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

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Online Appendix

Appendix A: RTC Adoption Dates

Table A1 of Appendix A lists each state's original RTC effective date and adjusted effective date (for our synthetic control analysis). For our panel data analyses, we use the same effective RTC dates used in Aneja, Donohue, and Zhang (2014), while specifying in column 2 the precise date on which an RTC law takes effect. Owing to the fact that the mechanics of the synthetic control methodology require us to specify a specific year for each state's RTC date, we alter the year used in the synthetic control analysis if the RTC law is in effect for less than half the year. Specifically, each state's effective year of passage in the synthetic control analysis is defined as the first year in which an RTC law was in effect for the majority of that year. 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 for our synthetic control analysis, since the state's RTC law took effect on November 1, 2011). The states in column 4 that show an RTC date of 0 are states that did not adopt an RTC law between 1977 and 2014.

While there have been numerous disagreements about the exact laws that should be used to determine when states made the transition to a "shall issue" state, we believe that the dates used in this paper accurately reflect relevant RTC effective dates.[1] We supplemented our analysis of the statutory history of RTC laws in different states with an extensive search of newspaper archives to ensure that our chosen dates represented concrete changes in concealed carry policy. We document the changes that were made to earlier selection of RTC dates and the rationales underlying these changes in Appendix G of Aneja, Donohue, and Zhang (2014). The coding of these dates may not reflect administrative or logistical delays that may have limited the full implementation of an RTC law after authorities were legally denied any discretion in rejecting the issuing of RTC permits. Ideally, we would be able to control for the actual level of RTC permits in existence each year for each state, instead of simply relying on a a mere indicator variable for the presence of an RTC law, but unfortunately such comprehensive information is not available.[2]

We also note that there has been confusion over the proper date of Virginia's RTC law, which we place in 1995, while Lott and Mustard (1997) had used 1988. Although many studies that have

[1] For instance, the Illinois shall issue law (430 ILCS 66/1) took effect on 7/9/13. It included the following provision: "The Department [of State Police] shall make applications for a license available no later than 180 days after the effective date of this Act." It did take the department the full 180 days until it opened the application process to citizens on 1/5/2014. Hence, January 5th 2014 is our effective RTC date for Illinois.

[2] RTC dates before the year 1977 may not be exact, since differences between these dates would neither affect our regression results nor our synthetic control tables. We follow earlier convention in the academic literature on the RTC issue in assigning pre-1977 RTC adoption dates for Alabama and Connecticut.

relied on the Lott and Mustard data have used the earlier adoption date for Virginia, the recent Rand report on gun science concluded that 1995 was the appropriate date that RTC (shall-issue) was established in Virginia (RAND 2018, p.173).

Appendix Figure A1 presents data on concealed carry permit applications from 1984-2008 from the relevant Virginia State of the Judiciary Reports.[3] The fact that permit applications were small in number and flat until 1995 when they jumped sharply confirms that Virginia's shift from a may-issue to a shall-issue (RTC) regime occurred in 1995.

Figure A1

Prior to 1995, the number of concealed carry permits remained low because of the requirement to establish "a need to carry" such weapons. So, for example, in February 1993, the Circuit Court of Virginia (Kulp 1993) stated:

> "The Court found Mr. Mack to be of good character but found that he had failed to demonstrate a need for a concealed weapon. An order was entered denying the application on January 8, 1993. [. . .]
>
> "The Court further finds that the other reasons outlined by Mr. Mack do not in themselves warrant the issuance of a concealed weapon permit. From time to time, most citizens carry valuables, including cash, in their vehicles. If this were sufficient criteria for the issuance of a concealed weapon permit, then all citizens are entitled to a concealed weapon permit. If the legislature had intended such a result, it surely would have said so."

[3]See for example, "The Virginia 1999 State of the Judiciary Report" (1999). The 1985 and 2008-2015 reports do not contain permit application data.

In 1995, Virginia Code Section 18.2-308 was modified to eliminate the requirement to demonstrate a *need to carry*. Thus, legally and practically, May 5, 1995 is the correct shall-issue law adoption date for Virginia.

Table A1: RTC Adoption Dates

State	*Effective Date of RTC Law*	*Fraction of Year In Effect Year of Passage*	*RTC Date (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 B1: Panel Data Violent Crime Coefficients using DAW and LM models, State and Year Fixed Effects

Dummy Variable Model Results

	(Table 3) *DAW Model*	(Table 4.A) *LM Model*
	(1)	(2)
Right-to-carry law	9.02*** (2.90)	−1.38 (3.16)
Lagged incarceration rate	0.04* (0.02)	
Lagged police employee rate	−0.05 (0.04)	
Lagged arrest rate for violent crimes		−0.16** (0.08)
Real per capita personal income (×100)	0.00 (0.00)	0.00* (0.00)
Real per capita unemployment insurance (×100)		0.00 (0.01)
Real per capita income maintenance		0.04 (0.03)
Real per capita retirement payments and other (Lott version) (×100)		0.00 (0.01)
Unemployment rate	−0.02 (0.78)	
Poverty rate	−0.32 (0.49)	
Beer	60.82*** (17.55)	
Population		0.00 (0.00)
Percent of the population living in MSAs	1.10*** (0.32)	
Population density		−0.01 (0.02)
Observations	1823	1896

Estimations include year and state fixed effects and are weighted by state population. Coefficients on demographic variables and the constant omitted. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The crime data is from the Uniform Crime Reports (UCR). * $p < .1$, ** $p < .05$, *** $p < .01$. All figures reported in percentage terms. The DAW model is run on data from 1979-2014, and the LM model from 1977-2014.

Appendix C: Panel Data Models Estimated for the Post-Crack Period

Our primary discussion has focused on panel data estimates of the impact of RTC laws on crime over the full period from the late 1970s through 2014. Zimmerman (2014) examines the impact of various crime prevention measures on crime using a state panel data set from 1999-2010. He finds that RTC laws *increased* murder by 15.5 percent for the eight states that adopted RTC laws over the period he analyzed. The advantage of using this data period to explore the impact of RTC laws is that it largely avoids the problem of omitted variable bias owing to the crack phenomenon, since the crack effect had largely subsided by 1999. The disadvantage is that one can only gain estimates based on the eight states that adopted RTC laws over that twelve-year spell.[4] 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).

Siegel et al. (2019) examine the relationship between ten different state firearm laws and the age-adjusted homicide and suicide rates using data from 1991-2016. The authors argue that their study is unique in that it is the first panel data study at the state level that simultaneously assesses the effects of multiple state firearm laws (e.g. universal background checks, shall issue laws, violent misdemeanor laws, etc.). After simultaneously controlling for ten gun laws, Siegel et al. (2019) find that RTC laws are significantly associated with 9 percent higher homicide rates. Robustness checks show that this effect is driven by firearm homicide, since shall issue laws are not significantly associated with the non-firearm homicide rate.

Crifasi et al. (2018) find additional evidence in a study using urban U.S. counties that RTC laws increase homicide. They employ an interrupted time series analysis of large urban counties with a mixed effect Poisson model to account for repeated measures by county. The authors find that RTC laws are associated with a 4 percent higher firearm homicide rate, with no change in non-firearm homicide rate, again lending credence to the causal link between RTC laws and crime.

Gius (2019) examines the effect of RTC laws on murder and firearm rates using state-level SHR data from 1990-2014. The author defines "treatment" as a state moving from prohibiting concealed carry to a shall-issue regime, resulting in a total of eight "treated" states. Gius (2019) employs both a synthetic controls and panel data approach. He finds states that moved from "prohibited CCW"

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

status to "shall issue" status experienced a 12.3 percent increase in firearm murder rates and a 4.9 percent increase in overall murder rates in the panel data model.

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

In Appendix Table C1, we show the results of running the DAW model for five crime measures over the period 2000-2014 for eleven RTC-adopting states.[5] The DAW panel data model mimics the Zimmerman finding of a large jump in murder, rising at a rate of over one percent each year the RTC law is in effect. The estimated increase in firearm homicide from RTC adoption is even greater. Appendix Figures C1-C4 present year-by-year estimates, which depict a generally flat series of pre-treatment dummies followed by a change in crime right at the time of the adoption of the RTC law. Indeed, even though the Appendix Table C1 DAW violent crime RTC dummy is not statistically significant (p-value = 0.165), Appendix Figure C1 further buttresses our earlier conclusion that RTC laws are associated with increases in violent crime. Again, as we saw in Figure 2 this increase becomes statistically significant after the RTC law has been in effect for at least a full year and does so for the next four years, after which the diminishing number of RTC-adopting states with more than 4 years of data and the widening confidence intervals render the unvaryingly positive subsequent year estimates statistically insignificant.

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

Table C1: Panel Data Estimates of the Impact of RTC Laws, DAW and LM specifications, 2000 - 2014.

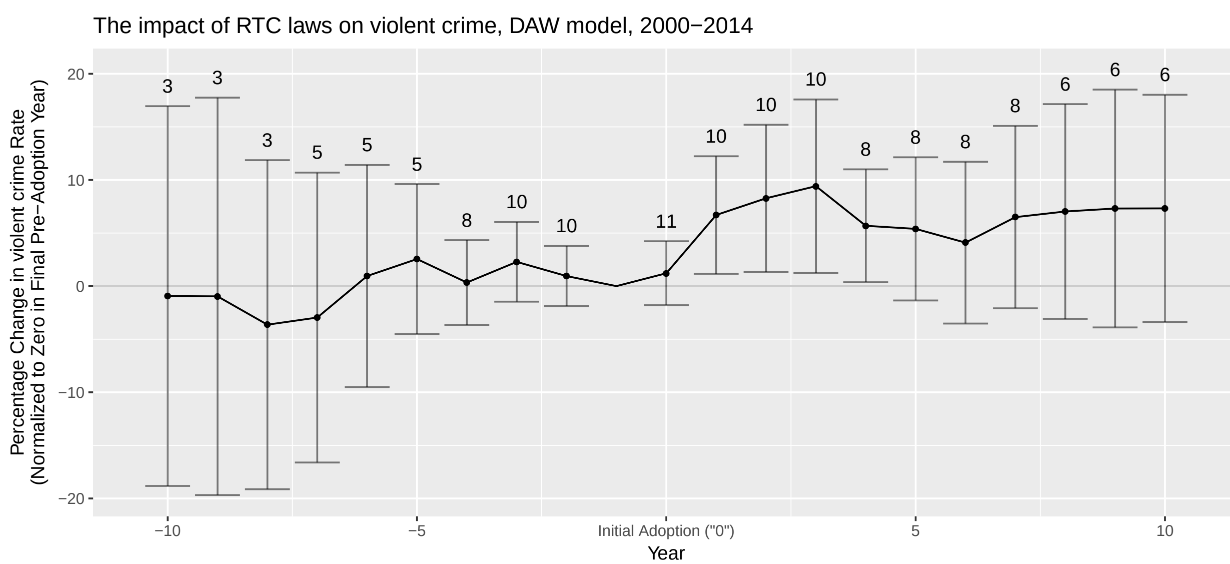
Panel A: Panel Data Estimates, State and Year Fixed Effects, DAW Regressors, 2000-2014

	Murder Rate	*Firearm Murder Rate*	*Non-Firearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	(2)	(3)	(4)	(5)
Dummy Variable Model	9.36*** (3.59)	12.81*** (4.30)	5.85 (4.44)	5.00 (3.55)	−1.50 (2.29)

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

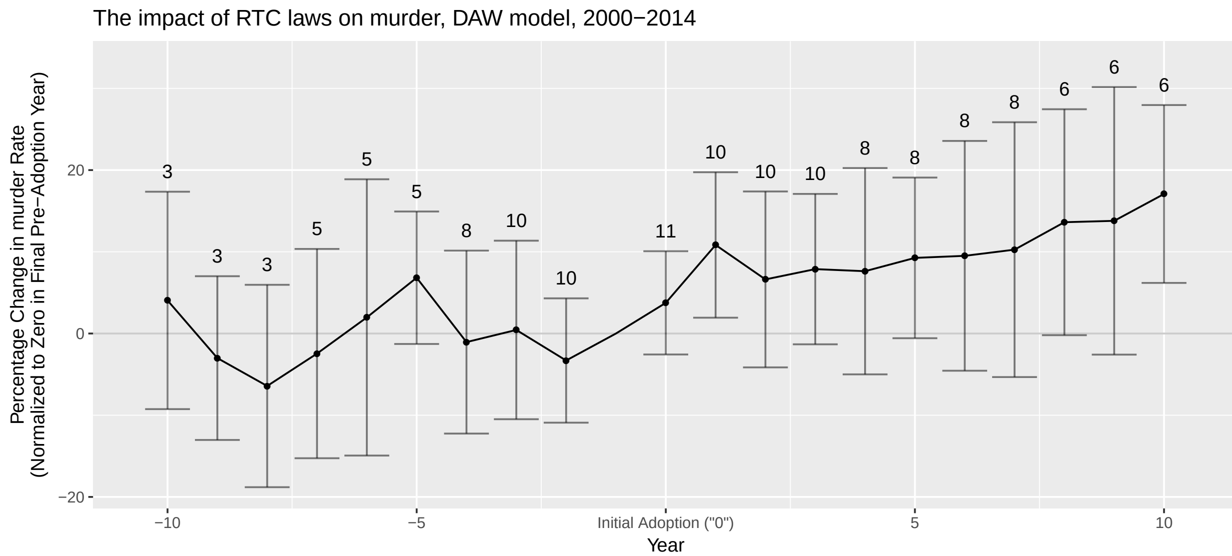
	Murder Rate	*Firearm Murder Rate*	*Non-Firearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	(2)	(3)	(4)	(5)
Dummy Variable Model	6.54** (3.05)	11.13*** (3.48)	1.81 (3.50)	−0.87 (3.35)	−3.06 (1.94)

All models include year and state fixed effects, and the OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. 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.



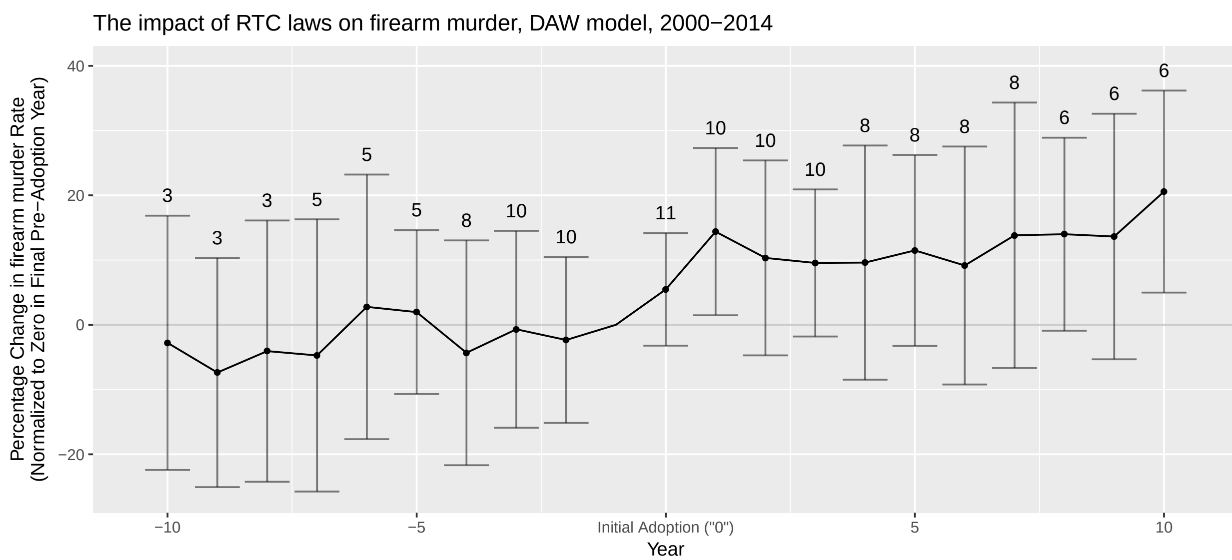
Note: We regress crime on dummies for pre− and post−passage years and DAW covariates. Reference year is year before adoption and adoption year is first year with RTC in place at any time, meaning that in states that adopt after January 1st, this will capture only a partial effect of RTC laws. We display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

Figure C1



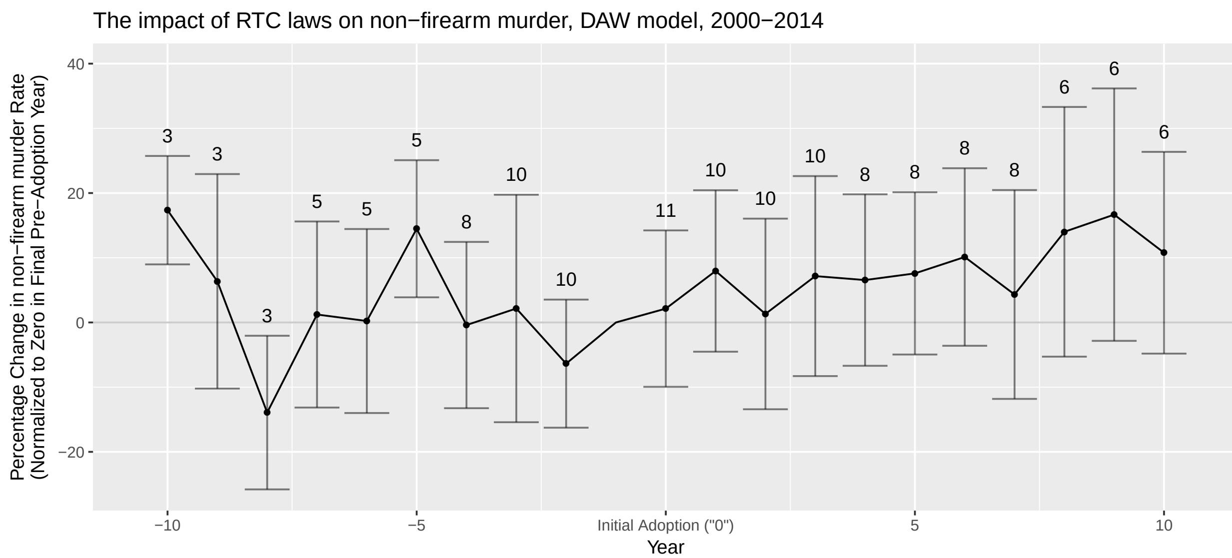
Note: We regress crime on dummies for pre− and post−passage years and DAW covariates. Reference year is year before adoption and adoption year is first year with RTC in place at any time, meaning that in states that adopt after January 1st, this will capture only a partial effect of RTC laws. We display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

Figure C2



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

Figure C3



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

Figure C4

Appendix D: Figures

A. Year-by-year Panel Data Estimates

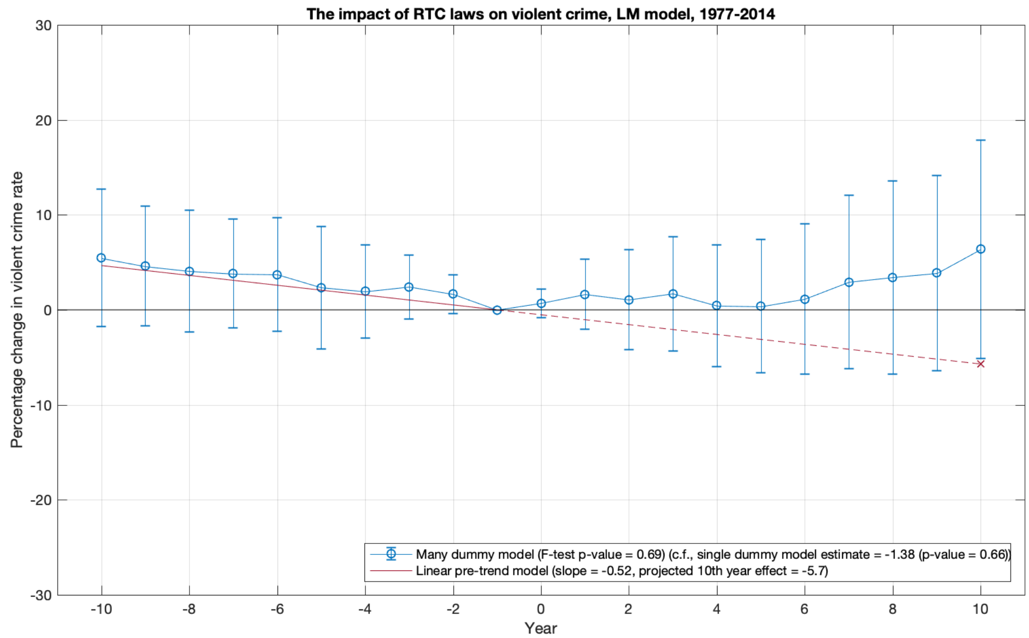


Figure D1

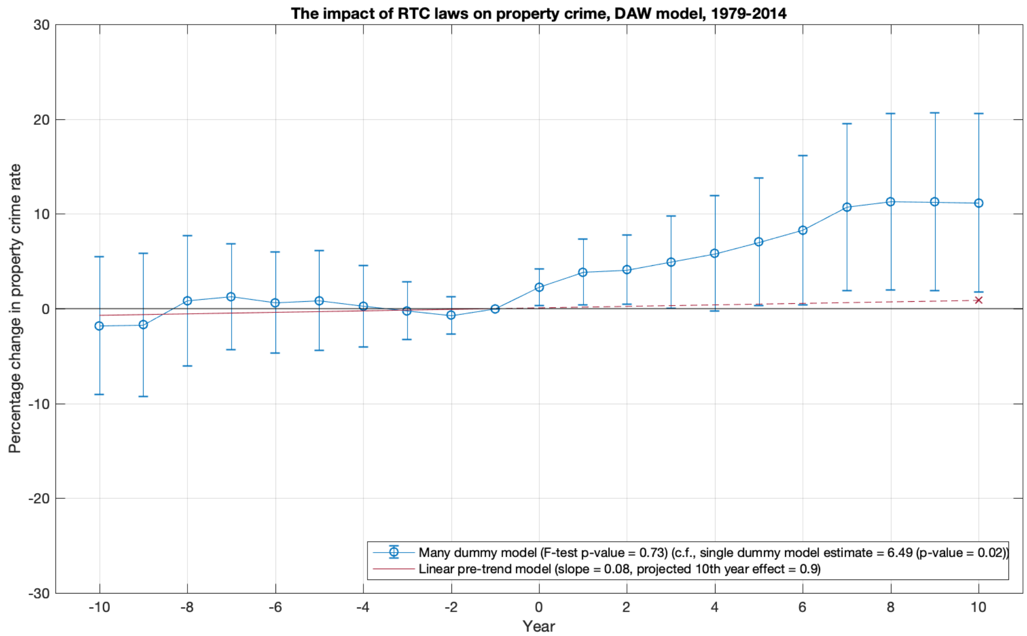


Figure D2

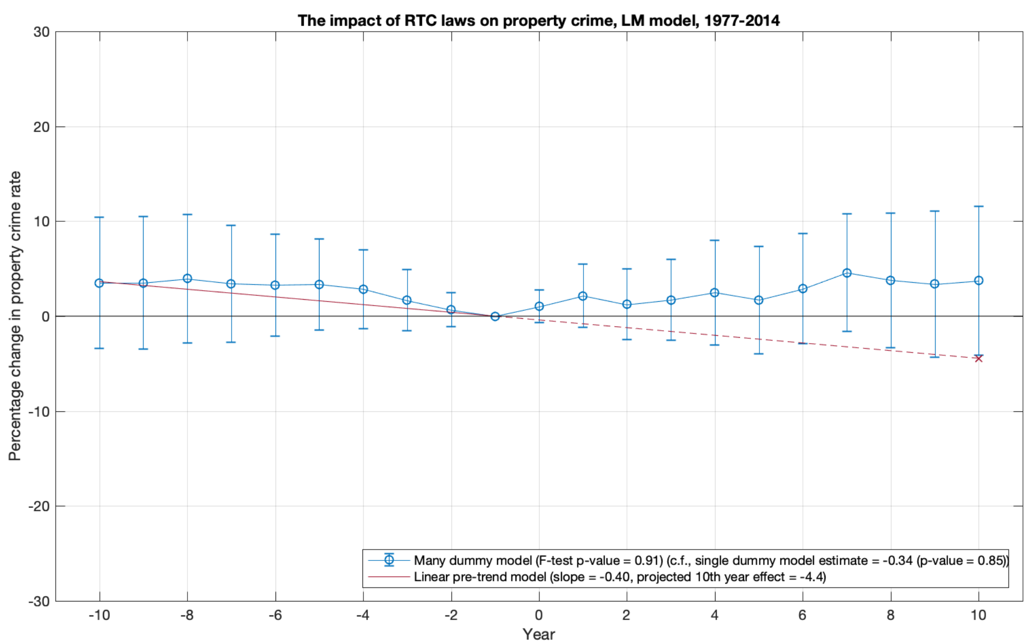


Figure D3

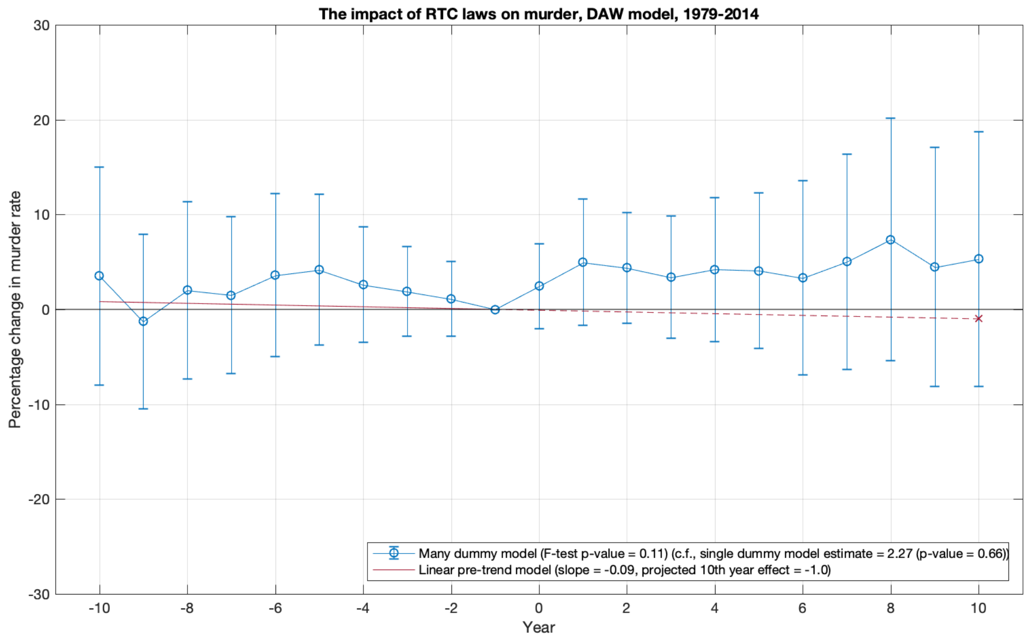


Figure D4

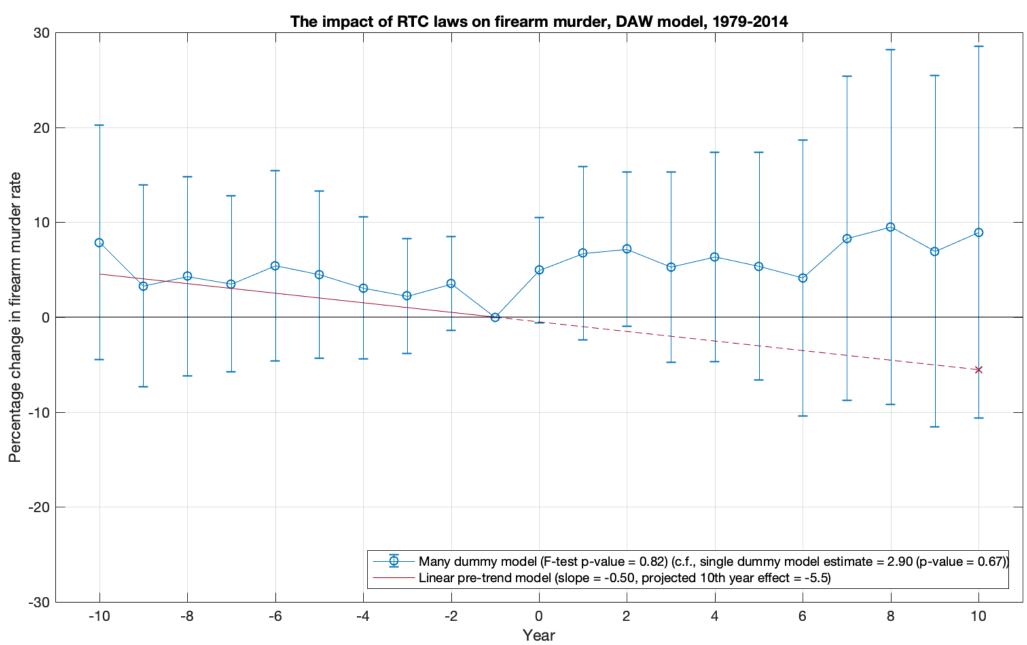


Figure D5

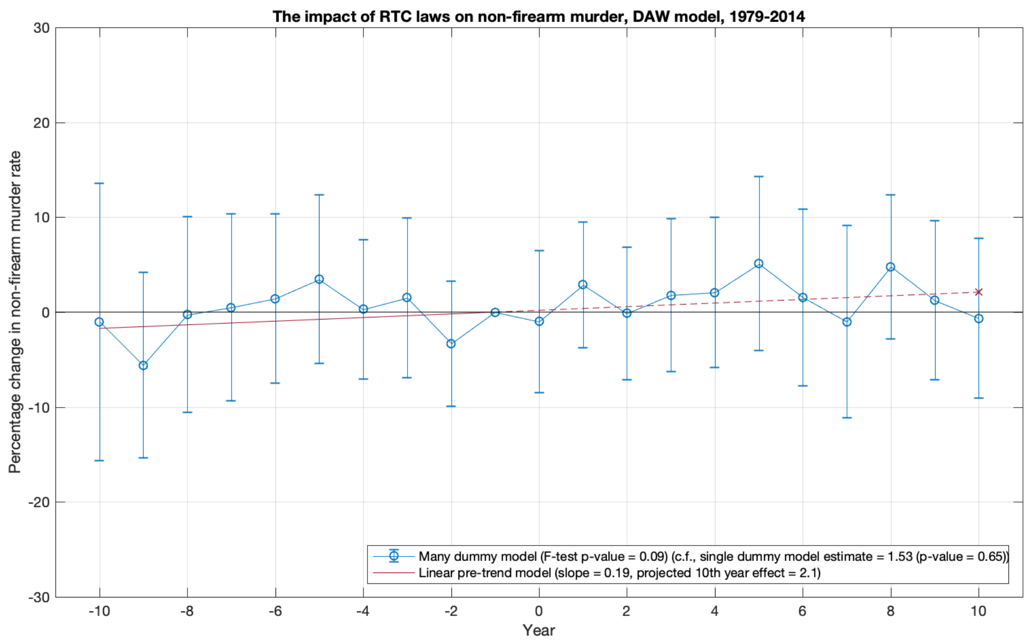

The impact of RTC laws on non-firearm murder, DAW model, 1979-2014
Percentage change in non-firearm murder rate
Year
30
20
10
0
-10
-20
-30
-10
-8
-6
-4
-2
0
2
4
6
8
10
Many dummy model (F-test p-value = 0.09) (c.f., single dummy model estimate = 1.53 (p-value = 0.65))
Linear pre-trend model (slope = 0.19, projected 10th year effect = 2.1)

Figure D6

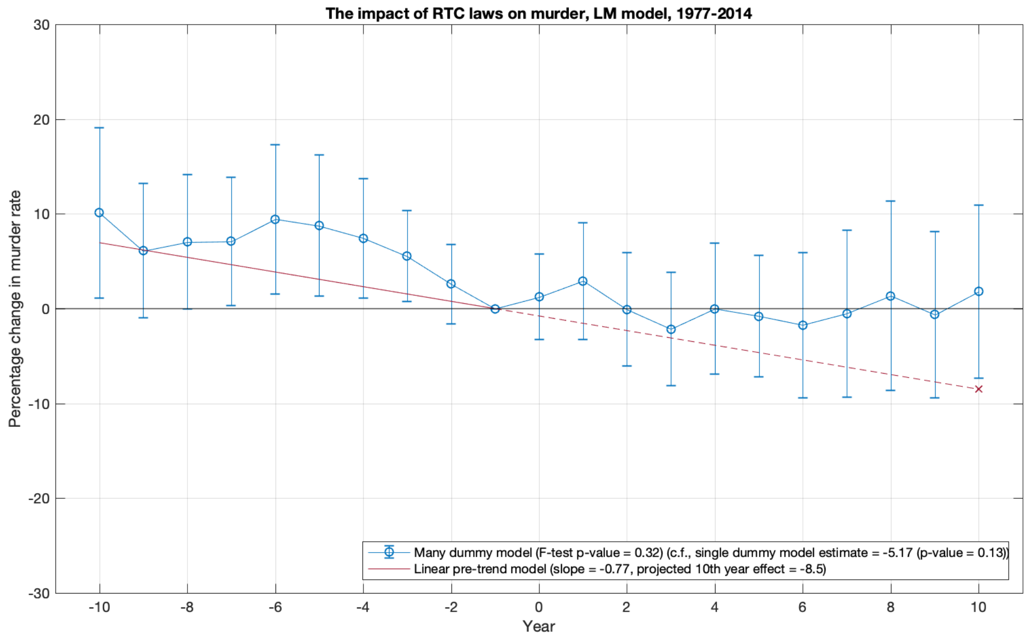


Figure D7

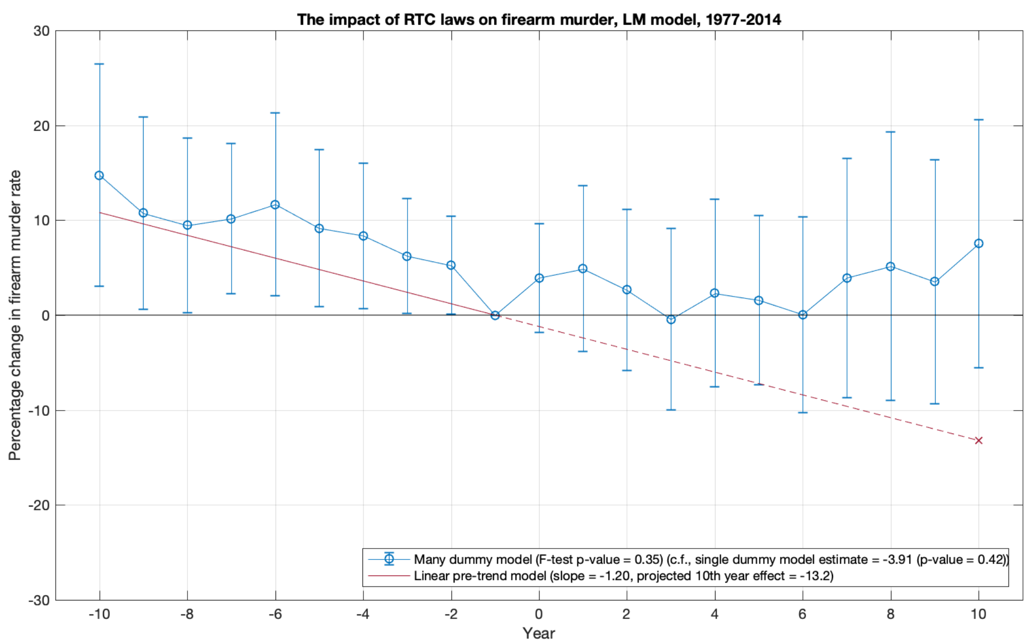
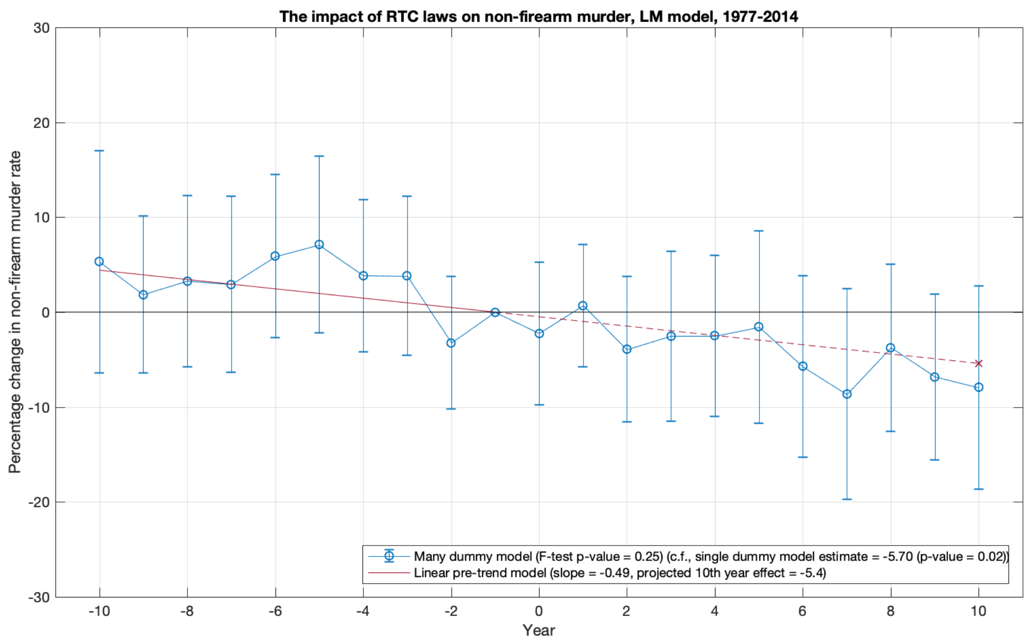


Figure D8



The impact of RTC laws on non-firearm murder, LM model, 1977-2014
Percentage change in non-firearm murder rate
Year
30
20
10
0
-10
-20
-30
-10
-8
-6
-4
-2
0
2
4
6
8
10
Many dummy model (F-test p-value = 0.25) (c.f., single dummy model estimate = -5.70 (p-value = 0.02))
Linear pre-trend model (slope = -0.49, projected 10th year effect = -5.4)

Figure D9

B. State Contributions to Synthetic Control Estimates

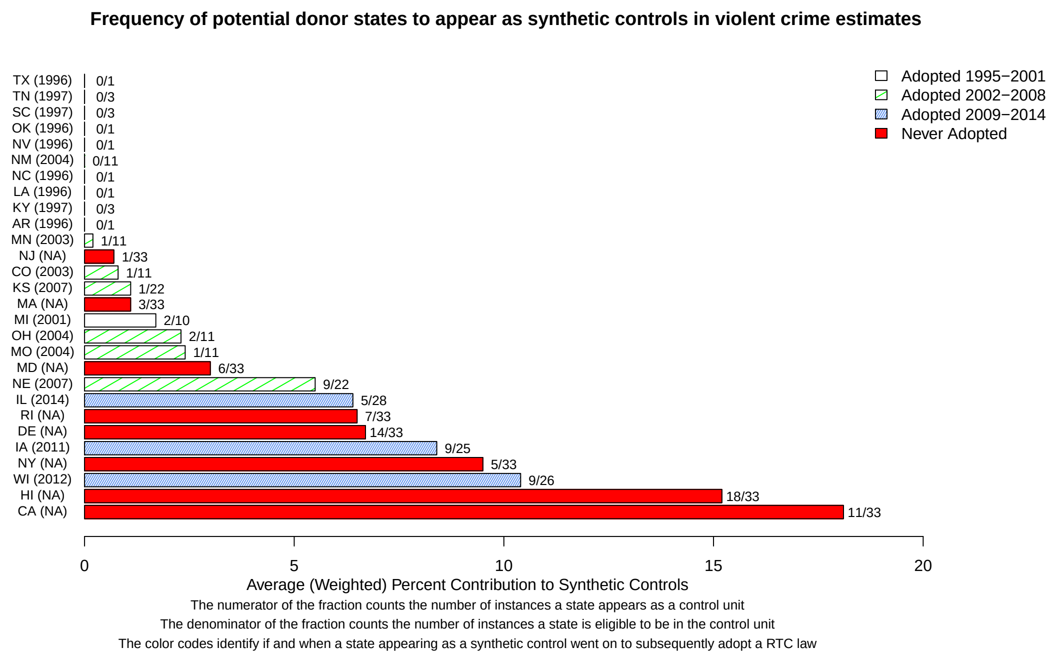


Figure D10

C. Synthetic Control Estimates for Impact on Violent Crime

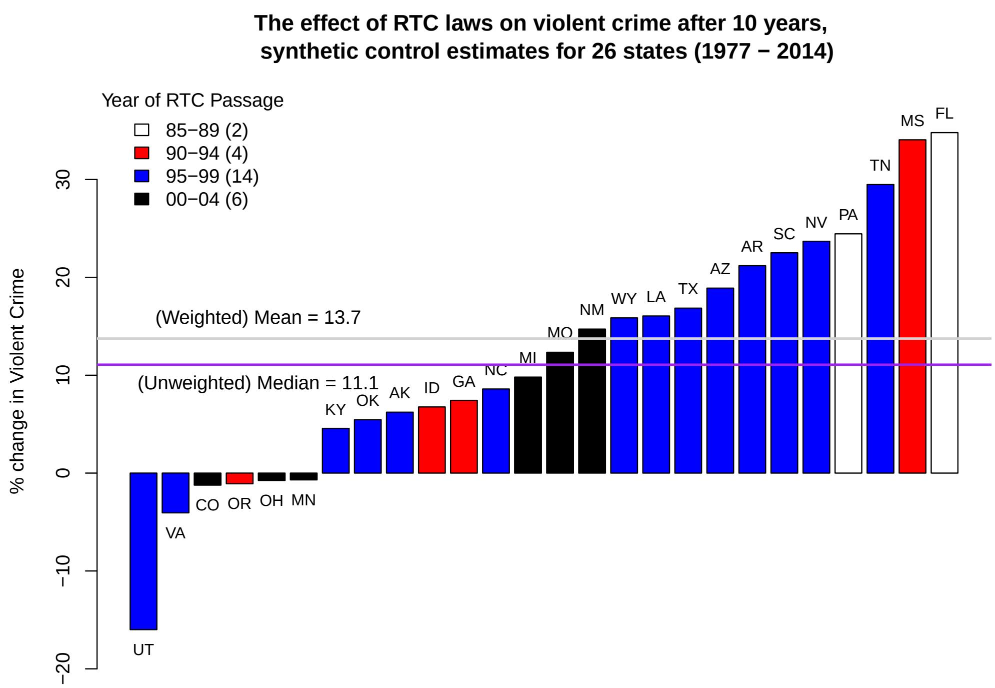
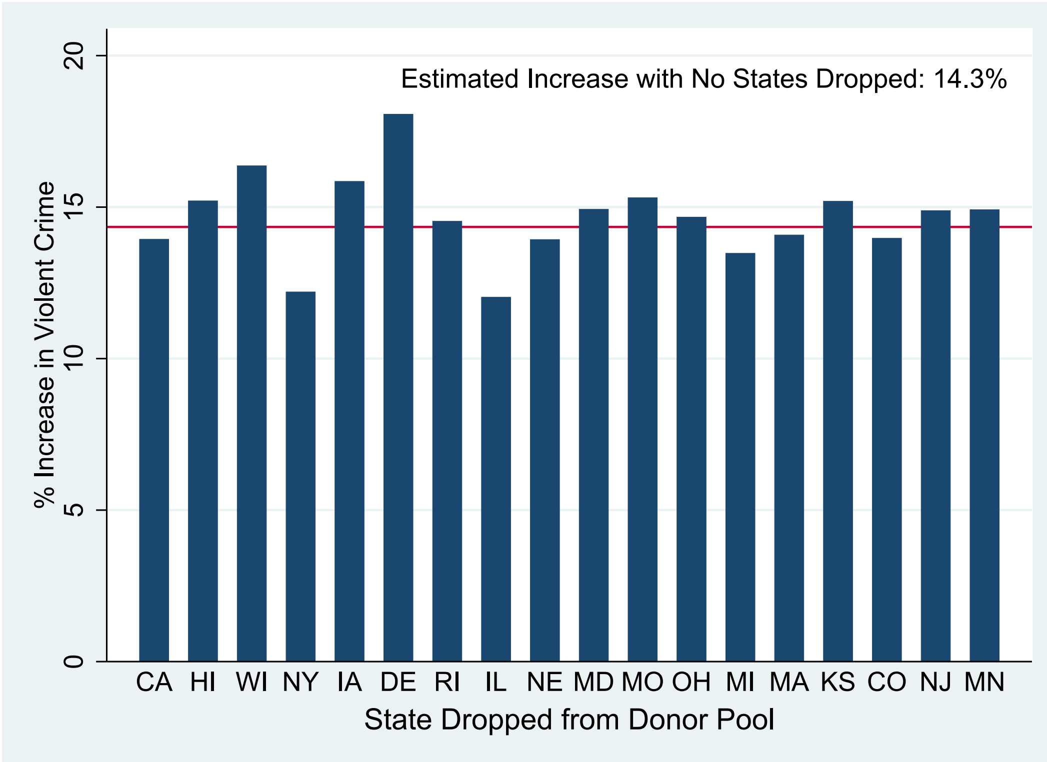


Figure D11



The graph shows the overall synthetic control estimate of the impact of RTC laws on violent crime ten years after adoption when barring individual states from inclusion in the synthetic control. (The horizontal line shows the estimate when no states are barred.) The states are arranged in declining order of population-weighted average contribution to synthetic controls (see Appendix Figure D10), from a high of 18.1 percent for California to a low of 0.2 percent for Minnesota.

Figure D12: Estimated increase in violent crime ten years after RTC adoption, dropping one donor state at a time

Appendix E: Synthetic Control Estimates of the Impact of RTC Laws on Murder and Property Crime for the DAW and LM Models

Our synthetic control estimates of the impact of RTC laws on murder and property crime appear in Appendix Tables E1-E8. In all cases except non-firearm murder the tenth-year effect for these crimes is positive, but the only statistically significant estimate is for murder (at the .10 level).

The relatively smaller impact of RTC laws on non-firearm murder and on property crime is not surprising. Much property crime occurs when no one is around to notice, so gun use is much less potentially relevant in property crime scenarios than in the case of violent crime, where victims are necessarily present. Most of the pernicious effects of RTC laws—with the exception of gun thefts—are likely to operate to increase violent crime more powerfully than property crime. The fact that the synthetic control approach confirms the DAW panel data estimates showing that RTC laws increase violent crime while simultaneously showing far more modest effects on property crime (thereby conflicting with the DAW panel data estimate showing substantial increases in property crime) may be thought to enhance the plausibility of the synthetic control estimates.[6]

The 8.7 and 15.3 percent increases in overall and firearm murder over a ten-year period are not small effects, but only the murder estimate performs well in our phantom laws test and its statistical significance is only at the 0.10 level. Part of the explanation for the lower level of statistical significance for murder is that we are able to get more precise estimates of the impact of RTC laws on violent crime than for the far less numerous, and hence much more volatile, crime of murder. Indeed, the standard errors for the synthetic control estimate of increased murder in the tenth year is 73 percent higher than the comparable standard error for violent crime (compare Table 5 with Appendix Table E1), and the differential is far greater still for the firearm murder estimates of Appendix Table E2.[7]

It is also worth considering another factor that likely causes the synthetic control approach to understate the increase in crime caused by RTC laws, particularly for murder. We know from Table 1 that RTC states increased police employment by 8.4 percent more and increased incarceration by almost seven percent more in the wake of RTC adoption than did non-RTC states. This suggests that our synthetic control estimates of the crime-increasing impact of RTC laws could be biased downward, and since police and incarceration are more effective in stopping murder than either

[6]Alternatively, the poorer performance of the property estimates in our phantom law test may be thought to weaken the value of the property synthetic control estimate (Appendix Table L3)

[7]One of the factors that contributed to the extreme volatility of murder was the crack cocaine epidemic, which also led some of the early studies of RTC law astray because this omitted variable made RTC laws appear much more benign than they in fact were. Our Appendix C analysis side-steps this problem by focusing on the post-crack period and generates large and statistically significant estimates that RTC laws increase overall and firearm murder.

overall violent or property crime, the extent of any bias would be greatest for the crime of murder. In other words, the greater ability of police and prison to stop murders than overall violent (or property) crime may explain why the synthetic control estimates for murder are weaker than those for violent crime. An increase in police employment of 8.4 percent alone would be expected to suppress murders in RTC states (relative to non-RTC states) by about 5.6 percent.[8] Since the synthetic control approach does not control for the higher police employment and incarceration in the post-adoption phase for RTC states, it may be appropriate to elevate the synthetic control estimates on murder to reflect the murder-dampening effect of the two factors.

To crudely adjust our synthetic control estimates of the impact of RTC laws on murder to reflect the post-adoption changes in the rates of police employment and incarceration, we can compare how these crime-reducing elements change in the wake of adoption for each RTC-adopting state and for its particular synthetic control. Consistent with the panel data finding of Table 1 that police and incarceration grew more post-RTC-adoption, we found that the population-weighted average percent change in the incarceration rate from the year of adoption to the 10th year after adoption (the 7th year after adoption for Kansas and Nebraska) is 28 percent for the treated unit and only 20 percent for the synthetic control unit. For the police employee rate, the analogous numbers are 9.1 percent for the treated unit and 7.6 percent for the synthetic control unit.[9]

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.[10] 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.[11] The impact of controlling for police and incarceration is substantial: the 10th year impact of RTC laws rises from 8.7 percent (t = 1.71) to 13.39 percent (t = 2.65).[12] In other words, the ostensible puzzle that

[8] 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.

[9] 22 of the 33 states experienced growth in the incarceration rate (17/33 for police employee rates) that was greater than their respective synthetic control growth rate (obtained using DAW covariates and the murder rate). The population-weighted fraction of states experiencing this greater increase was 67.3% for incarceration and 49.2% for police.

[10] 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 police and incarceration rates for the synthetic control unit were both ten percent greater than the actual rates in the 10th year after adoption for an RTC-adopting state, we would adjust the murder rate for the synthetic control unit downwards by 0.67*10 + 0.15*10 = 8.2 percent (thereby elevating the predicted impact of RTC laws on murder).

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

[12] If one only corrects for the larger jump in police experienced by the treatment states, the 10th year effect jumps from 8.7 percent (t = 1.71) to 11.48 percent (t = 2.27).

RTC laws generated a large and statistically significant increase in overall violent crime but led to a smaller and less statistically significant increase in murder may be explained by the fact that RTC-adopting states constrained the RTC-induced increase in murder by elevating their rates of police and incarceration.

Finally, we have chosen to present synthetic control estimates that subtract off the initial year discrepancy between the actual and synthetic control crime figures, which we think is validated by our Appendix L analysis. While these would be be our preferred estimates, Appendix F reveals that without subtraction the DAW tenth year synthetic control estimates of the increase in the overall and firearm murder rates from RTC adoption range rise substantially and are statistically significant at or above the .05 level.[13]

We would like to note that the placebo exercise works slightly differently with the murder, firearm murder, and non-firearm murder. In footnote 65 we state that we "randomly choose eight states to never pass RTC, six states to pass RTC before 1981, 33 states to pass RTC between 1981 and 2010, and three states to pass RTC between 2011 and 2014." This assignment of states only works for the placebo analysis of violent and property crime rates, since there are no missing state-years of data for the dependent variables. Since synthetic controls will not run if there are missing values for the outcome variable (crime rate), for murder, firearm murder and non-firearm murder, we must drop the states that are missing any years of data from 1977-2014 for the respective crime rates. For murder, we need to drop 4 states from our analysis. In our placebo exercise, we then randomly assign 8 states to never pass RTC, 4 states to pass RTC before 1981, 31 states to pass RTC between 1981 and 2010, and 3 states to pass RTC from 2011 to 2014. For firearm murder and non-firearm murder, we drop 9 states. We then randomly assign 6 states to never pass RTC, 4 states to pass RTC before 1981, 28 states to pass RTC between 1981 and 2010, and 3 states to pass RTC between 2011 and 2014. This allows our assignment of states to more accurately represent the actual RTC date distribution of states in the sample used in the synthetic controls analysis for each crime rate.

[13]The Appendix Table E1 DAW estimate for murder in the tenth year after the RTC adoption is 8.7 percent and not statistically significant (with subtraction) but rises to a statistically significant value of 18.9 percent without subtraction (Appendix Table F4). Similarly, when not subtracting the adoption year percentage difference, the tenth year TEP for property crime is larger and becomes significant at the five percent level (Appendix Table F13).

Table E1: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-1.070	-2.722	0.207	0.415	-1.770	-0.422	3.429	8.128	6.317	8.661*
	(2.053)	(3.991)	(5.606)	(6.010)	(5.993)	(5.738)	(6.768)	(6.190)	(5.355)	(5.062)
N	31	31	31	31	31	31	31	29	29	29

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E2: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-2.439	-3.811	-4.962	-2.286	-6.906	-3.253	3.878	10.289	9.485	15.321
	(3.364)	(5.309)	(7.441)	(7.849)	(7.920)	(8.625)	(11.009)	(10.125)	(8.962)	(9.308)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E3: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	3.691	-5.373*	0.840	1.178	8.906	-3.609	-3.027	5.694	2.024	-4.584
	(2.770)	(2.918)	(3.397)	(4.080)	(7.035)	(5.392)	(4.929)	(3.796)	(3.823)	(3.153)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in non-firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E4: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014

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

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable change in the percentage difference in property crime rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E5: The Impact of RTC Laws on the Murder Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-1.501	-4.881	-3.160	-0.172	-4.159	-1.655	1.262	6.085	4.477	7.122*
	(1.731)	(3.235)	(3.947)	(4.046)	(4.163)	(3.607)	(3.703)	(3.765)	(4.107)	(3.583)
N	31	31	31	31	31	31	31	29	29	29

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the non-nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E6: The Impact of RTC Laws on the Firearm Murder Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-1.078	-2.746	-5.755	-0.049	-5.693	-1.095	4.413	11.102	8.150	11.889
	(2.233)	(3.947)	(5.555)	(6.748)	(6.842)	(7.700)	(8.084)	(9.998)	(9.462)	(9.050)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the non-nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E7: The Impact of RTC Laws on the Non-Firearm Murder Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	3.021	-8.480***	-4.403	-3.612	-0.668	-6.624	-4.628	-2.427	-1.967	-7.903**
	(2.973)	(2.646)	(3.119)	(4.207)	(4.735)	(5.512)	(5.525)	(4.460)	(4.333)	(3.465)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in non-firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the non-nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E8: The Impact of RTC Laws on the Property Crime Rate, LM covariates, Full Sample, 1977-2014

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

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the change in the percentage difference in property rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the non-nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

Table F1: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Table F2: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Table F3: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Table F4: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	9.661**	7.983**	10.883**	11.057**	8.838*	10.166*	14.006*	18.520**	16.700**	18.856***
	(4.615)	(3.827)	(5.174)	(5.332)	(4.984)	(5.674)	(7.259)	(6.855)	(6.138)	(6.386)
N	31	31	31	31	31	31	31	29	29	29

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F5: The Impact of RTC Laws on the Murder Rate, DAW covariates, $<$ 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	7.815*	5.613*	9.558*	8.576*	6.923	9.278*	13.240*	17.981**	15.238**	17.561**
	(4.214)	(3.124)	(5.081)	(4.362)	(4.654)	(5.348)	(7.280)	(6.786)	(6.165)	(6.526)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F6: The Impact of RTC Laws on the Murder Rate, DAW covariates, $<$ 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	10.811**	6.066	10.396	9.105	5.344	10.874	15.187	22.691**	19.472**	23.938***
	(3.753)	(4.488)	(7.751)	(6.601)	(6.747)	(7.440)	(10.840)	(8.144)	(7.839)	(6.614)
N	15	15	15	15	15	15	15	15	15	15

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F7: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	8.490	7.107	5.936	8.583	3.928	7.564	14.699	20.293*	19.481**	24.994**
	(7.344)	(6.625)	(7.694)	(8.545)	(7.981)	(9.432)	(11.460)	(11.136)	(9.318)	(10.303)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the firearm murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table F8: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, $<$ 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	4.000	2.578	2.555	4.170	0.278	2.751	10.242	14.865	14.800*	21.084**
	(5.794)	(4.803)	(7.083)	(7.402)	(7.096)	(8.158)	(10.765)	(9.895)	(8.048)	(9.595)
N	26	26	26	26	26	26	26	24	24	24

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the firearm murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table F9: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, $<$ 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	5.285	0.376	1.932	6.550	-2.302	3.948	5.872	19.130	15.312	23.731**
	(5.740)	(3.902)	(7.394)	(9.382)	(7.922)	(9.234)	(10.708)	(11.945)	(9.723)	(9.597)
N	15	15	15	15	15	15	15	15	15	15

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the firearm murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table F10: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	15.379***	6.301*	12.508**	12.842***	20.572***	8.062	8.652*	17.000***	13.338**	6.691
	(4.313)	(3.671)	(5.348)	(4.629)	(6.960)	(4.891)	(4.798)	(5.490)	(4.790)	(4.393)
N	28	28	28	28	28	28	28	26	26	26

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the non-firearm murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F11: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	16.163***	7.056*	14.052**	13.712***	21.633***	9.074*	10.076**	18.444***	14.122***	6.470
	(4.455)	(3.816)	(5.509)	(4.742)	(7.227)	(5.069)	(4.854)	(5.495)	(4.918)	(4.526)
N	26	26	26	26	26	26	26	25	25	25

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the non-firearm murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F12: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	14.940***	6.408	13.004**	14.297**	19.986**	8.886	9.166	18.089***	13.742**	5.549
	(4.726)	(4.256)	(6.037)	(5.273)	(8.079)	(5.539)	(5.349)	(6.056)	(5.354)	(4.735)
N	19	19	19	19	19	19	19	19	19	19

Standard errors in parentheses
Column numbers indicate post-passage year under consideration; N = number of states in sample
Dependent variable is the percentage difference in the non-firearm murder rate at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
The synthetic controls method is run using the nested option.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F13: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Table F14: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, < 2x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Table F15: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, < 1x Average Coefficient of Variation of the RMSPE, 1977-2014, No Subtraction of Adoption Year Crime Differential

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

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

Appendix G: Data Methodologies

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 model. All variables are collected for the years 1977-2014 unless otherwise noted.[14]

Annual state-level violent crime and property crime rates are taken from the FBI's Uniform Crime Reporting program.[15] Annual state-level overall murder, firearm murder, and non-firearm murder rates are taken from the National Vital Statistics System (NVSS) Fatal Injury Reports.[16] While the initial report in the NVSS is filed through death certificates, which are subject to mandatory reporting requirements, the initial report in the UCR is filed through police reports, which are submitted on a generally voluntary basis. Similarly, the reporting responsibility falls on medical examiners and coroners in the NVSS but law enforcement officers in the UCR. Although both the NVSS and UCR have potential issues with data quality, the NVSS has consistently shown a higher total number and rate of homicides in the US (Bureau of Justice Statistics 2014).

Four state-level income variables (personal income, income maintenance payments, retirement payments, and unemployment insurance payments) are taken from the BEA's Regional Economic Accounts. The personal income, income maintenance, and unemployment insurance payment variables are estimated in real per capita terms (defined using the CPI). The LM specification uses alternative versions of the retirement variable that are described in footnote 14. State-level popu-

[14]Many of the data sources that we used in our earlier analysis are revised continuously, and we use a newer version of these data series in this paper than we did in the earlier ADZ analysis. We sometimes made data changes during the data cleaning process. For instance, a detailed review of the raw data underlying arrest statistics uncovered a small number of agencies that 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). 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. Our retirement variable in the LM specification uses the population over 65 as a denominator and uses a tighter definition of retirement payments.

[15]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.

[16]To maintain internal consistency and consistency with how the UCR crime rates are generated, we generate our three murder rates by dividing NVSS reported murder counts by NVSS reported state-level population. Note that the NVSS fatal injuries data is available through both the CDC WONDER and CDC WISQARS databases. We obtain our data from the CDC WISQARS database, which appears to be more complete than the WONDER data. Since the WONDER data does not appear to include deaths that occurred during terrorist attacks or operations of war, we replace our 2001 overall murder and non-firearm murder counts from our WISQARS NVSS data, with those from our WONDER NVSS data for NJ, NY, MA, MD, DC, CT and VA.

lation and the proportional size of LM's 36 age-race-sex demographic groups are estimated using the Census Bureau's intercensal population estimates. (When the most recent form of these data were not accessible at the state level, state-level figures were generated by aggregating the Census Bureau's county-level population estimates by age, sex, and race.) Population density is estimated by dividing a state's population by the area of that state reported in the previous decennial census. State-level unemployment rate data is taken from the Bureau of Labor Statistics, while the poverty rate is taken from two Census series (the 1979 state-level poverty rate is derived from the Decennial Census and the 1980-2014 poverty rates are generated using the Current Population Survey). The percentage of population living in an MSA was constructed as a hybrid of two measures to account for shifts over time.[17] A measure of incarceration (incarcerated individuals per 100,000 state residents) is calculated from tables published by the Bureau of Justice Statistics counting the number of prisoners under the jurisdiction of different state penal systems. Our primary estimates for crime-specific state-level arrest rates 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.[18]

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.[19] Consequently, we decided to exclude the District of Columbia from the synthetic control analysis owing to its status as a clear outlier whose characteristics are

[17] We use Census delineation and NBER population files to find the fraction of individuals residing in a county which at least partially overlap with an MSA in 1980 (some New England counties were assigned by town). Since MSA definitions shift over time, we use the UCR implied fraction of population living in an MSA beginning in 1981. Observations for states incorrectly reported as 0 percent MSA by UCR in those early years were replaced according to the 1980 definition with updated Census population estimates. These values jump due to MSA redefinition over time. When we checked the robustness of our panel results by replacing our percentage MSA definition with the predictions from state-specific second-order time trends to smooth out jumps (compare Appendix Table B1), DAW right-to-carry dummy variable estimates for violent crime increased by 1.5 to 10.56 and spline estimates increased by 0.17 to 0.20.

[18] 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.

[19] 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.

less likely to be meaningfully predictive for other geographic areas.We should note, however, that including DC in the synthetic control analysis has little impact on our estimates showing that RTC laws increase violent crime.

We collected data on two separate police measures. 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 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. These figures were also included in the data set featured in Lott and Mustard (1997).[20] 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.

[20]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.

Appendix H: Replicating Our Analysis

In implementing the synthetic control methodology, we discovered that our estimates could be affected by seemingly inconsequential details when using maximum likelihood to select the weights associated with different predictors in our analysis. Specifically, when using the excellent "synth" package for Stata created by Abadie, Hainmueller, and Diamond along with the *nested* option, 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."[21]

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 whenever this occurs. In our analysis, we use the UNIX version of Stata/MP owing to the well-documented performance gains associated with this version of the software package.

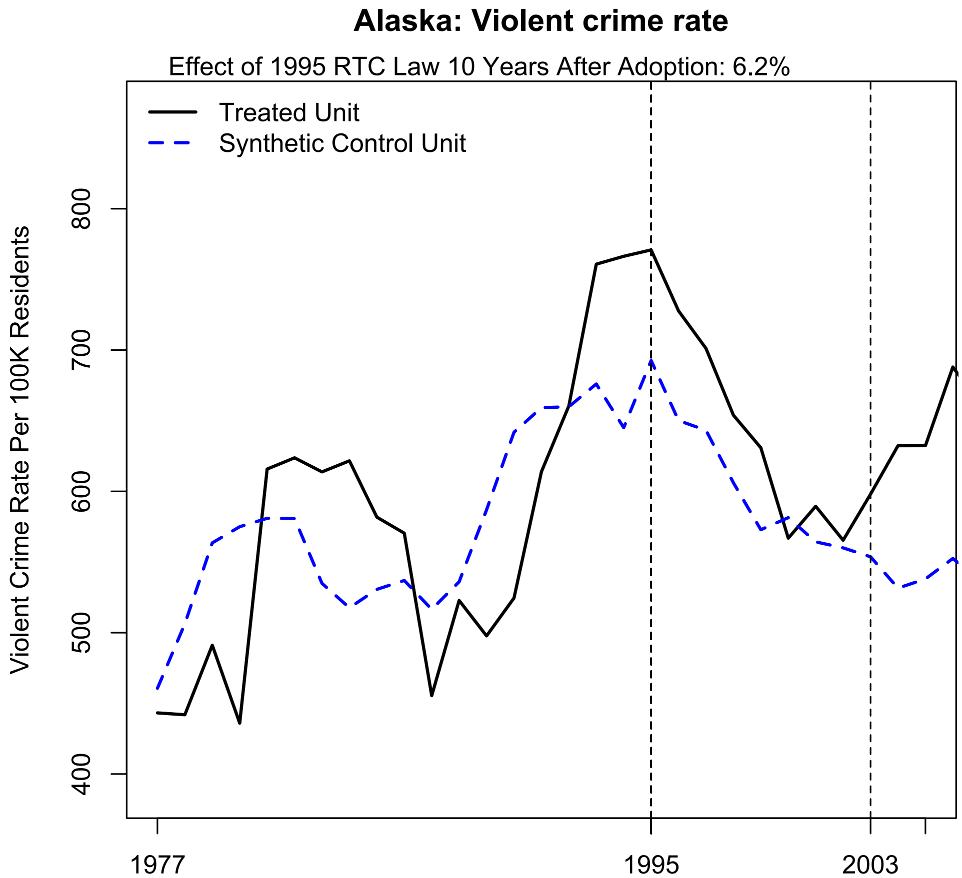
Another discrepancy that we encountered is that memory limitations sometimes caused our synthetic control analyses to crash when using the *nested* option. When this occurred, we would generate our synthetic control using the regression-based technique for determining the relative weights assigned to different predictors. We encountered this situation several times when running our Stata code on standard desktop computers, but this problem occurred less often when using more powerful computers with greater amounts of memory. For this reason, to replicate our results with the greatest amount of precision, we would recommend that other researchers run our code

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

on the same machines that we ran our own analysis: a 24-core UNIX machine with 96GB of RAM or a 16-core UNIX machine with 64GB of RAM running Stata/MP.

Appendix I: Synthetic Control Graphs Estimating Impact of RTC Laws On Violent Crime Using the DAW Model[22]

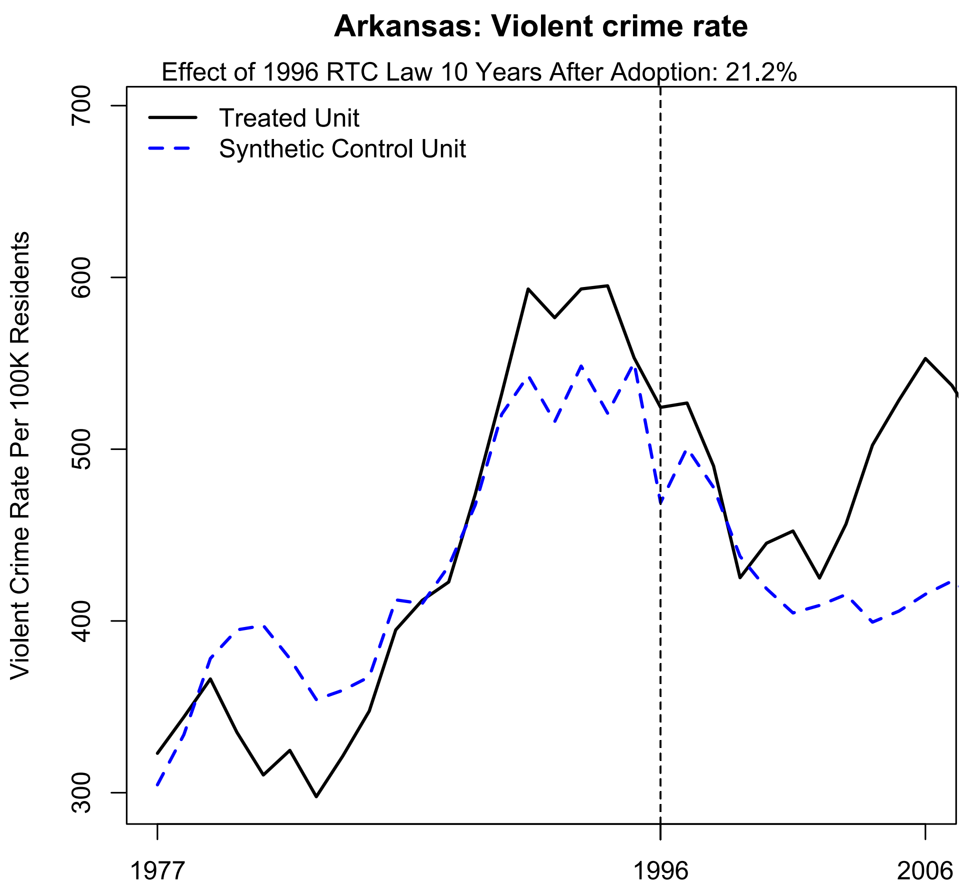
Figures I1-I33



Passage Year Difference From SC: 11.3%
Composition of SC: DE (0.25); HI (0.331); MD (0.419)
CVRMSPE: 0.133 (23 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MD
RTC Adopting States Included in Synthetic Control:

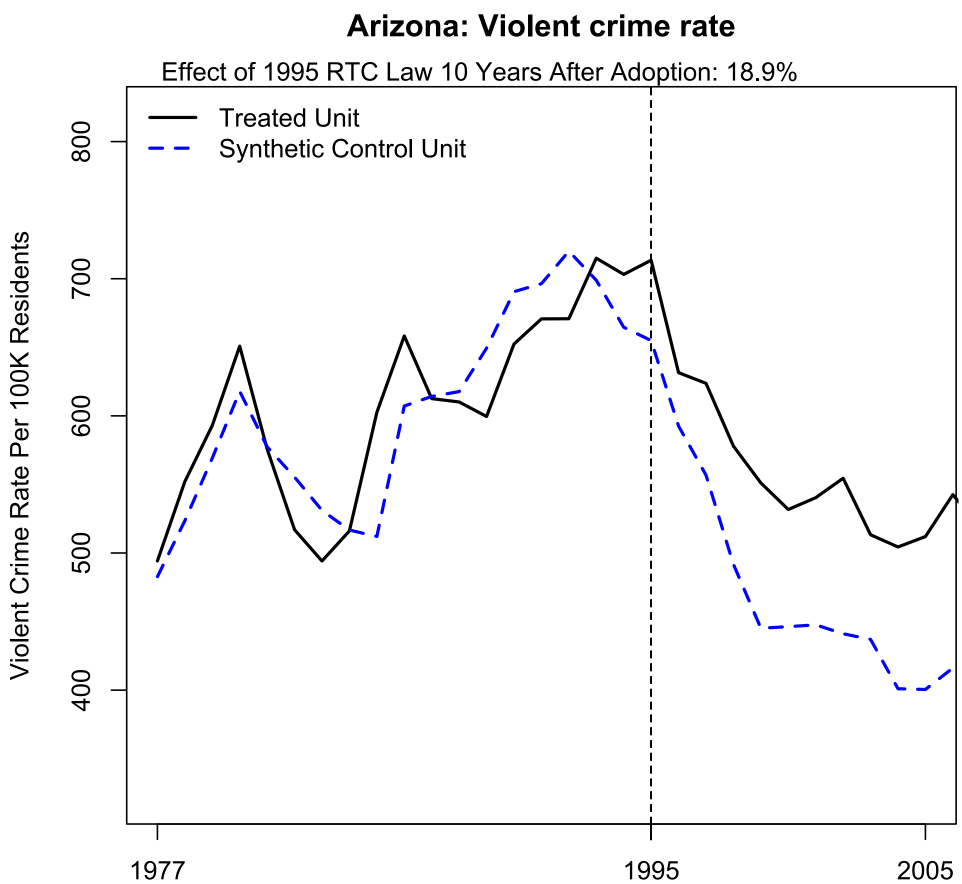
Figure I1

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



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

Figure I2



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

Figure I3

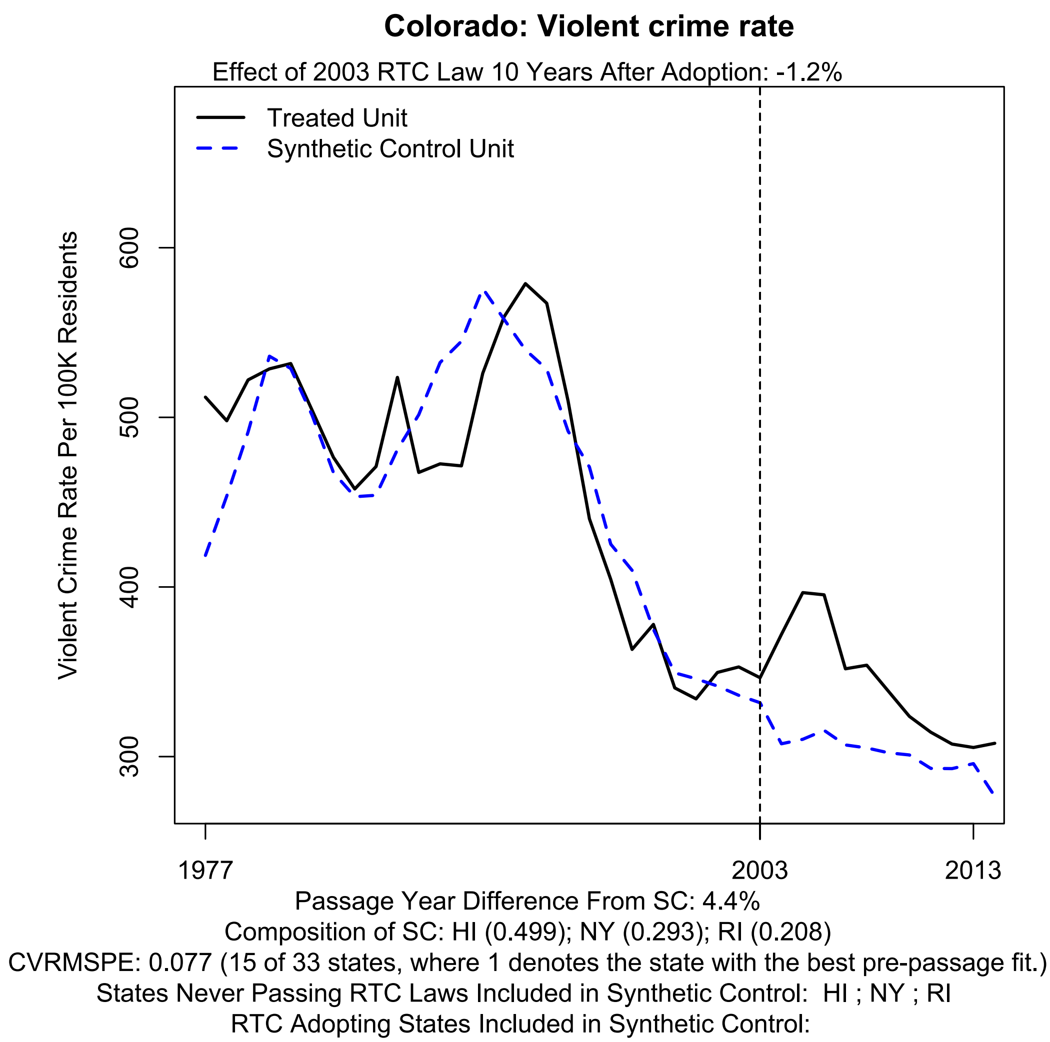
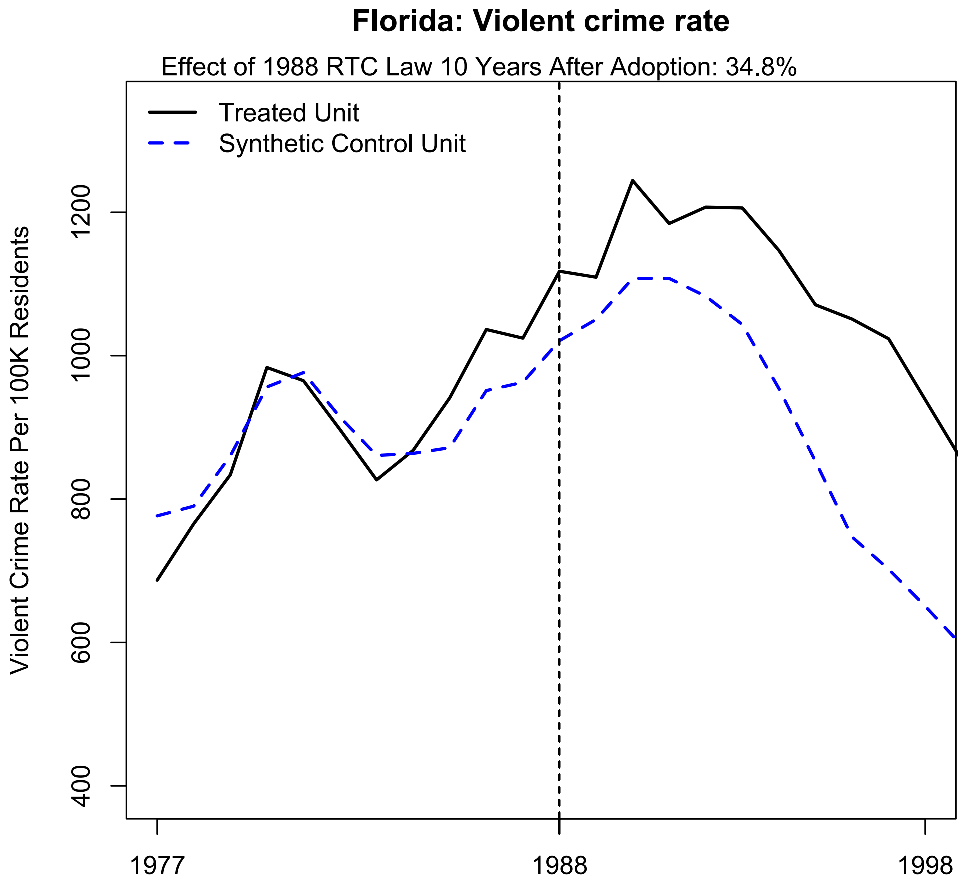
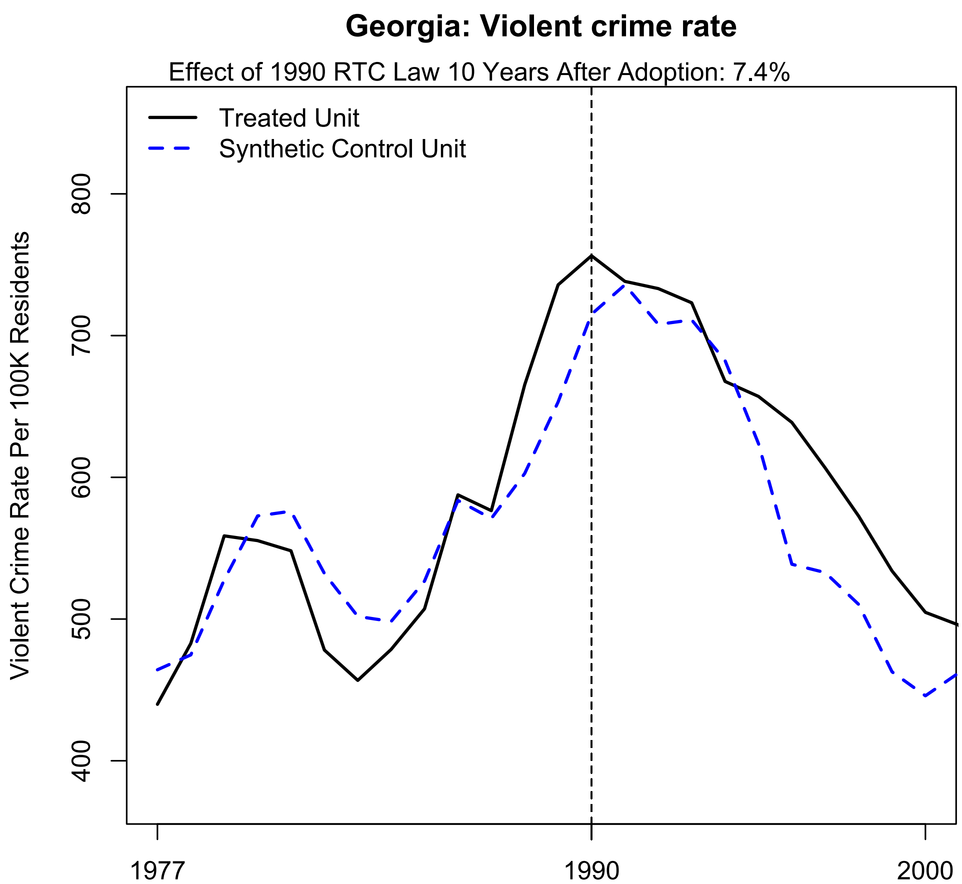

Colorado: Violent crime rate
Effect of 2003 RTC Law 10 Years After Adoption: -1.2%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
300
400
500
600
1977
2003
2013
Passage Year Difference From SC: 4.4%
Composition of SC: HI (0.499); NY (0.293); RI (0.208)
CVRMSPE: 0.077 (15 of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

Figure I4



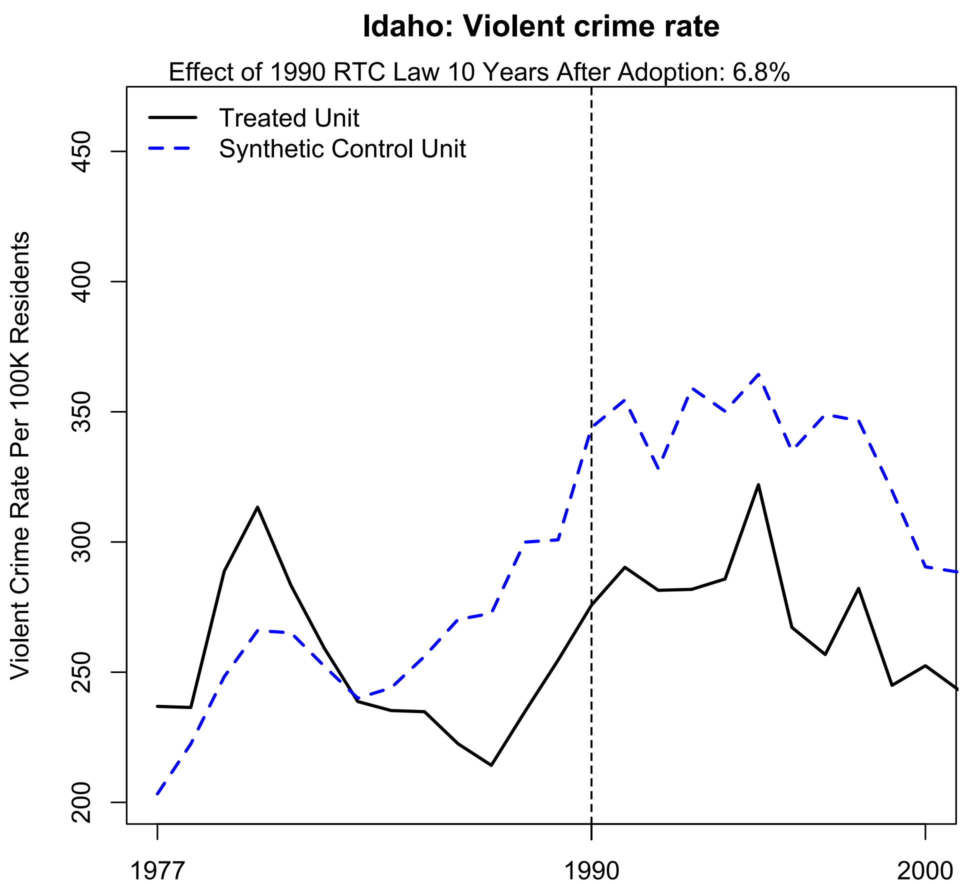
Passage Year Difference From SC: 9.5%
Composition of SC: CA (0.223); MI (0.11); NY (0.667)
CVRMSPE: 0.056 (7 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: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

Figure I5



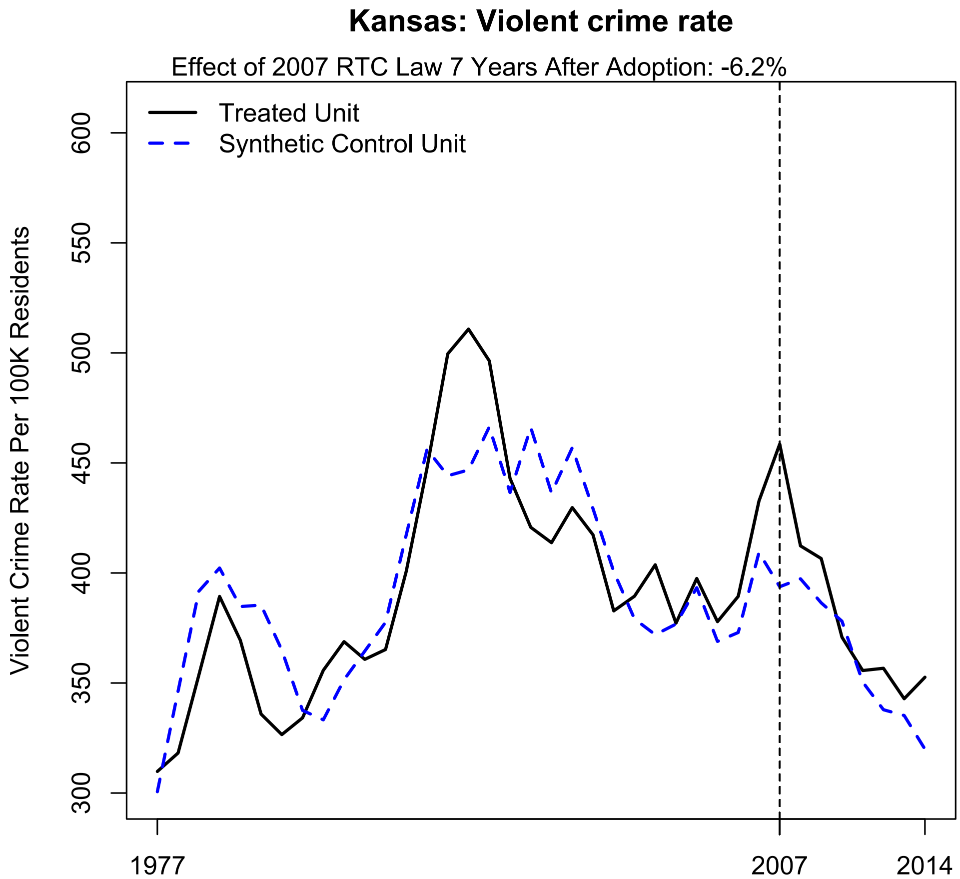
Passage Year Difference From SC: 5.8%
Composition of SC: IA (0.265); MO (0.499); NY (0.236)
CVRMSPE: 0.071 (13 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: NY
RTC Adopting States Included in Synthetic Control: IA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

Figure I6



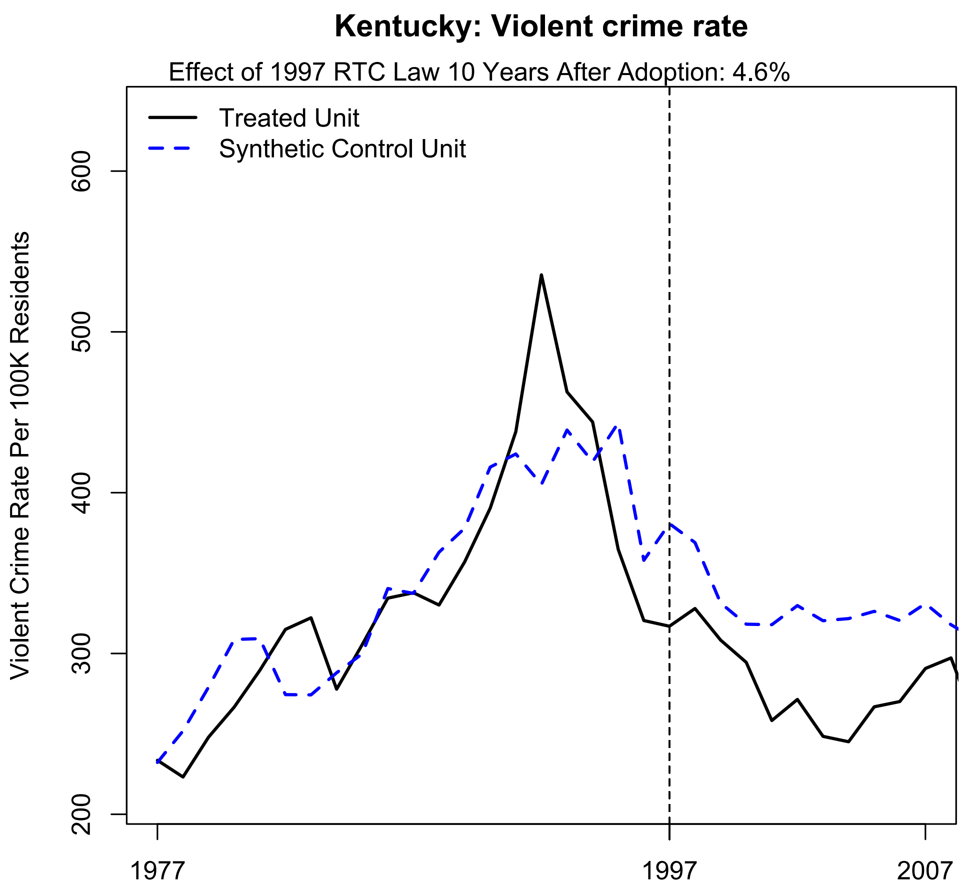
Passage Year Difference From SC: -19.8%
Composition of SC: HI (0.015); IA (0.45); NE (0.256); RI (0.278)
CVRMSPE: 0.149 (24 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States Included in Synthetic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

Figure I7



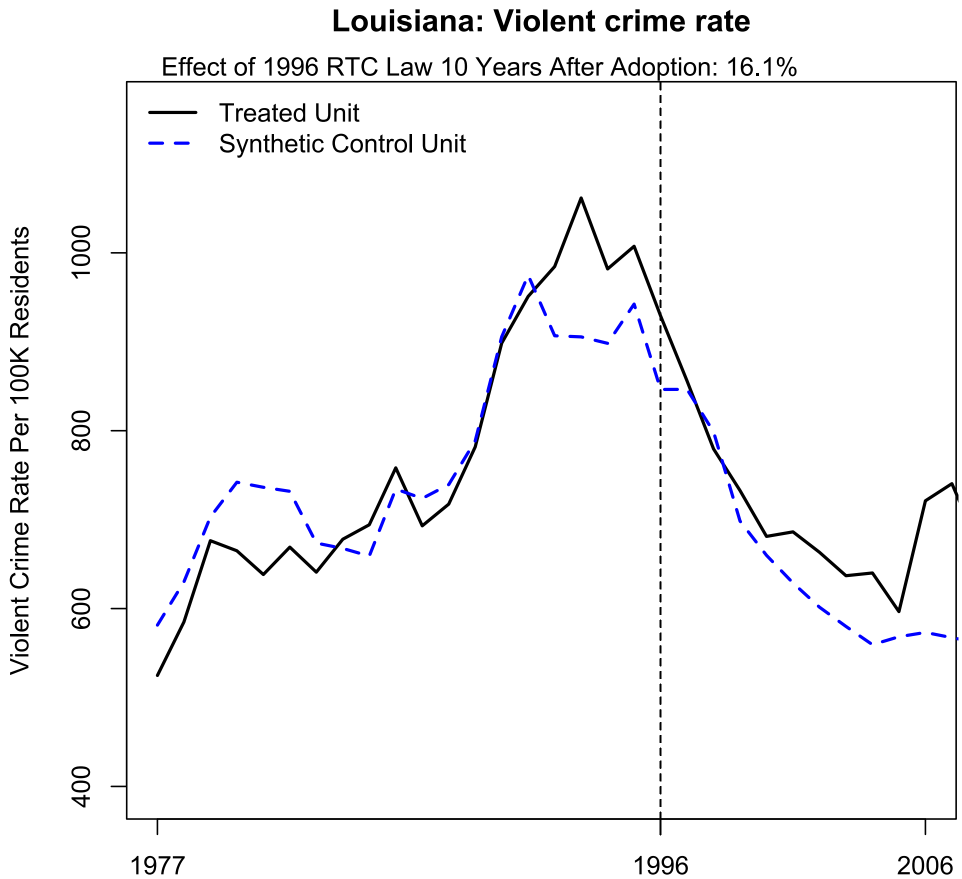
Passage Year Difference From SC: 16.5%
Composition of SC: DE (0.184); HI (0.583); MA (0.234)
CVRMSPE: 0.069 (12 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MA
RTC Adopting States Included in Synthetic Control:

Figure I8



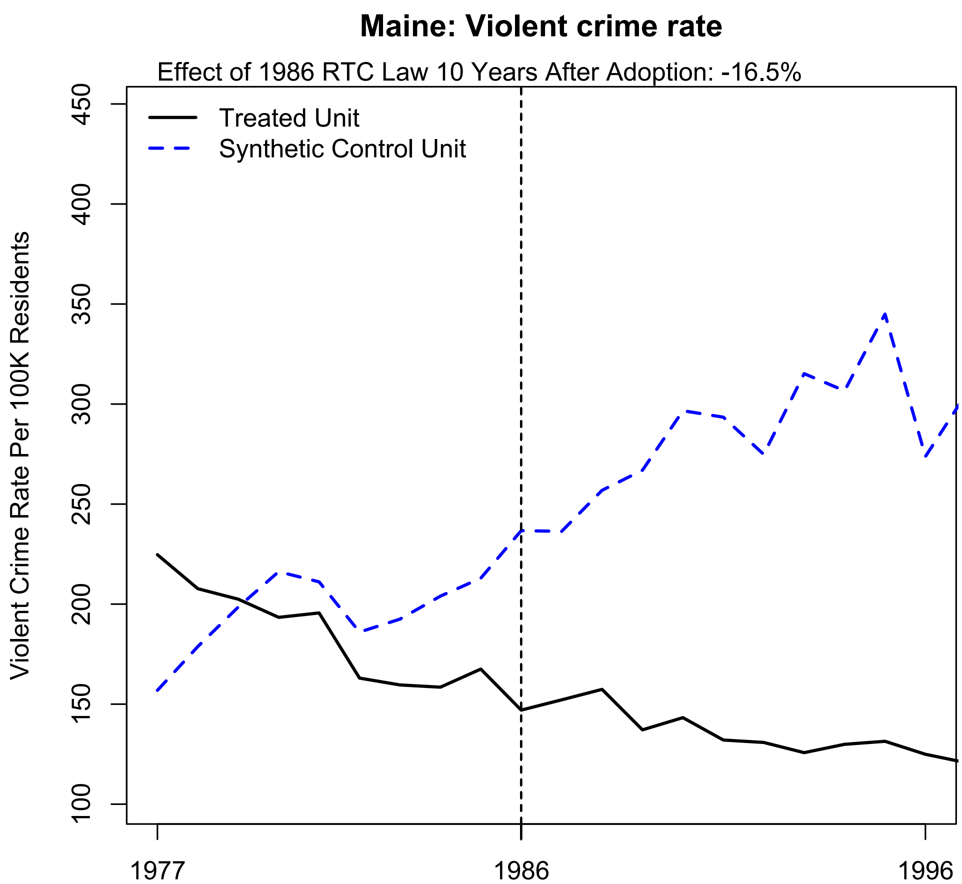
Passage Year Difference From SC: -16.8%
Composition of SC: CA (0.124); IA (0.849); NY (0.027)
CVRMSPE: 0.125 (22 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in Synthetic Control: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

Figure I9



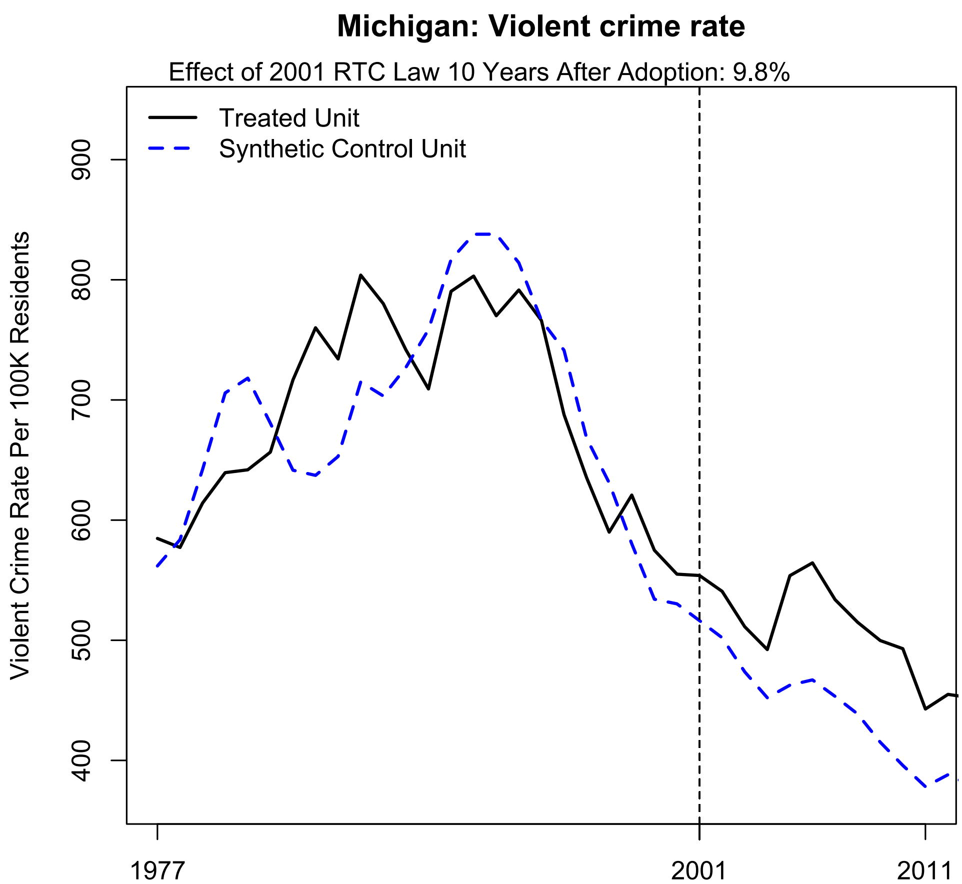
Passage Year Difference From SC: 9.8%
Composition of SC: DE (0.198); IL (0.802)
CVRMSPE: 0.08 (17 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014)

Figure I10



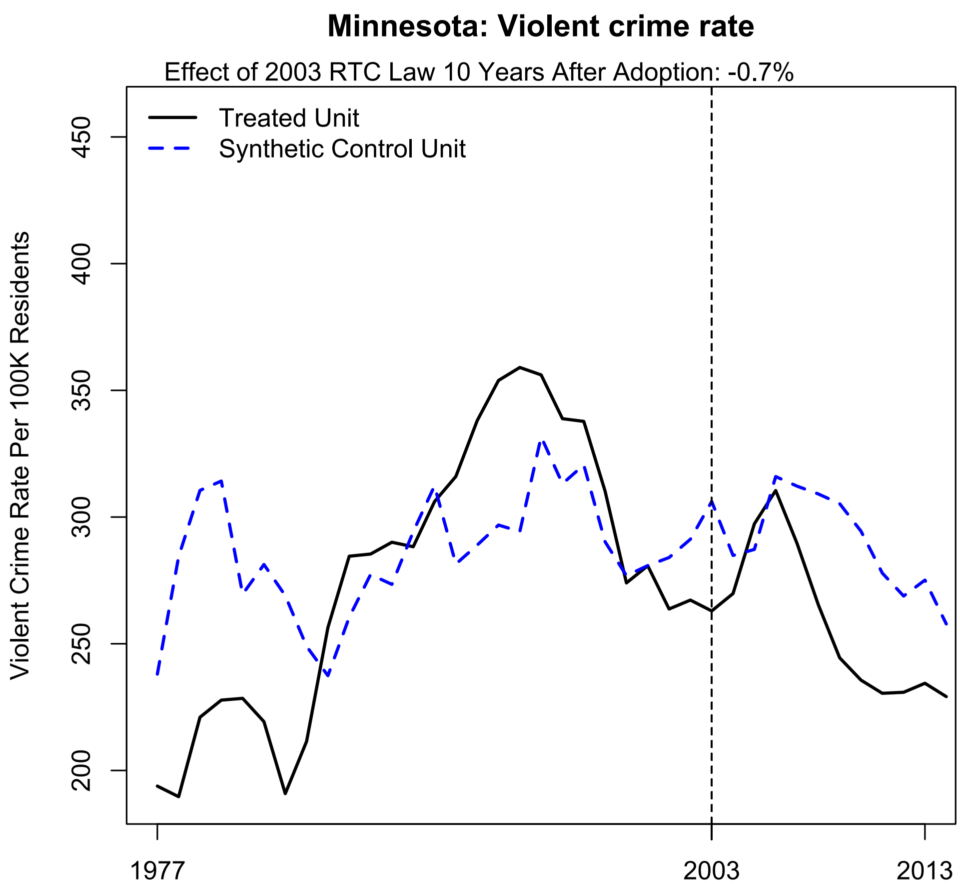
Passage Year Difference From SC: -37.9%
Composition of SC: HI (0.16); IA (0.84)
CVRMSPE: 0.196 (28 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: IA (2011)

Figure I11



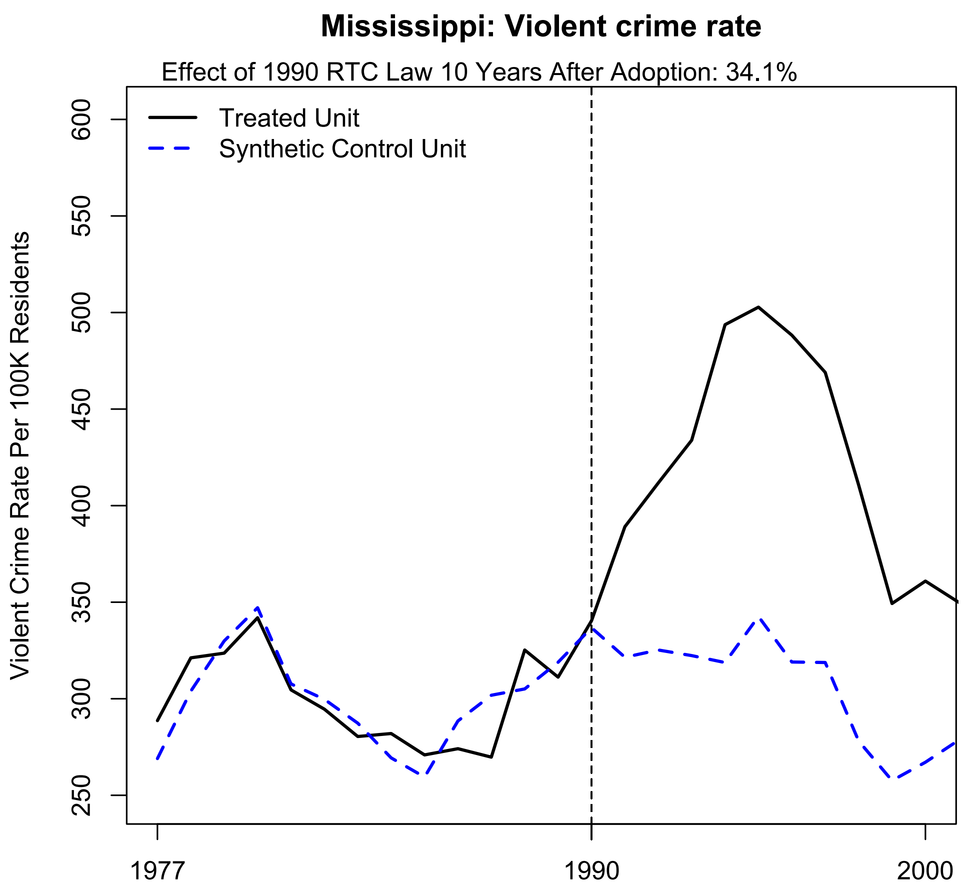
Passage Year Difference From SC: 7.3%
Composition of SC: CA (0.199); IL (0.054); MD (0.217); NY (0.231); RI (0.031); WI (0.268)
CVRMSPE: 0.08 (16 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; MD ; NY ; RI
RTC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

Figure I12



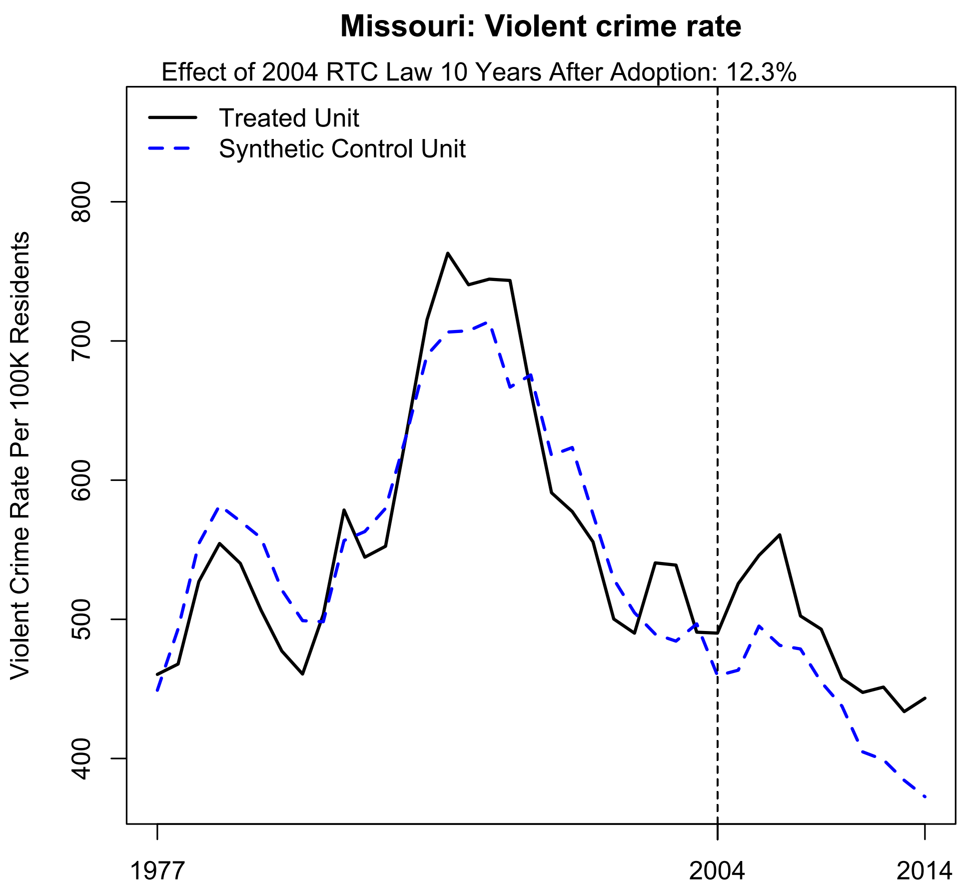
Passage Year Difference From SC: -14.1%
Composition of SC: DE (0.084); HI (0.916)
CVRMSPE: 0.166 (26 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic Control:

Figure I13



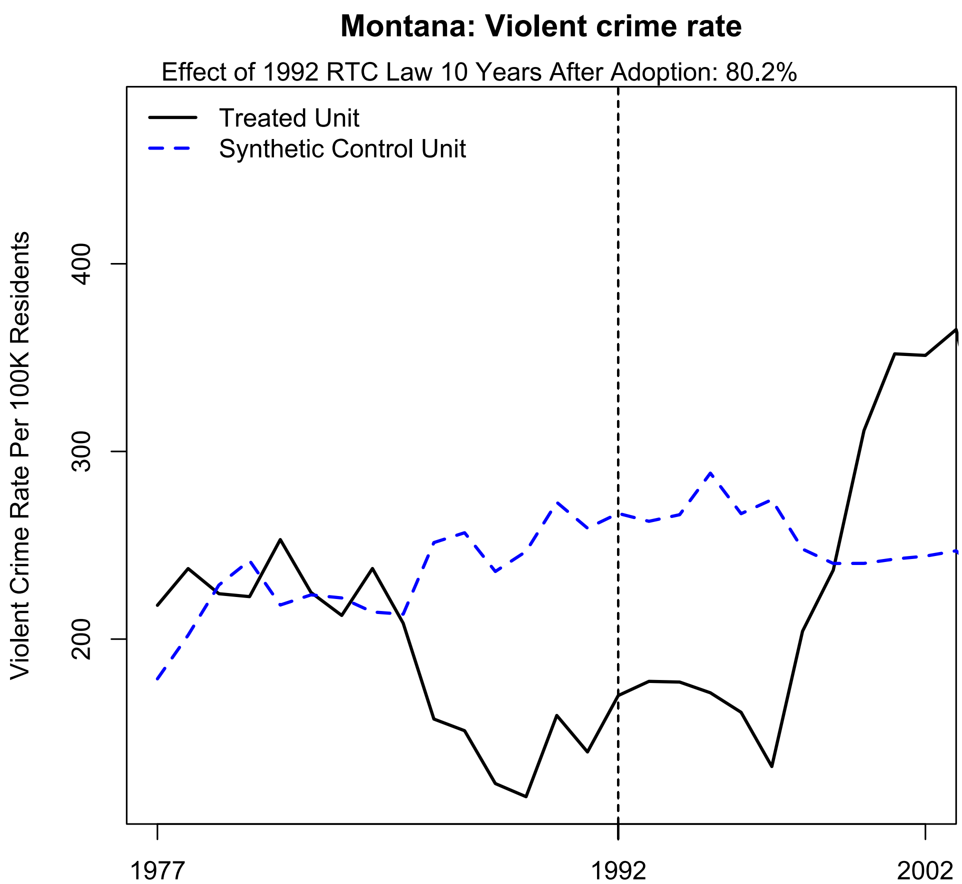
Passage Year Difference From SC: 1.1%
Composition of SC: HI (0.741); NE (0.014); OH (0.245)
CVRMSPE: 0.049 (5 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: NE (2007);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

Figure I14



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

Figure I15



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

Figure I16

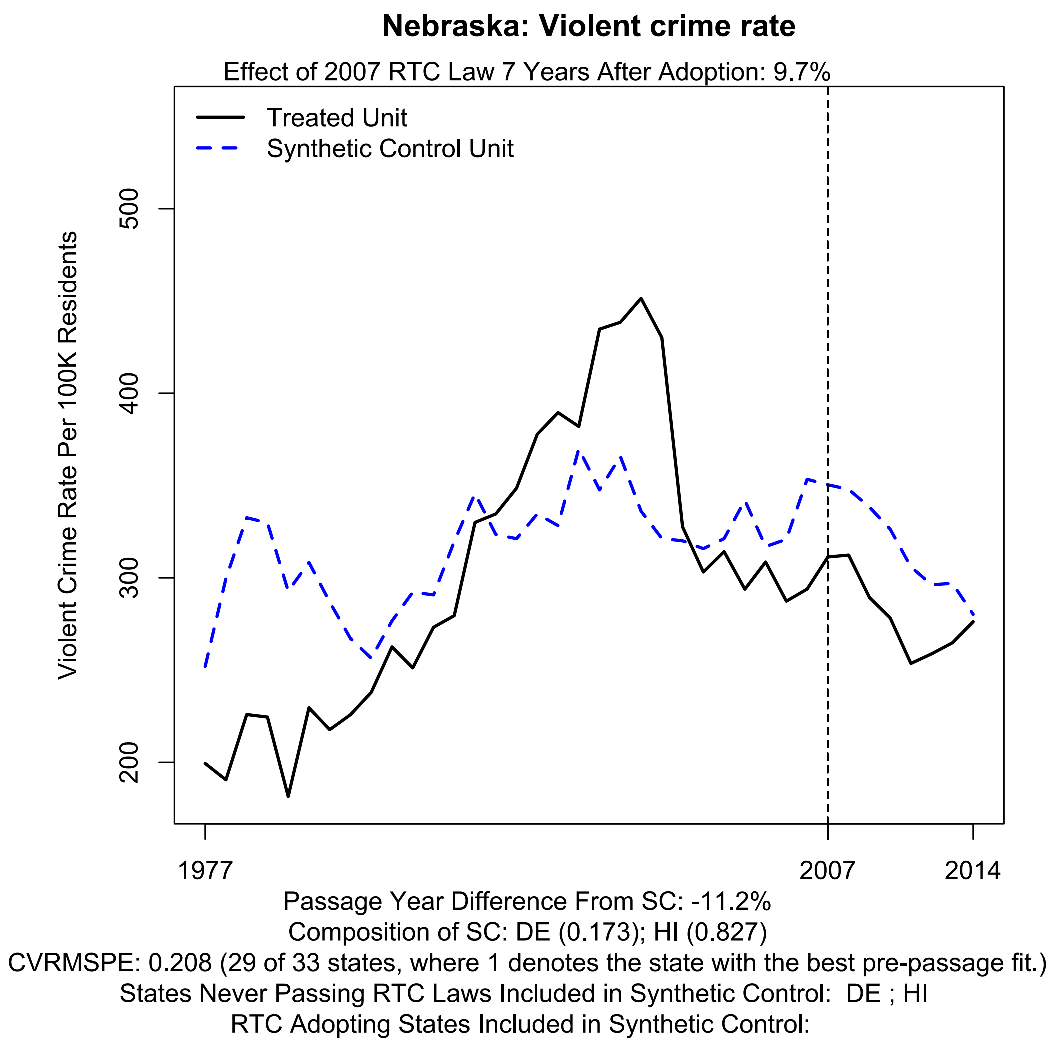
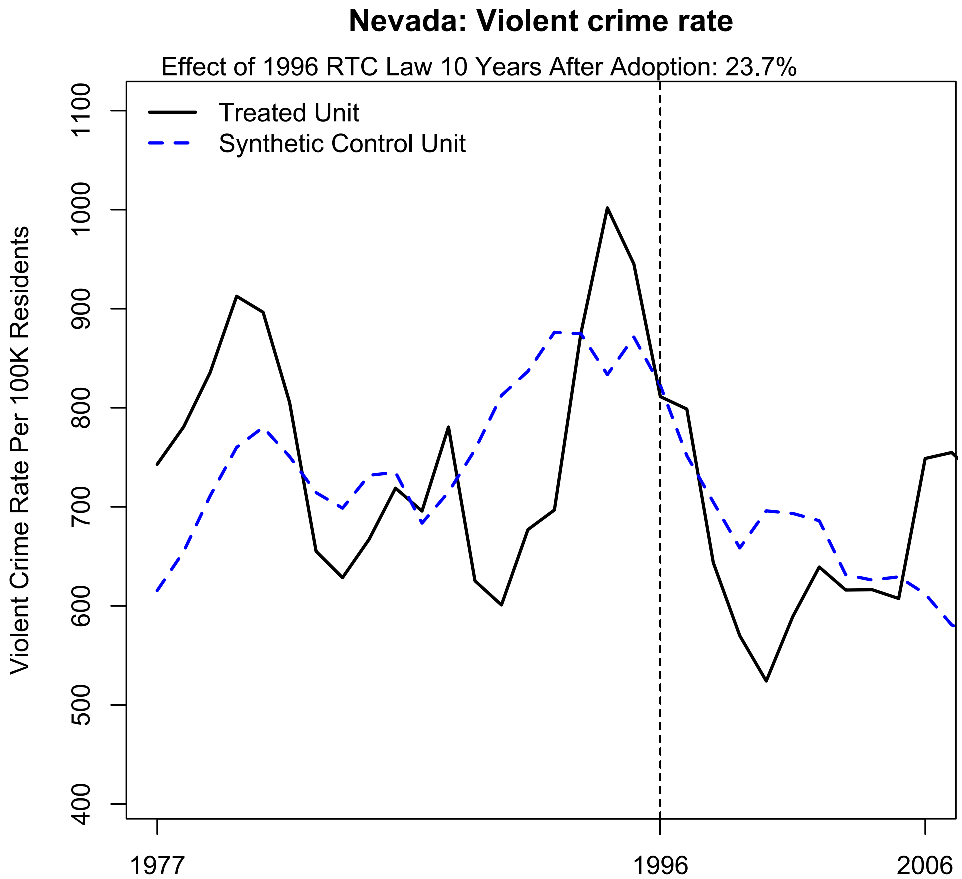

Nebraska: Violent crime rate
Effect of 2007 RTC Law 7 Years After Adoption: 9.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
200
300
400
500
1977
2007
2014
Passage Year Difference From SC: -11.2%
Composition of SC: DE (0.173); HI (0.827)
CVRMSPE: 0.208 (29 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic Control:

Figure I17



Passage Year Difference From SC: -1.4%
Composition of SC: HI (0.167); MD (0.833)
CVRMSPE: 0.152 (25 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; MD
RTC Adopting States Included in Synthetic Control:

Figure I18

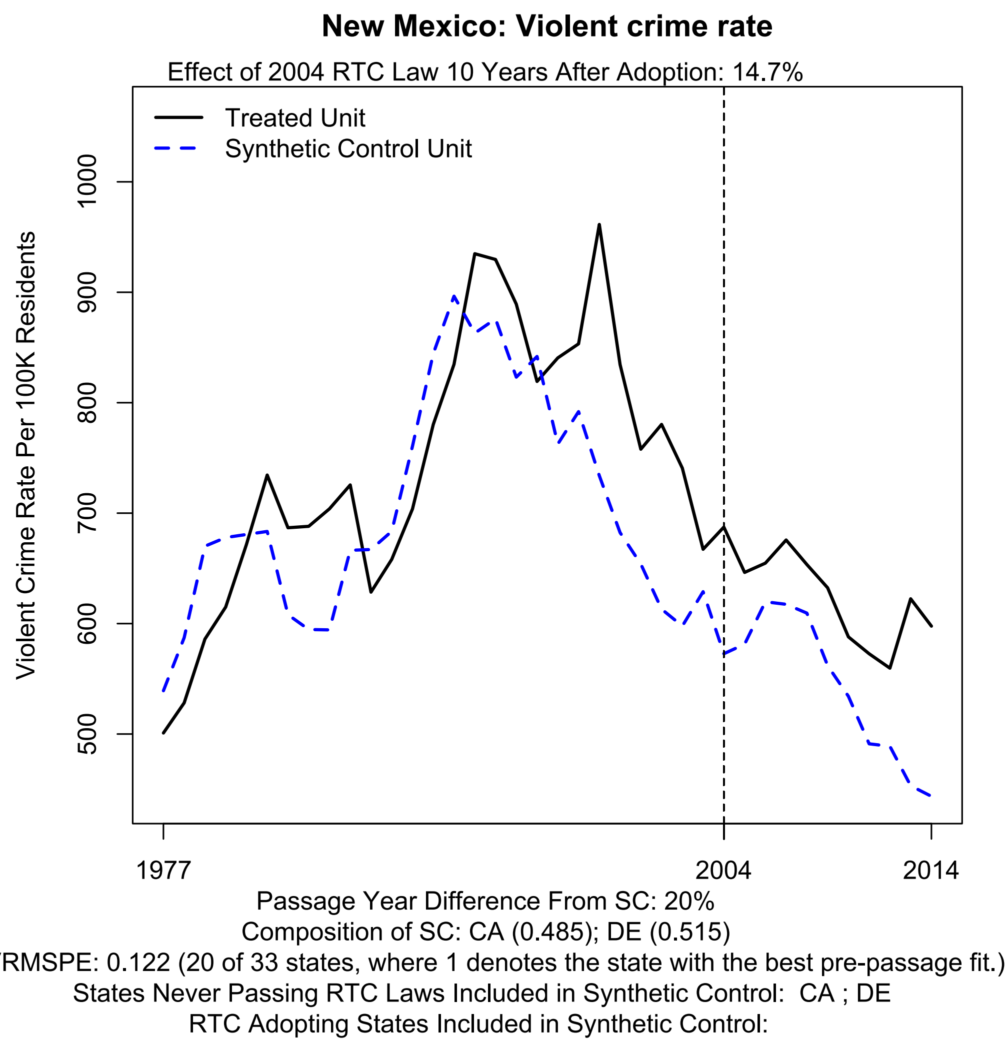

New Mexico: Violent crime rate
Effect of 2004 RTC Law 10 Years After Adoption: 14.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
1000
900
800
700
600
500
1977
2004
2014
Passage Year Difference From SC: 20%
Composition of SC: CA (0.485); DE (0.515)
CVRMSPE: 0.122 (20 of 33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

Figure I19

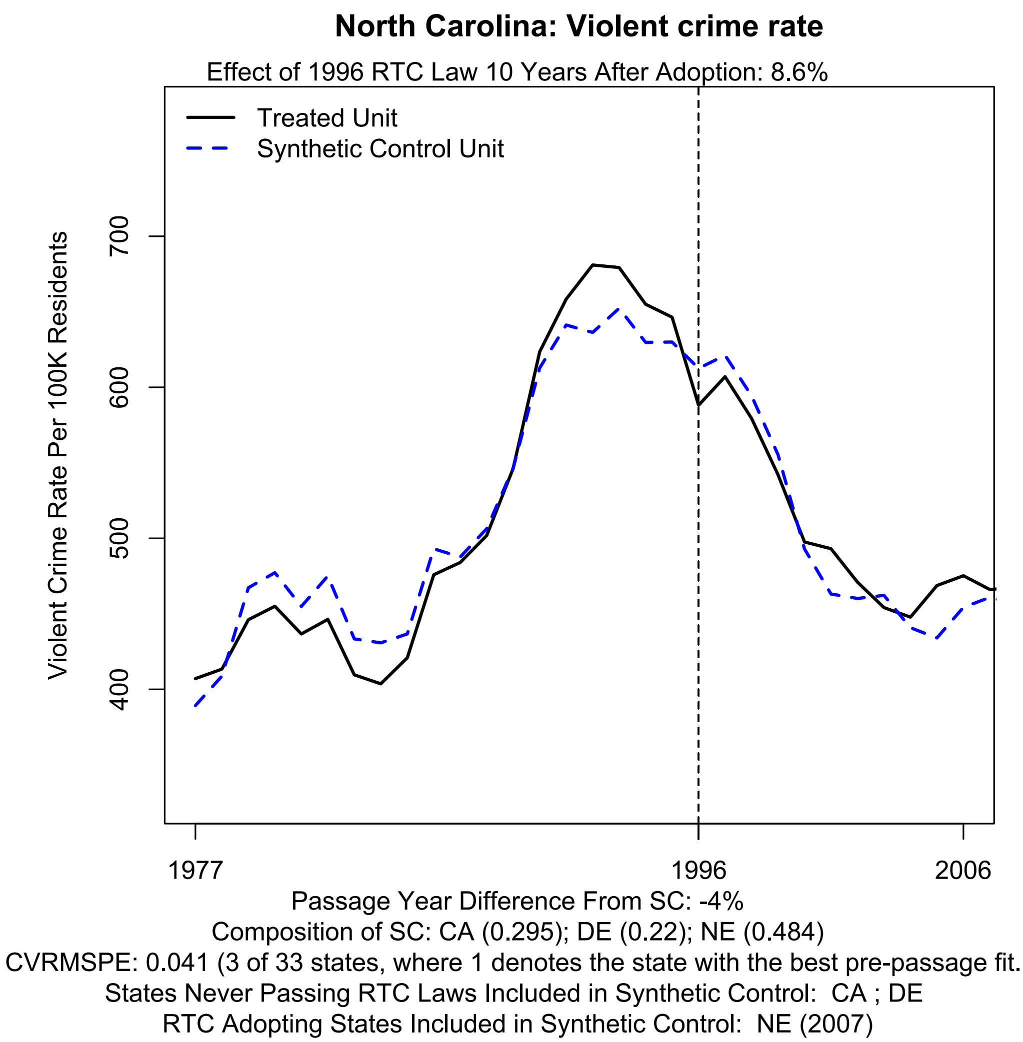
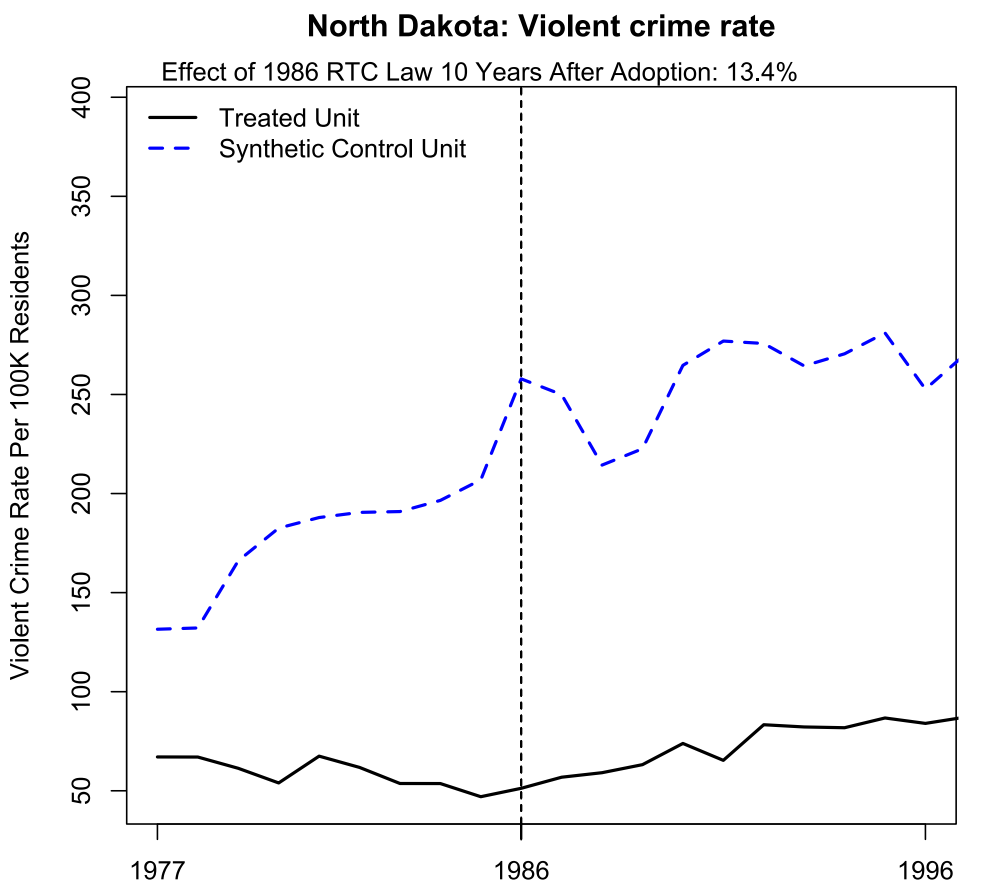
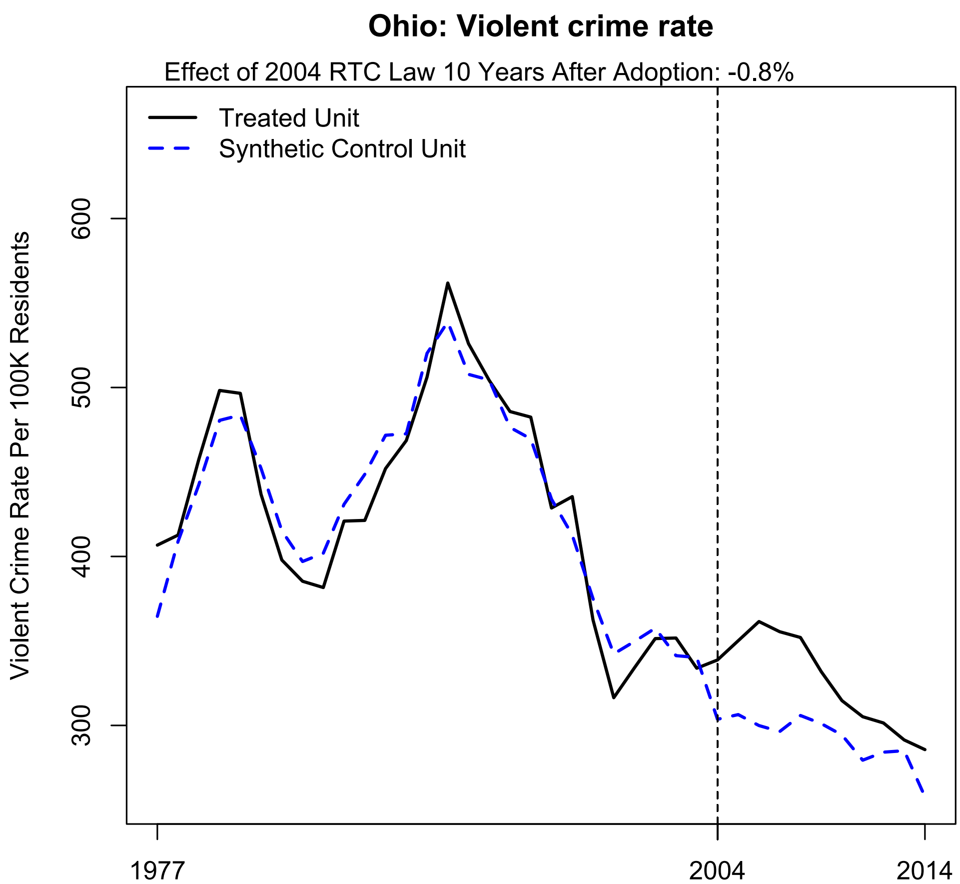

North Carolina: Violent crime rate
Effect of 1996 RTC Law 10 Years After Adoption: 8.6%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
400
500
600
700
1977
1996
2006
Passage Year Difference From SC: -4%
Composition of SC: CA (0.295); DE (0.22); NE (0.484)
CVRMSPE: 0.041 (3 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic Control: NE (2007)

Figure I20



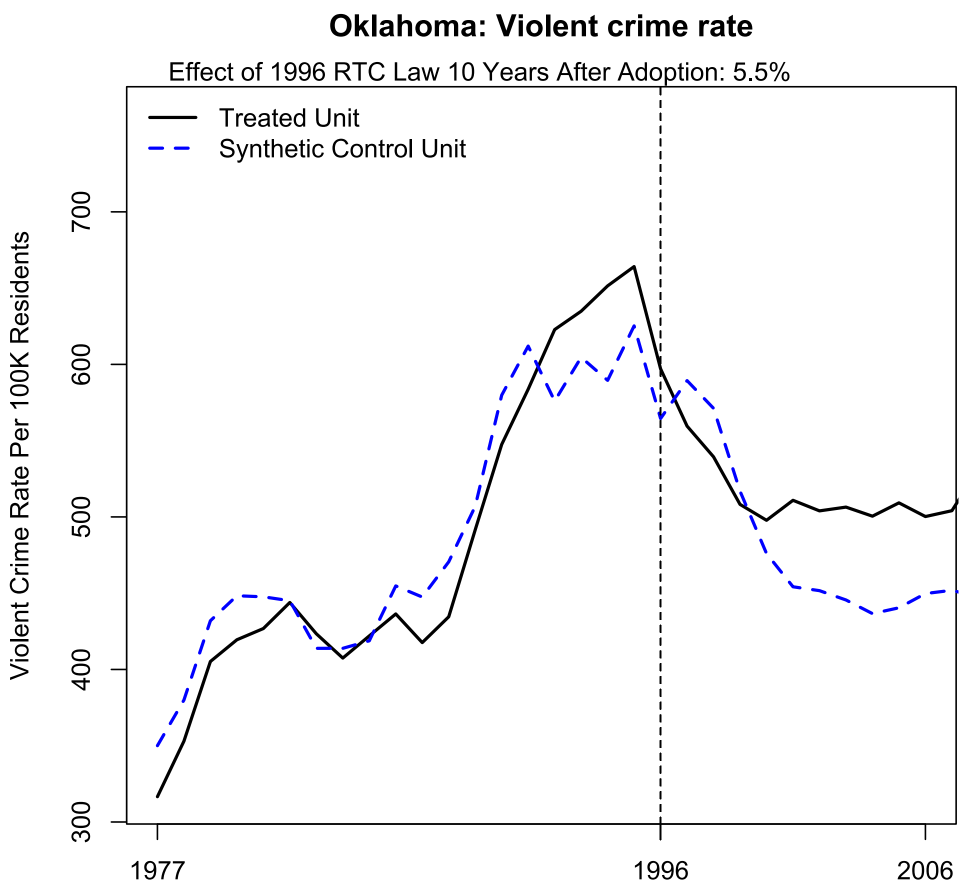
Passage Year Difference From SC: -80.1%
Composition of SC: WI (1)
CVRMSPE: 2.043 (33 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: WI (2012)

Figure I21



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

Figure I22



Passage Year Difference From SC: 5.8%
Composition of SC: DE (0.182); IL (0.259); IA (0.333); MD (0.026); MA (0.05); NE (0.15)
CVRMSPE: 0.063 (11 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; MA ; MD
RTC Adopting States Included in Synthetic Control: IA (2011); IL (2014); NE (2007)

Figure I23

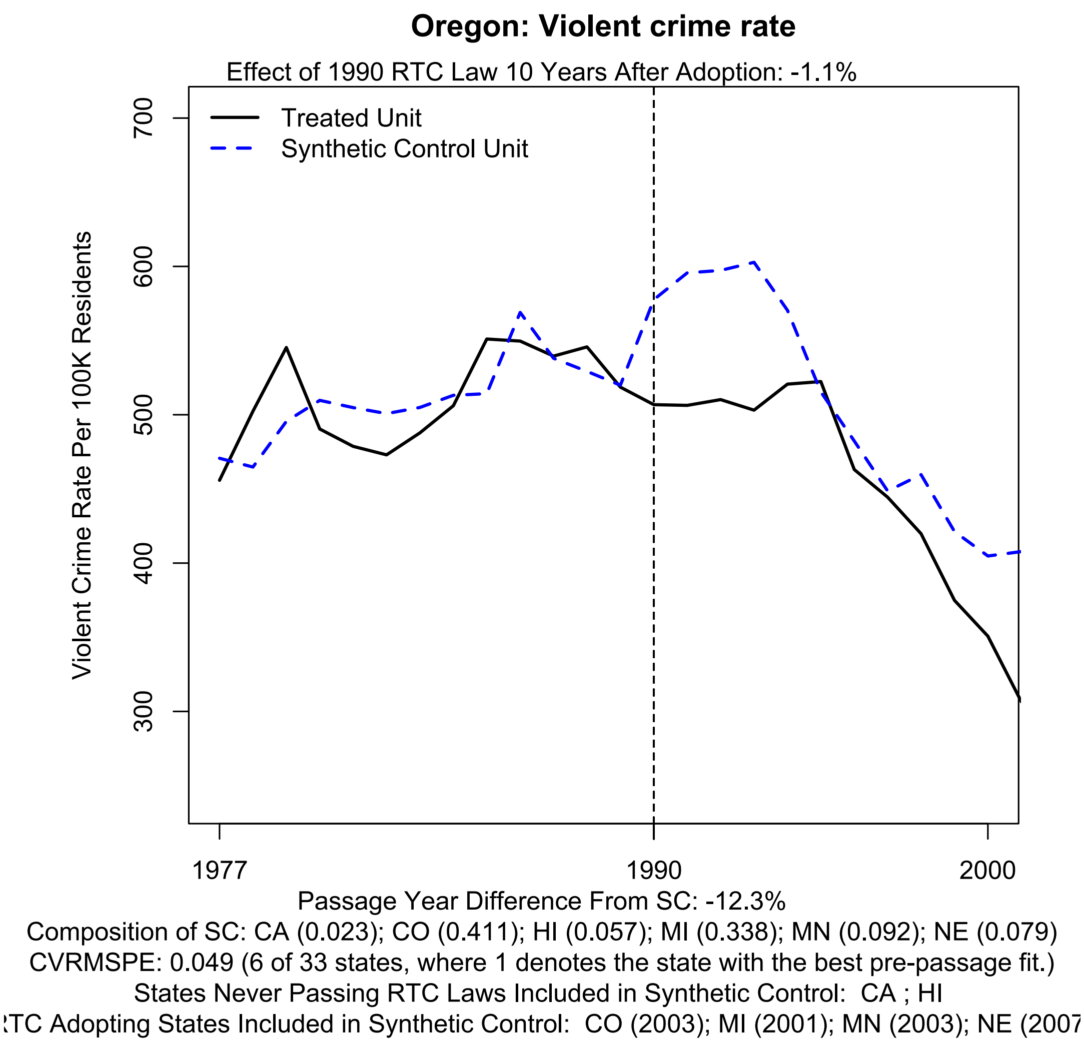

Oregon: Violent crime rate
Effect of 1990 RTC Law 10 Years After Adoption: -1.1%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
700
600
500
400
300
1977
1990
2000
Passage Year Difference From SC: -12.3%
Composition of SC: CA (0.023); CO (0.411); HI (0.057); MI (0.338); MN (0.092);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

Figure I24

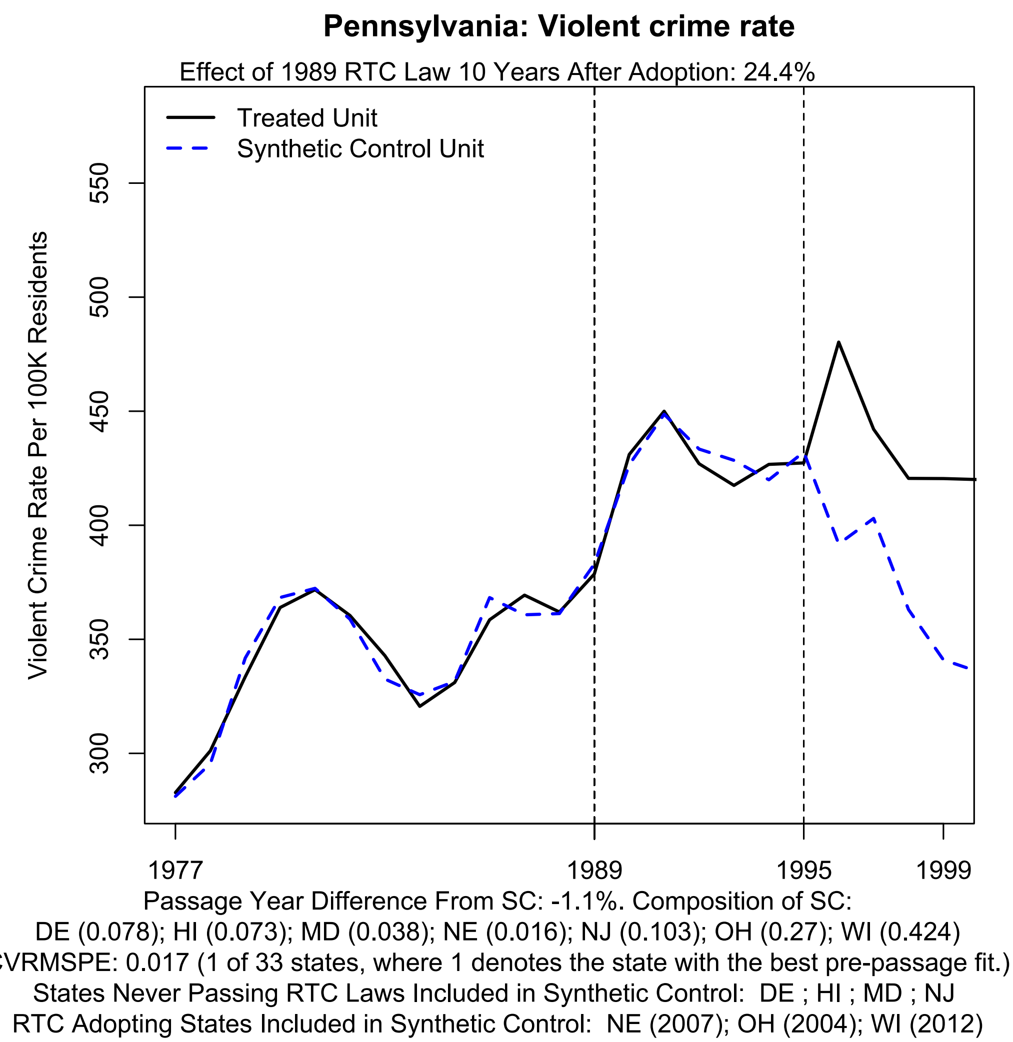
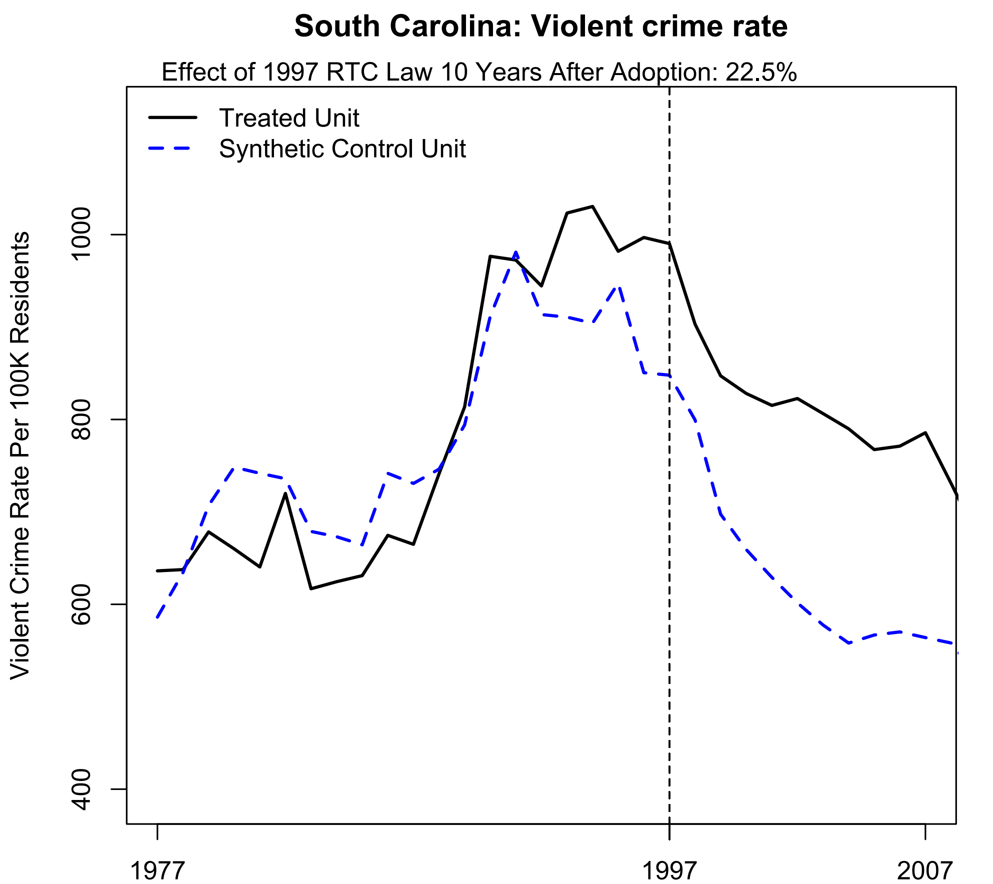
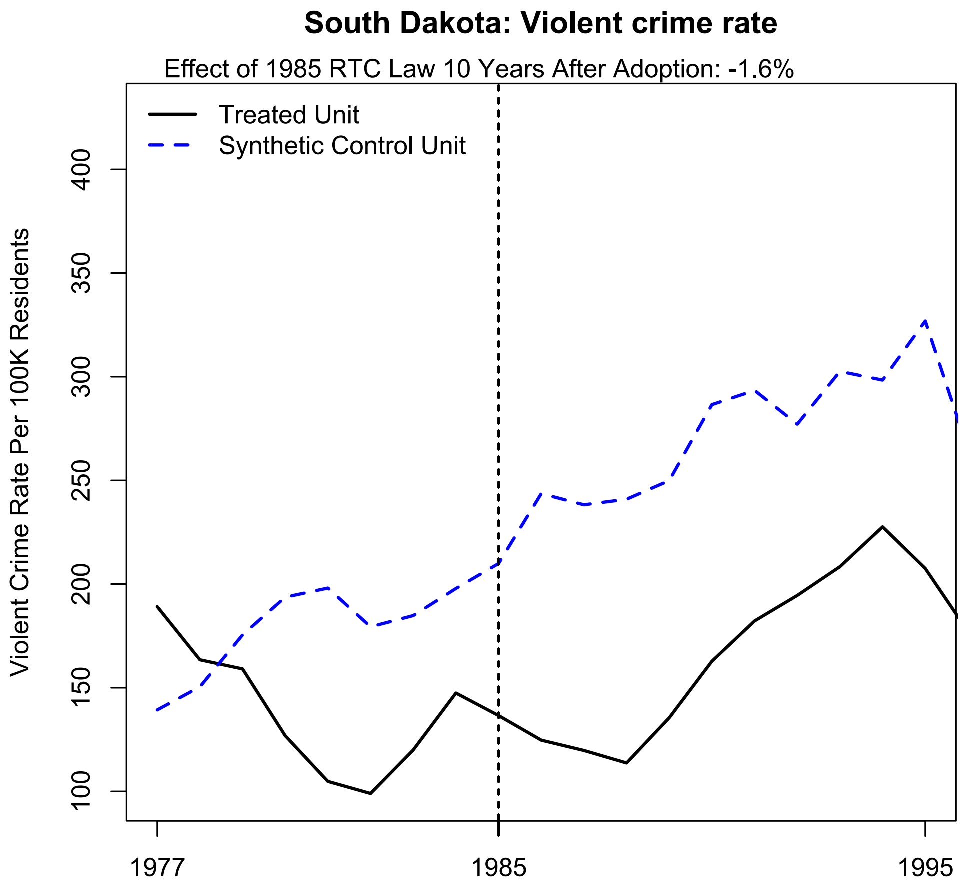

Pennsylvania: Violent crime rate
Effect of 1989 RTC Law 10 Years After Adoption: 24.4%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
550
500
450
400
350
300
1977
1989
1995
1999
Passage Year Difference From SC: -1.1%. Composition of SC:
DE (0.078); HI (0.073); MD (0.038); NE (0.016); NJ (0.103); OH (0.27); WI (0.424)
CVRMSPE: 0.017 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MD ; NJ
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004); WI (2012)

Figure I25



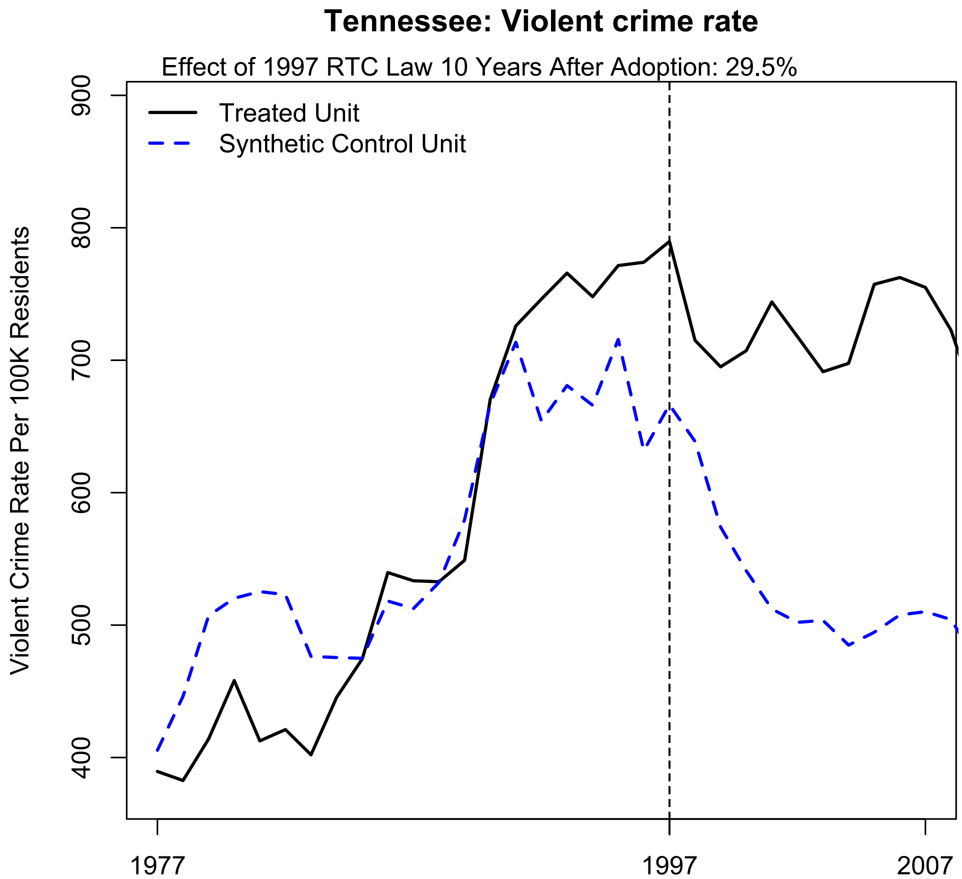
Passage Year Difference From SC: 16.8%
Composition of SC: DE (0.179); IL (0.821)
CVRMSPE: 0.088 (18 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014)

Figure I26



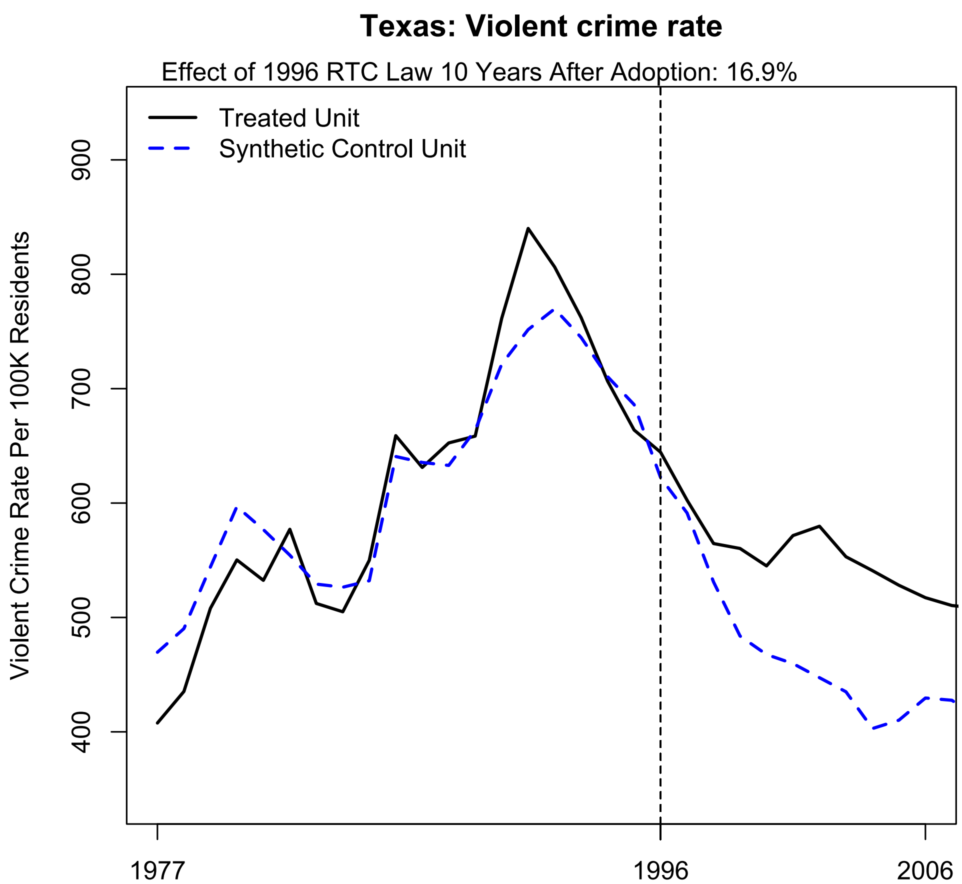
Passage Year Difference From SC: -34.9%
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: IA (2011); WI (2012)

Figure I27



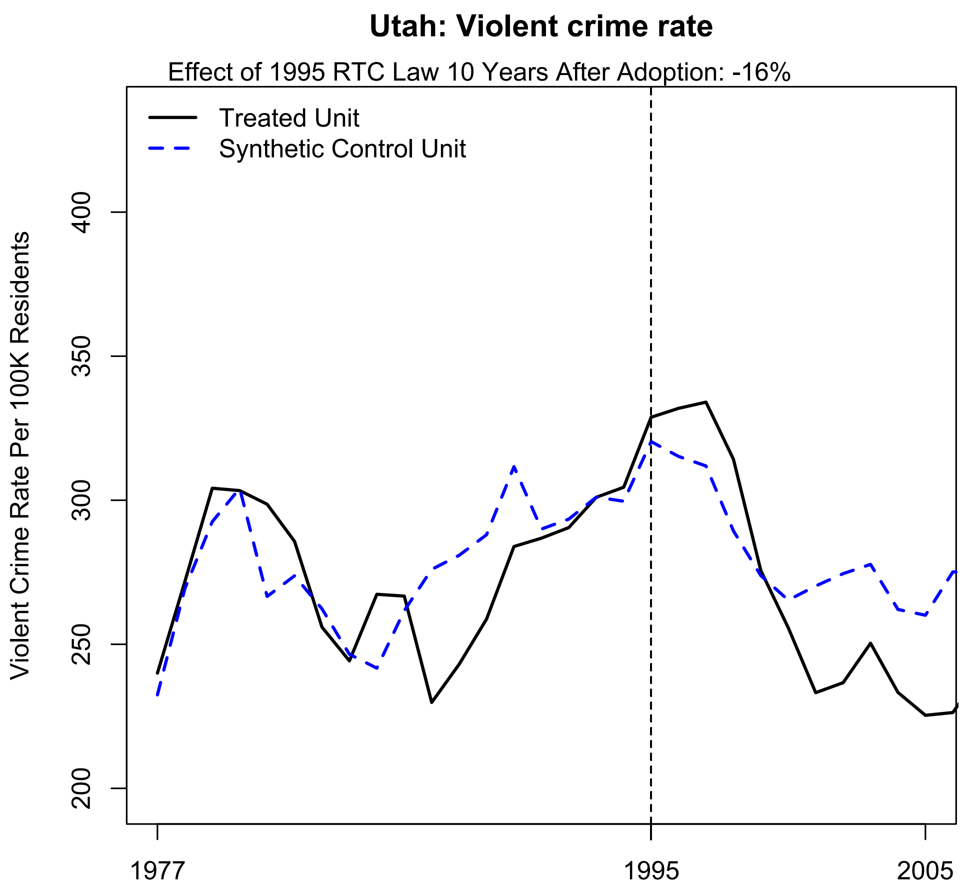
Passage Year Difference From SC: 18.5%
Composition of SC: DE (0.291); IL (0.395); IA (0.314)
CVRMSPE: 0.123 (21 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IA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

Figure I28



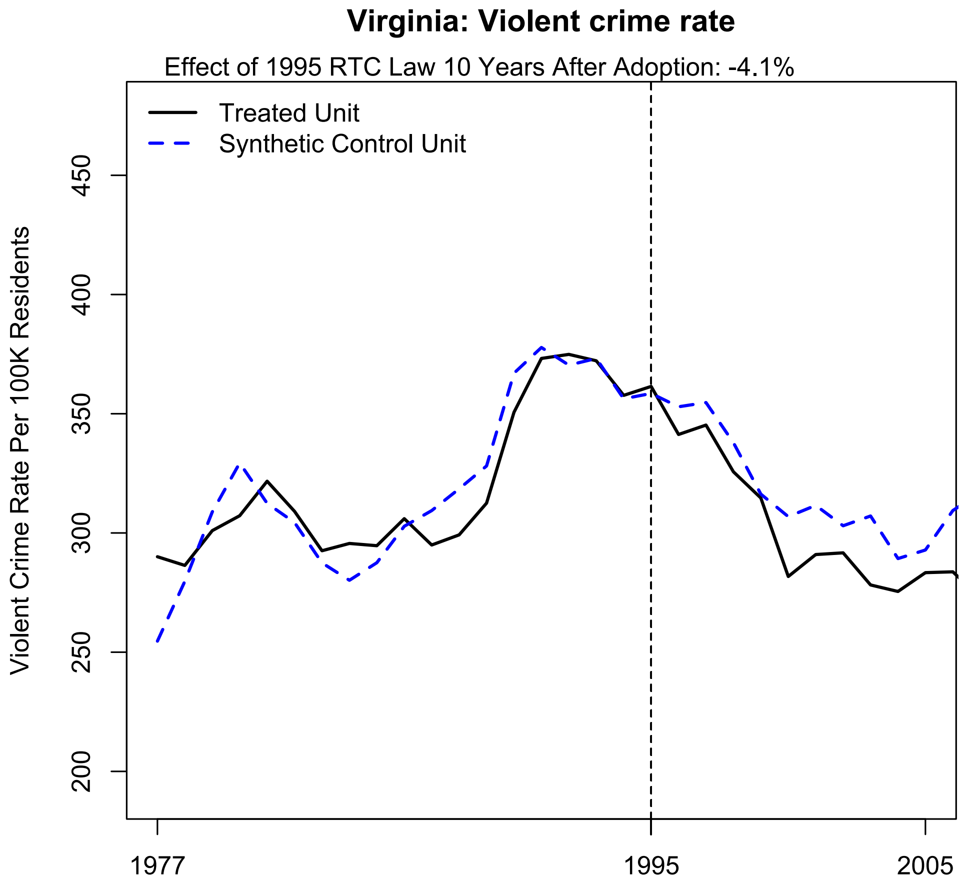
Passage Year Difference From SC: 3.6%
Composition of SC: CA (0.577); NE (0.097); WI (0.326)
CVRMSPE: 0.06 (8 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC Adopting States Included in Synthetic Control: NE (2007);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

Figure I29



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

Figure I30



Passage Year Difference From SC: 0.8%. Composition of SC:
DE (0.003); HI (0.279); KS (0.238); MD (0.003); NE (0.151); RI (0.246); WI (0.079)
CVRMSPE: 0.043 (4 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MD ; RI
RTC Adopting States Included in Synthetic Control: KS (2007); NE (2007); WI (2012)

Figure I31

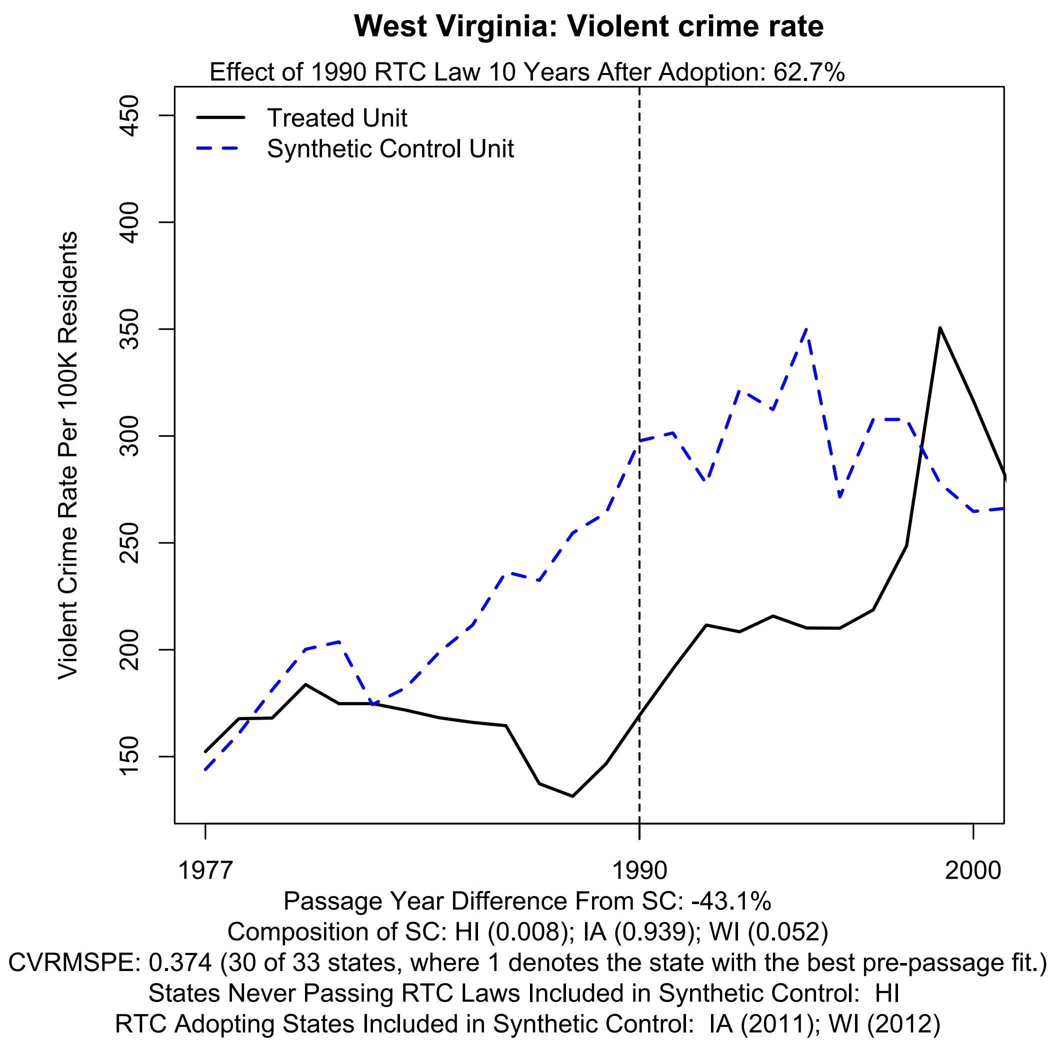

West Virginia: Violent crime rate
Effect of 1990 RTC Law 10 Years After Adoption: 62.7%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
150
200
250
300
350
400
450
1977
1990
2000
Passage Year Difference From SC: -43.1%
Composition of SC: HI (0.008); IA (0.939); WI (0.052)
CVRMSPE: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

Figure I32

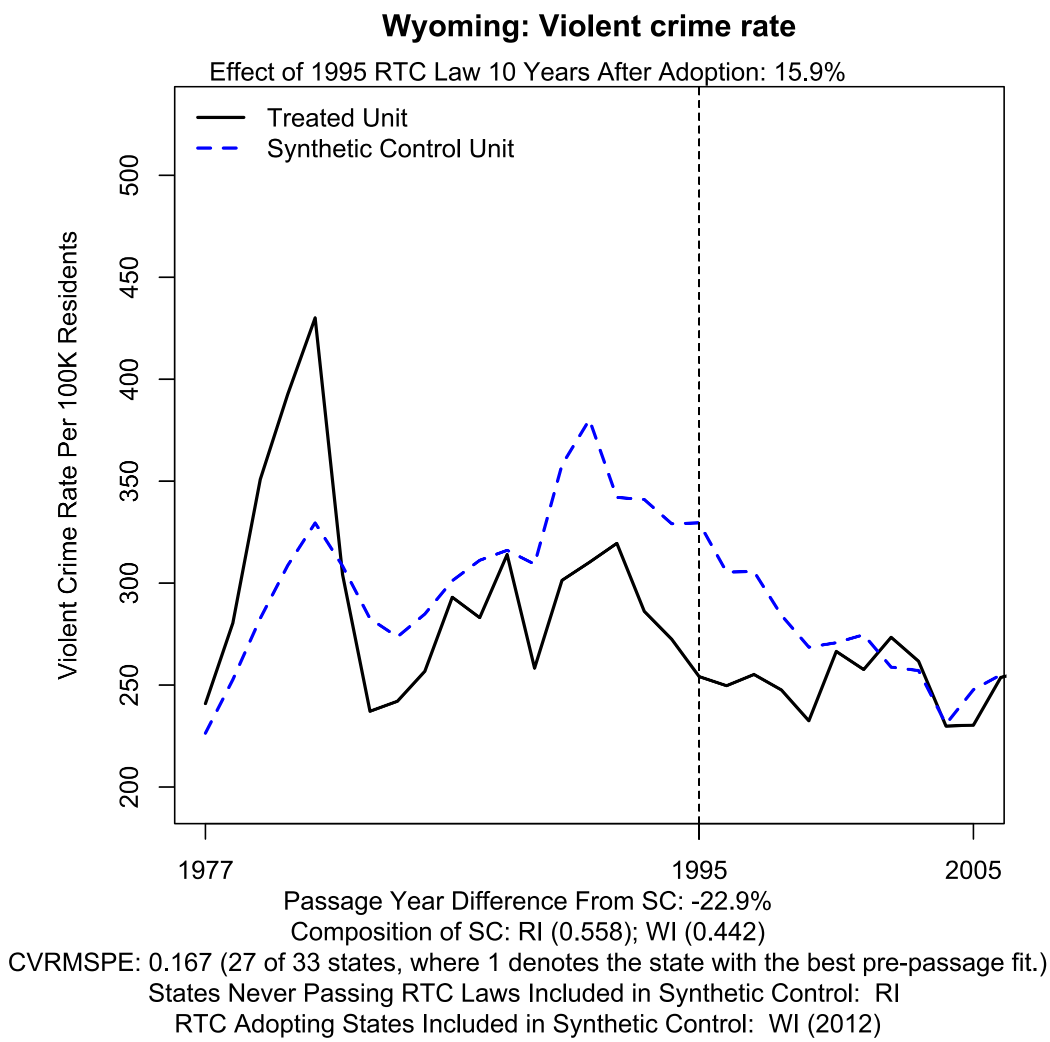

Wyoming: Violent crime rate
Effect of 1995 RTC Law 10 Years After Adoption: 15.9%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
200
250
300
350
400
450
500
1977
1995
2005
Passage Year Difference From SC: -22.9%
Composition of SC: RI (0.558); WI (0.442)
CVRMSPE: 0.167 (27 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: RI
RTC Adopting States Included in Synthetic Control: WI (2012)

Figure I33

Appendix J: Data Sources

Variable(s)	*Years Available*	*Source*	*Model(s)*	*Notes*
RTC variables (*shalll*)	1977-2014	State session laws	DAW, LM	Statutes researched via Westlaw and HeinOnline.
Crime	1977-2014	FBI/NVSS	DAW, LM	For violent and property: UCR Data Tool for data through 2013; Table 4 of 2015 crime report for data in 2014. For murder, firearm murder and non-firearm murder: CDC WISQARS for data through 2014. Each crime rate is the corresponding crime count, divided by the population metric used by the UCR or NVSS respectively, times 100,000.
Police staffing	1977-2014	FBI	DAW	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.
Population	1977-2014	Census	DAW, LM	Intercensal estimates are used, except in 1970 and 1980, for which decadal-census estimates are used.The DAW model weights regressions by population; the LM also includes it as a covariate.
Population by age, sex, and race	1977-2014	Census	DAW, LM	Intercensal estimates are used.
Income metrics	1977-2014	BEA	DAW, LM	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, LM	CPI varies by year but not by state.
Incarceration	1977-2014	BJS	DAW	The number of prisoners under the jurisdiction of a state as a percentage of its intercensal population.
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	The Census directly reports the percentage of the population earning less than the poverty line.
Unemployment rate	1977-2014	BLS	DAW, LM	
Arrests	1977-2014	FBI	LM	Agency-month-year-level arrests data, separated by age, sex, race, and crime category, were acquired from the FBI and aggregated by state-year. For each crime category, the arrest rate is the number of arrests for that crime as a percentage of the (UCR-reported) number of crimes.
Beer	1977-2015	NIH	DAW	The NIH reports per-capita consumption of ethanol broken down by beverage type, including beer.
Population in metropolitan statistical areas	1977-2014	Census / NBER, FBI / ICPSR	DAW	1977-1980: Intercensal estimated population in counties that at least overlapped with an MSA in 1980. 1981-2014: Obtained from ICPSR-provided UCR arrests data.

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

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

The prior synthetic control literature has used five different approaches concerning the inclusion of the dependent variable in selecting the best synthetic control: 1) lags of the dependent variable in every pre-treatment year, 2) three lags of the dependent variable,[23] 3) the average of the dependent variable in the pre-treatment period, 4) the value of the dependent variable in the year prior to RTC adoption, and 5) no lags of the dependent variable.[24] To choose the optimal approach among these five options, we use the following cross-validation procedure with overall violent crime rate as the dependent variable: we first define our training period as 1977 through the sixth year prior to RTC adoption, the validation period as the fifth year prior to RTC adoption through one year prior to RTC adoption, and the full pre-treatment period as 1977 through one year prior to RTC adoption. We then use data from the training period to determine the composition of the synthetic control (essentially acting as if the RTC law were adopted five years earlier than it was). Specifically, for each of the 33 treatment units, we assign the treatment five years before the treatment actually occurred, and then run the synthetic control program using the standard DAW predictors and a five year reporting window. We then examine the fit during the training period, the validation period, and the entire pre-treatment period to see how closely for each of our five lag options the synthetic control estimate matches each adopting state's violent crime time-series.

Note it is unsurprising that the DAW and LM synthetic control results with the yearly lag specification are very similar. According to Kaul et al. (2016), using all yearly lags essentially causes all other predictors to be discarded in the synthetic controls algorithm. The reason our DAW and LM results are not exactly the same is because including a different set of predictors for each model creates slightly different variable order for each specification. As we mentioned in Appendix H, the synthetic control methodology can generate slightly different results when even the order in which variables appears changes, so it is not surprising that adding different sets of (theoretically irrelevant) variables might also slightly change our findings as well. In the body of our paper, Table 5 reports the DAW violent crime synthetic controls estimates with yearly lags, while Table 6 reports the LM violent crime synthetic controls estimates with three lags. Tables K1 and K2 report analagous tables with the alternative lag choice (DAW with three lags and LM with

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

[24]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).

yearly lags). An examination of the four tables highlights the remarkable stability of the synthetic control results using yearly lag or 3 lags of the dependent variable and either the DAW or the LM set of controls.

Table K1: The Impact of RTC Laws on the Violent Crime Rate, LM covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized treatment effect percentage (TEP)	0.280	2.934*	4.716**	5.509**	7.630***	8.027**	11.741***	13.292***	14.306***	14.199***
	(1.182)	(1.503)	(1.949)	(2.153)	(2.544)	(3.121)	(2.957)	(3.930)	(3.751)	(2.888)
N	33	33	33	33	33	33	33	31	31	31
Pseudo p-value	0.852	0.214	0.088	0.110	0.064	0.090	0.034	0.028	0.036	0.034

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

Table K2: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.328	2.190	3.961**	4.957**	7.617***	8.210***	11.047***	13.577***	14.847***	15.411***
	(1.076)	(1.444)	(1.884)	(2.096)	(2.380)	(2.911)	(2.885)	(3.994)	(3.976)	(3.284)
N	33	33	33	33	33	33	33	31	31	31

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

Tables K3-K5 (Panel A) compare the fit of the five synthetic control estimates during the training period, validation period, and the entire pre-treatment period using three different loss functions. Table K3 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 K4 uses the mean of the absolute value of this difference between the actual value and synthetic control estimate; finally, Table K5 uses the CV of the RMSPE. For Tables K3-K5, an unweighted average of the error for each of the 33 treatment states is presented. For Tables K6-K8 (Panel B) a population-weighted average of the error for each of the 33 treatment states is presented, where population from the first year of the relevant period is used.[25]

The results from Tables K3-K8 provide strong evidence that using yearly lags of the dependent variable generates the best fit among the five options. As expected, across all six tables, the error in the *training period* is lowest using yearly lags, regardless of how the error is defined or whether population weights are used to aggregate the measure of error over all treatment states. Additionally, yearly lags provide the lowest error in the *validation period* in four of the six cases, being surpassed only marginally by the one lag average specification twice when using population

[25]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.

weights (Tables K6 and K7). Finally, yearly lags have the lowest error in all six tables over the full pre-treatment period.

A potential concern with using all pre-intervention outcomes of the dependent variable as synthetic control predictors is that the synthetic control unit will not closely match the treated unit on the explanatory variables during the pre-treatment period.[26] To explore this issue, we calculated for each DAW variable, state, and year, the absolute percentage difference between the true value of the variable and the value for the corresponding synthetic control across all five lag options. We then average by state and finally average across all RTC-adopting states for each explanatory variable. We then create the ratio of this statistic using a particular lag choice to the average of this statistic across all five lag choices. This ratio allows us to assess the predictor fit generated by each individual lag specification relative to the average fit.

Table K9 reveals that while yearly lags produces a good fit for an array of variables, the fit for the demographic variables is less good, particularly for the non-white non-black categories. To summarize the findings in Table K9, using all of the lags of the violent crime rate in generating a synthetic control generates the best fit in a number of measures of fit and prediction, but there are tradeoffs among the lag choices in terms of generating synthetic controls that more closely match all the explanatory variables of the DAW model. While we opted to rely on yearly lags in our main presentation to take advantage of the generally superior fit, a reasonable alternative might be the one lag average model. This specification better matches explanatory variables, while maintaining a reasonably close (but worse) fit of the dependent variable.

Importantly, our treatment effect percentage (TEP) results are robust to any of these five lag specifications. As Table K10 shows for violent crime using DAW covariates and five alternative lag specifications,[27] the point estimate of the tenth-year average treatment effect percentage ranges from 11.8 percent (one lag average) to 15.4 percent (three lags), while we highlight the estimate for yearly lags of 14.3 percent (which has the lowest standard error in the tenth year across all five models). In other words, for all five lag choices, we estimate RTC laws generate at least double-digit increases in the rate of violent crime.

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

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

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

Table K3: Define Fit Using Mean Squared Error

	Training Period; Mean Squared Error	*Validation Period; Mean Squared Error*	*Full Pre-Treatment Period; Mean Squared Error*
Three lags	2,686.622	7,595.525	4,207.864
Yearly lags	1,377.452	6,433.835	2,946.029
One lag average	1,752.449	7,855.294	3,546.032
One lag final pre-treatment year	3,903.140	8,920.437	5,517.578
No lags	2,421.579	8,559.487	4,253.367

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 K4: Define Fit Using Mean Absolute Difference

	Training Period; Mean Absolute Difference	*Validation Period; Mean Absolute Difference*	*Full Pre-Treatment Period; Mean Absolute Difference*
Three lags	33.414	65.556	43.740
Yearly lags	24.069	60.085	35.614
One lag average	27.885	65.127	39.546
One lag final pre-treatment year	38.077	67.925	47.813
No lags	34.676	71.569	46.511

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 K5: Define Fit Using CVRMSPE

	Training Period; CVRMSPE	*Validation Period; CVRMSPE*	*Full Pre-Treatment Period; CVRMSPE*
Three lags	0.132	0.251	0.191
Yearly lags	0.105	0.229	0.168
One lag average	0.116	0.245	0.179
One lag final pre-treatment year	0.146	0.261	0.201
No lags	0.143	0.274	0.206

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

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

Table K6: Define Fit Using Mean Squared Error

	Training Period; Mean Squared Error	*Validation Period; Mean Squared Error*	*Full Pre-Treatment Period; Mean Squared Error*
Three lags	1,831.318	5,432.279	2,940.866
Yearly lags	805.011	5,309.441	2,120.682
One lag average	1,135.997	5,285.855	2,329.984
One lag final pre-treatment year	2,551.610	6,075.208	3,694.090
No lags	1,718.201	6,197.124	3,015.222

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 K7: 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	26.799	53.647	35.243
Yearly lags	18.646	51.913	28.715
One lag average	22.887	50.601	31.491
One lag final pre-treatment year	29.342	54.235	37.234
No lags	30.319	60.414	39.664

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 K8: Define Fit Using CVRMSPE

	Training Period; CVRMSPE	*Validation Period; CVRMSPE*	*Full Pre-Treatment Period; CVRMSPE*
Three lags	0.074	0.129	0.105
Yearly lags	0.052	0.119	0.089
One lag average	0.062	0.121	0.094
One lag final pre-treatment year	0.082	0.135	0.111
No lags	0.086	0.149	0.119

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 K9: Ratio of mean economic predictor absolute percentage difference between treatment and synthetic controls to the average of this value for all five lag specifications

Variable	3 Lags	Yearly Lags	1 Lag Average	1 Lag final pre-Treatment Year	No Lags
Population	0.84	0.85	0.98	1.19	1.13
Poverty Rate	0.99	1.00	1.03	1.01	0.97
Lagged Incarceration Rate	0.95	1.01	1.00	1.03	1.00
Beer	0.95	1.02	1.02	1.05	0.96
Unemployment Rate	1.03	1.05	1.01	0.96	0.96
Lagged Police Employment	0.85	1.07	1.03	1.02	1.03
Real Income (p.c.)	0.97	1.07	1.00	0.94	1.01
Percent MSA	0.99	1.12	1.07	0.94	0.88
Age wm 2039	1.12	1.16	1.00	0.91	0.81
Age bm 1519	1.08	1.16	1.03	1.00	0.72
Age bm 2039	1.09	1.24	1.03	0.95	0.69
Age wm 1519	1.11	1.27	0.97	0.87	0.79
Age om 1519	0.92	1.56	1.16	0.72	0.65
Age om 2039	0.91	1.59	1.14	0.72	0.63

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

Table K10: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Various Lag specifications, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-0.328	2.190	3.961**	4.957**	7.617***	8.210***	11.047***	13.577***	14.847***	15.411***
3 Lags	(1.076)	(1.444)	(1.884)	(2.096)	(2.380)	(2.911)	(2.885)	(3.994)	(3.976)	(3.284)
N	33	33	33	33	33	33	33	31	31	31
Average normalized TEP	-0.117	2.629*	3.631*	4.682**	6.876***	7.358**	10.068***	12.474***	14.021***	14.344***
Yearly lag	(1.076)	(1.310)	(1.848)	(2.068)	(2.499)	(3.135)	(2.823)	(3.831)	(3.605)	(2.921)
N	33	33	33	33	33	33	33	31	31	31
Average normalized TEP	-0.184	2.045	3.366*	3.885*	5.856**	6.256*	8.595***	11.295**	11.840***	11.770***
1 Lag average	(1.157)	(1.355)	(1.924)	(2.151)	(2.492)	(3.076)	(2.877)	(4.327)	(4.219)	(3.734)
N	33	33	33	33	33	33	33	31	31	31
Average normalized TEP	0.325	3.293**	4.639**	5.083**	7.432***	8.084**	10.859***	13.187***	13.899***	14.222***
1 Lag final year	(1.175)	(1.539)	(1.921)	(2.094)	(2.371)	(3.047)	(2.887)	(4.175)	(4.187)	(3.359)
N	33	33	33	33	33	33	33	31	31	31
Average normalized TEP	-0.485	1.458	3.193**	4.183**	6.028**	6.320*	10.061***	12.266***	12.631***	13.751***
No lags	(1.155)	(1.723)	(1.536)	(1.879)	(2.443)	(3.183)	(3.557)	(4.144)	(4.115)	(3.917)
N	33	33	33	33	33	33	33	31	31	31

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

Appendix L: Simulating Earlier RTC Passage

Footnote 58 outlined an approach to validate our synthetic control estimates, using a "phantom-adoption" test. Essentially, we pretend that the RTC states adopted their laws five years earlier than they did, and we then used our synthetic control approach to estimate what the crime rate was for the five pre-adoption years. A perfect result would show a zero effect over that pre-adoption period.

Tables L1-L10 present both normalized and non-normalized synthetic control estimates for violent crime, murder, and property crime with a phantom RTC law five years before actual passage. Each table thus shows estimated effects of RTC laws on the five years prior to their adoption, as well as the ten years after. For the normalized versions, none of the estimates for pre-passage years are statistically significant, other than the year prior to true adoption for property crime. Conversely, for the non-normalized models, the pre-passage estimates are considerably larger and often highly significant. This distinction lends further credibility to the choice to use normalized estimates (subtracting off the differential between actual and synthetic control estimates in the last pre-adoption year) rather than using unadjusted figures.

This "phantom-adoption" test is particularly reassuring for violent crime since it yields relatively modest pre-treatment values (only a statistically insignificant 3.2 percent in the year prior to actual adoption, as seen in Table A28), and the estimates rise sharply after RTC adoption. Unfortunately, the results from this "phantom-adoption" test for the murder and property crime estimates are not as reassuring. For example, the synthetic control estimate for the year prior to adoption in our preferred normalized approach is roughly 8 percent for murder (Table A30) and 7 percent for property crime (Table A32). While neither of these estimates is statistically significant, they are both more than twice the size of the estimates for violent crime, which leads us to emphasize the results for violent crime more than those for our other crime measures.

Table L1: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014

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

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the difference between the percentage difference in the violent crime rate in treatment and synthetic control states at given simulated post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

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

States excluded for poor pre-treatment fit:

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

Table L2: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction

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

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval

Results reported for the constant term resulting from this regression

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

States excluded for poor pre-treatment fit:

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

Table L3: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	-0.081	-3.481	0.014	1.137	-0.120	2.663	7.030	12.426**	12.022***	15.116***	14.364***	13.507***	11.134**	12.006**	11.815**
	(2.072)	(3.653)	(4.948)	(5.062)	(4.875)	(4.489)	(5.029)	(4.738)	(3.425)	(4.286)	(4.848)	(3.569)	(4.476)	(4.788)	(5.041)
N	29	29	29	29	29	29	29	27	27	27	24	22	22	21	21

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the change in the percentage difference in murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L4: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	7.691*	4.252	7.706	8.786*	7.487	10.236*	14.580**	19.683***	19.259***	22.187***	19.503***	18.244***	15.867***	16.915**	16.696**
	(4.339)	(3.619)	(5.085)	(4.661)	(4.498)	(5.229)	(6.680)	(6.473)	(5.836)	(6.773)	(6.691)	(5.604)	(5.233)	(6.016)	(7.062)
N	29	29	29	29	29	29	29	27	27	27	24	22	22	21	21

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L5: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	-4.244	-5.502	-6.369	-3.596	-6.753	-3.845	4.104	10.667	10.054	15.696	4.576	2.382	-2.862	-5.472	-3.154
	(3.146)	(5.464)	(7.118)	(7.122)	(7.430)	(8.141)	(10.231)	(9.532)	(8.808)	(9.864)	(10.150)	(10.916)	(9.965)	(9.946)	(9.618)
N	28	28	28	28	28	28	28	26	26	26	23	21	21	20	20

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the change in the percentage difference in firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L6: The Impact of RTC Laws on the Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	4.268	2.998	2.106	4.838	1.638	4.515	12.449	18.105*	17.478**	22.882**	12.343	10.212	4.954	1.652	3.945
	(5.734)	(4.950)	(6.479)	(6.866)	(6.705)	(8.006)	(10.171)	(9.711)	(8.377)	(10.139)	(9.564)	(9.620)	(8.488)	(7.625)	(10.026)
N	28	28	28	28	28	28	28	26	26	26	23	21	21	20	20

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the firearm murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L7: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	-2.780	-10.998**	-2.528	0.112	-0.366	-6.232	-8.754	0.690	-3.355	-9.024**	-3.015	1.553	-1.442	4.214	2.603
	(4.164)	(4.044)	(5.052)	(6.187)	(6.254)	(5.814)	(5.966)	(5.140)	(4.164)	(4.312)	(4.785)	(4.080)	(4.220)	(5.316)	(6.068)
N	27	27	27	27	27	27	27	25	25	25	22	20	20	19	19

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the change in the percentage difference in non-firearm murder rate from the time of treatment to a given post-treatment interval between treatment and synthetic control states

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L8: The Impact of RTC Laws on the Non-Firearm Murder Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction of Adoption Year Crime Differential

	Prior to RTC Passage					*After RTC Passage*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average normalized TEP	12.230**	3.949	12.364*	14.951**	14.426**	8.517	5.958	15.077**	10.998**	5.243	8.115	11.453**	8.411*	13.735**	12.073
	(5.525)	(4.506)	(6.042)	(6.058)	(5.701)	(5.122)	(5.100)	(6.156)	(4.951)	(4.901)	(4.741)	(4.397)	(4.472)	(5.407)	(6.983)
N	27	27	27	27	27	27	27	25	25	25	22	20	20	19	19

Standard errors in parentheses

Column numbers indicate post-passage year under consideration; N = number of states in sample

Dependent variable is the percentage difference in the non-firearm murder rate at given post-treatment interval and at time of the treatment

Results reported for the constant term resulting from this regression

The synthetic controls method is run using the nested option.

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

Table L9: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014

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

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

Table L10: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014, No Subtraction

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

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

Appendix M: Panel and Synthetic Control Results for Robbery

Our panel data estimates for robbery are estimated from 1979-2014. These estimates are obtained from the standard UCR crime rate data.

Table M1: RTC Panel Data Results for Robbery, 1979-2014

	Robbery Rate	Ln Robbery Rate
RTC Dummy	4119.5***	8.901*
	(1331.6)	(4.811)
Observations	1823	1823

Standard errors in parentheses

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

The data on robberies committed with a firearm is obtained from the UCR Return A Masterfiles, as the standard UCR robbery data does not have a breakdown of robberies by weapon used. For each state-year we calculate the proportion of robberies committed using a firearm. Note that we include robberies where the weapon involved is unknown in the total number of robberies (the denominator of the proportion). We also impute cases where the number of firearm robberies in a state-year is missing or reported as 0. A state-year may be missing the number of firearm robberies if no agencies within a particular state submit their RETA form for that specific year. A state-year will incorrectly have 0 reported for the number of firearm robberies if all state agencies fail to report the weapon used in the robbery cases for a particular year (i.e. all robberies will be recorded as robberies where the weapon used is unknown and zero robberies will be recorded as committed with a firearm). In these two scenarios, we impute the number of robberies committed with a firearm by taking the average of the two neighboring non-missing values for a state-year. This helps us ensure a more complete dataset, which is important when we run our synthetic controls analysis since the method will not run properly when there are missing values for the dependent variable.

Using our preferred specification where the outcome is log transformed, our panel data results suggest that RTC increases robbery rates by almost 9 percent and increases the percentage of robberies committed with a firearm by over 18 percent.

Table M2: RTC Panel Data Results for the Proportion of Robberies Committed With A Firearm, 1980-2014

	Proportion Firearm Robberies	Ln Proportion Firearm Robberies
RTC Dummy	5.056***	18.08**
	(1.826)	(6.951)
Observations	1737	1737

Standard errors in parentheses.
The UCR RetA data is only available from 1980 onwards. We drop Illinois from the analysis because it is missing over 10 state-years worth of data. we impute cases where the number of firearm robberies in a state-year is missing or reported as 0. A state-year may be missing the number of firearm robberies if no agencies within a particular state submit their RETA form for that specific year. A state-year will incorrectly have 0 reported for the number of firearm robberies if all state agencies fail to report the weapon used in the robbery cases for a particular year (i.e. all robberies will be recorded as robberies where the weapon used is unknown and zero recorded as committed with a firearm). In these two scenarios, we impute the number of firearm robberies by taking the average of the two neighboring non-missing values for a state-year.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table M3: The Impact of RTC Laws on the Robbery Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.553	0.732	1.212	1.736	2.939	3.226	4.598	7.513	8.219*	7.969
	(1.810)	(2.751)	(3.193)	(3.881)	(4.837)	(5.508)	(5.864)	(5.568)	(4.735)	(4.847)
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 robbery rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table M4: The Impact of RTC Laws on the Proportion Of Robberies Committed With A Firearm, Weapon Unknown Included in Total, DAW covariates, Full Sample, 1980-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	18.013**	13.824**	23.662***	30.186***	28.883***	30.094***	41.391***	49.315***	46.202***	35.177***
	(6.997)	(5.551)	(7.066)	(8.928)	(7.608)	(6.738)	(8.552)	(11.198)	(14.157)	(4.476)
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 proportion of robberies committed with a firearm, (weapon unknown included in total) in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our synthetic control results support the panel data findings that RTC laws increase both robbery and firearm robbery. While the magnitude of the estimate is similar for robbery, with the finding that RTC increases robbery rates by almost 8 percent, the magnitude of our synthetic control estimate for the effect on the proportion of robberies committed with a firearm is much greater. We find that RTC laws significantly increase the proportion of robberies committed with a firearm by over 35 percent.

Appendix N: Robustness to Dropping States

The following section reports our synthetic control results when we drop Hawaii, New York, and California from our pool of potential donor states.

Table N1: The Impact of RTC Laws on the Violent Crime Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.582	0.562	2.538	2.742	4.264	3.957	5.913	6.844	7.895**	8.861**
	(1.447)	(1.987)	(2.819)	(3.035)	(3.350)	(3.738)	(4.645)	(4.313)	(3.695)	(3.548)
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
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table N2: The Impact of RTC Laws on the Murder Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-1.601	-4.802*	-4.564	-5.313	-7.313*	-8.566*	-6.194	-0.124	0.886	1.033
	(1.824)	(2.630)	(3.271)	(3.539)	(3.923)	(4.582)	(5.064)	(6.480)	(5.851)	(5.759)
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
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table N3: The Impact of RTC Laws on the Property Crime Rate, DAW covariates, Full Sample, 1977-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.437	0.290	0.033	-1.038	-0.503	1.700	2.688	4.008	3.327	2.741
	(1.088)	(1.080)	(1.407)	(2.024)	(2.583)	(2.805)	(3.364)	(3.676)	(3.492)	(3.080)
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
States excluded for poor pre-treatment fit:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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