anger, which are emotions that can invite more hostile confrontations leading to more violence. This attitude may be reinforced by the adoption of RTC laws. When Philadelphia permit holder Louis Mockewich shot and killed a popular youth football coach (another permit holder carrying his gun) over a dispute concerning snow shoveling in January 2000, the bumper sticker on Mockewich's car had an NRA bumper sticker reading "Armed with Pride" (Gibbons and Moran, 2000). If you are an angry young man, with somewhat of a paranoid streak, and you haven't yet been convicted of a crime or adjudicated to be a mental defective, it is likely that the ability to carry a gun will both be more attractive and more likely in a RTC state. That such individuals will, therefore, be more likely to be aggressive once armed and hence more likely to stimulate violence by others should not be surprising.

Second, individuals who carry guns around are a constant source of arming criminals. When Sean Penn obtained a permit to carry a gun, his car was stolen with two guns in the trunk. The car was soon recovered, but the guns were gone (Donohue, 2003). In July 2015 in San Francisco, the theft of a gun from a car in San Francisco led to a killing of a tourist on a city pier that almost certainly would not have occurred if the lawful gun owner had not left it in the car (Ho, 2015). Just a few months later, a gun stolen from an unlocked car was used in two separate killings in San Francisco in October 2015 (Ho and Williams, 2015). According to the National Crime Victimization Survey, in 2013 there were over 660,000 auto thefts from households. The more guns being carried in vehicles by permit holders, the more criminals will be walking around with the guns taken from the car of some permit holder. Of course, the San Francisco killer did not have a RTC permit; although the owner of the gun used in the killing did (Ho, 2015). Lost, forgotten, and misplaced guns are another dangerous by-product of RTC laws, as the growing TSA seizures in carry-on luggage attest.[46]

Third, as more citizens carry guns, more criminals will find it increasingly beneficial to carry guns and use them more quickly and more violently to thwart any potential armed resistance. Fourth, the passage of RTC laws normalizes the practice of carrying guns in a way that may enable criminals to carry guns more readily without prompting a challenge, while making it harder for the police to know who is and who is not allowed to possess guns in public. Having a "designated permit holder" along to take possession of the guns when confronted by police seems to be an attractive benefit for criminal elements acting in concert (Fernandez et al., 2015; Luthern, 2015). Fifth, it almost certainly adds to the burden of a police force to have to deal with armed citizens. A policemen trying to give a traffic ticket has far more to fear if the driver is armed. When a gun is found in a car in such a situation, a greater amount of time is needed to ascertain the driver's status as a permit holder. Police may be less enthusiastic about investigating certain suspicious activities given the greater risks that widespread gun carrying poses to them. Police resources used to process gun permits could instead be more efficiently used to directly fight crime. All of these factors are a tax on police, and therefore one would expect law enforcement to be less effective on the margin, thereby contributing to crime. Indeed, this may in part explain why RTC states tend to increase the size of their police forces (relative to non-adopting states) after RTC laws are passed.

The fact that two different types of statistical data – panel data regression and synthetic controls – with varying strengths and shortcomings and with different model specifications both yield consistent and strongly statistically significant evidence that RTC laws increase violent crime constitutes persuasive evidence that any beneficial effects from gun carrying are likely substantially outweighed by the increases in violent crime that these laws stimulate.[47]

[46] See Williams and Walstrip (2004).

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

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Appendix A: Tables

Table A1: RTC Adoption Dates

State	Effective Date of RTC Law	Fraction of Year In Effect Year of Passage	RTC (Date in Synthetic Controls Analysis)
Alabama	1975		1975
Alaska	10/1/1994	0.252	1995
Arizona	7/17/1994	0.460	1995
Arkansas	7/27/1996	0.433	1996
California	N/A		0
Colorado	3/17/2003	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	1985
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	9/26/2004	0.847	2004
Montana	10/1/1991	0.252	1992
Nebraska	1/1/2007	1.000	2007
Nevada	10/1/1996	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	1995
North Dakota	8/1/1985	0.419	1985
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

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

RTC dates before the year 1977 may not be exact, since differences between these dates would neither affect our regression results nor our synthetic control tables. For example, we only read Vermont's statutes up to the year 1970 to confirm there were no references to blanket prohibitions on carrying concealed weapons up to the year 1970, although it appears given widespread public commentary on this point that Vermont never had a comprehensive prohibition of the carrying of concealed weapons. We follow earlier convention in the academic literature on the RTC issue in assigning RTC adoption dates for Alabama and Connecticut.

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

Panel A: Dummy Variable Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law	9.49*** (2.96)	10.98*** (3.65)	−1.36 (3.15)	0.69 (0.77)
Lagged Incarceration Rate	0.04* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		24.00** (9.55)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		3.27 (13.59)		
Lagged Arrest Rate for Violent Crimes			−0.16** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				87.07*** (1.47)
Real Per Capita Personal Income	0.00 (0.00)		0.00* (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance			0.00 (0.01)	0.01* (0.01)
Real Per Capita Income Maintenance			0.04 (0.03)	0.02* (0.01)
Real Per Capita Retirement Payments and Other (Lott version)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version)				−0.00* (0.00)
Nominal Per Capita Income		−0.00 (0.00)		
Unemployment Rate	0.16 (0.77)	−1.00 (0.67)		−0.36 (0.23)
Poverty Rate	−0.29 (0.49)			0.13 (0.10)
Lagged Number of Executions		0.11 (0.16)		
Beer	65.27*** (17.58)	71.74*** (18.21)		
Population			0.00 (0.00)	−0.00 (0.00)
Percent of the population living in MSAs	0.94*** (0.29)			
Population Density			−0.01 (0.02)	
Observations	1823	1874	1896	1781

Panel B: Spline Model Results

	(Table 4) DAW Model	(Table 5.A) BC Model	(Table 6.A) LM Model	(Table 7.A) MM Model
	(1)	(2)	(3)	(4)
Right-to-Carry Law	0.05 (0.64)	0.19 (0.66)	0.41 (0.47)	0.17** (0.08)
Trend for Changer States	0.93* (0.49)	0.96* (0.53)	0.12 (0.39)	−0.07 (0.08)
Lagged Incarceration Rate	0.03* (0.02)			−0.00 (0.00)
Lagged Log of Per Capita Incarceration Rate		21.19*** (8.10)		
Lagged Police Employee Rate	−0.05 (0.04)			
Lagged Log of Sworn Police Officers Per Resident Population		2.34 (13.57)		
Lagged Arrest Rate for Violent Crimes			−0.16** (0.08)	−0.04** (0.02)
Lagged Dependent Variable				86.59*** (1.47)
Real Per Capita Personal Income	0.00 (0.00)		0.00** (0.00)	0.00 (0.00)
Real Per Capita Unemployment Insurance			−0.00 (0.02)	0.01 (0.01)
Real Per Capita Income Maintenance			0.03 (0.03)	0.01 (0.01)
Real Per Capita Retirement Payments and Other (Lott version)			0.00 (0.01)	
Real Per Capita Retirement Payments and Other (MM version)				−0.00** (0.00)
Nominal Per Capita Income		0.00 (0.00)		
Unemployment Rate	0.76 (0.87)	−0.29 (0.81)		−0.27 (0.23)
Poverty Rate	−0.41 (0.50)			−0.15 (0.10)
Lagged Number of Executions		0.16 (0.17)		
Beer	65.78*** (16.33)	67.19*** (15.41)		
Population			0.00 (0.00)	−0.00* (0.00)
Percent of the population living in MSAs	0.77*** (0.28)			
Population Density			0.00 (0.02)	
Observations	1823	1874	1896	1781

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

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	1.667	-1.400	-1.203	-1.644	-4.051	-6.915	-6.171	1.971	-0.130	4.613
	(2.006)	(4.009)	(4.415)	(4.534)	(5.136)	(4.478)	(5.309)	(4.738)	(4.280)	(3.692)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.415	1.149	2.195	0.720	0.444	1.345	0.578	1.261	1.013	0.070
	(0.923)	(1.219)	(2.549)	(2.693)	(2.764)	(2.593)	(2.638)	(2.341)	(2.367)	(2.330)
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 differences in the property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	2.314	-1.411	-0.465	-1.588	-3.970	-4.395	-4.904	2.813	1.205	4.574
	(2.044)	(3.874)	(4.307)	(4.412)	(5.038)	(4.535)	(4.850)	(4.503)	(3.038)	(3.169)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEP	-0.575	0.794	1.807	0.542	0.355	1.434	0.728	1.412	1.177	1.020
	(1.036)	(1.354)	(2.558)	(2.701)	(2.756)	(2.562)	(2.549)	(2.375)	(2.372)	(2.308)
N	33	33	33	33	33	32	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 differences in the property crime rate in treatment and synthetic control states at given post-treatment interval and at time of the treatment
Results reported for the constant term resulting from this regression
States in group: AK AR AZ CO FL GA ID KS KY LA ME MI MN MO MS MT NC ND NE NM NV OH OK OR PA SC SD TN TX UT VA WV WY
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEF	-0.107	-4.365	-2.770	-3.382	-5.269	-3.972	-4.913	2.619	1.633	4.542
	(1.713)	(4.158)	(4.501)	(4.561)	(5.313)	(5.155)	(5.684)	(5.512)	(4.968)	(4.141)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEF	-0.208	1.262	2.211	1.039	0.072	1.009	1.525	2.091	2.568	3.420
	(1.065)	(1.165)	(2.616)	(2.688)	(2.719)	(2.575)	(2.367)	(2.374)	(2.713)	(3.050)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEF	1.785	-2.359	-1.162	-1.838	-3.728	-3.175	-2.909	3.686	2.792	3.875
	(1.774)	(3.987)	(4.179)	(4.266)	(4.559)	(4.428)	(4.431)	(4.440)	(4.086)	(4.071)
N	33	33	33	33	33	33	33	31	31	31

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average Normalized TEF	-0.314	1.231	2.369	1.543	1.581	1.078	1.863	2.692	2.775	3.062
	(0.941)	(1.157)	(2.526)	(2.657)	(2.596)	(2.581)	(2.440)	(2.334)	(2.312)	(2.342)
N	33	33	33	33	33	33	33	31	31	31

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

Appendix B: Data and Methodological Appendix

I. Data Issues

The state-level data set used in this paper updated through 2014 earlier data sets used in Aneja et al. (2014) and Aneja et al. (2011). We further update this data set to incorporate changes to the various primary sources that have occurred since first released, and to include the additional predictor variables that are featured in the DAW and BC models. All variables are collected for the years 1977-2014 unless otherwise noted.[48]

Annual state-level crime rates are taken from the FBI's Uniform Crime Reporting program.[49] Four state-level income variables (personal income, income maintenance payments, retirement payments, and unemployment insurance payments) are taken from the BEA's Regional Economic Accounts. The personal income, income maintenance, and unemployment insurance payment variables are estimated in real per capita terms (defined using the CPI). The the LM and MM specifications use alternative versions of the retirement variable that are described in footnote 48. State-level population is generated using the Census Bureau's intercensal population estimates, while the proportional size of LM's 36 age-race-sex demographic groups are estimated using state-level population by age, sex, and race gathered by the Census. (In cases where the most recent form of these data were not easily accessible at the state level, state-level figures were generated by aggregating the Census Bureau's county-level population estimates by age, sex, and race.) Population density is estimated by dividing a given observation's population by the area of that state reported in the previous decennial census. State-level unemployment rate data is taken from the Bureau of Labor Statistics, while the poverty rate is taken from two Census series (the 1979 state-level poverty rate is derived from the Decennial Census and the 1980-2014 poverty rates are generated using the Current Population Survey). A measure of incarceration (incarcerated individuals per 100,000 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.[50] We also use the index of crack cocaine usage constructed by Fryer et al. (2013) for our analysis, which is only available between the years 1980 and 2000, and therefore we drop this variable from the MM model when we estimate this model on data through 2014. Since we already include controls that incorporate information on the racial composition of individual states in our analysis, we use the unadjusted

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

[49] 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.

[50] 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.

version of the crack index instead of the version that is adjusted to account for differences in state racial demographics.

No data for the crack cocaine index that we use was available for the District of Columbia, and our matching methodology does not allow the District of Columbia to be included in our analysis in specifications that include this variable as a predictor. After considering several different ways to confront this issue, we ultimately decided to exclude the District of Columbia from the synthetic controls analysis owing to its status as a clear outlier whose characteristics are less likely to be meaningfully predictive for other geographic areas. Abadie et al. (2010) emphasize that researchers may want to "[restrict] the comparison group to units that are similar to the exposed units [in terms of the predictors which are included in the model]." Given that the District of Columbia had the highest per capita personal income, murder rate, unemployment rate, poverty rate, and population density at various points in our sample, so Abadie's admonition would seem to support omitting the District as one of our potential control units.[51] We should note that even if we include DC in the synthetic controls estimates, it still shows RTC laws increase violent crime by 13.2% in the tenth year (as opposed to the 14.7% figure shown in Table 9).

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

II. The Dates of Adoption of RTC Laws

We use the same effective RTC dates used in Aneja et al. (2014) with one small modification. Owing to the fact that we are using annual panel data, the mechanics of the synthetic control methodology require us to

[51] 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.

specify a specific year for each state's RTC date. To take advantage of the information we have collected on the exact dates when RTC laws went into effect in each state, each state's effective year of passage is defined as the first year in which a RTC law was in effect for the majority of that year.[52] This causes some of the values of our RTC variable to shift by one year (for instance, Wisconsin's RTC date shifts from 2011 to 2012, since the state's RTC law took effect on November 1, 2011).[53]

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

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

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

Appendix C: Replicating Our Analysis

One issue which is rarely addressed directly in the existing literature surrounding the application of the synthetic control technique is the sensitivity of the selection of the synthetic control to seemingly inconsequential details when using maximum likelihood to select the weights associated with different predictors in our analysis. More specifically, when using the excellent "synth" package for Stata created by Abadie, Hainmueller, and Diamond along with the *nested* option (which implements the optimization technique described in footnote 20), both the version of Stata (e.g., SE vs. MP), the specifications of the computer running the command, and the order in which predictors are listed can affect the composition of the synthetic control and by extension the size of the estimated treatment effect.

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

> 'When more than one processor is used in Stata/MP, the computations for the likelihood are split into pieces (one piece for each processor) and then are added at the end of the calculation on each iteration. Because of round-off error, addition is not associative in computer science as it is in mathematics. This may cause a slight difference in results. For example, a1+a2+a3+a4 can produce different results from (a1+a2)+(a3+a4) in numerical computation. When changing the number of processors used in Stata, the order in which the results from each processor are combined in calculations may not be the same depending on which processor completes its calculations first.'[54]

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

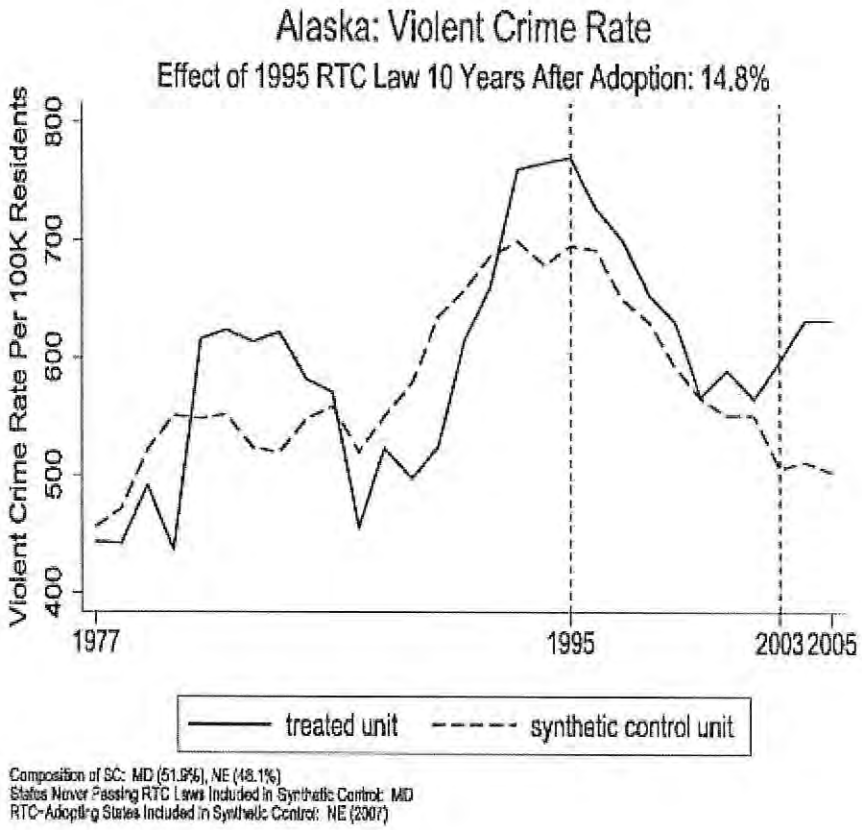
Another discrepancy that we encountered is that memory limitations sometimes caused our synthetic control analyses to crash when using the *nested* option. When this occurred, we would generate our synthetic control using the regression-based technique for determining the relative weights assigned to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

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

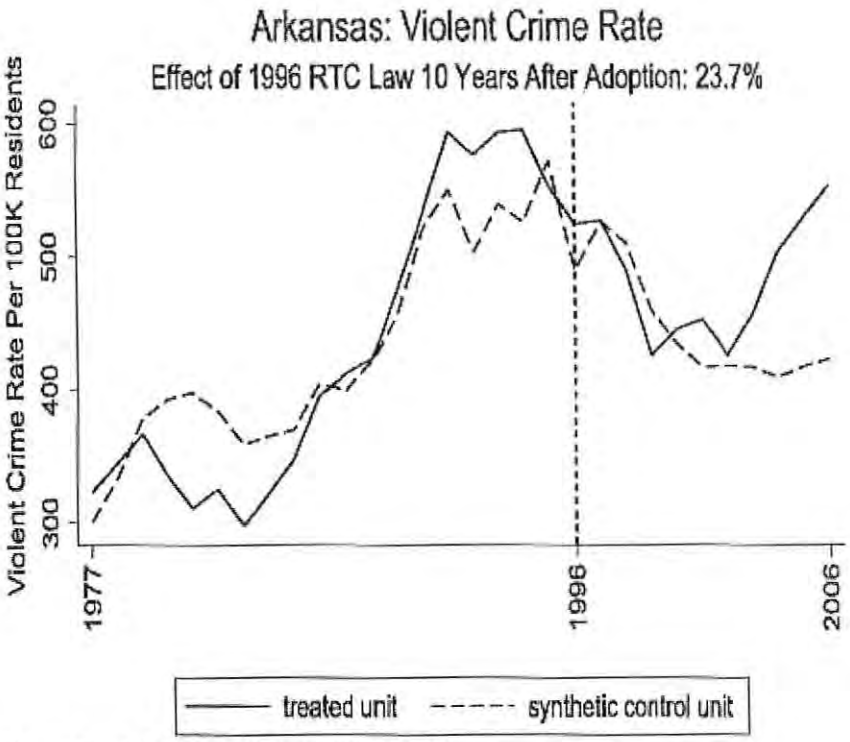
[54] This memo can be found at the following link: http://www.webcitation.org/6YsLV03SN.

specifications featuring different sets of predictors gives us greater confidence that our conclusions about these specifications are robust to changes in variable order as well.

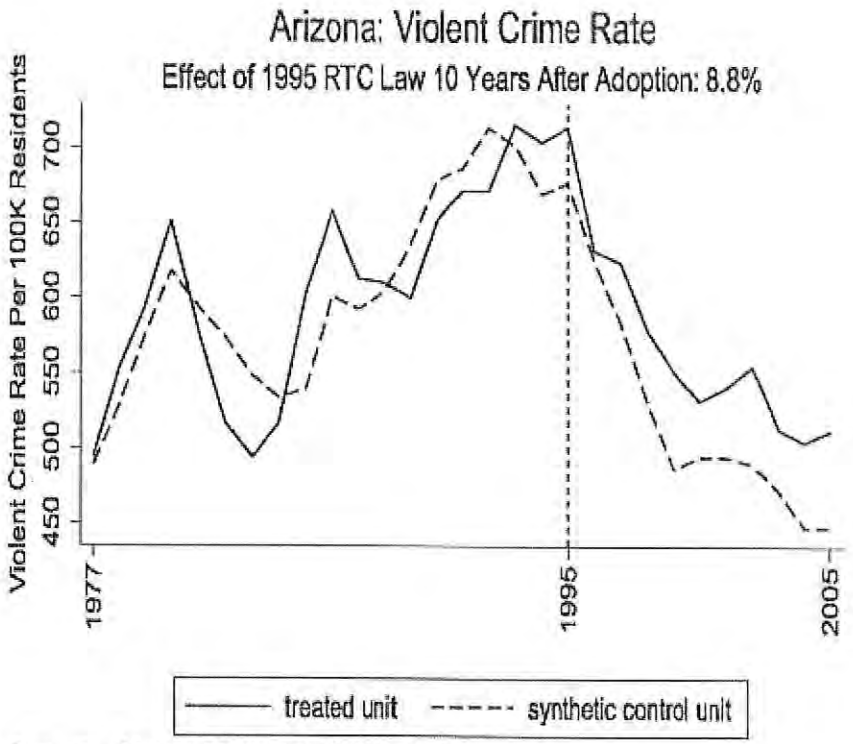
Appendix D: Synthetic Control Graphs Estimating Impact of RTC Laws On Violent Crime Using the DAW Model[55]



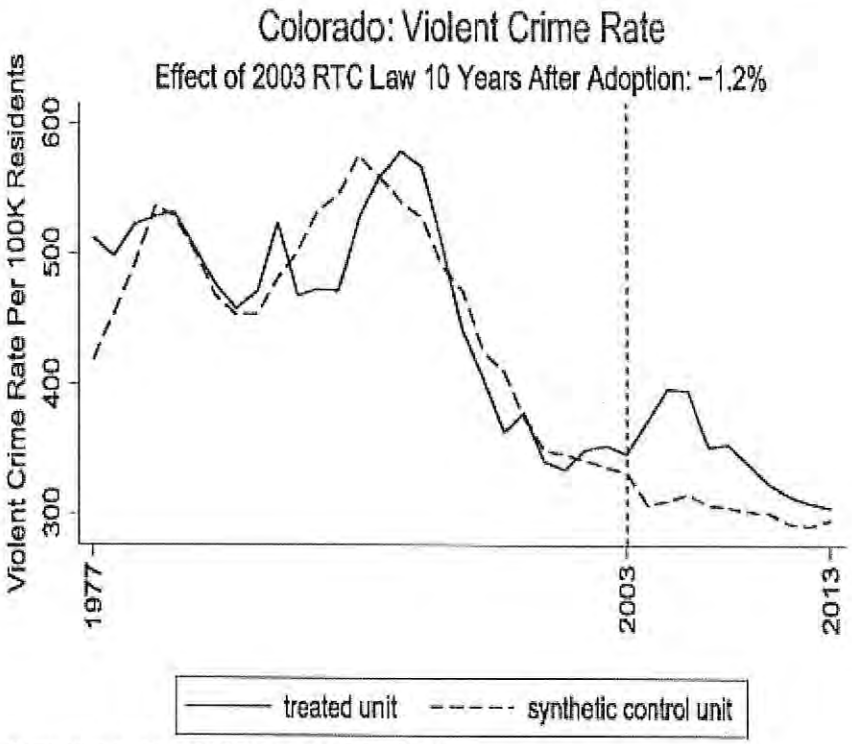
[55] Recall that each state's effective year of passage is defined as the first year in which a RTC law was in effect for the majority of that year.



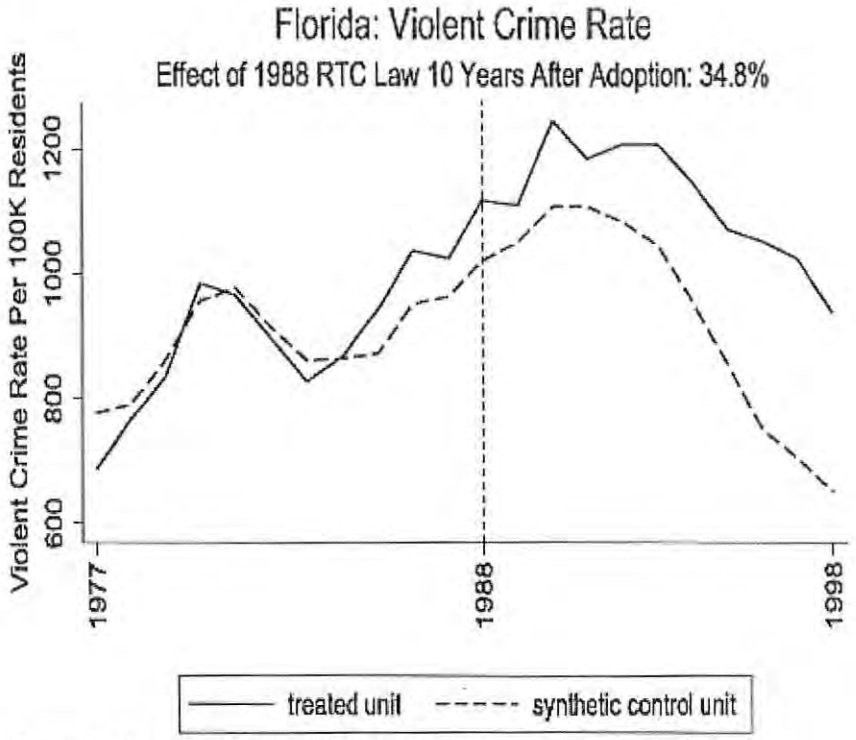
Composition of SC: DE (18.6%), IL (23.1%), IA (58.4%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC-Adopting States Included in Synthetic Control: IL (2014), IA (2011)



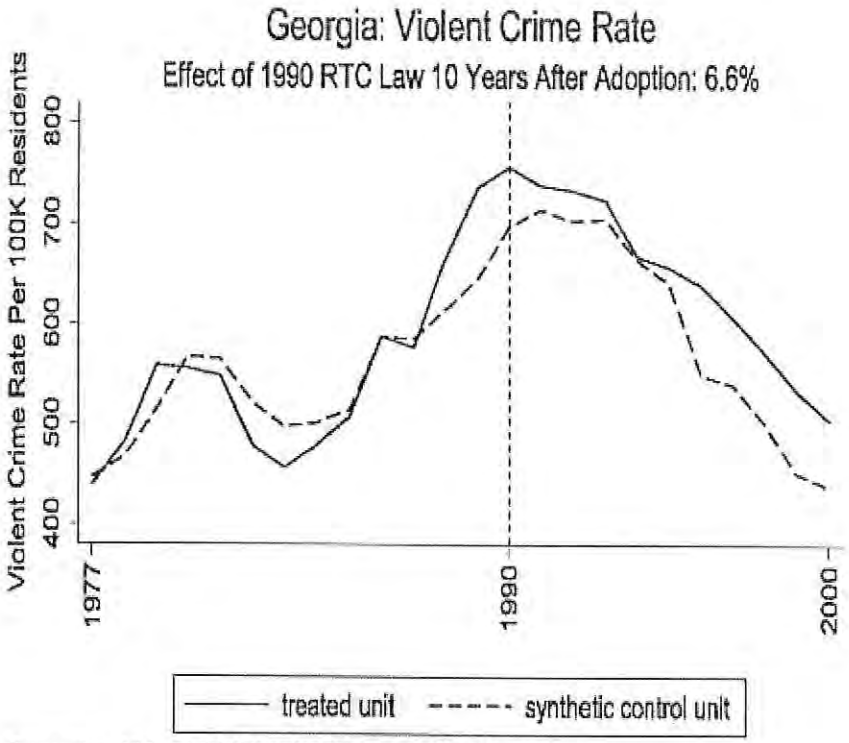
Composition of SC: CA (32.7%), HI (40.9%), MD (22.9%), NE (3.5%)
States Never Passing RTC Laws Included in Synthetic Control: CA, HI, MD
RTC-Adopting States Included in Synthetic Control: NE (2007)



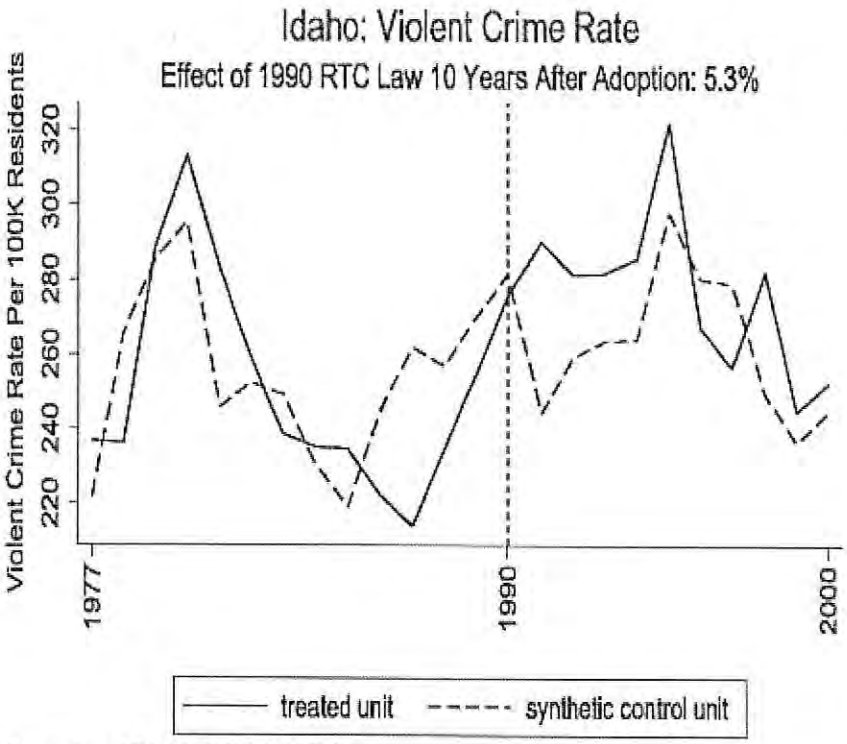
Composition of SC: HI (49.9%), NY (29.3%), RI (20.8%)
States Never Passing RTC Laws Included in Synthetic Control: HI, NY, RI
RTC-Adopting States Included in Synthetic Control:



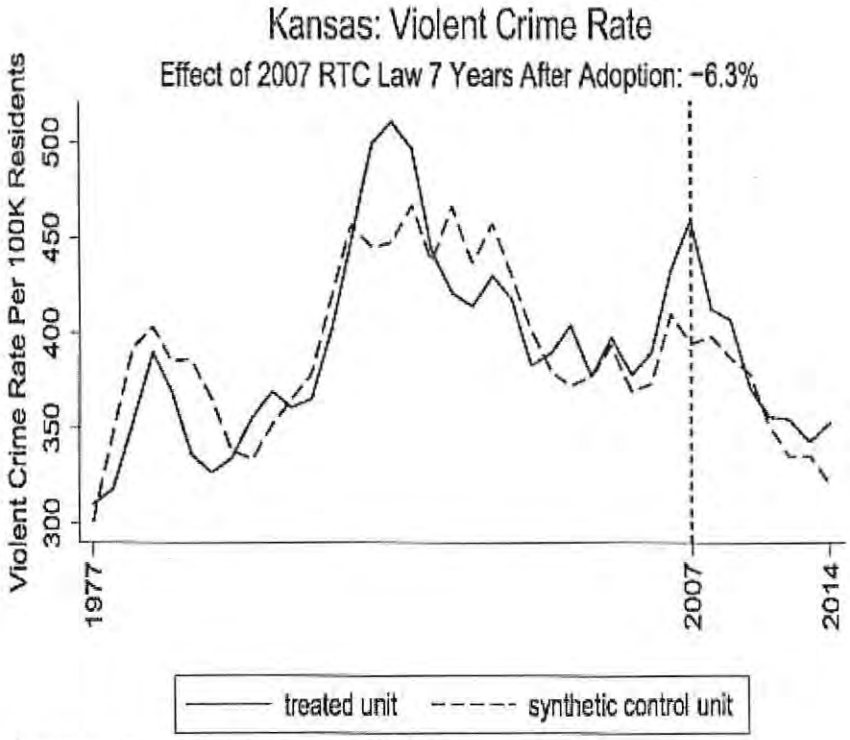
Composition of SC: CA (22.3%), MI (11.0%), NY (66.7%)
States Never Passing RTC Laws Included in Synthetic Control: CA, NY
RTC-Adopting States Included In Synthetic Control: MI (2001)



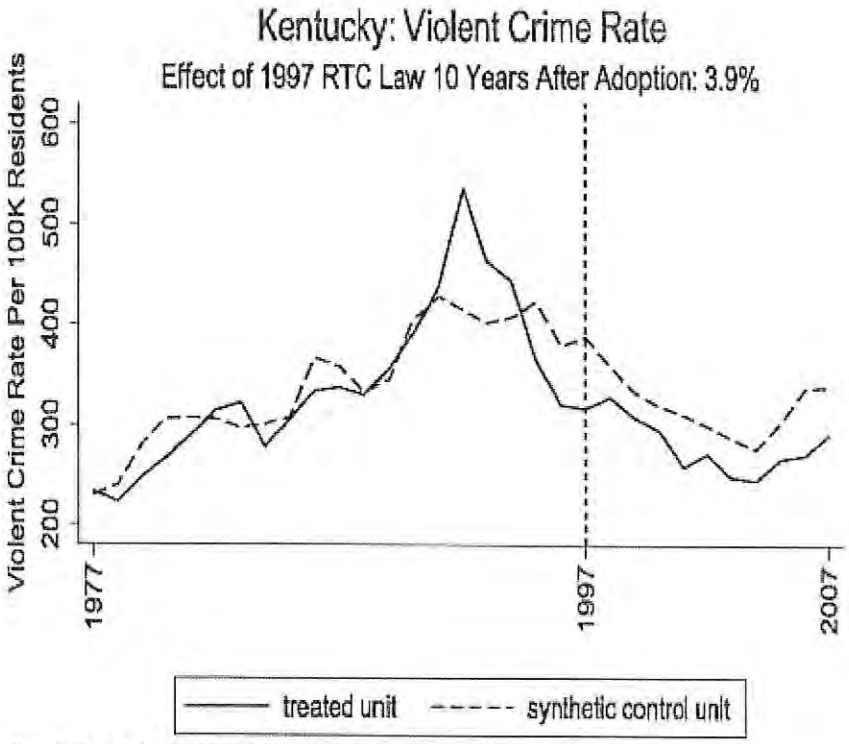
Composition of SC: CA (30.1%), IA (45.2%), MO (9.7%), NY (15.0%)
States Never Passing RTC Laws Included in Synthetic Control: CA, NY
RTC-Adopting States Included in Synthetic Control: IA (2011), MO (2004)



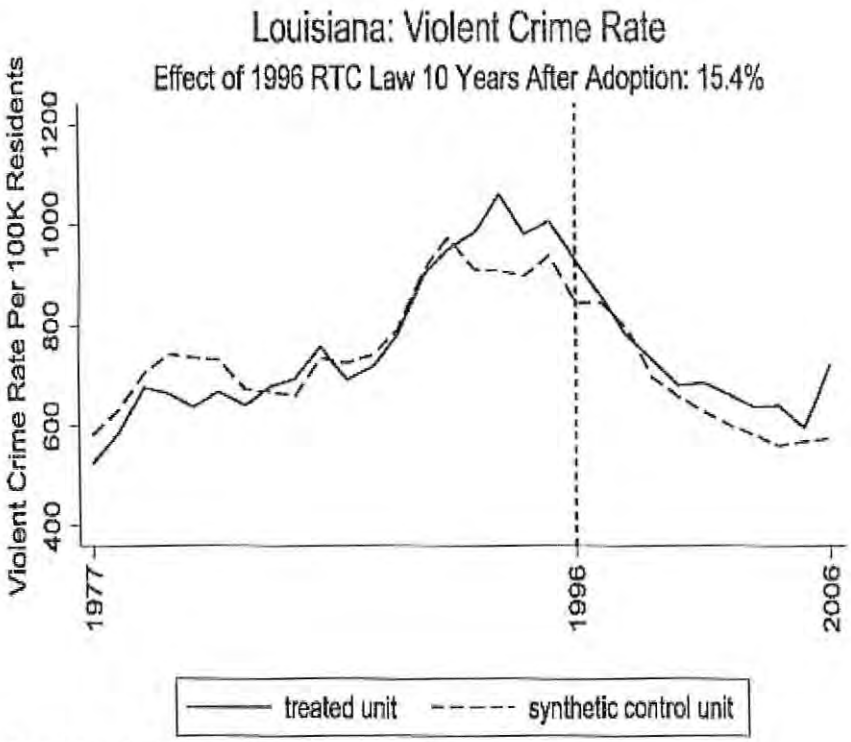
Composition of SC: HI (96.1%), IA (3.9%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC-Adopting States Included In Synthetic Control: IA (2011)



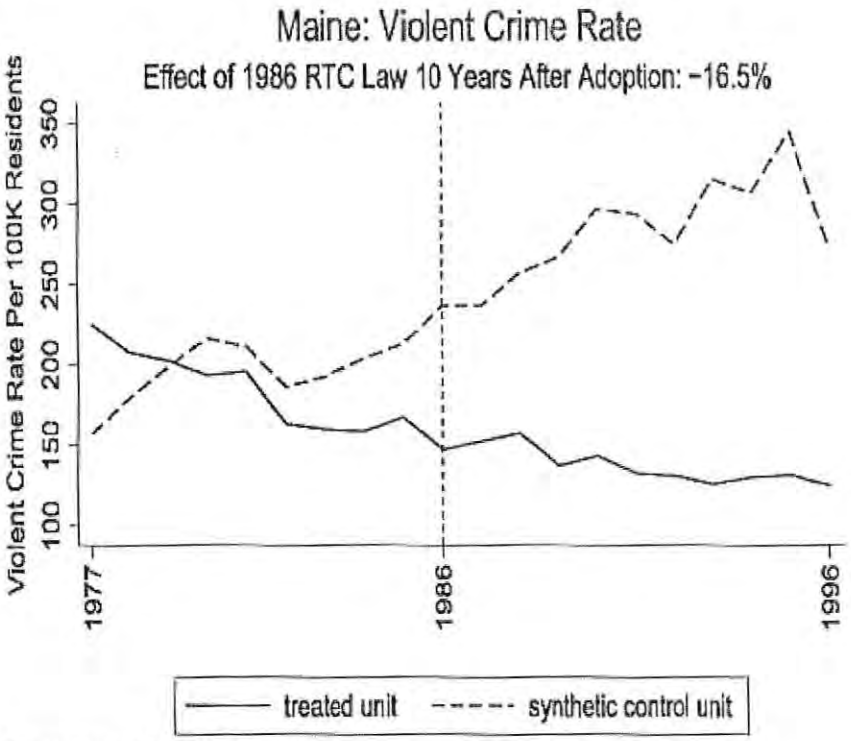
Composition of SC: DE (18.3%), HI (58.3%), MA (23.5%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI, MA
RTC-Adopting States Included in Synthetic Control:



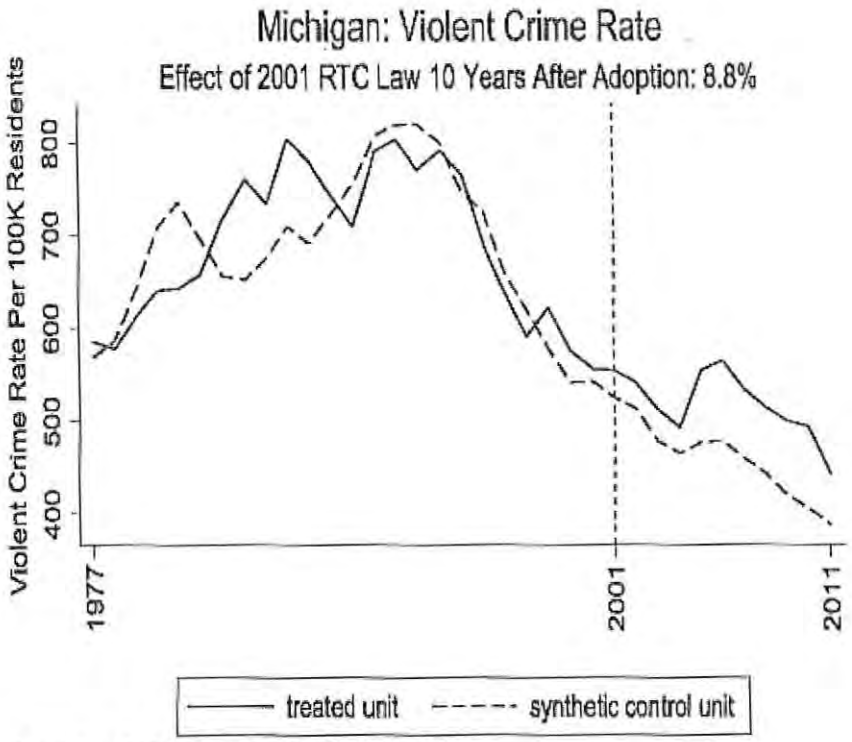
Composition of SC: IL (19.8%), WI (80.2%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC-Adopting States Included in Synthetic Control: IL (2014), WI (2012)



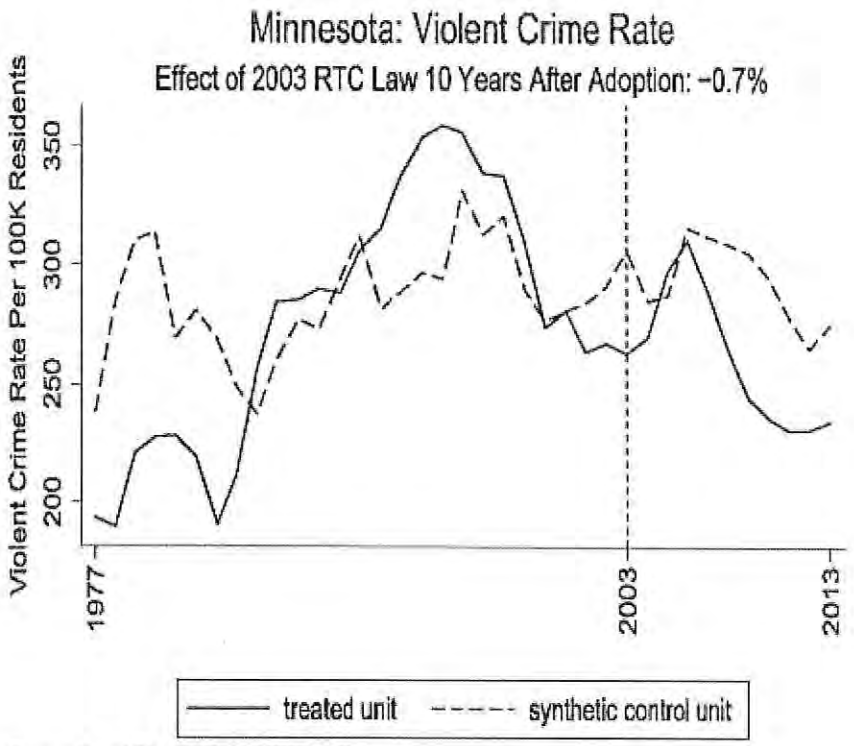
Composition of SC: CA (3.8%), DE (20.7%), IL (75.6%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE
RTC-Adopting States Included in Synthetic Control: IL (2014)



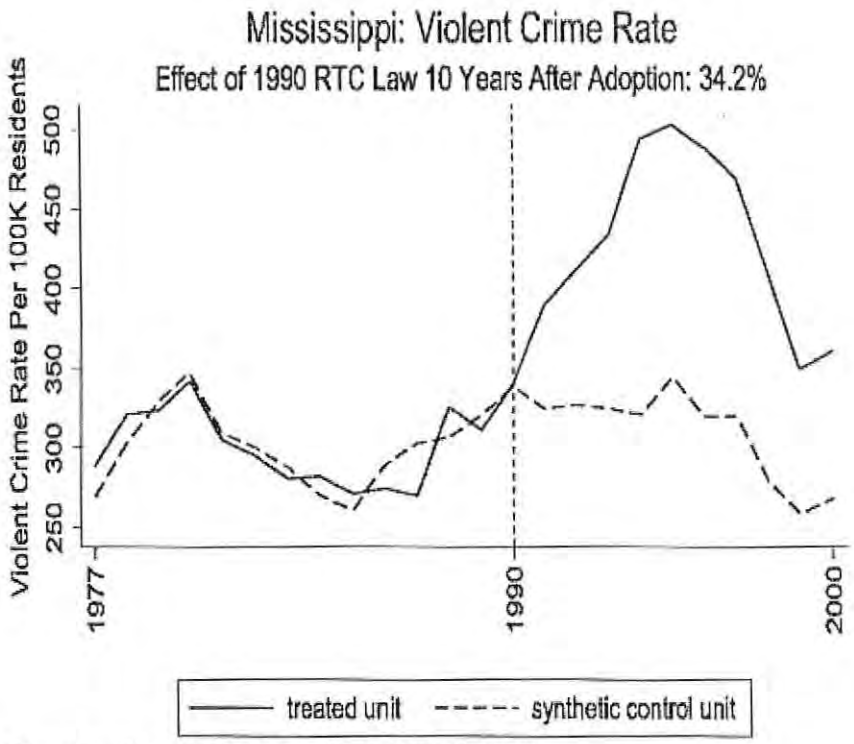
Composition of SC: HI (16.0%), IA (84.0%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC−Adopting States Included In Synthetic Control: IA (2011)



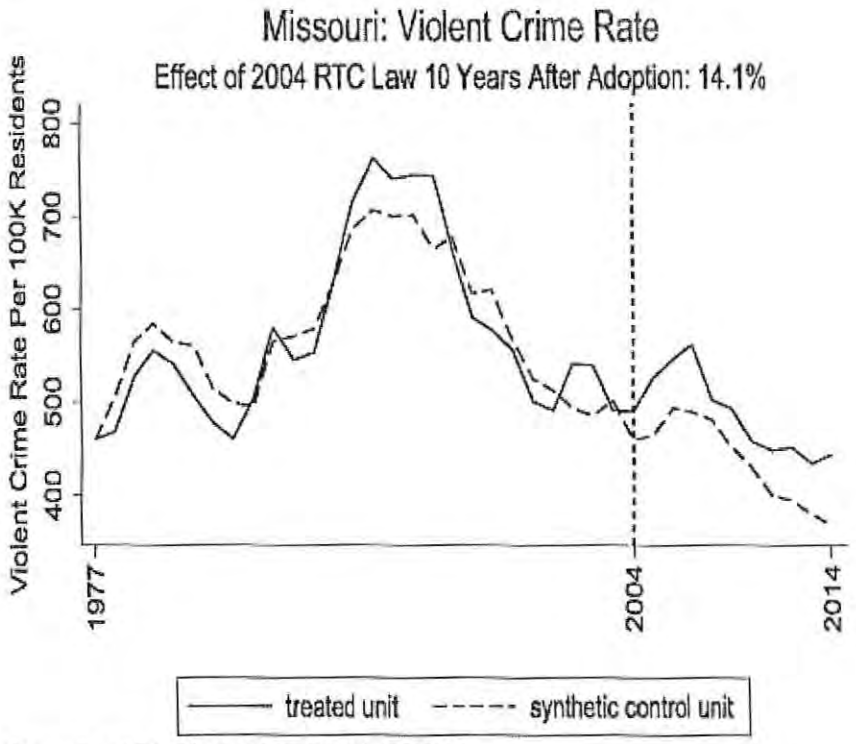
Composition of SC: MD (36.2%), NY (33.4%), WI (30.3%)
States Never Passing RTC Laws Included in Synthetic Control: MD, NY
RTC-Adopting States Included in Synthetic Control: WI (2012)



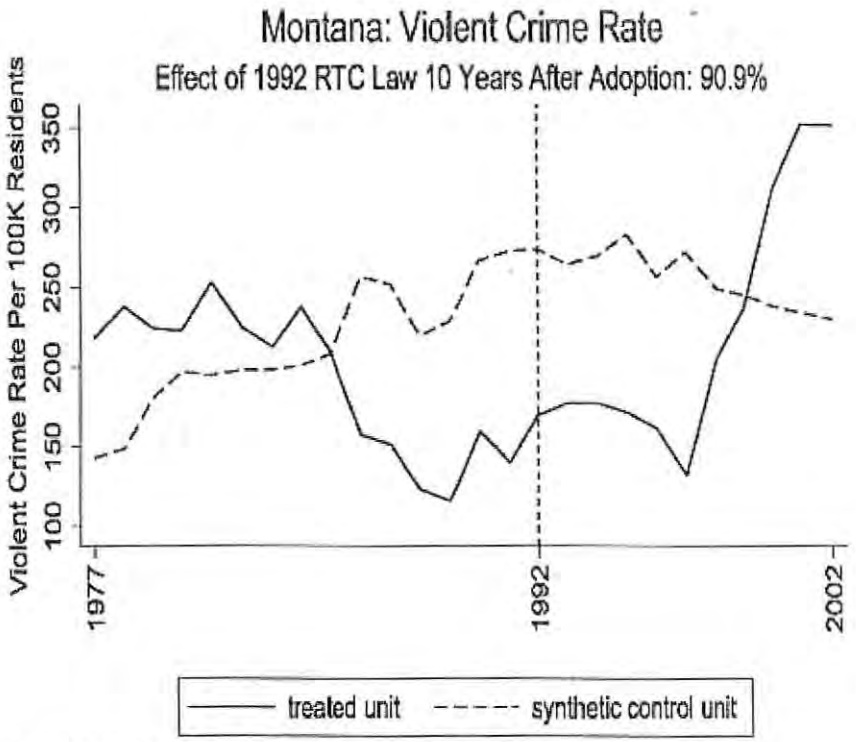
Composition of SC: DE (8.4%), HI (91.6%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI
RTC-Adopting States Included in Synthetic Control:



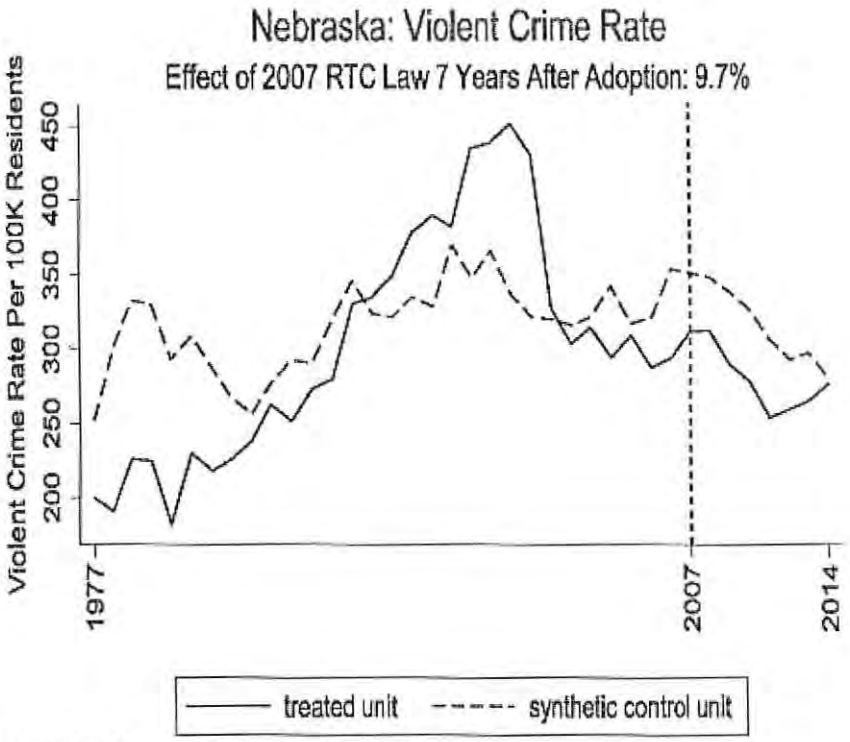
Composition of SC: HI (72.1%), IA (1.6%), NE (1.1%), OH (25.2%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC-Adopting States Included In Synthetic Control: IA (2011), NE (2007), OH (2004)



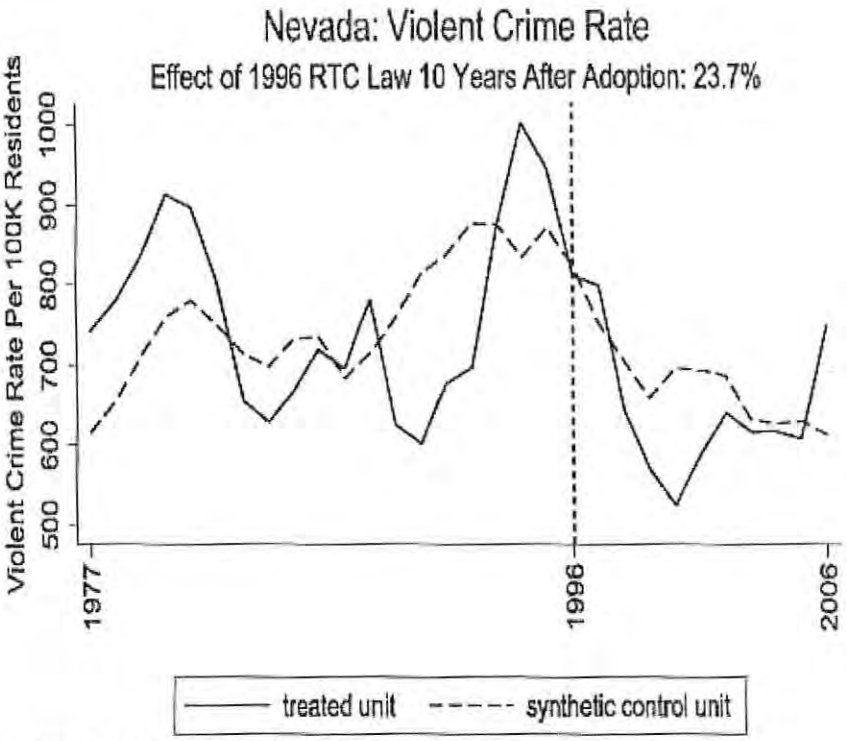
Composition of SC: CA (40.1%), DE (26.5%), HI (33.3%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE, HI
RTC-Adopting States Included in Synthetic Control:



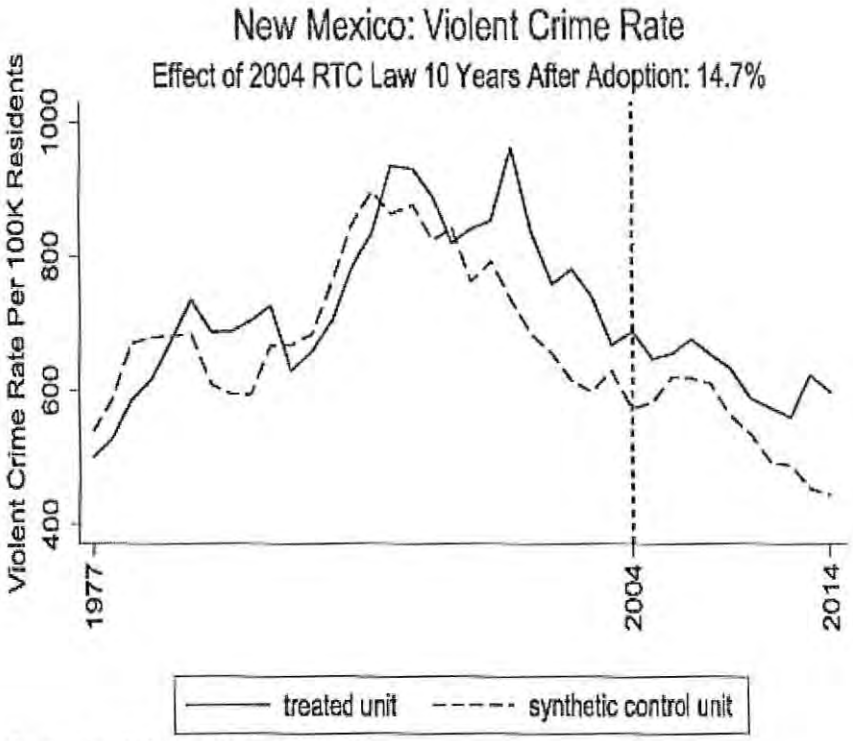
Composition of SC: HI (12.1%), WI (87.9%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC-Adopting States Included in Synthetic Control: WI (2012)



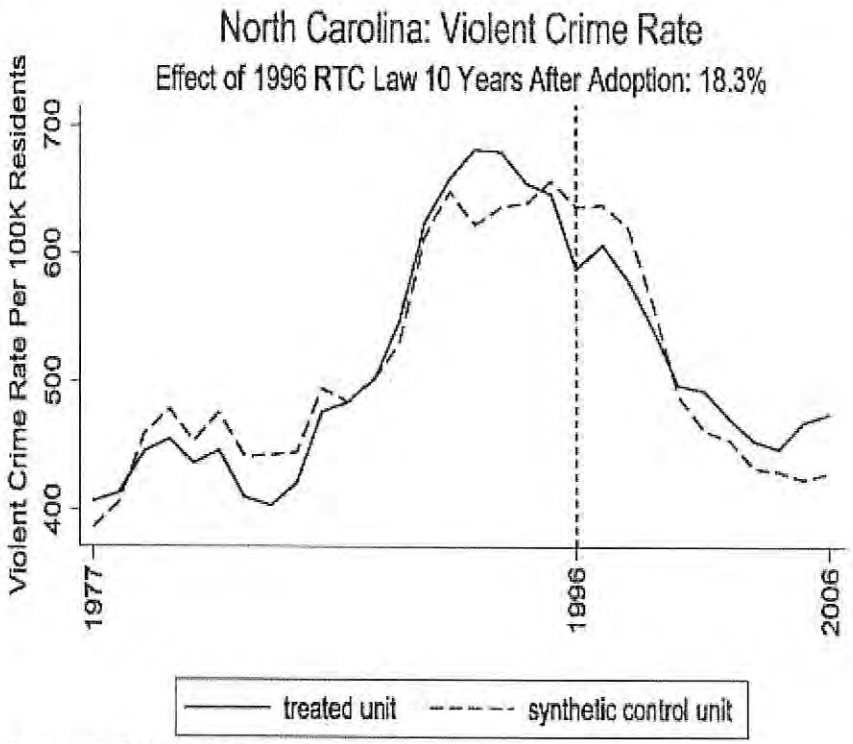
Composition of SC: DE (17.3%), HI (82.7%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI
RTC-Adopting States Included In Synthetic Control:



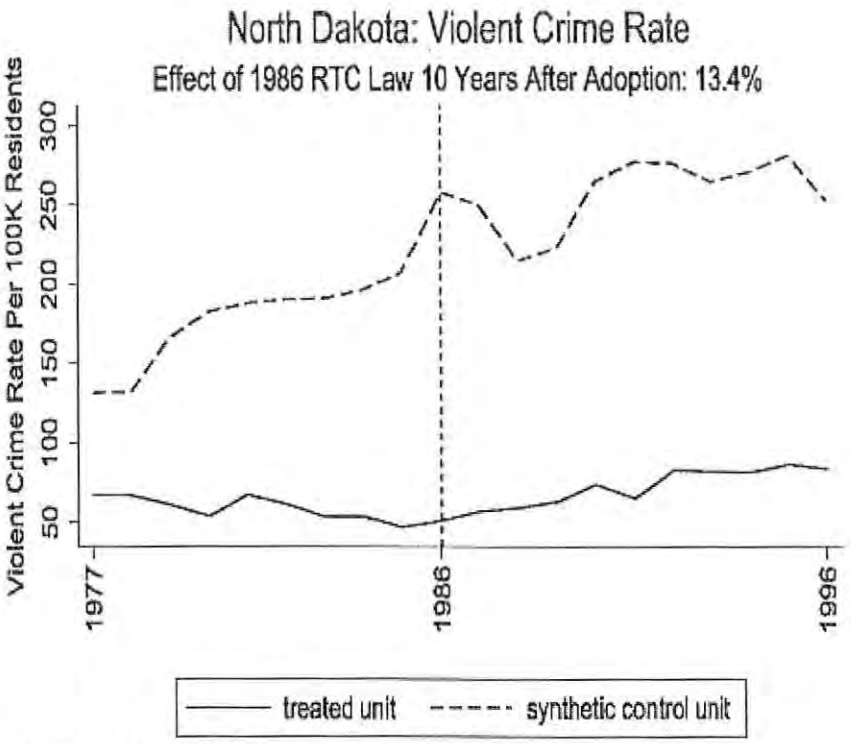
Composition of SC: HI (16.7%), MD (83.3%)
States Never Passing RTC Laws Included in Synthetic Control: HI, MD
RTC-Adopting States Included In Synthetic Control:



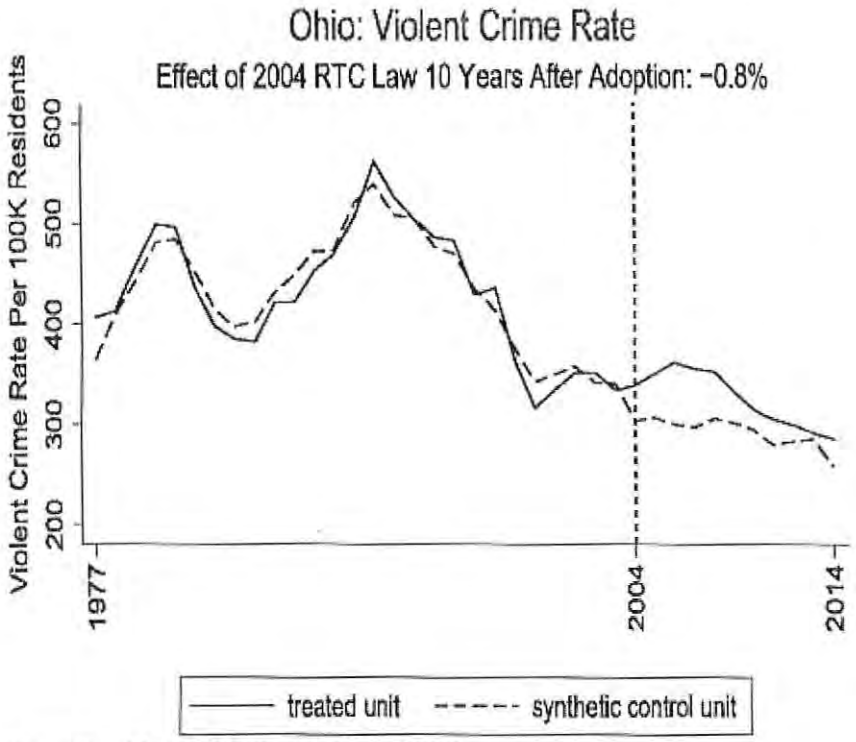
Composition of SC: CA (48.5%), DE (51.5%)
States Never Passing RTC Laws Included in Synthetic Control: CA, DE
RTC-Adopting States Included In Synthetic Control:



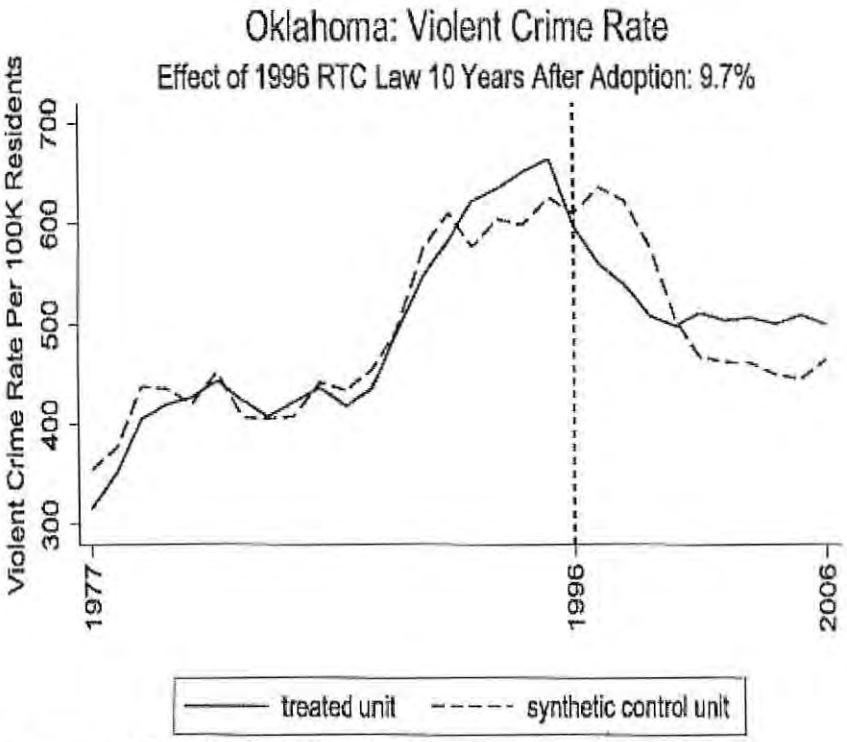
Composition of SC: DE (9.2%), IL (39.6%), NE (51.2%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC-Adopting States Included In Synthetic Control: IL (2014), NE (2007)



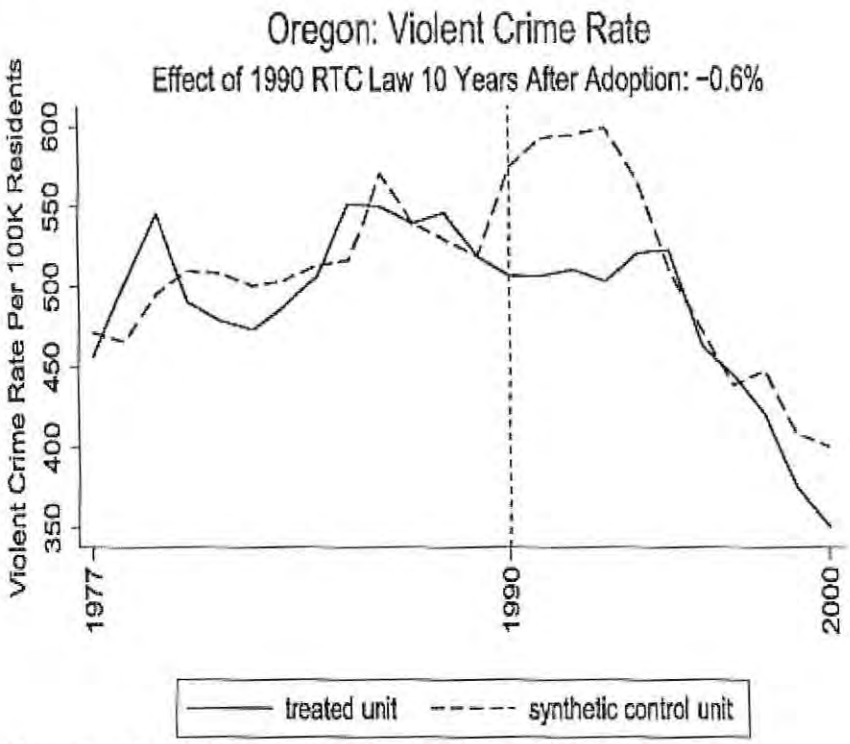
Composition of SC: WI (100.0%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC-Adopting States Included in Synthetic Control: WI (2012)



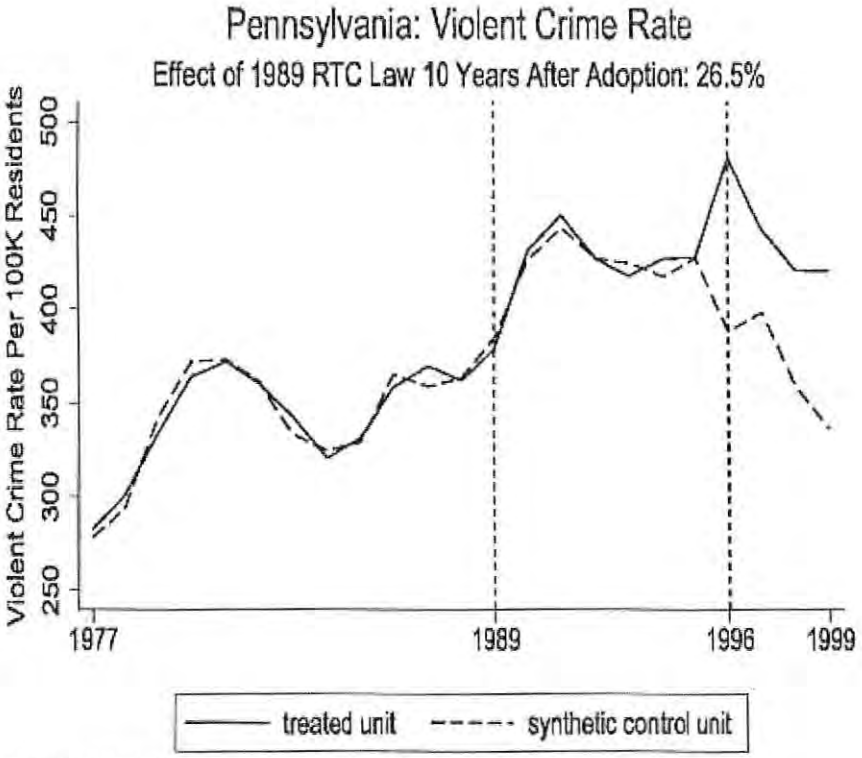
Composition of SC: CA (19.5%), HI (20.7%), RI (59.8%)
States Never Passing RTC Laws Included in Synthetic Control: CA, HI, RI
RTC-Adopting States Included In Synthetic Control:



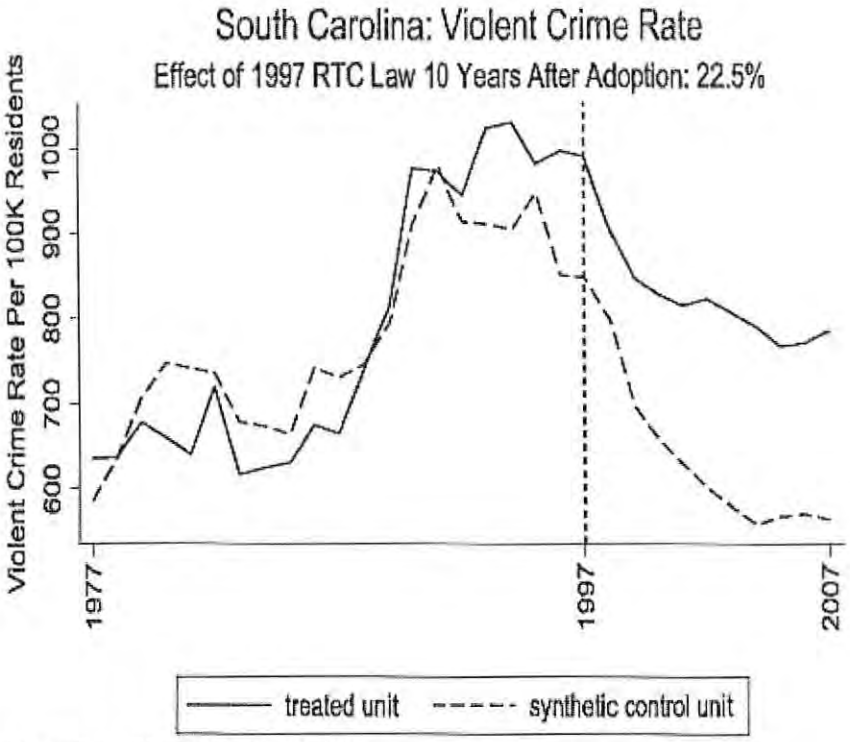
Composition of SC: DE (27.0%), IL (24.6%), NE (48.4%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC-Adopting States Included In Synthetic Control: IL (2014), NE (2007)



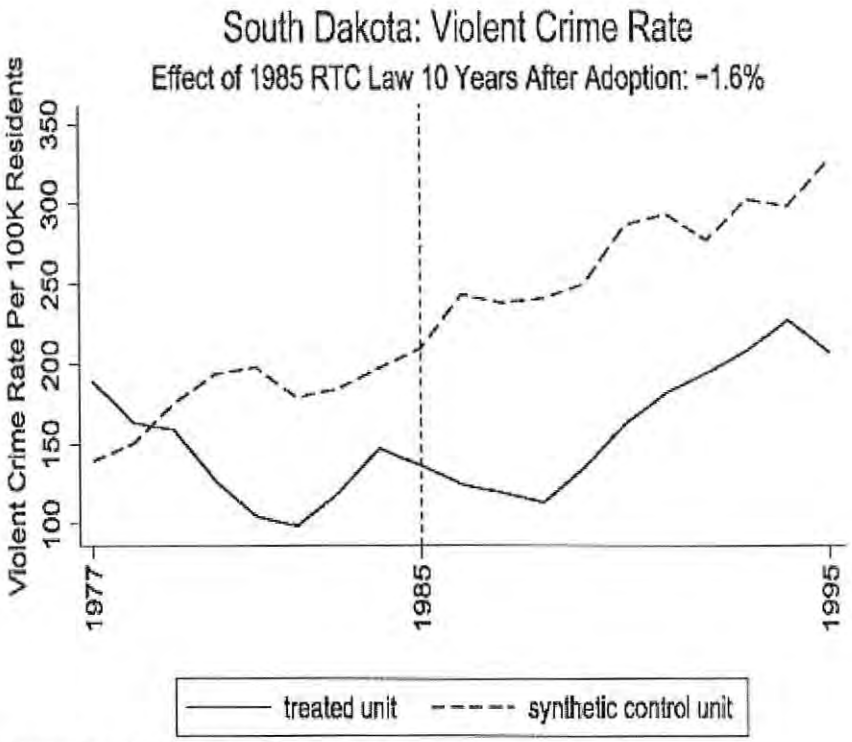
Composition of SC: CA (1.4%), CO (42.0%), HI (6.2%), MI (34.4%), MN (16.1%)
States Never Passing RTC Laws Included in Synthetic Control: CA, HI
RTC-Adopting States Included In Synthetic Control: CO (2003), MI (2001), MN (2003)



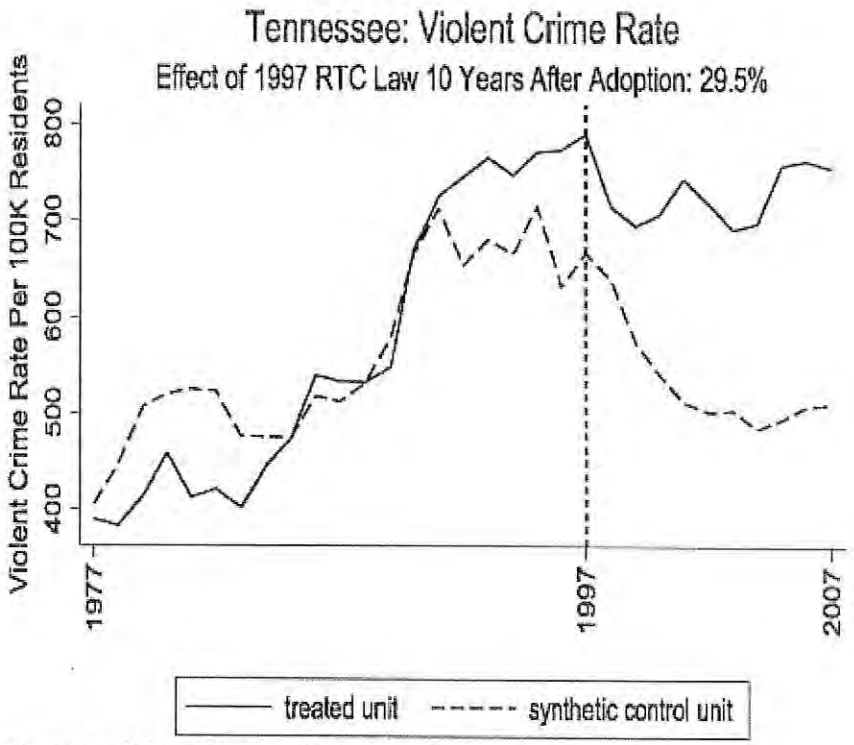
Composition of SC: DE (7.7%), HI (10.6%), NE (4.3%), NJ (17.0%), OH (25.6%), WI (34.8%)
States Never Passing RTC Laws Included in Synthetic Control: DE, HI, NJ
RTC-Adopting States Included in Synthetic Control: NE (2007), OH (2004), WI (2012)



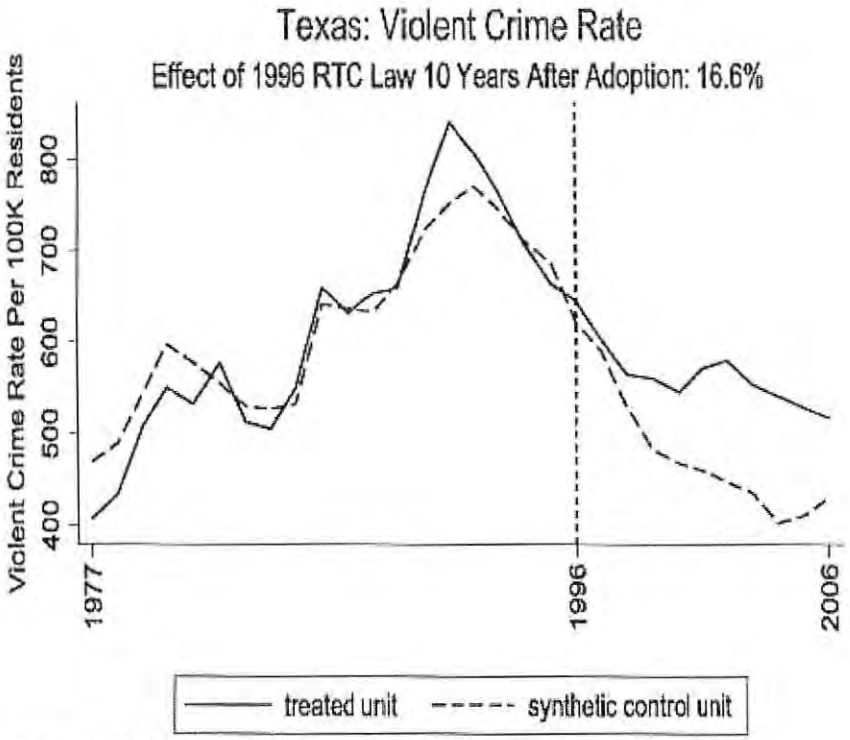
Composition of SC: DE (17.9%), IL (82.1%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC-Adopting States Included In Synthetic Control: IL (2014)



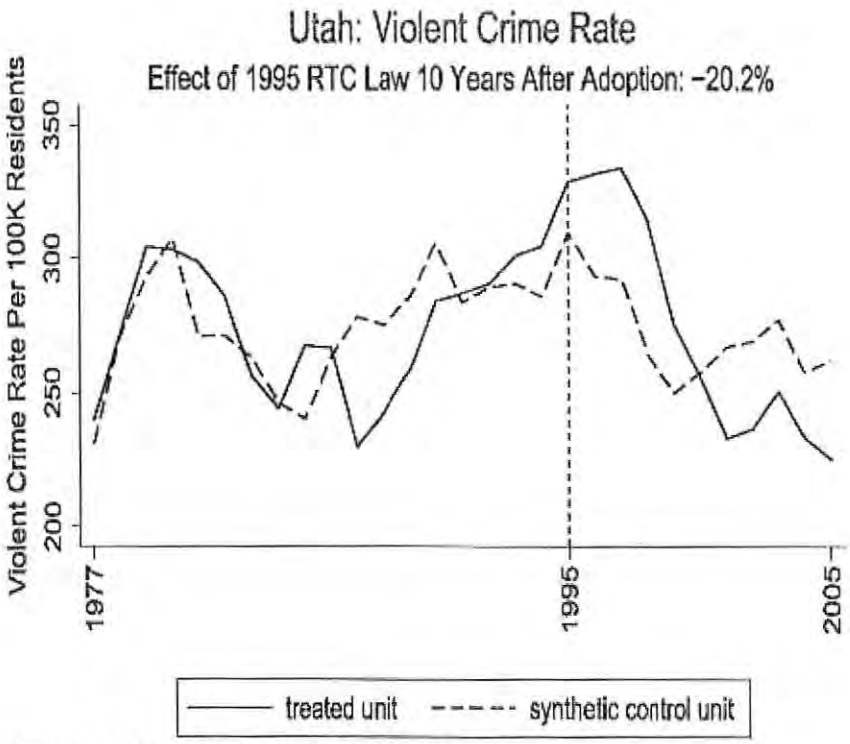
Composition of SC: IA (62.5%), WI (37.5%)
States Never Passing RTC Laws Included in Synthetic Control:
RTC-Adopting States Included in Synthetic Control: IA (2011), WI (2012)



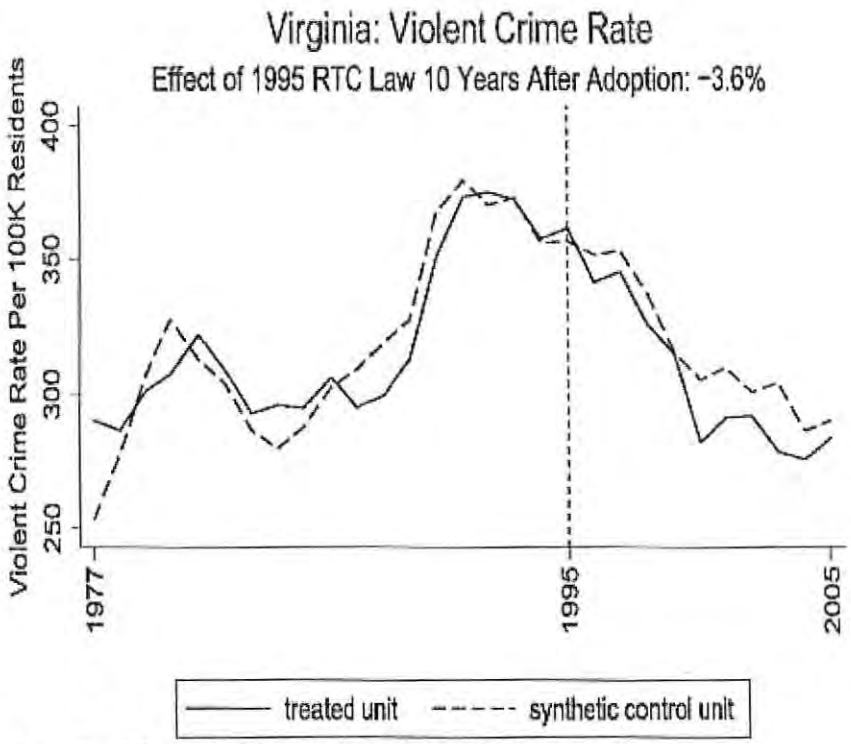
Composition of SC: DE (29.1%), IL (39.5%), IA (31.4%)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC-Adopting States Included in Synthetic Control: IL (2014), IA (2011)



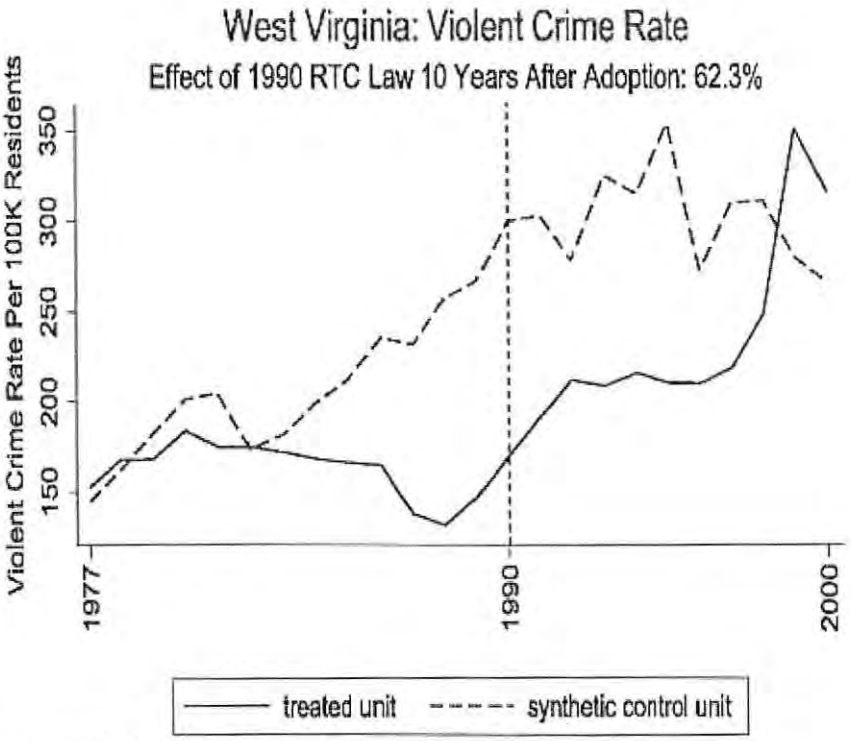
Composition of SC: CA (57.8%), NE (8.6%), WI (33.6%)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC-Adopting States Included in Synthetic Control: NE (2007), WI (2012)



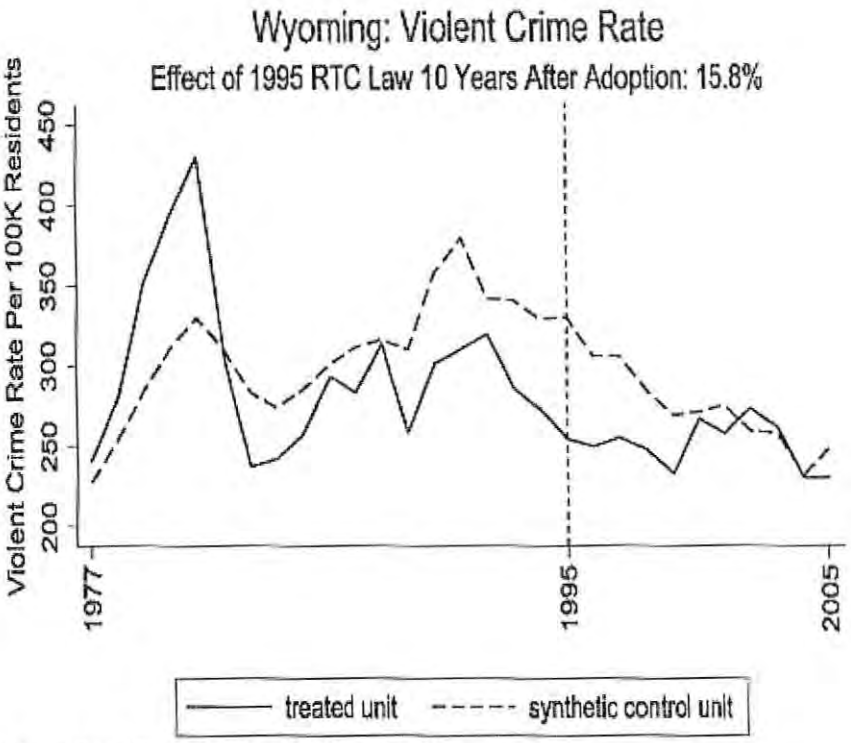
Composition of SC: HI (75.6%), KS (6.0%), RI (10.8%), WI (7.5%)
States Never Passing RTC Laws Included in Synthetic Control: HI, RI
RTC-Adopting States Included In Synthetic Control: KS (2007), WI (2012)



Composition of SC: HI (24.9%), KS (23.5%), NE (15.7%), RI (26.9%), WI (9.0%)
States Never Passing RTC Laws Included in Synthetic Control: HI, RI
RTC−Adopting States Included in Synthetic Control: KS (2007), NE (2007), WI (2012)



Composition of SC: HI (0.7%), IA (99.3%)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC-Adopting States Included in Synthetic Control: IA (2011)



Composition of SC: HI (1.5%), RI (55.5%), WI (43.1%)
States Never Passing RTC Laws Included in Synthetic Control: HI, RI
RTC-Adopting States Included in Synthetic Control: WI (2012)

Appendix E: Data Sources

Variable Name	Years Available	Modelled	Source	Notes
RTC Variables (shall-issue)	1977-2014	DAW BC LM MM	State Session Laws as gathered from Westlaw and HeinOnline	Our dummy model specification uses a variable that takes a value of one for every full year after each law takes effect and is equal to the fraction of the year that the law is in effect the first year it is implemented. Similarly our trend model specification uses a spline variable indicating the number of years post passage which takes into account the portion of the year the law was initially implemented.
Crime Variables	1977-2014	DAW BC LM MM	UCR Data Tool for data until 2012. UCR Table 5 for 2013 and 2014	Crime rates are generated by dividing the crime counts by the state-level population that the FBI uses and multiplying by 100,000.
Police Staffing	1977-2014	DAW BC	FBI Police Employment Data Set. Note: Via two CDs one for 1975-2012 and one for 2013-2014	Police employee rates are generated by dividing the total employees by the population as given in the FBI Police Employment Data Set. In BC this variable is the one year lagged log of police staffing per residential population.
State Population	1977-2014	LM MM	Intercensal population count (with the exception of 1970 and 1980)	DAW BC LM and MM models weight regressions by state population, while LM and MM models also include state population as a covariate.
State Population by Age Sex Race	1977-2014	DAW BC LM MM	Intercensal population count	DAW model includes a gross race demographic variable, while LM and MM models include BC age sex race demographic variables. BC uses 3 age groups (15-19, 20-24, 25-29) along with percentage of the black population.
Income Variables (Personal Income Unemployment Insurance Retirement Payments and Other Income Maintenance Payments)	1977-2014	DAW BC LM MM	BEA	DAW model only uses Total Per Capita Personal Income. All income measures are converted to 1982-1984 dollars and per capita values are used. BC only uses median nominal income per capita.
Incarceration Rate	1977-2014	DAW BC MM	BJS	We divide the number of prisoners under the jurisdiction of a state by the state population to generate the incarceration rate. We then lag this variable 1 year. In BC this variable is the one year lagged log of year end prisoner population per of the resident population.
Land Area	1977-2014	LM	Census	Land area for each year is based on the area of the given state calculated in the previous decadal Census. The density variable is state population divided by land area.
Poverty Rate	1979-2014	DAW MM	Census	We use the published Census estimates for the proportion of the population earning less than the poverty line each year.
Unemployment Rate	1977-2014	DAW BC MM	BLS	The unemployment rate is generated by dividing the number of unemployed individuals by the size of the labor force.
Arrest Rate	1977-2014	LM MM	CDs sent from the FBI	The FBI data contain the number of arrests for the various crime categories. For a particular crime category, the arrest rate is (Total arrests/Total crimes)*100.
Crack Index	1984-2000	MM	Donald Rostcher's web page	Following the MM model, we use the unadjusted version of the crack index.
Beer	1977-2001	DAW BC	National Institute of Health (NIH)	This variable represents the gallons of ethanol (in the form of beer) sold annually per capita in each state in each year.
Percentage of the Population Living in MSAs	1977-2014	DAW	Concatenated in the FBI SOR across state via a PSR	Values from 1977-1979 are interpolated.
Executions	1977-2014	BC	BJS Capital Punishment Series	The count of executions is lagged by one year.

Appendix F: Methodology to choose the number of lags of the dependent variable to include as inputs in synthetic controls

We use a cross validated approach to determine the optimal lag choice(s) to include as predictor(s) in the synthetic control model. We use this procedure to choose among four potential lag choices used in the synthetic control literature; these choices involve including lags of the dependent variable in every pre-treatment year, three lags of the dependent variable,[56] one lag which is the average of the dependent variable in the pre-treatment period, and one lag which is the value of the dependent variable in the year prior to RTC adoption.[57] To implement the cross validation procedure, we first define our training period as 1977 through the sixth year prior to RTC adoption, the validation period as the fifth year prior to RTC adoption through one year prior to RTC adoption, and the full pre-treatment period as 1977 through one year prior to RTC adoption. For each of our 33 treatment units, data from the training period is used to determine the composition of the synthetic control. Specifically, for each of the 33 treatment units, we assign the treatment 5 years before the treatment actually occurred, and then run the synthetic control program using the standard ADZ predictors defined in Aneja et al. (2011) and a 5 year reporting window. We then examine the fit during the training period, the validation period, and the entire pre-treatment period to see how closely the synthetic control estimate matches the value of the dependent variable for different lag choices.

Tables A11-A13 examine the fit of the synthetic control estimate during the training period, validation period, and the entire pre-treatment period using three different loss functions. Table A11 defines the error using the mean squared error between the actual value of the dependent variable and the synthetic control estimate during a given period; Table A12 uses the mean of the absolute value of the difference between the treated value and synthetic control estimate; finally, Table A13 uses the CV of the RMSPE. For Tables A11-A13, an unweighted average of the error for each of the 33 treatment states is presented. For Tables A14-A16, a population weighted average of the error for each of the 33 treatment states is presented, where population from the first year of the relevant period is used.[58]

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

A potential concern with using all preintervention outcomes of the dependent variable as synthetic control predictors is that the synthetic control unit will not closely match the treated unit on the non-lagged predictors during the pre-treatment period.[59] But as Table A17 shows, we do not find that the synthetic control unit's fit on the non-lagged predictors is worse using yearly lags. To generate the numbers in Table A17, for each treatment state, we first take a simple average of our predictor of interest over all pre-treatment years (1977 through the year prior to RTC adoption). A population weighted average of the predictor pre-treatment means is then taken over all treatment states to reach the figures presented, which represent an aggregate

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

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

[58] 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.

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

measure of the pre-treatment predictor means.[60] Based on the absolute value of the difference between the aggregate treated predictor means and the aggregate synthetic control predictor means, yearly lags has the second best performance. The aggegate synthetic control predictor means using yearly lags comes closest or second closest to the treated unit for 9/16 predictors. In comparison, one lag that is the average of the dependent variable in the pre-treatment period comes closest or second closest for 11/16 predictors, one lag that is the value of the dependent variable in the last pre-treatment year comes closest or second closest for 7/16 predictors, and three lags for 5/16 predictors.

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

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

Table A11: Comparison of Fit Across various Lagchoices; Define fit using Mean Squared Error

	training period; Mean Squared Error	validation period; Mean Squared Error	full pre-treatment period; Mean Squared Error
three lags	3,162.41	7,425.12	3,837.18
yearly lags	1,398.28	8,864.02	3,055.11
one lag average	3,440.30	7,799.31	4,330.60
one lag final pre-treatment year	3,605.44	7,369.31	4,511.81

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 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1

Table A12: Comparison of Fit Across various Lagchoices; Define fit using Mean Absolute Difference

	training period; Mean Absolute Difference	validation period; Mean Absolute Difference	full pre-treatment period; Mean Absolute Difference
three lags	30.75	54.19	41.61
yearly lags	33.35	41.39	32.78
one lag average	39.65	53.46	48.88
one lag final pre-treatment year	51.45	52.46	43.65

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 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1

Table A13: Comparison of Fit Across various Lagchoices; Define fit using CVRMSPE

	training period; CVRMSPE	validation period; CVRMSPE	full pre-treatment period; CVRMSPE
three lags	0.18	0.25	0.18
yearly lags	0.20	0.28	0.17
one lag average	0.16	0.26	0.20
one lag final pre-treatment year	0.13	0.24	0.18

Notes: After getting a measure of fit for each state, an unweighted average is taken to arrive at a single measure of fit. Training Period from 1977 through 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1

Table A14: Comparison of Fit Across various Lagchoices; Define fit using Mean Squared Error

	training period; Mean Squared Error	validation period; Mean Squared Error	full pre-treatment period; Mean Squared Error
three lags	[illegible]	[illegible]	[illegible]
yearly lags	[illegible]	[illegible]	[illegible]
one lag average	[illegible]	[illegible]	[illegible]
one lag final pre-treatment year	[illegible]	[illegible]	[illegible]

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 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1. Population from first year of relevant period is used

Table A15: Comparison of Fit Across various Lagchoices; Define fit using Mean Absolute Difference

	training period; Mean Absolute Difference	validation period; Mean Absolute Difference	full pre-treatment period; Mean Absolute Difference
three lags	[illegible]	[illegible]	[illegible]
yearly lags	[illegible]	[illegible]	[illegible]
one lag average	[illegible]	[illegible]	[illegible]
one lag final pre-treatment year	[illegible]	[illegible]	[illegible]

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 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1. Population from first year of relevant period is used

Table A16: Comparison of Fit Across various Lagchoices; Define fit using CVRMSPE

	training period; CVRMSPE	validation period; CVRMSPE	full pre-treatment period; CVRMSPE
three lags	[illegible]	[illegible]	[illegible]
yearly lags	[illegible]	[illegible]	[illegible]
one lag average	[illegible]	[illegible]	[illegible]
one lag final pre-treatment year	[illegible]	[illegible]	[illegible]

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 1970 year - 8; Validation Period from 1970 year - 5 through 1970 year - 1. Population from first year of relevant period is used

Table A17: Crime Predictor Means Before RTC Adoption

	treated	Synthetic: 3 lags	Synthetic: yearly lags	Synthetic: 1 lag avg	Synthetic: 1 lag final pre-treatment year
popstatecensus	7,459,183	8,026,132	8,479,127	7,278,594	9,161,988
l_incarc_rate	224.51	189.64	194.12	192.32	197.40
l_policeemployeerate0	248.41	272.85	276.75	275.58	271.52
rpcpi	12,827.91	14,382.73	14,450.62	14,430.30	14,464.70
rpcui	65.70	81.31	80.67	81.32	81.30
rpcim	186.27	200.49	202.72	192.14	204.76
rpcrpo	1,427.63	1,451.61	1,454.97	1,475.17	1,447.78
unemployment_rate	6.81	6.17	6.19	6.09	6.22
poverty_rate	14.61	12.13	12.32	11.89	12.07
density	123.51	262.32	236.30	309.65	282.99
age_bm_1019	1.26	0.71	0.76	0.82	0.76
age_bm_2029	1.11	0.71	0.75	0.80	0.75
age_bm_3039	0.83	0.53	0.56	0.62	0.57
age_wm_1019	6.68	6.23	6.21	6.31	6.25
age_wm_2029	7.11	7.12	7.09	7.14	7.16
age_wm_3039	6.45	6.22	6.22	6.28	6.28

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