Appendix B: Complete Regression Output

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

Panel A: Dummy Variable Model Results

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

Panel B: Spline Model Results

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Standard errors in parentheses

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

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

Results reported for the constant term resulting from this regression

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

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

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

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

Standard errors in parentheses

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

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

Results reported for the constant term resulting from this regression

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

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

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

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

Standard errors in parentheses

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

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

Results reported for the constant term resulting from this regression

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

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

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

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

Standard errors in parentheses

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

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

Results reported for the constant term resulting from this regression

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

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

Appendix D: Data Methodologies

I. Data Issues

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

Annual state-level crime rates are taken from the FBI's Uniform Crime Reporting program.[42] Four state-level income variables (personal income, income maintenance payments, retirement payments, and unemployment insurance payments) are taken from the BEA's Regional Economic Accounts. The personal income, income maintenance, and unemployment insurance payment variables are estimated in real per capita terms (defined using the CPI). The the LM and MM specifications use alternative versions of the retirement variable that are described in footnote 41. State-level population is generated using the Census Bureau's intercensal population estimates, while the proportional size of LM's 36 age-race-sex demographic groups are estimated using state-level population by age, sex, and race gathered by the Census. (In cases where the most recent form of these data were not easily accessible at the state level, state-level figures were generated by aggregating the Census Bureau's county-level population estimates by age, sex, and race.) Population density is estimated by dividing a given observation's population by the area of that state reported in the previous decennial census. State-level unemployment rate data is taken from the Bureau of Labor Statistics, while the poverty rate is taken from two Census series (the 1979 state-level poverty rate is derived from the Decennial Census and the 1980-2014 poverty rates are generated using the Current Population Survey). A measure of incarceration (incarcerated individuals per 100,000

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

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

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

No data for the crack cocaine index that we use was available for the District of Columbia, and our matching methodology does not allow the District of Columbia to be included in our analysis in specifications that include this variable as a predictor. After considering several different ways to confront this issue, we ultimately decided to exclude the District of Columbia from the synthetic controls analysis owing to its status as a clear outlier whose characteristics are less likely to be meaningfully predictive for other geographic areas. Abadie, Diamond, and Hainmueller (2010) emphasize that researchers may want to "[restrict] the comparison group to units that are similar to the exposed units [in terms of the predictors which are included in the model]" (496). Given that the District of Columbia had the highest per capita personal income, murder rate, unemployment rate, poverty rate, and population density at various points in our sample, Abadie's admonition would seem to support omitting the District as one of our potential control units.[44] We should note that even if we include DC in the synthetic controls estimates, it still shows RTC laws increase violent crime by 13.2 percent in the tenth year (as opposed to the 14.7 percent figure shown in Table 9).

We consider two separate police measures for the purposes of our analysis. Our reported results are based on the same police variable that we used in Aneja, Donohue, and Zhang (2014). To construct this variable, we take the most recent agency-level data provided by the FBI and use this information to estimate the number of full-time police employees present in each state per 100,000

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

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

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

II. The Dates of Adoption of RTC Laws

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

While there have been numerous disagreements about the exact laws that should be used to determine when states made the transition from a "may issue" to a "shall issue" state, we believe that the dates used in this paper accurately reflect the year when different states adopted their RTC law. We supplemented our analysis of the statutory history of RTC laws in different states with

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

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

an extensive search of newspaper archives to ensure that our chosen dates represented concrete changes in concealed carry policy. We extensively document the changes that were made to our earlier selection of right-to-carry dates and the rationales underlying these changes in Appendix G of Aneja, Donohue, and Zhang (2014). It is important to note that the coding of these dates may not reflect administrative or logistical delays that may have prevented the full implementation of a RTC law after authorities were legally denied any discretion in rejecting the issuing of RTC permits. Ideally, a researcher would be able to control for the actual level of RTC permits in existence each year for each state. Although this data would be preferable to a mere indicator variable for the presence of an RTC law, such comprehensive information unfortunately is not available.

Appendix E: Replicating Our Analysis

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

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

> "When more than one processor is used in Stata/MP, the computations for the likelihood are split into pieces (one piece for each processor) and then are added at the end of the calculation on each iteration. Because of round-off error, addition is not associative in computer science as it is in mathematics. This may cause a slight difference in results. For example, a1+a2+a3+a4 can produce different results from (a1+a2)+(a3+a4) in numerical computation. When changing the number of processors used in Stata, the order in which the results from each processor are combined in calculations may not be the same depending on which processor completes its calculations first."[47]

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

Another discrepancy that we encountered is that memory limitations sometimes caused our synthetic control analyses to crash when using the *nested* option. When this occurred, we would generate our synthetic control using the regression-based technique for determining the relative weights assigned to different predictors. We encountered this situation several times when running our Stata code on standard desktop computers, and these errors occurred less often when using

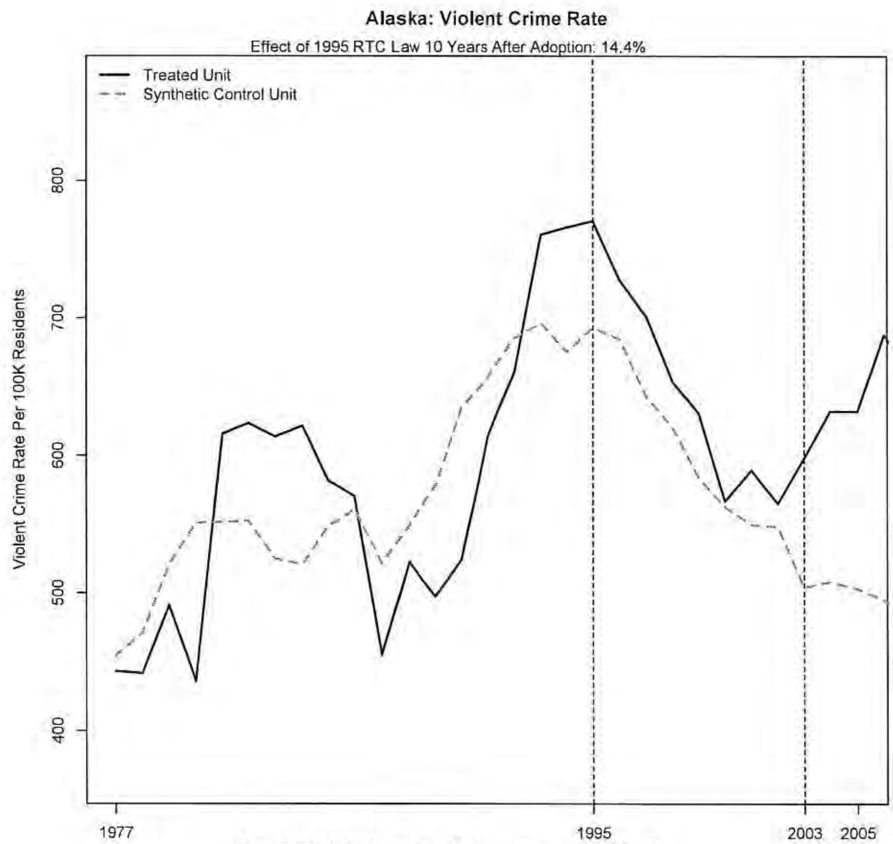
[47] This memo can be found at the following link: http://www.webcitation.org/6YeLV03SN.

more powerful computers with greater amounts of memory. For this reason, to replicate our results with the greatest amount of precision, we would recommend that other researchers run our code on the same machines that we ran our own analysis: a 24-core UNIX machine with 96GB of RAM running Stata/MP.

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

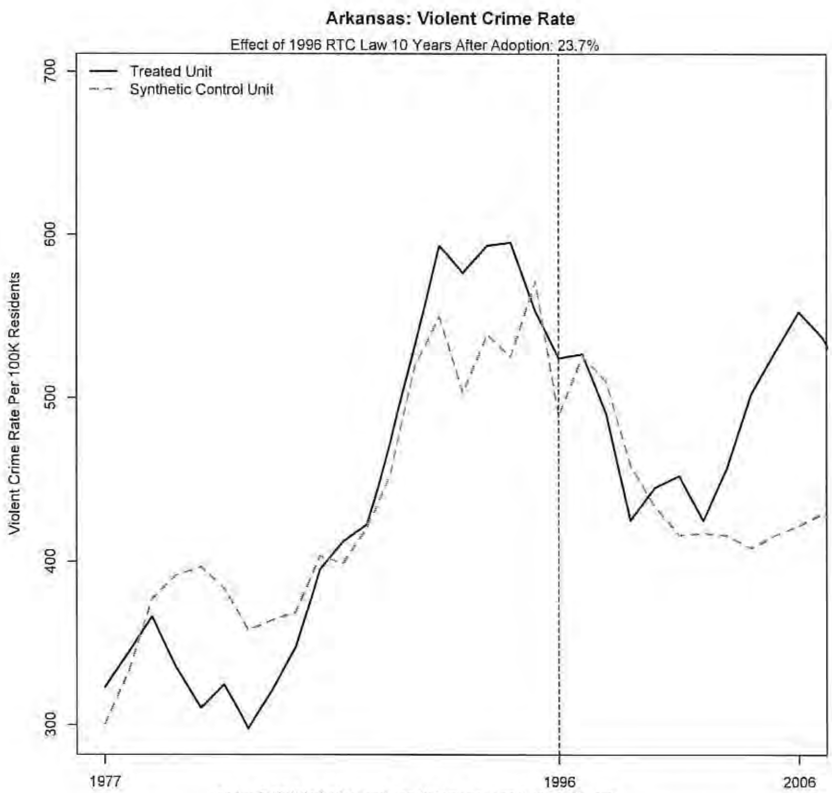
Appendix F: Synthetic Control Graphs Estimating Impact of RTC Laws On Violent Crime Using the DAW Model[48]

Figures A1-A33

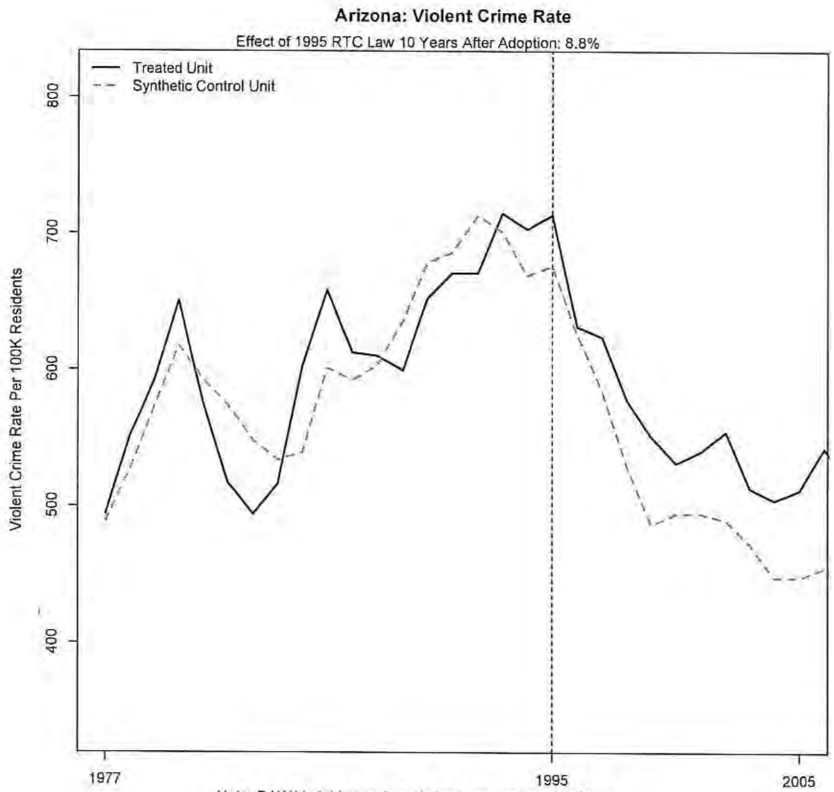


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: MD (0.519); NE (0.481)
CVRMSPE: 0.119 (21 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: MD
RTC Adopting States Included in Synthetic Control: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

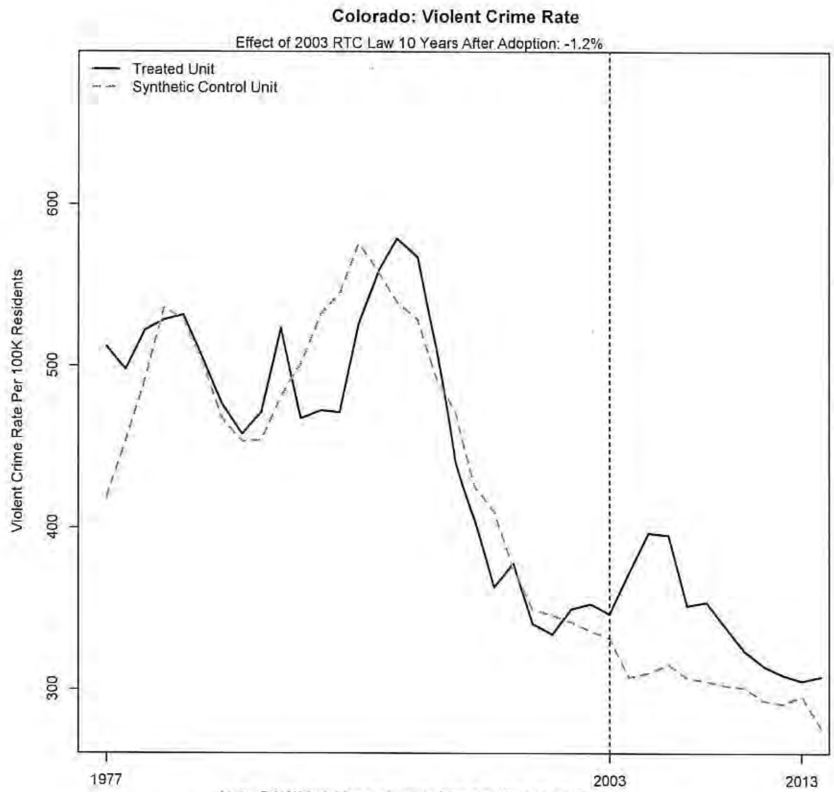
[48]Recall that each state's effective year of passage is defined as the first year in which a RTC law was in effect for the majority of that year.



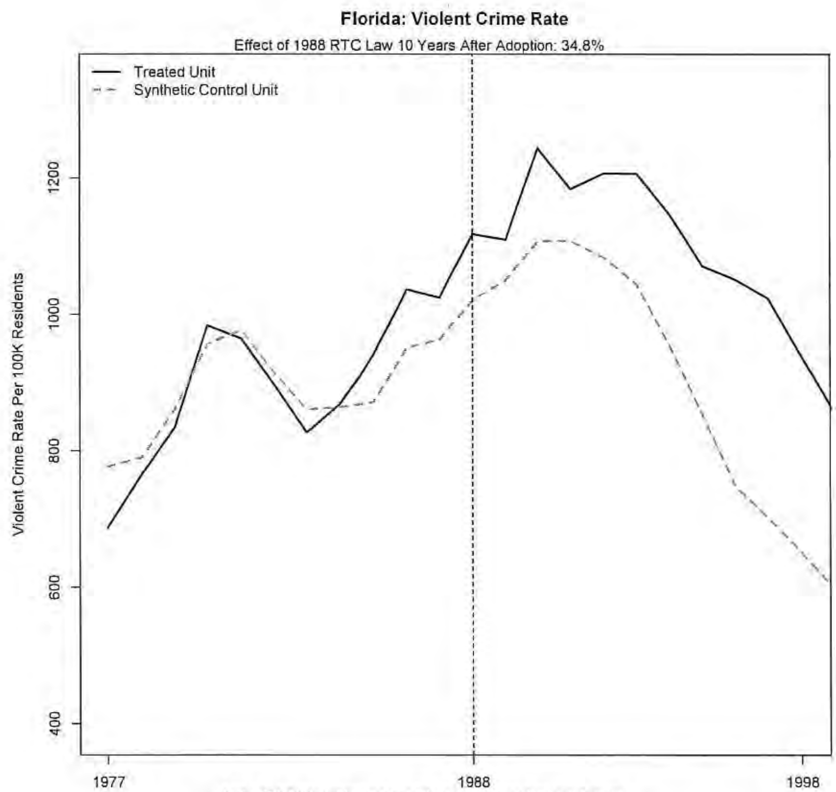
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.186); IL (0.231); IA (0.584)
CVRMSPE: 0.103 (20 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic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



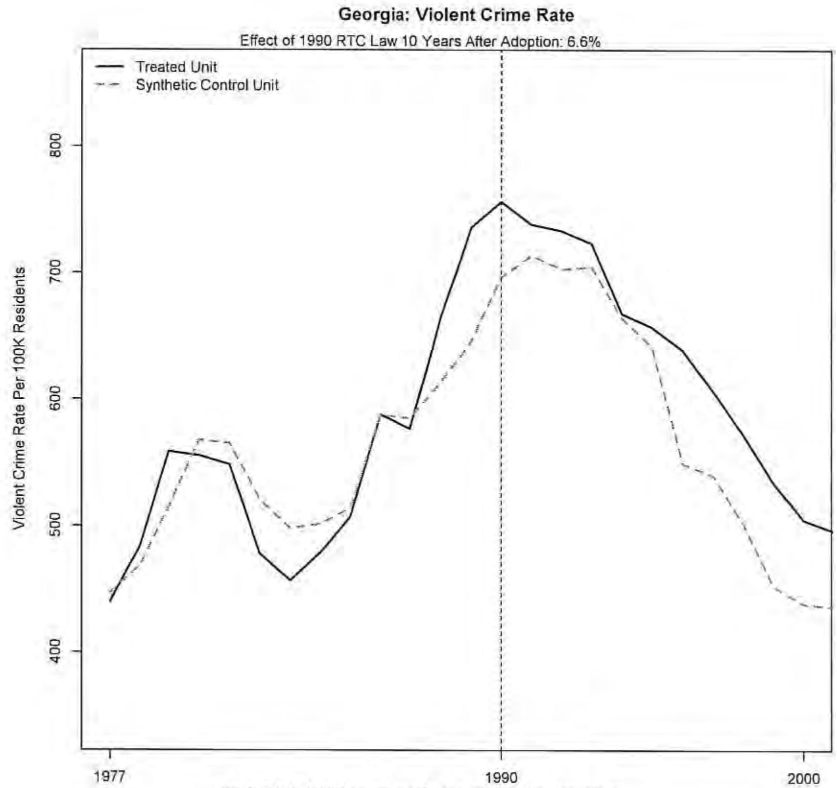
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.327); HI (0.409); MD (0.229); NE (0.035)
CVRMSPE: 0.058 (9 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI ; MD
RTC Adopting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



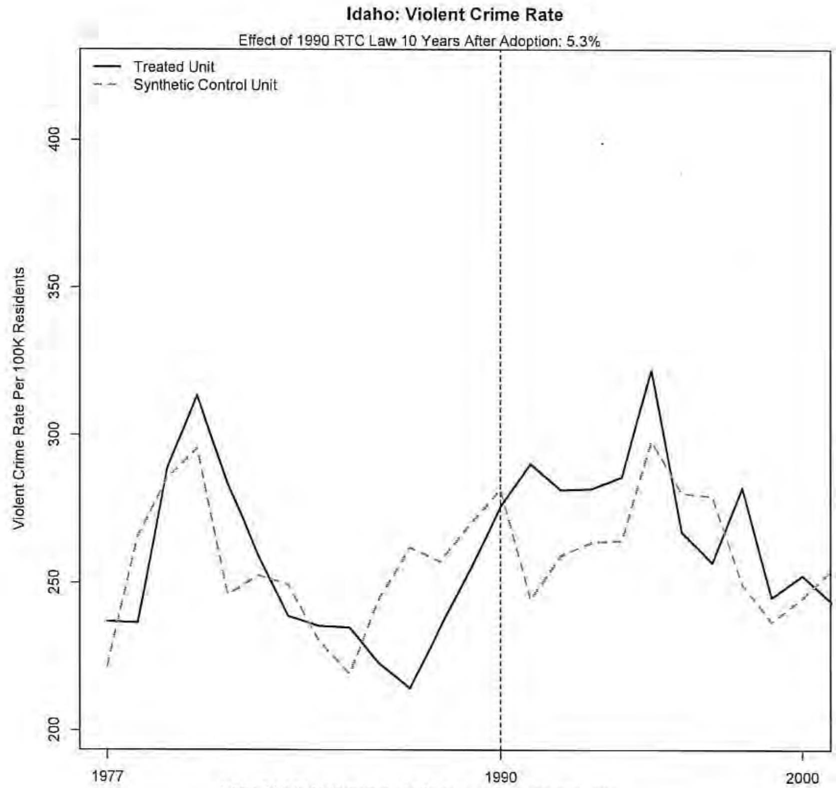
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.499); NY (0.293); RI (0.208)
CVRMSPE: 0.077 (16 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; NY ; RI
RTC Adopting States Included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



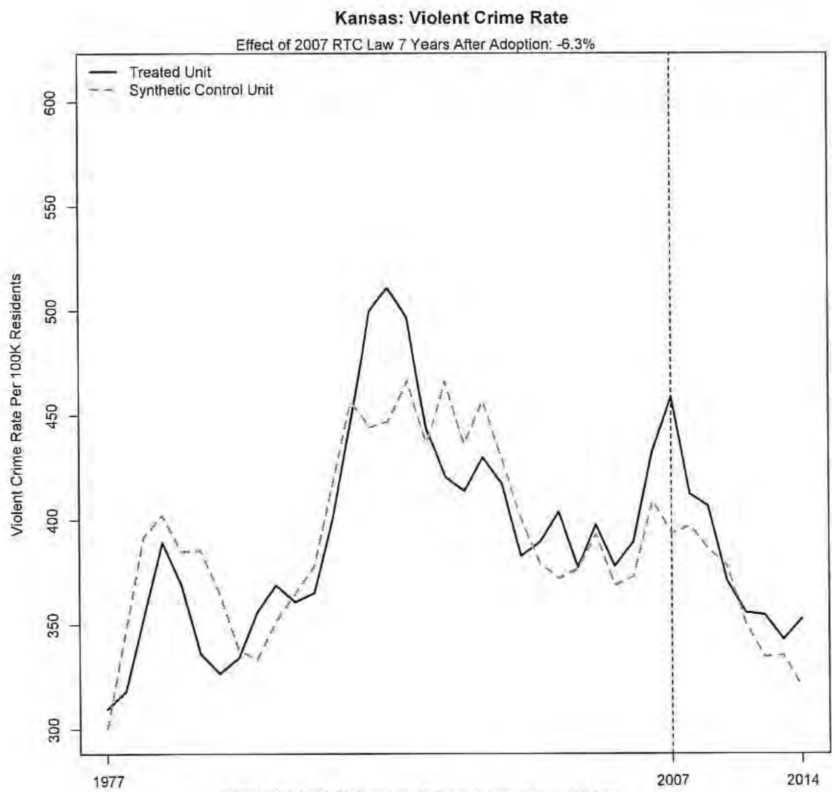
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.223); MI (0.11); NY (0.667)
CVRMSPE: 0.056 (8 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States Included in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



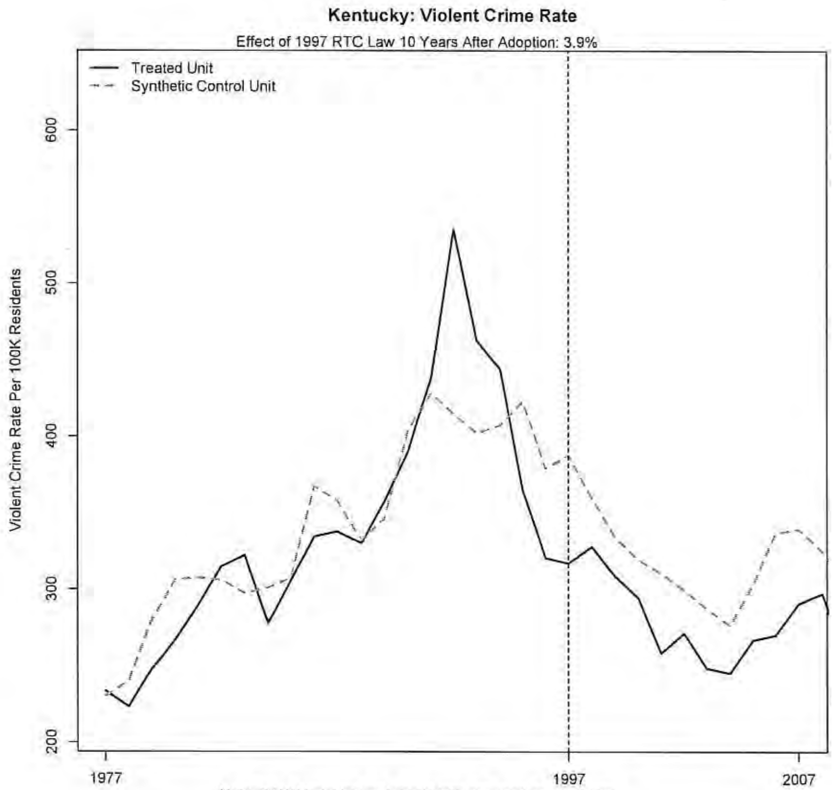
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.301); IA (0.452); MO (0.097); NY (0.15)
CVRMSPE: 0.068 (12 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; NY
RTC Adopting States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



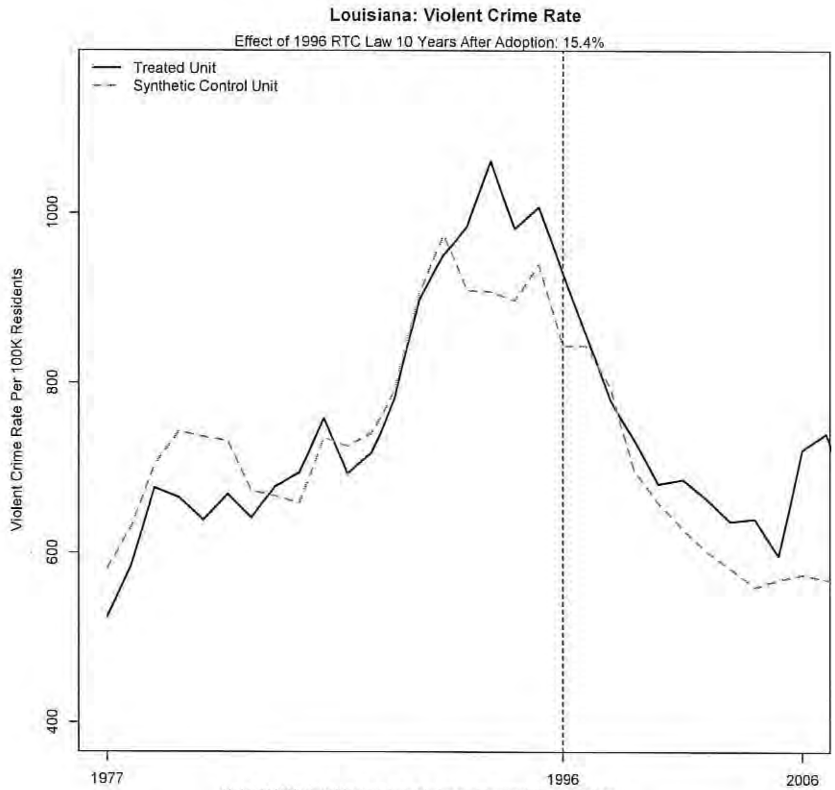
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.961); IA (0.039)
CVRMSPE: 0.091 (19 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: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



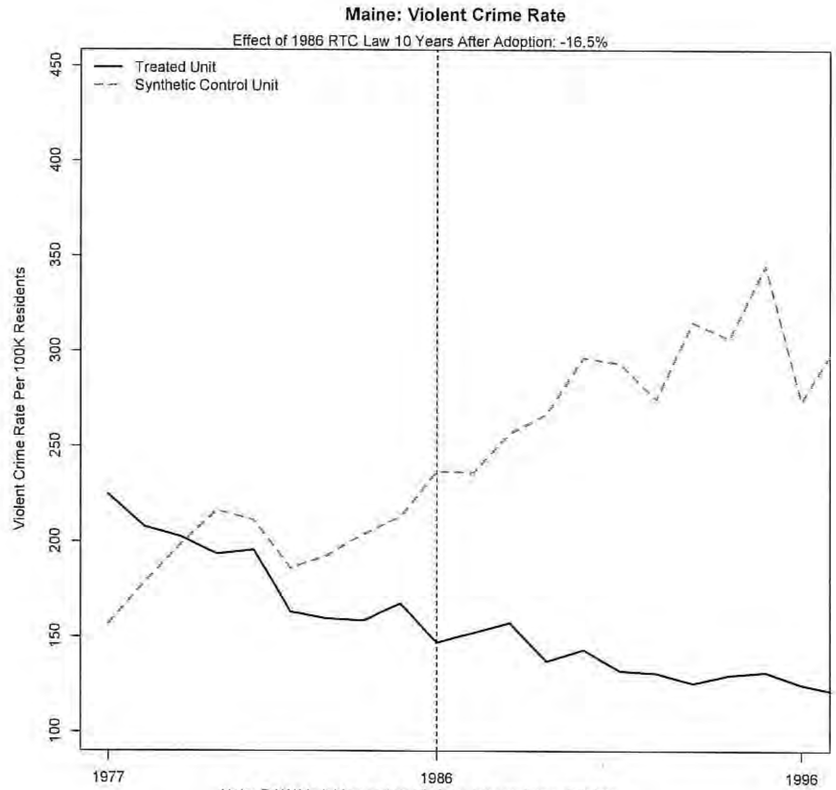
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.183); HI (0.583); MA (0.235)
CVRMSPE: 0.069 (13 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI ; MA
RTC Adopting States Included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



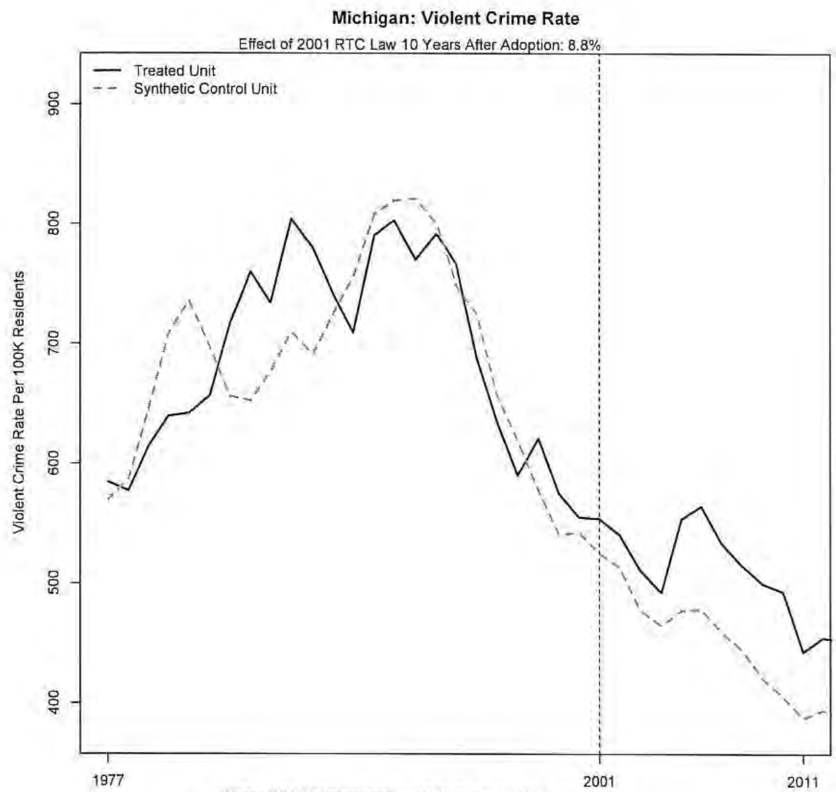
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: IL (0.198); WI (0.802)
CVRMSPE: 0.119 (22 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: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



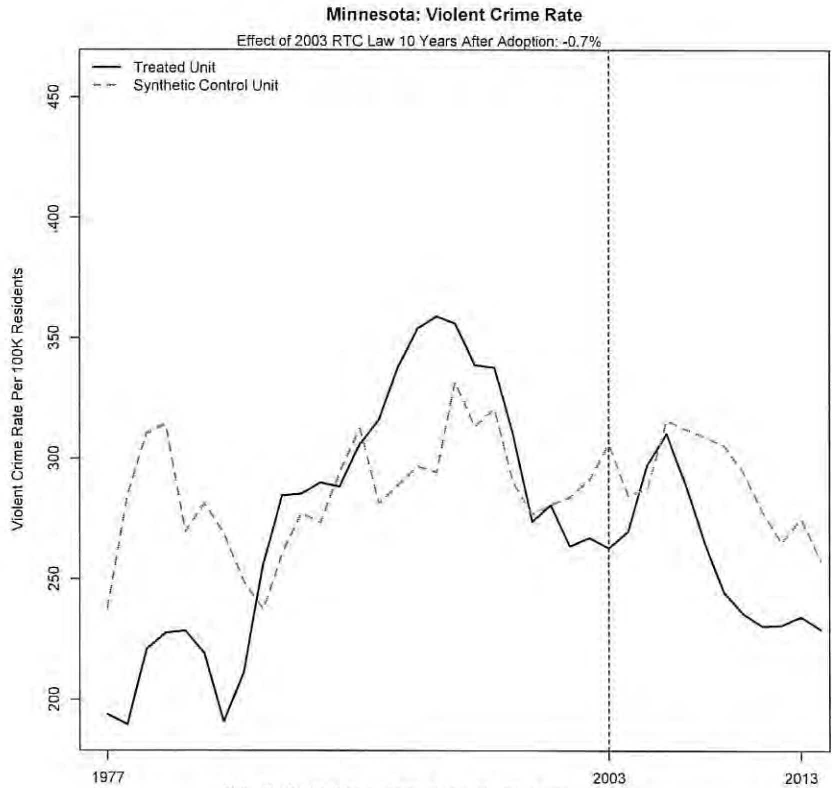
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.038); DE (0.207); IL (0.756)
CVRMSPE: 0.08 (17 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included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



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



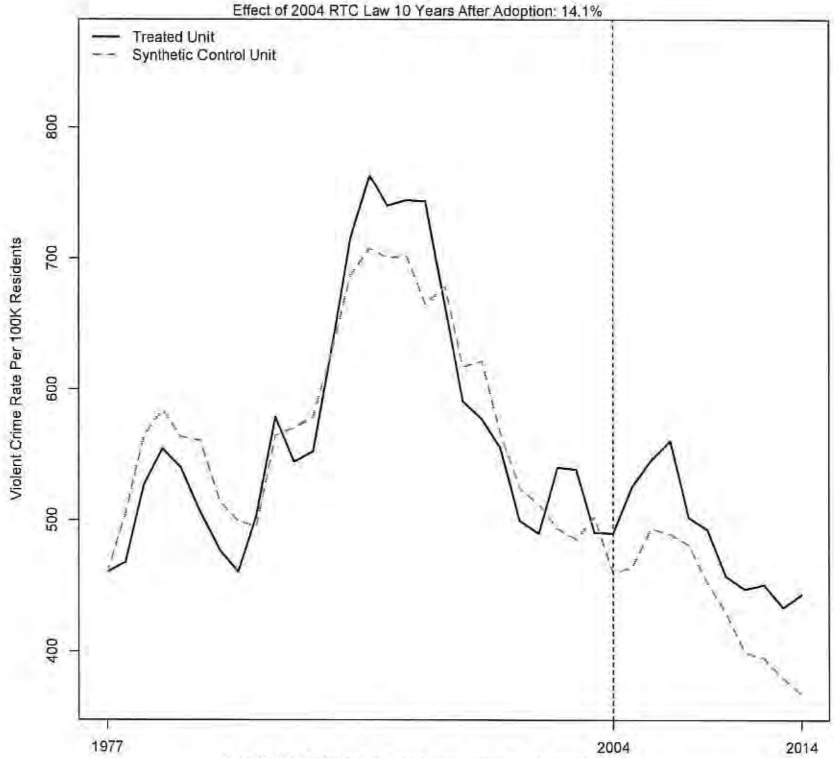
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: MD (0.362); NY (0.334); WI (0.303)
CVRMSPE: 0.075 (15 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: MD ; NY
RTC Adopting States Included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



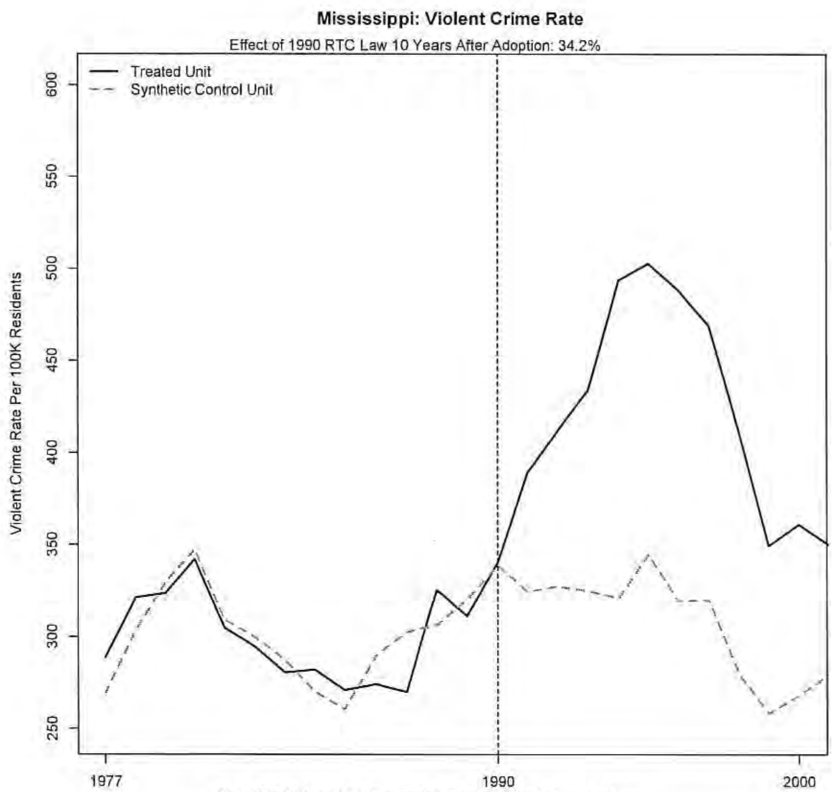
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.084); HI (0.916)
CVRMSPE: 0.166 (27 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE ; HI
RTC Adopting States Included in Synthetic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

Missouri: Violent Crime Rate

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



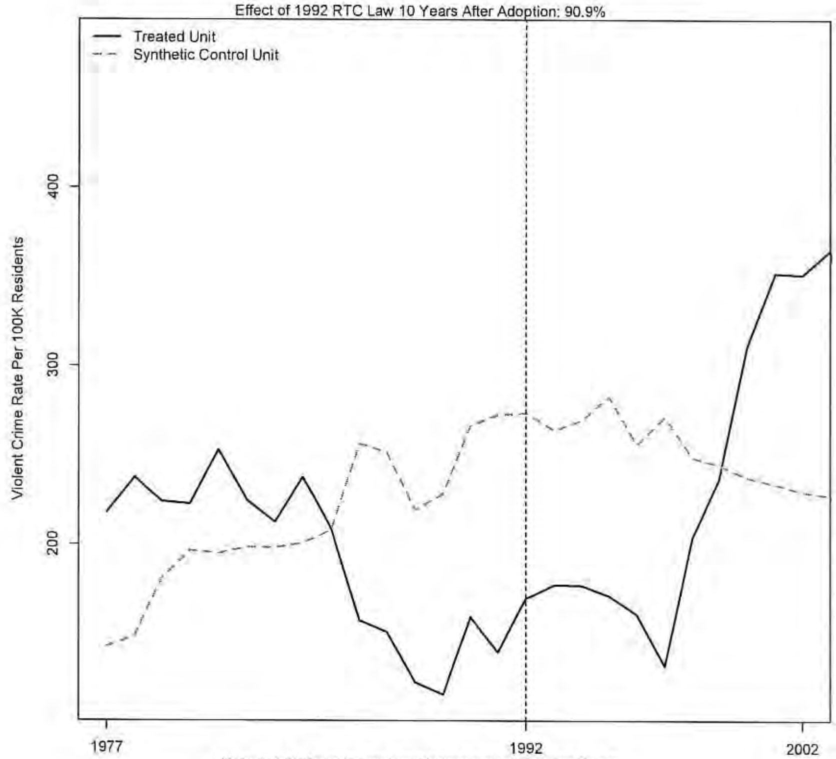
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.401); DE (0.265); HI (0.333)
CVRMSPE: 0.062 (11 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE ; HI
RTC Adopting States Included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



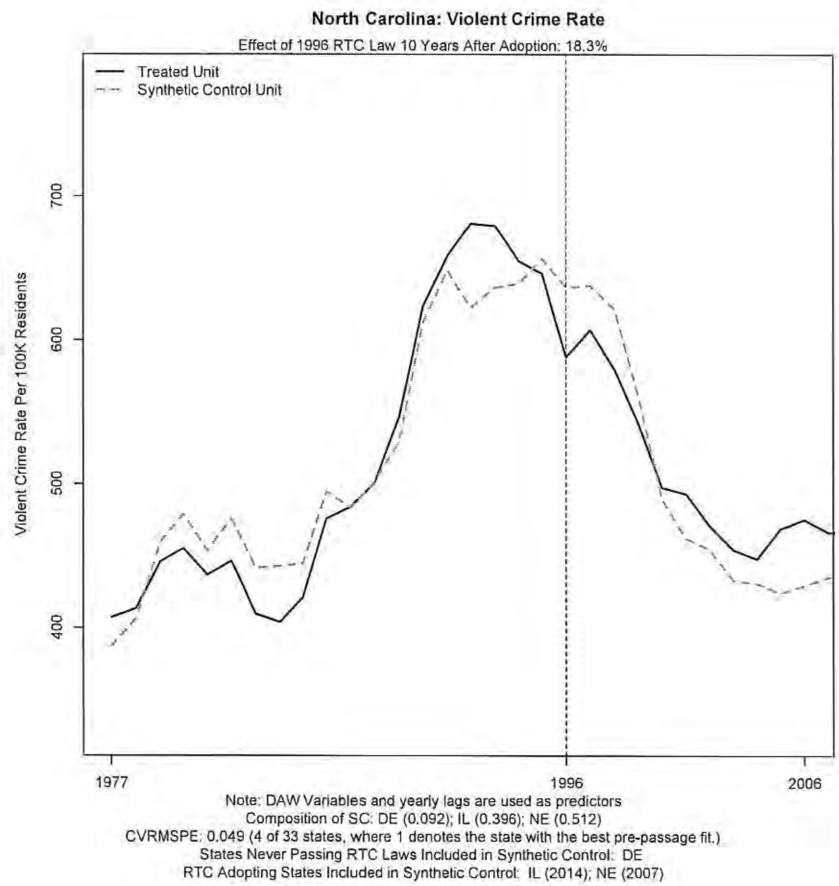
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.721); IA (0.016); NE (0.011); OH (0.252)
CVRMSPE: 0.05 (6 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included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

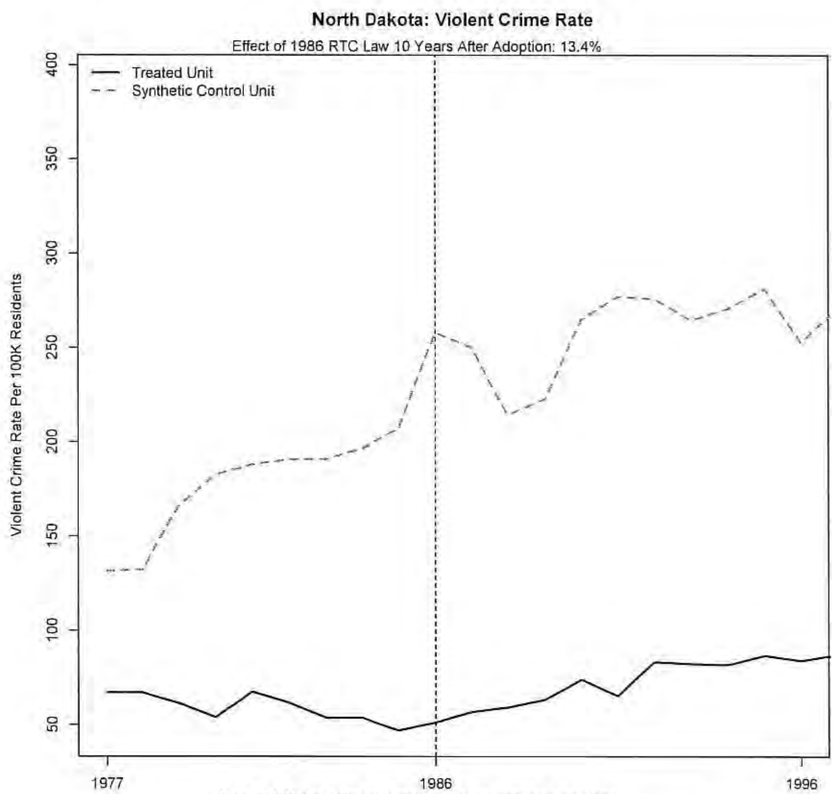
Montana: Violent Crime Rate

Effect of 1992 RTC Law 10 Years After Adoption: 90.9%

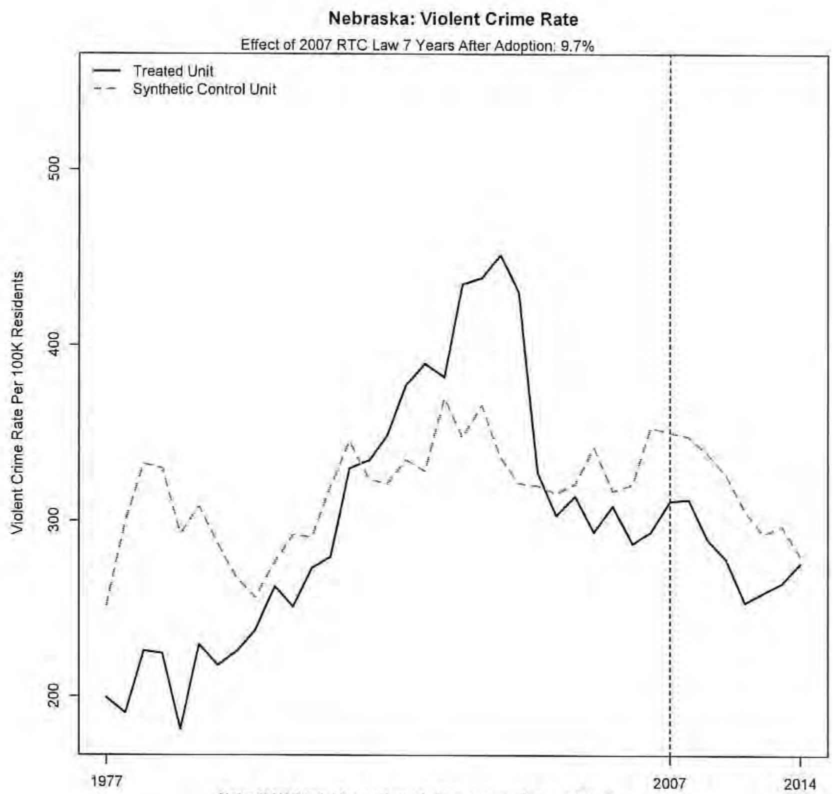


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.121); WI (0.879)
CVRMSPE: 0.409 (31 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI
RTC Adopting States Included in Synthetic Control: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

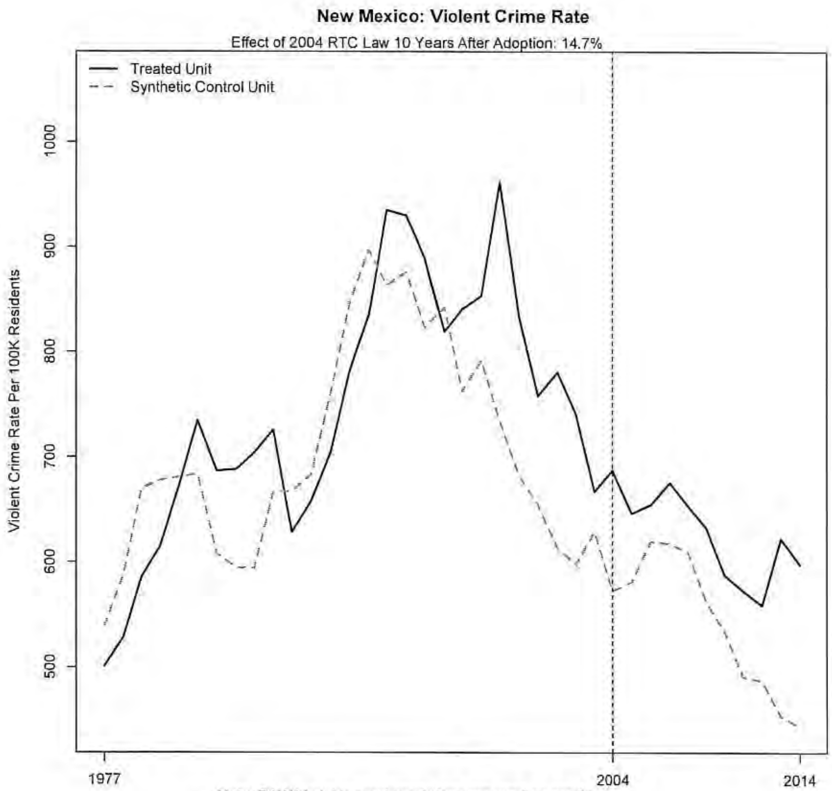

North Carolina: Violent Crime Rate
Effect of 1996 RTC Law 10 Years After Adoption: 18.3%
Treated Unit
Synthetic Control Unit
Violent Crime Rate Per 100K Residents
400
500
600
700
1977
1996
2006
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.092); IL (0.396); NE (0.512)
CVRMSPE: 0.049 (4 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic Control: IL (2014); NE (2007)



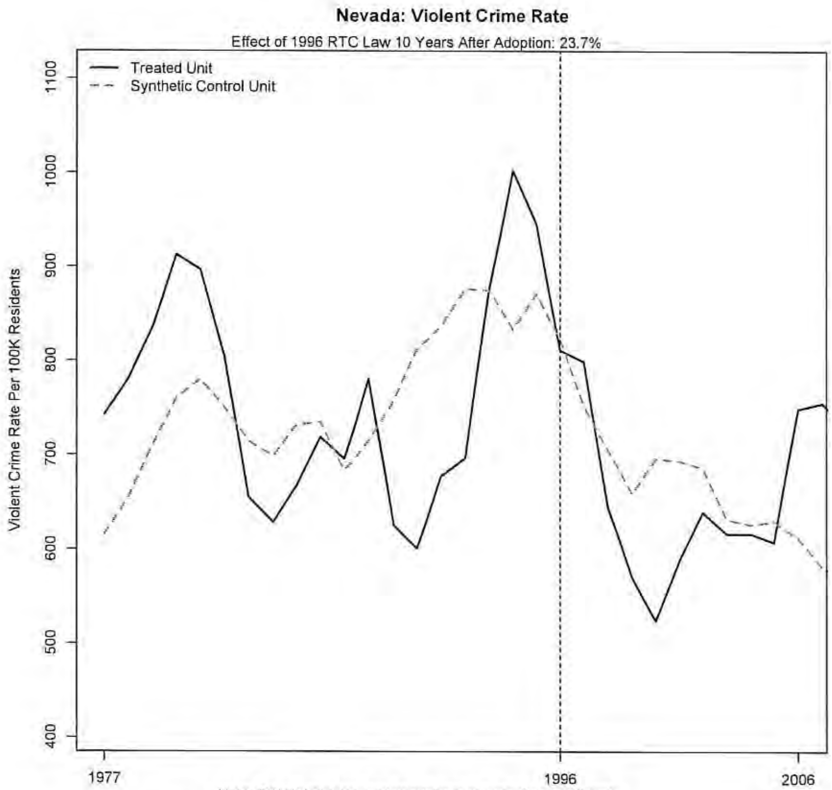
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: WI (1)
CVRMSPE: 2.043 (33 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: WI (2012)



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



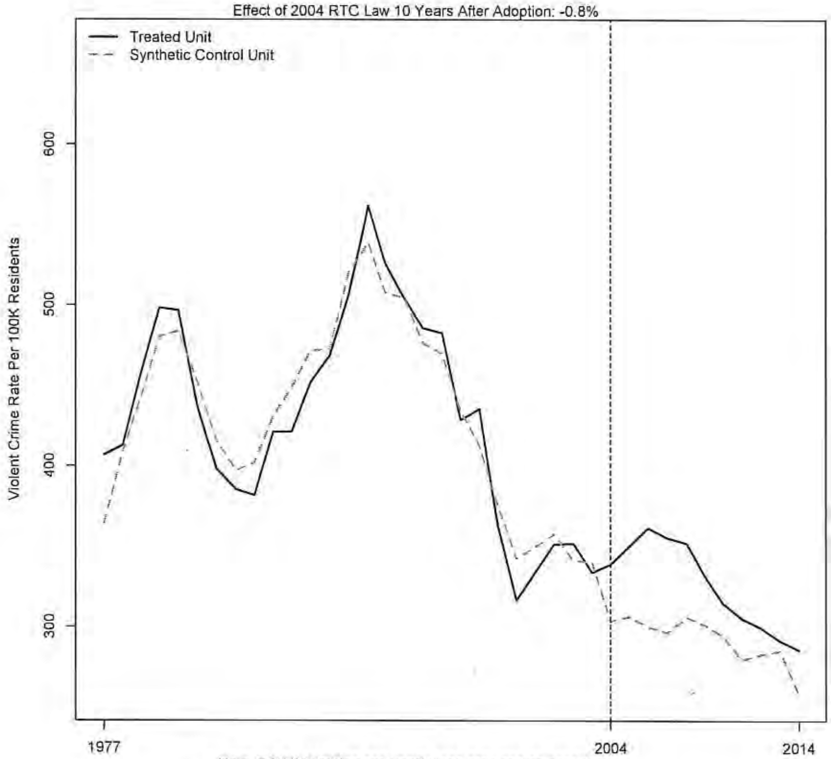
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.485); DE (0.515)
CVRMSPE: 0.122 (23 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; DE
RTC Adopting States Included in Synthetic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



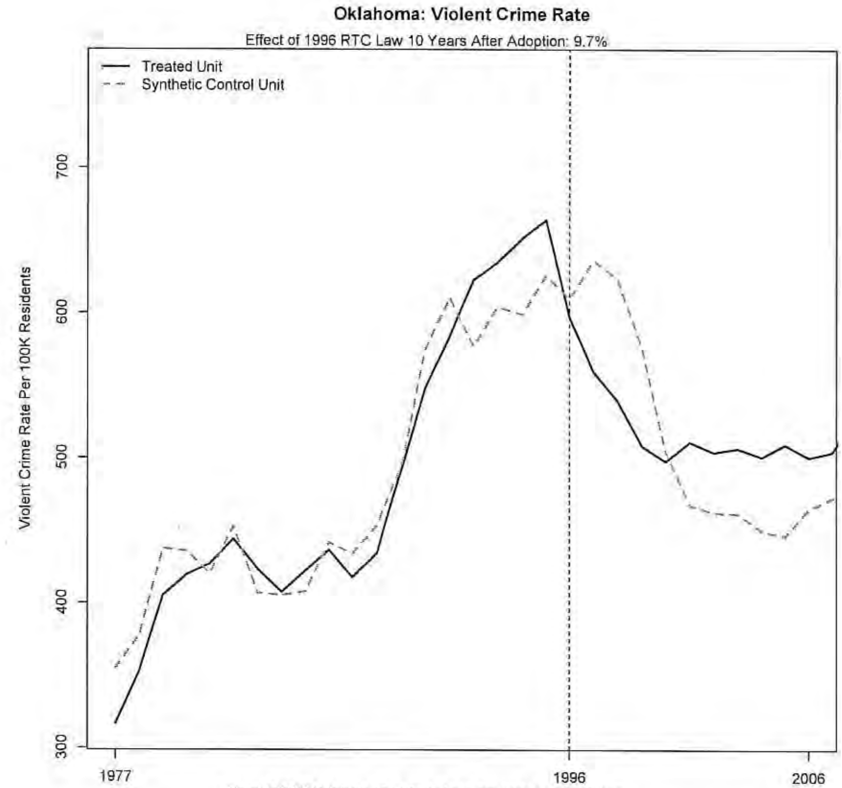
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.167); MD (0.833)
CVRMSPE: 0.152 (25 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; MD
RTC Adopting States Included in Synthetic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

Ohio: Violent Crime Rate

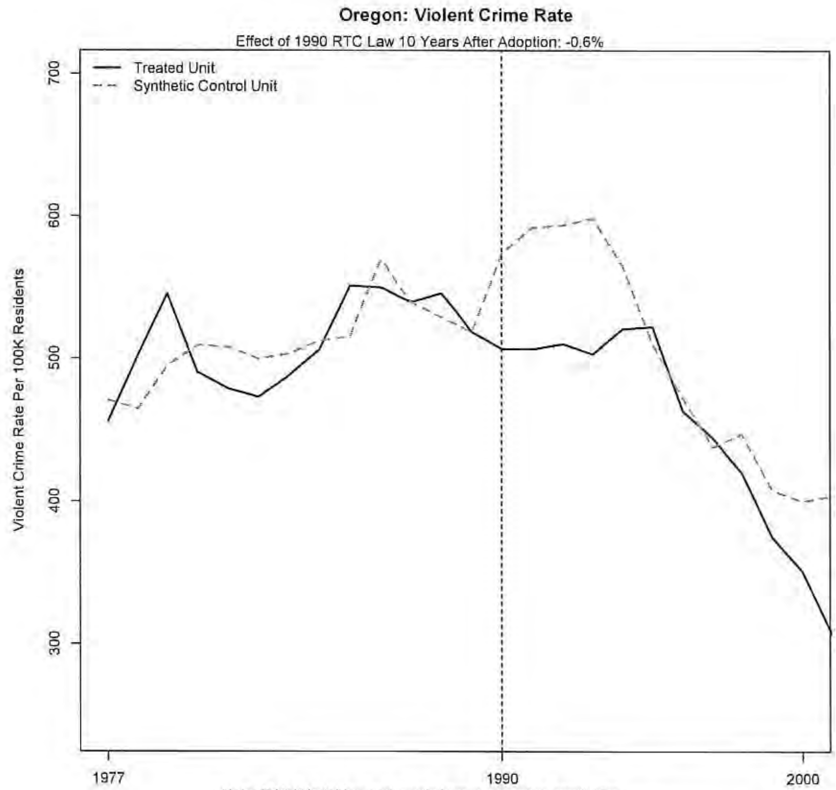
Effect of 2004 RTC Law 10 Years After Adoption: -0.8%



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



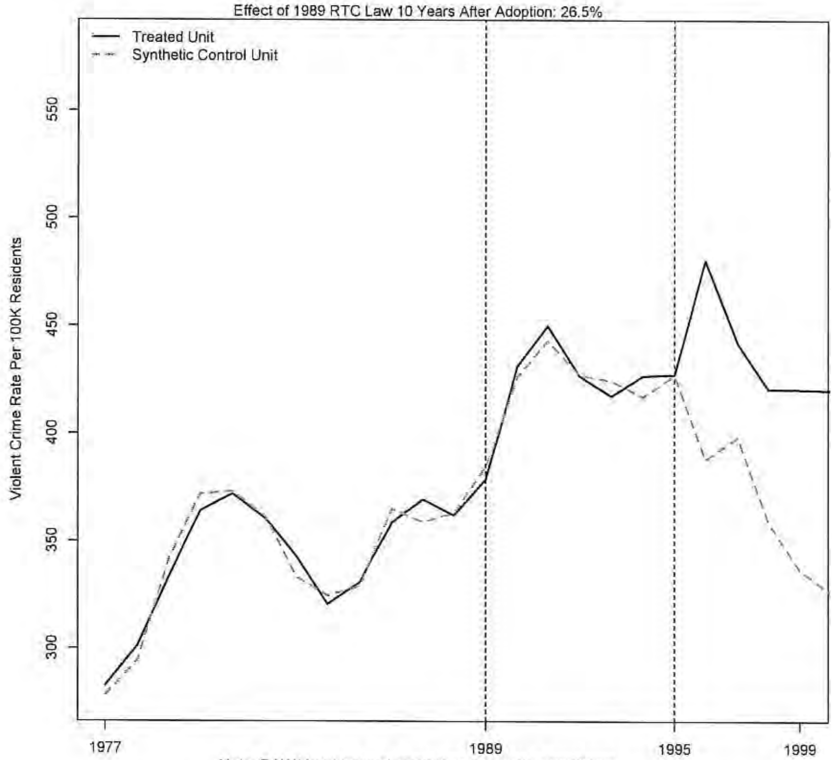
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.27); IL (0.246); NE (0.484)
CVRMSPE: 0.055 (7 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE
RTC Adopting States Included in Synthetic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



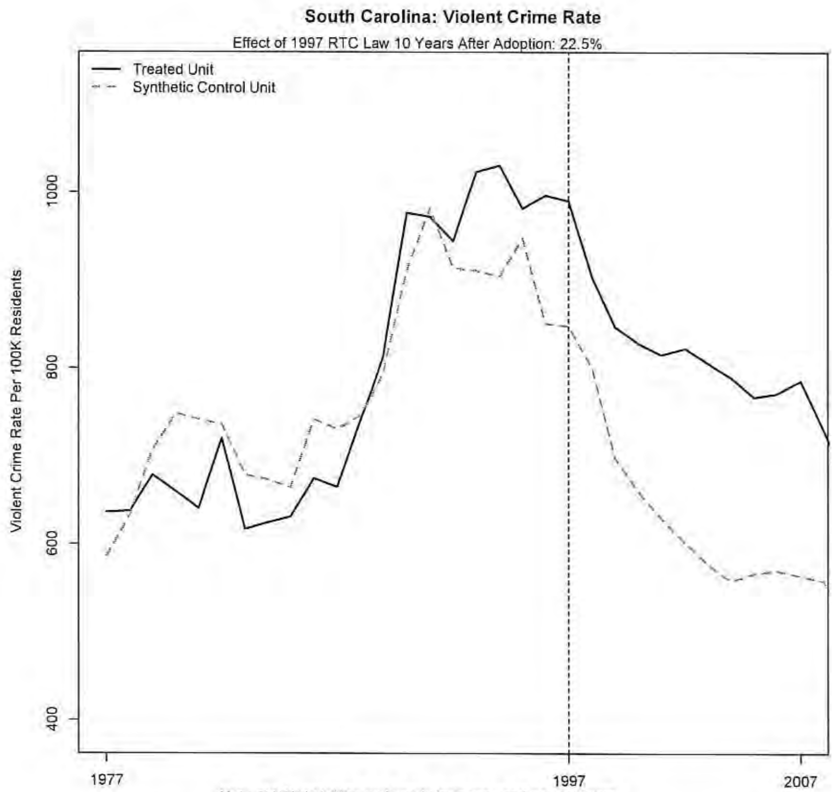
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.014); CO (0.42); HI (0.062); MI (0.344); MN (0.161)
CVRMSPE: 0.049 (5 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA ; HI
RTC Adopting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

Pennsylvania: Violent Crime Rate

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



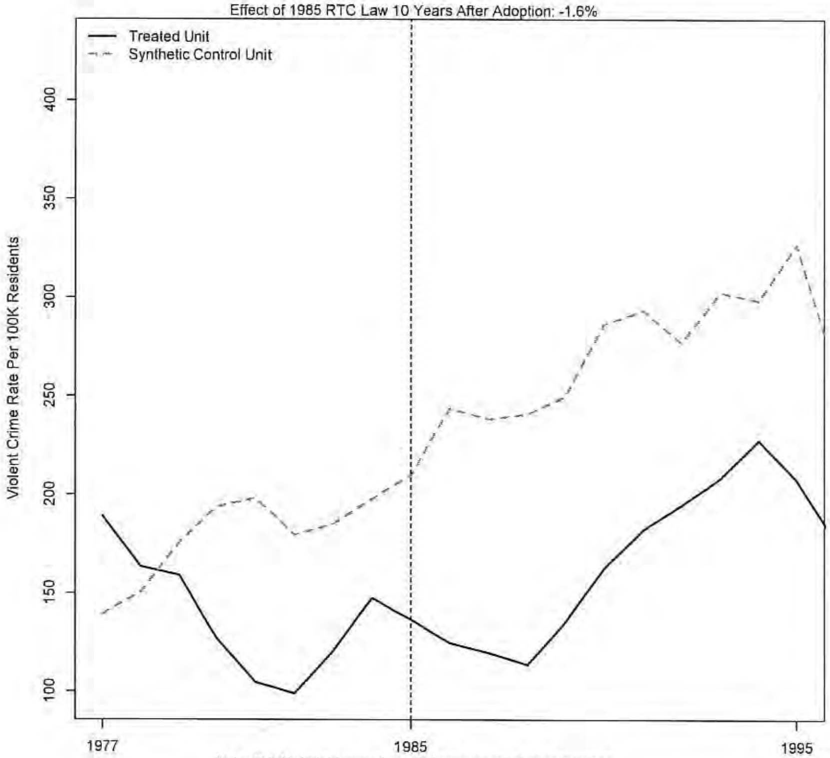
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: DE (0.077); HI (0.106); NE (0.043); NJ (0.17); OH (0.256); WI (0.348)
CVRMSPE: 0.018 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: DE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



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

South Dakota: Violent Crime Rate

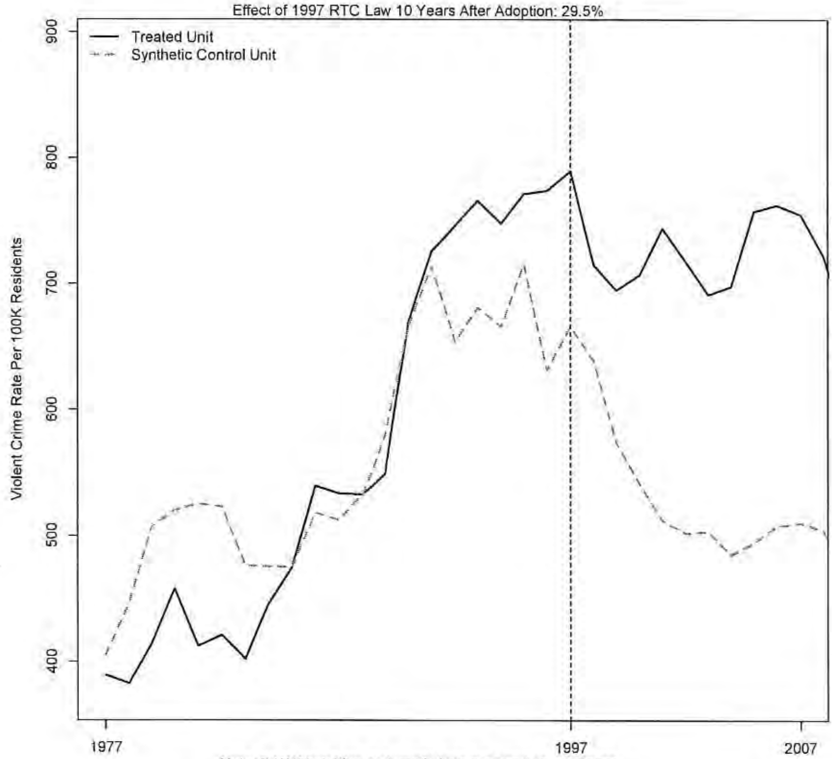
Effect of 1985 RTC Law 10 Years After Adoption: -1.6%



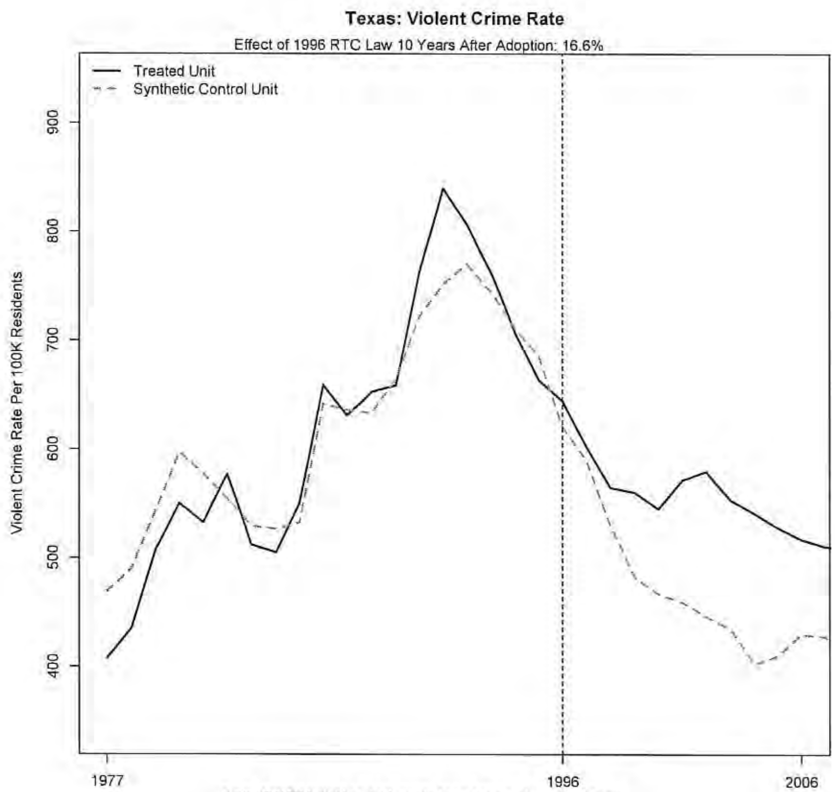
Note: DAW Variables and yearly lags are used as predictors
Composition of SC: IA (0.625); WI (0.375)
CVRMSPE: 0.436 (32 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control:
RTC Adopting States Included in Synthetic Control: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

Tennessee: Violent Crime Rate

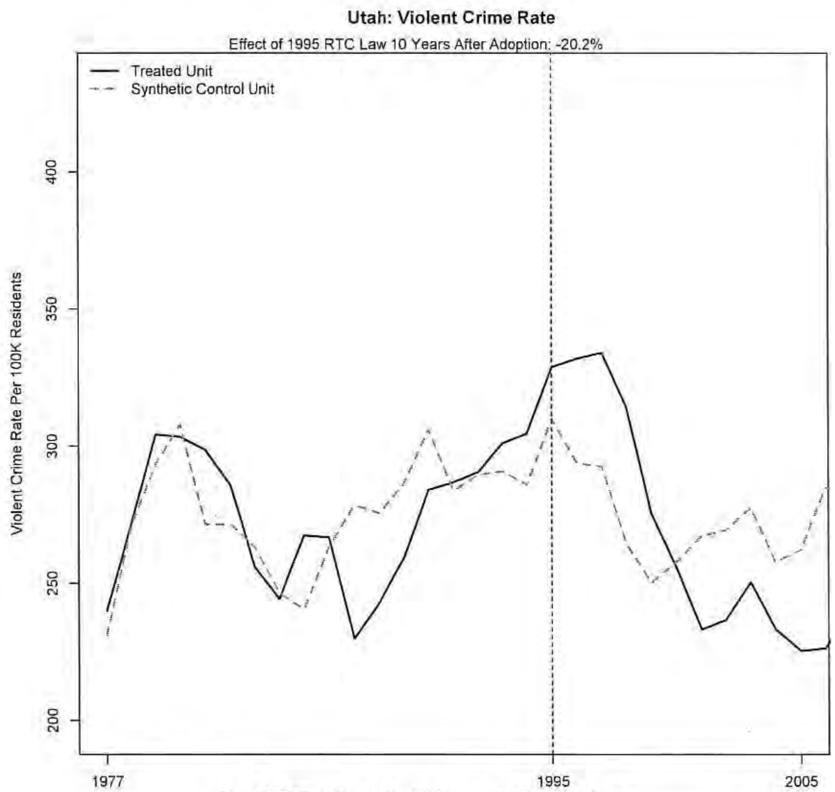
Effect of 1997 RTC Law 10 Years After Adoption: 29.5%



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

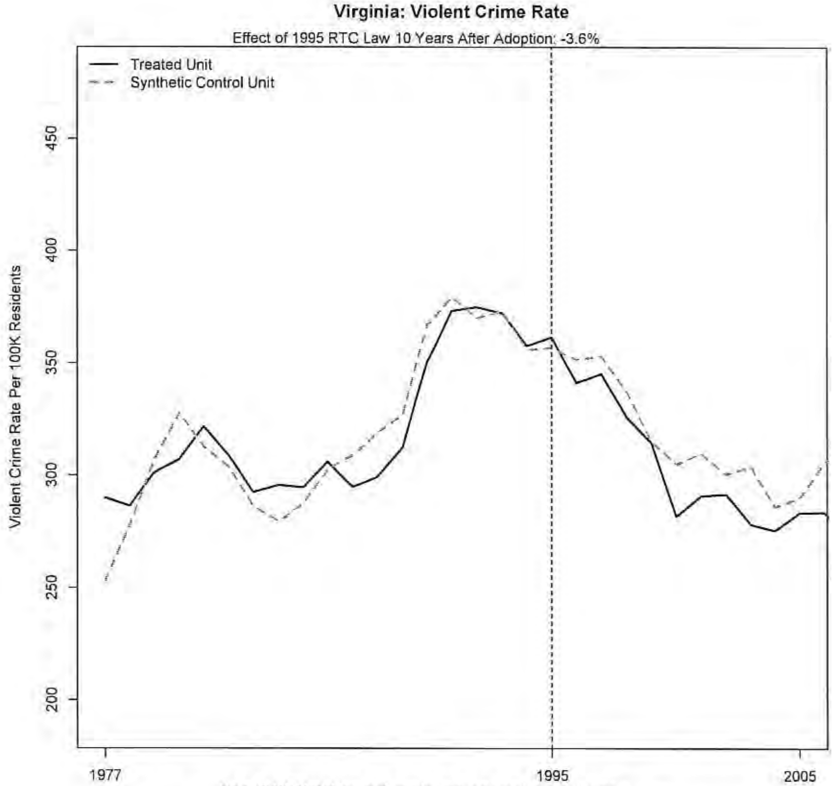


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: CA (0.578); NE (0.086); WI (0.336)
CVRMSPE: 0.06 (10 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: CA
RTC Adopting States Included in Synthetic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

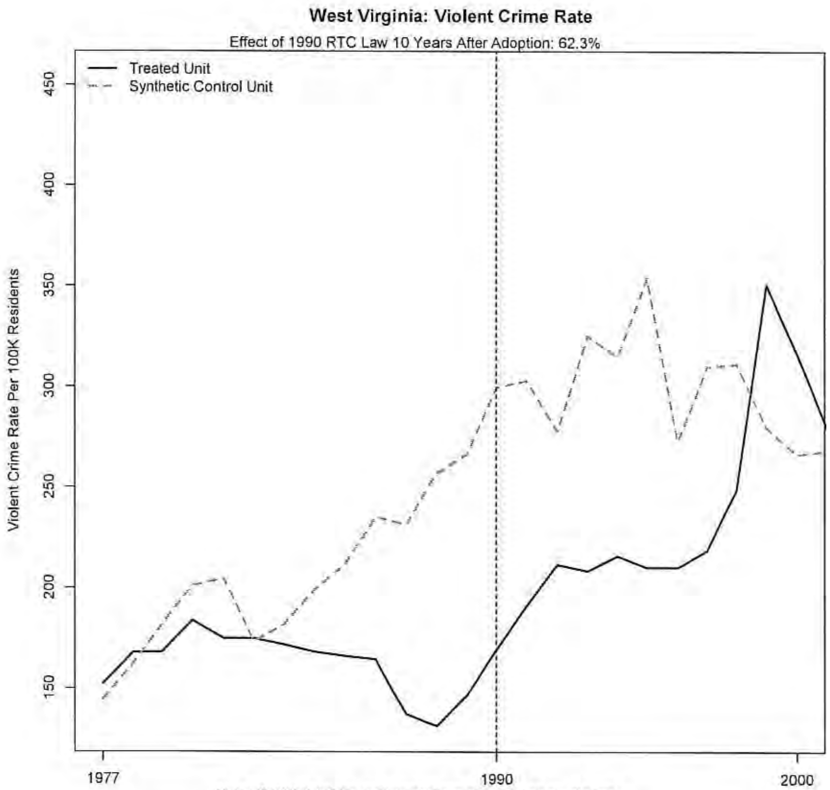


Note: DAW Variables and yearly lags are used as predictors
Composition of SC: HI (0.756); KS (0.06); RI (0.108); WI (0.075)
CVRMSPE: 0.072 (14 of 33 states, where 1 denotes the state with the best pre-passage fit.)
States Never Passing RTC Laws Included in Synthetic Control: HI ; RI
RTC Adopting States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

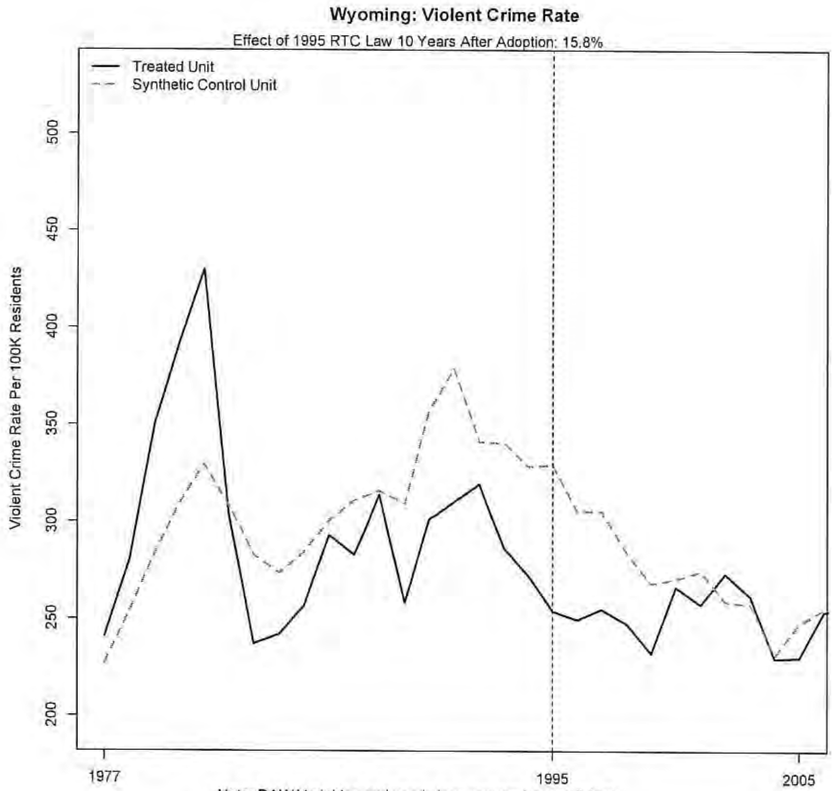
Virginia: Violent Crime Rate



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



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



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

Appendix G: Data Sources

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table A17: Crime Predictor Means Before RTC Adoption

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

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

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

I. Synthetic Control Estimates Using the BC Explanatory Variables

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

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

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

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

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

II. Synthetic Control Estimates Using the LM Explanatory Variables

In our Part II panel data analysis, we saw that RTC laws were associated with significantly higer rates of violent crime in the DAW model (Table 4), the BC model (Table 5, Panel A), and the MM model (Table 7, Panel A), but not in the LM model (Table 6, Panel A), although both the LM and MM models did show RTC laws increased murder. Table A19 estimates the impact of RTC laws on violent crime using the LM specification.[55] The detrimental effects of RTC laws on violent crime rates are statistically significant at the .05 level starting five years after the passage of a RTC law, and appear to increase over time. The treatment effects associated with violent crime in Table A19 range from 11.0 percent in the seventh post-treatment year to 12.8 percent in the tenth post-treatment year. Remarkably, the DAW, BC, and LM synthetic control estimates of the impact of RTC laws on violent crime are nearly identical (compare Tables 9, A18, and A19), and this is true even when we limit the sample of states in the manner described in Tables 10-11.[56]

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

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

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

III. Synthetic Control Estimates Using the MM Explanatory Variables

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

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

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

[57]For the same reasons described in footnote 55, we use the lagged violent or property crime arrest rate in our regression tables but use the contemporaneous violent or property crime arrest rate as a predictor in our synthetic controls code for the MM specification.

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

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

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

Standard errors in parentheses

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

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

Results reported for the constant term resulting from this regression

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

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

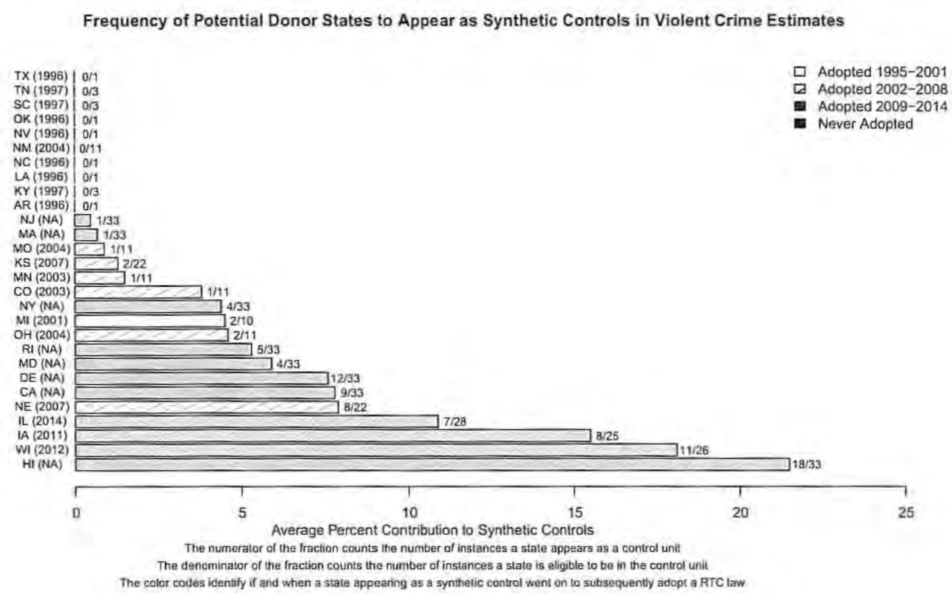
Turning our attention to property crimes, we find little systematic evidence that RTC laws influence property crime in the synthetic control approach, as our aggregate property crime results are never significant.

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

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

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

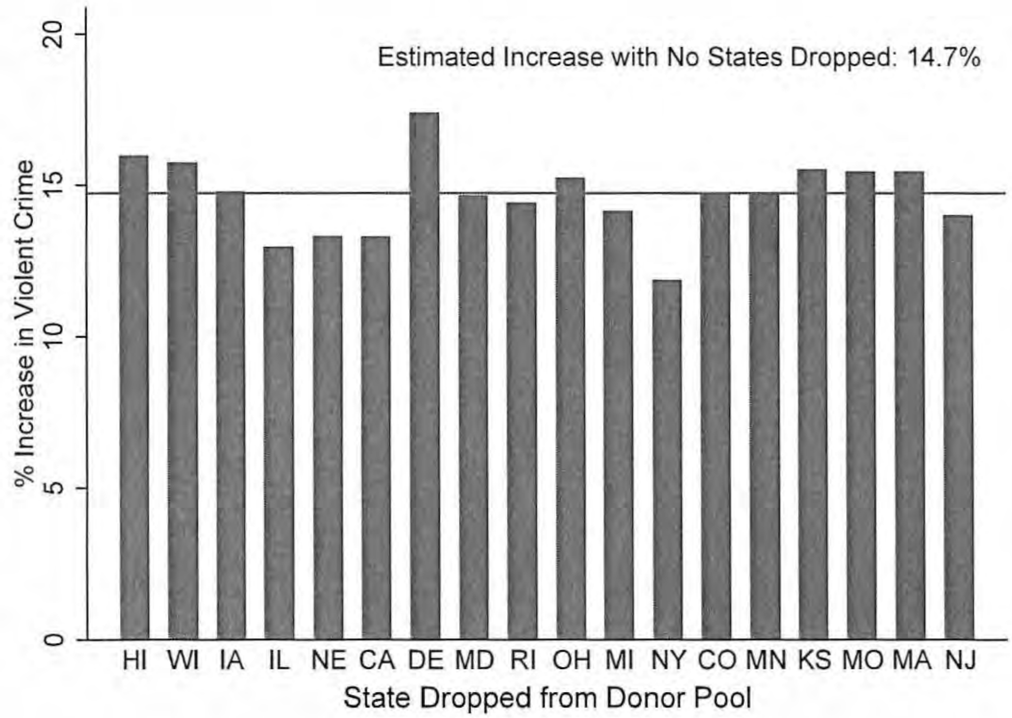
Figure A34



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

Figure A35

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



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

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

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

Figure A36

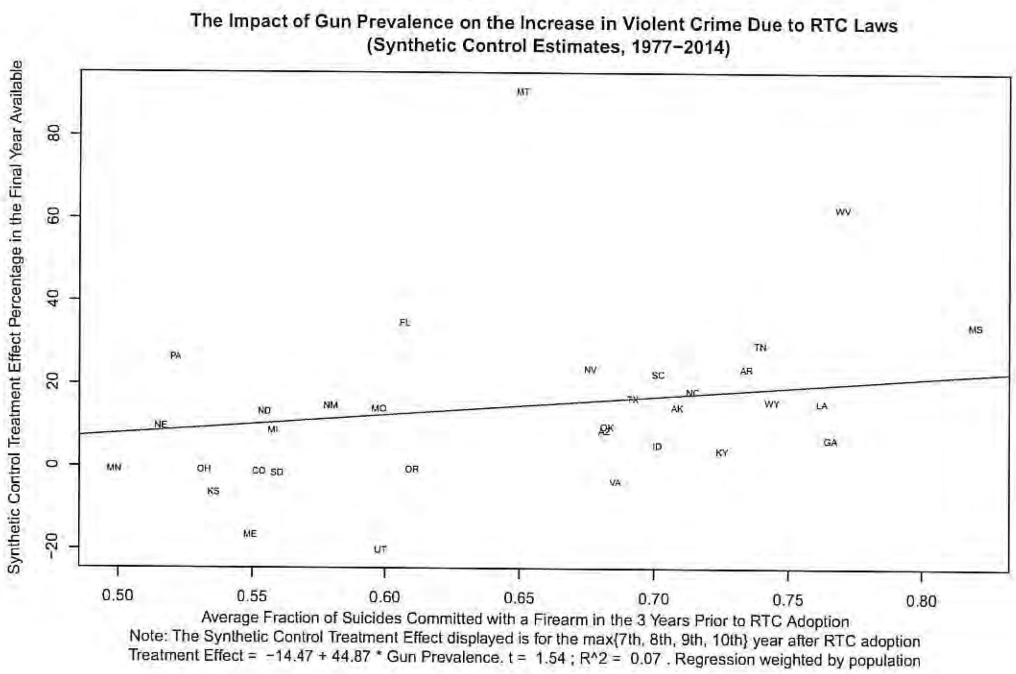


Figure A36 shows the scatter diagram for 33 RTC-adopting states, and relates the estimated impact on violent crime to a measure of gun prevalence. (Gun prevalence is proxied by the commonly used measure showing the fraction of suicides in a state that are committed with guns.) The last line of the note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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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