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

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ABSTRACT

The 2004 report of the National Research Council (NRC) on Firearms and Violence recognized that violent crime was higher in the post-passage period (relative to national crime patterns) for states adopting right-to-carry (RTC) concealed handgun laws, but because of model dependence the panel was unable to identify the true causal effect of these laws from the then-existing panel data evidence. This study uses 14 additional years of panel data (through 2014) capturing an additional 11 RTC adoptions and new statistical techniques to see if more convincing and robust conclusions can emerge.

Our preferred panel data regression specification (the "DAW model") and the Brennan Center (BC) model, as well as other statistical models by Lott and Mustard (LM) and Moody and Marvell (MM) that had previously been offered as evidence of crime-reducing RTC laws, now consistently generate estimates showing RTC laws increase overall violent crime and/or murder when run on the most complete data.

We then use the synthetic control approach of Alberto Abadie and Javier Gardeazabal (2003) to generate state-specific estimates of the impact of RTC laws on crime. Our major finding is that under all four specifications (DAW, BC, LM, and MM), RTC laws are associated with higher aggregate violent crime rates, and the size of the deleterious effects that are associated with the passage of RTC laws climbs over time. We estimate that the adoption of RTC laws substantially elevates violent crime rates, but seems to have no impact on property crime and murder rates. Ten years after the adoption of RTC laws, violent crime is estimated to be 13-15% percent higher than it would have been without the RTC law. Unlike the panel data setting, these results are not sensitive to the covariates included as predictors. The magnitude of the estimated increase in violent crime from RTC laws is substantial in that, using a consensus estimate for the elasticity of crime with respect to incarceration of .15, the average RTC state would have to double its prison population to counteract the RTC-induced increase in violent crime.

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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 RTC laws, arguably making the pair's 1997 paper in the *Journal of Legal Studies* one of the most consequential criminological articles published in the last twenty-five years.

The original Lott and Mustard paper as well as subsequent work by John Lott in his 1998 book *More Guns, Less Crime* used a panel data analysis to support their theory that RTC laws reduce violent crime. A large number of papers examined the Lott thesis, with decidedly mixed results. A number of studies, 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 2005 National Research Council report *Firearms and Violence: A Critical Review* (hereafter the NRC Report) would resolve the controversy over the impact of RTC laws, but this was not to be. While one member of the committee—James Q. Wilson—did partially endorse the Lott thesis by saying there was evidence that murders fell when RTC laws were adopted, the other 15 members of the panel pointedly criticized Wilson's claim, saying that "the scientific evidence does not support his position." The majority emphasized that the estimated effects of RTC laws were highly sensitive to the particular choice of explanatory variables and thus concluded that the panel data evidence through 2000 was too fragile to support any conclusion about the true effects of these laws.

This paper begins by revisiting the panel data evidence to see if extending the data for an additional 14 years, thereby providing additional crime data for prior RTC states as well as on 11 newly adopting RTC states, offers any clearer picture of the causal impact of allowing citizens to carry concealed weapons. Across seven different permutations from four major sets of explanatory variables—including our preferred model (DAW) plus models used by the Brennan Center (BC), Lott and Mustard (LM), and Moody and Marvell (MM)—RTC laws are associated with *higher* rates of overall violent crime and/or murder.

To answer the call of the NRC Report for new approaches to estimate the impact of RTC

[1]In support of Lott and Mustard (1997), see Lott's 1998 book *More Guns, Less Crime* (and the 2000 and 2010 editions). Ayres and Donohue (2003) and the 2005 National Research Council report *Firearms and Violence: A Critical Review* dismissed the Lott/Mustard hypothesis as lacking credible statistical support, as did Aneja, Donohue, and Zhang (2011) (and Aneja, Donohue, and Zhang (2014) further expanding the latter). Moody and Marvell (2008) and Moody et al. (2014) continued to argue in favor of a crime-reducing effect of RTC laws, although Zimmerman (2014) concludes that RTC laws *increase* violent crime, as discussed in Section II.B.6.

laws, we use a new statistical technique designed to address some of the weaknesses of panel data models, that has gained prominence in the period since the NRC Report's release (2005). Using the 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, Diamond, and Hainmueller (2010) and Abadie, Diamond, and Hainmueller (2014)—uses a matching methodology to create a credible "synthetic control" based on a weighted average of other states that best matches the pre-passage pattern of crime for each "treated" state, which can then be used to estimate the likely path of crime if RTC-adopting states had not adopted 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]

To preview our major findings, the synthetic controls estimate of the average impact of RTC laws across the 33 states that adopt between 1981 and 2007[4] indicate that violent crime is substantially higher after ten years than would have been the case had the RTC law not been adopted. Essentially, for violent crime, the synthetic controls approach provides a similar portrayal of RTC laws as that provided by the DAW and BC panel data models and undermines the results of the LM and MM panel data models. According to the aggregate synthetic control models—whether one uses the DAW, BC, LM, or MM covariates—RTC laws led to increases in violent crime of 13-15 percent after ten years, with positive but not statistically significant effects on property crime and murder. The median effect of RTC adoption after 10 years is 14.1 percent whether one considers all 31 states with ten years of data or limits the analysis to the 26 states with the most compelling pre-passage fit between the adopting states and their synthetic controls. Comparing our DAW-specification findings with the results generated using placebo treatments, we are able to reject the null hypothesis that RTC laws have no impact on aggregate violent crime.

The structure of the paper proceeds as follows. Part II discusses the panel data results for the four different models, showing that the DAW and BC models indicate that RTC laws have increased violent and property crime, while the LM and MM models provide evidence that RTC

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

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

[4] Note that we do not generate a synthetic control estimate for Indiana, even though it passed its RTC law in 1980, owing to the fact that we do not have enough pre-treatment years to accurately match the state with an appropriate synthetic control. We consider the effect of making Indiana a treatment state as a robustness check and find that this change does not meaningfully change our results. Similarly, we do not generate synthetic control estimates for Iowa and Wisconsin (whose RTC laws went into effect in 2011) and for Illinois (2014 RTC law), because of the limited post-passage data.

laws have increased murder. We argue that the DAW set of explanatory variables are the most plausible and show that modest and advisable corrections to the LM and MM specifications also generate estimates that RTC laws increase violent crime.

The remainder of the paper shows that the synthetic controls approach under all four sets of explanatory variables uniformly supports the conclusion that RTC laws lead to substantial increases in violent crime. Part III describes the statistical underpinnings of the synthetic controls approach and specific details of our implementation of this technique. Part IV provides our synthetic controls estimates of the impact of RTC laws, and Part V concludes with some thoughts on the mechanisms by which RTC laws increase violent crime.

I. Panel Data Estimates of the Impact of RTC Laws

A. The No-Controls Model

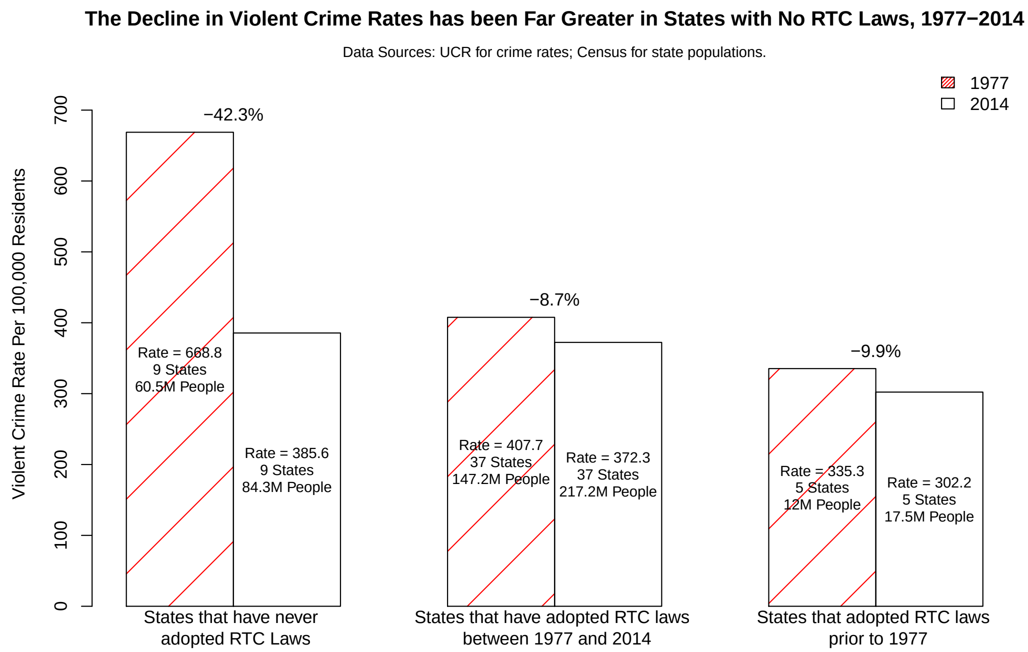
We follow the NRC Report by beginning with the basic facts about how crime has unfolded relative to national trends for states adopting RTC laws. Figure 1 depicts percentage changes in the violent crime rate over our entire data period for three groups of states: those that never adopted RTC laws, those that adopted RTC laws sometime between 1977 and 2014, and those that adopted RTC laws prior to 1977. It is noteworthy that the nine states that never adopted RTC laws experienced declines (in percentage terms) in violent crime that are greater than four times the reduction experienced by states that adopted RTC either prior to 1977 or during our period of analysis.[5]

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

We now estimate this same model using 14 additional years of data (through 2014) and 11 additional adopting states (listed at the bottom of Table 8). Row 1 of Table 1 shows the results of

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

Figure 1



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). According to this model, the average post-passage increase in violent crime was 20.2 percent, while the comparable increase in property crime was 19.2 percent. Row 1 also reports the impact of RTC laws on the murder rate (Column 1) and the murder count using a negative binomial model (Column 2), which provide statistically insignificant estimates that RTC laws increase murder by 4-5 percent.[6]

The NRC Report also presented a spline model to estimate how RTC adoption might alter the trend in crime for adopting states, which suggested violent crime and property declined relative to trend in the data through 2000, while the trend in murder was unchanged. Row 2 of Table 1 recomputes this "no-controls" spline model on data through 2014, which eliminates the earlier suggestion that RTC laws were associated with any drop (relative to trend) in violent or property crime, and reaffirms the null finding for murder.[7] In other words, more and better data have strengthened the

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

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

dummy variable model finding that RTC laws increase violent crime, and eliminated the earlier spline model showing of possible declines in violent and property crime.

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

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	3.83 (8.79)	1.049 (0.053)	20.21*** (6.83)	19.18*** (6.06)
Spline Model	−0.28 (0.61)	1.004 (0.004)	0.22 (0.79)	0.14 (0.50)

OLS estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. Incidence Rate Ratios (IRR) estimated using Negative Binomial Regression, where state population is included as a control variable, are presented in Column 2. The null hypothesis is that the IRR equals 1. The source of all the crime rates is the Uniform Crime Reports (UCR). * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

While the Table 1 dummy model indicates that RTC states experience a worse post-passage crime pattern, this does not prove that RTC laws increase crime. For example, it might be the case that some states decided to fight crime by allowing citizens to carry concealed handguns while others decided to hire more police and incarcerate a greater number of convicted criminals. If police and prisons were more effective in stopping crime, the "no controls" model might show that the crime experience in RTC states was worse than in other states even if this were not a true causal result of the adoption of RTC laws. As it turns out, though, RTC states not only experienced higher rates of violent crime but they also had larger increases in incarceration and police than other states. While the roughly 7 percent greater increase in the incarceration rate in RTC states is not statistically significant, the increases are large and statistically significant for police. Accordingly, Table 2 confirms that RTC states did *not* have declining rates of incarceration or total police employees after adopting their RTC laws that might explain their relatively bad crime performance.

B. Adding Explanatory Variables

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

Table 2: Panel Data Estimates Showing Greater Increases in Incarceration and Police Following RTC Adoption: State and Year Fixed Effects, and No Other Regressors, 1977-2014

	Incarceration	Police Employment Per 100k	Police Officers Per 100k
	(1)	(2)	(3)
Dummy Variable Model	6.78 (6.22)	8.39^{***} (3.15)	7.08^{**} (2.76)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The source of the police employment rate and the sworn police officer rate is the Uniform Crime Reports (UCR). The source of the incarceration rate is the Bureau of Justice Statistics (BJS) * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

to invest at least as heavily in prisons and actually invested more heavily in police than non-RTC states. While the theoretical predictions about the effect of RTC laws on crime are indeterminate, these two empirical facts based on the actual patterns of crime and crime-fighting measures in RTC and non-RTC states suggest that the most plausible working hypothesis is that RTC laws *increase* crime. The next step in a panel data analysis of RTC laws would be to test this hypothesis by introducing an appropriate set of explanatory variables that plausibly influence crime.

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

1. The DAW Panel Data Model

In addition to the state and year fixed effects of the no controls model and the identifier for the presence of a RTC law, our preferred "DAW model" includes an array of other factors that might be expected to influence crime, such as the levels of police and incarceration, various income, poverty and unemployment measures, and six demographic controls designed to capture the presence of males in three racial categories (Black, White, other) in two high-crime age groupings (15-19 and 20-39). The full set of explanatory variables is listed in Table 3, along with the regression models used in three other studies that have estimated the impact of RTC laws on crime.[8]

[8]While we attempt to include as many states in these regressions as possible, District of Columbia incarceration data is missing after the year 2001. In addition, a handful of observations are also dropped from the LM and MM regressions owing to states that did not report any usable arrest data in various years. Our regressions are performed

The DAW panel data model in Table 4 (run on data from 1979-2014) is consistent with the same basic pattern observed in Table 1:[9] RTC laws on average increased violent crime by 9.5 percent and property crime by 6.8 percent in the years following adoption according to the dummy model, but again showed no statistically significant effect in the spline model.[10] As we saw in the no-controls model, the estimated effect of RTC laws in Table 4 on the murder rate is also not statistically significant.

2. The BC Panel Data Model

Table 3 lists the variables used in the Brennan Center (BC) crime regression model, which differ in a few respects from the DAW model (although to a lesser degree than the LM and MM models) Roeder et al. 2015. The BC model controls for both incarceration and police rates (as in DAW), but the BC model takes the log of both these rates. The BC model alone controls for the number of executions, and unlike DAW does not control for either the state poverty rate or the percentage of the state population living in a Metropolitan Statistical Area. Moreover, while DAW includes six demographic variables, BC uses three age groupings over the ages 15-29, and simply controls for the black percentage of the state population.

The results of running the BC model over the period from 1978-2014 are presented in Table 5, Panel A. With the exception that the BC dummy variable model estimate of the increase in violent crime is somewhat higher than that for DAW (10.98 percent increase versus 9.49 percent increase), the DAW and BC model estimates are almost identical in suggesting higher rates of violent and property crime (the dummy models) but no impact in the spline models. If we replace the four BC demographic variables with the 6 DAW demographic variables (Table 5, Panel B), the size of the estimated increases in violent crime and property crime (in the dummy models) are only modestly lower than the DAW results in Table 4.

3. The LM Panel Data Model

Table 3's recitation of the explanatory variables contained in the Lott and Mustard (LM) panel data model reveals two obvious omissions: there are no controls for the levels of police and incarcer-

with robust standard errors that are clustered at the state level, and we lag the arrest rates used in both the LM and MM regression models. The rationales underlying both of these changes are described in more detail in Aneja, Donohue, and Zhang (2014). All of the regressions presented in this paper are weighted by state population.

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

[10] Defensive uses of guns are more likely for violent crimes because the victim will clearly be present. For property crimes, the victim is typically absent, thus providing less opportunity to defend with a gun. It is unclear whether the many ways in which RTC laws could lead to more crime, which we discuss in Part V, would be more likely to facilitate violent or property crime, but our intuition is that violent crime would be more strongly influenced, which is in fact what Table 4 suggests.

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

Explanatory Variables	DAW	BC	LM	MM
Right to Carry Law	x	x	x	x
Lagged Per Capita Incarceration Rate	x			x
Lagged Log of Per Capita Incarceration Rate		x		
Lagged Police Staffing Per 100,000 Residents	x			
Lagged Log of Sworn Police Officers Per Resident Population		x		
Lagged Number of Executions		x		
Poverty Rate	x			x
Unemployment Rate	x	x		x
Per Capita Ethanol Consumption from Beer	x	x		
Percentage of the State Population living in Metropolitan Statistical Areas (MSAs)	x			
Real Per Capita Personal Income	x		x	x
Nominal Per Capita Income (Median Income in BC)		x		
Real Per Capita Income Maintenance			x	x
Real Per Capita Retirement Payments			x	x
Real Per Capita Unemployment Insurance Payments			x	x
Population Density			x	
Lagged Violent or Property Arrest Rate			x	x
State Population			x	x
Crack Index				x
Lagged Dependent Variable				x
6 Age-Sex-Race Demographic Variables -all 6 combinations of black, white, and other males in 2 age groups (15-19, 20-39) indicating the percentage of the population in each group	x			
3 Age-Group Percentages (15-19, 20-24, 25-29), and Black Percentage of Population		x		
36 Age-Sex-Race Demographic Variables -all possible combinations of black and white males in 6 age groups (10-19, 20-29, 30-39, 40-49, 50-64 and over 65) and repeating this all for females, indicating the percentage of the population in each group			x	x

Note: The DAW model is advanced in this paper, while the other three models were previously published by the Brennan Center (BC), Lott and Mustard (LM), and Marvell and Moody (MM). See footnote 41 in Appendix B for an explanation of the differences in the retirement payments variable definition between the LM and MM specifications. The crack index variable in the MM specification is available only for 1980-2000.

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

	Murder Rate (1)	Murder Count (2)	Violent Crime Rate (3)	Property Crime Rate (4)
Dummy Variable Model	0.30 (5.35)	1.050 (0.052)	9.49*** (2.96)	6.76** (2.73)
Spline Model	−0.31 (0.53)	1.002 (0.004)	0.05 (0.64)	0.14 (0.38)

OLS estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. Incidence Rate Ratios (IRR) estimated using Negative Binomial Regression, where state population is included as a control variable, are presented in Column 2. The null hypothesis is that the IRR equals 1. The source of all the crime rates is the Uniform Crime Reports (UCR). Six demographic variables (based on different age-sex-race categories) are included as controls in the regression above. Other controls include the lagged incarceration rate, the lagged police employee rate, real per capita personal income, the unemployment rate, poverty rate, beer, and percentage of the population living in MSAs. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

ation in each state, even though a substantial literature has found that these factors have a large impact on crime. Indeed, as we saw above in Table 2 both of these factors grew after RTC law adoption, and the increase in police employment after RTC adoption is substantively and statistically significant. 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 LM arrest rates are also econometrically problematic since the denominator of the arrest rate is the numerator of the dependent variable crime rate, improperly leaving the dependent variable

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

Panel A: BC Regressors Including 4 Demographic Variables

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	3.45 (5.67)	1.050 (0.051)	10.98*** (3.65)	6.86** (3.26)
Spline Model	−0.49 (0.51)	1.003 (0.004)	0.19 (0.66)	0.12 (0.35)

Panel B: BC Regressors with 6 DAW Demographic Variables

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	1.88 (5.47)	1.057 (0.051)	8.97*** (3.29)	5.57* (2.85)
Spline Model	−0.33 (0.48)	1.003 (0.004)	0.24 (0.59)	0.16 (0.34)

OLS estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. Incidence Rate Ratios (IRR) estimated using Negative Binomial Regression, where state population is included as a control variable, are presented in Column 2. The null hypothesis is that the IRR equals 1. The source of all the crime rates is the Uniform Crime Reports (UCR). Four demographic variables (percent black, percent aged 15-19, percent aged 20-24, and percent aged 25-29) are included in the Panel A regressions. The 6 DAW demographic variables are used in the Panel B regressions. Other controls include log of the lagged incarceration rate, lagged police employment per resident population, the unemployment rate, nominal per capita income, lagged number of executions, gallons of beer consumed per capita. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

on both sides of the regression equation. We lag the arrest rates by one year to reduce this problem of ratio bias.

Lott and Mustard's use of 36 demographic variables is also a potential concern. With so many enormously collinear variables, the high likelihood of introducing noise into the estimation process is revealed by the wild fluctuations in the coefficient estimates on these variables. For example, consider the LM explanatory variables "neither black nor white male aged 30-39" and the identical corresponding female category. The LM dummy variable model for violent crime suggests that the male group will vastly *increase* crime (the coefficient is 211!), but their female counterparts have an enormously dampening effect on crime (with a coefficient of -255!). Both of those highly implausible estimates (not shown in Table A2) are statistically significant at the 1 percent level, and they are almost certainly picking up noise rather than revealing true relationships. Bizarre results are common in the LM estimates among these 36 demographic variables.[11]

Table 6, Panel A shows the results of the LM panel data model estimated over the period 1977-2014. As seen above, the DAW model generated estimates that RTC laws raised violent and property crime (in the dummy model of Table 4), while having no obvious impact on murders. The LM model flips these predictions by showing strong estimates of increased murder (in the spline model) and no evidence of increased violent or property crime. We can almost perfectly restore the DAW Table 4 findings, however, by simply following the typical pattern of crime regressions by limiting the inclusion of 36 highly collinear demographic variables and including measures for police and incarceration. These results appear in Panel B of Table 6, and this modified LM dummy variable model suggests that RTC laws increase crime. This finding is similar but somewhat stronger than the DAW dummy variable model estimate of higher violent and property crime.

In summary, the LM model that had originally been employed using data through 1992 to argue that RTC laws *reduce* crime, no longer shows any statistically significant evidence of crime reduction. Indeed, using more complete data, the LM spline model (Panel A of Table 6) suggests that RTC laws *increase* the murder rate and count by about 6 or 7 percent after 10 years, which are the only statistically significant results in Panel A—no other crime category is affected. Those who are skeptical of these results because the LM specification is plagued by omitted variable bias, flawed pseudo-arrest rates, too many highly collinear demographic variables, and other problems,

[11] 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 DAW variables reduces the multicollinearity for the RTC dummy to a tolerable level (with VIFs always below the desirable threshold of 5). Indeed, the degree of multicollinearity for the individual demographics of the black-male categories are astonishingly high with 36 demographic controls—in the neighborhood of 14,000! This analysis makes us wary of estimates of the impact of RTC laws that employ the Lott-Mustard set of 36 demographic controls (as does the MM model).

Table 6: Panel Data Estimates of the Impact of RTC Laws: State and Year Fixed Effects, Using Actual and Modified LM Regressors, 1977-2014

Panel A: LM Regressors Including 36 Demographic Variables

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	-4.55 (3.44)	1.030 (0.032)	-1.36 (3.15)	-0.33 (1.71)
Spline Model	0.65^{**} (0.33)	1.006^{**} (0.003)	0.41 (0.47)	0.28 (0.28)

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

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	3.60 (5.67)	1.058 (0.054)	10.06^{**} (4.54)	8.09^{**} (3.63)
Spline Model	0.30 (0.43)	1.003 (0.004)	0.50 (0.57)	0.50 (0.34)

Estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Panel A, 36 demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, only 6 demographic variables are included and controls are added for incarceration and police. For both Panels, other controls include the previous year's violent or property crime arrest rate (depending on the crime category of the dependent variable), state population, population density, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real retirement payments per person over 65. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

might prefer the estimates in Panel B, which simply limit the LM demographic variables from 36 to 6, and add the incarceration and police controls. These changes once again restore the Table 4 DAW dummy variable model result that RTC laws *increase* both violent and property crime.

4. The MM Panel Data Model

Table 3 reveals that the Moody and Marvell (MM) model improves on the LM model in that it includes the key incarceration variable, but MM also omit the critical police measure found in the DAW specification. The MM model also contains the problematic pseudo-arrest rates and over-saturated and highly collinear demographic variables that LM employ.[12] Panel A of Table 7 estimates the MM model for the period 1979-2014.[13] While MM's use of a potentially problematic lagged dependent variable control risks purging some of the effect of the RTC law, again we see evidence that RTC laws *increase* violent crime. The only other statistically significant estimate is for the murder rate in the spline model, which suggests that the murder rate would be roughly 4 percent higher ten years after RTC adoption. This finding is roughly similar to the Table 6, Panel A finding of increased murder in the LM model. Panel B of Table 7 mimics our previous critique of the LM model by including a measure of police and using more appropriate demographic controls. These modifications once again revive a dummy variable model estimate of increased violent crime.

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

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

[12]While our Table 6 MM panel data specification follows Moody and Marvell (2008) in including lagged values of the dependent variable as a regressor, no analogous variable is explicitly included below in our synthetic control analysis featuring the Moody-Marvell predictor variables. Since all lagged values of the dependent variable are already included as predictors in the synthetic controls analysis, including the lagged dependent variable would be redundant.

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

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

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

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	−1.81 (1.85)	1.022 (0.027)	0.69 (0.77)	0.48 (0.69)
Spline Model	0.38** (0.16)	1.003 (0.002)	0.17** (0.08)	0.10 (0.07)

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

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	1.22 (1.75)	1.031 (0.035)	1.50*** (0.53)	0.52 (0.53)
Spline Model	0.24 (0.17)	1.001 (0.003)	0.14 (0.09)	0.05 (0.05)

OLS estimations include year and state fixed effects and are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. Incidence Rate Ratios (IRR) estimated using Negative Binomial Regression, where state population is included as a control variable, are presented in Column 2. The null hypothesis is that the IRR equals 1. In Panel A, 36 demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, only 6 demographic variables are included and a control is added for police. For both panels, other controls include the lagged dependent variable, the previous year's violent or property crime arrest rate (depending on the crime category of the dependent variable), state population, the lagged incarceration rate, the poverty rate, the unemployment rate, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real per capita retirement payments. * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms.

6) almost perfectly replicates the increased violent and property crime estimates of DAW and BC, while the MM model (Table 7) continues to show a statistically significant increase in the violent crime rate.

The strongest result to emerge from the seven panels across the four sets of panel data specifications in Tables 4-7 is that 6 of these 7 panels show statistically significant evidence that RTC laws increase violent crime. The only exception (LM Panel A) shows statistically significant evidence of *increases* in murder. In other words, all 7 panels support the conclusion that RTC laws *increase* overall violent crime and/or murder. Across the 56 estimated effects in the seven panels, not one showed any evidence of a *decrease* in crime at the .05 level of significance.

6. The Zimmerman Model and Our 4 Panel Data Models Estimated for the Post-Crack Period

Our previous discussion has focused on panel data estimates of the impact of RTC laws on crime over the full period from the late 1970s through 2014. Zimmerman (2014) examines the impact of various crime prevention measures on crime using a state panel data set from 1999-2010. He finds that RTC laws *increased* murder by 15.5 percent for the eight states that adopted RTC laws over the period he analyzed. The advantage of using this data period to explore the impact of RTC laws is that it largely avoids the problem of omitted variable bias owing to the crack phenomenon, since the crack effect had subsided by 1999. The disadvantage is that one can only gain estimates based on the eight states that adopted RTC laws over that twelve-year spell.[14] Zimmerman describes his finding as follows: "The shall-issue coefficient takes a positive sign in all regressions save for the rape model and is statistically significant in the murder, robbery, assault, burglary, and larceny models. These latter findings may imply that the passage of shall-issue laws increases the propensity for crime, as some recent research (e.g., Aneja, Donohue, & Zhang, 2012) has suggested" (71).[15]

In Table 8, we show the results of all four basic models that we discussed above—DAW, BC, LM, and MM—when run over the period 2000-2014.[16] The DAW model mimics the Zimmerman finding of a large jump in the murder rate. The BC model weakly supports the increase in murder, and more strongly shows an 8 percent increase in the overall violent crime rate. The results for this

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

[15]Aneja, Donohue and Zhang (2011) also ran the ADZ model over the same 1999-2010 period that Zimmerman employs, which generated an estimate that murder rates rose about 1.5 percentage points each year that a RTC law was in effect.

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

shortened period using the LM and MM models are never statistically significant at the .05 level.

Table 8: Panel Data Estimates of the Impact of RTC Laws Using DAW, BC, LM, and MM specifications, 2000 - 2014.

Panel A: Panel Data Estimates Suggesting that RTC Laws increase Murder: State and Year Fixed Effects, DAW Regressors, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	5.49 (3.58)	1.02 (0.04)	4.98 (3.51)	−1.48 (2.27)
Spline Model	1.04* (0.57)	1.01** (0.01)	0.52 (1.11)	0.40 (0.43)

Panel B: Panel Data Estimates Suggesting that RTC Laws increase Violent Crime: State and Year Fixed Effects, Brennan Center Regressors, 2000-2014

	Murder Rate	Murder Count	Violent Crime Rate	Property Crime Rate
	(1)	(2)	(3)	(4)
Dummy Variable Model	7.14* (4.08)	1.03 (0.04)	8.00** (3.58)	−1.85 (2.47)
Spline Model	0.89 (0.66)	1.01* (0.01)	0.59 (1.30)	0.36 (0.46)

Panel C: Panel Data Estimates With 36 Collinear Demographic Variables Show No Effect of RTC Laws: State and Year Fixed Effects, LM Regressors, 2000-2014

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

Panel D: Panel Data Estimates With 36 Collinear Demographic Variables Show No Effect of RTC Laws: State and Year Fixed Effects, MM Regressors without Crack Cocaine, 2000-2014

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

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

7. Summary of Panel Data Analysis

The uncertainty about the impact of RTC laws on crime expressed in the NRC Report was based on an analysis of data only through 2000. The preceding evaluation of an array of different specifications over the full data period from the late 1970s through 2014 has eliminated any suggestion of benign effects on crime from the adoption of RTC laws and consistently shown evidence that RTC laws increase murder and/or overall violent crime. Three of five models estimated on post-crack-era data (Zimmerman, DAW, and BC) provide further support for this conclusion.

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

Despite the substantial panel data evidence in the post-NRC literature that supports the finding of the pernicious influence of RTC laws on crime, the NRC suggestion that new techniques should be employed to estimate the impact of these laws is fitting. The important paper by Strnad (2007) used a Bayesian approach to argue that none of the published models used in the RTC evaluation literature rated highly in his model selection protocol when applied to data from 1977-1999. 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 2005: 308). But owing to the substantial 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.

II. Estimating the Impact of RTC Laws Using Synthetic Controls

The synthetic controls methodology, which is becoming increasingly prominent in economics and other social sciences, is a promising new statistical approach for addressing the impact of RTC laws.[17] A number of papers have used the synthetic control technique to evaluate various influences on crime. Rudolph et al. (2015) construct a synthetic control for the state of Connecticut yielding evidence that the state's firearm homicide rate (but not its non-firearm homicide rate) fell appreciably after the implementation of a permit-to-purchase handgun law. Munasib and Guettabi (2013) use this methodology to examine the effect of Florida's "Stand Your Ground" law, concluding that this law was associated with an increase in overall gun deaths. Similarly, Cunningham and Shah (2017) study the effect of Rhode Island's unexpected decriminalization of indoor prostitution on the state's rape rate (among other outcome variables); Lofstrom and Raphael (2013) estimate the effect of California's public safety realignment on crime rates; and Pinotti (2013) examines the consequences of an influx of organized crime into two Italian provinces in the late 1970s.

While these papers focus on a single treatment in a single geographic region, we look at 33 RTC adoptions throughout the country. For each adopting (treated) state we will find a weighted average of other states designed to serve as a good counter-factual for the impact of RTC laws, because this "synthetic control" had a similar pattern of crime to the adopting state prior to RTC adoption. By comparing what actually happened for the adopting state post-passage to the crime performance of the synthetic control over the same period, we generate estimates of the causal impact of RTC laws on crime.[18]

A. The Basics of the Synthetic Control Methodology

The synthetic control method attempts to generate representative counterfactual units by comparing a treatment unit (*i.e.*, a state adopting a RTC law) to a set of control units across a set of explanatory variables over a pre-intervention period. The algorithm searches for similarities between the treatment state of interest and the control states during this period and then generates a synthetic counterfactual unit for the treatment state that is a weighted combination of the compo-

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

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

nent control states.[19] 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 believed to influence this variable.[20] As justified in Appendix H, we use every lag of the dependent variable as predictors in the DAW, BC, LM, and MM specifications.[21] Once the synthetic counterfactual is generated and the weights associated with each control unit are assigned, the *synth* program then calculates values for the outcome variable associated with this counterfactual and the root mean squared prediction error (RMSPE) based on differences between the treatment and synthetic control units in the pre-treatment period. The effect of the treatment can then be estimated by comparing the actual values of the dependent variable for the treatment unit to the corresponding values of the synthetic control.

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

To illustrate the procedure outlined above, consider the case of Texas, whose RTC law went into effect on January 1, 1996. The potential control group for each treatment state consists of all nine states with no RTC legislation as of the year 2014, as well as states that pass RTC laws at least ten years after the passage of the treatment state (e.g., in this case, those states passing RTC laws after 2006, such as Nebraska and Kansas, whose RTC laws went into effect at the beginning of 2007). Since we estimate results for up to ten years post-passage,[22] this restriction helps us avoid

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

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

[21] We considered using one lag, three lags, and yearly lags as predictors and we eventually chose to use yearly pre-treatment crime rates since that option minimized the average coefficient of variation of the RMSPE during the validation period. It is worth noting that the estimated treatment effect associated with the passage of a state-level RTC law remains similar for violent crime regardless of whether one, three, or every possible lag is included along with the DAW, BC, LM, and MM predictors (or whether these lags are excluded from the list of predictors entirely).

[22] Our choice of ten years in this context is informed by the tradeoffs associated with using a different time frame. 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

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 rationale behind this methodology is that a close fit in these time series of crime between the treatment state and synthetic control in the pre-passage period generates greater confidence in the accuracy of the constructed counterfactual. Computing the post-treatment difference between the dependent variables of the treatment state and the synthetic control unit provides the synthetic controls estimate of the treatment effect attributable to RTC adoption in that state.

1. Synthetic Controls Estimates of Violent Crime in Four States

Figure 2 shows the synthetic controls graph for violent crime in Texas over the period from 1977 through 2006 (ten years after the adoption of Texas's RTC law). The solid black line shows the actual pattern of violent crime for Texas, and the vertical line indicates when the RTC law went into effect. Implementing the synthetic control protocol identifies three states that generate a good fit for the pattern of crime experienced by Texas in the pre-1996 period. These states are California, which gets a weight of 57.8 percent owing to its similar attributes to Texas, Nebraska with a weight of 8.6 percent, and Wisconsin with a weight of 33.6 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 2 by the dashed line) matches the pre-RTC-passage pattern of violent crime to see whether the methodology is likely to generate a good fit in the ten years of post-passage data. Here the fit looks rather good in mimicking the rises and falls in Texas violent crime from 1977-1995. This pattern increases our confidence that synthetic Texas will provide a good prediction of what would have happened in Texas had it not adopted 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 2, we see that while both Texas and synthetic Texas (the weighted average violent crime performance of the three mentioned states) show declining crime rates in the post-passage decade after 1996, the crime drop is substantially greater in synthetic Texas, which had no RTC law over that period, than in actual Texas, which did. As Figure 2 notes, ten years after

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.

adopting its RTC law, violent crime in Texas was 16.6 percent *higher* than we would have expected had it not adopted a RTC law.[23]

Figure 2 also illustrates perhaps the most important lesson of causal inference: one cannot simply look before and after an event to determine the consequence of the event. Rather, one needs to estimate the difference between what did unfold and the counterfactual of what would have unfolded without the event. The value of the synthetic controls methodology is that it provides a highly transparent estimate of that counterfactual. Thus, when Lott (2010) quotes a Texas District Attorney suggesting that he had reversed his earlier opposition to the state's RTC law in light of the perceived favorable experience with the law, we see why it can be quite easy to draw the inaccurate causal inference that Texas' crime decline was facilitated by its RTC law. The public may perceive the falling crime rate post-1996 (the solid black line) but our analysis suggests that Texas would have experienced a more sizable violent crime decline if it had not passed a RTC law (the dotted line). More specifically, Texas experienced a 19.7 percent decrease in its aggregate violent crime rate in the ten years following its RTC law (between 1996 and 2006), while the state's synthetic control experienced a larger 30.8 percent decline. This counterfactual would not be apparent to residents of the state or to law enforcement officials, but our results suggest that Texas's RTC law imposed a large social cost on the state.

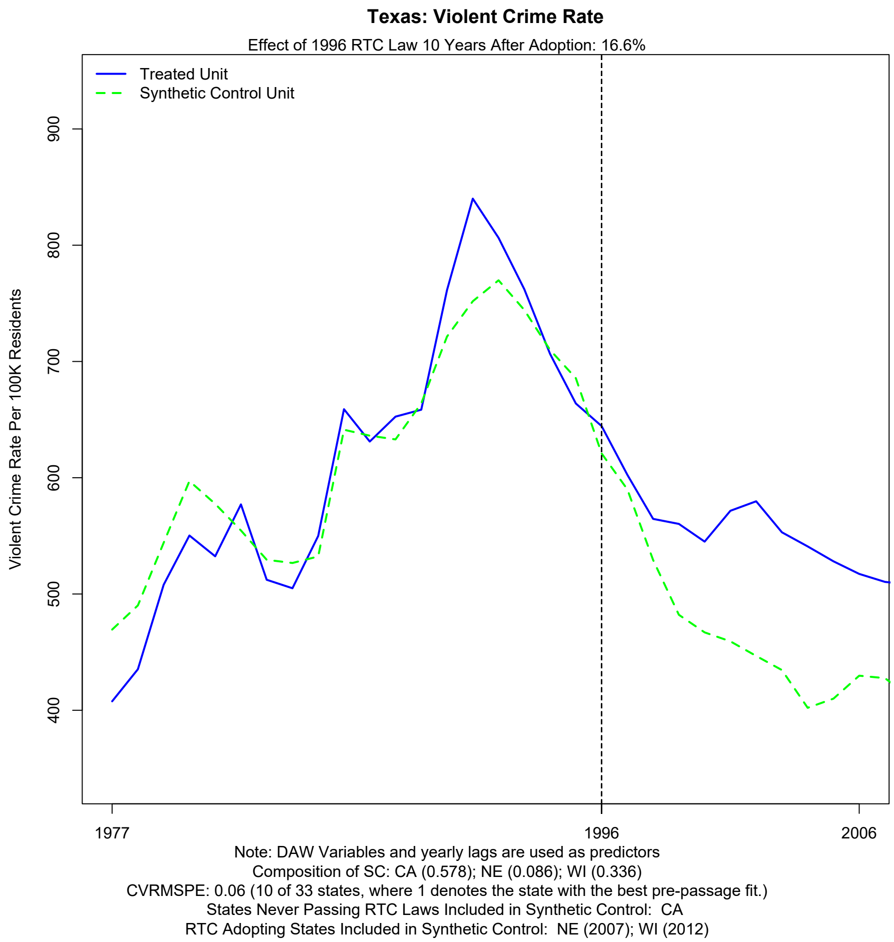
The greater transparency of the synthetic controls approach is one advantage of this methodology over the panel data models that we considered above. Figure 2 makes clear what Texas is being compared to, and we can reflect on whether this match is plausible and whether anything other than RTC laws changed in these three states during the post-passage decade that might compromise the validity of the synthetic 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 16.6 percent in Texas.[24] But the death penalty,

[23]Texas' violent crime rate ten years post-adoption exceeds that of "synthetic Texas" by $\frac{517-430}{430} \times 100 = 20.2\%$. Figure 2 shows the estimated violent crime increase in Texas of 16.6 percent, which comes from subtracting from 20.2 percent, the amount by which Texas' violent crime rate exceeded that of synthetic Texas in 1996 $= \frac{644-621}{621} \times 100 = 3.7\%$. (See footnote 31 for further discussion of this calculation.)

[24]Texas executed 275 convicts during the post-passage decade while California executed 11, Nebraska 2, and Wisconsin executed no one (Death Penalty Information Center 2015). Other key explanatory variables might be the incarceration and police rates. Because Texas had an enormous jump in its incarceration rate from 1992-1996, the growth in the Texas incarceration rate from 1996-2006 was only 7.6 percent, while for "synthetic Texas" the growth rate was 22.0 percent. Individually, the growths for the three synthetic control states were 6.8 percent (CA), 69.0 percent (WI), 26.6 percent (NE). The growth rate in the Texas police employment rate over the decade was -0.6 percent, while for "synthetic Texas" the growth rate was 7.8 percent. Individually, the growths for the three synthetic control states were 9.0 percent (CA), 5.5 percent (WI), 8.2 percent (NE). Using plausible crime elasticities for police and prison suggests that accounting for these two factors could conceivably shrink the estimated impact on violent crime by 30 percent. Even this would suggest that the Texas RTC law increased violent crime in the tenth year by

Figure 2



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.

Figure 3 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 after the RTC law is extended to Philadelphia in late 1995, as illustrated by the second vertical line at 1996.[25] The synthetic controls method estimates

roughly 10 percent. As we noted in Table 2, the overall increases in incarceration and police rates were greater in RTC states than in non-RTC states, which would tend to make our estimates of the increased violence in RTC states conservative.

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

that RTC laws in Pennsylvania increased its violent crime rate by 26.5 percent.

Figure 3

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.077); HI (0.106); NE (0.043); NJ (0.17); OH (0.256); WI (0.348)
CVRMSPE: 0.018 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; NJ
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004); WI (2012)

Figures 4 and 5 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 18.3 percent and in Mississippi of 34.2 percent.[26]

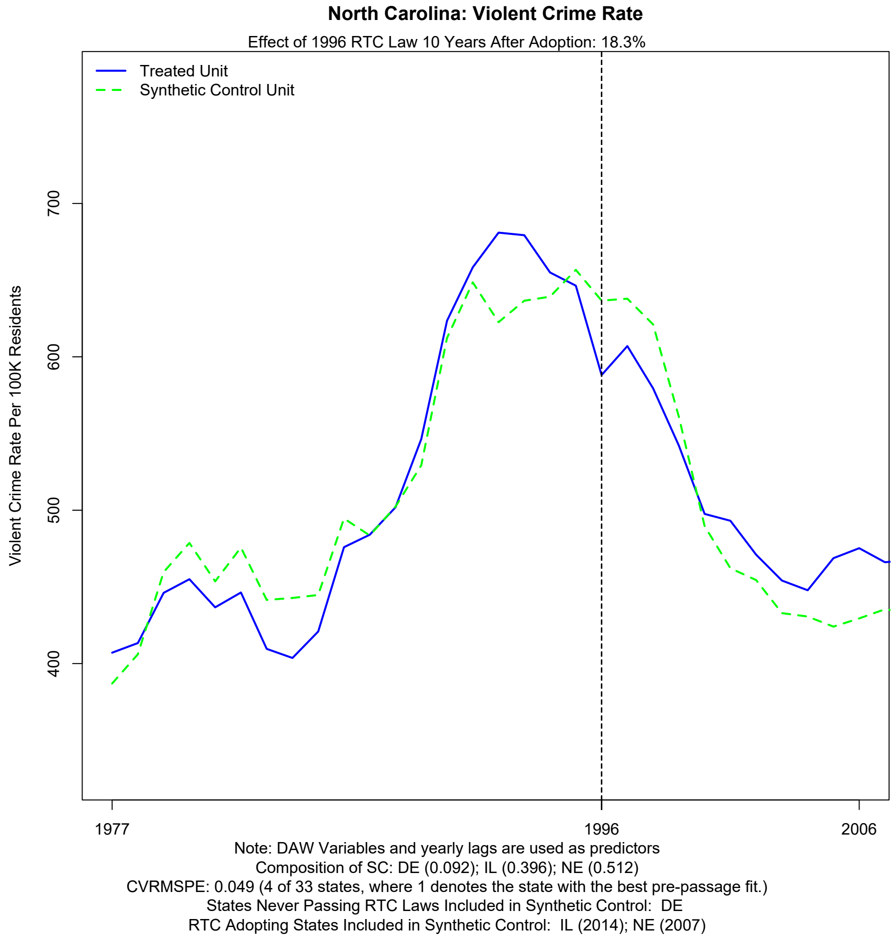
2. State-Specific Estimates Across all RTC States

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

Appendix B.I.

[26] In Appendix F we include all 33 graphs showing the path of violent crime for the treatment states and the synthetic controls, along with information about the composition of these synthetic controls, the dates of RTC adoption (if any) for states included in these synthetic controls, and the estimated treatment effect (expressed in terms of the percent change in a particular crime rate) ten years after adoption (or 7 years after adoption for 2 states that adopt RTC laws in 2007, since our data ends in 2014).

Figure 4

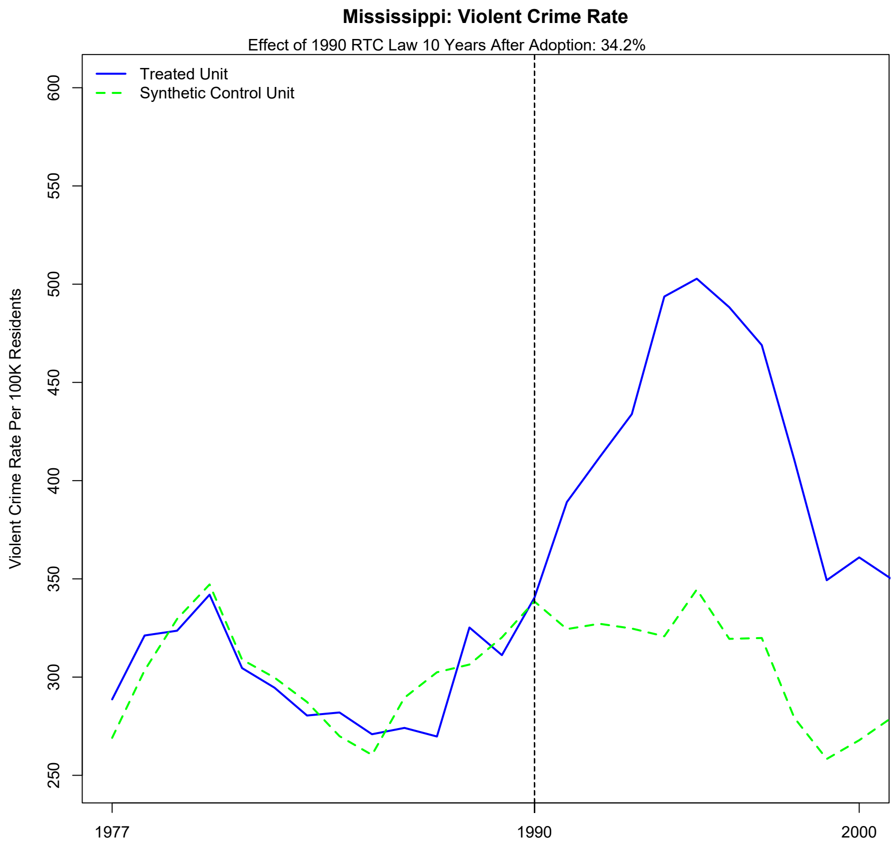


counterfactual. One of the advantages of our task is that we have a large number of states adopting RTC laws so that the over-estimates and under-estimates will tend to wash out in our mean treatment estimates. Figure 6 shows the synthetic control estimates on violent crime for all 31 states for which we have ten years of post-passage data. For 23 of the 31 states adopting RTC laws, the increase in violent crime is noteworthy. While three states were estimated to have crime reductions greater than the -1.6 percent estimate of South Dakota, if one averages across all 31 states, the (population-weighted) mean treatment effect after ten years is a 15.1 percent *increase* in violent crime. If one instead uses an (unweighted) median measure of central tendency, RTC laws are seen to *increase* crime by 14.1 percent.

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

Figure 5



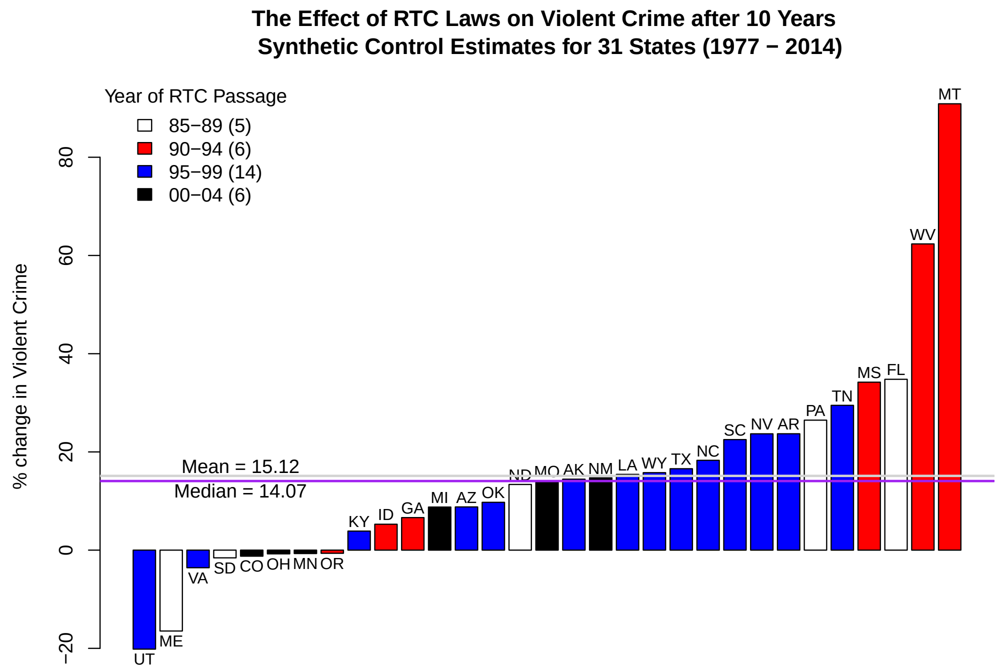
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.721); IA (0.016); NE (0.011); OH (0.252)
CVRMSPE: 0.05 (6 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011); NE (2007); OH (2004)

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 we 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 eight states showing improvements in crime following RTC adoption as indicated in Figure 6.

An examination of Figures 7 and 8 showing the synthetic controls estimates for these two states provides dramatic visual confirmation that the methodology has failed to provide a good pre-passage fit between the crime performance of the treatment states and a suitable synthetic control.

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 similar pattern can be seen for Maine, which again

Figure 6



undermines confidence in the synthetic controls estimates for these two states. The difficulty in generating good pre-passage matches for South Dakota and Maine stems from their unusually low violent crime in the pre-passage period.

Figure 9 reproduces Figure 6 while leaving out the four states for which the quality of pre-passage fit is clearly lower than in the remaining 27 states.[27] This knocks out ND, SD, MT, and WV, leaving a slightly lower estimated mean but the same median effect of RTC laws. As Figure 9 shows, the (weighted) mean increase in crime across the listed 27 RTC-adopting states is 11.3 percent while the (unweighted) median increase is 14.1 percent. Increases in violent crime of this magnitude are troubling. Consensus estimates of the elasticity of crime with respect to incarceration hover around .15 today, which suggests that to offset the increase in crime caused by RTC adoption, the median RTC state would need to approximately double its prison population.

[27]In particular, for these four states, the pre-passage CVRMSPE—that is, the RMSPE transformed into a coefficient of variation by dividing by average pre-passage crime—was significantly greater than for the other 27. See Footnote 33 for further discussion of this statistic.

Figure 7

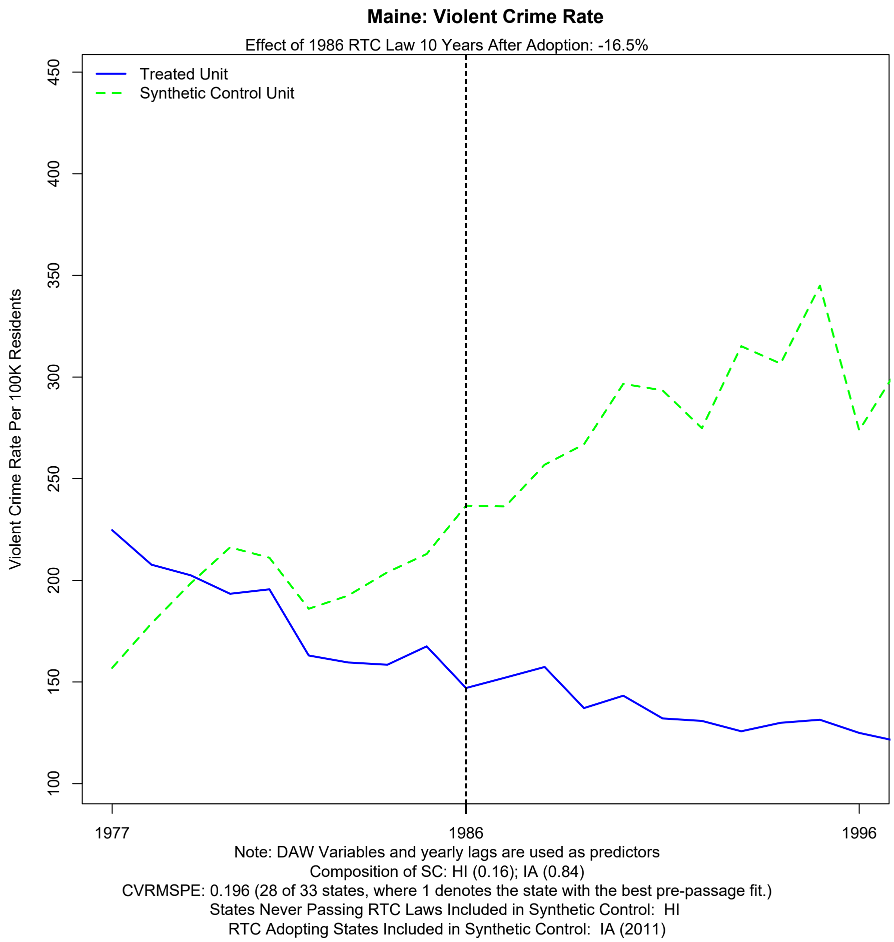
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)

III. Aggregation Analysis Using Synthetic Controls

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[28] We estimate the percentage difference in violent crime between each treatment (RTC-adopting) state and the corresponding synthetic control in both the year of the treatment and in the ten years following it (we obviously use data from fewer post-treatment years for the two treatment

[28]The closest paper to the present study is Arindrajit Dube and Ben Zipperer (2013), who introduce their own methodology for aggregating multiple events into a single estimated treatment effect and calculating its significance. Their study centers on the effect of increases in the minimum wage on employment outcomes, and, as we do, the authors estimate the percentage difference between the treatment and the synthetic control in the post-treatment period. While some papers analyze multiple treatments by aggregating the areas affected by these treatments into a single unit, this approach is not well-equipped to deal with a case such as RTC law adoption where treatments affect the majority of panel units and more than two decades separate the dates of the first and last treatment under consideration, as highlighted in Figure 6.

Figure 8



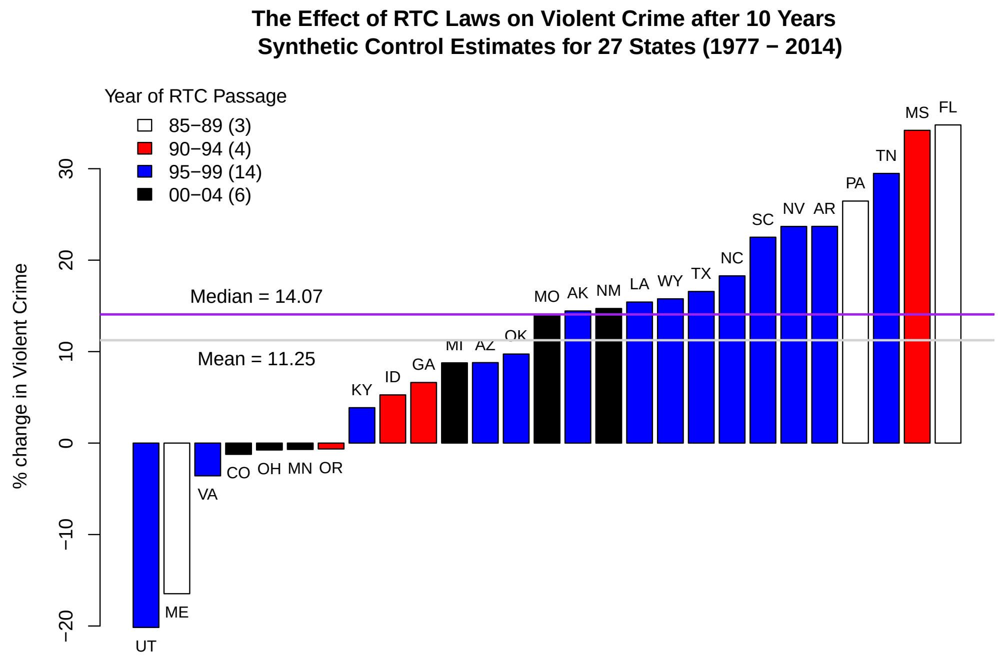
states[29] that had RTC laws that took effect less than ten years before the end of our sample).[30]

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

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

[30]Dube and Zipperer (2013) average the estimated 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.

Figure 9



would subtract 2 from 10 to obtain an estimated effect of RTC laws in year 10 of 8 percent.[31] We then aggregate all the state-specific estimates of the the impact of RTC laws on violent crime and test whether they are significantly different from zero.[32]

As we saw in Figures 2-5 and 7-8, the validity of using the post-treatment difference between crime rates in the treatment state (the particular state adopting a RTC law that we are analyzing) and its corresponding synthetic control as a measure of the effect of the treatment depends on the

[31]Both approaches should generate similar estimates on average, and in fact our mean estimated effects are more conservative with our preferred approach. The intuitive rationale for our choice of outcome variable was that pre-treatment differences between the treatment state and its synthetic control at the time of RTC adoption likely reflected imperfections in the process of generating a synthetic control and should not contribute to our estimated treatment effect if possible. In other words, if the treatment state had a crime rate that was 5 percent greater than that of the synthetic control in both the pre-treatment and post-treatment period, it would arguably be misleading to ignore the pre-treatment difference and declare that the treatment increased crime rates by 5 percent.

For violent crime, the mean (median) percentage difference between the treatment state and the synthetic control in the year of the treatment was 4.9 percent (1.3 percent), with 18 treatment states showing greater crime rates than their synthetic controls and 15 treatment states showing less crime than their synthetic control in that year. As a result, our estimate of the amount by which RTC laws increased violent crime was *lower* than would have been the case under the alternative appraoch.

[32]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.

strength of the match between these two time series in the pre-treatment period. To generate an estimate of pre-treatment fit that takes into account differences in pre-treatment crime levels, we estimate the coefficient of variation for the root mean squared prediction error (RMSPE), which is the ratio of the synthetic control's pre-treatment RMSPE to the pre-treatment average level of the outcome variable for the treatment state.[33] After generating the aggregate synthetic controls estimates of the crime impact of RTC laws described in the paragraph above using the full sample, we consider two subsamples of treatment states: states whose coefficients of variation are less than two times the average coefficient of variation for all thirty-three treatments and states whose coefficients of variation are less than this average. We then re-run our synthetic controls protocol using each of these two subsamples to examine whether restricting our estimation of the average treatment effect to states for which a relatively "better" synthetic control could be identified would meaningfully change our 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 DAW specification. Table 9 shows our results on the full sample examining violent crime.[34] Our estimates suggest that states that passed RTC laws experienced more deleterious changes in violent criminal activity than their synthetic controls in the ten years after adoption. On average, treatment states had aggregate violent crime rates that were around 7 percent higher than their synthetic controls five years after passage and almost 15 percent higher ten years after passage. Table 9 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.

Table 10 repeats the Table 9 analysis while dropping the four states with a CV of the RMSPE that is above twice the average of the sample. Table 11 uses a more stringent measure of assessing

[33]While the RMSPE is often 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 among the 33 treatment states that we consider. For example, the pre-treatment RMPSE associated with our synthetic control analysis using the DAW predictor variables and aggregate violent crime as the outcome variable is similar for Colorado (36.1) and Maine (36.4), but the pre-treatment levels of Colorado's aggregate violent crime rate are far greater than Maine's. To be more specific, Colorado's average violent crime rate prior to the implementation of its RTC law (from 1977 through 2002) was 467 violent crimes per 100,000 residents, while the corresponding figure for Maine was 186 violent crimes per 100,000 residents. For this reason, we have greater confidence in our estimates that in the tenth year, Colorado's RTC law had decreased violent crime by 1.23 percent than we do in an estimate that Maine's law had decreased violent crime by 16.5 percent, since the percentage imprecision in our synthetic pre-treatment match for Maine is so much greater than for Colorado.

[34]We discuss the synthetic controls estimates for murder and property crime in section G below. In all cases, the tenth-year effect is always positive—suggesting that RTC laws increase crime—but not statistically significant. The point estimates across the four models suggest RTC laws increase the murder rate by 5 to 6 percent, and the property crime rate by 1-3 percent after ten years.

Table 9: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.139	2.748**	3.676*	4.912**	6.989***	7.719**	10.802***	13.224***	14.445***	14.730***
	(1.122)	(1.331)	(1.862)	(1.986)	(2.524)	(3.057)	(2.739)	(3.752)	(3.577)	(2.957)
N	33	33	33	33	33	33	33	31	31	31
Pseudo P-Value	0.954	0.262	0.202	0.158	0.088	0.082	0.046	0.036	0.024	0.032
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.158	0.192	0.186	0.192	0.188	0.196	0.212	0.234	0.232	0.234
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.086	0.100	0.108	0.106	0.128	0.138	0.138	0.144	0.138	0.142
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.022	0.030	0.024	0.038	0.038	0.044	0.062	0.058	0.070	0.058

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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
The synthetic controls used to generate the placebo estimates in the table above were generated using the regression methodology described in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

how well the synthetic control fits the pre-passage data by dropping the six states with an above average CV for the RMSPE. It is striking how all three tables show roughly identical conclusions: RTC laws are consistently shown to increase violent crime starting three years after passage.

Table 10: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.087	2.602*	3.737*	4.910**	7.232***	7.541**	10.655***	12.845***	13.441***	13.853***
	(1.148)	(1.354)	(1.908)	(2.038)	(2.574)	(3.151)	(2.823)	(3.874)	(3.670)	(2.999)
N	29	29	29	29	29	29	29	27	27	27
Pseudo P-Value	0.960	0.292	0.198	0.160	0.082	0.088	0.050	0.034	0.042	0.034
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.154	0.194	0.176	0.172	0.176	0.200	0.204	0.240	0.214	0.218
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.084	0.100	0.096	0.104	0.118	0.132	0.144	0.138	0.134	0.144
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.022	0.026	0.022	0.034	0.026	0.034	0.048	0.044	0.068	0.050

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

B. The Placebo Analysis

Our ability to make valid inferences from our synthetic control estimates depends on the accuracy of our standard error estimation. To test the robustness of the standard errors that we present under the first row of Tables 9-11 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).[35] For this analysis, we generate 500 sets

[35] Ando (2015) examines the impact of constructing nuclear plants on local real per capita taxable income in Japan by generating a synthetic control for every coastal municipality that installed a nuclear plant. While the average treatment effect measured in our paper differs from the one used by Ando, we follow Ando in repeatedly estimating

Table 11: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, $<$ 1x Average Coefficient of Variation of the RMSPE, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.059	2.702*	3.942*	5.192**	7.514***	7.801**	10.924***	13.102***	13.733***	14.088***
	(1.172)	(1.379)	(1.934)	(2.059)	(2.602)	(3.191)	(2.848)	(3.876)	(3.667)	(3.002)
N	27	27	27	27	27	27	27	26	26	26
Pseudo P-Value	0.982	0.318	0.194	0.158	0.090	0.084	0.044	0.042	0.030	0.030
Proportion of Corresponding Placebo Estimates Significant at .10 Level	0.166	0.188	0.172	0.184	0.172	0.186	0.196	0.222	0.204	0.208
Proportion of Corresponding Placebo Estimates Significant at .05 Level	0.080	0.118	0.116	0.114	0.108	0.116	0.134	0.124	0.130	0.134
Proportion of Corresponding Placebo Estimates Significant at .01 Level	0.016	0.032	0.026	0.032	0.040	0.038	0.046	0.048	0.060	0.056

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of randomly generated RTC dates that are designed to resemble the distribution of actual RTC passage dates that we use in our analysis.[36] For each of the 500 sets of randomly generated RTC dates, we then use the synthetic control methodology and the DAW predictors to estimate thirty-three synthetic controls for each state whose randomly generated adoption year is between 1981 and 2010. We use this data to estimate the percentage difference between each placebo treatment and its corresponding synthetic control during both the year of the treatment and each of the ten post-treatment years (for which we have data) that follow it. We then test whether the estimated treatment effect for each post-treatment year is statistically significant (using the methodology described in footnotes 23 and 31). We also repeat our estimation of the average treatment effect associated with each of the ten post-treatment years after excluding states whose coefficient of variation is either one or two times the average observed for all (placebo) treatment states, leaving us with 30 coefficients and p-values corresponding to each of the 500 sets of randomly generated placebo treatments that we consider.

At the bottom of Table 9, 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 esti-

average placebo effects by randomly selecting different areas to serve as placebo treatments. (The sheer number of treatments that we are considering in this analysis prevents us from limiting our placebo treatment analysis to states that never adopt RTC laws, but this simply means that our placebo estimates will likely be biased *against* finding a qualitatively significant effect of RTC laws on crime, since some of our placebo treatments will be capturing the effect of the passage of RTC laws on crime rates.) The actual average treatment effect can then be compared to the distribution of average placebo treatment effects. Heersink and Peterson (2014) also perform a similar randomization procedure to estimate the significance of their estimated average treatment effect. 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.

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

mation, although (for the reason noted in footnote 35) it is likely that these placebo estimates are capturing some of the effect of RTC laws. Table 9 shows that the placebo results appear to be significant at the .01 level 2.2 percent of the time for our first year after passage to 5.8 percent in the tenth year. In other words, the standard errors we report at the top of Table 9 are potentially underestimated, as our placebo averages are statistically significant more often than would be expected by chance.[37]

As another check on the statistical significance of our results, we compare each of the ten coefficient estimates in Table 9 with the distribution of the 500 average placebo treatment effects that use the same crime rate, post-treatment year, and sample as the given estimate. To assist in this comparison process, we report a pseudo p-value which is equal to the proportion of our placebo treatment effects whose absolute value is greater than the absolute value of the given estimated treatment effect. This pseudo p-value provides another intuitive measure of whether our estimated average treatment effects are qualitatively large compared to the distribution of placebo effects. Our confidence that the treatment effect that we are measuring for RTC laws is real increases if our estimated treatment effect is significantly greater than the vast majority of our estimated average placebo treatment effects.[38] Examining our pseudo p-values in Tables 9-11, we see that our violent crime results are always statistically significant in comparison to the distribution of placebo coefficients at the .05 level after seven years or more have passed since the treatment date.

IV. Conclusion

The extensive array of panel data and synthetic controls estimates of the impact of RTC laws that we present uniformly undermine the "More Guns, Less Crime" hypothesis. There is not even the slightest hint in the data that RTC laws reduce violent crime. Indeed, the weight of the evidence

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

[38] Because of the computational demands required to perform this analysis using the maximum likelihood estimation technique—the *nested* option that we employed in our main analysis (mentioned in footnote 20)—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. This change should bias our estimates *against* finding a significant effect of RTC 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. Indeed, when performing an earlier version of this placebo analysis, 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.

from the panel data estimates as well as the synthetic controls analysis best supports the view that the adoption of RTC laws substantially raises overall violent crime in the ten years after adoption.

In our initial panel data analysis, our preferred DAW specification as well as the BC specification predicted that RTC laws have led to statistically significant and substantial increases in violent crime. When the LM and MM models were appropriately adjusted, they generated the same findings, but even without adjustment, these models showed RTC laws increased murder significantly. We then supplemented our panel data results using our synthetic control methodology, again using the DAW, BC, LM, and MM specifications. Now the results were uniform: for all four specifications, states that passed RTC laws experienced 13-15 percent *higher* aggregate violent crime rates than their synthetic controls after 10 years (results that were significant at either the .05 or .01 level after five years). The synthetic controls estimates for the impact of RTC laws on murder and property crime were also uniformly positive after ten years (but not statistically significant). If one adjusts the synthetic controls estimates for the increased rates of police and incarceration that follow RTC adoption, the RTC-induced increases in murder become large and statistically significant.

The synthetic controls effects that we measure represent meaningful increases in violent crime rates following the adoption of RTC laws, and this conclusion remained unchanged after restricting the set of states considered based on model fit and after considering a large number of robustness checks. While our placebo analysis suggests that the standard errors associated with some of these estimates may have been biased downward, the size of our average estimated treatment effect in comparison to the distribution of placebo effects indicates that the deleterious effects associated with RTC laws that we estimate for aggregate violent crime are qualitatively large compared to those that we would expect to observe by chance.

The consistency across different specifications and methodologies of the finding that RTC elevates violent crime enables far stronger conclusions than were possible back when the NRC Report was limited to analyzing data only through 2000 with the single tool of panel data evaluation. Nonetheless, estimation using observational data always rests on numerous assumptions, so one must always be alert to potential shortcomings. For example, if states that were expected to experience future increases in crime were more likely to adopt RTC laws, then we might exaggerate the detrimental effect of RTC laws on crime. Given the very limited ability of politicians, pundits, and even academic experts to correctly predict crime trends over this period, though, this problem of endogeneity is unlikely to mar our results. Panel data analysis can be susceptible to problems of omitted variable biase, but the synthetic controls approach was designed to better address that concern.

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 crime spikes of the late 1980s and early 1990s, most states experienced large and important crime decreases, including those adopting RTC laws. However, our analysis suggests that had states avoided adoption of RTC laws, they would have experienced greater drops in violent crime. Indeed, as Figure 1 illustrated, while RTC states have now fallen below their violent crime rates of 1977 by about 9 or 10 percent, the states that did not adopt RTC laws enjoyed violent crime drops from the late 1970s of over 40 percent.

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 net increases in violent crime; that is, the statistical evidence shows us that whatever beneficial effects RTC laws have in reducing violence, they are outweighed by greater harmful effects. 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. A number of high profile crimes by RTC permit holders would seem to follow this pattern: George Zimmerman, the popcorn killer at a Florida movie theater who was angry at a father texting a babysitter, and the angry gas station killer (shooting a black teen for playing loud rap music) are all individuals who would likely never have killed anyone had they not had an RTC permit (Trotta 2012; Luscombe 2014; Robles 2014). 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 (and hence overall violent crime).

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 clearly understate criminality by permit holders. For example, convictions for violent crime are far smaller than acts of violent crime, so many permit holders would never face official withdrawal of their permits even if they committed a violent criminal act that would warrant such termination. Moreover, official withdrawals will be unnecessary when the offending permit holder is killed. 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 would ever be recorded in a case like this (Stuart 2013).

The second critical 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 assaulted 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 messages of the gun culture can promote fear and anger, which are emotions that can invite more hostile confrontations leading to more violence. This attitude may be 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 somewhat of a 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, therefore, be more likely to be aggressive once armed and hence more likely to stimulate violence by others 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 soon recovered, but the guns were gone (Donohue 2003). In July 2015 in San Francisco, the theft of a gun from a car in San Francisco led to a killing of a tourist on a city pier that almost certainly would not have occurred if the lawful gun owner had not left it in the car (Ho 2015). Just a few months later, a gun stolen from an unlocked car was used in two separate killings in San Francisco in October 2015 (Ho and Williams 2015). According to the National Crime Victimization Survey, in 2013 there were over 660,000 auto thefts from households. The more guns being carried in vehicles by permit holders, the more criminals will be walking around with the guns taken from the car of some permit holder. 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). Lost, forgotten, and misplaced guns are another dangerous by-product of RTC laws, as the growing TSA seizures in carry-on luggage attest.[39]

Third, as more citizens carry guns, more criminals will find it increasingly 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. Having a "designated permit holder" along to take possession of the guns when confronted by police seems to be an attractive benefit for criminal elements acting in concert (Fernandez, Stack, and Blinder 2015; Luthern 2015). 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

[39] See Williams and Waltrip (2004).

resources used to process gun permits could instead be more efficiently used to directly fight crime. All of these factors are a tax on police, and therefore one would expect law enforcement to be less effective on the margin, thereby contributing to crime. Indeed, this may in part explain why RTC states tend to increase the size of their police forces (relative to non-adopting states) after RTC laws are passed.

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 increases in violent crime that these laws stimulate.[40]

[40] It should be noted that, even with the enormous stock of guns in the U.S., the vast majority of the time that someone is threatened with violent crime no gun will be wielded defensively. A five-year study of such violent victimizations in the United States found that victims failed to defend or to threaten the criminal with a gun 99.2 percent of the time — this in a country with 300 million guns in civilian hands (Planty and Truman 2013).

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

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

Appendix

Appendix A: RTC Adoption Dates

An RTC adoption year of 0 indicates that a state did not adopt a right-to-carry (RTC) law between 1977 and the early months of 2014. If the fraction of year in effect is less than 0.5, the RTC date used in the synthetic control analysis is the following year.

RTC dates before the year 1977 may not be exact, since differences between these dates would neither affect our regression results nor our synthetic control tables. 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 it appears given widespread public commentary on this point that Vermont 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 RTC adoption dates for Alabama and Connecticut.

Table A1: RTC Adoption Dates

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

Appendix B: Complete Regression Output

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

Panel A: Dummy Variable Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law	9.48*** (2.96)	10.98*** (3.65)	−1.38 (3.16)	0.69 (0.77)
Lagged Incarceration Rate	0.04* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		24.07** (9.56)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		3.18 (13.59)		
Lagged Arrest Rate for Violent Crimes			−0.16** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				87.12*** (1.45)
Real Per Capita Personal Income	0.00 (0.00)		0.00* (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance			0.00 (0.01)	0.01** (0.01)
Real Per Capita Income Maintenance			0.04 (0.03)	0.02** (0.01)
Real Per Capita Retirement Payments and Other (Lott version)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version)				−0.00* (0.00)
Nominal Per Capita Income		−0.00 (0.00)		
Unemployment Rate	0.16 (0.77)	−1.00 (0.67)		−0.37 (0.23)
Poverty Rate	−0.29 (0.49)			−0.12 (0.09)
Lagged Number of Executions		0.11 (0.16)		
Beer	65.41*** (17.59)	71.97*** (18.23)		
Population			0.00 (0.00)	−0.00 (0.00)
Percent of the population living in MSAs	0.95*** (0.29)			
Population Density			−0.01 (0.02)	
Observations	1823	1874	1896	1781

Panel B: Spline Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law	0.05 (0.64)	0.19 (0.66)	0.41 (0.47)	0.17** (0.08)
Trend for Changer States	0.93* (0.49)	0.96* (0.53)	0.12 (0.39)	−0.07 (0.08)
Lagged Incarceration Rate	0.03* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		21.25*** (8.12)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		2.25 (13.56)		
Lagged Arrest Rate for Violent Crimes			−0.17** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				86.65*** (1.46)
Real Per Capita Personal Income	0.00 (0.00)		0.00** (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance			−0.00 (0.02)	0.01 (0.01)
Real Per Capita Income Maintenance			0.03 (0.03)	0.01 (0.01)
Real Per Capita Retirement Payments and Other (Lott version)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version)				−0.00** (0.00)
Nominal Per Capita Income		0.00 (0.00)		
Unemployment Rate	0.70 (0.87)	−0.29 (0.81)		−0.28 (0.23)
Poverty Rate	−0.41 (0.50)			−0.15 (0.10)
Lagged Number of Executions		0.16 (0.17)		
Beer	65.93*** (16.33)	67.42*** (15.43)		
Population			0.00 (0.00)	−0.00* (0.00)
Percent of the population living in MSAs	0.78*** (0.28)			
Population Density			0.00 (0.02)	
Observations	1823	1874	1896	1781

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

Appendix C: Synthetic Control Estimates of the Impact of RTC Laws on Murder and Property Crime for 4 Different Models

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.667	-1.406	-1.203	-1.544	-4.951	-5.915	-5.171	1.971	-0.130	4.613
	(2.006)	(4.009)	(4.415)	(4.534)	(5.136)	(4.478)	(5.309)	(4.739)	(4.280)	(3.692)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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 A4: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.416	1.149	2.195	0.720	0.444	1.345	0.578	1.261	1.013	0.979
	(0.993)	(1.219)	(2.549)	(2.693)	(2.748)	(2.593)	(2.538)	(2.341)	(2.367)	(2.300)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.314	-1.411	-0.466	-1.588	-3.870	-4.236	-4.904	2.813	1.205	4.574
	(2.044)	(3.874)	(4.367)	(4.412)	(5.038)	(4.535)	(4.850)	(4.503)	(3.698)	(3.169)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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 A6: The Impact of RTC Laws on the Property Crime Rate, BC covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.575	0.794	1.907	0.543	0.355	1.434	0.728	1.412	1.177	1.020
	(1.036)	(1.254)	(2.558)	(2.701)	(2.755)	(2.562)	(2.549)	(2.375)	(2.372)	(2.308)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.107	-4.355	-2.770	-3.382	-5.262	-3.972	-4.913	2.619	1.633	4.542
	(1.713)	(4.166)	(4.501)	(4.661)	(5.313)	(5.155)	(5.484)	(5.512)	(4.968)	(4.141)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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 A8: The Impact of RTC Laws on the Property Crime Rate, LM covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.208	1.262	2.211	1.039	0.072	1.099	1.525	2.991	2.568	3.420
	(1.005)	(1.163)	(2.616)	(2.688)	(2.719)	(2.575)	(2.387)	(2.374)	(2.715)	(3.050)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.785	-2.359	-1.162	-1.538	-3.728	-3.175	-2.909	3.085	2.792	5.876
	(1.774)	(3.987)	(4.179)	(4.266)	(4.559)	(4.428)	(4.431)	(4.440)	(4.086)	(4.071)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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 A10: The Impact of RTC Laws on the Property Crime Rate, MM covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.334	1.231	2.369	1.543	1.581	2.676	1.863	2.692	2.775	3.062
	(0.941)	(1.157)	(2.526)	(2.637)	(2.596)	(2.381)	(2.440)	(2.334)	(2.312)	(2.342)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the 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 D: Data Methodologies

I. Data Issues

The state-level data set used in this paper updated through 2014 earlier data sets used in Aneja, Donohue, and Zhang (2014) and Aneja, Donohue, and Zhang (2011). We further update this data set to incorporate changes to the various primary sources that have occurred since first released, and to include the additional predictor variables that are featured in the DAW and BC models. All variables are collected for the years 1977-2014 unless otherwise noted.[41]

Annual state-level crime rates are taken from the FBI's Uniform Crime Reporting program.[42] Four state-level income variables (personal income, income maintenance payments, retirement payments, and unemployment insurance payments) are taken from the BEA's Regional Economic Accounts. The personal income, income maintenance, and unemployment insurance payment variables are estimated in real per capita terms (defined using the CPI). The the LM and MM specifications use alternative versions of the retirement variable that are described in footnote 41. State-level population is generated using the Census Bureau's intercensal population estimates, while the proportional size of LM's 36 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 reported in the previous decennial census. State-level unemployment rate data is taken from the Bureau of Labor Statistics, while the poverty rate is taken from two Census series (the 1979 state-level poverty rate is derived from the Decennial Census and the 1980-2014 poverty rates are generated using the Current Population Survey). A measure of incarceration (incarcerated individuals per 100,000

[41] 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 our estimates more closely approximated Lott's per capita retirement payment variable when we (a) used the total population as the denominator rather than population over 65 and (b) used as our numerator a measurement that includes retirement payments along with some other forms of government assistance. As a result, we use a modified retirement variable that incorporates these changes in the MM specification. Our retirement variable in the LM specification, in contrast, uses the population over 65 as a denominator and uses a tighter definition of retirement payments.

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

state residents) is calculated from tables published by the Bureau of Justice Statistics counting the number of prisoners under the jurisdiction of different state penal systems. Our primary estimates for crime-specific state-level arrest rates are generated by adding together estimates of arrests by age, sex, and race submitted by different police agencies. We then divided this variable by the estimated number of incidents occurring in the same state (according to the UCR) in the relevant crime category.[43] We also use the index of crack cocaine usage constructed by Fryer et al. (2013) for our analysis, which is only available between the years 1980 and 2000, and therefore we drop this variable from the MM model when we estimate this model on data through 2014. Since we already include controls that incorporate information on the racial composition of individual states in our analysis, we use the unadjusted version of the crack index instead of the version that is adjusted to account for differences in state racial demographics.

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 the synthetic controls analysis owing to its status as a clear outlier whose characteristics are less likely to be meaningfully predictive for other geographic areas. Abadie, Diamond, and Hainmueller (2010) emphasize that researchers may want to "[restrict] the comparison group to units that are similar to the exposed units [in terms of the predictors which are included in the model]" (496). Given that the District of Columbia had the highest per capita personal income, murder rate, unemployment rate, poverty rate, and population density at various points in our sample, Abadie's admonition would seem to support omitting the District as one of our potential control units.[44] We should note that even if we include DC in the synthetic controls estimates, it still shows RTC laws increase violent crime by 13.2 percent in the tenth year (as opposed to the 14.7 percent figure shown in Table 9).

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, Donohue, and Zhang (2014). To construct this variable, we take the most recent agency-level data provided by the FBI and use this information to estimate the number of full-time police employees present in each state per 100,000

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

[44] 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.

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

II. The Dates of Adoption of RTC Laws

We use the same effective RTC dates used in Aneja, Donohue, and Zhang (2014) with one small modification. Owing to the fact that we are using annual panel data, the mechanics of the synthetic control methodology require us to specify a specific year for each state's RTC date. To take advantage of the information we have collected on the exact dates when RTC laws went into effect in each state, each state's effective year of passage is defined as the first year in which a RTC law was in effect for the majority of that year.[45] This causes some of the values of our RTC variable to shift by one year (for instance, Wisconsin's RTC date shifts from 2011 to 2012, since the state's RTC law took effect on November 1, 2011).[46]

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 adopted their RTC law. We supplemented our analysis of the statutory history of RTC laws in different states with

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

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

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, Donohue, and Zhang (2014). It is important to note that the coding of these dates may not reflect administrative or logistical delays that may have prevented the full implementation of a RTC law after authorities were legally denied any discretion in rejecting the issuing of RTC permits. Ideally, a researcher would be able to control for the actual level of RTC permits in existence each year for each state. Although this data would be preferable to a mere indicator variable for the presence of an RTC law, such comprehensive information unfortunately is not available.

Appendix E: Replicating Our Analysis

One issue which is rarely addressed 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 20), 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."[47]

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

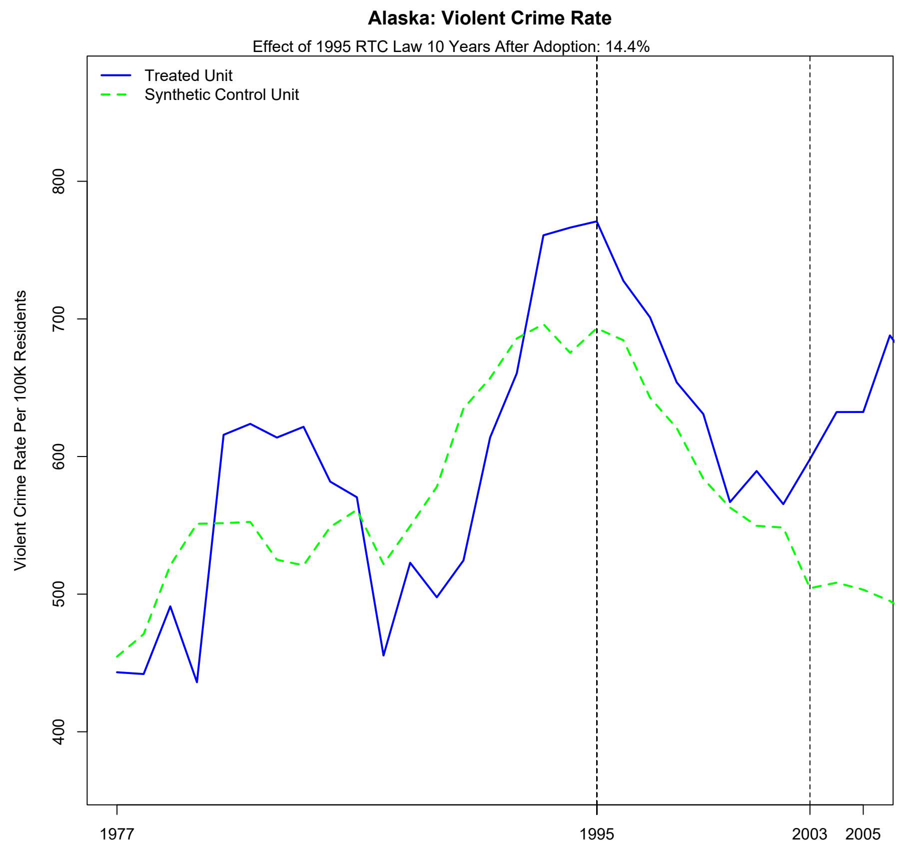
[47] This memo can be found at the following link: http://www.webcitation.org/6YeLV03SN.

more powerful computers with greater amounts of memory. For this reason, to replicate our results with the greatest amount of precision, we would recommend that other researchers run our code on the same machines that we ran our own analysis: a 24-core UNIX machine with 96GB of RAM 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 synthetic controls estimates for violent crime results are essentially 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.

Appendix F: Synthetic Control Graphs Estimating Impact of RTC Laws On Violent Crime Using the DAW Model[48]

Figures A1-A33

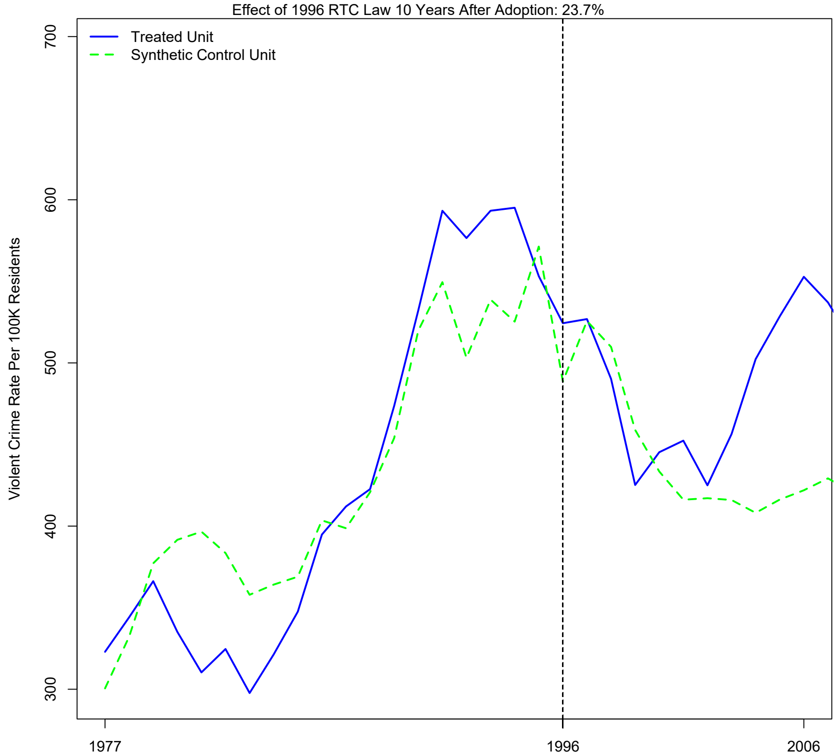


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: MD (0.519); NE (0.481)
CVRMSPE: 0.119 (21 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: MD
RTC Adopting States Included in Synthetic Control: NE (2007)

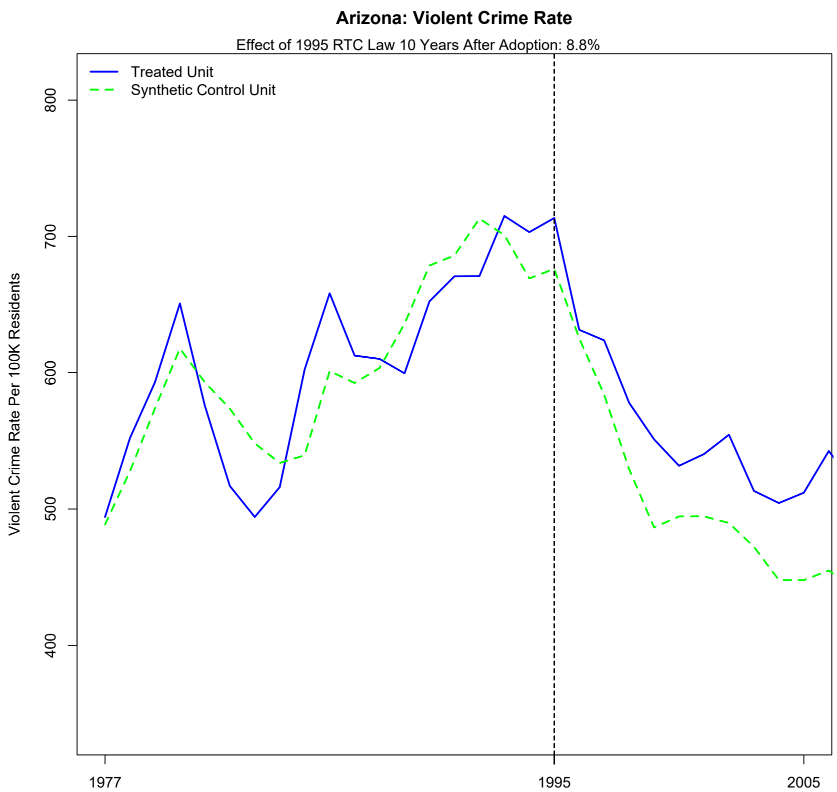
48 Recall that each state's effective year of passage is defined as the first year in which a RTC law was in effect for the majority of that year.

Arkansas: Violent Crime Rate

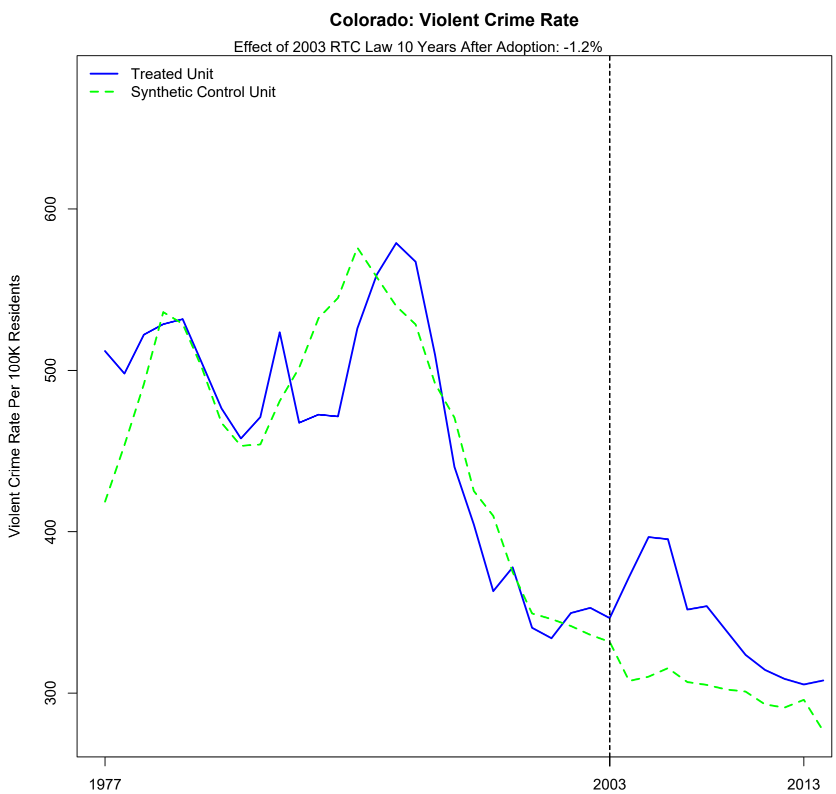
Effect of 1996 RTC Law 10 Years After Adoption: 23.7%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.186); IL (0.231); IA (0.584)
CVRMSPE: 0.103 (20 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IA (2011); IL (2014)

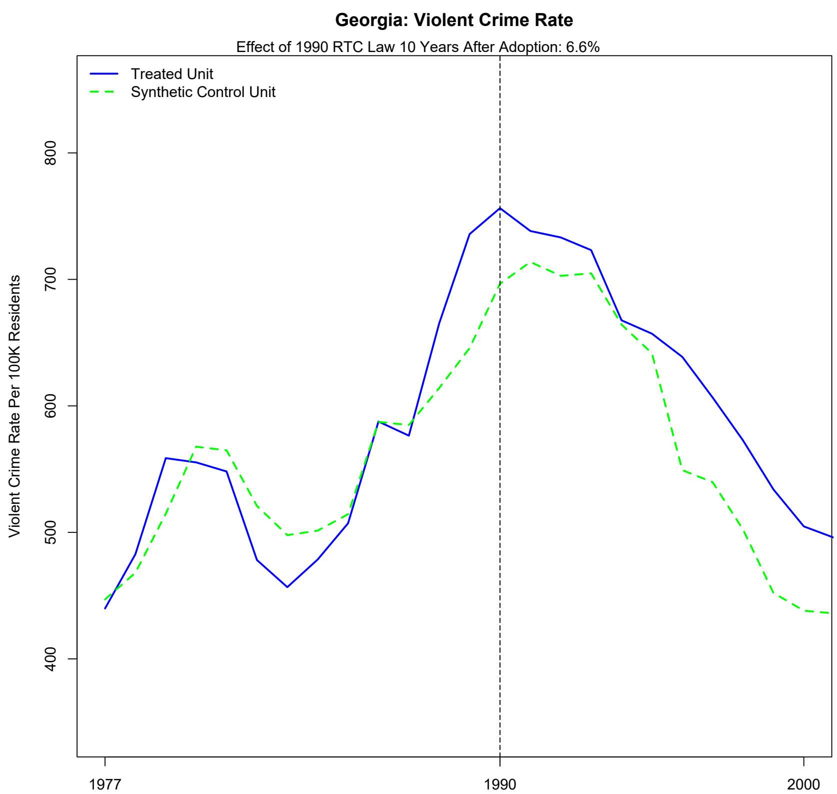


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.327); HI (0.409); MD (0.229); NE (0.035)
CVRMSPE: 0.058 (9 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI ; MD
RTC Adopting States Included in Synthetic Control: NE (2007)

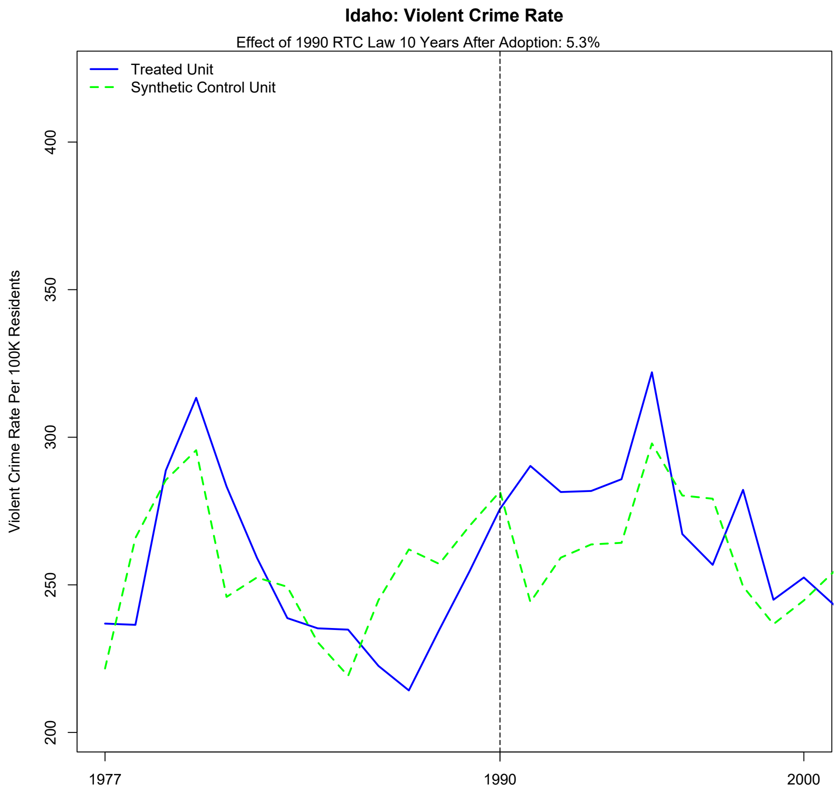


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.499); NY (0.293); RI (0.208)
CVRMSPE: 0.077 (16 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; NY ; RI
RTC Adopting States Included in Synthetic Control:

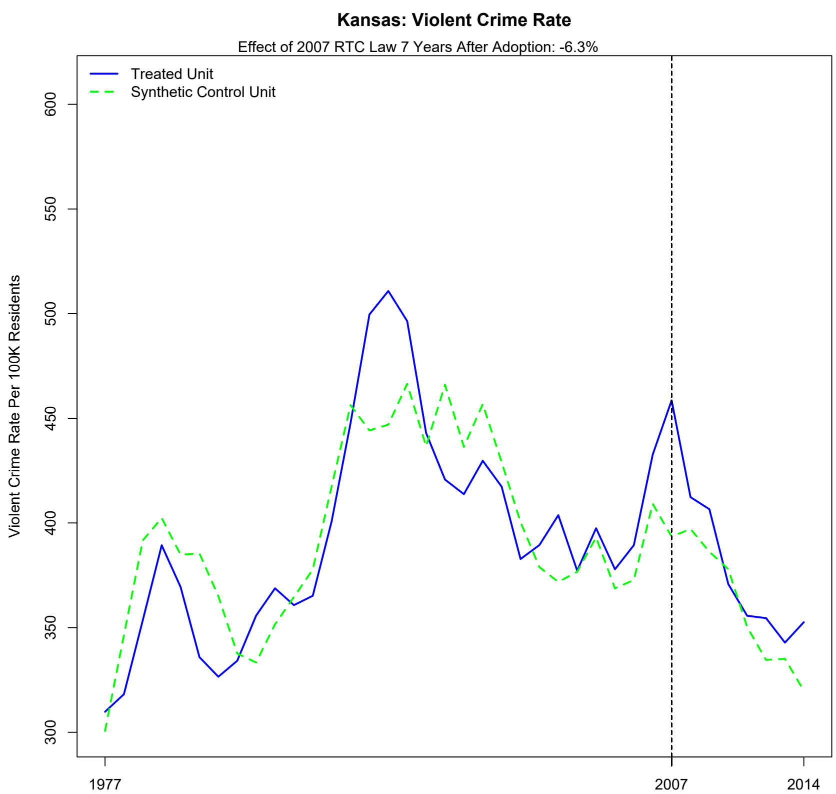
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.223); MI (0.11); NY (0.667)
CVRMSPE: 0.056 (8 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: MI (2001)



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.301); IA (0.452); MO (0.097); NY (0.15)
CVRMSPE: 0.068 (12 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: IA (2011); MO (2004)



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.961); IA (0.039)
CVRMSPE: 0.091 (19 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011)



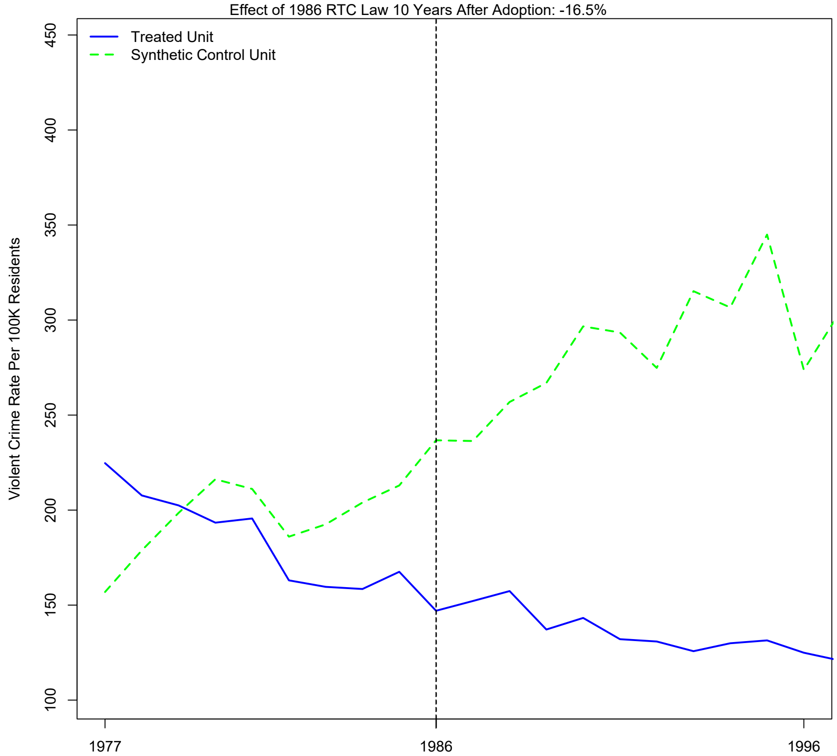
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.183); HI (0.583); MA (0.235)
CVRMSPE: 0.069 (13 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MA
RTC Adopting States Included in Synthetic Control:

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: IL (0.198); WI (0.802)
CVRMSPE: 0.119 (22 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IL (2014); WI (2012)

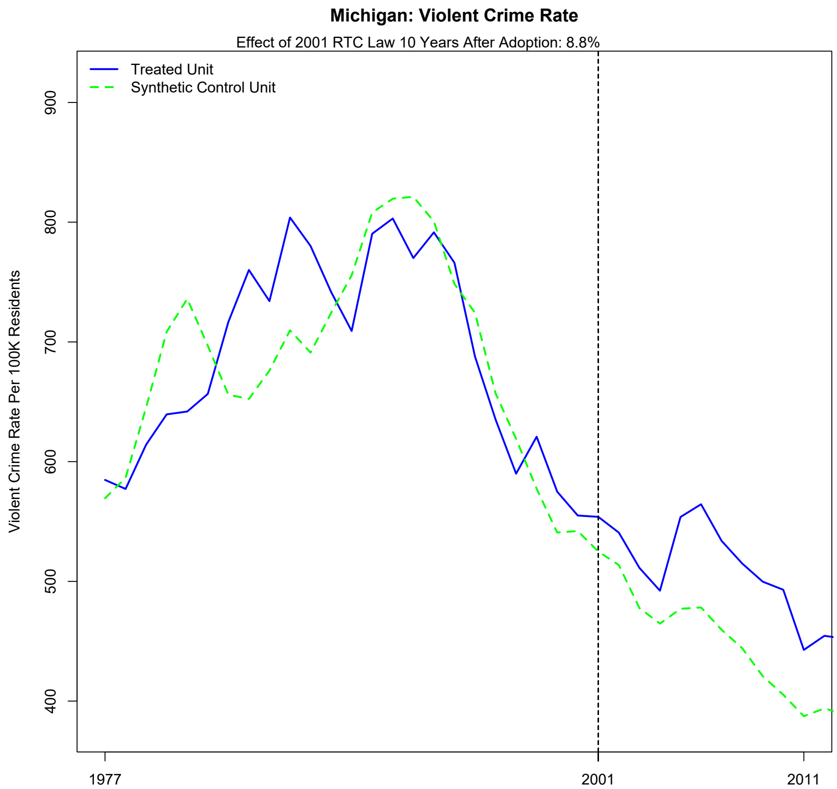
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.038); DE (0.207); IL (0.756)
CVRMSPE: 0.08 (17 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control: IL (2014)

Maine: Violent Crime Rate

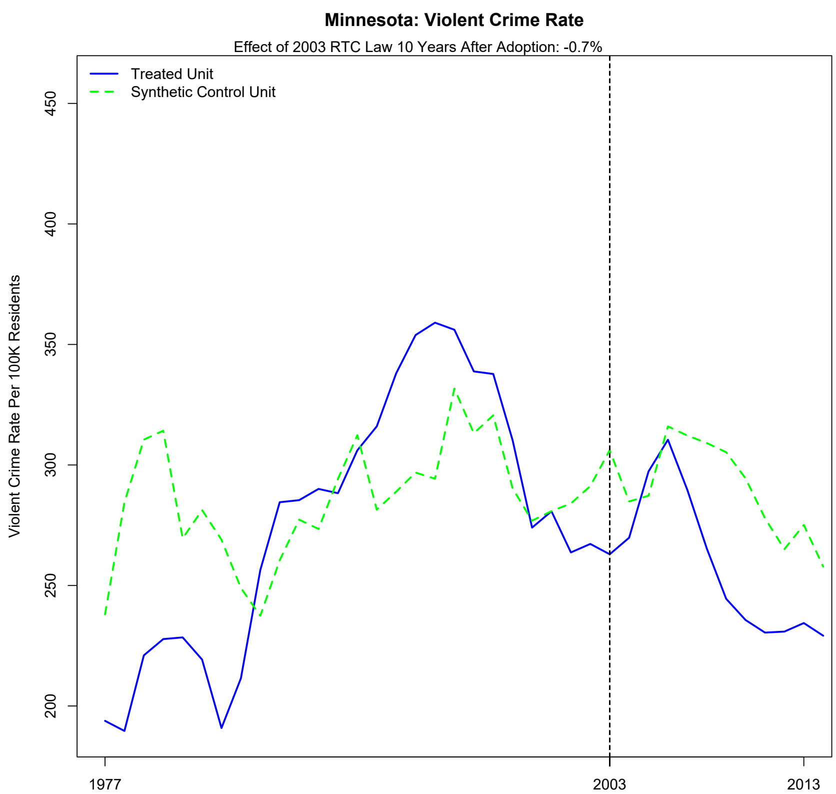
Effect of 1986 RTC Law 10 Years After Adoption: -16.5%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.16); IA (0.84)
CVRMSPE: 0.196 (28 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011)



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: MD (0.362); NY (0.334); WI (0.303)
CVRMSPE: 0.075 (15 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: MD ; NY
RTC Adopting States Included in Synthetic Control: WI (2012)

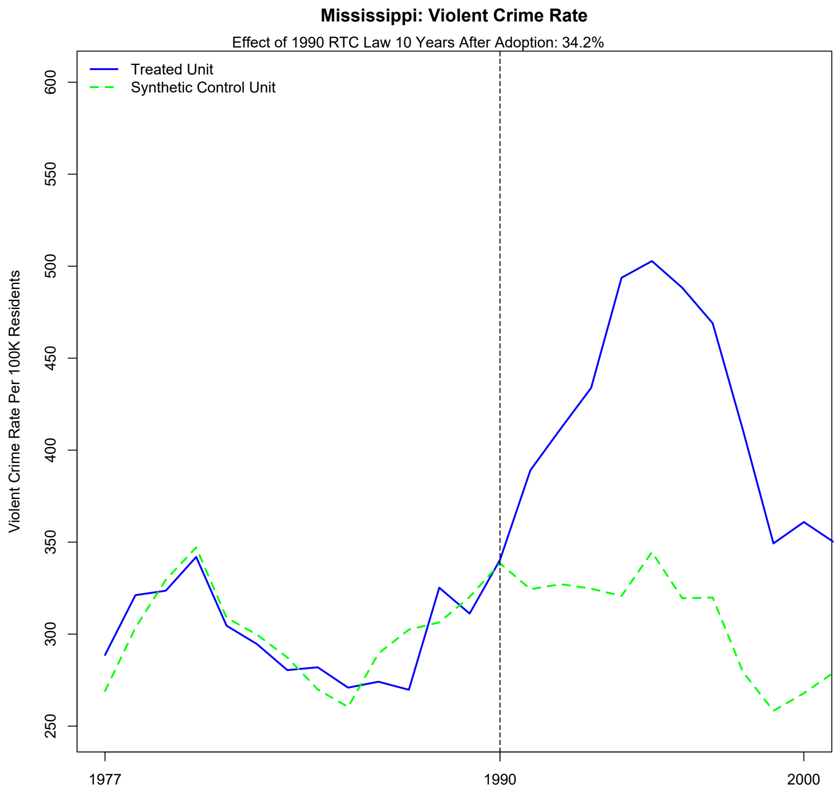


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.084); HI (0.916)
CVRMSPE: 0.166 (27 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic Control:

Missouri: Violent Crime Rate

Effect of 2004 RTC Law 10 Years After Adoption: 14.1%

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.401); DE (0.265); HI (0.333)
CVRMSPE: 0.062 (11 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE ; HI
RTC Adopting States Included in Synthetic Control:

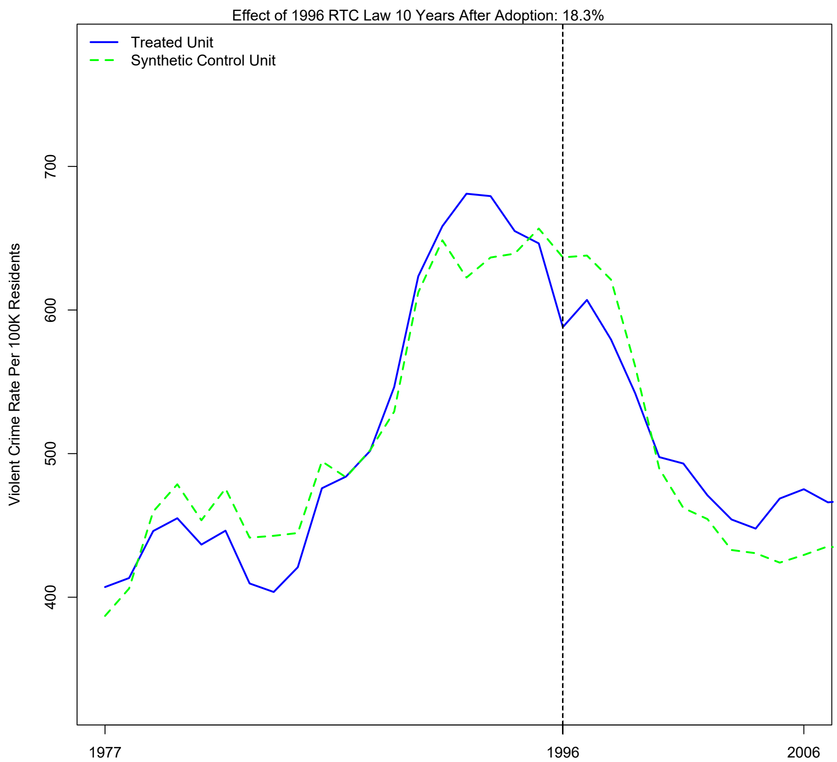


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.721); IA (0.016); NE (0.011); OH (0.252)
CVRMSPE: 0.05 (6 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011); NE (2007); OH (2004)

Montana: Violent Crime Rate

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.121); WI (0.879)
CVRMSPE: 0.409 (31 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: WI (2012)

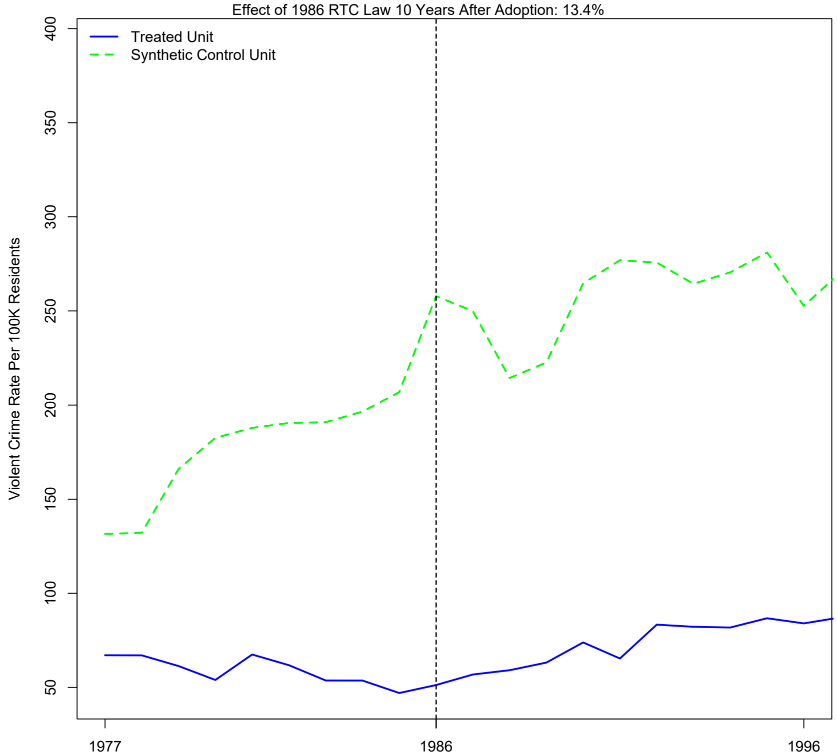
North Carolina: Violent Crime Rate



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.092); IL (0.396); NE (0.512)
CVRMSPE: 0.049 (4 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014); NE (2007)

North Dakota: Violent Crime Rate

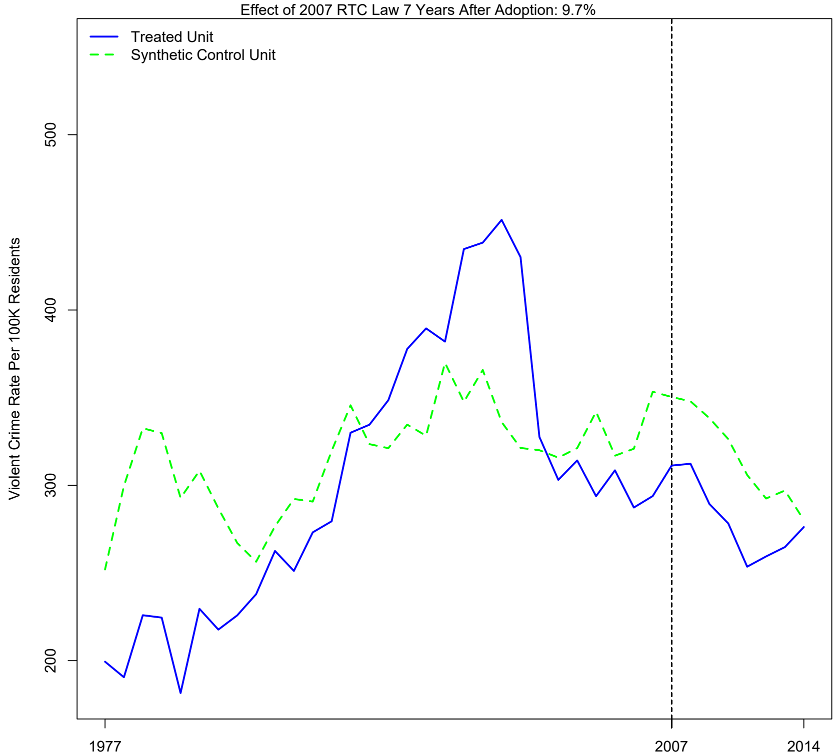
Effect of 1986 RTC Law 10 Years After Adoption: 13.4%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: WI (1)
CVRMSPE: 2.043 (33 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: WI (2012)

Nebraska: Violent Crime Rate

Effect of 2007 RTC Law 7 Years After Adoption: 9.7%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.173); HI (0.827)
CVRMSPE: 0.208 (29 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic Control:

New Mexico: Violent Crime Rate

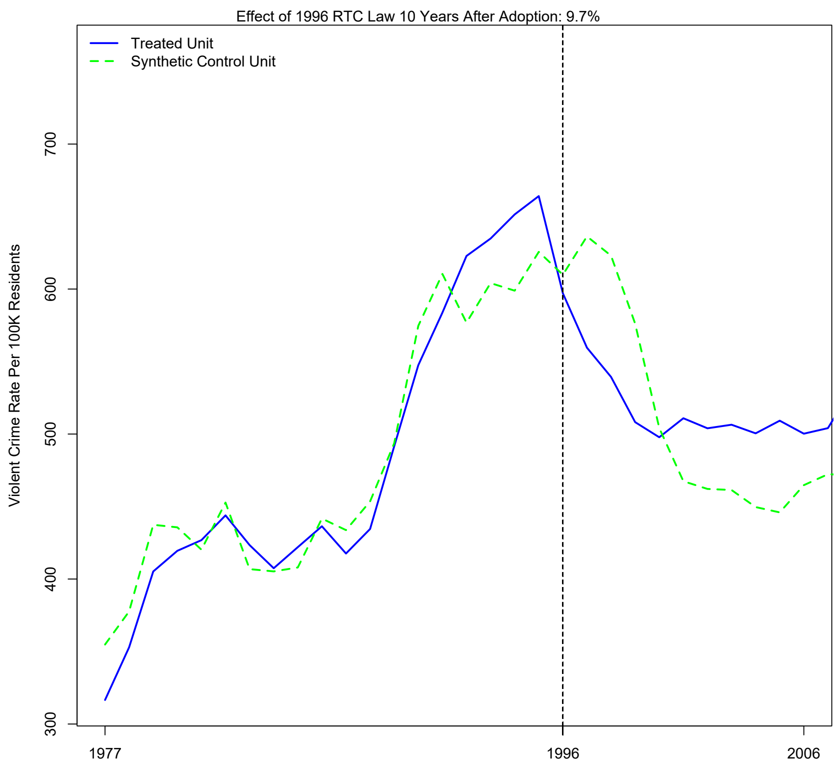
Effect of 2004 RTC Law 10 Years After Adoption: 14.7%

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.485); DE (0.515)
CVRMSPE: 0.122 (23 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control:

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.167); MD (0.833)
CVRMSPE: 0.152 (25 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; MD
RTC Adopting States Included in Synthetic Control:

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.195); HI (0.207); RI (0.598)
CVRMSPE: 0.04 (2 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI ; RI
RTC Adopting States Included in Synthetic Control:

Oklahoma: Violent Crime Rate

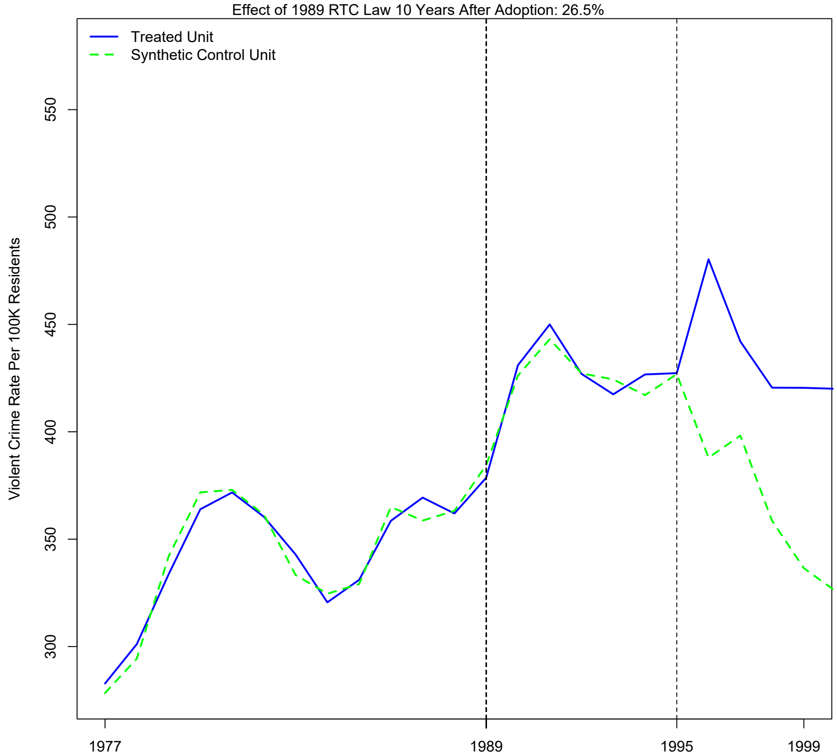


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.27); IL (0.246); NE (0.484)
CVRMSPE: 0.055 (7 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014); NE (2007)

Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.014); CO (0.42); HI (0.062); MI (0.344); MN (0.161)
CVRMSPE: 0.049 (5 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI
RTC Adopting States Included in Synthetic Control: CO (2003); MI (2001); MN (2003)

Pennsylvania: Violent Crime Rate

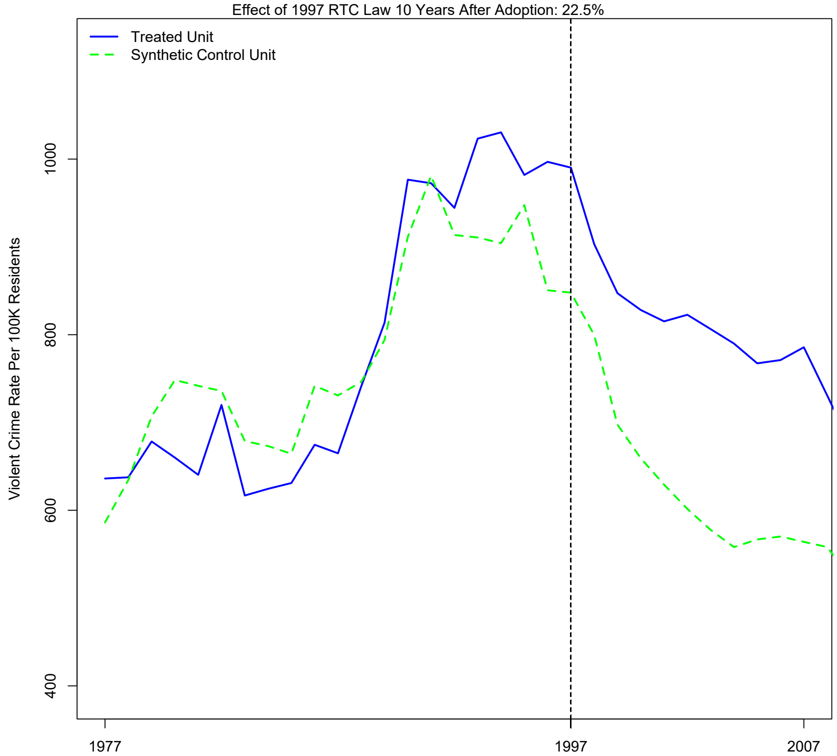
Effect of 1989 RTC Law 10 Years After Adoption: 26.5%



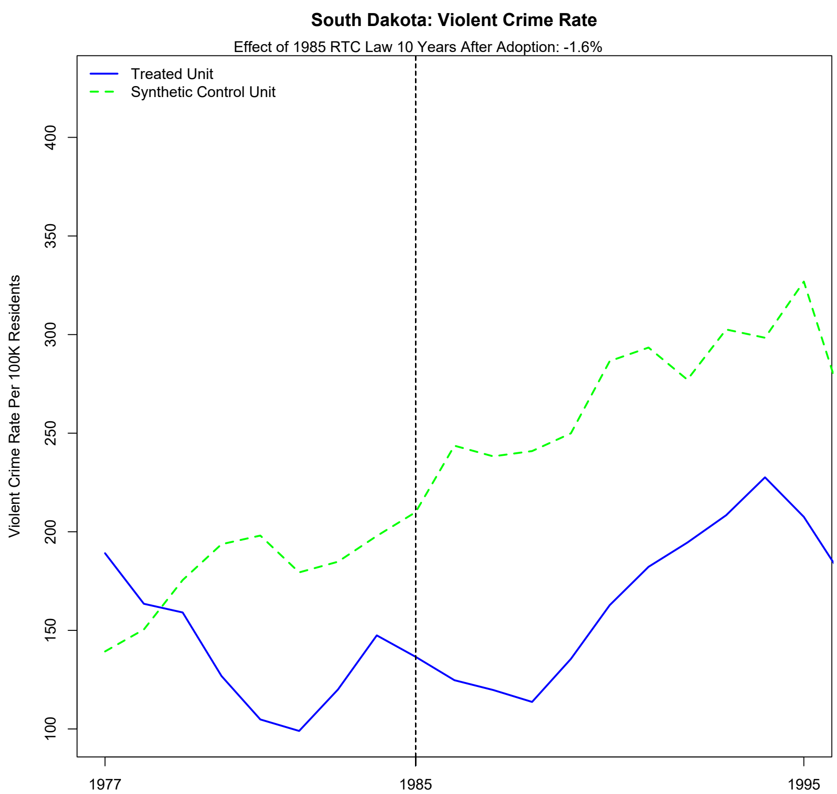
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.077); HI (0.106); NE (0.043); NJ (0.17); OH (0.256); WI (0.348)
CVRMSPE: 0.018 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; NJ
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004); WI (2012)

South Carolina: Violent Crime Rate

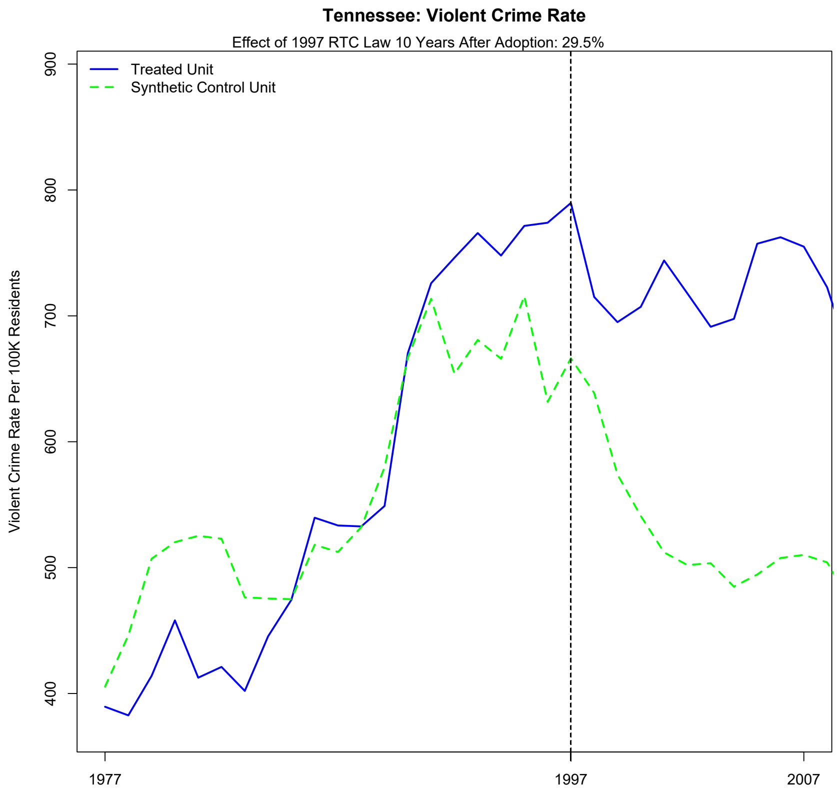
Effect of 1997 RTC Law 10 Years After Adoption: 22.5%



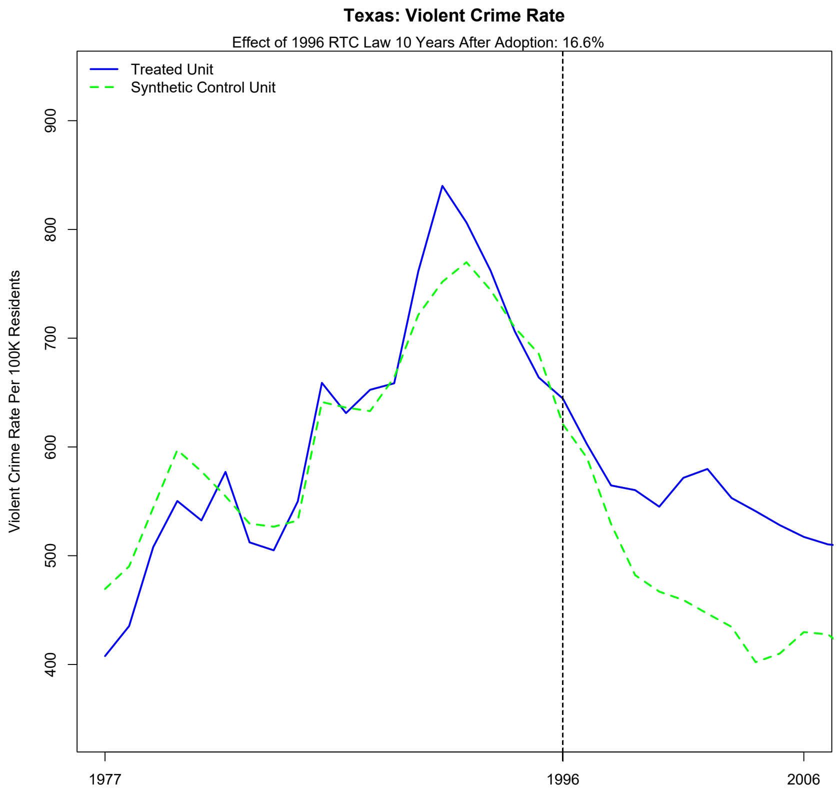
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.179); IL (0.821)
CVRMSPE: 0.088 (18 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014)



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)



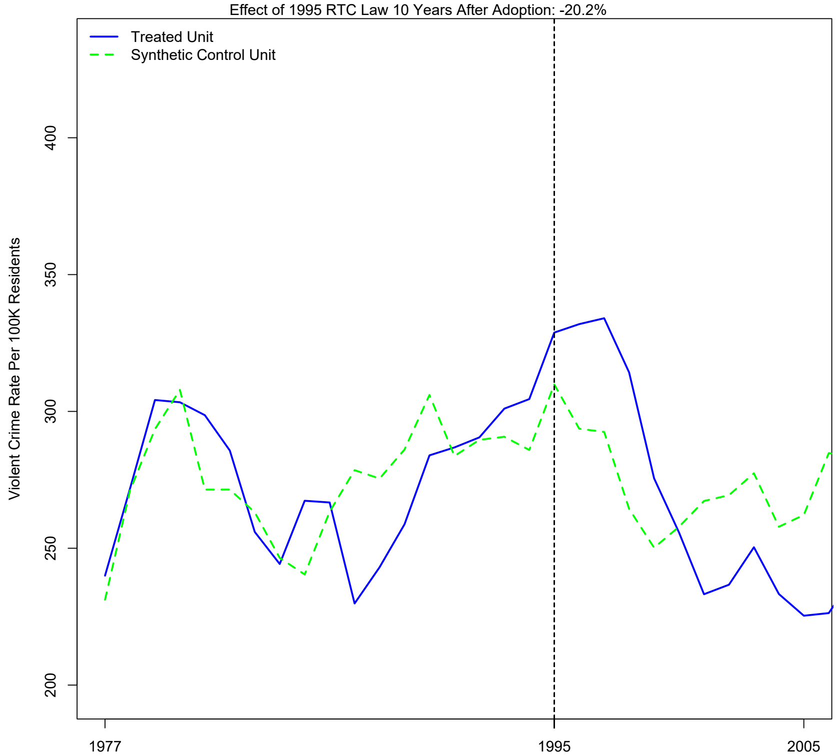
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.291); IL (0.395); IA (0.314)
CVRMSPE: 0.123 (24 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IA (2011); IL (2014)



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.578); NE (0.086); WI (0.336)
CVRMSPE: 0.06 (10 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC Adopting States Included in Synthetic Control: NE (2007); WI (2012)

Utah: Violent Crime Rate

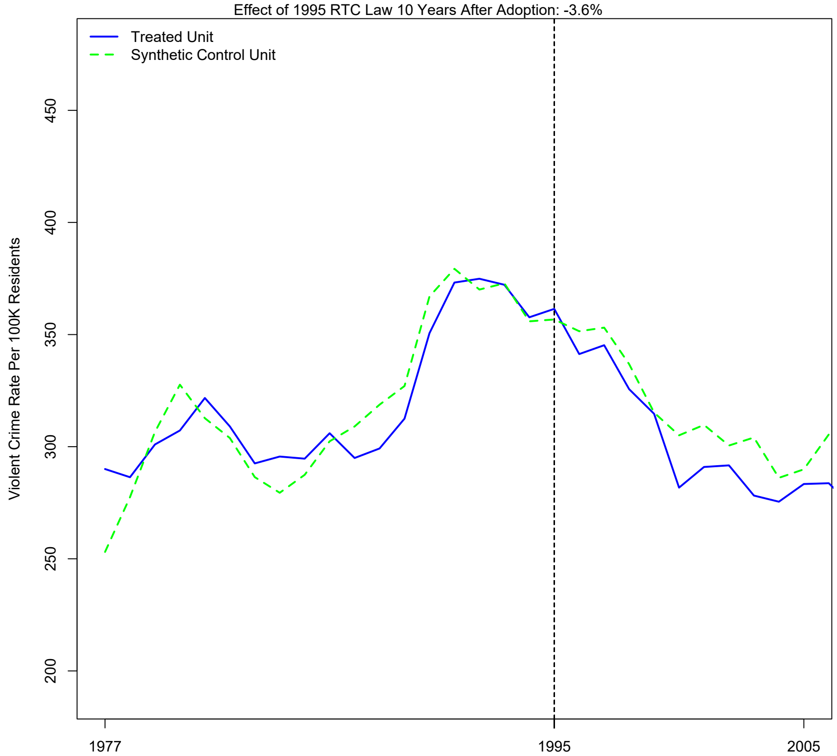
Effect of 1995 RTC Law 10 Years After Adoption: -20.2%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.756); KS (0.06); RI (0.108); WI (0.075)
CVRMSPE: 0.072 (14 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic Control: KS (2007); WI (2012)

Virginia: Violent Crime Rate

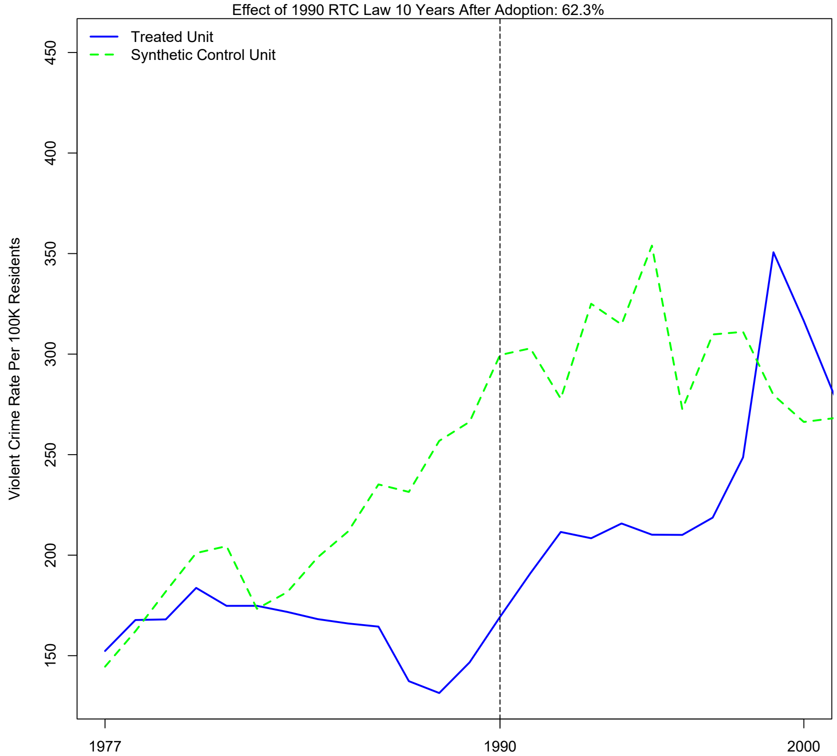
Effect of 1995 RTC Law 10 Years After Adoption: -3.6%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.249); KS (0.235); NE (0.157); RI (0.269); WI (0.09)
CVRMSPE: 0.044 (3 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic Control: KS (2007); NE (2007); WI (2012)

West Virginia: Violent Crime Rate

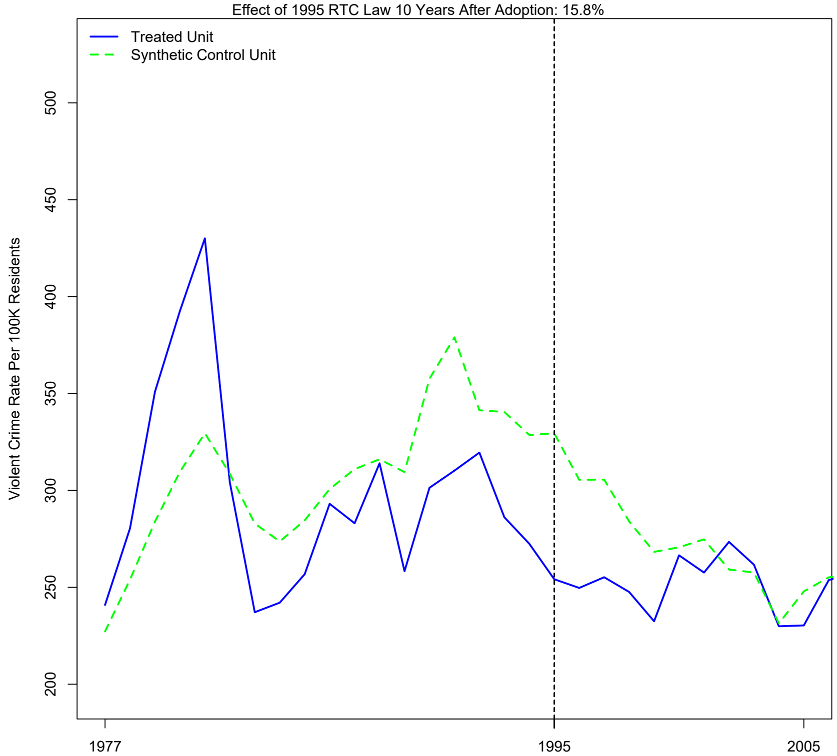
Effect of 1990 RTC Law 10 Years After Adoption: 62.3%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.007); IA (0.993)
CVRMSPE: 0.377 (30 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011)

Wyoming: Violent Crime Rate

Effect of 1995 RTC Law 10 Years After Adoption: 15.8%



Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.071); RI (0.525); WI (0.404)
CVRMSPE: 0.166 (26 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic Control: WI (2012)

Appendix G: Data Sources

Variable(s)	Years Available	Source	Model(s)	Notes
RTC Variables (*shalll* & *aftr*)	1977-2014	State session laws	DAW, BC, LM, MM	Statutes researched via Westlaw and HeinOnline. See footnotes 6 and 7 for explanations of these variables' constructions. Note that the spline variable is coded as 0 in all years for states that passed before the data period, which depends on the model under consideration. For example, for the DAW model (1979-2014), it is coded as 0 for states that passed before 1979.
Crime	1977-2014	FBI	DAW, BC, LM, MM	UCR Data Tool for data through 2013; Table 4 of 2015 crime report for data in 2014. Each crime rate is the corresponding crime count, divided by the population metric used by the FBI, times 100,000.
Police Staffing	1977-2014	FBI	DAW, BC	Agency-year-level police employment data were acquired from the FBI and aggregated to the state-year level. The police employee rate is the total number of employees, divided by the population as given in the same dataset. In the BC model, this variable is the one-year lag of logged police staffing per capita.
Population	1977-2014	Census	DAW, BC, LM, MM	Intercensal estimates are used, except in 1970 and 1980, for which decadal-census estimates are used. All models weight regressions by population; the LM and MM models also include it as a covariate.
Population by Age, Sex, and Race	1977-2014	Census	DAW, BC, LM, MM	Intercensal estimates are used.
Income Metrics	1977-2014	BEA	DAW, BC, LM, MM	Includes personal income, unemployment insurance, retirement payments and other, and income maintenance payments. All 4 measures are divided by the CPI to convert to real terms.
Consumer Price Index	1977-2014	BLS	DAW, BC, LM, MM	CPI varies by year but not by state.
Incarcerations	1977-2014	BJS	DAW, BC, MM	The number of prisoners under the jurisdiction of a state as a percentage of its intercensal population. In the BC model, this variable is the one-year lag of the log of year-end jurisdictional population per capita.
Land Area	1977-2014	Census	LM	Land area over a given decade is taken from the most recent decadal Census. The density variable is intercensal population divided by land area.
Poverty Rate	1979-2014	Census	DAW, MM	The Census directly reports the percentage of the population earning less than the poverty line.
Unemployment Rate	1977-2014	BLS	DAW, BC, LM	
Arrests	1977-2014	FBI	LM, MM	Agency-month-year-level arrests data, separated by age, sex, race, and crime category, were acquired from the FBI and aggregated to the state-year level. For each crime category, the arrest rate is the number of arrests for that crime as a percentage of the (UCR-reported) number of crimes.
Crack Index	1980-2000	Prof. Roland Fryer	MM	Following the MM model, we use the unadjusted version of the index.
Beer	1977-2014	NIH	DAW, BC	The NIH reports per-capita consumption of ethanol broken down by beverage type, including beer.
Population in Metropolitan Statistical Areas	1980-2014	FBI / ICPSR	DAW	MSA population counts obtained from ICPSR-provided UCR arrests data. 1979 values are linearly extrapolated.
Executions	1977-2014	BJS	BC	

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

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

We use a cross validated approach to determine the optimal lag choice(s) to include as predictor(s) in the synthetic control model. We use this procedure to choose among four potential lag choices used in the synthetic control literature; these choices involve including lags of the dependent variable in every pre-treatment year, three lags of the dependent variable,[49] one lag which is the average of the dependent variable in the pre-treatment period, and one lag which is the value of the dependent variable in the year prior to RTC adoption.[50] To implement the cross validation procedure, we first define our training period as 1977 through the sixth year prior to RTC adoption, the validation period as the fifth year prior to RTC adoption through one year prior to RTC adoption, and the full pre-treatment period as 1977 through one year prior to RTC adoption. For each of our 33 treatment units, data from the training period is used to determine the composition of the synthetic control. Specifically, for each of the 33 treatment units, we assign the treatment 5 years before the treatment actually occurred, and then run the synthetic control program using the standard ADZ predictors defined in Aneja, Donohue, and Zhang (2011) and a 5 year reporting window. We then examine the fit during the training period, the validation period, and the entire pre-treatment period to see how closely the synthetic control estimate matches the value of the dependent variable for different lag choices.

Tables A11-A13 examine the fit of the synthetic control estimate during the training period, validation period, and the entire pre-treatment period using three different loss functions. Table A11 defines the error using the mean squared error between the actual value of the dependent variable and the synthetic control estimate during a given period; Table A12 uses the mean of the absolute value of the difference between the treated value and synthetic control estimate; finally, Table A13 uses the CV of the RMSPE. For Tables A11-A13, an unweighted average of the error for each of the 33 treatment states is presented. For Tables A14-A16, a population weighted average of the error for each of the 33 treatment states is presented, where population from the first year of the relevant period is used.[51]

[49]The first lag is the value of the dependent variable in 1977, the second lag is the value of the dependent variable in the year prior to RTC adoption, and the third lag is the value of the dependent variable in the year that is midway between the year corresponding to the first and second lag. All results presented in Tables A11 through Table A16 use overall violent crime as the dependent variable.

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

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

The results from Tables A11-A16 provide strong evidence that using yearly lags of the dependent variable is the best option. As expected, across all six tables, the error in the training period is lowest using yearly lags. However, yearly lags also provides the lowest error in the validation period, regardless of how the error is defined or whether population weights are used to aggregate the measure of error over all treatment states. In addition, across all six tables, the error over the full pre-treatment period is lowest using yearly lags.

A potential concern with using all preintervention outcomes of the dependent variable as synthetic control predictors is that the synthetic control unit will not closely match the treated unit on the non-lagged predictors during the pre-treatment period.[52] But as Table A17 shows, we do not find that the synthetic control unit's fit on the non-lagged predictors is worse using yearly lags. To generate the numbers in Table A17, for each treatment state, we first take a simple average of our predictor of interest over all pre-treatment years (1977 through the year prior to RTC adoption). A population weighted average of the predictor pre-treatment means is then taken over all treatment states to reach the figures presented, which represent an aggregate measure of the pre-treatment predictor means.[53] Based on the absolute value of the difference between the aggregate treated predictor means and the aggregate synthetic control predictor means, yearly lags has the second best performance. The aggegate synthetic control predictor means using yearly lags comes closest or second closest to the treated unit for 9/16 predictors. In comparison, one lag that is the average of the dependent variable in the pre-treatment period comes closest or second closest for 11/16 predictors, one lag that is the value of the dependent variable in the last pre-treatment year comes closest or second closest for 7/16 predictors, and three lags for 5/16 predictors.

We thus choose yearly lags of the dependent variable as our optimal lag choice for two main reasons. The first is that yearly lags produces the lowest error not only in the training period, but also in the validation period and the full pre-treatment period. This statement is robust to various ways of defining the error and aggregating the error across treatment states. The second is that the synthetic control units using yearly lags do a fairly good job, relative to the other lag choices, of matching the pre-treatment (non-lagged) predictor means of the treatment states.

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

[53] Unlike Tables A11-A16 , where the treatment year for our 33 states of interest is assigned to five years before the actual year of RTC adoption, in Table A17, the treatment year is identical to the year of RTC adoption. For Table A17, the states eligible to be in a treated unit's synthetic control are those states that either never passed RTC laws, or passed more than 10 years after the treated unit adopted RTC laws. In contrast, for Tables A11-A16, the states eligible to be in a treated unit's synthetic control are those states that either never passed RTC laws, or passed any year after the treated unit adopted RTC laws.

Table A11: Comparison of Fit Across Various Lagchoices - Define Fit Using Mean Squared Error

	training period; Mean Squared Error	validation period; Mean Squared Error	full pre-treatment period; Mean Squared Error
three lags	2,162.41	7,435.12	3,827.18
yearly lags	1,393.09	6,893.02	3,036.10
one lag average	3,445.90	7,799.21	4,690.09
one lag final pre-treatment year	2,603.44	7,269.81	4,011.91

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

Table A12: Comparison of Fit Across Various Lagchoices - Define Fit Using Mean Absolute Difference

	training period; Mean Absolute Difference	validation period; Mean Absolute Difference	full pre-treatment period; Mean Absolute Difference
three lags	30.75	64.19	41.66
yearly lags	23.85	61.59	35.78
one lag average	39.95	68.46	48.88
one lag final pre-treatment year	31.43	62.65	41.68

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

Table A13: Comparison of Fit Across Various Lagchoices - Define Fit Using CVRMSPE

	training period; CVRMSPE	validation period; CVRMSPE	full pre-treatment period; CVRMSPE
three lags	0.12	0.25	0.18
yearly lags	0.10	0.23	0.17
one lag average	0.15	0.26	0.20
one lag final pre-treatment year	0.13	0.24	0.18

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

Table A14: Comparison of Fit Across Various Lagchoices - Define Fit Using Mean Squared Error

	training period; Mean Squared Error	validation period; Mean Squared Error	full pre-treatment period; Mean Squared Error
three lags	1,557.33	8,467.64	2,901.49
yearly lags	1,589.63	6,222.95	3,538.90
one lag average	4,218.08	6,178.57	6,111.82
one lag final pre-treatment year	3,711.16	13,492.12	5,716.22

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

Table A15: Comparison of Fit Across Various Lagchoices - Define Fit Using Mean Absolute Difference

	training period; Mean Absolute Difference	validation period; Mean Absolute Difference	full pre-treatment period; Mean Absolute Difference
three lags	26.30	67.65	35.62
yearly lags	25.92	58.07	38.91
one lag average	44.73	61.36	55.06
one lag final pre-treatment year	38.88	86.39	49.83

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

Table A16: Comparison of Fit Across Various Lagchoices - Define Fit Using CVRMSPE

	training period; CVRMSPE	validation period; CVRMSPE	full pre-treatment period; CVRMSPE
three lags	0.07	0.16	0.10
yearly lags	0.10	0.19	0.15
one lag average	0.11	0.19	0.13
one lag final pre-treatment year	0.13	0.21	0.17

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

Table A17: Crime Predictor Means Before RTC Adoption

	treated	Synthetic: 3 lags	Synthetic: yearly lags	Synthetic: 1 lag avg	Synthetic: 1 lag final pre-treatment year
popstatecensus	7,459,163.00	8,026,132.00	8,479,127.00	7,278,594.00	9,161,988.00
l_incarc_rate	224.51	189.64	194.12	192.32	197.40
l_policeemployeerate0	248.41	272.85	275.75	275.58	271.52
rpcpi	12,827.91	14,382.73	14,450.62	14,439.30	14,464.70
rpcui	65.70	81.31	80.67	81.32	81.30
rpcim	166.27	200.49	202.72	192.14	204.76
rpcrpo	1,427.63	1,451.61	1,454.97	1,475.17	1,447.78
unemployment_rate	6.81	6.17	6.19	6.09	6.22
poverty_rate	14.61	12.13	12.02	11.89	12.07
density	123.51	262.32	235.30	309.65	262.99
age_bm_1019	1.26	0.71	0.76	0.82	0.76
age_bm_2029	1.11	0.71	0.75	0.80	0.75
age_bm_3039	0.83	0.53	0.56	0.62	0.57
age_wm_1019	6.68	6.23	6.21	6.31	6.25
age_wm_2029	7.11	7.12	7.09	7.14	7.16
age_wm_3039	6.45	6.22	6.22	6.28	6.28

For each treatment state, the predictor of interest is averaged over all pre-treatment years (1977 through RTC year - 1)
a population weighted averageof this statistic is then taken over all treatment states to reach the figures presented

Appendix I: Synthetic Control Estimates Using Other Sets of Explanatory Variables

I. Synthetic Control Estimates Using the BC Explanatory Variables

Table A18 provides synthetic control estimates of the impact of RTC laws on violent crime using the BC model's set of predictors.[54] This model estimates that RTC laws increase violent crime consistently after adoption, rising to 13.3 percent after ten years (significant at the .01 level). This tenth-year effect is also quite close to the corresponding DAW model's synthetic control estimate (Table 9), as well as the DAW and BC panel data models' dummy variable coefficients (Tables 4-5).

Table A18: The Impact of RTC Laws on the Violent Crime Rate, BC covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.247	3.045**	4.014*	4.204**	6.278**	6.750**	9.489***	12.616***	13.077***	13.327***
	(1.107)	(1.488)	(1.990)	(2.016)	(2.458)	(3.080)	(3.184)	(4.046)	(3.828)	(3.402)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

II. Synthetic Control Estimates Using the LM Explanatory Variables

In our Part II panel data analysis, we saw that RTC laws were associated with significantly higer rates of violent crime in the DAW model (Table 4), the BC model (Table 5, Panel A), and the MM model (Table 7, Panel A), but not in the LM model (Table 6, Panel A), although both the LM and MM models did show RTC laws increased murder. Table A19 estimates the impact of RTC laws on violent crime using the LM specification.[55] The detrimental effects of RTC laws on violent crime rates 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 Table A19 range from 11.0 percent in the seventh post-treatment year to 12.8 percent in the tenth post-treatment year. Remarkably, the DAW, BC, and LM synthetic control estimates of the impact of RTC laws on violent crime are nearly identical (compare Tables 9, A18, and A19), and this is true even when we limit the sample of states in the manner described in Tables 10-11.[56]

Table A19: The Impact of RTC Laws on the Violent Crime Rate, LM covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.031	2.519	4.236**	4.599*	7.097**	7.687**	10.984***	12.592***	12.986***	12.801***
	(1.247)	(1.623)	(2.077)	(2.298)	(2.618)	(3.211)	(3.185)	(3.864)	(3.699)	(2.723)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

III. Synthetic Control Estimates Using the MM Explanatory Variables

Table A20 provides synthetic control estimates of the impact of RTC laws on violent crime using the MM predictors.[57] The table reveals that RTC states experienced overall violent crime rates that were roughly 15 percent greater than those of their synthetic controls ten years after passage, which was statistically significant at the .01 level. The similarity of the DAW, BC, LM, and MM

[55] The modified panel data analyses of LM and MM, shown in Panel B of Tables 6 and 7, did find RTC laws increase violent crime. In conducting the LM panel data analysis, we used the 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 variable on both sides of the regression model. This objection is less important under the synthetic control framework. For this reason, we use their contemporaneous crime-specific arrest rates in our synthetic control model using the Lott and Mustard (1997) control variables.

[56] The tenth-year effect in the synthetic controls analysis using the LM variables is 12.5 percent when we eliminate the states with more than twice the average CV of the RMSPE. Knocking out the six states with above-average values of this CV generates an almost identical 12.6 percent effect. We also estimated the impact of RTC laws on violent crime using the synthetic controls approach and the LM model modified to use six DAW demographic variables. This change increased the estimated tenth-year increase in violent crimes from 12.8 percent to 15.3 percent.

[57] For the same reasons described in footnote 55, 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.

synthetic controls estimates of the impact of RTC laws on crime is striking. Moreover, these four sets of estimates are remarkably consistent with the DAW and BC panel data estimates of the impact of RTC laws, which bolsters the case that the DAW and BC panel data specifications provide more reliable estimates of the impact of RTC laws on violent crime than either the LM or MM models.[58]

Table A20: The Impact of RTC Laws on the Violent Crime Rate, MM covariates, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	0.067	1.634	3.116*	4.708*	7.575**	8.196**	11.282***	13.434***	14.689***	15.290***
	(1.186)	(1.535)	(1.833)	(2.366)	(2.832)	(3.171)	(3.236)	(3.999)	(4.246)	(3.796)
N	33	33	33	33	33	33	33	31	31	31

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

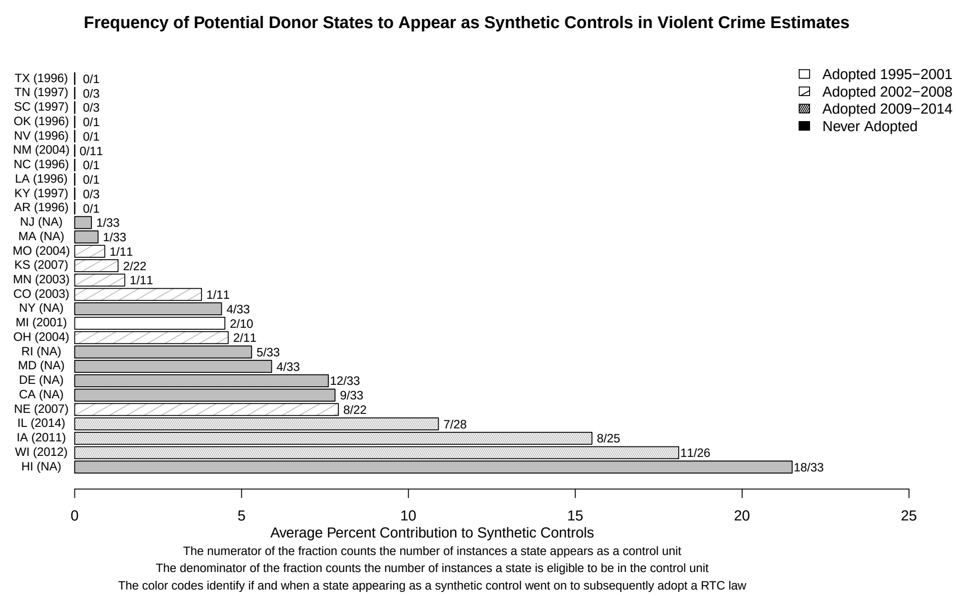
Turning our attention to property crimes, we find little systematic evidence that RTC laws influence property crime in the synthetic control approach, as our aggregate property crime results are never significant.

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

Appendix J: The Contributions of Donor States to the Synthetic Controls Estimates - Evaluating Robustness

One of the key elements of the synthetic controls approach is that, for each state adopting a RTC law in year X, the approach searches among states that do not have RTC laws through at least ten years after X—including never-adopting states—to select a plausible set of control states for the adopting state. Figure A34 lists all the states that are eligible, under this criterion, to serve as synthetic controls for one or more of the 33 adopting states, and shows how often they are in fact selected. The horizontal length of each bar tells us how much, on average, that state contributed to the synthetic controls in our violent crime estimates. As the Figure indicates, Hawaii appears most frequently—contributing to a synthetic control 18 of the 33 times it is eligible—and it has the largest average weight in the synthetic controls, of 21.5 percent.

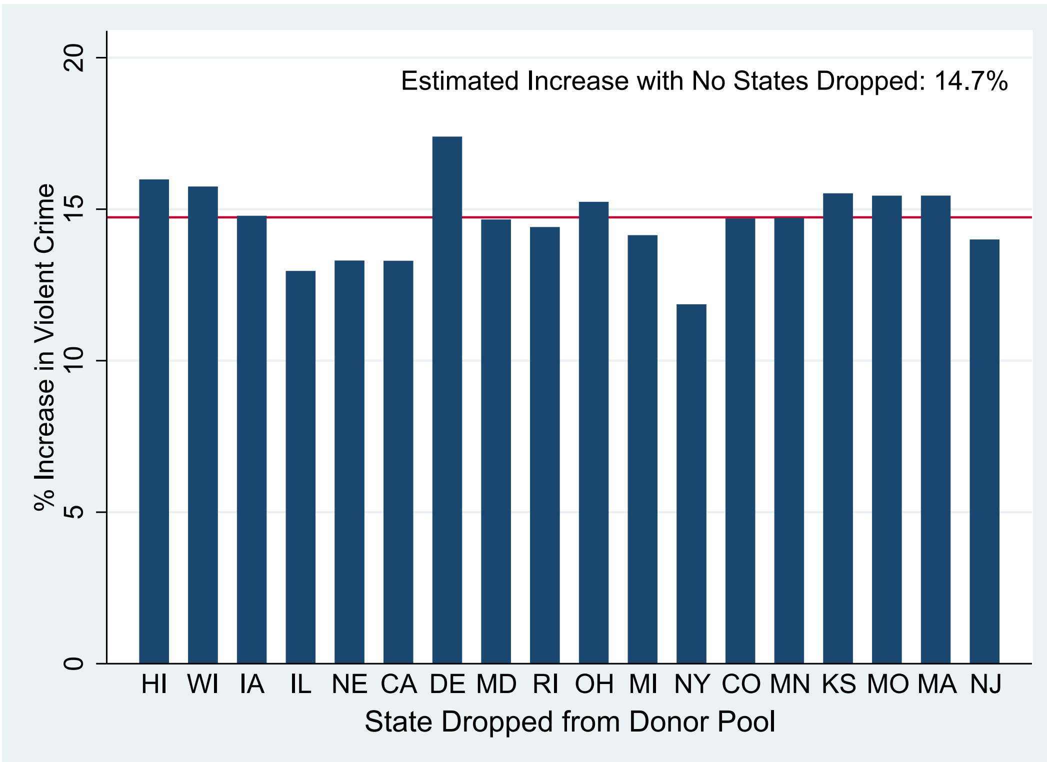
Figure A34



Given that Hawaii makes such a large contribution as a donor state in the synthetic controls estimates, and this small state might be unrepresentative of the states for which it is used as a control, one might be concerned that it might be unduly skewing the estimates of the impact of RTC laws on violent crime. To address this, as well as the analogous concern for other control states, we generated 18 additional TEP estimates, with each one generated by dropping a single one of the 18 states that appears as an element of our synthetic controls analysis (as identified in Figure A34). The results of this exercise are presented in Figure A35, which shows that our estimated increase in violent crime resulting from the adoption of a RTC law is extremely robust: All 18 estimates remain statistically significant at the 1 percent level, and the smallest TEP, which comes from dropping New York as a control state, is 11.9 percent.

Figure A35

Estimated Increase in Violent Crime 10 Years After RTC Adoption, Dropping One Donor State at a Time



This graph shows the overall synthetic-controls estimate of the impact of RTC laws on violent crime ten years after adoption when barring individual states from inclusion in the synthetic control. (The horizontal line shows the estimate when no states are barred.) The states are arranged in declining order of average contribution to synthetic controls (see Figure A34), from a high of 21.5 percent for Hawaii to a low of 0.5 percent for New Jersey.

Appendix K: Does Gun Prevalence Influence the Impact of RTC Laws?

The wide variation in the state-specific synthetic control estimates that was seen in Figures 6 and 9 suggests that greater confidence should be reposed in the aggregated estimates than in any individual state estimate, as averaging across a substantial number of states will tend to eliminate the noise in the estimates. Another way to distill the signal from the noise in the state-specific estimates is to consider whether there is a plausible explanatory factor that could explain underlying differences in how RTC adoption influences violent crime. One possible mechanism could be that RTC laws will influence crime differently depending on the level of gun prevalence in the state at the time of adoption.

Figure A36

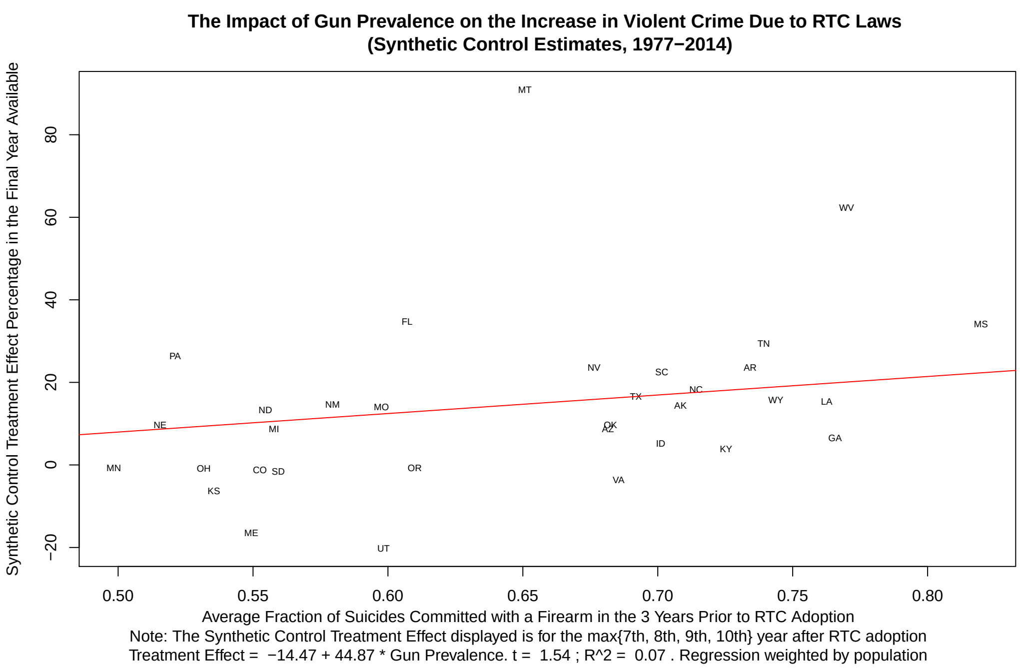


Figure A36 shows the scatter diagram for 33 RTC-adopting states, and relates the estimated impact on violent crime to a measure of gun prevalence. (Gun prevalence is proxied by the commonly used measure showing the fraction of suicides in a state that are committed with guns.) The last line of the note below the Figure provides the regression equation, which shows that the gun prevalence proxy is positively related to the estimated increase in crime, but the coefficient is not statistically significant (t = 1.54) and the R^2 value is very low.[59] The population-weighted

[59] A bivariate regression that weights by the inverse of the CV of the RMSPE, rather than by state population yields results substantively identical to those in Figure A36. We also repeat this analysis when dropping the 5 states with the worst pre-passage fit (NE, WV, MT, SD, and ND), and this modification again does not substantively change the Figure A36 regression results.

mean gun proxy level across our 33 states is 0.64 (roughly the level of Montana), which would be associated with a 14 percent higher rate of violent crime 10 years after RTC adoption.

Appendix L: The Murder and Property Crime Assessments with Synthetic Controls

Because the synthetic controls estimates of the impact of RTC laws on violent crime uniformly generate statistically significant estimates, we have heretofore focused on that analysis. Our synthetic control estimates of the impact of RTC laws on murder and property crime appear in Tables A3-A10 of the appendix. While in all cases the tenth-year effect for these crimes is positive, in no case is it statistically significant at even the .10 level. For murder, the point estimates suggest an increase of 4-5 percent, and for property crime, the point estimates range from 1-4 percent increases.

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

But then what are we to make of the relatively small estimated impact of RTC laws on murder? This might seem to be at odds with our theoretical expectations, and in conflict with the estimated increases in overall violent crime since one might expect violent crime and murder to move together. Part of the explanation is that we are able to get more precise estimates of the impact of RTC laws on violent crime then for the far less numerous, and hence much more volatile, crime of murder. Indeed, the standard errors for the synthetic controls estimate of increased murder in the tenth year is 25 percent higher than the comparable standard error for violent crime (compare Table 9 with Table A3).

But a second and more important fact is also at work that likely suppresses the true estimated impact of RTC laws on the murder rate. We know from Table 2 that RTC states increased police employment by 8.39 percent more in the wake of RTC adoption than did non-RTC states. This suggests that our estimates of the crime-increasing impact of RTC laws are biased downward, but since police are more effective in stopping murder than either overall violent or property crime, the extent of the bias is greatest for the crime of murder. In other words, the greater ability of police to stop murders than overall violent (or property) crime may explain why the synthetic controls estimates for murder are weaker than those for violent crime. An increase in police employment of 8.39 percent would be expected to suppress murders in RTC states (relative to non-RTC states) by about 5.6 percent.[60] Since the synthetic controls approach does not control for the higher police employment in the post-adoption phase for RTC states, it may be appropriate to elevate the synthetic controls estimates on murder to reflect the murder- dampening effect of their increased police presence.

To adjust our synthetic control estimates of the impact of RTC laws on murder to reflect the post-adoption changes in the rates of police employment and incarceration, we can compare how these crime-reducing elements changes in the wake of adoption for our RTC-adopting state and for the synthetic control. Consistent with the panel data finding of Table 2 that police and incarceration grew more post-RTC- adoption, we found that, over the 33 models using the DAW covariates and murder rate as the dependent variable, the population-weighted average percent change in the incarceration rate from the year of adoption to the 10th year after adoption (the 7th year after adoption for Kansas and Nebraska) is 28 percent for the treated unit and 19 percent for the synthetic control unit. For the police employee rate, the analogous numbers are 9.1 percent for the treated unit and 7.2 percent for the synthetic control unit.[61]

We correct for this underestimation by restricting the synthetic control unit to have the same growth rate in incarceration and police as the treated unit.[62] Once we have computed an adjusted murder rate for the 31 synthetic control units in the 10th year after adoption, we then use the formula described in part IV to construct an adjusted aggregate treatment effect.[63] The impact of controlling for police and incarceration are substantial: the 10th year impact of RTC laws rises from 4.68 percent (t = 1.28) to 9.75 percent (t = 1.98).[64] In other words, the ostensible puzzle that

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

[61] 21 of the 33 states experienced growth in the incarceration rate (17/33 for police employee rates) that was greater than their respective synthetic controls growth rate.

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

[63] Kansas and Nebraska, both 2007 adopters, have no comparable data for 10 years after adoption and are thus not included in this calculation.

[64] If one only corrects for the larger jump in police experienced by the treatment states, the 10th year effect jumps from 4.68 percent (t = 1.28) to 7.77 percent (t = 1.70). The 9.75 percent estimated jump in the murder rate in the text results from restricting the synthetic control unit to have the same post-adoption year *growth rate* in police and incarceration as the treated unit. One can also try to control for differential post-adoption movements in police and incarceration by focusing on the post-adoption change in the *levels* of the police employee rate and the incarceration rate. When we constrain the post-adoption change in police and incarceration between the treated and synthetic control unit to be the same 10 years thereafter, the aggregate 10th-year effect is 9.94 percent (t = 2.08). Using this second technique, if one only corrects for the larger jump in police experienced by the treatment states, the 10th-year effect is 8.06 (t = 1.83).

RTC laws increased overall violent crime but did not increase murder may be explained by the fact that RTC-adopting states masked the increase in murder by elevating their rates of police and incarceration.