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The Impact of Right-to-Carry Laws: A Critique of the 2014 Version of Aneja, Donohue, and Zhang

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LINK TO ABSTRACT

In 2005 the National Research Council (NRC) analyzed right-to-carry (RTC) laws, also known as shall-issue laws, which relax the requirements necessary to acquire a permit to carry a concealed weapon. The NRC essentially concluded that the data were not sufficient to determine whether RTC laws increased or decreased crime. However, a recent working paper from Abhay Aneja, John J. Donohue, and Alexandria Zhang (hereafter "ADZ") re-evaluates the NRC analysis and purports to find evidence that RTC laws increase murder, rape, robbery, and assault (ADZ 2014). ADZ's paper, issued by the National Bureau of Economic Research, has received substantial attention. In this article we re-analyze that paper from ADZ, using ADZ's own data and regression models, and contend that such conclusions are misleading.

ADZ have issued four versions of their paper "The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy" (ADZ 2010; 2011; 2012; 2014). The first version (ADZ 2010) was itself an extensively revised adaptation of Donohue's "The Impact of Concealed-Carry Laws" (2003). The first version and the latest two versions are working-paper versions, the latest two being issued by the National Bureau of Economic Research (NBER). As of the present writing (8 August 2017), the working-paper abstract

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page of the latest version, though prominently displaying in boldface, "Issued in August 2012," inconspicuously shows toward the bottom, "This paper was revised on November 7, 2014" (ADZ 2014). Here we address that latest incarnation of the ADZ paper.

ADZ make a number of choices that generate results that are then highlighted in the paper (by being featured in the abstract, for example). But those choices are sometimes unjustifiable, and ADZ sometimes oversell their results. Our criticisms of their highlighted results are many. Most importantly, ADZ use only part of the available data, arguing that a regime change requires that decades of data be thrown out—yet they do not test for the existence of such a regime change. Here we perform tests and find little evidence that such a regime change took place. Additionally, we note that ADZ compare states that have recently adopted laws with states that already had laws, that their standard errors are biased downward, that they exclude highly significant individual state trends, that they run multiple tests without adjusting significance levels, and that they fail to report significance tests on pre- and post-law dummy coefficients. We also remark on a related 2017 working paper (Donohue et al. 2017), pointing out that its results depend on excluding state trends, a decision we criticized here.

In the abstract of their paper, ADZ feature certain results. The following quotation comes from the abstract's two concluding paragraphs:

> 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 confidence 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.
>
> In addition to aggravated assault, the most plausible state models conducted over the entire 1979–2010 period provide evidence that RTC laws increase rape and robbery (but usually only at the .10 level). In contrast, for the period from 1999–2010 (which seeks to remove the confounding influence of the crack cocaine epidemic), the preferred state model (for those who accept the Wolfers proposition that one should not control for state trends) yields statistically significant evidence for only one crime—suggesting that RTC laws increase the rate of murder at the .05 significance level. (ADZ 2014, abs.)

Research findings pertaining to a heated, highly politicized issue like gun control quickly spread to public discourse, partisan debate, and court cases.[2] Particular results, generated from an untold number of trials and variations, get latched onto as debating points. For researchers to pretend that they can fully separate their own policy judgments from their research efforts would be an affectation. We therefore disclose that one of the authors of the present paper is generally libertarian and the other is agnostic with respect to policy questions in general. We are both in favor of good methodology. The research presented here is not the product of any sponsorship or funded research project.

In this paper we do not treat a 0.10 p-value as significant. We have noted the problem of the untold number of variations that researchers might perform. In what follows, when we say "highly significant" we mean a 0.01 p-value criterion, and "significant" a 0.05 p-value criterion.

Overview

The chief criticisms developed here are as follows. (1) The finding that RTC laws increase murder is based on a greatly truncated sample, and ADZ justify that truncation on the basis of an incorrect claim: that the 1984–1990 crack epidemic is a regime change that significantly affects the evaluation of RTC laws with respect to violent crime. (2) Furthermore, 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. (3) 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. (4) ADZ ignore results based on county-level data, which do not support their hypothesis that RTC laws increase crime. (5) They 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. (6) They estimate the same coefficients hundreds of times, yet do not make adjustments to the p-values indicating significance. (7) They present results from a set of pre- and post-law dummy variables without disclosing the associated significance levels.

2. Notably, Donohue (2003) was cited in *Peruta v. County of San Diego* (2016, 944).

ADZ's model

ADZ construct a fixed-effects model with unit (states or counties) and time (year) dummy variables:

$$y_{it} = \alpha_i + \gamma_t + \delta t + \beta_d \mathrm{RTC}_{it} + \beta_{tr} \mathrm{aftr}_{it} + X_{it} b + \varepsilon_{it} \tag{1}$$

where i and t refer to the state and year respectively; α_i is the state fixed effect and γ_t is the year fixed effect. The overall time trend is δt and X_{it} is a matrix of observations on the control variables. The two policy variables are: $\mathrm{RTC}_{\mathrm{it}}$, an indicator variable that equals the fraction of the year that the law is in effect the first year the law is implemented and equals one for each full year thereafter; and aftr_{it}, the post-law trend which is zero up to the year the law is imposed and equal to the number of post-passage years, adjusted by the fraction the law was in effect during the first year, for the remaining years. The control variables change according to the purpose of the analysis, but the most frequently employed are the "ADZ Preferred Controls" consisting of the following: prison population and police officers, both per capita and lagged; personal income, unemployment compensation, income maintenance expenditures, and pension payments, all per capita and in real dollars; the unemployment rate, the poverty rate, population density, and six demographic variables (percent black males and percent white males age 10–19, 20–29, 30–39). The regression is weighted by population, and it is estimated with and without state-specific trends. The standard errors are clustered by state to reduce the effect of serial correlation on the standard errors and t-ratios.

Do RTC laws significantly increase murder?

Even though the ADZ dataset extends from 1977–2010, for one set of analyses ADZ restrict the sample to 1999–2010, thus limiting the evaluation to the eight states that adopted RTC laws in that period: Colorado (2003), Kansas (2007), Michigan (2001), Minnesota (2003), Missouri (2004), Nebraska (2007), New Mexico (2004), and Ohio (2004). They argue that this limitation is necessary to avoid pernicious confounding effects of the 1984–1990 crack epidemic. It is, as reported in ADZ's abstract quoted above, only on the basis of the 1999–2010 sample that their claim "RTC laws increase the rate of murder at the .05 significance level" is made (ADZ 2014, abs.).

We find three reasons to cast doubt on the substance of ADZ's finding here.

Obviously, and foremost, reducing the sample from 34 years to 12 years limits the power of the analysis—and a careful investigation, presented in the next section of the paper, finds that the crack epidemic is not a valid reason for this data reduction. The two other reasons can be presented here briefly.

First, the comparison between the control and treated states is perhaps quite muddled because 39 states have RTC laws in some of the period 1977–2010, and so the control group for 1999–2010 consists of 31 states *with* RTC laws (the pre-1999 adopters)[3] plus ten states *without* RTC laws.

Second, by starting the analysis in 1999 and using only eight states for their dummy and spline variables, ADZ cause the clustered standard errors to be underestimated. ADZ's results with respect to the murder equation are presented in their Table 11a (p. 55) in which they report a positive and apparently highly significant break in the trend (= 0.014, t=2.68, p=0.01). While ADZ (2014, 32–35) had earlier in the paper purported to demonstrate, using the placebo law method, that clustering standard errors can resolve most of a concern that too many significant results will be obtained,[4] for that demonstration ADZ had used data for the longer 1977–2006 time period, which is notable because—as Timothy Conley and Christopher Taber (2011, 122, Table 3) show—clustered standard errors are seriously biased downward when there are few policy changes. To address our concern, we apply the placebo law technique, suggested by Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan (2004) and Eric Helland and Alexander Tabarrok (2004), in the case of the model reported in ADZ Table 11a, using ADZ's data and their model with "preferred" controls. For each iteration, we randomly select eight states, and for each state we generate a random year between 1999 and 2009. Given the state and year, we construct a RTC dummy and post-law trend. We then run the regression corresponding to the ADZ model with "preferred" controls. We repeat this process 9999 times. Since we know that 'laws' generated for random states and random years will have a true coefficient equal to zero, we then have 9999 observations on the distribution of the coefficients on the RTC dummy and the post-law trend which is centered on the true value of zero. With these data we calculate the values of the corresponding t-statistics and determine the 95th percentile values for the t-ratios corresponding to the RTC dummy and post-law trend. The result is that the five-percent critical value, two-tailed, for the post-law trend is 2.79. This critical value is greater than the ADZ t-ratio of 2.68 and thus indicates that the post-law trend is not significant at the .05 level. (The

3. This count includes five states (Alabama, Connecticut, New Hampshire, Vermont, and Washington) that have had RTC laws or their equivalent since before 1977, the first year in the ADZ state data set.

4. ADZ used the placebo law technique only to show that the usual ordinary least squares standard errors, used by Lott and Mustard (1997), are underestimated in the presence of serial correlation and that clustering corrects this problem.

corresponding five-percent critical value for the RTC dummy is 2.76, which is greater than the ADZ t-ratio of 1.09.) As a result of the small number of policy changes, as the placebo law approach shows, ADZ cannot claim a significant increase in murder in those states adopting RTC laws after 1999.

The crack epidemic is not legitimate grounds for dropping data

In some of ADZ's estimates they drop all data from 1977 to 1998, ostensibly because of the crack epidemic, and then highlight the resultant findings. That move tends to change results, as they note: "Of course, omitting an appropriate control for the criminogenic influence of crack is problematic if the high-crack states tend not to adopt RTC laws and the low-crack states tend to adopt. This is in fact the case" (2014, 61); "the criminogenic influence of crack made RTC laws look beneficial since crack was raising crime in non-RTC states" (ibid., 63): "the difficult-to-measure effect of the crack epidemic may influence our estimates" (ibid., 5); "In particular, we suspect that a major shortcoming of all of the models presented is the inability to account for the possible influence of the crack-cocaine epidemic on crime" (56–57); "the immense increases and then declines associated with the rise and fall of the crack epidemic, which threatened a key assumption of the panel data model of crime (since these dramatic crime shifts were not uniform across states and thus could not be expected to be adequately captured by year fixed effects)" (54).

But does the crack epidemic really bedevil our research efforts? Is throwing away a large amount of data better than trying to account for these different factors? Rather than throw away data, some researchers have tried the latter: John Lott and David Mustard (1997) controlled for the effects of cocaine using cocaine price data, and Moody and Marvell (2008) used the Roland Fryer et al. (2013) crack index (see also Bronars and Lott 1998; Lott 2010, 277–282).[5]

One way to demonstrate the effect of a regime change, such as the crack epidemic, on the coefficients of a policy variable is to do a recursive regression: The model is estimated on data before the purported regime change and observations are added one year at a time (see Hendry 1995, 613–614; Pollock 2003). The coefficients are plotted over time to see if they are relatively constant or are in fact affected by a regime change. We present the results of such an analysis for

5. ADZ (2014, 57 n.44) criticize the use of cocaine price data to control for the influence of crack. They also use the Fryer et al. (2013) crack index for illustrative purposes, but not to control for crack (ADZ 2014, 61–64).

the murder equation in Figure 1, where “dummy” is the RTC dummy and “postlaw_trend” refers to the post-law spline.

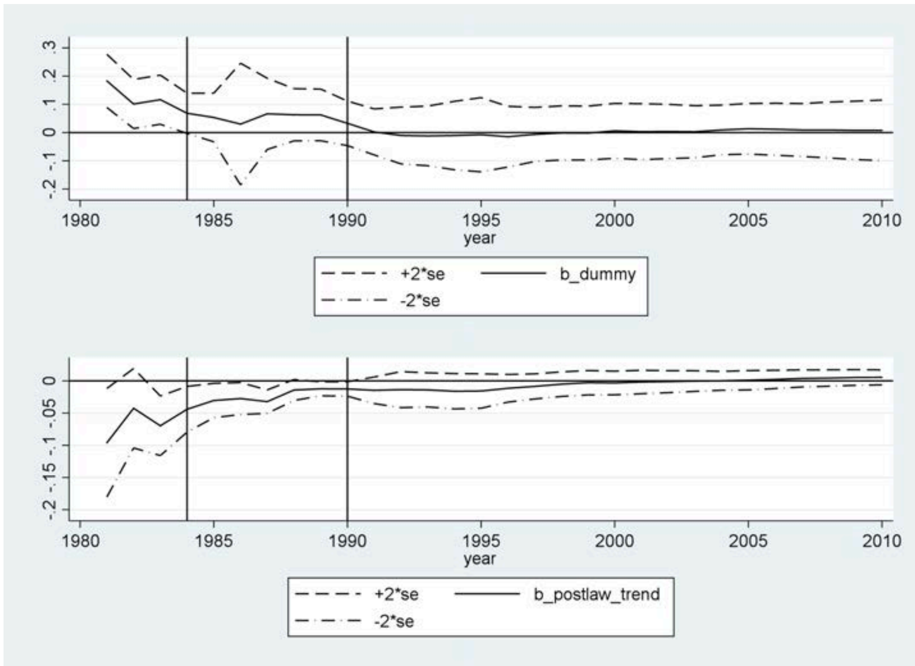

Figure 1. Recursive least squares: murder, dummy and post-law trend[6]

As the graph shows, there is no significant break during the crack years. The estimated coefficient on the RTC dummy starts significantly above zero, but drops steadily until it is virtually constant around zero from 1993 to the end of the sample. The coefficient on the post-law trend starts significantly negative. It drifts upward toward zero over time until it becomes insignificantly different from zero in 1990. In the case of the dummy, the error bands do not indicate more precision over time, but the error bands for the post-law trend indicate greater precision as the sample grows.

We also tested for a structural break in either the RTC dummy or the post-law trend in the ADZ model with “preferred” controls, with state trends, by including breaks for each year in turn (Stock 2003). That is, in equation (1) above, suppose the coefficient on the RTC dummy changes at some date τ, so that

6. We estimate coefficient for murder using the ADZ model with “preferred” controls and the ADZ data beginning in 1982, adding one year of data at a time and re-estimating. The error bands are plus/minus two standard errors. All results, data, and programs may be downloaded (**link**).

$$\beta_{d(t)} = \begin{cases} \beta_d & t \leq \tau \\ \beta_d + \theta & t > \tau \end{cases} \tag{2}$$

Similarly, suppose that the coefficient on the post-law trend changes at τ so that

$$\beta_{tr(t)} = \begin{cases} \beta_{tr} & t \leq \tau \\ \beta_{tr} + \varphi & t > \tau \end{cases} \tag{3}$$

Testing for a change in the coefficient estimates at τ is equivalent to testing the null hypothesis that θ=0 and/or φ =0 in the following regression.

$$y_{it} = \alpha_i + \gamma_t + \delta t + \beta_d \mathrm{RTC}_{it} + \theta Z_{1_t}(\tau) + \beta_{tr}\mathrm{aftr}_{it} + \varphi Z_{2_t}(\tau) + X_{it}b + \varepsilon_{it} \tag{4}$$

where $Z_{1t}(\tau) = \mathrm{RTC}_{it}$ if t>τ, zero otherwise and $Z_{2t}(\tau) = \mathrm{aftr}_{it}$ if t>τ, zero otherwise. The coefficients θ and φ capture the effects of any structural breaks at year τ on the RTC dummy and post-law trend, respectively.

Since we do not know whether there was a break during the crack epidemic, or any other year, we use the Quandt likelihood ratio, or QLR test (Quandt 1960; Andrews 1993). The test statistics are the maximum values of the t-tests corresponding to the hypotheses that θ=0 and φ=0, and the maximum value of the F-test corresponding to the null hypothesis that that θ=φ=0. Since the test fails at the endpoints, we follow convention and trim five years from each end (15 percent), yielding the sample 1982–2005. If the crack epidemic was as pernicious as ADZ claim, then we would expect to find a significant break in at least one of the years from 1984–1990 in each of the crime equations, although breaks at other years are also possible. The results are presented in Table 1.

TABLE 1. QLR test for structural breaks

	Max T-ratio dummy	Max T-ratio spline	Max F-ratio
Murder	0.75	2.26	6.216*
Rape	2.21	−0.36	3.482
Assault	1.30	2.56	3.326
Robbery	2.44	0.71	4.649
Auto theft	2.43	−0.67	3.919
Burglary	2.59	0.98	8.198*
Larceny	2.92	0.83	3.108

Note: *p<.05. ADZ model with "preferred" controls and state trends. The dummy is the RTC dummy, the spline is the post-law trend, and the F-test is for the joint hypothesis that both coefficients are equal to zero. The entries are the maximum values for the corresponding t-ratio or F-test.

The five-percent critical value for the t-tests is 2.95 and the corresponding five-percent critical value for the F-test is 5.86 (Stock and Watson 2007, 568). There are no significant structural breaks in any year between 1982 and 2005 for either the dummy or the post-law trend. However, there is a significant break in the F-test for the joint significance of both the dummy and the post-law trend for murder and burglary. The year corresponding to the maximum F-ratio for the murder equation is 1993. None of the other F-tests for the murder equation are significant. Since the crack epidemic spanned the years 1984–1990, it would seem that it has had no effect on murder, confirming the results of the recursive least squares test. With respect to burglary, there are five significant F-tests corresponding to the years 1984–1988. The overall conclusion is that the crack epidemic has had no significant effect on any crime equation except perhaps burglary. The effect on both murder and burglary is somewhat problematic since neither the coefficient on the dummy nor the coefficient on the post-law trend show any significant break.

ADZ elevate results that exclude county-level data

ADZ estimate their model using both county and state data. They find, using their "preferred" controls and county data, that RTC laws have no significant effect on any of the seven index crimes investigated (ADZ 2014, 41–42).

ADZ (2014, 43–44) argue, based on articles by Michael Maltz and Joseph Targonski (2002; 2003; Maltz 2006), that the county data is riddled with error and cannot be used for policy evaluation. They then turn to state-level data for the remaining analyses. They come to this conclusion despite the fact that county data have orders of magnitude more observations than state data, the errors are mainly in the smallest counties which can be easily dropped from the analysis, and the errors cannot be shown to be systematically associated with the variables of interest (Lott and Whitley 2003). To us, the case for highlighting state-level data is weak. Nevertheless, we follow ADZ and continue to focus on state-level data.

Should state trends be included?

ADZ (2014, abs., 36, 81) cite Justin Wolfers (2006) for justification for questioning whether state trends should be included in the regression model. Wolfers criticizes the Leora Friedberg (1998) study of the effect of the unilateral divorce law on the divorce rate as follows: the dummy variable "picks up a shift in the level of the divorce rate following the reform but leaves the subsequent downward trend following the initial post-reform spike in divorces to be picked up by state-specific trends" (Wolfers 2006, 1811). Thus, state trends can confound the results of a difference-in-differences analysis. This criticism, however, *applies to the use of a dummy without a post-law trend*. Since ADZ use a dummy and a post-law trend, the post-law trend presumably captures any subsequent trend due to the RTC laws.

In a study of divorce laws and family structure, Stéphane Mechoulan (2006) includes state-specific trends for the following reasons:

> These variables should capture several evolutions in society that are unrelated to divorce law changes (such as greater opportunities for women in the workplace) and that may have an impact on the outcomes of interest. In particular, they will take care of any convergence in the definition of marital property and property division, regardless of the laws as they appear in the books. (Mechoulan 2006, 156)

Thus, state-specific trends are included to control for otherwise unobserved slowly moving factors that could be correlated with the dependent variable and the policy variable(s). A cautious methodology would include all potentially relevant variables and dropping any only if found insignificant. Yet the individual states' trends are highly significant as a group (p<.0001) in all the ADZ regressions. State-specific trends are also very highly correlated with crime, the RTC dummy, and the post-law trend. In fact, regressing the RTC dummy and the post-law trend on the state trends yields F-tests that are significant at p<.0001. If the individual state trends are significant, estimates based on specifications with the trends left out would suffer from omitted variable bias. We conclude that individual state trends should be included in all models purporting to study the effects of RTC laws on crime.[7]

Number of trials and p-values

ADZ estimate their models many times over. In fact they estimate a grand total of 896 coefficient estimates on the RTC dummy and post-law trend. This allows them to say, for example, "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 confidence level" (2014, abs.). Looking only at those tables that report results with individual state trends, we find that ADZ

7. We estimated the ADZ model with "preferred" controls for all seven index crimes using both county and state data, adding an F-test for the state trends as a group. The F-tests were invariably significant with p<0.0001. All the programs, data, and log files may be downloaded (**link**).

reported 448 coefficient estimates, of which 47 are significant at the .05 level, as we show here in Table 2.[8]

TABLE 2. Number of significant coefficients

Table	Estimates	Murder	Rape	Assault	Robbery	Auto	Burglary	Larceny
4	28	0	0	2	0	0	0	0
5b	28	0	0	2	0	0	0	0
6b	28	0	0	0	0	0	0	0
7b	28	0	0	2	0	2	0	0
7d	28	0	0	2	0	1	0	0
8b	28	0	0	2	0	2	0	0
8d	28	0	0	2	0	3	0	0
9b	28	0	0	0	0	1	0	0
11b	28	0	2 (−)	0	0	0	0	1 (−)
12b	28	2 (−)	2 (−)	0	0	0	0	0
13b	28	0	0	0	0	2	0	2 (−)
B2	28	0	0	0	0	0	0	0
B4	28	0	0	2	0	0	0	2 (−)
C3	28	0	0	2	0	3	0	0
C4	28	0	0	0	0	3	0	0
D2	28	0	0	1	0	2	0	0
Total	448	2	4	17	0	19	0	5

Note: (−) indicates negative coefficients.

Breaking the ADZ results down by crime, we find that assault and auto theft account for 36 out of the 47 coefficient estimates that are significant at the .05 level. Robbery and burglary show no significant coefficients. Murder has two significant coefficients, both negative. Rape has four significant coefficients, all negative, and larceny has five significant coefficients, all negative. So, the most ADZ can claim is that there is some evidence that RTC laws increase assault and auto theft, but there is also some evidence that RTC laws reduce murder, rape, and larceny.

However, reporting the results of 448 trials increases the probability of false positives. There are well-known methods of correcting for such an effect, such as the Bonferroni adjustment. The 448 trials derive from seven crime equations, each estimated 64 times. The Bonferroni-corrected significance level would be .05/64=.0008, with a corresponding critical value of 3.16, an appropriately difficult hurdle to leap. But none of the coefficients in any of the regressions reported in these tables is significant at the .0008 level.[9]

8. The model with "ADZ preferred controls" is featured in ADZ tables 6, 8, 11, 12, 13, C1–C3, and D.

9. It could be argued that the tables in the appendices are merely robustness tests and therefore should

We conclude that none of ADZ's results from these regressions give reason to think RTC laws have had any effect on crime.

Misleading year-by-year estimates

ADZ report the results of analyzing dummy variables corresponding to years before and after the passage of the RTC law. Doing that has an advantage over dummy and spline variables in that it does not impose a structure on the behavior of the dependent variable after the passage of the law. The analysis output consists of graphs plotting the estimated coefficients. Unfortunately, ADZ did not see fit to report whether any of these estimated coefficients are significant. Nevertheless, ADZ conclude from the assault graph, for example: "The general story here seems to be that assault increases markedly over the time period after law passage, which squares with our results discussed in previous sections. One observes positive coefficient changes that are initially modest, but that increase dramatically and uniformly over the second half of the post-passage period" (2014, 72).

This is a bold conclusion. An interesting question would be: Are the coefficients on the ADZ dummies, on which these conclusions are based, significantly different from zero? Are they significantly different from the coefficient corresponding to the year of passage? Since ADZ do not report significance levels, we attempted to replicate their analysis from the information given in ADZ (2014).[10] We constructed eight yearly post-law year dummies. We also created eight pre-law year dummies plus a dummy for years 9 and earlier as an indicator of possible reverse causation from changes in crime rates and the passage of RTC laws. We used the full sample from 1977 and the ADZ model with "preferred" controls and state trends. The results are presented in Table 3.

There are seven significant coefficients in the seven crime equations reported in Table 3. There no significant post-law coefficients in the assault, robbery, auto theft, or burglary equations. F-tests on the difference between the post-law coefficients and the year-zero coefficient are all insignificantly different from zero except for murder (year 7, negative) and rape (year 5, negative). All of the F-tests for the difference between the pre-law coefficients and the year zero coefficient were insignificant except for auto theft (year 3 and year 8) indicating that there is no

not be counted as trials. If we ignore the last five rows of Table 2, the number of trials is 11*28/7=44. The Bonferroni significance level would be .05/44=.001, which yields a critical value of 3.10. None of the coefficients is significant at that level.

10. The programs used by ADZ to generate the graphs on which these conclusions were based are not included in the zip file provided to researchers by ADZ. We emailed Donohue to see if the programs were still available. Unfortunately, he was not able to provide the programs to us.

reverse causation from the crime rates to the passage of RTC laws, except perhaps auto theft. ADZ's bold conclusions concerning the behavior of assault after the passage of a RTC law, as well as their similar conclusions with respect to the other crimes, are unwarranted.

TABLE 3. Coefficients on post-year dummies

		Murder	Rape	Assault	Robbery	Auto theft	Burglary	Larceny
Year 0	Coeff.	0.041	−0.028	−0.054	0.008	−0.027	0.017	−0.021
	Adjusted	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	T-ratio	0.82	−0.71	−1.27	0.16	−0.49	0.55	−0.83
Year 1	Coeff.	0.030	−0.018	−0.056	0.025	0.002	0.022	−0.012
	Adjusted	−0.011	0.010	−0.002	0.017	0.029	0.005	0.009
	T-ratio	0.58	−0.42	−1.28	0.56	0.05	0.80	−0.50
Year 2	Coeff.	0.006	−0.030	−0.075	0.018	−0.007	0.022	−0.015
	Adjusted	−0.035	−0.002	−0.021	0.010	0.020	0.006	0.006
	T-ratio	0.12	−0.87	−1.75	0.40	−0.15	0.78	−0.60
Year 3	Coeff.	0.002	−0.042	−0.071	0.001	−0.006	0.004	−0.027
	Adjusted	−0.038	−0.014	−0.017	−0.007	0.021	−0.013	−0.006
	T-ratio	0.05	−1.19	−1.59	0.01	−0.14	0.14	−1.08
Year 4	Coeff.	−0.001	−0.073	−0.081	0.011	−0.006	0.004	−0.022
	Adjusted	−0.041	−0.045	−0.027	0.004	0.021	−0.012	−0.001
	T-ratio	−0.01	−2.14*	−1.69	0.28	−0.14	0.16	−0.92
Year 5	Coeff.	0.000	−0.082	−0.071	0.008	−0.007	0.005	−0.015
	Adjusted	−0.040	−0.054	−0.017	0.001	0.020	−0.011	0.005
	T-ratio	0.01	−3.00**	−1.53	0.23	−0.16	0.20	−0.72
Year 6	Coeff.	−0.017	−0.079	−0.075	0.004	0.002	0.023	−0.005
	Adjusted	−0.058	−0.051	−0.021	−0.003	0.028	0.006	0.016
	T-ratio	−0.43	−2.52*	−1.56	0.12	0.04	0.97	−0.30
Year 7	Coeff.	−0.067	−0.065	−0.058	0.025	0.018	0.020	0.023
	Adjusted	−0.108	−0.037	−0.004	0.017	0.045	0.004	0.044
	T-ratio	−2.19*	−2.30*	−1.55	0.85	0.53	1.36	1.55
Year 8	Coeff.	−0.002	−0.052	−0.039	0.018	−0.001	0.022	0.028
	Adjusted	−0.043	−0.024	0.015	0.010	0.026	0.006	0.049
	T-ratio	−0.07	−3.08**	−1.40	0.62	−0.02	1.59	2.02*

Notes: Adjusted equals the coefficient minus the coefficient for year zero. T-ratios are for the unadjusted coefficients. *p<.05; **p<.01.

Conclusions

The overall conclusion of our analysis of the ADZ model with "preferred" controls—but including state-specific trends and using all the data from 1977 to 2010—is that there is no evidence that the RTC laws have increased crime. That finding is a negative and agnostic one, and it is in line with what the NRC concluded in 2005.

Before closing, we relate our criticisms here to another recent NBER working paper by Donohue, Aneja, and Kyle Weber (2017). In that paper the authors re-estimate their model with a few more years of data, but now they omit individual state trends from all of their panel data models. As we showed above, such omission is not justified and changes the results. As of this writing, they have not made their dataset available for replication purposes.

Appendix

A zip file with all the data, Stata do-files, and results contained in this paper, plus a readme file with the necessary details for replication purposes, is available online (**link**).

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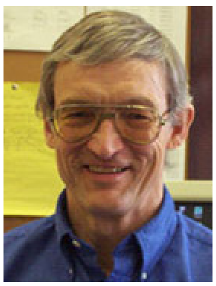

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