Do Handguns Make Us Safer? A State-Level Synthetic Controls Analysis of Right-to-Carry Laws

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

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John J. Donohue III (corresponding author): Stanford Law School, 559 Nathan Abbott Way, Stanford, CA 94305. Email: donohue@law.stanford.edu. Abhay Aneja: Haas School of Business, 2220 Piedmont Avenue, Berkeley, CA 94720. Kyle D. Weber: Columbia University, 420 W. 118th Street, New York, NY 10027.

Abstract

The 2004 report of the National Research Council (NRC) on *Firearms and Violence* recognized that crime was higher in the post-passage period (relative to national crime patterns) for states adopting right-to-carry (RTC) concealed handgun laws, but the panel was unable to identify the true causal effect of these laws from the then-existing panel data evidence. In this study, we use 12 additional years of data (through 2012) to see if more convincing and robust conclusions can emerge with more data and with better statistical techniques.

We show that our preferred panel data models created in Aneja, Donohue, and Zhang (2014, ADZ) indicate that RTC laws lead to higher levels of violent crime, particularly aggravated assault. Other statistical models offered by Lott and Mustard (LM) and Moody and Marvel (MM) do not show evidence of violent crime increases. We find these models to be inferior in that they omit critical explanatory variables such as police and/or incarceration rates and are marred by including an excessive number of highly collinear demographic variables. Correcting these two problems, the modified LM and MM panel data models also generate estimates that RTC laws increase violent crime.

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 three specifications (ADZ, LM, and MM), RTC laws are associated with *higher* aggregate violent crime and aggravated assault rates, and the size of the deleterious effects that are associated with the passage of RTC laws appears to climb over time. We estimate that the adoption of RTC laws substantially elevates violent crime rates, but seems to have no impact on property crime rates. Ten years after the adoption of RTC laws, violent crime is estimated to be 18 – 20 percent higher under the ADZ and MM specifications than it would have been without the RTC law (and 11-12 percent higher under the LM specification).

If we measure the average treatment effect for the 26 states with ten years of post-passage data using medians rather than means, RTC laws *increase* violent crime after ten years by 15.1 percent. If we limit our synthetic controls analysis to the 17 synthetic controls that best fit the data during the pre-passage period, RTC laws lead to a median 14 percent *increase* in the rate of violent crime after ten years. 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.

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Part I
Introduction:

For nearly 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 right-to-carry 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. A number, primarily 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 2004 National Research Council report *Firearms and Violence: A Critical Review* might be able to 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 specifically 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 begins by revisiting the panel data evidence to see if extending the data for an additional 12 years, thereby providing additional crime data for prior RTC states as well as on 11 newly adopting RTC states, offers any clearer picture of the causal impact of allowing citizens to carry concealed weapons. While the models that we argue are more compelling – the ADZ models discussed in Aneja, Donohue, and Zhang (2014) – do provide clear evidence that RTC laws increase violent crime, it is still true that models used by Lott and Mustard (LM) and Marvel and Moody (MM) generate the type of conflicting results that led the NRC panel to be pessimistic about the possibility of using panel data models to generate the impact of RTC laws "in a convincing and robust fashion." Connecting what we believe to be obvious problems with the LM and MM specifications restores the finding that RTC laws increase violent crime.

To provide further evidence of the impact of RTC laws, we use a new statistical approach designed to address some of the weaknesses of panel data models that has gained prominence in the period since the 2004 NRC report. Using this so-called synthetic controls methodology, we hope 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 et al (2010) and Abadie et al (2014) – uses a matching methodology to create a credible "synthetic control" based on a weighted average of other states that matches the pre-passage pattern of crime, which can then be used to estimate the likely path of crime if RTC-adopting states had not adopted a 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]

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[1]In support of the original 1997 Lott and Mustard paper, see Lott's 1998 book *More Guns, Less Crime* (and the 2000 and 2013 editions of this book). Ayres and Donohue (2003) and the 2004 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's 2011 *American Law and Economics* paper (and the 2014 NBER paper further expanding the ALER paper). Moody and Marvell (2008) and Moody, Marvell, Zimmerman, and Alemante's 2014 response to the 2011 ADZ ALER paper continued to argue in favor of a crime-reducing effect of RTC laws.

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

[3]The accuracy of this matching can be qualitatively assessed by examining the root mean square prediction error (RMSPE) of the synthetic control in the pre-treatment period (or a variation on this RMSPE implemented in this paper), and the 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.

To preview our major findings, the synthetic controls estimates of the average impact of RTC laws across the 33 states that adopt during our data period from 1979-2012 indicate that violent crime is substantially higher after ten years than would have been the case had the RTC law not been adopted. Essentially, the synthetic controls approach provides a similar portrayal of RTC laws as that provided by the ADZ panel data models and undermines the results of the LM and MM panel data models. According to the aggregate synthetic control models, RTC laws led to increases in violent crime of from 18-19 percent after ten years using either the ADZ or MM specifications and of 11-12 percent with the LM specification, with no apparent effect on property crime. The median effect of RTC adoption after 10 years is 15.1 percent across all 26 states with ten years of data and 14 percent for the 17 states with the most compelling pre-passage fit between the adopting states and their synthetic controls. Comparing our findings with the results generated using placebo treatments, we are able to reject the null hypothesis that our estimated treatment effects associated with aggregate violent crime and aggravated assault in the ADZ specification are generated from the same distribution that produced our dummy treatment effects. These placebo tests also suggest that the standard errors generated for our aggregate estimates may be biased downward.

The structure of the paper proceeds as follows. Part II discusses the panel data results for the three different models, showing that the ADZ model indicates that RTC laws have strongly increased violent crime, the LM model provides some admittedly inconsistent evidence that RTC laws decrease rape, robbery, and burglary, and the MM model suggests that RTC laws have no impact on crime. We argue that the ADZ set of explanatory variables are the most plausible and show that highly advisable corrections to the LM and MM specifications also generate strong evidence that RTC laws increase violent crime.

The remainder of the paper shows that, while the panel data estimates were in conflict using the unchanged LM and MM specifications, the synthetic controls approach under all three sets of explanatory variables supports the conclusion that RTC laws lead to substantial increases in violent crime. Part III describes the statistical underpinnings of the synthetic controls approach and specific details of our implementation of this technique. Part IV provides out synthetic controls estimates of the impact of RTC laws and Part V concludes.

Part II

Panel Data Estimates of the Impact of RTC Laws

A. 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. The NRC report called this the “no-controls” estimate, which is just the coefficient estimate on the variable indicating the date of adoption of a RTC law in a panel data model with state and year fixed effects. According to the NRC report, “Estimating the model using data to 2000 shows that states adopting right-to-carry laws saw 12.9 percent increases in violent crime – and 21.2 percent increases in property crime – relative to national crime patterns.”

We now provide this identical estimate on more complete and updated data for an additional 12 years through 2012. 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), and a spline model, which shows how the trend in crime changed after RTC laws (again relative to national trends). The dummy model, which corresponds to what we just quoted from the NRC report, shows the average post-passage increase in violent crime based on 12 additional years of data and 11 additional adopting states was 18.9 percent, while the comparable increase in property crime was 19.6 percent.[4] The spline model generated comparable results, suggesting violent crime grew about 1.5 percentage points per year more after RTC laws were adopted (relative to national trends), while property crime rose about 1.3

[4]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.

percentage points more per year.[5] All of these results are statistically significant. In other words, more and better data paint an even darker "first-blush" picture of the crime experience under RTC laws than was found in the NRC report.

Table 1: Panel Data Estimated Impact of RTC Laws: State and Year Fixed Effects, No Other Regressors (1977 - 2012)

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	18.873***	2.280	15.718*	14.490**	21.842**	19.585***	27.364**	23.810***	17.321***
	(6.819)	(8.495)	(9.012)	(5.556)	(9.039)	(6.093)	(13.350)	(8.119)	(5.721)
Spline Model	1.487**	0.395	1.090	1.235**	1.677**	1.341***	1.590	1.568**	1.217**
	(0.658)	(0.698)	(0.806)	(0.602)	(0.790)	(0.495)	(1.155)	(0.685)	(0.468)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors are provided beneath point estimates in parentheses and standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All Figures Reported in %.

Of course, knowing that RTC states experience a worse post-passage crime pattern does not prove that RTC laws increase. 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, then the "no controls" model might show that the crime experience in RTC states was worse than in other states even if this was 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 somewhat larger, albeit not statistically significant, increases in incarceration and police than other states. Since every estimate in Table 2 is positive, Table 2 confirms that RTC states did not have lower rates of incarceration, police officers, or total police employees after adopting their RTC laws.

Table 2: Estimated Impact of RTC Laws: State and Year Fixed Effects, No Other Regressors (1977 - 2012)

	Incarceration Rate	Police Employee Rate	Police Officer Rate
Dummy Variable Model	4.741	2.023	2.445
	(6.494)	(2.523)	(2.829)
Spline Model	0.594	0.196	0.165
	(0.510)	(0.212)	(0.230)

Estimations include year and state fixed effects. Robust standard errors are provided beneath point estimates in parentheses and standard errors are clustered at the state level. No demographics or other regressors are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All Figures Reported in %.

B. Adding Explanatory Variables

We know from the NRC report and from 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 continued to invest at least as heavily in police and prisons as 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 could 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

[5] 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 laws was in effect the first year after adoption. After this year, the value of the this variable is incremented by one annually for states that adopted right-to-carry laws between 1977 and 2012. Another variable (*overalltrend*) is also added to the spline model, representing the number of years that have passed since 1977.

Table 3: Table of Variables For Three Panel Data Studies

Explanatory Variables	ADZ	LM	MM
Right to Carry Law	x	x	x
Lagged per capita incarceration rate	x		x
Lagged police staffing per 100,000 residents	x		
Real Per Capita Personal Income	x	x	x
Real Per Capita Income maintenance	x	x	x
Real Per Capita Retirement payments	x	x	x
Real Per Capita Unemployment insurance payments	x	x	x
Poverty and unemployment rate	x		x
Population density	x	x	
6 age-sex-race demographic variables -all possible combinations of black and white males in 3 age groups (10-19,20-29,30-39) indicating the percentage of the population in each group	x	x	x
30 additional age-sex-race demographic variables (in addition to the 6 above) -adding 3 more age categories to the black and white male groups above, and then adding another racial category (neither white nor black) and repeating this all for females, indicating the percentage of the population in each group		x	x
Lagged violent or property arrest rate		x	x
State population		x	x
Crack Index			x
Lagged dependent variable			x

Note: See footnote 36 in Appendix B for an explanation of the differences in the Retirement variable definition between the three specifications.

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 policy instruments in the battle against crime.[6]

1. The ADZ Panel Data Model

In addition to the state and year fixed effects of the no controls model and the identifier for the presence of a RTC law, the ADZ model includes an array of other factors that might be expected to influence crime, such as the levels of police and incarceration, various income, poverty and unemployment measures, and six demographic controls designed to capture the presence of black and white males in their higher crime age categories between 10 and 39. The full set of explanatory variables are listed in Table 3.

The ADZ panel data model in Table 4 (run on data from 1979-2012) shows that violent crime is substantially higher in both the dummy and spline models following RTC adoption (relative to national trends).[7] Every estimate for the nine crime categories and two models (dummy and spline) is positive in Table 4 (suggesting higher crime rates associated with RTC laws), and violent crime is highly statistically significant in both the dummy and spline models. Table 4 suggests that RTC laws on average increased crime by 12.3 percent in the years following adoption (dummy model) or increased the level of violent crime by an additional 1.1 percentage points each year (spline model).[8]

[6]While we attempt to include as many states in these regressions as possible, the District of Columbia is dropped out of the ADZ regressions after the year 2000 (owing to missing incarceration data) and is dropped out of the MM regressions entirely (owing to missing crack index data), while 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 these 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 et al. (2014). All of the regressions presented in this paper (outside of those included in the Online Appendix) are weighted by state population.

[7]The complete set of estimates for all explanatory variables for the ADZ dummy model is shown in appendix Table A2. Two years of data (1977-78) are dropped from this analysis because usable state-level poverty data is not available for those years.

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

Table 4: Panel Data Estimates of Impact of RTC Laws: State and Year Fixed Effects, ADZ Regressors, 1979-2012

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	12.259**	3.168	10.814*	8.511*	13.644*	10.604**	17.871*	11.710*	9.363**
	(5.647)	(6.958)	(5.543)	(4.378)	(8.083)	(5.083)	(9.420)	(6.260)	(4.566)
Spline Model	1.093**	0.597	0.789	0.929*	1.187	0.790*	1.072	0.667	0.748*
	(0.539)	(0.577)	(0.519)	(0.529)	(0.711)	(0.438)	(0.756)	(0.557)	(0.424)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors are provided beneath point estimates in parentheses and standard errors are clustered at the state level. Six demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. Other controls include the lagged incarceration rate, lagged police employment per capita, the unemployment rate, the poverty rate, population density, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real per capita retirement payments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All Figures Reported in %.

2. The LM Panel Data Model

Table 3's recitation of the explanatory variables contained in the LM panel data model reveals two obvious omissions: there are no controls for the levels of police and incarceration in each state, even though we know that these factors have a large impact on crime and are at least somewhat elevated 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 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 arrest rates are also 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.

Even more problematic are the 36 demographic variables that Lott and Mustard use. 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. As shown in Appendix Table A3, the LM model finds that the male group will vastly increase crime (the coefficient is 277!), but their female counterparts have an enormously dampening effect on crime (with a coefficeint of -286!). Both of those highly implausible estimates have t-statistics well over 4, 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.[9]

Table 5, Panel A shows the results of the LM panel data model estimated over the period 1977-2012, and this mis-specified model yields somewhat anomalous results that contrast sharply with the ADZ results of Table 4. Unlike the ADZ results, the LM model shows conflicting albeit statistically insignificant coefficient estimates for violent crime, with the dummy model suggesting crime decreases and the spline model suggesting

[9]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 6 to 7 for the RTC variable in both the LM dummy and spline models when the 36 demographic controls are used. Using the 6 ADZ 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 – in the neighborhood of 14,000! This analysis makes us highly skeptical of any estimates of the impact of RTC laws that employ the Lott-Mustard set of 36 demographic controls.

crime increases of about the same magnitude. The LM dummy model suggests crime declines that are statistically significant for rape, robbery, and burglary, but the LM spline models offer no support for these findings. We suspect that the inclusion of 36 highly collinear demographic variables–contrary to standard practice in estimating crime regressions–improperly introduces too many time trends, thereby undermining the ability of the dummy model to generate plausible estimates. Recall that the Table 1 "no-controls" dummy model showed that rape, robbery, and burglary had all risen sharply in RTC states (relative to other states) by 16 to 24 percent. Since incarceration and police levels were somewhat higher after RTC adoption, the Table 5 results could only be plausible if movements in the other LM explanatory variables in the dummy model somehow caused the sign of the estimated effect of RTC adoption to reverse for these three crimes.

Panel B of Table 5 limits the number of demographic variables in the LM model to the six employed in the ADZ model and also adds the essential incarceration and police rate variables to the LM model. These clearly advisable modifications once again eliminate any hint that RTC laws have any beneficial effects. In particular, the modified LM model shows highly significant violent crime rate increases in both the dummy and spline models.

Table 5: Panel Data Estimates of Impact of RTC Laws: LM Regressors, 1977-2012

Panel A: LM Regressors including 36 Demographic Variables

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	-4.040	-5.115	-5.293***	-2.084	-8.815***	0.453	3.962	-3.865**	1.570
	(3.311)	(3.347)	(1.965)	(4.624)	(2.670)	(1.715)	(3.979)	(1.784)	(1.930)
Spline Model	0.315	0.468	-0.047	0.465	0.074	0.150	-0.367	-0.290	0.308
	(0.412)	(0.334)	(0.335)	(0.581)	(0.387)	(0.206)	(0.298)	(0.339)	(0.233)

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

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	11.509*	3.579	10.713*	7.649	12.909	11.222**	18.786*	12.091*	9.978**
	(6.671)	(7.078)	(5.552)	(5.235)	(9.093)	(5.192)	(10.837)	(6.291)	(4.388)
Spline Model	1.334**	0.735	0.947*	1.086*	1.565**	1.048**	1.512	0.958	0.961**
	(0.611)	(0.611)	(0.554)	(0.570)	(0.764)	(0.456)	(0.984)	(0.619)	(0.419)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors are provided beneath point estimates in parentheses and standard errors are clustered at the state level. In Panel A, thirty-six demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All Figures Reported in %.

In summary, the LM dummy model ostensibly supports a crime decline in three crime categories, but the LM spline model shows no such effect. Since the LM specification is plagued by omitted variable bias, flawed pseudo-arrest rates, too many highly collinear demographic variables, and other problems, we find the LM panel data results not to be credible. A simple correction of the demographic variables and the addition of the incarceration and police controls again reveals strognly significant estimates that RTC laws increase violent crime.

3. The MM Panel Data Model

Table 3 reveals that the MM model improves on the LM model in that it includes the key incarceration variable, but MM also omit the police measure found in ADZ's specification. The MM model also contains the problematic pseudo-arrest rates and over-saturated and highly collinear demographic variables that LM employ.[10] 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. Panel A of Table 6 shows that the estimated effects across the nine different crime categories are both small in absolute value, conflicting in

[10]While we follow Moody and Marvell (2008) in including lagged values of the dependent variable as a regressor in our MM panel data specification, no analogous variable is included below in our synthetic control analysis featuring the Moody-Marvell predictor variables. We excluded this lag from our synthetic control analysis, since we could not find any scholarly precedent for integrating the analysis of a first-differenced outcome variable into the synthetic control framework.

sign, and never statistically significant. If the estimates from this panel data model were correct, one would conclude that RTC laws had essentially no impact on crime. But the need to include a measure of police and use more appropriate demographic controls as we did with the LM model is obvious.

Panel B of Table 6 makes these changes and once again we see that panel data estimates using superior specifications show statistically significant estimates that RTC laws increase violent crime.

Table 6: Panel Data Estimates of Impact of RTC Laws: State and Year Fixed Effects, Lott-Mustard Demographic Variables, and MM Regressors, 1980-2000

Panel A: MM Regressors including 36 Demographic Variables

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	-0.381	-1.912	-1.759	-0.073	-0.210	0.783	1.571	-1.131	1.421
	(1.231)	(2.826)	(1.208)	(1.186)	(2.461)	(1.320)	(1.948)	(1.893)	(1.196)
Spline Model	0.191	-0.201	0.121	0.236	0.093	-0.047	-0.374	-0.147	0.014
	(0.168)	(0.504)	(0.183)	(0.179)	(0.292)	(0.175)	(0.233)	(0.248)	(0.164)

Panel B: MM Regressors with 6 ADZ Demographic Variables and Adding Control for Police

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Dummy Variable Model	2.338**	-0.301	1.183	2.045*	2.812	1.592	3.032*	0.422	2.029**
	(1.118)	(2.855)	(1.066)	(1.062)	(2.083)	(0.964)	(1.539)	(1.294)	(0.916)
Spline Model	0.427***	0.036	0.293*	0.446**	0.387	0.107	0.134	0.071	0.130
	(0.148)	(0.407)	(0.167)	(0.177)	(0.242)	(0.110)	(0.160)	(0.146)	(0.124)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors are provided beneath point estimates in parentheses and standard errors are clustered at the state level. In Panel A, thirty-six demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, 6 demographic variables are included and a control is added for 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, Freyer et al.'s state-level crack index, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All Figures Reported in %.

4. The Lessons from the Panel Data Studies

The strongest results to emerge from the three sets of panel data specifications are found in the ADZ specifications, finding that RTC laws increase violent crime. Do the conflicting results of the LM and MM specifications create uncertainty about how much we can trust any of the panel data models? We think much, if not all, of the uncertainty created by these models is eliminated by simply ensuring that appropriate demographic variables and the standard controls for police and incarceration are used. If the published panel data models are to be evaluated in this light, then it seems that the weight of the evidence strongly supports the finding that RTC laws increase violent crime.

Nonetheless, an 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. Moreover, 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, 2004: 308.) But Horowitz and indeed the entire NRC panel might well have felt differently about the problem of conflicting panel data results if they could have seen the uniform findings of the ADZ, LM, and MM results that are now available when appropriate demographic controls and police and incarceration rate variables are included. Thirteen more years of data, including information on 11 more adopting states, has now provided consistent and statistically strong evidence that RTC laws increase violent crime. But owing to the challenges of estimating effects from observational data, it will be useful to see if a different statistical approach that has different attributes from the panel data methodology can be brought to bear on the issue of the impact of RTC laws. The rest of this paper will present this new approach.

Part III

Estimating the Impact of RTC Laws Using Synthetic Controls

The synthetic controls methodology would seem to be a promising new development for addressing the impact of RTC laws. The synthetic control approach is becoming increasingly prominent in economics and other social sciences. The synthetic control methodology has been deployed in a wide variety of fields, including health economics (Nonnemaker et al., 2011), immigration economics (Bohn et al., 2014), political economy (Keele, 2009), urban economics (Ando, 2015), the economics of natural resources (Mideksa, 2013), and the dynamics of economic growth (Cavallo et al., 2013).

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 prior to the state's adoption of a permit-to-purchase handgun law, finding evidence that the state's firearm homicide rate (but not its non-firearm homicide rate) fell appreciably after the state's implementation of this law. Munasib and Guettabi (2013) use this methodology to examine the effect of Florida's "Stand Your Ground" law, finding no impact on crime but concluding that this law was associated with an increase in overall gun deaths. Similarly, Cunningham and Shah (2014) 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 (2012) examines the consequences of an influx of organized crime into two Italian provinces in the late 1970s. These papers focus on a single treatment in a single geographic region, while we look we look at 33 RTC adoptions throughout the country.[11]

A. The Basics of the Synthetic Control Methodology

We provide here a general overview of the synthetic control methodology. For a more detailed technical description of this method, we direct the reader to Abadie and Gardeazabal (2003), Abadie et al. (2010), and Abadie et al. (2014).

The synthetic control method attempts to generate representative counterfactual units by comparing a treatment unit 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 similar control states.[12] 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 and other predictors known to influence this variable.[13] Following Abadie et al. (2010), we use average pre-treatment values of the explanatory variables in the ADZ, LM, and MM specifications and three evenly spaced pre-treatment crime rates as predictors.[14] Once the synthetic counterfactual is generated and the weights associated with each

[11] Saunders et al. (2014) use the synthetic control methodology to examine the effect of an anti-drug intervention on crime rates, but their empirical approach involves using the SCM technique to generate weights which are then used in negative binomial regressions.

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

[13] Roughly speaking, the algorithm that we use finds W (the weights of the components of the synthetic control) that minimizes $\sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}$, where V is a diagonal matrix incorporating information about the relative weights placed on different predictors, W is a vector of non-negative weights that sum to one, X_1 is a vector containing pre-treatment information about the predictors associated with the treatment unit, and X_0 is a matrix containing pre-treatment information about the predictors for all of the control units. For our main analysis, 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 regression-based technique. Owing to computational constraints, we use this approach in our placebo analysis.

[14] The estimates we present use three pre-treatment crime rates, which is the number of pre-treatment crime rates as predictors that minimized the average RMSPE of our synthetic controls for both violent crime and aggravated assault when using the ADZ predictors. It is worth noting that the estimated treatment effect associated with the passage of a state-level RTC law remains

control unit are assigned, the *synth* program then calculates values for the outcome variable associated with this counterfactual and a 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.

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

To illustrate the procedure outlined above, consider the case of the state of Texas, whose RTC law went into effect on January 1, 1996. We adopt the convention that the potential control group for each treatment state consists of all 9 states that do not pass RTC legislation by the year 2012, as well as states that pass RTC laws at least 10 years after the passage of the treatment state (e.g., in this case, those states passing RTC laws after 2006, such as Nebraska and Kansas, whose RTC laws went into effect at the beginning of 2007). In our analysis, we estimate results for up to ten years post-passage,[15] and so this restriction helps us avoid including states with their own permissive concealed carry laws in the synthetically constructed unit.

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 closer these time series of crime are between the treatment and synthetic unit in the pre-passage period, the greater confidence we have in the accuracy of the match. 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 that particular intervention.

1. Synthetic Controls Estimates of Violent Crime in Four States

Figure 1 shows the synthetic controls graph for violent crime in Texas over the period from 1977 through ten years after the adoption of Texas's RTC law in 2006. 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 55.9 percent owing to its similar attributes to Texas, Nebraska with a weight of 26.5 percent, and Iowa with a weight of 17.6 percent.

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 1 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 1977-2005. This pattern gives us reasonable confidence that synthetic Texas will provide a good prediction of what would have happened in Texas had it not adopted a RTC law. Another advantage of the synthetic controls protocol is that one can consider the attributes of the three states that make up synthetic Texas to see if they plausibly match the features that generate crime rates in states across the country.

Looking at Figure 1's post-passage period after 1996, we see that while both Texas and synthetic Texas (the weighted average violent crime performance of the three mentioned states) show declining crime rates over that decade, 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 1 notes, ten years after adopting its RTC law, violent

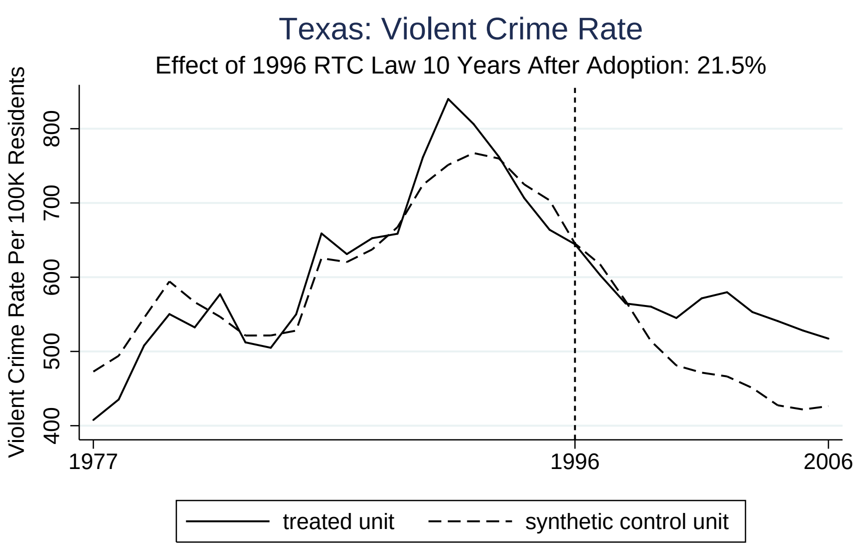
similar for violent crime regardless of whether one, two, three, four, or five of these lagged values are included along with the ADZ predictors (or whether these lags are excluded from the list of predictors entirely). We observe more variation in the estimated treatment effect associated with aggravated assault based on the number of pre-treatment lags included along with the other ADZ predictors. These results are shown in Tables 43 to 78 of the Online Appendix.

[15]Our choice of ten years in this context is informed by the tradeoffs associated with using a different post-passage period. 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 would likely reduce the accuracy of our estimates of the effect of the treatment in earlier periods. This degradation would occur owing to the exclusion of additional control states from consideration in the composition of our synthetic control, which would tend to reduce the quality of our synthetic control estimates for the earlier portion of the post-treatment period. Using a shorter post-passage period risks failing to capture effects of RTC laws that take a decade to unfold.

crime was lower than it had been but 21.5 percent higher than we would have expected had it not adopted a RTC law.

Figure 1 also illustrates perhaps the most important lesson of causal inference: one cannot simply look before and after an event to see what the consequence of the event was. Rather, one needs to estimate the difference between what did unfold with the counterfactual of what would have unfolded without the event. The value of the synthetic controls methodology is that it provides an estimate of that counterfactual. Thus, when Lott (2013) 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 sizable crime decline regardless of whether the state passed a RTC law (the dotted line). More specifically, Texas experienced a 19.7% 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 33.9% 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.

Figure 1



Composition of SC: CA (55.9%), IA (17.6%), NE (26.5%)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC−Adopting States Included In Synthetic Control: IA (2011), NE (2007)

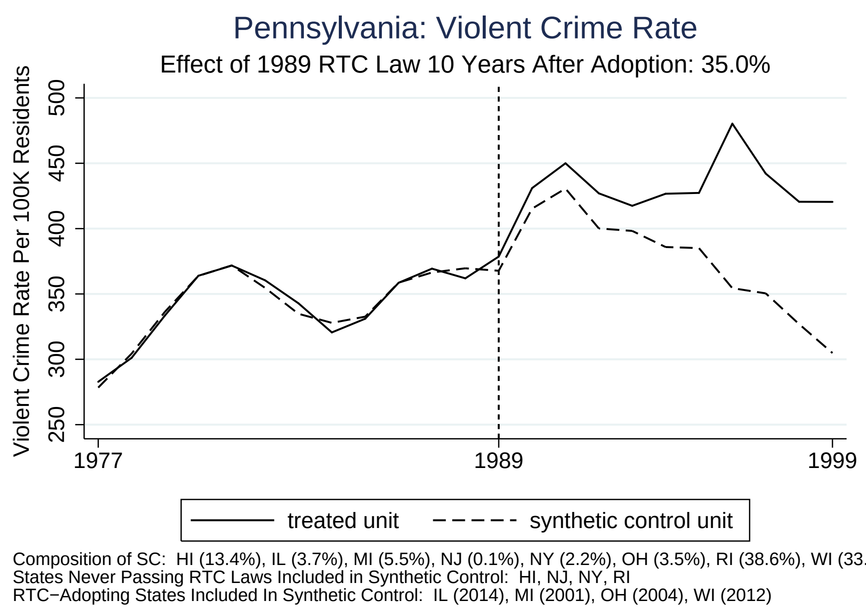
The greater transparency of the synthetic controls approach is one of the advantages of this methodology over the panel data models that we considered above. Figure 1 makes clear what Texas is being compared to and we can reflect on whether this match is plausible and whether anything over than RTC laws changed in these four states during the post-passage decade that might undermine 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 undermine the prediction that RTC laws increased crime by 21.5 percent in Texas.[16] But the death penalty, according to Lott, depresses crime, so to the extent the death penalty played a greater role in Texas than in synthetic Texas during the post-passage period (relative to the pre-passage period), then our estimate of the increase in violent crime generated by the RTC law would actually understate the true increase.

[16]Texas executed 275 convicts during the post-passage decade while California executed 11, Nebraska 2, and Iowa executed no one. The growth in the Texas incarceration rate over that same decade was 7.7%, while the growths for the three synthetic control states were 6.7% (CA), 34.5% (IA), 26.5% (NE) (Death Penalty Information Center, 2015).

Figure 2 shows our synthetic controls estimate for Pennsylvania, which adopted a 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 performance than actual Pennsylvania, and the adverse performance of Pennsylvania seems to grow after the RTC law is extended to Philadelphia in late 1995. The synthetic cohorts method estimates the impact of RTC laws in Pennsylvania is an increase of 35 percent in its violent crime rate.

Figure 2



Figures 3 and 4 show the comparable synthetic controls matches for North Carolina and Mississippi. Again both states show good pre-passage fit between the violent crime rates of the treatment state and the synthetic control. The methodology estimates that RTC laws led to an increase in violent crime in North Carolina of 8.5 percent and in Mississippi of 33.8 percent.[17]

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. One of the advantages of our task is that we have a large number of states adopting RTC laws so that the over-estimates and under-estimates will tend to wash out. Figure 5 shows the synthetic control estimates on violent crime for all 26 states for which we have ten years of post-passage data. One can see that there are a handful of states where the prediction is that crime fell with RTC adoption and then another handful for which the effect is modestly harmful. For 18 of the 26 states adopting RTC laws, the increase in violent crime is noteworthy. If one averages across all 26 states, the mean treatment effect after ten years is an 18.3 percent increase in violent crime. If one instead uses a median measure of central tendency, RTC laws are seen to increase crime by 15.1 percent.

[17] We include the graphs showing the path of the treatment states and the synthetic controls constructed for violent crime in Appendix D, 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 main outcome variable used in our analysis) based on the last post-treatment year included in our analysis. We also include a graph containing information about the individual treatment effects estimated ten years after adoption (for every state for which this information is available) in Appendix E.

Figure 3

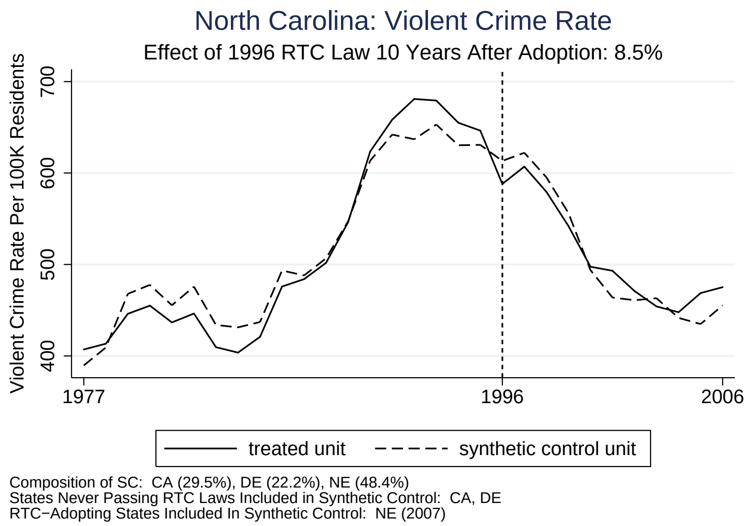
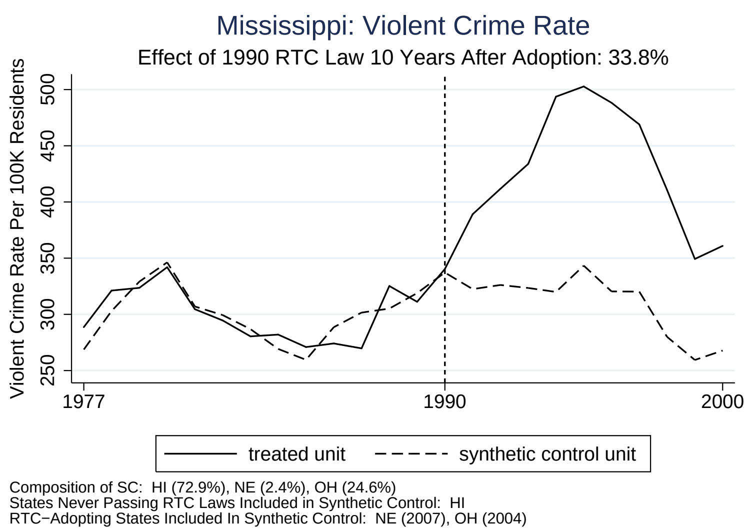


Figure 4



3. Less Effective Pre-Passage Matches

Section 1 above provided four examples in which the synthetic controls approach generated synthetic controls that matched the crime of the treatment states well in the pre-passage period, but this does not always happen. Again, one advantage of the synthetic controls approach is that one can assess the nature of this fit in the pre-passage period in order to determine how much confidence one can have in the post-passage prediction. Two states for which I would have considerably less confidence in the quality of the synthetic controls estimate are South Dakota and Maine, both of which happen to show declines in crime after RTC adoption. Indeed, these are two of the three showing notable improvements in crime following RTC adoption as indicated in Figure 5.

An examination of Figures 6 and 7 showing the synthetic controls estimates for these two states provides dramatic visual confirmation that the methodology has been unable to provide a good pre-passage fit between the crime performance of the treatment states and a suitable synthetic control.

Figure 5

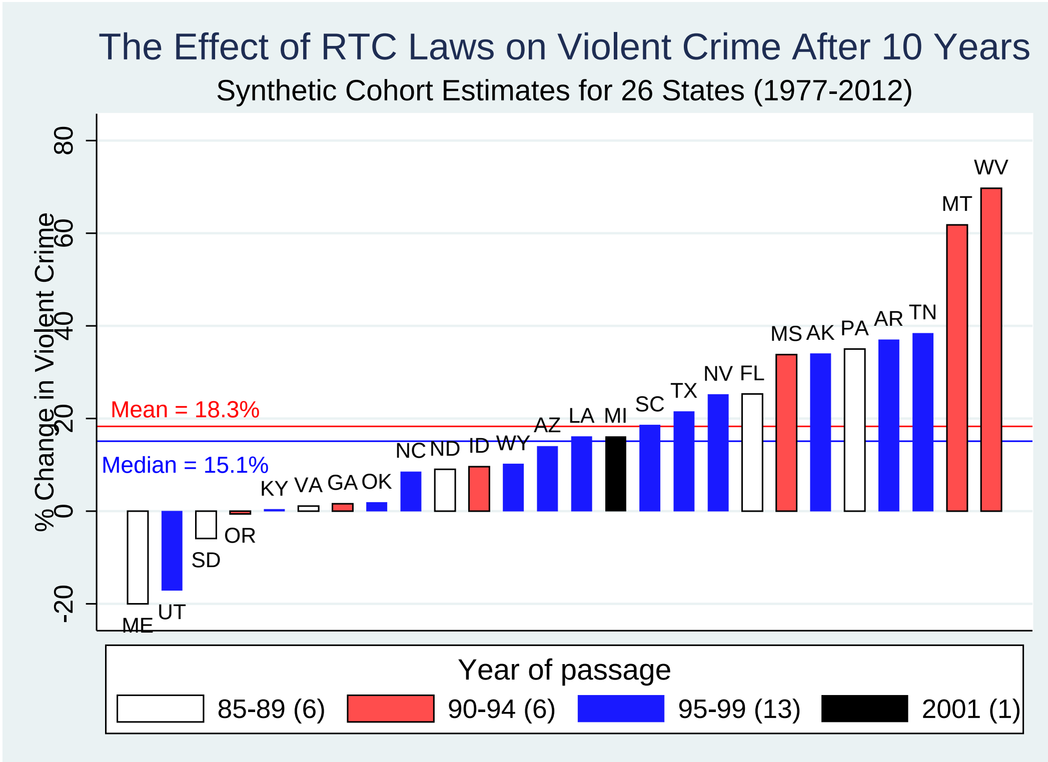
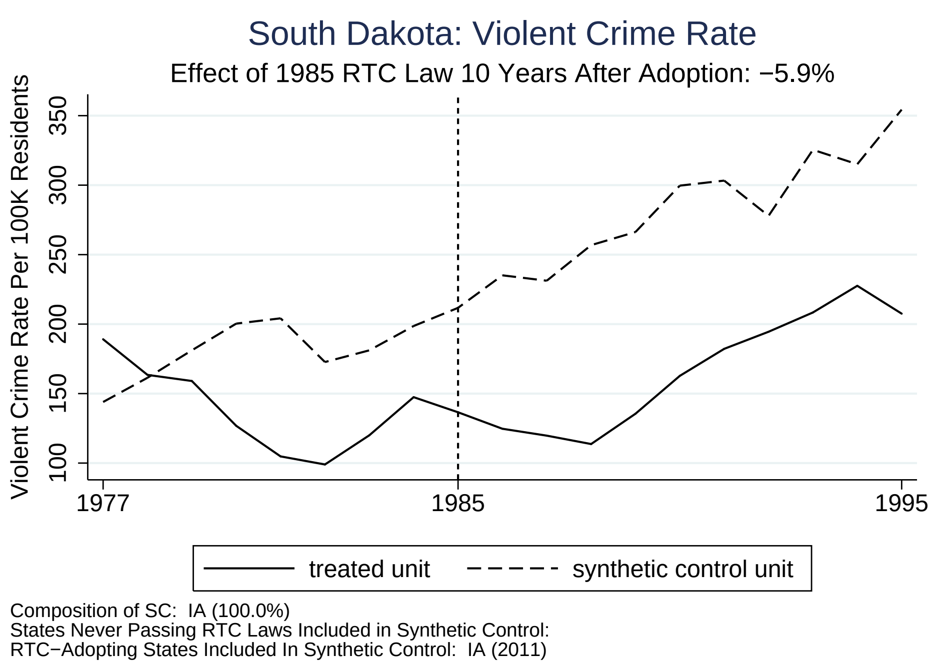


Figure 6



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 synthetic South Dakota had a far higher violent crime rate that was rising while actual South Dakota had a violent crime rate that was falling in 1985.

A very similar pattern can be seen for Maine, which again undermines confidence in the synthetic controls estimates for these two states.[18]

Figure 7

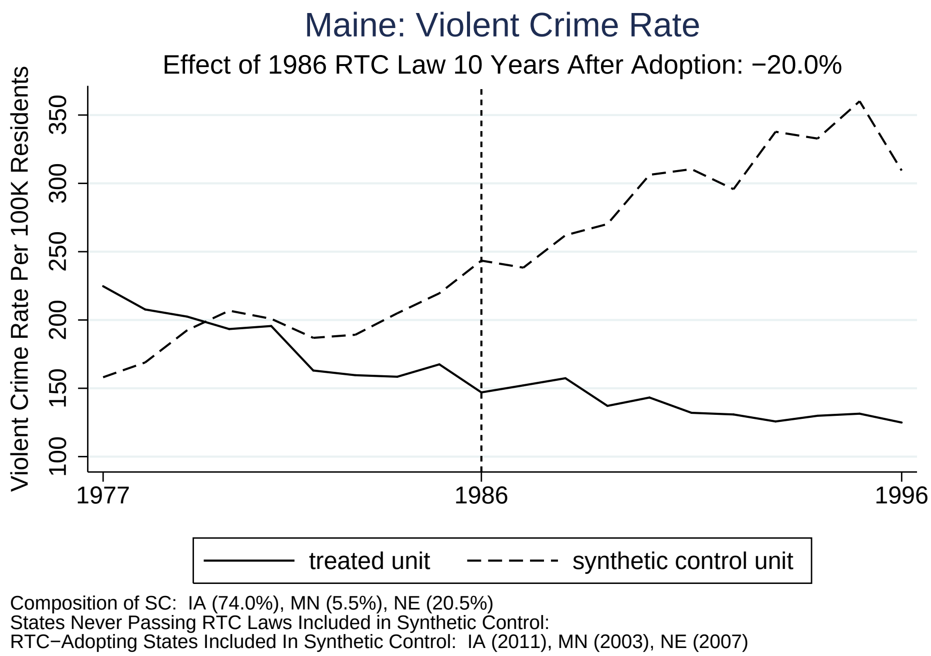


Figure 8

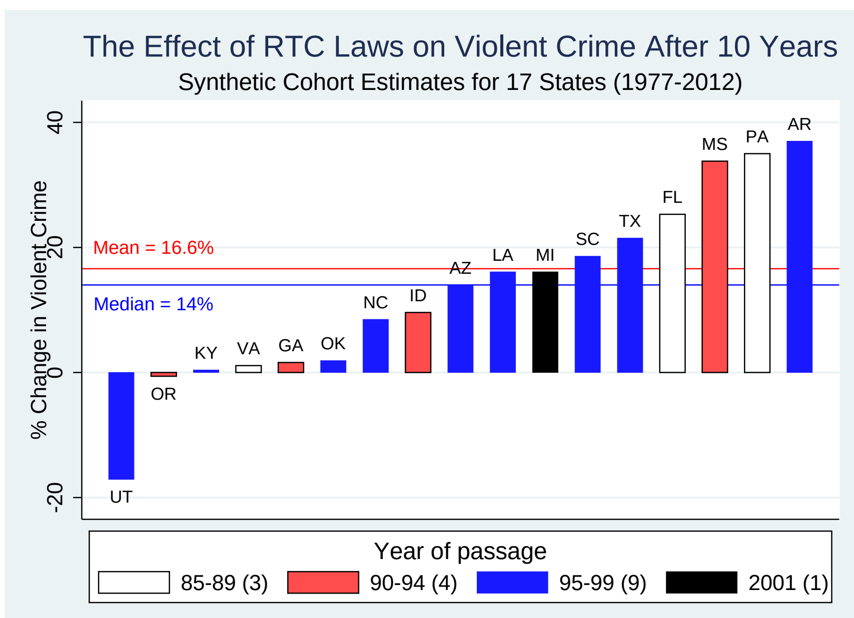


Figure 8 reproduces Figure 5 while leaving out the nine states for which the quality of pre-passage fit is

[18] As shown in Appendix D, Montana and West Virginia also have poor pre-passage fits although at least the patterns of crime of the state and the synthetic control are not going in different directions at the time of RTC adoption. Note in Figure 5 that these states have outlier estimates showing crime increases. All four of these states are dropped in the Figure 8 analysis.

clearly lower than in the remaining 17 states. This knocks out some of the outlier estimates at both ends of the spectrum, leaving a roughly similar estimated mean and median effect of RTC laws. As Figure 8 shows, the mean increase in crime across the listed 17 RTC adopting states is 16.6 percent while the median increase is 14.0 percent.[19] 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 double its prison population.

Part IV

Aggregation Analysis Using Synthetic Controls

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[20] The closest paper to the present study is Dube and 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. However, we scale this difference based on the size of the equivalent percentage gap seen between the treatment and synthetic control at the time of the treatment and report treatment effects for each of the ten years following the treatment, while Dube and Zipperer average these post-treatment percentage differences and convert this average into an elasticity. Thus, our work reports separate average treatment effects for ten yearly intervals following the time of the treatment, while Dube and Zipperer (2013) emphasize an average treatment effect (expressed as an elasticity) estimated over the entire post-treatment period. We also rely on a different procedure for using placebo treatment data to estimate the significance of our estimated average treatment effect. While Dube and Zipperer (2013) estimate the significance of their averaged treatment effects using the average rank associated with those treatments (compared to the distribution of placebo treatment effects associated with the control states used in each treatment), we compare our estimates of the average effect of RTC laws on crime rates with the distribution of average effects generated by creating placebo treatments for thirty-three randomly chosen states.

Another paper whose methodology is similar to ours is Ando (2015), which examines the impact of constructing nuclear plants on local real per capita taxable income in Japan. Ando (2015) examines multiple treatments by initially 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 in Ando (2015), we follow Ando's suggestion to repeatedly estimate average placebo effects by randomly selecting different areas to serve as placebo treatments.[21] The actual average treatment effect can then be compared to the distribution of average placebo treatment effects. Cavallo et al. (2013) perform a similar test to examine how the average of different placebo effects compares to the average treatment effect that they measure using synthetic control techniques, although their randomization procedure differs from ours by restricting the timing of placebo treatments to the exact dates when actual treatments took place. Heersink and Peterson (2014) also perform a randomization procedure to estimate the significance of their estimated average treatment effect that is similar to Ando (2015) and our own approach.

After generating nine synthetic controls (each corresponding to a different UCR crime rate) for each of the 33 states that adopted right-to-carry laws during our sample period, we estimate the percentage difference

[19] Note that Figure 5, above, reports the synthetic controls estimates concerning violent crime for the 26 states that had a full decade of post-RTC adoption data. Appendix figure E1 adds five additional states to the analysis that had at least 8 years of post-adoption data, simply showing the final year effect as though it had been a ten-year effect. The resulting mean and median effects in that figure show an increase in violent crime of 15.1% (mean) and 9.6% (median). Figure E2 then displays similar information, but restricts the states considered to those with better pre-passage fit, as shown in Figure 8. The average RTC effects on violent crime for the 22 states shown in Table E2 are 13.4% (mean) and 8.9% (median).

[20] While some papers analyze multiple treatments by aggregating the areas affected by these treatments into a single unit, this approach is not well-equipped to deal with a case such as RTC law adoption where treatments affect the majority of panel units and more than two decades separate the dates of the first and last treatment under consideration.

[21] 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 right-to-carry laws, but this simply means that our placebo estimates will likely be biased *against* finding a qualitatively significant effect of right-to-carry laws on crime (since some of our placebo treatments will be capturing the effect of the passage of right-to-carry laws on crime rates).

between the treatment state and the corresponding synthetic control in both the year of the treatment and in the ten years following it (we obviously use data from fewer post-treatment years for states that had RTC laws that took effect less than ten years before the end of our sample). We then subtract the percentage difference estimated for each state during the year of the treatment from the corresponding percentage difference estimated in each of the ten years following the passage of the RTC law to generate up to ten estimates of the change in the percentage difference between the treatment state and its synthetic control between the time of the treatment and a given post-treatment year.[22] We then aggregate these estimates of the changes associated with a specific crime rate and number of years since treatment and test whether they are significantly different from zero.[23]

As we saw in Figures 1-4 and 6-7, the validity of using the post-treatment difference between crime rates in the treatment state and its corresponding synthetic control as a measure of the effect of the treatment depends on the strength of the match between these two entities 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 equal to the ratio of the synthetic control's pre-treatment RMSPE and the pre-treatment average level of the outcome variable for the treatment state.[24] After estimating the regressions described in the paragraph above using the full sample, we consider two subsamples of treatment states: treatment 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 regressions using each of these two subsamples to examine whether restricting our estimation of the average treatment effect to states for which a relatively "better" synthetic control could be identified would meaningfully change our findings.

A. RTC Laws Increase Violent Crime

We now turn our attention to the aggregated results of our synthetic control analysis using predictors derived from the ADZ specification. Table 7 shows our results on the full sample examining violent crime.[25]. Our estimates suggest that states that pass RTC laws experienced more deleterious changes in violent criminal activity than their synthetic controls in the ten years after adoption. On average, treatment states have aggregate violent crime rates that are around 9% higher than their synthetic controls six years after passage and 18% higher ten years after passage. Table 7 suggests that the 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.

[22]An obvious alternative would be to simply estimate the percentage difference between the treatment and the synthetic control in each post-treatment period, and using this approach would not change our results. The intuitive rationale for our choice of outcome variable was that systematic pre-treatment differences between the treatment state and its synthetic control likely reflected imperfections in the process of generating a synthetic control and should not contribute to our estimated treatment effect if possible. In other words, if the treatment state had a crime rate that was 5% greater than that of the synthetic control in both the pre-treatment and post-treatment period, it would seemingly create a misleading impression to ignore the pre-treatment difference and declare that the treatment increased crime rates by 5%.

The mean (median) percentage difference between the treatment state and the synthetic control in the year of the treatment was 4.7% (0.14%), with 16 treatment states showing greater crime rates than their synthetic controls and 17 treatment states showing less crime than their synthetic control in that year.

[23]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.

[24]While the RMSPE is typically used to assess this fit, we believe that the use of this measure is not ideal in the present context owing to the wide variation that exists in the average pre-treatment crime rates between the thirty-three treatment states that we consider. For example, the pre-treatment RMPSE associated with our synthetic control analysis using the ADZ predictor variables and aggregate violent crime as the outcome variable is similar for Arkansas (44.2) and Arizona (44.4), but the pre-treatment levels of Arizona's aggregate violent crime rate are far greater than Arkansas's. (For purposes of comparison, Arizona's aggregate violent crime rate in the year prior to the implementation of its right-to-carry law was 703.1 violent crimes per 100,000 residents, while the corresponding figure for Arkansas was 553.2 violent crimes per 100,000 residents.)

[25]All of the aggregated ADZ synthetic cohort results are shown in Tables A5 to A31 of Appendix A.

Table 7: Synthetic Control Estimate of Impact of RTC Law, ADZ Specification, Violent Crime Rate, Full Sample, 1979-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.033	2.406*	3.673**	5.012**	8.721***	9.226***	11.819***	13.874***	17.610***	18.292***
	(1.183)	(1.393)	(1.632)	(2.173)	(2.453)	(2.801)	(2.989)	(3.430)	(3.230)	(2.966)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.968	0.292	0.204	0.156	0.060	0.072	0.040	0.032	0.016	0.020
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.116	0.168	0.192	0.172	0.192	0.212	0.196	0.220	0.212	0.252
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.084	0.104	0.108	0.120	0.128	0.128	0.140	0.140	0.176
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.032	0.036	0.036	0.060	0.044	0.044	0.056	0.076	0.068

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 8 repeats the Table 7 analysis while dropping the 3 states with a CV of the RMSPE that is twice the average of the sample. Table 9 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. All these tables show roughly identical conclusions. Across all three tables, RTC laws are consistently shown to increase violent crime starting three years after passage.

Table 8: ADZ Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1979-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.111	2.521*	3.881**	5.143**	8.992***	9.320***	11.816***	13.741***	17.393***	18.174***
	(1.202)	(1.414)	(1.643)	(2.198)	(2.467)	(2.838)	(3.036)	(3.486)	(3.291)	(3.013)
N	30	30	30	30	30	28	28	28	25	23
Pseudo P-Value	0.924	0.292	0.188	0.128	0.052	0.064	0.048	0.032	0.016	0.024
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.120	0.192	0.184	0.128	0.164	0.200	0.180	0.220	0.208	0.256
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.096	0.104	0.104	0.112	0.120	0.132	0.152	0.148	0.180
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.020	0.024	0.032	0.040	0.040	0.044	0.056	0.068	0.064

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
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 9: ADZ Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1979-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.014	2.491*	3.948**	5.301**	9.296***	9.408***	12.037***	13.763***	16.963***	17.825***
	(1.239)	(1.455)	(1.685)	(2.252)	(2.519)	(2.882)	(3.070)	(3.536)	(3.293)	(3.003)
N	27	27	27	27	27	26	26	26	23	21
Pseudo P-Value	0.996	0.272	0.196	0.124	0.028	0.040	0.024	0.020	0.016	0.016
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.092	0.152	0.148	0.152	0.168	0.180	0.196	0.192	0.204	0.232
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.080	0.096	0.092	0.096	0.128	0.128	0.124	0.132	0.164
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.012	0.012	0.012	0.016	0.036	0.040	0.032	0.032	0.056	0.048

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B. Impact of RTC Laws on Other Crimes

Examining the other ADZ synthetic cohort estimates in Appendix A reveals that RTC laws also increase aggravated assault. This is not surprising, given the high proportion of aggregate violent crime which is composed of aggravated assaults. All three of our treatment state selection criteria indicate that RTC laws significantly increase aggravated assault rates seven, eight, and nine years after passage, between 12% and 18% depending on the exact specification used.[26]

[26] We examine how our findings for aggregate violent crime and aggravated assault change under a large array of alternative specifications, including (a) excluding states adjacent to the treatment state from the pool of control states considered for inclusion in the synthetic control (tables 109-114 of the Online Appendix), (b) using the same retirement payments variable featured in the LM specification in the ADZ specification (tables 115-120 of the Online Appendix), (c) excluding California from

A weaker version of the pattern of estimated crime increases in the later years of the first decade of operation of RTC laws is also found for rape, murder, and robbery, although less powerfully than for aggregate violent crime and aggravated assault. The rape effect loses significance when the sample is restricted to those states with a below average CV of the RMSPE, murder is only significant at the .05 level in the tenth year under that same sample restriction (suggesting an almost 13 percent increase), and robbery rates are always higher five years after RTC adoption, they are never statistically significant in any of the three ADZ synthetic controls estimates.[27]

Contrary to Lott's contention that permissive concealed carry statutes will tend to significantly increase property crime, we find no evidence of any such effect on aggregate property crime. Our estimated average effects tend to be small in magnitude and not statistically significant. While there is some evidence of increases in burglary associated with RTC adoption, those positive estimates become insignificant and decline in magnitude when examining the results of our most restrictive treatment state selection criteria.

C. The Placebo Analysis

Our ability to make valid inferences from our synthetic control estimates depends on the accuracy of our standard error estimation. To determine whether our standard errors might be biased and to get a sense of whether the coefficients that we measure are qualitatively large compared to those that would be produced by chance, we incorporate an analysis using placebo treatment effects similar to Ando (2015). For this analysis, we generate 250 sets of randomly generated RTC dates that are designed to resemble the distribution of actual RTC passage dates that we use in our analysis.[28] We then use the synthetic control methodology and the ADZ predictors to estimate thirty-three synthetic controls for each possible combination of placebo dates and crime category. 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

the composition of all synthetic controls (tables 121-126 of the Online Appendix), (d) excluding Delaware from the composition of all synthetic controls (tables 127-132 of the Online Appendix), (e) excluding Hawaii from the composition of all synthetic controls (tables 133-138 of the Online Appendix), (f) excluding Iowa from the composition of all synthetic controls (tables 139-144 of the Online Appendix), (g) excluding Illinois from the composition of all synthetic controls (tables 145-150 of the Online Appendix), (h) excluding Maryland from the composition of all synthetic controls (tables 151-156 of the Online Appendix), (i) excluding Nebraska from the composition of all synthetic controls and from the list of treatment states under consideration (tables 157-162 of the Online Appendix), (j) excluding New York from the composition of all synthetic controls (tables 163-168 of the Online Appendix), (k) excluding Rhode Island from the composition of all synthetic controls (tables 169-174 of the Online Appendix), (l) excluding Wisconsin from the composition of all synthetic controls (tables 175-180 of the Online Appendix), (m) including Indiana as a treatment state (tables 181-186 of the Online Appendix), (n) using separate controls for % female, % white, % black, and % in 6 age categories instead of the six demographic variables mentioned above (tables 187-192 of the Online Appendix), (o) using the 36 Lott-Mustard demographic variables instead of the six demographic variables mentioned above (tables 193-198 of the Online Appendix), and (p) using the median of the coefficient of variance instead of the mean in our treatment state selection criteria (tables 199-204 of the Online Appendix).

For aggregate violent crime, we report treatment effects that are greater than 9% after seven years and greater than 14.5% after ten years for almost all of these specifications and for every treatment selection criteria combination. The only exceptions are the specifications excluding New York (estimated effect: around 8.4-8.6% after seven years and around 13.6-14.3% after ten years), Nebraska (estimated effect: around 8.8-8.9% after seven years and around 14.8-15.5% after ten years), and Maryland (estimated effect: around 8.3-8.5% after seven years and 10.8-11.4% after ten years) from our list of control units. Our results are also consistently significant five or more years post-passage.

Our results for aggravated assault are more inconsistent between specifications. We observe consistently large treatment effects (more than 10% eight years after passage and more than 12% ten years after passage in all three specifications) when using medians to determine the cutoffs for our treatment selection criteria, when including Indiana in the list of treatment states, when excluding California, Delaware, Hawaii, Wisconsin, or Rhode Island from the list of control units, when using the Lott demographic variables as predictors, and when excluding adjacent states from the list of control units. For several specifications – the one using an alternative measure of retirement payments, the specification using nine alternative demographic variables, and those which exclude Iowa, Illinois, Nebraska. or New York from the list of control states – we only observe treatment effects that meet these criteria when using the two less restrictive treatment state selection criteria. We observe our lowest estimated treatment effects ten years after passage when excluding Maryland from the sample, and our treatment effect estimates for this specification range between 10.4-12.0% eight years after passage and 10.5-11.5% ten years after passage.

[27]Prior to the tenth year, the murder rate results are far less precise than the violent crime estimates. The well-known inverse relationship between the criminogenic influence of the crack epidemic on a state's homicide rates and the likelihood of RTC adoption have made it particularly difficult to estimate the impact of RTC laws on murder during our data period.

[28]More specifically, we randomly choose eight states to never pass right-to-carry laws, six states to pass right-to-carry laws before 1981, 33 states to pass right-to-carry laws between 1981 and 2010, and three states to pass their right-to-carry 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.

years (for which we have data) that follow it. We take the difference between the percentage difference during a given post-treatment year and the year of the treatment and examine whether this difference is statistically significant (using the methodology described in footnote 21). We also repeat our estimation of the average treatment effect associated with each of the ten post-treatment years and nine UCR crime rates after excluding states whose coefficient of variation is either one or two times the average observed for all (placebo) treatment states, leaving us with 270 coefficients and p-values corresponding to each of the 250 sets of randomly generated placebo treatments that we consider.

At the bottom of Tables 7-90 (and all the synthetic controls tables of Appendix A), we list the proportion of each post-treatment year's placebo regressions that were significant at the .10 level, .05 level, and .01 level. We provide these proportions to give the reader an intuitive sense of the possible bias associated with our standard error estimation, although (for the reasons noted in the Methodology section) it is likely that these placebo estimates are capturing some of the effect of RTC laws. Table 7 shows that the placebo results appear to be significant at the .01 level 2 percent of the time for our first year after passage to 4.4 percent in the tenth year. In other words, the standard errors we report at the top of Table 7 are potentially underestimated, as our placebo averages are statistically significant more often than would be expected by chance.[29]

As another check on the statistical significance of our results, we compare each of the ten coefficient estimates in Figure 7 with the distribution of the 250 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 treatment effects whose absolute value is greater than the absolute value of the given estimated treatment effect. This pseudo p-value gives an intuitive sense 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 significantly greater than the vast majority of our estimated average placebo treatment effects.[30]Examining our Table 7 pseudo p-values, we see that our violent crime results are significant in comparison to the distribution of placebo coefficients at the .05 level after seven years have passed since the treatment date for all three of the treatment selection criteria that we consider.

Our coefficients for aggravated assault are significant in comparison to our placebo distributions at the .05 level after seven years for our two less restrictive selection criteria and after eight years for our most restrictive treatment state selection criteria.

None of our coefficients associated with our murder, property, and auto theft specifications are significant at the .10 level. One (negative) coefficient associated with the larceny rate one year after treatment in the <2X coefficient of variation is significant at the .10 level, although this is likely to be merely statistical noise.

D. Synthetic Control Estimates Using the LM Explanatory Variables

In spite of the marked differences that we observe between the ADZ and LM models when implemented in a panel data regression framework, we find that these models produce quite similar results when implemented under a synthetic control framework.[31] Specifically, the coefficients associated with our synthetic control

[29]In general, we reject the null hypothesis when using the placebo treatments at the .10 level between 15% and 32% of the time, at the .05 level between 8% and 25% of the time, and at the .01 level between 2% and 11% of the time. Unfortunately, we do not observe any consistent tendency for the size of our tests and the frequency with which we reject the null hypothesis to converge when restricting the sample to states with a relatively low coefficient of variation.

[30]Owing to the time that would be required to perform this analysis when using the maximum likelihood estimation technique mentioned in footnote 13, we perform this placebo analysis using the synth module's default regression-based technique for estimating the weights assigned to each predictor when constructing the synthetic control. We made this decision owing to the technical limitations surrounding the application of the synthetic control technique, as we estimated that it would take significantly longer for this analysis to be run using the *nested* option that we employed in our main analysis. This change should bias our estimates *against* finding a significant effect of right-to-carry laws on crime. Since the nested option tends to improve the matching process between states, we would expect larger deviations between our placebo treatments and their synthetic controls when this option is not utilized. This would suggest more dispersion in our estimated placebo treatment effects and a greater likelihood of estimating that our actual treatment effects were not significantly different from the distribution of placebo effects. Moreover, when performing an earlier version of the analysis included in this paper, we found that our estimated pseudo p-values were conservatively estimated when comparing our non-nested pseudo p-values with those produced using the *nested* function.

[31]In conducting the LM panel data analysis, we use the violent and property arrest rates rather than the crime-specific arrest rates described by Lott and Mustard (1997) owing to the fact that this would essentially (and improperly) place the same

models using the Lott and Mustard (1997) predictors (Appendix A Tables A32 to A58) suggest that RTC laws are associated with harmful effects on the overall violent crime rate and aggravated assault. Tables 10-12 report the three different results based on the full sample and our two restricted samples examining violent crime using the LM specification. The detrimental effects of RTC laws on violent crime rates are observable for all three of the treatment state selection criteria that we use, are statistically significant at the .05 level starting five years after the passage of a RTC law, and appear to increase over time. The treatment effects associated with violent crime in Tables 10-12 range between 8.0-8.2% in the seventh post-treatment year and 11.1-12.1% in the tenth post-treatment year.

Table 10: Lott and Mustard Specification, Violent Crime Rate, Full Sample, 1977-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.110	1.946	3.077	2.767	5.266**	5.140**	8.225***	9.573***	11.622***	12.141***
	(1.208)	(1.498)	(2.033)	(2.039)	(2.043)	(2.471)	(2.650)	(3.076)	(3.212)	(3.258)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Lott and Mustard Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1977-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.122	1.979	3.183	2.828	5.443**	5.073*	8.073***	9.275***	11.146***	11.687***
	(1.227)	(1.520)	(2.059)	(2.068)	(2.060)	(2.515)	(2.701)	(3.130)	(3.272)	(3.298)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Lott and Mustard Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1977-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.039	1.907	3.183	2.841	5.569**	5.050*	8.163***	9.161***	10.495***	11.133***
	(1.265)	(1.566)	(2.122)	(2.128)	(2.116)	(2.560)	(2.742)	(3.184)	(3.284)	(3.307)
N	27	27	27	27	27	26	26	26	23	21

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our ADZ aggravated assault results are consistently significant seven to nine years after treatment and range between 11% to 13% over this range for all three of the treatment state selection criteria that we consider.[32] The coefficient estimates associated with murder are negative (but insignificant) for all three

variable on both sides of the regression model. This objection is less important under the synthetic control framework. For this reason, we use the contemporaneous crime-specific arrest rates in our synthetic control model using the Lott and Mustard (1997) control variables.

[32]In Tables 205-210 of the Online Appendix, we re-run our analysis using the Lott and Mustard (1997) specification for aggravated assault and violent crime with one additional change: we substitute nine alternative demographic variables (% female, % white, % black, and % in 6 age categories). We find that this change amplifies our estimated treatment effects seven or more years after the treatment in the case of aggravated assault and four or more years after the treatment in the case of violent crime. For aggregate violent crime, our estimates of the average treatment effect range between 11.8-13.3% seven years after treatment and 17.8-18.7% ten years after treatment (our results for aggravated assault are similar).

treatment state selection criteria for the first seven post-passage years that we consider but are consistently positive (and significant) ten years after the treatment. We find no consistent patterns in our estimated average treatment effects when considering the robbery rate as an outcome variable. We estimate some significant positive coefficients associated with our average treatment effects between eight and nine years after treatment when using the rape rate as an outcome variable, and this finding is upheld for all three groups of treatment states that we analyze.

Our property crime results are also similar when using both the LM and ADZ specifications in the synthetic controls framework. We find no support for the contention that permissive concealed carry laws either increase or decrease the aggregate property crime rate, as the coefficients associated with our synthetic control estimates are both small and insignificant. We reach a similar conclusion when using the larceny or auto theft rate as the dependent variable. We observe scattered evidence that RTC laws are associated with increases in burglary rates, although this finding weakens considerably when restricting our sample to states with better pre-treatment fits.

E. Synthetic Control Estimates Using the MM Explanatory Variables

As we saw with both the ADZ and LM specifications, once we employ a synthetic controls approach we find strong evidence that RTC laws are associated with higher violent crime and aggravated assault rates when using the Moody and Marvell (2008) predictors.[33] These results are shown in Appendix Tables A59 to A85. More specifically, our violent crime estimates using the MM specification in Tables 13-15 are statistically significant starting only three years after passage of a RTC law, with the size of the estimated detrimental effect of these laws on crime steadily increasing from this point forward. All three of treatment state selection procedures indicate that RTC states experienced overall violent crime rates that were 18% to 20% greater than those of their synthetic controls nine to ten years after passage, a difference that was found to be significant at the .01 level.

Table 13: Moody and Marvell Specification, Violent Crime Rate, Full Sample, 1980-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.197	2.969*	4.812**	6.110**	9.052***	9.563***	12.216***	14.241***	18.511***	20.015***
	(1.261)	(1.716)	(1.997)	(2.326)	(2.735)	(3.075)	(3.371)	(4.171)	(4.287)	(4.202)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Moody and Marvell Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1980-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.260	3.053*	4.991**	6.230**	9.301***	9.623***	12.166***	14.061***	18.232***	19.849***
	(1.281)	(1.742)	(2.020)	(2.354)	(2.753)	(3.119)	(3.428)	(4.244)	(4.381)	(4.285)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We estimate a similar effect of RTC laws on aggravated assault, although the coefficients associated with this outcome variable are smaller and less frequently statistically significant. Nevertheless, our aggravated

[33] For the same reasons described in footnote 29, we use the lagged violent or property crime arrest rate in our regression tables but use the contemporaneous violent or property crime arrest rate as a predictor in our synthetic controls code for the MM specification.

Table 15: Moody and Marvell Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1980-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.385	2.275	4.014**	5.660**	9.566***	10.000***	12.876***	14.577***	18.596***	19.574***
	(1.243)	(1.685)	(1.880)	(2.427)	(2.929)	(3.260)	(3.527)	(4.417)	(4.489)	(4.371)
N	25	25	25	25	25	24	24	24	21	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA MI MO MS NC NM NV OH OK OR PA SC TN TX UT VA
States excluded for poor pre-treatment fit: ME MN MT ND NE SD WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

assault estimates indicate that RTC laws are associated with 13% to 14% higher aggravated assault rates nine to ten years after the passage of these laws.[34]

We also observe that RTC laws are associated with significant increases in sexual assault eight and nine years after passage under two of our three model selection criteria. However, no significant effect of RTC laws on sexual assault is observed when using the most restrictive treatment selection criteria. None of the coefficients associated with the MM robbery results are statistically significant. Similar to the LM specification, we find that our estimated treatment effects are statistically significant under the MM specification when using the murder rate as an outcome variable in the tenth post-treatment year. Once again, we observe that the magnitude of the deleterious effect of RTC laws on different categories of violent crime tends to increase over time.

Turning our attention to property crimes, we find little systematic evidence that RTC laws influence property crime in the synthetic control approach. Our aggregate property crime and larceny results, for instance, are never significant and change signs as our treatment selection criteria changes. The Moody and Marvell (2008) specification, like the Lott and Mustard (1997) predictors, provides no evidence that permissive concealed carry laws are associated with meaningfully lower or higher rates of auto theft. Burglary, however, is a more challenging case, as RTC laws are sporatically associated with significantly higher burglary when using two of our three treatment state selection criteria. However, no statistically significant effects are observed when restricting the sample to treatment states whose coefficients of variation are less than average.

F. Synthetic Control Estimates Using the LM Explanatory Variables and the MM Explanatory Variables with Alterations

In Sections B2-3 of Part II above, we discussed the deficiencies of the LM and MM specifications for including 30 unnecessary demographic variables and for failing to control for both incarceration and police. Tables 16 and 17 correct the LM and MM specifications for these elements in the synthetic controls framework, providing estimates for the impact of RTC laws on violent crime.[35]

[34] In Tables 211-216, we consider an alternative version of the MM specification that uses the same retirement variable that was originally used in the LM specification. The treatment effects that we estimate using this specification and violent crime as the outcome variable are qualitatively similar to the other results presented in this section. However, the treatment effects that we estimate for this robustness check using aggravated assault are only significant between seven and nine years after RTC adoption and are smaller than the effects that we estimate for our main MM specification. More specifically, our estimated treatment effects range between 9.6% and 10.7% seven years after passage and between 10.1% to 14.4% ten years after passage in this alternative specification.

[35] For both models, RTC laws are seen to substantially increase violent crime. Corrected LM and MM specifications for the other crime categories can be found in Tables A59-66 and Tables A94-101, respectively.

Table 16: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Violent Crime Rate, Full Sample, 1977-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.276	1.932	2.533	3.198	6.207*	6.790*	8.915**	10.419**	13.821***	13.238***
	(1.285)	(1.686)	(2.090)	(2.704)	(3.115)	(3.634)	(3.611)	(3.975)	(4.101)	(3.894)
N	33	33	33	33	33	31	31	31	28	26

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

In the case of the LM specification, we observe (by comparing Table 10 to Table 16) that using the six ADZ demographic variables–in place of the previous 36–and controlling for incarceration and police changes our treatment effects associated with violent crime from 8.23% to 8.92% in the seventh post-treatment year and from 12.14% to 13.24% in the tenth post-treatment year. Similarly, in the case of the MM specification, we observe (by comparing Table 13 to Table 17) that using the six ADZ demographic variables and controlling for police changes our violent crime treatment effects from 12.22% to 9.91% in the seventh post-treatment year and from 20.02% to 16.78% in the tenth post-treatment year. In both specifications, the tenth post-treatment year effects remain significant at the one-percent level.

Note that the synthetic controls estimates are far less sensitive to the changes in explanatory variables that were highly influential in the panel data estimates. Our best estimates of the tenth-year impact of RTC laws on violent crime from the aggregate synthetic controls approach range from a low of 13.2% in the LM model (Table 16) to a high of 18.3% in the ADZ model (Table 7). The MM synthetic controls results fall in between with an estimate that RTC laws increase violent crime by 16.8% (Table 17).

Table 17: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Violent Crime Rate, Full Sample, 1980-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.194	1.992	3.321*	4.401*	7.097**	7.605**	9.906***	12.184***	15.353***	16.776***
	(1.262)	(1.476)	(1.913)	(2.389)	(2.638)	(2.911)	(3.138)	(3.410)	(3.495)	(3.713)
N	33	33	33	33	33	31	31	31	28	26

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

Part V

Conclusion:

The results presented in this paper provide strong evidence undermining the "More Guns, Less Crime" hypothesis. Our preferred ADZ panel data 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. We then supplemented our panel data results using our synthetic control methodology, again using the ADZ, LM, and MM specifications. For all three specifications (specifications that reached opposing conclusions about the relationship between RTC laws and crime rates when implemented under a regression framework), states that passed RTC laws experienced higher aggregate violent and aggravated assault rates than their synthetic controls (results that were either significant at either the .05 or .01 level after seven to nine years in the case of aggravated assault and after five years in the case of violent crime). The effects that we measure represent meaningful shifts in crime rates, with our tables indicating that violent crime can be expected to increase by roughly 11-12% (under the LM specification) and 18-20% (under the ADZ and MM specifications) over the counterfactual case around ten years after a RTC law is passed. This conclusion remains unchanged after restricting the set of treatments 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 average placebo effects indicates that the deleterious effects associated with RTC laws that we estimate for aggregate violent crime and aggravated assault are qualitatively large compared to those that we would expect to observe by chance.

The similar results across the ADZ, LM, and MM specifications (as well as with the ADZ and modified LM and MM panel data estimates) and the robustness of our synthetic controls estimates to various robustness checks give us greater confidence in the validity of our finding that RTC laws are associated with increases in aggregate violent crime rates. Nonetheless, estimation using observational data always rests on numerous assumptions, so one must always be alert to potential shortcomings. For example, if states that were expected to experience future declines in crime were less likely to adopt RTC laws than states that anticipated future crime increases, we might estimate a detrimental effect of RTC laws on crime which is not (entirely) causal in nature. Given the very limited ability of politicians, pundits, and even academic experts to correctly predict crime trends over this period, this problem of endogeneity is unlikely to mar our results.

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" that RTC laws are not associated with a contemporaneous increase in crime rates. The conflict between these findings is resolved when one realizes that since the 1990s most states have experienced large and qualitatively important crime decreases, including those adopting RTC laws. However, our analysis reveals that the counterfactuals for these treatment states more often than not experienced even larger crime declines.

Finally, while this paper has focused on the statistical estimation of the impact of RTC laws, it is useful to consider the mechanisms by which RTC laws would lead to increases in violent crime. The most obvious mechanism is that the RTC permit holder may commit a crime that he or she would not have committed without the permit. This suggests that a RTC permit holder is not likely to be a Dylann Roof type of criminal (he purchased a gun – improperly – shortly before he committed mass murder in Charleston, South Carolina). Roof was going to use the gun to commit a crime as soon as he procured a weapon; he wasn't waiting around to get a gun permit. Instead, George Zimmerman, the popcorn killer at a Florida movie theater, and the angry gas station killer (shooting a black teen for playing loud rap music) are all individuals who were much more likely to commit a violent crime only because they had a RTC permit. Of course, aggravated assaults are far more common than murder (albeit far less visible to the public), so the same impulses that generate killings also work to stimulate aggravated assaults.

Some have questioned whether permit holders commit enough crime to substantially elevate violent criminality, citing apparently low rates of official withdrawals from permit holders convicted of crimes. Two points need to be made in response to this claim. First, official withdrawals will clearly understate criminality by permit holders. Convictions for aggravated assault are far smaller than acts of aggravated assault, so many permit holders would never face official withdrawal of their permits even if they committed a violent criminal act that would warrant termination of their permit. Second, 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. Again, without permits this would likely have not been a double homicide, but note that no official action to terminate permits is recorded in a case like this (Stuart, 2013).

The second point is that RTC laws also increase crime by individuals other than permit holders in a variety of ways. First, the culture of gun carrying can promote confrontations. Presumably, George Zimmerman would not have hassled Trayvon Martin if Zimmerman had not had a gun. If Martin had killed Zimmerman, the gun permit then could have been viewed as a stimulant to crime (even if the permit holder was not the ultimate perpetrator). The message of fighting back and standing your ground is a clear message of the gun culture that may well invite more hostile confrontations that lead to more violence. This attitude is likely reinforced by the adoption of RTC laws. 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, the bumper sticker on Mockewich's car had an NRA bumper sticker reading "Armed with Pride" (Gibbons and Moran, 2000). If you are an angry young man, with a bit of paranoid streak, and you haven't yet been convicted of a crime or adjudicated to be a mental defective, it is likely that the ability to carry a gun will both be more attractive and more likely in a RTC state. That such individuals will be more likely to engage in acts of violence once armed should not be surprising.

Second, individuals who carry guns around are a constant source of arming criminals. When Sean Penn obtained a permit to carry a gun, his car was stolen with two guns in the trunk. The car was recovered two days later but the guns were gone (Donohue, 2003). Just this month in San Francisco, the theft of a gun from a car in San Francisco led to a killing that almost certainly would not have occurred as a tourist walked on a city pier. 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. Of course, the San Francisco killer did not have a RTC permit; although the owner of the gun used in the killing did (Ho, 2015).

Third, as more citizens carry guns, more criminals will find it more beneficial to carry guns and use them more quickly and more violently to thwart any potential armed resistance. Fourth, 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. Fifth, it almost certainly adds to the burden of a police force to have to deal with armed citizens. 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. Police may be less enthusiastic about investigating certain suspicious activities given the greater risks that widespread gun carrying poses to them. Police resources are needed to process gun permits that could be use to directly fight crime. All of these are a tax on police, and therefore one would expect law enforcement to be less effective on the margin, thereby contributing to crime.

The fact that two different types of statistical data – panel data regression and synthetic controls – with varying strengths and shortcomings and with different model specifications both yield consistent and strongly statistically significant evidence that RTC laws increase violent crime constitutes persuasive evidence that any beneficial effects from gun carrying are likely substantially outweighed by the large increases in violent crime that these laws stimulate.[36]

[36]Unfortunately, even the enormous stock of guns in the U.S. is unable to be brought to bear in stopping criminal violence the vast majority of the time that someone is threatened with violent crime. A five-year study of violent crime 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 in civilian hand(Planty and Truman, 2013).

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Appendix A: Tables

Table A1: RTC Adoption Dates

state	RTC (Old)	RTC (New)	Fraction of Year In Effect Year of Passage
Alabama	1975	1975	
Alaska	1994	1995	0.2520547945
Arizona	1994	1995	0.4602739726
Arkansas	1995	1996	0.4328767123
California	0	0	
Colorado	2003	2003	0.6273972603
Connecticut	1970	1970	
Delaware	0	0	
District of Columbia	0	0	
Florida	1987	1988	0.2520547945
Georgia	1989	1990	0.3534246575
Hawaii	0	0	
Idaho	1990	1990	0.504109589
Illinois	2014	2014	
Indiana	1980	1980	0.9617486339
Iowa	2011	2011	1
Kansas	2007	2007	1
Kentucky	1996	1997	0.2513661202
Louisiana	1996	1996	0.7021857923
Maine	1985	1986	0.2849315068
Maryland	0	0	
Massachusetts	0	0	
Michigan	2001	2001	0.504109589
Minnesota	2003	2003	0.597260274
Mississippi	1990	1990	0.504109589
Missouri	2004	2004	0.8469945355
Montana	1991	1992	0.2520547945
Nebraska	2007	2007	1
Nevada	1995	1996	0.2520547945
New Hampshire	1959	1959	
New Jersey	0	0	
New Mexico	2004	2004	1
New York	0	0	
North Carolina	1995	1996	0.0849315068
North Dakota	1985	1986	0.4191780822
Ohio	2004	2004	0.7322404372
Oklahoma	1996	1996	1
Oregon	1990	1990	1
Pennsylvania	1989	1989	0.5424657534
Rhode Island	0	0	
South Carolina	1996	1997	0.3579234973
South Dakota	1985	1985	0.504109589
Tennessee	1996	1997	0.2513661202
Texas	1996	1996	1
Utah	1995	1995	0.6712328767
Vermont	1970	1970	
Virginia	1986	1986	0.504109589
Washington	1961	1961	
West Virginia	1989	1990	0.4876712329
Wisconsin	2011	2012	0.1671232877
Wyoming	1994	1995	0.2520547945

N.B: An RTC adoption year of 0 indicates that a state did not adopt a right-to-carry law between 1977 and the early months of 2014. 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. For example, we only read Vermont's statutes up to the year 1970 to confirm there were no references to blanket prohibitions on carrying concealed weapons up to the year 1970, although we suspect given widespread public commentary on this point that Vermont likely never had a comprehensive prohibition of the carrying of concealed weapons. We follow earlier convention in the academic literature on the RTC issue in assigning right-to-carry adoption dates for Alabama and Connecticut.

Table A2: ADZ Violent Crime Panel Data Model Coefficients, State and Year Fixed Effects (1979-2012)

Dependent variable: ln_violent_rate
Number of observations: 1723
R-squared: .9175
Adj R-squared: .9124

	Coefficient	Robust Standard Error	t	Pr >|t|
shalll	12.25876	5.646632	2.17	0.035
l_incarc_rate	-0.0027037	0.0206681	-0.13	0.896
l_policeemployeerate0	-0.0129753	0.0339201	-0.38	0.704
rpcpi	-0.0025733	0.0023608	-1.09	0.281
rpcui	0.0170905	0.0396817	0.43	0.669
rpcim	0.0933443	0.0480565	1.94	0.058
rpcrpo	-0.0214262	0.0254856	-0.84	0.405
unemployment_rate	-1.631775	1.352737	-1.21	0.233
poverty_rate	-1.572951	0.7022984	-2.24	0.03
density	-0.1503674	0.0562745	-2.67	0.01
age_bm_1019	8.86724	19.33017	0.46	0.648
age_bm_2029	2.412548	12.91252	0.19	0.853
age_bm_3039	41.79235	12.79649	3.27	0.002
age_wm_1019	-9.674583	6.004363	-1.61	0.113
age_wm_2029	-1.387978	6.622282	-0.21	0.835
age_wm_3039	-1.778765	4.488949	-0.4	0.694

Note: Standard errors are generated using a cluster by state command in Stata.

Table A3: LM Violent Crime Panel Data Model Coefficients, State and Year Fixed Effects (1977-2012)

Dependent variable: ln_violent_rate
Number of observations: 1785
R-squared: .9507
Adj R-squared: .9469

	Coefficient	Robust Standard Error	t	Pr >|t|
shalll	-4.040199	3.31051	-1.22	0.228
l_violent_arrestrate_Andrew	-0.1498498	0.082782	-1.81	0.076
popstatecensus	6.15E-07	1.06E-06	0.58	0.564
rpcpi	0.0031949	0.0015121	2.11	0.04
rpcui	-0.0012324	0.0168371	-0.07	0.942
rpcim	0.0216829	0.0324415	0.67	0.507
rpcrpo_alt_65	0.0007029	0.0064179	0.11	0.913
density	-0.0546752	0.0494103	-1.11	0.274
age_bm_1019	-25.01605	83.56751	-0.3	0.766
age_bm_2029	-36.71403	38.68104	-0.95	0.347
age_bm_3039	70.35587	58.40533	1.2	0.234
age_bm_4049	-92.54363	53.268	-1.74	0.088
age_bm_5064	-20.60418	61.61109	-0.33	0.739
age_bm_65o	-35.39381	80.55571	-0.44	0.662
age_bf_1019	44.87863	82.89152	0.54	0.591
age_bf_2029	25.6467	38.8631	0.66	0.512
age_bf_3039	-52.28756	47.67678	-1.1	0.278
age_bf_4049	84.33062	44.44217	1.9	0.064
age_bf_5064	11.49079	52.52295	0.22	0.828
age_bf_65o	-35.23747	60.96795	-0.58	0.566
age_wm_1019	-24.70652	37.44742	-0.66	0.512
age_wm_2029	10.16759	11.56267	0.88	0.383
age_wm_3039	-29.41234	19.89277	-1.48	0.146
age_wm_4049	-9.475461	24.58888	-0.39	0.702
age_wm_5064	56.31947	19.71963	2.86	0.006
age_wm_65o	5.138037	18.23748	0.28	0.779
age_wf_1019	28.22332	38.38409	0.74	0.466
age_wf_2029	-7.118937	13.84851	-0.51	0.609
age_wf_3039	25.00626	21.42293	1.17	0.249
age_wf_4049	-1.863945	23.82907	-0.08	0.938
age_wf_5064	-55.27579	17.64798	-3.13	0.003
age_wf_65o	3.449777	12.21129	0.28	0.779
age_nm_1019	-54.72619	120.0054	-0.46	0.65
age_nm_2029	69.71097	67.96028	1.03	0.31
age_nm_3039	276.9074	62.94529	4.4	0
age_nm_4049	-106.2504	121.2268	-0.88	0.385
age_nm_5064	-159.2733	97.63319	-1.63	0.109
age_nm_65o	-217.8283	82.08854	-2.65	0.011
age_nf_1019	159.0746	124.6653	1.28	0.208
age_nf_2029	-85.3074	59.5508	-1.43	0.158
age_nf_3039	-286.011	61.38375	-4.66	0
age_nf_4049	37.35708	104.2061	0.36	0.721
age_nf_5064	107.9243	80.92384	1.33	0.188
age_nf_65o	145.4541	48.86143	2.98	0.004

Note: Standard errors are generated using a cluster by state command in Stata.

Table A4: MM Violent Crime Panel Data Model Coefficients, State and Year Fixed Effects (1980-2000)

Dependent variable: ln_violent_rate
Number of observations: 1028
R-squared: .9887
Adj R-squared: .9872

	Coefficient	Robust Standard Error	t	Pr >|t|
shalll	-0.3806338	1.230732	-0.31	0.758
l_violent_arrestrate_Andrew	-0.0458342	0.0316412	-1.45	0.154
crackindex_unadjusted	-0.3170295	0.5530599	-0.57	0.569
l_incarc_rate	-0.0093209	0.0045882	-2.03	0.048
rpcpi	0.0005565	0.0007742	0.72	0.476
rpcui	-0.0031598	0.0168639	-0.19	0.852
rpcim	0.0106082	0.019786	0.54	0.594
rpcrpo	-0.0075244	0.0045197	-1.66	0.102
unemployment_rate	0.2310198	0.5027008	0.46	0.648
poverty_rate	-0.1793186	0.1403444	-1.28	0.207
popstatecensus	3.32E-07	5.68E-07	0.58	0.562
l_ln_violent_rate	77.74536	2.172051	35.79	0
age_bm_1019	51.04717	39.54475	1.29	0.203
age_bm_2029	-7.101872	17.85333	-0.4	0.693
age_bm_3039	4.353302	18.94564	0.23	0.819
age_bm_4049	-69.44301	30.20091	-2.3	0.026
age_bm_5064	30.02253	37.94124	0.79	0.433
age_bm_65o	-54.95048	22.07773	-2.49	0.016
age_bf_1019	-39.0045	38.76965	-1.01	0.319
age_bf_2029	-1.628515	17.25931	-0.09	0.925
age_bf_3039	16.79035	13.88036	1.21	0.232
age_bf_4049	44.14618	24.97735	1.77	0.083
age_bf_5064	-18.12549	35.78796	-0.51	0.615
age_bf_65o	23.91498	16.51378	1.45	0.154
age_wm_1019	-5.338427	13.72009	-0.39	0.699
age_wm_2029	3.116781	6.031148	0.52	0.608
age_wm_3039	-1.678455	9.655363	-0.17	0.863
age_wm_4049	-1.583623	10.10245	-0.16	0.876
age_wm_5064	9.764848	11.48095	0.85	0.399
age_wm_65o	9.967633	6.909549	1.44	0.155
age_wf_1019	7.318485	14.0671	0.52	0.605
age_wf_2029	1.806296	6.404197	0.28	0.779
age_wf_3039	7.166177	10.05813	0.71	0.48
age_wf_4049	6.015459	9.925588	0.61	0.547
age_wf_5064	-6.175092	9.600945	-0.64	0.523
age_wf_65o	2.510264	4.235509	0.59	0.556
age_nm_1019	-79.62381	47.59535	-1.67	0.101
age_nm_2029	62.07891	29.66987	2.09	0.042
age_nm_3039	61.12687	30.36949	2.01	0.05
age_nm_4049	-37.06724	58.95264	-0.63	0.532
age_nm_5064	-24.1397	54.09227	-0.45	0.657
age_nm_65o	-61.32353	57.37787	-1.07	0.29
age_nf_1019	112.9442	47.95562	2.36	0.023
age_nf_2029	-57.04549	22.22789	-2.57	0.013
age_nf_3039	-36.58894	24.24505	-1.51	0.138
age_nf_4049	0.7764863	51.78578	0.01	0.988
age_nf_5064	19.33592	45.30297	0.43	0.671
age_nf_65o	62.24225	28.4316	2.19	0.033

Note: Standard errors are generated using a cluster by state command in Stata.

Synthetic Control Models

Aneja, Donohue, and Zhang Specification

Table A5: ADZ Specification, Murder Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.180	-2.570	-2.140	-2.809	-5.909	-6.300	-5.260	-0.705	0.924	7.209*
	(1.964)	(3.496)	(3.527)	(3.983)	(4.171)	(4.499)	(4.669)	(5.179)	(5.821)	(4.132)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.728	0.488	0.604	0.600	0.288	0.308	0.428	0.916	0.868	0.336
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.132	0.136	0.128	0.148	0.144	0.172	0.180	0.156	0.176	0.200
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.056	0.048	0.092	0.080	0.136	0.108	0.088	0.124	0.132
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.008	0.016	0.008	0.032	0.028	0.028	0.028	0.044	0.044	0.040

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A6: ADZ Specification, Murder Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.944	-1.574	-1.207	-1.408	-4.684	-4.479	-3.143	1.612	2.622	7.048
	(1.540)	(3.407)	(3.512)	(3.966)	(4.074)	(4.167)	(4.341)	(4.752)	(5.787)	(4.179)
N	30	30	30	30	30	28	28	28	25	24
Pseudo P-Value	0.568	0.648	0.772	0.796	0.432	0.480	0.656	0.804	0.724	0.364
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.136	0.124	0.120	0.160	0.144	0.188	0.156	0.140	0.192	0.204
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.064	0.052	0.096	0.084	0.136	0.096	0.092	0.112	0.124
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.012	0.016	0.008	0.032	0.024	0.024	0.032	0.044	0.040	0.040

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TN TX UT VA WV WY
States excluded for poor pre-treatment fit: MN ND SD
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 A7: ADZ Specification, Murder Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.914	-2.593	-0.375	-2.150	-6.625	-3.408	-4.154	3.039	8.011	12.803**
	(1.928)	(3.383)	(4.429)	(5.190)	(4.153)	(5.444)	(5.541)	(6.255)	(7.344)	(4.619)
N	20	20	20	20	20	19	19	19	16	15
Pseudo P-Value	0.364	0.484	0.936	0.712	0.284	0.640	0.568	0.640	0.328	0.172
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.120	0.132	0.132	0.144	0.144	0.164	0.196	0.188	0.176	0.184
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.064	0.080	0.064	0.076	0.088	0.104	0.116	0.092	0.116	0.100
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.012	0.012	0.016	0.016	0.016	0.040	0.036	0.028	0.028

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL KS KY ME MI MO NC NM OH OK OR PA SC TN UT VA WV
States excluded for poor pre-treatment fit: AK GA ID LA MN MS MT ND NE NV SD TX WY
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: ADZ Specification, Rape Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.441	-1.122	-2.519	-0.121	0.587	2.337	3.472	10.483**	11.288**	9.608
	(2.620)	(3.248)	(3.946)	(3.180)	(3.369)	(3.751)	(3.423)	(4.260)	(5.180)	(5.826)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.816	0.632	0.424	0.968	0.872	0.600	0.496	0.040	0.048	0.144
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.100	0.172	0.196	0.184	0.204	0.200	0.204	0.236	0.264	0.268
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.052	0.084	0.108	0.108	0.132	0.144	0.128	0.156	0.168	0.164
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.028	0.024	0.040	0.056	0.056	0.060	0.056	0.080	0.044

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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: ADZ Specification, Rape Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.271	-1.027	-2.514	-0.642	0.458	1.949	2.668	9.899**	10.188*	8.487
	(2.697)	(3.353)	(4.073)	(3.268)	(3.471)	(3.880)	(3.505)	(4.385)	(5.366)	(6.067)
N	29	29	29	29	29	27	27	27	24	22
Pseudo P-Value	0.880	0.660	0.444	0.844	0.920	0.688	0.600	0.076	0.080	0.216
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.136	0.192	0.200	0.180	0.188	0.208	0.216	0.240	0.260	0.264
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.096	0.120	0.124	0.136	0.144	0.140	0.152	0.176	0.164
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.028	0.040	0.044	0.060	0.068	0.052	0.056	0.076	0.052

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL GA ID KS KY LA ME MI MN MO MT NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: AK MS ND SD
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: ADZ Specification, Rape Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-3.849	-4.969	-6.299	-4.611	-2.184	-0.670	0.448	7.449	6.522	7.475
	(2.371)	(3.153)	(4.116)	(3.217)	(3.924)	(4.506)	(3.802)	(4.794)	(6.031)	(6.582)
N	21	21	21	21	21	20	20	20	18	18
Pseudo P-Value	0.088	0.108	0.088	0.264	0.592	0.860	0.972	0.184	0.324	0.300
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.160	0.164	0.160	0.176	0.196	0.204	0.176	0.184	0.228	0.212
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.096	0.096	0.096	0.124	0.108	0.096	0.092	0.136	0.132
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.020	0.024	0.036	0.040	0.052	0.044	0.036	0.052	0.032

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ FL GA ID KS KY LA ME MO NC OH OK OR PA SC TN TX VA WV WY
States excluded for poor pre-treatment fit: AK CO MI MN MS MT ND NE NM NV SD UT
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 A11: ADZ Specification, Robbery Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-2.471	-1.513	-1.371	-0.234	2.747	4.425	3.133	3.450	5.228	4.972
	(2.088)	(2.519)	(3.722)	(4.448)	(5.799)	(6.198)	(6.963)	(6.432)	(7.268)	(7.798)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.308	0.700	0.748	0.972	0.656	0.516	0.676	0.660	0.548	0.652
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.204	0.208	0.212	0.204	0.204	0.220	0.236	0.224	0.228	0.272
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.116	0.104	0.132	0.128	0.108	0.148	0.148	0.152	0.140	0.204
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.028	0.032	0.044	0.044	0.044	0.032	0.060	0.068	0.108

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A12: ADZ Specification, Robbery Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-2.726	-1.734	-1.321	-0.200	2.729	4.466	3.205	3.526	5.298	4.963
	(2.131)	(2.576)	(3.834)	(4.581)	(5.974)	(6.308)	(7.085)	(6.545)	(7.417)	(7.977)
N	29	29	29	29	29	28	28	28	25	23
Pseudo P-Value	0.248	0.656	0.784	0.956	0.656	0.512	0.696	0.668	0.544	0.640
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.184	0.192	0.212	0.216	0.208	0.228	0.268	0.240	0.236	0.272
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.108	0.096	0.116	0.140	0.112	0.160	0.148	0.160	0.160	0.220
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.024	0.036	0.044	0.052	0.044	0.032	0.056	0.064	0.116

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: ID ND NE SD
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 A13: ADZ Specification, Robbery Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-2.811	-1.755	-1.230	-0.197	3.057	4.659	3.408	3.740	5.564	5.109
	(2.166)	(2.624)	(3.907)	(4.669)	(6.068)	(6.419)	(7.205)	(6.655)	(7.563)	(8.160)
N	26	26	26	26	26	25	25	25	22	20
Pseudo P-Value	0.268	0.684	0.824	0.972	0.640	0.556	0.708	0.680	0.532	0.608
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.176	0.168	0.196	0.204	0.208	0.248	0.260	0.272	0.280	0.324
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.104	0.092	0.108	0.132	0.136	0.172	0.184	0.164	0.188	0.248
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.024	0.032	0.028	0.032	0.064	0.048	0.072	0.084	0.112

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA WV
States excluded for poor pre-treatment fit: ID ME MT ND NE SD WY
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 A14: ADZ Specification, Aggravated Assault Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.065	2.300	4.027	4.373	6.922*	7.910*	12.668**	14.187***	18.010***	18.887**
	(1.496)	(1.878)	(2.754)	(3.409)	(4.032)	(4.466)	(4.639)	(4.914)	(5.950)	(8.952)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.968	0.364	0.244	0.276	0.156	0.180	0.036	0.024	0.008	0.024
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.132	0.136	0.164	0.172	0.184	0.212	0.192	0.204	0.216	0.200
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.088	0.092	0.104	0.104	0.112	0.132	0.128	0.148	0.120
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.016	0.032	0.032	0.040	0.040	0.040	0.048	0.036	0.072

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A15: ADZ Specification, Aggravated Assault Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.247	1.949	3.716	4.048	6.939	7.467	12.261**	13.128**	15.650**	16.517*
	(1.517)	(1.877)	(2.788)	(3.472)	(4.120)	(4.564)	(4.745)	(4.977)	(5.834)	(8.968)
N	30	30	30	30	30	28	28	28	25	23
Pseudo P-Value	0.880	0.476	0.264	0.340	0.156	0.216	0.048	0.048	0.024	0.036
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.132	0.132	0.176	0.140	0.160	0.200	0.196	0.208	0.216	0.216
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.080	0.092	0.084	0.092	0.100	0.120	0.124	0.124	0.140	0.124
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.020	0.036	0.028	0.032	0.036	0.036	0.052	0.032	0.064

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SD TN TX UT VA WY
States excluded for poor pre-treatment fit: MT ND WV
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 A16: ADZ Specification, Aggravated Assault Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.396	2.123	4.394	5.164	7.529	7.613	12.273**	13.170**	15.603**	15.736
	(1.670)	(2.051)	(3.101)	(3.826)	(4.565)	(5.004)	(5.108)	(5.413)	(6.338)	(9.874)
N	23	23	23	23	23	22	22	22	20	18
Pseudo P-Value	0.884	0.432	0.228	0.248	0.140	0.192	0.056	0.044	0.028	0.044
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.132	0.120	0.156	0.152	0.148	0.164	0.164	0.184	0.188	0.184
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.080	0.076	0.096	0.080	0.092	0.096	0.100	0.112	0.112	0.120
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.020	0.020	0.036	0.028	0.032	0.032	0.036	0.036	0.044

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS LA MI MN MO MS NC OH OK OR PA TN TX UT VA WY
States excluded for poor pre-treatment fit: KY ME MT ND NE NM NV SC SD WV
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 A17: ADZ Specification, Burglary Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.029*	3.384**	3.897	2.943	4.384	7.071	6.402	9.811**	8.345	8.986*
	(1.048)	(1.499)	(3.884)	(3.997)	(4.933)	(5.052)	(5.054)	(4.704)	(5.557)	(4.994)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.164	0.156	0.192	0.452	0.304	0.136	0.216	0.068	0.136	0.108
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.160	0.204	0.204	0.168	0.168	0.148	0.156	0.160	0.168	0.176
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.084	0.124	0.108	0.124	0.088	0.084	0.076	0.092	0.104	0.092
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.036	0.028	0.036	0.020	0.020	0.032	0.028	0.028	0.040

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A18: ADZ Specification, Burglary Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.146*	3.557**	4.157	3.119	4.498	7.090	6.433	9.873**	8.376	9.025*
	(1.067)	(1.525)	(3.957)	(4.077)	(5.035)	(5.100)	(5.102)	(4.746)	(5.617)	(5.053)
N	30	30	30	30	30	29	29	29	26	24
Pseudo P-Value	0.156	0.136	0.152	0.440	0.308	0.156	0.240	0.068	0.140	0.140
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.168	0.192	0.216	0.180	0.156	0.172	0.152	0.168	0.172	0.172
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.092	0.108	0.092	0.112	0.084	0.092	0.088	0.088	0.112	0.096
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.036	0.036	0.020	0.012	0.016	0.036	0.032	0.028	0.044

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: ND NE SD
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 A19: ADZ Specification, Burglary Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.556	2.984	-1.105	-1.608	-1.468	1.112	1.673	5.072	1.220	3.276
	(1.424)	(2.011)	(3.141)	(3.769)	(4.376)	(4.285)	(4.865)	(4.594)	(5.202)	(5.070)
N	23	23	23	23	23	22	22	22	20	19
Pseudo P-Value	0.324	0.264	0.712	0.688	0.772	0.812	0.776	0.420	0.876	0.656
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.128	0.180	0.196	0.196	0.196	0.208	0.192	0.192	0.208	0.208
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.080	0.112	0.100	0.120	0.116	0.088	0.112	0.112	0.124	0.116
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.024	0.032	0.028	0.024	0.020	0.036	0.036	0.036	0.032

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MO MT NV OH OR PA SC TN UT VA WY
States excluded for poor pre-treatment fit: MN MS NC ND NE NM OK SD TX WV
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 A20: ADZ Specification, Larceny Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.337	-0.720	0.937	-0.312	-0.804	0.244	1.108	3.289	2.345	1.690
	(1.130)	(1.330)	(1.356)	(1.618)	(1.607)	(1.658)	(2.425)	(2.435)	(2.056)	(2.266)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.180	0.644	0.664	0.920	0.792	0.944	0.720	0.384	0.540	0.672
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.116	0.160	0.144	0.144	0.156	0.140	0.156	0.168	0.188	0.196
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.088	0.084	0.088	0.092	0.088	0.096	0.112	0.104	0.112
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.024	0.020	0.032	0.024	0.036	0.020	0.024	0.040	0.036

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A21: ADZ Specification, Larceny Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.744	-1.383	0.194	-1.233	-1.409	-0.650	-0.362	1.577	0.408	-0.544
	(1.255)	(1.439)	(1.489)	(1.753)	(1.833)	(1.849)	(2.604)	(2.492)	(2.095)	(2.389)
N	29	29	29	29	29	27	27	27	24	22
Pseudo P-Value	0.088	0.432	0.940	0.656	0.640	0.836	0.908	0.692	0.916	0.904
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.124	0.172	0.148	0.136	0.144	0.156	0.208	0.180	0.184	0.192
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.056	0.096	0.100	0.080	0.080	0.072	0.088	0.112	0.116	0.116
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.012	0.036	0.028	0.024	0.032	0.020	0.032	0.028	0.044	0.036

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS PA WV
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 A22: ADZ Specification, Larceny Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.157	-0.535	0.508	-1.430	-1.136	-1.371	-0.871	1.023	0.028	-0.492
	(1.275)	(1.467)	(1.432)	(1.595)	(1.708)	(1.753)	(2.443)	(2.169)	(2.006)	(2.281)
N	27	27	27	27	27	25	25	25	22	20
Pseudo P-Value	0.284	0.784	0.840	0.616	0.708	0.700	0.780	0.784	0.996	0.924
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.092	0.148	0.140	0.148	0.176	0.176	0.200	0.204	0.224	0.196
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.056	0.072	0.060	0.108	0.092	0.104	0.108	0.120	0.136	0.120
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.012	0.028	0.028	0.040	0.024	0.036	0.024	0.032	0.052

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL ID KS LA ME MI MN MO MT NC ND NE NM NV OH OK OR SC SD TX UT VA WY
States excluded for poor pre-treatment fit: GA KY MS PA TN WV
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 A23: ADZ Specification, Auto Theft Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.511	4.422	2.920	0.920	0.616	-4.131	-4.477	-4.353	-0.918	-2.150
	(2.301)	(3.360)	(2.974)	(3.891)	(4.152)	(3.994)	(3.229)	(3.627)	(5.426)	(5.429)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.528	0.256	0.600	0.888	0.948	0.652	0.616	0.644	0.924	0.860
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.140	0.156	0.176	0.180	0.204	0.160	0.196	0.204	0.192	0.200
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.064	0.104	0.104	0.120	0.108	0.104	0.116	0.144	0.124	0.124
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.036	0.036	0.032	0.036	0.040	0.036	0.044	0.040	0.036

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A24: ADZ Specification, Auto Theft Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.281	5.107	3.554	0.292	-0.391	-4.195	-4.447	-4.313	-0.889	-2.178
	(2.343)	(3.481)	(3.045)	(3.999)	(4.262)	(4.014)	(3.243)	(3.646)	(5.456)	(5.458)
N	30	30	30	30	30	30	30	30	27	25
Pseudo P-Value	0.332	0.232	0.524	0.960	0.956	0.620	0.616	0.636	0.936	0.872
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.152	0.180	0.168	0.188	0.212	0.184	0.196	0.200	0.184	0.192
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.076	0.104	0.092	0.112	0.116	0.104	0.108	0.140	0.112	0.132
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.036	0.028	0.036	0.044	0.044	0.040	0.052	0.040	0.028

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit: KS ND NE
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 A25: ADZ Specification, Auto Theft Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.008	3.233	3.366	-1.624	-0.200	-5.388	-5.582	-6.005	-4.554	-2.115
	(2.236)	(3.562)	(3.484)	(4.460)	(4.920)	(4.630)	(3.686)	(4.000)	(5.361)	(6.185)
N	22	22	22	22	22	22	22	22	20	19
Pseudo P-Value	0.476	0.488	0.624	0.840	0.984	0.604	0.628	0.640	0.732	0.884
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.172	0.176	0.196	0.228	0.232	0.236	0.240	0.252	0.224	0.216
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.096	0.112	0.132	0.140	0.148	0.140	0.152	0.144	0.120	0.116
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.040	0.044	0.040	0.036	0.048	0.024	0.036	0.052	0.020

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR CO FL GA ID KY LA ME MI MO MT NC NV OH OR PA SC TN TX UT VA
States excluded for poor pre-treatment fit: AZ KS MN MS ND NE NM OK SD WV WY
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 A26: ADZ Specification, Violent Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.033	2.406*	3.673**	5.012**	8.721***	9.226***	11.819***	13.874***	17.610***	18.292***
	(1.183)	(1.393)	(1.632)	(2.173)	(2.453)	(2.801)	(2.989)	(3.430)	(3.230)	(2.966)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.968	0.292	0.204	0.156	0.060	0.072	0.040	0.032	0.016	0.020
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.116	0.168	0.192	0.172	0.192	0.212	0.196	0.220	0.212	0.252
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.084	0.104	0.108	0.120	0.128	0.128	0.140	0.140	0.176
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.028	0.032	0.036	0.036	0.060	0.044	0.044	0.056	0.076	0.068

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A27: ADZ Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.111	2.521*	3.881**	5.143**	8.992***	9.320***	11.816***	13.741***	17.393***	18.174***
	(1.202)	(1.414)	(1.643)	(2.198)	(2.467)	(2.838)	(3.036)	(3.486)	(3.291)	(3.013)
N	30	30	30	30	30	28	28	28	25	23
Pseudo P-Value	0.924	0.292	0.188	0.128	0.052	0.064	0.048	0.032	0.016	0.024
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.120	0.192	0.184	0.128	0.164	0.200	0.180	0.220	0.208	0.256
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.096	0.104	0.104	0.112	0.120	0.132	0.152	0.148	0.180
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.024	0.020	0.024	0.032	0.040	0.040	0.044	0.056	0.068	0.064

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
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 A28: ADZ Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.014	2.491*	3.948**	5.301**	9.296***	9.408***	12.037***	13.763***	16.963***	17.825***
	(1.239)	(1.455)	(1.685)	(2.252)	(2.519)	(2.882)	(3.070)	(3.536)	(3.293)	(3.003)
N	27	27	27	27	27	26	26	26	23	21
Pseudo P-Value	0.996	0.272	0.196	0.124	0.028	0.040	0.024	0.020	0.016	0.016
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.092	0.152	0.148	0.152	0.168	0.180	0.196	0.192	0.204	0.232
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.060	0.080	0.096	0.092	0.096	0.128	0.128	0.124	0.132	0.164
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.012	0.012	0.012	0.016	0.036	0.040	0.032	0.032	0.056	0.048

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A29: ADZ Specification, Property Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.611	0.575	1.730	0.692	0.446	0.917	0.910	1.946	0.625	1.007
	(1.065)	(1.364)	(2.773)	(2.858)	(2.832)	(2.648)	(2.614)	(2.268)	(2.432)	(2.504)
N	33	33	33	33	33	31	31	31	28	26
Pseudo P-Value	0.540	0.728	0.480	0.844	0.888	0.788	0.788	0.644	0.892	0.808
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.136	0.168	0.164	0.168	0.156	0.160	0.220	0.192	0.192	0.180
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.072	0.120	0.108	0.100	0.092	0.112	0.140	0.116	0.128	0.136
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.040	0.032	0.028	0.032	0.032	0.040	0.040	0.044	0.048

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
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 A30: ADZ Specification, Property Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.940	0.492	1.990	0.894	1.111	1.522	0.726	1.744	0.191	0.456
	(1.216)	(1.537)	(3.117)	(3.211)	(3.100)	(2.888)	(2.995)	(2.589)	(2.772)	(2.893)
N	29	29	29	29	29	27	27	27	24	22
Pseudo P-Value	0.352	0.780	0.396	0.808	0.740	0.712	0.848	0.724	0.952	0.940
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.144	0.172	0.176	0.184	0.156	0.200	0.232	0.200	0.196	0.192
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.068	0.112	0.112	0.096	0.108	0.120	0.148	0.120	0.116	0.120
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.020	0.048	0.028	0.036	0.044	0.036	0.036	0.036	0.040	0.044

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS PA WV
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 A31: ADZ Specification, Property Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.428	0.720	0.213	-0.976	-0.597	-0.530	-0.848	0.916	-1.394	-0.306
	(1.283)	(1.500)	(1.871)	(1.812)	(1.553)	(1.961)	(2.509)	(2.234)	(2.388)	(2.549)
N	24	24	24	24	24	23	23	23	20	18
Pseudo P-Value	0.688	0.720	0.940	0.768	0.892	0.904	0.836	0.852	0.744	0.944
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.108	0.168	0.156	0.132	0.144	0.168	0.144	0.168	0.180	0.172
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.048	0.104	0.092	0.108	0.096	0.092	0.104	0.096	0.112	0.108
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.032	0.012	0.040	0.044	0.016	0.032	0.044	0.032	0.032

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL ID KS LA ME MI MN MO MT NC NM NV OH OK OR SC TN UT VA WY
States excluded for poor pre-treatment fit: GA KY MS ND NE PA SD TX WV
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lott-Mustard Specification

Table A32: Lott and Mustard Specification, Murder Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.317	-4.247	-1.449	-3.118	-6.501	-4.357	-5.126	-0.280	1.756	7.462**
	(1.949)	(4.252)	(4.415)	(4.762)	(4.606)	(5.108)	(4.741)	(5.406)	(4.804)	(3.250)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A33: Lott and Mustard Specification, Murder Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.833	-3.398	-0.789	-1.953	-5.666	-2.561	-3.172	1.779	2.880	7.306**
	(1.645)	(4.265)	(4.483)	(4.857)	(4.677)	(4.900)	(4.503)	(5.154)	(4.860)	(3.288)
N	30	30	30	30	30	28	28	28	25	24

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TN TX UT VA WV WY
States excluded for poor pre-treatment fit: MN ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A34: Lott and Mustard Specification, Murder Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.074	-2.680	1.564	-2.219	-3.745	-2.268	-3.740	2.153	6.065	8.607**
	(1.583)	(4.785)	(5.054)	(5.640)	(5.230)	(5.213)	(4.876)	(5.622)	(5.148)	(3.535)
N	20	20	20	20	20	20	20	20	17	16

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL KY ME MI MO NC NM OH OK OR PA SC TN TX UT VA WV
States excluded for poor pre-treatment fit: AK GA ID KS LA MN MS MT ND NE NV SD WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A35: Lott and Mustard Specification, Rape Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.172	-0.912	-2.087	0.216	0.986	2.611	3.877	10.871**	12.778**	10.369*
	(2.645)	(3.061)	(3.867)	(2.937)	(3.243)	(3.740)	(3.479)	(4.071)	(4.693)	(5.516)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A36: Lott and Mustard Specification, Rape Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.069	-0.820	-2.043	-0.017	0.751	2.219	3.239	10.429**	12.353**	9.839*
	(2.676)	(3.104)	(3.922)	(2.975)	(3.284)	(3.800)	(3.499)	(4.114)	(4.772)	(5.617)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL GA ID KS KY LA ME 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: AK ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A37: Lott and Mustard Specification, Rape Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-3.095	-4.267	-5.764	-3.685	-1.340	-0.274	1.170	8.165*	9.232*	9.030
	(2.414)	(2.834)	(4.029)	(2.854)	(3.722)	(4.407)	(3.791)	(4.434)	(5.279)	(6.059)
N	21	21	21	21	21	21	21	21	19	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ FL GA ID KY LA ME MO MS NC OH OK OR PA SC TN TX VA WV WY
States excluded for poor pre-treatment fit: AK CO KS MI MN MT ND NE NM NV SD UT
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A38: Lott and Mustard Specification, Robbery Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.685	-1.473	-0.875	-0.483	1.395	1.164	0.645	2.246	3.398	3.831
	(2.215)	(2.850)	(3.897)	(4.404)	(5.721)	(6.483)	(7.093)	(6.307)	(7.190)	(7.426)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A39: Lott and Mustard Specification, Robbery Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.763	-1.546	-0.854	-0.458	1.504	1.200	0.676	2.336	3.493	3.788
	(2.262)	(2.913)	(3.990)	(4.508)	(5.852)	(6.642)	(7.268)	(6.458)	(7.389)	(7.659)
N	29	29	29	29	29	27	27	27	24	22

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA ME MI MN MO MS NC NE NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: ID MT ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A40: Lott and Mustard Specification, Robbery Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.896	-1.674	-0.679	-0.354	1.663	1.275	0.914	2.573	3.724	3.886
	(2.341)	(3.016)	(4.148)	(4.682)	(6.070)	(6.812)	(7.441)	(6.607)	(7.586)	(7.893)
N	25	25	25	25	25	24	24	24	21	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA
States excluded for poor pre-treatment fit: ID ME MT ND NE SD WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A41: Lott and Mustard Specification, Aggravated Assault Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.474	1.713	3.384	3.082	5.489	6.013	11.024**	11.848**	13.354**	14.149*
	(1.515)	(2.047)	(2.788)	(3.412)	(3.814)	(4.331)	(4.162)	(4.482)	(5.089)	(7.705)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A42: Lott and Mustard Specification, Aggravated Assault Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.497	1.711	3.379	3.072	5.509	6.013	11.043**	11.875**	13.372**	14.187*
	(1.521)	(2.057)	(2.800)	(3.427)	(3.831)	(4.351)	(4.182)	(4.503)	(5.116)	(7.750)
N	32	32	32	32	32	30	30	30	27	25

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN 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: ND
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A43: Lott and Mustard Specification, Aggravated Assault Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.719	1.858	3.905	4.331	6.608	6.341	11.562**	11.651**	11.667**	11.647
	(1.715)	(2.269)	(3.223)	(3.898)	(4.365)	(4.892)	(4.451)	(4.818)	(5.129)	(8.387)
N	23	23	23	23	23	22	22	22	20	18

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS LA ME MI MN MO MS NC OH OK OR PA TN TX UT WY
States excluded for poor pre-treatment fit: KY MT ND NE NM NV SC SD VA WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A44: Lott and Mustard Specification, Burglary Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	3.012**	4.530**	5.076	4.071	5.887	8.826*	9.192	11.487**	11.092*	11.046*
	(1.294)	(1.864)	(3.897)	(3.849)	(4.619)	(4.823)	(5.800)	(5.470)	(6.008)	(5.627)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A45: Lott and Mustard Specification, Burglary Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	3.152**	4.704**	5.347	4.261	6.022	8.855*	9.229	11.555**	11.130*	11.113*
	(1.314)	(1.891)	(3.963)	(3.922)	(4.711)	(4.868)	(5.854)	(5.519)	(6.072)	(5.691)
N	30	30	30	30	30	29	29	29	26	24

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: ND NE SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A46: Lott and Mustard Specification, Burglary Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.370	3.053	-0.101	-0.847	0.708	3.337	3.073	5.697	4.134	4.628
	(1.723)	(1.998)	(2.555)	(3.090)	(3.987)	(3.832)	(4.173)	(4.550)	(5.393)	(5.170)
N	24	24	24	24	24	23	23	23	21	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MO MT NV OH OK OR PA SC TN UT VA WY
States excluded for poor pre-treatment fit: MN MS NC ND NE NM SD TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A47: Lott and Mustard Specification, Larceny Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.872	-0.230	1.473	0.246	0.508	1.989	3.977	5.626	4.986	4.014
	(1.048)	(1.254)	(1.406)	(1.794)	(1.962)	(2.223)	(3.232)	(3.344)	(3.854)	(4.269)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A48: Lott and Mustard Specification, Larceny Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.197	-1.052	0.316	-1.500	-1.415	-0.404	-0.023	1.518	0.055	-1.406
	(1.174)	(1.322)	(1.448)	(1.676)	(1.905)	(2.077)	(2.214)	(2.298)	(2.643)	(3.287)
N	29	29	29	29	29	27	27	27	24	22

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS PA WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A49: Lott and Mustard Specification, Larceny Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.046	0.251	-0.046	-2.461	-2.455	-2.163	-1.866	-0.360	-1.650	-2.709
	(1.179)	(1.618)	(1.709)	(1.680)	(1.887)	(2.279)	(1.956)	(1.602)	(2.825)	(3.691)
N	25	25	25	25	25	23	23	23	20	18

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AZ CO FL ID KS LA ME MI MN MO MT NC ND NE NM NV OH OK OR SC SD UT VA WY
States excluded for poor pre-treatment fit: AR GA KY MS PA TN TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A50: Lott and Mustard Specification, Auto Theft Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.768	3.027	2.461	1.637	1.482	-1.386	-0.555	0.230	3.446	3.213
	(2.042)	(3.246)	(3.151)	(3.792)	(4.041)	(4.665)	(4.275)	(4.600)	(5.840)	(6.346)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A51: Lott and Mustard Specification, Auto Theft Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.116	3.282	2.662	0.824	0.244	-1.764	-1.049	-0.337	2.113	2.936
	(2.126)	(3.390)	(3.276)	(3.937)	(4.202)	(4.746)	(4.320)	(4.626)	(5.744)	(6.475)
N	30	30	30	30	30	30	30	30	27	25

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KY LA ME MI MN MO MT NC ND NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit: KS MS NE
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A52: Lott and Mustard Specification, Auto Theft Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.739	2.895	2.648	0.476	0.605	-1.773	-1.260	-1.387	0.853	2.699
	(2.204)	(3.637)	(3.623)	(4.313)	(4.625)	(5.239)	(4.768)	(5.018)	(5.990)	(6.638)
N	25	25	25	25	25	25	25	25	23	22

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KY LA ME MI MT NC NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: KS MN MO MS ND NE SD WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A53: Lott and Mustard Specification, Violent Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.110	1.946	3.077	2.767	5.266**	5.140**	8.225***	9.573***	11.622***	12.141***
	(1.208)	(1.498)	(2.033)	(2.039)	(2.043)	(2.471)	(2.650)	(3.076)	(3.212)	(3.258)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A54: Lott and Mustard Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.122	1.979	3.183	2.828	5.443**	5.073*	8.073***	9.275***	11.146***	11.687***
	(1.227)	(1.520)	(2.059)	(2.068)	(2.060)	(2.515)	(2.701)	(3.130)	(3.272)	(3.298)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A55: Lott and Mustard Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.039	1.907	3.183	2.841	5.569**	5.050*	8.163***	9.161***	10.495***	11.133***
	(1.265)	(1.566)	(2.122)	(2.128)	(2.116)	(2.560)	(2.742)	(3.184)	(3.284)	(3.307)
N	27	27	27	27	27	26	26	26	23	21

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A56: Lott and Mustard Specification, Property Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.401	0.628	1.949	1.210	0.942	1.937	1.939	2.816	1.437	1.160
	(0.988)	(1.263)	(2.735)	(2.897)	(2.895)	(2.788)	(2.505)	(2.058)	(2.240)	(2.335)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A57: Lott and Mustard Specification, Property Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.613	0.417	1.724	0.988	0.910	1.853	1.520	2.427	0.678	0.534
	(1.042)	(1.335)	(2.902)	(3.073)	(3.063)	(2.960)	(2.657)	(2.171)	(2.393)	(2.504)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A58: Lott and Mustard Specification, Property Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.863	-0.062	-0.751	-1.837	-1.700	-0.694	-0.847	0.848	-1.127	-1.037
	(1.341)	(1.567)	(1.844)	(1.819)	(1.951)	(1.935)	(2.267)	(2.108)	(2.164)	(2.772)
N	25	25	25	25	25	24	24	24	21	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS LA ME MI MN MO MT NC NM NV OH OK OR SC TN UT VA WY
States excluded for poor pre-treatment fit: KY MS ND NE PA SD TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A59: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Murder Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.473	-3.157	-0.019	-0.467	-4.771	-4.800	-5.115	-0.534	0.793	5.124
	(2.090)	(3.941)	(4.278)	(4.421)	(4.418)	(4.599)	(4.970)	(4.744)	(5.221)	(3.436)
N	33	33	33	33	33	31	31	31	28	26

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

Table A60: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Rape Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.463	-0.798	-1.895	0.092	0.806	1.893	2.825	9.807**	12.188**	8.856
	(2.669)	(3.327)	(4.072)	(3.180)	(3.443)	(3.820)	(3.525)	(4.146)	(4.818)	(5.666)
N	33	33	33	33	33	31	31	31	28	26

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

Table A61: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Robbery Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-2.465	-2.528	-2.562	-1.199	1.224	1.016	1.060	2.585	5.303	4.889
	(1.986)	(2.769)	(4.284)	(4.672)	(5.481)	(6.186)	(6.522)	(5.987)	(6.610)	(7.337)
N	33	33	33	33	33	31	31	31	28	26

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

Table A62: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Aggravated Assault Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.404	2.161	3.381	3.790	6.172	6.617	11.206**	12.641**	15.349***	15.819*
	(1.722)	(2.179)	(2.973)	(3.378)	(3.848)	(4.250)	(4.508)	(4.631)	(5.445)	(8.752)
N	33	33	33	33	33	31	31	31	28	26

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 aggravated assault 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A63: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Burglary Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.814*	3.031*	3.876	3.368	5.286	8.050	7.895	10.551**	8.658	9.693*
	(1.063)	(1.607)	(3.790)	(3.996)	(4.964)	(5.146)	(5.145)	(4.908)	(5.597)	(4.882)
N	33	33	33	33	33	31	31	31	28	26

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 burglary 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A64: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Larceny Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.287	-0.700	1.056	-0.509	-1.144	-0.002	1.277	3.458	2.970	2.281
	(1.109)	(1.311)	(1.302)	(1.717)	(1.840)	(1.908)	(2.795)	(2.789)	(2.632)	(2.755)
N	33	33	33	33	33	31	31	31	28	26

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 larceny 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A65: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Auto Theft Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	3.493	5.184	2.702	2.196	0.351	-3.353	-3.662	-1.744	3.637	3.981
	(2.378)	(3.981)	(2.800)	(4.477)	(4.792)	(4.933)	(4.113)	(4.790)	(6.170)	(7.465)
N	33	33	33	33	33	31	31	31	28	26

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

Table A66: Lott and Mustard Specification with 6 ADZ Demographic Variables and Adding Controls for Incarceration and Police, Property Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.572	0.701	2.043	1.017	0.323	0.578	0.746	1.986	0.260	0.793
	(0.997)	(1.309)	(2.713)	(2.826)	(2.806)	(2.585)	(2.970)	(2.567)	(2.430)	(2.542)
N	33	33	33	33	33	31	31	31	28	26

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

Moody-Marvell Specification

Table A67: Moody and Marvell Specification, Murder Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.289	-3.145	-0.183	-0.917	-1.530	0.069	1.765	8.927	11.406*	18.484***
	(1.717)	(3.239)	(3.676)	(4.173)	(4.480)	(5.014)	(5.465)	(6.192)	(6.186)	(4.882)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A68: Moody and Marvell Specification, Murder Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.298	-2.308	0.894	0.212	-0.451	1.942	3.734	11.559*	12.839**	18.287***
	(1.431)	(3.148)	(3.696)	(4.197)	(4.490)	(4.697)	(5.218)	(5.869)	(6.213)	(4.911)
N	31	31	31	31	31	29	29	29	26	25

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TX UT VA WV WY
States excluded for poor pre-treatment fit: MN ND
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A69: Moody and Marvell Specification, Murder Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	3.077*	-4.207	0.742	-1.168	-2.655	0.541	0.965	9.831	14.130*	20.585***
	(1.594)	(3.236)	(4.682)	(5.601)	(5.292)	(6.156)	(6.518)	(7.507)	(7.903)	(4.949)
N	20	20	20	20	20	20	20	20	17	16

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL KY ME MI MO MS NC NM OH OK OR PA SC TN UT VA WV
States excluded for poor pre-treatment fit: AK GA ID KS LA MN MT ND NE NV SD TX WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A70: Moody and Marvell Specification, Rape Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.099	-0.934	-2.283	0.623	1.396	2.197	4.076	11.285^{***}	13.222^{**}	11.411^{*}
	(2.633)	(2.993)	(3.838)	(2.813)	(3.019)	(3.530)	(3.395)	(4.004)	(4.895)	(5.619)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A71: Moody and Marvell Specification, Rape Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.160	-0.819	-2.239	0.362	1.133	1.762	3.446	10.742^{**}	12.704^{**}	10.683^{*}
	(2.662)	(3.034)	(3.893)	(2.847)	(3.054)	(3.585)	(3.411)	(4.040)	(4.973)	(5.713)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ CO FL GA ID KS KY LA ME 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: AK ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A72: Moody and Marvell Specification, Rape Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-3.619	-4.843^{*}	-6.115	-3.856	-1.800	-1.073	0.971	8.246^{*}	8.086	8.927
	(2.411)	(2.743)	(3.910)	(2.604)	(3.398)	(4.174)	(3.679)	(4.361)	(5.519)	(6.248)
N	22	22	22	22	22	21	21	21	19	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AR AZ FL GA ID KS KY LA ME MO MT NC OH OK OR PA SC TN TX VA WV WY
States excluded for poor pre-treatment fit: AK CO MI MN MS ND NE NM NV SD UT
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A73: Moody and Marvell Specification, Robbery Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.116	-1.531	0.586	1.494	3.832	3.781	4.816	6.361	7.361	7.311
	(2.388)	(3.098)	(4.315)	(4.805)	(5.906)	(6.897)	(7.448)	(6.433)	(6.821)	(7.385)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A74: Moody and Marvell Specification, Robbery Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.342	-1.686	0.739	1.656	4.040	3.909	4.950	6.580	7.607	7.396
	(2.468)	(3.214)	(4.492)	(5.001)	(6.138)	(7.091)	(7.657)	(6.607)	(7.033)	(7.648)
N	27	27	27	27	27	26	26	26	23	21

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA ME MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA WV
States excluded for poor pre-treatment fit: ID MT ND NE SD WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A75: Moody and Marvell Specification, Robbery Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.350	-1.692	0.781	1.675	4.180	4.007	5.169	6.764	7.798	7.595
	(2.489)	(3.242)	(4.532)	(5.046)	(6.186)	(7.150)	(7.712)	(6.654)	(7.090)	(7.719)
N	26	26	26	26	26	25	25	25	22	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA KS KY LA MI MN MO MS NC NM NV OH OK OR PA SC TN TX UT VA WV
States excluded for poor pre-treatment fit: ID ME MT ND NE SD WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A76: Moody and Marvell Specification, Aggravated Assault Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.005	1.621	3.404	3.812	6.427*	7.730*	11.208***	12.755***	14.796***	14.770*
	(1.410)	(2.080)	(2.572)	(2.961)	(3.495)	(4.121)	(4.045)	(4.339)	(5.144)	(7.366)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A77: Moody and Marvell Specification, Aggravated Assault Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.043	1.588	3.404	3.793	6.527*	7.566*	10.958**	12.183***	13.927**	13.780*
	(1.423)	(2.101)	(2.599)	(2.991)	(3.530)	(4.163)	(4.082)	(4.344)	(5.107)	(7.345)
N	31	31	31	31	31	29	29	29	26	24

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit: MT ND
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A78: Moody and Marvell Specification, Aggravated Assault Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.885	1.972	4.055	5.263	7.587*	7.760	11.934**	13.030**	13.772**	13.423
	(1.620)	(2.304)	(2.937)	(3.291)	(3.894)	(4.521)	(4.313)	(4.682)	(5.308)	(8.211)
N	22	22	22	22	22	21	21	21	19	17

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS LA ME MI MN MO MS NC OH OK OR PA TN TX UT
States excluded for poor pre-treatment fit: KY MT ND NE NM NV SC SD VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A79: Moody and Marvell Specification, Burglary Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.342**	3.999***	5.081	4.285	5.854	8.559*	7.413	10.171**	8.377	8.719
	(0.921)	(1.286)	(3.552)	(3.488)	(4.442)	(4.703)	(5.011)	(4.760)	(5.734)	(5.375)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A80: Moody and Marvell Specification, Burglary Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.483**	4.202***	5.353	4.441	5.981	8.639*	7.395	10.141**	8.185	8.533
	(0.950)	(1.325)	(3.664)	(3.602)	(4.589)	(4.803)	(5.121)	(4.864)	(5.878)	(5.515)
N	29	29	29	29	29	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC TN TX UT VA WY
States excluded for poor pre-treatment fit: ND NE SD WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A81: Moody and Marvell Specification, Burglary Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.959	3.425**	0.252	0.114	0.197	2.841	2.990	5.300	1.233	2.797
	(1.201)	(1.611)	(2.460)	(2.866)	(3.497)	(3.759)	(4.774)	(4.543)	(5.286)	(5.382)
N	24	24	24	24	24	23	23	23	21	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MO MS MT NV OH OR PA SC TN UT VA WY
States excluded for poor pre-treatment fit: MN NC ND NE NM OK SD TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A82: Moody and Marvell Specification, Larceny Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.835	-0.023	1.516	0.155	0.164	0.887	2.141	3.827	2.920	2.128
	(1.031)	(1.242)	(1.288)	(1.637)	(1.609)	(1.795)	(2.834)	(2.918)	(2.856)	(3.074)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A83: Moody and Marvell Specification, Larceny Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.203	-0.740	0.650	-0.948	-0.899	-0.409	-0.303	1.294	-0.077	-1.129
	(1.147)	(1.330)	(1.379)	(1.710)	(1.707)	(1.848)	(2.592)	(2.553)	(2.314)	(2.705)
N	29	29	29	29	29	27	27	27	24	22

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS PA WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A84: Moody and Marvell Specification, Larceny Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.421	0.094	-0.247	-2.366	-2.656*	-2.516	-2.950	-1.523	-2.634	-4.013
	(1.180)	(1.524)	(1.522)	(1.688)	(1.538)	(1.592)	(2.345)	(2.095)	(2.145)	(2.786)
N	27	27	27	27	27	25	25	25	22	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 SC SD UT VA WY
States excluded for poor pre-treatment fit: KY MS PA TN TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A85: Moody and Marvell Specification, Auto Theft Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.704	2.994	2.044	0.482	-0.397	-3.543	-3.682	-3.302	1.343	-0.577
	(2.109)	(3.618)	(3.369)	(4.400)	(4.213)	(4.516)	(3.925)	(4.216)	(6.175)	(6.182)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A86: Moody and Marvell Specification, Auto Theft Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.306	3.619	2.686	0.133	-1.180	-3.553	-3.581	-3.201	1.448	-0.508
	(2.189)	(3.779)	(3.489)	(4.568)	(4.359)	(4.559)	(3.963)	(4.261)	(6.250)	(6.258)
N	29	29	29	29	29	29	29	29	26	24

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KY LA ME MI MN MO MS MT NC NM NV OH OK OR PA SC TN TX UT VA WV WY
States excluded for poor pre-treatment fit: KS ND NE SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A87: Moody and Marvell Specification, Auto Theft Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.437	1.354	1.818	-2.559	-2.348	-5.720	-5.688	-5.835	-2.965	-0.625
	(2.090)	(3.787)	(3.855)	(4.861)	(4.858)	(5.019)	(4.256)	(4.504)	(6.070)	(6.835)
N	23	23	23	23	23	23	23	23	21	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR CO FL GA ID KY LA ME MI MO MT NC NV OH OK OR PA SC TN TX UT VA
States excluded for poor pre-treatment fit: AZ KS MN MS ND NE NM SD WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A88: Moody and Marvell Specification, Violent Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.197	2.969*	4.812**	6.110**	9.052***	9.563***	12.216***	14.241***	18.511***	20.015***
	(1.261)	(1.716)	(1.997)	(2.326)	(2.735)	(3.075)	(3.371)	(4.171)	(4.287)	(4.202)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A89: Moody and Marvell Specification, Violent Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.260	3.053*	4.991**	6.230**	9.301***	9.623***	12.166***	14.061***	18.232***	19.849***
	(1.281)	(1.742)	(2.020)	(2.354)	(2.753)	(3.119)	(3.428)	(4.244)	(4.381)	(4.285)
N	30	30	30	30	30	28	28	28	25	23

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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 WV WY
States excluded for poor pre-treatment fit: MT ND SD
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A90: Moody and Marvell Specification, Violent Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.385	2.275	4.014**	5.660**	9.566***	10.000***	12.876***	14.577***	18.596***	19.574***
	(1.243)	(1.685)	(1.880)	(2.427)	(2.929)	(3.260)	(3.527)	(4.417)	(4.489)	(4.371)
N	25	25	25	25	25	24	24	24	21	20

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS KY LA MI MO MS NC NM NV OH OK OR PA SC TN TX UT VA
States excluded for poor pre-treatment fit: ME MN MT ND NE SD WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A91: Moody and Marvell Specification, Property Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.261	1.888*	3.241	3.079	3.074	3.702	3.083	3.977*	3.457	3.934
	(0.804)	(1.023)	(2.409)	(2.549)	(2.428)	(2.398)	(2.395)	(2.210)	(2.482)	(2.726)
N	33	33	33	33	33	31	31	31	28	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A92: Moody and Marvell Specification, Property Crime Rate, < 2x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.033	1.814	3.249	2.965	3.255	3.858	2.524	3.632	2.969	3.475
	(0.929)	(1.154)	(2.781)	(2.941)	(2.767)	(2.716)	(2.772)	(2.536)	(2.871)	(3.202)
N	28	28	28	28	28	27	27	27	24	22

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AR AZ CO FL GA ID KS LA ME MI MN MO MT NC ND NM NV OH OK OR SC SD TN TX UT VA WY
States excluded for poor pre-treatment fit: KY MS NE PA WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A93: Moody and Marvell Specification, Property Crime Rate, < 1x Average CV of the RMSPE, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.249	1.429	0.646	0.152	0.460	1.229	-0.088	1.180	-0.017	0.854
	(1.095)	(1.265)	(1.644)	(1.783)	(1.669)	(1.907)	(2.052)	(2.165)	(2.730)	(3.596)
N	25	25	25	25	25	24	24	24	21	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
States in group: AK AZ CO FL GA ID KS LA ME MI MN MO MT NC ND NM NV OH OK OR SC TN UT VA WY
States excluded for poor pre-treatment fit: AR KY MS NE PA SD TX WV
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A94: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Murder Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.138	-3.734	-2.493	-3.716	-6.266	-6.594	-4.873	1.064	0.793	8.085**
	(1.939)	(3.441)	(4.012)	(4.452)	(4.610)	(4.860)	(5.840)	(5.438)	(5.595)	(3.761)
N	33	33	33	33	33	31	31	31	28	26

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

Table A95: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Rape Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.014	-1.243	-2.357	-0.126	0.483	1.981	3.575	10.844**	12.936**	10.442*
	(2.575)	(3.002)	(3.703)	(2.901)	(3.191)	(3.969)	(3.418)	(4.100)	(4.875)	(5.305)
N	33	33	33	33	33	31	31	31	28	26

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

Table A96: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Robbery Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.399	-0.135	0.569	1.535	3.179	2.802	2.830	4.064	5.880	6.820
	(2.238)	(3.040)	(4.161)	(4.547)	(5.732)	(6.608)	(7.226)	(6.297)	(6.631)	(6.932)
N	33	33	33	33	33	31	31	31	28	26

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

Table A97: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Aggravated Assault Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.441	2.688	4.228	4.301	7.081**	8.181**	11.356**	13.208***	16.131***	16.656**
	(1.665)	(2.190)	(2.630)	(2.907)	(3.375)	(3.832)	(4.526)	(4.539)	(5.343)	(7.802)
N	33	33	33	33	33	31	31	31	28	26

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 aggravated assault 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A98: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Burglary Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.564	3.111*	4.018	2.837	4.492	7.363	6.568	9.655*	8.378	9.803**
	(1.031)	(1.538)	(3.867)	(4.023)	(4.928)	(5.102)	(5.113)	(4.815)	(5.530)	(4.659)
N	33	33	33	33	33	31	31	31	28	26

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 burglary 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A99: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Larceny Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.195	-0.499	1.362	0.024	-0.119	0.818	2.568	4.495	4.148	3.334
	(1.148)	(1.364)	(1.397)	(1.760)	(1.950)	(2.058)	(3.335)	(3.293)	(3.444)	(3.498)
N	33	33	33	33	33	31	31	31	28	26

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 larceny 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 TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A100: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Auto Theft Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.863	4.506	2.285	1.377	1.138	-2.682	-1.016	1.906	7.212	8.087
	(2.317)	(3.816)	(2.990)	(4.504)	(4.396)	(4.331)	(3.602)	(4.175)	(4.725)	(6.256)
N	33	33	33	33	33	31	31	31	28	26

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

Table A101: Moody and Marvel Specification with 6 ADZ Demographic Variables and Adding Control for Police, Property Crime Rate, Full Sample, 1981-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.519	0.481	1.787	0.985	0.146	0.341	0.056	1.126	-0.004	0.563
	(1.013)	(1.304)	(2.797)	(3.031)	(2.991)	(2.998)	(3.210)	(2.675)	(2.552)	(2.510)
N	33	33	33	33	33	31	31	31	28	26

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

Appendix B: Data and Methodological Appendix

I. Data Issues

We begin with the state-level data set constructed for Aneja et al. (2014), which represented a comprehensive update of the data set used in Aneja et al. (2011). We further update this data set to include additional observations from the years 2011 and 2012, to incorporate changes to the various primary sources underlying this data set that have occurred since its release, and to include alternative versions of our main predictors variables (for various robustness checks). All variables are collected between the years 1977 and 2012 unless otherwise noted.[37]

Annual state-level crime rates are taken from the FBI's Uniform Crime Reporting program.[38] 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) for all three of the specifications that we consider, while the LM and MM/ADZ specifications use alternative versions of the retirement variable that were described in section III.4. State-level population is generated using the Census Bureau's intercensal population estimates, while the proportional size of Lott's thirty-six age-race-sex demographic groups are estimated using state-level population by age, sex, and race gathered by the Census. (In cases where the most recent form of these data were not easily 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 given observation's population by the area of that state estimated 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 rates is derived from the Decennial Census and the 1980-2012 poverty rates are generated using the Current Population Survey). A measure of incarceration (incarcerated individuals per 100,000 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 (aggregate violent and property crime arrest rates in the MM specification) 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.[39] 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. Since we already include controls that incorporate information on the racial composition of individual states in our analysis, we use the unadjusted version of this index instead of the version that is adjusted to account for differences in state racial demographics.

[37]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 our 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 (for whatever reason) our estimates more closely approximated Lott's per capita retirement payment variable when we used the total population as the denominator rather than population over 65 and used a measurement that includes retirement payments along with some other forms of government assistance, so we used this measure as a predictor in the MM and ADZ specifications. 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. Owing to the difficulty of incorporating a variable analogous to state-specific trends into the synthetic control analysis, we exclude them from our reported tables, even though these trends are utilized in Moody and Marvell's preferred specification. The findings of regression models including these trends can be seen in the Online Appendix accompanying this paper.

[38]For our main analysis, we use crime rates calculated from FBI reported crime counts and state-level populations, although as a robustness check we use the rounded state-level crime rates reported by the FBI while using the ADZ regressors and aggregate violent crime and aggravated assault as outcome variables. We find that this alternative crime rate definition does not qualitatively affect our findings for these two variables under this specification. These results can be seen in Tables 79-84.

[39]After much deliberation, 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 dataset which runs through the year 2005) than several alternatives that we constructed. Given that our data set includes some state-year cells for which no arrest rate could be calculated, we estimate average pre-treatment arrest rates in our synthetic controls approach based on years for which all treatment and control units reported arrest data.

Unfortunately, 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. After considering several different ways to confront this issue, we ultimately decided to exclude the District of Columbia from all three of the specifications that we analyze using the synthetic controls framework to ensure that the results from these three specifications are as directly comparable as possible. Moreover, there is a strong case to be made for excluding the District of Columbia in this framework owing to its status as a clear outlier whose characteristics are unlikely to be meaningfully predictive for other geographic areas. Abadie et al. (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]." 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, it is plausible in our view that this condition would not be upheld if we included the District as one of our control units.[40]

We consider two separate police measures for the purposes of our analysis. Our reported results are based on the same police variable that we used in Aneja et al. (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 ($r = .9719$) 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 Lott and 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 necessitated using published tables to correct these discrepancies.) 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 version of this variable in for another; this suggests that measurement error associated with our estimates of police activity is not driving our results.[41] Interestingly, we again observe that changing our model causes our estimated treatment effect associated with aggravated assault to decline, although our point coefficients still suggest meaningful crime increases attributable to right-to-carry laws.

We use the same effective RTC dates used in Aneja et al. (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 a RTC law was in effect for the majority of that year.[42] This causes some of the values of our RTC variable to shift by one year (for instance, Wisconsin's RTC date shifts from 2011

[40]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.

In spite of the difficulties mentioned above, we re-ran our analysis to include the District of Columbia using the ADZ specification (both with and without the interpolated incarceration data mentioned above) for violent crime and aggravated assaults, finding that our finding of generally deleterious effects associated with RTC laws still survived this alternative specification. Our violent crime results were almost identical after including the District of Columbia in our analysis (if anything this change increased association with RTC laws and post-passage increases in aggregate violent crime), although the treatment effects associated with aggravated assault generally decreased in magnitude after making this change. These results are shown in Tables 85-96 in the Online Appendix.

[41]These results are shown in Tables 97-102 of the Online Appendix.

[42]A table showing each state's original adoption date and adjusted adoption date is shown in Table A1 of Appendix A.

to 2012, since the state's RTC law took effect on November 1, 2011).[43]

While there have been numerous disagreements about the exact laws that should be used to 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 changed 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 extensively 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 et al. (2014). If anything, the main problem associated with our coding of RTC dates is the possibility that some states with a discretionary process for issuing concealed carry permits were actually less restrictive in issuing these permits than some "shall issue" states. If the actual change in the gun permitting behavior associated with the passage of right-to-carry laws is smaller than the statutory text of these laws would imply, we would expect our estimates of the effect of right-to-carry laws on crime to not fully capture the true effect of larger shifts in permitting behavior on crime rates.

II. Methodological Issues

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[44] The closest paper to the present study is Dube and 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. However, we scale this difference based on the size of the equivalent percentage gap seen between the treatment and synthetic control at the time of the treatment and report treatment effects for each of the ten years following the treatment, while Dube and Zipperer average these post-treatment percentage differences and convert this average into an elasticity. Thus, our work reports separate average treatment effects for ten yearly intervals following the time of the treatment, while Dube and Zipperer (2013) emphasize an average treatment effect (expressed as an elasticity) estimated over the entire post-treatment period. We also rely on a different procedure for using placebo treatment data to estimate the significance of our estimated average treatment effect. While Dube and Zipperer (2013) estimate the significance of their averaged treatment effects using the average rank associated with those treatments (compared to the distribution of placebo treatment effects associated with the control states used in each treatment), we compare our estimates of the average effect of RTC laws on crime rates with the distribution of average effects generated by creating placebo treatments for thirty-three randomly chosen states.

Another paper whose methodology is similar to ours is Ando (2015), which examines the impact of constructing nuclear plants on local real per capita taxable income in Japan. Ando (2015) examines multiple treatments by initially 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 in Ando (2015), we follow Ando's suggestion to repeatedly estimate average placebo effects by randomly selecting different areas to serve as placebo treatments.[45] The actual average treatment effect can then be compared to the distribution of average placebo treatment effects. Cavallo et al. (2013) perform a similar test to examine how the average of different placebo effects compares to the average treatment effect that they measure using synthetic control techniques, although their randomization procedure differs from ours by restricting the timing of placebo treatments to the exact dates when actual treatments took place. Heersink and Peterson (2014) also perform

[43]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 and aggravated assault codes under the ADZ specification without considering the modified RTC dates in our selection of control units, finding that this change did not affect our qualitative findings meaningfully. These results are shown in Tables 103-108 of the Online Appendix.

[44]While some papers analyze multiple treatments by aggregating the areas affected by these treatments into a single unit, this approach is not well-equipped to deal with a case such as RTC law adoption where treatments affect the majority of panel units and more than two decades separate the dates of the first and last treatment under consideration.

[45]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 right-to-carry laws, but this simply means that our placebo estimates will likely be biased *against* finding a qualitatively significant effect of right-to-carry laws on crime (since some of our placebo treatments will be capturing the effect of the passage of right-to-carry laws on crime rates).

a randomization procedure to estimate the significance of their estimated average treatment effect that is similar to Ando (2015) and our own approach.

Appendix C: Replicating Our Analysis

One issue which is rarely tackled directly in the existing literature surrounding the application of the synthetic control technique is the sensitivity of the selection of the synthetic control to seemingly inconsequential details when using maximum likelihood to select the weights associated with different predictors in our analysis. More 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 13), both 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."[46]

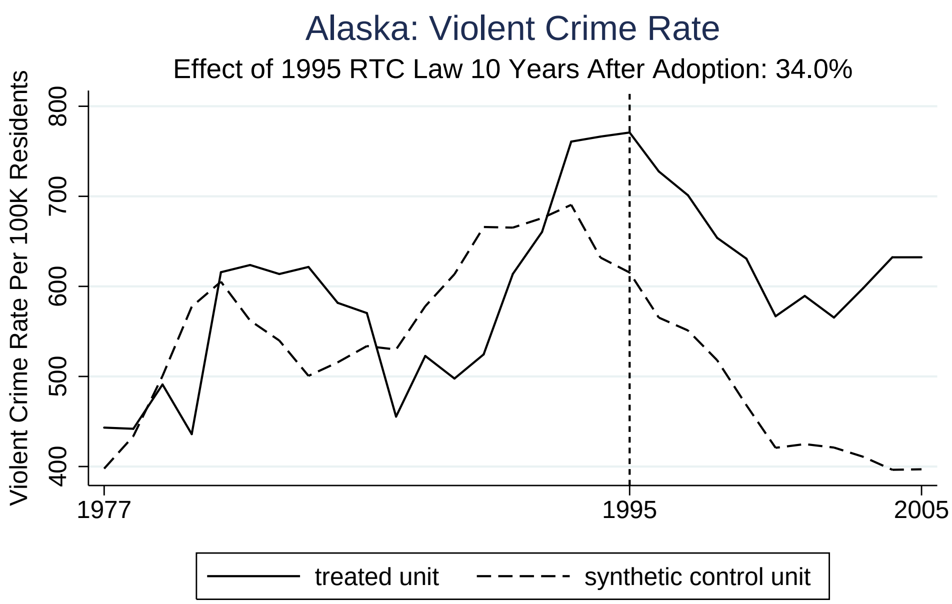
Moreover, this document goes on to note that the differences associated with using different versions of Stata can be minimized by setting a higher threshold for *nrtolerance()*. This optimization condition is actually relaxed by the synth routine in situations where setting this threshold at its default 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, and these errors occurred less often when using more powerful computers with greater amounts of memory. (For the results that we emphasize in this paper – aggravated assault and aggregate violent crime – we estimate our synthetic controls without using the *nested* option for six treatments of the 198 included in the three main specifications included in the main text of our paper.) 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 running Stata/MP.

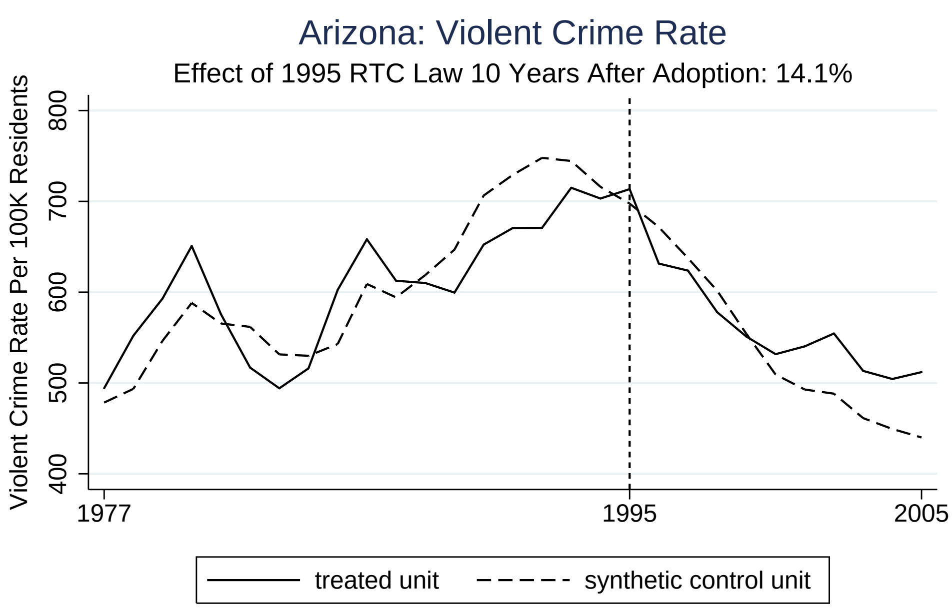
One final discrepancy that we are still in the process of investigating is the effect of changing the variable order in the synthetic control command on the composition of the synthetic control when using the *nested* option. Unfortunately, the large number of predictors included in the LM and MM specifications make it difficult to use a fixed criteria (e.g., minimizing the average coefficient of variation of the RMSPE) for determining the order in which variables should be listed. While we have not modified the order in which predictors were listed in our models after observing the results that we derived from that variable order, it is useful to be aware that different variable orders can alter estimates slightly. However, the observation that our violent crime and aggravated assault results generally remain unchanged after trying multiple specifications featuring different sets of predictors gives us greater confidence that our conclusions about these specifications are robust to changes in variable order as well.

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

Appendix D: Synthetic Control Graphs Based On Violent Crime Results



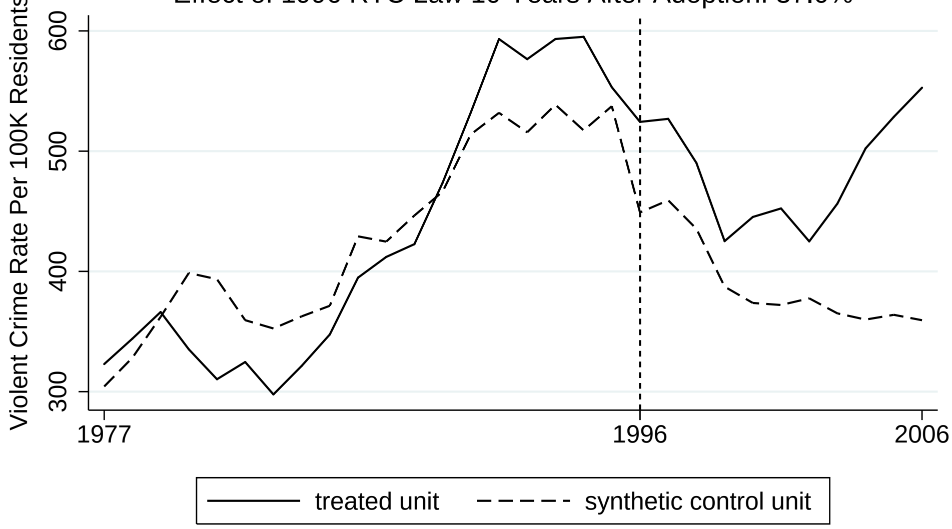
Composition of SC: MA (52.1%), NJ (35.0%), RI (12.9%)
States Never Passing RTC Laws Included in Synthetic Control: MA, NJ, RI
RTC−Adopting States Included In Synthetic Control:



Composition of SC: CA (32.2%), IL (2.4%), MD (14.7%), NE (45.5%), NY (5.2%)
States Never Passing RTC Laws Included in Synthetic Control: CA, MD, NY
RTC−Adopting States Included In Synthetic Control: IL (2014), NE (2007)

Arkansas: Violent Crime Rate

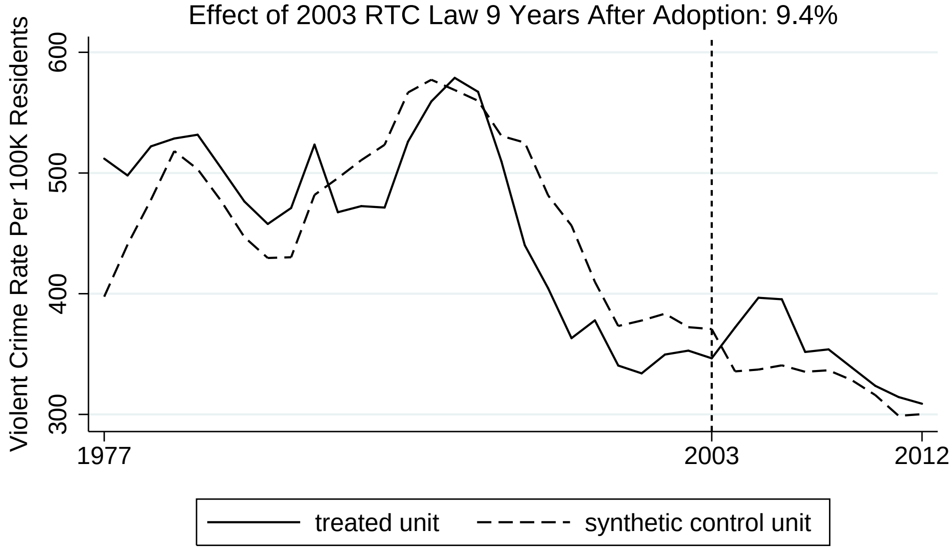
Effect of 1996 RTC Law 10 Years After Adoption: 37.0%



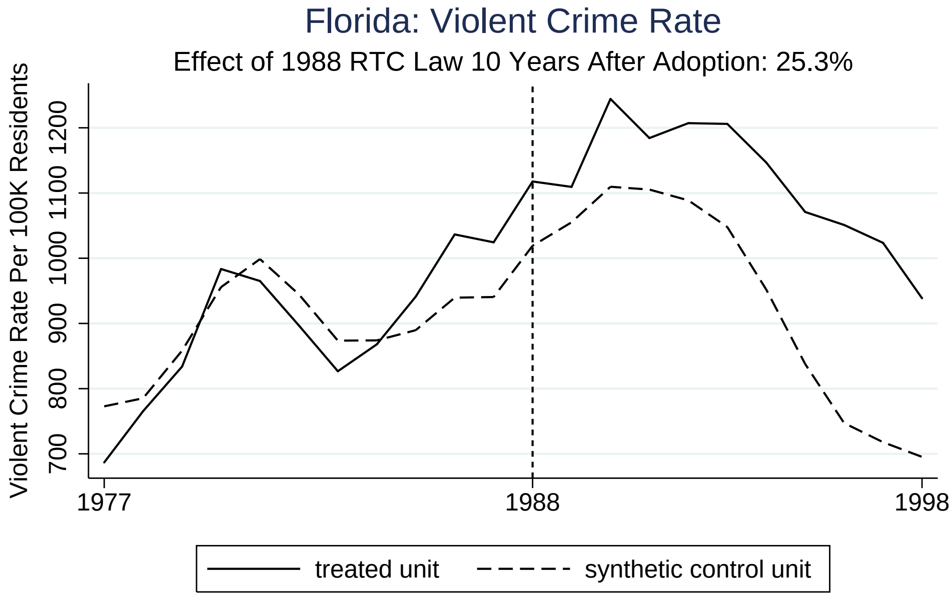
Composition of SC: CA (21.8%), IL (7.8%), IA (70.3%)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC−Adopting States Included In Synthetic Control: IL (2014), IA (2011)

Colorado: Violent Crime Rate

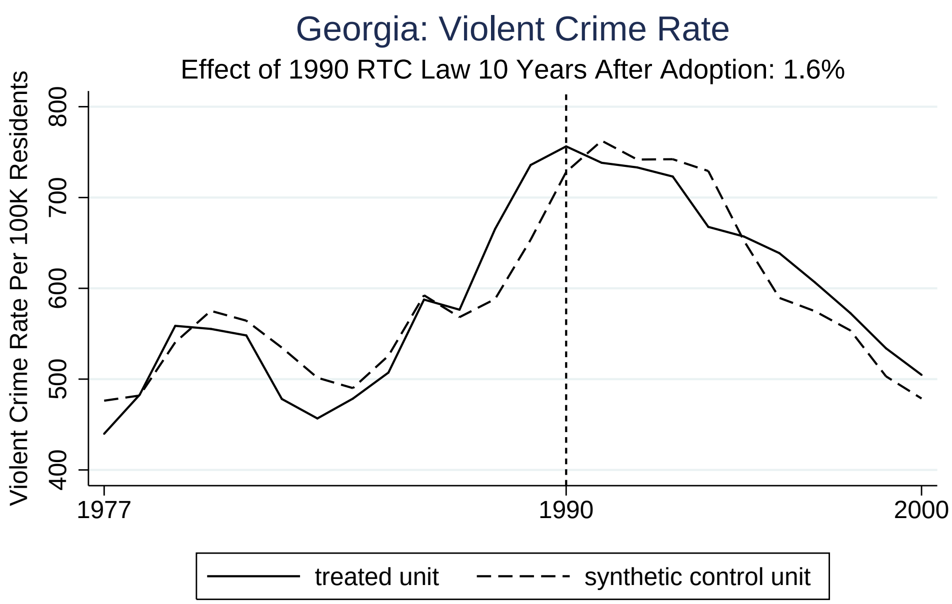
Effect of 2003 RTC Law 9 Years After Adoption: 9.4%



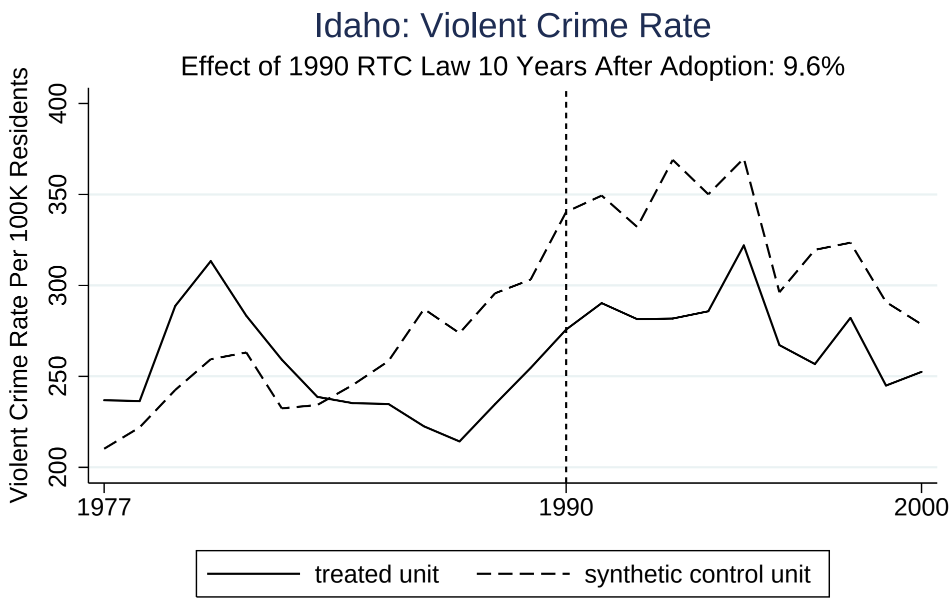
Composition of SC: CA (30.0%), HI (34.1%), MA (0.9%), RI (34.9%)
States Never Passing RTC Laws Included in Synthetic Control: CA, HI, MA, RI
RTC−Adopting States Included In Synthetic Control:



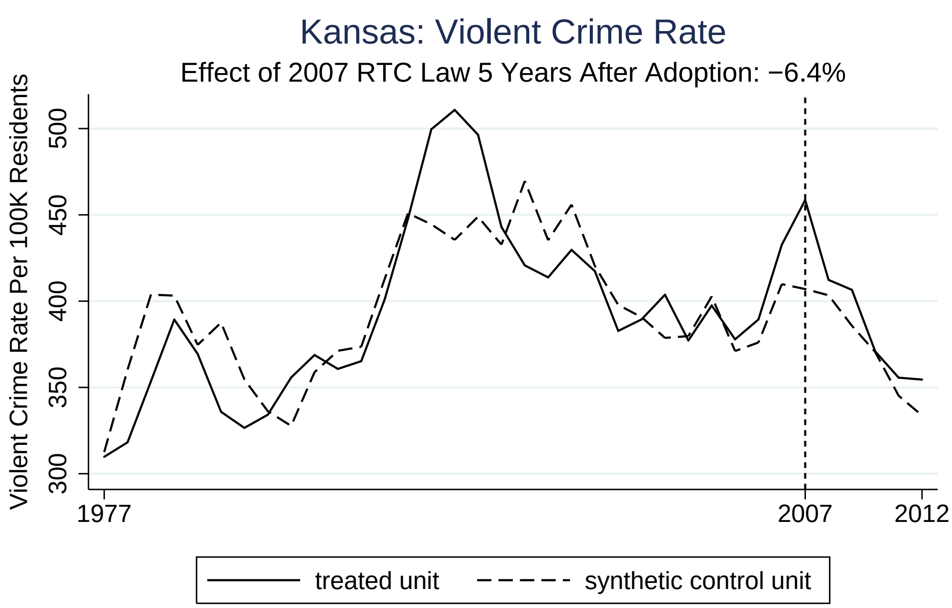
Composition of SC: NM (17.8%), NY (82.2%)
States Never Passing RTC Laws Included in Synthetic Control: NY
RTC−Adopting States Included In Synthetic Control: NM (2004)



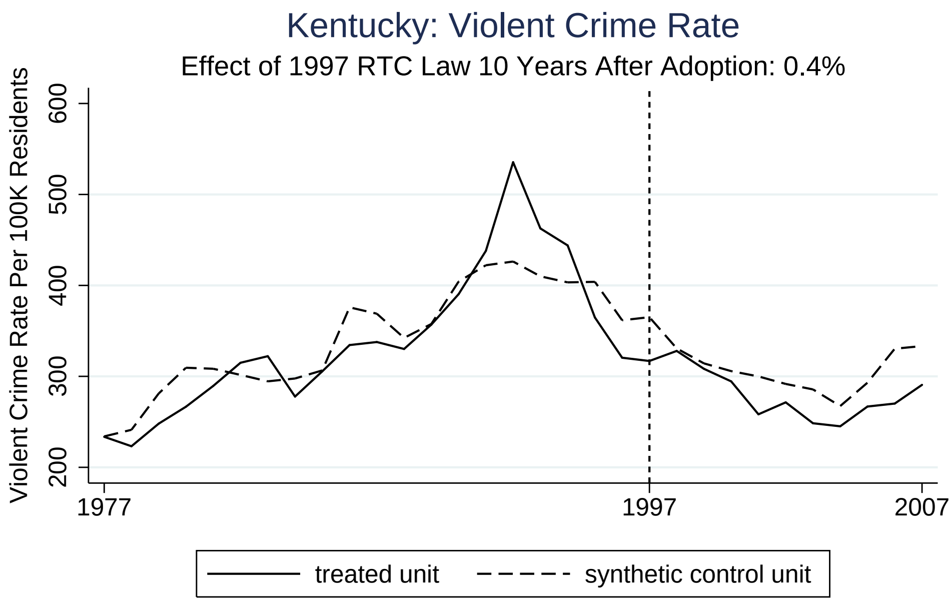
Composition of SC: MO (75.3%), NE (12.0%), NY (12.7%)
States Never Passing RTC Laws Included in Synthetic Control: NY
RTC−Adopting States Included In Synthetic Control: MO (2004), NE (2007)



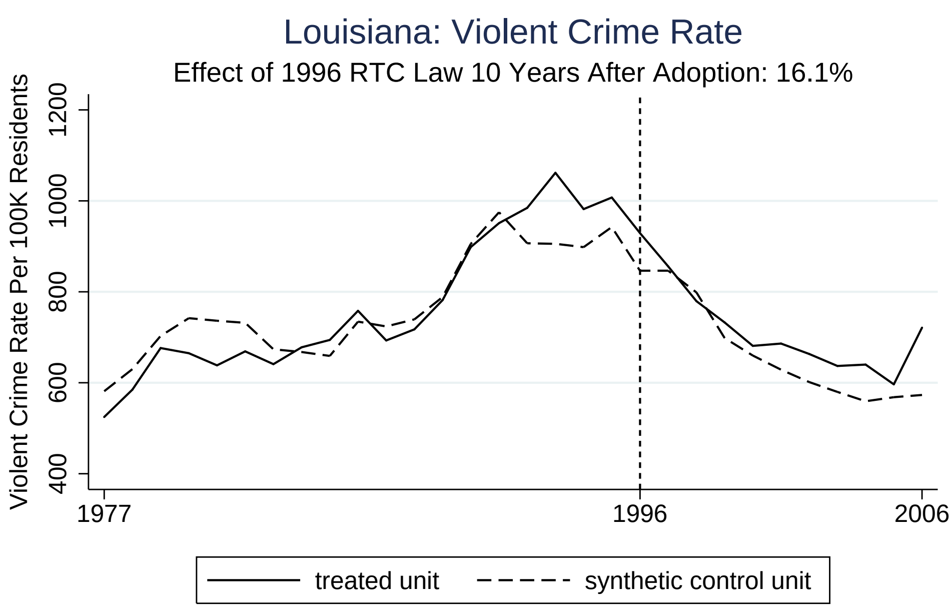
Composition of SC: CO (18.0%), IA (82.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: CO (2003), IA (2011)



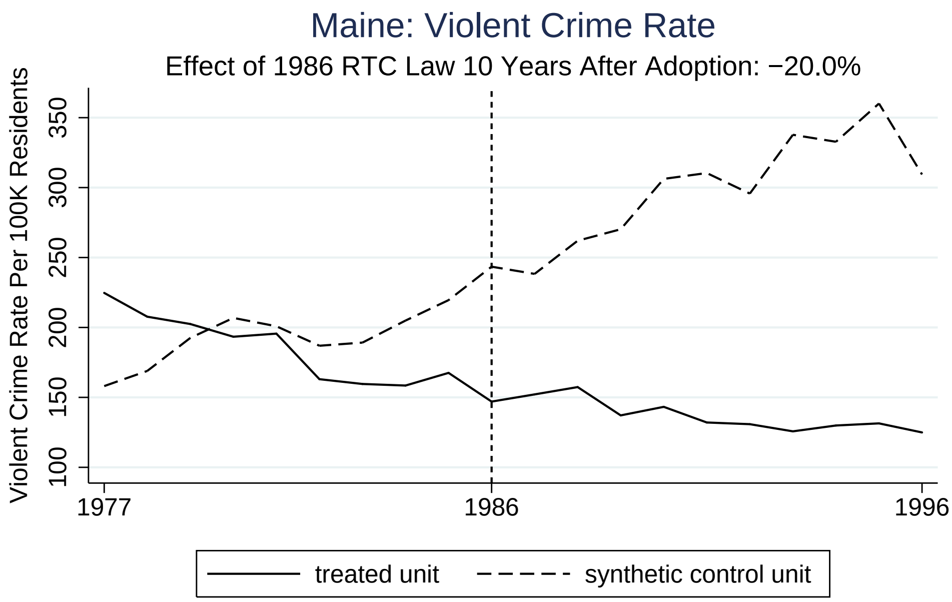
Composition of SC: CA (10.2%), DE (24.6%), HI (65.2%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE, HI
RTC−Adopting States Included In Synthetic Control:



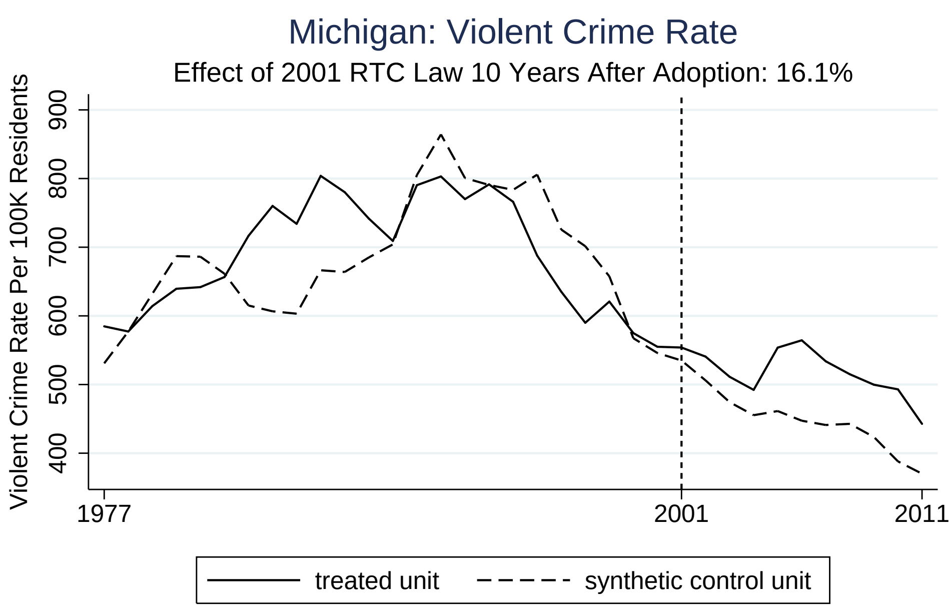
Composition of SC: CA (17.8%), IA (0.9%), WI (81.4%)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC−Adopting States Included In Synthetic Control: IA (2011), WI (2012)



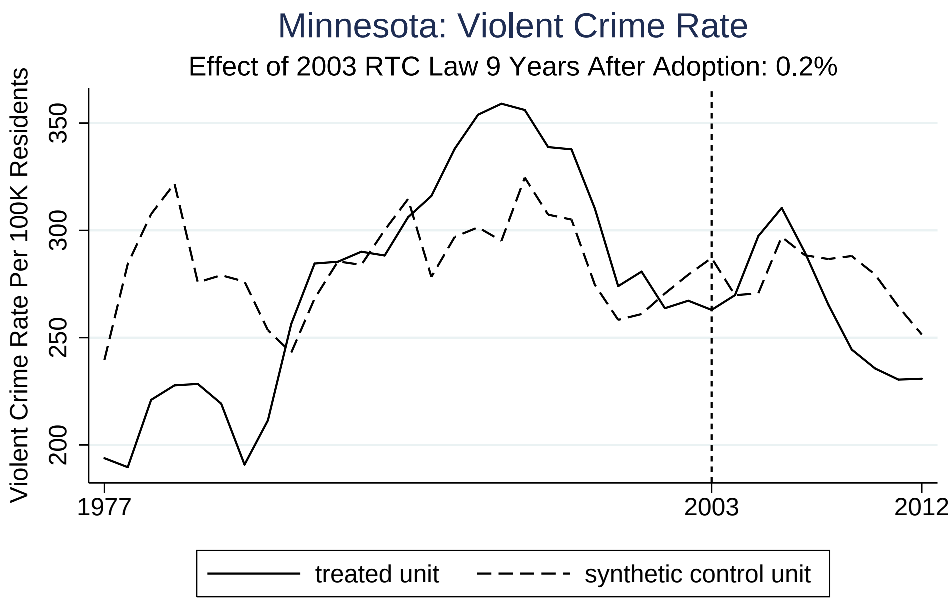
Composition of SC: DE (19.8%), IL (80.2%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC−Adopting States Included In Synthetic Control: IL (2014)



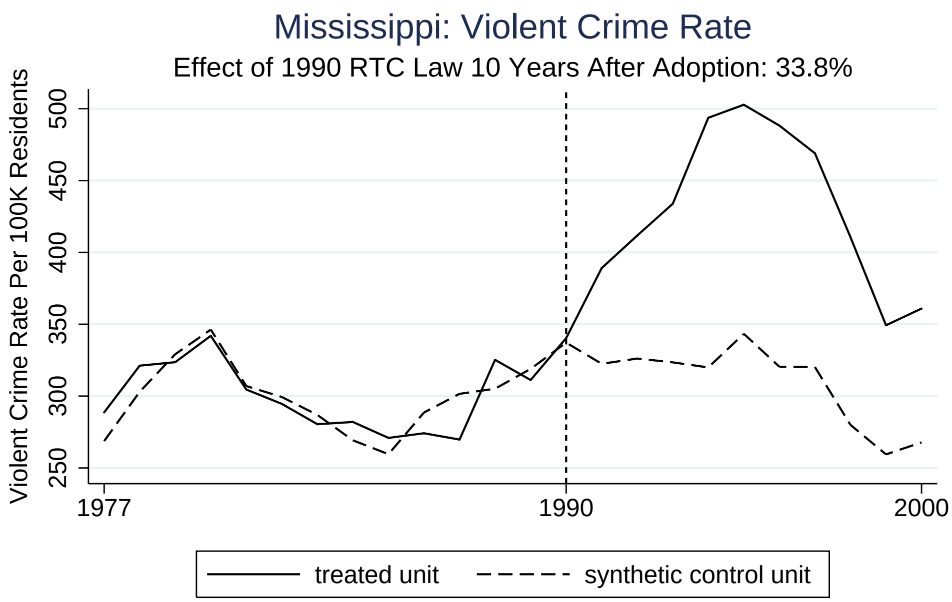
Composition of SC: IA (74.0%), MN (5.5%), NE (20.5%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: IA (2011), MN (2003), NE (2007)



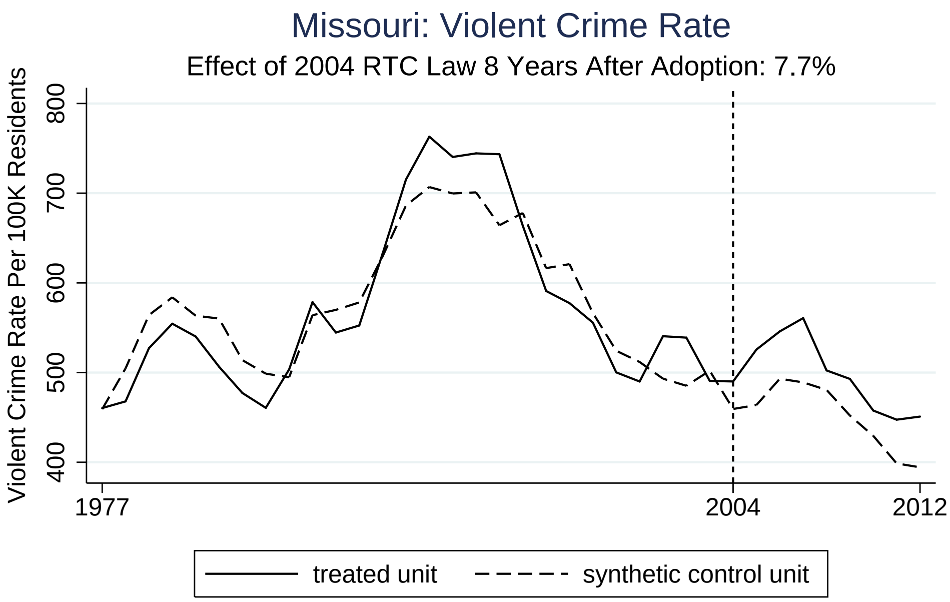
Composition of SC: IL (69.7%), RI (30.3%)
States Never Passing RTC Laws Included in Synthetic Control: RI
RTC−Adopting States Included In Synthetic Control: IL (2014)



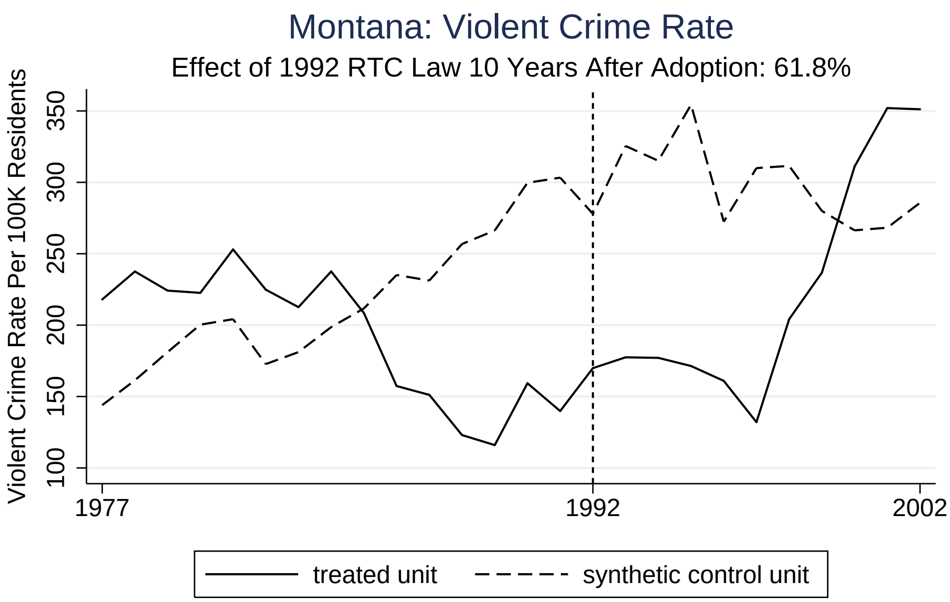
Composition of SC: HI (92.6%), MA (7.4%)
States Never Passing RTC Laws Included in Synthetic Control: HI, MA
RTC−Adopting States Included In Synthetic Control:



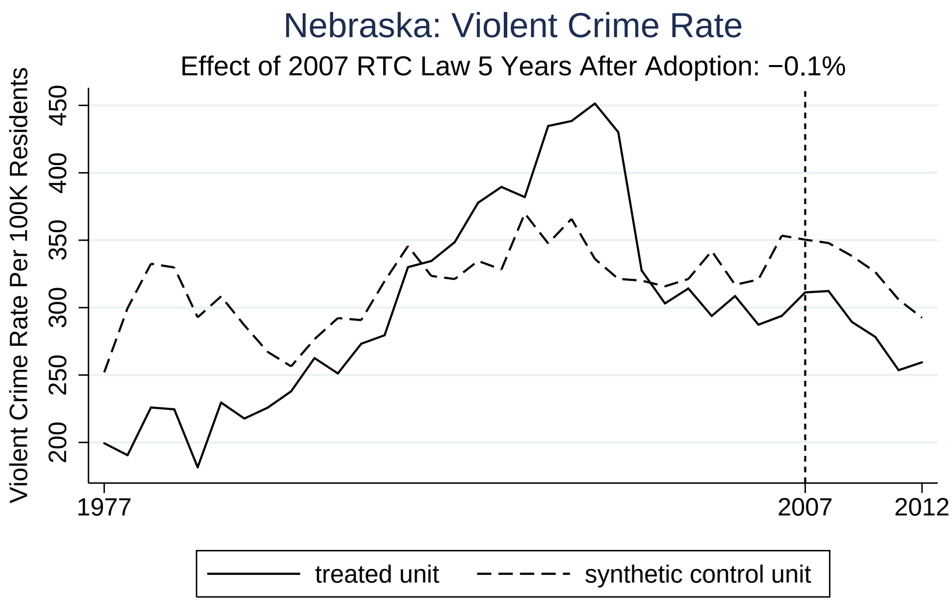
Composition of SC: HI (72.9%), NE (2.4%), OH (24.6%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC−Adopting States Included In Synthetic Control: NE (2007), OH (2004)



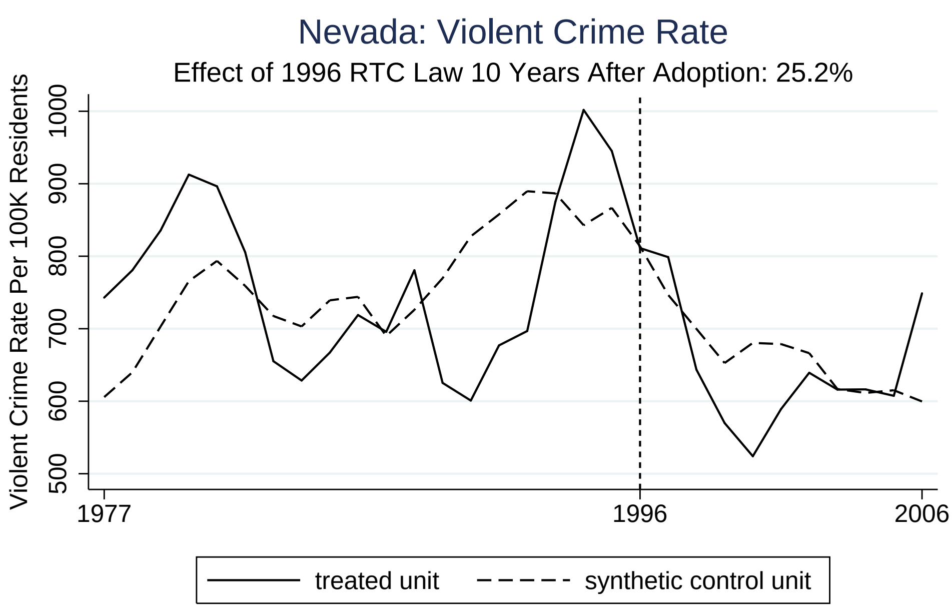
Composition of SC: CA (40.1%), DE (26.5%), HI (33.3%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE, HI
RTC−Adopting States Included In Synthetic Control:



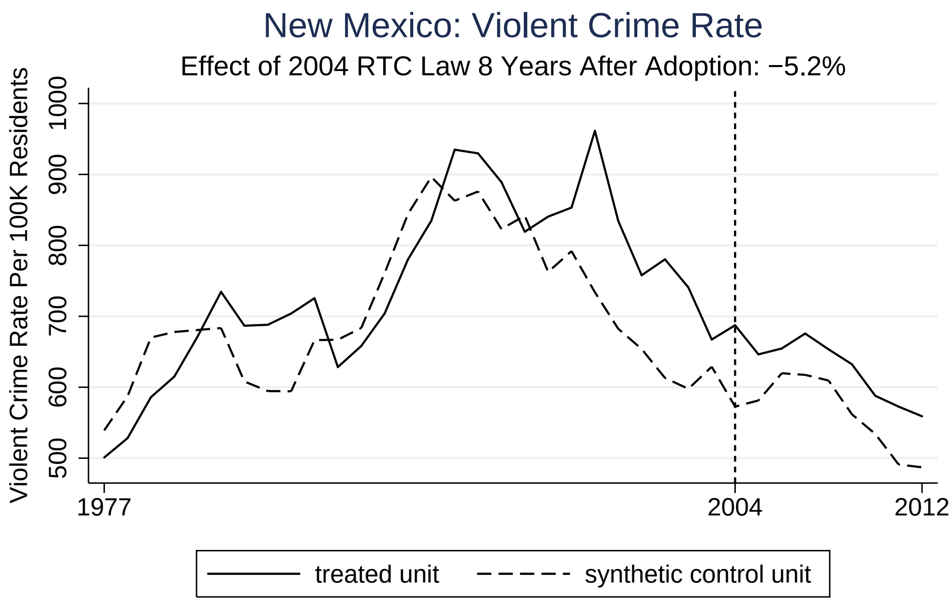
Composition of SC: IA (100.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: IA (2011)



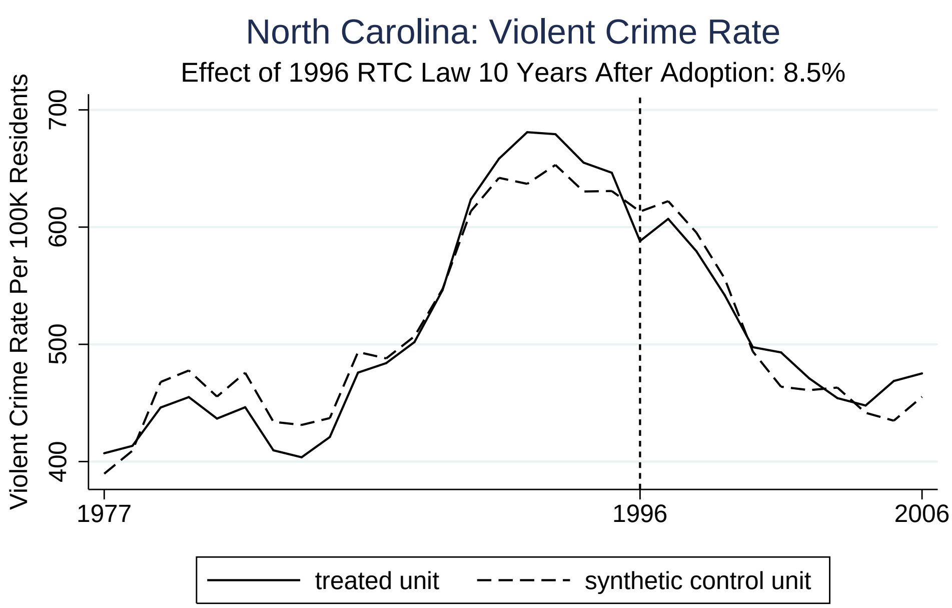
Composition of SC: DE (17.3%), HI (82.7%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI
RTC−Adopting States Included In Synthetic Control:



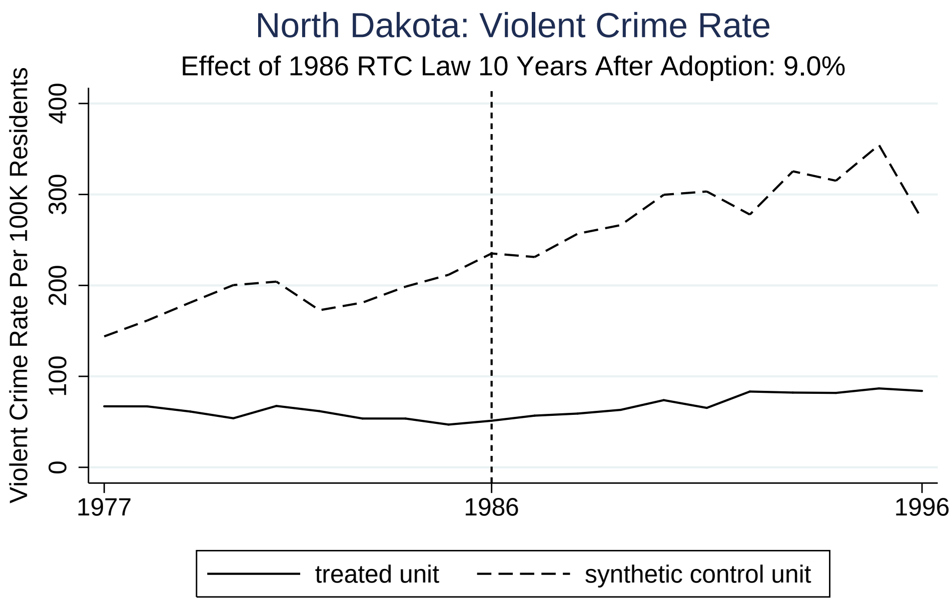
Composition of SC: KS (9.8%), MD (73.5%), NJ (16.7%)
States Never Passing RTC Laws Included in Synthetic Control: MD, NJ
RTC−Adopting States Included In Synthetic Control: KS (2007)



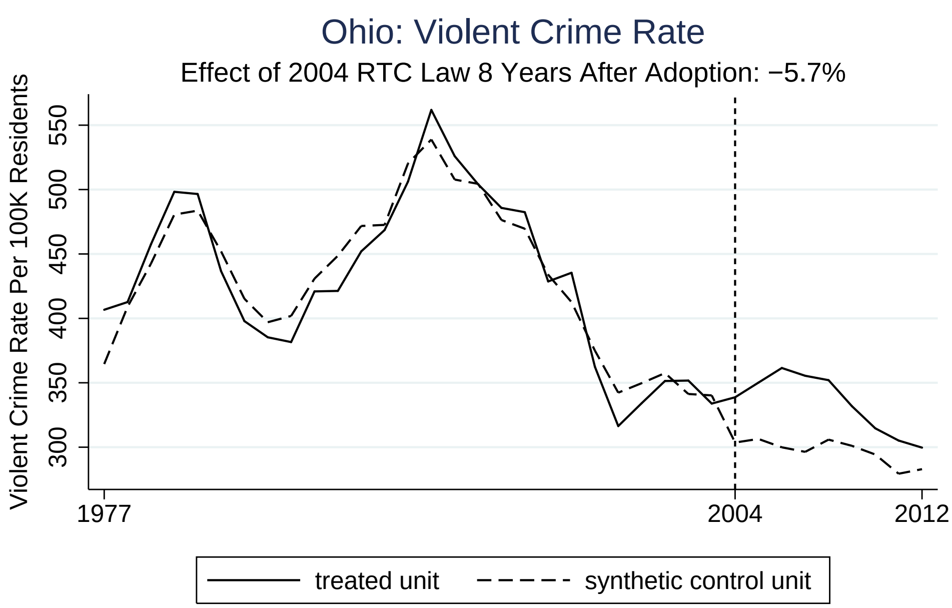
Composition of SC: CA (48.5%), DE (51.5%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE
RTC−Adopting States Included In Synthetic Control:



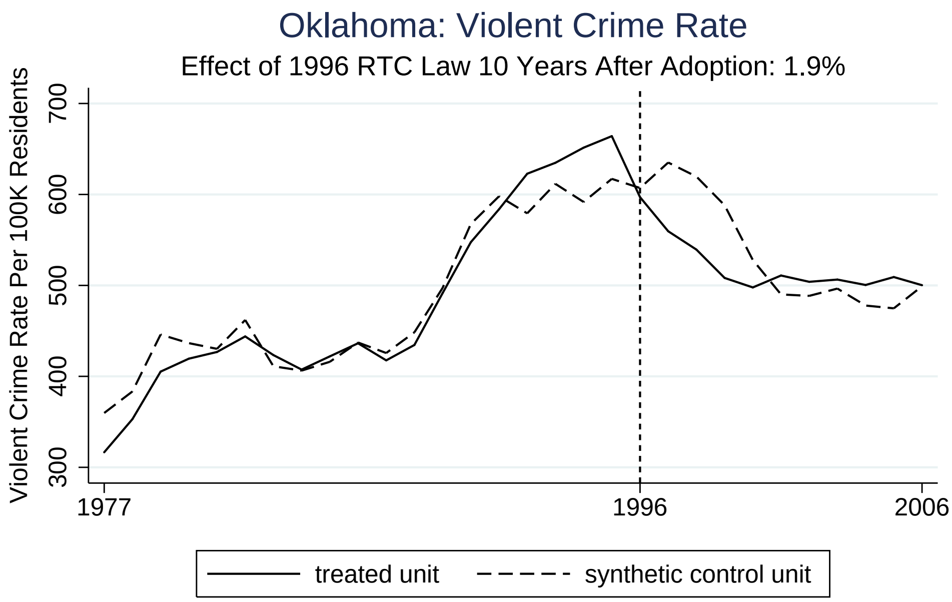
Composition of SC: CA (29.5%), DE (22.2%), NE (48.4%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE
RTC−Adopting States Included In Synthetic Control: NE (2007)



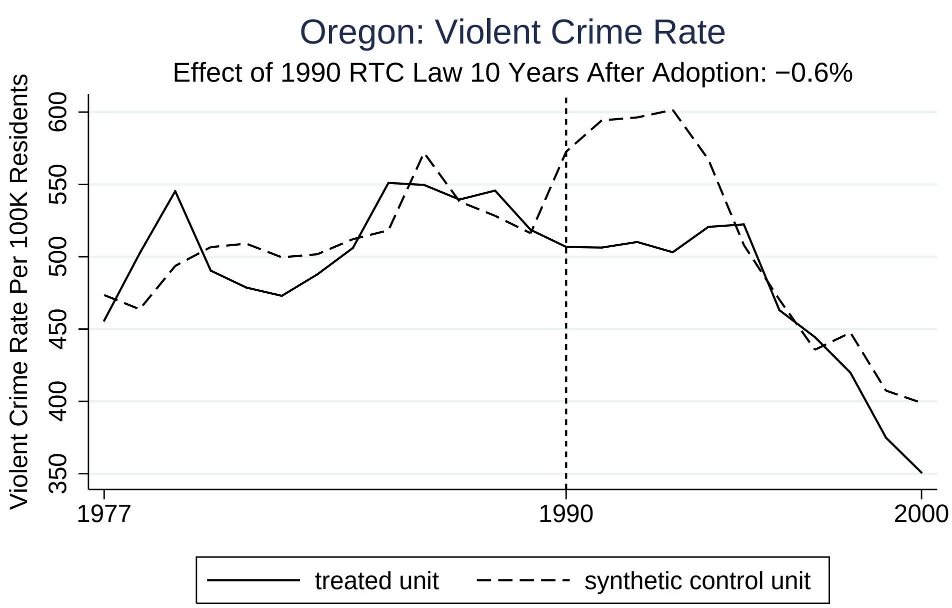
Composition of SC: IA (100.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: IA (2011)



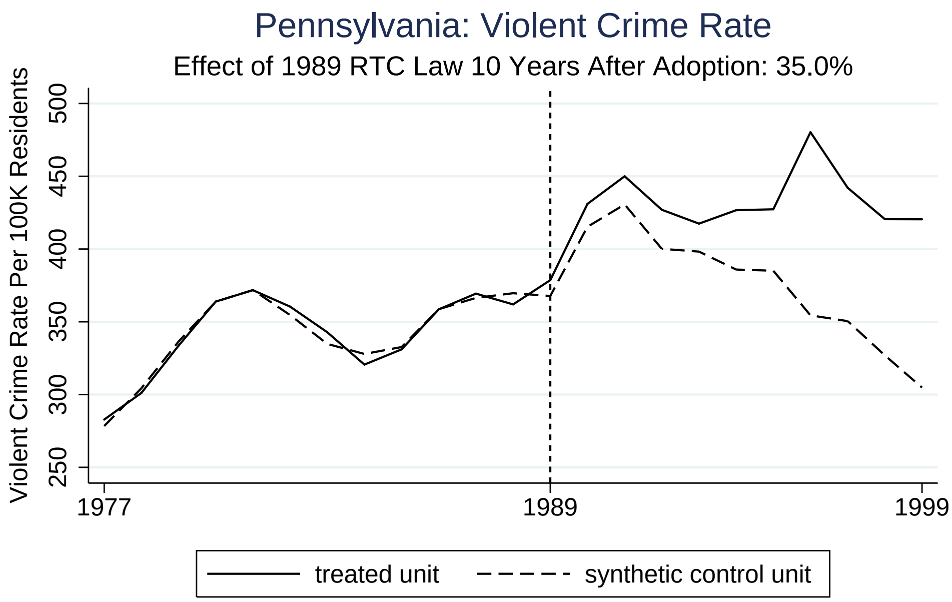
Composition of SC: CA (19.5%), HI (20.7%), RI (59.8%)
States Never Passing RTC Laws Included in Synthetic Control: CA, HI, RI
RTC−Adopting States Included In Synthetic Control:



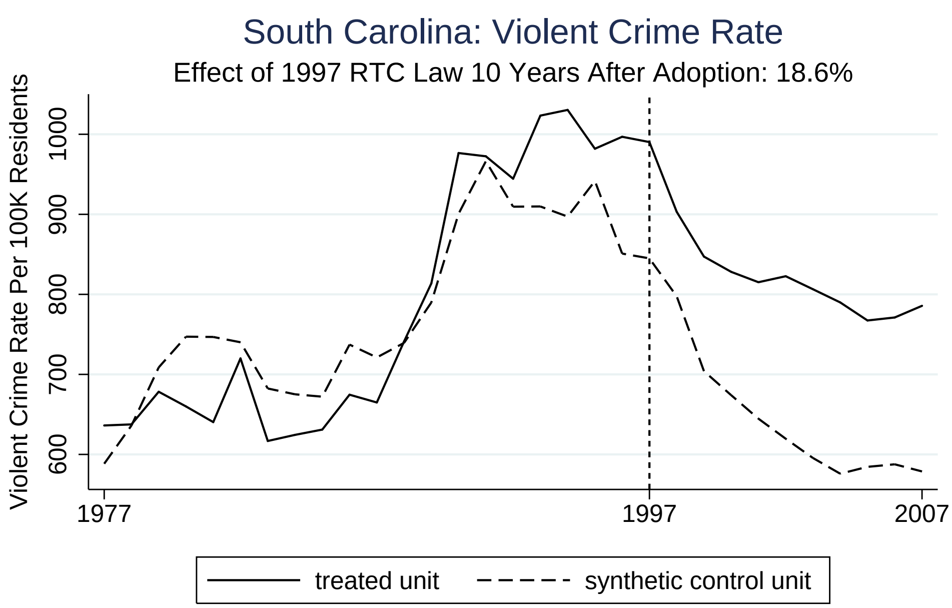
Composition of SC: CA (7.9%), DE (34.9%), MD (11.4%), NE (45.9%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE, MD
RTC−Adopting States Included In Synthetic Control: NE (2007)



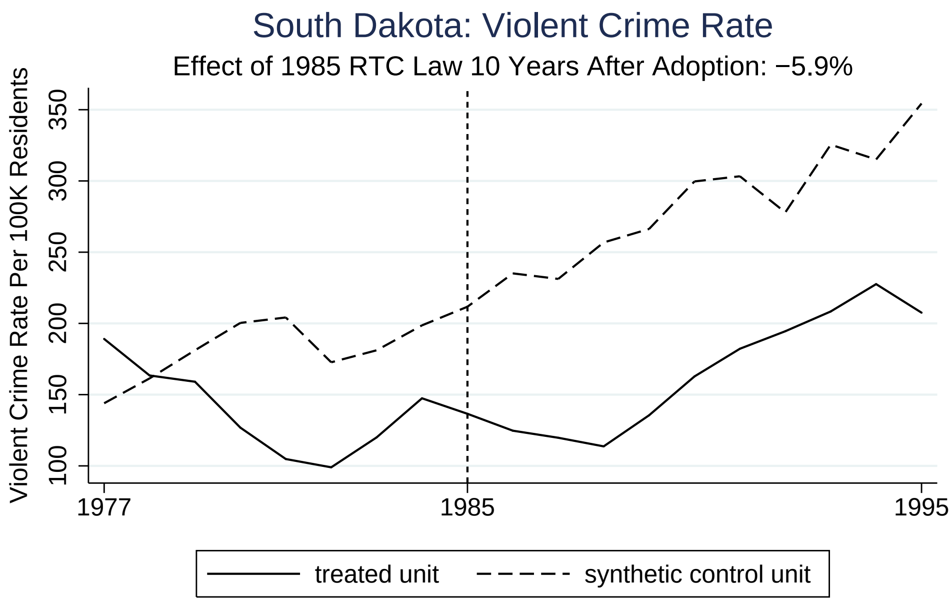
Composition of SC: CO (45.9%), MI (34.2%), MN (19.9%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: CO (2003), MI (2001), MN (2003)



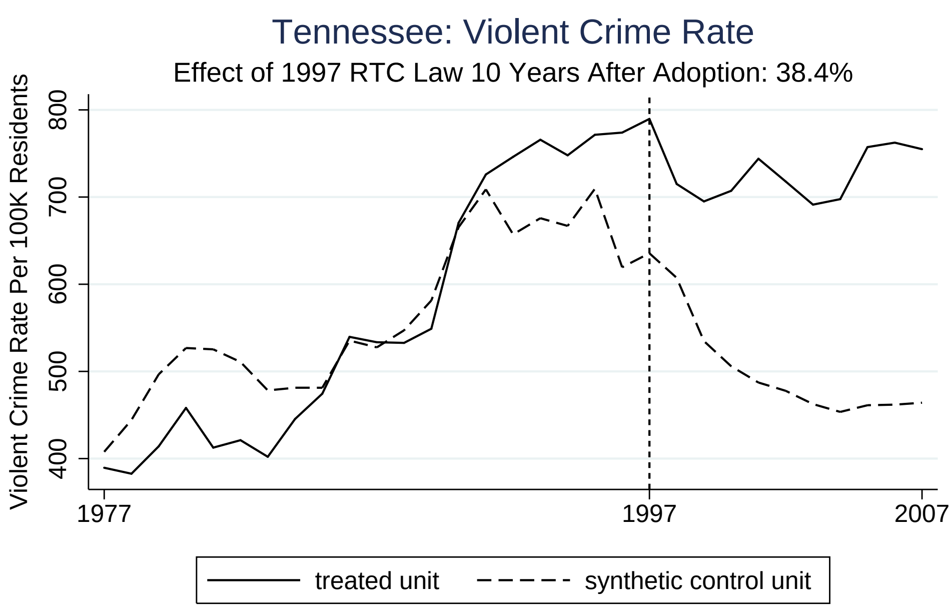
Composition of SC: HI (13.4%), IL (3.7%), MI (5.5%), NJ (0.1%), NY (2.2%), OH (3.5%), RI (38.6%), WI (33.
States Never Passing RTC Laws Included in Synthetic Control: HI, NJ, NY, RI
RTC−Adopting States Included In Synthetic Control: IL (2014), MI (2001), OH (2004), WI (2012)



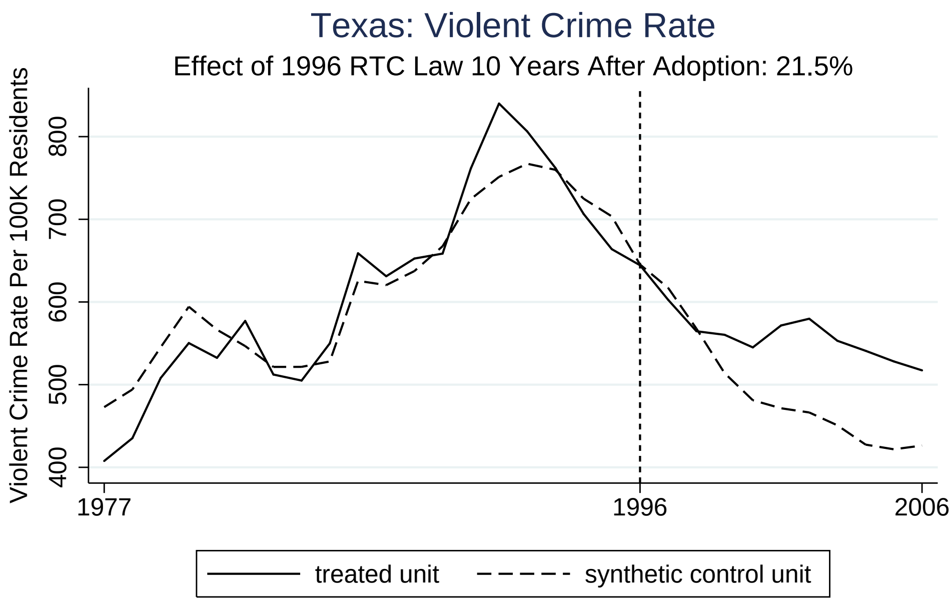
Composition of SC: DE (19.7%), IL (69.6%), MD (10.7%)
States Never Passing RTC Laws Included in Synthetic Control: DE, MD
RTC−Adopting States Included In Synthetic Control: IL (2014)



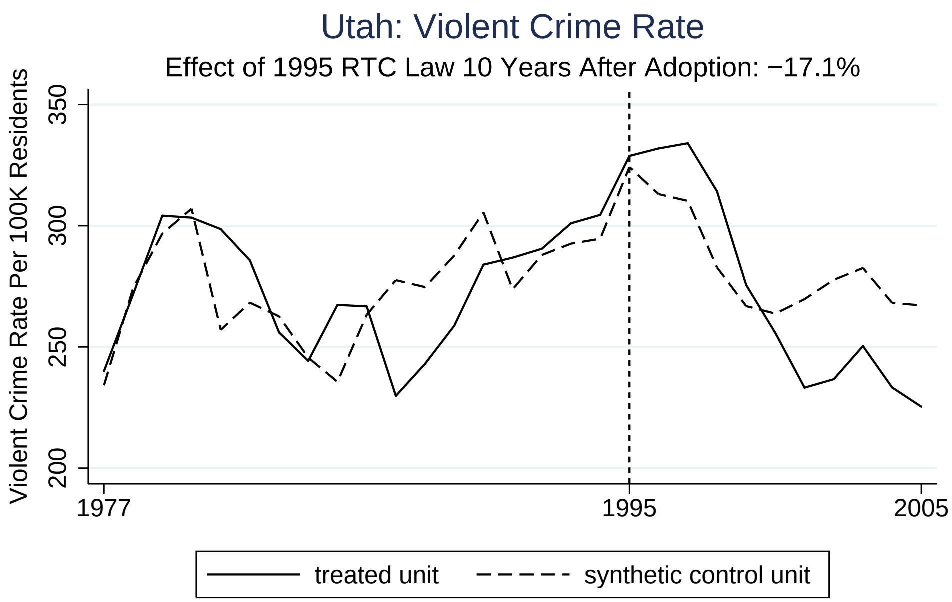
Composition of SC: IA (100.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: IA (2011)



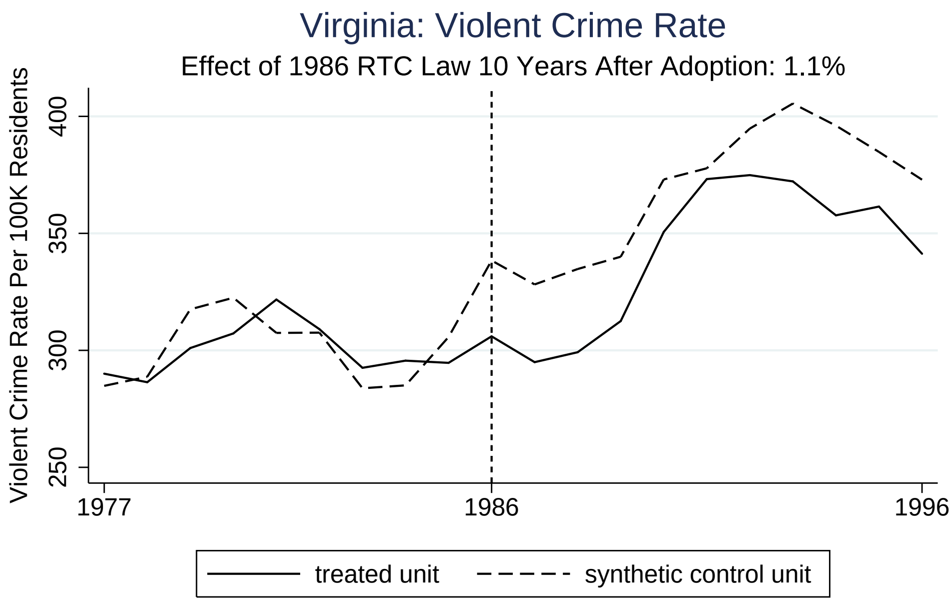
Composition of SC: DE (12.9%), IL (47.9%), IA (39.2%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC−Adopting States Included In Synthetic Control: IL (2014), IA (2011)



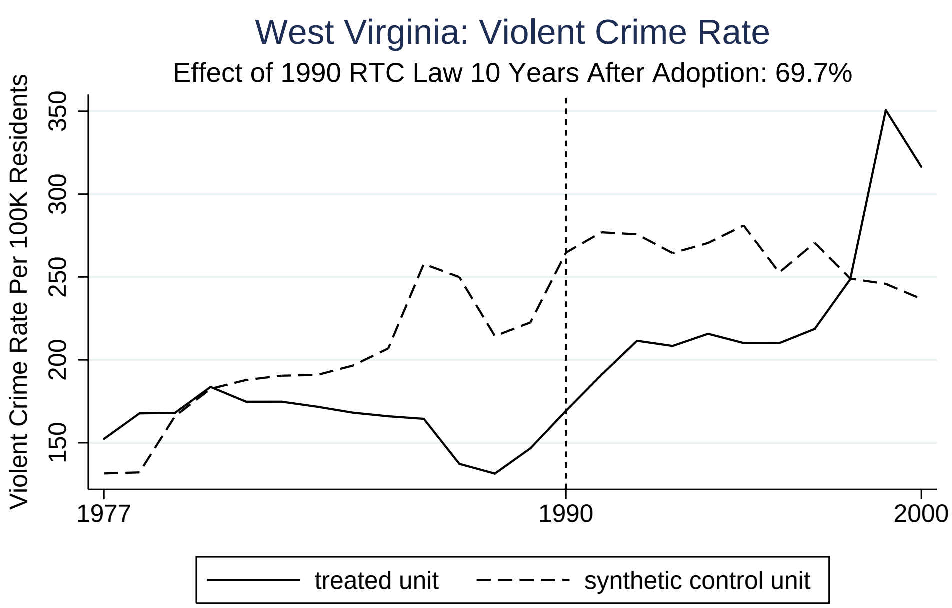
Composition of SC: CA (55.9%), IA (17.6%), NE (26.5%)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC−Adopting States Included In Synthetic Control: IA (2011), NE (2007)



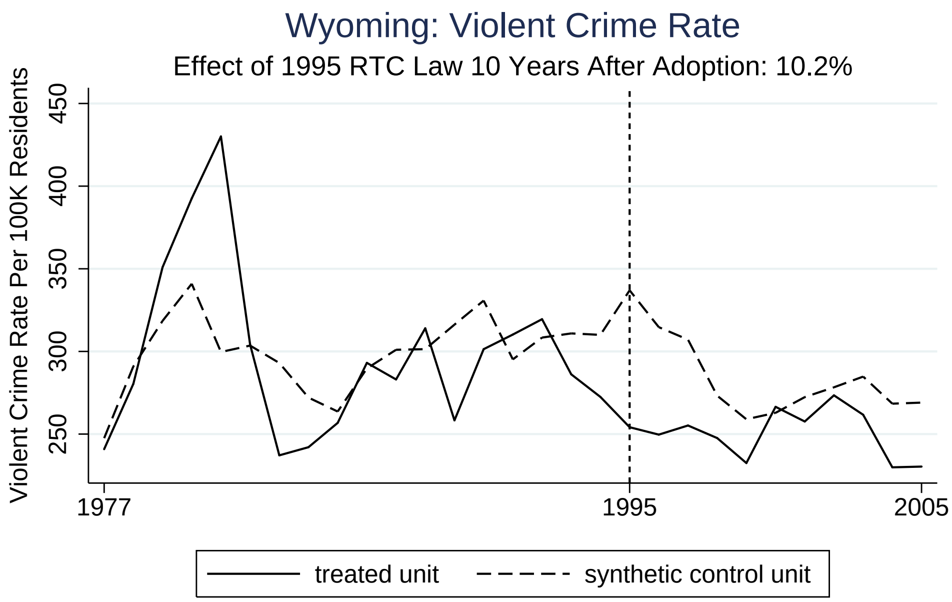
Composition of SC: HI (87.5%), IL (2.9%), NE (9.6%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC−Adopting States Included In Synthetic Control: IL (2014), NE (2007)



Composition of SC: CO (22.5%), DE (1.1%), HI (20.3%), MN (41.3%), NE (12.6%), SC (2.3%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI
RTC−Adopting States Included In Synthetic Control: CO (2003), MN (2003), NE (2007), SC (1997)



Composition of SC: WI (100.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC−Adopting States Included In Synthetic Control: WI (2012)



Composition of SC: HI (86.4%), NJ (13.6%)
States Never Passing RTC Laws Included in Synthetic Control: HI, NJ
RTC−Adopting States Included In Synthetic Control:

Appendix E: Graphs Showing Relative Size of the Estimated Treatment Effect Associated with RTC Laws (ADZ Predictor Variables)

Figure E1

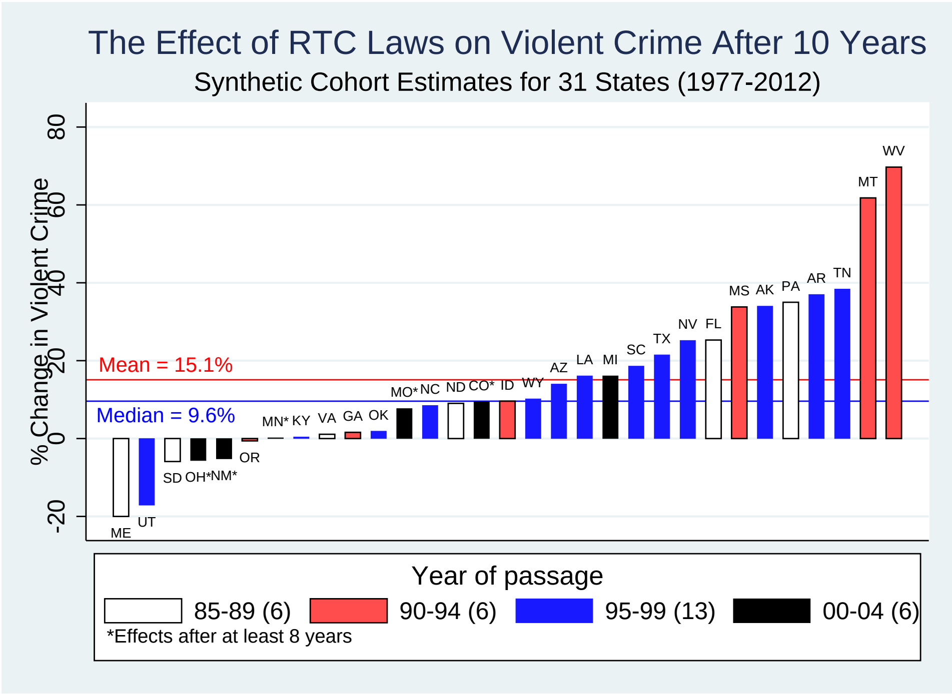


Figure E2