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More Guns, Less Crime Fails Again: The Latest Evidence from 1977 – 2006

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Abstract

Introduction

Moody and Marvell (MM) have now replied to our comment (Ayres and Donohue 2009) on their initial 2008 publication, "The Debate on Shall-Issue Laws." MM begin their latest effort—"The Debate on Shall-Issue Laws, Continued"—by declaring that they "are not advocates" of laws granting citizens the right to carry concealed handguns (RTC laws). In support of this claim, MM note that they have published papers finding that RTC laws both reduce crime (two papers) and do not reduce crime (three papers). Given the fact that neither MM (2008) nor their latest comment tries to reconcile the internal conflict in their own writings, let alone the larger literature, it is puzzling that a tweak on our 2003 paper would lead them to conclude their 2008 paper by stating, "In our judgment, the weight of the evidence … indicates that shall issue laws reduce crime." To the extent that they have now backed away from that claim, progress will have been made.

Nonetheless, MM claimed to find support for the crime-reduction hypothesis by extrapolating some trends estimated in Ayres and Donohue (2003). Indeed, MM criticized us for only evaluating the trend for five years, noting that 10 of the 24 states on which we generated our trend estimates contained data for at least six years. We would have thought, though, that one would want to be very cautious in extending trends beyond five years when 14 of the 24 states have no post-passage

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data beyond *three* years. Indeed, trying to draw inferences about what is happening to crime in 14 states five years after passage when one only has data for three years is the essence of extrapolating beyond the range of one's data, which is always a perilous exercise. We are thus puzzled by their latest assertion that perhaps we "did not believe the implications of [our] model, since [it] contained trend variables that continue indefinitely" (Moody and Marvell 2009). Of course, every linear model may be thought to continue indefinitely, but we certainly wouldn't encourage extrapolation beyond the range of the data over which the linear trend is estimated.

Moody and Marvell Misinterpret the Evidence from Donohue (2003)

MM's latest comment also states that while they overlooked Donohue (2003) in their 2008 literature review, they have now examined that paper and conclude that "Donohue's own coefficients indicate a considerable benefit of the shall-issue law" (Moody and Marvell 2009). Figure 1 in their current reply purports to graph violent crime coefficients from Table 8-5 in Donohue (2003), but there are two problems with this showing. First, they incorrectly graph the estimates from Donohue's table (which should appear as a step function based on a succession of two-year dummy variables rather than a linear graph). Second, and more importantly, MM misinterpret the estimates.[3]

The most useful information from the Table 8-5 estimates in Donohue (2003) was the comparison of the estimated effect from one or two years before passage to two to three years after passage. As that article made clear, this comparison showed *no* evidence of a decline for violent crime, or any crime category for that matter. (Indeed, the only evidence of any impact was in the direction of *increasing* crime in all four property crime categories.) As Donohue (2003) explained, looking at estimates beyond two to three post-passage years was unwise since, after that point, one would be comparing the pre-passage dummy for 24 states that were to adopt the RTC law with a post-passage estimate for only 10 of those 24 states. Thus, MM's Figure 1 fails the old apples-and-oranges test: the post-passage estimates on the right hand side of that figure are *not* comparable to the pre-passage figures on the left hand side of the figure. This point is so basic that one might have thought that it would only need to be made once (back in 2003), but apparently it needs repeating.

3 MM seek support for their position with a quote from David Mustard, but he, unfortunately, made the same mistake they make. Mustard (2003: 329).

New State Panel Data Evidence from 1977 – 2006[4]

MM then go on to tinker with their 1977-2000 county data in an effort to show that they can generate some results (using defective county data, including the highly flawed Florida data) that, in aggregate, will support their conclusion that the overall effect of RTC laws is a net reduction in crime.[5] But time has now passed them by, as we tried to show in our previous graphs of the very different trends in crime *after* 2000. MM take refuge in saying that our graphical depictions don't control for other factors influencing crime. True enough. But we thought that our graphs would have made them cautious about making claims based on their incomplete data that the patterns of crime in the more recent data were more favorable in states that did *not* have RTC laws than in states that did.

In any event, to address their concerns about the lack of controls, we extended our *state* panel data six additional years beyond that used by MM (who used *county* data from 1977-2000). Again, we believe that the advantages of county data (in enabling controls for county fixed effects) are outweighed by the poorer quality of county data. MM emphasized three econometric features in their estimations that we will follow in our analysis below. First, MM control for state trends. Second, they cluster the standard errors. And third, they emphasize the hybrid specification, which allows for both a shift (upward or downward) in crime as well as a change in the trend of crime (the slope) following the adoption of an RTC law.[6] MM note that, unless the shift variable is very large relative to the trend variable, in the end the trend variable will indicate the long-run effect of the law (assuming the specification is working well). All three of these features have become fairly standard in state panel data analyses, although the inclusion of state trends and clustering may be disputed by some.

Again, as we illustrate below, the evidence is most supportive of the claim that RTC laws *increase* aggravated assault. But the original Lott and Mustard (1997) specification suggests that RTC laws also increase rape, while the MM (2009) specification also suggests on its face that RTC laws reduce auto theft. But when we subject these specifications to a plausible robustness check, we see that while the evidence of an increase in aggravated assault is strengthened, the evidence concerning the effect of RTC laws on rape and auto theft become murkier still. The specific

4 Although our newly created state data set contains crime data from 1977 to 2007, two of the explanatory variables—arrest rate and police rate—are only available up to 2006. Thus, all of our specifications can only be run on data up to 2006.

5 Our previous comment on MM (2008) pointed out a number of problems with the county data. Note, too, that the FBI's Uniform Crime Reports have now withdrawn the 1993 county crime data used by MM, as it has been recognized as faulty. According to the National Archive of Criminal Justice Data (NACDJ), the FBI's 1993 county (but not state) crime estimates were imputed incorrectly and so were made unavailable to the general public. The NACJD anticipates being able to re-release updated county crime data for 1993 later this year. We also discuss below a problem with the MM incarceration data for 2000.

6 In addition to the hybrid model, we also generate estimates using the dummy variable and spline (or trend) models that we presented in our earlier comment.

explanatory variables used in these three specifications are shown in Table 1. In all cases, the dependent variable is the natural log of the crime rate (for each of seven Index I crime categories).

A. Lott and Mustard Specification

In responding to the implicit invitation by MM that we add controls to our added years of crime data, we of course have to make some choices about the appropriate explanatory variables. Since MM cited the original Lott and Mustard study as providing evidence in support of the proposition that RTC laws reduce crime, one assumes that they would be interested to know whether the Lott and Mustard explanatory variables would still provide support for the "more guns, less crime" hypothesis when estimated over the 1977-2006 period.[7] The Lott and Mustard specification is also important because the National Research Council (2005) report on RTC laws relied on this specification (run on county data) for the period 1977-2000.

The Lott and Mustard explanatory variables are depicted in the first column of Table 1 exactly as they were in the 1997 paper (and the National Research Council report), except for the three MM features described above and one that we added concerning the arrest rate variable that both Lott and Mustard and MM employ. While we are not enthusiastic about this pseudo-arrest rate variable (for reasons previously discussed on p. 52 in our initial reply and elsewhere), we believe it is particularly inappropriate as an econometric matter to include the arrest rate in its *contemporaneous* form, as both pairs of authors have done. To understand this problem, consider their murder regression. In using the contemporaneous arrest rate, Lott and Mustard and MM are essentially explaining murders (the left-hand side variable) with a ratio as a right-hand side variable that contains murders (in the denominator). To avoid this severe endogeneity problem, one should at least lag the arrest rate by one year—which we do below in every regression that uses that variable.

Table 2 shows the results of our estimates of the impact of RTC laws using the Lott and Mustard variables on the extended state panel data set. Looking at the hybrid model, one sees that for six of the seven crime categories, the table suggests that RTC laws *increase* crime, although only one of these seven—aggravated assault—is statistically significant at the .01 level. The only contrary evidence is in the property crime category, where on its face Table 2 would suggest that RTC laws are associated with a decrease in auto theft, albeit at a lower level of significance (the .05 level). The fact that six of the seven crime categories are rising at least raises the specter that the one contrary estimate for auto theft is spurious. But even if one accepts the facial findings of substantially higher aggravated assaults and somewhat lower auto thefts, this would be a bad tradeoff since aggravated assault is much more socially costly

7 Lott and Mustard (1997) analyzed crime data for U.S. counties from 1977 to 1992 and concluded that allowing citizens to carry concealed weapons deters crime.

than auto theft.[8] In any event, this is not exactly a ringing endorsement of the more guns, less crime hypothesis.

Table 1: Three Different Sets of Explanatory Variables Used to Estimate the Impact of RTC Laws[9, 10, 11, 12, 13]

Lott-Mustard (1997) & NRC (2005)	Moody-Marvell (2009)	Ayres-Donohue (2009) Robustness Check
RTC law dummy (see Appendix 1)	RTC law dummy	RTC law dummy
Post-passage RTC law trend	Post-passage RTC law trend	Post-passage RTC law trend
Lagged arrest rates[1]	Lagged arrest rates	Lagged police rate[2]
4 real per capita income measures[3]	4 real per capita income measures	4 real per capita income measures
State population	State population	
Population density[4]		Population density
Year dummies	Year dummies	Year dummies
With state trends	With state trends	With state trends
Percent pop white males 10-19		
Percent pop white males 20-29	Lagged incarceration rate[5]	Lagged incarceration rate
Percent pop white males 30-39	Unemployment rate	Unemployment rate
Percent pop white males 40-49	Poverty rate	Poverty rate
Percent pop white males 50-64	Percent pop black	
Percent pop white males 65+	Percent pop ages 10-19	Percent pop black males 10-19

8 Donohue (2009) indicates that aggravated assault is both more common and, on average, more socially costly than auto theft. On the relative frequency of the two crimes, see the first column of Table D1 in Donohue (2009). On the relative cost, note that low-end estimates of the cost per crime are $19,500 for assault and $1,200 for auto theft (Table 4). The respective high-end estimates are $91,800 and $5,700 (Table 5).

9 Lott-Mustard and NRC lagged arrest rates: The arrest rate variable used in the Lott and Mustard and MM specifications for the four violent crimes was calculated as the number of total arrests for violent crimes divided by the total number of violent crimes. For the three property crimes, the analogous overall property crime arrest rate is used.

10 Ayres-Donohue lagged police rate: The police rate was obtained from local-level police protection figures from the Census Bureau. According to the U.S. Bureau of Justice Statistics (BJS), "police protection" refers to officers who have "the function of enforcing the law, and preserving order and traffic safety and apprehending those who violate the law, whether these activities are performed by a police department, a sheriff's department, or a special police force" (Bureau of Justice Statistics 2008). Police rate was calculated as total police protection in a state per 100,000 population.

11 Lott-Mustard and NRC 4 real per capita income mesaures: The four individual real per capita income measures are personal income (defined in Appendix 4 below), unemployment benefits, income maintenance, and retirement payments.

12 Lott-Mustard and NRC population density was calculated by dividing a state's total population by the state's area per square mile.

13 MM lagged incarceration rate density was calculated by dividing a state's total population by the state's area per square mile.

Percent pop white females 10-19	Percent pop ages 20-29	Percent pop black males 20-29
Percent pop white females 20-29	Percent pop ages 30-39	Percent pop black males 30-39
Percent pop white females 30-39	Percent pop ages 40-49	Percent pop white males 10-19
Percent pop white females 40-49	Percent pop ages 50-64	Percent pop white males 20-29
Percent pop white females 50-64	Percent pop ages 65+	Percent pop white males 30-39
Percent pop white females 65+	Lagged dependent variable	
The above 12 demographic controls are repeated for blacks and "other" races (yielding a total of 36 demographic controls)	Crack index	

But there is more. The lack of any evidence in Table 2 of a decline in murder rates associated with the adoption of RTC laws is particularly significant because of the prominence given to this precise issue in the 2005 National Research Council (NRC) report. In the final report, 15 of the 16 members of the Committee were not persuaded by the claims that RTC laws reduced crime. James Q. Wilson, however, issued a dissent in which he opined, "I find that the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate, though their effect on other crimes is ambiguous" (NRC 2005, 271). In particular, Wilson stated that "it is hard for me to understand" why Lott's claims that RTC "laws drive down the murder rate … are called 'fragile'" (270).

Table 2 suggests that the NRC majority got it right: Lott's claimed murder effect disappears. This is the essence of fragility. The only difference between the regressions that persuaded Wilson that RTC laws reduce murder and the Table 2 regressions that undermine this view are: (1) Wilson was looking at county data for 1977-2000, while we present state data for 1977-2006; (2) we use the three MM features (state trends, clustering, hybrid model), while Wilson looked only at models without these features; and (3) we lagged the arrest rate to avoid the endogeneity problem from using contemporaneous arrest rates.

Despite the conflict between the NRC panel (which decided not to cluster the standard errors) and MM, who advocate clustering (thereby adhering to much current econometric practice—albeit on a question about which the literature has yet to achieve closure), this issue is irrelevant to the Table 2 finding of no support for the claim that RTC laws reduce murder. Whether one clusters the standard errors or not, the Table 2 coefficients do not support Wilson's conclusion. In fact, without clustering, Table 2 would indicate that not only do RTC laws *increase* aggravated assault (again at the .01 level of significance), but that they also *increase* murder, rape, robbery, and burglary (albeit at the .10 level).[14] Since Lott opposes clustering and Table 2 presents the Lott and Mustard model, his approach now supports a broad conclusion that more guns generate *more* crime.

Were one inclined to follow Wilson's approach and draw strong inferences from

14 Lott (2004) concludes that "clustering by state is inappropriate and biases the results against finding statistically significant changes in crime rates" (19-20).

one idiosyncratic set of regressions, one would presumably endorse the view that RTC laws increase aggravated assault and do not reduce murders, as shown in Table 2 (while decreasing auto theft). Yet how much confidence can we really have in the Lott and Mustard specification given that, in addition to its other infirmities, it does not even include a variable known to be a powerful factor in reducing crime—the incarceration rate (Marvell and Moody 1994; Levitt 1996)? If the incarceration rate is correlated with the presence of RTC laws, then the resulting estimates on the impact of these laws would be marred by omitted variable bias. Since this was the precise specification on which Wilson relied, one again sees how unguarded he was in reaching his dissenting opinion based on a regression with no control for perhaps the most important influence on crime over the last three decades.

Table 2: The Estimated Impact of RTC Laws, All Crimes, 1977-2006 State Data, Controlling for State Trends, Lott and Mustard Explanatory Variables, Lagged Arrest Rates, With Clustering[15]

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.63%	-2.49%	-1.14%	-2.48%	2.98%	-1.96%	1.22%
	3.02%	2.24%	2.71%	2.59%	2.51%	1.30%	1.04%
2. Spline model:	0.79%	0.44%	**2.48%**	0.65%	**-1.55%**	0.56%	0.36%
	0.86%	0.79%	**0.64%**	1.09%	**0.72%**	0.79%	0.62%
3. Hybrid model:							
Postpassage dummy	-3.38%	-2.95%	-3.20%	-3.10%	4.38%	-2.50%	0.98%
	3.17%	2.47%	2.76%	2.66%	2.66%	1.59%	1.23%
Trend effect	0.96%	0.59%	**2.64%**	0.80%	**-1.78%**	0.69%	0.31%
	0.87%	0.84%	**0.66%**	1.10%	**0.77%**	0.83%	0.65%

B. Moody and Marvell Specification

In their latest reply, MM also provide their own set of explanatory variables—some based on their own initial choices and some responding to our previous suggestions. The second column of our Table 1 above displays the MM explanatory variables, but with the lagged (instead of contemporaneous) arrest rate. Note that MM include incarceration rate as an explanatory variable. In at least this respect, the MM specification would appear to have a clear advantage over the Lott and Mustard specification, which had no control for incarceration.

Table 3 provides aggregate estimates of the effects of RTC laws based on the

15 For Table 2 as well as all subsequent tables, statistical significance is denoted as follows: estimates significant at the .10 level are underlined, estimates significant at the .05 level are bolded, and estimates significant at the .01 level are bolded and underlined. In addition, all tables presented in this paper display estimates for which robust standard errors are clustered by state.

MM specification, again employing state data over the years 1977-2006.[16] The Table 3 results are similar to those in Table 2, although all of the estimated effects shrink noticeably. The table shows statistically significant *increases* in rape (at the .05 level of significance) as well as aggravated assault (at the .01 level). All four violent crime categories are consistent with *increasing* crime rates, although neither the murder increase nor the smaller estimated increase in robbery is statistically significant.

Table 3: The Estimated Impact of RTC Laws, All Crimes, 1977-2006 State Data, Controlling for State Trends and Extending Crack, Moody and Marvell Explanatory Variables (with the exception of lagging the arrest rates), With Clustering

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	0.40%	-0.92%	1.02%	0.96%	1.36%	-0.20%	0.83%
	1.73%	1.27%	1.06%	1.41%	1.38%	1.06%	0.78%
2. Spline model:	0.34%	**0.70%**	**<u>0.98%</u>**	0.25%	-0.06%	0.08%	-0.05%
	0.60%	**0.34%**	**<u>0.37%</u>**	0.41%	0.35%	0.28%	0.21%
3. Hybrid model:							
Post-passage dummy	0.18%	-1.43%	0.34%	0.81%	1.45%	-0.26%	0.89%
	1.86%	1.32%	1.19%	1.37%	1.38%	1.02%	0.76%
Trend effect	0.33%	**0.76%**	**0.97%**	0.22%	-0.12%	0.09%	-0.09%
	0.62%	**0.35%**	**0.39%**	0.41%	0.34%	0.28%	0.21%

Once again, we should note that the MM specification undercuts Wilson's claimed murder suppression effect even more thoroughly than the Lott and Mustard specification did. On its face, it would appear that RTC laws are increasing rape and aggravated assault, but again we must ask: are these regression models working well? Appendix 2 tries to provide some information on that question by exploring the estimated effects that are generated for the other important variables of interest.

Table A1 in Appendix 2 begins by examining the estimated effect of the incarceration rate on crime for the MM specification. For four of the seven crime categories, this table suggests that increases in incarceration have essentially no effect on crime. This seems implausible and is dramatically inconsistent with a large literature (Donohue 2009), including an earlier paper by Marvell and Moody (1994).

16 The state-level crack measure—developed in Fryer et al. (2005) and used by MM—only goes up to 2000. Since the criminogenic influence of crack had largely abated by 2000 it is probably reasonable to extend the crack variable simply by using 2000 values for 2001 to 2006, which is what we depict in Table 3. It turns out that the crack variable makes little difference to the results from the MM regressions, whether one includes it or drops it for either 1977-2000 or 1977-2006 state-level data.

One has to ask: if the MM specification does a poor job of predicting the impact of incarceration on crime, should we believe that this specification can reliably estimate the impact of RTC laws on crime?

Further concerns about whether the MM specification is working well are provided in Appendix 2's Table A3, which shows the estimated effect of crack on crime. This table shows that MM's crack variable has no statistically significant effect in raising the murder rate, which is contrary to the almost universal opinion that the rise of the crack cocaine phenomenon had a powerful effect in increasing murders in the United States in the late 1980s. While this may simply reflect the fact that it is a tricky proposition to capture the criminogenic influence of crime in a statistical measure, it likely means that crack's large impact on murder is not adequately captured in the included crack variable. This implies that *all* regression estimates of the impact of RTC laws on crime are potentially marred by severe omitted-variable bias, especially since we know that the states that had the largest crack problems in the early days of the epidemic were states such as New York and California, which did *not* adopt RTC laws. Thus, what some of the early papers deemed to be a benefit of passing RTC laws in reducing murder may well be the effect of crack's harmful impact on murder in states that chose not to adopt RTC laws. Of course, the big question—yet to be resolved—is whether the apparent null effect of RTC laws on murders would be suggestive of crime *increases* if the regression models could correctly control for the impact of crack on murder.

C. A Robustness Check

While one might feel comfortable in concluding at this point that RTC laws *increase* aggravated assaults—and perhaps rape, if one prefers the MM specification to that of Lott and Mustard—we have seen enough fragility in the panel data estimates of the effect of RTC laws to be cautious about leaping to strong conclusions. Remember that all of our findings are based on statewide, aggregate hybrid models, estimated on state data, including state trends and clustering the standard errors. All of these points seem reasonable to us (and MM specifically endorse all but the *aggregate* estimates on *state* data), but it is probably useful to bear in mind that all of these choices have generated at least some contention in the published literature. Moreover, we ourselves have criticized elements of the Lott and Mustard and MM models, owing to (1) their reliance on questionable arrest rate data; (2) their failure to include highly important variables, such as the incarceration rate (in the case of Lott and Mustard) and a measure of police presence (which neither Lott and Mustard nor MM include); and (3) Lott and Mustard's reliance on overly numerous demographic variables that are likely flawed by measurement error (which MM have now abandoned in response

to our previous critique).[17]

To at least provide a check on how some variations in the explanatory variables might influence the results in Tables 2 and 3, we offer an alternative set of explanatory variables (while still retaining the core choices of aggregate, hybrid estimates on state data, including state trends and clustering the standard errors). Column 3 of Table 1 lists these explanatory variables, which contain various plausible modifications and additions to the previously discussed Lott and Mustard and MM specifications. These variables were chosen based on *a priori* consideration of potential weaknesses of the models depicted in Tables 2 and 3 and were not altered after seeing the regression results (although we do discuss some further permutations below, such as adding in the crack variable). Appendix 3 provides summary statistics for these variables.

The primary changes we employed in this robustness check were to alter the MM specification by dropping (1) the lagged dependent variable, (2) the crack variable (although this variable has little effect on the results whether included or not), (3) the conceptually flawed arrest rate variable, and (4) the state population control, which we believe is superfluous given the fact that we already control for population density (and weight the regression by population). As in Table 3, we use a control for the state incarceration rate.[18] We also introduce a variable that neither Lott and Mustard nor MM employed: the number of police per 100,000 population. Furthermore, as illustrated in Table 1, we control for a more limited number of demographic categories: black males 10-39 and white males 10-39 (a total of six demographic controls versus 36 in the Lott and Mustard specification and seven in the latest MM response). Finally, as in all of the regressions we report in this reply, we again cluster the robust standard errors and control for state trends.

Where does this robustness check leave us? On the positive side, in terms of being able to draw firm conclusions, RTC laws seem to be associated with substantial and statistically significant *increases* in aggravated assault across all three sets of models (with the Lott and Mustard estimates and our robustness check model yielding estimates that are both significant at the .01 level). Note that the Table 4 estimate of the increase in aggravated assault is quite large—roughly three times the size of

17 For example, the Lott and Mustard demographic variables identifying "other" races show abrupt upward jumps in 2000, likely owing to changing Census treatment of Hispanic residents.

18 Initially, we used incarceration rate data provided by the Sourcebook of Criminal Justice Statistics, but then realized that this rate did not include prisoners sentenced to less than one year. On the other hand, prison population data provided by BJS includes *all* prisoners. Formally, BJS defines prison population to include "individuals under the jurisdiction of state and federal correctional authorities" (West and Sabol 2009). Thus, we follow MM (2008, 2009) in using this prison population data (although we still call this variable "incarceration rate," which is calculated as total prisoners per 100,000 population by state). However, we did find that MM's prison population data for 2000 falls short of the actual year-end numbers provided by BJS, which leads us to suspect that their data was based on a less-than-full year tally that was never updated.

the Table 3 estimate, but only somewhat larger than the Table 2 estimate. Under any of the three estimates, RTC laws would have led to tens of thousands of additional aggravated assaults. Increased levels of assault could result from increased criminal assaults by RTC permit holders, from those who steal or find the less protected guns of permit holders, and from criminals who increase their gun-carrying or their aggressiveness in response to RTC laws.

Unfortunately, from the standpoint of gaining closure on the ultimate impact of RTC laws, the results on two of the other crimes once again prove to be more sensitive than one would like. While Table 3 had suggested that RTC laws lead to a not insubstantial *increase* in rape (statistically significant at the .05 level), Table 4 suggests a modest opposite effect (albeit at only the .10 level). Table 2 suggests, though, that RTC laws had no impact on rape. Indeed, if one adds crack as a control in Table 4, the rape results become smaller and statistically insignificant.

Table 4: The Estimated Impact of RTC Laws, All Crimes, 1977-2006 State Data, Controlling for State Trends, Ayres and Donohue Explanatory Variables, With Clustering

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	0.54%	-3.61%	-2.03%	2.40%	8.17%	1.51%	1.89%
	2.72%	1.83%	3.05%	3.67%	4.16%	2.18%	1.83%
2. Spline model:	0.83%	0.08%	**3.10%**	0.51%	**-1.84%**	-0.22%	-0.15%
	0.87%	0.79%	**0.81%**	1.29%	**0.82%**	0.88%	0.74%
3. Hybrid model:							
Post-passage dummy	0.11%	-3.70%	-3.68%	2.17%	**9.26%**	1.65%	1.99%
	2.86%	1.96%	3.15%	3.96%	**4.24%**	2.41%	1.97%
Trend effect	0.83%	0.19%	**3.21%**	0.44%	**-2.11%**	-0.27%	-0.20%
	0.89%	0.79%	**0.82%**	1.35%	**0.84%**	0.91%	0.77%

Moreover, the estimated impact of RTC laws on auto theft is fractured across the three tables, with Table 2 suggesting decreases, Table 3 suggesting no effect, and Table 4 showing mixed results with large initial crime increases being offset after five years by rising crime. Note that if one prefers the first two models in Table 4 to the hybrid model, one also sees conflicting results as the dummy model (row 1) shows large auto theft *increases*, and the spline model (row 2) shows large auto theft *decreases*.

Again, we need to ask whether the Ayres-Donohue (AD) robustness check specification is working well. If the RTC law would have any beneficial impact, it would likely reduce robbery, which is the crime most often committed in public. Yet in all three tables, RTC laws are associated with higher rates of robbery (although the results are statistically insignificant). It is very hard to come up with a plausible explanation for why auto theft would fall if robbery doesn't fall, since robbery always

involves confrontation with a victim (who, if armed, could threaten the robber), while auto theft rarely involves such a confrontation, thereby reducing any possible risk to the thief. Therefore, the strongly conflicting dummy and trend effects in Table 4 for auto theft are probably more suggestive of specification error than a true pattern of changing crime.

Appendix 2 provides some insight into the various regressions by examining the resulting estimates of RTC laws on other explanatory variables across Tables 2, 3, and 4. Note that the AD specification does generate more plausible estimates of the impact of incarceration on crime than the MM specification does (compare Appendix 2's Table A2 with Table A1). Nonetheless, the Table A2 values from this appendix might also suggest problems in the operation of the AD regressions for estimating the impact of incarceration (incarceration has no effect on assault and an unusually small, albeit negative and statistically significant, effect on rape). The inevitable question is whether poor estimates of the impact of incarceration on rape and aggravated assault provide a basis to question the Table 4 estimated effects of RTC laws on these two crimes. Finally, Appendix 2's Table A4, which illustrates the estimated impact of police on crime, shows the anomalous result that police *increase* the level of (measured) aggravated assault. This endogeneity problem is not uncommon in estimating the impact of police on crime, and one must ask whether the Table 4 regressions are marred by this endogeneity bias or whether the Table 2 and Table 3 regressions are marred by omitted-variable bias in failing to control for police presence in a state.

Conclusion

What is to be made of the aggregated evidence concerning the impact of RTC laws when one extends the state panel data through 2006? The one consistent finding that is statistically significant for the hybrid model in Tables 2, 3, and 4 is that RTC laws increase aggravated assault. The point estimates across all three tables are generally consistent with higher rates of murder and robbery, although these estimates are not statistically significant. In general, one might assume that the biases from inadequate controls for crack and general measurement error would tend to bias those results to zero, so it may be the case that better information and models would reveal that RTC laws increase murder and robbery as well as aggravated assault. The mixed evidence on rape and auto theft leaves little basis for conclusion with respect to these crimes.

Our Appendix 2 provides evidence concerning both the importance of the incarceration rate and the reliability of the various models. It does so by documenting the MM and AD estimates of the effect of incarceration rate on crime across all seven Index I crimes. As expected, the incarceration rate is seen to statistically significantly reduce crime, but this finding is far stronger in the AD specification than in the MM model. This raises the question: if the MM model cannot correctly predict the impact of incarceration on crime, why should we expect it to reliably tell us the impact of

RTC laws on crime?[19]

The clustering debate to which we have alluded provides one more indication that important econometric issues lurk in the background of many evaluation studies. Other issues in the RTC literature that can have important effects on the estimated effects are whether state or county data is preferable (we have come to prefer state data, as have most crime researchers, although Lott and Mustard and MM used county data) and whether state fixed effects should be included (as we and MM did, but the original Lott and Mustard paper did not—see generally on this point, Wolfers 2006). Using these different approaches would broaden the array of estimates for many crimes.

We also note that Jeff Strnad (2007) has recently advocated in the context of the RTC debate that Bayesian econometric approaches may provide a better way to identify the appropriate array of explanatory variables. In this regard, note that Strnad found that the two most important explanatory variables for identifying the impact of crime in his data were the abortion rate (see Donohue and Levitt 2001) and the incarceration rate.[20] If the binding limitation in these models is not the selection of the appropriate set of controls, but rather the need to correct for, say, endogenous state adoption of RTC laws, then the Bayesian models that Strnad presents will need to be further complicated.

Finally, it is always wise to look behind the regression results and explore whether any actions by the police and government officials that may be correlated with the presence of RTC laws have had substantial impacts on *reported* crime (as opposed to actual criminal conduct). States that are more sensitive in their handling of rape cases, for example, may increase the proportion of victims willing to come forward to press charges, which can lead to greater reporting of rapes and hence impart an upward bias in the trend in rape. Similarly, aggravated assaults are influenced by laws or practices that encourage the police to use "arrest" more often in the context of complaints of domestic violence. (Could this explain the finding above that more police in a state leads to more instances of aggravated assault?) If

19 The finding in Table A2 of Appendix 2 that incarceration in the AD specification has virtually no impact on aggravated assault also raises a number of intriguing questions. It might indicate that incarceration tends to dampen every Index I crime other than aggravated assault, which is unaffected by large increases in the incarceration rate of the type experienced in many states over the last few decades. Of the four other published estimates of this elasticity, none is statistically significant, one is zero (the MM 2009 figure that we report in Table A1 of Appendix 2 in this paper), one is positive (Johnson and Raphael 2006), and two have negative point estimates: -.056 (.053) from Marvell and Moody (1994), and -.410 (.249) from Levitt (1996). Alternatively, our Table A2 estimate of no effect of incarceration on assault could suggest that the AD specification-check model is not working well to explain aggravated assault. If this latter interpretation were correct, it would also indict the MM estimates for four of the seven crime categories in our Table A1 of Appendix 2, which all show no effect of incarceration. Even in the other three crime categories that do show an effect, the MM estimates on incarceration seem to be biased downward (again raising questions about the MM framework).

20 One would also have to explore whether these variables should enter in ln forms or with quadratic terms, which again would expand the array of permutations.

RTC laws are adopted in states that pursue these policies more (or less) aggressively, then the estimated effects of RTC laws can be biased.

In sum, while the best evidence to date suggests that RTC laws at the very least *increase* aggravated assault, this comment illustrates that it is not an easy task to tease out the net effects of RTC laws on crime via panel data analyses. Perhaps if the states that were influenced by the National Rifle Association's efforts to advance RTC laws had agreed both to randomly adopt the laws and to allow data to gather during an evaluation period of appropriate length, we would today have far more precise estimates of the impact of RTC laws on crime. Such knowledge would likely put us in a better position to address the distressingly high violent crime rates that, along with our singular reliance on the death penalty and our enormous number of prison inmates and guns, mark the U.S. as unique among Western democracies.

Appendix 1: Year of Adoption of RTC Laws in 39 States and Philadelphia[21, 22]

Alabama – pre-1970s
Alaska – 1994
Arizona – 1994
Arkansas – 1995
Colorado – 2003
Connecticut – 1969
Florida – 1987
Georgia – 1989
Idaho – 1990
Indiana – 1980
Kansas – 2006
Kentucky – 1996
Louisiana – 1996
Maine – 1985
Michigan – 2001
Minnesota – 2003
Mississippi – 1990
Missouri – 2003
Montana – 1991
Nebraska – 2006
Nevada – 1995
New Hampshire – 1959
New Mexico – 2003
North Carolina – 1995
North Dakota – pre-1970s
Ohio – 2004
Oklahoma – 1995
Oregon – 1990
Pennsylvania – 1989
Philadelphia – 1995
South Carolina – 1996
South Dakota – pre-1970s
Tennessee – 1994
Texas – 1995
Utah – 1995
Vermont – pre-1970s
Virginia – 1988
Washington – 1961
West Virginia – 1989
Wyoming – 1994

21 Moody and Marvell code Idaho's first full year as 1992, but Ayres and Donohue code the first full year as 1991. The official date of passage, according to Idaho's attorney general, is July 1, 1990.

22 There is some uncertainty as to when Indiana genuinely became a "shall-issue" state. In the end—after some internal debate—we adopted 1980 as the year in which the critical shift

Appendix 2: Estimated Effects of Incarceration and Other Explanatory Variables

Tables A1 and A2 present the estimated coefficients and significance of the incarceration rate variable from the Moody-Marvell (MM) and Ayres-Donohue (AD) specifications (this variable is not included in the Lott and Mustard specifications). As expected, all of the significant coefficients suggest that higher incarceration rates decrease crime. But note that the MM specifications essentially show no effect of incarceration for four of the seven crime categories, which may be suggestive of problems in the MM models.

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occurred. Indiana enacted a concealed-carry regulation as early as 1935, when the state's Uniform Firearms Act mandated that no person carry a pistol outside his or her "place of abode or fixed place of business" without a license. Studies that place Indiana's adoption at a year prior to 1977 would seem to rely on the 1935 Act. Two decades later, a state court upheld the constitutionality of the Act and its licensing procedure in Matthews v. State, 237 Ind. 677, 148 N.E.2d 334 (1958). Yet that court also held that the state's police superintendent, rather than the legislature, "is capable and qualified to determine whether an applicant for a license to carry a pistol has a 'proper reason' therefore, and whether he is a 'suitable person' to have a pistol in his possession at will"—a delegation of administrative authority indicative of a "may-issue" rather than a "shall-issue" policy.

In Schubert v. DeBard, 398 N.E.2d 1339 (Ind. App. 1980), however, an Indiana appeals court ruled that the police superintendent could not use discretion to deny a license to people asserting a "need" to carry a concealed weapon for "self-defense." (The statute in question, IC 35-23-4.1-5, stated that that the superintendent "shall issue" a license if "it appears to the superintendent that the applicant has a proper reason for carrying a handgun and is of good character and reputation and a proper person to be so licensed.") As Gregg Lee Carter (2002) has observed, the Schubert court thus made a concealed-carry permit in Indiana subject to "precise, open, and accessible licensing" (522-23). The same Indiana appeals court also reaffirmed the holding in Schubert the following year. In Shettle v. Shearer, 425 N.E.2d 739 (Ind. App. 1981), the court ruled that the superintendent could not investigate an applicant's assertion of needing a concealed-carry permit for self-defense, and instead would simply have to accept the claim. In short, it appears that Indiana did not genuinely become a shall-issue state at least until the Schubert decision in 1980 (or, arguably, not until 1981). We concur with the assessment in Grossman and Lee (2008), as well as the jurisprudential analysis in Carter (2002), that 1980 marked the key moment of change in Indiana's concealed-carry policy. Note, however, that the regressions in our initial reply to MM—Ayres and Donohue (2009)—employed the same coding as Moody and Marvell (2008), which treated Indiana as adopting before 1977.

Table A1: Moody-Marvell Specification: Estimated Coefficient on Incarceration Rate							
	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-0.07%**	0.00%	-0.01%	**-0.02%**	**-0.02%**	-0.01%	0.00%
	0.02%	0.01%	0.01%	**0.01%**	**0.01%**	0.01%	0.00%
2. Spline model:	**-0.07%**	0.00%	-0.01%	-0.02%	**-0.02%**	-0.01%	0.00%
	0.02%	0.00%	0.01%	0.01%	**0.01%**	0.01%	0.00%
3. Hybrid model:	**-0.07%**	0.00%	-0.01%	**-0.02%**	**-0.02%**	-0.01%	0.00%
	0.02%	0.01%	0.01%	**0.01%**	**0.01%**	0.01%	0.00%

Table A2: Ayres-Donohue Specification: Estimated Coefficient on Incarceration Rate							
	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-0.07%**	**-0.03%**	0.00%	**-0.09%**	**-0.12%**	**-0.07%**	**-0.05%**
	0.02%	**0.01%**	0.02%	**0.02%**	**0.03%**	**0.02%**	**0.01%**
2. Spline model:	**-0.07%**	**-0.04%**	-0.01%	**-0.09%**	**-0.11%**	**-0.07%**	**-0.05%**
	0.02%	**0.01%**	0.02%	**0.02%**	**0.03%**	**0.02%**	**0.01%**
3. Hybrid model:	**-0.07%**	**-0.03%**	0.00%	**-0.09%**	**-0.11%**	**-0.07%**	**-0.05%**
	0.02%	**0.01%**	0.02%	**0.02%**	**0.03%**	**0.02%**	**0.01%**

Table A3 presents the estimated coefficients and significance of the crack variables from the MM specification. Note that while crack is widely believed to have caused murder rates to rise in the late 1980s, this table suggests that crack had no effect on murder.

Table A3: Moody-Marvell Specification: Estimated Coefficient on Crack							
	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	1.38%	-0.65%	0.52%	**1.01%**	**2.45%**	0.71%	**0.61%**
	0.94%	0.34%	0.58%	**0.44%**	**0.58%**	0.37%	**0.21%**
2. Spline model:	1.43%	-0.57%	0.69%	**1.06%**	**2.47%**	0.71%	**0.62%**
	0.94%	0.33%	0.55%	**0.43%**	**0.60%**	0.37%	**0.20%**
3. Hybrid model:	1.43%	-0.52%	0.68%	**1.04%**	**2.44%**	0.72%	**0.59%**
	0.93%	0.34%	0.56%	**0.45%**	**0.59%**	0.37%	**0.21%**

Table A4 presents the estimated coefficients and significance of the police rate variables from the AD specification.

Table A4: Ayres-Donohue Specification: Estimated Coefficient on Police Rate							
	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.05%	0.04%	0.06%	-0.01%	-0.01%	-0.01%	0.02%
	0.05%	0.03%	0.03%	0.05%	0.06%	0.04%	0.03%
2. Spline model:	-0.04%	0.04%	**0.08%**	0.00%	-0.02%	-0.01%	0.02%
	0.05%	0.03%	**0.03%**	0.05%	0.06%	0.04%	0.03%
3. Hybrid model:	-0.04%	0.04%	**0.09%**	-0.01%	-0.03%	-0.01%	0.02%
	0.05%	0.03%	**0.03%**	0.05%	0.06%	0.04%	0.03%

The general effects of the rest of the explanatory variables are as follows. Lagged violent and property arrest rates are always negative, although not always significant. The unemployment rate has an ambiguous effect. The poverty rate has an ambiguous effect and is never significant in the MM specification. However, it is negative when significant in the AD specification. Personal income is negative when significant in the AD specification and negligible in the MM and Lott-Mustard (LM) specifications. Unemployment benefits have an ambiguous effect. Income maintenance has an ambiguous effect in the LM and AD specifications, but is for the most part negative when significant in the MM specification. Retirement payments are negative when significant.

The population control has a negligible effect. Density is negative when significant. The all-black demographic control in the MM specification is negative when significant. The 10-19 age group control in the MM specification is positive when significant (for rape). The 20-29 age group is consistently positive, but only significant for rape and auto theft. The 30-39 age group has an ambiguous effect and is never significant. The 40-49 age group is never significant and positive for all crimes except aggravated assault. The 50-64 age group is positive, but only significant for robbery. The 65-and-over age group has an ambiguous effect and is never significant. In the AD specification, black males ages 10-19 and 20-29 are always positive when significant. Black males ages 30-39 are always positive and mostly significant. White males ages 10-19 have an ambiguous effect and are never significant. White males ages 20-29 are always positive, although ambiguously significant. White males ages 30-39 are always negative when significant (for assault).

Appendix 3: Summary Statistics of Ayres-Donohue Variables – Identifying the State and Year That Minimum and Maximum Variables Were Obtained for All Explanatory and Dependent Variables

Explanatory Variable	Mean	Std. Dev.	Min	Max
RTC Dummy variable	0.3101	0.4627	0	1
RTC Trend variable	2.0620	4.4679	0	26
Lagged incarceration rate	310.5	170.1	28.5	1937.9
			(North Dakota-1980)	(D.C.-1995)
Lagged police rate	267.1	69.1	73.5	931.1
			(New Mexico-1994)	(D.C.-1991)
Real per capita:				
Personal income	19630.5	11635.48	2114.20	66392.23
			(Mississippi-1977)	(D.C.-2006)
Unemployment benefits	81.69	60.30	5.62	415.20
			(Oklahoma-1978)	(Washington-2002)
Income maintenance	279.70	189.07	12.93	1154.86
			(Wyoming-1977)	(Louisiana-2005)
Retirement payments	2331.66	1551.82	148.30	7555.15
			(Alaska-1977)	(Louisiana-2005)
Unemployment rate	6.11	1.88	2.3	17.4
			(New Hampshire-1987; Nebraska-1990; Connecticut-2000; Virginia-2000)	(West Virginia-1983)
Poverty rate	13.22	3.37	2.9	27.2
			(Connecticut-1989)	(Mississippi-1988)
Density	241.55	511.55	0.697	11176.49
			(Alaska-1977)	(D.C.-1977)
Percent pop black male 10-19	0.0114	0.0076	0.0003	0.0687
			(Montana-1980)	(D.C.-1977)
Percent pop black male 20-29	0.0101	0.0063	0.0003	0.0657
			(Vermont-1979)	(D.C.-1982)
Percent pop black male 30-39	0.0088	0.0055	0.0002	0.0537
			(Vermont-1977)	(D.C.-1992)
Percent pop white male 10-19	0.0613	0.0108	0.0116	0.0983
			(D.C.-1984)	(Vermont-1977)
Percent pop white male 20-29	0.0646	0.0120	0.0239	0.1084
			(Hawaii-2000)	(Wyoming-1980)
Percent pop white male 30-39	0.0641	0.0085	0.0231	0.0973
			(Hawaii-2004)	(Alaska-1986)

Dependent Variable	Mean	Std. Dev.	Min	Max
Murder rate	7.71	3.95	0.16	81.25
			(North Dakota-1994)	(D.C.-1991)
Rape rate	35.46	11.56	7.39	100.60
			(North Dakota-1985)	(Alaska-1981)
Aggravated assault rate	339.32	149.07	31.48	1562.05
			(North Dakota-1983)	(D.C.-1993)
Robbery rate	200.27	123.91	6.40	1632.78
			(North Dakota-1997)	(D.C.-1981)
Auto theft rate	497.53	222.15	91.06	1853.15
			(South Dakota-2006)	(D.C.-1996)
Burglary rate	1097.65	437.48	307.88	2871.15
			(New Hampshire-1999)	(Nevada-1980)
Larceny rate	2820.78	701.45	1179.00	5862.61
			(South Dakota-2006)	(D.C.-1995)

Appendix 4: Definition of Per Capita Personal Income

According to the Bureau of Economic Analysis (BEA), personal income is "the income received by all persons from all sources. Personal income is the sum of net earnings by place of residence, rental income of persons, personal dividend income, personal interest income, and personal current transfer receipts. Net earnings is earnings by place of work (the sum of wage and salary disbursements (payrolls), supplements to wages and salaries, and proprietors' income) less contributions for government social insurance, plus an adjustment to convert earnings by place of work to a place-of-residence basis. Personal income is measured before the deduction of personal income taxes and other personal taxes and is reported in current dollars (no adjustment is made for price changes)" (BEA 2009).

Furthermore, per capita personal income is "calculated as the total personal income of the residents of a state divided by the population of the state. In computing per capita personal income, BEA uses the Census Bureau's annual midyear population estimates" (BEA 2009). In 2008, the average per capita personal income in the United States was $39,751.

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