year Difference From SC: -37.9%
Composition of SC: HI (0.16); IA (0.84)
CVRMSPE: 0.196 (28 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011)

Figure A11



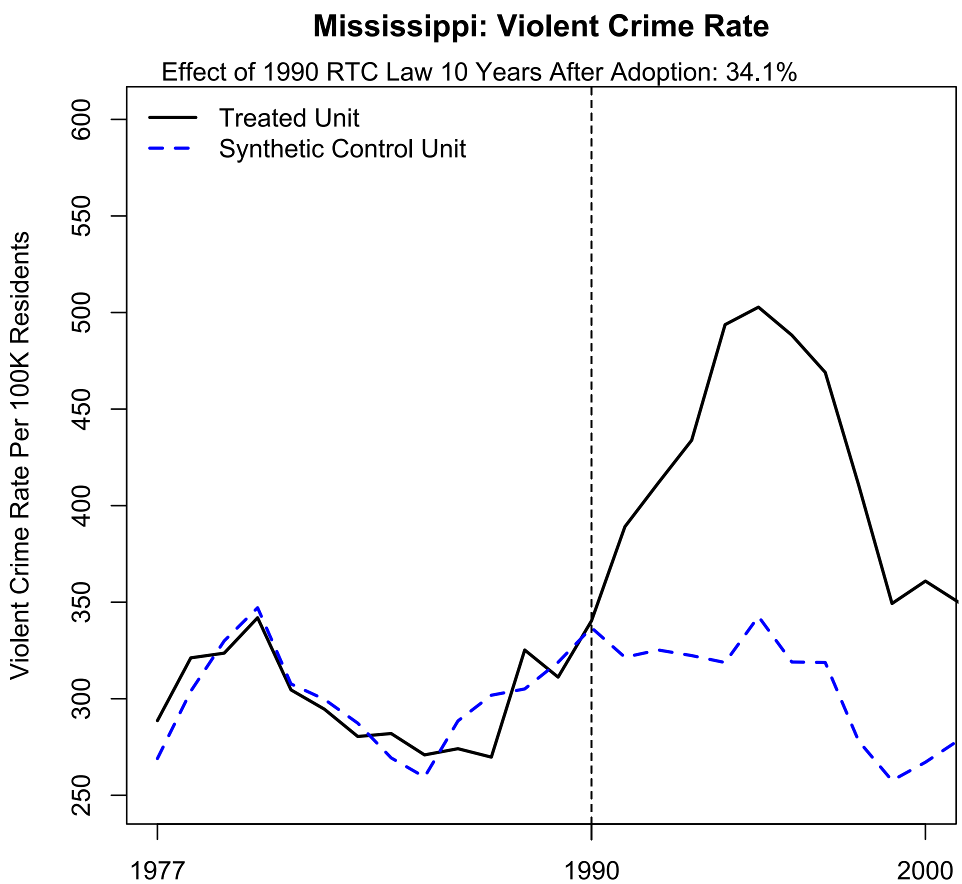
Passage Year Difference From SC: 7.3%
Composition of SC: CA (0.199); IL (0.054); MD (0.217); NY (0.231); RI (0.031); WI (0.268)
CVRMSPE: 0.08 (16 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; MD ; NY ; RI
RTC Adopting States Included in Synthetic Control: IL (2014); WI (2012)

Figure A12



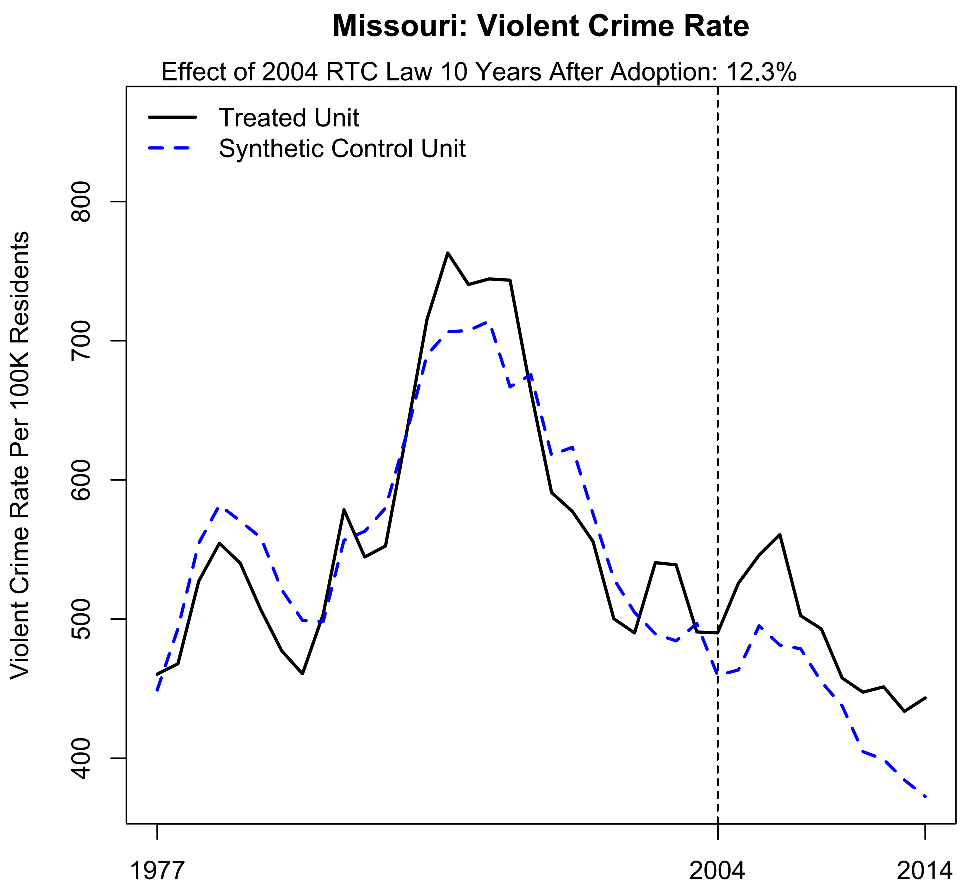
Passage Year Difference From SC: -14.1%
Composition of SC: DE (0.084); HI (0.916)
CVRMSPE: 0.166 (26 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic Control:

Figure A13



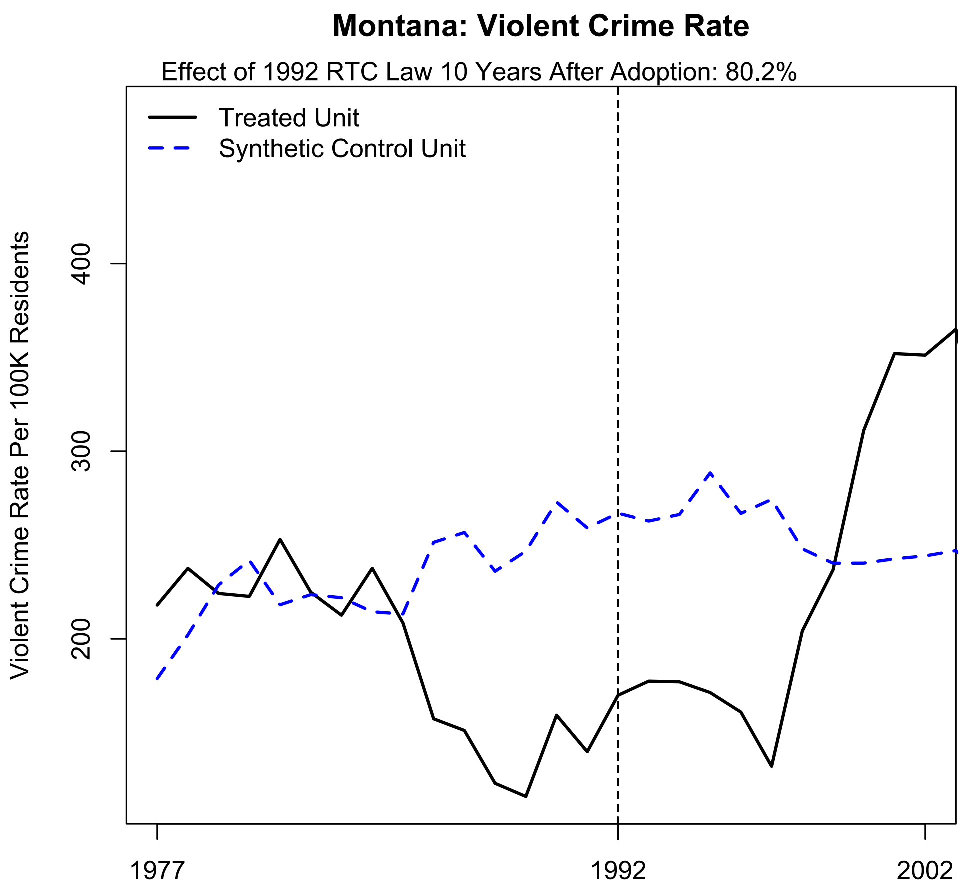
Passage Year Difference From SC: 1.1%
Composition of SC: HI (0.741); NE (0.014); OH (0.245)
CVRMSPE: 0.049 (5 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004)

Figure A14



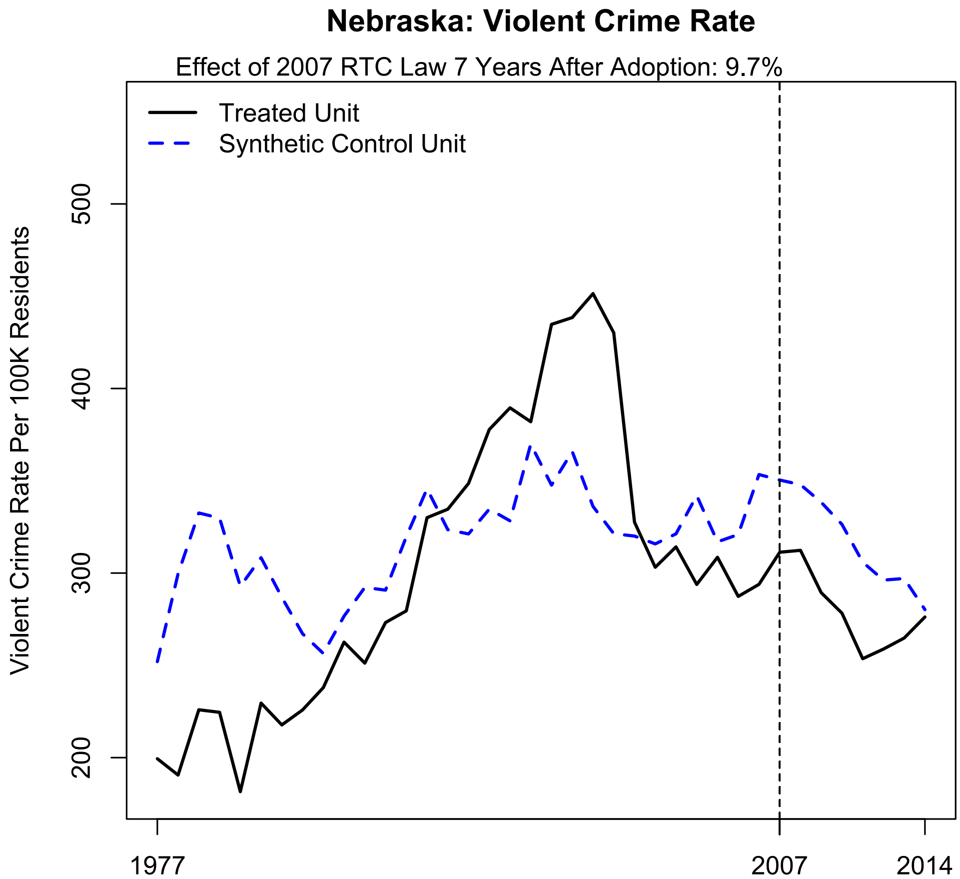
Passage Year Difference From SC: 6.7%
Composition of SC: CA (0.317); DE (0.225); HI (0.277); MA (0.181)
CVRMSPE: 0.061 (9 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE ; HI ; MA
RTC Adopting States Included in Synthetic Control:

Figure A15



Passage Year Difference From SC: -36.4%
Composition of SC: HI (0.507); WI (0.493)
CVRMSPE: 0.384 (31 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: WI (2012)

Figure A16



Passage Year Difference From SC: -11.2%
Composition of SC: DE (0.173); HI (0.827)
CVRMSPE: 0.208 (29 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
RTC Adopting States Included in Synthetic Control:

Figure A17

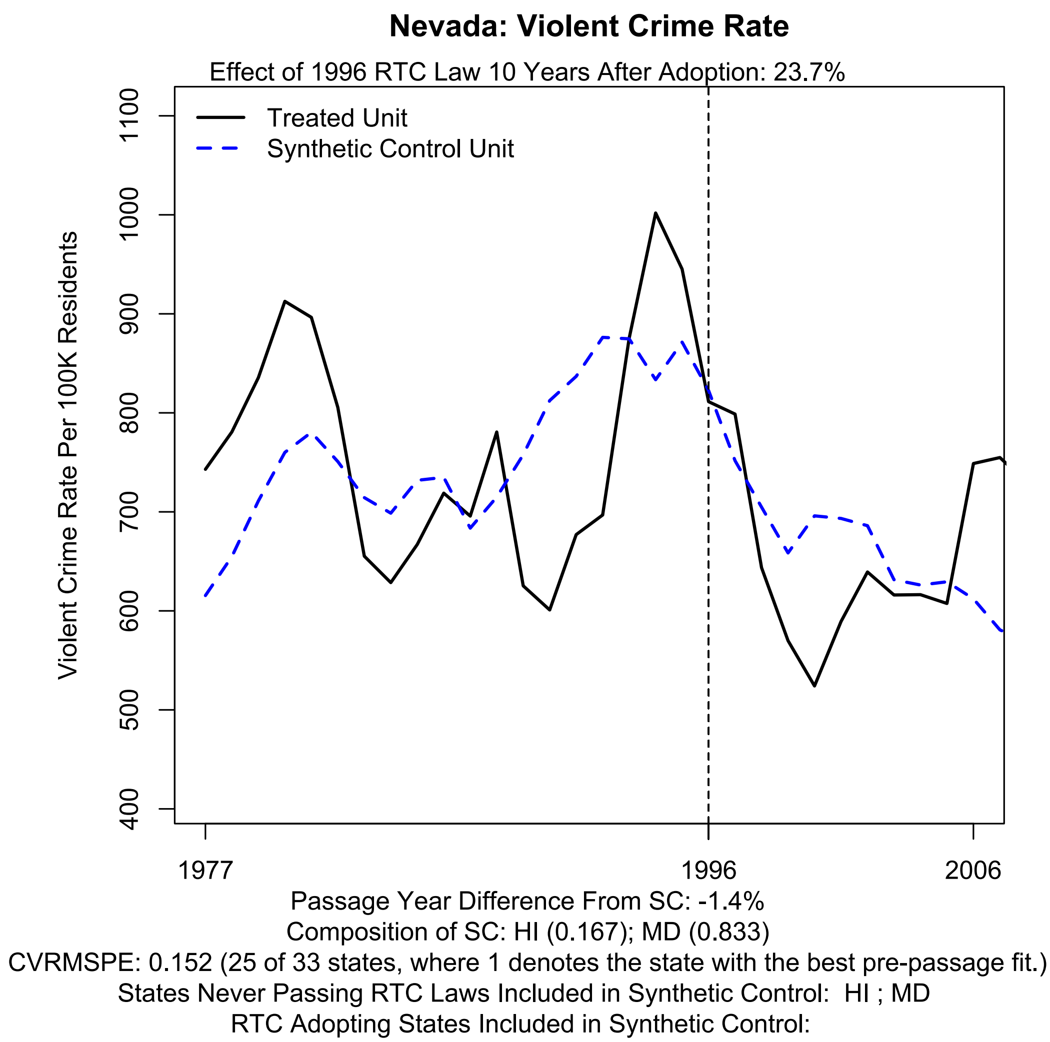

Nevada: Violent Crime Rate
Effect of 1996 RTC Law 10 Years After Adoption: 23.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
1100
1000
900
800
700
600
500
400
1977
1996
2006
Passage Year Difference From SC: -1.4%
Composition of SC: HI (0.167); MD (0.833)
CVRMSPE: 0.152 (25 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; MD
RTC Adopting States Included in Synthetic Control:

Figure A18

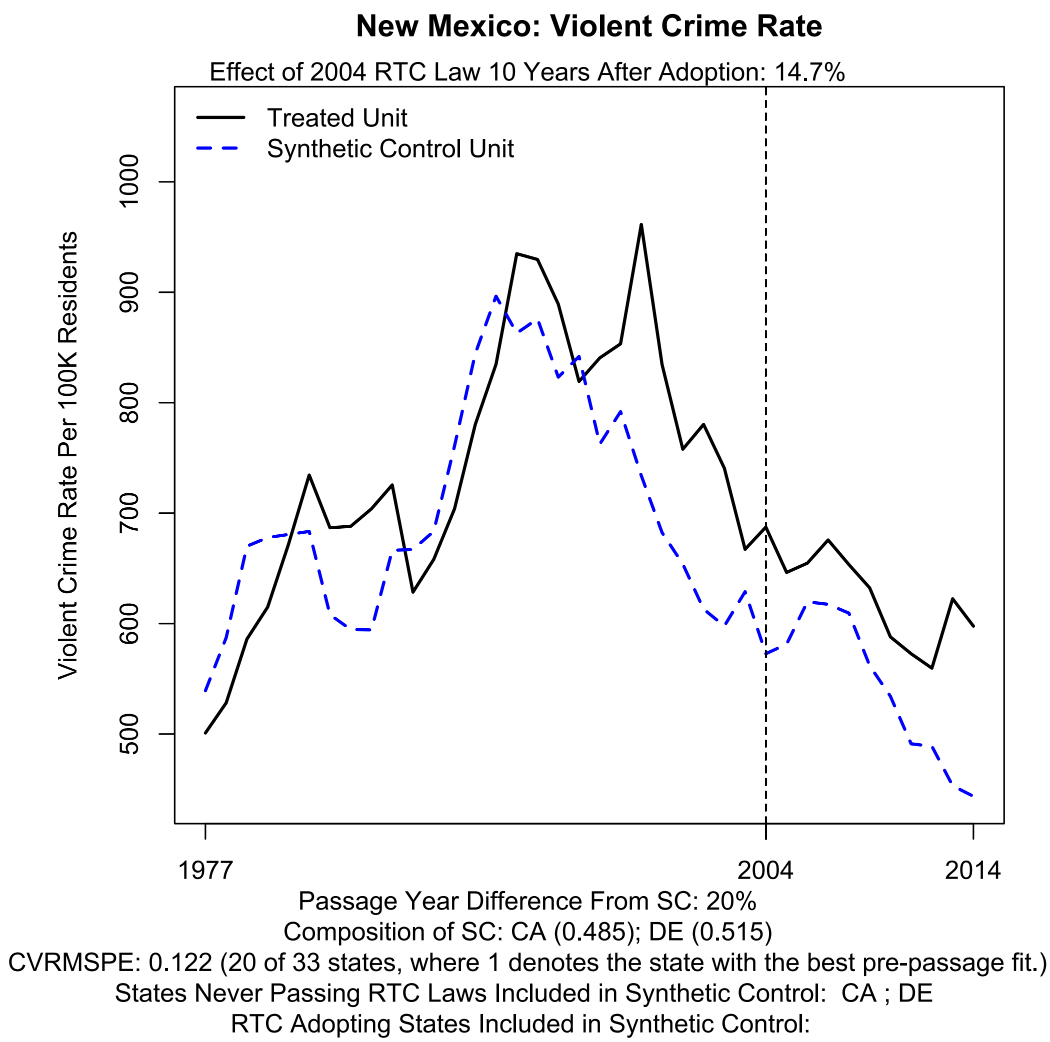
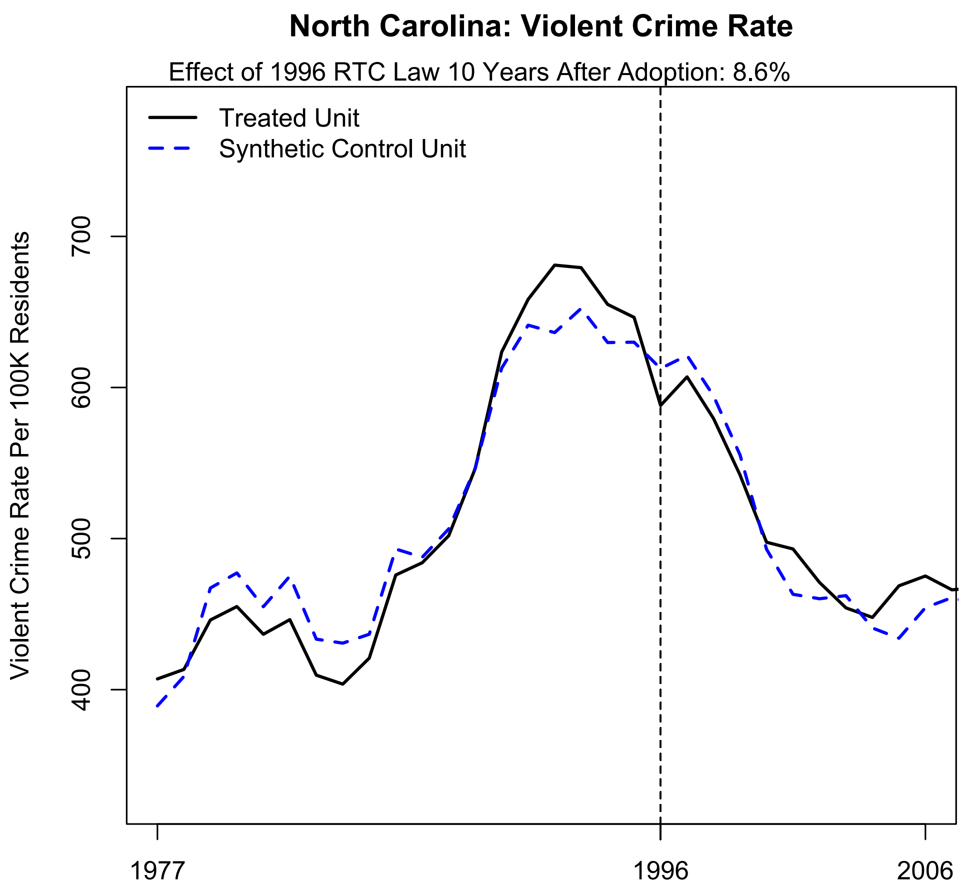
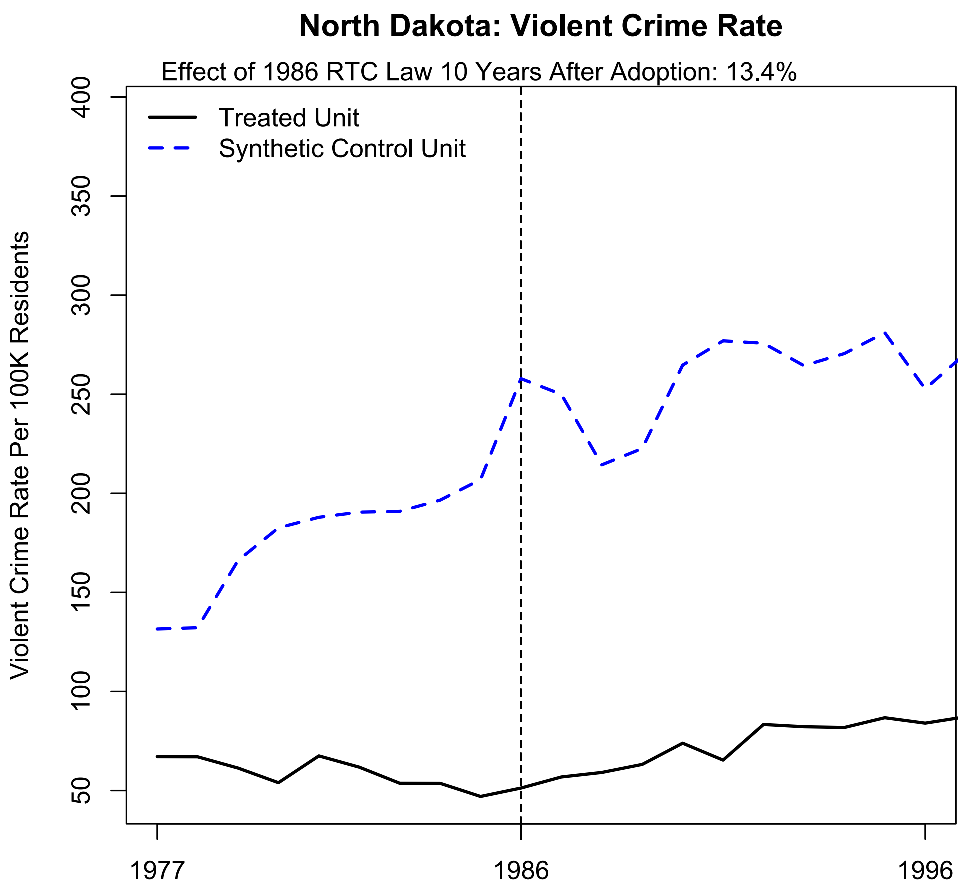

New Mexico: Violent Crime Rate
Effect of 2004 RTC Law 10 Years After Adoption: 14.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
500
600
700
800
900
1000
1977
2004
2014
Passage Year Difference From SC: 20%
Composition of SC: CA (0.485); DE (0.515)
CVRMSPE: 0.122 (20 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control:

Figure A19



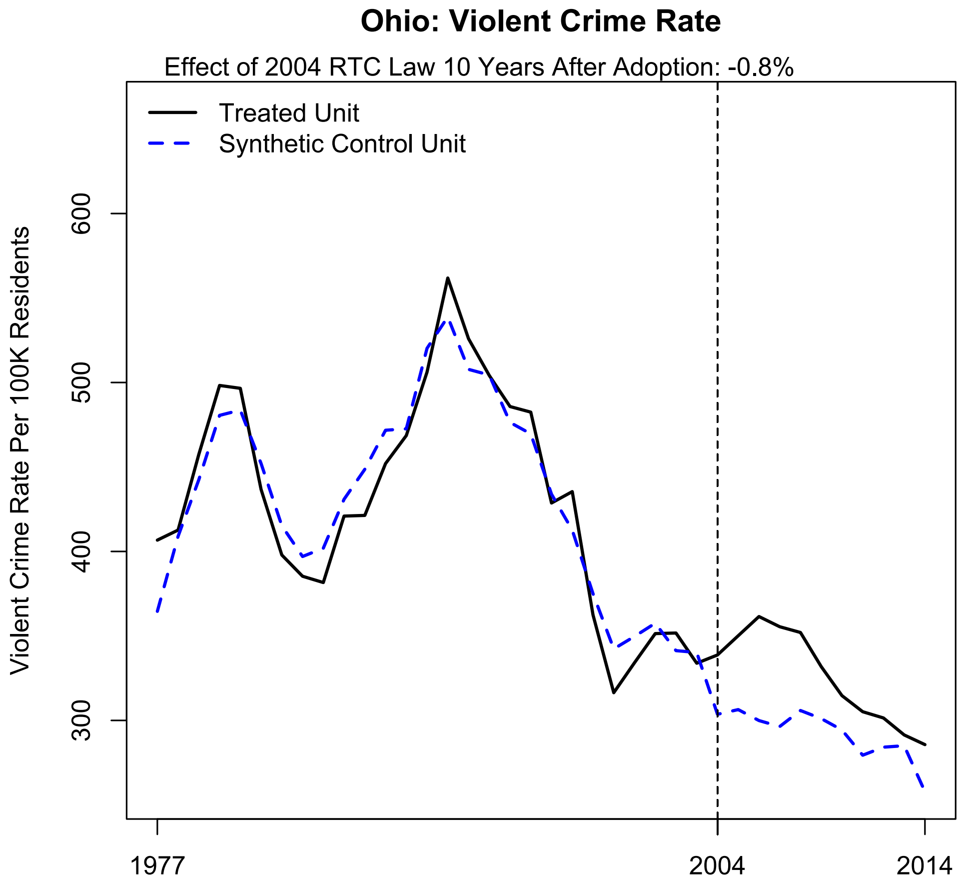
Passage Year Difference From SC: -4%
Composition of SC: CA (0.295); DE (0.22); NE (0.484)
CVRMSPE: 0.041 (3 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control: NE (2007)

Figure A20



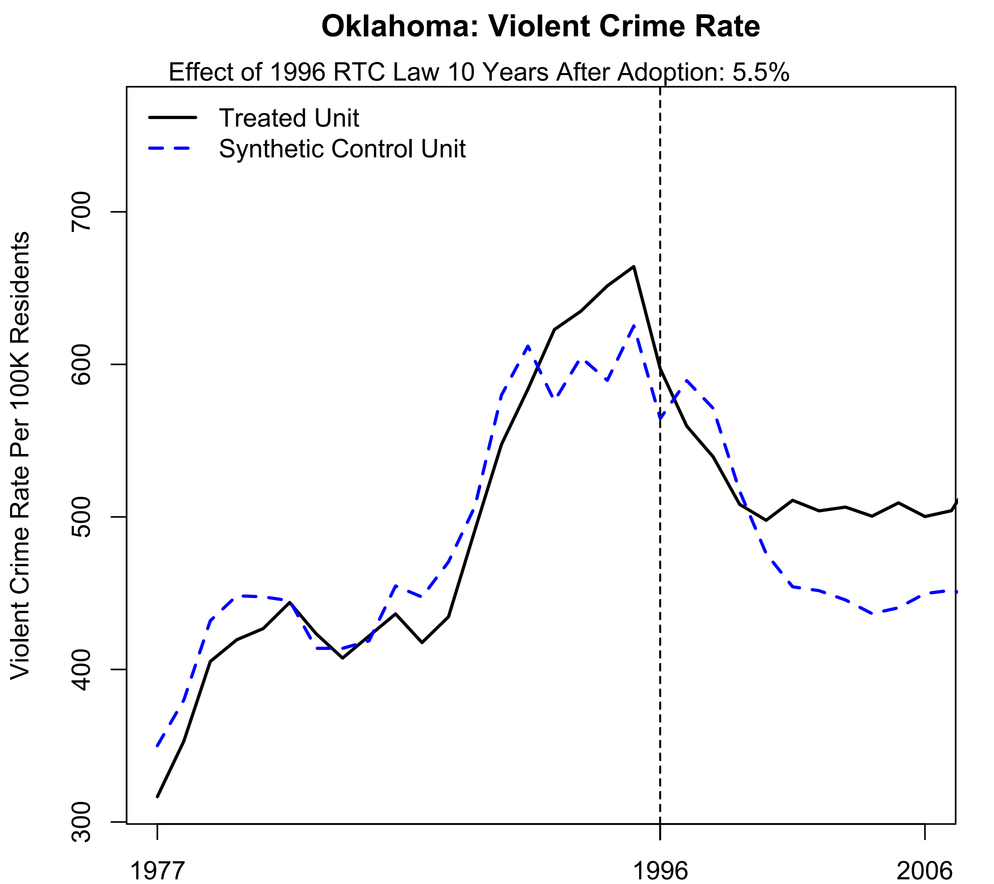
Passage Year Difference From SC: -80.1%
Composition of SC: WI (1)
CVRMSPE: 2.043 (33 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: WI (2012)

Figure A21



Passage Year Difference From SC: 11.6%
Composition of SC: CA (0.195); HI (0.207); RI (0.598)
CVRMSPE: 0.04 (2 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI ; RI
RTC Adopting States Included in Synthetic Control:

Figure A22



Passage Year Difference From SC: 5.8%

Composition of SC: DE (0.182); IL (0.259); IA (0.333); MD (0.026); MA (0.05); NE (0.15)

CVRMSPE: 0.063 (11 of 33 states, where 1 denotes the state with the best pre-passage fit.)

States Never Passing RTC Laws Included in Synthetic Control: DE ; MA ; MD

RTC Adopting States Included in Synthetic Control: IA (2011); IL (2014); NE (2007)

Figure A23

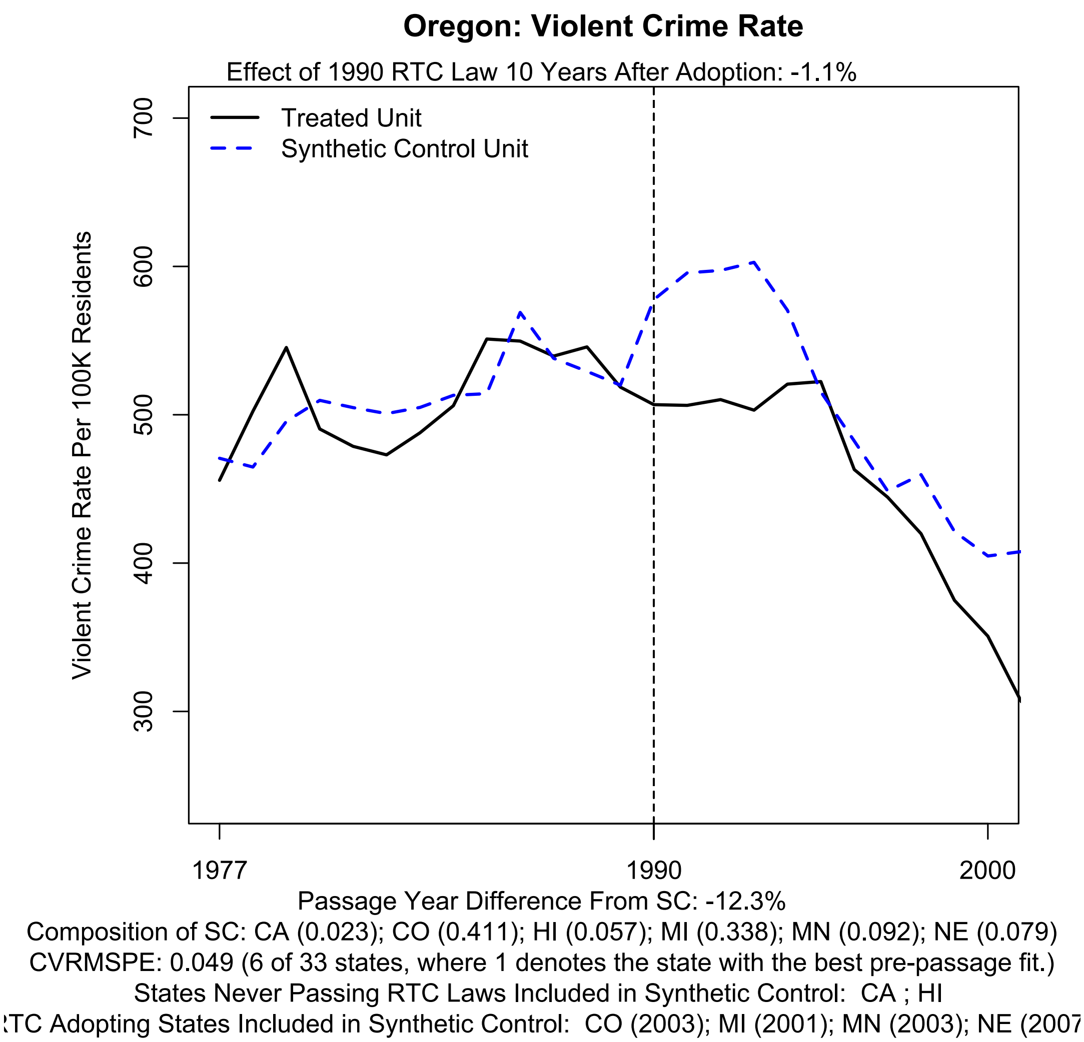
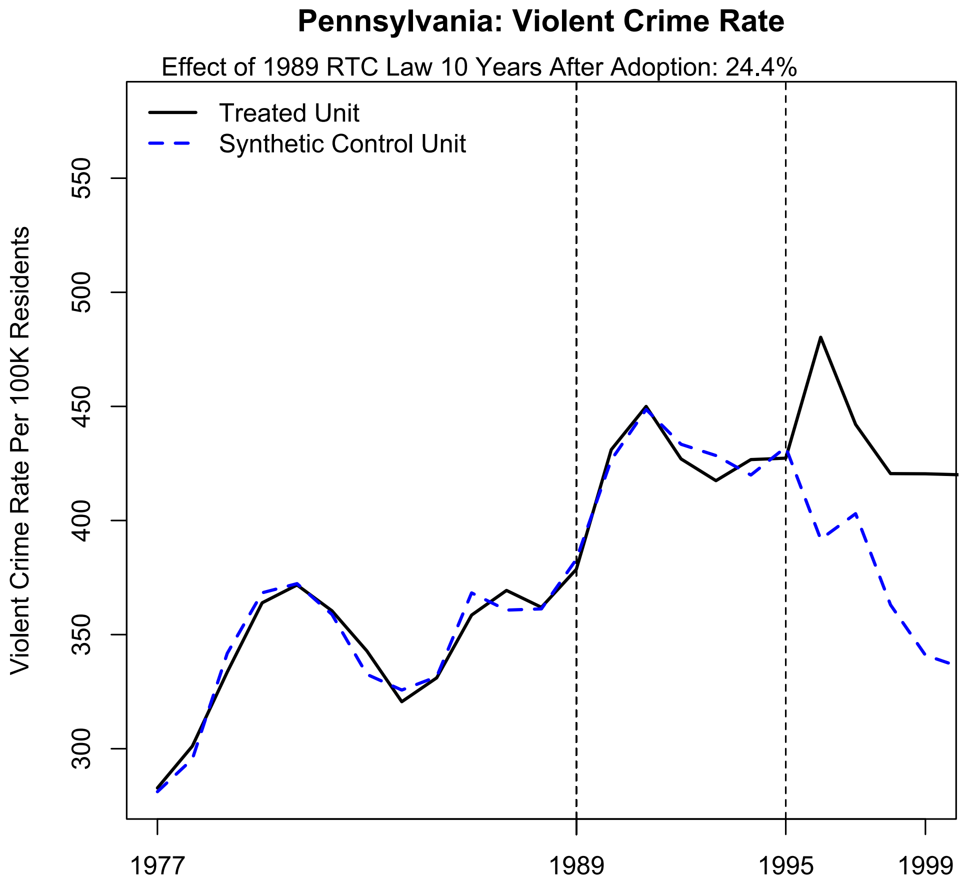
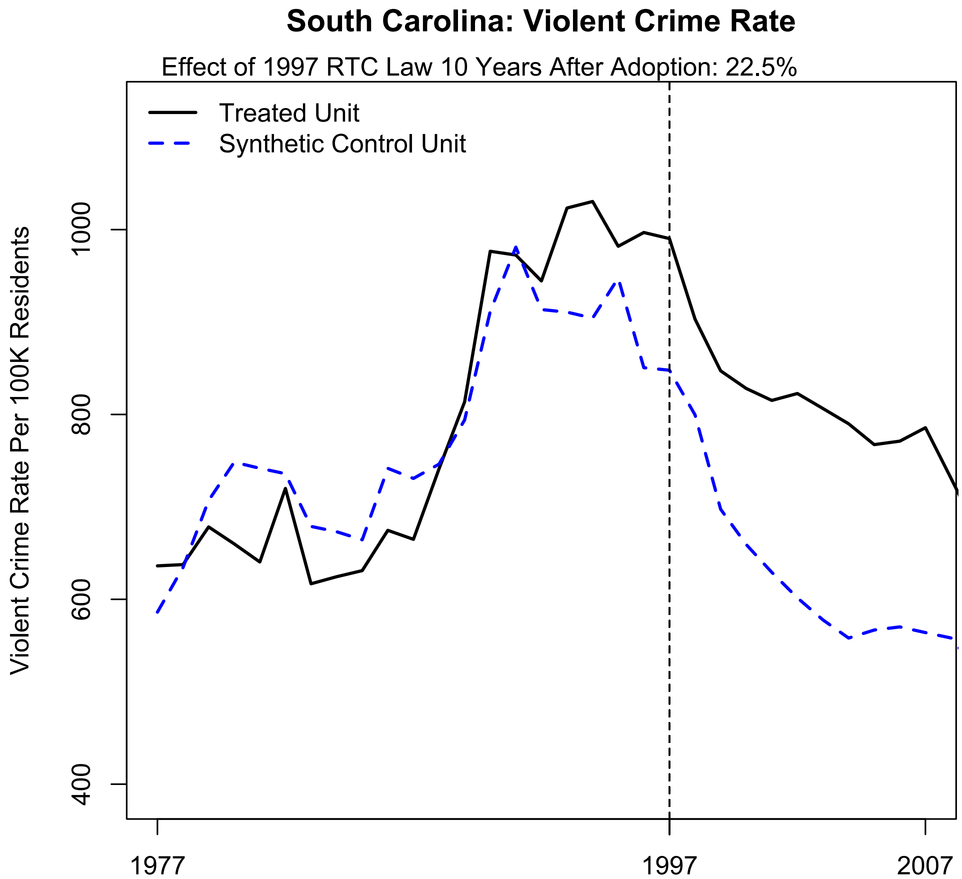

Oregon: Violent Crime Rate
Effect of 1990 RTC Law 10 Years After Adoption: -1.1%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
700
600
500
400
300
1977
1990
2000
Passage Year Difference From SC: -12.3%
Composition of SC: CA (0.023); CO (0.411); HI (0.057); MI (0.338); MN (0.092); NE (0.079)
CVRMSPE: 0.049 (6 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI
TC Adopting States Included in Synthetic Control: CO (2003); MI (2001); MN (2003); NE (2007

Figure A24



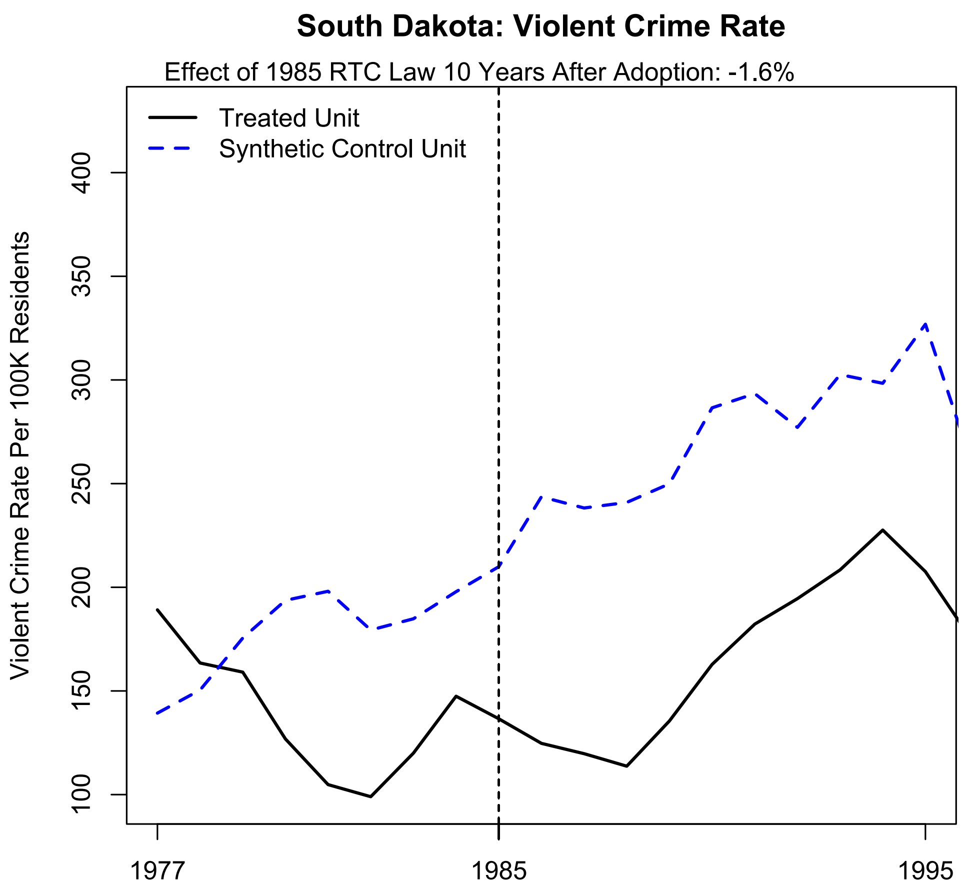
Passage Year Difference From SC: -1.1%. Composition of SC:
DE (0.078); HI (0.073); MD (0.038); NE (0.016); NJ (0.103); OH (0.27); WI (0.424)
CVRMSPE: 0.017 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ;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

Figure A25



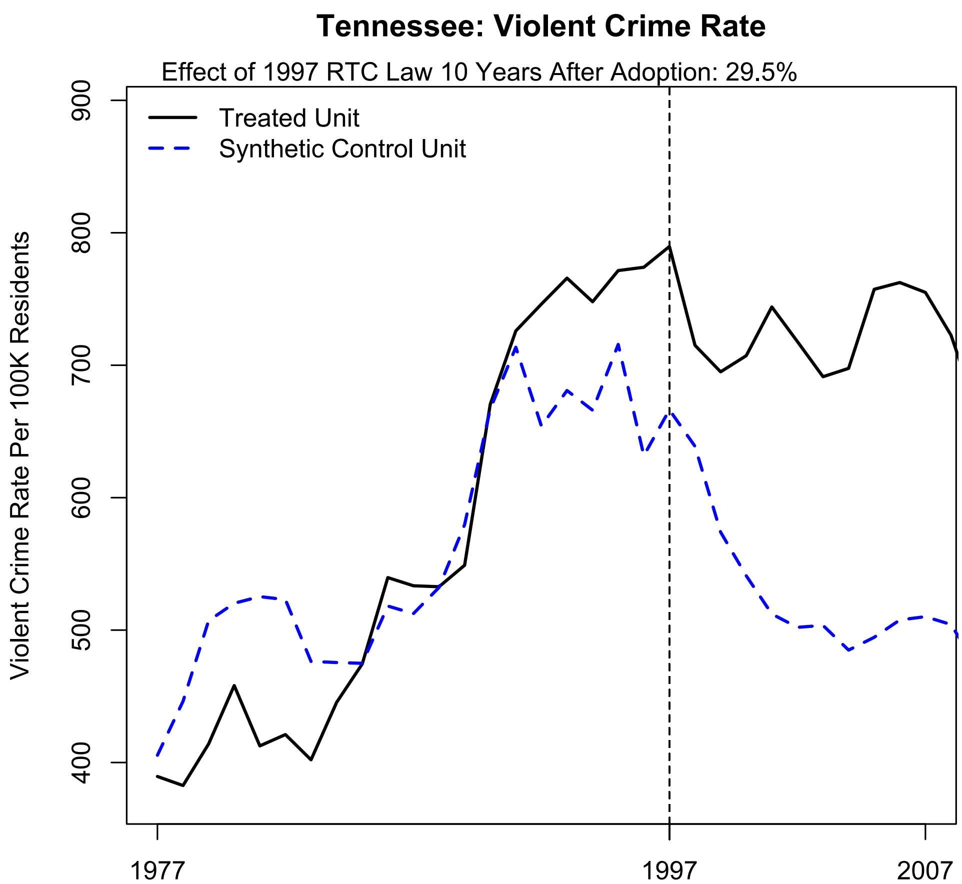
Passage Year Difference From SC: 16.8%
Composition of SC: DE (0.179); IL (0.821)
CVRMSPE: 0.088 (18 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014)

Figure A26



Passage Year Difference From SC: -34.9%
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)

Figure A27



Passage Year Difference From SC: 18.5%
Composition of SC: DE (0.291); IL (0.395); IA (0.314)
CVRMSPE: 0.123 (21 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IA (2011); IL (2014)

Figure A28

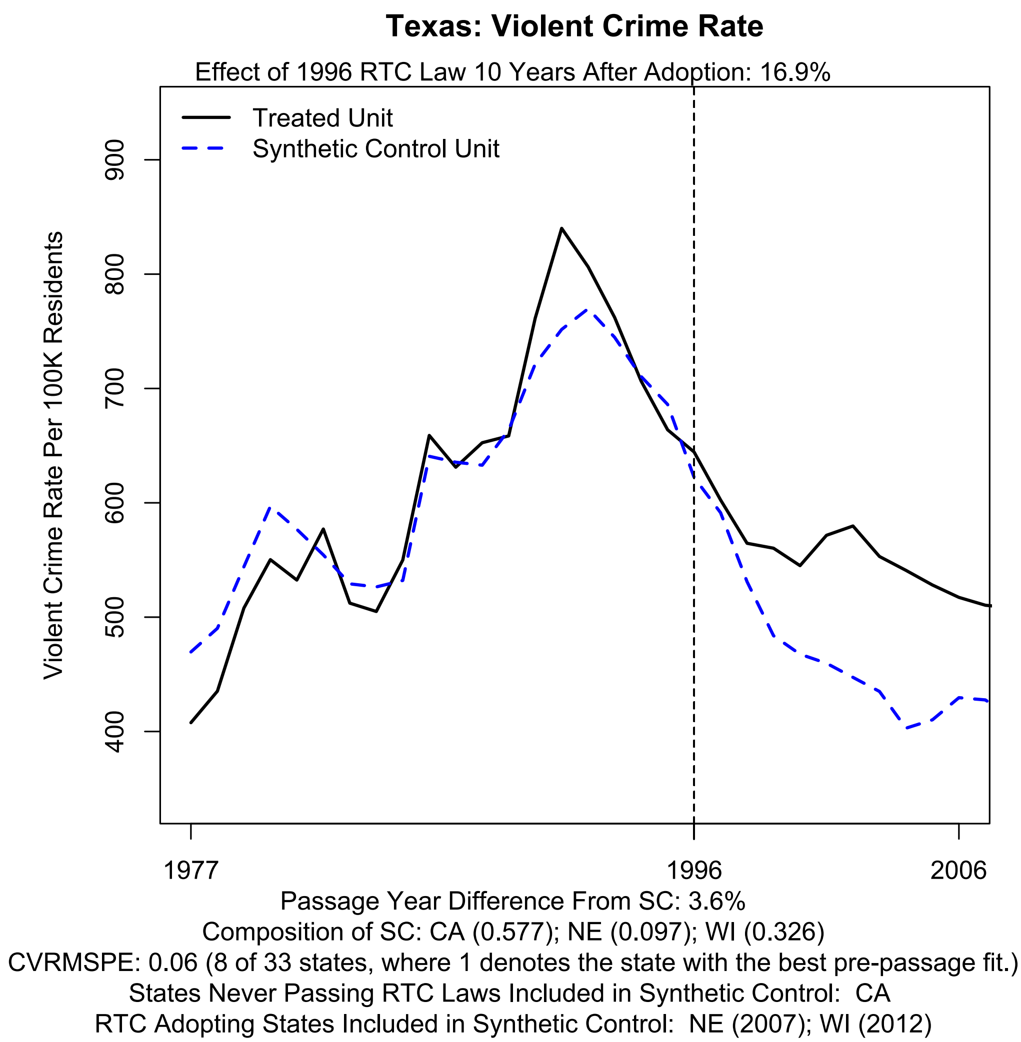
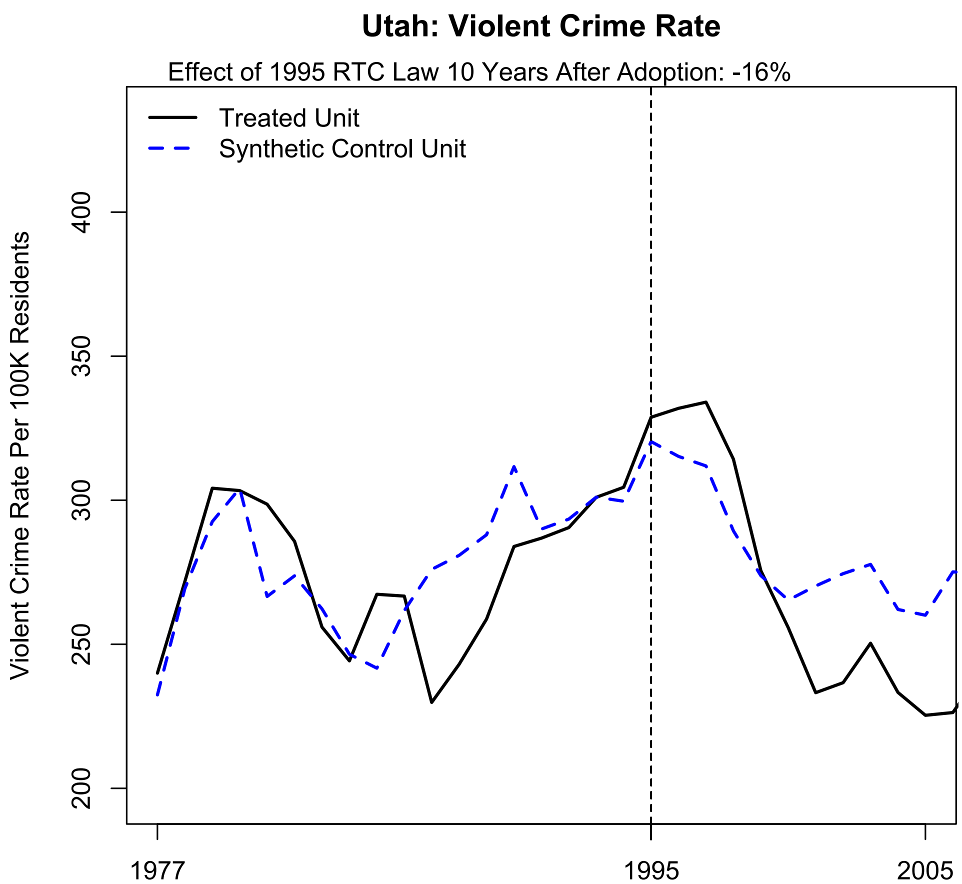
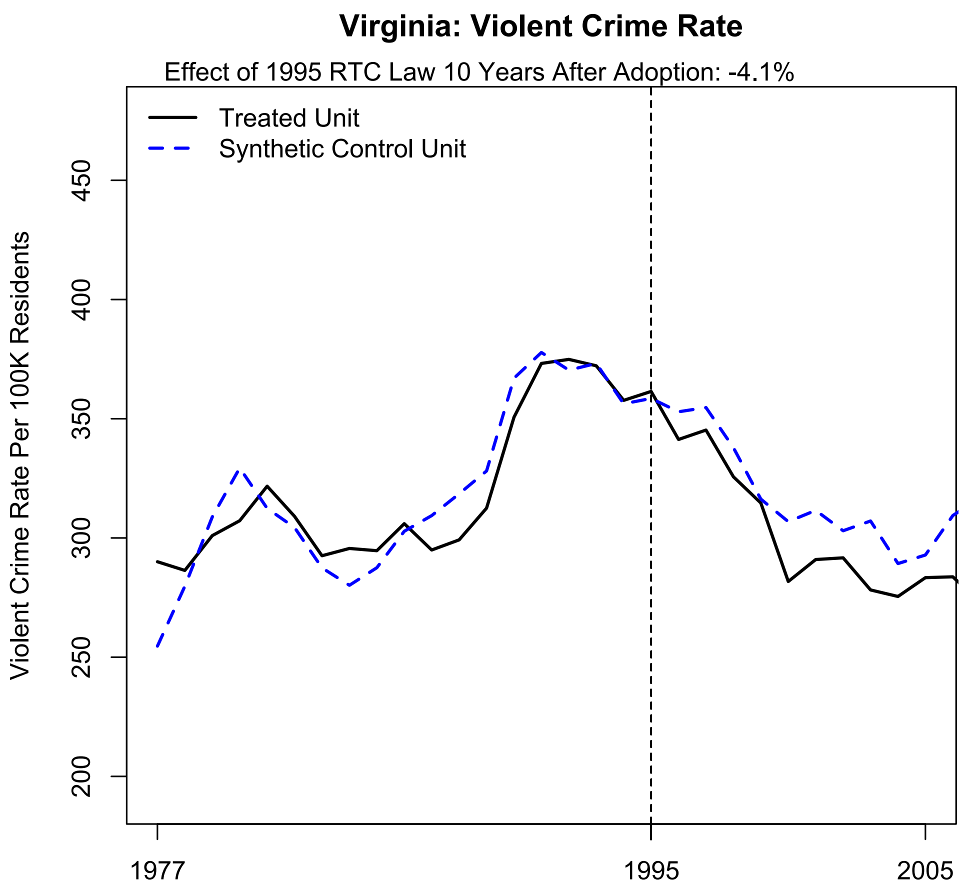

Texas: Violent Crime Rate
Effect of 1996 RTC Law 10 Years After Adoption: 16.9%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
400
500
600
700
800
900
1977
1996
2006
Passage Year Difference From SC: 3.6%
Composition of SC: CA (0.577); NE (0.097); WI (0.326)
CVRMSPE: 0.06 (8 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)

Figure A29



Passage Year Difference From SC: 2.6%
Composition of SC: HI (0.689); NE (0.159); RI (0.152)
CVRMSPE: 0.074 (14 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic Control: NE (2007)

Figure A30



Passage Year Difference From SC: 0.8%. Composition of SC:
DE (0.003); HI (0.279); KS (0.238); MD (0.003); NE (0.151); RI (0.246); WI (0.079)
CVRMSPE: 0.043 (4 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 ; RI
RTC Adopting States Included in Synthetic Control: KS (2007); NE (2007); WI (2012)

Figure A31

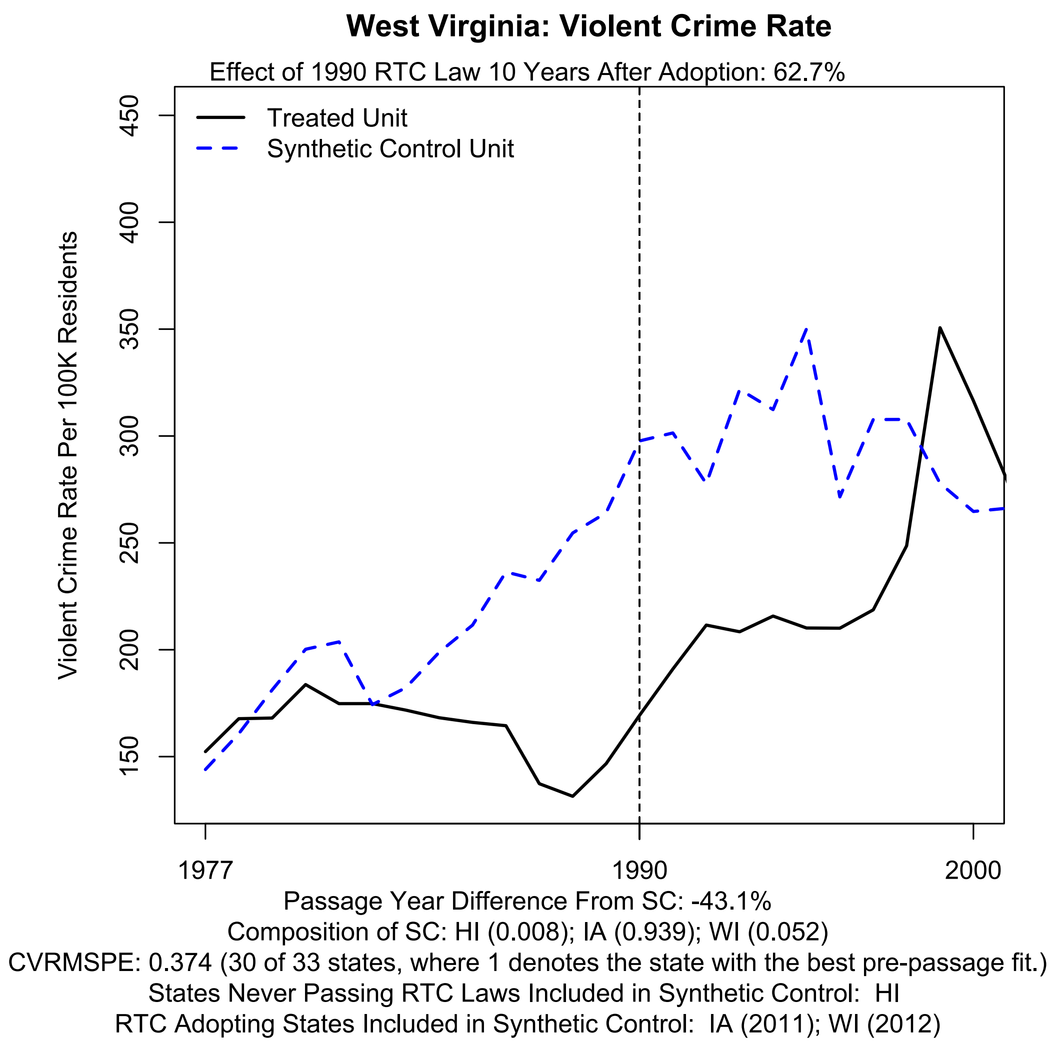
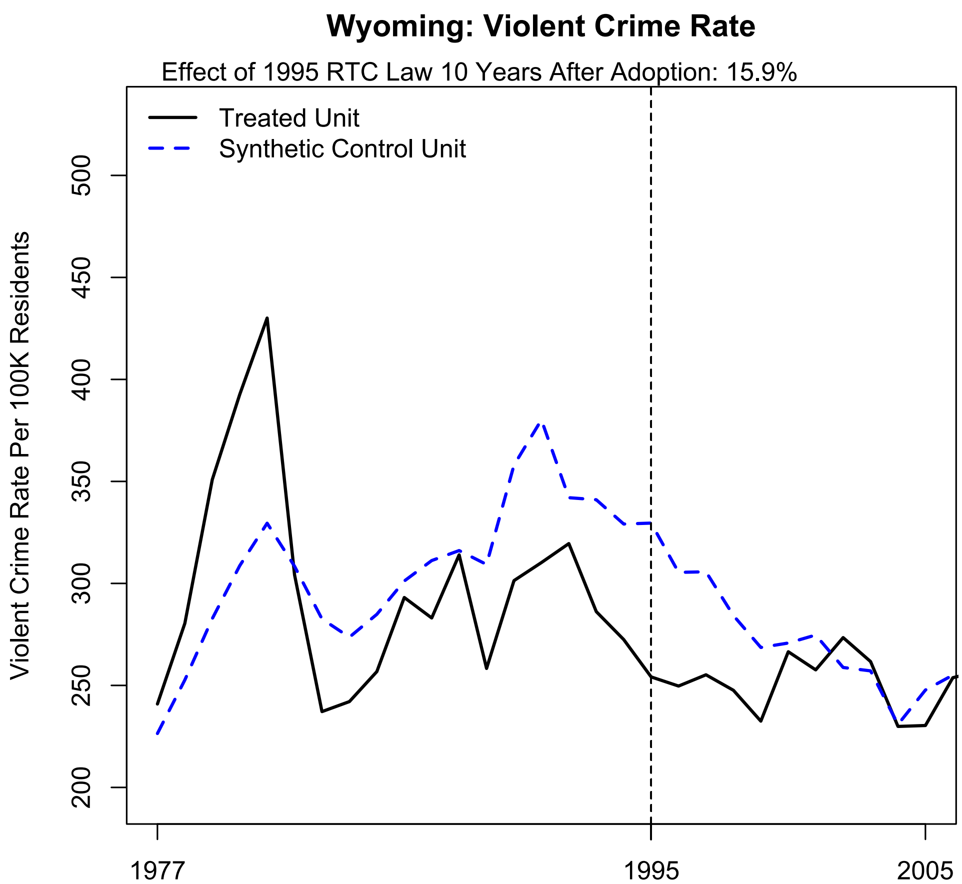

West Virginia: Violent Crime Rate
Effect of 1990 RTC Law 10 Years After Adoption: 62.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
450
400
350
300
250
200
150
1977
1990
2000
Passage Year Difference From SC: -43.1%
Composition of SC: HI (0.008); IA (0.939); WI (0.052)
CVRMSPE: 0.374 (30 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)

Figure A32



Passage Year Difference From SC: -22.9%
Composition of SC: RI (0.558); WI (0.442)
CVRMSPE: 0.167 (27 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: RI
RTC Adopting States Included in Synthetic Control: WI (2012)

Figure A33

Appendix H: Data Sources

Variable(s)	Years Available	Source	Model(s)	Notes
RTC Variables (*shalll* & *aftr*)	1977-2014	State session laws	DAW, BC, LM, MM	Statutes researched via Westlaw and HeinOnline. See footnote 26 for explanations of these variables' constructions. Note that the spline variable is coded as 0 in all years for states that passed before the data period, which depends on the model under consideration. For example, for the DAW model (1979-2014), it is coded as 0 for states that passed before 1979.
Crime	1977-2014	FBI	DAW, BC, LM, MM	UCR Data Tool for data through 2013; Table 4 of 2015 crime report for data in 2014. Each crime rate is the corresponding crime count, divided by the population metric used by the FBI, times 100,000.
Police Staffing	1977-2014	FBI	DAW, BC	Agency-year-level police employment data were acquired from the FBI and aggregated to the state-year level. The police employee rate is the total number of employees, divided by the population as given in the same dataset. In the BC model, this variable is the one-year lag of logged police staffing per capita.
Population	1977-2014	Census	DAW, BC, LM, MM	Intercensal estimates are used, except in 1970 and 1980, for which decadal-census estimates are used. All models weight regressions by population; the LM and MM models also include it as a covariate.
Population by Age, Sex, and Race	1977-2014	Census	DAW, BC, LM, MM	Intercensal estimates are used.
Income Metrics	1977-2014	BEA	DAW, BC, LM, MM	Includes personal income, unemployment insurance, retirement payments and other, and income maintenance payments. All 4 measures are divided by the CPI to convert to real terms.
Consumer Price Index	1977-2014	BLS	DAW, BC, LM, MM	CPI varies by year but not by state.
Incarcerations	1977-2014	BJS	DAW, BC, MM	The number of prisoners under the jurisdiction of a state as a percentage of its intercensal population. In the BC model, this variable is the one-year lag of the log of year-end jurisdictional population per capita.
Land Area	1977-2014	Census	LM	Land area over a given decade is taken from the most recent decadal Census. The density variable is intercensal population divided by land area.
Poverty Rate	1979-2014	Census	DAW, MM	The Census directly reports the percentage of the population earning less than the poverty line.
Unemployment Rate	1977-2014	BLS	DAW, BC, LM	
Arrests	1977-2014	FBI	LM, MM	Agency-month-year-level arrests data, separated by age, sex, race, and crime category, were acquired from the FBI and aggregated by state-year. For each crime category, the arrest rate is the number of arrests for that crime as a percentage of the (UCR-reported) number of crimes.
Crack Index	1980-2000	Prof. Roland Fryer	MM	Following the MM model, we use the unadjusted version of the index.
Beer	1977-2015	NIH	DAW, BC	The NIH reports per-capita consumption of ethanol broken down by beverage type, including beer.
Population in Metropolitan Statistical Areas	1977-2014	Census / NBER, FBI / ICPSR	DAW	1977-1980: Intercensal estimated population in counties that at least overlapped with an MSA in 1980. 1981-2014: Obtained from ICPSR-provided UCR arrests data.
Executions	1977-2014	BJS	BC	

All variables are at the state-year level unless otherwise noted. Variable creation scripts are available from the authors upon request.

Appendix I: Methodology to Choose the Number of Lags of the Dependent Variable to Include as Predictors in Synthetic Controls

The prior synthetic control literature has used five different approaches concerning the inclusion of the dependent variable in selecting the best synthetic control: 1) lags of the dependent variable in every pre-treatment year, 2) three lags of the dependent variable,[89] 3) the average of the dependent variable in the pre-treatment period, 4) the value of the dependent variable in the year prior to RTC adoption, and 5) no lags of the dependent variable.[90] To choose the optimal approach among these five options, we use the following cross-validation procedure with overall violent crime rate as the dependent variable: we first define our training period as 1977 through the sixth year prior to RTC adoption, the validation period as the fifth year prior to RTC adoption through one year prior to RTC adoption, and the full pre-treatment period as 1977 through one year prior to RTC adoption. We then use data from the training period to determine the composition of the synthetic control (essentially acting as if the RTC law were adopted five years earlier than it was). Specifically, for each of the 33 treatment units, we assign the treatment five years before the treatment actually occurred, and then run the synthetic control program using the standard DAW predictors and a five year reporting window. We then examine the fit during the training period, the validation period, and the entire pre-treatment period to see how closely for each of our five lag options the synthetic control estimate matches each adopting state's violent crime time-series.

Tables A20-A22 (Panel A) compare the fit of the five synthetic control estimates during the training period, validation period, and the entire pre-treatment period using three different loss functions. Table A20 defines the error using the mean squared error between the actual value of the dependent variable and the synthetic control estimate during a given period; Table A21 uses the mean of the absolute value of this difference between the actual value and synthetic control estimate; finally, Table A22 uses the CV of the RMSPE. For Tables A20-A22, an unweighted average of the error for each of the 33 treatment states is presented. For Tables A23-A25 (Panel B) a population-weighted average of the error for each of the 33 treatment states is presented, where population from the first year of the relevant period is used.[91]

The results from Tables A20-A25 provide strong evidence that using yearly lags of the depen-

[89] In the three-lag model, the first lag is the value of the dependent variable in 1977, the second lag is the value of the dependent variable in the year prior to RTC adoption, and the third lag is the value of the dependent variable in the year that is midway between the year corresponding to the first and second lag.

[90] The first choice is used, for example, in Bohn, Lofstrom, and Raphael (2014), the second choice is used by Abadie, Diamond, and Hainmueller (2010), and the third and fourth choices are suggested by Kaul et al. (2016).

[91] The first year of the training and full pre-treatment period is 1977, while the first year of the validation period is the fifth year prior to RTC adoption.

dent variable generates the best fit among the five options. As expected, across all six tables, the error in the training period is lowest using yearly lags, regardless of how the error is defined or whether population weights are used to aggregate the measure of error over all treatment states. Additionally, yearly lags provide the lowest error in the validation period in four of the six cases, being surpassed only marginally by the one lag average specification twice when using population weights (Tables A23 and A24). Finally, yearly lags have the lowest error in all six tables over the full pre-treatment period.

A potential concern with using all pre-intervention outcomes of the dependent variable as synthetic control predictors is that the synthetic control unit will not closely match the treated unit on the explanatory variables during the pre-treatment period.[92] To explore this issue, we calculated for each DAW variable, state, and year, the absolute percentage difference between the true value of the variable and the value for the corresponding synthetic control across all five lag options. We then average by state and finally average across all RTC-adopting states for each explanatory variable. We then create the ratio of this statistic using a particular lag choice to the average of this statistic across all five lag choices. This ratio allows us to assess the predictor fit generated by each individual lag specification relative to the average fit. It can be seen from Table A26 that yearly lags produces a good fit for an array of variables. The fit for the demographic variables is less good, particularly for the non-white non-black categories. To summarize, using all of the lags of the violent crime rate in generating a synthetic control generates the best fit in a number of measures of fit and prediction, but there are tradeoffs among the lag choices in terms of generating synthetic controls that match across all the explanatory variables of the DAW model.

Importantly, our treatment effect percentage (TEP) results are robust to any of these five lag specifications. As Table A27 shows for violent crime using DAW covariates and five alternative lag specifications,[93] the point estimate of the tenth-year average treatment effect percentage ranges from 11.8 percent (one lag average) to 15.4 percent (three lags), while we highlight the estimate for yearly lags of 14.3 percent (which has the lowest standard error in the tenth year across all five models). In other words, for all five lag choices, we estimate RTC laws generate at least double-digit increases in the rate of violent crime.

[92]See Kaul et al. (2016).

[93]Our results are also robust to the BC, MM and LM specifications, as well as crime rates for murder, property, aggravated assault, rape and robbery. Furthermore, lag choices do not influence TEP results after CVRMSPE-based exclusion. Results are available upon request.

A. Violent Crime Fit Comparison of 5 Lag Choices - Unweighted Average

Table A20: Define Fit Using Mean Squared Error

	Training Period; Mean Squared Error	Validation Period; Mean Squared Error	Full Pre-Treatment Period; Mean Squared Error
Three Lags	2,686.62	7,595.53	4,207.86
Yearly Lags	1,377.45	6,433.84	2,946.03
One Lag Average	1,752.45	7,855.29	3,546.03
One Lag Final Pre-Treatment Year	3,903.14	8,920.44	5,517.58
No Lags	2,421.58	8,559.49	4,253.37

Notes: After getting a measure of fit for each state, an unweighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1

Table A21: Define Fit Using Mean Absolute Difference

	Training Period; Mean Absolute Difference	Validation Period; Mean Absolute Difference	Full Pre-Treatment Period; Mean Absolute Difference
Three Lags	33.41	65.56	43.74
Yearly Lags	24.07	60.08	35.61
One Lag Average	27.88	65.13	39.55
One Lag Final Pre-Treatment Year	38.08	67.92	47.81
No Lags	34.68	71.57	46.51

Notes: After getting a measure of fit for each state, an unweighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1

Table A22: Define Fit Using CVRMSPE

	Training Period; CVRMSPE	Validation Period; CVRMSPE	Full Pre-Treatment Period; CVRMSPE
Three Lags	0.13	0.25	0.19
Yearly Lags	0.10	0.23	0.17
One Lag Average	0.12	0.25	0.18
One Lag Final Pre-Treatment Year	0.15	0.26	0.20
No Lags	0.14	0.27	0.21

Notes: After getting a measure of fit for each state, an unweighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1

B. Violent Crime Fit Comparison of 5 Lag Choices - Population Weighted Average

Table A23: Define Fit Using Mean Squared Error

	Training Period; Mean Squared Error	Validation Period; Mean Squared Error	Full Pre-Treatment Period; Mean Squared Error
Three Lags	1,831.32	5,432.28	2,940.87
Yearly Lags	805.01	5,309.44	2,120.68
One Lag Average	1,136	5,285.86	2,329.98
One Lag Final Pre-Treatment Year	2,551.61	6,075.21	3,694.09
No Lags	1,718.20	6,197.12	3,015.22

Notes: After getting a measure of fit for each state, a population weighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1. Population from first year of relevant period is used.

Table A24: Define Fit Using Mean Absolute Difference

	Training Period; Mean Absolute Difference	Validation Period; Mean Absolute Difference	Full Pre-Treatment Period; Mean Absolute Difference
Three Lags	26.80	53.65	35.24
Yearly Lags	18.65	51.91	28.71
One Lag Average	22.89	50.60	31.49
One Lag Final Pre-Treatment Year	29.34	54.23	37.23
No Lags	30.32	60.41	39.66

Notes: After getting a measure of fit for each state, a population weighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1. Population from first year of relevant period is used.

Table A25: Define Fit Using CVRMSPE

	Training Period; CVRMSPE	Validation Period; CVRMSPE	Full Pre-Treatment Period; CVRMSPE
Three Lags	0.07	0.13	0.10
Yearly Lags	0.05	0.12	0.09
One Lag Average	0.06	0.12	0.09
One Lag Final Pre-Treatment Year	0.08	0.14	0.11
No Lags	0.09	0.15	0.12

Notes: After getting a measure of fit for each state, a population weighted average is taken to arrive at a single measure of fit.Training Period from 1977 through RTC year - 6; Validation Period from RTC year - 5 through RTC year - 1. Population from first year of relevant period is used.

Table A26: Comparing daw Explanatory Variables in the RTC adopting states and their synthetic controls: Ratio of mean absolute percentage difference between treatment and synthetic controls for each variable to the average of this value for all five lag specifications

Variable	3 Lags	Yearly Lags	1 Lag Average	1 Lag final pre-Treatment Year	No Lags
Population	0.84	0.85	0.98	1.19	1.13
Poverty Rate	0.99	1.00	1.03	1.01	0.97
Lagged Incarceration Rate	0.95	1.01	1.00	1.03	1.00
Beer	0.95	1.02	1.02	1.05	0.96
Unemployment Rate	1.03	1.05	1.01	0.96	0.96
Lagged Police Employment	0.85	1.07	1.03	1.02	1.03
Real Income (p.c.)	0.97	1.07	1.00	0.94	1.01
Percent MSA	0.99	1.12	1.07	0.94	0.88
Age White Male 20-39	1.12	1.16	1.00	0.91	0.81
Age Black Male 15-19	1.08	1.16	1.03	1.00	0.72
Age Black Male 20-39	1.09	1.24	1.03	0.95	0.69
Age White Male 15-19	1.11	1.27	0.97	0.87	0.79
Age Other Male 15-19	0.92	1.56	1.16	0.72	0.65
Age Other Male 20-39	0.91	1.59	1.14	0.72	0.63
Mean, Non-Demographic Variables	0.95	1.02	1.02	1.02	1.00
Mean, Demographic Variables	1.04	1.33	1.05	0.86	0.72
Overall Mean	0.99	1.15	1.03	0.95	0.88

Notes: We take the average of the absolute percentage difference in economic predictors between Treatment and Synthetic Control states using five lag specifications. The values reported are the ratio of this statistic for each lag specification to the average of this statistic for all five lag choices. Age groups represent the percent of population that is white male, black male or other male in two age brackets (15-19 and 20-39).

Table A27: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Various Lag specifications, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.328	2.190	3.961^{**}	4.957^{**}	7.617^{***}	8.210^{***}	11.047^{***}	13.577^{***}	14.847^{***}	15.411^{***}
3 Lags	(1.076)	(1.444)	(1.884)	(2.096)	(2.380)	(2.911)	(2.885)	(3.994)	(3.976)	(3.284)
N	33	33	33	33	33	33	33	31	31	31
Average Normalized TEP	-0.117	2.629^{*}	3.631^{*}	4.682^{**}	6.876^{***}	7.358^{**}	10.068^{***}	12.474^{***}	14.021^{***}	14.344^{***}
Yearly Lag	(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
Average Normalized TEP	-0.184	2.045	3.366^{*}	3.885^{*}	5.856^{**}	6.256^{*}	8.595^{***}	11.295^{**}	11.840^{***}	11.770^{***}
1 Lag Average	(1.157)	(1.355)	(1.924)	(2.151)	(2.492)	(3.076)	(2.877)	(4.327)	(4.219)	(3.734)
N	33	33	33	33	33	33	33	31	31	31
Average Normalized TEP	0.325	3.293^{**}	4.639^{**}	5.083^{**}	7.432^{***}	8.084^{**}	10.859^{***}	13.187^{***}	13.899^{***}	14.222^{***}
1 Lag Final Year	(1.175)	(1.539)	(1.921)	(2.094)	(2.371)	(3.047)	(2.887)	(4.175)	(4.187)	(3.359)
N	33	33	33	33	33	33	33	31	31	31
Average Normalized TEP	-0.485	1.458	3.193^{**}	4.183^{**}	6.028^{**}	6.320^{*}	10.061^{***}	12.266^{***}	12.631^{***}	13.751^{***}
No Lags	(1.155)	(1.723)	(1.536)	(1.879)	(2.443)	(3.183)	(3.557)	(4.144)	(4.115)	(3.917)
N	33	33	33	33	33	33	33	31	31	31

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix J: Simulating Earlier RTC Passage

Footnote 56 in the text outlined an approach to validate our synthetic control estimates, using a "phantom-adoption" test. Essentially, we pretend that the RTC states adopted their laws five years earlier than they did, and we then used our synthetic controls approach to estimate what the crime rate was for the five pre-adoption years. A perfect result would show a zero effect over that pre-adoption period.

This validation test establishes two propositions: 1) our synthetic controls estimates for violent crime are buttressed by the modest pre-treatment values and the sharp rise after adoption, while some concerns are raised about the estimates for murder and property crime; and 2) better estimates are obtained by normalizing our estimates (subtracting off the differential between actual and synthetic control estimates in the last pre-adoption year) rather than using unadjusted figures.

Tables A28-A33 present both normalized and non-normalized synthetic controls estimates for violent crime, murder, and property crime with a phantom RTC law five years before actual passage. Each table thus shows estimated effects of RTC laws on the five years prior to their adoption, as well as the ten years after. For the normalized versions, none of the estimates for pre-passage years are statistically significant, other than the year prior to true adoption for property crime. The fit for violent crime is particularly reassuring since it yields relatively modest pre-treatment values, none of which are statistically significant, and the estimates rise sharply after RTC adoption. Conversely, for the non-normalized models, the pre-passage estimates are considerably larger and often highly significant. This distinction lends further credibility to the choice to use normalized estimates.

Table A28: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.889	1.896	2.600	1.065	3.241	3.066	6.103*	7.409**	7.640**	10.289***	11.294***	14.262***	17.476***	18.081***	18.396***
	(1.437)	(2.289)	(3.098)	(3.054)	(3.148)	(3.087)	(3.389)	(3.195)	(3.429)	(3.318)	(3.609)	(3.748)	(4.796)	(5.027)	(5.267)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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 simulated post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A29: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Non-Normalized TEP	1.104	3.916*	4.643**	3.125	5.316**	5.150**	8.194***	9.508***	9.744**	12.399***	13.418***	16.400***	19.715***	20.337***	20.679***
	(1.997)	(1.958)	(1.920)	(2.483)	(2.514)	(2.060)	(2.760)	(3.398)	(3.815)	(4.102)	(4.606)	(4.161)	(5.641)	(5.583)	(5.074)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A30: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.117	1.596	3.091	3.912	7.756	8.580	4.947	5.408	5.004	1.529	2.042	1.172	8.539	5.682	8.267
	(3.713)	(5.302)	(5.549)	(6.764)	(6.430)	(6.697)	(5.077)	(6.163)	(6.766)	(6.157)	(7.394)	(6.798)	(8.264)	(6.482)	(7.206)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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 murder rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A31: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Non-Normalized TEP	3.043	3.576	5.123	5.984	9.853*	10.699**	7.090*	7.575	7.198	3.751	4.296	3.445	11.855*	9.003*	11.558**
	(3.460)	(4.377)	(4.357)	(4.768)	(5.072)	(4.985)	(4.142)	(4.906)	(5.668)	(5.084)	(6.128)	(5.425)	(6.815)	(4.685)	(5.608)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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 murder rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A32: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.259	0.845	1.044	4.059	6.879*	6.223*	7.394**	8.239**	7.870*	7.145	8.716*	10.188**	11.625**	10.665*	11.518*
	(1.595)	(2.828)	(3.707)	(4.180)	(3.478)	(3.149)	(3.397)	(3.661)	(3.923)	(4.485)	(4.724)	(4.452)	(4.951)	(5.280)	(6.047)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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 property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A33: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction

	Prior to RTC Passage					After RTC Passage									
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Non-Normalized TEP	9.344**	10.512***	10.771**	13.844***	16.716***	16.096***	17.292***	18.165***	17.827**	17.135**	18.736**	20.234***	21.557***	20.629***	21.549***
	(3.607)	(3.709)	(4.169)	(4.644)	(4.682)	(4.693)	(5.147)	(6.206)	(6.476)	(6.973)	(7.039)	(6.645)	(6.790)	(6.951)	(7.279)
N	30	30	30	30	30	30	30	30	30	30	30	30	28	28	28

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 property crime rate in treatment and synthetic control states at given post-treatment interval
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA MI MN MO MS MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$