



RTC Laws Increase Violent Crime: Moody and Marvell Have Missed the Target

John J. Donohue¹, Abhay Aneja², and Kyle D. Weber³

[LINK TO ABSTRACT](#)

Donohue, Aneja, and Weber (2018), released as National Bureau of Economic Research working paper 23510, uses two distinct methodologies to provide the latest and most comprehensive evaluation of the impact on crime of state laws that confer on citizens a right to carry concealed weapons—so-called right-to-carry or RTC laws. Its most robust finding is that RTC laws *increased* violent crime: our preferred panel data estimate indicates a 9 percent increase, while our synthetic control analysis indicates that violent crime rose by about 14 percent in the first decade after RTC adoption.

In a comment on the Donohue, Aneja, and Weber (hereafter DAW) paper, Carlisle Moody and Thomas Marvell (hereafter MM) concede that the uniform approach of using population weights in panel data estimates of crime shows a strongly statistically significant increase of RTC laws on crime in the DAW model (MM 2019, 88). They make an unconvincing argument that the uniform practice should now be rejected and then proceed to show that simplistic panel data models not weighted by population (and using badly miscoded data) would diminish the strength of the finding that RTC laws increase violent crime (*ibid.*, 85–88). We show that both of the proffered MM models violate the basic ‘parallel trends’ requirement of a valid panel data analysis, so their resulting estimates must be rejected. But even with these serious flaws, a more nuanced implementation and

1. Stanford Law School, Stanford, CA 94305.

2. Graduate student, Stanford Law School, Stanford, CA 94305.

3. Graduate student, Columbia University, New York, NY 10027.

evaluation of the MM models with attention to the requirements of panel data can illustrate and buttress the basic finding of the DAW panel data analysis that RTC laws *increase* violent crime.

MM (2019, 89–94) then present their own synthetic control analysis, which purports to establish that 14 states show statistically significant increases in violent crime while 12 states show statistically significant decreases. We have many criticisms of their implementation of the synthetic control analysis, from using inappropriate states as potential controls to failing to account for major pre-treatment differences. These problems cause MM to generate many severely inaccurate predictions, particularly for small states. Nonetheless, a simple aggregation of MM's overall synthetic controls results—whether weighted by state population or the inverse of the pre-treatment error fit—reveals a strong pattern of increasing violent crime in the decade following RTC adoption.

We discuss these points in turn and then summarize in the final section.

DAW's population-weighted model is superior to MM's models, and it provides clear evidence that RTC laws increase crime

Weighting by population is conceptually superior

The uniform practice in the literature on estimating the impact of RTC laws on crime from the early work of John Lott through the DAW paper has been to present population-weighted panel data estimates. Every regression run by the authors of the National Research Council (2005) report examining RTC laws was weighted by population. In fact, this is the standard practice in virtually all panel data studies looking at state or county crime data,⁴ including in prior work by MM on RTC laws.⁵ In their current paper, however, they argue that the standard practice should now be rejected, and they would repose confidence in regressions that are not designed to reflect the relative population of each state.

MM acknowledge the reason that all researchers have used population-weighted regressions:

4. For just two very recent examples, see Chalfin and McCrary 2018; Anderson, Sabia, and Tekin 2018.

5. See Moody and Marvell 2018; 2008; Moody, Marvell, Zimmerman, and Alemante 2014; Kovandzic, Marvell, and Vieraitis 2005; Moody 2001.

[I]f the research goal is to estimate the overall national impact of a policy change, ... then weighting can be justified by arguing that the impact of laws in large states should be emphasized simply because they affect more people. (MM 2019, 85)

Put simply, we are trying to estimate the impact that RTC laws have had on Americans, and this can only be identified by a population-weighted regression. Following the unweighted approach that MM have suddenly decided to champion would imply that the impact of RTC laws on 600,000 individuals in Wyoming is considered to be equally important as the impact on 28 million Texans. To illustrate the importance of weighting by population, consider the MM synthetic control estimates of the impact on violent crime of the RTC laws in these two states. Using their non-normalized synthetic control approach, MM would predict that the Texas RTC law *increased* violent crime by 19.5 percent after ten years but that the Wyoming law had generated a 36 percent *decrease* in violent crime over the decade following adoption (although they never show these estimates in their paper). While we discuss below why we think MM's Wyoming estimate is so flawed, the decision to equally weight Texas and Wyoming, as MM would have us do, generates a prediction that the combined RTC laws *reduced* crime by 8.25 percent. A population-weighted average would show the total effect on the residents of these states to be an 18.3 percent *increase* in violent crime.⁶ In this example, the 18.3 percent increase would reflect the effect of RTC laws on the average American who experienced this legal adoption, and a population-weighted analysis alone would generate this estimate. MM's approach would badly mischaracterize the impact of RTC laws, heralding a significant decline in violent crime when in fact the two RTC laws led to a combined large increase in violent crime.

Having conceded the key reason for population weighting in the panel data regressions, MM (2019, 85–86) then mention a second possible advantage of population weighting: it may serve to address the problem of heteroskedasticity. This is not the primary rationale, but it is often—although not always—a secondary benefit of weighting by population. Since MM conclude that the White test indicates the presence of heteroskedasticity in the DAW population-weighted regressions, MM present estimates using a non-weighted regression approach (their OLS results) and a non-population-weighted approach that seeks to directly

6. MM's wildly inaccurate Wyoming estimate stems from their failure to normalize their synthetic control estimate, which leads them to attribute pre-treatment differences between the fit of the synthetic control and the treatment state to the effect of the treatment. Our DAW synthetic control estimates for the impact of RTC laws on violent crime showed a 16.9 percent increase for Texas and a 15.9 percent increase for Wyoming after ten years. The comparable *normalized* MM synthetic control estimates for these two states are a 13.4 percent increase for Texas and a 9.1 percent increase for Wyoming.

control for heteroskedasticity (Feasible Generalized Least Squares, or FGLS). Neither of these approaches can succeed in our primary mission, which is to estimate the experience of the average American exposed to RTC laws. But in addition to the conceptual flaw in failing to weight by population, both of the MM suggested alternatives have further problems, including the second problem that they both fail the very test for homoskedasticity that MM advocate using.⁷

The importance of investigating the parallel trends assumption

While that second problem underscores that the MM regressions are still marred by heteroskedasticity (or some specification error), a third problem with the simplistic MM models results from MM's failure to attend to the parallel trends assumption, which is critical to generating valid panel data estimates.

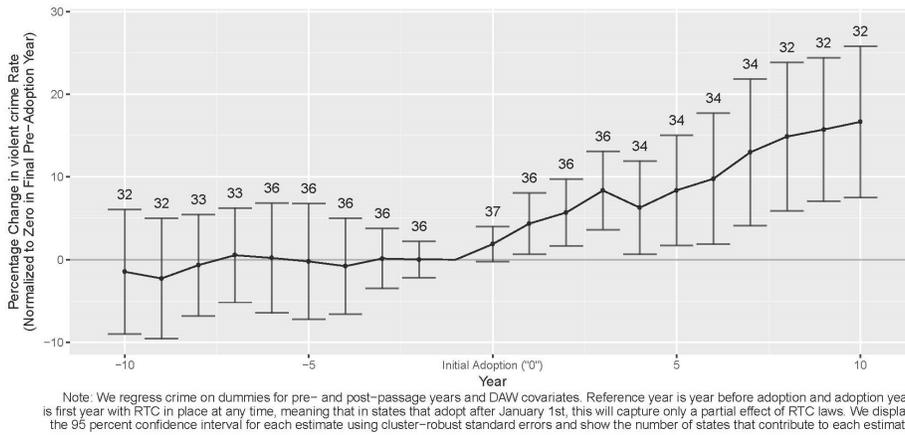
This third problem with MM's two new panel data regressions can be highlighted by comparing them to the results of the DAW population-weighted violent crime regression. The DAW paper provides the year-by-year effect on violent crime following RTC adoption from that regression (2018, 25), which we reproduce here as Figure 1 below. This figure illustrates the critical feature of a valid panel data model that the estimated values on the states that end up adopting RTC laws is virtually zero in the years prior to adoption. Not only are the deviations from zero small, but crucially there is virtually no slope to these pre-adoption values in the years prior to RTC passage. This is important because a panel data estimate will only reveal the causal effect of the RTC law if we can assume that the trends in crime between our two sets of states (adopters and non-adopters) would evolve similarly in the absence of the law.

Three lessons emerge from the Figure 1 DAW violent crime regression. First, we see an almost perfect pre-treatment pattern confirming the critical parallel trends assumption for a panel data regression. Controlling for an array of factors (the DAW explanatory variables), violent crime is flat prior to RTC adoption. Second, Figure 1 also reveals that there is a change in the previously stable relationship of crime in the RTC and non-RTC states, and that this change begins exactly in the year of adoption of the RTC laws. If RTC laws had no impact on violent crime, one would expect that flat pattern seen in the years before adoption would continue thereafter. If some factor other than RTC laws (and the array of explanatory variables controlled for in the DAW model) led to worse violent crime performance in RTC states, you would see an elevation in the violent crime estimates, but there

7. This is true for both the MM unweighted OLS regression and for their FGLS regression, both of which badly fail the White test for homoskedasticity with p-values < 0.00000001.

is no reason to think it would occur in exactly the year that the RTC law goes into effect. Figure 1 makes clear that a sharp secular increase in violent crime commences at the time of RTC adoption, again buttressing a causal interpretation of these results. Third, this increase in violent crime is statistically significant beginning in the first year after RTC adoption and every year thereafter.

Figure 1. The impact of RTC laws on violent crime, DAW model, 1979–2014 (population-weighted)



Evaluating MM’s simple panel models

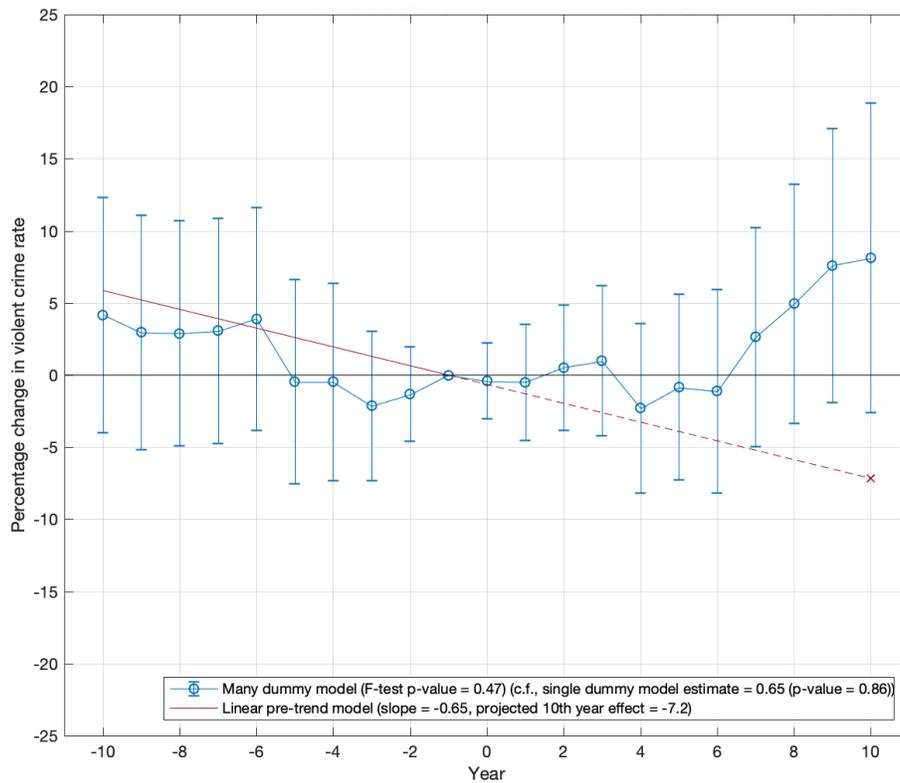
We can now compare the two alternative models—OLS and FGLS—that MM offer in place of the DAW violent crime estimates reflected in Figure 1. We must first discard all of the MM estimates because of serious coding errors they made in their panel data analysis. Specifically, the MM panel data analysis miscodes both North Dakota and South Dakota as having never adopted an RTC law during the 1977–2014 data period they analyze, even though North Dakota and South Dakota both adopted RTC laws in 1985. The error is perplexing because, in their subsequent synthetic control analysis, MM generate estimates for states adopting RTC laws, including both North and South Dakota, based on that actual year of adoption.⁸ MM also code the date of adoption for Virginia differently in their two analyses. They give Virginia a starting date of 1996 in their synthetic control

8. MM also have a less precise coding of their RTC law than we use in our DAW paper: they simply use a zero-one dummy that becomes one the first full year the RTC is in effect, while we use an RTC dummy that takes the value of the fraction of the year an RTC law was in effect during the year it was adopted. MM also exclude DC from their panel analysis, while we only exclude DC from our synthetic control analysis.

analysis, which is consistent with their protocol of turning on their RTC indicator in the year after adoption. In their panel data analysis, however, MM use a Virginia date of 1995, which is doubly wrong in being both a violation of their own protocol and inconsistent with their treatment of Virginia in their synthetic control analysis.

Figure 2 shows violent crime estimates using the preferable DAW data but following MM’s “OLS” approach, which does not weight by population. Three lessons emerge from this analysis. First, Figure 2 reveals a substantial violation of the critical parallel trends assumption: the red line illustrates the sharply sloping downward linear trend in crime for RTC states *prior to RTC adoption*.

Figure 2. The impact of RTC laws on violent crime, DAW model, 1979–2014 (not weighted by population)



Second, the dashed continuation of this line shows the predicted path of violent crime in RTC states had their pre-RTC-adoption trend continued, and by assumption of panel data analysis, the dashed line of Figure 2 suggests that crime would have fallen (relative to non-adopting states) by 7.2 percent after ten years without RTC adoption. Instead we see that the observed post-adoption crime path

is always above this predicted downward trend, suggesting RTC laws *increased* crime relative to trend.

Third, by the sixth year after adoption and beyond, the estimated increase in violent crime is always statistically significantly above this trend (at the .05 level). But instead of providing this more nuanced analysis, MM simply look at one number for the OLS violent crime estimate: they run a single dummy model for this unweighted regression, which generates the small positive estimate of 0.65 (as shown in the legend to Figure 2). But by failing to realize that such a simple model is marred by the violation of the parallel trends assumption, they merely present an inaccurate and misleading estimate of the impact of RTC laws on violent crime. In other words, MM's violent crime unweighted OLS estimate (MM 2019, 88, Table 1, row 1, column 5) is inaccurate and misleading.⁹

MM also include an FGLS model designed to address the problem of heteroskedasticity (although we have already noted this model's extreme failure of the White test). Figure 3 shows the DAW violent crime year-by-year estimates using this FGLS approach. What are the lessons from this MM-suggested model? First, unlike in the DAW model in Figure 1 where all the pre-treatment values are close to zero and flat in the years prior to RTC adoption, Figure 3 reveals both greater variability in those values and another departure from the ideal parallel trends as captured again in the downward-sloping red line in the period *prior to RTC adoption*. Indeed, this FGLS model fails the most basic test of parallel trends since its pre-trend dummy values are not jointly zero.¹⁰

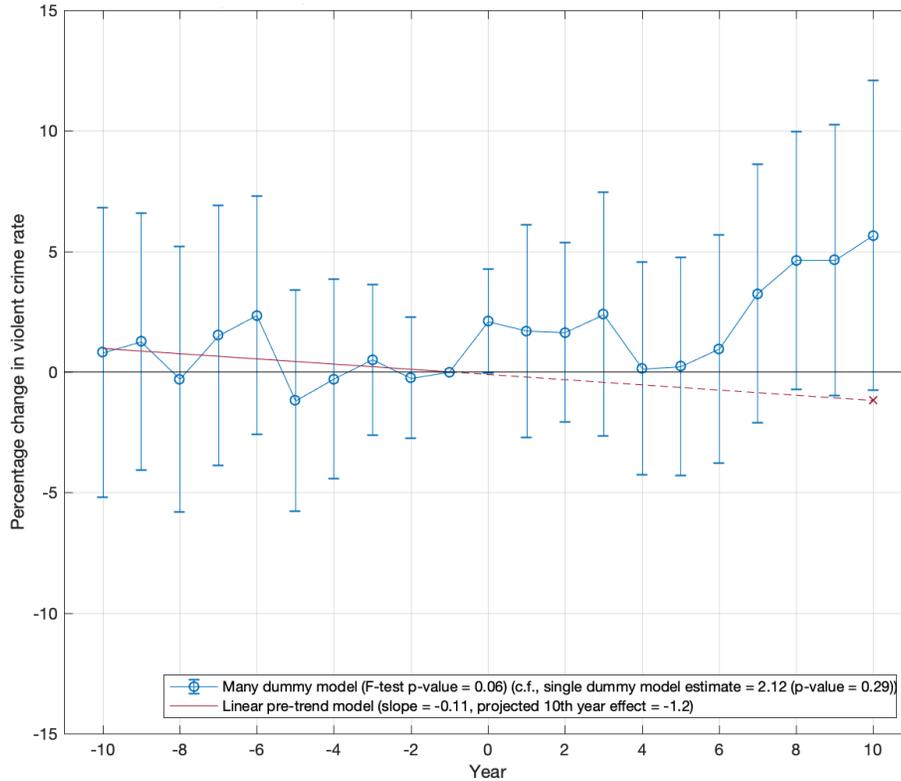
Second, the dashed continuation of this line shows the predicted path of violent crime in RTC states had their pre-RTC-adoption trend continued, and it suggests that crime would have fallen (relative to non-adopting states) by 1.2 percent after ten years without RTC adoption. As in Figure 2, we see that the observed post-adoption crime path is always above this predicted downward trend, again suggesting RTC laws *increased* violent crime relative to trend, and that this

9. MM also present an additional row of “spline” estimates in their Table 1, which is a practice that also dates back to the initial Lott and Mustard (1997) paper. Since the RAND Corporation (2018) study on gun violence research has now argued that “spline” results should not be relied upon, we ignore that component of the MM paper (and have also dropped this model in our own forthcoming work). The RAND analysis of gun research identifies “the use of spline and hybrid effect codings that do not reveal coherent causal effect estimates” as a limitation of earlier studies (2018, xxvii).

10. The most basic statistical test of the assumption of parallel trends uses an F-test of the null hypothesis that the pre-period dummies are jointly equal to zero. Applying this test in Figure 3 generates a p-value of .057, which is too low to support the parallel trends assumption. For this very permissive initial test, one would typically like this p-value to be greater than .50 and certainly no lower than .20, so the Figure 3 FGLS model fails this test badly in a way that obscures any increase in violent crime resulting from RTC adoption. For comparison, the p-value on the same F-test for our far superior Figure 1 DAW violent crime population-weighted regression is .87.

reversal in the path of violent crime occurred in the year of RTC adoption.

Figure 3. The impact of RTC laws on violent crime, DAW model, 1979–2014 (FGLS)



Third, Figure 3 shows that after the seventh year following RTC adoption, the estimated increase in violent crime is always statistically significantly (at the .05 level) above the dashed projected downward trend. Again, if one were to run the single dummy model for this FGLS regression and ignore the violation of parallel trends as MM do, one would not be presenting valid results. Accordingly, the small positive estimate of 2.12 (as shown in the legend to Figure 3) that emanates from this flawed model again yields an inaccurate and misleading picture of the true path of increased violent crime after RTC adoption. In other words, the MM violent crime regressions (2019, 88, Table 1, row 1, columns 5–6)—other than the population-weighted regression which shows a statistically significant increase in violent crime—are inaccurate and misleading. But note that both the Figure 2 and Figure 3 models that are merely more informative versions of the overly simplistic OLS and FGLS that MM present (using their badly miscoded data) still lead us to a

very clear conclusion: regardless of the flaws or limitations of the two models that MM present, their more accurate and revealing versions in Figures 2 and 3 can still detect that RTC states experience statistically significant increases in violent crime relative to pre-existing trends within a decade of adoption.

In other words, MM would reject the DAW panel data estimates that RTC laws increase violent crime by roughly 9 percent by instead offering regressions with key miscodings of RTC states that are conceptually inferior because they don't address the primary question of interest (which is the impact of RTC laws on Americans), empirically unsophisticated by virtue of their failure to address the parallel trends assumption, and offer no benefit in addressing the problem of heteroskedasticity.

MM's discussion of heteroskedasticity is largely a distraction from a more important issue: that the difference in results between the population-weighted and unweighted regressions is likely signaling a specification issue. This finding provides an additional reason to turn to the synthetic control analysis, which can give insight into this concern and also provide potentially superior estimates, at least for those states for which good pre-treatment matches can be found. But before turning to the synthetic control estimates, it is important to highlight once again that the DAW violent crime panel data model dominates the MM models both conceptually and econometrically for the reasons set out above.

Evaluating MM's synthetic control analysis, which despite its flaws is shown to reveal that RTC laws increase violent crime

The DAW synthetic control analysis aggregated across all RTC-adopting states generates a year-by-year prediction of the impact of RTC laws on violent crime over the ten years following adoption (2018, 36), shown here in Table 1.

TABLE 1. The impact of RTC laws on violent crime rate, DAW covariates, 1977–2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average normalized TEP	-0.117	2.629*	3.631*	4.682**	6.876***	7.358**	10.068***	12.474***	14.021***	14.344***
	(1.076)	(1.310)	(1.848)	(2.068)	(2.499)	(3.135)	(2.823)	(3.831)	(3.605)	(2.921)
N	33	33	33	33	33	33	33	31	31	31
Pseudo p-value	0.936	0.274	0.220	0.192	0.094	0.106	0.060	0.038	0.032	0.032
Notes: 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. See DAW (2018, 37–38) regarding how the pseudo p-value is estimated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.										

The synthetic control analysis of Table 1 shows that after RTC laws have been in effect for a year, violent crime starts steadily rising (relative to the synthetic control state). After ten years, the DAW synthetic controls analysis estimates that violent crime is about 14.3 percent higher than it would be in the absence of the RTC law. Note that even though Figure 1 (panel data) and Table 1 (synthetic control analysis) are derived from entirely different methodologies, they both estimate that RTC laws increasingly elevate violent crime in the ten years after adoption, which mutually reinforces this conclusion.

Moreover, DAW (2018) showed that the synthetic control result was extremely robust. Indeed, one would generate very similar estimates whether one used the control variables of DAW (those used to derive the estimates shown in Table 1) or those of other papers examining the impact of RTC laws, such as those by Lott and David Mustard (1997) and the Brennan Center (Roeder et al. 2015), or an earlier Moody and Marvell paper (2008). Similarly, one could drop any single control state from the analysis or even completely drop New York and California from the set of potential controls and the results remained strong: RTC laws consistently led to statistically significant *increases* in violent crime after a decade.

DAW (2018) also showed that the result that RTC laws increase violent crime was not sensitive to whether one normalized the synthetic control estimates to be zero at the time of adoption or simply allowed the estimates to emerge from the matching protocol without adjustment. Similarly, the result was robust to efforts to trim off treatment states for which the synthetic control did not well match the target state in the period prior to RTC adoption. DAW also showed the violent crime results remained strong whether one used any of four different approaches designed to improve the fit of the synthetic control by including pre-treatment values of violent crime in the matching protocol or whether one included none of these values.

Since our finding was so strong and robust, we were surprised that Moody and Marvell (2019) offered their own synthetic control analysis that appeared to question the DAW results. Unfortunately, MM's analysis has gone astray, and the short answer is that they have not undermined the synthetic control finding that RTC laws *increase* violent crime in the first decade following adoption.

MM's flaws in implementing their synthetic control analysis

The first step in a successful synthetic control analysis is to denote a set of possible states—called donor states—from which the synthetic control can be constructed. MM got off on the wrong foot by making a mess of that process. In total, we found 57 erroneous donor pool decisions by MM. Sometimes a state

that should not be in the donor pool was included; other times, states that should have been included were left out. For example, in their synthetic control analysis, MM erroneously treat Alabama as not becoming an RTC state until 2014 while the dominant coding that we employ treats Alabama as an RTC state as of 1975 (which MM also did in their panel data analysis).¹¹ Accordingly, as an RTC state, Alabama cannot serve as a control, yet MM treat it as a potential donor state for 26 out of the 33 RTC states they analyze (and a component of the synthetic control in 14 of those 26 RTC adopters). Seventeen states have some other difference between the donor pool used by Moody and Marvell (2019) and the appropriate states used by DAW (2018). Out of 33 states in the analysis, MM used only five donor pools identical to the correct pools used by DAW.¹²

While the various problems in the MM synthetic control analysis are not worth extended discussion, we just want to highlight how their abbreviated presentation omits any discussion of some of the major pitfalls in their approach. One obvious problem can be seen by examining their own synthetic control estimate of the impact of RTC laws on violent crime in Idaho. MM indicate that Idaho had a violent crime rate of 290 per 100,000 during the first full year of having a RTC law in 1991. Unfortunately, their poorly fit synthetic control had an estimated value of 500 per 100,000 that year. For the next two years, that rather wide disparity between the actual and MM synthetic control estimates of violent crime remained roughly stable, suggesting there had been little impact on crime in those two years, yet under MM's assumptions these were years of more than 40 percent crime drops engineered by the adoption of RTC laws! In other words, MM attributed the massive discrepancy between violent crime in synthetic Idaho and actual Idaho *before* Idaho's RTC law was adopted—resulting from their poor fit—as a crime-reducing benefit of the RTC law.

Over the ten-year period following RTC adoption, the violent crime drop

11. While there is some ambiguity in the appropriate date that Alabama should be coded as having an RTC law, we believe that MM were correct in their treatment of Alabama in their panel data analysis but wrong in using a 2014 RTC date for the state in their synthetic control analysis. The Rand Corporation's Gun Policy in America initiative "developed a longitudinal data set of state firearm laws" that codes the start of Alabama's RTC law as occurring in 1975, as we do (see <https://www.rand.org/pubs/tools/TL283.html> for the downloadable database). This is also consistent with the codings used by the National Rifle Association (NRA), John Lott, and the NRC *Firearms and Violence* report. Indeed, if one looks at Lott's estimated percentage of citizens with concealed carry permits, Alabama ranked first among all the states for which he had data. Lott lists the Alabama percentage as greater than 8 percent for 2007—seven years before the date that MM use for Alabama in their synthetic control analysis (Lott 2010, 238). Moreover, the 2014 date that MM use would imply that Alabama was one of the last states in the union to adopt a RTC law, which would not be consistent with the gun politics of the region nor the estimated percentage of permit holders in the state seven years prior to 2014. The NRA clearly would have successfully pushed for an RTC law in Alabama decades ago if Alabama was thought not to have one.

12. DAW (2018, 60, Table A1) provides the complete list of dates for RTC adoption.

in MM's synthetic Idaho was estimated to be over 35 percent (from 501 to 324), which was substantially better than the far smaller 16 percent drop in actual Idaho (from 290 to 243). Yet MM treat this as evidence of statistically significant and substantial crime drops caused by Idaho's RTC law. Note that the DAW synthetic controls analysis was superior because it produced a much better fit (the DAW initial year synthetic Idaho estimate was 344, versus the MM estimate of 501!), but also because DAW did not treat that pre-existing difference as evidence that the RTC law immediately caused a major drop in crime.¹³ By doing so, MM were able to mask the fact that their own analysis frequently showed that the synthetic control performed much better (with either larger crime drops or smaller crime increases) than the comparable RTC-adopting state over the ten years following adoption.¹⁴

Aggregating MM's synthetic control estimates reveals that RTC laws increase violent crime

This unpromising beginning ends in an array of synthetic control estimates that on the whole are considerably less promising than those contained in the DAW synthetic control analysis. Essentially, MM got some very bad fits on small states and then used those poor fits to argue that there is no support for the DAW position because 14 states adopting RTC laws experienced statistically significant increases in crime and 12 experienced decreases.¹⁵ (Note that our more accurate synthetic control analysis would show a 15-to-8 advantage for RTC laws causing statistically significant *increases* in crime, which grows to 16-to-4 if one limits the

13. One can see this same problem illustrated in MM's synthetic control graph of the murder rate in Texas (MM 2019, 92, Figure 1). MM's poorly fitting synthetic Texas has a substantially higher murder rate than actual Texas at the time of adoption of the Texas RTC law. Their graph highlights that this occurred because Texas enjoyed a substantial drop in murder relative to the synthetic control—*prior to the adoption of the RTC law!* The MM calculus treats that ill-fitting differential as a benefit of the law, even though if one examined how crime changed in both Texas and synthetic Texas in the aftermath of RTC adoption, no such murder-reduction benefit would be observed.

14. Since we were trying to show whether the panel data finding that RTC laws increased crime was supported by a synthetic control analysis, it was important to use the same 1979–2014 time period for both approaches, which we did. Extending the data set further backwards creates data problems for variables such as poverty and unemployment, which were either not available or not consistently gathered prior to 1979. Disregarding these concerns, MM started their panel data analysis in 1977, and, without explanation, used a different time period (extending back to 1970) for their synthetic control analysis.

15. MM show that, for their statistically significant results, the majority of states experienced an increase in violent crime using the preferred “nested approach” but then go on to present inferior “default” results perhaps because the inferior estimates weakened their finding of a 14-to-12 state dominance for RTC laws increasing violent crime. This is not good practice, and the “default” estimates, which are only appropriate when “nested” results cannot be computed, should be ignored in the MM paper. See the documentation for the Stata synth program, which states that the nested option offers “better performance” than the default option (Abadie, Diamond, and Hainmueller 2014).

analysis to six to ten years after adoption, reflecting the consistent pattern that the harm of RTC laws rises over the decade following adoption.) Even though the errors in implementation invalidate the MM synthetic control analysis, if MM had simply computed how much violent crime was estimated to have changed in aggregate for the 33 RTC-adopting states for each of the ten years using their own estimates, they would have generated the estimated impacts of RTC laws on violent crime shown in Table 2.

TABLE 2. The impact of RTC laws on the violent crime rate, MM synthetic control methodology and data, 1970–2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average non-normalized TEP	7.21*	7.61*	6.64	8.06	9.81*	10.97**	11.01**	12.55**	14.86**	16.26***
	(3.82)	(4.05)	(4.21)	(4.72)	(4.78)	(4.76)	(4.79)	(5.41)	(5.05)	(4.80)
N	33	33	33	33	33	33	33	33	33	31
P-value	0.07	0.07	0.13	0.10	0.05	0.03	0.03	0.03	0.01	0.00
Notes: Standard errors in parentheses. Column numbers indicate post-passage year under consideration; N = number of states in sample. Dependent variable is the percentage difference in the violent crime rate in treatment and synthetic control states at given post-treatment interval. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.										

Table 2 presents the aggregate, population-weighted impact of RTC laws on violent crime using MM’s own data and synthetic control methodology (which does not normalize the estimates to equate the actual and synthetic control crime rates at the time of RTC adoption). In other words, Table 2 just takes MM’s actual individual state estimates—which they fail to show—and aggregates them. The finding is clear: RTC laws consistently generated a statistically significant increase in violent crime, rising from a 7.2 percent increase in the first year to 16.3 percent in the tenth year. Note that this is even a larger violent crime increase than that predicted in the DAW synthetic control table reproduced in Table 1 above. Remarkably, MM have completely disguised the key finding of their own synthetic control analysis, which is that, in aggregate, RTC laws are estimated to have substantially increased violent crime.¹⁶

16. The MM (2019) synthetic control analysis goes astray so badly because their non-normalized violent crime estimates tend to be large and positive for big states (for example, four of the five highest population states have positive estimates and three of those four are bigger than 15 percent by the fifth year after RTC adoption) and large and negative for small states (four of the five lowest population states have negative estimates by the fifth year, ranging from -29 percent for Wyoming to -78 percent for North Dakota). Not surprisingly, the unrealistically large negative results tend to be found in the states with the worst pre-treatment fits between synthetic control and treatment states. The DAW (2018) paper documents the ratio of the root mean-squared prediction error (RMSPE) to the mean violent crime rate as a measure of goodness of pre-treatment fit and indicated particular concern when this value rose above 19 percent. To highlight how the MM synthetic control model was doing a particularly bad job for generating plausible controls for small states, note that the error ratio averaged a whopping 48.3 percent for MM’s estimates for

We are quite confident that the DAW (2018) paper has the best available synthetic control estimates of the impact of RTC laws on crime because our synthetic control analysis is done with greater care, with more accurate coding of RTC law adoption dates, and with a far more probing array of robustness checks than the MM analysis.

Conclusion: The best evidence shows that RTC laws increase violent crime

We have shown that the DAW population-weighted panel data estimates shown in Figure 1 satisfy the parallel-trends assumption of a valid panel data analysis, while neither of the alternative models advanced by MM do. This is on top of the serious miscoding problems of the MM panel data analysis. Nonetheless, a proper interpretation of the two MM models (shown in Figures 2 and 3) can reveal that RTC laws alter the path of violent crime starting at the date of adoption and generate statistically significant deviations from prior trends within a decade of passage.

Of course, the fact that our Figure 1 is the best panel data model does not mean it is perfect, and we take the MM critique as providing another reason to be interested in the results of the synthetic control approach to gain insight into the difficult problem of specification that exists in every panel data analysis. While we find the MM synthetic control approach to be too flawed and primitive to rival the more accurate, thorough, and sound analysis in the DAW paper, it is encouraging to see that their analysis conducted over a longer time frame (1970–2016, while ours extended from 1977–2014) and using a non-normalized set of estimates (in contrast to our normalized estimates) still found that a majority of states experienced statistically significant increases in violent crime from RTC adoption. It is likewise encouraging that the aggregated impact across all states mimicked our own analysis in finding strongly increasing violent crime over the decade following RTC adoption (compare our estimates, shown in Table 1, with those aggregated from the MM results, shown in Table 2).

In summary, there is consistent evidence that RTC laws elevate violent crime in the decade after adoption whether one looks at DAW's panel data estimates (Figure 1) or synthetic controls estimates (Table 1) or the properly interpreted

the nine smallest states and only 8.5 percent for the nine largest states. Accordingly, the clear pattern that RTC laws increase violent crime in the ten-year period following adoption emerges whether one weights the actual MM state estimates by population (as we show in Table 2), weights by the inverse of this error ratio, or simply drops the worst fits from the analysis.

panel data results using MM's suggested non-population weighted or FGLS approaches (Figures 2 and 3) or the MM synthetic controls estimates (Table 2). Policymakers and citizens should recognize that the best available empirical data to date supports the view that RTC laws have resulted in statistically significant increases in violent crime in the ten-year period after adoption.

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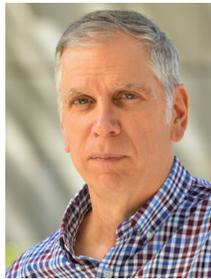
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About the Authors



John J. Donohue III is an economist as well as a lawyer and is well known for using empirical analysis to determine the impact of law and public policy in a wide range of areas, including civil rights and antidiscrimination law, employment discrimination, crime and criminal justice. Before rejoining the Stanford Law School faculty in 2010, Professor Donohue was the Leighton Homer Surbeck Professor of Law at Yale Law School. He is a member of the American Academy of Arts and Sciences, and the former editor of the *American Law and Economics Review* and president of the American Law and Economics Association and the Society of Empirical Legal Studies. He is also a Research Associate of the National Bureau of Economic Research. His email address is donohue@law.stanford.edu.



Abhay Aneja is a J.D./Ph.D. candidate studying at Stanford Law School and the University of California, Berkeley. His email address is aneja@berkeley.edu.



Kyle D. Weber is currently a doctoral student in Economics at Columbia University, having previously worked as a research fellow at Stanford Law School. His primary research interests are industrial organization and media economics. His email address is kdw2126@columbia.edu.

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