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More Gun Carrying, More Violent Crime

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I have written a number of articles, usually with various coauthors, discussing the issue of the impact of laws allowing citizens to carry concealed handguns on crime, and commenting on the challenging methodological issues involved in trying to estimate this impact from panel data models, looking at data for all states over an extended period of time. One feature of the literature trying to estimate the impact of these so-called right-to-carry (RTC) laws is that it has improved over time: Panel data econometric methodology has advanced in a number of ways since the initial paper by John Lott and David Mustard (1997) examining the question, and there is simply more data to analyze as time goes by and more states have adopted RTC laws. Moreover, additional scholarly work has improved the coding of key variables, such as dates of adoption of RTC laws, and my work has constantly tried to upgrade the quality of the data in this dimension as well as from FBI crime data corrections, which has been a continuing process. Importantly, new statistical techniques to supplement or replace panel data approaches have enabled a clearer picture to emerge of the likely impact of RTC laws than was previously available.

In contrast to this continuing set of advances, Carlisle Moody and Thomas Marvell (MM), in their latest attempt to criticize any evidence that RTC laws increase crime, take us back to an old paper I coauthored with Abhay Aneja and Alexandria Zhang (ADZ 2014) that analyzed county data through 2006 and state data through 2010. The abstract of that paper by ADZ notes: “It will be worth exploring whether other methodological approaches and/or additional years of

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data will confirm the results of this panel-data analysis and clarify some of the highly sensitive results and anomalies that have plagued this inquiry for over a decade.” A number of newer papers—including a major new work of mine co-authored with Aneja and Kyle Weber (DAW 2018)—have now forged ahead with various new approaches and more and better data, so I would rather focus attention on this new and improved literature rather than rehearsing old issues that in a number of respects have now been superseded by various statistical and data improvements.² Nonetheless, a few comments of retrospection may be valuable if coupled with some elaboration of the continuing advances in our knowledge. In the first section I will address the major points that MM make in their latest paper, and in the second section I will briefly comment on some of the latest developments in the literature addressing the impact of RTC laws on crime.

The MM criticisms and my response

MM begin their paper with a list of seven “chief criticisms,” six of which are simply wrong, misleading, and/or largely irrelevant, and one of which is worth considering (the issue of state-specific trends in panel data models). They also waste the reader’s time with a number of petty complaints that are irrelevant to the important issue of the impact of RTC laws.³

MM criticize the finding that RTC laws increased murder over the period 1999–2010

The first critique that MM level is that they do not like the ADZ analysis of data from 1999–2010 that was designed to estimate the impact of RTC laws over a period that would eliminate the confounding effect of the crack epidemic on crime. The matter is potentially important because we do not have a useful

2. For example, ADZ had followed a Lott and Mustard coding that set the date of the Virginia RTC law at 1988, when the correct date is now widely agreed to be in 1995. These and other codings were corrected in DAW (2018), so we would advise readers to focus on that paper, rather than the less accurate results presented in MM’s comment (or the earlier ADZ paper). Moreover, the ADZ model was designed to modify the Lott and Mustard model only by removing its most problematic features. DAW (2018) provides an independent attempt to generate the best model for estimating the impact of RTC laws on crime, which is an additional reason to prefer this more recent work to the older ADZ paper.

3. MM ramble on about the dates on the various drafts of a paper and even complain that one clearly demarcated date on an NBER working paper is not prominent enough for them. It is also a bit churlish for MM to end their paper with a complaint that I haven’t made available the data on a paper I am still working on and still revising, without even offering a trace of the customary thanks for data sharing in light of the fact that I furnished all the data and do files that MM used in drafting their paper.

explanatory variable that captures the criminogenic influence of the rise of crack cocaine markets in the late 1980s and early 1990s. But virtually everything MM say about it, except when they are summarizing points already made by ADZ, is incorrect or misguided.

There is no valid control for the criminogenic influence of crack

For example, MM hint that it might be better to come up with a control for the criminogenic influence of crack, such as price data used by Lott and Mustard or a crack index created by Roland Fryer et al. (2013). The price data is worthless, however, because no one has offered any logical linear relationship between the price of cocaine in a state in a given year and crime in that state and year, and in fact no such relationship has been found empirically. Nor is the crack index helpful as it has been shown to have no relationship to estimates of the impact of RTC laws (DAW 2018). The crack index is an index of crack prevalence, not of the criminogenic influence of crack on crime, which was largely mediated through gang rivalry over drug distribution. Moreover, since the crack index is only available for a limited set of years, its use would require throwing away years of data, which is the problem MM say they are most worried about.

MM coauthor Zimmerman also finds that post-crack era RTC laws increase murder

Interestingly, MM wrote an earlier paper attacking ADZ’s work on RTC laws, in which they teamed up with John Lott (the first to claim that RTC laws *reduced* crime) and Paul Zimmerman (Moody, Lott, Marvell, and Zimmerman 2012). Zimmerman has recently published his own work in this area, in which he looks at the exact 1999–2010 period that MM now say is inappropriate. Zimmerman finds that RTC laws increased murder by 15.5 percent, which provides support for the similar ADZ finding that RTC laws increased murder over the identical period (Zimmerman 2014, 71, 72). MM are curiously silent about Zimmerman’s finding, and perhaps MM should be taking up their complaint with their coauthor.

There is no merit to the MM complaint about power in the post-crack era analysis since the finding that RTC laws increase murder is statistically significant

MM’s complaint that ADZ examine a “greatly truncated sample” (again,

a complaint they might also have made about Zimmerman 2014) is consistent with MM's apparent inability to understand that many important issues are not susceptible to a simple binary resolution. You gain something from looking at post-crack era data, and you lose something. The resulting information needs to be weighed along with everything else we know. Yet MM tell us what they believe is the most important problem with the post-crack era analysis: "Obviously, and foremost, reducing the sample from 34 years to 12 years limits the power of the analysis" (2018, 55). This might indeed be a problem if ADZ and Zimmerman had been unable to detect a statistically significant increase in the homicide rate over this period, but, since they did detect such an increase, MM's "foremost" objection is utterly lacking in merit. One is concerned about power when the data cannot detect a true effect, so MM's critique would be valid had the regression failed to generate statistically significant evidence that RTC laws increase crime. The ADZ and Zimmerman finding that RTC laws generate a statistically significant increase in murder shows that power is not a problem in those regressions.

There is of course a trade-off in looking at a sub-period because one will necessarily be looking at only a subset of the RTC adoptions, since panel data will only generate estimates for RTC laws that change during the analyzed span of years. Looking at a sub-period would be most problematic if these late-adopting states were different enough that one would expect RTC laws would have a materially different impact in those states than it had on the larger number of earlier-adopting states. But MM spend a great deal of time arguing that the relationship between RTC adoption and crime has remained stable, so they are implicitly arguing that predictions that post-crack era RTC laws increase murder should constitute an unbiased estimate of the impact of RTC laws for all prior adopters.

MM's test shows the opposite of what they claim

MM's attempt to argue that the post-1998 data period was not substantially different from the earlier period of enormous crime increases followed by dramatic crime declines is designed to discredit the finding of increased rates of murder, but even the most cursory look at the data suggests that MM's contention is unlikely to be true. To buttress their implausible claim, MM seek to concoct some test that they argue will show that there was no regime change owing to crack. Amazingly, their own test shows that there *was* in fact a regime change for murder, with the break coming in 1993. In other words, while MM were hoping to establish that the ADZ (and presumably Zimmerman) analysis of 1999–2010 should be ignored, they have in fact proved exactly the opposite. Unless one can accurately model the differing regimes, it would be problematic to run a regression that spanned the two distinct crime regimes that MM have identified. In other words, in trying to undermine the

ADZ post-crack analysis, MM have completely substantiated the need for it.

But how then do they still maintain that their “test” shows that the ADZ and Zimmerman sub-period analyses are flawed? With a statement that seems as though it comes out of an *Airplane!* movie (“Surely you can’t be serious!” “I am serious, and don’t call me Shirley”), MM say that the data limitation is inappropriate because the regime change occurs in 1993, which means it can’t be caused by the crack epidemic as ADZ maintained. But the approach of ADZ (and Zimmerman) is validated as long as there is a regime change after 1993 that one cannot adequately control for, regardless of whether it is caused by crack or some other uncontrolled influence. In other words, MM have established a reason for skepticism of results estimated over the entire data period and fortified the rationale for conducting a sub-period analysis that is not marred by the regime change. Having just inadvertently established the wisdom of looking at a period after the large and opposing crime swings before and after 1993, MM then compound their error with the astonishingly obtuse claim: “Since the crack epidemic spanned the years 1984–1990, it would seem that it has had no effect on murder” (2018, 59).

Perhaps the one thing that every serious researcher on crime, as well as every urban police officer of the time, has agreed on is that the onset of the crack epidemic substantially increased the murder rate (Levitt 2004). The fact that MM think that they have proven otherwise shows that something is very wrong with their analysis. There is much more that could be criticized about their discussion of this issue,⁴ but the point should be clear: There was a regime shift in crime (many have attributed it to crack, but MM say it comes in 1993 after the crack period has ended), so it makes sense to look at the impact of RTC laws at a time period *after* the regime shift has occurred so as not to be confounded by it.⁵ ADZ did this, as

4. MM make a glaring error when they state: “the finding that RTC laws increase murder is based on comparisons between states that recently adopted laws with states that already had such laws, as opposed to the comparison to states that do not have the laws” (MM 2018, 53). This “criticism,” directed at the 1999–2010 analysis in the old ADZ paper and presumably at the Zimmerman panel data analysis over the same period, is actually a feature of every panel data analysis of RTC laws that includes all states, including all of MM’s past and current regressions on RTC laws. But even if there were merit in this objection, it can easily be addressed by simply running the panel data analysis only on never-passing states and those adopting RTC laws during the post-crack era, since that will limit the comparison to new adopters and never adopters. This restricted regression does not undermine the finding that RTC laws increase crime. There is simply nothing to this critique.

5. It should be noted that MM offer no evidence for their claim that the crack epidemic lasted from 1984–1990, nor even any explanation of what they mean by the crack epidemic (which could reflect a measure of crack consumption when we are interested in the contribution to increased crime caused by that new drug plague). Perhaps their current analysis here should be taken to mean that the end of the crack epidemic came in 1993. Moody has recently submitted an expert report for the National Rifle Association in litigation challenging California’s ban on large-capacity magazines in which he asserts without any justification that the crack era went from 1984–1991 (*Duncan et al. v. Becerra et al.*, S.D. Cal., case no. 3:17-cv-01017-BEN-JLB, report dated November 3, 2017), so there is clearly imprecision and flexibility in

did Zimmerman (2014) and Donohue (2017), and all found statistically significant evidence that RTC laws increased the rate of murder and/or firearm homicides.

MM argue the murder finding is based on incorrect standard errors

MM continue their critique of the finding that RTC laws increase murder by stating that “ADZ claim that finding is statistically significant, but we find that their standard errors and t-ratios are incorrect and that the finding is not significant” (MM 2018, 53). Just to be clear, MM found no error but rather think that they have a better way of adjusting the standard errors than a standard approach of using a cluster adjustment. In fact, the National Research Council (2005) report on RTC laws argued that no standard error adjustment of any kind was needed, and it was earlier ADZ work that showed that some adjustment to the standard errors was needed to avoid exaggerated findings of statistical significance. If the NRC report is correct on this issue (I don’t think it is) or ADZ’s clustering standard error adjustment is appropriate, then the finding that RTC laws increase murder is highly statistically significant. MM offer an alternative approach to estimate the standard errors for the RTC estimates but the impact of their adjustment is relatively slight: It barely nudges the level of significance of the estimate that RTC laws increase the murder rate slightly below the .05 level. MM seem to think that this undermines the finding that RTC laws likely increase the murder rate. It does not.

Even assuming that the standard error adjustment that MM made was preferable to the ADZ cluster adjustment (and to the NRC report view arguing for no adjustment), it would only raise the p-value to .056. But MM’s belief that results with a p-value below .05 should be believed while those above .05 by even the slightest amount should be rejected makes no sense, for the reasons recently set forth in *Nature* by the top statisticians Blakeley McShane and Andrew Gelman under the admonitory heading “Abandon statistical significance:”

This year has seen a debate about whether tightening the threshold for statistical significance would improve science. More than 150 researchers have weighed in. We think improvements will come not from tighter thresholds, but from dropping them altogether. We have no desire to ban *P* values. Instead, we wish them to be considered as just one piece of evidence among many, along with prior knowledge, plausibility of mechanism, study design and data quality, real-world costs and benefits, and other factors. (McShane and Gelman 2017)

MM’s delineation of the crack era.

We are speaking of an estimated substantial increase in murder emanating from adoption of RTC laws. It would be foolish indeed for a legislator (or judge) to ignore this evidence that RTC laws lead to more murder simply because the p-value was just barely above the .05 level. Indifference to evidence of increased murder in slavish adherence to what is little more than a rough rule of thumb is reckless at best.

MM complain that ADZ ignore county-data estimates

MM tell us that “ADZ ignore results based on county-level data, which do not support their hypothesis that RTC laws increase crime.” This is simply incorrect. In fact, ADZ present both county-level and state-level estimates, and allow the reader to decide which estimates are preferable. I reproduce two relevant figures from ADZ which each graphically depict 14 different estimates of the impact of RTC laws for both the dummy and spline models for aggravated assault using different data sets (state and county), time periods (through 2000, 2006, or 2010), and models (Lott and Mustard versus the ADZ preferred model, with and without state trends). For example, Figure E5 shows estimates of the impact on aggravated assault using the dummy model, which is designed to capture the average effect of RTC laws during the post-passage period. The first bar in each of the first six groupings corresponds to county-level estimates (generally run on data through 2006 unless otherwise noted); the second bar corresponds to state-level estimates (generally run on state data through 2010), for a total of 14 estimates per figure. Additionally, the last two estimates in each Figure only contain one bar corresponding to state models run between 1999 and 2010 (since our county data ended in 2006). The figure was designed to facilitate quick visual observation of the size and statistical significance of an array of estimates.

As the ADZ paper states:

The estimates of the impact of RTC laws on aggravated assault in Figures E5 and E6 are significant at at least the .10 level suggesting crime increases in 11 of the 28 estimates depicted, as indicated by the shading of the columns. Note that the overall impression from these two figures is suggestive that RTC laws *increase* aggravated assault, although the evidence is not uniformly strong in the more preferred models. No other crime category has as strong evidence of an impact of RTC laws as the findings on aggravated assault. (ADZ 2014, 100–101)

Figure E5. Various Aggravated Assault Estimates (Dummy Model)

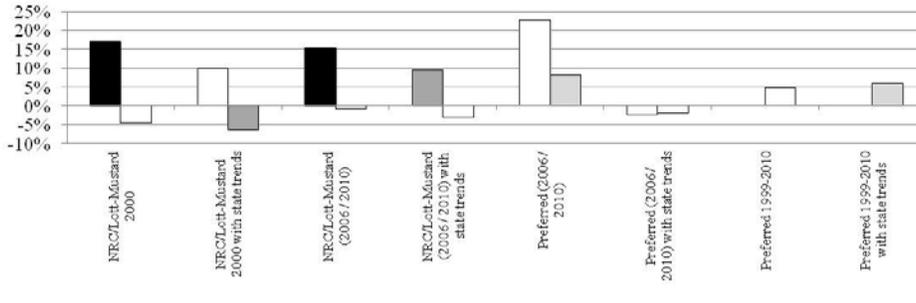
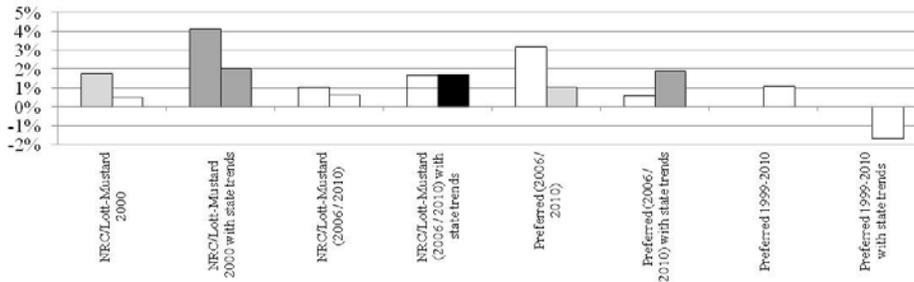


Figure E6. Various Aggravated Assault Estimates (Spline Model)



Note: No shading indicates insignificance, and the shading darkens as significance increases (from a light grey indicating significance at the .10 level, slightly darker indicating significance at the .05 level, and black indicating significance at the .01 level). (ADZ 2014, 101 n.72)

ADZ presented reasons for considering that county-data estimates were less reliable than state-level estimates (for example, the government withdrew the 1993 county data as too flawed to rely on and scholars have emphasized the superior reporting and more complete FBI validation in the state versus county data; in addition, the county data only went to 2006 while the state data analyzed in ADZ went through 2010). But ADZ certainly didn't ignore the county-data results, nor would they have any reason to do so to avoid showing uncongenial results (as MM recklessly suggest). Indeed, many of the county-data estimates (the left column in any pair) were larger and more statistically significant than the state-data estimates (the corresponding right column). Since MM have had much greater support for their coauthor Lott's regression model, their affection for the county-level data and for using the longest data period would presumably push them to embrace the models with those three attributes. But had they done so (whether they selected the model with or without state trends) they would have endorsed a finding that RTC laws substantially *increase* aggravated assault at the .05 level or better, as seen in the Lott and Mustard county-data estimates through 2006 in Figure E5. Thus,

MM should have written: ‘While ADZ chose to emphasize models selected on articulated and rational grounds regardless of the resulting prediction, MM chose to ignore the county-level Lott model that they have claimed to endorse, which showed a statistically significant *increase* in aggravated assault.’

MM complain that ADZ argue against including state-specific trends

MM state that ADZ “report the results of their model with ‘preferred’ controls with and without individual state trends. However, since the state trends are highly significant ($p < .0001$) and are highly correlated with both the dependent variable and the RTC dummy and post-law trend, the results without the state trends may suffer from omitted variable bias” (MM 2018, 53). This is actually the only issue in the MM paper worth considering: Is it advisable to include linear state-specific trends in panel data estimates of the impact of RTC laws on crime?

ADZ show estimates with state-specific trends that show RTC laws increase aggravated assault

Again we should emphasize that MM have been far more enthusiastic about their coauthor Lott’s county-data model with state trends, and as we just saw in Figure E5 from the ADZ paper reproduced above, that model shows a statistically significant *increase* in aggravated assault follows from RTC adoption. Moreover, as Figure E6 shows, RTC laws lead to a statistically significant *increase* in aggravated assault using state data with state-specific trends over the entire data period, whether one uses the Lott and Mustard or ADZ spline model. In other words, it certainly is not the case that using state trends eliminates the finding that RTC laws increase violent crime.

Potential problems with including state-specific linear trends, especially over long data periods with differing crime regimes

But should the state trends be included? As ADZ noted, adding state trends could be helpful if it corrects for an important omitted variable, but it could also be harmful because the state trends will not just pick up a pre-existing trend but will also pick up any effects of the RTC law that unfold over time in a similar fashion to the pre-existing trend. Contrary to what MM seem to believe, the fact that the state trends are statistically significant does not distinguish which of these situations is present.

The problem is illustrated by the well-known case of claims that the Civil Rights Act of 1964 could not have stimulated black economic welfare because the black-white wage gap closed at the same rate from 1940–1960 as it did from 1960–1980. A pre-existing linear trend that continued after the implementation of the 1964 law was (incorrectly) taken as a sign the law had no effect. In fact, the law had a substantial effect in the decade after adoption and the pre-existing trend of a narrowing wage gap that seemingly continued unabated would have ended without the new stimulus provided by the 1964 Act. Essentially there were different effects operating in each time frame and both elevated black wages: From 1940 to 1960 black migration from the poor rural south to Northern cities enhanced relative black earnings, but this migration ended by 1964. At that point the improving trend in the black-white wage gap continued, but now fueled by the demand stimulus provided by the 1964 Act: Blacks were suddenly allowed to enter entire industries from which they had previously been excluded (Donohue and Heckman 1991).

The general point is that state-specific trends don't just pick up pre-existing trends, they pick up any variation over time that hasn't effectively been modeled, and if the impact of RTC laws on aggravated assault rises over time as the number of permits rise—as the evidence seems to suggest—then this dynamic response will be obscured by the inclusion of a state-specific trend. In any event, ADZ included the estimates derived using state-specific trends, which at times yielded statistically significant increases in aggravated assault in Figures E5 and E6, so that readers could make their own judgment about the weight of the evidence. As the ADZ abstract states:

Our paper highlights some important questions to consider when using panel data methods to resolve questions of law and policy effectiveness. We buttress the NRC's cautious conclusion regarding the effects of RTC laws by showing how sensitive the estimated impact of RTC laws is to different data periods, the use of state versus county data, particular specifications (especially the Lott-Mustard inclusion of 36 highly collinear demographic variables), and the decision to control for state trends.

Across the basic seven Index I crime categories, the strongest evidence of a statistically significant effect would be for aggravated assault, with 11 of 28 estimates suggesting that RTC laws increase this crime at the .10 significance level. An omitted variable bias test on our preferred Table 8a results suggests that our estimated 8 percent increase in aggravated assaults from RTC laws may understate the true harmful impact of RTC laws on aggravated assault, which may explain why this finding is only significant at the .10 level in many of our models. Our analysis of the year-by-year impact of RTC laws also suggests that RTC laws increase aggravated assaults. Our analysis of admittedly imperfect gun aggravated assaults provides suggestive evidence that RTC laws may be associated with large increases in this crime, perhaps increasing such

gun assaults by almost 33 percent. (ADZ 2014, abs.)

While the issue of state-specific trends is a challenging one, the regime change—that we have just seen MM document—from a period of rapid crime rise to one of rapid crime decline is unlikely to be handled well by the inclusion of linear state-specific trends. In fact, this issue may only add to the concern over estimating panel data models over the extended data duration that MM seem to advocate. These considerations provide further reasons for turning to newer statistical approaches, such as synthetic controls, which is less sensitive to modelling choices and which has been deemed “arguably the most important innovation in the policy evaluation literature in the last 15 years” (Athey and Imbens 2017, 9).

Analysis of pre- and post-law dummy variables

MM complain that ADZ “present results from a set of pre- and post-law dummy variables without disclosing the associated significance levels” (MM 2018, 53). Oddly, MM then present a table based on panel data regressions with state-specific trends that purports to show essentially no effect of RTC laws using similar year-by-year dummy variables to ADZ, but the MM table contains two errors: (1) it fails to include the pre-law dummies, which are used to evaluate the key panel data assumption of parallel trends, and (2) it fails to disclose the correct significance levels. The second problem is particularly strange since MM seem to realize that the relevant dummy estimate is not the full dummy value for a given post-law year but rather the difference between that value and the initial dummy value (in order to capture the change in, say, aggravated assault after RTC adoption). Instead of providing t-statistics for the relevant adjusted dummy estimates, MM incorrectly provide them for the irrelevant unadjusted dummies.

Moreover, as we just saw, MM have given us a reason to be wary of the estimates run on the entire data period, namely the regime change they identified, and this would be a particular concern when including state-specific trends, as they do in their analysis. The combination of the problems of regime change, the uncertainties associated with the use of state-specific trends, and the evidence from the ADZ pre- and post-law dummy variable estimates that MM reference persuaded me that there were enough concerns about panel-data estimates, at least through 2010, that it would be worthwhile to try to secure more and better data and seek new and better estimation approaches, as the NRC report recommended. That is what gave rise to Donohue, Aneja, and Weber (2018), and now I turn to the latest evidence from this new and improved paper and other recent scholarly work in this area.

More data and new statistical techniques

Major conclusions from the prior discussion

The basic conclusions that should be drawn from the MM paper and the preceding comments are:

1. There are potential problems in generating panel data estimates over the entire period from the late 1970s through 2010 because there were two different regimes, pre- and post-1993.
2. These two regimes, of first sharply rising and then steeply declining crime rates, raise doubts about the wisdom of using state-specific linear trends to capture strong non-linear movements in crime over the full data period. Nonetheless, all state-level models with state trends showed statistically significant increases in aggravated assault (Figure E6), as did the Lott and Mustard county-data dummy model with or without state trends (Figure E5).
3. The evidence that RTC laws increase murder over the period from 1999–2010 is extremely strong if one subscribes to the NRC report's view that no adjustment of standard errors is necessary, very strong if one uses the ADZ cluster adjustment (also employed by Zimmerman 2014), and still quite strong with a p-value of .056 if one uses the proposed MM adjustment. A proper evaluation of the costs of Type I and Type II error in this domain suggests that the finding that RTC laws increase murder should be taken very seriously.

Massive gun thefts in RTC states fuel violent crime even in non-RTC states

New discoveries have also raised additional concerns about bias in the panel-data estimates. A growing body of evidence highlights the enormous increase in gun thefts that follow the practice of carrying guns outside the home (see DAW 2018, 10–11). A plausible estimate is that 100,000 guns a year are stolen from concealed-carry permit holders, and as Wayne LaPierre of the National Rifle Association has emphasized stolen guns play a significant role in increasing the rate of murder and violence (DAW 2018, 10 n.16, 10 n.17). This massive amount of gun theft not only elevates crime in RTC states, but also has harmful effects outside the

particular RTC state by contributing weapons to the Iron Pipeline that fuels crime in non-RTC jurisdictions such as New York City, California, Maryland, and (prior to 2014) Illinois. This phenomenon undermines a core assumption of panel-data models that the treatment will not influence crime in the control states (DAW 2018; Smith 2016). Consequently, a panel-data analysis will generate biased estimates that understate the crime-increasing impact of RTC laws.

Recent panel data studies finding that RTC laws increase crime

The likely bias in panel-data estimates against a finding that RTC laws increase crime, coupled with concerns about RTC estimates derived over the pre- and post-1993 crime regimes, again pushes us to examine post-crack era data. This makes Zimmerman’s finding noteworthy since it avoids the second concern by looking at 1999–2010 data and overcomes the first in that it finds that RTC laws increase crime: “The shall-issue coefficient takes a positive sign in all regressions save for the rape model and is statistically significant in the murder, robbery, assault, burglary, and larceny models. These latter findings may imply that the passage of shall-issue laws increases the propensity for crime, as some recent research (e.g., Aneja, Donohue, & Zhang, 2012) has suggested” (Zimmerman 2014, 71).

Other recent evidence buttresses the Zimmerman finding. In a recent paper in the *American Journal of Public Health*, I showed that over the 2000–2014 period, when 11 states adopted RTC laws, there was strong and statistically significant evidence that firearm homicides were elevated by RTC adoption (Donohue 2017). In the same issue of the *American Journal of Public Health*, Michael Siegel and co-authors (2017) concluded that RTC laws lead to a substantial increase in murders, almost all of which comes through increased firearms killings—specifically from handguns. And a new paper by Marjorie McElroy and Peichun Wang (2017) concludes, using a wholly different estimation approach, that violent crime would be one-third lower if a state had not adopted a RTC law.

LASSO and synthetic controls analyses show RTC laws increase violent crime

The new work by DAW (2018) adds four more years of data to the ADZ data set while fully updating many of its data variables including the dates of some RTC laws that earlier studies had mis-timed.⁶ DAW (2018) not only provides a panel data

6. Specifically, it is now widely accepted that Virginia did not adopt RTC until 1995, although Lott and

analysis on this improved dataset but also adds two newer statistical approaches: a LASSO treatment and a synthetic controls analysis.

The LASSO approach is designed to probe whether the RTC estimates remain consistent across an array of explanatory variables. Indeed, they do (even though this analysis is conducted over the entire data period from 1979–2014 and thus has the regime change issue discussed above): The LASSO analysis suggests across all values that RTC laws are associated with positive increases in violent crime ranging from 1.4 percent to 8.4 percent.

The synthetic-controls estimates are even larger, and this approach appears to do a much better job than the panel-data evaluation in generating robust and statistically significant evidence that RTC laws increase violent crime from 13–15 percent by the tenth year after adoption. Unlike the panel-data estimates, the finding of increased violent crime holds regardless of which set of explanatory variables is employed. Furthermore, some of the weaknesses in analyzing RTC with panel data—such as problems using linear state time trends over an extended time frame when crime was very volatile and bias caused by the flow of guns to non-RTC states and by omitted variables, such as the criminogenic impact of crack—are more readily addressed using the synthetic controls analysis.

The emerging consensus: RTC laws increase violent crime

At one point, some argued, with apparent belief, that RTC laws decreased crime. The libertarian instincts of Moody and Marvell seem to push them towards the view that perhaps there is a zero net effect of RTC laws on crime. Of course, if RTC laws really generated no net crime reduction, they must be socially harmful because there are obvious costs even beyond the billions of dollars spent on buying guns, training for their use, maintaining them, and carrying them around (not to mention the contribution to accidental shootings, plus the interference with policing that attends RTC adoption; see DAW 2018, 12–14).⁷ Those billions could clearly reduce crime if properly allocated towards known crime-reducing policies. But as Phil Cook and I, writing in *Science*, summarize the latest evidence: “With many years of post-crack-era data now available, there is an emerging consensus that, on balance, the causal effect of deregulating concealed carry (by replacing a restrictive law with an RTC law) has been to increase violent crime.”

Mustard (1997) followed an erroneous coding of 1988, as had the NRC report and ADZ (see ADZ 2014, 108). Accordingly, the NRC report and ADZ estimates should be recognized as having this error in state coding, which has now been corrected in the DAW data set.

7. Would MM want to argue that even if gun carrying doesn't make the public safer, the misperception that it does generates psychological benefits? Might this be part of their strategy to protect these soothing fantasies from any effort to illuminate the harsh truth?

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About the Author



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